

ABSTRACT

Title of Dissertation: EXAMINING TIME TO REARREST BY
DRUG TREATMENT EXPERIENCE OF
DRUG COURT ELIGIBLE OFFENDERS

Carlos M. Rocha, Doctor of Philosophy, 2011

Dissertation Directed By: Professor Denise C. Gottfredson, Department of
Criminology and Criminal Justice

This study explores the relationship between drug treatment experience and time until rearrest among a sample of drug court eligible offenders. The subjects for this work were randomized into an evaluation of the Baltimore City Drug Treatment Court. Of the 235 subjects who participated in this evaluation, 128 received some form of drug treatment during the three year follow-up period. Treated subject's time until rearrest was compared to the 107 subjects who did not receive drug treatment. Treatment experience was measured two different ways: first modality received and number of treatment episodes experienced. Kaplan-Meier survival estimates showed that the treated groups had longer survival times than the non-treated group. Cox regression analyses were then conducted to determine what explained this finding. Results showed that days of treatment and days of supervision were all significant predictors of time until rearrest, while first treatment modality and number of treatment experiences did not predict this outcome. Implications of these findings for policy makers and researchers are discussed.

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by

Carlos M. Rocha

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2011

Advisory Committee:

Professor Denise C. Gottfredson, Chair
Dr. Amelia Arria
Associate Professor Laura Dugan
Professor Gary Gottfredson
Professor Charles Wellford

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ACKNOWLEDGEMENTS

I would like to first thank my dissertation committee for their helpful insights in making this work what it is. Particular thanks go to my chairperson, Denise Gottfredson. Denise read more drafts of this dissertation than I can count but always offered great guidance and suggestions. It was a great pleasure to work with a true scholar of criminology.

I would also like to thank Mom, Andrea, and Pete for their support during this endeavor. Andrea would always ask how my “little paper” was going and never ceased to remind me that I could do it.

Special thanks go to my two little sources of gravity- Alicia Cecilia and Augustin Ramon- who always pulled me back to reality during my school work and reminded me what was truly important in life. I could not be more proud to call you my children.

But the one person who made this all possible was Diann. She put up with more than any spouse should have during this whole process but never once waived in her support for me. All that I have and will accomplish means absolutely nothing without her by my side.

DEDICATION

This work is in loving memory of Dan Robert Fay and Dr. Ramon Michael Rocha, who always taught me that nothing is impossible.

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Chapter 1: Introduction

This study examines the treatment experience and survival rates of individuals who participated in the Baltimore City Drug Treatment Court (BCDTC) evaluation in Baltimore, Maryland. According to drug court scholars, treatment and supervision are the two primary components of a drug court (Belenko, 2000). In most drug courts, offenders can be assigned to different treatment modalities, ranging from loosely structured programs to intense residential supervision. This evaluation addresses whether drug court eligible offenders who attend varying treatment modalities or have varying number of treatment episodes have significantly varying lengths of time until an arrest and, if so, why? The current work answers these questions with a sample of 128 subjects who received at least one form of certified drug treatment within three years after their randomization date into the evaluation and 107 subjects who did not receive certified treatment.

Answering these questions is integral to understanding how drug court eligible offenders respond to treatment. Practitioners can use these findings to gain a better understanding of the drug court treatment process as a whole, rather than through the limited experiences of those on their caseload. This, in turn, can enhance the practitioners' program placement decision making process. Moreover, findings can improve the effectiveness of the drug court experience by providing information about whether drug court eligible offenders perform differently based on the type or number of treatment they receive. Better performance in certain modalities may be cause for advocating higher availability of such modalities. To the contrary, modalities that appear

to have a limited effect on client behavior may need to be further studied to determine the reasons for such findings and subsequently altered. These actions can potentially increase the success of drug court eligible clients, which will reduce further monetary and resource strain felt by the criminal justice system in dealing with this population.

Drug court scholars have argued that the treatment component of drug courts has not yet been fully explored in the literature, although examining drug court treatment components can be helpful. The vast majority of studies to date have simply measured whether drug court participants perform better than non-participants, though more recent evaluations have begun to address more complex issues surrounding treatment in drug court. However, these studies have yet to explore the treatment component in depth, specifically looking at the influence of different treatment modality experiences on client success. Most studies that include treatment variables in their work either only report the percentage of the sample that attended the treatment or are limited to only one or two treatment modalities. This study addresses these shortcomings by using data on several treatment modalities and by using these data in a more sophisticated analysis to determine modality effect on time until re-arrest.

Past and Present U.S. Drug Control Policy

The current drug control policy in the United States can find its roots in the monumental Harrison Act of 1914. The goal of this legislation was to suppress the proliferation of narcotics and cocaine through criminalizing the manufacturing, selling and distribution of these substances. Similar federal and state legislation were enacted for marijuana in 1937 and psychoactive pharmaceutical drugs in the 1960's. The culmination

of these assorted mandates led to the Federal Controlled Substances Act in 1970 and similar parallel legislation within individual states soon followed. This criminalization of the production and distribution of these substances inherently placed the control efforts on the criminal justice system, particularly law enforcement agencies. The central tenant of the law enforcement model to combat drug use was deterrence; using the threat of sanctions to control involvement with these substances. In the 1970's, economic theory began to permeate the drug policy arena with scholars and policy makers viewing substance abuse control as a bifurcated effort, addressing the "supply side" and the "demand side." Focusing law enforcement resources on apprehending drug traffickers and distributors was the primary means of reducing supply, while targeted enforcement of individual drug buyers looked to stem the demand (Committee on Data and Research for Policy on Illegal Drugs, 2001).

Contributing to the support of the drug control law enforcement model was the ideological shift occurring during the same time period in regards to the treatment and rehabilitation of offenders. Publications by Martinson (1974) and Lipton, Martinson, and Wilks (1975) argued that the rehabilitative ideal that guided correctional policy in the 1960's and 1970's was ineffective and with few exceptions, the rehabilitative efforts that have been reported to that point had no appreciable effect on recidivism. Though his findings were somewhat taken out of context (Cullen, 2002), Martinson's sentiments echoed the lack of faith in the corrections system felt by policy makers during this time period. Now armed with the scholarly work of Martinson and his colleagues to support their argument, law makers and correctional administrators embraced philosophy and practice that emphasized incapacitation and formal control of drug offenders rather than

rehabilitation and treatment. For drug offenses, this meant an increase in law enforcement scrutiny, significant changes in sentencing practices, and reduced availability of corrections-based treatment.

In the following decades, the response of law enforcement in stemming illicit drugs varied little from this philosophical shift, leading to an unprecedented increase in arrests for drug crimes. Local and state arrests for drug offenses more than doubled between 1980 and 1994, from approximately 581,000 to over 1.3 million (Bureau of Justice Statistics, 2003). This time period included the modern day “War on Drugs,” a term first coined by the Nixon administration in the early 1970’s that aptly described the strategies and tactics policy makers and law enforcement leaders felt would best stem drug involvement. This militaristic approach rejected the notion that drug involvement was a medical problem that could be thwarted by treatment. Rather, it was a problem best dealt with by “cops rather than docs” (Committee on Data and Research for Policy on Illegal Drugs, 2001).

This approach greatly influenced sentencing policy as policy makers during the 1980’s and 1990’s sought to increase the certainty and severity of drug offenses. Durose, Levin and Langin (2001) found that the approximate likelihood of a felony arrest leading to a felony conviction of felony drug trafficking increased from 53% in 1990 to 68% in 1998, making this the highest probability for conviction of all major offense types. Additionally, the percent of time served by felony drug traffickers rose from 31% in 1990 to 41% in 1998 (Durose et al., 2001). This increase in incarceration rate was also accompanied by longer sentences and harsh mandatory minimum sentences for drug crimes. For example, the New York State Rockefeller drug laws enacted in the 1970’s

(and still meted out today) allowed judges to sentence a drug offender 15 years to life in prison for the possession of four ounces of a controlled substance (Drug Policy Alliance, 2004). This resulted in more drug offenders filling correctional facilities and an unprecedented prison population growth. Blumstein's (2002) report of incarceration rates from 1980 to 1997 showed the number of prisoners per 100,000 that were serving time for drug offenses grew from 15 to 148, more than a ninefold increase. In actual numbers, over 800,000 inmates were added to federal and state correctional facilities from 1990 to 2001, putting total incarceration figures at approximately 2 million inmates (Beck, Karberg, & Harrison, 2002). This not only put great logistical strains on local, state and federal corrections systems but financial strains as well. Indeed, local, state, and federal correctional systems spend over \$9 billion annually to cover the costs of incarcerating drug offenders (Beatty, Holman, & Schiraldi, 2000).

Correctional institutes were not only becoming more crowded, but were also offering less to their residents during the War on Drugs era. An examination by Petersilia (2003) highlighted how ideological shifts in corrections have resulted in fewer treatment opportunities for inmates. Incarceration figures showed that in 1996 only 18% of drug addicted inmates actually received treatment while incarcerated, which was down from 22% in 1993 (National Center on Addiction and Substance Abuse, 1998). Additionally, the Department of Health and Human Services national survey of substance abuse treatment found that 45% of state prisons and 68% of jails had no substance abuse treatment of any kind (Government Accountability Office, 2001). The rate of substance abuse treatment received while under community supervision mirrored that of prison inmates. Bonczar (1997) found that while 50% of those under probation services have

substance abuse treatment requirements, only 17% actually received these services. Further, Taxman and Bouffard (2003) argued that most of these services do not match the offenders' level of need, with many providing only non-clinical services. As Petersilia (2003) stated, reducing the availability of treatment both during and after incarceration stays reduces the probability of successful reentry by offenders.

Efficacy of U.S. Drug Control Policy

With the dramatic shift in policies and practices occurring during the War on Drugs era, there is cause to question whether these changes had any influence on drug markets. Several measures have been used to assess the efficacy of the U.S. drug control policy. One such measure is drug availability. Reuter and Kleiman (1986) contend that drug prices are a reasonable proxy for drug availability, assuming that illicit drugs follow a supply and demand market model. Examining the price of heroin and cocaine between 1981 and 1995, MacCoun and Reuter (1998) reported that prices have steadily *declined* during these years, indicating no effect of the heightened enforcement conducted during the War on Drugs era. Interestingly crack cocaine, which has been argued to be the target drug during this time period (Tonry, 1995) was no more expensive than powder cocaine. Almost 25 years after these initial findings, cocaine, crack and heroin were found to be one-fifth of their price compared to 1980 prices (Alcoholism and Drug Abuse Weekly, 2004). Following the economic model, it is possible that the decline in prices was a signal of lower demand. MacCoun and Reuter, however, note that consumption remained stable during this time period or as will be shown in the following paragraphs, increased for

some substances. Therefore, it is most likely that the price decrease was a result of increased supply rather than decreased demand.

Another way to measure the effectiveness of U.S. drug control policy is to examine trends of drug consumption. Two national substance abuse surveys, the National Household Survey on Drug Abuse (NHSDA) and Monitoring the Future (MTF), provide valuable measures on drug consumption. The NHSDA has been the primary source of information on the prevalence and incidence of illicit drug, alcohol and tobacco use in individuals over age 12 (Substance Abuse and Mental Health Services Administration, 1999). This survey was distributed to over 70,000 respondents across all demographics attempting to gauge substance use on both the state and national level. Because the survey has been used for the last three decades, it also allowed for the examination of substance use trends. Looking at drug consumption, the 1999 NHSDA showed that drug use in the month prior to the survey among all age categories experienced a decline since the mid-1980's with a slight upturn in drug usage in the late 1990's. However, figures on the number of new users showed significant increases from 1990 to 1998. During this period, there was a 37% increase in new cocaine users, a 63% increase in marijuana users, and a 92% increase in hallucinogen users. These findings are troubling because cohorts with high rates of initiation are likely to carry with them a high risk of dependence as they age. Similarly, the annual MTF survey among 12th graders showed that illicit drug use peaked in 1979, declined steadily during the 1980's, and began to increase in the early to mid-1990's (Johnston, O'Malley, Bachman, & Schulenberg, 2004). This survey is an ongoing study of the behaviors, attitudes and values of American secondary school students, college students and young adults. Like the NHSDA, the MTF

found cocaine use greatly increased during the 1990's. Figures showed that during the 30 days prior to the survey, the number of those who used cocaine tripled and the number who used heroin doubled from 1991 to 1998 among 12th graders. Use of any illegal drug in the last 30 days had a similar pattern. Approximately 16% of 12th graders used any illicit substance in the past month in 1991 but in 1997 nearly a quarter of all respondents had tried an illegal substance 30 days prior to survey completion.

Examining substance use among high school students, however, does not give an accurate portrayal of those in other demographics. For example, the criminal offending population has historically been involved in greater illicit substance use than non-offending populations (White and Gorman, 2001). Perhaps the best way to measure substance use among offender populations is through arrest data. The National Institute of Justice's (NIJ) Arrestee Drug Abuse Monitoring (ADAM) program, which in 2001 conducted drug tests of urine samples voluntarily given by arrestees in 33 participating jurisdictions, highlighted the disproportionate number of substance users who enter the criminal justice system. According to 2001 figures, approximately 63% of both male and female arrestees tested positive for one or more of the following drugs; opiates, marijuana, cocaine, methamphetamines, or PCP (Office of National Drug Control Policy, 2003). The range of arrestees testing positive for one of these five drugs by city was approximately 40% to 80%, suggesting significant jurisdictional variation. Among males, the most likely drug of choice was marijuana, followed by cocaine and opiates. For females, cocaine was the most popular drug of choice followed by marijuana and methamphetamines. These results mirror 1999 ADAM findings in which 50% to 77% of

all male arrestees and 22% to 81% of female arrestees tested positive for one of the five aforementioned drugs.

With a large number of those arrested testing positive for drugs, it is of little surprise that a large number of incarcerated individuals with drug problems have filled our correctional institutes. Mumola (1999) reported that over 83% of state prisoners and 73% of federal inmates used drugs in the prior year of being interviewed. Moreover, half of state prisoners and a third of federal prisoners were intoxicated with alcohol or drugs while committing their offenses that led to their incarceration. Research has shown, however, that time in prison does little to stem the return to drug use and criminal activity of this population. Langan and Levin (2002) found that over two-thirds of offenders, including drug offenders, are arrested for a new crime within three years of their release from prison. Additionally, a study done by Marlowe, Patapsis, and DeMatteo (2003) found that 85% of drug offenders returned to drug use within one year of release from prison with all but 5% returning to drug use within three years (Marlowe et al, 2003). These figures suggest that incapacitating offenders without (or possibly even with) providing treatment has done little to change their addictive behavior (MacKenzie, 1997). Moreover, housing this population has put a great strain on correctional facilities' ability to meet the health and treatment needs of this convicted group (Petersilia, 2003). This is particularly true for local jails, which are the first stop for all apprehended offenders, but usually the least equipped for their needs (Frase, 1998).

In sum, the drug retail and drug usage literature have shown the U.S. drug control policy throughout the last two decades to have had minimal success on these measures. Drug prices did not experience an increase during this time period and continue to remain

low to the present day. National substance abuse surveys found that usage of illicit drugs steadily increased from the early to late 1990's, particularly among America's youth. Moreover, the U.S. drug policy has particularly been ineffective within the criminal subpopulation in which figures show more than 6 out of 10 arrestees test positive for an illicit substance. As stated, ideological shifts have led to an increased criminalization of drug use, harsher sanctions, and reduced treatment availability. In sum, these tactics have had little success in changing the economics or consumption of illicit drugs.

A Different Approach: Drug Treatment Courts

An inherent result of increased criminalization of drug use and adopting policy that does not assist in changing offender behavior is a large influx of offenders entering the criminal justice system. Courts soon realized this phenomenon during the mid to late 1980's when drug offenders began to swell up court dockets (Belenko, 2000). Some jurisdictions responded to this problem by dedicating specific courtrooms to deal with drug offenses in an effort to speed up case processing time (Goldkamp, 2000). These courts, however, did not contain treatment mandates and the "revolving door" phenomenon continued. It was not until 1989, in Miami, Florida, that a dedicated drug offense court began to require treatment of drug offenders as part of their sanction mandate. This served as the beginning of the drug treatment court movement.

Drug Treatment Courts (DTCs), like other specialized courts, attempt to improve the judiciary's response to drug offenders by increasing coordination between criminal justice and social service agencies, emphasizing the need for defendant treatment, and separating and distinguishing the court process from the traditional court process (Gover,

MacDonald & Alpert, 2003). DTCs are based on the model of therapeutic-jurisprudence. The underlying premise of this model is that legal procedure and settings have a therapeutic quality. Fiorentine, Hillhouse, and Anglin (2002) stated that in this model, due process procedures can help the offender realize and understand their criminal behavior, help them recognize that they must take responsibility for this behavior, and begin initiating the process of change. In this respect, the legal system acts as a catalyst for changing offender behavior.

Since the first DTC was created in 1989, the number of such courts has increased at a rate of more than 90 being implemented per year. Fifteen years later, figures from the Office of Justice Programs (2005) showed that 1,262 drug courts were in operation with an additional 575 planning to open in the next couple of years. All 50 states, the District of Columbia, and three U.S. territories have at least one drug court in operation and 53 drug courts have also been implemented in Native American territories (known as tribal drug courts) where substance abuse is far greater than the U.S. average (U.S. Department of Justice, 1998).

Though the focus of a drug court may vary by jurisdiction, they have similar goals. According to Cooper and Trotter (1994), these include reducing drug use and associated criminal activity by mandating drug treatment and providing ancillary services, concentrating expertise about drug cases into a single courtroom, addressing other defendant needs through clinical assessment and effective case management, and freeing judicial, prosecutorial and public defense resources for non-drug cases. DTCs help judges become thoroughly knowledgeable about drug offender habits and behaviors, which increases the likelihood of engaging the offender in the best treatment for their

addiction. The target population for most DTCs is non-violent drug offenders with a severe addiction problem (for exceptions, see Saum, Scarpitti, & Robbins, 2001).

Typically, these individuals would receive probation or short jail sentences if they were to be processed through traditional criminal court. This sentence would likely result in minimal treatment involvement or close community supervision (Taxman, 1999). Thus, the DTC offers offenders a greater chance for receiving help for their addiction and provides the tools to accomplish this goal that traditional case processing would not.

Drug treatment courts deviate somewhat from past drug control policy practices and in doing so, address their shortcomings. First, by mandating treatment, DTCs recognize that an offender's drug use will not desist unless intervention is offered. This departs from the just deserts model that guided drug policy in the last two decades and the "new penology" movement that has permeated corrections during the same time period. Feeley and Simon (1992) argued this latter movement is characterized not by punishment or rehabilitation but rather by identifying and managing unruly groups. It emphasizes aggregate control and system management rather than individual success or failure. DTCs, however, take great strides to identify individual achievement by conducting individual status hearings on a regular basis and having graduation ceremonies for those who successfully complete the program. Second, DTCs offer an alternative to prosecution or incarceration. In pre-conviction DTCs, offenders have their charges suspended upon enrollment of the program and eventually dropped upon program completion. In post-conviction DTCs, the offenders' incarceration sentences are suspended upon program enrollment and dismissed upon program completion. This not only provides an incentive for treatment completion, but also helps alleviate the strain on

correctional systems that have problems with overcrowding. Finally, DTCs allow offenders to serve their sentences within the community. This arrangement helps offenders maintain relationships with family and other support systems that assist them in the rehabilitation process (Petersilia, 2003). In sum, these program characteristics support Belenko's (2002) claim that DTCs represent a dramatic shift in jurisprudence and treatment-criminal justice linkages from what characterized drug control policy during the last three decades.

DTCs, however, are not completely divorced from past drug policy. These courts still adhere to the deterrence philosophy by invoking graduated sanctions for violations of their drug court contract. That is, sanctions for non-compliance become more severe with each occurrence. These sanctions can range from a verbal warning to imposing the offender's entire suspended incarceration sentence. Evidence suggests that DTCs may in fact be more punitive than standard probation. Gottfredson, Najaka, Kearley, and Rocha (2005) found that DTC subjects were incarcerated significantly more often than standard probationers during a three year follow-up. Additionally, DTCs recognize that supervision is required throughout participation, with clients in most jurisdictions being monitored by probation officers while serving their sentence in the community. Supervision requirements are also based on a graduated sanctions system. Requirements such as office visits and drug testing can become stricter if offenders violate their DTC requirements.

DTC Components- Supervision and Treatment

As mentioned in the previous paragraph, one central component to all drug courts is the supervision of the offender, which in most jurisdictions is managed by probation officers. These officers are solely assigned drug court cases in order for them to become thoroughly familiar with the drug courts' function, goals, and practices. These officers monitor urine testing results, client treatment attendance, and involvement in criminal activity, in addition to assisting the search for employment and other ancillary services. DTC clients are subject to similar requirements as those on traditional probation. These include staying drug and crime free, avoiding association with criminal peers, volunteering in community activities and finding employment. Unlike traditional probation, DTCs use status hearings to review the involvement of the offender in these activities. The hearings consist of the offender and his or her case manager meeting with a drug court judge on a scheduled basis to evaluate client progress. Status hearings consist primarily of judges reviewing reports from treatment and correctional personnel to assess program compliance and conferring with the case manager about client progress. Additionally, unlike traditional criminal courts, the DTC judge and offender speak directly to one another during these status hearings, giving the offender a voice in his or her rehabilitation process (Belenko, 2002). Status hearings also act as the venue for adjustment of program supervision requirements. If a penalty or sanction is necessary to address non-compliance, drug court judges attempt to assign sanctions that are designed to augment or enforce treatment regimens rather than simply punish offenders (GAO, 1997). Conversely, if a client appears to be making progress, the judge may lessen supervision requirements (e.g. fewer drug tests, fewer probation officer meetings). These

hearings also allow for the client to receive positive reinforcement from judges and case managers and provide a means to respond to incidents of relapse (Taxman & Bouffard, 2003).

The second central component to DTCs is drug treatment. The main objectives of this treatment are to provide a comprehensive rehabilitation program that targets the offenders' addiction(s), supports pro-social behavior, and assists in re-entry to the community (Banks & Gottfredson, 2003). Belenko (2002) and Taxman and Bouffard (2003) noted that DTCs employ two basic treatment-delivery models: referral to multiple, existing, community-based programs usually selected based on the client's needs or geographic location or the use of treatment slots from a single provider that treats all clients regardless of the type or intensity of substance abuse problem. In general, drug courts have access to one or more residential, intensive outpatient, inpatient, or methadone maintenance programs (Peyton & Gossweiler, 2001). The type of treatment the offenders receive is often based on the severity of their addiction, with inpatient services and residential treatment being reserved for those with special needs. Treatment programs are typically one year in length, however, stay in treatment can be extended if the DTC judge feels it is warranted.

Gottfredson, Najaka, Kearley and Rocha (2002) stated that the treatment component of DTCs has three primary goals. The first is to eliminate the physical addiction through a detoxification period. Second, the treatment regimen addresses the psychological elements of the addiction through counseling, medication, drug education, or peer mentoring or a combination of these strategies. Finally, assigned treatment provides ancillary services such as increasing offenders' education level and employment

status. General education, vocational training and job placement services are generally offered during this final phase of treatment (GAO, 1997). Most DTCs attempt to begin the treatment process as soon as possible after arrest, as earlier treatment intervention significantly predicts client outcome (Najaka, Rocha, & Gottfredson, 2003).

The State of Drug Court Research

Evaluations of the many facets of DTCs are far from exhaustive. Turner, Longshore, Wenzel, Deschenes, Greenwood, Fain, Harrell, Morral, Taxman, Iguchi, Greene, and McBride (2002) stated we simply do not know enough about how drug courts work and which components are the most influential. The authors called for greater attention to the “conceptual ingredients” that lead to drug court success. Similarly, Goldkamp (2000) and Longshore, Turner, and Wenzel (2001) found in their reviews of DTC evaluations that gaps still remain in the effective components of DTCs, including client, organizational, and system factors. Recently, DTC researchers have noted that to address these critiques, further attention must be paid to the treatment component of DTCs. Turner et al. argued that evaluating the treatment components with more rigor is the only way to untangle the drug court package. Belenko (2001, 2002) stated that despite being a treatment-focused intervention, there has been relatively little research on the differential impact of different treatment models. Additionally, Taxman (1999) argued that little attention has been given to the role of the treatment process on drug court client compliance, retention and outcomes. Taxman and Bouffard (2003) aptly noted that with little information on the organizational and structural components of the drug treatment

services offered in drug courts and their impact on client outcomes, an understanding of the drug court model is limited (pg. vi).

Prompting such critiques is the limited extent to which treatment variables have been included in DTC evaluations. Most studies that have included treatment variables in predicting outcomes have examined the amount of time spent in treatment (Deschenes, Turner, & Greenwood, 1995; Saum et al, 2001; Goldkamp, White, & Robinson, 2001; Rempel & Destafano, 2001; Anspach & Ferguson, 2003; Rempel et al. 2003), or the percentage of clients that received treatment (Deschenes, Turner, & Greenwood). 1995; Harrell, Cavanagh, & Roman, 1998; Goldkamp et al., 2001; Peters & Murrin, 2000; Gottfredson, Najaka, & Kearley, 2003), with only two studies (Saum et al., 2001 and White, Goldkamp, and Robinson, 2006) examining participants' treatment modality. Additionally, the predictive strength of treatment variables is still in question. Broad DTC reviews (Belenko, 1998, 1999, 2001) suggested that drug court increased time in treatment, which increased the probability of better outcomes (drug court graduation or lower arrest rate). However, two studies (Saum et al., 2001; Rempel et al., 2003) found treatment length not to be a significant predictor of graduation or post-program recidivism while Banks and Gottfredson (2003) found treatment to be the most influential DTC component.

In an attempt to understand the nature of treatment delivery within DTCs, a recent topic of scholarly interest has been the evaluation of treatment service structure and characteristics. These evaluations (Bouffard & Taxman, 2004; Cooper, 2001; Peyton & Gossweiler, 2001; Taxman & Bouffard, 2002, 2003; Wenzel, Turner, & Ridgely, 2004) have utilized survey and observational methodology to explore the treatment counselor

philosophy, the implementation of treatment services, the integration of core concepts of treatment in the operational practices of treatment providers, the collaborative linkages between DTCs, treatment providers, and other social services, and the availability of treatment modalities within DTCs. The objectives of these evaluations were to explore the actual nature of services provided to offenders and to identify specific programmatic components that influence client outcome. Generally, these studies found a variety of treatment services being offered to DTC clients accompanied by varying treatment philosophies and approaches. Additionally, treatment service availability appears to have grown as the popularity of DTC continues; however, little is still known about the who, what, where and how of these treatment services (Taxman and Bouffard, 2002).

The above scholarly works provide valuable insight about the treatment component of DTCs, yet these studies have limitations and leave many issues unexplored. As stated, limited studies included treatment modality in explaining client outcomes. As Anglin and Hser (1990) found in their broad review of offender drug treatment, success varies by treatment modality. Yet the literature has not examined whether this holds true for DTC eligible offenders.

The current study addresses this shortcoming with data collected for a randomized evaluation of the BCDTC. These data, gathered from criminal justice sources, treatment agencies, and self-report interviews, provide a unique chance to explore performance by treatment experience among a criminal drug offending sample. Two hundred and thirty five offenders participated in the BCDTC evaluation and 128 of these individuals received at least one form of certified treatment. Using survival analysis, this work will explore the survival rates by treatment modality and the number of treatments

experienced during the three year follow-up period. The following chapter reviews the research literature examining drug treatment in both DTC and non-DTC environments. The methods chapter describes the BCDTC in detail, the sample to be evaluated, the proposed research questions, and the analyses used to answer the proposed research questions. This is followed by the results of the above analyses and the conclusions that can be drawn from these results, their limitations, and how future research can further explore the influence of drug treatment on crime.

Chapter 2: Literature Review

Introduction

This chapter summarizes the current DTC and non-DTC research literature that focuses on drug treatment effects on client outcome. Presented first are large scale studies and reviews focusing on the efficacy of drug treatment, followed by research that has focuses on specific treatment modalities. Focus then switches to the literature addressing the efficacy of DTCs, followed by the studies that specifically looked at the treatment components of DTCs. Studies using survival analysis are then highlighted due to the current study's use of this statistical technique. The chapter concludes with a summary of how the current study will fill the gaps in the current DTC research.

Drug Treatment Effectiveness on Reducing Illicit Activity

Supporting the integration of substance abuse treatment and the criminal justice system are the consistent findings that drug treatment is effective in reducing drug use, criminal activity, and changing criminal career paths. This section reviews large scale literature reviews, meta-analyses, and studies using national level data that demonstrate the effectiveness of substance abuse treatment among offender populations.

Wish and Johnson (1986) examined the drug treatment literature to review the influence of substance abuse treatment on criminal careers of primarily cocaine and heroin users. They found that offenders may be helped if they are mandated to participate in treatment that is accompanied by strict supervision and drug surveillance. In regards to the latter, Wish and Johnson cited urinalysis testing to be an effective tool in identifying drug using offenders, however, they conceded that more needs to be learned

on how to use this information to manage clients. Recognizing that what intervention works best for what individuals, and at what phase of the criminal justice process, is still an unknown in the drug/crime literature, however, the authors' review of the literature does firmly conclude that longer treatment participation decreased the likelihood of future criminal activity.

Lehmen and Simpson (1990) examined a sample 12 years after participation in the Drug Abuse Reporting Program (DARP) and focused on the criminality of subjects before, during, and after active addiction phases, as well as their criminal involvement during the 12th year of post-program completion. As expected, involvement in all crime types (violent, property, drugs, and prostitution) was highest during years of active addiction. However, involvement in these activities substantially decreased after this phase. Overall, there was approximately a 50% reduction of individuals involved in illegal activity from active to post addiction, with crimes against property experiencing the largest reduction (73% of the sample was involved during addiction while only 13% were involved after). Interviews conducted during year 12 showed that these reductions were not fleeting. There was only a 6% percentage increase (47% to 53%) from the post-addiction phase to year 12 of those involved in any illegal activity, with involvement being similar to pre-addiction levels. Unfortunately, the authors did not analyze the reduction of criminality by treatment modality.

In one of the most encompassing meta-analyses of correctional treatment intervention research to date, MacKenzie (1997) found that there was substantial evidence rehabilitation programs work in changing offender behavior. Meta-analyses allow for a quantitative research synthesis that summarizes, integrates and interprets

selected scholarly works (Lipsey & Wilson, 2001). Meta-analyses are more objective than traditional literature reviews and allow for determination of the statistical relationship (the effect size) between treatment intervention and recidivism (Cullen, 2002). In Mackenzie's work, programs that were most effective were those that were structured and focused, used multiple treatment components, focused on developing skills and behavior methods and allowed for substantial and meaningful contact between provider and client. She also found that substance abuse treatment "works" for reducing drug offender recidivism, particularly prison-based therapeutic community treatment. Successful programs were those that had strong implementation, highly trained staff, and took into account the learning styles and abilities of its target population. As previously suggested by Wish and Johnson (1986), MacKenzie also stated that effectiveness of drug treatment is directly related to the length of time an individual remains in treatment. The longer the treatment stay, the better the chances of changing offender behavior. Germane to the current study, Mackenzie also found that drug treatment combined with urine testing reduced recidivism among drug court participants.

Examining MacKenzie's (1997) review and other large scale evaluations, Taxman (1999) concluded that drug treatment was a vital component to change criminal behavior of drug offenders. Review of several meta-analyses showed that correctional industry programs, vocational education, and in-prison therapeutic communities with aftercare worked in reducing drug offender recidivism. Taxman's review noted three factors that seemed to separate successful programs from those that were less effective. First, successful programs employed a therapeutic emphasis that helped offenders change their behavior. Second, effective programs had long periods of participation and multiple

levels of care, allowing offenders to understand and implement the treatment message. Finally, successful programs utilized the leverage of the criminal justice system to sustain retention. Taxman noted that these factors illustrate the importance of considering and understanding the “action” in treatment services, such as program characteristics, duration, and number of treatment sessions. Additionally, Taxman argued that effective treatments need to provide proper assessment, use specific treatment criteria, address treatment readiness, and use behavioral contracts and behavioral incentives and sanctions. She concluded that these components are often missing in many programs and service systems, limiting their efficacy.

Prendergast, Podus, Chang, and Urada (2002) also conducted a meta-analysis examining the effectiveness of drug abuse treatment on future drug use and criminal activity. In their work, Prendergast et al. included only evaluations that had a non-treatment comparison group, resulting in 78 studies published between 1965 and 1996. Results showed drug abuse treatment having both a statistically significant and clinically meaningful effect in reducing drug use and crime, and that these effects were unlikely to be due to publication bias. Treatment programs tended to show larger effect sizes in studies in which treatment implementation was rated high, the degree of theoretical development of the treatment was rated low, or researcher allegiance to the treatment was rated as favorable. The authors also found that treatment was effective on crime outcomes, with treatment reducing crime to a greater degree among studies with samples consisting of younger adults as opposed to older adults. Prendergast et al. found no relationship between treatment modality and future drug use and criminal behavior.

Treatment Modality Effects Found in Non-DTC Research

Contrary to the Prendergast et al. (2002) findings, other studies conducted in non-DTC settings suggest that different treatment modalities do have varying effects on outcomes. Anglin and Hser (1990) review the effects of drug treatment on future drug and crime involvement using the DARP data, Treatment Outcome Prospective Study (TOPS) data, and individual program evaluations. Their review examined four community based treatment modalities; methadone maintenance, therapeutic communities, outpatient treatment, and detoxification. According to Mieczkowski, Anglin, Coletti, Johnson, Nadalman, and Wish (1992), over 90% of clients enrolled in public funded treatment are participants in one or more of these modalities. Examining results by modality, Anglin and Hser (1990) found methadone and therapeutic communities to be the most effective modes of treatment. Methadone studies showed daily narcotic use dropped by as much as 70% during treatment, while the number of property crime incidences and days of property crime involvement also dropped significantly. Similarly, therapeutic community evaluations consistently showed that participants had lower drug and criminal involvement and higher measures of pro-social behavior in both immediate and long-term follow-up periods. Outpatient treatment, however, did not show much effect on either drug use or crime. Indeed, results of studies focusing on this modality showed similar or even higher involvement in these activities during outpatient treatment. Limited evaluations of detoxification treatment did not allow for a firm conclusion on its' influence on drug use and criminal activity.

Anglin and Hser (1990) concluded that drug abuse treatment is effective in reducing drug and criminal activity, but future programs must incorporate four structural

features in order to change client behavior. These include providing lengthy treatment intervention, providing significant level of structure, being flexible, with no absolute mandates determining client management, and conducting regular evaluation to determine effectiveness and need for change. Additionally, programs need to provide psychiatric and ancillary services to confront the many problems that are associated with addiction.

Drug treatment effectiveness has also been well documented by the Drug Abuse Treatment Outcome Studies (DATOS) conducted by the National Institute on Drug Abuse (NIDA). Initiated in 1990, this series of studies collected data on over 10,000 participants of substance abuse programs. DATOS included 96 programs in 11 cities examining 4 modalities of treatment: residential, short-term in-patient, methadone maintenance, and outpatient. Hser, Grella, Chou, and Anglin (1998) conducted follow-up interviews with a subset of these clients approximately one year after discharge. The authors found that length of stay in treatment was significantly related to reduced cocaine use (but not heroin use) among clients in outpatient drug free and residential programs and reduced heroin use among clients in methadone maintenance. The authors also found evidence for cumulative treatment effects. Specifically, longer treatment careers were associated with increased length of stay in outpatient programs and were associated with reduced heroin use among clients treated in all modalities except for residential programs. These findings support the notion that treatment effects may accumulate across multiple treatment episodes.

Hubbard, Craddock, Flynn, Anderson, and Etheridge (1997) also used the DATOS data to explore further the effects of treatment modality on reductions in drug

and illegal activity. Following up approximately 3,000 participants after one year in the study, the authors selected a larger portion of clients who received at least three months of methadone, residential or outpatient treatment or one month of short-term inpatient. This ensured that participants would be engaged in treatment for a meaningful amount of time. Findings showed the prevalence of weekly or daily cocaine use was half of that in the preadmission year for those involved in residential and outpatient treatment. Reduction was greater for those involved three months or more. Less weekly or daily heroin use was also reported by those in methadone programs compared to those who left such programs. Multivariate analyses showed that 6 months or more in residential programs resulted in a 50% reduction in illegal activity and a 10% increase in employment. But even shorter stays may still be influential. Examining a separate DATOS sub-sample, Simpson, Joe and Brown (1997) reported that participation in residential treatment for as little as 3 months, however, still yielded better outcomes in all key areas of behavioral functioning than did earlier program dropouts.

Project MATCH (Matching Alcoholism Treatment to Client Heterogeneity) is one of the largest and most complex randomized clinical trials conducted on alcohol treatment to date (Project MATCH Research Group, 1993). The purpose of this project was to determine if varying types of alcohol-abusing and alcohol dependent patients respond differently to alternative interventions. This three phase evaluation spanned over six years and focused on three types of treatment interventions (provided in outpatient and aftercare settings): Twelve Step Facilitation (TSF), Motivational Enhancement Therapy (MET), and Cognitive-Behavioral Coping Skills Therapy (CBT).

The Project MATCH Research Group (1998) first explored the varying treatment modality effects on client outcome with a sample of 952 randomly assigned outpatient sample members. With regard to overall outcome, there were significant reductions in drinking after the first year of treatment for all treatment types and these reductions were sustained over the three year follow-up period (30% of subjects were totally abstinent in months 37 to 39). Focusing on drinking frequency and drinking intensity outcomes, the authors found that TSF clients attained higher rates of abstinence three years post randomization. In terms of duration and intensity, TSF members had 8% more days abstinent and scored lower on the drinking intensity scale than the other modalities. These results were found after the one year post treatment and though the difference among modalities waned by the three year post treatment follow-up, TSF continued to show a slight advantage over the other treatment interventions. The authors found that those who were in the TSF group were more likely to attend Alcoholics Anonymous (AA) during the follow-up period than those in the other interventions. This continuation of treatment may explain the greater success of the intervention group.

Drawing parallels to Project MATCH results, Donovan (1999) examined the three Project MATCH treatment modalities using data from the Department of Veterans Affairs. His findings supported Project MATCH results. TSF program participants had a higher percentage of days in which participants were abstinent for the one year follow-up period. Additionally, those in the TSF programs had fewer alcohol-related problems and were more likely to be employed at the one year follow-up than in the other treatment modalities. A shortcoming of Donavon's work and that of the Project MATCH evaluation was the lack of no-treatment comparison group. It could be argued that clients could not

have participated in any of the treatments but may have performed just as well as Donavon's or Project MATCH clients, mitigating the support for treatment effects. Addressing the shortcoming of these two studies, Moos, Finney, Quimette, and Suchinsky (1999) used Donavon's results to compare with outcomes from an earlier study that had a no-treatment group. Their results found that all treatment participants performed better than the comparison group and that more treatment was associated with better outcomes. Moos et al.'s work, however, only reported on treatment as a whole and did not break down success by treatment intervention.

Another large scale review that explored treatment modality effects on illicit activity was the National Treatment Improvement Evaluation Study (NTIES). This study was a national evaluation of the effectiveness of substance abuse treatment services delivered in comprehensive treatment demonstration programs supported by the Center for Substance Abuse Treatment (CSAT) (Ellis, 1993). This evaluation focused on five treatment settings designed to reduce both drug and alcohol abuse: methadone maintenance, non-methadone maintenance outpatient, short-term residential, long-term residential, and treatment in a correctional setting.

NTIES explored treatment provided to all substance users to determine which treatment settings and what treatment components have a positive impact on clients' abstinence from substance use. Using data derived from client interviews at treatment intake, treatment exit, and 12 months post treatment exit, the NTIES provided a wealth of information on treatment modality outcomes. In their analysis of treatment for cocaine and crack-cocaine dependence, Johnson and Gernstein's (2000) examination of the NTIES data found that correctional based programs had the largest percent reduction in

use of the two drugs, while methadone facilities had the smallest. Explanations for this finding include the requirement to attend the correctional treatment for criminal justice clients, as well as methadone being more effective for heroin users, rather than cocaine users. All treatment modalities showed that longer duration stays and more intense treatment had a positive effect on post treatment success. This result was supported by Orwin, Ellis, Williams, and Maranda (2000), who found that, with the NTIES sample, that longer treatment stays are beneficial to the client even if the treatment is not completed. Interestingly, Johnson and Gernstein found that in all types of modalities, respondents who had fewer, rather than more, prior treatment episodes had a greater reduction in post treatment cocaine or crack use.

Orwin et al. (2000) expanded the treatment modality literature with the NTIES sample by exploring the relationship of treatment modality and violent behavior. The authors cited a paucity of knowledge about the effect of treatment on specific violent behaviors as an impetus for their work. Using a sample of 4,411 NTIES clients that were actively violent prior to treatment, Orwin et al. examined four types of violent behavior: robbery, assault with a weapon, assault without a weapon, and severe injury to someone in another way. Reductions were found in involvement in all violent categories with all modalities as a whole, but modality differences were generally small and no modality showed consistent reductions across all four behaviors. The authors concluded that overall, substance abuse treatment does negatively affect violent behavior rates, however, specific modality linkages to this type of behavior are still nebulous.

Drug Treatment Courts

As Taxman (1999) stated, the leverage of the criminal justice system has the potential to improve outcomes in treatment programs. Most DTCs have several coercive elements to try to maximize this leverage. These include close judicial supervision and monitoring, regular drug tests, and graduated sanctions. In DTCs, defendants may also feel pressure to participate in a program because of the consequences of prosecution (Belenko, 2002). This section will explore whether the DTC structure of leveraged coercion into treatment has been influential on changing client behavior by presenting broad reviews of individual DTC evaluations and evaluations using high quality research designs. This section will conclude with research that has focused on the treatment delivery services of DTCs.

In 1997, the GAO conducted the first review of DTC evaluations in an effort to understand the characteristics, structure, and effectiveness of DTCs. Examining 20 evaluations of 14 DTCs nationwide and survey data of 134 operating DTCs, the report concluded that existing evidence was insufficient to draw any firm conclusions on the effectiveness of these programs on recidivism or drug relapse. The report cited that DTC evaluations were severely limited by several shortcomings: lack of comparison groups, limited or no follow-up data, no accounts for differing court and treatment operations, and relatively short observational periods. The GAO contended that one or more of these issues were present in each evaluation examined and called for better research design in federally funded DTCs.

Stephen Belenko (1998, 1999, 2001) conducted a series of reviews of DTC evaluations to examine their effectiveness and the general state of DTC research. In 1998, Belenko reviewed 30 DTC evaluations and found positive results in a variety of realms. DTCs appeared to reduce drug use and criminal activity during and after DTC participation, facilitated higher treatment retention rates and more comprehensive supervision of offenders than traditional community corrections options, were cost effective and helped bridge the gap between criminal justice practitioners and drug treatment providers. Belenko, however, tempered these findings by citing several shortcomings with the literature. These included short follow-up periods, lack of attention to outcomes other than recidivism, lack of attention to the treatment process, and the lack of experimental designs. Belenko's (1999, 2001) further reviews of DTC evaluations were generally consistent with his 1998 findings. DTCs continued to show positive results in the previously mentioned areas, including crime and drug use during and after program involvement. The additional number of evaluations, however, did not ameliorate the gaps and limitations found in prior research. Belenko also noted poor data quality and lack of information systems as hindering evaluation conclusions. Additionally, the effects of DTCs could not be considered conclusive because of the weak research designs often used. That is, randomized experiments continued to be the exception rather than the norm (Belenko 2002).

Wilson, Mitchell, and MacKenzie (2002) also conducted a review of DTC evaluations focusing on their effect on recidivism. Differing from Belenko's reviews, Wilson et al. used a meta-analytical framework to determine program effectiveness. Analyzing 38 DTC evaluations that had long term drug use and criminal activity as

outcomes, Wilson and colleagues' meta-analysis tentatively suggested that drug offenders participating in a drug court were less likely to reoffend than were similar offenders sentenced to traditional correctional options, such as probation. Overall offending was reduced approximately 26% by DTCs across all studies. Effects on drug use were more difficult to discern. The vast majority of studies relied on proxy measures of drug use behavior, such as arrest and conviction. Wilson et al. noted that these are not direct measures of client substance use behavior and limited conclusions can be drawn from these variables. The authors concluded that despite the plethora of studies fitting their inclusion criteria, only equivocal statements on program effectiveness can be argued due to the generally weak nature of the research design. Wilson and colleagues reiterated Belenko's (2002) critique that the lack of randomized experiments in DTC research limits firm conclusions on its effectiveness.

Because these large scale studies cite research design weakness as tempering effectiveness arguments, more can be learned by exploring the results of high quality randomized DTC studies. As Belenko (2002) stated, "experimental designs in various drug court models are important to provide more conclusive data on the efficacy and impact of drug courts" (p. 49). The few evaluations that have implemented this design have indicated support for DTCs changing client behavior.

RAND conducted the first randomized drug court experiment in Maricopa County, Arizona. Six hundred and thirty convicted drug offenders were assigned to one of four interventions, one was a drug court program and the remaining three were assigned to varying levels of drug testing requirements (Deschenes, et al., 1995). Results showed that 40% of DTC participants successfully completed the treatment program

within 12 months. Though there was no significant difference of rearrest between the four groups, DTC clients had a lower overall rate of technical violations with fewer drug violations. Upon a 36 month follow-up of the same DTC, results were more promising (Turner, Greenwood, Fain, and Deschenes, 1999). DTC participants continued to have significantly fewer technical violations than the comparison groups and significantly fewer arrests (33.1% vs. 43.7%). Additionally, almost half of the DTC participants performed community service and virtually all participated in counseling. These results suggested that the positive effects of DTCs may not be immediate, rather occur as a gradual process. Therefore, DTC evaluations with lengthy follow-up periods are needed.

The Urban Institute's evaluation of the District of Columbia's DTC randomly assigned pre-trial felony drug offenders to one of three tracks. These included standard processing (drug testing and standard monitoring), a graduated sanctions/drug testing track (drug testing, graduated sanctions, judicial monitoring, and treatment for those who requested it) and a DTC track (same requirements as the graduated sanctions and drug testing track, but mandatory intensive day treatment) (Harrell et al., 1998). As an incentive to participants, the DTC program looked to give pre-trial defendants an opportunity to avoid assignment to jail if they remained drug free prior to sentencing. Results showed that treatment participants (graduated sanctions/drug testing track and the DTC track) were significantly more likely to be drug free the month before sentencing than the standard processing track. This suggested that the graduated sanctions component of DTC may be just as effective whether treatment is required or not. Operational difficulties did not allow for rearrest rates to be analyzed with the DTC track, however, results did show that defendants processed in the sanctions/testing track did

have lower arrest rates (19% vs. 27%) than those assigned to standard court processing. In the year following sentencing for the initial arrest, there was no significant reduction in drug use among the DTC group and the graduated sanctions group. However, both of these groups were significantly less likely to be arrested for a drug crime compared to the standard processing track.

Gottfredson and colleagues conducted a randomized experiment of the BCDTC, with results being reported after each year of the three year follow-up (Gottfredson & Exum, 2002; Gottfredson, et al. , 2003; Gottfredson et al., 2005). In this evaluation, subjects that were eligible for the DTC were randomly assigned to the DTC or a treatment as usual track. Between February, 1997 and August, 1998, 235 individuals were placed in the study, with 139 assigned in the DTC and 96 placed in the comparison group. Each follow-up period showed DTC subjects performing better than their control group counterparts. After year one, DTC participants had significantly fewer arrests (0.9 vs. 1.3), fewer charges (1.6 vs. 2.4) and a lower group percentage of arrest for a new offense (48% vs. 64%). Year two and year three results showed a similar pattern. Specifically, year two rearrest figures favored DTC clients (66.2% vs. 81.3%), as did number of new arrests (1.6 vs. 2.3) and new charges (3.1 vs. 4.6). Third year results showed 78.4% of DTC clients being rearrested for a new offense compared to 87.5% of controls, in addition to having fewer new arrests (2.3 vs. 3.4) and new charges (4.4 vs. 6.1) (Gottfredson, et al., 2005). Recognizing that not all that are assigned to DTC actually attend treatment, Gottfredson et al. (2003) also examined the year two follow-up data by comparing those who actually received treatment in the DTC to the comparison group. Comparisons among the groups on recidivism outcomes during the two year follow-up

showed that treated drug court subjects were significantly less likely than both untreated DTC subjects and control subjects to be re-arrested (56.7% vs. 75% and 81.3%).

Gottfredson, et al. conducted a similar analysis with the third year results by examining arrests of DTC subjects by their amount of certified treatment received. Categorized into three groups (0 days, 1-178 days, and 179 or more days), the difference between the high implementation group and the low implementation group was significant at the .01 level (3.28 vs. 1.68 vs. 1.40 respectively). Moreover, the authors also conducted this analysis with three other drug court components: drug testing, status hearings and supervision. Again, those who received higher implementation of these components fared better than their counterparts, with the differences in the drug testing and status hearing categories being significant.

In addition to the official data collected for the above studies, Gottfredson, Kearley, Najaka, & Rocha (2005a) conducted an evaluation of the BCDTC using self-report data collected three years post-randomization. Interviewers were able to gather information from approximately 70% of both treatment and control groups, resulting in 157 interviews. These data again showed treatment participants performing better than the control group in arrest and drug use outcomes in the last 12 months of the 3 year follow-up. Specifically, treated BCDTC participants were less likely than controls to be arrested in the year prior to their interview (43.0% vs. 64.8%), and they participated in less serious crimes and their most serious crimes were not as severe as the control groups' most serious crimes. Drug use measures also favored the treated DTC participants. DTC clients had significantly fewer days of alcohol and heroin use, had lower measures of drug addiction severity, and used fewer types of drugs.

Interestingly, the authors found that crime and drug use outcomes were influenced by originating court (circuit vs. district). DTC subjects who were processed through the circuit court were significantly less likely to be involved 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 than their circuit court control group counterparts. These differences were not present among the sample that was processed through the district court. A preliminary examination of participant characteristics found no significant differences by court of assignment on age, gender, prior arrests and prior convictions measures. The authors concluded that this suggests that the mechanisms through which the treatment works to reduce crime and substance use were operationalized differently in the two courts.

Focus on Drug Treatment in Drug Courts

The large scale examinations of DTCs and the strong research design studies incorporating randomized experiments presented here suggest that DTCs are effective in reducing in-program and post-program crime and drug activity. DTCs also appear to be cost effective (Crumpton, Brekhus, Weller, & Finigan, 2004), keep subjects involved in treatment for a longer duration, and provide more comprehensive supervision than traditional correction programs (Belenko, 1998). Additionally, drug courts have also been lauded for their ability to integrate the criminal justice system and the treatment sector. Unfortunately, limited attention in DTC evaluations has been given to the treatment component. For example, Taxman (1999) argued “not enough attention is given to the adequate nature of treatment and services being offered [in drug courts]” (p.1669).

Belenko (2002) added that relatively little is known about the delivery of treatment services to drug court clients, which is a partial result of few drug court evaluations being able to incorporate detailed data on drug treatment and other health services and evaluators not recognizing the importance of these data. Also hindering the exploration of the drug treatment component is the greater focus on public safety outcomes. Policy makers' primary concern, particularly in the early years of program implementation, was "does it work?" rather than "how does it work?" DTC evaluations often reflect this viewpoint by placing greater attention on graduation and recidivism results rather than the treatment components that led to these outcomes.

The remaining portion of this section will review the limited number of evaluations that have included some measurement of the treatment components of the DTCs and their predictive strength on client outcomes. Following will be an examination of a series of studies that have explored the nature of DTC treatment program. This section will then conclude with a discussion of the gaps that still remain in the DTC treatment service literature.

Goldkamp et al. (2001) used a quasi-experimental design to examine the influence of drug treatment on program graduation recidivism in two of the longest running drug courts in the nation, Las Vegas, Nevada and Portland, Oregon. The authors conducted a retrospective evaluation using a stratified sampling strategy. From the Portland DTC, 150 participants were selected from 3, two-year time periods (1991-1992, 1993-1994, and 1995-996) and a one year time period (1997). The Las Vegas sample was comprised of 100 randomly sampled DTC participants from each year between 1993 and 1997. Treatment variables included in the models were length of treatment (measured

dichotomously as attending or not attending 50% of expected treatment regimen) and attending treatment (measured dichotomously as attending more or less than 30 times). Results revealed that those who attended treatment for over 90 days were more likely to graduate from the DTC in both jurisdictions. Rearrest (measured as any offense, drug offense, and non-drug offense within one year of program entry) figures, however, showed mixed results. In Las Vegas, those who attended more than half of their required treatment had fewer drug arrests, while those who attended more than 30 appointments with treatment providers were less likely to have any arrests and non-drug arrests. In Portland, time in treatment and number of treatment contacts were not significant predictors of any rearrest outcome.

Goldkamp et al. (2001) postulated that given the mix of rehabilitative and deterrent aims and methods represented by the drug court model, it is conceivable that DTC functions interact to produce an impact on outcomes over and beyond their specific contributions. In Las Vegas, the treatment (number of treatment contacts) and jail (any jail sanction) interaction variable showed those with less than 30 treatment contacts and one or more jail sanctions had a greater chance of any rearrests and non-drug arrests. In Portland, this interaction variable was a significant predictor for each type of rearrest. The authors noted that the two drug court elements- supervision and treatment- wield influence conjointly above and beyond their independent contributions to offender outcomes. Thus, treatment variables alone were not significant predictors of rearrest in Portland; however, the treatment participation/jail sanctions interaction was a significant predictor.

Saum et al. (2001) included treatment involvement as a predictor of program graduation in their evaluation of Delaware's Superior Court DTC. Diverging from most DTC evaluations, the authors used a sample of 452 clients that had extensive criminal histories, including histories of violent crimes. The authors chose this sample to explore whether DTCs were effective in changing criminal behavior of a violent sub-population. Only two treatment variables were included in the analysis: length of treatment (measured as the log transformation of days a client was in treatment) and whether or not the individual participated in a therapeutic community program (measured dichotomously). This was one of the few studies that examined a specific treatment modality as a predictor of client outcome. While multivariate regression results showed that age, crack use, and charge history were significant predictors of the dependent variable, neither treatment variable helped explain drug court graduation. Attempting to explain the non-significant effect of the treatment variables, the authors noted that within the drug court context, time in treatment measures are more complex than in other drug treatment evaluations. As often occurs, a DTC client who is not doing well may be sanctioned with a longer period of treatment; therefore, those with longer treatment participation may be those who experiencing the most difficulty with the programs, rather than the least.

Rempel and Destafano (2001) examined the influence of treatment retention (defined as completing 90 days of treatment) and legal coercion on completing four consecutive months (referred to as Phase 1) of drug-free and sanction-less participation in the Brooklyn DTC. The authors were able to examine two treatment variables; attending more than 90 days of treatment (treatment retention) and prior treatment episodes

(measured as a continuous variable). The authors allowed one year to elapse after program entry to give sufficient time to complete Phase 1. Logistic regression findings showed that longer treatment retention was a significant predictor of four month success, though the treatment modality is unknown. Of those who completed 90 days or more of treatment, 80-85% went on to finish Phase 1. The number of prior treatment episodes did not explain a significant amount of variance in successfully completing the same time period.

Additionally, Rempel and Destafano (2001) measured coercion by the pending charge (misdemeanor, 1st felony, multiple felonies, predicate felony, or having a pending case in family court) the offender would receive if he or she were terminated from drug court. As expected, the higher the level of coercion, the more likely the offender was to complete 90 days of treatment and be successful for four months. However, when controlling for treatment retention, legal coercion was no longer significant in predicting four month success. The only other significant predictor was age, with older participants more likely to complete Phase 1 than younger participants. These findings supported those of Goldkamp et al. (2001) that stated treatment retention strongly predicts client outcome, but also questions the influence of legal coercion throughout drug court participation. These findings suggested that legal coercion may be most influential in the beginning phase of drug court but this influence may eventually weaken.

Rempel et al. (2003) looked at treatment and recidivism in 6 of 11 New York State DTCs. The authors not only examined the treatment/recidivism relationship, but also which characteristics influenced placement in inpatient treatment (defined as residential or short-term rehabilitation). Data limitations did not allow for placement

analyses of other treatment modalities available to the DTCs. Looking first at the treatment placement, results showed that primary drug of choice (heroin, crack, and not marijuana), living situation (homeless at intake and not living with spouse at intake), employment/educational status (neither employed nor in school at intake) and being young were all significant predictors of placement in inpatient services. Although scholars have argued that criminal justice risk should be considered when determining how restrictive a modality to use with court mandated clients (Taxman, 1999), prior conviction was not a significant predictor of placement. Indeed, drug court project staff in three sites expressed that non-clinical factors were irrelevant to a proper determination of first modality and other aspects of the treatment plan.

In their recidivism analysis, Rempel et al. (2003) extended the follow-up period of the Brooklyn DTC previously reported in Rempel and Destafano (2001) to two years post-program. This was the only DTC in their multi-site evaluation that allowed for measurement of the treatment influence on client outcomes. Findings showed that those initially assigned to inpatient treatment had a higher probability of failure, most likely a result of this modality receiving the most serious offenders. Days in treatment were not a significant predictor of post-program recidivism when controlling for graduation/failure status. That is, if it was known whether a participant graduated or failed, it was not important to know how many days of treatment were completed. Recall Rempel and Destafano's (2001) previous analysis that showed 90 days of treatment attendance significantly predicted failure within the first four months of DTC participation. The findings by Rempel and colleagues (2003) suggested that the 90 day treatment threshold does not have a lasting effect at the two year mark. The authors concluded that with their

limited measures, treatment was not independently predictive of subsequent recidivism. They argued that these findings did not make it any more pronounced as to which components (case management, judicial status hearings, or treatment) of the drug court were the most critical in lowering recidivism. But the treatment influence on arrest could not be completely dismissed. Logistic regression results showed that prior experience with treatment before drug court participation led to a lower probability of post-program recidivism. This supports the cumulative treatment argument found by Hser et al. (1998) in their examination of the DATOS data.

Anspach and Ferguson (2003) examined the relationship between treatment attendance and percentage of post-program arrests in four geographically diverse DTCs. Their 12 month post-program follow-up used official data to track the success of 2,357 DTC participants. The authors were able to control for two treatment variables in the analyses: time spent in treatment (measured dichotomously as whether or not clients attended more or less than 70% of their required treatment sessions) and prior treatment (yes or no). Preliminary analyses showed that only 36% of participants met this minimum treatment requirement and that terminated subjects who did not attend the 70% minimum were more likely to be arrested than terminated subjects who completed this standard. Logistic regression results showed that prior treatment was not a significant predictor of post-program arrest but treatment attendance was significant in predicting recidivism in one of the four sites. However, path analyses indicated the effect of treatment attendance on arrest was mediated through drug court graduation in three of the four sites. In other words, more treatment attendance increased the chances of DTC graduation, which was

negatively associated with post-program arrest. Overall, program completion status was the most important factor associated with recidivism.

Finally, Gottfredson, Kearley, Najaka, and Rocha's (2005b) examination of theoretical mechanisms mediating the effects of the BCDTC on drug and crime outcome measure included days of drug treatment as a predictor variable. This study had three dependent variables: a crime variety scale (which combined subject self-reports of whether or not they committed 10 different crimes in the 12 months prior to the interview), a drug variety scale, (which combined self-reports of whether or not they used any of 13 different drug types in the 12 months prior to the interview) and a frequency of multi-drug use scale (which summed the number of times an individual consumed more than 1 drug at a time in the 12 months prior to the interview). Using structural equation modeling, the authors found that days of drug treatment significantly reduced multiple drug use frequency, but had no significant effect on the crime variety and drug variety measures. Further, days in drug treatment directly increased measures of social control, which reduced multiple drug use frequency. Interestingly, the authors also found that drug court participation did not substantially increase the days of treatment above what was available to control subjects. Though other analyses using official records of certified treatment attendance (Banks & Gottfredson, 2003; Gottfredson et al., 2003; Gottfredson et al., 2005) did find program participation increased the amount of treatment received, this same result was not evident in this study that used clients' self-reports.

The above studies are not only limited by the few treatment variables included in their analyses but also by their lack of accounting for variables that are associated with both participating in treatment and the outcomes of interest. Gottfredson et al. (2003)

cautioned in their analysis of the BCDTC that unmeasured factors that lead individuals to seek more treatment may also lead them to commit less crime. Therefore, there may be a correlation between the independent variable of interest and the disturbance term for the dependent variable. Not correcting for this omitted variable problem can result in a biased estimate of the effect of treatment on outcomes of interest (Gottfredson et al., 2005). Gottfredson et al. addressed this obstacle by conducting an Instrumental Variable (IV) analysis, which allowed for the inclusion of a variable that is uncorrelated with the disturbance term and correlated with the independent variable of interest. The variable, random assignment to treatment and control condition, removes from the independent variable the portion of variance that is correlated with the disturbance term (known as an instrumental variable). The author's IV analysis showed that a higher level of participation in the three drug court components (drug testing, status hearings, and certified treatment) reduced the number of arrests, as did originating court, age, and number of prior arrests.

Gottfredson et al. (2005) note that not accounting for omitted variable bias makes it difficult to discern the effects of treatment on client outcomes with certainty. Contributing to this shortcoming is the lack of a variable that can be used as an instrumental variable. This is particularly true in non-experimental designs in which the randomization variable (as used by Gottfredson et al.) does not exist. Though the Gottfredson et al. findings do suggest that DTC components (including participation in drug treatment) are effective in reducing arrest, further DTC research needs to mimic their rigorous methodology in order to be conclusive about the effects of DTC drug treatment on client outcomes.

The Issue of Non-Certified Treatments

Lacking in the majority of DTC studies presented thus far is a focus on the quality of treatments. Mackenzie (1997), however, highlights the importance of strong treatment structure in changing client behavior. Banks and Gottfredson (2003) do address this somewhat in their inclusion of only “certified” treatments (as deemed by the Baltimore Substance Abuse System (BSAS)) in their analysis of the BCDTC. The authors note that these treatments are categorized by BSAS as non-certified because they do not include a multi-phasal component, a commitment on the part of the drug treatment recipient, counseling sessions, or other “traditional” treatment components. Jail based acupuncture and twelve-step programs such as AA and Narcotics Anonymous (NA) are two such treatments. As the current study focuses on treatment modalities using the same data as Banks and Gottfredson, it is important to examine what the literature has shown in regards to these modalities.

Research exploring these two treatments and their influence on subsequent criminal behavior is limited in the drug court literature. The use of acupuncture in a drug court setting has been the subject of only one study conducted by White et al. (2006) in their evaluation of the Clark County, Nevada DTC. Their prospective modified experiment randomly assigned 336 drug court participants to acupuncture and no acupuncture groups. Findings showed no significant differences between the groups on several criminal measures, including time to re-arrest. Limitations of this study include a brief follow-up period (6 months) and lack of fidelity to the experimental design (40% of control group subjects received at least one acupuncture session).

Though AA and NA have been the subject of a large amount of research, no study has examined this modality in the drug court setting. Even in a broader context the effects of such twelve step programs on future substance abuse evaluations is mixed. As previously mentioned, Donavon's (1999) work did find positive outcomes for these types of modalities but larger, more encompassing studies have less conclusive results. For example, Kownacki and Shadish (1999) conducted a meta-analysis of controlled experiments looking at AA. The authors highlighted that there was very little quality research examining this program. Of the experiments that were included in the analysis, results showed AA participants performed no better than control groups in terms of abstinence and in some cases had worse outcomes than non-AA participants. Gossop, Stewart and Marsden (2008) concluded in their review of AA/NA participants that these programs are perhaps better suited as supplemental treatments rather than the primary drug treatment. Their longitudinal design study found that AA/NA participants did have higher abstinence rates for opiate use, but there was no overall change in alcohol abstinence.

Studies Using Survival Analysis

One drawback of the DTC studies presented thus far is that almost all have utilized static analytical methods that do not examine the time until the outcome of interest. The survival analysis technique provides a way to address this issue. This method computes the probability of failure for a given time interval for sample members still at risk (not arrested or incarcerated). It also allows the researcher to control for time-dependent and time-independent covariates and explore their effects on the probability of

failure at a given time period. Survival analysis is well suited for DTC evaluations where individuals are often in and out of treatment and incarceration stays. Despite this appeal, few DTC studies incorporating survival analysis have been conducted.

Peters and Murrin (2000) used survival analysis to explore time until rearrest among DTC graduates, a matched comparison group, and DTC non-graduates in two Florida counties. The comparison group consisted of drug offenders who were sentenced to probation and placed in the community during an equivalent time period to those who entered the drug court program. Each drug court participant from the two courts was matched to a non-drug court probationer on the basis of county of residence, gender, race, and type of offense leading to placement in the drug court program or probation. With failure defined as arrest, results showed that DTC graduates were significantly less likely to be arrested during the 12 month period of DTC involvement and the full 30 month follow-up period than both comparison groups. Moreover, survival curves in both jurisdictions were significantly different throughout the follow-up period for each group, with graduates performing better than both groups. Graduates also had significantly longer duration to arrest than both groups in both sites. Length of time in treatment was significantly related to the number of arrests for both graduates and non-graduates. Finally, in both courts the matched comparison groups performed better than DTC non-graduates.

The authors concede that though the findings appear positive, limitations of the data exist. Peters and Murrin (2000) cite the absence of follow-up measures on treatment and social services may have influenced client outcome. They state that it was not clear to what extent further aftercare treatment following the completion of the program may

have contributed to the reduction in follow-up arrests. Further, since the treatment group only consisted of DTC graduates, it was not possible to discern the differences in recidivism between all program participants and the control group.

Truitt and colleagues (2002) used survival analysis to assess the influence of two drug court programs (Kansas City, Kansas and Pensacola, Florida) on criminal recidivism. To reduce the threats of selection bias, the authors used instrumental variable techniques to compare time until first rearrest. With a 24 month observation period, the outcome analysis was conducted two ways - first by estimating a simple survival model and then by a split-population survival model. The latter method splits the population into two groups: subjects who will eventually recidivate and subjects who will never recidivate. This allowed for analyzing offenders who recidivate separately than those who did not. The advantage of this method was that it helped determine if the drug court had an effect on the incidence of failure as well as time until failure.

Simple survival models (with failure defined as a felony arrest) in both sites showed a significant treatment effect. In Pensacola, participation in the drug court reduced recidivism for new felonies from approximately 40% to 12% within the two year observational period. When defining rearrest as a felony or a misdemeanor, however, this large effect disappeared. Moreover, the split-population model in Pensacola did not show that the timing of recidivism was influenced by drug court participation. Conversely, results from the Kansas City split-population model showed that drug court participation significantly reduced the probability of recidivism and increased time to rearrest. Further, the drug court influence remained significant when failure was defined as any felony or

misdemeanor. Participation in the Kansas City DTC reduced recidivism for new felonies or misdemeanors from 65% to 45%.

Using data collected for the previously mentioned evaluation of the BCDTC, Banks and Gottfredson (2003) used life tables and Cox regression to examine which program components (supervision or treatment), or combination of components, led to lower risk of failure among DTC clients. The authors first examined the influence of supervision only. Conducting survival analyses with two groups (supervision and no supervision), the authors found participants who received supervision had a longer time until failure compared to those with no supervision (this difference approached significance). The second examination focused on the treatment components. This analysis showed similar results; however, the difference between the treatment and non-treatment group was significant. Specifically, the treatment group had approximately 60% of its members surviving the 24 month follow-up period while only 20% of the non-treatment group had a similar result. The last survival analysis included all possible drug court trajectories: supervision only, treatment only, and supervision and treatment. Comparing survival curves showed those who received treatment alone had significantly longer time to failure within the drug court sample. But the longest time to failure was among those who received both treatment and supervision.

Further analyses using Cox regression highlighted treatment as the most effective drug court component. Cox regression allowed for determination of which components were significant in predicting failure and for the introduction of background factors to help explore the possibility of confounding relationships. Again, individuals who received both supervision and treatment had the longest time until failure, but not

significantly longer than those who had treatment only. Additionally, those who received only supervision behaved much like those in the control sample who received traditional court processing. Recency of treatment was also an important predictor. Those who received treatment in the last 30 days before the beginning of time at risk had significantly lower risk of failure. Thus, unlike Rempel et al.'s (2003) findings, treatment was a strong predictor of client failure. Further, treatment appeared to have its greatest impact during the first four months at risk, with its' influence declining through the remainder of the two year follow-up. The authors concluded that these findings highlight the need to explore the treatment experience to help further predict the risk of failure by DTC participants.

Banks and Gottfredson's (2004) additional survival analysis of the BCDTC experiment showed that failure rates for both treatment and control were similar for the first four months, after which control group subjects failed at a higher rate through the 24 month follow-up period. Additionally, life table analysis and Cox regression examination of failures by re-arrest type suggested that drug court may prolong the arrest of drug crimes, but not property or personal crimes. The rate of reduction in the hazard rate for a drug crime was more than double the rate reduction when considering any failure arrest. The authors next examined the varying effect of drug court assignment by creating four month intervals to determine whether or not sample assignment had a greater impact during certain time intervals compared with others. Cox regression results indicated drug court had its greatest effect on the hazard rate in the middle of the first year at risk for failure. The authors noted that this time period is most likely when the sample was actively participating in the various drug court components, particularly drug treatment.

Another interesting finding in regards to drug treatment was the timing of treatment enrollment. Closer examination of the first four months at risk revealed that those who began treatment early were significantly less likely to fail during these four months. Banks and Gottfredson concluded that DTC may have greater impact on clients if certified treatment began immediately after the initial arrest.

Results from a non-DTC drug treatment evaluation using survival analysis showed similar results to the above studies. Hepburn (2005) examined arrest records of 3,328 drug using offenders, most of whom were eligible for diversion from prosecution to a community outpatient treatment program. The author categorized the sample into four groups: diversion eligible but did not enter program, diversion eligible and completed the program, diversion eligible and failed the program, and diversion ineligible.

Following the sample for five years after study entry, Hepburn's findings indicated a rejection of the null hypothesis that no difference across the groups existed. Program completers consistently had higher survival rates than the other three groups throughout the five years and, unlike the non-completer groups, did not experience an early and sudden drop in survival rates within the first year of follow-up. Hepburn next conducted regression analyses to determine whether or not exposure to treatment affects time to rearrest when controlling for offender and offense characteristics. His models found that, when controlling for treatment group, offenders' gender, prior arrest record, and age had a significant effect on the time to rearrest, with age being the weakest of the significant predictors. Specifically, those who were younger, male, and with more prior arrests had a shorter time to rearrest than their counterparts. Additionally, number of

charges filed and the type of drug also had a significant effect on time to rearrest. Although these offender and offense characteristics were found to be significant predictors, exposure to treatment remained significant when these variables were controlled. However, mere entry into treatment was not enough to change clients' time to rearrest. Hepburn's final analysis showed that program entry without completion of treatment was not as important as treatment completion in extending survival times.

The Current Study

The current study will add to the existing drug treatment literature by addressing several shortcomings. First, none of the DTC studies presented have examined the varying effects, if any, that different treatment modalities have on client outcomes; yet prior work has been equivocal in regards to treatment modality on offender outcome. Non-DTC literature has found treatment modality to be influential in client success (Anglin & Hser, 1990; Johnson & Gerstein, 2000; Orwin et al., 2000) while the two DTC studies that have looked at this issue did not support this claim (Saum et al., 2001; White et al., 2006). Saum and colleagues did not rigorously address this topic area; their study examined for only one type of treatment modality (therapeutic community) even though this is one of several treatment modalities found in most DTCs (Cooper, 2001). Moreover, the authors used logistic regression in their analyses. As Allison (1995) argued, logistic regression ignores the timing of the outcome of interest, does not adequately control for individuals who did not experience this outcome, and does not allow for inclusion of time dependent variables. This critique can be applied to any approach that does not use a time dimension. Examination of modality placement and

modality experience also was incorporated in Saum et al.'s (2000) work looking at New York drug courts, but the authors did not use this information to explore relationships between modality and re-arrest. White et al.'s analysis also attempted to explore a specific modality within the drug court setting, but, it too, had methodological limitations that prevented reaching solid conclusions about the effect of treatment on client outcome.

The primary objective of the current work is to explore whether or not subjects in different treatment modalities or subjects who had attended a different number of treatment episodes, experienced significantly different failure rates, controlling on other covariates that predict these failure rates. This type of analysis will be able to address the shortcomings of the time-static approaches mentioned in the previous paragraph. The few DTC and non-DTC studies that have utilized survival analysis appear to support the notion that drug treatment does extend survival times. Again, these studies fail to address what modality (or modality experiences) clients attended nor do they include more than one treatment modality in their analyses. This study will fill this void.

Chapter 3: Methodology

Introduction

This chapter describes the structure and conditions of the BCDTC , the evaluation design used in the study conducted by Gottfredson and her colleagues of this DTC; the sample used in this evaluation; how the data were collected; and how the data can address the shortcomings in the DTC treatment service literature. Additionally, this chapter provides detailed information on the measures to be used in this study and the type of analyses conducted.

The Baltimore City Drug Treatment Court

The BCDTC was created in 1994 in a response to the great number of addiction driven offenses committed in the city of Baltimore. A report conducted by the Russell Committee in 1990 estimated that 85% of all crimes committed in the city were a result of substance abuse. Moreover, the Maryland Division of Parole and Probation (DPP) estimated in 1992 that 80% to 90% of those under supervision had a substance abuse problem. Realizing that harsher sentencing policies were not stemming the quantity, accessibility, and affordability of illegal drugs, the Maryland Department of Public Safety and Correctional Services (DPSCS) applied and received two Bureau of Justice Administration grants to fund and support the city's first DTC.

Designed to provide a viable treatment option for the non-violent offender whose criminality was directly related to patterns of addiction, the BCDTC's primary focus is to break the arrest-incarceration-release cycle. The court adopted two ways of managing substance abusers: a prosecution track and a post-conviction track. In the prosecution

diversion track, offenders could avoid prosecution contingent upon successful completion of treatment-oriented agreements with the courts. Clients that were placed in this track were monitored by the Alternative Sentencing Unit (ASU). This track, however, was eliminated in December of 1999.

Those who are not eligible for diversion were processed through the DTC post-conviction track. The individuals in the post-conviction track generally enter the DTC as a condition of their probation and are closely supervised by parole and probation agents in the DTC probation unit. Clients in this track avoid standard adjudication of their cases by having their incarceration sentences suspended during participation in the DTC. The incarceration sentence remains suspended upon successful completion of the program while unsatisfactory performance results in the sentence being imposed. The type of offense that places the individuals in consideration of the DTC is determined by the court of jurisdiction. Those who are arrested for felony offenses are monitored by the circuit court while those convicted of misdemeanor offenses are monitored through the district court. Regardless of the track or court of jurisdiction, the BCDTC develops individualized treatment/supervision plans for the offenders dependent on their needs for treatment and risk to public safety (BCDTC, 1994).

Generally, pre-trial detainees who are housed in the Baltimore City Detention Center are considered for the program. These detainees have to meet several criteria in order to be considered for a drug court slot. Prospective clients have to be 18 years of age, be a resident of Baltimore City, and not have a violent criminal history. Additionally, offenders must have admitted to a substance abuse problem or have shown evidence of past substance use charges. The names of those who meet these criteria are referred to the

Jail Classification Unit to determine the offenders' security classification. Maximum security classification automatically disqualifies the individuals from participating in the DTC.

Those who are not disqualified and are interested in participating in the program meet with the public defender to discuss the structure and requirements of the BCDTC. If an offender is still interested, the public defender and state's attorney review the prospective client's history to determine if he or she is a viable candidate for program participation. The individual is then assessed by the DTC assessment unit using assessment tools such as Hare et al.'s (1990) Psychopathy Checklist Revised (PCL-R) and McClellan et al.'s (1992) Addiction Severity Index (ASI). The former helps gauge the client's suitability for the program, while the latter instrument helps determine the need and motivation for treatment. Additionally, information is gathered on prior and current substance abuse, family history, prior and current legal status, family and social relationship patterns, mental health history, and medical and employment status. Upon favorable review of this information, assessors send recommendations back to the public defender and the state's attorney, who agree to the terms of the individual's participation. The individual then appears in front of a DTC judge, who considers recommendations from the state's attorney, public defender, and the probation agent or case manager. The judge then renders a decision on the client's case in addition to giving specific instructions to the client. From this hearing, the client is then officially placed within the BCDTC. The goal of the program is to complete this process within 14 days.

As previously stated, the two central components of DTCs are supervision and treatment. The BCDTC supervision requirements state that all program participants must

be under intensive supervised probation. This requires three personal contacts with the probation officer every month, two home visits and verification of employment. Additionally, clients must attend status hearings once every two weeks and undergo urinalysis. Initially, drug testing is conducted twice a week. This phase continues for approximately three months depending on the compliance of the offender. If the offender does not show signs of drug use, their urinalysis requirement decreases to once a week. Drug free samples for approximately three months in this phase makes the offender eligible for the final stage of drug testing. This involves one sample per month for approximately six months. Case managers then have the option of random drug testing for the remaining period of program involvement. Satisfying urinalysis requirements, however, does not guarantee passage into the next phase of drug testing. If the likelihood of relapse is high even after successful completion of one of the phases, probation officers may keep the client at their current level of testing.

Clients of the BCDTC can attend several treatment modalities. Possible treatment modalities include jail based acupuncture, AA/NA, methadone maintenance, outpatient, residential, correctional, detoxification, and intensive outpatient. Table 1 gives a description of these treatment modalities. In theory, offenders are assigned to the modality that best fits their needs as deemed by the judge, public defender, and case manager. Logistics, however, also influence treatment assignment. That is, offenders could be assigned to treatment providers that are close to the offender's residence or are able to be reached without difficulty. Because all treatment facilities are located within Baltimore City limits, it is reasonable to assume that clients are able to attend any type of treatment by using public transportation. Also influencing client placement is space

availability. Personal communication with probation agents during the BCDTC evaluation described in the following section revealed that placement in treatment was influenced by the availability of treatment slots within a facility.

Table 1. Treatment Modalities and Their Services

Modality	Services
AA/NA	Client attends meetings with other addicts to discuss the implications of his/her drinking, draw support from other members in their quest for sobriety, and receive guidance in staying sober and adjusting to sobriety.
Jail-based Acupuncture	Client receives acupuncture while in the Baltimore City Detention Center from a licensed practitioner in a group setting. Client also receives drug counseling and education on relapse prevention skills and behavior modification.
Correctional	Client attends meetings focused on drug counseling and education while incarcerated. Programs vary in their extent and the services they provide
Detoxification	Client attends medically supervised detoxification in a hospital or general care facility. Client also receives help in management of withdrawal symptoms and referral to aftercare services.
Intensive Outpatient	Client receives individual, group, and family counseling, in addition to drug education and access to support groups. This is a non-residential program that allows the client's family to also participate in the treatment process. Client attends six or more hours a week.

Table 1.
Continued

Modality	Services
Methadone Maintenance	Client receives methadone to help control the desire for heroin or other opiates. Client also has access to individual, group, and family counseling, educational and skills development, medical services, and support groups.
Outpatient	Client receives diagnosis, treatment and rehabilitation for his/her addiction while functioning in his/her usual environments. Client also has access to individual, group, and family counseling, drug education, referral, and support groups.
Residential	Client receives intensive regime of individual and group therapy while living in a residential treatment facility. Client also participates in activities aimed at the physical, psychological, and social recovery from addiction. Medical services, support groups, counseling and essentials of daily living are also provided.

Source: Baltimore City Drug Treatment Court Program (1994)

The BCDTC Evaluation

As stated in the previous chapter, the BCDTC evaluation utilized a random assignment research design. From February of 1997 to August of 1998, BCDTC eligible offenders were randomized by staff at the University of Maryland (UMD) to the treatment group (drug court) or to the comparison group (treatment as usual). The randomization results were given to the drug court judge as a recommendation for what track in which to place the offender. In most cases, the judge followed the UMD staff recommendation. Of the 139 cases randomized into the treatment group, 91% were

actually placed into the drug court. For the 96 individuals that made up the randomized the comparison group, 93% were placed in the traditional processing track. The ratio of assignment differed by court of origination. Circuit court cases were assigned at a ratio of one treatment to one control while district court cases were assigned at a ratio of two treatments for one control. This was done at the behest of a District Court judge who was concerned that not all treatment slots would be filled if the one to one ratio was used. On occasion, this randomization process was halted due to staff turnover at the Central Booking Facility or at one of the participating agencies (Banks, 2001). This resulted in 661 cases being assigned to the drug court using the methods utilized prior to the randomization process. A comparison of this group to the offenders who were randomly assigned for the BCDTC evaluation showed no variation on measures of age, race and gender (Gottfredson & Exum, 2001).

The data collected for the official records portion of the study¹ of the BCDTC came from the Maryland DPCS and BSAS. BSAS is the designated agent of the Baltimore City Health Department (BCHD) that administers the city's substance abuse treatment system. From these two sources, information was obtained on a variety of measures during the three year follow-up period. These included demographic characteristics and prior offense history, recidivism, date and modality of drug treatment participation, drug testing, probation supervision, judicial monitoring, and date and length of incarceration periods in jail or prison. Official data were collected after each year for three years.

¹Interviews were also conducted with BCDTC evaluation sample. Data used from the self-report study were used to supplement and verify data on treatment received and drug of choice for the current study. Analyses were run to determine if there was a relationship between being interviewed (72% of the sample) and the independent variables in this study. Results showed that this was not the case.

Prior work using the BCDTC data (Banks & Gottfredson, 2003) showed that the drug court treatment and control groups were not significantly different from each other on demographic, criminal, and substance abuse variables. All subjects who received drug treatment, regardless of experimental status in the initial BCDTC study, are included as “treated” subjects for the purpose of the current investigation. Including clients from both sample assignments (treatment group and control group) allows for a larger sample size and hence greater representation of treatment modalities. It is important to note that the treatment modalities available to the individuals randomly assigned to the DTC were also available to those randomly assigned to the non-DTC group, and data were collected on their attendance episodes as well. These individuals could have received services from the same treatment provider as the treatment group members and may have been assigned treatment as a condition of their regular probation or may have voluntarily enrolled in treatment. Unlike DTC clients, they were not subject to other supervision requirements such as monthly status hearings and graduated sanctions.

Sample

Identifying the sample for the current study required preliminary analyses to determine whether or not to include non-certified treatment modalities. As stated previously, there were eight possible treatment modalities an individual could have attended during the observation period. These include: methadone maintenance, outpatient, residential, correctional, detoxification, and intensive outpatient. Additionally, clients could have attended AA/NA, and jail-based acupuncture. All programs are recognized by BSAS as certified treatment programs except AA/NA and jail-based

acupuncture (see previous chapter for non-certified treatment criteria). Prior evaluations of the BCDTC (Banks, 2001; Gottfredson and Exum, 2002) separated out non-certified treatment modalities when looking at treatment received as it was believed these treatments did not have the same level of structure or similar components as the other treatments. A preliminary analysis examining these two modalities and their relationship to time until rearrest was conducted and results showed that there was no significant difference between these two modalities and the non-treated group. Therefore, sample members that only attended these modalities are placed in the non-treated group². The end result is a sample size of 128 individuals who received drug treatment.

This sample used in the current study differs from the sample used in previous survival analyses (Banks and Gottfredson, 2003; 2004) of the BCDTC. The treated group in the current work includes all members who received some form of certified treatment, regardless of whether they were randomized into the BCDTC. Conversely, some members of the non-treated group were placed into the BCDTC for the initial randomized experiment but never received treatment. The following chapter presents the percentage of those in the treated and non-treated that were randomized into the BCDTC.

Measures

This section identifies the current study's measures and data sources. The appendix provides greater detail about each measure, including the type of measure and the coding scheme, and it provides a correlation matrix for all variables by each way treatment is measured. This section presents a comparison between the treated group and

² Those subjects that had a mixture of non-certified and certified treatments remained in the treated group, with their non-certified treatment episodes not included from their treatment history.

non-treated group on the control variables. The next chapter compares these groups on measures of treatment provided.

Time to Recidivism Measure

The dependent variable for this analysis is time (in days) until rearrest for any crime, adjusted for incarceration time. This adjustment is necessary because individuals were not at risk of being arrested while incarcerated. The intervals used to measure this variable are days. The sources for these data are the arrest records provided by the Maryland DPP, Maryland Division of Corrections (DOC), Baltimore City Jail, circuit and district court documents, client self-reports, and BSAS treatment data.

Time until failure begins on the date the subject began their treatment episode³ and ends on the date of an arrest⁴ (failure), or three years post randomization⁵. Because prior research has shown that clients in the current data set received treatment at various times throughout the follow-up period (Banks, 2001), individuals have slightly varying times in terms of their time at risk. For those who did not receive treatment, the time until failure begins on the day of their randomization⁶. Incarceration stays are used to adjust time at risk only once the individual has started their treatment episode⁷. Incarceration

³The start date for those who received correctional treatment was the day of their release as they could not have re-offended while being incarcerated.

⁴ Official arrest, as opposed to probation violation, is used as the failure event because of a substantial amount of missing data on the dates of probation violations.

⁵ Approximately 90% of cases received treatment within two years of randomization into the BCDTC evaluation, allowing for at least one year of follow-up time. An analysis was conducted comparing the full sample to only those that received their treatment within the first two years and results did not vary.

⁶ An analysis was conducted adjusting the start time of the non-treated group by the average amount of time it took the treated sample to begin treatment (9 months). Results did not greatly vary and the variations that did occur will be denoted.

⁷ Adjusting for incarceration stays is necessary because subjects could have received a shock incarceration sentence as part of their drug court contract (for those subjects who were in the drug court program) or in response to a possible probation violation, but not as a result of an official arrest.

data came from a variety of sources including the Baltimore City Jail, the Maryland DOC, the circuit and district courts and from client self-reports. These sources provided information on whether an incarceration sentence was given and the length of that sentence. Multiple sources were necessary because clients could have served their sentence under different custodians. That is, an offender may have served a brief time in the Baltimore City Jail, but then was transferred to a Maryland DOC facility if the sentence length warranted such a transfer. Using multiple jurisdiction data does increase the potential for discrepancy; however, UMD staff took great efforts to rectify such occurrences and validated the data using probation and parole, treatment, and arrest records. This information allowed for the calculation of time the individual was not at risk. Enrollment in a residential treatment, though restrictive, was not used to adjust the follow-up period because it was still possible for an individual to walk away from the facility and be arrested, as evidenced by anecdotal accounts during the self-report phase of the study.

Independent Variables

Treatment experience is measured two different ways. The first way to measure treatment experience is by first type of modality received. The first modality is used (as opposed to second or third) for two reasons. First, this captures the most cases because 72% of sample members who received treatment only experienced one treatment episode⁸. Second, the modality into which the client is first placed is presumably the modality thought to best serve the client's needs as determined by criminal justice

⁸ An episode is defined as attending one treatment that can end either in failure, when the subject drops out of the treatment, completion of treatment, or the beginning of another treatment (if the treatments are received consecutively).

practitioners. Exploring treatment this way can determine the appropriateness of the offender's placement. Another way of measuring treatment experience is the number of treatment episodes experienced by each offender. Clients are categorized as receiving one treatment episode or multiple treatment episodes.

The treatment experience variable is a combination of official records and self-report data collected three years post-randomization into the BCDTC evaluation study. Combining these data sets allows for reducing the amount of missing data on each measure and presents the most likely treatment experience the subject had. When consolidation of data occurred, great effort was taken to ensure that measures were identical in both data sets and that the time period in which the sample member self-reported a measure was the time period of interest. Table 3 provides the number of subjects who attended each modality. It is not uncommon for clients to attend more than one treatment modality during the follow-up period. Treatment stays of 10 days or longer are considered to be the minimal threshold of individual participation in a modality⁹.

⁹ The one exception is for detoxification, which, depending on the provider, may be designed to last only a few days.

Table 2. Attendance of Each Treatment Modality (N = 128)

Modality	Number of Clients
Methadone	23
Outpatient	75
Intensive Outpatient	47
Detoxification	10
Residential Treatment	32
Correctional Treatment	7

Control Variables

Several control variables are included in the survival analysis. Demographic variables such as age, gender, and race have demonstrated significance in prior DTC outcome-focused studies and are controlled here. This information is provided by the Maryland DPP. Additionally, client characteristics may also influence on client success. Criminal history (prior arrests and convictions) and randomization into the Baltimore City drug court are covariates that are controlled for in the analyses discussing the second research question. The source of both of these measures is the Maryland DPP.¹⁰ Finally, prior analyses using the current data set found that the court (district vs. circuit) in which

¹⁰ Two variables that may influence time until rearrest but were missing substantial data are prior participation in drug treatment before the follow-up period and primary drug of choice. Prior participation in drug treatment was coded dichotomously as either received or not received. Approximately 47% of cases were missing data on this variable. No correlation was found between prior treatment and the dependent variable for those cases that did have a value for prior treatment. Similarly, primary drug of choice was missing substantial amount of data for those that were not interviewed during the self-report portion of the BCDTC evaluation. This variable was not shown to be related to the independent variables or to the outcome. These variables are not included in the regression analyses.

the case was processed yielded a significant, independent effect on client outcome and warrants inclusion when examining time until failure. The source of this variable also is the Maryland DPP.

Additional treatment measures that describe sample member's total treatment experience during the follow-up period are also included. These include the total number of treatment days, number of treatment episodes attended, and number of days of total treatment. Finally, a time varying covariate measuring currently being involved in a treatment at the time of failure is also included. This variable is included as it is important to control for situational conditions that can influence the hazard. Percentages were calculated for those who received treatment to verify that there were subjects in treatment at the date of arrest or at the date of being censored (for those not arrested). Nineteen percent of cases fell into the former category, while 7% fell into the latter. As there are cases that fall into both categories, this time varying covariate can be included in the model without producing misleading hazard ratios. Additionally, prior research by Banks (2001) found that coding treatment as a time varying covariate influenced the survival times of the sample. The sources for these data are also the Maryland DPP, BSAS, and client self-reports.

Supervision by the criminal justice system may also influence time until re-arrest. Supervision may act as a catalyst for change in the subject's criminal activity by acting as a deterrent. Being on supervision, however, may increase the likelihood of re-arrest because the individual is being monitored more closely by criminal justice officials. Two supervision measures are included in the current sample. The first is the number of days on supervision during the follow-up period. The second is another time varying covariate

measuring whether the subject is currently being supervised at the time of failure. As with the time varying covariate for treatment mentioned above, percentages were calculated to verify that subjects were under supervision on the date of arrest or on the date of being censored (for those not arrested) to ensure that the variable can be included without producing misleading hazard ratios. Thirty six percent of subjects were arrested while on supervision while 10% of subjects reached their censored date while on supervision. For both variables, supervision could include either probation or parole. The source of these variables is the Maryland DPP.

Table 4 shows the descriptive statistics of the treated and non-treated groups. The sample is mostly African American males in their mid-thirties with substantial prior involvement in the criminal justice system. The groups are very similar on most measures, varying significantly only in the percent assigned to district court, the percent assigned to drug court, and the average days of supervision. With the latter variable, the treated group received on average 11 months of supervision, while the non-treated group received approximately 9 months on average.

Table 3. Descriptive Statistics of Treated and Non-Treated Group (N = 235)

Variable	Treated Group (N = 128)	Non-Treated Group (N = 107)
% Assigned to District Court*	48	60
Average Number of Prior Arrests (SD)	12 (7.8)	11 (8.6)
Average Number of Prior Convictions (SD)	5 (4.0)	5 (3.9)
Ethnicity		
% African American	92	86
% Caucasian	8	13
% Asian	0	1
% Male	73	75
Average Age (SD)	35 (7.5)	34 (7.6)
% Randomized to Drug Court*	67	50
Average Days of Treatment (SD)	194 (201)	-
Average Days of Supervision (SD)*	336 (263)	262 (272)
Average Number of Treatment Episodes Attended (SD)	1.5 (.99)	-

* $p < .05$

Survival Analysis

The data collected for the BCDTC evaluation are event history data. An event history is a longitudinal record of when events happened to a sample of individuals. An event can be defined as a qualitative change that occurs at a specific point in time, with the change being a disjunction between what precedes and what follows. In this work, the event of interest is an arrest. Event history data allow the researcher to explore the causes of events and how these factors prolong (or shorten) the time until such events occur. However, two features of event history data render standard statistical analyses inappropriate for examining these causes: censoring and time-dependent variables. Censored cases are those cases in which the individual does not experience the event. Such is the case in the current study in which slightly more than 32% of sample members were not arrested during the observational period. But if the interest is in the length of time until an individual is arrested, what is to be done with these cases? One possible solution would be to exclude these cases from the analysis. Excluding such cases, however, can result in substantial bias and potentially disregards a substantial amount of data. Allison (1984) stated that another alternative is to assign the maximum length of the time observed as the value of the dependent variable in these cases. But this, again, is not ideal because some individuals were never arrested.

Time-varying explanatory variables also pose difficulties for standard statistical procedures in event history analysis. A time varying covariate (sometimes referred to as a time-dependent covariate) is defined as any covariate that changes in value over the observation period. For example, in the current analysis an individual's supervision or treatment status may change throughout the follow-up period if he or she has fulfilled his

or her sentence. In coding the data, a sample member's follow up period was broken down into intervals based on the changing status of their treatment and/or supervision. An interval ends and a new one begins every time there is a change in status of treatment and/or supervision. For example, if a subject is in treatment at the start of their follow up period, they would have a value of one for this time frame for this variable. As soon as that individual exits treatment (for any reason other than an arrest), a new time interval begins with this variable now having a value of zero. Subjects, therefore, can have several intervals associated with their follow up period depending on how many times their treatment and/or supervision status changes.

Traditional regression models, however, assume that variables remain static throughout the observational period. Allison (1984) argued that one way to account for this changing status is to calculate dummy variables for each unit of time and incorporate them into a regression. However, this method is far from efficient and allows for independent variables to become the consequence, rather than the cause, of the failure event.

Survival analysis provides a statistical tool to address these problems. In short, survival analysis examines and models the time it takes for events to occur and focuses on the distribution of survival times. This method 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 in the current work; non-parametric tests and Cox regression. Non-parametric tests, such as the Kaplan-Meier estimates of the survivor function, are a common method of first presenting the survivor functions of the

groups in the study. The survivor function can be interpreted as the probability of surviving past time t . Survival function estimates can be graphed so that visual inspection of the groups can be conducted. Additionally, a Wilcoxon chi-square statistical test can be conducted to determine whether the survival functions between the group(s) of interest and the non-treated group differ.

A limitation of this method, however, is its inability to control for covariates, or factors that influence the hazard rate. Because we are interested in not only the survival rates of sample members but also in what factors influence these rates, a more sophisticated analysis is necessary. Cox regression is an ideal survival analysis technique for the second research question because of its relative ease of including time independent and time dependent covariates and determining the effects of these variables on the hazard (Allison, 1995). As mentioned in the previous chapter, prior drug treatment and drug court studies have suggested several variables that influence future criminal activity. These include time invariant demographic variables (age, gender, and race), substance history variables (prior treatment and drug of choice), criminal history variables (prior arrests and prior convictions), and criminal justice variables (originating court) as well as time varying variables, such as treatment and supervision. Cox regression is useful in that it can control for these differing variables. This survival analysis method also has several other features that make it attractive for the current work. First, Cox regression does not require choosing a particular probability distribution to represent survival times. That is, it is not necessary to determine how the hazard rate depends on time or the model's appropriate shape of the hazard function. Second, Cox regression is able to differentiate between censored and uncensored cases in

exploring the influence of covariates. For example, certain variables may lead to a client's, or group of clients', failing out of the study early. However, these variables may not be significant predictors of failure for those who survive until later into the observation period. Cox regression allows for only the factors of cases still at risk to influence these uncensored cases. Both of these features of Cox regression make it ideal for the proposed research questions.

Determining the probability of failure in survival analysis is accomplished through calculation of the hazard function, sometimes referred to as the hazard rate. Though the hazard function is not the only way of describing distributions, it is considered the most popular and perhaps more intuitive. The hazard function allows us to quantify the risk that an event will occur between the time interval t and $t + \Delta t$. This probability is conditional on the individual surviving at time t . That is, if an individual has already experienced the event, he or she are no longer at risk for the event. The definition of the hazard function is: $h(t) = f(t)/[1-F(t)]$. In this definition, $h(t)$ represents the hazard rate, $f(t)$ is the density and $F(t)$ is the distribution function. The denominator of this equation is referred to as the survivor function, which gives the probability of surviving beyond t . The density represents the failure rate at time t as a proportion of the population that has survived up to that point.

Although the hazard is often thought of as a probability of an event occurring at time t , it is not a true probability because its value can be greater than 1.0. Interpretation of the hazard requires knowing the units in which time is measured. In the current study time is measured in days. It is most useful to think of the hazard as a characteristic of individuals rather than of populations or samples. Because factors influencing failure

vary significantly by individual, it is rare that all individuals in a sample would have the same hazard rate. Further, hazard functions for individuals vary as their situations change. For example, an individual's hazard rate would be lower for an arrest if he or she were attending church service compared to a situation in which the individual is socializing with drug dealers on a street corner. Capturing all life situations that change hazard rates would be an insurmountable task. However, examining the effect of significant events, such as attending a drug treatment, on hazard rates is easier to capture because such events are usually relatively long in duration, with definitive beginning and end dates.

To simplify interpretation, the current study will report hazard ratios, which is the exponentiated hazard rate. Exponentiated coefficients may be interpreted as the ratio of the hazard for a one unit change in the corresponding covariate. For example, if a hazard ratio for age is 1.2, then a 1 year increase in age increases the hazard by 20%. If the hazard ratio is .8 then an increase in age decreases the hazard by 20%. If a hazard ratio for a male (with male coded as 1) is 1.05, then males face a hazard 5% greater than females. If a treated group has a hazard ratio of less than 1, this would indicate the percent of the hazard the treated group would have compared to the non-treated group. For example, a hazard ratio of .60 for a treated group would be interpreted as the treated group facing 60% of the hazard of the non-treated group.

Analyses

As previously noted, there are two different ways of measuring treatment in the current study: first treatment modality received and the number of treatment episodes (single or multiple). Non-parametric tests and Cox regression models are conducted for the research question (Do subjects in different treatment experiences have significantly different failure rates and if so, why?) for each way treatment is measured. First, the non-parametric tests illustrate the varying survivor function for each way treatment is measured at each point during the follow-up period. Graphs depicting the Kaplan-Meier survival function estimates for each of the independent variable groups are shown so as to visually depict the probability of survival past time t . Additionally, a post hoc test (Wilcoxon chi-square test) is conducted to formally test the hypothesis that the survivor functions across groups are equal. These tests do not test the equality of the survivor functions at a specific point in time; rather, they are global tests in that they compare the overall survivor functions. Once post hoc tests are conducted and the results show that there is significant variation among the groups, the next step is to determine which independent variable groups differ significantly in their time until rearrest and what covariates might also play an influential role on this dependent variable. These questions are answered using Cox regression.

The Cox regression models determine the influence of the two ways treatment is measured on time until failure. Two separate models are run for each of the ways treatment is measured. The first includes only the independent variables and the second model includes the independent variables and all covariates (time variant and time

invariant).¹¹ Conducting the analyses this way shows which, if any, measures of treatment are significant predictors of the hazard and whether or not their influence remains after the addition of the covariates.

The following chapter reports the results of the aforementioned analyses. The implications and limitations of the study are discussed in the final chapter.

¹¹ The same set of control variables will be included in the regression analyses for each way treatment is measured. This set consists of the variables for which the groups differed significantly for either way treatment was measured.

Chapter 4: Results

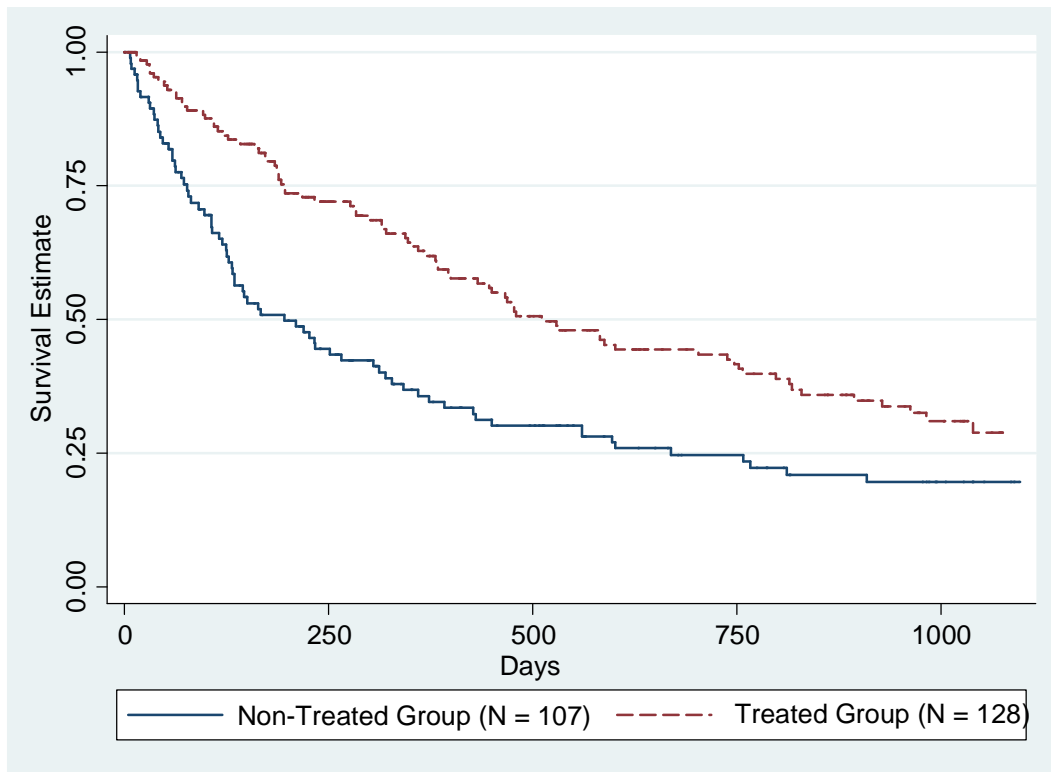
Introduction

This chapter presents the results of analyses designed to answer the research questions. First, a preliminary analysis of survival estimates is presented showing differences according to whether or not whether subjects received certified drug treatment at all during the follow-up period. The following section presents descriptive characteristics of the two ways in which treatment is measured and uses non-parametric tests to determine if their survival functions differ from the control group. Post-hoc tests are then presented to highlight exactly which of the independent variable groups differ from the control group in regards to their survival functions. The final portion of this chapter presents the Cox regression analyses which controls for extraneous variables on which the groups significantly differ. A summary of the results concludes this chapter.

Survival Rates: Certified Treatment vs. Non-Treatment

A good place to start to answer the proposed research questions is to examine whether drug treatment is related to time until arrest, regardless of modality or number of episodes. This establishes a foundation for the following analyses that explore the differences by treatment modality. A Wilcoxon non-parametric test to determine the equality of the survivor functions was conducted. Figure 1 shows the Kaplan-Meier survival estimates by group.

Figure 1. Kaplan-Meier Survival Estimates - Treated Group vs. Non-Treated Group



Wilcoxon chi-square = 19.02 (p = .000)

Figure 1 shows that, throughout the follow-up period, those who received drug treatment performed better than the control group. Using the aforementioned interpretation of the Kaplan-Meier estimate, we could conclude that, for instance, on day 250 of the follow-up period, a treated group member had an approximate probability of .75 of surviving past this point while a non-treated group member had an approximate probability of .45. The difference between the two groups appears to remain relatively proportional after the 250 day mark. Interestingly, it appears that the probability values drop most significantly within the first 200 days of the follow-up for the non-treated group, while the treated group has a more gradual downturn as the follow-up time passes.

The significance of the Wilcoxon statistic indicates that the overall survival functions do vary significantly between the groups. Visual inspection of the graph confirms that the treated group has a higher overall survival probability than the control group. With these results, we can conclude that treatment is related to time until re-arrest and we have reason to explore this relationship further.

Descriptive Characteristics and Non-Parametric Survival Tests

This section compares the characteristics of the different treatment experience groups. This provides information about what pre-existing characteristics related to treatment are statistically controlled when examining the association between treatment and the time until re-arrest.

First Treatment Modality

The first way treatment is measured uses first modality received. As stated in the previous chapter, there were 8 possible treatment modalities a subject could have first attended, two of which were non-certified treatments that appeared to be no different from the control group and are not included as treatments in this analysis. Table 4 gives the sample size for each first modality and the descriptive characteristics of members of these groups.

Table 4. Descriptive Characteristics by First Modality (N = 128)

Variable	Methadone (N = 17)	Outpatient (N = 47)	Intensive Outpatient (N = 30)	Detoxification (N = 8)	Residential (N = 19)	Correctional (N = 7)
Assigned to District Court (%)	53	43	52	60	36	57
Average Number of Prior Arrests	13	10	14	10	14	10
Average Number of Prior Convictions	5	4	6	3	6	3
Ethnicity (%)						
African American	88	89	97	90	95	100
Caucasian	12	11	3	10	5	0
Asian	0	0	0	0	0	0
Male (%)	76	67	79	50	79	100
Average Age*	39	33	34	35	35	41
Randomized to Drug Court (%)	41	80	79	60	47	57
Average Days of Supervision	360	377	337	415	258	187

* $p < .05$

The table shows that outpatient treatment was the most common first modality received, followed by intensive outpatient and residential treatment. The groups did not vary significantly on most background variables except for age at the beginning of the study, with those in methadone and correctional being older than the other first treatment modality attendees.

Subjects categorized by their first modality were also compared by treatment characteristics. These variables provide a snapshot of their treatment exposure during the follow-up period. Table 5 shows these variables.

Table 5. Treatment Experience by First Modality (N = 128)

Variable	Methadone (N = 17)	Outpatient (N = 47)	Intensive Outpatient (N = 30)	Detoxification (N = 8)	Residential (N = 19)	Correctional (N = 7)
Mean Days in Modality*	327	110	91	158	121	129
Mean Days of Total Treatment*	331	171	144	297	173	130
Mean Number of Treatment Episodes*	1.1	1.6	1.6	2.2	1.2	1.0

* $p < .05$

The different modalities varied on all three of the treatment variables. Methadone subjects had significantly more days in their methadone treatment as well as more days of treatment overall. Those who received detoxification first had a significantly higher number of treatment episodes during the follow-up period than the other modality groups. These findings are in line with what would be expected. Methadone is designed to be a long-term treatment with a primary purpose of preventing the symptoms of opiate

withdrawal. Detoxification, on the other hand, is designed to help addicts go through any physical withdrawal symptoms so they can then begin the process of addressing and coping with their addiction through other treatment modalities.

A good starting point for exploring whether there is any difference in time until rearrest between each modality and the control group is by conducting non-parametric tests. As stated in chapter 3, the starting point for the control group in this analysis is their day of randomization into the BCDTC evaluation. Figure 2 shows how the Kaplan-Meier survivor functions differ between the groups and table 6 gives the Wilcoxon chi-square value testing the hypothesis that the survival functions are equal.

Figure 2. Kaplan-Meier Survival Estimates - First Treatment Modality and Non-Treated Group (N = 235)

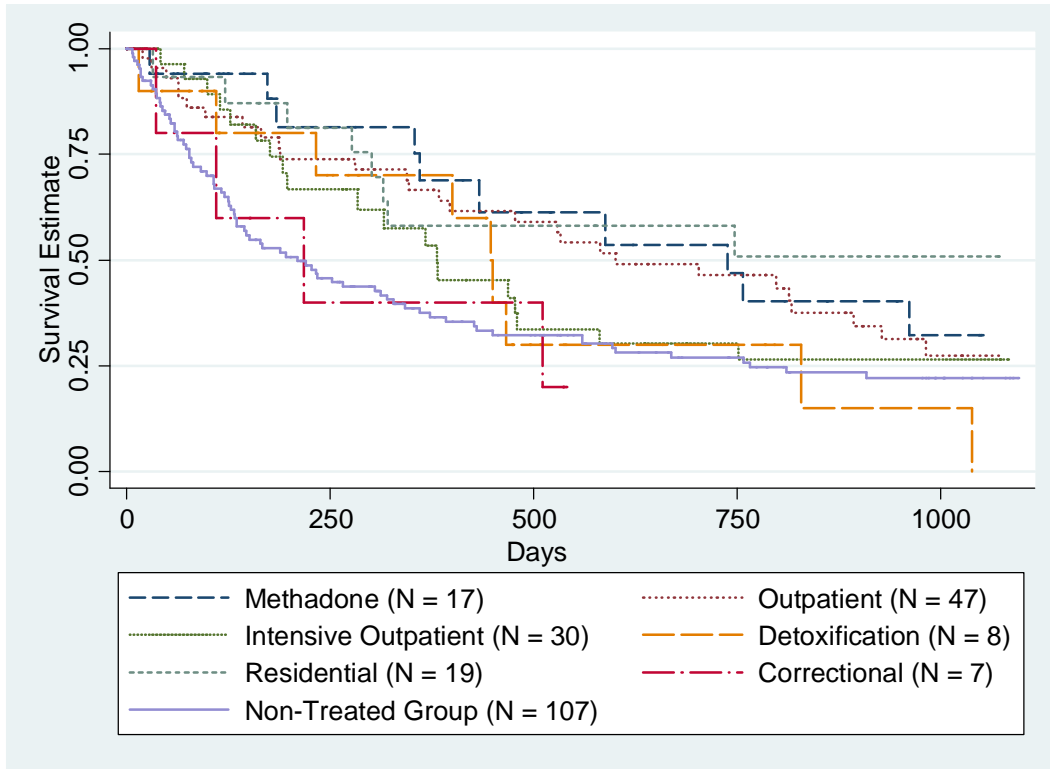


Figure 2 shows that most treatment modalities perform better than the non-treated group during the follow period. Correctional and detoxification treatments appear to be the only modalities that have a lower survival function than the non-treated group at certain times, whereas the intensive outpatient group appears have a similar survival function to the non-treated group beginning at about the 500 day mark. Residential, outpatient, and methadone cluster throughout most of the follow-up period, with their widest gap between these three modalities and the non-treated group occurring approximately at day 500. Looking at the Wilcoxon chi-square values, table 9 suggests

that the survival functions of four of the six modalities varied significantly from the non-treated group. Interestingly, none of the treatment survival functions varied significantly from the others, though several approached significance with the correctional modality.

Figure 2 and table 6 indicate that there is cause to further explore the relationship between first modality attended and time until re-arrest. Before addressing this relationship, the other measurement of treatment (number of treatment episodes) will be examined using the same non-parametric tests as used in this section to determine if measuring treatment this way also varies significantly from the non-treated group.

Table 6. Wilcoxon Chi-Square Values Comparing Survival Functions - First Treatment Modality and Non-Treated Group (N = 235)

	Methadone	Outpatient	Intensive Outpatient	Detoxification	Residential	Correctional	Non-Treated Group
Methadone	-						
Outpatient	.26	-					
Intensive Outpatient	1.36	.74	-				
Detoxification	.83	.85	0.0	-			
Residential	0.0	.37	1.61	1.22	-		
Correctional	3.10	2.24	.85	.31	2.76	-	
Non-Treated Group	5.77*	8.17*	3.68*	.99	5.86*	0.0	-

* $p < .05$

Number of Treatment Episodes Attended

This section presents treatment received as measured by single and multiple treatment episodes attended. Table 7 shows the sample size and the descriptive characteristics of each group, followed by table 8, which shows the treatment experience of these groups.

Table 7. Descriptive Characteristics of Single and Multiple Episode Groups (N = 128)

Variable	Single Episode Group (N = 92)	Multiple Episode Group (N = 36)
Assigned to District Court (%)	51	56
Average Number of Prior Arrests	12	12
Average Number of Prior Convictions	5	5
Ethnicity (%)		
African American	90	100
Caucasian	10	0
Male (%)	71	76
Average Age	35	36
Randomized into Drug Court (%)*	61	80
Average Days of Supervision*	284	444

* $p < .05$

Table 8. Treatment Experience by Single Episode and Multiple Episode (N = 128)

Variable	Single Episode (N = 92)	Multiple Episode (N = 36)
Mean Days of Total Treatment*	166	279
Mean Number of Treatment Episodes*	1	2.9

* $p < .05$

The descriptive characteristics and treatment experience comparisons of the two groups do not yield any surprises. Comparing the two group's backgrounds, the only significant difference is the greater percent of multiple episode subjects who were randomized to the drug court (80%) compared to the single episode group (61%). The treatment experience chart shows that multiple treatment episode groups have significantly higher mean days of treatment (279 vs. 166) and treatment episodes (2.9 vs. 1) than single treatment episode subjects.

The next step in examining this independent variable is determining whether the groups significantly vary from the control group in terms of their survivor functions. Figure 3 below shows how the survival functions of each group compared to the control group followed by table 9, which presents the Wilcoxon chi-square values.

Figure 3. Kaplan-Meier Survival Estimates - Single Episode, Multiple Episode, and Non-Treated Groups (N = 235)

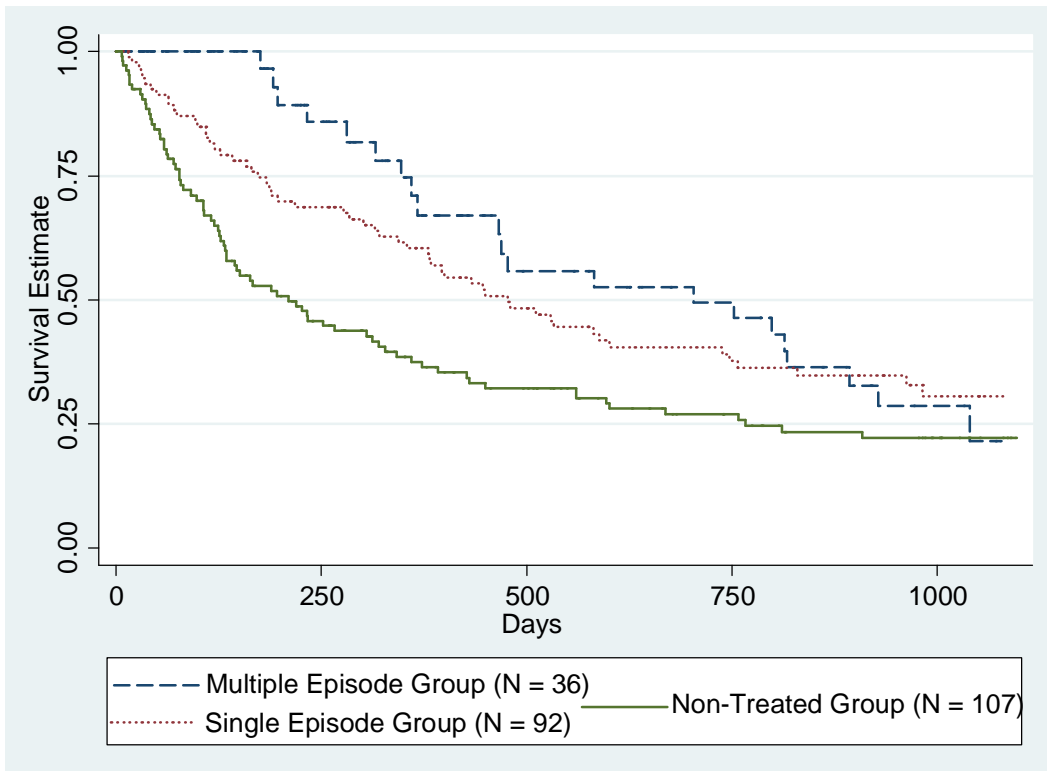


Table 9. Wilcoxon Chi-Square Values Comparing Survival Functions - Single Treatment Episode, Multiple Treatment Episode, and Non-Treated Groups (N = 235)

	Single Episode	Multiple Episode	Non-Treated Group
Single Episode	-		
Multiple Episodes	1.86	-	
Non-Treated Group	9.49*	11.27*	-

* $p < .05$

The figure and table show that there is a significant difference in the survivor function of the two groups. Multiple treatment episode subjects appear to perform better throughout most of the follow-up period, though around day 800 their survival function intersects and falls below the single episode group. Both treated groups appear to have higher survivor functions than the control group throughout the entire follow-up time. Wilcoxon chi-square values support the graphical output. The survival function curves of the treated groups compared to the control group are significantly different, with the multiple episode group having a higher value than the single episode group. The two treatment group's survivor functions, however, are not significantly different.

This section establishes that there are relatively few differences in the background characteristics between the single treatment episode and multiple treatment episodes groups but that they did vary significantly on how many days of treatment they received and the number of episodes they experienced. Non-parametric analyses show that the

groups do differ from the control group by survival functions, which warrants further exploration using Cox regression to answer why this is occurring.

This section thus far presents the non-parametric results using the two different ways of measuring treatment. The first modality and number of treatment episodes non-parametric analyses showed that no matter which way treatment is measured, the treated group performs better than the control group in time until re-arrest. On most measures in each of the independent variables, the groups were similar in background characteristics, though significant variation was present when looking at the treatment experience measures. These non-parametric tests establish only that treatment modality is related to survival time. There are a few possible explanations for this association. Those in the treated group could have been more motivated or better situated for success than the non-treated group at the beginning of the follow up period. Another possibility is that pre-existing characteristics or social conditions resulted in some sample members to fail early, placing them in the non-treated group when eventually they may have received treatment. The next section presents Cox regression analyses designed to clarify the meaning of these associations by examining the independent variables central to the study, controlling for the treatment and descriptive characteristics found to significantly differ between the groups in the previously presented tables.

Cox Regression Analyses

This section presents the Cox regression analyses which estimates the effects of the different ways of measuring treatment on time until rearrest while controlling for the covariates on which the groups differ. For the two ways in which treatment is measured,

the first model contains just the independent variables. This is followed by a model with the independent variables and the time stable covariates. The final model includes the independent variables and all covariates. Each table presents the hazard ratios, standard error, z value, and probability of z.

First Treatment Modality

Presented first are the regression results for the treatment measured as first treatment received during the follow-up. Tables 10a and 10b show these results. As in earlier analyses, the start date for the control group is the day subjects were randomized into the study.

Table 10a. Time to Rearrest Regressed on First Treatment Modality - Cox Regression (N = 235)

Variable	Hazard Ratio	Standard Error	Z value	P (z)
Methadone	.521	.175	-1.93	.053
Outpatient	.597	.132	-2.32	.020
Intensive Outpatient	.730	.187	-1.22	.222
Detoxification	.917	.324	-.24	.808
Residential	.413	.153	-2.37	.018
Correctional	1.188	.612	.33	.738

Table 10b. Time to Rearrest Regressed on First Treatment Modality and All Covariates-Cox Regression (N = 235)

Variable	Hazard Ratio	Standard Error	Z value	P (z)
Methadone	.862	.328	-.39	.697
Outpatient	.892	.253	-.40	.689
Intensive Outpatient	1.006	.315	.02	.983
Detoxification	1.585	.708	1.03	.302
Residential	.542	.217	-1.53	.127
Correctional	1.494	.794	.75	.451
Days of Total Treatment	.998	$8.53 \times e^{-4}$	-2.23	.026
Days under Supervision	.998	$3.58 \times e^{-4}$	-4.97	.000
Number of Treatment Episodes	1.051	.123	.43	.669
In Treatment	1.000	$8.40 \times e^{-4}$.44	.657

Examining table 10a, there does seem to be some significant variation between two of the treatment modalities and the non-treated group. Specifically, a subject who first attended outpatient treatment is estimated to have a hazard rate that is 41% $((1-.597)*100)$ of the hazard of a subject in the non-treated group, while a subject who attended residential treatment has 59% of the hazard of a non-treated group member. No

other modalities were significant in the model at the .05 level, though the methadone modality was significant at the .10 level.

In table 10b¹², which adds the covariates to the model, none of the first treatment modalities continue to be significant. Days of total treatment and days of supervision predict time to rearrest. Specifically, a one day increase in attending treatment results in a .2% decrease in the hazard. The variable days under supervision has the same hazard ratio as days in treatment with a similar interpretation; a one day increase in supervision results in a .2% decrease in the hazard of being rearrested. The number of treatment episodes and being in treatment are not significant predictors in this model.

Other variables, including age, being randomized to drug court, and being under supervision were also variables that were correlated with the outcome but once these variables were entered into the model, the parameters became unstable (e.g. the standard errors rose substantially and the hazard ratios for some of the variables became inflated). To address this problem, these covariates were removed from the analysis. Even after this omission, no treatment group performed significantly different from the non-treated group and including these covariates would unlikely change the results of the model.¹³

¹² One variable on which the groups differed significantly in table 8 and table 11 was days in first treatment. This variable, however, is highly correlated with days in total treatment. Therefore, it is not included in the analyses for each way treatment is measured.

¹³ In the model that included all covariates, the correctional modality group was significant different from the non-treated group in that the correctional group had a shorter time to failure. The large increase in hazard ratio coefficient and standard error, however, did not make this model plausible.

Number of Treatments Episodes Attended

The second way in which treatment was measured was by the number of treatments attended, measured as single episode or multiple episodes. Tables 14a and 14b present the models with the independent variables and covariates of interest.

Table 11a. Time to Rearrest Regressed on Number of Treatment Episodes - Cox Regression (N = 235)

Variable	Hazard Ratio	Standard Error	Z value	P (z)
Single Treatment Episode	.640	.112	-2.53	.011
Multiple Treatment Episode	.589	.145	-2.14	.033

Table 11b. Time to Rearrest Regressed on Number of Treatment Episodes and All Covariates- Cox Regression (N = 235)

Variable	Hazard Ratio	Standard Error	Z value	P (z)
Single Treatment Episode	.894	.233	-.43	.668
Multiple Treatment Episode	1.082	.617	.14	.890
Days of Total Treatment	.998	$8.01 \times e^{-4}$	-2.19	.029
Days under Supervision	.995	$3.59 \times e^{-4}$	-4.97	.000
Number of Treatment Episodes	1.013	.186	.07	.942
In Treatment	1.000	$8.26 \times e^{-4}$.62	.533

Looking first at Table 11a, results show that both single and multiple treatment groups had significant hazard ratios. Single treatment episode subjects had a hazard that was 64% of the non-treated group and the multiple treatment episode subjects had 58% of the hazard rate of those who did not receive treatment. Those with multiple treatment episodes, therefore, appear to do slightly better than the single treatment episode subjects.

Table 11b, however, shows that these effects disappear when all the covariates are introduced into the model and results are similar to the first treatment modality analysis found in table 10b. After inclusion of the covariates, neither treated group had a significantly longer survival time than the non-treated group.¹⁴ In this model, it again appears that the number of days in treatment (hazard ratio = .998) and the number of days

¹⁴ Instability in the model prevented inclusion of all relevant controls.

under supervision (.995) were the predictors of time until rearrest, while the number of treatment episodes and being in treatment did not explain the time to this event.

Conclusion

This chapter set out to accomplish several things. The first section established whether treated and untreated groups differed in terms of their survival function. Non-parametric tests confirmed that the groups did differ, which warranted further analyses.

The next section presented the two ways (first modality received and number of treatment episodes) in which treatment was measured in the current study, how these groups differed on several descriptive characteristics and treatment experience measures, and whether the survival functions of these groups differed significantly from the non-treated group. Measuring treatment by first modality received, results showed that the groups varied significantly by age and by the number of days in the first modality, total treatment days, and number of treatment episodes (table 8). Non-parametric tests showed that four of the six treatment modalities had significantly higher survival functions than the non-treated group. Measuring treatment by single and multiple treatment episodes received, the groups varied on the number of those randomized to the drug court, days of supervision, total treatment days and number of treatment episodes. Both groups had significantly higher survivor functions compared to the non-treated group. With the results of this section, it was determined that significant variation did exist between the two ways treatment is measured and the non-treated group and that regression analyses would be necessary to learn more about these relationships.

Using Cox regression, the final section looked to determine exactly which variables were driving the difference between the treated groups and the non-treated group. The first model showed that two of the treatment modalities, outpatient and residential, significantly influenced the hazard. But these results did not hold once control variables were added. Results suggest that it was not the type of treatment that led to a decrease in the hazard but rather the number of days in the treatment and the number of days on supervision.

The next regression analysis looked at the number of treatment episodes (categorized as single episode and multiple episodes) as the independent variable. Again, neither independent variable remained a significant influence on the hazard once the covariates were entered into the model. Additionally, the same variables that were significant in the first treatment modality analysis were significant in this analysis. Thus, it does not appear to matter how many treatments a subject receives but rather the number days of treatment received and number of days under supervision in predicting their time until rearrest.

In sum, the findings suggest support for supervision and treatment in increasing time until rearrest. The days of total treatment and days under supervision variables were significant predictors in the two ways treatment was measured, suggesting these two factors, regardless of treatment modality or number of treatments received, affects time until rearrest. However, being in treatment does not seem to have any effect on the hazard. The next chapter discusses the implications of these findings, the limitations, and what these results might offer future research.

Chapter 5: Discussion

Over the past three decades, the number of drug offenders entering into the criminal justice system has increased at a remarkable rate. As harsher sentencing and mandatory minimum sentences have become more prevalent for drug offenses, drug treatment has become less of a focus for the criminal justice system. Limited resources and ideological shifts regarding drug offenders led to such changes, leaving drug offenders ill prepared to live substance free. Recognizing that drug use had to be addressed in order to break the criminal cycle of these offenders, drug courts, began to emerge and gain popularity nationwide. With a focus on treatment and supervision, drug courts can be found in all major U.S. cities today.

Drug treatment and its effect on illicit activity has been the subject of several research studies both in and outside of the drug court setting. These studies vary considerably on how treatment was measured and the outcomes of interest. Overall, however, the literature suggests that drug treatment does work in reducing future drug use and criminal activity. In most studies, length of treatment was a significant predictor in a subject's successful outcome. Determining which type of treatment leads to a more successful outcome compared to other treatments, however, has been difficult. Prior drug treatment research had been equivocal on this topic and research using drug court samples has not filled this void. With subjects in the drug court setting, a paucity of research has delved into the specifics of treatment. Most studies that have addressed drug treatment in this environment have measured treatment dichotomously as having been received or not. Few studies have explored treatment effect by modality and those studies

that have been hindered by few modalities in the sample or methodological limitations. The current study fills this research gap.

This study examined the treatment experience of a sample of 235 subjects who were placed in the Baltimore City drug treatment court evaluation. This sample consisted of individuals who were arrested for a drug charge and received probation, had a non-violent criminal history, and were diagnosed with substance addiction. During the three year follow-up period, subjects could have received one of eight drug treatment modalities or no treatment. Six of these treatments were included in the analyses.

The studies objective was to use survival analysis to explore whether time until arrest significantly differed between the type of treatment modality received and the number of treatments received compared to the non-treated group within the evaluation. The below section states the study's key findings.

Summary of Study Findings

With these objectives in mind, the current study first addressed whether all treatments modalities should be included when examining the effects of treatment on the outcome. Prior research using the current data omitted the non-certified treatment modalities of jail-based acupuncture and AA/NA. The current study explored whether this omission was warranted. Results showed that these groups did not perform significantly different from the non-treated group and these sample members were then placed into the non-treated group.

The study then addressed whether treated subjects had longer times to survival than non-treated subjects and if so, whether there was a difference by modality received

and the number of treatments received. Treated sample members did have significantly longer survival estimates than the non-treated sample, supporting the finding of prior drug treatment research. This then warranted further exploration as to whether treatment modalities significantly increased the amount of time until a re-arrest after controlling for pre-existing characteristics of offenders and other experiences that were correlated with modality received.

In the first analysis measuring treatment by first modality received, initial results showed that those who received outpatient and residential treatment did have significantly longer survival time than the control group, while the other modalities were not significantly related to survival time. Controlling for covariates, however, this finding did not hold up. The number of days in treatment and the number of days under supervision were the only variables significant in this model.

It does not appear, however, that currently being in a drug treatment alters time until rearrest. It would be reasonable to assume that being currently exposed to treatment, in some cases on a daily basis, would introduce pro-social behaviors into an addict's life that would alter the chances of being arrested. In this analysis, and in the analysis exploring the number of treatment episodes received, this was not the case. This suggests that there is a lasting effect of treatment, but not an immediate effect. Further, having more days of supervision reduced the subjects' hazard ratio.

This study also explored whether measuring treatment using the number of treatment episodes attended varied significantly from the non-treated group in time until rearrest. Subjects were categorized into single or multiple treatment episode categories, as there were not enough treatment episodes to warrant more definitive categories.

Findings from this analysis were almost identical to the first modality analysis. The same covariates in the first modality analysis remained significant in this model, while neither single nor multiple treatment episode group clients had significantly different survival times from the non-treated group when these extraneous variables were controlled. These results suggest that the number of treatments a subject receives does not appear to be related to time until rearrest, but that success is determined by the number of days in treatment and number of days under supervision.

Study Findings Compared to Prior Research

The findings from the current study are similar to prior analyses examining the BCDTC. Banks and Gottfredson (2003) found that supervision and certified treatment were the key factors in explaining time until rearrest and the current study supports this claim. Though the authors did not measure treatment by modality type or the number of treatments received, it appears that measuring treatment these two ways does not yield any further insight to understanding time until rearrest. Additionally, Gottfredson et al. (2005) found with the BCDTC evaluation sample that certified treatment influenced future criminal behavior. The current study found both of this variable to predictor the outcome as well.

Looking specifically at the literature on treatment modality, the current findings are more in line with the conclusions of Prendergast et al.(2002) and Orwin et al. (2000) conclusion that no one modality can be associated with a reduction in future criminal behavior. Both studies, however, did find that treatment was influential on client success and the current study supports this notion. Treatment does appear to be influential but not

one specific treatment can be declared most effective. But this finding deviates somewhat from other work conducted by Anglin and Hser (1990) and Johnson and Gernstein (2000). Anglin and Hser found that two modalities, one of which being methadone, predicted less future criminal involvement. This study did find that those who first attended methadone did have longer time to arrest, but the relationship was not significant. The current work, however, had only 17 sample members first attending this treatment modality which limited the power to detect difference between this treated group and the non-treated group. The findings from the current study did, however, concur with the authors that outpatient treatment had no effect on criminal behavior.

Another treatment modality found to effect client outcome in the work of Johnson and Gernstein's (2000) was correctional treatment. The authors found that this modality reduced future drug use. As Johnson and Gernstein's work did not focus on criminal behavior, a possible explanation for this disparate finding is that subjects may have reduced drug use after receiving this treatment in the current study, but this reduction in substance abuse was insufficient to reduce time to rearrest or that the effects of their treatment were not long lasting. This may also be the case when comparing the findings of the current study to those of Hser et al. (1998) and Donovan (1999) in which certain treatment modalities were shown to be effective in reducing substance use, but future criminal involvement was not an outcome of interest.

The current findings also varied from those of Saum et al. (2001) which indicated that longer treatment involvement was not related to client success (as measured by drug court graduation). The author's study, however, only focused on one treatment modality (therapeutic community) when measuring treatment, which may be an explanation for

this finding. If this modality is not well structured or well delivered, longer times in treatment will most likely have little effect on client behavior. The current study's inclusion of only certified treatments avoids this issue as there were certain minimum requirements for a treatment to be deemed as certified (well structured, multi-phasal, and with counseling sessions available). Two modalities (jail based acupuncture and AA/NA) in the current study did not meet these requirements and results showed that subjects attending these modalities did not perform any better than the non-treated group. This supports the 1997 findings of Mackenzie's large scale evaluation that noted that treatments that were not structured, did not offer multiple components, and did not allow for substantial and meaningful contact between client and provider were least effective. The only study that examined one of these non-certified modalities (White, Goldkamp, and Robinson's 2006 work examining acupuncture) in a drug court setting found no relationship between this modality and future criminal behavior, as was also found in the current work.

In general, the findings of the current study are somewhat consistent with Kleiman's (2009) assertion that drug treatment is not effective with the criminal justice population. With no treatment modality or modalities appearing to lengthen time to arrest compared to those who didn't receive any form of treatment, this position has some support with the current findings. The current study, however, does find that length in treatment does lengthen time to arrest. Though this is contradictory of Kleiman's view, the current study design cannot rule out that some other unmeasured characteristic(s) are accounting for this relationship.

Limitations

The central finding of the current study is that the null hypothesis that specific treatment modality or modalities perform better than the non-treated group could not be rejected. A possible explanation for this finding is that all treatment modalities are similar in terms of their effects on time to rearrest. Limitations of the study, however, may also explain this finding.

One limitation is that treatment available to sample members may not be structured and delivered in a way that current research argues is most effective. A new approach to treating substance abuse has recently garnered support in the literature. Known as the *adaptive treatment* or *stepped care* model, this treatment approach requires some mechanism to be in place to adjust the initial assessment as a consequence of participants' subsequent performance in treatment (DeMatteo, Marlowe, Festinger and Arabia, 2009). McKay (2009) further states that this treatment approach is characterized by aggressive attempts to stay in contact with the patient for extended periods, systematic monitoring of treatment response, and ongoing modifications to treatment in response to progress, or the lack of progress. Interviews with subjects during the self-report phase of the BCDTC evaluation highlight that treatment was not structured using this model. For example, several subjects stated that receiving jail-based acupuncture "didn't do any good" as there was no explanation of how it worked, what they could expect from the treatment, and no follow up care after their release from jail. Work by Marlowe, Festinger, Arabia, Dugosh, Benasutti, and Croft (2009) has found this treatment approach to be beneficial to drug court clients in a recent pilot study and future work should test

whether this new approach results in fewer rearrests and/or longer time to rearrest compared to traditional ways of offering treatment such as that offered in the BCDTC.

Another limitation of the study is that there was no information on prior needs of subjects. This prevented examination of whether a particular treatment modality would outperform any other modality when applied to the appropriate population. Ideally, subjects are assigned to different treatment modalities based on diagnosis of an individual's needs and subjects would respond better when placed in a treatment modality designed to address these needs. The current data set does not have a way to verify that this occurred and findings may be a result of this "mismatch" between client and treatment modality.

Sample size and follow-up time also limited the extent to which the current study could explore the issue of treatment sequence and other lesser represented treatment modalities. With a three year follow-up period, and with the average time until a subject received treatment being approximately 9 months, the data covers just a small time frame of a substance abuser's total treatment experience. This resulted in very small sample size for some treatments, which limited the power to detect differences across modalities. A longer follow-up time would allow for more of a subject's drug treatments to be included.

Lack of subject randomization into each treatment modality limits the conclusions that can be drawn from the current work. Without this research design, it is not possible to conclude just exactly which variable(s) fully explain the difference in time to rearrest between the groups. Though with the study's research design doesn't eliminate the possibility of selection bias influencing the outcome, the study did attempt to address this issue by controlling for a variety of background factors. The study, however, does give

researchers a set of predictor variables to consider when exploring survival time and randomized experiments would be the ideal method of exploring the influence of these variables. The current data set did not allow for such an exploration.

Additionally, we do not know the individuals' internal motivation for attending the treatment. It is reasonable to assume that internal motivation to participate in drug treatment is a predictor of client outcome. Such measures, however, were not captured by BSAS or by UMD personnel. Moreover, data are not available on the reasons why clients left treatment. Individuals who repeatedly conflict with treatment staff or have difficulty getting to treatment (e.g. due to lack of transportation, child care) are most likely to have shorter treatment stays regardless of the treatment modality they were attending. These factors may have an influence on client success but due to data limitations they are beyond the scope of the current study. Additionally, whether a drug court judge alters a client's treatment modality during the hearing and for what reason are factors not measured by these data. The data also are limited in a few variables that may influence a client's success, one of which is prior drug treatment. As the results here showed that time in treatment was a significant predictor of the outcome, it is plausible that treatment stays before subjects entered the evaluation would have an effect on time until failure. Clients that attended treatment prior to their randomization into the drug court evaluation apparently did not achieve abstinence from drug use as they would not have qualified for the study without a drug arrest.

Also missing from these data is the type of drug individuals abused. This omission, however, is not particularly problematic as there was very little variation in drug preference among clients who did have information on this variable. Primarily,

heroin, and to a lesser extent cocaine, were the drugs of choice for sample members.

Subjects in the study mostly resided in East or West Baltimore; areas containing thriving and well documented heroin and cocaine markets. Having missing data on subject characteristics, such as employment and marital status, would have been useful in order to explore the relationship between pro-social obligations and their relationship to time until arrest.

The generalizability of the current study findings is most likely limited to drug offenders in urban settings. The treatment resources available to sample subjects may vary greatly compared to another sample in a rural or suburban area as may also the assistance offered to guide drug abusers into treatment. Additionally, variation in drug of choice may be different in other settings, perhaps requiring different treatment experiences than depicted here. The components of the BCDTC, however, do not vary significantly from the “typical” drug court. Gottfredson et al. (2003) note that “as with majority of drug courts, the BCDTC screens clients for substance use, assigns clients to treatment provided by community-based organizations, requires at least three contacts per week with the treatment provider, two urine tests per week, and weekly or biweekly contact with the drug court judge in the initial phases. The typical drug court uses increased frequency of court status hearings, urinalysis, and treatment as sanctions for relapse, and 60% use short periods of incarceration. Finally, the typical drug court imposes incarceration sentences on defendants who are unsuccessfully terminated from the program. The BCDTC is like the typical drug court on these dimensions.” (p.178)

Study Strengths and Policy Implications

Despite these limitations, the study raises the issue that treatment modality may not be relevant in determining time to rearrest once other features of drug court are controlled. Treatment and supervision, the key elements of drug court, were the only two variables that were significant in both regression models, indicating that increased exposure to both lengthens time to this event. In regards to the central question of this work, this suggests that duration may be the most important of aspect treatment and not the type of treatment received. Given the limitations stated in the previous section, however, the current study cannot conclude that type of treatment does not matter. Rather, what we can conclude is that this study failed to reject the null hypothesis that treatment modality does not add to the explanation of time to rearrest once treatment and supervision are considered. It is these two factors that appear to explain the difference between the treated and non-treated groups and not any of the specific modalities available to subjects in the study.

Future Research

The current study answers some questions that were not answered previously. Consequently, however, it raises issues that future research should consider.

Based on the limitations of the current study, future research of this topic area would ideally utilize a randomized design in which subjects would be screened for appropriateness for each modality and then randomly assigned from their respective pools into treatment and control groups. Firm conclusions about the influence of

treatment on client behavior could be drawn from such a study design and issues of selection bias, pre-existing conditions/characteristics, and treatment/subject matching could be better addressed. Ideally, the treatment offered to subjects would follow the adaptive approach or stepped care model mentioned previously as this type of treatment has shown some benefit to a drug court sample and may become more prevalent in treatment settings found in drug courts. Larger sample size in each treatment modality would also benefit such a study as variability within measures would be reduced.

Analyzing survival times to other outcomes, other than time to an arrest, would also benefit future research that aims to fully understand the impact of drug treatment on an addict's life. An arrest is often times the end result of an individual's downward spiral, with elongated periods of negative life events preceding this outcome. Knowing when these life events are likely to occur could assist in identifying time at risk. For example, time until unemployment and time until relapse are outcomes that would alert drug treatments practitioners when to look for negative changes in an individual that increases the probability of an arrest occurring. Such proactive steps would not only save criminal justice system resources in terms of judicial processing and correctional monitoring, but also give individuals the tools to break the cycle of drug abuse and arrest.

Finally, future research should consider the variability within modalities that occurs. There may be overarching uniformity in the structure of each modality represented in the current study, but the specifics of a treatment can vary greatly. The staff-to-client ratio, the experience of the staff, resources available, where and when the treatment is offered, and any additional services besides drug treatment that a treatment may offer are just some examples of how treatments can vary. With a population such as

the current study's, in which drug addiction is so severe that it leads criminal justice involvement, any nuance of treatment is worth exploring to determine its impact on client success.

Appendix

List of Variables

Variable	Type of Variable	Time Varying	Source(s)	Coding Scheme
<u>Dependent Variable</u>				
Time until Rearrest	Ratio	No	Maryland Division of Parole and Probation, Maryland Department of Corrections, Baltimore City Jail, Circuit and District Court documents, Client Self-Reports, BSAS Treatment Data	Number of Days
<u>Independent Variables</u>				
First Treatment Modality	Nominal	No	Client Self-Reports, BSAS Treatment Data	0=Not Received 1 = Received
Number of Treatment Episodes	Ordinal	No	Client Self-Reports, BSAS Treatment Data	0 = None 1 = Single Episode 2 = Multiple Episodes
<u>Control Variables</u>				
Original Court Assignment	Nominal	No	Maryland Division of Parole and Probation	0 = District 1 = Circuit
Randomized to Drug Court	Nominal	No	Maryland Division of Parole and Probation	0=No 1= Yes
Age	Ratio	No	Maryland Division of Parole and Probation	Years (at beginning of study) 0 = Caucasian
Ethnicity	Nominal	No	Maryland Division of Parole and Probation	1 = African American 2 = Asian
Gender	Nominal	No	Maryland Division of Parole and Probation	0 = Female 1 = Male

Variable	Type of Variable	Time Varying	Source(s)	Coding Scheme
Number of Prior Arrests	Ratio	No	Maryland Division of Parole and Probation	Number of Arrests
Number of Prior Convictions	Ratio	No	Maryland Division of Parole and Probation, Maryland Department of Corrections	Number of Convictions
Under Supervision at Censor Date	Nominal	Yes	Maryland Division of Parole and Probation, Maryland Department of Corrections, Client-Self Reports	0 = No 1 = Yes
In Drug Treatment at Censor Date	Nominal	Yes	BSAS, Client Self-Reports	0 = No 1 = Yes
Days in First Treatment	Ratio	No	Maryland Division of Parole and Probation and Client Self-Reports	Number of Total Days in Treatment
Total Number of Days in Treatment	Ratio	No	Maryland Division of Parole and Probation and Client Self-Reports	Number of Total Days in Treatment
Total Number of Days Supervised	Ratio	No	Maryland Division of Parole and Probation and Client Self-Reports	Number of Total Days Supervised
Number of Modalities Attended	Ratio	No	Maryland Division of Parole and Probation	Number of Modalities Attended

Bivariate Correlations- Treatment Measured by First Modality (N = 235)

	a.	b.	c.	d.	e.	f.	g.	h.	i.	j.	k.	l.	m.	n.	o.	p.	q.	r.	s.	t.	u.
Dependent Variable																					
a. Time Until Rearrest																					
Independent Variable																					
b. Methadone	.058																				
c. Outpatient	.110	-.255**																			
d. Intensive Outpatient	.009	-.191*	-.358**																		
e. Detoxification	-.103	-.090	-.169*	-.126																	
f. Residential	.002	-.145	-.272**	-.203*	-.096																
g. Correctional	-.148	-.102	-.191*	-.075	-.067	-.108															
Covariates																					
h. District Court	-.096	.022	-.090	.035	.000	-.103	.108														
i. Randomized to Drug Court	.252**	-.208	.190*	.137	-.092	-.170*	-.105	.164*													
j. Age	.203**	.235	-.120	-.049	.016	.029	.157	.077	.012												
k. African American	-.028	.033	.011	.104	-.028	.053	.087	-.100	-.006	.161*											
l. White	.039	.033	-.011	-.104	.028	-.053	-.087	.116	.023	-.154*	-.977**										
m. Asian	-.050	. ^a	. ^a	. ^a	. ^a	. ^a	. ^a	-.070	-.079	-.046	-.189**	-.022									
n. Male	-.047	-.033	-.080	.048	-.055	.056	.168*	-.064	.002	.073	.299**	-.281**	-.110								
o. Number of Prior Arrests	-.108	.064	-.164	.163	.012	.133	-.037	.033	.035	.192**	.028	-.019	-.041	.230**							
p. Number of Prior Convictions	-.096	.018	-.126	.147	-.014	.098	-.082	.084	.090	.179**	-.008	.016	-.035	.160*	.845**						
q. On Supervision at Censor Date	-.309**	-.131	-.021	.098	.075	.001	.068	-.273**	-.157*	-.088	.130*	-.147*	.070	.147*	-.004	.016					
r. In Treatment at Censor Date	-.152*	.035	.073	-.126	.032	.065	.072	-.028	-.045	-.023	-.033	-.003	.171**	-.123	-.089	-.127	.127				
s. Days in First Treatment	.300**	.413	-.042	-.105	.015	-.015	-.049	-.135*	.064	.084	-.045	.053	-.033	-.096	-.032	-.043	-.086	.192**			
t. Total Number of Treatment Days	.388**	.290	.010	-.033	.067	-.004	-.086	-.153*	.149*	.077	.010	-.002	-.039	-.094	-.061	-.085	-.096	.170**	.868**		
u. Number of Days Under Supervision	.663**	.035	.143	.015	.080	-.116	-.190	-.236**	.098	.137	-.003	.029	-.084	-.063	-.149	-.131	-.093	-.160*	.290**	.413**	
v. Number of Treatment Episodes	.281**	-.085	.180*	.202*	.121	-.056	-.118	-.110	.238**	.048	.125	-.117	-.051	.019	.004	-.052	-.019	.102	.238**	.594**	.282**

** $p < .01$, * $p < .05$, a. Cannot be computed because at least one of the variables is constant.

Bivariate Correlations- Treatment Measured by Number of Treatment Episodes (N = 235)

	a.	b.	c.
Dependent Variable			
a. Time Until Rearrest			
Independent Variable			
b. Single Treatment Episode	.048		
c. Multiple Treatment Episode	.204**	-.341**	
Covariates			
d. District Court	-.096	-.041	-.126
e. Randomized to Drug Court	.252**	.046	.185**
f. Age	.203**	.040	.075
g. African American	-.028	.022	.108
h. White	.039	-.011	-.104
i. Asian	-.050	-.052	-.028
j. Male	-.047	-.042	.036
k. Number of Prior Arrests	-.108	.060	.006
l. Number of Prior Convictions	-.096	.010	-.012
m. On Supervision at Censor Date	-.309**	-.019	.027
n. In Treatment at Censor Date	-.152*	.216**	.050
o. Days in First Treatment	.300**	.457**	.027
p. Total Number of Treatment Days	.388**	.262**	.401**
q. Number of Days Under Supervision	.663**	-.068	.252**
r. Number of Treatment Episodes	.281**	.137*	.805**

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

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