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Treatment and Program Effects in a Violence Reduction Program

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Abstract
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Wendy McClanahan

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TREATMENT AND PROGRAM EFFECTS IN A VIOLENCE REDUCTION PROGRAM

Dedication page

I dedicate this to my father, Dr. John Henry Thomas III. I wish he was still here to experience the end of this journey with me (and remind me that I am not a "real" scientist). I love you.

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ABSTRACT

TREATMENT AND PROGRAM EFFECTS IN A VIOLENCE REDUCTION PROGRAM

Wendy McClanahan

John MacDonald

Because serious crime has widespread negative effects on communities, families and our nation's young people—we must make our "go to" responses—such as policing, probation and incarceration—more effective. The current study will examine an intervention that aims to reduce recidivism through support and supervision of serious youthful offenders who live in high-crime urban neighborhoods. The program aims to bridge a critical tension faced by probation—the dueling goals of social control and social welfare (i.e., punishment and rehabilitation). Specifically, this research will aim to 1) determine the extent to which the program *causes* decreases in recidivism and 2) explore if level of contact with program staff (street workers) is related to recidivism outcomes. The information gleaned from this study will be useful to both researchers interested in serious and persistent youthful offenders and to practitioners and policy makers aiming to reduce serious crime and optimize community corrections.

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CHAPTER 1: THE PROBLEM OF SERIOUS CRIME AND VIOLENCE

Despite declines in crime nationally over the past eight years (FBI, 2010), citizens and leaders of urban areas across the nation still routinely cite crime and its related disorder as one of the most significant problems facing their cities and neighborhoods (see for instance, E-town Panel, 2006; Futrell et al., 2010): robberies, aggravated assaults, homicides, drug use and dealing and other crimes still number in the hundreds of thousands across our urban areas every year and place residents and their communities in jeopardy.

Historically to address this problem, Americans have relied primarily on criminal justice responses to improve public safety and reduce crime. In fact, due to increasingly punitive criminal justice policies over the last part of the 20th century, today there are more than 7.3 million Americans under some form of criminal justice supervision (The Pew Center on the States, 2009) at a cost of more than $185 billion per year on police, courts and corrections (Hughes, 2006). Yet, many experts believe that the "get tough on crime" movement of the 1980s, including harsher sentencing and, therefore, increased incarceration, did not result in benefits that justify its cost (both financially and to communities; Lynch & Sabol, 1997; The Pew Center on the States, 2011). In fact, there is evidence that traditional reactive policing methods have not produced as much success as proactive and place-base approaches designed to prevent serious crime (Braga, 2001; Braga et al., 1999; Cohen, Gorr, & Singh, 2003; Cohen & Ludwig, 2003; Sampson & Cohen, 1988; Sherman & Weisburd, 1995). Conventional approaches to probation and parole have met with criticism from experts, primarily because they do not seem to have strong crime-reducing effects among the most serious and persistent

offenders (Gill, 2010; Petersilia, 1997; Solomon, Kachnowski & Bhati, 2005).[1] Mass incarceration, arguably the most common result of the implementation of "get tough" policies, comes at a high cost to governments and taxpayers; yet, still about 40 percent of inmates return to incarceration within three years of release (The Pew Center on the States, 2011) —a dismal outcome if the goal is to reduce recidivism as well as crime. Recently, budget shortfalls dictated by the recession of 2008-2009, as well as building evidence that "traditional" criminal justice approaches are resulting in moderate impacts on national crime rates while requiring huge expenditures (see Anderson, 1999 and Levitt, 2004 for some estimates), has resulted in a shift in thinking about how to best structure responses to crime, both criminal justice, and non-criminal justice. Efforts are underway to reform policing, community corrections, and incarceration. Furthermore, more attention is being paid to prevention as an alternative approach (although it should be viewed as a complimentary approach) to reducing crime.

The Impact of Serious Crime

While changes are afoot, our nation's inability to effectively control and reduce crime, particularly serious crime, has resulted in vulnerable communities and families; individuals, both victims and perpetrators of these crimes, also face serious negative outcomes. For instance, street violence is an epidemic among our nation's poor urban male youth: murder is the second leading cause of death for males 15 to 24 years old and the number one cause of death for African American males in this age group (CDC, 2007, 2009). According to the Federal Bureau of Investigation, in 2010 there were an estimated 87,771 robberies in the U.S., and approximately 4 in 10 involved a firearm; 60

[1] In the last year, states nationwide began initiatives to increase their efforts for stricter parole enforcement. For instance, Illinois began requiring that parolees follow stringent new regulations, including verifying whereabouts on a daily basis, visiting a parole officer twice a week, and refraining from drinking liquor or having alcohol at their homes. Some parolees are being sent back to prison for testing positive for drug use or simply smelling of alcohol, transgressions that rarely landed individuals back behind bars prior to these reforms (O'Connor, 2010). It is still unknown, however, to what extent these changes will lead to reduced rates of violence.

percent were committed by young people between 15 and 24 years old, almost all of whom were males. Perpetrators of serious crimes will face deserved criminal justice sanctions, including prison time; if they are ever released (and most are), they will likely still have unmet needs and will therefore face a myriad of reentry challenges likely to perpetuate their involvement in crime. Those who are supervised in the community will face similar challenges. This is because young people who get involved in serious and persistent crime often live in impoverished, high-crime, urban neighborhoods, where there are limited services and resources to meet their needs. Furthermore, the reasons youth become involved in serious crime to begin with are multifaceted and run deep in families and communities. In addition to a host of personal characteristics that may contribute to early, frequent and persistent involvement in crime, some of these youth come from families that participate in or accept criminal behavior; they may suffer from child abuse, neglect or other family conflict. They often struggle throughout childhood with economic disadvantage and the ramifications of living in high-poverty neighborhoods. And, they are often witness to or victims of violence or serious crime, experience high rates of academic failure and have criminally involved or delinquent peers or siblings (Hawkins et al., 2000). As teens and young adults, some (not all) of these young men become involved crime and acquire a history of persistent and early delinquency. In some ways, they are our nation's "throw away" youth—many are urban males of color who become engaged in crime at an early age, drop out of school, and withdraw from the formal labor market (see, for example, Farrington, 1998).

Many of these young people have children and families of their own and therefore, the impact of their involvement in crime not only threatens their own well-being, but also affects their family through loss of income, fear and potentially loss of or separation from a loved one. Children of those involved in serious and violent crime fare

the worst—experiencing poor school performance, separation and potential changes in caregiving arrangements due to incarceration of a parent, and even continued perpetuation of crime (Bloom & Steinhart, 1993; Boswell & Wedge, 2002; Davis, 1993; McDermott & King, 1992; Murray & Farrington, 2005; Murray, Janson, & Farrington, 2007; Shaw, 1987, 1992). Neighborhoods, especially our nation's already fragile inner-city low-income neighborhoods, are also impacted by high concentrations of serious and violent crime (Krivo & Peterson, 1993). High rates of neighborhood violence and crime have led to urban flight (Morenoff & Sampson, 1997) and fear among residents that can hamper a community's ability to organize to fight against crime and violence (Skogan, 1986) which will, in turn, increase the opportunity for local youth to get and stay involved in crime. Furthermore, high crime communities have fewer strong social institutions—and as a result, youth experience unsafe schools and limited employment opportunities. This can lead to further neighborhood economic decline as residents who cannot or chose not to leave become increasingly impoverished and, possibly dependent upon crime (see, for instance, Wilson, 1987).

Finally, violence and other serious crime also have significant economic implications for society beyond just criminal justice costs. For instance, according to Corso and colleagues (2007), in 2000 in the US the costs of lethal and non-lethal interpersonal violence society accrued medical and lost-productivity costs of $59 billion due to lethal and non-lethal interpersonal violence. Some of the medical costs accrue to uninsured individuals, and while lost employment is the most obvious way productivity is impacted by violent crime, tax revenue, volunteerism, spending and other contributions to the economy are also affected. Anderson (1999) estimates a more comprehensive cost of crime to society. He found that total annual costs of crime, including direct costs, opportunity and social costs, and transfer costs, approximately equaled the amount

spent on health care per year. His estimated cost, in 1997 dollars, totaled $4,118 per capita.

An Overview of the Study

Because crime, especially serious crime, has widespread negative effects on governments, communities, families and our nation's young people—we must make our "go to" responses—such as policing, probation and incarceration—more effective as years of increased spending on imprisonment and policing,[2] as well as increased probation rolls, have not solved the crime or violence problem. This study examines an intervention that aims to break the cycle of persistent criminal offending by reducing recidivism, particularly for violent offenders, through enhancing probation. The program, the Youth Violence Reduction Partnership (YVRP), is specifically targeted to serious and persistent youthful (between the ages of 14 and 24) offenders who live in high crime urban neighborhoods. The program aims to bridge a critical tension faced by probation—the dueling goals of social control and social welfare (i.e., punishment and rehabilitation)--by providing participants with both intensive supervision from probation and police/probation patrols and support/connection to services from a street worker who works in close partnership with the probation officer. Specifically, the research aims to 1) determine the extent to which YVRP *causes* decreases in recidivism among participants and 2) explore if contacts with program staff, specifically, street workers, is related to recidivism outcomes for participants. The information gleaned from this study will be useful to both researchers interested in youth violence and to policy makers looking for programmatic efforts that practitioners can use to reduce it.

[2] Levitt (2004) found that increasing policing and imprisonment rates explained only about half of the decline in crime rates observed through the 1990's. Specifically, increasing the number of police officers per capita, the incarceration rates for drug-related offenses, the revocation rates of parole and the length of sentences for those convicted of crimes were particularly strong factors for explaining the decline seen during that decade. Baumer (2008) found similar results in his analysis of Uniform Crime Reports data from 1980 to 2004 in over 100 US cities with populations over 100,000.

Structure of this Paper

In the following sections I provide more background on and information about the proposed study. First, I provide a literature review presenting the current state of knowledge about efforts to reduce serious crime and violence, emphasizing prevention approaches, where we most commonly employ social welfare programs, and intervention approaches, where we most commonly employ social control efforts through the criminal or juvenile justice systems. The third chapter provides details on the study presented herein, as well as background on the Youth Violence Reduction Partnership, the program that is the topic of this research. The fourth chapter explains the methodology and provides an overview of causality, focusing particularly on the potential outcomes framework. Chapter five presents the analyses associated with creating the propensity scores used in many of the analyses. Chapter six includes results of the main analyses, answering the two main research questions of interest. The final chapter is a discussion section, which includes a summary of findings, study limitations, and concludes with substantive and programmatic implications that arise out of the study.

CHAPTER 2: SUBSTANTIVE BACKGROUND

Efforts to reduce serious crime and violence fall into two main camps—criminal justice responses which emphasize social control though supervision and punishment, and non-criminal justice efforts which have focused on social welfare programs. Criminal justice responses fill the *intervention* space—attempting to reverse the undesirable behavior. Non-criminal justice responses have most often taken the form of *prevention* efforts, where the targets of the program are youth who have not yet engaged in crime or have not been officially sanctioned for engaging in crime (and therefore are not yet engaged in the juvenile or criminal justice systems) and the goal of the program is to *keep* or prevent them from engaging in it in the future.

Traditionally, criminal justice responses have been largely reactive and based on the premise that increasing supervision, removing criminals from the community and punishing them will lead to reduced crime—in part by making other would-be criminals think twice before breaking the law. As opposed to the prevailing mantra in the 1970s and 1980s, more recent research has suggested that rehabilitation can work and therefore, recent best practices in criminal and juvenile justice include a myriad of alternatives that include rehabilitation and treatment principles as well as supervision, punishment and control (see, for instance, Andrews, Bonta & Hoge, 1990). This section summarizes the current state of knowledge about what works to reduce serious crime in each arena.

Prevention: Non-Criminal Justice Efforts to Reduce Serious Crime and Violence

In general, there is more confidence among practitioners and researchers in the efficacy of prevention efforts with children and juveniles who have not yet become

involved in serious crime (Caldwell & Van Rybroek, 2005) than in intervention and criminal justice responses. Multi-strategy programs that incorporate elements of the many ecological environments in which youth are immersed, such as family, school, and community, seem particularly successful in preventing crime and violence (Dusenbury, Falco, Lake, Brannigan, & Bosworth,1997; Greenberg, 2001; National Institutes of Health, 2006; US Department of Health and Human Services, 2001). Crossing over these contexts provides reinforcement of prevention efforts and messages.

Family focused programs. First, theories of crime and delinquency point to parenting and family characteristics as key factors in negative outcomes for youth (social control theories: Gottfredson & Hirschi, 1990; Hirschi, 1969; social learning theory: Burgess & Akers, 1966; developmental: Laub, Sampson & Allen, 2001; Sampson & Laub, 1990; Sampson & Laub, 1992; Sampson & Laub, 1993), yet research to substantiate these theories has mixed results. While some studies have shown that family can be a key protective factor among youth at risk for engaging in serious crime and violence (see for instance, Gorman-Smith, Henry & Tolan, 2004), others have shown that family may not have as large of a direct impact as these criminological theories would suggest (Derzon, 2010). Nonetheless, a review conducted by Piquero and colleagues (2009) shows strong support for family and parenting programs as an efficacious approach to delinquency and problem behavior reduction (Piquero, Farrington, Welsh, Tremblay, & Jennings, 2009). And individual evaluations of some programs have shown significant and long term impacts of family focused programs (see for instance, Alexander et al, 1998; Borduin et al., 1995; Henggeler, Melton & Smith, 1992; Henggeler, Melton, Smith, Schoenwald, & Hanley., 1993; Olds et al., 1998; Reynolds et al., 2007; Reynolds**,** Magnuson & Ou, 2010; Schaeffer & Borduin, 2005; Sexton & Turner, 2010**).**

School-based programs. School is a context the presents both opportunity for crime and violence as well as for preventing or intervening in it. Moreover, a commitment to school has been shown to provide protection against factors that place a child at risk for engaging in serious crime (US Department of Health and Human Services, 2001). And therefore, perhaps not surprisingly, school-based programs have also shown promise in reducing delinquency and violence (see for example, FindYouthInfo, n.d.; Barnoski 2004, Botvin, Griffin & Nichols, 2006; Bruene-Butler, Hampson, Elias, Clabby, & Schuyler, l997; Conduct Problems Prevention Research Group, 1999, 2002; Glick & Goldstein, 1987; Hawkins, Catalano, Kosterman, Abbott & Hill, 1999; OJJDP n.d; Olweus, 1994; Webster-Stratton, Reid & Hammond, 2004; Webster-Stratton & Hammond, 1997; Webster-Stratton & Reid, 1999 a,b; U.S. Department of Education 2001). School based programs adopt one of two approaches: they can aim to reach all youth in the school irrespective of their risk of delinquent behavior, or they can be targeted at students who are presenting with behaviors or characteristics that place them at high risk of engaging in crime. Overall, targeted school based programs have larger violence-reduction effects than prevention programs that focus on <u>all</u> students (Wilson & Lipsey 2007; Wilson, Gottfredson & Najaka, 2001). These programs almost always have higher doses than universal or primary prevention strategies (Fields & McNamara, 2003) and also have a greater potential to reduce negative behavior if targeting is done correctly (e.g., youth who are targeted for the program are actually youth who are going to engage in the behavior of interest). Furthermore, prevention programs that target the school's culture seem to perform favorably when compared to curricular approaches (U.S. Department of Health and Human Services, 2001). This may be because climate programs are often implemented

with greater fidelity than curriculum-based programs (Crosse et al., 2001; Gottfredson et al., 2000) and are often longer-term and more intensive (Mihalic & Irwin, 2003).

Although targeted interventions tend to have larger effect sizes than programs serving all students, these "universal" prevention strategies can also produce impacts, albeit smaller in size. Furthermore, there are clear advantages to universally school-based programs: all youth are included in the program and may benefit, they can be implemented in a natural setting and with a typical school process, since youth are not targeted, they will not be subjected to stigma or labeling (Fields & McNamara 2003); and youth are not served in groups bringing together at-risk youth and reinforcing negative behaviors through "deviancy training" (Dishion & Dodge, 2005; Dishion, McCord & Poulin, 1999).

While holding great promise, not all school based programs have demonstrated impacts. Programs that are seen as add-ons or mandated by external forces are likely to be implemented inconsistently or possibly not at all, resulting in poor student outcomes. Similarly, successful school based prevention programs should involve long term efforts (Dusenbury, Falco, Lake, Brannigan & Bosworth, 1997; Greenberg, 2001). Ideally, prevention strategies should be implemented beginning in early elementary school and reinforced through high school. Starting prevention activities early allows children to develop the skills and attitudes they need to avoid serious crime and violence before its peak in adolescence. Finally, the downside of school based programs is that youth who are chronically absent or have dropped out of school—many of whom may be among the youth most at risk for engaging in serious crime—will not be touched by the program.

Community-based programs and other related approaches. Although community is a key system in which youth develop, few community *prevention* approaches to

serious crime and violence prevention have been evaluated. Communities that Care is one notable exception; it combines proven effective prevention programs specific to the needs of each implementing community. Early results of the evaluation suggest that the approach is effective in preventing delinquency at seventh grade—but its impact on later serious crime or violence has not been assessed (Hawkins et al., 2008).

Finally, there are other approaches that have been shown to prevent serious crime; approaches that involve adults in the lives of youth. Mentoring has demonstrated violence and crime prevention effects (Tierney, Grossman & Resch, 2001). It is posited that mentors help because youth without strong and supportive adults in their lives are at greater risk for negative outcomes, including engaging in crime and violence (Walker & Freedman, 1996). Borowsky and colleagues (2004) demonstrated that primary care physicians have the ability to take steps that will reduce violence among youth. In their study, randomly selected children and adolescents who received a psychosocial screening and whose parents were offered a parent training had reduced levels of violence as compared with those were who not selected to receive the intervention. Finally, a comparison group evaluation of a gang prevention program implemented in Boys and Girls Clubs also demonstrated delays in deviant behavior (Arbreton & McClanahan, 2002).

Summary. Taken as a whole, several prevention efforts have undergone rigorous evaluation and have established effectiveness in preventing serious crime and violence. However, with the exception of some therapeutic interventions, like multisystemic therapy, they have not been designed to reduce crime among those already deeply engaged in the justice system, and therefore, do not work to reduce recidivism among existing serious offenders. Nonetheless, the promise of prevention programs, along with a growing acceptance of the illegitimacy of Martinson's (1974)

claim that rehabilitation does not work, has ignited interest in finding programs that will meet the criminogenic needs of offenders. Emphasis has been placed on employment programs, housing options, reunification support, and treatment for mental health, physical health and drug/alcohol problems. However, the main point of intervention for serious and violent young offenders remains the juvenile or criminal justice system, which I turn to in the next section.

Intervention: Criminal/Juvenile Justice Efforts to Reduce Serious Crime and Violence

The decision to intervene to reduce crime is premised on the notion that it is possible to change criminal behavior within individuals. And despite ample evidence of stability of antisocial behavior in some populations (Caspi, Elder & Bern, 1987; Huesmann, Eron & Lefkowitz, 1984, Jessor, Donovan, & Costa, 1991; Loeber, 1987; Olweus, 1979), policy makers continue to search for ways to reduce offending behavior in criminal populations or those at high risk for committing criminal acts. Indeed, our earliest notions of the criminal justice system were steeped in the belief that certainty, celerity (speed) and severity of punishment would deter would-be criminals and potential recidivists from committing offenses (Beccaria, 1986/1767). And we are currently in a policy environment where more attention than ever is being paid to *rehabilitation* of offenders, and not just punishment.

In the criminological literature there has been great debate about the nature of stability in antisocial and criminal behavior over the life course (see Sampson & Laub, 1990 for an excellent review). Some criminologists believe that childhood antisocial behavior does not affect adult antisocial behavior directly, but the relationship operates through an innate propensity for crime shown through self-control developed early in life (Gottfredson & Hirschi, 1990). Nagin and Paternoster (1991) adopt a slightly different

argument in favor of a causal impact of childhood antisocial behavior on adult criminal behavior. Nonetheless, much of the historic literature on continuity and stability comes from offender studies, and in reality, many antisocial children and adolescents go on to live crime-free lives as adults. Others engage in criminal behavior across their lifetime, but at varying levels. Indeed, evidence suggests that even as very serious and persistent offenders age they are significantly less likely to engage in criminal activity. Because of this evidence and our deep-rooted societal belief that intervening to change trajectories should be successful, we have a myriad of policies designed to intervene to reduce recidivism even after an individual has committed one, or several, crimes.

Intervention programs, that is activities designed to reduce violence or serious crime among youth who have been or are currently engaged crime are most often criminal justice responses[3] and include policing, courts and corrections, including community corrections and incarceration. Innovations and new programmatic approaches have been employed in all arms of the juvenile and criminal justice systems in recent years in an effort to promote better outcomes for criminals and victims; however, Taxman (2002) suggests that there has been more interest in innovation than there has been effort trying to document what works and doesn't within the system itself. Below I focus on explicitly on the workings of the various justice entities responsible for public safety and crime reduction.

Policing. A landmark study in the 1970's convinced many scholars and practitioners that policing did not deter or reduce crime (Kelling et al., 1974). Although the conclusions drawn from that study were challenged by other research demonstrating policing could work, skepticism ensued, and with good reason. A review by Weisburd and Eck (2004) finds little empirical support for "traditional" policing efforts, such as rapid

[3] Lipsey's 2009 review of programs for juvenile offenders found that only 18 percent of the evaluated programs serving this population were serving youth not currently involved in the justice system.

response to 911 calls, large scale community patrol, and marginally increasing the numbers of police (assuming they focus on arrest and punishment).

After the rapid growth of serious crime in the 1980s, the 1990s brought a call for policing reform that would result in safer neighborhoods. More tailored, focused and proactive strategies were tested, such as problem-oriented policing, hot spots policing and crackdowns, and community policing (Weisburd & Eck, 2004). These approaches often overlap in execution, and there are some commonalities that are worth noting. First, both problem-oriented and community policing utilize partnerships. In traditional policing models, little information was gathered from or shared with other community institutions or residents. Full implementation of problem-oriented or community policing often results in partnerships that help identify needs and solve community issues related to crime. Despite the theoretical importance of partnership, a 1998 study by Mazerolle and colleagues found that "place managers" —those individuals who discourage crime in particular geographic areas, like landlords—were no more effective in areas where they collaborated with police then where traditional policing occurred. But later research by Mazerolle suggested that partnerships with third-parties can be critical to policing success (Mazerolle, Kadleck, & Roehl, 1998; Mazerolle, Soole, & Rombouts, 2006).

Hot spots and problem-oriented policing are two strategies that grew out of the 1990's call to action. They are both tailored to specific geographic areas—ideally areas much smaller than traditional patrol units. And, research suggests that both hot spots and problem-oriented policing can be effective strategies to reduce crime in target areas (Berk & MacDonald, 2010; Braga, 2001; Braga & Bond, 2008; Braga, Kennedy, Warring, & Piehl, 2001; Cohen, Gorr & Singh, 2003; Cohen & Ludwig, 2003; Green 1995; Sherman & Weisburd, 1995; Weisburd, Waring, Mazerolle, Spelman & Gajewski, 1999). Policing "disorder", in the "broken windows" sense, has also been widely employed, and

is often a component of problem-oriented approaches (Weisburg & Eck, 2004). For instance, research on the SMARTS program by Green (1995) shows that enforcing code violations reduced drug crime in the targeted area. Other claims are more anecdotal than evidence-based; Kelling and Bratton (1998) believe that policing disorder was related to the steep decreases in crime experienced by NYC in the 1990s. An early study by Sampson and Cohen (1988) showed that aggressive policing on disorder, defined as DUI and disorderly arrests per officer, reduced crime.

One of the central questions when place-based police strategies do work in reducing crime is if the crime simply displaced to another geographic area. In other words, is it a zero-sum game (Cohen, Gorr & Singh, 2003; Sherman & Weisburd, 1995; Weisburd & Eck, 2004)? Research suggests that displacement of crime is not common; more often there are diffusion effects of the crime control effort that result in safer surrounding geographies as well as the primary geographical targets of the policing intervention.

Finally, a considerable investment has been made in community policing efforts. Community policing is not a single model, but comprised of multiple approaches to proactive policing in partnership with community leaders, residents and institutions. A review of the literature suggests that community policing efforts are difficult to evaluate because of their heterogeneity; but that they have most often demonstrated success not in crime reduction per se, but in reducing residents' fear of crime. Furthermore, although question remain about the extent to which funding was used to implement community policing models, research suggests that the COPS program did result in crime reduction benefits for many grantees (Evans and Owens, 2006; Zhao, Scheider & Thurman, 2002).

Courts. As with policing, courts have employed new approaches and techniques. After the first problem-oriented court, the Miami Drug Treatment Court, was started in 1989 as an alternative to incarceration to treat the root cause of the crime and not simply impose punishment, problem-oriented courts have proliferated, as drug courts, reentry courts, and domestic violence courts have been developed, evaluated and replicated throughout the country. The goals of problem-oriented courts are to address the criminogenic needs of offenders through treatment or rehabilitative services while providing supervision.

Drug courts, as a subset of specialty courts, have received the most research attention. As such, several meta-analyses of drug courts have been disseminated in recent years. Overall, they seem to suggest that drug courts in aggregate reduce recidivism about 10 percentage points (Aos, Miller, and Drake 2006; MacKenzie, 2006; Shafer, 2006; Wilson, Mitchell, and MacKenzie, 2006). More rigorous studies of drug courts have yielded smaller effect sizes (see, for instance, GAO, 2005), but still report positive crime reduction benefits among participants. Furthermore, Farole, Remple, Byrne and Chang (2008) found in their survey of over 1000 judges that they support problem-solving courts and embrace many of the associated principles. Despite the positive impacts of the approach in general, Downey and Roman (2010) conclude from their cost-benefit study of the courts that drug court costs seem to outweigh the benefits of the program from a monetary perspective. In part, this is due to the fact that drug courts seem to have effects on lower risk and cost crimes. Furthermore, in order to achieve the arguably modest benefits touted in previous studies, the drug court must be implemented with high quality—no small feat for programs of this nature. The literature also suggests that drug courts do not enjoy success with juvenile offenders.

Other court innovations include diversion programs. Diversion programs "attempt to divert, or channel out, youthful offenders from the juvenile justice system" (Bynum & Thompson, 1996: p.430). This approach is based on the notion that engaging youth in "the system" may do more harm than good—both for the youth himself and for the system more broadly (Lundman, 1976). Research on the efficacy of diversion programs has shown that they can be effective. Pogrebin, Poole, and Regoli (1984) demonstrated reduced recidivism among diversion program participants as compared to an experimental control group. Furthermore, research on the Adolescent Diversion Project in Michigan suggests that diversion programs may be effective for more serious juvenile delinquents, and not only for status or first time offenders (Davidson, Redner, Amdur & Mitchell, 1990). However, a recent meta-analysis of diversion programs for juvenile offenders indicates that taken as a whole they are not effective in reducing recidivism; yet, the same study suggests that family involvement in a diversion program may be the key to its success (Schwalbe, Gearing, MacKenzie, Brewer & Ibrahim, 2011).

Finally, reform in sentencing has also taken center stage in recent years. During the "get tough" era, the Sentencing Reform Act of 1984 was passed and federal sentencing guidelines were put into place in order to reduce discretion of judges and increase uniformity in sentencing. Furthermore, since recent research suggested that rehabilitation did not work, sentencing and corrections were now focused on retribution and not treatment. Possibly because of "get tough" policies, the United States has the highest incarceration rate of any industrialized nation, and convicts in the US are sentenced to longer sentences for similar crimes than peers in other countries. Because of the cost of corrections associated with the sentencing guidelines as well as the

acknowledgement that rehabilitation can work for some offenders, there have been recent calls for sentencing reform.

Unfortunately, there is little robust research on the effects of sentencing reforms on crime reduction because of design and data limitations (Wellford, 2007). We do know that sentencing reform has not solved the problem of disparate sentencing, but some have argued it has created modest improvements (Anderson, Kling & Stith, 1999; Hofer, Blackwell & Ruback, 1999). Others believe that these improvements don't exist, or have been offset by increased prosecutorial discretion. The 2005 Supreme Court decision in *United States v Booker* has resulted in increased in judicial discretion, but there remain questions about how the decision impacts racial disparity and, ultimately, crime rates (Hofer, 2007). Other changes in sentencing guidelines have appeared in recent years—and more, particularly a reconsideration of some harsher guidelines and mandatory-minimums, have been called for in a new report by the US Sentencing Commission (US Sentencing Commission, 2011).

Corrections. Corrections, which encompasses both incarceration and community-based supervision, is the final component of the criminal justice system. And like policing and courts, corrections has seen a shift in philosophy over the past 50 years that has resulted in significant reform. In the mid-1900's corrections was seen as a means to reduce crime through a myriad of approaches, including deterrence, incapacitation, punishment (just desserts), and rehabilitation (MacKenzie, 2006). In 1974, a seminal study declared that rehabilitation did not work (Martinson, 1974). Some experts assert that as a result of this claim, and the increase in the 1980s and 1990s in serious crime, corrections abandoned its rehabilitative goals in favor of a system driven by punishment and the belief that longer sentences would result in safer neighborhoods.

The result of this "get tough on crime" approach was mass incarceration and a dearth of efforts to provide services that would address criminals' criminogenic needs. The theory behind the movement was that harsher sentences would reduce crime in three ways. First, criminals, particularly the most active and serious, would be off the streets for longer and would not be able to commit crimes during that time. Second, the greater severity of the punishment would serve to further deter the offender from continuing to engage in crime once he was released. And finally, harsher sentencing and conditions would have a stronger deterrent effect on other would-be criminals than the shorter sentences of the past.

Recently, "get tough" corrections' (and sentencing) policies have been facing scrutiny, not only because the recent recession has placed undeniable pressure on governments to reduce spending on prisons and jails, but also because mass incarceration as a long term strategy might be untenable for social reasons. At the same time, there has been growing recognition that Martinson's claim that "nothing works" was inaccurate; some offenders predisposed to recidivate can be diverted from a life of crime. With rehabilitative supports, like drug treatment, and services, like employment training and placement, they will be able to live a crime-free life. And, finally, the results of the mass incarceration "experiment" are unclear. Researchers have struggled to ascertain with certainty how much, if any, of the recent decreases in crime are a result of "get tough" policies (versus other factors, like the aging population; Donohue, 1998; MacKenzie, 2006; Rosenfeld, 2000; Spelman, 2000;); nonetheless, most scholars agree that increases in incarceration and changes in sentencing probably *did* result in a decrease in crime. But there remain skeptics; some scholars believe that there is no impact on crime, particularly for those policies involving harsher sentences. For instance, recent research shows that there are limited returns from increasing the

severity of punishment—the cornerstone of the "get tough" movement. What seems to matter more in deterrence efforts is the *certainty* of punishment (Wright, 2010). Mandatory minimums (and other related sentencing policies), longer incarceration or supervision, and harsher confinement conditions seem unlikely to result in the perception of greater *certainty* of punishment—especially since there remains ample room for prosecutorial discretion. In this spirit, Durlauf and Nagin (2011) suggest that prison sentences should be made shorter in favor of additional funding for police, who can assure certainty, over corrections, which can only assure severity.

Overall, the crime reducing effects of incarceration are unclear. A review conducted by Donohue (2009), suggests that estimates of the impact of incarceration on crime rates has varied widely from study to study. This is, in part, due to different assumptions and model specifications (Donahue, 2009; MacKenzie, 2006). Furthermore, research shows that the relationship between incarceration and crime reduction is not linear; in other words it is not as simple as, "the imprisonment of an individual (x) leads to a reduction of y crime." This is because incarcerating low rate offenders is likely to result in a smaller impact on crime than the incarceration of high rate offenders (MacKenzie, 2006). And when incarceration rates are high, individuals entering prison have lower rates of criminal offending (Miranne & Geerken, 1991) which means that the benefit of incarceration will be lower (see Donahue, 2009 for estimates). Also, researchers have argued that the social impacts of mass incarceration outweigh any potential crime reduction benefits (Liedka, Piehl & Useem, 2006; Rose & Clear, 1998), and that a real assessment of the benefit of incarceration needs to take factors other than the crime rate into consideration. Researchers attempting to assess the impact of incarceration on individual behavior have met with even more challenges, as

estimating the rate, likelihood and duration of offending among prisoners has produced estimates that vary widely from study to study.

Despite the "get tough" mentality and Martinson's conclusion that nothing works, corrections have adopted or maintained several core programming efforts: education, vocation/employment, and therapy. Surprisingly, few high quality studies of corrections programming in these three areas exist, but utilizing those that do, MacKenzie (2006) has summarized "what works" in an effort to promote a culture of evidence based corrections. Overall, her meta-analyses conclude:

- Education programming in the form of GED training, adult basic education, and post-secondary education does reduce recidivism. The scope of the impact is moderate to small, depending on the type or combination of classes attended. Life skills education does not seem to have an impact on future crime.
- In the employment realm, only vocational education has an impact on recidivism; prison employment and work release do not have evidence of effectiveness.
- Therapy, specifically cognitive behavioral therapy, does reduce criminal behavior among offenders.

Like policing, where crime analysis is used to assesses risk and at the neighborhood or community level, corrections experts and researchers have also begun to explore the efficacy of risk assessment in corrections. Some have argued for incarceration only of those who present a high risk to public safety, utilizing community corrections for those who can be safely, and perhaps more effectively, supervised and rehabilitated in the community (Blumstein, 2011; The Pew Center on the States, 2009). Others have argued that correctional and rehabilitation *programming* should be provided

based on risk level. However, predicting who is at highest risk has presented a challenge to practitioners and researchers alike; clinical evaluations, the standard in risk assessment, have not demonstrated overwhelming accuracy, despite their wide-spread use. Newer actuarial assessments, which generate a risk score or categorization based on an algorithm generated by identifying the factors that are predictive of reoffending in a population (like age, gender, criminal history, academic achievement, etc.) have shown greater success (MacKenzie, 2006), but accuracy is still not as high as desired. Not surprisingly perhaps, few studies demonstrate support for these risk-based approaches.

Finally, prisons and jails have been facing increasing pressure to do more to meet the criminogenic needs of their prisoners to reduce the barriers they will face upon return to the community. Because of this pressure, in the past 15 years, corrections has focused more than ever on the transition from incarceration to the community—the reentry period—as research has shown that the first few months post-incarceration is when the formerly incarcerated are at the highest risk for recidivism (Grattet, Petersilia & Lin, 2008; Petersilia, 2003; Turner & Petersilia, 2011). With corrections playing an active role in this transition, including pre-release planning and post-release programming and continuity of services, research suggests that recidivism might be reduced (see, for instance, Petersilia, 2004; Seiter & Kadela, 2003). It is important to note that this effort is particularly difficult for jails, who most often hold inmates for only a short period of time (according to the Solomon, et al., 2008, 80 percent remain less than one month and of those sentenced to jail, the average length of stay is about 9 months).

Arguably, between community corrections' responsibility for upholding public safety as well as the increased need for its success due to the diversion of would-be prisoners from incarceration in prison or jail, effective community corrections could be considered the cornerstone of the criminal justice system. Success in this realm is

critical, as those on community corrections outnumber those who are incarcerated by more than double (The Pew Center on the States, 2009) and probation and parole, once reserved for low level offenders, is now expected to meet the needs of higher risk offenders. However, research suggests that probation and parole do not seem to have strong crime-reducing effects among the most serious offenders (Petersilia, 1997; Petersilia & Turner, 1993; Solomon, Kachnowski & Bhati, 2005).[4] Despite recent concerns about the effectiveness of community corrections, it is important to note that the majority of offenders do not reoffend while on probation (BJS, as cited in Taxman, 2002).

One theory as to why probation and parole are not more effective is the "dual responsibility" and seeming impossibility of delivering both rehabilitation and punishment successfully. In fact, a 1997 review of probation impact shows that probation can work, and that its success is partially contingent upon providing treatment for probationers as well as supervision (Sherman et al., 1997). In a subgroup analysis that did not take advantage of their randomized design, Petersillia and Turner (1993) found that intensive probation was more effective when probationers received some form of counseling, like employment counseling. Other research has also demonstrated that intensive probation alone is not effective in reducing recidivism, but that it is more effective when combined with rehabilitative efforts (Byrne & Kelly, 1989; Paparozzi & Gendreau, 2005; Pearson & Harper, 1990;). Despite the myriad of studies on intensive probation, not much attention has been paid to the quality, nature, duration, or frequency of supervision (Taxman, 2002). At the time of their inquiry into probation and parole practice,

[4] In the last year, states nationwide began initiatives to increase their efforts for stricter parole enforcement. For instance, Illinois began requiring that parolees follow stringent new regulations, including verifying whereabouts on a daily basis, visiting a parole officer twice a week, and refraining from drinking liquor or having alcohol at their homes. Some parolees are being sent back to prison for testing positive for drug use or simply smelling of alcohol, transgressions that rarely landed individuals back behind bars prior to these reforms (O'Connor, 2010). It is still unknown, however, to what extent these changes will lead to reduced rates of violence.

Paparozzi and Gendreau (2005) could find only one study of the nature of supervision, despite the common-sense notion that a punishment versus caseworker approach to supervision may result in different probationer outcomes.

While research has demonstrated that, as a whole, intensive probation programs do not reduce recidivism (in fact, they may increase incarceration because increased supervision means the increased detection of probation violations), there has been little research attempting to determine how much supervision *is* ideal under what circumstances (for instance, criminal history, age, geography, employment status or risk level). One recent departure is a study by Barnes et al. (2010) which demonstrated that outcomes for *low risk* probationers were not impacted negatively by lower levels of probation contacts. One criticism of intensive probation is that it all too often focuses on low-risk offenders and not those who might benefit from it most. Investigating this claim, Paparozzi and Gendreau (2005) found that in a high risk group of parolees, intensive supervision did reduce recidivism.

Because of this research, and the fact that more high risk offenders are being supervised in the community, experts have called for research into and implementation of best practices in probation and parole to reduce recidivism among serious offenders. For instance, Andrews, Bonta and Hoge (1990) suggested best practice principles for the treatment/rehabilitation aspect of the probation effort. They posited that the effectiveness of community corrections could be maximized though an explicit focus on risk, need and responsivity in treatment efforts.

The Risk Principle states that the level of service should be matched to the risk level of the offender with higher risk offenders receiving more treatment. The Need Principle states that the targets for intervention should be factors related to offending (often referred to as criminogenic needs) and the Responsivity Principle states that

interventions should be delivered in a manner that is appropriate to the learning styles of offenders (Bonta, Rugge, Scott, Burgoyne & Yessine, 2008, p.252). According to Andrews and Bonta (2006) implementing these three principles simultaneously can cut recidivism in half.

Criminal justice hybrids. In the absence of strict criteria for evidence based practice in probation—both supervision and treatment goals, as well as their intersection---several programmatic hybrids, where juvenile and/or criminal justice entities collaborate with non-justice programs and organizations, have been developed and implemented. The most widespread are programs based on the Cease Fire model; Los Angeles and Boston (both through Operation Ceasefire), Indianapolis (through the Indianapolis Violence Reduction Partnership) and Boston (through Safe Streets) have implemented interagency initiatives involving criminal justice and other officials, as well as community leaders and representatives from faith, educational and employment organizations. These collaboratives have provided both supervision of serious offenders as well as conveyance of the basic message that continuing violence will not be tolerated and that supports, such as employment assistance and educational programs, are available in their communities to help them move out of a life of violence and crime.[5] And many, but not all, evaluations of these initiatives have yielded some promising findings. For instance, the implementation of Operation Ceasefire was associated with significant reductions in youth homicide and gun assaults in Boston (Braga, Kennedy, Waring, & Piehl, 2001) and Skogan and colleagues (2008) found that Chicago's Ceasefire was associated with reductions in shootings in targeted neighborhoods. Yet the weak research design of the study makes it difficult to determine the true impact of

[5] The implementation of these initiatives has differed depending on the goals and characteristics of each city. For details on how the cities implemented their community-wide initiatives, see D.M. Kennedy et al.'s "Reducing Gun Violence: The Boston Gun Project's Operation Ceasefire" (2001), A.R. Gonzales et al.'s "Reducing Gun Violence: Operation Ceasefire in Los Angeles" (2005), Webster et al.'s "Interim Evaluation of Baltimore's *Safe Streets* Program" (2009) and S. Chermak et al. "Reducing Violent Crime and Firearms Violence: The Indianapolis Lever-Pulling Experiment" (2004).

Ceasefire efforts on crime reduction, particularly amidst significant broader reductions in crime throughout Chicago since 1992. Only one of the evaluations, of The Indianapolis Violence Reduction Partnership (Chermak & McGarrell, 2004), examined the individual-level outcomes of the participants. This study found the program had an impact on the attitudes and beliefs of participants but did not result in reductions in recidivism as compared to a matched comparison group of probationers. Finally, a recent study of a Pittsburgh violence reduction program, One Voice One Life, fashioned after Boston and others, found that the program did *not* reduce (and in some cases was associated with increases) in violence in the implementing neighborhoods (Wilson, McGarrell & Chermak, 2010).

Summary

The review of the literature presented here demonstrates that there are approaches, models and programs---both prevention and intervention-based—that work to reduce crime. There are several prevention programs that appear to keep youth from becoming engaged in the criminal or juvenile justice systems or from engaging in behaviors that will increase their risk of offending. There are many fewer programs that have been shown to be effective in the intervention social welfare space (preventing future offending/recidivism) for serious and persistent offenders, such as cognitive behavioral therapy, some education programs offered to incarcerated individuals, and vocational training.

On the intervention side, recent years have seen many criminal justice reforms. Perhaps the most successful have been in the realm of policing, where problem-oriented and hot-spots policing have proven reductions in crime in urban communities. Less is known about how to best structure community corrections. Attempts at intensive probation for high risk offenders have not reduced recidivism, and best practices in

supervision style, duration and frequency are scarce. Courts have also implemented innovations, most notably, problem-oriented court models. These models utilize community supervision partnered with rehabilitative social welfare programming, like drug therapy and vocational training, aim to address the criminogenic needs of offenders while simultaneously focusing on supervision and public safety. Yet the benefits of these programs seem not to outweigh their costs, and they have not been implemented with young people living in the community at high risk for continued engagement in serious crime or violence.

In the next section I describe my proposed study, which aims to add to the literature about best practices in community supervision for probationers who have extensive criminal histories and live in high crime communities. I will evaluate the effectiveness of a probation program that attempts to marry social welfare-oriented programming with intensive probation supervision for young people who are serious and persistent offenders, and attempt to understand how contact with line staff, including probation officers, is related to outcomes for these high risk youth.

CHAPTER 3: THE CURRENT STUDY

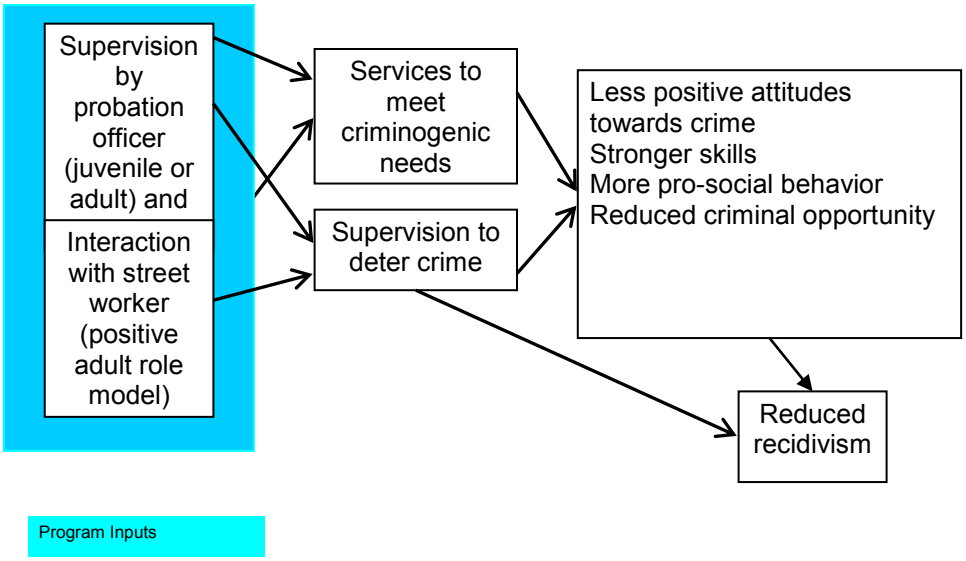
The current study seeks to add to the current body of literature about how probation can prevent future offending among serious adolescent and young adult offenders. Yet, as noted previously, probation often struggles to successfully deliver critical therapies/rehabilitation while simultaneously providing required or needed punishment/supervision. Philadelphia's Youth Violence Reduction Partnership (YVRP) is an *explicit* programmatic attempt to do both—reduce ongoing involvement in serious crime among persistent youthful offenders by addressing the root causes of crime (rehabilitation) while simultaneously reducing opportunity and maximizing deterrence (supervision) through intensive probation. YVRP achieves this by steering participants---young serious and persistent offenders living in high crime areas of Philadelphia who are on probation and between the ages of 14 and 24—away from crime through careful and constant supervision and providing them with the necessary supports to change their criminal trajectories and set them on the path to productive adulthood.[6] The model is implemented by teams of probation officers and community-based street workers (who are not tied to or funded by the criminal/juvenile justice system), and police. Street workers outreach to participants, aim to develop mentoring relationships with them, and strive to connect them with critically needed social supports ranging from mental health counseling to jobs. The bulk of in-person contacts occur in the participant's home, school or neighborhood. YVRP probation officers utilize an intensive probation model; they have much smaller caseloads than typical probation officers, allowing them time to more closely supervise participants, and they aim to meet with them in their homes and

[6] While YVRP is a community based program that was modeled loosely on Operation Ceasefire in Boston, YVRP is unique in that its strategy focuses narrowly on targeted high risk probationers providing coordinated supervision and support. Unlike YVRP, Boston, and other community based criminal justice hybrid programs like the Indianapolis Violence Reduction Partnership or Chicago Ceasefire, do not target probationers alone, and when probationers are identified as high risk, the program provides two interventions: 1) sending the message to them that violence will not be tolerated and 2) increasing supervision. But there is no explicit programmatic focus on connections to rehabilitative supports.

neighborhoods—even during evening and weekend hours—rather than reserving meetings for the probation office, the approach employed in traditional probation. Police conduct targeted patrols with probation officers to the homes and hangouts of participants, aiming to not only keep tabs on the YVRP probationers' activity and whereabouts, but to bolster their relationships with participants, their families and community members. These paired targeted patrols also permit information sharing between probation and police that before YVRP did not occur routinely. Most importantly, YVRP probation officers work collaboratively with street workers. Probation officers and street workers share responsibility for a caseload of probationers, informally discuss cases weekly to share information, create joint treatment plans, and are jointly held accountable for keeping the probationer on track. Although YVRP does not have an explicit logic model[7], nor was it developed based on criminological theory, in Figure 1, I present a possible model by which the program operates and achieves its outcome (at the participant level).

[7] A logic model is a depiction of how the programmatic model works: the context in which it operates, and its inputs, outputs and outcomes.

Figure 1: Possible model by which YVRP results in reduced involvement in violence and serious crime



This study aims to answer two main research questions of interest to the field:

• **Does participating in a program that attempts to explicitly coordinate both supervision/punishment and rehabilitation/treatment result in reduced recidivism for serious and persistent youthful offenders on probation?** Using multiple techniques, this study estimates the impact of YVRP on participants' arrests and convictions, arrests and corrections for violent crime, and time to recidivism over the 18 months post study enrollment. Logistic regression techniques are also be utilized and outcomes between the two approaches compared. Finally, the time to recidivism is also explored.

How is the Frequency of Contact with Street Workers Related to Recidivism Outcomes?

Using data from both YVRP participants and non-participants, this study, using a dose-response logistic model, investigates if there is an added effect of street worker contact on reducing recidivism (above and beyond the influence of probation contacts).

The sections that follow provide justification for the importance and relevance of these questions.

The promise and challenge of coordinating supervision and treatment. As probation departments became responsible for increasingly serious offenders, many probation departments, including Philadelphia, have invested in reduced caseloads and intensive probation efforts as a way to increase public safety. Previous research suggests that these efforts might not be successful if not paired with approaches that meet the criminogenic needs of probationers (see discussion of intensive supervision above).

In Philadelphia's probation departments the necessity to address criminogenic needs is not a new revelation. In fact, both Juvenile and Adult Probation departments have long-term efforts in place to attempt to provide probationers with referrals to possible supports. Yet often referrals are not enough; young offenders often face multiple barriers that can keep them from following up with a referral or sticking with it. For instance, a probationer may lack transportation to a job training program or may desire to attend school, but not be able to keep up with it when other demands, like parenting and paying bills, place pressure on him.

YVRP attempts to fill this gap by assigning participants to a YVRP street worker who aims to develop mentoring relationships with the participants and connect them with critically needed social supports ranging from mental health counseling to jobs, while

simultaneously assigning their cases to YVRP probation officers who have reduced caseloads and therefore, meet with youth frequently in their homes and neighborhoods even during evening and weekend hours. Some of these contacts occur with police for greater supervision impact; police-probation officer teams conduct targeted patrols to the homes and hangouts of participants.

Previous research on YVRP has shown that the implementation of the program in a particular area is associated with a decrease in youth homicide rates in that area, as well as a post-YVRP divergence in homicide trends from the city as a whole in the direction that suggests that the implementation of YVRP is associated with reductions in homicides in neighborhoods in which it operates. Specifically, McClanahan (2004) found that the average number of homicides per quarter (3-month intervals during the calendar year) significantly declined among young people aged 7 to 24 years in the first two police districts where YVRP was implemented. In addition, the rate of homicide reduction was greater in the YVRP districts than the city as a whole (e.g., the rate of homicides in the YVRP districts either declined or increased at a significantly slower rate after the start of YVRP compared to the rate of the city overall). A more recent analysis suggests that these findings did not persist when the program was expanded into new police districts; declines in homicide rates among youth relative to the city as a whole (minus implementing districts) were only observed in the two police districts in which the program originated, the 25th and (somewhat less so) the 24th districts (Kauh, McClanahan & Manning, unpublished manuscript).[8]

Yet these results do not speak directly to the effectiveness of YVRP in providing coordinated social control and welfare programming to probationers who have exhibited serious and persistent offending; instead they suggest that YVRP *may* reduce homicides

[8] The dates that YVRP began implementation: District 24 in June 1999, District 25 in October 2000, District 12 in August 2002, District 19 in April 2006, District 22 in July 2006, District 39 in January 2009.

at the *community level*, possibly through the participants themselves, but possibly because of the deterrent effect of increased police, probation and street worker presence on other would-be wrong-doers in the neighborhoods. Alternatively, the time series analysis may have been impacted by the implementation of other programs, or other social trends, and these may be responsible for the reduction in homicides, not YVRP.[9] Most other studies of criminal justice hybrids have not measured the impact of the program on individual participants (the exception is Chermak's evaluation of the Indianapolis Youth Violence Reduction partnership). The proposed study will attempt to add to the literature by determining if the dual approach, criminal justice hybrid, YVRP, is effective in reducing recidivism among serious and persistent adolescent and young adult offenders.

Dually focused probation: The interplay with probation type. As noted above, research has demonstrated that intensive probation is not effective unless paired with rehabilitative efforts (Pearson & Harper, 1990; 1989; Byrne& Kelly, 1989; Paparozzi & Gendreau, 2005). Yet, no research exists that has looked to see within this context if adding an additional rehabilitative support, in the form of street workers, or paraprofessionals, influence outcomes among juvenile and adult probationers. Since YVRP serves probationers on both adult and juvenile probation, this proposed research provides the opportunity to explore the answers to this question.

As compared to the adult criminal justice system, the juvenile system was created to meet the unique needs of juveniles. While experts agree that the juvenile justice system needs to provide both supervision of offenders and treatment, there exists differing opinions about which strategy is primary. According to Steinberg (2009), the juvenile system places rehabilitation first. Its goal is "to recognize the special needs and

[9] The author is not aware of any other programs that would have been responsible for a drop that would have not had a similar effect at the city level.

immature status of young people and to therefore emphasize rehabilitation over punishment." (Steinberg, 2009, p.460). Yet, in recent years the juvenile justice system has grown more punitive: "direct filing" juveniles into the adult criminal justice system for the most serious crimes, loosening juvenile confidentiality laws, and modifying sentencing regulations.

No matter where the emphasis lies, juvenile probation departments have an explicit charge to meet both the rehabilitative needs and supervision requirements of young probationers. And given that this is a departure from goals of the adult system, it is possible that contacts with street workers (in the context of intensive probation) may have different effects on youth in the juvenile justice system versus young people in the adult criminal justice system. Why? First, intensive supervision dictates smaller caseloads for probation officers. While research has shown that small probation caseloads are not effective, experts posit that the reason may lie in probation officer training. Without explicit instructions and training, probation officers whose responsibility is supervision, may not know how to make the most of smaller caseloads (Taxman, 2002). Yet, juvenile probation officers should be better suited to meet this challenge than adult probation officers as they are already used to smaller caseloads, may have had training specifically related to offender rehabilitation, and typically have more treatment resources at their disposal. The support of a partner street worker may help guide intensive probation. Second, as juvenile probation officers already have an explicit charge to address the criminogenic needs of youth on their caseloads, they may have more positive attitudes towards partnering with outside organizations and individuals, like street workers and counselors, to provide rehabilitative services. Finally, although there is no recent research documenting the stylistic differences between adult and juvenile probation officers, it seems common-sense that juvenile probation officers

may, on average, adopt a more rehabilitative approach to their supervision than adult probation officers and this may have implications of the effectiveness of street workers in the YVRP model.

Contribution to the Field

Clearly, documenting the value of YVRP in reducing crime among targeted individuals is a key facet of establishing this program's relative success and has the potential, combined with an exploration of the influence of contact levels, to help improve supervision and rehabilitation of serious and persistent offenders on probation. This research strives to make three unique contributions to the field. First, this study will focus on the population most commonly involved in serious crime—serious and persistent youthful offenders who are involved in the criminal or juvenile justice systems from high crime urban neighborhoods. This population is not routinely the focus of programmatic attention, nor evaluation. And, while hundreds of evaluations of programs seeking to reduce criminal activity exist, the majority of these are prevention programs or intervention programs for at risk youth. Much less is known about how to best structure probation, our nation's most frequent intervention for convicted offenders, to maximize its effectiveness for youth already deeply engaged in the juvenile or criminal justice system.

Second, this study utilizes rigorous methods to determine if a probation-based approach that aims to both rehabilitate and supervise probationers works. While criminal justice interventions, like probation, are increasingly dominated by a focus on supervision, few incorporate a simultaneous, coordinated, explicit, non-criminal justice effort to rehabilitate offenders. For instance, programs like Multi-Systemic Therapy attempts to rehabilitate adolescents who are serious and persistent offenders, but do not work closely with probation or police. Similarly, probation and parole attempt to control

behavior with supervision and sanctions, but do not often explicitly support the rehabilitation of offenders, especially offenders on adult probation. Finally, the few evaluated programs that are criminal justice hybrids, such as the Boston Gun Project's Operation Cease Fire (Kennedy, Braga, Waring, & Piehl, 2001), only measure community, and not individual level, effects.

Finally, while there exist many experimental and quasi-experimental evaluations of general crime reduction programs, there is much less in the literature about the conditions under which programs work best. This shortcoming is also true of criminal justice approaches. This study will seek to address this limitation by exploring if higher numbers of contacts with street workers is related to differential outcomes for youth on adult versus juvenile probation. It will explore the impact of contacts with street workers in each probation setting. Little research (in any) has been done to explore the differential impacts of dually focused probation efforts with juvenile versus adult probationers. Since, juvenile justice systems were established under the assumption, broadly, that youth, because of their status as children—not fully developed and responsible adults—cannot be held fully culpable for their crime (as opposed to adults), the goals of juvenile probation have historically been more explicitly focused on both social welfare and social control than adult probation. As a program that spans both juvenile and adult probation populations, this study presents the opportunity to explore how contacts with YVRP line staff influence participant outcomes in the context of probation department and philosophy. The results may have important implications for how and when an intensive probation approach is most appropriate.

CHAPTER 4: METHODOLOGY AND OVERVIEW OF CAUSALITY

Sample[10]

Study participants include 728 probationers ages 12[11] to 25 living in Philadelphia: 364 young people on adult probation[12] and 364 on juvenile probation. Juvenile and adult group distinctions refer to probation department, not to actual age of the study participant. In Philadelphia, almost all juveniles (under the age of 18) are adjudicated as juveniles and enter the juvenile justice system (those arrested for the most serious crimes can be, at the discretion of the district attorney's office, be prosecuted within the adult criminal justice system). Once on juvenile probation, these minors are not sentenced to a particular duration of supervision; instead, the probation officer determines, in conjunction with the family court, the point at which the young person is ready for release, and may stay in the juvenile justice system until their 21st birthday. However, if a young person is re-arrested once s/he turns 18, they are immediately discharged from the juvenile justice system and enter the adult criminal justice system. In this sample, thirty-one percent of juvenile probationers were between the ages of 18 and 21 indicating that they had not been rearrested as an adult. Of the adult probationers, 167 (46%) belong to the comparison group and 197 (54%) to the treatment group. Among juveniles, 212 (58%) belong to the comparison group and 152 (42%) to the treatment group. The treatment group consists of juvenile and adult probationers who were participating for the first time in YVRP from December 2003 through

[10] This study is secondary data analysis on a deidentified dataset provided to the investigator. As such, limited information is available about the selection of the comparison group.
[11] The program is designed for probationers between 14 and 24; however, 4 individuals under 14 were admitted to the program (and were, therefore, treatment group members) and 10 individuals under 14 were entered in the comparison group. 10 comparison group members were admitted to the study who were over the age of 25 but all were under 25.5 years old and therefore they remained in the analysis.
[12] There were, initially, 369 adult probationers in the dataset. One member of the adult probation sample was removed from analysis because he lived outside of the City of Philadelphia, and as a result, his "YVRP experience" was atypical. Four others were missing information about where they lived, which was likely an indication that they also lived outside the city, and were also excluded from analyses.

November 2006 who had not had any direct previous exposure to YVRP (e.g., because a family member or other person in their household had participated).[13] The comparison group included 379 non-YVRP probationers drawn from high-crime police districts where YVRP was not operational at the time of study intake.[14]

Comparison group members were identified through a combination of probation officer nominations and independent review of court records. As the selection process for YVRP at the time of the study was subjective (nomination and review of case by staff person at a partner agency, including police, probation or the streetworkers, it was challenging to identify comparison group members. Therefore, the comparison group member selection was also somewhat subjective. At the start of the study, the research team attempted to work with probation officers supervising probationers in covering non-YVRP police districts to collect their nominations for YVRP-like probationers. Probation officers were asked to complete a brief survey on each of their probationers who lived in non-YVRP police districts with high rates of crime (e.g., the 16th Police District). The survey included questions about the age, gender, criminal history, family beackground, educational background, employment status and probation officers rating of the likelihood that the probationer would kill or be killed. This nomination approach was abandoned as probation officers were non-compliant. The remainder of the comparison group was identified by the research team who sent part time staff to the courts to scour through and review probation and court records. These reviews sought to identify young probationers living in non-YVRP high crime police districts who had criminal profiles similar to those of YVRP participants. Specifically, the reviewers sought probationers that had multiple arrests and/or conviction with at least one for a serious or violent crime.

[13] Despite the fact that these participants were "new to" YVRP, the co-offending literature suggests that they may still have been impacted indirectly by YVRP due to associations with others on YVRP or though possible neighborhood effects of YVRP.

[14] Police districts that were identified for comparison group selection include Districts 1, 2, 14-18, 23, 26, 35, and 92.

The database provided for this study does not identify the source (probation officer nomination or research team identified) of each comparison group member. Excluded from the comparison group are any probationers who had previously been involved in YVRP, had been exposed to YVRP (through a family member or other person in the household), or were living in YVRP police districts but who were not involved in YVRP, because of the possibility of their exposure to YVRP as a result of targeted police-probation patrols, probation home visits, and street worker home visits.

Geography. As noted above, comparison group study participants were selected by record review and nomination from high crime, high poverty police districts in which YVRP was not operational.[15] Since police districts implementing YVRP were selected because they had the highest numbers of serious, particularly violent, crimes, it is not surprising that residents of YVRP police districts were, on average, experiencing significantly greater socioeconomic disadvantage than were residents of non-YVRP police districts. Table 2 shows that although we aimed to limit the comparison group sample selection to police districts most comparable to those of the YVRP districts, the residents of YVRP districts, on average, still experienced greater neighborhood disadvantage than do those in comparison group districts. Table 1 shows that in the "comparison districts" the average median household income was almost $4000 higher, the poverty rate was 7.2% lower, and the rate of adults with a Bachelor of Arts (BA) was

[15] Ultimately YVRP became operational in some of the districts in which the comparison group was drawn. For juveniles, 1 from the 19th district and 13 from the 29th district and for adults 18 from the 39th. However, the 39th district implementation does not overlap with the study timeline, so no contamination would occur for those individuals.

2.4% higher than in YVRP districts. Therefore, the analysis will control for geographic differences.[16]

Table 1: Average Socioeconomic Indicators for YVRP Police Districts versus Police Districts where Comparison Group Members Live, 2000

	Median Household Income ($)	Residents Living in Poverty (%)	Rate of Adults (25 yrs or older) with BA Degree
Average of YVRP Districts	22046.07	32.43	5.75
Average of Districts from Which Comparison Group was Drawn	26043.06	25.21	8.13

Source: U.S. Census Bureau. 2000 Population Survey. Generated using the Philadelphia Neighborhood Information System, Cartographic Modeling Lab, University of Pennsylvania. <http://www.cml.upenn.edu/nis> on December 29, 2009.
Note: (a) 2000 data are used since it was the Census immediately preceding the data collection period.

Baseline Differences Between Treatment and Comparison Group Members

Table 2 summarizes baseline demographic characteristics and criminal histories of the study participants by study group (treatment versus comparison) and probation department.

Table 2: Baseline Differences between Treatment and Comparison Group on Measured Potential Confounders

	Probation Type					
	Juvenile Probation Treatment Status			Adult Probation Treatment Status		
	Comparison	Treatment		Comparison	Treatment	
Male	.811 (.392)	.934 (.249)	***	.952 (.214)	.926 (.263)	
Hispanic	.033 (.179)	.125 (.332)	***	.120 (.326)	.158 (.366)	
Black	.925 (.265)	.823 (.378)	**	.850 (.358)	.812 (.392)	
Age	17.108 (1.69)	17.212 (1.42)		22.680[j] (1.650)	21.431 (1.652)	***
Number of prior arrests	3.414 (2.01)	3.110 (2.05)	[t]	1.868 (1.067)	1.649 (1.046)	*
Previously shot	.034[a] (.183)	.080[b] (.272)	*	.132[k] (.340)	.141[l] (.349)	
Most serious prior: violent crime	.944[c] (.230)	.795[d] (.405)	***	.558[m] (.498)	.440[n] (.498)	*
Age at first arrest	13.8[e] (1.72)	14.1 (1.78)		19.239[o] (1.276)	18.932[p] (1.282)	*
Drug use	.533[f] (.500)	.722[g] (.450)	***	NA	NA	
Designated as mentally retarded	.107[h] (.310)	.137[i] (.345)		NA	NA	
Lives in West-NW probation district	.486 (.501)	.730 (.445)	***	.461 (.500)	.658 (.475)	***
Lives in South Central probation district	.321 (.468)	.020 (.397)	***	.299 (.459)	.119 (.324)	***
Missing data dummy	.137 (.344)	.100 (.299)		.180 (.385)	.153 (.361)	
Marijuana use	NA	NA		.434 (.358)	.554 (.496)	**
Other drug use	NA	NA		.229 (.420)	.189 (.383)	
Most serious prior: drug crime	NA	NA		.337[q] (.474)	.387[r] (.488)	
Have child	NA	NA		.217[s] (.414)	.341[t] (.475)	**
Years previously incarcerated	NA	NA		1.180 (1.516)	.717 (1.206)	***

[16] Because police district perfectly defines the treatment and comparison designation, I use probation district as a proxy for geographic location in the descriptions and analyses that follow.

Table 2: Baseline Differences between Treatment and Comparison Group on Measured Potential Confounders

	Probation Type				
	Juvenile Probation Treatment Status		Adult Probation Treatment Status		
	Comparison	Treatment	Comparison	Treatment	
Years previously on probation	NA	NA	2.155 (2.226)	1.010 (1.218)	***
In drug/alcohol treatment	NA	NA	.114 (.318)	.089 (.286)	
Number of Observations	*152*	*212*	*167*	*197*	

Notes: (a) NA indicates that data were not used or available within the specific subgroup.
(b) Data for adult probationers are based only on adult court records. As such, it is possible that adult study participants have criminal histories from juvenile courts for which our study does not account.
(c) Arrest charges are listed in order of decreasing severity based on Uniform Crime Reports (UCR) criteria.
(d) Juvenile and adult group distinctions refer to probation department, not to actual age of the study participant. Thirty-one percent of juvenile probationers, for instance, were between the ages of 18 and 21.
(e) Standard deviations reported in parentheses.
(f) [a]9 missing cases, [b]2 missing cases, [c]15 missing cases, [d]1 missing case, [e]2 missing cases, [f]4 missing cases, [g]1 missing case, [h]7 missing cases, [i]13 missing cases, [j]1 missing case, [k]8 missing cases, [l]4 missing cases, [m]4 missing cases, [n]11 missing cases, [o]4 missing cases, [p]10 missing cases, [q]4 missing cases, [r]11 missing cases, [s]15 missing cases, [t]17 missing cases
(e) [t]p<.10, *p<.05, **p<.01, ***p<.001

The profile of juvenile probationers in the sample. As shown in Table 2, among juvenile probationers, treatment (JT) and comparison group members (JC) had different racial and gender profiles. Specifically, treatment group members were more likely to be male (81.1%% JC versus 83.4% JT) and Hispanic (3.3% JC versus 12.5% JT), and less likely to be African American (92.5% JC versus 82.3% JT) than their counterparts in the comparison group. This racial/ethnic disparity can be attributed to the fact that YVRP is operational in the largest enclave of Hispanic Philadelphia residents (the 24th and 25th police districts). Treatment group members were also more likely to have been shot before entry into the program (3.4% JC versus 8.0% JT). However, treatment group members were less likely than comparison group members to have perpetrated a violent crime as their most serious crime at the time of study intake (87.7% JC versus 78.9% JT). Treatment group members were more likely to be using illegal drugs at the time of their intake into the program than comparison group members (53.3% JC versus 72.2% JT). Finally, due to the geographic restrictions associated with YVRP, comparison and treatment group members necessarily came from different parts of Philadelphia. 48.6 percent of comparison group members lived in the West North West probation district of

Philadelphia as compared to 73.0 percent of treatment group members, and 32.1 percent of comparison group members lived within the boundaries of the South Central probation district as compared to 2.0 percent of treatment group members. The remainder lived in the East North East probation district. Together, these differences do not suggest a clear pattern of higher risk among either group. But the fact that the two groups are not equivalent on key characteristics indicates that careful matching (or controls) must be employed in order to compare their recidivism outcomes.

The profile of adult probationers in the sample. Adult probationers in the comparison and treatment groups were similar in terms of their gender and race/ethnicity. However, adult treatment group members (AT) had a younger age at first arrest in the adult system than those in the comparison group (AC) (19.2 AC versus 18.9 AT) yet they experienced significantly fewer years on adult probation (2.2 AC versus 1.0 AT) and incarcerated as an adult (1.2 AC versus .7 AT) than those in the comparison group (AC). Comparison group members also experienced, on average, more arrests (as an adult) before entering the study (1.9 AC versus 1.6 AT) and were more likely to have their most serious crime as an adult to have been a violent crime than treatment group study participants (55.8% AC versus 44.0% AT). Treatment group study participants were more likely to have children (21.7% AC versus 34.1% AT) and adult probationers in the treatment group were younger, on average, than those in the comparison group (22.7 AC versus 21.4 AT). Treatments on adult probation were more likely to be using marijuana at the time of study entry than the comparison group (43.4% AC versus 55.4% AT) but no more likely to be using other illegal drugs. Finally, similar to juvenile probation, the adult probationers in the treatment and comparison groups lived in different sections of Philadelphia. Among comparison group members, 46.1 percent

lived in the West North West probation district and 29.9 percent lived in the South Central district; whereas, 65.8 percent and 11.9 percent of adult treatment group members lived in the West North West and South Central probation districts, respectively. The remainder of both groups lived in the East North East probation district. As opposed to the juvenile probationers, adults in the treatment group appear, based on observable factors, to have lower risk on average for recidivism than the comparison group.

Summary. Since a disproportionate percentage of serious crime and street violence among youth is committed by a small percentage of young offenders (see, for instance, Howell, Kristberg & Jones, 1995), serious crime-prevention efforts might be best focused on this population. This is the group that YVRP leaders target and is also the group that was targeted for the comparison group. The youth and young people in the study had criminal records characterized by relatively high levels of criminal activity for relatively serious crimes. They were almost exclusively male (a risk factor for engaging in serious crime and violence) and were, by definition, young people. All came from neighborhoods that had economic challenges, and all youth were on probation. On average, all had more than one previous arrest: the juvenile probationers had several and the adults, on average, had more than one in the adult system even though their average age was about 22 years old. Despite our best attempts at finding comparable young people for the comparison group, the comparison and treatment groups are *not* comparable on many background characteristics and they did not come from identically disadvantaged neighborhoods. In order to assess the impact of the program and contacts with probation officers, the proposed study will aim to make the comparison and

treatment groups as comparable as possible based on the measured covariates available in the data.

Data Collection

This study utilizes data from two sources: 1) probation and court records, and 2) program data. For YVRP participants, enrollment in the study coincided with enrollment in YVRP, which could occur at any time during their probation sentence. Comparison group members were enrolled in the study at any point during their current probation sentence. In the section that follows, I describe the sources of the data (and some of the limitations), the process for collecting the data, and a description of each measure.

Court and probation-records data. In Philadelphia, entry into the adult system is predicated by any arrest at age 18 or older. The juvenile system accommodates any youth under the age of 18 at the time of their arrest and adjudication, but can maintain supervision of a young person through age 21.[17] Juvenile and adult court systems are independent; they each collect and track different information about probationers. Furthermore, the two are separate and do not "speak" to one another. Finally, many, but not all, data extractions require manual coding. As such, data available for each group differs, except for recidivism, used as the outcome in this data (follow up) which is the same for both groups.

Broadly, information, including probationers' criminal histories (e.g., arrests, convictions, probation and incarceration history) was collected from their court records. Data on adult probationers is limited to adult criminal histories; juvenile probationers have data on both adult and juvenile criminal histories. This means that for adults, the number of arrests at intake, as well as years on probation and spent incarcerated at

[17] Discharge from the juvenile system is at the probation officer's discretion, or upon arrest as an adult.

intake, represents the number of arrests and time served as an *adult* (18 and older) and does not include any data from time they may have spent in the juvenile system (adjudications under age 18). On the other hand, recidivism outcomes data from court records do include adult arrests and convictions for those individuals who were on juvenile probation at the start of the study. Specifically, household structure (e.g., whether or not the probationer has children), drug/alcohol treatment, previous number of days incarcerated (in the adult system), marijuana and other illegal drug use, most serious prior arrest was for a drug crime, and number of days on adult probation was available for adult probationers only. Finally, only juvenile probationers had data available indicating whether they had a diagnosis of mental retardation and whether they were using drugs at the start of the study.

Where possible, these data were provided for each study participant through a data extraction from juvenile and adult probation departments, in which each agency electronically downloaded data from their computer system into data files specifically configured for this study. When this was not feasible, data were hand-coded by researchers who reviewed each record individually. Electronic data extractions from probation were not feasible in the following situation. Adult probation did not begin to document supervision-related data (e.g., demographics, contacts with probation officers, employment, educational and family background) electronically until May 2006. Prior to this, supervision-related data were documented in paper files known as “streetbooks.” Therefore, for a subset of adult probationers, those for whom the 18-month study period did not overlap with the electronic probation monitoring system, data were only available through written notes in probation officers’ streetbooks which were coded on-site by researchers. Juvenile data were not available electronically; therefore, all juvenile data were coded by researchers.

While this multi-source strategy resulted in more complete data, there are also obvious drawbacks. Most notably, there may be differences in the completeness and accuracy of the data by source. For instance, while both street books and computerized data entries are reviewed by probation supervisors, it seems possible that the review is more thorough and completed more regularly using the computerized system. Furthermore, the computerized system provides options when capturing data. In other words, instead of having the freedom to record "German" as a race, the computerized system provides pre-set fields, which may result in different codes or information than would have been recorded in streetbooks. Unfortunately, information on the source of the data was not captured in the dataset, so it is not possible to explore if there are systematic differences based on data source. Table 3 displays information about missing data. Finally, administrative data, while valuable, is limited in terms of its robustness---an ideal design would provide more information on probationers' backgrounds, and interactions with the justice system.

Finally, it is worth noting that the quality of the data in criminal records is varied. Data that are generated by the system itself, such as years incarcerated, years on probation, number of prior arrests, age at first arrest and recidivism data are of high quality, meaning that they are accurate and input on a timely basis. However, other records data may be less reliable. For instance, race/ethnicity is recorded by the probation officer or courts and is not routinely based on information collected from the offender. Other fields that are recorded by the probation officer, such as drug use, parental status, and previously shot are all subject to similar collection protocols. Of similar questionable quality is probationer address, which is provided by the probationer but, because many probationers do not want to be found, may or may not represent the

location where the probationer actually lives most of the time (e.g., it may represent the address where the probationer receives mail).

Program data. As part of program practice, each month YVRP line staff--street workers and probation officers---are required to document contacts, outcomes and other relevant information on each YVRP participant to whom they are assigned. This information was collected on paper forms and submitted every month to the research team that lead the original evaluation. Using a list of YVRP participants maintained by the program staff, the evaluator generated pre-labeled forms with each participant's name, photo identification number, street worker, probation officer and month. Line staff was also required to complete a form for anyone who was newly added to the program that month; these forms were known as "unsolicited" and program staff was given multiple extra copies of the form for this purpose. The evaluator carefully screened the data for accuracy and completeness.

Overall, the program data were of high quality in terms of completeness (see Table 3 for a review of measures and missing data). Probation officers and street workers received numerous and ongoing data collection training by the evaluator. Furthermore, the data collection effort was not unique to this study. Probation officers and street workers have been required to submit this information monthly since 1999. And line staff supervisors are expected to review the quality and completeness of the data *before* it is submitted to the evaluator. Finally, each month the data is reviewed with leadership from the program and relevant agencies. When data are incomplete, each supervisor is asked why and expected to provide updated information, which was then incorporated into the dataset. For obvious reasons, including the collaborative nature of the program and accountability to superiors, the supervisors want and need the data to be accurate. In sum, YVRP has a data-driven culture which has resulted in

complete programmatic data. Nonetheless, it is possible that this data-driven culture has implications for the accuracy of the data. For instance, if line staff are feeling pressure to meet contact benchmarks, they may inflate the number of contacts to improve their performance vis a vis this benchmark. The YVRP evaluators took every step possible to avoid this possibility by both including reporting categories for attempted contacts, and coaching supervisors on how to make the best use of the data.

Measures

Information on the measurement of each of the variables is provided in Table 3 below.

Table 3: Summary of Measures

Baseline variables	**Coding**	**Source (CR=crim rec, PR=prog rec)**	**n (juv)**	**n (adult)**
Gender	0 (Female), 1 (Male)	CR	364	364
Race/ethnicity: Hispanic[a]	0 (no), 1 (yes)	CR	364	364
Race/ethnicity: Black[a]	0 (no), 1 (yes)	CR	364	364
Age	In years	CR	364	364
In drug/alcohol treatment	0 (no), 1 (yes)	CR	0	364
Years on probation[b]	In years	CR	0	364
Years incarcerated[b]	In years	CR	0	364
Number of prior arrests[b]	count	CR	364	364
Previously shot	0 (no), 1 (yes)	CR	353	357
Most serious crime[b]	Coded by offense	CR	348	354
In mental health treatment	0 (no), 1 (yes)	CR	0	364
Have biological child	0 (no), 1 (yes)	CR	0	337
Age at first arrest[b]	In years	CR	362	355
Use illegal drugs (any)	0 (no), 1 (yes)	CR	359	0
Use marijuana	0 (no), 1 (yes)	CR	0	364
Use other illegal drugs	0 (no), 1 (yes)	CR	0	364
Mentally retarded	0 (no), 1 (yes)	CR	344	0
Probation district West NW	0 (no), 1 (yes)	CR/shape file	364	364
Probation district South Central	0 (no), 1 (yes)	CR/shape file	364	364
Missing data dummy	0 (none), 1 (some)	Created	364	364

Table 3: Summary of Measures				
	Coding	**Source (CR=crim rec, PR=prog rec)**	**n (juv)**	**n (adult)**
In-program variables over 18 months				
Street worker contact (18 mos)	Count total	PR	364	364
Probation officer contact (18 mos)	Count monthly	PR	364	364
Follow up recidivism variables at 18 months				
Arrested for a new crime	0 (no), 1 (yes)	CR	364	364
Arrested for new violent crime	0 (no), 1 (yes)	CR	364	364
Convicted of new crime	0 (no), 1 (yes)	CR	364	364
Convicted of new violent crime	0 (no), 1 (yes)	CR	364	364
Notes: (a) Race was coded by probation department. Hispanic was coded as a "race" and not an ethnicity (i.e., it was a mutually exclusive category). (b) For adults includes only data from adult probation. For instance, age at first arrest is the age at which the individual was first arrested as an adult.				

For this study, crimes were coded and assigned a severity of offense based on the Federal Bureau of Investigation's Uniform Crime Reports (UCR). Criminal history data were classified under the UCR typology of Part 1 Offenses. All lesser Part II Offenses were lumped together into a single category except for drug-related offenses due to their association with violent crimes. As such, recidivism for violence includes homicide, rape, robbery and assault. However, it is important to note that weapons offenses are crimes that may result in entry into YVRP if program leaders believe the crime is an indication that the youth is on the path to violence. Nonetheless, weapons violations are not included in the violence recidivism measure because the nature of the data does not permit the assessment of the nature of the crime (e.g., having a knife in a car during a motor vehicle theft versus loitering on a corner with an illegal gun). Using this data, we identified the most serious crime they had been arrested for and how many times they had been arrested before study intake. Again, in both instances data were available for adult probationers only on crimes committed while they were adults. Ultimately, the following variables were used in analysis: violent crime (juveniles and adults) and drug crime (adults only).

Missing data. Missing data was handed in three ways utilizing three separate analytical strategies. In the first instance, cases with missing data were dropped using a listwise deletion protocol. The second missing data protocol used mean substitution to replace missing values on independent variables, and included a missing data dummy variable indicating, for each individual, whether s/he has no missing data on any variable (coded 0) or missing data on one or more variables (coded 1). The last approach was multiple imputation to generate values for missing values. Five data sets were created imputing the missing values and analysis was conducted on each of these data sets. The average value of the parameter estimates across the five datasets was used for the point estimate. The standard error receives an appropriate correction to reflect the fact that the estimate was generated over multiple datasets.

Other covariates. Ideally, a more robust set of confounders would have been utilized in the analysis of this data. Some key measures likely associated with both the propensity to receive treatment and recidivism over 18 months were not available, either because they are not collected by either the program or by criminal justice or because the data that were available was of highly questionable quality. For instance, employment status and educational history have both been shown to be associated with criminal offending. Having an immediate family member, such as a parent or sibling, who has been involved in the criminal justice system is also associated with criminal offending in children and siblings. These are all factors that could be easily collected by courts and probation. Similarly, research shows that persistent offenders (those that continue offending into adulthood) are first arrested at younger ages and for more serious crimes than those teens who desist from crime as the mature into adulthood. As such, data about the juvenile records of adult probationers (e.g., age at first arrest,

number of juvenile arrests) would have bolstered this study, as well as adult probation's efforts to identify the riskiest probationers.

Design

One of the goals of this study is to estimate the treatment effect of participating in YVRP on recidivism among serious and persistent youthful offenders. Establishing causality in social sciences, particularly when data are observational in nature, is complex and challenging and, therefore, the subject of much philosophical, scientific and statistical thought and exploration. So, in the section that follows, I provide a *brief* overview of causality and propensity scores, the approach I adopt herein, as a tool to achieve balance between comparison and treatment group participants.

Background on causal inference.[18] The goal of this study is to estimate a treatment effect; however, this study is observational--in other words, the exact mechanism for determining who received the treatment (YVRP treatment group) and who did not (comparison) is not known and not random. While there has been a long history of causal estimation in observational sciences (see, for instance, Berk, 2004), the potential outcomes framework is now prominent, and is the framework I will apply in this study.

The potential outcomes framework's main assumption is that every individual has a potential outcome under alternative treatments (Morgan & Winship, 2007). In other words, at the individual level, individual *i* has a potential outcome under the condition of receiving YVRP (y_i^1) and a potential outcome under the condition of not receiving YVRP (y_i^0). Of course, it is impossible to observe the same individual under

[18] Information in this and the next section draws heavily on class notes from lectures provided by Dr. Berk and Dr. Joffe in 2008 and 2009 (Berk, 2009a,b; Joffe, 2008 a, b, c, d). It also draws on Morgsn & Winship (2004), Imbens (2004); Rubin (1997); Hernan & Robins (n.d.); Braitman & Rosenbaum, 2002; and Holland (1986).

both conditions— participating in YVRP and not participating in YVRP---at the same time.[19]

Since it is impossible to observe what would happen to a single individual if they did and did not receive a treatment at any point in time, we must rely on an average treatment effect based on the observed outcomes (Y) of individuals with known exposure to either the treatment (D=1) or comparison condition (D=0). In any sample of individuals, we can observe ($y_i^0, d_i^0;$ for those where D=0) or (y_i^1, d_i^1; for those where D=1) and a set of covariates x_i. Comparing the average outcomes of the individuals who were treated (D=1) to those who did not receive treatment (D=0) provides an estimate of the association between an outcome and treatment if a researcher can meet certain assumptions, presented below (Hernan & Robbins, n.d.).

Ignorability. Unlike well-implemented random assignment designs, observational studies are characterized by the researchers' inability to control treatment status (Rosenbaum, 2002). Causal inference requires that treatment status is independent of all functions of the potential outcomes. That is, one must break the link between the characteristics of the individual and the receipt of treatment or the individual's outcomes. One option to achieve ignorability is by randomly assigning individuals to treatment or control. In this case, if randomness is preserved and the sample is large enough, assignment condition will be independent of potential outcomes. Similarly, even in an observational study, if a researcher knew the exact criteria upon which an individual was selected for (or self-selected for) the treatment versus the control or comparison group (and all relevant variables measured accurately), assignment would be ignorable

[19] The only possibility to observe the same individual as both a YVRP participant and a non-participant would be by letting them be involved in YVRP at one time and not at another time. In this situation, one *could* look to see if the individual was more likely to commit a murder when not enrolled in YVRP, but because other factors about the individual may have changed, such as age, and because the individual, although no longer involved in YVRP, could still be affected by previous involvement in YVRP, it would be difficult to disentangle the effect of those factors from the actual effect of the program (for instance, did the individual not engage in murder because he was older or because of his involvement in YVRP?) unless we knew the exact form of the relationship, which is unlikely.

conditional upon measured covariates. However, most researchers are not lucky enough to know the exact treatment selection criteria in an observational study. In many cases, the decision about whether to provide treatment is likely based on characteristics of the individual *and* on his potential outcome. For instance, a doctor's decision about whether or not to administer chemotherapy is likely to be based on how likely he thinks it is that the patient will die from the cancer. The doctor cannot know the true likelihood that the individual will die, and therefore might use characteristics of the individual to make assumptions about that likelihood, such as the severity of the cancer (e.g., how widespread it is) and the age of the patient. Both of these factors are related to the patient's potential outcome (e.g., if the individual has more advanced cancer, they are also more likely to die). Under these conditions, theoretical and experiential assumptions about the selection mechanism can be made and techniques can be used to achieve ignorability assuming all relevant confounders are measured accurately, the treatment selection mechanism is modeled correctly, and other assumptions (listed below) are met.

Positivity. Causal inference, by definition, requires a counterfactual, which necessitates that the chance of being assigned (or receiving) treatment must be greater than 0 but less than 1. This is known as positivity. Positivity is achieved through random assignment, because individuals are randomly selected to either get or not get the treatment. But in observational studies, positivity is not guaranteed, and must be present for valid causal inference.

Stable Unit Treatment Value Assumption (SUTVA). SUTVA is the assumption that the potential outcomes of individuals are unaffected by the treatment status of others. In other words, the effect of treatment should not change depending on the composition of the groups receiving and not receiving treatment. SUTVA is rigorous: it

requires no spillover effects of the treatment onto control group members, that the effect of the treatment on an individual is equivalent regardless of the number of individuals receiving treatment, and it requires a well-defined treatment (treatment applied according to a strict protocol—e.g., taking Aspirin versus taking 100 mg Aspirin at 8am and 8pm). Furthermore, unlike ignorable treatment assignment and positivity assumptions, which can be achieved with random assignment, random assignment does not assure SUTVA.

Propensity score balancing: One approach to estimating average treatment effects from observational data. This study compares the outcomes of YVRP participants with outcomes of comparison probationers who are similar but who could not participate in YVRP because they resided outside of the geographical program boundaries (e.g., in police districts not served by YVRP). As previously mentioned, the most rigorous method for assessing if a program *causes* impacts is a random assignment study. However, this study is observational; the researchers did not have control over the assignment of participants to the treatment group.

The researchers chose comparison group members as similar to YVRP participants as possible on factors related to eligibility criteria for the program and location of residence in city (in a high crime neighborhood). Even using these methods to choose the comparison group, as discussed in the "participants" section of this paper, the comparison and treatment groups are not comparable. One method for attempting to achieve ignorability is propensity score. A propensity score, if accurate, breaks the link between confounders and the treatment assignment. It is an estimate of the probability (likelihood) that an individual (treatment or comparison) would receive treatment given his/her characteristics and experiences. This score can be used to create comparability among groups or matches of treated and untreated individuals along a single numerical

dimension, at least to the extent that differences between the groups are captured in the data and controlled for by the researcher. The benefit of propensity scores over other techniques is that propensity scores summarize the influence of all confounding variables into one estimate (Rubin, 1997).

Propensity scores are typically created by using a probit or logit regression model where receiving treatment is the outcome and is predicted by (some relevant subset of available and theoretically meaningful) confounding covariates, resulting in a predicted probability of receiving treatment (propensity score). Assuming the estimated propensity score is the true propensity to receive treatment, it can be used in multiple ways to achieve balance between treated and untreated groups. For instance, a propensity score can be controlled for in a regression model which results in an estimation of the outcome conditional upon propensity score, treatment status and other covariates of interest.

An alternative approach is to match study participants in the treatment and comparison groups on propensity score. While many matching algorithms exist, one to one matching is common. Assuming the propensity score is an accurate estimate of the likelihood of receiving treatment given confounding covariates, the propensity score results in a mean for treated individuals about the same as the mean for untreated comparison group members on characteristics related to the treatment and their potential outcomes, and thus ignorability would apply.

Finally, perhaps the most intuitive approach is to assess differences in the outcome of interest within strata of propensity score. Rubin (1997) suggests that dividing subjects into five to six propensity score groups or strata should create groups where treatment or comparison status is independent of measured covariates. Within each strata, differences in the mean of the outcome can be calculated. This information,

along with size of strata, can then be used to construct an estimate of the average treatment effect.

While propensity score methods have been championed by several researchers and statisticians (Rubin, 1997; Rosenbaum & Rubin, 1983), there remain limitations to this approach. First is the problem of unmeasured covariates. The propensity score balances the treated and untreated groups on *observed* confounding covariates. If a perfect propensity score was generated and one met all assumptions above, using it in analysis would yield the same causal estimates as a perfectly implemented randomized control trial; however, because data on all relevant confounders is often unavailable to researchers, the assumption of balance, and thus ignorability is frequently unjustified. Second, propensity scores require modeling, which risks misspecification of the assignment to treatment mechanism, even in the case where propensity scores are backed with solid theory.

Analytical Strategy

The analytical strategy used to answer the research questions posed in this study is described in detail below. All analyses are conducted separately for adult and juvenile probationers because these two probation departments maintain different data, adopt different theories of change, and serve different populations. Further, each set of analyses is conducted using all three missing data handling methods described earlier in this paper: listwise deletion, mean substitution with a missing data dummy, and multiple imputation.

Research question 1: Does participating in YVRP lead to lower recidivism? In this study, recidivism is defined in four ways:

- Any arrest at all over the 18 month follow up period
- Any arrest for a violent crime over the 18 month follow up period

- Any conviction at all over the 18 month follow up period
- Any conviction for a violent crime over the 18 month follow up period

Recidivism does not include violations of probation that did not result in a new arrest or conviction. To estimate the treatment effect, three analytical approaches were used, traditional logistic regression, stratification by propensity score, and covariate adjustment using propensity score (the latter two are within the potential outcomes framework described above).

First, traditional logistic regression modelling (Allison, 2012 was used as a guide) is employed as an analytical strategy. The following model is used to estimate the relationship between program participation and recidivism (treatment effect):

$$\text{Logit } (P)=B_0+B_{TC*}\text{treatment}+B_1X_{1+}\ldots.B_nX_n,$$

where P is the probability of recidivism and $X_1\ldots.X_n$ are measured confounders of treatment and recidivism and treatment is membership in the YVRP or comparison group.

The second two approaches are both steeped in the potential outcomes framework and employ propensity scores as a tool. Propensity scores were generated as balancing scores using the data available from probation/court and program records. To calculate the propensity score, the user generated pscore protocol in STATA (Becker & Ichino, 2002) was used:

$$\text{Probit}(P) =B_0+ B_1X_1+B_2X_2\ldots..+B_nX_n$$

Where $X_1\ldots.X_n$ are measured confounders of treatment and recidivism.
The estimate of Y based on this model forms the propensity score.

These propensity scores are used in two ways. Using methods summarized by Rubin (1997), study participants with the closest propensity scores were subclassified into strata based on propensity score. Rubin asserts that with a large enough sample,

quintiles of propensity score balance treatment and comparison group on confounding measured covariates, approximating randomization within each strata. Because the sample size for this study is small, I used strata defined by STATA in the *pscore* command. This STATA protocol results in strata that are divided until t-tests of the treatment versus comparison propensity scores within each block were non-significant and there are no statistically significant differences on measured covariates within strata.

Within each strata the difference in means between treatment group members and comparison are calculated. Each mean difference receives a weight equal to the size of the strata in proportion to the size of the total sample, and a marginal average treatment effect on the treated is calculated. Those falling outside of the region of common support are excluded from analyses (STATA's common support option was employed, which excludes treatment cases with estimated propensity scores higher than the highest score among comparison group members, and those of comparison group members which fall below the lowest score among the treated).

Next, the propensity score is used as a summary measure in a logistic regression using the following model:

$$\text{Logit } (P)=Y_{recidivism}=B_0+B_{TC^*}\text{treatment}+B_{ps^*}\text{pscore}$$

Again, those falling outside of the region of common support are excluded from the analysis.

Finally, an alternative to estimating the treatment effect on the incidence of recidivism is to asses if YVRP *delays* recidivism. I used Cox regression (see Allison, 2014 for an explanation) to assess if the time to failure (in this case, recidivism) differs for YVRP participants versus those in the comparison group. The following two models were tested:

$$\text{Log } (h)=B_0+B_{TC}\text{treatment}+B_1X_{1+}\ldots.B_nX_n \text{ where h is the hazard}$$

$$\text{Log (h)} = B_0 + B_{TC}\text{treatment} + B_{ps}\text{pscore}$$

Where $X_1 \ldots X_n$ are measured confounders of treatment and recidivism and treatment is membership in the YVRP or comparison group and pscore is the propensity score.

Research question 2. How is the frequency of contact with street workers related to recidivism outcomes?

The following logistic regression models were run to estimate the association between probation and street worker contacts and recidivism (as defined above).

$$\text{Logit (P)} = B_0 + B_{pcont}*\text{probcontact} + B_{swcont}*\text{swcontact} + B_{ps*}\text{pscore}$$

Where pscore is a composite of $X_1 \ldots X_n$ (measured confounders) of treatment and recidivism and probcontact is contact with the probation officer and swcontact is contact with the street worker (only treatment group members have the possibility of contact with the street worker and comparison group members are included in the analysis with a value of swcontact=0).

Sensitivity analysis. In order to test the robustness of findings, I have conducted several sensitivity analyses. Detecting both Type I and Type II errors is critical because of the cost of the intervention and the high stakes outcome of interest. Type I error would result in high spending for an ineffective program (when we thought it was actually effective). Type II error would declare the program ineffective when it was, in fact, reducing involvement in ongoing crime.

First, as described above, I have assessed the extent to which estimates change utilizing different missing data protocols. Second, I use multiple approaches to estimate the effect of YVRP on participant outcomes: both through logistic regression and propensity score methods. I use the propensity scores in two different ways—stratification and regression adjustment—to determine if they yield similar estimates.

CHAPTER 5: PROPENSITY SCORES

The Models

Propensity scores were generated using the pscore program in STATA 12 (Becker & Ichino, 2002). Several iterations of the model were tested. The final models are shown in Tables 4 and 5.

Table 4: Juvenile Probationer Propensity Score Models

	Listwise Deletion		Mean Substitution		Multiple Imputation[a]	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Male	0.75**	0.26	0.77**	0.25	0.75**	0.40
Black	0.05	0.37	0.07	0.35	0.07	0.32
Hispanic	1.29**	0.48	1.18**	0.44	1.17**	0.66
Age	0.01	0.06	0.01	0.06	0.01	0.05
Number of prior arrests	-0.11*	0.06	-0.06	0.05	-0.06	0.05
Previously shot	1.07**	0.39	0.95*	0.38	0.84*	0.46
Most serious prior: violent crime	-0.82***	0.25	-0.87***	0.24	-0.85***	0.43
Age at first arrest	-0.01	0.06	0.01	0.06	0.01	0.05
Drug use	0.35*	0.18	0.40*	0.16	0.41*	0.23
Lives in West-NW probation district	0.57*	0.23	0.53*	0.22	0.52*	0.30
Lives in South Central probation district	-1.33***	0.37	-1.40***	0.36	-1.38**	0.69
Designated as mentally retarded	0.16	0.24	0.16	0.24	0.21	0.23
Missing data dummy	NA	NA	-0.23	0.23	NA	NA
Constant	-0.37	1.10	-0.76	1.03	-0.83	1.00
Number of observations included in analysis	320		364		364	
Correctly classified	70.00%		70.33%		69.92%	

Note: (a) Five imputations: coefficient is the average across imputations and SE is calculated as follows: $SQRT(1/M*SUM(s_k^{2)})+((1+1/M)*(1/(M-1))*SUM(a_k-a)^2$ where M is number of imputations, s is SE, k is the number of data sets, and a_k are coefficients for each imputation and a is the average coefficient across imputations (Rubin, 1987) (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.

Table 5: Adult Probationer Propensity Score Models						
	Listwise deletion		Mean Substitution		Multiple Imputation[a]	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Male	-0.42	0.36	0.56[t]	0.32	-0.55[t]	0.38
Black	-0.86	0.65	-0.38	0.49	-0.27	0.44
Hispanic	-0.18	0.69	0.34	0.52	0.43	0.49
Age	-0.27***	0.08	-0.25***	0.07	-0.28***	0.14
In drug/alcohol treatment	0.05	0.27	-0.03	0.25	-0.06	0.22
Years previously on probation	-0.32***	0.08	-0.28***	0.07	-0.26***	0.36
Years previously incarcerated	0.12	0.07	0.11	0.07	0.12	0.08
Number of prior arrests	0.14	0.10	0.10	0.09	0.12	0.09
Previously shot	0.46	0.25	0.27	0.22	0.28	0.24
Most serious prior: violent crime	-0.30	0.26	-0.33	0.23	-0.24	0.24
Most serious prior: drug crime	-0.32	0.26	-0.28	0.24	-0.19	0.23
Have child	0.45*	0.18	0.48**	0.18	0.44	0.26
Age at first arrest	0.02	0.09	0.02	0.08	0.06	0.08
Use marijuana	0.26	0.20	0.25	0.18	0.25	0.19
Other drug use	0.15	0.25	0.11	0.23	0.09	0.21
Lives in West-NW probation district	0.68*	0.27	0.77**	0.26	0.72*	0.40
Lives in South Central probation district	-0.08	0.32	0.11	0.29	0.06	0.26
Missing data dummy	NA	NA	-0.07	0.20	NA	NA
Constant	6.50	1.55	5.76	1.37	5.42	2.78
Number of observations	312		364		364	
Correctly classified	74.68%		72.83%		73.90%	

Note: (a) Five imputations: coefficient is the average across imputations and SE is calculated as follows: $SQRT(1/M*SUM(s_k^{2)})+((1+1/M)*(1/(M-1))*SUM(a_k-a)^2$ where M is number of imputations, s is SE, k is the number of data sets, and a_k are coefficients for each Imputation and a is the average coefficient across imputations (Rubin, 1987) (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.

Assessment of Propensity Score Models

Figures 2 through 7 display the distribution of the estimated propensity scores for juvenile and adult probationers handling missing data in each of the ways described in the previous chapter. Ideally, the distributions for the treatment and comparison groups would demonstrate significant overlap. Figures 2 through 7 show that some overlap

between groups exists using the propensity scores generated with the models delineated above. Scores on the "extremes" will be removed from analyses, focusing only on the region of common support (defined in more detail later in this paper). This means that the group of YVRP participants included in the analysis are those whose profiles were in the lower ranges of propensity to receive treatment (in other words, based on their profiles, those who are excluded are statistically, given the data available on covariates, those who best "fit" the profile of a YVRP participant).

Figure 2: Juvenile Probationer Propensity Score Distribution Listwise Deletion

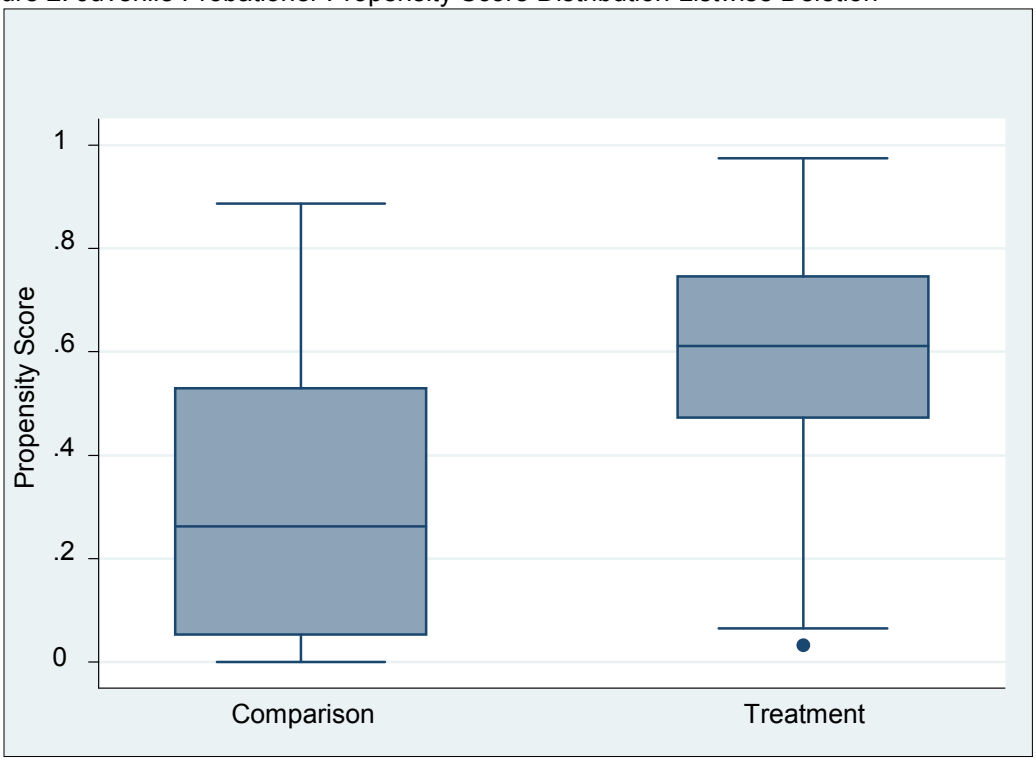


Figure 3: Juvenile Probationer Propensity Score Distribution Mean Substitution

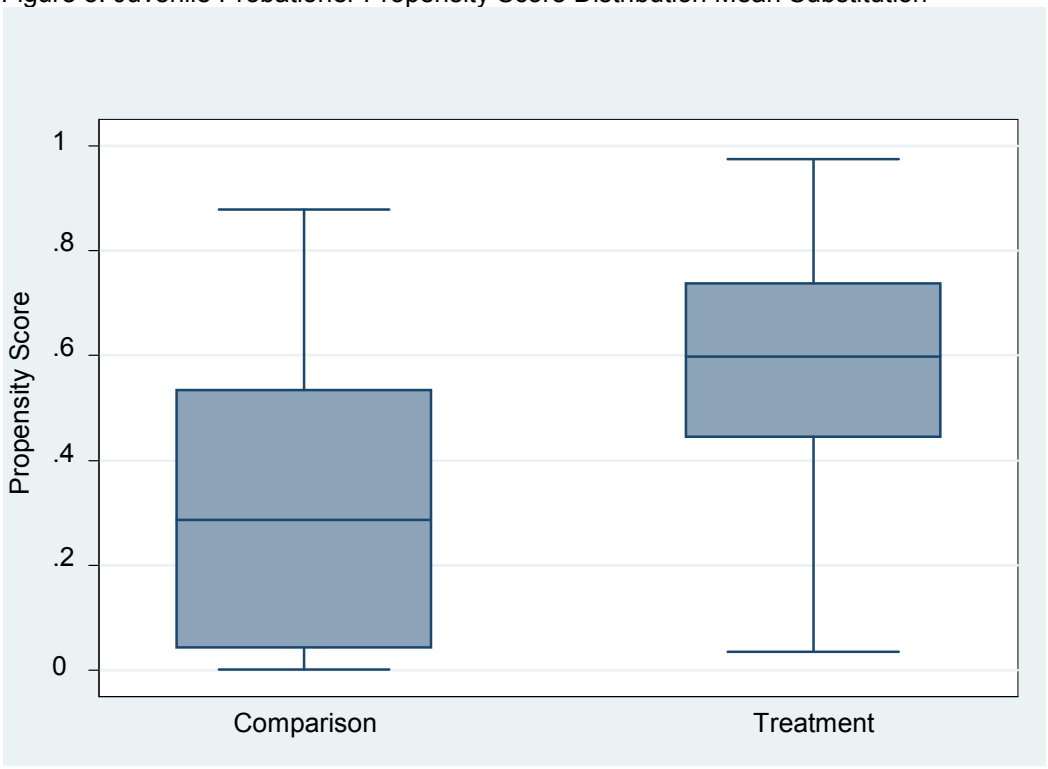


Figure 4: Juvenile Probationer Propensity Score Distribution Multiple Imputation

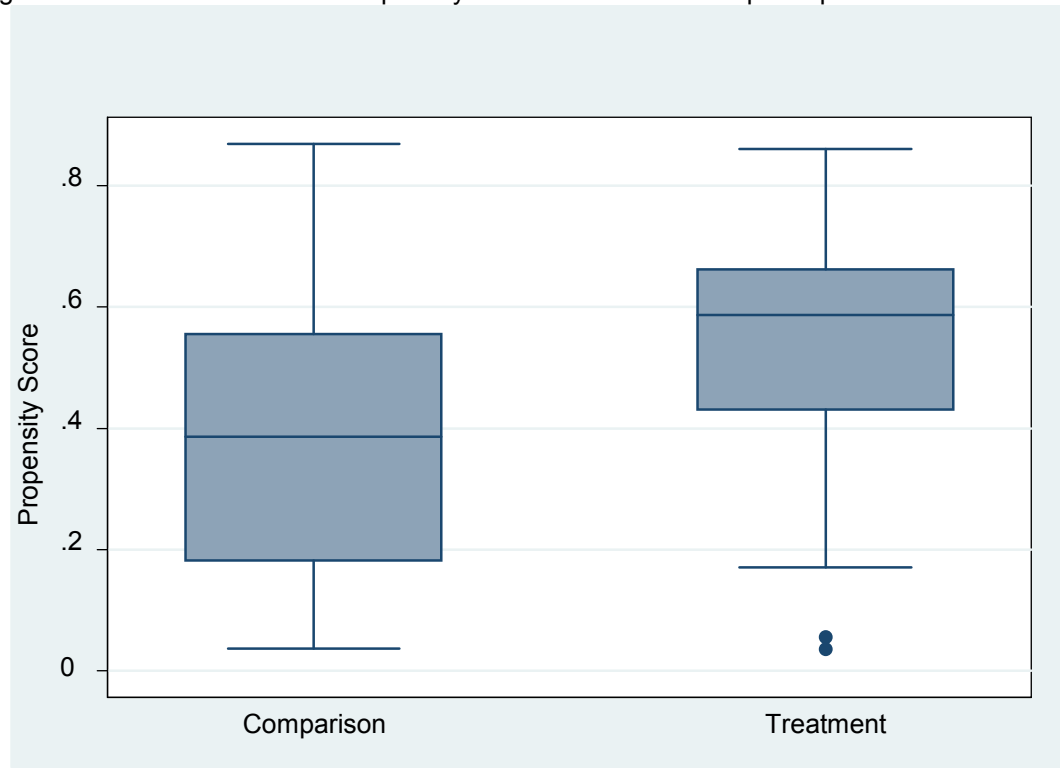


Figure 5: Adult Probationer Propensity Score Distribution Listwise Deletion

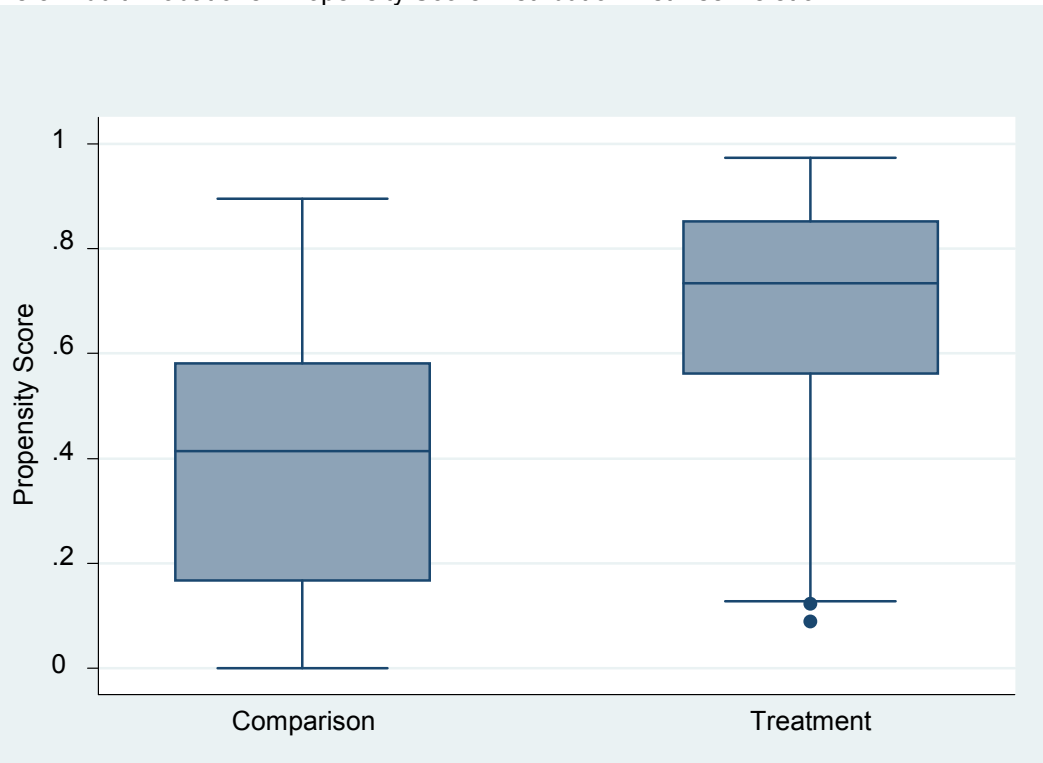


Figure 6: Adult Probationer Propensity Score Distribution Mean Substitution

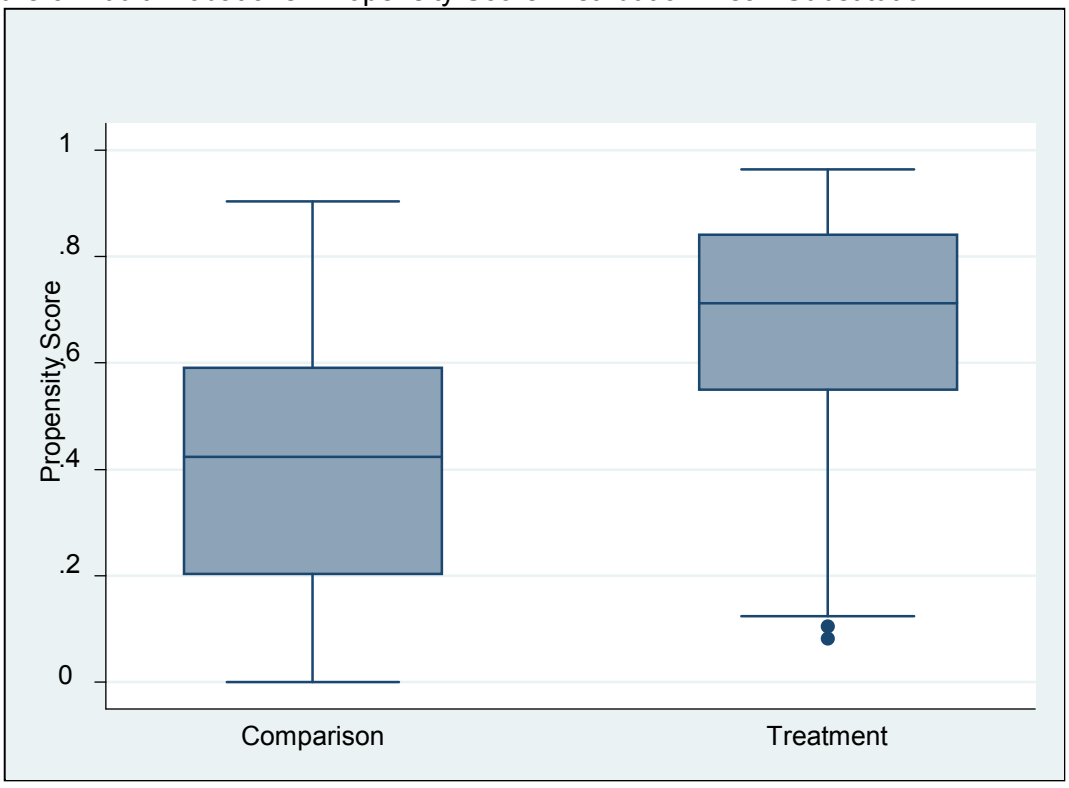
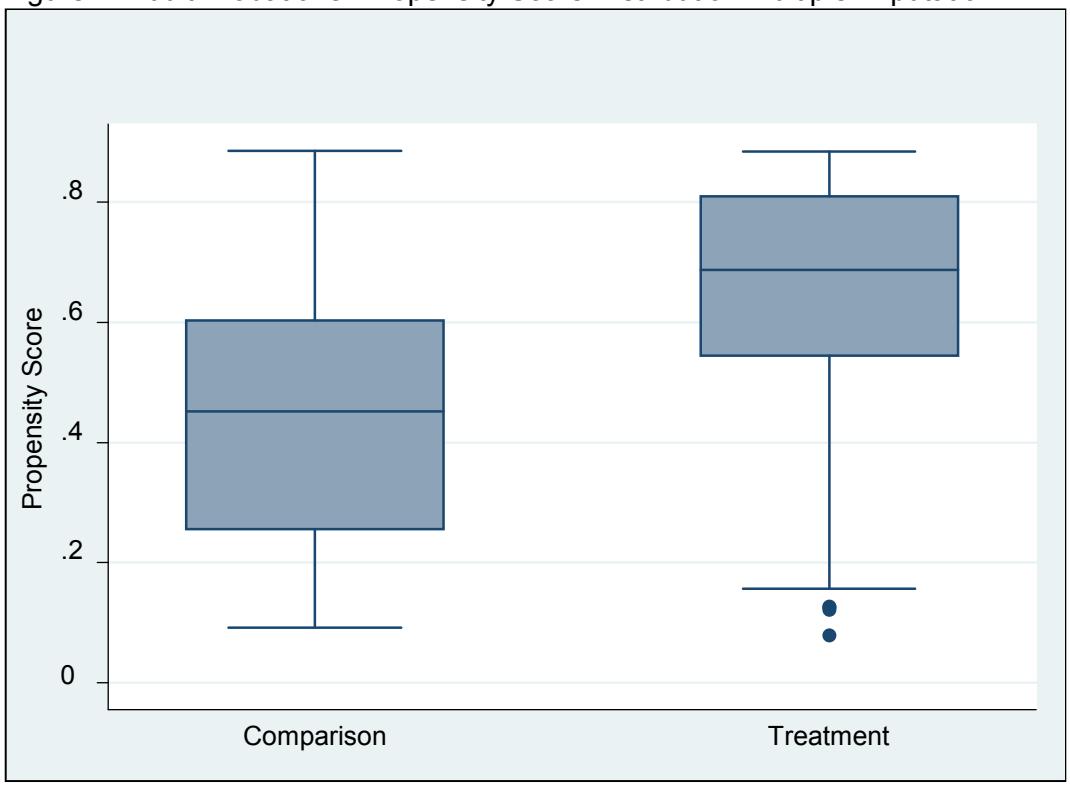


Figure 7: Adult Probationer Propensity Score Distribution Multiple Imputation



A second approach to assess the quality of the propensity score models is to assess the extent to which treatment and comparison group members are similar within strata of propensity score. This analysis utilizes STATA generated blocks (strata) of propensity score (within which there are no significant differences between treatments and comparison group members on covariates included in the model); however, Rubin (1997) asserts that with a large enough sample, splitting the data into quintile strata of propensity score will work equally well. In addition to assuring that no significant differences exist within each block, assessing the standardized differences (effect size) of the mean differences between treatment and comparison groups within strata is a prudent test of the effectiveness of the propensity score's balancing property.

Tables 6 and 7 show the standardized differences by strata of propensity score. Within strata there remain, in some cases, quite large differences. However, averaging across all propensity scores yields reasonable marginal standardized differences.

One indication that the propensity score is well-specified is that as the propensity score gets larger, the proportion of participants in the treated condition should grow and the proportion in the untreated group should diminish. This does not fully true in Table 7[20]; nonetheless, with the variables available, this was the strongest distribution achieved.

[20] It is important to note that both the lowest and highest strata have members removed due to the region of common support restriction.

Table 6: Assessment of Effectiveness of Propensity Score at Balancing among Juvenile Probationers: Cohen's D

	Listwise Deletion						Mean Substitution						Multiple Imputation								
	Block						Block						Block								
Variable	1	2	3	4	5	*Ave*	1	2	3	4	5	*Ave*	1	2	3	4	5	6	7	8	*Ave*
Male	-0.14	-0.17	0.03	0.24	N/A	*-0.01*	-0.38	0.24	-0.18	0.24	N/A	*-0.02*	-0.05	0.11	-0.32	N/A	0.22	0.30	N/A	N/A	*0.03*
Most serious prior: violent crime	-0.12	0.02	-0.09	-0.07	0.02	*-0.05*	-0.10	-0.04	0.02	-0.04	-0.08	*-0.05*	-0.10	-0.08	-0.34	N/A	0.22	0.13	0.11	-0.26	*-0.04*
Drug use	0.07	-0.43	0.30	-0.00	0.22	*0.03*	0.13	-0.15	-0.01	0.07	0.24	*0.06*	0.14	-0.13	-0.09	-0.28	-0.01	-0.12	1.50	N/A	*0.13*
Hispanic	N/A	N/A	-0.03	0.11	0.11	*0.04*	N/A	-0.10	0.18	0.14	-0.39	*-0.04*	N/A	-0.36	0.32	N/A	0.47	-0.29	0.34	-0.38	*0.01*
Black	-0.18	0.22	-0.02	-0.12	-0.11	*-0.04*	-0.10	0.14	-0.12	-0.15	0.39	*0.03*	-0.09	0.07	-0.21	N/A	-0.47	0.37	-0.34	0.38	*-0.04*
Number of prior arrests	-0.45	-0.27	-0.06	0.54	-0.23	*-0.09*	0.04	-0.20	-0.18	0.43	0.81	*0.18*	0.09	-0.12	-0.37	-0.44	0.41	0.40	0.67	0.72	*0.17*
Previously Shot	0.22	0.31	-0.24	0.03	-0.31	*0.00*	0.20	0.28	N/A	-0.30	0.04	*0.04*	0.20	0.26	N/A	0.28	-0.29	N/A	-0.45	0.06	*0.01*
Age	0.31	-0.08	-0.11	0.04	0.03	*0.04*	-0.14	0.32	-0.23	0.25	-0.22	*0.00*	-0.30	0.48	0.12	-1.03	0.02	0.58	0.15	-0.40	*-0.05*
Designated as mentally retarded	0.26	-0.32	0.07	0.18	-0.22	*-0.01*	0.20	-0.14	-0.16	0.39	0.25	*0.11*	0.20	0.02	-0.39	0.16	0.07	0.44	-0.14	0.36	*0.09*
Age at first arrest	0.44	0.14	0.09	-0.28	0.30	*0.14*	-0.11	0.30	0.15	-0.22	-0.47	*-0.07*	-0.33	0.38	0.43	-0.34	-0.06	0.14	-0.94	-0.71	*-0.18*
Lives in West-NW probation district	-0.45	-0.01	0.08	-0.12	-0.11	*-0.12*	0.25	-0.32	-0.03	-0.17	0.39	*0.03*	-0.07	-0.18	-0.24	N/A	0.11	0.49	-1.13	0.38	*-0.08*
Lives in South Central probation district	0.17	0.07	N/A	N/A	N/A	*0.05*	0.08	0.02	N/A	N/A	N/A	*0.02*	0.12	-0.09	N/A	N/A	N/A	N/A	N/A	N/A	*0.00*
Missing data dummy	N/A	N/A	N/A	N/A	N/A	*N/A*	-0.30	0.10	-0.02	-0.05	N/A	*-0.05*	N/A	N/A	N/A	N/A	N/A	N/A	N/A	N/A	*N/A*

Table 7: Assessment of Effectiveness of Propensity Score at Balancing among Adult Probationers: Cohen's D

	Listwise Deletion						Mean Substitution						Multiple Imputation						
	Block						Block						Block						
Variable	1	2	3	4	5	*Average*	1	2	3	4	5	*Average*	1	2	3	4	5	6	*Average*
Male	0.41	N/A	-0.33	-0.29	0.00	*-0.04*	0.28	0.56	-0.19	-0.25	-0.03	*0.07*	0.29	0.68	-0.18	-0.29	N/A	-0.20	*0.05*
Black	-0.34	0.49	-0.11	0.02	0.17	*0.05*	-0.28	0.11	0.06	-0.10	0.32	*0.02*	-0.04	0.06	0.04	-0.22	0.09	0.47	*0.07*
Hispanic	0.23	-0.49	0.05	0.02	-0.10	*-0.06*	0.20	-0.19	-0.06	0.10	-0.28	*-0.05*	-0.06	0	-0.04	0.18	0.06	-0.42	*-0.05*
Age	0.05	-0.18	-0.03	0.21	0.11	*0.03*	0.18	-0.04	-0.11	0.08	0.07	*0.04*	0.10	0.31	-0.30	0.07	0.12	0.07	*0.06*
In drug/alcohol treatment	-0.45	-0.08	-0.01	0.01	0.19	*-0.07*	0.00	-0.04	0.15	-0.13	-0.14	*-0.03*	-0.37	0.43	0.21	-0.19	0.24	-0.25	*0.01*
Previously shot	-0.62	0.42	-0.10	-0.07	0.17	*-0.04*	-0.45	0.41	-0.28	0.13	0.28	*0.02*	-0.18	0.40	-0.21	0.04	0.01	0.25	*0.05*
Most serious prior: violent crime	0.13	0.20	0.13	-0.07	-0.06	*0.07*	0.54	0.17	-0.13	-0.03	0.07	*0.12*	0.42	0.36	-0.20	0.17	-0.53	0.06	*0.05*
Most serious prior: drug crime	-0.26	-0.33	-0.18	0.26	0.14	*-0.07*	-0.54	-0.32	0.11	0.22	-0.06	*-0.12*	-0.42	-0.38	0.12	-0.17	0.82	-0.08	*-0.02*
Have child	0.25	0.29	0.05	-0.04	-0.13	*0.08*	-0.09	0.36	0.20	-0.12	-0.13	*0.04*	0.09	0.59	-0.03	-0.07	-0.30	0.02	*0.05*
Marijuana use	0.25	0.29	-0.13	-0.37	0.19	*0.05*	0.07	0.32	-0.13	-0.13	0.24	*0.07*	0.08	0.41	-0.09	-0.35	0.00	0.26	*0.05*
Other drug use	-0.02	-0.36	0.09	0.32	-0.29	*-0.05*	0.21	-0.19	0.12	-0.01	-0.23	*-0.02*	-0.02	0.07	0.02	0.40	-0.45	-0.30	*-0.05*
Number of prior arrests	-0.43	-0.32	-0.38	0.54	0.21	*-0.08*	-0.45	0.04	-0.38	0.46	0.19	*-0.03*	-0.29	0.19	-0.52	0.29	0.48	0.53	*0.11*
Age at first arrest	0.54	0.26	-0.26	0.00	-0.13	*0.08*	0.60	0.15	-0.10	-0.20	-0.11	*0.07*	0.60	0.39	-0.23	-0.28	0.05	-0.24	*0.05*
Years previously on probation	-0.20	-0.42	0.21	0.34	0.06	*-0.00*	-0.62	-0.02	0.20	0.28	0.07	*-0.02*	-0.36	0.54	0.14	0.18	0.24	0.41	*0.19*
Years previously incarcerated	-0.91	-0.33	0.03	0.38	0.20	*-0.13*	-0.90	-0.23	0.03	0.35	0.17	*-0.12*	0.14	0.17	-0.02	0.41	0.03	0.15	*0.15*
Lives in West-NW probation district	0.33	-0.40	0.02	0.11	-0.07	*-0.00*	-0.07	-0.02	0.07	-0.04	0.03	*-0.01*	-0.05	-0.18	0.02	-0.18	0.23	0.76	*0.10*
Lives in South Central probation district	-0.13	0.39	-0.15	-0.19	0.47	*0.08*	0.00	-0.08	-0.06	0.02	0.53	*0.08*	0.03	-0.09	-0.13	0.01	0.01	-0.40	*-0.10*
Missing data dummy	N/A	N/A	N/A	N/A	N/A	*N/A*	-0.31	-0.11	0.07	0.10	0.05	*-0.04*	N/A	N/A	N/A	N/A	N/A	N/A	*N/A*

Table 8: Number of Study Participants Falling into Strata by Probation Type and Treatment Status						
	Listwise Deletion		Mean Substitution		Multiple Imputation	
	Comparison	Treatment	Comparison	Treatment	Comparison	Treatment
	Juvenile Probationers					
Block						
1	34	5	45	5	50	5
2	42	19	43	24	32	20
3	48	39	57	48	37	29
4	19	43	19	45	21	14
5	2	31	3	30	13	27
6	N/A	N/A	N/A	N/A	8	16
7	N/A	N/A	N/A	N/A	6	9
8	N/A	N/A	N/A	N/A	3	24
missing	67	15	45	0	42	8
Total	212	152	212	152	212	152
	Adult Probationers					
Block						
1	21	8	27	9	36	14
2	29	8	37	11	32	8
3	39	38	48	42	42	32
4	26	55	34	72	26	54
5	7	63	6	68	10	26
6	N/A	N/A	N/A	N/A	7	47
missing	45	30	15	0	14	16
Total	167	202	167	202	167	197

Summary

The propensity scores presented here are imperfect, primarily because 1) the treatment and comparison group members are not comparable and 2) the observed variables included in the propensity score model, while theoretically important, are unlikely to be the only confounding variables. Other unmeasured factors are likely related to both the treatment selection mechanism and the likelihood of recidivism such

as (but not limited to) family background, and connections to school and work. Also, the geographical variables are rough and may not provide enough information to balance the treatment and comparison groups on neighborhood characteristics. Further, the propensity scores do an adequate job of marginal balancing (d<.10) but there remains large differences between treatment and comparison group members on confounders within strata of propensity score.

Lastly, each propensity score model was run using three different missing data methods. In all cases, all three models (for adults and juveniles) had very similar success rates in terms of accurately predicting treatment and comparison status (juvenile ranging from 69.92% to 70.33% accurately predicted; adult ranging from 72.83% to 74.68% accurately predicted). In both sets of analyses, the multiply imputed data resulted in the most unique (as compared to listwise deletion and mean substitution) distribution of propensity scores, resulting in more outliers, and a distribution skewed more towards 1 among treatment group members. For both adult and juvenile probationers, the case could be made that the propensity scores created using multiply imputed data were the least effective in balancing within strata. In the next chapter I will explore the extent to which propensity scores generated with the different missing data protocols yield different estimates of recidivism outcomes.

CHAPTER 6: RESULTS

This chapter presents the results of analyses assessing the extent to which YVRP results in reduced recidivism and how contacts with YVRP street workers are associated with recidivism outcomes. The first section describes the recidivism outcomes explored in this study. The second section presents analyses that are designed to answer the first research question: does participation in YVRP result in reduced recidivism? In the first half of this section, I explore if there is an effect of treatment on the incidence of recidivism. The results of three approaches to assess causality, basic logistic regression controlling for covariates that are associated with the outcomes, stratification based on propensity score, and covariate adjustment with propensity score in a logistic regression are presented. In each instance, the treatment effect estimates are generated three times, one for each of the missing data protocols adopted in this study (listwise deletion, mean substitution, and multiple imputation). This section concludes with a comparison of the findings gleaned from the different analytical approaches and different missing data protocols. Also included in this section is an exploration of the extent to which participation in YVRP may delay participation in crime; while all three missing data protocols are implemented, these analyses do not include the stratification method. This section also concludes with a comparison of estimates. The third and final section presents findings that will answer the second research question: how is the frequency of contact with street workers related to recidivism outcomes? This information is of direct use to program operators.

Recidivism Outcomes

This study is attempting to estimate the extent to which participation in YVRP results in a lower incidence of or delayed recidivism. The four recidivism outcome variables are:

- Any arrest/time to arrest for a violent crime over the 18 month follow up period
- Any arrest/time to arrest for any crime over the 18 month follow up period
- Any conviction/time to conviction for a violent crime over the 18 month follow up period
- Any conviction/time to conviction for any crime over the 18 month follow up period

Table 9 displays the proportion of treatment and comparison group members who experienced recidivism in one of these four ways within the 18 month follow up period. More juvenile probationers experienced recidivism than adults. Twenty-four percent of juvenile probationers in the comparison group were rearrested as were 22 percent juvenile probationers in the treatment group. Only a portion of these rearrests among juvenile probationers were for violent crimes: eighteen percent of comparison group members as compared with 15 percent of treatment group members on juvenile probation experienced rearrest for a violent crime. Of those, a smaller proportion, were convicted of those crimes. Among adults, 12 percent of comparison group members were rearrested during the follow up period, whereas 13 percent of treatments were rearrested. Four percent of comparison group members and 3 percent of treatment group members on adult probation were rearrested for a violent crime. Among those, all were convicted. This proportion is relatively small, which could impact regression models.

Table 9: Proportion of Treatment and Comparison Group Members Experiencing Recidivism Outcomes

	Comparison	Treatment
	Juvenile Probationers	
Any arrest at 18 months	0.24	0.22
Any violent arrest at 18 months	0.18	0.15

Any conviction at 18 months	0.20	0.18
Any violent conviction at 18 months	0.14	0.11
	Adult Probationers	
Any arrest at 18 months	0.12	0.13
Any violent arrest at 18 months	0.04	0.03
Any conviction at 18 months	0.09	0.12
Any violent conviction at 18 months	0.04	0.03

Does Participation in YVRP Result in Reduced Recidivism?

Incidence. Three analytical methods were used to assess if participation in YVRP results in reduced recidivism: logistic regression with individual covariates, propensity score stratification method, and propensity score regression method. The results of each analysis are presented and discussed below. The section concludes with a comparison of the three approaches.

Logistic regression. Regression presents the opportunity to break the connection between measured confounders and the outcome of interest, and because of this, is not based on the potential outcomes framework which emphasizes breaking the link between confounders and treatmebt. Nonetheless, it was the go-to approach for assessing causality in observational studies in many social sciences until recently. Tables 10 and 11 display the estimates from logistic regressions predicting each of the four recidivism outcomes of interest in this study for adult and juvenile probationers. It is important to note that this analysis takes advantage of the entire sample with valid cases; whereas the analyses using propensity score methods in the following sections are limited to cases that fall within the region of common support.

Table 10: Juvenile Probationer Logistic Regression Predicting Recidivism									
	Listwise Deletion			Mean Substitution			Multiple Imputation		
Any Arrest Over 18 Months	Coefficient	SE		Coefficient	SE		Coefficient	SE	
Treatment	0.40	0.29		0.49	0.27	t	0.50	0.27	t
Male	1.15	0.41	**	1.14	0.38	**	1.13	0.38	**
Black	-0.15	0.62		-0.38	0.59		-0.38	0.59	
Hispanic	0.37	0.79		-0.17	0.73		-0.17	0.73	
Age	-0.52	0.11	***	-0.46	0.10	***	-0.45	0.10	***
Number of prior arrests	0.24	0.09	**	0.18	0.08	*	0.18	0.08	*
Previously shot	-0.40	0.53		-0.38	0.52		-0.28	0.52	
Most serious prior: violent crime	0.25	0.38		0.18	0.37		0.17	0.37	
Designated as mentally retarded	0.47	0.39		0.49	0.38		0.48	0.38	
Age at first arrest	0.08	0.09		0.02	0.09		0.19	0.09	
Lives in West-NW probation district	-0.76	0.39	t	-0.91	0.37	*	-0.92	0.37	*
Lives in South Central probation district	-0.26	0.45		-0.36	0.43		-0.37	0.43	
Drug use	0.37	0.27		0.30	0.25		0.27	0.25	
Missing data dummy	N/A	N/A		-0.45	0.37		N/A	N/A	
Constant	5.82	1.79		6.37	1.67		6.17	1.65	
Number of observations included in analysis	320			364			364		
Correctly classified	65.00%			64.01%			71.96%		
Violent Arrest Over 18 Months									
Treatment	0.12	0.30		0.18	0.28		0.19	0.28	
Male	0.98	0.45	*	0.97	0.42	*	0.97	0.41	*
Black	-0.71	0.62		-0.68	0.59		-0.68	0.59	
Hispanic	-0.38	0.77		-0.76	0.72		-0.76	0.72	
Age	-0.52	0.11	***	-0.48	0.10	***	-0.47	0.10	***
Number of prior arrests	0.19	0.08	*	0.13	0.07	t	0.13	0.08	t
Previously shot	-0.75	0.63		-0.76	0.62		-0.65	0.62	
Most serious prior: violent crime	-0.55	0.38		-0.40	0.37		-0.38	0.37	
Designated as mentally retarded	0.46	0.38		0.51	0.37		0.50	0.37	
Age at first arrest	0.09	0.10		0.02	0.09		0.02	0.09	
Lives in West-NW probation district	0.01	0.41		-0.25	0.38		-0.26	0.38	
Lives in South Central probation district	0.56	0.48		0.36	0.45		0.35	0.45	
Drug use	0.37	0.28		0.29	0.26		0.27	0.26	
Missing data dummy	N/A	N/A		-0.47	0.39		N/A	N/A	
Constant	6.29	1.80		6.65	1.69		6.43	1.67	
Number of observations included in analysis	320			364			364		
Correctly classified	68.44%			68.96%			71.32%		

Any Conviction Over 18 Months									
Treatment	0.33	0.29		0.40	0.27		0.39	0.27	
Male	1.56	0.50	**	1.66	0.48	***	1.66	0.48	***
Black	0.42	0.67		0.06	0.60		0.06	0.60	
Hispanic	1.07	0.79		0.39	0.71		0.39	0.71	
Age	-0.40	0.11	***	-0.34	0.10	***	-0.34	0.10	***
Number of prior arrests	0.27	0.09	**	0.20	0.08	*	0.20	0.08	*
Previously shot	-0.32	0.53		-0.29	0.52		-0.21	0.52	
Most serious prior: violent crime	0.55	0.40		0.43	0.38		0.41	0.38	
Designated as mentally retarded	0.26	0.38		0.30	0.37		0.29	0.37	
Age at first arrest	0.14	0.09		0.06	0.09		0.05	0.09	
Lives in West-NW probation district	-0.35	0.39		-0.60	0.36	t	-0.60	0.36	t
Lives in South Central probation district	-0.10	0.46		-0.20	0.43		-0.21	0.43	
Drug use	0.42	0.27		0.34	0.26		0.32	0.26	
Missing data dummy	N/A	N/A		-0.17	0.37		N/A	N/A	
Constant	0.97	1.72		1.94	1.60		1.90	1.59	
Number of observations included in analysis	320			364			364		
Correctly classified	64.69%			67.03%			69.36%		
Violent Conviction Over 18 Months									
Treatment	0.06	0.31		0.12	0.30		0.12	0.29	
Male	1.10	0.50	*	1.21	0.49	*	1.20	0.49	*
Black	-0.13	0.72		-0.24	0.65		-0.24	0.65	
Hispanic	0.73	0.85		0.11	0.77		0.11	0.77	
Age	-0.43	0.12	***	-0.37	0.10	***	-0.37	0.10	***
Number of prior arrests	0.16	0.09	t	0.09	0.08		0.09	0.08	
Previously shot	-0.72	0.68		-0.74	0.68		-0.61	0.68	
Most serious prior: violent crime	-0.19	0.40		-0.07	0.39		-0.08	0.39	
Designated as mentally retarded	0.44	0.39		0.49	0.38		0.45	0.38	
Age at first arrest	0.12	0.10		0.05	0.10		0.05	0.10	
Lives in West-NW probation district	0.43	0.45		0.04	0.41		0.02	0.40	
Lives in South Central probation district	0.78	0.53		0.48	0.48		0.46	0.48	
Drug use	0.30	0.29		0.22	0.28		0.23	0.28	
Missing data dummy	N/A	N/A		-0.43	0.42		N/A	N/A	
Constant	2.74	1.82		3.19	1.71		3.04	1.69	
Number of observations included in analysis	320			364			364		
Correctly classified	73.75%			74.73%			67.42%		

Note: (a) The multiple imputation regression estimates are based on the average of 5 imputations, with SE adjustments. (b) t p<.10, *p,.05, **p<.01, ***p,.001 (c) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.

Table 11: Adult Probationer Logistic Regression Predicting Recidivism

Any Arrest After 18 Months									
	Listwise deletion			Mean Substitution			Multiple Imputation		
Variable	Coefficient	SE		Coefficient	SE		Coefficient	SE	
Treatment	-0.10	0.32		-0.28	0.29		-0.31	0.29	
Male	0.30	0.68		0.55	0.65		0.49	0.66	
Black	0.27	0.90		0.68	0.84		0.80	0.84	
Hispanic	0.55	0.99		0.76	0.93		0.87	0.92	
Age	-0.02	0.13		-0.07	0.12		-0.06	0.12	
In drug/alcohol treatment	0.28	0.43		0.18	0.40		0.04	0.42	
Previously shot	-0.78	0.47	t	-0.75	0.43	t	-0.72	0.46	
Most serious prior: violent crime	-0.21	0.42		-0.02	0.41		0.10	0.43	
Most serious prior: drug crime	-0.37	0.43		0.05	0.41		0.14	0.44	
Have child	0.03	0.31		0.04	0.30		0.05	0.30	
Marijuana use	0.81	0.37	*	0.62	0.34	t	0.72	0.34	*
Other drug use	0.97	0.44	*	0.88	0.40	*	0.90	0.41	*
Number of prior arrests	0.11	0.17		0.20	0.15		0.20	0.15	
Age at first arrest	-0.19	0.16		-0.11	0.14		-0.15	0.15	
Years previously on probation	-0.31	0.14	*	-0.21	0.12	t	-0.21	0.12	t
Years previously incarcerated	0.25	0.13	*	0.24	0.12	*	0.24	0.12	*
Lives in West-NW probation district	0.13	0.48		0.22	0.45		0.17	0.45	
Lives in South Central probation district	0.65	0.54		0.48	0.50		0.42	0.50	
Constant	1.93	2.67		0.23	0.35		0.78	2.49	
Missing data dummy	N/A	N/A		0.56	2.48		N/A	N/A	
Number of observations included in analysis	312			364			364		
Correctly classified	76.28%			75.54%			66.44%		
Violent Arrest After 18 Months									
Treatment	-0.08	0.59		-0.52	0.50		-0.46	0.50	
Male	0.51	1.15		0.52	1.10		0.50	1.10	
Black	-0.72	1.31		0.00	1.21		0.33	1.20	
Hispanic	1.03	1.45		0.94	1.30		1.25	1.31	
Age	0.16	0.23		0.20	0.19		0.23	0.19	
In drug/alcohol treatment	-0.99	1.12		-1.11	1.07		-1.11	1.08	
Previously shot	0.60	0.64		0.55	0.57		0.56	0.58	
Most serious prior: violent crime	0.19	0.73		0.56	0.74		0.50	0.74	
Most serious prior: drug crime	-1.37	0.93		-0.47	0.83		-0.51	0.83	
Have child	0.37	0.54		0.25	0.50		0.32	0.53	
Marijuana use	0.71	0.64		0.60	0.57		0.59	0.58	
Other drug use	1.36	0.75	t	1.01	0.66		0.97	0.67	

Table 11: Adult Probationer Logistic Regression Predicting Recidivism									
Number of prior arrests	-0.62	0.38		-0.23	0.29		-0.23	0.28	
Age at first arrest	-0.22	0.27		-0.29	0.23		-0.34	0.24	
Years previously on probation	-0.38	0.26		-0.45	0.23	t	-0.46	0.23	*
Years previously incarcerated	0.32	0.18	t	0.27	0.16	t	0.26	0.16	t
Lives in West-NW probation district	0.82	1.05		0.43	0.85		0.39	0.84	
Lives in South Central probation district	1.79	1.11		0.92	0.91		0.84	0.90	
Missing data dummy	N/A	N/A		-0.19	0.62		N/A	N/A	
Constant	-2.24	4.48		-2.19	4.05		-2.31	4.02	
Number of observations included in analysis	312			364			364		
Correctly classified	92.95%			93.21%			74.78%		
Any Conviction After 18 Months									
Treatment	-0.02	0.34		-0.13	0.30		-0.17	0.31	
Male	0.07	0.69		0.30	0.66		0.21	0.66	
Black	0.07	0.90		0.42	0.85		0.57	0.84	
Hispanic	0.59	1.00		0.71	0.94		0.84	0.93	
Age	-0.03	0.14		-0.07	0.12		-0.06	0.13	
In drug/alcohol treatment	0.23	0.46		0.01	0.44		-0.14	0.45	
Previously shot	-0.62	0.47		-0.56	0.44		-0.49	0.47	
Most serious prior: violent crime	-0.10	0.44		0.03	0.43		0.19	0.46	
Most serious prior: drug crime	-0.25	0.46		0.17	0.44		0.30	0.47	
Have child	-0.13	0.33		-0.14	0.32		-0.13	0.32	
Marijuana use	0.81	0.39	t	0.59	0.35	t	0.70	0.36	t
Other drug use	1.01	0.46	t	0.81	0.42	t	0.82	0.43	t
Number of prior arrests	0.20	0.18		0.22	0.16		0.23	0.16	
Age at first arrest	-0.14	0.17		-0.11	0.15		-0.15	0.15	
Years previously on probation	-0.37	0.15	t	-0.28	0.14	*	-0.28	0.14	*
Years previously incarcerated	0.21	0.13	t	0.24	0.12	*	0.24	0.12	*
Lives in West-NW probation district	0.31	0.52		0.29	0.48		0.22	0.49	
Lives in South Central probation district	1.00	0.58	t	0.80	0.54		0.72	0.54	
Missing data dummy	N/A	N/A		0.11	0.37		N/A	N/A	
Constant	1.03	2.74		0.66	2.58		0.87	2.61	
Number of observations included in analysis	312			364			364		
Correctly classified	78.85%			78.80%			66.94%		
Violent Conviction After 18 Months									
Treatment	0.06	0.61		-0.41	0.50		-0.37	0.51	
Male	0.38	1.16		0.41	1.10		0.39	1.11	
Black	-0.84	1.27		-0.07	1.20		0.24	1.19	
Hispanic	0.92	1.43		0.87	1.29		1.15	1.31	

Table 11: Adult Probationer Logistic Regression Predicting Recidivism

Age	0.09	0.23		0.14	0.19		0.18	0.19	
In drug/alcohol treatment	-0.98	1.13		-1.10	1.08		-1.10	1.08	
Previously shot	0.65	0.64		0.61	0.57		0.63	0.57	
Most serious prior: violent crime	0.13	0.74		0.50	0.74		0.44	0.74	
Most serious prior: drug crime	-1.34	0.92		-0.43	0.83		-0.47	0.82	
Have child	0.18	0.56		0.07	0.52		0.15	0.55	
Marijuana use	0.62	0.65		0.52	0.58		0.51	0.58	
Other drug use	1.41	0.76	t	1.05	0.66		1.02	0.67	
Number of prior arrests	-0.56	0.38		-0.18	0.28		-0.19	0.28	
Age at first arrest	-0.21	0.27		-0.30	0.24		-0.34	0.24	
Years previously on probation	-0.33	0.26		-0.41	0.23	t	-0.42	0.23	t
Years previously incarcerated	0.30	0.19		0.25	0.16		0.25	0.16	
Lives in West-NW probation district	0.81	1.06		0.40	0.86		0.36	0.85	
Lives in South Central probation district	1.91	1.14	t	1.01	0.92		0.92	0.91	
Missing data dummy	N/A	N/A		-0.11	0.62		N/A	N/A	
Constant	-0.75	4.54		-0.86	4.12		-0.94	4.09	
Number of observations included in analysis	312			364			364		
Correctly classified	93.27%			93.48%			73.78%		

Note: (a) The multiple imputation regression estimates are based on the average of 5 imputations, with SE adjustments. (b) t p<.10, *p,.05, **p<.01, ***p<.001 (c) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.

Among juveniles, age and being male are consistently associated with the likelihood of recidivism. Younger youth are more likely to recidivate, as are males. For all recidivism outcomes except conviction for a violent crime, a greater number of prior arrests also is associated with a greater likelihood of recidivism. And in some cases, living in the West-NW probation district was significantly associated with a lower likelihood of recidivism. Two models generated an estimate of the treatment effect among juveniles that was marginally significant (p<.01), suggesting that participation in YVRP is associated with a *greater* likelihood of being rearrested over the follow up period.

There were no statistically significant treatment effects in the adult probation sample, nor did age and gender have a significant impact on outcomes. However,

models predicting any arrest or any conviction suggest that adult probationers with more years on probation are less likely to experience recidivism over the 18 month follow up period, while those who had more years of incarceration experience a greater likelihood of recidivism. Marijuana and other illegal drug use was also associated consistently with the likelihood of a new arrest or conviction over the 18 month follow up period, but not with arrests or convictions for violent crimes (except trending toward significance for arrest for a violent crime in the model using a mean substitution protocol). While treatment status was not significantly related to the likelihood of recidivism in any of the models; however the direction of the estimate is in the hypothesized direction (those with YVRP participation were less likely to experience recidivism).

The three missing data protocols produced fairly disparate estimates both among juvenile and adult probationers. Only in two cases were the treatment estimates approaching significance ($p<.10$); both were among juveniles for the outcome "any arrest over 18 months." Both estimates were similar: mean substitution as a missing data protocol resulted in an odds ratio of 1.63 and multiple imputation resulted in an odds ratio of 1.64 (the odds of recidivism over 18 months is 63 (MS) or 64 (MI) percent higher among treatment group participants than comparison group members). The fact that the estimates generated under these three missing data protocols are not more similar is suggestive of the fact that missing data matters—a point I will return to throughout this paper.

Propensity score methods. The propensity scores presented previously in this study were used in two different ways to estimate the impact of YVRP on recidivism. Only cases within the area of common support were included in the analyses that follow.

Stratification method. The stratification method involves calculating mean differences in the outcome of interest within strata of study subjects defined by

propensity score within which treatment and comparison group members are similar (not meaningfully or significantly different) on confounding variables. In essence, the stratification method is most akin to a random assignment design, and theoretically, if the assumptions of the approach are met, a simple weighted difference of block-specific treatment effects (means) will produce the average treatment effect on the treated (ATT). While Rubin (1997) suggests that strata can be generated in large datasets by creating strata based on quintiles of propensity score, because this study has a small sample size, propensity score strata were created using STATA's *pscore* command which automates the process of defining strata within which there are no significant differences between treatment and comparison group members on measured confounding variables. It is important to note that the assumptions of the stratification method—that treatment and comparison group members within strata are very similar on measured confounders, is not met in this study (see discussion of Tables 6 and 7).

Tables 12 and 13 display the average effect of treatment on the treated for juvenile and adult probationers. Overall, the stratification analysis revealed that participation in YVRP does not have a significant impact on future recidivism among participants. For juvenile probationers, the direction of the treatment effect varied across and within measures of recidivism (from a 5.7 percentage point reduction in recidivism to a 9.6 percentage point increase in recidivism among treatments), and generally, analyses using multiple imputation for missing data resulted in the largest treatment effect estimates. For adult probationers, the treatment effect estimate, while also not significant, was more consistent, especially in terms of the direction of the effect. Among adults, estimates range from a 1.9 percentage point decrease in recidivism (conviction for a violent crime) to 9.9 percentage point reduction (for any arrest). Again, propensity scores created using the multiply imputed datasets resulted in the largest estimates.

Covariate adjustment using propensity score. Tables 14 and 15 display the results of analyses where the various propensity scores are used as an adjustment in a logistic regression. Focusing only on the treatment dummy, the analyses reveal there are no significant differences between treatment and comparison group members on any of the four recidivism outcomes. There are small differences among the models. In every case, for both the juvenile and adult probation samples, models with propensity scores generated with listwise deletion produce the smallest estimate of the treatment effect.

Comparison of approaches. The logistic regression approaches (with individual covariates and the composite covariate propensity score) are comparable (coefficients are logged odds[21]), while the stratification method produces an estimate of the average mean differences within strata of propensity score.

[21] In this study, the logged odds were not converted to odds ratios due to the fact that no coefficients were significant and the conversion would not aid in comparisons.

Table 12: Estimate of the Average Treatment Effect on the Treated Among Juvenile Probationers: YVRP Participation and Recidivism

Missing Data Protocol	Any Arrest at 18 Months			Any Violent Arrest at 18 Months			Any Conviction at 18 Months			Any Violent Conviction at 18 Months		
	ATT	SE	t	ATT	SE	t	ATT	SE	t	ATT	SE	t
Listwise deletion	-0.034	0.061	-0.557	0.004	0.127	0.028	-0.049	0.058	-0.842	-0.019	0.125	-0.154
Mean substitution	0.009	0.058	0.158	-0.057	0.052	-1.082	0.028	0.087	0.325	-0.045	0.082	-0.551
Multiple imputation	0.096	0.075	1.285	0.076	0.066	1.141	0.089	0.073	1.222	0.072	0.064	1.134

Note: (a) No t value is significant at p<.05

Table 13: Estimate of the Average Treatment Effect on the Treated Among Adult Probationers: YVRP Participation and Recidivism

Missing Data Protocol	Any Arrest at 18 Months			Any Violent Arrest at 18 Months			Any Conviction at 18 Months			Any Violent Conviction at 18 Months		
	ATT	SE	t	ATT	SE	t	ATT	SE	t	ATT	SE	t
Listwise deletion	-0.064	0.087	-0.726	-0.025	0.058	-0.421	-0.057	0.086	-0.667	-0.019	0.058	-0.325
Mean substitution	-0.074	0.084	-0.888	-0.060	0.062	-0.973	-0.061	0.083	-0.740	-0.056	0.062	-0.905
Multiple imputation	-0.099	0.072	-1.386	-0.07	0.055	-1.259	-0.086	0.07	-1.241	-0.065	0.055	-1.186

Note: (a) No t value is significant at p<.05

Table 14: Estimate of the Treatment Effect Using Propensity Score as a Covariate in a Logistic Regression Among Juvenile Probationers: YVRP Participation and Recidivism						
Any Arrest After 18 Months						
	Listwise Deletion		Mean Substitution		Multiple Imputation	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Propensity Score	0.49	0.58	0.63	0.55	0.09	0.09
Treatment	0.26	0.27	0.35	0.25	0.35	0.25
Constant	-0.48	0.27	-0.64	0.26	-0.65	0.28
Number of observations included in analysis	282		319		314	
Correctly classified	52.84%		55.17%		56.69%	
Violent Arrest After 18 Months						
Propensity Score	0.32	0.60	0.28	0.58	0.00	0.09
Treatment	0.11	0.28	0.17	0.27	0.15	0.27
Constant	-0.89	0.29	-0.96	0.28	-0.84	0.30
Number of observations included in analysis	282		319		314	
Correctly classified	66.31%		67.71%		68.47%	
Any Conviction After 18 Months						
Propensity Score	0.68	0.59	0.91	0.57	0.14	0.09
Treatment	0.25	0.28	0.32	0.26	0.32	0.26
Constant	-0.97	0.29	-1.15	0.28	-1.16	0.30
Number of observations included in analysis	282		319		314	
Correctly classified	62.41%		63.01%		63.69%	
Violent Conviction After 18 Months						
Propensity Score	0.75	0.65	0.70	0.64	0.05	0.10
Treatment	0.06	0.31	0.12	0.29	0.12	0.29
Constant	-1.48	0.33	-1.53	0.31	-1.38	0.33
Number of observations included in analysis	282		319		314	
Correctly classified	74.47%		75.55%		76.11%	
Note: (a) No propensity score or treatment dummy coefficients are significant at p<.05 (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.						

Table 15: Estimate of the Treatment Effect Using Propensity Score as a Covariate in a Logistic Regression Among Adult Probationers: YVRP Participation and Recidivism						
Any Arrest After 18 Months						
	Listwise Deletion		Mean Substitution		Multiple Imputation	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Propensity Score	0.51	0.65	0.28	0.59	0.07	0.08
Treatment	-0.10	0.31	-0.26	0.28	-0.32	0.28
Constant	-1.35	0.36	-1.12	0.32	-1.20	0.31
Number of observations included in analysis	289		350		334	
Correctly classified	75.17%		75.14%		75.74%	
Violent Arrest After 18 Months						
Propensity Score	-0.17	1.03	0.21	0.98	0.09	0.15
Treatment	-0.13	0.51	-0.52	0.47	-0.58	0.47
Constant	-2.34	0.56	-2.38	0.52	-2.57	0.51
Number of observations included in analysis	289		350		334	
Correctly classified	92.52%		92.66%		92.51%	
Any Conviction After 18 Months						
Propensity Score	0.66	0.68	0.46	0.62	0.09	0.09
Treatment	-0.02	0.33	-0.13	0.30	-0.20	0.30
Constant	-1.65	0.39	-1.48	0.35	-1.56	0.33
Number of observations included in analysis	289		350		334	
Correctly classified	78.23%		78.53%		79.04%	
Violent Conviction After 18 Months						
Propensity Score	-0.21	1.05	0.14	0.99	0.09	0.15
Treatment	-0.01	0.52	-0.42	0.47	-0.50	0.48
Constant	-2.43	0.58	-2.43	0.53	-2.64	0.52
Number of observations included in analysis	289		350		334	
Correctly classified	93.86%		92.94%		92.81%	
Note: (a) No propensity score or treatment dummy coefficients are significant at p<.05 (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.						

A comparison of the estimates produced using logistic regression with individual covariates versus the estimates produced when the propensity score was used in the model as a summary of the covariates, is presented in Table 16.

Table 8: Comparison of Treatment Effect Estimates From Logistic Regression Using Individual Covariates Versus Propensity Score

	Listwise Deletion		Mean Substitution		Multiple Imputation	
	Individual Covariates	Propensity Score	Individual Covariates	Propensity Score	Individual Covariates	Propensity Score
Juvenile Probationers						
Any Arrest Over 18 Months	0.40	0.26	0.49[t]	0.35	0.50[t]	0.35
Violent Arrest Over 18 Months	0.12	0.11	0.18	0.17	0.19	0.15
Any Conviction Over 18 Months	0.33	-0.25	0.40	0.32	0.27	0.32
Violent Conviction Over 18 Months	0.06	0.06	0.12	0.12	0.12	0.12
Adult Probationers						
Any Arrest Over 18 Months	-0.10	-0.10	-0.28	-0.26	-0.31	-0.32
Violent Arrest Over 18 Months	-0.08	-0.13	-0.52	-0.52	-0.46	-0.58
Any Conviction Over 18 Months	-0.02	-0.02	-0.13	-0.13	-0.17	-0.20
Violent Conviction Over 18 Months	0.06	-0.01	-0.41	-0.42	-0.37	-0.50

Note: (a) Estimates are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient
(b) t p<.10, *p,.05, **p<.01, ***p<.001

Overall, estimates from two models predicting any arrest over 18 months among juvenile probationers were approaching significance (p<.10; this pattern of findings is similar to the pattern observed using a logistic regression model controlling for individual covariates). In both cases, juvenile probationers receiving YVRP were 49 percent (MS(or 50 percent (MI) more likely to recidivate. The estimates generated using the varied missing data protocols were more similar in the adult probation sample than the juvenile sample. Furthermore, the direction of the treatment effects are comparable across all three analytical approaches, Table 17 shows that in the adult probation sample every estimate was in the direction favoring YVRP with the exception of one estimate (violent conviction over 18 months using logistic regression with individual covariates and listwise deletion for missing data); however, none of these treatment effects reached statistical significance. The direction of the treatment effect is more varied in the juvenile

probation sample. All three analytic approaches using multiple imputation of missing data result a treatment effect that was not in favor of YVRP participation for all four measures of recidivism. Using mean substitution, all but one treatment effect-- the effect of YVRP participation on a violent arrest over the follow up period using the stratification method—was in the direction favoring the comparison group. Finally, the listwise deletion protocol in the juvenile sample produced the most varied direction of treatment effects, with no clear directional pattern. Given the number of outcomes investigated, it is possible that the significant findings (denoted by bold in Table 17) were due to chance. Taken together, the results suggest no statistically significant impact of YVRP on recidivism.

Table 9: Comparison of Direction of Treatment Effect Estimates From Three Analytical Approaches

	Listwise Deletion			Mean Substitution			Multiple Imputation		
	LR: IC	SM	LR: PS	LR: IC	SM	LR: PS	LR: IC	SM	LR: PS
Juvenile Probationers									
Any Arrest Over 18 Months	+	-	+	**+**	+	+	**+**	+	+
Violent Arrest Over 18 Months	+	+	+	+	-	+	+	+	+
Any Conviction Over 18 Months	+	-	-	+	+	+	+	+	+
Violent Conviction Over 18 Months	+	-	+	+	+	+	+	+	+
Adult Probationers									
Any Arrest Over 18 Months	-	-	-	-	-	-	-	-	-
Violent Arrest Over 18 Months	-	-	-	-	-	-	-	-	-
Any Conviction Over 18 Months	-	-	-	-	-	-	-	-	-
Violent Conviction Over 18 Months	+	-	-	-	-	-	-	-	-

Note (a)LR:IC-Logistic regression with individual covariates, SM: Stratification Method, LR:PS: Logistic regression using propensity score as covariate

Hazard Rate. An alternative approach to assessing the effect of participating in YVRP on the incidence of recidivism is to consider whether participating in YVRP results in a reduction in the hazard of recidivism. Answering this question requires an appropriate analytical approach; typical regression approaches do not take into account

the survival distribution nor can they handle censoring. This survival analysis was conducted with Cox proportional hazards regression to assess if the hazard of recidivism for YVRP participants is lower than that of comparison group members.

Logistic regression with individual covariates. Tables 18 and 19 display the results of Cox regressions with individual covariates predicting the hazard of recidivism among the juvenile and adult probation samples.

Table 10: Juvenile Probationer Cox Regression Predicting Hazard of Recidivism

Variable									
	Listwise Deletion			Mean Substitution			Multiple Imputation		
Any Arrest Over 18 Months	Coefficient	SE		Coefficient	SE		Coefficient	SE	
Treatment	0.31	0.19		0.36	0.18	t	0.36	0.18	*
Male	0.82	0.32	*	0.82	0.30	**	0.81	0.30	**
Black	0.04	0.41		-0.11	0.38		-0.12	0.38	
Hispanic	0.17	0.47		-0.09	0.43		-0.10	0.43	
Age	-0.29	0.07	***	-0.25	0.06	***	-0.24	0.06	***
Number of prior arrests	0.15	0.05	**	0.09	0.04	*	0.09	0.04	*
Previously shot	-0.19	0.38		-0.18	0.38		-0.11	0.37	
Most serious prior: violent crime	0.11	0.25		0.09	0.24		0.07	0.24	
Designated as mentally retarded	0.17	0.24		0.20	0.23		0.19	0.24	
Age at first arrest	0.03	0.06		-0.01	0.06		-0.02	0.06	
Lives in West-NW probation district	-0.55	0.25	*	-0.66	0.23	*	0.09	0.17	
Lives in South Central probation district	-0.20	0.30		-0.24	0.28		-0.68	0.23	
Drug use	0.14	0.18		0.10	0.17		-0.26	0.28	
Missing data dummy	N/A	N/A		-0.34	0.26		N/A	N/A	
Number of observations included in analysis	320			364			364		
Violent Arrest Over 18 Months									
Treatment	0.13	0.23		0.16	0.22		0.17	0.22	
Male	0.78	0.37		0.80	0.35	*	0.79	0.35	*
Black	-0.35	0.46		-0.31	0.44		-0.29	0.44	
Hispanic	-0.13	0.56		-0.37	0.53		-0.37	0.53	
Age	-0.36	0.08	***	-0.33	0.07	***	-0.32	0.07	***
Number of prior arrests	0.14	0.06		0.08	0.05	t	0.07	0.05	
Previously shot	-0.66	0.52		-0.68	0.52		-0.59	0.52	
Most serious prior: violent crime	-0.39	0.27		-0.27	0.27		-0.27	0.27	
Designated as mentally retarded	0.26	0.27		0.31	0.26		0.30	0.28	
Age at first arrest	0.06	0.07		0.01	0.07		0.00	0.07	

Table 10: Juvenile Probationer Cox Regression Predicting Hazard of Recidivism

Lives in West-NW probation district	-0.10	0.31		-0.28	0.29		-0.31	0.29	t
Lives in South Central probation district	0.35	0.36		0.25	0.33		0.22	0.33	
Drug use	0.24	0.21		0.19	0.20		0.17	0.20	
Missing data dummy	N/A	N/A		-0.40	0.31		N/A	N/A	
Number of observations included in analysis	320			364			364		
Any Conviction Over 18 Months									
Treatment	0.28	0.22		0.32	0.20		0.32	0.20	
Male	1.31	0.43	*	1.41	0.43	***	1.41	0.43	**
Black	0.31	0.53		0.06	0.45		0.06	0.45	
Hispanic	0.58	0.60		0.17	0.51		0.17	0.51	
Age	-0.26	0.08	***	-0.22	0.07	***	-0.21	0.07	**
Number of prior arrests	0.16	0.05	**	0.10	0.04	*	0.10	0.04	*
Previously shot	-0.19	0.40		-0.15	0.40		-0.90	0.40	
Most serious prior: violent crime	0.36	0.30		0.30	0.29		0.27	0.29	
Designated as mentally retarded	0.14	0.26		0.17	0.25		0.16	0.26	
Age at first arrest	0.08	0.07		0.02	0.06		0.02	0.06	
Lives in West-NW probation district	-0.33	0.28		-0.53	0.25	*	-0.55	0.25	*
Lives in South Central probation district	-0.10	0.34		-0.20	0.32		-0.21	0.31	
Drug use	0.24	0.20		0.20	0.19		18.00	0.19	
Missing data dummy	N/A	N/A		-0.23	0.28		N/A	N/A	
Number of observations included in analysis	320			364			364		
Violent Conviction Over 18 Months									
Treatment	0.08	0.26		0.12	0.24		0.13	0.25	
Male	0.97	0.45	*	1.08	0.44	*	1.06	0.44	*
Black	-0.07	0.62		-0.14	0.56		-0.12	0.56	
Hispanic	0.55	0.71		0.11	0.65		0.10	0.65	
Age	-0.35	0.09	***	-0.32	0.08	***	-0.30	0.08	***
Number of prior arrests	0.12	0.07	t	0.07	0.06		0.06	0.06	
Previously shot	-0.70	0.60		-0.72	0.60		-0.60	0.60	
Most serious prior: violent crime	-0.19	0.33		-0.08	0.33		-0.09	0.33	
Designated as mentally retarded	0.33	0.30		0.37	0.30		0.35	0.31	
Age at first arrest	0.08	0.08		0.04	0.08		0.30	0.08	
Lives in West-NW probation district	0.26	0.38		-0.05	0.34		-0.08	0.34	
Lives in South Central probation district	0.59	0.44		0.36	0.40		0.32	0.40	
Drug use	0.26	0.24		0.19	0.23		0.20	0.23	
Missing data dummy	N/A	N/A		-0.43	0.36		N/A	N/A	
Number of observations included in analysis	320			364			364		

Note: (a) The multiple imputation regression estimates are based on the average of 5 imputations, with SE adjustments. (b) t p<.10, *p,.05, **p<.01, ***p,.001 (c) coefficients are log-hazards, the hazard ratio can be calculated by taking the exponential of the coefficient.

Table 11: Adult Probationer Cox Regression Predicting Hazard of Recidivism									
Any Arrest After 18 Months									
	Listwise deletion			Mean Substitution			Multiple Imputation		
Variable	Coefficient	SE		Coefficient	SE		Coefficient	SE	
Treatment	-0.09	0.27		-0.25	0.24		-0.24	0.24	
Male	0.30	0.61		0.52	0.60		0.51	0.60	
Black	0.33	0.75		0.76	0.74		0.72	0.74	
Hispanic	0.61	0.82		0.85	0.81		0.82	0.81	
Age	-0.05	0.11		-0.07	0.10		-0.06	0.10	
In drug/alcohol treatment	0.10	0.36		0.08	0.34		0.07	0.34	
Previously shot	-0.64	0.42		-0.60	0.38		-0.60	0.40	
Most serious prior: violent crime	0.00	0.37		0.14	0.37		0.14	0.37	
Most serious prior: drug crime	-0.17	0.38		0.15	0.37		0.13	0.37	
Have child	0.01	0.26		0.02	0.25		0.00	0.25	
Marijuana use	0.84	0.33	*	0.65	0.30	*	0.73	0.35	*
Other drug use	0.83	0.40	*	0.71	0.35	*	0.65	0.30	*
Number of prior arrests	0.14	0.14		0.19	0.12		0.17	0.12	
Age at first arrest	-0.14	0.13		-0.08	0.10		-0.10	0.12	
Years previously on probation	-0.23	0.12	*	-0.15	0.08		-0.15	0.10	
Years previously incarcerated	0.18	0.09	t	0.16	0.38	*	0.15	,08	t
Lives in West-NW probation district	0.02	0.41		0.08	0.42		0.10	0.38	
Lives in South Central probation district	0.49	0.45		0.31	0.30		0.33	0.42	
Missing data dummy	N/A	N/A		0.23	2.48		N/A	N/A	
Number of observations included in analysis	312			364			364		
Violent Arrest After 18 Months									
Treatment	-0.07	0.57		-0.45	0.48		-0.44	0.48	
Male	0.49	1.09		0.52	1.06		0.50	1.06	
Black	-0.60	1.16		0.30	1.11		0.33	1.11	
Hispanic	0.86	1.24		1.07	1.21		1.11	1.20	
Age	0.13	0.21		0.18	0.17		0.21	0.18	
In drug/alcohol treatment	-0.89	1.06		-1.03	1.04		-1.04	1.04	
Previously shot	0.49	0.59		0.43	0.53		0.48	0.54	
Most serious prior: violent crime	0.04	0.69		0.49	0.71		0.52	0.70	
Most serious prior: drug crime	-1.39	1.32		-0.48	0.80		-0.39	0.79	
Have child	0.29	0.29		0.21	0.47		0.28	0.49	
Marijuana use	0.70	0.70		0.62	0.55		0.90	0.63	
Other drug use	1.32	1.32	t	0.92	0.62		0.58	0.55	
Number of prior arrests	-0.58	0.36		-0.19	0.26		-0.22	0.26	
Age at first arrest	-0.21	0.24		-0.27	0.22		-0.32	0.22	
Years previously on probation	-0.31	0.24		-0.39	0.22	t	-0.40	0.21	t

Table 11: Adult Probationer Cox Regression Predicting Hazard of Recidivism										
Years previously incarcerated	0.22	0.14		0.20	0.12	t	0.20	0.12		
Lives in West-NW probation district	0.64	0.89		0.30	0.77		0.29	0.76		
Lives in South Central probation district	1.51	0.96		0.69	0.82		0.68	0.82		
Missing data dummy	N/A	N/A		-0.08	0.91		N/A	N/A		
Number of observations included in analysis	312			364			364			
Any Conviction After 18 Months										
Treatment	-0.05	0.29		-0.13	0.26		-0.13	0.26		
Male	0.10	0.10		0.29	0.61		0.27	0.61		
Black	0.21	0.75		0.57	0.74		0.55	0.74		
Hispanic	0.68	0.85		0.79	0.83		0.78	0.83		
Age	-0.05	0.12		-0.07	0.11		-0.06	0.11		
In drug/alcohol treatment	0.02	0.40		-0.11	0.39		-0.12	0.39		
Previously shot	-0.50	0.42		-0.45	0.39		-0.42	0.40		
Most serious prior: violent crime	0.08	0.41		0.21	0.41		0.20	0.41		
Most serious prior: drug crime	-0.08	0.42		0.29	0.41		0.27	0.41		
Have child	-0.09	0.28		-0.10	0.28		-0.13	0.27		
Marijuana use	0.86	0.36	*	0.64	0.33	t	0.71	0.38	t	
Other drug use	0.90	0.42	*	0.70	0.38	t	0.64	0.32	*	
Number of prior arrests	0.22	0.15		0.22	0.13		0.21	0.13		
Age at first arrest	-0.12	0.14		-0.08	0.13		-0.11	0.13		
Years previously on probation	-0.29	0.13	*	-0.21	0.11	t	-0.22	0.11	t	
Years previously incarcerated	0.15	0.09		0.17	0.08	t	0.16	0.08	t	
Lives in West-NW probation district	0.16	0.46		0.12	0.43		0.13	0.43		
Lives in South Central probation district	0.77	0.50		0.56	0.47		0.56	0.47		
Missing data dummy	N/A	N/A		0.07	0.34		N/A	N/A		
Number of observations included in analysis	312			364			364			
Violent Conviction After 18 Months										
Treatment	0.07	0.59		-0.33	0.48		-0.33	0.48		
Male	0.37	1.10		0.42	1.06		0.39	1.07		
Black	-0.72	1.15		0.26	1.12		0.26	1.12		
Hispanic	0.81	1.25		1.04	1.22		1.06	1.21		
Age	0.07	0.21		0.13	0.18		0.17	0.18		
In drug/alcohol treatment	-0.90	1.08		-1.02	1.04		-1.03	1.04		
Previously shot	0.56	0.59		0.51	0.52		0.55	0.53		
Most serious prior: violent crime	-0.02	0.74		0.44	0.72		0.46	0.70		
Most serious prior: drug crime	-1.42	0.89		-0.45	0.81		-0.37	0.78		
Have child	0.11	0.52		0.03	0.50		0.12	0.52		
Marijuana use	0.63	0.62		0.54	0.55		0.96	0.63		
Other drug use	1.38	0.72	t	0.97	0.63		0.50	0.55		
Number of prior arrests	-0.53	0.36		-0.15	0.26		-0.19	0.26		

Table 11: Adult Probationer Cox Regression Predicting Hazard of Recidivism												
Age at first arrest	-0.21	0.25		-0.27	0.22		-0.32	0.22	
Years previously on probation	-0.27	0.24		-0.35	0.21	t	-0.36	0.21	t
Years previously incarcerated	0.21	0.14		0.19	0.12		0.18	0.12	
Lives in West-NW probation district	0.67	0.93		0.28	0.79		0.27	0.79	
Lives in South Central probation district	1.70	1.00	t	0.78	0.84		0.78	0.84	
Missing data dummy	N/A	N/A		-0.02	0.60		N/A	N/A	
Number of observations included in analysis	312			364			364		

Note: (a) The multiple imputation regression estimates are based on the average of 5 imputations, with SE adjustments. (b) t p<.10, *p,.05, **p<.01, ***p,.001 (c) coefficients are log-hazards, the hazard ratio can be calculated by taking the exponential of the coefficient.

Not surprisingly, many of the same covariates that predicted the incidence of recidivism, age and gender among juveniles and marijuana/drug use and years on probation/in incarceration—also were associated with the hazard of recidivism. These analyses also reveal that the treatment effect of YVRP on the hazard of recidivism was significant in one model only: logistic regression predicting any arrest over the 18 month follow up period where multiple imputation was used in the juvenile probation sample. Specifically, participating in YVRP increases the hazard of rearrest for any crime by 43 percent. The model using the mean substitution protocol was also approaching significance. In both cases, the direction of the treatment effect suggests that participation in YVRP is associated with a greater hazard of recidivism than the comparison group. Comparing the various missing data protocols shows that in all cases the multiple imputation missing data protocol produced the largest estimates of the treatment effect.

In the adult probation sample, there was no significant effect of treatment on the hazard of recidivism; however, in all cases, the direction of the estimate was in favor of the treatment group. Marijuana use and illegal drug use was associated with hazard of recidivism for arrest or conviction for any crime, but not violent crimes. Those who used marijuana and other illegal drugs had a higher hazard than those who did not use any

illegal drugs. In many cases, years incarcerated was significantly associated with the hazard of recidivism among adult probationers; those with fewer years of imprisonment experienced a lower hazard rate. In terms of the missing data protocol, listwise deletion produced the smallest estimates of the treatment effect.

Tables 20 and 21 display the results of the models run using propensity score as a summary measure of covariates. None of the models resulted in a statistically significant treatment effect.

Table 12: Estimate of the Treatment Effect Using Propensity Score as a Covariate in a Cox Regression Among Juvenile Probationers: YVRP Participation and Hazard of Recidivism

	Listwise deletion		Mean Substitution		Multiple Imputation	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Any Arrest After 18 Months						
Propensity Score	0.15	0.37	0.15	0.35	0.11	0.35
Treatment	0.2	0.19	0.26	0.18	0.28	0.18
Number of observations included in analysis	320		364		364	
Violent Arrest After 18 Months						
Propensity Score	0.16	0.43	0.13	0.21	-0.05	0.42
Treatment	0.09	0.23	-0.01	0.42	0.14	0.22
Number of observations included in analysis	320		364		364	
Any Conviction After 18 Months						
Propensity Score	0.27	0.41	0.3	0.39	0.27	0.39
Treatment	0.19	0.22	0.24	0.2	0.25	0.2
Number of observations included in analysis	320		364		364	
Violent Conviction After 18 Months						
Propensity Score	0.35	0.49	0.19	0.48	0.16	0.48
Treatment	0.04	0.26	0.09	0.25	0.10	0.24
Number of observations included in analysis	320		364		364	

Note: (a) No propensity score or treatment dummy coefficients are significant at $p<.05$ (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.

Table 13: Estimate of the Treatment Effect Using Propensity Score as a Covariate in a Cox Regression Among Adult Probationers: YVRP Participation and Hazard of Recidivism						
Any Arrest After 18 Months						
	Listwise deletion		Mean Substitution		Multiple Imputation	
Variable	Coefficient	SE	Coefficient	SE	Coefficient	SE
Propensity Score	0.5	0.53	0.39	0.5	0.43	0.5
Treatment	-0.1	0.28	-0.23	0.25	-0.24	0.25
Number of observations included in analysis	312		364		364	
Violent Arrest After 18 Months						
Propensity Score	0.33	0.96	0.58	0.91	0.57	0.92
Treatment	-0.09	0.51	-0.49	0.46	-0.48	0.46
Number of observations included in analysis	312		364		364	
Any Conviction After 18 Months						
Propensity Score	0.56	0.57	0.48	0.54	0.55	0.55
Treatment	-0.05	0.3	-0.14	0.27	-0.15	0.27
Number of observations included in analysis	312		364		364	
Violent Conviction After 18 Months						
Propensity Score	0.27	0.99	0.51	0.93	0.52	0.94
Treatment	0.04	0.52	-0.39	0.46	-0.39	0.47
Number of observations included in analysis	312		364		364	
Note: (a) No propensity score or treatment dummy coefficients are significant at p<.05 (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.						

Comparison of approaches. Table 22 is a summary table comparing the treatment effect coefficients derived from the series of cox regressions presented in the previous section. Similar to the effect of treatment on the incidence of recidivism, Table 22 demonstrates that the coefficients estimated using the three missing data protocols in the adult probation sample are more similar than those among the juvenile probationers. As noted above, this suggests that the data missing introduced more instability in the models among juvenile probationers than among adult probationers.

Table 22 Comparison of Treatment Effect Estimates From Logistic Regression Using Individual Covariates Versus Propensity Score						
	Listwise Deletion		Mean Substitution		Multiple Imputation	
	Individual Covariates	Propensity Score	Individual Covariates	Propensity Score	Individual Covariates	Propensity Score
Juvenile Probationers						
Any Arrest Over 18 Months	0.31	0.20	0.36	0.26	0.36*	0.28
Violent Arrest Over 18 Months	0.13	0.09	0.16	-0.01	0.17	0.14
Any Conviction Over 18 Months	0.28	0.19	0.32	0.24	0.32	0.25
Violent Conviction Over 18 Months	0.08	0.04	0.12	0.09	0.13	0.10
Adult Probationers						
Any Arrest Over 18 Months	-0.09	-0.10	-0.25	-0.23	-0.24	-0.24
Violent Arrest Over 18 Months	-0.07	-0.09	-0.45	-0.49	-0.44	-0.48
Any Conviction Over 18 Months	-0.05	-0.05	-0.13	-0.14	-0.13	-0.15
Violent Conviction Over 18 Months	0.07	0.04	-0.33	-0.39	-0.33	-0.39
Note: (a) Estimates are log hazards (b) t p<.10, *p<.05, **p<.01, ***p,.001 (b) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient.						

How is the Frequency of Contact with Street Workers Related to Recidivism Outcomes?

Many program evaluations focus solely on the treatment effect, and do not explore factors associated with implementation that may be related to participant outcomes. Luckily, YVRP, with external support, has consistently collected high quality data on program inputs. Taking advantage of these data, it is possible to explore the extent to which contacts with line staff are associated with recidivism over the follow up period.

Specifically, the models presented below are designed to explore the additive effect of contacts with street workers with respect to recidivism among juvenile and adult probationers. Data from comparison group members is included in the analysis, as is the average monthly total (face to face and other) contact with probation officer. The propensity score is included as a summative score of confounding covariates.

Figures 8 and 9 display the distribution of contacts with probation for treatment and comparison group members. There is a clear difference in the distributions and the

average number of monthly contacts; YVRP participants had much more contact with probation than comparison group members. This is not surprising since YVRP is defined by higher levels of probation contact than "traditional" probation. Furthermore, the average juvenile probationer who participated in YVRP had more contacts with probation than adult probation YVRP participants. This is likely due to the fact that adult YVRP probation officers have significantly larger caseloads than those supervising YVRP caseloads in juvenile probation. More surprising is the fact that there is not a remarkable difference between the average monthly probation contacts between comparison group members in the adult versus juvenile samples.

Figure 8: Distribution of Probation Contacts Among Treatment and Comparison Group Members Juvenile Probationers

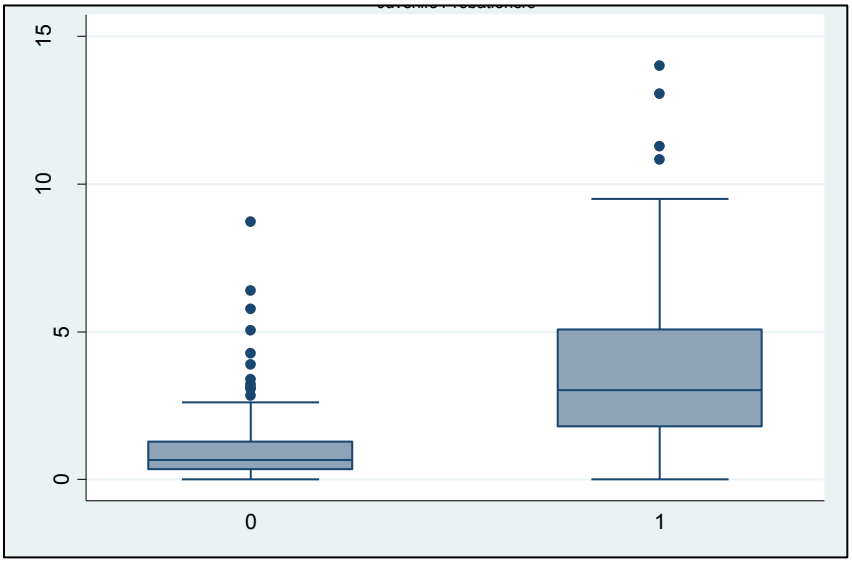
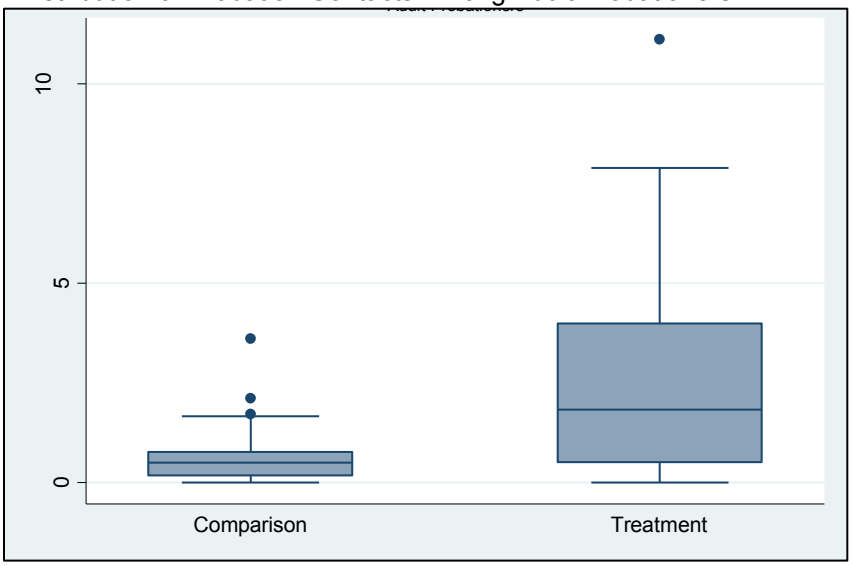


Figure 9: Distribution of Probation Contacts Among Adult Probationers



Figures 10 and 11 display the distribution of street worker contacts among YVRP participants (comparison group members do not have street workers and therefore do not have street worker contact). Street worker contacts are the average monthly contact (of any variety) for the months that the participant was active in YVRP.

Figure 10: Distribution of Street Worker Contacts Among Treatment Group Members Juvenile Probationers

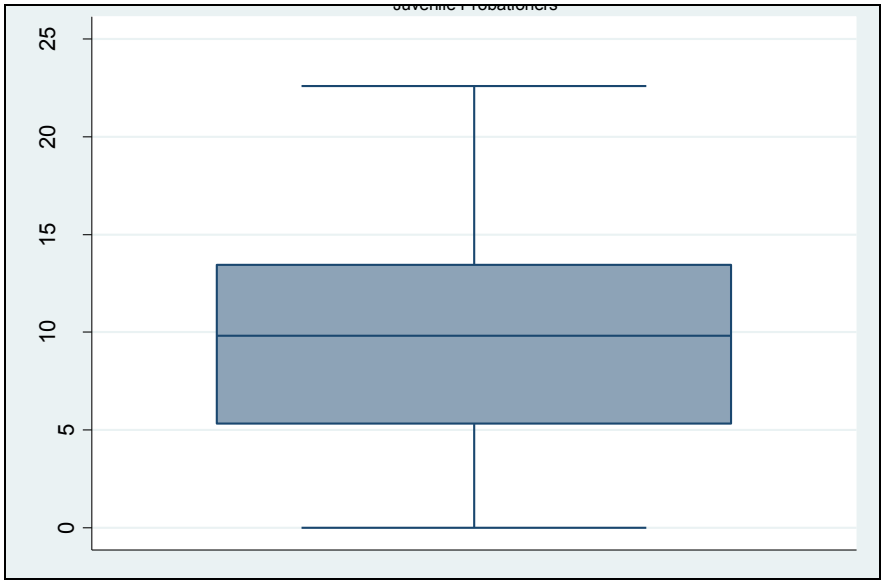
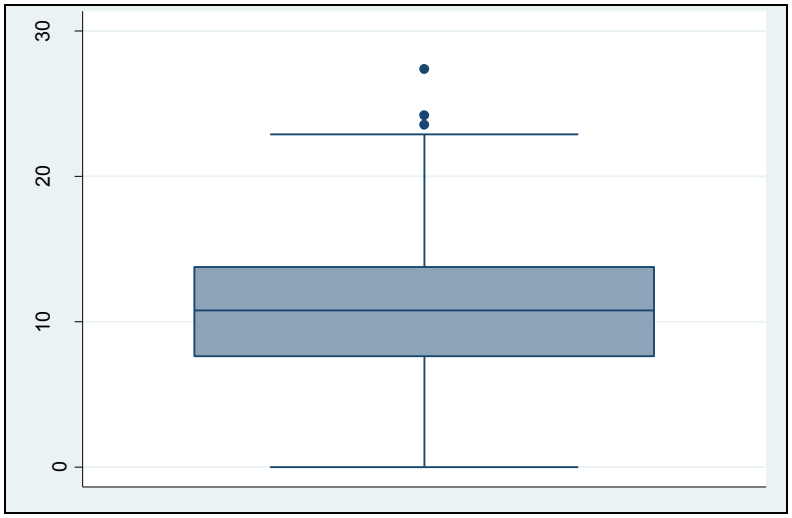


Figure 11: Distribution of Street Worker Contacts Among Treatment Group Members Adult Probationers



What is most notable about street worker contacts is simply the sheer volume—in any given month, YVRP participants in this study had, on average, approximately 10 contacts with their street worker.

Tables 23 and 24 display the results of logistic regressions assessing the additive effect of street worker contacts on recidivism outcomes[22]. Among juvenile probationers, contacts with street workers offered no statistically significant benefit over probation contacts alone. However, street worker contact was related to lower levels of recidivism in the adult probation sample. Specifically, adult probationers with more street worker contact were less likely to be arrested for or convicted of a new crime over the 18 month follow up period. One additional monthly contact with a street worker was associated with a 9 percent or 8 percent (depending on the model) reduction in the odds of experiencing a new arrest or conviction over the follow up period.[23] However, due to the nature of the analysis, the direction of the association is unclear. For instance, it is

[22] While the association of probation contacts with recidivism was not a focus on this study, the models reported in Tables 23 and 24 suggest that probation contact was not associated with recidivism among juvenile probationers. Among adult probationers, probation contact was approaching significance as a predictor of recidivism in one model only: any arrest over 18 months using mean substitution as a missing data protocol. In this case, more contact with probation was associated with a greater likelihood of rearrest.

[23] If comparison group members are excluded from the analysis, the associations between street worker contacts and the various measures of recidivism become larger.

possible that more street worker contact results in reduced recidivism *or* it is possible that those YVRP probationers who are least likely to recidivate are easier to work with and therefore are open to and receive more street worker contact as a result.

Table 14: Association between Probation and Street Worker Contacts and Recidivism: Juvenile Probationers

	Listwise Deletion		Mean Substitution		Multiple Imputation	
Any Arrest Over 18 Months	Coefficient	SE	Coefficient	SE	Coefficient	SE
Propensity Score	0.33	0.47	0.39	0.44	0.34	0.45
Average Monthly Street Worker Contact	0.00	0.03	-0.01	0.02	-0.01	0.02
Average Monthly Probation Officer Contact over 18 Months	0.05	0.06	0.08	0.06	0.08	0.06
Constant	-0.370	0.22	-0.47	0.21	-0.45	0.21
Number of Observations	320		364		364	
Violent Arrest Over 18 Months						
Propensity Score	0.23	0.49	0.07	0.47	0.02	0.47
Average Monthly Street Worker Contact	-0.01	0.03	-0.02	0.03	-0.02	0.03
Average Monthly Probation Officer Contact over 18 Months	0.05	0.07	0.10	0.06	0.10	0.06
Constant	-0.84	0.23	-0.86	0.22	-0.84	0.22
Number of Observations	320		364		364	
Any Conviction Over 18 Months						
Propensity Score	0.41	0.48	0.45	0.46	0.43	0.46
Average Monthly Street Worker Contact	0.01	0.03	0.01	0.03	0.01	0.03
Average Monthly Probation Officer Contact over 18 Months	0.01	0.06	0.04	0.06	0.05	0.06
Constant	-0.76	0.23	-0.83	0.22	-0.82	0.22
Number of Observations	320		364		364	
Any Violent Conviction Over 18 Months						
Propensity Score	0.41	0.53	0.21	0.51	0.18	0.51
Average Monthly Street Worker Contact	0.00	0.03	-0.01	0.03	-0.01	0.03
Average Monthly Probation Officer Contact over 18 Months	0.01	0.07	0.06	0.06	0.06	0.06
Constant	-1.24	0.26	-1.26	0.24	-1.25	0.24
Number of Observations	320		364		364	

Note: (a) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient (b) no covariate coefficients are significant at *p<.05 or better

Table 15: Association between Probation and Street Worker Contacts and Recidivism: Adult Probationers

	Listwise Deletion			Mean Substitution			Multiple Imputation		
Any Arrest Over 18 Months	Coefficient	SE		Coefficient	SE		Coefficient	SE	
Propensity Score	1.23	0.57	*	0.83	0.54		-1.38	0.31	
Average Monthly Street Worker Contact	-0.09	0.03	**	-0.09	0.03	**	-0.08	0.03	**
Average Monthly Probation Officer Contact over 18 Months	0.11	0.10		0.15	0.09	t	0.14	0.09	
Constant	-1.53	0.33		-1.35	0.30		-1.38	0.31	
Number of Observations	312			364			364		
Violent Arrest Over 18 Months									
Propensity Score	1.04	0.94		1.06	0.90		1.06	0.91	
Average Monthly Street Worker Contact	-0.05	0.05		-0.06	0.05		-0.08	0.05	
Average Monthly Probation Officer Contact over 18 Months	-0.21	0.22		-0.19	0.21		-0.18	0.21	
Constant	-2.63	0.55		-2.57	0.52		-2.59	0.52	
Number of Observations	312			364			364		
Any Conviction Over 18 Months									
Propensity Score	1.42	0.60	*	1.05	0.57	t	1.10	0.58	
Average Monthly Street Worker Contact	-0.10	0.04	**	-0.09	0.03	**	-0.09	0.03	**
Average Monthly Probation Officer Contact over 18 Months	0.12	0.11		0.15	0.10		0.14	0.10	
Constant	-1.78	0.35		-1.67	0.33		-1.70	0.33	
Number of Observations	312			364			364		
Any Violent Conviction Over 18 Months									
Propensity Score	1.05	0.96		1.04	0.92		1.07	0.93	
Average Monthly Street Worker Contact	-0.05	0.06		-0.08	0.05		-0.08	0.05	
Average Monthly Probation Officer Contact over 18 Months	-0.18	0.21		-0.17	0.21		-0.16	0.20	
Constant	-2.73	0.57		-2.64	0.53		-2.66	0.53	
Number of Observations	312			364			364		

Note: (a) coefficients are log-odds, the odds ratio can be calculated by taking the exponential of the coefficient (b) t p<.10, *p<.05, **p<.01, ***p,.001

The different missing data protocols did not produce large differences in the estimate of the association between street worker contact and recidivism. However, they did produce (sometimes wildly) different estimates of the association between the propensity score and recidivism. While this estimate is not interpretable, it suggests the missing data protocol introduces some instability in the models.

CHAPTER 7: DISCUSSION

This study examines an intervention, YVRP that aims to break the cycle of persistent criminal offending by reducing recidivism, particularly for violent or potentially violent offenders, through enhancing probation. The program targets serious and persistent youthful (between the ages of 14 and 24) offenders who live in high crime urban neighborhoods who are on probation. It strives to bridge a critical tension faced by probation--the dueling goals of social control and social welfare (i.e., punishment and rehabilitation)--by providing participants with both intensive supervision from probation and police/probation patrols and support/connection to services from a street worker. Previous research has demonstrated that the implementation of YVRP is associated with a reduction in violence in targeted police districts. This study was designed to assess if YVRP is associated with a reduction in recidivism at the individual level. Specifically, the goals of the study were to answer the following two questions 1) Does participation in YVRP *cause* decreases in recidivism among participants, and 2) do contacts with program staff, specifically, street workers, result in a reduced likelihood of recidivism.

Furthermore, because the evaluation of this program is “high stakes” insomuch as it has implications for program participants, staff, and funding allocations, several analytical approaches were utilized. The approaches to answer the first question--does YVRP reduce recidivism--are summarized in Table 25.

Table 16: Summary of Analytic Approaches Used in This Study

	Logistic or Cox Regression with Individual Covariates			Logistic or Cox Regression with Propensity Score Covariate Adjustment			Stratification method (average treatment effect on the treated)		
	LD	MS	MI	LD	MS	MI	LD	MS	MI
Incidence of new arrest	✓	✓	✓	✓	✓	✓	✓	✓	✓
Incidence of new arrest for a violent crime	✓	✓	✓	✓	✓	✓	✓	✓	✓
Incidence of new conviction	✓	✓	✓	✓	✓	✓	✓	✓	✓
Incidence of a new conviction for a violent crime	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time to new arrest	✓	✓	✓	✓	✓	✓			
Time to new arrest for a violent crime	✓	✓	✓	✓	✓	✓			
Time to new conviction	✓	✓	✓	✓	✓	✓			
Time to new conviction for a violent crime	✓	✓	✓	✓	✓	✓			

The second research question, pertaining to the association between street worker contacts and recidivism was answered less rigorously. Associations were explored using logistic regression with covariate adjustment by the propensity score used in question one. However, the models were run with all three missing data protocols.

Overall, the analyses revealed that there was no consistent treatment effect; that is, participating in YVRP was not consistently associated with lower or delayed recidivism. While one model yielded a statistically significant effect, and two were marginal, it is more likely due to chance (given the number of models run) than a meaningful finding. What the analyses did reveal, however, is that street worker contact

had an additive effect on recidivism reduction among adult probationers. Specifically, adults with greater contact with street workers had significantly lower odds of recidivating with a new crime or conviction over the 18 month follow up period than those with less contact. One additional monthly contact with a street worker was associated with a .91 to .92 (depending on the model) decrease in the odds of experiencing a new arrest or conviction over the follow up period. However, these effects may be endogenous; it is unclear if more street worker contact reduces the risk of recidivism or if those participants who are more likely to desist are easier to work with and therefore receive more street worker contact.

Limitations

This study has several limitations which have implications for the interpretation of the findings. First, and most notably, this study is observational in nature. Ideally, a random assignment study would have been conducted (with a larger sample size—see sample size limitation discussed below) in order to assess the impact of YVRP on recidivism.

While a well-implemented randomized control trial (RCT) is the "gold standard" when attempting to assess program treatment effects, many evaluations, such as this one, must or chose to rely on less rigorous designs. YVRP leaders were interested in assessing their impact on participants; however, for several reasons, an RCT was not conducted. Although YVRP leaders acknowledged that YVRP may not "work," they were not willing to withhold YVRP for any young people who were appropriate for the program and living in police districts targeted by YVRP, which would be necessary for an RCT where individuals are randomly assigned to the treatment or control group, nor were they willing or able to relinquish control over which police districts were selected to implement YVRP, a requirement of a cluster RCT.

According to Boruch and Riecken (1975) there are many misconceptions about RCTs. Service provision programs often opt out of RCTs despite their strengths for non-methodological (e.g., political or ideological) reasons. Common arguments against an RCT from the programmatic standpoint include:

1) "It's unfair." Many program staff believe that choosing who receives treatment at random is unfair, and therefore, they wish to maintain control over the process to assure that only the most eligible and/or needy individuals are the beneficiaries of program resources. Furthermore, randomization challenges the program's ability to respond to the requests of community or school leaders and also does not leave room for program staff to intake "special cases," such as people related to staff, funders, or other influential individuals.

2) "It's not ethical." Many programs believe that randomly choosing individuals to receive program services breeds vulnerability—insomuch as those assigned to the control group are not benefitting from program services. Program staff frequently believe strongly in the effectiveness of their work; therefore, they do not want to deny services to anyone who could potentially benefit from them.

3) "It costs too much." Program and organizations believe that an RCT is more expensive to conduct than a quasi-experiment. In some cases, this is true. For instance, the community based nature of YVRP (where probation officers and street workers are out in YVRP neighborhoods, checking hot spots and visiting youth partner's homes and homes of their friends and neighbors, which could contaminate the control group, hence violating SUTVA) would require a cluster RCT, whereby police districts are randomly selected for YVRP implementation (among high crime police districts). It may be that the number of police districts and participants required to achieve adequate

power to detect meaningful impacts was larger than the number of districts in which YVRP had funding to operate.

RCTs *are* possible under many circumstances, and each of these programmatic arguments against RCT has a compelling counter-argument. First, RCTs should only be undertaken when there is oversubscription to a program (or a waiting list) or a surplus of "clusters" for which the program is a fit. Without this surplus, implementing an RCT can result in a program not filling all available slots, which can lead to funding, staffing, and other programmatic challenges. Assuming oversubscription is true, a well implemented RCT results in less bias in the selection of participants or clusters than the typical first-come/first-served model or the "ready, willing and able" model, as participants or implementing entities are selected at random from a pool of those that are "qualified." Second, the argument that RCT's are not ethical rests on the assumption that the program works, which is exactly the question that the RCT seeks to answer. It is important that program staff believe that their services work—it is likely they would provide sub-standard service otherwise. Therefore, it is important to reinforce the importance of the RCT as a method for quantifying the value of the program to either communicate that broadly to support service provision or, in the event the program is deemed ineffective, to redirect funds to another approach that does work. Finally, the argument that it costs too much is not always true—the cost of an RCT or a quasi-experiment varies depending on the design. In fact, depending on the composition of the comparison group, a quasi-experiment could be more expensive than an RCT.

On the other hand, there are several other strong arguments against RCTs. First, RCTs do not guarantee generalizability. If an RCT reveals a positive program effect in a few sites, with a few staff, or in a few areas, there is no guarantee that the program would work in a different context. Second, in cases where the "requirements" of an RCT

cannot be met (for instance, no oversubscription at the individual or cluster level) or the design will violate an assumption of causal inference, an RCT should not be undertaken, either because the findings will not be valid or because the program will suffer irreversible challenges (such as funding). Third, if an RCT cannot be implemented with fidelity (e.g., there are ways for staff to "game" the assignment mechanism, etc.), the researcher may want to implement an observational or quasi-experimental study. Finally, RCTs should only be implemented when a program is running as intended, as a test of the true program model, and not when there are program implementation challenges.

Given the high-stakes of YVRP, YVRP leaders did not want to risk conducting an RCT—they believed that even if the key stakeholders understood the underpinnings of the design, the public at-large would not. Given the fact that an RCT where individuals are randomly assigned to either the treatment or control conditions would have violated SUVTA in the case of YVRP and there were insufficient resources for a group-level RCT (a point I will return to below, in the policy implications section of this paper), the choice of a quasi-experimental study may have been the right one. However, there are additional designs that could have been utilized to estimate the causal impacts of YVRP on its participants.

First, when data for this study of YVRP was collected, YVRP leaders were using subjective review of programmatic nominations to identify probationers to assign to the program. However, in more recent years, YVRP in adult probation has adopted a more strategic approach to identifying young adult probationers living in YVRP police districts. This new risk assessment allows probation and YVRP leaders to select only those probationers deemed "at high risk" of engaging in a violent crime for YVRP. This approach could be used with a regression discontinuity design, whereby the outcomes of

those probationers just under the “high risk” cut off are compared to those just over the “high risk” cut off. A regression discontinuity design that meets the assumptions indicated by the approach provides estimates very similar to that of an RCT. However, statistical power (the power to detect that meaningful differences are not due to chance) is weaker than in a similar sized RCT, which could provide a challenge for YVRP. Another option is a variant on the RCT, the stepped wedge design, which would sequentially rolls out YVRP to different police districts over time randomly. One of the particular strengths of this design for YVRP is that it is particularly appropriate for community-based interventions, as it addresses carry over effects and it provides more power over similarly sized cluster evaluations. One of the central drawbacks, however, is that YVRP only expands as additional funding for expansion becomes available.

A second limitation of the study is the small sample size. The limited sample, both of treatments and comparison group members posed challenges for the generation of propensity scores *and* for the estimation of treatment effects. Propensity score modeling works best with large samples, particularly when the two groups—treatments and comparisons—are fairly similar on confounding variables. If both are true, and the appropriate covariates are measured well, propensity scores can be a robust tool for estimating treatment effects, using a number of analytical approaches. In this study, the small sample resulted in imperfect balancing, even on observed covariates, as displayed in Tables 6 and 7. The small sample also has implications for detecting a treatment effect. While some will argue that statistical significance is not the holy grail of *meaningfulness* (in other words, some believe that the effect size is as, if not more, important than statistical significance), small sample sizes come with limited power to detect statistically significant differences. This is a point I will return to later in this discussion.

Applying the potential outcomes framework to observational studies using propensity score methods requires that the researcher have access to accurate measures of all confounding variables—those associated with treatment selection mechanism AND potential outcomes of interest. The data available for this study was, arguably, missing key pieces of data that would have bolstered the propensity score's balancing property. For instance, in some cases a YVRP participant was nominated and admitted into the program despite not having a serious or prolific criminal history[24], if s/he had a sibling that was heavily involved in the drug or gun trade and/or violent crime or if s/he lived on a particularly notoriously violent block of the targeted police district. Clearly, this is data that, if collected systematically and included in the propensity score model, would have resulted in enhanced balancing between the treatment and comparison groups and a more accurate estimate of the treatment effect. Other less obvious measures would also have made the analyses more robust--including family background and connections to school and work[25]—all of which are likely to influence outcomes and treatment status.

Notably, the comparison and treatment groups in this study, despite attempting to identify comparison group members with similar profiles to participants, were different. Adult probationers in YVRP, based on measured covariates, appear to have a lower risk for recidivism than adult probationers in the comparison group. Among juvenile probationers, while the treatment and comparison groups were dissimilar on many critical factors, it does not appear that one group is more at risk than the other. However, the general lack of equivalence results in a smaller overlap between the two groups. To partially address this challenge, STATA's common support option was employed, which excludes treatment cases with estimated propensity scores higher than

[24] There was no systematic entry criteria for YVRP; however, persistent offending with violent offenses, although loosely defined, was a primary determinate of program entry.
[25] Interestingly, motivation is not a factor in the assignment mechanism because YVRP participants were mandated to participate. So while motivation is likely related to outcomes, and perhaps in-program experiences, it is not a factor associated with program selection.

the highest score among comparison group members, and those of comparison group members which fall below the lowest score among the treated. This further reduces the sample size and also limits generalizability—the estimation of treatment effects is limited to a special subset those treatment group members who are most similar on measured covariates to the comparison group members who are also in the region of common support. This approach, while assuring that only similar treatment and comparison group members are compared, also results in the exclusion of treatment group members who are statistically the most "ideal" YVRP participants, thus limiting the ability to detect the extent to which YVRP is associated with lower recidivism among individuals who best fit the profile of YVRP participants.

Finally, missing data was a challenge for this study. In the juvenile probation sample 12 percent of cases were missing at least one data point; fourteen percent among adult probationers. This study utilized three different protocols for handling missing data, all of which require that the data are missing at random, which is a hard assumption to meet in practice. The first approach, listwise deletion, deletes each case in which there is a missing data point. Assuming the data are missing at random listwise deletion will produce an accurate estimate; however the associated loss in power, especially when the sample size is already small, has significant implications for the interpretation of the data. The second approach used is mean substitution. Essentially, this approach replaces a missing data point with the mean of the group. Mean substitution, as an approach to missing data, is no longer in favor. This is primarily because mean substitution results in smaller standard errors, while not adding any new information to the model. The final approach used in this study is multiple imputation. Generally, using this approach one creates multiple datasets imputing estimates of missing values. Analyses are run on each dataset, the average of the estimates are used and a

correction is made to the standard error. In the end, overall, the estimates produced under each of these protocols produced relatively similar estimates of the treatment effect; however, there still remain notable differences. Furthermore, it is unlikely that the data in this study are missing completely at random, and the fact that the estimates generated by listwise deletion and mean substitution are not identical.

While these limitations are perhaps the most notable, they are surely not the only weaknesses of the study. Others, such as data reliability and measurement error, are challenges that face most researchers, and are worthy of mention, even if not of deep exploration.

Policy Implications

One goal of this study is to inform the thinking of policy makers striving to reduce violent crime. Specifically, previous research suggests that intensive probation alone does not prevent recidivism. Despite the intuitive appeal of an approach that combines intensive probation supervision with support, like YVRP, the evidence presented herein suggests that it does not reduce recidivism among serious and persistent young offenders living in high violence neighborhoods. However, these findings are not definitive, as several significant study limitations may have influenced the findings (see discussion of limitations above). Furthermore, previous research has demonstrated that the combination of intensive probation and support in YVRP is associated with reductions in violence at the community level.

This study does, however, provide some evidence to suggest that intensive probation may be bolstered by frequent contract between a probationer and a street worker, who, in YVRP, works in concert with the probation officer to help connect participants with needed services and provide them with other supports (e.g., emotional encouragement, healthy recreational opportunities, employment assistance, etc.).

Specifically, adult probationers who had more contact with their street worker, holding other factors, including probation contact, constant, had lower recidivism than those with less street worker contact. However, this association is correlational, and therefore, does not prove that more contact with a street worker prevents recidivism. In fact, it is possible that YVRP participants who had more street worker contact were those who were already more likely to experience positive outcomes and most willing to work with the street worker to help them avoid recidivism. Interestingly, street worker contacts were not associated with recidivism among YVRP participants on juvenile probation. This may be due to the fact that juvenile probation emphasizes both supervision and rehabilitation, and therefore, juvenile probation officers may be more skilled at providing rehabilitative supports to their probationers, and street worker support may be redundant. Nonetheless, this finding, suggests the need for further exploration of the value of supports provided by a non-justice partner (e.g., street workers) in supporting reduced recidivism among serious and persistent young offenders on intensive adult probation.

Although not an explicit goal of this study, this research has implications for how policy makers think about the use of evidence in their decision making. This evaluation of YVRP utilized a quasi-experimental design, due to the fact the study was observational. And while this study did not reveal a positive impact of YVRP on individual level recidivism, it is important to keep in mind that this study is far from a perfect test of the efficacy of the model. Limitations, many of which may be impossible to overcome, such as identifying comparison group members who are more like YVRP participants and increasing sample size, limit our ability to say definitively if the program works or does not work. Many programs face similar challenges in evaluating their impacts; in the case of YVRP, and other programs, it may be impossible to generate

and/or implement an evaluation design that can determine definitively if a program works. Policy makers considering whether or not to fund or continue to fund a program must understand the nuances associated with program evaluation—and adopt a broad view of a program's value.

Secondly, as mentioned above, this study has multiple limitations and could have been stronger if the conditions and will had existed for a RCT. Either of both of these conditions could be remedied by increased funding for research and evaluation in the criminal justice sector. YVRP was subject to limited programmatic and research funding, arguably impacting the ability to conduct a RCT (stepped wedge or cluster). In fact, almost all funding for YVRP's over 10 years of evaluation were supported by private funders. As demonstrated by Laurie Robinson during her tenure at The Office of Justice Programs, dedicating funds to research and evaluation led to the generation and use of a myriad of information that pushed criminal justice efforts to be more effective. Furthermore, her leadership laid the groundwork for implementing a research-informed approach to funding and implementation—a tactic which had meaningful implications for crime reduction strategies (Robinson, 2014). Policy makers are now, more than ever being held accountable for funding evidence-based strategies, but we, as a field, are far from identifying optimal solutions to crime. Without adequate funding for research and evaluation efforts, innovations will be stymied and evaluations will face limitations similar to those faced in this study, resulting in inconclusive findings that are not up to the standards of current evidence-based approaches.

Programmatic Implications

As noted previously, with respect to the first research question posed in this study, the analyses suggested there was no statistically significant effect of YVRP on recidivism. However, among adult probationers, more street worker contact was

associated with a lower likelihood of recidivism. So what does this mean programmatically? Most critically, YVRP needs to undergo additional evaluation with a larger sample size, a more similar comparison group, a broader array of data-- including outcome measures, and over a longer period of time. While this study does not lend support to YVRP as an intervention to reduce recidivism over 18 months, it is entirely possible that the ultimate benefits of YVRP—desisting from crime, particularly violent crime—are benefits that are not realized over 18 months. It is well documented that as young people, particularly men, age, their likelihood of engaging in criminal activities wanes. It may be that YVRP participants do not fully desist from crime until they are approaching their late 20s but that YVRP reduces the severity or frequency of the crimes they commit. Neither of these outcomes were investigated in the current study. Further, it would be prudent to see if interim benchmarks of programmatic success, like securing and maintaining employment or advancing education and/or training, *are* achieved in the shorter term. However, in addition to generating additional outcome measures, testing these hypotheses stringently would require a larger sample size with adequate power to detect meaningful (defined by the program leaders) differences and, perhaps (depending on the outcome of interest) and longer time frame. Furthermore, since this study of YVRP required a quasi-experimental design (for political and financial reasons), the evaluation would benefit from a more inclusive set of covariates that could be included to more accurately estimate the treatment assignment mechanism, including, but not limited to, more detailed geographic information, family background (including criminal involvement among family members), employment and educational trajectories, probation officer or standardized assessments of risk, and others. Ideally, researchers would work together with program leaders to assure that a much more robust set of covariates associated with the propensity to receive treatment and with outcomes, are

identified and accurately measured. Similarly, it is possible that this study produced null results due to the fact that the comparison and treatment groups were not similar enough—the comparison group did not contain individuals who were statistically similar to treatment group members who best fit the profile of the ideal YVRP participant. Future evaluation efforts must include comparison group members who are more similar to YVRP participants in order to test the efficacy of the program for all participants.

Because of the limitations outlined above, the conclusions suggested by this analysis are not definitive. Importantly, the discussion below draws on some of the findings in this study that were not statistically significant; given the small sample size, and the high stakes associated with the program, they warrant further consideration. First, this study's findings suggest that YVRP *may* have a negative impact on recidivism among juvenile probationers. Treatment effects estimated using logistic regression (either with individual covariates or with the propensity score) produced estimates that suggest that juvenile probationers in YVRP experience odds of recidivating with a new arrest about 1.5 times larger than the comparison group.[26] This is not entirely surprising, or bad news, particularly because it seems intuitive that the increased surveillance and contact with juvenile justice afforded by YVRP (both directly and indirectly through the street workers) may increase the likelihood with which these young people are *caught* engaging in illegal activities. On the other hand, it could be that these estimates are not suggestive of a negative program impact but simply equivalent to zero (in fact the estimates generated using the stratification approach, suggests that the average mean difference in recidivism is either in the expected direction or zero). Finally, estimates of the treatment effect for adult probationers were uniformly in the positive direction.

[26] Except where propensity score was used in the model and multiple imputation was employed—that estimate is 1.10 increased odd among treatment group members.

Relatedly, the findings here suggest that there *may* be, if sample size was larger and matching was of higher quality, a positive impact of YVRP on adult probationers. While the estimates vary in size, generally, the treatment effect is the desired direction. The consistency with which the direction of the estimates is in favor of YVRP, in combination with the fact that other findings reported herein suggest that more street worker contact is associated with a lower likelihood of recidivism suggest that it would be valuable to reevaluate the impact of YVRP on recidivism and other outcomes of interest in the future.

Third, this study suggests that more street worker contact is associated with a lower likelihood of recidivism among adult probationers but not among juvenile probationers. It would be valuable to explore, qualitatively, why this may be the case. What is it about street worker contacts that are helping adult probationers stay on track? Why aren't street worker contacts similarly supporting juvenile probationers (e.g., do they have different needs that street workers are not good at meeting, are the street workers working with juvenile probationers of lower quality; is there something about their relationship with juvenile probation officers that inhibits their effectiveness—as compared to adult probation officers and street workers or is it simply that youngsters on juvenile probation have not matured enough to embrace desistence, whereas some adults have)? The lessons gleaned from the answer to each of these questions could be used for program improvement: to direct efforts to bolster street worker effectiveness with the juvenile probation population.

Finally, YVRP leaders should develop a more robust logic model and data collection strategy that mirrors that model. It is entirely possible that YVRP is not designed to reduce recidivism due to the increased surveillance afforded by participation in the program. It may be that the most appropriate long term outcome for YVRP

participants is consistent engagement in the legal workforce, increased levels of education and or training, lower levels of substance abuse or family stability. Furthermore, most serious violence occurs at the spur of the moment—a situation that YVRP seems very unlikely to influence. YVRP may benefit from rethinking its goals and how it strives to achieve those goals. Simply keeping these serious offenders alive until they mature could be a worthy goal. This study was unable to assess the extent to which YVRP participants are less likely to be the victim of a homicide (or commit one), but this would also be a valuable research question to answer in the future.

In summary, while this study does not confirm YVRP's value in reducing recidivism among its participants, it is not a definitive assessment. Nonetheless, YVRP leaders should take the results of this study seriously, invest in additional research, and revisit the program's theory of change and bolster data collection efforts, if possible. Similarly, YVRP funders should not be dissuaded by this study; this study only explored a single outcome—recidivism over 18 months. YVRP may still be a worthy investment, as many other goals have been achieved through the program's work that are outside the scope of this study, such as previous research suggesting that the implementation of the program is associated with reduced violence in YVRP police districts (McClanahan, 2004), but more research must be undertaken to confirm its impact on participants.

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