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Drug Courts' Effects on Criminal Offending for Juveniles and Adults

Ojmarrh Mitchell, David B. Wilson, Amy Eggers, Doris L. MacKenzie

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Corresponding author	Ojmarrh Mitchell, Ph.D. Assistant Professor Department of Criminology 4202 E. Fowler Ave. SOC107 University of South Florida Tampa, FL 33620 omitchell@usf.edu

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The Campbell Collaboration
P.O. Box 7004 St. Olavs plass
0130 Oslo, Norway
www.campbellcollaboration.org

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Abstract

BACKGROUND

Drug courts are specialized courts in which court actors collaboratively use the legal and moral authority of the court to monitor drug-involved offenders' abstinence from drug use via frequent drug testing and compliance with individualized drug treatment programs. Drug courts have proliferated across the United States in the past 20 years and been adopted in countries outside the United States. Drug courts also have expanded to non-traditional populations (juvenile and DWI offenders).

OBJECTIVES

The objective of this review is to systematically review quasi-experimental and experimental (RCT) evaluations of the effectiveness of drug courts in reducing recidivism, including drug courts for juvenile and DWI offenders. This systematic review critically assesses drug courts' effects on recidivism in the short- and long-term, the methodological soundness of the existing evidence, and the relationship between drug court features and effectiveness.

SEARCH STRATEGY

We used a multi-pronged search strategy to identify eligible studies. We searched bibliographic databases, websites of several research organizations involved in drug court research, and the references of eligible evaluations and prior reviews.

SEARCH CRITERIA

Evaluations eligible for inclusion in this review were evaluations of drug courts that used an experimental and quasi-experimental comparison group design. Studies must also have had an outcome that examined criminal or drug-use behavior (recidivism).

DATA COLLECTION AND ANALYSIS

From each evaluation, we coded an effect size that quantified each court's effect on various measures of recidivism (general recidivism, drug-related recidivism, and drug use). We also coded features of the drug court program, research methodology, and sample. We analyzed effect sizes using the random-effects inverse-variance weight method of meta-analysis.

MAIN RESULTS

One hundred fifty-four independent evaluations of drug courts met our eligibility criteria; 92 of these assessed adult drug courts, 34 examined juvenile drug courts, and 28 investigated DWI drug courts. If all of the evaluations are considered, the evidence suggests that adult and DWI drug courts reduce general and drug-related recidivism; in fact, the mean effect size for both adult and DWI drug courts is analogous to a drop in recidivism from 50% for non-participants to approximately 38% for participants. Moreover, the effects of adult drug courts appear to persist for at least three years. If only the three experimental evaluations of adult drug courts are considered, the evidence still supports the effectiveness of adult drug courts, as all three experimental evaluations find sizeable reductions in recidivism, although there was inconsistency in the durability of the effects over time. Three of the four experimental evaluations of DWI drug courts find sizeable reductions in recidivism; however, one experimental evaluation found a negative effect. Thus, the evidence is suggestive of effectiveness of DWI drug courts but this conclusion is not definitive. For juvenile drug courts we find considerably smaller effects on recidivism. The mean effect size for these courts is analogous to a drop in recidivism from 50% for non-participants to roughly 43.5% for participants.

CONCLUSIONS

These findings support the effectiveness of drug courts in reducing recidivism, but the strength of this evidence varies by court type. The evidence finds strong, consistent recidivism reductions in evaluations of adult drug courts. DWI drug courts appear to be strong but this evidence is less consistent, especially in experimental evaluations. More experimental researching assessing the effects of DWI drug courts is clearly needed. For juvenile drug courts, the evidence generally finds small reductions in recidivism. More evaluations of juvenile drug courts, especially experimental and strong quasi-experimental evaluations, are needed.

PLAIN LANGUAGE SUMMARY

Drug courts are specialized courts in which court actors collaboratively use the legal and moral authority of the court to monitor drug-involved offenders' abstinence from drug use via frequent drug testing and compliance with individualized drug treatment programs. The objective of this review was to systematically review quasi-experimental and experimental evaluations of the effectiveness of drug courts in reducing future offending and drug use. The systematic search identified 154 independent, eligible evaluations, 92 evaluations of adult drug courts, 34 of juvenile drug courts, and 28 of drunk-driving (DWI) drug courts. The findings most strongly support the effectiveness of adult drug courts, as even the most rigorous evaluations consistently find reductions in recidivism and these effects generally persist for at least three years. The magnitude of this effect is analogous to a drop in general and drug-related recidivism from 50% for non-participants to approximately 38% for participants. The evidence also suggests that DWI drug courts are effective in reducing recidivism and their effect on recidivism is very similar in magnitude to that of adult drug courts (i.e., a reduction in recidivism of approximately 12 percentage points); yet, some caution is warranted, as the few available experimental evaluations of DWI drug courts do not uniformly support their effectiveness. For juvenile drug courts we find considerably smaller effects on recidivism. The mean effect size for these courts is analogous to a drop in recidivism from 50% for non-participants to roughly 43.5% for participants

1 Background

The drug court model combines drug treatment with the legal and moral authority of the court in an effort to break the cycle of drug use and drug related crime (GAO, 1997; National Association of Drug Court Professionals, 1997). Briefly, a prototypical drug court operates as follows (National Association of Drug Court Professionals, 1997; Mitchell, 2011): Shortly after arrest, drug-involved offenders who appear to be eligible for drug court participation are identified and screened for program eligibility. Arrestees deemed eligible are offered entry into the drug court with an agreement that the charges against them will be reduced or dismissed upon successful program completion. Arrestees who agree to enter the drug court become drug court "clients." Once in the drug court, clients have their cases handled non-adversarially in one of two ways. In the "pre-plea" case processing method clients waive their right to a speedy trial and enter drug court; if they successfully complete court requirements, then their charges are dropped. In the "post-plea" case processing method, clients are admitted to drug court after conviction but before sentencing. Clients who successfully complete the program typically receive a sentence of time served or probation.[1] As a condition of program entry, drug court clients agree to abide by the court's demands, which typically include frequent urine testing, treatment attendance, and appearance before the court for status hearings. These status hearings are crucial as it is here that the drug court judge and clients converse directly, and it is in these hearings where judges in collaboration with other court actors most clearly use the authority of the court to motivate compliance. The court uses various rewards (e.g., praise, tokens of achievement, movement to the next phase of the program) and sanctions (e.g., increased treatment attendance or urine testing, short jail stays) to compel compliance to program requirements. Compliant clients advance through three or more, progressively less intense stages before

[1] Some courts use both methods of case disposition for different groups of offenders. For example, Delaware used the diversionary approach with youthful, less criminally involved offenders, and the post-plea approach with more serious offenders. Those on the diversionary track were required to participate in drug court for six to twelve months; whereas, the more serious offenders on the post-plea track were required to be involved for longer periods of time.

completing the drug court, which typically takes at least one year. Ultimately, successful drug court clients are acknowledged at a formal graduation ceremony.

Drug court eligibility requirements vary across the thousands of jurisdictions operating such courts. In the majority of jurisdictions, however, eligibility is restricted to non-violent offenders with evidence of substance dependence (Belenko, 1998). Most commonly, non-violent offenders are defined as offenders neither charged with, nor previously convicted of, a serious violent offense. While not all jurisdictions restrict eligibility to non-violent offenders, the vast majority of drug courts do, in part because this criterion is necessary to be eligible for federal drug court funding.[2] Many courts also exclude arrestees charged with drug trafficking offenses, with three or more prior felony convictions, or with serious mental health issues (see e.g., Kalich & Evans 2006). In the end, most eligible offenders are charged with drug or property offenses and have relatively few prior felony convictions.

It is important to emphasize that the program requirements for drug courts are often strict and clients are closely monitored for adherence to the demands of the program. Perhaps the best evidence of the rigor of the drug court model is the high percentage of drug court clients who fail to graduate from these programs. For example, a GAO survey of drug courts operating at the end of 1996 found that "about 48%" of drug court clients successfully completed the program (GAO 1997, p. 56). Similarly, Belenko's (2001) review of drug court evaluations found an average graduation rate of 47% with a range of 36% to 60%. Thus, the best estimate of drug court graduation rates is just under 50%.

Taken together, the key components of drug courts are: (1) collaborative, non-adversarial, outcome driven court processing, (2) early identification of eligible offenders; (3) drug treatment integrated into criminal justice case processing; (4) urine testing; (5) judicial monitoring; and, (6) the use of graduated sanctions/rewards (National Association of Drug Court Professionals, 1997; Hora, 2002). These components combine to form individualized interventions that simultaneously provide drug treatment to drug abusing offenders and hold them accountable for their behavior.

In just two decades, drug courts have gone from a single court in one jurisdiction to an international movement with thousands of courts in operation. Dade County Florida was the first jurisdiction to develop and adopt the drug court model described above. Dade County's innovative drug court was viewed as a success and its approach has been widely adopted. In 1994, five years after the initial drug court was opened for operations, 40

[2] According to a 1997 GAO report (1997, p. 38), approximately 80% of drug courts operating at the end of 1996 received federal funds; and therefore, at least 80% of drug courts restrict eligibility to non-violent offenders.

drug courts were in operation. Five years later, 472 courts were operating. By 2004, the number of operating drug courts stood at over 1,600 (Huddleson, Marlowe, & Casebolt, 2008). The most recent data indicate that there were over 2,400 drug courts in operation in the United States (Huddleson & Marlowe, 2011). Drug courts have also spread to other nations such as Canada, the United Kingdom, New Zealand, Australia, South Africa, Bermuda, and Jamaica (Berman & Feinblatt, 2005).

Not only have the number of drug courts increased, but also drug courts have increased in kind. Originally, drug courts were local courts that primarily served adult offenders with illicit substance abuse problems ("adult drug courts"). In recent years, however, drug courts for juvenile offenders and offenders charged with driving while under the influence (DWI) of alcohol have been opened and proliferated. Currently, there are 476 juvenile and 172 DWI drug courts in operation (Huddleston & Marlowe, 2011). Further, the drug court model also has begun to make inroads in federal and tribal jurisdictions (Huddleston et al., 2008).[3]

It is interesting, albeit common in the criminal justice system, that drug courts' initial expansion occurred without a solid body of empirical evidence establishing their effectiveness in reducing criminal behavior. In fact, an early review of the drug court literature conducted by the U. S. General Accounting Office (1997) concluded that the existing evidence was insufficient to draw any firm conclusion on the effectiveness of these programs with respect to recidivism. More specifically, the U. S. General Accounting Office (1997) identified several limitations of the 20 evaluations examined, including a failure to examine 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)

A later review by 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 of the evaluations reviewed by Belenko examined drug use or other

[3] The drug court model also has been applied outside of criminal courts; family drug courts are relatively new advents that handle family court issues (e.g., parental rights, allegations of neglect) in cases in which drug abuse is determined to be a factor.

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).

In 2005, the GAO updated its review of drug court evaluations (U. S. General Accountability Office, 2005). This review examined 27 evaluations that used comparison groups (i.e., two-group designs) and this time concluded that the evidence indicates drug courts reduce recidivism in the period of time corresponding to the drug court treatment, but drug courts' effects on recidivism beyond this period and on drug use were questionable. This study also reviewed four cost-benefit evaluations and concluded that drug courts do yield a net benefit.

Most recently, Wilson, Mitchell, and MacKenzie (2006) synthesized the findings of 55 drug court evaluations. These authors tentatively concluded that drug court participants have lower rates of recidivism (drug and non-drug offending) than similar offenders who did not participate in drug courts. These findings held for evaluations that measured recidivism during and after program participation. Like the earlier reviews, these findings were tempered by the generally weak methodological rigor of the evaluations.

Taken together, existing reviews of drug court evaluations reveal growing support for the effectiveness of drug courts, especially during the period of program participation. Many questions, however, remain unresolved. First, it is still uncertain whether drug courts' effects reliably persist beyond the period of program participation. Second, it is unclear whether juvenile drug courts and DWI drug courts are effective in reducing recidivism. Third, and most important, it is unclear which drug court features are associated with greater effectiveness in reducing recidivism. Drug courts differ in approach and structure and these differences may influence effectiveness. Longshore et al. (2001) provide a useful conceptual framework for thinking about variation in drug courts. They hypothesize that the most effective drug courts: (1) use the courts' *leverage* (rewards and sanctions) to motivate offender change; (2) serve *populations* with less severe problems; (3) have high *program intensity*; (4) apply rewards and sanctions *predictably*; and, (5) emphasize offender *rehabilitation* as opposed to other court goals like speedy case processing and punitive sanctioning. This framework provides a potentially useful framework for attempting to understand variation in drug court effectiveness. Fortunately, numerous new evaluations have been completed in recent years. This ever-expanding body of research permits an examination of these unresolved issues.

2 Objectives

Our objective was to systematically review quasi-experimental and experimental evaluations of the effectiveness of drug courts with respect to future criminal offending and drug use behavior. This systematic review focused on estimating the effectiveness of these programs relative to "standard" criminal justice system case processing. The review critically assessed drug courts' effects on recidivism in the short- and long-terms, the methodological soundness of the existing evidence, and the relationship between drug court features and effectiveness (i.e., what court features are associated with greater reductions in recidivism?).

3 Methods

3.1 CRITERIA FOR INCLUSION AND EXCLUSION OF STUDIES IN THE REVIEW

The population of evaluations eligible for this review was experimental and quasi-experimental evaluations of drug courts that utilized a comparison group. In brief, the criteria for inclusion were that: (1) the evaluation examined a drug court program (defined as specialized courts for handling drug-involved cases that are processed in a non-adversarial manner, refer offenders to appropriate treatment programs, regularly test offenders for drug use, and have a judge who actively monitors progress and provides sanctions for misbehavior); (2) the evaluation included a comparison group that was treated in a traditional fashion by the court system (e.g., probation with or without referral to treatment); (3) the evaluations 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); and, (4) enough information was reported to compute an effect size. The form for establishing study eligibility is provided in Appendix A.

In regards to the first eligibility criterion, our operational definition of a "drug court" included specialized courts or court dockets that fit the five essential features of drug courts listed above (see pages 6-7). The task of identifying drug courts was made easy by the fact that nearly all of the interventions meeting this criterion self-identified as an evaluation of a "drug court." The only ambiguity regarding this criterion concerned speedy case processing drug courts (e.g., see Belenko, Fagan, Dumanovsky, and Davis, 1993) and evaluations of the Breaking the Cycle (BTC) demonstration project (e.g., see Harrell, Mitchell, Hirst, Marlowe, and Merril, 2002). Speedy case processing drug courts were ruled ineligible because they focused on expedited case processing of drug cases, not substance abuse treatment with judicial monitoring of drug-involved cases. Evaluations of the BTC demonstration project were also ruled ineligible. While this intervention was based on the drug court model, a judge generally did not actively

monitor clients and most clients, rarely, if ever had a status hearing.[4] Perhaps the clearest indication that BTC demonstration project was not a drug court program is the fact that a recent multi-site evaluation of drug courts used Pierce County's BTC program as a non-drug court, comparison program (see Rossman, Rempel, Roman, Zweig, Lindquist, Green, Downey, Bhati, and Farole, 2011).

The second criterion specified that all included evaluations must have a comparison/control group that received standard criminal justice processing. Essentially, this criterion required all evaluations to have a comparison and the comparison group received no drug treatment or minimal drug treatment. We excluded evaluations that compared drug court treatment to another drug treatment program of similar intensity (i.e., treatment-treatment comparisons or dose-response evaluations). Furthermore, we excluded evaluations in which the comparison group was comprised predominantly or solely of dropouts from the drug court. Evaluations that utilized program drop-outs as the comparison group were excluded because these represent a particularly weak research design with a serious threat to internal validity. Those who drop-out from a drug-court are likely to differ from those who remain in a drug court on a host of important variables, some observable and others not (see e.g., Mateyoke-Scrivner, Webster, Staton, Leukefeld, 2004).

The third and fourth criteria are largely self-explanatory. Simply put, eligible evaluations could use a wide variety of recidivism measures from official measures such as arrest, conviction, and re-incarceration to self-report measures of criminal offending. Last, all eligible evaluations had to report sufficient information for effect size calculation.

3.2 SEARCH STRATEGY FOR IDENTIFICATION OF RELEVANT STUDIES

The goal of the search strategy was to identify all evaluations, published or unpublished, meeting the above eligibility criteria. In order to achieve this objective, a multi-pronged search strategy was utilized. The search began by conducting a computerized keyword search of bibliographic databases. In particular, we conducted a search of the following databases: NCJRS, Criminal Justice Abstracts, Dissertation Abstracts, PsycINFO, Sociological Abstracts, Social Science Citation Index, Sciences Citation Index Expanded, Arts & Humanities Citation Index, Conference Papers Index, Ingentaconnect, C2 SPECTR, and CINAHL, as well as Google internet searches. The keywords used were: drug court (drug court*), DWI court (DWI court*), DUI court (DUI court*), evaluation,

[4] Note that the lead author was part of the team that evaluated BTC.

recidivism, re-arrest, and re-conviction. Each of the first three keywords was used in combination with each of the four latter terms.

We also searched for eligible evaluations by carefully reading retrieved studies and existing reviews of drug court research for unfamiliar evaluations. In particular, we reviewed the reference lists of existing reviews of drug court evaluations to identify eligible evaluations (Belenko 2001, GAO, 1997; 2005; Latimer, Morton-Bourgon, & Chretien, 2006; Marlowe, Festinger, Arabia, Croft, Patapis, & Dugosh, 2009; Shaffer, 2006, 2011) Likewise, many of the included evaluations reviewed prior drug court research. Unfamiliar studies referenced by eligible evaluations were also assessed for eligibility.

Further, we searched websites of several prominent research organizations. Specifically, we searched for relevant research reports on the following websites: NPC Research (http://www.npcresearch.com/); National Drug Court Institute (http://www.ndci.org/ndci-home/); the Drug Court Clearinghouse via American University's Justice Programs Office webpage (http://www1.spa.american.edu/justice-old/index.php); RAND Drug Policy Research Center (http://www.rand.org/multi/dprc/); The Urban Institute's website (http://www.urban.org/index.cfm); and, the University of Cincinnati's School of Criminal Justice publications page (http://www.uc.edu/ccjr/reports.html).

All studies that appeared to be eligible based on a preliminary review of the title and abstract were retrieved and closely scrutinized to determine final eligibility status. Specifically, we reviewed the title and abstract of each search result for clear evidence of ineligibility. Those studies that could not be ruled as ineligible based on the title/abstract review were retrieved for further assessment of eligibility. Retrieved studies were read closely to determine final eligibility status.

Note that the last search was conducted in August 2011. Given our extensive search we are confident that we have identified the overwhelming majority of eligible evaluations. It is possible, however, that studies made available close to August 2011 may have been missed, as there often is a lag between the availability of a study and its appearance in bibliographic databases and search engines.

3.3 DESCRIPTION OF METHODS USED IN THE COMPONENT STUDIES

The basic research design used in eligible evaluations was a two-group (treatment and comparison group) design with a post-release outcome measure of interest, such as post-

release criminal arrest or drug use. The comparison groups employed in eligible evaluations were constructed using a variety of methodologies including historical comparisons, drug-involved probationers from nearby jurisdictions, drug-involved offenders eligible for the treatment program who chose not to participate, eligible offenders who did not participate for a variety of reasons (e.g., not referred, rejected by program administrators), and random assignment. The included evaluations also varied widely in regards to the degree to which they employed statistical controls (matching, covariate analysis, etc.) to reduce the threat of selection bias. Our coding forms were designed to capture these methodological variations.

3.4 CRITERIA FOR DETERMINATION OF INDEPENDENT STUDIES

Several types of statistical dependencies were evident in evaluations of drug court programs. One common dependency was created by multiple measures of criminal behavior (e.g., re-arrest, re-conviction, drug use) or multiple follow-up periods for the same indicator of criminal behavior ups (e.g., 6 months, 12 months). Another common dependency was produced by multiple studies reporting findings from the same sample of research participants.

The statistical methods detailed below required statistical independence. In other words, each evaluation had to use a unique research sample. We utilized several strategies to maintain the statistical independence. First, all evaluations (i.e., treatment/comparison contrasts) were cross-checked against one another to ensure that multiple studies reporting the results of the same evaluation do not contribute multiple estimates of program effects to any analysis. Second, in evaluations that report multiple measures of criminal behavior, rather than averaging these multiple outcomes, we applied a set of selection criteria that created three data sets of effect sizes, with a particular evaluation contributing only one effect size to each of the data sets.[5] In the first data set preference was given to effect sizes that: (1) were general (i.e., covered all offense types as opposed to being offense specific), (2) were based on arrest, (3) were dichotomous, and (4) followed sample members for 12 months. We preferred effect sizes meeting these criteria, because such effect sizes were the most commonly reported outcome measure. The goal here is to calculate one effect size from each evaluation meeting these criteria to maximize the comparability of recidivism measures across evaluations. If no such effect size was available, we selected the effect size that most closely matched these criteria. For example, property offenses were more general than violent offenses, effect sizes based on

[5] If multiple effect sizes from were available from one evaluation and no one effect size met the selection criteria, as a last resort the available effect sizes were averaged.

re-convictions were preferred over re-incarcerations, and effect sizes following sample members closest to 12 months were preferred over other effect sizes. Each independent evaluation contributed one, and only one, effect size to this "general recidivism" data set. This general recidivism data set served as the main data set in the analyses that follow.

We also created two other more specific data sets: one data set for measures of drug related recidivism (e.g., arrest or conviction on drug charges), and another data set for measures of drug use (via self-reports or drug tests). In creating these data sets we had considerably fewer effect sizes to choose from. When multiple effect sizes were available for any of these data sets, preference was given to effect sizes that: (1) were more general (e.g., encompassed multiple types of non-drug offending, instead of one specific type of non-drug offending), (2) were dichotomous, and (3) followed sample members for 12 months. If an evaluation did not report one of these specific types of outcomes, then that evaluation did not contribute to the particular data set.

3.5 DETAILS OF STUDY CODING CATEGORIES

The coding forms employed in this review are provided in Appendix B. These coding forms were structured hierarchically, in order to explicitly recognize the nested nature of effect sizes within studies. Any number of effect sizes could be coded from each evaluation using these forms [see Lipsey and Wilson (2001) for a discussion of this issue].

The coding forms captured key features of the nature of the treatment, research participants, research methodology, outcome measures, and direction and magnitude of observed effects. Two coders assessed each study. Discrepancies between coders were resolved by the lead author.

3.6 STATISTICAL PROCEDURES AND CONVENTIONS

An effect size was calculated for each evaluation contrast. We utilized the odds-ratio effect size, as this type of effect size is the most appropriate effect size for dichotomous outcomes, like our preferred recidivism measure (Lipsey and Wilson, 2001). When continuous outcome measures best fit the effect size selection criteria (see section 3.4), the standardized mean difference effect size was used. These effect sizes were coded in a manner such that positive effect sizes indicate the treatment group had a more favorable outcome than the comparison group (i.e., less recidivism or drug use).

Odds-ratio effect sizes and standardized mean difference effect sizes were combined using the methods developed by Hasselblad and Hedges (1995). Specifically, mean difference effect sizes were transformed to odds-ratios.

Our analyses of these effect sizes utilized the statistical approach outlined by Lipsey and Wilson (2001). In particular, we used the inverse variance method and assumed that the true treatment effects varied as a function of both measured (i.e., coded study features) and unmeasured differences between studies, that is, a random-effects model.

Our analyses employed Stata macro programs written by David B. Wilson.[6] These macro programs calculated the random effects variance component discussed above and computed various statistics such as the overall mean effect and the homogeneity of effects statistic. We also used these macro programs to determine which study features were associated with observed study effects via meta-analytic analogs to analysis of variance and regression, assuming a mixed-effects model estimated via full-information maximum-likelihood (Raudenbush, 1994; Overton, 1998).

3.7 TREATMENT OF QUALITATIVE DATA

We did not include qualitative research in this systematic review. In future updates to this systematic review, however, we are open to suggestions from and collaboration with researchers specializing in such techniques.

[6] As of this writing, these macro programs are available to the public at: http://mason.gmu.edu/~dwilsonb/ma.html

4 Findings

4.1 DESCRIPTION OF ELIGIBLE STUDIES

Our search found 370 potentially eligible studies. We retrieved 365 of the potentially eligible studies for further review. (The remaining five studies cannot be located.) Of the retrieved studies, 181 were eligible for this systematic review. Many of the eligible studies, however, utilized overlapping samples or the same sample in initial and follow-up evaluations of one drug court. These 181 studies yielded 154 independent evaluations of drug court programs. These 154 eligible and independent evaluations form the sample for this systematic review.

Tables 1-3 provide descriptive information on the evaluations and drug courts examined in this review. Most of the drug court evaluations examined the effectiveness of adult drug courts (i.e., drug courts designed for adult illicit substance users). Ninety-two of the 154 evaluations (60%) assessed adult drug courts. Another 34 (22%) evaluations examined juvenile drug courts and the remaining 28 (18%) evaluations probed DWI (driving while intoxicated) courts.

All but eight of the eligible, independent evaluations examined U.S. drug courts. Four of the remaining evaluations examined adult drug courts in Australia. Two evaluations were of Canadian drug courts (Toronto and Vancouver). One evaluation assessed a juvenile drug court in New Zealand, and another examined an adult drug court in Guam. A majority of the evaluations have been conducted since 1999, enhancing the external validity of these evaluations to the contemporary criminal justice context, at least in the United States.

Overall, this body of literature is methodologically weak with few randomized studies of each type of drug court and only a modest number of rigorous quasi-experimental studies of adult drug courts and juvenile drug courts. This will be discussed in greater detail below in the context of the study findings.

The evaluations exhibited relatively little variation in terms of sample characteristics (see Table 3). Overwhelmingly, the samples used in these evaluations were composed predominantly of males. The vast majority of courts restricted eligibility to non-violent offenders. Approximately 25% of samples used in adult drug courts had minor criminal history; whereas, less than 10% of the samples in juvenile and DWI drug courts had minor criminal history, that is, they were mostly first-time offenders.

4.2 OVERALL MEAN EFFECTS BY TYPE OF DRUG COURT

We calculated effect sizes that measure drug courts' effects on three outcomes: (1) general recidivism (typically measured as re-arrest for any offense), (2) drug related recidivism (typically measured as re-arrest for a drug offense), and drug use (usually measured as self-reported drug use or via urinalysis), for each type of court. Table 4 displays the mean odds-ratio for each court type (adult, juvenile, and DWI) by outcome type (general recidivism, drug recidivism, and drug use). The forest plots for the general recidivism effects are shown in Figures 2 through 4. The forest-plots show a clear pattern of evidence favoring drug courts, with most studies observing effects favoring the drug court (88%, 70%, and 85%, for adult, juvenile, and DWI, respectively). The overall mean odds-ratio for the general recidivism measure is small to moderate in size and statistically significant for all three court types (mean odds-ratio of 1.66, 1.37, and 1.65, for adult, juvenile, and DWI, respectively). Relative to a 50% recidivism rate in the comparison group (a typical value), these odds-ratios translate into recidivism rates for the respective drug court groups of 37.6%, 42.2%, and 37.7%. Thus, on average participants in adult and DWI drug courts have recidivism rates approximately 12 percentage points lower than non-participants, while on average participants in juvenile drug courts have recidivism rates approximately 8% lower than non-participants.

The effects of these courts on drug related recidivism (i.e., drug related crimes) are very similar for adult and DWI drug courts with random effects odds-ratios of 1.70 and 1.65, respectively. However, for juvenile drug courts, the results on drug related recidivism outcomes were less encouraging. The mean odds-ratio was 1.06. In practical terms, this odds-ratio is essentially null, or a no-difference effect. Thus, the current evidence raises the possibility that juvenile courts are not effective in reducing drug related crime or that any effect that is produced is small.

Surprisingly few evaluations assessed the effect of drug court participation on measures of actual drug use (i.e., urinalysis or self-reported drug use). We found only nine independent evaluations that reported useable post-program entry measures of drug use for both drug court participants and non-participants. Four of the nine effect sizes assessed the effect of participation in adult drug court programs, three were from

evaluations of juvenile drug courts, and the two remaining effect sizes were from evaluations of DWI drug courts. For each type of court, the average effect size was positive, indicating reduced drug use for drug court participants in comparison to non-participants. However, because of the small number of drug use effect sizes, the average effect size for each type of court was not statistically significant.

Although the evaluation of all three types of drug courts indicate that drug court participants have, on average, lower rates of general and drug recidivism than non-drug court participants, these findings need to be interpreted within the context of the methodological rigor of the studies. As discussed below, this body of literature is generally weak methodologically.

4.3 ROBUSTNESS OF FINDINGS TO METHODOLOGICAL WEAKNESSES

Evaluations were placed into four categories: (1) weak quasi-experiments, (2) standard quasi-experiments, (3) rigorous quasi-experiments, and (4) randomized experiments. Rigorous quasi-experiments typically used subject-level matching on key variables or propensity score matching. Standard quasi-experiments typically used either a historical comparison group that met drug court eligibility criteria constructed from archival data or a group of offenders who were eligible but not referred to the drug court program. The critical feature here is that the participants did not self-select into the drug court or comparison condition. Weak quasi-experimental designs typically involved comparing drug court clients to drug offenders who were eligible for participation in a drug court but declined participation ("refusers") or were referred to the drug court but were rejected by drug court administrators ("rejects"). Such designs have questionable internal validity because refusers and rejects are likely to differ on factors like pre-treatment motivation, perceived seriousness of drug problem, and self-efficacy, among many other potentially important factors. It's important to note that evaluations that used refusers or rejects as the comparison group but included efforts to minimize selection bias (e.g., controlled for many variables in the analyses) were given higher ratings depending on the nature of the efforts to minimize selection bias.

Tables 5-7 display the mean effect size by methodological features for general recidivism and drug recidivism. Focusing on adult drug courts, we find that the magnitude of the estimated effects of participation in adult drug courts generally declines as methodological rigor increases, but this relationship is not statistically significant. In particular, evaluations rated as weak or standard quasi-experiments found sizeable and statistically significant effects of drug court participation on measures of recidivism. These evaluations, however, are plagued by a substantial threat of selection bias. The

clearest evidence of the effectiveness of adult drug courts is provided by evaluations rated as strong (rigorous) quasi-experiments. The mean effect sizes for these evaluations are of a meaningful size and statistically significant.

Of concern is the smaller average effects found in the three most rigorous, experimental evaluations of adult drug courts. In fact, for both general recidivism and drug recidivism, the mean odds-ratio for experimental designs is not statistically significant across the three experimental evaluations, and near the null value for drug related recidivism. These findings from these studies deserve greater exploration. The three randomized experimental evaluations of adult drug courts included in this synthesis evaluated: (1) a drug court in Maricopa County (Deschenes, Turner, & Greenwood, 1995; Turner, Greenwood, Fain, & Deschenes, 1999); (2) Baltimore City's drug court (Gottfredon, Najaka, & Kearley, 2003; Gottfredon, Najaka, Kearley, & Rocha, 2006); and, (3) a drug court in New South Wales, Australia (Shanahan, Lancsar, Haas, Lind, Weatherburn, & Chen, 2004). All three evaluations reported findings on the effectiveness of drug court participation on measures of general and drug related offending in the first 12 months after program entry, which are the effects included in our primary analyses. The results for these experimental evaluations are inconsistent (heterogeneous), with two of the three evaluations finding modest positive results (odds-ratios of 1.65 and 1.82), and one evaluation (Deschenes et al., 1995) with a near null effect (1.06) on the general recidivism outcome measure, and two of the three studies finding small negative effects on the drug recidivism outcome measure (Deschenes et al., 1995, and Gottfredson et al., 2003). Thus, the finding that experimental evaluations of adult drug courts do not collectively find statistically significant reductions in recidivism is driven by inconsistent results across the three evaluations and the small number of experimental evaluations (i.e., low statistical power), which is evidenced by the rather large confidence intervals around the mean effect sizes.

All three evaluations have unique characteristics; however, the Maricopa County program had the unusual characteristic of comparing the drug court participants to a control group of offenders involved in a drug-testing program. In fact, the drug-testing control group was drug tested more often than the drug court group—a highly unusual finding in this body of research. Consequently, the evaluation of Maricopa County lacked a key feature of drug court evaluations, a treatment group that received more frequent drug testing that the comparison group. This fact, among others, makes this evaluation problematic. An important issue concerns the effect of this unique evaluation on the results reported in Table 5. We find that if this evaluation is removed for the analysis, the mean odds-ratio for experimental evaluations on the general recidivism outcome measure is 1.73 (95% C.I. of 1.18 to 2.53), which is statistically significant and larger than the effect for the rigorous quasi-experimental designs. The mean odds-ratio for the drug

recidivism measure remains essentially unchanged (1.05, with a 95% confidence interval of 0.69 to 1.60). Another complication with the evaluation of the Maricopa County program is that the results changed substantively in a follow-up evaluation that reported outcomes three-years after program entry (Turner et al., 1999). In this subsequent evaluation, participants in the Maricopa County drug court program had significantly less recidivism (general and drug) than the control group. If the effect sizes from this subsequent evaluation are used instead of those from the original evaluation, then the mean odds-ratio for experimental evaluations is 1.65 (95% C.I. of 1.25 to 2.18) for general recidivism, which is statistically significant and virtually identical to the mean effect size for all evaluations of adult drug courts, and mean odds-ratio for drug recidivism is 1.19 (95% C.I of 0.82 to 1.73), which is not statistically significant. Thus, the results of the Maricopa County evaluation materially affect the size and statistical significance of the mean odds-ratio for general recidivism, but not drug recidivism.

We draw three conclusions from the above analyses. First and foremost, all three experimental evaluations of adult drug courts provide evidence of these courts' effectiveness in reducing recidivism. Two of the three evaluations find recidivism reductions in the first year after program entry, and the remaining evaluation finds recidivism reductions at three years post-program entry. Second, because the vast majority of adult drug court evaluations, even the most rigorous evaluations, find moderate reductions in general recidivism, we believe that the evidence indicates that adult drug courts reduce recidivism. Third, the mean odds-ratio effect size measuring these courts effect on general recidivism appears to be approximately 1.65, which translates into an average recidivism rate of 38% for drug court participants, if we assume a 50% recidivism rate for non-participants.

In regards to evaluations of juvenile drug courts, we find that these courts have small effects on recidivism, especially in methodologically rigorous evaluations. The strongest evidence of the effectiveness of juvenile drug courts comes from weak quasi-experimental evaluations, as in these evaluations the general recidivism mean odds-ratio is relatively large (1.85) and statistically significant. However, the general recidivism mean odds-ratios are considerably smaller in evaluations with higher levels of methodological rigor. For standard quasi-experimental evaluations, the mean odds-ratio is 1.32 (95% C.I. of 1.07 to 1.62). The mean odds-ratios for strong (rigorous) quasi-experimental and experimental evaluations are similar in magnitude 1.32 and 1.22, respectively; neither is statistically significant. However, if the rigorous quasi-experimental and experimental evaluations are combined, the mean effect size is 1.28 (95% C.I. of 1.03 to 1.61), which is statistically significant. These findings indicate that evaluations of juvenile drug courts reduce general recidivism, but the magnitude of these effects is smaller than that of adult drug courts. For drug related recidivism, juvenile

drug courts' strongest effects are found in more rigorous evaluations, and in these evaluations the magnitude of the effect is similar to the general recidivism means odds-ratio (approximately 1.30). Thus, the most rigorous evaluations of juvenile drug courts indicate that these courts have small effects on recidivism and the magnitude of these effects is approximately an odds-ratio of 1.30, which translates into a 43.5% recidivism rate for drug court participants, if we assume a 50% recidivism rate for non-participants.

It is important to note that three evaluations of juvenile drug courts used experimental designs. Two of these evaluations assessed two different cohorts of participants in the Summit County (Ohio) Juvenile Drug Court (Dickie, No Date). These two evaluations had large attrition problems (both had more than 50% total attrition and one of the cohorts had significant differential attrition). Because of these attrition problems, we rated these two compromised experiments as strong quasi-experiments. Thus, the results presented in Table 6 for experimental designs are for the single high-quality randomized trial. This study found positive effects, albeit not statistically significant, that are only slightly smaller than the overall effects for adult drug courts (i.e., an odds-ratio of 1.39 for general recidivism and 1.38 for drug recidivism). If the two compromised evaluations are rated as experiments, our findings are essentially unchanged: the mean odds-ratio for general recidivism is 1.44. While the mean effect size is not statistically significant, it is important to note that the effect sizes from these three evaluations are all positive, indicating less recidivism for juvenile drug court participants. These findings continue to support the conclusion that juvenile drug courts have small effects on recidivism.

The substantive findings regarding the effectiveness of DWI drug courts are similar to those for adult drug courts. Just as with evaluations of adult drug courts: (1) the largest effects of DWI drug courts are found in methodologically weak evaluations, (2) collectively, experimental evaluations find small and non-statistically significant mean odds-ratios for general and drug related recidivism; and, (3) the mean odds-ratios for experimental evaluations are greatly influenced by one evaluation that found a negative effect of participation. In regards to the first point, the mean odds-ratio is moderate and statistically significant for the three quasi-experimental design categories, but relatively small and non-statistically significant for the four evaluations in the experimental category (1.27 with 95% C.I. of 0.87 to 1.85). Thus, quasi-experimental evaluations yield substantively different findings than experimental evaluations of DWI drug courts. In regards to the second point, experimental evaluations find a small and non-statistically significant difference in general and drug related recidivism between participants and non-participants. And finally, additional analysis of the four experimental DWI drug court evaluations reveals that one such experimental evaluation conducted by MacDonald, Morral, Raymond, & Ebner (2007) heavily influences the mean odds-ratio

for the experimental evaluations. While the three other experimental evaluations of DWI courts found positive odds-ratios on the general recidivism measure ranging from 1.39 to 2.25, MacDonald et al.'s evaluation found a negative effect (odds-ratio of 0.73). If MacDonald et al.'s evaluation is omitted from the general recidivism analysis, then the mean odds-ratio becomes 1.58 (95% C.I. of 0.99 to 2.54) and has a p-value of 0.057. Similarly, if this evaluation is omitted from the drug related recidivism analysis, the mean odds-ratio becomes 1.43 (95% C.I. of 0.83 to 2.50) with a p-value of 0.194. Thus, the MacDonald et al. evaluation is highly influential on the results of the experimental evaluations, especially the general recidivism analysis.

The evidence presented above finds considerable evidence of the effectiveness of drug courts, but the strength of this evidence varies by type of court. In regards to adult drug courts, over 90 independent evaluations have been conducted and the overwhelming majority of these evaluations find that drug court participants have less recidivism than non-participants. Further, experimental evaluations of these courts also consistently find sizeable reductions in recidivism. Thus, the evidence indicates that adult drug courts are effective in reducing recidivism. Likewise, we characterize the evidence as cautiously supporting the effectiveness of DWI drug courts, because while quasi-experimental evaluations find strong and consistent indications that these programs reduce general and drug related recidivism, randomized experimental evaluations find a small, non-statistically significant reduction in recidivism. Yet, the findings from experimental evaluations of DWI drug courts are ambiguous in that the majority of these evaluations find positive effects but a single, influential evaluation with negative findings heavily influences the mean effect. Clearly, only additional evaluations using experimental methods can definitively resolve the remaining ambiguity surrounding the effectiveness of DWI drug courts general effectiveness. Evaluations of juvenile drug courts, especially more rigorous evaluations, consistently indicate that these courts have relatively small effects on recidivism.

4.4 DRUG COURTS' LONG-TERM EFFECTS

An important issue in drug court research is whether the effects last long-term. Assessing drug courts' long-term effect, however, is challenging. There are two inter-related issues. The first is whether the observed pattern of positive results reflects a suppression effect. Many of the outcomes are examined for recidivism during the course of drug court participation. It is possible that drug courts suppress offending behavior while someone is active in the program but that this effect disappears post-program once behavioral contingencies are removed. The second issue is simply whether observed effects continue long-term, such as three years post-program.

We examined the first issue of suppression by coding whether the recidivism tracking period overlapped: (1) completely, (2) partially, or (3) not at all with the period of drug court participation. If drug courts' effects on recidivism are limited to the period in which participants are active in the court, then the mean effect size should be largest when the treatment and recidivism tracking periods overlap completely and the mean effect size should decrease as the amount of overlap between the treatment and recidivism tracking periods decreases. Table 8 displays the mean effect size for each of these categories and shows that the effects of adult drug courts remain post-program. That is, the positive results do not appear to be simply a temporary suppression effect.

We examined the second issue by computing mean effect sizes for different follow-up periods. Recall that evaluations most commonly reported recidivism rates 12 months after drug court entry (or termination). When effect sizes measuring recidivism at multiple time points were available, we preferred effect sizes based on this most common time period (i.e., 12 months) to facilitate between study comparisons. However, not all evaluations measured recidivism at 12-months and some evaluations reported recidivism at multiple time-points (e.g., 12, 24, and 36-months). Consequently, there is variation both within and between studies in the length of recidivism tracking period. We exploit both sources of variation. Between study variation was examined by calculating the mean effect size by length of recidivism tracking period. These findings (Table 8) show a roughly stable mean odds-ratio for the different follow-up periods. A complication with this analysis is the possible confounding of evaluation features with follow-up length. To address that issue, we also examined within study variation by analyzing the subset of evaluations that reported results at both the 12 and 24 month follow-up period (21 studies) and the subset of evaluations that reported results at the 12, 24, and 36 month follow-up period (8 studies).As shown in Table 8, adult drug court effects remain remarkable stable over time from 12-months through 36-months.

The analyses presented above support the conclusion that any effect adult drug courts have on recidivism are not limited to the short-term. Rather, the available research suggests that drug court participants have reduced recidivism during and after drug court treatment, and these effects appear to last at least three years post-drug court entry.

4.5 FEATURES OF THE DRUG COURT

The various drug courts examined by this collection of evaluations vary in potentially important ways (see Tables 2 and 3). We were guided by Longshore et al.'s conceptual framework for understanding differences across drug courts.

Longshore et al's framework has five dimensions: leverage, population severity, program intensity, predictably, and rehabilitative emphasis. We were able to code measures tapping the first three dimensions. We were unable to code measures of the two latter concepts, because few evaluations reported the kinds of information necessary to measure these concepts. While we were able to code measures of leverage, population severity, and program intensity, many of our measures contain a large amount of missing data because evaluations often failed to provide relevant information (esp. regarding program intensity).

Longshore and colleagues argued that drug courts in which participants face greater consequences if they fail to meet program requirements have greater leverage. For example, they contend drug courts that process cases using the post-plea method have greater leverage because offenders have been convicted and face immediate sentencing upon failure in drug court. Likewise, drug courts that dismiss charges or expunge convictions have greater leverage because such incentives are more attractive motivators than reductions in charges/sentences. Therefore, we coded courts' method of case disposition (pre-plea, post-plea, or mixed) and what happens to the charges/sentences upon graduation (dismissed/expunged or not dismissed/expunged). Again evaluations often failed to report relevant information. However, when evaluations did report such information, post-plea case processing was most common (see Table 2). Further, most adult drug courts dismissed charges upon graduation, but dismissal was less common in juvenile and DWI drug courts.

We found mixed support for the relationship between leverage and outcomes (Table 9). As anticipated, drug courts that dismiss/expunge charges upon graduation had higher mean odds-ratios on both measures of recidivism. This difference is statistically significant for the drug recidivism measure, but is non-significant for the general recidivism measure. Counter to our expectations, however, drug courts that predominantly used post-plea case processing did not have greater reductions in recidivism than other courts.

We coded multiple measures designed to tap program intensity, another aspect of Longshore et al.'s framework (see Table 2). Moderator analyses of the relationship between coded measures of program intensity and effect size generally did not generally support the hypothesis that more intense programs are associated with greater reductions in recidivism (Table 9). In fact, most of these relationships reveal that the mean odds-ratios were substantively and statistically similar across the categories of the coded program intensity measures. One notable possible exception to this conclusion is the relationship between number of status hearings and effect size. The adult drug court analysis finds that courts with more than two status hearings in first treatment phase

had larger effects of drug related recidivism than other courts. Other than the number of status hearings, these findings suggest that drug courts with greater program intensity are no more effective in reducing recidivism than other courts.

We replicated these analyses for juvenile and DWI drug courts (see Tables 10 and 11). These analyses were hindered by the relatively small number of evaluations of these programs and by missing data. Few differences were found. Yet, it is important to note that these analyses found that juvenile drug courts with more frequent status hearings had larger effects on general recidivism than other courts—a finding that is substantively similar to that found in the analysis of adult drug court evaluations.

Perhaps most controversially, Longshore et al. hypothesize that drug court programs that serve less severe populations, in terms of criminal history and substance abuse problems, are more successful in reducing recidivism. This hypothesis is controversial as it directly contradicts a core element of Andrews et al.'s (1990) principles of effective intervention. One of Andrews et al.'s principles is that more effective programs serve more severe populations, and this "risk principle" has found empirical support (see e.g., Andrews & Bonta, 1992). To test the relationship between population severity and program effectiveness (i.e., effect size), we coded two measures of population severity. The first measure assessed whether offenders with violent criminal history were allowed into the program. The second measure concerned each sample's extent of criminal history. Many drug courts also limit eligibility to offenders without extensive prior convictions. We inductively coded a measure flagging evaluations that used samples with limited criminal history. More specifically, we took notes on information relating to eligibility restrictions on criminal history and/or descriptive statistics concerning criminal history. After all evaluations were coded, we read over these notes and distinguished samples with relatively minor criminal history. For example, if 70% of the sample of drug court participants had no prior convictions or less than 3 prior arrests, then this sample was coded as having minor criminal history.

Table 12 reports the mean odds-ratios by coded sample characteristics from evaluations of adult drug courts. Analyses of the general recidivism effect sizes support Longshore and colleagues prediction. Specifically, samples that included only non-violent offenders had statistically larger mean odds-ratios on the general recidivism measure than samples that included violent offenders. Similarly, samples with minor criminal history had larger mean odds-ratios than evaluations based on samples with more criminal history; however, this difference was not statistically significant. These findings were not replicated in the analysis of the drug related recidivism outcome measure. Here there were no differences in the mean effects odds-ratios on either of the measures of

population severity. Thus, these findings are more supportive of Longshore et al.'s hypothesis than Andrews et al.'s risk principle.

Again, we replicated these analyses for juvenile and DWI drug courts (see Tables 13 and 14) and no meaningful relationships were between effect size and population severity. We also coded important drug features that are not directly related to Longshore et al.'s framework. One such measure is program graduation rate (see Tables 2 and 9). Here we found that most drug court participants do not successfully complete the program. The median graduation rate for adult, juvenile, and DWI drug courts are 39%, 47%, and 62%, respectively. Interestingly, for all three types of drug courts, we find a strong, non-linear relationship between graduation rate and both general and drug related recidivism. Specifically, Tables 9, 10, and 11 indicate that courts with graduation rates between 26% and 50% had substantively smaller effects on general and drug related recidivism than courts with either higher or lower graduation rates. Additional analyses (not shown in the tables) indicate that all of these differences are statistically significant (each has a p-value less than 0.01). Thus, our results indicate that courts with graduation rates between 26% and above 50% had larger effects than courts with graduation in between these percentages. It is unclear whether this reflects a feature of the drug court itself or of the client pool.

Another potentially important measure is the network of treatment providers utilized by the drug court. Some research suggests that courts employing a single treatment provider are more likely to use cognitive-behavioral programs (Peyton & Gossweiler, 2001), which have been shown to be relatively effective (MacKenzie, 2002, 2006). Additionally, the use of a single treatment provider may indicate stronger lines of communication between the court and the treatment providers. As such, courts using a single treatment provider may be more effective in reducing recidivism than other courts. Despite these predictions, drug courts that utilize a single treatment provider had similar mean effect sizes as other courts.

4.6 ADDITIONAL SENSITIVITY ANALYSES

The majority of the evaluations included in this review were unpublished technical reports produced by government or private research entities. Such a large proportion of unpublished evaluations reduced the likelihood of publication bias affecting our estimates of drug courts' effectiveness. As shown in Table 5, the results for published and unpublished evaluations of adult drug courts were roughly similar. This finding is also true for juvenile and DWI courts (see Tables 6 and 7).

Other forms of publication selection bias, such as outcome selection bias, may be present. To assess whether our estimates were upwardly biased due to some form of publication bias, we performed the Duval and Tweedie trim-and-fill analysis for each court type. This method assumes that in the absence of publication bias, the scatterplot between effect size and standard error of the effect size will have a funnel shape and augments the data to achieve this shape. Using this method, the distribution of general recidivism effect sizes for adult drug court was filled with 23 effect sizes, which reduced the overall random effects mean odds-ratio from 1.66 to 1.34. The latter remained statistically significant ($z = 5.18$, $p < 0.01$). The distribution of drug recidivism effect sizes for adult drug courts did not require filling. For juvenile drug courts, the distribution of general recidivism effect sizes did not require filling but the distribution of drug recidivism was augmented with one additional effect size, which reduced the random effects means odds-ratio from 1.06 to 1.01 (neither was statistically significant). For the DWI drug courts, five effect sizes were added to the general recidivism distribution, which reduced the mean odds-ratio to 1.53 ($z = 3.57$, $p < 0.01$); two effect sizes were added to the drug related recidivism distribution, which reduced the mean odds-ratio to 1.57 ($z = 4.59$, $p < 0.01$). Thus, the conclusions remain robust even under the trim-and-fill model that tends to over-fill when there is substantial heterogeneity, as was the case here.

We also performed sensitivity analyses to determine the effect on our results of allowing a few authors to contribute many effect sizes to our analyses. Specifically, we flagged authors who contributed 10% or more of the effect sizes to any of the court specific analyses, and then re-ran the analysis without these effect sizes. For adult drug courts, only NPC research contributed 10% or more of the effect sizes ($k = 15$). While the effect sizes from NPC had a larger mean odds-ratio effect size (1.94) than other evaluations (1.63), this difference was not statistically significant and the mean odds-ratio effect size for the other evaluations is virtually identical to the mean effect size with the NPC evaluations included. We performed similar analyses with the juvenile and DWI drug court effect sizes. These sensitivity analyses revealed that our results are substantively unchanged by multiple effect sizes from the same author(s).

Last, we assessed the applicability of our findings beyond the United States by examining the eight international evaluations. Seven of the eight evaluations examined adult drug courts (1 in Guam, 2 in Canada, and 4 in Australia). Six of these seven evaluations had positive general recidivism effect sizes, four of which were moderate in size (odds ratio greater than 1.60), and the mean general recidivism odds-ratio is 1.62 with a 95% confidence interval of 1.11 to 2.36, which is statistically significant. Notice that this mean general recidivism odds-ratio is virtually identical to the mean odds-ratio reported above for all adult drug court evaluations (1.66). Only one international evaluation of a juvenile

drug court met our eligibility criteria; this evaluation assessed a juvenile drug court in New Zealand (Searle & Spier, 2006). This evaluation found a negative effect indicating that the juvenile drug court did not reduce recidivism. These international evaluations mirror our findings on adult and juvenile drug courts. Thus, our findings appear to accurately reflect existing evaluations of drug courts outside the United States.

5 Conclusions

The rapid proliferation of drug courts across the United States has been remarkable. In approximately twenty years, drug courts have gone from a solitary court in one jurisdiction to a national phenomenon with thousands of courts in operation. The drug court phenomenon has become an international movement, as courts are now in operation in several nations.

Perhaps even more remarkable has been the results of empirical evaluations of drug courts. The literature assessing the effectiveness of drug courts is large and diverse. As this synthesis reveals, the vast majority of evaluations of adult drug court programs find that participants in these programs have lower recidivism than non-participants, and often these differences are considerable. Our analyses indicate that on average the effect of participation in an adult drug court is equivalent to a reduction in general recidivism from 50% to approximately 38% and a reduction in drug-related recidivism from 50% to approximately to 37%; these reductions in recidivism persist for at least three years after program entry. Thus, the accumulated evidence suggests that adult drug courts are effective in reducing recidivism and the policy implication of this conclusion is that continued funding, development, and operation of adult drug courts is warranted.

The evidence on the effectiveness of DWI drug courts is very promising but is not unambiguous, given the mixed and sometimes null findings from the most rigorous experimental evaluations. The magnitude of the effects is comparable to those of the adult drug courts. Yet, because of the ambiguous findings of the most rigorous, randomized experimental evaluations, we believe that additional experimental evaluations of DWI courts are needed.

For juvenile drug courts, we find that these courts have considerably smaller effects on recidivism than either adult or DWI drug courts. Evaluations of these courts indicate that the average effect of participation in a juvenile drug court is equivalent to a reduction in recidivism from 50% to approximately 43.5%. This average effect is more than 40% smaller than the average estimated effects of participation in an adult or DWI drug court. The question becomes: Why are juvenile drug courts less effective than other

kinds of drug courts? Obviously, we cannot answer this question with certainty, yet two factors seem relevant. First, juvenile drug courts generally provide services to relatively high-risk offenders, whereas other kinds of drug courts typically exclude high-risk offenders. Second, juvenile drug courts appear to be less demanding interventions than adult drug courts, in that, drug testing and status hearings appear to be less frequent, and the period of program participation appears to be shorter in duration.

Beyond these general conclusions about the effectiveness of drug courts, it is important to emphasize that the estimated effects of drug court participation are highly variable. This large variability in findings across evaluations suggests differential effectiveness across variations in drug courts. We attempted to explore the sources of this variability by examining drug court structure, implementation, and participant characteristics. Our analyses were informed by Longshore and colleagues (2001) framework for understanding differences across drug courts. We found some evidence supporting the importance of *leverage* in that drug courts that dismissed charges or expunged convictions had larger reductions in recidivism than other courts, but this finding was only meaningful for drug-related recidivism in the analysis of adult drug courts. Interestingly, we found relatively little variation in observable measures of program *intensity*, and courts that required more than the standard number of phases, drug tests were no more effective than other courts. This finding does not support Longshore and colleagues prediction regarding the relationship between program intensity and program effectiveness. The one finding that does support the importance of program intensity is the relationship between frequency of status hearings and program effectiveness. Specifically, courts with more than two status hearings in the first treatment phase exhibited larger reductions in recidivism than courts with two or fewer status hearings; however, this difference was only statistically significant for drug-related recidivism in the analysis of adult drug courts. We believe that more primary research comparing the effectiveness of drug courts with varying features needs to be conducted to confirm these meta-analytic findings.

In support of Longshore et al.'s conception framework of drug courts, we find that programs with less severe populations are more effective in reducing general recidivism. Specifically, evaluations of programs that only allowed non-violent offenders to participate had larger reductions in general recidivism than other program evaluations. This finding holds in various sensitivity analyses. Because this finding conflicts with the risk principle from Andrews et al.'s principles of effective intervention, it is sure to be met with considerable controversy and criticism. Yet, given the strength and consistency of this finding, we believe that this finding deserves consideration and further empirical scrutiny.

Our finding that courts with violent offenders are less effective in reducing general recidivism seems to contradict findings from other drug court researchers who have found that violent offenders perform as well in drug courts as non-violent offenders (see e.g., Saum, Scarpitti, & Robbins, 2001). Closer inspection, however, reveals that findings like Saum et al.'s examine a *different unit of analysis* than our meta-analytic research. In essence, these researchers work concerns the recidivism of *individuals* with evidence of prior violence in comparison to non-violent drug court participants; whereas, our meta-analytic findings concern the reduction in recidivism between *courts* that allow violent drug court clients in comparison to other courts. These two questions are distinct and the answers to these questions need not match. As an example, consider the research of Saum et al. (2001), who as previously mentioned found that drug court participants with evidence of prior violence exhibited comparable reductions in recidivism as non-violent drug court participants. This study examines the individual-level of analysis. At the court evaluation-level of analysis, we find that the court evaluated in Saum et al'.s research (coded here under Scarpitti, Saum, and Robbins, 2001) had relatively small effects on recidivism in comparison to other drug court evaluations; in fact, this evaluation found that participants had more recidivism than non-participants. Rather than contradicting our finding, the results of Saum et al. buttress our conclusion that courts that accept violent offenders are less effective than other courts. In short, it is entirely possible that both sets of findings are correct; violent drug court participants do as well as non-violent participants in drug courts, and courts that accept violent offenders are less effective than other courts.

While our finding regarding the inclusion of violent offenders will be controversial, it is important to note that this finding supports not only Longshore and colleagues' theoretical perspective but also federal regulations that require drug courts to restrict eligibility to non-violent offenders in order to reach federal funds. Title V of the Violent Crime Control and Law Enforcement Act of 1994, for example, authorized federal funding of drug courts but prohibited the distribution of federal funds to drug courts that allow clients with current or prior serious violent convictions to participate (GAO 1997, p. 41). To our knowledge, this prohibition remains in place. Simply put, our results, while certainly not definitive, suggest that such restrictions have merit.

One important issue, which goes beyond our current data, concerns whether drug courts restriction program eligibility in other ways, which may not be warranted. For example, courts exclude non-violent offenders currently charged or previously convicted of distribution/sales offenses from eligibility. Given that a large proportion of drug distribution/sales offenses are committed by drug users trying to support their expensive drug habits (see e.g., Johnson et al. 1985), such policies may exclude a substantial population of offenders who could benefit from drug court treatment. Likewise, many

courts also exclude drug abusing offenders with extensive criminal histories and serious mental health issues. While such offenders are obviously more of a risk to public safety, offering effective drug treatment to such offenders holds the promise of producing reductions in re-offending—which is precisely what the "risk principle" hypothesizes.

There is some evidence outside of the drug court context that suggests expanding the drug court model to broader populations of offenders can be effective. Perhaps the most prominent example of this is found in the research of Adele Harrell and colleagues who evaluated the National Demonstration of the Breaking the Cycle (BTC) project. This project applied an intervention based on the drug court model to *nearly all drug abusing offenders* arrested on felony charges in three sites (Tacoma, WA, Birmingham, AL, and Jacksonville, FL). In spite of partial program implementation, the evaluation found that participation in the BTC was associated with reductions in criminal behavior (Harrell et al. 2002; Mitchell and Harrell 2006). The findings of the BTC demonstration project suggest that drug courts could be applied to a wider range of offenders and still reduce recidivism. Further, BTC's findings are buttressed by a recent simulation analysis that indicates relaxing the eligibility criteria for criminal justice based drug treatment programs would substantially increase the number of offenders eligible for treatment, and this expansion would avert several million crimes that these offenders who otherwise commit (Bhati & Roman, 2010). Such research suggests that the adult drug court model could be expanded to include more serious, non-violent offenders and still reduce recidivism.

6 Plans for Updating the Review

We plan to update this systematic review every three years in accordance with Campbell Collaboration guidelines. However, given the quantity of quasi-experimental evaluations and the consistency of their findings, we see little value in continuing to synthesis these evaluations. In the updated review, we plan to review only the findings of experimental evaluations.

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9 Included Studies

Note: The studies marked with an asterisk (*) were included in the analyses reported here. The other studies listed are eligible but statistically dependent.

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10 Tables

10.1 KEY FEATURES OF EVALUATIONS

Variable	Adult Drug Court (k^a = 92) Frequency (%)	Juvenile Drug Court (k = 34) Frequency (%)	DWI Drug Court (k = 28) Frequency (%)
Publication type			
Journal/book chapter	21 (23%)	3 (9%)	4 (14%)
Unpublished	71 (77%)	31 (92%)	24 (86%)
Publication year			
1989-1993	1 (1%)	0 (0%)	0 (0%)
1994-1998	13 (14%)	1 (3%)	0 (0%)
1999-2003	41 (45%)	10 (29%)	4 (14%)
2004 or later	35 (38%)	14 (41%)	16 (57%)
No date	2 (2%)	9 (26%)	8 (29%)
Outcome measure			
General offending only	49 (53%)	20 (59%)	13 (47%)
Includes drug offending measure	39 (42%)	11 (32%)	13 (47%)
Includes drug use measure	4 (4%)	3 (9%)	2 (7%)
Max length of follow-up			
12 months or less	42 (46%)	18 (53%)	21 (75%)
12.01-24 months	23 (25%)	8 (24%)	2 (7%)
25.01-36 months	6 (7%)	2 (6%)	0 (0%)
36+ months	8 (9%)	4 (12%)	2 (7%)
No information/unclear	13 (14%)	2 (6%)	1 (4%)
Follow-up overlap with treatment period			

Variable	Adult Drug Court (k^a = 92) Frequency (%)	Juvenile Drug Court (k = 34) Frequency (%)	DWI Drug Court (k = 28) Frequency (%)
Complete overlap	26 (28%)	7 (21%)	5 (17%)
Partial overlap	46 (50%)	7 (21%)	14 (50%)
No overlap	17 (18%)	9 (26%)	8 (29%)
No information/Unclear	3 (3%)	11 (32%)	1 (4%)
Methodological rigor rating			
Weak quasi-experiment	18 (20%)	4 (12%)	3 (11%)
Standard quasi-experiment	51 (55%)	17 (50%)	7 (25%)
Rigorous quasi-experiment	20 (22%)	11 (32%)	5 (18%)
Random experiment	3 (3%)	1 (3%)	4 (14%)
No information/unclear	0 (0%)	1 (3%)	9 (32%)
Type of comparison group used			
Declined/rejected	28 (30%)	5 (15%)	1 (4%)
Historical controls	24 (26%)	7 (21%)	5 (18%)
Eligible non-referred	9 (10%)	0 (0%)	1 (4%)
Comparable (randomization)	3 (3%)	3 (9%)	5 (18%)
Regular probation	15 (16%)	10 (29%)	2 (7%)
Other Non-eligible drug offenders	9 (10%)	1 (3%)	5 (18%)
Not reported/can't tell	4 (4%)	2 (6%)	9 (32%)
Overall attrition>20%			
Yes	4 (4%)	4 (12%)	3 (11%)
No	87 (95%)	28 (82%)	25 (89%)
No information/unclear	1 (1%)	2 (6%)	0 (0%)
Differential attrition>20%			
Yes	5 (4%)	3 (8%)	2 (7%)
No	87 (95%)	30 (88%)	26 (93%)
No information/unclear	1 (1%)	1 (3%)	0 (0%)

[a] Number of evaluations

10.2 KEY FEATURES DRUG COURTS

Variable	Adult Drug Court (k^a = 92) Frequency (%)	Juvenile Drug Court (k = 34) Frequency (%)	DWI Drug Court (k = 28) Frequency (%)
Maturity of court[b]			
New (first two years of operation)	54 (59%)	17 (50%)	16 (57%)
Developing (third or fourth year)	12 (13%)	9 (27%)	0 (0%)
Mature (beyond fourth year)	10 (11%)	1 (3%)	0 (0%)
Not reported	16 (17%)	7 (21%)	12 (43%)
Method of disposition			
Pre-plea	21 (23%)	0 (0%)	0 (0%)
Post-plea	36 (39%)	18 (53%)	11 (39%)
Uses Both	13 (14%)	1 (3%)	1 (4%)
Not reported	22 (24%)	15 (44%)	16 (57%)
Charges dismissed upon graduation			
Yes	34 (37%)	8 (24%)	1 (4%)
No	19 (21%)	3 (9%)	12 (43%)
Not reported	39 (42%)	23 (68%)	15 (57%)
Number of phases			
Two	4 (4%)	0 (0%)	0 (0%)
Three	41 (45%)	7 (21%)	3 (11%)
Four	16 (17%)	16 (47%)	7 (25%)
Five	5 (4%)	0 (0%)	3 (11%)
Doesn't use phases	2 (2%)	1 (3%)	1 (4%)
Not reported	24 (26%)	10 (29%)	14 (50%)
Number of drug tests/week[c]			
Two or less	21 (23%)	5 (15%)	5 (18%)
More than two	16 (16%)	3 (9%)	1 (4%)
Not reported	55 (60%)	26 (74%)	22 (79%)
Number of treatment meetings/week[c]			
Three or less	9 (10%)	2 (6%)	1 (4%)
More than three	6 (7%)	3 (9%)	1 (4%)

Not reported	77 (84%)	29 (85%)	26 (93%)
Number of status hearings/month[c]			
Two or less	19 (21%)	7 (21%)	8 (29%)
More than two	16 (17%)	3 (9%)	0 (0%)
Not reported	57 (62%)	24 (71%)	20 (71%)
Min. time to graduation			
Less than 12 months	20 (22%)	17 (50%)	4 (14%)
12 to 15 months	46 (50%)	13 (38%)	8 (29%)
More than 15 months	12 (13%)	0 (0%)	5 (18%)
Not reported	14 (15%)	4 (12%)	11 (39%)
Graduation rate[d]			
.00 to .25	10 (11%)	1 (3%)	0 (0%)
.26 to .50	38 (41%)	15 (44%)	2 (7%)
.51 to .75	13 (14%)	10 (29%)	10 (36%)
More than .75	0 (0%)	0 (0%)	5 (18%)
Not reported	31 (34%)	8 (24%)	11 (39%)
Single treatment provider			
Yes	15 (16%)	3 (9%)	0 (0%)
No	28 (30%)	8 (24%)	5 (18%)
Not reported	49 (53%)	23 (68%)	23 (82%)

[a] Number of evaluations
[b] At beginning of evaluation period.
[c] In first treatment phase of drug court.
[d] Graduation rate is calculated as proportion of terminated clients who completed program successfully (i.e., excludes those currently active in program).

10.3 KEY FEATURES OF SAMPLES

Variable	Adult Drug Court (k^a = 92) Frequency (%)	Juvenile Drug Court (k = 34) Frequency (%)	DWI Drug Court (k = 28) Frequency (%)
Gender composition			
All male (90%+ male)	1 (1%)	2 (5%)	1 (4%)
Mostly male (60-90% male)	77 (84%)	32 (94%)	19 (68%)
Approx. equal (59-40% male)	7 (8%)	0 (0%)	0 (0%)

Mostly female (39-10% male)	0 (0%)	0 (0%)	0 (0%)
All female (<10% male)	0 (0%)	0 (0%)	0 (0%)
Not reported	7 (8%)	0 (0%)	8 (29%)
Offender type			
Only non-violent offenders	72 (79%)	18 (53%)	23 (82%)
Includes violent offenders	16 (17%)	4 (12%)	0 (0%)
Not reported	4 (4%)	12 (35%)	5 (18%)
Minor criminal history			
Yes	22 (24%)	2 (6%)	2 (7%)
No	48 (52%)	17(50%)	14 (43%)
Not reported	22 (24%)	15 (44%)	12 (50%)

[a] Number of evaluations

10.4 MEAN RANDOM-EFFECTS ODDS-RATIO BY TYPE OF RECIDIVISM MEASURE

		95% Confidence Interval				
Outcome	Mean ES	Lower	Upper	*Q*	*k*[a]	Tau[2]
Adult drug courts						
General recidivism[b]	1.66*	1.50	1.84	442.19*	92	0.178
Drug recidivism[c]	1.70*	1.39	2.08	323.98*	42	0.368
Drug use	1.45	0.92	2.28	15.78*	4	0.165
Juvenile drug court						
General recidivism	1.37*	1.15	1.63	66.31*	34	0.105
Drug recidivism	1.06	0.69	1.63	29.65*	14	0.357
Drug use	1.50	0.67	3.34	2.05	3	0.359
DWI drug court						
General recidivism[d]	1.65*	1.35	2.02	78.40*	28	0.159
Drug recidivism[e]	1.59*	1.22	2.09	16.84	14	0.054
Drug use	1.87	0.34	10.23	5.02*	2	1.227

[a] Number of evaluations

[b] The mean effect size is 1.57 (95% C.I. 1.43-1.72), when three large positive effect sizes were removed.
[c] The mean effect size is 1.46 (95% C.I. 1.28-1.67), when two large positive effect sizes were removed.
[d] The mean effect size is 1.63 (95% C.I. 1.33-1.99), when one large positive effect sizes was removed.
[e] The mean effect size is 1.57 (95% C.I. 1.20-2.04), when one large positive effect sizes was removed.
* $p < 0.05$

10.5 MEAN RANDOM EFFECTS ODDS RATIOS OF ADULT DRUG COURTS BY METHODOLOGICAL FEATURES

	General Recidivism			Drug Recidivism[b]		
Variable	Mean	95% C.I.	*k*[c]	Mean	95% C.I.	*k*
Publication status	$Q_B = 0.05$, *df* = 1, $p = 0.828$, $tau^2 = 0.220$			$Q_B = 0.51$, *df* = 1, $p = 0.477$, $tau^2 = 0.787$		
Published	1.63	1.29-2.07	21	1.97	1.23-3.16	15
Unpublished	1.68	1.48-1.91	71	1.59	1.11-2.27	28
Recidivism data source	$Q_B = 0.00$, *df* = 1, $p = 0.998$, $tau^2 = 0.220$			$Q_B = 0.34$, *df* = 1, $p = 0.562$, $tau^2 = 0.788$		
Arrest records	1.67	1.47-1.90	74	1.82	1.29-2.57	30
Other	1.67	1.33-2.10	22	1.52	0.92-2.51	13
Comparisons declined/rejected drug court involvement	$Q_B = 0.06$, *df* = 1, $p = 0.800$, $tau^2 = 0.197$			$Q_B = 0.47$, *df* = 1, $p = 0.492$, $tau^2 = 0.800$		
Yes	1.66	1.37-2.01	31	1.51	0.90-2.53	13
No	1.61	1.41-1.84	60	1.87	1.32-2.66	29
Historical comparisons	$Q_B = 0.00$, *df* = 1, $p = 0.956$, $tau^2 = 0.197$			$Q_B = 0.06$, *df* = 1, $p = 0.803$, $tau^2 = 0.807$		
Yes	1.62	1.31-2.01	24	1.66	0.97-2.82	13
No	1.63	1.44-1.85	67	1.79	1.27-2.54	29
Overall attrition>20%	$Q_B = 0.52$, *df* = 1, $p = 0.471$, $tau^2 = 0.220$			$Q_B = 0.03$, *df* = 1, $p = 0.865$, $tau^2 = 0.795$		
Yes	1.37	0.78-2.40	4	1.62	0.77-3.41	6
No	1.69	1.51-1.89	87	1.74	1.28-2.37	37
Differential attrition>20%	$Q_B = 0.27$, *df* = 1, $p = 0.601$, $tau^2 = 0.221$			$Q_B = 0.01$, *df* = 1, $p = 0.965$, $tau^2 = 0.796$		
Yes	1.95	1.09-3.46	4	1.76	0.59-5.30	3
No	1.66	1.49-1.87	87	1.72	1.28-2.31	40
Methodological rigor rating	$Q_B = 2.35$, *df* = 3, $p = 0.504$, $tau^2 = 0.212$			$Q_B = 2.04$, *df* = 3, $p = 0.563$, $tau^2 = 0.755$		

Weak	1.97	1.54-2.54	18	2.05	0.98-4.27	6
Standard	1.63	1.40-1.89	51	1.53	1.00-2.33	20
Strong	1.57	1.25-1.97	20	2.07	1.28-3.33	14
Experimental	1.45	0.80-2.62	3	1.03	0.37-2.88	3

[a] Number of evaluations
[b] If two large positive effect sizes are removed, the substantive findings are nearly identical to these; however, the variance component shrinks to approximately 0.10.

10.6 MEAN RANDOM EFFECTS ODDS RATIOS OF JUVENILE DRUG COURTS BY METHODOLOGICAL FEATURES

	General Recidivism			Drug Recidivism		
Variable	Mean	95% C.I.	k	Mean	95% C.I.	k
Publication status	Q_B = 0.76, *df* = 1, *p* = 0.383, tau² = 0.093			Q_B = 0.24, *df* = 1, *p* = 0.627, tau² = 0.304		
Published	1.70	1.02-2.83	3	0.85	0.32-2.28	2
Unpublished	1.34	1.12-1.59	31	1.11	0.71-1.74	12
Recidivism data source	Q_B = 0.00, *df* = 1, *p* = 0.986, tau² = 0.095			Q_B = 0.09, *df* = 1, *p* = 0.769, tau² = 0.304		
Arrest records	1.37	1.15-1.63	31	1.10	0.68-1.78	10
Other	1.36	0.79-2.35	3	0.96	0.43-2.12	4
Comparisons declined/rejected drug court involvement	Q_B = 9.83, *df* = 1, *p* = 0.002, tau² = 0.068					
Yes	0.66	0.40-1.09	5	----	----	--
No	1.49	1.25-1.76	27	----	----	--
Historical comparisons	Q_B = 2.91, *df* = 1, *p* = 0.088, tau² = 0.092					
Yes	1.72	1.23-2.40	7	----	----	--
No	1.24	1.01-1.52	25	----	----	--
Overall attrition>10%	Q_B=0.02, *df* = 1, *p* =0.886, tau² = 0.093			Q_B = 1.11, *df* = 1, *p* = 0.293, tau² = 0.294		
Yes	1.27	0.66-2.44	4	2.67	0.46-15.66	2

No	1.33	1.11-1.58	28	1.01	0.66-1.53	12
Differential attrition>10%	Q_B = 0.01, *df* = 1, *p* = 0.952, tau² = 0.097			Q_B = 1.11, *df* = 1, *p* = 0.293, tau² = 0.294		
Yes	1.38	0.68-2.77	3	2.67	0.46-15.66	2
No	1.35	1.13-1.61	30	1.01	0.66-1.53	12
Methodological rigor rating	Q_B = 3.14, *df* = 2, *p* = 0.208, tau² = 0.062			Q_B = 2.25, *df* = 1, *p* = 0.133, tau² = 0.236		
Weak	1.85	1.26-2.72	4	0.53	0.15-1.93	1
Standard	1.32	1.07-1.62	17	0.85	0.42-1.69	4
Strong	1.22	0.90-1.65	11	1.31	0.76-2.23	8
Experimental	1.39	0.50-3.85	1	1.38	0.37-5.07	1

10.7 MEAN RANDOM EFFECTS ODDS RATIOS OF DWI DRUG COURTS BY METHODOLOGICAL FEATURES

	General Recidivism			Drug Recidivism		
Variable	Mean	95% C.I.	k	Mean	95% C.I.	k
Publication status	Q_B = 0.19, *df* = 1, *p* = 0.661, tau²=0.165			Q_B = 0.75, *df* = 1, *p* = 0.388, tau²=0.065		
Published	1.50	0.91-2.46	4	1.38	0.87-2.14	3
Unpublished	1.69	1.35-2.12	24	1.76	1.23-2.51	11
Recidivism data source	Q_B = 0.74, *df* = 1, *p* = 0.389, tau² = 0.158			Q_B = 0.01, *df* = 0, *p* = 0.043, tau² = 0.000		
Arrest records	1.61	1.30-1.99	25	1.60	1.20-2.12	13
Other	2.20	1.11-4.34	3	1.57	0.44-5.60	1
Comparisons declined/rejected drug court involvement	Q_B = 0.02, *df* = 0, *p* = ., tau² = 0.067					
Yes	1.95	1.01-3.78	1	----	----	--
No	1.85	1.47-2.39	18	----	----	--
Historical comparisons	Q_B = 0.23, *df* = 1, *p* = 0.635, tau² = 0.062			Q_B = 1.86, *df* = 0, *p* = ., tau² = 0.050		
Yes	1.99	1.32-3.00	5	7.78	0.53-115.0	1
No	1.84	1.40-2.40	14	1.45	1.07-1.97	12

Overall attrition>20%	Q_B = 1.33, *df* = 1, *p* = 0.250, tau² = 0.155			Q_B = 0.15, *df* = 0, *p* = ., tau² = 0.062		
Yes	2.58	1.18-4.65	3	1.38	0.63-3.04	1
No	1.60	1.30-1.97	25	1.63	1.21-2.19	13
Differential attrition>20%	Q_B = 0.61, *df* = 1, *p* = 0.435, tau² = 0.162					
Yes	2.40	0.92-6.26	2	----	----	----
No	1.63	1.32-2.00	26	----	----	----
Methodological rigor rating	Q_B = 21.76, *df* = 3, *p* = 0.001, tau² = 0.000			Q_B = 8.44, *df* = 2, *p* = 0.015, tau² = 0.000		
Weak	1.99	1.49-2.65	3	7.78	0.55-110.9	1
Standard	2.56	1.66-3.96	7	2.13	1.29-3.51	4
Strong	1.99	1.49-2.65	5	1.78	1.30-2.44	5
Experimental	1.15	0.79-1.67	4	1.03	0.70-1.51	3

10.8 MEAN RANDOM EFFECTS ODDS RATIOS OF ADULT DRUG COURTS OVER TIME

	General Recidivism			Drug Recidivism		
Between evaluation comparisons	Mean	95% C.I.	*k*[a]	Mean	95% C.I.	*k*
Recidivism measure overlaps with drug court	Q_B = 1.66, *df* = 2, *p* = 0.435, tau² = 0.213			Q_B = 1.77, *df* = 2, *p* = 0.412, tau² = 0.766		
Complete overlap	1.57	1.27-1.94	26	1.35	0.71-2.56	8
Partial overlap	1.77	1.51-2.06	47	2.04	1.42-2.94	26
No overlap	1.49	1.16-1.92	18	1.41	0.74-2.69	8
All Available Outcomes by Follow-up Length	Q_B = 1.33, *df* = 3, *p* = 0.723, tau² = 0.141			Q_B = 6.39, *df* = 3, *p* = 0.094, tau² = 0.807		
12 months or less	1.62	1.37-1.92	43	1.50	0.92-2.46	15
12.01-24 months	1.65	1.31-2.07	26	1.56	0.62-3.93	4
24.01-36 months	1.70	1.09-2.66	6	3.72	1.92-7.20	8
36+ months	2.22	1.53-3.22	8	1.67	0.84-3.31	7
Evaluations that measure recidivism at 12 and 24 months						
12 months	1.74	1.40-2.17	21	-----	-----	--

24 months	1.66	1.38-1.98	21	-----	-----	--
Evaluations that measure recidivism at 12, 24, and 36 months						
12 months	1.71	1.18-2.48	8	-----	-----	--
24 months	1.72	1.29-2.29	8	-----	-----	--
36 months	1.80	1.44-2.24	8	-----	-----	--

[a] Number of evaluations

10.9 MEAN RANDOM EFFECTS ODDS RATIOS OF ADULT DRUG COURTS BY DRUG COURT FEATURES

	General Recidivism[a]			Drug Recidivism[b]		
Variable	Mean	95% C.I.	*k*[c]	Mean	95% C.I.	*k*
Drug court maturity	Q_B = 4.95, *df* = 2, *p* = 0.084, tau² = 0.228			Q_B = 3.43, *df* = 1, *p* = 0.064, tau² = 0.979		
New	1.86	1.59-2.16	55	2.21	1.44-3.39	24
Developing	1.32	0.97-1.79	12	1.09	0.50-2.35	7
Mature	1.63	1.17-2.27	10	1.61	0.22-11.81	1
More than 3 phases	Q_B = 0.29, *df* = 1, *p* = 0.657, tau² = 0.188			Q_B = 3.18, *df* = 1, *p* = 0.075, tau² = 0.909		
Yes	1.60	1.29-2.00	21	1.07	0.51-2.21	8
No	1.69	1.45-1.96	50	2.06	1.38-3.06	25
More than 2 drug tests/week	Q_B = 1.55, *df* = 1, *p* = 0.214, tau² = 0.172			Q_B = 0.06, *df* = 1, *p* = 0.802, tau² = 0.109		
Yes	1.42	1.11-1.81	16	1.46	1.07-2.00	7
No	1.62	1.32-1.99	24	1.41	0.99-2.02	6
More than 2 status hearings/month	Q_B = 0.05, *df* = 1, *p* = 0.816 tau² = 0.176			Q_B = 28.58, *df* = 1, *p* = 0.001, tau² = 0.020		
Yes	1.57	1.22-2.03	16	2.26	1.64-3.11	4
No	1.54	1.23-1.92	19	1.37	1.10-1.70	7
More than 3 treatment meetings/week	Q_B = 2.91, *df* = 1, *p* = 0.088, tau² = 0.179			Q_B = 1.48 *df* = 0, *p* = ., tau² = 0.099		
Yes	1.50	1.08-2.07	9	1.60	0.75-3.44	1
No	1.80	1.17-2.78	6	1.32	0.91-1.92	5
Minimum time to graduation	Q_B = 1.07, *df* = 2,			Q_B = 3.03, *df* = 2,		

Variable	General Recidivism[a] Mean	95% C.I.	k[c]	Drug Recidivism[b] Mean	95% C.I.	k
	p = 0.586, tau^2 = 0.155			p = 0.220, tau^2 = 0.765		
Less than 12 months	1.58	1.29-1.94	22	1.54	0.82-2.86	9
12-15 months	1.77	1.54-2.03	47	2.03	1.41-2.92	25
More than 15 months	1.61	1.22-2.12	12	0.78	0.24-2.55	3
Method of Disposition	Q_B = 0.40, df = 2, p = 0.820, tau^2 = 0.195			Q_B = 5.50, df = 2, p = 0.064, tau^2 = 0.819		
Pre-plea	1.74	1.37-2.19	22	1.52	0.89-2.60	13
Post-plea	1.60	1.35-1.89	38	1.52	0.91-2.54	13
Uses both	1.57	1.19-2.06	13	4.51	1.97-10.33	6
Charges dismissed upon graduation	Q_B = 0.87, df = 1, p = 0.350, tau^2 = 0.167			Q_B = 6.18, df = 1, p = 0.013, tau^2 = 0.020		
Yes	1.65	1.40-1.95	36	1.54	1.35-1.76	19
No	1.50	1.21-1.86	20	1.21	0.97-1.49	7
Graduation rate	Q_B = 12.79, df = 2, p = 0.001, tau^2 = 0.172			Q_B = 10.18, df = 2, p = 0.006, tau^2 = 0.082		
.00-.25	1.91	1.38-2.64	11	1.40	0.98-2.01	5
.26-.50	1.41	1.20-1.64	39	1.29	1.06-1.57	17
.51-.75	2.17	1.65-2.87	14	2.48	1.48-4.17	2
Single treatment provider	Q_B = 0.00, df = 1, p = 0.984, tau^2 = 0.182			Q_B = 0.10, df = 1, p = 0.753, tau^2 = 0.099		
Yes	1.55	1.18-2.04	16	1.48	0.98-2.25	6
No	1.56	1.29-1.87	30	1.56	1.25-1.95	12

[a] If three large positive effect sizes are removed, there are no substantive differences.
[b] If two large positive effect sizes are removed, the substantive findings are highly similar to these; however, the variance component shrinks to 0.10 or less.
[c] Number of evaluations

10.10 MEAN RANDOM EFFECTS ODDS RATIOS OF JUVENILE DRUG COURTS BY DRUG COURT FEATURES

Variable	General Recidivism Mean	95% C.I.	k	Drug Recidivism Mean	95% C.I.	k
Drug court maturity	Q_B = 4.91, df = 1, p =0.027, tau^2 = 0.068			Q_B = 8.91, df = 1, p =0.003, tau^2 = 0.099		

	General Recidivism			Drug Recidivism		
Variable	Mean	95% C.I.	k	Mean	95% C.I.	k
New	1.32	1.05-1.67	17	0.50	0.28-0.91	4
Developing	1.20	0.92-1.57	9	1.42	0.92-2.20	7
Mature	2.29	1.31-4.00	1	----	----	--
More than 3 phases	Q_B = 0.15, *df* = 1, *p* = 0.700, tau² = 0.124			Q_B = 4.35, *df* = 1, *p* =0.037, tau² = 0.169		
Yes	1.27	0.98-1.65	16	1.32	0.85-2.07	8
No	1.18	0.84-1.66	8	0.59	0.32-1.11	4
More than 2 drug tests/week	Q_B = 0.61, *df* = 1, *p* = 0.430, tau² = 0.112			Q_B = 16.98, *df* = 0, *p* = ., tau² = 0.000		
Yes	2.03	1.47-2.81	2	0.53	0.22-1.26	1
No	1.31	0.69-2.47	1	1.82	0.99-3.33	2
More than 2 status hearings/month	Q_B = 4.86, *df* = 1, *p* = 0.027, tau² = 0.101			Q_B = 10.97, *df* = 0, *p* = ., tau² = 0.111		
Yes	1.65	1.06-2.56	3	0.53	0.18-1.57	1
No	1.43	1.00-2.04	6	1.68	0.71-3.98	2
More than 3 treatment meetings/week	Q_B = 18.44, *df* = 1, *p* = 0.000, tau² = 0.036					
Yes	2.03	1.53-2.69	4	----	----	--
No	1.07	0.71-1.61	2	----	----	--
Minimum time to graduation	Q_B = 0.07, *df* = 1, *p* = 0.795, tau² = 0.100			Q_B = 1.06 *df* = 1, *p* = 0.304, tau² = 0.290		
Less than 12 months	1.38	1.11-1.73	17	0.80	0.40-1.60	4
12-15 months	1.32	0.99-1.76	13	1.23	0.72-2.12	7
More than 15 months						
Method of Disposition	Q_B = 1.45, *df* = 0, *p* = ., tau² = 0.161					
Pre-plea	----	----	--	----	----	--
Post-plea	1.26	0.98-1.63	18	----	----	--
Uses both	0.58	0.17-2.00	1	----	----	--
Charges dismissed upon graduation	Q_B = 1.63, *df* = 1, *p* = 0.202, tau² = 0.113			Q_B = 9.21 *df* = 0, *p* = ., tau² = 0.000		
Yes	1.71	1.22-2.40	8	0.46	0.23-0.91	2
No	1.35	0.85-2.13	3	1.10	0.47-2.53	1

	General Recidivism			Drug Recidivism		
Variable	Mean	95% C.I.	k	Mean	95% C.I.	k
Graduation rate	Q_B = 10.86, *df* = 1, *p* = 0.001, tau² = 0.054			Q_B = 5.93, *df* = 1, *p* = 0.015, tau² = 0.178		
.00-.25	2.29	1.38-3.80	1	----	----	--
.26-.50	1.11	0.88-1.40	15	0.76	0.47-1.22	7
.51-.75	1.63	1.24-2.13	10	2.01	0.97-4.18	3
Single treatment provider	Q_B = 0.15, *df* = 1, *p* = 0.699, tau² = 0.105			Q_B = 22.19 *df* = 0, *p* = ., tau² = 0.000		
Yes	1.62	0.79-3.35	3	3.19	1.32-7.72	1
No	1.46	1.11-1.93	8	0.77	0.42-1.41	2

10.11 MEAN RANDOM EFFECTS ODDS RATIOS OF DWI DRUG COURTS BY DRUG COURT FEATURES

	General Recidivism			Drug Recidivism		
Variable	Mean	95% C.I.	k	Mean	95% C.I.	k
Drug court maturity						
New	----	----	--	----	----	--
Developing	----	----	--	----	----	--
Mature	----	----	--	----	----	--
More than 3 phases	Q_B = 4.77, *df* = 1, *p* = 0.03, tau² = 0.000			Q_B = 0.094, *df* = 1, *p* = 0.760, tau² = 0.000		
Yes	2.37	1.67-3.32	10	1.87	1.21-2.88	7
No	1.87	1.41-2.48	4	1.74	1.30-2.31	3
More than 2 drug tests/week	Q_B = 1.13, *df* = 0, *p* = ., tau² = 0.000					
Yes	1.24	0.36-4.22	1	----	----	--
No	1.70	1.35-2.13	6	----	----	--
More than 2 status hearings/month						
Yes	----	----	--	----	----	--
No	----	----	--	----	----	--
More than 3 treatment						

	General Recidivism			Drug Recidivism		
Variable	Mean	95% C.I.	k	Mean	95% C.I.	k
meetings/week						
Yes	----	----	--	----	----	--
No	----	----	--	----	----	--
Minimum time to graduation	Q_B = 0.24, *df* = 2, *p* = 0.887, tau^2 = 0.0112			Q_B = 2.29, *df* = 2, *p* = 0.318, tau^2 = 0.064		
Less than 12 months	1.82	1.05-3.15	4	1.29	0.60-2.73	2
12-15 months	1.89	1.24-2.88	8	2.05	1.02-3.21	7
More than 15 months	1.70	1.08-2.70	5	1.33	0.87-2.03	3
Method of Disposition	Q_B = 0.69, *df* = 0, *p* = ., tau^2 = 0.102			Q_B = 0.37, *df* = 0, *p* = ., tau^2 = 0.063		
Pre-plea	----	----	--	----	----	--
Post-plea	1.74	1.25-2.42	11	1.42	1.01-2.00	9
Uses both	3.21	0.79-13.09	1	0.66	0.06-7.59	1
Charges dismissed upon graduation	Q_B = 5.77, *df* = 0, *p* = ., tau^2 = 0.109					
Yes	5.05	1.08-23.70	1	----	----	--
No	1.73	1.25-2.46	12	----	----	--
Graduation rate	Q_B = 12.89, *df* = 2, *p* = 0.002, tau^2 = 0.000			Q_B = 2.09, *df* = 1, *p* = 0.149, tau^2 = 0.000		
.00-.25	----	----	--	----	----	--
.26-.50	1.15	0.66-2.00	2	----	----	--
.51-.75	2.12	1.57-2.84	10	1.84	1.29-2.65	6
.75+	1.73	1.29-2.33	5	1.31	0.76-2.26	4
Single treatment provider						
Yes	----	----	--	----	----	--
No	----	----	--	----	----	--

10.12 MEAN RANDOM EFFECTS ODDS RATIOS FOR ADULT DRUG COURTS BY SAMPLE CHARACTERISTICS

	General Recidivism[a]			Drug Recidivism[b]		
Variable	Mean	95% C.I.	*k*[c]	Mean	95% C.I.	*k*
Gender composition f sample	Q_B = 1.63, *df* = 1, *p* = 0.202, tau² = 0.124			Q_B = 1.59, *df* = 1, *p* = 0.207, tau² = 0.249		
All male (90%+ male)	1.55	0.44-5.50	2	-----	-----	--
Mostly male (60-90%)	1.61	1.46-1.77	79	1.74	1.28-2.36	37
Approx. equal (59-40%)	2.06	1.42-2.97	8	0.84	0.14-5.13	2
Offender type	Q_B = 7.38, *df* = 1, *p* = 0.007, tau² = 0.147			Q_B = 0.03, *df* = 1, *p* = 0.856, tau² = 0.749		
Non-violent only	1.68	1.51-1.86	70	1.63	1.19-2.24	34
Includes violent offenders	1.25	1.03-1.52	16	1.74	0.93-3.26	8
Minor criminal history	Q_B = 2.91, *df* = 1, *p* = 0.088, tau² = 0.130			Q_B = 0.15, *df* = 1, *p* = 0.700, tau² = 0.000		
Yes	1.80	1.48-2.20	23	1.52	0.75-3.07	8
No	1.51	1.33-1.72	50	1.75	1.18-2.60	27

[a] Three large positive effect sizes were removed.
[b] If two large positive effect sizes are removed, there are no substantive differences.
[c] Number of evaluations

10.13 MEAN RANDOM EFFECTS ODDS RATIOS OF JUVENILE DRUG COURTS BY SAMPLE CHARACTERISTICS

	General Recidivism			Drug Recidivism		
	Mean	95% C.I.	k	Mean	95% C.I.	k
Gender composition of sample	Q_B = 0.96, *df* = 1, *p* = 0.327, tau² = 0.089			Q_B = 1.59, *df* = 1, *p* = 0.207, tau² = 0.249		
All male (90%+ male)	0.96	0.46-2.00	2	1.90	0.71-5.11	2
Mostly male (60-90%)	1.40	1.18-1.65	32	0.95	0.62-1.45	12
Approx. equal (59-40%)	----	----	--	----	----	--
Offender type	Q_B = 0.04, *df* = 1, *p* = 0.839, tau² = 0.095			Q_B = 0.01, *df* = 0, *p* = ., tau² = 0.000		
Non-violent only	1.50	1.20-1.88	18	0.58	0.35-0.96	5
Includes violent offenders	1.57	0.98-2.50	4	0.59	0.17-2.03	1

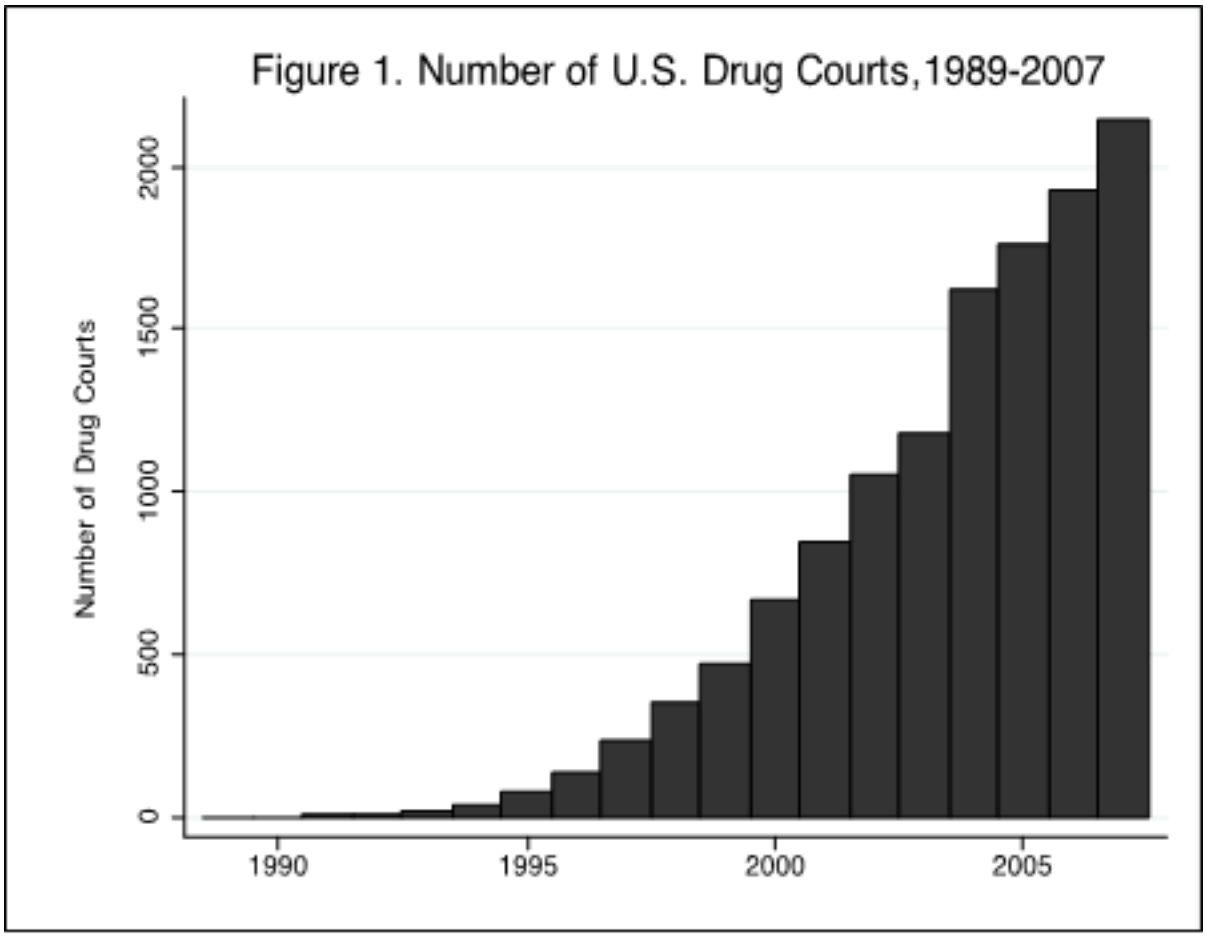
Minor criminal history	Q_B = 0.11, *df* = 1, p = 0.736, tau^2=0.136					
Yes	1.22	0.59-2.54	2	----	----	--
No	1.35	1.04-1.76	17	----	----	--

10.14 MEAN RANDOM EFFECTS ODDS RATIOS OF DWI DRUG COURTS BY SAMPLE CHARACTERISTICS

	General Recidivism			Drug Recidivism		
	Mean	95% C.I.	k	Mean	95% C.I.	k
Gender composition of sample	Q_B = 23.83, *df* = 0, p = ., tau^2 = 0.000			Q_B = 8.60, *df* = 0, p = ., tau^2 = 0.000		
All male (90%+ male)	0.73	0.42-1.27	1	0.73	0.42-1.27	1
Mostly male (60-90%)	1.91	1.61-2.25	19	1.80	1.42-2.28	12
Approx. equal (59-40%)	----	----	--	----	----	--
Offender type						
Non-violent only	----	----	--	----	----	--
Includes violent offenders	----	----	--	----	----	--
Minor criminal history	Q_B = 1.955, *df* = 1, p = 0.213, tau^2 = 0.100			Q_B = 0.01, *df* = 0, p = ., tau^2 = 0.101		
Yes	1.40	0.54-3.64	2	1.57	0.42-5.93	1
No	2.10	1.58-2.79	14	1.64	1.14-2.35	10

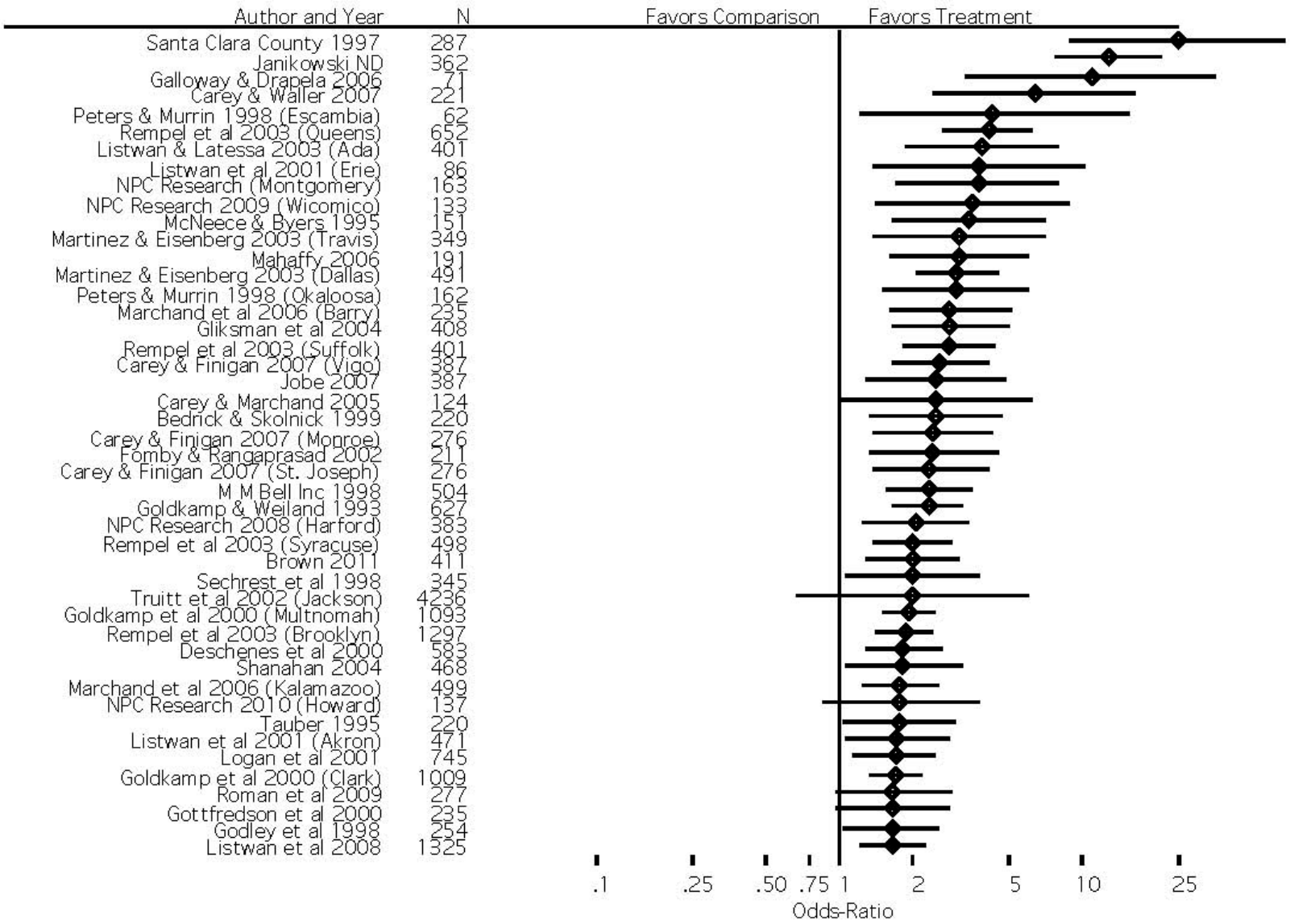
11 Figures

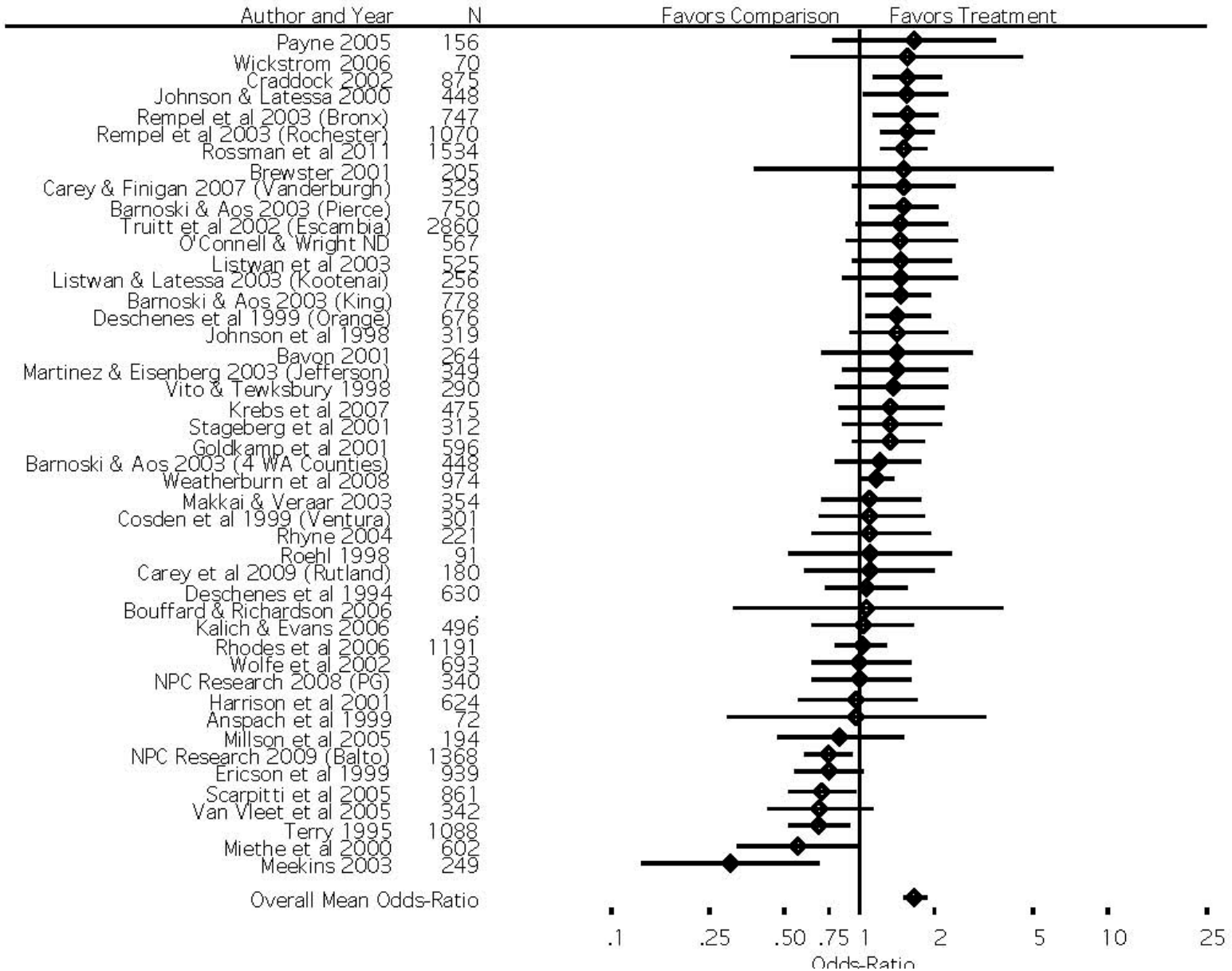
11.1 NUMBER OF U.S. DRUG COURTS, 1989-207



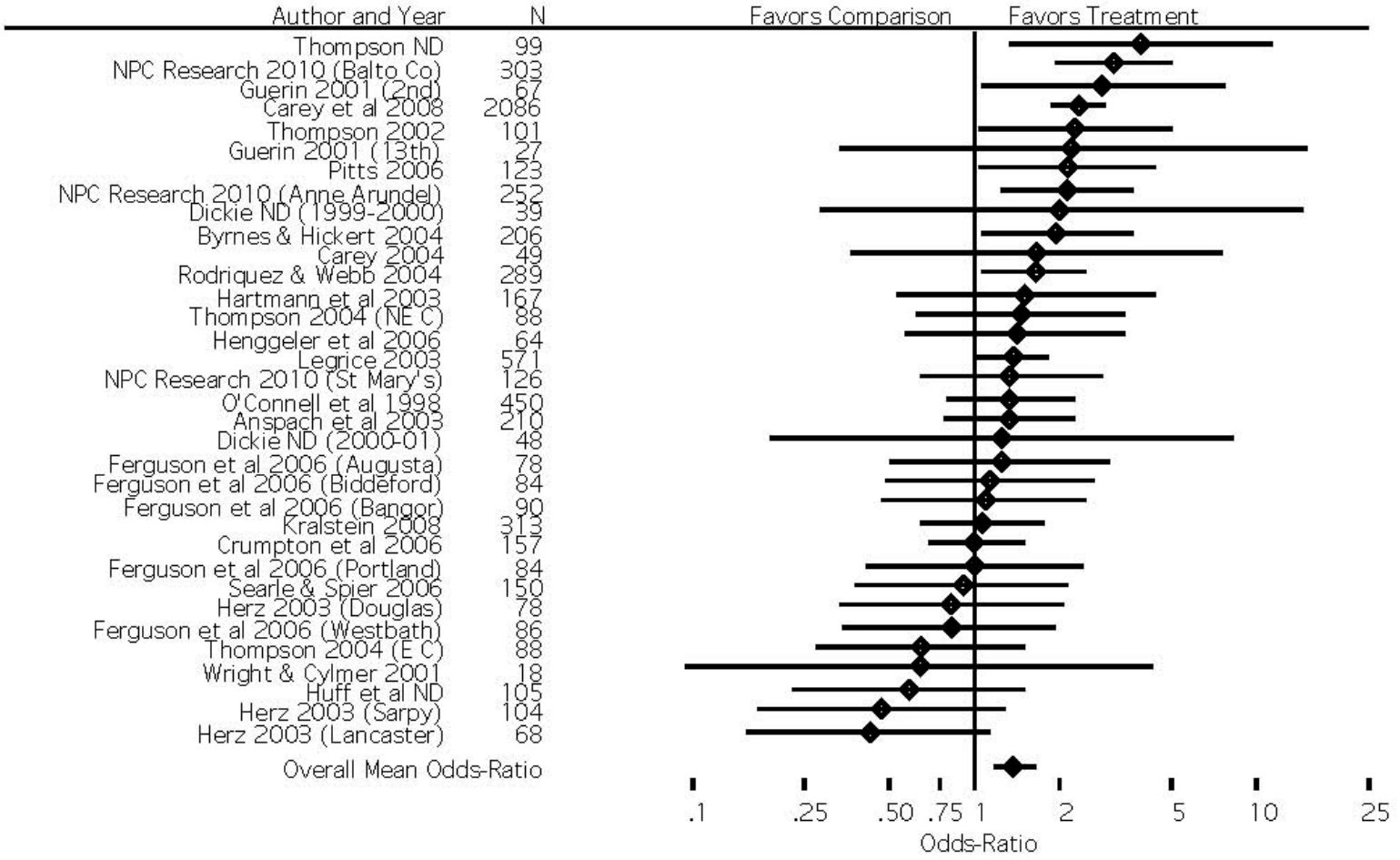
Source: Huddleston, Marlowe, and Casebolt (2008)

11.2 FOREST PLOT OF GENERAL RECIDIVISM EFFECT SIZES FOR ADULT DRUG COURTS

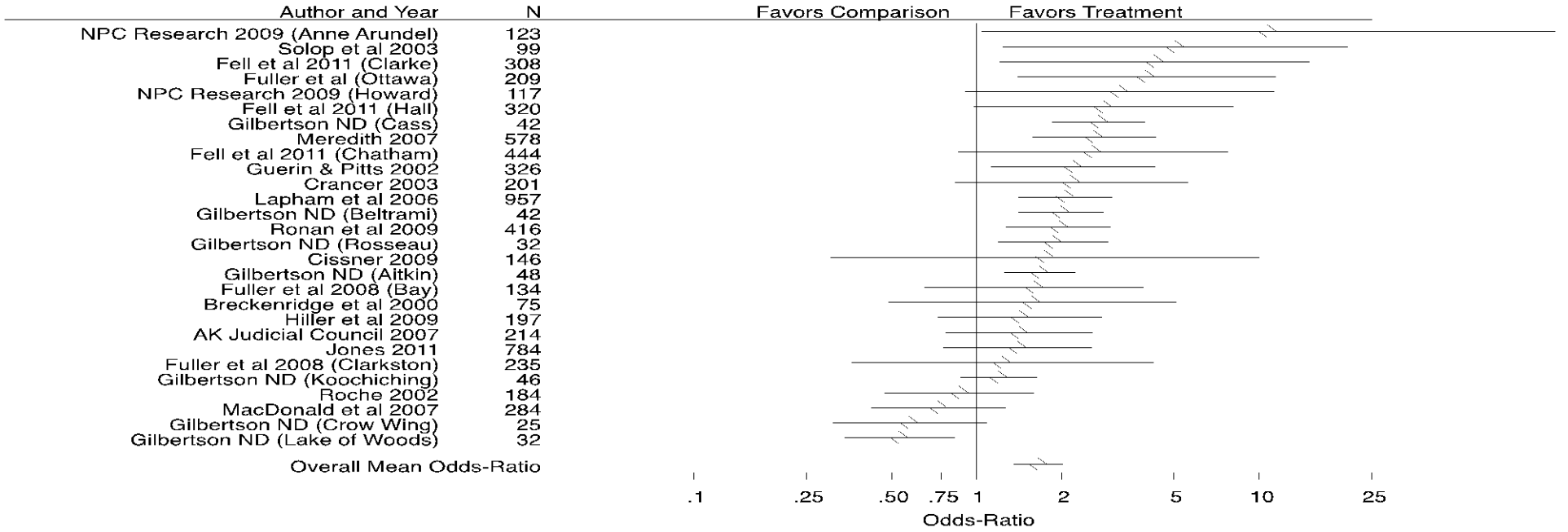



Author and Year
N
Favors Comparison
Favors Treatment
Payne 2005 156
Wickstrom 2006 70
Craddock 2002 875
Johnson & Latessa 2000 448
Rempel et al 2003 (Bronx) 747
Rempel et al 2003 (Rochester) 1070
Rossman et al 2011 1534
Brewster 2001 205
Carey & Finigan 2007 (Vanderburgh) 329
Barnoski & Aos 2003 (Pierce) 750
Truitt et al 2002 (Escambia) 2860
O'Connell & Wright ND 567
Listwan et al 2003 525
Listwan & Latessa 2003 (Kootenai) 256
Barnoski & Aos 2003 (King) 778
Deschenes et al 1999 (Orange) 676
Johnson et al 1998 319
Bavon 2001 264
Martinez & Eisenberg 2003 (Jefferson) 349
Vito & Tewksbury 1998 290
Krebs et al 2007 475
Stageberg et al 2001 312
Goldkamp et al 2001 596
Barnoski & Aos 2003 (4 WA Counties) 448
Weatherburn et al 2008 974
Makkai & Veraar 2003 354
Cosden et al 1999 (Ventura) 301
Rhyne 2004 221
Roehl 1998 91
Carey et al 2009 (Rutland) 180
Deschenes et al 1994 630
Bouffard & Richardson 2006
Kalich & Evans 2006 496
Rhodes et al 2006 1191
Wolfe et al 2002 693
NPC Research 2008 (PG) 340
Harrison et al 2001 624
Anspach et al 1999 72
Millson et al 2005 194
NPC Research 2009 (Balto) 1368
Ericson et al 1999 939
Scarpitti et al 2005 861
Van Vleet et al 2005 342
Terry 1995 1088
Miethe et al 2000 602
Meekins 2003 249
Overall Mean Odds-Ratio
.1
.25
.50
.75
1
2
5
10
25
Odds-Ratio

11.3 FOREST PLOT OF GENERAL RECIDIVISM EFFECT SIZES FOR JUVENILE DRUG COURTS



11.4 FOREST PLOT OF GENERAL RECIDIVISM EFFECT SIZES FOR DWI DRUG COURTS



12 Appendices

12.1 ELIGIBILITY FORM

|___|___|___| Document Identification Number

_____ First Author's Last Name

yes *no*

|__| |__| This study assessed a drug court program
Note: A drug court is defined as a specialized court that combines substance abuse treatment, frequent UA testing, and judicial monitoring of client progress.

|__| |__| This study included a comparison group that received either (a) no treatment, (b) treatment as usual, or (c) a minimal treatment intervention that is clearly hypothesized to be less effective.

|__| |__| This study reported a post-program entry measure of drug use and/or criminal behavior that is measured in the same manner for both groups.

|__| |__| This study reported sufficient information to calculate an effect size

[If “yes” to all of the above, then mark this study as “eligible” in the database]

|__| |__| The study is ineligible but it is a review article that is relevant to this project.

[if "yes" to the above question, then mark this document as a “relevant review” in the database]

[If “no” to all of the above questions, then mark this study as “ineligible” in the database]

12.2 CODING FORMS

12.2.1 Study Level Code Sheet

Identifying Information

Study (document) identifier [StudyID]

If multiple documents were used to code this study, indicate the supplemental study ID numbers

Cross references document identifier [CROSREF1]

Cross references document identifier [CROSREF2]

Cross references document identifier [CROSREF3]

Coder's initials [Coder]

Date coded [Date]

Author: [Author]

Publication type [PubType]

1 Book **4** Gov't Report, State/local

2 Book Chapter **5** Journal (peer reviewed)

3 Gov't Report, Federal **6** Unpublished (tech report, convention paper, dissertation

Year of publication:

Number of different "modules" included in report [MODS]

Is the same control/comparison group used in different modules? **(1 = Yes; 0 = No)** [SAME_CG]

12.2.2 Treatment-Comparison Contrast Level Code Sheet

A study may report on multiple independent evaluations, such as independent treatment and control group contrasts, or may have a design that includes multiple interventions of interest contrasted with a single control group. Each of these treatment/control contrasts of interest is treated as a separate "module" for coding purposes. Note that the treatment groups across modules must have independent (non-overlapping) subjects. A single control group may be used in more than one module.

Identifying Information

Study (document) identifier [StudyID]

Module identifier [ModID]

Coder's initials [CoderMod]

Name/Jurisdiction of drug court: (**text**) [DC_Name]

Year first opened (i.e., first client admitted): (**text**) [YearOpen]

Program description: (**text**) [ProgDes1]

Type of drug court [DC_Type]

1 Adult drug court (i.e., adults with illicit substance abuse problems)

2 Juvenile drug court

3 DWI/DUI drug court (i.e., offenders predominantly charged with DWI/DUI offenses)

Court features

Number of phases **(0 = if doesn't use phases, -99 = not reported)** [Phases]

Number of drug tests per week in phase 1 **(-99 = not reported)** [UAs]

Number of treatment meetings/week in phase 1 [TxMeet]

Note: AA/NA are not counted. **(-99 = not reported)**

Number of status hearings/month in phase 1 **(-99 = not reported)** [StatHear]

Graduation rate (number of graduations divided by number of **terminated** clients) **(-99 = not reported)** [GradRate]

What happens to charges/sentence upon graduation? [GradChrg]

1 Dismissed/Expunged

2 Reduced

3 Discretionary (i.e., judge decides on case-by-case basis)

9 Not reported

One treatment provider [OneTreatr]

0 No (multiple providers)

1 Yes

9 Not reported

Method of case processing [CaseProc]

1 Pre-plea

2 Post-plea

3 Uses both pre- and post-plea case processing

9 Not reported

Length of primary intervention in months (weeks/4.3)

a Minimum [TxMon1]

b Maximum [TxMon2]

c Mean [TxMon3]

d Fixed (same for all subjects) [TxMon4]

Length of aftercare or follow-up program component (weeks/4.3) [TxAfterM]

Describe the program for the **comparison group** if other than no treatment or treatment as usual. [ProgDes 2]

(text)

What happens to the comparison group? [CompGrp]

1 No treatment

2 Historical controls

3 Eligible but not referred to drug court

4 Treatment as usual (e.g., probation)

5 Random assignment

6 Other

9 Cannot tell

Methodological Rigor

Use of control variables in statistical analyses to account for initial group differences **(1=Yes; 0 = No)** [CntrlVar]

Use of random assignment to conditions **(1=Yes; 0 = No)** [Random]

Use of subject level matching **(1=Yes; 0 = No)** [Matching]

Measurement of prior criminal involvement; not necessarily arrest **(1=Yes; 0 = No)** [PreTest]

Rating of initial group similarity **(7=highly similar; 1=highly dissimilar)** [SimRate]

Anchors:

7 Randomized design large N or small N with matching

5 Nonrandomized design with strong evidence of initial equivalence

1 Nonrandomized design, comparison group highly likely to be different or known different that are related to future recidivism

Was attrition discussed in the study reported? **(1=Yes; 0 = No)** [Attrit1]

Is there a potential generalizability threat from overall attrition? [Attrit2]

0 No

1 yes

8 N/A, no attrition problem

9 cannot tell

Is there a potential threat from differential attrition? [Attrit3]

(same as above)

Did the statistical analysis of outcome effects attempt to control for differential attrition effects? [Attrit4]

(1=Yes; 0=No; 8=NA)

Use of statistical significance testing **(1=Yes; 0 = No)** [SigTest]

Maryland methodology rating **(see Maryland scale)** [MethScor]

2 A comparison group is present but lacks comparability to the treatment group

3 A comparison group is present but differs slightly from the program group

4 A comparison group is present and it is very similar to program group, or a comparison group is present but it differs slightly from the program group, however, the data analysis controls for observed differences, or random assignment with large attrition

5 Random assignment and analysis of comparable program and comparison groups, including controls for attrition

Notes on Methodology

(text)

12.2.3 Level Code Sheet

Since a study may report results separately for distinct samples, a sample is a separate "level" in the coding scheme. For example if a study reports the results separately for

Identifying Information

Study (document) identifier	[StudyID]
Module identifier	[ModID]
Sample identifier (Note: each sample within a study gets a unique number)	[SampID]
Coder Initials	[CoderSmp]

Sample Description

Sample description treatment group (extent of prior history, mean number of arrests, convictions, etc.) [SampDes1]

(Text)

Sample description comparison group (location, level of security, prior history, etc.) [SampDes2]

(Text)

Total number of individuals in treatment group at beginning of study	[TxN]
Total number of individuals in comparison group at beginning of study	[CgN]

Note: Above must equal the total sample size prior to any attrition. If multiple samples per module are being coded, the sum across samples must equal the total sample size prior to any attrition.

Approximate age range of study participants [Age]

1	Adolescent (12 to 18)	**4**	Adolescent and young adult
2	Young Adult (19 to 25)	**5**	Adolescent and adult
3	Adult (18+)	**9**	Unspecified or cannot tell

Young age included in sample (**99 if unknown)** [YngAge]

Oldest age included in sample (**99 if unknown)** [OldAge]

Exact proportion of males in sample if reported [Males]

Approximate gender description of sample [Sex]

1 All males (>90%)

2 More males than females (60% to 90% males)

3 Roughly half males and half females

4 More females than males (60% to 90% females)

5 All females (>90%)

9 Cannot tell

Offender type general categories [SampType]

1 Violent, person crimes

2 Nonviolent, nonperson crimes

3 Mixed

12.2.4 Outcome (DV) Level Code Sheet

Identifying Information

Study (document) identifier	[StudyID]
Outcome identifier (each coded outcome within a study gets a unique number)	[OutID]
Coder Initials	[CoderDV]

Outcome Information

Outcome label (label used in report)

[label]

(text)

Recidivism construct represented by this measure **(1=Yes; 0 = No)**

A) **A**	Arrest	[DV1]
b	Conviction	[DV2]
c	Reinstitutionalization / reincarceration	[DV3]
d	Revocation	[DV4]
e	Technical supervision violation	[DV5]
f	Drug use	[DV6]
g	Other indicator of criminal involvement	[DV7]

Specific types of offenses included in recidivism measure **(1=Yes; 0 = No)**

B) **A**	All offenses	[DVType1]
b	Drug offenses (including measures of drug use)	[DVType2]
c	Person offenses, sexual	[DVType3]
d	Person offenses, nonsexual	[DVType4]
e	Person offenses, unspecified	[DVType5]
f	Property offenses	[DVType6]
g	Technical supervision or status offense	[DVType7]
h	Other:	[DVType8]

Type of measurement scale [Scale]

1 Dichotomy
2 Tricotomy
3 4-9 discrete ordinal categories
4 >9 discrete ordinal categories or continuous

Source of data [Source]

1 Self-report
2 Other report (e.g., teacher, parent)
3 Official record (e.g., school, police, probation, court, institution)
4 Other (e.g., urinalysis)
9 Cannot tell

Is this a valid or reasonable measure of recidivism? [Valid]
(1 = questionable; 2 = acceptable)

12.2.5 Effect Size Level Code Sheet

Identifying Information

Study identifier	[StudyID]
Module identifier	[ModID]
Sample identifier	[SampID]
Outcome identifier	[OutID]
Effect size identifier (number each effect size within a study sequentially)	[ESID]
Coder's Initials	[CoderES]

Effect Size Information

Effect size type [ES_Type]

1 Baseline (pretest; prior to start of intervention)

2 Post-test (first measurement point, post intervention)

3 Follow-up (all subsequent measurement points, post intervention)

Which group does the raw effect (difference) favor (ignoring statistical significance)? [ES_Direc]

1 Treatment group

2 Comparison group

3 Neither (ES equal zero)

9 Cannot tell (ES cannot be used if this option is selected)

Is this difference reported as statistically significant by the investigator? ES_Sig]

0 No **8** Not tested

1 yes **9** Cannot tell

Time frame in months captured by measure (weeks/4.3)

a	Minimum	[ES_Time1]
b	Maximum	[ES_Time2]
c	Mean	[ES_Time3]
d	Fixed (same for all subjects)	[ES_Time4]

Does recidivism tracking period overlap with participation in drug court? [Es_PstTx]

1 Completely

2 Partially

3 No overlap (recidivism is after participation ends)

9 Can't tell

Effect Size Data

Treatment group sample size for this effect size [ES_TxN]

Comparison group sample size for this effect size [ES_CgN]

Treatment group mean (clearly indicate decimal point) [ES_TxM]

Comparison group mean (clearly indicate decimal point) [ES_CgM]

Are the above mean adjusted? **(1=Yes; 0 = No)** [ES_MAdj]

Treatment group standard deviation (clearly indicate decimal point) [ES_TxSD]

Comparison group standard deviation (clearly indicate decimal point) [ES_CgSD]

Treatment group standard error (clearly indicate decimal point) [ES_TxSE]

Comparison group standard error (clearly indicate decimal point) [ES_CgSE]

Treatment group; number successful [ES_TxNS]

Comparison group; number successful [ES_CgNS]

Treatment group; proportion successful [ES_TxPS]

Comparison group; proportion successful [ES_CgPS]

Are the above proportion adjusted for initial group nonequivalence? [ES_PAdj]
(1=Yes; 0 = No)

t-value from an independent t-test or square root of F-value from a one-way analysis of variance with one *df* in the numerator (only two groups)	[ES_T]
Exact probability for a t-value from an independent *t*-test or square root of F-value from a one-way analysis of variance with one *df* in the numerator (only two groups)	[ES_T_P]
Chi-square value with *df* = 1 (2 by 2 contingency table)	[ES_ChiSQ]
Correlation coefficient (point biserial)	[ES_RPB]
Correlation coefficient (phi)	[ES_RPHI]
Computer Calculated ES	[ES]
Hand Calculated ES	[HAND_ES]
Hand Calculated SE of ES	[HAND_SE]