REVISITING THE PERSON-CONTEXT NEXUS: AN EMPIRICAL TEST OF A REVISED TRAIT SUPRESSION HYPOTHESIS

by

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ABSTRACT

Given mixed findings concerning the interaction of neighborhood disadvantage and self-control in understanding crime and delinquency, this work develops a revised theory to explain the conditional effect of self-control across neighborhoods while drawing on existing social psychological and criminological theory. This revised trait hypothesis posits (1) the level of crime is uniformly high and low in high and low disadvantage neighborhoods, respectively, and (2) that the effect of self-control in both high and low disadvantage neighborhoods is attenuated relative to the effect of self-control in medium disadvantage neighborhoods. The revised theory is tested using panel data collected from a sample serious youth offenders using multiple analytic approaches. While the first prediction is supported, the data fail to support the second. A number of explanations for the lack of full support are considered, and it is concluded that self-control and neighborhood disadvantage exert independent effects on crime in this sample.

I. INTRODUCTION

Criminological theories primarily take two approaches: (1) contextual or ecological and (2) individual or processual (Akers & Sellers, 2013). Whereas contextual theories focus on how aggregate characteristics of places affect criminal behavior within them (e.g., Shaw & McKay, 1942), individual theories concentrate on time-stable characteristics or developmental changes of people that contribute to increased risk of offending (Gottfredson & Hirschi, 1990; Sampson & Laub, 1993). An emergent theoretical framework, interactional theory, not only blends the ecological and individual approaches, but also argues each approaches' set of causes is dependent on the other (Bronfenbrenner, 1977; Thornberry, 1987; Wikstrom & Sampson, 2006). While there are several subareas of research being conducted in the interactional framework, perhaps the most developed of these focuses on the interdependence of the neighborhood, the family, and the individual in understanding adolescent development, delinquency, and crime (Brooks-Gunn, Duncan, Klebanov, & Sealand, 1993; Jones & Lynam, 2009; Lynam et al., 2000; Piquero, Moffitt, & Lawton, 2005; Simons, Johnson, Beaman, Conger, & Whitbeck, 1996; Teasdale & Silver, 2009; K. A. Wright, Kim, Chassin, Losoya, & Piquero, 2014; Zimmerman, 2010; Zimmerman & Messner, 2011).

The contextual roots of integrated theory lie in research that examined adolescent outcomes and focused on the role of the neighborhood (Jencks & Mayer, 1990; Leventhal & Brooks-Gunn, 2000). Of course, one cannot speak of contextual effects without addressing the concept of disadvantage (Shaw & McKay, 1942; Wilson, 1987). Indeed, the impetus for almost all such research conducted in this area is interest in poor neighborhoods and their effects on child development. Disadvantage, or concentrated

disadvantage, is a well-established risk factor for a number of developmental outcomes (Leventhal & Brooks-Gunn, 2000), yet the pathways through which a neighborhood's socio-economic status leads to developmental deficiencies are not well understood. Neighborhoods have been theorized to have mostly indirect effects on adolescent development, and sometimes disadvantage plays a distal role. For instance, some institutional resource models posit neighborhoods characterized by concentrated disadvantage lack recreational, educational, and childcare resources that impact the quality of parenting (i.e., through supervision and decision making that affects the child's environment), thus affecting the physical, emotional, and cognitive development of children (Elliott et al., 1996; Kohen, Leventhal, Dahinten, & McIntosh, 2008; Sampson & Groves, 1989; Sampson, Raudenbush, & Earls, 1997). Some have argued that extreme neighborhood disadvantage leads to the development of alternative codes of behavior that condone criminal behavior and violence (Anderson, 1999; Cohen, 1955). Generally, these models argue that neighborhood residents collectively propagate oppositional norms when faced with structural disadvantages and blocked opportunity. Even still, some models suggest that the neighborhoods disadvantage covaries with the exercise of social control, and that resources impact the quality of relationships between residents, the formation of networks, and thus resident's ability to control what goes on inside the neighborhood (Bursik & Grasmick, 1993b; Sampson et al., 1997).

While neighborhoods have consistently been found to exert their own effects, recent theoretical developments have focused on integrating neighborhood, family, and individual explanations of crime, specifically (Wikstrom & Sampson, 2006). The philosophy of an integrated approach is that a more complete understanding of offending

comes from examining people in context (Bronfenbrenner, 1977) rather than examining them independently. The present study focuses on the interaction of concepts central to two prominent theories of crime: *neighborhood disadvantage*, an antecedent of informal social control, collective efficacy and social disorganization, (Bursik & Grasmick, 1993b; Sampson et al., 1997; Shaw & McKay, 1942; Wilson, 1987) and *low self-control* (Gottfredson & Hirschi, 1990).

The simplest and most intuitive hypothesis for how risk factors for crime interact is known as amplification. The idea is that the whole is greater than the sum of its parts—in other words, "exposure to multiple causes of crime produces an increase in crime that exceeds the sum of those causes' independent effects" (Hay, Fortson, Hollist, Altheimer, & Schaible, 2006, p. 328). Versions of this argument are found throughout the criminological literature, at least as far back as Reiss (1951), but more recently by Moffitt (1993), Wright, Caspi, Moffitt, and Silva (2001), and Agnew (2005), and have been applied in various criminological settings. Applying this idea to this study, disadvantaged neighborhoods (those lacking strong informal social controls, or institutional resources) represent risky settings in which individuals with low self-control will be more apt to offend and offend at higher rates. The key hypotheses are that (1) self-control is a strong protective factor in the absence of informal social control and (2) neighborhoods with strong informal controls can push individuals with low self-control to conformity.

A competing explanation for the interaction of self-control and neighborhood disadvantage has its roots in the social psychological literature on person-situation interactions (Mischel, 1977). This hypothesis makes predictions opposite of amplification, saying instead that all youth in disadvantaged neighborhoods will

experience strong expectations to follow an informal code of violence (Anderson, 1999). These expectations become so strong that individual differences in self-control become irrelevant (Mischel, 1977; Zimmerman, 2010). This hypothesis, known as trait suppression, predicts increases levels of neighborhood disadvantage will substantially reduce the effects of self-control (Zimmerman, 2010). On the other hand, there should strong effects of self-control in less disadvantaged neighborhoods where expectations for behavior (i.e., conforming or deviant) are more ambiguous.

These competing ideas (Reviewed in Chapter II) have been important in framing and interpreting research findings on self-control, neighborhood disadvantage, and crime. In Chapter III, I review a recent flurry of research on the interdependence of these variables and their effects on crime and delinquency, which has produces mixed results (Jones & Lynam, 2009; Lynam et al., 2000; Meier, Slutske, Arndt, & Cadoret, 2008; Vazsonyi, Cleveland, & Wiebe, 2006; Wikstrom & Loeber, 2000; Zimmerman, 2010; Zimmerman, Botchkovar, Antonaccio, & Hughes, 2015). These studies have predominately modelled the interaction of disadvantage and self-control using product terms in a regression context—some researchers have found a positive effect of neighborhood disadvantage on the effect of self-control, consistent with amplification, while others have found negative effects, consistent with trait suppression. Alas, others have found these variables to have independent (i.e., non-interactive) effects. The theoretical task taken up in this study is to consider these hypotheses and develop alternative hypothesis to explain the mixed results of previous studies.

This revised theory is constructed after considering the logic of trait-suppression and its applications to the moderating effects of neighborhood disadvantage. Briefly,

Mischel (1977) describes strong social situations as conditions where expectations for behavior determine the behavior in that setting. In other words, strong social situations are those where everyone is expected to behave in a certain manner. While it is possible that disadvantaged neighborhoods represent strong social situations in that the conditions within them produce uniform expectations for offending, it does not necessarily follow that affluent neighborhoods are contexts where expectations for behavior are ambiguous. In fact, it is possible that affluent neighborhoods represent strong social situations in that expectations for conformity are strong. If neighborhoods at either end of the disadvantage continuum represent strong social situations, it follows that the neighborhood conditions that produce ambiguous expectations for behavior would exist in neither disadvantaged, nor affluent neighborhoods, but somewhere in the middle.

Moving past a simple linear conception of the moderating effects of neighborhood disadvantage, the methodological task for this study is to specify an empirical model to test this revised hypothesis against the predictions of its predecessors (Chapters IV, V, and VI). While most interaction effects modeled in criminal justice and criminological research are linear in that the largest effects of a focal variable are expected at one extreme of the moderating variable (but see Zimmerman & Vasquez, 2011), the revised hypothesis tested here posits the maximum effect of self-control occurs in the middle of the disadvantage continuum. Alternative statistical models and approaches to examining moderation effects (Osgood, Finken, & McMorris, 2002; Vasquez, 2010) are considered to address the added complication of this empirical complexity. Using data from a panel study of high-risk youth, I test this revised hypothesis and present the results in Chapter VI.

Briefly, I fail to find any support for the trait suppression hypothesis (Zimmerman, 2010), nor the vulnerability hypothesis (Lynam et al., 2000), and find mixed support for the revised hypothesis developed in Chapter II. While I find some support for the central claim that, the impact of self-control is largest in average neighborhoods, the results of multiple models fail to support all of the nuanced predictions made by the revised trait-suppression hypothesis. The limitations and conclusions are discussed in Chapter VII.

II. THEORETICAL BACKGROUND

This chapter considers the theories that contribute to the interactional framework used in this study. The development of the theories is highlighted. Importantly, the theories are discussed chronologically, beginning with ecological theories of crime, then early and contemporary control theories, and culminating in recent work explicating an interactional perspective. This serves to demonstrate the historical ebb and flow of ideas, and to show the intellectual history of the interactive perspective, particularly as it leads to the integration of self-control and ecological theories of crime. While other interactional perspectives exist (e.g., Thornberry, 1987), the focus here is theoretical developments relevant to self-control, and neighborhood conditions.

Neighborhood Disadvantage and Crime

Explanations of the concentration of criminal behavior in urban places originated in the Chicago School of sociology in the 1920s and 1930s with the work of Louis Wirth, Amos Hawley, Earnest Burgess, Robert Park, Henry McKay, and Clifford Shaw[1]. These sociologists focused on the changes to society concurrent with urbanization. Of particular interest were changes in the size, density and heterogeneity of the urban population, and how these changes affected interactions among people. Their studies examined crime at the ecological level. Wirth discussed the increase in size, density, and heterogeneity of the city as consequential for the organization of society—in the urban city "traditional ties of human association are weakened" and the ability of society to control the behavior of the individuals is lessened (1938, p. 22).

[1] See Wirth (1938), Hawley (1986); Shaw and McKay (1942), Park and Burgess (1924).

A corollary to the increase in population size and density is competition for scarce resources, which led to an emphasis on the economic differentiation of places. In Park and Burgess' (1924) view, this competition (referred to as the "biotic order") made parcels of the urban landscape more desirable. Differentiation in the desirability of places directly affected their value and affordability, as well as the interest of landowners in improving or maintaining them.

The economic differentiation of places led to the proliferation of economically disadvantaged neighborhoods. While economic disadvantage has been viewed as a central concept in theories of variation in neighborhood crime rates, the view of Shaw and McKay (1942) was that ecological processes giving rise to economically disadvantaged neighborhoods also contributed to high delinquency rates (Bursik & Grasmick, 1993b, p. 33). Within economically disadvantaged areas, rapid population turnover, ethnic and racial heterogeneity, low density of interpersonal network ties, among other neighborhood characteristics, have been viewed as the structural sources of variation in crime rates (see Sampson & Groves, 1989). For Wilson (1987), disorganization and deprivation were closely related; the fragmented family structures of some disadvantaged families necessitate reliance on public assistance. Their economic situation and family structures then further dislocate them from broader networks and connections to conventional institutions.

Networks, Social Capital and Collective Efficacy. Elaborations of social disorganization theory focus on the causes of variation in neighborhood in crime rates. For example, families in economically disadvantaged neighborhoods seek to move to more desirable neighborhoods as soon as economically feasible, making the

establishment of internal control institutions more difficult (Bursik & Grasmick, 1993b, p. 33). Additionally, the ever-changing composition of the neighborhood restricts the formation of dense interpersonal networks and impedes development of primary relationships (Bursik, 1988). Central to the capacity of the neighborhood to control behavior is the establishment of interpersonal networks, and furthermore, the qualities and merits of established networks.

One extension of Shaw and McKay (1942) examines systemic indicators of interpersonal networks, such as the size and density of informal networks within the community. Bursik and Grasmick (1993a), for example, examined residential mobility, percent of properties owner occupied, the net migration percentage, and the percent of children in two-parent households. They argue that these variables are indicative of the extent to which the population in a given neighborhood is stable and has the enduring capacity to regulate behavior. Friendship networks may develop into more effective informal social controls where there is less turnover in the composition of the community—a higher percentage of families with two parents will increase the supervisory ability of the community as a whole. Warner and Rountree (1997) measured the extent of these friendship networks by asking respondents about their interactions with neighbors (e.g. whether they had borrowed tools or food from a neighbor). Bursik further developed these ideas by examining the extent of friendship networks as well as connectedness among neighbors, using items such as "I can count on my neighbors if I needed something" (1999, p. 91). In addition to measures of connectedness, Sampson & Groves (1989) measured the presence of unsupervised peer groups, inferring from the presence of unsupervised teenagers that the neighborhood lacked the ability to supervise

and control delinquency. These studies consistently found the extent and density of informal networks were significantly, and negatively related to crime and victimization rates.

A number of studies have examined the extent as well as the qualities of friendship networks (Rose & Clear, 1998; Rosenfeld, Messner, & Baumer, 2001; Sampson & Groves, 1989). Theoretical elaborations by Rose and Clear (1998) posit social capital as source of control as it stems from trust and obligation developed through interactions in the community. Typically, measures of social capital stress "resources needed to effect positive change in neighborhood life" (Rose & Clear, 1998, p. 454) particularly through the social skills of neighbors. The concept of social capital also emphasizes cooperation of neighbors to solve problems (Rosenfeld, et al., 2001). Sampson and Groves (1989) examined the extent of prosocial interactions likely to foster trust and obligation using participation in meetings, committees, and clubs in the community. Similarly, Rosenfeld et al. (2001) examined social trust and civic engagement. These studies found that social capital was significantly related to homicide rates, and mediated various structural background variables (Rosenfeld et al., 2001; Sampson & Groves, 1989)

While some neighborhoods with mutual trust and cohesion may be effective at accomplishing some tasks, it is possible that they remain ineffective in controlling delinquency and crime (Kubrin & Weitzer, 2003; Morenoff, Sampson, & Raudenbush, 2001). Collective efficacy has been proposed as an alternative to social capital—it is explained in the context of self-efficacy, which is the *task-specific* capacity of individuals to accomplish actions (Sampson et al., 1997). The effectiveness of a neighborhood at

supervising children and maintaining public order is tapped by examining perceptions of respondents' neighbors' willingness to intervene in delinquent youths' activities. This stresses the shared expectations within the community for community members to do something about crime and disorder, rather than inferring the specific ability from cohesion and trust alone. In Pratt and Cullen's (2005) meta-analysis of macro-level predictors of crime, the effect of collective efficacy was the strongest of all predictors in the social disorganization theory tradition (B=-.303, 95% CI [-.255, -.351]).

Routine Activity Perspective. Yet another view of neighborhood variation in crime rates concerns availability of crime opportunities. Routine activity theory (L. E. Cohen & Felson, 1979), as well as the extensive elaborations of the theory, view crime as the temporal and spatial convergence of a motivated offender and a suitable target in the absence of a capable guardian. Therefore, variation in crime rates across neighborhoods is viewed as a function of increased volume of such convergences, rather than as a function of the neighborhood's capacity for controlling the outcome of these convergences. For example, some argue disadvantaged areas offer more outlets for stolen goods because economically disadvantaged people are more likely to favor used or secondary merchandise for economic reasons (Felson & Boba, 2010, p. 92). This economic position provides demand for second-class goods, attracts property offenders and potentially drug offenders, and more serious forms of crime are assumed to follow. Therefore, there is nothing inherently criminogenic about the people living in disadvantaged neighborhoods, nor the regulatory capacity of disadvantaged neighborhoods, *per se*. Rather, the economic situation promotes an increased volume of convergences of victims and offenders, in a context where individuals are less likely to

intervene, weakening the capability of guardianship within the neighborhood. While theories disagree on the mechanisms through which neighborhood disadvantage operates, its presence has consistently attracted the attention of scholars seeking to understand urban crime.

Early Control Theories

Following the proliferation of social disorganization theory, many researchers observed that living in disadvantaged contexts was not sufficient for criminal or delinquent behaviors. In fact, early control theorists took interest in individuals from economically disadvantaged areas who were non-criminal or non-delinquent (e.g., Reckless, Dinitz, & Murray, 1957). Early control theories recognized the importance of the community in social control, sometimes adopting sociological explanations of inter-neighborhood variations in crime rates. Reiss, for example, drew on the community as a form of social control and recognized community instability as problematic, stating "[c]ommunity institutions are probably more effective in controlling the behavior of family members when influence is exercised over a relatively long period of time" (1951, p. 201). Early control theories though, focused on variation in individuals' commission of crime and delinquency *within* neighborhoods, viewing such variation as a function of personal characteristics. Thus, they examined the joint influence of social control *and* internal personal controls on delinquent behavior, typically using family-level variables as indicators of external control or containment.

Reiss (1951) viewed delinquent behavior as the consequence of the failure of personal *and* social controls. Nye (1958) also examined internalized controls (i.e., the agreement of parent and child on important values), and direct social control (i.e.,

discipline, restrictions), finding the effects of each, individually, to be weaker than their combined effects. Reckless (1961), in generalizing Reiss & Nye's theories, argued weak containment accounted for delinquent outcomes. Containment is the most abstract or inclusive concept of the early control theories, encompassing internal controls (inner containment) like self-concept, self-control, a consistent moral front, and external controls (outer containment) like effective supervision and discipline, belongingness, and others (Reckless, 1961). The general prediction of containment theory is that individuals are differentially impacted by pushes and pulls to criminal and delinquent behavior because some are insulated through strong containment, while others are not. Rates of delinquent behavior are highest among those with weak inner *and* outer containment and lowest among those with strong inner and outer containment. Rates are also higher among those with weak inner and strong outer containment than individuals with weak outer and strong inner containment (Schrag, 1971). Similarly, Nye (1958) argued that if internal controls are strong, there is less need for other types of control to suppress delinquent behaviors.

As should be clear from this brief discussion of early control theorists, not only were internal and external controls important to their ideas, the effects of internal and external control were dependent on one another. Among these early studies, the effects of internal controls on delinquency were dependent on the type of boy, the boy's family structure, or the neighborhood in which the boy grew up. Similarly, the boy's race was typically examined as a conditioning factor in these works. Importantly, one should acknowledge that early control theorists, though they commonly do not receive credit for these views, predicted that the absence of social control and internal control would result

in greater involvement in criminal and delinquent behavior than the absence of one or the other alone, hence, an interaction. However, following these works, the interactive nature of the relationships among social and internal controls and crime received less attention, particularly as contemporary control theories were explicated.

Contemporary Control Theory

The advancement of contemporary control theory can be attributed almost entirely to the work of Travis Hirschi and Michael Gottfredson (Gottfredson & Hirschi, 1990; Hirschi, 1969). Social bonding theory and The General Theory of Crime, as well as revisions and elaborations, are substantial theoretical contributions to criminology. Hirschi's work, which draws on Durkheim and a hedonistic conception of humankind, is different from earlier control theories in that it explicitly disregards variations in motivation to commit crime. Indeed, Gottfredson and Hirschi (1990) pass over the concept of criminality in preference of self-control because criminality implies variations in the extent to which individuals are compelled to commit crimes. Self-control and social bonding theories are both consistent with the notion that variation in pushes to commit criminal behavior are of little interest because the ease of obtaining rewards through crime is sufficiently greater than the ease of obtaining the same rewards through conformity to the law. As such, individuals are uniformly motivated to crime through the desire to advance their desires through the least effortful means.

The General Theory of Crime (Gottfredson & Hirschi, 1990) posits individual variation in self-control as the *primary* cause of in crime and delinquency (Hirschi & Gottfredson, 1993, p. 50). They describe individuals with low self-control as impulsive, non-cognitive, non-persistent, self-centered, physical, irritable, short-sighted, insensitive,

easily frustrated, and more (Gottfredson & Hirschi, 1990, pp. 89-90). The breadth of content that their concept covers is substantial—it is conceptually heterogeneous. The concept has become closely related to the first operationalization set forth by Grasmick, Tittle, Bursik Jr., and Arneklev (1993). Principally, Gottfredson and Hirschi (1990, 2004) argue the vast majority of crime is very simple, easy to commit, requires no special skills and equally attractive to all people; therefore, an individual's ability to consider the consequences of their actions is the most consequential determinate in choosing whether to refrain from committing crime.

While criminology has been dominated by Gottfredson and Hirschi's (1990) definition and the measure used by Grasmick et al. (1993), the amount of conceptual and empirical work towards identifying individual differences in self-control, outside of the field of criminology, is staggering. There are innumerable mental images associated with of self-control. Researchers, theorists, and practitioners in psychology, economics, criminology, psychiatry, and other behavioral sciences all have an interest in individuals' self-regulation capacities. As a result, there are many definitions or conceptualizations of self-control across and even within multiple academic disciplines. Even within certain conceptualizations, researchers disagree on the appropriate measurement approaches, a problem that contributes to the large number of self-control scales in existence.

Impulses, temptations, and immediate gratifications and pleasures, however, are always central to the discussion of self-control (see Fujita, 2011; Funder & Block, 1989, p. 1041; Gottfredson & Hirschi, 1990, pp. 89-90; Mischel, 1974, p. 249; Tangney, Baumeister, & Boone, 2004, p. 272). The most general definitions of self-control involve the ability to delay gratification (Funder & Block, 1989), regulation of one's desires

(Baumeister, Heatherton, & Tice, 1994), or the extent to which individuals can overcome the "temptations of the moment" (Gottfredson & Hirschi, 1990). Some definitions explicitly describe the preference for larger delayed rewards over smaller immediate rewards (Ainslie, 1975). Relatedly, the study of impulsivity specifically details how individuals discount the value of distal rewards (Madden & Bickel, 2010). In reviewing self-control conceptualizations, Fujita (2011) finds the inhibition of impulses to be the common thread in the definition of self-control in the field of psychology.

More complex definitions of self-control emerging from psychology and economics, however, tend to focus on a "hierarchy of goals" (Carver & Scheier, 1982) and define self-control as persisting towards higher-order goals (e.g., Baumeister et al., 1994; Fujita, 2011). "Self-regulation" researchers define this hierarchy as organizing processes or goals along the dimensions of time, abstraction, and meaningfulness—distal, abstract and meaningful[2] processes or goals are superordinate to proximate, concrete, and inconsequential ones (Baumeister et al., 1994). In short, self-control reflects the ability to pursue distant, meaningful processes or goals in spite of temptation towards more immediate and tangible ones. According to Fujita (2011, p. 353), "self-control is thus the process of advancing abstract, distal motives over concrete, proximal motives when the two motives directly conflict." In a recent work on defining self-control, Hay and Meldrum (2016) take stock of the criminological and psychological conceptualizations of self-control. They similarly conclude self-control "is the practice of overriding immediate impulses to replace them with responses that adhere to higher-order standards that

[2] What seems to be implied by meaningful goals is fecundity—that is, the notion that pursuit of a goal will lead to additional pleasures at a later time.

typically follow from values, social commitments, and interest in long-term well-being" (Hay & Meldrum, 2016, p. 7).

The Interactional Perspective

Generally, theories of crime and delinquency have not integrated person-level explanations of behavior with neighborhood-level explanations of crime rates. Whereas each of the theories discussed above is type-of-person or type-of-place oriented, an alternative perspective views the explanation of crime and delinquency as requiring a type-of-person *in* type-of-place approach. Individuals with the same traits will experience different outcomes when placed in different contexts (Bronfenbrenner, 1977). Not only does this approach view individual and contextual variables as important, it views them as interdependent. The rebirth of these views, and the origin of a concerted effort to explicate an integrated theory with both contextual and individual-level explanations of crime occurred in two conferences in Sweden in 1992 (Wikstrom & Sampson, 2006).

A general example of the theoretical approach spurred in these conferences is the integrative framework explained by Wikstrom and Loeber (2000). This framework views risk and protective factors unique to the individual, as well as risk and protective factors in the social, economic, and family context in which potential offenders find themselves, as interdependent. Their conceptualization of individual and community risk is, what they term, holistic, meaning a host of factors predisposing individuals to choose criminal courses of action represent individual risk, and a host of factors that may lure potential offenders into criminal actions represent community risk. In this way, individual variations in skills, temperament, and moral conscience are used to rank individuals' predispositions, and community characteristics, such as considerable offending

temptations, allow for differentiation of risky and non-risky communities. They predict highest rates of offending among the highest-risk youth living in the highest-risk neighborhoods. They further predict the effect of individual protective factors on serious offending varies by level of community risk. The specific mechanisms through which community risk operates on individual risk are less clear. Observe:

> The rationale for a "holistic" approach is that individuals act as individuals (and in doing so draw upon a large number of individual characteristics operating jointly in complex ways) and that communities (social contexts) influence individual action through a number of complex characteristics and processes operating in conjunction. (Wikstrom & Loeber, 2000, p. 1118)

Wikstrom and Loeber (2000) do not discuss the complex ways individual risk factors affect offending, and the ways in which neighborhood risk moderates this effect. However, at least two perspectives that integrate individual and neighborhood level variables have specified mechanisms through which individual and neighborhood influences operate jointly.

Community, family, and individual risk, jointly, are particularly important to Moffit's taxonomy of criminal behavior (1993). Essentially, neuropsychological deficits contribute to verbal executive dysfunction and offending among some individuals. However, individuals who experience a mix of neuropsychological, familial, and community risk factors are more likely to persist in violating the law into adulthood and beyond. Individuals born with neuropsychological deficits into families that do not have the resources to cope with these deficits are unlikely to receive the socialization or supervision needed to prevent their commission of criminal or delinquent acts (Moffitt, 1993). This view is somewhat congruent with Sampson's (2003) view of adolescent

development in urban communities. Sampson (2003) reports that neighborhoods with high rates of homicide, suicide, illness, accidents, and infant mortality tend to be spatially contiguous. One might expect this pattern to emerge in disadvantaged areas because communities that are highly disadvantaged lack resources for the adequate rearing of children. The quality of prenatal care and social support for families is diminished in disadvantaged areas as well (Sampson, 2003). Each of these perspectives argues that the effect of neighborhood disadvantage operates through family socialization mechanisms in impeding the ability of the community and the family to provide for children with diminished capacities. As such, among children reared in disadvantaged neighborhoods, the effects of neuropsychological deficits can be very deleterious.

After Wikstrom and Loeber (2000) and Lynam et al. (2000), interest in person-context interactions concerning self-control, began to receive more attention. These studies built on the interactional perspective in blending the views of the ecological, sociological, and control theory approaches discussed above. However, as the contextual approach found in the works of Bursik and Grasmick, and Sampson and colleagues, focuses on the collective control of a neighborhood, and the revisions to control theory focused on the intertwinement of self-control and social control (Hirschi, 2004), these studies focused on narrower concepts. In doing so, specific mechanisms through which neighborhood characteristics moderate the effect of self-control would be specified in a number of studies.

Self-Control and Neighborhood Disadvantage

There are multiple theoretical arguments about how self-control could have differential effects depending on the level of neighborhood disadvantage. The

vulnerability hypothesis posits the strongest effect of self-control is among individuals in disadvantaged areas. The trait suppression hypothesis posits the weakest effect of self-control among individuals in disadvantaged areas. A third hypothesis, advanced mainly by Vazsonyi et al. (2006) and their interpretation of Gottfredson and Hirschi (1990), is that the effect of self-control is independent of neighborhood context—it is an equally strong protective factor in all neighborhoods (referred to as the invariance hypothesis). In short, different predictions about the moderating effect of neighborhood disadvantage on self-control not only suggest that disadvantage may amplify the effects of self-control, but alternatively that disadvantage may dampen the effects of self-control, or disadvantage may have no effect on the effect of self-control.

Invariance Hypothesis. The invariance hypothesis of the self-control-neighborhood disadvantage interaction is that the effect of self-control is constant across neighborhoods. This is similar to arguments that opportunity is unimportant etiologically (Gottfredson & Hirschi, 1990; Hirschi & Gottfredson, 1993). The general theory, as well as interpretations of the general theory (Vazsonyi et al., 2006) hold that opportunities are limitless and, therefore, do not vary in the population. Hirschi and Gottfredson (1993) argue that opportunities are generally independent of self-control, and that some necessary ingredients for certain crimes (e.g., knowledge of how to smuggle drugs) may be acquired through processes relevant to self-control. They conclude the interaction between self-control and opportunity is not consistent with the explication of the general theory. Extending these ideas to neighborhood context, Vazsonyi et al. (2006) argue, in conjunction with Gottfredson and Hirschi (1990), that as individuals within

neighborhoods vary with respect to self-control, the effect of self-control on crime will be invariant across neighborhoods.

Vulnerability Hypothesis. That the effect of low self-control on crime and delinquency is greater in disadvantaged neighborhoods is the vulnerability hypothesis of the self-control-neighborhood disadvantage interaction. A way of thinking about this is to posit that both a lack of social control and a lack self-control are necessary for criminal behavior (Nye, 1958; Reckless, 1961; Reckless et al., 1957). Though overly simplistic, this would imply that only individuals with low self-control would commit crime when placed in disadvantaged contexts. The corollary is that differences in criminal behavior between low self-control and high self-control individuals would be positive in disadvantaged neighborhoods, and zero in non-disadvantaged neighborhoods. Thinking probabilistically—the probability of committing crime decreases slowly as self-control increases among people in non-disadvantaged areas, but decreases much more rapidly among individuals in disadvantaged areas.

This argument relies on a vulnerability premise, that risk factors, such as low self-control, make individuals more susceptible to offending temptations (i.e., opportunities). In disadvantaged neighborhoods, where the volume of temptations to commit crime is presumably higher, individuals encounter a greater number of offending opportunities and, therefore, experience failures in self-control more often. The precise role that disadvantage plays in generating temptations has received considerably less attention, though two views dominate.

The interpretation of the general theory (Gottfredson & Hirschi, 1990) as predicting an interaction between self-control and criminal opportunities is one plausible mechanism behind the vulnerability hypothesis. A number of theories would predict disadvantaged neighborhoods present more opportunities for crime. According to Cohen and Felson (1979), disadvantaged neighborhoods could present more opportunities because the volume of convergences of motivated offenders and suitable targets is simply be greater in disadvantaged areas for economic reasons discussed above. Furthermore, these areas may lack effective guardianship. Those writing on defensible space have argued disadvantaged areas experience significant physical and social disorder that leads residents to retreat within their homes, meaning there is less natural surveillance and a decreased likelihood that criminal behavior is detected (Merry, 1981; Newman, 1973). Collective efficacy theory posits residents of disadvantaged neighborhoods may be less willing to intervene when crime occurs (Sampson & Groves, 1989; Sampson et al., 1997). Therefore, even if crimes are detected, the likelihood of formal or informal consequences being applied is smaller than in low disadvantage areas. High disadvantage areas then would suffer from a lack of capable guardianship as compared to low disadvantage areas. Whether through increased convergences or lack of capable guardianship, the number of crime opportunities can be expected to be larger in high disadvantage neighborhoods as compared to low disadvantage neighborhoods. This implies that there is nothing inherently criminogenic (i.e., no deviant subculture) about disadvantaged neighborhoods except that there are more criminal opportunities in them (Felson & Boba, 2010). If this is the case, neighborhood disadvantage acts as a proximate measure of criminal opportunity, and evidence of a positive statistical interaction (effect

of self-control becomes larger as disadvantage increases) is consistent with this interpretation of the general theory (Gottfredson & Hirschi, 1990).

Alternatively, neighborhood disadvantage could be viewed as an indicator of the degree of informal social control exerted by the collective neighborhood over the behaviors of individuals. When placed in contexts where private and parochial controls are weakened, individuals have fewer external restraints on their behavior (Bursik & Grasmick, 1993b; Hunter, 1985). When viewed from an integrated control-theory perspective, strong social controls *or* strong self-control can sufficiently insulate individuals from participating in criminal behavior, consistent with social psychological research. This suggests that joint failure of social and self-control will result in substantially higher rates of criminal and delinquent behavior than failure of self- or social control alone (Nye, 1958; Reckless, 1961; Reiss, 1951). Therefore, in non-disadvantaged areas (where social controls are presumably strong), individuals with low and high self-control alike will be restrained from participation in criminal behavior. Where social controls are weak however, the gap between low and high self-control individuals in criminal behaviors should widen.

Overall, the vulnerability hypothesis is that the effects of self-control increase with neighborhood disadvantage. In low disadvantage areas, behavior is influenced by the social context. As such, among the most privileged, guarded, and controlled areas, individual differences in self-control will have little effect on criminal behavior. However, among high disadvantage areas that are presumably less guarded and controlled, individual differences in self-control will manifest in differences in criminal behavior.

Trait Suppression Hypothesis. The trait suppression hypothesis contends that "strong" social situations (Mischel, 1977) create pressures to behave in certain ways, and as a result, individual differences are less important to behavior. Typically, strong social situations are conceived as situations where there are salient pushes to offend (Zimmerman, 2010). Thus, some have argued that self-control will have a weak effect in high disadvantage areas (Wikstrom & Loeber, 2000). Mechanisms explaining how low and high self-control individuals alike will be pushed into criminal offending in high disadvantaged areas are necessity and conformity to behavioral norms. Necessity is a clear dampener of individual differences because it implies that no other course of action is possible. To illustrate, individuals with chemical dependencies to narcotics need money to support their dependency, but often are unable to retain jobs, thus necessitating larceny. In Anderson's *Code of the Street* (1999), acting in a violent manner is sometimes necessary to protect one's reputation, or to preempt an attack (i.e., self-defense). The code itself is a set of behavioral norms that sanction interpersonal violence, retaliation for disrespect, ascription of heightened status for those with greater physical/mental abilities (street smarts) and desirable possessions. According to Anderson, the competition for respect pushes street youth to acquire those desirable possessions through criminal means because doing so enhances one's own worth (1999).

A central prediction of the trait suppression hypothesis is that in street culture an individual's self-image, even if the individual is not inclined to commit criminal behavior, depends on the ability to respond to victimization through means sanctioned by the code. Anderson describes how individuals outside of the street culture feel they have

the option *not* to participate in criminal behaviors, implying those immersed in street culture do not:

> The issue of respect is thus closely tied to whether a person has an inclination to be violent, even as a victim. In the wider society people may not feel required to retaliate physically after an attack, even though they are aware that they have been degraded or taken advantage of. They may feel a great need to defend themselves during an attack, or to behave in such a way as to deter aggression (middle-class people certainly can and do become victims of street-oriented youths), but they are much more likely than street-oriented people to feel that they can walk away from a possible altercation with their self-esteem intact. (1994, p. 89)

In all, the code of the streets purportedly (1) generates a set of expectations of how all individuals should behave in disadvantaged neighborhoods, (2) motivates individuals on the street to enforce these expectations in a way that enhances their reputation, and as a result (3) forces even those individuals not predisposed to criminal behavior to commit crimes. According to the trait suppression hypothesis then, only in low disadvantage areas can self-control differentiate individuals in terms of criminal and delinquent behavior. This is opposite of the vulnerability hypothesis, that the strongest effects of self-control will manifest in high disadvantage areas.

In summary, these hypotheses make different predictions about the direction and strength of the effects of neighborhood disadvantage on the effects of self-control. The trait suppression hypothesis posits strong situations are present in disadvantaged areas; these situations push individuals towards crime, and thus dampen the effects of individual traits. Consequently, neighborhood disadvantage has a negative effect on the effects of self-control and only in less disadvantaged neighborhoods does self-control matter as far as crime is concerned. The vulnerability hypothesis proposes a positive effect of neighborhood disadvantage on the effects of self-control on crime and delinquency. As

neighborhood disadvantage increases, the effect of self-control increases. Finally, the invariance hypothesis predicts neighborhood disadvantage has no effect on the effects of self-control, or invariance in the effect of self-control across neighborhoods.

A Revised Trait Suppression Hypothesis

In considering strong social situations, it is implied that individual differences would be dampened in situations where expectations for behavior clearly demand conformity *or* deviant behavior. Consider Michel:

> [strong situations] lead everyone to construe the particular events the same way, induce uniform expectancies regarding the most appropriate response pattern, provide adequate incentives for the performance of that response pattern and require skills that everyone has to the same extent. (1977, p. 347)

Interestingly, when describing how gangs in Los Angeles demand that individuals behave in certain ways, Shakur (1993) describes the push to go to college among suburban youth as a prosocial example of the degree to which individuals in certain contexts are expected to behave in a uniform matter. Furthermore, Lynam et al. (2000) use an example of the universal expectation that drivers stop at red traffic lights as a way to demonstrate strong pushes towards certain behavior. Despite highlighting the possibility that pushes to *prosocial* behavior could dampen the effects of individual traits, trait suppression has typically been articulated in such a way that self-control should have a stronger relationship with offending in low disadvantage neighborhoods as opposed to high disadvantage neighborhoods.

Zimmerman (2010), however, articulates how low disadvantaged neighborhoods could be strong contexts, stating:

> [O]ne could argue that criminogenic neighborhoods represent ‘‘weak’’ situations because they lack informal social controls (and cohesiveness), leading to unclear rules of conduct. Under this interpretation, neighborhoods with high levels of collective efficacy would represent ‘‘strong’’ social settings and suppress the effects of impulsivity on offending. (2010, p. 306)

He further states:

> [The] ‘‘vulnerability’’ hypothesis presumes that informal neighborhood controls should have a similarly restrictive effect on impulsive and non-impulsive individuals in lower risk areas. A similar contention is that a ‘‘good’’ neighborhood can push an individual to conformity just as a ‘‘bad’’ neighborhood can push an individual to offend. This argument suggests that individual traits such as impulsivity should be suppressed under strong neighborhood conditions, *either those that promote conformity or deviant behavior. [emphasis added]* (2010, p. 306)

Insofar as disadvantage acts as an indicator of the strength of behavioral norms, the strongest expectations to offend may exist in high disadvantage areas, and the strongest expectations to conform may exist in low disadvantage areas. A strict application of the strong social situation hypothesis would require self-control to have weak effects in both high and low disadvantage neighborhoods. As disadvantage increases or decreases from average, the effects of self-control will become weaker because the behavioral norms demanding conformity or offending become stronger, thus dampening individual differences. In medium disadvantage neighborhoods, there is a mix of behavioral expectations, leading to ambiguous expectations for behavior. Thus, individual traits can be expected to differentiate behaviors to a greater extent in such a context. Taken together, this revised trait suppression hypothesis predicts that the effect of neighborhood disadvantage on the effect of self-control is neither linear-positive nor linear-negative. Rather, it is dependent on the level of neighborhood disadvantage—its form is convex,

where increasing from low to average disadvantage increases the effects of self-control and increases beyond average disadvantage decrease the effects of self-control.

The corollary of this argument is that the effect of neighborhood disadvantage on crime is not only non-linear, the non-linearity is conditional on level of self-control. As Zimmerman (2015) indicates, the qualities of the neighborhood can *push* individuals into conforming or deviant behaviors depending on the degree of internal control. Individuals with low self-control could be pushed into conforming behavior if the neighborhood informal social control were strong, just as individuals with high self-control could be pushed into deviance in neighborhoods where pressures to offend are strong. These arguments imply the effect of neighborhood disadvantage follows a saturation effect among individuals with low self-control—increases in pressure to offend will have a considerable immediate impact, but level off relatively quickly. Thus the effect of disadvantage is quite large at first, but is smaller beyond a certain level of disadvantage. Quite the opposite is argued for individuals with high self-control. Increases in pressure to offend will have little impact, but at a certain degree of disadvantage, Anderson (1999) implies that nearly everyone will eventually be pushed into crime. As such, the effect of disadvantage on crime would be relatively weak at low levels of disadvantage but would strengthen as disadvantage increases.

Taken together, the revised trait suppression hypothesis suggests that levels of crime vary substantially by level of neighborhood disadvantage and level of self-control and the effects of these variables are not independent. Perhaps most importantly, scholars advancing the trait suppression hypothesis imply (1) the effect of self-control is weak in disadvantaged neighborhoods, and (2) the average level of crime is quite high. Likewise,

scholars advancing the vulnerability hypothesis argue that (3) lack of opportunity causes self-control to have a weak effect in low disadvantage neighborhoods, which implies (4) the level of crime is quite low as well. The revised trait suppression hypothesis retains these four predictions, and adds three more. (1) The effect of self-control is at its maximum in neighborhoods with moderate levels of disadvantage, and, (2) the level of crime among those with low self-control in low disadvantage neighborhoods is not higher than similarly situated individuals in high disadvantage neighborhoods. Likewise, (3) the level of crime among those with high self-control in high disadvantage neighborhoods is not lower than similarly situated individuals in low disadvantage neighborhoods.

From a theoretical point of view, the predictions made by the vulnerability hypothesis could manifest as *pressures to conform, dampening the effect of individual differences,* thus supporting the strong social situation hypothesis integral to the trait suppression hypothesis. It is important to note that this possibility is empirically indistinguishable from the trait suppression hypothesis if one conceives social control in less disadvantaged areas as a dampener of individual traits. Statistically, the trait suppression and vulnerability hypotheses are typically modelled using a product term to permit the statistical interaction of self-control and neighborhood disadvantage. This approach estimates a linear effect of neighborhood disadvantage on the effect of self-control across the entire distribution of neighborhood disadvantage scores. Evidence for the trait suppression view would manifest in the average difference in the effect of low self-control on crime across neighborhoods decreasing as a function of disadvantage. Evidence for the vulnerability hypothesis would manifest in the average difference in the effect of self-control on crime across neighborhoods increased as a function of

disadvantage. If a linear interaction effect is posited, the model must predict the maximum effect of self-control is strongest in either the highest or lowest disadvantage neighborhoods. A single product term between self-control and neighborhood disadvantage will not allow for the strongest effect to manifest in medium disadvantage neighborhoods. In fact, if the strongest effect of self-control is in medium disadvantage neighborhoods, and the effect of self-control is lower in low and high disadvantage neighborhoods, it is likely that evidence of no interaction will be found, and would be interpreted as consistent with the invariance hypothesis. In order to conclude the effect of self-control is invariant across level of neighborhood disadvantage, one must rule out other patterns of variation, including a curvilinear moderation effect. It is, therefore, the goal of this study to investigate whether specifying a curvilinear effect of neighborhood disadvantage on the effect of self-control provides an adequate explanation of crime. Further, this study aims to investigate whether specifying such an effect assists in organizing and understanding existing research findings, which are considered next.

III. LITERATURE REVIEW

Though several studies have examined the interaction of individual risk and neighborhood-level variables such as disadvantage, organization, or socio-economic status, the seven most relevant studies for the current research are displayed in Table 1. Each study examines the effect of self-control or a dimension of self-control, specifically, and predicts that neighborhood disadvantage moderates the effects of self-control. These studies uniformly create product terms to model the statistical interaction by multiplying the variables of interest and including the product in the regression equation. The findings column in Table 1 indicates the direction of the effect of the product term modelling the moderation of the effect of low self-control by neighborhood disadvantage. A positive sign indicates a positive effect of neighborhood disadvantage on the effect of low self-control (consistent with the vulnerability hypothesis), whereas a negative sign indicates a negative effect of neighborhood disadvantage on the effect of low self-control (consistent with the trait suppression hypothesis). A zero indicates no interaction was found. Most importantly, even a cursory review of Table 1 reveals there is evidence consistent with each hypothesis, and there is no consensus about the direction of the moderation effect.

Previous Studies

Lynam et al. (2000) found evidence consistent with the vulnerability hypothesis, namely, that impulsivity was unrelated to offending in high socioeconomic status (SES-low disadvantage) neighborhoods, but strongly related to offending in lower SES (high disadvantage) neighborhoods across multiple offending measures (variety, theft, and violent offending indices). Using the National Longitudinal Study of Adolescent Health, Vazsonyi et al. (2006) found no evidence of an interaction between impulsivity and

neighborhood disadvantage among male adolescents. Among female adolescents, however, as neighborhood disadvantage increased, the effects of impulsivity were dampened for general delinquency, and non-violent delinquency, consistent with the trait suppression view. Overall though, the Vazsonyi study found that the effects of impulsivity were not consistently moderated by neighborhood disadvantage, which they argue is consistent with the invariance hypothesis.

Meier et al. (2008), using data from a large regional study of youth (n = 85,000) found that the effects of impulsivity were amplified among youth living in riskier neighborhoods and similarly for boys and girls. Their study also included an indicator of callousness, which interacted with neighborhood risk. While callousness and impulsivity were positively related with delinquency in less risky neighborhoods, their effects became stronger in riskier neighborhoods. This study generally conforms with the findings in Lynam et al. (2000) and with the vulnerability hypothesis. The findings were also replicated in a separate sample. Jones and Lynam (2009) examined the moderating effects of perceived supervision on the effects of thrill and adventure seeking and lack of premeditation (facets of impulsivity) in a sample of youth. They found that these facets of impulsivity exerted significant positive effects on offending, but that among youth who perceived neighborhood supervision to be high, the effects were weaker. This suggests that the effects of impulsivity are stronger in neighborhoods with less supervision (presumably more disadvantaged neighborhoods).

Zimmerman (2010) took a different methodological approach to the impulsivity-neighborhood disadvantage interaction using the data from the Project of Human Development in Chicago Neighborhoods. This study generated findings in contrast with

Lynam et al. (2000), Meier et al. (2008), and Jones and Lynam (2009). Specifically, the positive effect of impulsivity on offending was only found in high SES neighborhoods. In low and middle SES neighborhoods, impulsivity was unrelated to offending. Zimmerman (2010) also examined collective efficacy as a mediator between neighborhood SES and the effect of impulsivity on offending, finding impulsivity to have a small positive effect on offending in neighborhoods with low-medium collective efficacy and strong positive effect in neighborhoods with high collective efficacy. The whole of the evidence presented by Zimmerman is consistent with the trait suppression hypothesis.

Yet another study by Zimmerman et al. (2015) was undertaken in Russia and Ukraine with adults. Though this study relied on measures that were very different from previous studies, they found the effects of self-control on projected offending became stronger as neighborhood SES increased. In other words, the effects of self-control were larger in low disadvantage areas and smaller in high disadvantage areas, again suggesting trait suppression. Finally, a recent article by Vogel (2016) re-analyzed Add Health data (although the sample was different from the Vazsonyi, 2006 study). Using the same impulsivity measure as the Vazsonyi (2006) study, Vogel found evidence consistent with the vulnerability hypothesis when using measures of disadvantage aggregated to the tract level, but evidence consistent with the invariance hypothesis when using measures of disadvantage aggregated to the block group level.

Importantly, the studies in Table 1 differ in terms of several important features that will be reviewed in turn. First, the measurement of impulsivity and self-control varies greatly from study to study. Second, measures of neighborhood disadvantage are also different. Third, there are slight differences in the outcome variables examined.

Relatedly, the modelling approach has varied from study to study. There are some similarities, however; the researchers, while using different populations, have typically examined youth (with one exception).

Differences in Measures of Self-control/ Impulsivity

Lynam et al. (2000) used a multi-method, multi-source measure of impulsivity that relied on a battery of tests administered to respondents, their parents and teachers to measure impulsivity. Multiple types of tests (computer, paper and pencil, etc.) were included to ensure respondents' engagement in the tasks, thus increasing the accuracy of the measures for respondents with attentional problems. Further, the Lynam et al. (2000) included multiple sources to assess impulsivity more reliably. The authors also aimed to measure multiple dimensions of impulsivity, while employing only previously empirically scrutinized measures with evidence of reliability and criterion validity. This measure included a time-perception task, the Stroop Color and Word Association Test (Stroop, 1935), the Trail Making Test (Lezak, 1983), the Circle Tracing Task (Bachorowski & Newman, 1985), a computerized delay of gratification task (Krueger, Caspi, Moffitt, White, & Stouthamer-Loeber, 1996), a computerized card-playing task (J. P. Newman, Patterson, & Kosson, 1987), a measure of ego undercontrol from the California Child Q-set completed by the caregiver (Caspi et al., 1992), the self-report Eysenck Impulsiveness Scale (Eysenck, Easting, & Pearson, 1984), six impulsivity items from the Teacher Report Form (Achenbach, 1991), and observations made by raters who were blind to respondents' scores. Six of the eleven tests rate performance on tasks. For example, in the circle-tracing task, respondents are asked to trace a circle as slowly and accurately as possible. Impulsive individuals tend to rush the task and record shorter

time-to-completion on successive trials (Bachorowski & Newman, 1985). As a result, the full battery of tests has been found to tap behavioral and cognitive impulsivity (White et al., 1994). However, Lynam et al. (2000) used a single scale based on a moderate correlation between the dimensions ($r = .53$). The estimate of reliability of the scale was found to be very good ($\alpha = .92$).

Vazsonyi et al. (2006) as well as Vogel (2016), employing the data from the National Longitudinal Study of Adolescent Health, utilized a four-question scale of impulsivity administered to respondents in a self-report questionnaire. The questions included: (1) "When you have a problem to solve, one of the first things you do is get as many facts about the problem as possible," (2) "When attempting to find a solution to a problem, you usually try to think of as many different ways to approach the problem as possible," (3) "When making decisions, you generally use a systematic method for judging and comparing alternatives," and (4) "After carrying out a solution to a problem, you usually try to analyze what went right and what went wrong." The measure taps cognitive impulsivity. However, the questions tap a very narrow range of content covered by the concept of impulsivity. Generally, the questions pertain to the respondents' decision-making abilities, and in particular, whether the respondents use a methodical approach in making and evaluating decisions. The variable analyzed was the un-weighted mean of the four Likert-type items. The estimate of the reliability of the scale was acceptable ($\alpha \approx .75$).

To measure impulsivity, Meier et al. (2008) also used a four-question scale collected in a self-administered questionnaire. The questions were (1) "I think things through carefully before I make a decision," (2) "Even if it is dangerous, I like to do

exciting things,, (3) “I believe that working hard now will make my life successful in the future,” and (4) “When I have problems, I am good at finding ways to fix them.” The content included in this measure is broader as compared to the measure in Vazsonyi et al. (2006), but is limited to cognitive impulsivity. The questions tap aspects of impulsivity, such as risk-seeking or thrill- and adventure-seeking, lack of premeditation, future orientation, as well as methodical problem solving and decision-making. This particular measure used the unweighted sum of the items, with a low reliability estimate ($\alpha \approx .5$) The low reliability was anticipated by the authors due to the broad range of content covered (they note that the questions cover three facets of impulsivity outlined in Whiteside & Lynam, 2001).

In addition to impulsivity, Meier et al. (2008) include a measure of callousness or lack of empathy in their study. This measure draws on three questions: (1) “I care about other people’s feelings,” (2) “I feel sorry for people who have had things stolen or damaged,” and (3) “It is important to help other people.” This measure reflects content that might be included to tap an element of self-control: self-centeredness. These questions resemble questions from other self-control scales, such as “I'm not very sympathetic to other people when they are having problems” (Grasmick et al., 1993), “I care about how others feel” (Vogel & Messner, 2012), and “I really don't care all that much about people's problems or illnesses” (Evans, Cullen, Burton Jr., Dunaway, & Benson, 1997). Lack of empathy could be identified as a correlate of self-centeredness, or could be conceptualized as a facet of self-centeredness. For the present purposes, it is important to note that the measure of callousness used by Meier et al. (2008) contains content that is inclusive in the conceptual discussion of self-centeredness in Gottfredson

and Hirschi, which describes individuals with low self-control as indifferent or insensitive to the suffering and needs of others (1990, p. 89). However, self-centeredness could contain a number of other facets besides lack of empathy (selfishness, antagonism, agreeableness, conscientiousness). The estimate of the reliability of the callousness scale was within acceptable limits ($\alpha = .75$).

Jones and Lynam (2009) examined data from the Lexington Longitudinal study and employed measures of impulsivity derived from the Urgency-Premeditation-Perseverance-Sensation Seeking (UPPS) framework (Whiteside & Lynam, 2001). The measures included 13 questions assessing consideration of consequences before acting, and 10 questions assessing desires to participate in physical activates construed as risky from well-validated instruments. These questions tap into the lack-of-premeditation and sensation-seeking facets of impulsivity. The authors found the scales to have acceptable reliability (both scales $\alpha = .81$).

Zimmerman (2010) analyzed data from the Project on Human Development in Chicago Neighborhoods and measured impulsivity using primary caregiver reports of respondents' behaviors. The questions involved assessing concentration, attention, sitting still, restlessness, hyperactivity, demand for attention, nervousness, being accident-prone, acting without stopping to think, twitches, and repetitive or compulsive thoughts. The items were derived from the Achenbach Child-Behavior Checklist (Achenbach, 1993), and the data were collected at the time the respondents were 12 or 15 years of age. The authors noted that many of the items do not align well with other measures of impulsivity (such as repeats actions over and over, or is confused or in a fog). The full scale certainly covers a broad range of content, but places emphasis on hyperactive characteristics, such

as twitches, restlessness, nervousness, or a tense disposition. A revised scale including only the questions that pertained to perseverance or lack of premeditation was constructed as well. Despite differences in the scales, the conclusions drawn from the analyses were unchanged. The alpha coefficient estimates of the reliabilities of the 10- and 4-item scales were .80 and .64, respectively.

Finally, Zimmerman et al. (2015) examine *self-control*, which is different from the previous four studies that examine impulsivity specifically. They measure self-control behaviorally as well as attitudinally. The behavior measure relies on the frequency of engaging in several behaviors: smoking tobacco, forgetting to take medication, taking extra precaution to avoid illness, arriving late for work or appointment, saving money, exercising regularly, losing things, and engaging in risky sexual behaviors. They dichotomized each measure and created a variety index for regularly exercising behavior consistent with self-control. They also relied on the Grasmick et al. (1993) self-control scale to measure self-control attitudinally. This measure covers the six elements of self-control (risk seeking, impulsivity, self-centeredness, temper, and physical and simple task preference). The reliability estimate of the attitudinal measure was acceptable ($\alpha = .82$).

This collection of studies uses divergent measures of the central theoretical construct, making comparison of the findings difficult. The measures range from very narrowly constructed scales quantifying the methodical nature of respondents' decision-making (Vazsonyi et al., 2006; Vogel, 2016), to measures inclusive of multiple facets of impulsivity (Jones & Lynam, 2009), to broad indices of impulsivity inclusive of behavioral, performance-based, and cognitively focused tests (Lynam et al., 2000). Even still, one article (Zimmerman et al., 2015) focuses on the broader concept of self-control,

inclusive of the elements outlined by Gottfredson and Hirschi (1990). Meier et al. (2008) also measured callousness in their study, covering a larger range of content for the concept of self-control than the studies measuring impulsivity alone.

There are some similarities between the scale used by Vazsonyi et al. (2006) and Vogel (2016), and the revised scale used in Zimmerman (2010). Both emphasize the way respondents report making and evaluating decisions, measuring impulsivity as the degree to which respondents are methodical in problem solving. Unfortunately direct comparisons of their findings are not possible as Zimmerman (2010) statistically controlled for respondent gender in his analysis and Vazsonyi et al. (2006) estimated gender-specific models. While Vazsonyi et al. (2006) found that the effect of impulsivity on general delinquency and aggression was stronger in less disadvantaged neighborhoods, this effect was found only for female respondents. Zimmerman (2010), however, found the effect of impulsivity on violent and non-violent crime was stronger in less disadvantaged neighborhoods net of respondent gender. Comparing Zimmerman (2010) and Vogel (2016), using similar measures these researchers reached the opposite conclusion.

Figure 1 visually displays the elements of self-control with reference to prior studies' measures of the concepts. The circles represent the six elements of self-control[3]. It should also be noted that the broader conceptualization of impulsivity, conceived as delay-discounting, is mapped into Figure 1 with a dashed line. Importantly, this broad conceptualization includes the elements of impulsivity and risk seeking from the general

[3]A later section (see pp. 43-46) discusses the exclusion of preferences for simple or physical tasks from the conceptualization of self-control. These circles are smaller to visualize the emphasis on the remaining four dimensions.

theory (Gottfredson & Hirschi, 1990) and its subordinate facets in hexagons (e.g., Whiteside & Lynam, 2001; Zuckerman, 1994). The hexagons in Figure 1 represent the facets of each concept utilized in prior studies' measures of self-control. Facets of the UPPS scale (Whiteside & Lynam, 2001), for example, are located within of impulsivity and risk seeking. However, impulsivity as conceived by Gottfredson and Hirschi (1990) subsumes a number of facets developed in psychology, including lack of persistence, lack of premeditation, urgency, present and future orientations, and perhaps hyperactivity.

Empirically, the measure employed by Lynam et al. (2000) covers the majority of content included in the impulsivity circle. Jones and Lynam (2009) used a measure tapping two facets of the broader conception of impulsivity. Vazsonyi et al. (2006) and Zimmerman (2010) employ measures that focus on thorough decision making or premeditation, although Zimmerman's measure does include some elements of hyperactivity. The measure used in Zimmerman et al. (2015) was the Grasmick scale. The impulsivity dimension of the Grasmick scale focuses on lack-of-premeditation and measures the remaining elements broadly. As noted above, there is some overlap with the measure of callousness used by Meier et al. (2008) and the measure of self-centeredness from the Grasmick scale. Further, Meier et al.'s (2008) measure of impulsivity contained questions targeting lack of persistence, future orientation, risk seeking, and lack of premeditation.

Having located the relative position of previous measures in a conceptual map of self-control permits a critical examination of the content validity of each prior study's measures. Sufficient coverage of conceptual breadth would provide evidence of content validity. A well-covered concept or dimension of a concept is measured using questions

that tap each facet of the dimension or each dimension of the concept. By these standards, studies employing measures of impulsivity that are very narrow, such as the 4-item reduced scale from Zimmerman (2010) and the scale used in Vazsonyi et al. (2006), insufficiently cover the concept of impulsivity, and necessarily undercover the concept of self-control. The conclusions drawn from these two studies should be limited to the effects of neighborhood disadvantage on lack of premeditation or methodical problem-solving approaches specifically, and not impulsivity or self-control, generally. In contrast, the measures from Lynam et al. (2000) and Meier et al. (2008) provide better coverage of the facets of impulsivity. Finally, the measure used in Zimmerman et al. (2015) covers all dimensions of the elements of self-control, thereby covering the concept of self-control sufficiently. As should be apparent, the discussion of Figure 1 further highlights the stark differences in measures of the concept central to tests of the self-control-neighborhood disadvantage interaction. Even studies purporting to measure impulsivity alone have used highly divergent measures, calling into question the comparability of their findings. For now, this discussion should highlight the need to measure self-control in such a way that sufficient coverage of the breadth of the concept is achieved.

Differences in Measures of Neighborhood Disadvantage

While these studies also used divergent neighborhood measures, two types of measures can be identified. Several authors relied on factor scores from factor analytic models of census data commonly used in the measurement of neighborhood socio-economic status. Others used self-report questionnaires tapping neighborhood conditions. Study number one from Lynam et al. (2000), and studies by Vazsonyi et al. (2006), and Zimmerman (2010) and Vogel (2016) used census data, such as the percent of families

below the poverty line, percent of households with incomes lower than $15,000, percent of households receiving public assistance, percent of non-intact families with children, percent of population unemployed, percent of adult male population unemployed, median household income, and percent of population non-White. Each study utilized a variation of a factor score derived from principal components analysis. Study one by Lynam et al. (2000) and Zimmerman (2010) created discrete SES categories based on quartiles, while Vazsonyi et al. (2006) and Vogel (2016) used the factor scores as a continuous indicator of SES, as well as deciles. Despite similarities in the construction of the scale, the level of aggregation varied across these studies. Vogel (2016) compared the effects when examining disadvantage at the block group and tract levels, whereas Vazsonyi et al. (2006) examined only census block groups. The neighborhood measures in Zimmerman (2010) were derived from neighborhood clusters, which are well established groups of census tracts in Chicago, and Lynam et al., (2000) used a similar approach (measured neighborhood disadvantage by aggregating tracts to the neighborhood level in Pittsburg).

Study number two from Lynam et al. (2000), Meier et al. (2008), Jones and Lynam (2009), and Zimmerman et al. (2015) used self-report questionnaires to assess neighborhood conditions. For the second study reported by Lynam et al. (2000), respondents were asked whether assaults, muggings, delinquent gangs, drug use, drug dealing, unemployment, run-down or poorly kept buildings, and abandoned houses were problems in their neighborhood. The measure was not aggregated to the neighborhood level, but instead was used as an individual-level predictor in their analyses. The neighborhood disorder index had good internal consistency ($\alpha = .89$). The Meier et al. (2008) study assessed neighborhood risk by asking respondents if (1) “My neighbors get

along well," (2) "If someone in my neighborhood or community saw me doing something wrong, they would tell one of my parents," (3) "Adults in my community care about people my age," (4) There are enough places for kids my age to go that are alcohol and drug free," (5) "Adults in my neighborhood or community help me when I need help", (6) "Adults in my neighborhood or community let me know they are proud of me," and (7) "Adults in my neighborhood or community spend time talking to me." These questions were dichotomized (yes/no), summed, and the index used as an individual-level predictor of neighborhood quality.

Jones and Lynam (2009) used two questions closely related to the collective efficacy scales from Sampson et al. (1997). They assessed the extent to which youth perceived neighborhood residents supervised youth's behavior, tapping into perceptions of informal social control. Finally, Zimmerman et al. (2015) asked respondents whether they could afford to (1) 'buy all of the groceries they needed," (2)" buy all of the clothes they needed," (3) "buy all of the durable goods they needed," (4) "travel abroad for leisure," (5) "buy a car," and (6) "buy an apartment or house." The items were dichotomized, summed for each respondent, and then averaged across all respondents within neighborhoods to create an aggregate "SES" measure for each neighborhood.

These measures ultimately pit objectively measured criteria pertaining to the economic and social conditions within a given geographic boundary against perceived measures of the respondents' (and their neighborhoods') economic and social well-being, or in the case of Jones and Lynam (2009), respondents' perceptions of informal social control. While the objective and perceptual measures are not tapping precisely the same constructs, it is plausible that the objective neighborhood conditions affect crime and the

effects of individual variables through perceptions of these conditions. Therefore, one may expect the perception variables to exert larger effects, and the objective measures to exert somewhat attenuated effects.

Differences in Dependent Variable and Modeling Approaches

With the exception of Zimmerman et al. (2015) who examined projections of the likelihood of offending in the future, each study examined self-reported, past delinquency as the dependent variable. In addition to general delinquency (a variety index of 35 different offenses), Lynam et al. (2000) examined categories of offenses including vice offenses, status offenses, theft, and violence separately. Each scale was positively skewed, and there was no correction for non-normality. A multilevel linear model was employed to explain each outcome. Vazsonyi et al. (2006) and Zimmerman (2010) examined non-violent or property offenses separately from violence or aggression. While the general, aggression, and non-violent delinquency scales were positively skewed, Vazsonyi et al. (2006) employed a square root transformation to create a normal distribution. The data were then analyzed using a multivariate, multilevel linear model. Zimmerman (2010), on the other hand, addressed the highly skewed and censored nature of the offending data by using a multivariate, multilevel Rasch model (see Raudenbush, Johnson, & Sampson, 2003), as opposed to transforming the data. Meier et al. (2008) examined general delinquency as an index of seven different offenses, including some property and some violent offenses. The distribution of the variety index in this study was positively skewed and left-censored, and there was no correction to induce normality.

The analytic framework utilized by Lynam et al. (2000), Vazsonyi et al. (2006), and Meier et al. (2008) was the multilevel linear model. Linear models may be

problematic when examining highly skewed and censored distributions, such as those generated when examining multiple item measures of crime and deviance (Osgood, Finken, et al., 2002). One problem concerns the possibility of generating out-of-range predicted values, such as negative participation in crime and delinquency (a problem similar to applying the linear probability model to dichotomous outcomes, which generates negative predicted probabilities). More important for the present purposes are the consequences of violating assumptions of OLS (and other linear models) in the context of modelling statistical interactions. Highly skewed and limited distributions violate the additivity assumptions of linear models, where the effects of explanatory variables are constant across the full range of values of the dependent variable (Vasquez, 2010). As a consequence, statistical complexities, such as non-linearity or statistical interaction between two variables, may result as a consequence of the violated assumptions, and not for substantive reasons (Osgood, et al., 2002).

Problem and Hypotheses

The substantive issue at hand is that a consensus on the direction and functional form of the effects of neighborhood disadvantage on the effect of self-control on crime has yet to be reached. In Chapter 2, a revised trait suppression hypothesis was discussed, and it was highlighted that investigators modelling linear moderation effects may conclude the effects of self-control are invariant if the moderation effect is curvilinear. Thus, one explanation for mixed findings is model mis-specification, a substantive issue. However, as was made clear in the previous chapter, there are several differences in the designs of the studies examining the self-control-neighborhood disadvantage interaction. Foremost among these differences was the large amount of variability in measures of

self-control. The previous chapter examined the content validity of these measures, highlighting coverage of the concept as problematic for some studies. Another design feature that is problematic in this context is the use of OLS or other linear models when examining crime as an outcome and modelling a statistical interaction. While these design features could contribute to the possibility of mixed findings, this study will not investigate the effects of changing design features on conclusions regarding the substantive hypotheses.

Rather, this study aims to revisit the self-control-neighborhood disadvantage interaction, and provide a critical examination of the vulnerability, trait suppression and revised trait suppression hypotheses. These hypotheses concern the additivity/interactivity of self-control and neighborhood disadvantage in predicting crime and delinquency. Four theoretical rationales have been explicated at length above. Most importantly, a revised trait suppression hypothesis posits a curvilinear effect of neighborhood disadvantage on the effects of self-control on crime. This curvilinear effect is an attractive explanation for the collection of existing findings, as one might expect positive, negative and null effects to emerge from sample to sample if a curvilinear effect was modeled as a linear effect. The theoretical rationale for a curvilinear effect is also compelling; pushes to conform (whether in criminal or prosocial ways) exist in various forms across different types of neighborhoods.

In order to test these arguments, they are translated formally into Hypotheses 1-4:

Hypothesis 1: There is a positive effect of neighborhood disadvantage on the effect of self-control on crime and delinquency.

Hypothesis 2: There is no effect of neighborhood disadvantage on the effect of self-control on crime and delinquency.

Hypothesis 3: There is a negative effect of neighborhood disadvantage on the effect of self-control on crime and delinquency.

Hypothesis 4: There is a curvilinear effect of neighborhood disadvantage on the effect of self-control on crime on delinquency, where the strongest effect of self-control exists in neighborhoods with an average level of disadvantage.

IV. CONCEPTUALIZATION AND OPERATIONALIZATION CONSIDERATIONS

Conceptualization of Self-Control

This study is fundamentally concerned with self-control as it pertains to criminal offending, specifically in the etiology of crime and delinquency. Central to this problem is defining self-control in a way that maintains the complexity of the concept and still proves useful to criminologists. To try to accommodate all conceptions of self-control is to make the concept too broad to be useful. Recalling the conceptualization recently explicated by Hay and Meldrum as " the practice of overriding immediate impulses to replace them with responses that adhere to higher-order standards that typically follow from values, social commitments, and interest in long-term well-being" (2016, p. 7), in contrasting this definition with the "elements of self-control" (Gottfredson & Hirschi, 1990, pp. 89-91), two groupings of elements become apparent. The first grouping, preference for simple tasks and preference for physical tasks, contrasts with the second, impulsivity, risk-seeking, temper, and self-centeredness. Importantly, preferences for physical tasks and simple tasks, are not necessarily linked to the pursuit of lower-order goals in the way that temper, self-centeredness, risk seeking and impulsivity are. Physical tasks associated with great athleticism often take years of dedication, practice, and attention to physical fitness. While more complex tasks often coincide with the pursuit of more advanced goals, such as earning professional degrees in law or medicine, individuals pursuing craft professions are pursuing the same abstract goals in sometimes less complex ways.

This is not to deny that in *some* cases, a preference for physical or simple tasks reflects the substitution of more exciting, less complicated, or more pleasurable acts because the ability to adhere to higher-order standards is weak. However, because offenders pursuing self-interest may take an *easier* route to obtaining money or relief from momentary irritation by committing acts of fraud or force, it does not follow that they would choose the *simplest* or *most physical* approaches to force or fraud. It is much more likely that they will choose the least risky opportunities to avoid detection. They may choose a physical act of aggression *or* a mental act of coercion. They may often choose the simplest route because of practical constraints. It does not necessarily follow from a conceptualization of self-control, self-interest, or impulsivity that no "immediate planning" (Polakowski, 1994; Shapiro, 1965) occurs, or that the cognitive processes involved in criminal decision-making occur in a very simple, or physical way. For this reason, indicators of preference for simple or physical tasks are of less utility in measuring self-control.

In prior studies of self-control, these elements were not found to be very problematic. For example, in a study by Arneklev, Grasmick, Tittle, and Bursik Jr. (1993), excluding these elements from a composite scale increased the standardized effects of self-control on several imprudent behavior measures by at least 25%. This is likely because the content within each dimension (preference for physical tasks, etc.) related enough with self-control that its departures from the concept (i.e., unique contributions) were averaged out when the dimensions were combined. Where measures of the dimensions are used to predict criminal outcomes separately, the limitations of measures of these dimensions become more apparent. For example, when the dimensions

are examined separately, the effects of preferences for physical and simple tasks are substantially smaller than the effects of the remaining dimensions (Longshore, Turner, & Stein, 1996). The variation in simple task preference and physical task preference items, once the variation in those items due to self-control is partialed out, is less predictive of crime and victimization, and for simple task preference, the relationship is in the opposite direction than expected (Ward, Nobles, & Fox, 2015).

Ultimately, impulsivity, risk-seeking, temper, and self-centeredness reflect basic drives central to the assumptions of control theory (hedonism or egoism). Further, the tendencies toward seeking excitement, immediate gratification, irritability, hot headedness, or selfishness are very closely linked to a conception of the pursuit of lower-order goals. Impulsive individuals, by definition, pursue more concrete and proximate goals, discounting the value of distal, more valuable ones (Madden & Bickel, 2010). As excitement or sensation is linked to the release of neuro-transmitters associated with rewards or pleasure (Zuckerman, 1994), a tendency towards excitement seeking can also be viewed as a way of pursuing immediate rewards over those provided in the future for less exciting and persistence-demanding tasks. Taking the conceptualizations of self-control proposed by Fujita (2011) and Hay and Meldrum (2016) as the basis for this study, therefore, requires a narrower view of self-control than explained by Gottfredson and Hirschi (1990). Here, self-control is viewed as comprised of impulsivity, temper, self-centeredness, and risk-seeking.

Conceptualization of Neighborhood Disadvantage

Neighborhood disadvantage has generally been conceived as poor economic and social well-being, both at the neighborhood level (i.e., the physical and economic

conditions of the neighborhood) as well as compositionally in terms of the aggregate characteristics of the population (i.e., people and families' wealth or SES). One view of disadvantage is that it is produced in conjunction with disorganization in urban places because of scarcity of resources (i.e., biotic order see Park & Burgess, 1924). Disadvantage can be conceived as the inability of neighborhood residents to mobilize resources to shape the structure, characteristics, and well-being of the environment and its residents (Bursik & Grasmick, 1993b). As such, indicators of disadvantage and social disorganization have been used to characterize the risk level of neighborhoods, as influence in economic, political, and organizational spheres is central to creating and maintaining desirable areas and preventing crime. The structural variables in Shaw & McKay's (1924) social disorganization framework serve as useful starting places in conceiving neighborhood disadvantage: low socioeconomic status of residents, high population turnover, high proportion of female headed households, and a high degree of urbanization (see also Wilson, 1987). Economic well-being (i.e., median income), ethnic heterogeneity, and family structure, however, have been key variables in measuring neighborhood SES for decades. The physical characteristics of disadvantaged neighborhoods have also been invoked as part of the concept of neighborhood disadvantage. The physical or social disorder within a neighborhood is reciprocally related with residents' involvement in preventing crime in defensible space, as advanced by broken windows theories (Kelling & Wilson, 1982; Merry, 1981; O. Newman, 1973). Taken together, the disadvantaged neighborhood suffers from a number of physical, social, economic, and organizational ills discussed in many studies of the urban

landscape. Thus disadvantage could be conceptualized as inclusive of many content domains.

Conceptualization of Crime

To Gottfredson and Hirschi (1990), crimes are acts of force or fraud performed in pursuit of self-interest. Based on the theoretical traditions examined here, this conceptualization is fitting. However, two separate, more in-depth issues concerning the conceptualization of crime have been at the forefront of the criminological literature. Primarily, whether particular types of criminal behavior can be identified has received considerable theoretical and empirical attention. A debate on the specialization or generality of offending is relevant to this issue. A secondary issue concerns the nature of criminal behavior and in particular theorizing a scale upon which criminal behavior can be measured. The latter issue is related to the specialization debate, but receives more attention in a later section. What is important at present is proposing and defending a conceptualization of crime that will be used here.

The offender specialization-generality debate has relevance to the current concern of conceptualizing criminal behavior because it brings to bear evidence on whether a single underlying trait underlies responses to self-report offending questions about a variety of different types of crime. The dominant assertion concerning offender "specialization" is that offenders do not specialize in any single type of behavior, or engage only in one particular offense or type of offense repeatedly (Gottfredson & Hirschi, 1990). If offenders exclusively participate in types of crime, such as drug crime, auto-related crime, violent crime, theft, and so on, one could argue there are dimensions to engaging in criminal behavior. Perhaps these various dimensions would be

differentially related to etiological variables. On the whole though, there is a substantial body of evidence that offenders are versatile, engaging not only in a wide variety of different crimes, but also in a variety of non-criminal acts, and "analogous behaviors." Using a variety of methodologies and samples, considerable support for a single underlying dimension characterizing measures of self-report offending has accumulated (Donovan & Jessor, 1985; Hindelang & Weis, 1972; Nye & Short, 1957; Rowe & Flannery, 1994).

Only recently have studies revisited this issue and called into question the evidence supporting offender generality by claiming that such evidence is biased by the aggregation of offense types, and time-periods. Sullivan, McGloin, Pratt, and Piquero (2006) tested the claim that offenders will tend to repeat types of behavior in the short-run by creating diversity index scores (Agresti & Agresti, 1978) and examining these scores over monthly, yearly, and 3-year time periods. The authors found that offenders tended to engage in fewer types of criminal behaviors in the monthly time-period as compared to longer temporal frames.

Evidence on either side of the specialization debate, whether it be low diversity index scores, or a substantively significant correlation between versatility indices and predictors of crime and delinquency, has been taken as support for specialization *and* generality. On the one hand, the variety of criminal behaviors engaged in by people is commonly used as an indicator of the extent of their criminal behavior (aside from the number of offenses committed) (Hindelang, Hirschi, & Weis, 1981; Osgood, McMorris, & Potenza, 2002). However, opportunities for crimes or certain types of crime are thought to contribute to whatever specialization may occur (Gottfredson & Hirschi, 1990,

p. 92). In other words, the effect of opportunity relative to the effect of individual propensity in explaining *offending versatility* is of central importance to the generality-specialization debate. Should variation in life circumstances or repeated opportunities for the same crime or type of crime fail to explain away a substantial portion of variation in even short-term specialization, there would be less reason to expect offender generality. McGloin, Sullivan, Piquero, and Pratt (2007) found that a number of life circumstances do affect specialization in the short term, but concluded that individual propensities as well as changes in life circumstances predict offending specialization at the monthly temporal resolution.

Nonetheless, when examining time periods longer than one month, the empirical evidence consistently demonstrates that individuals engaging in a larger number of offenses tend to engage in a wider variety of offenses (Hindelang et al., 1981; McGloin et al., 2007; Sullivan et al., 2006). Many studies of crime and delinquency also examine time periods no shorter than 6-12 months (in order to increase variation in offending); therefore the degree of specialization observed is typically smaller. In addition to the empirical evidence, criminological theories often attempt to explain the etiology of criminal behaviors in general, as opposed to subtypes of criminal behavior. Therefore, a conceptualization of criminal behavior consistent with evidence and theory is one that treats types of criminal behaviors as convenient labels that are less important for understanding the etiology of crime. For the present purposes, what differences do exist in the mechanisms that produce violent or property offending or other types of offenses are set aside. Not only is this approach consistent with the theoretical arguments

advanced by Gottfredson and Hirschi (1990); it is parsimonious. In this work, criminal behavior is conceptualized as a unitary and continuous concept.

V. METHODOLOGY

In order to test the hypothesis that neighborhood disadvantage moderates the effect of self-control, such that the effect of self-control is strongest in moderately disadvantaged places, but weaker in low disadvantage and high disadvantage places, several research design concerns are relevant. Primarily, because of the proposed curvilinear functional form of the effect of neighborhood disadvantage on the effect of self-control, it is important to examine cases that lie along the full range of the distribution of neighborhood disadvantage. If the moderation effect is curvilinear, samples where the range of neighborhood disadvantage is restricted may find neighborhood disadvantage to have a positive (low to medium disadvantage), negative (medium to high disadvantage), or null (medium disadvantage only) effect on the impact of self-control. Some prior research associates neighborhood conditions, particularly concentrated disadvantage and neighborhood level income, with high school graduation (Brooks-Gunn et al., 1993; Woodtke, Harding, & Elwert, 2011). Therefore, attrition rates in nationally- representative school-based studies of youth can be expected to be different based on the level of neighborhood disadvantage as youth in high disadvantage neighborhoods are more likely to drop out of school. Thus, they have a lower probability of being observed or retained in such studies. If youth in high disadvantage neighborhoods are included, they may not be representative of youth living in high disadvantage places. This suggests nationally representative, school-based surveys of youth may not capture the full distribution of neighborhood disadvantage scores.

Another concern, in terms of research design is the quality of measurement and the breadth of content covered in measures of the key concepts. As was highlighted

above, several prior studies have used measures of self-control that were quite narrowly constructed. Thus, these measures may capture content that is related to self-control, but not the full range of content relevant to the concept. As the goal of this study is to examine the effects of self-control as a broad concept, coverage is a central concern. In all, measurement and sampling concerns primarily drove the selection of the archived data collected in the Pathways to Desistence study, which used high quality measures in face-to-face interviews of high-risk youth. The details of the study are discussed below.

Pathways to Desistance Research Design & Sampling

The Pathways to Desistance (PTD) study is a completed longitudinal study of high-risk youth aged 14 to 17 in two counties in the United States. Detailed information about Pathways to Desistance is available in an article by Schubert, Mulvey, Steinberg, Cauffman, Losoya, Hecher, Chassin, and Knight (2004). In the following paragraphs, details from their description of the key design features are drawn on to properly contextualize this research. Below, the overall goals of the PTD study as well as the processes for (1) site selection, (2) sampling, (3) interview design, (4) data collection, and (5) retention are discussed.

The purpose of the PTD study was to investigate systematically the process by which serious adolescent offenders desist from antisocial activity. As such, the study aimed to gather information to understand why adolescents engaged in crime, often-serious crime, slow their rate of offending or stop offending altogether later in life. For participants, prior involvement in serious crime was obviously a necessary criterion for inclusion in the PTD study. Similarly, as the study aimed to assess how official interventions impact cessation from criminal behavior, adolescents who were processed

in the criminal justice system were of particular interest. As such, the population of interest in the PTD study was adolescents adjudicated delinquent or found guilty of a serious offense (felony offenses with the exception of less serious property crimes, misdemeanor weapons offenses and misdemeanor sexual assault)[4]. In order to increase generalizability of inferences about the role of the juvenile justice system in bringing about cessation from criminal behavior, several U.S. counties were considered. Maricopa County, Arizona and Philadelphia County, Pennsylvania were chosen based on the rate of serious crime committed by juveniles, racial and ethnic diversity of potential respondents, percent of female offenders, differences in the operation of official systems, political and research support, and practical data collection reasons. The enrollment period lasted from November 2000 to January 2003 (27 months) during which, 3,807 youth met the charge, age, and adjudication criteria for inclusion in the study (the 3,807 youth are the study population). PTD researchers attempted to enroll 2,009 of the 3,807 eligible youth. Not all eligible youth were approached because the caseload would have overloaded interviewers, or because the study was nearing its cap on drug offenders. In total, 1,354 of the 2,009 solicited youth (67%) enrolled in the study.

In terms of assessing the representativeness of the study sample to the study population, Schubert, et al. (2004) conducted a comparison of the not enrolled-adjudicated youth (n = 2,443) and the study sample (n = 1,354). They found the enrolled group to be slightly younger on average (0.2 years) than the not enrolled group ($t = -4.42$, $p < .05$, *Cohen's D* = .11). In the enrolled group, the mean number of prior petitions was equal to 2.1, compared to 1.5 prior petitions in the not enrolled group ($t = 8.78$, $p < .05$,

[4] The percent of male juveniles with drug offenses was limited to 15% in each site. All female juveniles were eligible for the study, as well as all youth being considered for trial in the adult system.

Cohen's D = .27). Further, the average age at first juvenile court petition was 13.9 in the enrolled group and 14.2 in the not enrolled group ($t = -3.29$, $p < .05$, *Cohen's D* = .18). Earlier onset is associated with more serious and continued offending (see Moffitt et al., 2008, pp. 4-5). As the study sample was younger, and obviously had greater mean levels of prior contact with the juvenile court, the study sample is composed of more serious offenders than the study population. This evidence is bolstered by the fact that the proportion of youth in the enrolled group receiving harsher penalties (residential placement/jail = 0.21) was greater than the proportion of youth in the not enrolled group receiving the same penalties (0.10-0.15). The effect sizes of these differences, however, are relatively small, and were anticipated due to the cap placed on less serious drug offenders.

The interview design and data collection were substantial undertakings in the PTD study. The initial interview lasted approximately four hours, and the follow-up interviews, two hours each. The interviews covered several content areas and methods of obtaining information about changes in the adolescents' lives. There were numerous factors considered in the selection of measurement instruments for use in this high-risk, adolescent population. Similarly, extensive pilot testing of measurement instruments was undertaken to assist in constructing the comprehensive battery of measures in the PTD study. Additionally, to capture certain domains of contextual and developmental change, a life-calendar method was utilized (Caspi et al., 1996; Horney, Osgood & Marshall, 1995). This involved providing a calendar to the respondents to track changes in residence, employment, criminal involvement, among others, by month. The interviews were conducted by trained interviewers who were randomly assigned at the beginning of

the study and remained paired with the respondent throughout the study. The random assignment and consistency in the pairing of interviews of participants is beneficial to the study in a number of ways (e.g., development of rapport, ability to recall and cross reference information to increase accuracy). The data were collected using laptop computers. The interviews were often conducted with the interviewer and respondent side-by-side with the computer screen visible to both.

Finally, the lives of the respondents in the PTD study were relatively chaotic and unstable, which posed challenges in the way of retention. Not only did the PTD researchers rely on a standardized protocol for attempting to contact the respondents or the respondent's family, contact with professionals responsible for monitoring participants (probation officers/ case workers) permitted PTD researchers to retain respondents that were difficult to contact. For the initial interview and follow-ups 1 and 2, 92% of the sample provided full data (present at enrollment, and follow-ups 1 and 2). The PTD researchers were quite successful in collecting complete data from a difficult population to follow over a long time period. The foregoing discussion relies entirely on the detail presented by Schubert et al. (2004), however, only a portion of the design features were discussed here. More thorough information about the design of the PTD study is provided by Schubert et al. (2004) and additional detail reference the theoretical approach to studying desistence and the existing literature is provided in Mulvey et al. (2004).

Current Study Design

This research utilizes secondary data analysis of observational data. Of central concern is the temporal ordering of the concepts in this analysis. The theoretical models

proposed above uniformly position neighborhood disadvantage as a contemporaneous influence of criminal behavior and the effect of self-control. That is, the hypotheses above suggest that neighborhood disadvantage acts in the process of deciding whether to participate in a criminal act. On the other hand, self-control is viewed as an exogenous influence of crime, positioned in time before the opportunity for crime. While the General Theory of Crime (Gottfredson & Hirschi, 1990) and its revision (Hirschi, 2004) imply refrainment from crime is due to the process of considering the costs of a potential act, the individual characteristic exogenous to both crime and cost consideration is self-control. This suggests that self-control exists in time before both deliberation and crime. Therefore, a longitudinal design is necessary capture the theoretical process hypothesized here. In terms of the temporal ordering of concepts, self-control at time one is hypothesized to affect offending at time two, with neighborhood disadvantage contemporaneously acting on the effect of self-control on crime and on crime, itself. This research utilizes PTD data collected at enrollment and in follow-up interviews one and two. The analysis examines follow-up interviews one and two simultaneously, to form the observation period time two. As such, additional steps to pool the data were necessary. Where variables were combined from follow-up interviews one and two, the procedure used is explicitly noted.

Case Selection. Because this research focuses on the contemporaneous effects of neighborhood disadvantage on offending and the effect of self-control on offending, respondents who were not exposed to community contexts are censored for meaningful neighborhood disadvantage measures. Three steps were taken to identify cases with sufficient exposure to community contexts. First, interview completion variables were

examined. Second, respondents detained for the duration of the follow-up periods were identified. Third, those who completed interviews and were not detained for the duration of the follow-up periods, the amount and proportion of time spent in the community was used to select cases. Respondents' interview completion statuses, detainment status, and degrees of community exposure are broken down in Table 3.

Respondents who failed to complete both interviews were excluded (n = 48), this group comprised 3.55% of the full sample[5]. However, respondents who completed one follow-up interview but missed the other (n = 85), about 6.80% of the full sample, were not categorically excluded from the analysis. This included 44 respondents present at follow-up 1 but not 2, and 48 respondents present at follow-up 2 but not 1. After examining interview completion status, using in the PTD data[6], respondents who were in secure detention facilities for the duration of follow-ups one or two were identified. Of the 1,221 respondents who completed both interviews, 239 were detained for the entirety of both recall periods and were necessarily excluded. Of the respondents who completed only one follow-up interview, 32 were detained for the entirety of the recall period for which they were present at the interview—these cases were also excluded. A total of 271 respondents (20.01% of the full sample) were excluded based on their detention status.

[5] A logistic regression predicting completion of both follow-up interviews was estimated using study predictors drawn from the intake interview (including a variety index from the intake interview) after imputing the mean value of peer delinquency for respondents missing data on the peer delinquency measure. This model showed that after controlling for the other variables in the model, younger respondents, and respondents with two parents had higher log odds of completing both interviews (b_{age} = -.229, z - -2.06; $b_{parents}$ = .697, z = 1.90). Respondents with two parents were two times as likely (OR = 2.00) as respondents without two parents in the household to complete both follow-ups (on average). Compared to respondents one year older, younger respondents were about 1.35 times as likely to complete both interviews.

[6] Questionnaires for PTD are not publicly available. Skip patterns were identified through examining missing values in questionnaire sections (such as routine activities and neighborhood conditions). Respondents identified by the PTD data as "locked up for the entire recall period" were consistently coded as missing in these sections with a unique code. Locked up variables were created using PTD variables S1HOOD and S2HOOD. Responses of -100 for each variable were coded as locked up for each wave.

Next, respondents spending a small amount of time in the community were identified. Pathways to Desistance data report variables in each follow-up interview (subscript k) quantifying the proportion of time the respondent spent in settings with no community access[7] (notated as p_k). As the goal was to identify respondents with little community exposure, the inverse ($1-p_k$, notated as q_k), which represents the proportion of time the respondent spent in the community, was calculated. However, for respondents who missed an interview but were present for the other, zeros were imputed for all time variables in the interview missed. For each follow-up, the number of valid months covered in the recall period[8] (notated as m_k), which ranged from 2 to 8 in each wave, and 5 to 15 for both waves, was multiplied by the proportion of time in the community to give the number of months spent in the community. Once obtained for each wave, the number of months in the community for follow-up one and two were summed. The number of months in the recall periods for follow-ups one and two were also summed. The final measure of proportion of time spent in the community was equal to the ratio of months in the community to months in the recall period (see Equation 1)[9].

Equation 1: $$q_{follow-ups\ 1\ \&\ 2} = \frac{q_1(m_1)+q_2(m_2)}{m1+m2} = \frac{n\ months\ in\ community}{n\ months\ in\ recall\ periods}$$

Because the effects of neighborhood are contemporaneous, even a short amount of time in the community could theoretically exert effects on offending and on the effect of self-control on offending. Therefore, respondents who reported spending more than

[7] Pathways variables S1PROPTIMESECURESETTINGS & S2PROPTIMESECURESETTINGS
[8] Pathways variables S1NMONTHS & S2NMONTHS
[9] Imputing zeros for time variables where a respondent missed one interview allows for the retention of these cases, without affecting the calculation of time variables. This allows for the examination of the valid data (provided at the interview the respondent completed) while treating the missed interview as unobserved.

one quarter of the total time in the recall periods ($\pi > .25$) or more than two months of the time covered in the recall periods with access to the community were included in the analysis. A total of 181 respondents were excluded based on this criterion (13.37%)[10]. Nonetheless, 854 cases were retained for analysis (63.07% of the full sample); community exposure ranged from 2.75 to 15 months, with an average of 9.20 months. Of these 854 cases, 808 completed both interviews, with an average combined recall period lasting 12 months, and average time spent in the community of 9.4 months. The remaining 46 cases completed only one interview with an average recall period lasting 6.4 months, and average time spent in the community of 5.55 months.

Comparing the 854 respondents retained for the analysis to the respondents who spent little to no time in the community (n = 452) and respondents who failed to complete both follow-up interviews (n= 48) reveals several differences. Primarily, the analysis sample was 82% male, compared to 85% in the missing interview group, and 95% in the limited community exposure group ($\chi^2_{[df\ =\ 2]} = 42.21$, $p < .05$). The racial composition of the analysis sample was 24% white, 36% black, and 36% Hispanic. A disproportionate number of the respondents in the limited community exposure and missed interview groups were black (47% & 52% respectively). Likewise, white (15% & 14%) and Hispanic (29% & 28%) respondents were underrepresented in the limited community

[10] A logistic regression predicting sufficient time spent in the community (of the 1,306 respondents completing at least one interview) was estimated using study predictors drawn from the intake interview (including a variety index from the intake interview) after imputing the mean value of peer delinquency for respondents missing data on the peer delinquency measure. These results showed some statistically significant relationships. Higher intake variety indices, being male, being black, and having lower social capital scores lead to increased odds of falling in the limited community exposure group. These relationships are not surprising considering the increased odds of formal processing at the intersection of these statuses (Leiber & Johnson, 2008; Steffensmeier, Ulmer, & Kramer, 1998). Self-control and neighborhood disadvantage were not significantly related with the log odds of being in the limited community exposure group. A multinomial regression model predicting group (analysis sample, limited community exposure, missed both interviews) found results consistent with the model discussed in this footnote and in footnote 5.

exposure and missed-interview groups ($\chi^2_{[df = 6]} = 47.05$, $p < .05$). Average baseline neighborhood disorder in the retained sample was slightly smaller (less than .1 standard deviations) than the average neighborhood disorder scores for respondents in the limited community exposure group and missing interview group ($F_{[df = 2, 1349]} = 6.69$, $p < .05$). In terms of age, retained respondents were 0.45 years younger on average than the respondents who missed both follow-up interviews, but similar in age to the limited community exposure group ($F_{[df = 2, 1351]} = 4.38$, $p < .05$). Likewise, the groups did not differ in terms of average scores on the Weinberger Adjustment Inventory subscales (Suppression of Aggression- $F_{[df = 2, 1348]} = 0.02$, $p > .05$; Consideration of Others- $F_{[df = 2, 1348]} = 0.31$, $p > .05$; Impulse Control- $F_{[df = 2, 1348]} = 0.55$, $p > .05$) or the Future Outlook Inventory ($F_{[df = 2, 1343]} = 0.64$, $p > .05$). Respondents in the limited community exposure group and the missing both follow-up interviews group reported participating in approximately 1 more type of crime than respondents in the analysis sample ($F_{[df = 2, 1348]} = 12.96$, $p < .05$).

Though the groups excluded were different from the analysis sample due to community exposure, the focus of the research necessitated the exclusion of these cases. Prior studies have also excluded categories of respondents such as youth not living with parents (e.g., Chung & Steinberg, 2006) for theoretical reasons. While the generalizability of these analyses to the full PTD sample is limited, this work is primarily concerned with testing for the presence of the moderating effect of neighborhood disorder, and the precise magnitude of the effect is of secondary interest. Generally, the presence of the moderating effect should manifest in all samples with sufficient variation in the key variables, regardless of representativeness. Should the effect manifest,

inferences about the magnitude of the effect should be applied to populations other than the analysis sample with caution.

A related issue concerns the possibility of sample selection bias introduced by the exclusion of cases with limited community exposure (separate from another selection concern that involves crime prone individuals selecting into disadvantaged neighborhoods). Indeed, these cases were involved in significantly more criminal behavior than the analysis sample. Therefore the outcome of interest in this study, offending, is dependent on the process that determined if individuals were retained for analysis—whether they were in the community. Obviously, when respondents in this sample were not in the community, it was because they were housed in secure facilities in the follow-up period, which was due to involvement in criminal behavior at an earlier time.

However, an understanding of how selection occurs is a separate issue from the extent to which it influences the estimates of the parameters of interest. While one prior study of neighborhood effects using the PTD study has employed the Heckman correction for sample selection bias, utilizing exclusion restrictions (Wright et al., 2014), the degree of selection bias in estimates of the effect of neighborhood disadvantage on crime, let alone the effect of neighborhood disadvantage on the effect of self-control on crime, is unknown. As some research suggests the two-step correction can provide estimates of regression coefficients with larger amounts of bias than uncorrected OLS estimates, and corrected coefficients in models employing polynomial (X, X^2) or product terms can experience extreme instability due to multicollinearity (see Stolzenberg & Relles, 1997), it seems prudent to address the degree of sample selection bias introduced

in selecting cases with a sufficient degree of community exposure. Unfortunately, incarceration for the entire recall period triggered a skip pattern that automatically generated missing data for neighborhood disadvantage, making estimation of this bias difficult.

Measures

Self-Control. The PTD study included a well validated measure of restraint; the Weinberger Adjustment Inventory (Weinberger, 1997). While the full inventory assesses both the subjective experience of distress, and self-restraint, only the self-restraint portion of the WAI is utilized here. The subscales comprising the restraint scale, as conceptualized by Weinberger, provide a close fit to the conceptualization of self-control employed in this study:

> [R]estraint (i.e., self-restraint), encompasses domains related to socialization and self-control and refers to *suppression of egoistic desires in the interest of long-term goals* and relations with others. Thus, restraint is superordinate… to tendencies to inhibit aggressive behavior, to exercise impulse control, to act responsibly, and to be considerate of others. [*emphasis added*] (Weinberger & Schwartz, 1990, p. 382)

It should also be noted there is a close conceptual fit between the elements of self-control (Gottfredson & Hirschi, 1990) and Weinberger's conception of the components of restraint. The WAI restraint scale was originally composed of four dimensions: impulse control, suppression of aggression, consideration of others, and responsibility. The questions wordings are included in Table 2. Question content in the impulse control subscale covers two facets of impulsivity; lack of premeditation ("I say the first thing that comes into my mind without thinking enough about it.") sensation seeking ("I'm the kind of person who will try anything once, even if it's not that safe.").

Questions assessing suppression of aggression are remarkably similar to questions assessing temper from the Grasmick et al. (1993) scale (compare WAI item "If someone does something I really don't like, I yell at them about it" with the Grasmick scale item "When I have a serious disagreement with someone, it's usually hard for me to talk calmly about it without getting upset"). It is important to note though that there are some differences in the suppression of aggression scale as compared to the Grasmick et al. temper subscale. The suppression of aggression scale does address antagonistic and retaliatory portions of aggression ("I pick on people I don't like" & "If someone tries to hurt me, I make sure I get even with them") whereas the temper subscale of the Grasmick et al scale is purely reactive.

The consideration of others subscale of the WAI is somewhat similar to previously used measures of callousness (Meier et al., 2008) and self-centeredness (Evans et al., 1997; Grasmick et al., 1993; Vogel & Messner, 2012). For example, the WAI assesses consideration of others with "I make sure that doing what I want will not cause problems for other people" and the Grasmick et al. scale assesses self-centeredness with "I will try to get the things I want even when I know it's causing problems for other people." Again, there is not perfect agreement in question content, however, the questions tapping each concept are similar.

The responsibility subscale of the WAI presents problems in the context of predicting crime and delinquency in that it assesses behavioral tendencies that overlap with the outcomes of interest. For example, "When I have the chance, I take things I want that don't really belong to me" and "I break laws and rules I don't agree with" both measure participation in illegal behavior. As the purpose of this study is to examine

offending behavior, using the responsibility scale would represent a circular approach. As such, the subscale is excluded in this study.

In terms of the content, based on the conceptualization discussed above, these subscales seem to tap quite well the elements of impulsivity (inclusive of lack of premeditation and risk seeking), volatile temper, and a lack of self-centeredness. The included subscales of the restraint measure were drawn from the initial interview. Researchers from the PTD study examined the subscales for their psychometric properties. Each subscale was examined separately using a unidimensional confirmatory factor model. The fit indices provided by the PTD researchers evidenced adequate fit. For each subscale, Chronbach's alpha was greater than .73, Normed Fit Indices were greater than .95, Nonnormed Fit Indices were greater than .95, and Root Mean Squared Errors of Approximation were less than .07. Based on conventional cutoffs (Bentler & Bonett, 1980; Browne & Cudyeck, 1993) these models provide adequate fit and evidence the reliability of the subscales individually. Each subscale was created by averaging the responses provided respondents answered 5 of 7 questions composing the subscale (or 6 of 8, in the case of the impulse control measure). The raw scores for each subscale ranged from one to five, however, the analysis relied on standardized scores.

The Future Outlook Inventory (FOI) was also included as a measure of consideration of future consequences in the PTD questionnaire. The FOI examines overlapping facets of impulsivity such as persistence or dedication to boring tasks, and an orientation towards more distal goals. The FOI taps different content than the impulse control subscale of the WAI. While the impulse control subscale examines thinking before speaking and acting, as well as enjoying or participating in risky or thrilling

behaviors, those questions require respondents to think about the instantaneous outcomes of behaviors. For example, thinking before speaking has immediate consequences. In contrast, the FOI questions ask respondents to think about their behavior in reference to more distal consequences. This scale taps content consistent with Hirschi's redefinition of self-control as the tendency to consider the long term consequences of one's actions (2004). Respondents rate the degree to which they adhere to more distal, abstract goals in spite of immediate temptations and therefore the consequences the respondents consider in the FOI questions are very different from the ones they consider in the impulse control subscale. In combining the FOI with the subscales of the WAI, greater coverage of the concept of self-control is achieved.

The FOI measure includes eight questions rated by respondents on a 4-point scale (Never true-always true). The PTD researchers assessed the psychometric quality of the full measure using a unidimensional confirmatory factor model. Although the model required error correlations it was determined that the scale was reliable (NFI = .96, NNFI = .96, RMSEA = .03). The scores for each question were averaged into a single index provided respondents answered 6 of 8 questions. The FOI score was standardized for the analysis.

Figure 2 locates the measures used in this study (rectangles with bold-italicized text) within the conceptual diagram displayed in Figure 1. Several advantages of this study's measures should be noted. Primarily, the goal of covering content from each element was met—a rectangle overlaps with a portion of each of the large circles. Second, the measures employed in this study are relatively comparable to previous measures—rectangles encompass at least a portion of several hexagons. The Future

Outlook Inventory is similar to impulsivity scales used by Vazsonyi et al. (2006) and Zimmerman (2010). The Impulse Control subscale is similar to impulsivity measures used by Lynam et al. (2000), Meier et al. (2008) and Jones and Lynam (2009). Third, temper has not been examined specifically with regards to the interaction of self-control and neighborhood disadvantage (as indicated by the absence of a hexagon within the temper circle), although Zimmerman et al. (2015) included temper in the full Grasmick et al. scale. This study employs a measure that taps content inclusive in the element of temper in the suppression of aggression subscale. The standardized scores of each subscale of the WAI and the FOI were summed to form a total self-control scale. The FOI was incomplete for two cases. The self-control score was calculated as the average of the three WAI subscales for these two cases. The full measure was standardized for the analysis.

Neighborhood Disadvantage. Neighborhood disadvantage was measured using a measure of perceived neighborhood conditions[11] (Sampson & Raudenbush, 1999). This variable was generated using the respondent measures data files of the PTD study for follow-up interviews one and two because the effects of neighborhood conditions were hypothesized to be contemporaneous. Respondents were asked to report on a four-point scale (none of the time to all of the time) the extent to which the neighborhood experienced several social and physical ills (cigarettes on the streets or in the gutters, graffiti or tags, adults fighting of arguing loudly, people using needles or syringes to take drugs). The total scales at follow-up one and two had high internal consistency ($\alpha = .96$). The scale was computed by PTD researchers as the average of 21 items, provided the

[11] PTD Variables S1HOOD & S2HOOD

respondents answered 16 of the 21 questions. The PTD scale ranged from one to four. The neighborhood measure used here combined the values for follow-up interviews one and two. It is possible that respondents have missing values for the neighborhood disorder variable because they were detained for the entirety of one recall period, but lived in the community for the other. It is also possible for respondents to have different disorder scores because they moved from one neighborhood to another, or the conditions in their neighborhood changed over time. For the most part, the neighborhood disorder scores were consistent from follow-up interview one to follow-up interview two. Of the 661 respondents who provided valid data in both follow-ups, 84.72% of scores from follow-up interview two were within one standard deviation of the score from follow-up interview one. The correlation between waves was .72.

The follow-up interviews occurred approximately 6 and 12 months following the initial interview. The amount of time that passed between the initial interview and follow-up 1 ranged from 4 to 8 months and the amount of time that passed between follow-up 1 and follow-up 2 ranged from 3 to 8 months. In short, the amount of time during the two recall periods were unbalanced between follow-up interviews. In order to more accurately construct the disadvantage variable in the follow-up period for analysis, raw values from the follow-up interviews were weighted before being combined. The weight variable used was time spent in the community (numerator of Equation 1). Consider an extreme case where a youth was interviewed 4 months after the initial interview, then interviewed again 8 months later. The values provided in the first follow-up pertain to one-third of the time spent in the community for the respondent, and the values in the second period pertain to two-thirds of the time. Simply averaging the values provided in both waves

would more heavily weight the final combined score by the values provided in the shorter time period. Rather than averaging, the raw scores from follow-ups 1 and 2 were weighted by time spent in the community before combining. In order to return the combined scores to the original metric, the total weighted score from follow-ups 1 and 2 is divided by the time spent in the community. This more accurately represents the values of the variables for respondents whose time in the community was not balanced between waves.

For respondent's providing valid data at both follow-ups, a weighted average was computed by multiplying the neighborhood disorder score for each follow-up (h_k) by the time spent in the community in each follow-up ($q_k[m_k]$ from Equation 1). The sum of these products for each interview was divided by the sum of time spent in the community (numerator from Equation 1). The computation (Equation 2) resulted in a scale ranging from one to four.

Equation 2: $$\frac{h_1(q_1 \times m_1)+h_2(q_2 \times m_2)}{q_1(m_1)+q_2(m_2)}$$

For respondents that did not provide valid data in one follow-up, the score from the follow-up interview with valid data was used. After weighting and combining the data from waves one and two, the score was standardized for the analysis.

Crime and Delinquency. The PTD study collected self-report crime and delinquency information based on the scale developed in the National Youth Survey (Elliott, 1990). Respondents were asked about 24 different illegal behaviors, most of

which were used in this analysis[12]. The offenses used here are listed in Table 4. The question wordings and response options were identical across waves.

The PTD questionnaire asked respondents whether they had participated in several behaviors during the recall period. Respondents answering affirmatively were asked several follow up questions, including the number of times the respondents participated in each behavior. Relatively speaking, some offenses were very rare (arson & carjacking), and some offenses more frequent (stealing & fighting). The offending measure used in this study is a versatility or variety index of offending (Hindelang et al., 1981), which asks whether respondents had or had not participated in a set of behaviors during a given recall period and subsequently adds affirmative responses (herein referred to as a variety index). For the current study, the variety index was computed using the following steps. First, the questions asking respondents whether they had participated in each criminal act were used to create dichotomous yes/no variables for all offenses in follow-ups one and two. Each offense was coded as yes if the respondent provided an affirmative response in follow-up one or two. Then, only if the respondent said no in both waves was the offense coded as "no" for not participating in either wave. This means respondents with missing data in one follow-up and did not provide an affirmative response in the other were coded as missing because there was not enough information to determine if they had or had not participated in the act. This affected three values in total. The variety index was then created by summing the dichotomous items, so long as there were non-missing values for 17 of 20 offenses. This procedure removed 4 cases due to

[12] Rape and murder were not available in the PTD public use data files for confidentiality reasons. Paying for sex was extremely rare, tended to be uncorrelated with the remaining offenses, and was excluded. Shooting at someone was included and shooting at someone (bullet hit) was excluded.

missing data[13]. The variety index (n=850) ranged from zero to 20, with a mean of 3.06, standard deviation of 3.89, and skewness of 1.71. Approximately 30% of respondents reported not participating in any of the 20 criminal behaviors. Additionally, the KR20 coefficient of reliability was equal to 0.894. The reliability of the summative index was acceptable. Descriptive statistics for the variety index are included in Table 6. The variety index shows that more than half the sample perpetrated at least one type of crime in the follow-up, and the average number of different crimes was about 3, indicating a severe positive skew.

Control Variables. In order to account for competing explanations of crime, several control variables were used in the first stage of this analysis. The demographic control variables used here are age (as a continuous variable), race, and gender (See Table 5 for full sample descriptive statistics). The number of months respondents spend in the community controlled as well (street time). Theoretically important control variables include social capital, unsupervised routine activities, peer delinquency, moral disengagement, and family structure. Social capital is assessed using five questions pertaining to respondents' connectedness within their communities (Nagin & Paternoster, 1994). The measure indicates the extent to which (1) respondents' parents know the parents of other youth in their neighborhood, (1) respondents' parents know the names of the respondents' teachers, and (3) respondents' perceive opportunities to work in their neighborhood. Greater scores show greater levels of integration and capital. Unsupervised routine activities were assessed using the scale developed by Osgood,

[13] These 4 cases were dropped from the analysis due to a programming error that caused all self-report offending questions to be skipped during the interview.

Wilson, O'Malley, Bachman, and Johnston (1996). The scale is comprised of four questions gauging the frequency of the youth's participation in parties, social gatherings and other recreational activities. The scale focuses on socializing with peers in an unsupervised context. The routine activity variables were drawn from follow-ups one and two in order to permit their contemporaneous effect. A weighted average of the routine activity variables from follow-ups one and two was computed using the same procedure for computing perceived disorder scores. Peer delinquency was assessed using perceptions of peers' behavior provided by the respondent. The scale captures the how many of the respondents' friends (1-none of them to 5-all of them) engage in 12 different illegal behaviors. Higher scores indicate greater levels of peer delinquency. Family structure was assessed using data from the initial interview, and simply measured whether the respondent lived with two parents. A dummy variable was created where 1 indicates two-parent household, and 0 indicates all other family structures. Descriptive statistics for the control variables are presented in Table 6. The continuous variables were standardized for the analysis. Of the 854 cases in the analysis sample, 22 cases were missing data for at least one variable in the analysis. Cases missing self-report offending data were deleted from the analysis (n = 4) as was one case missing data for 5 variables. The remaining cases (n= 17) were missing data on the peer delinquency measure and were also excluded, leaving 832 cases for analysis.

Analytical Approach

A common approach to modelling statistical interactions is to multiply the independent variables involved in the interaction and include the product as an additional variable in the analysis (Jaccard & Turrisi, 2003). In using this product-term approach to

model crime in OLS analyses however, one would expect that statistically significant interaction effects manifest due to the distribution of the response variable, and not necessarily for theoretical reasons. Vasquez (2010) discusses the consequences of applying the OLS model to censored outcome measures (i.e., a substantial proportion of cases lie at the boundary of observed values) in the context of statistical complexities. Briefly, two groups of cases exist—cases where the value of crime is unobserved (crime = 0), and cases where the value of crime is observed (crime > 0). For the first group, the observed outcome variable is a constant, and therefore does not covary and is thus, unrelated to all predictors. For the second group, predictors are related to crime as a function of the slope estimates; the relationship between an independent variable and the dependent variable depends on whether cases are censored (Vasquez, 2010, p. 956). In other words, the effect of an independent variable is conditional in the first place. Therefore, several authors have cautioned against interpreting statistically significant product term coefficients as substantively meaningful when estimated using OLS and censored data (Osgood, et al., 2002; Ousey & Wilcox, 2007; Vasquez, 2010). Formally, the utilization of an OLS model will violate the assumption that the regression function characterizes the entire distribution of the outcome variable. In models excluding interactive and curvilinear relationships, however, OLS models are robust to this violation (Osgood, et al., 2002, p. 320). As such, OLS models not involving statistical complexities could provide acceptable estimates of the slopes and accurate conclusions of statistical significance (Loftin & McDowall, 1981; Vasquez, 2010). Therefore, the analysis proceeds in two phases.

Group Analyses. The first phase involves an analysis of discrete groups of the full PTD sample. The central question to be addressed in the first analysis is "to what degree does the slope of self-control vary across groups?" Using a split sample approach allows the slope of self-control to be estimated while capitalizing on the robustness of linear models to violations of assumptions when the model contains only additive effects. Recall the measure of neighborhood disadvantage used in this study is a continuous variable that ranges from one to four. This variable indicates the extent to which respondents perceived several physical and social incivilities as problematic—here, larger scores indicate greater disadvantage. Using the neighborhood disadvantage measure, discrete groups were constructed to represent low, medium and high levels of disadvantage. The distribution is relatively platykurtic, with most respondents clustering between the scores of one and three. For the purposes of the split sample analyses, quartiles are used to assign cases to groups. The lower 25% of the distribution defines the low disadvantage group, the upper 25% defines the high disadvantage group, and the middle 50% defines the medium disadvantage group. This approach is common for binning continuous disadvantage scores into discrete groups for the purposes of examining the self-control X neighborhood disadvantage interaction (Lynam et al, 2000; Wikstrom & Loeber, 2000).

Several preliminary analyses will be presented; (1) scatter plots summarizing the relationship between self-control and offending for each group; (2) correlation coefficients describing the bivariate relationship between self-control and offending measures for each group, and (3) regression coefficients for the effect of self-control on offending measures, holding constant the potentially confounding effects of other

variables for each group. The expected relationship between self-control and crime is negative—increases in self-control will reduce the amount of crime, on average; however, the slope is expected to vary across groups such that the effect is largest in the medium disadvantage group.

Full Sample Analysis. The second phase of the analysis involves testing the hypotheses using product-terms to model the statistical interaction while using the full sample and employing a more appropriate statistical model. The evidence from the preliminary analyses will guide the selection of product-terms to be modelled in regression analyses. A linear moderation effect, where the effect of the focal variable is conditional on the level of the moderating variable will indicate the effect of a one-unit increase in the moderator variable on the magnitude of the coefficient for the focal variable. Importantly, the magnitude of this moderation effect is not conditional on the level of the moderator variable. This situation will be modelled using a single product term where the focal variable is multiplied by the moderating variable, and the resulting product term is included as a predictor in the regression model (Equation 3).

Equation 3: $Crime = \beta_0 + \beta_1(SC) + \beta_2(ND) + \beta_3(SCXND) + \cdots + \beta_k X_k + e$

The effect of self-control in the jth neighborhood is equal to the main effect (β_1), plus the coefficient of the product term (β_3) times the value of neighborhood disadvantage in the jth neighborhood (Equation 4).

Equation 4: $\beta_{self-control\ j} = \beta_1 + \beta_3(ND_j)$

If evidence that the moderation effect is curvilinear emerges from the group analyses, additional coefficients are required to permit the maximum effect of self-control

to be in the middle of the disadvantage distribution. The revised trait suppression hypothesis suggests that changes in neighborhood disadvantage do not uniformly change the effect of self-control. Increases in neighborhood disadvantage from low to medium disadvantage increase the absolute magnitude of the effect of self-control—increases from medium to high disadvantage decrease the absolute magnitude of the effect of self-control. The effect of neighborhood disadvantage on the effect of self-control is conditional on the level of neighborhood disadvantage. This is conceptually equivalent to a three-way interaction, where neighborhood disadvantage is interacting with itself and self-control.

In order to model this situation, additional variables are necessary. In addition to the product term generated by multiplying self-control and neighborhood disadvantage [product term 1], a squared neighborhood disadvantage variable, and an additional product term where the squared neighborhood disadvantage variable is multiplied by self-control [product term 2] will be added to the regression model (Equation 5) (see Baron & Kenny, 1986, pp. 1175-1176).

Equation 5: $$Crime = \beta_0 + \beta_1(SC) + \beta_2(ND) + \beta_3(SCXND) + \beta_4(ND^2) + \beta_5(SCXND^2) + \cdots + \beta_k(X_k) + e$$

The effect of self-control for the *j*th respondent becomes the main effect of self-control (β_1) plus the coefficient of product term 1 (β_3) times the value of neighborhood disadvantage in the *j*th respondent's neighborhood plus the coefficient of product term 2 (β_5) times the squared value of neighborhood disadvantage in the *j*th respondent's neighborhood (Equation 6).

Equation 6: $\beta_{self-control\ j} = \beta_1 + \beta_3(ND_j) + \beta_5(ND_j^2)$

The coefficients added to the model will be interpreted in the following fashion. In the case of the most complex model (the curvilinear moderation effect), the main effect of self-control is conditional on average neighborhood disadvantage. A unit increase in self-control in the average neighborhood will correspond to a β_1 (the main effect slope) unit decrease in crime, on average and while holding other variables constant. The coefficient for product term 1 (β_3) represents the expected change in the effect of self-control in the average neighborhood given an infinitesimally small increase in neighborhood disadvantage while controlling for the other variables. Product term two (β_5) represents the effect of neighborhood disadvantage on the magnitude of product term one. A unit increase, or a unit decrease in neighborhood disadvantage (because of the squared term) will change the effect of neighborhood disadvantage on the effect of self-control crime by β_5, on average and while controlling for the other variables in the model.

According to the revised trait suppression hypothesis, the expected signs for the coefficients are negative for the main effect of self-control, zero for product term 1, and positive for product term 2. A positive coefficient for product term two indicates the effect of low self-control is dampened as neighborhood disadvantage increases or decreases from average. The directions of the slopes are explicitly anticipated by the theory, and therefore specified *a priori*. As such, one-tailed significance tests are employed for these coefficients. Because of the conditional nature of the coefficients, simple slopes will be calculated to demonstrate variability in the slopes. The level effect of low self-control in the jth neighborhood will be estimated at five levels: the 10^{th}, 30^{th}, 50^{th}, 70^{th}, and 90^{th} percentile of neighborhood disadvantage scores. This permits the

examination of the effect across a wide range of neighborhoods while ensuring the slope estimates are not based on a very small number of observations (i.e., the slopes will be larger than at least 10% of the cases). The function that predicts the magnitude of the slope of self-control (Equation 6) will also be plotted to demonstrate the variability in the slope and the extent of the curvature.

Observed product term coefficients, their tests of statistical significance, and increase in R^2 values that are used in field studies to quantify moderation effects using the product-term approach in multiple regression have been found to be conservative compared to those generated in studies employing ideal experimental designs (McClelland & Judd, 1993). In short, factorial experimental designs may employ optimal distributions of the factors by creating large differences between levels of the factors. Thus, extreme values of each factor will always occur jointly—this design capitalizes on mechanical advantage and increased statistical power. Compared to experimental studies, field studies have a more difficult time substantiating moderation effects because observational data often present non-optimal distributions of the variables of interest. In some cases, moderation is not possible (where the conditional distribution of X does not vary for every value of Z and vice versa), however in some cases the design simply leads to a lack of efficiency and larger standard errors (McClelland & Judd, 1993). These issues are exacerbated upon the entry of higher order interactions such as the one hypothesized in this study, suggesting observational studies may have extreme difficulty detecting curvilinear moderation effects without massive sample sizes (i.e., n > 3,000-5,000) (McClelland & Judd, 1993, p. 385).

Statistical Models

OLS models assume the outcome variable is continuous. This implies the dependent variable has no upper or lower bound and may be infinitely subdivided. The distribution of the variety index here is a poor fit to the OLS model because (1) its scores have a lower bound of zero, and (2) infinitely dividing the number of different types of crime one commits provides nonsensical values (e.g., 1.5 different types of crime). As a result, estimates from OLS may be biased and inefficient unless the mean is sufficiently large (Cameron & Trivedi, 1998, p. 2; Long, 1997, p. 217)[14]. There appear two plausible alternatives for modelling a variety index of crime (1) the Tobit regression model (Tobin, 1958) and the Poisson family of regression models.

The Tobit model accounts for censoring by modelling the two domains of the outcome distribution separately. The process of crossing the censoring threshold is modeled with probit regression, and differences among individuals having crossed the censoring threshold are modeled with the standard linear model (Tobin, 1958). Strictly speaking, Tobit regression is problematic in this context as is also intended for continuous variables (i.e., household expenditures, Tobin, 1958, p. 25)—but the Tobit model could be appropriate where the log of a variety index is analyzed, as the observed variable would then take on fractional values. Though logging the variety index would not permit the observed variable to take on all characteristics of a continuous variable because it would still have a lower bound, Tobit regression accounts for the remaining complication by mixing probit and linear models. The Tobit model can be an

[14] For all OLS models presented in the split sample analysis, the residuals were positively skewed (the models routinely failed to predict cases with large variety index values) and the errors were severely heteroscedastic. Log transformation of the variety index improved the distribution of the residuals, but the errors remained heteroscedastic.

improvement over OLS when its assumptions are met (Vasquez, 2010). These include homogeneity of error variance, and normality of the underlying latent variable, but the model also assumes the effects of the independent variables are identical above and below the censoring threshold (Long, 1997, p. 206). Estimates provided by Tobit can be equally biased or worse than OLS estimates where its assumptions are not met (Ousey & Wilcox, 2007).

The Poisson family of regression models were designed explicitly to take into account the discrete and limited nature of counts—observed variables that only take on the values of zero and positive integers. The variety index is not a count of the number of offenses committed, however, it is discrete in nature and takes on only counting numbers (i.e.,[1,2,3,4…]). Though conceptually the variety index is not a perfect match to the Poisson process, the conceptual match of the variety index to the Poisson model is much clearer than for the OLS or the Tobit models. The variety index is distributed very similarly to variables where Poisson regression is frequently used. Osgood, et al. (2002, p. 326) also recognize that the variety index is similar to event counts and therefore count models could be an effective alternative to OLS, although this has not been empirically examined. While multiple recent studies have employed count models to explain variety indices (Sweeten, Pyrooz, & Piquero, 2013; Vogel, 2016; Vogel & South, 2016), the Poisson model's appropriateness as a robust alternative to OLS where empirical complexities are present has not been determined.

Comparing the alternatives, Tobit regression makes more assumptions that prove to be problematic when examining variety indices than do the models in the Poisson family. Though the adequacy of count models in explaining variety indices of crime has

yet to be determined, the count model is more defensible conceptually. Furthermore, the Poisson model would be preferred to the Tobit model and the OLS model if those models' assumptions are violated. To examine the robustness of the OLS estimates, the split sample models are replicated using negative binomial regression due to overdispersion (Long, 1997, p. 230). As the recall period length varies within the sample from 8-15 months with an average of 12 months, the respondents' differential opportunity is adjusted using the recall period length in months as an exposure variable in all negative binomial models. The exposure option was used in Stata and the recall period length variable was therefore not logged.

VI. RESULTS

Disadvantage Group Comparisons

Table 7 presents comparisons of disadvantage groups for each variable in the analysis. The full sample is primarily black (35%) and Hispanic (37%), though a quarter of respondents were white, and 4% of respondents identified as another race or ethnicity. Examining the marginal distributions in each row, it is evident the racial and ethnic composition varies across neighborhood disadvantage groups ($\chi^2_{[df = 6]} = 107.534$, $p < 0.05$, Cramer's $V = 0.254$). In the low disadvantage group, 42% of respondents are white, whereas in medium and high disadvantage groups only 21% and 11% of respondents are white, respectively. In the high disadvantage group, 57% of respondents are black; however only 14% and 35% of respondents in low and medium disadvantage groups are black, respectively. The high disadvantage group has a disproportionately large number of black respondents and the low disadvantage group has a disproportionately large number of white respondents. That black respondents are over-represented within high disadvantage areas is consistent with prior research (e.g., Lynam et al., 2000; Wilson, 1987).

Moving to gender, the oversampling of female offenders manifested proportionately more female respondents in low disadvantage neighborhoods and proportionately fewer female respondents in high disadvantage neighborhoods. Nonetheless, gender and disadvantage group were unrelated and approximately 80%-85% of each subsample was male ($\chi^2 = 2.079$, $p > 0.10$). Likewise, disadvantage group was unrelated to age—the average age was about 16 in each group ($F = 1.39$, $p > .10$). However, youth in the high disadvantage group are much less likely to have two parents

in the household (31%) than youth in medium (36%) and low (48%) disadvantage groups ($\chi^2 = 14.26$, $p < .05$, Cramer's V = 0.13).

Examining indicators of criminal offending, 63%, 71% and 75% of respondents in low, medium and high disadvantage groups, respectively, participated in at least one crime during the follow-up period ($\chi^2 = 7.514$, $p < 0.05$). Histograms describing the distribution of the variety index for each group are provided in Figure 3. The mean level of the variety index (number of crime types) is 2.26, 3.35 and 3.33 in low, medium and high disadvantage groups, respectively ($F = 6.08$, $p < .05$). The youth living in medium and high disadvantage neighborhoods were involved in about one more crime type than youth living in low disadvantage neighborhoods, on average. Likewise, the median number of crime types for low, medium and high disadvantage groups is 1, 2, and 1.5, respectively. In this particular sample, however, disadvantage level, as measured by groups, is not a strong predictor of offending ($R^2 = .01$), and the effects appear to level off after increasing from low to medium disadvantage. These differences, however, do not account for differential opportunity as the average length of the recall period was not constant across groups, nor does it account for differences in time spent outside of detainment, which also varied by group.

Mean levels of self-control are nearly identical across groups ($F = 0.33$, $p > .10$). Furthermore, Figure 4 shows the groups' distributions are similar and, within each group, approximately normal. Likewise, the degree of unsupervised activity is constant across groups ($F = 0.12$, $p > .10$). The moral disengagement index was related to neighborhood disadvantage level ($F = 3.08$, $p < .05$); however differences are substantively small—the high disadvantage group mean was 0.24 ($t = 2.48$) standard deviations higher than the

low disadvantage group mean, which was indistinguishable from the medium disadvantage group mean (t = 1.39). Disadvantage level is not a strong predictor of moral disengagement in this sample (R^2<.01). Similarly, there are statistically significant differences in peer delinquency between neighborhood disadvantage groups (F = 12.93, p < .05, R^2 = .03). Compared to the low disadvantage group, the medium and high disadvantage group means are higher by 0.18 (t = 2.14) and 0.48 (t = 5.00) standard deviations, respectively. Finally, social capital was related to level of neighborhood disadvantage (F = 7.65, p < .05). The medium and high disadvantage group means were 0.20 (t = 2.35) and 0.38 (t = 3.91) standard deviations larger than the low disadvantage group mean for social capital, respectively.

The predictors discussed above were categorized in order to describe their joint distributions with respect to neighborhood disadvantage. Dummy variables for self-control, moral disengagement, social capital, unsupervised routine activities, peer delinquency and two-parent households were constructed to indicate whether a case had a value indicative of risk (1 for high risk, 0 for low risk). Risky values were defined as greater than one standard deviation above average on moral disengagement, peer delinquency, and unsupervised routine activities, and less than one standard deviation below average on social capital, self-control, and respondents without two parents in the household. Table 8 displays the cross-classification of number of risk factors with neighborhood disadvantage group, as well as the conditional distribution that describes the percent of cases within each level of disadvantage with *k* risk factors. For example, 21.15% of respondents in the low disadvantage group possess zero risk factors.

Examining the variables jointly, none of the respondents possessed all six risk factors, yet there was some clustering of high risk cases (multiple risk factor simultaneously) in the high disadvantage neighborhood group. Table 8 shows that high-risk respondents are clustered in high disadvantage neighborhoods and low-risk respondents are somewhat clustered in low disadvantage neighborhoods. For a given risk level, clustering is evident when the percent of cases in one disadvantage group is greater than the percentages for other levels of disadvantage. About 17.78% of respondents in the high disadvantage group possessed three or more risk factors, as did 15.63% of respondents in the medium disadvantage group, and 10.57% of respondents in the low disadvantage group. Likewise, a smaller percentage of respondents in the medium (16.59%) and high (11.06%) disadvantage groups possess zero risk factors than respondents in the low disadvantage group (21.15%). While this pattern is evident at the highest and lowest levels of risk, neighborhood disadvantage group was unrelated to the number of risk factors in this sample ($\chi^2 = 10.571$, $p > .10$). When examining risk factors as a continuous variable, however, the average number of risk factors was related to neighborhood disadvantage ($F = 4.04$, $p < .10$). The only significant group difference was between the low and high disadvantage groups, where the mean number of risk factors was 1.293 and 1.591, respectively ($t = 2.84$). Similar to the prior comparisons, disadvantage was a weak predictor of risk level ($R^2 = .01$).

In summary, the distributions of several variables are related to neighborhood disadvantage in this sample. Foremost, neighborhood disadvantage is positively related to offending, but the association is weak. Peer delinquency and moral disengagement are positively related to neighborhood disadvantage, and social capital is negatively related.

Some variables in this analysis, however, are unrelated to neighborhood disadvantage (i.e., self-control and unsupervised routine activities). The reason unsupervised routine activities as well as self-control were unrelated to neighborhood disadvantage is unclear. Sample selection bias introduced by examining a sample composed only of offenders, however, could influence these relationships. For instance, though neighborhood disadvantage and disorganization is associated with parenting practices, such as consistency and punitiveness (Kohen et al., 2008), which are moderately related to self-control (Gottfredson & Hirschi, 1990; Hay, 2001), family instability is also a factor that contributes to a higher likelihood of formal processing. In a formally processed sample, one would expect a high proportion of youth to experience family instability, regardless of the characteristics of the community. Though some variables were related (weakly) to neighborhood disadvantage, level of disadvantage was a weak predictor of a respondent's overall number of risk factors. This is again consistent with the idea that the process of selecting juveniles for formal processing would attenuate the observed relationship between individual risk and neighborhood risk.

Split-Sample Analysis

Having described the respondents within each group, the analysis proceeds to an estimation of the effects of self-control on crime within each group. Readers are reminded there are several differences between the samples aside from the level of neighborhood disadvantage. Nonetheless, the bivariate relationship is described first. Figure 5 displays scatterplots of the respondents' self-control and variety index scores by level of neighborhood disadvantage. Negative relationships are observed for each level of disadvantage. At each level, the range of the variety index is approximately equal and

there is a high degree of clustering around 0-2 crimes. For the low disadvantage group, the scatterplot shows a negative relationship, but the slope is relatively less steep than for the medium and high neighborhood disadvantage groups. The medium and high disadvantage groups appear to have equal slopes. Table 9 displays the Pearson product moment correlation coefficients, and bivariate ordinary least squares & negative binomial metric slope estimates of the effects of self-control on the variety index within each neighborhood disadvantage group. Accompanying the measures of association are estimates of the means and standard deviations for self-control and crime. Notably, the variability in self-control is similar across groups. This suggests differences across groups in the effects of self-control may not be substantially impacted by differences in the variability of self-control.

Examining the pattern of correlation coefficients, the strongest association between self-control and crime is within the medium disadvantage group ($r = -0.429$)—the association is slightly weaker in the low ($r = -0.259$) and high ($r = -0.388$) disadvantage groups. This pattern is replicated in the bivariate OLS models. The slope estimates in Table 9 are x-standardized, as self-control scores are entered in the models as z-scores (see Long, 1997, p. 37). For a standard deviation increase in self-control, the expected number of crime types decreases by about 0.842 for the low disadvantage group, 1.695 for the medium disadvantage group, and 1.674 for the high disadvantage group, on average. This study hypothesizes the largest (least negative) effect in medium disadvantage neighborhoods. The point estimates are in the pattern predicted by the revised trait suppression hypotheses as the largest effect is within the medium disadvantage group. Formally, the difference in the slope between medium and low

disadvantage groups, as well as the difference between medium and high disadvantage groups, is hypothesized to be negative. The difference between the slope in medium disadvantage neighborhoods (b_{med}) may be compared to the slopes in low or high disadvantage neighborhoods (b_k) with a z-test using the following equation,

Equation 7: $z_{b_{med}-b_k} = \frac{b_{med}-b_k}{\sqrt{SE^2_{b_{med}}+SE^2_{b_k}}}$

which is discussed in Paternoster, Brame, Mazzerole, and Piquero (1998). Turning to Panel B in Table 9, for the OLS slope estimates, the difference in the slopes between medium and low disadvantage neighborhoods is statistically significant and negative, indicating stronger effects in medium disadvantage neighborhoods (z = -3.043). However, the difference in the slopes between medium and high disadvantage neighborhoods is not statistically significant (z = -0.063), which is inconsistent with the revised trait suppression hypothesis.

Examining the negative binomial coefficients in the far right column of Panel A in Table 9, a standard deviation increase in self-control reduces the log rate of the number of crime types by 0.353 in the low disadvantage group. Conversion of the slopes to incidence rate ratios reveals a standard deviation increase in self-control reduces the number of different crime types by $(100\% \times (\exp[b] - 1))$ 30% in the low disadvantage group, 43% in the medium disadvantage group, and 42% in the high disadvantage group. The effect of self-control in the low disadvantage group is 0.217 units smaller than the effect in the medium disadvantage group (z = -1.910), a statistically significant difference (one-tailed p value < .05). The effect of self-control in the high disadvantage group is .029 units smaller than the effect in the medium disadvantage

group (z = -0.265). While the pattern of the point estimates is consistent with the correlations, only the low and medium disadvantage groups' negative binomial coefficients are statistically distinguishable from one another, suggesting the slope is equal within medium and high neighborhood disadvantage groups.

As there were differences between the disadvantage groups in addition to the level of neighborhood disorder, the next set of estimates attempt to hold these differences constant by including control variables. For continuity, OLS (Table 10) and negative binomial (Table 11) models are presented. Variance inflation factor estimates indicate multicollinearity is not problematic for any model. The multivariate OLS estimates show the slope of self-control is attenuated due to the introduction of control variables and the degree of attenuation varies by level of neighborhood disadvantage. The slope in the low disadvantage group changes from -0.842 to -0.341 (60% reduction), and is only statistically significant in the model without control variables ($p < .05$). The slope in the medium disadvantage group changes from -1.695 to -0.855 (50% reduction), and the slope in both models is statistically significant. Finally, in the high disadvantage group, the effect of self-control changes from -1.674 to -0.686 (59% reduction), and the slope in both models is statistically significant. Now holding control variables constant, a standard deviation increase in self-control corresponds to an average decrease of 0.341, 0.855 and 0.686 crime types in the low, medium and high disadvantage groups, respectively. The effect of self-control in the low disadvantage group however, is not statistically distinguishable from zero.

Examining the negative binomial slope estimates reveals similar findings. The slope of self-control is reduced from to -0.352 to -0.213 (39% reduction) in the low

disadvantage group, -0.570 to -0.286 (50% reduction) in the medium disadvantage group, and -0.541 to -0.133 (75% reduction) in the high disadvantage group. A standard deviation increase in self-control reduces the count of crime types by 19%, 25%, and 12% on average in the low, medium and high disadvantage groups, respectively. However, while controlling for the other variables in the model, the effect of self-control in the high disadvantage group is not statistically significant. In the OLS model, the partial effect of self-control was reduced nearly to zero in the low disadvantage group, while the same reduction was observed in the high disadvantage group in the negative binomial models. Backwards deletion was employed to investigate possible multicollinearity. The slopes were re-estimated after removing one independent variable at a time from the high disadvantage group negative binomial model[15]. The z-statistic for the effect of self-control became significant only upon deletion of the unsupervised routine activities variable from the model (b = -0.278, SE = .099, z = -2.80). The correlation between unsupervised routine activities and self-control is not large enough on its own to warrant concerns about collinearity (r = -0.450), nor were the variance inflation factor estimates (self-control = 1.51, unsupervised routine activities = 1.12). Without the routine activities variable in the high neighborhood disadvantage group model, the partial effect of self-control is still smaller than the effect of self-control in models estimating the same effect excluding routine activities in the medium disadvantage group (b = -0.288, SE = 0.066, z = -4.33), though the difference is not statistically significant (p > .05).

[15] Backwards deletion results not displayed.

Table 12 summarizes the slope estimates of self-control from models employing control variables in Panel A. Turning to the tests of equality of slopes in Panel B, the differences in the slopes are in the expected direction. The effect of self-control in low and high disadvantage neighborhoods is smaller than the effect of self-control in medium disadvantage neighborhoods. In the OLS estimates, the slope for the effect self-control in low and high disadvantage neighborhoods is smaller in absolute magnitude than the slope in medium disadvantage neighborhoods by 0.514 and 0.169 units, respectively. In the negative binomial models, the slopes in low and high disadvantage neighborhoods are smaller in absolute magnitude than the slope in medium disadvantage neighborhoods by 0.073 and 0.153 units, respectively. None of the differences are statistically significant.

While the slopes from the OLS and negative binomial models are not directly comparable, the relative magnitudes of the slopes can be compared to determine the extent to which the models provide consistent results. The magnitude of the effect of self-control in the low disadvantage group is 0.40 and 0.74 times the magnitude of the effect in the medium disadvantage group (b_{low}/b_{med}) for the OLS and negative binomial models, respectively. On its face, whether the slope for self-control varies substantially between the medium and low disadvantage neighborhoods is unclear. According to the negative binomial model, the slope for low disadvantage is about 25% smaller than the slope for medium disadvantage, but 60% smaller according to the OLS estimates. The conservative estimate provided by the negative binomial model (25% smaller) is divergent from the estimate provided by the OLS model. Given bias in the OLS estimates due to the distribution of the variety index, the negative binomial estimates are more trustworthy.

On the other hand, the effect of self-control in the high disadvantage group is 0.80 and 0.47 times the effect in the medium disadvantage group (b_{high}/b_{med}) for the OLS and negative binomial models, respectively. Again, the negative binomial estimate of the relative magnitude of the effect is divergent from the OLS estimate. The effect in the high disadvantage group is about 53% smaller than the medium disadvantage group according to the negative binomial estimates, but only 18% smaller according to the OLS estimates. Again, the negative binomial model is more trustworthy, yet the estimate is relatively larger than the estimate provided by OLS. The larger difference between neighborhood groups belongs to different pairs (low-med, med-high) across statistical models. However, as the differences are not statistically distinguishable from zero, one would conclude the effect of self-control is invariant across groups.

Taken together, the split sample analyses suggest two possibilities. Interpreting the results strictly—the effect of self-control is independent of neighborhood disadvantage, as the tests of statistical significance failed to consistently find differences in the slope across levels of neighborhood disadvantage. As these tests are very conservative and the estimates consistently revealed a u-shaped pattern of the point estimates across methods, the relationship warrants further investigation. All analyses estimated the largest coefficient within the medium disadvantage group, yet the lowest estimates were found in the high and low disadvantage group depending on the analytical approach. There were no consistent findings concerning the group with the smallest coefficient, and there was no evidence garnered in support of either the vulnerability or the trait suppression hypotheses in the split sample analyses.

While the split sample analyses presented allow for a strict test of the revised trait suppression hypothesis, there are several problems to note. Primarily, the designation of groups based on continuous variables and arbitrary cut-points discards information about the neighborhood disadvantage variable. Furthermore, the sample sizes for the split sample analyses are necessarily smaller than full sample analyses. This leads to a lack of statistical power, and an increased probability of Type II error—the estimates are unbiased but not efficient. Another problem concerns the estimates of the effects of the control variables. In a split sample approach the slope estimates of all the independent variables are free to vary across models. In short, estimating the same model over three groups implies that all the independent variables interact with the grouping variable. The theory presented herein argues the effect of self-control varies across neighborhoods, but no other variables are hypothesized to have differential effects. Aside from the lack of theoretical rationale for permitting all of the independent variables to interact with neighborhood disadvantage, there is a severe loss of efficiency as the number of parameters estimates is nearly three times larger than alternative methods of assessing the interaction.

Full Sample Analyses

The alternative to the split sample approach is to permit the effect of self-control to vary across neighborhoods while constraining the effect of the remaining variables to be equal across neighborhoods. To do so, the outcome can be modeled as a function of the independent variables and the product of the variables involved in the interaction (Jaccard & Turrisi, 2003). In this case, additional variables are necessary to permit a curvilinear moderation. In any event, statistically significant product-term coefficients

should be expected in OLS regression and should be interpreted as artifactual before they are interpreted as substantively meaningful (Vasquez, 2010). Therefore, negative binomial models are utilized to provide a more robust method of evaluating the slope estimates and tests of statistical significance. OLS models explaining the variety index and Tobit models explaining the logged variety index provided poor fits. Most importantly, the assumption of normality of errors in Tobit regression was. violated, and the estimates are inconsistent (Long, 1997). The results presented here focus on negative binomial models, and return only briefly to the OLS and Tobit models to discuss how they diverge from the negative binomial models. They are not to be trusted—the estimates are biased. Implications for the modelling of variety indices with these models will be considered.

The full sample analysis proceeded in several steps. First, the effects of self-control and neighborhood disadvantage were estimated without the entry of additional control variables (Model A). Model A is additive—the effects of self-control and neighborhood disadvantage are not dependent on one another. Then, two progressively more complex models were estimated. A linear moderation model (Model B) was investigated by entering the product of the self-control and neighborhood disadvantage variables into the regression equation. If the linear moderation model fails to provide an improvement in the fit of the additive model, this could be due to a curvilinear moderation effect. To permit the curvilinear moderation effect, two additional variables were entered into Model C: a quadratic neighborhood disadvantage term (ND^2) and the product of the self-control and quadratic neighborhood disadvantage variables (Baron & Kenny, 1986). Because the self-control and neighborhood disadvantage variables were

centered, a u-shaped moderation effect will manifest with a non-significant product term (SC X ND) coefficient, a positive quadratic neighborhood disadvantage coefficient (ND^2) and a positive quadratic-product term coefficient (SC X ND^2). Then a model estimating the effects of the control variables was estimated (Model D). Finally, the additive (Model E), multiplicative (Model F), and curvilinear multiplicative (Model G) models were estimated with the full set of control variables.

Table 13 displays the parameter estimates for the negative binomial full-sample models. Comparing the models without control variables (Models A, B, & C) reveals the curvilinear moderation model is the best fitting model by conventional standards ($p < .05$). The likelihood ratio test ($-2LL_{complex} - -2LL_{simple}$) comparing the linear moderation model to the additive model evidenced no improvement in fit ($\chi^2 = 2.30$, $p > .10$). In Model B, the estimate of the effect of self-control in the average neighborhood is -0.512, which is identical to the estimate of the effect of self-control in the sample on average (Model A, b = -0.512). Therefore it is not surprising the addition of the product term failed to improve the fit of the model. The likelihood ratio test comparing the curvilinear moderation model to the additive model shows the addition of the two variables provided significant improvement in fit ($\chi^2 = 5.79$, $p < .05$). The estimates from Model C are considered next.

While it is possible to interpret each product term and main effect coefficient in Model C, doing so can lead to confusion because of the conditional nature of each coefficient. Even the first-order product term coefficient (self-control X neighborhood disorder) is conditional on average neighborhood disorder. The negative sign of this coefficient does not necessarily mean the data fit the vulnerability hypothesis because the

additional variables (quadratic terms) are in the model. The direction of the product term coefficient shows the effect of neighborhood disadvantage on the effect of self-control is negative in *neighborhoods with average disorder*.[16] The self-control X neighborhood disadvantage^2 coefficient indicates dampening or magnifying of the effect of self-control at both ends of the neighborhood disadvantage distribution. The quadratic product-term coefficient (self-control X neighborhood disorder^2) must be positive in order for the revised trait suppression hypothesis to remain viable—the coefficient is in the expected direction but is not statistically significant ($z = 0.71$, $p > .10$). This coefficient indicates for a standard deviation change (increase or decrease) in neighborhood disadvantage, the magnitude of *the product term* (self-control X neighborhood disadvantage) increases by 0.033 units, on average. In other words, increasing neighborhood disadvantage from average accelerates the rate at which increases in neighborhood disadvantage dampen the effect of self-control. Likewise, decreasing neighborhood disadvantage from average accelerates the rate at which decreases in neighborhood disadvantage dampen the effect of self-control. This effect, however is not statistically distinguishable from zero. The improvement in fit of Model C over Model B appears to be attributable to the addition of the conditional non-linear effect of disadvantage on crime—that is considered in more detail below. To investigate this possibility and to provide a clearer description of the curvilinear moderation model, predicted values of the log count of crime types by level of neighborhood disadvantage and self-control are presented in Figure 6. Levels of the predicted outcome ($E[y] = \ln[\lambda]$) lie along the Y-axis and levels of self-control lie along

[16] This coefficient describes the slope of a line tangent to the curve that summarizes the effect of self-control across level of neighborhood disadvantage. A tangent with a slope of zero identifies the local maximum or minimum of a function. That the slope is very close to zero identifies the maximum or minimum effect is near average disadvantage in this case.

the x-axis of Figure 6. Levels of neighborhood disadvantage are differentiated with different dashed lines. Predicted values were generated for youth in neighborhoods at the 10^{th}, 30^{th}, 50^{th}, 70^{th}, and 90^{th} percentile of observed disadvantage scores using the regression equation. No additional conditions were required because the model did not include control variables. The level of crime decreases as levels of self-control increase, but there is only slight variation in the effect by neighborhood, as the different lines are nearly parallel. The slopes for each neighborhood (10^{th} – 90^{th} percentile) are -0.571, -0.564, -0.533, -0.467, -0.382, respectively. When exponentiating the coefficients, the percent reduction in the count of different types of crime, given a standard deviation increase in self-control ($100\% \times (\exp[b] - 1)$), varies from 31.7% ($90^{th}$ percentile) to 43.5% (10^{th} percentile). Importantly, self-control has its largest impact within neighborhoods near the maximum in disadvantage scores.

Another way of displaying this information is to use the regression equation to plot the magnitude of the effect of self-control across the full range observed values of neighborhood disadvantage as opposed to estimating the magnitude of the effect for discrete values of neighborhood disadvantage. The estimated slope (the change in log count of crime types given a standard deviation increase in self-control) across a large range of neighborhood disadvantage scores (10^{th} to 90^{th} percentiles) is plotted in Figure 7. The plot was generated from the coefficients in the regression equation by the following function,

$$b_{SC} = b'_{SC} + b_{SCxND}[ND_j] + b_{SCxND^2}[ND_j^2]$$

which gives the value of the slope given the level of neighborhood disadvantage for the j^{th} respondent, where b'_{SC} is the effect of self-control for respondents at average neighborhood disadvantage. This plot displays the effect in a continuous manner (rather than using discrete groups), which is important in the context of a curvilinear moderation effect. The quadratic product term permits the plot to take a curvilinear form. Though the quadratic product term coefficient is in the expected direction, the slope values for each level of disadvantage are relatively close to the average slope in the sample (-0.512-from Model A). Examining Figure 7 reveals that while the estimate of the effect of neighborhood disadvantage on the effect of self-control does not conform to an inverted u-shape, and furthermore, the variation in the effect in negligible. Self-control dampens criminal behavior at only one end of the continuum (low disadvantage)—yet across the full range of the plot, the effect of self-control is moderate. When compared to the maximum effect, which exists in neighborhoods with disadvantage scores about one standard deviation higher than average, the reduction in the effect at the opposite end of the continuum is small, and certainly not substantively important. Based on the results from Models A-C, one would conclude the effect of self-control is moderate and uniform across all levels of neighborhood disadvantage.

Next, the differential non-linearity implied by Model C is considered. The addition of quadratic and quadratic product terms allows for the effect of neighborhood disadvantage to vary depending on the level of self-control. Furthermore, the effect of neighborhood disorder on crime is nonlinear, and the degree of concavity is conditional on level of self-control. To display these effects, the effects of neighborhood disadvantage on crime at five levels of self-control (10^{th}, 30^{th}, 50^{th}, 70^{th}, and 90^{th}

percentiles) are displayed in Figure 8. Examining Figure 8 shows that the effect of disadvantage is non-linear at all levels of self-control. The effects are consistent with a saturation effect—increases in neighborhood disadvantage lead to greater levels of crime, however the effect tapers off such that increases in disadvantage have a much smaller effect on crime as neighborhood disadvantage increases beyond 0.5 standard deviations above the mean. The degree of concavity is similar across levels of self-control (as implied by the non-significance of the self-control X neighborhood disadvantage^2 coefficient) suggesting a lack of support for *differential* non-linearity. Clearly, the Model C is considerably more complex than Model B, and improvements may be attributed to permitting the effect of neighborhood disadvantage to be non-linear. As the differential non-linearity does not emerge, Figure 8 is inconsistent with the notion that high degrees of disadvantage are required to affect individuals with high self-control. If this were the case, the line displaying the effect of disadvantage for those in the 90th percentile would be convex, indicating an amplifying effect. While these conclusions are implied by Figure 6, it should be noted Figure 8 simply displays the exact same information in a different way.

Models D-G, where control variables are entered, yield different conclusions concerning the functional form of the moderation effect. The model level tests indicate the linear moderation model fails to provide improvement in fit over the additive model ($\chi^2 = 0.02$, $p > .10$). In fact, the product-term coefficient is positive when control variables are entered, but the product-term coefficient was negative without the entry of control variables in Model B. However, when control variables are included, the curvilinear moderation model provides an improvement in fit over the additive model (χ^2

= 11.85 $p < .05$). Examining the coefficients in Model G, each coefficient is in the expected direction. The partial effect of self-control in the average neighborhood is negative—a standard deviation increase in self-control in the average neighborhood reduces the number of crime types by 28% on average. The product-term coefficient (self-control X neighborhood disadvantage) is very near zero and not statistically distinguishable from zero ($p > .10$), indicating the maximum effect of self-control is in neighborhoods near the average level of disadvantage. Likewise, the quadratic product-term coefficient (self-control X neighborhood disadvantage^2) is positive, indicating that the effect of self-control is dampened as disadvantage increases and decreases from average. The one tailed p-value for this coefficient is less than 0.05, indicating the effect is statistically significant. Given the conservative nature and relatively low statistical power of the method employed to detect this effect, that the one tailed p-value of the coefficient is less than the conventional alpha level is notable.

The procedure used to create Figure 6 using Model C was replicated using Model G in Figure 9. However, predicted values were obtained for white male respondents with two parents at average levels of the remaining variables as Model G includes the curvilinear moderation model coefficients *and* control variables. Examining Figure 9, there are substantively important differences in the *slopes* of the lines, and furthermore, these differences are consistent with the revised trait-suppression hypothesis. The least steep lines are for youth living in neighborhoods at the 10^{th} and 90^{th} percentile of disadvantage scores (b_{10} = -0.195 & b_{90} = -0.202), and the steepest line (largest effect) is for youth living in medium disadvantage neighborhoods (b_{50} = 0.324). The magnitude of the effect of self-control in neighborhoods at the 10^{th} and 90^{th} percentile of disadvantage

scores is 0.61 times the magnitude of the effect of self-control in neighborhoods at the 50th percentile (roughly 40% smaller). Examining this figure, however, reveals that a central prediction of the revised trait suppression hypothesis, namely that levels of crime in disadvantaged neighborhoods are uniformly high, fails to emerge. Figure 9 reveals that the model predicts the highest level of crime would occur in individuals with low self-control living in a moderately disadvantaged neighborhood (50th percentile).

Figure 10 replicates the procedure used to examine the effect of self-control across the distribution of neighborhood disadvantage scores in Figure 7. Compared to the models without control variables, the effects are attenuated across the full neighborhood disadvantage distribution. The curvature in Figure 10 is more extreme as compared to Figure 7. Consistent with the prediction made by the revised trait suppression hypothesis, the effect of self-control varies across levels of disadvantage with the maximum effect being near the median level of disadvantage rather than at the maximum or minimum level of disadvantage as predicted by the vulnerability and trait suppression hypotheses, respectively.

Again, the differential non-linearity implied by Model G is examined in Figure 11. Here, the varying degrees of concavity manifest—when examining the extremes, the line for the 10th percentile of self-control is flat, but the line for the 90th percentile of self-control is concave indicating a saturation effect. Interestingly though, the effect of neighborhood disadvantage actually becomes negative under certain circumstances; when self-control is less than the median and neighborhood disadvantage is above the median. Among those with the self-control scores at the 30th percentile or smaller, changes from moderate disadvantage to extreme disadvantage (.5 standard deviations above the mean

to 1.35 standard deviations above the mean) actually reduce the predicted level of crime. Another interesting feature of Figure 11 is the amount of variability in predicted levels of crime among those in the highest and lowest levels of disadvantage (the extreme left and right of the chart). While this variation is considerably smaller than the variation the center of the figure, the theory implies there should be considerably less variation than emerged. The left and right ends of the lines in the chart represent those at the lowest and highest levels of disadvantage respectively. The theory implies that the degree of crime among these groups should be uniformly low or high, however there remains an effect of self-control on crime in the extremes of the disadvantage distribution (i.e., 10^{th} and 90^{th} %ile slope estimates from Figure 9).

VII. DISCUSSION AND CONCLUSIONS

This study fits into a broader literature that examines the effects of co-occurring risks for crime. The dominant theoretical view concerning co-occurring risks has been the amplification thesis (Hay et al., 2006) which posits that co-occurring risks interact to amplify levels of crime. Empirical support for this general prediction is abundant and inclusive of substantively important interactions taking place between person, family, and community level variables (Hay et al., 2006; Jones & Lynam, 2009; Lynam et al., 2000; Meier et al., 2008; Nye, 1958; Piquero et al., 2005; Reiss, 1951; Tibbetts & Piquero, 1999; Wright, Caspi, Moffitt, & Paternoster, 2004; Wright et al., 2001). The concepts central to this study were self-control and neighborhood disadvantage—the amplification thesis posits individuals lacking self-control that live in disadvantaged contexts will have the highest risk for offending. Furthermore, individuals lacking self-control that live in disadvantaged contexts will have a higher average level of offending than the sum of the average level of offending for individuals lacking self-control and individuals in disadvantaged neighborhoods. The implication is that the effect of self-control in low disadvantage neighborhoods is relatively weaker than less disadvantaged neighborhoods. Empirical tests of this hypothesis have typically examined interaction effects modelled by group analyses or with product terms and hypothesized the effect of self-control is strongest in disadvantaged neighborhoods (vulnerability hypothesis, see Chapter 2).

Not unlike other areas of criminological research (e.g., Hay, et al., 2006), empirical support for the vulnerability hypothesis, however, is inconsistent. Studies examining self-control and neighborhood disadvantage have found evidence consistent with amplification, dampening, and invariance. The opposite of the vulnerability

hypothesis focuses on the (in)ability to use individual characteristics to differentiate individual outcomes in certain situations or contexts (trait suppression). The social psychological literature (Mischel, 1977) provides a theoretical basis for the argument that certain situations (i.e. risky contexts) actually attenuate the effects of individual protective factors. For instance, under certain conditions pressures to offend become so overwhelming even individuals with high self-control become involved in crime (Anderson, 1999; Zimmerman, 2010).

A critique of this argument, which views both advantaged and disadvantaged neighborhoods as potentially strong contexts, was first stated by Zimmerman (2010). Zimmerman's observations were further developed in the initial chapters of this study to form a tentative revised trait suppression hypothesis. The assertion central to the revised hypothesis is that non-disadvantaged neighborhoods could represent a strong social context in that expectations for conforming behavior are strong within these neighborhoods. Thus, the effects of individual self-restraint should be relatively weaker under both extreme disadvantage and advantage as compared to a social context where a mix of expectations for deviant and conforming behavior are present.

Figure 12 displays the predictions of the revised theory graphically. The theory can be summarized by two predictions: (1) relative to the effect of self-control in medium disadvantage neighborhoods, the effects of self-control are attenuated in extremely high or low disadvantage neighborhoods and (2) relative to the level of crime in medium disadvantage neighborhoods, overall levels of crime in high and low disadvantage neighborhoods are high and low, respectively. The purpose of this study was to translate

this revised theory to an empirical model, and confront it with data. The results are discussed below.

Relative Effects of Self-Control

The results here provide tentative support the first prediction. Across several methods, including split sample and product term analyses, it was evident the effect of self-control was strongest in neighborhoods characterized by average physical and social disorder and the effects were attenuated under extreme disadvantage or advantage. The degree of attenuation, in terms of statistical and substantive significance, varied across methods.

Considering the split sample analyses, the coefficients quantifying the causal impact of self-control were attenuated and found not to be statistically significant in (1) the high disadvantage group in a negative binomial model with control variables, (2) the low disadvantage group in an OLS model with control variables. The remaining point estimates across disadvantage level groups were statistically significant. No clear patterns in the point estimates emerged from the split sample analyses—across methods and model specifications the only consistent result was that the largest coefficients were observed in the medium disadvantage group. Nonetheless, the effect of self-control remained a protective factor across neighborhood disadvantage level groups.

Consistent with this fact, the statistical significance of differences *in the effects of self-control* between disadvantage level groups was sporadic and inconsistent. There were two statistically significant differences—in models without control variables, the effect of self-control for the low disadvantage group was smaller than for the medium

disadvantage group in both OLS and negative binomial models. The effect in the high disadvantage group was very similar to the medium disadvantage group in these models. In the split sample models with control variables, the largest difference in the slopes was between the low and medium disadvantage groups in the OLS models, but was between the high and medium disadvantage groups in the negative binomial model. None of the differences in models with control variables were statistically significant.

Turning to the full sample analyses (i.e., product term analyses), there was no support for the first prediction when examining the effect of self-control across neighborhoods in models without control variables. The effect was found to be invariant across level of disadvantage as neither the linear moderation model nor the curvilinear moderation model showed substantively significant variations in the magnitude of the slope. However, the final model, which utilized control variables, showed support for the first prediction as the effect of self-control was largest in medium disadvantage neighborhoods. While the effect of self-control remained substantively important among respondents in high and low disadvantage neighborhoods, the effect of self-control in medium disadvantage neighborhoods (50^{th} percentile) was attenuated by about 40% when examining respondents at the 10^{th} and 90^{th} percentile of neighborhood disadvantage scores.

That the effect of self-control was not fully attenuated (reduced to zero) in neighborhoods with extremely high or low disadvantage is not problematic. This study relied on estimates of the effect of self-control in neighborhoods at the 10^{th} through the 90^{th} percentile of disadvantage scores. The values of neighborhood disadvantage at the 10^{th} and 90^{th} percentile, in standardized scores, were -1.330 and 1.314, respectively. The

minimum and maximum scores were -1.70 and 2.25. Clearly, there were respondents who reported living in neighborhoods with considerably more disorder than were presented in the figures in the previous chapter. However, inferences about the effect of self-control among respondents at more extreme scores would have been based on relatively few cases, and therefore would have been less reliable. The more reliable estimates of the degree of attenuation at the 10^{th} and 90^{th} percentile of disadvantage are therefore, conservative estimates of the substantive significance of this effect.

Considering the statistical significance, there is evidence to suggest field studies utilizing the product term approach to quantifying conditional effects experience higher Type II error rates (failing to reject a false null of no moderation). Furthermore, field studies generate conservative estimates of the change in proportional reduction in error measures after accounting for moderation as compared to experimental studies (McClelland & Judd, 1993). Many of these difficulties can be attributed to the characteristics of the joint distribution of the variables involved in the interaction. Examining the joint distribution of self-control and neighborhood disadvantage in this sample (See Figure 13: Observed Distribution) relatively few cases lie at the corners of the distribution where extreme values are observed. These cases are crucial for detecting statistically significant interaction effects (McClelland & Judd, 1993, p. 382) Furthermore, this distribution is characteristic of typical joint distributions of variables in field studies which have been shown to be less efficient than joint distributions produced by experimental studies. For example, the ideal distribution presented in Figure 13 maximizes the joint variability of the variables involved in the interaction thus maximizing statistical power. The differences between the observed and ideal joint

distributions suggest the ability (i.e., statistical power) to detect an interaction effect in this sample is severely limited due to increased residual variance of the product term (self-control X neighborhood disadvantage) and this limitation compounds upon the introduction of higher order interaction effects (McClelland & Judd, p. 385). It should however be noted that McClelland and Judd (1993) found no differences in the accuracy of the parameter estimates between experimental and field studies in a simulation (i.e., estimates remained unbiased). While the product terms failed to reach conventional levels of statistical significance, and their inclusion failed to improve prediction substantially, McClelland and Judd's simulation suggests the parameter estimates are accurate, assuming the models are correctly specified. Considering the whole of the evidence concerning the first prediction, though the results of the split sample models suggested invariance, the general consistency of the location of the maximum effect in these analyses (medium disadvantage group) paired with the results of Model G and suggests that prediction one should not necessarily be rejected outright.

Differential-Nonlinear Effects of Neighborhood Disadvantage

Considering the evidence for prediction two, however, shows that the data fail to support the more nuanced predictions about the interrelationships between self-control, neighborhood disadvantage and crime. Prediction two posits individuals in high (or low) disadvantage neighborhoods will uniformly commit (or abstain from) crime. Perhaps more conservatively, it posits criminal behavior is nearly absent in low disadvantage neighborhoods and that the probability of involvement in crime is much greater in high disadvantage neighborhoods as compared to low disadvantage neighborhoods. The split sample analyses showed disadvantage level group was a weak predictor of participation

in any crime, as well as involvement in crime as measured by the variety index although the level of participation in crime was high in this sample across groups. However, there were small statistically significant differences in crime by disadvantage level. These differences, though, do not quantify the unique impact of neighborhood disadvantage on crime as level of disadvantage was also related to a number of risk factors (peer delinquency, moral disengagement, social capital).

Moving beyond descriptive statistics, the full sample regression models showed that the effect of self-control was conditional on the level of neighborhood disadvantage in an inverted u-shape pattern, which, in these models, implies the effect of neighborhood disadvantage is (1) conditional on level of self-control, (2) nonlinear, and (3) the degree of concavity is also conditional on self-control. Reconsidering Figure 9, the intercept of the line for respondents at the 90th percentile is lower than anticipated by the theory. To a lesser extent, the same is true of the line for respondents at the 70th percentile. That the line for the respondents at the 90th percentile of disadvantage scores crosses other lines suggests their level of participation in crime is similar to respondents with lower disadvantage scores, whereas the theory predicts these respondents will be involved in substantially more crime. Likewise, the intercept of the line representing respondents at the 10th percentile is somewhat higher than anticipated by the theory.

The differentiation of the intercepts of the lines in Figure 9 is partially determined by the magnitude of the neighborhood disadvantage coefficient. As this coefficient becomes larger, the lines expand outwards from the line representing the 50th percentile. In Model G, the coefficient was equal to 0.124—holding constant the other estimates, the lines in Figure 9 become fully distinct when the neighborhood disadvantage coefficient is

equal to 0.410. The observed coefficient is approximately 70% smaller than necessary to support the theory.

Two corollaries concerning the conditional effects of neighborhood disadvantage stem from prediction two. First, the impact of increasing neighborhood disadvantage from low to medium for individuals with high self-control should be minimal; however, these individuals will experience a breaking point where the pressures towards offending become so strong that disadvantage has a profound impact. Considering the lowest line in Figure 11, this was clearly not the case. The effect of neighborhood disadvantage was linear and positive. The data failed to support the assertion that individuals with high self-control require extreme levels of disadvantage to be involved in criminal behavior in this sample.

Second, the opposite prediction is made for individuals with low self-control. These individuals experience rapid increases in crime for changes in disadvantage from low to medium, but beyond medium disadvantage, the effect tapers off and smaller increases in crime are hypothesized when disadvantage increases from moderate to high. For individuals with low self-control in this study (10^{th} percentile), the effect of neighborhood disadvantage *was* nonlinear-positive with a diminishing return. The data somewhat support corollary two, though the concavity of the effect was exaggerated such that increases in neighborhood disadvantage lead to a decrease in crime among individuals with low self-control.

Overall, the full sample negative binomial models do not provide clear evidence in support of the revised trait suppression hypothesis. While some evidence supports

predictions made by the theory, there are several departures from the expected values of crime based on the joint distribution of self-control and neighborhood disadvantage scores. On the whole, these analyses fail to find support for the revised theory, though there are a number of limitations that must be discussed to properly condition this conclusion.

Limitations

Sample Selection Bias. As discussed above, the effect of neighborhood disadvantage was smaller than anticipated by the theory. There are two plausible reasons for the discrepancy between the anticipated and observed relationship between neighborhood disadvantage and crime, both of which pertain to sampling bias. First, the sample examined in this study was composed entirely of offenders. Not only were the respondents offenders, they had been arrested or petitioned *and* adjudicated. Prior research comparing detained and non-detained high school youth found large differences in participation in theft, drug selling and use, and serious violence (Edwards, 1996)—about 70-80% of delinquent (formally processed) youth reported engaging in these offenses as compared to 10-30% of non-delinquent youth. More to the point, the population of interest in the Pathways to Desistance Study was a small segment of the general population of youth that should be expected to exhibit higher levels of participation in crime. Given the expectation of a positive relationship between neighborhood disadvantage and crime, an elevated level of offending within the sample could certainly have attenuated the relationship between disadvantage and offending (i.e., a ceiling effect).

A related problem concerns the consequences of differential selection for formal processing in the juvenile justice system that occurred in the Pathways to Desistance Study. In this study, a substantial proportion of the original sample was incarcerated during the follow-up period examined herein, and were therefore excluded from the analysis. The mean level of neighborhood disadvantage at the intake interview was greater for respondents excluded from the analyses for incarceration than for those included in the analyses. In this sample, a logistic regression (See Table 14) predicting confinement for the entire first follow-up recall period as a function of age, race, sex, neighborhood disorder, family structure, and a variety index (with independent variables constructed from the intake interview) revealed a standard deviation increase in perceived disorder increased the log odds of confinement by 18% ($z = 2.57$, $p < .05$), on average.

Turning to Figure 14, which displays the predicted probability of confinement for an average black male with one parent by level of neighborhood disadvantage: a standard deviation increase in neighborhood disadvantage corresponds to an average increase in probability of confinement of about .04. In this sample, the probability of being confined for the entirety of the first recall period for an average black male with one parent in a neighborhood at the 95th percentile of neighborhood disadvantage was 0.53. The probability of confinement for an average black male with one parent in a neighborhood at the 5th percentile of neighborhood disadvantage was 0.39. The positive relationship between neighborhood disadvantage and probability of confinement, while controlling for prior offending suggests that selection by the criminal justice system of youth in more disadvantaged neighborhoods results in biased estimates of the slopes for neighborhood disadvantage, and the product terms, as well as the slope estimates for self-control.

Control Variable Inclusion. There were large differences in the models including control variables and models excluding control variables. Control variables were utilized to estimate the effects of self-control and neighborhood disadvantage while holding constant other variables that were related to disadvantage. Whether this is justified is a theoretical issue that is unresolved. Inclusion of control variables allows the unique effect of each variable to be estimated—that is, the regression coefficients reflect the expected change in the dependent variable given an increase in the independent variable while holding other potentially confounding variables constant. This could be problematic if pressures towards offending in disadvantaged neighborhoods are captured by the control variables. If this is the case, the unique effect of neighborhood disorder may represent some processes other than increased pressure to offend or conform.

The most threatening of these control variables seems to be exposure to delinquent peers because perspectives on the transmission of deviant subcultures identify delinquent peer associations, particularly with older peers, as central (Akers, 1977; A. K. Cohen, 1955; Harding, 2009; Sutherland & Cressey, 1966). In Anderson's Code of the Street (1999) neighborhood differences are thought to be subcultural—an informal code transmitted through interpersonal relationships condones certain forms of criminal behavior and thus dampens individual differences. If peer associations are the primary mechanism through which pressures to offend dampen individual differences, estimates of the effects of neighborhood disadvantage while holding constant delinquent peer associations likely reflect some other process. However, when excluding peer delinquency from Model G, the results are substantively unchanged. The trait suppression hypothesis argues that pressures *within* high disadvantage neighborhoods are what leads

to high levels of crime and dampened individual differences. Whether this effect manifests because individual risks that accumulate because a neighborhood is disadvantaged (i.e., disadvantage leads to less supervision, more delinquent peers, lower self-control, lower moral engagement, higher strain) or because the neighborhood has unique influence above and beyond these risks is unclear and requires more theoretical attention.

Measures

There are numerous issues surrounding the measurement of neighborhood disadvantage that merit discussion. The measurement of neighborhood disadvantage in this study was approached from a perpetual angle—respondents were asked to report the level of physical and social disorder they perceived in their neighborhood. Typically, neighborhood disadvantage is measured by creating a formative index comprised of aggregate information on economic prosperity, family composition, home ownership, racial heterogeneity, among other variables. These are sometimes called structural antecedents (Sampson & Groves, 1989). Most theories addressing neighborhood disadvantage rely on these indicators to characterize the objective socio-economic qualities of neighborhoods (i.e., Bursik & Grasmick, 1993; Sampson, Raudenbush & Bryk, 1997) which ultimately impact the ability of neighborhood residents to control crime. There may, however, be important distinctions between objective and perceived characteristics of the neighborhood. A neighborhood disadvantage measure that relies on objective indicators drawn from the U.S. 2000 Census was available for the current study but was not utilized in the statistical models. Before considering reasons for differences

in perceptual and objective measures, the convergent validity of these measures in the PTD study is discussed.

Respondents in the PTD study were asked at each interview to provide physical addresses for locations in which they were living during each month of the recall periods from follow-up interviews one and two. Indicators of neighborhood socio-economic status from the Census data used were (1) percent of households below the poverty line, (2) percent of households receiving public assistance, (3) percent of 16 and older unemployed male residents, (4) percent of female headed households, (5) number of different races in the neighborhood, (6) percentage of foreign born residents in the community, (7) percent of renter-occupied households, and (8) percent of residents who have moved within the last five years. Chung and Steinberg (2006) found among the respondents in Philadelphia that these variables clustered along the orthogonal dimensions of concentrated disadvantage, ethnic heterogeneity, and residential stability. The Census disadvantage measure is a factor score computed using the first four indicators listed above, and as such measures the degree of economic disadvantage at the Census block group level. Researchers generated neighborhood disadvantage scores for each address for each respondent.

This procedure produced an observation for each person-month in the recall period. Disadvantage scores varied from month to month given a respondent moved between Census block-groups. When in the community, the vast majority of respondents lived in the same place within each follow-up for the entirety of the recall period (677 respondents in follow-up 1, 660 in follow-up 2). About 67.8% of respondents reported living in only one location in the community for the entirety of both follow-up periods. A

standard deviation score quantifying the variability in monthly disadvantage scores was computed for each respondent. Based on the standard deviations, the majority of respondents who moved between neighborhoods moved to neighborhoods with similar levels of disadvantage, with very few respondents experiencing extreme changes in disadvantage scores. Finally, the Census disadvantage score was calculated as the average disadvantage score across all valid months in the recall period for both follow-ups (sum of monthly disadvantage scores divided by number of valid months). The perceived disorder and Census based disadvantage scores were moderately correlated ($r = .53$).

Nonetheless, *systematic* observations and measurements of physical and social disorder were found to have a robust relationship with neighborhood disadvantage in Chicago—likewise, when neighborhood residents' perceptions of disorder were aggregated, this neighborhood measure of disorder was significantly related to systematic measurements of disorder ($r \approx .55$, Sampson & Raudenbush, 1999). The strength of these relationships (structural indicators of disadvantage → objective disorder → aggregate perceived disorder) as well as the moderate correlation between perceptual and objective measures in this study, suggests that individual perceptions of disorder, while related to disadvantage, are not a true substitute. The measures are not correlated so highly as to suggest that they are interchangeable—however, the measures are correlated just above .5 which Carlson and Herdman (2012) identify as a lower bound for identifying reasonably convergent measures. In short, individual perceived disorder is a proximate measure that may interact differently with self-control than an objective measure of neighborhood

disorder. Whether these differences are due to statistical or methodological artifact or due to real conceptual differences is an important question.

Considering these measures separately, it is possible that each fails to adequately discriminate between neighborhoods characterized by relatively high (or low) disadvantage. As evidenced in Figure 15, there is considerable variability in Census disadvantage scores for each level of perceived disorder and the highest Census disadvantage scores lie in the middle of the perceived disorder distribution. This suggests that neighborhoods at various levels of structural disadvantage and social/physical disorder are heterogeneous when it comes to capturing neighborhood disadvantage. While the perceptual measure provides thorough coverage of disorder, physical and social disorder are only two pieces of disadvantage and there are other ways in which disadvantage varies further than this measure can capture. Obviously, there are elements of disorder such as poverty, unemployment, and the labor market, but additional elements such as cultural heterogeneity, residential mobility, family composition, and housing policies contribute to unique social situations in each neighborhood (Elliott et al., 1996; Land, McCall, & Cohen, 1990; Sampson & Groves, 1989; Shaw & McKay, 1942; Wilson, 1987). While the approach to measuring disadvantage in this study was reflective (i.e., disorder was thought to be an indicator, *caused by* neighborhood disadvantage) there are other approaches to measuring neighborhood disadvantage. Elliott and colleagues (1996) explicated a theoretical model that views disadvantage as multidimensional and drew on Shaw and McKay's (1942) argument that co-existing neighborhood risk factors would amplify the detrimental effects of the neighborhood, necessitating the distinction between elements of disadvantage. Finally, disadvantage could be viewed from a

formative measurement approach (Bollen, 2002) as individuals' socioeconomic status is traditionally understood. In this statistical framework, indicators of disadvantage are thought to cause variation in levels of neighborhood disorder—the construct is defined by the indicators rather than the reverse (i.e., reflective measurement). If one takes a formative approach to measuring neighborhood disadvantage, the viability of proxy measures (like the one used here) is highly questionable.

Though individual perceptions of disorder have shortcomings as indicators of disadvantage, they are advantageous for other reasons. As is the case in much macro-level research (e.g. Land, et al., 1990), selecting the geographic boundaries at which neighborhood level measurements are taken has proven controversial (Hipp, 2007). There are a number of different administrative boundaries utilized for measuring neighborhoods (e.g., zip-codes, Census boundaries, wards). Even studies using Census geographic boundaries have utilized different spatial resolutions (i.e., block groups or tracts) to define neighborhood boundaries (Chung & Steinberg, 2006; Sampson & Raudenbush, 2004). Others have aggregated Census information (or other administrative boundaries) relying on more nuanced and context specific information to construct neighborhood boundaries and measures of disadvantage (Sampson et al., 1997, see also Crime Survey for England & Wales). Recent research by Hart and Walker (2013) also suggests that there are discrepancies between perceived boundaries of neighborhoods and administrative boundaries (Zip codes & Census geography), calling into question the spatial concurrence between disadvantage measures and the experiences of neighborhood residents.

The use of a perceptual measure somewhat hurdles these definitional difficulties by allowing youth to define their neighborhood as somewhat amorphous and unique to their experience. It is possible that respondents reported perceptions of disorder based on their awareness space (Brantingham & Brantingham, 1984) that is likely to overlap various geographic boundaries. Notwithstanding the criticism that the actual level of disorder is heterogeneous even within an individual's awareness space, the perceptual approach gives information about locations where respondents actually spend their time. An objective measure may give information about the area where a person sleeps (i.e., the area around their home) but fail to give information about where they spend time away from their home. That discrepancies between where a person sleeps and where they spend their time when away from the home exist may explain why the area surrounding a person's "neighborhood" matters in addition to their neighborhood when predicting crime, delinquency and collective efficacy (Graif, 2015; Sampson, Morenoff, & Earls, 1999; Vogel & South, 2016). Given the call to address neighborhood effects in a way unbounded by geographic limits (Hipp & Boessen, 2013), the perceptual approach seems to be a useful alternative. Finally, it has been argued that objective neighborhood qualities operate largely through perceptions and cognitive processes (Jones & Lynam, 2009; Wikstrom & Treiber, 2007), therefore perceptual measures may better characterize the theoretical process whereby personal and social (neighborhood) controls interact in decision making.

One final comment concerning the measurement of neighborhood conditions relates to statistical adjustment for non-independence of observations. Scholars examining the joint effects of individual and neighborhood level variables commonly

analyze data with a hierarchical structure. That is, data are collected using multi-stage cluster sampling strategies to obtain samples of individuals nested within neighborhoods. Studies of neighborhood effects also typically employ fixed effects or mixed effects models to account for the fact that respondents living in the same neighborhood have more in common than respondents in different neighborhoods (e.g., Sampson et al. (1997). Failing to account for these similarities results in biased estimates of the standard errors and inflated test statistics (Hox, 1995). An alternative to random effects models and cluster-robust standard errors is the cluster-robust standard error, which adjusts standard errors for non-independence using neighborhood information. Unfortunately, the PTD data do not include geographic information that would permit researchers to identify respondents living in the same geographic area. The models presented here do not account for the geographic clustering of respondents.

The measure of self-control in this study was a combined index of the three subscales of the Weinberger Adjustment Inventory (impulse control, suppression of aggression, and consideration of others) and the Future Outlook Inventory. The main advantage of this broad measure of self-control is its adherence to content validity while taking Gottfredson & Hirschi's conceptualization as authoritative. Indeed, this measure captures self-control in various ways, providing outstanding coverage of the concept. However, there is less emphasis given to risk seeking in this measure, a key element of self-control. Nonetheless, there was abundant evidence of discriminant validity of the measure used here as the full sample correlation between self-control and (1) the variety index at intake (ever) was $r = -0.47$ ($n = 1{,}348$, $p < .01$) and (2) the variety index for the follow-up period was $r = -0.36$ ($n = 1{,}298$, $p < .01$). Whether this measure is comparable

to other measures of self-control, however, is an important question for gauging the comparability of the results of this study and others.

Duckworth and Kern (2011) examined 7,782 estimates of the convergent validity of various self-report inventories designed to measure self-control and found average convergence of 0.50, suggesting marginal convergence. While this study did not provide detailed information on specific scales, an unpublished study by Jones (2016) found the Grasmick et al. scale (1993) to be correlated with the WAI (r = .69) and these scales were similarly related to the six domains of the HEXACO Personality Inventory. Likewise, the correlations with various offending measures were similar between the two scales (Jones, 2016) and were stronger than an alternative measure (Tangney et al., 2004). Comparing this measure to narrower measures of impulsivity such as those used by Zimmerman (2010), Vazsonyi and colleagues (2009), the measure used in this study is considerably more broad, and while it may share common components, is likely not a substitute. As such, the conclusions of those studies are narrower as they pertain to an element of self-control rather than the full concept. Similarly, behavioral measures, though correlated, are not convergent enough with self-report measures to suggest these conclusions would likely hold if behavioral measures were substituted in a different sample (Walters, 2016).

While this study examined self-control as an abstract concept, ignoring distinctions between the elements of self-control, it is possible that the elements interact differently with neighborhood disadvantage. While Meier and colleagues found neighborhood disadvantage amplified the effects of impulsivity and callousness separately (2008), no research has examined the remaining elements (i.e., temper, risk seeking) separately. This study hypothesized that neighborhood disadvantage acted as a

proximate measure of the strength of social contexts such that the effect of self-control varied according to expectations for behavior within these neighborhoods—as such, a curvilinear effect manifests. It could very well be the case that the effect could be decomposed such that neighborhood disadvantage amplifies the effects of certain elements of self-control but dampens the effects of others. However, there exists no well-developed theoretical framework to explain why some elements of self-control would be suppressed by disadvantage and others amplified. As demonstrated in the Chapter 2, arguments have been made for each perspective separately. While examining the elements separately could prove worthwhile, Ward et al. (2015) recommend examining elements only after partialling the general factor of self-control out with a bifactor measurement model. This considerably complicates the analytical approach by demanding that latent variables interact in an SEM framework with a non-normally distributed dependent variable.

Conclusions

The purpose of this study was to form a tentative theory aimed at explaining disparate research findings on the interaction of individual self-control and neighborhood disadvantage. After testing the revised theory with data and finding mixed support, the results of this study suggest that the alternative explanation, namely that the effects of neighborhood disadvantage and self-control are independent, best explains these data. However, further research and replication of this model is needed. Special consideration to the distribution of neighborhood disadvantage is also warranted. Specifically, this research suggests that the effect of neighborhood disadvantage on the effect of self-control may depend on the level of neighborhood disadvantage. Studies examining

different segments of the distribution of neighborhood disadvantage, assuming this model is correct, will necessarily find different effects. Considering the contexts in which several prior studies have been conducted, it may be the case that evidence consistent with amplification manifests in somewhat more affluent samples (such as Meier et al., 2008 in Iowa), evidence consistent with trait suppression manifests in somewhat more disadvantaged contexts (such as Zimmerman, 2010 in Chicago), and null effects are found in nationally representative samples (as in Vogel, 2016 & Vazsonyi et al., 2006). This does not explain why Lynam et al. (2000) found evidence consistent with amplification in Pittsburg, an area where concentrated disadvantage clearly manifests. Alas, this explanation is, of course, conjecture. For primary data collection efforts, special attention to oversampling within particularly disadvantaged or affluent neighborhoods may be necessary for full tests of the revised trait suppression hypothesis.

The evidence presented here suggests replication of this model utilizing the Add Health data, in which Vazsonyi et al. (2006) found there was no interaction, and Vogel (2016) found evidence consistent with amplification under certain circumstances, would be worthwhile. The results of the Vazsonyi study found a small effect of neighborhood disadvantage on aggression, and that the largest correlations between impulsivity and multiple measures of offending (not controlling for other variables) were located in neighborhoods closer to average in terms of disadvantage. The Add Health data may also be advantageous in that they will be less affected by attrition due to incarceration than the Pathways to Desistance Data. However, considering the link between neighborhood disadvantage and academic attainment (Brooks-Gunn et al., 1993; Woodtke et al., 2011), this dataset is not free from sample selection bias concerns. Furthermore, it is possible

that a full range of disadvantage is not captured by the sampling design of the Add Health Study.

While the findings here suggest none of the more complex hypotheses linking self-control and neighborhood disadvantage are supported, it is important to note that a finding of no linear interaction does not necessarily rule out the possibility of a curvilinear interaction. In fact, there is clear value in the rejection of a curvilinear moderation effect when linear moderation effects also fail to manifest, as this provides direct evidence for invariance—a point often overlooked (Vazsonyi, et al, 2006; Vogel, 2016).

Given the logic of the trait suppression perspective discussed by Mischel (1977), it is possible that a curvilinear moderation models apply well to other areas of research where consistently detecting interaction effects have proven elusive. This logic could easily be applied to other interactional hypotheses (e.g., social bonds & self-control, sanction risk and self-control), so long as the moderating variable could be expected to impact individual's perceptions of how they are expected to behave. This suggests moderator variables with strong bivariate effects will differentiate the level of offending across levels of the moderator, not just in the magnitude of the effect of the focal variable, making support for these types of interactions more clear. The simplicity and logic of a curvilinear moderation effect also suggests that (1) such a model should be tested particularly in exploratory settings where the direction or magnitude of an interaction effect is not clearly anticipated by any theory and (2) researchers should not shy away from the empirical complexity of additional product terms. Just as standard bivariate curvilinear effects often improve our knowledge of crime curvilinear

moderation models can help us understand the contexts in which variables matter most. These findings of course should as always be subjected to replication whenever possible.

A central tenant of the revised trait-suppression hypothesis is that mixed expectations for offending within the neighborhood context maximizes the causal significance of individual traits. However, how neighborhood disadvantage is linked with expectations for offending is ambiguous in this study. Subcultural explanations anticipate disproportionately black, impoverished inner city neighborhoods will have their own normative system conducive to violence (Anderson, 1999) and evidence has accumulated for this claim (Matsueda, Drakulich, & Kubrin, 2005; Stewart & Simons, 2010, but see Browning, Feinberg & Dietz, 2004). On the other hand, elaborations upon the social disorganization framework (Bursik & Grasmick, 1993b; Sampson et al., 1997) have linked disadvantage with the capacity for the collective neighborhood to control behavior. Social disorganization and cultural transmission theories identify one dimension (social disorganization, street code adherence) to characterize the neighborhood in terms of its influence on crime and delinquency. Furthermore, these theories treat the measurement of these dimensions as continuous and their relationships with crime and delinquency as linear. In other words, social organization/collective efficacy is thought to have crime dampening effects, and a neighborhood code of the street—criminogenic effects. These theories are also traditionally viewed as competing, meaning pushes to offending such as subcultural adherence and insulation from offending through social control or collective efficacy cannot simultaneously exist in the same neighborhood.

While these theories are certainly parsimonious, the revised trait suppression hypothesis necessitates that pushes towards crime as well as insulation from offending

exist simultaneously in average neighborhoods. It is the ambiguity in expectations for behavior that drives individual differences to become important. It could be the case that neighborhood disadvantage is linked with both subcultural codes that promote criminal behavior (particularly in average and disadvantaged neighborhoods) and neighborhood regulatory processes like social control or collective efficacy that dampen crime (particularly in average and affluent neighborhoods). Careful consideration to operationalization of these concepts and the study of their joint emergence in different types of neighborhoods is necessary for the advancement of theory. Nonetheless, viewing these theories as competing, this study falls short of adjudicating between them empirically.

Table 1: Studies of the self-control X neighborhood disadvantage interaction

Study	Sample	Dependent Variable	Impulsivity Measure
Lynam et al. (2000)	PYS- 12-13 year old boys N = 425	SRDI (Elliott, Huzinga, Ageton, 1985) Status, vice, theft, violence, variety	Multi-method, Multi-source (Self, teacher, parent reports- Nunnally, 1978)
	PYS- Same cohort as above + follow-up at 17 N= 80	SRDI (Elliott, Huzinga, Ageton, 1985) Status, vice, theft, violence, variety	Multi-method, Multi-source (Self, teacher, parent reports- Nunnally, 1978)
Vazsonyi et al. (2006)	ADD Health N = 20,000	SRDI- General Delinquency, Nonviolent Delinquency, Aggression- Square root corrected scores	4 question, attitudinal- systematic problem solving approach emphasized
Meier et al. (2008)	Iowa School Children 6, 8, 11 grade N = 85,301	SRDI- 7 item variety index	4 question, attitudinal- systematic problem solving approach, risk seeking, working hard, fix problems **Callousness-** Help others, Care about others feelings, feel sorry for victims
Jones and Lynam (2009)	Lexington Longitudinal Study- High School Youth Follow-up as young adults	SRDI- Variety Index	Attitudinal self-report; Lack of Premeditation and Sensation Seeking; 23 items.
Zimmerman (2010)	PHDCN- 12 & 15 year olds N = 1,191	SRDI- Violent & Property offenses	Parent report on 10 items: Hyperactivity, lack of persistence, etc. Alternative 4 item scale with greater face validity also used.
Zimmerman et al. (2015)	International Sample of Adults (Russia & Ukraine)	Projected offending	Behavioral self-control scale, Grasmick Scale
Vogel (2016)	ADD Health N = 11,288	SRDI – Variety of Violent acts	4 question, attitudinal- systematic problem solving approach emphasized

Table 1 Continued

Study	Neighborhood Measure	Analysis	Findings (Change in effect of self-control as disadvantage increases)
Lynam et al. (2000)	SES- Census Based (SES, Poverty, & Public Housing) Discrete 4-point scale	OLS, HLM (Linear)	Variety (+) Status (0) Vice (0) Theft (+) Violence (+)
	Self-report neighborhood conditions Continuous scale	HLM (Linear)	Variety (+) Status (0) Vice (0) Theft (+) Violence (+)
Vazsonyi et al. (2006)	SES-Census based on Single parent households, Poverty rate, Unemployment rate Continuous scale, block group level only	HLM (Linear)	Male- General (0) Male- Non- Violent (0) Male- Aggression (0) Female- General (-) Female- Non- Violent (0) Female- Aggression (-)
Meier et al. (2008)	Self-report neighborhood questions about how adults in the neighborhood behave (Get along, care for others, alcohol & drug free, talking, etc.) Continuous Scale	HLM (Linear)	Impulsivity Isolated (+) Callousness Isolated (+) Simultaneous: Impulsivity (+) Callousness (+)
Jones and Lynam (2009)	Self-report perceptions of neighborhood supervision; similar to collective efficacy scale from Sampson	OLS	TAS (+) LoP (+)
Zimmerman (2010)	SES-Census Based on poverty, public assistance, non-intact families with children, unemployment, household income, non-white Discrete 3-point scale	Multivariate-multilevel Rasch model	Full impulsivity scale: Violent (-) Property (-) Abbreviated impulsivity scale: Violent (-) Property (-)
Zimmerman et al. (2015)	Self-reported affordability of neighborhood. Neighborhood mean collapsed. Continuous scale	HLM (Linear)	Projections (-)
Vogel (2016)	SES-Census based on Single parent households, Poverty rate, Unemployment rate Continuous scale, tract and block group level compared	Negative Binomial, and other sensitivity analyses	Violence-block group measure (0) Violence-tract measure (+)

Table 2: Weinberger Adjustment Inventory-Restraint subscales & question wordings	
Subscale	Question Wording
Impulse Control	I'm the kind of person who will try anything once, even if it's not that safe. I should try harder to control myself when I'm having fun. I do things without giving them enough thought. I become "wild and crazy" and do things other people might not like. When I'm doing something for fun (for example, partying, acting silly), I tend to get carried away and go too far. I like to do new and different things that many people would consider weird or not really safe. I say the first thing that comes into my mind without thinking enough about it. I stop and think things through before I act.
Suppression of Aggression	People who get me angry better watch out. If someone tries to hurt me, I make sure I get even with them. If someone does something I really don't like, I yell at them about it. I lose my temper and "let people have it" when I'm angry. I pick on people I don't like. I say something mean to someone who has upset me. When someone tries to start a fight with me, I fight back.
Consideration of Others	Doing things to help other people is more important to me than almost anything else. I often go out of my way to do things for other people. I think about other people's feelings before I do something they might not like. I enjoy doing things for other people, even when I don't receive anything in return. I make sure that doing what I want will not cause problems for other people. Before I do something, I think about how it will affect the people around me. I try very hard not to hurt other people's feelings.
Responsibility	I do things that are against the law more often than most people. When I have the chance, I take things I want that don't really belong to me. I do things that are really not fair to people I don't care about. I will cheat on something if I know no one will find out. I break laws and rules I don't agree with. People can depend on me to do what I know I should. I do things that I know really aren't right.
Adapted from Weinberger (1997)	

Table 3: Case interview, detention, and community exposure breakdown

	Interviews Completed									
	Neither Interview		Follow-up 1 only		Follow-up 2 only		Both follow-up interviews		Total	
Missed both interviews	48	(3.55%)	-		-		-		48	(3.55%)
Detained for the duration of the recall period	-		22	(1.62%)	10	(0.74%)	239	(17.65%)	271	(20.01%)
Less than 3 months in the community	-		3	(0.22%)	4	(0.30%)	174	(12.85%)	181	(13.37%)
More than 3 months in the community	-		19	(1.40%)	27	(1.99%)	808	(59.68%)	854	(63.07%)
Total	48	(3.55%)	44	(3.25%)	41	(3.03%)	1221	(90.18%)	1354	(100.00%)

Table 4: Self-report criminal offending from follow-up interviews one and two			
	Relative Frequencies		
Offense	0 times	1 time	Missing
Fight	42.51	57.38	0.12
Steal	73.65	26.00	0.35
Drive Drunk	74.82	24.71	0.47
Sell Marijuana	77.63	21.78	0.59
Carry gun	78.81	20.73	0.47
Destroy Property	80.80	18.74	0.47
Sell Other Drugs	81.50	18.03	0.47
Beat up	83.72	15.69	0.59
Rob without weapon	85.01	14.52	0.47
Shoplift	85.25	14.29	0.47
Joyriding	84.54	14.17	1.29
Gang Fight	87.59	11.83	0.59
Enter car to steal	89.34	10.19	0.47
Shot at	91.57	7.96	0.47
Rob with weapon	91.69	7.85	0.47
Steal car	93.56	5.97	0.47
Burgle	91.69	5.74	2.58
Illegal Credit	95.78	3.75	0.47
Set Fire	94.03	2.46	3.51
Carjack	97.42	2.11	0.47
N = 854			
Source: Pathways to Desistence, Follow-up interviews 1 & 2 combined			

Table 5: Demographic characteristics of analysis sample

Variable	Freq.	Relative Freq.	**Variable**	Freq.	Relative Freq.
Age			**Race**		
14	105	12.30	Black	301	35.22
15	176	20.61	White	204	23.91
16	252	29.51	Hispanic	314	36.77
17	264	30.91	Other	35	4.10
18	57	6.67			
Family Structure			**Gender**		
Two parent HH	320	37.47	Male	700	81.97
Other	534	62.53	Female	154	18.03

N = 854

Note: Hispanic and Other collapsed in regression analyses, and age was analyzed as a continuous variable after centering.

Table 6: Descriptive statistics for analysis sample

Variable	Minimum	Mean	Maximum	Std. Dev.
Variety Index	0.00	3.06	20.00	3.90
Impulse Control	1.00 (-2.09)	2.97 (0.00)	5.00 (2.15)	0.94 (1.00)
Suppression of Aggression	1.00 (-1.82)	2.77 (0.00)	5.00 (2.32)	0.96 (1.00)
Consideration of Others	1.00 (-2.96)	3.49 (0.00)	5.00 (1.80)	0.84 (1.00)
Future Outlook Inventory	1.00 (-2.43)	2.32 (0.00)	4.00 (3.09)	0.54 (1.00)
Full Self-Control Scale	(-2.55)	(0.00)	(2.60)	(1.00)
Perceived Disorder	1.00 (-1.70)	2.29 (0.00)	4.00 (2.25)	0.76 (1.00)
Unsupervised Routine Activities	1.00 (-2.69)	3.29 (0.00)	5.00 (2.01)	0.85 (1.00)
Peer Delinquency	1.00 (-1.39)	2.24 (0.00)	5.00 (3.11)	0.89 (1.00)
Social Capital	1.00 (-2.03)	2.11 (0.00)	4.00 (3.40)	0.55 (1.00)
Moral Disengagement	1.00 (-1.76)	1.61 (0.00)	3.00 (3.97)	0.35 (1.00)
Age	14.00 (-1.98)	15.99 (0.00)	18.00 (2.01)	1.13 (1.13)

Note: Standardized values in parentheses; Age variable is centered, not standardized
N = 854 for Perceived Disorder, Social Capital, Age
N = 853 for Impulse Control, Suppression of Aggression, Consideration of Others, Routine Activities, Moral Disengagement
N = 851 for Future Outlook Inventory
N = 850 for Variety Index
N = 836 for Peer Delinquency
N = 832 for all Standardized values except Future Outlook Inventory for which N = 830
Source: Pathways to Desistence

Table 7: Group comparisons: Disadvantage level by analysis variables

Level of Neighborhood Disadvantage								
Race/Ethnicity	Low		Medium		High		Total	
	f	%	*f*	%	*f*	%	*f*	%
White	87	41.83	87	20.91	23	11.06	197	23.68
Black	29	13.94	144	34.62	120	57.21	293	35.22
Hispanic	82	39.42	166	39.90	59	28.37	307	36.90
Other	10	4.81	19	4.59	6	2.88	35	4.21
$\chi^2_{[df = 6]}$ = 107.534, p <0.001, Cramer's V = 0.254								

Gender	Low		Medium		High		Total	
	f	%	*f*	%	*f*	%	*f*	%
Male	169	81.25	335	80.53	177	85.10	681	81.85
Female	39	18.75	81	19.47	31	14.90	151	18.15
$\chi^2_{[df = 2]}$ = 2.014, p > 0.10, Cramer's V = 0.049								

Two parent household	Low		Medium		High		Total	
	f	%	*f*	%	*f*	%	*f*	%
Yes	100	48.08	150	36.06	64	30.77	314	37.74
No	108	51.92	266	63.94	144	69.23	518	62.26
$\chi^2_{[df = 2]}$ =14.261, p <0.01, Cramer's V = 0.131								

Any crime	Low		Medium		High		Total	
	f	%	*f*	%	*f*	%	*f*	%
Yes	131	62.98	295	70.91	156	75.00	582	69.95
No	77	37.02	121	29.09	52	25.00	250	30.05
$\chi^2_{[df = 6]}$ = 7.514, p <0.05, Cramer's V = 0.095								

Continuous variables	Low		Medium		High			
	Mean	SD	Mean	SD	Mean	SD	F	R^2
Variety index	2.260	3.245	3.353	4.103	3.332	4.052	6.08*	0.014
Self-control [a]	0.035	0.994	-0.028	1.035	0.020	0.938	0.33^{ns}	<0.001
Unsupervised routine activities [a]	0.016	0.962	-0.017	0.942	0.019	1.143	0.12^{ns}	<0.001
Moral disengagement [b]	1.572	0.324	1.613	0.344	1.657	0.377	3.08*	0.007
Peer delinquency [b]	2.049	0.870	2.208	0.858	2.478	0.915	12.93*	0.030
Social capital [b]	2.014	0.529	2.123	0.549	2.224	0.567	7.65*	0.018
Age [b]	15.952	1.115	15.950	1.106	16.101	1.181	1.39^{ns}	0.003
Street Time [b]	9.784	3.073	9.424	3.060	8.245	3.438	14.08*	0.033
Recall Period Length	11.697	1.694	11.748	1.368	11.721	1.852	0.07^{ns}	<0.001

[a] Standardized variable

[b] Unstandardized variable analyzed in this table- standardized (or centered- Age) variables used in regression analyses

N = 832, * p < .05

Source: Pathways to Desistance

Table 8: Disadvantage level by number of risk factors

Number of risk factors	Low		Medium		High		Total	
	f	%	*f*	%	*f*	%	*f*	%
0	44	21.15	69	16.59	23	11.06	136	16.35
1	93	44.71	186	44.71	90	43.27	369	44.35
2	49	23.56	96	23.08	59	28.37	204	24.52
3	13	6.25	44	10.58	26	12.50	83	9.98
4	6	2.88	16	3.85	6	2.88	28	3.37
5	3	1.44	5	1.20	4	1.92	12	1.44
6	0	0.00	0	0.00	0	0.00	0	0.00

$\chi^2_{[df = 12]} = 13.719$, $p > 0.10$, Cramer's V = 0.091

	Low		Medium		High			
	Mean	SD	Mean	SD	Mean	SD	F	R^2
Risk factors	1.293	1.052	1.440	1.080	1.586	1.059	3.92*	0.009

N = 832, * $p < .05$

Source: Pathways to Desistance

Table 9: Bivariate estimates of the association between self-control and variety index by level of neighborhood disadvantage

Panel A: Slope Estimates

Level of Neighborhood Disadvantage	Variety Index Mean (SD)	Self-Control Mean (SD)	Correlation	OLS Slope (SE) [t]	Negative Binomial Slope (SE) [t]
Low	2.25 (3.25)	0.03 (1.00)	-0.259*	-0.842* (0.219) [-3.85]	-0.353* (0.096) [-3.69]
Medium	3.35 (4.10)	-0.03 (1.04)	-0.429*	-1.695* (0.175) [-9.67]	-0.570* (0.061) [-9.37]
High	3.33 (4.05)	0.02 (0.94)	-0.388*	-1.674* (0.277) [-6.04]	-0.541* (0.090) [-6.00]

Panel B: Slope Comparisons

	OLS slope estimates Medium disadvantage compared to:		Negative binomial slope estimates Medium disadvantage compared to:	
	Low disadvantage	High disadvantage	Low disadvantage	High disadvantage
Difference in *bs*	-0.853*	-0.021	-0.217*	-0.029
z value of difference	-3.043	-0.063	-1.910	-0.265

* p < .05

Table 10: Ordinary least squares regression models explaining variety index of offending by level of neighborhood disadvantage

Variable	Low disadvantage		Medium disadvantage		High disadvantage	
	Controls only	Full model	Controls only	Full model	Controls only	Full model
Self-Control	-	-0.341	-	-0.856	-	-0.686
	-	(0.242)	-	(0.200)	-	(0.296)
	-	[-1.41]	-	[-4.28]	-	[-2.31]
Male	1.085	1.020	0.610	0.542	1.228	1.291
	(0.528)	(0.529)	(0.442)	(0.433)	(0.673)	(0.666)
	[2.05]	[1.93]	[1.38]	[1.25]	[1.83]	[1.94]
Age	0.213	0.234	-0.152	-0.090	-0.511	-0.468
	(0.180)	(0.180)	(0.158)	(0.155)	(0.202)	(0.201)
	[1.19]	[1.30]	[-0.97]	[-0.58]	[-2.53]	[-2.33]
Black	-0.738	-0.582	-1.165	-0.887	-1.879	-1.591
	(0.634)	(0.642)	(0.489)	(0.483)	(0.781)	(0.783)
	[-1.16]	[-0.91]	[-2.39]	[-1.84]	[-2.40]	[-2.03]
Other	-0.932	-0.892	-0.623	-0.428	0.712	0.871
	(0.442)	(0.441)	(0.456)	(0.449)	(0.809)	(0.803)
	[-2.11]	[-2.02]	[-1.37]	[-0.95]	[0.88]	[1.08]
Street time	-0.304	-0.299	-0.126	-0.144	-0.092	-0.074
	(0.066)	(0.066)	(0.057)	(0.056)	(0.071)	(0.071)
	[-4.60]	[-4.54]	[-2.20]	[-2.55]	[-1.30]	[-1.05]
Unsupervised routine activities	0.571	0.578	0.831	0.772	1.141	1.048
	(0.218)	(0.218)	(0.190)	(0.187)	(0.208)	(0.209)
	[2.62]	[2.66]	[4.37]	[4.13]	[5.49]	[5.00]
Moral disengagement	0.424	0.275	0.653	0.317	0.591	0.374
	(0.234)	(0.257)	(0.191)	(0.203)	(0.224)	(0.241)
	[1.81]	[1.07]	[3.42]	[1.57]	[2.64]	[1.55]
Peer delinquency	0.681	0.605	1.407	1.141	0.308	0.217
	(0.227)	(0.233)	(0.202)	(0.208)	(0.245)	(0.246)
	[3.00]	[2.60]	[6.96]	[5.50]	[1.26]	[0.88]
Social capital	0.223	0.254	-0.052	0.020	-0.112	-0.016
	(0.213)	(0.213)	(0.185)	(0.182)	(0.238)	(0.239)
	[1.05]	[1.19]	[-0.28]	[0.11]	[-0.47]	[-0.07]
Two parent household	0.856	0.763	-0.740	-0.628	-0.098	-0.092
	(0.407)	(0.412)	(0.359)	(0.352)	(0.504)	(0.498)
	[2.10]	[1.85]	[-2.06]	[-1.78]	[-0.20]	[-0.19]
Constant	1.886	1.926	3.884	3.688	2.984	2.776
	(0.589)	(0.588)	(0.520)	(0.511)	(0.907)	(0.901)
	[3.20]	[3.27]	[7.47]	[7.21]	[3.29]	[3.08]
N	208		416		208	
F	7.78	7.29	18.26	18.97	11.58	11.25
R^2	0.28	0.29	0.31	0.34	0.37	0.39
RMSE	2.82	2.81	3.44	3.38	3.29	3.26

Table 11: Negative binomial regression models explaining variety index of offending by level of neighborhood disadvantage

Variable	Low disadvantage		Medium disadvantage		High disadvantage	
	Controls only	Full model	Controls only	Full model	Controls only	Full model
Self-Control	-	-0.213	-	-0.286	-	-0.133
	-	(0.102)	-	(0.065)	-	(0.095)
	-	[-2.09]	-	[-4.41]	-	[-1.40]
Male	0.623	0.571	0.407	0.369	0.538	0.576
	(0.242)	(0.241)	(0.145)	(0.143)	(0.229)	(0.230)
	[2.58]	[2.37]	[2.80]	[2.59]	[2.35]	[2.51]
Age	0.101	0.116	-0.015	0.003	-0.119	-0.110
	(0.078)	(0.078)	(0.051)	(0.050)	(0.061)	(0.061)
	[1.29]	[1.48]	[-0.29]	[0.06]	[-1.95]	[-1.81]
Black	-0.163	-0.084	-0.346	-0.262	-0.660	-0.601
	(0.264)	(0.265)	(0.151)	(0.149)	(0.225)	(0.226)
	[-0.62]	[-0.32]	[-2.30]	[-1.76]	[-2.93]	[-2.66]
Other	-0.413	-0.439	-0.344	-0.270	0.040	0.058
	(0.187)	(0.186)	(0.140)	(0.139)	(0.227)	(0.225)
	[-2.22]	[-2.36]	[-2.46]	[-1.95]	[0.18]	[0.26]
Street time	-0.147	-0.146	-0.048	-0.059	-0.036	-0.031
	(0.028)	(0.028)	(0.018)	(0.018)	(0.022)	(0.022)
	[-5.21]	[-5.19]	[-2.66]	[-3.32]	[-1.65]	[-1.42]
Unsupervised routine activities	0.262	0.277	0.303	0.302	0.423	0.396
	(0.093)	(0.092)	(0.061)	(0.060)	(0.069)	(0.071)
	[2.83]	[3.02]	[4.97]	[5.05]	[6.14]	[5.60]
Moral disengagement	0.221	0.125	0.329	0.224	0.128	0.086
	(0.091)	(0.101)	(0.060)	(0.063)	(0.071)	(0.076)
	[2.43]	[1.23]	[5.51]	[3.56]	[1.79]	[1.12]
Peer delinquency	0.258	0.217	0.464	0.357	0.167	0.148
	(0.097)	(0.099)	(0.063)	(0.066)	(0.077)	(0.077)
	[2.64]	[2.19]	[7.39]	[5.45]	[2.17]	[1.92]
Social capital	-0.026	-0.008	-0.042	-0.011	-0.092	-0.074
	(0.084)	(0.084)	(0.060)	(0.059)	(0.080)	(0.080)
	[-0.31]	[-0.10]	[-0.70]	[-0.19]	[-1.15]	[-0.92]
Two parent household	0.463	0.393	-0.229	-0.197	-0.011	0.002
	(0.174)	(0.176)	(0.112)	(0.110)	(0.151)	(0.150)
	[2.67]	[2.24]	[-2.05]	[-1.80]	[-0.08]	[0.01]
Constant	-2.302	-2.249	-1.494	-1.572	-1.713	-1.780
	(0.267)	(0.266)	(0.164)	(0.162)	(0.286)	(0.289)
	[-8.62]	[-8.47]	[-9.11]	[-9.72]	[-5.98]	[-6.17]
Alpha	0.769	0.747	0.704	0.651	0.567	0.547
	(0.146)	(0.143)	(0.083)	(0.079)	(0.102)	(0.101)
N	208		416		208	
χ^2	77.40	81.79	182.33	201.72	104.10	106.02
$p > \chi^2$	0.01	0.01	0.01	0.01	0.01	0.01

Table 12: Multivariate estimates of the association between self-control and variety index by level of neighborhood disadvantage

Panel A: Slope estimates

Level of neighborhood disadvantage	OLS slope	Negative binomial slope
Low	-0.341	-0.213*
Medium	-0.855*	-0.286*
High	-0.686*	-0.133

Panel B: Slope comparisons

	OLS slope estimates Medium disadvantage compared to:		Negative binomial slope estimates Medium disadvantage compared to:	
	Low disadvantage	High disadvantage	Low disadvantage	High disadvantage
Difference in *b's*	-0.515	-0.170	-0.073	-0.153
z value of difference	-1.641	-0.474	-0.602	-1.329

* $p < .05$ † $p < .10$

Table 13: Full sample negative binomial models explaining crime

	Model	Model	Model	Model	Model	Model	Model
Variable	A	B	C	D	E	F	G
Self-Control	-0.512	-0.512	-0.534	-	-0.267	-0.267	-0.324
	(0.045)	(0.044)	(0.062)	-	(0.047)	(0.047)	(0.060)
	[-11.50]	[-11.52]	[-8.59]	-	[-5.68]	[-5.67]	[-5.36]
Neighborhood disadvantage	0.148	0.145	0.159	-	0.102	0.102	0.124
	(0.044)	(0.044)	(0.044)	-	(0.043)	(0.043)	(0.043)
	[3.33]	[3.26]	[3.63]	-	[2.36]	[2.37]	[2.89]
Self-control X neighborhood disadvantage	-	-0.071	-0.073	-	-	0.006	-0.001
	-	(0.047)	(0.046)	-	-	(0.043)	(0.042)
	-	[-1.52]	[-1.58]	-	-	[0.15]	[-0.03]
Neighborhood disadvantage²	-	-	-0.100	-	-	-	-0.110
	-	-	(0.043)	-	-	-	(0.039)
	-	-	[-2.30]	-	-	-	[-2.83]
Self-control X neighborhood disadvantage²	-	-	0.033	-	-	-	0.072
	-	-	(0.046)	-	-	-	(0.040)
	-	-	[0.71]	-	-	-	[1.78]
Male	-	-	-	0.434	0.418	0.417	0.419
	-	-	-	(0.112)	(0.110)	(0.110)	(0.109)
	-	-	-	[3.89]	[3.81]	[3.80]	[3.84]
Age	-	-	-	-0.031	-0.014	-0.015	-0.012
	-	-	-	(0.036)	(0.036)	(0.036)	(0.036)
	-	-	-	[-0.86]	[-0.40]	[-0.42]	[-0.33]
Black	-	-	-	-0.422	-0.408	-0.409	-0.446
	-	-	-	(0.109)	(0.112)	(0.112)	(0.112)
	-	-	-	[-3.90]	[-3.65]	[-3.65]	[-3.98]
Other	-	-	-	-0.236	-0.231	-0.232	-0.250
	-	-	-	(0.101)	(0.099)	(0.010)	(0.100)
	-	-	-	[-2.35]	[-2.32]	[-2.32]	[-2.50]

Table 13 Continued							
Street time	-	-	-	-0.065	-0.066	-0.066	-0.072
	-	-	-	(0.013)	(0.012)	(0.013)	(0.013)
	-	-	-	[-5.08]	[-5.30]	[-5.30]	[-5.71]
Unsupervised routine activities	-	-	-	0.327	0.313	0.314	0.315
	-	-	-	(0.042)	(0.041)	(0.042)	(0.042)
	-	-	-	[7.72]	[7.54]	[7.50]	[7.57]
Moral disengagement	-	-	-	0.262	0.152	0.153	0.155
	-	-	-	(0.043)	(0.045)	(0.045)	(0.045)
	-	-	-	[6.12]	[3.36]	[3.36]	[3.46]
Peer delinquency	-	-	-	0.346	0.257	0.257	0.243
	-	-	-	(0.045)	(0.046)	(0.046)	(0.045)
	-	-	-	[7.77]	[5.64]	[5.64]	[5.34]
Social capital	-	-	-	-0.040	-0.019	-0.019	-0.015
	-	-	-	(0.043)	(0.042)	(0.042)	(0.042)
	-	-	-	[-0.93]	[-0.45]	[-0.46]	[-0.36]
Two parent household	-	-	-	-0.032	-0.005	-0.006	0.013
	-	-	-	(0.082)	(0.081)	(0.081)	(0.081)
	-	-	-	[-0.40]	[-0.07]	[-0.09]	[0.16]
Constant	-1.468	-1.470	-1.377	-1.690	-1.721	-1.720	-1.611
	(0.043)	(0.043)	(0.060)	(0.126)	(0.124)	(0.125)	(0.130)
	[-34.10]	[-34.22]	[-22.86]	[-13.43]	[-13.85]	[-13.80]	[-12.37]
Alpha	1.101	1.097	1.083	0.804	0.738	0.739	0.719
	(0.082)	(0.082)	(0.081)	(0.066)	(0.063)	(0.063)	(0.062)
χ^2	138.28	140.58	146.37	298.95	335.68	335.70	347.56
$p > \chi^2$	0.01	0.01	0.01	0.01	0.01	0.01	0.01
LR χ^2	-	2.30	5.79	-	-	0.02	11.85
p	-	0.13	0.05	-	-	0.89	<0.01
Recall period length entered as exposure variable in all models							

Table 14: Logistic regression predicting confinement for entirety of recall period in follow-up 1			
Variable	Slope	SE	z
Male	1.227*	0.237	5.17
Age	0.099	0.055	1.81
Black	1.052*	0.187	5.60
Other	0.416*	0.187	2.26
Neighborhood Disadvantage	0.224*	0.087	2.57
Two-parent Household	0.059	0.130	0.46
Intake Variety Index	0.069*	0.016	4.22
Constant	-5.1481*	0.919	-5.44
N = 1,308	Model $\chi^2 = 130.29$*	* $p < .05$	

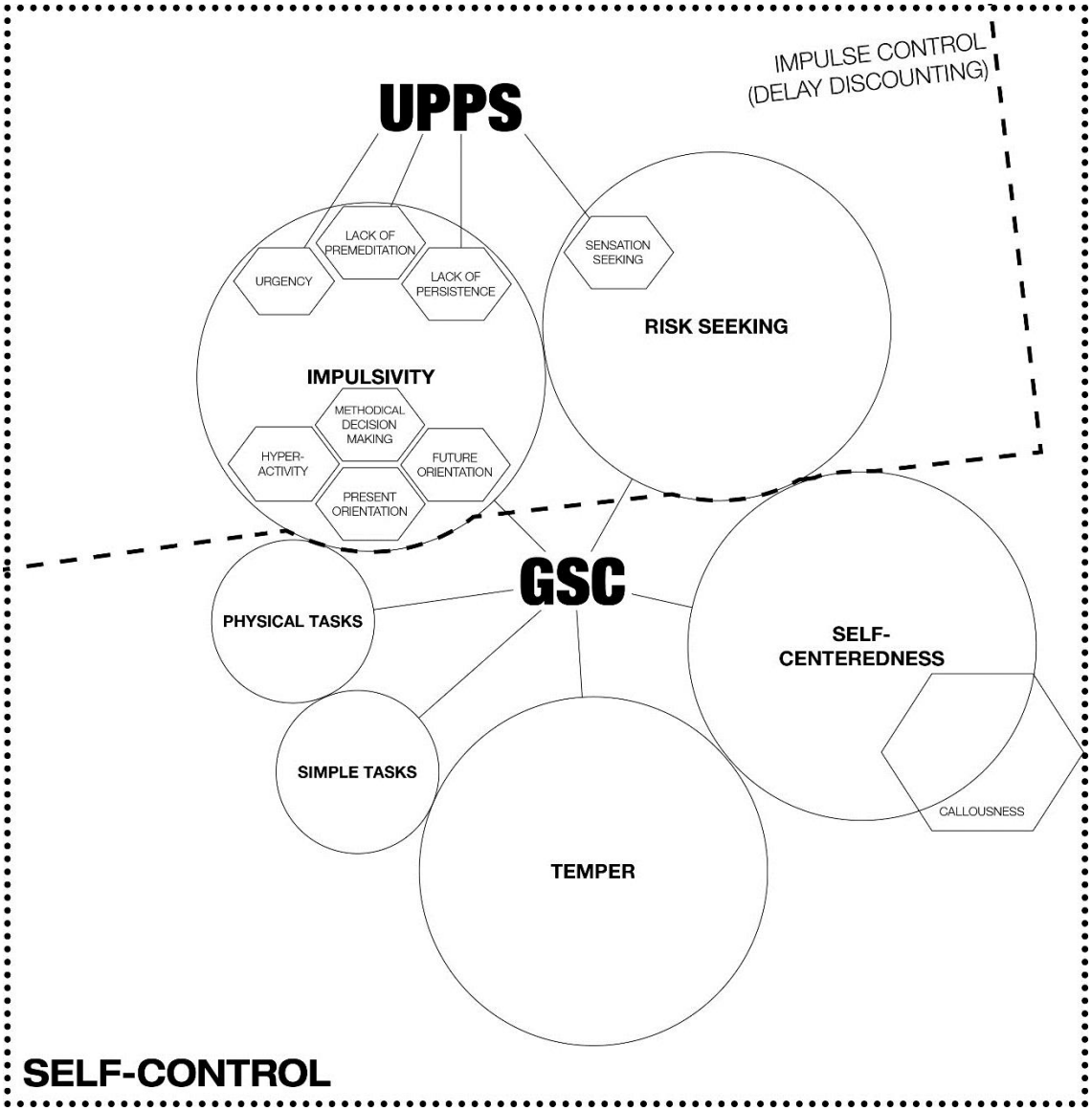


Figure 1: Concept map with facets and prior measures

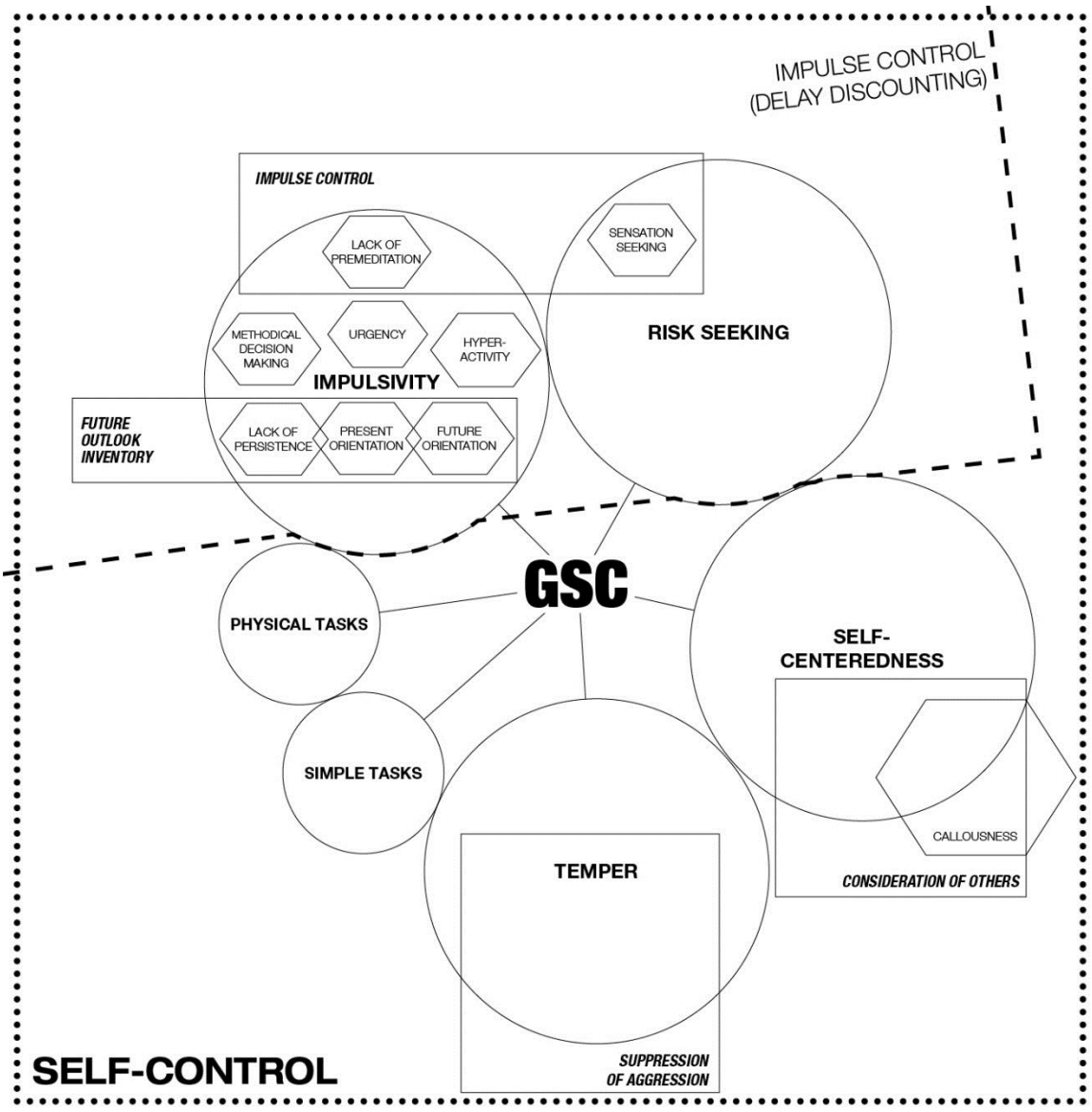


Figure 2: Measures of self-control dimensions relative to previously studied facets and the elements of self-control

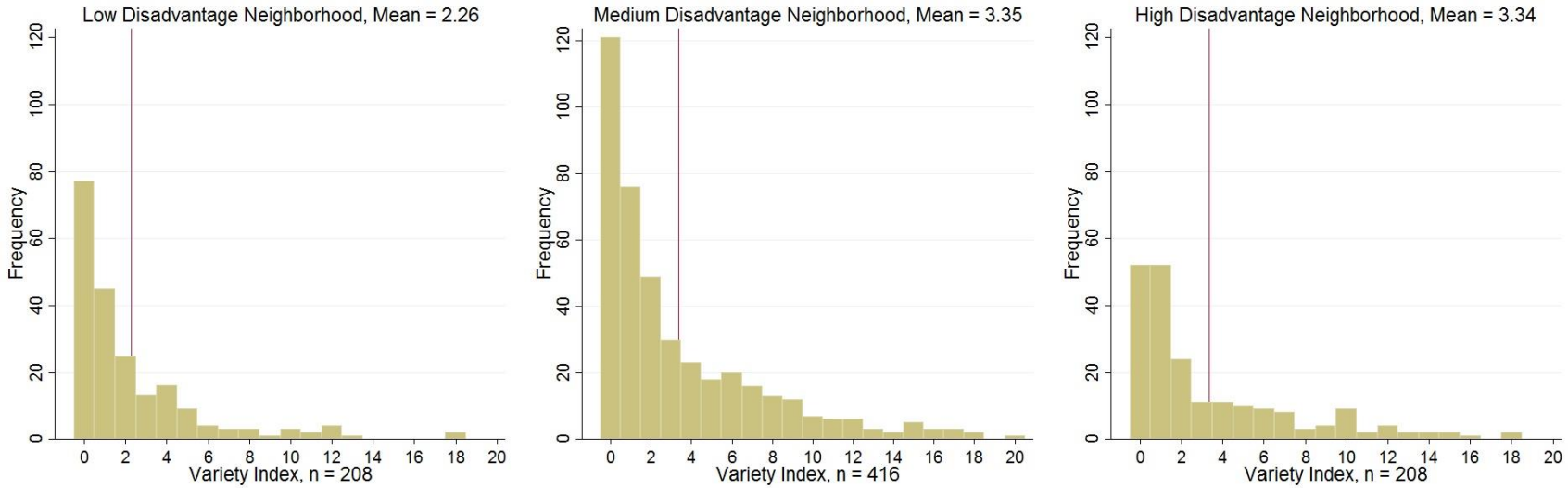


Figure 3: Variety index histograms by level of neighborhood disadvantage

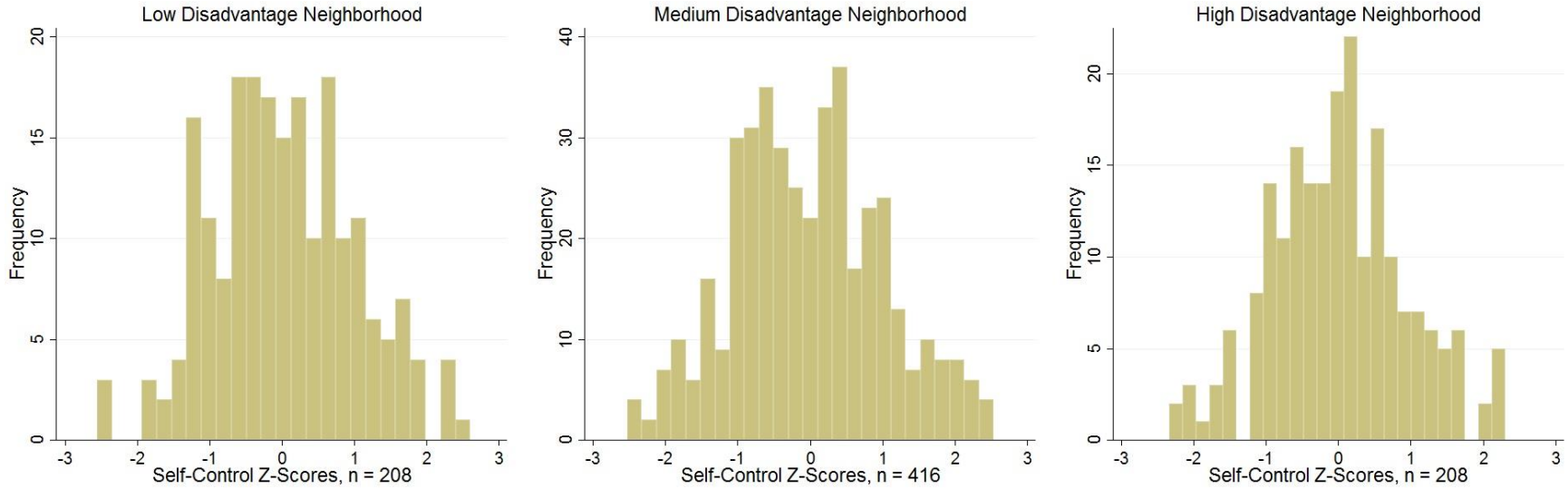


Figure 4: Self-control histograms by level of neighborhood disadvantage

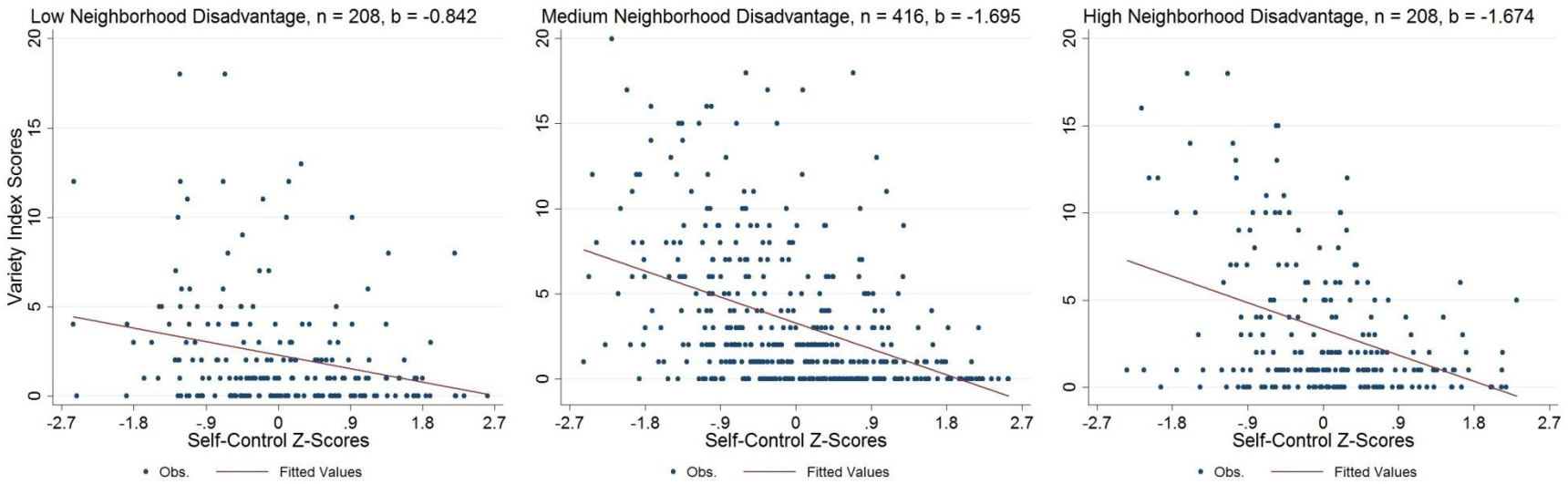


Figure 5: Scatterplots with fitted values of variety index by self-control by level of neighborhood disadvantage

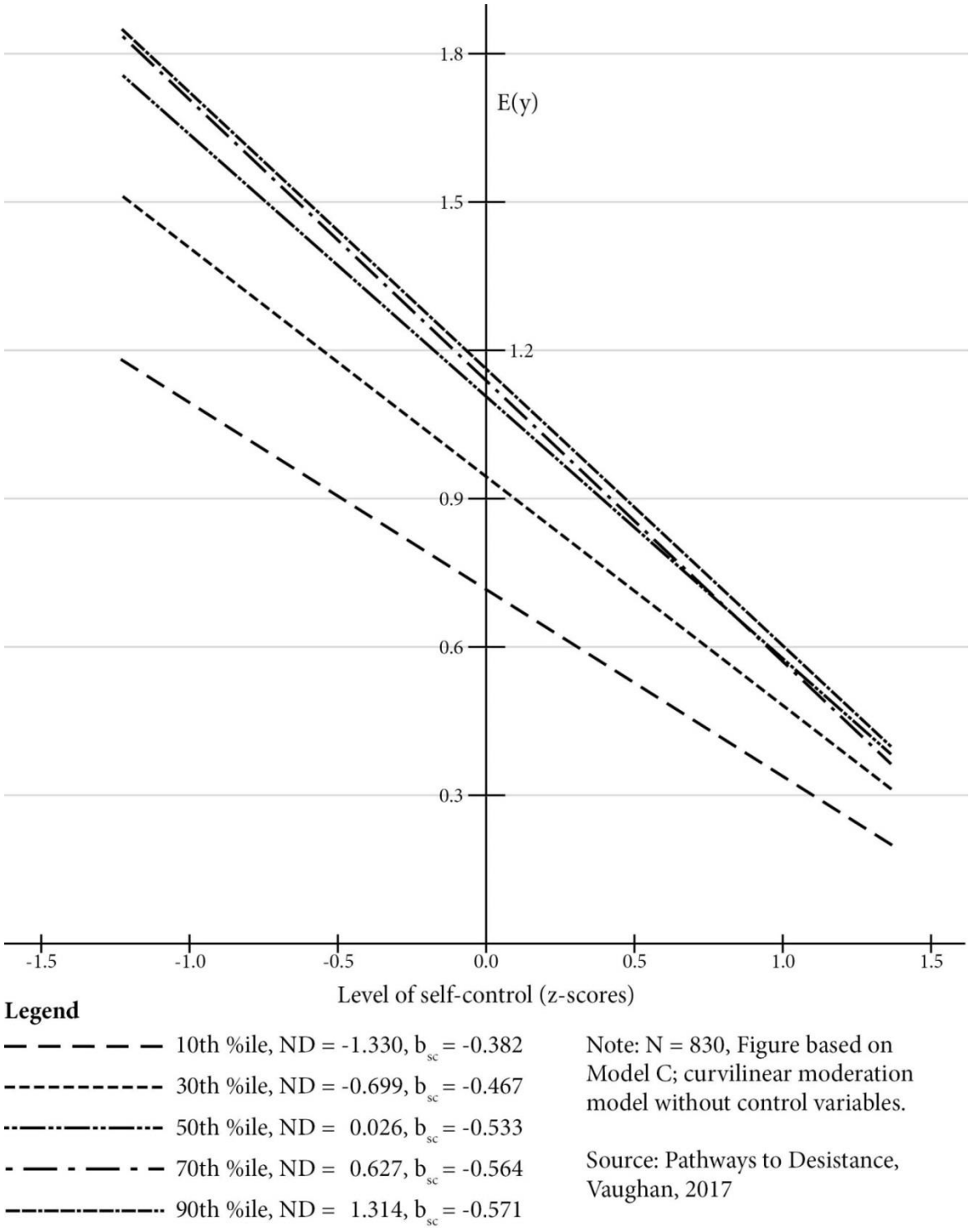
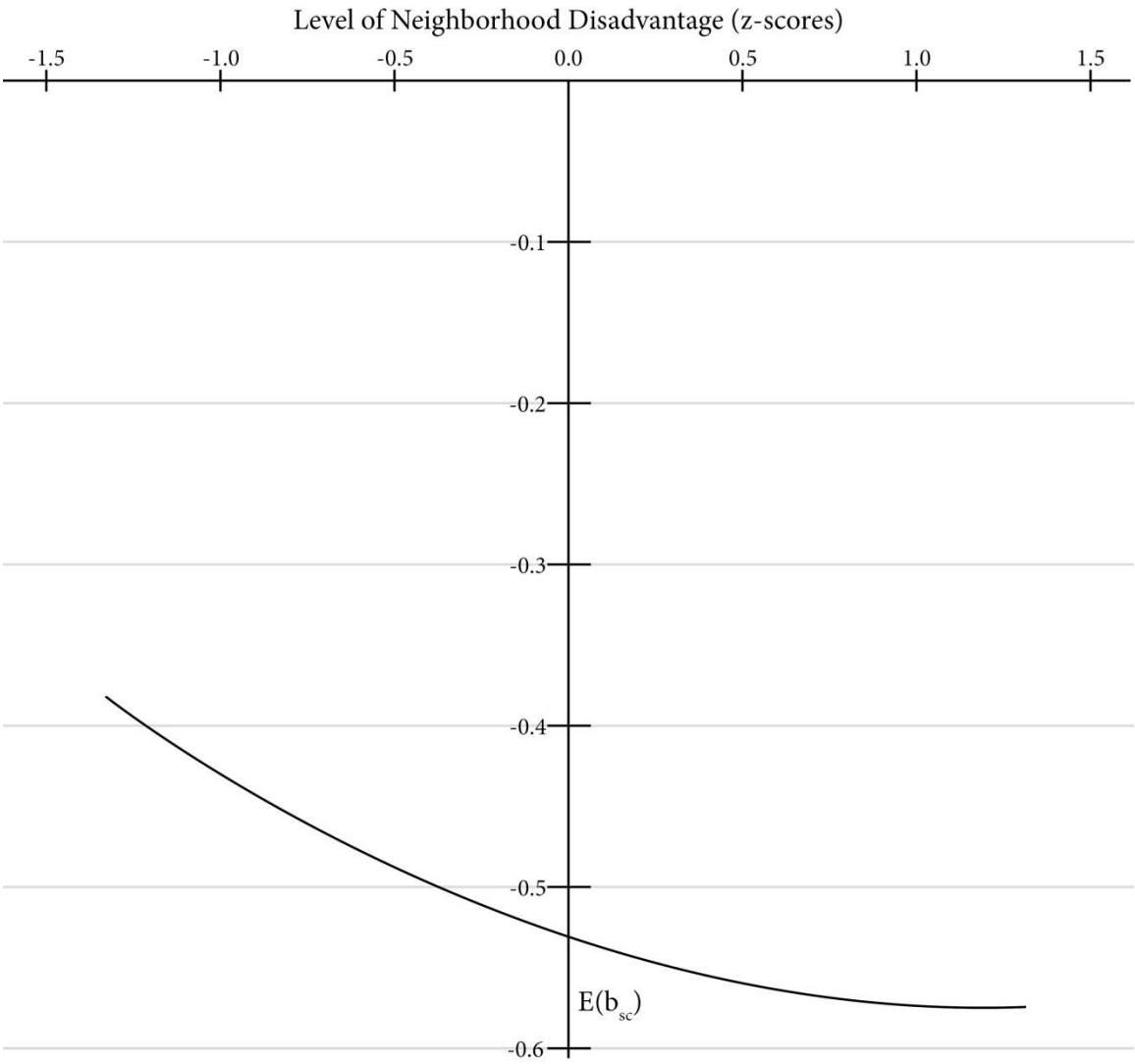


Figure 6: Predicted values of log count of crime types by level of self-control (x-axis) and level of neighborhood disadvantage (differentiated lines)-Based on Model C



Note: Line represents estimated slope value of self-control for a given level of neighborhood disadvantage (x-axis), ND = Neighborhood Disadvantage
N = 830, Figure based on Model C; curvilinear moderation model without control variables.
$b_{sc} = b_{sc}' + b_{scXnd}(ND) + b_{scXnd^2}(ND^2) = -0.534 - .073(ND) + .033(ND^2)$

Source: Pathways to Desistance, Vaughan, 2017

Figure 7: Predicted slope values for the effect of self-control by level of neighborhood disadvantage-Based on Model C

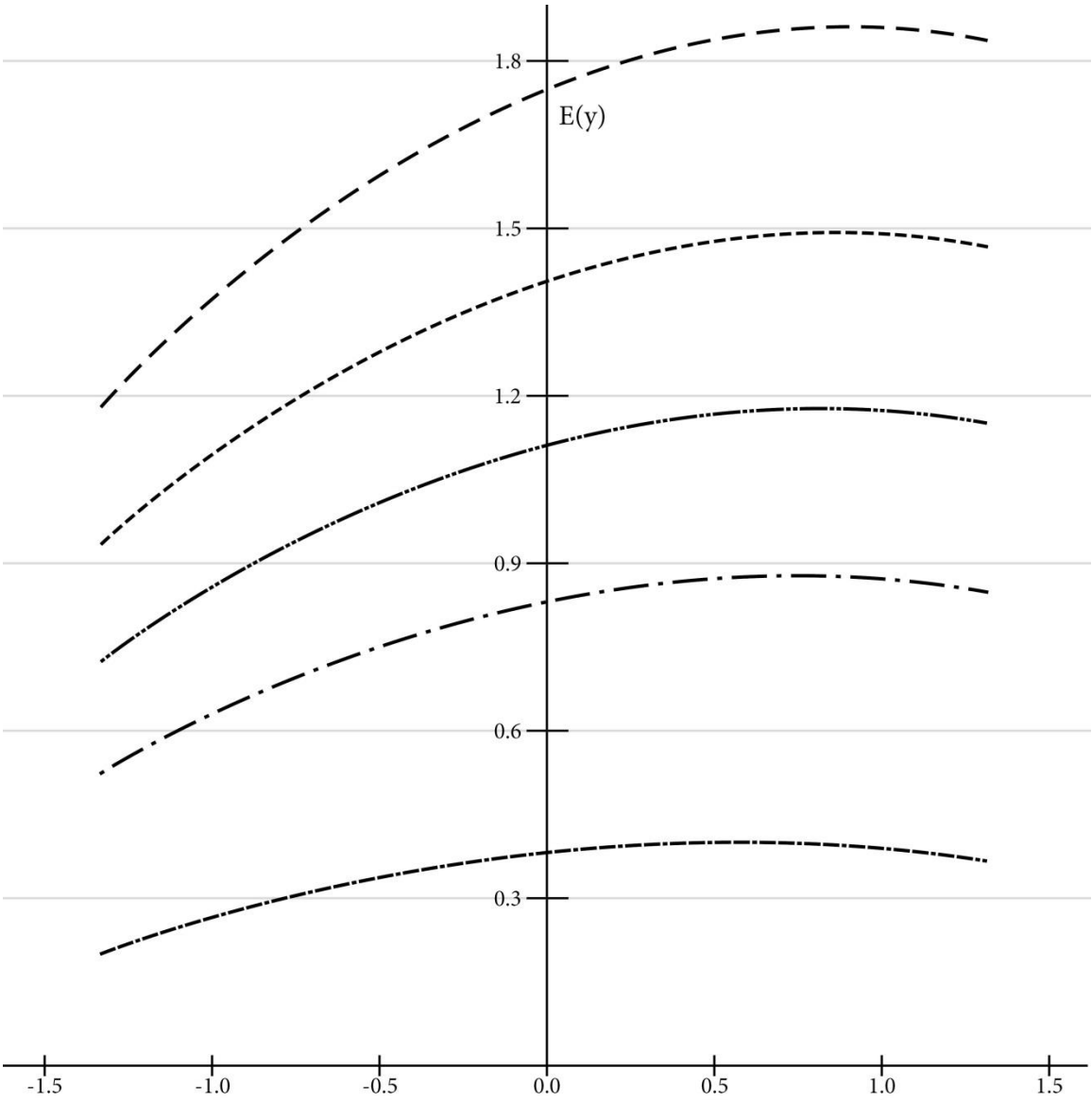


Figure 8: Predicted values of log count of crime types by level of neighborhood disadvantage (x-axis) and level of self-control (differentiated lines)-Based on Model C.

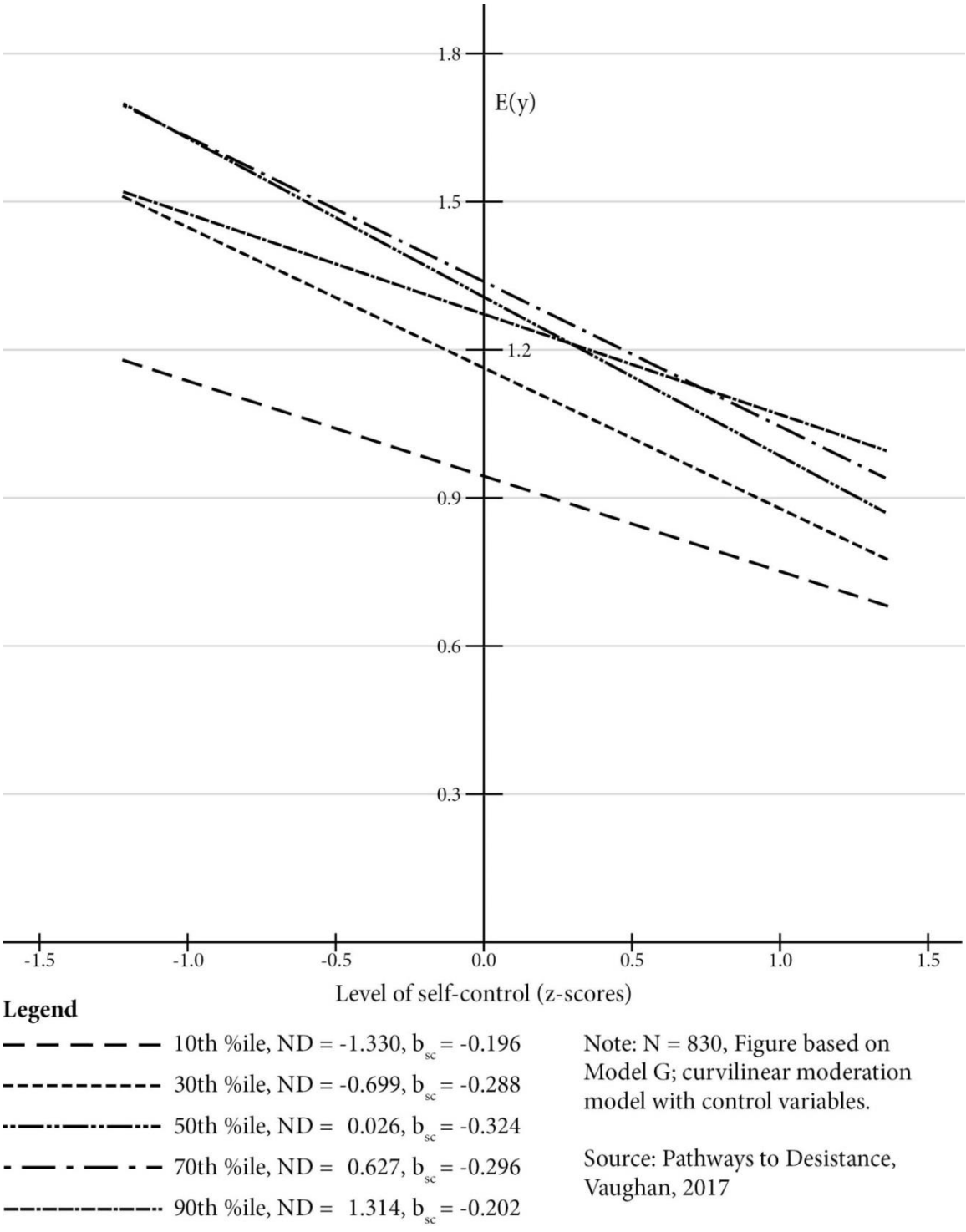
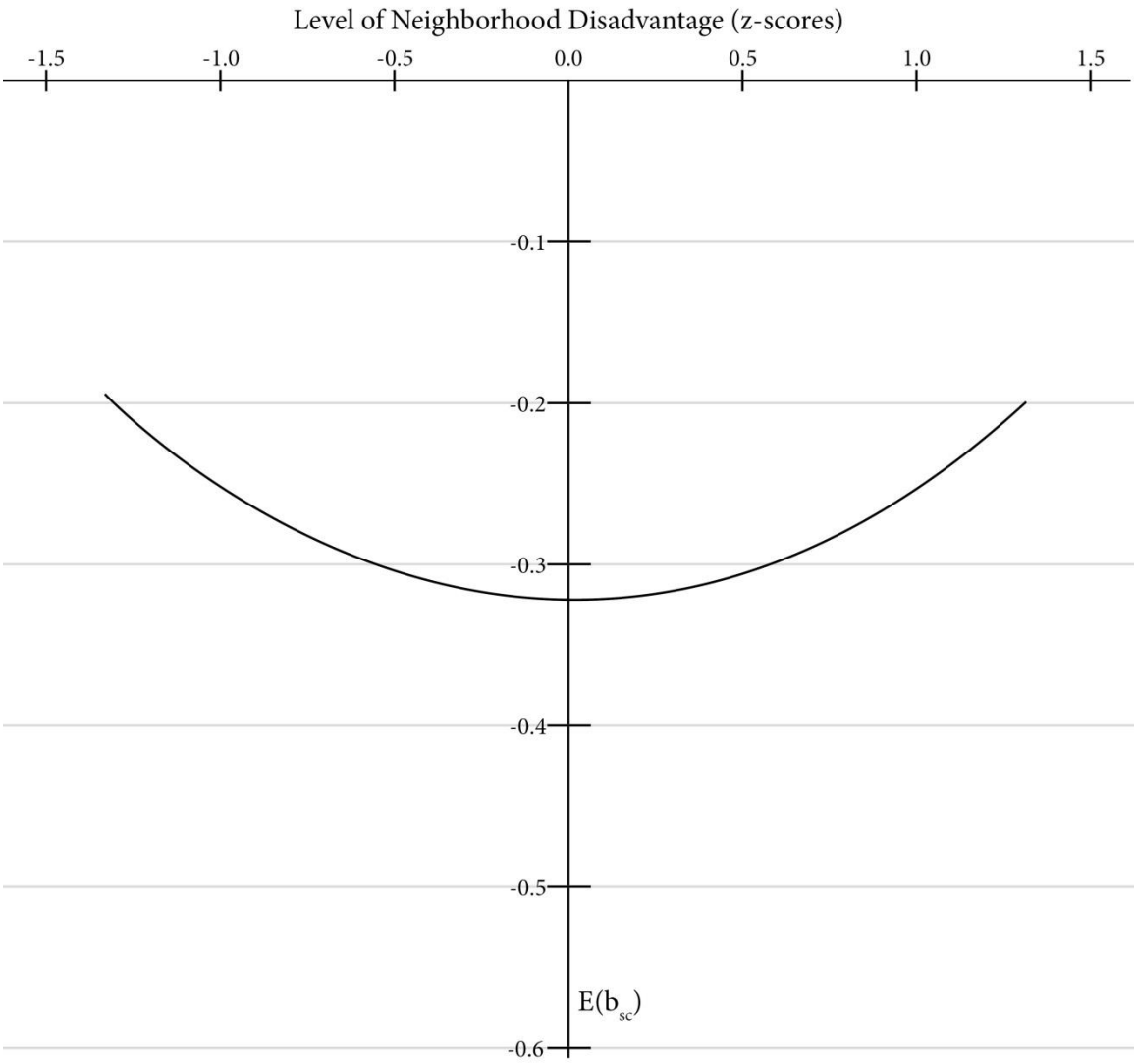


Figure 9: Predicted values of log count of crime types by level of self-control (x-axis) and level of neighborhood disadvantage (differentiated lines)-Based on Model G



Note: Line represents estimated slope value of self-control for a given level of neighborhood disadvantage (x-axis), ND = Neighborhood Disadvantage
N = 830, Figure based on Model G; curvilinear moderation model without control variables.
$b_{sc} = b_{sc}' + b_{scXnd}(ND) + b_{scXnd^2}(ND^2) = -0.324 - .001(ND) + .072(ND^2)$, assuming average white male with two parents.

Source: Pathways to Desistance, Vaughan, 2017

Figure 10: Predicted slope values for the effect of self-control by level of neighborhood disadvantage-Based on Model G

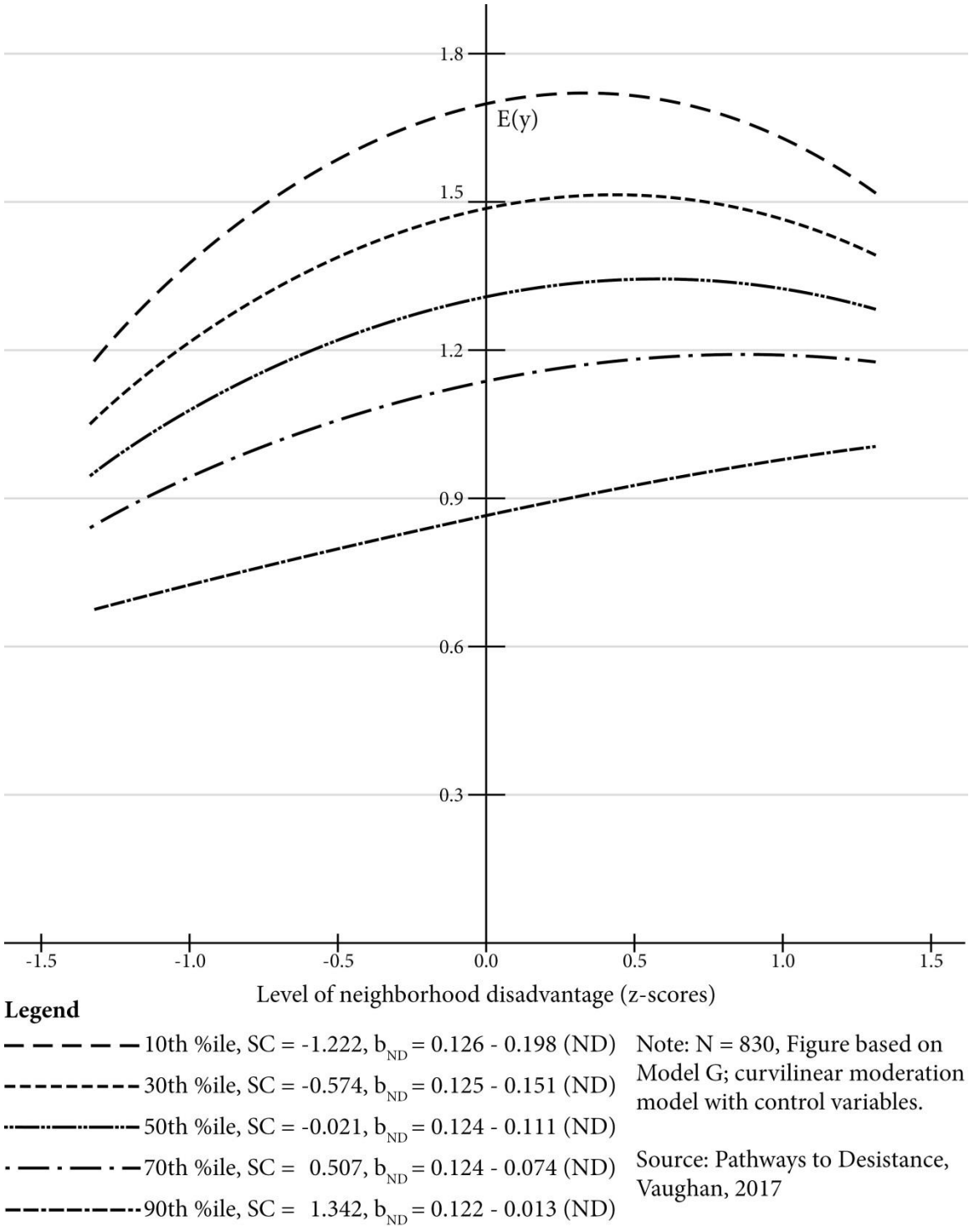


Figure 11: Predicted values of log count of crime types by level of neighborhood disadvantage (x-axis) and level of self-control (differentiated lines)-Based on Model G.

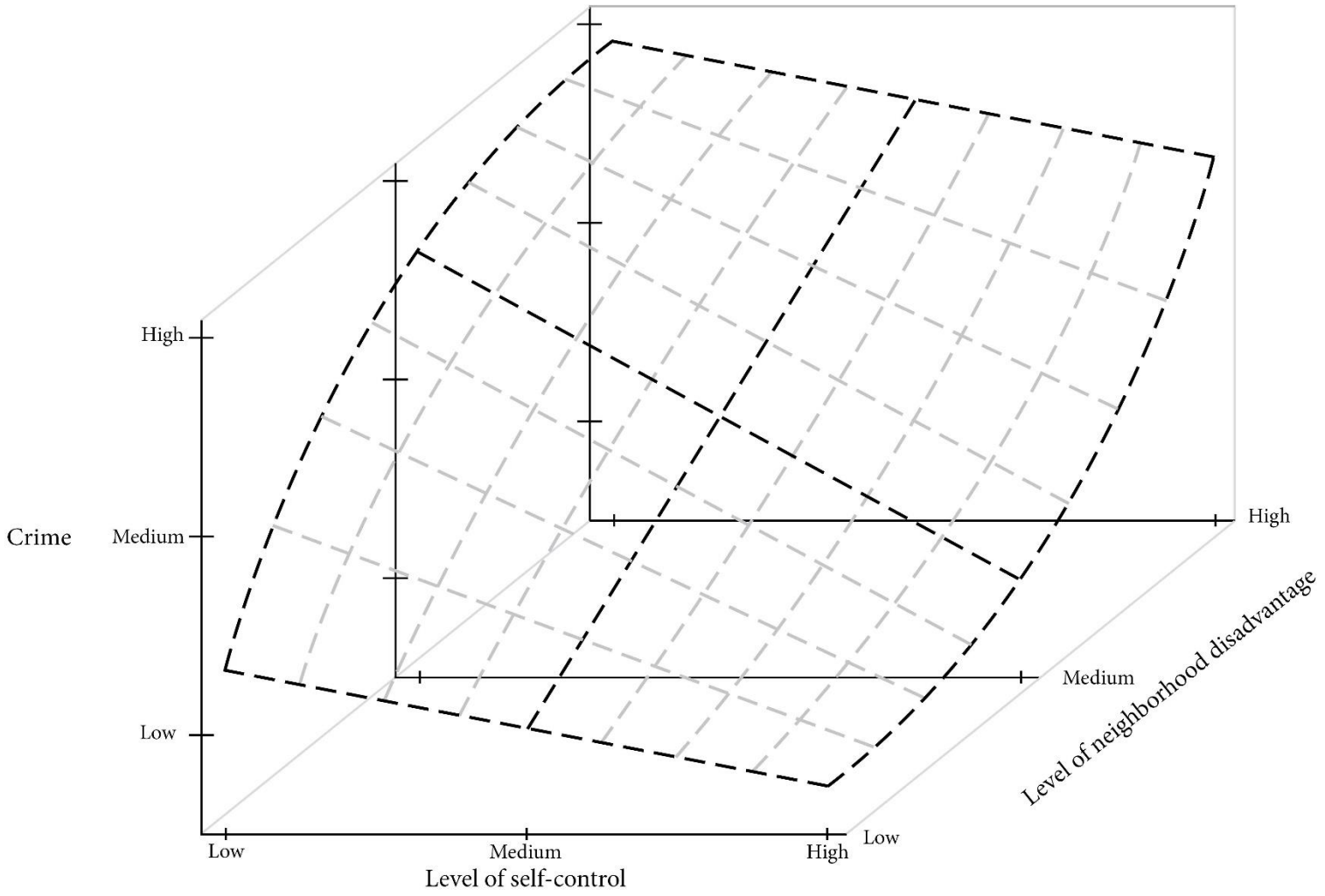


Figure 12: Crime by self-control and neighborhood disadvantage: Theory implied values

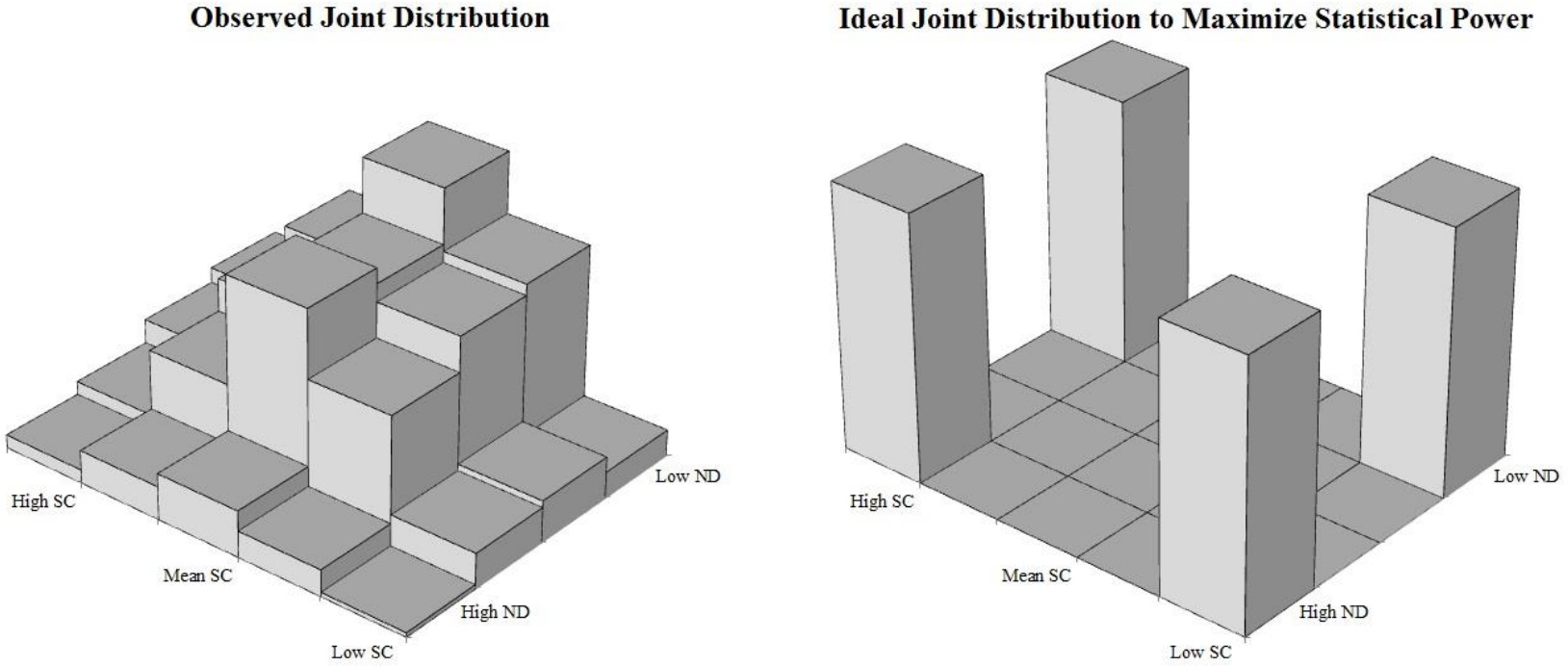


Figure 13: Joint distributions of neighborhood disadvantage and self-control: Observed and ideal distribution for maximizing statistical power and ability to detect interaction effects

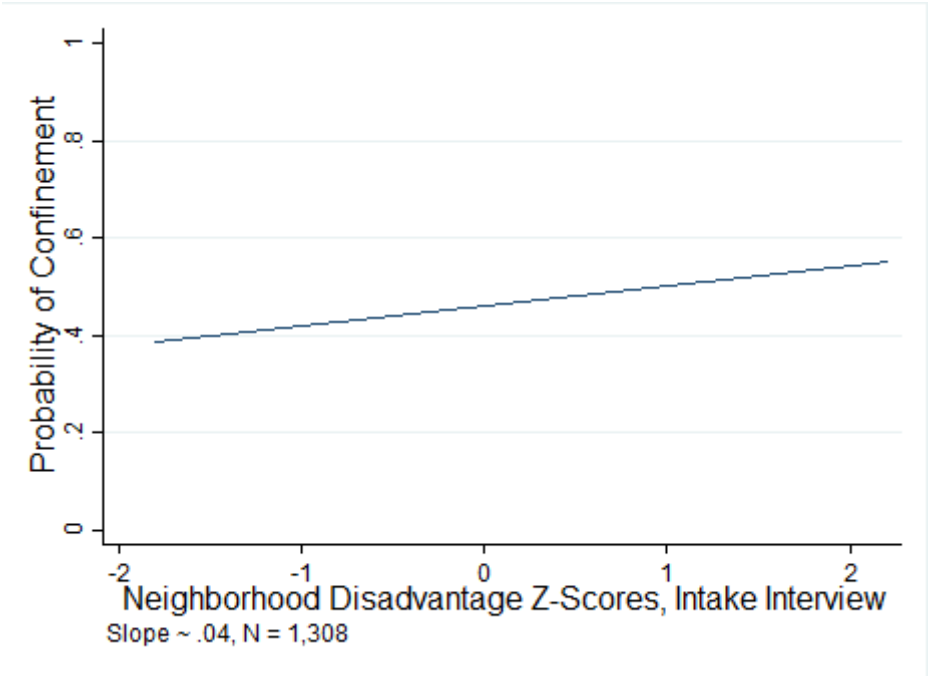


Figure 14: Probability of Confinement in Follow-up 1 by Intake Level of Neighborhood Disadvantage

Based on Model from Table 14. Values reflect expected probabilities for an average black male respondent with one parent.

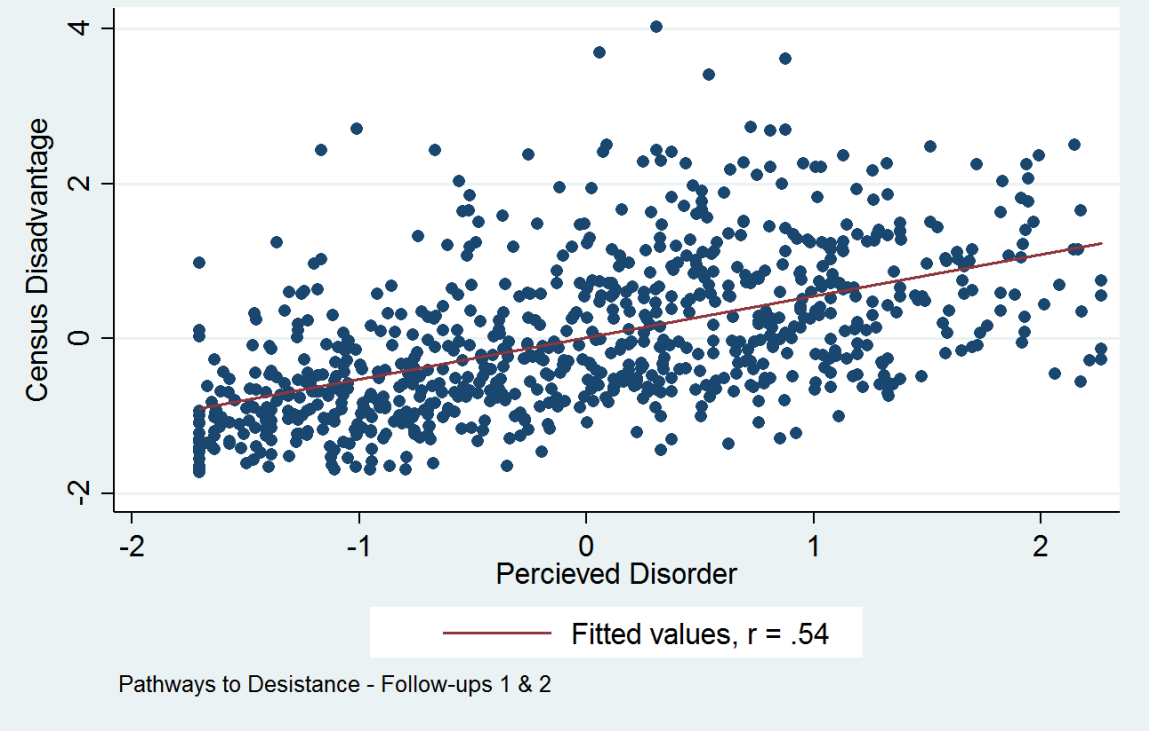


Figure 15: Scatterplot – Census disadvantage by perceived disorder with curve of best fit

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