

ABSTRACT

Title of dissertation: **CAN DRUG COURTS IMPROVE PUBLIC SAFETY?
EXPLORING THE IMPACTS OF DRUG COURT ON
CRIME**

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Drug courts represent one of the largest and most widespread criminal justice programs specifically developed to provide treatment and intensive supervision to drug-involved offenders. Most of the literature about the effects of drug court programs involves individual-level analyses of recidivism or drug use for program participants. Very little is known about the broader community-wide impact of drug courts on public safety measures. The current research uses a subset of 63 drug court jurisdictions (cities and counties) drawn from a systematic review of drug court programs to assess the impact of program implementation on crime and arrest rates. A fixed-effects analysis was used to assess whether drug court implementation was associated with significant changes in specific types of violent and property crime rates. Changes in arrest rates for violent, property and drug crimes were also examined, and differential effects were

explored based on effectiveness of the drug court in reducing participant recidivism and jurisdictional population size. Results indicate that drug courts are associated with decreases in overall crime rates, with marked decreases in burglary, property, and robbery rates. Drug court implementation was associated with increases in drug arrests and decreases in homicide arrests. Small jurisdictions with average populations of less than 100,000 people were found to have a different pattern of results when measuring both crime and arrest rates. These results are discussed within the context of understanding the broader policy impacts of drug court implementation.

CAN DRUG COURTS IMPROVE PUBLIC SAFETY?
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by

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CHAPTER 1: INTRODUCTION

Drug court programs offer a holistic response to drug offenders' needs that address the root of the problem (drug addiction) while offering support and monitoring. The first drug courts in the United States were implemented as a new, delineated strategy to manage high recidivism rates within the drug offender population during the "war on drugs" in the 1980s. Evaluations of individual drug court programs were conducted to determine whether the combination of intensive supervision and addiction treatment was better able to decrease recidivism rates within this offender population than the traditional incarcerative or probation strategies. Positive findings among the first drug court evaluations (Goldkamp and Weiland, 1993; Tauber, 1995; Broward County Commission Auditor's Office, 1995; Department of Community Corrections, 1994; Deschenes, et al., 1995) spurred the continued proliferation of drug court programs across the country. The body of drug court literature grew with the need for continued monitoring and evaluation of this popular new public policy.

Based on two decades of research, drug courts are touted as the best way to reduce drug use and recidivism among drug-addicted offenders. "The effectiveness of Drug Courts is not a matter of conjecture... We know beyond a reasonable doubt that Drug Courts significantly reduce drug use and crime..." (NADCP, 2010: 1). The majority of studies of drug courts consist of evaluations of individual-level outcomes and meta-analyses of these studies (e.g., Gottfredson et al., 2003; Mitchell, et al., 2012; Wilson, et al., 2006). These studies generally focus on process measures and reductions in drug court participants' recidivism, while a handful have also looked at drug use and other psychosocial outcomes. It is important to note that most of the literature in this area is

methodologically weak, with few randomized studies and only a modest number of rigorous quasi-experimental studies (Mitchell, et al., 2012).

Several meta-analyses of drug court evaluations find that drug courts are more effective for reducing recidivism among drug offenders than traditional probation processing. In a systematic review of 55 drug court evaluations, Wilson, Mitchell, and MacKenzie (2006) found that average reductions in recidivism were between 14 and 26% for drug courts over traditional probation. Similarly, in a systematic review of 154 independent evaluations of drug courts, Mitchell and colleagues found that drug courts significantly reduce general and drug-related recidivism and that those effects appear to persist for at least three years. The authors relate the mean effect size as analogous to a drop in recidivism from 50% for non-participants to approximately 38% for participants (Mitchell, et al., 2012). Further, drug courts with high-fidelity program implementation (i.e., provide access to effective treatment and consistent intensive supervision) are much more effective, reducing recidivism over probation comparison groups by up to 40% (Shaffer, 2006; Lowenkamp, et al., 2005).

Increasing public safety through reduction in drug crimes and drug use-related crimes is generally indicated as one of the primary goals of drug court programs. The National Association of Drug Court Professionals (NADCP) (2008: 2) includes as its first guiding principle that sentencing of drug offenders should “reduc[e] substance abuse and criminal recidivism and improv[e] the health and psychological functioning of *individuals, their families and their communities*” (emphasis added). Further, the NADCP (no date) states as its guiding mission that the expansion of drug courts is necessary to truly impact the cycle of addiction in the United States. The NADCP’s latest

research brief indicates that the next big hurdle for drug court proliferation is to maintain fidelity of the program while expanding services to a larger proportion of the drug offender population (NADCP, 2010).

This rationale follows a specific deterrence framework through the use of therapeutic jurisprudence (Wexler, 1993). One of the goals of the criminal justice system is to deter or prevent future crime out of the fear of legal punishment (Gibbs, 1975). The speculation that drug courts can reduce crime is rooted in the notion that drug courts, as a form of therapeutic jurisprudence, offer a legal punishment that reduces future offending for the population of drug offenders it is able to reach, thus reducing aggregate crime within the larger community.

Since most drug court participants are generally drug-addicted offenders that are deemed likely to fail on standard supervision (Cooper, 1997; Tauber, 1998; Rossman et al., 2011), the drug court model directly impacts those that are likely to contribute to the crime and arrest rate. This rationale points specifically to a decrease in crime by reducing the likelihood that drug court participants will reoffend (specific deterrence), subsequently reducing the potential offender population. Scaling up drug courts to reach a larger proportion of the drug offender population should lead to measurable reductions in crime rates.

The only evidence to date that supports this hypothesis is an extensive simulation study measuring the predicted benefits of offering drug treatment to all eligible drug offender populations. Bhati, Roman, and Chalfin (2008) estimate that approximately half (about 55,000) of the total eligible drug offender population (about 100,000) is being served by drug court programs. This indicates that of the small proportion of drug

offenders eligible for drug court participation, only half of those eligible offenders are actually being supervised in drug court programs. The other drug offenders that are eligible to participate are not interested, capable, or otherwise able to get involved with drug court. The authors find that the benefit to cost ratio of expanding drug courts to meet the needs of all currently eligible offenders only fractionally reduces to 2.14 from 2.21. That is, expanding drug courts is costly but the benefits still outweigh the costs. This expansion in treatment translates to a benefit to society of over \$1.17 billion. Bhati et al. (2008), however, caution that these estimates are based on simulated data and the true costs of drug courts going to scale are still unknown.

Others assert that drug courts may have a public safety impact despite the relatively small proportion of drug offenders who actually receive services. It is possible that reducing the criminal behavior of a subset of the population may be strong enough to reduce specific types of crime in which these offenders are most likely to engage (Jeglic, Maile, & Calkins-Mercado, 2011). Another mechanism may involve strengthening informal social control by promoting sobriety, reducing the likelihood of criminal participation, and providing the means by which drug court participants can become positive members of their community. Positive community membership involves guardianship of property, active surveillance of youth behavior, contributing to the local economy, and strengthening social networks. By keeping drug offenders in the community the collateral consequences that may occur if such an offender were incarcerated can be avoided (Rose & Clear, 1998).

The stated rationale through which drug courts might reduce community-wide crime is incomplete for several reasons. It is widely acknowledged that the restrictions

regarding participation and the intensive nature of the drug court model limit drug court participation to a small proportion of drug offenders within a given jurisdiction. On average, they serve a relatively small proportion of the drug offending population, estimated at 5% or less (Belenko, 2002; Sevigny, et al., 2013). One central reason for this is that drug court eligibility criteria tend to be highly restrictive -- generally serving offenders that have no previous convictions for violent crimes (Franco, 2010). It logically follows that the impact of even the most effective drug court may not be potent enough to decrease crime rates community-wide.

Although eligibility guidelines vary by state and local legislative decisions, the majority of adult drug courts do not allow the participation of violent offenders. The evidence also does not indicate that reducing drug use and abuse would have an impact on violent crime (Gottfredson, et al., 2008; Anglin and Perrochet, 1998; Johnson, et al., 1985). Based on the offender population currently served by drug courts, there is no support for the expectation that participation would have an impact on violent crime.

Finally, the effectiveness of the drug court program is likely to change if scaled-up. The current focus of the NADCP is to broaden the reach of drug court programs to a larger census of participants (NADCP, 2010). This translates to broadening the scope of programming to provide services to more participants. Studies of scale-up efforts have shown that programs delivered on a smaller scale are generally more effective than when they are scaled up (Allen, Linnan, and Emmons, 2012). It is not currently known whether drug courts are able to maintain program fidelity while also expanding their reach. The NADCP (2010) argues that standardizing the best practices of drug courts so they can be reliably implemented to more and larger programs is the next challenge for the drug court

field to address.

The purpose of the current study is to broaden the spectrum of drug court evaluation to identify the impact of drug court implementation on public safety. While there is a large body of literature that supports the overall effectiveness of drug courts in reducing crime among the individuals who receive the program (Mitchell, et al., 2012), to date, only one study has examined the aggregate-level effects of drug courts on crime and public safety outcomes in the communities that implement these programs.

The current research compares crime trends across jurisdictions that have implemented drug courts. Specifically, jurisdictions with evaluated drug courts will be included in the analysis in order to leverage the variation in timing of drug court implementation relative to changes in outcomes. If other confounding factors can be ruled out, the “drug court effect” can be isolated as the likely cause in any changes in crime rates after implementation. The FBI’s Uniform Crime Reports reported crime and arrest files are used to identify changes in specific types of violent and property crimes before and after implementation of drug court programs.

CHAPTER 2: LITERATURE REVIEW

History of Drug Courts

Addictive drug use has long been considered a social problem that should be handled through the criminal justice system. The Nixon administration set the stage for the “war on drugs” with the establishment of the Drug Enforcement Administration (DEA) in the 1970s. Reagan fueled the war on drugs in the 1980s through decreased funding for treatment and increased funding for interdiction efforts in order to manage drug use and trade in America. The most stringent federal drug policies were enacted, including mandatory minimum sentences for possession and distribution and establishment of the White House Office of National Drug Control Policy. Arrests for drug offenses doubled as these legislative efforts solidified the criminal justice system as the means by which illicit drug use was to be controlled (Tiger, 2012). Never before had jails and prisons been dominated by individuals incarcerated for illicit drug use, abuse, and dependency.

Specialized court responses to drug cases have historically been adopted as a response to increased arrests and crowded court dockets for drug crimes. The earliest form of specialized court intervention for drug cases began as a response to rising narcotics (e.g. heroin) cases in the mid-1950s caused by the introduction of mandatory minimum sentencing laws and increasingly strict drug laws. These laws, combined with more organized law enforcement strategies, dramatically increased the number of drug arrests. Chicago and New York were among the first to develop dedicated drug case dockets in courts, with New York creating the first “narcotics court” in the 1970s. These

dedicated court dockets were created to streamline drug cases as they moved through the criminal justice system. These efforts lacked standardization and offered few treatment resources to offenders, but they represent the first judicial attempts at addressing drug addiction (White and Gorman, 2000).

The concentration on drug enforcement during the mid to late 1980s brought about the introduction of modern drug courts in the United States. Specifically, the Miami-Dade County drug court program began in 1989 as a response to the considerable growth in the drug-related caseload, strain on an already taxed criminal justice system, and perceptions of a negative impact of drug-related crime and criminals on public safety (Terry, 1999). These issues were not unique to Miami's jurisdiction. By the late 1990s, there were 140 drug courts in operation and many more in the planning stages across the country (U.S. General Accounting Office, 1997).

Currently, over two decades after introduction to the American criminal justice system, there are nearly 3,000 drug courts operating throughout the United States (National Drug Court Resource Center, 2012). This proliferation is likely to continue, with the Obama administration nearly doubling federal funding for drug courts recently, from \$25 million to \$45 million through the Department of Justice (Tiger, 2012). It is clear that drug courts are established to be *the* drug control strategy of contemporary America.

With the introduction of a professional organization for drug courts has come a concentrated effort to streamline the drug court model. The National Association of Drug Court Professionals (NADCP), in conjunction with the U.S. Department of Justice,

identifies ten key components of drug court programs that are deemed necessary for the success of this type of intervention (NADCP, 1997):

1. Integration of alcohol and other drug treatment services with justice system case processing.
2. Use of a non-adversarial approach.
3. Prompt identification and placement of eligible participants into the drug court program.
4. Provides access to a continuum of alcohol, drug, and other related treatment and rehabilitation services.
5. Monitor abstinence by frequent alcohol and other drug testing.
6. A coordinated strategy governs drug court responses to participants' compliance.
7. Ongoing judicial interaction with each drug court participant.
8. Monitoring and evaluation measure the achievement of program goals and gauge effectiveness.
9. Continuing interdisciplinary education promotes effective drug court planning, implementation, and operations.
10. Forging partnerships among drug courts, public agencies, and community-based organizations generates local support and enhances drug court effectiveness.

These key components reflect expected court operations, drug court personnel cohesiveness, and collaboration with local communities. The ten key components are often used by researchers to gauge the effectiveness of individual drug courts, and are used to measure individual components of the drug court that impact outcomes.

Almost half of the key components of drug courts dictate important court and treatment processes. Drug courts must consist of drug treatment and intensive supervision as core services provided to participants. Treatment services should be tailored to the needs of individuals and can include a variety of in-patient and outpatient drug treatment approaches, as well as cognitive behavioral programming. Intensive supervision includes frequent drug testing and ongoing judicial interaction that can be reduced as the participant progresses through the program. Combined, these core components are

identified as necessary features of any drug court; however, implementation strategies vary across drug courts.

Several of the ten key components of drug courts note the importance of collaboration with all drug court stakeholders to ensure the successful operation of the drug court program. Drug courts use a non-adversarial team approach (e.g., judges, defense, prosecutor, treatment staff, probation, etc.) that identifies effective interventions while also ensuring advocacy for individual legal rights. This approach is aimed at ensuring compliance through effective rewards (i.e. positive reinforcement) for good progress and sanctions that try to correct behaviors that impede successful outcomes. Drug courts must have a strong relationship with local law enforcement agencies to support early identification of potential drug court participants and fast case processing. A short time between arrest, conviction, and placement into drug court is valuable to all drug court stakeholders in that it connects the drug crime with a rehabilitative criminal justice response for each individual offender.

Finally, the NADCP (1997) identifies three key components that must be present to ensure the continued operation of drug courts within given local communities. Drug court professionals are encouraged to continually revisit “what works” in drug court programming to maintain and adjust programming based on effective intervention principles. Drug court evaluations for individual programs are not only required to maintain funding, but also offer insight into whether the process is being implemented as expected, and whether participant outcomes are favorable. To maintain support from local communities, drug courts are encouraged to forge relationships with community faith, law enforcement, and treatment organizations.

The sustainability of drug court programs within communities and over time depends upon long-term funding strategies. It is clear that the federal government is committed to the drug court model by providing seed money for almost all new drug courts and continued funding for courts that establish a need. The Federal Drug Court Discretionary Grant Program has appropriated over \$530 million in funding for drug courts from 1995-2010, with authorization requests of \$250 million a year in recent years. Under this program, drug courts can apply for federal funding of up to 75% of their operating costs and must establish a need for funding of a new drug court or improvement of an existing drug court. Other federal funding sources include funding from the Edward Byrne Memorial Justice Assistance Grants (JAG), the Office of Juvenile Justice and Delinquency Prevention (OJJDP), and by the Department of Health and Human Services, Substance Abuse and Mental Health Services Administration (SAMHSA). It is unknown exactly how much each agency contributes to funding drug courts as they do not provide appropriations totals to that degree of detail (Franco, 2010). Though it is unclear exactly how many drug courts are funded with federal dollars each year, estimates indicate a large majority of drug court programs have used federal funds to implement and operate their program.

State and local funding sources are often used to provide long-term sustainability and expand capacity beyond that which the federal grants allow (e.g., federal funding requires the exclusion of violent offenders), and to further solidify the court's role in the community. Funding sources include state and local governments (through grants and/or fees), non-profit organizations, interagency partnerships, and community partnerships with local charity and service organizations (Reilly & Pierre-Lawson, 2008).

The Emergence of Drug Courts

The institution of the first drug courts was rooted in a strategy to reduce recidivism of drug offenders that was believed to be more effective than traditional criminal justice processing for this offender population. The Department of Justice Drug Courts Program Office (2000: 1) explains that drug courts use “the coercive power of the court to force abstinence from drugs and alter behavior of substance-abusing offenders.” Drug courts integrate substance abuse treatment, sanctions, and incentives with case processing to manage nonviolent drug-involved offenders with a rehabilitative focus.

Based on interviews with drug court advocates around the country, Tiger (2012) identifies three main reasons for the emergence of drug courts. First, drug courts address institutional problems in handling the problem of addiction, which is believed to motivate much criminal behavior. Second, Tiger (2012) finds that advocates refer to the “revolving door nature” of justice for drug offenders as the institutional failure that drug courts address. Instead of punishing addiction and treating drug use as a criminal act, drug courts treat addiction as a symptom of an illness and use the coercive nature of the criminal justice system to treat that illness. Finally, advocates describe a balanced philosophy of a tough and caring approach to rehabilitating drug offenders. Tiger (2012) indicates that the emergence of drug courts came out of a need for the judicial branch to reestablish its relevance. The stringent drug policies enacted in the 1980s removed most judicial discretion and replaced it with mandatory sentencing laws. Drug courts allow judicial authority to exert compassion for people through treatment while still maintaining the punitive focus of the criminal justice system through supervision and sanctions.

This explanation of the impetus of drug courts in the United States aligns with what is known about effective ways to treat drug offenders within the criminal justice system. Tiger's analysis of the emergence of drug courts indicates that drug courts came out of a renewed focus on the relationship between substance use and crime. This relationship is a topic of considerable interest and scholarship. The drug-crime connection is explored primarily by crime type to obtain a greater understanding of the types of crime in which drug offenders engage. This is of particular interest within the drug court context given the emphasis on public safety goals.

Tiger's (2012) second main reason for the emergence of drug courts lies in the drug court's focus on addiction. Rather than continuing to focus on the efficiency of case processing for this high volume offender population, drug courts specifically address addiction as the primary focus of the program. Most drug courts embrace the disease model of addiction, utilizing the treatment and supervision resources of the program to support the offender in working toward sobriety (NADCP, 2010)

Jellinek (2010) formally developed the disease model of addiction for alcohol in 1960, though it has been adopted as a framework for drug addiction as well. The disease model brings addiction under the wing of medical science, leaving behind the notion that addiction was caused by a lack of moral character. Jellinek asserts that addiction is a diagnosable condition with characteristic symptoms (i.e., tolerance, withdrawal, loss of voluntary control) that can be controlled through abstinence and specialized medical treatment. Drug court programming uses this model by creating a support structure (intensive supervision) for abstinence and drug treatment focused on cognitive and behavioral modification, (Terry, 1999).

Finally, Tiger's (2012) study indicates a shift in judicial authority that moves toward a therapeutic jurisprudence approach to handling drug offender cases. Mandatory minimums and other restrictions on judicial discretion block judges from carrying out justice in ways that they think might align better with judicial goals (i.e., reducing recidivism). The introduction of drug courts allows judges to refocus on the rehabilitative aspect of criminal justice. Wexler (1990) describes therapeutic jurisprudence as the study of the role of law as a therapeutic agent. Legal actors are social forces that can produce therapeutic or anti-therapeutic consequences, and the primary focus of justice is to support rehabilitation of the offender. Drug courts allow judges to be a major stakeholder in decisions regarding the supervision and treatment of drug offenders participating in drug court programs (Burke, 2010). Pre-trial conferences are used as a way for the drug court team (treatment personnel, law enforcement, prosecution, defense attorneys, judge, probation officers) to talk about each participant and address any concerns with their rehabilitative goals.

Therapeutic jurisprudence is intimately tied to the concept of procedural justice. Procedural justice includes providing the offender with a voice or opportunity to tell their story, maintaining respectful relationships between legal actors and the offender, a trustworthy decision-making process, offender understanding of their rights, and perceptions that legal actors are interested in the offender's personal challenges (Tyler, 1990). Drug courts provide these elements of procedural justice and have been found to increase perceptions of procedural justice over traditional court processing (Frazer, 2006). Further, drug court participants that have higher perceptions of procedural fairness were less likely to engage in crime (Gottfredson, et al., 2007).

Overall, the rationale behind the emergence of drug courts firmly represents why they were so readily adopted throughout the nation. The timing was ripe in that the convergence of a high volume of drug offenders and a frustrated judicial authority fueled the adoption of non-traditional case processing for drug offenders. Within this context, the drug court program model makes “sense” for this offender population. It combines individualized supervision and treatment with a rehabilitative focus on addiction with specific goals to reduce drug use and crime. The program model incorporates theoretical jurisprudence and procedural justice to provide participants with a holistic rehabilitative program rooted in the criminal justice system. To this end, the drug court movement is considered the most influential criminal justice innovation in the American criminal justice system (Terry, 1999). A later section describes how implementation of a drug court may be able to provide increases in public safety through individual-level (i.e., specific deterrence) and community-level (i.e., procedural justice, informal social control) mechanisms.

Linking Drug Use and Crime

The drug court movement is rooted in the notion that there is a strong connection between drug use and criminal offending. Nurco, Kinlock, and Hanlon (1990) explain that there was a great interest in understanding the connection between drug addiction and criminal involvement from the 1920s to the 1970s, but researchers lacked the methodological rigor to fully root out the relationship between addiction and crime. During that time, the prevailing thought was that addicts were inherently criminal and drug use was just another expression of their anti-social behavior. More rigorous research

since the 1970s identifies criminal involvement among drug users as a means by which individuals can support their addictive habit. Further, the highest frequency users are those that engage in the most crime and individuals commit more crime during periods of addiction than when they are sober (Nurco, et al., 1990). The types of crimes in which drug users engage is varied, but typically involves drug distribution crimes and property crime (i.e., income-generating crimes). Violent crime makes up a smaller proportion of crimes committed by drug users and the connection is often indirectly linked to drugs through drug-seeking activities (Nurco, et al., 1990). This evidence indicates that drug users engage in different types of crime and thus programs (like drug courts) designed to serve drug offenders can expect differential effects on specific types of crimes. The connection between drug use and violent, drug, and property crime is discussed below.

In 1985, Paul Goldstein laid out a tripartite conceptual framework that outlines three explanatory mechanisms by which drug use might lead to violent crime, including psychopharmacological, economic-compulsive, and systemic. The psychopharmacological explanation posits that violence is due to direct, acute effects of a drug on a user. In other words, some individuals, as a result of short or long term ingestion of drugs, may become excitable, irrational, and exhibit violent behavior. The economic-compulsive explanation suggests that violence is often committed instrumentally while generating money to purchase drugs. Individuals who commit these instrumental crimes are not motivated by violent impulses per se, instead, violence results from some aspect of the social context such as the perpetrator's own nervousness, the victim's reaction, or the intercession of bystanders.

Finally, the systemic explanation asserts that violence is an inherent part of illicit drug markets, e.g. drug disputes, turf battles, robberies of drug dealers and related violent retaliation. Since a substantial number of drug users become involved in drug distribution as their drug-using careers progress, they risk becoming perpetrators of systemic violence (Goldstein, 1985). Relatedly, Blumstein (2002) suggests a fourth connection between drug use and crime that concerns the community impacts of drug markets. Community disorganization caused by drug markets and their operation can influence the behaviors of others in the community not directly involved in the drug market. Drug sellers often carry firearms and this can compel others to arm themselves for defense or to settle disputes.

While the Goldstein tripartite framework has been invaluable as an organizing scheme, it has had few direct and rigorous empirical tests (MacCoun, et al., 2003). Among the studies that have examined the framework, the dominant view is that psychopharmacological violence, as opposed to economic-compulsive or systemic, is rare, and that the social environment is a much more powerful contributor to the outcome of violent behavior (MacCoun, et al., 2003; Fagan, 1990; Parker and Auerhahn, 1998; White and Gorman, 2000). While there is some evidence to suggest that certain drugs, such as amphetamines, may increase violent crimes (Gelles, 1994), the vast majority of research supporting pharmacological effects is on the relationship between alcohol use and violence (Exum, 2002; Collins and Messerschmidt, 1993; Cuellar, Markowitz, and Libby, 2004). In several studies of drug users, researchers found that violent acquisitive crime (e.g., robbery) was avoided if non-violent alternatives existed (Johnson et al., 1985; Goldstein, 1985).

More often, the drugs-crime literature links drug use to drug and acquisitive forms

of crime as compared to strictly violent crime (Gottfredson, et al., 2008; Anglin and Perrochet, 1998; Johnson, et al., 1985). For example, a recent survey of State prisoners found that 1 in 3 property offenders and 1 in 4 drug offenders reported drug money as a motive for their crime, as compared to only 10% of violent offenders (BJS, 2006). Using data from a larger study of the Baltimore City drug court, Gottfredson, et al. (2008) found that use of alcohol, heroin, and cocaine were related to increases in self-reported income-generating crime but not violent crime. In a review of 25 years of research conducted at UCLA's Drug Abuse Research Center, Anglin and Perrochet (1998) concluded that the commission of property crimes almost always increased to support dependence level abuse of drugs, including "hard" drugs such as heroin and crack on down to "soft" drugs such as marijuana. Similar studies of narcotic addicts found that individual property crime rates were significantly higher during periods of addiction versus non-addiction (Nurco, 1998; Harrison, 1992; Caulkins, et al, 1997). Additionally, a recent study of methamphetamine addicts reached similar conclusions: users' drug spending was positively associated with earnings from both property crime and drug dealing (Wilkins and Sweetsur, 2010).

The evidence regarding drug use and crime type informs the expectations for successful drug court programs. If drug courts were to be responsible for reductions in aggregate crime rates, it is most likely the impact would fall on rates for drug and property crimes. Reducing drug use for individual drug court participants is likely to reduce their engagement in drug crimes and the crimes associated with supporting those drug habits (e.g., drug distribution, burglary, larceny, theft, etc.). Since the proportion of drug offenders engaging in violent crimes is low, but the actual number of violent crimes

committed by drug offenders is high, it is possible that successful drug courts may reduce violent crime rates but this reduction is likely to be small.

Evaluations of Drug Courts

The majority of the drug court literature consists of process and outcome evaluations of individual drug courts in the United States (Gottfredson, et al., 2003; Mitchell, et al., 2012; Wilson, et al., 2006). Periodic meta-analyses and systematic reviews have consistently found that drug court programs show a lower rate of recidivism than that of comparison groups; however most of the drug court literature is methodologically weak, with few randomized studies and only a small number of rigorous quasi-experimental studies (Mitchell, et al., 2012).

In one of the few randomized tests of the effectiveness of drug courts, Gottfredson et al. (2003) found significant support for drug courts' effect on recidivism. The study consisted of 235 high-risk offenders (i.e., those likely to continue substance use and criminal behavior based on risk assessment tools) in Baltimore, Maryland who were randomly assigned to drug court or traditional probation groups. The authors measured recidivism, offense history, treatment, supervision, and jail time over a 24-month follow up period. Gottfredson et al. found that the drug court group was significantly less likely than the control group to be arrested for a subsequent drug offense within a two-year follow-up period (40.6% vs. 54.2%). Additional analyses indicate that recidivism is lowest among those that participated in drug treatment and those that were engaged with intensive supervision (status hearings and regular drug testing) (Gottfredson, et al., 2006).

The authors also found that drug court delayed time to re-arrest and reduced risk

for future drug crimes. A survival analysis indicates that though the drug court and control groups were rearrested at similar rates during the first four months of the 2 year follow-up period, the drug court group survived longer than the control group (Banks & Gottfredson, 2004). That is, drug court participants had a longer time to re-arrest than those who did not participate in the drug court. Further, participating in drug court significantly reduced the risk for re-arrest of a drug crime (but not violent or property crime) and indicated that once a person survived to a certain point, they were likely to continue to survive. The control group, however, remained at risk for re-arrest throughout the follow-up period.

Other drug court evaluations report similar reductions in recidivism. An analysis of the Suffolk County drug court in Massachusetts found that drug court was more effective than traditional probation in reducing arrests, future incarceration, and parole suspensions and revocations (Rhodes, Kling, and Shively, 2006). Though a relatively new drug court program, the Adair/Casey Drug Court in Kentucky found that drug court participants had low recidivism rates (approximately 15%) during a one-year follow up (Havens and Cobb, 2006). Marchand, Waller, and Carey (2006a, 2006b) found in an analysis of the Kalamazoo County (Michigan) Adult Drug Treatment Court and the Barry County (Michigan) Adult Drug Court that drug court participants were significantly less likely to be arrested than a non-participating matched group during a 2-year follow up.

In one of the few multi-site studies to examine the impact of drug courts on outcomes beyond recidivism, researchers from the Urban Institute, Research Triangle International, and the Center for Court Innovation collaborated on NIJ's Multi-Site Adult Drug Court Evaluation (MADCE). The authors examined drug courts' impact on a host

of outcome variables including crime, drug use, socioeconomic outcomes, family functioning, and mental health (Rossman, et al., 2011). They found that drug court participants were significantly less likely to report committing crimes, and among those who did commit crime, committed fewer crimes than the comparison group. Results from official data showed that drug court participation reduced the probability of re-arrest over 24 months (52% vs. 62%), but those results were not statistically significant.

Additionally, drug court participants were significantly less likely than the comparison group to report using any type of drugs and less likely to report using “serious” drugs in the year prior to their 18-month follow-up.

Periodic government assessments of drug court success have also been frequent in the drug court literature. In a 2001 review of 37 outcome evaluations, the Government Accountability Office (GAO) found that most evaluations report within-program recidivism reductions, especially for high-risk individuals. For example, 19% of high-risk individuals participating in a drug court program in California reoffended as compared to 38% of individuals in a comparison group. Evaluations that isolate post-program (i.e. following completion of drug court and/or probation requirements) recidivism are not as common in the literature. Drug court participants, however, typically show lower recidivism rates than those in comparison groups. Post-program recidivism rates averaged 13% lower for drug court participants compared to individuals in comparison groups (Belenko, 2001).

Two subsequent GAO reviews show similar findings. In 2005 a review of 27 drug court evaluations with clear outcome information and an acceptable evaluation design indicated that drug court participants show lower levels of recidivism than comparison

groups. Specifically, drug court participants show a 10 - 30% reduction in recidivism for felony and drug offenses compared to non-participants within a two-year follow-up period (GAO, 2005). The most recent GAO review includes data from 32 programs and shows a 6 – 26% reduction in recidivism for drug court participants over comparison group members. Re-arrest rates for drug court graduates were between 12% and 58% below those in the comparison group (GAO, 2011).

More recent meta-analyses of drug court evaluations also conclude that drug courts are a better criminal justice response for drug offenders than traditional probation processing. In a systematic review of 55 drug court evaluations, Wilson, Mitchell, and MacKenzie (2006) found that average reductions in recidivism were between 14 and 26% for drug courts over traditional probation. In a systematic review of 154 independent evaluations of drug courts, Mitchell and colleagues found that drug courts significantly reduce general and drug-related recidivism and that those effects appear to persist for at least three years. The authors relate the mean effect size as analogous to a drop in recidivism from 50% for non-participants to approximately 38% for participants (Mitchell, et al., 2012).

Drug courts vary in their overall effectiveness in reducing recidivism for specific types of crimes. Gottfredson, et al. (2003) evaluated the effectiveness of the Baltimore City Adult Drug Treatment Court using a randomized-controlled trial. Results from that rigorous design indicate that the court functioned to significantly reduce recidivism for drug crimes when compared to traditional court processing (40.6% vs. 54.2%), and showed non-significant reductions in recidivism for property and violent crimes (25.5% vs. 33.7% and 10.9% vs. 17%, respectively). Mitchell, et al. (2012) found similar

evidence using information from 92 adult drug court evaluations. The authors found that drug courts reduced recidivism for drug crimes at a rate similar to what was found when measuring any recidivism, regardless of type of crime. Evidence from the MADCE also found drug court to be responsible for reductions in arrests for drug crimes (17% vs. 22%), however these differences were not statistically significant (Rossman, et al., 2011). When investigating self-reported criminal behavior, Rossman, et al. (2011) did find that drug court participants reported significantly less involvement in drug possession, drug sales, DWI/DUI, and property crimes than those not involved in drug court programming. Taken together, the literature indicates drug courts are most likely to reduce recidivism for drug and property crimes. There is no strong evidence supporting an increased public safety benefit of implementing drug court to reduce violent crimes.

There is considerable variability across courts in their effectiveness of reducing recidivism more than treatment as usual for drug offenders. Of the 92 adult drug court evaluations studied in Mitchell, et al. (2012), 41 (44.6 percent) had effect size estimates that either indicated no statistical difference between treatment and comparison group or favored the comparison group for recidivism reductions. The odds ratios ranged from 0.3 to 24.4 (mean odds ratio = 1.66), with an odds ratio of 1 or below indicating no difference between groups or lower recidivism for the comparison group. Despite general success of drug courts in reducing recidivism, the magnitude of that reduction varies by a number of factors. Researchers have begun to explore this variability by investigating implementation quality.

Drug courts share many common elements (described in a previous section) but vary widely in implementing policies and practices (Carey, Finigan, & Pukstas, 2008).

An analysis of the 23 drug courts included in the MADCE found that though all of the courts were effective at reducing substance use and recidivism, the top-performing courts shared several common elements. These courts implemented multiple best practices (i.e., the 10 key components of drug court programs discussed above) including predictable and individualized sanctions, positive judicial attributes (i.e., actions and demeanor of judges towards clients that are respectful, fair, attentive, enthusiastic, consistent/predictable, caring, and knowledgeable) and participants that all entered post-plea (Zweig, Lindquist, Downey, Roman, & Rossman, 2012). Less effective courts implemented fewer best practices or did not implement them with high fidelity.

Drug court programs are expected to be most applicable to high-risk offenders. In this context, high-risk refers to offenders not likely to succeed on standard supervision and continue their criminogenic behavior (Marlowe, 2009). The structure of drug courts combine supervision, behavioral intervention, and treatment – all programmatic elements that are most applicable to high-risk populations (Andrews & Bonta, 2006). Researchers analyzing drug court outcomes by offender risk show the greatest effects for high-risk participants. Such participants had longer criminal histories, had a history of mental illness, and had previously failed less intensive criminal justice punishments (Lowenkamp et al., 2005; Festinger et al., 2002; Fielding et al., 2002; Marlowe et al., 2006, 2007).

The evidence for drug court effectiveness indicates that they are at the very least an improvement over traditional case processing for non-violent drug offender populations. Participants have lower rates of drug use and criminal behavior than those in comparison groups given traditional criminal justice services for this population of drug

offenders. Characteristics of drug courts that are best able to reduce substance use and recidivism are those that implement multiple best practices with high fidelity and serve higher-risk offender populations (Lowenkamp et al., 2005). As drug court expansion continues, and scale-up efforts are underway to broaden the reach of drug court services (NADCP, 2013), it is important to assess the aggregate impact drug courts may have on public safety within the communities in which these courts are housed.

Drug Courts and Public Safety Outcomes: Theoretical Mechanisms

The stated goals of drug court include an interest in improving public safety within the communities in which the program operates (NADCP, 2008). Public safety is a broad term that often refers to agencies or policies that support the public in preventing and protecting from harmful instances such as crime. When relating public safety to crime, public safety is often measured based on incident levels of violent and property crimes, as well as self-reported crime. Focusing specifically on drug courts, goals related to improvements in public safety refer to individual-level and community-level changes. This review will discuss two mechanisms by which drug courts may improve public safety: through specific deterrence and broader community-wide change.

The previous review of the drug-crime connection indicates that drug offenders are implicated in property, drug, and to a lesser extent violent crimes. The goal, then, is that introduction of a drug court within a community or jurisdiction will reduce the likelihood that these crimes will occur through specific deterrence of drug court participants.

The NADCP's argument that drug courts are capable of reducing drug use and crime may have merit based on the rationale that drug court programming targets high-risk and high-need offenders. These are offenders with severe substance use disorders that are likely to fail in less intensive community supervision or rehabilitation programs (Marlowe, 2009). The principles of effective correctional intervention indicate that programs should be reserved for higher risk offenders as they have the most room for improvement and are arrested more frequently. Low risk offenders are less likely to reoffend and intensive interventions could detrimentally impact their lives (Cullen and Gendreau, 2000). Drug courts may be potent enough at reducing recidivism and drug use for high-risk drug court participants that this impact would be carried forward into the community by removing those offenders from the potential offender pool.

Drug court programs are often characterized as excluding offenders that may be most applicable to the intensive programming model provided by this criminal justice intervention. The earliest drug courts necessarily concentrated on diverting offenders with drug possession charges into drug court so as not to appear "soft on crime" (Marlowe, 2012). A recent analysis of the potential impact of drug courts on prison and jail populations indicates that drug court eligibility criteria often exclude drug-involved individuals that are most likely to be incarcerated (Pollack, Reuter, & Sevigny, 2011). However, by 1998, 70% of drug court programs targeted individuals with extensive criminal histories and an average history of drug use of 15 years (Tauber, 1998). Further, an early survey of drug courts indicated most drug court participants have moderate to severe substance abuse histories (Cooper, 1997), and though they may not be able to substantially impact incarceration (primarily because of eligibility restrictions to only

non-violent offenders), the intent of the drug court model was to provide an alternative criminal justice program for offenders likely to fail on standard community supervision, not necessarily those likely to become incarcerated. This evidence indicates that drug courts may not serve a substantial portion of all high-risk offenders, but rather serve the subset of high-risk offenders most likely receive and fail standard community supervision.

A survey of adult drug courts in 2004 indicates that drug courts have expanded to include other drug-involved offenders, focusing primarily on offenders with felony and misdemeanor forgery, property offenses, probation and parole violations, and prostitution charges in addition to felony and misdemeanor drug possession and sales charges.

Almost all drug courts surveyed limit eligibility based on the offender's criminal history with two thirds allowing nonviolent felonies as the most serious type of prior conviction permitted and about one-quarter of surveyed courts limiting prior convictions to nonviolent misdemeanors. Despite these restrictions, approximately 85% of drug courts do not have a limit on the maximum number of prior convictions allowed (Rossman et al., 2011). This snapshot of drug court programs indicates most eligible participants are drug-involved offenders with current drug or property crime felonies that have a criminal history involving nonviolent crime.

It remains unclear whether drug courts as a whole summarily exclude high-risk offenders from participating. Rossman et al.'s (2011) snapshot of adult drug court programming included less than half of the adult drug court programs in the United States and did not provide a summative measure of average offender risk for recidivism. Analyses estimating whether drug courts reduce incarceration for drug-involved

offenders offers information about the impact of excluding drug-involved violent offenders but does not fully inform whether drug courts are serving other high-risk offenders (Pollack et al., 2011; Sevigny et al., 2013). The potential for drug courts, serving limited caseloads of primarily nonviolent offenders, to reduce crime through specific deterrence hinges on making large changes for a small group of individuals engaged in frequent criminal behavior.

Lipsey's (2009) recent meta-analysis discussing the factors that characterize effective interventions with juvenile offenders indicates that targeting high-risk offenders is a major correlate of program effectiveness. Drug courts that target drug offenders at high risk for reoffending are most likely to effectively reduce crimes related to drug use and abuse by impacting those most likely to be responsible for those crimes.

This type of specific deterrent effect at the population level has been demonstrated in other programs that target high-risk individuals. For example, Operation Ceasefire was originally developed in Boston to address gang activity and rising violent crime rates. It uses a focused deterrence strategy to increase the perceived certainty of arrest for those gang members engaging in violent behavior (i.e., high-risk youth). This strategy also focused on firearms traffickers in order to address the supply side of gun violence (Braga, et al., 2001). Using a basic one-group time-series design, Braga, et al. found that there was a 63% reduction in average monthly youth homicide victims (youth under age 24) and a 25% decrease in the monthly number of citywide gun assaults. It is not clear how much of the reduction was due to reduced behavior among targeted youth as opposed to among non-targeted youth who were deterred from committing crimes by the program activities.

There is also evidence that targeting drug-involved arrestees as they enter the criminal justice system can provide significant benefits to the community. An evaluation of Breaking the Cycle, a broad-scale drug court-like program aimed at felony arrestees involved with illicit drugs indicated that focusing resources on those in need of drug treatment at the pre-trial phase significantly reduced self-reported reoffending, jail overcrowding, received positive political support, and increased perceptions of public safety (Mitchell & Harrell, 2006). The program represents a focused effort on a specific sub-set of offenders (i.e., drug-involved arrestees) that was able to garner positive local change.

The evidence from these programs suggests that programs or policies aimed at reducing aggregate crime or prevalence rates are successful if they impact those at greatest risk for reoffending. It still may be the case, however, that drug courts are not capable of impacting public safety because they dilute their participant population with lower risk drug offenders and may actually contribute to increases in crimes given the interruption this program creates in the lives of low-risk offenders (Marlowe et al., 2006; Festinger et al., 2002).

It is often argued that drug courts do not serve a large enough proportion of the high-risk population they are designed to serve. Bhati et al. (2008) estimate that, nationally, drug courts serve approximately half of the potentially eligible drug-involved offenders the program aims to impact. However, this analysis does not further restrict the potential drug court participant population to offenders with diagnosed substance use disorder – the population drug courts were designed to serve (Marlowe, 2012). Rossman et al.'s (2011) survey of adult drug courts in 2003 indicated that of the 368 courts

responding to the survey, about 46% indicated their courts had less than 50 active participants. In terms of case flow, the drug courts averaged about 90 new admissions in the surveyed year, and most operated at or above capacity.

The survey also asked courts about admissions criteria based on substance use. Approximately 38 percent of the surveyed courts limited participation only to those with diagnosed substance use disorders, with the rest of the courts additionally allowing frequent users (33%) and anyone who uses illegal drugs (29%). This evidence indicates that courts attempt to concentrate on offenders with substance use disorders, but some unknown proportion of participants are drug users without a substance use disorder (Rossman et al., 2011).

The national proportion of offenders with treatment needs that drug courts serve can be roughly estimated using active participant totals from Rossman et al.'s survey of drug court programs in 2003, the 2003 Uniform Crime Reports (UCR) arrest information, and the Arrestee Drug Abuse Monitoring program (ADAM) data from 2003. The ADAM data estimates the proportion of arrestees that are drug-involved and at risk for drug dependence through urinalysis and self-reported behavior. The 2003 ADAM data include a sample of 39 counties and indicates 67% of arrestees in the sample tested positive for cocaine, marijuana, methamphetamines, opiates, or phencyclidine (PCP). Approximately 39% of arrestees indicated they were at risk for drug dependence, an indication for the need for drug treatment (Zhang, 2003). Using UCR information, this translates to approximately 5.3 million arrestees (Snyder, 2012).

According to Rossman et al.'s (2011) survey of drug courts, almost half of drug courts served less than 50 participants, with about four percent serving over 400

participants in 2003. On average, the surveyed drug courts admitted 89 participants in the previous year, which translates to about 135,102 people if multiplied to cover the approximate number of adult drug court programs in the nation (1,518). Conservatively, this represents 2.5% of the population of arrestees with substance use disorders in the United States, an admittedly small proportion of the population drug courts are intended to serve. However, this total is a national representation, including a large majority of areas that do not have drug courts within their jurisdiction and masking jurisdiction variation in drug court size and the drug-involved arrestees they are designed to serve. The only conclusion that can be derived from this rough analysis is that drug courts do not serve a large proportion of arrestees with substance use disorders nationwide.

A more focused way to estimate the proportion of drug-involved offenders drug courts serve is to estimate the totals within drug court jurisdictions. For example, Denver, CO and Hennepin County, MN are jurisdictions that are included in both the ADAM data for 2003 and the sample of drug court jurisdictions in the current analysis. The ADAM data indicate that 31.5% and 32.3% of arrestees in Denver and Hennepin County (respectively) are in need of drug treatment. Based on admissions information from Denver and Hennepin County (approximately 478 and 584 admissions per year, respectively), it is estimated that these two jurisdictions serve 59% and 52.6% of offenders with substance use disorders each year.¹ If the inclusion criteria were expanded to any arrestee testing positive for drugs, it is estimated that the drug courts serve 18.5% and 17% of drug-involved arrestees. Though this estimate does not represent a definitive

¹ Evaluators for the Madison County, IL drug court estimated the share of drug-involved offenders admitted to the drug court during the first two years of operation (Godley et al., 1998). They estimated the drug court reaches 49% of drug-involved offenders within the county, a comparable proportion to the estimates provided for Denver and Hennepin County.

conclusion about the proportion of drug-involved offenders drug courts may serve, it indicates that drug courts may include a large proportion of offenders with substance use disorders within the jurisdictions they serve.

A second mechanism by which drug courts can impact public safety is through community-wide change. Effective drug courts could start a ripple effect given the peripheral outcomes drug courts may support, such as reductions in drug market activity, reducing incidence of generational detriments (e.g., fewer babies born in disadvantaged environments), and increases in positive social support mechanisms within the community. Drug court participants that achieve successful outcomes (sobriety and reduced recidivism) may be more capable of participating as a positive member of the community by being more active family members, economic consumers and producers, and work in partnership with their neighbors to benefit the community.

A comprehensive analysis of the social impacts of drug court was estimated in the Multi-Site Adult Drug Court Evaluation (MADCE). Net benefits to society during the 18-month follow-up period were estimated and include social productivity (e.g., employment and education), criminal justice system (e.g., police, courts), crime and victimization, service use (e.g., drug, medical, and mental health treatment), and financial support use (e.g., government, friends and family). The authors found that the net benefit of drug court participation is nearly \$6,000 per offender, a cost-benefit ratio of 1.92:1 (Rossman, Rempel, Roman, Zweig, et al., 2011). Similarly, the Washington State Institute for Public Policy estimates a 1.74:1 cost-benefit ratio of drug court programs (WSIPP, 2003). Though not a direct measure of improvements in the social fabric of the community, these analyses indicate the monetary value of drug court implementation.

Few drug court evaluations have directly explored peripheral effects of drug court. Evaluation of the Baltimore City Drug Treatment Court included interviews of drug court participants and control group members to assess differences in crime participation, drug addiction, employment, physical and mental health, and family and social indicators. Drug court participants were found to be significantly less likely to be on welfare three years after participation as compared to the randomized control group members that were interviewed (Gottfredson, Kearley, Najaka, & Rocha, 2005). Green and Rempel (2012) use information from the MADCE to assess whether drug courts significantly impacted socioeconomic well-being (e.g., employment, education, financial assistance needs), family relationships, mental health, and homelessness 18 months after participation. The authors found that drug court participants had more favorable outcomes (e.g., less reliance on public financial assistance, higher annual income, stronger mental health) but they were not statistically significant. Drug court participants were, however, significantly more likely to have less family conflict and stronger family emotional support.

Further support for possible community-wide impacts of drug court is focused on prison diversion. Drug court participants remain in the community rather than going to prison. Incarcerating drug-involved offenders creates barriers to drug treatment (Mauer & King, 2007) and exposes them to an environment that can be mildly criminogenic (Nagin, 2009). These effects are compounded when offenders face reentry barriers when released back into the community. Reentry barriers include negotiating family and social ties that were disrupted during incarceration and strained upon return to the community (Rose & Clear, 1998). Supervising drug offenders in drug courts can avoid the collateral

consequences of incarceration by allowing offenders to remain in the community and offering them resources to promote sobriety, encourage employment, and strengthen social connections through drug treatment and intensive court supervision.

Drug Courts and Public Safety Outcomes: Existing Research

To date, there is only one published analysis of the impact of drug courts on jurisdiction-level outcomes. Lilley (2013) uses nationwide data from 1995 to 2002 to assess the impacts of drug court implementation grants on felony violent and property offenses in over 5,000 jurisdictions. The time span used in this analysis represents the impact of the early drug courts in a time during which the majority of drug court participants did not successfully complete the program. Lilley uses drug court implementation grants to represent the timing of drug courts within jurisdictions (lagged 1 year to allow for programmatic effects to accumulate). Other justice grants (COPS, Weed and Seed, State Criminal Alien Assistance, and Local Law Enforcement Block grants) are also controlled in the analysis in order to try to isolate the drug court implementation grant as the key indicator for changes in violent and property crime. Other control variables represent time-varying demographics that differ across jurisdictions (percent nonwhite, percent age 15-24, per capita income, and employment rate). These variables are included because they can impact both the independent variable of interest (drug court implementation) and the dependent variables (property and violent crime rates). Lilley uses a fixed effects modeling strategy to rule out time-invariant characteristics of jurisdictions (e.g., region, jurisdiction size, type of government) and isolate the effect of drug court implementation on crime rates.

Lilley (2013) found that drug court implementation grant funding was associated with net increases in felony violent and property crime with the exception of murder. This relationship was most robust for vehicle theft, finding that a 10 percent increase in drug court grant funding was associated with approximately 13 additional vehicle thefts per 100,000 people. The author found that among the measured justice grants, drug court grants were the only funding sources associated with increased crime rates while all the others were associated with decreases in crime. This indicates that the relationship between drug court grants and crime rates is not due to an interaction with other justice grant funding. The author tested an alternative model isolating just the drug court jurisdictions in order to rule out the possibility that the drug court jurisdictions were different in some fundamental way from jurisdictions without a drug court. Overall, Lilley (2013) found that the positive relationship between drug court implementation and property offenses was more consistent than the relationship between implementation and violent offenses. The author argues that the finding is likely due to the low graduation rate of the early drug courts, which made up a large proportion of the sample of drug court jurisdictions. These courts experienced high non-completion rates where participants who either “failed out” of drug court or opted not to continue were placed on community supervision (or given no supervision at all) rather than sent to jail or prison.

Lilley’s analysis of drug court implementation and crime rates represents a first step in understanding the community-level impacts of drug court programs. However, this evidence needs to be taken further to identify the potential reasons for the relationship the author found, especially since drug court implementation was found to be connected to increases in crime rates rather than the expected decreases in crime the

evaluation literature would indicate. Further, Lilley does not provide information about the effect of drug court implementation on jurisdictional drug crime participation.

Though the author tested many different alternative models to identify the robustness of the effect, this evidence lacks information about the quality of the drug court programs funded through the implementation grants. Drug courts vary widely in their implementation fidelity, especially the early drug court models. Further evidence is required in order to accurately assess whether the effects of well-implemented and effective drug court programs differ from the effects of drug courts evaluated to be ineffective at reducing recidivism.

Research Question

The goal of the current research is to explore whether drug courts can impact public safety. The guiding research question asks whether the introduction of a drug court within a jurisdiction is responsible for significant changes in specific types of reported violent and property crime. Arrest information (drug arrests and specific types of arrests for property and violent crimes) is also used to measure the relationship between drug court and public safety. Though drug court advocates, critics, and researchers have all weighed in on the potential for drug courts to achieve this goal, the direct impact of drug court programs on public safety remains largely unmeasured. The specific data and methodology used to explore this question is detailed in the following section.

CHAPTER 3: METHODS

The impact of drug courts on public safety was measured by collecting yearly reported crime from 1990 to 2008 for jurisdictions with drug courts evaluated in a recent meta-analysis. A list of evaluated drug courts with varying effectiveness was compiled based on the studies reviewed in the Mitchell, et al. (2012) systematic review of drug court programs.² The implementation date for each drug court was identified by searching information provided within evaluation reports, using administrative documents available to the public, or by contacting drug court coordinators. Yearly reported crime and arrest data for each of the drug court locations is available from the FBI's Uniform Crime Reports master files.

A panel fixed effects model is used to analyze jurisdiction-year panel data. This method leverages the variation in timing of drug court implementation relative to changes in reported crime. That is, this model is capable of determining if changes in the dependent variables (specific types of violent and property crimes) coincide with the introduction of the drug court.

Data Sources

The current research uses several data sources to determine the impact drug courts have on public safety within a community (Table 1). For the purposes of this study, “community,” “location,” and “jurisdiction” all refer to either cities or counties. The sample of drug court locations and their effectiveness has been garnered from the list of

² A sensitivity analysis of differences between effective and ineffective drug courts, based on their effect sizes in the Mitchell et al. (2012) meta-analysis was originally intended for this study. There was a lack of variability between “effective” and “ineffective” drug court programs that prevented a meaningful comparison by type of court. That is, the ineffective courts were not different enough from the effective courts to create a true comparison of aggregate crime outcomes based on drug court quality.

studies included in Mitchell's, et al. (2012) systematic review of adult drug court programs. The authors include in their review 92 studies of drug court programs in the United States, Canada, and Australia. Locations were excluded from the current analysis if they were outside the United States, or were calculated as part of a pooled location sample (as with the Multi-Site Adult Drug Court Evaluation). For example, six studies included multiple locations and thus are excluded from the current analysis since it is unclear from the pooled effect size whether the sites included in each of those studies varied by individual court effectiveness. Three other locations (Bronx, Brooklyn, and Queens County, New York) were excluded because their crime information was combined with other locations and reported to the FBI by a larger state agency. The resulting sample represents 30 states and includes 63 unique locations (Table 2)³. There are 54 counties and 9 cities represented in these data.

The second data source included in the current research is yearly crime and arrest rates (per 100,000 people). The FBI's Uniform Crime Reports master reported crimes file (Return A) and the master arrest file include crime counts for specific types of violent and property crimes and arrest counts for specific types of violent, property, and drug arrests. These data begin one year prior to the earliest drug court implementation year and end 3 years after the most recent drug court implementation year to create a balanced data set across all included jurisdictions (1990 – 2008). The reported crimes data file includes four types of violent crime and three types of property crime. Though all crime types are explored, research regarding the relationship between drugs and crime indicates that the

³ Originally, this sample included one other jurisdiction, Miami-Dade, Florida and included the years 1986 to 2008. Investigation of the dependent variable information revealed substantial missing data in the late 1980s, primarily due to non-reports for the entire state of Florida in 1988. To minimize threats to validity, the time period was constrained to include the years 1990-2008. Since Miami-Dade's drug court was implemented in 1989, the jurisdiction was excluded from the current analysis.

property crimes included in this analysis (burglary, larceny, and motor vehicle theft) are the most likely to be impacted by drug court implementation (Nurco, et al., 1990).

Violent crime impacts are also explored, with changes in robbery and assaults more directly linked to drug offenders than homicide or rape because robbery and assault are more directly implicated as violent actions associated with drug markets (Blumstein, 2002).

Dependent variables for violent and property arrests are offered as a sensitivity test to investigate differences with the reported crimes file. The current analysis uses reported crimes as the main dependent variables as it provides a better connection to assessing whether public safety is related to drug court implementation. Measuring public safety via arrest is not as directly linked to the relationship of interest because arrest only counts police encounters with criminal suspects and therefore excludes crimes committed but for which there is no suspect captured (e.g., a reported burglary that does not result in apprehension). Since the reported crimes file does not measure drug activity, and the analysis concerns criminal justice services provided to drug-involved offenders, the UCR master arrest file is used as a proxy to measure community-wide changes in drug crimes with drug arrest information.

The master arrest file includes arrest for the same types of violent and property crimes as are available in the master reported crimes file, with the addition of arrests involving weapons charges, arson, forgery or fraud, embezzlement, buying or receiving stolen property, vandalism, and sex crimes other than rape (e.g., prostitution). The master arrest file also includes arrest counts for sale or possession of illegal drugs, with some reporting agencies indicating the drug type for which the arrest was made (e.g.,

marijuana, opiates, other narcotics, and dangerous non-narcotics). Finally, this file includes arrest counts for various other crimes such as gambling, vagrancy law violations, drunkenness, disorderly conduct, and driving under the influence. Arrests for homicide, rape, robbery, assault, burglary, larceny, vehicle theft, and total drug crimes (with and without marijuana) are analyzed in the current study to offer direct comparison to the investigation of the reported crimes file, with the addition of drug arrests. The other types of crime found in the master arrest file are not as consistently reported and suffer from low values conflating true zeros with zeros due to non-reporting of those specific crime types.

Both sets of dependent variables were converted to rates in order to standardize across jurisdictions of different sizes. The UCR crime and arrest counts for each jurisdiction and year were divided by the jurisdiction population for the corresponding year and then multiplied by 100,000. This result converts the dependent variables to crimes and arrests per 100,000 people. Arrest information for violent and property crimes is used as dependent variables for the purposes of offering a sensitivity test of the main research question. This investigates whether the impact of drug court implementation differs based on whether reported crimes or arrests are measured. In this analysis, drug arrests serve as a proxy for drug market activity within the sample and whether this activity changes when drug courts are implemented. Other sensitivity analyses are conducted to investigate whether the inclusion or exclusion of certain types of drugs (e.g., marijuana) changes the pattern of results.

For multi-agency jurisdictions (e.g., counties), data from “zero-population agencies” are excluded because their jurisdictions overlap with the primary law

enforcement agencies within these populations (Maltz, 1999). These agencies include state police, campus, airport, and other special police agencies. Data for partial and non-reporting agencies were imputed based on the FBI's imputation strategy. More details regarding missingness within the data are presented in the next chapter.

The third data source involves information gathered from each of the drug court programs. Drug court documents, evaluations, or personal correspondence are used to identify the year each drug court began providing services to drug offenders and the year the drug court stopped providing services if applicable.

Finally, data regarding the time-variant control variables was collected to measure demographic characteristics of the locations that have the potential to change over the time span. Jurisdiction-specific characteristics vary over time and may be directly linked to the dependent variables: proportion of 15-24 year olds in the population, percent non-white, per capita income, and the employment rate. Data from the Bureau of Economic Analysis (per capita income), the US Census Bureau (percent 15-24 year olds and percent non-white), and the Bureau of Labor Statistics (unemployment rate) all provide annual information about the jurisdictional demographics of interest in this analysis.

These demographic controls are used in all analysis of the dependent variables because they are important correlates of crime (Zimring, 2008; Blumstein & Wallman, 2000). Specifically, the age-crime curve indicates that young people engage in crime more frequently than older adults and thus increases in the proportion of young people (ages 15-24) within jurisdictional populations would affect increases in crime. Race is also a demographic correlate of crimes wherein people of color come into contact with the criminal justice system at a higher rate than whites. Economic forces are also related

to fluctuations in crime, though the mechanisms by which this connection occurs are not fully developed. Unemployment rates and personal income represent economic forces that may provide opportunity and incentive to participate in income-generating crimes as well as provide opportunities for violent crimes either as perpetrators or victims. Chapter 4 investigates each of these control variables to identify whether they vary significantly over the time period of interest. Though they are all important correlates of crime, it is only necessary to control for jurisdiction-specific time-variant correlates of the dependent variable as the fixed effects absorb time-invariant unobserved heterogeneity.

Analytic Strategy

A panel model using fixed effects is used to answer both the primary research question and in the sensitivity analyses. Specifically, this analysis estimates whether implementing a drug court is related to changes in crime rates. A fixed effects model is used to identify the effect drug court implementation has on changes in crime (lagged 2 years). The rationale for lagging two years follows with many other studies of policy effects on crime (e.g., Lilley & Boba, 2009; Lilley, 2013; Zhao, Scheider, & Thurman, 2002). Since drug court programming usually lasts at least one year, lagging two years takes into account the expected delayed effects of implementing the program. The fixed effects approach controls for differences across locations in time-invariant predictors, which reduces omitted variable bias (Angrist and Pischke, 2009). The strength of this approach is that the model is not biased by omitted time-invariant characteristics of each of the drug court locations. The model coefficient, after controlling for time-variant characteristics, reflects a stronger estimate of the effect of the change in policy (drug

court implementation) by controlling for much of the unique unobserved characteristics of each drug court location. The equation for this estimation is as follows:

$$(1) \ln Crime_{it} = \beta_1 drugcourt_{it-2} + \beta' x_{it} + jurisdiction_i + year_t + \varepsilon_{it}$$

The dependent variable, $\ln Crime_{it}$ refers to each of the dependent variables: reported crime and arrest rates for homicide, rape, robbery, assault, burglary, larceny, motor vehicle theft, and arrest rates for drug crimes. The natural log of all dependent variables was used to normalize the error terms in the models. Crime is defined as a rate representing the number of actual offenses that were reported in the jurisdiction for the year in question, excluding unfounded, false, or baseless complaints. Arrest refers to the number of people formally taken into custody as an arrest by police. β_1 represents the coefficient for the independent variable of interest: the two year lagged implementation year for each drug court location. This variable is equal to 0 during the years the drug court was not operating and turns to 1 the second year after it is implemented (and stays 1 for all years in which it operates) for each jurisdiction. $\beta' x_{it}$ represents time-variant control variables that may confound the drug court-crime relationship. Jurisdiction specific characteristics may vary over time and may be directly linked to the dependent variables: percent of the population aged 15-24 years old, percent non-white, per capita income, and employment rate. These controls may appear more time-invariant in shorter time spans, however, the current analysis includes data spanning 19 years, increasing the likelihood that these characteristics would change enough to make it necessary to control for them in the fixed effects model. The two fixed effects terms, $jurisdiction_i$ and $year_t$ account for time-invariant unobserved differences across jurisdictions (e.g., geographic location, historical development of the legal system) and unobserved heterogeneity across

time that affected all jurisdictions (e.g., the general downward trend in crime over the time span). Finally, ε_{it} represents the remaining unobserved error for each jurisdiction and year.

Equation 1 will also be used in independent-sample sensitivity tests. Once estimated, a statistical test of the differences between the β_1 coefficient estimates will be performed to determine whether the effect of drug courts on public safety differs by altering the sample. For example, the outlier analysis presented in Chapter 4 necessitates a test of independent sample differences to fully understand the implications of removing outliers from the main analysis. Paternoster, Brame, Mazerolle, and Piquero (1998) note inconsistencies with the proper estimation of regression coefficient differences found within criminological literature. Based on their findings, Equation 2 will be used to determine whether the two coefficients statistically differ by considering the difference between the coefficients and their respective standard errors.

$$(2) Z = \frac{\beta_{1a} - \beta_{1b}}{\sqrt{SE\beta_{1a}^2 + SE\beta_{1b}^2}}$$

The current research tests whether adult drug court implementation is related to changes in specific types of reported crimes (homicide, rape, robbery, assault, burglary, larceny, and motor vehicle theft) using 63 county and city jurisdictions over a 19-year panel. A secondary test measures whether this effect differs based on specific jurisdiction characteristics (e.g., jurisdiction size). Other sensitivity tests include measuring the impact on arrests rather than reported crimes, as well as various iterations of the model based on exclusions of outliers and highly imputed jurisdictions. The following section presents the results of these analyses.

CHAPTER 4: RESULTS

This chapter discusses results of the analysis strategy discussed in Chapter 3. A description of the demographic characteristics of the jurisdictions in the sample is presented first. This is followed by a detailed discussion of missing data in the dependent variable and the imputation strategy used to estimate crime and arrest totals for partial and non-reporting agencies. Results are presented for the main research question regarding the impact of drug court on specific violent and property crime rates. Sensitivity analyses are presented, involving the use of alternative dependent variables (arrest rates for specific type of violent, property, and drug crimes), whether the impact differs by jurisdiction size, and by removing jurisdictions with highly imputed crime and arrest information.

Description of Jurisdictions

The sample consists of 63 unique jurisdictions, including 54 counties and 9 cities. Six of the 9 cities in the sample represent county equivalents (i.e., the city is the county) with the other three cities (Oakland, CA, Bismark, ND, and Syracuse, NY) serving as county seats within their respective counties (Alameda, Burleigh, and Onondaga). Control variable information was collected at the county level and thus overestimates demographics in the three cities that serve as county seats. The remaining 60 jurisdictions have geographically identical control and dependent variable information.

Descriptive statistics for this non-random sample of drug court jurisdictions are shown in Table 3. The sample includes four of the ten largest population counties nationwide (Maricopa County, Los Angeles County, Orange County, and Dallas County). Of the 30 states included in the sample, 16 jurisdictions represent the largest county

within their state. Examination of the sample indicates that eight jurisdictions have average populations of less than 100,000 people. Table 3 presents the summary statistics of the sample for all control and dependent variables. The sample mean represents the average value for each variable across jurisdiction and time (N=1197). Since these panel data involve a time component, the range presented in Table 3 represents the range in average values across jurisdiction. For example, the jurisdiction with the lowest average population over the 19-year period had an average population of 12,821 people (Iron County, MI). The jurisdiction with the highest average population over the 19-year period had an average population of 9.4 million people (Los Angeles County, CA). Column 5 in Table 3 represents the average standard deviation from the mean within each jurisdiction for each variable. For example, the average homicide rate for the sample was 7.13 homicides per 100,000 people. The standard deviation represents the average deviation from each jurisdiction's mean homicide rate over the 19-year time period (2.60). That is, this standard deviation offers information about how much the homicide rate changed for each jurisdiction between 1990 and 2008 and gives some insight into time-variance of the control and dependent variables.

Table 3 indicates that jurisdictions in the sample vary by demographic indicators. The jurisdictions varied widely by race with some locations representing majority nonwhite populations (e.g., Prince George's county, MD had an average nonwhite population of 71% over the time period) and some representing majority white populations (e.g., Rutland County, VT was 2% nonwhite on average). The average proportion of youth aged 15-24 within each jurisdiction deviated by an average of only 0.01 (1%) over the time period, with a minimum average proportion of youth in the

population of 0.09 (9%) and a maximum of 0.33 (33%). Per capita income averaged approximately \$38,856 over the time period with jurisdictional averages ranging from \$26,546 to \$60,980⁴. The average unemployment rate for the sample was 5, ranging from 2.49 for the jurisdiction with the lowest average unemployment rate and 9.75 for the jurisdiction with the highest. Overall, the sample primarily represents a geographically diverse set of primarily large metropolitan areas with age, race, and economic demographics consistent with national estimates for large metropolitan areas.

A review of the drug court evaluations available for each jurisdiction offers insight into the types of offenders participating in the non-random sample of drug court programs represented in the current jurisdiction sample. Consistent with the national drug court surveys described in Chapter 2, most (81%) of the drug court programs in the sample exclude offenders with prior violent convictions, though some specified exceptions based on the type of violent crime involved (e.g., domestic violence charges) and how long ago the conviction occurred (e.g., more than 5 years ago). Also consistent with national surveys, nearly all of the programs in this sample required participants to have a substance use disorder and/or a long history of drug use.

Most of the drug courts in the sample were felony courts, indicating the instant offense involved either drug-related felonies (excluding profit-generating drug trafficking) or felonies for property crimes (e.g., theft or forgery). The drug courts varied widely in the approximate number of active participants and admissions to the court each year. Consistent with Rossman et al.'s (2011) survey of adult drug courts, about 35% of the drug courts in the current sample of jurisdictions have the capacity to serve

⁴ Per capita income was adjusted for inflation using 2008 dollars to eliminate substantial overestimates of income growth due to inflation occurring between 1990 and 2008. Bureau of Labor Statistics inflation adjustment factors were used.

approximately 50 active participants, with an average yearly participant population of 117 people.⁵ Based on this information, the jurisdictions in this sample likely impact non-violent drug-involved offenders in need of drug treatment that have multiple prior convictions for drug and property crime. Though a summary measure of offender risk is not available, the offenders in this sample likely include those considered high risk for failure on standard supervision, thus characteristic of the type of offenders drug courts are designed to serve.

Of particular interest for the fixed effects analysis involves examination of the variability within each of the control variables. Standard control variables for crime estimates include population characteristics that are correlated with crime: youthfulness (represented by the proportion of the population comprised of the age of individuals most likely to commit crimes: 15-24 year olds), race, unemployment, and income. These population characteristics are only necessary to include in the fixed effects analysis if they are found to significantly vary within jurisdiction over time because this variation is not absorbed by the fixed effects estimator.

Table 3 presents average standard deviation representing how much variability exists, on average, within each jurisdiction for each variable. Though the average youth population between jurisdictions ranges widely (0.09 to 0.31), the average within-jurisdiction dispersion is relatively low (0.01). To identify whether each control variable varied significantly over time, each were regressed against indicator variables representing each year in the analysis. Results indicated that per capita income, unemployment rate, and proportion nonwhite significantly varied over time but the

⁵ This figure is based on information from 39 drug courts and includes estimates of yearly admissions, capacity, and estimates of current active participants. A strong and consistent measure of the number of drug court participants served by each drug court was not available.

variable representing the proportion of youth aged 15-24 in the population did not. Zero-order correlations with the dependent variables also revealed a lack of significant relationship between the youthful population and these crime and arrest measures. Since only time-variant control variables that are correlated with the dependent variable are appropriate for fixed-effects analysis, the youth population variable was not included in the final models.

Further investigation of the sample jurisdictions uses a class of algorithms called blocked adaptive computationally efficient outlier nominations developed by Billor, Hadi, and Velleman (2000) to identify jurisdictions that may affect the estimation of drug court implementation on crime and arrest rates. This process uses iterative steps of groups of observations to identify those in the sample that deviate substantially from the majority. This approach confirmed that the eight small jurisdictions were significant outliers in the sample for dependent variables involving violent crimes. Additionally, two other jurisdictions were found as outliers in the analysis of homicide rate: Monroe County, IN and Harford County, MD. The average population of these two jurisdictions were also relatively small, with Monroe County averaging approximately 120,000 people and Harford County averaging 217,000 people over the 19 year sample period. This contributed to lack of variation in the dependent variables relative to the dispersion found in the rest of the jurisdictions. For this reason the eight small jurisdictions were excluded from analyses of crime and arrests involving rape and robbery, and the 10 outlier jurisdictions were excluded from the analysis of crime and arrests involving homicide. These jurisdictions were not flagged as outliers for the analysis of the other dependent variables, however a sensitivity test that removes all eight jurisdictions from all

dependent variables (with the additional two removed from the homicide models) was performed to identify if these small jurisdictions were affecting the models. The results for all sensitivity tests are presented later in this chapter.

Description of Missing Data

The data for this analysis include yearly crime and control variable information for 63 adult drug court jurisdictions from 1990 to 2008. For the dependent variables, each jurisdiction represents the sum total of specific types of reported crimes and arrests by reporting agency within that jurisdiction. Table 4 lists each jurisdiction, the number of reporting agencies (excluding zero population agencies and agencies covered by others in the jurisdiction), and the average proportion of reporting agencies with 10 to 12 months of missing data within each jurisdiction.

It was necessary to assess the nature of zeros in the dependent variables to differentiate between whether the zero represented the actual absence of crime or the absence of a crime report by the reporting agency to the UCR program. To differentiate between these two, indicators were used based on the information provided in the documentation for the UCR Return A and Arrest files. For the UCR Arrest file, there is an indicator variable coded as 1 if the agency “reported no data.” For agencies with this indicator turned on, zero values in arrest and reported crimes variables were converted to missing. For the UCR Return A, the difference between true zeros and zeros representing missing data relied on missing data in the variable that records the date that the monthly report was last updated. When an agency does not report crime information for a given month, the variable indicating the date of the report is represented by six zeros rather than

the two-digit month, day, and year of the report. If zeros were present in this variable for a reporting agency in a given month and year, the reported crime data was recorded as missing rather than zero. The remaining agencies with zero values in the reported crime and arrest variables were assumed to be true zeros rather than zeros due to missing data.

The jurisdictions within the sample vary widely by the number of agencies that report information to the FBI's Uniform Crime Reports (Table 4). The average number of reporting agencies within each jurisdiction is 14 (SD=15.298), ranging from 1 to 89 reporting agencies. Eight of the 63 locations in the sample are single-agency jurisdictions. To create a balanced dataset, variable information for all 19 years was collected for all jurisdictions, regardless of their drug court implementation date. This required an analysis of missing data for all jurisdictions over all 19 years, with particular focus on jurisdictions that were missing data from all of their reporting agencies for any given year. Of the single-agency jurisdictions, only Baltimore, MD and Oakland, CA were missing data that required imputation for non-reporting agencies. Baltimore was missing dependent variable information for 1999 and Oakland was missing data for 1995.

Jurisdictions varied by the proportion of reporting agencies that were missing 10-12 months of data each year, requiring diagnostics by year and jurisdiction to study how this may influence the fixed effects analysis. The 10-month threshold defining an agency as a "non-reporting agency" was set by the FBI as the threshold that triggers imputation for non-reporting agencies rather than partial reporting agencies (this strategy is discussed in the "Imputation Strategy" subsection of this chapter). Approximately 14% of the reporting agencies in the sample were missing 10-12 months of data. Madison County, IL was missing data for all reporting agencies from 1994 to 2005 and Rutland

County, VT and Polk County, IA were missing data for all reporting agencies in 1997 and 1991, respectively.

A sensitivity analysis (discussed in a later subsection of the this chapter) explores whether the results for this study differ significantly when the five jurisdictions with highly imputed data for at least one year (Baltimore, MD; Oakland, CA; Madison County, IL; Rutland County, VT; and Polk County, IA) are excluded from the sample.

Imputation Strategy

Missing data in the sample were imputed using the strategy the FBI uses to impute the UCR for their annual publications. This strategy differs based on how many months the reporting agency reported data each year (Maltz, 1999). If an agency is missing less than 10 months of data, the average value in the non-missing months was applied to the missing months. If an agency is missing 10 to 12 months of data, the imputation strategy involves estimating values based on the crime rate for fully reported agencies within the population group and metropolitan status of the reporting agency with missing data. For example, the dependent variable estimates for an agency missing 10-12 months of data reflect the crime rate of reporting agencies of the same population size (e.g., cities with populations 10,000 – 25,000) within the same state. If there are no comparable fully-reported agencies in the same state, geographic region is used (eg., Southwestern United States, Mid-Atlantic states).

Contention over using the FBI's imputation strategy exists primarily for studies that analyze these data monthly or for small locales. The shortcomings of this approach are rooted in the volume of missing data inherent in the UCR data collection (Maltz,

1999). Scholars have developed sophisticated strategies that offer more precise monthly estimates of UCR data that the FBI's imputation strategy is not able to achieve (Maltz, Roberts, & Stasny, 2006; Targonski, 2001; Lavallo, Haas, Turley, & Nolan, 2013). However, the intended purpose of the UCR was to provide annual national estimates of crime in the United States. The relatively simple imputation method used by the FBI since 1958 is precise enough to provide such estimates (Lynch & Jarvis, 2008).

Lavallo, et al. (2013) offer a comparison of imputation strategies for partial and non-reporting agencies in West Virginia. They found that for partial reporting agencies the FBI method did not achieve significantly different estimates than two of the three other imputation methods used. Alternative imputation strategies involved calculating averages of a subdivision of months nearby the missing month, rather than using reported data for the full year to fill in the missing information. For non-reporting agencies, the authors found that regression-based imputation strategies performed better at the agency level, but did not differ significantly from the FBI method when scaled up to larger jurisdictions or when the level of missing data exceeded 50% over a given time period. The overall conclusion regarding use of imputation for UCR data indicates that any imputation strategy is better than doing nothing (Lavallo, et al., 2013).

The current analysis of UCR crime data uses the FBI's imputation strategy for two reasons. First, annual data are less likely to be affected by the pitfalls of imputation strategies (Lynch & Jarvis, 2008). The unit of analysis in this sample is annual crime and arrest information and uses fixed effects to identify whether changes in these annual totals are related to drug court implementation within a given year. Second, most of the sample jurisdictions (87.3%) are multi-agency jurisdictions relying on data from an

average of 14 reporting agencies. The reliability of the imputation strategy is strengthened given the multiple agencies that capture crime information within each jurisdiction, especially considering the fact that most missing data come from smaller jurisdictions rather than county seats (Maltz, 1999). This means that agencies most likely to report are those that are also most likely to report most of the crime or arrest information for larger geographic jurisdictions (e.g., counties). Therefore, the imputation used for non-reporting agencies within the sample is likely to be minimally impactful of the larger jurisdictional estimates of crime within each location. A sensitivity analysis of the effect of including or excluding highly imputed jurisdictions is presented later in a subsection of this chapter.

Analytic Model Diagnostics

A panel fixed effects model is used to analyze these jurisdiction-year panel data to identify whether a change within a jurisdiction (in this case drug court implementation) is related to a change in the crime or arrest rate. The fixed effect that is being controlled here is the time invariant unobserved factors contributing to higher crime rates in some jurisdictions and lower crime rates in other jurisdictions (e.g., geographic location). Unobserved heterogeneity across time that affected all jurisdictions is also captured in this model (e.g., general downward trend in crime over time). The strength of this approach lies in the ability to remove these unobservable characteristics and isolate the relationship of primary interest (drug court implementation and crime/arrest rates). However, it is still necessary to develop a model that is robust to the pitfalls of linear regression (heteroskedasticity) and panel designs (autocorrelation and cross-sectional

dependence). This required investigation of the model using statistical tests that specifically address whether these concerns are present.

Post-estimation tests determined that all three problems were present. Heteroskedasticity biases standard errors and could lead to erroneous inferences. A modified Wald test for groupwise heteroskedasticity indicated the presence of heteroskedastic error terms. Wooldridge's F test for autocorrelation in panel data indicated first order correlation was present. Finally, Pesaran's test of cross sectional independence indicated these data are cross-sectionally dependent (DeHoyos & Sarafidis, 2006). That is, changes in one jurisdiction during a specific year were correlated with changes in another jurisdiction that same year. Ignoring cross-sectional dependence can severely bias standard errors (Driscoll & Kraay, 1998). To account for all three sources of bias in standard errors, Driscoll-Kraay standard errors are used. These errors are robust to cross-sectional and temporal dependence, heteroskedasticity, and autocorrelation (Hoechle, 2007). All dependent variables were log transformed to normalize skew found in the distribution of the error terms. Observations with zeros for specific types of crime or arrest rates (primarily found in homicide rates) were recoded with a very small constant (0.0000001) prior to log transformation to avoid missing values generated when log transforming zero values.

Impact of Drug Court on Reported Crime

The impact of adult drug court implementation on specific types of reported crime is presented as Model 1 in Table 5. Holding time-variant jurisdictional demographics constant, drug court implementation was significantly related to increases in the average

rate of reported homicide (0.344) and rape (0.450) in the second calendar year after the year the court was implemented. That is, implementing a drug court was associated with a 34.4% and 45% increase in the rate of reported homicide and rape, per 100,000 people within each jurisdiction. Drug court implementation was associated with significant decreases in two of the three property crimes, with a significant impact on burglary (-0.091) and larceny (-0.026) but not motor vehicle theft (-0.037). The impact of drug court implementation on total crime was significant, with drug court implementation related to a 3.1% overall decrease in the crime rate. This was likely driven by the significant relationship between drug court and decreased property crime (0.046), with the coefficient estimating total violent crime impacts positive and non-significant (0.019). The substantial and unexpected impact of drug court implementation on homicide and rape required further investigation of the robustness of the model to rule out estimation errors that may have inflated these results.

The previously discussed sensitivity analyses to check for an influence of outliers and highly imputed jurisdictions indicates the violent crime estimates presented in Model 1 are inflated due to outlier jurisdictions. Model 2 in Table 5 presents crime estimates using the restricted sample of 55 jurisdictions, with an additional two jurisdictions removed as outliers in the homicide estimate. The significant relationship between drug court and rape disappears when outlier jurisdictions are excluded from the analysis, while the positive association between drug court implementation and increased reported homicide becomes negative and non-significant. Further, the estimated effect of drug court implementation on robbery rates becomes significant, indicating an average decrease in robbery rates of 7.4%. Investigation of the differences in estimates of

property crime between the two models indicates that while the effect on overall property crimes remains significant (-0.046), larceny is no longer significantly related to drug court implementation. The two model coefficients were also compared using Equation 2 presented in Chapter 3. Though the outliers influence significance tests and magnitudes of the estimated effects, statistical comparison of coefficients across models revealed no significant differences. However, to provide the most conservative estimates of the impact of drug court on crime and arrest rates, Model 2 will be used for all further analyses of all dependent variables, including estimates regarding the impact of drug court on arrest. Table 6 provides the full estimated effect on crime rates for Model 2.

To provide context, one is able to apply the results in Table 6 to any of the drug court jurisdictions using the known crime and arrest rates within given years.

Hillsborough County implemented a drug court in 1994 and consisted of a total population of 893,438 people in 1996. Based on the estimation of the effect of drug court implementation on crime, drug court implementation was associated with decreases of 282 robberies, 1,705 burglaries, 3,254 total property crimes, and an overall estimated decrease of 3,283 crimes.⁶

The time-variant control variables and overall goodness-of-fit of the model (within R^2) also offer information about the estimated effects of changes in jurisdictional crime rates over time. Overall, the models for property crimes are more capable of explaining the average variance of the dependent variables within each jurisdiction than are the violent crime models. However, comparison of the confidence intervals

⁶ To estimate crime totals, the regression coefficient was multiplied by the rate for each specific crime to obtain the rate change. This was divided by 100,000 and then multiplied by the population of the jurisdiction in the 2nd calendar year after the drug court was implemented. For example, the total crime rate in Hillsborough in 1996 was 10,497 crimes per 100,000 people. The equation to obtain the estimated change in the actual crime count is as follows: $(0.035 * 10497) / 100000 * 893438 = 3283$.

surrounding the drug court coefficients for each dependent variable indicates overlapping magnitude of effect. That is, drug court implementation did not contribute to increases or reductions in any one specific type of crime above all others. The Per capita income (in thousands) and the variable for the unemployment rate had the expected effect on crime rates for most models, with increases in per capita income associated with decreases in crime rates and higher unemployment rates related to more crime.

The variable representing the proportion of the nonwhite population in each jurisdiction had a strong negative association with crime rates. This finding was unexpected, but may be related to the nonrandom selection of the jurisdictions in the sample and immigration over the 19-year time period. The research regarding the relationship between race and crime indicates that most of this positive relationship is related to evidence that African Americans are more likely to be disproportionately involved with the criminal justice system (Sampson and Wilson, 1995). Recent studies of immigration and crime indicate that increases in Hispanic populations within communities are not associated with increased crime (Chalfin, 2014). Investigation of the jurisdictions by specific race and ethnicity indicates that the nonwhite population increases were driven by increases in populations of Hispanics. On average, the Hispanic population increased twofold in the jurisdictions from 1990 to 2008, with minimum percent increases around 40% in jurisdictions like Denver, CO, and locations in California that have historically high Hispanic populations. The largest increases in Hispanic populations were found in the south central United States, where jurisdictions like Jefferson County, KY and Shelby County, TN had Hispanic populations that increased over 400%. In comparison, black and Asian populations had more modest

average increases (28% and 57%, respectively) and American Indian/Alaskan Native populations experienced an average decrease in proportion of approximately 11% over the time period. It is likely that increases in nonwhite populations were measuring shifts in immigration within these mostly large metropolitan areas, rather than representing changes in the size of African American communities within jurisdictions over time that are most often associated with increased crime rates.

Analysis of Arrests

Table 7 shows the impact of drug court implementation on select drug, violent, and property arrest rates. Estimates of the control variables indicate similar patterns to those found in the estimates for reported crimes. Comparison of the R-squared statistic indicates that the power of the models estimating impacts on arrest is much lower than the explained variance of reported crimes. Except for homicide, drug court implementation was not significantly associated with arrests for any of the violent or property crime categories, though most coefficients indicated a negative relationship (arrests for vehicle theft was positive and non-significant). An analysis of arrests for drug crimes indicates drug court implementation was associated with significant increases in arrest rates for all drug crimes (0.238) and for drugs other than marijuana (0.259). That is, drug court implementation was associated with a 23.8% increase in drug arrest rates, with a larger magnitude effect on drug arrests involving illicit drugs other than marijuana (25.9%). Using the contextual example presented previously for Hillsborough County, FL, drug court implementation is estimated to be associated with a decrease of 16 homicide arrests and an increase of 700 drug arrests in 1996, 301 of which involve drugs

other than marijuana. Possible explanations for why drug courts may be related to an uptick in drug arrests are discussed in the next chapter.

Analysis of Small Population Jurisdictions

The eight outlying jurisdictions were all jurisdictions with average populations of less than 100,000 people. This similarity across jurisdiction was used to examine the impact of drug court implementation on public safety for locations in which the drug court serves small communities. The jurisdictions in this analysis include: Kootenai County, ID; Wicomico County, MD; Barry County, MI; Iron County, MI; Bismarck, ND; Erie County, OH; Charlottesville, VA; and Rutland County, VT. Three of these eight jurisdictions were evaluated to effectively reduce recidivism: Wicomico County, MD; Barry County, MI; and Erie County, OH. Table 8 provides summary statistics for this small group of drug court jurisdictions. Compared to the full sample (Table 3), these jurisdictions represent small communities with an average population of approximately 63,000 people. These jurisdictions had an average nonwhite population smaller than that for the full sample (.11 and .28, respectively). Crime and arrest rates were also lower in this sample compared to the full sample.

Column 6 in Table 8 indicates wide variability in the stability of the fixed effects model in providing estimates of crime and arrest impacts of the independent variable. The reported model F-statistic can only identify if there is strong evidence that the best fitting linear model has at least one predictor with a non-zero coefficient. The hypothesis testing whether one can reject the null hypothesis that the coefficients equal zero can serve as a loose barometer for whether the model is strong enough to provide confidence in

estimates of each predictor's impact on the dependent variable. F-statistic values indicate a strong likelihood that the small jurisdiction sample does not provide enough variability in specific crime and arrest rates for a measurable effect to be detected and points to a general instability in estimates.

The significant drug court coefficients among the stronger models were those measuring the impact of drug court implementation on summary violent ($b=0.288$, $p<.05$), property ($b=0.247$, $p<.05$), and total ($b=0.209$, $p<.05$) arrest rates. These results indicate marked increases in arrests two years after drug court implementation in small communities, but no significant impacts on reported crimes. These increases in arrests counter that of the findings for the full sample, and when statistically compared (using Equation 2), are significantly different from the non-significant decreases in arrests found when estimating the impact of drug court implementation on arrest rates using the full sample ($N=55$). Also of note are significant differences between the two samples in estimates of drug arrests. Estimation of the larger jurisdictions indicated that drug court implementation was associated with significant increases in drug arrests. Though not significant within the models (likely due to small sample size), the small jurisdiction coefficients for drug arrests were significantly different from the large jurisdiction estimates, representing an estimated decrease in drug arrests in the two years after drug court implementation. The small sample size prevents comparison of the small jurisdictions by effectiveness of drug court.

Sensitivity to Imputation

The five jurisdictions that were found to have highly imputed data for one or more years were removed from the full sample and the restricted sample with outliers excluded. Neither test resulted in significant differences between any of the models or coefficients. This indicates that though some jurisdictions had highly imputed information for one or more years, the annual estimates were not affected by the amount of imputation imposed on the sample or for the number of years in which such imputation was necessary. This confirms the conclusions of others (e.g., Lynch & Jarvis, 2008; Lavallo, et al., 2013) that when used correctly, the FBI imputation strategy for annual crime and arrest estimates does not pose a significant threat to estimation procedures.

Summary of Major Findings

The current analysis of the impact of drug court implementation on public safety indicates that drug courts have an overall public safety benefit with differential impacts on specific types of crime and arrests, and this impact may vary by type of court and community characteristics. Drug court implementation was associated with a 3.5% decrease in overall reported crime rates. Decreases in total property crime rates (4.6%) were largely driven by significant reductions in burglary rates (8.9%). Though robbery rates were also found to decrease (7.4%), non-significant associations with reductions in homicide and increases in rape and assault likely minimized the overall effect of drug court implementation on violent crime rates. An analysis of arrest rates indicated that although a non-significant reduction in homicide rates was found, actual arrest rates for homicide crimes were significantly decreased after the introduction of a drug court into a

jurisdiction (38%). The arrest analysis also revealed significant increases in drug arrests after drug court implementation, a finding that remained consistent when assessing the impact on drug arrests not involving marijuana. A sensitivity analysis of the eight small jurisdictions in the sample indicated a pattern of increases in violent and property arrests, but did not show the same increases in drug arrests found to be significant in the larger jurisdiction sample. A discussion of these findings is provided in the next chapter.

CHAPTER 5: DISCUSSION & CONCLUSIONS

The main finding of the current study supports the conclusion that drug court implementation is related to overall reductions in crime. Specifically, the expectation that drug courts are better suited to impact property crimes was supported, however, the impact of drug court on violent crime remains tenuous. The analysis of drug arrests indicates increases in arrests for drug crimes, however that relationship does not hold true for small drug court jurisdictions serving average community populations of 100,000 or less. This chapter discusses the pattern of results with respect to variations in impact on specific types of crime, why drug courts may increase drug arrests, and offers a review of the limitations that temper this analysis and how future research can address them.

The primary research question tested in the current study explored whether drug court implementation was associated with significant decreases in crime rates. The vast majority of evidence regarding the benefits of drug courts involves analysis of recidivism reductions of drug court programs and does not explore the potential aggregate impact that drug court may have on the communities in which they serve. Given the wide proliferation of drug court programs around the country, and the stated overarching goal of building healthy individuals, families and communities (NADCP, 2008), it is necessary to empirically address the aggregate impact of this alternative approach to treatment of drug-involved offenders in the criminal justice system. Exploring the potential benefits of drug courts is timely, given recent criticisms of the approach's merits in addressing illicit substance use.

Results in Context: Criticisms of Drug Court

Several national policy groups have recently published reports arguing that drug courts are not a good use of government funds to reduce drug use and crime. The Justice Policy Institute acknowledges that drug courts are a better alternative for drug offenders than incarceration, however the author argues that there are other options available to drug offenders that are potentially more impactful on public safety outcomes (Walsh, 2011). Walsh cites a recent meta-analysis conducted by the Washington State Institute for Public Policy that, in part, examines the effectiveness of programs for drug-involved offenders for reducing recidivism (Drake, Aos, and Miller, 2009). Participation in intensive supervision programs that were treatment-oriented decreased recidivism by nearly 18% (based on 11 studies) while adult drug court programs reduced recidivism by 8.7% (based on 57 studies). Walsh (2011) argues that this evidence indicates that addressing addiction for those involved with the justice system by using drug court is an inefficient use of resources given the benefits of other strategies. However, the author translates recidivism reduction to decreases in crime rates (increasing public safety) without an empirical understanding of how these programs may impact the aggregate.

The Drug Policy Alliance contends that though drug courts adopt a disease model of addiction, the overuse of punitive responses to relapses does not represent a quality approach to addiction management. The report from the Drug Policy Alliance (2011) argues that drug court eligibility criteria is so restrictive that it screens out drug offenders that may pose a threat to the safety of persons or property, leaving only low level drug offenders unlikely to engage in serious crimes. This statement is echoed by a report from the National Association of Criminal Defense Lawyers argues that drug courts contribute

to “net-widening” of the criminal justice system by focusing on lower risk drug offenders that may be better served through a public health approach to drug addiction (NACDL, 2009). Though these organizations do not discredit the effectiveness of drug courts for what they are, they do argue that drug courts are an inappropriate medium for substantially reducing drug use and crime within communities.

While the current research cannot address criticisms related to whether drug courts impart the most efficient means by which to supervise and treat drug-involved offenders, it can inform questions about whether adoption of a drug court program within a community represents a step in the right direction. The current analysis of drug court jurisdictions found that implementation of a drug court reduces crime rates by an average of 3.5% two years after implementation of a court. This crime reduction was driven by significant reductions in burglary and robbery rates. A more consistent relationship exists between drug court implementation and property crime reductions, with an inconsistent pattern of findings for the violent crime and arrest variables contributing to a lack of evidence that drug court is related to overall violent crime patterns.

Results in Context: Literature Contribution

This research adds to the very small body of literature regarding aggregate effects of drug court programs. Lilley (2013) measured the impact of receiving federal drug court grants on the same crime rate information used in the current analysis. However, Lilly’s primary conclusion was that receiving a federal drug court grant was related to increases in crime rates (primarily vehicle theft and rape). The author surmised that these increases were likely due to the crime participation of large proportions of unsupervised

drug court dropouts that may have occurred during the adoption of early drug courts in the mid-1990s. Though the current analysis found non-significant increases in vehicle theft and rape, his conclusion runs counter to the significant findings discussed in the current research.

Several reasons may explain the different results of these two bodies of work. First, Lilley's analysis included more jurisdictions than the current analysis, perhaps providing a clearer picture of drug court impacts. The author's sensitivity analysis of only drug court jurisdictions included drug courts that received federal drug court grants from 1990 to 2002, generating an unbalanced dataset of approximately 129 drug courts.⁷ Though there is likely overlap in jurisdictions between the two samples, it is possible the current study of 63 primarily large drug court jurisdictions are fundamentally different from the jurisdictions included in the author's analysis. Specifically, the author's larger sample may include a higher proportion of smaller drug court jurisdictions, which may have biased his conclusions given the differences found between small and large jurisdictions in the current analysis. Further, the current analysis restricts the sample to evaluated drug courts, a characteristic that may separate this sample from a more inclusive sample of evaluated, non-evaluated, or evaluated jurisdictions but with negative findings. That is, the differences between the current research and Lilley's analysis may be due to the characteristics of the drug courts included within the respective sampling frames.

Second, Lilley used receipt of drug court grants as the indicator for the implementation data of the drug court programs, potentially confounding the analysis.

⁷ Based on the number of observations and years reported in the article (N=1680, 1990-2002), the lack of a whole number (1680/13=129.23) indicates some jurisdictions were missing data for some years.

The Bureau of Justice Assistance's Discretionary Drug Court Grant Program offers grants to new drug courts as well as drug courts redeveloping their program. It is possible that receipt of the grant did not result in the actual implementation of the drug court, thus changing the nature of the time threshold by which drug courts are evaluated to impact dependent variables. The current analysis offers a different time threshold (calendar year in which the drug court program began) and thus contributes to knowledge about how and when drug courts may impart community-wide change.

Finally, Lilley used a 1-year lag to identify changes in the crime rate related to drug court grants. This may not be enough time to assess marked changes in public safety due to drug court implementation because most of the first successful participants might not have completed the program yet, nor is it enough time to allow for community-wide changes to potentially take place. Despite the differences in results between the current analysis and Lilley's (2013) analysis of drug court grant impacts, both works provide the first steps in identifying whether a widely-adopted criminal justice policy is producing community-level impacts in crime and arrest.

Results in Context: Addressing Theoretical Mechanisms

Chapter 2 developed two rationales whereby drug court implementation may impact crime rates. The first mechanism is indicative of a specific deterrent effect, indicating drug courts may impact crime through cumulative change in the offending patterns of the participants. Systematic reviews of drug court programs indicate a general focus on drug-addicted offenders in need of treatment that could benefit from consistent criminal justice supervision (NADCP, 2012). Focusing on such individuals can produce

more intense programmatic impacts (Cullen & Gendreau, 2000) even if only offered to a small group of high-risk individuals (Jeglic, Maile, & Calkins-Mercado, 2011). The results presented in Chapter 4 indicate that drug court may be associated with public safety benefits because it is able to offer the right services to the right group of people, but it is beyond the scope of the current research to truly identify the theoretical mechanism that may be driving changes in crime rates.

The drug court literature shows that stronger drug court impacts are likely to be found in high fidelity courts (Shaffer, 2006), indicating that courts more effective in reducing recidivism are courts focused on a high-risk/high-need model program framework (Cullen & Gendreau, 2000). The current analysis included a limited sample of drug courts that, though Mitchell et al. (2012) found some to be ineffective at reducing recidivism, were not significantly different enough to make a strong comparison of “effective” and “ineffective” courts. To assess whether drug court implementation is associated with significant changes in public safety due to specific deterrence, future research would need to include in the sampling frame all drug court jurisdictions where some consistent measure of “effectiveness” is known and “ineffective” courts are truly different from “effective” courts on several measures that assess drug court quality. Alternatively, one could study all persons in the community and measure crime participation. If it was found that decreases in crime participation were attributable to the drug court participants, this would be evidence for a specific deterrence effect of drug court.

Evidence from the current analysis may also indicate that drug court programs are developing unobserved changes in the community that go beyond simple measures of

recidivism reductions. The public safety benefit of drug court implementation may be related to broader community-wide change, linked not only to the specific deterrence of drug court participants but also to the strengthening of communities to which these courts may contribute. The peripheral impacts of drug courts discussed in Chapter 2 include reductions in the use of public assistance, decreases in family conflict, and increases in emotional support systems for drug court participants (Gottfredson, et al., 2005; Green & Rempel, 2012). This empirical analysis of the effects of drug court implementation points to significant benefits with regard to reductions in crime rates upon adoption of a drug court program.

To provide a more complete test of whether this mechanism influences the relationship between drug court implementation and public safety, these peripheral indicators should also be measured to identify community-wide changes due to the ripple effect of drug courts. Similar to the example provided above, a study could measure peripheral impacts of drug court participation by including all persons in a community and measuring specific expected peripheral impacts of drug courts (e.g., participation in public assistance, decreases in generational detriments, decreases in use of public assistance, increased employment, etc.). If these peripheral impacts can be attributed to drug court participants, this would provide evidence for a community repair mechanism.

Decreases in property crime, burglary, and robbery coincide with expectations that reducing recidivism for drug offenders would impart similar changes in aggregate crime rates. The drug-crime literature indicates drug-involved offenders are likely to engage in property crimes either as a symptom of their addiction (e.g., necessity because the addiction interferes with their ability to generate income by ordinary means) or as a

solution to maintaining the addiction (Gottfredson, et al., 2008). The evidence presented in the current analysis supports the notion that if participants in drug court programs are those offenders at high risk of engaging in income-generating crime and are in need of services and supervision in order to change their behavior, it would follow that implementing a drug court within the community may be potent enough to change behaviors of this high risk/high need group. The systemic explanation of the drug-crime connection (Goldstein, 1985) may explain the significant decrease in robbery rates found in the current analysis. Since a substantial number of drug users become involved in drug distribution as their drug using careers progress, removing them from that environment removes them from the systemic violence in which they may once have played a part.

Results in Context: Impacts on Arrest

The analysis of arrest rates with respect to drug court implementation offers only a cursory view of how drug court may impact drug markets. Drug court implementation was significantly related to increases in total drug arrests, with particular impacts on drug arrests involving drugs other than marijuana. However, analysis of smaller communities did not produce the same increases in drug arrests, but rather non-significant decreases in both drug arrest measures. Smaller jurisdictions were found to have significant increases in property, violent, and overall arrest rates after drug court implementation, but these increases were not found in large jurisdictions. Further, arrests for homicide were significantly reduced in the larger jurisdiction sample.

The findings regarding drug court impacts on arrests may be indicators for broader criminal justice changes that relate to implementation of a drug court, thus not

representing good indicators of actual offender behavior. The introduction of a drug court likely coincides with a jurisdictional focus on illicit drug markets within the community. This focus may involve introduction of a drug court as a part of a deeper parallel strategy involving police focusing on getting drug-involved offenders into drug court to more adequately address the “revolving-door” of the criminal justice system (Tiger, 2012). An assessment of the first ten years of drug court implementation indicated that criminal justice professionals at all levels regarded the adoption of a drug court the means by which the jurisdiction could more accurately address substance use (Drug Court Clearinghouse, 1998). Further, the dynamic nature of drug market cycles indicates that the expectation that drug courts would be able to prevent community-wide involvement in illicit drugs (indicative of a decrease in arrests) is relatively low. Law enforcement strategies in dealing with drug markets are the most impactful in diverting emerging drug markets and as harm reduction agents by being the most likely change agent for getting drug-involved offenders into treatment (Strang, Babor, Caulkins, et al., 2012).

Increases in violent and property arrests found in small jurisdictions requires additional explanation. It is possible that small jurisdictions, representing a nonrandom set of mostly rural communities, are less embracing of the drug court model than large jurisdictions concerned with established drug markets as well as criminal justice case overload. Drug court implementation is often met with resistance by law enforcement entities due to a lack of collaboration or buy-in during the establishment of the court (NADCP, 2003). It is possible that arrests for violent and property crimes in the sub-sample of small jurisdictions analyzed were due in part to this lack of cohesiveness in jurisdictional approach to an emerging community-wide substance use problem.

However, more research on the nature of drug markets in small jurisdictions is needed for an empirical treatment of this hypothesis.

Limitations

There are several limitations of the current analysis that qualify the results as patterns needing empirical replication, rather than definitive proof of the public safety effects of drug court implementation. The current analysis can only be generalized to relatively large communities (with populations over 100,000 people) that are uniquely situated to produce methodologically strong evaluations of their drug court programs. The findings in the current analysis are based on a specific subset of drug court programs that restricts generalization beyond the sample of jurisdictions analyzed herein. This subset of jurisdictions with drug courts represent mostly large jurisdictions. All have relatively strong evaluation evidence of their effectiveness for reducing recidivism. The presence of a published evaluation may indicate that though some programs were deemed ineffective in Mitchell et al.'s (2012) meta-analysis, they may still represent the “cream of the crop” of active drug court programs across the nation. Therefore these findings are only generalizable to jurisdictions in the sample, representing large jurisdictions with common drug court characteristics that allowed for methodologically sound evaluation.

The main constraint of this research is the lack of high quality evaluation evidence for drug court programs. Noted in the Mitchell et al. systematic review, the major limitation of understanding the effects of drug court programs is the relatively weak methodologies used in evaluating individual programs. Only three randomized-controlled trials have been conducted to study drug court program outcomes and the rest

of the literature includes quasi-experimental designs that often do not include comparison groups. Mitchell and colleagues isolated the drug court evaluations that were methodologically strong enough to identify effect sizes for recidivism, but very few evaluations discuss the impact drug court has on actual drug use. Given these limitations inherent within the drug court literature, this sample represents only 63 of the nearly 3,000 jurisdictions in the United States (approximately 2.1% of drug courts), nearly all of which represent large metropolitan areas. The sample of small jurisdictions was not representative enough to allow for direct comparison of differences in drug court implementation effects that may be apparent for smaller communities.

This analysis may suffer from missing data limitations that could have affected measurement of the dependent variable. The time span included in the current analysis includes 19 years for 63 jurisdictions with approximately 13% of the reporting agencies in the sample missing 10-12 months of data. Data were imputed using the FBI imputation strategy in order to develop a strongly balanced dataset. This strategy can be problematic because it hides distinct changes that may have occurred during missing time periods. However, imputation methods are capable of providing good estimates of annual crime data when done appropriately (Fox & Swatt, 2008; Haas et al., 2012; Lynch & Jarvis, 2008).

For the current analysis, monthly crime and arrest information of all the reporting agencies for each jurisdiction was explored to diagnose the extent of missing data in the sample and imputation was used to approximate annual crime and arrest totals for locations that required this estimation procedure. Nearly every jurisdiction needed some type of imputation (either partial imputation to develop annual estimates or full

imputation in cases where reporting agencies were missing more than nine months of information. Sensitivity analyses concluded that dropping the most problematic jurisdictions from the sample did not significantly change the results. Though this provides confidence in the inferences drawn from these data, the potential error in drawing conclusions remains present. Future research can expand this analysis to other jurisdictions in order to provide further confidence in these results.

This analysis may suffer from two additional data limitations. First, a relatively small sample size when trying to detect differences in crime and arrest estimates with respect to sub-groups of jurisdictions likely masked detection of a more precise measurement of association between the dependent and independent variables. The low statistical power of assessing changes in the dependent variables for the sub-sample of small jurisdictions only allowed for a superficial view of the impact of drug court implementation in these kinds of communities. Further, it prevented full direct comparison of the impact of drug courts that might differentially occur in small and large metropolitan areas.

Another data limitation involves the possibility that the regression models mischaracterized the relationship between drug court and public safety. Although this analysis attempts to identify the relationship using sophisticated statistical techniques and by appropriately controlling for other time-variant observables impacting the dependent variables, it is possible there are other hidden elements acting in tandem with drug court implementation that are driving the effects. That is, drug court implementation may be only a part of a systematic change wherein other elements may be driving changes in crime and arrests or drug courts impart only a fraction of the changes found in the current

analysis. These other factors were not controlled in the model and thus represent possible error due to omitted-variable bias. The structure of local criminal justice systems would require specific analysis of each jurisdiction to fully understand whether hidden elements may be acting as change agents influencing the measurable relationship between drug court and public safety. Though such an analysis was beyond the scope of the current study, fully identifying the direct relationship between drug court and public safety would involve a deeper analysis of each jurisdiction to rule out a common pattern of other influencers.

The extreme variability of drug court programming, between programs and within programs over time, represents a challenge related to isolating the effect of a policy that is essentially a “moving target.” This analysis loosely defined drug court jurisdictions as communities (counties or cities) that implemented an alternative court and sentencing structure for certain kinds of adult drug or drug-involved offenders that departed from the traditional method of processing such offenders within the criminal justice system. Each drug court jurisdiction had been evaluated for their impact on recidivism and largely consisted of a structure similar to that which is outlined in the NADCP key components of drug courts. Therefore this analysis is only capable of determining whether this loose general structure of drug court is related to crime and arrest rates and is incapable of addressing nuances within or between courts that may be more or less related to the outcomes of interest. Such nuances include the type of drug markets most present within the community, the collaborative nature of the drug court, and the specific inclusion criteria allowing participation in the program.

Implications for Policy

The current analysis of drug court implementation on public safety offers several implications for criminal justice policy. Findings suggest that drug court implementation is related to overall decreases in crime rates, giving support for the adoption of this program by large metropolitan areas concerned with identifying ways to handle illicit substance use disorders among the offender population. These findings are more tenuous for jurisdictions with small populations and thus more research needs to explore the relationship between drug court and public safety before stronger conclusions can be made about the potential impact this criminal justice program may have in smaller communities.

Recent research has demonstrated that the possible impact of drug court programs may be constrained by the restrictive eligibility criteria inherent to most courts that prevent them from serving violent offenders or a larger base of offenders with substance use disorders. The current research offers support for the conclusion that drug courts may be serving enough drug-involved offenders to provide a public safety impact in the surrounding community. These results provide support for measured relaxation of inclusion criteria with continued monitoring of public safety impacts. Relaxing inclusion criteria to include a wider base of drug-involved offenders may have the drawback of diluting programming if the relaxation is not also met with increased resources for the court to maintain program fidelity.

Future Research

This analysis of the impact of drug court implementation on public safety represents a first step in understanding whether this criminal justice program is capable of producing community-wide change. Drug court implementation in this constrained sample of primarily large jurisdictions was found to decrease overall crime rates, with particular reductions in burglary and robbery. A tenuous relationship between drug court and subsequent rates of violent crime, as well as an inconsistent pattern of results concerning arrests, requires further investigation of this important policy question. Future research can broaden the sample to include more jurisdictions and allow for specific investigation of large and small drug court jurisdictions as well as a comparison of effective and ineffective drug court programs. Exploration of other analytic methods involving different dependent variables or qualitative data analysis of other common variables temporally correlated with drug court implementation may be able to reconcile some of the questions generated by comparing the current analysis with Lilley (2013). Further study of the impact of drug courts based on program quality can also provide a deeper understanding of how drug courts may work as community agents of change. Drug courts represent one of the most widely adopted criminal justice programs integrating treatment and supervision to address substance use among drug-involved offenders. It stands to reason that such wide acceptance of a criminal justice policy should be evaluated not only by the program's ability to change individual offending behavior, but also by its relative influence on the communities in which it serves.

TABLES AND FIGURES

Table 1. Data source information and variables derived from them.

Data Source	Description	Type of Variable	Variables
Mitchell, et al. (2012)	Systematic review of 92 drug court programs, of which 63 are included in the current analysis	Independent	Jurisdiction Effectiveness
FBI Uniform Crime Reports, Reported Crimes Master File (Return A), 1986-2008	Monthly counts of actual crimes reported to police by reporting agency	Dependent	Homicide, rape, robbery, assault, burglary, larceny, motor vehicle theft
FBI Uniform Crime Reports, Master Arrest File, 1986-2008	Monthly counts of arrests reported to police by reporting agency	Dependent	Arrests for: homicide, rape, robbery, assault, burglary, larceny, motor vehicle theft, total drug, drugs excluding marijuana
Bureau of Labor Statistics, Local Area Unemployment Statistics Program	County-level (by FIPS code) estimates of annual unemployment rate	Control	Unemployment rate
Bureau of Economic Assistance, Local Area Personal Income	County-level (by FIPS code) estimates of annual per capita income	Control	Per capita income
United States Census Bureau, Population Estimates Program, Intercensal Estimates	County-level (by FIPS code) estimates of annual local area population demographics	Control	Percent nonwhite Percent 15-24 year olds

Table 2. Sample locations sorted by effectiveness in reducing recidivism based on Mitchell, et al. (2012) systematic review.

Location	Start Year	Effect Size
Effective Courts (N = 39)		
Anchorage, AK	1999	1.82
Los Angeles County, CA	1994	1.94
Oakland, CA	1991	2.72
Orange County, CA	1997	1.49
Riverside County, CA	1996	2.20
Santa Clara County, CA	1994	28.89
Escambia County, FL	1993	5.27
Hillsborough County, FL	1994	3.82
Okaloosa County, FL	1993	3.38
Ada County, ID	1998	4.32
Madison County, IL	1996	1.76
Monroe County, IN	1999	2.64
St. Joseph County, IN	1997	2.57
Vigo County, IN	1996	2.76
Vanderburgh County, IN	2001	1.61
Harford County, MD	1997	2.22
Montgomery County, MD	2004	4.17
Wicomico County, MD	2005	4.06
Baltimore, MD	1994	1.79
Barry County, MI	2001	3.13
Kalamazoo County, MI	1997	1.87
Oakland County, MI	2001	1.89
Douglas County, NE	1997	2.77
Clark County, NV	1992	1.74
Rochester, NY	1995	1.58
Suffolk County, NY	1996	2.99
Syracuse, NY	1997	2.11
Erie County, OH	1996	4.41
Hamilton County, OH	1995	1.64
Summit County, OH	1995	1.84
Multnomah County, OR	2002	1.99
Marion County, OR	2001	2.88
Lancaster County, PA	2005	3.39
Shelby County, TN	1997	13.75
Dallas County, TX	1998	2.66
Travis County, TX	1993	3.57
King County, WA	1994	2.50
Pierce County, WA	1994	1.56
Dane County, WI	1996	2.13
Ineffective Courts (N = 24)		
Maricopa County, AZ	1992	1.13
Monterey County, CA	1995	1.23

Location	Start Year	Effect Size
San Mateo County, CA	1995	1.08
Ventura County, CA	2000	1.20
Denver, CO	1994	1.06
Broward County, FL	1991	0.71
Polk County, IA	1996	1.43
Kootenai County, ID	1998	1.58
Jefferson County, KY	1992	1.46
Suffolk County, MA	1999	1.05
Howard County, MD	2004	1.97
Prince George's County, MD	2002	1.08
Cumberland County, ME	1998	1.17
Iron County, MI	2003	1.84
Hennepin County, MN	1997	0.79
Jackson County, MO	1993	2.36
Bismarck, ND	2001	1.30
Chester County, PA	1997	1.38
Philadelphia, PA	1998	1.87
Jefferson County, TX	1993	1.50
Tarrant County, TX	1995	1.55
Salt Lake County, UT	1996	0.75
Charlottesville, VA	1997	0.34
Rutland County, VT	2004	1.19

Table 3. Descriptive statistics for the sample (1990-2008).

Variable	Sample Mean	Minimum Jurisdiction Mean	Maximum Jurisdiction Mean	SD Within Jurisdiction Over Time
Dependent Variables				
Total Reported Crime Rate	6259.50	2244.95	12343.57	1152.40
Total Violent	1749.42	545.49	4662.12	387.22
Homicide Rate	7.13	1.56	43.09	2.60
Rape Rate	40.11	14.08	82.25	12.26
Robbery Rate	206.47	7.36	1079.19	82.35
Assault Rate	1495.71	510.35	3480.36	346.72
Total Property	4508.93	1460.70	8418.04	913.91
Burglary Rate	931.94	298.73	1994.12	253.40
Larceny Rate	3028.99	1072.66	5270.39	582.66
Vehicle Theft Rate	548.00	65.64	1494.57	186.39
Total Arrests	1077.55	258.99	3195.54	291.98
Total Violent	591.19	178.60	2012.87	184.60
Homicide	5.48	0.54	37.65	3.38
Rape	9.48	2.50	27.01	5.43
Robbery	38.57	1.03	200.48	17.32
Assault	537.66	153.38	1903.44	174.36
Total Property	486.37	80.39	1182.67	137.75
Burglary	77.10	18.44	312.56	30.36
Larceny	368.39	56.76	899.09	108.54
Vehicle Theft	40.88	5.19	174.62	17.81
Total Drug	475.14	84.40	2926.05	194.71
Drugs Excluding Marijuana	304.44	26.82	2490.80	139.97
Sample Descriptives				
Population	840950.60	12821.47	9373569.00	115527.90
Control Variables				
Proportion Age 15-24	0.15	0.09	0.31	0.01
Proportion Nonwhite	0.28	0.02	0.71	0.05
Per-capita Income	38855.81	26546.03	60980.14	4110.12
Unemployment Rate	5.00	2.49	9.75	1.12

N = 1,197 annual observations (63 jurisdictions)

Minimum and Maximum Jurisdiction Mean refers to range in means by jurisdiction

SD refers to the average deviation from jurisdiction average over time

Per capita income adjusted for inflation using 2008 dollars.

Table 4. Mean and range of reporting agencies and average percent of reporting agencies with 10-12 months of missing data each year over the study time period (1990-2008).

Jurisdiction	Mean Number of Agencies (SD)	Minimum Number of Agencies	Maximum Number of Agencies	Mean Percent Nonreport
Anchorage, AK	1 (0.000)	1	1	0
Maricopa Cty, AZ	19.74 (0.113)	18	20	9.35
Los Angeles Cty, CA	88.22 (0.301)	85	89	0
Monterey Cty, CA	13 (0.000)	13	13	0
Oakland, CA	1 (0.000)	1	1	4.35
Orange Cty, CA	32.13 (0.567)	27	35	0
Riverside Cty, CA	23.74 (1.058)	14	27	4.76
San Mateo Cty, CA	22.35 (0.264)	20	23	10.21
Santa Clara Cty, CA	16.48 (0.106)	16	17	2.81
Ventura Cty, CA	10.96 (0.043)	10	11	0
Denver, CO	1 (0.000)	1	1	0
Broward Cty, FL	29.65 (0.162)	28	31	10.29
Dade Cty, FL	28.74 (0.446)	26	31	9.98
Escambia Cty, FL	3 (0.000)	3	3	27.54
Hillsborough Cty, FL	4 (0.000)	4	4	6.52
Okaloosa Cty, FL	7.70 (0.277)	5	9	31.68
Polk Cty, IA	10.30 (0.098)	10	11	5.61
Ada Cty, ID	4.78 (0.088)	4	5	15.65
Kootenai Cty, ID	5.35 (0.264)	3	6	24.64
Madison Cty, IL	20.26 (0.261)	18	21	68.36
Monroe Cty, IN	2 (0.000)	2	2	10.87

Jurisdiction	Mean Number of Agencies (SD)	Minimum Number of Agencies	Maximum Number of Agencies	Mean Percent Nonreport
St. Joseph Cty, IN	3.96 (0.277)	3	6	11.38
Vanderburgh Cty, IN	2 (0.000)	2	2	0
Vigo Cty, IN	3 (0.000)	3	3	33.33
Jefferson Cty, KY	16.26 (0.538)	11	18	44.80
Suffolk Cty, MA	4 (0.000)	4	4	20.65
Baltimore, MD	1 (0.000)	1	1	4.35
Harford Cty, MD	4 (0.000)	4	4	0
Howard Cty, MD	1 (0.000)	1	1	0
Montgomery Cty, MD	4.96 (0.285)	2	6	44.78
Prince George's Cty, MD	23.26 (0.309)	19	24	13.10
Wicomico Cty, MD	4 (0.000)	4	4	0
Cumberland Cty, ME	15.70 (0.132)	14	16	9.24
Barry Cty, MI	6.96 (0.172)	6	8	24.38
Iron Cty, MI	6.35 (0.264)	4	7	34.78
Kalamazoo Cty, MI	12.78 (0.088)	12	13	23.16
Oakland Cty, MI	44.04 (0.347)	41	45	7.96
Hennepin Cty, MN	33.78 (0.088)	33	34	5.63
Jackson Cty, MO	12.74 (0.328)	11	15	8.49
Bismark, ND	2.26 (0.094)	2	3	0
Douglas Cty, NE	6.96 (0.239)	5	8	27.10
Clark Cty, NV	4.78 (0.088)	4	5	4.78
Rochester, NY	1 (0.000)	1	1	0
Suffolk Cty, NY	28.83 (0.879)	21	31	32.88

Jurisdiction	Mean Number of Agencies (SD)	Minimum Number of Agencies	Maximum Number of Agencies	Mean Percent Nonreport
Syracuse, NY	1 (0.000)	1	1	0
Erie Cty, OH	4.39 (0.104)	4	5	8.91
Hamilton Cty, OH	38.65 (0.312)	36	40	27.90
Summit Cty, OH	22.52 (0.474)	19	25	32.83
Marion Cty, OR	14.57 (0.176)	13	15	17.32
Multnomah Cty, OR	4.39 (0.104)	4	5	0
Chester Cty, PA	38.65 (0.938)	29	42	18.40
Lancaster Cty, PA	44.26 (0.706)	37	46	16.11
Philadelphia, PA	1 (0.000)	1	1	0
Shelby Cty, TN	6 (0.000)	6	6	4.35
Dallas Cty, TX	23.91 (0.060)	23	24	0
Jefferson Cty, TX	6.78 (0.208)	6	8	9.78
Tarrant Cty, TX	32.70 (0.098)	32	33	0.14
Travis Cty, TX	10.74 (0.113)	9	11	5.14
Salt Lake Cty, UT	12.26 (0.261)	10	13	19.51
Charlottesville, VA	1 (0.000)	1	1	0
Rutland Cty, VT	6.09 (0.259)	4	7	27.74
King Cty, WA	32.13 (1.173)	23	37	5.49
Pierce Cty, WA	17.87 (0.418)	16	20	16.25
Dane Cty, WI	20.61 (0.852)	14	24	8.79

Table 5. Comparison of estimates of all dependent variables with and without outliers.

Variable	Model 1	Model 2
Total Crime	-0.031** (0.011)	-0.035* (0.013)
Total Violent Crime	0.019 (0.024)	-0.008 (0.020)
Homicide ¹	0.344* (0.148)	-0.057 (0.037)
Rape	0.450* (0.208)	0.401 (0.268)
Robbery	-0.077 (0.047)	-0.074** (0.026)
Assault	0.035 (0.023)	0.008 (0.019)
Total Property Crime	-0.051*** (0.011)	-0.046** (0.016)
Burglary	-0.091*** (0.026)	-0.089** (0.032)
Larceny	-0.026** (0.010)	-0.017 (0.012)
Vehicle Theft	-0.037 (0.022)	-0.041 (0.036)
Total Arrests	0.010 (0.034)	-0.022 (0.029)
Total Drug Arrests	0.234* (0.091)	0.238* (0.095)
Excluding Marijuana	0.266* (0.122)	0.259* (0.126)
Total Violent Arrests	0.025 (0.043)	-0.017 (0.041)
Homicide	-0.244 (0.201)	-0.291** (0.101)
Rape	0.055 (0.168)	-0.076 (0.096)
Robbery	0.403 (0.247)	-0.029 (0.034)
Assault	0.035 (0.047)	-0.009 (0.044)
Total Property Arrests	0.007 (0.032)	-0.031 (0.025)
Burglary	-0.035 (0.039)	-0.067 (0.037)
Larceny	0.084 (0.073)	-0.018 (0.033)
Vehicle Theft	0.215 (0.134)	0.036 (0.072)
N observations (jurisdictions)	1197 (63)	1045 (55)

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

¹Homicide was estimated using a restricted sample of 53 jurisdictions (n=1,007)

Driscoll-Kraay standard errors are in parentheses. All dependent variables are log transformed.

Table 6. The impact of drug court on crime rates two years after drug court implementation.

Variable	Homicide ¹	Rape	Robbery	Assault	Burglary	Larceny	Vehicle Theft	Total Violent	Total Property	Total Crime
Drug Court	-0.057 (0.037)	0.401 (0.268)	-0.074** (0.026)	0.008 (0.019)	-0.089** (0.032)	-0.017 (0.012)	-0.041 (0.036)	-0.008 (0.020)	-0.046** (0.016)	-0.035* (0.013)
Nonwhite	-1.435*** (0.322)	-5.042* (2.263)	-1.639*** (0.400)	-0.667*** (0.174)	-1.866*** (0.368)	-1.647*** (0.117)	-1.522*** (0.387)	-0.917*** (0.141)	-1.607*** (0.170)	-1.398*** (0.130)
Unemploy	0.036* (0.016)	-0.038 (0.030)	0.053** (0.015)	0.003 (0.008)	0.054*** (0.009)	0.024*** (0.004)	0.025 (0.017)	0.012 (0.008)	0.031*** (0.006)	0.027*** (0.005)
Income	-0.016* (0.006)	-0.006 (0.011)	0.0001 (0.006)	-0.006* (0.003)	-0.012*** (0.003)	-0.013*** (0.001)	-0.010*** (0.003)	-0.005 (0.003)	-0.012*** (0.002)	-0.010*** (0.002)
Constant	2.679*** (0.236)	5.045*** (0.522)	5.372*** (0.256)	7.646*** (0.174)	7.607*** (0.161)	8.871*** (0.082)	6.902*** (0.166)	7.795*** (0.175)	9.200*** (0.084)	9.407*** (0.099)
Within R^2	0.190	0.008	0.271	0.057	0.618	0.590	0.236	0.116	0.634	0.553

* $p < .05$, ** $p < .01$, *** $p < .001$.

¹Homicide was estimated using a restricted sample of 53 jurisdictions ($n=1,007$) due to two additional outliers.

$N = 1,045$ annual observations (55 jurisdictions)

Driscoll-Kraay standard errors are in parentheses. Jurisdictions and year fixed effects were also included. All dependent variables are log transformed. Per capita income was adjusted to 2008 dollars.

Table 7. Drug court impact on arrest rates two years after drug court implementation.

Variable	Homicide ¹	Rape	Robbery	Assault	Burglary	Larceny	Vehicle Theft	Total Drug Arrests	Drug Arrests (ex. Marijuana)
Drug Court	-0.384** (0.139)	-0.076 (0.096)	-0.029 (0.034)	-0.009 (0.044)	-0.067 (0.037)	-0.018 (0.033)	0.036 (0.072)	0.238* (0.095)	0.259* (0.126)
Nonwhite	0.336 (1.189)	-3.587*** (0.333)	-2.038** (0.678)	-0.401 (0.245)	-1.527*** (0.336)	-1.597** (0.487)	-1.684* (0.678)	-1.165** (0.374)	-2.418*** (0.610)
Unemploy	-0.026 (0.017)	-0.021 (0.013)	0.036 (0.019)	-0.054*** (0.011)	0.031 (0.019)	0.016 (0.012)	-0.024 (0.018)	-0.046** (0.016)	-0.015 (0.021)
Income	-0.031* (0.014)	-0.028*** (0.004)	0.001 (0.007)	-0.014*** (0.003)	-0.014* (0.007)	-0.023*** (0.004)	-0.015* (0.006)	0.007 (0.005)	0.009 (0.006)
Constant	2.829*** (0.524)	4.290*** (0.210)	3.904*** (0.309)	7.078*** (0.187)	5.123*** (0.307)	7.147*** (0.109)	4.679*** (0.220)	6.146*** (0.171)	5.701*** (0.271)
Within R^2	0.051	0.170	0.111	0.086	0.226	0.290	0.066	0.087	0.018

* $p < .05$, ** $p < .01$, *** $p < .001$.

¹Homicide was estimated using a restricted sample of 53 jurisdictions ($n=1,007$) due to two additional outliers.

$N = 1,045$ annual observations (55 jurisdictions)

Driscoll-Kraay standard errors are in parentheses. Jurisdiction and year fixed effects were also included. All dependent variables were log transformed.

Table 8. Descriptive statistics for the outlier sample (1990-2008).

Variable	Sample Mean	Minimum Jurisdiction Mean	Maximum Jurisdiction Mean	SD Within Jurisdiction Over Time	Model F-Statistic
Dependent Variables					
Total Reported Crime Rate	4796.89	2244.95	7958.72	657.73	2.23
Total Violent	1398.43	545.49	2726.31	329.21	59.42***
Homicide Rate	3.23	1.85	7.87	2.98	6.42*
Rape Rate	33.90	15.61	57.90	14.79	1.46
Robbery Rate	64.94	7.36	191.91	17.27	1.34
Assault Rate	1296.37	510.35	2468.63	323.67	54.79***
Total Property	3400.56	1460.70	5226.86	636.95	25.61***
Burglary Rate	642.78	298.73	1004.78	142.30	35.71***
Larceny Rate	2587.83	1096.34	4139.67	525.23	13.24**
Vehicle Theft Rate	169.94	65.64	334.33	37.34	1.39
Total Arrests	937.65	258.99	1838.12	275.78	6.35*
Total Violent	553.04	178.60	1349.35	197.25	25.37***
Homicide	2.99	0.54	6.40	3.23	3.99
Rape	10.32	4.68	22.93	6.57	1.97
Robbery	19.19	1.03	56.31	9.39	8.64**
Assault	520.53	171.05	1300.47	193.85	20.31***
Total Property	384.61	80.39	749.08	116.84	11.00**
Burglary	62.24	18.44	144.01	23.77	11.49**
Larceny	303.62	56.76	566.85	100.30	1.71
Vehicle Theft	18.75	5.19	51.30	10.04	7.95**
Total Drug	318.90	84.40	762.73	161.43	55.33***
Excluding Marijuana	163.44	26.82	337.40	93.42	13.58**
Sample Descriptives					
Population	63228.41	12821.47	104172.7	7792.46	
Control Variables					
Proportion Age 15-24	0.15	0.09	0.27	0.02	
Proportion Nonwhite	0.11	0.02	0.30	0.02	
Per-capita Income	32906.94	26546.03	41184.85	3268.01	
Unemployment Rate	5.27	3.03	8.12	1.28	

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

N = 152 annual observations (8 jurisdictions)

Minimum and Maximum Jurisdiction Mean refers to range in means by jurisdiction

SD refers to the average deviation from jurisdiction average over time

Per capita income adjusted for inflation using 2008 dollars.

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