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A Multilevel Examination of the Effects of Neighborhood Structural Conditions, Program Availability, and Program Attendance on Parole Supervision Outcomes

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Abstract

A Multilevel Examination of the Effects of Neighborhood Structural Conditions,

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by

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Between 2008 and 2016 the parole population in the U.S. increased by 44,000 to 870,500 (Kaeble & Cowhig, 2018) and about one third of prison admissions annually are parole violators (Carson, 2015). Participation in programs that address criminogenic needs leads to fewer re-incarcerations (Andrews & Bonta, 2010). Moreover, neighborhood conditions and the availability of programs across neighborhoods are related to returns to prison (Hipp et al., 2010).

This study extends research on parole outcomes by investigating whether neighborhood conditions related to disadvantage negatively affect the number of treatment programs available, the number of program attendances by parolees, and the occurrence of five parole outcomes. Moreover, this study examines the direct effects of program attendance on parole outcomes and whether program attendance moderates the negative effects of community conditions on parole outcomes. Outcomes were examined individually and two groups – outcomes likely to occur early in the parole episode (e.g. violation, positive drug test, violation arrest) and outcomes likely to occur later in the parole episode (e.g., felony arrest, revocation). Both single level and multilevel models were utilized to test hypotheses. The study sample consisted of 1,637 parolees living in 65 census tracts in five urban areas in Georgia who ended supervision between 2011 and 2013.

Findings indicate that among individual level attributes, only parolee risk score predicted all five parole outcomes and being male predicted three of the five parole outcomes. Program attendance predicted an increased likelihood of early outcomes and a reduced likelihood of late outcomes. The number of programs available in a neighborhood did not predict program attendance. Program attendance moderated the effects of several individual level attributes: younger parolees experienced the beneficial effects of program attendance by decreasing the likelihood of felony arrest. Similarly, the beneficial effects of attendance are strengthened for female and white parolees which decreases their likelihood of revocation. None of the neighborhood conditions (level of disadvantage, mobility, or proportion of black population) significantly predicted program attendance. Multi-level analysis examined only the likelihood of early parole outcomes revealing that as a neighborhood's mobility increased, the likelihood of violation, positive drug test, or violation arrest increased.

A MULTILEVEL EXAMINATION OF THE EFFECTS OF NEIGHBORHOOD STRUCTURAL CONDITIONS, PROGRAM AVAILABILITY, AND PROGRAM ATTENDANCE ON PAROLE SUPERVISION OUTCOMES

BY

JOHN PAUL PREVOST

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Acceptance

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CHAPTER I: INTRODUCTION

Parole is a vital component of the correctional system within the United States. By easing the transition of prison inmates back into communities with programs and other assistance, parole works to reduce the number of offenders who return to prison. Unfortunately, in what many critics see as a sign of the failure of parole, across the U.S. parole violators continue to represent a significant portion of new prison admissions. For example, in 2014,[1] a total of 28% (159,506) of all prison admissions in the U.S. were parolees arrested for a new crime or for violating the conditions of supervision (Carson, 2015). Yet, states are increasingly relying on parole to ease prison populations. Between 2005 and 2015 the total U.S. state parole population increased by 86,100 individuals. Parolees now account for 14% (870,500) of the overall U.S. adult correctional population (Kaeble & Bonczar, 2016; Kaeble & Glaze, 2016). Given the increased numbers of offenders on parole, the high proportion that returns to prison, and the steadily rising cost of incarceration, parole success is a significant concern to corrections' officials.

Parole has come under increasing pressure to improve outcomes. At the center of parole, the conditions of supervision serve as the road map for successful parole completion using both the threat of sanctions calculated to steer parolees away from negative influences and requirements to address proactively issues of daily living. Particularly important for parole success may be access and enrollment in programs to address mental health, drug use problems, and criminal thinking. Research evidence suggests that programs applying learning theories to

[1] 2014 is the most recent year prison admissions for parolees are reported separately from probationers.

address mental health, drug abuse, and criminal thinking reduce the likelihood of both re-offending and returning to prison (Andrews & Bonta, 2010). However, there is a significant portion of the variance in re-offending behavior that remains unexplained.

In summary, parole supervision agencies direct parolees to programs built on learning theories but the hoped-for outcomes have not been achieved. This dissertation investigates whether a significant amount of the continuing parole failure can be explained by the conditions in the communities where parolees reside. These conditions, as described by social disorganization theory, may affect the availability of community programs and parolee violation behavior. Also investigated is whether participation in programs that follow learning theories moderate the negative effects of community conditions. This research contributes to the literature by including measures for the number of programs used by parolees and by their program attendance which has not been used in previous research. The next section explains how neighborhood conditions may be associated with variation in crime.

Neighborhood Context

One element that may account for some of the unexplained variance in research findings related to re-offending is the broader context in which offenders live. Context can be important in many ways including increasing motivation for criminal offending, decreasing protective factors such as informal social control, and providing access to the programs required by conditions of parole. Several macro level theories of crime, especially strain/anomie and social disorganization theories, argue structural conditions in the wider community play a role in offending (Merton, 1938; Shaw & McKay, 1969). Anomie is concerned with conformity to social norms - the institutionalized rules individuals follow to achieve goals. Individuals comply with social norms when equal emphasis is placed on both rule compliance and access to legitimate pathways to achieving goals, in particular, financial success (Merton, 1938). In part, anomie theory argues that for many individuals, legitimate pathways to financial success are blocked. Residents of disadvantaged neighborhoods have less access to education, good jobs, and higher pay. If legitimate means to achieve goals are blocked, illegitimate means are used, such as drug dealing and theft, to meet financial obligations (Merton, 1938).

Social disorganization theory asserts a more complex role of neighborhoods in relation to crime. Neighborhood structural conditions, and the neighborhood processes that arise from such structural conditions, affect residents' likelihood of exerting informal social control over behavior that occurs in their neighborhoods (Shaw & McKay, 1969). Specifically, low socio-economic status, increased racial heterogeneity, and residential mobility hinder the development of social networks that facilitate communication among neighbors. Weak communication hinders reaching agreement on common values and acceptable behavior. Neighborhood agreement on acceptable behavior is associated with a greater likelihood of informal social control over residents and others in the neighborhood (Kornhauser, 1978, p. 63).

Informal social control is an important protective factor for crime. Lower levels of informal social control are associated with neighborhoods that have higher levels of racial heterogeneity and residential mobility, and low socio-economic status. Consequently, such neighborhoods are more likely to experience higher rates of offending and thus, greater exposure to offenders. In terms of examining parolee success, this may mean that these types of neighborhoods will provide a richer environment for criminal learning and support of criminal offending (Cloward & Ohlin, 1960). Social disorganization theory also discusses the importance of social ties to agencies extending to the neighborhood (Bursik, 1988). Socially disorganized neighborhoods may also be less able to marshal the external resources to bring necessary services to the neighborhood and keep out unwanted types of businesses or organizations (Shaw & McKay, 1969, pp. 170-172). Thus, businesses or groups that support or provide conventional opportunities for offenders may not be in sufficient supply to address the problems offenders may encounter following release from prison.

Unfortunately, neighborhoods like those described by social disorganization theory and anomie theory are the types of neighborhoods parolees are most likely to return to (Hipp, Jannetta, Shah, & Turner, 2011; Hipp & Yates, 2009). Some research has shown the importance of neighborhood characteristics for re-offending (Kubrin & Stewart, 2006). In addition to the direct effects of neighborhood characteristics on reoffending, this research seeks a better understanding of the roles of programs and program attendance to directly influence parolee violations and to moderate the negative effects of neighborhood conditions. The next section investigates what is known about offender programs and program participation.

Program Assignment and Relevance

Many residents, including returning parolees, living in socially disorganized neighborhoods do not commit crime. On the other hand, some parolees returning to such neighborhoods who attend programs do re-offend. While the interplay between neighborhood conditions and program attendance is not well understood, more is known about the relationship between program attendance and re-offending. Offenders returning from prison to communities face a range of obstacles to successful parole completion and community reintegration. The Risk-Need-Responsivity (RNR) model of offender rehabilitation specifies that variation in the obstacles to parole completion and re-integration translates into variation in risk to re-offend. Offenders who are low risk to re-offend should receive very little or no programming. In contrast, offenders with drug use/addiction, mental illness, and antisocial thinking significantly raise the risk to re-offend and, therefore, should be targeted for assignment to programs (Bonta & Andrews, 2007; Andrews & Bonta, 2010; Andrews and Bonta, 2010a; Ward, Mesler, & Yates, 2007). High risk offenders should not only attend programs but program length should increase as risk increases.

The RNR model specifies that assessing each offender's risk is essential to identify the highest risk offenders for programming and to uncover the specific factors associated with re-offending. In fact, RNR states that assigning low risk offenders to programs may increase their risk to re-offend. RNR stresses the importance of 'responsivity,' that is, delivering programs using a cognitive-behavioral approach found to be most effective for the learning styles of offenders (Bonta & Andrews, 2007; Andrews & Bonta, 2010; Lowenkamp & Latessa, 2005). Finally, responsivity pays attention to a host of offender-specific concerns related to program participation such as child care, transportation, distance to programs, and days and times of program activities to accommodate offender employment. Distance to programs is of particular interest for parolee success under supervision (Hipp, Petersilia, & Turner, 2010).

Therefore, RNR makes clear that delivering programs to offenders is not enough to reduce re-offending. Rather, program effectiveness hinges on sorting out the parolees who are at highest risk to reoffend and assigning these parolees to programs delivered in a manner most conducive to learning and change. Thus, the RNR model has implications for investigating whether programming can moderate the effects of neighborhood conditions on offender outcomes. Failing to account for the appropriateness of program assignment could lead to misattributing parole failure to neighborhood structural conditions when, in fact, programs were inappropriately assigned.

Program Attendance, Responsivity, and Disorganized Communities

Even when programs are appropriately assigned, parolees too often do not attend at all or do not attend at the level of intensity called for based on their level of risk. The responsivity component of RNR and social disorganization theory both suggest related explanations for failure to attend programs. Responsivity calls for addressing the practical needs of offenders to reduce legitimate reasons for non-compliance. One potential reason for non-compliance may be the distance to treatment. Limited funds and transportation often make traveling any distance to a program a hardship. Responsivity suggests the distance to programs is a relevant factor for program assignment. Likewise, social disorganization theory suggests that neighborhoods experiencing high levels of residential mobility, racial heterogeneity, and poverty are likely to have weak, ineffective, or inadequate institutional resources (Kornhauser, 1978, pp. 78-82). Institutions may lack sufficient funds to meet community needs and residential mobility may weaken the ability of neighbors to build momentum in organizations. Moreover, such communities may lack the stature to command the attention of officials to generate services or the correct services. The result is institutional structures unable to meet community needs (Kornhauser, 1978, pp. 78-79). Therefore, one concern of responsivity, namely, access to programs, may be weakened due to a lack of programs near where clusters of offenders reside.

Outcome evaluations that do not account for the availability of programs may be mis-attributing the effect of this factor to some other variable, such as parolee resistance to attending programs, or assigning its effects to unexplained variance in outcomes. Similar to the argument made by the responsivity component of RNR, it is possible that the distance offenders must travel to programs has an effect on program attendance. Parolees who lack reliable transportation or funds for gasoline or mass transportation may find it more difficult to attend programs at increased distance from home. In fact, some evidence suggests proximity to community services, such as to drug and mental health treatment, is related to offender outcomes with supervision completion rates increasing where services are closer to offenders' residences (Hipp, Jannetta, Shah, & Turner, 2011). The level of program participation has not been tested as a possible moderator on the negative effects of neighborhood structural conditions conducive to re-offending.

The Current Study

While offender attributes, program participation, community structural conditions, and the availability of services have separately been examined related to supervision outcomes, these individual, community, and program variables as a set have not been investigated. Furthermore, no research has investigated the joint effects of offender proximity to community-based programs and measures of parolee participation in programming on parolee outcomes. Additionally, research has not examined how variation in neighborhood structural conditions (economic measures, residential mobility, and heterogeneity) may be related to program availability and parolee participation in programming, and whether program participation can moderate the effects of neighborhood conditions on parolee outcomes. This study will examine the effects of individual risk factors, neighborhood structural conditions, the availability of programs, and parolee program attendance on parolee outcomes.

Supervision outcomes have been measured using a number of different variables. The measures most frequently used are arrest and/or revocation/re-incarceration. This dissertation includes three additional dependent variables not often included in offender supervision research for a total of five dependent variables: positive drug test, technical violation, technical violation arrest, new felony crime arrest, and revocation back to prison. Different dependent variables allow for a more nuanced consideration of how individual, programmatic, and community level factors affect supervision.

Research Setting and Data

The sample for this study is a subset of a population study generated for a National Institute of Justice (NIJ) funded research project. The NIJ population has been described as a 'discharge cohort' but includes all parolees who ended supervision for any reason (sentence end, revocation, and commutation) from January 1, 2011 to December 31, 2013. Offenders selected for the research sample include all parolees in selected urban cities and counties in the state of Georgia who began parole supervision on or after January 1, 2008.

Since 1998 Georgia's Board of Pardons and Paroles has deployed a computer-based case management system. The case management system serves as the collection point for all information related to offender supervision and the source for making operational decisions about offender supervision and for research and analysis. The NIJ project database includes data from the case management system as well as criminal history records and institutional data aggregated from other government databases. A more detailed description of the data and corresponding variables is included in Chapter 4 – Methods.

Data for measuring community structural conditions comes from the U.S. Census Bureau 2010 American Community Survey (ACS) five-year estimates. Each parolee's first home address after release from prison is geo-coded into census tracts in the selected cities/counties. All tracts have a minimum of 19 parolee addresses. Addresses for treatment programs providing mental health, substance abuse, and thinking skills classes also were obtained and geo-coded into census tracts.

Conclusion

It is unlikely that offender behavior is driven by any one circumstance or attribute but rather by a simultaneous host of forces and influences. These influences include individual attributes, neighborhood conditions, and programs that teach new skills and change thinking. Parole officers spend time out of the office encountering parolees in communities where they live and know how neighborhoods differ. By quantifying how these differences impact parole outcomes, corrections managers can consider how and by how much to adjust scarce supervision and programming resources to boost outcomes.

Chapter 2 provides more in-depth explanations of the origins and principle tenets of social learning theory and social disorganization theory and the relevance of each theory for explaining variations in parole supervision outcomes. Chapter 3 reviews what the existing research literature has found related to the level of significance of each theory for explaining reoffending, especially as it applies to parolees in the community. Chapter 4 describes the data and methods to be used in the analysis and Chapter 5 reports the results. Finally, Chapter 6 summarizes and discusses the findings, identifies study limitations and policy implications, and ends with final comments.

CHAPTER II: THEORETICAL LITERATURE REVIEW

Introduction

It could be argued that the theoretical foundation for correctional programming over the past 35 years emerged out of pessimism about changing offender behavior. A review of 231 correctional programs by Martinson (1974) with the goal of assessing treatment effectiveness summarized its findings by saying, "rehabilitative efforts that have been reported so far have had no appreciable effect on recidivism" (p. 48). Described as "nothing works" (Gendreau, Smith, & French, 2006, p. 420), many concluded that the only reasonable option was for state correctional agencies across the U.S. to shift away from rehabilitative programming, to embrace deterrence, and to increase the use of incarceration. Martinson seemed to be suggesting that of all people, only offenders were incapable of learning pro-social behavior (Gendreau, et al., 2006).

Although Martinson's critique initially led to a shift away from offender programming, his findings were not left unchallenged (Gendreau et al., 2006). For example, Palmer (1975) used a more nuanced approach to re-examine the same programs included in Martinson's critique finding that, in fact, some programs reduced recidivism for some types of offenders. Other researchers pointed to a substantial body of research related to learning theory. In fact, differential association theory, originally described in 1947 (Sutherland & Cressy, 1966), and further refined and expanded as social learning theory (Burgess and Akers, 1966), had been successfully applied to offender programming. The principles of social learning theory eventually emerged as an important foundation for explaining offending and for application in offender programming to curb offending behavior (Pratt & Cullen, 2005). The first part of this chapter is a review of the theoretical literature related to how social learning theory both explains offending behavior and is employed in programs with the goal of lowering rates of re-offending.

From here, the chapter turns to a consideration of the broader context in which learning takes place – the neighborhood – and how that may also be related to parolee success. The understanding of the importance of neighborhoods is discussed in terms of social disorganization theory. Social disorganization theory suggests that offending behavior occurs as a result of neighborhood conditions that weaken the ability of residents to exert informal social control over the behavior of residents and others in the neighborhood (Shaw & McKay, 1969). Weakened informal social control creates openings for re-offending. Therefore, social disorganization theory may explain some of the variation in crime among offenders based on differences in neighborhood conditions. The second part of this chapter reviews the theoretical literature related to the significance of social disorganization theory for explaining offending and variation in offending across neighborhoods.

Social Learning Theory

Social learning theory finds its roots in differential association theory, a nine-proposition model that holds a prominent position as a major theory of crime causation (Andrews & Bonta, 2010, pp. 121-123; Sutherland & Cressy, 1966). Interest in differential association led to the development of social learning theory (Burgess & Akers, 1966), a seven-proposition "reformulation" (Akers 1985, p. 41) of differential association theory that ties the propositions more closely to learning principles uncovered in psychology. The result is a more comprehensive and detailed model for explaining the mechanisms of differential association/social learning and thus, how individuals become engaged in criminal behavior (Burgess & Akers, 1966).

Like differential association theory, social learning theory assumes “criminal behavior is learned through the same processes and involves the same mechanisms as conforming behavior” (Burgess & Akers, 1966, p. 132). The *first* proposition of social learning theory is that offending is learned and learning offending behavior includes all the mechanisms included in any other learning, whether offending or lawful behavior (Burgess & Akers, 1966). In particular, learning occurs through operant conditioning or differential reinforcement wherein individuals are either encouraged or discouraged to repeat behavior through positive or negative reinforcement, or through punishment. Positive reinforcement is a “pleasant, pleasing, or desirable event” (Akers, 1985, p. 43) following a behavior. Negative reinforcement encourages behavior by removing from the “environment” (p. 44) something that is negative, unpleasant, or undesirable when the behavior occurs. Punishment suppresses unwanted behavior by following it with an unpleasant response or by removing a privilege when the behavior occurs. The greatest influence over individuals occurs when others control the “major sources of reinforcement” (Akers, 1985, p. 46).

Second, criminal behavior is learned in both social situations where others’ behavior is reinforcing and in non-social situations that are reinforcing of the behavior. In cases such as stealing or drug and alcohol use, reinforcement may be derived not from other people but from the social desirability of what is acquired or the physical effects of the substance (Akers, 1985, p. 46). Nonetheless, the foundation for the majority of learning of offending behavior is social interaction, direct or indirect, through which reinforcement occurs (Akers & Jensen, 2008). Further, the person learning the behavior may be actively performing the behavior or may be an observer. The key is that the behavior is reinforced or perceived to be reinforced (Burgess & Akers, 1966).

The *third* proposition of social learning theory is that learning offending behaviors occurs through groups with whom the learner is primarily associated (Burgess & Akers, 1966). Learning may occur in many different settings and situations, even when not in the immediate presence of others as through the mass media. Variation that occurs in the duration and level of exposure to different groups affects the learning process.

Fourth, offending, which may include “specific techniques, attitudes, and avoidance procedures” (Burgess & Akers, 1966, p. 140-141) is more easily learned when reinforced and the reinforcers are readily available (Burgess & Akers, 1966). Reinforcers include positive feedback or approval, financial rewards, and improved status in the group. This proposition highlights the importance of knowing the specific reinforcers that have the greatest effect on behavior and the rationalizations individuals and groups tell themselves to avoid punishment or personal social disapproval for deviant acts.

The *fifth* proposition, closely aligned with the fourth, states that the learning of specific types of behaviors and how often the behavior is repeated depend not only on the availability and effectiveness of reinforcers and the accepted ways reinforcers are applied but also on the “rules or norms” (p.142) that establish how they are applied (Burgess & Akers, 1966). Knowledge of the rules groups use to select reinforcers and the specific reinforcers used help to explain what behavior individuals may adopt or will dominate in groups.

The *sixth* proposition of social learning theory, considered the “heart” (Burgess & Akers, 1966, p. 14) of differential association (Sutherland & Creesey, 1966, p. 81), is that the more offending behavior is expected, preferred, and “reinforced” (Burgess & Akers, 1966, p. 143) in contrast to lawful behavior, the more it will occur (Burgess & Akers, 1966). The amount of “definitions” (p. 143) (thoughts/beliefs/justifications) that place offending in a favorable light is key to choosing offending behavior. Offending behavior can also arise when lawful behavior is not sufficiently reinforced creating a gap where offending behavior is likely to be positively reinforced.

Lastly, the *seventh* proposition is that as the level of reinforcement – amount, frequency, and likelihood – increases so too does the “strength” (p. 144) of the delinquent behavior (Burgess & Akers, 1966). This proposition summarizes key elements that underlie, and to a degree, repeat the previous propositions. The greater the positive reinforcement of offending the more the behavior is likely to be repeated. The greater the frequency of positive responses to any behavior - the shorter the time between responses - the more likely the behavior is to be repeated. Finally, more responses (behavior) will occur as the ratio of reinforcements to responses increases. In other words, the more frequently a response is followed by a reinforcement the more the behavior is likely to be repeated.

In summary, social learning theory has four major dimensions. First is differential association – more frequent direct interaction, association, and identifying with others who share beliefs, values, and attitudes supportive of the behavior in question. Differential association may also include indirect contact such as through the internet or mass media. These associations are of higher priority than other relationships, consume more time, and occur more frequently. Second, social learning involves being exposed to and personally adopting definitions - “rationalizations, justifications, and excuses” (Akers & Jensen, 2008, p. 39) that justify the behavior as more right or wrong. Definitions favorable to offending allow offenders to deflect criticism of offending. In other words, the more a person agrees with definitions favorable to offending, the more likely that individual is to participate in the behavior (Akers & Jensen, 2008).

The third, and most important dimension of social learning, is differential reinforcement which is the balance of rewards and punishments associated with behavior. Any behavior, including offending, is more likely to be repeated when followed closely by personal, direct, positive reinforcement from others. Rewards can be as simple as verbal approval, acceptance in a group, personal pleasure, or monetary. Positive reinforcers are far more effective in eliciting repeated behavior in the future. Behavior is more likely to occur and be repeated when the balance of reinforcers is supportive of the behavior, and when reinforcers occur more quickly after the behavior and with greater frequency.

The fourth dimension of social learning is imitation – the direct or indirect observation of others performing the behavior. Imitation has its effects through the individuals/groups performing the behavior, the actual behavior observed, and the observed or imagined consequences/reinforcement received from the behavior. Imitation is most useful when an individual is first learning the behavior. Finally, imitation is important for learning complex behaviors.

Social learning theory is argued to explain ALL learning and the behavior that follows from it, both conforming and offending. Behavior is therefore malleable, based on the beliefs, attitudes, behaviors, justifications, and reinforcers the individual hears, sees, and experiences. Consequently, even the beliefs, attitudes, behaviors, justifications, and reinforcers that lead to and support offending are not fixed and permanent in the individual. Behavior can also be shifted in the opposite direction away from offending through the application of the same principles of social learning theory. Indeed, all of the principles of social learning theory are employed to develop and implement programming designed to help offenders learn and choose lawful behavior over offending.

Social Learning Theory and Correctional Programming

The ideas of social learning theory have been adopted and specified for use in correctional programming. Correctional programs that employ social learning techniques aim to change the thinking (thoughts, beliefs, justifications) and behaviors that lead to offending. Risk-Need-Responsivity, also known as the theory of effective correctional intervention (Gendreau et al., 2006) or the theory of offender rehabilitation (Ward, Mesler, & Yates, 2007), is a comprehensive model that addresses how learning theory should be applied to achieve the greatest positive effect on offender supervision outcomes (Andrews & Bonta, 2010, p. 45; Andrews & Bonta, 2010a; Bonta & Andrews, 2007). Risk-need-responsivity (RNR) is a rehabilitation theory and not a theory about the origins of any particular type of crime (Ward, Melser, & Yates, 2007). RNR encompasses how offender candidates for programming are identified, the characteristics and behaviors that are the focus of interventions, and program delivery and management. The risk principle is discussed first.

Risk Guides Supervision and Programming

Risk is defined as behaviors, beliefs, states, and other attributes that increase the propensity or likelihood of re-offending. Parolees and other offenders vary in their risk of re-offending (Andrews., Bonta, & Hoge, 1990). RNR specifies a number of personal attributes including being younger and male, impulsivity, a history of antisocial behavior, and having antisocial associates that place offenders at heightened risk of re-offending. Risk to re-offend is best determined through validated, actuarial risk assessment instruments (Andrews, Bonta & Wormith, 2006). Levels of supervision, intensity of programs offenders are assigned to, and whether assigned to programs or not, depend on each offender's risk to re-offend (Bonta & Andrews, 2007). High-risk offenders should receive the most attention and programming; whereas, low risk offenders should receive little or no programming. Indeed, low risk offenders' likelihood of re-offending increases if over-supervised (Bonta & Andrews, 2007; Andrews & Bonta, 2010, p. 48; Lowenkamp & Latessa, 2005).

The risk principle specifies that the level of risk be matched with program intensity – the amount of time offenders engage in programs. Higher risk offenders need more time in programs to change risk factors. Moreover, programming engages offenders during what otherwise might be 'risky' free time in neighborhoods where associates providing support and encouragement for offending are likely to be found. Criminal associates are a powerful predictor of future criminality (Andrews & Bonta, 2010, p. 110).

Needs – Matching Offenders to Programming

The second principle of the RNR model -need- answers the question, "What should be the targets of offender treatment?" Whereas, the risk principle identifies who and how much to treat, the need principle identifies the specific thinking (definitions) and behaviors that should be the targets of treatment. Offender needs associated with an increased likelihood to re-offend include definitions that justify offending (criminal thinking); associating with others who adhere to similar definitions and rationalizations justifying offending; impulsivity; poor problem solving that leads to offending supportive definitions; substance abuse; and mental health problems (Andrews and Bonta, 2010, pp. 48-49, 59-60; Andrews, Bonta & Hoge, 1990; Bonta & Andrews, 2007). The RNR model makes clear that needs not associated with offending, such as low self-esteem, should not be the primary targets of offender programming. Moreover, attributes that cannot change, called "static factors" such as age are also an inappropriate target for offender programming (Andrews & Bonta, 2010, pp. 48-49). Much community-based offender programming focuses on definitions and justifications favorable to offending summarized as criminal thinking, and substance abuse and mental health.

Responsivity

The responsivity component of RNR employs the propositions of social learning theory to deliver programs for the greatest effect. Responsivity is defined as applying a personalized approach to the design and delivery of programs. Responsivity leads to program delivery times, places, and teaching techniques that work best for offenders assigned to those programs. Responsivity may include programming that is specific to the differing psychological and emotional needs of males or females, or delivered in a class setting that is gender specific.

Effectively delivered programs are responsive to the thinking, learning styles, and abilities of offenders as well as to practical considerations such as distance to programs (Andrews & Bonta, 2010, pp.49-50; Andrews et al., 1990; Gendreau & Ross, 1987). Offender-focused programs under the RNR model incorporate cognitive-behavioral techniques to change thinking and teach new skills. For example, the program Thinking for a Change (T4C) includes several sessions on problem-solving (Bush, Glick, & Taymans, 1998). Many offenders lack the skills to identify and solve problems they encounter each day, problems such as lacking transportation to work. The cognitive component of T4C teaches how to identify physical indicators of a problem such as increased anxiety, how to accurately identify the problem, how to brain-storm to recognize all possible solutions, and how to implement the best solution. Answers at each step are elicited from program participants with participants resolving differences and 'correcting' each other. The behavioral component is hands-on practice of each step using real-life problems offenders frequently encounter. RNR suggests practice is the most effective way to learn new skills.

Offenders also lack social skills such as asking for help. Once again, the cognitive steps are followed to recognize the internal cues that say there is a problem followed by the other steps. A common problem that offenders encounter is in how they interact with others when asking for help. Teaching how to effectively ask for help requires that the offender learn how to identify specifically what help is needed and how to ask for help in an assertive rather than in an aggressive manner. Too often offenders act aggressively when asking for help which results in resistance from the helper. Assertiveness, which has to be taught, is seen as 'being weak.' Practice is again required to learn these social skills. Offender behavior change is positively associated with programs that pay attention to responsivity (Allen, MacKenzie, & Hickman, 2001; Lipsey & Cullen, 2007; Lipsey, Landenberger, & Wilson, 2007).

In summary, the Risk-Need-Responsivity (RNR) model of offender rehabilitation puts forward an evidence-based, integrated approach to reducing re-offending. First, the risk principle points to 'who' should be the focus of the highest levels of offender programming and supervision. Low risk offenders receive both low levels of supervision and little or no programming. As the risk to re-offend increases, offenders receive increasing levels of programming. Second, the need principle points to ‘what’ should be treated to reduce recidivism. Dynamic/changeable attributes associated with specific risks are the targets for offender programming. Third, the responsivity principle is the ‘how’ of the RNR model. Offenders’ risk, learning styles, and other personal attributes inform how to deliver programs.

While RNR has importance for explaining parole success it is primarily focused on programming and ignores the impact of other broader contexts. For example, parolees at all levels of risk, many of whom may indeed benefit from participation in programs to address criminogenic need, may also live in neighborhoods that are more or less criminogenic. Variation in the underlying structural conditions across neighborhoods may account for variation in both the availability of appropriate programs and offender outcomes.

Macro-level theories of offending such as anomie theory and social disorganization theory attempt to explain the influence of community conditions on offending. Anomie theory attributes offending to conditions surrounding poverty and the consequential inability of residents to achieve goals. Anomie theory suggests that a mismatch between goals and access to the mechanisms for goal achievement leads to crime when the rules for “institutionally appropriate” (Merton, 1938, p. 673) methods for achieving goals are weak or ignored. In the context of widely accepted cultural goals but unequal access to legitimate means, an unskilled worker may turn to selling drugs to make up for low pay that does not permit self-sufficiency. Social disorganization theory attributes offending to neighborhood structural conditions and the consequent effects on community processes that lead to a lack of informal social control.

Social Disorganization Theory – Considering Community Influence on Offending

It has long been known that delinquency rates, overall crime rates, and indeed parole failure rates[2] vary across neighborhoods. Social disorganization theory suggests a significant portion of the difference in these rates is due to variation in the strength of certain neighborhood structural conditions (Pratt & Cullen, 2005). Neighborhood structural conditions related to poverty, mobility, and racial/ethnic heterogeneity create openings for crime to increase. Surprisingly, although many neighborhoods where offenders reside are high in levels of these structural conditions, a limited amount of research has investigated community effects on offender supervision outcomes (Kubrin & Stewart, 2006). Therefore, social disorganization theory provides a promising and practical framework for examining the role of community structural conditions in conjunction with offender attributes, available programs, and the effects of programming on re-offending behavior. This section of the literature review begins with a brief history of the development of social disorganization theory. Interest then turns to disorganization theory and the availability of community services.

Social Disorganization Theory – A Brief History

Social disorganization theory is concerned with neighborhood structure and the consequential processes through which neighborhoods and communities self-manage the behavior of community members (Kubrin & Weitzer, 2003). The theory was proposed to explain variation in crime rates across neighborhoods in the city of Chicago (Shaw & McKay, 1969).

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[2] Parole completion rate by parole office has served for many years as a key performance measure in the state from which the data for this research is drawn. While this variation has been a long-standing concern, no research has been undertaken to uncover underlying causes.

Comparing characteristics of neighborhoods across the city, higher rates of delinquency were found in neighborhoods with high rates of decreasing population (pp. 144-45), neighborhood ethnic heterogeneity (pp. 152-53), and low economic status (pp. 147, 149). Further, high rates of delinquency continued over time even as neighborhood ethnic makeup changed from one group to another (pp. 70, 153, 157, 162, 384, 435). More intriguing, delinquency rates declined in the children of immigrants who moved out of such neighborhoods (Shaw & McKay, 1969, pp. 157, 381, 384). Rates of delinquency across the city did not seem to depend on race or ethnicity nor were economic status and population change argued to lead directly to delinquency. Rather, delinquency was argued to be associated with the social conditions in the area (p. 384).

Between low and high crime areas, Shaw and McKay (1969) described "differential systems of values" (p. 170) with stronger and more consistent messages of conformity to values consistent with lawful activity found in low delinquency, higher status neighborhoods. In such neighborhoods, a higher and more uniform level of agreement focused attention on education and positive leisure activities (pp. 170-71). By resisting activities that imperiled the maintenance of conforming values, low delinquency neighborhoods protected residents from exposure to opposing values (pp. 170-171).

In contrast, residents in high delinquency neighborhoods were exposed to "competing and conflicting moral values" (p. 170). In addition to exposure to the symbols of conventional values such as legitimate employment, low economic status neighborhoods with high rates of delinquency also experienced much greater direct exposure to opposing values such as the material rewards derived from crime (p. 171). Individuals in neighborhoods experiencing high rates of decreasing population, increased ethnic heterogeneity, and low economic status heard more positive messages about crime and were exposed to more opportunities to participate in criminal activity (Shaw& McKay, 1969, p. 171, 174). Moreover, fewer legitimate job opportunities created openings for illegitimate jobs (p. 438).

Shaw and McKay (1969) suggest low and high delinquency areas differ in one other important way. Low delinquency areas present an unambiguous and consistent message about community values while agreement on values is impossible to achieve in areas of high delinquency. Conflicting values in areas with high rates of delinquency lead to a lack of shared agreement on community problems and solutions (p.184). The absence of shared agreement leads to an inability to collectively exert informal social control over the behavior of community residents and others in the neighborhood (Kornhauser, 1978, pp.73-78). A community experiencing such disruption is "one unable to realize its values" (Kornhauser, 1978, p. 63).

In summary, social disorganization theory argues that neighborhood structural conditions measured as low economic status, population change, and population composition are related to the strength of community social processes. Low economic status, population change, and population composition weaken community social processes making it difficult for community residents to acknowledge and come to agreement on common values and to then implement effective informal social controls. Weakened neighborhood social processes enable crime to increase.

Social Disorganization Theory and Community Services

Social disorganization theory not only argues that increased levels of neighborhood poverty, ethnic heterogeneity, and population mobility lead to weakened informal social control but that these attributes also lead to inadequate funding and skills. Inadequate funding and skills to support neighborhood-wide institutions contribute to their isolation and instability (Kornhauser, 1978, p. 78). Organizations that do form may not thrive as populations change and members move to more stable neighborhoods. One example of lost institutional support is churches that make space available for community programs. Churches may dissolve as members move on or move with their congregations to other neighborhoods. Conversely, neighborhoods with low resident turnover, less poverty, and that are more ethnically similar build more stable and long-lasting communication networks and relationships among neighbors that lead to more enduring neighborhood organizations. When organizations are stable and enduring, they are better able to identify and effectively serve the needs of community members (Kornhauser, 1978, p. 79).

Neighborhood institutions with high turnover become weak or ineffective. These include neighborhood-wide groups that provide services or assistance to community members. Neighborhood voluntary associations and institutions become isolated without "linking structures and functions" (Kornhauser, 1978, p. 79) that unify neighborhood groups, and tie groups together across the community. Communication limited by "ethnic and racial cleavages" (Kornhauser, 1978, p. 79) makes it more difficult to recognize common values and interests that serve as the seeds for creating effective communitywide organizations. As Kornhauser (1978) has noted, neighborhoods with "heterogeneous, poor, and mobile populations lack community of purpose, money, skills and will" to form the "voluntary associations to protect and defend their interests and values" (p. 79). When neighborhood structural conditions create obstacles to forming and sustaining neighborhood groups, organizations that represent or serve the entire community are also missing (p.79). Such communities may be unable to provide drug treatment and mental and addiction counseling, vital services often needed by offenders.

Conclusion

In summary, social learning theory attempts to explain how offending behavior is learned and justified. Building on this theoretical argument, correctional programming has focused on getting needed programs to offenders most at risk to restore them to lives focused on lawful behavior. Importantly, social learning theory does not consider the context in which offenders live. Neighborhood level theories such as social disorganization theory suggest that neighborhood conditions should also be considered when examining parole success. These conditions affect community processes, such as informal social control, that are also related to offending. Further, neighborhood conditions may also reduce institutional capacity to address the needs of offenders returning from incarceration.

Theoretical Model

Neither measures of individual-level attributes of offenders nor measures of the extent and appropriateness of offender treatment fully explain variation in rates of parole success. An important missing piece for understanding variation in parole success may be the community context in which parolees live, including the availability of and types of services that may be important for successful completion of parole. While offender attributes, program participation and duration, community structural conditions, and proximity to services have been examined separately for their relationship to supervision outcomes, these individual, community, and supervision variables as a set have not been investigated. Furthermore, no research has investigated the joint effects on supervision outcome of offender proximity to community-based programs and measures of parolee participation in programming. Neither has research examined how variation in neighborhood structural conditions (economic measures, mobility, and heterogeneity) is related to program availability and consequently, parolee participation in programming. A better understanding of the interplay of these variables will assist researchers in identifying their influence on re-offending and may inform policy makers' decisions about how to allocate resources to improve supervision outcomes across neighborhoods and cities.

Independent variables examined include offender risk characteristics, the availability and types of services, service dosage, and U.S. Census tract measures of community structure associated with social disorganization theory. These variables are examined in relation to offender supervision outcomes including a positive drug test, technical violation and technical violation arrest, new felony crime arrest, and revocation back to prison. Different outcome measures allow for a more nuanced consideration of how individual and community level factors affect supervision.

Figure 2.1 depicts the theoretical model to be examined. Path 1, which is important to control, examines the direct influence of parolee attributes on re-offending and other negative outcomes. Following the tenants of RNR and of greater importance, Path 2 examines the direct effect of program attendances on re-offending and other negative parole outcomes; and, Path 3 posits that program attendance moderates the effects of individual risk factors, such that as program attendance increases, the negative effects of individual risk factors on re-offending and all other outcome variables will decrease. Path 4 hypothesizes that individual parolee attributes may indirectly affect re-offending through their influence on program attendance or through their direct influence on re-offending. Based on social disorganization theory, Path 5 predicts that neighborhood structural conditions of poverty, heterogeneity, and residential mobility, contribute to offender re-offending and negative parole outcomes (e.g., technical violations, positive drug tests, arrests, revocations) and Path 6 predicts that program attendances moderate the negative effects of neighborhood structure on re-offending and other parole outcomes. Path 7 predicts that these same neighborhood structural conditions affect program availability which consequently affects attendances which then influence parole outcome measures. In the model, criminal thinking programs are abbreviated as COG, mental health programs are abbreviated as MH, and substance abuse programs are referred to as SA.

Overall the model suggests that both neighborhood level and individual level factors have significant effects on outcomes. However, key to these effects are the intervening and moderating influences of program attendance, which is itself influenced by the availability of programs in the parolee's census tract. Chapter 3 investigates what the research and evaluation literature has found related to each of the above paths.

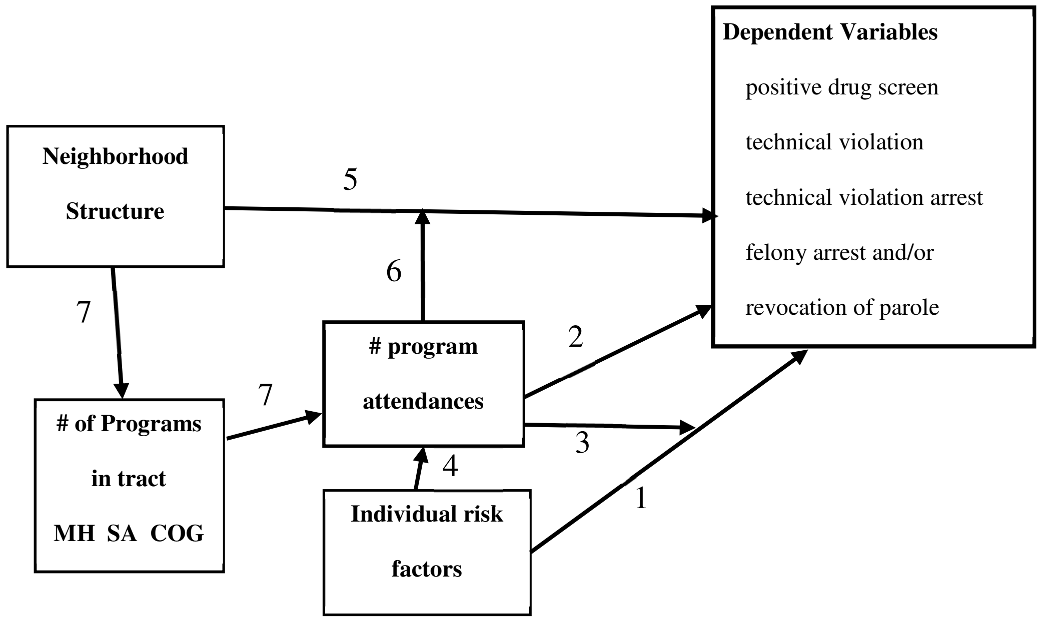

Figure 2.1
Theoretical Model Illustrating Seven Hypotheses

CHAPTER III: REVIEW OF THE EMPIRICAL RESEARCH

Introduction

This dissertation focuses on whether parole supervision outcomes can be explained by community conditions as suggested by social disorganization theory and by participation in programs as argued by Risk-Need-Responsivity (RNR). Parolees living in neighborhoods with elevated levels of poverty, mobility, and heterogeneity, and in neighborhoods where programs are less available, are posited to have higher levels of parole violations and supervision failure. On the other hand, parolees who attend mental health, pro-social (thinking skills), and drug/alcohol programs appropriate to their individual level of risk exhibit lower levels of violations and parole failure. Moreover, increasing levels of attendance for higher risk offenders will moderate the effects of disruptive neighborhood conditions. Little is known about how the level of program participation and neighborhood conditions, when considered together, may affect parole supervision outcomes. This chapter reviews empirical findings of the validity of the RNR approach to offender change, the effects of neighborhood factors on parole outcomes, and the combined effects of community level attributes and program participation on re-offending. The literature testing the Risk-Need-Responsivity model is reviewed first.

Risk-Need-Responsivity (RNR)

Risk

Within the RNR model of offender change, risk is considered first when addressing re-offending. As explained in the previous chapter, the effectiveness of the RNR model is based on accurately identifying and referring to programming those offenders who are at elevated risk to re-offend. Today, practitioners and researchers use validated actuarial risk instruments to measure risk to re-offend. This section assesses the empirical literature related to offender risk prediction and re-offending.

Structured offender risk prediction instruments are not new to corrections, having been developed and validated beginning in the 1920's. One of the earliest validated instruments used the "Burgess method" (Hakeem, 1948, p. 384) to calculate risk scores and was later revised and refined (Allen, 1942; Burgess, 1936; Lanne, 1935; Monachesi, 1941; Tibbits, 1932). Other validated instruments include the Salient Factors Score for Federal offenders (Ferguson 2016; Hoffman, 1983; Hoffman & Beck, 1974), Risk Prediction Index (Administrative Office of the US Courts, 2011; Johnson, Lowenkamp, VanBenschoten, & Robinson, 2011), and Federal Post Conviction Risk Assessment (PCRA) (Luallen, Radakrishnan, & Rhodes, 2016). In efforts to validate risk instruments, common dependent variables or outcomes included any re-arrest, revocation, or arrest for a new offense.

The Level of Service Inventory-Revised (LSI-R) and Correctional Offender Management Profiling for Alternative Sanctions (COMPAS) are popular commercially developed risk and need assessment instruments that have become the predominant tools for assessing offender risk. Earlier risk instruments as well as the LSI-R and COMPAS use a common set of risk predictors in different combinations related to criminal history, age, education, employment, attitudes and beliefs, associates, and alcohol/drug use. Based on the common factors used across different risk assessments, individuals at high risk to re-offend can be described in general as younger with more extensive criminal histories, less educated and with chronic periods of unemployment, drug users, and able to rationalize their deviant behavior (Andrews & Bonta, 2010, pp 58-60).

RNR asserts that as risk increases, the level and intensity of programming must also increase to achieve reductions in re-offending (Andrews et al., 2006). The traditional approach to supervision, however, assumed all offenders had some risk to re-offend and, therefore, would benefit from attending correctional programs. Andrews and Bonta (2010) state that not only should low risk offenders receive little or no programming, exposing low risk offenders to programming may increase re-offending (p. 48). Validated actuarial risk instruments provide a mechanism for sorting out which offenders released from prison are, in fact, high risk to reoffend. The LSI-R and COMPAS claim to assess accurately overall risk to re-offend at the time supervision begins and to assess offenders for criminogenic needs. The LSI-R is discussed next.

LSI-R: The LSI-R is the risk instrument most closely associated with the RNR social learning theory approach to offender supervision (Andrews & Bonta, 2010, p. 58-59). The first published validation of what is now called the LSI-R was conducted on 192 young adult probationers under supervision an average of 28 months (Andrews, Kiessling, Mickus, & Robinson, 1986). Domain scores and overall predictive accuracy were compared to scores from six other validated assessments. Pearson correlations comparing effect sizes found the LSI "marginally exceeded" (p. 462) the predictive ability of other instruments with highly significant correlations between LSI score and recidivism. (Andrews et al., 1986). The present version of the instrument, the Level of Service Inventory-Revised (LSI-R) is 54 questions in 10 domains: criminal history, education/employment, financial, family/marital, accommodation/housing, leisure/recreation, companions, alcohol/drugs, emotional/personal, and attitudes/orientation (Hsu, Caputi, & Byrne, 2009; Kelly & Welch, 2008).

Several studies validated the ability of the LSI-R to accurately predict re-offending risk in community corrections. In one study, trained probation staff administered the LSI-R to 2,107 federal probationers between 2001 and 2003 (Flores, Lowenkamp, Smith, & Latessa, 2006). The outcome variable, return to incarceration in a Federal facility, was compared to LSI-R total scores. Using Pearson correlations, predictive validity "supported the LSI-R as a significant predictor of subsequent incarceration" (p. 47). A multivariate logistic regression including the LSI-R total score, sex, age, and ethnicity found the LSI-R strongly predicted incarceration as did age at a lower level of significance but not ethnicity. The cohort was 65% Hispanic (Flores et al., 2006).

Another study used the LSI-R and three other validated assessments (Psychopathy Checklist-Revised (PCL-R), Violence Risk Appraisal Guide (VRAG), and General Statistical Information on Recidivism (GSIR)) to demonstrate the commonality of questions among validated assessments (Kroner, Mills, & Reddon, 2005). Prior to release from prison a total of 205 prison inmates were assessed with all four instruments. Next, "new" instruments were created by repeating four times the random selection of 13 questions from a pool of all questions in the four instruments with one additional question, number of prior criminal incarcerations, added to each instrument. Dependent variables were criminal conviction or parole revocation (Kroner et al., 2005). All four assessments highly correlated with half of the cohort and question analysis revealed four factors: criminal history, persistent criminal lifestyle, psychopathic personality, and alcohol/mental health issues that demonstrated almost perfect congruence when applied to the second half of the cohort (Kroner et al., 2005). The four established instruments and the four randomly selected assessments predicted risk equally as well. One conclusion that might be drawn from this test is that valid risk prediction can be created relatively easily by incorporating variables used in other established and validated risk instruments (Kroner et al., 2005).

Two other validation studies using the LSI-R accurately predicted reconviction over 2.5 years for 176 probationers in the first study with criminal history being the most significant predictor domain (Girard & Wormith, 2004). The second study predicted return to corrections custody for 41,000 offenders under supervision seven to 10 months in Australia (Hsu, Caputi, & Byrne, 2009). Both studies found the strongest predictors related to measures of criminal history. Next is a brief review of literature on predicting risk in specific offender groups.

LSI-R brief version: Considering the length and complexity of the LSI-R as well as the time required for completion and scoring, some researchers have worked to construct shorter versions that retain similar levels of accuracy. In response to this need, the LSI-R:SV (short version) was created. Lowenkamp, Lovins, and Latessa (2009) conducted a validation study to compare the eight question LSI-R:SV to the full LSI-R to predict the re-arrest or re-incarceration of 483 probationers in one state. The average follow-up period was 18 months. Pearson’s r was used to compare total scores to outcomes. Both instruments’ scores were highly correlated with arrest/re-incarceration. As levels of risk increased, the number of offenders rearrested increased. The LSI-R:SV was very effective in identifying low-risk probationers but only “moderately effective” (Lowenkamp et al., 2009, p, 198) in identifying moderate and high-risk probationers.

Another comparison between the LSI-R and LSI-R:SV to predict reconviction among 905 probationers in Great Britain found the LSI-R:SV “almost as accurate as the full LSI-R” (Raynor, 2007, p. 128). Finally, one other study investigated the short version of the LSI-R on mentally ill and drug using offenders. The LSI-R:SV was effective in predicting re-offending among 208 mentally ill offenders under supervision in the community (Ferguson, Ogloff, & Thomson, 2009). Applying receiver operating characteristics, the LSI-R:SV predicted recidivism "significantly above chance for any new offense" (p. 15). Reviewed next are two studies using the LSI-R that investigated how time in prison may affect the accuracy of risk assessment.

Time in prison and risk prediction: The LSI-R is the only risk assessment that has been studied for validity based on the amount of time incarcerated. This issue is relevant as prison sentences in Georgia have slowly increased over time and many offenders are serving longer sentences before release to community supervision. The LSI-R was originally tested using offenders with sentences up to two years (Manchak, Skeem, & Douglas, 2008) which leads to questions about the validity of risk assessment in general with offenders who have served much longer periods of incarceration. Two studies address this question. The LSI-R was administered before release to 555 prisoners who served at least 10 years but received no supervision after release. The authors describe this as a true natural experiment of predictive validity (Manchak et al., 2008). After release, over the next year each offender was tracked for new convictions. Cox proportional hazards analysis found the LSI-R score significantly predicted general recidivism. Each one-point increase in the LSI-R score corresponded to a 6% increase in risk to recidivate (Manchak et al., 2008).

The second study involved 129 former inmates, 80% convicted of violent crimes and 83% with at least one previous incarceration. Time served before release averaged five years. It is unclear how many of the total number were under supervision when they left prison (Simourd, 2004). Recidivism (re-arrest/violent re-arrest, reconviction, re-incarceration, and supervision violation) was tracked over 15 months. Although LSI-R scores placed more inmates at higher levels of risk than would be expected in the normal offender population, LSI-R scores predicted overall recidivism (Simourd, 2004). The next section reviews COMPAS, which, like the LSI-R, has evolved to predict risk and needs. A summary follows and then a review of risk related to specific offender groups.

COMPAS: COMPAS assesses for general risk to re-offend and for criminogenic needs through 137 questions in 15 domains/scales: criminal involvement, history of noncompliance, history of violence, current violence, criminal associates, substance abuse, financial problems, vocational/educational, family criminality, social environment, leisure, residential instability, social isolation, criminal attitudes, and criminal personality (Brennan, 2009). COMPAS' validation involved 2,328 probationers interviewed by probation officers between 2001 and 2004 as part of presentence investigations or during probation intake (Brennan, Dieterich, & Ehret, 2009). Validation used three dependent variables: arrest for any offense, arrest for a person offense, and felony arrest (Brennan et al., 2009). A multivariate Cox regression found six of the 15 scales significant in predicting a felony offense: history of non-compliance, criminal associates, substance abuse, financial problems, vocational or educational, and social environment.

The only other COMPAS validation study that could be found tracked violations and returns to prison for 91,334 offenders released from incarceration to parole supervision in California (Zhang, Roberts, & Farabee, 2014). Pearson rank-order correlation coefficients confirmed a significant relationship between increasing COMPAS scores and arrest for any reason over the two-year follow-up. In order to address questions about the time necessary to complete the assessment, a second comparison was conducted. Logistic regression was used on split samples to re-evaluate the odds of arrest for each incremental increase in COMPAS score compared to the predictive ability of a group of four separate well-known factors also available in offenders' records (gender, age, age at first arrest, number of prior arrests).

Both COMPAS and the grouped well-known factors demonstrated similarly high levels of accuracy predicting re-arrest. Study authors suggested time and resources could be saved by using the group of four well-known factors to determine risk level rather than administering the full COMPAS (Zhang et al., 2014). This research also points out the powerful significance of age and measures of criminal history to predict re-offending and the fact that using more variables does not necessarily improve predictive accuracy.

In summary, the research reviewed thus far highlights the many decades of development, review, and refinement of tools to improve the accuracy of offender risk prediction. Validated risk instruments are actuarial in design, basing estimates on the analysis of the attributes of large numbers of offenders. Highly predictive risk instruments have been constructed with as few as seven to as many as 100 predictors or more. A core set of predictors including information related to criminal history are found in virtually every validated re-offending risk prediction instrument. The discussion on risk next turns to risk prediction for specific subgroups of offenders.

Race/ethnicity and risk prediction: Five studies were found that investigate the validity of risk assessment instruments with African American and Hispanic offenders. Fass, Heilbrun, Dematteo, and Fretz (2008) compared the LSI-R to COMPAS using 975 randomly selected Caucasian, African American (AA), and Hispanic offenders noting any re-arrest over 12 months.

This research focused on four common criminal history variables found in both instruments (previous adult and juvenile arrests, previous adult convictions and parole violations) and the overall LSI-R and COMPAS scores. The first analysis using logistic regression examined the predictive ability of the four criminal history variables on each offender group (133 Caucasian, 696 AA, 146 Hispanic). Criminal history was significant in predicting Hispanic re-arrest ($p = .031$). Although 87% of Hispanics were accurately predicted to not be rearrested, only 40% of arrests were accurately predicted (Fass et al., 2008). The criminal history variables were not significant in predicting Caucasian ($p = .374$) or African American ($p = .143$) arrests (Fass et al., 2008).

Turning to the analysis of each assessment instrument, total scores for the 696 LSI-R recipients were statistically significant ($p = .001$) in predicting re-arrest. When the results were examined by race/ethnicity the prediction for re-arrests of African American offenders ($p = .001$) was significant but not significant for Caucasian or Hispanic offenders. A similar analysis of the 276 offenders who completed COMPAS was not significant when the entire cohort was examined or for any of the three groups analyzed separately (Fass et al., 2008). Overall, these authors summarize the instruments as generally over-classifying or under-classifying offenders based on race and the risk instrument used.

On the other hand, Brennan et al.'s (2009) validation study with 2,328 probationers using Cox proportional hazards models found COMPAS recidivism risk scales estimated the area under the curve (AUC) at .69 for white males for any crime arrest and .71 for a felony arrest. The analysis for African American men found an AUC of .67 for any crime arrest for males and .73 for felony arrest.

A third study investigated the validity of the LSI-R to assess for re-arrest and re-conviction with 445 African American (n = 333) and Hispanic (n = 112) parolees in halfway houses and day reporting centers (Schlager & Simourd, 2007). Offenders were followed for two years. Overall mean LSI-R composite scores between the two groups were not statistically significant. When examined as one group, the overall correlations for re-arrest (r = .06) and for reconviction (p = .09) were not significant. The authors point to research by Gendreau, Goggin, and Smith (2002) which indicates much higher correlations (r = .42) are more typical.

Examination by group found correlations for Hispanics were also not significant. Correlations with African Americans' LSI-R scores found re-arrest (r = .08) was not significant but re-conviction (r = .11) was significant. An examination of LSI-R subcomponents found that offenders reconvicted had greater mean total LSI-R (p = .05) and education/employment (p = .05) scores than non-recidivists. Schlager and Simourd (2007) summarize by saying that, on the one hand, overall scores suggest the LSI-R, at best, weakly predicts recidivism. On the other hand, higher mean scores in the African American and Hispanic subgroups are in line with what would be expected in higher risk offender groups "suggest[ing] that the LSI-R is effective" (Schlager & Simourd, 2007, p. 553).

The Federal Post Conviction Risk Assessment (PCRA) was investigated for validity with 84,000 Black and white Federal probationers tracked for arrests over a 12-month period (Lowenkamp, Holsinger & Cohen, 2015). Examining white and black probationers separately, arrest rates were calculated at four different risk levels. For both groups, as predicted by PCRA scores, as risk level increased from low to high, the rate of arrest increased for both black and white probationers (5% to 37% black, 3% to 32% white). Moreover, AUC's were .70 for Black and .74 for white probationers, suggesting acceptable and similar levels of prediction.

Finally, in a more global look at potential race and ethnicity effects with overall LSI-R scores, Olver, Stockdale, and Wormith (2014) reviewed 126 studies covering the time period 1981-2012, finding total LSI-R score predicted general recidivism from the community with "moderate accuracy" (p. 160). LSI-R scores significantly predicted general recidivism for Hispanic and African American offenders but at higher levels than for white offenders (Olver et al., 2012). Based on the research reported in this section related to risk assessment and race/ethnicity, risk assessment instruments can be constructed to account for differences in race and ethnicity. However, overall validity can mask differences among subgroups which suggests that validity tests should analyze subgroups separately.

Risk instrument validation by population: Disappointing validity tests of risk instruments with Hispanics and African Americans highlight a wider issue of the wisdom of widespread use of risk instruments across different populations in different geographic areas. Researchers have argued that risk prediction instruments may need to be adjusted to account for variation in the influence of individual variables across different populations (Flores, Lowenkamp, Smith, & Latessa, 2006; Luallen et al., 2016). Wright, Clear, and Dickson (1984) tested the validity of a risk instrument that had been validated elsewhere on a cohort of 366 probationers in New York City who had completed supervision. A chi square test found 10 of the 15 factors to not be significant in predicting the outcome leading to the conclusion that risk models "developed on one population do not necessarily transfer readily to other populations"

(Wright et al., 1984, p. 122). The next section investigates the research related to “over-treating” low-risk offenders.

Low risk offenders and risk prediction: RNR argues that the violation and re-offending rates of low-risk offenders could increase if treated like high-risk offenders such as when required to attend programs. (Andrews & Bonta, 2010, p. 48; Bonta, Wallace-Capretta, & Rooney, 2000). It is important to account for the adverse effects of over-treating low-risk offenders. Accounting for these effects allows for a more accurate accounting of community conditions. One line of thinking suggests increased re-offending among low-risk offenders required to attend programs may be due to exposure to high-risk offenders who also are attending the same programs. This is known as “iatrogenic effects” (Wiener, 1998, p.39) or countervailing risks. Attending intensive correctional interventions exposes low-risk offenders to high-risk offenders who present strong antisocial influences toward re-offending (Dishon, McCord. & Poulin, 1999; Lowenkamp & Latessa, 2005). Requiring attendance may also disrupt the prosocial ties low-risk offenders already have in the community (Lowenkamp & Latessa, 2005). A study investigating this question with probationers required to attend an intensive program found a significant reduction in re-convictions among high-risk probationers and an increase in re-convictions among low risk compared to a control group (Bonta et al., 2000).

Another study investigated the question of risk and program intensity with 3,056 offenders under intensive community supervision in 44 prison and jail diversion programs matched to a control group consisting of regular supervision probationers (Lowenkamp, Latessa & Holsinger, 2006). Weighted least squares regression found the intensive supervision jail diversion programs were “associated with a substantial increase in recidivism” (p. 85). However, when parsed by risk score, higher risk offenders who attended multiple programs of longer duration achieved significant reductions in recidivism while lower risk offenders who attended multiple programs of longer duration experienced an overall increase in recidivism. “Placing offenders who are lower risk in structured programs (whether treatment or supervision oriented) clearly demonstrates that recidivism can actually be increased” (Lowenkamp et al., 2006, p. 89). An examination of results from seven other studies found low-risk offenders showed no improvement or worse outcomes when subjected to intensive programming (Andrews, Bonta, & Hoge, 1990).

On the other hand, a meta-analysis of 225 studies found positive effects from programming with low-risk offenders but at half the level of effects achieved with high-risk offenders (Andrews & Dowden, 2006). Programs for all types of criminogenic needs except substance abuse treatment, when delivered to high risk offenders, demonstrated higher mean effect sizes than the same programs when delivered to low risk offenders (Andrews & Dowden, 2006). The evidence suggests risk should be considered when deciding how best to address offender supervision requirements for achieving the best outcomes. Over-supervising low-risk offenders at best leads to minimal improvement in outcomes and under other circumstances can lead to worse outcomes.

Attention next turns to need, the second component of the RNR model of offender change. Need refers primarily to the changeable criminogenic offender attributes associated with offending. The first section is a review of the literature involving the effectiveness of programs that seek to change criminogenic attributes to reduce re-offending.

Need

Corrections agencies such as Georgia parole that adhere to the RNR model, assign parolees to certain programs in the belief that behavior change can occur, thus lowering the rate of re-offending (Andrews & Bonta, 2010, p. 58-59). RNR suggests a number of deficits such as faulty thinking, substance use, and mental illness are all associated with increased risk for re-offending and; therefore, offenders should be provided with appropriate programming to address these needs. Studies have examined mental health, substance abuse, and thinking deficits as they relate to offending.

For example, Kirk (1976) investigated the outcomes for all community members of mental health treatment programs focusing on hospital re-admissions. Mental health programs that adjusted the number of outpatient visits to the level of mental illness experienced fewer re-admissions. The key to the reduction was a significant increase in the number of outpatient clinic visits for the patients with higher levels of mental illness. As the number of visits increased the number of re-hospitalizations decreased. Re-admissions were 33% for patients who had many clinic visits versus 50% for those who had fewer visits (Kirk, 1976). Conversely, in line with the risk principle, re-admissions for less chronic patients did not vary as the number of clinic visits increased.

Similarly, Banks and Gottfredson (2003) investigated the effectiveness of drug treatment delivered to 138 probationers and parolees who had been randomly assigned to a drug court either through diversion or probation. This study was a two-year follow-up focused only on the differences in successes and failures among drug court participants and did not evaluate the control group. A total of 42 completed the drug court program while 92 failed. Treatment included any of a number of options (intensive outpatient care, methadone maintenance, inpatient treatment, and transitional housing). Supervision and treatment were coded as dichotomous variables; therefore, the number of each was not considered in the analysis. A series of seven models was tested using Cox regression. Six of the seven models included the treatment variable. Treatment was significant for longer survival times in every model (sig = .016 or smaller; Exp(B) = .183 to .342).

Another study evaluated an intensive community treatment program (n = 53) addressing several criminogenic needs (anger management, criminal thinking, and substance abuse using a cognitive-behavioral approach) (Bonta et al., 2000). Risk was determined by LSI-R scores. High-risk offenders attending the program achieved significantly lower recidivism rates (31%) than high-risk offenders in the control group (51%) (Bonta et al., 2000). Moreover, the opposite was also found ($p < .01$) where low-risk offenders who participated in treatment had higher recidivism rates than non-participants.

Turning to problem solving/thinking skill programs, research has investigated the effectiveness of programs targeting the broad area called ‘cognitive’ skills. An evaluation of a program (Reasoning and Rehabilitation [R&R]) teaching problem solving, anger management, and social skills involved 62 high-risk probationers randomly assigned to no program (n = 23), life skills (n = 17) or thinking skills (Ross, Fabiano, & Ewles, 1988). The dependent variable was conviction for a new offense during the nine-month follow-up period. The results found reconvictions for 70% who received no programming, 48% who received life skills training, and 18% who attended R&R (Ross, Fabiano, & Ewles, 1988).

However, a much larger study (n = 468) of the same curriculum with parolees also using random assignment found a non-significant but still higher supervision completion rate for program participants. Importantly, parolees who completed the program had significantly higher parole completion and employment rates than both non-completers and controls (Van Voorhis, Spruance, Ritchey, Listwan & Seabrook, 2004). However, Tong and Farrington (2008) conducted a meta-analysis of 15 R&R programs across several countries which included the programs reviewed here. Overall, these authors found R&R was associated with a 14% reduction in re-convictions. The authors note that it is more effective in smaller programs.

Similar to the review by Van Voorhis et al. (2004), for the most part in the U.S. other evaluations do not appear to have effects significant enough to clearly find benefits for these types of programs. For example, Allen, MacKenzie, and Hickman (2001) reviewed evaluations of two programs designed to change offenders' antisocial thinking, improve problem-solving skills, advance moral reasoning to a higher level, and teach social skills. Fourteen evaluations of Moral Reconation Therapy (MRT) "seemed to support the proposed beneficial influence of MRT on moral development and criminal behavior" (p. 502) when applied to drunk drivers, felony drug offenders, and general felony offenders (Allen et al., 2001). Most of the published evaluations reviewed were conducted by the program authors on the same samples, reporting recidivism over additional lengths of time. Eight evaluations of the second program, R&R, which included Van Voorhis et al. (2004) "tended to support the beneficial influence of the R&R program in reducing recidivism" (Allen et al., 2001, p. 507). Lastly, a meta-analysis of 20 studies, some of which are included in the Allen et al. (2001) analysis concluded – more definitively - that cognitive-behavioral treatment programs were significant in reducing re-offending (Wilson, Bouffard, & MacKenzie, 2005). This review of offender programs concludes with a meta-analysis of offender programming in general.

Andrews, Zinger, Hoge, Bonta, Gendreau, and Cullen, (1990) conducted a meta-analysis of community-based programs of all types compiling 154 correlations with phi coefficients. Types of treatment were categorized (criminal sanctions, inappropriate service, unspecified service, and appropriate service). Appropriate service was defined as following the principles of RNR. Type of service was strongly and significantly associated with program effects. The mean phi coefficient for appropriate services was strongest ($\varphi = .30$, $n = 54$) and significantly greater ($p < .05$) than all of the other types of service.

In summary, a number of studies have shown that programs addressing criminal/antisocial thinking, drug abuse, mental illness, and other risk factors can reduce re-offending. In line with RNR, there is also evidence that the amount of reduction in re-offending increases as programs target higher risk offenders and increase the level of program duration and intensity (Bonta et al., 2000; Kirk, 1976). None of these studies has assessed whether the effects of program attendance are influenced by variation in neighborhood structural conditions related to poverty, mobility, and heterogeneity. That is, the effects of programming may shield offenders from the effects of neighborhood conditions, thus further improving outcomes.

Responsivity

According to RNR, responsivity refers primarily to the approach and manner in which programs are delivered to offenders answering the question – How to treat? (Andrews & Bonta, 2010, p. 309). Responsivity to the unique needs of offenders is assumed. Beyond addressing general learning style and its components, responsivity is the "least developed of the three core principles" (p. 8) of the RNR model.

Responsivity is regarded as "theoretically unsophisticated: a catch all category" (Polaschek, 2009, p. 8). Kennedy (2015) describes responsivity as a mostly ignored area of study despite the knowledge that responsivity and offender motivation are vital to treatment success. Van Voorhis (2009) laments that "responsivity is seldom incorporated into correctional treatment or evaluation of correctional programs" (p. 137) and, thus may mask any treatment effects that might be present. "We repeatedly hear of programs that 'failed' when in fact they probably succeeded with certain types of offenders and failed with others" (p. 137). Thus, the research literature is largely silent related to responsivity.

Responsivity types are identified as general and specific (Andrews & Bonta, 2010, p.49-50). The most common example of general responsivity is the use of cognitive-behavioral techniques in program delivery as the most effective teaching method for all humans (Andrews & Bonta, 2010, p. 49-50; Andrews et al., 1990; Antonowicz & Ross, 1994; Goggin & Gendreau, 2006; MacKenzie, 2000). Specific responsivity addresses factors unique to individuals that inhibit program effectiveness. A common example is denial of a problem such as drug addiction resulting in resistance to participation in programs. Motivational interviewing (MI) is a brief "client-centered, directive method for enhancing intrinsic motivation to change by exploring and resolving ambivalence" (Vasilaki, Hosier, & Cox, 2006). While MI was found to significantly reduce alcohol consumption (Vasilaki et al., 2006) and improve motivation and program outcomes (Hettema, Steele, & Miller, 2005), MI demonstrated no significant change in supervision when used by probation officers with probationers (Walters, Vader, Nguyen, & Harris, 2010).

In summary, research has investigated only the effects of cognitive-behavioral approaches to program delivery and consistently finds significant effects. Research on attributes associated with specific responsivity such as resistance to change and personality find increased re-offending when not properly addressed. Motivational interviewing, at least in a professional counseling setting, is effective in addressing treatment resistance. Unfortunately, examination of the effects of responsivity requires detailed information on the context of programs and the learning styles of offenders and this is generally not available for large-scale studies.

While studies of offender needs, availability of programs, and level of program attendance have been central to the investigation of the RNR model, this research has neglected to examine the broader context in which programming and attendance at programs occurs. The next section provides a general review of findings related to neighborhood control and offending.

Social Disorganization Theory and Neighborhood Crime

Social disorganization theory posits that certain neighborhood structural conditions, namely poverty, mobility, and racial/ethnic heterogeneity are associated with increasing crime. Residents in such neighborhoods may be impeded from getting to know each other. Lack of communication makes it more difficult to agree on norms for acceptable behavior and how to enforce community norms through informal social control over residents and others in the neighborhood. Many studies on social disorganization have examined the effects of neighborhood structural conditions on crime and delinquency. Most of this literature has found strong positive effects for poverty (Arthur, 1991; Beasley & Antunes, 1974; Browning, Feinberg,

& Deitz, 2004; Danzinger, 1976; Gordon, 1967; Hannon, 2005; Kovandzic, Vieraitis, & Yeisley, 1998; Kubrin & Herting, 2003; Land, McCall, & Cohen, 1990; Lauritsen, 2001; Liberman & Smith, 1986; Liska, Logan, & Bellair, 1998; McNulty & Holloway, 2000; Mears & Bhati, 2006; Messner, Baumer, & Rosenfeld, 2004; Morenoff, Sampson, & Raudenbush, 2001; Reiss & Rhodes, 1961; Sampson, 1985; Sampson & Groves, 1989; Sampson & Raudenbush, 1999; Schuerman & Kobrin, 1986; Shaw & McKay, 1969; Simcha-Fagan & Schwartz, 1986; Taylor & Covington, 1988; Warner & Pierce, 1993) and mobility (Crutchfield, Geerken, & Gove, 1982; Lauritsen, 2001; Patterson,1991; NcNulty & Holloway, 2000; Sampson & Groves, 1989; Sampson & Raudenbush, 1999; Schuerman & Kobrin, 1986; Smith & Jarjoura, 1988; Sun, Triplett, & Gainey, 2004; Taylor & Covington, 1988).

Effects for heterogeneity are more mixed. For example, Bursik (1986); Browning, Feinberg, and Deitz (2004); Byrne (1986); Hansmann and Quigley (1982); Markowitz, Bellair, Liska, and Liu (2001); Mears and Bhati (2006); Morenoff, Sampson, and Raudenbush (2001); Rountree, Land, and Miethe (1994); Sampson (1985); Schuerman and Kobrin (1986); Sun, Triplett, & Gainey (2004); and Wadsworth and Kubrin (2004) have all found positive effects of heterogeneity on crime. Alternatively, Hipp (2010) found no relationship or a negative relationship between heterogeneity and crime. A mix of effects/no effects was found when crime was the dependent variable (Lauritsen, 2001) or depending on the source of the dependent variable (Morenoff, Sampson, & Raudenbush (2001). Still other research on heterogeneity is more of a mixed bag with no effects (Browning, Feinberg, & Deitz, 2004; Patterson, 1991), effects for some but not other crimes (Bellair, 1997; Smith & Jarjoura, 1988; Warner & Pierce, 1993), and negative effects (Sampson & Raudenbush, 1999; Velez, 2001).

Therefore, on the whole, as predicted by social disorganization theory, various measures of poverty/disadvantage and neighborhood mobility predict variation in crime rates across neighborhoods while measures of heterogeneity have been less consistent in predicting neighborhood crime rates. The more important question for this dissertation is whether research predicting the effects of neighborhood structural conditions on neighborhood crime rates can be applied to specific offender groups such as parolees.

Social Disorganization Theory and Offender Supervision Outcomes

While there is a substantial body of research investigating the relationship between neighborhood structural conditions and specific crimes or crime rates in general, research is more limited related to the effects, if any, of neighborhood structural conditions on offender supervision outcomes. One of the first studies in this area involved 500 offenders released beginning in 1978 to 90 randomly selected neighborhoods in Baltimore (Gottfredson & Taylor, 1986). Blocks were coded for environmental (building types, appearance, land use, and social climate) features that were empirically or theoretically associated with crime. Dependent variables included success or failure under supervision, the seriousness of any new crimes committed under supervision, and time free.

A regression analysis found individual level variables were significant in explaining variance in recidivism. By itself, environment was not significant but an interaction term (environment x offender[3]) was significant for both crime seriousness and time to arrest

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[3] The "offender" variable was created using "clusters" of offender characteristics (representing criminal history, social history, financial need, and dependency). A footnote indicated a more detailed description of what is meant by "cluster" could be found in Gottfredson and Taylor, 1982. That citation could not be located.

(Gottfredson & Taylor, 1986). Overall, an offender's arrest varied based on the offender's characteristics and the type of environment where the offender lived. Whereas, higher risk offenders were more likely to be arrested in "bad" (p.148) environments and less likely to be arrested in better environments; lower risk offenders were less likely to be arrested in bad environments and MORE likely to be arrested in good environments[4] (Gottfredson & Taylor, 1986). The authors explain this finding by suggesting that high-risk offenders may be known to and under greater surveillance by law enforcement in bad environments, thus leading to more arrests. While high-risk offenders' arrest rates may be "lower" in better neighborhoods they are still high (Gottfredson & Taylor, 1986). These authors further suggest the increase in arrest rates among low-risk offenders in better-off neighborhoods may not be due to police surveillance but rather to their being known to and watched more closely by neighborhood residents. In better neighborhoods, low-risk offenders come under greater scrutiny and surveillance by residents who observe and report misdeeds to the police at greater rates. The likelihood of arrest across the interaction term varied by as much as 13% (Gottfredson & Taylor, 1986).

Kubrin and Stewart (2006) provide one of the earliest investigations of the effects of poverty/disadvantage on offender outcomes. This research involved 4,630 probationers and parolees residing in 156 neighborhoods across one urban county during the year 2000. Disadvantage (a composite of three variables) and an index of concentration at the extremes (ICE) were calculated using census variables at the tract level. Individual level factors (race, age,

[4] Similar to footnote 3, the reader is referred to Gottfredson and Taylor (1982) for a more in-depth explanation of how risk was determined. This study (p.139) lists criminal and social histories, current offense, demographic characteristics and performance after release were chosen to measure risk due to their ability to predict recidivism.

supervision level, whether a property or drug offender) predicted just over half the variance in recidivism. Both disadvantage and ICE were also significant in predicting recidivism. The predicted probability of recidivism ranged from 42% in less disadvantaged neighborhoods to 60% in more disadvantaged areas. Recidivism varied from 33% to 54% using the ICE variable. This research did not include measures related to supervision activity such as program participation.

Similar to the mixed findings listed earlier in this chapter, research on community conditions and offenders has also been met with mixed results. For example, research using county rather than census tract for neighborhood found that residential stability had an inverse effect on re-conviction rates for 5,027 offenders released from prison while no effect was found for concentrated disadvantage or immigrant concentration (Tillyer & Vose, 2011). The authors suggest that the lack of significance for measures of disadvantage and immigrant concentration might be due to the dependent variable not accounting for unprosecuted re-offending. Moreover, using county as a proxy for neighborhood may be too large to measure conditions in specific neighborhoods where offenders reside (Tillyer & Vose, 2011).

Wehrman (2010) found concentrated disadvantage (composite of five U.S. Census variables) was not significant in predicting felony reconviction for 1,515 offenders across 546 census tracts in one Michigan county. Wehrman suggested that using reconviction as the dependent variable may not be sensitive enough to the true new offense rate. Moreover, compared to the overall U.S. population, Wehrman described his cohort as much higher in level of poverty (9% across the U.S. versus 24% in the sample). The higher level of disadvantage may also have affected the findings. In a third example of mixed findings, using Cox regression, Chamberlain and Wallace (2016) also found that concentrated disadvantage had no effect on the re-arrest, reconviction, or re-incarceration for 31,191 inmates released on parole in three large cities in Ohio. On the other hand, stability had a significant and predicted downward effect on the outcome variables (Chamberlain & Wallace, 2016). Finally, while investigating parolee residential moves and proximity to each other, Stahler, Mennis, Belenko, Welsh, Hiller, & Zajac, (2013) found concentrated disadvantage had no effect on the recidivism of 5,354 inmates released between 2002 and 2006 and living in Philadelphia.

Morenoff and Harding (2011) expanded research on the causes of offender recidivism by examining if and how the employment and recidivism of parolees released to supervision in 2003 were affected by neighborhood conditions. The cohort consisted of 11,064 parolees across Michigan from which a random 1/6 sample (1,848) was used for additional analysis. Variables included a measure of employment and U.S. Census data at the tract level. Dependent variables were five measures of recidivism (new offense arrest, return to prison for technical violations, return to prison for new conviction, absconding, and new felony conviction regardless of incarceration). Using Cox regression, residential stability was found to predict return to prison for both a new offense and absconding (as stability increased return to prison for a new offense and absconding decreased) but the coefficients were small. Similarly, as neighborhood affluence increased the rates of both technical violations and absconding decreased (Morenoff & Harding, 2011). While this research found modest effects of certain neighborhood conditions on parolees, it leaves unanswered questions about the effects of program participation.

Taken as a whole, there is simply not much research related to how neighborhood conditions affect probation and parole outcomes. The two strongest studies (Kubrin and Stewart, 2006; Morenoff and Harding, 2011) provide a case for neighborhood structural conditions as posited by social disorganization theory. Other studies do not affirm the theory, have weak effect sizes, or provide explanations why the research design may miss neighborhood effects.

Variation Across Communities in Services, Amenities, and Employment

One important aspect of communities less frequently investigated is the level of programs and services that may be needed by parolees. Neighborhood services related to health, education, drug, mental health, employment, and other services have been investigated and found to vary across communities. Pearce, Witten, Hiscock, and Blakely (2006) found travel times to health and education related resources in the least deprived neighborhoods exceeded travel times in the most deprived neighborhoods. Allard, Tolman, and Rosen (2003) found the distance was shorter to drug treatment and mental health services for African-American welfare recipients than for whites. Another study investigating the availability of drug and mental services in high poverty neighborhoods found the mean number of service providers in large central cities higher than in suburban tracts with the same level of poverty and the number of service providers increased as the level of poverty increased (Allard, 2004). However, distance to services in suburban areas increased as the level of poverty increased. Although the number of services was higher in central cities, the higher density of low-income residents in central cities called into question the capacity to provide services (Allard, 2004).

Slocum, Rengifo, Choi, and Herrmann (2013) investigated the effect of service availability on neighborhood violent and property crime rates. Among nine types of organizations, a combination of bridging organizations and organizations that promote family well-being was the only category significantly associated with reductions in both crime types

(Slocum et al., 2013). Bridging organizations might be able to marshal additional resources for isolated and socially weak communities (Slocum et al., 2013). Mobility had no effect and disadvantage had almost no effect on either crime type. The empirical literature review next turns from general effects on crime to what is known more specifically about offender supervision outcomes, community resources, and community conditions.

Chamberlain, Boggess, and Powers (2016) investigated the relationship between recidivism and the locations of employment opportunities for parolees, hypothesizing that more jobs closer to parolee residences would translate into reductions in recidivism measured as returns to prison. The study involved 31,000 inmates released to parole between 2000 and 2009 in three large cities in Ohio. Time under supervision averaged 3.5 years. Variables included census information related to neighborhood characteristics and jobs at the block group level. Cox proportional hazard analyses examining neighborhood conditions found only residential stability consistently related to a reduction in recidivism. An analysis of job opportunities, job types, and job wages within two, five, and ten miles found an increasing likelihood of recidivating regardless of distance, type of job, or wage. Chamberlain et al. (2016) suggested in good times parolees may not feel a commitment to employment, especially low paying jobs. Moreover, familiarity with close-by areas may present too many opportunities for crime and, therefore, parolees may have to travel even further to reduce crime propensity.

Hipp, Petersilia, and Turner's (2010) study is one of the few investigations of the relationship between parolees' neighborhoods and the distance to services for drug treatment and other needs. Proximity to services raises questions related to the conditions in neighborhoods where parolees reside. Research data consisted of geocoded addresses for 227,000 parolees released to supervision in California during 2005 and 2006, a total of 6,015 service providers (housing, networking, education, general social services, etc.) and their locations, the distance between each parolee home address and service provider, and U.S. Census tract variables for residential stability, concentrated disadvantage, and racial/ethnic heterogeneity. The analysis, which used Cox regression, found a powerful recidivism reducing effect associated with service providers within two miles of a parolee's home. A one standard deviation increase in the number of service providers reduced the likelihood of returning to prison by 41%. Only concentrated disadvantage was significant in increasing recidivism in the parolee's home census tract and in the adjacent tract. While proximity to services may be a critical component of offender success under supervision, it should be noted that the research did not include whether parolees were enrolled in or attended any programs.

One more analysis is noted related to the differential effects of program availability on black and white parolees depending on the level of concentrated disadvantage in the tract. In tracts two standard deviations above the mean for concentrated disadvantage, white parolees were 30.8% more likely to recidivate and African American parolees were 47.2% more likely to recidivate compared to parolees in tracts at the average level of disadvantage. Services had a particularly strong protective effect on Black parolees where "seven service providers nearby have the same risk of recidivism as a white parolee with no service providers nearby" (Hipp et al., 2010, p. 968-969). The strong protective effect for African Americans led the authors to suggest placing greater numbers of service providers near parolees in the hope that it might eliminate differences in recidivism between white and African American offenders (Hipp et al., 2010).

While the primary focus of this research is on how parolees are affected by community and nearby levels of concentrated disadvantage and mobility, another finding is important for this research. The authors found in one analysis that an expected positive effect of residential stability had disappeared. Further investigation found that residential stability was negatively correlated ($r = -.34$) with the number of programs in the tract. In summary, overall Hipp et al. (2010) found concentrated disadvantage highly significant for predicting recidivism as was proximity to service providers and the number of service providers nearby.

The last study (Hipp, Jannetta, Shah, & Turner, 2011) expands on the analysis of the relationship between offender attributes and service provider location and capacity using the same statewide data as Hipp et al. (2010). The emphasis is on understanding which types of service providers are in close proximity to parolees. Thirteen broad categories of service providers were consolidated into four groups (social services, self-sufficiency, family and housing, linking with the community) and a variable calculated for the number of each service type within two miles of each parolee. A calculation also determined "potential demand" (p. 111) for services by each parolee. Although the number of services within two miles for African American parolees was higher than for white parolees, the estimate for total potential service demand in areas where African American parolees resided was 65% higher and 28% higher for Latino parolees than for white parolees. Minority parolees were closer to more service providers but clustered more closely together, potentially overtaxing available services. The analysis also included violent and property offenders as separate independent measures of risk. The analysis found each additional violent crime offender was associated with a decrease in the number of social services within two miles by 2.2% and by 2.1% for each additional property crime offender. Social services included drug and mental health counseling (Hipp et al., 2011).

In conclusion, in one state a mixed picture emerges as to where parolees live in relation to services nearby and the capacity of services to meet local demand. This research brings attention to important policy questions but does not answer questions about the actual use of services or the impact of such use. The availability of services may be irrelevant if parolees have no desire to address alcohol, drug or mental health problems associated with re-offending or if agents providing supervision do not refer parolees to service providers and monitor attendance. The question remains as to the relationship between community conditions and parolee program attendance in real world settings.

The Challenge of Parole Reentry

Parolees present a unique set of circumstances and challenges when it comes to succeeding in the community. Probation sentences generally allow offenders to remain in the community, maintain family and work relationships, and retain other relationships established across the community. Probationers' ties to the community are maintained. However, by definition offenders on parole are returning to a community after serving months or more often years in prison. While serving time in prison, inmates' families may move, jobs are lost and employment gaps created, job skills may weaken or become obsolete, and community ties weaken or disappear altogether. Prison inmates who are granted parole are required to secure a residence before release. Release residences are often with family members and many times only temporarily to allow the release from prison to go forward. This first residence may be in a neighborhood or city unfamiliar to the new parolee. Other prison inmates may be released to temporary housing in an unfamiliar area. Access to transportation is another concern. In summary, unlike probationers and others living in the community, prison inmates released to parole are likely to have few if any resources and weak or no personal support system.

For all these reasons, the availability and proximity of social services are especially important to parolee success under supervision. Weak or broken personal networks point to the need for the right services nearby for mental health and drug treatment services, employment opportunities, and other support structures such as offered by churches and community organizations. This research focuses on the effects of the proximity and availability of the three most frequently used services, mental health and drug treatment, and thinking skills training.

Conclusion

Offender programming following the principles of RNR has a significant positive effect on supervision outcomes. In particular, application of the risk principle leads to identifying offenders most likely to return to prison and, therefore, in greatest need of programming to address attributes associated with re-offending. Application of the risk principle also leads to high risk offenders attending programs of longer duration. When applied correctly, the need and responsivity principles lead to targeting criminogenic risk factors and delivering programs using cognitive-behavioral techniques. Despite supervision that follows the principles of RNR, significant variation is still found in offender outcomes across geographic areas. Empirical research has found that social disorganization theory explains variation in crime and re-offending rates across geographic areas over and above individual level attributes. Moreover, a positive relationship exists between parolee residence and the presence of program providers nearby.

This study combines measures related to RNR, specifically, the total number of three types of offender programs in the community with measures of neighborhood conditions and offender personal attributes to explore the combined and interactive effects on parolee outcomes. However, unlike previous studies, this research also includes a measure of actual program attendance to better understand if and how variation in program participation may moderate the effects of neighborhood conditions on parolee outcomes. If research related to RNR has found that reoffending rates can be reduced when the level of program participation increases commensurate with the offender's risk score, it may also be true that program participation moderates the effects of neighborhood conditions. This study furthers past research by including the number of program attendances to investigate this question. It may be that additional program attendance is necessary for high risk parolees in highly disadvantaged neighborhoods in order to maintain lower levels of violations and re-offending. Thus, this dissertation moves the empirical research forward on the causes of re-offending among parolees by investigating the effects of program participation, program availability, and neighborhood conditions related to social disorganization theory on five measures of success under supervision. The next chapter describes specific hypotheses to be tested, the setting, data, and empirical approach.

CHAPTER IV: DATA AND METHODOLOGY

Research Hypotheses

The goal of this dissertation is to investigate the effects of neighborhood conditions on program availability and program attendance, and the effects of program attendance on the relationship between neighborhood conditions and parole supervision outcomes. Figure 2.1 (p. 28) depicts the theoretical model to be examined. Using a multilevel approach, seven hypotheses are tested. The key variables of interest which move the research forward are measures of neighborhood program availability and parolee program attendance in relation to neighborhood structural conditions.

Tests of social disorganization theory find that variation in crime rates across neighborhoods is associated with neighborhood structural conditions (mobility, heterogeneity, and economic conditions). While evidence from individual level studies suggests that program attendance improves offender supervision outcomes, the literature is silent on whether program attendance may ameliorate the negative effects of neighborhood conditions on supervision outcomes. Relatedly, the literature is mixed on the effects of neighborhood conditions on program availability and how availability affects program attendance.

This research first examines questions related to individual risk factors, program attendance, individual level risk factors, and re-incarceration and other outcomes. Therefore,

> *H1* Individual level risk factors will affect parole outcomes; that is, as age and education increase, re-incarceration and other outcomes will decrease; as risk score increases, re-incarceration and other outcomes will increase; and, race (black) and being male will be associated with an increase in re-incarceration and other outcomes.

RNR holds that program attendance improves outcomes. Therefore,

H2: Program attendance will have an inverse effect on all parolee outcomes; that is, as the number of program attendances increases, the likelihood of re-incarceration and other outcomes will decrease.

Research has shown that certain individual level attributes of offenders associated with improved or worsening outcomes should be accounted for in studies of recidivism. This research examines how these effects may be moderated by program attendance. Thus;

H3: Program attendance will moderate the effects of individual risk factors on parolee outcomes; that is, as the number of program attendances increases and age and education increase, re-incarceration and other outcomes will decrease more than would be expected with age and education alone; conversely, as the number of program attendances increases and risk increases, re-incarceration and other outcomes will increase less than would be expected with risk score alone; and finally, as the number of program attendances increases for black and male offenders, re-incarceration and other outcomes will increase less than would be expected.

Finally, related to the relationship between individual risk factors and program attendance,

H4: Individual level risk factors will affect program attendance; that is, as age and education increase, program attendance will increase; as risk score increases, program attendance will decrease; and, race (black) and being male will be associated with a decrease in program attendance.

This research then examines the relationships between neighborhood conditions and programs, program attendance, and re-incarceration and other outcomes. Therefore, *H5:* Neighborhood structural conditions will have a positive effect on supervision outcomes; that is, increasing levels of mobility, heterogeneity, and disadvantage will likewise, increase the likelihood that parolees will have positive drug screens, technical violations, technical violation arrests, and felony arrests, and revocations.

The research literature highlighted here, at best, offers ambiguous findings related to if or how levels of mobility, heterogeneity, and disadvantage affect the availability of programs across neighborhoods. Although neighborhood conditions will have a negative effect on program attendance through the number of programs, parolees who do attend programs will be less affected by neighborhood conditions. Therefore,

H6: Program attendance will moderate the effects of neighborhood structural conditions on all dependent variables; that is, for high risk parolees who attend programs, the effects of mobility, ethnic heterogeneity, and disadvantage on re-incarceration and other outcomes will diminish as the number of attendances increases.

Some research indicates that increasing levels of neighborhood disadvantage are associated with services closer to where clients live or are associated with increased numbers of some types of services (Allard, 2004; Allard et al., 2003; Pearce et al., 2006). In contrast, Hipp et al. (2010) found that increasing mobility in census tracts decreases the number of programs in the tract. The next two hypotheses address this question by extending the examination of neighborhood influence through analyzing neighborhood effects on the number of programs and program attendances.

H7a: Neighborhood structural conditions will be associated with a decrease in program attendance but this effect will be mediated through a decrease in the number of programs;

that is, changes in mobility, ethnic heterogeneity, and disadvantage will have a significant negative effect on the number of programs which will have a significant negative effect on the number of program attendances.

H7b Neighborhood structural conditions will influence parolee outcomes but this effect will be mediated through the number of programs and program attendance; that is, change in the levels of mobility, ethnic heterogeneity, and disadvantage will have a significant effect on the number of programs which in turn will have a significant effect on the number of program attendances which will significantly affect re-incarceration and other outcomes.

The discussion now turns to the site of the current study, the sample from which the data is drawn, data and units of analysis, and measures used to test hypotheses. Lastly, the analytical plan is described.

Research Sample

The sample used in this research is a subset of parolees originally studied in a National Institute of Justice (NIJ) funded research project on parole supervision. The NIJ project file is a supervision 'discharge' population, that is, prison inmates released to discretionary parole who ended community supervision for any reason including sentence end, revocation for technical violations or a new crime, or sentence commutation by the parole board during calendar years 2011, 2012, or 2013. Where an offender had two parole episodes that ended over the three-year time period, only the most recent parole episode is included in the NIJ project file. Parolees released to an out-of-state address are excluded.

Additional criteria were applied for inclusion in the research sample. First, release from prison to parole had to be on or after January 1, 2008. Second, at the time of release from prison, parolees in the sample were required to have at least 365 days remaining on the sentence. However, parole could have ended prior to 365 days due to parole revocation. Third, parolees included in the research sample were required to have their first address at release from prison in one of Georgia's six core urban areas of metropolitan Atlanta (cities of Atlanta, East Point, and College Park in Fulton County; and all of DeKalb and Clayton counties), and the cities of Albany, Augusta, Columbus, Macon, and Savannah. A total of 5,333 parolees met these inclusion criteria.

One additional step was required to arrive at the final sample total. While the effects of neighborhood conditions on programs and parolee outcomes are subjects of this research, the research sample required further refinement by assigning parolees living in the target areas to census tracts. Address information included the date that the residence was established, street address, city, and ZIP code. It was expected that home addresses would, in general, be accurate since supervising parole officers and others would need accurate information to locate parolees, especially in emergencies. Some initial address clean-up was required such as correcting city names and removing extraneous information from street address fields.

The primary problem in assigning census tracts turned out to be inaccurate ZIP codes. The U.S. Postal Service ZIP code on-line look-up tool was used to assign correct ZIP codes. Each parolee's first address was flagged and assigned to a census tract through a mapping tool

(Maptitude 2014[5]). The majority of census tract assignment failures required ZIP code correction. Only nine addresses could not be assigned to a census tract. A total of 5,324 of 5,333 unique parolees were assigned to 554 census tracts in the target areas, with the number residing in each tract from one to 72 parolees.

Since this study is considering neighborhood effects, clusters of parolees will share the characteristics of the census tracts in which they reside. Research related to multilevel analysis has investigated both the number of the units (ex. people) in each group and the number of groups (ex. census tracts) (Maas & Hox, 2005; Moineddin, Matheson, & Glazer, 2007). This research recommends at least 50 groups and 50 in each group but emphasizes that more important is the number of groups (Maas & Hox, 2005; Scherbaum & Ferreter, 2009) and the number in each group may be fewer as the number of groups increases (Woltman, Feldstein, MacKay, & Rocchi, 2012). A number of multilevel studies have successfully used groups of 74 (Slocum et al., 2013), 45 (Wyant. 2008), and three (prisons with large number of inmates) (Woolridge, Griffin & Pratt, 2001). As for the number in each group, Yuan and McNeeley (2016) used census tracts with 21 to 100 in each tract. In order to minimize problems with estimation, census tracts in this study were required to have a minimum of 19 parolees. The final sample totals 1,637 parolees in 65 census tracts ranging from 19 to 72 parolees.

Unit of Analysis

A multilevel design is used in this study to test the effects of both individual level variables and neighborhood conditions on parolees clustered within U.S. Census tracts. The first

[5] Maptitude 2014 software is used to map geographic data. Maptitude uses street address and ZIP code to assign a U.S. census tract to each address. https://www.caliper.com/maptitude/mappingsoftware.htm

level of analysis examines individual level variables. The second level examines neighborhood conditions and the number of programs available within the neighborhood. Census tract is the geographic area used for neighborhood. The U.S. Census Bureau describes census tracts as small subdivisions of counties and other entities. Census tracts typically include 1,200 to 8,000 people with an average of 4,000.

Census tracts as well as smaller and larger ecological units have been used successfully in neighborhood research. Sampson, Morenoff, and Gannon-Rowley (2002) state that "virtually all studies of neighborhoods we assess rely on geographic boundaries defined by the Census Bureau or other administrative agencies" (p. 445) including census tracts. Sampson et al. (2002) compared 40 studies, 17 of which used census tract to identify 'neighborhood.' Measures of neighborhood conditions using data by census tract were significantly associated with rates of crime (Sampson et al., 2002). Other studies using census tract to measure the effects of neighborhood structural conditions on crime include Boardman et al. (2001), Hannon (2005); Lauritsen (2001); Mears and Bhati (2006); Peterson, Krivo, and Harris (2000); and Sun, Triplett, and Gainey (2004). More specific to the focus of this research, studies have used census tract to investigate community conditions and offenders under community supervision (Hipp et al., 2010; Hipp & Yates, 2009; Kubrin & Stewart, 2006; Morenoff & Harding, 2011). The next sections describe data sources and measures used in the study.

Data Sources

Data for this study come from five sources: 1) NIJ project file; 2) Georgia Board of Pardons and Parole's case management supervision file tables containing home addresses, program attendance records, and program vendor information; 3) Georgia Alcoholics

Anonymous (AA); 4) Georgia Narcotics Anonymous (NA); and, 5) 2010 U.S. Census Bureau American Community Survey Five Year Estimates. The *NIJ project file* is a combination of records from several sources merged to create one record for each offender. Department of Corrections' data includes demographic, medical, mental health, education, and institutional program information related to prison incarcerations, and felony probation supervision records.

Criminal history records are from the Georgia Crime Information Center (GCIC) which is the state's official criminal history repository. Finally, Parole Board case management system records include family history information and details of all supervision activity in the community. The measures of interest for this research from these data sources are a select subset of parolees living in urban areas, personal characteristics associated with risk to reoffend, and supervision outcomes extracted from the NIJ project file; the total number of program attendances; and home addresses and vendor locations used for assignment to census tracts.

The home address file, also from the Parole Board case management system, includes the parolee's residence address and residence start date established as the parolee moves from one residence to the next during supervision. The first address is used for assignment to a census tract. The *program attendance* file is documentation of every parolee program attendance. Specific measures include attendance dates, program type, and attendance type (attended, failed to attend, excused). Program areas of interest for this research are mental health (MH), thinking skills (COG), and drug treatment (SA). The case management *program vendor* file is a list of all organizations approved to provide treatment services for parolees across the state. The vendor list is maintained by parole agency program managers and includes the name of each vendor, program types delivered by each vendor, and address(es) where services are delivered.

Two other approved vendors in the case management system are *Alcoholics Anonymous (AA)* and *Narcotics Anonymous (NA).* Attendance at AA and NA meetings is recorded in the case management system as a program attendance but unlike formal treatment providers, AA and NA meeting addresses are not maintained in the case management system. AA and NA meeting location addresses in the study areas will be obtained from the regional and state offices of AA and NA and accompanying websites. The final data source is the *U.S. Census Bureau 2010 American Community Survey Five Year Estimates* for all census tracts in Georgia from which neighborhood variables related to poverty, residential mobility, and community racial makeup are drawn.

Measures

Dependent Variables

Outcome measures: The five outcome measures in the NIJ project file are also used in this study – *positive drug test, technical violation, technical violation arrest, new felony arrest, and parole revocation*. The review of risk prediction instruments in Chapter 3 finds re-arrest, re-incarceration, and re-conviction most frequently used as measures of offender outcomes. From a practical perspective, these measures are easiest to find in offender records. The inclusion of 'supervision' measures (i.e. related to compliance with technical conditions of supervision and determined to a large degree by the supervising parole officer) may provide new insights into the relationship between community conditions and routine supervision activities. Morenoff and Harding's (2011) research on parolees is one of the few studies found using specific dependent measures related to supervision activity – absconding and return to prison for a technical violation. As noted next, the variables used to determine parole success across the U.S. are described in general terms to facilitate the collection process.

The U.S. Bureau of Justice Statistics' (BJS) annual report on probation and parole identifies seven parole 'exit' types, three of which are relevant here (Kaeble & Bonczar, 2016). "Completion" is defined as "finished the term of supervision and not in custody." "Returned to custody" is defined as "with a new sentence, with revocation, or other/unknown." The third category is "other unsatisfactory" described as those who "failed to meet all conditions of supervision" (pg. 6). These general categories of supervision exits are indicative of the underlying difficulty in both collecting more nuanced measures and reaching agreement on more specific definitions for supervision completion or failure.[6]

Under the BJS definition, completions may include parolees who are unemployed, have one or more positive drug screens, are homeless, have failed to comply with requests from parole officials, or are non-compliant with numerous other technical conditions of supervision. Moreover, "returned to custody" may include offenders who were then immediately re-released to supervision. It is left up to each state to determine which parolees in the completion category should be in what might arguably be called the even more general "other unsatisfactory" category. Travis and Lawrence (2002) point out that differences among the states related to the conditions of release and how violations are determined make the task of agreeing on a standard nearly impossible. Dependent measures available for the NIJ project and used here span the BJS categories and include more nuanced interim measures of parolee compliance and success.

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[6] Based on personal communication between this author and officials at BJS who publish the annual report "Probation and Parole in the U.S."

One other measure where agreement has been elusive significantly influences supervision outcomes. The length of time on supervision also known as time-at-risk presents significant variation that makes it difficult to compare success and failure (Travis & Lawrence, 2002). Disagreement related to time under supervision is once again reflected in the BJS annual probation and parole reports which do not consider this factor at all when calculating exit types (Kaeble & Bonczar, 2016). Validation studies of risk instruments cited in Chapter 3 use follow-up periods of seven to 10 months (Hsu et al., 2009); 12 months (Fass et al., 2008; Manchak et al., 2008); 15 months (Simourd, 2004); 18 months (Lowenkamp et al., 2009); two years (Zhang et al., 2014); 31 months (Girard & Wormith, 2004) as well as time varying analyses (Andrews et al., 1986). Parolees in the present study were released from prison with at least one year remaining on the sentence. Half the sample has two years or less to sentence discharge. In order to retain a sufficient number of parolees for the analyses, the occurrence of each dependent measure is assessed over a 12-month time-period beginning the first day of parole.

Durose and Cooper (2014) suggest tracking offenders for as little as a year may miss a significant portion of re-arrests and re-incarcerations. However, that study tracked all prison releases while only active parolees are included in the present study. Perhaps more significant to the present study is an analysis conducted as part of an NIJ funded project on an earlier population of parolees from the same state (Meredith & Prevost, 2009). This study found many types of violations are ‘front-loaded,’ occurring during the first year of supervision. Forty-three percent of parolees committed one or more violations of any type during the first 12 months of supervision versus only three percent more or 46% of parolees over the first three years. Similarly, 33% had a positive drug screen in the first 12 months versus 40% over three years;

18% had technical violation arrests over the first 12 months versus 25% over three years; and for felony arrests 14% over 12 months versus 21% over three years. Finally, 11% had their parole revoked in the first 12 months versus 23% over three years. In summary, while additional numbers of parolees commit technical violations after the first year, the greatest numbers occur in the first year. Differences in felony arrests and revocations are greater and cause for more discussion as the analysis proceeds.

Since this research is interested in only the first year of supervision, variables in the NIJ project file are used which dichotomously code for whether each of the dependent measures occurred in the first 12 months of supervision (1 if the event occurred within 12 months of the parole begin date and, 0 otherwise). The presence of a *positive drug test* depends on several factors related to whether and when drug tests are conducted.[7] Parolees with no drug history may never be tested depending in part on the discretion of the parole officer. Otherwise, drug tests are performed when mandated as a condition of release for parolees with a drug use history, when there is some suspicion or evidence that the parolee may be using drugs, or when the parolee is selected among all parolees for monthly random drug testing.

A *technical violation* includes any of 18 rule or procedure violations (failing to follow instructions, work, attend a program, report to the parole officer, or pay fees or child support; not being truthful; not notifying the officer of an arrest; changing jobs without permission; and others). The presence of one or more technical violations that occur within the first year of supervision triggers this measure.

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[7] Knowledge of parole agency operations and procedures in this section comes from the author's former employment in that organization.

The next dependent measure, *technical violation arrest* represents whether or not there was an arrest for non-crime violations. Technical violations trigger an arrest depending on other factors such the parolee's risk score, length of time under supervision, number of previous violations, and employment status. Warrant officers in parole's central violations unit work closely with parole board members to implement violation and arrest policies that ensure consistency in the application of parole board arrest authority. Fair and consistent application of board policy is of particular importance in studies such as this involving several urban areas across the state. A technical violation arrest typically leads to incarceration in a local jail for only a few days and then release back to the community.

A f*elony crime arres*t represents an incarceration in a local jail for a new felony committed while on parole. Parole officials record all arrests in the case management system. The last dependent measure is *parole revocation,* which represents the end of supervision by order of the parole board. Parole revocation may occur as an administrative matter by the parole board after a conviction for a new crime in court or a parolee admits violations and voluntarily agrees to revocation by the parole board. Revocation may also occur after the parole board holds a formal parole revocation hearing. As with the other dependent measures, a revocation is coded as 1 and 0 otherwise. The next section describes the assignment of parolees to census tracts, followed by descriptions of key independent measures, the first of which (vendors) follows a similar process for assignment to census tracts.

Other dependent measures*:* Programs and program attendance are thought to be critical components of parolee success under supervision and key measures in this study. This section describes how the measures were ascertained for the number of programs and program attendances. As indicated under data sources, *program vendor* information for ‘formal’ programs is found in the case management vendor file. The measure for programs will be the combined *program total* of all COG, MH, and SA programs by census tract to test the combined effects of the number of programs. Each vendor’s address is listed in the vendor file. Vendors may be listed multiple times, once for each program type offered. Each entry is counted as a separate vendor. Therefore, a vendor at a single location delivering programs for COG, MH, and SA is counted as three programs. As ‘official’ data, vendor information including addresses is obtained directly from vendors, entered in the vendor file, and maintained by parole program managers. This process results in far fewer errors and should result in greater success assigning vendors to tracts. Vendor addresses will be assigned to census tracts using Maptitude 2014. One additional source of programs used in the study is described next.

As noted earlier, parolees may be assigned to attend AA or NA as alcohol/drug treatment (SA). During supervision, attendance at AA and NA is entered by the parole officer as an ‘SA’ program attendance. However, due to the number of AA and NA groups and meeting sites, meeting locations are not maintained in the case management system. Therefore, the main offices of AA and NA organizations in the target areas were contacted and their accompanying websites reviewed to find addresses for meetings.

AA and NA are unique in that different meeting groups form with different members and group leaders, which are said to create cohesiveness and a bond particular to that group. Different groups often meet at the same location but at different times of day or days of the week. While the location may be the same for several groups, the number of different groups may be indicative of the strength of community organization and the ability of communities to self-manage as suggested by social disorganization theory. For this reason, every AA and NA group is treated as a separate vendor and SA program. All AA and NA meeting group names and addresses are compiled in an Excel file, assigned to census tracts using Maptitude 2014, and assigned as SA vendors. The final step is merging AA/NA meeting groups assigned to tracts into the main file resulting in a combined total number of *MH, COG, and SA* programs for each tract. The next section addresses how parolee attendance totals are determined.

Parole officers record every *program Attendance* in the case management system where they are stored in the program attendance file. Each program attendance record includes attendance date, program type (COG, MH, SA) and sentence end date, and a code indicating successful attendance. Although program attendances could be aggregated in different ways, such as during the first or second half of the 12-month observation period, the program attendance measure for each parolee is the first-year combined total of attendances for the three program types. Program attendances beyond a year after the parole begin date are not counted. It should be noted that AA and NA attendance is entered in the attendance file and, therefore, is included in the attendance total.

Neighborhood Conditions

This section describes the steps to be taken to create measures of neighborhood structural conditions. All community measures are taken from the U.S. Census Bureau 2010 American Community Survey (ACS) Five Year Estimates. Census tract data comes from the 65 census tracts with 19 or more parolees. Census tract measures selected for this study are consistent with what has been used in other studies investigating social disorganization theory, crime, and offenders (Boardman et al., 2001; Kubrin & Stewart, 2006; Morenoff et al., 2001). Census variables will be analyzed with principle components factor analysis using varimax rotation to reduce the number of tract level variables and simplify the analytical models that use tract level data. It is expected that the factor for disadvantage will include: 1) percent with no high school diploma, 2) percent below the poverty line, 3) percent receiving food stamps, 4) percent of occupied housing that is rented, and 5) percent female headed households with children. *Proportion Black* in the census tract will measure heterogeneity and mobility will be represented by percent moved in the past five years.

Interaction Terms

Two of the hypotheses in this research propose moderating effects that use interaction terms. Program attendance is hypothesized to moderate the effects of neighborhood conditions on supervision outcomes. Before the interaction terms are created each variable will be mean centered. Interaction terms will be created using the measures for neighborhood structural conditions multiplied by the number of program attendance totals for each parolee at the individual level. The second hypothesized moderation includes an interaction term between program attendance and individual level risk factors.

Individual Level Measures

Individual level measures for this study are drawn from the variables in the NIJ project file. Guided by what is known from research related to RNR and risk prediction and used in the risk prediction research cited in Chapter 3, a number of measures will be tested in the preliminary analyses to determine the strongest individual level predictors to use in the full analysis. The first measure, *risk score,* comes from one of two risk instruments created using the parole population in Georgia to predict arrest for a new felony offense (Meredith & Prevost, 2010; Meredith, Speir, & Johnson, 2007). The first instrument was applied to parolees up to the year 2010. Five of the nine variables in the instrument are related to criminal history (whether or not the most serious offense is property, whether or not the most serious offense is drug sales, number of prior juvenile and adult incarcerations, number of prior drug sale/possession convictions, whether or not the offender has a prior probation or parole revocation). The four remaining variables are associated with personal attributes (age at sentencing, whether or not the offender has a history of mental illness, whether or not the offender has a history of assaultive offenses/behavior, whether or not the offender has a history of drug or alcohol abuse) (Meredith et al., 2007).

The revised risk instrument, implemented in September 2010, includes 10 variables. Five variables from the first instrument are retained or refined (age, whether or not the offender has a prior probation/parole revocation, number of incarcerations as an adult, whether or not the offender has a history of alcohol abuse, whether or not the offender has a history of mental health treatment). The four other variables were replaced and a 10th variable added (whether or not the primary offense is forgery, whether or not the primary offense is theft[8], whether or not the primary offense is drug possession, number of felony conviction sentences, whether or not the offender has a history of chronic illness[9]).

Both risk instruments were created and validated on split samples of Georgia parolees, thus addressing the concern highlighted in the literature that instruments should be validated on the population to which they are applied. Not surprisingly, in line with the risk prediction

[8] Significant for males but not for females
[9] Significant for reducing risk for females but not significant for males

research, both instruments rely heavily on criminal history measures. Similarly, the other measures related to personal characteristics are found in other risk instruments reviewed in Chapter 3. The first instrument validation resulted in 65% of arrests correctly predicted. Risk scores were set on a 1-10 scale with 18% arrested for a new felony at risk score one, 44% at risk score five, and 78% at risk score ten. Score cut-offs were assigned based on each number from one to ten representing the next 10% of those arrested for a new felony.

Variables for male and female offenders were pooled for the first instrument due to the low number of females in the population. The second revalidation using 38,000 parolees with 6,000 females as opposed to a total 6,327 males and females in the first instrument, allowed for creating and validating separate instruments by gender. The larger research population in the second validation achieved results similar to the first instrument (Meredith & Prevost, 2010). The use of both types of variables in a validated instrument reduces the necessity in this research for a large number of other control variables.

NonWhite is a dichotomous variable where 1 indicates the parolee is NonWhite and 0 as otherwise. Only nine of the parolees in the research sample are not white or black. Gender is also included with *Male* coded as 1 and female coded as 0. *HighSchool* is a dichotomous measure with 0 indicating no graduation and 1 indicating graduation from high school. *Age* is the parolee’s age in years on the date of release from prison to parole supervision. The final section of this chapter outlines the analytical strategy.

Analytical Methods

Hierarchical Linear Modeling (HLM, version 7) will be employed to test most of the hypotheses in this study. A total of 1,637 parolees and their activities at level one and structural conditions in 65 census tracts as well as number of programs at level two provide data for the analyses. Parolees self-select into the neighborhoods where they live which is assumed to not be random. It is likely that parolees who share the same tract may be more alike than parolees in other tracts. Since all parolees in a census tract at level two share the same data for that tract, multilevel analysis accounts for the non-independence of observations and shared variance among measures for parolees in the same tract (Raudenbush & Bryk, 2002; Woltman, Feldstein, Mackay, & Rocchi, 2012). Multilevel analysis allows for the examination of measures at the individual level and at the tract level simultaneously (Woltman, et al., 2012).

Preliminary Analyses

Since the majority of the 554 census tracts in the target areas did not have enough parolees to qualify for the analysis, t-tests will be conducted to compare excluded and included parolees and their assigned census tract measures to examine if and how the excluded parolees and neighborhoods may be different from those included in the analyses. The five dependent measures in this research, which are dichotomous, and two-level variable structure will require using single and multilevel logistic regression for the majority of the analyses. Hypotheses 1-4 will be conducted at level one. All variables in the multilevel analyses will be grand mean centered.

CHAPTER V: RESULTS

This chapter statistically explores the variables used in the analysis, reviews the equations that are used, and reports the results of the analytical tests investigating the effects of community conditions, parolee program attendance, and individual parolee characteristics on five parole outcomes. First, descriptive findings for each dependent, neighborhood-level, and individual-level variable and interaction term in the study sample (parolees in census tracts with 19 or more parolees) are discussed. Then, characteristics of the study sample are compared to characteristics in the non-study sample (census tracts with 1-18 parolees) to explore generalizability of the findings. Next, equations used in the analyses are described. Finally, bivariate findings are reported followed by a report of multi-variate findings for tests of each of the seven hypotheses.

Descriptive Results for Dependent Variables

Outcomes

The five dichotomous dependent variables in this study are technical violation, positive drug test, technical violation arrest, felony arrest, and parole revocation during the first 12 months of supervision. A total of 47.2% of the 1,637 parolees in the study committed one or more technical (non-crime) violations (*Violation*) (Table 5.1). This variable indicates the presence of at least one of 18 possible technical violations. Technical violations are allowed to accumulate before stronger actions such as an arrest are ordered. Thus, slightly less than half of those with a technical violation - 22% of the study sample - were arrested for a technical violation (*ViolArrest*). A subset of 1,504 (92%) parolees was drug tested one or more times with 42.2% positive for an illicit drug (*Drug*). Unlike technical violations which may not lead to an arrest, the majority of parolees accused of a felony crime are arrested. Nineteen percent of the

Table 5.1.
Descriptive Statistics: Sample variables

Measure	N	Mean	SD	Minimum	Maximum
Dependent Measures					
Violation	1637	.472	.499	0	1
Drug[a]	1504	.422	.494	0	1
ViolArrest	1637	.221	.415	0	1
FelArrest	1637	.190	.392	0	1
Revocation	1637	.096	.295	0	1
Independent Measures					
Male	1637	.91	.285	0	1
NonWhite	1637	.85	.353	0	1
Highschool	1637	.37	.482	0	1
Risk	1637	5.3	2.21	1	10
Age	1637	34.82	10.35	18	64
AttdsTotal	1637	5.02	10.99	0	183
Disadvantage	65	0	1	-1.717	2.450
Mobility	65	0	1	-1.779	2.515
Proportion Black	65	.799	.174	.351	.998
TractPgms	65	3.48	6.1	0	29

[a] Number drug tested at least one time

sample had a felony crime arrest (*FelArrest*). The fifth outcome, revocation back to prison represents the most serious outcome but the one employed the least, thus during the first year of supervision 9.6% were revoked back to prison (*Revocation*) (Table 5.1).

Tract Level Total Programs

This study hypothesizes that the number of programs in a census tract depends in part on the levels of community conditions and effects of other independent variables *(H7a)*. *TractPgms*, measured at the census tract level, consists of the combined total of formal cognitive skills

Table 5.2.
Distribution of Programs Among 65 Study Tracts (N=226)

Number of tracts	Number of programs/tract
26	0
10	1
10	2
4	3
1	5
4	6
1	7
1	8
1	9
1	11
1	13
1	14
1	18
1	22
1	24
1	29

(COG), mental health (MH), and substance abuse (SA) programs in each of the 65 study census tracts. Parolees are also given credit for attending 12 step programs (AA, NA, or CA). Therefore, the total number of these programs in the tract is added to the number of formal programs in the tract for a grand total of 226 programs in the 65 census tracts distributed as follows: no programs in 26 tracts; one program in each of 10 tracts; two programs in each of 10 tracts; three programs in each of four tracts; five programs in one tract; six programs in each of four tracts; and one tract each with seven, eight, nine, eleven, thirteen, fourteen, eighteen, twenty-two, twenty-four and twenty-nine programs (Table 5.2).

Descriptive Results for Independent Variables

Neighborhood Conditions (Level 2)

A primary aim of this study is to gain a better understanding of the effects of neighborhood conditions on parole outcomes as explained by social disorganization theory. A total of seven neighborhood level variables from the 2010 U.S. Census (five-year estimates) at the tract level (Table 5.3) were selected with the goal of creating composite measures for disadvantage, mobility, and heterogeneity. An examination of the census tract measures reveals significant variation in conditions across study neighborhoods. The Proportion Black in study tracts ranges from .35 to almost completely black (.998). Proportion Black in 59 of 65 census tracts is .50 or higher. Percent below the poverty line ranges from 8.35% percent to 70.58%. In 17 or 26% of the 65 census tracts, 40% of the population have incomes that are at or below the poverty line. In previous research, census tracts with 40% below the poverty line have been labeled as “extremely disadvantaged” (Krivo & Peterson, 1996, p. 620), suggesting a high correlation between Proportion Black and extreme poverty.

The number of residents receiving Food Stamps ranges from 3.03% to 55.33%. The average percent of residents without a high school diploma is 23.62% but ranges from a low of 7.55% to a maximum of only 42.93% in the most educated census tract. In 40 census tracts half or more of occupied housing is rented with nine tracts having 70 to 85.9% of occupied housing rented. The neighborhood average of rental housing is 52.36%.

Table 5.3.
Descriptive Statistics: Study Census Tract Level Variables (N = 65)

	Mean	SD	Minimum	Maximum
No high school diploma (%)	23.62	9.19	7.55	42.93
Below poverty line (%)	30.47	14.16	8.35	70.58
Receiving food stamps (%)	21.93	11.37	3.03	55.33
Female-headed households (%)	29.54	8.10	11.93	52.91
Housing rented (%)	52.36	16.89	13.90	85.90
Moved last five yrs. (%)	44.67	12.08	18.50	73.80
Proportion Black	.799	.174	.351	.998

The percent of female-headed households is between 11.93% and 52.91%. As for mobility, the average percent of residents who moved in the last five years is 44.67%. In 21 of 65 census tracts 50% or more of residents moved during the five-year time period. Table 5.3 displays descriptive statistics for U.S. Census variables.

As discussed in Chapter 4, neighborhood research has a long history of using U.S. Census data as indicators of neighborhood conditions. Because many of these variables tend to be highly correlated, factor analysis has often been used to create indices for these neighborhood conditions (Hannon, 2005; Kubrin & Stewart, 2006; Lauritsen, 2001; McNulty & Holloway, 2000; Morenoff & Harding, 2011; Morenoff et al., 2001; Slocum et al., 2013; Velez, 2001). The goal is to create factors to represent three underlying (latent) neighborhood conditions (disadvantage, mobility, heterogeneity).

Using SPSS version 24, the seven variables loaded on only two significant factors representing *Disadvantage* and *Mobility*. *Proportion Black* loaded on both factors. When *Proportion Black* loaded on *Mobility* the load values for other variables were in the opposite direction of what was expected, and when loaded on Disadvantage other poverty related variables loaded at high levels on mobility as well. With *Proportion Black* removed from the analysis very high load values in the expected directions emerged for both the *Disadvantage* (eigenvalue = 3.336) and *Mobility* factors (eigenvalue=1.254) (Table 5.4). Therefore, *Proportion Black* was removed from the factor analysis and maintained as a stand-alone indicator of neighborhood racial context. The items with the highest factor loading on the *Disadvantage* factor are: percent below the poverty line (.851), percent with no high school diploma (.801), percent receiving food stamps (.891), and percent of households headed by females (.778). The items with their highest loading on the *Mobility* factor are: percent of occupied housing that is rented (.751) and percent who moved within the past five years (.945) (Table 5.4). Regression-based factor scores were obtained from the program (Table 5.2). *Proportion Black* was retained as the third neighborhood level measure for tract heterogeneity.[10]

Table 5.4.
Factor Analysis for Neighborhood Constructs (N=65)

	Disadvantage	Mobility
No high school diploma (%)	.801	
Below poverty line (%)	.851	
Receiving food stamps (%)	.891	
Female-headed households (%)	.778	
Moved last five yrs. (%)		.945
Housing rented (%)		.751

[10] While Proportion Black does not account for all the differences among racial groups in a tract, Shaw and McKay (1969) used it in their analysis. Also, a dissimilarity index was created to test differences in analyses using this neighborhood level variable. Where significance effects were uncovered, they were found using both variables.

Individual Level Variables

A total of five individual level variables are included in the analysis and one supervision activity variable (Table 5.1). Perhaps the most important individual level measure is risk to reoffend (*Risk*). As reviewed in chapter 4, assessing risk to reoffend has become a staple in community corrections for determining the level of supervision each parolee receives and whether to refer the parolee to programming. The methods chapter also notes that the risk score (*Risk*) is a composite of 10 variables on a 10-point scale from 1= lowest risk to 10 = highest risk of re-arrest for a new offense (Table 5.1) with mean = 5.3 and standard deviation = 2.21. *Risk* is normally distributed (skewness = .053, s.e. .060). *Male* (1 = male), *NonWhite* (1= nonwhite with the more than 99% being black), and *HighSchool* (1= graduated high school) are dichotomous variables. The study sample is 91% *Male,* 85% *NonWhite* and 37% *HighSchool* graduates (Table 5.1). A*ge,* the parolee's age on the first day of parole, ranges from 18 to 64 with a mean of 34.8 years and standard deviation of 10.4.

The final individual level variable is the number of program attendances (*AttdsTotal*). Parolees at lower levels of supervision which corresponds to lower risk are not required to participate in programs which helps to explain why 52% (N = 855) of the study sample do not have any program attendances (Table 5.5). At the other end of the scale, one parolee has 183 program attendances. The mean number of program attendances is 5.02. A total of 262 parolees have from one to five program attendances while another 295 attended 6-10 times and 97 attended 11-15 times. The number of attendances continues to increase but includes far fewer parolees (51 attending 16-20 times; 36 attending 21-30 times; 11 attending 31-40 times; 21

Table 5.5.
Distribution of Program Attendance Among Study Parolees (N = 1637)

# of Attendances	# of Parolees
0	855
1-5	262
6-10	295
11-15	97
16-20	51
21-25	18
26-30	18
31-40	11
41-57	21
61	1
66	1
77	1
81	1
88	1
105	1
108	1
151	1
183	1

attending 41-57 times; and finally, 9 parolees attending 61-183 times) (Table 5.5).[11]

Attendance distribution was found to be highly skewed (6.818, s.e. = .060). To reduce the skewness the nine cases with attendances greater than 60 were recoded as 60 which resulted in a reduction in the skewness by half to 3.37 (s.e.= .060). Since program attendance is both a predictor and dependent variable in the hypotheses, additional treatment of the variable is addressed when those hypotheses are tested.

[11] Among the three types of programs that make up this total, 679 parolees have 1-60 SA attendances, 157 have 1-37 COG attendances, and 59 have 1- 42 MH attendances.

Interaction Terms

As explained in Chapter 2, social disorganization theory suggests that neighborhood conditions impede the development of informal social controls over the behavior of neighborhood residents. On the other hand, research related to RNR has shown that increasing participation in programs such as mental health and drug treatment, and thinking skills classes improves parolee supervision outcomes. This research hypothesizes that in addition to the direct effects of program attendance on supervision outcomes, program attendance moderates the adverse effects that neighborhood disadvantage, mobility, and heterogeneity may have on parole outcomes (*H6).* Interaction terms are used to test the presence of moderating effects (Baron & Kenny, 1986). Cross level interactions are examined using multilevel modeling which frees the individual level variable to vary across neighborhood contexts.

Furthermore, RNR also posits that increasing numbers of program attendances may increase or decrease the effects of individual level variables (*Male, NonWhite, HighSchool*, *Age, Risk*) (*H2*) on outcomes. All variables for these interaction terms are at the individual level. Interaction terms for testing the moderating effects of attendance are created by centering each parolee's program attendance around its mean and multiplying this number by each of the five individual level risk factors centered around its mean. The five interaction terms to test *H2* are *Male*Attds; NonWhite*Attds; HighSchool*Attds; Age*Attds; and Risk*Attds.*

Generalizability of Study Sample Variables

The sample of 1,637 parolees selected for this study is drawn from a larger cohort of parolees whose first home address after release from prison is in neighborhoods as defined by U.S. Census tracts in several urban areas in Georgia. These include the cities of Atlanta, College

Park, and East Point in Fulton County; Clayton and DeKalb Counties; and the cities of Albany, Augusta, Macon, and Savannah. Among the 554 census tracts originally examined, in order to assure a sufficient number of individuals in each census tract for the group level analysis, only the 65 census tracts with 19 or more parolees are included in the research sample. A number of t-tests were conducted to examine the generalizability of any findings across the entire cohort of included and excluded census tracts.

Comparing Individual Level Variables Between Sample and Non-Sample Parolees

The first t-tests compare individual level attributes of the 1,637 study parolees with the 3,696 non-study parolees (Table 5.6). The two groups are significantly different on five of 11 variables. The study sample has a higher proportion that are *Male* (.91) compared to the non-study cohort (.89). Similarly, a significantly higher proportion of the study sample are *NonWhite* (study = .85 compared to non-study = .79). The non-study cohort has a significantly higher proportion of *HighSchool* graduates (.431 vs .368). Importantly, however, neither *Age* nor *Risk* vary significantly between the two groups.

Turning to the t-test comparison of outcomes, considering risk scores do not vary between the groups, it might be expected that outcomes would also not vary. Among the five outcomes, three are not significantly different between the study and non-study samples (*Violation, ViolArrest, Revocation*). Study sample means are significantly higher for *Drug* (study =.423 vs. non-study = .389) and *FelArrest* (study = .190 vs. non-study = .167). No difference is noted between the study and non-study sample related to other outcome variables. Similarly, the final individual level variable, *AttdsTotal,* is not significantly different from the non-study

Table 5.6.
t-Tests for Individual Level Variables

	Study $\overline{x}$ N = 1637	Non_Study $\overline{x}$ N = 3696	t
Male	.911	.890	2.428*
NONWHITE	.854	.791	5.741***
HighSchool	.368	.431	-4.381***
AGE	34.82	34.55	.910
Risk	5.300	5.248	.784
Violation	.472	.444	1.865
Drug[a]	.423	.389	2.226*
ViolArrest	.221	.208	1.063
FelArrest	.190	.167	1.984*
Revocation	.096	.079	1.950
AttdsTotal	5.018	5.610	-1.948

*** p. < .001; ** p < .01; * p < .05;

[a] N = 1,504 drug tested in study cohort and 3,437 in non-study cohort

sample with a mean of 5.018 attendances among the study sample and 5.61 in the non-study sample.

Comparing Neighborhood Level Variables

One important aspect of this research is the hypotheses that neighborhood conditions are associated with a significant effect on parolee outcomes, program availability, and program attendance. Because the outcomes of the factor analyses depend in part on the sample, the factor structures were not the same in the study and non-study sample and cannot be compared. Therefore, the seven U.S. Census tract level variables used to create the neighborhood level indices are compared to the variables in the non-study sample using t-tests. (Table 5.7).

Table 5.7.
t-Tests for Level-Two Variables

	Sample $\overline{x}$ N = 65	Non-Sample $\overline{x}$ N = 489	t
No high school diploma (%)	23.62	16.48	4.989***
Below poverty line (%)	30.48	21.57	4.520***
Receiving food stamps (%)	21.93	13.49	5.448***
Female-headed households (%)	29.54	19.67	8.724***
Moved last five yrs. (%)	44.67	46.57	-1.145
Housing rented (%)	52.36	47.29	2.155*
Proportion Black	.799	.551	9.490***
TractPgms	3.48	2.82	.802

*** p <.001; ** p <.01; * p <.05

The differences in variable means are significant in six of the seven neighborhood variables. Sample neighborhoods have a higher average mean percentage for *No high school diploma* (23.62% vs 16.48%), living *Below poverty line* (30.48% vs 21.57%), *Receiving food stamps (*21.93% vs 13.49%), and *Female-headed households* (29.54% vs 19.67%). Comparing the number of programs across study and non-study census tracts (t = .802), no significant differences were found. The mean number of programs in study census tracts is 3.48 and 2.82 in non-study census tracts (Table 5.7). The difference in means for *Proportion Black* (.799 vs .551) is striking with a .25 gap between non-study census tracts and study census tracts.

Taken as a whole, the six measures of neighborhood conditions that are significantly different all find sample neighborhoods poorer, less educated, with more rented housing, more female-headed households and non-white residents, and higher means for residents receiving food stamps. In all cases the sample census tracts are significantly more disadvantaged. The differences raise questions about the relationship between the disadvantaged neighborhoods and the fact that they are the neighborhoods where the greatest number of parolees reside, with 19 or more parolees. Considering these differences, any findings from this study may not be generalizable across other more diverse geographic areas but rather more applicable to the most disadvantaged neighborhoods. Attention now turns to the equations used to test the seven hypotheses and preliminary analyses.

Analyses Equations

The seven hypotheses in this study examine variables at level-1, level-2, and investigate effects at levels one and two simultaneously. SPSS 24 is used for bivariate analyses. A total of 1,637 parolees and their activities at level-1 and structural conditions in 65 census tracts as well as number of programs at level-2 provide data for the analyses. Parolees self-select into the neighborhoods where they live which is assumed to not be random. It is probable that parolees who share the same tract may be more alike than parolees in other tracts. Since all parolees in a census tract at level-2 share the same data for that tract, multilevel analysis accounts for the non-independence of observations and shared variance among measures for parolees in the same tract (Raudenbush & Bryk, 2002; Woltman, Feldstein, Mackay, & Rocchi, 2012). Multilevel analysis allows for the simultaneous examination of measures at the individual level and at the census tract level (Woltman, et al., 2012). To account for the theorized shared error terms, HLM-7 is used to test both single and multilevel hypotheses. This study hypothesizes that neighborhood level measures influence individual parolee outcomes and negatively influence participation in programs that lower the likelihood of committing violations.

Since all dependent variables are dichotomous (0, 1) a logit link function which follows the Bernoulli distribution was used in the analyses (Hox, Moerbeek, & Schoot, 2018, p. 108). Because neighborhoods are the focus, a random intercept, fixed slope model is used to test for effects on outcomes. Level-1 models investigate questions related to RNR beginning with the individual level variable effects on each of the five dependent variables (*H1, H2*). Level-1 equations take the form:[12]

$$\mathbf{Prob(FelArrest}_{ij} = 1 \mid \boldsymbol{\beta}_j) = \boldsymbol{\varphi}_{ij}$$

$$\mathbf{log}[\boldsymbol{\varphi}_{ij} \,/\, (1 - \boldsymbol{\varphi}_{ij})] = \boldsymbol{\eta}_{ij}$$

$$\boldsymbol{\eta}_{ij} = \boldsymbol{\beta}_{ij} + \boldsymbol{\beta}_{1j}(\boldsymbol{Risk}_{ij}) + \boldsymbol{\beta}_{2j}(\boldsymbol{NonWhite}_{ij}) + \boldsymbol{\beta}_{3j}(\boldsymbol{Male}_{ij}) + \boldsymbol{\beta}_{4j}(\boldsymbol{HighSchool}_{ij}) + \boldsymbol{\beta}_{5j}(\mathbf{Age}_{ij}) + \boldsymbol{\beta}_{6j}(\boldsymbol{AttdsTotal}_{ij})$$

It is hypothesized that program attendance will moderate the effects of individual level-1 parolee attributes on all outcome measures (*H3*). Separate equations are used to test each of the individual level attributes (*Male, HighSchool, NonWhite, Age*, and *Risk*) and its corresponding interaction term (Individual level variable * AttdsTotal). Since the dependent measures are dichotomous these analyses are conducted as logistic regressions which take the form:

$$\boldsymbol{B}_{0j} = \boldsymbol{\beta}_{ij} + \boldsymbol{\beta}_{1j}(\boldsymbol{Risk}_{ij}) + \boldsymbol{\beta}_{2j}(\boldsymbol{NonWhite}_{ij}) + \boldsymbol{\beta}_{3j}(\boldsymbol{Male}_{ij}) + \boldsymbol{\beta}_{4j}(\boldsymbol{HighSchool}_{ij}) + \boldsymbol{\beta}_{5j}(\mathbf{Age}_{ij}) + \boldsymbol{\beta}_{6j}(\boldsymbol{AttdsTotal}_{ij}) + \boldsymbol{\beta}_{7j}(\boldsymbol{Risk}_{ij} * \boldsymbol{Attds}_{ij})$$

[12] Used here are symbols typically found in multilevel texts (Tabachnick, B. G. & Fidell, L. S. (2013*). Using multivariate statistics.* Upper Saddle River: NJ, Pearson Education Inc).

Additionally, RNR posits and it is hypothesized (*H4*) that the individual attributes of parolees will have significant direct effects on the number of program attendances. The equation for this test takes the form:

$$B_{0j} = \beta_{ij} + \beta_{1j}(Risk_{ij}) + \beta_{2j}(NonWhite_{ij}) + \beta_{3j}(Male_{ij}) + \beta_{4j}(HighSchool_{ij}) + \beta_{5j}(\mathrm{Age}_{ij})$$

Hypotheses 5-7 examine whether neighborhood conditions as posited by social disorganization theory have significant effects on parolee outcomes, the availability of programs, and program attendance. Community (level-2) conditions - *Disadvantage, Mobility,* and *Proportion Black* - and the number of programs in each census tract (*TractPgms)* should have an effect on the five outcome measures (*H5*). Each outcome measure is tested separately. Equations for these multilevel tests take the form:

$$B_{0j} = \gamma_{00} + \gamma_{01}(Disadvantage_j) + \gamma_{02}(Mobility_j) + \gamma_{03}(Proportion\ Black_j) + \gamma_{04}(TractPgms_j) + u_{0j}$$

Furthermore, this study hypothesizes that program attendance moderates the effects of level-2 community conditions on supervision outcomes (*H6*). Each equation with community conditions (*Disadvantage, Mobility, Proportion Black*) and its corresponding interaction term (community condition * *AttdsTotal*) in these multilevel analyses take the form:

$$B_{0j} = \gamma_{00} + \gamma_{01}(Disadvantage_j) + \beta_{1j}(AttdsTotal_{ij}) + \gamma_{02}(DIS_j{*}Attds_{ij}) + \beta_{1j}(Male_{ij}) + \beta_{1j}(NonWhite_{ij}) + \beta_{1j}(HighSchool_{ij}) + \beta_{1j}(Age_{ij}) + \beta_{1j}(Risk_{ij}) + u_{0j}$$

Finally, *H7* specifies that the effects of community-level conditions (level-2) on parolee outcomes are mediated by the number of programs in the tract which are then mediated by the number of program attendances which are hypothesized to be associated with parolee outcomes (dependent variables). This series of effects represents two mediations. Each mediation is a 2-step process. The first equation for the first mediation is a multilevel analysis of the effect of the independent variable (*neighborhood conditions)* on the dependent variable (*program attendance*) as illustrated by:

$$\boldsymbol{AttdsTotal_{ij} = \gamma_{00} + \gamma_{01}(Disadvantage_j) + \gamma_{02}(Mobility_j) + \gamma_{03}(Proportion\ Black_j) + u_{0j}}$$

The second equation investigates whether adding the mediator variable in the equation dampens or eliminates the effects of neighborhood conditions on *AttdsTotal.* This equation takes the form:

$$\boldsymbol{AttdsTotal_{ij} = \gamma_{00} + \gamma_{01}(TractPgms_j\ ) + \gamma_{01}(Disadvantage_j) + \gamma_{02}(Mobility_j) + \gamma_{03}(Proportion\ Black_j) + u_{0j}}$$

If the mediator is significant and the effects of the independent variables are significantly reduced or are no longer statistically significant, a mediation effect has been shown.

The second half of the mediation hypothesis (*H7b*) examines whether the effect of the number of programs on outcomes is mediated by the number of program attendances. This hypothesis is important because the number of programs is thought to increase access to programs which leads to more program attendance and thus, improved outcomes. This hypothesis (*7a-b*) suggests the dampening effects of neighborhood conditions extends through *TractPgms* which has an effect on *AttdsTotal* which then affects outcomes. The test for mediation of *TractPgms* through *AttdsTotal* on the outcome variables follows a similar 2-step path as illustrated by:

$$\boldsymbol{B_{0j} = \gamma_{00} + \gamma_{01}(TractPgms_j) + \beta_{1j}(Male_{ij}) + \beta_{1j}(NonWhite_{ij}) + \beta_{1j}(HighSchool_{ij}) + \beta_{1j}(Age_{ij}) + \beta_{1j}(Risk_{ij}) + u_{0j}}$$

The second equation investigates whether adding the mediator variable to the equation dampens or eliminates the effect of *TractPgms* on outcomes.

$$\boldsymbol{B_{0j} = \gamma_{00} + \gamma_{01}(TractPgms_j) + \beta_{1j}(AttdsTotal_{ij}) + \beta_{1j}(Male_{ij}) + \beta_{1j}(NonWhite_{ij}) + \beta_{1j}(HighSchool_{ij}) + \beta_{1j}(Age_{ij}) + \beta_{1j}(Risk_{ij}) + u_{0j}}$$

Bivariate Analyses

Bivariate Correlations for Individual Level Measures

Including level-1 and level-2 variables, interaction terms, and dependent measures, a total of 24 variables are used to investigate the seven hypotheses. Bivariate correlations provide preliminary information about relationships and evidence for possible serious collinearity between variables and among interaction terms and the variables used to create them. The bivariate analyses begin with individual level variables, interaction terms, and dependent variables (Table 5.8).

Bivariate correlations indicate the five individual level characteristics (*Male, NonWhite, HigbSchool, Age, Risk*) are all significantly related to two or more of the dependent measures. *Male* is positively correlated with *Violation*, *FelArrest* and *Revocation*. *NonWhite* is negatively correlated with *Drug* and *ViolArrest*; both *HighSchool* and *Age* are negatively correlated with all dependent measures except *ViolArrest.* Research related to reoffending cited in Chapter 3, in general, supports these bivariate correlations related to felony arrest and revocation. Previous research also suggests *NonWhite* would be associated with negative outcomes but the correlations here indicate no significant correlation with felony arrest or revocation. The two significant correlations noted above suggest *NonWhite* is slightly less likely to be associated with positive drug tests and technical violation arrests. The final individual level measure, *Risk* is

Table 5.8.
Bivariate Correlations for Individual Level Measures, Moderators, and Independent Measures (N= 1,637)

		1	2	3	4	5	6	7	8
1	Male	1							
2	NonWhite	.059*	1						
3	HighSchool	-.095**	-.061*	1					
4	Age	-.056*	-.153**	.233**	1				
5	Risk	.043	.072**	-.217**	-.504**	1			
6	AttdsTotal	-.060*	-.060*	.055*	.057*	-.025	1		
7	Male*Attds	-.090**	.003	.005	-.007	-.001	-.310**	1	
8	NonWhite*Attds	.002	-.186**	-.006	.027	.008	.022	-.105**	1
9	HighSchool*Attds	.005	-.01	-.252**	-.064**	.060*	.298**	-.425**	-.097**
10	Age*Attds	-.011	.026	-.071**	-.319**	.160**	.387**	-.147**	-.222**
11	Risk*Attds	.002	.013	.063*	.159**	-.312**	-.331**	.010	.159**
12	Violation	.072**	-.036	-.106**	-.068**	.145**	.115**	-.041	-.011
13	Drug	.034	-.054*	-.065**	-.071**	.170**	.247**	-.031	-.001
14	ViolArrest	.011	-.051*	-.03	.032	.021	.237**	-.033	-.027
15	FelArrest	.075**	.041	-.085**	-.100**	.146**	-.039	-.002	-.017
16	Revocation	.058*	.035	-.072**	-.074**	.224**	-.069**	-.006	0

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

Table 5.8.
Bivariate Correlations for Individual Level Measures, Moderators, and Independent Measures (N= 1,637)

		9	10	11	12	13	14	15	16
9	HighSchool*Attds	1							
10	Age*Attds	.332**	1						
11	Risk*Attds	-.270**	-.567**	1					
12	Violation	.074**	.074**	-.119**	1				
13	Drug	.061*	.062*	-.087**	.296**	1			
14	ViolArrest	.075**	.024	-.080**	.563**	.353**	1		
15	FelArrest	.03	.094**	-.087**	.513**	.074**	-.032	1	
16	Revocation	.028	.026	-.111**	.336**	.072**	.092**	.472**	1

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

positively correlated with four of the five outcome measures (*Violation*, *Drug, FelArrest,* and *Revocation*) (Table 5.8). As the risk score increases the likelihood of a technical violation, positive drug test, felony arrest, and revocation increases. *Risk* is not significantly correlated with *ViolArrest.*

Turning to one of the key individual level measures related to supervision activity, somewhat unexpectedly *AttdsTotal* is significantly and positively associated with *Violation* ($r = .115$, $p < .01$), *Drug* ($r = .247$, $p < .01$), and *ViolArrest* ($r = .237$, $p < .01$). On the other hand, *AttdsTotal* is not significantly correlated with *FelArrest* but is significant and negatively correlated (as expected) with *Revocation* ($r = -.069$, $p < .01$) (Table 5.8). Violations and violation arrests, and drug tests, all related to non-crime supervision activity, often guide how parole officers conduct supervision. One response is to enroll a parolee in a program which then requires attendance. On the other hand, program attendance may uncover violations or create opportunities for other violations. As such, this set of correlations may point to a more complex relationship between these three dependent measures, calling for further discussion after examining *H2.*

The final set of relationships in Table 5.8 are the interaction terms created to test program attendance moderation on the dependent measures. Mean-centering of each variable was conducted prior to creating interaction terms. The highest correlation among variables included in an equation is -.504, between *Risk* and *Age.* Neither the *Male*Attds* nor *NonWhite*Attds* interaction terms are significantly correlated with any of the five dependent measures suggesting that neither gender nor race is moderated by attendance. Significant correlations were found between *HighSchool*Attds*, *Age*Attds* and *Risk*Attds* and one or more of the dependent measures.

Bivariate Correlations for Neighborhood Level Measures

Neighborhood conditions are hypothesized to affect the number of neighborhood programs as well as parolee outcomes. The adverse effects of neighborhood conditions are also hypothesized to be moderated by program attendance. Among the three neighborhood context measures (*Disadvantage, Mobility,* and *Proportion Black)*, only *Mobility* is significantly correlated with one dependent variable, *ViolArrest* ($r = .053$, $p < .05$) (Table 5.9).

Neighborhood conditions are predicted to adversely affect *AttdsTotal* (H7) but no significant correlations were found with *Disadvantage, Mobility,* or *Proportion Black.* *TractPgms* is predicted to be negatively affected by adverse neighborhood conditions (*H7*). In fact, *Disadvantage* ($r = -.241$, $p < .01$) and *Proportion Black* ($r = -.324$, $p < .01$) are negatively correlated with *TractPgms* while *Mobility* ($r = .283$, $p < .01$) is positively correlated with *TractPgms*. Also, contrary to *H7,* the number of *TractPgms* is positively correlated with *Violations* ($r = .072$, $p < .01$), *Drug* ($r = .071$, $p < .01$), and *ViolArrest* ($r = .106$, $p < .01$). *TractPgms* is not significantly correlated with *FelArrest* or *Revocation* (Table 5.9). However, *TractPgms* is positively correlated with *AttdsTotal;* as the number of programs in a neighborhood increases, program attendance also increases ($r = .061$, $p < .05$). With all correlations below .6, there does not appear to be a strong concern about multicollinearity.

Table 5.9.
Bivariate Correlations for Neighborhood Level Predictors, Moderators, and Individual Level Measures (N= 1,637)

		1	2	3	4	5	6
1	Disadvantage	1					
2	Mobility	-.013	1				
3	Proportion Black	.349**	-.244**	1			
4	TractPgms	-.241**	.283**	-.324**	1		
5	AttdsTotal	-0.01	-.008	.015	.061*	1	
6	DIS*Attds	-.306**	.002	-.089**	.050*	.330**	1
7	MOB*Attds	.003	-.398**	.080**	-.130**	-.060*	-.192**
8	Proportion Black*Attds	-.102**	.075**	-.347**	.026	.376**	.581**
9	Violation	.013	.041	-.023	.072**	.115**	.021
10	Drug	-.033	.035	-.031	.071**	.247**	.007
11	ViolArrest	-.02	.053*	-.011	.106**	.237**	.028
12	FelArrest	.047	.016	.027	-.036	-.039	-.018
13	Revocation	.039	.026	.002	-.006	-.069**	-.032

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

Table 5.9.

Bivariate Correlations for Neighborhood Level Predictors, Moderators, and Individual Level Measures (N= 1,637)

		7	8	9	10	11	12	13
7	MOB*Attds	1						
8	Proportion Black*Attds	-.276**	1					
9	Violation	-.018	.018	1				
10	Drug	.009	.029	.296**	1			
11	ViolArrest	-.006	.021	.563**	.353**	1		
12	FelArrest	-.027	.016	.513**	.074**	-.032	1	
13	Revocation	-.033	.003	.336**	.072**	.092**	.472**	1

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

Statistical Models Testing Study Hypotheses

Hypotheses at Level-1 Investigating RNR

Individual level characteristics predicting outcomes (H1): Hypotheses H1-H4 investigate the level and direction of significant effects between individual level attributes of parolees (*Male, NonWhite, HighSchool, Age, Risk*), program attendance (*AttdsTotal*), and the five dependent measures. First, individual level risk measures are hypothesized (H1) to be significantly related to supervision outcomes (*Violation, Drug*, *ViolArrest*, *FelArrest*, *Revocation*) over the first 12 months of supervision. Because parolees are nested within neighborhoods, the level-1 variables are analyzed in hierarchical models using logistic regression in HLM 7.3 with robust standard errors. Logistic regression is used when the dependent variable is dichotomous producing a log-odds of the likelihood of the outcome depending on the level of the independent or predictor variable. The five individual level predictors were fit in models for each outcome (See Table 5.10).

Table 5.10 illustrates that neither *NonWhite* nor *Age is* significant in predicting any of the five dependent measures. Controlling for the other variables, *Risk* is significant in predicting all five dependent measures (*Violation* ($b = .123$, $p < .001$), *Drug* ($b = .155$, $p < .001$), *ViolArrest* ($b = .040$, $p < .05$), *FelArrest* ($b = .142$, $p < .001$), *Revocation* ($b = .375$, $p < .001$). A one point increase in the risk score raises the odds of *Violation* by 13% ((Exp(.123) -1= .131) *100), the odds of *Drug* by 17% ((Exp (.155) - 1 = .167)*100), the odds of *VioArrest* by 4% ((Exp (.040) - 1= .152) *100), the odds of *FelArrest* by 15% ((Exp (.142) -1= .152) *100), and raises the odds of *Revocation* by 45% ((Exp (.375) -1 = .456) *100).

Table 5.10.
Logistic regression: Dependent Measures on Individual-level Variables (N = 1,637)

	Violation	Drug[a]	ViolArrest	FelArrest	Revocation
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Intercept	-.970**	-1.143**	-1.775***	-2.178***	-5.868***
	(.341)	(.366)	(.419)	(.544)	(.681)
Male	.493**	.289	.151	.747*	.863*
	(.168)	(.169)	(.214)	(.316)	(.394)
NonWhite	-.219	-.274	-.184	.181	.247
	(.151)	(.157)	(.208)	(.203)	(.251)
HighSchool	-.319**	-.180	-.183	-.262	-.195
	(.118)	(.104)	(.134)	(.151)	(.210)
Age	.002	.001	.011	-.007	-.012
	(.005)	(.007)	(.006)	(.008)	(.013)
Risk	.123***	.155***	.040*	.142***	.375***
	(.026)	(.032)	(.031)	(.030)	(.034)

*** p <.001; ** p < .01; * p < .05
[a] 1,504 parolees tested one or more times

Controlling for the other variables in the model, *Male* increases the likelihood of three outcomes, *Violation* (b = .493, p < .01), *FelArrest* (b = .747, p < .05), and *Revocation* (b = .863, p < .05). Males are expected to have 63% greater odds than females of committing a *Violation* ((Exp (.493) - 1 = .637) *100), 111% greater odds of *FelArrest* ((Exp (.747) - 1 = 1.111) *100) and 137% greater odds of *Revocation* ((Exp (.863) -1 = 1.370) *100). *HighSchool* (b = -.319, p < .01) reduces the odds of *Violation* by 37.5% ((Exp (-.319) – 1 = .375) *100) for parolees with a high school diploma or GED. Hypotheses-testing next turns to the relationships between program attendance, individual level variables, and outcomes beginning with the effects of program attendance on the five parole outcomes.

Program attendance and outcomes (H2) – Investigating level-1 variables: RNR suggests supervision outcomes will improve by adjusting the level of program intensity to the risk and needs of each offender. Increasing program attendance, especially for high-risk parolees, is hypothesized to decrease all outcomes *(H2)*. It was noted earlier that 855 of the 1,637 parolees do not have any program attendances while one parolee has 183 attendances. Parolees may not attend programs for several reasons: no criminogenic need is found to justify program attendance; a program provider's assessment determines the parolee does not need treatment services, or a criminogenic need triggers enrollment in a program but the parolee chooses not to attend. Program enrollment signals the parole officer's intent for the parolee to attend treatment. Among the 1,637 parolees in the study sample, a total of 968 were enrolled in one or more COG, MH, or SA programs which translates into 637 parolees not required to attend a program. Since program attendance would not be expected or required of parolees not enrolled in programs, only the 968 "enrolled" parolees are used in level-1 hypotheses that include program attendance.

Program attendance for the 968 program-enrolled parolees was found to be highly skewed (5.854, s.e.= .079). Among the subset of parolees enrolled in a program, a total of 186 had no program attendance. As noted above, to obtain a more normal distribution, cases with more than 60 attendances were recoded to 60 which reduced the skewness to 2.720 (s.e. = .079). The variable was then transformed to the natural log which further reduced the skewness (-.194, s.e. = .079). A set of preliminary logistic regression equations testing differences in fit using each version of the variable found little difference in the results and standard errors. Therefore, for ease of interpretation of the results, the non-transformed version of the program attendance variable is used. Before turning our attention to tests related to program attendance one other consideration is noted related to the reduced sample size.

In addition to a review of the research in Chapter 4 related to sufficient sample size, Austin & Steyerberg (2017) have investigated "events per variable (EPV)" (p. 797) in logistic regression where the outcome variable is dichotomous. EPV is defined as the "smaller of the number of subjects who experienced the outcome and the number of subjects who did not experience the outcome" (p. 797). Research in this area suggests estimation problems may occur when the number of EPV's is below 20 and that much more research is necessary to determine acceptable levels of EPV depending on the estimation method. (Austin & Steyerberg, 2017; Smeden, deGroot, Moons, Collins, Altman, Eijkmans, & Reitsma, 2016). The program-enrolled sub-sample of 968 parolees versus 1,637 parolees in the full sample translates into a smaller number of events for each outcome. The smallest number of events in the program-enrolled subsample used in the EPV calculation is the number of parolees who experienced *Revocation* (n = 88 (9%)) with *FelArrest* second (n = 185 (19%)) followed third by *ViolArrest* (n = 282 (29%)). The analyses proceed with an understanding that potential issues are possible in that limited variation in the outcome variables may lead to a smaller likelihood of significant effects being found in the full statistical models related to *FelArrest* and *Revocation* depending on the number of variables included in the final model.

H2 examines the effects of program attendance on the outcome measures for the 968 program-enrolled parolees. Initially, two models were fit for each of the outcomes in HLM 7.3 using logistic regression since all outcomes are dichotomous (Table 5.11a-c). Model-1 for each dependent measure uses only the five individual-level control variables. Model-2 adds program

Table 5.11a.
Logistic Regression: The Effects of Program Attendance on Outcome Measures (n = 968)

	Violation		Drug[a]		ViolArrest	
	1	2	1	2	1	2
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-.084	-.249	-.402	-.796	-1.209*	-1.715***
	(.422)	(.436)	(.424)	(.425)	(.462)	(.479)
Male	.389	.418	.160	.221	.187	.286
	(.223)	(.224)	(.213)	(.230)	(.245)	(.258)
NonWhite	-.498***	-.489**	-.203	-.161	-.301	-.287
	(.152)	(.157)	(.181)	(185)	(.204)	(.216)
HighSchool	-.224	-.248	-.110	-.152	-.112	-.190
	(.133)	(.134)	(.143)	(.143)	(.142)	(.150)
Age	-.001	-.001	-.004	-.005	.010	.011
	(.007)	(.007)	(.008)	(.008)	(.008)	(.008)
Risk	.077*	.080*	.164***	.174***	.009	.019
	(.033)	(.033)	(.042)	(.042)	(.038)	(.038)
AttdsTotal		.016**		.034***		.042***
		(.005)		(.008)		(.005)

*** p. <.001; ** p < .01; * p< .05

[a] N= 932 parolees enrolled in a program and drug tested at least once

attendance. *AttdsTotal* is statistically significant for predicting four of the five outcomes. *AttdsTotal* does not predict *FelArrest* but is associated with a significant reduction in *Revocation* (b = -.059, p < .01) (Table 5.11b). The odds of *Revocation* decrease 5.9% for each additional attendance ((Exp (-.059) - 1= .059) *100). However, unexpectedly and contrary to the hypothesis, *AttdsTotal* is positively related to *Violation* (b = .016, p < .01) *Drug* (b = .034, p < .001) and *ViolArrest* (b = .042, p < .001) (Table 5.11b). As the number of program attendances increases, the odds of *Violation* increase 2.5% for each additional attendance, the odds of *Drug*

Table 5.11b.

Logistic Regression: The Effects of Program Attendance on Outcome Measures (n = 968)

	FelArrest		Revocation	
	1	2	1	2
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	- 2.018***	-1.840**	- 5.043***	- 4.644***
	(.607)	(.614)	(790)	(.801)
Male	.452	.429	.380	.354
	(.390)	(.394)	(.451)	(.467)
NonWhite	.067	.056	-.093	-.093
	(.238)	(.232)	(.301)	(.301)
HighSchool	-.152	.127	.010	.077
	(.174)	(.176)	(.271)	(277)
Age	-.014	-.014	.010	.010
	(.010)	(.010)	(.015)	(.015)
Risk	.110**	.106*	.369***	.364***
	(.042)	(.041)	(.046)	(.047)
AttdsTotal		-.018		-.059**
		(.012)		(.019)

*** p. <.001; ** p < .01; * p< .05

increase by 3.4% for each additional attendance and the odds of *ViolArrest* increase by 4.3% for each additional attendance.

Nonwhite is only significant in predicting a decrease in *Violation* (b = -.489, p < .01) (Table 5.11a). Black parolees enrolled in a program have 63% reduced odds of *Violation* ((Exp (-.489) = 1.63 – 1 = .63) *100. Comparing the findings reported in Table 5.11 on the program-enrolled subsample with findings reported in Table 5.10 which examined the full sample (N = 1,637), *NonWhite* in the full sample is not significant for predicting *Violation* or any other outcome.

Table 5.11c.
Logistic Regression: The Effects of Program Attendance on Outcome Measures (n = 968)

	EarlyOut		LateOut	
	1	2	1	2
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	.521 (.472)	.260 (.491)	-2.069*** (600)	-1.862*** (.600)
Male	.469 (.243)	.506* (.249)	.414 (.350)	.387 (.354)
NonWhite	-.334 (.184)	-.302 (.187)	-.032 (.227)	-.046 (.220)
HighSchool	-.441** (.148)	-.446** (.146)	-.225 (.165)	-.198 (.166)
Age	-.011 (.008)	-.011 (.008)	-.008 (.010)	-.008 (.010)
Risk	.160*** (.036)	.165*** (.037)	.135*** (.040)	.132*** (.039)
AttdsTotal		.025*** (.008)		-.022 (.012)

*** p. <.001; ** p < .01; * p< .05

Also, while *Male* and *HighSchool* are significant in predicting *Violation* in the full sample (Table 5.10), the two variables are not significant in predicting *Violation* in the program-enrolled subsample. These differences suggest the individual level characteristics of the full sample and the program-enrolled subsample also have differing effects on outcomes. An inspection of the three variables found that although the program-enrolled subsample has 669 fewer parolees, the differences in the distribution of *Male, HighSchool,* and *NonWhite* between the two samples varies by less than one percent.

A number of steps were taken to further explore the disappointing and surprising findings overall related to the effects of program attendance on outcomes. In order to increase the number of events per variable (EPV) and thus, the likelihood of achieving a significant finding, the five outcome measures were aggregated into two new measures. Early outcomes (*EarlyOut)* was formed by merging *Violation, Drug, and ViolArrest*, the measures that least result in ending supervision. A second aggregate measure, *LateOut,* was created by merging *FelArrest* and *Revocation*, the measures associated with ending supervision.

Similar to the other outcome measures, the new measures are dichotomous (0 or 1) signifying whether one or more of the outcomes occurred or not. Among the 968 parolees in the program-enrolled subsample there are 700 'events' or 72% of outcomes in *EarlyOut* (one or more *Violation, Drug,* or *ViolArrest*). This contrasts with 526 events or 56% of the *Drug* sample, which is the highest percent for the three variables in *EarlyOut.* A total of 402 parolees in *EarlyOut* have two or more events. This number of events contrasts with *LateOut,* in which 21.5% or 208 parolees have a *FelArrest* and/or *Revocation* event compared to 185 events or 19.1% of parolees in *FelArrest.* A total of 65 parolees have both *FelArrest* and *Revocation.*

The analysis found that controlling for the other variables in the model and similar to findings in models for two of its three component outcomes, *AttdsTotal* is associated with an increase in *EarlyOut* ($b = .025$, $p <.001$) (Table 5.11c) which translates into a 2.5% increase in the odds of *EarlyOut* for each additional attendance. Among the individual level variables, *HighSchool,* which is not significant in any of the other six models, is significant in predicting a decrease in *EarlyOut* ($b = -.446$, $p < .001$). A high school diploma or GED decreases the odds of *EarlyOut* by 56% ((Exp(-.446) – 1= .562)* 100).

As noted above, although *AttdsTotal* is significant in predicting a reduction in *Revocation,* attendance is not significant in the *LateOut* model that combines *FelArrest* and *Revocation.* The hoped-for improvement in prediction in the combined model appears to have been sacrificed by the non-significant findings related to the model for *FelArrest.* In summary, program attendance has unexpected positive effects on *Violation, Drug, ViolArrest,* and the combined variable *EarlyOut,* no effects on *FelArrest* and *LateOut,* and, as hypothesized, negative effects on *Revocation.*

Attendance moderating the effects of individual level variables on outcomes (H3): As noted in Chapter 2, certain attributes of offenders such as being younger, less educated, and with higher risk scores are associated with higher levels of violation and supervision failure. Not only is program attendance hypothesized to reduce negative outcomes, *H3* goes a step further hypothesizing that program attendance moderates the effects of individual level variables on the dependent measures. Models testing moderation require the creation of interaction terms between the independent predictor variables and the moderator.

Five interaction terms were created by multiplying the mean centered values of each individual level predictor and the mean centered values for attendance.

Hypothesis tests involving program attendance use the 968 program-enrolled subsample. With dichotomous dependent measures, logistic regression in HLM 7.3 is used for the analysis. Seven models were tested on each of the five dependent measures. Table 5.12a-g displays the results. Five significant moderation effects are revealed. While the individual level variable -*Age*- is not significant in any of the models, the *Age*Attds* moderator has significant effects

Table 5.12a.
Attendance Moderation of Individual level Variables Predicting Outcomes (n= 968)

	Violation				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-152* (.027)	.154* (.073)	.153* (.074)	.150* (.073)	.147* (.073)
Male	.426 (.226)	.417 (.233)	.419 (.228)	.415 (.225)	.411 (.222)
NonWhite	-.489** (.134)	-.491** (.157)	-.487** (.184)	-.482** (.158)	-.475** (.159)
HighSchool	-.249 (.134)	-.247 (.134)	-.249 (.143)	-.244 (.133)	-.245 (.132)
AGE	-.001 (.007)	-.001 (.007)	-.001 (.008)	-.001 (.033)	-.001 (.007)
Risk	.080* (.033)	.080* (.033	.080* (.034)	.081* (.033)	.079* (.033)
AttdsTotal	.015** (.005(	.016** (.005)	.015* (.007)	.014** (.005)	.014** (.005)
Male*Attds	-.005 (.013				
NonWhite*Attds		.005 (.015)			
HighSchool*Attds			.004 (.013)		
Age*Attds				.001 (.001)	
Risk*Attds					-.005 (.003)

*** p. <.001; ** p < .01; * p< .05

Table 5.12b.

Attendance Moderation of Individual level Variables Predicting Outcomes (n= 932)

	Drug				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-.007 (.086)	-.005 (.088)	-.008 (.084)	-.001 (.088)	-.010 (.086)
Male	.212 (.230)	.226 (.233)	.220 (.229)	.220 (.231)	.224 (.231)
NonWhite	-.162 (.185)	-.163 (.186)	-.163 (.185)	-.158 (.186)	-.166 (.185)
HighSchool	-.151 (.143)	-.156 (.143)	-.153 (.143)	-.150 (.143)	-.154 (.143)
Age	-.005 (.008)	-.005 (.008)	-.005 (.008)	-.005 (.008)	-.005 (.008)
Risk	.174*** (.042)	.175*** (.042)	.174*** (.042)	.174*** (.042	.176*** (.043)
AttdsTotal	.034*** (.008)	.033*** (.008)	.034*** (.008)	.033*** (.008)	.025*** (.008)
Male*Attds	.008 (.019)				
NonWhite*Attds		-.019 (.022)			
HighSchool*Attds			-.005 (.017)		
Age*Attds				.000 (.001)	
Risk*Attds					.002 (.004)

*** p. <.001; ** p < .01; * p< .05

Table 5.12c.

Attendance Moderation of Individual level Variables Predicting Outcomes (n= 968)

	ViolArrest				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-.993*** (.086)	-.936*** (.085)	.940*** (.087)	-.993*** (.084)	-.936*** (.086)
Male	.249 (.258)	.287 (.258)	.289 (.263)	.290 (.257)	.285 (.216)
NonWhite	-.287 (.218)	-.281 (.221)	-.282 (.195)	-.305 (.216)	-.285 (.216)
HighSchool	-.188 (.150)	-.192 (.150)	-.200 (.162)	-.201 (.150)	-.190 (.150)
Age	.011 (.008)	.011 (.008)	.011 (.008)	.012 (.008)	.011 (.008)
Risk	.020 (.038)	.020 (.038)	.019 (.038)	.019 (.039)	.019 (.038)
AttdsTotal	.043*** (.005)	.042 (.005)	.042*** (.007)	.046*** (.005)	.042*** (.005)
Male*Attds	.012 (.014)				
NonWhite*Attds		-.005 (.013)			
HighSchool*Attds			.008 (.014)		
Age*Attds				-.001* (.001)	
Risk*Attds					-.001 (.004)

*** p. <.001; ** p < .01; * p< .05

Table 5.12d.
Attendance Moderation of Individual level Variables Predicting Outcomes (n= 968)

	FelArrest				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-1.504*** (.084)	-1.501*** (.082)	-1.501*** (.084)	-1.552*** (086)	-1.521*** (.090)
Male	.487 (.395)	.424 (.396)	.430 (.394)	.421 (.400)	.421 (.393)
NonWhite	051 (.23)	.076 (.230)	.055 (.234)	.073 (.238)	.081 (.392)
HighSchool	-.132 (.176)	0.126 (.176)	-.130 (.176)	-.101 (.182)	.081 (.239)
Age	-.014 (.010)	-.014 (.010)	-.014 (.010)	-.010 (.011)	-.014 (.010)
Risk	.106* (.041)	.105* (.041)	.107* (.041)	.106** (.040)	.093* (.044)
AttdsTotal	-.020 (.012)	-.018 (.013)	-.018 (.012)	-.030 (.012)	-.021 (.012)
Male*Attds	.039 (.038)				
NonWhite*Attds		.018 (.046)			
HighSchool*Attds			-.003 (.024)		
Age*Attds				.003*** (.001)	
Risk*Attds					-.009 (.006)

*** p. <.001; ** p < .01; * p< .05

Table 5.12e.
Attendance Moderation of Individual level Variables Predicting Outcomes (n= 968)

	Revocation				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-2.761*** (.157)	-2.716*** (.145)	-2.667*** (.148)	-2.663*** (.144)	-2.649*** (.134)
Male	1.560* (.726)	.331 (.391)	.348 (.470)	.359 (.466)	.354 (.473)
NonWhite	-.109 (.312)	.488 (.391)	-.091 (.311)	-.093 (.311)	-.056 (.319)
HighSchool	.034 (.281)	.069 (.277)	.010 (.311)	.079 (.278)	.087 (.280)
Age	.006 (.015)	.005 (.015)	.006 (.015)	.009 (.016)	.006 (.015)
Risk	.364*** (.048)	.355*** (.048)	.364*** (.047)	.364** (.047)	.332*** (.587)
AttdsTotal	-.077*** (.019)	-.069*** (.018)	-.059** (.020)	-.059** (.019)	-.049** (.018)
Male*Attds	.247* (.109)				
NonWhite*Attds		.139* (.059)			
HighSchool*Attds			.009 (.040)		
Age*Attds				.001 (.002)	
Risk*Attds					-.012 (.1010)

*** p. <.001; ** p < .01; * p< .05

Table 5.12f.
Attendance Moderation of Individual level Variables Predicting Outcomes (n= 968)

	EarlyOut				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	1.030*** (.086)	1.027*** (.087)	1.027*** (.086)	1.022*** (.088)	1.019*** (.087)
Male	.504* (.248)	.509* (.250)	.506* (250)	.506* (.250)	.498* (.247)
NonWhite	-.303 (.187)	-.304 (.184)	-.301 (.188)	-.283 (.188)	-.284 (.189)
HighSchool	-.445** (.146)	-.448** (.147)	-.444** (.146)	-.435** (.143)	-.440** (143)
Age	-.011 (.008)	.011 (.008)	.011 (.008)	-.008 (.008)	-.011 (.008)
Risk	.165*** (.037)	.166*** (.037)	.165*** (.037)	.167*** (.037)	.159*** (.036)
AttdsTotal	.026*** (.008)	.025*** (.008)	.025*** (.008)	.021** (.007)	.022** (.007)
Male*Attds	.004 (.021)				
NonWhite*Attds		-.009 (.021)			
HighSchool*Attds			.003 (.015)		
Age*Attds				.002 (.001)	
Risk*Attds					-.007 (.004)

*** p. <.001; ** p < .01; * p< .05

Table 5.12g.
Attendance Moderation of Individual level Variables Predicting Outcomes (n= 968)

	LateOut				
	1	2	3	4	5
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-1.363*** (.078)	-1.136*** (.75)	-1.356*** (.077)	-1.397*** (.079)	-1.380*** (.083)
Male	.491 (.369)	.380 (.356)	.389 (.354)	.382 (.358)	.378 (.354)
NonWhite	-.053 (.220)	-.005 (.243)	-.048 (.222)	-.034 (.223)	-.019 (.170)
HighSchool	-.206 (.166)	-.195 (.166)	-.204 (.167	-.177 (.169)	-.188 (.170)
Age	-.008 (.010)	-.008 (.010)	-.008 (.010)	-.004 (.010)	-.009 (.101)
Risk	.131*** (.039)	.129*** (.039)	.132*** (.039)	.132*** (.038)	.117** (.042)
AttdsTotal	-.024* (.012)	-.022 (.013)	-.021 (.012)	-.032* (.012)	-.024* (.012)
Male*Attds	.055 (045)				
NonWhite*Attds		.029 (.046)			
HighSchool*Attds			-.007 (.024)		
Age*Attds				.003*** (.001)	
Risk*Attds					.202 (.006)

*** p. <.001; ** p < .01; * p< .05

related to *ViolArrest* (b = -.001, p < .05) (Table 5.12c), *FelArrest* (b = .003, p < .001) (Table 5.12d) and *LateOut* (b = .003, p < .001) (Table 5.12g). As *Age* increases the positive effects of attendance are slightly diminished, thus reducing the likelihood of *ViolArrest*. The effect of age (risk decreases as age increases) in this moderation conforms to what has been found in the RNR research related to risk. As attendance increases for older parolees, the likelihood of *ViolArrest* is lower.

As for *FelArrest* and *LateOut,* age moderates the effect of attendance in the opposite direction from *ViolArrest*. As age increases the effect of attendance on these outcomes becomes more positive. Thus, the likelihood of a felony arrest and late outcome increases. However, for younger parolees (below the mean age), age moderates attendance such that attendance decreases the likelihood of these outcomes. The overall effect is small but accumulates with each program attendance. For younger parolees attending programs, the likelihood of *FelArrest* and *LateOut* is diminished while older parolees (over the mean age) attending programs have an increased likelihood of *FelArrest* and *LateOut.*

Two moderators in the *Revocation* models (Table 5.12e), *Male*Attds* (b =.247, p < .05) and *NonWhite*Attds* (b = .139, p < .05) are significant. For males, the male-attendance interaction produces a positive value, but this positive effect increases the likelihood of *Revocation*. In the moderation female parolees strengthen the negative effect of program attendance on *Revocation.* Thus, the parolee's gender serves as a moderator whereby the *Revocation*-reducing effects of program attendance are weakened for males and strengthened for females. *NonWhite* moderates the effectiveness of program attendance in a similar way. The beneficial effects of program attendance are weakened for black parolees, thus increasing the likelihood of *Revocation.* Conversely, for white parolees the beneficial effects of program attendance are enhanced which decreases the likelihood of *Revocation.* Moreover, since in these models the effect of the independent variable *AttdsTotal* is negative which reduces the likelihood of *Revocation*, through the moderation this beneficial effect of program attendance is lessened for black and male parolees and enhanced for white and female parolees. What may lead to the varying effects of program attendance across gender and race is explored in greater detail in the next chapter. The next hypothesis (*H4*) investigates more directly whether individual level risk measures significantly affect program attendance.

Individual characteristics and program attendance (H4): Similar to the effect on supervision outcomes, the individual level characteristics of parolees are also hypothesized to have an effect on program attendance (H4). Parolees who are nonwhite, male, younger, not a high school graduate, and higher risk should all be associated with less program attendance. The results of the regression of program attendance (*AttdsTotal*) on the five individual characteristics in HLM 7.3 are reported in Table 5.13. *HighSchool* ($b = 1.390$, $p < .05$) is the only individual level characteristic with a significant effect on *AttdsTotal*. High school graduates are expected to have .39 more attendances than parolees without a high school diploma or GED (Table 5.13).

Thus far, analyses of the effects of individual level variables on outcomes and attendance have found that only Risk consistently predicts outcomes but it does not predict attendance. Comparing results in Tables 5.12 and 5.13 reveals that the individual level characteristics of parolees influence how attendance affects other dependent variables but, except for education, individual level characteristics do not directly affect the level of program attendance. The study turns next to multilevel analyses.

Table 5.13.
Regression of Program Attendance on Five Individual Level Variables (n = 968)

	B (S.E.)
Constant	10.570***
	(2.703)
Male	-1.492
	(1.504)
NonWhite	-.942
	(.890)
HighSchool	1.390*
	(.686)
Age	.005
	(.042)
Risk	-.205
	(.154)

*** p. <.001; * p. < .05

Social Disorganization Theory - Testing Multilevel Hypotheses (H5, H6, H7)

Social disorganization theory posits that structural conditions explain differences in crime across neighborhoods. This study examines whether parole outcomes, programs used by parolees, and program participation are associated with variation in structural conditions across neighborhoods where parolees reside. *H5* states that increasing levels of neighborhood *Disadvantage, Mobility*, and *Proportion Black* will be associated with an increase in the number of parolees who experience arrests, revocations and other outcomes. *H6* states that program attendance moderates the effects of neighborhood structural conditions on parole outcomes. *H7* examines possible mediating processes that involve both neighborhood and individual level variables. The level-1 variables in these multilevel hypotheses include the outcome measures, individual level characteristics, and program attendance. Neighborhood (census tract) level variables include the three measures of structural conditions discussed earlier, as well as the number of programs in each neighborhood.

The creation of appropriate models for multilevel analysis first requires an examination to determine the intraclass correlation coefficient (ICC). The ICC is a measure of the variance that can be attributed to differences between groups (Bryk & Raudenbush, 1992, pp. 94-95; Kreft & DeLeeuw 1998, p. 9; Snijders & Bosker, 1999, pp. 16-17). Since not all grouping results in significant differences in variance between groups, the ICC indicates whether multilevel analysis is justified. To determine how much of the variance in the dependent variables is between groups an intercept only model is examined. Based on the form of the dependent variable, two equations are used to determine the ICC for the eight outcome measures in the multilevel hypotheses (*H5-H7*). For the seven principle dependent measures which have a Bernoulli distribution (Snijders & Bosker, 1999, p. 224) the equation for the ICC is:

$$\rho_{\mathrm{I}} = \frac{\tau_0^2}{\tau_0^2 + \pi^2/3}$$

Intercept only models in HLM 7.3 produced values for τ to determine the ICC (Table 5.14). The highest level of ICC (variance explained) for a dependent variable at level-2 is for *EarlyOut* at 4.1% with all other variables at 1.4% or less. The ICC for the other five dependent measures is < 1%. Except for *EarlyOut*, none of the levels of variance explained justify using multilevel models for any of these dependent measures (*Violation, Drug, ViolArrest, FelArrest, Revocation, LateOut*). As noted earlier, program attendance *(AttdsTotal)* is treated as a normal distribution and thus, for determining the ICC, uses the equation (Snijders & Bosker, 1999, p. 17):

$$\rho_I = \frac{\tau^2}{\tau^2 + \sigma^2}$$

The predicted variance in program attendance explained across neighborhoods is 24%, supporting a more in-depth analysis of the effects of neighborhood conditions on program attendance (Table 5.14). The study next turns to examinations of the multilevel hypotheses.

Table 5.14.
Intraclass Correlation Coefficients for Eight Dependent Variables

Outcome Variable	ICC	Variance Explained
Violation	.00412	.41%
Drug	.00212	.21%
ViolArrest	.0142	1.4%
FelArrest	.0001	.01%
Revocation	.0009	.09%
EarlyOut	.041	4.1%
LateOut	.001	.10%
AttdsTotal	.238	23.8%

H5 and intraclass correlation coefficients: Multilevel analysis using HLM 7.3 is conducted based on the premise that cases clustered in different ways, such as in neighborhoods, may violate the assumption of independence of error terms. The violation of independence may arise from neighborhood specific experiences that are not likely to be shared outside the neighborhood.

Therefore, clustering effects must be considered to more fully account for differences in outcomes. Social disorganization theory adopts this view by suggesting that variation in levels of poverty, mobility, and heterogeneity across neighborhoods influences residents and others in the neighborhood. Thus, *H5* holds that neighborhood conditions (*Disadvantage, Mobility, Proportion Black, TractPgms)* will have significant effects on the seven primary dependent measures (*Violation, Drug, ViolArrest, FelArrest; Revocation, EarlyOut, LateOut*).

Based on the extremely low ICC's for all of the dependent measures except *EarlyOut* (Table 5.14), it has been shown that for this sample of parolees there is insufficient variation in outcomes across the 65 neighborhoods (census tracts) to justify multilevel analyses. Multilevel analysis will not find a significant relationship between six of the supervision outcomes related to neighborhood levels of *Disadvantage, Mobility, Proportion Black, and TractPgms.* Therefore, for these outcomes the null hypothesis for *H5* cannot be rejected.

To test *H5* with the outcome *EarlyOut,* HLM 7.3 is used with a Bernoulli distribution for the outcome. Level-1 variables, which are all mean centered, include the five individual characteristics. Level-2 variables include the three neighborhood level conditions and *TractPgms.* Since the analysis does not include attendance, the full sample of 1,637 paroles is used. Model-1 in Table 5.15 includes only the individual level variables. *Male, HighSchool,* and *Risk* are significant in the expected directions and remain significant in all models. These levels of significance are consistent with previous analyses of *EarlyOut* using the 968 program-enrolled subsample. Model-2 in Table 5.15 adds neighborhood programs (*TractPgms*); Model-3 includes individual level characteristics and the three neighborhood conditions; and model-4 includes all

Table 5.15.
Multilevel Analyses of the Effects of Programs and Neighborhood Conditions on EarlyOut (N = 1,637)

	1	2	3	4
	b	b	b	b
	(s.e.)	(s.e.)	(s.e.)	(s.e.)
Constant	.449***	.449***	.453***	.452***
	(.707)	(.066)	(.066)	(.064)
Male	.546**	.559**	.545**	.555**
	(.179)	(.182)	(.181)	(.181)
NonWhite	-.198	-.175	-.189	-.179
	(.155)	(.154)	(.156)	(.156)
HighSchool	-.402***	-.410***	-.406***	-411***
	(.105)	(.105)	(.106)	(.106)
Age	-.004	-.004	-.005	-.005
	(.005)	(.005)	(.005)	(.005)
Risk	.159***	.159***	.160***	.159***
	(.026)	(.026)	(.026)	(.027)
TractPgms		.025		.019
		(.014)		(.016)
Disadvantage			-.064	-.045
			(.081)	(.083)
Mobility			.131*	.100
			(.061)	(.065)
Proportion Black			-.019	.083
			(.439)	(.392)

*** p. <.001; ** p < .01; * p< .05

level-2 variables. Neither *TractPgms* nor any of the neighborhood conditions is significant except for *Mobility* (b = .131, p < .05) which is only significant in model-3 without programs. As neighborhood mobility increases poorer outcomes become more likely. However, *Mobility* is no longer significant in the full model which includes *TractPgms.* This is likely due to the significant correlation between *Mobility* and *TractPgms* (Table 5.9, r = .283, p < .01) which renders both variables as insignificant. Therefore, when all the variables are included in the analysis, neighborhood conditions and the number of programs in a neighborhood do not have any significant effect on early parole outcomes and the *H5* null hypothesis cannot be rejected. The research next turns to whether program attendance moderates the effects of neighborhood conditions on outcomes.

Program attendance moderating neighborhood conditions (H6): In addition to the direct effect that program attendance has on supervision outcomes, H6 states that AttdsTotal also moderates the adverse effects of community structural conditions on outcomes. While the low ICC's for six of the seven outcomes do not permit testing these outcomes for a direct relationship with community conditions, moderation effects related to program attendance may still be present. Therefore, the seven outcome variables are included in the examination for moderating effects. All variables were grand mean centered. Moderation requires the creation of interaction terms between attendance and each of the neighborhood conditions. A facility in HLM 7.3 frees the individual level variable to vary across neighborhoods. This process is handled by the HLM software.13 Moderation results are found in Table 5.16a-g.

Models 2-4 for each outcome report the results of any effects for the *DIS*Attds*, *MOB*Attds*, and *Proportion Black*Attds* moderators respectively. *MOB*Attds* is the only statistically significant moderator for *Revocation* (b = -.052, p < .001). As with the previous

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[13] It is noted that the smaller number of parolees in this sample of 'enrolled in program' resulted in a reduction in the number of parolees in several census tracts, with six census tracts having from seven to ten parolees. The other 59 census tracts have from 10 to 44 parolees.

Table 5.16a.
Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 968)

	Violation			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	.150* (.072)	.151* (.071)	.151* (.072)	.150* (.071)
Male	.423 (.223)	.419 (.223)	.411 (.224)	.415 (.222)
NonWhite	-.470** (.154)	-.454** (.159)	-.451** (.158)	-.451** (.160)
HighSchool	-.250 (.134)	-.234 (.135)	-.236 (.136)	-.237 (.135)
Age	-.001 (.007)	-.002 (.007)	-.002 (.007)	-.002 (.007)
Risk	.080* (.033)	.079* (.033)	.079* (.034)	.079* (.033)
AttdsTotal	.015** (.005)	.015*** (.005)	.016** (.005)	.016** (.005)
TractPgms	.010 (.012)	.008 (.013)	.008 (.013)	.007 (.013)
Disadvantage		.053 (.074)	.051 (076)	.051 (.076)
Mobility		.037 (.077)	.038 (.077)	.037 (.077)
Proportion Black		-.224 (.395)	-.216 (.396)	-.218 (.392)
DIS*Attds		.004 (.005)		
MOB*Attds			.002 (.005)	
Proportion Black*Attds				.001 (.030)

*** p. <.001; ** p < .01; * p< .05

Table 5.16b.

Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 932)

	Drug[1]			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	.269*** (.072)	.274*** (.070)	.282*** (.070)	.276*** (.070)
Male	.233 (.232)	.224 (.234)	.214 (231)	.230 (.233)
NonWhite	-.127 (.186)	-.086 (.185)	-.101 (.186)	-.083 (.188)
HighSchool	-.159 (.143)	-.166 (.144)	-.154 (.144)	-.161 (.145)
Age	-.005 (.008)	-.004 (.008)	-.004 (.008)	-.005 (.008)
Risk	.173*** (.042)	.175*** (.043)	.174*** (.043)	.175*** (.043)
AttdsTotal	.033*** (.008)	.035*** (.009)	.036*** (.043)	.035*** (.008)
TractPgms	.016 (.011)	.010 (.012)	.010 (.012)	.010 (.012)
Disadvantage		-.047 (.095)	-.035 (.094)	-.034 (.093)
Mobility		.048 (.073)	.068 (.078)	.048 (.073)
Proportion Black		-.337 (.416)	-.338 (.420)	-.414 (.422)
DIS*Attds		-.011 (.007)		
MOB*Attds			.016 (.011)	
Proportion Black*Attds				-.036 (.044)

*** p. <.001; ** p < .01; * p< .05

[1] Parolees enrolled in a program and drug tested one or more times

Table 5.16c.
Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 968)

	ViolArrest			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-.948***	-.958***	-.952***	-.951***
	(.082)	(.081)	(.090)	(.080)
Male	.301	.278	.279	.280
	(.258)	(.261)	(.260)	(.260)
NonWhite	-.249	-.254	-.262	-.249
	(.221)	(.234)	(.260)	(.233)
HighSchool	-.198	-.190	-.261	-.186
	(.150)	(.152)	(.233)	(.154)
Age	.011	.011	.010	.010
	(.008)	(.008)	(.008)	(.008)
Risk	.017	.017	.016	.017
	(.039)	(.039)	(.039)	(.039)
AttdsTotal	.042***	.043***	.042***	.044***
	(.005)	(.005)	(.005)	(.005)
TractPgms	.026	.024	.024	.024
	(.015)	(.017)	(.017)	(.017)
Disadvantage		.039	.036	.039
		(.085)	(.594)	(.085)
Mobility		.134	.132	.134
		(.077)	(.077)	(.077)
Proportion Black		.230	.214	.196
		(.600)	(.594)	(.598)
DIS*Attds		-.008		
		(.004)		
MOB*Attds			.004	
			(.006)	
Proportion Black*Attds				-.033
				(.034)

*** p. <.001; ** p < .01; * p< .05

Table 5.16d.
Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 968)

	FelArrest			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	1.496***	-1.499***	-1.502***	-1.498***
	(.080)	(.080)	(.080)	(.080)
Male	.406	.409	.413	.400
	(.408)	(.408)	(.410)	(.405)
NonWhite	-.015*	-.044	-.038	-.039
	(.233)	(.239)	(.239)	(.405)
HighSchool	-.124	-.122	-.132	-.039
	(.177)	(.179)	(.180)	(.240)
Age	-.013	-.014	-.013	-.013
	(.010)	(.010)	(.010)	(.010)
Risk	.111**	.110**	.110**	.110**
	(.041)	(.040)	(.040)	(.040)
AttdsTotal	-.017	-.018	-.018	-.017
	(.012)	(.012)	(.012)	(.008)
TractPgms	-.029**	-.027*	-.028*	-.028*
	(.010)	(.011)	(.011)	(.011)
Disadvantage		.006	-.004	-.005
		(.079)	(.073)	(.073)
Mobility		.033	.020	.034
		(.079)	(.084)	(.078)
Proportion Black		.328	.320	.314
		(.440)	(.445)	(.455)
DIS*Attds		.007		
		(.012)		
MOB*Attds			-.010	
			(.013)	
Proportion Black*Attds				-.012
				(.078)

*** p. <.001; ** p < .01; * p< .05

Table 5.16e.

Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 968)

	Revocation			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	-2660***	-2.703***	-2.735***	2.697***
	(.144)	(.141)	(.147)	(.154)
Male	.320	.296	.326	.315
	(.473)	(.466)	(.470)	(.470)
NonWhite	-.143	-.079	-.068	-.088
	(.311)	(.323)	(.322)	(.316)
HighSchool	.074	.097	.063	.099
	(.277)	(.283)	(.283)	(.278)
Age	.007	.008	.006	.005
	(.015)	(.015)	(.016)	(.015)
Risk	.368***	.377***	.374***	.372***
	(.048)	(.048)	(.049)	(.049)
AttdsTotal	-.058**	-.060***	-.066***	-.064***
	(.019)	(.017)	(.017)	(.020)
TractPgms	-.022	-.028	-.028	-.026
	(.014)	(.015)	(.015)	(.015)
Disadvantage		.016	.135	.126
		(.138)	(.100)	(.010)
Mobility		.110	-.066	.102
		(.120)	(.123)	(.119)
Proportion Black		-.677	-.761	-.156
		(.577)	(.587)	(.698)
DIS*Attds		-.028		
		(.018)		
MOB*Attds			-.052***	
			(.015)	
Proportion Black*Attds				.175
				(.130)

*** p. <.001; ** p < .01; * p< .05

Table 5.16f.
Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 968)

	EarlyOut			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	1.026*** (.086)	1.044*** (.085)	1.056*** (.087)	1.047*** (.087)
Male	.510* (.252)	.521* (.252)	.506* (.251)	.517* (.251)
NonWhite	.278 (.187)	-.253 (.195)	-.258 (.195)	-.240 (.198)
HighSchool	-.450** (.146)	-.465** (.148)	-.457** (.150)	-.465** (.149)
Age	-.011 (.008)	-.102 (.008)	-.010 (.008)	-.011 (.008)
Risk	.164*** (.037)	.168*** (.038)	.169*** (.038)	.169*** (.038)
AttdsTotal	.025*** (.008)	.026*** (.008)	.029*** (.008)	.028*** (.008)
TractPgms	.016 (.012)	.002 (.014)	.003 (.014)	.002 (.014)
Disadvantage		-.107 (.092)	-.103 (.094)	-.101 (.094)
Mobility		.164 (.083)	.190* (.083)	.164 (.083)
Proportion Black		-.371 (.592	-.360 (.602)	-.448 (.623)
DIS*Attds		-.003 (.007)		
MOB*Attds			.017 (.010)	
Proportion Black*Attds				-.041 (.051)

*** p. <.001; ** p < .01; * p< .05

Table 5.16g.
Multilevel Test of the Effects of Attendance Moderating Neighborhood Conditions (n = 968)

	LateOut			
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	1.351***	-1.352***	-1.357***	-1.355***
	(.072)	(.070)	(.071)	(.071)
Male	.362	.358	.368	.357
	(.368)	(.367)	(.369)	(.367)
NonWhite	-.120	-.126	-.119	-.132
	(.214)	(.222)	(.222)	(.222)
HighSchool	-.195	-.183	-.296	-.187
	(.167)	(.167)	(.169)	(.168)
Age	-.007	-.008	-.008	-.008
	(.010)	(.010)	(.010)	(.010)
Risk	.137***	.136***	.137***	.136***
	(.012)	(.038)	(.039)	(.039)
AttdsTotal	-.020	-.021	-.021	-.022
	(.012)	(.012)	(.012)	(.012)
TractPgms	-.030***	-.031***	-.031***	-.031***
	(.008)	(.009)	(.009)	(.009)
Disadvantage		.027	.018	.017
		(.076)	(.070)	(.070)
Mobility		.067	.048	.068)
		(.082)	(.867)	(.081)
Proportion Black		.096	.083	.163
		(.340)	(.345)	(.374)
DIS*Attds		.006		
		(.012)		
MOB*Attds			-.013	
			(.013)	
Proportion Black*Attds				.038
				(.080)

*** p. <.001; ** p < .01; * p< .05

findings (Table 5.12-e), the independent variable *AttdsTotal* decreases the likelihood of revocation. The findings here related to *MOB*Attds* also suggest that program attendance may be particularly beneficial in neighborhoods with high Mobility. For this sample, in neighborhoods with higher levels of *Mobility*, program attendances are increasingly likely to decrease *Revocation*. Thus, while higher levels of the independent variable *Mobility* are hypothesized to lead to higher levels of revocation, this analysis finds that as the level of *Mobility* in the interaction term increases, the likelihood of program attendance reducing revocation also increases.

Put another way, this moderation reveals that the revocation-enhancing effects of higher mobility are dampened by program attendance leading to a reduced likelihood of revocation. This finding confirms the hypothesis that at least for one neighborhood level condition, program attendance lowers the positive effects of higher levels of *Mobility* on *Revocation* which is one of the two most important outcomes.

Two other findings are noted in the above analyses. The single variable *Mobility* ($b = .190$, $p < .05$) has a significant and positive effect on *EarlyOut* but only in the model testing the *MOB*Attds* moderator which is not significant. As mobility increases the likelihood of one or more of the three supervision activity non-criminal violations in *EarlyOut* also increases. It should be noted that *EarlyOut* was the only outcome with an ICC large enough to justify testing for neighborhood effects.

Second, increasing the number of programs in a neighborhood lowers the likelihood of *FelArrest* and *LateOut* in all models (Table 5.16d & g). Since *TractPgms* is not associated with *Revocation,* and *LateOut* is the combined total of *FelArrest* and *Revocation,* the significant effect with *LateOut* is likely from the contribution of *FelArrest* and not *Revocation*. *TractPgms* is not significant in Table 5.16f related to *EarlyOut* nor is it significant in the *EarlyOut* analysis reported in Table 5.15. However, unlike the program-enrolled subsample (n = 968) reported on in Table 5.16f, the earlier table (5.15) reports on the full sample of parolees enrolled and not enrolled in programs (N = 1,637) and does not include attendance or the neighborhood *condition * attendance* moderators. Therefore, interpreting the results related to *TractPgms* is confusing due to the samples used, the variables used, or by not examining an important group – non-enrolled on *FelArrest, Revocation* and *LateOut*. The low ICC's contribute to creating this confusion.

Because significant results were found with *FelArrest* and, since a principle concern of this study is understanding whether variation in the number of programs across neighborhoods has any effect on outcomes, additional exploratory analyses were conducted (not reported here). The purpose was to more fully examine whether the influence of *TractPgms* may also apply to parolees not enrolled in programs. The variables used in the analysis and reported in Table 5.15 (individual level characteristics, *TractPgms, Disadvantage, Mobility, Proportion Black*) were used in these analyses examining the possible effects of *TractPgms* with the full sample of parolees (N = 1,637) and then with the program-enrolled subsample (n = 968) on *FelArrest*, *Revocation,* and *LateOut.* The analyses were repeated with the program-enrolled subsample to compare with the results in Table 5.15 on *EarlyOut.* The ICC's for the outcomes on these sample outcomes were below .10%.

The three analyses using the full sample of enrolled and not enrolled (N = 1,637) parolees found *TractPgms* is not a significant predictor of *FelArrest*, *Revocation,* or *LateOut.* As for the analysis of the program-enrolled subsample, the results in Table 5.16d & g are confirmed. *TractPgms* is significant for predicting *FelArrest* and *LateOut* but not *Revocation.* As for *EarlyOut*, the results did not change; *TractPgms* does not predict *EarlyOut* for the subsample of parolees enrolled in programs. Therefore, the analyses suggest that for parolees enrolled in programs, more programs in a neighborhood is associated with a reduction in *FelArrest* and *LateOut* but is not associated with the aggregated supervision activity outcome *EarlyOut.*

Additionally, the exploratory analyses with the full sample suggest that the number of programs in a neighborhood may not benefit parolees who are not enrolled in programs. Nonetheless, the *FelArrest* finding related to program-enrolled parolees is noteworthy because *FelArrest* along with *Revocation* are the most important and most reported outcomes due to the impact of serious crime on the community and on correctional costs. The importance of programs nearby confirms other research findings (Hipp et al., 2010) and will be discussed in greater detail in the next chapter.

Lastly, related to community conditions, it is again noted that in this sample of 968 parolees who are required to be involved in programming, program attendance further decreases the likelihood of *Revocation* in neighborhoods with elevated levels of *Mobility.* This research now turns to the final hypothesis (*H7*) which posits mediating effects by neighborhood programs on program attendance and by program attendance on outcomes.

Mediation of neighborhood conditions (H7): According to social disorganization theory, adverse conditions such as disadvantage, mobility, and heterogeneity weaken neighborhood social structure which leads in-part to inadequate resources to serve community needs. RNR suggests that program access and program attendance are important for reducing reoffending and other types of violations. *H7a* examines links between these two theories whereby variation in neighborhood conditions may be associated with variation in the number of neighborhood programs frequently used by parolees. Specifically, *H7a* states that the number of programs in a neighborhood mediates the effects of neighborhood conditions on parolee program attendance.

HLM 7.3 was employed to conduct the first step in the multilevel regression of the dependent measure - *AttdsTotal* - on the independent variables (*Disadvantage, Mobility, Proportion Black*) (Table 5.17, model-1). The analysis found that none of the neighborhood conditions is significant in predicting attendance, hence, no mediation exists for this sample of parolees and neighborhoods (Baron & Kenny, 1986). However, related to social disorganization theory and apart from the mediation hypothesis, questions remain as to the relationships between programs and the level of program attendance, and neighborhood conditions and the number of programs across neighborhoods. These questions are examined in Table 5.17. Model-2 illustrates the results of the regression of *AttdsTotal* on *TractPgms* which indicates that variation in the number of programs across neighborhoods has no effect on the level of program attendance for this sample of parolees.

Next, *TractPgms* was regressed on the three neighborhood conditions (*Disadvantage, Mobility, Proportion Black*) to investigate possible effects on variation in the number of programs. Neighborhood level structural conditions and program totals are all aggregated at the neighborhood level which allows for single level models with N= 65. The number of programs in a neighborhood is a skewed count variable. The analysis was carried out using negative

Table 5.17.
Mediation of Neighborhood Conditions and Programs on Attendance and Outcomes (N = 968)

	AttdsTotal (n = 968)		Pgms (N = 65)	EarlyOut (n = 968)
	1	2	3	4
	b (s.e.)	b (s.e.)	b (s.e.)	b (s.e.)
Constant	8.101***	4.501***	1.830	.1.012***
	(.422)	(.375)	(1.168)	(.085)
Male	-1.456	-1.055		.473
	(1.508)	(.992		(.247)
NonWhite	-1.070	-1.367		-.305
	(.868)	(.638)		(.184)
HighSchool	1.320	-1.367*		-.416**
	(.688)	(.638)		(.147)
Age	.007	.030		-.011
	(.042)	(.426)		(.008)
Risk	-.204	.056		.159***
	(.155)	(.109)		(.012)
Disadvantage	-.383		-.432†	
	(.401)		(.250)	
Mobility	.003		.421†	
	(.477)		(.244)	
Proportion Black	2.315		-.959	
	(2.222)		(1.44)	
TractPgms		.084		.018
		(.063)		(.012)

*** p. < .001; ** p. < .01; * p. < .05; † p. < .10

binomial with a log-link function to account for the over-dispersed distribution of the program types. The deviance value for the analysis was just over one indicating an acceptable model fit. In this analysis with N = 65, a significance level of $p < .10$ is noted. Table 5.17, model-3 illustrates that both *Disadvantage* (b = -.432. p < .10) and *Mobility* (b = .421, p < .10) are marginally significant neighborhood predictors of the number of neighborhood programs. As the level of neighborhood *Disadvantage* increases, the number of neighborhood programs decreases.

On the other hand, *Mobility* has the opposite effect. An increase in neighborhood *Mobility* is associated with an increase in the number of neighborhood programs. Therefore, although no significant mediating path was uncovered between neighborhood conditions, programs, and attendance, this analysis did establish a weak but significant relationship between conditions in neighborhoods and the presence of programs used by parolees in those neighborhoods. The final question in this research investigates the second part of *H7.*

H7b states that *AttdsTotal* mediates the effect of neighborhood programs on outcomes. An earlier hypothesis -*H2*- addressed the question of the effects of program attendance on the seven outcomes (Table 5.11). Program attendance was found to have positive, negative, and no effects depending on the outcome. The question of the influence of the number of programs on program attendance is important as effective supervision may be undermined if program attendance is adversely associated with areas low in program availability. Hipp et al. (2010) found a strong positive relationship between the number of programs nearby offenders and supervision outcomes. The key first question for this test of mediation is whether there is a statistically significant relationship between the independent variable in the mediation - *TractPgms* - and the dependent variable - outcomes. The examination of this question is limited to *EarlyOut*, the only outcome with an ICC of sufficient size to justify a multilevel test. Following steps used in the previous mediation, regression using a Bernoulli distribution in HLM 7.3 was constructed with dependent measure *EarlyOut* and independent measure *TractPgms.* The results are shown in Table 5.17 model-4. *TractPgms* is not associated with change in *EarlyOut*. Indeed, the findings in this model which includes *TractPgms* are very similar to the model in 5.11-c (Model 1) which examines just the individual level variables. Therefore, it cannot be shown that *AttdsTotal* mediates the effects of *TractPgms* on the outcome variable *EarlyOut* and the null hypothesis for *H7b* cannot be rejected.

One interesting finding in this research is noted related to testing the effects of program attendance on outcomes. The research cited above related to ICC values is clear that multilevel research analyses be guided by this preliminary investigation of when to account for group effects and use multilevel models. Table 5.16 reports on the results of the investigation of moderation of neighborhood conditions by program attendance using the program-enrolled subsample of 968 parolees. Each model in the set of moderation equations includes the five individual level characteristics of each parolee, *AttdsTotal,* and *TractPgms.* Based on the ICC values for each outcome, *TractPgms* would not be expected to be statistically significant.[14] However, *TractPgms* is significant ($p < .05$) in the models for *FelArrest* and for *LateOut* which combines *FelArrest* and *Revocation.* Interestingly, *AttdsTotal* is significantly related to the other five outcomes but not to *FelArrest or LateOut.* This will be discussed in greater detail in the next chapter.

In summary, while the core of the *H7* mediation analyses were not significant or could not be tested due to lack of variation in the dependent measures across neighborhoods, certain paths such as the effects of neighborhood conditions on programs and the effects of program

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[14] ICC's not reported here were calculated on the seven outcomes for the subset of parolees enrolled in a program (N = 968). The highest value was 1.00% for ViolArrest with all other values being .20% or less.

attendance on outcomes are significant. Neighborhood conditions matter for the level of programs, as does attendance for parolee outcomes although in some cases the results are not in the desired direction. The next chapter discusses findings from the seven hypotheses, places them in the context of the communities where these parolees reside, and attempts to make sense of the results within the wider framework of other research on community corrections and what is known about social disorganization theory and Risk-Need-Responsivity.

CHAPTER VI: DISCUSSION AND CONCLUSION

Introduction

Between 2011 and 2016 the total U.S. prison population experienced a net decline of 89,000 inmates (Kaeble & Cowhig, 2018). Similarly, beginning in 2008 the total U.S. probation population began a steady decrease, falling by 80,000 in the number of offenders under supervision. On the other hand, the total U.S. parole population increased during all but two years of this time period, from 826,100 in 2011 to 870,500, an overall increase of 44,000 (Kaeble & Cowhig, 2018). At the same time, 28% (159,506) of 2014 prison admissions in the U.S. were parolees arrested for a new crime or for violating the conditions of supervision (Carson, 2015). In the state of Georgia, a total of 2,525 parolees were sent back to prison during 2018 but less than 2% of revocations were based solely on non-criminal violations. Failure on parole undermines attempts to reduce overall prison populations.

The continuing contribution of parole failure to overall prison populations has not gone unnoticed. During this time, a parallel movement known as “reentry” (National Research Council, 2008, p. 14) sought to improve supervision practices and programs provided to offenders leaving prison. The goal of reentry is to reduce the number of offenders who return to prison for violations and new crimes. Reentry strategies are being tested throughout the U.S. with a focus on employment and services offered directly to these ‘returning’ offenders (D’Amico, Geckeler, & Kim, 2017).

This well-intentioned and necessary focus on offender-specific criminogenic needs which is encapsulated in the Risk-Need-Responsivity (RNR) model, may be neglecting an important factor in re-offending. Community conditions such as poverty, mobility, and heterogeneity across neighborhoods are also thought to affect rates of re-offending. Therefore, a more expansive approach to understanding re-offending that includes consideration of individual level characteristics, activities that address criminogenic needs, and community conditions is necessary to close the gap in our understanding of why offenders re-offend.

In fact, recent research has begun to highlight the combined effects of addressing parolee specific needs and community influence on parole outcomes. Two studies (Hipp et al., 2010; Hipp et al., 2011) found that the presence and estimated capacity of programs, and the proximity of programs to where parolees live are significantly associated with better outcomes. Living closer to programs yields better outcomes, especially for black parolees. Related to neighborhood conditions, these studies also found a positive relationship between disadvantaged neighborhoods and increasing parole failure. But the research did not include any information related to the actual effects of program attendance.

The present study extends this research to include active program attendance, particularly focusing on programs vital to successful offender reentry [e.g., programs addressing criminal thinking (COG), mental health (MH), and substance abuse (SA)]. Moreover, the two studies by Hipp et al. (2010, 2011) are advanced by using multilevel models to investigate whether neighborhood structural conditions (*Disadvantage, Mobility, Proportion Black*) influence and are influenced by program attendance and whether the number of programs affects the level of program attendance. By including measures at the neighborhood level, this study addresses the fundamental question of whether neighborhood structural conditions directly or through program availability and attendance affect parole outcomes.

Furthermore, this research investigates the influence of neighborhood conditions on a variety of supervision outcomes. Risk-Need-Responsivity (RNR) suggests programming addressing criminogenic needs is key to reducing parole failure. If neighborhood conditions lead to fewer programs of the type that are associated with reductions in re-offending, supervision agencies that fail to acknowledge and address these disparities will continue to register unnecessarily higher supervision failure rates.

The review of findings and discussion is organized around the two theoretical lenses that underlie this study, social learning theory as operationalized through RNR and social disorganization theory. RNR focuses on the attributes and behaviors associated with offending. The RNR associated variables used in this research are individual level characteristics that theoretically identify the highest risk offenders with the greatest needs and program attendance, the supervision activity theorized to reduce the risk of violations and reoffending. Social disorganization theory suggests certain neighborhood conditions where offenders reside have an adverse effect on their supervision outcomes. The neighborhood level variables in this study are the levels of *Disadvantage, Mobility,* and *Proportion Black,* and the number of programs (*TractPgms*) in each neighborhood.

This study investigates the possible direct effects of community conditions on parole outcomes and whether program attendance moderates the effects of community conditions on outcomes. Lastly, the study examines the intervening effects of neighborhood program availability and program attendance on outcomes. The next section summarizes and discusses the findings from the current study.

Summary and Discussion of Research Findings

The summary and discussion of research findings begins with a description and brief discussion of the dependent variables used in this research to establish a context for understanding the effects of independent variables. The chapter then turns to hypotheses related to RNR, then hypotheses having to do with social disorganization theory and last, to hypotheses that integrate both theories. A final section highlights the important findings and questions raised by the research. The last sections are the limitations of this study, policy implications, and conclusion.

Dependent Variables

This research is unique in several respects, one of which is the more nuanced ways in which the dependent measures of parole failure are captured, especially as compared to other research examining the outcomes of offenders during their time under community supervision. Re-arrest for a new crime, re-conviction, reincarceration, and revocation are often found in the literature as dependent measures for offender supervision outcomes. These benchmarks are found in risk assessment instruments noted in the empirical review in Chapter 3 and in Chapter 4 regarding Bureau of Justice Statistics' (BJS) annual reports of measures of probation and parole failure. Felony arrest and revocation are two of the dependent measures in the present study.

The present study includes three other dependent measures - technical violation (*Violation*), positive drug test (*Drug*), and technical violation arrest (*ViolArrest*) - which could also be described as failures related to supervision activity. While these activities are common and routine to parole and probation supervision, the research literature is largely silent about such measures as stand-alone dependent variables. Instead, when these measures are included as dependent variables, they are aggregated along with new crime activity in a catch-all dependent variable called re-incarceration or revocation. Therefore, prior to this research little was known about these three supervision ‘non-crime’ activities as unique dependent variables separate from new crimes.

Moreover, the literature is mostly silent about how failures related to supervision activity may be influenced by the individual characteristics of parolees, by other supervision activity such as program attendance, or by how supervision is administered. Finally, the literature is silent about the relationship between such activity measures and neighborhood conditions. The findings discussed here raise important questions about how to assess the roles of community supervision, parolee characteristics and supervision activity, and neighborhood context in parole outcomes.

RNR and Reoffending

Table 6.1 summarizes the findings related to four research questions: the effects of individual level measures theoretically associated with RNR on arrests and other supervision outcomes (model-1); the effects of program attendance (*AttdsTota*l) on outcomes (model-2); the moderating effects of program attendance on individual level characteristics (model-3); and the effects of individual level characteristics on program attendance (model-4). The effects of individual level characteristics on outcomes are summarized and discussed first.

Individual level characteristics: Risk is the first principle of the RNR model of offender change. Which offenders need more supervision and enrollment in programs is determined by the level of risk to reoffend. Identifying which offenders need programs is important for two reasons. Program attendance is thought to lower the re-offending rates of high-risk offenders.

Conversely, the re-offending rates of low-risk offenders may increase if needlessly required to attend programs.

As reviewed in Chapter 3, risk assessment has a long history in offender management and a core set of offender characteristics have been used to assess risk including the five variables in this research. Unlike the other four individual level characteristics, *Risk* is a composite of ten measures which include age, seven criminal history measures, and two measures related to health. The *Risk* measure was created to predict arrest for a new felony offense. As importantly, the *Risk* variable used here was created and validated on the population from which the sample is drawn. The empirical literature has consistently found that composite instruments consisting of as few as five to ten variables emphasizing criminal history and physical/mental health are highly predictive of future criminal behavior (Kroner et al., 2005; Zhang et al., 2014). Moreover, the empirical literature review cites research finding composite risk measures such as the LSI-R and COMPAS consistently reliable predictors of reoffending, re-arrest, and return to incarceration. Of the five individual level characteristics in this study, *Risk* is the only variable that consistently predicts an increase in the seven outcomes across single and multilevel models except for predicting violation arrest (Tables 6.1; 6.1; & 6.3). As risk score increases, so does the likelihood of the outcome. Importantly, the risk score significantly predicts an increasing likelihood of both *FelArrest* and *Revocation*.

Male, NonWhite, and *HighSchool* are significant in a small number of models across dependent measure tests but in no specific patterns across the single and multilevel level analyses and *Age* does not predict any of the outcomes. The research literature has found that being male is associated with higher supervision failure and re-arrest rates (Andrews & Bonta, 2010). In this study, *Male* is associated with an increase in *Violation*, *FelArrest,* and *Revocation* but not across all models. Black parolees have an increased likelihood only across all models for *Violation.* Contrary to the hypothesis, black parolees are less likely than white parolees to have a violation. Both Kubrin and Stewart (2006), and Hipp and colleagues (2010) found race and gender significant across all models in predicting re-arrest or return to prison. That research had much larger cohorts but did not include a composite risk variable. Although, as noted here, *NonWhite, Male* and *HighSchool* are significant in some models, the importance of *Risk* is demonstrated by its significance in every model for all outcomes except *ViolArrest*.

Two explanations may account for these differences in significance. Gender and race may indeed predict re-arrest and return to prison but at much smaller levels of significance for most of the outcomes and therefore, require larger sample sizes to detect. Second, the validated composite risk measure used here was created by first sorting through many possible individual level measures to arrive at the best fitting risk prediction model. In so doing, it may reduce to non-significance otherwise significant additional individual level variables in the model. It should be noted that gender and race are not among the measures that comprise the risk score and may still be tapping into, although in a limited way, unaccounted for risk especially with *Male* related to three outcomes. Discussed next is the last individual level characteristic, *Age*. Unlike gender and race, the parolee's age is included as one of the 10 variables in the risk composite score.

Perhaps most unexpected among individual level characteristics is that *Age* does not predict any of the seven outcomes. *Age* is non-significant regardless of the other independent variables in the equation (single or multilevel) or whether examining the full sample or smaller

Table 6.1.
Regression of All Outcomes on Individual-Level Variables, Attendance, and Moderators; and Attendance on Individual-Level Variables

	Violation			Drug[1]			ViolArrest			FelArrest			Revocation			EarlyOut			LateOut			AttdsTotal
	1	**2**	3	1	**2**	3	1	**2**	3	1	**2**	3	1	**2**	3	1	**2**	3	1	**2**	3	4
Male	+									+			+				+					
Nonwhite	-	-	-																			
HighSchool	-															-	-				-	+
Age																						
Risk	+	+	+	+	+	+	+			+	+	+	+	+	+	+	+		+	+	+	
AttdsTotal			+		+	+		+	+		-			-	-		+				-	
Male*Attds															+							
Nonwhite*Attds															+							
Highschool*Attds																						
Age*Attds									-			+									+	
Risk*Attds																						

+ Independent variable has a significant positive effect on the odds of the outcome variable

- Independent variable has a significant negative effect on the odds of the outcome variable

Dark cells = variable not in model

Column 1 - Full sample (1,637) - individual level characteristics

Column 2 - Enrolled in a program (968) - individual level characteristics and attendance

Column 3 - Enrolled in a program (968) - individual level characteristics, attendance, and interaction terms

AttdsTotal - Column 4 - Enrolled in a program (968) - individual level characteristics

[1] Subset of 932 parolees enrolled in a program and drug tested at least one time

number of parolees enrolled in a program. *Age* is a staple for risk prediction, is included in both the LSI-R and COMPAS, and is significant in other research on parolees in the community (Hipp et al., 2010, Kubrin & Stewart, 2006; Tillyer & Vose, 2011). One explanation for the insignificance of *Age* in this study is that it is one of the variables included in the 10 variable composite *Risk* score and is highly correlated with *Risk* (Table 5.9, r = -.504). In fact, in an analysis not shown here using all of the individual level characteristics except *Risk* to predict the five outcomes, *Age* significantly predicts five of the seven outcomes; as age increases the likelihood of *Violation, Drug, FelArrest, EarlyOut,* and *LateOut* decrease.

Previous research suggested the five individual level characteristics of parolees would have significant effects in predicting the two most important outcomes - *FelArrest* and *Revocation*. This was not the case. While including the composite risk score in the analyses may explain the limited significance of the individual level characteristics, another possible explanation is suggested. Significant t-tests results between the means for *NonWhite* and *HighSchool* (Table 5.6) in the study and non-study samples may indicate the study sample is not representative of the population of parolees, and a broader more representative sample may have provided significant results. On the other hand, the consistently significant results related to *Risk* question this explanation of a restricted sample.

Overall, other than the composite risk score, *Male* predicts three of the five outcomes (*Violation, FelArrest, Revocation*), and *NonWhite* predicts *Violation* but opposite of what was predicted. *HighSchool* predicts *Violation* in the expected direction. *Violation* may be the outcome most associated with non-crime supervision activity. The literature has not previously examined how race and education may be related to non-crime supervision activity.

Program attendance and outcomes: The foundation of the RNR approach to offender change is participation by high risk offenders in programs that address criminogenic needs, that is, attributes associated with re-offending. As program participation increases the likelihood of all outcomes should decrease. Table 6.1 models 2 and 3 summarize the findings for the effects of program attendance on the seven outcomes. In line with the tenets of RNR, program attendance is indeed associated with a decrease in *FelArrest, Revocation,* and *LateOut* and the effect of attendance on these outcomes is cumulative. Each additional attendance reduces the likelihood of these outcomes which highlights the importance of the frequency of attendance. As noted earlier, new offenses and return to prison are among the most frequently cited dependent variables in offender research and arguably the most important outcomes for community supervision.

RNR argues and evidence presented in Chapter-3 suggests that attending programs using a cognitive-behavioral approach and that address criminogenic needs reduces re-arrests, re-convictions, and revocations (Andrews & Bonta, 2010). Research related to reducing drug use also confirms the benefits of program attendance. Unexpectedly, program attendance is associated with an increase in one or both models for the three supervision activity outcomes (Table 6.1, *Violation*, *Drug, ViolArrest*). One explanation may be that program attendance places another supervision requirement on parolees enrolled in programs. A missed program attendance is another way to acquire a technical violation or an additional technical violation leading to a violation arrest.

Another explanation for the unexpected positive effect may be that a supervision activity measured with a dichotomous variable is too limited for examining the effects of program attendance. The supervision activity outcomes can and often do occur more than one time during supervision. Technical violations are defined as at least one of 18 different non-compliance activities. A violation arrest is also associated with the number of non-compliance violations and, depending on the parolee's criminal record and risk to re-offend, can occur after one or many technical violations have been recorded. There is no specific number of technical violations that may trigger one or more violation arrests. Correspondingly, parolees can be drug tested many times over the course of the first 12 months of supervision and beyond.

RNR suggests that program attendance reduces the likelihood of the outcome over time. In this case, the expected effect would be a reduction over time in the number of technical violations, positive drug tests, and arrests for technical violations. The variable coding that would capture this change is the total number of attendances and infractions in a time series. This contrasts with the effects of program attendance on felony arrests and revocation which occur most often one time at the end of supervision after all attendances have occurred. Therefore, the nature of supervision activity infractions as outcomes may require rethinking how effects of supervision are assessed and the types and forms of data needed to examine these issues.

Another explanation which may account for the unexpected positive effects of attendance is related to the temporal order of the dependent measure and attendance. A parolee could be enrolled in a program either prior to or after the occurrence of the supervision activity outcome. For example, knowledge of prior drug use by a person soon to be released from prison may prompt a parole officer to enroll the person in a program at release to supervision which, according to RNR, should dampen the likelihood of drug use during supervision. Similarly, records from a previous parole may prompt enrollment in a COG skills program upon release from prison, once again potentially dampening the likelihood of technical violations leading to a violation arrest. Conversely, enrollment in a program may occur after a positive drug test or series of technical violations that culminate in a violation arrest. In this case the ‘outcome’ occurs first followed then by enrollment and program attendance. Since the attendance date is not known, it is impossible to estimate the correct temporal order between program attendance and the outcome.

A third explanation for the positive association between program attendance and the supervision activity outcomes is a mismatch between program type and outcome. A parolee attending a COG program may also have a drug abuse problem that is unaddressed leading to a positive drug test. Similarly, a parolee attending a drug program may also have an undiagnosed mental health condition that leads to one or more technical violations that may culminate in a violation arrest. These mismatches do not invalidate RNR which specifies a match between needs and appropriate programs. However, even considering mis-matches, program attendance is still associated with a lowered likelihood of felony arrest and revocation.

An exploratory analysis was conducted on the SA, COG, and MH program attendances to better understand the program attendance findings. Although the numbers for each program type were small, thus requiring caution, COG program attendance was associated with reductions in *Violation, FelArrest, Revocation, EarlyOut,* and *LateOut.* Although SA, which had the greatest number of attendances, was associated with a reduction in *Revocation,* SA attendance was also associated with an increase in *Violation, Drug,* and *ViolArrest.* MH attendance was associated with an increase in *Violation, ViolArrest,* and *Revocation.*

The SA attendance analysis prompts questions about whether programs adhere to the RNR model or more fundamentally, the validity of the RNR model. For the most part, programs approved for use by parolees in Georgia must first be reviewed by parole agency program managers. However, a program review does not apply to 12-step groups such as Alcoholics Anonymous (AA) and Narcotics Anonymous (NA). Substance abuse treatment represents the largest number of program attendances and 12-step meetings are an unknown portion of these attendances. It may be that these types of programs do not adhere to the requirements for programs in an RNR model. Therefore, attendance may not result in the offender change that can be achieved by more structured programs which then leads to more violation arrests and continued drug use. It should be noted that this explanation runs counter to the finding of negative effects for *Revocation.*

Another explanation found here and seen in other community level offender research is that the study sample does not represent the population of all parolees such that the effects of program attendance are non-generalizable at least as it relates to *Violation, Drug, and ViolArrest.* In this study the neighborhoods in which the sample resides may also not represent a cross-section of all neighborhoods. T-tests provide evidence that both the study sample and neighborhoods in which they reside may not be a normally distributed random sample of the overall population of parolees and neighborhoods. The study sample has significantly less education, a higher percentage that is black, and significantly more individuals with drug and felony arrest outcomes. Reviewed next are findings related to the examination of the moderating effects of program attendance on individual level characteristics of parolees.

Program attendance moderating individual level characteristics: Table 6.1 model-3 provides a summary of the findings examining the influence of program attendance (*AttdsTotal*) to moderate the effects of individual level characteristics on each of the seven outcomes. Across the seven outcomes and five individual level characteristics, moderation was found to have both positive and negative effects related to only three individual level characteristics on four outcomes. Perhaps the most intriguing of these moderating effects is related to *Age.* In other research and as one of ten variables in the composite risk score used in this study, *Age* is a significant predictor of re-arrest and return to prison. Moreover, in the bivariate correlations (Table 5.9) *Age* has a significant and, as expected, negative correlation with four of the five outcomes. In the present study, *Age* moderates the effects of *AttdsTotal* on *ViolArrest* such that as age increases, the positive effects of *AttdsTotal* are slightly diminished reducing the likelihood of *ViolArrest.*

Unexpectedly, *Age* moderates the effects of *AttdsTotal* such that the likelihood of *FelArrest* and *LateOut* increase with each additional program attendance. For older parolees, attendance increases the likelihood of *FelArrest* and *LateOut.* The effect of the *Age* moderator is reversed with younger parolees (below the mean age) such that each attendance decreases the likelihood of these outcomes. This *Age* effect on attendance is good news for younger parolees who tend to be higher risk but in this moderation the effect of age is the opposite of what is expected for older parolees and predicted by RNR - increasing age combined with increasing numbers of attendances would be expected to lower the likelihood of *FelArrest*. One explanation may be that older parolees in this sample are in-fact lower risk but contrary to the recommendations of RNR, are required to attend programs which RNR suggests could lead to worse outcomes.

Two other significant moderation effects are noted (Table 6.1, *Revocation*, model-3). *Male* and *Nonwhite* are both moderated by attendance such that the beneficial effects of program attendance are weakened for male and black parolees which increases their likelihood of *Revocation.* Conversely, the beneficial effects of attendance are strengthened for female and white parolees, which decreases their likelihood of *Revocation.* Once again, as noted earlier, no other research was found that has tested moderating effects using actual measures of attendance.

These moderation findings raise questions about program effectiveness across race and gender, and perhaps age as well, related to the responsivity component of RNR. Responsivity means paying attention to the specific attributes of offenders to ensure programs have the highest level of effectiveness (Andrews & Bonta, 2010). For example, a cognitive-behavioral approach to program delivery is most effective with offenders. Unfortunately, many questions remain because responsivity is the least investigated component of the RNR model. The findings here point to the need to understand why the effects of program attendance vary by gender and race. One investigated area of responsivity is variation across offenders in their denial of problems such as addiction, leading to resistance to attending programs. For such individuals, breaking through denial may require using 'motivational interviewing' before admission to a program.

As noted in the review of research on RNR, the void in research and knowledge related to responsivity is substantial. Almost nothing is known about differences that may exist in how specific offender groups such as males and females; and black, white, and other races may respond to program curriculum or program delivery, or whether it matters if the program is delivered by a male or female, black or white person. Another responsivity related attribute is psychological state. An evaluation of a thinking skills program for men found that overall it had no significant effect despite the program being delivered with fidelity and attendance tracked. A later re-analysis of outcomes found that white program participants did significantly better than the control group while program failures among black parolees rated as high-anxiety on a personality inventory were significantly higher than the control group (Van Voorhis et al., 2013). This finding suggested that black parolees would have fared better in the program if the results of the screening tool had been used to address this responsivity issue.

Screening for responsivity is also a lingering problem with most 12-step programs. Alcohol and drug treatment program types such as AA and NA, may be less rigorous and less structured than curriculum-based programs delivered by trained clinicians in formal treatment programs. The structure of 12-step programs and the fact that no fee is required to attend may lead to reliance on and attendance at these programs by parolees who are less well-off financially which could result in variation across groups in program effectiveness. Examination of moderation effects may shine a light on unaddressed responsivity to identify subgroups of offenders where programs are not especially effective. Very few, if any, investigations of parole outcomes include the level of program attendance in analyses and no research was noted that tested the moderating effects of attendance on individual level characteristics of parolees.

One additional practical point is made regarding how program attendance moderation affects supervision outcomes. Generally, offender programs in the community are in short supply and many offenders must pay a fee, however small it may be, to attend. If the moderating effects found in this study related to program attendance are associated with inattention to responsivity, substantial sums of money are being wasted for payments to program providers and for the subsequent reincarceration costs of those parolees who, despite attending, still fail supervision. Next, the final hypothesis at the individual level examines if and how parolee individual level characteristics may influence program attendance.

The effects of individual level characteristics on program attendance (H4): The last hypothesis related to RNR examines how individual level characteristics affect program attendance. Table 6.1, *AttdTotal,* model-4 provides the results which show that *HighSchool* is the only individual level characteristic with a significant effect on program attendance. As expected, the level of program attendance is significantly higher for parolees with a high school diploma. Unfortunately, less than half the study sample has a high school diploma. The good news is that with this sample of parolees the other four individual level characteristics do not lower the likelihood of program attendance. However, as noted above, certain individual level characteristics do serve as moderators which affect the direction and strength of program attendance.

Summarizing the findings in this study related to RNR, other than for *Risk,* on the whole individual level characteristics of offenders are not consistently associated with the seven dependent measures, although males have a higher likelihood of failure on three of the five primary outcomes. Program attendance has both positive and negative effects depending on the outcome, and the findings related to moderation raise important questions about how the responsivity principle is employed to ensure that programs benefit all offenders. The summary and discussion next turn to hypotheses related to social disorganization theory and then hypotheses combining RNR and social disorganization theory.

Social Disorganization Theory and Reoffending

Tables 6.2 and 6.3 summarize the findings related to the effects of neighborhood conditions on parole outcomes, neighborhood programs, and program attendance. This study hypothesized that, in line with social disorganization theory, neighborhood structural conditions

(*Disadvantage, Mobility, Proportion Black*) significantly increase parole failures, and decrease both the number of programs across neighborhoods and the level of program attendance. Due to the low Intraclass Correlation Coefficients (ICC), the only parole outcome variable tested is *EarlyOut.*

This study only considered parole failures that occurred during the first year of release and the number of failures due to Revocation and Felony arrest were rather limited (10% and 19%, respectively). This limited amount of variability in these two dependent variables was mostly accounted for by individual level variables, especially Risk scores. While there was considerably more variation in outcomes related to supervisory activities, the neighborhoods in the sample selected for this study may have been too similar to provide enough neighborhood variation.

The 1,637 parolees in the study sample were drawn from a cohort of 5,333 spread across 554 census tracts in six large cities. However, only 65 census tracts met the requirement for the minimum number per tract. Subsequent t-tests found that these 65 tracts were significantly more economically disadvantaged and more mobile. They also had a significantly higher percentage of black residents. These differences from the overall population suggest that the neighborhoods in the study sample were relatively homogeneous, limiting neighborhood variation. Given that most of the ICC's for primary outcome variables were non-significant, the summary of findings begins with the effects of neighborhood conditions on *EarlyOut* followed by findings for other hypotheses related to moderation and mediation of neighborhood level conditions.

Neighborhood conditions and early outcomes: *EarlyOut* is the combined total of parolees who experienced at least one technical violation, one positive drug test, or one violation arrest. Table 6.2, *EarlyOut,* models a-d summarize the results of the tests for the effects of neighborhood conditions on *EarlyOut.* A total of four models were constructed. Among the

Table 6.2.
Regression of EarlyOut On Neighborhood Conditions; and, Mediation of Neighborhood Conditions

	EarlyOut on Neigh. Cond. (*H5*)				Multilevel Mediation (*H7*)[2]			
	EarlyOut[1]				AttdsTotal		TractPgms	EarlyOut
	a	**b**	c	**d**	1	2	3	4
Male	+	+	+	+				
Nonwhite								
HighSchool	-	-	-	-		-		+
Age								
Risk	+	+	+	+				+
TractPgms								
Disadvantage							-	
Mobility			+				+	
Proportion Black								

+ Independent variable has a significant positive effect
- Independent variable has a significant negative effect
Dark cells = variable not in model
[1] Uses full sample of 1,637 parolees
[2] Equations used to test paths in mediation

neighborhood level variables (*Disadvantage, Mobility, Proportion Black, TractPgms*) only *Mobility* is significant. As neighborhood *Mobility* increases the likelihood of one of the *EarlyOut* violations also increases. This finding supports the hypothesis that increasing neighborhood mobility worsens parole outcomes. Similarly, previous research using aggregate outcome variables found higher levels of mobility in the parolee's neighborhood were significant in predicting return to prison (Hipp et al., 2010), and reincarceration (Chamberlain & Wallace, 2016).

Although high mobility neighborhoods may attract parolees by offering available and less expensive housing, these neighborhoods may lack the social networks that are necessary to support new residents getting settled and seeking employment. Parolees without jobs may be more likely to commit supervision related infractions such as not paying fees and not reporting to avoid the parole officer. High mobility neighborhoods may also be associated with more vacant housing where illegal activity like drug use can occur, creating a nearby temptation for parolees trying to remain drug free. Further, a sense of anonymity in such neighborhoods may embolden parolees to more freely commit violations such as using or selling drugs with less fear of being found-out or reported to parole authorities. One other explanation has to do with how parolees living in such neighborhoods may be monitored by parole officers. Parole officers' overall perceptions of less-stable neighborhoods and the activities they may attract, may lead to greater levels of parolee monitoring and drug testing that uncovers otherwise undetected violations or drug use.

Significant bivariate correlations between *TractPgms* and all three outcomes comprising *EarlyOut* suggested *TractPgms* might also predict *EarlyOut* (see Table 5.9). That did not turn out to be the case. Therefore, despite using the full sample (N = 1,637) and the aggregate outcome variable, of the three neighborhood level conditions, only *Mobility* has a significant effect on the aggregate outcome variable *EarlyOut.* The summary of findings turns next to hypotheses at the intersection of RNR and social disorganization theory having to do with moderation and mediation of neighborhood conditions, programs, and program attendance.

Neighborhood conditions, RNR, and moderation: Social disorganization theory suggests neighborhood conditions should increase arrests, revocations and other outcomes. RNR posits that supervision outcomes should improve when higher risk parolees participate in programs that address their criminogenic needs. This study therefore, hypothesized that program attendance moderates the effects of neighborhood conditions on parole outcomes. Table 6.3 summarizes the findings from the four models constructed to investigate moderation. The only moderating effect uncovered is for the interaction of *Mobility*AttdsTotal* on *Revocation*. In agreement with the hypothesis, as attendance increases the positive effect of mobility on revocation is decreased.

This examination also revealed the importance of *TractPgms* for reducing *FelArrest* and *LateOut.* Proximity to programs falls within the realm of responsivity. An increase in the number of neighborhood programs is associated with a lowered likelihood of arrest for a new felony crime. Additional analyses not shown here found that the significant relationship between *TractPgms* and *FelArrest* in the program-enrolled subsample did not extend to the full sample of enrolled and not-enrolled parolees. Similarly, an analysis to compare the non-significant effects of *TractPgms* on *EarlyOut* in the full sample (Table 6.2 *EarlyOut*) with the program-enrolled subsample also failed to find significant effects. While previous research found a clear and

Table 6.3.

Multilevel Analyses of Neighborhood Conditions Moderated by Attendance Predicting Outcomes

	Violation				Drug[1]				ViolArrest				FelArrest				Revocation				EarlyOut				LateOut			
	a	**b**	c	**d**	a	**b**	c	**d**	a	**b**	c	**d**	a	**b**	c	**d**	a	**b**	c	**d**	a	**b**	c	**d**	a	**b**	c	**d**
Male																					+	+	+	+				
Nonwhite	-	-	-	-									-															
HighSchool																					-	-	-	-				
Age																												
Risk	+	+	+	+	+	+	+	+					+	+	+	+	+	+	+	+	+	+	+	+	+	+	+	+
AttdsTotal	+	+	+	+	+	+	+	+	+	+	+	+					-	-	-	-	+	+	+	+				
TractPgms													-	-	-	-									-	-	-	-
Disadvantage																												
Mobility																							+					
Proportion Black																												
DIS*Attds																												
MOB*Attds																			-									
Black*Attds																												

+ Independent variable has a significant positive effect

- Independent variable has a significant negative effect

Dark cells = variable not in model

[1] Parolees enrolled in a program and drug tested one or more times

strong relationship between the number of programs nearby where parolees live and re-arrest for any reason (Hipp et al., 2010) it seems clear from the findings reported here that the presence of programs in neighborhoods is important only for those who are in need of their services, which is logically consistent with the idea that people who need services will use them more if they are easily available.

The earlier finding of the significance of *TractPgms* for predicting a reduction in the likelihood of *FelArrest,* which does not include supervision activity measures, may be a more positive affirmation that programs do reduce the likelihood of new criminal activity. Proximity to programs falls within the realm of responsivity. Before moving to a further discussion of these findings, the final hypothesis test related to mediation by and on neighborhood conditions is reviewed.

Mediation of neighborhood conditions: This study hypothesized that neighborhood programs mediate the effects of neighborhood conditions on program attendance. As indicated in Table 6.2 (*AttdsTotal* - model 1), no mediation exists since none of the three independent variables (*Disadvantage, Mobility, Proportion Black)* has a significant effect on the dependent variable (*AttdsTotal)* (Baron & Kenny, 1986). Other relationships in this mediation are also explored. In agreement with social disorganization theory, *Disadvantage* is associated with a decrease in the number of neighborhood programs (Table 6.2, *TractPgms,* model 3). The analysis illustrated in Table 6.2 found that more programs decrease the likelihood of *FelArrest.* Therefore, it appears that *Disadvantage* indirectly increases the likelihood of *FelArrest* through its significant negative effect on the number of neighborhood programs. Conversely, and contrary to the hypothesis, increased *Mobility* is associated with an increase in *TractPgms* which leads to an indirect effect, lessening the likelihood of *FelArrest.* Lastly, *TractPgms* has no effect on *AttdsTotal* (Table 6.2, *AttdsTotal* - model 2*).*

Finally, the hypothesis that *AttdsTotal* mediates the effects of *TractPgms* on outcomes is also not demonstrated in this analysis (Table 6.2, *EarlyOut* – model 4). It was shown in earlier hypotheses that *AttdsTotal* is associated both positively and negatively with different outcomes. Therefore, summarizing the different paths analyzed related to neighborhood conditions, programs, and program attendance, *Mobility* is associated with the likelihood of an increase in an *EarlyOut* violation and an increase in the number of programs in the census tract*. Disadvantage* is associated with a decrease in the number of neighborhood programs. *Disadvantage* and *Mobility* have indirect and opposite effects through tract programs on the likelihood of *FelArrest*. The last section in the discussion of findings elaborates on the important questions illuminated in this research.

Findings, Previous Research, and Future Research Directions

By definition, parolees return to communities after a period of incarceration that is typically at least several months long and more often, significantly longer. Like other individuals released from confinement at the end of a sentence, parolees must find their way to housing and employment, and navigate through other issues of daily living. Unlike other individuals released from confinement, parolees are also subject to a host of conditions and requirements all monitored to one degree or another by a parole officer. Some conditions grant significant authority and discretion to parole officers in order to effectively manage parolee behavior. The result is a wide range of approaches to supervision based on the parole officer's personal skills, beliefs and attitudes, the officer's training and experience, and workload (Bourgon & Gutierrez, 2012; Clear & Latessa, 1993; Grattet, Lin, & Petersilia, 2011; Kennealy, Skeem, Manchak, & Eno Louden, 2013; Klockars, 1972; Ricks & Eno Louden, 2015). Moreover, supervision requirements and activities across parole offices may also be influenced by the person managing each parole office and agency administrative goals.

In this study the near absence of effects on the three supervision activity dependent measures or effects in an unexpected direction (program attendance) prompt a closer comparison with dependent measures of active parolees in previous research. Unlike the present study, the previous research combines both non-crime and criminal violations into one dependent measure making it impossible to tease out the effects on each outcome (Chamberlain & Wallace, 2016; Hipp et al., 2010; Kubrin & Stewart, 2006). Previous research is silent on the question of parole officer effects. Although parole officer effects were not examined here, other research suggests there may be a range of parole officer effects or a "supervision regime" (Grattet, Petersilia & Lin, 2011, p. 373) accounting for a portion of the effects on parole outcomes. Findings from the three supervision activity dependent measures used in this study may be providing some indication of infractions that result from the effects of parole officer attributes or officer effects.

Contrary to RNR, across statistical models for the supervision activity variables, the number of program attendances increases the likelihood of these outcomes. This is a particularly important and a troubling finding considering the resources invested in offender programming, especially if these outcomes are precursors to new crime and revocation. Timing, how the variable is aggregated, and RNR does-not-apply are all suggested as explanations. In contrast to the supervision activity outcomes and in agreement with RNR, program attendance has significant negative effects on felony arrest and revocation. This is especially good news since the felony arrest measure represents only a new crime and revocation represents complete parole supervision failure. Each program attendance decreases the likelihood of these two outcomes.

A more troubling result is revealed in the test of the moderating effects of program attendance on parolee individual level characteristics. This research may represent one of the first examinations of these variables in moderation. The revocation reducing effects of program attendance are dampened for black and male parolees. A similar dampening effect is found for program attendance with older parolees. Although program specific information was not examined for this study, from an RNR perspective, these results prompt questions within the realm of responsivity about how programs are shaped and delivered to accommodate different ages, genders, and cultures.

The final significant result has to do with neighborhood programs. This research finds that the number of programs in a neighborhood is negatively affected by increasing neighborhood *Disadvantage* but positively affected by increasing *Mobility*. This is good and bad news for lowering felony arrests. Since the number of programs decreases the likelihood of felony arrest, *Disadvantage* and *Mobility* have indirect effects on felony arrest through their opposing effects on the number of neighborhood programs.

Limitations and Future Research

A number of limitations are noted related to the master file from which the study sample was drawn. As with much research, the master file was created for other purposes and therefore, established as a discharge cohort. In the master file all parolees ended supervision during a specific three-year time-period which resulted in many thousands of otherwise eligible parolees outside this limited time period not being included in the master file. For example, parolees released to supervision five years prior to the selected discharge years were not included in the master file if their parole ended up to the first day of the earliest discharge year. Moreover, going back in time the potential pool of parolees for inclusion in the master cohort was smaller each previous year of release to supervision because parolees had to be under supervision longer and longer to have ended supervision during the fixed three-year discharge window.

The decision to use a fixed, uniform time period under supervision for all parolees in the study sample led to a decision to examine the first 12 months of supervision because the largest number of parolees in the master file had parole end dates between one and two years after release. While this is an acceptable sample and time period for the analysis, a longer time period with additional parolees in the sample may have allowed for more complete analyses of all the outcome variables.

The significant reduction in the number of parolees in the master file resulted in a much smaller number of census tracts (65 out of 554) meeting the requirement for the minimum number of parolees to be included in the study sample. T-tests between study and non-study tracts illustrated clearly that study tracts were more disadvantaged in all the measures used. The largest difference in means was *Proportion Black* which was 20% higher in study sample census tracts. Finally, there was insufficient variation in six of the seven dependent measures across census tracts to conduct many of the multilevel analyses.

Questions about the differing effects of the three program types could not be answered due to the small numbers of persons attending different types of programs. An exploratory analysis by type uncovered interesting but tentative results. In particular, answers to questions about the effects of specific types of programs might have provided important insights into the relationship between attendance and certain outcomes but this was not pursued in the study.

The origin of the limitations discussed above was the three-year restriction placed on the parole end date in the master file. By removing this restriction so that the master file includes parolees with any parole end date, it is estimated that the number of cases in the file would increase by at least a few thousand, which would then increase the number of parolees with different lengths of time on parole and their distribution across census tracts, increase the number enrolled in and attending programs, and increase variation across dependent measures. The larger cohort would likely make the distribution of neighborhood conditions more normally distributed. The larger cohort would likely also include greater numbers of parolees attending programs which would then allow for an examination of subgroup effects such as with attendance by program type.

Using the existing sample, an option for expanding the number of census tracts holding the minimum number of parolees might have been accomplished by merging data from ‘similar’ census tracts. Similar census tracts are defined as having non-significant differences in some or all of the census variables used to create the neighborhood condition variables. Tract averages for neighborhood conditions would then be used in the analyses.

Two other data limitations noted above which are related, are the use of dichotomous variables for the supervision activity dependent measures and the way certain data were configured. These may be important dependent measures not only for understanding how program attendance affects supervision outcomes but also for examining possible officer affects. It was not possible to know if any of the three supervision activity outcomes occurred before or after any program attendances which likely confounded the analysis. When examined in relation to outcomes such as a new felony crime arrest or revocation, which generally end supervision, the number of attendances can be analyzed in relation to whether the outcome occurs or not. The three supervision activity outcomes are different in that they may have occurred many times throughout the 12 months under review before, after, or along with program attendances over that time period. Configuring the outcomes as the total number of each supervision activity would be more sensitive to weighing the RNR posited cumulative effects of program attendance. Another possible way to analyze the effects of program attendance would be to compare program attendance in each month to the number of supervision activities the following month through the 12-month time period.

A final limitation in this study was the use of the first home address for assignment to a census tract. Approximately half the study sample had only one address. For those parolees who moved, the length of time at the first address varied significantly. Many parolees moved within a relatively brief time after release from prison while others moved but relatively late in the 12-month time period. Often the first address is a placeholder which allows the parolee to be released from confinement with no real intent to live at that address permanently.[15] The data also revealed that some parolees had several different addresses while other parolees moved numerous times but back and forth between two addresses. It should be noted that residence changes do not necessarily change residence census tracts. If the goal of neighborhood research is understanding neighborhood effects on parole outcomes, some accounting should be made for

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[15] Personal knowledge of the author.

the various locations parolees live across census tracts. This type of analysis would also require accounting for the parolee's activities (program attendances, violations, etc.) while living in each neighborhood.

Case management systems that allow for this type of analysis are constructed such that information is entered as data in designated, formatted fields. To minimize errors, fields could be filled by selecting from a list of possible choices. Ideally, from a research perspective, all activities would be date stamped with dates formatted or selected from an on-screen calendar. To enhance place-based or neighborhood analyses, addresses would include several fields for residence name such as apartment complex name, street number, street, city, ZIP, state, and county with verification linked to postal records to ensure accuracy. Zip codes are particularly important as they are often required for census tracts. Finally, data extraction for analysis would allow for linking all variables associated with each offender.

This research investigated neighborhood effects on parolees under active supervision using two types of measures. For one type of measure, the commission of a new crime, the parole officer has almost no discretion in how to respond. The parolee is arrested and the case submitted to a higher authority for a decision. For non-crime infractions, whether or not the infraction is noted as such in the parolee's record is subject to a host of objective and subjective considerations by the parole officer and others. Emerging research and the findings in this study related to supervision activity outcomes suggested these considerations may play a role in charging a non-crime infraction. Little is known about what effects may accrue from the parole officer's supervision and no independent measures of parole officer effects were available to examine this question.

In order to test for the possible influence of the parole officer on outcomes separate from neighborhood and other independent effects, certain measures must be disaggregated and new measures collected. Catch-all outcome measures must be disaggregated or coded to separate outcomes associated with new crimes from outcomes associated with non-criminal infractions. Also, new measures must be gathered that assess differences among parole officers and parole office climate that are suggested as underlying what has been called the “supervision regime” (Grattet et al., 2011, p. 373). In recent years research on parole officer effects has accelerated. Other measures should also be included in these models such as the total number of program attendances that reflect what may be called ‘positive’ demands placed on parolees by parole officers.

A final limitation of this study plagues most research investigating the application of RNR to offender change. Responsivity means the needs and situations of specific offenders are addressed and accommodated so that barriers to the change process are removed. In this study attendance moderation uncovered important adverse effects for black, male, and older parolees. Since no previous research was found assessing moderating effects of this type, it may be that programs to which parolees are assigned are administered in a way that does do not specifically address the needs of these parolees.

One approach to understanding and eliminating the source(s) of these adverse responsivity effects would be to examine outcomes for specific program types with specific program providers with specific offender types. Small numbers of parolees in subgroups in this study did not permit this level of analysis. Other variables of importance to examine include programs with mixed gender participants versus single gender, or whether the program is delivered by a person of the same gender or race as the parolee. On the other hand, additional assessments of offender characteristics such as psychological states like anxiety which was noted above in a previous study (Van Voorhis et. al., 2013) may also illuminate the need for alternative programs. The unfortunate fact that so little is known about specific responsivity means numerous explanations are possible.

Policy Implications

Findings from this analysis show that increased program attendance lessens the likelihood of the two most important parole outcomes: new felony arrest and revocation. The effects of program attendance are assumed to be cumulative. Therefore, monitoring and encouraging attendance should result in fewer new felony crimes committed and lower numbers of revocations. Programs are particularly beneficial for female, white, and younger parolees. On the negative side, some programs may not be delivering the beneficial effects of program attendance for male, black, and older parolees. The reasons for these differences are unknown but may be related to responsivity. Corrections agencies should consider periodically evaluating programs first by race and gender and by more in-depth factors if available to better understand and rectify these discrepancies.

Associated with attendance is the number of programs available to parolees. The most disadvantaged neighborhoods tend to have fewer programs which is associated with higher levels of felony arrest but mobility has the opposite effect. Corrections agencies' policies should require monitoring the availability of programs across neighborhoods, program enrollment versus capacity, and the time from referral to the first program attendance to ensure timely access by all parolees.

Considering the effects found here related to program attendance weighed against the high cost of imprisonment in Georgia ($24,000 per year),[16] correctional agencies may come out ahead by redirecting operational funds to ensure programs are available in the communities where offenders reside. A critical point is assessing program performance to make sure the benefits accrue to all groups required to attend. The pay-off is potentially too great to be satisfied with only whatever types of programs and level of services happen to be available.

Frequently, as is the case in the state where this study was conducted, correctional agencies rely on existing programs in the community to provide services for parolees who are then required to attend. Many of the programs examined in this research to which parolees were enrolled had to be approved by the parole agency. Other organizations such as AA and NA support groups were also available and 'counted' by the parole agency but were not required to meet any specific program standards. Ineffective programs may be one explanation for the positive effects of program attendance on the supervision activity outcomes. Given the cost and potential benefits of program attendance, it would serve correctional agencies' goals to develop metrics to track parolee outcomes by program and conduct periodic analyses to assess program effectiveness.

The unexpected positive significance of program attendance and the paucity of other significant effects on supervision activity outcomes raise important questions. A growing body of other research suggests the infractions that underlie these supervision violations may be driven, at least in part, by variation in how parolee supervision is conducted. Parole officers are

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[16] http://www.dcor.state.ga.us/sites/default/files/FY2017%20CPD%20Consolidated%20Summary.pdf

afforded reasonable discretion to manage non-crime infractions within certain limits considering the circumstances of each situation. An important concern for organization management has to do with the consistent application of that discretionary authority. This is especially important if non-crime infractions lead to re-incarceration or revocation. Finally, this research also found the composite risk score was the only individual level characteristic that consistently predicted risk across outcomes. The first principle of RNR is assessing for risk and using risk to guide assignment to programs and level of supervision.

Conclusion

This research found that program attendance is associated with decreases in felony arrest and revocation for female, white, and younger parolees during the first year of supervision. Each additional program attendance further reduces the likelihood of these outcomes. An increasing number of programs in census tracts lowers the likelihood of felony arrests for those parolees enrolled in programs. However, census tracts that are more disadvantaged tend to have fewer programs.

Since the vast majority of offenders sentenced to prison eventually return to the community, it behooves community leaders and correctional managers to seek out and provide tools that have shown the greatest positive effect on future behavior. Community leaders and those who make policy and budget decisions should embrace programs that address the attributes most associated with reoffending – mental illness, substance abuse, and faulty thinking. The findings in this study add support to the effectiveness of this approach for slowing the revolving door of incarceration and improving public safety.

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VITA

John Paul Prevost was born in 1952 in Saint Johnsbury, Vermont. In 1976 he graduated from Saint Meinrad College and was awarded a B.S. degree in Biology. Soon thereafter, he began a career in criminal justice in Atlanta, GA, first in the juvenile justice system and then with the Georgia Board of Pardons and Paroles. John's first assignment as a parole officer was supervising a large caseload of young adult parolees. During his 32-year career in parole John held a number of positions including Chief Parole Officer and Assistant Director of Research, Evaluation, and Technology. Among his responsibilities in this latter position were managing strategic planning, developing agency performance measures, and working with the team that developed and implemented an innovative electronic case management system. During his career with the Parole Board, John was also involved in numerous projects including developing and delivering the first curriculum for parole officers to meet requirements for law enforcement certification; developing various policies including for electronic monitoring and supervising HIV diagnosed parolees; and parolee drug testing. He also implemented and managed the agency's first intensive parole supervision program and in-house drug counseling program.

While working in parole, John attended Georgia State University and was awarded a Masters of Public Administration degree in 1982. He continued with the Parole Board and retired in May 2011 following his admission the previous fall to the inaugural graduate Ph.D. class in Criminal Justice and Criminology at Georgia State University. This dissertation marks his completion of that program. John will be awarded his Ph.D. in August, 2019.

John co-authored three publications during his tenure with the Parole Board and a fourth during his graduate studies. The most recent journal article is an investigation into the subject matter of home visits in parole supervision published in *Criminal Justice and Behavior* in 2017 and titled "Home Visits in Community Supervision: A Qualitative Analysis of Theme and Tone." The authors are Finn, Prevost, Braucht, Meredith, Johnson, and Hawk. The second publication in 2011 on the subject of electronic monitoring is found in the *American Journal of Criminal Justice* and titled "Measuring Electronic Monitoring Tools: The Influence of Vendor Type and Vendor Data." The authors are Blackwell, Payne, and Prevost. Another article published in 2001 in the *Journal of Offender Monitoring* also on the subject of electronic monitoring is titled "Georgia Parole Board's Use of Electronic Monitoring with High-Risk Offenders: Enhancing Community Safety and Offender Reintegration Through Greater Accountability." The authors here are Finn, Blackwell, Oxford, Braucht, and Prevost. The fourth publication is a book chapter by Prevost, Rhine, and Jackson in the 1993 publication *Reclaiming Offender Accountability: Intermediate Sanctions for Probation and Parole Violators* published by the American Correctional Association.

John is presently involved in evaluating an innovative accountability court program. He continues to pursue opportunities to share in a classroom setting his practical and academic knowledge of community corrections. He is also working to develop a criminal justice research consortium to collaboratively investigate and implement more effective strategies for people leaving incarceration and returning to communities across the state. John makes his home in Atlanta, GA and may be contacted at john.prevost77@gmail.com.