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Systematic review

A systematic review of drug court effects on recidivism

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Abstract. Drug courts have been proposed as a solution to the increasing numbers of drug involved offenders entering our criminal justice system, and they have become widespread since their introduction in 1989. Evaluations of these programs have led to mixed results. Using meta-analytic methods, we systematically reviewed the extant evidence on the effectiveness of drug courts in reducing future criminal offending. Fifty studies representing 55 evaluations were identified, including both experimental and quasi-experimental comparison group designs. The overall findings tentatively suggest that drug offenders participating in a drug court are less likely to reoffend than similar offenders sentenced to traditional correctional options. The equivocation of this conclusion stems from the generally weak methodological nature of the research in this area, although higher quality studies also observed positive results. Furthermore, the evidence tentatively suggests that drug courts using a single model (pre- or post-plea) may be more effective than those not employing these methods. These courts have a clear incentive for completion of the drug court program.

Key words: drug courts, meta-analysis, recidivism

The "get-tough on crime" strategies of the past two and a half decades have inundated the United States' court system, probation offices, jails, and prisons with a significant number of offenders convicted of drug crimes and suffering from drug addiction (U. S. General Accounting Office 1997). According to the results from the 2000 Arrestee Drug Abuse Monitoring (ADAM) study, between one-fourth and one-half of all adult males arrested, and roughly one-half of all females arrested, were at risk for drug dependence; few had been treated for drug or alcohol use in the prior year (Arrestee Drug Abuse Monitoring Program 2003). Furthermore, state and local police reported 1.5 million drug abuse violation arrests to the Federal Bureau of Investigation (FBI) during 2001. All told, drug involved offenders consistently represent an important and large portion of the criminal justice population.

Several early innovations in expedited case processing and diversion of drug offenders to alleviate jail overcrowding provided a foundation for the emergence of

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the drug court (Drug Court Professionals 1997). These early innovations did not integrate treatment into judicial processing. The first drug court to incorporate substance abuse treatment and represent what is currently meant by the phrase "drug courts" was in Dade County, Florida, in 1989. Since that time, drug courts have become wildly popular. As of March 2005, there were over 1,300 established or recently implemented drug courts in the United States, with over 500 being planned (BJA Drug Court Clearinghouse Project 2005).

The popularity and widespread adoption of drug courts had many influences. One important contributor to drug court expansion has been public and private funding. According to the U. S. General Accounting Office (1997), from 1989 through 1997, over $80 million in federal funding had been made available for all aspects of drug court expansion, including the planning, implementation, enhancement, and evaluation. Substantial additional funding for drug courts has come from state and local governments as well as non-governmental sources, exceeding $45 million, according to the U. S. General Accounting Office (1997). The broad support for drug courts may also stem from the perception that these courts hold drug offenders accountable for their irresponsible behavior through sanctions for continued drug use and, at the same time, provide drug abusers with access to needed treatment.

In its simplest form, a drug court uses the power and authority of a judge to keep a drug offender in treatment, providing rewards for successes and sanctions for failures (U. S. General Accounting Office 1997; Drug Court Professionals 1997). Typically, a judge closely monitors the progress of a drug offender (generally referred to as a client) and doles out sanctions for drug use relapse, failure to attend treatment, or other drug court infractions. The judge also reinforces successes through praise and encouragement and, possibly, a reduction in formal requirement or other "reward." Depending on the structure of the drug court, successful completion may be accompanied with dropping the charges that brought the offender before the court (pre-plea/diversionary court) or expunging the offense from the record (post-plea court). Many drug courts also have a formal graduation ceremony for those successfully completing the program. The atmosphere of the drug court is non-adversarial and provides a case management function, connecting drug abusers with appropriate treatment programs. As described by the Drug Court Professionals (1997),

> The judge is the central figure in a team effort that focuses on sobriety and accountability as the primary goals. Because the judge takes on the role of trying to keep participants engaged in treatment, providers can effectively focus on developing a therapeutic relationship with the participant. In turn, treatment providers keep the court informed of each participant's progress so that rewards and sanctions can be provided. (Drug Court Professionals 1997, p. 7)

The essential features of a drug court are (a) the integration of alcohol and other drug treatment and justice system case processing, (b) a non-adversarial courtroom approach, (c) random urine drug screens or other monitoring of abstinence, (d) judicial monitoring of a participants progress via status hearings, and (e) a system

of sanctions and rewards for program infractions and achievements (Drug Court Professionals 1997; Marlowe et al. 2006).

Drug courts are presumed to affect an offender's drug use and criminal behavior through both the actions and influences of the court and the involvement of the offender in mandated drug and alcohol abuse treatment (e.g., Marlowe et al. 2006; Banks 2001). Drug involved offenders managed in a traditional manner by the criminal justice system are also routinely referred to drug treatment. However, treatment compliance is a major problem with this population and a clear impediment to successful outcomes (Simpson et al. 1997; Drug Court Professionals 1997). The design and structure of drug courts is intended to address this problem by using the power of the judge, as described below, to compel treatment compliance.

A debate within the drug abuse treatment literature is whether coercing someone with a drug or alcohol problem into treatment can be effective (Farabee et al. 1998). Some argue that treatment will be effective only when abusers of drugs and alcohol participate in treatment of their own free will and are genuinely motivated to change their lives. The empirical evidence fails to support this view. For example, Farabee et al. (1998) reviewed the evidence on the effectiveness of using the criminal justice system to coerce drug abusers into treatment and concluded that coercion does not undermine program effectiveness.

Drug courts address the problem of an offender's retention in mandated drug and alcohol abuse treatment through status hearings before the judge. Random urine analyses are also typically part of the courts' monitoring of an offender's compliance with treatment. This monitoring is paired with sanctions and rewards in a fashion that is consistent with the principles of behavior modification (Marlowe et al. 2006). In many drug courts the rewards and sanctions are clearly laid out and communicated to the offender. Furthermore, Marlowe et al. have argued that judges are the only members of society with sufficient power to apply "substantial sanctions and rewards ... with consistently and certainty" (Marlowe et al. (2006), p. 71), features essential to effective behavior modification.

Not all drug courts are alike, and the differences in drug court approach and structure may influence effectiveness. Longshore et al. (2001) provide a useful conceptual framework for thinking about the dimensions along which drug courts may differ (see also Turner et al. 2002). These dimensions are: (1) leverage, (2) population severity, (3) program intensity, (4) predictability, and (5) rehabilitative emphasis. Leverage refers to aspects of the court structure, such as the percentage of pre-plea and post-plea participants and the perceived aversiveness of discharge from the program. Essentially, this relates to the rewards and sanctions available to the court. Characteristics of the offender population, such as drug use history and involvement in other criminal activities, comprise the second dimension of population severity. There is some evidence to suggest, for example, that drug courts may be more effective for more seriously involved drug offenders (Marlowe et al. 2006; DeMatteo et al. 2006). Program intensity reflects structural aspects of both the court and the treatment services provided, such as the frequency of urine testing and court appearances and the amount of treatment. Predictability is the celerity, certainty, and

consistency of the judicially imposed rewards and sanctions as well as the compliance of these with the drug court protocol. And finally, rehabilitative emphasis incorporates a range of rehabilitative features, such as the degree of collaborative decision making, attention to multiple needs, and flexibility in procedures. This framework clearly identifies defining features of a drug court, such as treatment and judicially imposed rewards and sanctions, but also incorporates other dimensions that may impact on the drug courts' functioning and effectiveness, such as the particular mix of clients and basic drug court structure.

Reviews of the drug court literature have come to rather disparate conclusions, including an assessment that they definitively work (Meyer and Ritter 2002) and a contrasting interpretation that the excitement over drug courts is misguided (Anderson 2001). A review conducted by the U. S. General Accounting Office (1997) concluded that the existing evidence was insufficient for any firm conclusion to be drawn on the effectiveness of these programs with respect to recidivism. More specifically, this review identified several limitations of the 20 evaluations examined, including a failure to test outcomes beyond program participation and a failure to use a comparison group design. Twelve of these evaluations included a comparison group, and six of these examined recidivism post-program. Summarizing these studies, the GAO stated that:

> Some studies showed positive effects of the drug court programs during the period offenders participated in them, while others showed no effects, or effects that were mixed, and difficult to interpret. Similarly, some studies showed positive effects for offenders after completing the programs, while others showed no effects, or small and insignificant effects. (U. S. General Accounting Office 1997, p. 85)

Belenko (2001) drew a cautious but positive conclusion on the impact of drug courts on long-term drug use and criminal offending, based on a review of 37 evaluations. Not all the evaluations reviewed by Belenko examined drug use or other criminal activity outcomes. Belenko was critical of the field's dearth of evaluations that examined post-program drug use and other criminal behavior, noting that only six of the studies he reviewed examined the long-term effects of these programs. The process data reviewed by Belenko suggested that "drug courts have achieved considerable local support and have provided intensive, long-term treatment services to offenders with long histories of drug use and criminal justice contacts, previous treatment failures, and high rates of health and social problems" (Belenko 2001, p. 1).

A more recent systematic review of drug court evaluations was recently completed by the GAO (U. S. General Accountability Office 2005). It examined 27 evaluations and concluded that "adult drug court programs led to recidivism reductions during periods of time that generally corresponded to the length of the drug court" (p. 5). The evidence on the effectiveness of drug courts to reduce substance use, based on this review, was mixed. This study also reviewed four cost-benefit evaluations and concluded that drug courts do yield a net benefit.

The rise of drug courts in the United States during the past 14 years has been nothing short of phenomenal and is often described as a "movement" (e.g.,

Goldkamp 1994a; Nolan 2001). Despite widespread belief in the effectiveness of drug courts at reducing criminal behavior, the evidence justifying these beliefs requires careful scrutiny, given the tremendous public expenditures involved. This study addresses this need by meta-analyzing all available comparison group evaluations of drug courts. Prior reviews of drug court evaluations, such as those by Belenko (2001), U. S. General Accounting Office (1997), and U. S. General Accountability Office (2005) have relied on narrative review techniques that do not fully exploit the empirical evidence. The objectives of this review are to synthesize the extant evidence of the effectiveness of drug courts with respect to drug use and other criminal behavior, to examine moderators of drug court effectiveness (e.g., the dimension proposed by Longshore et al. 2001), and to critically assess the methodological quality of the available evaluations. The latter will provide a context for establishing the robustness of the results. The findings will be discussed with respect to both future research needs and implications for criminal justice system policy.

Method

Eligibility criteria

The population of studies eligible for this review was experimental and quasi-experimental evaluations of drug courts that utilized a comparison group. The criteria for inclusion were (a) that the study evaluated a drug court program (i.e., a specialized court for handling drug cases that was non-adversarial, had a mechanism for referring offenders to appropriate treatment programs, with a judge who actively monitored progress and provided sanctions for misbehavior); (b) that the study included a comparison group that was treated in a traditional fashion by the court system (this excluded drug court dropouts and alternative innovative programs also designed to reduce drug use); and (c) that the study reported a measure of criminal behavior, such as arrest or conviction for some measurement period following the start of the program (the measure may have been based on official records or self-report and may have been reported on a dichotomous or continuous scale).

Search strategy

The search strategy was designed to locate all eligible evaluations of drug courts, published or otherwise. Toward this aim, we performed keyword searches of the following databases: Criminal Justice Periodical Index, Dissertation Abstracts Online, Government Publications Office Monthly Catalog, Government Publications Reference File, NCJRS, PsychINFO, Sociological Abstracts, Social SciSearch, and U.S. Political Science Documents. The initial search of these bibliographies was part of a larger search for all drug treatment evaluations that used a broad range of terms for drug treatment, offenders, and evaluation or

research. A more focused set of searches was performed using the term *drug court*. To identify additional unpublished evaluations, we reviewed recent years (1999–2004) of the *American Society of Criminology* conference program for relevant studies. We also contacted several authors working in the area to assist in the identification of additional hard-to-find works. Finally, we examined the bibliographies of reviews of the drug court literature (e.g., Belenko 2001; U. S. General Accounting Office 1997; U. S. General Accountability Office 2005). This process identified 167 documents. We retrieved and evaluated these documents for eligibility using the above criteria. Sixty-eight documents met our eligibility criteria. The results for 18 studies were reported across multiple publications. As such, these 68 documents (marked with an asterisk in the reference list) represented 50 studies and 55 independent drug court-comparison contrasts.

Coding protocol

An elaborate coding protocol was developed for this systematic review that provided a method of extracting information regarding each study's research design, program and offender characteristics, nature of the outcome measure, and outcome data. To the extent possible, we coded features of the drug court that theoretically might relate to effectiveness, such as dimensions proposed by (Longshore et al. 2001). Unfortunately, as is often the case in meta-analysis, the level of descriptive information provided in the reports about the nature of the drug courts was woefully inadequate. We were, however, able to code the basic structure of the drug court (i.e., pre-plea, post-plea, ad hoc, etc.). This feature relates to both the leverage and predictability dimensions elaborated by Longshore et al. (2001). The ad hoc model tends to be less predictable than either of the pre-plea or post-plea models, as the sanctions and rewards are not predetermined and clearly communicated to the offender, whereas this is typically the case in the pre-plea and post-plea models. Population severity information was generally lacking throughout this literature and, as such, could not be examined. The rehabilitative emphasis was examined in a limited fashion by coding whether the drug court model incorporated formal clinical assessment (any psychosocial assessment reported by the study) and whether the court referred offenders to a single treatment provider or to a range of providers available in the community. At a more generally level, this lack of descriptive clarity suggests substantial unobserved variability in the nature of the drug court across studies beyond the self-identification as a drug court and the essential features of a non-adversarial courtroom process, referral to treatment, and judicial monitoring.

We also coded elaborate information regarding each study's research design, the nature of the outcome measures, and outcome data. The protocol allowed for the coding of any number of effect sizes for each drug court-comparison contrast reported in a study. These multiple effect sizes per court-comparison contrast are statistically dependent. The method of handling this statistical dependency is discussed below. Also, note that the protocol defined the drug court-comparison

contrast as the primary unit of analysis. Four studies (Goldkamp et al. 2000; Peters and Murrin 2000; Rempel et al. 2002; Truitt et al. 2002) reported findings on more than one such contrast (e.g., different locales). Thus, the 50 unique studies reported results on 55 independent drug court-comparison contrasts. A copy of the coding protocol is available from the first author.

Statistical analysis

The effect of a drug court on recidivism was encoded using the odds ratio. The odds ratio is well suited to dichotomous outcomes such as those commonly used in drug court evaluations (e.g., percentage of the drug court and comparison group sample with an arrest or conviction at follow-up). When the drug use or criminal behavior was measured continuously, we computed a standardized mean difference type effect size and transformed it into an odds ratio (see Hasselblad and Hedges 1995; Lipsey and Wilson 2001).

All analyses were performed on the logged odds ratio using the inverse variance method (see Lipsey and Wilson 2001). We assumed that the true program effects estimated by the studies varied both as a function of measured between-study differences (moderators, such as method of assignment and type of drug court) and unmeasured differences. As such, we implemented a random-effects model, or, in the case of moderator analyses, a mixed-effects model (Lipsey and Wilson 2001; Raudenbush 1994). These analyses were performed using program code written for Stata available at http://mason.gmu.edu/~dwilsonb/ma.html.

Most studies reported data on more than one indicator of recidivism, and a few studies reported on the same indicator at multiple follow-up points. The multiple effect sizes coded from a single drug court-comparison contrast are statistically dependent (see Gleser and Olkin 1994, for a discussion). Several methods exist for addressing this complication of meta-analytic data (Lipsey and Wilson 2001). The approach adopted in this synthesis was to select one effect size for analysis, based on explicit criteria. If multiple effect sizes met the criteria, then an average of those effect sizes was used. Thus, any given analysis presented below used only a single effect size per drug court-comparison contrast (or an average effect size in some cases).

Results

Description of studies

Tables 1 and 2 present descriptive information for the studies included in this meta-analysis, the majority of which (62%) were unpublished technical reports. Such a high proportion of unpublished studies greatly reduced the possibility that the meta-analytic results were influenced by publication bias. Publication bias results from statistically significant findings being more likely to be published than statistically non-significant findings, leading to an upward bias in meta-analytic

Table 1. Drug court study characteristics.

Characteristic	*Frequency*	*Percent*
Publication type[a,c]		
Journal/book chapter	19	38
Unpublished	34	62
Publication year[b,c]		
2003–2004	7	14
2000–2002	18	36
1996–1999	17	34
1993–1995	5	10
No date	3	6
Age of sample		
Juvenile	6	11
Adult	49	89
Gender of sample		
All male	0	0
Mostly male (60%–90%)	44	80
50/50 Male/female	2	4
All female	1	2
Not reported	8	15

[a]Coded as published (journal/book chapter) if any of the documents used to code the study had been published.
[b]Earliest year for studies with multiple reports.
[c]Denominator is the total number of studies (50), not the number of independent comparison–contrasts (55), as with all other analyses.

results based solely on published studies (Rothstein et al. 2005). Most of these evaluations were current, with half having appeared since 2000, inclusively. Only five of the 50 reports pre-dated 1996. Many of these recently completed studies had not been included in prior reviews.

According to a 2003 summary of state and county drug court activity published by the Drug Court Clearinghouse (2003), only 93 juvenile drug courts had been in operation at that time for at least 2 years, compared with roughly 800 adult drug courts. As such, it was not surprising that we identified only six evaluations of juvenile drug courts. Most of the evaluations in this synthesis included men and women (or boys and girls), although men tended to predominate most of the evaluations. There was only a single evaluation focusing exclusively on the effectiveness of drug courts for women (Harrell et al. 2001).

Drug courts share many common features, such as the non-adversarial nature of the courtroom and mandatory involvement in drug abuse treatment. However, drug courts differ from one another in important ways. The original Miami drug court model has been re-interpreted and adapted to the needs and legal systems of other states and counties. As mentioned in the Methods section, our ability to capture

Table 2. Characteristics of the drug courts.

Characteristic	*Frequency*	*Percent*
Drug court model		
Pre-plea (diversion)	16	29
Post-plea (suspended sentence)	11	20
Mixed pre- and post-plea	2	4
Ad hoc (judge decides)	8	15
Not reported	18	33
Single treatment provider		
Yes	20	36
No	27	49
Not reported	8	15
Formal clinical assessment		
Yes	31	56
No	19	35
Not reported	5	9

these distinctions, particularly those dimensions elaborated by Longshore et al. (2001), was limited, due to the lack of descriptive information in the research reports regarding the nature of the drug courts and, also, due to the contextual nature of some of these differences, such as the mixture of treatment services available to any given drug court. The descriptive information available from the written reports did not allow for a differentiation beyond basic characteristics of the offenders and the nature of the drug court.

The distributions of the distinctions across the studies that we were able to code are reported in Table 2. As shown in the table, the pre-plea or diversion model was most prevalent, after the not-reported category. Drug courts using this approach defer prosecution dependent on the offender's agreement to participate in the drug court program. Failure to complete the drug court program results in prosecution for the original offense. Often the offender must admit to the evidence presented in the police report, ensuring the certainty of a guilty verdict and criminal sanctions if the offender fails to graduate from the drug court. In contrast, an offender participating in a post-plea drug court pleads guilty to the charge(s) and sentence is suspended or deferred. Upon successful completion of the program the sentence is waived, and, in many cases, the record is expunged. Both these approaches provide an offender with a powerful incentive to complete the requirements of the drug court. A few drug courts used a mixed approach, including both pre- and post-plea cases or a model where the consequences and incentives were ad hoc at the sole discretion of the judge. These ad hoc models individualized the consequences of failure to complete the drug court requirements and, as such, can be considered to be lower on the Longshore et al. dimension of predictability. Roughly half the evaluated programs utilized multiple service providers, and roughly a third used a dedicated single provider. We could not determine the service provider

arrangement for the remaining studies. Formal assessment of treatment needs, such as a psychosocial evaluation, was relatively common. It is recognized that such assessments vary considerably across courts.

The eligibility criteria for this meta-analysis were intentionally inclusive with respect to methodological quality and included quasi-experimental studies that used a comparison group design. Many of the quasi-experimental studies included in this synthesis have clear threats to internal validity. These studies, however, provide valuable information on the generalizability of observed effects and are useful for exploring moderating relationships. The inclusion of quasi-experimental studies in this meta-analysis necessitated careful analysis and the explicit examination of the relationship between method and observed effects (Wilson and Lipsey 2001). The methodological characteristics of these studies are shown in Table 3.

Only five studies used random assignment to conditions. Unfortunately, two of these suffered from attrition in excess of 40% (Dickie 2000, 2001), severely undermining the benefits of random assignment. The use of statistical controls to account for measured pretest differences, such as the use of analysis of covariance or multiple regression analysis, was common. Additionally, subject level matching was used in roughly one-fifth of the studies. About half of the studies employed some method of reducing selection bias, through randomization, the use of statistical controls, or subject level matching. Conversely, about half of the studies used a weak quasi-experimental design, that is, they did not use random assignment, statistical control, or subject level matching.

Table 3. Study methodological characteristics.

Characteristic	*Frequency*	*Percent*
Nature of the comparison sample		
Declined/rejected	20	36
Historical controls	13	24
Eligible non-referred	8	15
Comparable (randomization)	5	9
Regular probation/diversion	4	7
Non-eligible drug offenders	2	4
Non-drug court cases	2	4
Not reported	1	2
Method features[a]		
Used random assignment	5	9
Used statistical controls	21	38
Used subject level matching	10	18
Used any of above methods	28	51
Attrition		
Attrition >10%	12	22
Differential attrition >10%	10	18

[a]These categories are not mutually exclusive.

Selection bias was a clear threat to the interpretation of the findings of several of these designs. Comparison groups composed of offenders who declined participation in the program are clearly different from those who agreed to participate. It is impossible to know, from the information at hand, whether such individuals would be more or less likely to recidivate than would those offenders agreeing to participate in the drug court in the absence of the drug court intervention. The effect of the nature of the comparison group type on the observed results is examined below.

Effect size analysis

We coded 402 odds ratios for the 55 independent drug court-comparison samples, the vast majority of which favored the drug court over the comparison condition (314 or 78%). That is, the drug court had a lower rate of drug use or other criminal behavior than the comparison had. These odds ratios reflected various indicators of criminal activity, including measures of all offenses (e.g., any arrest), drug offenses (e.g., any drug related arrest), and non-drug offenses (e.g., arrests for person and/or property offenses). In the analyses below, we examined each of these offense types separately. To maintain statistical independence between odds ratios, we included only a single odds ratio per drug court-comparison contrast in a single analysis (e.g., mean odds ratio). Thus, we applied a decision rule to select the most general indicator within each of the three offense categories. The decision rule gave preference to dichotomous measures of arrest. Studies often measured outcomes at multiple time points, and, in these cases, we selected the time point furthest from the start of the program. We excluded from consideration, however, odds ratios based on samples with greater than 10% attrition relative to the odds ratio with the largest sample size. These decisions produced a single odds ratio based on a dichotomous measure for all offenses for 49 of the 55 drug court-comparison contrasts. For the remaining six (Roehl 1998; Dickie 2000; Craddock 2002; Listwan et al. 2003; Wolfe et al. 2002; Rodriquez and Webb 2004), an average odds ratio was computed across the separate indicators of recidivism. For non-drug offenses, an average odds ratio was computed for seven of the 55 drug court-comparison contrasts (Gottfredson et al. 1996; Johnson et al. 1998; Johnson and Latessa 2000; Roehl 1998; Cosden et al. 1999; Listwan et al. 2001a, b), and, for drug offenses, an average odds ratio was computed for two of the 55 drug court-comparison contrasts (Roehl 1998; Rodriquez and Webb 2004).

The vast majority of the odds ratios were based on indicators of arrest. However, some were based on conviction, self-report, or a drug screening test. The distribution of the nature of the outcome measure for the three offense types is presented in Table 4.

The random effects mean odds ratio and related statistics are presented in Table 5 for each offense type. Odds ratios greater than 1 indicate that the odds of an offense was lower for the drug court condition than for the comparison condition (i.e., evidence of drug court effectiveness). The recidivism rate for all offenses and

Table 4. Nature of the outcome measure for each offense type (values represent frequencies).

	Offense type		
Indicator type	*All*	*Drug*	*Non-drug*
Arrest	47	24	16
Conviction	8	6	2
Self-report	0	1	0
Drug screen	0	3	0

drug offenses were lower, on average, for the drug court participants than for the comparison group participants. This distribution included an outlier (see Figure 1) that, when removed, reduced the overall mean odds ratio to 1.62. This mean is statistically significant, with a 95% confidence interval of 1.42 to 1.85. An odds ratio of 1.62 is equivalent to a reduction in recidivism from 50% for a given sample to 38%, a moderate reduction in the percentage of offenders likely to engage in further criminal behavior.[1]

Drug courts and the treatment programs that are part of the drug court system focus on reducing drug use among offenders. It is expected that this will lead to lower levels of drug offenses. Thus, we would anticipate that drug courts would affect non-drug offenses to a lesser extent than they would affect drug offenses. The data were consistent with this expectation.

The overall mean effect size tells an incomplete story. The characteristics of these studies varied considerably, leading to substantial variability in effects across studies, as indexed by statistically significant Q statistics. This variability is visually evident in Figures 1, 2, and 3. These figures present the odds ratio and 95% confidence interval for each study for the three different offense types and show that most evaluations observed a moderate positive effect, with a few studies observing a large positive effect or a small negative effect. The large variability in effects across studies reduces the meaningfulness of a single mean effect size as a descriptor of the effects, suggesting moderating effects of both substantive and methodological features of the studies. Below, we examine the relationship between the odds ratios and both methodological and substantive features. We also

Table 5. Mean odds ratios and 95% confidence intervals (*C.I.*) for all offenses, drug offenses, and non-drug offenses.

		95% C.I.				
Offense type	*Odds ratio*	*Lower*	*Upper*	*z*	*Q*	k^a
All	1.66*	1.46	1.90	7.55	219.65[b]	55
Drug	1.64*	1.37	1.96	5.52	131.43[b]	34
Non-drug	1.38*	1.08	1.68	1.76	53.50[b]	18

*$P \leq 0.05$; mean odds ratio statistically different from 1.
[a]Number of effect sizes contributing to each analysis.
[b]Distribution of odds ratios is heterogeneous.

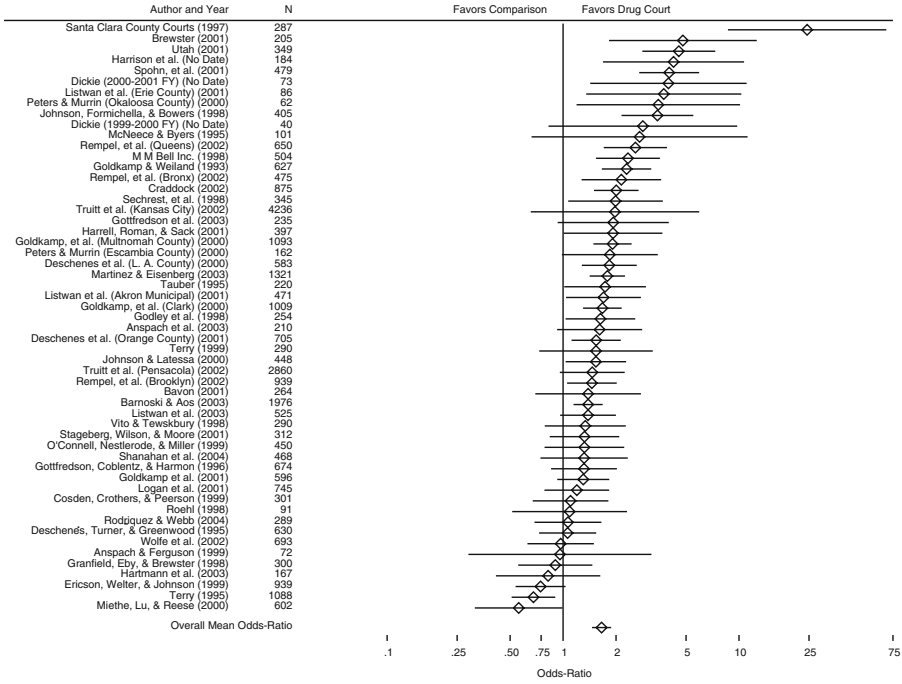

Figure 1. Odds ratios and 95% confidence intervals for all offenses by study.

examine the hypothesis that effects decay over time and assess the plausibility that publication/selection bias affected our results.

Methodological variation

Recall that the eligibility criteria for this meta-analysis allowed for the inclusion of methodologically weak studies, raising the possibility that the above findings are spurious and reflect a pervasive selection bias across studies. This section examines the robustness of the overall findings to methodological variation, such as the nature of the comparison group.

Only five of the studies used random assignment to the drug court or comparison condition (Deschenes et al. 1995; Dickie 2000, 2001; Gottfredson et al. 2003; Shanahan et al. 2004). As mentioned previously, two of these studies suffered from excessively high attrition (Dickie 2000, 2001). The mean odds ratio for all offense types for the three randomized studies with low attrition was positive (1.35) but not statistically significant (95% confidence interval of 0.70 to 2.61). The mean odds ratio for drug offenses was of similar magnitude (1.29). These three exceptional studies provide a mixed picture of the overall effectiveness of drug courts. The mean translates into a small reduction in recidivism of 14% (from 50% to 43%). Unfortunately, the null hypothesis of no effect remains plausible. As is evident by the large confidence interval, the statistical power for the mean odds ratio is low, and a moderately large positive effect is also plausible.

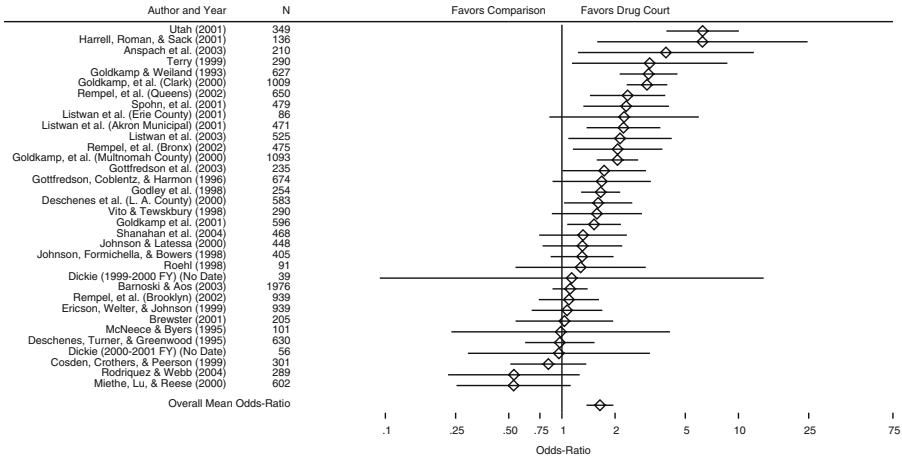

Figure 2. Odds ratios and 95% confidence intervals for drug offenses by study.

Gottfredson et al. (2003) provided the strongest evidence that drug courts can be effective in reducing recidivism, reporting a very large, by correctional intervention standards (and statistically significant), difference in any re-arrest between the drug court and comparison conditions at 24 months after admission into the program (odds ratio = 2.22). This effect was still large at 36 months (odds ratio = 1.93, the effect used in the above analysis), although no longer statistically significant at a conventional level. This high-quality randomized study helps establish that drug courts can be effective at reducing future criminal behavior.

Beyond random assignment to conditions, studies varied with respect to the use of subject level matching, covariate adjustments of effect estimates, degree of attrition, and the nature of the comparison group. The mean odds ratio for both all offenses and drug offenses for the categories of these variables are presented in Tables 6 and 7. No consistent pattern emerged with respect to the relationship between the odds ratio and quality of the study method. Studies that used subject level matching, adjusted the effect for baseline covariates, or had low levels of attrition had findings similar to those that did not have these characteristics. Differential attrition, however, did appear to upwardly bias the mean odds ratio for all offenses. We operationalized differential attrition as a

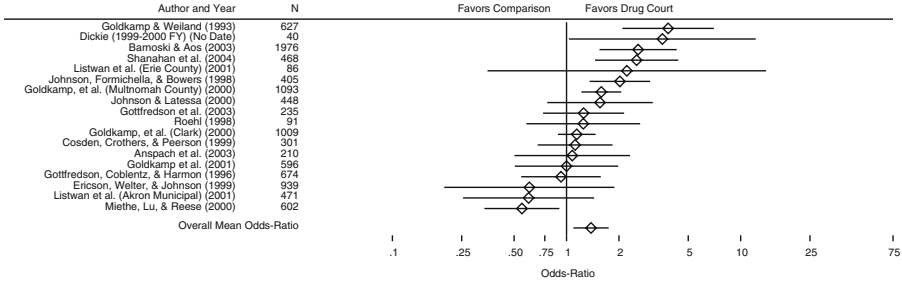

Figure 3. Odds ratios and 95% confidence intervals for non-drug offenses by study.

Table 6. Mean odds ratio by study method feature.

	All offenses		*Drug offenses*	
Method feature	*Odds ratio*	*k*[a]	*Odds ratio*	*k*[a]
Random assignment				
Yes, all	1.66*	5	1.24	5
Yes, with no attrition	1.35	3	1.30	3
No[c]	1.61*	49	1.71*	29
Matching[b]				
Yes	1.98*	9	1.75*	6
No[c]	1.53*	40	1.69*	23
Effect adjustment for covariates[b]				
Yes	1.63*	15	1.30	6
No[c]	1.55*	34	1.80*	23
Attrition <10%				
Yes	1.93*	12	1.52*	11
No[c]	1.55*	42	1.68*	23
Differential attrition <10%[d]				
Yes	2.22*	10	1.68*	10
No[c]	1.53*	44	1.63*	24

*$P \leq 0.05$; mean odds ratio statistically different from 1.
[a]Number of effect sizes contributing to each analysis.
[b]Excludes randomized studies.
[c]One outlier removed from the all offense analysis (a large positive effect).
[d]Difference between means statistically significant at $P \leq 0.05$.

Table 7. Mean odds ratio by comparison group type.

	All offense		*Drug offenses*	
Type of comparison group[b]	*Odds ratio*	*k*[a]	*Odds ratio*	*k*[a]
Comparable (randomization)	1.66*	5	1.24	5
Historical controls	1.76*	13	1.90*	8
Eligible non-referred[c]	1.58*	7	1.86*	6
Declined/rejected	1.51*	20	1.75*	10
Non-eligible drug offenders	1.52	4	0.97	4
Regular probation/diversion	1.89*	4	1.60	1
Not reported	1.32	1		

*$P \leq 0.05$; mean odds ratio statistically different from 1.
[a]Number of effect sizes contributing to each analysis.
[b]Difference across categories not statistically significant for either offense type.
[c]One outlier removed from the al-offense analysis (a large positive effect).

difference greater than 10% in the rate of attrition between the two groups. The mean effect for studies with low levels of differential attrition, including no attrition, had a mean odds ratio that was only slightly lower than the overall distribution.

Quasi-experimental designs are vulnerable to selection bias, although a well-done quasi-experimental study can reduce the plausibility of selection bias as a rival explanation. An important issue at the meta-analytic level is whether we would expect a particular design, such as the use of historical controls, to yield a consistent bias across studies. For many of the comparison group types listed in Table 7, a consistent bias is plausible. For example, offenders who are not eligible for participation in a drug court, or offenders who decline participation, are likely to be less invested in abstinence from drug use than offenders graduating from the program. Thus, positive findings from these studies are difficult to interpret.

Arguably the strongest quasi-experimental comparison types among these studies are the historical controls and the eligible but non-referred drug offenders. The primary concern with the former is the possibility that the probability of criminal justice system detection (i.e., arrest) differs between the historical control period and the drug court period. Although it is unlikely to be identical, there is no reason to assume that the direction of any bias is consistent across studies. The results for the ten studies using historical controls are consistent with the overall findings of a moderately large positive effect. Eligible but non-referred drug offenders also provide a credible comparison group in some cases. For example, the comparison group for Brooklyn Drug Court in the Rempel et al. (2002) study consisted of offenders arrested in non-participating prosecution zones who were otherwise eligible. Studies using non-referred but eligible offenders for the comparison group also observed a moderate reduction in reoffending, although the mean odds ratio for all offenses was not statistically significant at a conventional level. The remaining comparison group types have a selection bias that likely would lead to higher rates of reoffending in the comparison group.

The quasi-experimental designs produced effect estimates that were slightly larger than those of the two high-quality randomized designs, suggesting an upward bias in these estimates. Thus, although the evidence is consistently positive, the methodological shortcomings of many of these studies weaken any conclusion regarding the general effectiveness of drug courts. The evidence is promising, but not convincing.

It is important to note that what happens to the comparison group also varies across studies. Typically, this was routine criminal justice system processes. Descriptively, very little information was provided by the studies on this issue. Offenders in these comparison groups may receive drug treatment. The primary distinction is that they are not receiving the drug court package that includes judicial monitoring, sanctions and rewards, referral to treatment, etc. The implication for the interpretation of the above results is that these are not treatment versus no-treatment studies. Something happens to the participants in the comparison, and that something may be effective at reducing future offending relative to a true no-treatment condition (i.e., the criminal justice system doing absolutely nothing). Thus, the effects tested above are relative to more routine

criminal justice system processing and represent the potential "value added" of drug courts.

Substantive variation

The large variability in effects across studies suggests that some drug courts are more effective than others. There are many potential characteristics of the drug offenders, drug court programs, treatment services, and community context that may affect the effectiveness of a drug court program (e.g., Turner et al. 2002; Goldkamp et al. 2001c; Longshore et al. 2001). As discussed earlier, we were limited by the descriptive information provided by the studies in the extent to which we could explore these potential moderators of drug court effectiveness, including the possibility of drug treatment in the comparison condition. We did, however, examine several substantive moderators with respect to their ability to explain differences across studies. Specifically, we examined participant age (juvenile or adult), the nature of the dependent variable (arrest, conviction, etc.), the nature of the reward for program completion, the use of formal assessment of treatment needs, and the use of a single treatment provider. These analyses were performed on the odds ratios representing all offenses and drug offenses. There were too few non-drug offenses odds ratios for meaningful analysis. The mean odds ratio and related statistics for each category of the substantive variables are presented in Table 8. All analyses, except the age breakout, included both the adult and juvenile drug court samples. The results are essentially the same if based solely on the adult drug courts.

Drug courts that used either the pre-plea model or the post-plea model had larger effects than those that used a mixed approach or an ad hoc approach in which the judge decided. The difference between mean odds ratio for the combined pre- and post-plea category and the combined ad hoc and mixed category was statistically significant. Drug courts using the mixed or ad hoc approach tended not to have an established "reward" for program completion, such as dismissal of charges. For example, the judge in the drug court evaluated by Miethe et al. (2000) decided whether to dismiss the charges, reduce the severity of the charges, or consider successful participation in the drug court as satisfying one condition of probation. Within the (Longshore et al. 2001) framework, these drug courts are low on the predictability dimension. Unfortunately, this finding does not appear to be robust in outcome type as it is not evident for the drug use effect sizes. Clearly, this aspect of a drug court and its relationship with effectiveness deserves future research attention. Overall, the findings tentatively suggest that a clear incentive for completion of the program requirements may be an important element of successful drug courts.

A consistent pattern across both outcome types, albeit statistically non-significant under a random-effects model (the fixed-effects models were statistically significant, however), was the slightly larger effects for drug courts that relied on a single treatment provider. Peyton and Gossweiler (2001) provided a plausible explanation for this finding. The 1999 National Drug Court Survey showed that the treatment provided to drug court participants was more likely to be based on cognitive–behavioral principles, or at least more closely aligned with these principles, when the drug court used dedicated providers. Cognitive–behavioral interventions have been shown to be

Table 8. Mean odds ratio by study substantive feature.

	All Offenses		*Drug offenses*	
Study feature	*Odds ratio*	k^a	*Odds ratio*	k^a
Age				
Juvenile	1.44	6	0.99	2
Adult	1.63*,[b]	48	1.71*	26
Dependent variable				
Arrest	1.63*,[b]	46	1.76*	24
Conviction	1.49*	8	1.48*	6
Self-report			1.83	1
Drug screen			0.80	3
Drug court model				
Pre-plea (diversion)	1.86*	16	1.61*	11
Post-plea (suspended sentence)	1.71*	11	1.64*	6
Mixed pre- and post-plea	1.08	2	1.08	2
Ad hoc (judge decides)	1.35	8	1.85*	6
Not reported	1.72*,[b]	18	1.72*	9
Single treatment provider				
Yes	1.73*	20	1.79*	12
No	1.60*,[b]	27	1.54*	17
Formal clinical assessment				
Yes	1.67*,[b]	31	1.62*	19
No	1.63*	19	1.69*	13

*$P \leq 0.05$, mean odds ratio statistically different from 1.
[a]Number of effect sizes contributing to each analysis.
[b]One outlier removed (a large positive effect).

effective in treating a wide range of criminal offenders (MacKenzie 2002). Single provider arrangements will always be dedicated, whereas multiple provider arrangements may or may not be so. Thus, the drug courts that use a single treatment provider may be more likely to have a dedicated provider relationship and, hence, more likely to provide cognitive–behavioral treatment. The dedicated provider relationship of the single provider model may also enhance treatment integrity.

Presumably, any positive effects achieved by a drug court are mediated to some extent by reductions in drug use by the participants. The vast majority of studies, however, relied on proxy measures of drug use behavior, such as arrest and conviction. These arrests and convictions may be for drug distribution or drug trafficking and, as such, are not a direct measure of use on the part of the study participant. Only four studies directly measured drug use, one through self-report (Harrell et al. 2001), and three through a drug-screening test (Brewster 2001; Dickie 2001; Rodriquez and Webb 2004). The former observed a mean odds ratio that was similar to the overall mean odds ratio for drug offenses. The odds ratio based on a drug-screening test produced a negative effect (higher levels of drug use

in the drug court sample than in the comparison sample). This is disconcerting, given that a primary focus of drug courts is the reduction of drug use. Because it is based on only three studies, each with clear methodological weaknesses, a strong conclusion cannot be drawn from this findings. Future evaluations should include drug screening as an outcome to enhance our knowledge in this area.

None of the remaining variables accounts for a meaningful portion of variability in the odds ratios. Most of the differences between the mean odds ratios reported in Table 8 are small (e.g., pre-plea versus post-plea court model), except for means based on only a few studies (e.g., the mean odds ratio for drug offenses for juvenile drug courts). This is likely to be due in part to the crude nature of these measures and the multiplicity of influences on the effectiveness of drug courts.

Effect decay over time

A concern raised in the literature (U. S. General Accounting Office 1997) was that the positive effects of drug courts might decay over time. This is based on the observation that many studies only examined reoffending during program participation, that is, while the participants were supervised by the drug court (or probation, in the case of the comparison condition) and involved in drug abuse treatment. Thus, reoffending may be lower for the drug court participants due to the court's supervision and drug abuse treatment. Offending, it was cautioned, may return to baseline levels following graduation from the program.

To test this hypothesis, we coded whether an outcome overlapped entirely with the program period (e.g., a 12-month period starting at adjudication for a study with a 12-month drug court program), overlapped partially with the program period (e.g., an 18-month period starting at adjudication for a study with a 12-month drug court program), or did not overlap (e.g., a 12-month period starting after the program). Exactly a third of the effect sizes in the all-offenses analyses and drug offenses were of each type. For the non-drug use offenses, a third overlapped with the program, half overlapped partially with the program, and a sixth were entirely post-program. Treating these categories as an ordinal scale, we regressed the logged odds ratios on the degree of overlap with the program period. For all three outcome types, the regression coefficient was positive, albeit statistically non-significant, suggesting that the observed reductions in offending for the drug court participants was not a function of the outcome measures overlap with drug court supervision and participation in treatment.

A weakness of the above analysis is the potential confounding of other study features with an outcome measure's degree of overlap with program participation. The analysis also did not explicitly examine follow-up length, although follow-up length was negatively correlated with degree of overlap. Four studies representing five drug court-comparison contrasts measured the same outcome at multiple follow-up points, including at least one follow-up post-program, and had the same sample size across these measurement occasions. Table 9 shows the odds ratio by months of follow-up for these studies and indicates the linear slope of the effect over time.

For three of these studies, the slope is negative for all outcomes, indicating a decay in the program effect over time, and, for one study, the slope is negative for all but one outcome. In contrast, Gottfredson et al. (2003) observed a slight

Table 9. Odds ratios by months of follow-up.

	Odds ratio by months of follow-up[a]						
Author, 0utcome	*12*	*18*	*24*	*30*	*36*	*48*	*Slope*
Gottfredson et al. (2003)							
Any arrest	1.6		2.2		1.9		+
Any conviction	1.1				1.3		+
Violence charge	2.4		1.7				−
Property charge	1.1		1.5		1.3		+
Drug charge	0.9		1.7		1.7		+
Harrell, Roman, & Sack							
Number of arrests	1.8		1.6				−
Logan et al.							
Felony charge	1.7					1.2	−
Felony conviction	1.7					1.7	+
Misdemeanor charge	1.9					1.2	−
Misdemeanor conviction	1.7					0.4	−
Peters & Murrin (Escambia)							
Arrest	3.0			1.8			−
Peters & Murrin (Okaloosa)							
Arrest	4.3			3.5			−

[a]Included only effects based on the same sample over time, with no attrition.

increase in the effect over time for all but one outcome (violence charge). For the any-arrest measure, the difference between the odds ratio for any arrest at 12 months (1.7) and 36 months (1.9) was slight but positive. For each study, the odds ratio at the longest follow-up point favored the drug court, with the exception of convictions for misdemeanor (Logan et al. 2001). Although there is evidence of some decay in effects over time, positive effects appear to remain at post-program time points. The overall positive findings from this meta-analysis do not appear to be the result of outcome measures during the program period.

Publication/selection bias

Publication/selection bias (the possibility that our search strategy was more likely to find studies with positive and statistically significant results than null or negative findings) is an additional potential source of bias in the above estimates of the effectiveness of drug courts. This is a common concern when one is conducting a meta-analysis and there are several strategies for addressing it. First, our search strategy placed emphasis on identifying unpublished studies. Roughly a third of the studies meeting our eligibility criteria have been published, and the results from the published studies were similar those from the unpublished studies, with no consistent positive bias for published studies relative to unpublished studies (1.57 versus 1.70 for all offenses, 1.42 versus 1.76 for drug offenses, and 1.77 versus 1.27 for the non-drug offenses, respectively). Second, we employed the trim-and-fill non-parametric method

of assessing publication bias (Duval and Tweedie 2000). This method assumes that, in the absence of publication bias, the scatterplot between effect size and standard error of the effect size will appear as a funnel and augments the data to achieve that shape. With this method, the distribution of odds ratios for all offenses was filled with ten effect sizes, reducing the overall mean odds ratio from 1.66 to 1.43. The latter remained statistically significant ($z = 5.13$, $P \leq 0.05$). Neither the drug offenses nor the non-drug offenses odds ratio distributions required filling. Taken together, the evidence suggests that, at most, any publication/selection bias in our data is slight.

Discussion

The findings presented above tentatively suggest that drug offenders participating in a drug court are less likely to reoffend than similar offenders sentenced to traditional correctional options, such as probation. This meta-analysis examined all available drug court evaluations that used a comparison group design and examined some form of criminal activity. The pattern of results across studies consistently favored the drug court over the comparison group participants; that is, the majority of studies observed reductions in reoffending among the drug court participants relative to the comparison participants. Translating the results into practical terms, we found that the reduction in overall offending was roughly 26% across all studies and 14% for the two high-quality randomized studies.

It is unfortunate that this large collection of studies leads to an equivocal statement on the effectiveness of drug courts. This equivocation is due to the generally weak nature of the research designs. Only five of the 55 drug court-comparison contrasts were constructed using random assignment methods, and two of these contrasts were seriously degraded. Roughly half the quasi-experimental designs made no attempt to statistically control for differences between drug court and comparison participants. A common comparison group, offenders who declined participation, may have a bias favoring the drug court condition. The higher quality quasi-experimental designs, however, produced findings consistent with the hypothesis that drug courts are effective, or at least that they can be effective.

Belenko (2001) also arrived at a tentative but optimistic conclusion regarding the effects of these programs on criminal behavior and drug use. Our meta-analysis extends Belenko's review by including substantially more studies (only 17 of our 50 studies overlapped with Belenko's review), and we applied the statistical methods of meta-analysis, allowing us to estimate both the overall effect across studies and the relationship between effectiveness and characteristics of the study methods and nature of the drug court.

Our results provide weak evidence that the nature of the drug court model affects drug court effectiveness. Pre- and post-plea drug court models that either dismissed charges or expunged a conviction from an offender's record upon graduation appeared more effective than courts with mixed approaches and no uniform incentive for the completion of the court's requirements. The pre- and post-plea may implement sanctions and rewards in a more consistent and certain

fashion. A finding by Young and Belenko (2002) supports this possibility. They compared drug offenders' perception of legal pressure and length of time spent in treatment under conditions of a highly structured drug court with clear judicial contingencies or regular probation. The drug court participants were more likely to believe that there would be consequences for failure to participate in treatment. Furthermore, drug court participants remained in treatment longer. Thus, a clear set of judicial contingencies can increase the amount of treatment received. A drug court that uses a single model with a standard incentive structure may be more effective in communicating these contingencies and their certainty to an offender.

Drug courts are typically embedded in a network of community services to which they refer their clients. The effectiveness of the drug court program depends, in part, on the effectiveness of the services provided to the drug court clients. The meta-analytic data suggests, albeit weakly, that drug courts that used a single drug abuse treatment provider had slightly larger effects, on average, than those drug courts that used multiple drug abuse treatment providers. Several explanations for this seem plausible. A single provider arrangement may increase the communication between the drug court and the service provider, enhancing the court's supervision of the drug offender's progress, including earlier detection of program failures, and facilitating the courts ability to dole out sanctions and rewards. Additionally, the court's oversight may increase treatment quality or influence the type of treatment provided. The 1999 National Drug Court Survey (Peyton and Gossweiler 2001) showed that "dedicated" service providers were more likely to use treatments based on cognitive–behavioral principles and to incorporate a treatment component that addressed criminological thinking. Cognitive–behavioral approach have generally been shown effective and often superior to alternatives (e.g., MacKenzie 2002; Taxman and Bouffard 2003). A study by Taxman and Bouffard (2003), however, suggests that providers of substance abuse treatment tend not to have a strong affiliation to a given therapeutic model. That is, although programs often state that they use cognitive–behavioral models, in practice they provide a more eclectic treatment that is not clearly cognitive–behavioral. Despite this, the single treatment provider approach may increase the coordination of services or help ensure that an effective set of services is provided. More research into the quality and nature of the treatment services provided to drug court clients is clearly needed. For a recent meta-analytic review differentiating effective from ineffective drug abuse treatment programs, see Prendergast et al. (2002).

We have argued above that the extant evidence is supportive of the hypothesis that drug courts are effective at reducing future drug use and other criminal behavior but that the evidence is not convincing from a social scientific standpoint. More simply, rival hypotheses for the observed positive findings cannot be fully ruled out. Policy decision making, however, is quite different from scientific decision making. The criminal justice system does not currently have the option of "doing nothing" with the large numbers of drug offenders arrested each year. Some action is required. In our opinion the evidence presented in this article suggests that the drug court approach is more likely to lead to the desired outcome of assisting these offenders in becoming drug-free and productive members of society than is the traditional approach. The latter typically involves prison or jail time, probation,

and possible referral to a substance abuse program, with few, if any, consequences for failure to attend and participate in a treatment program.

A value of synthesizing the extant literature within a given research domain is that it clarifies what we do not know, providing a sound basis for planning future studies. It should be clear from the discussion thus far that we believe that additional high-quality studies examining the effectiveness of drug courts are needed. Furthermore, studies need to better describe the drug court model that is actually implemented and to collect data on the nature of the treatment services provided. This information would provide insights into the relationship between these features and effectiveness. Additionally, studies that use drug screening as an outcome to assess the impact of these programs on actual drug use are needed, as few studies have used such measures and the effectiveness of drug courts on actual drug use behavior remains unknown. Finally, studies need to describe the nature of the comparison group in greater detail to allow for a better understanding of what is being tested by the study. Furthermore, this detail may allow future meta-analyses to better model the variability in results across studies.

Most of the existing drug court evaluations treat the drug court as a "black box" rather than explicitly testing for the effectiveness of specific elements. Goldkamp et al. (2001c) argued for the importance of examining the latter. An important question is whether the court supervision enhances the effectiveness of the rehabilitative services received by the offenders; that is, does it "galvanize the treatment process into a more powerful and accountable form of rehabilitation" (Goldkamp et al. 2001c, p. 29). Preliminary in-program results from a randomized study examining different schedules of judicial hearing has not found a beneficial effect of intensive judicial supervision (Marlowe et al. 2003). Additional studies of this type are needed. For example, a therapeutic jurisprudence hypothesis could be tested by randomly assigning drug offenders to a drug court program and an alternative that differed only in the absence of judicial hearings and the rewards and sanctions that the judge provided. Additionally, this meta-analysis raises the possibility that the nature of the relationship between the drug court and the service provider affects effectiveness, suggesting the need for research specifically examining the influence of different drug court and treatment provider arrangements on the type and quality of treatment.

Note

1 The equation for this computation is $p_a = 1 - \frac{o(p_b)}{1+o(p_b)-p_b}$ where p_b is the control group recidivism rate, o is the odds ratio, and p_a is the drug court recidivism rate.

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