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The Multi-Site Adult Drug Court Evaluation:

The Impact of Drug Courts

Shelli B. Rossman, John K. Roman, Janine M. Zweig, Michael Rempel, and Christine H. Lindquist (Editors)

Volume 4 Authors:

Shelli B. Rossman
Michael Rempel
John K. Roman
Janine M. Zweig
Christine H. Lindquist
Mia Green
P. Mitchell Downey
Jennifer Yahner
Avinash S. Bhati
Donald J. Farole, Jr.

URBAN INSTITUTE
Justice Policy Center

Final Version

URBAN INSTITUTE
Justice Policy Center
2100 M STREET, NW

WASHINGTON, DC 20037
www.urban.org

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Florida
Osceola County Drug Court—Kissimmee, FL
Volusia County Adult Drug Court Program—Deland, FL

Georgia
Fulton County Drug Court—Atlanta, Georgia
Hall County Drug Court—Gainesville, GA

Illinois
Cook County Drug Court Rehabilitation Alternative Program (R.A.P.)—Chicago, IL
Kane County Rehabilitation Court—St. Charles, IL

New York
Auburn Drug and Alcohol Treatment Court—Auburn, NY
Batavia City Drug Treatment Court—Batavia, NY
City of Niagara Falls Drug Treatment Court—Niagara Falls, NY
Finger Lakes Drug Court (Canandaigua City)—Canandaigua, NY
Finger Lakes Drug Court, Felony Division (Ontario County)—Canandaigua, NY
Lackawanna City Drug Court—Lackawanna, NY
Syracuse Community Treatment Court—Syracuse, NY
Wayne County Drug Treatment Court—Lyons, NY

Pennsylvania
Chester County Drug Court—West Chester, PA
Philadelphia Treatment Court—Philadelphia, PA

South Carolina
York County Drug Treatment Court—York, SC

Washington
CHART Court (Snohomish County)—Everett, WA
King County Drug Diversion Court—Seattle, WA
Kitsap County Adult Drug Court—Port Orchard, WA
Pierce County Felony Drug Court—Tacoma, WA
Thurston County Drug Court Program—Olympia, WA

Comparison Sites
Human Services Associates, Inc.—Orlando, FL
Stewart-Marchman Center for Chemical Independence—Daytona Beach, FL
Illinois TASC—Chicago, IL
Judicial Division 3, North Carolina Probation—NC
Judicial Division 4, North Carolina Probation—NC
Pierce County Drug Offender Sentencing Alternative and Breaking the Cycle—Tacoma, WA

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Highlights

Key Features of the Multi-Site Adult Drug Court Evaluation

The Justice Policy Center at the Urban Institute, RTI International, and the Center for Court Innovation conducted a multi-year, process, impact, and cost-benefit evaluation of drug court impact funded by the National Institute of Justice. The objectives of the National Institute of Justice's *Multi-Site Adult Drug Court Evaluation* (*MADCE*) were to evaluate the effects of drug courts on substance use, crime, and other outcomes, and to illuminate which policies and practices, and which offender attitudes, are responsible for any positive effects that were detected.

Portrait of Adult Drug Courts. A web-based survey of drug courts that primarily served adult clients and had been operational at least one year was conducted between February through June 2004 to develop a portrait of drug courts, and to identify variation across key participant and program domains. Of 593 drug courts that met those criteria, 380 (64 percent) completed the Adult Drug Court Survey.

Process, Impact, and Cost-Benefit Components. The MADCE study tests a series of theoretically-grounded hypotheses on drug court participants and comparison group subjects across 23 drug courts, and 6 comparison sites. NIJ's evaluation (1) tests the hypothesis that drug court participants have lower rates of drug use and criminal activity and show improved functioning compared to similar offenders not offered drug court; (2) tests the effects of variation in drug courts on the outcomes of participants; and (3) assesses drug court costs and benefits. Impact analyses incorporate a multi-level framework. Specifically, individual-level outcomes are modeled as a function of drug court status (drug court or comparison site); exposure to various court policies (e.g., treatment, judicial status hearings, drug testing, and case management), and offender attitudes (e.g., perceptions of the judge, perceived consequences of noncompliance, and motivation to change), while controlling for personal and community characteristics on which the 1,781 offenders and 29 sites may differ.

Findings from the Adult Drug Court Survey guided the selection of adult drug courts, and comparison sites, which were chosen to ensure variation in eligibility criteria, program requirements, community settings, and treatment and testing practices. *MADCE drug courts* included two courts in Florida, two courts in Illinois, two courts in Georgia, eight courts in New York, two courts in Pennsylvania, one court in South Carolina, and six courts in Washington. *Comparison sites* included two sites in Florida, one site in Illinois, two sites in North Carolina, and one site in Washington. Site visits were conducted to each location from mid-year 2004 through early 2005, and again in the spring of 2006, to review program operations, hold semi-structured interviews with key stakeholders, and perform structured court observations.

Study participants were recruited using a rolling enrollment from March 2005 through June 2006. Three waves of participant surveys were administered using Computer Assisted Personal

Interview (CAPI) technology, and Buccal Swab Oral Fluids drug tests were collected at the third survey wave from consenting non-incarcerated participants, as shown below:

Survey and Oral Sample Data Collection and Response Rates

	Dates of Survey Administration	Drug Court Group	Comparison Group	Total Number
Baseline Interviews	March 2005 – June 2006	1,157	627	1,784
6-Month Interviews	August 2005 – December 2006	1,012	528	1,540 (86% of baseline sample)
18-Month Interview	September 2006 – January 2008	952	525	1477 (83% of baseline sample)
18-Month Oral fluids Samples	September 2006 – January 2008	764	383	1147 (95% of non-incarcerated, 18- month sample)

Additional data were obtained from administrative records from the National Crime Information Center at the Federal Bureau of Investigation and state-level databases to capture recidivism at 24 months following baseline.

Design Strengths. Overall, the MADCE research approach has a number of strengths. First, the study was theory-driven based on a conceptual framework spelling out the linkages between drug courts strategies and individual behavior change. Second, the size of the pooled sample and the collection of both offender data and process evaluation data from courts allowed us to open the “black box” of effective drug court practices far beyond past studies of individual drug courts. Third, although quasi-experimental, the MADCE design affords many benefits that a traditional experimental study could not provide. Since we did not require courts to be large enough to generate potentially eligible drug court participants to populate both treatment and control samples, we were able to include small- to medium-sized courts, as well as large courts, the latter of which had already been the subject of a sizable number of drug court studies. The results of this diverse range of community contexts are likely to yield more generalizable results than those from courts in only the largest urban centers. Fourth, by including courts that vary in size, we likely increased the breadth of variation in drug court practices that we were able to study, beyond what would have been possible in the limited number of sites that might have supported a randomized experiment. Lastly, we ultimately were able to include many more drug courts—23 in total—than was originally planned given our ability to geographically cluster sites and pool data across sites.

Given the MADCE quasi-experimental design, however, we had to address three important threats to validity when implementing the impact study: (1) selection bias, (2) attrition bias, and (3) clustering of outcomes within sites. The first two problems—selection and attrition—were handled simultaneously with *propensity score modeling* and a strategy that we refer to as *super weighting*. The third problem—site-level clustering—was handled with *hierarchical modeling*.

Volume 4. The Multi-Site Adult Drug Court Evaluation: The Impact of Drug Courts

This volume details the methodology used for both NIJ's impact evaluation and cost-benefit analyses (see Chapters 2 and 9, and Appendices A, D, E, and F). Key findings are presented that answer the questions, (1) *do drug courts work in reducing substance abuse, crime, and other psychosocial problems;* (2) *do drug courts work better for some types of participants than others;* (3) *what are the mechanisms through which drug courts achieve positive effects; and 4) what are the net benefits of drug courts.* Lastly, this volume summarizes the research team's conclusions regarding the implications of the MADCE study for policy, practice, and future research.

- Drug courts produce significant reductions in drug relapse**.** In the year prior to the 18-month interview, drug court participants were significantly less likely than the comparison group to report using all drugs (56 percent versus 76 percent) and also less likely to report using "serious" drugs (41 percent versus 58 percent), which omit marijuana and "light" alcohol use (fewer than 4 drinks per day for women or less than 5 drinks per day for men). On the 18-month oral fluids drug test, significantly fewer drug court participants tested positive for illegal drugs (29 percent versus 46 percent). Further, among those who tested positive or self-reported using drugs, drug court participants used drugs *less frequently* than the comparison group. [Chapter 3]

- Statistically significant percentages of drug court participants report no relapse during the 18-month period; similarly, drug court participants were statistically significantly less likely to relapse in the first six months. Conversely, a small, but statistically significant, percentage of the comparison group reported no sobriety within the 18 months. [Chapter 3]

- Drug courts produce significant reductions in criminal behavior. In the year prior to the 18-month interview, drug court participants were significantly less likely than the comparison group to report committing crimes (40 percent versus 53 percent), and of those who committed any crime, drug court participants committed fewer. Thus, although both samples averaged large numbers of criminal acts at 18-month follow-up, drug courts reduced that number by half (43.0 versus 88.2 criminal acts in the prior year). Among specific offenses, drug court participation reduced drug possession, drug sales offenses, driving while intoxicated, and property-related crime. Finally, drug courts reduced the probability of an official re-arrest over 24 months (52 percent versus 62 percent), but this last effect was not statistically significant. [Chapter 4]

- With respect to both substance use and crime, improved outcomes at the 6-month interviews were nearly identical to improvements reported at the 18-month interviews, which included at least some *post-program* time for 72 percent of the drug court sample.

For instance, drug court participants were significantly less likely to report drug use in the prior six months (41 percent) than the comparison group (62 percent), a gap that was then largely sustained in the six months prior to the subsequent 18-month interview (46 percent versus. 68 percent). [Chapters 3 and 4]

- Drug court participants experience select benefits in other areas of their lives besides drug use and criminal behavior. At 18 months, drug court participants were significantly less likely than comparison offenders to report a need for employment, educational, and financial services, suggesting that drug court participation addressed those needs. Further, drug court participants reported significantly less family conflict than comparison offenders. However, there were only modest, non-significant differences in 18-month employment rates, income, and family emotional support; and the samples did not differ in reported symptoms of depression or in experiencing homelessness. [Chapter 5]

- Given that analyses showed drug courts produce substantial reductions in substance use and crime, we tested whether these effects were especially pronounced among some, but not other categories of offenders, defined by demographics, social ties, prior drug use, criminality, or mental health. Across multiple categories of offenders, extremely few differences in the magnitude of the drug court impact were found. Nearly all categories of offenders benefitted comparably from the drug court intervention, suggesting that widespread drug court policies to restrict eligibility to narrow sub-populations may be counter-productive. Specifically, there were not any subgroup-based differences in the rate of positive drug tests, and only 3 of 17 subgroups self-reported less drug use at 18 months. Drug courts also impacted criminal behavior similarly across most subgroups. However, a small number of subgroups experienced differential effects: relative to similar offenders in the comparison group, those reporting more frequent drug use at baseline showed a particularly large reduction in drug use at the 18-month follow-up. Concerning criminal behavior, offenders with violent histories showed a greater reduction in crime than others at follow-up. We also found that those showing symptoms of mental health problems (narcissism and depression, but not antisocial personality disorder) evidenced smaller reductions in drug use and crime than those without these problems. [Chapters 3, 4, 5]

- There is a direct effect of drug court participation on desistance from drug use and criminality; after controlling for all significant individual risk factors, court practices, and theoretical mediators, there remains an independent effect of drug court on improved behavior. Drug courts participants reported fewer subsequent days of drug use and crimes committed per month, on average across all courts, 18 months later, and, they expressed more positive attitudes toward the judge at their 6-month interview, which in turn was associated with lower levels of drug use and crime at their 18-month interview, on average across all courts. [Chapter 6]

- Drug courts increased court appearances, weeks of drug treatment, drug tests, and sanctions. Although there were no indirect between-courts effects of drug court on drug use via court practices, there was a within-courts effect of certain court practices on attitude toward judge, such that individuals who made more court appearances, received more weeks of drug treatment, and were subjected to more drug tests had better attitudes toward the judge. [Chapter 6]

- Judicial interactions with drug court participants are key factors in promoting desistance. Multilevel structural equation modeling (MSEM) found no evidence that treatment motivation, specific deterrence, fairness of one's court outcome, or a broad measure of procedural justice are associated with desistance in the MADCE sample. There are three potential explanations for this finding: (1) the results signify exactly what they purport—namely, that those theoretical processes are not associated with better outcomes in drug court; (2) possibly the MADCE drug courts failed to effectively implement practices that would promote those theoretical mechanisms (i.e., although drug courts self-reported adherence to best treatment practices, the treatment may not have been implemented in ways consistent with effective evidence-based practices); or (3) the power of the judge (typed by legal scholars as therapeutic jurisprudence) is so strong that it effectively suppressed all other theoretical mechanisms. [Chapter 6]

- MSEM found that drug courts appear to be equally effective for everyone, and, that the mechanisms of effectiveness are the same for all participants. While some subgroups (such as younger participants or participants with anti-social personality disorder) have worse outcomes, those attributes did not moderate the drug court effect. Simply restated, while we find evidence that those groups do worse than average, they appear to have similar improvements as other participants, and thus do better than they would have without drug courts. This finding argues against the common drug court practice of attempting to identify ex ante a population that is at a lower risk of recidivism. [Chapter 6]

- Court-level analyses were performed to explore which policies and practices—leverage, predictability of sanctions, adherence to treatment best practices, drug testing, case management, judicial status hearings, point of program entry, multi-disciplinary decision-making among drug court team members, positive judicial attributes, and judicial interaction—predict drug court effectiveness. Leverage, predictability of sanctions, the point of entry into the program during the criminal justice, and positive attributes of the judge were found to be effective at crime prevention; and three of the four (excluding leverage) were found to be effective in substance use prevention. Specifically, the courts that prevented higher numbers of criminal acts per month demonstrated high leverage, medium predictability of sanctions (i.e., the court formally communicated how and when participants would be sanctioned for noncompliance, but retained some flexibility in applying the pre-determined sanctioning schedules); client populations that enter at the same time point in the criminal justice process (i.e., either all pre-plea or all post-plea), and medium or high scores on positive judicial attributes. Courts that prevented more days of drug use per month evidenced medium predictability of sanctions, client populations that enter at pre-plea, and high scores on positive judicial attributes. Additionally, when courts implemented the combined practices, there appears to be a synergistic effect such that they are able to prevent the most crimes and the most days of drug use for many subgroups. [Chapter 7]

- A dosage analysis was performed—comparing drug court clients who received more of certain services/interventions to those who received lower levels—using weighted samples that allowed estimation of the effect of receiving low, medium, or high levels of court services/interventions as if dosage were randomly assigned within the population. Drug court clients who received higher levels of judicial praise, judicial supervision, and case management reported fewer crimes and fewer days of drug use after balancing the dosage levels on attributes related to client risk for these behaviors. In addition, drug court clients who participated in more than 35 days of drug treatment had fewer crimes at 18 months and fewer days of drug use at both 6 and 18 months, although treatment in excess of 65 days did not produce additional reductions beyond that provided by 36 to 65 days of treatment. The effect of leverage provided by a very severe sentence for drug court failure was limited to a reduction in days of drug use at 18 months, an important outcome. Some domains did not have the expected effect on drug use and crime. Providing drug treatment in the first month of drug court (an immediate intervention) was associated with increases in numbers of crimes and a slight increase in drug use reported at six months. Increases in the number of support services similarly was related to increases, not decreases, in number of crimes and days of drug use at 18 months. It is possible that risk factors not controlled by balancing drove the early treatment and additional support service decisions. A medium level use of jail sanctions (between 1 and 20 percent of imposed sanctions) was associated with increased number of crimes and days of drug use at 6 months, and to a lesser extent, with an increased number of days of drug use at 18 months. [Chapter 8]

- Drug courts invest more money than the comparison sites in community-based services and in court supervision. Drug courts costs are higher than business-as-usual case processing due to larger program investments, including significantly more drug tests, judicial status hearings, time with case managers, and substance abuse treatment. [Chapter 9]

- Drug courts save money through improved outcomes. Drug courts save money through improved outcomes, primarily savings to victims from significantly fewer crimes, re-arrests, and days incarcerated (whereas a slight increase in participant wages relative to the comparison group was not statistically significant). [Chapter 9]

- Overall, the net benefit of drug courts is an average of $5,680 to $6,208 per participant, returning $2 for every $1 of cost, but these findings are not statistically significant. Rather, in this study, findings were driven by a reduction in the most serious offending by relatively few individuals, not by a widespread reduction of serious offending. Drug courts prevent a great deal of crime, but the majority of crimes have small costs to society. An important implication is that drug courts are especially likely to save money if they enroll serious offenders (who, in the absence of drug court, are particularly likely to engage in serious future offending). [Chapter 9]

- MADCE generated a number of key implications for practice with regard to the role of the judge, drug court eligibility requirements, the use of leverage, drug testing. Additionally, the findings from this research strongly substantiate that drug courts work and should be encouraged both to include more serious offenders to achieve greater returns on drug court investments, and to serve greater numbers of participants, so that positive impacts are not limited to small numbers of offenders. [Chapter 10]

Chapter 1. Introduction: Key Features of the Impact Evaluation

Shelli B. Rossman

Beginning in 2003, the Justice Policy Center at the Urban Institute (UI-JPC) partnered with RTI International (RTI) and the Center for Court Innovation (CCI) to conduct the Multi-Site Adult Drug Court Evaluation (MADCE) funded by the National Institute of Justice (NIJ). The main objectives of this project were to evaluate the effect of drug courts compared to other criminal justice responses for individuals with substance use issues, and to examine the effect of different drug court practices and key components on participant outcomes. The project was structured in two phases. During the first phase, the research team undertook a one-year planning process in which we developed instruments and data collection protocols, as well as conducted a web-based survey to (1) develop a countrywide picture of adult drug courts and (2) support site selection for the research to be undertaken in the second phase. The second phase entailed three major components focused on performing process, impact, and cost-benefit evaluations.

The objectives of the MADCE study are to

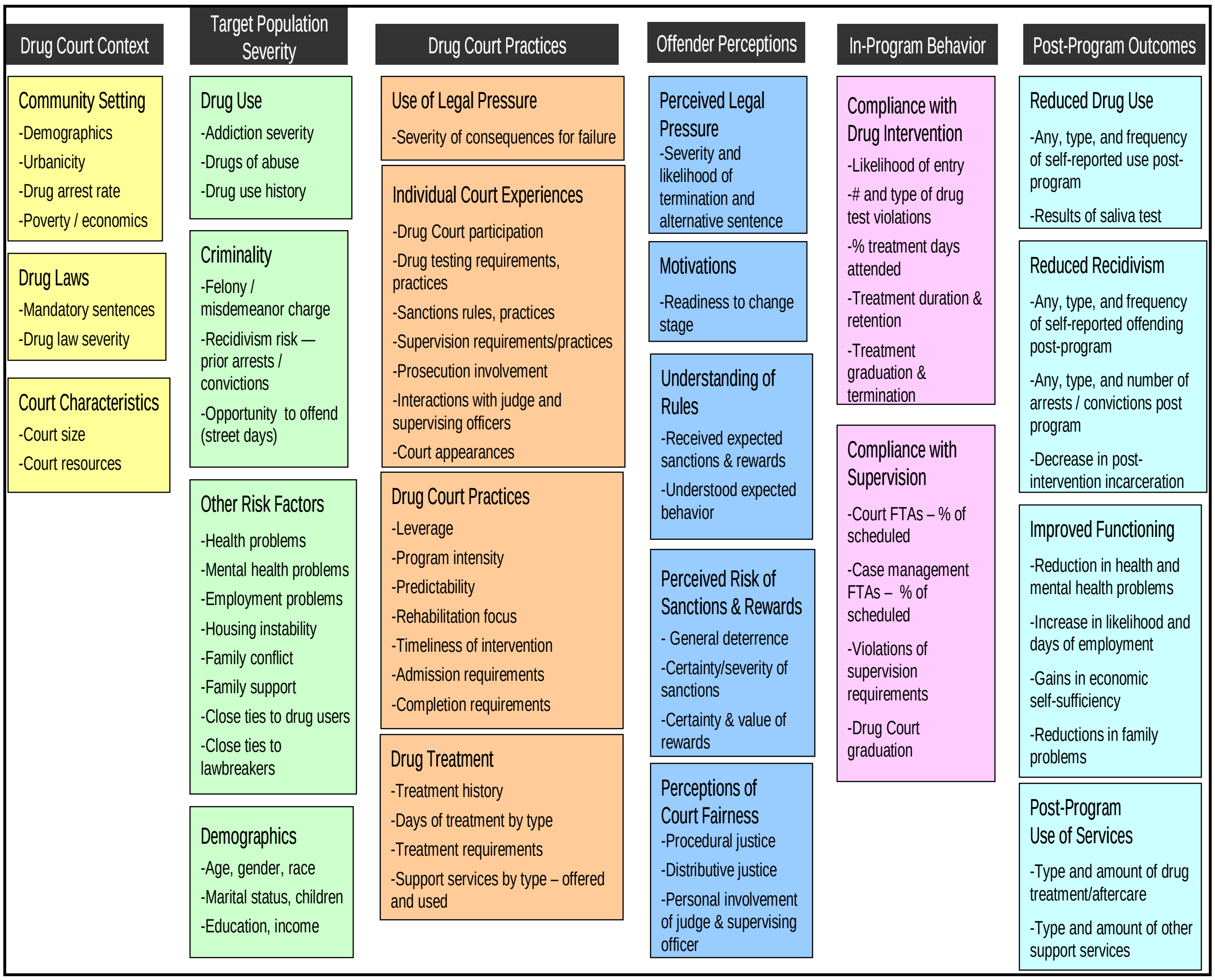
- Test the hypotheses that drug court participants achieve better outcomes related to continued substance use and recidivism than similar offenders not exposed to drug courts;
- Isolate key individual and program factors that influence the outcomes; and
- Test effects of variations in implementing the drug court model on participant outcomes.

The MADCE research design is in a strong position to yield unbiased answers that can be reasonably generalized to drug courts nationwide. As described in earlier Volumes (e.g., Chapter 3, Volume 1) results are based on a sample of 23 adult drug courts and 6 comparison sites from 8 states located throughout the country. Although we did not employ a systematic random sample of sites, and some regions of the country are under-represented, the study nonetheless represents the largest and broadest multi-site effort to date, providing a unique opportunity to estimate the likely average effects of today's adult drug courts.

As previously described, we collected a wealth of offender participation and outcome data, extending well beyond the restriction of most previous studies to official recidivism impacts only. The design included a baseline and two follow-up waves of offender surveys at 6- and 18-months post-enrollment, as well as official crime records at 24 months, which allowed us to examine whether drug court effects are durable or recede over time. Additionally, the multi-wave design enabled us to (1) model the relationship between offender program experiences and attitudes during the first 6 months with outcomes at the 18- and 24-month marks and (2) compare drug court effects on resource allocations to courts. *Chapter 2. Impact Methodology* and *Appendix A. Technical Appendix: Analytic Strategy for Producing Unbiased Estimates of Drug Court Impact* detail the methodology we used to produce unbiased estimates of drug court impact.

The topics addressed in this Volume are driven by our interest in testing the conceptual framework for drug courts that we developed during the proposal stage of this research. The framework, introduced in Chapter 1 in Volume 1, is again presented here for the convenience of the reader. As described in Volume 1, the MADCE framework builds on the earlier models proposed by Temple University (Goldkamp, White, and Robinson 2001) RAND (Longshore, Turner, et al. 2001), and the Urban Institute (Butts, Roman, Rossman, and Harrell 2004), by hypothesizing causal linkages to be tested in the evaluation (see Figure 4-1.1).

Figure 4-1.1. NIJ's Multi-Site Adult Drug Court Evaluation Conceptual Framework



Prior drug court evaluations relied heavily on recidivism as the key measure of impact, despite the centrality of the goal of reducing drug use. By comparison, NIJ's MADCE study was planned to measure multiple outcomes following the period of drug court completion as shown in the far right column, based on information self-reported by subjects, and supplemented and validated by criminal records and drug testing. In particular, the impact evaluation was designed to test whether adult drug courts reduce drug use, criminal behavior, and other associated problems, including socioeconomic dislocation, family dysfunction, mental illness, and incarceration time.

Although relatively few of the extant drug court studies address substance abuse outcomes (Brewster 2001; Deschenes, Turner, and Greenwood 1995; Gottfredson, Kearley, et al. 2005; Harrell, Roman, and Sack 2001), the MADCE research does. In *Chapter 3. Do Adult Drug Courts Reduce Drug Use?*, we examine a series of hypotheses regarding drug courts' impacts on drug use, including that:

- Adult drug courts reduce drug and alcohol use.
- Substance use program impacts persist over time.
- Trajectories of relapse and recovery demonstrate favorable results in terms of (1) delaying the time at which relapse occurs, (2) decreasing the total number of months during which those who relapse continue to use, (3) decreasing the frequency of use during months when drug users are using, and (4) harm reduction (i.e., those who relapse use less severe drugs than their initial primary drug of choice).

In addition, although barely examined in prior research, we test whether drug courts are particularly effective in reducing drug use among specific categories of offenders, defined by their baseline characteristics (e.g., more severely addicted offenders, those with stronger community ties, or those with co-occurring mental health disorders).

Chapter 4. Do Drug Courts Reduce Crime and Incarceration? focuses on the criminal justice effects of adult drug courts, many of which have been well documented in the literature (Finigan, Carey, and Cox 2007; Goldkamp et al. 2001; Gottfredson, Kearley, et al. 2006; Government Accountability Office 2005; Rempel, Fox-Kralstein, et al. 2003; Roman and DeStefano 2004; Schaffer 2006; Wilson, Mitchell, and MacKenzie 2006) . As in Chapter 3, the analyses test such hypotheses as (1) reductions in criminal behavior result from participation in treatment courts, and (2) effects on criminal behavior are durable over time. Additionally, we examine whether drug courts demonstrate different levels of effectiveness in achieving reductions in crime depending on offenders' risk levels for future criminality. Lastly, we test whether adult drug courts provide true "alternatives to incarceration," such that program participants spend less time in custody on the precipitating criminal case than otherwise would have been the case.

Little extant research has examined whether, and to what extent, adult drug courts impact psychosocial or health outcomes, either during or beyond program participation. What's more, the findings from such studies (Cosden, Peerson, and Orliss 2000; Gottfredson et al. 2005; Harrell et al. 2001; Harrell, Cavanagh, and Roman 1999) evidence mixed results. In *Chapter 5. Beyond Crime and Drug Use: Do Adult Drug Courts Produce Other Psychosocial Benefits?*, we test hypotheses regarding the ancillary benefits of drug court participation. In particular, we test results in four domains in both the 6- and 18-month timeframes:

- Socioeconomic status, measuring employment, educational, and supportive services outcomes.

- Mental and physical health status, including receipt of public healthcare assistance.
- Family support and conflict.
- Homelessness.

Drug court strategies combine coercion and persuasion with the goals of encouraging treatment participation, and reducing substance use and criminal behavior. As depicted in Figure 4-1.1, post-program outcomes are hypothesized to result both from (1) the behavior of offenders while under supervision of the court, particularly their participation in drug treatment and compliance with drug court supervision (shown in the second column from the right) and (2) participant perceptions and responses to court practices (third column from the right) that are hypothesized to be the process leading to behavioral change. Virtually, no other drug court evaluations—with the exception of Gottfredson and colleagues (2007)—have directly examined pathways to desistance from drug use and crime.

Chapter 6. How Do Drug Courts Work? reports the results of a multilevel structural equation model that empirically tests theoretical pathways to desistance from substance use and criminal behavior. The path model delineates how drug court practices change perceptions and attitudes, and how such changes subsequently affect drug use and crime. Mediators include:

- Changes in court practices (e.g., court appearances, drug testing, and treatment) and psychological characteristics such as perceived risk and reward (deterrence).
- Perceived legitimacy (procedural justice).
- Attitudes toward the judge.
- Motivation to change one's own behavior through substance abuse treatment.

However, as the MADCE conceptual framework anticipates (third column from the left), it is also likely that variation in implementation of court policies and practices across drug courts is associated with differential effectiveness. While various drug courts share some elements in common, it is also the case that prior studies have documented rather substantial variation in the implementation of core policies and practices (Carey, Finigan, and Pukstas 2008; Rempel et al. 2003). Therefore, the MADCE was specifically designed to support examination of the impact of implemented policies and practices on client outcomes. Such an approach is feasible given the relatively large number of courts (N=23) that were purposefully selected to reflect variation in key policies and practices (see Volume 1, Chapter 3 for details).

Given our conceptual framework, in *Chapter 7. Impacts of Court Policies and Practices*, we chose ten specific court policies and practices to explore in relation to drug courts' abilities to prevent future substance use and crime. Specifically, we tested the effects of court implementation of policies and practices related to:

- Leverage.
- Predictability of sanctions.
- Adherence to treatment best practices.
- Drug testing.
- Case management.
- Judicial status hearings.
- Point of entry into the program.
- Multidisciplinary decision-making among the drug court team.
- Positive judicial attributes.
- Judicial interaction.

Here again, our findings are presented for numerous client subgroups (based on demographic characteristics, previous drug use and treatment history, and criminal history), reflecting a growing body of literature that supports the notion that not all participant subgroups respond identically. Since few previous studies have isolated the impact of court policies and practices on drug court effectiveness, we believe that our findings in this regard will have practical utility for drug court practitioners in aiding their efforts both to introduce evidence-based program refinements and to target policies or practices specific to the participant subgroups they serve.

Chapter 8. Drug Court Practices: An Analysis of Dosage Effects also addresses the relationship between program practices and outcomes, by performing a dosage analysis that compares drug court clients who received more of selected services to those who received lower levels. The MADCE research, like other studies of human services programs, was interested in establishing the extent to which different levels of services, such as substance abuse treatment, impact client behavior. However, we recognized that in some nontrivial way, the amount of services individuals need is related to their general riskiness: those who are at low risk of bad behavior receive different frequencies and quantities of drug court interventions than those at higher risk. Individual drug court clients are heterogeneous in their ex ante needs for varying drug court services, according to both the underlying risk and more direct underlying needs for each service. So, for instance, the amount of drug treatment provided varies according to both (1) the client's treatment needs and (2) temporally endogenous responses to the client's bad behavior. In our view, an ex-ante measure of risk can be constructed that accounts for the endogenous response to behavior and differential need for services that confounds the drug court effect.

Concerned with the issue of endogeneity,[1] we considered ways to mitigate the reverse causality problem. One such approach that has gained popularity in other disciplines is to use an instrumental variable to break the endogeneity. However, as detailed in Chapter 8, our solution was to use a conceptually similar model—propensity score weights—for resolving the problem of endogenous regressors. Our analysis assessed the effect of variations in the dose of nine practices—amount of drug treatment, immediacy of intervention, legal leverage, severity of sanctioning, rewards, level of judicial supervision, level of case management, level of drug

[1] A factor is *endogenous* to a system if it is determined within the system, and *exogenous* if it is determined outside. While it is relatively easy to postulate whether a variable is endogenous or exogenous in a theoretical model, there is always an empirical question as to whether the model is adequate, and thus whether variables that are theoretically exogenous are in fact endogenous to the system being modeled. For additional discussion of *endogeneity*, see Chapter 8 in this Volume or Millimet (2001), available online at http://www.stata.com/support/faqs/stat/bias.html.

testing, and support services received—on (1) the number of crimes reported per day of street time within the first 6 months and (2) the entire 18 months, and (3) the number of months of drug use per day of street time within the first 6 months and (4) the entire 18 months after study enrollment.

In *Chapter 9. Cost-Benefit Analyses*, we move from analysis of the impacts of drug courts to a consideration of the economic ramifications of such interventions. Unlike many other studies, our approach for the MADCE is to use a bottom-up approach, in which we iteratively aggregate cost categories (e.g., drug tests, hearings, case management, drug treatment, and administrative costs) with benefits (which are generally measured as reductions in costs associated with the individual, such as costs of new crimes) into a single net benefits variable, measured on the individual level. While many extant studies have mainly focused on a very limited set of potential benefits of drug court that could yield benefits to society, our approach is considerably more expansive in detailing a variety of benefits not usually reported in the existing literature. In particular, the detailed, extensive data collection undertaken in the MADCE afforded us the opportunity to examine employment, welfare and financial support, medical and health care costs, child support payments, and a number of other potential benefits.

Lastly, in *Chapter 10*, we summarize the key findings from the process, outcome, impact, and cost-benefit components of the MADCE study; and, importantly, we identify implications for practice, policy, and future research. Practical implications include recommendations related to the role of the judge, drug court eligibility requirements, case processing, sanctioning policies and practices, leverage, case management, and treatment. Policy implications are related to the best use of funding for drug courts, and the advisability of developing standards of practice for the field. Research implications include identifying the next steps for the field in terms of remaining research questions.

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Chapter 2. Impact Methodology

Michael Rempel, with Donald J. Farole, Jr.

The MADCE study had to address three important threats to validity: (1) selection bias, (2) attrition bias, and (3) clustering of outcomes within sites.

- *Selection Bias*: This problem would arise if drug court and comparison offenders significantly differed in their baseline characteristics (e.g., demographics, social ties, drug use history, criminal history, or mental health status). Such differences might endow one group, or the other, with inherent advantages that increase the likelihood of positive outcomes, independent of the effects of drug court participation per se.

- *Attrition Bias*: This problem would arise if a significant percentage of offenders could not be located for follow-up surveys, and if the characteristics of those surveyed at follow-up significantly differed from those surveyed at baseline. In such a case, it might only be possible to generalize the results to a narrow sub-sample of the true population of interest (e.g., only to low-risk offenders, who may be easier to locate at follow-up).

- *Site-Level Clustering*: This problem would arise if offender outcomes clustered at the site level—with some sites producing a systematically different range of outcomes than others. If site-specific factors other than drug court status led drug court sites to produce better or worse outcomes than comparison sites, the reported results would be biased.

The first two problems—selection and attrition—were handled simultaneously with a strategy that we refer to as *super weighting*. The third problem—site-level clustering—was handled with *hierarchical modeling*. This chapter introduces both of those strategies, whereas finer details are reserved for a technical appendix (see Appendix A).

Super Weighting

The "super weighting" strategy for NIJ's study was adapted from a multi-site evaluation of two specialized domestic violence courts (Harrell, Newmark, et al. 2007). The essential outline is as follows. First, we used standard propensity score modeling techniques to correct for baseline differences between the drug court and comparison samples (selection bias). Next, we employed a parallel set of techniques to correct for baseline differences between retained and attrited cases as of the two follow-up surveys (attrition bias). Finally, we combined the two adjustments into a single weight variable that could be used to weight cases before conducting final impact analyses.

Adjusting for Selection Bias: Propensity Score Modeling

The first step in adjusting for selection bias was to determine the precise extent of that bias, answering to what extent the 1,156 drug court offenders differed at baseline from the 625 comparison offenders. If the samples did not differ, further adjustments would be unnecessary. However, considering that the two samples were each from different sites and were unequally distributed across eight states, it would have been remarkable if no differences arose.

We selected 61 characteristics from the baseline offender survey, spanning demographics, social ties, drug use history, criminal history, and mental health status (see the full variable list in Appendix A). We then measured bivariate sample differences and found significant differences on 37 of the 61 items (at least at $p < .05$), indicating a severe selection bias problem.

We next implemented a series of standard propensity score modeling procedures (see Luellen, Shadish, and Clark 2005; Rosenbaum and Rubin 1983, 1984; Rubin 1973). In brief, a propensity score is a number from 0 to 1 that can be assigned to each offender, reflecting the predicted probability that the offender falls into one as opposed to another of two samples—in this case, the drug court as opposed to the comparison sample. The propensity score can derive from a large number of baseline characteristics, and represents their summary effect in leading some cases to be statistically more likely than are others to be in one of two samples. For modeling purposes, we decided to include all baseline characteristics whose bivariate comparison revealed a p-value of .50 or less. Overall, we included 47 of the 61 variables whose bivariate differences were examined, enabling us to account for even slight differences on an unusually large number of baseline characteristics. As detailed in Appendix A, we re-ran our propensity model several times in response to initial diagnostics (e.g., adding additional variables or interaction terms), until ultimately arriving at a model that proved to be effective in addressing all baseline differences. We then re-ran our final model in order to generate separate propensity scores for four sub-samples. They were (1) retained for the 6-month survey, (2) retained for the 18-month survey, (3) retained for *both* follow-up surveys, and (4) retained for the oral fluids drug test.

Although our propensity model appeared highly effective in taking potential biases into account that were based on *observed* offender characteristics, we next contemplated whether there might be *unobserved* characteristics that could importantly differentiate the samples. Our dataset was vast; however, we unfortunately did not collect baseline data on motivation to change. If drug court participants were more motivated than comparison offenders, participants might show better outcomes for this reason, rather than due to the impact of the drug court per se.

For two reasons, however, we did not consider our inability to observe motivation at baseline to create a plausible source of bias. First, we considered it likely that many of the almost five dozen observed characteristics on which we *could* adjust would be correlated with motivation. Therefore, even if we could not control for motivation directly, we presumed that we were most likely controlling for it indirectly through other measures with which motivation would be correlated. Additionally, through other analyses, we were able to determine that motivation was not a strong predictor of outcomes. Specifically, our study *did* include a motivation index. We did not view the results for this index to comprise a “true baseline” measure, because the baseline surveys were administered approximately one month after entry into the drug court or comparison conditions, and we believed that motivation was a factor that could change rapidly within that first month. Nonetheless, the existence of a “one-month motivation” measure allowed us to test the association of early motivation with outcomes. We found no connection between motivation and criminal activity at 18 months. Although the one-month motivation measure had a slight positive relationship with several drug use outcomes at 18 months, the relationship was weak and often non-significant. For example, the simple correlation between the one-month motivation score and days of drug use per month at the 18-month mark was only -.047 ($p = .069$). We concluded that although having a valid true baseline measure of motivation would have been helpful to the study, there was little reason for concern that the lack of such a measure could create any meaningful selection bias.

Adjusting for Attrition Bias: Retention Score Modeling

We essentially handled attrition in a parallel fashion as selection, with one caveat: we hypothesized that any differential probability of attrition might ultimately have less to do with the baseline characteristics of different offenders than with the community-level characteristics of some, but not other sites; or with the effectiveness of the team of research interviewers that were assigned to some, but not other state-based geographic clusters. For this reason, we proceeded first by constructing court-level dummy variables (coded 0 or 1) for each state cluster (Florida, Georgia, Illinois, New York, North Carolina, South Carolina, Pennsylvania, and Washington). We then examined bivariate differences between retained and attrited cases on the same 61 baseline characteristics that were analyzed for selection bias and on the aforementioned state cluster variables. Separate comparisons were conducted between those retained versus attrited at 6 months, 18 months, and both periods. As detailed in Appendix A, we found relatively few significant differences in the baseline characteristics of cases that were respectively retained and attrited. However, consistent with our hypothesis that locating offenders at follow up might be systematically easier in some locations than others, we did detect multiple significant differences between retained and attrited cases on our state cluster variables. In particular, retention rates were significantly higher in New York, North Carolina, and Washington; and significantly lower in Florida, Illinois, and Pennsylvania.

We next developed a *retention model*, whose meaning is essentially the same as propensity model above except that a retention model predicts the likelihood of retention at follow-up, rather than the likelihood of falling into the drug court or comparison sample. In all, we entered 18 baseline characteristics and 6 state cluster variables in all retention models, essentially including those variables on which retained and attrited cases appeared to differ, based on the bivariate comparisons (see Appendix A for details and rationale).

Computing Super Weights

The essential concept of super weighting involves assigning a different inverse probability weight to each case based on the product of its propensity score and retention score (specific formulas are presented in Appendix A). On an intuitive level, the effect is to combine the propensity and retention scores in a fashion that accords a higher weight to underrepresented categories of offenders and a lower weight to overrepresented categories. For example, the many drug court offenders with a high propensity score (overrepresented) each received a lower weight than did the few drug court offenders with a low propensity score (underrepresented). Conversely, the few comparison offenders with a high score (underrepresented) received a higher weight than did comparisons with a low score (overrepresented). Analogous implications applied in the handling of retained and attrited cases. Separate super weights were computed for retention at 6 months, 18 months, both periods, and for the oral fluids test.

The super weights virtually eliminated observable selection and attrition bias. After weighting, there were not any significant differences between drug court and comparison offenders at 6 months, two differences at 18 months, one at both periods, and none for those who took the oral fluid test. In Appendix A, Table 4-A.4 illustrates the effect of super weighting for those who were surveyed at 18 months. The table compares the samples on our standard set of 61 baseline characteristics, first using unweighted data and then weighted data. The results demonstrate dramatic and consistent reductions in the magnitude and significance of sample differences.

Regarding attrition bias, the 6-month weights eliminated all except three significant differences between those retained and attrited at 6 months; the 18-month weights and the weights for those retained versus attrited at both follow-up periods each left two significant differences. In Appendix A, Table 4-A.5 illustrates the effect of super weighting by comparing those who were retained versus attrited as of the 18-month survey. These comparisons include our standard 61 characteristics along with the state cluster variables. The results demonstrate that super weighting reduced the magnitude and number of significant differences, particularly with respect to the state clusters, on which extensive differences existed when using unweighted data.

Super Weighting for Official Recidivism Outcomes

We next duplicated the same process described above to create a special set of super weights to apply exclusively in analyses of official records outcomes. We sought to obtain such records for a 24-month tracking period for the 1,577 offenders (89 percent of our initial sample) who gave explicit permission during our informed consent process. Specifically, official records data were obtained from Statistical Analysis Centers (SACs) in five of the eight states that housed our 29 total sites; in two states, we negotiated data-sharing agreements with multiple state agencies to collect the desired data, and in the remaining state, despite having successfully negotiated a data-sharing agreement, criminal history data and incarceration records were collected manually from agency websites (i.e., the state's department of corrections and bureau of investigation). We also obtained official records data from the National Crime Information Center (NCIC) of the Federal Bureau of Investigation (FBI). Although the NCIC is a single data source, its dataset reflects information that was separately submitted by numerous local police departments nationwide, such that the data are not necessarily comparable across sites. Of some particular concern, federal reporting requirements are more stringent for serious than for low-level (e.g., misdemeanor) cases; hence, the NCIC data are likely to exclude many re-arrests on less serious charges. For this reason, we relied on the state SAC data for all in-state arrests, and supplemented with NCIC data for out-of-state arrests. We also relied on NCIC data for two sites in a state where the SAC could only provide incomplete records. Of the 1,577 consenting offenders, we obtained an official criminal records match for 1,534 (97 percent), including 1,015 drug court and 519 comparison offenders. By comparison, the NCIC data provided criminal record information on 89 percent of the 1,577 consenting offenders, a substantially lower match rate than what we obtained by relying primarily on the state-based SACs.

Having finalized our official records dataset, we then repeated in full an equivalent process to the super weighting strategy that was described above. The precise process paralleled that described above and, for that reason, is not reiterated. In short, we matched on the same 61 baseline characteristics and state cluster variables noted above, although for the propensity model, we added five measures of *official* criminal history. They were (1) whether the offender had a prior arrest, (2) the number of prior arrests, (3) whether the offender had a prior drug arrest, (4) the number of prior drug arrests, and (5) the type of instant case arrest charge that brought the offender into the study. For this last variable, we recoded the dozens of state-specific charges obtained from each state-based source into three broad categories: drug related, property related, or other charge. As shown in Table 4-A.6 of Appendix A, the two samples differed on four of these measures (the exception being the percentage of offenders with at least one prior arrest, which was exactly 90 percent for both samples). Accordingly, the four other measures were all added to the propensity model.

Hierarchical Modeling

As in all multi-site evaluations, the individual observations in the data—that is, the individual offenders—do not necessarily comprise independent observations, as is required by the assumptions of standard bivariate and multivariate methods. Instead, the observations are each nested within 1 of 29 distinct sites. These sites differ in whether they are drug court or comparison sites. They also may differ in other ways that are observable (e.g., community-level demographics) or unobservable (e.g., nuances of drug law enforcement or community-level collective efficacy). As a result, it is possible that key outcomes of interest (e.g., criminal behavior, drug use, socioeconomic gains, etc.) cluster at the site level—that offenders from the same sites exhibit a site-specific mean and variance. In this study, that could comprise a source of bias, especially if drug court offenders averaged systematically better or worse outcomes than comparison offenders due not to drug court participation per se, but to other systematic differences between drug court and comparison communities. As an intuitive example, if the location of the comparison sites entailed, on average, easier access to illegal drugs, stronger deviant peer influences, or weaker collective efficacy than the location of the drug court sites, drug court offenders might show better outcomes exclusively for these contextual reasons—a bias that could be masked if one relies on standard statistical methods.

Hierarchical modeling techniques adjust for the clustering of outcomes within sites (see Raudenbush and Bryk 2002). In particular, these techniques correct the degrees of freedom based on the much smaller number of sites (29) than of offenders (1,781). Furthermore, drug court status appropriately becomes a “Level 2” characteristic of sites, rather than a “Level 1” characteristic of individuals. By treating drug court status as a Level 2 variable, we avoid the appearance of statistically significant drug court effects that, in fact, might be spurious, due only to one or a few high-volume sites happening to produce especially positive or negative outcomes. In short, hierarchical modeling reduces the probability of Type I errors that involve incorrectly reporting an effect as significant. The intuitive drawback is that, since statistical power is greatly reduced at Level 2, hierarchical modeling raises the prospect of Type II errors that involve not reporting a significant effect when it truly exists in the real world. Accordingly, although the adoption of a hierarchical modeling framework is a conservative and logical choice, it does carry the practical risk of leaving some research questions unanswered, should seemingly meaningful effect sizes fail to reach statistical significance. By comparison, a traditional analytic strategy might have lent itself to the production of more definitive answers for practitioners, though the answers would have been based on a less rigorous and conservative strategy.

Acknowledging this tradeoff, we determined to employ hierarchical modeling, so long as it was indeed the case that our outcomes of interest were clustered within sites. In analyses reported within Appendix A (e.g., see Table 4-A.7), we found that site-specific clustering was indeed present for all of our major outcomes, confirming the need for a hierarchical framework.

Analytic Plan

Impact Analyses: Do Drug Courts Work?

In answering whether drug courts produce positive benefits, we ran all final models using weighted data and hierarchical modeling methods in HLM 6.04. We divided our many outcome measures among seven domains:

- *Drug Use*: e.g., whether the offender used drugs, days of drug use per month, and results of the oral fluids drug test.

- *Criminal Activity*: e.g., incidence and prevalence of official re-arrest and of self-reported criminal behavior (up to 18 months for self-report, and 24 months for official recidivism).

- *Incarceration*: e.g., number of days incarcerated up to 18 months post-baseline on the offender survey, number of days sentenced to jail or prison up to 24 months post-baseline in official records data, and number of days sentenced to jail or prison specifically in the precipitating criminal case.

- *Socioeconomic Status*: e.g., employment status, school status, and annual income.

- *Mental Health*: e.g., classified as “depressed” (based on multi-item instrument) and self-reported assessment of mental health (excellent, very good, good, fair, or poor).

- *Family Support and Conflict*: e.g., drawing on multi-item indices, the extent of family conflict, family emotional support, and family instrumental support.

- *Homelessness*: e.g., whether the offender was homeless since the previous survey point.

When analyzing results on each outcome measure, we entered drug court status (drug court or comparison site) as a single Level 2 predictor variable—without any other predictors. As discussed above, our weighting strategy successfully adjusted for baseline differences between drug court and comparison offenders. Accordingly, having balanced the samples through weighting, we considered it unnecessary to add multivariate controls.

Since the 18-month weights were effective in eliminating selection bias among cases that were retained at all other periods (6 months, both periods, oral fluids drug test, or recidivism data available), we opted to employ these weights universally, rather than change the weights for different analyses. To ensure that this decision—which is primarily one of elegance, rather than the most insistently conservative analytic decision—did not substantively affect the reported findings, we conducted sensitivity analyses, as reported below.

For each outcome measure, we selected the most appropriate regression specification, of those that are available in HLM: ordinary least squares for normally distributed outcomes, logistic regression for dichotomous measures (any criminal behavior), and Poisson regression for count distributions that are right-skewed. Unfortunately, HLM software does not enable using a negative binomial specification, which is designed for the same kind of data as Poisson regression, but where the skewing is particularly extreme. To provide easily interpreted "bottom-line" results, we transformed the regression coefficients for the intercept and for drug court status to produce adjusted mean outcomes for drug court and comparison offenders on each measure.

In other words, our analytic chapters present readily interpretable percentages or averages—percent using drugs, percent engaged in criminal activity, average days incarcerated, etc.—rather than a litany of regression coefficients. However, it is crucial to keep in mind that all such seemingly simple outcomes are never based on the raw data, but are always *adjusted*, as described above—through weighting and hierarchical modeling.

Impact Analyses: Other Predictors of Offender Outcomes

In several analyses, we also sought to test substantive hypotheses regarding the impact of other baseline offender characteristics besides drug court status. For these analyses, we added a standard set of predictor variables, each estimated as fixed effects. That is, we sought to obtain the average effect of each baseline characteristic on select outcome measures for the entire offender sample, rather than engaging in a more nuanced set of analyses that would distinguish whether the average effect size of a particular characteristic varied from one site to another—as in a random effects model. We conducted test random effects models, whose results made clear that extremely few of our predictor variables exerted significantly different effects by site. Our hypotheses, and the baseline variables used to operationalize each one, were as follows:

- *Demographics*: Offenders who are older, male, white, high school graduates, or with a higher income at baseline will have better outcomes than other subgroups:
  - Age
  - Sex
  - Race/ethnicity: black, Hispanic, or other nonwhite (vs. white)
  - High school degree or GED
  - Base 10 logarithm of income (to correct for its extremely skewed distribution)

- *Social Ties*: Offenders with more mainstream social ties and who have a greater "stake in conformity" (more to lose from noncompliance) will have better outcomes than others:
  - Employed or in school
  - Married
  - Homeless (in the six months pre-baseline)
  - Blood relatives involved with crime or drugs (based on multi-item instrument);

- *Prior Drug Use*: Offenders with a more severe prior drug use history will have greater difficulty in recovering and will therefore have worse outcomes than others:
    - Average days per month of drug use (in the six months pre-baseline)
    - Primary drug of choice: alcohol, marijuana, or cocaine (vs. other drugs)
    - Any residential treatment (in the six months pre-baseline)

- *Prior Criminal History*: Offenders with a more extensive prior criminal history will have worse outcomes than others:
    - Number of criminal acts (self-reported in the six months pre-baseline)

- *Mental Health*: Offenders with co-occurring mental health disorders at baseline will have worse outcomes than others:
    - Depressed (classification based on multi-item instrument)
    - Anti-social personality disorder (classification based on multi-item instrument)
    - Narcissistic personality disorder (classification based on multi-item instrument)

Subgroup Analyses: For Whom Do Drug Courts Work?

Besides understanding the overall effects of offender baseline characteristics on outcomes, we also sought to understand whether the drug court intervention exerts a greater or lesser impact—*relative to the comparison group*—for some categories of offenders than for others. We first identified offender characteristics from five domains:

- *Drug Use History*: We hypothesized that drug courts work better with offenders whose substance abuse history was more serious (more days of use, primary drug other than marijuana, primary drug of alcohol, or primary drug of cocaine).[2]

- *Prior Criminality*: We hypothesized that drug courts would work best with "higher risk" offenders, defined by greater criminality (e.g., prior arrests, convictions, and violence).

- *Mental Health*: We hypothesized that drug courts would be particularly effective with offenders who have anti-social or narcissistic personality disorder, both of which suggest a rational-manipulative orientation that might create receptivity to drug courts' deterrence strategies. However, we hypothesized that drug courts would be less effective with substance abusers who suffer from co-occurring depression, which could constitute an added barrier to recovery and problems requiring evidence-based ancillary services.

- *Social Ties*: We hypothesized that drug courts work better with offenders who had a greater "stake in conformity" (e.g., through employment, school attendance, or marriage).

- *Demographics*: Although we did not pose any hypothesis, we considered it important to understand whether age, sex, and race/ethnicity moderated the drug court impact.

[2] The percentages of offenders with a primary drug of heroin or methamphetamine were too small to test the effect of the drug court intervention specifically with those subgroups.

For each specific characteristic examined, we ran three-predictor regression models, including drug court status, the given characteristic, and an interaction term. Significant interaction terms meant that the drug court produced *especially* better or worse outcomes than the comparison group for offenders with the given characteristic. If our results had produced many significant interactions, we planned to combine multiple baseline measures into theoretically-based scores (e.g., "high" or "low" risk classifications) and to add more control variables to our models. This step became superfluous, as remarkably few significant interactions were detected.

The Impact of Policies, Practices, and Offender Attitudes: How Do Drug Courts Work?

We also sought to examine the intervening effects of different court policies and practices (e.g., judicial status hearings, case management, drug testing, legal incentives) and of offender attitudes (perceived procedural justice and sanction severity). Key domains are listed below:

- *Court Policies and Practices*:
  - TREATMENT: e.g., number of days of any treatment, residential, outpatient, or self-help groups; whether or not the offender completed more than 90 days of treatment
  - IMMEDIACY: e.g., whether the offender attended any treatment within the first 30 days after program entry
  - INTENSIVE SUPERVISION: e.g., frequency of judicial status hearings, case management or other supervision officer contacts, and drug tests
  - LEGAL LEVERAGE: e.g., nature and severity of sentence if failing drug court
  - INTERIM SANCTIONS AND INCENTIVES: e.g., number of sanctions, number of rewards, percent of sanctions that involve jail stays, and ratio of sanctions to infractions
  - SUPPLEMENTAL SERVICES: e.g., employment and educational assistance; family support; child services; and administrative, logistical, or legal services
- *Offender Attitudes*:
  - PROCEDURAL JUSTICE: e.g., perceived fairness of judge, supervision officer, and court
  - DETERRENCE: e.g., perceived likelihood of noncompliance detection, certainty of sanctions, certainty of jail sanctions, and severity of penalty for program failure
  - MOTIVATION: e.g., motivation to change and recovery

The analysis followed two distinct approaches. In the first, we focused on the 23 drug court sites only, enabling us to test which factors led some drug courts to have better outcomes than other drug courts (see details in Chapter 8 and Appendix A). In the second approach, we included all 29 sites, enabling us to test which program-level and attitudinal factors explained the impact of the drug court, *relative to the comparison group* (see details in Chapters 6 and 7 and Appendix A). Of particular interest, where including all 29 sites, some of our analyses utilized a structural equation modeling (SEM) framework. Such a framework gains the advantage of more fully modeling the direct and indirect pathways in which each variable produces its effects. SEM essentially produces an ordering of variables in an empirically based left-to-right path model: (1) drug court participation status and other baseline characteristics to (2) program policy and practice factors to (3) offender attitudes to (4) drug use and crime outcomes. The approach enables testing both the direct and indirect effects of each predictor variable—for example, the degree to which drug court participation directly influences outcomes and indirectly influences them through enhanced perceptions of procedural justice or enhanced perceptions of deterrence.

Sensitivity Analyses

Our final analytic plan was not the only one that might have been attempted. To investigate the possible impact of method on outcomes, we conducted a series of *sensitivity analyses*. The first such analysis explored the issue of *time at risk*, determining the impact on drug use and recidivism outcomes of adjusting for the number of days during each tracking period when the offenders were incarcerated. The second sensitivity analysis explored whether weighting, hierarchical modeling, or several other methods for addressing selection or site-level biases produced substantively different results. The third analysis explored the implications of using the 18-month weights universally, throughout all analyses involving offender survey outcomes. The methods employed and results of these analyses are fully documented in Appendix A. The essential upshot is that these analyses confirmed a need for both weighting and hierarchical modeling, as some impact findings varied significantly when omitting those steps. Otherwise, the impact findings demonstrated little sensitivity to specific nuances or changes in precise weighting or modeling methods.

Design Strengths and Limitations

The MADCE results have particularly strong external validity, because they are based on a multi-site sample of 23 drug courts, including a broad mix of urban, suburban, and rural courts from 7 geographic clusters nationwide. Also distinctive was the avoidance of a strict no-treatment comparison group in preference for a set of six comparison sites that represented a realistic range of business-as-usual conditions. In fact, our results demonstrated that even though the drug court sample averaged far more days in treatment, judicial status hearings, case management meetings, drug tests, sanctions, and incentives than the comparison sample, a meaningful fraction of the comparison sample nonetheless received some of these interventions, and more than one-third (36 percent) received substance abuse treatment in particular. What distinguished the comparison sites, however, was the lack of a robust *package of interventions*, spanning treatment, as well as multiple forms of court oversight, as is routinely found in drug courts.

Our results also had strong internal validity. We drew upon an unusually rich baseline dataset; a series of propensity score-based adjustment methods (“super weighting”) to control for both selection and attrition bias; and hierarchical modeling techniques to adjust for the clustering of outcomes at the site level. The survey response rates were remarkably high for a study of this nature (e.g., 83 percent at the 18-month wave), signifying little attrition bias and a sizable offender sample size at all follow-up timeframes. We also encountered important study limitations, especially concerning the substantial variability we uncovered from one site to another in the inherent probability of experiencing an official re-arrest.

The findings of this study are qualified by several limitations. Although we conducted a multi-site study of national scope, we did not base our findings on a random sample of sites or of drug court-eligible offenders; hence, we cannot claim that our results are perfectly representative of the country. In fact, some geographic areas are underrepresented (e.g., the Southwest and much of the Midwest). Additionally, all of our selected sites, drug court and comparison, had in common that they were willing and interested in participating in the study, whereas several sites that we attempted to include declined to participate. It is unclear whether those sites differed in other ways besides the amenability of their court administrators to research.

Concerning the data we collected, as noted previously, there were wide inter-site variations (not reducible to drug court status) in official re-arrest outcomes. Although we cannot be certain of the reasons for these variations, they most likely stemmed from differences in law enforcement practices or possibly in geo-spatial factors that made official detection of criminal activity—especially drug-related criminal activity—more or less likely in different jurisdictions. Utilizing hierarchical modeling techniques, we were able to adjust for these variations before reporting our outcomes or estimating their statistical significance. However, given that we had only 23 drug court and 6 comparison sites to work with, it is still plausible that a different set of sites might have yielded somewhat different raw effect sizes. Recognizing the possibility, our hierarchical modeling approach produced relatively high standard errors in estimating the impact of drug court participation, but the ramification of doing so was that our statistical power to detect a significant effect was limited. Thus, effect sizes for re-arrest impacts that ordinarily might be statistically significant given our individual offender sample size were not in this study.

When shifting from official recidivism to self-reported criminal behavior, the limitations of any self-reported data are self-evident. We have no reason to believe that the inherent biases entailed by self-reported information were differentially present between the drug court and comparison samples, but we cannot rule out the possibility. Such a concern notwithstanding, overall, we consider the use of self-report data to comprise an invaluable study *asset*, because these data enabled developing estimates for multiple types of criminal behavior that were not limited to what could be detected through official criminal justice contacts. Moreover, as our analysis of official re-arrests itself demonstrated, official recidivism estimates are vulnerable to law enforcement or detection biases, whereas self-reported criminal behavior estimates are not. For this reason, it is unfortunate that virtually all prior drug court evaluations relied exclusively on official re-arrest or re-conviction measures to estimate recidivist behavior.

Finally, we sought to examine the durability of program impacts during both in-program and post-program periods, but our timeframes were not of a truly long-term duration. For drug court offenders, we averaged only about 3 months of post-program time for 18-month survey data and 9 months for administrative records data.

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Chapter 3. Do Adult Drug Courts Reduce Drug Use?

Shelli B. Rossman, Mia Green, Michael Rempel, and P. Mitchell Downey

A rich literature suggests that drug courts appear to reduce recidivism (Aos, Barnoski, and Lieb 2001; Carey, Crumpton, et al. 2005; Finigan, Carey, and Cox 2007; Goldkamp, White, and Robinson 2001; Gottfredson Kearley, et al. 2006; Government Accountability Office 2005; Latessa, Shaffer, and Lowenkamp 2002; Rempel, Fox-Kralstein, et al. 2003; Roman and DeStefano 2004; Schaffer 2006; Wiest, Carey et al. 2007; Wilson, Mitchell, and MacKenzie 2006); however, surprisingly few studies test the relationship between drug courts and drug use, citing mixed results (Brewster 2001; Deschenes, Turner, and Greenwood 1995; Gottfredson Kearley, et al. 2005; Harrell, Roman, and Sack 2001). The widely promulgated theory of change regarding drug courts is that they work by ameliorating the addiction to drugs that is believed to be at the root of the users' criminal behavior. Nonetheless, it is also plausible that drug courts produce strong disincentives to illegal behavior through their aggressive use of judicial supervision, sanctions, and incentives, but that such interventions do not trigger true and lasting recoveries from substance abuse.

The MADCE research collected both self-reported information on substance use at baseline, and at 6- and 18-month follow-up intervals, and oral fluids drug tests (using Buccal swabs) at the 18-month follow up. The offender survey data included information about the use of eight drugs: marijuana, alcohol, cocaine, heroin, hallucinogens/designers drugs, amphetamines, illegal use of prescription drugs, and illegal use of methadone. For alcohol, separate questions concerned the use of any and "heavy" alcohol use. Heavy use is defined as at least four drinks per day for women and at least five drinks per day for men. Separate drug use data were collected for each individual month: that is, each of the 6 months prior to the 6-month survey and each of the 12 months prior to the 18-month survey. Offenders were asked how often they used each drug during each month, where they were to select from answers: never, once per month, a few times per month, a few times per week, and every day. The oral fluids test was sensitive to five types of drugs: marijuana, cocaine, opiates, amphetamines, and PCP.

Research Questions

Despite the dearth of prior research examining drug use impacts directly, the positive recidivism literature suggests as our primary hypothesis that adult drug courts reduce drug use. Importantly, the collection of self-report data at two follow-up points allows testing whether program impacts persist or subside over time; that is, does the magnitude of impact change when comparing 6-month to 18-month impacts?

Additionally, we hypothesized that analysis of the trajectories of relapse and recovery would demonstrate that the impact of drug courts is favorable with regard to:

- *Onset:* Drug courts delay the time at which relapse occurs.

- *Duration:* Drug courts decrease the total number of months during which those who relapse continue to use.

- *Intensity*: Drug courts decrease the frequency of use during months when drug users are using.

- *Harm Reduction*: Drug court participants who relapse use less severe drugs than their initial primary drug of choice.

Lastly, our data enabled testing whether drug courts are particularly effective in reducing drug use among specific categories of offenders, defined by their baseline characteristics—more severely addicted offenders, those with stronger community ties, those with or without co-occurring mental health disorders, or those with a certain demographic background. Although barely examined in prior research, certain offender subgroups either may have greater motivation to be responsive (e.g., those subject to more social controls through marriage or employment) or simply be more suited to the intervention (e.g., those suffering from a more severe addiction). Given findings that drug court impacts on criminal behavior vary across different types of individuals (Marlowe, Festinger, et al. 2003), it seems reasonable that drug court impacts on drug use would also vary.

Design and Methodology

Essential features of the study design and methodology are found in Volume 1 (Chapter 3) and this Volume (Chapter 2 and Appendix A), and are not detailed here. As noted, the study was ultimately implemented in 23 drug court sites and 6 comparison sites to reflect a range of counter-factual conditions. The comparison group is not a strict no-treatment sample, since in the real world outside of drug courts, offenders are still ordered to treatment through a variety of other mechanisms. In fact, as shown in Table 4-3.1, all comparison sites indicated that they order at least "some," if not "all," offenders to substance abuse treatment, and more than one-third of the comparison sample in fact received treatment in the first six months after baseline. On the other hand, the table also shows that, on average, drug court offenders were relatively *more* likely to receive treatment, as well as a series of other interventions, including: judicial status hearings, case management, drug testing, and interim sanctions and incentives. In short, the drug court sample tended to receive a total package of treatment and court oversight interventions that, together, comprise the "drug court model" (e.g., see Office of Justice Programs and National Association of Drug Court Professionals 1997). While the comparison sample did not consist exclusively of a no-treatment group, the average range of interventions was far less than for those who were enrolled in drug court.

The final survey sample (drawn from March 2005 through June 2006) included 1,781 offenders: 1,156 from the drug court and 625 from the comparison sites. Follow-up response rates were 86 percent at 6 months, 83 percent at 18 months, and 76 percent at both periods.

Table 4-3.1. Program Activities of Drug Court and Comparison Offenders

	Drug Court	Comparison Group
Number of Sites	**23**	**6**
Number of Offenders	**1,009**	**524**
Data for First Six Months Since Baseline:		
Substance Abuse Treatment		
Substance abuse treatment requirement:		
Required of all offenders	23/23 sites	2/6 sites
Required of some offenders	0/23 sites	4/6 sites
Required of no offenders	0/23 sites	0/6 sites
Percent with any treatment	83%***	36%
Average days in treatment	59***	23
Percent with residential treatment	25%	14%
Percent with outpatient treatment	77%***	30%
Judicial Supervision		
Percent with any judicial hearings	93%***	14%
Average number of hearings	10.3***	1.2
Case Management and Other Supervision		
Percent with any contact with supervision officer	96%**	71%
Average number of face-to-face contacts	17.2***	6.4
Average number of phone contacts	6.8*	3.8
Drug Testing		
Percent with any drug test	95%***	61%
Average number of drug tests	30.9***	4.3
Sanctions and Incentives		
Percent receiving any incentive/reward	86%***	37%
Percent receiving praise from the judge	76%***	10%

Notes: The results reported in this table were computed in HLM 6.04 (utilizing hierarchical modeling), and the data were weighted, as described in the methodology section (Chapter 2 and Appendix A). The following variables had small numbers of missing cases: both measures on judicial hearings (53), any contact with supervision officer (8), number of face to face contacts with supervision officer (10), number of phone contacts with supervision officer (15), and both measures on drug tests (46).
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Additionally, oral fluids drug tests were administered at 18 months to 1,147 offenders (i.e., 764 drug court participants and 383 comparison members) who were not incarcerated or in residential treatment at the time (83 percent of those interviewed) and who gave specific consent to the test (94 percent of those who were eligible). Our oral fluid "consent" rate is equivalent for the two groups. However, slightly more comparison group members were incarcerated at the 18-month interview; therefore, the overall rate for oral fluid collection is lower for the comparison group than for the treatment group.

As reported in Chapter 2 and Appendix A, we used a "super weighting" strategy similar to that applied by Harrell and colleagues (2007) and standard propensity score diagnostics to adjust for selection and attrition bias. Additionally, we employed hierarchical modeling in final analyses using HLM 6.04 software (see Raudenbush and Bryk 2002), the Mixed procedure in SAS, the GLLAMM program in Stata (see Rabe-Hesketh and Skrondal 2008), and the lme4 package in R (see Bates, Maechler, and Bolker 2010) to adjust for site-level variation.

Outcome Measures

To address broad questions regarding impact of drug courts on substance abuse, we created two summary measures for each tracking period for each drug and for all drugs combined: (1) was any use reported and (2) the average number of days of use per month. We also created the equivalent summary measures for any "serious" drug use, which we defined to exclude marijuana and non-heavy alcohol. (Thus, consistent with standard clinical classifications, we defined "heavy" alcohol use as serious.) Since previous studies of drug use often isolate the most recent 30-day period, we also identified and conducted some of our analyses exclusively for the most recent month prior to each of the two follow-up surveys. In addition, using the 18-month orals fluids test results, we created two summary measures for whether the offender tested positive for any drug and for any "serious" drug (excluding marijuana).

In addition to the question of whether drug courts reduce drug use, we also examined trajectories of relapse and recovery, which includes questions about timing and intensity of drug use. For this, we conducted analyses on any drug use, as well as separately for "hard" drugs, defined as heroin, methadone (no prescription), cocaine (crack or powder), and amphetamines, and "lighter" drugs, including marijuana, hallucinogens, prescription drugs (without a prescription), and heavy alcohol (more than four drinks for women and five for men), using multilevel Poisson regression (for months of use and the timing of use) and standard multilevel regression (for the log odds of trajectory group membership and the average days of use per month *among users*).

Comparing Self-Report and Oral Fluids Data

One particular concern involved the validity of the findings based on self-report. Because we collected both self-report and oral fluids data, we could investigate how their results compared and whether drug court or comparison offenders were systematically more likely to underreport their drug use.

As shown in Table 4-3.2, in 69 percent of the cases, self-reported and oral test data were consistent with each other (i.e., both negative or both positive); 23 percent (i.e., 266 respondents) of the 1,147 tested reported non-use, but tested positive; and 8 percent reported drug use in the prior month that was not confirmed by the oral testing. We suspect this latter circumstance likely is not "over-reporting," but rather is due to drug use earlier in the month such that too much time had elapsed for the testing to detect it. Table 4-3.3 presents the incidence of those who falsely reported no drug use, by group and substance tested. There were no statistically significant differences in reporting, by group or substance used.

Table 4-3.2. Comparison of Self-Report and Oral Fluids Data at 18 Months

Self Report	Negative Oral Test	Positive Oral Test	Total
***No* drug use self-reported**	639	266	905
Drug use self-reported	89	153	242
Total	728	419	1,147

Table 4-3.3. Percent of Respondents Who Failed Oral Drug Tests while Falsely Reporting No Drug Use in the Month before Testing

	Drug Court Participants Tested N=764	Comparison Group Tested N=383	Total Tested N=1,147
All tested substances	71.9% (n=242)	79.7% (n=177)	74.0% (n=419)
Marijuana	45.6% (n=99)	60.0% (n=70)	49.6% (n=169)
Cocaine	83.2% (n=128)	81.8% (n=98)	83.0% (n=226)
Heroin	82.6% (n=45)	88.0% (n=39)	83.8% (n=84)
Hallucinogens	100% (n=1)	100% (n=2)	100% (n=3)
Amphetamines	73.2% (n=7)	100% (n=4)	87.8% (n=11)

*+p<.10, *p<.05, **p<.01, ***p<.001.*
Note: Number of individuals who failed each test (denominator in percentage calculation) shown in parentheses underneath.

Analytic Strategy

As detailed in Chapter 2 and Appendix A, after computing simple drug court impacts on our outcomes of interest, we re-ran certain analyses after adjusting for *time at risk*—that is, discounting time spent incarcerated, given the (perhaps questionable) premise that offenders have fewer opportunities to use drugs when they are in jail or prison. We proceeded by re-computing all days of drug use measures after first subtracting the number of days incarcerated from the given tracking period. In comparing impacts with and without this adjustment, no findings shifted between significance and non-significance, and the substantive differences were marginal, if discernible at all. Therefore, in reporting our results below, we omitted a time at risk adjustment from the main text and tables, but included the adjusted findings within footnotes under the appropriate results tables.

Besides testing the impact of drug court status, we also performed several multivariate analyses examining the impact of other offender characteristics, such as demographic background, drug use history, criminal history, and mental health status. We entered all such characteristics as fixed rather than random effects. In test models (not shown), we confirmed that none of our predictors exerted significantly different effects by site. For this reason, our final models exclusively examined the *average* effect of each characteristic throughout all of our sites.

To test whether drug courts were particularly effective for some as opposed to other categories of offenders, we proceeded as follows. First, we selected two key outcomes, the average number of days of drug use per month in the year prior to the 18-month survey; and whether the drug test administered at that survey was positive for any illegal substance. Second, we selected a series of baseline characteristics of interest in four basic areas that we thought might moderate the drug court impact: (1) drug use history, (2) prior criminality, (3) mental health, (4) social ties (e.g., to employment or marriage), and (5) primary demographics (e.g., age, race, and sex). Third, we constructed a series of three-predictor models to test whether the selected baseline characteristics in fact moderated the effectiveness of drug courts. The three predictors were participation status, the baseline characteristic of interest, and an interaction term for the two. Significant interaction terms would indicate that the net benefit of the drug court intervention varied based upon the given background characteristic. We contemplated, but ultimately did not build up to more complex, multivariate analyses, because extremely few interaction terms turned out to be significant in the initial three-variable models.

We also used the monthly calendared data on self-reported drug use to construct trajectories of drug use during the 18 month follow-up period (Nagin and Land 1993). Trajectories were created separately for three types of drugs, all depending on the primary drug of choice at the baseline interview: a lesser drug than the primary, the same drug as the primary, and a harder drug than the primary. The trajectories were estimated using the Traj procedure developed for SAS (Jones, Nagin, and Roeder 2001).

Next, we extended the analyses discussed above to explore whether drug court has an impact on not just the incidence of drug use or average days of drug use among the entire sample, but to identify impacts on how early drug use begins, how many months during which drug use occurs, and the frequency of drug use during months when individuals are using. Finally, we examined how use of different drugs are related by examining the transitions from lesser drug use trajectories to harder drug use trajectories and the probabilities of different types of concurrent drug use.

Results

Drug Use at Six Months

As shown in Table 4-3.4, offenders in the drug court used drugs significantly less often than did offenders in the comparison group during the initial six-month tracking period. Overall, 40 percent of drug court participants as compared with 55 percent of comparison offenders reported that they had used at least one of the eight measured substances ($p < .05$). Drug court offenders also averaged fewer days of drug use per month (1.5 vs. 3.7 days; $p < .01$) and fewer days of *serious* use per month (1.0 vs. 2.2 days, $p < .05$).[3] Regarding specific illegal substances, 13 percent of the drug court sample had used marijuana versus 26 percent of the comparison group ($p < .05$). One- third (32 percent) of the drug court sample reported drinking alcohol compared to more than half (52 percent) of the comparison group ($p < .05$). A significantly lower percentage of drug court offenders also reported illegal prescription drug use (6 percent versus 10 percent; $p < .05$). Analogous findings were detected when isolating drug use in just the most recent month prior to the six-month survey. Finally, drug court and comparison offenders had, on average, similar six-month scores on the Addiction Severity Index (5.4 versus 5.0; non-significant) and experienced similar reductions between baseline and six months (-4.0 vs. -3.7; n.s.).

Table 4-3.4. Drug Use at Six Months

	Drug Court N=1,009	**Comparison Group N=524**
Overall Drug Use - Previous Six Months		
Any drug use - eight drugs	40%*	59%
Any serious drug use	32%	40%
Days of use/month - eight drugs[1]	1.5**	3.7
Days of serious use/month[2]	1.0**	2.2
Any Use by Drug - Previous Six Months		
Marijuana	13%*	26%
Alcohol	32%*	52%
Heavy alcohol (≥ 4-5 drinks/day)	19%	28%
Cocaine	17%	19%
Heroin	4%+	6%
Amphetamines	4%	7%
Hallucinogens	3%	4%
Prescription drugs (illegal use)	6%*	10%

(continued)

[3] Note that both of these figures for average days of drug use per month refer to the entire sample and all months. Estimates of average days of drug use per month based on only the sample that used and only months during which they were using are discussed later. Both estimates provide different, useful perspectives.

Table 4-3.4. Drug Use at Six Months (Cont'd)

	Drug Court N=1,009	Comparison Group N=524
Methadone (illegal use)	1%	2%
Overall Drug Use - Most Recent Month		
Any drug use - eight drugs	18%***	43%
Any serious drug use	13%**	26%
Days of use - eight drugs	1.2***	4.2
Days of serious use	0.8**	1.9
Any Use by Drug - Most Recent Month		
Marijuana	4%**	17%
Alcohol	14%**	32%
Heavy alcohol ($\geq$ 4-5 drinks/day)	7%*	17%
Cocaine	5%*	10%
Heroin	2%+	4%
Amphetamines	1%	2%
Hallucinogens	1%*	5%
Prescription drugs (illegal use)	2%**	6%
Methadone (illegal use)	1%	1%
Addiction Severity Index (ASI)		
Score at six months (0-18, higher = more addicted)	5.4	5.0
Change between baseline and six months	-4.0	-3.7
Family Drug Use and Treatment (Since Baseline)		
Blood Relatives with drug and/or alcohol problems	26%	23%
Other Relatives/Friends with drug or alcohol problems	23%	32%
Blood Relatives treated for drug and/or alcohol use	8%	6%
Other Relatives/Friends treated for drug alcohol use	14%	14%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*
Notes: Serious drug use includes heavy alcohol, cocaine, heroin, amphetamines, hallucinogens, prescription drugs (illegal use), and methadone (illegal use). All outcomes are computed in HLM 6.0, with sample defined as a Level 2 variable. Any use measures are calculated with a logistic regression specification (Bernoulli), days of use measures are calculated with a Poisson specification, and addiction severity index measures are calculated with a normal specification. Regression coefficients are then transformed into simple percentages and means.

[1] After adjusting the tracking period to cover time at risk only, the means are 1.9 and 4.2 (p < .001).
[2] After adjusting the tracking period to cover time at risk only, the means are 1.4 and 2.4 (p < .05).

Drug Use at 18 Months

As shown in Table 4-3.5, drug court offenders continued to report less drug use than the comparison group in the year prior to the 18-month survey. Drug court offenders had significantly fewer occurrences of any use (56 percent vs. 76 percent, $p < .01$), serious use (41 percent vs. 58 percent, $p < .01$), days of use per month (2.1 vs. 4.8, $p < .001$), and days of serious use per month (1.1 vs. 2.3; $p < .001$). Regarding specific substances, drug court offenders were significantly less likely than the comparison group to report use of marijuana (23 percent vs. 36 percent), alcohol (47 percent vs. 67 percent), "heavy" use of alcohol (29 percent vs. 42 percent), illegal use of prescription drugs (6 percent vs. 15 percent) and illegal use of methadone (2 percent vs. 4 percent). Although the sample differences for cocaine, heroin, amphetamines, and hallucinogens were all non-significant, every one of these latter differences also trended towards less use in the drug court sample. Once again, in the most recent month prior to the 18-month follow up, the differences between the two samples generally mirrored those detected for the entire previous year.

Table 4-3.5. Drug Use at 18 Months

	Drug Court N=951	**Comparison Group N=523**
Overall Drug Use - Previous Year		
Any drug use - eight drugs	56%**	76%
Any serious drug use	41%**	58%
Days of use/month - eight drugs[1]	2.1***	4.8
Days of serious use/month[2]	1.1***	2.3
Any Use by Drug - Previous Year		
Marijuana	23%*	36%
Alcohol	47%**	67%
Heavy alcohol (≥ 4-5 drinks/day)	29%*	42%
Cocaine	19%	24%
Heroin	5%	7%
Amphetamines	3%	9%
Hallucinogens	3%+	6%
Prescription drugs (illegal use)	6%**	15%
Methadone (illegal use)	2%**	4%
Overall Drug Use - Most Recent Month		
Any drug use - eight drugs	28%**	45%
Any serious drug use	17%**	28%
Days of use - eight drugs	2.4*	3.9
Days of serious use	1.1***	2.2

(continued)

Table 4-3.5. Drug Use at 18 Months (Cont'd)

	Drug Court N=951	Comparison Group N=523
Any Use by Drug - Most Recent Month		
Marijuana	14%	16%
Alcohol	23%**	39%
Heavy alcohol (≥ 4-5 drinks/day)	13%**	23%
Cocaine	4%	5%
Heroin	2%	2%
Amphetamines	1%	2%
Hallucinogens	1%	1%
Prescription drugs (illegal use)	1%**	5%
Methadone (illegal use)	0%**	1%
Addiction Severity Index (ASI)		
Score at 18 months (0-18, higher = more addicted)	5.4	5.7
Change between baseline and eighteen months	-3.9+	-2.8
Change between six and eighteen months	-0.1+	0.5
Family Drug Use and Treatment (Since Six-Month Interview)		
Blood Relatives with drug and/or alcohol problems	28%	28%
Other Relatives/Friends with drug or alcohol problems	11%	11%
Blood Relatives treated for drug and/or alcohol use	19%	17%
Other Relatives/Friends treated for drug alcohol use	28%	28%

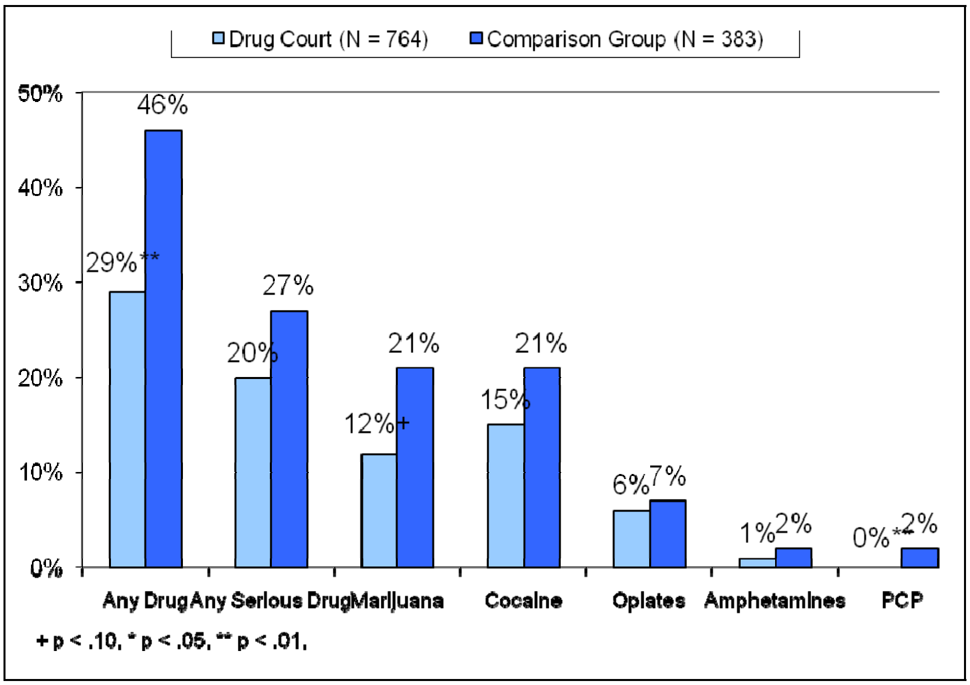
Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*
Notes: Serious drug use includes heavy alcohol, cocaine, heroin, amphetamines, hallucinogens, prescription drugs (illegal use), and methadone (illegal use). All outcomes are computed in HLM 6.0, with sample defined as a Level 2 variable. Any use measures are calculated with a logistic regression specification (Bernoulli), days of use measures are calculated with a Poisson specification, and addiction severity index measures are calculated with a normal specification. Regression coefficients are then transformed into simple percentages and means.
[1] After adjusting the tracking period to cover time at risk only, the means are 2.8 and 5.6 ($p < .001$).
[2] After adjusting the tracking period to cover time at risk only, the means are 1.4 and 2.9 ($p < .01$).

On the other hand, as in the six-month wave, the Addiction Severity Index (ASI) scores were broadly similar between the samples at 18 months, although there was some indication that the ASI scores declined relatively more among drug court offenders when comparing the 18-month mark to earlier waves (p <.10). Lastly, although drug court offenders used drugs less often than the comparison group, the samples did not differ in the degree of drug involvement of their friends and family members.

Drug Test Results

As noted above, drug tests results are exclusively for out-of-custody offenders who were administered an oral saliva test during the 18-month survey wave. The results do not reflect offenders who were incarcerated at the time the survey was conducted. As shown in Figure 4-3.1, drug court offenders had a significantly lower rate of testing positive than the comparison group (29 percent vs. 46 percent, $p < .01$). Specific differences with respect to serious drug use, marijuana, cocaine, opiates, or amphetamines were not statistically significant, although the percentages suggest the possibility of modest effects and trend consistently toward lower rates of positive drug tests among offenders enrolled in drug courts.

Figure 4-3.1. Drug Test Results at 18 Months



Predictors of Drug Use at 18 Months

We were also interested in the degree to which subsequent drug use depended on preexisting offender characteristics, such as their age, sex, stake in conformity (e.g., as measured through involvement in marriage, employment, or through a higher income), drug use history, criminal history, and mental health status. Accordingly, Table 4-3.6 displays our findings for the impact of drug court participation and a group of 16 baseline characteristics on average days of drug use per month and on having a positive drug test at the time the 18-month survey was administered.

Table 4-3.6. Baseline Predictors of Drug Use at 18 Months

	Average Days of Drug Use per Month	Positive Drug Test (Any Drug)
Drug Court Participation		
Participant sample	-.946***	.473***
Demographics		
Age	-.003	.984*
Sex	.185	1.95***
Race/ethnicity[1]		
Black	.171	2.11***
Hispanic	.153	1.17
Other racial group	.232	1.33
Social Ties		
High school degree or GED	-.104	.889
Income (base 10 log of income)	.053	.978
Employed or enrolled in school	-.032	.841
Married	-.214	.808
Homeless (any time in 6 months pre-baseline)	.037	.471**
Blood relatives involved with crime or drugs[2]	-.013	.943
Drug Use		
Primary drug of choice[3]		
Marijuana	.336	1.43+
Alcohol	.473+	1.18
Cocaine (any form)	.105	1.45*
Average days of use/month (6 months pre-baseline)	.029***	1.01*
Any residential treatment (6 months pre-baseline)	.348	1.49
Criminal History		
Number of criminal acts (6 months pre-baseline)	.100	1.17
Mental Health		
Depression	.135	1.32*
Anti-social personality disorder	.307**	.798+
Narcissistic personality disorder	.025	.991
Number of cases	1474	1147

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*
Notes: The coefficients for average days of drug use are simple regression coefficients, based on a Poisson specification.

The coefficients for whether the offender tested positive are logistic regression odds ratios.
[1] White is the reference category. "Other racial group" includes Native-American, Alaskan Native, Asian, East Indian, Native Hawaiian, Pacific Islander, and non-Hispanic multi-racial.
[2] The blood relatives measure represents an index of 10 questions, each coded on a 0-10 scale.
[3] The reference category includes primary drugs of heroin, amphetamines, prescription drugs, miscellaneous other drugs, and those who did not claim to have any particular primary drug. All of these categories combined total 30 percent of the sample.

Consistent with the results reported above, we found that drug court participation led to significantly less drug use on both outcome measures in Table 4-3.6. Not surprisingly, we also found that a greater frequency of drug use at baseline significantly predicted a greater frequency at 18 months for both drug court and comparison offenders. Among other background characteristics, a younger age, male sex, black race, having been classified with depression (based on a multi-item screening tool), and having been classified with anti-social personality disorder (also based on a multi-item tool), all predicted greater drug use on at least one if not both of the outcome measures in Table 4-3.6. On the other hand, our results did not provide any evidence of a relationship between offender social ties (e.g., based on employment/school status, marital status, involvement of blood relatives with crime or drugs) and less drug use; nor did we find that prior criminal history predicted future drug use. Overall, the strongest and most consistent predictors of drug use outcomes were participation in the drug court, less frequent drug use at baseline, and the absence of mental health problems at baseline.

Trajectories of Relapse and Recovery

As shown in Figure 4-3.2, both the drug court and comparison samples demonstrated evidence of reduced drug use between the baseline and six-month marks. However, the reduction was significantly greater among those enrolled in the drug court. Subsequently, between the 6- and 18-month marks, there was a small, but insubstantial increase in drug use among offenders from both samples; but the overall magnitude of the difference between the samples did not change. In short, in comparing the 6- and 18-month results, the gains made by those receiving the drug court intervention appeared to have been retained over time. (Test analyses that employed slightly different measures—e.g., focusing on serious drug use or examining the number of days of drug use per month—revealed essentially the same pattern across the three survey waves.)

We were interested in whether drug courts have an impact beyond reducing the likelihood of use by (1) delaying the time at which relapse occurs (onset), (2) decreasing the total number of months during which substance abusers continue to use (duration), or (3) decreasing the frequency of use during months when drug users are using (intensity). We conducted four analyses to explore these issues. Table 4-3.7 presents the first analysis, comparing the timing of the first relapse for drug court participants to that of the comparison group during the 18-month timeframe after the baseline interview. Statistically significantly larger percentages of drug court participants report no relapse during this period; similarly, drug court participants are statistically significantly less likely to relapse in the first six months. Conversely, a small, but important percentage of each group report no sobriety within the 18 months, though this was significantly higher in the comparison group.

Figure 4-3.2. The Trajectory of Recovery: Percent Used Drugs in Prior Six Months

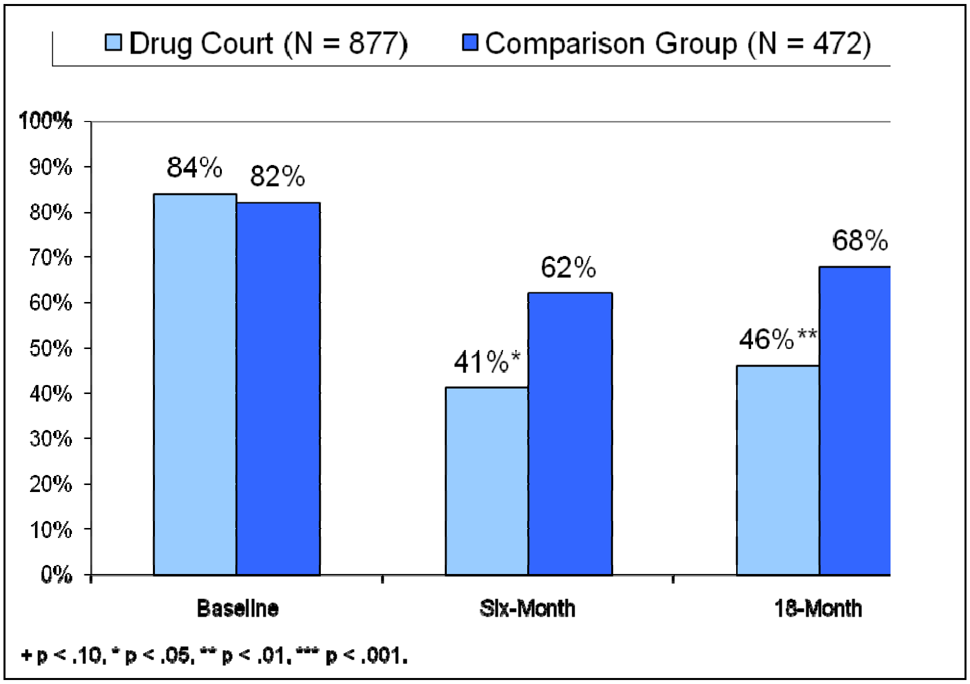


Table 4-3.7. Occurrence of First Relapse, by Group

Timing of Relapse	Drug Court Participants N=877	Comparison Group N=472
No relapse	**0.34*** **	0.22
First relapse in Months 1- 6	**0.37*** **	0.47
First relapse in Months 7-12	0.15	0.17
First relapse in Months 13-18	0.13	0.10
Continued Use Since Baseline	**0.01*** **	0.04

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

For the second analysis, we conducted a multilevel Poisson regression to address questions of onset and duration (described above), and for intensity, we conducted a standard multilevel regression (normally distributed errors). The earlier analyses confirmed that drug court decreased the likelihood of use. Since we were interested in extending our consideration beyond this impact, we restricted our sample to only those who reported some use. That is, these analyses tested whether drug court had an impact on onset, duration, or intensity, *among those who used*. For those analyses, we looked at any drug use, as well as separately for "*hard*" drugs, defined for these analyses as heroin, methadone (no prescription), cocaine (crack or powder), and amphetamines, and "*lighter*" drugs, including marijuana, hallucinogens, prescription drugs (without a prescription), and heavy alcohol (more than four drinks for women and five for men), as presented in Table 4-3.8.

Table 4-3.8. Drug Court Impact on Relapse, Duration, and Frequency of Use

	Drug Court Mean	Comparison Mean	Treatment Difference
First Month With Use			
Any drugs	1.7	1.7	0.03
Hard drugs[a]	1.8	1.9	-0.15**
Lighter drugs[b]	1.8	1.8	-0.03
Number of Months Using			
Any drugs	1.8	2.0	-0.26***
Hard drugs	1.1	1.8	-0.68***
Lighter drugs	1.8	2.0	-0.20***
Days per Month When Using			
Any drugs	8.4	10.1	-1.69**
Hard drugs	6.0	9.7	-3.61*
Lighter drugs	8.4	8.2	0.20

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*
[a] Hard" drugs for these analyses, refer to heroin, methadone (no prescription), cocaine (crack or powder), and amphetamines.
[b] Lighter drugs includes marijuana, hallucinogens, prescription drugs (without a prescription), and heavy alcohol (more than four drinks for women and five for men).

These findings suggest that, among those who relapse, drug court participants use statistically significantly less per month and use for fewer months than the comparison group. This pattern holds for all drugs, as well as heavy and lighter drugs, separately. The difference is greatest for hard drugs, where drug court participants are estimated to use during 0.7 fewer months and, during months they are using, are expected to use 3.6 fewer days per month. There was not a statistically significant difference in the number of days per month for lighter drugs. Although drug court participants who relapse to heavy drugs tended to do so earlier, this difference is trivial in magnitude, though statistically significant (1.8 months into the study vs. 1.9 months). Thus, it appears that drug court reduces the likelihood of drug use, the length of time over which drug use occurs, and the intensity of that use, though does not appear to delay that relapse.

As a third analysis, we used the monthly calendared data on self-reported drug use to construct trajectories of drug use during the 18-month follow-up period (Nagin and Land 1993).[4] For this analysis, we constructed three different trajectories, all based on the self-reported primary drug of choice during the 18-months preceding baseline.[5] We constructed trajectories for use of a less

[4] The trajectories were constructed using the Traj procedure developed for SAS (Jones, Nagin, and Roeder 2001).
[5] Individuals who reported not using drugs during the year before baseline or who reported that multiple drugs were the primary drugs were excluded from this analysis because we could not identify which were lesser or more serious drugs.

serious drug than the primary drug, the same drug, or a "more serious" drug than the primary drug, based on the following ordering:

Least Serious
Alcohol
Prescription Drugs (without prescription)
Marijuana
Hallucinogens
Amphetamines
Cocaine (powder and crack)
Methadone (without prescription)
Heroin
Most Serious

After estimating trajectories for each of these three types of drug use, we estimated the effect of drug court membership and a number of other individual characteristics on group membership. For each individual, for each group, the probability of group membership is automatically calculated through the Traj procedure. We took the log odds of group membership to transform this probability into a roughly normally distributed random variable and estimated the log odds of group membership as a function of drug court status and other characteristics using a hierarchical linear model to account for clustering within court.

In the past, in an effort to understand the impact of individual characteristics on group membership, individuals in the sample have been classified into their most likely groups and logistic regression was used to estimate the effect of characteristics (Nagin, Farrington, and Moffit 1995; Laub, Nagin, and Sampson 1998). That method does not account for uncertainty in group assignment, present in any analysis using latent variables (Eggleston, Laub, and Sampson, 2004). As an alternative, the Traj procedure has a built-in command allowing "risk" factors' impact on group membership to be simultaneously estimated with the estimation of the groups (Jones, Nagin, and Roeder 2001). However, such estimation uses a multinomial logistic method, in which the impacts of covariates are evaluated relative to some arbitrarily selected reference group, for which no information is available. Our method avoids both of these pitfalls, as it is based on the probabilities of group membership rather than any deterministic assignment and does not require an arbitrary reference group.[6]

In addition to drug court membership, we used the following individual characteristics (all based on self-reported information at baseline): an indicator of "poor" or "fair" mental health (1 or 2 on a 5-point scale), an indicator of low-level addiction (6 or less on the 18-point Addiction Severity Index [ASI]), an indicator of high-level addiction (14 or more on the ASI), and an indicator of clinical depression (10 or more on a 30-point scale).

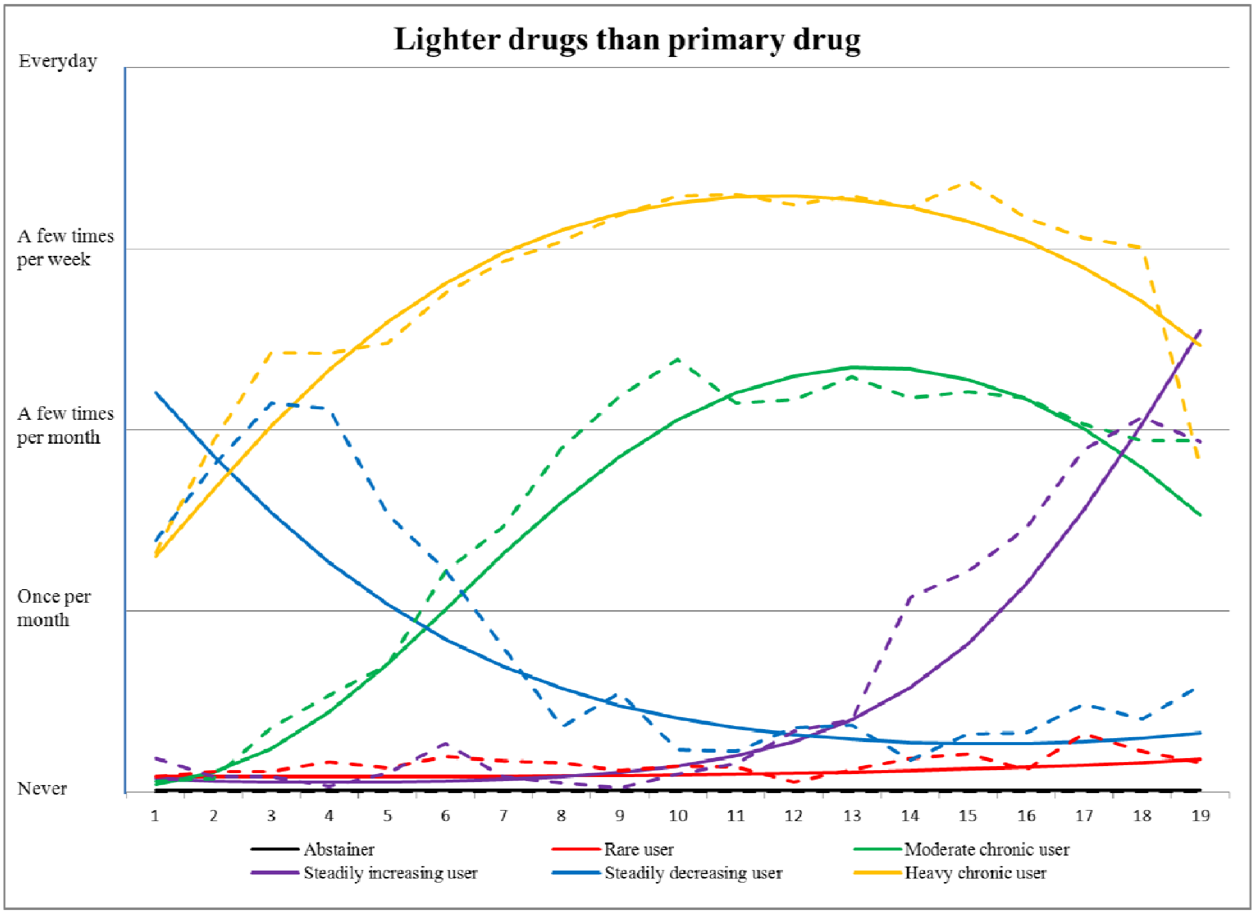
The results are displayed in a number of tables below. Because the dependent variable in the regressions was the log odds, accompanying each table of estimated coefficients is a supplemental table providing a more intuitive presentation of whether each characteristic makes

[6] Coefficients should be interpreted as the effect on the probability of belonging to a group relative to not belonging to that group (that is, compared to all other groups).

membership statistically significantly more likely, less likely, or has no statistically significant impact (indicated by blank cells).

First, in Figure 4-3.3, we look at the trajectories of use of drugs which are less severe ("lighter," as operationally defined above) than an individual's drug of choice prior to enrollment in the study. The dashed lines indicate the mean days of use, measured on a 5-point scale,[7] based on the probability that each individual belongs to each group, while the solid lines indicate the fitted polynomial (always quadratic). That is, the solid lines are estimated to fit the dashed lines.

Figure 4-3.3. Use of Lighter Drugs than Primary Drug of Choice



The trajectories above display that drug-involved offenders had dramatically different experiences with regard to drug use during the 19 months following baseline. We describe these trends in the following table, which presents membership in each trajectory by drug court participants and comparison group members. The statistically significant differences are based

[7] We decided to use the 5-point scale that participants used to respond rather than transforming that scale into number of days per month. We did so because the categories do not exactly correspond to days of use per month (i.e., "A few times per week" does not mean 3 x 4.3 = 12.9 days per month, necessarily). Further the model tended to yield better fit when based on the categorical scale (using a Poisson distribution) rather than the crude approximation to a more continuous measure of days per month (whether Poisson or normal distributions were used).

on the results from the hierarchical linear model presented immediately afterward in Table 4-3.10.

Table 4-3.9. Group Memberships in Lighter Drug Use Trajectory Groups

Lighter Drugs Than Primary Drug			Total	Drug Court Participant		Comparison Group
Group 1	L1	Abstainer	48%	51%	*	44%
Group 2	L2	Rare	23%	23%	+	23%
Group 3	L3	Moderate chronic	12%	11%		14%
Group 4	L4	Steadily increasing	6%	7%		5%
Group 5	L5	Steadily decreasing	5%	5%		5%
Group 6	L6	Heavy chronic	5%	4%		8%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-3.10. Results from Hierarchical Model, Controlling for Drug Addiction and Mental Health Status at Baseline

		Intercept		Drug Court Participation		Poor/Fair Mental Health		Low Addiction		High Addiction		Depression	
L1	Abstainer	-3.48	***	1.45	*	1.18	*	0.58		-2.13	***	-1.15	**
L2	Rare	-2.94	***	0.78	+	0.60	*	0.49	+	-0.80	*	-0.27	
L3	Moderate chronic	-6.29	***	-0.31		-0.32		0.83	*	0.43		0.75	+
L4	Steadily increasing	-5.83	***	0.50		0.51	+	0.27		-0.69	+	0.32	
L5	Steadily decreasing	-7.37	***	0.48		0.56		0.53		0.01		0.12	
L6	Heavy chronic	-7.96	***	-0.001		-0.13		0.56		0.92	+	0.13	

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

When considering Table 4-3.10, it is important to remember that the trajectories displayed in Figure 4-3.3 refer only to use of a lighter drug than the primary drug of choice. Thus, they do not consider use of harder drugs or the same drug as the primary drug of choice. These are considered below. Because the dependent variable is the log odds of belonging to a particular group, the coefficients are difficult to directly interpret. For this reason, we summarize the above table by presenting Table 4-3.11 indicating merely whether each variable makes membership to a particular group more likely or less likely. Blank cells indicate that drug court had no statistically significant impact on the probability of group membership.

As displayed in Table 4-3.10, drug court participation only has a statistically significant impact on the likelihood of membership in two of the groups: the abstention group and the rare use group, making both more likely, consistent with earlier estimates that drug court decreases the likelihood of using (Table 4-3.6) and the amount of use even among users (Table 4-3.8). The other characteristics discussed in Tables 4-3.10 and 4-3.11 are interesting, as well. Mental health issues appear to be more prevalent among abstainers, rare users, and steadily increasing users.

Those with low levels of addiction (compared to moderate addiction) appear to be most likely to be rare or moderately chronic users of lighter drugs than their primary drug of choice, while those with high levels of addiction appear to be most likely to be heavy chronic users, and less likely to abstain, use rarely, or steadily increase use. Finally, clinical depression makes abstention less likely and moderate levels of chronic use more likely.

Table 4-3.11. Factors Influencing Lighter Drug Use Trajectory

		Drug Court Participation	Poor/Fair Mental Health	Low Addiction	High Addiction	Depression
L1	Abstainer	More Likely	More Likely		Less Likely	Less Likely
L2	Rare	More Likely	More Likely	More Likely	Less Likely	
L3	Moderate chronic			More Likely		More Likely
L4	Steadily increasing		More Likely		Less Likely	
L5	Steadily decreasing					
L6	Heavy chronic				More Likely	

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

We followed the same process for those who relapsed using the same drug as the substance listed as their primary drug prior to drug court enrollment; and for those who relapsed using a more serious substance than their initial primary drug (see Figures 4-3.4 and 4-3.5, and Tables 4-3.12 through 4-3.17.

Table 4-3.12 displays the membership in each of the trajectory groups displayed below. The largest group is the black group, the abstainers from drug use, followed by the red group, who use mild amounts. Of concern, those in the group we titled mild drug use appear to be slowly increasing use throughout the follow-up period, although 19 months after baseline, they still tend to be using less than once per month, on average. Membership in both of these groups is statistically significantly more likely among drug court participants, as based on the hierarchical models displayed in Tables 4-3.13 and 4-3.14.

Table 4-3.13 displays how membership in each of the groups displayed in Figure 4-3.4 is affected by drug court participation and other individual characteristics measured at baseline. Again, because coefficients in regressions where the dependent variable is the log odds ratio are difficult to interpret, Table 4-3.14 presents intuitive interpretations of Table 4-3.13.

Together, Tables 4-3.13 and 4-3.14 indicate that predicting relapse to the primary drug of choice is somewhat more difficult than predicting relapse to a lesser drug (fewer statistically significant predictors). Drug court participants are far more likely to never use or to use only mild amounts (less than once per month). Individuals with low levels of addiction at baseline were also statistically significantly more likely to abstain from using the primary drug of choice, unsurprisingly. Individuals with low levels of addiction also appear to be statistically significantly less likely to steadily decrease drug use, likely because they are able to abstain early on, rather than building up to abstention throughout the treatment process. Surprisingly, however, individuals with high levels of addiction are less likely to eventually become daily users of their original primary drug of choice (represented by the orange trajectory in Figure 4-3.4).

Figure 4-3.4. Use of Primary Drug of Choice

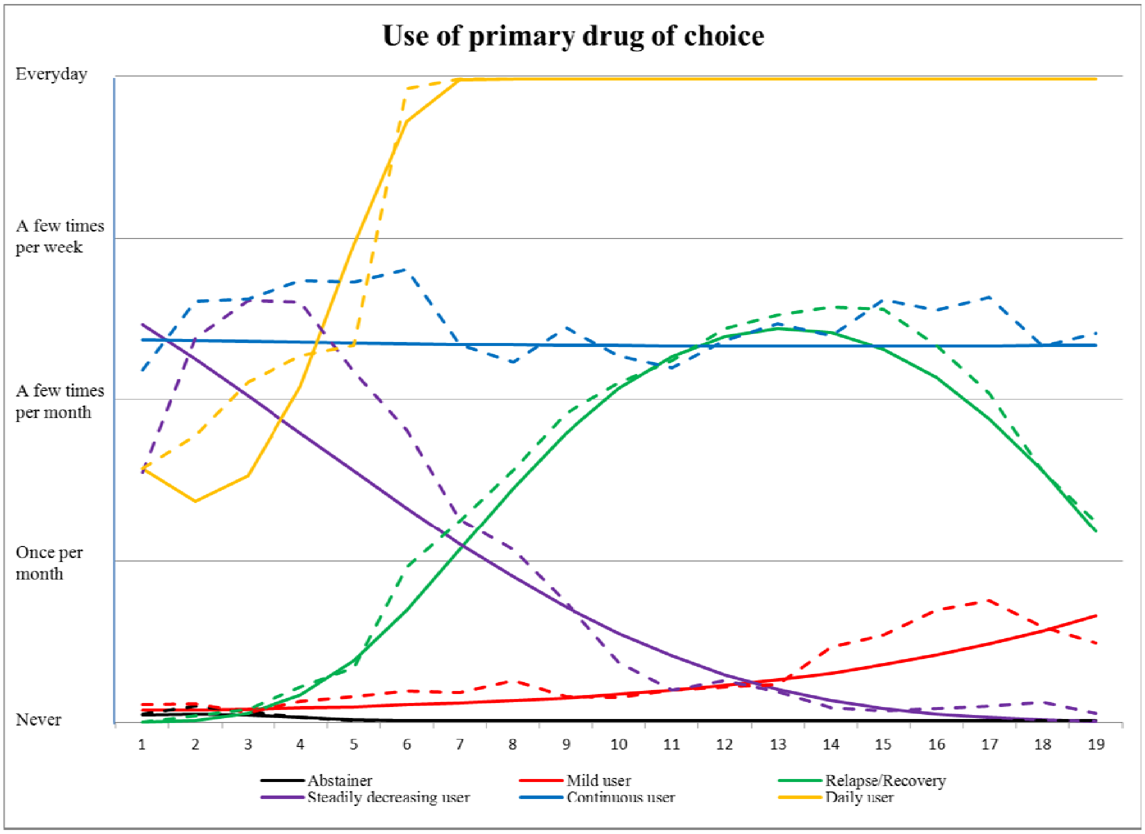


Table 4-3.12. Group Memberships in Primary Drug Use Trajectory Groups

Drug Use Equivalent to Primary Drug			Total	Drug Court Group		Comparison Group
Group 1	S1	Abstainer	55%	58%	*	48%
Group 2	S2	Mild	19%	20%	**	16%
Group 3	S3	Relapse/recovery	11%	10%		13%
Group 4	S4	Steadily decreasing	8%	6%		12%
Group 5	S5	Continuous user	6%	5%		9%
Group 6	S6	Daily user	1%	1%		1%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-3.13. Factors Influencing Primary Drug Use Trajectory

		Intercept		Drug Court Participation		Poor/Fair Mental Health	Low Addiction		High Addiction		Depression
S1	Abstainer	-3.66	***	2.2	*	-0.12	1.23	**	-0.37		0.1
S2	Mild	-4.09	***	1.54	**	0.51	0.28		-0.67		0.13
S3	Relapse/recovery	-7.62	***	0.3		0.54	-13		0.21		-0.64
S4	Steadily decreasing	-6.08	***	-1.31		-0.27	-0.67	+	-0.1		0.43
S5	Continuous user	-7.52	***	-0.83		0.13	-0.25		-0.64		-0.18
S6	Daily user	-10.6	***	0.68		-0.03	0.33		-0.78	+	0.001

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-3.14. Factors Influencing Primary Drug Use Trajectory

		Drug Court Participation	Poor/Fair Mental Health	Low Addiction	High Addiction	Depression
S1	Abstainer	More Likely		More Likely		
S2	Mild	More Likely				
S3	Relapse/recovery					
S4	Steadily decreasing			Less Likely		
S5	Continuous user					
S6	Daily user				Less Likely	

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Finally, Figure 4-3.5 presents the estimated trajectories for use of more serious drugs than an individual's primary drug of choice. The trajectories were estimated following the same process outlined above. This set of trajectories is particularly important, because it represents individuals who transitioned to worse drugs following baseline.

The number of trajectory groups, as with the other types of drug use, was decided by the Bayesian Information Criteria (BIC) following Jones et al. (2001). For use of harder drugs, the data appear to be best summarized by four groups. Membership in these groups is displayed in Table 4-3.15. As shown, 80 percent of drug court participants and 75 percent of the comparison group never reported using more serious drugs than their primary drug of choice at baseline. Only 5 percent of drug court participants and 9 percent of comparison group members tended to use harder drugs more than once per month.

Table 4-3.16 displays the factors that significantly predict membership in each of the groups presented in Figure 4-3.5. Tables 4-3.16 and 4-3.17 indicate that drug court participation makes abstention statistically significantly more likely. Those who reported low addiction also were more likely to fall into this group (compared to those with moderate addiction), while those with high levels of addiction at baseline were less likely (again, compared to those with moderate levels of addiction). Those with high addiction appeared to be more likely to shift into the moderate chronic group, where low levels of addiction were significantly less common. Mental health status, including depression, did not significantly predict membership in any of the groups.

Figure 4-3.5. Use of Harder Drugs than Primary Drug of Choice

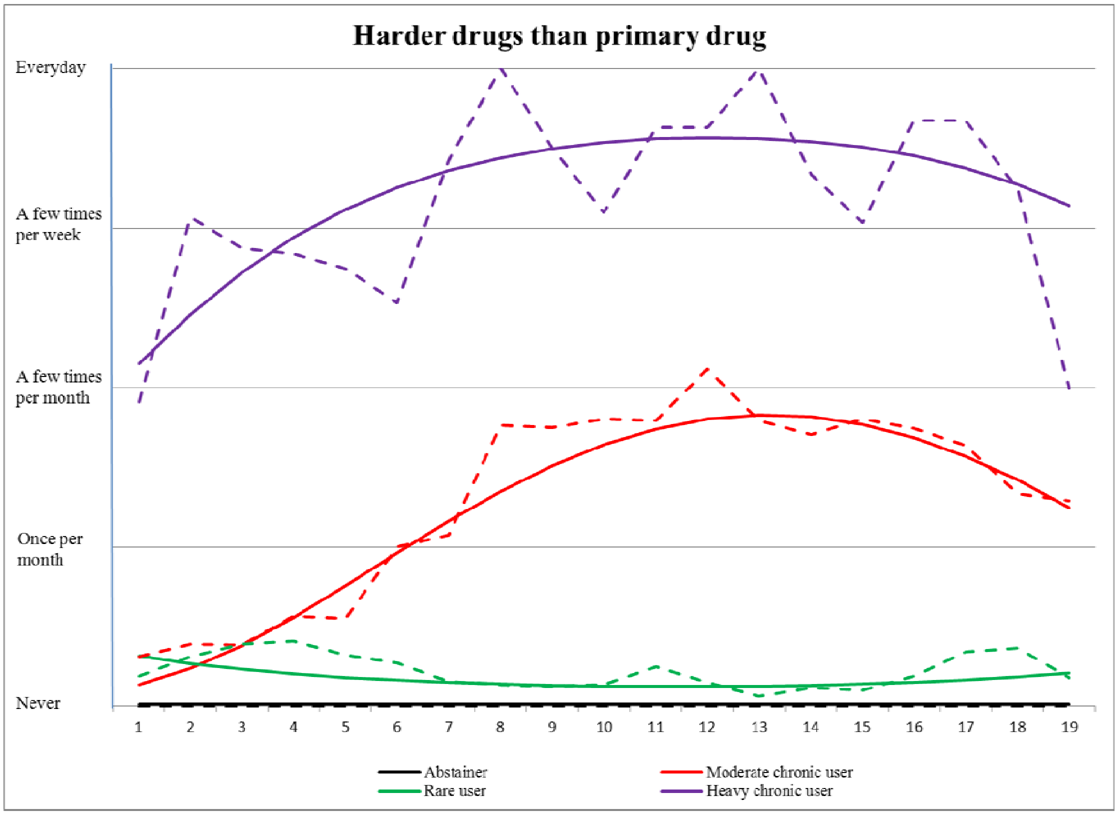


Table 4-3.15. Group Memberships in More Serious Drug Use Trajectory Groups

More Serious Drug Used Than Primary Drug			Total	Drug Court Group	Comparison Group
Group 1	H1	Abstainer	78%	80% +	75%
Group 2	H2	Moderate chronic	5%	4%	8%
Group 3	H3	Rare	16%	15%	17%
Group 4	H4	Heavy chronic	1%	1%	1%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-3.16. Factors Influencing More Serious Drug Use Trajectory

	Intercept	Drug Court Participation	Poor/Fair Mental Health	Low Addiction	High Addiction	Depression
H1 Abstainer	0.17	0.95 +	-0.16	0.76 *	-1.73 **	0.08
H2 Moderate chronic	-7.24 ***	-0.76	-0.05	-0.79 *	1.32 **	0.03
H3 Rare	-2.36 ***	0.29	0.02	-0.12	0.39	0.1
H4 Heavy chronic	-10.2 ***	0.4	-0.08	-0.34	0.01	0.16

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-3.17. Factors Influencing Harder Drug Use Trajectory

	Drug Court Participation	Poor/Fair Mental Health	Low Addiction	High Addiction	Depression
H1 Abstainer	More Likely		More Likely	Less Likely	
H2 Moderate chronic			Less Likely	More Likely	
H3 Rare					
H4 Heavy chronic					

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001.*

An important question is how these trajectories are related. That is, since each individual belongs to one trajectory in each set, it is important to know the connections among them. For instance, it is possible that a trajectory may appear to be a success (such as continuously declining use of lighter drugs), when in fact, offenders are just switching to another drug (such as the primary drug of choice). In this case, we would expect to see that being in the steadily declining group makes one more likely to be in the mild or everyday group for the primary drug of choice. We calculated the conditional probabilities of group membership to see whether this is substantiated.

Table 4-3.18 presents the probability of belonging to each more serious drug trajectory group (the groups along the top) given membership in each lighter drug trajectory group (along the left side). For instance, the first row can be interpreted as follows: given that an individual was in L1 (abstained from lighter drugs), there is an 83 percent chance that s/he will abstain from drugs more serious than his/her primary drug of choice, a 5 percent chance that s/he will be a moderate chronic user of harder drugs, a 12 percent chance that s/he will rarely use hard drugs, and a 1 percent chance that s/he will be a heavy chronic user of hard drugs. Notable in the table, abstainers from light drugs are most likely to also abstain from harder drugs. Also notable, we find some evidence for switching from lighter to harder drugs. Those who steadily decrease lighter drug use are least likely to abstain from harder drug use. Thus, indications that an individual transferred early to a lighter drug, while steadily diminishing lighter drug use during program participation, may be a warning sign that the individual is using other, more serious drugs.

Table 4-3.19 presents the probability of membership in each “same drug” trajectory group, conditional on membership in each light drug trajectory group. The first row can be interpreted as: given that an individual abstained from use of lighter drugs, there is a 65 percent chance that s/he will also abstain from the primary drug of choice, a 16 percent chance s/he will be a mild user of primary drug of choice, a 7 percent chance that s/he will go through the relapse recovery trajectory, a 6 percent chance s/he will exhibit a pattern of steadily decreasing use of primary drug of choice, a 5 percent chance s/he will be a continuous user of primary drug of choice, and a 1 percent chance that s/he will be a daily user of primary drug of choice.

Table 4-3.18. More Serious Drug Group Probability Conditional on Light Drug Group

		H1 Abstainer	**H2** Moderate chronic	**H3** Rare	**H4** Heavy chronic	**Total**
L1	Abstainer	83%	5%	12%	1%	100%
L2	Rare	76%	5%	19%	1%	100%
L3	Moderate chronic	73%	8%	19%	1%	100%
L4	Steadily increasing	76%	7%	17%	1%	100%
L5	Steadily decreasing	67%	5%	26%	2%	100%
L6	Heavy chronic	72%	6%	20%	1%	100%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

Table 4-3.19. Same Drug Group Probability Conditional on Light Drug Group

		S1 Abstainer	**S2** Mild	**S3** Relapse/ recovery	**S4** Steadily decreasing	**S5** Continuous user	**S6** Daily user	**Total**
L1	Abstainer	65%	16%	7%	6%	5%	1%	100%
L2	Rare	50%	25%	10%	7%	5%	1%	100%
L3	Moderate chronic	38%	19%	22%	11%	7%	3%	100%
L4	Steadily increasing	49%	23%	16%	5%	7%	1%	100%
L5	Steadily decreasing	40%	13%	9%	25%	13%	1%	100%
L6	Heavy chronic	38%	18%	12%	13%	15%	4%	100%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

We also present the “reverse” conditional probabilities. That is, given interest in a particularly concerning trajectory of use of a more serious drug or the same drug displayed in Figures 4-3.4 or 4-3.5, Tables 4-3.20 and 4-3.21 indicate the likelihood that the individual belonged to each trajectory group for lighter drugs.

These findings indicate that study participants had widely different experiences. Generally speaking, however, individuals who appear to be (un)successful with one drug are also (un)successful with others. Thus, the positive impact that drug court had on some trajectories is not mitigated by switching to other drugs. In general, drug court appears to be significantly related to the most successful trajectories of use for each type of drug, and success with one drug type appears to be related to success with others.

Table 4-3.20. Lighter Drug Use Probability Conditional on Hard Drug Group

		L1 Abstainer	L2 Rare	L3 Moderate chronic	L4 Steadily increasing	L5 Steadily decreasing	L6 Heavy chronic	Total
H1	Abstainer	51%	22%	12%	6%	4%	5%	100%
H2	Moderate chronic	41%	22%	19%	8%	5%	6%	100%
H3	Rare	37%	27%	15%	7%	8%	7%	100%
H4	Heavy chronic	40%	19%	11%	5%	13%	12%	100%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

Table 4-3.21. Lighter Drug Use Probability Conditional on Same Drug Group

		L1 Abstainer	L2 Rare	L3 Moderate chronic	L4 Steadily increasing	L5 Steadily decreasing	L6 Heavy chronic	Total
S1	Abstainer	58%	21%	9%	5%	4%	4%	100%
S2	Mild	41%	31%	12%	8%	3%	5%	100%
S3	Relapse/ recovery	33%	22%	26%	9%	4%	6%	100%
S4	Steadily decreasing	34%	21%	17%	4%	15%	8%	100%
S5	Continuous user	38%	20%	13%	6%	10%	12%	100%
S6	Daily user	32%	19%	26%	3%	4%	16%	100%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

The fourth analysis attempted to formalize questions of whether using less serious drugs led to using more serious drugs, and if so, whether individuals in such circumstances subsequently would be able to abstain (measured by whether they reported not using hard drugs at all during the last three months of the follow-up period). The analysis was restricted to the 486 individuals whose primary drug of choice was heroin, cocaine, amphetamines, or methadone (referred to here as "hard or serious drugs"). Among those individuals, 56 percent relapsed. Among those who relapsed, 31 percent started by using less serious drugs, while the remaining 69 percent started with harder or serious drugs. Of those who started with the less serious substances, 44 percent eventually relapsed back to the more serious drugs. None of the individuals who used the more serious drugs (regardless of whether they began by using less serious drugs after initially relapsing, or not) were able to demonstrate three consecutive clean months by the end of the sample period.

Although the findings seem to suggest that among this subset of individuals, those in drug court were less likely to start using again, less likely to use the serious drugs when they did start using, and less likely to transition from less serious to more serious drugs, none of these differences were statistically significant. Thus, the bottom line is:

- Relapsing to serious drugs is very dangerous because none of the individuals who relapsed to the harder drugs were able to quit.

- And, relapsing to less serious drugs is also dangerous due to the high chance that it will lead to further use of hard drugs (44 percent chance).

Thus, drug courts and treatment programs should strive to ensure abstinence from all drugs. We find little evidence that transitioning serious drug users to less serious drugs is a viable long-term strategy. Fortunately, however, our findings suggest that drug courts are successful in reducing all forms of drug use, and do not simply allow offenders to switch from one substance to another.

Concurrent Poly-Drug Use

In looking at patterns of drug use, we also examined whether users of less serious drugs were concurrently using more serious drugs. For each individual using less serious drugs (i.e., alcohol, prescription drugs, marijuana, and hallucinogens), we calculated the percent of the months during which the individual was using both the less serious substance and at least one more serious or hard drug (e.g., methadone, amphetamines, cocaine, and heroin). We also calculated the percent of individuals who used alcohol, prescription drugs, marijuana, and hallucinogens during the follow-up who also used a hard drug (heroin, non-prescription methadone, crack or powder cocaine, and amphetamines) at least once.

Table 4-3.22. Concurrent Use of Less Serious Substances and More Serious or Hard Drugs

Less Serious Substance Use Drug	Percent of Time Also Using More Serious or Hard Drugs	Percent of Less Serious Substance User Who Also Used Hard Drugs
Alcohol	33%	47%
Prescription drugs	18%	31%
Marijuana	52%	49%
Hallucinogens	17%	47%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

The results above seem to confirm the hypothesis of gateway drugs. For most softer substances, nearly half of those who used also used more serious drugs at some point. The only exception is prescription drugs, where only one-third also used hard drugs at some point (as shown in Table 4-3.22).

Confirming this, among individuals who drank heavy alcohol at some point during the follow-up, one-third of the months during which they drank heavily, they also used at least one hard drug at least once. For marijuana, more than half the time individuals using marijuana, they also were using harder drugs, on average. These numbers are considerably lower for prescription drugs and hallucinogens, which seem to have a much lower rate of concurrent use.

In total, it seems that use of lighter drugs is often associated with more serious drug use. To further explore the relationships among different types of drugs, we expanded these results by replicating column 1 of the above table for every drug interaction. That is, the Table 4-3.23 shows, for each pair of drugs, the average percent of time that one drug was accompanied by the other.

Table 4-3.23. Concurrent Use of All Drugs

Percent of time on X also on Y	Y = Alcohol	Prescrip. Drugs	Marij.	Halluc.	Amphet.	Cocaine	Methadone	Heroin	Any Drug
X = Alcohol	-	8%	**32%**	5%	6%	27%	1%	8%	**53%**
Prescription drugs	**40%**	-	**41%**	6%	12%	**37%**	9%	20%	**80%**
Marijuana	**42%**	11%	-	7%	8%	24%	2%	5%	**60%**
Hallucinogens	**65%**	20%	**66%**	-	15%	27%	1%	8%	**88%**
Amphetamines	**39%**	22%	**43%**	9%	-	**35%**	4%	10%	**66%**
Cocaine	**44%**	14%	**32%**	5%	10%	-	3%	13%	**69%**
Methadone	26%	**48%**	**39%**	6%	12%	**47%**	-	**42%**	**76%**
Heroin	**37%**	**32%**	28%	4%	9%	**49%**	8%	-	**80%**

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
Note: For display, figures larger than 30% are bolded.

The results displayed in the Table 4-3.23 indicate that there is significant overlap in drug use. In fact, if an individual is using any drug, there is more than a 50 percent chance that s/he is concurrently using another. At the lowest end, there is a 53 percent chance that heavy drinking is occurring in the same timeframe that the individual is using another drug, while there is an 88 percent chance that an individual on hallucinogens is concurrently using another drug.

It is important to acknowledge some potentially misleading suggestions from Table 4-3.23. The second and fourth columns seem to suggest that alcohol and marijuana are the most likely drugs to be used in combination with others. This is largely because of the relative prevalence of these drugs. As shown in the last column, alcohol users and marijuana users are the least likely to simultaneously be using other drugs.

Several interesting patterns arise. First, the severity of drugs can somewhat be judged by the rate of co-abuse with other serious drugs. For instance, though often considered harmless, prescription drugs seem to be closely associated with more serious drugs: 37 percent of the time individuals are using prescription drugs, they also are using cocaine; and 20 percent of the time, they are also using heroin. Thus, use of prescription drugs, though argued by many to be relatively benign in its own right, can be seen as an indicator of more serious drug problems.

It is also important to note clustering in the most severe drugs. For instance, methadone users are among the least likely to also use marijuana or alcohol (often considered the most benign drugs), but there is a 42 percent chance of concurrent use of heroin (the highest) and a 47 percent chance of concurrent cocaine use (the second highest).

For Whom Drug Courts Work

The purpose of these analyses was to identify whether certain categories of offenders were especially likely to *benefit* from the drug court intervention—relative to how they would have performed without the intervention. In Table 4-3.24, we again focused on the two 18-month outcomes of the average days of use per month during the previous year and whether the offender tested positive for any drug. Separate models examined whether there was a significant moderating effect on the magnitude of the drug court impact based on each of a series of baseline characteristics related to drug use, criminality, mental health, community ties, and demographics.

Table 4-3.24. Interaction Effects for Drug Court Participation and Select Baseline Characteristics on Drug Use at 18 Months

Number of Cases	Average Days of Drug Use per Month N=1,474	Positive Drug Test (Any Drug) N=1,147
Sample: Drug Court (vs. Comparison)	-.812***	.490**
Drug Use		
Sample	-.752***	.525*
Alcohol as Primary Drug	.414*	1.23
Sample * Alcohol	-.250	.602
Sample	-.930***	.459**
Marijuana as Primary Drug	.117	1.27
Sample * Marijuana	**.438+**	1.38
Sample	-.796***	.436**
Cocaine as Primary Drug	-.270**	.951
Sample * Cocaine	.003	1.23

(continued)

Table 4-3.24. Interaction Effects for Drug Court Participation and Select Baseline Characteristics on Drug Use at 18 Months (Cont'd)

Number of Cases	**Average Days of Drug Use per Month N=1,474**	**Positive Drug Test (Any Drug) N=1,147**
Sample	-.575**	.638+
Average Days of Drug Use	.040***	1.03+
Sample * Average Days of Drug Use	**-.015***	.979
Criminality		
Sample	-.784***	.505**
Prior Convictions	.420**	.807
Sample * Prior Convictions	-.283	.575
Sample	-.749***	.528**
Prior Violent Convictions	.445***	.873
Sample * Prior Violent Convictions	**-.449+**	**.535+**
Sample	-.102***	.581
Number of Criminal Activities (6 Month Pre Baseline)	.445**	1.51
Sample * Criminal Activities	.019	.789
Mental Health		
Sample	-.941***	.535*
Depression	.041	1.51
Sample * Depression	**.298****	.789
Sample	-.830***	.544*
Antisocial Personality Disorder	.552***	1.16**
Sample * ASPD	-.016	.772
Sample	-1.07***	.529*
Narcissism	.019	.235*
Sample * Narcissism	**.437****	.473
Community Ties		
Sample	-.805***	.457**
Married	-.366	.525
Sample * Married	.152	1.79

(continued)

Table 4-3.24. Interaction Effects for Drug Court Participation and Select Baseline Characteristics on Drug Use at 18 Months (Cont'd)

Number of Cases	Average Days of Drug Use per Month N=1,474	Positive Drug Test (Any Drug) N=1,147
Sample	-.487**	.472*
Employed or Enrolled in School	-.034	.750
Sample * Employment or School	**-.532+**	1.03
Sample	-.723***	.578*
Blood Relatives' Involvement in Drug/Crime	.054	1.02
Sample * Family Criminal Involvement	-.047	.907
Basic Demographics		
Sample	-.901***	.456**
Black Defendants	-.121	1.72**
Sample * Black Defendants	**.303+**	1.24
Sample	-.760***	.483**
White Defendants	-.065	.555*
Sample * White Defendants	-.084	1.04
Sample	-.836***	.597
Male Sex	.260*	2.26**
Sample * Male Sex	.025	.736
Sample	-.439	.401
Age of Defendant	-.015*	.983
Sample * Age	-.012	1.01

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

Notes: The coefficients for average days of drug use are simple regression coefficients, based on a Poisson specification. The coefficients for whether the offender tested positive are logistic regression odds ratios. The first indicated regression equation (top row in the table for "Sample") models the impact of drug court participation status on each of the two outcome variables. All subsequent regression equations include different sets of three independent variables: (1) sample (drug court participant or comparison group offender), (2) a given baseline characteristic, and (3) an interaction term combining the first two variables (1*2).
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Overall, it does not appear to be the case that drug court interventions were only or disproportionately effective with some categories of offenders as opposed to others. In fact, in all the models predicting a positive drug test result, none of the interaction terms was significant at the standard .05 level. (Significant interaction terms indicate a particularly large or small drug court impact among offenders with the given baseline characteristic.) In the models predicting days of drug use per month, we found that the drug court model produced a greater reduction in drug use among those who used drugs more days per month at baseline ($p < .05$). We also found that the interaction effects of drug court participation with the two mental health conditions of depression and narcissism were significant ($p < .01$)—in both cases suggesting that those who possessed a co-occurring mental health disorder at baseline benefited *less* from the drug court intervention. Several other interaction terms suggested a possible effect at a weak significance threshold ($p<.10$); in particular, those who were employed or in school at baseline may have benefited more from the intervention, whereas black offenders and those whose primary drug was marijuana, a less serious drug, may have benefited less.

Conclusions

Based on our analyses mainly of the self-report data, we find that drug courts were effective in reducing substance abuse relapse. Six months after entering drug court, program participants averaged fewer days of drug use per month (1.5 vs. 3.7 days; $p < .01$) and fewer days of *serious* drug use per month (1.0 vs. 2.2 days, $p < .05$) than the comparison group. By the 18-month follow up, the drug court cohort had significantly fewer occurrences of any drug use (56 percent vs. 76 percent, $p < .01$), serious use (41 percent vs. 58 percent, $p < .01$), days of use per month (2.1 vs. 4.8, $p <.001$), and days of serious use per month (1.1 vs. 2.3; $p < .001$). Regarding specific substances, drug court participants were significantly less likely than the comparison members to report use of marijuana, alcohol, “heavy” use of alcohol, illegal use of prescription drugs, and illegal use of methadone; there were no significant differences between the two groups for cocaine, heroin, amphetamines, and hallucinogens, although the differences reported trended in the direction of less use by the drug court sample.

Additionally, statistically significant percentages of drug court participants report no relapse during the 18-month period; similarly, drug court participants were statistically significantly less likely to relapse in the first six months. Conversely, a small, but statistically significant, percentage of the comparison group reported no sobriety within the 18 months.

In looking at baseline characteristics as predictors of future drug use, we used two outcomes: (1) days of drug use per month and (2) having a positive Buccal swab sample collected when the 18-month survey was administered. Not surprisingly, we found that a greater frequency of drug use at baseline significantly predicted a greater frequency at 18 months for both drug court and comparison offenders. Other baseline characteristics that predicted greater drug use on at least one, if not both, of the outcome measures included: a younger age, male gender, black race, having been classified with depression (based on a multi-item screening tool), and having been classified with anti-social personality disorder (also based on a multi-item tool). However, we did find evidence of a relationship between offender social ties (e.g., based on employment or school status, marital status, involvement of blood relatives with crime or drugs) and less drug use; nor did we find that prior criminal history predicted future drug use. Overall, the strongest and most consistent predictors of drug use outcomes were participation in the drug court, less frequent drug use at baseline, and the absence of mental health problems at baseline.

We constructed trajectories to examine whether those who relapsed resumed use of a "less serious" drug than their primary drug of choice at baseline, the same drug, or a "more serious" drug than their primary drug. For example, with respect to using less serious drugs than one's drug of choice, drug court participation only has a statistically significant impact on the likelihood of being in the groups who abstain or rarely use. Mental health issues appear to be more prevalent among abstainers, rare users, and steadily increasing users of less serious drugs than their primary choice. Those with low levels of addiction (compared to moderate addiction) appear to be most likely to be rare or moderately chronic users of lighter drugs than their primary drug of choice, while those with high levels of addiction appear to be most likely to be heavy chronic users, and less likely to abstain, use rarely, or steadily increase use. Finally, clinical depression makes abstention less likely and moderate levels of chronic use more likely.

When looking at the relationships among the three "types" of users (i.e., those who use less serious drugs than their primary choice, those who use the same, and those who use more serious), we find that those who appear to be (un)successful in abstaining from using one drug are also (un)successful with others. Thus, the positive impact that drug court had on some trajectories is not mitigated by switching to other drugs. In general, drug court appears to be significantly related to the most successful trajectories of use for each type of drug, and success with one drug type appears to be related to success with others.

Finally, we addressed the question of "for whom drug courts work." Overall, it does not appear to be the case that drug court interventions were only or disproportionately effective with some categories of offenders as opposed to others. In the models predicting days of drug use per month, we found that the drug court model produced a greater reduction in drug use among those who used drugs more days per month at baseline ($p < .05$). We also found that the interaction effects of drug court participation with depression and narcissism were significant ($p < .01$), suggesting that those who possessed a co-occurring mental health disorder at baseline benefited *less* from the drug court intervention. Several other interaction terms suggested a possible effect at a weak significance thresholds ($p<.10$); in particular, those who were employed or in school at baseline may have benefited more from the intervention, whereas black offenders and those whose primary drug was marijuana, a less serious drug, may have benefited less.

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Chapter 4. Do Drug Courts Reduce Crime and Incarceration?

Michael Rempel and Mia Green

For the past two decades, dozens of evaluations have tested whether adult drug courts reduce official recidivism, and most have found that they do (Aos, Phipps, et al. 2001; Government Accountability Office 2005; Roman and DeStefano 2004; Schaffer 2006; Wilson, Mitchell, and MacKenzie 2006). However, many of the completed studies were based on flawed methodologies, involving inappropriate comparison groups or insufficient methods to control for baseline differences between the drug court and comparison samples (Roman and DeStefano 2004, Wilson et al. 2006). Among the stronger studies in the literature, most show reductions in re-offending, but the results are not uniformly positive (see Deschenes, Turner, and Greenwood 1995; Macklin, Lucas et al. 2009; Miethe, Lu, and Reese 2000). Moreover, the meta-analysis by Wilson and colleagues (2006) revealed a wide range of specific effect sizes, rendering problematic any effort at estimating an average impact. Although several studies employed a single methodology to estimate average effects across multiple sites, they were each statewide evaluations whose results were limited to a single state court system (Aos et al. 2001; Carey, Crumpton, et al. 2005; Latessa, Shaffer, and Lowenkamp 2002; Rempel, Fox-Kralstein, et al. 2003; Wiest, Carey, et al. 2007).

Aside from these limitations, little is known about whether and to what extent drug courts reduce the exponentially larger quantity of criminal behavior that goes officially undetected. Furthermore, only a handful of studies have examined outcomes other than recidivism. For example, regarding the *criminal justice* effects of adult drug courts, one particular interest is whether they truly serve as an "alternative to incarceration" on the precipitating criminal case, leading program participants to spend less time in custody than they would have otherwise. Finally, across all outcomes, little is known concerning whether drug courts are especially effective or less effective with specific categories of offenders, defined by their criminal history, drug use severity, mental health, or other factors.

Research Questions

This chapter focuses on the criminal justice effects of adult drug courts. Specific questions are:

1. To what extent do adult drug courts reduce criminal behavior?

2. How durable are drug court effects beyond the period of active program participation?

3. If drug courts reduce criminal behavior, are such effects mediated by reduced drug use? That is, does the evidence support the hypothesized link between drug abuse and crime, whereby addressing the underlying addiction leads, inexorably, to less crime as well?

4. For which categories of offenders is the drug court intervention more or less effective? Does effectiveness depend on whether the offenders are generally at a "high risk" or "low risk" of future criminality?

5. Do adult drug courts provide a true "alternative to incarceration"—leading participants to spend less time in custody on the precipitating criminal case?

Design and Methodology

The research for this chapter focuses specifically on the criminal justice impacts of adult drug courts: including criminal behavior and incarceration time on the precipitating criminal case. Essential features of the study design and methodology are found in Volume 1 (Chapter 3) and this Volume (Chapter 2 and Appendix A), and are not detailed here.

Outcome Measures

Criminal Behavior

The offender survey included questions on multiple types of criminal behavior that may not have been officially detected (e.g., drug sales, drug possession, property offenses, violent offenses, etc.). The 6-month data covered the preceding 6 months, and the 18-month data covered the preceding year. For those offenders who were surveyed at both follow-up waves, we constructed summary measures that totaled responses throughout the entire 18-month tracking period.

Official Re-Arrests

The survey data also included self-reported official re-arrests, but as is customary in such studies, we measured official re-arrests with official administrative data instead. Such data were collected for a 24-month tracking period for the 1,577 offenders (89 percent of our initial sample) who gave explicit permission during our informed consent process. As previously indicated, administrative data were obtained from the SACs or other state sources in each of the eight states where drug courts and comparison sites were located, and from the FBI's NCIC where data provided by local law enforcement are not necessarily comparable across sites. Of particular concern, NCIC reporting requirements are more stringent for serious than for low-level (e.g., misdemeanor) cases, such that the NCIC data are likely to exclude many re-arrests on less serious charges. Consequently, we ultimately relied on the state/SAC data for all in-state arrests, supplemented with NCIC data for out-of-state arrests. We also relied on NCIC data for our two Georgia sites, since other sources we could access provided incomplete records.

Of the 1,577 consenting offenders, we obtained an official criminal records match for 1,534 (97 percent), including 1,015 drug court and 519 comparison offenders.[8] We then inspected the results, performing a simple comparison of survey- and administrative data re-arrests for an 18-month tracking period that both datasets could encompass (see Table 4-4.1). As is entirely

[8] By comparison, the NCIC data only provided criminal record information on 89 percent of the 1,577 consenting offenders, a substantially lower match rate than what we obtained by relying primarily on the state-based SAC data.

commonplace in such comparisons (e.g., Harrell, Roman, and Sack 2001), there was a substantial amount of contradiction between the data sources, and the offenders self-reported fewer re-arrests than were found in the administrative data. Encouragingly, however, results from the two data sources were significantly correlated (r = .213), and additional analyses (not shown) did not detect systematic differences by state in the relationship between self-reported and administrative data results. (We had been concerned that due to obtaining administrative data from separate state-based SACs, administrative data quality might vary by state, but this did not appear to be the case.)

In preparing the administrative data for our impact analysis, we repeated in full an equivalent process to the super weighting strategy that was outlined in Chapter 2 and Appendix A of this Volume. That is, we computed a special propensity model for the 1,534 offenders for whom we obtained official records data; a special retention model comparing those offenders to the 247 offenders for whom we lacked administrative data (because they did not consent or because we did not receive a criminal records match); and a special set of administrative data super weights. The propensity model incorporated several official criminal history measures as follows: whether the offender had a prior arrest, whether the offender had a prior drug arrest, the number of prior arrests, the number of prior drug arrests, and the top charge on the instant offense (drug, property, or other). The administrative data super weights were used when analyzing both official re-arrest and official incarceration data (see next sub-section).

Table 4-4.1. Official vs. Self-Reported Re-Arrests Up to 18 Months

		Official Re-Arrest?		Correlation of the Two Measures
1. Entire Sample (N = 1,179)				
		Yes	No	
Self-Reported Re-Arrest?	Yes	95	32	R = .213
	No	428	624	
2. Drug Court (N = 775)				
		Yes	No	
Self-Reported Re-Arrest?	Yes	50	20	R = .172
	No	293	412	
3. Comparison Group (n = 404)				
		Yes	No	
Self-Reported Re-Arrest?	Yes	45	12	R = .280
	No	135	212	

Notes: The sample size (*N* = 1,179) includes cases for which offender survey data was available at both 6 months and 18 months (so that a combined 18-month measure could be computed) and for which administrative records data were available. An offender is coded as “yes” if the data included at least one re-arrest.

Incarceration

The offender survey data included responses for how many days the offenders was incarcerated during the 6 months prior to the 6-month survey and during the year prior to the 18-month survey. These responses also were totaled to create 18-month summary measures. In addition, our administrative dataset for all states except North Carolina included the sentence for each case in the criminal record. For North Carolina, we did not know the original sentence length, but we did have variables for "in" and "out" dates, indicating exactly when each offender was incarcerated during our 24-month tracking period. From these administrative sources, we constructed a measure for each offender for days sentenced to custody on the precipitating criminal case; as well as a measure for days sentenced to custody on any case (the precipitating case or re-arrest cases) within the same 24-month tracking period during which we measured official re-arrests. Since we used sentencing rather than correctional data for all states except North Carolina, our official measures may not include time spent in pretrial detention or time spent serving jail sanctions. Since that time was covered in the 18-month self-report measures, it is not surprising that the self-report measures yielded higher average incarceration totals than the administrative data.

Analytic Strategy

We began by confirming the need for hierarchical modeling. Table 4-A.7 (see Appendix A in this Volume) displays these results for four core outcomes: (1) the number of criminal acts in the year prior to the 18-month survey, (2) the number of official re-arrests over 24 months, (3) the number of days incarcerated over 24 months (based on administrative data), and (4) the number of days incarcerated on the precipitating case (based on the administrative data). The results (see Level 2) confirm that the sites significantly differed on all three outcomes ($p < .001$ for each outcome). Of some concern, for official re-arrests over 24 months, the results also indicate an exceptionally high intraclass correlation coefficient (ICC) of .262; in other words, more than 26 percent of the variation in official re-arrests is explained by site-level differences, presumably resulting in large part from differential law enforcement practices. By comparison, the ICC for criminal behavior was only .026, still significant, but exactly one-tenth the magnitude of the coefficient for official re-arrests. Furthermore, when separately running null models only for the 23 drug court sites and only for the 6 comparison sites (results not shown), the ICC for official re-arrests remained similarly high (.264 and .271, respectively). Thus, the inter-site variation in re-arrest outcomes was not merely reducible to differences between drug court and comparison sites. We suspect that the actual explanation has to do with differential police deployment and enforcement practices across our sites. Since we cannot control for such differential law enforcement practices, it represents an important study limitation. By using hierarchical modeling techniques to take into account the high inter-site variations in re-arrest outcomes, we can at least adopt an appropriately conservative standard before reporting as statistically significant any observed differences between drug court and comparison offenders.

All analyses were conducted in HLM.[9] We also investigated adjusting our criminal behavior outcome estimates for *time at risk*—that is, discounting the time that offenders spent incarcerated, when they presumably had fewer opportunities to commit new crimes (see Chapter 2 and Appendix A). Regarding specific results, the magnitude of the drug court impact on criminal behavior *increased* slightly after adjusting for time at risk, but the essential nature of the findings was unchanged. Thus, our reported results include ones that adjust for time at risk only on a small number of key count measures (number of criminal acts or number of re-arrests).[10]

Besides testing the impact of drug court status, we included several multivariate models examining the impact on criminal behavior of other baseline characteristics, such as demographic background, drug use history, criminal history, and mental health (see specific measures in Table 4-4.4).[11] Finally, we sought to examine whether drug courts exerted relatively greater or lesser impacts with some as opposed to other categories of offenders. For these analyses, we selected baseline characteristics in five areas that we thought might moderate the drug court impact: (1) drug use history, (2) prior criminality, (3) mental health, (4) social ties (e.g., to employment or marriage), and (5) primary demographics (e.g., age, race, and gender). We then constructed a series of three-predictor models to test whether the selected characteristics moderated the drug court impact. (The three predictors were drug court status, the baseline characteristic of interest, and an interaction term for the two.) We contemplated building up to more complex models, but because extremely few interaction terms turned out to be significant in the initial three-variable regressions, further steps were deemed superfluous.

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[9] The HLM 6.04 statistical package enables running ordinary least squares regression, logistic regression, and Poisson regression. We selected the most appropriate specification for each outcome, using logistic regression for dichotomous measures (e.g., any criminal behavior) and Poisson regression for right-skewed count distributions (with many zeros, some ones, fewer twos, and declining higher values). A Poisson specification proved to be most appropriate for all continuous criminal behavior, official re-arrest, and days of incarceration measures. For our results tables, we transformed the HLM regression coefficients for the intercept and for drug court status to produce adjusted means for drug court and comparison offenders, respectively. Although our reported results include simple percentages or averages (percent engaged in criminal behavior, average number of criminal acts, etc.), all such outcomes are never based on the raw data but are always *adjusted*—through hierarchical modeling and super weighting. Final analyses of offender survey outcomes incorporated the weights that were initially designed for offenders who were retained at the 18-month survey wave. Sensitivity analyses determined that the use of specially tailored weights (e.g., 6-month weights for 6-month outcomes or weights for those retained at both periods for outcomes drawing on both 6- and 18-month data) did not influence the reported results or their significance.
[10] For dichotomous outcome measures (e.g., re-arrested or not), alternative results are not presented with a time at risk adjustment. However, in test analyses (not shown), we analogously determined that when inserting days incarcerated as an independent control variable, our impact findings on those measures did not meaningfully change.
[11] In these multivariate models, we entered all background characteristics as fixed, rather than random effects. In other words, our analyses sought to model the *average* effect of each characteristic across *all* of the sampled sites, without examining whether the nature of that impact varied by site. Supporting this decision, in random effects models whose results are not shown, we found that of 20 specific regression parameters, the effect of only three parameters significantly varied by site: criminal behavior or drug involvement of blood relatives; number of prior criminal acts; and classification with depression at baseline. Since merely chance variation could have contributed to the appearance of one or two of these significant random effects, we did not believe that the evidence was sufficient to necessitate shifting to a random effects framework with respect to any of other parameters.

Results

Comparing the Drug Court and Comparison Group Interventions

Our research design eschewed a no-treatment comparison group, seeking instead a more realistic and varied mix of counter-factual conditions to drug court participation. Table 4-4.2 compares the resulting interventions that were in fact received by both samples. Interestingly, more than one-third (36 percent) of the comparison sample reported receiving at least some treatment during the initial six-month period after baseline, a meaningful fraction, even if it is far less than the 83 percent of drug court offenders who reported receiving treatment. Otherwise, drug court offenders were far more likely than the comparison group to have had at least one judicial status hearing (93 percent vs. 14 percent); and drug court offenders averaged many more such hearings (10.3 vs. 1.2), supervision officer contacts (17.2 vs. 6.4), and required drug tests (30.9 vs. 4.3) than the comparison group. Underlining the unique role of the drug court judge, whereas 76 percent of the drug court sample reported receiving judicial "praise" at some point, only 10 percent of the comparison sample reported ever receiving praise from the judge. On the other end of the spectrum, half (50 percent) of the drug court, as compared with 15 percent of the comparison sample reported receiving an interim sanction for noncompliance. Taken together, these results highlight that, consistent with the official drug court model, the drug court sample averaged a far more comprehensive package of interventions than the comparison group. At the same time, it was certainly not the case that business as usual, as embodied in our comparison group, involved a strict "nothing" or "no treatment" condition.

The Drug Court Impact on Criminal Behavior and Incarceration

Table 4-4.3 compares outcomes between drug court and comparison offenders on criminal behavior (regardless of whether it was officially detected), re-arrests, and incarceration. In the first six months of follow up, we found that drug court offenders were significantly less likely than the comparison group to report engaging in any criminal behavior (28 percent vs. 40 percent, $p < .05$); and drug court offenders averaged significantly fewer total instances of such behavior (12.8 vs. 34.1 criminal acts, $p < .001$). We detected additional significant differences in the prevalence of drug-related, DWI/DUI, and property-related criminal behavior.

During the following year (the one-year period prior to the 18-month survey), the same patterns persisted. Specifically, drug court offenders were significantly less likely to engage in any criminal behavior (40 percent vs. 53 percent), drug-related crime (36 percent vs. 50 percent), DWI/DUI (19 percent vs. 27 percent), and property crime (4 percent vs. 10 percent). Among drug crimes, drug court offenders were significantly less likely to engage in both drug possession and drug sales offenses. Finally, drug court participation appeared to have a powerful impact on the total quantity of criminal activity, averaging more than 50 percent fewer criminal acts than the comparison group (43.0 vs. 88.2, $p<.01$); and more than 50 percent fewer drug-related crimes as well (30.6 vs. 83.1, $p<.001$). Of final note, both samples engaged in little violent, weapons-related, or public order offending, and differences on these latter measures were not significant.

Table 4-4.2. Interventions Received by Drug Court and Comparison Offenders

Offender Data for First Six Months Since Baseline	Drug Court N=1,009	Comparison Group N=524
Substance Abuse Treatment		
Percent of offenders with any treatment	83%***	36%
Average days in treatment	59***	23
Percent with residential treatment	25%	14%
Percent with outpatient treatment	77%***	30%
Judicial Supervision		
Percent of offenders with any judicial status hearings	93%***	14%
Average number of status hearings	10.3***	1.2
Case Management and Other Supervision		
Percent with any contact with supervision officer	96%**	71%
Average number of face-to-face contacts	17.2***	6.4
Average number of phone contacts	6.8*	3.8
Drug Testing		
Percent with any drug test	95%***	61%
Average number of drug tests	30.9***	4.3
Sanctions and Incentives		
Percent receiving any judicial sanction	50%***	15%
Percent receiving any incentive/reward	86%***	37%
Percent receiving praise from the judge	76%***	10%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

Notes: The individual-level results reported in this table were computed in HLM 6.04 (utilizing hierarchical modeling), and the data were weighted, as described in Chapter 2 (the methodology section). The following variables had small numbers of missing cases: both measures on judicial status hearings (53), any contact with supervision officer (8), number of face to face contacts with supervision officer (10), number of phone contacts with supervision officer (15), and both measures on drug tests (46).
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-4.3. Criminal Behavior, Official Re-Arrest, and Incarceration Impacts

Outcome Measure	Drug Court	Comparison Group
1. Criminal Behavior: Six Months Prior to Six-Month Survey	**N = 1,009**	**N = 524**
Any criminal act: eight types	28%*	40%
Number of criminal acts: eight types	12.8***	34.1
Number of criminal acts per year at risk/not incarcerated	30.3***	77.6
Any drug-related act (includes sales, possession, and others)	25%*	38%
Number of drug-related acts	8.8***	26.3
Number of drug-related acts per year at risk/not incarcerated	22.4***	60.3
Percent engaging in specific types of criminal activity:		
(1) Any drug use or possession	24%*	37%
(2) Any drug sales	6%	11%
(3) Any other drug crimes (manufacturing, trafficking, etc.)	2%	4%
(4) Any DWI/DUI	9%*	15%
(5) Any violent crime/crime against people	1%	1%
(6) Any weapons possession	4%	5%
(7) Any property crimes	2%*	5%
(8) Any public order crimes (e.g., prostitution, vagrancy)	1%	1%
2. Criminal Behavior: One Year Prior to 18-Month Survey	**N = 951**	**N = 523**
Any criminal act: eight types	40%*	53%
Number of criminal acts: eight types	43.0**	88.2
Number of criminal acts per year at risk/not incarcerated	51.2***	103.0
Any drug-related act (includes sales, possession, and others)	36%**	50%
Number of drug-related acts	30.6***	83.1
Number of drug-related acts per year at risk/not incarcerated	40.5***	101.6
Percent engaging in specific types of criminal activity:		
(1) Any drug use or possession	34%**	50%
(2) Any drug sales	9%*	16%
(3) Any other drug crimes (manufacturing, trafficking, etc.)	2%	2%
(4) Any DWI/DUI	13%*	20%
(5) Any violent crime/crime against people	4%	3%
(6) Any weapons possession	7%	8%
(7) Any property crimes	3%*	6%
(8) Any public order crimes (e.g., prostitution, vagrancy)	1%	1%
3. Criminal Behavior: 18-Month Tracking Period (Combined Follow Up)	**N = 877**	**N = 472**
Any criminal act: eight types	49%*	64%
Number of criminal acts: eight types	52.5***	110.1

(continued)

Table 4-4.3. Criminal Behavior, Official Re-Arrest, and Incarceration Impacts (Cont'd)

Outcome Measure	Drug Court	Comparison Group
Number of criminal acts per year at risk/not incarcerated	41.3***	93.8
Any drug-related act (includes sales, possession, and others)	44%*	61%
Number of drug-related acts	38.1***	100.4
Number of drug-related acts per year at risk/not incarcerated	32.3***	83.8
Percent engaging in specific types of criminal activity:		
(1) Any drug use or possession	43%*	61%
(2) Any drug sales	12%*	21%
(3) Any other drug crimes (manufacturing, trafficking, etc.)	4%	5%
(4) Any DWI/DUI	19%*	27%
(5) Any violent crime/crime against people	5%	4%
(6) Any weapons possession	8%	9%
(7) Any property crimes	4%**	10%
(8) Any public order crimes (e.g., prostitution, vagrancy)	2%	3%
4. Official Re-Arrests: 24 Months Post-Enrollment	**N = 1,022**	**N = 512**
Any re-arrest	52%	62%
Number of re-arrests	1.24	1.64
Number of re-arrests per year at risk/not incarcerated	0.72+	1.01
Any drug re-arrest	17%	22%
Number of drug re-arrests	0.21	0.26
Number of drug re-arrests per year at risk/not incarcerated	0.13	0.15
5. Incarceration and Sentence Length		
18 Months Post-Enrollment (using survey data)	N = 877	N = 472
Any days incarcerated	58%	57%
Number of days incarcerated	62.7	95.3
24 Months Post-Enrollment (using administrative data)	N = 967	N = 491
Any days sentenced to custody	19%	26%
Number of days sentenced to custody	32.1*	59.4
Days Incarcerated on the Precipitating Case (using administrative data)	N = 943	N = 460
Any days sentenced to custody	22%	22%
Number of days sentenced to custody	97.2	76.7

Notes: The following variables had small numbers of missing cases for the 18-month wave only: any DWI/DUI (28), any weapons possession (3), any property crimes (3), and any public order crimes. The same numbers of cases also were missing for the equivalent combined variables that totaled responses from the 6- and 18-month waves. There also were small numbers of cases missing data for the official sentencing data, which is why the sample sizes indicated in Table 4.3 are lower for administrative data sentencing outcomes (part 5 of the table) than for official re-arrests (part 4 of the table).
*+p<.10, *p<.05, **p<.01, ***p<.001.*

When totaling responses across the two follow-up survey waves to create a complete set of 18-month criminal behavior measures, the patterns were comparable (see Table 4-4.3, above). Almost half of the drug court sample (49 percent) reported at least one criminal act across the full 18-month tracking period; yet, this figure remains significantly lower than the 64 percent figure for comparison offenders ($p<.05$). When counting the total number of criminal acts, the differences were again magnified, with drug court offenders again averaging more than 50 percent fewer criminal acts than the comparison group (52.5 vs. 110.1, $p < .001$).

Regarding criminal acts that were officially detected, 52 percent of drug court offenders compared with 62 percent of comparison offenders were re-arrested over 24 months. Drug court offenders also averaged fewer total re-arrests than the comparison group (1.25 vs. 1.66). Yet, these results, as well as additional results that isolated drug-related re-arrests, were not statistically significant. (After implementing a time-at-risk adjustment, drug courts did appear to produce fewer re-arrests per year at risk at the .10 significance threshold.)

A virtually identical percentage of drug court and comparison offenders reported that they experienced at least some incarceration during the 18 months (58 percent vs. 57 percent, n.s.). Drug courts may have reduced the total number of days incarcerated, as the drop from 95.3 days on average for the comparison group to 62.7 days for the drug court sample represents a meaningful 34 percent relative reduction, but the difference was not statistically significant. Additional analyses (not shown in Table 4-4.3) revealed drug court graduation to be a critical intervening factor. Among drug court offenders who had completed their participation by the 18-month mark ($n = 630$), 27 percent of graduates compared with 93 percent of those failing experienced any incarceration; and graduates averaged only 11.8 days incarcerated, compared with 143.9 days for those failing.

We further found that drug court offenders averaged significantly fewer days sentenced to jail or prison during the 24-month tracking period covered by administrative records (32.1 vs. 59.4 days). When isolating the sentence on just the precipitating criminal case, however, the differences between the samples were not significant, and the raw data pointed to somewhat more days sentenced among those enrolled in the drug court (97.2 vs. 76.7). An obvious explanation for these seemingly dissimilar findings is that drug courts may be able to reduce the total quantity of time sentenced to custody across all cases specifically by reducing incarceration time on *future* (recidivist) cases, but not necessarily by reducing average sentence length on the initial case that brought the offender into the program.

Reconfirming the link between graduation status and sentencing outcomes, among drug court offenders who exited the program as of the 18-month survey and for whom official sentencing data were available ($N = 579$), the average number of days sentenced to jail or prison during 24 months was 6.6 for graduates and 104.8 for those failing. Indicating a similar disparity, average days sentenced on the precipitating case was 24.5 and 272.6 days, respectively ($N = 559$).

Trajectory of Re-Offending: Baseline to Six-Month to 18-Month Wave

Figure 4-4.1 compares the percentages of drug court and comparison offenders who reported engaging in at least one criminal act during the six-month period immediately preceding each survey wave (baseline, 6 months, and 18 months). As of the final 18-month wave, only 28 percent of the drug court sample was still actively enrolled in the program. Hence, we were interested in whether differences in criminal behavior that were detected after six months, representing an in-program timeframe, persisted at the 18-month mark, after most drug court offenders had reached post-program status. We found that at baseline, reflecting the impact of our weighting strategy, there were no significant differences between the samples (75 percent of both samples reported criminal behavior). However, results were significantly different and the effect size remained the same (a difference of exactly 12 percentage points) during both follow-up timeframes.

Figure 4-4.1. Criminal Activity in Prior Six Months: Baseline vs. Six-Month vs. Eighteen-Month Surveys

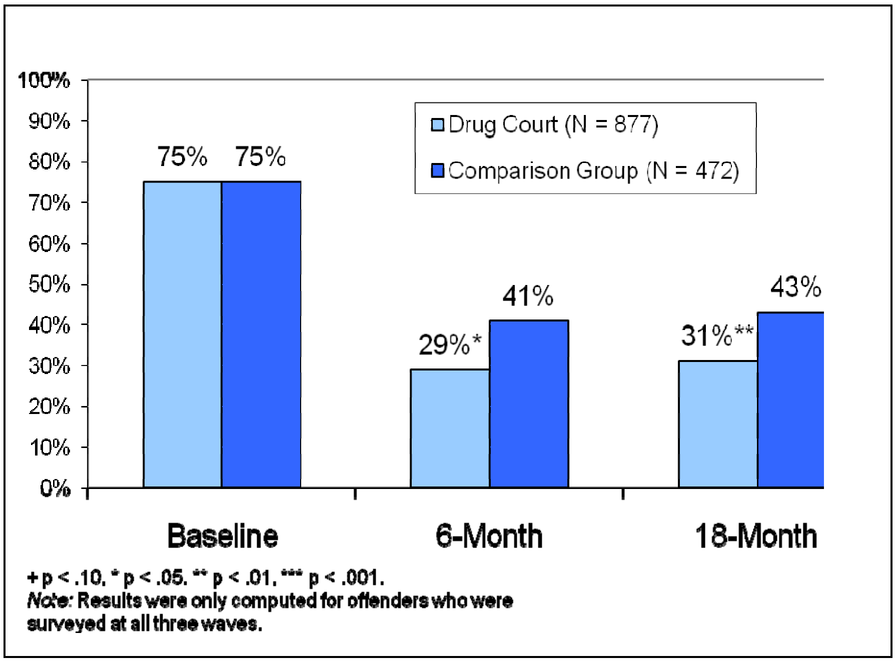
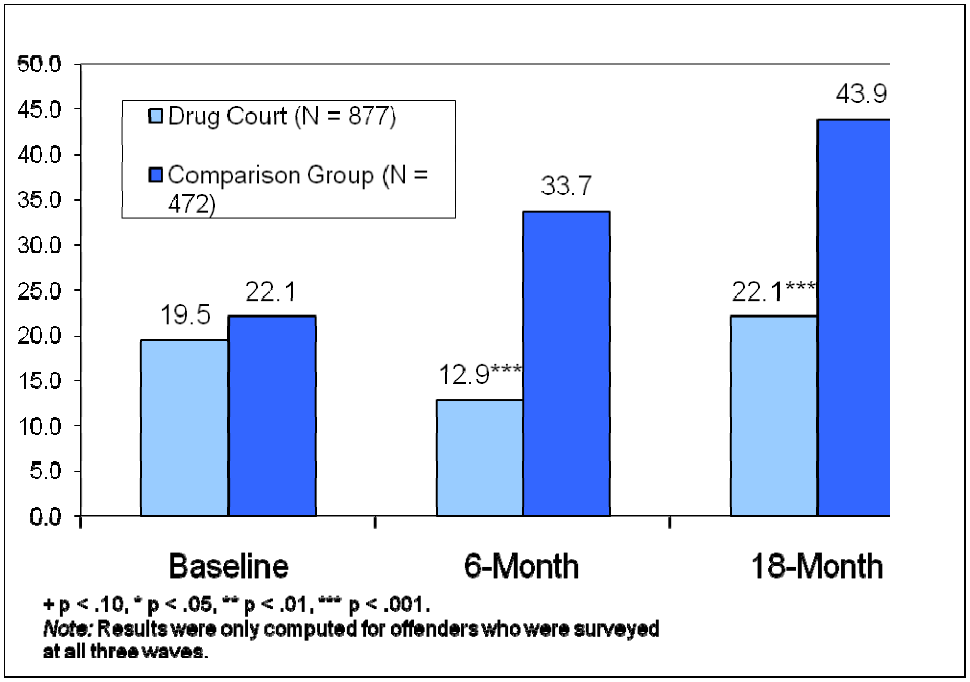


Figure 4-4.2 displays outcomes for the same three tracking periods, but indicates the total number of criminal acts, rather than the percentage who engaged in at least one. We found that at baseline, the samples did not significantly differ, whereas by the six-month wave, drug court offenders engaged in fewer than half as many criminal acts as the comparison group ($p < .001$). This gap between the samples remained approximately the same at the 18-month waves—but, interestingly, both samples showed a noticeable *increase* in criminal behavior by the time of the final follow-up wave. In the case of the drug court sample, the increase from the six-month to 18-month wave followed an initial decline from baseline to six months, whereas the comparison sample averaged steadily increasing amounts of criminal behavior across all three periods.

Figure 4-4.2. Number of Criminal Acts in Prior Six Months: Baseline vs. Six-Month vs. Eighteen-Month Surveys



Predictors of Future Criminal Behavior

Besides testing the impact of drug court participation, we also were interested in the degree to which other preexisting offender characteristics influenced their propensity for re-offending. Of further interest, we sought to examine the drug court model's presupposition that it is through treating the offender's underlying addiction to drugs that the model successfully decreases crime.

Table 4-4.4 reports the results of several Poisson regression models predicting the number of criminal acts in the year prior to the 18-month survey. Model 1 includes drug court status, along with a standard set of offender background characteristics. Model 2 examines the mediating role of ongoing drug use, adding a variable for the number of days of drug use per month in the year prior to the 18-month survey. As context for this last measure, we determined that drug court participation produced significantly fewer days of drug use per month than the comparison group (2.1 vs. 4.8 days in the year prior to the 18-month survey), as well as a significantly lower probability of using drugs (56 percent vs. 76 percent). Thus, we had already established that drug court participation reduced offender drug use (see details in Chapter 3); but the critical linkage between reduced drug use and reduced criminal behavior remained to be analyzed. Model 3 added a variable for the number of noncompliant acts with the terms of supervision in the year prior to the 18-month interview. (Supervision could include drug court supervision, probation, or perhaps other forms of supervision in the case of the comparison group.) Examples of noncompliance would be missed court hearings, missed days of treatment, and violations of program rules. The rationale for including this last measure is clarified below.

Table 4-4.4. Predictors of Criminal Activity at 18 Months and Re-Arrests at 24 Months

Dependent Variable	Number of Criminal Acts: Year Prior to 18-Month Survey		
	Model 1	Model 2	Model 3
Drug Court Participation			
Participant sample	-.880**	-.608+	-.670*
Demographics			
Age	-.019*	-.014*	-.013+
Male	.210+	.123	.079
Race/ethnicity[1]			
Black	.040	-.210	-.278*
Hispanic	-.136	-.424*	-.343+
Other Racial Group	.225	.195+	.187+
High school degree/GED	-.046	-.014	.064
Income (base 10 log of income)	-.006	.023	-.010
Social Ties			
Employed or in school	-.191+	-.336***	-.290**
Married	-.489**	-.322*	-.351**
Homeless (6 months pre-baseline)	.334*	.341**	.287**
Blood relatives involved with crime or drugs[2]	-.019	-.038	-.032
Drug Use and Treatment History			
Primary drug of choice[3]			
Alcohol	-.228	-.520**	-.424**
Marijuana	.196	.270	.354*
Cocaine (any form)	-.156	.149	.168
Average days of use/month (6 months pre-baseline)	.010*	-.012**	-.011*
Any residential treatment (6 months pre-baseline)	.099	-.123	-.095
Criminal History			
Number of criminal acts (6 months pre-baseline)	.005**	.005*	.004+
Mental Health			
Depression	.120	.079	.065
Anti-social personality disorder	.484***	.345***	.275**
Narcissistic personality disorder	.339**	.258*	.292**

(continued)

Table 4-4.4. Predictors of Criminal Activity at 18 Months and Re-Arrests at 24 Months (Cont'd)

Dependent Variable	Number of Criminal Acts: Year Prior to 18-Month Survey		
	Model 1	Model 2	Model 3
<u>18-Month Drug Use and Compliance Measures</u>[4]			
Days of drug use per month		.107***	.106***
Total number of supervision violations			.002**
Intercept	4.34***	3.948***	3.97***
Number of Cases	1473	1473	1455

Notes: The coefficients are simple regression coefficients, based on a Poisson specification (in HLM). For Model 5, test analyses revealed multiple significant random effects. Accordingly, that model treats the following variables as random effects: sex, race/ethnicity, high school degree/GED, employed or in school, homeless status, marital status, primary drug, days of treatment, and days of drug use at 18 months.

[1] White is the reference category. "Other racial group" includes Native-American, Alaskan Native, Asian, East Indian, Native Hawaiian, Pacific Islander, and non-Hispanic multiracial.

[2] The blood relatives measure represents an index of 10 questions, each coded on a 0-10 scale.

[3] The reference category includes primary drugs of heroin, amphetamines, prescription drugs, miscellaneous other drugs, and those who did not claim to have any particular primary drug. All of these categories combined total 30% of the sample.

[4] The two measures in this section were all taken from the 18-month survey and reflect activity during the preceding year: days of drug use per month and number of violations of supervision conditions respectively.

*+p<.10, *p<.05, **p<.01, ***p<.001.*

The results in Model 1 confirmed, once again, that drug court participation led to significantly less criminal behavior ($p < .001$). Consistent with nearly all recidivism research in other settings, we also found that younger offenders and those with more prior criminal activity re-offended significantly more often than did others. Also not surprising, those averaging more days of drug use at baseline—that is, those who had to overcome a more severe addiction problem—re-offended more than did others. Furthermore, baseline mental health status strongly predicted re-offending: those with anti-social and narcissistic personality disorders (each measured using multi-item inventories) re-offended significantly more than did others. Finally, having a greater "stake in conformity" (i.e., more to lose) appeared to serve as a deterrent to re-offending, as those who were married, not homeless, or who were employed or in school at baseline engaged in less re-offending (although the effect of employment/school status met only a .10 significance standard).

In Model 2, after adding a measure of offender drug use at follow-up, the effect of drug court participation noticeably weakened (coefficient from -.880 in Model 1 to -.608 in Model 2). At the same time, drug use, itself, exerted a powerful direct effect on re-offending ($p < .001$). In short, as hypothesized, the significant drug court impact in reducing criminal behavior was indeed largely mediated by the intervening drug court impact in reducing drug use.

However, one counter-explanation for the seemingly powerful linkage between reduced drug use and reduced criminal behavior is that both outcomes are, essentially, measures of compliance with drug court, probation, or other supervision requirements. In this view, it is perfectly logical that offenders who were more influenced to become compliant with drug court requirements in one way (by reducing their drug use) also were more likely to comply in another way (by reducing their criminal behavior). Hence, the existence of a strong and direct statistical association between the two outcomes might not mean that reduced drug use *brought about* the reduced criminal behavior. Rather, reduced drug use and criminal behavior might be better interpreted as parallel outcomes, both involving compliance. One way of examining this counter-explanation would be to include a variable that directly taps compliance. After including such a variable, one could then test whether compliance appeared to be the essential link explaining reduced criminal behavior, or whether reduced drug use truly exerted an independent effect. For this reason, Model 3 included a variable for the number of supervision violations in the year prior to the 18-month survey. As expected, more supervision violations predicted more criminality. Yet, drug use still exerted a strong, independent effect on criminal behavior as well. Therefore, the results in Model 3 provide additional reason to conclude that a causal linkage does exist between reduced drug use and crime.

For Whom Drug Courts Work

The purpose of this section is to examine whether the drug court intervention is particularly effective in reducing re-offending among certain categories of offenders, based on their prior drug use history, criminal history, mental health, or other characteristics. Since our analysis had detected a statistically significant and powerful overall drug court impact on the number of criminal acts in the year prior to the 18-month survey ($p < .001$), we used that same outcome measure to investigate the presence or absence of *differential effects* across different offender subgroups. In assessing the results (see Table 4-4.5), it is important to keep in mind that only the interaction terms answer this section's research question (significant interactions are in bold).

Overall, extremely few interaction terms were significant, broadly indicating that drug courts were comparably effective for all types of drug-involved offenders. Three exceptions were as follows. The drug courts were especially likely to produce a reduction in criminal behavior among offenders with a history of violence—indicated by a self-reported prior violent conviction ($p < .001$). On the other hand, drug courts were especially unlikely to produce a reduction in criminal behavior among offenders with narcissistic personality ($p < .05$) and among black offenders ($p < .05$). None of 14 other tested interactions was significant in either direction. As a follow-up analysis, we had contemplated developing a "risk" classification, enabling us to draw general conclusions as to whether drug courts work particularly well with high- or low-risk offenders. However, because so few of the interaction terms were significant in our initially parsimonious three-variable models—and of those that were significant, only prior violence was readily classifiable as connoting "risk," we considered it pointless to take that next step.

Table 4-4.5. Interaction Effects for Drug Court Participation and Select Baseline Characteristics on Criminal Behavior

Dependent Variable	Criminal Acts (Year Prior to 18-Month Survey) N=1,474
Sample	-.716***
Drug Use	
Sample	-.682**
Alcohol as Primary Drug	-.150
Sample X Alcohol	-.297
Sample	-.788***
Marijuana as Primary Drug	.369**
Sample x Marijuana	.290
Sample	-.966***
Cocaine as Primary Drug	-.320+
Sample X Cocaine	.054
Sample	-.602*
Average Days of Drug Use	.031***
Sample X Average Days of Drug Use	-.008
Criminality	
Sample	-.666***
Any Prior Conviction	.645***
Sample X Any Prior Conviction	-.319
Sample	-.614**
Any Prior Violent Conviction	.781***
Sample X Any Prior Violent Conviction	**-.779*** **
Sample	-.471
Number of Criminal Acts (6 Month Pre-Baseline)	.874***
Sample X Criminal Activities	-.278

(continued)

Table 4-4.5. Interaction Effects for Drug Court Participation and Select Baseline Characteristics on Criminal Behavior (Cont'd)

Dependent Variable	Criminal Acts (Year Prior to 18-Month Survey) N=1,474
Mental Health	
Sample	-.745**
Depression	.288**
Sample X Depression	.062
Sample	-.822**
Antisocial Personality Disorder	.796***
Sample X ASPD	.105
Sample	-1.14***
Narcissism	.297**
Sample X Narcissism	**.586***
Community Ties	
Sample	-.724***
Married	-.956***
Sample X Married	.135
Sample	-.472*
Employment or Education	-.326
Sample X Employment or School	-.325
Sample	-.674*
Blood Relatives' Involvement in Drug/Crime	.059
Sample X Family Criminal Involvement	-.012
Basic Demographics	
Sample	-.871***
Black Defendants	-.214
SampleX Black Defendants	**.519***

(continued)

Table 4-4.5. Interaction Effects for Drug Court Participation and Select Baseline Characteristics on Criminal Behavior (Cont'd)

Dependent Variable	Criminal Acts (Year Prior to 18-Month Survey) N=1,474
Sample	-.571+
White Defendants	.008
Sample X White Defendants	-.241
Sample	-.873***
Male Sex	.206
Sample X Male Sex	.204
Sample	-.354
Age of Defendant	-.030***
Sample X Age	-.012

Notes: Simple regression coefficients are displayed, based on a Poisson specification. The first indicated regression equation (top row in the table for "Sample") models the impact of drug court participation status on criminal behavior. All subsequent regression equations include different sets of three independent variables: (1) sample (drug court participant or comparison offender), (2) a given baseline characteristic, and (3) an interaction term (1*2).
+p<.10, * p<.05, ** p<.01, ***p<.001.

Conclusions

Given both the strengths and limitations in our design (see Chapter 2 and Appendix A for details), this section reviews the essential answers to our five research questions.

1. To what extent do adult drug courts reduce criminal behavior?

We found that at both follow-up survey waves, drug court participation produced significantly less criminal behavior than the comparison group. During the full 18-month tracking period, the sampled drug courts reduced the probability of re-offending by 23 percent relative to the comparison group (from 64 percent to 49 percent); and reduced the total number of criminal acts by 52 percent (from 110.1 to 52.5). In addition, the drug court impact on drug-related crime, including both drug possession and sales offenses, was even greater in magnitude than the impact on all crimes combined. Drug courts also significantly reduced driving while intoxicated and property-related crime. Significant effects were not apparent, however, on violent, weapons-related, or public order offenses, all of which had extremely low rates of prevalence in both samples.

We also found that drug court participation led to an apparent reduction in official re-arrests, although these results were not statistically significant. The 24-month re-arrest rate dropped from 62 percent to 52 percent, and the total number of re-arrests dropped from an average of 1.66 to 1.25. Our 10 percent effect size for re-arrest rate reduction is consistent with the national average reported in the Schaffer (2006) meta-analysis, although it is slightly smaller than what is reported in the overlapping Wilson et al. (2006) meta-analysis. The failure of this effect to reach statistical significance is somewhat troubling. However, there is already a sizable complementary literature that examines drug court impacts on official re-arrests. For this reason, we believe that our study's most critical contribution lies in the application of its rich, longitudinal dataset to measure the impacts on multiple forms of actual criminal behavior, regardless of whether it is officially detected. (In fact, the results revealed truly large quantities of criminal behavior in both samples, underlining the extensive amount of criminal behavior that official data excludes.)

2. How durable are drug court effects beyond the period of active program participation?

Previous research indicates that for those who successfully graduate from adult drug courts, participation lasts an average of about 15 months, whereas those who fail typically drop out after somewhat less time (Rempel et al. 2003; also see Volume 2 of this report). In this study, the collection of criminal behavior data at both 6- and 18-month waves enabled examining an early point in time, when nearly all drug court offenders were actively enrolled, as well as a later point, when most such offenders would have been out of the program for at least 3 months. In fact, only 28 percent of the drug court sample was still actively enrolled at the 18-month mark. When comparing criminal behavior outcomes across our two follow-up waves, the evidence indicates a nearly identical gap between the drug court and comparison samples—although notably, both samples averaged somewhat more crimes in the second than in the first follow-up timeframe. These results suggest that the drug court impact on criminal behavior is durable. Yet, given that 18 months is still a relatively short period, we do not know whether the gap in re-offending between the samples would have persisted or attenuated after even longer measurement periods.

3. Is the impact of drug courts mediated by reductions in drug use?

Adult drug courts are predicated on a theory of change, whereby they use their legal authority to coerce offenders into the treatment they need, thus addressing the debilitating addiction that underlies their criminality. The results confirm that the strong positive effect of the drug court in reducing *drug use* played a powerful intervening role in reducing subsequent criminality. In short, our data appeared to confirm the fundamental nexus between drug use and crime. (Given the strength of the widely theorized linkage between drug use and crime, it should be noted one could assess the same results as pointing to a slightly different interpretation: reduced drug use led to reduced crime, but multiple additional factors continued, as well, to contribute to crime reduction. In short, the propensity for ongoing criminal behavior is not exclusively reducible to drug involvement, even if the two factors are strongly associated.)

4. For which categories of offenders is the drug court intervention more or less effective?

This study provided little evidence that specific categories of offenders benefit more or less than other categories from the drug court intervention. Although offenders with a violent history saw a relatively greater reduction in criminal behavior stemming from drug court participation, and narcissistic and black offenders experienced a somewhat lesser impact, the magnitude of the drug court impact did not significantly vary across each of 14 other offender subgroups. Since other studies have not provided a preexisting body of evidence regarding "for whom drug courts work," the remarkably few significant findings that we did obtain are insufficient to merit new limits on drug court eligibility or strong conclusions that can be reliably generalized. The one potentially notable policy implication is that the *positive* effect of a violent history on offender responsiveness would seem at least to suggest that the preexisting restriction of many drug courts to nonviolent offenders does *not* necessarily represent an evidence-based practice. However, the broader finding that emerges from this study is that drug courts do not demonstrate clearly greater or lesser effects across different sub-categories; hence, efforts to limit drug court eligibility to narrow offender sub-populations may be counter-productive, restricting the opportunity to participate from populations that might otherwise benefit.

5. Do adult drug courts provide a true "alternative to incarceration"?

The results suggest that drug courts reduce incarceration time over the long-term, but not necessarily on the initial case that precipitated drug court participation. During the 18-month survey tracking period, drug court offenders seemed to spend fewer days incarcerated than the comparison group (62.7 vs. 95.3 days), although this effect was not significant. For the longer, 24-month administrative data tracking period, the gap between drug court and comparison offenders was greater (32.1 vs. 59.4 days), and these results did reach significance. While these two results are encouraging, they contrast to those we obtained when isolating the sentence on the precipitating criminal case that led to drug court or comparison group membership. On that one case, there was not a significant difference in the probability of a custodial sentence (22 percent for both samples) or in its average length, and the raw data pointed to a slightly higher average length among those in the drug court (97.2 vs. 76.7 days). Given the mixed findings obtained from the few previous studies on this question, it now appears doubtful that drug courts produce a consistent reduction in incarceration on the precipitating case. A more qualified set of findings is that drug courts nearly eliminate custodial time among those who graduate, but those benefits are counterbalanced by the high sentences imposed on those who fail the program. In short, the ultimate, long-term reductions in incarceration that drug courts produce stem largely from the reductions they produce in re-offending, which in turn leads to less incarceration on *future* cases.

Other Notable Findings

Besides the impact of drug court participation, we found that several preexisting offender characteristics also predicted re-offending. In particular, more prior criminal behavior, a greater frequency of drug use at baseline, and a younger age all significantly predicted future criminal behavior. In addition, having a greater "stake in conformity" (e.g., through marriage, employment, or a stable living situation) served as a deterrent to re-offending. Finally, personality disorders of anti-social personality and narcissism both predicted increased re-offending, whereas self-reporting positively for depression at baseline had no effect. (Classifications for all three disorders were based on multi-item inventories administered as part of the baseline survey.) Depression is a far more treatable and less static condition than the two personality disorders. Hence, depression, itself, may change or subside over time, perhaps mitigating the importance of whether or not an offender screens as depressed initially, upon program entry. In interpreting these findings, it is important to restate that whereas some categories of offenders are at an inherently greater risk of re-offense than are others, as described under point #5 above, nearly all categories of offenders benefit about evenly from drug court participation when comparing their outcomes to what they would have been in the absence of drug court.

As has been the case in most previous studies, our results suggest that drug courts reduce official re-arrests. Yet, given the paucity of prior research in these areas, we view our more important contributions to concern impacts on the much more prolific outcome of unofficial criminal behavior, as well as time spent incarcerated. Our criminal behavior results point to an extensive level of criminal activity among those in both the drug court and comparison samples, dispelling the notion that drug courts can realistically serve as a panacea. Yet, for both criminal behavior in general and drug-related criminal behavior in particular, we found that drug court participation reduced the number of criminal acts during an 18-month tracking period by more than half, a truly remarkable effect size. Lending support to the underlying theory of change that gave rise to drug courts, we also found that reductions in drug use are directly connected to reductions in criminal behavior (although the former reductions did not fully explain the latter, signifying that drug use and criminal behavior outcomes are not quite perfectly intertwined).

Our results also suggest that drug courts reduce the time that their participants spend incarcerated. However, we did not find that drug courts reduce incarceration on the initial case that led to drug court or comparison group membership—mainly due to the lengthy sentences that drug courts impose on those who fail the program. In short, on the initial case before them, drug courts do not operate as reliable “alternatives to incarceration,” but primarily because they reduce future re-offending, they do produce fewer future cases on which lengthy periods of incarceration are possible.

Interestingly, we attempted to isolate whether the drug court intervention is especially effective with certain offender subgroups and did not generally find this to be the case. The significant reductions in criminal behavior that we detected apply broadly across nearly all categories of offenders examined, suggesting that any efforts to restrict program eligibility may be misguided. Rather than highlighting a need for selective targeting, our results strongly support increasing the numbers of offenders who can enroll. In this regard, we know from other research that only a fraction of drug-involved offenders currently participates in adult drug courts (Bhati, Roman, and Chalfin 2008). For the intervention to have a truly systemic effect on drug-related crime, expanding drug courts, or comparable programs, to far greater numbers of offenders is perhaps the most pressing policy imperative to emerge from the latest drug court research.

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Chapter 5. Beyond Crime and Drug Use: Do Adult Drug Courts Produce Other Psychosocial Benefits?

Mia Green and Michael Rempel

This chapter examines the impact of adult drug courts on a series of largely understudied outcomes such as socioeconomic gains in employment, education, or income; mental and physical health; family support and conflict; and homelessness. The consistent effects of adult drug courts in reducing recidivism have been well documented (Finigan, Carey, and Cox 2007; Goldkamp, White, and Robinson 2001; Gottfredson, Kearley, et al. 2006; Government Accountability Office 2005; Rempel, Fox-Kralstein, et al. 2003; Roman and DeStefano 2004; Schaffer 2006; Wilson, Mitchell, and MacKenzie 2006). Yet, more than 20 years after the first drug court opened in 1989, our knowledge is virtually non-existent as to whether these specialized courts improve other psychosocial or health outcomes, either during or after program participation. Among the few previous efforts to explore whether such benefits arise, the results are mixed (Cosden, Peerson, and Orliss 2000; Gottfredson, Kearley, et al. 2005; Harrell, Roman, and Sack 2001; Harrell, Cavanagh, and Roman 1998).

This represents an unfortunate limitation in our knowledge, especially considering that individuals who enter the criminal justice system due to a drug dependency often experience other personal difficulties related to their addiction. These can include problems in maintaining stable employment, school attendance, and housing; in fostering healthy relationships; and in maintaining emotional health (Wolf and Coyler 2001). Even if drug courts are primarily designed to target substance abuse and related criminal behavior (e.g., see Office of Justice Programs and National Association of Drug Court Professionals 1997), their potential ancillary benefits in alleviating these other problems merit investigation.

Design and Methodology

The findings reported here are based on individual responses to the three waves of offender surveys conducted for the MADCE research: the baseline sample included 1,156 drug court participants and 625 comparison offenders; follow-up surveys were successfully conducted with 1,533 offenders (86 percent) at 6 months, 1,474 (83 percent) at 18 months, and 1,349 (76 percent) at both follow-up periods. As previously indicated, essential features of the MADCE study design and methodology are found in Volume 1 (Chapter 3) and this Volume (Chapter 2 and Appendix A).

Outcome Measures

For this analysis, four categories of outcome variables were included:

- *Socioeconomic Status*: Measures included current employment and school enrollment status, weeks worked since last interview, and current annual income. In addition to overall income, separate measures were constructed totaling annual income that respectively came from employment, friends and family, disability or other government sources, illegal activities, and other sources. A final series of measures included survey answers concerning whether offenders perceived that they wanted or needed the following types of services: employment services, educational services (e.g., related to GED classes or adult education), financial assistance, or public financial assistance (e.g., related to public disability or welfare).

- *Mental and Physical Health Status*: There were two mental health items. The first was a single question with a scale of five possible response options, “In general, would you say your current emotional or mental health is excellent, very good, good, fair, or poor?” The second was the result of a 10-item depression inventory designed to tap feelings and behaviors during the previous week and, after scoring, to yield a dichotomous outcome for whether the offender was depressed. Regarding physical health, responses were examined on single question, “Do you have any chronic medical problems which continue to interfere in your life?” Finally, we analyzed results on two questions respectively concerning whether or not the offenders believed they needed public healthcare services (e.g., Medicaid or Medicare), and whether they received such services since their previous survey. These responses were considered on the theory that those offenders whose healthcare and insurance needs appeared to have been met would be more likely to seek and receive the healthcare they needed going forward.

- *Family Support and Conflict*: The family conflict measures included a single item concerning the number of conflicts with family members during the previous 30 days; and the mean response to three statements, each scored on a five-point Likert scale, regarding the degree of family conflict (Cronbach’s alpha = .700 on the 18-month interview). Family emotional support included the mean response to 10 statements (on a five-point scale) regarding the strength of the offenders’ relationships with and support from family members during the previous 30 days (Cronbach’s alpha = .899, 18-month interview). Family instrumental support included the mean response to five statements (on a five-point scale) regarding expectations that family members would provide tangible assistance if needed, such as a job, financial support, or a place to live (Cronbach’s alpha = .898, 18-month interview).

- *Homelessness*: One measure tapped whether the offender had been homeless since the last interview; and a second measure concerned whether, for the same period, the offender had wanted or needed “help with finding or keeping a place to live.”

In each case, the equivalent measures were analyzed at both the 6- and 18-month marks.

Analytic Plan

In testing the impact of drug courts, we calculated a series of simple outcomes at 6 and 18 months for the drug court and comparison samples, using weighted data and hierarchical modeling techniques, as described in Chapter 2 and Appendix A. The next step of analysis was to examine which additional baseline characteristics influenced our outcomes of interest. For this purpose, we selected three core 18-month outcomes: whether the offender (1) was employed or in school, (2) was classified as depressed based on the 10-item inventory, and (3) experienced any family conflict in the previous 30 days. As independent predictors, besides drug court participation, we included measures tapping basic demographics (e.g., age, gender, and race/ethnicity), offender stake in conformity (e.g., baseline employment, education, income, and marital status), frequency of drug use at baseline, primary drug of choice, prior treatment history, prior criminal offending, and mental health status at baseline.

In additional multivariate models, we sought to test the direct linkage between reductions in drug use and reductions in other psychosocial problems. To do this, we included as a predictor variable the number of days of drug use per month in the year prior to the 18-month follow-up survey. Our hypothesis was that less drug use at follow up would predict other positive outcomes. To test the effects of specific community-based services received, we also included as predictors the number of days of substance abuse treatment received in the year prior to the 18-month survey and the number of ancillary services received in each of three separate categories: (1) employment and education, (2) life and interpersonal skills, and (3) financial-related services. Our hypotheses included logical relationships such as receiving more employment or education services would increase the likelihood that the offender had become employed or was attending school, and receiving more services related to life or interpersonal skills would reduce the likelihood of family conflict.

Results

Impact of Drug Court Participation

Table 4-5.1 compares the drug court and comparison samples on all outcomes of interest. Results are included for both the 6-month survey, when nearly all (88 percent) of the sampled drug court offenders were actively enrolled in the program, and for the 18-month survey, capturing a post-program period for most drug court offenders (only 28 percent still enrolled).

Socioeconomic Outcomes

The drug court sample was better off than the comparison sample on 23 of 28 individual socioeconomic measures examined, although few differences were statistically significant. We found that drug court participants were significantly more likely than were comparison members to be enrolled in school at six months (16 percent vs. 8 percent, p<.001). In addition, at 18 months, fewer drug court offenders reported a need for employment services (27 percent vs. 42

percent, p < .10), educational services (25 percent vs. 36 percent, p < .05), and financial assistance (28 percent vs. 44 percent, p < .05), suggesting that such needs were more likely to have been met among those enrolled in the drug court. The results also suggest that drug court offenders were slightly more likely to be employed or in school at 18 months (66 percent vs. 60 percent), and averaged a higher annual income, but these latter differences were modest in magnitude and not statistically significant.

Table 4-5.1. Impact of Adult Drug Courts on Psychosocial Outcomes

Outcome Measure	Drug Court	Comparison Group
6-Month Outcomes	**N = 1,009**	**N = 524**
18-Month Outcomes	**N = 951**	**N = 523**
Socioeconomic Status:		
6-Month Outcomes		
Currently in School or Employed	56%	55%
Currently in School	16%***	8%
Currently Employed	52%	48%
Weeks worked since baseline	11.0	10.0
Annual Income	$12,933	$11,495
From employment	$8,877	$8,132
From friends and family	$1,912	$1,229
From disability or other government sources	$1,498+	$982
From illegal activities	$300	$634
From other sources	$399	$467
Services wanted or needed		
Employment services	55%	66%
Educational services (e.g., GED or adult education)	47%	56%
Financial assistance (e.g., loans or housing deposits)	45%	56%
Public financial assistance (e.g., disability or welfare)	51%	56%
18-Month Outcomes		
Currently in School or Employed	66%	60%
Currently in School	11%	10%
Current Employed	61%	55%
Weeks worked since six-month interview	26.0	30.3
Annual Income	$17,172	$14,304
From employment	$12,746	$10,532
From friends and family	$1,712	$2,159
From disability or other government sources	$1,394+	$945

(continued)

Table 4-5.1. Impact of Adult Drug Courts on Psychosocial Outcomes (Cont'd)

Outcome Measure	Drug Court	Comparison Group
6-Month Outcomes	**N = 1,009**	**N = 524**
18-Month Outcomes	**N = 951**	**N = 523**
From illegal activities	$320	$320
From other sources	$945+	$462
Services wanted or needed		
Employment services	27%+	42%
Educational services (e.g., GED or adult education)	25%*	36%
Financial assistance (e.g., loans or housing deposits)	28%*	44%
Public financial assistance (e.g., disability or welfare)	31%	42%
Mental and Physical Health:		
6-Month Outcomes		
Depressed (10 or more on 30-item instrument)	26%	28%
Current Emotional or Mental Health Status (5-point scale)	3.69*	3.48
Percent rating "very good" or "excellent" (4 or 5 on scale)	51%+	58%
Any chronic medical problems	25%	23%
Need for public healthcare assistance (e.g., Medicaid, Medicare)	43%	48%
Received public healthcare assistance in previous 30 days	73%**	46%
18-Month Outcomes		
Depressed (10 or more on 30-item instrument)	27%	29%
Current Emotional or Mental Health Status (5-point scale)	3.63	3.66
Percent rating "very good" or "excellent" (4 or 5 on scale)	56%	58%
Any chronic medical problems	25%	24%
Need for public healthcare assistance (e.g., Medicaid, Medicare)	31%	42%
Received public healthcare assistance in previous 30 days	31%**	17%
Family Support and Conflict:		
6-Month Outcomes		
Any Serious Conflicts - since baseline	25%	27%
Number of conflicts - since baseline	0.70*	0.98
Family Conflict Index- mean of 3 statements	2.23+	2.43
Family Emotional Support Index - mean of 7 statements	4.32+	4.15
Family Instrumental Support Index - mean of 5 statements	4.11	3.98

(continued)

Table 4-5.1. Impact of Adult Drug Courts on Psychosocial Outcomes (Cont'd)

Outcome Measure	Drug Court	Comparison Group
6-Month Outcomes	**N = 1,009**	**N = 524**
18-Month Outcomes	**N = 951**	**N = 523**
18-Month Outcomes		
Any Serious Conflicts - since six-month interview	25%	30%
Number of serious conflict - since six-month interview	1.16	1.45
Family Conflict Index- mean of 3 statements	2.24*	2.44
Family Emotional Support Index - mean of 7 statements	4.27+	4.12
Family Instrumental Support Index - mean of 5 statements	4.04	3.96
Homelessness and Living Situation:		
6-Month Outcomes		
Any Homelessness Since Baseline	4%	3%
Wanted help in finding or keeping a place to live	30%	35%
18-Month Outcomes		
Any Homelessness Since Six-Month Interview	4%	4%
Wanted help in finding or keeping a place to live	27%	35%

Source: Urban Institute MADCE Substance-Abusing Offenders Survey
*+p<.10, *p<.05, **p<.01, ***p<.001*

Mental and Physical Health

Drug court and comparison offenders both reported that their mental health fell between "good" and "very good," with drug court offenders reporting a slightly, yet significantly, higher average rating at six months ($p < .05$), but with a significant difference not persisting at 18 months. Virtually identical percentages of offenders were classified as suffering from depression at both follow-up waves. The samples also did not differ on reports of medical problems at either wave. In the event of medical problems, however, the drug court sample was significantly more likely to have been set-up with public insurance (Medicaid or Medicare).

Family Conflict and Support

The results pointed to several significant differences in family conflict and emotional support. Focusing on the 18-month timeframe, drug court offenders averaged significantly less family conflict on the three-item index ($p < .05$); although on the single question eliciting the total number of family conflicts during the previous 30 days, the differences were not significant. Significant differences also were not apparent on any other measures in this domain, although drug court participants appeared to rate their family's emotional support slightly higher than did the comparison group (marginal significance, $p < .10$).

Homelessness and Living Situation

We found no differences in the rates of homelessness and in the average level of interest in receiving housing services between the drug court and comparison groups. These results remained stable between the 6- and 18-month marks.

Other Predictors of Psychosocial Outcomes

Analyses were performed to determine the effects of baseline characteristics on three key 18-month outcomes: (1) whether the offender was employed or in school, (2) depression (based on our multi-item inventory), and (3) family conflict. We also tested the impact of several "in-program" measures tapping the offender's ongoing use of drugs and receipt of treatment and other services.

The Impact of Offender Baseline Characteristics

As shown in Table 4-5.2, small numbers of baseline characteristics were statistically associated with each outcome measure. Among the relationships that spanned multiple outcomes, male offenders were less likely to self-report clinical depression, as well as less likely to report family conflict at 18 months. Offenders with high school degrees or GEDs were less likely to report depression, and more likely to be employed or in school. Suffering from depression, itself, when indicated in the original baseline survey, predicted ongoing depression at 18 months; and depression was also associated with a lower probability that the offender was employed or in school at 18 months.

The Impact of Ongoing Drug Use, Treatment, and Other Services

The results in Table 4-5.2, Model 2 (for all three outcomes) confirm the hypothesized linkage between an offender's drug problems and additional problems in the offender's life. Specifically, averaging more days of drug use during the year prior to the 18-month wave was consistently associated with worse outcomes: a *lower* probability of employment or school enrollment at 18 months and a *higher* probability that the offender suffered from depression and family conflict.

As for the direct effects of substance abuse *treatment* on other psychosocial outcomes, more days in a treatment program were associated with an increased likelihood that the offender was employed or in school at 18 months; however, treatment dosage was unrelated to depression or family conflict. The small positive effect of treatment on employment and school status may reflect the presence within many treatment programs of employment specialists or related services. Regarding other findings of note, offenders who directly reported that they received employment or educational services, not surprisingly, were significantly more likely than others

Table 4-5.2. Predictors of Select Psychosocial Outcomes at 18 Months

	Employed or Attending School		Classified as Depressed		Any Family Conflict (prior 30 days)	
	Model 1	**Model 2**	**Model 1**	**Model 2**	**Model 1**	**Model 2**
Drug Court Participation						
Participant sample	1.01	1.27	.994	.962	.991**	1.26
Demographics						
Age	.992	.998	1.01	1.01	.992+	.987**
Male	1.29+	1.28	.994*	.693*	.900*	.889
Race/ethnicity[1]						
Black	.710+	.708	.964	.970	.692	.716*
Hispanic	2.02**	2.03**	.884	.874	.860	.851
Other racial group	.763	.780	1.16	1.14	.455**	.435***
High school degree or GED	1.47**	1.47**	.722*	.728*	1.22	1.21
Income (base 10 log of income)	.947	.967	.952	.937	1.02	1.01
Social Ties						
Employed or enrolled in school	3.62***	3.48***	.779	.797	.831	.857
Married	.827	.802+	.877	.901	1.59**	1.68**
Homeless (any time in 6 months pre-baseline)	.844	.824	1.42*	1.43*	.807	.779
Blood relatives involved with crime or drugs[2]	1.01	1.00	1.04	1.05	1.09**	1.10**
Drug Use and Treatment History						
Primary drug of choice[3]						
Alcohol	.873	.956	1.09	.994	1.08	1.10
Marijuana	.876	.958	1.23	1.17	1.32+	1.28
Cocaine (any form)	.815+	.813*	.894	.891	1.58***	1.60***
Average days of use/month (6 months pre-baseline)	1.00	1.00	1.00	1.00	.997	.993
Any residential treatment (6 months pre-baseline)	1.17	1.21	1.14	1.08	1.10	1.05
Criminal History						
Number of criminal acts (6 months pre-baseline)	1.01+	1.00+	.995	.995	1.00	1.00

(continued)

Table 4-5.2. Predictors of Select Psychosocial Outcomes at 18 Months (Cont'd)

	Employed or Attending School		Classified as Depressed		Any Family Conflict (prior 30 days)	
	Model 1	Model 2	Model 1	Model 2	Model 1	Model 2
Mental Health						
Depression	.713**	.728**	2.89***	2.87***	1.33+	1.30+
Anti-social personality disorder	.855	.868	1.13	1.07	1.51***	1.39**
Narcissistic personality disorder	1.11	1.12	1.30**	1.29*	1.37+	1.32+
18-Month Drug Use and Treatment/Services						
Average days of drug use/month (over prior year)		.965*		1.05***		1.03***
Days of substance abuse treatment (over prior year)		1.00*[7]		1.00		1.00
# Employment and educational services (past 30 days)[4]		1.32***		.818+		.858
# Life and interpersonal skills services (past 30 days)[5]		.959		1.07		1.12
# Financial-related services (past 30 days)[6]		.802**		1.11		1.03
Number of Cases	1448	1445	1451	1445	1448	1444

Source: Urban Institute MADCE Substance-Abusing Offenders Survey

Note: Coefficients are logistic regression odds ratios (computed in HLM).

[1] White is the reference category. "Other racial group" includes Native-American, Alaskan Native, Asian, East Indian, Native Hawaiian, Pacific Islander, and non-Hispanic multi-racial.

[2] The blood relatives measure represents an index of 10 questions, each coded on a 0-10 scale.

[3] The reference category includes primary drugs of heroin, amphetamines, prescription drugs, miscellaneous other drugs, and those who did not claim to have any particular primary drug. All of these categories combined total 30 percent of the sample.

[4] A combined measure coded 0, 1, 2, or 3 based on the number of services that were reportedly received of: employment services, obtaining documents for employment, and educational services.

[5] A combined measure coded 0, 1, 2, 3, or 4 based on the number of services that were reportedly received of: life skills, anger management, batterer intervention program, and family understanding of substance abuse.

[6] A combined measure coded 0, 1, 2, or 3 based on the number of services that were reportedly received of: financial assistance, public *assistance, and public healthcare assistance.*

[7] The exact odds ratio is 1.001154 (i.e., more treatment is associated with employment or school), which rounds to 1.00.

*+p<.10, *p<.05, **p<.01, ***p<.001*

to be employed or in school at 18 months. Conversely, those who reported receiving financial services were less likely to be employed or in school at 18 months. (Causality may be reversed on this last finding, with those unable to find gainful employment responding by seeking out and receiving financial services.)

Conclusions

Overall, we found some evidence that adult drug courts yield improved socioeconomic and family conflict-related outcomes. Across 28 individual socioeconomic measures examined, spanning employment status, school status, and annual income from a number of distinct sources, the results on 23 measures trended in favor of the drug court sample. However, the effect sizes were modest, and only three total differences were significant at the standard .05 level. We also found that drug court participation led to less family conflict and greater emotional support from family members, but these effects also were modest in magnitude and reached statistical significance on some, but not all specific measures. Finally, there was little evidence at all that adult drug courts led to improved mental or physical health or to less risk of homelessness, particularly at the final 18-month mark. (There was a significant effect on the self-reported quality of mental health at 6 months that disappeared by 18 months.)

Nonetheless, confirming the existence of a broad nexus between drug use and other psychosocial problems, we found that across the entire sample (both drug court and comparison offenders), higher levels of drug use in the year prior to the 18-month mark predicted worse outcomes across all domains examined. Such a finding provides at least indirect support for the idea that achieving reductions in drug use (e.g., through drug court participation or other mechanisms) could comprise a meaningful first step towards greater well-being in other areas. We also found that spending more days in a substance abuse treatment program had a specific positive effect on employment and school status at 18 months, perhaps due to the ancillary employment and training services that some treatment programs provide. In other notable relationships, we found that suffering from depression at baseline predicted a significantly greater incidence of depression at follow-up and, in addition, predicted significantly worse socioeconomic outcomes. Further, we found that female offenders were independently more likely to be depressed and to experience family conflict at follow-up.

Since the previous literature is so sparse, this study provides among the first real tests nationwide for how drug courts affect psychosocial problems other than drug use and criminal behavior. Our findings broadly echo the mixed but, on average, slightly positive effects of adult drug courts that were found across the few previous studies to examine similar outcomes. In short, our findings do not justify the conclusion that adult drug courts can bring about the wholesale rehabilitation of each offender's personal and psychological problems. Yet, adding to their well-documented effects on drug use and criminal behavior—the two problems that drug courts were explicitly designed to mitigate—it appears that drug courts may at least make modest differences in a select number of other areas, socioeconomic well-being and family conflict in particular.

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Chapter 6. How Do Drug Courts Work?

John K. Roman, Jennifer Yahner, and Janine M. Zweig

The general conclusion in the literature is that drug courts have a significant and negative (though modest) effect on in-program criminality that appears to persist, particularly for graduates, for some time after they leave the program (Government Accountability Office 2005; Shaffer 2006; Wilson, MacKenzie, and Mitchell 2006). Despite the breadth of prior work, little is known about how drug courts achieve that success (but see Goldkamp, White, and Robinson 2001; Gottfredson, Kearley, et al. 2007). This question is important not only for the drug court field, but for criminology and affiliated fields as well; the mechanisms employed in a drug court are not unique to that setting, and thus, lessons from drug court may have broad applicability.

A number of theories have been proposed—but not tested—to answer the question of *how* drug courts work. Conceptual models of the behavioral pathways engaged by the drug court generally hypothesize that the most critical link is between drug court practices, a change in participant attitudes and beliefs, and subsequent behavior modification (Butts, Roman, et al. 2004; Goldkamp, White, and Robinson 2001; Longshore, Turner, et al. 2001).

Generally, prior research has identified four theoretical mechanisms that promote desistance. One theory, drawn from the economic and criminological literature, suggests that the threat of sanctions, including incarceration, deters drug use and offending. Second, from the public health literature, there is the theory that increasing participant motivation to change (using the coercive elements of the criminal court) promotes desistance. A third theory, drawn from the psychological literature, posits that engaging drug-involved defendants in a holistic and transparent process that maximizes perceptions of equality, fairness, and justice (e.g., procedural justice) leads to desistance. In a similar vein, legal scholarship has identified participants' attitudes toward the judge—or their beliefs about the judges' competence, impartiality, and concern for their general well-being—as being critical to subsequent desistance, under the rubric of therapeutic jurisprudence (Hora, Schma, and Rosenthal 1999; Wexler and Winnick 1996). To that, we add a fifth theoretical mechanism, distributive justice, as measured by participants' perceptions of the justness of court outcomes.

This chapter reports the results of a multilevel structural equation model (MSEM) that empirically tests theoretical pathways to desistance from drug use and criminal behavior. A path model is proposed that delineates how drug-court practices change perceptions and attitudes, and how these changes subsequently affect drug use and crime. Proposed mediators include changes in court practices (e.g., court appearances, drug testing, and treatment) and psychological mediators, including perceived risk and reward (deterrence), perceived legitimacy (procedural justice), attitude toward the judge, and motivation to change one's behavior through substance abuse treatment.

We believe there is only one prior attempt to isolate the mechanisms through which drug court effects behavior: Gottfredson and colleagues (2007) used a randomized experiment in the

Baltimore, MD, drug court to inform a structural equation model of the mediational pathways through which drug court promoted desistance. That study tested two theoretical measures of drug court. A social control variable combined five questions regarding the respondents' perceptions of the risk of penalty from the judge and the probation officer, and the desire to show the judge, probation officer, and respondent's family that they could control their drug use. A procedural justice scale combined 13 items and included "measures of representation, consistency, impartiality, accuracy, correctability, and ethicality" (Gottfredson et al. 2007:17). Three dependent variables were regressed on these theoretical constructs, including two scales measuring the variety of crime committed and variety of drugs consumed, and the frequency of multiple drugs used.

We extend the work of Gottfredson et al. (2007) in several important ways. First, we examine a greater range of theoretical predictors of desistance, adding therapeutic jurisprudence, treatment motivation, and distributive justice measures to the deterrence and procedural justice measures included in the Gottfredson et al. (2007) study. Second, to account for heterogeneity in the theoretical mediators, we include multiple measures of deterrence (i.e., certainty and severity of response) and treatment motivation, and include many more items in each measure to expand the number and type of theoretical mechanisms through which the mediators may operate. Third, while we do not have the advantage of a randomized experiment, we do examine a larger number of courts (23 drug courts and 6 comparison courts) rather than just one. Fourth, we use interview data collected from study respondents contemporaneously at baseline and at 6 and 18 months after baseline, rather than data collected three-years retroactively, thus reducing the threat of bias from recall errors. Fifth, we model relationships among independent variables, mediators, and dependent variables in a temporally ordered multilevel path model. Through a multilevel approach, we distinguish the within- and between-court components of drug court participation's direct and indirect effects on drug use and criminal activity, yielding a more thorough understanding of these inherently hierarchical processes. Sixth, we collected expansive data on critical moderators, including, for instance, a 30-item antisocial personality disorder scale, to attempt to replicate the findings of previous researchers that court effects can vary depending on the criminal propensity of participants (e.g., see Festinger, Marlowe, et al. 2002; Marlowe, Festinger, and Lee 2003, 2004; and Marlowe, Festinger, et al., 2003). Finally, we test whether the pathways between drug court participation and subsequent outcomes vary by other factors, such as age, gender, prior drug use, and criminal history.

Research Questions

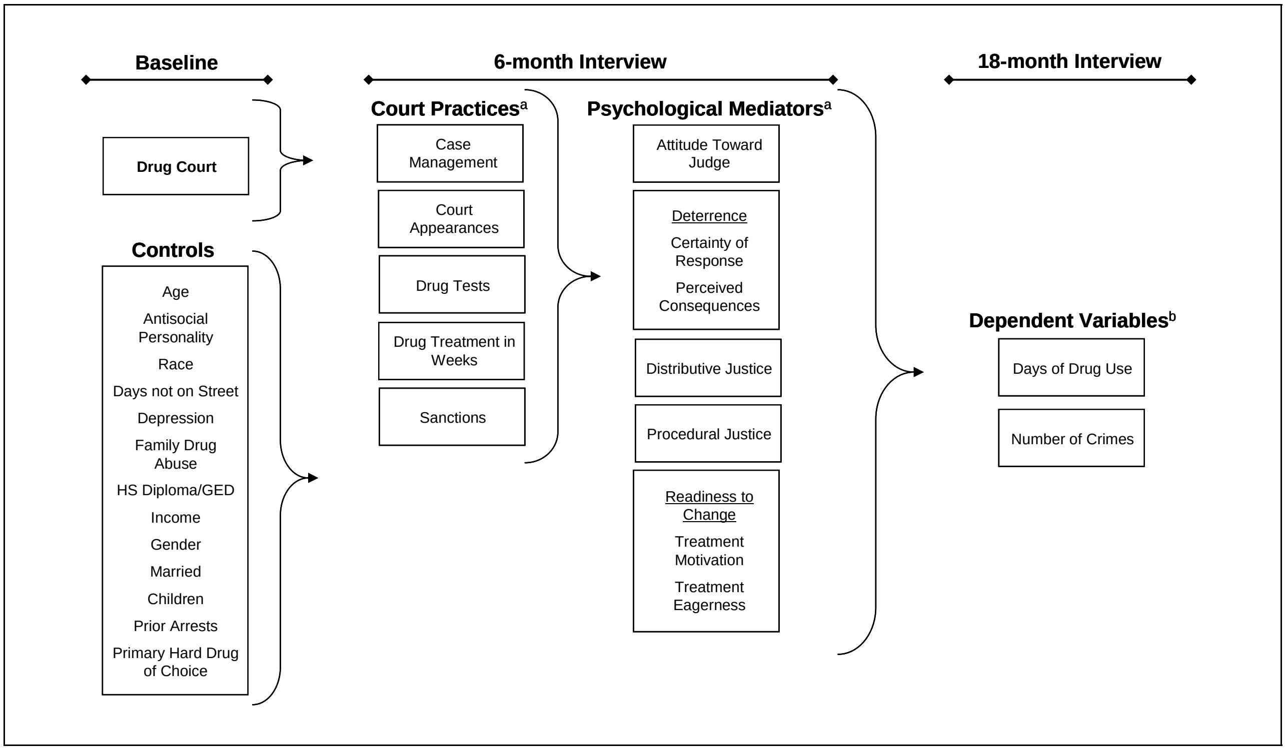
The following research questions guided our multilevel path analyses, as did the proposed model shown in Figure 4-6.1:

1. How do drug courts work to reduce drug use? Do they affect drug use indirectly through the court practices and psychological mediators we examine?

2. How do drug courts work to reduce crime? Do they affect criminal activity indirectly through the court practices and psychological mediators we examine?

3. Are the paths from drug court participation to subsequent outcomes (direct or indirect) moderated by other factors, such as depression, antisocial personality disorder, or age, gender, prior drug use, and criminal history?

Figure 4-6.1. Proposed Model of How Drug Courts Reduce Drug Use and Crime



Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=29 courts; N=1,349 respondents.

Notes: Results will be weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). Model will allow significant within-level covariances between exogenous variables, as well as significant between-level correlations between residual errors in equations predicting endogenous variables as needed.

[a] Outcomes will be modeled using negative binomial regression in Mplus 6.0 (Muthén and Muthén 1998-2010).

[b] Outcomes will be modeled using linear regression in Mplus.

* *p<.05* ** *p<.01* *** *p<.001*

Our first two questions represent a test of *mediation*, and following the findings from Gottfredson et al. (2007), we hypothesized that the relationship between drug court participation and subsequent outcomes will be at least partially mediated by court practices (in particular, by court appearances and drug treatment) and the psychological impacts of those practices (especially procedural justice). The third question represents a test of *moderation*, something that Gottfredson et al. (2007) did not examine, but which importantly relates to the generalizability of drug court effects across offenders of different ages, genders, and criminal propensities. Specifically, we examined moderation across groups defined by age, gender, number of prior arrests, prior hard drug use and frequent drug use, depression, and antisocial personality disorder.

Given that no study has explored this last question to date, we offer no *a priori* hypotheses other than the expectation of some variation across groups.

Design and Methodology

We rely on multilevel path analysis, a special case of structural equation modeling that employs a single variable or scale for each theoretical construct in the model. Through multilevel path analysis, we can (1) measure both individual change over time and differences between groups simultaneously (Willett and Sayer 1994); (2) specify both direct and indirect effects of independent variables on dependent variables of interest, that is, conduct tests for mediation (Vogt 1993); and (3) test for moderation of relationships between particular independent and dependent variables for both main effects and mediated effects.

Moderators, Mediators, and Moderated-Mediation

When testing complex theory, it is important to distinguish between moderators and mediators, and to examine the combined effect of moderated-mediation. *Moderators* are independent variables that alter the causal relationship between another independent variable and the dependent variable (Baron and Kenny 1986). The direction or intensity of a relationship between an independent variable and a dependent variable is changed based on the level of the moderator variable (e.g., a relationship may only exist between an independent variable and a dependent variable for certain groups or may vary in intensity for different groups).

Mediator variables signify the mechanism through which an independent variable affects a dependent variable (Baron and Kenny 1986), and function in path analyses as both independent and dependent variables. Mediation analysis identifies the underlying processes that trigger human behavior (MacKinnon and Fairchild 2009); thus, mediators are the mechanisms by which drug courts are effective. Often programs are designed to change such mechanisms with the goal of changing an outcome of interest. If a mediation process can be identified, interventions can be purposively designed to target that process and be much more efficacious (MacKinnon and Fairchild 2009).

Moderated–mediation occurs when the strength of the mediation or the nature of the mediated relationships varies based on a moderator variable (MacKinnon and Fairchild 2009). In particular, if the mediated relationship is heterogeneous across subgroups, we can document the generalizability of the relationship and identify the mechanism by which drug court effects vary across subgroups.

Thus, multilevel path analysis allows us to examine which theory, or combination of theories, mediate the relationship between drug court participation and desistance from crime and drug use. We test which of five theories mediate the relationship between drug court participation and desistance: deterrence, procedural justice, positive attitude toward the judge, treatment readiness for change, or distributive justice. Further, we test if receipt of intervention practices (i.e., number of drug tests, number of court appearances, etc.) or if individual characteristics (i.e., depression, features of antisocial personality disorder, or having family members with substance use issues) moderate the variables in the mediated relationships. Finally, we examine if the mediated relationships are moderated or hold across subgroups (i.e., age, gender, number of previous arrests, and primary drug of choice at baseline).

Measures

Dependent Variables

We selected as our dependent variables the two most important indicators of whether a drug court has achieved its stated objective of reducing drug-related criminal activity: drug use and crime. *Days of drug use per month* is the average number of days of drug use per month that respondents reported for the year between the 6- and 18-month interviews. Eight drugs are included: alcohol, marijuana, cocaine, heroin, hallucinogens, amphetamines, prescription drugs, and methadone. Nominal responses were re-coded into the following numeric values: *every day* = 30; *a few days per week but not daily* = 8.58; *a few days per month* = 3; *once per month* = 1, and *never* = 0. For each month, the highest rate of drug use reported for any individual drug was selected for use in calculations,[12] and the average across all months was then computed. *Number of crimes per month* is the average number of crimes per month that respondents reported committing in the year between the 6- and 18-month interviews. Respondents were asked how many times they committed each of the following six crime types: (1) violent crimes, including harassment; (2) carrying a weapon; (3) drug activity, including possession, sales, and other drug activity; (4) driving while intoxicated; (5) property crimes; and (6) public order crimes, including prostitution and vagrancy. Responses for all six crime types were then summed across the 12-month period, truncated to the 95th percentile value of 365 (which allows a maximum of one crime per day), and divided by 12 to compute the average number of crimes per month.

Court Practices

Potential court mediators were measured at the six-month interview, and include practices that generally distinguish drug courts from non-specialized courts. In other words, these are the elements of the criminal court system that drug courts purport to change.[13] *Case management contacts* are the number of times that respondents had either phone or face-to-face contact with their supervision officer in the last 30 days (coded as 0 for the 102 comparison court respondents who reported no supervision officer).[14] *Court appearances* are the number of times that respondents reporting having appeared in court since their baseline interview. *Drug tests* are the number of non-alcohol drug tests to which respondents said they were subjected in the previous six months. *Drug treatment in weeks* is the number of weeks during which respondents said they received any of the following types of drug treatment in the previous six months: drug treatment in a hospital (including emergency room visits and detoxification), residential treatment,

[12] This strategy may have underestimated cumulative use, but helped avoid double-counting days of use.
[13] Gottfredson et al. (2007) describe these as the drug court implementation variables.
[14] Because phone contact and face-to-face contact questions were asked separately, the combined measure represents the higher number of times indicated on either question, which may underestimate cumulative contact, but avoids double-counting days of contact.

medicinal interventions (e.g., methadone maintenance), outpatient group and/or individual counseling, and self-help groups such as alcoholics anonymous (AA) and narcotics anonymous (NA).[15] *Sanctions* are the number of sanctions that respondents reported receiving in the previous six months, including reprimands, warnings, written assignments, community service, increased drug tests, increased attendance at AA/NA meetings, increased amount or intensity of drug treatment, sitting in the jury box to observe court hearings, being kicked out of court, electronic monitoring, day reporting, house arrest, community control, spending nights in jail, and formal supervision violations.[16] All of these court practices were tested as predictors of drug use, crime, and the psychological mediators described below.

Psychological Mediators

Nine theoretical constructs were examined as possible psychological mediators of drug courts' effects on the dependent variables (see Appendix B for all scale reliabilities and items). All nine were self-reported by respondents six months after the baseline interview. We note that the psychological mediators describe perceptions and attitudes at the six-month interview, whereas the court practices variables measure events before the six-month interview, and thus the two domains are not contemporaneous. Each theoretical mechanism (except distributive justice) is measured using more than one scale.

We measure deterrence in three ways. The *deterrence score* (certainty * severity) represents the product of two scales that measure the certainty of a court response to, and perceived consequences of, respondents' drug use. Higher deterrence scores indicate greater perceived certainty and more perceived consequences of court responses to respondents' drug use. The deterrence scales were also tested separately in models as individual mediators. The *certainty of responses scale* ($\alpha = .82$) is the average of ten items indicating how likely respondents believed it would be that their judge or supervision officer would respond with a sanction, if the judge/officer thought the respondent was using drugs. Each of the ten items refers to one of the sanctions described previously in the *Court Practices* section (e.g., increased drug testing, community service, electronic monitoring, spending nights in jail). Each sanction item had possible response values ranging from 1 to 4, with 4 representing very likely. Thus, higher values of the scale represented greater certainty of response. Similarly, the *perceived consequences scale* ($\alpha = .84$) measures respondents' perceptions of the undesirability of the aforementioned sanctions. Scale scores represent the average of 12 items indicating how bad respondents believed it would be to experience the sanction. Possible responses ranged from 1 to 3, with 3 representing extremely bad; thus, higher values of the scale equate to greater perceived consequences or undesirability of sanctioning.

The *procedural justice scale* ($\alpha = .94$) is the average of 18 items measuring respondents' perceptions about their most recent courtroom proceedings. The included items, as follows, measured the degree to which respondents felt they had: the opportunity to express their views in

[15] The only treatments excluded from this measure are those described by respondents as "alternative methods."
[16] The measures of court appearances, drug tests, and sanctions were capped at their 95th percentile response to eliminate extreme outliers (i.e., values higher than the 95th percentile were re-coded to equal the 95th percentile response).

court; a fair chance to bring out the facts; enough control over the way things were run; been intimidated or scared to say what they felt (reverse-coded); been pushed around by people with more power (reverse-coded); people in court speak up on their behalf; the court take into account what respondent said in deciding the case; been pushed into things they did not agree with (reverse-coded); been treated the same way as others who committed the same offense; been disadvantaged in the court because of their age, income, sex, race, or other reason (reverse-coded); experienced the court getting the facts wrong (reverse-coded); been were able to get the facts corrected; been treated unfairly by the court or the police (reverse-coded); gotten their complaint about being treated unfairly heard; been treated politely by people in court; understood what was going on in court; understood what their rights were during processing of the case; and been treated with respect in the court. Responses to each item range from one to five, with five indicating *strongly agree*; thus, higher scale values equate to greater perceptions of procedural justice.[17] Notably, at the six-month interview, procedural justice questions were only asked of respondents who had made at least one court appearance after the baseline interview, which included 742 drug court participants (74%) and 95 comparison cases (18%). For those respondents who were not asked these questions at the six-month interview, their answers to the same set of procedural justice questions were taken from the baseline interview. Thus, this scale measures respondents' perceptions of procedural justice during their most recent courtroom proceedings, as of the six-month interview.

Our test of therapeutic jurisprudence involved the *attitude toward judge scale* ($\alpha = .90$), which is the average of nine items measuring the extent to which respondents agreed that their judge was knowledgeable about their case, knew them by name, helped them to succeed, emphasized the importance of drug and alcohol treatment, was intimidating or unapproachable (reverse-coded), remembered their situations and needs from hearing to hearing, gave them a chance to tell their side of the story, could be trusted to treat them fairly, and treated them with respect. Item responses range from one to five, with five representing *strongly agree*; thus, higher values equate to more positive attitudes toward the judge.

The *readiness to change score* is the average of two scales measuring the degree of drug treatment motivation and treatment eagerness, which were also tested separately. The *treatment motivation scale* ($\alpha = .77$) represents the average of the four subscales of the TCU Treatment Motivation scale (Knight, Holcom, and Simpson 1994), which measure the degree to which respondents recognize that they have a drug problem, desire help for that problem, are ready for treatment, and feel external pressure to participate in treatment. Responses to items range from 1 to 8, with higher values indicating greater treatment motivation. The *treatment eagerness scale* ($\alpha = .77$) represents the average of three subscales that measure the degree to which respondents recognize their drug problem, lack ambivalence toward treatment, and are taking steps toward receiving treatment. Responses range from 1 to 8, with higher values equating to greater treatment eagerness.

Finally, the *distributive justice indicator* is a single item measuring respondents' ratings of the fairness of the outcome they received in the case that brought them into the study. Responses range from one to four, with one indicating *very unfair* and four indicating *very fair*.

[17] Two were converted from a 4-point to 5-point scale.

Independent Variables

As shown in Table 4-6.1, we examined a number of independent variables as controls or moderators of the relationship between drug court participation and the dependent variables. All of the independent variables were measured at baseline, with the exception of days unavailable on street (described shortly), which captures the time the respondent was not in a custodial environment between the 6 and 18 months following the baseline interview. Variables that were tested as possible controls include several measures that prior research has shown to be related to drug use or criminal activity, including age, race/ethnicity, gender, education, income, marital status, presence of minor children, family drug abuse, primary drug of choice, and prior arrests. The controls also include *days unavailable on street*, which is a count of the days during which respondents were either incarcerated or hospitalized, as a measure of times during which respondents were at minimal risk of drug use and criminal behavior. The remaining independent variables listed in Table 4-6.1 were tested as either controls or moderators of drug courts' effect on outcomes, along with age, gender, prior arrests, and primary drug of choice. *Antisocial personality disorder* (ASPD) is coded as "1" if respondents' answers to 34 questions regarding childhood deviance and violence, frequent absences from work or school, failure to think about the negative consequences of one's behavior, and pervasive patterns of disregard and violation of the rights of others indicate signs of ASPD and "0" if not. Similarly, *depression* is coded as "1" if respondents indicated "yes" to ten or more items on a scale ranging from 0 to 30 that represented classic features of depression, including: feeling depressed, fearful, lonely, and unhappy, and having restless sleep (see Appendix B for a full listing of ASPD and depression items). *Frequent drug user* was coded as "1" if respondents reported using alcohol or drugs an average of 20 to 30 days per month for the 6 months prior to the baseline interview and "0" if not.

Descriptive statistics for all study variables, including the key theoretical constructs (e.g., psychological mediators) are presented in Table 4-6.1, with means and standard deviations. As noted above, the dependent variables were measured at 18 months, the court practices and psychological mediators at 6 months, and the independent variables were generally measured at baseline. As shown, there were a number of statistically significant ($p<.05$) differences between drug court and comparison court respondents with regard to the dependent variables, court practices, and psychological mediators, but there were no significant differences across the independent variables after controlling for baseline differences and attrition. As shown in Table 4-6.1, missing data were sufficiently limited that we could assume they were missing completely at random (Allison 2001). Thus, respondents with missing data on any covariates included in the estimated models were simply excluded, with model Ns ranging from a low of 1,211 to the full value of 1,349.

Drug court and comparison court respondents showed statistically significant bivariate differences (at $p<.001$) for all of the dependent variables, court practices, and potential psychological mediators measured. Eighteen months after their baseline interviews, drug court respondents reported significantly fewer days of drug use per month (2.38 compared to 4.40) and fewer crimes committed (3.20 compared to 5.36). During the six months following their baseline interviews, drug court respondents reported experiencing greater involvement in court proceedings than the comparisons, as measured by higher numbers of self-reported case

Table 4-6.1. Descriptive Statistics for Variables, by Drug Court Status

				Drug Court		Comparison Court	
	Percent Missing	Min	Max	Mean or %	Std Dev	Mean or %	Std Dev
Dependent Variables							
Days of drug use per month	0%	0	30	2.38 ***	5.05	4.40	7.13
Number of crimes per month	0%	0	30	3.20 ***	7.44	5.36	9.65
Court Practices							
Case management contacts	2%	0	30	11.10 ***	8.40	5.13	6.13
Court appearances	<1%	0	21	9.26 ***	6.01	1.45	2.88
Drug tests	3%	0	65	27.05 ***	19.18	4.81	7.62
Drug treatment in weeks	0%	0	26	13.22 ***	8.53	5.18	8.24
Sanctions	<1%	0	8	1.96 ***	2.53	0.74	1.65
Psychological Mediators							
Attitude toward judge scale	<1%	1	5	4.09 ***	0.70	3.22	0.81
Deterrence score	9%	2.3	12	7.10 ***	2.09	6.07	1.96
Certainty of response scale	9%	1	4	2.98 ***	0.64	2.70	0.77
Perceived consequences scale	<1%	1	3	2.38 ***	0.42	2.28	0.47
Distributive justice indicator	<1%	1	4	3.25 ***	0.97	2.87	1.04
Procedural justice scale	<1%	1	5	3.76 ***	0.77	3.22	0.76
Readiness to change score	<1%	1	8	3.16 ***	0.86	2.61	0.83
Treatment motivation scale	<1%	1	8	3.03 ***	0.86	2.37	0.78
Treatment eagerness scale	<1%	1	8	3.30 ***	0.95	2.84	0.98
Independent Variables							
Age	<1%	18	68	33.84	10.72	34.29	10.33
Antisocial personality disorder	0%	0	1	0.42	0.49	0.44	0.50
Black/African American	0%	0	1	0.32	0.47	0.35	0.48
Days unavailable on street	0%	0	365	64.89	104.41	72.73	111.19
Depression[a]	0%	0	1	0.39	0.49	0.37	0.48
Family drug abuse	0%	0	1	0.82	0.39	0.82	0.39
High school diploma or GED	0%	0	1	0.59	0.49	0.58	0.49
Income[b]	0%	$0	$180K	$10,510	$16,514	$11,526	$19,157
Male gender	0%	0	1	0.71	0.46	0.70	0.46
Married or in relationship	0%	0	1	0.51	0.50	0.54	0.50
Minor children (<18 years)	0%	0	1	0.49	0.50	0.48	0.50
Prior arrests	3%	0	25	7.55	8.02	8.23	8.03
Primary hard[c] drug of choice	0%	0	1	0.59	0.49	0.57	0.50
Frequent drug user[d]	0%	0	1	0.34	0.47	0.33	0.47
White/Caucasian	0%	0	1	0.56	0.50	0.54	0.50

Notes: N=1,349 respondents; 877 from drug courts and 472 from comparison courts. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009).

[a] Measured at baseline, the scale on which this measure is based indicated the number of depressive symptoms reported by respondents (ranging from 0 to 30); those characterized as "depressed" exhibited scores of 10 or more.

[b] For analysis purposes, this variable was transformed into its natural log to minimize skewness.

[c] Includes cocaine, heroin, amphetamines, and other drugs. Comparison group is alcohol and marijuana.

[d] Used alcohol/drugs an average of 20–30 days per month for the six months before the baseline interview.

** p<.05 ** p<.01 *** p<.001*

management contacts (11.10 compared to 5.13), court appearances (9.26 compared to 1.45), drug tests (27.05 compared to 4.81), drug treatment in weeks (13.22 compared to 5.18), and sanctions (1.96 compared to 0.74). At the six-month interview, drug court respondents reported significantly more positive attitudes toward the judge (4.09 compared to 3.22); greater perceptions of deterrence (7.10 compared 6.07), including a higher perceived certainty of response to drug use (2.98 compared to 2.70) and perceived consequences (2.38 compared to 2.28); greater perceptions of distributive justice (3.25 compared to 2.87) and procedural justice (3.76 compared to 3.22); and more readiness to change (3.16 compared to 2.61) as measured by a higher level of treatment motivation (3.03 compared to 2.37) and treatment eagerness (3.30 compared to 2.84) than comparison respondents.

Analytic Strategy

Multilevel Structural Equation Modeling (MSEM)

Our analytic strategy consisted of three steps, each of which involved multilevel structural equation modeling (MSEM) in the M*plus* 6.0 statistical software program (Muthén and Muthén 1998-2010). M*plus* is a flexible and revolutionary statistical modeling program that can estimate a variety of complex statistical models, at multiple levels, using both continuous and categorical latent and observed variables. Important to our research questions, MSEM in M*plus* allowed us to (1) simultaneously estimate multiple regression equations to assess the direct and indirect effects of drug court participation on each of the dependent variables,[18] while accounting for (2) the effects of court practices and psychological mediators, (3) the hierarchical clustering of respondents into 29 courts, (4) differentiation of between-courts effects from within-courts (individual-level) effects, and (5) possible moderation of effects by other factors, such as age, gender, and prior arrests. By using M*plus*, we also were able to account for the over-dispersed distribution of both dependent variables by modeling their prediction using negative binomial regression.[19]

Each construct in our MSEM was measured by a single variable and was therefore observed rather than latent, with the exception of the random intercepts representing court-level variation in the dependent variables. Drug court participation was the only variable measured at the between-courts level; all other variables were measured at the within-courts level. However, as specified clearly in Preacher, Zyphur, and Zhang (2010), any effects involving a between-groups variable must occur at the between-groups level. Therefore, in our models, all effects of drug court on other variables occurred at the between-courts, rather than within-courts level. The only effects that occurred at the within-courts level were those exclusively involving the within-courts variables.

[18] Models predicting drug use were estimated separately from models predicting criminal activity.
[19] Although both variables also had a fair number of zero responses, zero-inflated negative binomial estimation was not warranted according to tests specified in Erdman et al. (2008:5) (i.e., the zero-inflated intercepts were statistically significant and significantly negative).

The statistical significance of model parameters was assessed by their p-values, with values less than .05 indicating statistical significance. All models were run using maximum likelihood estimation with a sandwich estimator to compute robust standard errors, which accounted for the non-normality of outcomes and the non-independence of observations due to cluster sampling (Muthén and Muthén, 1998-2010). Measures of model fit for negative binomial path models are currently unavailable in M*plus*; however, observed fit indices for the same models run using linear regression were acceptable, based on the criteria used by Gottfredson et al. (2007) (e.g., comparative fit indices were 0.9 or more; root mean square errors of approximation were less than 0.05).

To test moderation of drug courts' effects on other variables (our third research question), we used chi-square difference testing of nested models (Muthén and Muthén 2005, Satorra and Bentler 1999).[20] The nested models that we compared consisted of (1) a model restricting drug courts' effects on mediators and dependent variables to be equivalent across groups defined by factors such as age (young versus old) and gender (male versus female), and (2) another unrestricted model that permitted drug courts' direct and indirect effects on the dependent variables to vary across groups. By comparing the chi-square values obtained from each of these models, we were able to determine where allowing differentiation by group significantly improved the fit of the model to the data—in other words, where drug courts' effects on outcomes were moderated by age, gender, and so forth.

Controls for Comparability and Attrition Bias

Inverse probability weights (IPW) were used in all analyses to adjust for comparability between drug court and comparison court respondents at baseline, as well as the relatively small amount of attrition[21] following the baseline interview (Rempel and Green 2009). Increasingly popular among economists and statisticians, IPW methods provide an intuitive approach to correcting for non-representation by weighting sample members by the inverse probability of their being selected (Hirano, Imbens, and Ridder 2003; Wooldridge 2002). The IPWs performed well in adjusting the equivalence of respondents at baseline and after attrition, as demonstrated in Table 4-6.1 by the nonsignificant differences across the independent variables (also, see Rempel and Green, 2009).

Building the Final Models

Given the large number of variables we wanted to examine, we adopted an approach similar to that of Gottfredson et al. (2007) by building our final MSEMs in the stages described below. This approach—which aims for parsimony in the final models—is especially relevant in the context of multilevel modeling, where the identification of parameter estimates can be unstable when the number of parameters exceeds the number of hierarchical clusters (in this case, 29

[20] For tests of moderation, effects on both dependent variables were estimated using linear, rather than negative binomial regression due to insufficient memory space for the complex algorithmic integration required. However, results from the linear regressions were substantively similar to those obtained using negative binomial regression and only the chi-square values associated with restricted and unrestricted models are presented in the results.
[21] Recall that 76 percent of those interviewed at baseline completed both the 6- and 18-month interviews, with identical attrition rates between the drug and comparison court samples (each was 76 percent).

courts). For this reason, models predicting drug use were conducted separately from those predicting criminal activity. The final models estimated approximately 37 parameters, which represents only those effects that were statistically significant in both the interim and final models—and notably, the direction of all effects remained consistent across models. Thus, we feel confident in our final models' parameter estimates given the relatively small discrepancy between their number and the number of court clusters (37 compared to 29), and personal reassurance from one of M*plus*' creators, Linda Muthén (personal communication, October 11, 2010).

The questions below outline the interim models (A through H) that we estimated, in the process of building our final models (I).

Model A) Does drug court participation predict the dependent variable?

Model B) What other independent variables (controls) predict the dependent variable?

Model C) Does drug court participation predict the dependent variable, conditioned on the controls?

Model D) What psychological mediators predict the dependent variable?

Model E) What court practices predict the dependent variable?

Model F) What court practices predict the psychological mediators from Model D?

Model G) Does drug court participation predict the psychological mediators identified in Model D?

Model H) Does drug court participation predict the court practices in Models E or F?

Model I) Allowing all significant paths from A through H above, through which mediators does drug court participation predict the dependent variable?

For reasons just described, we retained only statistically significant ($p<.05$) parameters from each interim model for estimation in subsequent stages. In the next section, we present results from interim Models A through H and final Models I.

Results

Tables 4-6.2 through 4-6.5 and Figures 4-6.2 and 4-6.3 show results from the key interim Models A-H estimated, as well as the final MSEMs (Model I). Results are discussed separately for each research question.

How Do Drug Courts Work to Reduce Drug Use?

Results from Interim Models A through H

Multilevel Model A confirms the bivariate finding that drug court participation significantly predicted fewer days of drug use per month (at 18 months), even after accounting for the clustering of individuals into courts (between-courts beta[22] was -0.831; its p-value < .001; the intercept = 1.428). Estimation of Model B (Table 4-6.2) finds four independent variables that significantly affected drug use when all were examined simultaneously—age, antisocial personality disorder, prior arrests, and (approaching significance at .058) primary hard drug of choice. Thus, offenders who were younger, showed features of ASPD, had higher numbers of prior arrests, and who preferred alcohol or marijuana (rather than harder drugs) reported *greater* drug use in the 18-month interview. In Model C, we re-estimated the effect of drug court participation on drug use (Model A), controlling for respondent age, ASPD, prior arrests, and primary hard drug use, and we again found drug court participation to be a significant predictor of reduced incidence of drug use (beta was -0.973; its p-value < .001; the intercept = 1.988).

Model D tests the effect of psychological mediators on drug use, and Model E tests the effect of court practices on drug use. Despite the bivariate finding that all court practices and psychological mediators affect drug use, as shown in Table 4-6.2, we identified only *two* court practices and *one* potential psychological mediator that significantly affected days of drug use one year later—namely, drug tests, sanctions, and attitude toward the judge. Regardless of the type of court in which they were placed, the more drug tests respondents report six months after the baseline interview, the fewer days of drug use they reported one year later (at the 18-month interview). Conversely, the more sanctions that respondents received by the six-month interview, the more days of drug use they reported subsequently. With regard to the one significant psychological mediator, respondents who displayed a more positive attitude toward the judge six months after the baseline interview (e.g., said their judge was knowledgeable about their case, gave them a chance to tell their side of the story, could be trusted to treat them fairly, treated them with respect) reported fewer days of drug use in the subsequent 18-month interview.[23] Model F identifies the court practices that were significantly related to attitude toward judge (from Model D), Model G tests whether drug court participation was a significant predictor of attitude toward judge, and Model H tests whether participation in drug court was a significant predictor of either of the court practices identified in Model E. Although parameter estimates are not reported to conserve space, we summarize results here. From Model F, we determined that three court practices—court appearances, drug tests, and drug treatment—significantly predicted more positive attitudes toward the judge, while case management contacts and sanctions had no statistically significant effects.[24] From Model G, we confirmed that respondents who participated

[22] All beta coefficients reported are unstandardized.

[23] A test of whether this within-courts effect of attitude toward judge on subsequent drug use varied between those who participated in drug court and those who did not revealed no significant cross-level variation (i.e., the p-value of the cross-level interaction was 0.491).

[24] Prediction of attitude toward judge was modeled using both Poisson and linear regressions, and both showed the same substantive results; however, in the final MSEM, attitude toward judge was estimated using linear regression, because there was insufficient memory for Poisson integration.

in drug court subsequently reported more positive attitudes toward the judge, on average, at a p-value less than .001 (between-courts unstandardized beta was 0.894; intercept = 3.223). And from Model H, we found that drug court participation was associated with increased levels of the following court practices: court appearances, drug tests, drug treatment, and sanctions, when all variables' predictions were modeled simultaneously in a multilevel model that accounted for the clustering of individuals into courts.[25]

Table 4-6.2. Interim Models B, D, and E, Predicting Days of Drug Use per Month

	Model B		Model D		Model E	
	Beta	*Std Err*	*Beta*	*Std Err*	*Beta*	*Std Err*
Court Practices						
Case management contacts					-0.003	0.009
Court appearances					-0.032	0.017
Drug tests					-0.013 *	0.005
Drug treatment in weeks					-0.021	0.012
Sanctions					0.107 ***	0.027
Psychological Mediators						
Attitude toward judge scale			-0.304 **	0.107		
Deterrence score			-0.046	0.045		
Certainty of response scale			0.017 [a]	0.096		
Perceived consequences scale			-0.305	0.189		
Distributive justice indicator			-0.059	0.061		
Procedural justice scale			-0.093	0.110		
Readiness to change score			0.017	0.084		
Treatment motivation scale			0.044 [b]	0.078		
Treatment eagerness scale			-0.015	0.061		
Independent Variables						
Age	-0.024 **	0.009				
Antisocial personality disorder	0.384 **	0.127				
Black/African American	0.121	0.202				
Days unavailable on street	-0.007	0.010				
Depression	0.081	0.082				
Family drug abuse	0.027	0.182				
High school diploma or GED	-0.054	0.114				
Income	0.002	0.031				
Male gender	0.063	0.141				
Married or in relationship	0.069	0.105				
Minor children (<18 years)	-0.181	0.119				
Prior arrests	0.020 **	0.007				
Primary hard drug of choice	-0.183 [c]	0.097				
White/Caucasian	-0.110	0.187				
Dispersion	3.213 ***	0.335	3.592 ***	0.389	3.373 ***	0.342

*Notes: N*s ranged from 1,211 to 1,320 respondents; N=29 courts in all models. Results are weighted to control for comparability at

[25] Similar to the prediction of attitude toward judge (described in previous footnote), prediction of court practices was modeled using both Poisson and linear regressions; however, in the final MSEM court variables were estimated using linear regression.

baseline and attrition bias (Rempel and Green 2009). Interim models were estimated in response to the following questions: (B) What independent variables significantly predict the outcome? (D) What psychological mediators significantly predict the outcome? (E) What court practices significantly predict the outcome? All models were estimated using negative binomial regression in Mplus 6.0 (Muthén and Muthén 1998-2010).

[a] *Certainty of response and perceived consequences were estimated in models separate from that including the deterrence score.*

[b] *Treatment motivation and eagerness were estimated in models separate from that including the readiness to change score.*

[c] *P-value approached significance at .058.*

** p<.05 ** p<.01 *** p<.001*

Results from the Final Model I

Combining results from the interim models above, we estimated one final MSEM of the relationship between drug court participation and drug use. Effects that were not statistically significant in the final model were removed—which included the control variable effects of prior arrests and primary hard drug use on the dependent variable, and the within-courts level effect of attitude toward judge on drug use (only the between-courts effect was statistically significant). Effect estimates from the final MSEM showing how drug courts reduce drug use are presented in Table 4-6.3, while the pathways illustrating those effects are shown in Figure 4-6.2. There are three main takeaway points from this model.

First, at the between-courts level, drug courts had both a direct effect (beta=-0.608, P<0.01) and an indirect effect (beta=-1.585, p<0.05)—through attitude toward judge—on reducing subsequent drug use.[26] Respondents who participated in drug courts reported fewer subsequent days of drug use per month, on average across all courts, 18 months later; and, they expressed more positive attitudes toward the judge (beta=0.941, p<0.001) at their 6-month interview, which in turn was associated with lower levels of drug use at their 18-month interview (beta=-1.685, p<0.01), on average across all courts.

Second, drug courts were associated, at the between-courts level, with increased court appearances (beta=9.230, p<0.001), weeks of drug treatment (beta=8.684, p<0.001), drug tests (24.861, p<0.001), and sanctions (1.237, p<0.001). Although none of these court practices had significant between-courts effects on subsequent drug use (meaning an indirect effect of drug court on drug use via court practices could not be found; see Preacher et al. 2010), they did have significant within-courts effects on attitude toward judge and drug use. Thus, while drug court participation meant more intensified involvement with the court, the courts that showed more intensified involvement, on average, did not have significantly lower drug use. Rather, within any particular court (drug court or comparison), more frequent involvement—as measured by a higher number of court appearances (beta=0.027, p<0.001), weeks of drug treatment (beta=0.017, p<0.001), and drug tests (beta=0.004, p<0.05)—was associated with more positive attitudes toward the judge, at the individual-respondent level. Similarly, within any particular court, the more frequently a respondent was tested for drugs, the lower their subsequent drug use (beta=-0.018, p<0.001); and the more sanctions that a respondent reported receiving at six months, the more subsequent drug use they reported one year later (beta=0.076, p<0.01).

[26] Asterisks correspond to statistical significance levels defined as: * p<.05, ** p<.01, and *** p<.001.

The third and final takeaway point has to do with the significant effects of the two control variables included in the final MSEM. Respondents who were younger (beta=-0.016, p<0.05) at the baseline interview and those who showed features of antisocial personality disorder (beta=0.494, p<0.001) reported higher levels of drug use 18 months later.

Table 4-6.3. MSEM Showing How Drug Courts Reduce Drug Use

	Attitude Toward Judge Scale[a]		Days of Drug Use per Month[b]	
	Beta	***Std Err***	***Beta***	***Std Err***
Between Courts Predictors				
Attitude toward judge scale			-1.685 **	0.646
Drug court (direct effect)	0.941 ***	0.120	-0.608 **	0.233
Drug court (indirect effect)[c]			-1.585 *	0.697
Intercept	3.191 ***	0.109	1.759 ***	0.298
Within Courts Predictors				
Age			-0.016 *	0.008
Antisocial personality disorder			0.494 ***	0.138
Court appearances	0.027 ***	0.006		
Drug tests	0.004 *	0.002	-0.018 ***	0.005
Drug treatment in weeks	0.017 ***	0.003		
Sanctions			0.076 **	0.028
Dispersion			3.329 ***	0.327
Additional Effects (Between Courts)				
	Beta	***Std Err***	***Intercept***	***Std Err***
Drug court (direct effect) on …				
Court appearances	9.230 ***	1.06	1.590 ***	0.426
Drug tests	24.861 ***	2.642	4.551 ***	0.678
Drug treatment in weeks	8.684 ***	1.290	4.996 ***	1.084
Sanctions	1.237 ***	0.294	0.812 ***	0.229

Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. *N*=29 courts; *N*=1,297 respondents.

Note: Beta coefficients are unstandardized; standard errors are robust. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). Model allows significant within-level covariances between court appearances, drug tests, and drug treatment, as well as significant between-level correlations between residual errors in equations predicting drug treatment and sanctions.

[a] Modeled using linear regression in Mplus 6.0 (Muthén and Muthén 1998-2010).

[b] Modeled using negative binomial regression in Mplus.

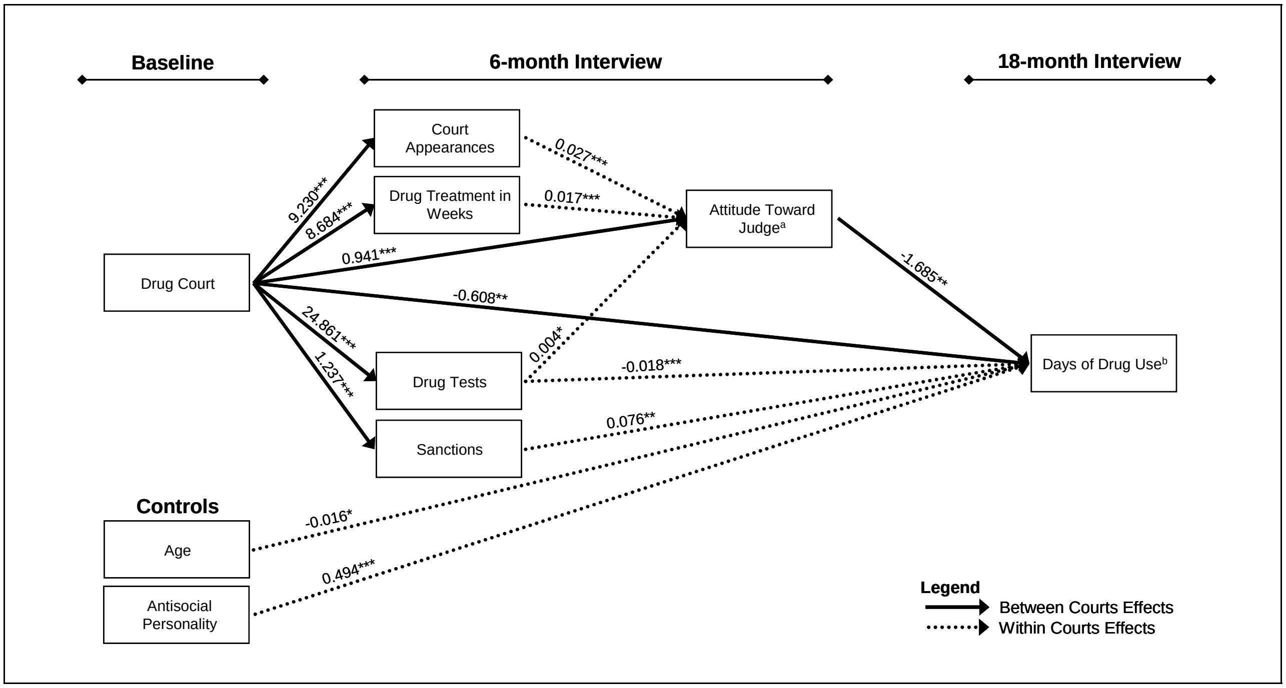
[c] Equals the product (b_1*b_2) of the effect of drug court on attitude toward judge (b_1) and the effect of attitude toward judge on drug use (b_2).

* *p<.05* ** *p<.01* *** *p<.001*

How Do Drug Courts Work to Reduce Crime?

The number of crimes that respondents reported committing at the 18-month interview was, not surprisingly, highly correlated with the average days of drug use they reported in the same interview (rho=0.662, p<0.001). Thus, although we intentionally separated prediction of these two dependent variables for parsimony's sake (to preserve the number of parameters estimated), results from our repetition of steps described in Models A through I with regard to the crime outcome were substantively similar to those described previously in relation to drug use.

Figure 4-6.2. MSEM Showing How Drug Courts Reduce Drug Use (Illustration of Table 6.3)



Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=29 courts; N=1,297 respondents.

Notes: Beta coefficients are unstandardized; standard errors are robust. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). Model allows significant within-level covariances between court appearances, drug tests, and drug treatment, as well as significant between-level correlations between residual errors in equations predicting drug treatment and sanctions. Intercepts are not shown in diagram.

[a] Modeled using negative binomial regression in Mplus 6.0 (Muthén and Muthén 1998–2010).

[b] Modeled using linear regression in Mplus.

* *p<.05* ** *p<.01* *** *p<.001*

Results from Interim Models A through H

Model A confirmed that drug court participation at baseline predicted fewer crimes committed per month, as reported in the 18-month interview, even after accounting for the clustering of individuals into courts (beta=-0.622; p<0.01). Estimation of Model B, as shown in Table 4-6.4, showed that age, antisocial personality disorder, and prior arrests were significantly associated with higher numbers of crimes reported in the 18-month interview. Model C's re-estimation of the effect of drug court participation on crime, controlling for age, ASPD, and prior arrests, confirmed that it was still a significant predictor (beta=-0.976; p<-.001).

From estimation of Models D and E, as shown in Table 4-6.4, we again found that only attitude toward judge predicted the number of crimes reported one year later, as did drug treatment and

Table 4-6.4. Interim Models B, D, and E, Predicting Number of Crimes per Month

	Model B		Model D		Model E	
	Beta	*Std Err*	*Beta*	*Std Err*	*Beta*	*Std Err*
Court Practices						
Case management contacts					-0.007	0.016
Court appearances					-0.018	0.009
Drug tests					-0.005	0.009
Drug treatment in weeks					-0.030 **	0.009
Sanctions					0.105 *	0.041
Psychological Mediators						
Attitude toward judge scale			-0.272 **	0.092		
Deterrence score			-0.030	0.059		
Certainty of response scale			-0.006 [a]	0.145		
Perceived consequences scale			-0.172	0.157		
Distributive justice indicator			-0.072	0.063		
Procedural justice scale			-0.195	0.100		
Readiness to change score			0.047	0.101		
Treatment motivation scale			0.081 [b]	0.086		
Treatment eagerness scale			0.008	0.086		
Independent Variables						
Age	-0.037 **	0.011				
Antisocial personality disorder	0.800 ***	0.183				
Black/African American	0.015	0.293				
Days unavailable on street	0.012	0.015				
Depression	0.061	0.161				
Family drug abuse	0.176	0.202				
High school diploma or GED	-0.003	0.120				
Income	0.028	0.039				
Male gender	-0.250	0.171				
Married or in relationship	-0.107	0.133				
Minor children (<18 years)	-0.225	0.153				
Prior arrests	0.020 *	0.009				
Primary hard drug of choice	0.070	0.162				
White/Caucasian	-0.155	0.220				
Dispersion	7.001 ***	0.628	8.095 ***	0.786	7.766 ***	0.747

*Notes: N*s ranged from 1,211 to 1,320 respondents; N=29 courts in all models. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). Interim models were estimated in response to the following questions: (B) What independent variables significantly predict the outcome? (D) What psychological mediators significantly predict the outcome? (E) What court practices significantly predict the outcome? All models were estimated using negative binomial regression in Mplus (Muthén and Muthén 1998-2010).

[a] Certainty of response and perceived consequences were estimated in models separate from that including the deterrence score.

[b] Treatment motivation and eagerness were estimated in models separate from that including the readiness to change score.

* *p<.05* ** *p<.01* *** *p<.001*

sanctions.[27] Models F, G, and H resulted in identification of the following significant effects: court appearances, drug tests, and drug treatment predicted attitude toward judge; drug court participation predicted attitude toward judge (beta=0.894; p<0.001); and drug court participation predicted court appearances, drug tests, drug treatment, and sanctions.

Results from the Final Model I

Effect estimates from the final MSEM showing how drug courts reduce crime are presented in Table 4-6.5, while the pathways illustrating those effects are shown in Figure 4-6.3. Similar to that with the MSEM predicting drug use, there are three key points.

First, at the between-courts level, drug courts had both a direct (beta=-0.971, p<0.01) and an indirect effect (beta=-1.397, p<0.05)—through attitude toward judge—on reducing subsequent crime. Respondents who participated in drug courts reported committing fewer crimes per month, on average across all courts, 18 months later; and, they expressed more positive attitudes toward the judge (beta=0.967, p<0.001) at their 6-month interviews—which was associated with fewer crimes reported at their 18-month interviews (beta=-1.445, p<0.05), on average across all courts.

Second, drug courts were associated, at the between-courts level, with increased court appearances (beta=9.279, p<0.001), weeks of drug treatment (beta=8.728, p<0.001), drug tests (24.832, p<0.001), and sanctions (1.203, p<0.001). Although none of these court practices had significant between-courts effects on subsequent drug use (again, meaning an indirect effect of drug court on crime via court practices could not be found as specified in Preacher et al. 2010), they did have significant within-courts effects on attitude toward judge and criminal activity. Thus, while drug court participation meant more intensified involvement with the court, the courts that showed more intensified involvement, on average, did not have significantly lower rates of crimes reported. Rather, within any particular court (drug court or comparison), more frequent involvement—as measured by a higher number of court appearances (beta=0.026, p<0.001), weeks of drug treatment (beta=0.017, p<0.001), and drug tests (beta=0.004, p<0.05)—was associated with more positive attitudes toward the judge, at the individual-respondent level. Similarly, within any particular court, the more weeks of treatment a respondent received, the lower their subsequent criminal activity (beta=-0.017, p<0.001). Further, the more sanctions that a respondent said were imposed at six months, the more subsequent crimes they reported committing one year later (beta=0.088, p<0.01).

The third and final takeaway point again relates to the control variables included in the final MSEM: respondents who were younger (beta=-0.034, p<0.001) at the baseline interview, those who showed features of antisocial personality disorder (beta=0.712, p<0.001), and those with

[27] A cross-level interaction testing whether the within-courts effect of attitude toward judge on subsequent crime varied between those who participated in drug court and those who did not revealed no significant variation (the p-value was 0.676).

more prior arrests (beta=0.020, p<0.05) reported higher subsequent levels of crime 18 months later.[28]

Table 4-6.5. MSEM Effects Showing How Drug Courts Reduce Crime

	Attitude Toward Judge Scale[a]		Number of Crimes per Month[b]	
	Beta	*Std Err*	*Beta*	*Std Err*
Between Courts Predictors				
Attitude toward judge scale			-1.445 *	0.575
Drug court (direct effect)	0.967 ***	0.136	-0.971 **	0.306
Drug court (indirect effect)			-1.397 *	0.629
Intercept	3.174 ***	0.126	2.439 ***	0.337
Within Courts Predictors				
Age			-0.034 ***	0.009
Antisocial personality disorder			0.712 ***	0.187
Court appearances	0.026 ***	0.006		
Drug tests	0.004 *	0.002		
Drug treatment in weeks	0.017 ***	0.003	-0.021 *	0.009
Prior arrests			0.020 *	0.008
Sanctions			0.088 **	0.026
Dispersion			6.771 ***	0.573
Additional Effects (Between Courts)				
	Beta	*Std Err*	*Intercept*	*Std Err*
Drug court (direct effect) on …				
Court appearances	9.279 ***	1.053	1.561 ***	0.428
Drug tests	24.832 ***	2.631	4.517 ***	0.689
Drug treatment in weeks	8.728 ***	1.316	4.971 ***	1.125
Sanctions	1.203 ***	0.303	0.809 **	0.233

Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=29 courts; N=1,259 respondents.

Notes: Beta coefficients are unstandardized; standard errors are robust. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). Model allows significant within-level covariances between court appearances, drug tests, and drug treatment, as well as significant between-level correlations between residual errors in equations predicting court appearances and drug tests, and drug treatment and sanctions.

[a] Modeled using linear regression in Mplus 6.0 (Muthén and Muthén 1998–2010).

[b] Modeled using negative binomial regression in Mplus.

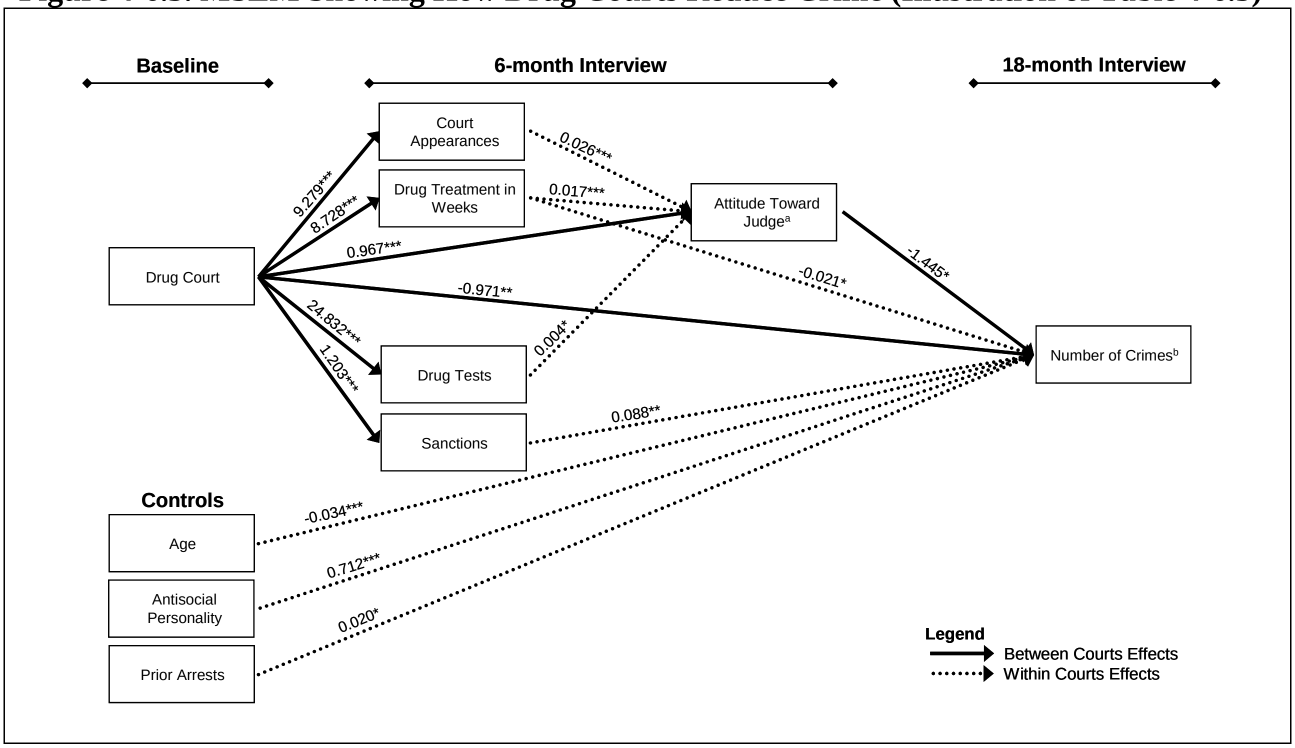
** p<.05 ** p<.01 *** p<.001*

[28] As a follow-up test, we reran Model I results for both the crime and drug use outcomes, substituting the previously described psychological mediators, one by one, in place of attitude toward judge, to see if any functioned similarly in the model. We again found that neither deterrence (certainty or perceived consequences), distributive justice, nor readiness to change (treatment motivation or eagerness) significantly mediated the relationship between drug court participation and outcomes. However, procedural justice *did* function as a psychological mediator, when substituted for attitude toward judge, for both crime and drug use outcomes. Notably, procedural justice perceptions were highly and significantly correlated (rho=.684, p=.000) with attitudes toward the judge. Thus, although attitude toward judge was the "superior" mediator—i.e., captured a greater amount of the variation in the relationship between drug court participation and outcomes—perceptions of procedural justice, with which it was highly correlated, also functioned similarly if placed in a model in lieu of attitude toward judge.

Are the Paths From Drug Court Participation to Subsequent Outcomes Directly or Indirectly Moderated by Other Factors?

To address our last research question—are the paths from drug court participation to subsequent outcomes directly or indirectly moderated by other factors, such as age, gender, prior drug use, and criminal history, we re-estimated the final Models I from above, predicting drug use and crime as shown in Figures 4-6.2 and 4-6.3, but focused explicitly on variations in the between-courts paths from (1) drug court to attitude toward judge, (2) attitude toward judge to each dependent variable, and (3) drug court to each dependent variable directly. In the first set of models, we restricted each of these three effects to be equivalent across subgroups defined by age, gender, and so forth; while in the second set of models, we permitted these effects to vary across subgroups.

Figure 4-6.3. MSEM Showing How Drug Courts Reduce Crime (Illustration of Table 4-6.5)



Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=29 courts; N=1,259 respondents.

Notes: Beta coefficients are unstandardized; standard errors are robust. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). Model allows significant within-level covariances between court appearances, drug tests, and drug treatment, as well as significant between-level correlations between residual errors in equations predicting court appearances and drug tests, and drug treatment and sanctions. Intercepts are not shown in diagram.

[a] Modeled using negative binomial regression in Mplus 6.0 (Muthén and Muthén 1998–2010).

[b] Modeled using linear regression in Mplus.

* *p<.05* ** *p<.01* *** *p<.001*

The unrestricted models necessarily provided a better fit to the MADCE data (i.e., a lower chi-square value); however, only when this improvement in fit was statistically significant could we determine that group variation—or moderation—in the direct and indirect effects of drug court participation on the dependent variables existed. The difference in chi-square values between the restricted and unrestricted models was calculated by subtracting the unrestricted model chi-square from the restricted model chi-square and dividing the result by the Satorra-Bentler (1999) scaling correction factor (c), based on the approach described in Muthén and Muthén (2005). The significance of the resulting difference was assessed using a chi-square distribution table, p-value cutoff of .05, and the corresponding three degrees of freedom.

As shown in Table 4-6.6, we examined the possible moderation of drug courts' effects on drug use and crime (moderated-mediation) across the following grouping variables, listed in alphabetical order: age, ASPD, depression, frequent drug user, gender, primary hard drug of choice, and prior arrests. Each grouping variable divided the sample of 1,349 offenders into exactly two subgroups, as described in Table 4-6.6 (e.g., under 30 versus 30 and older, male versus female, 5 or more arrests versus less).

Table 4-6.6. Testing Moderation in How Drug Courts Reduce Drug Use and Crime across Groups

	Drug Courts' Effects on Drug Use			Drug Courts' Effects on Crime		
Groups Tested	**Restricted Model**	**Unrestricted Model**	**Difference**	**Restricted Model**	**Unrestricted Model**	**Difference**
Age	χ^2=254.95	χ^2=251.81	χ^2=1.84	χ^2=281.93	χ^2=280.07	χ^2=2.09
Under 30 vs. 30 or older	df=85, c=1.5	df=82, c=1.5	df=3, c=1.7	df=93, c=1.4	df=90, c=1.4	df=3, c=0.9
Antisocial Personality Disorder	χ^2=230.02	χ^2=223.61	χ^2=5.04	χ^2=306.75	N/A[a]	N/A
Shows features vs. does not	df=74, c=1.6	df=71, c=1.6	df=3, c=1.3	df=93, c=1.4		
Depression	χ^2=219.08	χ^2=218.72	χ^2=0.33	χ^2=245.74	χ^2=242.17	χ^2=3.55
Shows features vs. does not	df=85, c=1.5	df=82, c=1.55	df=3, c=1.1	df=93, c=1.4	df=90, c=1.4	df=3, c=1.0
Frequent Drug User	χ^2=319.36	χ^2=303.92	χ^2=10.10 *	χ^2=266.55	χ^2=262.22	χ^2=4.75
20–30 days per month vs. less	df=85, c=1.5	df=82, c=1.5	df=3, c=1.5	df=93, c=1.5	df=90, c=1.5	df=3, c=0.91
Gender	χ^2=201.45	χ^2=198.88	χ^2=2.33	χ^2=219.88	χ^2=217.91	χ^2=0.06
Male vs. female	df=85, c=1.4	df=82, c=1.4	df=3, c=1.1	df=93, c=1.3	df=90, c=0.2	df=3, c=32.9
Primary Hard Drug of Choice	χ^2=277.35	χ^2=271.11	χ^2=5.05	N/A[a]	N/A[a]	N/A
Yes vs. no	df=85, c=1.6	df=82, c=1.6	df=3, c=1.2			
Prior Arrests	χ^2=243.84	χ^2=239.04	χ^2=3.55	χ^2=231.27	χ^2=228.77	χ^2=2.79
5 or more vs. less	df=85, c=1.6	df=82, c=1.6	df=3, c=1.4	df=93, c=1.5	df=90, c=1.5	df=3, c=0.9

*Notes: N*s ranged from 1,259 to 1,297 respondents; N=29 courts in all models. Results are weighted to control for comparability at baseline and attrition bias (Rempel and Green 2009). All dependent variables were modeled using linear regression. Tests of moderation were conducted via chi-square difference testing of nested models estimated in Mplus 6.0 (Muthén and Muthén 1998-2010; Satorra and Bentler 1999). Restricted models held the direct and indirect (through attitude toward judge) effects of drug court participation equal across groups, while the unrestricted models allowed the effects to vary. The difference indicates whether the unrestricted model offered a significant improvement in model fit, thereby indicating moderation of effects. The difference was calculated by subtracting the restricted and unrestricted model chi-squares and then dividing the result by the Satorra-Bentler scaling correction factor (c), based on the approach described in Muthén and Muthén (2005).

[a] *Model estimation did not terminate due to an ill-conditioned Fisher information matrix.*

* *p<.05* ** *p<.01* *** *p<.001*

The only evidence of significant moderation in drug courts' effects occurred with regard to drug use (but not with regard to crime) between respondents who reported frequent drug use at baseline—an average of 20-30 days per month—compared to those who reported less drug use. The effect of drug court participation on reductions in drug use directly was stronger for those who initially reported frequent drug use (beta=-3.946, $p<0.01$ for frequent drug users, compared to beta=-1.437, $p<0.001$ for non-frequent users), and the effect of attitude toward judge on drug use also was stronger for frequent drug users (beta=-8.013, $p<0.001$, compared to beta=-2.494, $p<0.001$). However, the effect of drug court participation on attitude toward judge was somewhat weaker for frequent drug users (beta=0.843, $p<0.001$ for frequent drug users, compared to beta=0.961, $p<0.001$ for non-frequent users).

Limitations

The appeal of multilevel structural equation modeling—and particularly its advanced implementation using sophisticated software such as M*plus*—is that the complex realities of human and group behavior can be modeled simultaneously. However, like all statistical representations of causal processes, there are limitations of these models in applied research. The models we hypothesized were carefully guided by theory (therapeutic jurisprudence, procedural justice, distributive justice, deterrence, and readiness to change) and practical considerations (e.g., the present cannot influence the past). However, there are other ways of modeling data from NIJ's MADCE; and thus, there is always the possibility that other MSEMs exist that may provide a comparable or even more accurate model of the relationships among the variables we analyzed. It is also certain that we did not include all of the possible influences on drug use and crime as we were limited by those available to us in the data we selected. We note that the mere fact that drug court participation persistently had a "direct" effect on the dependent variables meant that we must have omitted some other important mediating court-related or psychological factor.

Additionally, all data analyzed in this paper were self-reported, and there have been some indications that individuals tend to underreport undesirable behaviors or overreport those that are socially desirable. For our purposes, however, such under- or overreporting would have only compromised the results if we had good reason to believe that it was unequally distributed across drug and comparison courts. Given the relatively large sample sizes of MADCE respondents, courts, and states, we feel fairly confident that any biasing effects of our analyzing self-reported data were minimal.

Conclusions

We report four (consistent) findings about how drug courts lead to desistance. First, we find substantial evidence that there is a direct effect of drug court participation on desistance from drug use and criminality. Even in models that control for all significant individual risk factors, court practices, and theoretical mediators, there remains an independent effect of drug court on improved behavior. Second, there is a strong judge effect: at the between-courts level, drug courts had an indirect effect, through attitude toward judge, on reductions in subsequent drug use and criminal behavior. Drug courts participants reported fewer subsequent days of drug use and crimes committed per month, on average across all courts, 18 months later, and, they expressed more positive attitudes toward the judge at their 6-month interview, which in turn was associated with lower levels of drug use and crime at their 18-month interview, on average across all courts. Third, drug courts increased court appearances, weeks of drug treatment, drug tests, and sanctions. Although there were no indirect between-courts effects of drug court on drug use via court practices, there was a within-courts effect of certain court practices on attitude toward judge, such that individuals who made more court appearances, received more weeks of drug treatment, and were subjected to more drug tests had better attitudes toward the judge. Fourth, we find evidence that younger drug court participants and those with antisocial personality disorder had a higher incidence of drug use at follow up; however, there is no evidence that this moderated the mediational effect of drug court on outcomes—both directly or indirectly through positive attitudes toward the judge.

The most striking finding in this research is the power of the judge, and judicial interactions with the offenders, to promote desistance. We find no evidence that motivation for treatment, specific deterrence, fairness of one's court outcome, or a broad measure of procedural justice are associated with desistance in our sample. We posit three potential explanations for this finding. First, it is possible that the results signify exactly what they purport, that is, that those theoretical processes are not associated with better outcomes in drug court. Second, it is possible that the drug courts in our sample did not effectively implement practices that would promote those theoretical mechanisms. Thus, for example, it is entirely possible that although drug courts self-report adherence to best treatment practices, treatment was not implemented in these drug courts in a manner consistent with effective evidence-based practice. Finally, it is possible that the power of the judge (typed by legal scholars as therapeutic jurisprudence) is so strong that it effectively suppresses all other theoretical mechanisms.

The other striking finding is that drug court appears to be equally effective for everyone, and, that the mechanisms of effectiveness are the same for all participants. That is, while we find evidence that some subgroups (such as younger participants or participants with ASPD) have worse outcomes, those attributes do not moderate the drug court effect. Put another way, while we find evidence that those groups do worse than average, they appear to have similar improvements as other participants, and thus do better than they would have without drug courts. This finding argues against the common drug court practice of skimming, or attempting to identify ex ante a population that is at a lower risk of recidivism.

In summary, these findings suggest that although drug courts are effective at promoting desistance in their present form, there is potential for drug courts to be even more effective. First, even though we find that the judge has a prime role in shaping participant behavior, we note that drug courts do not necessarily maximize the potential of judge—as many drug courts engage in practices (such as rotating judges or having multiple drug court judges) that would be expected to diminish judicial effectiveness. And finally, although other theoretical mechanisms were not shown here to be effective at modifying behavior, a substantial body of literature supports many of the underlying premises of deterrence and treatment motivation and eagerness. Thus, it is probably fair to conclude that if drug courts used these mechanisms more effectively, drug court results likely would be even better.

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Chapter 7. Impacts of Court Policies and Practices

Janine M. Zweig, Christine H. Lindquist, P. Mitchell Downey, John K. Roman, and Shelli B. Rossman

Since 1997, ten components have been consistently recommended as key to the drug court model; however, many have not been subject to any empirical investigation.[29] Previous research on the effectiveness of specific drug court policies and practices has focused on only a few drug court components, primarily: court appearances, treatment, and sanctions. Further, much of the previous work has focused on client-level experiences of drug court components, rather than court-level policies or practices.[30]

One of the original objectives of the MADCE was to identify the variation in policies and practices across drug courts, and determine whether this variation impacts program effectiveness. Although we recognize that client-level variation in which drug court components were received may influence outcomes as well, the focus in this chapter is on court-level policies and practices. In addition to identifying policies and practices that are effective for drug court participants overall, our analyses explore "what works *for whom,*" in recognition of the fact that not all participant subgroups respond to policies and practices in the same manner.

As reported in Chapters 3 and 4 of this Volume, the results from NIJ's MADCE indicate that 18 months after study enrollment, individuals who participated in drug court programs had significant reductions in self-reported drug use and criminal activity relative to comparable offenders who were not part of a drug court program. However, NIJ's MADCE and previous multi-site evaluations have found substantial variation in client success across drug courts, with some courts producing much larger effects than others. Some of this variation is likely due to the riskiness of the population served by particular drug courts, as courts that serve higher-risk

[29] Specifically, the ten key components are (Office of Justice Programs and National Association of Drug Court Professionals 1997): (1) that the drug court integrates alcohol and other drug treatment services with justice system case processing; (2) that the drug court use a non-adversarial approach with prosecution and defense counsel working together to promote public safety and protect participants' due process rights; (3) that eligible participants are identified early in case processing and placed in the program; (4) that the drug court provides access to a variety of substance abuse treatment programs and other rehabilitative services; (5) that the drug court monitor abstinence through frequent alcohol and drug testing; (6) that the court use a coordinated strategy to respond to participants' compliance with sanctions and incentives; (7) that the court provides ongoing judicial interaction with each drug court participant; (8) that court's monitor their effectiveness; (9) that the drug court team members continue with interdisciplinary education that promotes effective drug court planning, implementation, and operations; and (10) that partnerships among drug courts, public agencies, and community-based organizations are forged to enhance drug court program effectiveness.

[30] The limited previous research on court-level policies and practices finds that clients assigned to dockets using graduated sanctions and clients assigned to dockets in which treatment was required have better outcomes than those in standard dockets (Harrell, Cavanagh, and Roman 1998, 2000). Based on the one court-level study of biweekly status hearings compared to as-needed hearings, the use of regular status hearings does not appear to be associated with better outcomes for drug court clients as a whole (Marlowe, Festinger, and Lee 2004, Marlowe, Festinger, Lee et al. 2003, Marlowe, Festinger, et al. 2005). Instead, the effectiveness of status hearings may vary based on participant characteristics, with higher risk participants benefiting from biweekly hearings (Festinger, Marlowe, et al. 2002). Also, Finigan, Carey, and Cox (2007) examined whether judges differed in their success in reducing recidivism among drug court participants, and whether they improve with experience, finding that they did.

clients may have worse outcomes on average when compared to those that exclude riskier clients (where risk is defined as more serious criminal histories, dependence on more serious drugs, or lower motivation to change). However, it is also likely that variation in implementation of court policies and practices across drug courts is associated with differential effectiveness. Indeed, although drug courts share some common elements, substantial variation in how policies and practices are implemented across courts has been documented (Carey, Finigan, and Pukstas 2008; Rempel, Fox-Kralstein, et al. 2003).

Therefore, the MADCE was specifically designed to allow for an examination of the impact of the implementation of specific policies and practices on client outcomes, in that the study included a large number of courts (N=23) that were purposefully selected to reflect variation in key policies and practices (for more details, see Volume 1, Chapter 3). The conceptual framework designed for the MADCE research (see Figure 4-7.1) allows one to better understand the impact of drug courts by linking drug court practices (the third, orange column) to outcomes (the last, light blue column) that might produce the desired reduction in drug use and crime. The drug court practices documented in this list include elements of previous conceptual frameworks for drug court programs posited by Temple University (Goldkamp, White, and Robinson 2001) and the RAND Corporation (Longshore, Turner et al. 2001), as well as an expansion of practices we hypothesized to be important to drug court success.

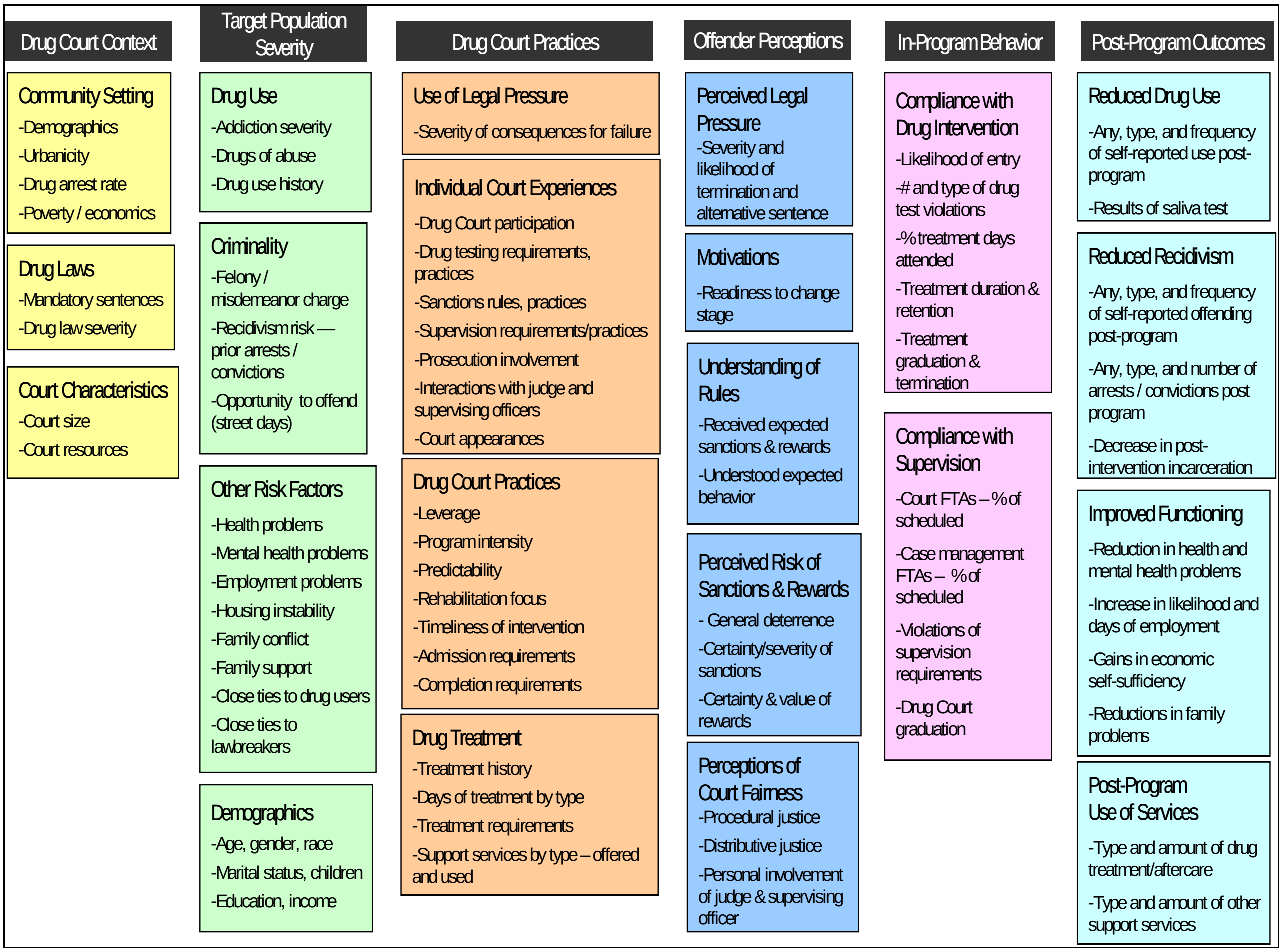
Based on this conceptual framework, we chose ten specific court policies and practices to explore in this analysis that might be related to drug courts' abilities to prevent future substance use and crime. Specifically, we examined the effects of court implementation of policies and practices related to (1) leverage, (2) predictability of sanctions, (3) adherence to treatment best practices, (4) drug testing, (5) case management, (6) judicial status hearings, (7) point of entry into the program, (8) multidisciplinary decisionmaking among the drug court team, (9) positive judicial attributes, and (10) judicial interaction.

Importantly, our findings for these drug court practices are presented for numerous client subgroups (based on demographic characteristics, previous drug use and treatment history, and criminal history), reflecting a growing body of literature that supports the notion that not all participant subgroups respond to policies and practices in the same manner. We believe that the findings presented in this chapter have practical utility for drug court practitioners to help refine their programs and to target policies or practices to specific participant subgroups served. Very few previous studies have attempted to isolate the impact of court policies and practices on drug court effectiveness.

Design and Methodology

We employed two complementary approaches, using both quantitative and qualitative methodologies. First, we tested the effectiveness of particular policies and practices using a traditional quantitative approach—hierarchical modeling. Traditionally, hierarchical models are used to analyze nested data. Similar to students in a classroom, drug court clients in a courtroom are generally repeatedly exposed to the same judge. Thus, it is easy to confuse the effect of the judge on outcomes for the effect of the drug court on outcomes. Hierarchical models parse out individual effects on outcomes from court effects on outcomes. Using this approach, the first level is the individual participants, with the model controlling for differences among individuals. The second level is the courts into which individuals are clustered, with Level-2 variables including court-level policies and practices. The analysis reflects which practices, in general, are associated with the best outcomes, after controlling for individual-level differences. Hence, we present findings from this approach for each court policy and practice using hierarchical analysis of variance with follow-up Tukey tests of group comparisons.

Figure 4-7.1. The MADCE Conceptual Framework



Second, we employed an innovative approach that ranks courts in levels of effectiveness at preventing drug use and crime (overall, and for various subgroups). Once ranked, each court is characterized based on the way it implemented particular policies and practices, enabling a qualitative comparison of patterns of practices observed among the most effective courts. Toward this end, we created a counterfactual for each individual in our dataset that is the difference between a person's expected outcome for drug use and expected outcome for criminal behavior to their observed outcomes in drug court. Thus, we predict what participants' drug use and criminal activity would have been without drug court, conditional on their particular characteristics, and subtract observed outcomes from this predicted expected outcome. Next, we ranked courts based on the average performance of their participants. Positive court average values indicate that, overall, the court's participants did better as a result of being in drug court; negative values indicate that, on average, participants in that court did worse than would be expected. Courts are ranked based on two outcomes of interest: (1) days of drug use prevented and (2) number of criminal activities prevented.

Once the rankings were created for the two outcomes, we assigned color codes to each court that characterized the way they implemented particular policies and practices. From these data, we can identify if there are patterns for effective courts in how they implement policies and practices compared to ineffective courts. We also can identify if, among effective courts, there are patterns among top-performing courts in how they implemented policies and practices as compared to lower-performing courts.

Data Sources

The data for this analysis come from three sources. First, data are used from NIJ's MADCE Adult Drug Court Survey (see Volume 2 for details),[31] which included five major sections, as well as subsections covering more specific topics within each area:

1. *General Information*, including population served, points of entry into the program, and case flow.

2. *Program Structure*, including program characteristics, eligibility criteria, and substance abuse assessment.

3. *Program Operations*, including management information systems, entry into the drug court program, program staffing, case management, and program contacts.

4. *Treatment and Drug Testing,* including substance abuse treatment services and drug testing.

5. *Courtroom Practices*, including courtroom practices, infractions and sanctions, achievements, and graduation).

The second source of data was the process evaluation that included multiple contacts with the courts considered for, or ultimately included, in the outcome study. Phone interviews were conducted with potential study courts during the site selection phase in summer 2004 and many questions were asked about court operations. The purpose was to understand the court's adherence to best practices related to leverage, sanctioning, and treatment so that the research team could select a sample of courts that varied across these areas. Once the impact study began,

[31] During the initial phase of the study (between February and June 2004), we conducted a web-based survey of drug courts that primarily served adults and had been in operation for at least one year at that time. A total of 380 drug courts completed the survey, including the 23 courts that were ultimately selected for the outcome study. The survey response rate was 64 percent of the 593 courts identified across the U.S. that met the eligibility requirements of primarily serving adults and being in operation for at least one year at that time. Although national in scope, the sample is not nationally representative. However, it provides an important foundation for understanding drug court programs throughout the country.

we also conducted site visits to the 23 outcome sites. Between February and June 2006, evaluation team members visited each court to interview stakeholders, and conduct observations of drug court staffing meetings and court hearings. Program structure and management, operations, treatment, drug testing, and courtroom practices were assessed through open-ended questions and observations.

The third source of data was three waves of in-person, computer-assisted personal interviews with 1,781 offenders across the 23 drug court and 6 comparison sites (for more information, see Volume 1) conducted at (1) baseline, which reflects the time when individuals enroll in drug courts or are sent to the treatment alternatives in comparison sites, (2) 6 months after the baseline interview, and (3) 18 months after baseline.[32]

Development of Outcome Measures

The two outcomes measures in the current analysis are self-reported number of criminal activities per month and days of drug use per month in the year prior to the 18-month interview. Days per month was used to adjust for street time, meaning that criminal activities and substance use were counted only for days that offenders were free to commit such acts, and not if they were in a situation that was out of their daily routine. Specifically, street time adjustments included incarceration, hospital/emergency room stays (only if they were overnight stays), and participation in residential mental health treatment, residential substance abuse treatment, and residential detoxification services.

We regressed the two dependent variables on a comprehensive set of independent variables. Independent variables in the crime model included: age, race/ethnicity, gender, relationship status, primary drug of abuse, use of aggression drugs, number of relatives and friends (both blood relatives and social ties) who have been convicted of a crime, features of ASPD, number of prior arrests, and number of prior incarceration starts. Independent variables in the drug use model included: age, race/ethnicity, gender, relationship status, primary drug of abuse, age of first use of any illicit drug (including marijuana), user status for all drugs (including heavy alcohol/binge drinking and categories ranged from non-user to regular user), number of relatives and friends with drug or alcohol problems, number of days prior to baseline that individuals spent in drug or alcohol treatment, features of depression, and number of prior arrests.

Both of the dependent variables are count variables that have a number of important properties. Count variables can only take integer values; tend to be skewed so that their distributions are asymmetric and have long positive tails; and, and more importantly, can never be below zero. Based on these properties, analyses requiring the assumptions of normality (such as ordinary least squares or traditional hierarchical linear modeling) would have been inappropriate to use

[32] Eighty-six percent completed the 6-month interviews, and 83 percent completed the 18-month interviews. The interviews lasted between 1.5 and 2 hours, and covered a variety of topics including: background characteristics (demographics, drug use and treatment history, criminal history, physical health, mental health, employment, housing, family conflict and support, and close ties to drug users and those involved in the criminal justice system); in-program behavior (treatment receipt, receipt of other services, supervision intensity, case management, drug testing, noncompliance with regulations, sanctions and rewards received, terminations, graduations); and outcomes (self-reported criminal behavior, drug use, and other personal functioning).

when creating outcomes. Thus, we chose negative binomial regression because it can be seen as a more general form of the Poisson model sometimes used for count data. The negative binomial model allows for between-subject heterogeneity and accounts for some over-dispersion. In this case, however, an excess of over-dispersion remained unaccounted for by this model, so a zero-inflated negative binomial model was adopted.

Estimated coefficients from the comparison group were then applied to drug court participants' characteristics (i.e., their values on the variables listed above), to determine the expected behaviors for each individual had they not been in the drug court program. For each drug court participant, actual outcomes were subtracted from the expected outcomes, resulting in a difference score that indicates the impact of drug court participation on the individual's outcomes—that is, the number of criminal acts curtailed and the number of days of drug use curtailed. For example, a drug court participant's actual observed outcome may be two days of drug use per month. But, the same person's expected outcome had they not been in drug court might be ten days of drug use per month. Thus, this participant's score on number of days of drug use prevented per month is eight days.

Overall, the average number of criminal activities per month prevented across all drug courts was 1.7 (standard deviation=16, range -264 to 32). The average number of days of drug use prevented per month across all drug courts was 1.6 (standard deviation=7, range -33 to 37). As noted above, a court's performance is scored as the average of its participants' outcomes, and then courts are simply ranked based on that performance score. Courts were ranked in general, and then for particular subgroups of participants.

Court Rankings[33]

In order to identify whether the effect of court practices varied across participants, we created 31 subgroups based on client attributes as self-described in the baseline interview. We chose these 31 measures for a several reasons. First, the effectiveness of drug court has been shown to vary based on some individual characteristics, such as participants' substance use histories and criminal histories. Second, we identified individual characteristics that seem to be related to substance use and criminal behavior even if they have not been studied as part of a previous drug court evaluation, hypothesizing that the effectiveness of drug court may vary based on these measures as well. Thus, the 31 subgroups for which rankings were created reflect three broad categories: (1) background characteristics (age 30 and over, under age 30, male, female, in an intimate relationship, not in intimate relationship, having features of depression, not having features of depression, having features of ASPD, and not having features of ASPD); (2) criminal history (no prior arrests, one to four prior arrests, more than four prior arrests, previous incarceration, no previous incarceration, any relatives or friends with a conviction, and no relatives or friends with a conviction); and (3) substance use factors (age of first drug use 15 or younger, age of first drug use over age 15, any substance abuse treatment during the six months before baseline, no substance abuse treatment before baseline, any relatives or friends with drug problems, no relatives or friends with drug problems, primary drug of choice—including alcohol,

[33] Throughout the rankings, each court is represented by a letter rather than court name or other identifying information in order to protect confidentiality.

marijuana, amphetamines, cocaine, and other drugs [heroin, hallucinogens, prescription drugs], drug use of any kind other than marijuana, if ever tried aggression drugs [amphetamines, cocaine], and never tried aggression drugs).

Court Rankings for Crimes Prevented

Table 4-7.1 describes the court rankings, both overall and across the subgroups for crimes prevented. Courts above the red line in each column are courts whose actual outcomes were better than the expected outcomes—that is, effective courts. Courts below the red line are those for which participants' actual outcomes were worse than their expected outcomes. In columns for which no red line is displayed, all courts in the column had positive results.

In each column, letters that are bolded, underlined, and italicized represent the top three courts with the most participants meeting that subgroup criterion. In order to be identified in this way, a court had to have at least 50 percent of its population meeting that criterion. Subgroup columns with no such highlighting indicate that no court had more than 50 percent of its population meeting that criterion. In addition, in order for a court to be included in a particular ranking, it must have contributed a minimum sample size in the outcome study of five participants in that particular subgroup. Therefore, some subgroups include only a small number of courts because other courts did not have at least five individuals meeting this type of criteria (e.g., individuals with features of depression, individuals with no prior arrests, individuals whose primary drug of choice is amphetamines, etc.). All remaining tables have similar structures.

The general ranking indicates that 18 of the 23 courts in our study were effective at preventing crime for their client populations. However, there is substantial variation by subgroups. On average, more courts perform positively for:

- People age 30 and over, compared to younger than 30.
- Males, compared to females.
- Those with one to four prior arrests, compared to those with no prior arrests or those with more than four prior arrests.
- Those with no previous incarceration, compared to those who have been incarcerated before.
- Those with any relatives or friends with a conviction, compared to those with no such relatives or friends.
- Those whose age of first drug use was older than 15, compared to those who were 15 or younger at their first use.
- Those with any relatives or friends with drug problems, compared to those with no such relatives or friends.

Table 4-7.1. Court Rankings for Number of Crimes Prevented at 18 Months

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of ASPD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

Table 4-7.1. Court Rankings for Number of Crimes Prevented at 18 Months (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

Table 4-7.1. Court Rankings for Number of Crimes Prevented at 18 Months (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug : Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines ,	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

Table 4-7.1. Court Rankings for Number of Crimes Prevented at 18 Months (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes. *2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").* *3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.*

We also examined court success for subgroups of clients characterized by primary drug of choice, and courts varied in their success based on these subgroups. Courts were more effective at preventing crime when the clients' primary drugs of choice included alcohol, amphetamines, cocaine, and other drugs (heroin, hallucinogens, and prescription drugs). All courts that have clients with a primary drug of choice that is another drug are effective at preventing future criminal behavior, and all courts but one that have clients whose primary drug of choice is alcohol or is amphetamines have positive outcomes. Drug courts were less effective at preventing crime when the subgroup's primary drug of choice is marijuana. Of the 17 courts with clients whose primary drug of choice is marijuana, only 9 are effective (that is, ranked above the red line).

When looking across columns, it appears that the top-performing courts are effective across a range of client types, although the exact placement of the courts in the rankings varies somewhat across subgroups. For example, Court S is ranked third in the general ranking, second for those age 30 and over, and eighth for those under age 30. In addition, although rankings vary by subgroup, a set of high-performing courts can be identified—with the top courts largely remaining the same across subgroups—and a set of low-performing courts can be identified. The top five courts in the general ranking are G, L, Q, S, and W. Four of these courts appear in the top five courts across subgroups routinely (G is in the top five courts 15 times, Q is in the top five courts 19 times, S is in the top five 19 times, and W is in the top five 18 times). The other court that appears in the top five courts across subgroups is Court D—ranked sixth in the general ranking—and ranked in the top five in 12 subgroups.

Court Rankings for Substance Use Prevented

Table 4-7.2 shows the court rankings in general and across the subgroups for days of substance use prevented. The general ranking indicates that 22 of the 23 courts in our study were effective at preventing future substance use for their client populations, overall. Thus, more drug courts in the MADCE were effective at preventing substance use than criminal behavior.

Again, there is substantial variation across subgroups. With respect to preventing substance use, on average, more courts perform positively for:

- People age 30 and over, compared to younger than 30.
- Males, compared to females.

Table 4-7.2. Court Rankings for Substance Use Prevented at 18 Months

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

Table 4-7.2. Court Rankings for Substance Use Prevented at 18 Months (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

Table 4-7.2. Court Rankings for Substance Use Prevented at 18 Months (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

Table 4-7.2. Court Rankings for Substance Use Prevented at 18 Months (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

- Those with no previous incarceration, compared to those who have been incarcerated before.
- Those with any relatives or friends with a conviction, compared to those with no such relatives or friends.
- Those whose age of first drug use was aged 15 or younger, rather than older.
- Those who had some substance abuse treatment the six months before baseline, compared to those who did not have such treatment.
- Those with any relatives or friends with drug problems, compared to those with no such relatives or friends.

The pattern of court effectiveness based on clients' drugs of choice subgroups for substance use prevented is similar to that found for crimes prevented. Court performance varies based on the primary drugs of choice among clients served by the court. Courts seem effective at preventing crime when the clients' primary drugs of choice include alcohol, amphetamines, cocaine, and other drugs (heroin, hallucinogens, and prescription drugs), but seem less effective at preventing crime for the subgroup of clients whose primary drug of choice is marijuana. All courts that have clients with a primary drug of choice that is another drug are effective at preventing future substance use, all courts but one that have clients whose primary drug of choice is amphetamine have positive outcomes, and the same is true for all but two of the courts with clients who have alcohol as their drug of choice. Of the 17 courts with the subgroup of clients whose primary drug of choice is marijuana, only 14 are effective. Therefore, although not all courts with clients whose primary drug of choice is marijuana are effective; more of these courts are better at preventing substance use than they are at preventing crime.

As with the criminal behavior outcome, although rankings shift somewhat for the substance abuse outcome, it appears that there is a set of high performing courts—with the top courts largely remaining the same across subgroups—and a set of low performing courts. The top five courts in the general ranking are G, I, M, Q, and U, and these same five appear in the top five performing courts across subgroups the most times (G is in the top five courts 14 times, I is in the top five courts 17 times, M is in the top five 24 times, Q is in the top five courts 19 times, and U is in the top five 18 times). Thus, we conclude based on these rankings that the top performing courts at preventing substance use are the same when considering both their overall population served as well as specific client types. In addition, it is important to note that two courts (G and Q) appear in the top 5 for both the crime and substance abuse outcomes.

Results

Below are the results of the analyses for each of the ten policies and practices examined. First, we present how the policy or practice was measured and operationalized in this study. Then we present findings from both the qualitative and quantitative analyses. For each policy and practice, we first describe the results for the criminal behavior outcome, followed by the substance use outcome.

Leverage

Leverage is a construct originally described by the RAND Corporation (Longshore et al. 2001) that measures the coercive power of the drug court. Leverage is considered important because it is believed that the more leverage the court has over an individual, the more likely it is that individual will comply with the drug court requirements and succeed in the program. Data for the leverage measure were collected from telephone interviews conducted before the impact study began. We operationalized leverage based on five factors that were characterized in a series of questions. The responses were scored and summed for an overall leverage score. The five factors were:

1. Case management was conducted by someone who was an actual employee of the drug courts (2 points).

2. Drug court participants regularly participated in court hearings (2 points).

3. The court had explicit consequences for dropping out or failing out (2 points).

4. The client was told about the explicit consequences (1 point).

5. The explicit consequences were in a contract for the client to sign (1 point).

Each court was classified as high (7 or 8 points; 11 courts total), medium (5 or 6 points; 6 courts total), or low (0 to 4 points; 6 courts total).

The qualitative analysis is depicted in Table 4-7.3. It shows the drug court rankings for criminal acts prevented, with courts color-coded based on leverage scores. Purple shading reflects courts classified as being high on leverage, yellow shading indicates courts with medium leverage, and green shading indicates courts with low leverage. Nearly all of the high leverage courts are effective (that is, above the red line). Additionally, many of the high leverage courts are clustered toward the top of the ranks for the crime outcome, indicating that the highest-performing courts have high leverage and lower-performing courts have either low or medium leverage. In addition to this, no medium leverage courts are ineffective.

Table 4-7.3. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Leverage Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of ASPD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

High Leverage   Medium Leverage   Low Leverage

Table 4-7.3. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Leverage Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

High Leverage — Medium Leverage — Low Leverage

Table 4-7.3. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Leverage Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Leverage Medium Leverage Low Leverage

Table 4-7.3. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Leverage Scores (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

The results of the quantitative analysis indicate that high-leverage courts are significantly more effective at preventing crime than low-leverage courts (F=4.15, $p < .05$). No statistically significant differences were found between medium- and high-leverage courts or between low and medium courts for preventing crime. The average number of crimes prevented per month for high-leverage courts was 4.1, compared to 1.4 for low-leverage courts. Medium-leverage courts prevented 2.0 crimes per month.

Table 4-7.4 shows the same leverage coding for court rankings preventing substance use. Again, most of the high-leverage courts are effective courts. However, the positive clustering of high-leverage (purple) courts in the crime table is less pronounced for the substance use outcome. Low- and medium-leverage courts are spread throughout the ranks of effective courts, but no medium-leverage courts are ineffective.

In terms of preventing substance use, marginally statistically significant differences between courts with different levels of leverage were found (F=2.38, $p < .10$). High-leverage courts prevented an average of 2.6 days of substance use per month, medium-leverage courts prevented 3.1 days, and low-leverage courts prevented 1.8 days.

Predictability of Sanctions

Predictability of sanctions measures the extent to which the court communicated to participants how and when they would be sanctioned. A coordinated sanction policy (OJP/NADCP 1997, Goldkamp et al. 2001) and the extent to which participants are: aware of the policy, aware of the consequences for noncompliance, able to predict when a sanction will occur, and able to predict what the specific sanction will be (Longshore et al. 2001) are believed to be related both to a client's likelihood of complying with program requirements and to program success. The concept was measured during process evaluation telephone interviews. We operationalized predictability of sanctions based on three factors:

1. The court maintained an official schedule of sanctions (2 points).

2. Clients were provided with the official schedule of sanctions (2 points).

3. The official schedule of sanctions was always, or almost always, followed by the court (2 points).

Table 4-7.4. Court Rankings for Substance Use Prevented at 18 Months: Coded for Leverage Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of ASPD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

High Leverage | Medium Leverage | Low Leverage

Table 4-7.4. Court Rankings for Substance Use Prevented at 18 Months: Coded for Leverage Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

High Leverage   Medium Leverage   Low Leverage

Table 4-7.4. Court Rankings for Substance Use Prevented at 18 Months: Coded for Leverage Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Leverage Medium Leverage Low Leverage

Table 4-7.4. Court Rankings for Substance Use Prevented at 18 Months: Coded for Leverage Scores (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

The responses were scored and summed for an overall score rating predictability of the sanction policies, and each court was classified as high (6 points; 9 courts total), medium (3 to 5 points; 4 courts total), or low (0 to 2 points; 10 courts total).

Table 4-7.5 ranks drug court effectiveness at preventing crime with courts color-coded based on predictability of sanctions scores. Purple shading is used to indicate courts classified as having high predictability of sanctions, yellow shading indicates courts with medium predictability, and green shading indicates courts with low predictability. All but one of the medium predictability courts and many of the low predictability courts were more successful than predicted. Additionally, the high predictability of sanctions courts are dispersed throughout the ranks of effective courts and clustered below the red line.

The results of the quantitative analysis indicate that, for the overall model, statistically significant differences exist between courts at various levels of predictability of sanctions (F=3.31, $p < .05$). However, the follow-up Tukey tests of between-group differences were not able to identify which groups were significantly different from one another. This is likely because Tukey tests of between-group comparisons are a conservative method for identifying group differences. However, reviewing the means for each group indicates that the medium-predictability-of-sanctions courts are most effective at preventing future crimes, followed by the low-predictability courts. The average number of crimes prevented per month for medium-predictability-of-sanctions courts was 4.3, compared to 3.9 for low-predictability courts and 1.8 for high-predictability courts. Nearly all medium-predictability-of-sanctions courts were among effective courts, and courts with high predictability of sanctions were generally ineffective courts.

Table 4-7.6 ranks drug court effectiveness at preventing substance use, conditioned on predictability of sanctions. Our qualitative analysis shows a similar pattern holds here as in Table 4-7.5 when it comes to the high-predictability-of-sanctions courts. However, for the drug outcome, all of the medium-predictability-of-sanctions courts are effective and clustered toward the top of the rankings. Low-predictability-of-sanctions courts are dispersed throughout the rankings.

In terms of preventing substance use, medium-predictability-of-sanctions courts prevented significantly more days of substance use than court with high predictability of sanctions (F=4.32, $p < .05$). Medium-predictability-of-sanctions courts prevented an average of 4.1 days of

Table 4-7.5. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Predictability of Sanctions Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

High Predictability of Sanctions | Medium Predictability of Sanctions | Low Predictability of Sanctions

Table 4-7.5. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Predictability of Sanctions Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

High Predictability of Sanctions — Medium Predictability of Sanctions — Low Predictability of Sanctions

Table 4-7.5. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Predictability of Sanctions Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Predictability of Sanctions | Medium Predictability of Sanctions | Low Predictability of Sanctions

Table 4-7.6. Court Rankings for Substance Use Prevented at 18 Months: Coded for Predictability of Sanctions Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

High Predictability of Sanctions   Medium Predictability of Sanctions   Low Predictability of Sanctions

Table 4-7.6. Court Rankings for Substance Use Prevented at 18 Months: Coded for Predictability of Sanctions Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

High Predictability of Sanctions — Medium Predictability of Sanctions — Low Predictability of Sanctions

Table 4-7.6. Court Rankings for Substance Use Prevented at 18 Months: Coded for Predictability of Sanctions Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Predictability of Sanctions — Medium Predictability of Sanctions — Low Predictability of Sanctions

Notes below apply to Tables 4-7.5 and 4-7.6.

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

substance use per month, compared to 2.0 for high-predictability courts. Low-predictability courts prevented 2.7 days of substance use.

Adherence to Treatment Best Practices

The provision of treatment is considered a core aspect of the drug court model (OJP/NADCP 1997). In order to be included in the MADCE study, the court had to have reported they provided some type of substance abuse treatment to their program participants. In an attempt to understand the quality of the treatment being provided, we asked a short series of questions during telephone interviews with potential sites as part of the evaluation site selection. These questions do not cover a full set of best practices for treatment provision, but did allow us to capture a picture of the treatment being provided. Thus, we operationalized adherence to treatment best practices based on the following five factors:

1. The treatment provided by the drug court was structured—that is, a treatment program manual was followed (2 points).

2. A clinical assessment was conducted for treatment needs (1 point).

3. Individualized treatment plans were developed for each client (1 point).

4. Individualized treatment plans were used to make referrals (1 point).

5. Individualized treatment plans were updated periodically (1 point).

The responses were scored and summed for an overall score of adherence to best practices and each court was classified as high (6 points; 15 courts total), medium (4 to 5 points; 6 courts total), or low (0 to 3 points; 2 courts total).

Table 4-7.7 shows the drug court rankings for preventing criminal acts with courts color-coded based on adherence to treatment best practices scores. Purple shading indicates courts with high adherence to best practices, yellow shading indicates courts with medium adherence, and green shading indicates courts with low adherence. Looking across columns, no clear patterns are evident. Although three medium-adherence courts appear toward the top of the rankings for

Table 4-7.7. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

High Adherence to Treatment Best Practices (purple) | Medium Adherence (yellow) | Low Adherence (green)

Table 4-7.7. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

High Adherence to Treatment Best Practices — Medium Adherence — Low Adherence

Table 4-7.7. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Adherence to Treatment Best Practices — Medium Adherence — Low Adherence

Table 4-7.7. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

several subgroups, this is not true across all subgroups, and a different set of medium-adherence courts tend to be consistently toward the bottom of the ranks. Also, neither of the two low-adherence courts is ineffective. Further, no statistically significant differences between low-, medium-, and high-adherence courts were found for crimes prevented during the quantitative analysis.

Table 4-7.8 shows the drug court rankings for preventing substance use with courts color-coded based on adherence to treatment best practices scores. The same absence of a pattern is evident here as for the analysis of the crime outcome. Similarly, no statistically significant differences between low-, medium-, and high-adherence courts were found for substance use prevented.

Limited variation on the adherence to treatment best practices measure may have restricted our ability to detect differences in outcomes based on this. One striking difference between Tables 4-7.7 and 4-7.8, compared to Tables 4-7.3, 4-7.4, 4-7.5, and 4-7.6, is that there is little variation as 15 of the 23 courts were coded as high adherence to treatment best practices.

Drug Testing

Routine drug testing to examine compliance with drug use requirements is an important element of the drug court model (OJP/NADCP 1997), and we included it in our conceptual framework as a critical drug court practice for individuals. The policy for the purpose of this analysis was measured during the MADCE Adult Drug Court Survey and was operationalized by a survey question about the frequency of drug testing during Phase 1 (or first two months) of each court's program. Each court was classified as high frequency of drug tests (more than one time per week; 19 courts total), medium (one time per week; 4 courts total), or low (less than one time per week or not at all; zero courts)

The results for frequency of drug testing during the first two months of the program mirror the results for adherence to best practices. Table 4-7.9 shows the drug court rankings for preventing criminal acts with courts coded based on frequency of drug testing scores; Table 4-7.10 shows the same for preventing substance use. Purple shading reflects courts that drug test more than once per week, yellow shading indicates courts that drug test once per week, and green shading indicates courts that drug test less than once weekly or not at all. Notably, 19 courts administer drug tests to clients more than one time per week, and four courts drug test clients once per

Table 4-7. 8. Court Rankings for Substance Use Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

High Adherence to Treatment Best Practices — Medium Adherence — Low Adherence

Table 4-7.8. Court Rankings for Substance Use Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

High Adherence to Treatment Best Practices — Medium Adherence — Low Adherence

Table 4-7.8. Court Rankings for Substance Use Prevented at 18 Months: Coded for Adherence to Treatment Best Practices Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Adherence to Treatment Best Practices | Medium Adherence | Low Adherence

Table 4-7.9. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Drug Test Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

(purple)	More than one time per week	(yellow)	One time per week
(green)	Less than one time per week or not at all		

Table 4-7.9. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Drug Test Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

More than one time per week — One time per week — Less than one time per week or not at all

Table 4-7.9. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Drug Test Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

More than one time per week | One time per week | Less than one time per week or not at all

Table 4-7.10. Court Rankings for Substance Use Prevented at 18 Months: Coded for Drug Test Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

More than one time per week | One time per week | Less than one time per week or not at all

Table 4-7.10. Court Rankings for Substance Use Prevented at 18 Months: Coded for Drug Test Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

More than one time per week | One time per week | Less than one time per week or not at all

Table 4-7.10. Court Rankings for Substance Use Prevented at 18 Months: Coded for Drug Test Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

More than one time per week — One time per week — Less than one time per week or not at all

Notes below apply to Tables 4-7.8,4-7.9, and 4-7.10.

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

week. No courts administer drug tests less frequently. Looking across columns, the mostly purple shading indicates the lack of variation on this measure. Nearly all courts have similar frequency for administering drug tests. In addition, the results of the quantitative analyses indicate no statistically significant differences between courts coded for drug test frequency for either crimes prevented or substance use prevented. Thus, the lack of variation in implementation of the frequency of drug testing makes it difficult to adequately assess the effectiveness of this component.

Case Management

All drug courts in the MADCE sample have case managers who interact with clients to oversee their progress in the drug court program and assist them in accessing necessary services. We are interested in understanding if the frequency of contact with case managers is related to program success. The policy for the purpose of this analysis was measured during the MADCE Adult Drug Court Survey and was operationalized by a survey question about the frequency that clients saw case managers during Phase 1 (or the first two months) of the program. Each court was classified as high frequency (more than one time per week; 6 courts total), medium (one time per week; 13 courts total), or low (less than one time per week or not at all; 4 courts total).

Table 4-7.11 shows the drug court rankings for preventing criminal acts, with courts color-coded based on frequency of case management during the first two months of the program. Purple shading indicates courts that require meetings more than one time per week, yellow shading indicates courts that require meetings once per week, and green shading indicates courts that require meetings less than once weekly or not at all. No strong pattern is evident based on frequency of case management contacts, but some configurations seem to exist. Most of the courts in which clients meet with their case managers more than once per week are effective. All but a couple of the courts that have case managers meeting with clients less than once weekly or not at all are ineffective or among lower-performing courts. Courts that have case managers meet with clients one time per week are dispersed throughout the ranks, both above and below the red line, and are represented in the top two courts for several of the subgroups.

Although no clear patterns were identified based on the qualitative coding, the results of the quantitative analyses show evidence of some relationships between frequency of case management and court effectiveness. In terms of preventing criminal acts, the model was marginally significant (F=2.84, $p < .10$). Courts with case managers who met with clients more than one time per week prevented more criminal acts per month than those where case managers met with clients less than once weekly or not at all. Courts with case management meetings

Table 4-7.11. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Case Management Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

More than one time per week (purple) | One time per week (yellow) | Less than one time per week or not at all (green)

Table 4-7.11. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Case Management Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

More than one time per week — One time per week — Less than one time per week or not at all

Table 4-7.11. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Case Management Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

More than one time per week — One time per week — Less than one time per week or not at all

Table 4-7.11. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Case Management Scores (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

more than one time per week prevented an average of 4.3 criminal acts per month as compared to 1.2 acts for courts with case management meetings less than once per week or not at all. Courts with case management meetings one time per week prevented an average of 3.0 criminal acts per month.

Table 4-7.12 shows the drug court rankings for preventing substance use with courts color-coded based on frequency of case management. As with the crime outcome, no clear pattern of shading is apparent. Many of the courts that have case managers meet with clients more than once per week are effective, and all of the courts that have clients meet with case managers less than once per week or not at all are as well. Courts that have case managers meet with clients one time per week are dispersed throughout the rankings.

The quantitative analysis testing prevention of substance use shows marginally statistically significant differences between courts with different frequencies of case management meetings (F=2.50, p < .10). Courts with case management meetings more than one time per week prevented an average of 3.0 days of substance use per month, courts with case management meetings one time per week prevented an average of 2.1 days of substance use, and courts with case management meetings less than one time per week or not at all prevented 3.2 days of substance use. Notably, courts that have infrequent case management meetings tend to rely on treatment providers to do more of this type of work, with perhaps the client seeing the treatment provider more times per week in that setting than in other programs. This might explain why the courts with both high and low frequency of case management meetings prevent about the same numbers of days of drug use.

Judicial Status Hearings

Regular contact between drug court participants and the drug court judge is considered an essential aspect of the drug court model (OJP/NA 1997, Longshore et al. 2001) and the contact between participant and judge is thought to be an essential catalyst to program compliance and success. As such, we included it in the MADCE conceptual framework as an important drug court practice. The practice for the purpose of this analysis was measured through questions asked during process evaluation site visits, and was operationalized as average frequency of judicial status hearings each month. Each court was classified as high (four times per month; 16 courts total), medium (two times per month; 4 courts total), or low (one time per month; 1 court). Two courts were missing data on this variable (see Table 4-7.13).

Table 4-7.12. Court Rankings for Substance Use Prevented at 18 Months: Coded for Case Management Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

More than one time per week | One time per week | Less than one time per week or not at all

Table 4-7.12. Court Rankings for Substance Use Prevented at 18 Months: Coded for Case Management Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

More than one time per week | One time per week | Less than one time per week or not at all

Table 4-7.12. Court Rankings for Substance Use Prevented at 18 Months: Coded for Case Management Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug:	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

More than one time per week | One time per week | Less than one time per week or not at all

Table 4-7.13. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Status Hearing Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

Four times per month   Two times per month   One time per month

Table 4-7.13. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Status Hearing Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

Four times per month | Two times per month | One time per month

Table 4-7.13. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Status Hearing Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

Four times per month | Two times per month | One time per month

Notes below apply to Tables 4-7.12 and 4-7.13.

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

The results for frequency of judicial status hearings mirror the results for the other low variability practices (i.e., drug testing and adherence to treatment best practices). Table 4-7.13 shows the drug court rankings for preventing criminal acts with courts coded based on frequency of judicial status hearings, and Table 4-7.14 shows the same for preventing substance use. Purple shading is used to represent courts that have judicial status hearing for clients four times per month, on average. Yellow shading indicates courts that have hearings twice per month, and green shading indicates courts that have hearings once per month. Notably, the shading is mostly purple as 16 courts have judicial status hearings for clients four times per month. Most of these courts, as well as the four courts that provide status hearings twice monthly, are effective. In addition, the results of the quantitative analyses indicate no statistically significant differences between courts coded for frequency of judicial status hearings. Once again, the lack of variation in this drug court practice makes it difficult to adequately assess the effects of drug courts based on frequency of judicial status hearings.

Point of Entry into Drug Court Program

Both the Temple University (Goldkamp et al. 2001) and RAND Corporation (Longshore et al. 2001) conceptual frameworks identify the point in the criminal justice process at which clients enter the drug court program—either pre- or post-plea—as important to the drug court model. The RAND framework identifies it as an aspect of leverage, while the Temple University framework describes it as a court processing focus. Like the Temple University framework, MADCE incorporates this concept as its own unique feature of the model. When in the criminal justice process clients enter the program may have implications for how well they perform in the program and their ability to achieve success. We measured this concept through a series of questions asked during process evaluation site visits about what point in the criminal justice process clients entered into the drug court program. We operationalized the concept here as pre-plea entry—diversion strategies—and post-plea entry—in which convictions stand or are lessened after completion of the program. Courts were classified as those that were pre-plea (meaning all clients entered as part of a diversion strategy; 7 courts total), a combination (meaning courts in which some clients came into the program pre-plea and some came in post-plea; 6 courts total), or post-plea (meaning all clients came into the program post-plea; 10 courts total).

Table 4-7.15 shows the drug court rankings for preventing criminal acts with courts coded based on the point in the criminal justice system process at which clients enter the drug court program. Purple shading indicates pre-plea courts, yellow shading indicates combination courts—some

Table 4-7.14. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Status Hearing Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

Four times per month | Two times per month | One time per month

Table 4-7.14. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Status Hearing Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

Four times per month — Two times per month — One time per month

Table 4-7.14. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Status Hearing Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

Four times per month   Two times per month   One time per month

Table 4-7.15. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for When Client Enters Program

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

All pre-plea — Some pre-plea, some post-plea — All post-plea

Table 4-7.15. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for When Client Enters Program (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

All pre-plea | Some pre-plea, some post-plea | All post-plea

Table 4-7.15. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for When Client Enters Program (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

All pre-plea — Some pre-plea, some post-plea — All post-plea

Notes below apply to Tables 4-7.14 and 4-7.15.

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

clients come into the program pre-plea and some that come in after making a plea, and green shading indicates post-plea courts. Pre- and post-plea courts seem to cluster toward the upper portions of the rankings across subgroups. Combination courts—those with a mixed pool of clients—seem to be more dispersed throughout the rankings and most of the ineffective courts are combination courts. Thus, courts that have one point of entry into their program seem to be more effective at preventing crime than those that allow multiple points of entry.

The quantitative analysis supports this claim. Statistically significant differences (F=7.42, $p <$ .05) exist between courts in which all the clients come into the program pre-plea and combination courts. Also, significant differences exist between post-plea courts and combined courts. The average number of crimes prevented per month for pre-plea courts was 4.6, the average number of crimes prevented for post-plea courts was 3.6, and the average number of crimes prevented for combined courts was 0.8.

Table 4-7.16 shows the same coding for point of entry in the program for the rankings on preventing substance use. A similar pattern holds here as in Table 4-7.15 when it comes to the shading. Courts that have one type of point of entry into their program seem to be more effective at preventing substance use. Additionally, courts with clients who enroll post-plea prevent significantly more days of drug use per month than combined courts that have a mix clients coming in both pre- and post-plea (F=3.88, $p < .05$). The average number of days of drug use prevented per month for post-plea courts was 3.0 compared to 1.7 for combined courts. The average number of days of drug use prevented per month for pre-plea courts was 2.9.

Multidisciplinary Team Decision Making

The foundation of the drug court model includes an interdisciplinary team of interested parties, including court staff, treatment staff, prosecutors, defense attorneys, etc. (OJP/NADCP 1997). The MADCE hypothesized that the extent that team members participated in team activities in a collaborative manner—that is, the extent to which members attend and participate in team staffings and decisions about specific clients—may affect program outcomes. Thus, during the process evaluation site visits, we observed interactions among team members during court staffing meetings. Multidisciplinary team decision-making was operationalized by the attendance and level of participation of the following stakeholders at court staffings: judges, prosecutors, defense attorneys, program coordinators, case managers, probation officers, treatment liaison staff, and other stakeholders. Scores of one to five were assigned to each stakeholder (with 0 points assigned if the stakeholder did not attend the staffing) and the scores were summed to reflect overall participation from various stakeholders. Each court was

Table 4-7.16. Court Rankings for Substance Use Prevented at 18 Months: Coded for When Client Enters Program

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

All pre-plea | Some pre-plea, some post-plea | All post-plea

Table 4-7.16. Court Rankings for Substance Use Prevented at 18 Months: Coded for When Client Enters Program (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

All pre-plea | Some pre-plea, some post-plea | All post-plea

Table 4-7.16. Court Rankings for Substance Use Prevented at 18 Months: Coded for When Client Enters Program (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

All pre-plea — Some pre-plea, some post-plea — All post-plea

Table 4-7.16. Court Rankings for Substance Use Prevented at 18 Months: Coded for When Client Enters Program (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

classified as high (23-25 points; 8 courts total), medium (18-22 points; 6 courts total), or low (15-17 points; 6 courts total). Three courts were not scored due to missing data.

Table 4-7.17 shows the drug court rankings for preventing criminal acts with courts color-coded based on multidisciplinary team decision-making practices; Table 4-7.18 shows the same for preventing substance use. Purple shading represents courts classified as being high in multidisciplinary decision-making, yellow shading indicates courts that have medium multidisciplinary decision-making, and green shading indicates low. For both outcomes, the purple, yellow, and green shading is dispersed throughout the ranks, and no clear patterns of shading are evident. In addition, the results of the quantitative analyses indicate no statistically significant differences between courts based on coding for multidisciplinary decision making for either preventing crime or substance use. Thus, multidisciplinary team decision making does not seem to be directly related to outcomes for clients.

Positive Judicial Attributes

The Temple University (Goldkamp et al. 2001), RAND Corporation (Longshore et al. 2001), and MADCE conceptual frameworks all include courtroom dynamics and interactions with judges as important factors of the drug court experience for program clients. The idea is that clients develop a relationship with the judge, and the extent to which clients see this relationship as constructive may contribute to their program compliance and success. One way MADCE tried to capture the nature of this is through a measure of positive judicial attributes. Positive judicial attributes were measured during process evaluation site visit observations of drug court hearings. They reflect the site visit team's impressions of the judges on various attributes, based on the team's observations of the judges' actions and demeanor toward the clients during court proceedings. The site visit team assigned drug court judges a value of one to five on each of the following dimensions: respectful, fair, attentive, enthusiastic, consistent/predictable, caring, and knowledgeable. The ratings for each judge were summed, and divisions were created to classify the courts into three approximately equal categories. Seven courts were classified as low, based on summary values of 26 points or fewer. Seven courts were classified as medium, based on summary values of 27 to 29 points. Eight courts were classified as high, based on summary values of 30 or more points.

Table 4-7.19 shows the drug court rankings for preventing criminal acts with courts coded based on positive attributes of the drug court judge (as documented during court observations). Purple shading represents courts with high scores on positive judicial attributes, yellow shading

Table 4-7.17. Court Rankings for Number of Crimes Prevented at 18 Months: Coded for Multidisciplinary Decision Making Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

High Multidisciplinary Decision Making

Medium Multidisciplinary Decision Making

Low Multidisciplinary Decision Making

Table 4-7.17. Court Rankings for Number of Crimes Prevented at 18 Months: Coded for Multidisciplinary Decision Making Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

High Multidisciplinary Decision Making | Medium Multidisciplinary Decision Making | Low Multidisciplinary Decision Making

Table 4-7.17. Court Rankings for Number of Crimes Prevented at 18 Months: Coded for Multidisciplinary Decision Making Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Multidisciplinary Decision Making

Medium Multidisciplinary Decision Making

Low Multidisciplinary Decision Making

Table 4-7.18. Court Rankings for Substance Use Prevented at 18 Months: Coded for Multidisciplinary Decision Making Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

High Multidisciplinary Decision Making | Medium Multidisciplinary Decision Making | Low Multidisciplinary Decision Making

Table 4-7.18. Court Rankings for Substance Use Prevented at 18 Months: Coded for Multidisciplinary Decision Making Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

High Multidisciplinary Decision Making

Medium Multidisciplinary Decision Making

Low Multidisciplinary Decision Making

Table 4-7.18. Court Rankings for Substance Use Prevented at 18 Months: Coded for Multidisciplinary Decision Making Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines ,	Never Tried Aggression Drugs (amphetamines ,
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Multidisciplinary Decision Making | Medium Multidisciplinary Decision Making | Low Multidisciplinary Decision Making

Table 4-7.19. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Attributes Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

High Judicial Attributes — Medium Judicial Attributes — Low Judicial Attributes

Table 4-7.19. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Attributes Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

High Judicial Attributes | Medium Judicial Attributes | Low Judicial Attributes

Table 4-7.19. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Attributes Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Judicial Attributes | Medium Judicial Attributes | Low Judicial Attributes

Notes below apply to Tables 4-7.17, 4-7.18 and 4-7.19.

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

indicates courts with medium scores on positive judicial attributes, and green shading indicates courts with low scores on positive judicial attributes. Across several subgroups, courts with high and medium scores on positive judicial attributes seem to cluster toward the upper portions of the rankings. Those with low scores on positive judicial attributes seem to cluster toward the bottom of the ranks with a few exceptions. Based on shading, it seems that courts with high and medium scores on positive judicial attributes are more likely to be among top performing courts than among ineffective courts.

The results of the quantitative analysis reveal statistically significant differences between courts depending on how they are coded for positive judicial attributes (F=5.81, $p < .05$). Significant differences exist between courts with high scores on positive judicial attributes and courts with low scores on positive judicial attributes. Also, significant differences exist between courts with medium scores on positive judicial attributes and courts with low scores on positive judicial attributes. The average number of crimes prevented per month for courts with high scores on positive judicial attributes was 3.6, the average number of crimes prevented for courts with medium scores on positive judicial attributes was 4.2, and the average number of crimes prevented for courts with low scores on positive judicial attributes was 0.7.

Table 4-7.20 shows the same coding for positive judicial attributes as in Table 4-7.19 for the rankings on preventing substance use. As with crimes prevented, a similar pattern holds for preventing substance when it comes to the shading, but less pronounced. In this case, green shading seems more dispersed among the rankings. In terms of the quantitative analysis, courts with high scores on positive judicial attributes were found to prevent significantly more days of drug use per month than courts with low scores on positive judicial attributes (F=3.16, $p < .05$). The average number of days of drug use prevented per month for courts with high scores on positive judicial attributes was 3.2, compared to 1.9 for courts with low scores on positive judicial attributes. The average number of days of drug use prevented per month for courts with medium scores on positive judicial attributes was 2.6.

Judicial Interaction

In addition to positive judicial attributes, the MADCE team created a second measure to capture interaction between drug court clients and judges, and courtroom dynamics. Judicial interaction was measured during process evaluation site visit observations of drug court hearings and reflects the site visit team's documentation of the frequency with which the drug court judge engaged in various interactive behaviors during the court session. For each case reviewed by the judge during the session, the site visit team documented the following actions: whether the judge made regular eye contact with the defendant (for most of the appearance); whether the judge

Table 4-7.20. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Attributes Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

High Judicial Attributes — Medium Judicial Attributes — Low Judicial Attributes

Table 4-7.20. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Attributes Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

High Judicial Attributes | Medium Judicial Attributes | Low Judicial Attributes

Table 4-7.20. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Attributes Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Judicial Attributes | Medium Judicial Attributes | Low Judicial Attributes

Table 4-7.20. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Attributes Scores (Cont'd)

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

talked directly to the defendant (as opposed to through the defendant's attorney); whether the judge asked non-probing questions (e.g., yes/no or other questions eliciting one-word answers); whether the judge asked probing questions; whether the judge imparted instructions or advice; whether the judge explained the consequences of future compliance (e.g., phase advancements, graduation, etc.); whether the judge explained consequences of future noncompliance (e.g., jail or other legal consequences); and whether the judge allowed the defendant to ask questions or make statements. For each of these eight actions, a variable was created reflecting whether the judge engaged in that action for more than 50 percent of his/her cases that were observed during the session. Then, a simple count was created, which reflected the total number of actions that the judge regularly displayed (i.e., actions that the judge displayed for more than 50 percent of his or her cases). The courts were then assigned a value of low, medium, or high based on this count measure, with the cut points selected to create relatively even numbers of courts in each category. Seven courts were classified as low judicial interaction, based on the judge engaging in three or fewer actions. Seven courts were classified as medium judicial interaction, based on the judge engaging in four or five actions. Six courts were classified as high judicial interaction, based on the judge engaging in six or more actions.

Table 4-7.21 shows the drug court rankings for preventing criminal acts with courts coded based on the extent of judicial interaction that takes place during court hearings, and Table 4-7.22 shows the same for preventing substance use. Purple shading represents courts that were classified as having high judicial interaction, yellow shading indicates courts that have medium judicial interaction, and green shading indicates low judicial interaction. For both outcomes, high, medium, and low judicial interaction courts are dispersed throughout the ranks, and no clear patterns of shading are evident. In addition, the results of the quantitative analyses did not demonstrate any statistically significant differences between courts coded for different levels of judicial interaction and the outcomes of interest. Thus, the extent of judicial interaction during status hearings does not seem to be directly related to preventing future crime and substance use for clients.

Table 4-7.21. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Interaction Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	W	***W***	Q	Q	W	***Q***	D	E	T	Q	W
2	Q	***S***	M	W	S	W	S	R	E	W	L
3	S	***G***	G	G	Q	G	W	A	//	G	S
4	G	Q	L	***L***	I	T	I	//	//	D	***G***
5	L	L	D	D	V	V	M	//	//	S	Q
6	D	V	V	B	M	M	***L***	//	//	M	V
7	M	D	T	M	T	S	V	//	//	V	D
8	V	B	S	S	U	N	K	//	//	L	M
9	T	R	U	V	G	L	G	//	//	T	N
10	N	N	K	K	O	D	B	//	//	R	***I***
11	I	I	I	R	R	O	***R***	//	//	I	B
12	R	M	N	N	C	R	N	//	//	N	K
13	B	T	O	T	K	***I***	T	//	//	O	T
14	K	K	R	E	E	B	E	//	//	B	E
15	O	O	E	I	B	E	J	//	//	K	U
16	E	J	B	O	***P***	K	O	//	//	J	O
17	F	E	J	***J***	A	A	C	//	//	E	R
18	J	A	***P***	F	//	J	P	//	//	C	P
19	C	C	C	C	//	***U***	U	//	//	A	***F***
20	U	U	***H***	A	//	H	F	//	//	P	J
21	P	F	A	U	//	C	A	//	//	U	C
22	A	H	//	***H***	//	P	***H***	//	//	H	A
23	H	//	//	//	//	//	//	//	//	//	H

High Judicial Interaction — Medium Judicial Interaction — Low Judicial Interaction

Table 4-7.21. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Interaction Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	W	R	L	W	I	Q	W	T	G	W	I	***Q***
2	Q	S	D	G	W	S	***Q***	V	S	***Q***	***W***	G
3	S	Q	***M***	S	O	D	***S***	K	W	***D***	S	T
4	G	P	N	***L***	S	M	G	M	Q	L	***L***	S
5	L	D	V	M	Q	V	D	O	V	S	M	***D***
6	D	O	Q	V	T	G	***L***	P	L	M	G	V
7	M	A	T	T	K	W	V	I	I	T	K	L
8	V	H	K	***J***	R	F	***M***	B	M	G	N	U
9	T	J	C	B	V	***L***	R	H	N	V	O	M
10	N	K	U	I	M	***N***	I	C	R	K	E	B
11	I	T	S	K	E	U	E	A	T	B	***R***	N
12	R	//	G	R	C	T	T	E	O	C	H	R
13	B	//	I	E	A	B	***N***	J	B	N	A	K
14	K	//	B	O	U	K	B	R	E	R	P	O
15	O	//	E	***F***	//	R	J	//	K	I	B	E
16	E	//	O	U	//	I	K	//	***F***	O	C	J
17	F	//	R	C	//	E	O	//	A	E	J	***F***
18	J	//	A	A	//	O	C	//	***P***	***J***	T	***C***
19	C	//	P	P	//	***J***	F	//	U	U	U	I
20	U	//	J	H	//	A	A	//	C	A	//	A
21	P	//	H	//	//	C	P	//	***H***	P	//	P
22	A	//	//	//	//	P	U	//	J	H	//	H
23	H	//	//	//	//	H	H	//	//	//	//	//

High Judicial Interaction; Medium Judicial Interaction; Low Judicial Interaction

Table 4-7.21. Court Rankings for Number of Criminal Acts Prevented at 18 Months: Coded for Judicial Interaction Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	W	Q	T	M	S	***V***	Q	M	Q	Q	A
2	Q	W	F	I	T	U	S	K	***M***	W	I
3	S	S	O	G	Q	W	M	T	G	S	O
4	G	D	P	L	G	S	W	E	W	G	K
5	L	G	C	***N***	B	T	K	R	V	***D***	***P***
6	D	***L***	I	C	K	D	R	O	***D***	L	E
7	M	***M***	K	J	V	R	L	S	S	M	C
8	V	V	H	A	O	//	E	P	I	***V***	J
9	T	E	R	T	M	//	J	//	L	N	//
10	N	I	A	K	R	//	I	//	***N***	T	//
11	I	***N***	E	//	I	//	V	//	E	E	//
12	R	T	J	//	***P***	//	B	//	R	I	//
13	B	R	//	//	E	//	T	//	T	K	//
14	K	K	//	//	C	//	O	//	K	B	//
15	O	B	//	//	A	//	A	//	J	***R***	//
16	E	J	//	//	J	//	C	//	B	O	//
17	F	O	//	//	U	//	H	//	O	J	//
18	J	C	//	//	//	//	U	//	P	P	//
19	C	A	//	//	//	//	//	//	C	C	//
20	U	U	//	//	//	//	//	//	U	F	//
21	P	P	//	//	//	//	//	//	A	U	//
22	A	H	//	//	//	//	//	//	F	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Judicial Interaction | Medium Judicial Interaction | Low Judicial Interaction

Table 4-7.22. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Interaction Scores

	General Ranking	Age 30 and Over	Under Age 30	Male	Female	In Intimate Relationship	Not in Intimate Relationship	Features of Depression	No Features of Depression	Features of APSD	No Features of ASPD
1	G	M	G	G	M	G	D	E	E	G	L
2	M	B	U	Q	W	***U***	I	R	T	D	U
3	Q	I	Q	U	S	M	M	A	//	Q	M
4	U	Q	D	M	U	***Q***	U	//	//	M	Q
5	I	L	M	V	I	***I***	S	//	//	U	***I***
6	D	N	S	I	Q	T	V	//	//	S	N
7	S	U	V	K	T	W	***L***	//	//	I	***G***
8	L	C	I	T	***P***	S	N	//	//	V	V
9	F	***G***	K	***L***	G	V	C	//	//	C	***F***
10	V	***S***	L	F	V	B	O	//	//	T	T
11	C	***W***	T	C	O	K	G	//	//	K	C
12	T	T	***P***	S	R	D	K	//	//	W	W
13	W	V	C	B	C	P	W	//	//	L	B
14	K	O	***H***	D	E	L	T	//	//	O	S
15	N	R	O	E	B	C	J	//	//	P	E
16	B	J	A	W	A	E	B	//	//	R	K
17	P	E	N	O	K	N	***R***	//	//	H	P
18	O	D	E	N	//	R	P	//	//	B	O
19	E	K	R	R	//	A	E	//	//	A	D
20	R	A	J	***J***	//	H	F	//	//	N	R
21	J	F	B	A	//	O	A	//	//	J	J
22	A	H	//	***H***	//	J	***H***	//	//	E	A
23	H	//	//	//	//	//	//	//	//	//	H

High Judicial Interaction | Medium Judicial Interaction | Low Judicial Interaction

Table 4-7.22. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Interaction Scores (Cont'd)

	General Ranking	No Prior Arrests	One to Four Prior Arrests	More Than Four Prior Arrests	Previous Incarceration	No Previous Incarceration	Any Relatives or Friends with a Conviction	No Relatives or Friends with a Conviction	Age of First Drug Use: 15 or Younger	Age of First Drug Use: Over Age 15	Any Substance Abuse Treatment Before Baseline	No Treatment Before Baseline
1	G	S	Q	G	I	U	G	T	U	G	I	U
2	M	D	U	U	O	Q	***Q***	V	M	L	C	G
3	Q	P	***M***	M	W	M	I	O	Q	***Q***	***L***	M
4	U	R	V	I	Q	F	***M***	I	G	M	S	***Q***
5	I	Q	C	***L***	M	G	U	B	I	I	M	T
6	D	J	K	P	T	S	***S***	C	S	W	G	***D***
7	S	H	L	T	K	D	D	P	V	S	***W***	V
8	L	A	T	S	C	V	C	K	***F***	T	E	B
9	F	O	D	K	S	I	***L***	A	C	***D***	N	S
10	V	T	S	V	R	***L***	V	E	T	U	P	***F***
11	C	K	N	W	E	C	T	R	W	C	K	***C***
12	T	//	G	A	V	T	W	J	L	K	O	I
13	W	//	B	***J***	U	***N***	K	H	K	V	U	K
14	K	//	I	C	A	K	F	//	***P***	B	***R***	R
15	N	//	O	B	//	P	E	//	A	N	T	L
16	B	//	E	E	//	B	B	//	O	R	H	J
17	P	//	P	***F***	//	O	J	//	E	O	A	O
18	O	//	R	O	//	E	R	//	B	P	J	P
19	E	//	A	R	//	W	***N***	//	R	E	B	E
20	R	//	J	H	//	R	P	//	***H***	***J***	//	A
21	J	//	H	//	//	A	O	//	//	A	//	H
22	A	//	//	//	//	***J***	A	//	//	H	//	//
23	H	//	//	//	//	H	H	//	//	//	//	//

High Judicial Interaction

Medium Judicial Interaction

Low Judicial Interaction

Table 4-7.22. Court Rankings for Substance Use Prevented at 18 Months: Coded for Judicial Interaction Scores (Cont'd)

	General Ranking	Any Relatives or Friends with Drug Problems	No Relatives or Friends with Drug Problems	Primary Drug: Alcohol	Primary Drug: Marijuana	Primary Drug: Amphetamines	Primary Drug: Cocaine	Primary Drug: Other Drugs (heroin, hallucinogens, prescription drugs)	Primary Drug: Anything Other Than Marijuana	Ever Tried Aggression Drugs (amphetamines , cocaine)	Never Tried Aggression Drugs (amphetamines , cocaine)
1	G	I	F	I	Q	***V***	U	M	G	G	I
2	M	Q	C	M	V	U	S	K	U	M	A
3	Q	G	T	C	S	S	Q	T	***M***	U	K
4	U	***M***	P	G	I	T	M	S	Q	Q	O
5	I	U	O	***N***	M	D	J	E	I	***D***	***P***
6	D	D	I	L	K	W	R	P	***D***	I	C
7	S	S	R	T	B	R	T	O	S	S	E
8	L	V	E	J	G	//	W	R	C	L	J
9	F	***L***	K	A	C	//	E	//	T	C	//
10	V	K	A	K	***P***	//	I	//	V	***V***	//
11	C	T	H	//	U	//	C	//	J	T	//
12	T	E	J	//	T	//	L	//	L	W	//
13	W	W	//	//	A	//	O	//	O	F	//
14	K	C	//	//	O	//	V	//	E	E	//
15	N	J	//	//	E	//	B	//	B	K	//
16	B	***N***	//	//	J	//	K	//	R	B	//
17	P	B	//	//	R	//	A	//	F	P	//
18	O	O	//	//	//	//	H	//	***N***	N	//
19	E	R	//	//	//	//	//	//	K	O	//
20	R	P	//	//	//	//	//	//	W	***R***	//
21	J	A	//	//	//	//	//	//	P	J	//
22	A	H	//	//	//	//	//	//	A	A	//
23	H	//	//	//	//	//	//	//	H	H	//

High Judicial Interaction | Medium Judicial Interaction | Low Judicial Interaction

Notes below apply to Tables 4-7.21 and 4-7.22.

1. Courts below the red line are ones in which we predict that clients' expected outcomes were better than their actual outcomes.
2. Courts are not included in the ranking if they had fewer than five people meeting the criteria of the particular category (indicated by the symbol "//").
3. Courts whose number is in Bold/Italic/Underline represent the top three courts in that category for percent of population meeting the particular criteria. For categories where no such court is highlighted in this way, no court had more than 50 percent of their population meeting that criterion.

Conclusions

This analysis examined the relationship between variation in implementation of ten drug court policies and practices and client outcomes. Among the drug court policies and practices examined, four predict court effectiveness:

- Leverage.
- Predictability of sanctions.
- The point of entry into the program during the criminal justice process.
- Positive judicial attributes.

All four of these policies and practices were found to be effective at crime prevention; and all, but leverage, were found to be effective in substance use prevention. More specifically, courts that prevent higher numbers of criminal acts per month had high leverage, medium predictability of sanctions, client populations that enter at the same time point in the criminal justice process—either all pre-plea or all post-plea, and medium or high scores on positive judicial attributes. Courts that prevent more days of drug use per month have medium predictability of sanctions, client populations that enter at pre-plea, and high scores on positive judicial attributes.

In addition, when courts implement the combined practices, there appears to be a synergistic effect such that they are able to prevent the most crimes and the most days of drug use for many subgroups. Table 4-7.23 identifies the court policies and practices of the top performing courts, with respect to the four components that emerged in our analyses. Recall that two courts were in the top five ranked courts for both crime and drug use prevention—courts G and Q. As shown in Table 4-7.23, court Q implements all four policies in the ways we found to be effective, and court G implements three of the four policies in those ways. The remaining three courts in the top five for crime prevention (L, S, and W) and the remaining three courts in the top five for substance use prevention (I, M, and U) all implement at least two or three of the four policies in the ways that seem to matter to produce positive outcomes.

These top-performing courts seem to be purposeful in the ways they implement policies and practices described here as most effective. The combination of these practices implies that these courts simply do not implement such components in a random way. Instead, these courts perhaps purposively fit the practices together. They seem to also differentiate clients either according to risk, need, or circumstance, rather than trying to fit one model of the drug court program to all participants. Additionally, these courts seem to have judges that understand the value of building a relationship with the client in which the client might feel respected and supported, perhaps inclining them toward more success.

Table 4-7.23. Court Policies and Practices for Top Performing Courts

Court Policy/Practice	Top Performers for Both Crime and Drug Use Prevention		Remaining Three Top Performers for Crime Prevention			Remaining Three Top Performers for Drug Use Prevention		
	G	Q	L	S	W	I	M	U
Leverage	High	High	Medium	High	High	Low	High	Medium
Predictability of Sanctions	High	Medium	High	Low	High	Low	Low	Medium
Point of Entry Into Program	All post-plea	All post-plea	All post-plea	All pre-plea	All pre-plea	All post-plea	All post-plea	All pre-plea
Positive Judicial Attributes	High	High	Medium	Medium	Medium	High	High	Low

Several of the policies and practices we examined here have not been previously examined in the literature. Specifically, no previous studies of which we are aware have examined the differential effectiveness of programs based on their clients' stage of criminal justice system processing. In addition, although leverage has been hypothesized to be a critical factor for drug court success (Longshore et al 2004), this is the first study to empirically document that courts classified as having high levels of leverage are the most effective at reducing criminal behavior among their clients. Interestingly, however, leverage was not found to be influential in reducing substance abuse.

Other findings generated from these analyses build on previous court-level research. For example, Harrell and colleagues (1998) demonstrated that graduated sanctions (as a court-level characteristic) were more effective than standard dockets in reducing arrest and the number of offenses committed among program participants. We build on these findings by examining the *predictability* of sanctions as a court-level characteristic. Interestingly, although highly predictable sanctioning practices are considered a cornerstone in achieving a coordinated strategy governing court responses to participants' compliance (and are listed as one of the drug court "key components") we do not find empirical support for this practice. Courts classified as having medium predictability of sanctions were the most effective, which suggests that flexibility in responding to clients' performance may be desirable.

In addition, we find strong evidence that positive judicial attributes positively influence client performance. Previous studies have identified substantial variation in client success among various drug court judges (Finigan et al. 2007). We find that drug courts with judges with more positive attributes are better able to prevent criminal behavior and substance use.

We did not, however, find support for the importance of multidisciplinary decision-making in the integration of substance abuse treatment with justice system case processing in terms of directly effecting client outcomes, nor did we find support for this when it comes to judicial interaction in the courtroom. We attempted to explore several other key components, including treatment best practices, the frequency of drug testing, and the frequency of status hearings. However, because most of the courts included in MADCE were following a high standard with respect to these policies and practices, insufficient variation made it difficult to establish their effectiveness empirically.

Despite the contributions these findings make to the literature, this analysis has two limitations. The first limitation is the quantitative approach is restricted in its ability to detect findings. Having only 23 drug courts as the Level-2 observations limits our ability to identify differences in effectiveness of practices. Given the small sample, we may erroneously find that a real effect is non-significant simply due to the small number of observations. Further, one would expect different drug court practices to have different effects for different types of individuals. However, many of these Level-1 coefficients could not be estimated for all drug courts because not all types of individuals are present in each drug court's sample. For instance, data suggest that being previously incarcerated is related to future criminal activity, so it is important to control for this factor. However, several courts do not have individuals in their samples who have been previously incarcerated, so estimating the court-specific effect of previous incarceration for every court would be impossible.

More generally, since many of the courts serve a small number of participants (seven courts included in the MADCE outcome study contributed 25 or fewer individuals to the baseline sample), it would be impossible to accurately estimate the court-specific effects of the numerous individual characteristics deemed to predict drug use and crime. Even if the Level-2 models for all 15 individual characteristics of interest could be accurately estimated, with only 23 drug courts and so few participants in many of the courts, it would be challenging to aggregate across all of them. In sum, hierarchical modeling is a useful technique, but it is not ideal in this setting. Very few Level-2 observations and a limited number of Level-1 observations in many of these courts limit its applicability, particularly when Level-1 coefficients vary across Level-2 observations.

Because of these limitations, we chose to complement the quantitative approach with our innovative qualitative approach. The qualitative method provides a number of contributions beyond the quantitative analysis. First, small drug court sample sizes are less of a problem because no regressions are estimated using the drug courts. Second, individual-level factors are implicitly allowed to vary across all drug courts. Third, since all information has been compressed to single outcome measures (which are estimated at the individual level), it is straightforward to look at subgroups of the full drug court sample. Thus, we are able to rank court performance for particular subgroups of interest (e.g., the most effective courts for males versus females), as well as provide an overall ranking of courts. Finally, this technique provides a sensible way to aggregate Level-2 differences across Level-1 characteristics.

The second limitation to this study is that some court policies and practices—in particular, adherence to treatment best practices, frequency of drug testing, and frequency of judicial status hearings—lack variation, making it impossible for us to detect their contribution to outcomes. One might simply recommend that these variables be recoded so that more equal groups fall into high, medium, and low levels of these variables. However, in these cases, it is impossible to recode these for two reasons. First, in part, the way we asked the questions does not allow us to unpack the answers any more than they have been already. For example, the response options for frequency of drug testing were *more than once per week*, *once per week*, *less than one time per* *week*, and *not at all*. We cannot recode these to actual numbers that may show more variation. Second, it is just simply the case that nearly all courts adhered to treatment best practices at a medium or high level, all provided drug testing at least once weekly or more frequently, and nearly all conducted judicial status hearings on average four times per month or twice per month. Future evaluations interested in teasing out the effects of these particular court practices may want to purposively sample courts that provide more variation in how these practices are implemented. Such studies also may want to do more complete studies of the quality of treatment being provided and an assessment of adherence to a fuller set of best practices as promulgated by the treatment field. By doing so, more variation in adherence to such practices might be observed, allowing for a more refined examination of how this affects a drug court's ability to prevent substance use and crime.

Despite the limitations noted, this study contributes to our understanding of how drug courts should implement practices to increase their ability to prevent drug use and crime. First, courts with high leverage, medium predictability of sanctions, single points of entry into the program, and high positive judicial attributes seem to be better at preventing criminal activities and substance use. More specifically, drug courts with high leverage have regular monitoring contact with clients through drug court case managers and judicial hearings, and have explicit consequences for failure in the program that clients know about and about which clients sign a contract. These practices might focus a client's attention on the fact that the alternative to drug court is not desirable and that they are being monitored closely making the consequence of noncompliance and the alternative for failure very real. These findings also imply that courts with low leverage—that is, those courts that, from the client's perspective, neither seem to have obvious consequences for failure, nor appear to be closely monitoring program compliance—are unable to achieve success when it comes to preventing crime.

Second, drug courts with medium predictability of sanctions have sanction schedules that clients may or may not know about and that may or may not always be followed. Thus, it appears that these courts have a coordinated sanctioning strategy, yet they exercise some flexibility in its implementation in a way that seems to matter to clients. Perhaps clients perceive flexibility in implementation of sanctions as more fair than those courts that strictly follow a schedule that does not take into account particular individuals or circumstances. While it seems clients clearly need to know that sanctions are a consequences of noncompliance in the program, sanctions that are rigidly set or perceived of as unfair may actually contribute to client frustration or lack of interest in complying with program requirements. In addition, if programs with rigid, highly predictable sanctioning practices were shown as the most effective in this analysis, that finding would run counter to our other finding on positive judicial attributes. Programs with judges that treated clients fairly and respectively were shown to achieve better success than programs without such judges. Perhaps rigid sanctioning practices and some features of positive judicial attributes do not easily coexist in a single court.

Third, drug courts with single points of entry into their program have client populations that either all entered the program before they entered a plea—a diversion program—or all entered the program after their plea. These courts do not have a mix of clients that represent different stages of the criminal justice system process. Perhaps courts that have a singular focus of client population might be better able to tailor their practices in such a way to meet the needs of a pre-adjudication or a post-adjudication population. When a mix of clients is in the program, courts may be less organized in their approach, or may be uniformly implementing practices when such practices might not be appropriate for some portion of their clientele.

Finally, drug courts that have high scores on positive judicial attributes are those courts in which judges demonstrate to defendants respect, fairness, attentiveness, enthusiasm, consistency/predictability, caring, and knowledge about the person's case and situation. Our courtroom observations of judicial attributes indicate that how the judge builds a relationship with clients, treats clients, and behaves in the courtroom matters for client outcomes. This finding once again underlines the role of therapeutic jurisprudence in problem-solving courts such as drug courts.

In addition to the substantive findings generated from these analyses, this study also makes several methodological advancements in looking at what works for whom in drug courts. We developed an approach for examining patterns of court performance with an eye toward how courts were characterized along ten different policy and practice domains. Furthermore, we conducted these comparisons for 31 population subgroups, in order to assess the comparative effectiveness of policies and practices for specific populations served by drug courts. Although the findings described in this chapter generally focused on practices that appeared to be most effective for the largest number of subgroups (rather than documenting detailed patterns for specific subgroups), the detailed tables presented in this chapter can be used by policymakers and practitioners as they seek information on policies and practices that are effective for the specific subgroup(s) they serve. We find that while the top-performing drug courts tend to be effective across a variety of subgroups, variability exists in terms of specific practices that are most effective for different groups. This finding builds on the limited previous research, which indicates that not all practices are equally effective across the population subgroups served by drug courts (see, for example Marlowe et al. 2003, 2004, 2005; Festinger et al. 2002). Clearly, more detailed analyses of what works for specific subgroups can be conducted based on the findings presented in this chapter. Other future research directions include looking more closely at different combinations of policies and practices in order to identify critical combinations that appear to account for most of the variability in program effectiveness. Future work examining individuals' receipt of particular policies and practices should continue, while developing appropriate strategies for overcoming the methodological challenges associated with the fact that clients performance is closely related to their receipt of drug court components.

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Chapter 8. Drug Court Practices: An Analysis of Dosage Effects

John K. Roman, Shelli B. Rossman, and Avinash S. Bhati

Studies of human services programs are typically interested in learning the extent to which different levels of services, such as substance abuse treatment, effect client behavior. For MADCE, this question was addressed by performing a dosage analysis that compared drug court clients who received more of selected services to drug court clients who received lower levels of these services. The goal is to help drug courts identify which practices are likely to improve outcomes.

The challenge with this kind of analysis, however, is that the results often are hopelessly confounded by circular reasoning. A reverse causality problem occurs when drug courts respond to client behavior by changing the type or quantity of services the client receives. One simple example is a test of the effect of court appearances on client outcomes. Clients who commit no infractions stay in drug court longer (until graduation), attending court for routine status hearings; however, their long attachment to the program increases their total number of court appearances. By comparison, clients who commit many infractions appear in court more frequently, but are not likely to remain in the program as long. However, the total number of court hearings could be relatively the same for clients with high rates of infractions and those with low rates of infractions. The problem can be somewhat mitigated by controlling for time in the analysis, but that strategy is only effective when the pattern of infractions and response are clear; since most drug court clients relapse at least once during their participation in the program, the behavioral pattern is generally very complicated.

Even in cases where clear patterns can be identified, the analysis is logically circular. In the example above, for instance, the causal order of events is that a client engages in bad behavior and then comes to court for a sanction. Analytically, values are observed and those outcomes are then regressed on court appearance, which is nothing more than a proxy for bad behavior in this case. Thus, in essence, bad behavior is regressed on bad behavior.

In the language of statistics, this is described as an endogeneity problem. The goal of multivariate regression analysis—typically the statistical tool used to evaluate drug courts—is to parameterize the effect of a predictor variable (often referred to as an independent variable) on a response variable (often referred to as the dependent variable). The response variable is endogenous, meaning the values are determined by the predictor variables in the model, and the predictor variable is exogenous, meaning that the values of the predictor are completely determined by factors outside the model. Age, for instance, predicts post-drug court criminality, but age is in no way affected by drug court. In the court appearances example, bad behavior causes court appearances and post-drug court criminality. Court appearances—even rated by time in the program—are not an exogenous predictor. Thus, the causal logic of the statistical model is imperiled by the fallacy of circular thinking, and the results of that analysis are unreliable.

The challenge for drug court researchers is that to the extent that drug courts vary services in response to clients' behavior (as most drug courts report they operate), this circular logic accurately describes virtually all drug court practices from drug treatment to drug testing.

Design and Methodology

There are several ways to mitigate the reverse causality problem. One way is to avoid doing the analysis at the individual-level: for example, in Chapter 7 (this Volume), we looked at court-level averages, comparing the average number of court appearances to the average effect on behavior to determine whether a given court as a whole does better, or worse, than other courts with different average numbers of court appearances. This model, while still not causal, is at least one step removed from the problem. However, this strategy may, in turn, be plagued by aggregation bias.

The other approach to resolving this problem is to use an instrumental variable to break the endogeneity. In the court appearances example, the idea is to find something that correlates very highly with court appearances, but isn't, itself, affected by the outcome (i.e., bad behavior). The use of instrumental variables has become very popular in economics and sociology, and is increasingly being applied in criminology. Instrumental variables, however, are notoriously difficult to identify since virtually every practice in the criminal justice system that effects behavior is itself affected by that bad behavior. Angrist (2006) proposes an instrumental variables technique that uses group assignment and compliance rates as an instrument to solve equations with endogenous regressors.

Here, we propose a conceptually similar model for resolving the problem of endogenous regressors—propensity score weights. It is clear from the discussion earlier in this chapter that in some nontrivial way the amount of services individuals will need is related to their riskiness in general. Thus, those who are at low risk of bad behavior will receive different frequency and quantities of drug court interventions than those at higher risk. Further, we propose that individual drug court clients are heterogeneous in their ex ante needs for varying drug court services, according to both the underlying risk and more direct underlying needs for each service. So, for instance, the amount of case management will vary according to both (1) the client's need for case management and (2) the temporally endogenous response to bad behavior. We posit, however, that bad behavior, itself, is a function of individual risk for bad behavior. Summing up these propositions, we hypothesize that an ex ante measure of risk can be constructed that will account for the endogenous response to behavior and differential need for services that confounds the drug court effect.

Propensity scores are often used in criminological research—especially in quasi-experiments as they were in MADCE—to account for potential confounding from selection bias due to problems in group assignment. Drug courts, for instance, are almost always voluntary, and thus constructing an appropriate comparison group is difficult. A group of similar individuals in the same system at the same time who were not offered drug court would typically be considered a strong comparison group. However, that group likely includes individuals who would have refused to participate in drug court if they had been offered the opportunity. To the extent that volunteering covaries with outcomes (it seems reasonable to predict that refusers will do worse), bias is introduced into the analysis.

In the propensity score framework, the analysis would be constructed in two steps. First, a logistic regression would regress group assignment on various attributes plausibly associated with being a member of one group or another. In the example above, some measure of treatment variation would improve the model's predictiveness and account for selection. Even if no such measure is available, however, if the variables that are included in the model covary with unobserved attributes of motivation, some reductions in selection bias can be achieved. This model then produces a weight which balances the two groups on observable attributes (see Chapter 2 and Appendix A in this Volume for a complete description of the propensity scores used in the MADCE). Thus, for instance, age differences between two groups can be statistically adjusted so there is no confounding effect.

Here, we apply the same logic to resolve the individual-level analysis of the effect of court practices on outcomes. Details on the methodology can be found in the technical appendix (Appendix C). The approach is summarized here.

First, we construct measures of nine common court practice:

- Amount of drug treatment
- Immediacy of intervention
- Legal leverage
- Severity of sanctioning
- Rewards
- Level of judicial supervision
- Level of case management
- Level of drug testing
- Support services received

Each of the measures (save immediacy) is stratified into three categories: low, medium, and high, as described below. Next, for each individual in the drug-court sample, we construct a propensity measure of the likelihood of receiving each of the nine court practices. Thus, we create a three-level propensity measure for getting high, medium, and low amounts of various court practices. These propensities are calculated separately for each of the nine measures and are computed using a variety of observed attributes theorized to be related to the outcomes of interest.

Weights are then created using the inverse of the probability of receiving the current dosage level (low, medium, or high). When these weights are applied to the data and the three dosage groups are compared on the included attributes, they are typically balanced on these attributes. In effect, the weights allow us to mimic a setting where the dosage levels are randomly distributed among the population of interest. This is just an approximation because we can never know if the dosage levels are balanced on all *unobserved* attributes whereas a true randomization would ensure that.

The weighted sample therefore allows one to estimate the effect of receiving low, medium, or high levels of that court practice as if dosage was randomly assigned within the population. So, for instance, each of those individuals who were assigned to the low risk of drug treatment have similar need for drug treatment, and the response of drug courts to behaviors (the endogenous component of the analysis) has been explained by the propensity score. That is, the propensity score effectively controls for all differences in risk within each measure, and thus resolves the endogeneity problem because receipt of services is now independent of behavior. What is left then is the independent effect of program services on outcomes. We stress again that, as with all propensity score based approaches, the strategy only allows one to control for observable confounders (those attributes included in the propensity score model).

Measures of Court Practices

We examined the differential effects of nine types of services or interventions selected from those identified as critical components in the extensive research literature on drug courts (see Chapter 2 in Volume 1 for a detailed review of the literature). The domains and the indicators[34] we used include:

1. *Amount of Drug Treatment*, defined as the number of days of residential, outpatient group, and outpatient individual drug treatment.[35] These were divided into three levels: low <= 35 days, medium > 35 days and <= 65 days, and high > 65 days.

2. *Immediacy of Intervention*, defined as any drug treatment of the above types in the first month after program entry. In the analysis, this was coded 0 = no treatment in the first month and 1= some treatment in the first month.

3. *Legal Leverage*, defined by the severity of the sentence likely to be imposed for drug court failure as rated by drug court participants as 1 = not bad at all, 2 = somewhat bad, or 3 = extremely bad.

4. *Severity of Sanctioning*, defined as the percentage of imposed sanctions involving jail time.[36] The percentages were divided into three levels of severity: low = 0, medium > 0 and <= 20 percent, and high > 20 percent.

5. *Rewards*, defined as the number of times the client received praise from the judge for drug court compliance and progress. These were divided into three levels: low = 0 times, medium > 0 and <= 10 times, and high > 10 times.

[34] The following describe strata for 18-month observations. Strata for 6-month observations are available upon request.
[35] This excludes self-help groups (e.g., 12-step programs like AA/NA) and alternative approaches (e.g., acupuncture).
[36] This was based only analysis of clients receiving sanctions.

6. *Level of Judicial Supervision*, defined as the number of status hearings. These were divided into three levels: low <= 5 hearings, medium > 5 and <= 10 hearings, and high > 10 hearings.

7. *Level of Case Management*, defined as the number of face-to-face contacts with the supervising officer. These were divided into three levels: low <= 10 contacts, medium > 10 and <= 25 contacts, and high > 25 contacts.

8. *Level of Drug Testing*, defined as the total number of drug tests administered. These were divided into three levels: low <= 9 tests, medium > 9 and <= 37 tests, and high > 37 tests.

9. *Support Services Received*, defined by the sum of all types of service received by the client (employment and education, life and interpersonal skills, child-related services). These were divided into three levels: low <= 1 service, medium > 1 and <= 3 services, and high > 3 services.

The selection of domain indicators was based on factor analysis results of items related to each domain. The factor analysis found a single factor structure in all cases. We selected single items to represent each domain because these items were typically very highly correlated with the single factors recovered. Models also were run using factor scores, and results did not change. Therefore, the single item findings are presented for ease of interpretation.

The analysis assessed the effect of variations in the dose of these services on four outcomes:

- The number of crimes reported per day of street time within the first six months.

- The number of crimes reported per day within the entire 18 months.

- The number of months of drug use per day of street time within the first six months.

- The number of months of drug use per day of street time within the entire the first 18 months.

The outcomes are based on responses provided by clients at the 6- and 18-month interviews. The number of days of street time was computed as the reported number of days clients were not incapacitated in any way.

The Propensity Score Analysis

As discussed above, the dosage analysis required a methodology that controls for the wide variation in client characteristics across drug courts, diversity in age, race, gender, drug use patterns, and other factors that might distort the analysis of the effects of drug court practices on outcomes. For example, individuals who are thought to be more at risk of drug use may require more intensive treatment. Unless adequate controls are introduced to capture the effects of "risk," an analysis comparing actual drug use and drug treatment participation will lead to incorrect inferences.

As explained above, to minimize the potential for bias due to differences in client risk, we applied propensity score-based balancing techniques to ensure that, in the aggregate, clients receiving low, medium, and high dosage for each of the drug court components considered had similar (or balanced) confounding characteristics. The list of variables used to balance clients receiving different dosage levels included: age, race (black, white, Hispanic/Latino, and other), married, gender, primary drug of abuse (alcohol, marijuana, cocaine, other, and none listed), age at first drug use, use level (none, moderate, regular, heavy), number of prior arrests, and which drug court. In addition to this basic list, the crime risk predictors included: use of drugs associated with aggression (amphetamines, crack/cocaine, or hallucinogens), number of friends and relatives convicted of a crime, diagnosis for personality disorder, and number of prior incarcerations. In addition to the basic list, the drug use risk predictors included the number of blood relatives with alcohol problems, the number of days of prior drug or alcohol treatment, and high scores on a depression scale. Each of these measures was independently assessed for each of the nine court practice measures.

The effectiveness of the balancing procedures is illustrated in Figures 4-8.1 and 4-8.2, which compare the results of the dosage analysis before and after risk balancing for each of the domains of interest. In these figures, the statistical significance of the relationship between the dosage variables and client characteristics is indicated by one or two asterisks (for a $p<.10$, and $p<.05$). After re-weighting the sample, only a few significant relationships between dosage and client characteristics remain—notably court identity. As a result, court identity was included as a fixed effect in all the weighted models assessing the effect of dosage level on outcomes of interest.

Results

Because the outcome variables—number of crimes and number of days of drug use—are count variables, negative binomial regression was used to assess the dosage effects in each of the nine domains, as presented in Table 4-8.1. The dosages are entered in categories as defined above. The negative binomial regression coefficients and p values show the estimated effect (and its significance) of increasing the dosage from low to medium and from low to high. The dy/dx is the estimated increase (or decrease) in units of the outcome variable (number of crimes or days of drug use) associated with the change in dosage from low to medium or from low to high. For the single category variable--immediacy--only the one level of increase in dosage is shown.

Reviewing the domains of drug court services and interventions one at a time we find that for:

- *Amount of Drug Treatment*: 36 to 65 days of treatment, compared to fewer days of treatment, was significantly related to fewer crimes reported at 18 months and fewer days of drug use reported at both 6 and 18 months. The estimates (dy/dx) show an expected reduction of 5 fewer crimes reported at 18 months, and just over 2.5 fewer days of drug use reported at both time periods, compared to the reports of those clients who received

Figure 4-8.1. Eighteen-Month Model of Sample Balancing for Drugs

	Unweighted												Weighted											
	Amt. of Treatment	Substance Abuse Factor	Immediacy of Intervention	Legal Leverage	Sanctioning Severity	Sanctioning Factor	Rewards	Rewards Factor	Judicial Supervision	Case Management	Drug Testing	Support Services	Amt. of Treatment	Substance Abuse Factor	Immediacy of Intervention	Legal Leverage	Sanctioning Severity	Sanctioning Factor	Rewards	Rewards Factor	Judicial Supervision	Case Management	Drug Testing	Support Services
18-Month Models																								
Age	**	*		*			**	**																
White	**	**	**	**							**													
Black/African Amer.	**	**	**	**					**	**	**													
Hispanic/Latino																								
Married/Relationship																								
Male	*	**	*	**			*	**		*	*	**												
Primary Drug-Alcohol	**	**		*					**		*	*												
Primary Drug-Marijuana		**					*				*													
Primary Drug-Cocaine				**							*													
Primary Drug-Hallucin/Heroin	**	**	**	*			**	**	**		**													
Age 1st Used, Excluding. Alcohol						**																		
Occasional Use	**										*													
Moderate Use					*	**					**													
Regular Use																								
Blood Relative Involved With Drugs	**	**	**						**			**												
Total Days Clinical Treatment	**	**	**		**	**				**		**			**									
Depression Scale		*		*			**					**												
# Prior Arrests	**	**					**	**				**												
Court	**	**	**	**	**	**	**	**	**	**	**	**	**	**	**				**	**		**	**	**

Note: 6-month model available on request. **p<.10, **p<.05.*

Figure 4-8.2. Eighteen-Month Model of Sample Balancing for Crime

Unweighted

18-Month Models	Amt. of Treatment	Substance Abuse Factor	Immediacy of Intervention		Sanctioning Severity	Sanctioning Factor	Rewards	Rewards Factor	Judicial Supervision	Case Management	Drug Testing	Support Services
Age	**	*		*			**	**				
White	**	**	**	**							**	
Black/African Amer.	**	**	**	**					**	**	**	
Hispanic/Latino												
Married/Relationship												
Male	*	**	*	**			*	**		*	*	**
Primary Drug-Alcohol	**	**		*					**		*	*
Primary Drug-Marijuana		**					*				*	
Primary Drug-Cocaine				**							*	
Primary Drug-Hallucin/Heroin	**	**	**	*			**	**	**		**	
Ever Used-Cocaine, Hallucinogen, Amphetamine		**	**									
Relative Convicted	**	**	**				**	*	**		**	**
ASPD	**	*	*	*								**
# Prior Arrests	**	**					**	**				**
# Prior Incarceration	*	*	*	*			**	**	**	**		**
Court	**	**	**	**	**	**	**	**	**	**	**	**

Weighted

18-Month Models	Amt. of Treatment	Substance Abuse Factor	Immediacy of Intervention	Legal Leverage	Sanctioning Severity	Sanctioning Factor	Rewards	Rewards Factor	Judicial Supervision	Case Management	Drug Testing	Support Services
Age												
White												
Black/African Amer.												
Hispanic/Latino												
Married/Relationship												
Male												
Primary Drug-Alcohol												
Primary Drug-Marijuana												
Primary Drug-Cocaine												
Primary Drug-Hallucin/Heroin												
Ever Used-Cocaine, Hallucinogen, Amphetamine												
Relative Convicted												
ASPD												
# Prior Arrests												
# Prior Incarceration												
Court	**	*	**				**	**		*	**	

Note: 6-month model available on request. *$p<.10$, **$p<.05$.

Table 4-8.1. Dosage Effects at Six and Eighteen Months, by Domain

Dosage Amount	Crime - 6 months				Crime - 18 months				Drugs - 6 months				Drugs - 18 months			
	Coeff	p-val	dy/dx	p-val	Coeff	p-val	dy/dx	p-val	Coeff	p-val	dy/dx	p-val	Coeff	p-val	dy/dx	p-val
Substance Abuse Treatment																
M	-0.18787	0.49	-0.95714	0.492	-1.25158	0	-5.03395	0	-0.87108	0	-2.55717	0	-0.92741	0	-2.54552	0
H	-0.44121	0.09	-1.99362	0.106	-0.56462	0.008	-3.04191	0.013	-0.74633	0	-2.31265	0.002	-0.16788	0.357	-0.65088	0.363
Substance Abuse Factor Score																
M	-0.42994	0.115	-2.15784	0.126	-1.04434	0	-5.1735	0	-0.91379	0	-2.64447	0	-0.9116	0	-2.98469	0
H	-0.51015	0.037	-2.46745	0.052	-0.58418	0.01	-3.53193	0.016	-0.68997	0.001	-2.20038	0.003	-0.2381	0.217	-1.05729	0.226
Immediacy																
M	1.195524	0.001	5.281023	0.01	0.687013	0.008	3.60353	0.007	0.445197	0.086	1.334575	0.086	0.253237	0.237	0.777306	0.218
H																
Legal Leverage																
M	0.455671	0.135	3.156546	0.166	0.376345	0.241	3.23608	0.233	0.119976	0.594	0.477399	0.593	0.094435	0.711	0.575433	0.709
H	-0.10021	0.702	-0.52141	0.703	0.015731	0.959	0.112288	0.959	0.004531	0.983	0.017008	0.983	-0.50807	0.034	-2.31447	0.069
Sanctions																
M	0.812409	0.007	10.19366	0.027	0.442663	0.102	4.342236	0.117	0.835016	0.004	5.360961	0.007	0.423765	0.065	2.343804	0.078
H	-0.22571	0.529	-1.64335	0.508	0.017083	0.953	0.134358	0.953	0.2377	0.44	1.102425	0.451	0.179432	0.42	0.872929	0.424
Sanctions Factor Score																
M	0.321938	0.293	3.931012	0.328	0.172331	0.521	1.545854	0.522	0.458095	0.093	2.871855	0.112	0.218788	0.322	1.157999	0.326
H	-0.79507	0.019	-5.67658	0.017	0.004427	0.989	0.036464	0.989	-0.14788	0.625	-0.67942	0.623	0.154217	0.489	0.789514	0.493

(continued)

Table 4-8.1. Dosage Effects at Six and Eighteen Months, by Domain (Cont'd)

Dosage Amount	Crime - 6 months				Crime - 18 months				Drugs - 6 months				Drugs - 18 months			
	Coeff	p-val	dy/dx	p-val	Coeff	p-val	dy/dx	p-val	Coeff	p-val	dy/dx	p-val	Coeff	p-val	dy/dx	p-val
Rewards																
M	-1.71938	0	-16.828	0	-0.94688	0	-7.57317	0	-1.41006	0	-7.60304	0	-0.88678	0	-4.60154	0
H	-2.8737	0	-19.3433	0	-1.55835	0	-9.76906	0	-2.57214	0	-9.29043	0	-1.61684	0	-6.27194	0
Rewards Factor Score																
M	-1.79182	0	-10.5655	0	-1.23427	0	-8.49747	0	-1.71668	0	-5.83569	0	-1.0572	0	-4.53814	0
H	-2.53943	0	-11.678	0	-1.85276	0	-10.1065	0	-2.32362	0	-6.41718	0	-1.64622	0	-5.6136	0
Judicial Supervision																
M	-1.2796	0	-8.85487	0	-0.79688	0	-5.00733	0.002	-0.68789	0.001	-3.15764	0.001	-0.79477	0	-3.14731	0
H	-1.90542	0	-10.4421	0	-0.92373	0	-5.49688	0	-1.777	0	-5.27487	0	-1.06954	0	-3.77021	0
Case Management																
M	-1.60646	0	-9.03871	0	-0.72633	0.002	-4.22226	0.004	-1.29717	0	-3.81873	0	-0.85991	0	-3.34334	0
H	-1.52353	0	-8.8426	0	-0.65701	0.005	-3.93836	0.007	-1.10008	0	-3.50585	0	-0.89975	0	-3.43914	0
Drug Testing																
M	-1.29109	0	-10.5647	0	-0.67878	0.001	-4.59987	0.004	-0.97699	0	-4.38755	0	-0.80469	0	-3.52561	0
H	-2.23559	0	-13.0133	0	-1.18389	0	-6.4776	0	-1.46558	0	-5.41132	0	-1.12726	0	-4.31205	0
Support																
M	0.270567	0.306	1.325397	0.316	0.662554	0.004	3.199142	0.012	-0.08255	0.709	-0.23276	0.708	0.332316	0.067	0.969053	0.082
H	0.263221	0.346	1.284473	0.357	0.761796	0	3.888098	0.001	0.096108	0.626	0.296347	0.627	0.549634	0.005	1.801012	0.01

less drug treatment. More than 65 days of drug treatment, compared to less than 35 days, was significantly related to fewer crimes at 18 months and fewer days of drug use at 6 months, but the estimated size of the reductions was somewhat smaller.

- *Immediacy of Intervention:* The receipt of any drug treatment within the first month of drug court was significantly related to *higher* numbers of crimes reported at both 6 and 18 months. Clients entering treatment immediately estimated to report 5 more crimes at 6 months, and 3.6 more crimes at 18 months. In addition, early treatment entry was associated significantly ($p<.1$) with additional days of drug use at 6 months, which were estimated to be 1.3 *more* days than those entering treatment later. Early treatment entry was not significantly related to days of drug use reported at 18 months.

- *Legal Leverage:* This was related to only one outcome. When the expected legal sentence for drug court failure was rated as ext*remely* serious, clients reported significantly fewer days of drug use at 18 months (estimated at 2.3 fewer days) than clients who rated the expected sentence as “not bad at all.”

- *Sanction Severity:* The severity of sanctions did not appear to reduce the number of crimes or days of drug use. When the percentage of sanctions resulting in jail time was medium (i.e., between 1 and 20 percent), the number of crimes and days of drug use reported at 6 months were higher than for clients who did not receive jail time sanctions. They were estimated to commit 10 more crimes and report 5.4 more days of drug use than those with no jail sanctions.

- *Rewards*: Rewards in the form of praise by the judge for drug court accomplishments were significantly related to fewer crimes and fewer days of drug use at both 6 and 18 months. Consistently, the higher level of rewards produced the larger reductions in crime and drug use. Compared to no rewards, high levels of praise reduced the expected number of crimes by 19.3 at 6 months and 9.8 at 18 months, and reduced the expected days of drug use by 9.2 at 6 months and 6.3 at 18 months.

- *Judicial Supervision:* Status hearings were significantly related to fewer crimes and days of drug use at both 6 and 18 months. Compared to 5 or fewer status hearings, more than 10 status hearings consistently produced the largest reductions in crime and drug use. For crime, the reductions are estimated at 10.4 fewer crimes at 6 months and 5.5 fewer crimes at 18 months. For drug use, the reductions are estimated at 5.3 fewer days of use at 6 months and 3.8 fewer days of use at 18 months. The medium level of judicial supervision, i.e., 6 to 10 status hearings, also produced significant reductions in crime and drug use, but not as large.

- *Case Management*: Contact with the supervising officer was significantly related to reductions in crime and drug use at both 6 and 18 months. Compared to low levels of contact with the supervising officer (i.e., 10 or fewer contacts), both high (i.e., more than 25) and medium (i.e., 11 to 25) levels of contact were related to significantly fewer crimes and days of drug use. Both high and medium levels of case management were estimated to have similar reductions in numbers of crime about 9 at 6 months and about 4 at 18 months; and also showed similar reductions in days of drug use, about 3.5 at 6 and 18 months.

- *Drug Testing:* Testing was significantly related to reductions in crime and drug use at both 6 and 18 months. Compared to low numbers of drug tests (9 or fewer tests), high (more than 37) and medium (9 to 37) episodes of testing were related to significantly fewer crimes and days of drug use. High numbers of drug tests reduce the number of crimes by 16 at 6 months, and 6 at 18 months. Medium reduced crimes by 11 and 5 events at 6 and 18 months, respectively. High and medium numbers of drug tests reduced the number of days of drug use by about 5 at 6 months and 4 at 18 months.

- *Support Services*: The number of support services appeared significantly related to increases in both crime and drug use at 18 months. Those who received more than one service, especially those who received more than three kinds of service, reported more crimes and days of drug use.

Conclusions

As with any study that uses propensity scores to statistically adjust data, we note that this approach can only approximate, but not reproduce, a random assignment study. And, to the extent that there are unobservable factors related to risk and service receipt, our analysis does not adjust for those factors. We are encouraged by the finding that statistically significant differences among key predictors and service receipt are removed by the propensity scores. However, we cannot determine whether unobserved factors also effect that relationship, and thus confound our results.

Three of our outcome measures have additional limitations. We have some concern about the validity of the sanctions measure because in the survey we simultaneously ask about the respondents' number of criminal incidents and number of sanctions received, and thus respondents may conflate their responses. With respect to the sanctions measure, it is possible that the propensity model does a good job of adjusting for risk of sanctions (e.g., certainty), but may not be as effective for severity (since the judge's response may be focused on the particular incident at hand and not the respondent's overall risk). Finally, it is possible that the propensity scores are not as accurate with respect to initial placement measure. For instance, it is possible that the drug courts do a better job of determining who needs immediate placement than do the propensity models, as would be the case for someone with visible withdrawal symptoms who required detoxification.

Several of the domains set forth as core drug court components received strong support from these results. Drug court clients who received higher levels of judicial praise, judicial supervision, and case management reported fewer crimes and fewer days of drug use after balancing the dosage levels on attributes related to client risk for these behaviors. In addition, drug court clients who participated in more than 35 days of drug treatment had fewer crimes at 18 months and fewer days of drug use at both 6 and 18 months, although treatment in excess of 65 days did not produce additional reductions beyond that provided by 36 to 65 days of treatment.

The effect of leverage provided by a very severe sentence for drug court failure was limited to a reduction in days of drug use at 18 months, an important outcome.

Some domains did not have the expected effect on drug use and crime. Providing drug treatment in the first month of drug court (an immediate intervention) was associated with increases in numbers of crimes and a slight increase in drug use reported at six months. Increases in the number of support services similarly was related to increases, not decreases, in number of crimes and days of drug use at 18 months. It is possible that risk factors not controlled by balancing drove the early treatment and additional support service decisions.

Perhaps the most surprising finding is the relationship between the severity of sanctions and outcomes, particularly at 6 months. A medium level use of jail sanctions (between 1 and 20 percent of imposed sanctions) was associated with increased number of crimes and days of drug use at 6 months, and to a lesser extent, with an increased number of days of drug use at 18 months.

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Chapter 9. Cost-Benefit Analyses

P. Mitchell Downey and John K. Roman

In this chapter, we build on prior estimates of drug court impact by monetizing outcomes from the MADCE to compare the net social costs and benefits of drug court. To make this comparison, we first identify all of the costs of drug court programming, which we refer to as inputs, and then convert all relevant benefits into dollars. Rather than directly comparing costs and benefits—which tends to underestimate variance, and thus overestimates statistical significance—we convert all of the economic effects of drug court participation into a single measure that includes both "costs" and "benefits" and that we refer to as the individual harms metric or an individual's net benefits.[37] For each individual, we sum all of the costs of drug court participation, all other costs associated with the processing of the original case that led to drug court participation, and the monetized value of all available outcomes. This includes any positive outcomes such as increases in wages, and any negative outcomes such as any new offending. Thus, possible values of the individual harms metric include both positive and negative values. Ultimately, this variable is the dependent variable that is regressed on a binary indicator of drug court participation and other covariates. Negative values for that parameter indicate that the drug court reduced net benefits and increased individual harm. A positive value indicates that net benefits increased or harms decreased by drug court participation. Once we estimate this parameter, we conduct several additional analyses, first to measure whether average net benefits are meaningfully explained by outliers, and then to examine correlates of net costs, including the co-variation between net benefits and service receipt. We conclude by exploring whether (and for whom) comparison sites would have benefitted from processing drug-involved offenders through a drug court.

The cost collection literature differentiates between top-down estimators of cost and bottom-up estimators (Netten 2010). The analysis discussed here differs substantially from most of the extant cost-benefit analysis (CBA) literature. Typically, due to data constraints, drug court CBAs apply some type of top-down strategy to estimate costs. This approach is potentially quite misleading since it compares either average or individual drug court benefits to average costs. Thus, while benefits vary among study participants, costs are assumed to be equal across all individuals. Of course, not all individuals are equally costly to put through the system. Thus, the costs of drug court also should be allowed to vary across individuals. For this reason, we employ a bottom-up approach, which though theoretically reasonable, has rarely been used due to data limitations.

The top-down approach simply takes a total cost statistic, and then either divides by the number of program participants to estimate an average cost per participant or compares those average costs to individual benefits. The specifics of this approach have varied, from carefully estimating program costs from aggregate administrative records of staff and expenditures (Harrell, Cavanagh, and Roman 1999; Washington State Institute for Public Policy 2003), to simply

[37] Since net harms are negative net benefits, we use them interchangeably.

estimating program costs from the amount of the grant (Harrell, Mitchell, et al. 2003). The bottom-up approach, by contrast, takes the *price* of each input (such as an hour of treatment) and multiplies it by the *quantity* (i.e., the number of times that cost was incurred, such as the number of treatment episodes) for each cost input. Thus, the bottom-up approach yields an estimate for each individual's cost. The bottom-up approach is thus strongly preferred to top-down, however, we note that individual, activity-level data are rarely available.

The extant drug court CBA literature has two other serious limitations that we sought to avoid. As is shown toward the end of this chapter, the top-down cost estimation approaches that are so common in this literature tend to over-estimate statistical significance by under-estimating variance. In addition, in cases where average costs are compared to average benefits, there is no way to express any uncertainty about that average difference, since standard errors cannot be calculated. Studies that compare average costs to individual benefits can approximate a standard error, but this only expresses the uncertainty around benefits, not of the costs.

Additionally, many of the studies divide project activities into "costs and benefits" categories, a distinction that we consider inherently arbitrary. For instance, prison for drug court failures can be considered a program activity and counted as a cost, or considered an outcome of the program and counted as a (negative) benefit. Since reasonable people can disagree, the choice is arbitrary. In addition, estimation of separate costs and benefits categories naturally leads to a comparison often in the form of a cost-benefit ratio. These ratios have two serious limitations. First, they do not account for scale (a program that saves $200 on a $100 investment receives the same score as a program that saves $20,000 on a $10,000 investment). Second, if the costs and the benefits have different signs, the results are difficult to interpret. Suppose a program *saves* $200 and has benefits of $100 the cost-benefit ratio is -$2 to 1, which is nonsensical.[38] Instead, we create a measure of the net benefits for each individual in our sample, and the ultimate measure of drug court success is simply whether average net benefits for drug court participants exceed average net benefits of the comparison.

Our study has the advantage of individual-level data on the use of a variety of program inputs (such as number of: hearings, drug tests, treatment episodes, meetings with a case manager, etc.). Therefore, we incorporate variation in program costs in addition to the variation in program benefits. We aggregate cost categories (drug tests, hearings, case management, drug treatment, and administrative costs) with benefits (which are generally measured as reductions in costs associated with the individual, such as costs of new crimes) into a single net benefits variable, measured on the individual level. In doing so, we examine the effects of drug court by incorporating variation in both costs and benefits of program participation.

Finally, we note that many extant studies have indicated that the literature mainly focuses on a very limited set of potential benefits of drug court that could yield benefits to society. Aos and colleagues (WSIPP 2003: 11) acknowledge that in their evaluation, "we only estimated the effect

[38] In effect, cost-benefit ratios have two standards. For positive ratios, values less than 1 indicate success. For negative ratios, values greater than -1 indicate success. However, even that complexity masks an even greater complexity, which is that as ratio values approach 0 from either side, the benefits asymptotically approach infinity compared to the costs. The second problem can be somewhat overcome by the use of benefit-cost ratios, but using benefit-cost ratios reverses the first problem making the results even less intuitive when values are negative.

that drug courts have on crime. We did not attempt to determine whether drug courts improve other outcomes, such as decreases in substance abuse, increases in employment, or reduction in welfare or medical costs. As a result, our cost-benefit analysis does not include these other potential benefits of drug court." Harrell et al. (1999: 22) write, "The project was designed to estimate only the benefits associated with averted crime. As a result, several potential benefits of these programs are not included in the estimation of benefits, including improved health of program participants (through decreased use of publicly funded medical care), increases in tax collection resulting from greater employment by participants, and increases in child-support payments that lessen the public burden." Similarly, Mackin, Lucas, et al. (2010: 38) recognize that "some possible costs or cost savings related to the program are not considered in this study. These include the number of drug-free babies born, health care expenses, and drug court participants legally employed and paying taxes."

It is often said that benefits from a successful intervention flow away from an individual like waves from a rock thrown into a pond. The advantage of cost-benefit analysis is that many of these benefits can be incorporated into a single metric. However, the approach also reveals that the proximate outcomes are only a part of the story, and there are many distal outcomes—waves far from the point of impact—that ought to be considered. While many benefits cannot be addressed in this study, we observe more than are usually available. In particular, the detailed, extensive data collection allows us to examine employment, welfare and financial support, medical and health care costs, child support payments, and a number of other potential benefits. These are displayed in Table 4-9.1, which describes the net benefits domains.

Table 4-9.1. Components of Net Benefits

Category	Sub-Category
1. Social productivity	A. Employment B. Education C. Services and Support Provided
2. Criminal justice system	A. Monitoring B. Police C. Courts D. Corrections E. Drug court
3. Crime and victimization	
4. Service use	A. Drug treatment B. Medical treatment C. Mental health treatment D. Other
5. Financial support use	A. Government B. Other

Note: In the sections that follow, we assign some of these net benefits categories to the "cost" discussion and some to the 'benefits' discussion. In general, we assigned monitoring, courts, drug court, and all types of treatment to the cost category. We assigned the social productivity measures, crime and victimization, and financial support to the benefits category.

Design and Methodology

Prices used in this study were estimated from a number of sources. Data on some salaries, treatment costs, and drug-testing costs for some of the sites were estimated from prior research on this sample (Bhati, Roman, and Chalfin 2008). Other estimates were developed from a wide range of extant research and official reports. For instance, many salaries were collected from the Occupational Employment Statistics database maintained by the Bureau of Labor Statistics; many incarceration costs were collected from official financial reports from relevant Departments of Corrections; and many drug treatment costs were collected from other studies of drug treatment. In general, almost all prices were obtained from published research papers and reports. Appendix D lays out these data sources and how they were used in much greater detail.

We use the second and third wave of data from the individual surveys to estimate the quantity of resources consumed/harms from victimization for both the drug court participants and the comparison cohort. These included a wide range of resources, including program activities (such as hearings and meetings with case managers) and program outcomes (such as use of government support and public services). Occasionally, when data were not directly observable (such as when a reported period of employment began), we used the baseline interview to guide interpolations. Other assumptions were guided by responses to the MADCE Adult Drug Court Survey (such as what types of drug tests each court used). Some information from site visits to the courts also was used, primarily to inform estimates of administrative costs of program participation (such as the frequency and length of staff meetings, and which staff members were in attendance). Some information about court personnel who attend hearings was developed from prior research on our sample (Bhati et al. 2008). Finally, we developed quantity estimates from administrative records, which were used to estimate (1) the number of arrests, (2) the number of crimes (from the number of arrests), and (3) the length and frequency of incarcerations. Appendix D also details how these data sources were used to estimate the quantities of resources use and behaviors.

Finally, we note that in order to be included in this analysis, an individual must (1) have completed at least the 18-month follow-up survey, (2) have consented to have administrative records searched, and (3) have been located in all administrative databases (arrest and corrections). Of 1,781 study participants, 1,474 met the first requirement. Of those, 1,341 met the second criterion, and of those 1,288 met the third criterion. See Chapter 3 in Volume 1 for more information on sample composition.

We performed these analyses in several iterations. First, we developed estimates of the price of drug court inputs, drawing from data specific to our drug court sample, and from national estimates as needed. Then, we used data from the MADCE 6- and 18-month surveys to estimate each individual's quantity of services used by the drug court and comparison cohorts. Next, we estimated what are traditionally labeled the "benefits" of drug court, including averted arrests, incarceration, and criminal victimization. We valued arrests and incarceration using a combination of national and local price estimates, and valued victimization using jury award data and NIBRS data from Roman (2009).

Overall, the individual harm metric can be conceptualized as the product of two different components: the quantity of resources used/harm of new offending and the price of those resources/harms. Thus, for each individual *i*, costs are the product of the price of each input and outcome (*k*), represented as:

$$\text{INDIVIDUAL HARM}_i = \text{PRICE}_k \text{ X QUANTITY}_{ik}$$

Thus, again, individual harm includes the price and quantity of drug court inputs and the quantity of a particular behavior (such as crime) and the price of that behavior (e.g., the costs of victimization). We estimated an individual harm measure for all members of both the treatment and comparison cohorts.

We then conducted several tests of bivariate differences between *groups* using aggregated individual estimates. In order to estimate the bivariate differences in operational costs between drug court and business-as-usual operations, we first compared differences in costs (price times quantity) for each type of service, and then compared costs at the service domain level (including five cost domains), and then across all domains. Next, we estimated the (bivariate) benefits of drug court by aggregating all three types of benefits (averted arrests, incarceration, and criminal victimization) within each *individual* and then compared the group means. Finally, we estimated the bivariate net benefits of drug court and aggregated all five types of costs with all three types of benefits within each individual, and compared the group means.

Some caution should be used interpreting the cost and benefit data in the bivariate analysis. In our final models, we do not distinguish whether an event is a "cost" or a "benefit," simply what the value of the event was and thus what the net benefit is. This, however, obscures some important data, such as the total cost of drug court and the benefit of drug court compared to business as usual. We use the bivariate analyses to make general estimates of costs and benefits. However, since this is not the focus of our analysis, we did not identify whether an event occurs during or after drug court (or business-as-usual case processing), since that has no bearing on net benefits estimation. And, we also note that the timing of some service delivery is not available in our data. Thus, for instance, we count all drug treatment as a drug court "cost" or a comparison "cost," regardless of when it occurred. Similarly, we count all prison in the "benefits" category. Since we do this for both the treatment and comparison groups, we do not anticipate that this introduces bias in the analysis, but it may cause the observed cost levels to be different than if we carefully adjusted for when a service was delivered. We also note, however, that *any* analysis that cleaves net benefits into separate "costs" and "benefits"' categories is inherently ad hoc since decisions about what to count as a cost and what to count as a benefit are inherently subjective.

Finally, we estimated individual net benefits by employing hierarchical linear modeling to account for the clustering of individual net benefits within courts. We note that we include many outcomes in this analysis that have rarely or never been considered in cost-benefit analyses of drug courts (Table 4-9.1).[39] We considered each outcome separately to estimate the impact of drug court on that outcome, and aggregated all outcomes to estimate the impact of drug courts on total social benefits.

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[39] More detailed information about variable construction and estimation can be found in Appendix D in this Volume.

The results indicate that drug courts appear to yield large (but statistically insignificant) net benefits to society, mostly driven by large reductions in criminal victimization, and that many social costs that drug courts were expected to reduce (welfare and health care use, for instance) are as high or higher among drug court participants. We note that the bivariate results are significant, and our use of hierarchical models introduces substantial variation into the model since the number of Level-2 (court-level) variables (N=28)[40] is so small.

Two other important features drive the results: there were several outliers in the comparison group with substantially larger cost estimates than were observed in the treatment group; this dramatically increased the overall estimate of a drug court net benefit. To address this, we first tested the hypothesis that these highly costly outliers were in the comparison group by chance and that drug court participants are equally likely to become high cost outliers. The results reject this hypothesis, indicating that drug court participants are considerably less likely to produce such drastically high social costs. Next, we conducted a Wilcoxon Rank Sum Test, which should be less sensitive to outliers and wide variation in outcomes. The results indicated that ranking individuals based on their net benefits to society (irrespective of magnitudes) indicates that drug court participants tend to yield higher net benefits. Finally, we exclude these outliers from the hierarchical analysis. The substantive findings are robust to the exclusion of these outliers. These three analyses indicate that while outliers are important to consider (from a statistical and practical standpoint), they do not drive the finding that drug court is cost-effective. We conclude this section by discussing how our results may differ from prior research.

We append to those results several analyses that take advantage of the rich data available to identify associations between the estimated net benefits of drug court and court practices. We examined the correlations between various types of social costs and social benefits to illustrate different drug court experiences. We also confirmed that the results are robust to different price levels by estimating the net benefits of drug courts using the full set of prices from each of the 28 sites, rather than the cross-site aggregate used throughout. We then analyzed for which types of individuals drug courts are most cost-effective by considering differences in drug use, criminal history, mental health, and life circumstances at baseline. We extend this analysis by illustrating how these methods can be used to estimate expected net benefits for each site, based on their population's characteristics and their own price structure. We demonstrate by estimating the net benefits expected to accrue to the largest four comparison sites if they implement a drug court under varying eligibility and enrollment scenarios. We conclude by discussing the limitations of this analysis and its implications for research and policy.

Results

Estimating the Additional Cost of Drug Court

In this section, we estimate the additional cost of drug courts. We first describe the development of price estimates for various inputs and outcomes, and then use MADCE survey data to estimate

[40] Since there was no variation in processing for North Carolina probationers, we counted North Carolina as a single site, and thus have one fewer court level observations than other analyses in this Volume.

the quantity of these inputs and outcomes used by drug court and non-drug court clients. We then put these two estimates together to develop an estimate of the cost of drug court. We note again that our main focus is on developing a single individual-level metric of net benefits, and we present these “cost” and “benefits” data as both a means of explaining how we developed the individual harms estimates and because of general interest in these estimates. Thus, we lump all inputs as “costs” regardless of when they occurred, and all outcomes as “benefits” again regardless of timing. We again note that this approach is no more or less arbitrary than any other approaches to estimating “costs” and “benefits,” and that it is that arbitrariness that leads us to focus on net benefits, rather than on separate “cost” and “benefits” estimates.

The Price of Drug Court Inputs and Outcomes

As discussed above, for each individual in the sample, we estimated both the price of program inputs and outcomes and the quantities of each individual’s use of these resources. Table 4-9.2 describes prices developed from data acquired from the MADCE courts. For some inputs, however, jurisdiction-specific prices were not available so we used data on national averages (Table 4-9.3). However, the prices of services vary substantially across sites, and we were thus concerned that our final estimates would be simply a function of differences in prices, rather than differential effects of drug court. To avoid conflating cross-site differences in prices and cross-site differences in outcomes, we created a single price level as the weighted mean of all site-specific prices, weighted by the number of participants in our study. This single price is used throughout our analyses unless otherwise indicated. Again, more information can be found in Appendix D.

Quantity of Drug Court Inputs

Next, we used data from the MADCE survey of both cohorts to estimate the amount of services used by each individual. Table 4-9.4 describes the average amount of each input used during the 18-month period of observation. We observed that drug court participants used significantly *more* of many key drug court components, including community service, drug tests, regularly scheduled hearings, case manager sessions, months in the program, days in residential treatment, sessions of individual outpatient treatment, sessions of group outpatient counseling, and nights in halfway houses. Drug court participants used significantly less days in prison, sessions with a probation officer, and nights in homeless shelters. No differences were observed in 10 of the 23 inputs.

The far right column multiplies the prices in Tables 4-9.2 and 4-9.3 by each individual’s use of each input. The value represents the average net benefit of differences in the use of that service for drug court participants. A positive value indicates that drug court participation led to the use of fewer resources and thus a positive net benefit. A negative value indicates that drug court participation led to the use of more resources and thus a negative net benefit. A significant reduction in the use of prison was the biggest net benefit of drug court. A significant increase in the use of residential drug treatment and significant increases in nights in halfway houses were the sources of the largest negative net benefits.

Table 4-9.2. Price of Inputs (and Outcomes) Calculated from MADCE Sites

Price Domain		Weighted Mean	Standard Deviation	Maximum
Court				
	Jail (Day)	$86.81	$25.68	$141.46
	Prison (Day)	$79.41	$19.90	$97.30
	Drug Court Administrative Costs (per enrollee per month)	$73.27	$71.51	$237.30
	Status Hearing (per hearing)	$15.17	$10.44	$48.52
	Other Hearing (per hearing)	$14.63	$9.15	$36.56
Personnel				
	Parole/Probation Officers (Hour)	$26.22	$4.19	$34.14
	Case Managers (Hour)	$25.48	$4.28	$39.54
Service Use				
	Homeless Shelter (Day)	23.22	$4.10	$28.08
	Public Housing (Month)	452.18	$126.55	$686.00
	Section 8 (Month)	445.93	$58.26	$527.00
Other				
	Wages (calculated from CPS)	$15.47	$2.41	$27.13
	Wages (minimum wage)	$6.48	$0.97	$8.07
Source: Semi-structured interviews and document reviews with MADCE courts (N=28 sites).				

Table 4-9.3. Price of Inputs (and Outcomes) Calculated from National Estimates

Price Domain		Price
Treatment (per day)		
	Residential Drug Treatment	$187.88
	Outpatient Group Counseling	$10.28
	Outpatient Individual Counseling	$79.36
	Medicinal (e.g., methadone)	$19.15
	Emergency Room (for drugs)	$3,399.94
	Emergency Room (for alcohol)	$4,101.56
	Hospital Detoxification (drugs)	$2,713.31
	Hospital Detoxification (alcohol)	$2,900.79
	Residential Mental Health Treatment	$167.50

(continued)

Table 4-9.3. Price of Inputs (and Outcomes) Calculated from National Estimates (Cont'd)

Price Domain		Price
Community Supervision		
	Halfway House (Day)	$48.88
	Electronic Monitoring (Day)	$7.74
	Drug Test (urinalysis)	$4.20
Source: External data sources (see Technical Appendix).		

Table 4-9.4. Quantity Estimates of Differential Use of Resources

	Differences[1]		
	Drug Court	**Comparison**	**Net Benefits**
1c. Other services and support			
Sessions of community service	0.4***	0	$13.58
2a. Monitoring			
Sessions with probation officer	7.8***	10.6	$46.82
Drug tests	59.3***	14.3	-$344.99
Days of electronic monitoring	1.9	1.6	-$1.87
2c. Courts			
Regularly scheduled hearings	16.8***	1.5	-$232.49
Other hearings	1.3***	1.8	$7.32
2d. Corrections			
Days in prison	29.7***	54.5	$2,438.48
Days in jail	9.9	9.6	-$26.04
Days in other incarceration[2]	6.2	4.9	$33.09
2e. Drug Court			
Sessions with case manager	14.1***	1.2	-$327.82
Months in program	12***	1.3	-$785.09
4a. Drug Treatment			
Days in ER	0.02	0.05	$80.20
Days in hospital detoxification	0.2	0.3	$387.20
Days in residential drug treatment	33.7***	15	-$3,504.44
Sessions of medicinal treatment	7.2	5.2	-$46.37
Sessions of individual outpatient counseling	15.0***	2.4	-$998.73

(continued)

Table 4-9.4. Quantity Estimates of Differential Use of Resources (Cont'd)

	Differences[1]		
	Drug Court	Comparison	Net Benefits
Sessions of group outpatient counseling	63.3***	15.2	-$490.55
4b. Medical treatment			
Days in hospital (non-drug related)	1.3	1.1	-$571.33
4c. Mental health treatment			
Days in residential mental health treatment	1.3	1.0	-$48.80
4d. Other services			
Nights in homeless shelter	2.4**	5.1	$64.36
Nights in halfway house	69.3***	20.5	-$2,382.09
Days in public housing	2.9	2.3	-$10.27
Days in Section 8 housing	2.9	2.3	-$20.42

Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=28 sites; N=1,288 respondents.
Note: Significance levels are based on group mean comparison tests. All analyses are conducted using sampling weights.

[1] *This analysis does not account for clustering of individuals within courts.*
[2] *Some jurisdictions reported days spent in a correctional facility without designating whether it was a jail or prison. Sentences less than 1 year were assumed to be jail sentences. To value the cost of the remainder of these days, we used the simple mean of the daily cost of prison and jail.*
** p<.05, ** p<.01, *** p<.001*

Costs of Drug Court

To develop an estimate of the direct cost of drug court, we follow the extant literature, and count the additional costs of drug court typically considered to include additional court hearings, drug treatment, and drug tests in addition to meetings with drug court case managers and the administrative costs of drug court, costs which, it is often assumed, the comparison group does not incur. We consider these costs, here, using independent sample t-tests.[41] On average, drug court participants received roughly $455 in drug tests, compared to only $110 among comparison individuals. Thus, drug courts cost roughly $345 per participant in drug tests ($p < 0.001$). Equally large was the difference in court resource use. Hearings for the comparison group cost, on average, $56, compared to $417 for drug court participants. Thus, the court costs of drug court participation are roughly $361 ($p < 0.001$).

[41] These t-tests do not account for the hierarchical nature of the data. Further extensions that do address this issue are conducted later.

However, these are relatively small costs. The larger costs include case management, drug treatment, and administrative costs. The average drug court participant cost $897 in administrative costs (valued at $73 per enrolled month[42]), and used $358 in case management. By far, the most sizable cost was drug treatment. In our sample, the average treatment usage among drug court participants was $8,979 during the 18 months following program entry, roughly double the $4,407 for the comparison individuals in the same time frame. Thus, the treatment costs of drug court amount to $4,572 ($p < 0.001$). Based on summing means of each category, we estimate that drug court participation costs roughly $6,533 more per individual than the alternative. These differences, estimated at 6 months and for the full 18-month period, are displayed below in Table 4-9.5.

Table 4-9.5. Bivariate Estimate of Drug Court Costs

	First 6 Months			Full 18 Month Follow-up		
	Drug Court	**Comparison**	**Difference**	**Drug Court**	**Comparison**	**Difference**
Drug Tests	$190	$40	$150***	$455	$110	$345***
Hearings	199	24	175***	417	56	361***
Administrative Costs	388	0	388***	897	0	897***
Case Management	136	0	136***	358	0	358***
Drug Treatment	4,712	1,764	2948***	8,979	4,407	4,572***
Total	**$5,625**	**$1,828**	**$3,797**	**$11,106**	**$4,573**	**$6,533**

Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=28 sites; N=1,288 respondents.
Note: Significance levels are based on group mean comparison tests. All analyses are conducted using sampling weights.

** p<.05, ** p<.01, *** p<.001*

Estimating the Benefits of Drug Court

Next, we estimated the benefits of drug court. As noted above, our individual harms/net benefits variable does not distinguish between costs and benefits, only the total use of a resource. So, it does not matter that drug court participants use more residential treatment both during (increasing costs) and after (decreasing benefits), only that both values are negative (drug court uses more of that resource). However, it is valuable to consider how the benefits generally compare to the costs before proceeding to estimation of individual net benefits.

We focus our benefits estimation on outcomes alone, and do not count inputs that occur after program participation ends. Our outcomes include: costs to victims of crime, criminal justice system costs of arrest and incarceration, wages, and governmental and other support. The price of each of these—except costs of criminal victimization—can be found in Tables 4-9.2 and 4-

[42] This figure is a weighted average of administrative costs for each drug court in our sample, weighted by the number of participants in the study. There was tremendous variation in administrative costs of drug courts, ranging from $1.84 per participant per month in King County to $237 per participant per month in Wayne County, which had a full-time program director for only 25 participants.

9.3, and the quantities can be found in Table 4-9.4. A description of the method used to estimate costs of criminal victimization can be found in Appendix D.

To estimate benefits, we first conducted an independent samples t-test on the weighted samples[43] to determine whether drug court resulted in a statistically significant decrease in crime, arrests, and incarceration, measured using official records (Table 4-9.6). The mean control group participant committed $16,887 worth of crime during the 18 months following study enrollment. Drug court participants, on average, committed only $7,111 worth of crime. We estimate that drug court resulted in $9,776 of victim crime costs prevented during the 18 months following program entry (p = 0.001).[44] We estimate that drug court reduced police arrest costs from $115 per individual to $44 per individual, for a savings of $71 per participant (p = 0.001), and reduced costs of jails and prisons from $5,441 to $2,768, for total savings in corrections in the 18 months following program entry of $2,673 per drug court participant (p < 0.001).[45] Aggregating across savings from prevented crime, arrests, and incarceration, drug court produces, on average, $11,408 of benefits.

Table 4-9.6. Simple Estimate of Drug Court Benefits

	Drug Court	Comparison	Difference
Victimization (mean costs)[1]	($7,111)	($16,887)	$9,776***
Victimization (median costs)	(3,480)	(6,610)	3,130**
Arrests	(44)	(115)	71***
Incarceration	(2,768)	(5,441)	2,673***
Employment	18,982	19,909	(927)
Government financial support	(2,129)	(1,056)	(1,073)***
Other financial support	(2,270)	(3,158)	888**
Total (with mean victimization costs)	**$4,660**	**($6,748)**	**$11,408*** **

Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=28 sites; *N=1,297 respondents.*
Notes: The parentheses signify negative values, such that the overall the benefit is less than zero. Thus, the smaller the negative benefit the better, meaning that there is less loss to society. Significance levels are based on group mean comparison tests. All analyses are conducted using sampling weights.
[1] Using median costs of victimization, the average benefit of treatment is $8,291, the average benefit to the comparison is $3,529, and the average net benefit (the difference) is a positive value, $4,762.

* *p*<.05, ** *p*<.01, *** *p*<.001

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[43] In this analysis, all of the weights are used in the same manner as in Chapter 2. That is, the appropriate super weight given the sampling wave of the outcome variable is used in every analysis.
[44] For theoretical reasons previously discussed, the use of mean costs and probabilities is preferable to median costs. Accordingly, these calculations were made using mean costs of crime (Roman 2009). If, instead, median costs of crime are used (Roman 2009), comparison individuals committed, on average, $6,610 in crime, compared to $3,480 among participants, yielding $3,130 in benefits (p = 0.017). Other median analyses are available from the authors.
[45] This includes additional short-term stays as sanctions among drug court participants.

Estimating the Net Benefits of Drug Court

In this section, we describe two analyses to estimate the net benefits of drug courts using our individual harm metric and dispensing with the arbitrary distinction between costs and benefits. First, we simply estimated the bivariate differences across all drug court and no-drug court observations. Then, we conducted a hierarchical linear model using propensity score weights to adjust for both selection and attrition to arrive at a final estimate of the net benefits of drug courts. We follow the discussion of these models with a brief discussion of the effect of outliers on these estimates, and then discuss the differences between our approach and extant estimates.

Using independent samples t-tests, we found that the average comparison individual cost society $14,575 during the year and a half following their initial arrest. We estimate that the average drug court participant cost society only $12,362. The difference, $2,213—the net benefit—is nearly identical to those estimated above. However, the standard error of this estimate is $3,682, making the t-statistic 0.60 and the associated p-value 0.548, which is much higher than previous analyses indicated. Thus, at the bivariate level, there is no significant reduction in net benefits for drug court participants. And, as we discuss later, introducing the true variation in participants' experiences (both costs *and* benefits) significantly increased the variation in net benefits.

Accounting for the Hierarchical Structure of the Data

The previous section describes the results of weighted t-tests on the sample, which is equivalent to an Ordinary Least Squares estimator where the only covariate was drug court/non-drug court status. As discussed in Chapter 2 and Appendix A, these bivariate methods do not account for the hierarchical nature of our sample data. Individuals within our sample are clustered within particular courts. Ignoring this fact has been shown to bias estimates, particularly by adding upward bias to the statistical significance of findings.

Following the methods described in Chapter 2 and Appendix A, we employ hierarchical models of the net benefits of drug court, where individuals are considered Level-1 observations and the courts in which they are nested are considered Level-2. We first conducted simple hierarchical linear analyses where the only covariate was whether the individual belonged to a drug court or a comparison site (a Level-2 variable). These analyses replicate the impact analyses in Chapters 3, 4, and 5, including the use of propensity scores, and account for the nested structure of the data.

Table 4-9.7 describes the results of a series of hierarchical linear models. Each of these models tests only the drug court indicator (1=drug court) along with the propensity score weight in hierarchical models. The last row is the effects of drug court on the aggregate net benefits (including all domains), while the preceding rows display the effects of drug court on each domain separately. Treatment and control differences, statistical significance, and intraclass correlation are displayed.

Overall, we found that drug court participants cost society $11,206 to $13,102 during the 18 months we observed their behavior, depending on the assumptions made about their earnings (see Appendix D). However, those who do not receive drug court cost society $16,886 to

$19,310 during the same period. The difference in the social costs—the net benefits—is considerable, totaling between $5,680 and $6,208, but is not significant (p=0.76; p=0.40). Virtually all of the benefit of drug court appears to be to people not involved in drug court, e.g., private citizens who are not victimized. Drug court appears to prevent about $11,566 (p=0.03) in criminal victimizations compared to not receiving drug court. The other benefits are a substantial average reduction in the use of corrections ($2,795) that is marginally significant (p=0.09) and a modest average reduction in the use of policing resources ($120) that is statistically significant (p=0.04).

Table 4-9.7. Drug Court Outcomes by Type

Outcome	Drug Court Mean	Comparison Group Mean	Difference (net benefit)	P-Value	ICC
Employment[1]	$17,943	$15,596	$2,347	0.74	0.054
Employment[2]	$19,833	$18,029	$1,804	0.55	0.054
Community Service	520	336	184	0.35	0.048
Monitoring	(648)	(240)	(408)	<0.01	0.217
Police	(45)	(165)	120	0.04	0.078
Courts	(500)	(57)	(443)	<0.01	0.460
Corrections	(2,467)	(5,262)	2,795	0.09	0.136
Drug Court	(1,219)	(112)	(1,107)	<0.01	0.111
Crime[3]	(6,665)	(18,231)	11,566	0.03	0.023
Drug Treatment	(9,730)	(3,789)	(5,941)	0.05	0.141
Medical Treatment	(2,543)	(2,047)	(496)	0.50	0.001
Mental Health Treatment	(286)	(138)	(148)	0.66	0.092
Halfway Houses	(2,998)	(939)	(2,059)	0.04	0.084
Shelters	(50)	(95)	45	0.30	0.010
Public Housing	(80)	(52)	(28)	0.66	0.007
Gov. support	(2,248)	(1,032)	(1,216)	0.04	0.047
Family/friends support	(2,393)	(2,681)	288	0.70	0.020
Total[1,3]	($13,102)	($19,310)	$6,208	0.40	0.036
Total[2,3]	($11,206)	($16,886)	$5,680	0.76	0.039

Source: Urban Institute MADCE Substance-Abusing Offenders Surveys at baseline, 6-months, and 18-months. N=28 sites; N=1,288 respondents.
Notes: In this table, the parentheses signify negative values, that is, they signify that the overall the benefit is less than zero. Thus, the smaller the benefit the better, meaning that there is less of a loss to society.
[1] Valued using minimum wage.
[2] Valued using "average" wage calculated from similar respondents to the Current Population Survey.
[3] Valued using average victim costs of crime (Roman 2009). Estimates using median costs of crime are available from the author.

There are many significant increases in costs from drug court participation. Drug court participants use substantially more drug treatment services ($5,941, p=0.05), more days in halfway houses ($2,059, p=0.04), and receive more government support ($1,216, p=0.04). The cost of administering the drug court is also substantial ($1,107, p<0.01). We also note small and significant increases in costs associated with other court processing and other monitoring. Among other high value items, drug court participants earn roughly $2,000 more during this period than the comparison (depending on assumptions about wages), but the difference is not statistically significant (p>0.50).

The intraclass correlation (in the far right column) measures how much of the variation in a particular variable is explained solely by assignment to a particular drug court. Though we use the same prices across drug courts, it is clear that many of the standard drug court costs—hearings and monitoring, in particular—are largely court-specific. Nearly half of the between-individual variation in hearings costs, for example, is due to the drug court. This finding, suggests (1) that the drug courts within our sample have different program models, which causes variation in the standard practices across the courts, and (2) that within-court practices are consistent across all individuals, because there is relatively little variation among individuals within the same court.

This is also somewhat true of drug treatment costs where 14 percent of the variation is explained by court. Similarly, more than 10 percent of the variation in incarceration costs is explained by court. One possible explanation for why treatment and incarceration are correlated within courts is that the courts use different program models. Some courts are treatment-intensive, while some are sanctions-intensive, and rely on less treatment. For more information on the differential effects of different drug court policies and practices see Chapter 7 (see also Harrell et al. 1999).

Testing the Effects of Outliers

A closer examination of our dependent variable—net harms—reveals that there is tremendous variation in participant outcomes, across courts and across individuals within the same court. The following plot (Figure 4-9.1) displays the net harms of each individual in the sample (N = 1,288), by court. Black dots represent individuals in drug courts, and red dots identify individuals in comparison sites. The courts are along the x-axis, and the net benefits to society are along the y-axis. Each dot represents an individual. The black and red horizontal lines indicate fixed effects means of drug court participants and comparison individuals, respectively.[46] A circle parallel to the bottom value of -1e+06 indicates that individual totaled $1 million in social harms (negative net social benefits).

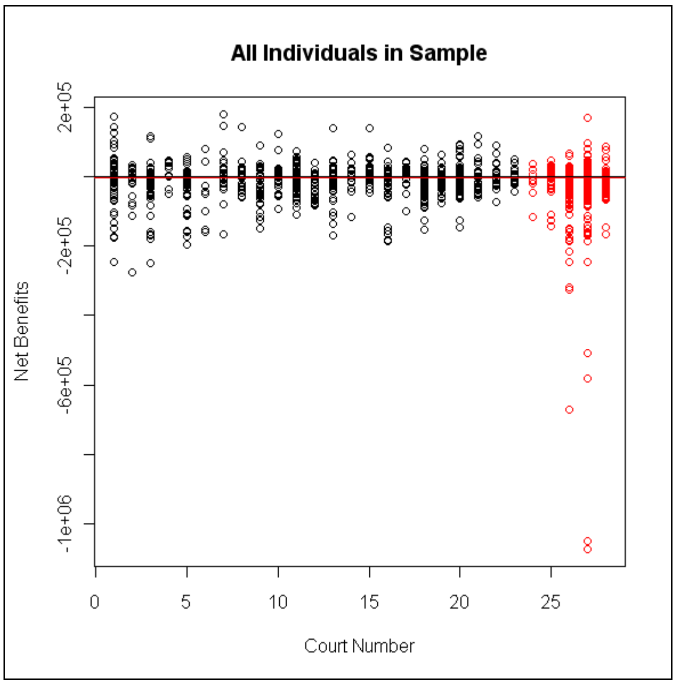
The first striking result from Figure 4-9.1 is that a few individuals spread across the comparison sites are tremendous outliers. Upon excluding these outliers, it becomes difficult to discern any systematic difference in outcomes (see Figure 4-9.2). These plots beg the question of the impact of these outliers, which are described in more detail in Appendix E. In particular, is the mean difference in net harms, though not statistically significant, driven by these outliers?

[46] If there are J courts, each with court mean m*j* and variance v*j*, then the fixed effects mean is the weighted mean of court means, weighted by the inverse variance of each court. Thus, (1/v*j*)(m*j*)/[J/v*1* + … + J/v*J*].

The Wilcoxon Rank Sum Test

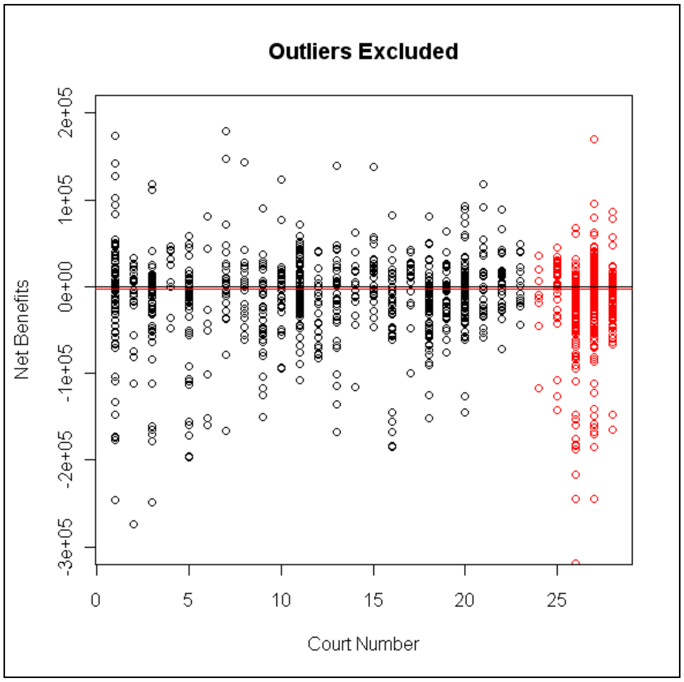
To answer this question, we first conducted a Wilcoxon Rank Sum Test to determine whether drug court participants tended to have higher net benefits than their comparison counterparts, ignoring the magnitude of these differences (Lehman and D'abrara 1975). Because the Wilcoxon test considers only the ordering of sample participants, it is not sensitive to extreme values like those displayed above (Bhattacharyya, Johnson, and Neave, 1971). It considers a comparison sample member whose net benefits are $1,000,000 less than those of the lowest drug court participant no differently than a comparison group member whose net benefits are $1 lower than those of the lowest drug court participant.

Figure 4-9.1. Distribution of Net Benefits with Outliers in the Comparison Group



More specifically, the Wilcoxon Rank Sum Test (a variation of the Mann-Whitney U Test) ranks all participants within the full sample, so that the lowest observation is scored a one and the highest is scored N. For each subgroup, the ranks are then summed. When N > 20, under the null hypothesis that both groups come from the same distribution, this sum of ranks is normally distributed with mean 0.5(n_a)(N+1) and standard deviation $\sqrt{\frac{n_a n_b (N+1)}{12}}$, where n_a individuals are in the first group, n_b individuals are in the second group, and $n_a + n_b = N$ individuals are in the full sample (Cohen 2001). Clearly, then, a z statistic can be calculated, under the null hypothesis, by subtracting the mean and dividing by the standard deviation.

Figure 4-9.2. Distribution of Net Benefits without Outliers in the Comparison Group



The corresponding z-statistic was calculated to determine whether, based on ranks, drug court participants and comparison individuals appeared to have the same net benefits to society. The average ranking of the drug court sample was 658 (out of 1,288), while the average ranking of the comparison sample was 618, where lower ranks indicate lower net benefits (greater net harms). Thus, based simply on means, drug court participants appear to have typically produced greater net benefits than the comparison sample. The corresponding z statistic was 1.85, which is marginally significant ($p = 0.065$). Thus, we can reject the null hypothesis that the net benefits of drug court participants and comparison individuals are drawn from the same distribution, and conclude that drug court participants tend to yield greater net benefits to society, irrespective of the influence of outliers on mean outcomes.

The Probability of Outliers

The seven individuals with net harms greater than $300,000, clearly displayed as outliers in Figure 4-9.1, are important in their own right. Appendix E details these seven individuals and the outcomes that resulted in so much estimated harm to society. To varying degrees, they were all comparable to the drug court population and could have been reasonably expected to participate in a drug court program if one was offered in their jurisdiction. Though they seem like outliers in the data, they hold important information. While it is a relevant and important question to ask whether drug court improves the outcomes of the typical participant, it is a separate, but equally important question to ask whether drug court impacts the likelihood that a single drug-involved offender will result in substantial harms to society. From another perspective, this is the same as asking whether drug court improves the outcomes of the (potentially) most serious offenders.

If we define "extremely costly individuals" arbitrarily to be those with social costs greater than \$300,000 (those considered here), we can calculate the probability that the drug court sample and comparison sample have the same likelihood of producing extremely costly individuals. Seven of the 440 comparison individuals were extremely costly, making the proportion 1.6 percent. If the same proportion of drug court participants was extremely costly, we would expect that our sample of 848 would produce 13 extremely costly individuals. Instead, our sample of 848 produced zero. Using the cumulative distribution function for the binomial distribution, we can calculate the probability that zero out of 848 individuals would be extremely costly, given that the true probability was 1.6 percent. This probability is just over 1 in a million (1.24 in 1 million). Thus, we find it implausible that drug courts have a 7/440 chance of producing extremely costly individuals.

Instead, we can assume that this particular comparison sample was a fluke, and that the true proportion of comparison individuals is lower. Seven individuals out of 440 could be the upper end of the 95 percent predictive interval if the true probability were 8/1000 or the upper end of the 99 percent predictive interval if the true probability were 6/1000. Given these "true" probabilities (our null hypothesis), the probability that 848 drug court participants would produce zero extremely costly individuals is 0.001 and 0.006, far below conventional significance levels. In fact, a simple t-test for difference in proportions indicates that the corresponding t-statistic is 3.28. Thus, we can confidently assert that drug courts are less likely than comparison courts to produce extremely costly individuals. This information—along with, or instead of, the estimated net benefits from drug court—is important for policy-makers to consider, and should not be disregarded.

Hierarchical Models without Outliers

Finally, we adopt a more traditional approach to analyzing outcomes in the presence of outliers, which is to exclude the outliers. This method is concerning because outliers often contain important information about the underlying distribution. If the outliers are genuinely drawn from the same distribution as the rest of the sample, then it is important to include them because they represent improbable, but distinctly possible outcomes. As discussed in Appendix E, this seems to be the case. Regardless, it is an important policy question to ask whether drug courts pose net benefits in addressing only the population of "typical" drug offenders, or whether the benefits are driven entirely by preventing a few serious cases.

We replicated the hierarchical analysis of the net benefits to society excluding the seven outliers from our sample. The estimated net benefits of participating in drug court declined from \$6,345 per participant to \$1,778. The statistical significance of this difference also declined, from p = 0.40 to p = 0.81. This is a result from the significantly higher average net benefits per comparison group participant (rising from -\$19,310 to -\$14,642). In spite of this, however, as confirmed by the above analyses, drug court participation does appear to yield small net benefits to society.

Limitations

This study is not without its limitations. For one, self-reported data was used to estimate almost all social costs and benefits. While self-report data has its advantages, dishonesty and poor recall are constant threats to the internal validity of all estimates discussed here. In particular, exaggerations in the number of treatment episodes could quickly inflate social costs, given the expense of some treatment modalities. Exaggerations of earnings can have an equally pronounced effect in the opposite direction.

Further, many of the positive findings presented here are driven largely by the effects of drug court in reducing crime. The costs of crime are substantial, which motivates the development and analysis of programs like drug courts in the first place, but no one has fully determined how large yet. The figures used here were based on research by Roman (2009), which is only a first step towards fully understanding the volatile and important phenomenon of the costs of crime. We chose to use mean costs of crime to reflect the probability that a given victimization will be particularly heinous and costly, but should one choose the median costs of crime (ignoring this probability in favor of considering the costs of a "typical" victimization), the results change considerably (see Table 4-9.6).

Additionally, our estimates of the impact of drug court participation on crime are based solely on administrative data. We assumed that the number of arrests is proportional to the number of offenses committed. We estimated this proportion using national trends on the proportion of crimes that result in arrest. If either of these assumptions fall, our estimates of the benefits of averted crime could be considerably distorted. We see no desirable alternative to the methods employed here. It is the nature of criminal justice research that the outcome of prime interest is inherently difficult to observe, given that the parties in the study have an incentive to conceal their involvement.

In addition to the use of administrative data to estimate the reduction in criminal activity, we relied primarily on national-level data to estimate the costs of different drug treatment modalities, rather than directly observing the treatment providers as is often done. This method provides some benefits. Because national prices are drawn from a large number of treatment providers, estimates are more generalizable to other settings and jurisdictions. Further, top-down data collection from one or two treatment providers closely associated with the drug court (as is often done) misses private providers. To the extent that private providers are more costly than publicly-funded and prescribed providers, this method understates the true resource cost associated with drug treatment. That said, we attempted to use information provided directly by sites in our study to estimate the site-specific costs of drug treatment. However, low response rates limited this utility. We still used the information available, however, it was largely supplemented by larger, national estimates.

Finally, small sample sizes in many of the sites in the study limited the ability to examine, in depth, the relationships between various court policies and their potential social costs and benefits. We were unable to analyze treatment-intensive courts separately, for instance, or to truly understand how court procedures can translate into both impacts and resource use. This information is particularly relevant for jurisdictions considering implementing their own drug courts, as a wide range of models are available. Each is likely to serve a different purpose, and it is improbable that there is a single "best" model that does not depend on the specific population being treated or the relevant price level of the jurisdiction.

Conclusion

In this analysis, we created a single aggregate variable at the individual level to incorporate all measured outcomes. By doing so, we estimate a single individual's total impact on society and can statistically test the effects of drug court and other characteristics on that impact. Findings suggest that the average drug court participant still does more harm to society than benefit. However, participating in drug court appears to lower this harm by between $5,600 and $6,200 per participant. This difference, though, is not statistically significant. This is due mainly to the considerable variation in outcomes. We note that additional tests, including the Wilcoxon Rank Sum Test, suggest that there is a positive benefit of drug court, even if the aggregate net benefits are not significantly different. We also note that although the largest outliers are all within the comparison group, removal of these outliers does not meaningfully change our results.

Thus, we are left to conclude that drug courts—while effective at reducing costly criminal offending—are also expensive enough to offset those costs. Drug court increases many costs, notably drug treatment, halfway house usage, and court-related monitoring by substantial amounts. Drug courts do not appear to have much of an effect on resources in two areas where big benefits are possible—improved labor market participation and health. While drug court improves outcomes on both measures, neither improvement is statistically significant. Even the crime improvements are not particularly robust: when a small number of outliers are removed (whose costs are mainly from the commission of serious crimes), most of the benefit of drug court disappears. We interpret this as meaning that most of the crime reduction is from reductions in low-level offending, with only a few serious crimes being prevented. The main positive finding, however, that drug courts appear to make those who would commit the most serious harms less harmful to society is not a trivial one. Other studies have found that homicide in the United States accounts for roughly half of all the harms from crime (Roman 2009). Thus, reducing a very small number of the most serious crimes would have a substantial impact on net social welfare.

Beyond these findings, we note that a traditional cost-benefit analysis would likely have found positive and significant benefits. One advantage of the within-individual analysis conducted here is that it avoids a bias toward finding significant effects. We replicated a traditional cost-benefit analysis of drug courts and estimated court-level net benefits using our data where we first test for group differences in each cost/benefit category (i.e., court, health, etc.). We then summed the mean differences in each category to estimate the aggregate effect of drug court. Following that strategy, which is a common one, we estimated that drug court participation costs roughly $6,533 (identical to our estimate of costs in Table 4-9.5). If you compare this figure to the $12,520 benefits of drug court that accrue from the most commonly studied drug court benefits, we estimate that the net benefits of drug court participation are $5,987 per participant, which is very close to our final estimate of net benefits. The corresponding benefit-cost ratio is 1.92:1. This is closely comparable to the estimate from meta-analytic data in WSIPP (2003) of 1.74:1. To contextualize these results, Appendix F compares findings from past cost-benefit analyses of drug court with comparable results generated by the present data. Clearly, however, stating that the net benefits are almost twice the costs obscures a much more complicated story with a much more ambiguous conclusion.

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Chapter 10. What Have We Learned From The Multi-Site Adult Drug Court Evaluation? Implications for Policy, Practice, and Future Research

Shelli B. Rossman and Janine M. Zweig

Introduction

As described in Chapter 1, Volume 1 of this report, drug courts emerged spontaneously during the late 1980s and early 1990s in response to burgeoning drug offender arrests and prosecutions that overwhelmed the capacity of numerous courts to expeditiously process such cases. Nearly three dozen drug courts were established prior to the passage of Title V of the Violent Crime Control and Law Enforcement Act of 1994 (Public Law 103-322)—also known as the 1994 Crime Act—that authorized the Attorney General to (1) award and administer discretionary grants to states, local governments, Indian tribal governments, and state or local courts to plan, implement, or enhance drug courts in which judges continuously supervised the progress of nonviolent offenders with substance abuse problems; and (2) provide for a national evaluation of the impact and effectiveness of the federal grants.[47] A national evaluation was contemplated in 1998, but not pursued.

Subsequently, in 2002, the National Institute of Justice (NIJ) commissioned the *National Drug Court Evaluation Multi-Site Longitudinal Impact Study* (NIJ 2002)─later renamed *NIJ's Multi-Site Adult Drug Court Evaluation (MADCE)*─intended to conduct offender-based, longitudinal research to evaluate the impact of drug court participation on post-program outcomes, specifically, recidivism.

In response, researchers within the Urban Institute's Justice Policy Center (UI-JPC), RTI International (RTI), and the Center for Court Innovation (CCI) teamed to conduct the MADCE, beginning in 2003, whose key objectives were to:

- Test whether drug courts reduce drug use, crime, and multiple other problems associated with drug abuse, in comparison with similar offenders not exposed to drug courts.
- Address how drug courts work and for whom by isolating key individual and program factors that make drug courts more or less effective in achieving their desired outcomes.
- Explain how offender attitudes and behaviors change when they are exposed to drug courts, and how these changes help to explain the effectiveness of drug court programs.

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[47] Until the 1994 Crime Act, there was no federal grant program specifically designed for drug courts. However, some drug court programs received funding or technical assistance from the Bureau of Justice Assistance (BJA) or Department of Health and Human Services' Center for Substance Abuse Treatment (CSAT).

- Examine whether drug courts generate cost savings for the criminal justice system and other public institutions.

This chapter provides an overview of NIJ's Multi-Site Adult Drug Court Evaluation; summarizes key findings from the outcome, impact, and cost-benefit components of the study; and identifies implications for practice, policy, and future research.

The MADCE Conceptual Framework

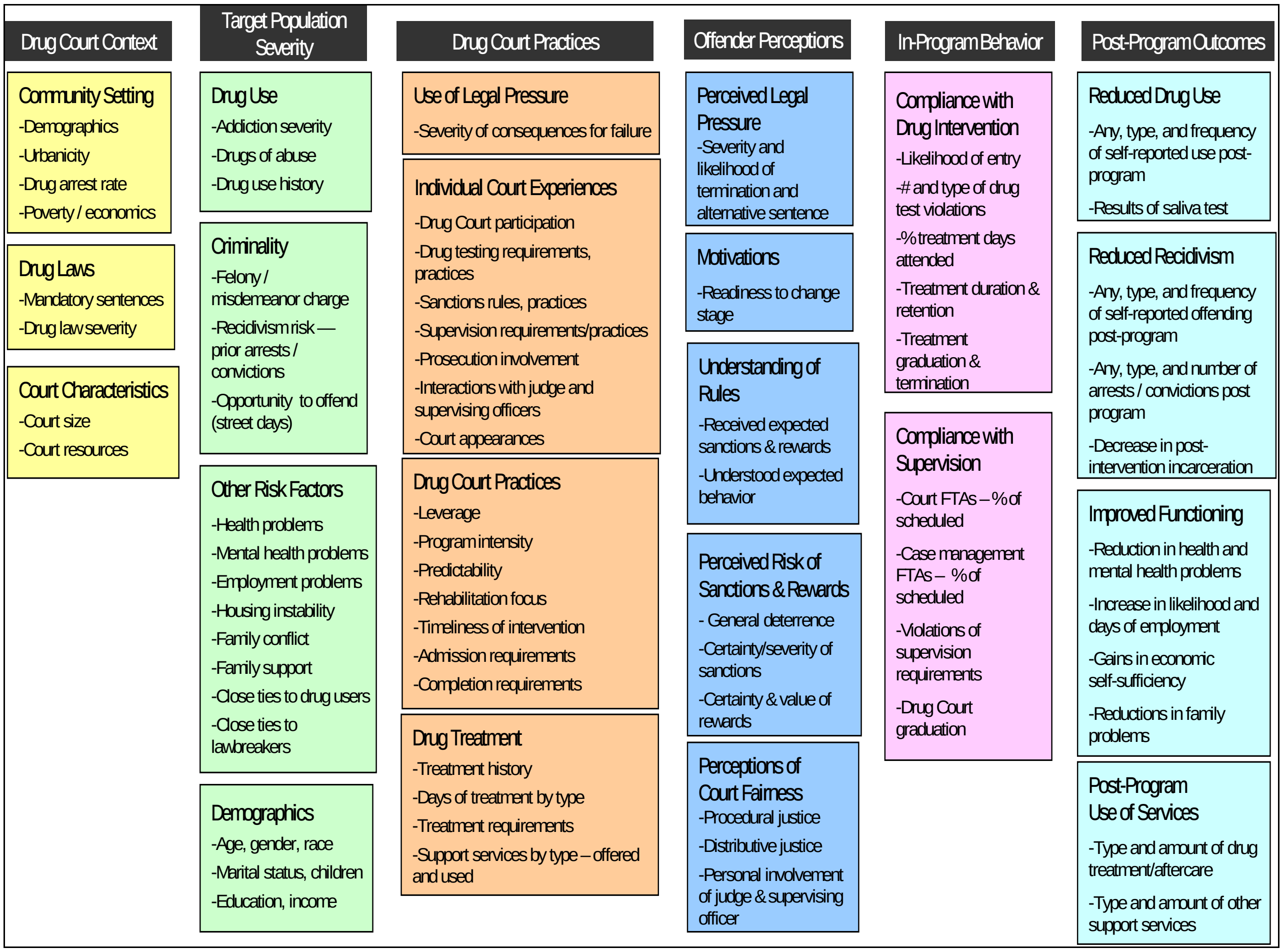
Building on earlier conceptualizations of drug courts shaped by Temple University (Goldkamp, White, and Robinson 2001), the Urban Institute (Butts, Roman, et al. 2004), and RAND (Longshore, Turner, et al. 2001), UI and its partners developed a conceptual framework (see Figure 4-10.1) to guide the MADCE. Despite the centrality of the goal of reducing drug use, most prior drug court evaluations relied on recidivism as the sole measure of impact. MADCE, however, was planned to measure both intermediate and end outcomes (as shown in the three columns on the right)—individual attitudes, behavior in compliance with treatment and supervision requirements, reductions in drug use and crime, improvements in psychosocial functioning, and continued use of pro-social services, including ongoing treatment—based on information self-reported by subjects, and supplemented and validated by criminal records and drug testing.

In our conceptual model, post-program outcomes are hypothesized to result from the behavior of offenders while under supervision of the court and, in particular, their participation in drug treatment and compliance with drug court supervision. The additional measurement of offender perceptions (third column from the right) is an important clarification to existing frameworks: participant perceptions and responses to court practices are hypothesized to be the process that leads to behavior change.[48] Drug court strategies combine coercion and persuasion with the goal of encouraging treatment participation and abstinence; both court program experiences and drug treatment are expected to move clients to abstinence and crime-free lifestyles by increasing internal motivations to desist.

Drug court operations are hypothesized to affect offender perceptions, in-program behavior, and post-program outcomes, directly and indirectly. The domains of drug court practices (shown in the third column from the left) include: drug treatment, legal pressure, monitoring/offender accountability procedures, and supervision style. We anticipated selecting drug courts that represented considerable variation along the following critical dimensions:

[48] These mechanisms had not been directly examined by extant evaluations when MADCE was initiated; however, Gottfredson, Kearley, et al. (2007) later used a randomized experiment in the Baltimore, MD, drug court to inform a structural equation model of the mediational pathways through which drug court promoted desistance from crime.

Figure 4-10.1. MADCE Conceptual Framework



- *Legal Pressure*. Existing research indicates that legal pressure, defined by the severity of the alternative sentence, is positively correlated with treatment retention (Condelli 1989, Rempel and DeStephano 2001). However, findings on legal pressure are mixed, suggesting that perceptions of legal pressure mediate the relationship (Young 1997; Young and Belenko 2002).

- *Monitoring/Accountability*. The day-to-day use of drug tests, judicial review hearings, and case management are hypothesized to be very important determinants of offender perceptions and behavior, based on theory and findings from earlier studies (Harrell 1998; Harrell, Cavanagh, and Roman 1999; Harrell and Kleiman 2001).

- *Judicial Interaction and Supervision Style*. The drug court model simultaneously stresses the importance of sanctions and rewards; consequently, different courts and judges tend to strike widely varying balances between positive and negative reinforcement, or both. We planned to measure this domain and isolate distinct supervision strategies that are effective or ineffective by using participants' self-report and structured courtroom observations (Satel 1998).

- *Drug Treatment*. One of the most important, yet difficult to measure, variables in drug court is the treatment. Although measurement in these areas is not well developed, the framework recognizes the potential value in measuring not only the amount and modality of treatment delivered to drug court participants, but also its quality, comprehensiveness, and cultural appropriateness (see Johnson, Hubbard, and Latessa, 2000). At minimum, quality treatment uses procedures that are documented ("manualized" for consistent replication), demonstrated to be successful, and meet professional certification standards for staff training and content.

The two columns on the left describe differences in the context and characteristics of participants hypothesized to affect drug court outcomes. Differences in eligibility criteria and screening procedures mean that the addiction severity and risk of recidivism are far higher in some courts than in others. In addition, regional variation in drug use patterns may affect outcomes, as may local law enforcement and prosecutorial decision making. Court location also may affect the resources available to support recovery.

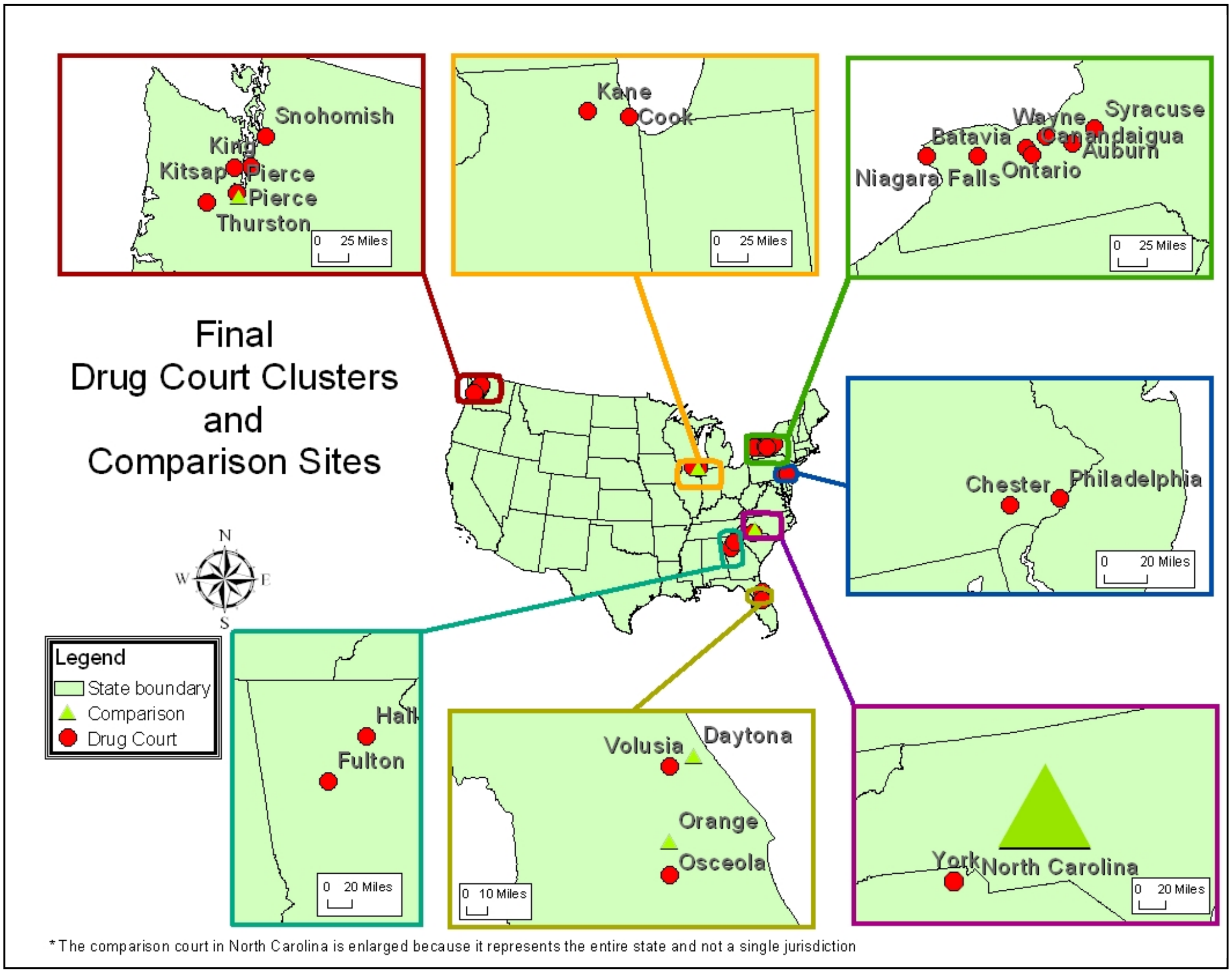
The MADCE Sample

The MADCE used a quasi-experimental design for which we conducted an extensive site selection process to identify drug court and comparison sites that met basic evaluability criteria and collectively reflected substantial variation in substance abuse treatment intensity, the leverage the courts have in monitoring clients, and court sanctioning and supervision policies. Between February and June 2004, we administered a web-based survey to 593 active adult drug courts that were in operation for at least one year at that time, of which 380 courts (64 percent) completed the survey (see Volume 2).

We used the data from the MADCE Survey of Adult Drug Courts to choose 23 drug courts located in 8 states, constituting 7 geographic "clusters." Additionally, we selected six "comparison" sites to represent the diverse set of activities employed in jurisdictions that do not implement drug courts. Comparison conditions included several Treatment Alternatives for Safe Communities (TASC) programs, a Breaking the Cycle program, and standard court-referred, probation-monitored treatment.[49] Figure 4-10.2 presents the locations of the MADCE drug court and comparison sites.

[49] In North Carolina, comparison respondents were drawn from a statewide probation dataset that identified substance abusers. Since the state is divided into two judicial districts, we divided that cohort along jurisdictional lines and treated those respondents as representing two comparison sites.

Figure 4-10.2. MADCE Drug Court Clusters and Comparison Sites



MADCE Research Strategy

NIJ's MADCE research—which constitutes the largest, most comprehensive study of drug courts to date—comprises four major components: *process, outcome*, and *impact evaluations* and a *cost-benefit analysis*. The **process evaluation** combines data from site visits, courtroom observations, and offender interviews to detail how drug courts vary in their determinations of program eligibility; case management and supervision, including drug testing; use of treatment and other social services; program participation and graduation requirements; and court operations and management approaches, including team collaboration, decision making, and other key policies and practices.[50]

The **outcome** (i.e., drug court only) and **impact components** (i.e., drug courts versus comparisons) address the study's main research objectives:

[50] See Volume 3 of the MADCE Final Report for process evaluation findings.

- Drug court and comparison group members are compared on key outcomes—self-reported drug use, self-reported and official recidivism, and psychosocial outcomes to answer the question, *"do drug courts work?"*

- Analyses determine the extent to which drug courts have a greater or lesser impact on key categories of offender, such as high- versus low-risk offenders, more- versus less-severely addicted individuals, and those with or without anti-social personality disorders to answer *"for whom drug courts work best."*

- A series of analyses identify the policies, practices, and offender perceptions that appear to play significant roles in providing answers to *"how drug courts work."*

Lastly, the **cost-benefit analysis** evaluated the costs and benefits associated with drug courts. Costs and benefits were measured in a single variable—net harm to society—as the difference in use of resources between drug court and comparison sites and the difference in key outcomes (e.g., official recidivism, wages, etc.) between the two groups. These differences were translated into monetary values so that overall social benefits can be directly compared.

The MADCE research used a variety of data sources, including:

- *Field Visits.* We conducted site visits to all 29 drug court and comparison locations to document program characteristics and operations. In particular, these visits included interviews with key stakeholders and structured observations of courtroom proceedings. These data were used to provide a comprehensive account of the operations and contextual characteristics of each site. Guided by the MADCE conceptual framework in Figure 4-10.1, key site-level measures include treatment availability, treatment immediacy (time to placement), legal incentives to succeed in the program, intensity and quality of judicial interaction, rewards and sanctions, case management, drug testing, and nature and consistency of policies.

- *Self-Report Surveys.* A sample of 1,784 offenders (1,157 drug court participants and 627 comparison group members) were interviewed at three intervals: (1) baseline when participants enrolled in either drug courts or the alternative conditions within comparison sites, (2) 6 months after the baseline interview, and (3) 18 months after the baseline.[51] Subsequently, The interviews lasted between 1.5 and 2 hours and covered a variety of topics, including: background characteristics (demographics, drug use and treatment history, criminal history, physical health, mental health, employment, housing, family conflict and support, and close ties to drug users and those involved in the criminal justice system), respondent perceptions (perceived legal pressure, motivations, understanding of rules, perceived risk of sanctions and rewards, perceptions of court and judicial fairness), in-program behavior (participation in treatment or other services, supervision intensity, case management, drug testing, noncompliance with regulations, sanctions and rewards receipt, terminations, graduations), and interim and final outcomes with respect to substance use, criminal activities, and other psychosocial functioning.

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[51] Surveys were completed with high response rates: 86 percent of baseline respondents completed 6-month interviews, and 83 percent completed 18-month interviews.

- *Oral Fluids Test.* A Buccal swab oral fluids drug test was administered during the 18-month interview for respondents who were not incarcerated or in residential treatment at that time.

- *Administrative Records.* Official criminal history and recidivism data were obtained from state administrative data sources and the National Crime Information Center (NCIC) of the Federal Bureau of Investigation (FBI) at 24 months after study enrollment, allowing for an analysis of recidivism two years after entry into drug court or the comparison circumstances.

- *Cost and Benefit.* For each program activity, the offender interviews and official recidivism records were used to estimate the amount of each activity used and outcome generated by each individual. These quantities were multiplied by the price of each service, activity, outcome, which was developed from a combination of stakeholder interviews, review of official budget and other administrative records, and national estimates of prices.

Overview of the Outcome, Impact, and Cost-Benefit Findings

A summary of key MADCE findings includes:

- *Drug court participants are significantly less likely to relapse, and those who do use drugs less after participating in the program.* Compared to individuals in the comparison group, fewer drug court participants reported using drugs—and fewer had positive oral swab tests for drug use—at 18 months post-enrollment. Among those who were using, drug court participants used drugs less frequently than the comparison group in general, and used serious drugs less frequently as well. Based on results from the oral swab drug tests, drug court prevented substance use equally across subgroups of participants when examining demographics, drug use history, criminality, mental health, and community ties. Based on self-reported drug use, only two subgroups seemed to benefit less from drug court, and that was for those with particular mental health issues (narcissism and depression, but not antisocial personality disorder).

- *Drug court participants commit less crime after participating in the program.* We found that at both 6- and 18-months after enrolling in the study, drug court participants committed significantly less crime than comparison group members. At the 18-month survey, drug court participation reduced the likelihood of reoffending relative to the comparison group, as well as reduced the total number of criminal acts committed. Drug court participation also reduced drug-related crime, including drug possession, sales offenses, and driving while intoxicated, as well as property-related crime. Although drug courts impacted crime behavior similarly across most participant subgroups, the effect of drug court participation was even greater for offenders with violent histories. Alternatively, drug courts were less likely to impact the criminal behavior of narcissistic offenders.

- *Drug court participants experience benefits in other areas of their lives besides drug use and criminal behavior during and after participating in the program.* More drug court offenders were enrolled in school at six months compared to the comparison offenders. In addition, at 18 months, fewer drug court than comparison offenders reported a need for employment services, educational services, and financial assistance, suggesting that such needs were more likely to have been met among those enrolled in the drug court. Further, drug court offenders reported significantly less family conflict than comparison group offenders.

- *The mechanism by which drug courts reduce substance use and crime is through participants' attitudes toward the judge.* When participants have more positive attitudes toward the judge, they have better outcomes. This is true across all subgroups of participants when examining demographics, drug use history, criminality, and mental health.

- *Based on court-level policies of how drug court practices are implemented, the practices related to court effectiveness are: leverage; predictability of sanctions; the point of entry into the program during the criminal justice process; and positive judicial attributes.* More specifically, comparisons of the 23 participating drug courts showed that the programs preventing higher numbers of criminal acts per month evidenced high leverage, medium predictability of sanctions, client populations that entered at the same point in the criminal justice process—either all pre-plea or all post-plea—and medium or high scores on positive judicial attributes. The drug courts that prevented more days of drug use per month had medium predictability of sanctions, client populations that entered at pre-plea, and high scores on positive judicial attributes.

- *Based on individual dosage of drug court program activities, practices related to reductions in crime and substance use are: judicial status hearings, judicial praise, case management, substance abuse treatment, and leverage.* Among the court policies and practices examined at individual dosage levels after the sample was balanced based on the propensity of receiving such services, drug court clients who received higher levels of judicial praise, judicial supervision, drug testing, and case management reported fewer crimes and fewer days of drug use. In addition, drug court clients who participated in more than 35 days of drug treatment had fewer crimes at 18 months, and fewer days of drug use at both 6 and 18 months. Regarding leverage, individuals who rated the alternative sentence for drug court failure as very severe were more likely to have reduced days of drug use at 18 months.

- *Drug courts return a net benefit of $5,680 to $6,208 per participant, or $2 for every $1 of cost, but this finding is not statistically significant.* The finding is driven by a reduction in the most serious offending by relatively few individuals, not by a widespread reduction of serious offending. Drug courts prevent a great deal of crime, but the majority of crimes have small costs to society.

Implications for Practice

Criminal justice programs, generally speaking, are being pushed to implement evidence-based practices to optimize the chances of reducing recidivism and increasing public safety. MADCE findings provide such evidence for drug courts. Summarizing across all the findings, we can identify several implications for practice, allowing us to recommend approaches drug courts should implement to improve drug court operations and outcomes.

The Role of the Judge

The role of the judge in the drug court process is important. Judges exert considerable influence and authority over offenders; and, when used in a strategic manner, such influence can elicit positive change. The courtroom setting in drug court programs creates an opportunity to use judicial authority in a constructive way. The setting allows judges to engage with participants in ways that are meaningful in terms of interpersonal interaction, while the repeated status hearings afforded by the drug court process allow for judges to establish ongoing relationships with offenders that can be caring and supportive, as well as disciplinary. In three different sets of MADCE analyses, we identified the judge as central to the process of reducing crime and substance use. Specifically:

1. Drug court participants who reported more positive attitudes toward the judge had greater reductions in drug use and crime. To understand participants' attitudes toward the judge, we asked them during the survey to rate the extent to which the judge: was knowledgeable about their case, knew them by name, helped them to succeed, emphasized the importance of drug and alcohol treatment, was not intimidating or unapproachable, remembered their situations and needs from hearing to hearing, gave them a chance to tell their side of your story, could be trusted to treat them fairly, and treated them with respect.

2. Drug courts that had judges with medium or high scores on positive judicial attributes prevented greater numbers of crimes among their participants, and courts that had judges with high scores on positive judicial attributes prevented greater numbers of days of drug use. To understand positive judicial attributes, members of the research team observed drug court sessions and systematically rated each judge on the extent to which s/he was respectful, fair, attentive, enthusiastic, consistent/predictable, caring, and knowledgeable.

3. Drug court participants who received judicial praise more often and who had higher numbers of judicial status hearings reported committing fewer crimes and using drugs on fewer days.

There are several ways these findings lend themselves to implications for future drug court practice. We recommend the following:

- *Hold frequent judicial status hearings*. Frequent status hearings increase participants' contact with judges, which this research has shown to be critically important.

Additionally, in light of previous research on this topic, consider increasing the frequency of status hearings for "high risk" participants in particular.

- *If the jurisdiction allows it, choose drug court judges carefully.* Not all individual judges are suited to the drug court model in terms of disposition and attitudes toward offenders and the judicial relationship. Thus, drug courts will be best served if administrators intentionally assign judges to the drug court docket who are committed to the problem-solving court model and interested in serving in this role. Assigning judges who fundamentally do not believe in engaging offenders in an interpersonal relationship or who do not support the concept of therapeutic jurisprudence virtually ensures a lack of success for the drug court.

- *Give them time.* Judges may take some time developing effective approaches to the drug court bench, and therefore, a reasonable period of program leadership may be needed before their style affects change in offender behaviors. For this reason, routinely rotating judges on and off drug court benches will likely decrease not only judges' ability to successfully implement their roles, but also the overall success of drug court programs in jurisdictions that circulate judicial assignments to drug court.

- *Monitor "client satisfaction."* Drug courts may want to begin monitoring how well their judge is doing in terms of participant perceptions. Courts could periodically conduct brief participant surveys assessing clients' attitudes about the judge. The nine items from the attitudes toward judge scale we used for MADCE (identified above) could easily be used or adapted so that participants could rate particular judges. If judges elicit negative attitudes from participants, corrective action could be taken by improving the judges' responsiveness to clients through training or, if needed, by making data-driven management decisions to select different judges for the drug court assignment.

- *Train judges on best practices regarding judicial behavior.* Judges do not necessarily innately have the traits that elicit the most positive outcomes from participants. As a result, drug court training programs should be developed to specifically address best practices around judicial behavior, and drug courts programs should send judges to such trainings.

Drug Court Eligibility Requirements

A pressing question for the drug court field has been "*for whom drug courts work*." A critically important finding emerging from the MADCE study is that *drug courts work equally well in reducing crime and drug use for nearly all client subgroup populations*, and the mechanism through which these reductions result—positive attitudes towards the judge—is the same across subgroups, even when accounting for client demographics, drug use and criminal histories, and mental health.[52] One positive exception to this is that for offenders with violent criminal

[52] Exceptions to this are that (1) when using the self-reported drug use outcome, individuals with depression or narcissism had less positive impact on reductions in drug use, and (2) when examining self-reported crime,

histories, drug court had a greater positive impact on reducing crime. Thus, practical implications of these findings are:

- *Consider broadening drug court eligibility requirements.* Since drug courts seem to work equally well across most subgroups of client populations, it stands to reason that courts with narrow eligibility requirements are preventing particular groups of offenders from receiving the opportunity to be part of a program that has real potential to benefit them. As such, courts may want to design eligibility requirements that do not restrict individuals based on drugs of choice, criminal histories, or particular mental health issues.

- *Consider including violent offenders with substance use issues.* Many drug courts intentionally do not allow substance-using offenders with current charges that include violence or with violent criminal histories into their drug courts. However, the MADCE findings indicate that such individuals are helped equally well as others in terms of reducing drug use, and are helped even more so than others in terms of reducing criminal behaviors. Courts may want to consider allowing individuals with violent criminal histories or current charges with particular violence characteristics to be included.

- *Consider the larger substance-using offender population.* Finally, our findings suggest that there is no evidence that "creaming"—including just those offenders in the program that one assumes will do the best in the program—will improve a drug court's outcomes. Thus, individual staff hunches about who might succeed in drug court are likely not to be accurate.

Case Processing

Clients enter drug courts at many points in the criminal justice process including before entering a plea—a diversion strategy, after entering a plea with drug court functioning in lieu of a sentence, and as part of a reentry program. Some drug courts allow clients to enter the program at more than one point in the process. MADCE found that drug courts that have mixed populations of clients based on when in the process they entered into the program, were less effective at preventing crime than courts with client that are either all pre-plea or all post-plea. Courts with mixed populations are also less effective at preventing substance use than diversion program courts. Perhaps courts that have a singular focus of client population are better able to tailor their practices to meet the needs of either pre-adjudication or post-adjudication populations. When a mix of clients is in the program, courts may be less organized in their approach, or may be uniformly implementing practices across the board when such practices might not be fully appropriate for some portion of their clientele. A practical implication of this finding is:

- *Only include clients in the drug court program who enter during one point in the criminal justice process.* Courts that allow clients to come in during multiple points in the

individuals with narcissism had less positive impact on reductions in crime. However, when using the oral swab drug test for use, individuals with these mental health issues did equally well as other drug court participants.

criminal justice process should consider becoming courts that are either entirely diversion programs or entirely post-adjudication programs.

Sanctions Policies and Practices

Graduated sanctions and incentives are a key component of the drug court model, as well as other offender behavior interventions. The MADCE focuses particularly on sanctions policies, and the predictability that sanctions will be imposed given an infraction. This predictability, or the certainty an offender has that s/he will be sanctioned in a particular way for a particular infraction, is a cornerstone in what has been considered best practices around sanctioning. Yet, MADCE courts that prevent higher numbers of criminal acts per month and more days of drug use per month had medium predictability of sanctions. What does medium predictability of sanctions mean? It appears that these courts have a coordinated sanctioning strategy, yet they exercise some flexibility in its implementation in a way that seems to matter to clients. Perhaps clients perceive flexibility in implementation of sanctions as more fair than those courts that strictly follow a schedule that does not take into account particular individuals or circumstances. While it seems clients clearly need to know that sanctions are a consequence of noncompliance in the program, sanctions that are rigidly set or perceived as unfair may actually contribute to client frustration or lack of interest in complying with program requirements. Based on these findings, one might suggest:

- *Have a written schedule of graduated sanctions (and incentives) and share it with clients, yet maintain some flexibility when following it.* Courts should have a logical set of sanctions in a written schedule that increases in severity with repeated noncompliance. Clients should be given a copy of the schedule so that they expect sanctions for noncompliance and expect more severe (graduated) sanctions with repeated infractions. On occasion, however, the drug court team may deviate from the schedule to account for individual circumstances or characteristics that favor a different reaction from the court.

Leverage

The MADCE findings show that the leverage courts have over clients makes a difference in terms of outcomes. With respect to court policies, drug courts with high leverage prevent more crime than other courts. Such courts have regular monitoring contact with clients through drug court case managers and judicial hearings, and have explicit consequences for failure in the program that clients know about and agree to in signing the drug court contract. These practices might focus a client's attention on the fact that the alternative to drug court is not desirable and that they are being monitored closely, making the consequence of noncompliance and the alternative for failure very real. Indeed, we found that clients who perceived the sentence for drug court failure as very severe had greater reductions in drug use. As a result, a practical recommendation would be to:

- *Increase participants' perceptions of the court's leverage.* Courts can accomplish this by providing case management using drug court personnel, rather than treatment providers or other social service actors; providing regular judicial status hearings; deciding explicitly about what the consequences of failing drug court would be at the beginning of program participation and informing clients of these explicit consequences; and asking clients to sign contracts acknowledging the alternative sentence to ensure they understand the consequences of drug court failure.

Case Management

Based on MADCE findings for individual program dosage, drug court clients who received more case management reported fewer crimes and fewer days of drug use. Clients who had more than ten contacts with a case manager had better outcomes, after controlling for an individual's propensity to be given case management. MADCE findings on court policies indicated a similar finding, although the findings were only marginally significant. That is, courts with case managers who met with clients more than once per week prevented more criminal acts per month than those who met with clients less than one time per week or not at all. Courts with case managers who met with clients more than one time per week or met with clients less than one time per week or not at all prevented more substance use than courts that met with clients one time per week. While the substance use finding seems contrary to the crime finding, it is notable to inform the reader that courts that have infrequent case management meetings tend to rely on treatment providers to do more of this type of work, with perhaps the client seeing the treatment provider more times per week in that setting than in other programs. This might explain why the courts with both high and low frequency of case management meetings prevent about the same numbers of days of drug use compared to courts with a medium amount of case management contacts. Given this, a practical recommendation for court's to consider is:

- *Provide case management as frequently as possible, perhaps more than one contact with a case manager per week.* Whether or not the primary case manager is a drug court staff person or a treatment provider, if they are able to have contact with clients more frequently than one time per week, clients will likely have better outcomes. Given the myriad challenges that substance-using offenders may face, weekly contact may be necessary, but not sufficient to help them tackle such issues. Also, frequent case management may increase clients' favorable perceptions regarding the leverage the court has over the program participants.

Drug Testing

Based on NIJ's MADCE findings for individual program dosage, drug court clients who received more drug tests reported fewer crimes and fewer days of drug use. Clients who had more than 17 drug tests before the 6-month interview and more than 9 drug tests before the 18-month interview had better outcomes, after controlling for an individual's propensity to be given drug tests. Regular monitoring participant compliance with program regulations related to drug use is critical to the drug court model. As we know from our process evaluation, most courts in our sample tested clients more than once per week during the initial phase of the program. Given the findings related to individual receipt of drug tests, a logical implication for courts to practice is to:

- *Provide drug tests frequently, certainly more than one time per week during the initial phase of the program.* Drug tests not only assist program staff in monitoring program compliance, but also communicate to clients that they are being watched, perhaps increasing perceptions of leverage that the court has over clients.

Treatment

Providing substance abuse treatment is integral to the drug court model. Based on MADCE findings for individual program dosage, drug court clients who received between 35 and 65 days of treatment had fewer crimes at 18 months and fewer days of drug use at 6 and 18 months. However, participating in extremely high amounts of treatment (more than 65 days) did not show any added benefit beyond the benefits achieved from 35 to 65 days of treatment. Notably, the measure used here is literally a count of the number of days a person was treated. For example, a person with 30 days of treatment might be in that treatment one month (representing consecutive days in residential treatment), might be in that treatment for ten weeks (representing outpatient counseling that meets three days a week), or might be in that treatment 30 weeks (representing outpatient counseling that meets one day per week). Whatever the scenario, the practical implication is to:

- *Provide a sufficient amount of substance abuse treatment to participants.* Courts that offer treatments of short duration may not allow clients enough time to tackle their substance use problems and alter their attitudes and behaviors accordingly. Funding should be sufficient such that treatment is available to clients long enough to achieve success.

Implications for Policy

Policymakers have consistently funded drug court programs across the country for two decades, and the number of programs has grown exponentially throughout that time period. What do the MADCE findings mean to policymakers? The findings lead us to three specific implications for policy:

- *Drug courts work, so ensure that provisions are made to fund their existence.* A large literature exists documenting the effectiveness of drug courts, but many of these studies were single jurisdictions or only included a small group of jurisdictions. NIJ funded the MADCE team to put to rest the question of drug court effectiveness. Given the large-scale nature of MADCE, the sophistication of the analytic approaches, and the exceptional survey retention rates that allowed us to look at a large group of offenders over time, we can say with confidence that drug courts successfully prevent crime and drug use. Thus, government agencies that spend resources funding drug court programs and providing training and technical assistance should continue to do so, and should encourage the implementation of evidence-based practices.

- *In keeping with the emphasis on evidence-based practices, develop standards to guide drug court practice.* Given the large set of implications for practice that emanate from the MADCE findings, along with previous literature on the importance of particular practices and how these should be implemented, it is perhaps time to consider developing standards of practice for drug courts. The field has matured and amassed enough evidence-based information about what works best to achieve reductions in crime and drug use among substance-using offenders in these programs. Our study shows that the most effective courts tend to implement practices in particular ways. Learn from this and help the rest of the field achieve the same success. Once standards of practice are developed, there is the potential for developing an accreditation program.

- *Encourage programs to include more serious offenders in programs to achieve greater returns on drug court investments.* Given the MADCE findings that show drug court programs work equally well for clients with varying criminal histories and may even work better in terms of preventing crime for those with violent offense histories, federal funders and more local policymakers may want to encourage drug courts to expand eligibility to include more serious offenders. The most efficient use of resources is to keep people with the potential for committing the more serious crimes from doing so. Drug courts can help these more serious offenders, and funders will see a greater net benefit from their funding.

Implications for Research

NIJ's MADCE has contributed greatly to the knowledge of how drug courts work and for whom. But, some questions remain that future evaluations might consider answering:

- *Given the importance of participant attitudes toward the judge, do participant attitudes toward other drug court partners matter?* Although beyond of the scope of the current study, it may be useful to examine the role of other drug court actors and whether clients' perceptions of them also influence reductions in drug use and crime. Specifically, researchers could examine attitudes toward the primary case manager (either drug court staff or otherwise), the treatment provider staff, supervision officers, prosecutors, and defense attorneys.

- *Given that this study did not find differences in outcomes for courts' adherence to treatment best practices, does the way in which these practices are implemented matter?* The MADCE research team believes that treatment quality likely matters in the drug court model. However, the group of courts included here were quite consistent in how they reported that they implemented treatment leading us to lack sufficient variation in practice to accurately test for differences. Most courts reported adhering to treatment best practices, but we used a crude measure of adherence to only a small set of such practices. Future evaluations may want to assess courts for site selection based on the National Institute of Drug Abuse's full set of treatment best practices and then purposefully sample courts that vary in adherence to the entire set of practices. By doing so, more variation in adherence to such practices might be observed, allowing for a more refined examination of how this affects a drug court's ability to prevent substance use and crime.

- *Do participants' attitudes toward the judge influence the amount of drug court dosage they receive?* In the MADCE study, we examined how receipt of drug court practices influenced attitudes, yet we do not know if the reverse might indeed be true. Thus, future studies should examine path models similar to those presented here and consider changing the order of the modeling such that attitudes precede program practice dosage.

- *Given MADCE findings show that drug courts work for numerous subgroups of client populations, does it matter if several groups are represented in one program or not?* In other words, does program effectiveness change in courts that serve a heterogeneous population of clients compared to a homogeneous population? Our analysis showed that courts that mix clients who come into the program at different points in the criminal justice system are less effective than those that have clients enter at one point in the process. Is a similar pattern possible when one considers client characteristics?

- *Does drug court effectiveness change based on having general versus specific deterrence practices?* Specific deterrence matters because clients who perceive the alternative criminal response for drug court failure as severe are more likely to succeed. However, it is not clear if more general deterrence practices matter. For example, ordering cases on the drug court docket such that clients have to observe other clients receiving sanctions may deter some from committing infractions. More needs to be known about how the courtroom process can play a role in affecting drug court outcomes.

Conclusions

The MADCE has shown that drug courts prevent crime and substance use, work equally well for most subgroups of client populations, are effective through the role of the judge, and can increase effectiveness if they implement program practices in particular ways. The implications identified here represent our perspective on how the findings might affect implementation of program practices, policies and funding for drug court programs, and future evaluations.

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Appendix A. Analytic Strategy for Producing Unbiased Estimates of Drug Court Impact

Michael Rempel, Center for Court Innovation, With Donald J. Farole, Jr.

This technical appendix essentially follows the same outline as in Chapter 2, but provides additional details for those readers who are interested in a more comprehensive description of the impact methodology, how that methodology was implemented, and what kinds of sensitivity analyses were conducted to ensure that the primary methodology was appropriate.

Adjusting for Selection Bias: Propensity Score Modeling

Comparison of Drug Court and Comparison Offender Characteristics

The first step was to determine whether the 1,156 drug court offenders differed at baseline from the 625 comparison offenders. If the samples did not differ, further adjustments would be unnecessary. However, considering that the two samples were each from different sites and were unequally distributed across eight states, it would have been remarkable if no differences arose.

We selected 61 characteristics from the baseline offender survey, and measured bivariate sample differences using simple t-tests (or chi square tests for categorical variables). The 61 characteristics were as follows (see footnotes under Table 4-A.1 for clarification of how certain measures were constructed):

- *Demographics*: age, sex, race/ethnicity (white, black, Hispanic, other), high school degree or G.E.D., and base 10 logarithm of income (to correct the extremely skewed income distribution).

- *Social ties*: married, care for children less than 18 years, homeless in past 6 months, ever homeless, currently employed or in school, currently employed, currently in school, weeks worked over last 6 months, family or friends involved with criminal justice system or drugs (36-item index), and blood relatives involved with criminal justice system or drugs (10-item index).

- *Drug use history*: As described in previous Volumes (e.g. Chapter 3, Volume 1), we collected voluminous baseline drug use data, but focused on the 26 variables listed below, which we believed were the most likely to influence outcomes and thus comprise a potential source of selection bias:

    - *Years of use:* years of use or years excluding alcohol and marijuana.

    - *Use up to six months pre-baseline:* any use, any use excluding marijuana and light alcohol (defined as less than four drinks per day for women and less than five drinks per day for men), any use excluding marijuana and all alcohol, and any use by drug: heavy alcohol, marijuana, cocaine, heroin, hallucinogens, amphetamines, and illegal prescription use.

- *Days per month of use up to six months pre-baseline:* days per month of any drug, any drug excluding marijuana and light alcohol, any drug excluding marijuana and all alcohol, and any drug by drug: heavy alcohol, marijuana, cocaine, heroin, hallucinogens, amphetamines, and illegal prescription use.

- *Primary drug of choice:* alcohol, marijuana, cocaine, heroin, amphetamines, other or none.

- *Addiction Severity Index:* 18-item index.

- *Treatment history up to six months pre-baseline:* any drug or alcohol treatment and days per month of drug or alcohol treatment.

- *Criminal history*: As described in previous Volumes (e.g., Chapter 3, Volume 1), we collected voluminous self-report data on both official arrests and criminal activity that may or may not have been detected, but focused on the 11 baseline offender survey variables listed below, which yielded substantial variation across our sample and appeared likely to influence outcomes:

    - *Prior arrests and convictions:* any prior arrests, base 10 logarithm of the number of prior arrests (to correct the extremely skewed distribution of this variable), any prior convictions, and any prior violent convictions.

    - *Prior criminal behavior up to six months pre-baseline:* any criminal activity, base 10 logarithm of the number of criminal acts (to correct the extremely skewed distribution of this variable), any drug-related criminal acts, number of drug-related criminal acts, and any driving while intoxicated.

- *Mental and physical health*: overall mental health on five-point scale, depressed (based on multi-item instrument), anti-social personality disorder (based on multi-item instrument), narcissistic personality disorder (based on multi-item instrument), psychotic (both anti-social and narcissistic), victim of any abuse in previous year, victim of any physical abuse in previous year, and any chronic medical problems.

- *Control variables for calendared data* (labeled above as "up to six months pre-baseline"): exact number of pre-baseline months that could be measured in the baseline offender survey; depending on the exact timing of the baseline survey, offenders could be measured over six (17 percent), five (67 percent), four (15 percent), or two (1 offender, <1 percent) months, total days incarcerated during the pre-baseline period, total days incarcerated or in residential treatment during the pre-baseline period.

Table 4-A.1. Comparison of Baseline Sample Differences

Characteristics	Drug Court N= 1,156	Comparison Group N= 625
Demographics		
Age	32.97***	35.06
Male	68%+	72%
Race/Ethnicity	***	
White	57%	50%
Black/African-American	29%	41%
Hispanic / Latino	7%	5%
Other (incl. multiracial)	7%	5%
HS degree/GED or higher	61%*	55%
Base 10 Logarithm of Annual Income	4.54***	$4.27
Social Ties		
Married	11%	11%
Primary care responsibility for children less than 18	19%+	16%
Homeless: prior 6 months	11%	13%
Ever homeless	50%	52%
Currently employed or in school	43%+	38%
Currently employed	38%**	32%
Currently in school	8%	9%
Weeks worked: last 6 months	9.07*	7.79
Blood relatives/other family/friends involved with crime or drugs[1]	6.36	6.28
Blood relatives involved with crime or drugs[2]	1.77	1.65
Drug Use		
Years of drug use		
Years of drug use (8 drugs)[3]	19.34***	21.29
Years of drug use (6 drugs, excluding alcohol and marijuana)	13.24**	14.60
Drug use in the 6 months prior to baseline: summary measures:		
Any use (8 drugs)[3]	84%***	76%
Any use (7 drugs)[4]	74%***	63%
Any use (6 drugs, excluding alcohol and marijuana)	61%***	51%

(continued)

Table 4-A.1. Comparison of Baseline Sample Differences (Cont'd)

Characteristics	Drug Court N= 1,156	Comparison Group N= 625
Days of use per month (8 drugs)[3,5]	13.64**	11.79
Days of use per month (7 drugs)[4,5]	9.79**	8.41
Days of use per month (6 drugs, excluding alcohol and marijuana)[5]	7.90**	6.58
Drug use in the 6 months prior to baseline: by drug:		
Any use of heavy alcohol[6]	41%	38%
Days of use per month of heavy alcohol[6]	3.33	3.18
Any use of marijuana	46%**	38%
Days of use per month of marijuana	6.18***	4.21
Any use of cocaine	44%***	34%
Days of use per month of cocaine	4.17+	3.48
Any use of heroin	11%	13%
Days of use per month of heroin	1.72	2.10
Any use of hallucinogens/designer drugs	9%*	5%
Days of use per month of hallucinogens/designer drugs	.22*	0.08
Any use of amphetamines	15%*	11%
Days of use per month of amphetamines	1.89*	1.18
Any use of prescription drugs: illegal use	16%	13%
Days of use per month of prescription drugs: illegal use	1.05*	0.84
Primary drug of choice[7]	***	
Alcohol	12%	15%
Marijuana/hashish	22%	20%
Cocaine (powder)	11%	9%
Crack cocaine	26%	24%
Heroin	4%	8%
Amphetamines (incl. methamphetamine)	11%	7%
Other or claimed not using drugs	14%	17%
Addiction severity index[8]	9.53***	8.36
Previous treatment in six months prior to baseline		
Any drug/alcohol treatment	35%***	23%
Days of treatment per month	2.53***	1.08
Criminal History		
Any prior arrests	86%***	92%
Base 10 logarithm of # prior arrests	1.62***	1.77
Any prior convictions	68%***	77%

(continued)

Table 4-A.1. Comparison of Baseline Sample Differences (Cont'd)

Characteristics	Drug Court N= 1,156	Comparison Group N= 625
Any prior violent convictions	13%***	20%
Criminal activity in 6 months prior to baseline (not limited to arrests)		
Any criminal activity	75%*	71%
Base 10 logarithm of # criminal acts	1.94**	1.84
Drug activity (incl. possession, sales, and other drug activity)	70%*	65%
Mean number of drug-related criminal acts	13.85**	12.00
Drove while intoxicated	35%	31%
Mental and Physical Health		
Overall mental health (5-point scale from excellent to poor)	3.43	3.41
Percent depressed (multi-item inventory)	39%	37%
Anti-Social personality disorder (multi-item inventory)	43%	42%
Narcissistic personality disorder (multi-item inventory)	49%	50%
Psychotic: anti-social personality PLUS narcissistic personality	28%	26%
Victimized by any abuse in past year[9]	40%+	45%
Victimized by any physical abuse in past year[9]	28%*	35%
Any chronic medical problems (currently)	26%	28%
Control Variables for Calendared Data		
Total days incarcerated pre-baseline	20.86***	39.38
Total days incarcerated or in residential treatment pre-baseline	25.59***	42.18
Exact number of pre-baseline months measured in baseline survey	4.99**	5.08

Note: Select variables were missing for small numbers of cases: age (1), years used drugs (4), years used drugs other than marijuana or alcohol (1), days of drug use of heavy alcohol, alcohol, cocaine, heroin, hallucinogens, amphetamines, and prescriptions drugs (1 for each of those variables), any physical health problems (9), any prior arrest (16), and number of prior arrests (49). In addition, 685 cases were missing on the two measures of prior abuse, any abuse, and any physical abuse, as a result of the addition of these measures after survey implementation was already underway.

[1] This score is the summation of 36 dichotomous measures of family/friends involvement in the criminal justice system and/or drugs/alcohol use/treatment. The score ranges from 0-36. The Cronbach's alpha is 0.861.

[2] This score is the summation of 10 dichotomous measures of blood relatives' involvement in drugs/alcohol use/treatment. The score ranges from 0-10. The Cronbach's alpha is 0.688.

[3] The eight drugs are marijuana, alcohol, cocaine, heroin, amphetamines, hallucinogens, prescription drugs (illegal use), and methadone (illegal use).

[4] The seven drugs are marijuana, heavy use of alcohol, cocaine, heroin, amphetamines, hallucinogens, prescription drugs (illegal use), and methadone (illegal use). Heavy use of alcohol is defined as four or more drinks per day for women, and five or more drinks per day for men.

Table 4-A.1. Comparison of Baseline Sample Differences (Cont'd)

[5] Respondents were asked the number of days of use per drug per month for six months before the baseline interview; possible answers were every day; a few days per week, but not daily; a few days per month; and once per month. These variables were recoded so that everyone who said they did not use drugs were recoded to zero days of use in all months and the categorical answers were recoded to a specific number of days and extrapolated for the entire month ("every day" = 30; "a few days per week, but not daily' = 8.58; "a few days per month" = 3; "once per month" = 1). After determining the monthly average for each drug for each month, average use was taken across drugs and across months to determine monthly average use measures.

[6] Heavy use of alcohol is defined as four or more drinks per day for women, and five or more drinks per day for men.

[7] Primary drug of choice was not initially asked at the baseline survey. However, we constructed a proxy measure, based on responses to the series of questions regarding drug use during the previous six months. The proxy measure defined the primary drug as the one used during the most months of the previous six. In the event of ties involving marijuana or alcohol plus one other drug, we defined the primary drug as the other drug. If marijuana and alcohol were in a tie with each other, we defined the primary drug as marijuana. If two other drugs were in a tie, we defined the primary drug as "multi-drug." If the primary drug was cocaine, we assigned to crack or powder cocaine based on the most frequent method during the last six months.

[8] The Addiction Severity Index is a summary scale consisting of the answers to 20 questions. Two of the questions, however, are only asked if the respondent has been employed at some point in the prior six months. Since a majority of these samples are unemployed, all respondents were scored out of a possible 18. The Cronbach's alpha is 0.744.

[9] The questions related to prior abuse were, in fact, omitted from the baseline survey and then asked at the six-month follow up. Although for analytic purposes, we are treating these measures as indicative of baseline, the responses are not truly for a baseline period of time.

*+p<.10, *p<.05, **p<.01, ***p<.001.*

As shown in Table 4-A.1, there were significant differences on 37 of 61 tests (at least at $p < .05$), indicating a severe selection bias problem. The substantive findings indicated that drug court offenders were significantly younger, whiter, higher in socioeconomic status, more drug involved (on most, but not all of the drug use measures), and less criminally involved (on most, but not all criminal history measures) than the comparison group. Some of these differences suggested that the drug court population is at a higher risk of negative outcomes (younger and more drug involved), whereas other differences suggested that the drug court population is at a lower risk (higher SES and less criminally involved). Although it is possible that these biases would cancel each other out, the sheer number and magnitude of the baseline differences made extremely risky any strategy that would shun explicit steps toward statistical adjustment.

Implementation of Propensity Score Modeling

We next implemented a series of standard propensity score modeling procedures (see Luellen, Shadish, and Clark 2005; Rosenbaum and Rubin 1983, 1984; Rubin 1973). In brief, a propensity score is a number from 0 to 1 that can be assigned to each offender, reflecting the predicted probability that the offender falls into one as opposed to another of two samples—in this case, the drug court as opposed to the comparison sample. The propensity score can derive from a large number of baseline characteristics, and represents their summary effect in leading some cases to be statistically more likely than are others to be in one of two samples.

In executing a propensity model, the procedure is to run a logistic regression. The dependent variable is sample status (0 = comparison group, 1 = drug court). The independent variables are baseline characteristics that possibly might be associated with differential sample membership. The question of whether or not to include a given baseline characteristic in a propensity model is not one of formal statistical significance. Rather, a liberal inclusion criterion that involves including many more variables than those on which the bivariate differences reach a standard statistical significance threshold maximizes the balancing effect of the resulting propensity scores (e.g., see Rosenbaum 2002; Rubin and Thomas 1996).

We decided to include all baseline characteristics whose bivariate comparison revealed a p-value of .50 or less.[53] Overall, we included 47 of the 61 variables whose bivariate differences were examined. We ran backward stepwise regression, with a liberal variable deletion criterion of $p > .50$— i.e., deleting only those variables with patently no effect on sample membership. For cases that were missing data in the initial propensity model, propensity scores were computed based on more limited models that eliminated small numbers of variables that had one or more missing cases (per Rosenbaum and Rubin 1984). In practice, missing data on select variables necessitated computing four total propensity models. The first generated propensity scores for the majority (61 percent) of the sampled cases. The second model omitted a single variable for which there was extensive missing data (any physical abuse) and generated scores for 37 percent of the cases. The third model omitted four additional variables, and generated scores for 28 total cases (1.6 percent of the sample). The fourth model omitted two more variables, and generated scores for 11 total cases (0.6 percent of the sample). As these percentages indicate, missing data did not pose a serious problem; almost 98 percent of all propensity scores were based on all 47 selected baseline characteristics or on 46 of the 47 (after deleting just one variable in the second model).

We then examined the resulting propensity scores to determine whether our model was effective. Effectiveness is defined pragmatically based on whether adjusting for propensity scores appears to eliminate the previously observed significant differences between the samples. The typical examination method is to divide the full sample into quintiles (i.e., five strata) based on five equal ranges of propensity scores, and then to test for sample differences separately within each quintile. Accordingly, for each of the 61 key baseline characteristics, we examined whether there were significant differences between the samples both within each propensity score quintile, as well as overall after controlling for quintile membership. To do this, we ran two-way ANOVAs on each baseline characteristic, in which the independent variables were sample (drug court or comparison), propensity score quintile, and the interaction term for sample*quintile. Of the 61 tests, we found significant differences on two main effects (sample) and two interaction effects (sample*quintile). In general, the performance of a propensity model is acceptable if less than 5 percent of all main effects and of all interaction effects are significant. Therefore, our results at this stage were acceptable; nonetheless, we attempted to improve upon them.

We re-ran the four propensity models, changing the stepwise deletion criteria from $p > .50$ to $p > .60$ and adding an interaction term involving two of the variables with persistent significant effects (any criminal activity in the six months pre-baseline * any prior violent conviction). Table 4-A.2 illustrates our results by providing simple regression coefficients for the first of the four final models—the model that included all 47 baseline characteristics without omissions to

[53] Due to extraordinarily high inter-correlations, we only included any physical abuse, but not any abuse; only included years of drug use for drugs other than alcohol and marijuana, but not years of drug use for any drug; and omitted a few of the many drug use and criminal history measures that met the $p < .60$ threshold.

address missing data. We re-examined the effectiveness of this second set of propensity models by re-dividing the sample into five quintiles, as described above. This time we detected zero significant main effects (0 of 61, 0 percent), and two significant interaction effects (2 of 61 = 3 percent). We considered this state of affairs to be more than adequate, and the model effective.

Having established a working set of baseline characteristics for the propensity model and a working stepwise exclusion criterion ($p > .60$), we re-ran our models and generated separate propensity scores for those (1) retained for the 6-month survey, (2) retained for the 18-month survey, (3) retained for *both* follow-up surveys, and (4) retained for the oral fluids drug test.[54]

Omission of Official Criminal History Controls

Throughout the aforementioned modeling procedures and those described in the next section, we did not include any baseline measures of *official* (not self-reported) criminal history. As described below, we were concerned that the official criminal justice records that we received may not have been perfectly comparable across each of the eight states from which our offenders were drawn. Different states may not have used the same criteria in determining whether to share data with us on specific criminal justice events, especially with regard to less serious events such as misdemeanors or ordinance violations or events that are subject to state-specific sealing procedures. Accordingly, we did not want our effort to control for criminal history to risk the possible inadvertent and counterproductive consequence of introducing a state-specific bias into our calculations. This concern led us to decide that whenever analyzing outcomes that were drawn from the offender survey, we would adjust only for baseline differences that were likewise drawn from the survey. On the other hand, for analyses of official records outcomes, controlling for official records criminal history made sense, and those special steps are described below.

[54] In each of our multiple propensity models, small numbers of cases fell outside of the region of common support (i.e., comparison offenders with a lower propensity score than any drug court offender or drug court offenders with a higher score than any comparison offender). Virtually all such cases had a propensity score that was within a small fraction (0.02) of the common support region. Although in propensity score analysis, researchers will occasionally delete cases that fall outside the region of common support, we opted not to do so. First, our super-weighting strategy ultimately took into account selection, as well as attrition bias; hence, eliminating cases due to their location on the propensity score continuum (i.e., selection) might inadvertently eliminate cases from under-represented categories with respect to attrition. Second, our final weighting scheme (see details below) censored extreme weights (no lower than 0.25 and no higher than 4.00), meaning that we did ultimately take steps to reduce the impact on the analysis of any cases with extreme locations on the *combined* selection/attrition spectrum. Third, given that we had multiple propensity score schemes (e.g., for cases retained at 6 months, 18 months, or both time periods respectively), our judgment was that it would be needlessly confusing and arbitrary to eliminate different sets of 5-15 cases for different highly specific analyses. Finally, as noted above, the actual number of cases that fell outside the region of common support was small. For instance, when taking the propensity model conducted on the full sample of 1,781 cases, the lowest propensity score for a drug court offender was 0.11; only 11 comparison cases had a lower score, and 7 of those 11 cases had a score of 0.08, 0.09, or 0.10 (reflecting a miniscule difference). On the same propensity model, the highest propensity score for a comparison offender was .96, nearly at the top of the spectrum (whose maximum value is, of course, 1.00).

Table 4-A.2. Logistic Regression of Baseline Characteristics on Sample Status

Dependent Variable	**Sample (Drug Court vs. Comparison)**
Number of Cases	**N=1,062**
Number of steps	12
Chi-square for final model	215.423***
Nagelkerke R^2 for final model	0.250
Independent Variables:	**Regression Coefficient**
Age	-0.010
Male sex	-0.170
Race/Ethnicity	
White	-0.332
Black/African-American	-0.348
HS degree/GED or higher	0.217
Base 10 Logarithm of Annual Income	0.183*
Primary care responsibility for children less than 18	0.239
Homeless: prior 6 months	-0.250
Currently employed	-0.117
Currently in school	-0.404
Years of drug use (6 drugs, excluding alcohol and marijuana)	-0.018
Drug use in 6 months prior to baseline	
Any drug use (6 drugs, excluding alcohol and marijuana)	-0.189
Days of drug use per month (8 drugs)	0.006
Any use of heavy alcohol	-0.398*
Any use of marijuana	-0.143
Any use of cocaine	0.668*
Any use of heroin	-0.570*
Any use of hallucinogens/designer drugs	0.321
Any use of amphetamines	-0.373
Primary drug of choice	
Cocaine (powder)	0.135
Heroin	-0.787*
Amphetamines (incl. methamphetamine)	1.24**
Addiction severity index	0.150***
Previous treatment in six months prior to baseline	
Any drug/alcohol treatment	0.664**
Days of treatment per month	0.046*
Any prior convictions	-0.358*
Any prior violent convictions	-1.072*
Criminal activity in 6 months prior to baseline	
Base 10 logarithm of # criminal acts	0.154
Any criminal activity * any prior violent conviction	0.330
Any drug-related criminal activity	-0.299
Any driving while intoxicated	-0.444*
Classified with depression (multi-item inventory)	-0.155
Psychotic: anti-social personality PLUS narcissistic personality	0.153
Victimized by any physical abuse in past year	-0.536**
Any chronic medical problems (currently)	-0.143
Total days incarcerated or in residential treatment pre-baseline	-0.007***

(continued)

Table 4-A.2. Logistic Regression of Baseline Characteristics on Sample Status (Cont'd)

Independent Variables:	Regression Coefficient
Exact number of pre-baseline months measured in baseline survey	-0.305*

Note: The following variables were deleted during the stepwise procedure: Hispanic race/ethnicity, worked in past 6 months, score for involvement of blood relatives with drugs or alcohol, any illegal use of prescription drugs in 6 months prior to baseline, primary drugs of alcohol, marijuana, and cocaine, any prior arrest, base 10 logarithm of the number of prior arrests, mental health (self-rated on 5-point scale), and days incarcerated in past 6 months. See note and footnotes underneath Table 4-A.1 for additional information on the variables in this table.
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Adjusting for Attrition Bias: Retention Score Modeling

Overall, we considered the possibility of attrition bias to pose a far lesser threat to study validity than the possibility of selection bias. For one, our attrition rates were remarkably low, for 86 percent of the sample was successfully surveyed at 6 months, 83 percent at 18 months, and 76 percent at both follow-up periods. Furthermore, even if the relatively small percentage of attrited cases differed from those that were retained at follow up, it was not apparent why this would create a substantively important bias. Ordinarily, attrition bias undermines the general ("external") validity of study findings, given the standard assumption that one's initial baseline sample is more typical of the population of interest than the smaller sub-sample that is reached at follow up. Yet, this assumption did not clearly apply here. On the one hand, we selected a range of sites whose offenders we believed would be far more representative of the national drug court-eligible population than in any previous evaluation. On the other hand, we did not draw a random national sample. Although we had hoped to focus on geographic clusters and sites with a representative mix of offenders, we cannot stipulate with any certainty that our sites perfectly approximated the national population. Hence, in the event of relatively small (even if formally significant) differences in the background characteristics of our baseline, 6-month, and 18-month samples, it is unclear which one is in fact the most representative of the true national population of interest. Thus, to the extent that attrition differences may exist, it seemed highly suspect whether they would pose more than an academic threat to the validity of our findings.

Nonetheless, we determined that ignoring attrition entirely would be an unusual and unnecessary step, when we had at our disposal a perfectly sound strategy for addressing it. Moreover, if offenders from some state clusters were more likely than were those from other clusters to be retained at follow up, or if attrition rates turned out to be dramatically higher for some rather than other categories of offenders, or if drug court offenders were more or less likely to be retained than comparison offenders, such developments would certainly be undesirable. Therefore, we decided to adjust for retention-attrition differences in a parallel fashion as in the case of selection (above). As discussed below, because we did not believe that attrition posed as serious a potential problem as selection bias, we adopted a less aggressive approach to our attrition adjustment.

In proceeding, we hypothesized that any attrition bias might ultimately have less to do with the characteristics of different offenders than with the community-level characteristics of some, but not other sites; or with the effectiveness of the team of research interviewers that was assigned to some, but not other state-based geographic clusters. For this reason, we constructed court-level variables for each state cluster: coded 0 or 1 for offenders who were respectively from Florida, Georgia, Illinois, New York, North Carolina, South Carolina, Pennsylvania, and Washington.[55] We then examined bivariate differences between retained and attrited cases on the same 61 baseline characteristics that were analyzed for selection bias (see above) and on the aforementioned state cluster variables. Separate comparisons were conducted between those retained versus. attrited at 6 months, 18 months, and both periods.

At this equivalent point in the earlier analysis of selection bias, 37 of 61 baseline differences between drug court and comparison offenders were significant at least at $p < .05$. By comparison, far smaller numbers of significant differences arose between retained and attrited cases:

- *Six-month retention*: There were significant differences on 11 of 61 measures. They were: sample (drug court or comparison), race/ethnicity, drug or criminal involvement of friends/family members, drug or criminal involvement of blood relatives, any drug use (calendared up to six months pre-baseline), any heroin use (calendared), days per month of heroin use (calendared), primary drug, any treatment (calendared), days incarcerated (calendared), and days incarcerated or in residential treatment (calendared).

- *Eighteen-month retention*: There were significant differences on 8 of 61 measures: race/ethnicity, drug or criminal involvement of friends/family members, drug or criminal involvement of blood relatives, years of drug use, primary drug, any treatment (calendared), days of treatment (calendared), any prior arrest.

- *Six- **and** eighteen-month retention*: There were significant differences on 10 of 61 baseline measures: race/ethnicity, income, drug or justice involvement of friends/family members, drug or criminal involvement of blood relatives, any heroin use (calendared), days of heroin use (calendared), primary drug, any treatment (calendared), days of treatment (calendared), and days incarcerated (calendared).

Consistent with our hypothesis that locating offenders at follow up might be systematically easier in some locations than others, we did detect multiple significant differences between retained and attrited cases on our state cluster variables. In general, retention rates were significantly higher in New York, North Carolina, and Washington; and significantly lower in Florida, Illinois, and Pennsylvania. (Small numbers of cases came from the two Georgia sites or the one South Carolina site, and their retention rates did not significantly differ from the others.)

[55] Based on preliminary analyses, we slightly modified the cluster variable for New York to exclude Syracuse (a drug court), which had a clearly higher attrition rate than the seven other New York State sites. We similarly modified the court cluster variable for Washington to exclude the Pierce County Breaking the Cycle program (a comparison site), which had a clearly higher attrition rate than the six other Washington sites.

Our next step was to model the attrition process in an effort to better understand which factors were *most* tied to retention/attrition. Table 4-A.3 provides results for the modeling of attrition at the 18-month follow-up survey. (Separate and parallel analyses, not shown, were conducted for each other retention/attrition period.) Model 1 included 18 offender baseline characteristics with a significant or suggestive bivariate relationship with retention at least at one follow-up period ($p < .10$), and Model 2 added the six significant state cluster variables.

The results demonstrated that state cluster was generally more influential than individual offender characteristics. For instance, as shown in Table 4-A.3, only 6 of the 28 individual parameters in Model 2 were state cluster variables, but those variables almost doubled the total percent of variation explained (pseudo R^2) in 18-month retention: .042 for Model 1 and .082 for Model 2. In addition, in Model 2, four of the six individual parameters with a statistically significant effect on retention were state cluster variables: location in New York, Illinois, North Carolina, and Washington. Notable, however, is that even after controlling for all of the variables shown in Model 2, an R^2 statistic of .082 is relatively low, signaling relatively little differential attrition, despite the vast number of observed characteristics for which we were able to control.

Table 4-A.3. Predicting Survey Retention at 18 Months

Dependent Variable	**Retention in 18-Month Survey (Yes or No)**	
	N= 1,759	
	Model 1	**Model 2**
Chi-square for step	44.613**	44.287***
Chi-square for model	44.613**	88.900***
Nagelkerke R^2 for model	0.042	0.082
Independent Variables:	**Regression Coefficient**	
Male sex	-0.098	-0.114
Race/Ethnicity		
White	0.440*	0.285
Black/African-American	0.357+	0.285
Base 10 Logarithm of Annual Income	0.100	0.134*
Blood relatives/other family/friends involved with crime or drugs	0.037	0.037
Blood relatives involved with crime or drugs	-0.002	0.003
Years of drug use	0.011	0.008
Any drug use in 6 months prior to baseline	-0.110	-0.021
Any use of heroin in 6 months prior to baseline	-.012	-.069
Days of heroin use per month in 6 months prior to baseline	-0.009	-0.012

(continued)

Table 4-A.3. Predicting Survey Retention at 18 Months (Cont'd)

Independent Variables:	Regression Coefficient	
Primary drug of choice		
Alcohol	0.428	0.321
Marijuana	0.323	0.309
Cocaine (powder)	0.444	0.452
Crack cocaine	0.360	.386
Heroin	.119	0.032
Amphetamines (incl. methamphetamine)	0.378	0.465
Any previous drug/alcohol treatment in 6 months prior to baseline	.497**	0.465*
Any prior arrest	.314	.185
Anti-social personality disorder (based on multi-item inventory)	-0.075	-0.051
Psychotic: anti-social personality PLUS narcissistic personality	-0.019	0.038
Overall mental health (5-point scale from excellent to poor)	0.085	0.096
Total days incarcerated in 6 months pre-baseline	0.001	0.002
Total days incarcerated or in residential treatment pre-baseline	-.001	-.002
Court Cluster		
Pennsylvania court cluster		.024
New York court cluster		1.319***
Illinois court cluster		.647**
Florida court cluster		.015
North Carolina court cluster		1.096***
Washington court cluster		.517*

Note: The New York cluster variable included 7 of the 8 New York State drug courts, omitting Syracuse, whose pattern of attrition was significantly different than the other New York courts (Syracuse had greater attrition). The Washington cluster variable included 6 of the 7 Washington State drug courts, omitting Pierce Breaking the Cycle, a comparison site, whose pattern of attrition was significantly different than the other Washington courts (Pierce had greater attrition). See note and footnotes underneath Table 4-A.1 for additional information on the variables in this table.
*+p<.10, *p<.05, **p<.01, ***p<.001.*

In test models, we were specifically interested in clarifying whether sample status (drug court or comparison offender) predicted retention. In simple bivariate comparisons, we found that sample status was significantly associated with retention at six-month follow up (drug court offenders were more likely to be retained, $p < .05$), but sample status did not predict retention at any other period. After controlling for other characteristics, the effect of sample status fully disappeared at six months ($p = .661$ when controlling for all of the other characteristics shown in Table 4-A.3). We concluded that sample status in itself did not influence the probability of retention/attrition.

We next developed a final model for the purpose of generating *retention scores* for each case. The concept of a retention score is essentially parallel to that of a propensity score. The score derives from a logistic regression predicting retention at follow up (dependent variable: 0 = attrited, 1 = retained). For our final regression models, we included any predictor whose bivariate relationship with retention in at least one of the follow-up periods had a p-value at $p < .10$. We then ran backward stepwise removal, with variables deleted at $p > .20$.[56] Notably, these criteria are less inclusive than in our propensity models (see above). Our rationale was our belief that attrition differences posed far less threat to study validity than selection bias: overall attrition rates were low, and, as discussed above, we believed that attrition in this study was unlikely to compromise external validity in a substantively meaningful way. Since we ultimately planned to implement a combined "super weighting" adjustment, we did not want the factoring in of the attrition portion of that adjustment to compromise the effectiveness of the selection portion. In short, we were most determined to ensure that after implementing our combined adjustment, the participant and comparison samples would be as statistically identical as possible—that we would thoroughly wipe out selection bias. In all, we entered 18 baseline characteristics and 6 state cluster variables in all retention models (including the same variables shown in Table 4-A.3). To address missing data on 22 cases, we ran a second set of models that deleted 4 of the 18 baseline characteristics (i.e., years of drug use, average days per month of heroin use, any prior arrest, and self-rated mental health status).

Computing Super Weights

As noted in Chapter Two, super weights were computed based on the following formulas.

- ✓ Retained drug court offenders: 1 / (propensity score * retention score)
- ✓ Retained comparison offenders: 1 / ((1 – propensity score) * retention score)
- ✓ Attrited drug court offenders: 1 / (propensity score * (1 – retention score)
- ✓ Attrited comparison offenders: 1 / ((1 – propensity score) * (1 – retention score))

Separate super weights were computed for retention at 6 months, 18 months, both periods, and for the oral fluids test.[57] In accordance with common best weighting practices, each set of super weights was standardized to have a mean of one, extremely high weights were truncated to four, and extremely low weights were raised to 0.25. For no set of weights did more than one percent of the sample begin with extreme weights that had to be modified in this fashion.

[56] An initial set of models set a criterion of $p > .10$ for backward stepwise removal, but when we later completed the super weighting process and examined the performance of the weights, we determined that they did not adequately correct for attrition bias and then adjusted the removal criterion slightly upwards to $p > .20$.

[57] Two caveats follow. First, for each retention period, propensity scores had not been computed for attrited cases, but only for those that were retained. To generate weights for attrited cases at each period, we used the propensity scores that were generated in the original propensity models for all 1,781 offenders surveyed at baseline. Second, we did not construct a special retention score for those who took the oral fluids test, but simply used the 18-month score. Our reasoning was that only 5 percent of eligible offenders outright refused the oral fluids drug test, an extremely low refusal rate. Therefore, any special attrition from those who took the 18-month survey to the smaller subset that took the drug test was almost exclusively due to the bias entailed by our inability to test incarcerated offenders (who comprised 17 percent of those surveyed at 18 months). We viewed this bias as inescapably inherent in the drug test sub-sample and, indeed, an important limitation in the drug test data, but not one that could be honestly explained away through special modeling efforts.

Performance of the Super Weights

The super weights virtually eliminated observable selection and attrition bias. Table 4-A.4 illustrates the effect of super weighting for those who were surveyed at 18 months. The table compares the samples on our standard set of 61 baseline characteristics, first using unweighted data (leftmost columns) and then weighted data (rightmost columns). We found similar effects of super weighting among those surveyed at each other follow-up period.

Regarding attrition bias, the 6-month weights eliminated all except three significant differences between those retained and attrited at 6 months; the 18-month weights and the weights for those retained versus attrited at both follow-up periods each left two significant differences. On the other hand, more than one dozen significant differences persisted between offenders who did and did not take the oral fluids drug test. Although problematic, this last finding reflected the inherent biases entailed by not drug testing incarcerated offenders (see Volume 1, Chapter 3 for details). In fact, we separately determined that those who were incarcerated as of their 18-month survey averaged consistently greater self-reported drug use and criminal behavior than those who were not incarcerated. In other words, the external validity of the oral fluids data is necessarily restricted to a somewhat “lower risk” sub-population than our full sample. We considered it impossible to eliminate this bias through statistical modeling, since it followed inescapably from the exclusion of incarcerated offenders from the oral fluids drug test. While acknowledging this limitation, by successfully addressing the selection bias problem between drug court and comparison offenders who *could* be administered the drug test (see above), we were at least confident that our results had strong internal validity among those who *were* tested.

Table 4-A.4. Comparison of Baseline Sample Differences: Offenders Surveyed at 18-Month Follow Up

	UNWEIGHTED		WEIGHTED	
Characteristics	**Drug Court N=951**	**Comparison Group N=523**	**Drug Court N=951**	**Comparison Group N=523**
Demographics				
Age	33.19**	35.14	33.69	34.28
Male	68%	72%	70%	69%
Race/Ethnicity	***			
White	59%	51%	55%	54%
Black/African-American	28%	41%	32%	25%

(continued)

Table 4-A.4. Comparison of Baseline Sample Differences: Offenders Surveyed at 18-Month Follow Up (Cont'd)

	UNWEIGHTED		WEIGHTED	
Characteristics	**Drug Court N=951**	**Comparison Group N=523**	**Drug Court N=951**	**Comparison Group N=523**
Hispanic / Latino	6%	4%	6%	5%
Other (incl. multiracial)	7%	4%	7%	6%
HS degree/GED or higher	62%**	54%	59%	60%
Base 10 Logarithm of Annual Income	4.57***	$4.29	$4.46	$4.46
Social Ties				
Married	11%	12%	11%	12%
Primary care responsibility for children less than 18	19%	16%	18%	17%
Homeless: prior 6 months	12%	13%	12%	12%
Ever homeless	46%	48%	47%	45%
Currently employed or in school	43%	38%	41%	42%
Currently employed	38%*	33%	37%	37%
Currently in school	8%	9%	8%	9%
Weeks worked: last 6 months	9.07*	7.71	8.73	8.62
Blood relatives/family/friends involved with crime/drugs	6.60	6.33	6.33	6.32
Blood relatives involved with crime or drugs	1.86*	1.64	1.77	1.67
Drug Use				
Years of drug use				
Years of drug use (8 drugs)	19.65*	21.39	20.09	20.49
Years of drug use (6 drugs, excl. alcohol and marijuana)	13.5+	14.58	13.80	13.66
Drug use in the 6 months prior to baseline: summary:				
Any use (8 drugs)	84%**	77%	83%	81%
Any use (7 drugs)	73%**	65%	72%+	67%
Any use (6 drugs)	61%**	52%	58%	56%
Days of use per month (8 drugs)	13.72**	11.69	13.16	12.77
Days of use per month (7 drugs)	9.91**	8.46	9.50	8.83
Days of use per month (6 drugs)	7.9*	6.58	7.45	7.09
Drug use in the 6 months prior to baseline: by drug:				
Any use of heavy alcohol	40%	40%	41%	38%

(continued)

Table 4-A.4. Comparison of Baseline Sample Differences: Offenders Surveyed at 18-Month Follow Up (Cont'd)

Characteristics	UNWEIGHTED Drug Court N=951	UNWEIGHTED Comparison Group N=523	WEIGHTED Drug Court N=951	WEIGHTED Comparison Group N=523
Days of use per month of heavy alcohol	2.77	3.18	3.45	3.15
Any use of marijuana	45%*	39%	45%	42%
Days of use per month of marijuana	6.16***	4.00	5.87*	4.68
Any use of cocaine	44%**	36%	42%	39%
Days of use per month of cocaine	4.12	3.65	3.84	3.91
Any use of heroin	11%	13%	11%	12%
Days of use per month of heroin	1.63	2.05	1.73	1.71
Any use of hallucinogens/designer drugs	9%**	5%	7%	7%
Days of use per month of hallucinogens/designer drugs	0.21**	0.06	0.19*	0.06
Any use of amphetamines	15%*	11%	13%	14%
Days of use per month of amphetamines	2.04**	1.12	1.72	1.63
Any use of prescription drugs: illegal use	16%	14%	16%	14%
Days of use per month of heavy alcohol	2.77	3.18	3.45	3.15
Primary drug of choice	***			
Alcohol	12%	16%	13%	14%
Marijuana/hashish	22%	19%	22%	21%
Cocaine (powder)	12%	9%	11%	10%
Crack cocaine	26%	27%	27%	25%
Heroin	3%	8%	5%	5%
Amphetamines (incl. methamphetamine)	12%	6%	9%	10%
Other or claimed not using drugs	12%	15%	13%	15%
Addiction severity index	9.52***	8.48	9.23	8.95
Previous treatment in six months prior to baseline				
Any drug/alcohol treatment	37%***	20%	29%	29%
Days of treatment per month	2.67***	1.18	2.02	2.15
Criminal History				
Any prior arrests	87%**	93%	88%	91%
Base 10 logarithm of # prior arrests	1.63***	1.77	1.66	1.69
Any prior convictions	69%***	78%	72%	72%
Any prior violent convictions	12%***	20%	15%	14%

(continued)

Table 4-A.4. Comparison of Baseline Sample Differences: Offenders Surveyed at 18-Month Follow Up (Cont'd)

	UNWEIGHTED		WEIGHTED	
Characteristics	Drug Court 951	Comparison Group 523	Characteristics	Drug Court 951
Criminal activity in 6 months prior to baseline:				
Any criminal activity	75%	71%	74%	74%
Base 10 logarithm of # criminal acts	1.96**	1.84	22.38	21.70
Drug activity	70%+	65%	69%	67%
Mean number of drug-related criminal acts	14.08**	12.02	13.52	12.89
Drove while intoxicated	35%	32%	35%	33%
Mental and Physical Health				
Overall mental health (5-point scale, poor to excellent)	3.45	3.42	3.44	3.43
Percent depressed (multi-item inventory)	39%	37%	39%	37%
Anti-Social personality disorder (multi-item inventory)	43%	42%	43%	43%
Narcissistic personality disorder (multi-item inventory)	48%	50%	48%	50%
Psychotic: anti-social PLUS narcissism	27%	26%	27%	26%
Victimized by any abuse in past year	41%	46%	42%	43%
Victimized by any physical abuse in past year	29%*	35%	30%	32%
Any chronic medical problems (currently)	26%	27%	27%	25%
Control Variables: Calendared Data				
Total days incarcerated pre-baseline	20.97***	38.33	27.76	29.56
Total days incarcerated or in residential treatment	25.92***	41.25	31.71	33.10
Number of pre-baseline months in baseline survey	5+	5.06	5.04	5.04

Note: See note and footnotes underneath Table 4-A.1 for additional information on the variables in this table. Missing data on select variables among those offenders who were surveyed at 18-month follow up was as follows: age (1), years used drugs (4), years used drugs other than marijuana or alcohol (1), any physical health problems (4), any prior arrest (14), and number of prior arrests (44). In addition, 516 cases were missing on any abuse and any physical abuse, as a result of the addition of these measures after survey implementation was already underway.
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Table 4-A.5 illustrates the effect of super weighting in addressing possible attrition bias by comparing those who were retained versus attrited as of the 18-month survey. The first set of comparisons used unweighted data (leftmost columns), and the next set used weighted data (rightmost columns). These comparisons included our standard 61 characteristics along with the state cluster variables. The results demonstrate that super weighting reduced the magnitude and number of significant differences, particularly with respect to the state clusters, on which extensive differences existed when using unweighted data.

We then sought to examine whether the super weights that were designed for one period—those retained at 18 months—could suffice to address selection and attrition differences at other periods (e.g., 6 months, both periods, or for the oral fluids sub-sample). We confirmed that the 18-month weights were universally effective in reducing baseline differences between drug court and comparison offenders at all other periods. With regard to attrition, although the 18-month weights sharply reduced the magnitude of the differences between retained and attrited cases at all other periods, 10 or more significant differences consistently remained (e.g., at 6 months or both periods). In sum, whereas the 18-month weights could thoroughly handle selection bias in any conceivable analysis, we found that only the weights that were specially designed for a given follow-up period were fully adequate to eliminate its attrition differences.

Table 4-A.5. Comparison of Baseline Sample Differences: Retained Versus Attrited at 18 Months

	UNWEIGHTED		WEIGHTED	
Characteristics	**Retained N=1,156**	**Attrited N=625**	**Retained N=1,156**	**Attrited N=625**
Demographics				
Age	33.88	32.86	33.90	33.55
Male	69%	71%	70%	68%
Race/Ethnicity	*			
White	56%	48%	55%	51%
Black/African-American	33%	35%	33%	36%
Hispanic / Latino	5%	10%	6%	8%
Other (incl. multiracial)	6%	7%	6%	5%
HS degree/GED or higher	59%	58%	59%	59%
Base 10 Logarithm of Annual Income	4.47+	$4.36	$4.46	$4.43
Social Ties				
Married	11%	9%	11%	11%
Primary care responsibility for children less than 18	18%	18%	17%	20%

(*continued*)

Table 4-A.5. Comparison of Baseline Sample Differences: Retained Versus Attrited at 18 Months (Cont'd)

Characteristics	UNWEIGHTED Retained N=1,156	UNWEIGHTED Attrited N=625	WEIGHTED Retained N=1,156	WEIGHTED Attrited N=625
Homeless: prior 6 months	12%	12%	12%	11%
Ever homeless	47%	51%	46%	47%
Currently employed or in school	41%	42%	42%	38%
Currently employed	36%	36%	37%	33%
Currently in school	8%	10%	9%	9%
Weeks worked: last 6 months	8.59	8.79	8.69	8.06
Blood relatives/family/friends involved with crime/drugs	6.5**	5.50	6.33	6.19
Blood relatives involved with crime or drugs	1.78*	1.50	1.74	1.79
Drug Use				
Years of drug use				
Years of drug use (8 drugs)	20.27*	18.86	20.23	20.02
Years of drug use (6 drugs, excl. alcohol and marijuana)	13.88	12.92	13.75	13.45
Drug use in the 6 months prior to baseline: summary:				
Any use (8 drugs)	82%	80%	82%	80%
Any use (7 drugs)	70%	70%	70%	68%
Any use (6 drugs)	58%	56%	57%+	52%
Days of use per month (8 drugs)	13.00	12.94	13.02	12.41
Days of use per month (7 drugs)	9.40	8.87	9.26	8.64
Days of use per month (6 drugs)	7.43	7.45	7.32	6.95
Drug use in the 6 months prior to baseline: by drug:				
Any use of heavy alcohol	40%	42%	40%	44%
Days of use per month of heavy alcohol	3.36	2.87	3.34	3.00
Any use of marijuana	43%	45%	44%	43%
Days of use per month of marijuana	5.39	5.97	5.45	5.28
Any use of cocaine	41%	40%	41%+	35%
Days of use per month of cocaine	3.95	3.80	3.87	3.54
Any use of heroin	12%	13%	11%	13%
Days of use per month of heroin	1.78	2.23	1.72	1.79

(continued)

Table 4-A.5. Comparison of Baseline Sample Differences: Retained Versus Attrited at 18 Months (Cont'd)

Characteristics	UNWEIGHTED Retained N=1,156	UNWEIGHTED Attrited N=625	WEIGHTED Retained N=1,156	WEIGHTED Attrited N=625
Any use of hallucinogens/designer drugs	7%	8%	7%	8%
Days of use per month of hallucinogens/designer drugs	0.16	0.24	0.14	0.22
Any use of amphetamines	14%	12%	14%	12%
Days of use per month of amphetamines	1.72	1.29	1.69	1.15
Any use of prescription drugs: illegal use	16%	13%	15%+	11%
Any use of hallucinogens/designer drugs	7%	8%	7%	8%
Days of use per month of hallucinogens/designer drugs	0.16	0.24	0.14	0.22
Any use of amphetamines	14%	12%	14%	12%
Days of use per month of amphetamines	1.72	1.29	1.69	1.15
Any use of hallucinogens/designer drugs	7%	8%	7%	8%
Days of use per month of hallucinogens/designer drugs	0.16	0.24	0.14	0.22
Primary drug of choice	*		*	
Alcohol	14%	11%	13%	14%
Marijuana/hashish	21%	24%	22%	25%
Cocaine (powder)	11%	9%	10%	8%
Crack cocaine	26%	23%	27%	23%
Heroin	5%	7%	5%	6%
Amphetamines (incl. methamphetamine)	10%	7%	9%	8%
Other or claimed not using drugs	13%	19%	16%	16%
Addiction severity index	9.15	8.94	9.13	8.91
Previous treatment in six months prior to baseline				
Any drug/alcohol treatment	31%**	20%	29%	28%
Days of treatment per month	2.14*	1.42	2.07	1.69
Criminal History				
Any prior arrests	89%+	85%	89%	88%
Base 10 logarithm of # prior arrests	1.68	1.63	1.67	1.66

(continued)

Table 4-A.5. Comparison of Baseline Sample Differences: Retained Versus Attrited at 18 Months (Cont'd)

Characteristics	UNWEIGHTED Retained N=1,156	UNWEIGHTED Attrited N=625	WEIGHTED Retained N=1,156	WEIGHTED Attrited N=625
Any prior convictions	72%	68%	72%	75%
Any prior violent convictions	15%	17%	15%*	20%
Criminal activity in 6 months prior to baseline:				
Any criminal activity	74%	74%	74%	73%
Base 10 logarithm of # criminal acts	1.91	1.87	1.91	1.84
Drug activity	68%	68%	68%	65%
Mean number of drug-related criminal acts	13.35	12.49	13.30	12.01
Drove while intoxicated	34%	32%	34%	30%
Mental and Physical Health				
Overall mental health (5-point scale, poor to excellent)	3.44	3.36	3.44	3.42
Percent depressed (multi-item inventory)	38%	38%	38%	39%
Anti-Social personality disorder (multi-item inventory)	43%	43%	43%	44%
Narcissistic personality disorder (multi-item inventory)	49%	50%	49%	53%
Psychotic: anti-social PLUS narcissism	27%	28%	28%	29%
Victimized by any abuse in past year	43%	37%	42%+	34%
Victimized by any physical abuse in past year	31%	25%	31%+	23%
Any chronic medical problems (currently)	27%	26%	26%	29%
Control Variables: Calendared Data				
Total days incarcerated pre-baseline	27.13	28.44	28.40	25.80
Total days incarcerated or in residential treatment	31.36	31.68	32.20	30.02
Number of pre-baseline months in baseline survey	5.02	5.05	5.04	5.07
Court Cluster				
Pennsylvania court cluster	6%**	10%	6%	6%
New York court cluster	13%**	5%	11%	12%
Illinois court cluster	15%	15%	15%	16%

(continued)

Table 4-A.5. Comparison of Baseline Sample Differences: Retained Versus Attrited at 18 Months (Cont'd)

	UNWEIGHTED		WEIGHTED	
Characteristics	Retained N=1,156	Attrited N=625	Retained N=1,156	Attrited N=625
Florida court cluster	13%**	19%	15%	14%
North Carolina court cluster	15%**	8%	14%	14%
Washington court cluster	20%+	15%	18%	19%

Note: See note and footnotes underneath Tables 4-A.1 and 4-A.3 for additional information on the variables in this table.
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Super Weighting for Official Recidivism Outcomes

As was fully described in Chapter 2, we next duplicated the same process described above to create a special set of super weights to apply exclusively in analyses of official records outcomes. Table 4-A.6 compares the drug court and comparison samples on baseline criminal justice measures that we were able to collect from official records. As the table indicates, the two samples were significantly different on all measures except for the percentage of offenders with at least one prior arrest. All measures except any prior arrest were added to our special administrative records propensity model.

Table 4-A.6. Comparison of Official Criminal History Differences

	Drug Court	Comparison Group
Number of Cases	**1,022**	**512**
Any prior arrest	90%	90%
Number of prior arrests	9.03***	13.32
Any prior drug-related arrest	64%***	74%
Number of prior drug-related arrests	1.92***	3.00
Current arrest charge[1]	***	
Drug-related	42%	39%
Property-related	40%	34%
Other	18%	27%

Note: Statistical significance is based on t-tests for prior criminal history variables and chi square test for arrest charge.
[1] The arrest charge variable is missing 74 drug court and 58 comparison cases (thus N = 948 and N = 454 respectively).
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Hierarchical Modeling

To confirm the need for a hierarchical modeling framework, the preferred method is to *partition the variance* in outcomes. This step involves distinguishing the proportion of the variance that lies between individual offenders ("within-site" variance) and the proportion that lies between sites—of the 29 total sites in the study—and thus systematically differentiates the sites ("between-site" variance). To do this, one executes an *unconditional (or null) model*. An unconditional model is equivalent to a one-way ANOVA─it includes an outcome measure without any predictor variables. The resulting Level 1 (individual-level) equation predicts the given outcome measure, based on the mean values (i.e., the intercept) within each of the sites and the error for each of the study offenders. The resulting Level 2 (site-level) equation models each site's intercept in the first place as a function of the grand mean (i.e., the overall mean outcome across all sites) and each site's deviation from that mean. The resulting variance components can be used to calculate an *intraclass correlation coefficient (ICC)* that is the ratio of the Level 2 between-site variance in outcomes to total variance (between-site plus within-site). A significant intraclass correlation coefficient or, in other words, a statistically significant amount of between-site variance, indicates a significant clustering of outcomes at the site level. In this case, hierarchical modeling adjustments are indicated. On the other hand, non-significant between-site variance indicates that the use of hierarchical modeling techniques would yield little additional information as compared with standard t-tests or regression methods.

In proceeding, we ran unconditional models on one or two critical outcome measures from each of seven domains:

- *Drug use*: average days of drug use per month in the year prior to the 18-month survey; and whether the offender tested positive for any drugs in the 18-month drug test.

- *Criminal activity*: number of official re-offenses up to 24 months after baseline; and number of self-reported criminal acts in the year prior to the 18-month survey.

- *Incarceration*: number of days incarcerated over 24 months on any case and number of days incarcerated on the precipitating criminal case (both based on official records data).

- *Socioeconomic status*: employed or in school as of the 18-month survey; and total annual income as of the 18-month survey.

- *Mental health*: classified as "depressed" on the 18-month survey (multi-item instrument).

- *Family support and conflict*: family conflict index on the 18-month survey (mean of three statements on conflict in the past year); and family emotional support index on the 18-month survey (mean of seven statements on emotional support in the past year).

- *Homelessness*: whether the offender was homeless at any time in the year prior to the 18-month survey.

For each measure, we ran an unconditional model using HLM 6.04 software. Table 4-A.7 displays the results (see Level 2), which universally confirm the presence of site-level clustering. Of particular concern, for official re-arrests over 24 months, the results also indicate an exceptionally high intraclass correlation coefficient (ICC) of .262; in other words, more than 26 percent of the variation in official re-arrests is explained by site-level differences, presumably resulting in large part from differential police enforcement practices. By comparison, the ICC for criminal behavior was only .026, still significant, but exactly one-tenth the magnitude of the coefficient for official re-arrests. Furthermore, none of the intraclass correlation coefficients for any other measure in Table 4-A.7 exceeded .110 (and none of the intraclass correlation coefficients for survey-based data exceeded .060), indicating that inter-site variation was uniquely and uncharacteristically high with respect to official re-arrests. The most likely reason is that, unlike all of our other outcome measures, the likelihood that a person is re-arrested reflects not only their own actions, but also local law enforcement practices. It would seem that in some jurisdictions, whether due to differences in police deployment patterns and enforcement, or due to differences in geo-spatial opportunities to avoid detection, criminal behavior is significantly less likely to result in an arrest than in other sites. The practical implication of the high ICC for official re-arrests (reflecting high inter-site variation) is that it may prove difficult for this study to produce statistically significant estimates regarding the overall drug court impact on re-arrests.

In results not shown, we re-ran the same unconditional models separately for drug court and comparison sites—to ensure that the observed site-level clustering was not simply a function of the 23 drug courts clustering together and the 6 comparison sites clustering together. In these latter models, the between-site variance continued to be significant. We thus resolved to conduct all analyses of drug court impacts on criminal behavior, drug use, and other psychosocial outcomes in HLM and to treat drug court status as a Level 2 predictor: a characteristic of sites, not of individuals. (As described below, certain additional methods were utilized when modeling the role of intervening policy, practice, and attitudinal characteristics, such as judicial status hearings, drug testing, treatment dosage, and perceptions of the judge.)

Analytic Plan

Most elements of the analytic plan were introduced previously in Chapter 2. To review briefly, in answering whether drug courts produce positive benefits, we ran all final models using weighted data and hierarchical modeling methods in HLM 6.04. We divided our many outcome measures among seven domains: (1) drug use, (2) criminal activity, (3) incarceration, (4) socioeconomic status, (5) mental health, (6) family support and conflict, and (7) homelessness. When analyzing results on each outcome measure, we entered drug court status (drug court or comparison site) as a single Level 2 predictor variable—without any other predictors. Since the 18-month weights were effective in eliminating selection bias among cases that were retained at all other periods (6 months, both periods, oral fluids drug test, or recidivism data available), we opted to employ these weights universally, rather than change the weights for different analyses. For each outcome measure, we selected the most appropriate regression specification, of those that are distributions that are right-skewed. To provide easily interpreted "bottom-line" results, we

Table 4-A.7. Unconditional HLM Models for Major Offender Outcomes

Drug Use				
Days of Drug Use Per Month: Year Prior to 18-Month Survey (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	2.8	0.3	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	1.9	1.4	28	0.000
Level 1, R	33.8	5.8		
Intraclass correlation coefficient	0.052			
Positive Drug Test at 18-Month Survey (N = 1,147):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	-0.72	0.14	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.45	0.67	28	0.000
Criminal Behavior				
Number of Criminal Acts (Self-Reported): One Year Prior to 18-Month Survey (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	53.3	5.5	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	468.6	21.6	28	0.000
Level 1, R	17258.1	131.4		
Intraclass correlation coefficient	0.026			
Number of Official Re-Arrests: 24 Months Post-Enrollment (N = 1,534):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	1.25	0.13	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.64	0.41	28	0.000
Level 1, R	1.80	3.24		
Intraclass correlation coefficient	0.262			

(continued)

Table 4-A.7. Unconditional HLM Models for Major Offender Outcomes (Cont'd)

Incarceration				
Number of Days Incarcerated (Administrative Data): 24 Months Post-Enrollment (N = 1,458):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	41.4	6.7	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	921.4	30.4	28	0.000
Level 1, R	13736.5	117.2		
Intraclass correlation coefficient	0.063			
Number of Days Incarcerated (Administrative Data) on the Precipitating Criminal Case (N = 1,403):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	68.8	14.6	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	4981.3	70.6	28	0.000
Level 1, R	41554.8	203.8		
Intraclass correlation coefficient	0.107			
Socioeconomic Status				
Employed or in school: 18-Month Mark (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	0.60	0.11	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.26	0.51	28	0.000
Annual Income at the 18-Month Mark (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	16742.8	1010.8	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	21185315.6	4602.8	28	0.000
Level 1, R	341169816.9	18470.8		
Intraclass correlation coefficient	0.058			

(continued)

Table 4-A.7. Unconditional HLM Models for Major Offender Outcomes (Cont'd)

Mental Health				
Classified as Depressed at the 18-Month Mark (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	-0.94	0.08	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.08	0.28	28	0.005
Family Support and Conflict				
Family Conflict Index Score (3-item index) on the 18-Month Survey (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	2.28	0.04	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.04	0.19	28	0.000
Level 1, R	0.78	0.88		
Intraclass correlation coefficient	0.05			
Family Emotional Support Index Score (7-item index) on the 18-Month Survey (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	4.23	0.04	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.02	0.15	28	0.000
Level 1, R	0.51	0.71		
Intraclass correlation coefficient	0.05			
Homelessness				
Homeless at any Time in the Year Prior to the 18-Month Survey (N = 1,474):				
Fixed effects	*Coefficient*	*Stnd. Error*	*Df*	*p value*
Intercept, G0	3.22	0.19	28	0.000
Random effects	*Variance*	*Stnd. Deviation*	*Df*	*p value*
Level 2, U0	0.80	0.89	28	0.000

Note: For purposes of these unconditional models, a simple OLS model was run on all measures with more than two possible values, whereas logistic regression (Bernoulli) models were run on all dichotomous measures (drug test result, employed or in school, classified as depressed, and homelessness status. Models were run for the maximum available samples (N's in parentheses), and data was unweighted.

transformed the regression coefficients for the intercept and for drug court status to produce adjusted mean outcomes for drug court and comparison offenders on each measure.

available in HLM: ordinary least squares for normally distributed outcomes, logistic regression for dichotomous measures (any criminal behavior), and Poisson regression for count distributions that are right-skewed. To provide easily interpreted "bottom-line" results, we transformed the regression coefficients for the intercept and for drug court status to produce adjusted mean outcomes for drug court and comparison offenders on each measure.

Where including additional predictor variables, we estimated them as fixed effects, based upon test random effects models, whose results made clear that extremely few of our predictor variables exerted significantly different effects by site.[58] When analyzing whether the magnitude of the drug court impact was greater or lesser for offenders with select characteristics, we ran three-predictor regression models, including drug court status, the given characteristic, and an interaction term. Significant interaction terms meant that the drug court produced *especially* better or worse outcomes than the comparison group for offenders with the given characteristic. If our results had produced many significant interactions, we planned to combine multiple baseline measures into theoretically based scores (e.g., "high" or "low" risk classifications) and to add more control variables to our models. This step became superfluous, as remarkably few significant interactions were detected.

Finally, when examining *how* drug courts work, we adopted two distinct approaches. In the first, we focused on the 23 drug court sites only, enabling us to test which factors led some drug courts to have better outcomes than other drug courts. In the second, we included all 29 sites, enabling us to test which program-level and attitudinal factors explained the impact of the drug court, *relative to the comparison group*. Within this second approach, we then conducted separate analyses using (1) a conventional hierarchical modeling framework and (2) a structural equation modeling framework. These varying methods are summarized in the three sub-sections that follow.

Analysis of Effective Policies and Practices in Drug Court Sites Only

For this analysis, we employed an innovative approach that ranked courts in levels of effectiveness at preventing drug use and crime (overall and for various subgroups). Once ranked, each court was characterized based on the way they implemented particular policies and practices, enabling a qualitative comparison of patterns of practices observed among the most effective courts. Toward this end, we created a counterfactual for each individual in our dataset that is the difference between a person's expected outcome for drug use and expected outcome for criminal behavior (based on data from like individuals in the comparison group) to their observed outcomes in drug court. Thus, we predicted what participants' drug use and criminal activity would have been without drug court, conditional on their particular characteristics, and subtracted observed outcomes from this predicted expected outcome. Next, we ranked courts based on the average performance of their participants. Positive court average values indicate

[58] In test random effects models, we found that of 20 parameters from the baseline variables, only three exerted significantly different effects by site on the number of self-reported criminal acts at 18 months: criminal behavior or drug involvement of blood relatives, number of prior criminal acts, and classification with depression at baseline. Furthermore, none of our independent predictors exerted significantly different effects by site on the average days of drug use per month in the year prior to the 18-month survey. We therefore did not have good reason to believe that a random effects framework was necessary in our final models.

that, overall, the court's participants did better as a result of being in drug court; negative values indicate that, on average, participants in that court did worse than would be expected. Courts are ranked based on two outcomes of interest: days of drug use prevented and number of criminal activities prevented.

Once the rankings were created for the two outcomes, we assigned color codes to each court that characterize the way they implemented particular policies and practices. From these data, we can identify if there are patterns for effective courts in how they implement policies and practices compared to ineffective courts. We also can identify if, among effective courts, there are patterns among top performing courts in how they implement policies and practices as compared to lower performing courts.

Analysis of Effective Policies and Practices Across All 29 Sites: Hierarchical Modeling

As discussed above, hierarchical models are traditionally used to analyze nested data. Similar to students in a classroom, drug court clients in a courtroom are generally repeatedly exposed to the same judge. Thus, it is easy to confuse the effect of the judge on outcomes for the effect of the drug court on outcomes. Thus, hierarchical models parse out individual effects on outcomes from court effects on outcomes. Using this approach, the first level is the individual participants, with the model controlling for differences among individuals. The second level is the courts in which individuals are clustered, with Level 2 variables including court-level policies and practices. The analysis reflects which practices, in general, are associated with the best outcomes, after controlling for individual level differences. Thus, in Chapter 7, we present findings from this approach, testing the impact not only of drug court participation in general, but of each individual court policy and practice, using hierarchical analysis of variance with follow-up Tukey tests of group comparisons.

Analysis of Theoretical Pathways That Effect Changes in Court Practices, and Offender Attitudes, Perceptions, and Behaviors

We further explored the theoretical pathways through which drug courts effect changes in court practices, and offender attitudes, perceptions, and behavior, by empirically testing those pathways using multilevel structural equation modeling (MSEM). We focused on the 1,349 respondents who completed all three interviews—at baseline, and at 6 and 18 months post-enrollment. This sample represented an impressive retention rate of 76 percent of drug court respondents (n=877) and 76 percent of comparison court respondents (n=472), based on those originally interviewed at baseline. All of the analyses were weighted to adjust for initial differences between drug court and comparison court respondents at baseline, as well as any attrition bias. The final sample of 1,349 respondents was mostly male (70 percent), with an average age of 34 years. Fifty-five percent were Caucasian and one-third were Black/African-American. More than half (59 percent) had their high-school diploma or GED equivalent, and the average annual income reported was $10,866. Nine in ten respondents had one or more prior arrests. Details of the MSEM are presented in Chapter 6.

Sensitivity Analyses

Our final analytic plan was not the only one that might have been attempted. To investigate the possible impact of method on outcomes, we conducted a series of *sensitivity analyses*. The first such analysis explored the issue of *time at risk*, determining the impact on drug use and recidivism outcomes of adjusting for the number of days during each tracking period when the offenders were incarcerated. The second sensitivity analysis explored whether weighting, hierarchical modeling, or several other methods for addressing selection or site-level biases produced substantively different results. The third analysis explored the implications of using the 18-month weights universally, throughout all analyses involving offender survey outcomes.

Adjusting for Time at Risk

Offenders presumably have less opportunity to use drugs and commit crimes when they are incarcerated. For this reason, studies of recidivism and drug use occasionally control for *time at risk* (i.e., time not incarcerated) when conducting outcome analyses. Although outcomes that are thereby adjusted for time at risk can provide useful information, they do not come without methodological problems of their own. For one, is it truly safe to assume that offenders are not at risk of drug use when they are incarcerated? If, by contrast, offenders can access drugs while incarcerated, discounting time incarcerated from the outcome variables might inadvertently introduce a new bias by, in effect, inappropriately assigning an increased adjusted rate of drug use to offenders who were incarcerated for longer periods. Second, with specific respect to recidivism outcomes, one might posit that if an intervention makes incarceration more or less likely, and that in turn contributes to reduced or increased recidivism, it is no less the case that the intervention itself legitimately affected recidivism. In this perspective, incarceration might be viewed as an intervening mechanism, perhaps to be included in more complex modeling (i.e., impact of intervening policies and practices), but not something to be controlled away when reporting simple impacts (e.g., do drug courts work). Finally, controlling for time at risk poses methodological problems, because the direction of causality is often unclear: Did an offender have less opportunity to commit crimes due to incarceration, or was the offender incarcerated in the middle of a follow-up tracking period due to committing crimes earlier in the tracking period? In fact, more incarceration time is often found to be positively associated with recidivism, because even though incarcerated offenders had less time at risk, their incarceration resulted, in the first place, from their preceding criminal behavior.

These concerns dictate the use of care before adjusting outcomes for time at risk—and suggest that if adjustments are made, outcomes should still be reported both with and without adjustments. In this study, we began simply by investigating whether a time at risk adjustment would make any real difference in our findings. To do this, we created a series of rate variables that discounted days spent incarcerated. To illustrate, modifying our days of drug use per month measure, we created a new measure reflecting days of drug use per month *at risk* (truncating the measure at a maximum of 30 days). Modifying self-reported criminal acts over 18 months, we created a rate measure for the number of criminal acts per year at risk. Modifying official re-arrests over 24 months, we created a rate variable for re-arrests per year at risk. A small number of outliers were capped at a rate of 1,000 criminal acts per year; and one extreme outlier for the adjusted re-arrest measure was truncated at 12.67 arrests per year (the next highest value).

Table 4-A.8 compares drug court impacts on all of our key drug use, criminal behavior, and official recidivism count measures—with and without a time-at-risk adjustment. None of the outcomes shifted between statistical significance and non-significance, depending on whether or not we adjusted the outcomes. The only semi-exception is that after adjusting for time at risk, the number of re-arrests per year went from non-significant to suggestively significant at the .10 threshold. Overall, the regression coefficients were similar, with some specific measures suggesting a slightly weaker and some a slightly stronger drug court impact after making the adjustment. As a result, we opted to omit a time-at-risk adjustment from most of our primary narrative and tables—given its exceptionally marginal substantive import along with the concerns raised above surrounding the appropriateness of such an adjustment. However, we decided to enable readers to evaluate drug court impacts both ways, at the least, by inserting adjusted differences between drug court and comparison offenders in clearly marked footnotes under the appropriate tables.

Alternatives to Weighting and Hierarchical Modeling

We investigated the impact on our core outcomes of employing five alternative methods for addressing selection and site-level biases:

- *Unadjusted*: Impacts were computed in a standard regression framework—not in HLM—on the original unweighted data.
- *Weighted*: Impacts were computed in a standard regression framework—not in HLM—but the data were weighted using the weights designed for 18-month outcomes.
- *Hierarchical modeling*: Impacts were computed in HLM on unweighted data.
- *Weighted and hierarchical modeling*: Impacts were computed in HLM, and the data were weighted using the weights that were designed for 18-month outcomes. *This is our preferred model specification and the one employed in all analytic chapters that address straightforward "do they work" questions.*
- *Weighted and hierarchical modeling and individual covariates*: This specification is the same as the previous one (starred), but to enhance the control for any possible selection bias, we added a standard set of individual offender baseline characteristics as Level 1 control variables. The control variables were all drawn from the baseline offender survey and were identical to the variables delineated just above in the previous subsection.

Table 4-A.8. The Impact of Time at Risk Adjustment on Drug Use and Recidivism Impacts: Regression Coefficients for Impact of Drug Court Versus Comparison Group Membership

	N	Model 1: Not Adjusted for Days Incarcerated	Model 2: Adjusted for Days Incarcerated
Drug Use Outcomes			
Six Months Prior to Six-Month Survey			
Days of drug use per month	1,533	-.884**	-.808***
Days of serious drug use per month	1,533	-.758**	-.554*
Year Prior to 18-Month Survey			
Days of drug use per month	1,474	-.808***	-.716***
Days of serious drug use per month	1,474	-.762***	-.697**
Criminal Behavior Outcomes			
Six Months Prior to Six-Month Survey			
Number of criminal acts per year	1,533	-.931***	-.939***
Number of drug-related criminal acts per year	1,533	-1.035***	-.989***
Year Prior to 18-Month Survey			
Number of criminal acts per year	1,474	-.768**	-.700***
Number of drug-related criminal acts per year	1,474	-1.034***	-.920***
Complete 18-Month Tracking Period			
Number of criminal acts per year	1,349	-.683**	-.821***
Number of drug-related criminal acts per year	1,349	-.881***	-.952***
Official Re-Arrest Outcomes			
Official Re-Arrests over 24-Month Tracking Period			
Number of re-arrests per year	1,458	-.245	-.335+
Number of drug-related re-arrests per year	1,458	-.177	-.159

Note: Poisson models were run for each of these models in HLM 6.04. Weighted data utilized the super weights that were designed for cases retained at 18 months, except in the case of official re-arrest outcomes, which used the special super weights designed for official records data. "Serious" drug use was defined to include all drugs except marijuana and non-heavy use of alcohol. (Heavy use is at least four drinks per day for women and at least five drinks per day for men, see Table 4-A.1, footnote 4.)
*+p<.10, *p<.05, **p<.01, ***p<.001.*

For one or two core outcomes from each domain (same outcomes as those highlighted in Table 4-A.7 above), Table 4-A.9 displays regression coefficients and significance levels for the impact drug court participation under each alternative specifications. The results are largely self-explanatory, but broad themes are summarized as follows.

Impact of Weighting

As shown in comparing Model 1 to Model 2, and again in comparing Model 3 to Model 4, weighting has little substantive impact on drug use, criminal behavior, and mental health outcomes. However, weighting weakens the drug court impact on official re-arrests and substantially weakens the impact (causing a reversal of direction) on days sentenced to custody on the precipitating criminal case. In both of these two cases, the corrective effect of weighting likely results in particular from controlling for official criminal history, which is more extensive among those in the drug court than the comparison sample. Weighting also weakens the impact (and its significance) on socioeconomic outcomes, weakens the impact on homelessness (though all effects remain non-significant), and slightly strengthens the impact on family emotional support. Again, these latter effects are mostly a logical and fully desirable result of our effort to use weighting to adjust for selection bias. For instance, at baseline, the drug court sample has a higher socioeconomic status on most measures, so after controlling for those differences, it makes sense that the detected impact on socioeconomic outcomes would weaken. In short, the results in Table 4-A.9 broadly justify the use of weighting.

Impact of Hierarchical Modeling

As shown in comparing Model 1 to Model 3, or Model 2 to Model 4, hierarchical modeling consistently weakens our statistical power to detect significant effects. Nonetheless, there is ultimately little change in the statistical significance of the detected drug court impacts on drug use and criminal behavior. As for other outcomes, the impact of hierarchical modeling is quite dramatic. In comparing Model 2 to Model 4, the drug court impact on days sentenced to custody on the precipitating case shifts from significant to non-significant; the impact on employment/school status shifts from suggestive ($p < .10$) to non-significant; the impact on income shifts from significant to non-significant; and the impact on both family relationship measures weakens in significance. These results are unsurprising, since by definition, hierarchical modeling serves to adjust the degrees of freedom to the much smaller total number of sites (29) than of offenders. As we discussed previously, hierarchical modeling therefore poses a tradeoff: a more conservative and rigorous response to the reality of site-level clustering, leading to a reduced risk of Type I errors, but an increased risk of Type II errors. Table 4-A.9 provides several clear examples of this practical effect.

Table 4-A.9. Estimating the Impact of Drug Court vs. Comparison Group Membership on Select Outcomes: Regression Coefficients for Five Alternative Model Specifications (N = 1,474 except where specified)

	Model 1	Model 2	Model 3	Model 4	Model 5
	No Adjustments	**Weighted Only**	**HLM Only**	**HLM + Weighted: Proposed Method**	**HLM + Weighted + Individual Covariates[1]**
Drug Use					
Days per month: Year prior to 18-month survey	-.623***	-.659***	-.740***	-.808***	-.782***
Positive drug test at 18-month survey (N = 1,147)	-.617***	-.542***	-.723**	-.713**	-.748**
Criminal Behavior					
Number of criminal acts: Year Prior to 18-Month Survey	-.446***	-.472***	-.642**	-.716**	-.880**
Number of re-arrests: 24 months post-enrollment (N = 1,534)	-.183	-.037	-.241	-.275	-.338
Incarceration					
Days incarcerated: 24 Months (administrative data)	-.595***	-.573***	-.635*	-.615*	-.710*
Days incarcerated: Precipitating Case (administrative data)	-.247***	.107***	.165	.237	.264
Socioeconomic Status					
Employed or in school at 18-month mark	.358**	.199+	.433+	.268	-.315
Annual income at 18-month mark	4893.907***	2779.962**	4912.215*	2867.503	3037.233
	-	-	-		
Mental Health					
Classified as depressed at the 18-month mark	-.140	-.039	-.181	-.082	-.124
Family Support and Conflict					
Family conflict index score at 18-month mark	-.160**	-.156**	-.194*	-.198*	-.218*
Family emotional support score at 18-month mark	.091*	.117**	.126	.151+	.158+
Homelessness					
Homeless at any time in year prior to 18-month survey	-.363	-.167	-.416	-.136	-.317

Table 4-A.9. Estimating the Impact of Drug Court vs. Comparison Group Membership on Select Outcomes: Regression Coefficients for Five Alternative Model Specifications (Cont'd)

Notes: Models were run using the most methodologically appropriate specification, given the distribution of the dependent variable. OLS models were run on distributions that best (if not precisely) approximated normal (income, family conflict, and family emotional support), logistic regression models were run on all dichotomous outcomes (positive drug test, employment, depressed, and homeless), and Poisson models were run on count variables with a right-skewed distribution (days of drug use per month, number of criminal acts, number of re-arrests, and days incarcerated). Models with weighting used the super weights that were originally designed for cases retained in the 18-month survey, except in the case of official re-arrest outcomes, which used the special super weights for official records outcomes. Model 1 was run in SPSS 16.0. All other models were run in HLM 6.04.
[1] Baseline control variables were as follows in Model 5: age, sex, race, high school degree, income (log of income) employment/school status, marital status, homeless status, index score for level of drug/alcohol use and criminal involvement of blood relatives, primary drug of choice, average days of drug prior to baseline, depressed, anti-social personality disorder, and narcissistic personality disorder. For the purpose of these analyses, all control variables were treated as fixed effects, with the exception of the model predicting the number of re-arrests at 24 months post-enrollment, for which test analyses detected multiple significant random effects. For that model, the following covariates were treated as random effects: sex, race, high school degree, employment/school status, marital status, homeless status, and primary drug of choice.
[2] Community-level control variables were as follows in Model 5: score on racial/ethnic diversity index, percent black, annual number of arrests per 1,000 residents, annual number of drug arrests per 1,000 residents, and the number of police officers per 1,000 residents.
*+p<.10, *p<.05, **p<.01, ***p<.001.*

Impact of Adding Multivariate Controls

As discussed previously, our super weights balance the samples on a large number of individual baseline characteristics. The question is whether, in addition to super weighting, adding specific covariates of concern—because of their hypothesized strong relationship to outcomes—produces a notable, further refinement in our estimates. In comparing Models 4 and 5, it appears that the addition of specific covariates is an unnecessary step. Only small differences are apparent in the regression coefficients for drug court status, and for not a single outcome represented does the specific significance level change (.001, .01, .05, .10, or non-significance). Accordingly, we conclude that the extra steps taken in Model 5 are substantively superfluous.

Conclusion

The results in Table 4-A.9 largely support our specific approach to weighting and hierarchical modeling. The major caveat is for readers to understand the serious practical, if methodologically conservative, ramifications on our statistical power of shifting from a standard regression to a hierarchical modeling framework (see especially Model 2 vs. Model 4).

Universal Application of 18-Month Weights on Offender Survey Outcomes

We next tested the implications of using 18-month super weights uniformly. Initially, we had planned to use specially tailored weights for each analysis: 6-month weights for 6-month outcomes, 18-month weights for 18-month outcomes, weights designed for offenders retained at both periods for outcomes that depended on data from both periods, and special oral fluids weights for outcomes from the oral fluids drug test. However, at a meeting of external substantive and technical experts held in early 2009, the methodologists who were present argued that such an approach might be unduly complex and practically unnecessary. The advisors recommended selecting one set of weights and using them throughout our impact evaluation, unless it turned out to be the case that doing so would make a real difference in what would otherwise have been our detected effects.

Testing for whether the precise weighting scheme affected our reported impacts, Table 4-A.10 reports the impact of drug court participation on select 6-month outcomes, alternatively using the 18-month and 6-month weights; and on three outcomes from the oral fluids sample, alternatively using the general 18-month and the special oral fluids weights. Of the three sets of sensitivity analyses we conducted, this one showed the least impact of method on outcomes. Across 19 tests conducted, the regression coefficients displayed in Table 4-A.10 barely changed (no changes to a regression coefficient exceeded .100). Furthermore, there were only two slight changes in significance level, neither shifting between statistical significance and non-significance: the number of criminal acts ($p < .001$ to $p < .01$ level for 18-month and 6-month weights, respectively) and positive drug test for serious drugs (non-significant to suggestive, $p < .10$, for 18-month and oral fluids weights, respectively). We concluded that it was justified to follow the recommendation of our external advisors and use a single weight adjustment throughout the report. Importantly, although we thus resolved to employ the 18-month weights universally when analyzing all offender survey outcomes, as explained previously, we used a separate set of specially designed weights for the official records outcomes.

Table 4-A.10. The Impact of Alternative Weight Computations on Drug Court Impacts: Regression Coefficients for Effect of Drug Court Participation on Select Outcomes

Six-Month Outcomes (N = 1,533)	18-Month Weights	6-Month Weights
Drug Use in Prior Six Months		
Any drug use	-.761*	-.745*
Days of drug use per month	-.884**	-.854**
Any serious drug use[1]	-.334	-.337
Days of serious drug use per month[1]	-.758**	-.730**
Criminal Behavior in Prior Six Months		
Any criminal act	-.517*	-.507*
Number of criminal acts	-.976***	-.876**
Any drug-related criminal act	-.587*	-.579*
Number of drug-related criminal acts	-1.104***	-1.013***
Days Incarcerated in Prior Six Months	-.396	-.374
Socioeconomic Status at Six-Month Mark		
Employed or in school	.047	.024
Annual income	$2,868	$3,369
Classified as Depressed as of the Six-Month Mark	-.137	-.109

(continued)

Table 4-A.10. The Impact of Alternative Weight Computations on Drug Court Impacts: Regression Coefficients for Effect of Drug Court Participation on Select Outcomes (Cont'd)

Six-Month Outcomes (N = 1,533)	**18-Month Weights**	**6-Month Weights**
Family Support and Conflict		
Family conflict index score (3-item index)	-.198*	-.203*
Family emotional support score (7-item index)	.151+	.149+
Family instrumental support (5-item index)	.080	.086
Homeless in Prior Six Months	.129	.136
Oral Fluids Test Outcomes (N = 1,147)	**18-Month Weights**	**Oral Fluids Weights**
Positive drug test for any drug	-.713**	-.771**
Positive drug test for serious drug (excl. marijuana)	-.440	-.477+
Positive drug test for marijuana	-.658+	-.692+
Positive drug test for cocaine	-.386	-.424

*+p<.10, *p<.05, **p<.01, ***p<.001.*

Note: Models were run using the most methodologically appropriate specification, given the distribution of the dependent variable. OLS models were run on distributions that approximated normal, logistic regression models on dichotomous outcomes, and Poisson models on count variables with a right-skewed distribution (additional details in note under Table A.9).

[1] See Table A.1, footnote 4 for the drugs included as "serious."

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Appendix B. Index of Scale Items and Reliabilities

Scale Items	Possible Responses	Cronbach's Alpha
Antisocial Personality Disorder[a]		
Before you were 15, did you bully or threaten other kids?	0=No 1=Yes	0.82
Before you were 15, did you start fights?		
Before you were 15, did you hurt or threaten someone with a weapon like a bat, brick, broken bottle, a knife, or gun?		
Before you were 15, did you deliberately torture someone or cause someone physical pain or suffering?		
Before you were 15, did you torture or hurt animals on purpose?		
Before you were 15, did you mug, rob, or forcibly take something from someone by threatening him or her?		
Before you were 15, did you force someone to have sex with you, get undressed, or touch you sexually?		
Before you were 15, did you set fires?		
Before you were 15, did you deliberately destroy things that weren't yours?		
Before you were 15, did you lie a lot?		
Before you were 15, did you steal or commit forgery on more than one occasion? By stealing we mean taking property when the owner was not present.		
Before you were 15, did you break into houses, other buildings, or cars?		
Before you were 15, did you lie a lot or con other people?		
Before you were 15, did you sometimes steal or shoplift things or forge someone's signature?		
Before you were 15, did you run away and stay away overnight either permanently or more than once?		
Before you were **13**, did you often stay out very late, long after the time you were supposed to be home?		
Before you were **13**, did you often skip school?		
The next two questions are about right now. Do you often find that you have to lie to get what you want?		
Do you often do things on the spur of the moment without thinking about how it will affect you or other people?		
Was there **ever** a time when you had no regular place to live?		
Since you were 15 have you been in more than one fight?		

Appendix B (Cont'd)

Scale Items	Possible Responses	Cronbach's Alpha
Since you were 15 have you hit or thrown something at your spouse or partner?		
Since you were 15 have you physically threatened or hurt anyone else?		
Have you gotten more than two speeding tickets ever in your life?		
Have you been in more than two car accidents ever in your life?		
In the past 5 years have you been unemployed for 6 months or more when you were expected to work and work was available or have you been out of school for 6 months or more when you were expected to be attending an academic program? (include academic behavior if person is a student)		
In the past 5 years, when you **were** working, did you have frequent absences that were not the result of your or your family's illness?		
In the past 5 years, have you walked off more than one job without having another one to go to?		
In the past 5 years, have you owed people money and not paid them back?		
(If R has children) In the past 5 years, have you failed to pay child support, or give money to children who depended on you?		
The next question refers to the *present* time. Do you feel that any of the things that you have done are wrong in any way?		
Attitude toward Judge Scale[b]		
The judge is knowledgeable about your case.	1=Strongly disagree	0.90
The judge knows you by name.	2=Disagree	
The judge helps you to succeed.	3=Neither disagree nor agree	
The judge emphasizes the importance of drug and alcohol treatment.	4=Agree	
The judge is intimidating or unapproachable. (reverse-coded)	5=Strongly agree	
The judge remembers your situations and needs from hearing to hearing.		
The judge gives you a chance to tell your side of your story.		
The judge can be trusted to treat you fairly.		
The judge treats you with respect.		
Certainty of Response Scale[c]		
If Judge/Supervision Officer thought you were using drugs, how likely would they respond with:	1=Very unlikely	0.82
Increased drug testing or treatment requirements	2=Unlikely	
Increased supervision requirements	3=Likely	
Community service, writing assignment, jury box	4=Very likely	
Formal warning in writing		

Appendix B (Cont'd)

Scale Items	Possible Responses	Cronbach's Alpha
Informal verbal warning		
Electronic monitoring		
Day reporting		
House arrest / Community control		
Few days in jail		
Long time in jail or prison		
Depression[d]		
Next I will ask you about the ways you may have recently felt or behaved in the past 7 days.	0=Never in the past 7 days	0.81
You were bothered by things that don't usually bother you	1=Rarely in the past 7 days	
You had trouble keeping your mind on what you were doing.	2=Sometimes in the past 7 days	
You felt depressed.	3=Often in the past 7 days	
You felt like everything you did was an effort.	4=Always in the past 7 days	
You felt hopeful about the future.		
You felt fearful.		
Your sleep was restless.		
You were happy.		
You felt lonely.		
You could not get going.		
Perceived Consequences Scale[e]		
How bad would it be to be put on house arrest/community control	1=Not bad at all	0.84
How bad would it be to be put on electronic monitoring	2=Somewhat bad	
How bad would it be to have to do community service	3=Extremely bad	
How bad would it be to have to take drug tests more often		
How bad would it be to be put in jail for 1–3 consecutive nights		
How bad would it be to be put in jail for 4+ consecutive nights		
How bad would it be to increase your time in treatment		
How bad would it be to increase the number of AA/NA meetings required		
How bad would it be to get a warning from your Supervision Officer		
How bad would it be to get a warning from the judge		
How bad would it be to be charged with a violation of supervision (only asked of those in drug court or on supervision)		
How bad would it be to be arrested for a new charge		

Appendix B (Cont'd)

Scale Items	Possible Responses	Cronbach's Alpha
Procedural Justice Scale[f]		
You felt you had the opportunity to express your views in the court.	1=Strongly disagree	0.94
You felt too intimidated or scared to say what you really felt in the court. (reverse-coded)	2=Disagree	
People in the court spoke up on your behalf.	3=Neither disagree nor agree	
The court took account of what you said in decision what should be done.	4=Agree	
How much influence did you have over the agreement reached in the Court? (1=none at all, 2=not much, 3=some, 4=a lot) (converted from 4 to 5-point Likert scale)	5=Strongly agree	
You felt you had enough control over the way things were run in the court.		
You understood what was going on in the court.		
You understood what your rights were during the processing of the case.		
All sides had a fair chance to bring out the facts in the court.		
You felt that people who committed the same offense were treated the same way by courts.		
You were disadvantaged in the court because of your age, income, sex, race, or some other reason. (reverse-coded)		
You felt pushed around in the court case by people with more power than you. (reverse-coded)		
During the court you felt pushed into things you did not agree with. (reverse-coded)		
You were treated unfairly by the court or the police. (reverse-coded)		
People were polite to you in court.		
You feel that you were treated with respect in the court.		
How much did you feel the court respected your rights? (1=none at all, 2=not much, 3=some, 4=a lot) (converted from 4 to 5-point Likert scale)		
The court got the facts wrong. (reverse-coded)		
You were treated unfairly by the court or the police. (reverse-coded)		
Treatment Motivation Scale[g]		
Please listen to the following statements and indicate the answer that best describes you or the way you have been feeling **in the past 30 days.** Please tell me if these statements never, rarely, sometimes, often, or always apply:	1=Never felt that way in the past 30 days	0.77
Problem-Recognition (mean of 9 items)	2=Rarely felt that way in the past 30 days	
Your drug or alcohol use has been a problem for you.	3=Sometimes that way in the past 30 days	

Appendix B (Cont'd)

Scale Items	Possible Responses	Cronbach's Alpha
Your drug or alcohol use has been more trouble than it is worth.	4=Often felt that way in the past 30 days 5=Always felt that way in the past 30 days	
Your drug or alcohol use has been causing problems with the law.		
Your drug or alcohol use has been causing problems in thinking or doing your work.		
Your drug or alcohol use has been causing problems with family or friends.		
(If person NOT incarcerated for the whole six months before baseline or time since last interview) Your drug or alcohol use has been causing problems finding or keeping a job.		
Your drug or alcohol use has been causing problems with your health.		
Your drug or alcohol use has been making your life worse and worse.		
Your drug or alcohol use is going to cause your death if you do not quit soon.		
Desire for Help (mean of 7 items, 1 reverse coded)		
You need help in dealing with your drug or alcohol use.		
It is urgent that you find help immediately for your drug or alcohol use.		
You are tired of the problems caused by drugs or alcohol.		
You will give up your friends and hangouts to solve your drug or alcohol problems.		
You can quit using drugs or alcohol without any help.		
Your life has gone out of control.		
You want to get your life straightened out.		
Treatment Readiness (mean of 6 items, 3 reverse coded)		
You have too many outside responsibilities now to be in a treatment program.		
Treatment programs seem too demanding for you.		
Treatment may be your last chance to solve your drug or alcohol problems.		
Treatment programs will not be very helpful to you.		
Treatment programs can really help you.		
You want to be in a drug or alcohol treatment program.		
External Pressure (mean of 5 items)		
(If person NOT incarcerated for the whole six months before baseline or time since last interview) You could be sent to jail or prison if you are not in treatment.		

Appendix B (Cont'd)

Scale Items	Possible Responses	Cronbach's Alpha
You feel a lot of pressure to be in treatment.		
You have legal problems that require you to be in treatment.		
You are concerned about legal problems.		
You have family members who want you to be in treatment.		
Treatment Eagerness Scale[h]		
Please listen to the following statements and indicate the answer that best describes you or the way you have been feeling **in the past 30 days.** Please tell me if these statements never, rarely, sometimes, often, or always apply:	1=Never felt that way in the past 30 days 2=Rarely felt that way in the past 30 days 3=Sometimes that way in the past 30 days 4=Often felt that way in the past 30 days 5=Always felt that way in the past 30 days	0.77
Problem-Recognition (mean of 7 items)		
You really want to make changes in your drinking or drug use.		
If you don't change your drinking or drug use soon, your problems are going to get worse.		
You are a problem drinker		
You have serious problems with drinking or drug use		
Your drinking or drug use is causing a lot of harm.		
You know that you have a drinking or drug problem		
You are an alcoholic or addict		
Ambivalence (mean of 4 items)		
Sometimes you wonder if you are an alcoholic or drug addict.		
Sometimes you wonder if your drinking or drug use is hurting other people.		
Sometimes you wonder if you are in control of your drinking or drug use.		
There are times when you wonder if you drink or use too much.		
Taking Steps (mean of 8 items)		
You have already started making some changes in your drinking or drug use.		
You were drinking or using too much at one time, but you've managed to change your drinking or drug habits.		
You're not just thinking about changing your drinking or drug habit, you're already doing something about it.		
You have already changed your drinking or drug use, and you are looking for ways to keep from slipping back into your old pattern.		
You are actively doing things now to cut down or stop drinking or using drugs.		

Appendix B (Cont'd)

Scale Items	Possible Responses	Cronbach's Alpha
You want help to keep from going back to the drinking or drug problems that you had before.		
You are working hard to change your drinking or drug use.		
You have made some changes to your drinking or drug habits, and you want some help to keep from going back to the way you used to drink or use drugs		

[a] Based exactly on DSM-IV criteria for Antisocial Personality Disorder. A subject is considered to have met the criteria for antisocial personality disorder if s/he has shown evidence of Conduct Disorder with onset before 15 years of age, has shown a pervasive pattern of disregard for and violation of the rights of others since the age of 15, is over the age of 18, and the occurrence of antisocial behavior is not exclusively during the course of a Schizophrenic or Manic Episode (DSM-IV, p. 649-650).

[b] Adapted from Florida Drug Court (RTI) and Hirst A. and A. Harrell. Measuring Perceptions of Procedural Justice Among Court-Monitored Offenders. Paper presented at American Society of Criminology 52nd Annual Meeting, San Francisco, CA, Nov. 17, 2000.

[c] Developed for the MADCE study.

[d] Taken without modification from Andresen E.M., J.A. Malmgren, W.B. Carter, and D.L. Patrick. Screening for Depression in Well Older Adults: Evaluation of a Short Form of the CES-D (Center for Epidemiologic Studies Depression Scale). American Journal of Preventive Medicine. 1994; 10: 77–84.

[e] Developed for the MADCE study.

[f] Adapted from Sherman L.W., J. Braithwaite, H. Strang, and G.C. Barnes. Reintegrative Shaming Experiments (RISE) in Australia, 1995-1999 [Computer file]. ICPSR version. College Park, MD: University of Maryland [producer], 2000. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2001.

[g] Adapted from the four subscales of the TCU Treatment Motivation scale. Knight K., M. Holcom, and D.D. Simpson. (February, 1994). TCU Psychosocial Functioning and Motivation Scales: Manual on Psychometric Properties. Fort Worth: Texas Christian University, Institute of Behavioral Research.

[h] Adapted from Miller W.R. and J. S. Tonigan. (1996). Assessing Drinker's Motivation for Change: The Stages of Change Readiness and Treatment Eagerness Scale (SOCRATES). Psychology of Addictive Behaviors, 10(2), 81-89.

Appendix C. Generalized Propensity Score Weighting

When using observational data for inferring causal effects—e.g., inferring program treatment effects—a common problem is that of endogeneity. The main treatment indicator (or treatment dose indicator) is confounded with observed or unobserved attributes. As a result, the effects of treatment on the outcomes of interest are said to be biased. For example, individuals who are thought to be more at risk of recidivism may require more treatment. Unless adequate controls are introduced to capture the effects of "risk," an analysis comparing actual criminal behavior and program participation will lead to incorrect inferences.

A common approach to mitigating the effects of confounding variables in recovering effect sizes is to utilize propensity scores (Rosenbaum and Rubin 1983, Rosenbaum 1995). Propensity scores are commonly used in one of two ways, to:

- Match individuals (or records) from the treated and the untreated group.

- Create weights which, when applied to the data, provide a means of recovering the unbiased treatment effects.

There is a rich literature on the application of propensity scores in the setting where there are binary treatment indicators (Imbens and Wooldridge 2009, Wooldridge 2002). In general, propensity scores are used in this literature to balance the treatment and control groups on all observable attributes. One of the key advantages of this approach is that it permits the analyst to save valuable degrees of freedom. For instance, if an analysis is conducted on a sample of 500 observations with 20 to 30 attributes that need to be balanced, a standard multivariate regression analysis would eat up valuable degrees of freedom. Under the propensity score balancing approach, the propensity score—used either as an additional attribute or used to generate inverse probability of treatment weights—would permit the analysis to proceed.

A recent development of this literature is the application of propensity score balancing techniques to the case of multi-valued or continuous treatment indicators (Imbens 2000, Foster 2003, Hirano and Imbens 2004). In this setting, the propensity score is termed a Generalized Propensity Score (GPS). For example, the GPS approach allows the analyst to assess the effects of treatment dosage on outcomes of interest in much the same way as traditional propensity score methods support analyses of the effects of binary treatment on these outcomes. However, the task of assessing the extent to which balance has been achieved on observable attributes is much harder in GPS applications than in traditional propensity score applications.

In this report, we apply the GPS methodology to balance key observables across drug court treatment dosage measures converted into three categories—low, medium, or high. We estimate the GPS and use it to create an inverse probability of treatment weight that is then used to weight the data for the final analysis.

Assume that the treatment dose is defined as z_{ij} where $j = 1,2,3$ denote the three dosage categories (low, medium, or high) and $i = 1, \ldots, n$ denote individuals in the sample. Let x_{ik} denote the $k = 1, \ldots, K$ observable attributes that need to be balanced across the three categories and let y_i denote the outcome of interest. The analyst is interested in assessing the effects of z on y, while controlling for the confounding effects of the x variables. The steps used for developing these estimates are enumerated below:

1. The first step is to develop a generalized propensity score—the probability that an individual with attribute set $x_i = x_{i1}, x_{i2}, \ldots, x_{iK}$ will receive low, medium or high treatment dosage. Let this probability be defined as $e_{ij} = e_j(x_i)$. The probability may be estimated using fully parametric methods—i.e., multinomial logit model—or non-parametric methods. The traditional propensity score literature suggests that the final analysis is less sensitive to the method used for developing the propensity score than to the inclusiveness of the relevant attributes in the model. In this report, we have used a multinomial logit model to estimate the propensity scores. As a result the propensity score is defined as:

$$e_{ij} = \frac{\exp(\sum_k x_{ik}\beta_{kj})}{\sum_j \exp(\sum_k x_{ik}\beta_{kj})}$$

2. The next step is to develop the inverse probability of treatment weights for each of the individuals in the sample. These weights are based on the probability of receiving the dose level that they actually received. Therefore, the weights are based on the following calculations:

$$w_i^* = \frac{1}{\sum_j z_{ij} e_{ij}}$$

Where since only one of the treatment doses will equal 1 the weight will be the inverse of the propensity of receiving the dosage currently received.

3. The weights are next normalized by dividing by their mean. This ensures that the weights will, in fact, sum to the original sample size.

$$w_i = N \times \frac{w_i^*}{\sum_i w_i^*}$$

4. Finally, the weights are applied to the data. The data are first assessed to ensure that the weights do in fact balance the attributes across the three categories. If they do, then they are applied to the final analysis of z on y, and causal inferences are derived. If the weights do not completely balance one or more attributes then, in addition to the weights, the unbalanced attributes are included in the final analysis as additional controls. This ability to ensure unconfoundedness of the treatment indicators by using two methods—indirectly via the weights, and directly via controlling in a multivariate setting—is termed the double-robustness property.

5. Assessing the balance becomes more complicated when using GPS. In this report, we utilize a multivariate logit model before and after applying the GPS-based weights. In addition, we estimate the effects of each attribute on the dose categories one attribute at a time—in a bivariate analysis. To ensure that the weighting has indeed rendered the attribute incapable of contributing to the model, we assess the model χ^2 values for each model.

6. Finally, since all the outcomes (drug use and crime) are count measures, we use a negative binomial regression framework to assess the effects of the treatment dose on the outcome of interest.

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Appendix D. Constructing the Net Benefits Variable

As discussed in the Methods section in Chapter 3 of Volume 1, our goal is to create a single measure of social impacts, termed net benefits to society, to conduct multilevel analyses to determine the effects of drug court on social welfare. This Appendix discusses the creation of that individual-level variable. For further details on the analysis, we refer the reader to the Methods section.

To structure the analysis, individuals' impacts on society were divided into the following categories and sub-categories. This stratification is only a conceptual tool—since prices and quantities will be aggregated across all components to determine each individual's total impacts on society, it is unimportant whether a particular item is improperly categorized. Table 4-D.1 displays all categories, sub-categories, and the impacts considered. Each is discussed in turn. We note again that all of the calculations described below were performed for all observations in our sample; thus, the net benefits for the drug court group and the comparison group were calculated in an identical manner.

Table 4-D.1. Components of Net Benefits

Category	Sub-Category	Impacts
1. Social productivity	A. Employment B. Education C. Services and Support Provided	Earnings Schooling Child support payments, community service
2. Criminal justice system	A. Monitoring B. Police C. Courts D. Corrections E. Drug court	Probation officer meetings, drug tests, electronic monitoring Arrests Hearings Jail and prison (sanctions or otherwise) Case management, administrative costs
3. Crime and victimization		Crimes committed
4. Service use	A. Drug treatment B. Medical treatment C. Mental health treatment D. Other	ER, detoxification, residential care, outpatient, methadone Non-drug related hospital stays Non-drug related stays in mental health facilities Halfway houses, public housing, homeless shelters
5. Financial support use	A. Government B. Other	Welfare, disability, and other entitlements Money from family and friends

Social Productivity

1A. Employment/Earnings

Several survey variables ask about employment and we used these variables to estimate earnings. The foundational question is: "Have you been employed the whole time since the last interview?" If the answer was "yes," we simply multiplied last month's earnings, obtained from another question, by the number of months since the last interview.

When the answer was "no," we measured how many weeks the respondent reported working since the last interview. If this answer is not zero, indicating that the respondent had worked some, but not all of the time since the last interview, an assumption must be made as to whether (1) finding employment took the respondent some time since the last interview, but once employment was secured, it was steady; or (2) the respondent has been continuously, but inconsistently employed since the last interview. To inform this assumption, we examined the individual's response to the prior survey wave to determine whether the respondent reported being employed at that time.

If the respondent was not employed at the time, but has been employed since, we assume that last month's earnings, reported in the survey, are typical of earnings during months when the respondent worked. Last month's earnings are divided by 4.3 to determine weekly earnings, and then those earnings are multiplied by the number of weeks the respondent reported working since the last interview.

If the respondent was employed at the time of the last interview, but had not been employed the entire time since, we checked whether the respondent reported any earnings in the last month. If they did, we assumed that it was a typical month, and multiplied earnings by the number of months since the last interview. Thus, we assume that the last month's work patterns were the same as previous months.[59] If there were no earnings last month, or the respondent reported no longer being employed, then we cannot assume that last month was typical, as we know that the respondent had worked some since the last interview. In this case, we used the number of weeks reported working and the number of hours worked in a typical week to develop the number of hours worked since the last interview. We used two methods for valuing these hours (that is, two methods of estimating wages), and conducted an analysis to determine how sensitive our results were to the method.

First, we obtained the effective minimum wage from each state during each year that the survey was conducted.[60] These figures were obtained from a historical table on state wage laws compiled by the Wage and Hour Division of the US Department of Labor. As an alternative, we obtained data from the US Census Bureau's Current Population Survey (CPS), a nationally representative survey conducted each month.[61] Using data from each month during which the survey was conducted, we constructed a dataset of all employees paid hourly who reported their wages (N=296,076). Because of the large volume of the data, we were able to divide the full

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[59] This is not the same as the previous assumption that the respondent was employed all of the prior month. That assumption led us to calculate weekly earnings, and multiply that figure by weeks of employment. This method, on the other hand, assumes that the respondent was not employed all of last month, so last month cannot be used to calculated weekly earnings. Instead, we must assume that last month's patchy earnings were the same as in previous months. The second assumption will necessarily lead to higher estimates than the first.

[60] The effective minimum wage refers to the higher of two applicable minimum wages: that of the state and the federal minimum wage.

[61] All CPS data was accessed through Data Ferret, a free program available on the Census Bureau's website.

sample into a subsample for each site.[62] Then, for each site, we regressed the real wage, adjusted to 2008 dollars, on age,[63] race and ethnicity, gender, and level of education—variables that we were able to match to our survey data.

The results of the regression are displayed in Table 4-D.2, below. Together, the coefficients make an equation to predict an hourly employee's average wage, given his or her age, race and ethnicity, gender, and education. To determine the wage of our respondents, we simply put their information into the equation, and generated their expected labor market wage.

The minimum wage is probably an underestimate of earnings, given that only 2.2 percent of hourly workers earn the federal minimum wage (BLS 2007). The average wage is likely an overestimate, given our sample's history of crime and drug use, which cannot be controlled for using the CPS data. We conducted a sensitivity analysis using average wage as an upper bound and minimum wage as a lower bound and report the entire confidence interval.

1B. Education

An extensive review of the economics of education literature by Lange and Topel (2006) indicates that there is no strong evidence for social benefits of education, itself. Instead, educational benefits tend to manifest in the form of reduced crime and improved employment outcomes. Since we explicitly account for these outcomes, including them in a valuation of education would be double counting. Wider benefits to aggregate economic growth, national output, or income distribution have not been sufficiently demonstrated. Thus, we chose to exclude educational outcomes from the cost-benefit calculation.

1C. Service and Support Provided

The service and support provided sub-category included community service and child support payments. We also considered court fees and fines; however, the data were not adequate to responsibly estimate these.

Community Service

Survey data indicated how many times each individual was assigned to do community service. To determine the amount of community service performed, we turned to the CPS data, and made use of the Annual Volunteering Supplement. Using all available data, 2002-2008, we restricted our focus to those who were ordered by the court to volunteer (N=230). Unfortunately, our survey contains data on the number of times individuals volunteer, while the CPS data contains the number of volunteering hours per volunteering week. We therefore assumed that most individuals court ordered to volunteer do so only once per week. If individuals volunteered

[62] Sites with larger populations had more responses. In all sites, enough observations were available for reliable estimates. N ranged from 5,350 in Chicago to 177 in Olympia. The average N was 1,655. The sample size was less than 400 in only two sites.
[63] Following the standard practice in labor economics, we used a linear and a quadratic term for age. This allowed the data to conform to the well-documented pattern where wages rise with age before peaking and eventually falling. In all sites, our estimated coefficients conformed to this expectation.

Table 4-D.2. Estimation of Average Wage

Site	Inter-cept Term	Male	Age	Age Squared	Race					Highest Attained Level of Education							
					White	Black	Indian	Asian	Hispanic	6 or Less	7–9	10–12	HS Grad or GED	Some College No Deg	Assoc Deg	Bach Deg	Advanced Deg
Osceola County Drug Court	0.20	1.64	0.577	-0.0060	0.00	-1.25	-0.80	-0.27	-0.97	-1.50	-0.65	-0.97	0.00	1.48	2.51	2.94	9.81
Volusia County Adult Drug Court Program	-2.38	2.01	0.682	-0.0071	0.00	-4.04	-4.04	-0.50	-1.75	-0.07	0.37	-0.80	0.00	0.95	3.66	6.83	4.58
Fulton County	0.67	2.17	0.584	-0.0059	0.00	-1.74	0.78	-3.43	-2.53	-1.86	-1.36	-1.85	0.00	0.64	2.18	4.15	10.71
Hall County Drug Court	-0.28	2.60	0.581	-0.0056	0.00	-2.00	-2.07	-4.39	-1.70	-2.51	-2.77	-1.95	0.00	0.61	1.45	5.90	8.72
Rehabilitation Alternative Program (R.A.P.)	-3.88	2.97	0.800	-0.0080	0.00	-2.39	0.67	-1.35	-1.53	-3.52	-3.09	-1.22	0.00	1.48	1.04	3.56	8.64
Kane County Rehabilitation Court	-3.88	2.97	0.800	-0.0080	0.00	-2.39	0.67	-1.35	-1.53	-3.52	-3.09	-1.22	0.00	1.48	1.04	3.56	8.64
Auburn Drug and Alcohol Treatment Court	-2.96	3.10	0.639	-0.0064	0.00	0.36	0.80	-2.31	1.05	-0.59	-1.79	-0.60	0.00	1.38	3.06	2.98	7.60
Lackawanna City Drug Court	-2.96	3.10	0.639	-0.0064	0.00	0.36	0.80	-2.31	1.05	-0.59	-1.79	-0.60	0.00	1.38	3.06	2.98	7.60
Batavia City Drug Treatment Court	-2.96	3.10	0.639	-0.0064	0.00	0.36	0.80	-2.31	1.05	-0.59	-1.79	-0.60	0.00	1.38	3.06	2.98	7.60
City of Niagara Falls Drug Treatment Court	-7.12	1.95	0.938	-0.0097	0.00	-2.59	1.88	-4.76	-2.15	-3.73	-1.73	-0.87	0.00	1.03	1.59	3.31	8.47
Syracuse Community Treatment Court	-2.96	3.10	0.639	-0.0064	0.00	0.36	0.80	-2.31	1.05	-0.59	-1.79	-0.60	0.00	1.38	3.06	2.98	7.60
Finger Lakes Drug Court	-3.98	1.35	0.817	-0.0083	0.00	-1.43	-3.28	-2.34	-1.47	-3.19	-0.87	-1.86	0.00	0.55	1.80	3.31	11.27
Finger Lakes Drug Court, felony division	-3.98	1.35	0.817	-0.0083	0.00	-1.43	-3.28	-2.34	-1.47	-3.19	-0.87	-1.86	0.00	0.55	1.80	3.31	11.27

Wayne County Drug Treatment Court	-3.98	1.35	0.817	-0.0083	0.00	-1.43	-3.28	-2.34	-1.47	-3.19	-0.87	-1.86	0.00	0.55	1.80	3.31	11.27
Chester County Drug Court Program	-3.23	2.66	0.801	-0.0081	0.00	-2.26	-0.32	-0.99	-1.62	-3.60	-2.48	-2.29	0.00	1.02	2.04	5.84	8.70
Philadelphia Treatment Court	-3.23	2.66	0.801	-0.0081	0.00	-2.26	-0.32	-0.99	-1.62	-3.60	-2.48	-2.29	0.00	1.02	2.04	5.84	8.70
York County Drug Treatment Court	-2.04	2.56	0.705	-0.0072	0.00	-1.93	-2.66	-0.36	-1.19	-2.89	-2.06	-1.37	0.00	0.30	2.76	4.32	11.05
Seattle Drug Court	-7.33	2.96	1.043	-0.0109	0.00	-2.41	-0.47	-1.86	-0.83	-6.87	-1.15	-0.46	0.00	0.80	2.48	5.60	7.70
Kitsap County Adult Drug Court	-5.78	3.25	0.870	-0.0088	0.00	-1.52	-0.61	-4.09	3.24	0.00	0.25	-0.47	0.00	1.70	2.79	7.95	4.28
Pierce Felony Drug Court	-7.33	2.96	1.043	-0.0109	0.00	-2.41	-0.47	-1.86	-0.83	-6.87	-1.15	-0.46	0.00	0.80	2.48	5.60	7.70
CHART Court	-5.78	3.25	0.870	-0.0088	0.00	-1.52	-0.61	-4.09	3.24	0.00	0.25	-0.47	0.00	1.70	2.79	7.95	4.28
Thurston County Drug Court	-6.36	2.84	0.915	-0.0088	0.00	-2.58	-2.81	-1.73	-0.34	0.00	2.01	1.73	0.00	0.98	-0.97	4.30	11.43
King County Drug Court	-7.33	2.96	1.043	-0.0109	0.00	-2.41	-0.47	-1.86	-0.83	-6.87	-1.15	-0.46	0.00	0.80	2.48	5.60	7.70
H.S.A	0.20	1.64	0.577	-0.0060	0.00	-1.25	-0.80	-0.27	-0.97	-1.50	-0.65	-0.97	0.00	1.48	2.51	2.94	9.81
Stewart Marchman	-2.38	2.01	0.682	-0.0071	0.00	-4.04	-4.04	-0.50	-1.75	-0.07	0.37	-0.80	0.00	0.95	3.66	6.83	4.58
IL TASC	-3.88	2.97	0.800	-0.0080	0.00	-2.39	0.67	-1.35	-1.53	-3.52	-3.09	-1.22	0.00	1.48	1.04	3.56	8.64
NC Probation	-2.40	2.14	0.761	-0.0081	0.00	-1.96	2.00	2.80	-1.54	-2.92	-3.09	-2.81	0.00	0.43	2.98	4.21	13.79
Pierce County TASC/ DOSA– Breaking The Cycle	-7.33	2.96	1.043	-0.0109	0.00	-2.41	-0.47	-1.86	-0.83	-6.87	-1.15	-0.46	0.00	0.80	2.48	5.60	7.70

multiple times per week, hours per volunteering session will be biased upward. To better account for this assumption, we eliminated from the CPS sample individuals who volunteered more than 24 hours (obviously more than one session), and used the median number of hours, rather than the mean. We estimated that each court-ordered volunteering session lasts seven hours.

To determine the extent to which volunteer work comprises a social contribution, we follow the method used by Jastrzab, Masker, et al. (1996) to estimate the costs and benefits of the American Conservation and Youth Service Corps. Using surveys of organizations that use volunteer services, they found that 24.6 percent of project sponsors reported that none of the work would have been done without those particular volunteers. Another 55.9 percent reported that only some of the work would have been done. We assume that for these cases, half the work would have been done, and thus, volunteers increase total production by 24.6 + (0.5)(55.9) = 47.6 percent of the work that they do, with the remaining 52.4 percent being work that would have been completed by other volunteers.

Using the market analogy method, well accepted in cost-benefit analysis, we value this production at each individual's calculated wage rate, following classic economic theory that an individual's wage reflects how productive an individual is in the market place. We assume that organizations using volunteers appropriately take advantage of their skills, making them equally productive in volunteering as they are in the private market.

Wages were calculated by dividing earnings, computed in 1C, by the number of reported weeks since the last interview, and the number of reported hours worked in a typical week. For individuals who had no earnings, we used the minimum wage to value community service.

Child Support Payments

Child support payments constitute a transfer of wealth and should not always be counted. However, given that our standing excluded the offender, they are appropriate here. The survey collected data whether the respondent was required to pay child support and the amount, if any, of back child support that the individual owed. We did not, however, have data on the amount of required child support payments.

To compute these values, we obtained child support schedules from each state in our sample. In all states but Florida, only the most recent schedule was available, while in Florida we were able to obtain the schedule in effect in 2006, the middle of our surveyed years. We do not expect this incongruence to bias our estimates. In Illinois and New York, child support payments are based on a flat rate of payer's monthly income, 20 and 17 percent, respectively. In the other five states, child support payments were established in a way that there was a fixed payment established for each level of monthly income, in intervals of $50.

To simplify the analysis, we elected not to match each individually reported income from the surveys with a corresponding payment amount. Instead, we developed an equation to determine payment amount from monthly income. All states have a minimum income threshold below which child support payments are a fixed amount, and a maximum threshold above which payment amounts no longer increase with income. To develop the equations, we truncated each state's data at its respective minimum and maximum thresholds, restricting our focus only to the portion where payment amount increases with income.

Then, for each of the six states with a fixed payment schedule,[64] we regressed payment amount on income, logged income, and squared income, to allow the equation to fit the data well. All values of R-squared were 0.997 or above, indicating that each state's equation is a sufficient substitute for the actual figures. For each of these six states, if the respondent's income is below the minimum or above the maximum threshold, we used the minimum or maximum payment, respectively. If income lies between the two, we use the equation. The equation and threshold values are displayed in Table 4-D.3.

Table 4-D.3. Estimation of Child Support Obligations

	Minimum Threshold and Payment		Maximum Threshold and Payment		Regression Results for Other Payment			
State	Min. Income	Min. Pay	Max. Income	Max. Pay	Constant	Income	Squared Income	Log Income
Florida	650	74	10,000	1,437	-260.455	0.238	-1.04E-05	36.414
Georgia	850	197	30,000	2,236	-1362.12	0.069	-8.43E-07	222.701
Pennsylvania	850	50	20,000	2,301	-3105.48	-0.026	2.88E-06	482.582
North and South Carolina	1,000	50	25,000	1,819	-2374.28	0.01	4.62E-07	363.764

Notes: All payment amounts are based on net monthly income. In the analysis, participants' reported income was adjusted to net income using the Federal Income Tax Brackets.

Criminal Justice

2A. Monitoring

The survey asked about several types of monitoring. Monitoring can broadly be thought of as supervision requiring reporting to persons. Individuals under supervision were asked who the main person to whom they report is a parole or probation officer, a pretrial supervision officer, or a drug court case manager. This section considers the services of only the parole or probation officers and the pretrial supervision officers, as drug court case managers are discussed in section 2E, below.

Primary Supervision

The survey asked how many times since the last interview respondents met face-to-face or had phone contact with their primary supervision officer. Unfortunately, we do not know how long each meeting was. Following the convention in the criminal justice literature, we estimated each in-person and phone meeting to be 20 minutes in length. Estimated wages for these supervision

[64] South Carolina also had a fixed payment schedule; however, it was available only through an online calculator that required detailed input about parent characteristics. As this was not practical, we assumed that South Carolina's child support payments follow the same trend as North Carolina's, an imperfect assumption, but likely one with little substantive effect.

officers were obtained directly from the sites, where possible, through the aforementioned secondary set of interviews (Bhati, Roman, and Chalfin 2008).[65]

The salaries of probation or parole officers were unknown in a number of sites. To impute these salaries, we used a method for adjusting site-specific wages to drug court-relevant price structures. We did so using the Occupational Employment Statistics (OES) maintained by the Bureau of Labor Statistics (BLS 2009). Every 6 months, the BLS surveys about 200,000 employers from 450 different industries about the number and wages of employees falling into 800 different occupational categories. The resulting OES estimates are available for very specific occupational classifications and geographic regions.

For each Metropolitan Statistical Area (MSA) in our sample, we obtained the median wage of "Probation Officers and Correctional Treatment Specialists." Then, to account for the possibility that probation and parole officers involved in drug courts may earn different wages than general probation or parole officers, for all sites that reported their probation and parole officers' wages in the phone interview, we computed the ratio of the wage of drug court probation officers at that site to the median wage of all probation officers in that MSA. We took the average of this ratio across all available sites, and used the average ratio of drug court wages to median wages to scale the median wages by site.

This process was followed for a number of occupations discussed later. We see strong advantages to using site-specific wages, and when sites are unable to provide this information directly, we believe that adjusting wages using the average relationship between drug court wages and aggregate regional wages is a suitable approximation. To enable other researchers to use this technique, Table 4-D.4 displays the average ratio of drug court wages to all wages, for a variety of different types of employees. Future researchers can obtain site-specific wages through the OES, and use these values to scale the site-specific wages accordingly to estimate site-specific drug court wages.

Secondary Supervision

We were not able to directly observe how often participants reported to other supervision officers. Unfortunately, the survey only specifies whether respondents were supposed to meet with another type of supervision officer, not the number of meetings. We assumed that the frequency of meetings with a secondary supervision officer was half of that with the primary supervision officer. This ratio is arbitrary; however, preliminary investigation found that the results were not sensitive to the assumption. Meetings with a secondary supervision officer were valued in the same way as those with the primary supervision officer.

[65] We were concerned that this is only a reflection of average costs, rather than marginal costs upon which cost-benefit theory is built. However, the extensively well-documented caseload overburden faced by supervision officers indicates that additional cases force officers to either compromise the quality of other cases or devote additional time, beyond full-time employment, to cases. In either situation, these costs should be valued, so in this context, average costs are an accurate representation of marginal costs.

Table 4-D.4. Ratio of Drug Court Wages to Local Wages

Position	Estimate Based on N Sites	Mean Ratio	Standard Error of Mean Ratio
Prosecutors	14	0.668	0.15
Defense attorneys	10	0.622	0.214
Judges	10	1.082	0.312
Rehabilitation counselors	4	1.56	0.301
Substance abuse social workers	5	1.099	0.469
Probation officers	4	0.840	0.132
Court reporters	4	0.887	0.323
Law clerks	8	1.045	.403
Bailiffs	2	0.968	0.336

Electronic Monitoring

The other questions about criminal justice monitoring related to drug tests and electronic monitoring. These estimates were fairly straightforward to value. Only North Carolina has publicly available estimates of the cost of house arrest, so the estimates do not vary across sites. However, since the cost is nearly trivial, $7.74 per day after adjusting for inflation, this is unimportant.

Drug Tests

Estimates of the costs of drug tests were obtained from the aforementioned phone interview of drug courts in the study.[66] The survey of individuals asked only how many times the individual was tested for drugs, not what type of test was conducted. However, in order to further specify our estimates, we used responses from the MADCE Adult Drug Court Survey (see Volume 2 of this report), which asked drug court administrators what type of tests they used, choosing from saliva, urine, hair, and patch tests. For each individual in our sample, we assumed that all tests were of the type their drug court reported using. When the drug court reported using multiple types, we assumed that the drug tests the individual reported taking were evenly distributed across the multiple types the individual's court reported using.

For sites that did not have drug courts, we have no response to the MADCE Adult Drug Court Survey with which to estimate what type of tests those individuals took. We assumed that non-drug courts have the same preferences for drug test type as drug courts, and used the total responses from the Adult Drug Court Survey to predict that 13.8 percent of courts test saliva, 96.3 percent test urine, 8.2 percent test hair, and 14.6 percent use patch tests. Thus, for each individual, whether in the drug court or not, we have reported number of drug tests taken and estimated number of each of these four types taken.

[66] In addition to drug testing, the survey also asked about the number of times respondents received a breathalyzer test, but because breathalyzer systems carry only fixed costs and no marginal costs, they were excluded from this analysis.

We compiled drug test cost estimates from multiple sources. Patch test costs, $32.49 in 2008 dollars, were directly laid out in Henry and Clark (1999). Other cost estimates were made indirectly. Henry and Clark estimate most drug tests cost $10-$20 each. DuPont, Campbell, and Shea (2003) evaluated eight school-based drug testing programs, and reported median costs per test to be $19. Finally, the Office of National Drug Control Policy (ONDCP 2002), also discussing school-based drug testing, estimated that most drug tests cost $10-$30 each, with hair tests being somewhat higher. Therefore, we estimated the cost of a hair test, also described as more expensive by Henry and Clark, to be the higher end of this range: $30 in 2002 dollars or $35.90 after inflation adjustment. Saliva tests—supposedly more expensive than urine tests, but less expensive than hair tests—were assumed to lie in the middle of the ONDCP range, which happens to match the DuPont et al. median at $20 ($23.94 after inflation adjustment). Finally, the cost of a urine test, reported by all three sources to be less expensive than other tests, was available directly from the sites themselves. Several sites reported this cost during the phone interview. We used each site's self-reported cost where possible, and where this wasn't possible, we used the average of the self-reported costs, $4.21.

2B. Police

Police costs, based on number of arrests, were estimated from official arrest records. An important shortcoming of the criminal justice cost-benefit analysis literature is the lack of estimates pertaining to police costs. Virtually all studies use estimates from Miller, Cohen, and Rossman (1994), whether they cite the work directly or indirectly, through a later paper that uses those estimates. Investigation, arrest, and processing costs, or the components which make up those costs, such as time use, are almost nonexistent. A major priority for the research should be updating these costs, currently based on 20-year-old data.

That said, we did little to correct this omission. We did make some effort to make estimates more site-specific. We assumed that 75 percent of investigation, arrest, and processing costs are labor costs. We then customized inflation adjusted cost estimates from Miller et al. (1994) using the above mentioned Occupational Employment Statistics (OES) maintained by the BLS (2009). For each site, we obtained median wages of Police and Sherriff's Department Officers from that site's MSA for 2008. We then compared these median wages to national median wages, to develop a ratio of site-specific labor costs to national labor costs. We weighted the costs of arrest as follows:

$$Cost_i = 0.25 * Cost_N + (Wage_i/Wage_N) * 0.75 * Cost_N$$

where $Cost_i$ and $Cost_N$ are the costs of an arrested for site i and the nation as a whole, respectively, and $Wage_i$ and $Wage_N$ are the median police officer wages for site i and the nation as a whole, respectively. Wages of police officers, along with those of many other relevant criminal justice employees, are displayed in Table 4-D.5.

Table 4-D.5. Wages of Relevant Criminal Justice Employees

Site	Parole & Probation Officers	Case Man-agers	Judges	Prose-cutors	Defense Attorneys	Police Officers
Osceola County Drug Court	23.62	21.10	85.62	40.41	35.43	22.2
Volusia County Adult Drug Court Program	26.70	25.60	86.77	45.68	40.05	18.14
Fulton County	19.85	23.36	85.03	57.98	50.83	19.75
Hall County Drug Court	17.64	25.60	42.99	28.50	24.99	15.06
Rehabilitation Alternative Program (R.A.P.)	29.91	25.60	103.74	62.44	54.74	34.89
Kane County Rehabilitation Court	24.65	39.54	93.45	57.39	50.32	34.89
Auburn Drug and Alcohol Treatment Court	34.14	13.35	78.72	102.32	89.71	20.9
Lackawanna City Drug Court	27.90	25.60	86.55	45.40	39.81	20.9
Batavia City Drug Treatment Court	27.90	24.08	86.55	45.40	39.81	20.9
City of Niagara Falls Drug Treatment Court	29.52	25.60	83.87	42.19	36.99	27.9
Syracuse Community Treatment Court	32.60	27.39	91.29	44.62	39.12	23.51
Finger Lakes Drug Court	28.09	27.98	58.78	33.47	29.35	25.18
Finger Lakes Drug Court, felony division	31.58	25.60	54.43	23.42	20.54	25.18
Wayne County Drug Treatment Court	32.09	25.60	77.02	36.60	32.09	25.18
Chester County Drug Court Program	32.16	35.32	96.69	57.48	50.40	29.69
Philadelphia Treatment Court	29.52	25.60	98.03	56.66	49.68	29.51
York County Drug Treatment Court	20.04	21.23	77.02	51.24	44.93	20.78
Seattle Drug Court	26.67	25.60	73.00	50.66	44.42	32.36
Kitsap County Adult Drug Court	25.50	25.60	70.27	31.70	27.79	28.35
Pierce Felony Drug Court	23.80	25.60	67.54	41.70	36.56	29.86
CHART Court	26.67	25.60	70.27	50.66	44.42	32.36
Thurston County Drug Court	25.32	25.60	82.85	73.35	64.31	31.41
King County Drug Court	26.67	25.60	73.00	50.66	44.42	32.36
H.S.A	23.62	25.60	85.62	50.75	44.50	22.2
Stewart Marchman	26.70	25.60	86.77	33.71	29.56	18.14
IL TASC	29.91	25.60	103.74	62.44	54.74	34.89
NC Probation	20.13	25.60	80.26	49.58	43.47	15.73
Pierce County TASC/ DOSA– Breaking The Cycle	23.80	25.60	67.54	41.70	36.56	29.86

2C. Courts

The survey asked respondents how many times they were in court for any type of hearing, and how frequently they met with the judge or a lawyer outside of hearings. The information needed to value these occasions was drawn primarily from the phone interview of drug courts included in the study. Because the interview included only treatment sites and not every court responded or provided all necessary information, we imputed missing values for each court. We took all possible measures to ensure that imputed values were empirically driven and as site-specific as possible. We used a multi-tiered approach.

When needed data were missing from the phone interview, we first turned to the process evaluation, where estimates were observed during a visit by the research team. This is less preferable because the interview asked about typical operations, whereas the process evaluation is based on observations from a single point in time.

When information, such as salaries, was not included in the process evaluation, we turned to the aforementioned OES database. We used the same process explained above to adjust site specific median wages to wages relative to drug court. Again, see Table 4-D.4 for the ratio of drug court wages to median wages for various positions.

When salary information was not available through the OES, we assumed that relative salaries tend to cluster by court. For the positions of bailiff, clerk, court reporter, and court officer, we calculated the average salary from all reporting sites. For each site, we then computed the deviation of each of these positions' salaries from the mean. We created an index value for each site reflecting the average deviation from the mean of that site's non-legal courtroom positions. For example, suppose a site reported employing a bailiff, clerk, and court reporter, but provided salary information for only the bailiff and the court reporter. Suppose the bailiff is paid 10 percent more than the average bailiff from our sample, and the court reporter is paid 15 percent more than the respective average. We assumed that the clerk is paid 12.5 percent more than the average clerk in our sample, and estimated the salary accordingly.

Our final recourse, when none of these methods were viable, was to use a simple average of comparable estimates. For example, labor loading rates were used to obtain the true value of each individual's work. When labor loading rates were not available for a specific site, we calculated them as the average labor loading rate for the same position in other sites.[67] The same procedure was used to impute missing attendance rates at court hearings for various positions.

The survey also asked how often respondents met outside of court with the prosecutor, their defense attorney, and the judge. With insufficient data on the length of these meetings, we assumed that each lasted approximately five minutes. Wages of relevant parties specific to each site are presented along with those of law enforcement officers in Table 4-D.5.

2D. Corrections

The individual survey asked respondents how many days they were in jail or prison since the last interview, using the calendaring method to improve memory recall. However, self-reported responses are notoriously unreliable, due to both poor recall and dishonesty. Thus, we used official records for how many days the respondent was in jail and prison. We valued days in prison using financial records from each state's Department of Corrections. We used the Bureau of Justice Statistics' 2006 Census of Jail Facilities (BJS 2006), available through the Interuniversity Consortium for Political and Social Research (ICPSR), to calculate daily jail costs specific to each MSA.[68] Table 4-D.6 displays each site's daily prison and jail costs.

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[67] Our initial expectation was that labor loading rates would cluster in sites, not positions, but informal inspection of the data indicated that the opposite was true. Thus, looking at loading rates for the same position at other sites is more appropriate than looking at loading rates for other positions at the same site.
[68] Per diem, per capita costs are not available through the Census of Jail Facilities. We calculated them as the sum of annual wage costs, operating expenses, and capital expenses divided by the average daily population divided by 365.

For sites where official records did not include brief jail stays used as sanctions, we supplemented the administrative data with data from the individual survey questions: "How many times have you received one to three consecutive nights in jail since your last interview?" and "How many times have you received four or more consecutive nights in jail since your last interview?" Since these questions explicitly referred to court sanctions and were separate from questions about incarceration for a new offense, we did not feel that they were double counting official incarceration records. We assumed that each stay of the first type lasted 2.5 days and each stay of the second type lasted 7 days.

2E. Drug Court

The survey asked two questions specific to drug court participation and services. Because the survey asked only about "regularly scheduled" court hearings, all drug court hearings are included in section 2C. The survey did, however, importantly ask how many months since the last interview, if any, the respondent participated in drug court. The administrative costs for nearly all sites were obtained through the aforementioned phone interview. Nearly every site reported employees, even when salaries were not reported, so we could make very accurate approximations about how many full-time employees oversaw drug court administration. When salaries, details of staff meetings, and other employees were not reported, we used the techniques described above to impute this information. Administrative drug court costs are presented with daily jail and prison costs in Table 4-D.6.

The survey also asked about meetings with a drug court case manager. These questions mirrored those about supervision officers discussed in section 2A, and the same protocol was used to estimate case manager meeting time and wage, both when the case manager was the primary person to whom the respondent reported and when reporting to the case manager was assigned as a secondary responsibility. Estimated salaries of case managers are reported in Table 4-D.5.

Crime and Victimization

3A. Number of Crimes

An important impact to measure is the crimes committed by participants and the effects they have on victims. We used official data on arrests to estimate the number of crimes committed. Because we do not expect that respondents were arrested for every crime they commit, we scaled this figure up using national clearance rates (FBI 2008).[69]

For sites whose jails did not report information for the Census, we used the daily jail cost for the geographically closest county in the same state that did report to the Census.

[69] Clearance rates are the proportion of crimes reported that end in arrest. This process has been used in past criminal justice literature (Roman 2009) and specifically in drug court cost-benefit analyses (Washington State Institute for Public Policy 2003).

Table 4-D.6. Costs of Incarceration and Administrative Costs of Drug Court

Site	Daily Cost of Jail	Daily Cost of Prison	Drug Court Monthly Administrative Costs per Enrollee	Costs Per Hearing[70]	
				Regularly Scheduled Hearings	Other Hearings
Osceola County Drug Court	69.50	55.09	30.04	11.23	8.12
Volusia County Adult Drug Court Program	52.78	55.09	60.64	24.95	26.98
Fulton County	62.29	48.05	143.92	17.88	8.95
Hall County Drug Court	45.80	48.05	108.57	3.15	4.36
Rehabilitation Alternative Program (R.A.P.)	88.27	64.97	33.59	14.56	10.18
Kane County Rehabilitation Court	88.27	64.97	63.03	26.96	24.89
Auburn Drug and Alcohol Treatment Court	112.01	95.45	94.86	16.23	14.37
Lackawanna City Drug Court	112.01	95.45	11.76	9.69	9.35
Batavia City Drug Treatment Court	84.93	95.45	13.40	4.04	4.54
City of Niagara Falls Drug Treatment Court	84.93	95.45	3.48	3.01	4.46
Syracuse Community Treatment Court	119.21	95.45	39.37	2.99	5.66
Finger Lakes Drug Court	103.04	95.45	191.65	6.36	8.89
Finger Lakes Drug Court, felony division	103.04	95.45	229.07	15.41	6.83
Wayne County Drug Treatment Court	133.12	95.45	237.30	14.16	9.33
Chester County Drug Court Program	67.05	85.93	35.84	48.52	36.56
Philadelphia Treatment Court	97.44	91.77	22.14	10.48	7.13
York County Drug Treatment Court	24.83	44.34	166.60	4.45	10.42
Seattle Drug Court	79.88	97.30	18.27	14.03	17.63
Kitsap County Adult Drug Court	75.20	97.30	88.70	13.21	11.86
Pierce Felony Drug Court	88.76	97.30	17.55	17.66	25.13
CHART Court	86.77	97.30	62.52	1.15	2.53
Thurston County Drug Court	141.46	97.30	177.51	21.89	18.84
King County Drug Court	79.88	97.30	1.84	29.05	28.73
H.S.A	69.50	55.09	30.04	8.12	8.12
Stewart Marchman	52.78	55.09	60.64	26.98	26.98
IL TASC	88.27	64.97	48.31	17.53	17.53
NC Probation	95.58	73.10	166.60	10.42	10.42
Pierce County TASC/ DOSA– Breaking The Cycle	88.76	97.30	17.55	25.13	25.13

[70] Costs per hearing primarily depend on who attends the hearing. At times, drug court hearings cost more than other hearings. This is because many individuals, whose time must be valued, attend these drug court hearings who would not attend other hearings. These might include treatment providers or case managers. On the other hand, sometimes the cost of a drug court hearing is lower. This is because in some sites, prosecutors and other employees do not attend drug court hearings, but do attend other hearings. We found wide variation across sites in terms of the types of employees who attended hearings, and this accounts for wide variation in hearing costs.

3B. Price of Crime

Robust estimates of the price of criminal victimization, measured as the costs of crime to victims, inform a broad range of policy analysis and are widely applied. However, the most commonly cited studies are constrained by limited data and cannot directly estimate prices; thus, the studies cannot correct for sampling bias and do not report estimated variance in prices. A recent study (Roman 2009) combines individual and aggregate data, and analyzes individual-level data from two sources: (1) jury award and injury data from the RAND Institute of Civil Justice and (2) crime and injury data from the National Incident-Based Reporting System (NIBRS). Propensity score weights were developed to account for heterogeneity in jury awards with respect to legal claims. Data from the jury awards are interpolated onto the NIBRS data using combinations of all attributes observable in both data sets. From the combined data, estimates are developed of the price of crime to victims for 31 crime categories, and these prices of crime are used in this research. For each type of crime, we used both the mean cost and the median cost to determine the extent to which our results were sensitive to this choice.

Service Use

4A. Drug Treatment

The MADCE survey includes many drug treatment questions. From the survey, we are able to determine the number of days each month (since the last interview) that the respondent:

- Received inpatient drug or alcohol detoxification at a hospital.
- Was treated for drugs or alcohol in the emergency room.
- Participated in a residential drug treatment program.
- Received medicinal treatment (such as methadone, Naltrexone, or Buprenorphine).
- Participated in outpatient group therapy.
- Received outpatient individual counseling.

These services were valued using a number of extant sources, displayed along with the estimates of prices for each modality in Table 4-D.7. Several sites reported costs in responding to the aforementioned phone interview. For these sites, we used their self-reported costs. For the rest, however, we chose to rely on national estimates since they were invariably based on a much larger sample size than we had available from the interview.

Emergency room and hospital detoxification were valued using publicly available data from Healthcare Cost and Utilization Project (HCUP) conducted by the US Department of Health and Human Services' Agency for Healthcare Research and Quality. The HCUP combines data from state, public, and private databases into a large, nationwide dataset. From HCUPnet,[71] we obtained data on emergency room visits and inpatient hospital stays for which the primary diagnosis was drugs or alcohol, where averages were reported for drugs separately from those for

[71] Available at http://hcupnet.ahrq.gov/.

alcohol. We computed average daily cost for each as the mean cost divided by mean length of stay. For each individual who reported either emergency room treatment or hospital detoxification, we determined whether to use the value for a drug or an alcohol diagnosis by referring to the individual interview, which asked the primary drug of choice. If the response was alcohol, we assumed the visit was for alcohol. If they responded with a drug, we assumed the visit was drug-related. Lastly, if they responded that they are not currently using drugs, we valued the reported visit as the average between a drug and an alcohol visit.

Table 4-D.7. Drug Treatment Costs and Sources

Treatment Type	Source	Data Year	Sample Size	Cost	Per
Emergency room (drug)	HCUP	2006	All Hospitals	3,340	Day
Emergency room (alcohol)	HCUP	2006	All Hospitals	4,102	Day
Hospital detox (drug)	HCUP	2006	All Hospitals	2,713	Day
Hospital detox (alcohol)	HCUP	2006	All Hospitals	2,901	Day
Residential drug treatment	SAMHSA	1997–1999	48	82	Day
Medicinal treatment	SAMHSA	1997–1999	44	19	Visit
Outpatient group counseling	SAMHSA	1997–1999	215	10	Session
Outpatient individual counseling	SAMHSA	1997–1999	215	100	Session
In-prison therapeutic community	Roebuck et al.	1993–2002	8	8.43	Day

We valued the daily cost of residential drug treatment, outpatient medicinal treatment, outpatient group counseling, and outpatient individual counseling using figures reported in the Drug and Alcohol Services Study (ADSS) carried out by the Substance Abuse and Mental Health Services Agency (SAMHSA 2003). Phase II of ADSS was a cost analysis derived from interviews with directors of 280 treatment programs around the country. To approximate medicinal treatment costs, we used methadone treatment costs (methadone being the most commonly used medicinal treatment). Because methadone treatment is outpatient, we used estimates per visit, and matched these costs to the number of days respondents reported receiving the service. We also assume, based on the ADSS study's recommendation, that each individual and group counseling session lasts one hour, allowing us to match ADSS hourly counseling estimates with survey responses on number of days receiving counseling.

Finally, the survey allowed respondents to specify whether treatment was received "on the street" or while incarcerated. To our knowledge, the Drug Abuse Treatment Cost Analysis Program (DATCAP) is the only source that reports the costs of in-prison drug treatment services (Roebuck, French, and McClellan 2003). The eight in-prison treatment programs from which this average cost estimate is derived are all considered group-based residential treatment. This cost estimate is, therefore, directly used as the cost of in-prison group counseling and in-prison residential treatment. To adjust for prison cost structure, while still trying to obtain the most specific estimates possible, two cost ratios were calculated: (1) the ratio of outpatient individual counseling to outpatient group counseling and (2) the ratio of outpatient medicinal treatment to outpatient group counseling. Each ratio was multiplied by the cost of in-prison treatment to obtain cost estimates for in-prison individual counseling and in-prison medicinal treatment, respectively.

For individuals who reported that any particular month's treatment was received both on the street and while incarcerated, we took the total number of days in the month that the individual was incarcerated divided by 30 (days in a month) and used that to weight estimates. For example, if an individual specified that the treatment they received in August was received both on the street and during incarceration, and this individual was incarcerated for 10 days during August, we weighted the in-prison cost estimates by one-third and the "on the street" cost estimates by two-thirds and averaged the two.

4B. Medical Treatment

The MADCE survey also asked about overnight hospital and emergency room visits that were not for drug treatment. To value these, we again returned to the HCUP data, obtaining 2006 data on mean cost and mean length of stay for all hospital stays. We obtain average daily cost by dividing average cost by average length of stay to calculate average cost per day of overnight hospital stays.[72] Because the HCUP data are so rich, we are able to develop more specific average daily costs based jointly on whether the individual is over or under 45 and what type of health insurance, if any, the individual has.[73] The individual interview collected age and insurance information, allowing us to match these estimates of cost per day, given age and insurance, to the reported number of days spent in the hospital for non-drug related reasons. These figures are displayed in Table 4-D.8.

[72] We acknowledge that some debate exists surrounding the choice of median versus mean hospital costs, as medical expenses vary wildly and mean costs are likely much higher than typical costs. We believe that this is problem is addressed when dividing mean costs by mean length of stay, as anomalously high hospital stays likely bias both the numerator and the denominator proportionally. More generally, we feel that mean values are advantageous to policy-makers, as they take into account the small probability of extremely expensive stays, where these stays would be disregarded altogether when using median costs..

[73] Because only two-way tables were available, we were only able to simultaneously use two controls. We selected these two because they exhibited the greatest variation across groups.

Table 4-D.8. Costs of Overnight Hospital Stays by Age and Insurer

	Age 18–45	Age 46+
Medicare	1,560.46	1,929.11
Medicaid	1,554.18	1,872.39
Private	1,964.40	2,665.61
No insurance	1,854.12	2,109.36
Other	2,041.10	2,430.52

4C. Mental Health Treatment

Another survey question asked respondents how many days were spent in residential mental health treatment not related to drug or alcohol use. Because of the decentralization of the mental health system in the United States, estimates comparable to the medical care figures obtained through HCUP are not available (US Surgeon General 1999). In fact, few studies have surveyed a wide range of mental health treatment providers to develop cost estimates for the sector as a whole.

We turned to research from the Veterans Affairs Health Economics Resource Center (Barnett and Berger 2003). Using two comprehensive national databases maintained by the US Department of Veterans Affairs and a survey of hospital directors, the authors obtained direct costs (staff and supplies) and estimate indirect costs (including administrative, facilities, and general operating costs). Because this study drew research from hospitals around the country, albeit only VA Hospitals, we determined that it was the ideal source of estimates for the present analysis.[74] We used reported estimates of total direct and indirect costs. Of the available estimates, we selected those that excluded research and teaching expenses, because we believe that these expenses are unique to VA Hospitals, and not incurred by typical facilities. Our estimated cost of a single day in residential mental health treatment was $175.06.[75]

4D. Other Service Use

The key additional services used are services related to housing provision. The individual survey asked respondents to choose from a list of places where they had lived since the last interview. It then asks them to indicate in which of the selected choices they mostly lived. The survey does not specify what portion of the time since the last interview was spent in each place. We weighted responses as conservatively as was reasonable. If an individual reported living in a total of k different locations since the last interview, we assumed that the respondent spent 1/(k+1) of the time in each location, except that s/he spent 2/(k+1) of the time in the primary location reported in the next question. Thus, we assume that the respondent lived twice as long in the place they lived “most of the time” than any other place.

[74] Most other studies we were able to locate were evaluations of specific hospitals or programs and relied on small samples. The VA is one of the largest providers of mental health services in the country, and so Barnett and Berger were able to use a much larger sample.
[75] Again, in several cases, sites reported their own residential mental health facility costs. We used these when available, but used national estimates otherwise, because of the excellent sample size on which estimates are based.

Three of the seven responses are important for the cost-benefit analysis: halfway houses, homeless shelters, and public housing (or section 8 housing). Each was valued using extant literature, from a variety of sources. To estimate the daily cost of halfway houses, we used figures reported in Jody Klein-Saffran's (1995) summary of her dissertation. Adjusted for inflation, this daily cost is $49.75.

Cost estimates for homeless shelters were not as simple. We obtained estimated daily shelter costs for North Carolina from three reports submitted to the National Alliance to End Homelessness (NAEH) as 10-Year Plans to End Homelessness (Durham County, Raleigh and Wake County, and Asheville and Buncombe County). These reports are available through the online publication library of the NAEH.[76] Although most sites in our sample had submitted 10-Year Plans to End Homelessness, few of them had cost estimates, and most which were not as thorough as those from North Carolina. Therefore, we turned to a report compiled by the Lewin Group (2004) for the Corporation for Supportive Housing that provided cost estimates from nine major cities, several of which were included in our sample. Finally, we obtained an estimate of the daily shelter costs in New York City (Culhane, Metraux, and Hadley 2002).

Thus, in all, cost estimates were available for 13 different locations, including 3 of the 17 cities and 5 of the 8 states in our sample. Through the U.S. Department of Housing and Urban Development's (HUD) selection of online datasets, we then obtained data on median rent values for every MSA in the country, and restricted the dataset to only MSAs in which there was an MADCE site or for which we had a homeless shelter cost estimate. Assuming that homeless shelter costs would vary proportionally with rent values, for each site for which we did not have a specific estimate, we assumed that daily homeless shelter costs were the same as those of the site with the most similar rent values. Our results using this method are displayed in Table 4-D.9.

Finally, the individual survey allowed respondents to specify that they lived in public housing and Section 8 housing.[77] This could be done in one of two ways. Individuals could report either living mostly in their own home or mostly living in another person's home, and two following questions would ask if this was Public Housing or Section 8 housing. We chose to value only those who lived in their own home, as we presumed that others who allowed the respondent to live with them would have received subsidized housing regardless, so the individual carries no marginal cost. To value subsidized housing, we returned to HUD. Using the dataset "A Picture of Subsidized Households—2000," [78]the most recent year publicly available free of charge, we obtained the average monthly cost per unit of Public Housing and Section 8 housing for each state in our sample. These results, too, are displayed in Table 4-D.9.

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[76] Available at http://www.endhomelessness.org/section/library?keywords=&issue=20.
[77] Some would argue that Section 8 housing would be more appropriately classified as financial support; however, due to the organization of the survey, it was easier to group with services used. Again, this misclassification is trivial, as all costs and benefits across all categories are aggregated to form total net benefits.
[78] Available at http://www.huduser.org/portal/picture/query.html.

Table 4-D.9. Costs of Other Service Use

Site	Daily Costs of Homeless Shelter	Monthly Costs of Public Housing	Monthly Costs of Section 8 Housing
Osceola County Drug Court	$25.90	$354	$442
Volusia County Adult Drug Court Program	25.38	354	442
Fulton County	12.54	325	404
Hall County Drug Court	12.54	325	404
Rehabilitation Alternative Program (R.A.P.)	25.07	686	476
Kane County Rehabilitation Court	25.07	686	476
Auburn Drug and Alcohol Treatment Court	25.38	545	527
Lackawanna City Drug Court	25.38	545	527
Batavia City Drug Treatment Court	25.38	545	527
City of Niagara Falls Drug Treatment Court	21.72	545	527
Syracuse Community Treatment Court	25.38	545	527
Finger Lakes Drug Court	25.38	545	527
Finger Lakes Drug Court, felony division	25.38	545	527
Wayne County Drug Treatment Court	25.38	545	527
Chester County Drug Court Program	28.08	595	361
Philadelphia Treatment Court	28.08	595	361
York County Drug Treatment Court	25.36	359	342
Seattle Drug Court	19.38	351	460
Kitsap County Adult Drug Court	19.38	351	460
Pierce Felony Drug Court	19.38	351	460
CHART Court	19.38	351	460
Thurston County Drug Court	19.38	351	460
King County Drug Court	19.38	351	460
H.S.A	25.90	354	442
Stewart Marchman	25.38	354	442
IL TASC	25.07	686	476
NC Probation	25.36	359	342
Pierce County TASC/ DOSA– Breaking The Cycle	19.38	351	460

Financial Support Use

5A. Government Financial Support

Valuing government financial support is not necessary when the standing includes the offender, as it is merely a transfer. However, since our standing did not include the offender, it was appropriate to include and was fairly straightforward to value. The survey asked how much money respondents received from disability or another government program in the prior month. For each individual, we looked back at how much they reported receiving from disability or government programs in the month prior to the previous survey. We then estimated that each month’s financial support was a linear progression from the previous survey to the current one.

If an individual received the same amount of financial support each month until the last month, when the amount declined or the individual stopped receiving support altogether, we will be underestimating government support. If, on the other hand, the individual received a given amount or no financial support during each month from the last interview to the final month, then the individual started receiving more support, our method will overestimate financial support. With no recourse, we accept this imprecision.

5B. Other Financial Support

The survey also asked whether the individual received financial support from friends or family. Again, when the unit of standing includes the respondent, this transfer is not counted. We estimated other financial support using the same method, to which the same caveat applies, as we did for government financial support.

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Appendix E. Examination of Outliers

Seven individuals within the sample are clear outliers, most clearly visible in Figure 4-9.1. These outliers are worth discussing for three reasons. First, they may unduly affect the findings. Indeed, when these outliers were removed, the estimated net benefits of drug court participation decreased dramatically. Second, outliers illustrate the most extreme instances of our methodology. Their benefits to society are estimated in the same way as everyone else's, and resultant extreme values can highlight assumptions that may be inappropriate. Finally, outliers are, in this case, instances of extreme social costs. That is, they are rare, but important events. Recognizing and understanding these rare events demonstrates a different side of the potential social benefits or costs of drug court.

All seven of the extreme outliers belong to two of the five comparison sites, Chicago and North Carolina. According to the propensity score analysis, all of these individuals could reasonably have been expected to participate in drug court, were one available to them. Before discussing these outliers, in random order, we briefly remind readers of our approach for estimating the costs of crime. If an individual was arrested and charged with a particular offense, we assume that the individual committed the offense. Moreover, if only one in *n* of that type of offense ends in an arrest, we assume the individual is responsible for *n-1* of those offenses before being detected. Thus, the costs of crime, which are responsible for six of these seven outliers' high social costs, are driven by (1) the number and type of arrests, (2) the average cost to victims of a single incident of that offense (Roman 2009), and (3) the percentage of those offenses, nationwide, which lead to an arrest (the smaller the proportion that end in arrest, the more we assume the individual "got away with" before being caught).

The first individual is from Chicago and is the only individual whose social costs are not driven by criminal activity. This individual spent a total of seven months (217 days) in the hospital, for a total cost of $300,123. Though it is not relevant when valuing net costs to society, this individual had health insurance through Medicare. The individual accumulated a total harm to society (across all domains) of $319,650. This individual was the least likely of the seven to participate in drug court, although still could have reasonably been expected to. His/her associated propensity score was 0.40, which was higher than that of 31 percent of the comparison group.

The rest of the individuals accumulated high social costs through criminal activity. One of these individuals, on probation in North Carolina, was arrested for three charges: a drug charge, a fraud, and a robbery. Given that the average robbery leads to $279,000 in damage to the victim (Roman 2009), and that roughly five robberies occur for each robbery arrest (FBI 2008), the social costs implied by a single robbery arrest are substantial. The conviction status of these charges was unknown at the time of data collection. The social costs of victimization from this individual were $1,045,751 and the total social costs were $1,072,179. According to the propensity score analysis, this individual was more likely to participate in drug court than 72 percent of comparison group participants.

The next individual, also from Chicago, Illinois, was arrested and charged with five different assault charges (three simple, two aggravated), and convicted of four. Given that the average victimization costs of a simple assault are more than $100,000, and those for an aggravated assault exceed $283,000, even though 62 percent of simple and 55 percent of aggravated assaults end in arrest, the social costs of five assault arrests are quite large. This individual's harm to society through crime and victimization was $680,048, bringing his/her total social costs to $672,532. This individual was more likely to participate in drug court than 46 percent of individuals in the comparison group.

The fourth individual, from North Carolina, accumulated a total of $1,041,362 of harms to victims of crime and $1,048,896 of harms to society through a burglary (charge dropped), an abduction (charge dropped), and a robbery (disposition unknown at the time of data collection). To prevent estimated impacts from being driven completely by the occurrence of extremely rare events, we did not include social costs of extremely rare crimes like murder or abduction, though they are substantial.[79] Thus, rare events are prevented from dominating the analysis, and this individual's harm to society is not influenced by the abduction charge, but by the robbery and burglary charges, both of which carry substantial social costs. This individual was slightly more likely than most comparison individuals to have participated in drug court. S/he was at the 51st percentile of propensity scores.

The next individual was from Chicago, and was arrested on three charges of simple assault, leading to estimated costs of criminal victimization of $326,238, contributing to aggregate social costs of $325,165. It is important to note that all three charges were dropped. This individual was more likely to participate in drug court than 87 percent of the comparison group.

The sixth individual, from North Carolina, was arrested on four charges of assault (three simple and one aggravated). All charges were dropped. This individual could be reasonably expected to have participated in drug court, given the chance, with a propensity to participate that was higher than 32 percent of the comparison group. This individual's harms to society were $509,111, of which $489,356 was the result of criminal victimization.

The final social cost outlier was from North Carolina. This individual was more likely to participate in drug court than 74 percent of the comparison group and was arrested for a simple assault (charge dropped), four thefts (dispositions unknown), and several smaller charges with little or no social costs. These arrests resulted in an estimated $548,001 of harm to victims of crime, which contributed to a total of $580,069 of costs to society.

These individuals represent significant harms to society. To varying degrees, they were all comparable to the drug court population and could have been reasonably expected to participate in a drug court program. Though they seem like outliers in the data, particularly in the context of Figure 4-9.1, they hold important information about the probability of extreme criminal and other events in the absence of drug court.

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[79] The decision to exclude the costs of rare events like abduction and murder was made before examining the data to determine whether anyone in the sample had committed these crimes. In the full sample, there were four arrests for abduction (all in comparison sites and all dismissed), and two arrests for murder (one in a comparison site and one in a drug court, one dismissed and one with unknown disposition).

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Appendix F. Comparison of MADCE Findings with Past Research

Many of the results of this cost-benefit analysis extend beyond past drug court research. Some are counterintuitive and some are inconsistent with past research findings. For this reason, we believed that it was important to contextualize our findings by replicating past research findings and discussing some disparities. Here, we compare our findings to those of nine past cost-benefit analyses. Four of these were conducted by NPC Research, a small research organization in Portland, OR (Carey and Waller 2008; Finigan 1998; Finigan, Carey, and Cox 2007; Mackin, Lucas, et al. 2010). Two were conducted by the Urban Institute (Harrell, Cavanagh, and Roman 1999; Harrell, Mitchell, et al. 2003). The remaining three were each conducted independently (Loman 2004; Washington State Institute for Public Policy 2003; Zarkin, Cates, and Bala 2005).

Among these nine analyses, there has been substantial variation in methods and data employed. The majority have used only administrative data (Carey and Waller 2008, Finigan 1998, Finigan et al. 2007, Loman 2004, Mackin et al. 2010, WSIPP 2003, Zarkin et al. 2005). Only Harrell et al. (1999) and Harrell et al. (2003) employed self-reported data. A number of these only considered crime and continued involvement with the criminal justice system as potential benefits (Finigan et al. 2007, Harrell et al. 1999, Harrell et al. 2003, WSIPP 2003), while several do not even consider reduced crime as a benefit of the program (Carey et al. 2008, Mackin et al. 2010, Zarkin et al. 2005).[80] Only two considered other potential benefits, such as employment and welfare usage (Finigan 1998, Loman 2004). The length of follow-up also varied widely. The shortest study only considered the effects of drug court for the nine months following enrollment (Harrell et al. 2003), while the longest look at outcomes up to ten years later (Finigan 2007, Mackin 2010).

Methods employed also vary widely. Only one study used a control group based on random assignment to identify program impacts (Harrell et al. 1999). All four NPC analyses use individuals who did not participate in drug court, with little or no consideration as to why they did not participate. Zarkin et al. (2005) compare drug court participants with drug-involved offenders who were sentenced to prison instead of a diversionary program. Loman (2004) considers only drug court graduates, but compares them only to probation completers to restrict the sample to only those motivated to succeed in a criminal justice supervision program. Harrell et al. (1999) analyze two different "types" of drug courts, a treatment intensive court and a sanctions-reliant court, and compare these to outcomes among a population treated as usual. Aos and colleagues (WSIPP 2003) employ one of the most rigorous quasi-experimental designs by drawing two different comparison groups: drug-involved offenders arrested before the drug court began and drug-involved offenders arrested in neighboring counties, without drug courts. Employing both samples and a number of methodologies, Aos and colleagues (WSIPP 2003) conduct six separate analyses and meta-analyze them to produce a single impact estimate.

Regardless of the comparison group, many of the studies employed matching techniques to balance the treatment and comparison samples. These matches, however, were often based only

[80] Mackin et al. (2010) also did not consider drug treatment as a cost. Thus, the most substantial cost and benefit of drug court, according to the data used in this study, were both excluded.

on age, race, gender, number of prior arrests, and current offense (Carey et al. 2008, Finigan 1998, Mackin et al. 2010). While some others account for juvenile record (WSIPP 2003), only two were able to account for history of drug abuse and motivation to seek treatment (Loman 2004, Zarkin et al. 2005). The other studies did not use matching techniques either because they employed pre-post comparison methods (Harrell et al. 2003), random assignment (Harrell et al. 1999), or simply did not balance the samples (Finigan et al. 2007).

Thus, methodological rigor varies widely across the nine studies considered here. As a result, the impact estimates underlying the cost estimates are not strictly comparable. Many estimates of the same impact by different authors are substantially different. It is our purpose, in this appendix, to demonstrate that results generated using this data fall in the same universe as past research. That is, they are within the range of reasonably expected values. Where our estimates diverge considerably, discussion is provided in an effort to explain why. In short, this appendix seeks to validate the findings displayed in the full chapter, by contextualizing them among the larger body of drug court research.

Because different studies presented findings differently and considered different impacts, it was not possible to produce a single replication to be directly compared to each past analysis. Instead, we replicated, as closely as possible, the results presented for each analysis separately. Table 4-F.1 demonstrates four studies for which the results overlap significantly. Each row represents a different cost domain. The four columns to the right represent the four studies that are most directly comparable to one another. Each box displays the estimated mean among drug court participants, the comparison group, and the associated estimated difference between the two. The first column replicates each cost category that can be replicated using the MADCE data. Towards the bottom, for each study there is a reported difference in total net benefits between drug court participants and the comparison group: "Net Benefits (reported)." We also have displayed only the net benefits that we can attempt to replicate using data collected in this study: "Net Benefits (replicable)."[81] Finally, each column includes our closest possible replication of the reported estimate: "Net Benefits (replication)." To generate these numbers, we considered only the cost domains included in the particular study.

In general, the numbers displayed in Table 4-F.1 indicate that our data are comparable to that used in the other three studies. When considering differences of magnitude, it is important to consider that the length of follow-up for our data was only 18 months, while Finigan et al. (2007) and Mackin et al. (2010) had more than 10 years of follow-up information. Though shorter, Loman (2004) uses data outcome data drawn over the two years following program completion (for a total of approximately three years after program enrollment). Carey et al. (2008) have the most comparable follow-up period of two years. This differences explains, for instance, why savings though incarceration are higher in Finigan et al. and Mackin et al.

Administrative costs are roughly comparable to the one study which reported them. Though those costs are somewhat higher in this study, as discussed in the full chapter, administrative costs vary widely across different drug courts, so this finding is not particularly surprising. Monitoring and drug test estimates seem to be in line with other estimates.

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[81] The net benefits of drug court reported in Loman (2004), for instance, include differences in taxes paid. The current study cannot replicate this estimate because this information was not collected.

Table 4-F.1. Comparison of Results with Loman, Finigan et al., Carey and Waller, and Mackin et al.

Domain	These Data			Loman (2004)			Finigan et al. (2007)			Carey & Waller (2008)			Mackin et al. (2010)		
	Drug Court	Comparison	(Difference)	Drug Court	Comparison	(Difference)	Drug Court	Comparison	(Difference)	Drug Court	Comparison	(Difference)	Drug Court	Comparison	(Difference)
Administration	-$879	-$94	(-$785)	-567	-258	(-309)									
Supervision (during program)							-914	-1,407	(493)	-2,344	-4,763	(2,419)			
Supervision (during and after program)	-474	-193	(-281)	-82	-107	(25)	-3,183	-4,242	(1,059)	-6,056	-12,440	(6,384)	-4,382	-4,618	(236)
Monitoring and Drug Testing	-470	-123	(-347)	-860	-53	(-807)				-533	0	(-533)			
Treatment (during program)							-1,797	-2,851	(1,054)						
Treatment (during and after program)	-8,979	-4,406	(-4,573)	-2,330	-650	(-1,680)	-3,243	-4,699	(1,456)	-3,688	-1,217	(-2,471)			
Hearings	-417	-56	(-361)	-666	-313	(-353)	-797	-741	(-56)	-6,489	-5,768	(-721)	-11,735	-11,009	(-727)
Incarceration (during program)							-1,056	-1,291	(235)	-579	-7,177	(6,598)			
Incarceration (during and after program)	-2,768	-5,441	(2,673)	-3,121	-5,029	(1,907)	-12,054	-17,453	(5,400)	-17,259	-28,148	(10,889)	-44,480	-47,968	(3,488)
Re-Arrest	-44	-115	(71)				-885	-1,243	(358)	-300	-591	(291)	-1,145	-1,092	(-53)
Government Support	-2,129	-1,056	(-1,073)	-2,593	-2,874	(281)									
Earnings	17,028	17,369	(-341)	24,107	22,220	(1,888)									

Table 4-F.1. Comparison of Results with Loman, Finigan et al., Carey and Waller, and Mackin et al. (Cont'd)

Domain	These Data		Loman (2004)		Finigan et al. (2007)		Carey & Waller (2008)		Mackin et al. (2010)	
	Drug Court	Comparison	Drug Court	Comparison	Drug Court	Comparison	Drug Court	Comparison	Drug Court	Comparison
	(Difference)		(Difference)		(Difference)		(Difference)		(Difference)	
Health care	-2,556	-1,985	-2,592	-3,339						
	(-571)		(748)							
Mental Health Services	-218	-169	-63	-205						
	(-49)		(141)							
Crime	-7,111	-16,887	-634	-2,356	-23,198	-28,882				
	(9,776)		(1,722)		(5,684)					
Total Program Costs	-11,219	-4,872	-2,286	-678	-5,778	-7,835	-13,100	-18,926		
	(-6,347)		(-1,609)		(2,057)		(5,825)			
Total Program Benefits	-3,086	-12,229	-9,003	-13,803	-36,136	-47,578	-20,692	-29,238	-61,742	-64,687
	(9,143)		(4,800)		(11,442)		(8,546)		(2,944)	
Net Benefits (replicable)			-11,290	-14,481	-41,914	-55,413	-33,793	-48,164	-61,742	-64,687
			(3,191)		(13,499)		(14,371)		(2,944)	
Net Benefits (replication)			-24,647	-30,123	-19,793	-27,099	-13,152	-10,335	-3,287	-5,750
			(5,476)		(7,306)		(-2,817)		(2,463)	
Net Benefits (reported)	-14,305	-17,101	-5,964	-9,418	-45,383	-59,516	-34,896	-49,557	-61,742	-64,687
	(2,796)		(3,454)		(14,133)		(14,661)		(2,944)	

Notes: All figures presented in 2008 dollars.
Loman did not include earnings benefits when calculating total program benefits.
Loman's reported benefits also included additional tax payments, which we did not collect and thus could not replicate.
Finigan, et al., Carey and Waller., and Mackin, et al., all include costs of bookings in reported costs. We did not collect this information.

Treatment costs, however, are significantly different and warrant some discussion.[82] While Loman and Carey and Waller are roughly comparable, and found that drug court participants used three to four times as much drug treatment as comparison group members, their estimates of magnitude are considerably lower than those derived from this data. There are two plausible explanations: data source and price. Both authors used administrative data to estimate treatment use. This means that private treatment (whether prescribed by the drug court or not) may not have been included.

Additionally, the prices at which treatment use was valued may not be strictly comparable. Loman used data that reported the amount that clients paid to participate. This may miss some of the true resource cost of treatment provision if treatment providers are publicly subsidized to provide a service with important social benefits. Carey and Waller, on the other hand, report the specific prices at which treatment is valued. Though the costs of group treatment presented there are close to those used in this study, the estimated cost of individual treatment is roughly half of what was used in this study ($39 compared to $79) and the estimated costs of residential treatment ($75 per day) seem excessively low relative to the costs used here ($187 per day), reported by sites in our study ($134-$292), and expected (as residential treatment is considered one of the most costly forms of treatment).[83] Thus, differences in data sources and different prices of resource use likely explain the differences in the size of estimated treatment costs, and we gain confidence in that the ratio of participant costs to non-participant costs estimated in this study (2:1) is roughly comparable to those found in those two studies (3:1 in Carey and Waller, and 3.5:1 in Loman).

Finigan et al., however, found substantially different results than this analysis, Carey and Waller, or Loman. They actually found that drug court participants had lower treatment costs than the comparison group. This difference largely drove their finding that putting an individual through drug court is less expensive than the status-quo, ignoring long-term outcomes.[84] Closer examination reveals that this finding is inconsistent with the findings in this study and the conceptual model of drug courts. Treatment savings are largely the result of different prices per treatment episode between participants and the comparison group (an average day of treatment for a drug court participant costs roughly one-third an average day for a non-participant). However, drug court participants also, reportedly, only have access to group-based therapy (the least expensive form of treatment). Individuals not in drug court, on the other hand, have access to a far wider range of treatment options, including rigorous and intensive treatment such as residential treatment. This seems somewhat counterintuitive.

Costs of hearings and savings from avoided incarceration and arrests are all comparable across the five studies displayed in Table 4-F.1. Loman (2004) provided a more detailed analysis than most other studies, however, by collecting data on receipt of government support, earnings, and physical and mental health care costs. Findings presented in that study are almost completely different from those in the present study. Government support was slightly lower among drug

[82] Mackin et al. (2008) did not consider drug treatment as one of the costs of drug court.
[83] More detailed explanations of how we estimated the unit costs of drug treatment and all other domains considered in this analysis can be found in Appendix D.
[84] Finigan et al. report that putting an individual through drug court instead of the alternative saves $1,432 without even considering outcomes. 73 percent of these savings come in costs of treatment ($1,054).

court participants. This difference is likely fueled by the longer follow-up period used by Loman, on average, an additional year and one half. A possible explanation is that case managers increase access to public support during treatment, but as the participant is successfully integrated into society during and following the program, reliance on government support decreases, and in the long-run, welfare receipts decline. While this is a plausible explanation, consistent with past research, theory, and findings in this report that drug courts effectively reduce drug use, it cannot be explicitly tested here.

This explanation, however, is inconsistent with the finding presented here that drug court participation decreases earnings. This finding is likely spurious. The more rigorous hierarchical models of the effects of drug court on earnings presented in the full chapter produce findings very similar to those in Loman. Regardless, we draw confidence from the fact that the level of earnings are comparable with those found by Loman (even if differences between participants and non-participants are not the same), which further illustrates that the population of drug-involved offenders who enroll in drug court tend to be distinctly disadvantaged.

Health and mental health expenditures are also inconsistent between Loman and the data used in this study. These findings may be driven by the same conceptual mechanism as differences in receipt of government support. It is likely that in the short-term (this study), case managers encourage participants to access all possible services available, while in the long-term (Loman), successful case management helps participants reduce reliance on these services and move toward a more productive and independent lifestyle.

Among the most important benefits to consider, as discussed in the full chapter, are those benefits of prevented crime. Of the four studies presented in this table, only Loman and Finigan et al. consider this benefit. Our findings of crime reduction are somewhat larger than theirs, which can likely be attributed to differences in the costs of crime. As discussed elsewhere, we believe that the mean costs of crime are the most theoretically appropriate for a purpose such as this. However, if we value each crime incident using median instead of mean costs, our estimate ($3,020) is very similar to those found by others.

Finally, it is worth mentioning the row of Table 4-F.1 labeled “Net Benefits (replication).” This row represents our best efforts to replicate the findings of the other authors using our data, by calculating net benefits using the most comparable set of impacts possible. In general, this effort demonstrates that the data used in this study are sufficiently comparable to those from other studies. The estimate of net benefits per participant most comparable to Mackin et al., for instance, is only a few hundred dollars off. That for Loman is somewhat further, but still close. While our estimate meant to replicate Finigan et al. is still substantially lower, it is the highest of our generated estimates. The remaining disparity is largely driven by differences in the estimated marginal costs of treatment. Our estimate of net benefits of drug court participation is $6,193 per participant lower than Finigan et al. However, when considering that we estimate that the marginal treatment costs of drug court participation are $6,029 per participant greater than Finigan et al. estimate, the remaining disparity in estimated net benefits reduces to less than $200.

When replicating Carey and Waller, however, we find very different results. These differences can be easily explained. Carey and Waller estimated that drug court participants use $6,384 less in supervision resources than comparison individuals. Though much of this difference is driven by lower probation costs resulting from lower offending, even during the drug court program, participants use an estimated $2,419 less in supervision resources. Further examination indicates that this finding seems somewhat incongruous. In association with the initial arrest under consideration, that which would have led to drug court, comparison individuals were on probation for an average of 380 days, or a little longer than one year. Drug court participants, however, only meet with their case manager as long as they are in the program, which Carey and Waller estimate as only 153 days on average, or 5 months. Thus, higher supervision costs among the comparison group are largely driven by Carey and Waller's estimate that drug court lasts only 5 months, less than half as long as probation, and substantially less than the 12 to 15 months that most drug court programs last.

With these limitations considered, the data used in this study appear to be consistent with the detailed results presented in Loman (2004), Finigan et al. (2007), Carey and Waller (2008), and Mackin et al. (2010). We extend the discussion of comparability by examining two more studies that presented far less detailed results.

Both Aos and colleagues (WSIPP 2003) and Zarkin et al. (2005) presented cost-benefit findings of drug court using very broad categories. Table 4-F.2 demonstrates our replication of the costs in these categories, total program costs and benefits, and our best effort to replicate their specific estimates. For comparability, the results presented for Zarkin et al., are only those based on the two years following drug court enrollment, rather than the full six years of follow-up also presented in their report.

When considering Aos et al.'s (WSIPP 2003) slightly longer follow-up period, results using our data are very similar to those reported in that report. The largest difference is the costs of crime, again driven by our use of mean costs instead of median. Zarkin et al.'s results, however, are inconsistent with those generated by this data in addition to those presented in all other studies. This is because of the comparison group selected by Zarkin, et al. All comparison individuals were sentenced to prison. Thus, the entire comparison sample was incarcerated the entire first year following participants' enrollment in drug court, and much of the sample was incarcerated through the second year as well. Accordingly, criminal justice system costs for the comparison group were considerable.

Finally, Harrell et al. (1999) analyzed two different dockets: a treatment-intensive docket and a sanctions-intensive docket. In essence, this is analogous to evaluating two very different drug courts. In the full report, only the marginal costs of each docket, relative to business as usual, are presented. To be comparable with this approach, Table 4-F.3 presents findings from this data in the same way. All domains discussed by Harrell et al. (1999) are very comparable to our data. As drug courts in our sample were not intended to be a specific type, and all blended sanctions and treatment, one would expect our estimated costs and benefits to lie somewhere between the two dockets. Indeed this is the case, with the exception of costs of averted arrest and incarceration, which are explained because Harrell et al. (1999) did not find that the drug court being analyzed statistically significantly reduced re-arrest. Thus, our findings reflect the broader body of drug court research, which tends to find significant reductions in re-arrest and re-incarceration.

Taken as a whole, the three tables above demonstrate that the data used in this study are generally consistent with past findings of cost-benefit analyses of drug courts. Additionally, Harrell et al. (2003), while not presenting detailed cost-benefit findings, did estimate benefit-cost ratios for three different drug court programs of 2.3:1, 2.6:1, and 5.3:1. Other benefit-cost ratios estimated have been 2.2:1 (Zarkin et al. 2005), 2.6:1 (Finigan et al. 2007), 2.8:1 (Loman 2004), and 1.7:1 (WSIPP 2003). Thus, the benefit-cost ratio estimated from this data, 1.9:1 fits nicely into the larger literature.

Table 4-F.2. Comparison of Results with WSIPP. and Zarkin et al.

	These Data			WSIPP (2003)			Zarkin, et al (2005)		
Domain	**Treat**	**Control**	**(Difference)**	**Treat**	**Control**	**(Difference)**	**Treat**	**Control**	**(Difference)**
Drug Court costs (treatment, administration, monitoring, supervision, drug testing)	-$10,803	-$4,817	(-$5,986)	-5,382	0	(-5,382)	-46,961	0	(-46,961)
Hearings	-417	-56	(-361)	-3,898	-2,087	(-1,810)			
Sanctions during program				-4,369	-6,830	(2,461)	-5,805	-59,365	(53,560)
Long-term criminal justice system costs	-2,812	-5,556	(2,744)		-4,570	(4,570)	-9,822	-34,359	(24,537)
Crime	-7,111	-16,887	(9,776)		-3,671	(3,671)			
Total Program Costs	-11,219	-4,872	(-6,347)	-13,649	-8,917	(-4,732)	-52,766	-59,365	(6,599)
Total Program Benefits	-3,086	-12,229	(9,143)	8,241		(8,241)	-9,822	-34,359	(24,537)
Net Benefits (replicable)				-5,407	-8,917	(3,510)	-62,587	-93,723	(31,136)
Net Benefits (replication)				-21,141	-27,315	(6,174)	-13,614	-10,373	(-3,241)
Net Benefits (reported)	-14,305	-17,101	(2,796)	-5,407	-8,917	(3,510)	-62,587	-93,723	(31,136)

Notes: All figures presented in 2008 dollars.
For maximum comparability, displayed figures from Zarkin et al., are for the first two year only (full follow up period was six years).
For Zarkin et al., "Sanctions during program" refers to criminal justice system costs incurred during the first year following enrollment.
"Long-term criminal justice system costs" refers to all criminal justice system costs incurred during the second year.
Where treatment or control means are not included, they were not presented in the original report. Only marginal benefits were reported.

Table F-3. Comparison of Results with Harrell et al.

Domain	These Data (Marginal Benefits)	Harrell, et al. (1998) Treatment (Marginal Benefits)	Harrell, et al. (1998) Sanctions (Marginal Benefits)
Administration and non-Detox Treatment	-$6,073	-8,169	-2,108
Hearing costs	-361	-811	-466
Drug tests	-345	-1,057	-301
Detoxification	-572	0	-329
Crime	9,776	33,948	8,763
Arrests	71	0	-32
Incarceration (during and after program)	2,673	0	58
Total Costs	-6,392	-10,036	-3,204
Total Benefits	12,520	33,948	8,789
Net Benefits (replicable)	6,128	23,912	5,585
Net Benefits (reported)			4,200

Notes: All figures presented in 2008 dollars.
Harrell et al. costs include only treatment directly prescribed by the court. The court did not prescribe detoxification to those participating in the treatment program.
Zero marginal benefits in arrests and incarcerations reflect that the difference between treatment and comparison individuals was not statistically significantly different from zero.

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About the Authors

Editors

Shelli B. Rossman, M.A., is a senior fellow in the Justice Policy Center at the Urban Institute, where her research has focused on both criminal justice and public safety, including reentry, problem-solving courts, community-based and correctional supervision, case management and comprehensive service delivery for offender populations, delinquency prevention and intervention, and victimization; and public health, focused on substance abuse, HIV/AIDS, and reproductive health issues. In addition to serving as Principal Investigator for NIJ's MADCE study, she directed the NIJ-funded evaluation of the *Opportunity to Succeed (OPTS)* project, the first and only multi-site randomized clinical trial (RCT) of a prisoner reentry model. She has authored or collaborated on several articles and book chapters, in addition to authoring numerous technical reports.

John K. Roman, Ph.D., is a senior fellow in the Justice Policy Center at the Urban Institute where his research focuses on evaluations of innovative crime control policies and justice programs. Dr. Roman also serves as the executive director of the Washington, DC, Crime Policy Institute. He directs several studies funded by the National Institute of Justice, including two RCTs of the use of DNA in burglary and motor vehicle theft investigations, an evaluation of post-conviction DNA evidence testing to estimate rates of wrongful conviction, and a study developing a blueprint for the use of forensic evidence by law enforcement. He is the coeditor of *Juvenile Drug Courts and Teen Substance Abuse* and the author of numerous articles and book chapters

Janine M. Zweig, Ph.D., a senior research associate in Urban Institute's Justice Policy Center, has research and evaluation experience addressing issues relating to vulnerable populations, including both victims of violence and offenders. In addition to being part of the team studying adult drug courts, her projects have examined sexual violence in prison systems in the United States; the effectiveness of victim service, criminal justice, and welfare agency responses to victims of sexual assault and domestic violence; the co-occurrence of health risk behaviors during adolescence (including substance use, sexual activity, suicide, and violence); and the effectiveness of employment programs for individuals leaving prison and returning to communities. She received her Ph.D. in human development and family studies from the Pennsylvania State University.

Michael Rempel, M.A., is research director at the Center for Court Innovation, overseeing all research conducted at the agency. He is actively involved in projects concerning drug courts, offender reentry, violence against women, and applications of problem-solving techniques within traditional court settings.

Christine H. Lindquist, Ph.D., received her doctorate in Medical Sociology from the University of Alabama at Birmingham. She is currently a senior research sociologist in the Crime, Violence, and Justice research program at RTI International. Her research interests and areas of expertise include prisoner reentry, families and incarceration, and violence against women.

Authors

Avinash S. Bhati, Ph.D., is President of Maxarth LLC. **Dr. Avinash Bhati** formerly was a senior research associate for the Urban Institute's Justice Policy Center. He supervised research activities that included primary data collection efforts, data organization and synthesis, data checking, data analysis, report writing, and dissemination. Research projects Dr. Bhati participated in or led covered such topics as the evaluation of programs aimed at reducing crime; analysis of sentencing outcomes; spatial analysis of crime; understanding supervision failure, modeling individual offending patterns; and the development of innovative analytical methods for analyzing crime and justice related issues

P. Mitchell Downey, M.A., is a research associate II in the Urban Institute's Justice Policy Center. He has conducted quantitative analysis for multiple projects, using such methods as time-series analysis, Bayesian methods, random effects and hierarchical models, latent class growth modeling, meta-analysis, spatiotemporal analysis, Heckman error correction, and others. He also has conducted cost-benefit analyses for diverse projects, including drug courts and other problem-solving courts, use of DNA in criminal investigations, jail sexual assault prevention programs, applications to meta-analytic results, and others. Mr. Downey earned his degree in economics from the University of Missouri-St. Louis.

Kristine Fahrney is a survey director who has been with RTI since 1999. During this time, she has led data collection efforts for a number of studies involving research subjects who are incarcerated or otherwise involved with the criminal justice system including the Multi-Site Drug Court Evaluation; the Study of Development Across the Lifecycle; and the Evaluation of the Administration of Children and Families Responsible Fatherhood, Marriage and Family Strengthening Grants for Incarcerated and Re-entering Fathers and Their Partners. Prior to coming to RTI, Ms. Fahrney was involved in several other data collection efforts involving similar populations including: Evaluation of Offenders in Lockup Status in the Iowa State Penitentiary, Women Offenders Treatment Program Year 1 Evaluation, and Sex Offender Treatment Program Ongoing Evaluation.

Donald J. Farole, Jr., Ph.D., is a statistician with the prosecutions and adjudications unit at the Bureau of Justice Statistics, U.S. Department of Justice. His research interests include justice system responses to intimate partner violence, recidivism, and pro se litigants in criminal and civil litigation. Prior to joining the BJS, he conducted research and evaluation on drug courts and other innovative justice system programs at the Center for Court Innovation. He also has published research on state appellate courts, including a book and several articles in professional journals. Dr. Farole received his Ph.D. from Indiana University.

Mia Green, Ph.D., is a senior research associate at the Center for Court Innovation. Ms. Green's prior research has included studying the effects of reentry of formerly incarcerated women and pretrial release conditions on misdemeanor and felony defendants. Ms. Green received a master's in education from San Francisco State University and a Ph.D. in criminal justice from the Graduate Center of the City University of New York.

Kelli Henry, Ph.D., is a senior research associate at the Center for Court Innovation. She has done research on drug courts, juvenile mental health programs for justice-system involved youth, and community courts. Prior to joining the Center, Dr. Henry worked as Europe Editor at the Economist Intelligence Unit and taught college courses in social science research methods. Dr. Henry earned her Ph.D. in sociology from New York University.

Dana Kralstein, M.P.P., the associate director of Research at the Center for Court Innovation, is currently participating in an evaluation of the Bronx Family Treatment Court, as well as a study of child protective cases in New York State's Family Court. She is the author of an evaluation of the Suffolk County Juvenile Drug Court, a study of the Baltimore Community Justice Initiative, a community court evaluation literature review, and is a coauthor of the New York Statewide Adult Drug Court Evaluation. Ms. Kralstein attended the University of Rochester as an undergraduate, and received her master's in public policy from the Ford School at the University of Michigan, with a concentration in social policy. Ms. Kralstein is currently working towards a Ph.D. in political science at the Graduate Center at the City University of New York.

Erica Lagerson was formerly a research associate in the Justice Policy Center at the Urban Institute, where she contributed to research projects through quantitative and qualitative data collection and analysis.

Joshua A. Markman is a research associate II with the Urban Institute's Justice Policy Center. He is currently the Policy Area Manager for the District of Columbia Crime Policy Institute (DCPI), where he is responsible for day-to-day project activities. Mr. Markman has also managed several projects, primarily those relating to the evaluation of DNA and its use by various criminal justice actors. Before coming to the Justice Policy Center, Mr. Markman studied criminology and sociology at the University of Pennsylvania.

Courtney Schafer was formerly a research associate in the Justice Policy Center at the Urban Institute, where she contributed to research projects through quantitative and qualitative data collection and analysis.

Janeen Buck Willison, M.S.J. is a senior research associate in the Justice Policy Center at the Urban Institute. She has more than a decade of experience managing and directing complex experimental and quasi-experimental multi-site fieldwork studies of youth and adult populations. Her research focuses on diverse juvenile and criminal justice issues, including specialized courts, delinquency prevention, mental health interventions for offenders, faith-based re-entry programs, and the link between substance abuse and crime. She serves as principal investigator for the NIJ-sponsored study of faith-based reentry programs, co-principal investigator for the process evaluation of the Transition from Prison to Community (TPC) Initiative; and was project director for the recently completed Assessing Policy Options (APO) project, and project manager for the national evaluation of the Robert Wood Johnson Foundation's Reclaiming Futures initiative.

Jennifer Yahner (nee Castro) is a research associate in the Urban Institute's Justice Policy Center. She has been conducting criminal justice research for more than a decade and has experience in issues surrounding prisoner reentry, court-ordered supervision, intimate partner violence, and high-risk youth program evaluation. She is skilled in large-scale data management and statistical analysis using a variety of quantitative methodologies. Her responsibilities on projects such as the *Returning Home* studies and the *Transitional Jobs for Ex-Offenders* and *Judicial Oversight Demonstration* evaluations have included data acquisition, management, scale construction, statistical analysis (e.g., attrition/sample representativeness, structural equation modeling, social network, cost analysis), and reporting.