ABSTRACT

Title: LONG TERM EFFECTS OF DRUG COURT PARTICIPATION: EVIDENCE FROM A 15-YEAR FOLLOW-UP OF A RANDOMIZED CONTROLLED TRIAL

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Substance use disorders and related negative outcomes are on the rise in America. Among jail and prison populations, approximately half of all inmates meet DSM-IV criteria for substance dependence or abuse. Two decades of drug court research indicate that these specialized courts reduce recidivism among participants when compared to traditional probation processing. However, few high quality studies have been conducted and important gaps in our understanding of the model's effectiveness and population suitability remain. Additionally, little is known regarding the long-term impacts of drug courts or the courts' effects on outcomes beyond recidivism and drug use.

One of the most rigorous primary studies to date is the randomized trial of the Baltimore City Drug Treatment Court (BCDTC). Three-year follow-up data from this study showed that participation in the program reduced recidivism and that subjects self-reported less crime and substance use than did controls. This dissertation compares 15-year recidivism, incarceration, and mortality outcomes for the 235 BCDTC subjects. Additionally, it compares differences in recidivism growth over time between the two conditions. The work extends one of the few randomized trials of an established drug court and includes a group of offenders with substantial criminal and substance abuse histories.

Findings suggest that participation in Baltimore City's Drug Treatment Court resulted in significantly fewer arrests, charges, and convictions across the 15-year follow-up period, to include several crime-specific differences in arrests and convictions. Originating court was shown to moderate the effect of drug court participation for convictions, such that those participating in the Circuit drug court had significantly better outcomes than those participating in the District drug court. Drug court participants also had significantly lower rates of growth over time in both arrests and convictions. While differences were sustained across the 15-year period, differences in the rate of growth did not appear to increase over time as hypothesized. Participation in Baltimore City's Drug Treatment Court did not have a significant effect on total days of sentenced incarceration, nor did it have an impact on mortality risk.

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by

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CHAPTER 1: INTRODUCTION

Statement of the Problem

Despite stringent drug laws and high levels of enforcement and drug-related prosecutions, the United States continues to have the highest rate of illegal drug use when compared to other countries in the Americas, Europe, Asia, the Middle East, Africa, and Oceania (Degenhardt et al., 2008). Beyond the risk of addiction, individuals who use illicit drugs are at greater risk for a host of other health issues and problem behaviors. Consequences of substance use include elevated risk of injury, sexually and intravenously transmitted disease and infection, unintended pregnancy, substance-induced mental illness, reduced scholastic and work performance, violence, and other forms of criminal involvement (Biglan, 2004). Recent figures from the Centers for Disease Control and Prevention (CDC) report that drug overdose was the leading cause of injury death among people 25 to 64, surpassing motor vehicle accidents (CDC, 2014). From 1999 to 2013, mortality rates among middle age whites increased at an unprecedented rate due in large part to alcohol and drug poisoning and chronic liver diseases and cirrhosis (Case & Deaton, 2015). This trend reversed decades of progress in life expectancy and was unique to the United States.

While the true costs of substance use are difficult to quantify, as disentangling them from other risk factors is problematic, the economic costs are estimated at over $467 billion annually (Center on Addiction and Substance Abuse [CASA], 2015). Drug misuse resulted in approximately 2.5 million emergency department visits in 2011, over half of which were related to prescription drugs such as opioid painkillers and

benzodiazepines (Substance Abuse and Mental Health Services Administration [SAMHSA], 2013). An estimated 21.6 million Americans age 12 or older currently meet DSM-IV criteria for substance dependence or abuse, equating to 8.2 percent of the total population 12 or older (SAMHSA, 2014). Among offender populations, those numbers jump to a staggering 53 percent of state and 45 percent of federal prisoners (Mumola & Karberg, 2006). A study of arrestees in 5 major metropolitan areas across the country found that between 63 to 83 percent of booked arrestees tested positive for at least one illicit drug (Office of National Drug Control Policy [ONDCP], 2014).

The question of whether persistent drug use represents a moral failure or a medical affliction has been a longstanding debate in this country, and the bifurcated approach of current national drug policy largely reflects these two notions. On the one hand, drug use is treated as a crime that must be punished; on the other hand, it is treated as a chronic relapsing disease or behavioral condition that requires ongoing treatment and support. Some scholars view these two approaches as wholly contradictory, arguing that criminalizing drug use is inappropriate and counter productive. Others point to research that suggests these distinct mechanisms (sanctions and treatment) may actually complement one another, such that they perform better together than they could alone. From this complementary perspective, law enforcement may put pressure on drug users to seek and remain in treatment and drug treatment may help law enforcement by providing a more effective response to persistent drug use than jail or prison. Of great importance then is some clarity regarding the extent to which coerced treatment strategies provide substantial benefits over alternatives without widening the reach or deepening the intensity of punishment (Committee on Data and Research for Policy on Illegal Drugs,

2001). Drug courts are one popular strategy involving coerced treatment for drug offenders.

Drug courts emerged in the late 1980s in response to escalating drug use and interdiction efforts and the subsequent rise in drug offender arrests and prosecutions that overwhelmed the capacity of court systems around the country (Belenko, 1993; Controlled Substances Act 1988). Since being first introduced in Miami-Dade County, drug courts have expanded rapidly, with nearly 3,000 courts operating in the U.S. and its territories as of June 2014 (National Drug Court Resource Center [NDCRC], 2015). Drug courts allow judges to offer treatment services while still maintaining the punitive focus of the criminal justice system. These specialized courts focus on the perceived root of the problem – drug addiction – via a team approach that includes judicial support, intensive probation monitoring, and supportive services such as drug treatment (Harrell, Cavanagh, & Roman, 2000). Sometimes referred to as "problem solving" courts, the logic of the drug court model suggests that if individuals stop using drugs and alcohol at abusive levels, there will be sustained reductions in or desistance from criminal activity and improvements in other life outcomes.

Drug court evaluation research suggests that drug courts represent an improvement over traditional court processing in reducing recidivism for targeted drug offender populations. Yet despite the popularity of drug courts and generally positive research findings, their effectiveness remains somewhat ambiguous because much of the existing research is methodologically weak and plagued by short follow-up periods (Belenko, 2001; U.S. General Accounting Office, 1997, 2005; Shaffer, 2011; Wilson, Mitchell, & MacKenzie, 2006). While drug courts were designed to break the cycle of

addiction and related crimes, very few studies have examined whether or not participants do, in fact, make sustained life changes beyond their period of program involvement. Additionally, few studies have looked at the impact of drug court on outcomes beyond recidivism, even though research from the broader addiction literature demonstrates a clear relationship between reductions in disordered substance use and improvements in a variety of life domains including health (Mitchell, Wilson, Eggers, & MacKenzie, 2012; Scott, Dennis, Laudet, Funk, & Simeone, 2011).

While drug courts have proliferated across the country, on average, they serve a relatively small proportion of the drug offending population, estimated at 5% or less (Belenko, 2002; Sevigny, Pollock, & Reuter, 2013). One central reason for this is that drug court eligibility criteria tend to be highly restrictive – generally serving non-violent, low-risk offenders (Franco, 2010). This despite evidence to suggest that higher risk offenders do equally well or better than their low risk counterparts (Marlowe, Festinger, & Lee, 2003; Marlowe, Festinger, & Lee, 2004; Marlowe, Festinger, Dugosh, Lee, & Benasutti, 2007; Rossman & Zweig, 2012). For example, results from a recent multi-site study including twenty-three adult drug courts from seven regions in the U.S. found that offenders with violent offense histories reduced their substance use at rates equal to their non-violent counterparts and reduced criminal activity at greater levels (Rossman & Zweig, 2012). A meta-analysis by Lowenkamp, Holsinger, & Latessa (2005) found that the effect size for drug court participation was twice the magnitude for high-risk participants as compared to low-risk participants. In some instances, placing low-risk individuals into residential or group-based treatment has been shown to lead to iatrogenic effects, including poorer outcomes and higher recidivism (Lovins, Lowenkamp, Latessa,

& Smith, 2007; Lowenkamp & Latessa, 2005). This is because low-risk offenders may learn antisocial attitudes and behaviors from associating with high-risk offenders and the restrictions of the court may limit or disrupt pro-social involvement with family and employment (Lowenkamp & Latessa, 2004; McCord, 2003).

Research Aims

The primary aim of this dissertation is to examine the extent to which the drug court has a lasting effect on offender behavior: whether the drug court experience represents a turning point in participants' offending trajectories. Numerous criminological studies have demonstrated that desistance from offending is often gradual (Joliffe, Farrington, & Howard, 2013; Bushway, Paternoster, & Brame, 2003). Similarly, the addictions literature has shown that remission from disordered substance use often includes bouts of relapse (Teruya & Hser, 2010; Hser, Longshore, & Anglin, 2007). As such, even if the drug court is successful in impacting long terms patterns of crime and drug use, it will more likely reduce the number of offenses (and perhaps the seriousness of offending) rather than the overall likelihood of recidivism, which would be expected to be quite high for both groups given the sample's offending histories. To avoid the potential "ceiling effect" resulting from a simple consideration of whether or not an individual offended in a given year, this dissertation will examine cumulative recidivism rates and growth over time.

The initial three-year follow-up of the Baltimore City Drug Treatment Court showed that the drug court experience reduced drug use and crime among participants at rates higher than their control counterparts, both during the program and immediately

following completion. The study also showed that there was a clear connection between drug use and changes in offending behavior, such that when drug use decreased, involvement in crime, particularly income generating crime, also decreased. The current research will examine differences in recidivism by crime type. Given that drug courts are designed to reduce drug use, it is expected that drug offenses will be lower for the treatment group. Given that prior examination of the sample found that reductions in drug use led to reductions in income generating crime, it is expected that property offenses will also be lower for the treatment group. Additionally, this study will examine whether the changes observed between treatment and control participants are sustained over the 15 year follow-up period and the extent to which observed effects grow or degrade over time.

While it is possible that time will result in a diminishment of program effects on participants' life outcomes, it is also possible that program effects will continue to grow over time. Mediation analysis at the three-year follow-up showed that participation in the Baltimore City Drug Treatment Court (BCDTC) increased both social controls and perceptions of procedural justice (Gottfredson, Kearley, Najaka, & Rocha, 2007). It is possible that the differences in offending trajectories between drug court participants and those who experience traditional adjudication may accumulate over time, as social capital builds and drug use and criminal attitudes and activity subside (Laub, Nagin, & Sampson, 1998). To the author's knowledge, this is the first study to empirically examine this potential over a 15-year period.

Additionally, this dissertation will explore the degree to which treatment effect heterogeneity may impact long-term drug court outcomes. The initial study of the

Baltimore City Drug Treatment Court found that District Court DTC clients spent considerably more time in jail than their control counterparts and reported higher rates of negative consequences at follow-up than Circuit Court DTC participants, despite entering the program on lesser charges and having shorter suspended sentence lengths. These findings suggest differences at either the participant/risk level, the program/drug court implementation level, or both. Measures of client characteristics at intake were limited and, of those available, no significant differences in addiction severity or offending were observed. Differences in drug court implementation are also difficult to ascertain as it is unclear whether variability in court actions were due to implementation differences or in response to differences in participant compliance. As such, this dissertation will explore treatment effect heterogeneity with the inclusion of originating court as a moderator variable. One could expect, for example, that the differences in observed jail time may have an effect on long term outcomes, such that any positive effects observed would be less pronounced or perhaps even negatively impacted for those who performed poorly and as a result received more jail time than their traditional adjudication counterparts. Severe incarcerative sanctions have the potential to decrease perceptions of procedural justice, attenuate bonds to prosocial institutions and others, and increase association with criminal peers. This research will explore the potential long-term implications of said negative consequences on future life outcomes and offending patterns.

Finally, this dissertation examines whether involvement in the drug court has an impact on mortality outcomes. Drug-related morbidity and mortality are areas of pressing concern in the United States, particularly among opiate users. The majority of the Baltimore City Drug Treatment Court study sample cited heroin as their primary drug

at intake. While reducing mortality is not a principal objective of the drug court model, reductions in drug use and related risk behaviors have the potential to reduce mortality risk. To date, no known study in the published drug court literature has examined this potential.

This work addresses a number of the criticisms of previous drug treatment court studies by comparing 15-year recidivism, incarceration, and mortality outcomes for a sample of drug offenders randomized to receive either drug court or traditional adjudication. It builds upon earlier work conducted on the Baltimore City Drug Treatment Court, which showed reductions in recidivism, drug use, and welfare participation for drug court participants relative to controls at the three-year follow-up, though some findings varied by originating court with Circuit Court subjects generally having better outcomes than District Court subjects. This dissertation is unique in that it extends one of the few randomized trials of an established drug court, includes a group of offenders with substantial criminal and substance use histories, examines outcomes well beyond the typical 6- to 12-month follow-up period, uses an analytic approach that mimimizes potential ceiling effects, and provides data on a pressing area of concern: mortality.

The following hypotheses related to the long-term effects of drug court participation will be tested:

H1: ***Individuals who participated in the Baltimore City Drug Treatment Court will have lower cumulative rates of recidivism (including both arrests and convictions per***

days free in the community) over the 15 years following randomization than individuals who received traditional adjudication.

H1a. ***These effects will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having significantly lower cumulative rates of recidivism than District drug court participants.***

H2: ***Individuals who participated in the Baltimore City Drug Treatment Court will have fewer days of incarceration over the 15 year follow-up than individuals who received traditional adjudication.***

H2a. ***These effects will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having fewer days of incarceration than District drug court participants.***

In addition to whether or not the differences between the two groups are sustained, this dissertation will also look at the nature of change over time. If differences are sustained, do they grow or degrade with time? In the initial evaluation of the Baltimore City Drug Treatment Court, differences in the arrest rate between the treatment and control groups were actually larger post-treatment than in-treatment, suggesting that not only were the effects of the program sustained during the three year follow-up period, but that the differences between the two groups grew over time. This may be due to lower levels of scrutiny rather than actual changes in behavior post-treatment. However, it is also possible that, through treatment and court oversight during the program, those in the drug court condition were able to eventually stabilize or end their drug taking and, as

a result, committed fewer crimes over time, looking less and less like their substance abusing counterparts in the control condition. This dissertation will test the following hypothesis regarding the form of change:

H3. ***Individuals who participated in the Baltimore City Drug Treatment Court will have a faster rate of desistance as compared to individuals who received traditional adjudication and the differences between the two groups will grow over time.***

> **H3a.** ***These effects will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having a significantly greater rate of desistance as compared to District drug court participants.***

Finally, this work is one of the first to examine whether or not drug courts have an impact on public health outcomes. This is surprising given the strong link between drug use (particularly opiate and injection drug use) and overdose or deaths by related diseases and infections. This dissertation will address this limitation of current scholarship by testing the following hypothesis regarding drug court's impact on mortality:

H4: ***Individuals who participated in the Baltimore City Drug Treatment Court will have lower mortality rates over the 15 years following randomization than individuals who received traditional adjudication.***

Summary

Substance use disorders and related negative outcomes are on the rise in America. Among jail and prison populations, approximately half of all inmates meet DSM-IV criteria for substance dependence or abuse. Two decades of drug court research indicate that these specialized courts reduce recidivism among participants when compared to traditional probation processing (Mitchell et al., 2012; Wilson et al., 2006; Aos, Mayfield, Miller, & Yen, 2006). However, important gaps in our understanding of the model's effectiveness and population suitability remain. Additionally, little is known regarding the long-term impacts of drug courts or the courts' effects on outcomes beyond recidivism and drug use.

The current study seeks to advance our understanding of the long-term effects of drug courts, as well as provide insight into the processes and characteristics that impact individuals' persistence in or desistance from drug-related crime. Analysis of the first three years of data showed that participation in the drug court program yields positive change, but the ultimate goal is long-term, meaningful change for offenders.

Given what is known about addiction, successful treatment of drug offenders usually occurs incrementally over an extended period of time (Hser, Longshore, & Anglin, 2007). Therefore, it is critical to assess the extent to which early behavior change due to participation in drug court is converted into social capital that continues to accrue benefits throughout the life course. By contributing to our understanding of the long-term effects of drug court programs, this dissertation aims to provide the criminal justice field with evidence regarding the efficacy of drug courts and the utility of their use on a wider scale. Additionally, this dissertation explores whether or not the effects of the

court are heterogeneous across differing risk populations and whether the drug court experience reduces mortality risk.

The remaining sections of this document take the following form: Chapter 2 provides a brief history of drug courts, to include the historical context and impetus for the courts and an overview of the model and its key components. The chapter also considers the implicit theoretical underpinnings of drug courts, synthesizes prior work on the courts effectiveness in practice, and outlines the limitations of existing research and other model critiques. Chapter 2 concludes with the current study's primary research aims. Chapter 3 details the RCT design and data used in this dissertation, describes the interventions received by experimental condition, and discusses the modeling strategies employed to examine outcomes of interest. Specifically, the analyses utilize negative binomial regression for the 15-year end point recidivism and incarceration outcomes, growth curve modeling for the longitudinal recidivism outcomes, and survival analysis for the mortality outcome. Chapter 4 presents the results of the analyses. Finally, Chapter 5 summarizes the research findings, considers the study's limitations, and concludes with implications for public policy and future research.

CHAPTER 2: LITERATURE REVIEW

American Drug Policy and the Impetus for Drug Courts

The historical antecedents of America's contemporary drug policy can be traced to the temperance movement of the late 19th century, which sought to limit or prohibit the use of alcohol due to the drug's negative mental and physical health consequences and the belief that consumption led to immorality and criminal behavior. [1] A constitutional amendment (Eighteenth Amendment) was eventually signed into law prohibiting the production, sale, and transport of alcohol. The Volstead Act of 1919 was later enacted to carry out the intent of the Eighteenth Amendment and the Jones Five and Ten Law of 1929 was enacted to enforce stringent penalties, e.g., five years in prison, a $10,000 fine or both for first time offenders (Musto, 1999). Referred to as "a noble experiment" by then President Herbert Hoover, prohibition was nonetheless struck down in 1933. This change was due to a variety of factors, including the Great Depression, which led many to question the expense of interdiction and enforcement efforts; waning public support due to what were perceived as excessive penalties and underwhelming reductions in use; widespread corruption among law enforcement and politicians; and intensifying levels of organized and violent crime surrounding the illegal alcohol trade (Goode, 2012; Belenko & Spohn, 2015).

Despite the failure of prohibition, drug regulation and interdiction efforts continued. As with the Progressive era's temperance movement, the concerns were

[1] While a detailed history of American drug policy is beyond the scope of this dissertation, it has been covered extensively in several well-researched works (see Musto, 1999; Goode, 2012).

largely due to health concerns and the growing number of individuals who became dependent on substances such as morphine and cocaine. The Harrison Narcotics Tax Act of 1914 sought to regulate and tax the production, importation and distribution of opiates and cocoa products (Goode, 2012). Eliminating the opiate maintenance practices of some physicians and clinics was a primary focus, and the act did, in fact, sharply reduce prescribing and use for a time (MacCoun & Reuter, 2001). Similar federal and state legislation was enacted for marijuana in 1937 and psychoactive pharmaceutical drugs in the 1960's. In response to rising drug use and a wave of heroin addiction during the 1960s, these accumulated statutes were replaced by the federal Controlled Substances Act and by parallel acts at the state level (Committee on Data and Research for Policy on Illegal Drugs, 2001). In 1971, President Nixon declared a "War on Drugs," vastly increasing the size and presence of federal drug control agencies.

The criminalization of the production and distribution of these substances placed control efforts largely within the purview of the criminal justice system, particularly law enforcement agencies. The number of people incarcerated began to rise due in large part to lengthy mandatory sentences for drug convictions. However, efforts to reduce drug use were not totally focused on deterrence style sanctioning methods. As was true during the prohibition era, some within the medical and legal community believed drug addiction was a disorder, with roots in biology, social conditions, or the dependence-producing power of the drug themselves. These individuals advocated for a public health approach and pushed for alternatives to the law enforcement model.

The first coerced treatment programs were instituted in California under the California Civil Addict Program, which permitted the state to involuntarily commit

people to inpatient drug treatment and follow-up. Federal policy in the form of the Narcotic Addict and Rehabilitation Act was passed in the late 1960s permitting all states to implement coerced treatment programs. Drug treatment and prevention efforts were expanded, most dramatically under the Nixon Administration, with nearly two-thirds of the federal "drug war" budget earmarked for prevention and treatment efforts (Belenko & Spohn, 2015).

While a priority during the Vietnam Era and corresponding counterculture movement, the emphasis on prevention and treatment did not last long. Publications such as the infamous Martinson report (1974) argued that the rehabilitative ideal and related programs guiding drug and correctional policy were largely ineffective. Lawmakers and correctional administrators increasingly embraced a "just deserts" philosophy to drug offenders that focused on incapacitation and formal control mechanisms. This philosophical shift signaled an increase in law enforcement scrutiny, tougher sentencing practices, and reduced availability of drug treatment.

There were a number of pivotal events that further hardened drug policy during the early 1980s, marking the start of what is considered the contemporary War on Drugs. One large concern was the Medellin drug cartel, a network of Colombian traffickers who cooperated in the manufacturing, distribution and marketing of cocaine to the U.S., and who were responsible for concerning levels of domestic and international violence. In 1982, there was a high profile seizure of almost 4,000 pounds of cocaine, valued at over $100 million wholesale, from a Miami International Airport hangar, which permanently altered law enforcement's approach toward interdiction efforts.

In the mid-1980s, crack cocaine markets began to flourish in certain regions of the country, the impacts of which were felt most acutely in inner city neighborhoods. This led to a corresponding spike in negative health outcomes due to growing use and public safety concerns as drug dealers fought for control of lucrative crack markets. A New York Times cover story in 1985 brought the drug to national attention, and from there, mainstream media latched onto crack cocaine as a regular news story. The high profile cocaine-related deaths of promising college basketball star Len Bias and Cleveland Browns football player Don Rogers led to sensationalistic news coverage. Stories focused on "crack babies" and innocent bystanders caught in the crossfire of drive by shootings, prompting Newsweek to proclaim that crack was "the most addictive drug known to man." Ensuing media coverage highlighted the health risks of cocaine and further catapulted the drug as a hot political issue.

In the midst of a surge of articles regarding the crack epidemic, both the United States Senate and the House of Representatives held hearings on the perceived crisis. At these hearings, it was asserted that crack was: (1) more addictive than powder cocaine, (2) produced physiological effects that were different from and worse than those caused by powder cocaine, (3) attracted users who could not afford powder cocaine, especially young people, and (4) led to more crime than powder cocaine did.

These hearings laid the groundwork for the Anti-Drug Abuse Act of 1986, which established the basic framework of statutory penalties applicable to federal drug trafficking offenses. Congress differentiated between the two principal forms of cocaine – powder and crack – and provided significantly higher punishment for crack cocaine offenses. Around this same time, the U.S. Sentencing Commission was in the process of

developing the initial sentencing guidelines, and they generally incorporated the statutory mandatory minimum sentences into the guidelines and just extrapolated upward and downward to set guideline sentencing ranges for different drug quantities.

The Anti-Drug Abuse Act of 1988 soon followed, establishing a mandatory minimum penalty for simple possession of crack cocaine, which was the only federal mandatory minimum penalty for a first offense of simple possession of a controlled substance. The consequence of this was that possession of five grams or more of crack cocaine triggered a mandatory minimum sentence of five years in prison. In other words, an offender who simply possessed five grams of crack cocaine received the same five-year mandatory minimum penalty as a trafficker of other drugs. State drug laws were similarly revised, though most did not include the same magnitude of sentencing disparity between crack and powder cocaine.

These changes in sentencing and enforcement efforts had substantial effects on arrests and criminal prosecution of drug offenders and contributed greatly to widening racial disparities. Between 1980 and 1994, local and state arrests for drug offenses more than doubled, from approximately 581,000 to over 1.3 million (Snyder & Mulako-Wangota, 2003). Over a similar timeframe, incarceration rates for state and federal prisons increased nine-fold for drug offenses from 15 inmates to 148 inmates per 100,000 adults, with drug offenders comprising 60 percent of the federal prison population and 23 percent of state prison populations (Blumstein & Beck, 1999).

This influx of drug offenders into local, state and federal corrections systems led to prison overcrowding and financial strain. As a result, or perhaps simply a reflection of ideological shifts, offenders were offered fewer opportunities for treatment. For example,

in 1996, despite the growing number of drug offenders in jails and prisons, only 18% of addicted inmates received treatment while incarcerated (Petersilia, 2003).

The Drug Court Model

Drug courts emerged in response to these escalating War on Drugs initiatives and the subsequent rise in drug offender arrests and prosecutions that overwhelmed the capacity of court systems around the country (Belenko, 1993; Controlled Substances Act 1988). The first court began in Miami, Fl, which was a primary gateway for Latin American trafficking at the time and particularly hard hit by the emergence of crack cocaine (Goldkamp, 1999). Court professionals had grown increasingly frustrated with the revolving door of recidivism and criminal justice involvement among drug offenders and were searching for more sustainable solutions (National Association of Drug Court Professionals [NADCP], 2012). Some scholars have argued that the movement was also supported by a desire on the part of judges to reestablish their relevance after stringent mandatory sentencing laws removed most judicial discretion (Goldkamp, 2000; Tiger, 2012).

Drug courts grant judges the authority to offer treatment services while still maintaining the punitive focus of the criminal justice system. The programs attempt to address the perceived root of the problem – drug addiction – via judicial support, intensive probation monitoring, and supportive services such as drug treatment (Harrell, Cavanagh, & Roman, 2000). While drug courts vary from jurisdiction to jurisdiction in terms of scope and focus, the primary goals of all courts are to reduce crime and

recidivism, reduce substance use, and rehabilitate participants (U.S. Department of Justice [USDOJ], 2012).

Drug courts are typically managed by a multidisciplinary team, including judges, prosecutors, defense attorneys, social workers, and treatment service and community corrections professionals (USDOJ, 2012). The judge has a central role, prosecution and defense counsel work in a cooperative, non-adversarial fashion, and noncompliance (typically related to substance use) is expected and does not necessarily result in the immediate application of a traditional disposition. Participants enter the program voluntarily, either pre- or post-adjudication, and are offered a reduction in penalties or dismissal of charges upon successful completion. Since being first introduced in Miami-Dade County in 1989, drug courts have expanded rapidly, with nearly 3,000 courts operating in the U.S. and its territories as of June 2014 (National Drug Court Resource Center [NDCRC], 2015).

Key Components of Drug Courts. In the late 1990s, the National Association of Drug Court Professionals (NADCP) brought together a diverse group of drug court practitioners and academics to develop a set of standards for drug courts nationally (NADCP, 1997). The goal was to bring some consistency and clarity to the model by outlining the central aims and values of the drug court (NADCP, 1997). From that meeting, ten key components and associated benchmarks were set forth to describe the very best practices, designs and operations of adult drug courts. While the ten key components were originally designed to provide guidance to jurisdictions that were considering implementing a drug court, this document is now used as a standard by

which funders, policy makers, and researchers evaluate the degree to which a drug court is operating with fidelity to the model (Hiller et al., 2010).

The ten key components of drug courts include the following: 1) integrated alcohol and other drug treatment services with justice system case processing; 2) a non-adversarial approach in which prosecution and defense counsel promote public safety while protecting participants' due process rights; 3) identification of eligible participants early and promptly; 4) access to a continuum of alcohol, drug, and other related treatment and rehabilitation services; 5) frequent alcohol and other drug testing to monitor abstinence; 6) a coordinated strategy governing drug court responses to participants' compliance; 7) ongoing judicial interaction with each drug court participant; 8: monitoring and evaluation to measure the achievement of program goals and to gauge effectiveness; 9) continuing interdisciplinary education to promote effective drug court planning, implementation, and operations; and 10) partnerships among drug courts, public agencies, and community-based organizations to generate local support and enhance drug court program effectiveness (NADCP, 1997, 2004). They focus primarily on court and treatment processes, emphasizing the importance of intensive supervision as an accountability mechanism and drug treatment as a rehabilitative mechanism. Several other components underscore the importance of collaboration and a non-adversarial approach among drug court stakeholders to ensure swift and proper identification of eligible offenders and rapid case processing. The final three components focus on the efforts required to ensure the long-term effectiveness and sustainability of drug courts, to include things such as program monitoring and performance evaluation, continuing education for drug court professionals, and collaboration and capacity building efforts.

While adherence to the ten key components varies widely across courts, research has demonstrated that drug courts with high-fidelity program implementation (e.g., those that provide access to effective treatment, consistent intensive supervision, a collaborative, non-adversarial court team, etc.) are much more effective and reduce recidivism over probation comparison groups by up to 40% (Zweig, Lindquist, Downey, Roman, & Rossman, S.B., 2012; Shaffer, 2006; Lowenkamp et al., 2005). However, these effects are not necessarily consistent across setting.

Additional studies have attempted to identify the most important components of the drug court model; these studies of drug court elements have typically employed either a dismantling strategy or a best practices approach (Marlowe, 2010). Dismantling strategies experimentally manipulate different components of the drug court model in order to isolate the effects of the components that contribute to positive outcomes (Festinger, et. al., 2002; Marlowe et al., 2003). Dismantling strategies yield the strongest evidence for the importance of a particular component. Best practices strategies compare the elements of drug courts that had positive outcomes with drug courts that had negative or null effects and attempt to discern the services provided only by the effective programs and only by the ineffective program (Carey, Finigan, & Pukstas, 2008). The services of effective programs are then considered to contribute to positive outcomes and the services of ineffective programs are considered to be suspect or superfluous. These strategies are less reliable because they do not make use of experimental control, but they are still instructive and, with replication, contribute to the knowledge base regarding what works and what does not. Additionally, qualitative studies conducted with drug court

participants shed light on the components participants themselves deem most important (Berman, Rempel, & Wolf, 2007).

Judicial Monitoring. Results from several dismantling studies have shown that judicial monitoring is an important drug court mechanism (Marlowe et al., 2003, 2005) compared outcomes among a sample of high-risk drug court offenders who were randomly assigned to receive bi-weekly status hearings with a judge versus those who were assigned to monitoring by a treatment case manager and found that those under judicial supervision had significantly higher rates of counseling attendance, drug abstinence, and graduation. These findings held in both misdemeanor and felony drug court settings, serving both urban and rural populations (Marlowe et al., 2004). Gottfredson and colleagues (2007) found that judicial status hearings were related to fewer subsequent arrests, controlling for several important background characteristics. Additional analyses of BCDTC programmatic and theoretical mediators showed that status hearings reduced participants' self-reported crime (via increased procedural justice) and drug use (Gottfredson et al., 2007). A study by Carey and colleagues (2008) found drug courts that require bi-weekly status hearings for at least the first several months of the program had significantly better outcomes than courts that held less frequent hearings. In interviews and focus groups, drug court participants consistently attribute judicial contact as key to their success in the program (Saum et al., 2002; Berman et al., 2007).

Drug Testing. Regular drug testing, particularly at the initial stages of drug court involvement, is essential to ensure drug abstinence and program compliance. To be effective, testing must be paired with swift and certain punishment for positive drug

screens (Harrell & Roman, 2001; Harrell & Kleiman, 2002; Hawken & Kleiman, 2009). Additionally, drug testing is optimized when it is performed on a random basis so that participants cannot plan "safe" drug use days and so that adulterants and other countermeasures cannot be used to create false negative results. Gottfredson and colleagues (2006) found that increased days of drug testing was negatively associated with self-reported days of multiple drug use. Best practices research suggests that the most effective drug courts conduct urinalysis at least twice per week during the first phase of the program, leading to a four-fold increase in such programs' cost-benefit in terms of reductions in recidivism (Carey et al., 2008). Interviews with drug court participants consistently link the importance of drug testing to accountability and success within the program (Turner, Greenwood, Fain, & Deschenes, 1999; Cresswell & Deschenes, 2001; Belenko, 2001).

Drug Treatment. Drug treatment is considered a core element of drug courts. Despite this, or perhaps because of it, few studies have isolated the independent effect of drug treatment. While there is a good deal of evidence to support drug treatment's ability to reduce offending generally (Hubbard, Craddock, & Anderson, 2003; McGovern & Carroll, 2003), within drug court settings most of the evidence is inferential, i.e., days of drug treatment are associated with better outcomes but motivation cannot be ruled out as the causal mechanism. Banks & Gottfredson (2003) found that attending treatment reduced the risk of recidivism over a two year follow-up and a later instrumental variables analysis of the same sample showed that recidivism was lowest among subjects who participated at higher levels in certified drug treatment, status hearings, and drug testing (Gottfredson, Najaka, Kearley, & Rocha, 2006). Some researchers and

practitioners have suggested that drug treatment may be unnecessary for the vast majority of offenders, provided they are subject to close monitoring, drug testing, and swift and certain sanctions: the Project HOPE model reflects the success of such a model (Hawken & Kleiman, 2009).

Treatment quality in criminal justice settings is often poor and non-evidence based and there is research to suggest this extends to at least some drug courts. Several reports have underscored the difficulty of accessing effective treatment and have found that providers often lack advanced credentials or a coherent treatment philosophy. However, the scale of these issues is not known (Taxman & Bouffard, 2003). Research has demonstrated that improvements in treatment quality lead to better drug court outcomes (Heck, 2008), which suggests logically that when properly implemented, drug treatment does have at least some impact on drug court effectiveness.

Drug Courts in Theory: An Exploration of Underlying Mechanisms

Social scientists involved in program and policy evaluation are rarely asked whether an intervention makes sense theoretically. More often the questions posed relate to whether or not an intervention works (Sherman, 2010). While answering the question of "what works" is certainly a valid area of inquiry that satisfies citizens' and policy makers' real world concerns, an equally compelling and perhaps better first question is a theoretical one: why does the program work? What theory or theories underlie the design of the program and how do they operate to affect long-term change?

In many respects, the first drug court in Miami developed in an improvisational manner, based on the practical experience of the court actors involved (Goldkamp, 2010). Elements of the program such as the nonadversarial approach of defense and state's

attorneys, the tie between treatment providers and the court, and the use of acupuncture, were implemented in a step wise fashion and then adjusted along the way as needed. Because this first court did not necessarily rely on a guiding theoretical framework or set of principles, later versions of the drug court model often departed considerably in both theory and practice from the original court in Miami. In fact, some of what were considered to be essential elements of the initial drug court, in terms of values, procedures, and operating philosophy, were often missing from later court models (Goldkamp, 1999).

Despite these realities, attempts have been made to tie the drug court model to various theoretical perspectives, the most common of which include deterrence, therapeutic jurisprudence, and procedural justice (see Wexler and Winick, 1991; Goldkamp & Weiland 1993; Goldkamp, 1994, 1999, 2000; Goldkamp, White, & Robinson, 2001; Hora, Schma, & Rosenthal, 1998; Longshore, Turner, & Wenzel, 2001; Nolan 2002). Some researchers have also tested these theories in the drug court context (Senjo & Leip, 2001; Miethe, Lu, & Reese, 2000; Marlowe & Kirby, 1999; Marlowe et al., 2005; Marlowe, Festinger, Lee, Dugosh, & Benasutti, 2006; Gottfredson et al., 2007; Rossman, Roman, Zweig, Rempel, & Lindquist, 2011).

The following sections outline the implicit theoretical underpinnings of drug courts that are proposed to affect behavior change via both instrumental (deterrence) and normative (therapeutic jurisprudence, procedural justice) mechanisms. Relevant to the current study, the life course perspective is then introduced as a framework for understanding the potential long-term contribution of drug court involvement on

desistance via mechanisms such as strengthened social bonds, structured routines, and renewed commitment to a conventional lifestyle.

Deterrence. Deterrence theory assumes that offenders are rational actors who seek to maximize their benefits while minimizing their pains from crime (Bentham, 1996). The theory posits that criminal conduct is inhibited due to the fear of sanctions, and that the most effective sanctions must be swift, severe, and certain yet proportionate to the crime (Beccaria, 1764). Two principles are central to deterrence theory: 1) specific punishments imposed on an offender will deter them from committing future crimes, referred to as specific deterrence; and 2) would be offenders will be deterred from offending by seeing others punished, referred to as general deterrence (Gibbs, 1968; Becker, 1968).

Drug courts incorporate deterrence principles through the use of close judicial supervision and monitoring, regular drug tests, and graduated sanctions. Offenders routinely witness the sanctioning of other non-compliant drug court offenders during regular status hearing with their peers and are themselves subject to formal monitoring by treatment and probation staff and to sanctions for noncompliant behavior during status hearings before the judge. This instrumental leverage of the criminal justice system is hypothesized to improve drug treatment compliance and drug and crime outcomes. (Taxman, Faye, Gelb, & Soule, 1999).

Therapeutic Jurisprudence. Therapeutic jurisprudence regards the law as a social force that produces both behaviors and consequences. Central to this normative perspective is the notion that – intentional or not – substantive rules, procedures, and

legal roles have therapeutic or antitherapeutic effects (Wexler & Winick, 1996; Wexler, 2004). Therapeutic jurisprudence focuses on the extent to which the law can be made or applied with a therapeutic emphasis without subordinating other core values of the justice system such as due process.[2] As such, legal actors are encouraged to consider and select options that enhance the psychological and/or physical well being of offenders.

Judge Peggy Fulton Hora and colleagues were the first to propose therapeutic jurisprudence as the drug court movement's jurisprudential foundation (Hora et al., 1998). Several scholars have since characterized drug courts as an illustration of therapeutic jurisprudence in action (Nolan, 2002; Senjo & Leip, 2001). The legal system acts as a catalyst for change for offenders by helping them understand the patterns of their destructive substance use and related behaviors, holding them responsible for those behaviors, and assisting them with the process of change (Fiorentine, Hillhouse, & Anglin, 2002). The framework has increasingly been provided as the theoretical foundation for other "problem-solving" courts, such as domestic violence and mental health courts, where the role of the court has been similarly transformed.

Procedural Justice. Procedural justice suggests that people are more likely to obey the law and engage in self-regulatory behavior when they view the criminal justice system and the behavior of legal actors as fair and legitimate (Tyler & Huo, 2002; Tyler, 1990). In contrast to compliance brought about through external coercive actions, the

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[2] Critics of therapeutic jurisprudence models note that judges often lack the skills required for the model, moving from neutral arbiter to leader of a treatment team. They have wide discretion in the handling of cases, resulting in inconsistent and sometimes unduly punitive treatment of offenders. See Nolan, 2001 for a thoughtful critique of drug courts and the implications of this philosophical shift.

theory posits that people will be internally motivated to suspend self-interested decision making when they deem a rule or authority entitled to determine their behavior.

Within the context of drug courts, the impact of procedural justice on outcomes is most often attributed to the unique role of the drug court judge. Drug court judges give attention to, and are often evaluated on, their approachability, respectful treatment, knowledge of the defendant's case, efforts to help the defendant succeed, and allowing the defendant to tell his or her side of the story. These attributes are consistent with many of the key elements of procedural justice, which include: voice, respect, neutrality, understanding, and helpfulness (Tyler, 1990).

The Life Course Perspective. One area that has been relatively unexplored in discussions of drug courts is the intervention's potential impact on the life course of treated offenders. This is somewhat surprising given that drug and other "problem solving" courts were developed in an attempt to address the underlying root causes of crime. The logic of the drug court model suggests that, if successful, individuals should stop using drugs and alcohol at abusive levels, which in turn should lead to reductions in or desistance from criminal activity and improvements in other life outcomes.

The life course perspective has its origins in longitudinal studies conducted in the early 20th century among cohorts experiencing events such as the great depression and World War II. These events inspired scholars to think holistically about how lives are given distinctive form by historical and contextual factors (Elder, 1994). The conceptual framework of the life course perspective has gained greater clarity in the past several decades, but it remains broad in scope, reflecting a theoretical orientation, a research methodology, and an empirical field of study (Laub, 2016).

In the context of this framework, the life course is defined as "a sequence of culturally defined age-graded roles and social transitions that are enacted over time" (Caspi, Elder, & Herbener, 1990). Central to the life course perspective are concepts such as trajectories, transitions, and turning points and a consideration of both behavioral continuity and change. The perspective focuses on the interdependence of multiple trajectories - for the purposes of this dissertation, of behaviors such as drug use and criminal activity - and considers their influence on behavioral continuity and change. Trajectories can be thought of as long-term behavioral patterns whereas transitions are short-term changes embedded in trajectories. Transitions that result in a change in trajectory are considered turning points (Laub & Sampson, 1993).

Examining criminological theory and research through a life course framework has led to numerous insights into patterns of offending. Life course criminology examines both between-individual differences in offending over time as well as the factors associated with within-individual change (Farrington & Welsh, 2006).
This dissertation takes a state dependence approach with a corresponding believe that adult social bonds to individuals and institutions help to explain patterns of behavioral continuity and change.

In a recent work regarding life course research and public policy, Laub (2016) outlines the ways in which social institutions, including the criminal justice system, have the potential to modify trajectories in either a positive or negative direction. Laub points to four underlying mechanisms responsible for positive change, including "a 'knifing off' from one's delinquent past; monitoring coupled with social support; enactment and the reinforcement of new routines; and cognitive identity shifts and new "life scripts." Laub

also points to findings from his work with Robert Sampson that suggest the criminal justice system can have negative effects, such as the collateral consequences of severe sentences and long term impacts on future employment (Sampson & Laub, 1995).

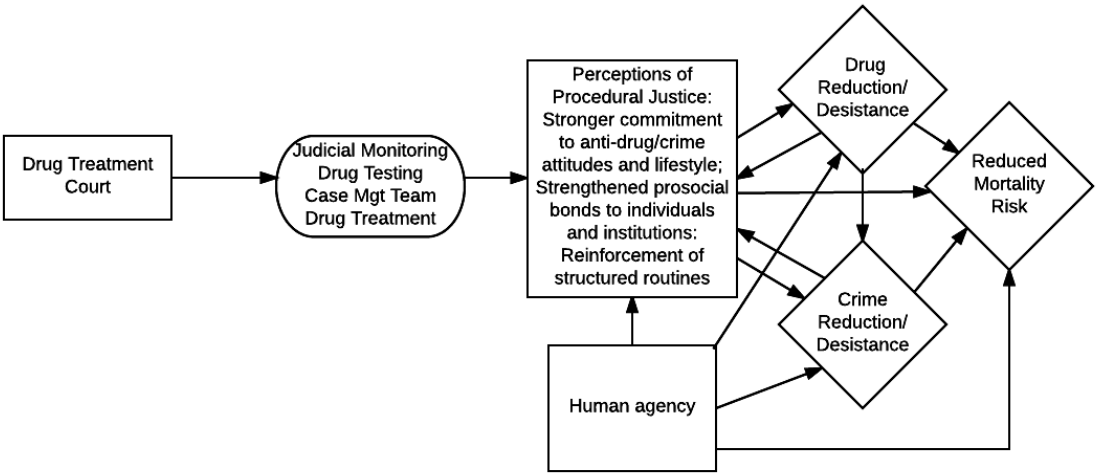
Several examples from the broader literature illustrate the potential of social programs, both within and outside of the criminal justice system, to positively modify life trajectories and also underscore the ways in which program impacts can change over time. The Nurse-Family Partnership program provides nurse home visits to first time pregnant women, most of whom are unmarried, low-income teens. The nurses teach positive health related behaviors, child care skills, and maternal development in areas such as family planning, educational attainment and workforce participation. The program resulted in significant reductions in child abuse, neglect, and injuries (20-50% across 3 RCTs). Additionally, a 19-year follow-up revealed that first-born children of the nurse-visited women were 43% less likely to have been arrested and 58% less likely to have been convicted of a crime. The Serious and Violent Offender Reentry Initiative (SVORI) provides funding for the development, enhancement, and expansion of services to facilitate the reentry of adult and juvenile offenders. An initial evaluation of SVORI found significant but modest effects on employment, health and housing 3 months following release but no significant impact on rearrest or reincarceration (Visher, Lattimore, Barrick, & Tueller, 2016). However, a 5-year follow-up revealed that participation in the program was associated with longer time to arrest and fewer arrests. The High Intensity Training (HIT) program in England was an intensive military regime with significant rehabilitative components (to include CBT, drug education, and work placement) delivered to 18- to 21-year-old male offenders. Participants were

significantly less likely to be reconvicted up to Year 5 when the percentage having been convicted of any crime was quite high for both groups (suggesting a ceiling effect). However, analysis over 10 years of data showed that the cumulative *number* of convictions saved increased steadily from 1.35 per offender at 2 years to 3.35 convictions per offender at 10 years (Jolliffe et al., 2013).

Drug Courts as a Turning Point in the Life Course. While this dissertation does not seek to directly test a theory of desistance per se, it is important to consider the mechanisms that would explain the drug court's impact on long-term patterns of behavior change. For each individual, there are a number of possible drug court-related outcomes: no effect, and short term and long term effects, with each of these having the possibility of being either positive or negative.

To be clear, the data for this dissertation do not permit a fine-grained analysis of the factors leading to increases or decreases in substance use and criminal activity over the entire 15-year period. As such, Figure 1 provides a *conceptual* desistance model that draws on earlier analyses of the sample and describes the manner in which the intervention has the potential to serve as a catalyst for long-term behavior change.

Figure 1. Conceptual Desistance Model for Drug Treatment Court Participants



The model in Figure 1 recognizes the role of human agency: that an individual may choose to desist from drug use and crime irrespective of drug court involvement, indeed many make this choice without intervention. As noted by Giordano and colleagues, "Unlike a change in careers…desistance is achieved when one simply stops engaging in the criminal behaviors in question" (Giordano, Cernkovich, & Rudolph, 2002). The first step of agreeing to take part in the voluntary drug treatment court program suggests some level of intrinsic motivation to reduce or desist from drug use.

That being said, practically speaking, the chances of behavior change are often enhanced considerably via external catalysts that seek to increase social control and encourage prosocial cognitive shifts (Sampson & Laub, 1995; Giordano et al., 2002; Sampson & Laub, 2003; Paternoster & Bushway, 2009). These catalysts range from informal sources such as family and friends to formal coercive strategies presented by employers and the criminal justice system (Anglin, Prendergast, & Farabee, 1998).

The drug court has the potential to represent such a catalyst and exerts its influence via a number of empirically supported deterrent and therapeutic mechanisms (referenced earlier in this document, e.g., judicial monitoring, drug testing, drug treatment). These drug court mechanisms are hypothesized to reduce drug use and crime by enhancing perceptions of procedural justice, promoting commitment to prosocial attitudes and lifestyle, strengthening bonds to prosocial institutions and others, and enhancing participants' development of structured routines.

Perceptions of procedural justice among the DTC group are influenced by regular meetings with the drug court judge and others in the case management team. Increased perceptions of legitimacy promote commitment to prosocial attitudes and lifestyle and are hypothesized to generate a lasting downward deflection in drug court participants' drug use and offending trajectories.

With regard to strengthening bonds to prosocial institutions and others, involvement in the drug court requires that individuals report regularly to a drug court judge and case management team. Additionally, participants are expected to seek or maintain employment and involvement in a recovery community via drug treatment, AA/NA or both. The intensive nature of the drug court program requires that participants organize their schedules in order to meet all program requirements, e.g., status hearings, drug testing, and drug treatment. As a result, participants' time is highly structured around these responsibilities as well as any outside obligations to work and family. Participants create linkages with prosocial others through the recovery community who provide support and a shared commitment to sobriety and positive change. In many instances, individuals in treatment reference these new peer associations as integral to

their desistance process (Giordano et al., 2002). These exogenously generated transitions are hypothesized to generate a lasting downward deflection in drug court participants' drug use and offending trajectories.

Consistent with Laub, Nagin, and Sampson's (1998) conception, it is hypothesized that these acquired forms of social capital will continue to grow gradually over time. As such, it is possible that the differences in offending trajectories between drug court participants and those in the traditional adjudication group may be cumulative, as social capital builds and drug use and criminal activity subside. In addition, these effects may be reciprocal such that social controls and budding social capital lead to decreases in drug use and criminal activity and those reductions in drug use and crime further support the expansion of social controls and increases in social capital.

Importantly, one could expect that these differences in offending trajectories may be moderated by differences in case handling, such that the positive effects would be less pronounced or perhaps even negatively impacted for those in the drug court who performed poorly and as a result received more jail time than their traditional adjudication counterparts. Severe sanctions have the potential to decrease perceptions of procedural justice, attenuate bonds to prosocial institutions and others, and increase association with criminal peers. This possibility provides the rationale for the study's moderation hypotheses.

The three-year follow-up of the Baltimore City Drug Treatment Court sample showed significant and sustained positive average treatment effects of drug court participation on recidivism. Additionally, differences in the arrest rate between the treatment and control groups were actually larger post-treatment than in-treatment,

suggesting that the differences between the two groups grew over time. While it is possible these differences were driven by lower levels of scrutiny rather than actual changes in behavior post-treatment, it is also possible that through treatment and court oversight, those in the drug court condition were able to reduce to non-compulsive levels or end their drug use and, as a result, committed fewer crimes over time.

An analysis of BCDTC mediators determined that drug court participation increased perceptions of procedural justice, which in turn reduced crime. Drug court participation also increased social controls, which then reduced drug use. There was support for all of the major mechanisms of the model (hearings attended, drug testing, and drug treatment) in reducing drug use and crime (Gottfredson et al., 2007).

In summary, the drug court model provides support for all of the primary mechanisms of change outlined by Laub and Sampson, e.g., monitoring and social support, enactment and reinforcement of new routines. As such, the drug court experience could function as a positive turning point in the life course for these offenders by strengthening ties to prosocial institutions and others, the echoes of which have gradual yet far reaching implications for change. Alternatively, the court experience could function as a negative turning point if, for example, drug court involvement inadvertently results in a more punitive response than traditional adjudication, potentially further limiting an individual's access to and involvement with prosocial institutions and others and undermining perceptions of the criminal justice system's legitimacy. This possibility is explored within the treatment heterogeneity hypotheses as treatment cases within the District Court spent more time in jail on average than control cases within the District Court during their period of supervision.

The Drugs/Crime Nexus. Figure 1 also outlines the ways in which reductions in substance use are hypothesized to lead to reductions in crime. Substance use researchers have recently begun to consider the life course framework as a useful way of studying long-term patterns of use (Teruya & Hser, 2010; Hser et al., 2007). Addiction can be thought of as a chronic and recurring condition, and as such, many providers and researchers in the field have embraced long-term care models for understanding and treating drug addiction, as well as assessing the role of treatment in health related outcomes. The life course framework holds the promise of increasing our understanding of why and how drug users transition through periods of abuse and abstinence over the life course and may be particularly fruitful in unpacking the mechanisms involved in long-term sobriety.

Much has been written about the chronic nature of drug addiction, particularly for substances such as heroin, as well as its relationship to crime (Nurco, 1987). However, there is a large body of literature to support the influence of treatment on reducing lifetime prevalence of substance use and for aiding participants in short- and long-term abstinence and reductions in criminal offending (Prendergast, Podus, Chang, & Urada, 2002; Kinlock, Gordon, Schwartz, Fitzgerald, & O'Grady, 2009). In the Baltimore City Drug Treatment Court study, drug treatment was negatively related to drug using during the three-year follow-up (Gottfredson et al., 2007). There is also evidence to suggest that prolonged treatment and multiple episodes of treatment may be required to achieve long-term abstinence and fully restored functioning (National Institute on Drug Abuse [NIDA], 2000).

The drugs-crime nexus literature most often links drug use to drug and acquisitive forms of crime and these findings were supported in earlier analyses of the BCDTC (Gottfredson, Kearley, & Bushway, 2010; Anglin & Perrochet, 1998). 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 (Harrison & Beck, 2006). In a review of 25 years of research conducted at UCLA's Drug Abuse Research Center, Anglin & Perrochet (1998) concluded that the commission of property crimes almost always increased to support dependence level use 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 nonaddiction (Nurco, 1998). A more 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, 2011).

With regard to the study sample, an analysis of life history calendar data that modeled change within subjects over the three-year follow-up found that increased substance use, to include cocaine or heroin or alcohol, was significantly related to increases in self-reported income generating crime. The study also found that drug treatment in the last month had a significant impact on income generating crime via the mediating effect of reduced drug use (Gottfredson et al., 2010).

The Drugs/Mortality Nexus. In addition to drug use and offending outcomes, Figure 1 outlines the ways in which the drug court experience is hypothesized to have an impact on mortality. A recent study in the American Journal of Public Health used

longitudinal data from community-based substance use disorder treatment facilities to examine how and to what extent drug treatment and abstinence mediate mortality risk (Scott et al., 2011). The authors found that higher percentages of time abstinent and longer durations of continuous abstinence were associated with reduced risk of mortality. Treatment readmission in the first 6 months after baseline was related to an increased likelihood of abstinence, whereas readmission after 6 months was related to a decreased likelihood of abstinence. The authors concluded that the addiction treatment field should move from an acute care model to a chronic disease management paradigm and suggested the need for more aggressive screening, intervention, and addiction management over time.

Drug induced deaths are one of the leading causes of mortality in the United States (Centers for Disease Control [CDC], 2014). Further, the association between drug use and other prominent causes of mortality and chronic disease are well documented. Individuals who use illicit drugs are more likely to die due to an accident or high-risk sexual behavior and are significantly more likely to die from diseases such as cancer, heart disease, or respiratory illness (Mokdad, Marks, Stroup, & Gerberding, 2004). A study examining the survival rates within a Baltimore-based sample found that individuals with a drug diagnosis die on average 22.5 years earlier than those without the diagnosis (Neumark, Van Etten, & Anthony, 2000).

Drug Courts in Practice: Findings from Empirical Research

The majority of the extant drug court literature consists of individual drug court evaluations and meta-analyses of drug court evaluations (Gottfredson, Najaka, Kearley,

2003; Mitchell et al., 2012; Wilson et al., 2006; Lowencamp et al., 2005). These studies generally focus on process measures and reductions in drug court participants' recidivism, while only a handful has looked at drug use and other psychosocial outcomes. None of the drug court evaluations to date have examined the model's potential impact on mortality and none examine long-term outcomes.

Several recent meta-analyses of drug court evaluations conclude that drug courts are a better criminal justice response for drug offenders than traditional probation processing. 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 mean effect size is analogous to a drop in recidivism from 50% for non-participants to approximately 38% for participants (Mitchell et al., 2012). Similarly, 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.

However, a recent meta-analysis by Sevigny, Fuliehan, & Ferdik (2014) found that while drug courts significantly reduced the incidence of incarceration for the precipitating offense, they did not significantly reduce the average amount of time participating offenders spent behind bars, as those who fail drug court often receive longer sentences. These findings raise the question as to whether drug courts, as currently implemented, serve as an effective alternative to incarceration. Findings from a randomized trial by Gottfredson and colleagues (referenced in detail below) found elevated incarceration risk for some segments of the drug court population under study,

suggesting that attention must be paid to potential differences in drug court's stated aims and actual practice.

Despite largely positive findings, it is important to note that most of the drug court literature is methodologically weak, with few randomized studies and only a modest number of rigorous quasi-experimental studies (Mitchell et al., 2012). In a recent quantitative review, Guiterrez and Bourgon (2012) analyzed the quality of 96 drug court studies. Using Collaborative Outcome Data Committee (CODC) guidelines[3], the researchers found that over three-quarters of the studies (n=78) were "rejected" by CODC standards, 23 were rated as "weak" and only 2 studies rated as "good." None of the studies reviewed were rated as "strong." The potential for bias in weak drug court studies is particularly pronounced as treatment assignment is often based on situational (e.g., type of arrest) or personal characteristics (e.g, motivation, risk level). This has prompted a number of scholars to conclude that the lack of randomized experiments in drug court research limits firm conclusions on the model's effectiveness (Wilson et al., 2006; Belenko, 2002).

The following section will focus on findings from some of the few randomized controlled trials of drug court outcomes with specific concentration on the original evaluation of the Baltimore City Drug Treatment Court. The studies generally support the positive effects of drug courts found in less rigorous designs but raise important

[3] The CODC Guidelines are a comprehensive scale developed for the purposes of rating the study quality of treatment outcome research in order to reduce bias in systematic reviews. It was originally created for a review of the literature on sex offender treatment programs but has been modified for broader use.

questions regarding model mechanisms, treatment heterogeneity, and the extent to which the model offers an alternative to incarceration.

Drug Court Findings from Randomized Controlled Trials

The first randomized controlled trial of a drug court was conducted by the RAND Corporation in Maricopa County, Arizona. Drug offenders were assigned to one of four conditions, including drug court and three different levels of supervision and drug testing (Deschenes, Turner, & Greenwood, 1995). At the 12-month follow-up, there were no significant differences in arrest between the conditions, but the drug court group had lower rates of technical violations while under supervision with fewer drug violations. At the 36-month follow-up, the drug court group continued to have significantly fewer technical violations than comparison groups and they also had significantly fewer new arrests (Turner et al., 1999). These results suggest that the positive effects of drug courts may not be immediate, instead reflecting a more gradual process of change. This finding further underscores the need for long-term follow-up studies of drug court cohorts.

In a randomized controlled trial of the District of Columbia's drug court, drug offenders were assigned to one of three tracks, including standard adjudication involving drug testing and supervision; a track involving graduated sanctions, judicial monitoring, drug testing, and treatment only for those who requested it; and a drug court track that included all of the components of the graduated sanctions track along with mandatory intensive day treatment (Harrell et al., 2000). Results showed that participants in both the graduated sanctions and drug court track were significantly more likely to be drug free prior to sentencing than the standard adjudication track. At 12-months post sentencing for the initial arrest, both groups were also significantly less likely to be arrested for a

drug crime compared to the standard processing track. While the results showed support for drug courts over standard adjudication practice, they also suggested that the graduated sanctions track may be just as effective whether or not treatment is received.

The Baltimore City Drug Treatment Court (BCDTC). The original BCDTC study began in 1997. The first report from this randomized controlled trial (Gottfredson & Exum, 2002) found that 63.5% of control cases were arrested for new offenses versus 48.2% of drug court cases during the first 12 months following random assignment. The drug court sample also had significantly fewer arrests (0.9 vs. 1.3) and significantly fewer charges (1.6 vs. 2.4), as compared to controls.

Findings from the second year of the study showed sustained treatment differences with regard to recidivism (Gottfredson et al., 2003). Specifically, 66.2% of drug court and 81.3% of control subjects were arrested for new offense. The number of new arrests (1.6 vs. 2.3) and new charges (3.1 vs. 4.6) were also significantly lower for treatment than control group members, and these difference remained significant even after taking into account time not at risk during the follow-up period due to incarceration. Effects favored the BCDTC cases for every type of crime examined, but were statistically significant only for drug-related crime charges. This dissertation also compared BCDTC subjects who participated in treatment with those who did not and with control subjects. The study found that treated drug court subjects were significantly less likely to recidivate than both untreated drug court subjects and control subjects.

Survival analyses examining time to rearrest in the first 24 months following randomization showed that assignment to the drug court significantly increased time to rearrest (Banks & Gottfredson 2003). When the survival functions were examined

separately for drug court cases who actually received drug treatment services and those who did not, again the results showed that attending treatment significantly decreased the risk of failure over a two-year follow-up period.

Results from a three-year evaluation of BCDTC using official records showed a sustained treatment effect on recidivism, controlling for time at risk. This effect was not limited to the period during which services were delivered. Rather, it persisted even after participation in the drug court program ceased (Gottfredson, Kearley, Najaka, & Rocha, 2005). Results from an instrumental variables analysis that controlled for substantial selection effects also showed that recidivism is lowest among subjects who participated at higher levels in certified drug treatment, status hearings, and drug testing (Gottfredson et al., 2006). This study was the first attempt at decomposition of effects in the drug court literature to control for selection artifacts as all previous analyses simply compared completers to non-completers.

Further, results from follow-up interviews with 157 research participants 3 years after randomization into treatment and control conditions showed that program participants reported less crime and substance use than did controls (Gottfredson, et. al, 2005). Few differences between groups were observed on other biopsychosocial outcomes, although treatment cases were less likely than controls to be on the welfare rolls at the time of the interview. These positive findings are tempered with findings that more than three-fourths of clients were re-arrested within three years, regardless of participation in the drug court, and that drug court cases spent approximately the same number of days incarcerated as control cases.

Treatment Heterogeneity. Research has shown that drug courts often produce the greatest benefit to those who have the highest need (Taxman & Marlowe, 2006). These "high-risk" offenders[4] tend to have more severe antisocial backgrounds and treatment-resistant histories and, as such, poorer prognosis for success in standard rehabilitation programs (Marlowe, 2012). The drug court model, with its emphasis on intensive supervision services, helps disrupt these ingrained, negative behavioral patterns. Recent research found that offenders with violent offense histories reduced their substance use at rates equal to their non-violent counterparts and reduced criminal activity at greater levels (Rossman & Sweig, 2012) Conversely, low-risk offenders whose behavioral patterns are less entrenched may receive substantially fewer benefits but at the same substantial cost. Of particular concern are iatrogenic findings suggesting that placing low-risk individuals in group-based or residential treatment may actually produce worse outcomes and higher recidivism (Lowenkamp & Latessa, 2004; McCord, 2003; Lowenkamp & Latessa, 2005).

Analyses from the original BCDTC evaluation showed that program outcomes often differed significantly by originating court, with Circuit BCDTC cases fairing significantly better relative to controls than District Court participants, according to tests of statistical interaction between originating court and treatment status. Specifically, DTC subjects who were processed through the Circuit Court were significantly less likely to report involvement in different types of crimes, had fewer days of cocaine use in the 12 months prior to the interview, and had lower scores on the drug addiction severity scale relative to controls. No significant differences were found on these outcomes for

[4] It should be noted that this term pertains to prognostic risk and not necessarily dangerousness risk. In fact, there is evidence that offenders with violent histories performed as well, or better, than nonviolent offenders in drug courts (Carey et al., 2008; Saum & Hiller, 2008; Saum, Scarpitti, & Robbins, 2001).

District Court cases (Gottfredson et al., 2005). These findings suggest a few possibilities: there may be fundamental differences in the operation of the two courts reflecting heterogeneity in the treatment; the two courts may contain populations with differing risk levels and the outcomes reflect heterogeneity in response to treatment; or the differences in outcomes are explained by some combination of court and client characteristics.

Findings from the original BCDTC study provide some analysis of court and client characteristics by originating court. Implementation data (presented in more detail in the Methods section) show that both the District and Circuit Court drug court clients attended a significantly greater number of status hearings than control clients. However, the difference was larger among Circuit Court cases, with Circuit drug court cases attending twice as many hearings on average as District drug court cases. Additionally, District drug court participants wound up spending more than twice as many days incarcerated as a result of the initial arrest compared to their control counterparts and almost as many days incarcerated as the Circuit drug court cases. Additional analyses found that these incarceration stays were largely driven by noncompliance in drug court (Gottfredson et al., 2006).

One could expect that the differences in jail time may have a negative effect on outcomes, such that any positive effects observed would be less pronounced or perhaps even negatively impacted for those who performed poorly and as a result received more jail time than their traditional adjudication counterparts. Severe incarcerative sanctions have the potential to decrease perceptions of procedural justice, attenuate bonds to prosocial institutions and others, and increase association with criminal peers.

Analysis of participant characteristics by originating court produced no significant differences on age, gender, prior arrest and prior conviction measures (Gottfredson & Exum, 2002). However, measures of client characteristics were fairly limited in scope, and as such, it remains possible that the two groups varied in terms of prognostic risk level. Originating court does in some sense serve as a proxy for lower risk in the sense that the District Court cases are for misdemeanor offenses while Circuit Court cases relate to felony charges.

The Current Study

To the author's knowledge, the current research represents the first 15-year follow-up of an established drug court that examines recidivism, incarceration time, and mortality outcomes. The use of a randomized controlled trial reduces the possibility of bias and strengthens the study's overall findings and impact. This evaluation will further provide evidence regarding the utility of drug courts for offenders with significant criminal history records and chronic drug abusing histories and will investigate heterogeneity of treatment effects.

State governments are re-examining their mandatory minimum laws under increasing pressure to reduce prison populations. Federal correctional policy is similarly being revised to improve outcomes and reduce reliance on approaches shown to be costly or ineffective. In this climate, it is vital to obtain comprehensive understanding of the effectiveness of policy alternatives like drug courts to improve outcomes for drug offenders. The current work will contribute to the growing body of literature examining the effectiveness of drug courts by comparing long-term criminal justice and mortality outcomes among a sample of chronic drug- and criminally-involved individuals randomly

assigned to drug court or traditional adjudication.

CHAPTER 3: DATA AND METHODS

Design

This research builds upon previous work summarized above and retains the original experimental research design: a non-blinded, parallel trial with unequal allocation. Beginning in February of 1997, eligible drug-involved offenders were randomly assigned to the drug court or to treatment as usual in the Baltimore District and Circuit Courts. Randomly generated treatment allocations were prepared by the research team and then placed within sealed and numbered opaque envelopes. When court personnel identified an eligible and interested defendant, their name was then sent via secure fax to the research team. The research team then opened the next envelope in the numeric sequence and communicated the randomization results to court personnel just prior to the subject's appearance before the judge. The randomization results were given to the judges as a recommendation and were followed in nearly all cases because the court did not have the capacity to serve all eligible and interested drug offenders within the drug court. As such, randomization represented a fair and ethically defensible method of allocating services to eligible offenders, and the judges agreed to abide by the results of random assignment.

Randomization continued through August of 1998, at which time 235 clients had been assigned randomly to one of the two conditions. Study participants were randomly assigned at a ratio of one treatment to one control for Circuit Court cases and at a ratio of two treatment to one control for District Court cases. This was done at the request of the District Court judges who were concerned that all lower court drug court slots might not

be filled if the study kept with a one-to-one ratio. As shown in Figure 2, 91% of the 139 cases (n=126) randomly assigned to the treatment group were actually dealt with in the drug court. In comparison, approximately 93% of the 96 cases (n=89) randomly assigned to the control condition were actually dealt with via traditional adjudication.

Benefits of Random Assignment. Unlike many other methods, random assignment allows for strong internal validity (Campbell & Stanley, 1963; Shadish, Cook, & Campbell, 2002). Internal validity reflects the extent to which a causal conclusion based on a study is warranted. Researchers examining the impact of a program or policy are typically interested in determining the causal effect of said treatment, often referred to as the average treatment effect (ATE). This effect is ideally expressed using a counterfactual framework; that is, as the difference between an individual's value of the response variable when he or she is treated and that same individual's value of the response variable when he or she is not treated.

The difficulty comes in constructing the desired counterfactual because only one of the two potential outcomes is observed at any time (Holland, 1986; Heckman & Smith, 1995; Apel & Sweeten, 2010). This makes causal inference difficult to achieve because a number of plausible rival hypotheses might explain a correlation, e.g., individual characteristics such as criminal propensity or motivation for change. In fact, these potential confounds are one of the central impediments to coming to a strong conclusion about the nature and/or size of a treatment effect (Angrist & Pischke, 2009; Loughran & Mulvey, 2010).

When done properly, a randomized trial is the strongest research design that can be employed to eliminate competing explanations for a treatment effect (Sherman, 2010).

In large enough samples, random assignment produces similar distributions of the potential causes of any future behavior by members of the treatment and control groups. Therefore, the only average difference between the two groups should be the treatment effect independently manipulated via the experiment. Put differently, randomization generally achieves balance between groups on known and unknown confounding variables allowing the control group to be used as a valid counterfactual to the treatment group (Apel & Sweeten, 2010).

Expressed formally then the average treatment effect in a randomly assigned sample is simply the mean difference in the outcome for treated and untreated individuals in the target population:

$$ATE = E(Y_i - Y_0)$$

Where Y_i denotes some outcome with treatment and Y_0 denotes some outcome without treatment.

Additionally, because randomization implies that all individuals within the population are equally likely to be treated, the average treatment effect should be equivalent to another value of interest, the average treatment effect on the treated (ATT). The reason this is so is because initial pre-treatment characteristics should be randomly distributed between the groups such that treatment status is independent of the potential outcome:

$$ATT = E(Y_i - Y_0 \mid Z = 1)$$

Where Y_i denotes some outcome with treatment and Y_0 denotes some outcome without treatment and Z represents treatment. This equation highlights the counter-factual nature of a causal effect: it examines the impact of some treatment on a

population of interest (observable) against the impact on the same population of interest had they not been treated (unobservable but possible to estimate via a control group).

Limitations of Random Assignment. While the promise of random assignment is great, it is not without its own set of limitations. To start, many questions of interest to social scientists cannot be studied via an RCT because of practical, legal or ethical considerations. Additionally, when RCTs are conducted, external validity is often diminished, particularly if the population selected for study inclusion does not represent the broader population of interest or if the conditions of the experiment do not mimic those of real world practice (Shadish, et al. 2002). RCTs can be further threatened if participants fail to comply with their assigned treatment status, either by being in the treatment group but not receiving treatment (treatment dilution) or by being in the control group and receiving treatment (treatment migration). The effect of noncompliance often leads to an underestimation of the treatment effect (Angrist, 2006). Differential attrition from experimental and control groups can also produce noncomparable groups, leading to low internal validity. Finally, when RCTs do not blind the results of participants' randomization status from researchers, practitioners, and subjects, various forms of bias can be introduced, e.g. differential treatment, biased assessment of outcomes, etc. (Farrington, Loeber, & Welsh, 2010). These issues, and the extent to which they impact the current study, are addressed in more detail in the results and conclusion chapters.

Unique Benefits and Challenges of Longitudinal Experimental Studies. Farrington Ohlin, & Wilson (1986) first outlined the need for longitudinal experimental studies in criminology in their influential book *Understanding and Controlling Crime: Toward a New Research Strategy*. The authors argued that the following elements were needed:

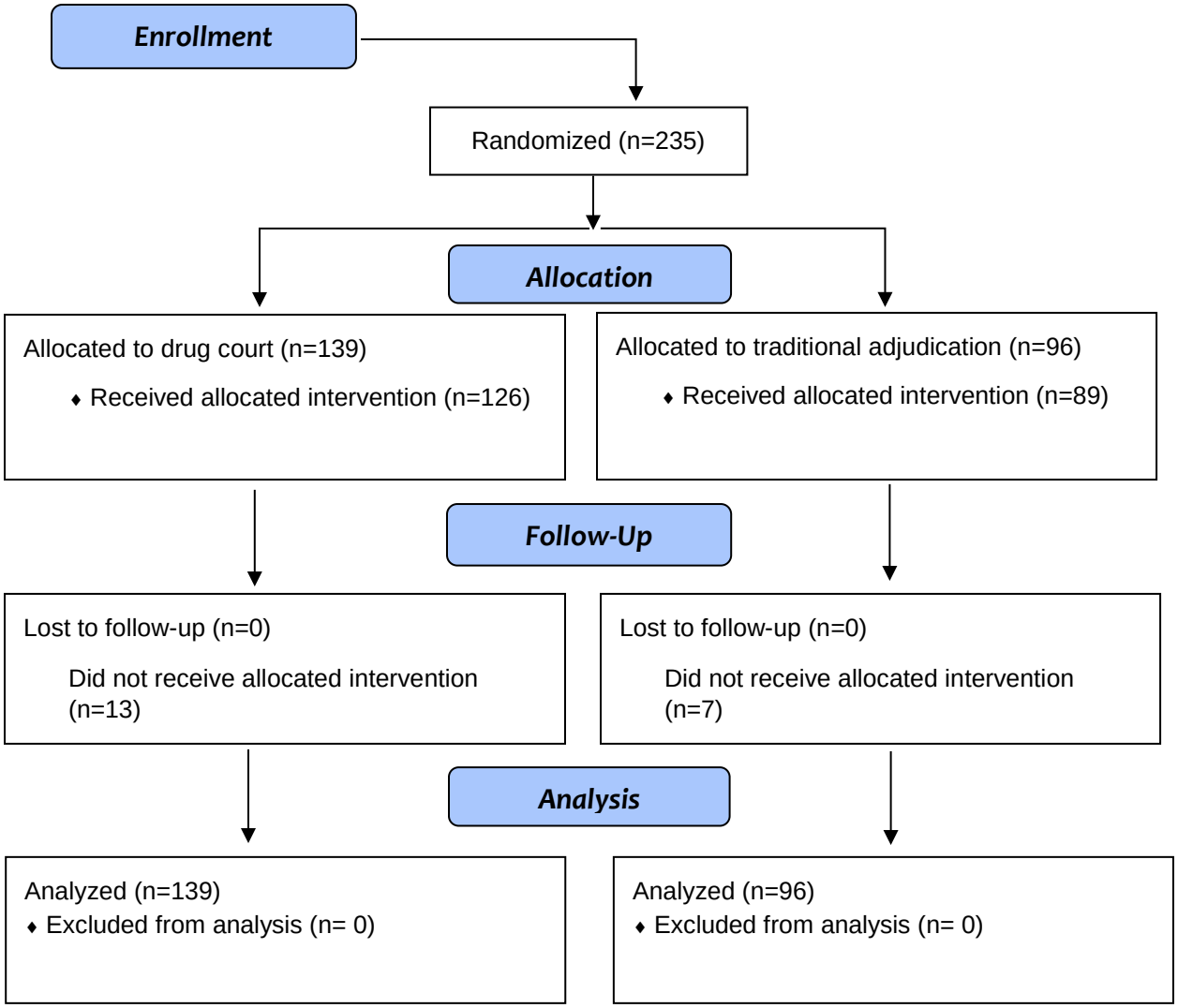
1) several data collections, covering several years; 2) the experimental intervention; and 3) several more data collections, covering several years, afterwards. To date, very few studies examining offending have met these conditions, with no known studies using interview data and only a few carried out using official records data (See McCord, 1978; Tremblay, Mâsse, Pagani, & Vitaro, 1996; Schweinhart et al., 2005; & Olds et al., 1998 for some examples).

Examining the long-term effects of treatment using a randomized design has a number of important benefits. First, long term follow-up of study subjects may reveal treatment effects that were not apparent in the short term. Additionally, this strategy makes it possible to compare short-term and long-term effects and the factors linking them (Farrington et al., 2010). A simple pretest-posttest design is not able to distinguish between several time-dependent effects (Farrington & Welsh, 2006). For example, it was only after long-term follow-up of the Perry Preschool Project that an important feature of the project was discovered. While the intelligence of the experimental group was found to be higher at age 4–5, it was no different from the control group by age 8–9, leading to an argument that such programs were ineffective. However, by the age of 27, the experimental group had half the number of arrests (2.3 vs 4.6 respectively), had significantly higher income, and was more likely to be homeowners than their control group counterparts (Schweinhart et al., 2005).

With regard to the challenges of longitudinal experimental design, the simple passage of time may present obstacles. For example, the treatment may no longer be of interest to policy makers or the public due to changes in theory, public sentiment, methodology, or policy or because of some other practical constraint. Attrition will also

likely increase over time and differential attrition represents one of the most serious threats to the original design and corresponding assumptions (Farrington et al., 2010).

Figure 2. Flowchart of Participants' Progress Through Each Phase of the Trial[5]



[5] Note that Figure 2 represents a modified version of the CONSORT chart. Data on the number of individuals considered for eligibility and enrollment were not collected. Additionally, the number who discontinued the two interventions is not clear, particularly for the traditional adjudication group. While the vast majority of drug court participants received drug court services, the actual graduation rate was 38% at the time of the 3-year follow-up. Data on hearings, probation supervision, and service implementation stemming from the initial arrest are provided for both experimental conditions.

Study Participants

The BCDTC is a voluntary program for District and Circuit Court cases supervised by the Baltimore City Division of Probation and Parole and was designed to serve non-violent drug offenders. To meet initial eligibility requirements for program participation, an offender must: (1) reside in Baltimore City; (2) have no current or previous arrests for violent offenses; and (3) be at least eighteen years of age. Eligible defendants who express an interest in the program meet with the Public Defender to discuss their possible participation. If after this meeting the defendant remains interested in the drug court program, record checks are completed and reviewed by the State's Attorney. The State's Attorney then meets with the Public Defender to determine which defendants would be best served by the program. For this subset of defendants, personnel in the Drug Court Assessment Unit then administer the Psychopathy Checklist (Hare, Harpur, Hakstian, & Forth, 1990) to evaluate the offenders' suitability for the program, and the Addiction Severity Index (McLellan et al., 1992) to assess their motivation and need for treatment. Additional data regarding drug history, medical history, employment status, and aspects of the defendants' families and social relations are also collected. Upon completion of these assessments, the assessor recommends a defendant for the program, or not.

Eligibility requirements for study participation were the same as those described above; thus, the study population is representative of the typical offender processed through the BCDTC (at that time). Study participants include the 235 offenders who met the initial eligibility requirements and were assigned randomly to receive either BCDTC services (N= 139) or treatment as usual in the traditional court (N= 96). Of the 139 cases

assigned to the treatment condition, 84 were handled in the District Court and 55 were handled in the Circuit Court. Of the 96 cases assigned to the control condition, 42 were handled in the District Court and 54 were handled in the Circuit Court. Study participants are all adults, with an average age of 35 at random assignment. Because participants were randomly assigned to treatment and control conditions, group assignment should be uncorrelated with unmeasured "person effects" and other sources of bias that might contaminate drug court effects.

Table 1 reports data on study participant characteristics by treatment condition at baseline. Comparisons between the two randomly assigned study groups produced no statistically significant differences on prior offending or demographics, indicating that the randomization procedure produced similar groups. Approximately 89% of the total sample is African American and 74% are male. Study subjects have considerable criminal histories: at the time of randomization into the study, drug court subjects averaged 12 prior arrests, while control subjects averaged 11.3 prior arrests. Both groups averaged approximately five prior convictions. The majority of the sample entered the study as the result of a drug crime (71.5%), followed by property and public order crimes (19.9% and 7.6% respectively). Less than three percent of the sample entered the study due to a violent crime.

Study subjects also had considerable substance use histories. While the data are incomplete[6] and therefore cannot be compared across experimental condition, nearly all (94.3%) of the sample for whom data are available self-reported heroin or cocaine as their primary drug of choice. More than half of the sample for which data are available

[6] Substance use history data were collected from probation intake and treatment intake forms. These data were available for 92 treatment and 30 control cases.

(66.1%) reported using crack, cocaine, or heroin three or more times per week. Less than two percent of this same sample reported marijuana as their primary drug of choice. These data make clear that unlike many drug courts that serve low risk offenders whose drug use is not severe, the BCDTC serves a more serious criminal- and drug-involved population.

Table 1. Participant Characteristics at Baseline by Experimental Status

	Experimental Status	
Participant Characteristic	*Treatment (n=139)*	*Control (n=96)*
Male	74.1	74.0
Black	89.2	89.6
Age		
M	34.8	34.7
SD	7.5	7.9
Prior Arrests		
M	12.0	11.3
SD	8.8	7.1
Prior Convictions		
M	5.3	4.6
SD	4.3	3.4
Current offense: percentage of Participants with at least one		
Violent or sex	1.4	4.2
Property	21.0	18.8
Drug	71.0	71.9
Prostitution or solicitation	5.8	4.2
Violation of probation	2.2	4.2
Weapons	.7	3.1
Public order	10.9	4.2

Note: No differences between treatment and control groups were statistically significant at $p < .05$.

Description of the Intervention[7]

The BCDTC is comprised of four main elements: intensive probation supervision, drug testing, drug treatment, and judicial monitoring. Intensive probation supervision includes three face-to-face probation contacts per month, two home visits, and verification of employment status. Probation officers also frequently review their clients' criminal records for violations. After a sustained period of compliance, defendants' level of supervision is downgraded from "intensive" to "standard high."

Drug testing is performed in a series of phases of decreasing intensity similar to probation supervision. Phase I, which lasts approximately 3 months, requires defendants to submit two urine samples per week. Phase II, also 3 months in length, requires one sample per week. Phase III, lasting a period of 6 months, requires one sample per month. After that time, drug testing is completed randomly over the defendants' remaining time in the drug court.

Drug treatment is provided by one of eight providers located throughout Baltimore. These programs vary in terms of their treatment components and include three intensive outpatient centers, two methadone maintenance clinics, two residential treatment facilities, and one transitional housing complex. In addition to drug treatment, each program offers educational opportunities, job training, and life-skills training, and housing assistance. Drug court participants are assigned to the program that best suits their treatment needs.

Judicial monitoring takes place in the form of frequent status hearings. At

[7] This section describes the BCDTC as implemented in 1997 and 1998.

these hearings, the judge reviews reports from treatment and probation personnel to assess a participant's program compliance. Failure to comply with program requirements can result in a variety of sanctions including increased status hearings, increased probation supervision, increased drug testing, and curfews. The sanctions graduate to more severe measures such as home detention, temporary incarceration, and community service. In response to extreme noncompliance, the judge can reimpose the original sentence, which is often more severe than what might have been imposed under traditional adjudication.

Implementation of the Intervention[8]

As shown in Table 2, participants in the treatment condition were significantly more likely than controls to receive drug testing and to have attended status hearings.[9] Specifically, 86.9% of drug court subjects were tested for drugs, as compared to only 40.2% of control subjects, and 84.2% of drug court subjects attended at least one status hearing, as compared to only 7.3% of control subjects. Drug court participants attended a significantly greater number of status hearings than control counterparts, and there was a significant experimental condition by court interaction for the number of status hearings attended. In both the District and Circuit Court, drug court clients attended a significantly greater number of status hearings than control clients. However, the difference was larger among Circuit Court cases (a difference of 14.6 hearings) as

[8] The tables and descriptions in this section are reproduced with minor revision from Gottfredson, D. Najaka, S., Kearley, B. and Rocha, C. (2006). Long-term effects of participation in the Baltimore City drug treatment court: Results from an experimental study. *Journal of Experimental Criminology*, 2: 67-98.

[9] Implementation findings were collected at the three year follow-up, at which time 94% of the sample had completed all services related to the initial arrest.

compared to cases handled in the District Court (a difference of 6.5 hearings), and Circuit drug court cases on average attended twice as many hearings as District drug court cases (15.0 vs. 7.4). Treatment and control subjects were about equally likely to have received probation supervision during the study period, and the two groups had a similar number of days supervised.

Table 2. Characteristics of Supervision

	Experimental Status			
	Treatment		*Control*	
Percentage Drug Tested	86.9**	(130)	40.2	(92)
Percentage with at least one hearing attended	84.2**	(139)	7.3	(96)
Number of hearings attended				
All subjects				
M	10.4**+	(139)	0.6	(96)
SD	8.9		2.4	
Percentage supervised	83.3	(138)	85.4	(96)
Number of days supervised				
All subjects				
M	445.5	(138)	479.4	(96)
SD	326.5		337.5	
Supervised subjects				
M	534.6	(115)	561.3	(82)
SD	282.9		295.1	

Note: Numbers in parentheses are the number of cases for which valid data are available.
Significance tests for the treatment/control comparisons are based on logistic regression for dichotomous variables and OLS regression for continuous variables.
**$p < .01$.
+Interaction of experimental condition and court is significant, $p < .01$.

Table 3 summarizes the level of drug treatment services received by the study groups during the three-year follow-up period. It should be noted that the BCDTC program used a jail based acupuncture program as a sanction for relapsing subjects (Gottfredson et al., 2003). Although considered a treatment, this program is not

recognized as a certified drug treatment program by BSAS, the organization that coordinates drug treatment services in Baltimore. As such, the program is included separately from the other certified drug treatments in the table. During the three years following entry into the study, 71.2% of the drug court group received some form of drug treatment, as compared with 27.1% of the control group. These differences remain significant when only certified drug treatment is considered (53.2% and 22.9% respectively).

Table 3. Drug Treatment Experiences

	Experimental Status			
	Treatment		*Control*	
Percentage receiving				
Any treatment	71.2**	(139)	27.1	(96)
Certified drug treatment	53.2**	(139)	22.9	(96)
Methadone maintenance	7.2	(139)	9.4	(96)
Outpatient	30.2**	(139)	8.3	(96)
Residential	10.1	(139)	7.3	(96)
Correctional	2.2	(139)	0.0	(96)
Detoxification	2.2	(139)	0.0	(96)
Intensive outpatient	20.9**	(139)	5.2	(96)
Other treatment	2.2	(139)	0.0	(96)
Jail-based acupuncture	48.9**	(139)	8.3	(96)
Duration of treatment (any treatment)				
All subjects				
Mean	139.8**	(139)	48.4	(96)
SD	187.0		131.6	
Treated subjects				
Mean	196.3	(99)	178.7	(26)
SD	195.0		204.0	
Duration of treatment (certified treatment)				
All subjects				
Mean	119.9**	(139)	45.2	(96)
SD	184.5		130.5	
Treated subjects				
Mean	225.3	(74)	197.3	(22)
SD	200.8		213.6	

Note: Numbers in parentheses are the number of cases for which valid data are available.
Significance tests for the treatment/control comparisons are based on logistic regression for dichotomous variables and OLS regression for continuous variables.
** $p < .01$.

Because the BCDTC program was designed as an alternative to incarceration, it is also of interest to examine the days incarcerated as a result of the arrest that led to study involvement. Table 4 shows the actual days incarcerated as a result of the initial arrest. The estimates of days incarcerated include all periods of incarceration in jail and prison occurring during the three-year follow-up period that were associated with the initial arrest. They capture time served as a direct result of the initial arrest (both pre- and post-disposition), as well as time served due to probation violations, where the term of probation was associated with the initial arrest. For drug court clients, the estimates also include temporary incarceration periods resulting from failure to comply with the requirements of the drug court (e.g., failure to appear for status hearings). The results are presented separately for each court, as experimental condition was found to interact with originating court for two of the measures included on the table. Overall, similar percentages of drug court and control subjects were incarcerated as a result of the initial arrest (89.2% vs. 83.3%). However, drug court subjects were incarcerated significantly more often during the follow-up than controls, averaging 2.3 episodes of incarceration. Despite this difference in the number of times incarcerated, the number of days incarcerated was not significantly different for drug court and control subjects (158.9 vs. 156.9 days). There was, however, a large interaction of experimental condition and court. After three years, the District drug court cases wound up spending more than twice as many days incarcerated as their control counterparts and almost as many days incarcerated as the Circuit drug court cases. This result is surprising given previously reported findings that in the Circuit Court, sentences were longer, and the treatment/control difference in the sentence to be served was larger, with control cases

expected to spend more days incarcerated than drug court cases (Gottfredson & Exum 2002). Table 4 shows that when the number of incarceration days are separated out by reason, it appears that the difference between the courts in the use of incarceration is in large part explained by the use of incarceration in response to noncompliance. There is a large difference between the drug court and control cases in the use of incarceration for noncompliance in the District Court (a difference of 70.6 days) but not the Circuit Court (a difference of only 7.0 days). In short, incarceration was used a lot as a response in the District Court, resulting in more bed space used by drug court offenders than by those who were processed as usual. This finding was central to the decision to explore the potentially heterogeneous effects of drug court participation by originating court.

Table 4. Days Incarcerated as a Result of the Initial Arrest, by Originating Court

	District				Circuit				Total			
	Treatment		*Control*		*Treatment*		*Control*		*Treatment*		*Control*	
Percentage incarcerated	91.7	(84)	90.5	(42)	85.5	(55)	77.8	(54)	89.2	(139)	83.3	(96)
Incarceration episodes												
Mean	2.3**	(84)	1.4	(42)	2.2	(55)	1.6	(54)	2.3**	(139)	1.5	(96)
SD	1.5		0.9		1.6		1.4		1.6		1.2	
Days incarcerated												
Mean	151.0**	(84)	69.0	(42)	171.1	(55)	225.3	(54)	158.9+	(139)	156.9	(96)
SD	194.0		96.9		247.3		267.8		216.0		223.9	
Days incarcerated as a result of:												
Predisposition commitment												
Mean	12.5*	(84)	21.2	(42)	19.2**	(55)	51.1	(54)	15.1**	(139)	38.0	(96)
SD	12.9		23.0		56.8		66.7		37.0		54.2	
Assigned sentence												
Mean	5.0	(84)	8.5	(42)	15.9*	(55)	78.3	(54)	9.3**+	(139)	47.8	(96)
SD	31.9		32.4		89.6		205.9		61.5		159.1	
Response to non-compliance												
Mean	94.3**	(84)	23.7	(42)	85.3	(55)	78.3	(54)	90.7	(139)	54.4	(96)
SD	153.4		79.1		175.1		167.9		161.8		138.5	
Reason unknown												
Mean	39.2	(84)	15.5	(42)	50.8	(55)	17.6	(54)	43.8*	(139)	16.7	(96)
SD	119.1		54.7		124.9		55.3		121.1		54.7	

Numbers in parentheses are the number of cases for which valid data are available.

*$p < .05$; **$p < .01$.

+Interaction of experimental condition and court is significant, $p < .05$.

Data

Sources of Official Records

Previously Obtained Administrative Data. All administrative data from the initial study came from official records of the Maryland Department of Public Safety and Correctional Services and the Baltimore Substance Abuse Systems, Inc. (BSAS), an organization that coordinates drug treatment services in Baltimore. Data were collected on demographic characteristics and prior offense history, as well as recidivism (arrests and convictions), drug treatment, drug testing, probation supervision, judicial monitoring, and time spent incarcerated in jail and prison through three years following randomization. Select variables from this data will be used in this dissertation as control variables and in the consideration of the moderating effect of court of assignment.

15-Year Follow-up Data. This dissertation supplements the previously obtained administrative data with an additional 12 years of recidivism data, as well as days spent incarcerated in jail and prison, for a total of 15 years of follow-up data from official records of the Maryland Department of Public Safety and Correctional Services. Mortality data from the Maryland Vital Statistics Administration and from U.S. Social Security Death Index was also obtained for the 15-year period and includes date of death and cause of death information.

Recidivism and incarceration data collection follows the protocol laid out in the initial study of the BCDTC and is limited to data from Maryland's Department of Public Safety and Correctional Services. While it is possible that study participants may have committed crimes in other states or crimes that were pursued by federal authorities, there

is no reason to believe that any potential omissions would be systematically different for treatment versus control group participants. That is, while the data collected may underestimate the magnitude of offending among study participants, this potential should be equally distributed across study conditions.

Additionally, federal prison admissions represent a small fraction (approximately 13%) of Maryland offenders prosecuted. For example, in 2013, Maryland's state jails and prisons held 21,335 offenders, while only 3,198 offenders from Maryland's judicial District were under the jurisdiction of the Federal Bureau of Prisons (Bureau of Justice Statistics, 2013). Approximately 47% of those federal inmates were charged with a drug offense as their most serious commitment. However, the vast majority of drug offenders in federal prisons are serving sentences for drug trafficking (approximately 99.5%), while the vast majority of BCDTC participants were serving sentences for drug possession, property, and other lesser crimes (Taxy, Samuels, & Adams, 2015).

Anecdotal accounts from court personnel suggest that study participants had limited mobility, with the majority of participants living below the poverty line and residing and/or returning to the same neighborhoods throughout their period of supervision. Additionally, while tracking participants for follow-up interviews, 100% of participants interviewed (66.8% of the total sample) were found to be living in the State of Maryland (either in residences or local jails and prisons) at the time of their interview 3 years post randomization. These findings did not differ by treatment condition, which provides further support for the assertion that out of state movement/offenses are not likely to be related to treatment status. Similarly, mortality data collected via the U.S

Social Security Death Index indicated that all but one of the deceased were located in the State of Maryland at the time of death.

Measures

Outcomes of Interest

Arrest. Total arrests are measured in two ways: 1) a count of the total number of arrests and charges cumulatively across the 15-year period; and for the purposes of the longitudinal analysis, 2) a count of the total number of arrests for each year of the follow-up. To examine potential differences by crime type, four additional variables were constructed that examine the number of person, property, drug, and VOP charges across the 15-year follow-up period. These additional measures are examined in the cumulative analyses.

Conviction. Similar to the arrest measure, total convictions are measured in two ways: 1) a count of the total number of arrest and charge convictions across the 15-year period; and for the purposes of longitudinal analysis, 2) a count of the total number of convictions for each year of the follow-up. To examine potential differences by crime type, four additional variables were constructed that examine the number of person, property, drug, and VOP charge convictions across the 15-year follow-up period. These additional measures will be examined in the cumulative analyses only.

Exposure. A measure of exposure time is included in the recidivism analyses to account for the amount of time that each participant spent free in the community. This measure of exposure time is the total number of days that each participant was in his or her community during the 15-year follow-up period.

Incarceration. Incarceration is measured as a count of the total number of days sentenced to incarceration in Maryland prisons and the Baltimore City jail for new charges and VOPs across the 15-year period. Due to the quality of the data, two variables were created, one in which undetermined charge sentences were treated as being served concurrently and one in which undetermined charge sentences were treated as being served consecutively.

Mortality. Mortality is measured as a dichotomous variable (1 = deceased, 0 = alive) and reflects all study participant deaths occurring and reported to the Maryland Vital Statistics Administration or the U.S. Social Security Death Index across the 15-year follow-up period.

Note that for all outcomes, the follow-up period is measured in years from the individual's date of randomization into the study.

Independent Variable

This dissertation seeks to examine the long-term effects of participation in the Baltimore City Drug Treatment Court. As such, one independent variable reporting the results of randomization is used. Participation in the BCDTC is measured with a dichotomous variable (1 = BCDTC/treatment, 0 = traditional adjudication/control).

Control Variables

Four demographic control variables are included: Black/African American (1 = yes, 0 = other), Male (1 = yes, 0 = no), age at randomization, and the number of prior

convictions indicated in official records.[10]. Consistent with the analysis of data from earlier time points, models are run with and without weighted data that corresponds to subjects probability of assignment to the treatment and control groups in order to take account of the different selection ratios used in the two courts. All subjects originating in the Circuit Court were given a weight of 1, as these cases were randomly assigned to the drug court and control conditions using a one-to-one ratio. In comparison, District Court cases were randomly assigned using a two-to-one ratio, resulting in a drug court sample twice the size of the control sample. Individuals in the control sample will be given twice as much weight in the weighted analyses, i.e., control subjects weight =1.5 and drug court subjects weight = .75. These weight values were used (as opposed to 2 and 1) because they produced a weighted sample size equal to the unweighted sample size (N = 235) while creating roughly equal numbers in the drug court (N = 118) and control (N = 117) samples.

Moderator Variable

Prior analyses from the original BCDTC evaluation showed that program outcomes varied by originating court, with Circuit BCDTC cases fairing better relative to controls than District Court participants, and that the experiences of subjects differed considerably by court. These findings suggest heterogeneity of treatment effect: the source of which might be either at the participant/risk level, at the program/drug court implementation level, or some combination of the two. Measures of client characteristics at intake were limited and, of those available, no significant differences in addiction

[10] These variables did not differ between treatment and control but are included as controls to increase statistical power in the analyses.

severity or offending were observed. Differences in drug court implementation are also difficult to ascertain as it is unclear whether variability in court actions were due to implementation differences or in response to differences in participant compliance. As such, a dichotomous variable indicating originating court (1 = Circuit Court, 0 = District Court) is used to explore, albeit in a limited way, the extent of treatment effect heterogeneity. As discussed earlier, these differences in drug court experiences and outcomes provide a rationale for expecting that the life course trajectories post treatment might also differ according to originating court. As such, several research questions posed in this long-term follow-up study examine the relationship between BCDTC and subsequent offending stratified by originating court.

Table 5 presents summary statistics for all the variables included in the models.

Table 5. Summary Statistics of all Variables Included in the Models

Variables	Measurement	Mean	SDs	Minimum	Maximum
Dependent variables					
Total Arrests	Count	7.02	8.01	0.000	72.0
Total Charges	Count	14.9	15.8	0.000	125.0
Total Drug Charges	Count	6.80	7.95	0.000	42.0
Total Property Charges	Count	4.22	8.01	0.000	57.0
Total Person Charges	Count	2.24	5.33	0.000	50.0
Total VOP Charges	Count	0.68	1.50	0.000	12.0
Total Unique Arrest Convictions	Count	3.59	4.56	0.000	40.0
Total Charge Convictions	Count	4.79	6.24	0.000	56.0
Total Drug Charge Convictions	Count	2.31	2.74	0.000	12.0
Total Property Charge Convictions	Count	1.26	3.34	0.000	26.0
Total Person Charge Convictions	Count	0.37	1.79	0.000	24.0
Total VOP Charge Convictions	Count	0.52	1.14	0.000	7.00
Days Incarceration, Undetermined Sentences Treated as Concurrent	Count	1,182	2,099	0.000	15,535
Days Incarceration, Undetermined Sentences Treated as Consecutive	Count	1,205	2,110	0.000	15,535
Died during the 15-Year Follow-up	0/1	0.21	0.41	0.000	1.00

Table 5. Summary Statistics of all Variables Included in the Models (cont.)

Variables	Measurement	Mean	SDs	Minimum	Maximum
Control Variables					
Black/African American	0/1	0.89	0.31	0.000	1.00
Male	0/1	0.74	0.44	0.000	1.00
Age	Continuous	34.8	7.62	18.0	59.0
Prior Convictions	Count	4.90	3.97	0.000	24.0
Moderator Variable					
Originating Court – Circuit	0/1	0.46	0.50	0.000	1.00
Exposure Variable					
Days Free During the 15-Year Follow-up	Count	4,500	1,449	125	5,475

Note: N = 235 for all variables

Analytic Strategy

Intent to Treat Analysis

In all analyses of the BCDTC intervention, study participants are treated as randomized. That is, participants randomly assigned to the drug court and control conditions will be analyzed as members of those groups regardless of their actual treatment, e.g., withdrawal from treatment or deviation from the treatment protocol. The most recent CONSORT Statement supports this "intent-to-treat" analysis as the preferred method because it preserves the baseline comparability of the study groups (Schulz, Altman, & Moher, 2010). This is a critical point, as the strength of an RCT rests on the removal of bias via random allocation of interventions to trial participants. Alternative

strategies such as “as treated” analysis, which analyze participants according to the treatment received rather than the treatment assigned, and “per protocol” analysis, which include only individuals who adhered exactly to clinical trial instructions, are subject to the same bias and potential confounds as observational studies.

In ITT analysis, the estimate of the treatment effect is generally more conservative due to the possibility of treatment dilution and migration, and, therefore, this strategy produces results that are more susceptible to Type II error (“false negatives”). However, ITT analysis produces a better estimate of effectiveness compared to analyses that use only treatment completers since those strategies may overestimate the effectiveness of a program by ignoring untreated participants and dropouts who, in the case of criminal justice interventions, tend to have the highest recidivism rates (Seager, Jellicoe, & Dhaliwal, 2004). Additionally, ITT analysis preserves study sample size, whereas other methods that exclude certain individuals decrease the sample size and corresponding statistical power. Finally, ITT analysis provides a more practical and reliable estimate of true treatment effectiveness because it acknowledges and captures the reality of treatment noncompliance, which is commonplace in real world settings (Gupta, 2011; Newell, 1992). While alternative strategies that attempt to isolate the effect of treatment on the treated provide estimates of what an intervention might achieve if fully implemented, in reality, most treatments cannot ever achieve this aim. This is a particularly salient point when considering interventions designed for drug offenders as they tend to have high rates of recidivism and program noncompliance (Langan & Levin, 2002). Additionally, by building the attrition into the estimate, ITT analysis allows for consideration of potentially positive and negative aspects of an intervention (Loughran &

Mulvey, 2010). As such, ITT analysis provides the most policy relevant approach because it considers an intervention's effectiveness under observed conditions. It is also worth noting that over 90% of study participants (91% of treatment and 93% of control cases) received services as randomized, so the results of a TOT analysis would be fairly consistent with the ITT results.

Power analysis

Prior to the start of analyses, a post-hoc power analysis was conducted to evaluate the statistical power of the current study (Rosner, 2011). Power estimates were calculated assuming two independent study groups. The means and standard deviations for treatment and control participants on the cumulative number of arrests across the 15-year period were used for the power calculation, assuming a *p* value of .05. Post hoc power was determined to be .72, slightly below the standard convention of .80.

Statistical models

Survival analysis is employed to compare mortality outcomes across experimental condition. Statistical corrections are considered for the remaining models if differences in the survival curves by treatment condition emerge in order to address differential attrition and any potential bias that may be introduced as a result.

Regression models test for group differences on each of the recidivism outcome variables both at the 15-year endpoint and in the parameters describing growth patterns over the 15-year period. Additional models assess whether or not originating court moderates the treatment effect. In all outcome analyses, two-tailed tests of statistical significance are employed. However, because the study is somewhat underpowered and

because, for many of the outcomes, earlier analyses of the BCDTC indicated that drug court participants fared significantly better on arrest and conviction outcomes (suggesting that a one-tailed test would be appropriate), differences observed at an alpha level of $p <$ .10 are considered statistically significant. Regression models appropriate for each dependent variable are used and models are run with and without consideration of exposure time, e.g., time free in the community.

In addition, because the randomization procedure resulted in a disproportionate number of drug court participants originating in the District Court, the data are analyzed once using unweighted data, giving all sample members equal weight regardless of whether they originated in the District Court or the Circuit Court, and once using weighted data according to originating court. Any differences in outcomes are noted within the tabled data.

The following sections restate the research hypotheses and provide corresponding detail regarding the analytic strategy for each hypothesis. As Hypotheses 1 and 2 are answered using the same analytic strategy, they are combined for discussion purposes.

H1: ***Individuals who participated in the Baltimore City Drug Treatment Court will have lower cumulative rates of recidivism (including both arrests and convictions) over the 15 years following randomization than individuals who received traditional adjudication*****.**

H1a. ***These effects will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having significantly lower cumulative rates of recidivism than District drug court participants.***

H2: ***Individuals who participated in the Baltimore City Drug Treatment Court will have fewer days of incarceration over the 15-year follow-up than individuals who received traditional adjudication.***

H2a. ***These effects will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having fewer days of incarceration than District drug court participants.***

Poisson regression was originally considered for the arrest, conviction, and incarceration outcome variables. This analytic strategy was selected because the Poisson distribution, rather than the normal distribution, is especially suited for count or rate data where many of the values are zero and/or when the distribution is positively skewed. Poisson regression uses maximum likelihood estimation (MLE), which selects the parameter estimates that are most likely to produce the observed data. Poisson does not assume that the error term is normally distributed, and therefore yields more statistical power than OLS (Cameron and Trivedi, 1998). Instead the Poisson distribution includes a family of distributions in which the mean and the variance are equal, so that as the number gets larger its distribution appears wider and more similar to the normal distribution.

However, since the data in this sample are overdispersed and the conditional variance exceeds the conditional mean, negative binomial regression models are used. Negative binomial regression can be considered a generalization of Poisson regression since it has the same mean structure as Poisson regression with an extra parameter,

sometimes referred to as the dispersion parameter, to model the overdispersion. Using negative binomial regression in instances of overdispersion allows for more efficiency and narrower confidence intervals as compared to those from Poisson regression (Cameron & Trivedi, 1998).

Negative binomial regression models are run to estimate effects of treatment on the cumulative number of arrests, convictions, and days of incarceration measured at the 15-year end-point. The model for each dependent variable includes a dichotomous variable measuring assignment to treatment condition and the above mentioned control variables. For H1a a dichotomous originating court variable and a treatment condition * originating court interaction term are also included.

H3. ***Individuals who participated in the Baltimore City Drug Treatment Court will have a greater rate of desistance as compared to individuals who received traditional adjudication and the differences between the two groups will grow over time.***

H3a. ***These effects will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having a significantly greater rate of desistance as compared to District drug court participants.***

Longitudinal data allow one to model the effect of an intervention on developmental trajectories rather than simply focusing on end point differences (Na, Loughran, & Paternoster, 2015). For this research question, negative binomial growth curve models are run to examine whether the hypothesized desistance patterns (both

arrest and conviction) are observed over the 15-year follow-up period and to test for differences in the rate of decline across experimental groups.

Individual growth is visualized through graphical analysis to determine the general shape of the growth curve, e.g., linear vs. non-linear, and the specific shape if non-linear (Carrig, Wirth, & Curran, 2004). This determines the specific shape factors used in the latent growth curve models. Multi-level growth models are then run (Singer and Willet, 2003). These models reveal, for example, if the BCDTC participants decreased their rate of offending at a faster pace than did the control group.

Examining recidivism outcomes longitudinally using growth curve modeling allows for the examination of whether the life event of drug treatment court represents a turning point for offenders. The fundamental question is whether the "assignment effect" of drug treatment court determines whether or not the drug court experience permanently alters the offending trajectory for this group. This is a conservative test of the effect because it examines the impact of program involvement, regardless of the level of participation.

The model for each dependent variable includes a measure of time across the 15 years, a dichotomous variable measuring assignment to treatment condition, and the above mentioned control variables. Models examining differences in the rate of growth by treatment condition include a time * treatment condition interaction term.

A number of studies have suggested that the drug treatment court model may be more effective for higher rather than lower risk individuals (Marlowe et al., 2003, 2004, 2007; Fetsinger et al., 2002). This is compatible with life course research, which suggests

that turning point events can have different effects on different kinds of individuals depending on a number of factors including prior offending.

For H3a, a dichotomous originating court variable and a treatment condition * originating court interaction term are also included. While it is not possible to disentangle the degree to which originating court may represent a proxy for offender risk versus other explanations such as implementation differences, the analysis will still provide an examination of the extent to which treatment effect heterogeneity is observed in the BCDTC sample.

H4: ***Individuals who participated in the Baltimore City Drug Treatment Court will have lower mortality rates over the 15 years following randomization than individuals who received traditional adjudication.***

Survival analysis is used to examine whether or not the probability of surviving to the end of the 15-year follow-up differs significantly by treatment condition. Survival analysis allows for examination of the number of participants who experience an event – in this dissertation the event of death – but also allows for examination of the times at which the events occur. This strategy also accounts for right censoring. In the current study, right censoring is present because not all cases have an observed outcome of death by the end of the 15-year follow-up. Survival analysis allows for the inclusion of cases that did and did not experience the event of interest by using maximum likelihood or partial likelihood methods in a way that produces consistent estimates of the parameter of interest.

Two survival analysis techniques are used to examine time to death: Kaplan Meier and Cox regression. The Kaplan Meier method is used to produce survival curves and is a common first step in analysis of this kind as the curves can be graphed to visually inspect the data. The survivor function in this method is the probability of surviving to time t. The survival curves are then compared across group using the log rank test, which employs something similar to a chi-square test statistic. The log rank test compares the survival experience between two or more independent groups to test the null hypothesis of no difference in survival outcomes.

Because the Kaplan Meier method does not allow for the inclusion of covariates, Cox regression, sometimes referred to as proportional hazards regression, is run as a second step. Cox regression reports the counter of the Kaplan Meier, in this instance the probability of dying by time t. While the expectation is that the analysis will produce results consistent with the Kaplan Meier method, Cox regression allows for the inclusion of additional independent variables and is consistent with other study analyses which include controls for gender, race, age at randomization, and number of prior convictions.

If statistically significant differences in survival curves by treatment condition emerge, statistical corrections will be considered for the remaining models to address differential attrition due to mortality and any potential bias that may be introduced as a result.

Summary

The methodological strategies proposed in this dissertation are designed to answer questions regarding the long-term impact of involvement in the BCDTC; that is, whether

or not involvement in the drug court reduces offending behavior, thus representing a structurally-induced turning point for treated offenders. Cumulative recidivism and incarceration outcomes are examined for a sample of chronic drug offenders randomized to receive either drug court or traditional adjudication. Growth in arrest and conviction patterns among the treatment and control groups are also assessed over the 15-year period to determine whether treatment effects are stable, decaying, or increasing over time. If the effect of drug court on offending is statistically distinguishable from zero, and that effect persists over time, then there is support for BCDTC involvement as a turning point.

While the drug court sample had lower rates of recidivism at the three-year follow up, there was significant variability by originating court. During interviews 3 years post-randomization, District BCDTC cases were more likely to self-report involvement in different types of crimes, more days of cocaine use in the 12 months prior to the interview, and higher scores on the drug addiction severity scale relative to controls than their Circuit Court counterparts. Additionally, they spent more time incarcerated as a result of the initial arrest, most often due to noncompliance. As such, it will be important to assess the growth curves of these groups to determine whether originating court had a long-term moderating effect on recidivism trajectories.

Finally, mortality outcomes are assessed to examine any differences in the total number of deaths and time to death by experimental condition. While reducing mortality among drug offenders is not an express focus of drug courts, program supported reductions in drug use have the potential to reduce both drug-related overdose and risk of exposure to life threatening infectious diseases such as HIV and hepatitis. No moderator hypothesis is proposed for this outcome.

CHAPTER 4: RESULTS

This chapter begins with an examination of the impact of drug court participation on mortality, then compares days of sentenced incarceration by treatment condition, before moving on to a comparison of recidivism, both cumulatively and in growth over time. In doing so, the results break from the numeric ordering of hypotheses and instead follow the sequential steps taken to conduct the analyses.

Mortality analyses are conducted first as they have potential impact on the methods for the remaining analyses. If statistically significant differences in survival curves by treatment condition emerge, statistical corrections will be considered for the remaining models to address differential attrition due to mortality and any potential bias that may be introduced as a result. Analysis of incarceration data are presented next because, as with the mortality findings, the data are needed first to calculate participants' time at risk across the 15-year follow-up. These analyses include the main effects of treatment along with the moderating effects of originating court. In the following section, negative binomial models are compared by treatment condition on the cumulative recidivism outcomes, along with an exploration of the potential moderating effect of originating court. Finally, growth curve models are presented to compare the desistance process of Baltimore City Drug Treatment Court participants and those who received traditional adjudication together with an examination of the potential moderating effects of originating court.

The Impact of Drug Court on Mortality

H4 predicts that individuals who participated in the Baltimore City Drug Treatment Court will have lower mortality rates over the 15 years following randomization than individuals who received traditional adjudication.

Twenty-one percent (n=49) of the total sample died within the 15-year follow-up (see Table 6). Among those, 77.6% were male and 91.8% were Black. The deceased had an average of 13.9 arrests and 6.1 convictions at the time of study entry, representing slightly higher offending histories than the total sample at baseline. The average age at time of death for the sample was 46.6 years, with a minimum age of 27 and a maximum age of 66. The average age at time of death for this population is approximately 26 years earlier than projected life expectancy for the general population in Baltimore City and over 32 years earlier than national projections (Maryland Vital Statistics, 2015; Xu, Murphy, Kochanek, & Arias, 2016).

Average age at time of death was somewhat lower for female participants as compared to males (43.4 vs 47.5) despite the fact that general life expectancy estimates tend to be higher for women. While the deceased sample is small (n=11 females; n=38 males) and, as such, caution must be taken in attempting to extrapolate to a general offender population, this finding is consistent with a body of literature that suggests women in the criminal justice system often present with more serious physical and mental health needs (Bloom, Owen, & Covington, 2003).

Of those with valid cause of death data (n=39), 64% of deaths were directly

attributable to drugs/alcohol (23.1%) or related disease/infection[11] (41.0%). This finding is particularly concerning as it underscores the fact that most of these individuals died of preventable and treatable health conditions. Only one person died violently via homicide by an unspecified firearm discharge and no suicides were reported.

Table 6. Descriptive Characteristics of the Deceased

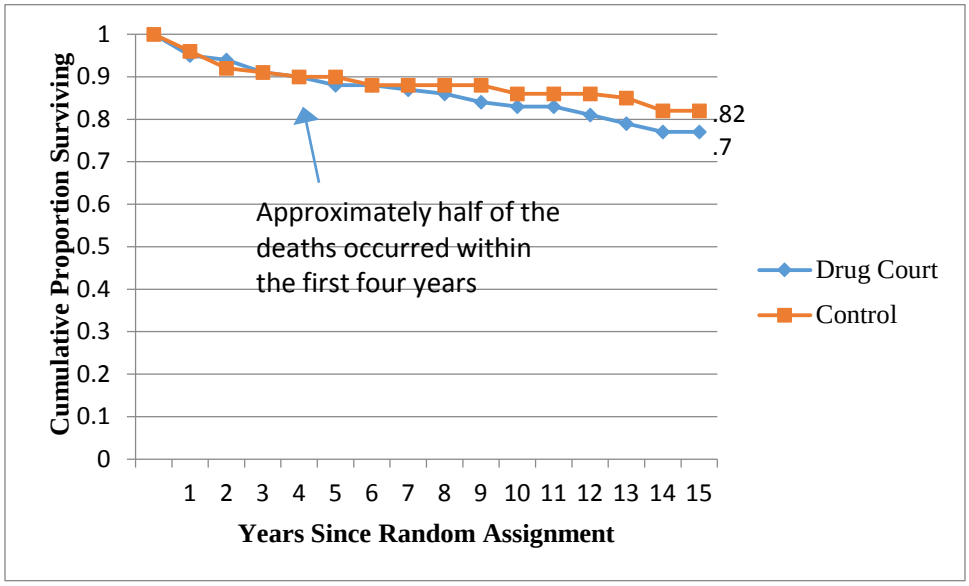
Participant Characteristics	*Mean/ %*	*SD*	*N*
Percent Male	77.6		49
Percent Black	91.8		49
Age	46.6	10.7	49
Prior Arrests	13.9	11.9	49
Prior Convictions	6.1	5.2	49
Cause of Death			39
Percent Drug intoxication	23.1		
Percent Drug-related Disease/Infection	41.0		
Percent Other	35.9		

Figure 3 shows that the survival curves for drug court participants and those receiving traditional adjudication were similar across the 15-year follow-up period. The mortality risk for both groups is highest during the first 4 years, particularly the first year following randomization. Approximately half of the deaths occurred within 4 years of randomization, with the trend line becoming more gradual over the remaining 11 years and slightly but non-significantly greater for the treatment group. In total, 18% of control

[11]Included are conditions such as HIV, hepatitis C, and sepsis, where substance use disorders often plays a significant role in infection and disease progression.

condition participants and 23% of drug court participants died across the 15-year follow-up.

Figure 3. Kaplan-Meier Survival Estimates by Treatment Condition



Results from the log rank test, which tests the hypothesis that the survivor functions across the groups are equal, were not statistically significant ($\chi^2 = .887$, $p = .346$), suggesting that the differences observed in the groups' survival estimates are likely due to chance.

Cox regression was also run to allow for the inclusion of control variables and the differences between treatment and control groups remained non-significant ($p = .352$). A final weighted Cox regression model was run and the differences between groups were, again, non-significant ($p = .248$).

The Impact of Drug Court on Incarceration

H2 predicts that individuals who participated in the Baltimore City Drug Treatment Court will have fewer days of incarceration over the 15-year follow-up than individuals who received traditional adjudication.

Table 7 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 2, total cumulative incarceration days. In this model, any instance in which arrest/charge sentences could not be determined as being served consecutively or concurrently is treated as being served concurrently. While the drug court coefficient is negative, and thus in the hypothesized direction, it does not achieve significance in the model suggesting that participants in the drug court condition did not spend significantly fewer days incarcerated than those in the control

condition.[12] [13] A similar model that treated undetermined sentences as being served consecutively was run and the outcomes were consistent (β=-0.13 and SE=0.34). See also Appendix E for mean comparisons by treatment condition, with undetermined sentences calculated as being served both consecutively and concurrently.

Table 7. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Incarceration Sentences for New Charges and VOPs Across the 15-Year Follow-up with Undetermined Sentences Treated as Concurrent

	β	*SE*	*IRR*
Randomized to Drug Court	-0.14	0.33	
Age	-.076**	0.02	
Male	0.47	0.41	
Black/African American	1.11+	0.59	
Number of Prior Convictions	0.086+	0.05	

+ $p < .10$; ** $p < .01$ (two-tailed tests)

[12] The negative binomial model results in β=-0.13 and SE=0.33 when run with weighted data. Substantive interpretation is unchanged.

[13] Incidence rate ratios (IRRs) are reported for significant relationships between the independent and dependent variables. IRRs represent a variable's exponentiated coefficient value.

Moderating Effect of Originating Court on Incarceration Outcomes

H2a predicts that the effects of drug court participation on incarceration will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having fewer days of incarceration than District Court participants.

Results from negative binomial models that examined incarceration sentences with undetermined cases treated as concurrent and included a drug court * court of assignment interaction term were not significant (β=-0.74 and SE=0.68).[14]

The Impact of Drug Court on Recidivism

H1 predicts that individuals who participated in the Baltimore City Drug Treatment Court will have lower cumulative rates of recidivism (including both arrests and convictions) over the 15 years following randomization than individuals who received traditional adjudication.

All cumulative recidivism models were run three ways: negative binomial regression with IV and control variables included, negative binomial regression with IV and control variables and data weighted to adjust for imbalance on originating court, and negative binomial regression with IV and control variables and an exposure variable that accounts for each participant's number of days free in the community across the 15-year follow-up. Weighted and exposure variable betas and standard errors are footnoted. However, the substantive findings were consistent across models.

[14] The negative binomial model results in β=-0.81 and SE=0.66 when run with weighted data. Substantive interpretation is unchanged.

Arrest Data. Table 8 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total unique arrests across the 15-year follow-up. Drug court participants had significantly fewer arrests across the 15-year follow-up than control participants (β =-0.39, $p < .01$). [15] The incidence rate ratio for those in the drug court condition was 0.68, representing an arrest incidence rate for drug court participants that is 32.4% less than the arrest incidence rate for controls while holding all other variables in the model constant.

Table 8. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Number of Unique Arrests Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.39**	0.12	0.68
Age	-0.051**	0.009	
Male	0.13	0.15	
Black/African American	0.25	0.21	
Number of Prior Convictions	0.081**	0.017	

** p <.01 (two-tailed tests)

Table 9 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total cumulative arrest charges. Drug court participants had significantly fewer charges across the 15-year follow-up than control participants (β =-0.40, $p < .01$).[16] The incidence rate ratio for those in the drug court

[15] The negative binomial model results in β=-0.44 and SE=0.17 when accounting for exposure time, and β=-0.42 and SE=0.12 when run with weighted data. Substantive interpretations are unchanged.

[16] The negative binomial model results in β=-0.36 and SE=0.19 when accounting for exposure time, and β=-0.40 and SE=0.13 when run with weighted data. Substantive interpretations are unchanged.

condition was 0.67, representing a charge incidence rate for drug court participants that is 32.7% less than the charge incidence rate for controls while holding all other variables in the model constant.

Table 9. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Number of Total Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.40**	0.14	0.67
Age	-0.053	0.010	
Male	0.26	0.17	
Black/African American	0.37	0.23	
Number of Prior Convictions	0.068	0.019	

** p <.01 (two-tailed tests)

Table 10 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total drug charges. Drug court participants had significantly fewer drug charges across the 15-year follow-up than control participants (β =-0.29, p < .10).[17] The incidence rate ratio for those in the drug court condition was 0.75, representing a drug incidence rate for drug court participants that is 25.5% less than the drug incidence rate for controls while holding all other variables in the model constant.

[17] The negative binomial model results in β=-0.19 and SE=0.22 when accounting for exposure time, and β=-0.27 and SE=0.16 when run with weighted data. Substantive interpretations are unchanged.

Table 10. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Number of Total Drug Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.29^{+}	0.17	0.75
Age	-0.034**	0.012	
Male	0.085	0.20	
Black/African American	0.66*	0.29	
Number of Prior Convictions	0.036	0.023	

$^{+}$ $p < .10$; * $p < .05$; ** $p < .01$ (two-tailed tests)

Table 11 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total property charges. Drug court participants had significantly fewer property charges across the 15-year follow-up than control participants ($\beta = -0.52^{*}$, $p < .05$).[18] The incidence rate ratio for those in the drug court condition was 0.60, representing a property incidence rate for drug court participants that is 40.5% less than the property incidence rate for controls while holding all other variables in the model constant.

[18] The negative binomial model results in β=-0.56 and SE=0.27 when accounting for exposure time, and β=-0.57 and SE=0.22 when run with weighted data. Substantive interpretations are unchanged.

Table 11. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Number of Total Property Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.52*	0.23	0.60
Age	-0.055**	0.016	
Male	0.78**	0.28	
Black/African American	0.067	0.39	
Number of Prior Convictions	0.11**	0.031	

* $p < .05$; ** $p < .01$ (two-tailed tests)

Table 12 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total person charges. Drug court participants had significantly fewer person charges across the 15-year follow-up than control participants ($\beta = -0.49^{*}$, $p < .10$).[19] The incidence rate ratio for those in the drug court condition was 0.61, representing a person incidence rate for drug court participants that is 39.0% less than the person incidence rate for controls while holding all other variables in the model constant.

[19] The negative binomial model results in β=-0.69 and SE=0.31 when accounting for exposure time, and β=-0.40 and SE=0.24 when run with weighted data. Substantive interpretations are unchanged.

Table 12. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Person Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.49^{+}	0.25	0.61
Age	-0.12**	0.021	
Male	0.37	0.31	
Black/African American	0.76^{+}	0.44	
Number of Prior Convictions	0.088*	0.036	

$^{+}$ $p < .10$; * $p < .05$; ** $p < .01$ (two-tailed tests)

Table 13 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total VOP charges. While the drug court coefficient is negative, and thus in the hypothesized direction, it does not achieve significance in the model.[20]

Table 13. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total VOP Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court[1]	-0.28	0.29	
Age	-0.069**	0.023	
Male	0.15	0.34	
Black/African American	0.15	0.47	
Number of Prior Convictions	0.014	0.043	

** $p < .01$ (two-tailed test)

[20] The negative binomial model results in β=-0.26 and SE=0.38 when accounting for exposure time, and β=-0.30 and SE=0.28 when run with weighted data. Substantive interpretations are unchanged.

Conviction Data. Table 14 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total unique arrests resulting in at least one convicted charge. Drug court participants had significantly fewer convictions across the 15-year follow-up than control participants (β -0.32*, p < .05).21 The incidence rate ratio for those in the drug court condition was 0.73, representing a conviction incidence rate for drug court participants that is 27.2% less than the conviction incidence rate for controls while holding all other variables in the model constant.

Table 14. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Unique Arrests Resulting in at Least One Conviction Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.32*	0.15	0.73
Age	-0.050**	0.011	
Male	0.051	0.19	
Black/African American	0.30	0.26	
Number of Prior Convictions	0.089**	0.021	

** p <.01; * p < .05 (two-tailed tests)

Table 15 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total number of convicted charges. Drug court participants had significantly fewer convicted charges across the 15-year follow-up than control participants (β = -0.36*, p < .05).[22] The incidence rate ratio for those in the drug

[21] The negative binomial model results in β=-0.35 and SE=0.20 when accounting for exposure time, and β=-0.36 and SE=0.14 when run with weighted data. Substantive interpretations are unchanged.
[22] The negative binomial model results in β=-0.33 and SE=0.21 when accounting for exposure time, and β=-0.40 and SE=0.15 when run with weighted data. Substantive interpretations are unchanged.

court condition was 0.70, representing a convicted charge incidence rate for drug court participants that is 30% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

Table 15. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Convicted Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.36*	0.15	0.70
Age	-0.049**	0.011	
Male	0.11	0.18	
Black/African American	0.36	0.26	
Number of Prior Convictions	0.084**	0.021	

** p <.01; * p < .05 (two-tailed tests)

Table 16 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total number of convicted drug charges. While the drug court coefficient is negative, and thus in the hypothesized direction, it does not achieve significance in the model.[23]

[23] The negative binomial model results in β=-0.11 and SE=0.22 when accounting for exposure time, and β=-0.19 and SE=0.16 when run with weighted data. Substantive interpretations are unchanged.

Table 16. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Convicted Drug Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.17	0.16	
Age	-0.032**	0.012	
Male	0.084	0.20	
Black/African American	0.55+	0.29	
Number of Prior Convictions	0.045*	0.022	

** p <.01; * p < .05; + p < .10 (two-tailed tests)

Table 17 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total number of convicted property charges. Drug court participants had significantly fewer convicted property charges across the 15-year follow-up than control participants ($\beta = -0.55$, $p < .10$).[24] The incidence rate ratio for those in the drug court condition was 0.58, representing a convicted charge incidence rate for drug court participants that is 42% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

[24] The negative binomial model results in β=-0.11 and SE=0.22 when accounting for exposure time, and β=-0.19 and SE=0.15 when run with weighted data. Substantive interpretations are unchanged.

Table 17. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Convicted Property Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-0.55^{+}	0.30	0.58
Age	-0.044*	0.021	
Male	0.76^{+}	0.39	
Black/African American	0.19	0.52	
Number of Prior Convictions	0.14**	0.040	

** p <.01; * p < .05; $^{+}$ p < .10 (two-tailed tests)

Table 18 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total number of convicted person charges. Drug court participants had fewer convicted person charges across the 15-year follow-up than control participants (β = -1.11, p < .01).[25] The incidence rate ratio for those in the drug court condition was 0.33, representing a convicted charge incidence rate for drug court participants that is 67% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

[25] The negative binomial model results in β=-1.71 and SE=0.59 when accounting for exposure time, and β=-1.11 and SE=0.42 when run with weighted data. Substantive interpretations are unchanged.

Table 18. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Convicted Person Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court	-1.11**	0.42	0.33
Age	-0.16**	0.041	
Male	1.04+	0.60	
Black/African American	2.65*	1.21	
Number of Prior Convictions	0.15**	0.058	

** p <.01; * p < .05; + p < .10 (two-tailed tests)

Table 19 presents the estimated coefficients and standard errors for the negative binomial model addressing Hypothesis 1, total number of convicted VOP charges. While the drug court coefficient is negative, and thus in the hypothesized direction, it does not achieve significance in the model.[26]

[26] The negative binomial model results in β=-0.50 and SE=0.24 when accounting for exposure time, and β=-0.34 and SE=0.29 when run with weighted data. Substantive interpretations are unchanged.

Table 19. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Model: Total Convicted VOP Charges Across the 15-Year Follow-up

	β	*SE*	*IRR*
Randomized to Drug Court[1]	-0.29	0.29	
Age	-0.073**	0.023	
Male	-0.10	0.34	
Black/African American	0.31	0.49	
Number of Prior Convictions	0.016	0.044	

** p <.01 (two-tailed tests)

[1] Negative binomial model that includes exposure time is significant (β = -0.50, p < .01, IRR 0.61, p < .05)

Moderating Effect of Originating Court on Recidivism Outcomes

H1a predicts that the effects on recidivism will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having significantly lower cumulative rates of recidivism than District drug court participants.

Originating court was shown to moderate the effect of drug court participation for two outcomes: total unique arrest convictions and total charge convictions (see Tables 20 and 21). In each case, those participating in the Circuit drug court had significantly better outcomes than those participating in the District drug court.

In his book, Negative Binomial Regression, Hilbe (2011) provides a strategy for constructing and interpreting the estimated incidence rate ratio from the interaction terms used for count response models.

To calculate the incidence rate ratios for unique arrest convictions and total charge convictions, the following equation is used:

$$IRR_{Interaction} = \exp[\beta_{drugcourt} + \beta_{interaction*originating\ court}]$$

For Total Unique Arrest Convictions Across the 15-year follow-up for the Circuit Court,

$$IRR_{Interaction} = \exp[-0.18 - 0.48*1] = 0.52$$

For Total Unique Arrest Convictions Across the 15-year follow-up for the District Court,

$$IRR_{Interaction} = \exp[-0.18 - 0.48*0] = 0.84$$

The incidence rate ratio for those in the Circuit drug court condition was 0.52, representing a unique arrest conviction charge incidence rate for drug court participants that is 48% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

In contrast, the incidence rate ratio for those in the District drug court condition was 0.84, representing a unique arrest conviction charge incidence rate for drug court participants that is 16% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

Table 20. Estimated Coefficients and Standard Errors for Negative Binomial Model: Total Convictions Across the 15-Year Follow-up, with Moderating Variables

	β	*SE*	*IRR*
Randomized to Drug Court	-0.18	0.20	
Age	-0.052**	0.011	
Male	0.061	0.17	
Black/African American	0.42+	0.25	
Number of Prior Convictions	0.083**	0.019	
Circuit Court	-0.12	0.21	
Randomized to Drug Court * Circuit Court	-0.48+	0.29	

** p <.01; + p < .10 (two-tailed tests)

For Total Charge Convictions Across the 15-year follow-up for the Circuit Court,

$$IRR_{Interaction} = \exp[-0.22 - 0.50*1] = 0.49$$

For Total Charge Convictions Across the 15-year follow-up for the District Court,

$$IRR_{Interaction} = \exp[-0.22 - 0.50*0] = 0.80$$

The incidence rate ratio for those in the Circuit drug court condition was 0.49, representing a charge conviction incidence rate for drug court participants that is 51% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

In contrast, the incidence rate ratio for those in the District drug court condition was 0.80, representing a charge conviction incidence rate for drug court participants that

is 20% less than the convicted charge incidence rate for controls while holding all other variables in the model constant.

Table 21. Estimated Coefficients and Standard Errors for Negative Binomial Model: Total Charge Convictions Across the 15-Year Follow-up, with Moderating Variables

	β	*SE*	*IRR*
Randomized to Drug Court	-0.22	0.21	
Age	-0.051**	0.011	
Male	0.12	0.18	
Black/African American	0.50+	0.26	
Number of Prior Convictions	0.078**	0.021	
Circuit Court	-0.18	0.22	
Randomized to Drug Court * Circuit Court	-0.50+	0.31	

** p <.01; + p < .10 (two-tailed tests)

The Impact of Drug Court on Desistance from Crime

This section of the chapter presents growth curve models related to unique arrest and unique arrest conviction patterns by year across the 15-year follow-up. Prior to model building, growth in each outcome was visually inspected and compared across treatment condition. Following visual inspection, models were run in STATA using the MENBREG command, which provide a generalization of the negative binomial model and are suitable for overdispersed, count data. As a robustness check, models were also run using the MIXED command, which is a generalization of a linear model with corresponding assumptions about the underlying distribution of the data. In every case,

the models produced consistent substantive findings regarding the nature of the relationships between the independent and dependent variables over time.

Figure 4 displays arrest patterns across the 15-year follow-up by treatment condition. This visual inspection of the data shows that arrests are declining in a fairly linear manner in both groups over time but that the average number of arrests appears to be smaller in the drug court condition across each time point. The details of these trends will be reported in the forthcoming Table 22.

Figure 4. Mean Number of Unique Arrests per Year by Treatment Condition

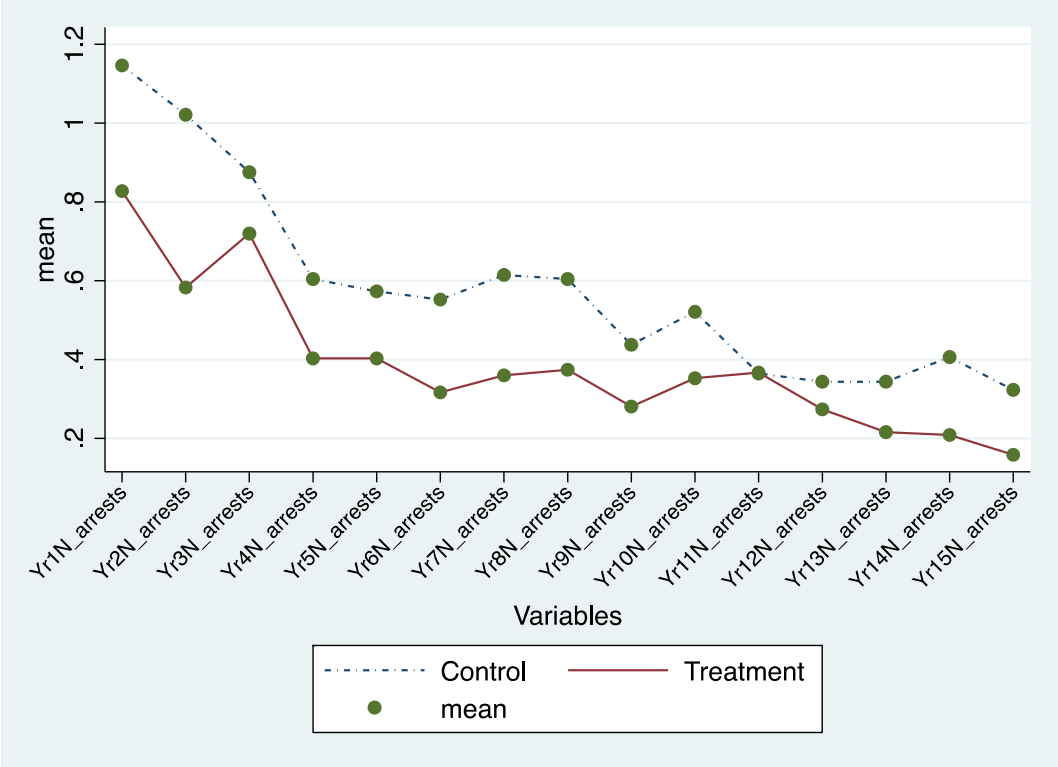
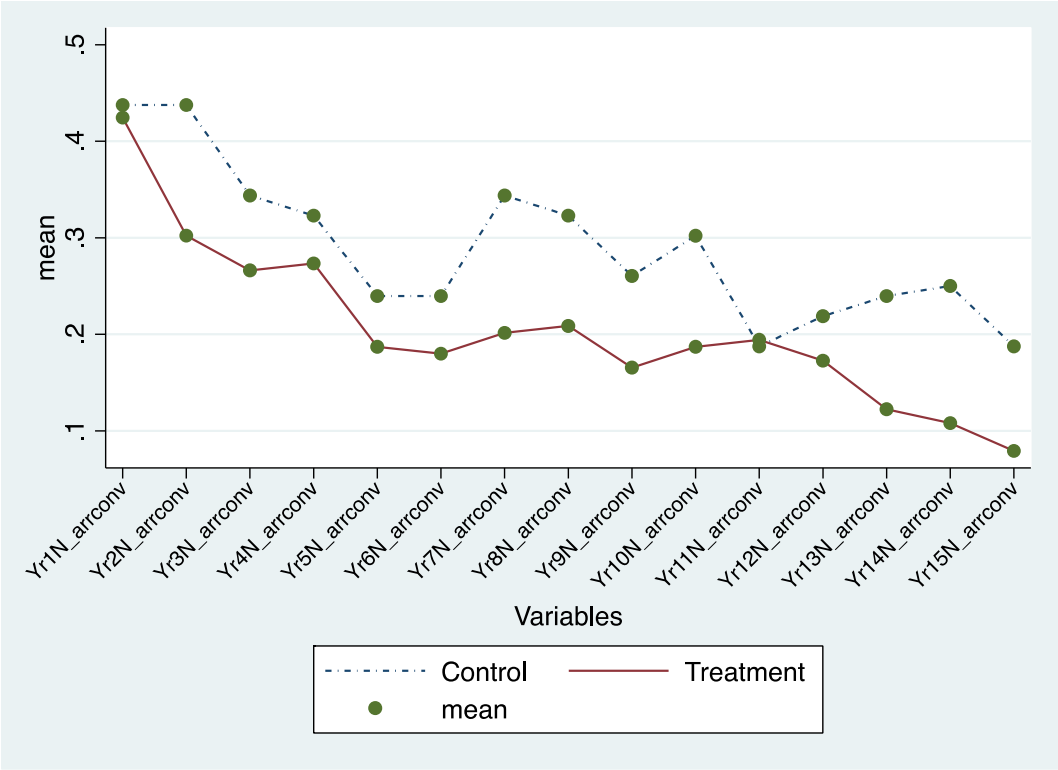


Figure 5 displays conviction patterns across the 15-year follow-up by treatment condition. Similar to arrests, this visual inspection of the data shows that convictions are declining in a fairly linear manner in both groups over time but that the mean number of

convictions appears to be smaller in the drug court condition across each time point. The details of these trends will be reported in the forthcoming Table 23.

Figure 5. Mean Number of Arrests Resulting in at Least One Conviction per Year by Treatment Condition



H3. Predicts that individuals who participated in the Baltimore City Drug Treatment Court will have a faster rate of desistance as compared to individuals who received traditional adjudication and the differences between the two groups will grow over time.

Model building begins with estimation of a fixed effects model that includes a wave variable (measuring time in years from randomization date 1-15). Following, I tested for whether the inclusion of random intercepts would improve model fit and found that the likelihood ratio test comparing a model that incorporates random intercepts to one without is statistically significant in favor of rejecting the fixed intercept model. As such, I was able to conclude that model fit is improved by the introduction of random

intercepts. I then tested a model that allowed for the inclusion of both random intercepts and slopes. The likelihood ratio test comparing the model of the arrest growth curve with only random intercepts to the model that added random slopes to the model again resulted in a statistically significant finding in favor of rejecting the random intercept only model. As such, future models allow for both random intercepts and slopes.

Treatment condition was then added to the model along with the control variables (see Table 22). Drug court participants had fewer arrest charges over time than control participants ($\beta = -.42$, $p < .01$). The incidence rate ratio for those in the drug court condition was 0.65, 35% less than the incidence rate for controls while holding all other variables in the model constant. Inclusion of a time*randomization condition interaction term was not significant, suggesting that while the arrests start at a lower value for those in the drug court condition the desistance rate does not decrease at a faster pace over time for the drug court group.

Table 22. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Growth Curve Models: Total Unique Arrests Across the 15-Year Follow-up

	β	*SE*	*IRR*
Time from Randomization,Years	-0.18**	0.15	0.84
Randomized to Drug Court	-0.42**	0.12	0.65
Age	-0.044**	0.009	
Male	0.095	0.15	
Black/African American	0.14	0.21	
Number of Prior Convictions	0.063**	0.016	

** p <.01 (two-tailed tests)

Model building for the conviction outcome also begins with estimation of a fixed effects model that includes a wave variable (measuring time in years from randomization date 1-15). Following, I tested for whether the inclusion of random intercepts would improve model fit and found that the likelihood ratio test comparing a model that incorporates random intercepts to one without is statistically significant in favor of rejecting the fixed intercept model. As such, I was able to conclude that model fit is improved by the introduction of random intercepts. I then tested a model that allowed for the inclusion of both random intercepts and slopes. The likelihood ratio test comparing the model of the arrest growth curve with only random intercepts to the model that added random slopes to the model again resulted in a statistically significant finding in favor of rejecting the random intercept only model. As such, future models allow for both random intercepts and slopes.

Treatment condition was then added to the model along with the control variables (see Table 23). Drug court participants had fewer arrest convictions over time than control participants ($\beta = -.27$, $p < .05$). The incidence rate ratio for those in the drug court condition was 0.76, 24% less than the incidence rate for controls while holding all other variables in the model constant. Inclusion of a time*randomization condition interaction term was not significant, suggesting that while arrest convictions start at a lower value for those in the drug court condition the desistance rate does not decrease at a faster pace over time for the drug court group.

Table 23. Estimated Coefficients, Standard Errors and Incidence Rate Ratios for Negative Binomial Growth Curve Models: Total Unique Arrests Resulting in at Least One Conviction Across the 15-Year Follow-up

	β	*SE*	*IRR*
Time from Randomization, Years	-0.16**	0.018	0.85
Randomized to Drug Court	-0.27*	0.14	0.76
Age	-0.04**	0.01	
Male	-0.001	0.17	
Black/African American	0.23	0.24	
Number of Prior Convictions	0.075**	0.017	

** p <.01; * p < .05 (two-tailed tests)

Moderating Effect of Originating Court on Growth Outcomes

H3a predicts that the effects of drug court participation on desistance will be moderated by originating court with participants in Baltimore City Drug Treatment's Circuit Court having a faster rate of desistance than District Court participants.

Results from the negative binomial models that examined rates of growth on the total unique arrests and included a drug court * court of assignment interaction term were not significant (β=-0.16 and SE=0.24). Results from the negative binomial models that examined rates of growth on the total unique arrest convictions and included a drug court * court of assignment interaction term were significant (β=-0.46 and SE=0.27; p < .10).

The Influence of Control Variables: All Models

While not a focus of the work, this section provides a brief overview of the control variables included in the analytic models. Table 24 includes a summary of the models that report significant or marginally significant relationships between each control variable and the 7 arrest and 7 conviction outcomes. With regard to trends, age at the time of randomization was significantly and negatively related to all 7 arrest and all 7 conviction outcome categories. Being male was significantly and positively related to property charge arrests and to property and person convictions. Being black was significantly and positively related to both drug and person arrests and convictions. Finally, number of prior convictions at the time of randomization was significantly and positively related to unique arrests, total charges, property charges and person charges, as well as unique arrest convictions, convicted charges, convicted drug charges, convicted property charges, and convicted person charges.

Table 24. Number of Models (Maximum of 7 per Outcome) Displaying a Significant Relationship Between the Control Variables and the Dependent Variables

	Arrest Outcomes		Conviction Outcomes	
Relationship direction	+	-	+	-
Age	0	7	0	7
Male	1	0	2	0
Black/African American	2	0	2	0
Number of Prior Convictions	4	0	6	0

CHAPTER 5: CONCLUSION

This chapter begins with a review of findings regarding recidivism, incarceration, desistance patterns, and mortality outcomes among Baltimore City Drug Treatment Court participants and participants receiving traditional adjudication. Following is a discussion of the limitations of this work. A set of proposed policy implications follow, guided by the study's findings and with recognition that replication is a necessary first step. Finally, the chapter concludes with several possibilities for future research.

Summary of Results

This dissertation represents the first long-term follow-up of drug court outcomes among participants randomly assigned to drug court or traditional adjudication. Findings suggest that participation in Baltimore City's Drug Treatment Court resulted in significantly fewer unique arrests, total charges, and total drug, property, and person charges across the 15-year follow-up period. Participation in Baltimore City's Drug Treatment Court also resulted in significantly fewer unique arrest convictions, total charge convictions, and total person and property charge convictions across the 15-year follow-up period. Two notable areas in which drug court effects were not demonstrated were in total drug charge convictions and total VOP charges and convictions.

Although drug charge arrests were significantly lower among drug court participants, their drug charge convictions were no different than participants receiving standard adjudication. It is possible that increased scrutiny of drug court participants drug use may have led to the finding of no difference in drug convictions between those in the drug court versus those in traditional adjudication. In addition to increased

monitoring, the drug court model includes swift and certain sanctions for non-compliance, and as such drug court participants were perhaps more likely to be convicted for new drug offenses as well as any corresponding VOP charge convictions. Drug court participants also had significantly lower rates of both unique arrests and unique arrest convictions in the growth models. However, those differences between the groups did not grow over time.

Originating court was shown to moderate the effect of drug court participation for total unique arrest convictions and total charge convictions, such that those participating in the Circuit drug court had significantly better outcomes than those participating in the District drug court. A significant moderating effect favoring the Circuit Court was also observed in the conviction growth model. The findings suggest that additional work in this area is warranted to fully uncover potential sources of treatment effect heterogeneity.

Participation in Baltimore City's Drug Treatment Court did not have a significant effect on total days of sentenced incarceration during the 15-year follow-up. This finding is consistent with several studies that have found that drug courts do not necessarily serve as an effective alternative to incarceration (Pollack, Reuter, & Sevigny, 2011; Sevigny, Pollack, & Reuter, 2013). Oftentimes, the incarceration gains of those who are successful in the drug court are offset by the high sentences imposed by those who are unsuccessful (Rossman, et al., 2011). It is also consistent with results from the 3-year follow up of the BCDTC, which found no differences in incarceration time by treatment condition overall but significantly greater days incarceration for those in the District drug court. When assessing conviction rates over the follow-up – i.e., among those who were arrested, the proportion who were convicted – no differences are observed by treatment

condition so it does not suggest that there was a lingering punitive effect of drug court involvement on future judicial decision making. When examining the incarceration variables' standard errors, they were quite large for both groups but particularly so for the treatment group suggesting again that the results may be driven in part by those who were unsuccessful in the program.

Participation in Baltimore City's Drug Treatment Court did not result in a reduction in mortality risk, and in fact, the percentage of those who died during the 15-year follow-up was slightly higher though not statistically significant among the drug court group. Approximately 21% of the total sample died during the 15-year follow-up. Most deaths occurred within the first few years post-randomization - the vast majority of which were directly attributable to drugs and alcohol or related disease and infection. In considering the concentration of deaths within that timeframe, a number of plausible explanatory factors emerge. First, the vast majority of participants were users of hard drugs – primarily heroin – and many were daily users at the time of study entry, representing an extremely high-risk group. Additionally, both treatment and control participants were in and out of jail and prison, under court supervision, and some were in drug treatment during those first few years post-randomization. While those mechanisms generally reduce overall levels of use, there is a large body of research to suggest that the period immediately following release from incarceration or drug treatment is associated with elevated overdose risk (Binswanger, Blatchford, Mueller, & Stern, 2013; Andrews & Kinner, 2012; Krinsky, Lathrop, Brown, & Nolte, 2009; Binswanger et al., 2007; Merrall et al., 2010). This is because these individuals may go through a period of prolonged sobriety, which lowers their drug tolerance, and leaves them at higher risk of

overdose should they reinitiate use at or near previous consumption levels. Additionally, these individuals may lose contact with earlier sources of opiates and, due to the unregulated nature of street drugs, may purchase a similar quantity from a different source that is of much greater potency or adulterated.

Limitations

This dissertation has several limitations worth noting that relate to both the generalizability of the findings and to the lack of information available for participants over the 15-year period beyond official records of recidivism and mortality.

The generalizability of the study is somewhat limited to drug offenders in urban settings who have chronic criminal and substance use disorder histories. However, in terms of prognostic risk and need, this population represents current recommendations regarding those best suited to the intensive requirements and resource expenditure of the drug court model (Marlowe, 2012). Additionally, the Baltimore City Drug Treatment Court contains all of the key components of the model (e.g., judicial monitoring, drug treatment, intensive supervision) and, as such, does not vary significantly from the "typical" drug court (NADCP, 1997). Still, the characteristics of the sample, the scale of the court, and the community-based resources available may be unique to larger, urban areas.

A related limitation is that while the Baltimore City Drug Treatment Court is still in operation and represents a typical court in terms of it's basic structure, the implementation and features of the court have likely changed in many ways over the past 15+ years. Although the intent of this dissertation was to provide a look at the long-term

effects of involvement in the court as implemented in the late 1990s – early 2000s, the outcomes may differ in important ways for those participating in the contemporary court. Discussions with a drug court coordinator revealed the following changes in the program's implementation since that time: average length of stay in the program has changed from 12 to 18 months; community service requirements have increased by 40 hours, aftercare resources that focus on stepped down treatment supports have been expanded; increased focus is paid to health care access, increased acceptance and expansion of medication assisted therapy (MAT), and more tailored case management for special populations such as young adults and women with children.

This dissertation is also limited by a lack of information regarding the intervening mechanisms at play throughout the 15-year follow-up, which if available, would provide needed context to the outcomes observed. By virtue of its design, the possibility of confounding explanations of the drug court effect observed are minimized. However, because the data do not include consistent measures of substance use patterns, social bonds, and other mechanisms believed to be at work over time, it is not possible to put together a full picture of the change process. These data deficiencies also limit the ability to properly test the life course perspective.

The lack of data regarding participant characteristics at baseline and the ways in which the implementation of Baltimore's District and Circuit Drug Treatment Courts differ also limits the ability to explain the sources of treatment heterogeneity observed over some time periods of the study. This limitation prevents the possibility of a concrete explanation for the differences observed. Further, this limitation prevents the formation of targeted policy recommendations regarding population suitability or model fidelity.

Additionally, the incarceration data included in this dissertation is an approximation limited to data collected from assigned sentence lengths captured in CJIS with subsequent checks in OBSCIS to distinguish whether assigned sentence lengths were served concurrently or consecutively. The data also did not explicitly capture pretrial detention and therefore may underestimate incarceration stays served prior to adjudication. Data from the initial three-year follow-up of the BCDTC was taken from rap sheets and the Judicial Information System (JIS) and included detail on incarceration as a result of the initial arrest leading to study involvement, including pre-disposition incarceration. The mean number of days incarcerated as a result of the initial arrest was 158 with 24 of those days served pre-disposition. Although a crude estimate from which to extrapolate, it suggests that, on average, approximately 15% of days BCDTC participants spend incarcerated are spent in pre-trial detention. Although there is no reason to anticipate a treatment-control difference in the number of days spent in pretrial detention during the follow-up period, the study is not able to test this possibility.

Finally, while the sample size was adequate to detect treatment effects across the 15-year period, it is possible that low statistical power, coupled with count data with a high degree of observed variance, may have hampered detection of meaningful differences between treatment and control conditions and sub-group analyses by originating court.

Policy Implications

Results from the Baltimore City Drug Treatment Court suggest that drug courts have the potential to lead to sustained effects on drug use and criminal activity for a

population of chronic drug offenders. Drug courts success with high-risk populations has now been demonstrated in several studies and, in the current sample, appeared to work better in some cases for those with more serious charges.

Due to the cost and additional administrative burdens of drug courts, states and localities should consider reserving these specialized courts for those with the greatest need while providing less intensive services for those who are able to regulate their substance use on their own. Several risk need responsivity models have shown promise. For example, a behavioral triage model (BTM) uses an offender's observed behavior as a signal of their need for services (Hawken, 2010). BTM relies on a probationer's observed behavior under regular random drug testing combined with swift, certain but proportionate sanctions. Among those who can abstain from drug use under these conditions alone, additional services such as drug treatment are not indicated. Thus the resources that are provided in the drug court model can be saved for individuals with more serious substance use disorders. This resource-allocation approach also helps ensure the availability of higher-quality, longer-term care for those in greatest need.

Additionally, drug courts should carefully monitor for potential iatrogenic effects among participants. Concerns about net widening are potentially salient among low risk offenders and those who do not perform well in the drug court. Expansion of treatment services to offenders is a noble goal but should not be tied to undue risks of more incarceration time should they be unsuccessful.

While substance use disorders are preventable and treatable, drug-related morbidity and mortality continue to rise. In the current study, Baltimore City Drug Treatment Court participants did not have significantly different mortality outcomes than

their control counterparts. Drug court programs looking to improve outcomes in this area should consider focusing efforts on reducing the stigma surrounding addiction, increasing access to evidence-based drug treatment and other health services, and increasing access and training in the administration of naloxone.

Stigma is a central barrier to both seeking and receiving help for a substance use disorder. Among pressing public health concerns, it could be argued that no other condition is as stigmatized as addiction. Changing the perception that addiction is a moral failure and educating drug court stakeholders about the changes to brain chemistry that develop after prolonged periods of use are important first steps. While some may trivialize the importance of language in discussions of stigma, there have been a number of empirical studies that find commonly used words or phrases (e.g., drug "abuser," "dirty" urine) induce implicit cognitive biases against those living with a substance use disorder and may influence judgments regarding blameworthiness and decrease a person's own sense of hope and self-efficacy for change (Kelly, Saitz, & Wakemen, 2016; Kelly & Westerhoff, 2010). Drug court personnel and treatment staff should have a clear understanding of the behavioral and neurological changes that occur as a result of compulsive drug use and should use language that is clinically appropriate.

Access to evidence-based drug treatment, to include medication-assisted treatment (MAT) delivered via primary care physicians, has been shown to reduce infectious disease transmission and mortality (Thomas, Melinda, Jinhee, Mitra, & Robert, 2015). A recent study examining mortality patterns over a 14-year period found that increasing the availability of MAT (methadone and buprenorphine) led to a near 50% decrease in the number of fatal heroin overdoses (Schwartz et al., 2013). Despite positive

outcomes, MAT is underutilized in many communities due to lack of access, lack of training for providers, negative attitudes toward MAT by the public, providers, and patients, and treatment program and insurance restrictions on who can receive MAT and for what duration (Volkow, Frieden, Hyde, & Cha, 2014).

Drug court stakeholders should work to ensure that MAT options are available to participants and should advocate for the expansion of MAT services in their community. Additionally, drug court stakeholders should work to educate themselves, their peers, and participants on the positive outcomes associated with MAT use and dispel common myths and inaccuracies about MAT, e.g., that MAT replaces one addiction for another; MAT is only for the weak (Matusow et al., 2013)

Future Research

The results of this dissertation suggest several avenues for future research. Very few long-term follow-up studies of criminal justice interventions of any kind exist. The results of this dissertation show a sustained effect of the Baltimore City Drug Treatment Court on patterns of arrests and convictions generally and across several crime types. Replication of these results is warranted, particularly among drug court evaluations with a strong research design.

Another avenue of research worth consideration is supplementing long-term follow-up of official records with interview data. Qualitative data often provides a more nuanced and detailed understanding of the processes involved in both continued criminal involvement and desistance, and, as mentioned earlier, would make it possible to properly test desistance theories. Interview data would also allow for comparison across

a greater array of outcomes, to include substance use, health, housing, employment, and family functioning to name just a few. Qualitative data can also provide an important check or counter point to official records of recidivism. Several studies have shown that individuals often self-report criminal involvement and substance use during periods that are not reflected in official records (Bachman, Kerrison, Paternoster, Smith, & O'Connell, 2015; Piquero, Schubert, & Brame, 2014). As such, this data has the potential to provide a more accurate representation of drug court participants' substance use and offending patterns, as well as a better understanding of the underlying drivers of their decision-making.

Cost benefit analyses of drug courts with appropriate counterfactuals are needed. Ideally, these calculations would include a broad array of costs and benefits that include items related to administrative costs, incarceration, crime, drug use, employment, physical and mental health and family functioning. Such analyses would abe of great value when considering whether the drug court model warrants expansion to communities that do not currently have a drug court.

Finally, while this dissertation answers affirmatively that drug court involvement has lasting impacts on average recidivism rates, a large degree of variability was observed in participant outcomes. Although the current work did explore that variability by testing for the moderating effect of originating court, future analyses will examine treatment heterogeneity using a growth mixture model approach such as group-based trajectory modeling (not constrained to just the moderating influence of originating court). This next step will provide additional examination of potentially important subgroup differences.

APPENDICES

APPENDIX A. Comparison of Arrests and Charge Types Across the 15-Year Follow-up by Treatment Condition

	Experimental Status							
	Treatment (n =139)				Control (n = 96)			
	Mean	SD	Median	% Involved	Mean	SD	Median	% Involved
Number of Arrests	5.84	6.03	4.00	86.3	8.73	10.0	6.00	93.8
Total Number of Charges	12.1	12.6	8.00	*****	18.9	18.9	15.0	*****
Total Person Charges	1.71	4.21	.000	40.3	3.01	6.56	1.00	53.1
Total Drug Charges	5.93	7.27	3.00	74.8	8.06	8.73	6.00	80.2
Total Property Charges	3.26	6.33	1.00	52.5	5.59	9.84	2.00	62.5
Total VOP Charges	.56	1.46	.00	23.7	.84	1.56	.00	39.6

APPENDIX B. Comparison of Arrests and Charge Types Across the 15-Year Follow-up by Treatment Condition and Originating Court

	Experimental Status											
	District Court						Circuit Court					
	Treatment (n = 84)			Control (n = 42)			Treatment (n = 55)			Control (n = 54)		
	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
Number of Arrests	6.39	6.59	4.00	8.93	9.44	5.00	5.00	4.98	4.00	8.57	10.5	6.00
Total Number of Charges	12.8	13.3	7.50	17.0	17.1	13.0	11.1	11.5	8.00	20.3	20.3	15.0
Total Person Charges	1.19	3.21	.000	2.10	3.92	.500	2.49	5.33	1.00	3.72	8.00	1.00
Total Drug Charges	6.08	7.75	3.00	6.64	8.84	3.00	5.69	6.55	4.00	9.17	8.55	7.50
Total Property Charges	3.92	7.14	1.00	6.45	10.7	2.50	2.27	4.72	1.00	4.93	9.18	2.00
Total VOP Charges	.74	1.76	.000	.83	1.81	.000	.29	.74	.000	.85	1.35	.000

APPENDIX C. Comparison of Convictions and Charge Conviction Types Across the 15-Year Follow-up Period by Treatment Condition

	Experimental Status							
	Treatment (n = 139)				Control (n = 96)			
	Mean	SD	Median	% Involved	Mean	SD	Median	% Involved
Number of Arrests Resulting in Conviction	3.07	3.55	2.00	75.5	4.33	5.65	3.00	81.3
Number of Convicted Charges	3.98	4.65	3.00	*****	5.97	7.88	4.00	*****
Total Person Convictions	.19	.70	.000	10.8	.64	2.66	.000	21.9
Total Drug Convictions	2.14	2.75	1.00	63.3	2.56	2.73	2.00	71.9
Total Property Convictions	.96	2.31	.000	30.9	1.70	4.41	.000	37.5
Total VOP Convictions	.42	.98	.000	20.9	.67	1.32	.000	34.4

APPENDIX D. Comparison of Convictions and Charge Conviction Types Across the 15-Year Follow-up Period by Treatment Condition and Originating Court

	Experimental Status											
	District Court						Circuit Court					
	Treatment (n = 84)			Control (n = 42)			Treatment (n =55)			Control (n=54)		
	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
Number of Arrests Resulting in Conviction	3.73	4.05	3.00	4.50	5.42	2.00	2.07	2.32	1.00	4.20	5.87	3.00
Number of Convicted Charges	4.90	5.35	3.00	6.40	7.78	3.00	2.56	2.82	2.00	5.63	8.01	4.00
Total Person Convictions	.13	.53	.000	.48	1.50	.000	.27	.89	.000	.76	3.30	.000
Total Drug Convictions	2.48	3.09	1.00	2.45	2.72	2.00	1.62	2.05	1.00	2.65	2.76	2.00
Total Property Convictions	1.32	2.82	.000	2.31	5.09	.000	.40	.91	.000	1.22	3.79	.000
Total VOP Convictions	.54	1.17	.000	.76	1.61	.000	.24	.58	.000	.59	1.06	.000

APPENDIX E. Comparison of Incarceration Sentences by Treatment Condition

	Experimental Status							
	Treatment (n=139)				Control (n=96)			
	Mean	SD	Median	% Involved	Mean	SD	Median	% Involved
Days Incarceration, [a] Concurrent	1071.39	2215.41	95.00	64.0%	1340.92	1917.61	290.00	72.9%
Days Incarceration, [b] Consecutive	1090.26	2218.68	120.00	64.0%	1371.02	1942.06	290.00	72.9%

[a] Includes all incarceration sentences received during the 15-year follow-up, with unclear charge sentences treated as concurrent.
[b] Includes all incarceration sentences received during the 15-year follow-up, with unclear charge sentences treated as consecutive.

APPENDIX F. Comparison of Incarceration Sentences by Treatment Condition and Originating Court

	Experimental Status											
	District Court						Circuit Court					
	Treatment (n=84)			Control (n=42)			Treatment (n=55)			Control (n=54)		
	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median	Mean	SD	Median
Days Incarceration, [a] Concurrent	1194.62	2479.52	172.50	1156.07	1609.25	135.00	883.18	1741.64	60.00	1484.69	2130.67	481.50
Days Incarceration, [b] Consecutive	1207.44	2480.10	172.50	1200.36	1640.33	150.00	911.29	1752.61	60.00	1503.76	2153.58	481.50

[a] Includes all incarceration sentences received during the 15-year follow-up, with unclear charge sentences treated as concurrent.
[b] Includes all incarceration sentences received during the 15-year follow-up, with unclear charge sentences treated as consecutive.

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