UNIVERSITY OF CALIFORNIA,
IRVINE

Under the Radar or Under Arrest: How Does Contact with the Juvenile Justice System Affect Delinquency and Academic Outcomes?

DISSERTATION

submitted in partial satisfaction of the requirements
for the degree of

DOCTOR OF PHILOSOPHY

in Psychology and Social Behavior

by

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2014

Table of Contents

List of Figures

List of Tables

Acknowledgements

First and foremost, I owe my deepest appreciation and gratitude to my mentor and committee chair, Professor Elizabeth Cauffman. Throughout my tenure in graduate school, Professor Cauffman has been an unwavering source of academic, emotional, and moral support. This dissertation would not have been possible without her guidance on every aspect of this project.

I would also like to express sincere gratitude to my committee members, Professor AnneMarie Conley and Professor Jodi Quas, for providing helpful feedback on previous drafts and for their help in the design and framing of this project. I would also like to thank Professor Chuansheng Chen and Professor Candice Odgers for serving on my advancement committee and for their help in the design, conceptualization, and plan of analysis for this project. I am also grateful for my wonderful undergraduate research assistants, without whom data collection would never have finished in time.

This project also would not have been possible without the generous support from the National Science Foundation, National Institute of Justice, American Psychology-Law Society, Alison Clarke-Stewart Dissertation Grant, and the University of California, Irvine. I am very grateful for their support.

I also would like to thank the Crossroads study investigators, Professor Cauffman, Professor Paul Frick, and Professor Laurence Steinberg, for generously allowing me to use a portion of their data for this project.

Finally, I heartily thank my family and friends for their steadfast and unconditional love and support.

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Bechtold, J., (2014). *Adolescence: An instructor's manual to accompany Steinberg's Adolescence* (10th ed.). New York, NY: McGraw-Hill, Inc.

Bechtold, J., Cauffman, E., & Monahan, K. (2011). *Are minority youth treated differently in juvenile probation?* Chicago, IL: John D. & Catherine T. MacArthur Foundation Models for Change Research Initiative.

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Manuscripts under review

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Manuscripts in progress

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Shulman, E. P., Boessen, A., **Bechtold, J**., & Cauffman, E. (2009, March). *Iatrogenic effects of incarceration for first time juvenile offenders.* Paper session at the annual meeting of the American Psychology-Law Society, San Antonio, TX.

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SERVICE

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Abstract of the Dissertation

Under the Radar or Under Arrest: How Does Contact with the Juvenile Justice System Affect Delinquency and Academic Outcomes?

By

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Doctor of Philosophy in Psychology and Social Behavior

University of California, Irvine, 2014

Professor Elizabeth Cauffman, Chair

Although many studies have found that arrested youth are more likely than non-arrested youth to experience later maladjustment, methodological limitations restrict the generalizations of prior work. Perhaps the most noteworthy limitation in prior work is the possibility of selection effects, with arrested youth likely to have very different psychological and behavioral profiles pre-justice system contact than non-arrested youth. This leaves us wondering whether the observed maladjustment is due to the type of adolescent who comes to the attention of law enforcement or due the type of justice system interventions that arrested youth experience.

This study overcomes these limitations by comparing the outcomes of demographically similar male adolescents who have committed the same crimes but who differ with regard to whether they were "caught" for their crimes. Using propensity score matching to compare arrested and non-arrested youth, I investigated whether contact with the justice system does, in fact, contribute to school-related outcomes, substance use, and delinquency and whether these relations vary based on whether arrested youth are formally processed or diverted from the system.

When selection effects are taken into consideration, results indicate that contact with the juvenile justice system does not have a universally harmful effect on development. Diversion (informally processing youth) actually deters future offending, school misconduct, school truancy, and school suspensions. However, both diverted and formally processed youth, regardless of their actual antisocial and illegal behavior, are more likely than no-contact youth to be arrested during the study period, according to official court records. The risk of re-arrest is highest for formally processed youth. Formally processed youth are also more likely than no-contact and diverted youth to be transferred to an alternative or continuation school.

Taken together, results suggest that increased justice system surveillance might improve school performance and deter offending, but it also might lead to more contact with the system. Although an adolescent's first arrest might lead to positive outcomes in the immediate future, the effects of subsequent contacts are unknown. As such, the data suggest that the default policy should be to divert low-level first-time offenders and keep the justice system's involvement to a minimum.

I. Research Objectives and Rationale

Study Overview

The proclivity to engage in antisocial, illegal, and violent behavior increases sharply between childhood and adolescence and declines thereafter (e.g., Gottfredson & Hirschi, 1987; Sampson & Laub, 1993). Indeed many American adolescents self-report engaging in at least one behavior for which they could be arrested (Farrington, 2009). Although some youth who engage in unlawful behavior are prosecuted by the justice system, there is a substantial proportion of youth who engage in the same illegal behaviors but are never arrested (Erickson & Empey, 1963; Farrington, Jolliffe, Hawkins, Catalano, Hill, Kosterman, 2003; Gold, 1966; Short & Nye, 1958). While minor delinquency in adolescence is typically transient and exploratory, it has been suggested that, notwithstanding limitations in prior work, youth who become ensnared in the justice system are likely to suffer serious maladjustment (e.g., Moffitt, 1993). In this study, I use a unique sampling method to ask whether and why adolescents who are arrested have worse outcomes than their peers who violate the same laws but are never arrested. I also investigate whether this effect is magnified based on characteristics of the youth or characteristics of the justice system experience.

These aims were investigated in a sample of delinquent youth who engaged in the same types of illegal activity and were either: (1) "formally processed"—required to make a court appearance and stand before a judge; (2) "informally processed"—arrested but diverted from the justice system and given the option to have all charges dismissed if conditions were satisfied; or (3) never arrested ("no-contact" youth)—youth who were never caught for their crimes. A control group of adolescents who have engaged in illegal behavior but never been arrested is possible given that approximately 60-80% of American adolescents self-report engaging in some

form of delinquency (Moffitt, 1993) but only 16-27% are arrested for a non-traffic violation by the age of 18 (Brame, Turner, Paternoster, & Bushway, 2012). Indeed much adolescent illegal behavior is either unreported or undetected by law enforcement (Black & Reiss, 1970; Farrington, Loeber, & Stouthamer-Loeber, 2003; Kraus & Hasleton, 1982). Undetected criminal behavior is especially apparent when U.S. Department of Justice (DOJ) victimization data is compared to Uniform Crime Rates (UCR), with data suggesting that most victims do not report their perpetrator (i.e., UCR are drastically lower than NCVS [National Crime Victimization Survey [NCVS] rates; U.S. Department of Justice, 2000).

In conjunction with the statistical analysis, the inclusion of delinquent youth who were and were not caught, and the inclusion of youth who received different types of justice system sanctions, helps isolate the effect(s) that different degrees of juvenile justice experiences has on adolescent development. Specifically, the study investigates *whether* and, if so, *how* contact with the system is related to subsequent delinquency, substance use, and academic outcomes. I also ask whether younger youth are more negatively affected by contact with the justice system than older youth.

The issue of juvenile justice system contact is of substantial importance: In any year, over 2.3 million arrests involve juveniles (US Federal Bureau of Investigation, 2004), over 1.5 million cases are handled in delinquency courts (Puzzanchera & Kang, 2011), 350,000 minors are housed in detention facilities (Adams & Puzzanchera, 2007), and 370,000 minors are placed on probation (Puzzanchera & Sickmund, 2008). Indeed official data suggest that about 9% of American males between 10 and 17 are arrested every year (OJJDP, 2009). Although some of the most salient tenets of the juvenile justice system are to rehabilitate youth and reduce recidivism (e.g., Steinberg & Schwartz, 2000), many empirical studies suggest that contact with

the juvenile justice system may not universally achieve these ideals. This study builds on prior work by not only looking at whether contact is related to maladjustment, I also investigate whether this effect is moderated by the age of youth, and whether the effect of the justice system is mediated by individual and contextual variables.

II. Review of the Literature

Theoretical Foundations

Deterrence theory. Deterrence theory posits that formal sanctions (i.e., strict, harsh, punitive responses) are effective means of preventing and controlling illegal behavior (Morris, 1966; Zimring & Hawkins, 1973). There are two distinct aspects of this theory: specific deterrence (sanctioned youth are personally affected by harsh treatment) and general deterrence (non-sanctioned youth are vicariously affected by harsh treatment imposed on others). Specific deterrence suggests that individuals who experience harsh sanctions will desist from (or at least reduce) criminal behavior. Supporters of this theory argue that the juvenile justice system should intervene early and punish first-time offenders with harsh sanctions in an effort to prevent future, often escalating, crime. However, many examples that have been used to support specific deterrence theory have been derived from national averages (i.e., aggregate data). For example, Fabelo (1995) found support for deterrence theory by using national averages to show that incarceration rates had increased 30% while crime rates had decreased 5% during the same five-year period. Although aggregate data can be informative, these data should be interpreted with caution, as causality cannot be inferred from correlational studies and individual-level interpretations cannot be drawn from aggregate-level data (also known as the *Ecological Fallacy;* Robinson, 1950). For example, there are many outside factors that might influence both crime rates and incarceration rates (e.g., national budgets).

General deterrence suggests that witnessing others experience punitive or certain treatment by the justice systems will prevent individuals from committing similar crimes out of fear that they too will be punished (Ernest Van den Haag, 1982; Andenaes, 1974). However, research that has tracked changes in transfer to adult court policies has indicated that broader transfer laws (i.e., laws that allow more individuals to be transferred to adult court, which would be a "stricter" policy) do not prevent subsequent serious juvenile delinquency (Jensen & Metsger, 1994; McGowan, Hahn, Liberman, Crosby, Fullilove, Johnson, Moscicki, Price, Snyder, Tuma, Lowy, Briss, Cory, & Stone, 2007). Nonetheless there is some evidence in support of the effectiveness of deterrence theory in specific situations. For example, the threat of arrest may deter property crimes (Kohfeld & Sprague, 1990) and neighborhoods with certain and predictable punishments may have lower crime rates than neighborhoods with less predictable law enforcement policies (Klepper & Nagin, 1989).

Labeling theory. Like deterrence theorists, labeling theorists posit that the juvenile justice system's response to juvenile delinquency has profound effects on subsequent behavior. However, in contrast to deterrence theory, labeling theory proposes that the effect will be in the opposite direction, particularly that contact with the juvenile justice system will have negative effects on behavior. Specifically, labeling theory suggests that involvement with the justice system will create stigmas and deviant self-identities that will lead to continued or escalating illegal behavior (Becker, 1963; Lemert, 1951). Part of this effect is due to youth self-identifying as delinquents, however, part of this is due to the stigmatizing effect of official sanctions and the ensuing legitimate reductions in social opportunities (Becker, 1963; Lemert, 1951).

Self-identifying as delinquents. Labeling theory suggests that youth who become embedded in the juvenile justice system may develop delinquent self-concepts (Lemert, 1967)

and may start acting in ways consistent with this identity (Matsueda, 1992). Although this theory originated in the fields of sociology and criminology, this idea is consistent with what researchers have learned from developmental and social psychology. It has long been known that identity formation is an important developmental task of adolescence (e.g., Erikson, 1968). This is due in part to the cognitive advances during this period, including increases in the ability to think about the future (Steinberg, Graham, O'Brien, Woolard, Cauffman, & Banich, 2009) and the ability to imagine what life may be like if various identities are adopted ("possible selves" Markus & Nurius, 1986). Identity is not solidified until early adulthood, which leaves adolescents with inchoate identities and a tendency to experiment with various identities, roles and self-conceptions (Steinberg, 2011)—including those defined by risky and illegal behavior. As identity is fluid and in flux during adolescence, identity in adolescence may be particularly sensitive to external influences. Insofar as people are motivated to behave in ways that are consistent with their beliefs (see Leon Festinger's theory of cognitive dissonance; Festinger, 1962), involvement with the juvenile justice system may lead adolescents to adopt delinquent identities and this may cause them to behave in ways consistent with this identity, such as engaging in criminal behaviors and not pursuing academic success.

In addition to reducing the dissonance between youth's perceived identity and their behavior, youth may be motivated, either consciously or unconsciously, to conform to the expectations of others. Social relationships are excessively salient during adolescence, producing a heightened sensitivity to the opinions of others. To the extent that others may perceive juvenile offenders as "criminals," youth may be aware of these stereotypical expectations and they may behave in ways to confirm these beliefs (e.g., looking glass self; Pygmalion effect; Harter, Stocker, & Robinson, 1996; Rosenthal & Jacobson, 1968).

Finally, as a result of adopting a deviant identity, youth may reject conventional, pro-social behaviors and people (Kaplan & Fukurai, 1992) and may seek out friendships with peers who are equally stigmatized (i.e., other delinquents). Alternatively, pro-social peers may reject individuals typecast as delinquents.

***Reductions in conventional opportunities*.** Compared to non-delinquent youth, individuals who have been sanctioned by the juvenile justice system (e.g., arrested, convicted, incarcerated) may perceive or experience reductions in access to conventional resources (e.g., school related opportunities, part-time jobs, extracurricular clubs and sports; Matsueda, 1992; Moffitt, 1993). In addition, not only may delinquent youth reject pro-social conventions, but social peers may reject delinquent adolescents. Juvenile justice system involved individuals may experience differential treatment by educational institutions. For example, delinquent youth may be segregated from non-delinquent students and required to attend specific programs or classes, which may reduce opportunities to form positive relationships. Schools may even push delinquent youth into continuation or alternative schools. Unfortunately, many studies have shown that grouping like-minded delinquents together may have unintended consequences ("deviancy training", discussed later; Dishion, Spracklen, Andrews, Patterson, 1996). As a result, labeled individuals may continue engaging in illegal behaviors and demonstrate diminished achievement in academic and occupation domains.

The influence of context. Dating back to at least 1970s, developmental psychologists have recognized that adolescent development cannot be fully understood without a consideration of the contexts to which individuals are exposed (Bronfenbrenner, 1979). Indeed there are many environments that are known to have harmful effects on development and behavior in adolescence. Environmental risk factors may stem from the adolescent's family (e.g., marital

conflict, abusive or neglectful parenting; Chassin, Hussong, Beltran, 2009; Davies & Lindsay, 2004; Dishion, Capaldi, & Yoerger, 1999; Dobkin, Tremblay, & Sacchitelle, 1997), peers (e.g., antisocial peers, peer group identity, rejection by peers; Farrington, 2009; Chassin et al., 2009; Savin-Williams & Berndt, 1990; Veronneau, Vitaro, Brendgen, Dishion, & Tremblay, 2010), neighborhood and community (e.g., poverty, availability of drugs, enforcement of laws; Chassin et al., 2009; Leventhal, Dupéré, & Brooks-Gunn, 2009; Stanley, Henry, & Swaim, 2011;).

What is unknown, however, is whether (and how) the context of an adolescent's first contact with the juvenile justice system is related to adverse outcomes. It has been suggested that involvement with the justice system (i.e., long-term incarceration sentences) may stifle normative development (Steinberg, Chung, Little, 2004). In fact, research has shown that incarceration in adolescence is strongly related to academic failure, adult unemployment, and reduced adult earnings (Fagan & Freeman, 1999; Ward & Tittle, 1993; Wolfgang, Thornberry, & Figlio, 1987), although it is unclear who the appropriate comparison group would be in studies that look at the effects of incarceration. Youth who commit crimes that lead to long stays in secure facilities are likely very different than youth who are never incarcerated. As such, it is important to bear in mind that juvenile incarceration and adult outcomes could both be caused by the same measured or unmeasured third variable, which could lead to a spurious relation between incarceration and adult outcomes. Although research has indicated that incarceration may have important, and irreversible, effects on adult outcomes, it is less clear how less serious involvement with the justice system (i.e., arrest, court appearance, supervised probation) is related to more proximal outcomes, such as academic performance and academic engagement, substance use, and illegal behavior. In an effort to reduce the risk of selection effects and biased treatment effects, one study matched incarcerated and non-incarcerated youth pre-confinement

within depressive and anxious symptomatology trajectory groups on 26 potential confounding variables. In this study, the researchers found that incarceration did not exacerbate internalizing symptomatology (White, Shi, Hirschfield, Mun, Loeber, 2010). This third variable problem is an argument that has been vehemently argued by Gottfredson and Hirschi (1987).

Findings from Prior Research

Differences between arrested and non-arrested youth. Empirical research studies demonstrate that arrested individuals are likely to have very different demographic, contextual, and psychological profiles than non-arrested youth, above and beyond just crimes committed. For example, one study investigated whether gang membership, race and ethnicity, or socioeconomic status predicted likelihood of being arrested, over and above prior self-report of offending (Brownfield, Sorenson, & Thompson, 2001). Interestingly, once the frequency and type of prior offending was partialed out of the model, gang membership did not uniquely predict whether youth were arrested. However, being Black or poor did increase an individual's likelihood of arrest, regardless of prior delinquency. Similarly, researchers who compared multiple large-scale data sets found that Black and Latino individuals were disproportionately more likely to be arrested for drug-related, particularly crack cocaine, offenses than White individuals (Beckett, Nyrop, Pfingst, & Bowen, 2005). In addition to race and ethnicity, other studies have also found that arrested youth are more likely to be impulsive or hyperactive, be slightly higher in sensation seeking and aggressive behavior, be male, be substance users, have deviant peers, be poorly supervised by their parents, experience parental conflict, have parents who have criminal histories, be poor, and have educational difficulties (Hirschfield, Maschi, White, Traub, Loeber, 2006; Kirk & Sampson, 2012; Gatti, Tremblay, & Vitaro, 2009). Interestingly, one study of male adolescents found that internalizing disorders might *decrease* the

likelihood of an arrest after statistically adjusting for demographic (e.g., socioeconomic status and race and ethnicity), behavior (e.g., prior self-report of offending and substance use), peer effects (e.g., time spent with peers), and school variables (e.g., school problems; Hirschfield et al., 2006).

Effect of contact with the justice system on subsequent achievement and behavior. Although theoretical frameworks for understanding how contact with the juvenile justice system might impact youth have suggested both positive and negative effects of the justice system, empirical research to date suggests a universally negative effect of contact.

Prior experimental work. Early studies investigating the effect of involvement with the juvenile justice system were able to randomly assign youth to undergo various justice system interventions. A recent meta-analysis investigated the effect sizes of 29 experiments that randomly assigned youth who violated non-serious laws (primarily property, drug, and status offenses) to receive different justice system sanctions (Petrosino, Turpin-Petrosino, and Guckenburg, 2010). Although the specific types of sanctions that youth received in each experiment varied, in general, youth (juveniles 17 years of age or younger) were randomly assigned to be either formally processed, diverted with services, or diverted with no known consequences (i.e., released to parents). Petrosino and colleagues (2010) concluded that formal processing was related to more subsequent crime than diverting youth with services or diverting youth with no consequences. There is also some evidence that, compared to formal processing, diverting with services was a slightly better deterrent of future crime than diversion with nothing. Although the findings from the 29 studies were fairly consistent, it is important to bear in mind that the majority of the studies included in the review were conducted prior to 1990. This is important because "formal processing" in the 70s and 80s might look very different than formal

processing in the contemporary context of the juvenile justice system.

Prior observational studies. Although it would be nearly impossible for present-day researchers to randomly assign youth to receive or not receive justice system sanctions, modern researchers can use other research methodologies to approximate the treatment effect of justice system contact on subsequent achievement and delinquency. Below I describe recent work that has investigated this issue using large-scale longitudinal observational data. In all of these studies, the outcome is academic and occupational attainment or recidivism and the researchers use an arrest or a court appearance as the predictor variable. In general, all of these studies confirm the findings from the experimental data described previously.

Achievement. Using the London Panel data set, one researcher investigated whether self-reported "conviction," which basically means formal processing by the justice system, was related to a latent measure of status achievement at age 18-19, which was measured with 3 objective indicators of educational and occupational attainment (De Li, 1999). This researcher was particularly interested in whether the impact of conviction was stronger for youth who were processed between 10 and 13 compared to youth who were processed between 14 and 16. Controlling for attachment to parents, commitment to education, involvement in schoolwork, intelligence, childhood misconduct at age 8-9, social disadvantage in the family, self-report of delinquency, and parental criminal history, results indicated that conviction at age 10-13 was directly and indirectly related to lower status achievement at age 18-19, although conviction at 14-15 was not directly related to status achievement at 18-19. De Li also tested whether adolescent unemployment mediated this relation but found only partial support for this pathway.

Although De Li's (1999) work primarily investigated how the age of contact affects the impact of the justice system, other researchers have asked whether different types of contact with

the justice system differently affect youth outcomes. Indeed there are many different experiences that youth who violate the law may encounter. For example, youth may be arrested but not charged, arrested and diverted with services, arrested and formally charged, required to make a court appearance, adjudicated, convicted, or incarcerated. Using large-scale longitudinal data sets, researchers have tested whether having at least one contact with police or at least one more severe justice system contact contributes to lower high school graduation rates. Controlling for behavioral, demographic, and familial variables, three separate studies found that police and more intense juvenile justice involvement are indeed related to reduced odds of high school graduation, with more intense involvement (i.e., court appearance) having a stronger effect than police contact alone (Bernburg & Krohn, 2003; Hjalmarsson, 2008; Sweeten, 2006).

Interestingly, it appears that the negative achievement outcomes associated with having justice system contact is also related to the type of crime that youth commit. One study looked at 5 self-reported delinquency scales to see if academic and occupational failure had a stronger association with certain types of delinquency (Tanner, Davies, & O'Grady, 1999). Higher scores on self-reported truancy and self-reported drug use were related to less high school achievement, controlling for socioeconomic status, race and ethnicity, family structure, number of siblings, cultural capital (coded at age 14 and ranging from 0 to 3; a count of whether family subscribed to magazines and newspapers, and had a library card), cognitive skill, and academic expectations. Interestingly, the only self-reported delinquency scale that predicted reduced adult occupational status and adult unemployment was higher scores on the property crimes scale. This suggests that crime in adulthood might be more closely related to system contact in adolescence than adolescents' actual behavior.

Although the previously described observational studies are strengthened by their large

longitudinal data sets, none of them statistically matched arrested and non-arrested youth. These studies indeed controlled for differences between arrested and non-arrested youth, however, it is likely non-arrested youth have significantly lower and non-overlapping risk factors (thus, including covariates does not eliminate group differences). For example, as discussed in a previous section, pre-contact, arrested and non-arrested youth may differ drastically in school achievement, school engagement, school misconduct, all of which could be related to later academic attainment. To reduce the risk of the omitted third variable problem, one recent study matched arrested youth to non-arrested counterparts using propensity score matching, the statistical technique used in the present study. This study investigated whether and why contact with the justice system in high school is related to increased likelihood of dropping out (Kirk & Sampson, 2012). After matching on 82 variables in the propensity score matching analysis, these researchers found that matched arrested youth (n=79) had 22% greater likelihood of dropping out of high school than non-arrested youth. However, it is important to note that in this sample, non-arrested youth had a 51% likelihood of dropping out, which is substantially higher than the national rate of 8.2%[1] (National Center for Education Statistics, 2010). Kirk and Sampson (2012) also investigated whether three mediators might explain why being arrested increases the odds of high school dropout—school expectations, school attachment, and supportive social relationships—but found minimal support for these posited causal pathways.

Recidivism. As discussed previously, prior studies have consistently shown that arrested youth have worse achievement-related outcomes than non-arrested youth and more intense involvement with the justice system seems to be related to poorer academic and occupational attainment. In addition, many researchers have been interested in whether justice system

[1] "Dropouts" include the percent of 16 to 24 year olds who are not enrolled in school and who have not completed a high school program or GED.

experiences increase or decrease juvenile offenders' likelihood of engaging in subsequent crime. Bernburg and Krohn (2003) found that both police and court were related to an increased frequency of criminal behavior in young adulthood (as measured by self-report of criminal behavior), with unemployment and academic attainment partially mediating the relation between juvenile justice system contact in adolescence and adult crime. These researchers also found that youth who experienced more intense contact engaged in more crime than youth who had more superficial contact. Of course, from this study, it is hard to rule out the possibility that youth who tend to engage in more severe forms of illegal behavior are likely to be arrested and processed more harshly than youth who engage in very few illegal behaviors. As such, it could be that the type of adolescent who is arrested is more likely to continue to engage in crime as a young adult than the adolescent who is never arrested.

In addition to self-report of delinquency, other researchers have found that contact with the justice system in adolescence increases the odds of being arrested as an adult, according to official court records. On study in Canada recruited 1,037 kindergarten-aged youth in disadvantaged areas of Montreal and followed them for 20 years (Gatti et al., 2009). Due to missing data in the later waves, only 779 youth were included in the final analytic sample. Data were obtained from parents, teachers, classmates, and from youth self-report when youth were in kindergarten, at age 10, and annually after age 10.

The outcome of interest was official adult crime, which was assessed with official court records and coded to reflect whether individuals had at least one criminal (or delinquent) record before age 25. The primary independent variable was official records of juvenile justice contact occurring between 12-17 years of age, which was coded as the receipt of one of three different sanctions: (1) placement in an institution; (2) supervised probation (regular meetings with

probation officer; juvenile justice system record); and (3) non-supervised sanction (e.g., community service; no juvenile justice system record). Seven control variables were used: average self-reported general delinquency for ages 10-12 and ages 13-17; family income (as reported by parents); whether the youth lived in a single parent family; verbal ability; mother and teacher report of impulsivity-hyperactivity (scores were combined and averaged); classmate report of deviant peers; and parental supervision (parent and youth scores were combined to form one composite of parental supervision).

Results indicated that self-reported delinquency, teacher and mother report of impulsivity-hyperactivity, youth and parent report of parental supervision, presence of deviant peers, and family income predicted whether youth received any juvenile justice contact by age 17 (which is consistent with the work presented previously that distinguished arrested from non-arrested youth). Conversely, there were no differences based on these predictors in which of the three sanctions youth received. This means that, in this sample, demographic, individual, contextual factors, and behavioral factors predicted whether youth were arrested in the first place, but did not relate to the type of intervention that was utilized (i.e., arrested and non-arrested youth might be different types of adolescents but youth in the three different intervention groups are likely to have very similar behavioral and psychological profiles).

All three types of juvenile justice system sanction predicted having a criminal record by the age of 25, with placement having a stronger effect on adult crime than supervision, and both placement and supervision having a stronger effect than non-supervised sanctions (Gatti et al., 2009). As such, this study elegantly demonstrated that both lenient and harsh juvenile justice system are predictive of prolonged court involvement, with more punitive sanctions having the biggest impact.

Using the same data set of low-income Montreal youth, these researchers re-analyzed the data using propensity score matching to match youth who were formally processed (sent to court) and informally processed (arrested and sanctioned but not sent to court; Petitclerc, Gatti, Vitaro, & Tremblay, 2013). Results confirmed their prior work; youth who were required to make a court appearance in adolescence were more likely to have an official adult arrest than their matched counterparts who were arrested but not required to make a court appearance. When adult crime was categorized into violent and non-violent crimes, results indicated that formally processed youth were more likely to be arrested for both violent and non-violent crimes in adulthood. These findings were robust against sensitivity analyses that tested alternative model specifications, including, among other things, their decision to impute missing data and their chosen matching algorithms. Although these researchers compared formal and informal youth, neither group was compared to the non-arrested group—likely because of the no-contact group's non-overlapping propensity score (as evidenced by the fact that non-arrested youth were significantly lower than arrested youth on many risk factors).

In summary, prior work has shown that, legal and non-legal variables, such as familial socioeconomic status, a youth's race or ethnicity, impulse control, and school problems, may increase an individual's likelihood of being arrested. Researchers use the term *selection effects* or *selection bias* to explain phenomena like this where individuals are not randomly selected into groups. In order to draw non-biased conclusions, researchers needs to adjust for such selection effects—either with a true experimental design or with advanced statistical methodology—to rule out that possibility that these factors are not biasing the treatment effects. Prior experimental and observational work has also indicated that early contact with the justice system may be more detrimental than later contact, that more severe contact may be more detrimental

than less severe contact, and that adolescent delinquency may disrupt academic attainment during the high school years, which in turn, may cause adult crime. Although complete support for the meditational pathway has not been demonstrated.

Limitations and Gaps in Prior Studies

Without question, the present study is not the first to investigate the relation between contact with the juvenile justice system and subsequent behavior. Many large longitudinal studies have found that involvement with the juvenile justice system during adolescence is related to persistent offending as well as academic failure and unemployment (Bernburg & Krohn, 2003; Bushway & Reuter, 2002; De Li, 1999; Hirschfield, 2004; Hjalmarsson, 2008; Gatti et al., 2009; Sweeten, 2006). However, there are unresolved issues that need to be addressed before science, practice, and policy can move forward.

First, many studies omit relevant control variables (e.g., lifetime delinquency, exact number of prior contacts with the justice system, peer delinquency), and there is an overreliance on the same data collection method (i.e., large scale, existing data sets that were not specifically designed to answer these research questions). Furthermore, most (if not all) prior studies have been conducted from a criminology perspective, and, as a result, have not been sensitive to developmental phenomena. Only one study has tested whether younger adolescents are more affected by juvenile justice system contact than older individuals (and, the one study that did test whether early contact was more detrimental than later contact did not adequately control for selection effects that may be related to early conviction [i.e., De Li, 1999]). As early adolescence is a critical, malleable, and vulnerable period with regard to many aspects of development (e.g., Lerner & Steinberg, 2009), non-normative experiences (e.g., contact with the juvenile justice system) may have greater long-term consequences for younger individuals than

older adolescents (discussed in the next section). Although De Li (1999) did, in fact, find that being convicted of a crime between the ages of 10 and 13 had stronger effects on achievement at 18 and 19 than convictions between 14 and 16 years old, most prior studies have given very little attention to developmental science.

Additionally, although some studies have shown that a lack of educational attainment (or unemployment) could be responsible for the link between adolescent crime and young adulthood crime (e.g., Bernburg & Krohn, 2003; De Li, 1999), the reason that adolescent crime disrupts or hinders academic achievement in the high school years has not been sufficiently investigated. Hjalmarsson (2008) and Kirk and Sampson (2012) did test whether potential mechanisms (school quality, incarceration time, stigmas held by students and teachers, school attachment) could explain why juvenile justice system contact was related to school dropout but the data do not conclusively support these hypothesized mechanisms.

In fact, very few studies have uncovered the mechanisms that may explain why adolescents who have more severe involvement with the justice system are likely to demonstrate lower academic success and higher subsequent delinquency than individuals with little to no juvenile justice system involvement. In addition, very few studies have looked at more nuanced measures of school achievement such as school attachment, school climate, school aspirations, and the value and expectancy of future success that may alter, buffer, or explain how the justice system affects academic attainment. Prior research has also not looked at how contact with the justice system may be related to subsequent truancy and other school misconduct. There is also little to no available research (to my knowledge) that has investigated how contact with the justice system is specifically related to subsequent substance use—which is known to be highly predictive of future crime (e.g., D'Amico, Edelen, Miles, & Morral, 2008; Dembo, Wareham, &

Schmeidler, 2007; Sullivan & Hamilton, 2007).

Perhaps the most noteworthy limitation in prior work is the possibility of selection effects, with arrested youth likely to have very different psychological and behavioral profiles pre-justice system contact than non-arrested youth (i.e. propensity to commit a crime; see Gottfredson & Hirschi, 1987). As such, any later observed differences in arrested and non-arrested individuals' achievement and behavior could be spurious, with both the arrest and later outcomes explained by the same individual or contextual variables that preceded (or contributed to) the police contact. This third variable problem is not new. Gottfredson and Hirschi argued as early as the 1980s that "The apparent effect of criminal justice processing is merely an artifact of selection 'bias' in the longitudinal design" (Gottfredson & Hirschi, 1987, pp. 601-602). For example, Gatti et al. (2009) found significant differences between adolescents who received a justice system sanction and individuals who never came to the attention of law enforcement. If these two groups are qualitatively different and do not overlap on any of the control variables, it is not sufficient to simply include these variables as covariates in the model. This study overcomes these limitations by not only comparing formally and informally processed youth, but by also comparing the outcomes of demographically similar youth who have committed the same crimes but who were never caught for their crimes. I also control for potential selection effects with a statistical technique designed to approximate random assignment to treatment for observational studies (in this study, "treatment" represents "contact with the justice system")—propensity score matching.

Developmental Science

Risk taking and antisocial behavior during adolescence. It is well known that adolescents are more likely to engage in risky, dangerous, and antisocial behavior than individuals in other developmental stages (e.g., Blum & Nelson-Mmari, 2004; Casey, Getz, & Galvan, 2008; Steinberg, 2008; Williams, Holmbeck, & Greenley, 2002). In fact, some researchers have suggested that up to 80% of adolescents engage in some type illegal behavior (e.g., Moffitt, 1993).

Another well-supported corollary is that risk-taking and antisocial behavior peak during adolescence, with most adolescent offenders desisting from criminal activity before reaching adulthood (e.g., Moffitt, 1993). One of the most oft cited theories of adolescent delinquency is Terrie Moffitt's theory of adolescent-limited and life-course persistent antisocial behavior (Moffitt, 1993; 2006). As the names of these categories suggest, adolescent limited (or adolescent-onset) antisocial behavior is delinquency that begins and ends during adolescence and represents the vast majority of adolescent offenders. Life-course persistent antisocial behavior (i.e., childhood-onset) represents patterns of antisocial behavior that appear in early childhood and that tend to continue in adulthood (Moffitt, 1993; 2006). Empirical research consistently shows that there are only a small percent (5-10%) of adolescent offenders who fall on the life-course persistent antisocial trajectory (Moffitt, 1993; 2006; Monahan, Steinberg, Cauffman, & Mulvey, 2009; Monahan, Steinberg, Cauffman, Mulvey, 2013). Indeed research shows that about 5-10% of adolescent offenders are responsible for the majority of adolescent crime (Piquero, Farrington, & Blumstein, 2003). Although the risk factors for life-course-persistent offending are severe and difficult to mitigate—poor or single-parent families, abusive or neglectful parents, neuropsychological deficits, and biological predispositions (Compton,

Snyder, & Schrepferman, 2003; Dogan, Conger, Kim, & Masyn, 2007; Farrington, 2009; Moffitt, Lynam, Silva, 1994; Patterson, Degarmo, & Knutson, 2000)—the risk factors for adolescent-limited delinquency are more transient and amenable to interventions. For example, low parental monitoring and having delinquent peers—two risk factors particularly salient in adolescence—are strong predictors of the majority of adolescent offending (adolescent-limited). Furthermore, it has been theorized that adolescent-limited offending may result from a mismatch between the current context in which adolescents live and their evolutionary roots. In previous eras, adolescents achieved biological maturity (puberty) and social maturity (responsibilities and privileges of adults) at approximately the same time. However, in today's world, adolescents are achieving biological maturity years before they are afforded adult rights and privileges. As such, it has been suggested that adolescent-limited antisocial behavior is quite normative, and expected, and results from the mismatch between biological and social maturity ("maturity gap" Moffitt, 1993).

Another line of research suggests that normative adolescent risk taking may be caused by structural and functional changes in the brain. Results from a longitudinal study with structural Magnetic Resonance Imaging (MRI) indicate that the brain goes through an "explosive change during the teen years" and is not fully mature until the early 20s (Giedd, 2004). In particular, adolescent brain changes involve increases in prefrontal cortex synaptic pruning (Casey, Tottenham, Liston, & Durston, 2005) and myelination (Paus, 2009; Spear, 2010). Synaptic pruning helps the adolescent brain refine and reorganize neuronal pathways related to judgment, impulse control, and other higher-order cognitive functioning, which eventually allows the brain to function more efficiently (e.g., Giedd, 2004; Giedd, 2008). Myelination occurs among the neurons within the prefrontal cortex and between the prefrontal cortex and other brain regions.

In particular, the connections between the prefrontal cortex and the part of the brain responsible for processing emotions, rewards, and other socially relevant information (i.e., limbic system) are strengthened (Paus, 2009; Spear, 2010), which improves adolescents' ability to control their emotions (Steinberg, 2007; Steinberg, 2008).

At the same time that adolescent brains are undergoing significant restructuring via synaptic pruning and myelination, limbic system neurotransmitters that regulate rewards (dopamine) and moods (serotonin) are experiencing transformations. Some of the features associated with neurotransmitter changes in the limbic system include more vulnerability and responsiveness to stress (which may lead to depression and other internalizing disorders; Gunnar, Wewerka, Frenn, Long, & Griggs, 2009), greater likelihood of engaging in risky, impulsive, or reward-seeking behavior (which may put adolescents' health at risk or cause them to be more likely to become addicted to drugs and alcohol; Ernst, Nelson, Jazbec, McClure, Monk, Leibenluft, Blair, & Pine, 2005; Galvan, Hare, Voss, Glover, & Casey, 2007; Paus, Keshavan, & Giedd, 2008), and having a heightened sensitivity to and preference for rewards and a diminished sensitivity for punishment avoidance (Ernst et al., 2005). It is important to emphasize that these changes in the limbic system occur relatively early in adolescence while the prefrontal cortex—the cognitive control of the brain—is not finished developing until the early 20s (e.g., Steinberg, 2008). It has been suggested that his time-lag between the excitement of the limbic system and the development of the fully mature prefrontal cortex may be at the root of why adolescents are more likely than any other age group to engage in risky, sensation-seeking behaviors (Spear, 2010; Steinberg, 2008).

The direct causal pathway between brain and behavior is yet to be demonstrated empirically (Kuhn, 2009). However, these biological changes *suggest* that much risk-taking

during adolescence, including illegal behavior, may be normative and may result from immature judgment and impulse control capacities, an underdeveloped ability to regulate behavior in emotionally salient (e.g., social, exciting) situations, and an inherent desire to seek risky, exciting experiences. Finally, the intensive restructuring and rewiring of the brain suggests that environmental insults and deprivations (e.g., incarceration) during adolescence may hinder normative development and may have powerful long-term effects.

Due to normative maturation, many youth are likely to engage in minor delinquency. However, there are some young people who are particularly likely to break the law. For example, males, youth who are undercontrolled or impulsive, youth who are high in both extraversion and neuroticism, youth who are poor, youth have low intelligence, youth who have poor parent-child relationships or poor rearing environments, youth who have low educational achievement, youth who have witnessed or experienced serious victimization, youth exposed to high degrees of family conflict, youth with antisocial or criminal parents, and youth who have deviant peers are all particularly at risk for high degrees of delinquency and antisocial behavior (for a review, see Farrington, 2009).

Although some youth may be more likely to engage in crime than other youth, research indicates that the majority of adolescent risk taking and delinquency is transient and exploratory. As such, intense justice system interventions may be unnecessary for the majority of youth. Indeed the majority of adolescents *age-out* or desist from criminal behavior when they transition into adult roles—without any interventions (Massoglia & Uggen, 2010). Unfortunately, contact with the juvenile justice system may interfere with this natural tendency to age out of delinquency (e.g., Steinberg et al., 2004) and, in fact, may exacerbate antisociality and cause youth to penetrate deeper into the justice system. As youth penetrate deeper into justice system,

it becomes increasingly more difficult to breakaway from what some advocates have termed the "revolving door of the justice system." Indeed Moffitt (1993) posits that some juvenile offenders may have difficulty deflecting from criminal trajectories because of becoming so entrenched in the criminal life-style and because of a lack of age-appropriate opportunities to develop pro-social behavioral scripts.

In the next sections, I describe how contact with the justice system may be even more deleterious for younger individuals. After that, I describe theoretically relevant mediators that might explain how or why contact with the system may lead to later maladjustment.

Early adolescence as a particularly vulnerable period. There are characteristics of early adolescence that suggest younger individuals could be more negatively affected by contact with the juvenile justice than older adolescents. As discussed in the previous section, changes in neurotransmitters in the limbic system may render early adolescents particularly vulnerable and responsive to stress; therefore, they may be more vulnerable to stressful contexts (e.g., juvenile justice system contact) than older adolescents.

In addition to brain changes, there are social features of early adolescence that increase their likelihood of being negatively influenced by contact with the justice system. One of the most noteworthy—and noticeable—changes during adolescence is the salience of peers (Brown & Larson, 2009; Steinberg & Monahan, 2007). As identities are fluid during early adolescence, younger adolescents are likely to obtain self-identities based on large, reputation-based crowds (Brown & Larson, 2009; Newman & Newman, 2001), which could be problematic if justice system contact propels younger adolescents toward delinquent crowds and pushes them away from prosocial social contacts. Furthermore, at the same time that crowds are disproportionately influential, susceptibility to peer influence is also at its highest: susceptibility to peer pressure

increases from childhood to early adolescence and declines thereafter (Brown & Larson, 2009). As such, the type of peers with whom younger adolescents socialize—whether delinquent or prosocial youth—may substantially influence their own identity development and behavior.

Juvenile Justice System Pathways of Influence

The first aim in the present study is to confirm or disconfirm findings from prior work by specifically recruiting non-arrested youth who have engaged in the same criminal behavior as arrested youth and by using propensity score matching. In addition to looking at the outcome variables examined in prior studies, academic attainment and recidivism, the present study also looks at other important education outcomes, such as perceived value of future success and perceived opportunities for future success, and other behaviors, such as substance use and school misconduct. If data generated in the present study find a relation between contact with the justice system and later development, another goal of this study is to test whether individual, peer and social, parent, and school related mediators explain why contact with the juvenile justice system affects adolescent development.

Individual. As discussed earlier, labeling theory suggests that youth who have contact with the juvenile justice system will escalate in deviant behavior and decrease in academic performance due to an adoption of a deviant identity and to reductions in conventional opportunities and resources (e.g., Becker, 1963; Lemert, 1951; Matsueda, 1992). In fact, contact with the justice system, and the resulting self-deviant identities and perceived reductions in opportunities, may cause individuals to decrease their perceptions of future success, the value they place on future success, their desire for future success, their motivation to succeed, or their self-esteem. It is also possible that offenders may decrease the value they place on future of educational attainment because of feeling like the stigma attached to a criminal record is so

pervasive that educational attainment "is not worth it" (this concept is similar to what Cernkovich & Giordano [1992] termed a perceived "job ceiling" [pp. 263]). It is also possible that teachers may knowingly or unknowingly modify their expectations or treatment of delinquent youth, which is known to predict students' effort, motivation, performance—particularly among low achieving or at risk youth (Eccles & Roeser, 2011). The value that an adolescent attributes to future success and his perceived likelihood of achieving the desired success is strongly predictive of an individual's effort, goals, and, ultimately, achievement (Eccles & Wigfield, 2000; Eccles & Roeser, 2011). Furthermore, school commitment, such as time spent on homework, concern for future achievement, and high aspirations for the future, has been shown to predict delinquency in samples of Black and White students (Cernkovich & Giordano, 1992). In general, school failure and problems at school have been shown to be an extremely powerful predictor of subsequent delinquency, often explained by strain theory (Cernkovich & Giordano, 1992; Cloward, Ohlin, & Cloward, 1960; Cohen, 1955; Empey, Stafford, & Hay, 1982; Merton, 1938).

Furthermore, social control theory suggests that juvenile justice system involvement may cause youth to disengage from school, thus reducing their attachment or weakening their bonds to school. An adolescent's relationship with and perceptions of his school and his teacher may have profound effects on his achievement (Eccles & Wigfield, 2000; Eccles & Roeser, 2011) and his choice to engage in or abstain from antisocial and illegal behaviors (Cernkovich & Giordano, 1992; Farrington, 2009). For example, school bonding, perceptions of the school climate, and quality of teacher-student relationships could be particularly predictive of a student's motivation to learn and subsequent achievement (Eccles, 2004; Eccles & Wigfield, 2000; Witkow & Fuligni, 2007; Eccles & Roeser, 2011) as well as his delinquency (Cernkovich & Giordano,

1992). This theoretical orientation, social control theory, also suggests that reductions in school attachment could encourage school truancy and other school misconduct. Furthermore, as discussed in the labeling theory discussion, it is possible that schools will change their treatment of youth after they assign *delinquent* labels, hence, an adolescent's perceptions of his school may become far less favorable after contact with the juvenile justice system.

In addition to the perceived climate and attachment to school, there could be real, structural impediments, like school absences, that limit an individual's prospects for academic success. Being involved with the justice system may require juvenile offenders to make court appearances or attend meetings with probation officers during school hours. This increased risk of school absences may cause youth to fall behind in school, which, in turn, could decrease academic performance and increase the likelihood of dropping out (Hirschfield, 2009). Furthermore, falling behind in school may cause youth to further disengage from school-related activities and adopt more antisocial activities. It is worth noting that delinquent youth may even experience excessively harsh treatment by schools due to zero tolerance policies and No Child Left Behind Acts, which may cause at risk youth to be pushed to continuation or alternative schools (Skiba & Peterson, 1999).

Parent. Studies conducted over the last few decades have consistently found that children have the best outcomes when parents are warm and supportive, are involved in the child's life, and when parents set predictable and fair boundaries (often referred to as "authoritative" parenting; Steinberg, 2001). Indeed this type of parenting is related to positive adolescent outcomes regardless of a youth's ethnicity, social class, or family structure (Steinberg, 2001). As such, it is likely that arrested adolescents who have parents who are highly

involved in their life might be buffered against the negative effects of contact with the justice system.

Peer. Deviancy training theory argues that contact with the juvenile justice system increases contact with delinquent peers, which causes adolescents to learn skills that help them become more effective delinquents (e.g., Dishion et al., 1996). Because of the salience of peers during adolescence, it is possible that involvement with the juvenile justice system may create opportunities for youth to develop more deviant peer groups (Bernburg, Krohn, & Rivera, 2006) and to learn more effective delinquency skills from those peer groups (e.g., Dishion, McCord, & Poulin, 1999). In fact, interventions that group like-minded delinquent youth together may inadvertently exacerbate problem behaviors (Dishion et al., 1999; Lipsey, 2006; Warr, 2002). For example, results from Dishion and colleagues have indicated that youth who did not engage in delinquency at ages 13-14 had an increased probability of trying tobacco, alcohol, and marijuana when they were ages 15-16 if their friendships were characterized by positive reactions to rule-breaking discussions (Dishion, Capaldi, Spracklen, & Li, 1995). Additionally, adolescents who had friendships characterized by positive reactions to rule-breaking at ages 13-14 were more likely to exhibit higher rates of self-reported delinquency (Dishion et al., 1996) and self-reported as well as police-reported violent behavior (Dishion, Eddy, Haas, Li, & Spracklen, 1997) at ages 15-16.

Similarly, Conger (1976) found that youth, on average, increased their delinquent behaviors when the number of delinquent peers a youth had increased and the number of positive influences a youth had remained relatively low. In addition, youth tended to engage in the same delinquent behaviors as their peers, suggesting that peers can influence—either through modeling, legitimizing, or reinforcement—the types of delinquent acts one chooses to commit

(Conger, 1976). There are many hypotheses that might explain how deviancy training operates. One hypothesis is that conversations about rule breaking (or witnessing rule-breaking) might serve to legitimize, normalize, teach, and reinforce deviant behaviors. Cultural norms may also devalue academic success and value antisocial behavior in delinquent peer groups. As such, grouping delinquent youth together might provide a platform from which lower level, first-time offenders become embedded in a deviant lifestyle. Even if arrested youth are not enrolled in a group therapy class, it is also possible that contact with the justice system will simply lead youth to meet other arrested youth, which could cause them to form new friendships with other, perhaps more delinquent, peers.

The Present Study

The present study investigated whether adolescents who come to the attention of law enforcement have worse outcomes than their counterparts who violate the same laws but are never arrested. This study overcomes the limitations in prior studies by recruiting a sample of demographically similar youth who have engaged in the same types of illegal behavior but who differ in one important respect. Only some participants have had official contact with the juvenile justice system. Although other studies have compared the outcomes of arrested and non-arrested youth, prior studies cannot rule out the possibility that pre-existing differences may have contributed (at least in part) to the pervasive finding that justice system youth have worse outcomes than non-justice system youth. Indeed it is unclear whether the previously observed differences in delinquency and academic failure are due to the type of juvenile justice experiences or whether the observed relations are due to the type of youth who is and is not arrested. If contact with the system does not *cause* arrested youth to have worse outcomes than non-arrested youth, the previously observed relations could be explained as spurious, with both

the arrest and the later outcomes explained by unique, person-specific individual and contextual variables that contribute both to initial contact as well as later maladjustment.

No prior study has recruited a comparison group of non-arrested youth that has engaged in the same types of illegal behavior as a sample of arrested youth. Although prior work indeed *controls* for measures of prior delinquency, if no-contact youth engage in statistically fewer, less severe, or qualitatively different behaviors (i.e., the range of these variables are not overlapping), controlling for prior delinquency does not completely account for pre-existing differences and possible selection effects. Carefully recruiting individuals who have committed the same types of crimes as their arrested counterparts substantially reduces the risk that the comparison group (no-contact youth) is qualitatively different than, and thus non-comparable to, the arrested youth.

In addition to the unique comparison group, there are three advantages of the present study. First, a multi-method approach was utilized with variables obtained from both official records and self-report. Specifically, as one of the primary outcome variables was recidivism, both self-reports of illegal behavior and official arrest records—which are likely to provide very different information were collected. Another advantage of the present study is that multiple entry points along the justice system pipeline were measured; youth who were arrested and diverted (informally processed youth) and youth who were formally processed were recruited. This means that the present study is well positioned to answer two important questions: whether any contact with the justice system leads to positive or negative outcomes, and whether different degrees of involvement with the juvenile justice system has differential effects on an adolescent's behavior. Last, a specialized statistical technique (propensity score matching) was used to boost comparability between the arrested and non-arrested samples. This technique helps

account for the fact that youth are likely to have different risk and protective factors that increase or decrease their likelihood of being caught (i.e., selection effects).

With this sample of arrested and non-arrested delinquent youth, I ask whether, how, and for whom, contact with the justice system has positive or negative effects on adolescents' subsequent antisocial behavior, school outcomes, and substance use. Most importantly, I test these relations after partialing out the effects of theoretically relevant variables that may contribute to selection effects.

Research aims and hypotheses.

Research Aim 1. Examine whether contact with the juvenile justice system contributes to decreases in school attachment and school performance, and whether contact with the juvenile justice system contributes to increases in school misconduct, substance use, and delinquent behavior.

Hypothesis 1: Compared to the no-contact youth, participants in the juvenile justice system contact group will demonstrate decreases in school attachment, decreases in self-report of grades in school, increases in self-reported school misconduct, increases in truancy, increases in antisocial and illegal behavior, and increases in substance use at the follow up (Time 2). At the baseline interview, I think there may be small differences ("trends") between those who have contact and those who do not, but I hypothesize that these differences will not reach conventional levels of statistical significance (i.e., *p*s will not be <.05). I do not hypothesize that there will be significant differences at baseline (Time 1) because baseline data will be collected between 1 and 42 days after processing decisions are finalized (for the arrested sample). This may be enough time for some youth to be affected by involvement with the justice system but it may be too soon to see an effect in youth whose baseline interview immediately follows their arrest. Between the

baseline (Time 1) and 6-month follow-up interview (Time 2), youth who are involved with the juvenile justice system will display greater increases in truancy, increases in school misconduct, and a greater increase in receipt of disciplinary action (i.e., punitive treatment by schools, such as suspensions, expulsions, school transfers) than no-contact youth, after controlling for baseline values of these variables and adjusting for selection effects with propensity score matching. More intense involvement with the juvenile justice system (i.e., formal rather than informal case processing) will magnify these effects. Importantly, I hypothesize that no-contact youth will not demonstrate any significant change on these outcomes between baseline and the follow up. See Figure 1 and 2.

Figure 1. Hypothesized Change in Self-Report of School Attachment and Grades in School

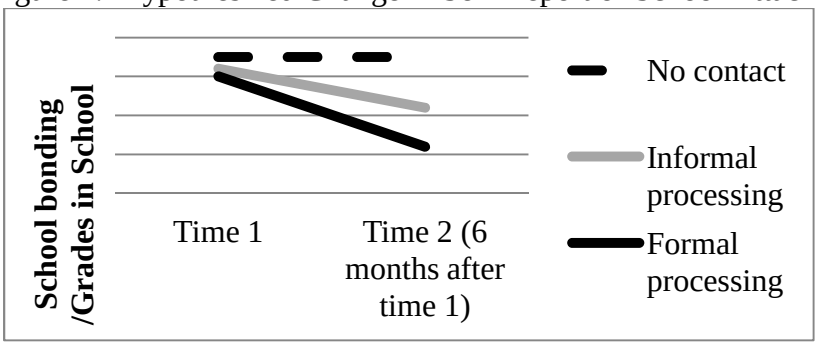
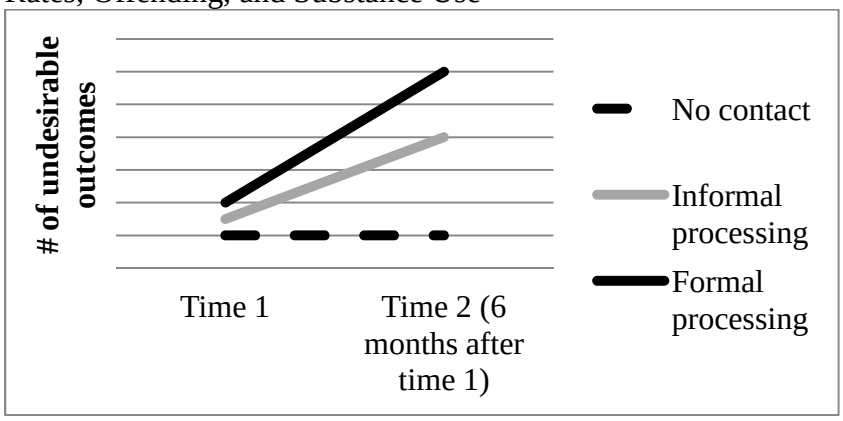


Figure 2. Hypothesized Change in Punitive Treatment by Schools, School Misconduct, Dropout Rates, Offending, and Substance Use



Research Aim 2. Identify whether the effects of juvenile justice system contact on subsequent academic outcomes, delinquency, and substance use (Aim 1) are more detrimental for younger youth than for older youth.

Hypothesis 2. Younger youth will be more negatively affected (evince greater absolute changes between baseline and follow up) by juvenile justice system contact than older youth. See Figure 3.

Figure 3. Hypothesized Change in School Attachment and Grades in School by Age.

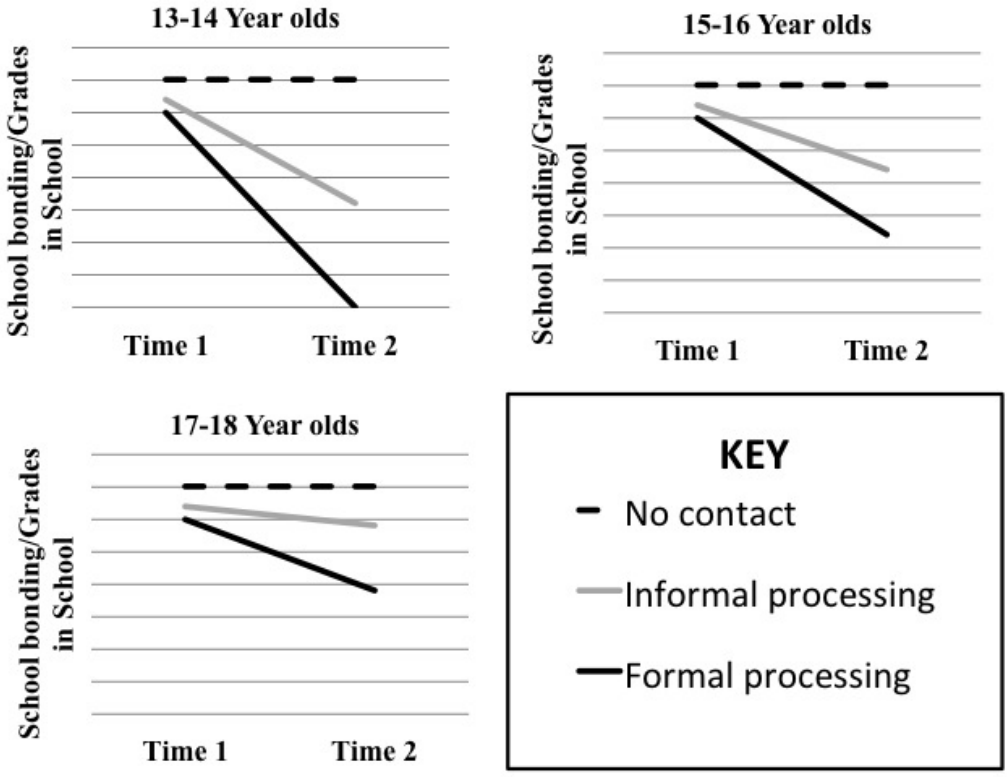
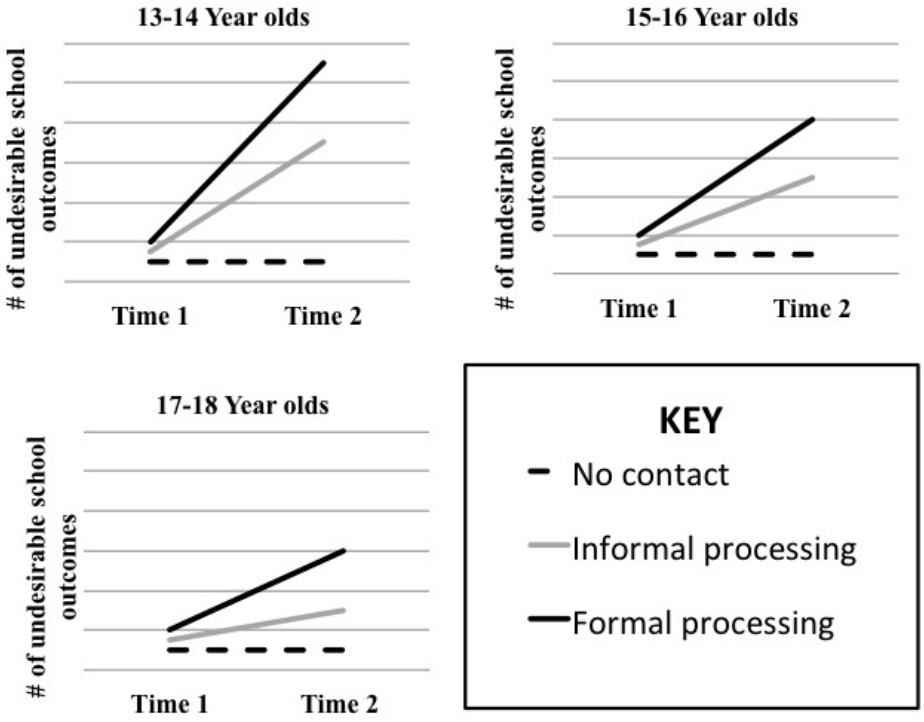


Figure 4. Hypothesized Change in Punitive Treatment by Schools, School Misconduct, Dropout Rates, Substance Use, and Delinquency by Age.



Research Aim 3. Investigate whether any effect of juvenile justice contact on behavioral outcomes (Aim 1) is attributable to changes in youth (e.g., expectations for future success and the value that adolescents place on future success), parent (e.g., parental involvement) peer and social context (e.g., increases in peer delinquency).

Hypothesis 3a (individual mediators). Decreased expectations for, and value placed on, future success in school and work, decreased attachment to school, lower self-esteem, and increases in truancy will mediate (at least partially) the effects of juvenile justice contact on academic outcomes, delinquency, and substance use. Specifically, youth who have contact with the justice system will decrease in their perceptions of their opportunities for future success, decrease in the value they place on future success, decrease in their overall self-esteem, decrease in their attachment to school, and increase in the amount of time they are truant from schools

between the baseline and follow up interviews. As a result of decreases on these individual measures, arrested youth will evince lower school attachment and lower grades in school, and higher rates of delinquency, substance use, and school misconduct. (See Figures 5 and 6.)

Figure 5. Hypothesized Mediating Role of Self-Esteem, Value and Importance of Future Success, Truancy and Expectations for Future Success on the Relations between Juvenile Justice System Contact, School Bonding, and Grades in School.

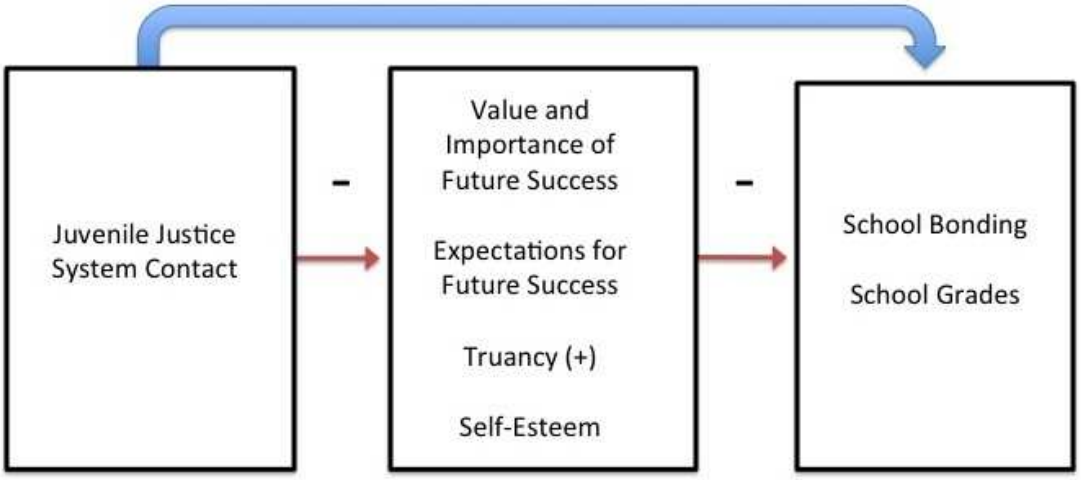
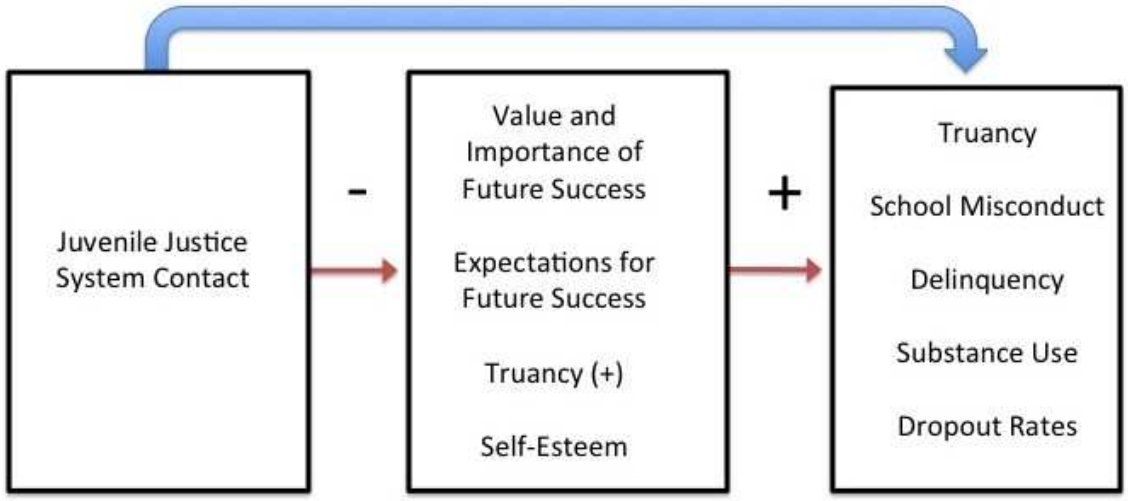


Figure 6. Hypothesized Mediating Role of Self-Esteem, Value Placed on Future Success, Expectations for Future Success, Truancy, and School Attachment on the Relations between Juvenile Justice System Contact and Truancy, School Misconduct, Delinquency, Substance Use, and Dropout Rates



Hypothesis 3b (peers and social context). Increased associations with delinquent peers will mediate (at least partially) the effects of juvenile justice system contact on academic

outcomes, substance use, and delinquent behavior. Specifically, after the arrest, youth will increase in the extent to which they affiliate with delinquent peers, and as a result of increased peer delinquency, arrested youth will demonstrate steeper decreases in academic performance and greater increases in delinquency, substance use, and misconduct. Similarly, a youth's perception of the opportunities within his neighborhood (i.e., neighborhood climate or neighborhood norms regarding academic and occupational success) will decrease for arrested youth, which will be related to worse outcomes at the follow up interview (See Figures 7 and 8).

Figure 7. Hypothesized Mediating Role of Deviant Peers on the Relations between Juvenile Justice System Contact, School Bonding, and Grades in School.

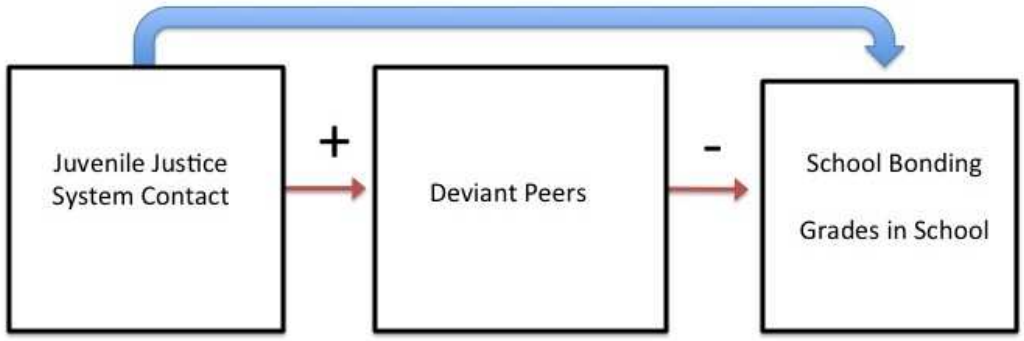


Figure 8. Hypothesized Mediating Role of Deviant Peers on the Relations between Juvenile Justice System Contact and Truancy, School Misconduct, Delinquency, Substance Use, and Dropout Rates

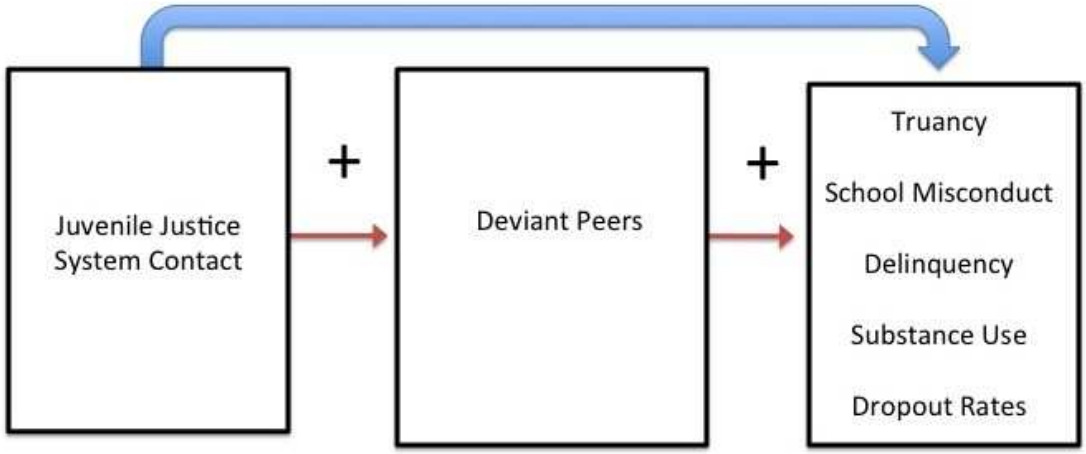


Figure 9. Hypothesized Mediating Role of Neighborhood Climate on the Relations between Juvenile Justice System Contact, School Bonding, and Grades in School.

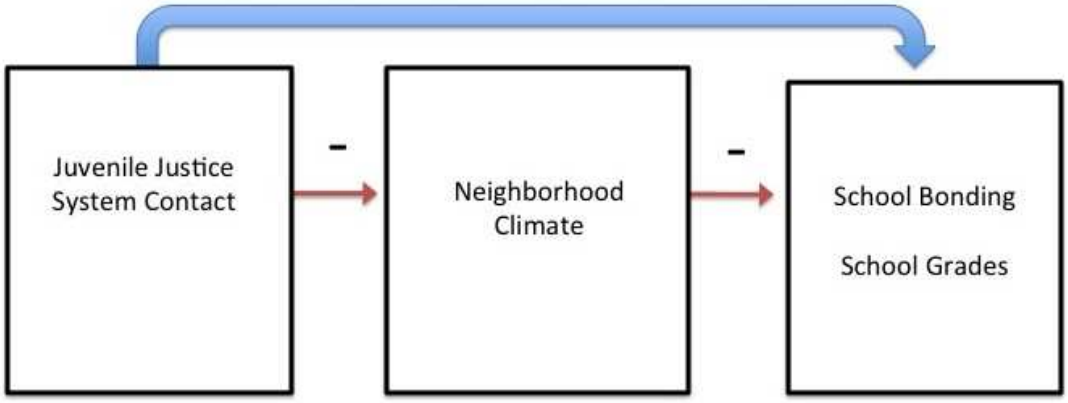
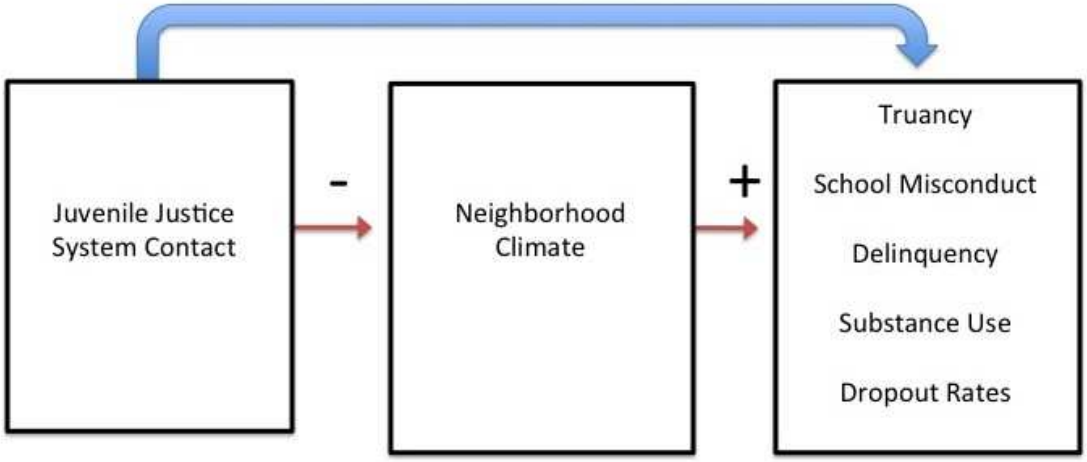


Figure 10. Hypothesized Mediating Role of Neighborhood Climate on the Relations between Juvenile Justice System Contact and Truancy, School Misconduct, Delinquency, Substance Use, and Dropout Rates



Hypothesis 3c (parent). As a response to an adolescent's first contact with the justice system, I hypothesize that arrested individual's parents will increase in parental knowledge, parental effort, and parental monitoring which will mitigate and mediate (at least partially) the effects of juvenile justice system contact on academic outcomes, delinquent behaviors, and substance use (see Figures 11 and 12). Specifically, the increased parental involvement will be related to positive outcomes (increases in school performances and decreases in problematic behavior).

Figure 11. Hypothesized Mediating Role of Parental Knowledge, Parental Effort, and Parental Monitoring on the Relation between Juvenile Justice System Contact, School Bonding, and self-reported Grades in School.

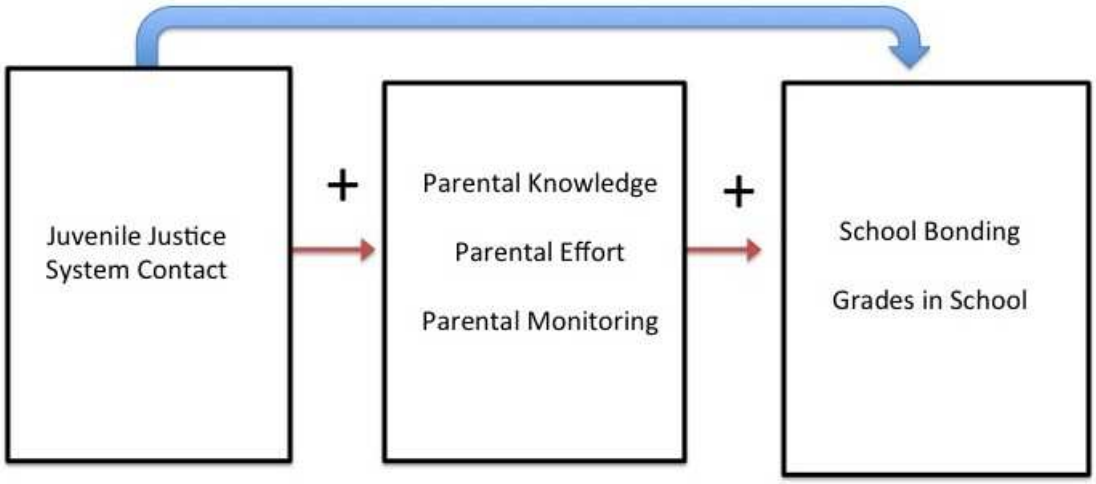
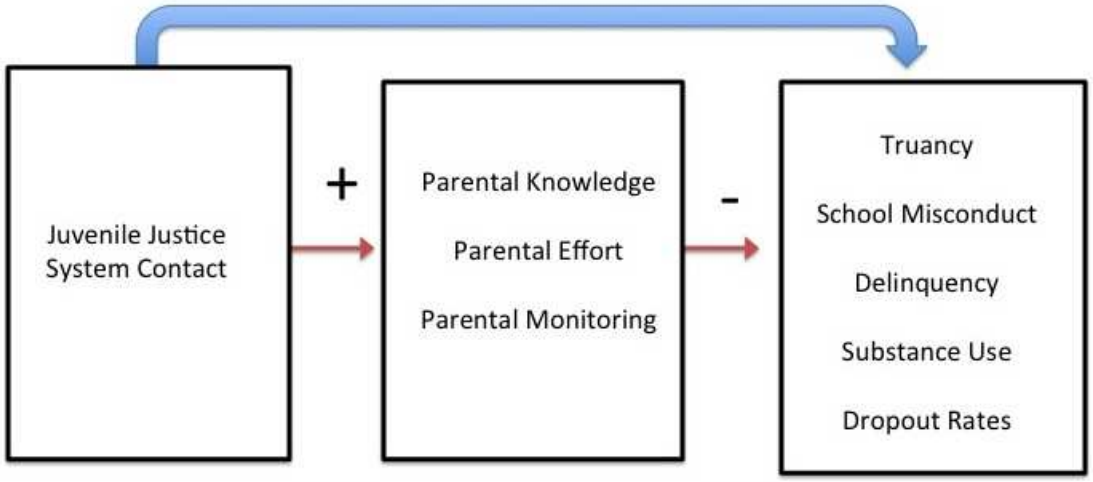


Figure 12. Hypothesized Mediating Role of Parental Knowledge, Parental Effort, and Parental Monitoring on the Relations between Juvenile Justice System Contact and Truancy, School Misconduct, Delinquency, Substance Use, and Dropout Rates.



III. Research Design and Methods

Overview

This study compared youth who did and did not have contact with the juvenile justice system and investigated whether and how the first contact with the juvenile justice system was related to subsequent educational outcomes, substance use, and delinquency. To achieve this goal, this dissertation augmented an existing study, the Crossroads study—a $3.8 million project funded by the John D. & Catherine T. MacArthur Foundation and the Office of Juvenile Justice and Delinquency Prevention (OJJDP). The Crossroads study was designed to investigate how different types of juvenile justice system experiences are related to multiple domains of adolescent development. One thousand two hundred and sixteen first time juvenile offenders were recruited to participate in the Crossroads study. Youth were interviewed every six months for 36 months. Although the Crossroads study is well-positioned to illuminate the effects of different juvenile justice responses to youth who come into contact with the system, it leaves an important question unanswered. To answer the question of whether contact with the justice system in any form has a positive or negative effect on adolescents, I recruited a sample of youth who engaged in similar illegal activities but never had any prior contact with the juvenile justice system. With support from National Science Foundation, National Institute of Justice, and American Psychology-Law Society, this dissertation added a no-contact sample to the Crossroads study. This allowed a comparison between two groups of delinquent youth: (1) those who are caught and processed by the justice system; and (2) those who engaged in the same criminal behavior but managed to evade law enforcement and remain free from contact with the juvenile justice system.

Sample

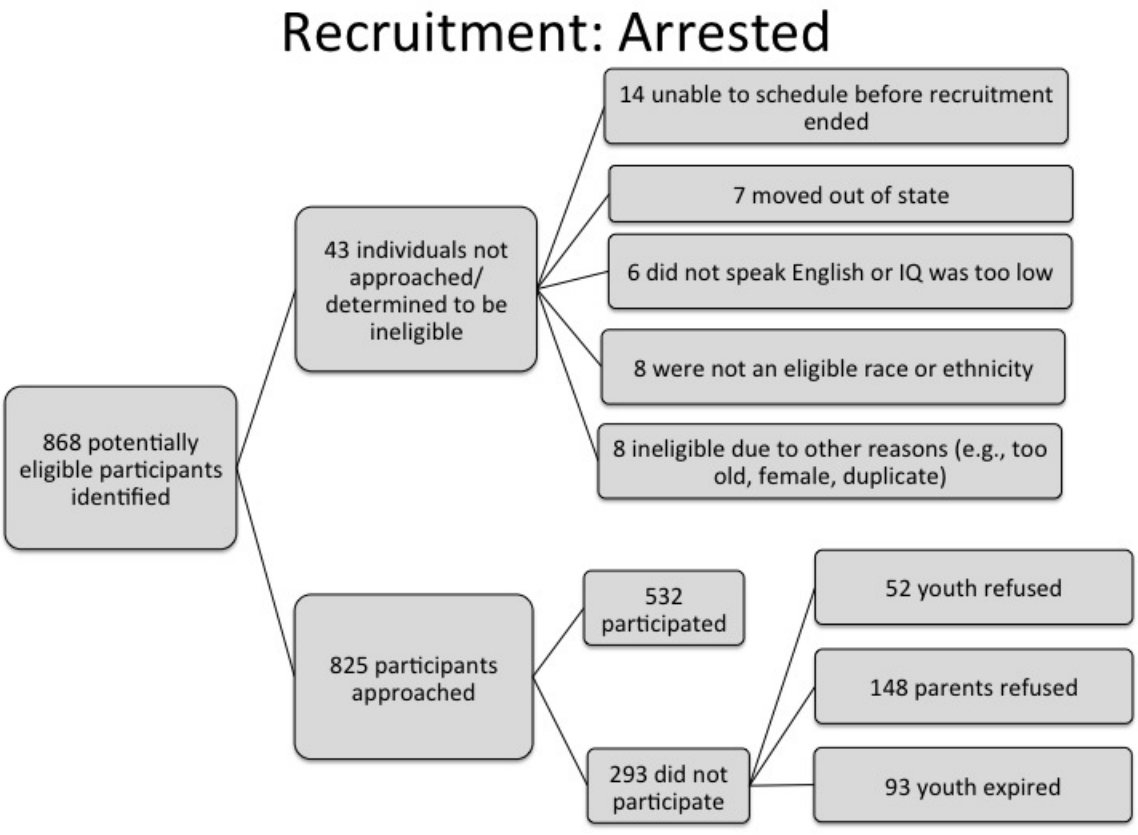
Arrested sample (Crossroads Study) case selection. The goal of the Crossroads study is to determine whether an adolescent's first experience with the justice system—specifically whether he is formally processed (required to make a court appearance) or informally processed (diverted from official justice system processing and usually given the opportunity to have charges dismissed if an alternative program is successfully completed)—differentially influences multiple domains of functioning, such as behavior, development, and mental and physical health. There are two groups of arrested youth in the Crossroads study: youth who were *formally processed* by the juvenile justice system and youth who were *informally processed*.

The Crossroads study is unique because, before the study commenced, we analyzed a data set that included all filed charges and the corresponding dispositions for juvenile offenders over a recent five-year period. This historical data analysis was used to determine the charges that have similar probabilities of being formally processed or informally processed for first time juvenile offenders in the study site. As such, youth were only eligible to participate in the Crossroads study if they were male first-time offenders (according to official record reviews in probation and court in databases), spoke English, had been charged with one of the eligible charges (i.e., charges that have equal probability of being processed formally and informally; see Appendix 1), were between the ages of 13-17, and were White, Black, or Hispanic.

Arrested youth sampling technique. The Crossroads project coordinator reviewed court and probation records to identify participants who fit study eligibility criteria. Eight hundred sixty-eight potentially eligible participants were identified in official databases, and contact information for these potential participants was extracted from these databases. Crossroads research assistants contacted all potentially eligible youth via telephone, letter, house visit, at

court, or at the probation office. During initial contact, research assistants described the study and determined whether the youth and his parents were interested in participating. If youth and parent were interested, formal consent and assent were obtained. After initial contact and a second review of all potential participants, 43 participants were determined to be ineligible. Of the remaining 825 potential participants, 532 participated. The 293 youth did not participate because either the youth refused (n=52), the parent refused (n=148), or we could not schedule the interview within six weeks of case processing (n=93). See Figure 13.

Figure 13. Recruitment Flowchart for Arrested Sample.



Arrested youth sample size. The Crossroads study is a 3-site study that is following 1,216 first-time offenders for 3 years: 533 youth from Philadelphia, PA; 151 youth from

Jefferson Parish, LA; and 532 youth from Orange County, CA. Only youth in the California site were included in the present study (271 informally processed youth and 261 formally processed youth).

No-contact sample (dissertation sample) case selection. The no-contact control sample included demographically similar youth who engaged in the same illegal behaviors as the Crossroads youth but were never arrested. The main difference between the Crossroads and no-contact sample is that the no-contact sample was never arrested as a result of their crimes. Youth were only eligible for this dissertation study if they met the eligibility requirements for the Crossroads study (see previous section) but never had any official contact with the juvenile justice system.

No-contact youth sampling technique. To identify a well-matched no-contact sample, we initially only recruited youth who were members of the same peer groups as enrolled Crossroads participants. The Crossroads interview asked participants to list the names of their five closest friends. Follow-up questions assessed whether each friend has (a) been arrested or (b) spent time in jail. At the end of the interview, the computer software auto-generated the names of the nominated peers who had never been arrested and never spent time in jail. The interviewer asked the youth for permission to contact these friends to inquire about whether they were interested in participating in a similar but different research study. We also asked participants to nominate any other friends who might be interested in participating in a similar research study. If permission was granted, participants provided contact information for the nominated peers and signed a Peer Locator Sheet providing us with their permission to contact these friends (see Appendix 2). After the interviews, the project coordinator for the no-contact sample (this graduate student) compiled the new peer nominations, checked for duplicates with

previous nominations, and distributed unique, new cases to undergraduate research assistants. Research assistants and I then contacted and screened the nominated peers to see whether they indeed met the study eligibility requirements (see Table 1 and Table 2). Importantly, as access to probation and court records has been secured, I was able to confirm that the youth in the no-contact control sample did not have any prior charges officially filed within the Orange County probation and court databases[2].

Due to challenges with recruitment (i.e., recruitment was too slow), we sought and received permission to recruit directly at two local high schools (both charter schools; IRB-approved). Specifically, research assistants and I made announcements in classrooms and passed out flyers to interested students. Because the school visits did not significantly boost the rate of recruitment, we expanded the pool of potential participants by recruiting directly in the community. Specifically, research assistants and I passed out flyers to anyone who could potentially be eligible for study participation. We also posted study flyers on community bulletin boards.

One of the most important aspects of the screening process was to ensure that members of the no-contact sample had committed at least one of the Crossroads study's eligible charges in the last year—but had never been caught. To do this, research assistants and I asked nominated peers a series of questions that required youth to state the last time they engaged in certain risky or illegal behaviors (see Table 2).

[2] Two of the participants in the no-contact sample had charges that were dismissed before the baseline interview. Twelve no-contact participants went to truancy court prior to the baseline interview.

Table 1. Screening Questionnaire.

Screening Questionnaire	
In your lifetime, have you ever…	
Been arrested	YES / NO
Spent time in juvenile detention/jail	YES / NO
Been on probation	YES / NO
Been to court (for something other than a traffic violation)	YES / NO
Been driven home by police	YES / NO
Been required by some type of law enforcement to attend a class/program	YES / NO
Do you consider yourself to male or female?	
What is your DOB?	
What is your race/ethnicity?	

Table 2. Eligible Charge Screening Questionnaire

In the past 12 months, when was the last time you engaged in the following behaviors…	
	Interviewer: Did youth engage in behavior in last 12 months? Circle YES or NO
Used a fake ID	YES / NO
Skipped school without permission	YES / NO
Drunk a bottle or a glass of beer or other alcohol	YES / NO
Ridden a bike without a helmet	YES / NO
Copy homework or a class assignment off somebody else	YES / NO
Vandalized property/done graffiti/tagged (worth less than $400)	YES / NO
Destroyed or ruined public or private property (worth more than $400)	YES / NO
If so, what did you do?	
Taken or stolen something?	YES / NO
If so, what did do and what did you take?	
Did you intend/plan to take it before you actually stole it?	
Possessed switchblade knife	YES / NO
Physically attacked a teacher or another adult at school	YES / NO
Obstruct/interfered/resisted/ran from police officer because of something you were doing that might be considered illegal?	YES / NO
If so, what did you do?	
Got into a physical fight at school or another public place	YES / NO
Been in possession of drugs (not including marijuana)	YES / NO
Used force/unlawful physical contact against a police officer, emergency personnel, school employee	YES / NO
If so, what did you do?	
Engaged in unlawful physical contact, use of force against another person	YES / NO

If so, what did you do?	
Attacked someone with the idea of seriously hurting them	YES / NO
Caused someone to fear violence, without actual physical contact	YES / NO
If so, what did you do?	

Note. Only shaded rows represent eligible behaviors. The purpose of the first 5 questions was to build rapport with potential participants.

No-contact youth sample size. The primary analyses in the present study utilized the statistical technique of propensity score matching to approximate the random assignment of juvenile justice contact. In particular, I matched youth in the no-contact sample to youth in the contact sample based on their probabilities of having justice system contact (discussed later), which is computed with respect to values on variables that are theoretically related to having contact with the justice system. As such, 99[3] adolescent males who did not have any prior contact with the juvenile justice system were recruited for this dissertation sample. These 99 no-contact youth were matched with the 532 arrested youth in OC. Post hoc power analyses are presented in the results section.

Sample descriptives. As mentioned previously, youth in the present study were between 13 and 17 years old at the time of their baseline interview. The average age was 15.55 years old (*sd* = 1.22) and the sample was representative of the demographic distribution of the study site: 77.34% Hispanic, 18.54% White, 1.27% Black, and 2.85% Multiethnic (see Table 3 for sample descriptives broken down by group). See Figure 14 for a map that shows the neighborhoods from which formal, informal, and no-contact youth were recruited. As demonstrated in Figure 14, the three groups were similarly distributed throughout Orange County.

[3] There were originally 100 no-contact youth but one youth was determined to be ineligible.

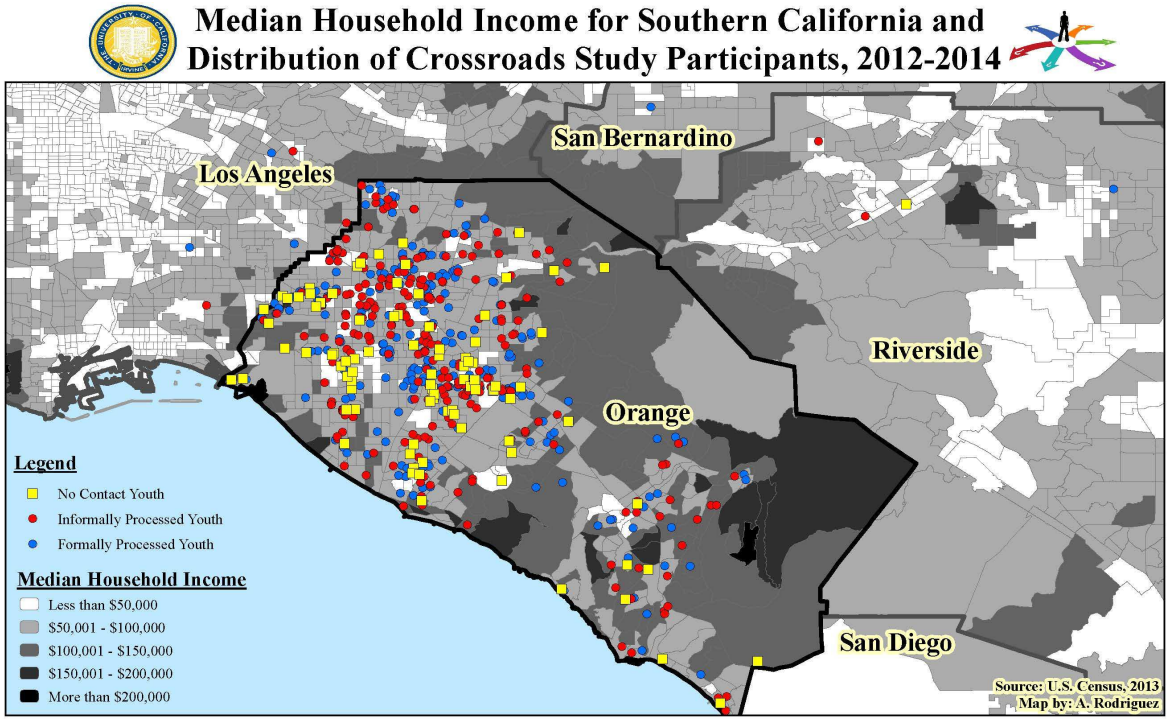
Table 3. Sample Descriptives.

Sample Descriptives by Group.

	No-contact youth (n=99)	Informal sample (n=271)	Matched[1] informal youth (n=62)	Formal sample (n=261)	Matched[1] formal youth (n=63)
Age at baseline					
Range	13 to 17	13 to 17	13 to 17	13 to 17	13 to 17
m(*sd*)	15.85 (1.20)	15.44 (1.30)	15.79 (1.13)	15.54 (1.13)	15.76 (1.17)
% 13 y/o	6.06%	8.49%	1.61%	3.45%	1.59%
% 14 y/o	8.08%	28.82%	19.35%	17.24%	17.46%
% 15 y/o	19.19%	20.66%	8.06%	24.52%	20.63%
% 16 y/o	28.28%	24.72%	40.32%	31.03%	23.81%
% 17 y/o	38.38%	27.31%	30.65%	23.75%	36.51%
Race and ethnicity					
% White	24.24%	19.19%	27.42%	15.71%	25.26%
% Black	3.03%	0.74%	3.23%	1.15%	3.16%
% Hispanic	71.72%	76.38%	67.74%	80.46%	70.53%
% Other	1.01%	3.69%	1.61%	2.68%	1.05%

[1] Values in this table represent the unweighted sample descriptives for the formal and informal youth. Analyses were conducted with the weighted samples.

Figure 14. Geographical Distribution of No-Contact, Informally Processed, and Formally Processed Youth.



Eligible charges (arrested sample). The most common charge for the Crossroads youth was vandalism (38.35% of youth). The breakdown for the other charges was: theft (21.05%), burglary (12.03%), resisting arrest (7.33%), battery (6.95%), assault and battery (6.20%), assault (4.32%), drug possession (not including marijuana; 1.69%), fighting in public (1.50%), and possession of a switchblade knife (0.56%). Crossroads youth only had one eligible offense. See Table 4.

Table 4. Distribution of Eligible Offenses for Arrested Youth.

Distribution of Eligible Offenses for Arrested Youth.

	Informal sample (n=271)	Matched informal[1] youth (n=62)	Formal sample (n=261)	Matched formal youth[1] (n=63)
Assault	2.58%	4.84%	6.13%	4.76%
Assault & battery	7.75%	11.29%	4.60%	3.17%
Battery	6.27%	6.45%	7.66%	12.70%
Burglary	2.95%	4.84%	21.46%	25.40%
Fighting in public	2.95%	1.61%	0.00%	0.00%
Petty theft	30.26%	25.81%	11.49%	11.11%
Possess switchblade	0.00%	0.00%	1.15%	1.59%
Possession of controlled sub	1.85%	6.45%	1.53%	3.17%
Resisting arrest	8.86%	9.68%	5.75%	3.17%
Vandalism	36.53%	29.03%	40.23%	34.92%

[1] Values in this table represent the unweighted sample descriptives for the formal and informal youth. Analyses were conducted with the weighted samples.

Eligible charge (no-contact youth). Because of the nature of the screening instrument, the no-contact sample could have multiple eligible charges. Approximately 26% reported engaging in one only eligible charge. Of the participants who only had one eligible charge, the most common charge was fighting in public (n=10). On average, youth had 3.32 eligible charges (*sd* = 1.39, range: 1 to 13, median = 3, mode = 1).

The most commonly endorsed behaviors on the eligibility screener for no-contact youth were fighting in public[4] (62.60% of no-contact youth) and theft (61.2% of no-contact youth). Of the youth who reported engaging in theft (i.e., "taking or stealing something"), 35% said that they had intended to do it before it happened (i.e., the legal distinction between burglary and theft falls at whether the youth *intended* to steal something; to receive a "burglary" charge, an individual must *plan* or *intend* to steal something before going inside the store, home, etc.). The next most common behavior was vandalism: 23.2% of no-contact youth reported vandalizing property worth less than $400 and 20.2% of no-contact youth reported vandalizing property worth more than $400. Furthermore, 31.1% reported obstructing, interfering, or running from a police officer because of something they were doing that they considered to be illegal, 31.3% caused someone to fear violence without physical contact (i.e., assault), 29.3% engaged in unlawful physical contact by using force against another person, and 10.1% of youth said that they had attacked someone with the idea of seriously hurting them (i.e., battery). Furthermore, 26.3% of youth reported being in the possession of a switchblade knife, 20.2% of youth were in possession of drugs (not including marijuana), 4% used force or unlawful physical contact against a police officer, emergency personnel, or school employee, and 3% reported physically attacking a teacher or adult at school. To be eligible, youth needed to self-report engaging in these behaviors sometime in the last year. See Table 5.

[4] Depending on the nature of the fight, if arrested and charged, fighting in public could be filed as assault or battery.

Table 5. Distribution of Eligible Offenses for No-Contact Youth.

Distribution of Eligible Offenses for No-Contact Youth[1].

	No-contact sample (n=99)
Fight in public	62.63%
Theft	39.80%
Resisting arrest	31.31%
Caused someone to seriously fear violence	31.31%
Unlawful physical contact against person	29.29%
Possess switchblade	26.26%
Vandalism <$400	23.23%
Burglary	21.21%
Vandalism > $400	20.20%
Possess drugs (not marijuana)	20.20%
Attacked to seriously hurting someone	10.10%
Unlawful force against police officer	4.04%
Attack teacher at school	3.03%

[1] No-contact youth could have multiple eligible charges.

Procedures (Both Samples)

The Institutional Review Board of the University of California, Irvine approved all study procedures and materials for Crossroads as well as this dissertation (HS #2010-7867). Importantly, the same study procedures were used for the arrested and no-contact samples. The only procedural difference between the two groups was with respect to recruitment techniques (discussed previously). After participants were identified and parental consent was obtained, youth participated in a two to three hour interview (referred to as the "baseline" or "Time 1" interview). The location of the interview was either at the participant's home (approximately 49% of interviews) or at a public restaurant or coffee shop (approximately 49% of interviews). (The remaining interviews, for the arrested sample, were in a residential facility, jail or detention facility, or other location [all of the no-contact interviews were conducted in the participants' home or a public venue in his community].) Each interview was conducted with a research assistant on a laptop computer with computer-assisted interview software. Items in the interview

assessed constructs in a variety of behavioral, academic, and psychological domains. Interviewers and participants sat side-by-side so both individuals could view the laptop screen; interviewers were required to read each question aloud. This process helped minimize issues and errors that could be attributed to a participant's reading and comprehension ability. Participants were offered a keypad, which allowed them to answer sensitive questions privately. One follow-up interview (referred to as "the follow up" or "Time 2") was conducted approximately 6 months after the baseline interview and consisted of the same interview battery[5].

Importantly, because the main Crossroads study is funded by OJJDP and this dissertation sample is funded by NIJ, the no-contact sample was automatically protected under a Certificate of Confidentiality (U.S. Code § 3789g Sec.812 I). This means that researchers were (and are not) legally allowed to release any personally identifiable information to any interested parties (even if the data are subpoenaed).

Retention. The retention of both samples was excellent. Ninety-eight percent of no-contact youth who have been recruited for the follow up completed the interview[6] and 97.5% of Crossroads youth completed the 6-month follow up. As mentioned previously, the ideal day for the follow up interview was exactly 6 months after the baseline interview (the "target" date). We initiated the search for each participant 6 weeks prior to the target date (the "search window"). When the search window opened, we mailed a postcard to the participant, which asked him to give us a call to schedule his next appointment. If we did not hear from the youth within two weeks, we called or visited his home. If we could not schedule the follow up on or before the target date, we allowed a 6-week window post-target date to complete the interview (the "late

[5] Crossroads participants were followed every 6 months for 3 years.
[6] Data collection is ongoing. At the writing of this dissertation, 70 no-contact you had completed the follow up interview. Retention reflects the follow ups that have been missed to date.

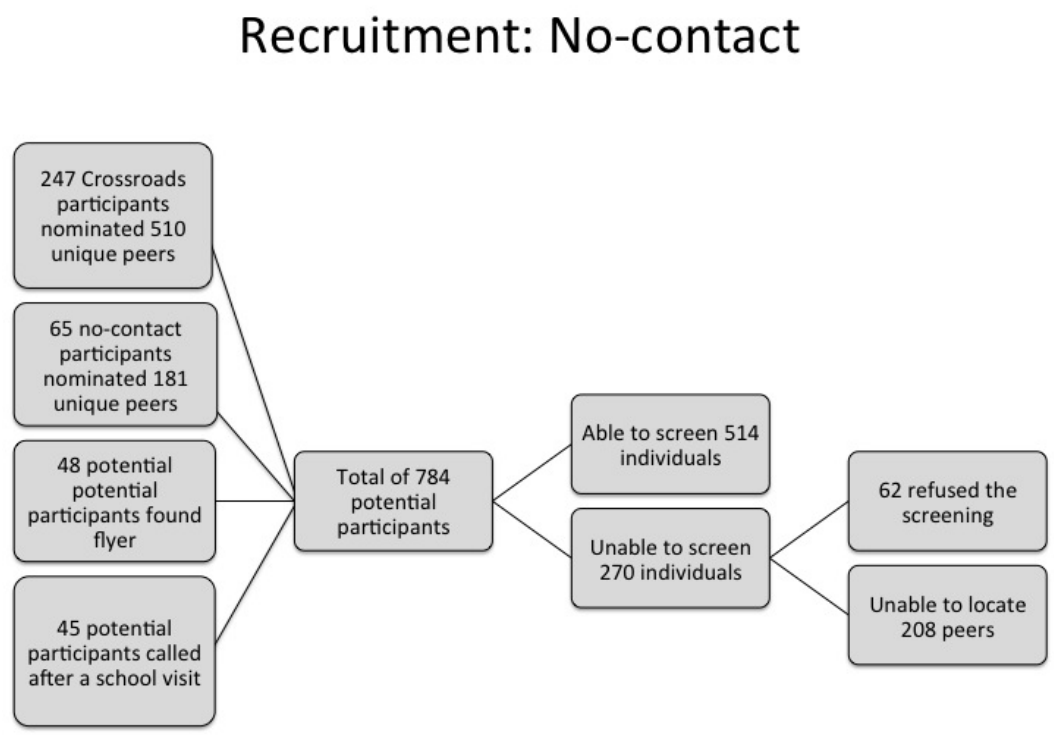
window"). The last day to conduct the follow up interview was 6 weeks after the target date (which was 6 months and 6 weeks after the baseline interview).

Post hoc examinations of the data indicate that we successfully interviewed youth during the correct follow up window. The average time between baseline and the follow up for the no-contact sample was approximately 6.01 months (s = .32), ranging from 5.26 months to 7.07 months. In regard to the arrested sample, the average time between baseline and the follow up was approximately 6.10 months (s = .22), ranging from 5.00 months to 7.50 months.

We incentivized participants by compensating them for their time. For the first half of the recruitment of the no-contact sample, we paid participants $35 for the baseline interview and $40 for the follow up interview. However, recruitment was very slow (we averaged 1.39 new baselines per week with this payment). In an effort to enhance recruitment of the no-contact sample, we increased the participant payment to $50 (baseline) and $55 (follow up). The revised payment scale is similar to the participant payment in the Crossroads study. Crossroads youth were paid $50 for the baseline and $65 for the 6-month follow up. Crossroads youth were paid slightly more because their interview was slightly longer. The increase in payment was successful: after the payment was increased to $50, we averaged 2.35 new baselines per week. Furthermore, to the extent possible, the same research assistant interviewed youth at both time-points. (For the no-contact sample, the same interviewer, when possible, also screened youth and obtained parental consistent.) We believe that maintaining consistent interviewers encourages interviewer-participant rapport and enhances participation.

Recruitment statistics. Two hundred and forty-seven[7] Crossroads participants (46.5%) nominated 510 unique peers. Sixty-five no-contact participants (65.0%) nominated a total of 181 potential participants. Forty-eight potential participants obtained information about the study via the flyer[8] and contacted the research staff and 45 potential participants called the research staff after we made a school visit. Of these 784 individuals, we were able to locate and screen 514 potential participants. We were unable to screen 270 participants. The primary reason that nominated youth were not screened was because we did not have working contact information. Only 62 individuals were not interested in being screened (7.9% of potential participants). See Figure 15.

Figure 15. Recruitment Flowchart for No-Contact Youth #1



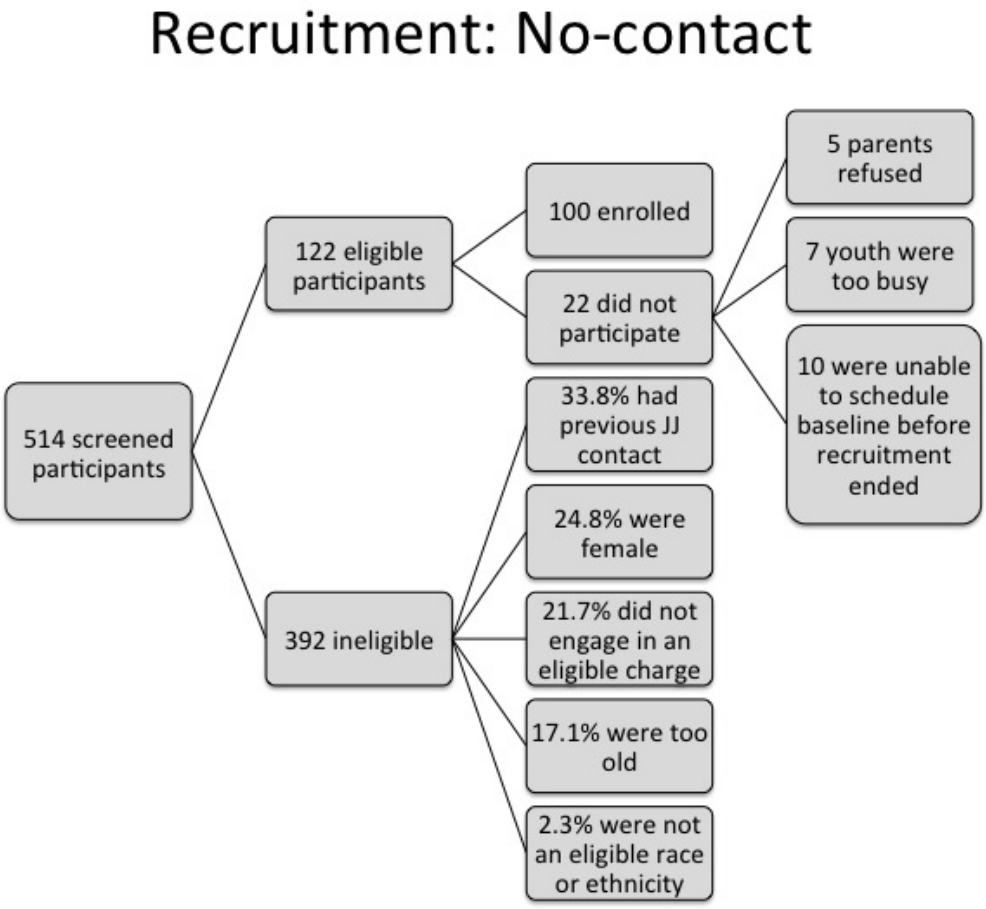
[7] This only includes Crossroads nominators who nominated a "new" peer. Duplicate nominations were not counted in these numbers.
[8] It is possible that some participants gave flyers to their peers instead of nominating them at the interview.

Of the 514 screened participants, 99 eventually enrolled in the study. Of the individuals who were screened (n=514), 8.3% obtained a study flyer[9], 62% were nominated by a Crossroads participant[10], 20.6% were nominated by a member of the no-contact sample[11], and 8.7% were located through a school visit. Approximately 23.7% of these individuals were eligible (n=122). Of the 122 participants who were eligible, seven youth were too busy or not interested in participating (5.7% of eligible participants). Only five parents of eligible participants refused to allow their child to participate (4.0% of eligible participants). The ten remaining eligible participants did not participate because we were not able to schedule their baseline interviews before the recruitment period ended.

The most common reason for ineligibility was having previous contact with the justice system (33.8% of screened individuals). Youth were also ineligible if they were female (24.8% of screened individuals), did not self-report engaging in one of the eligible illegal behaviors (21.7% of individuals), were too old (17.1% of screened individuals), or were not Black, White, or Hispanic (2.3% of screened individuals). See Figure 16 for a recruitment flowchart.

[9] In addition to research assistants passing out flyers to potential participants, enrolled participants were offered flyers to distribute to their peers.
[10] Nominated by 185 Crossroads participants (34.8% of Crossroads sample).
[11] Nominated by 52 no-contact participants (52% of the no-contact sample).

Figure 16. Recruitment Flowchart for No-Contact Youth #2



Measures

The Crossroads interview battery included a wide variety of background, psychological, behavioral, and academic variables, some of which were not used in the present study. The measures outlined below include only those that were needed to address the specific aims of this study. The specific items associated with each of these measures are listed in Appendix 3. Importantly, almost all measures in the Crossroads battery were obtained in both interviews with the no-contact sample. See Table 6 for a list of the measures used to conduct the analyses in the present study.

Table 6. List of Variables Used to Address the Study Aims.

Variables used in the present study

Measure/Construct	Reference/Source
Independent variable	
Juvenile justice system contact	OC Court and Probation department
Dependent variables (change between baseline and follow up)	
Delinquent behavior	
Self-report of offending (variety and frequency)	Huizinga, Esbensen, & Weihar, 1991
Official records (likelihood of [re-]arrest)	OC Court and Probation department
Substance use (variety and frequency)	Chassin, Rogosch, & Barrera, 1991
School and teacher attachment	Cernkovich & Giordano, 1992 and items created for Crossroads
Academic achievement (average grades in school and dropout status)	Self-report
School misconduct (e.g., truancy and misconduct in school)	
Self-report	Eccles, Wigfield, & Schiefele, 1998; Cernkovich & Giordano, 1992
School discipline (school suspensions and expulsions)	Self-report
School movement (likelihood of moving to continuation school)	Self-report
Mediator variables (measured at the follow up)	
Neighborhood climate	Eccles et al., 1998
Importance, value, and expectancy of future success in school and work	Menard & Elliott, 1996
School aspirations and expectations	Eccles et al., 1998
Self-esteem	Rosenberg, 1989
Peer delinquency	Thornberry, Lizotte, Krohn, Farnworth, & Jang, 1994; Monahan et al., 2009
Parental involvement	Steinberg, Dornbusch, & Darling, 1992
School and teacher attachment	Cernkovich & Giordano, 1992 and items created for Crossroads
School truancy	Self-report
Matching variables (measured at baseline)	
Demographics (DOB, race/ethnicity, SES)	Self-report
Prior offending	Huizinga et al., 1991
Prior substance use	Chassin et al., 1991
Intelligence	Wechsler, 1999
Maturity of judgment	
Weinberger Adjustment	Weinberger & Schwartz, 1990

Inventory	
Future Outlook Inventory	Cauffman & Woolard, 1999
Psychosocial Maturity Inventory	Greenberger, Josselson, Knerr, & Knerr, 1974
Resistance to Peer Influence	Steinberg & Monahan, 2007
School misconduct	Eccles et al., 1998; Cernkovich & Giordano, 1992
Parental involvement	Steinberg et al.,, 1992
Parental criminality	Self-report
Parental antisocial behavior	Based on the peer delinquency scale (Thornberry et al., 1994)
Neighborhood characteristics	Sampson & Raudenbush, 1999; Sampson, Raudenbush, & Earls, 1997; Sampson, 1997
Peer delinquency	Thornberry et al., 1994, Monahan et al., 2009

Primary independent variable.

Juvenile justice system contact. The primary independent variable, contact with the justice system, was assessed with official Orange County court and Probation records. For the no-contact sample, self-report questions also assessed whether youth had any official contact with the juvenile justice system in any jurisdiction (i.e., we were only able to confirm that youth did not have a prior record in the OC database). In the present analyses, youth were considered to have had "contact" with the system (i.e., eligible for the Crossroads study, arrested sample) if they had one charge formally processed or informally processed in Orange County at the time of study recruitment. Crossroads youth could not have additional prior sustained arrests (i.e., Crossroads youth were first time offenders). Youth in the no-contact sample did not have any histories of arrests or contact with the juvenile justice system. To understand whether more severe forms of contact have more detrimental outcomes for youth, no-contact youth were compared to both formally processed youth and informally processed youth.

Dependent variables.

Delinquent behavior.

Offending. Antisocial and illegal behavior was assessed using a revised version of the Self-Report of Offending scale (SRO; Huizinga et al.,, 1991). The SRO is a 24-item scale that assessed whether (and how many times) youth engaged in different types of criminal activity over "the last 6 months." Frequency (sum of the total times that youth endorsed engaging in any of the possible illegal behaviors) and variety (total count of the types of delinquent behaviors that youth positively endorsed) were computed at baseline and the follow-up interview, with higher scores indicating more illegal behavior. Although frequency and variety scores are correlated *(r* = .54 in the present study), and frequency scores might be subject to more recall errors than variety scores, results are presented with both frequency and variety scores. Baseline variety of offending was used for the propensity score matching (discussed later) and the difference between baseline and the follow up on variety and frequency of offending were calculated and used as a primary outcome variables. Self-report of offending is considered an accurate and valid assessment of an adolescent's true degree of antisociality as youth have access to information to which observers of their behavior (i.e., police) might not be privy (e.g., Maxfield, Weiler, Widom, 2000). As much illegal behavior among adolescents goes undetected by law enforcement (Brame, Fagan, Piquero, Schubert, & Steinberg, 2004) and the correlation between self-report and official arrest data is often relatively low (*r*=.22; Monahan et al.,, 2009), self-report can provide information that is very different than official arrest records.

Official (re-)arrest data. Data from the Orange County Probation department and the Orange County Juvenile Court were obtained to assess whether youth were arrested, according to official data, during the study period. These records contained information regarding youth's involvement with the justice system. In particular, we extracted details regarding every arrest that occurred between baseline and the follow-up interview. We coded the number of (re-)

arrests and the corresponding sustained charges. Because of the restricted range in number of arrests between baseline and the follow up, this variable was dichotomized to represent whether youth had any (re-)arrests during the study period (0=no, not arrested during study period; 1=yes, arrested at least once between baseline and follow up). We also investigated the type of charge for the most serious offense on the petition (e.g., drug-related, vandalism, theft).

Substance use. Substance use was assessed with an adapted version of the Substance Use and Abuse Inventory (Chassin, et al., 1991). This measure produces two measures of substance use in the prior 6 months: A variety score (count of different substances used in the previous 6 months) and a maximum frequency score (maximum frequency of any drug). First, youth were asked to state which drugs they have used in the previous 6 months (whether they have used marijuana, cocaine, ecstasy, etc.). If youth used any substances in the recall period, follow up questions asked him to state the maximum frequency that he has used the drug in the previous 6 months (0=not at all; 1=less than 1X every 3 months, 2=less than 1X per month, 3= 1X per month, 4=2-3X per month, 5=1X per week, 6=2-3X per week, 7=everyday; higher scores indicate more frequent substance use). The maximum frequency for any drug was calculated and used as the substance use frequency variable. The differences between baseline and the follow up for substance use variety and substance use frequency were calculated and used as outcome variables. Baseline values were used as control variables in models wherein substance use was an outcome variable. (Baseline variety of lifetime substance use was used in the propensity score matching analysis.)

School outcomes.

Teacher attachment/student-teacher relationships. Attachment to teachers was assessed with items created by Cernkovich and Giordano (1992) and consisted of 3 items: *Most of my*

teachers treat me fairly, I care what my teachers think of me, and I like my teachers. Youth responded to these items on a 5-point likert scale (1=strongly disagree, 2=disagree, 3=neither agree nor disagree, 4=agree, 5=strongly agree). Scores on the three scales were averaged together at baseline and at the follow up. Higher scores indicate more teacher attachment. Reliability and confirmatory factor analysis indicated that the scale had great fit (α =.64; CFI = 1.00, RMSEA = .00). Change scores that represented the difference between baseline and follow up was used as an outcome variable, however, we controlled for baseline values (Time 1) when change in teacher-student relationships was an outcome variable.

School attachment/affect at school. Attachment to school was assessed with 3-items designed for the Crossroads study: *I enjoy being there, I am happy when I am there, I feel like I am a part of that school* and one item from Cernkovich and Giordano's (1992) school bonding scale: *I like school.* All items ranged from 1 (strongly disagree) to 5 (strongly agree) and were averaged together at baseline and at the follow up. Higher scores indicate more attachment to school or more positive affect at school. A change score that represents the difference between baseline and the follow up was used as one of the outcome variables. The baseline value was used as a control variable when this change score was used as the outcome variable. Reliability and confirmatory factor analysis indicated that these items fit well together (α = .76; CFI = .995; RMSEA = .05)

Academic achievement. Self-report questions asked youth to state whether they were currently enrolled in school (coded: yes [1], currently enrolled; no [0], not currently enrolled) and whether they dropped out of school (coded: yes [1] dropped out in the previous 6 months; no [0], did not drop out in previous 6 months). Youth responses at the follow-up interview were used as outcome variables. Youth were also asked to indicate what their grades were like in

school (possible responses: 8= mostly As; 7= about half As and half Bs; 6=mostly Bs; 5= about half Bs and half Cs; 4= mostly Cs; 3= about half Cs and half Ds; 2=mostly Ds; 1= mostly below Ds). Scores were coded such that better grades (more As) received higher values. The difference between baseline and the follow up was used as an outcome variable. The baseline scores were used as control variables.

School misconduct. School misconduct was obtained from previously established self-report measures (Eccles, Wigfield, & Schiefele, 1998; Cernkovich & Giordano, 1992). Questions assessed truancy from school over the past 6 months: *How many times have the following things happened to you over the past six months…I was late for school, I cut or skipped school, I was absent from school, I got in trouble for missing too many days, I had to go to truancy court*. Youth responded to these 5 questions with a 5-point likert scale (0=never,1=1-2 times, 3=3-6 time, 7=7-10 times, 10=10 or more times). Scores on these items were averaged together, with higher score indicating that a youth was truant for more days of school. The scale demonstrated adequate internal consistency and was adequately represented by a one-factor solution (α = .69; CFI = 1.00; RMSEA = .00). Additionally, 7 self-report questions asked youth to state the frequency with which they have engaged in other school-related misconduct in the last 6 months (e.g., copied homework, cheated on a test, gotten in trouble for disturbing the class). Youth responded to these questions on a likert scale (1=not at all, 2=once or twice, 3=several times, 4=often/many times). Scores on these items were averaged together, with higher scores indicating more misconduct (α = .77; one factor scale: CFI = .79; RMSEA = .13). Change scores that represented the difference between baseline and the follow up on each of these scales were used as outcome variables (baseline school misconduct was used in the propensity score matching). The last self-reported misconduct variables (assessed at baseline

and the follow up) asked youth to state whether (and how many times) they were suspended or expelled from school in the last 6 months. Whether or not they were expelled between the baseline and follow up and the change in number of suspensions (assessed at each interview to represent the count of suspensions in the previous 6 months) between baseline and the follow up were used as outcome variables. School misconduct and school disciplinary responses (tested individually) between baseline and the follow up were used as outcome variables and school misconduct prior to baseline was used in the propensity score matching (discussed later).

School type/likelihood of moving to continuation school. The type of school—whether it was a traditional junior high school, high school, 2-year college, 4-year university, or continuation school—was obtained via self-report at the baseline and the follow up interviews. Whether (=1) or not (=0) youth changed from a traditional school at baseline to a continuation school at the follow up was coded and used as an outcome variable.

Mediating variables.

Individual.

Academic aspirations and academic expectancies. Aspirations and expectations for future schooling were assessed with two items from Eccles and colleagues (1998): *How far would you like to go in school*? (response options: 1=drop out before graduation; 2=graduate from high school, 3=go to a business, technical school or junior college, 4=graduate from college, 5=go to graduate or professional school) and *How far do you think you'll go in school?* (1=drop out before graduation; 2=graduate from high school, 3=go to a business, technical school or junior college, 4=graduate from college, 5=go to graduate or professional school). Higher scores on these scales indicate more school aspirations and more school expectations. The degree of

change between baseline and the follow up was calculated for each of these variables and used as potential mediator variables.

Perception of opportunities. Six items were designed to measure participants' beliefs about the value of and their perceptions of their potential for future success in school and work (adapted from Menard & Elliott, 1996). Three items gauged the value that adolescents' place on future success (*How important is it to you*…) and three items assessed the adolescents' expectations for future success (*What do you think your chances are*…). Items assessed participants' value of, and expectation for, earning a good living, having a good job or career, and graduating from college. The value and expectancy scales required youth to respond to each item using a 5-point likert scale (value: 1=not at all important, 2=not too important, 3=somewhat important, 4=pretty important, 5=very important; expectations: 1=poor, 2=fair, 3=good, 4=very good, 5=excellent). The present data indicate that these two subscales have adequate internal consistency (value: $\alpha = .74$, CFI = 1.00, RMSEA = 0.00; expectancy: $\alpha = .88$, CFI = 1.00, RMSEA = 0.00). Higher scores on these scales indicate more value or importance of future success and more expectations for future success. Change scores that represented the difference between baseline and the follow up were calculated and included as potential mediators.

Self-esteem. Self-esteem was assessed with the Rosenberg Self-Esteem scale (Rosenberg, 1979). This measure consists of 10 items and was designed to assess overall self-esteem in adolescence (e.g., *I feel that I'm a person of worth, at least on an equal plane with others; At times, I think I am no good at all* (reverse coded); *On the whole, I am satisfied with myself*). Youth responded on a 4-point likert scale (0=strongly disagree, 1=disagree, 2=agree, 3=strongly agree). Self-esteem was assessed at both time-points. Individual items on the scale were averaged together at baseline and at the follow up. Higher scores are related to higher overall

self-esteem. The Rosenberg Self-Esteem measure is well established and has been shown to be internally consistent (αs from a prior study ranges from .88-.90 across 6 time-points that spanned a 3-4 year period; Robins, Hendin, & Trzesniewski, 2001). Although a reliability analysis suggests that the scale in the present study had acceptable internal consistency (α = .86), a confirmatory factor analysis suggests that the scale is not a particularly good fit to the data (CFI = .75; RMSEA = .17). A change score that represented the difference between baseline and the follow up was tested as a potential mediator.

I used two of the previously described outcome variables to serve as mediators. In particular, I tested whether the effect of involvement with the justice system was attributable to changes in school attachment or school truancy. The change scores for each of these scales, which represented change from baseline to the follow up, were calculated and used as potential mediators (these two change scores were also used as outcome variables, see previous section).

Peer and social context.

Peer delinquency. The degree of association with delinquent peers was based on a method previously used with a sample of serious juvenile offenders (Monahan et al., 2009). In particular, I first calculated four variables to represent four unique peer delinquency domains: peer antisocial behavior, peer antisocial influence, proportion of close friends who have been arrested, and the proportion of close friends who have spent time in jail. First, peer delinquency antisocial behavior and peer delinquency antisocial influence were measured with the Association with Deviant Peers scale (Thornberry et al., 1994). This measure captures two characteristics of peer relationships: the degree of peers' antisocial *behavior* and the extent of peers' antisocial *influence*. Specifically, the peer delinquency antisocial behavior subscale required youth to state how many of their friends (5-point likert scale; 1=none of them, 2=very

few of them, 3=some of them, 4=most of them, 5=all of them) have engaged in 13 different delinquent behaviors in the past 6 months (e.g., stolen something worth more than $100, hit or threatened to hit someone, carried a gun). The peer delinquency antisocial influence subscale consisted of 7 items and asked whether any of youth's friends have suggested that the participant engage in illegal behaviors. Questions were asked similarly to the peer antisocial behavior subscale (i.e., *How many of your friends have*…) and items included behaviors such as suggested that you should steal something, suggested that you should carry a weapon, and suggested or claimed that you have to be high to have a good time. Response options for the peer delinquency antisocial influence scale were the same as the response options for the peer delinquency antisocial behavior scale (1=none of them to 5= all of them). These two scales were individually standardized and higher scores on these scales indicate more antisocial behavior or more antisocial influence (peer delinquency antisocial behavior: α= .92, CFI = .705, RMSEA = .152); peer delinquency antisocial influence: α= .85; CFI = .806, RMSEA = .211). The next two peer delinquency measures assessed the extent that participants' closest friends have been involved in the justice system. As mentioned previously in the recruitment section, youth nominated up to 5 of their closest friends and follow-up questions assessed whether each of these friends was ever arrested or jailed. If the participant nominated the same peer in the follow up interview, the questions asked whether the friend was arrested or jailed since the baseline interview. If the participant nominated a new friend at the follow up interview, the questions asked whether the peer had ever been arrested (same as the baseline). The proportion of nominated peers who have been arrested (calculated by dividing the total number of friends who were arrested by the total number of close friends [maximum 5 close friends]) and the proportion of peers who have spent time in jail (calculated by dividing the total number of close friends who have spent time in jail

by the total number of close friends) were computed. Both variables represent the proportion of close friends who have *ever* been arrested (or jailed). Proportion scores were standardized for each time-point and combined with the antisocial behavior and antisocial influence scales mentioned previously to form one composite of peer delinquency at Time 1 and one at Time 2 (to be consistent with Monahan et al., 2009). Although the data in the present study demonstrated that the four variable composite had high internal consistency ($\alpha = .78$), the fit statistics of the one-factor solution were poor (CFI = .770; RMSEA = .447). Baseline peer delinquency was used in the propensity score matching analysis and change scores that represented the difference between baseline and follow scores were tested as potential mediators.

Neighborhood climate was assessed with a scale from Eccles et al., (1998). This measure consisted of 6 items that assessed the subject's perception of academic and occupational opportunities within his neighborhood (e.g., *My chances of getting ahead and being successful are not very good* (reverse coded), *In my neighborhood, it's pretty easy for a young person to get a good-paying, honest job*). Youth responded on a 5-point likert scale (1=strongly disagree, 2=disagree, 3=neither agree nor disagree, 4=agree, 5=strongly agree). Average scores at Time 1 and Time 2 were computed (after reverse coded items were recoded). Higher scores are representative of a youth perceiving more opportunities for future success within his neighborhood ($\alpha = .66$; CFI = .86; RMSEA = .12). This measure has also been used in research with a similar sample of juvenile delinquents (Chung, Mulvey, & Steinberg, 2011).

Parents.

Parental involvement. Parental involvement was assessed with an adapted version of the Parental Monitoring Inventory (Steinberg et al., 1992). Fourteen items assessed parental effort (5 items), parental knowledge (5 items), and parental monitoring (4 items). Sample effort items

include *How much does* (primary caregiver) ***try to know*** *who you spend your time with*? *How much does* (primary caregiver) ***try to know*** *where you go at night?* We used the same type of questions to assess parental knowledge by changing the stem to *"How much does* (primary caregiver) ***really know****…"* (instead of *How much does primary caregiver try to know.)* For example, sample items from the parental knowledge scale include *How much does* (primary caregiver) ***really know*** *who you spend your time with? How much does* (primary caregiver) ***really know*** *where you go at night?* Youth responded to these 10 parallel questions (5 effort items and 5 knowledge items) on 4-point likert scale (effort: 1=doesn't try at all, 2=tries a little bit, 3=tries a lot, 4=tries extremely hard; knowledge: 1= doesn't know at all, 2=knows a little bit, 3=knows a lot, 4=knows everything). Additionally, four questions assessed the degree of monitoring or supervision. A sample item from the parental monitoring scale includes *How often do you have a set time to be home on school or work nights?* Youth responded to the parental monitoring scale using a 4-point likert scale (1=never, 2=sometimes, 3=usually, 4=always). Items on each of these scales were averaged together to form three scales: parental effort, parental knowledge, and parental monitoring. Reliability analysis with the present data indicated that these scales had low, but acceptable internal consistency (effort: α =.77; knowledge: α =.84; monitoring: α =.65). However, confirmatory factory analysis indicated that the scales were not a great fit to the data (effort: CFI = .96, RMSEA = .11; knowledge: CFI = .98, RMSEA = .09). The monitoring factor had the poorest fit in the present data (CFI = .89; RMSEA = .20). All three scales were standardized and averaged together to form a composite of parental involvement (α =.86; 3-factor solution: CFI = .85; RMSEA = .09). The parental involvement composite was assessed at both time-points, and the difference between baseline and follow up scores on each scale was included as a potential mediator.

Matching variables[12] .

Demographics. Demographics were self-reported during the baseline interview, and this information was confirmed with official records for the arrested sample. Specifically, date of birth, race and ethnicity, socioeconomic status (highest education of participants' parents), and whether the youth's biological parents were currently married were obtained via self-report and these variables were included in the matching analysis.

Prior (lifetime) offending. Prior offending was assessed via self-report using the previously described Self-Report of Offending scale (Huizinga et al., 1991). The prior offending variable represents the count of unique types of behaviors (up to 24) that a youth engaged in at any point prior to the baseline interview. This lifetime variety score was based on baseline data, with higher scores indicating that a youth has engaged in more types of illegal behavior over his lifetime. As mentioned previously, official records were used to confirm that youth in the no-contact sample never had any official contact with the juvenile justice system and that the Crossroads sample only had one contact with the juvenile justice system (the arrest that made them eligible for Crossroads).

Substance use. As described previously, substance use was assessed with an adapted version of the Substance Use and Abuse Inventory (Chassin et al., 1991). The lifetime variety score at baseline was used in the matching analysis. As a reminder, the variety score represents the count of different substances used over the participant's lifetime. For example, if a youth has tried marijuana, alcohol, and cocaine at least once in his life, he would receive a substance use variety score of 3.

[12]Variables that were used to match arrested and no-contact participants. These are variables that are theoretically related to the propensity of being caught (i.e., having juvenile justice system contact).

Intelligence. An IQ proxy was assessed with the Wechsler Abbreviated Scale of Intelligence (WASI; Wechsler, 1999). Although the WASI consists of four subtests, only two were used in the present study: Vocabulary and Matrix Reasoning. These two subtests, which can be completed in approximately 15 minutes, can be combined to produce a proxy for an intelligence quotient (IQ). This IQ proxy can be used to screen and evaluate individuals between the ages of 6 and 89 (scores on the WASI have been normed and standardized). WASI scores were checked for accuracy two times: once by an RA who did not do the original administration or scoring and once by this graduate student supervisor. Research has indicated that WASI scores are highly correlated with summary scores on the Wechsler Adult Intelligence Scales—Third Edition (WAIS-III) with normative samples (r = .84 to .92) and clinical samples(r = .71 to .82; Axelrod, 2002).

Maturity of judgment (psychosocial maturity). Maturity of judgment was assessed with three components (Steinberg & Cauffman, 1996): temperance (impulse control and suppression of aggression), perspective (consideration of others and future orientation), and responsibility (personal responsibility and resistance to peer influence) and was assessed using four measures: the Weinberger Adjustment Inventory (WAI; Weinberger & Schwartz, 1990), the Future Outlook Inventory (FOI; Cauffman & Woolard, 1999), the Psychosocial Maturity Inventory (PSMI; Greenberger et al., 1974), and the Resistance to Peer Influence scale (RPI; Steinberg & Monahan, 2007). One index of psychosocial maturity was created by individually standardizing all of the subscales obtained in the baseline interviews (impulse control, suppression of aggression, consideration of others, future outlook inventory, personal responsibility, and resistance to peer influence) and averaging these standardized scores (6 scales used to create the MOJ composite α = .60; 3 factor solution—temperance, perspective, and responsibility: CFI =

.65, RMSEA= .05). Higher scores on this composite are indicative of more psychosocial maturity.

The WAI (Weinberger & Schwartz, 1990) is a 23-item[13] scale that assessed temperance (impulse control and suppression of aggression) and consideration of others. Youth used a 5-point likert scale to state how true each statement was for them (1=false, 2=somewhat false, 3=not sure, 4=somewhat true, 5=true). Temperance is the mean of 15 items (α = .84) and has items in two domains (impulse control and suppression of aggression)[14]. Sample items for the impulse control domain (α = .76) are *I'm the kind of person who will try anything once, even if it's not safe* (reverse coded*), I become 'wild and crazy' and do things other people might not like* (reverse coded), and *I stop and think things through before I act*. Sample items for the suppression of aggression domain (α = .79) are *If someone does something I really don't like, I yell at them about it* (reverse coded), *People who get me angry better watch out* (reverse coded), and *When someone tries to start a fight with me, I fight back* (reverse coded). The consideration of others scale consists of 7 items (α = .67). These are behaviors such as *Doing things to help people is more important to me than almost anything else, I enjoy doing things for other people, even when I don't receive anything in return,* and *I try very hard not to hurt other people's feelings*. Higher scores on all of these subscales indicate that the youth has higher maturity (i.e., more impulse control, more suppression of aggression, or greater consideration for others). Research with a sample of serious juvenile offenders found similar reliability coefficients with their data: impulse control: α = .76; suppression of aggression: α = .78; temperance (combined impulse control and suppression of aggression subscales): α = .84; consideration of others: α = .73; Mulvey, Steinberg, Piquero, Besana, Fagan, Schubert, Cauffman, 2010). Standardized

[13] One of the original WAI items was not used to calculate any of the subscales.
[14] Impulse control and suppression of aggression were highly correlated in this sample (*r* = .51).

scores for temperance and consideration of others were created with the baseline data and these two scales were included in the maturity of judgment composite.

The FOI (Cauffman & Woolard, 1999) was developed based on items created for the Life Orientation Task (Scheier & Carver, 1985), the Zimbardo Time Perspective Scale (Zimbardo, 1990), and the Consideration of Future Consequences Scale (Strathman, Gleicher, Boninger, & Edwards, 1994) and consisted of 7 items (α = .63). Sample items include *I will keep working at difficult, boring tasks if I know they will help me get ahead later, I will give up my happiness now so that I can get what I want in the future, I don't plan* (reverse coded), and *I take each day as it is* (reverse coded). Youth responded on a 4-point likert scale (1=never true, 2=rarely true, 3=often true, 4=always true). Internal consistency of the FOI was similar in a sample of serious adolescent offenders (α = .68; Mulvey et al., 2010). Higher scores indicate more future orientation. The seven items were standardized (with the baseline data) and were used as one aspect of the maturity of judgment composite.

The PSMI (Greenberger et al., 1974) was a 30-item measure that assessed personal responsibility and aspects of psychosocial maturity in three domains (10 items for each domain): self-reliance, identity, and work orientation. Self reliance items include behaviors such as *It's not very practical to decide what kind of job you want because that depends so much on other people* (reverse coded), *Luck decides most things that happen to me* (reverse coded), and *It is best to agree with others, rather than say what you really think, if it will keep the peace* (reverse coded). Sample items for the identity subscale are *I'm the sort of person who can't do anything really well* (reverse coded), *I act like something I'm not a lot of the time* (reverse coded), and *Nobody knows what I'm really like* (reverse coded). The work orientation subscale included items such as *Hard work is never fun* (reverse coded), *I hate to admit it, but I give up on my work*

when things go wrong (reverse coded), and *I seldom get behind on my work*. When used with a similar sample, the internal consistency of these subscales was adequate (total score: α = .89; self reliance: α =.77; identity: α =.78; work orientation: α =.73; Mulvey et al., 2010). Analyses in the present study also indicate that the PSMI subscales were a good fit to the data (total score: α = .86; self reliance: α =.73; identity: α =.76; work orientation: α =.74). All items were averaged and standardized, and this variable was included in the maturity of judgment composite.

The RPI (Steinberg & Monahan, 2007) is a 10-item measure that assessed the degree to which adolescents engage in independent decision-making in the presence of their peers. The RPI is a two-part measure. First, youth were presented with two opposing situations and they decided which statement was more similar to how they usually act (*Some people change the way they act so much when they are with their friends that they wonder who they really are* **or** *Other people act the same way when they are alone as they do when they are with their friends*; *Some people will not break the just because their friend say that they would* **or** *Other people would break the law if their friends said they would do it*). After participants decided which statement was more representative of how they usually act, they had to decide whether the statement was *really true* or *sort of true* for them. Participants followed this same format for 10 different scenarios (all testing different aspects of peer influence). This measure had adequate internal consistency (α = .71) in the present data as well as in a sample of serious delinquents (α = .73; Steinberg & Monahan, 2007). Higher scores indicate that a youth has a higher resistance to peer influence (i.e., less susceptible to the influence of peers). This scale was standardized and used as one aspect of maturity of judgment.

Callous-unemotional traits. Participants' callous-unemotional traits were assessed with the Inventory of Callous-Unemotional traits (Frick, 2004). This 24-item scale measured the

presence of callous-unemotional traits in youth and was developed based on the Antisocial Process Screening Device (APSD; Frick & Hare, 2001). Sample items include *I express my feelings openly; I do not care who I hurt to get what I want; and I do not care if I get into trouble.* Youth state the degree to which each statement is representative of how they usually act/feel, and they respond to items on a 4-point likert scale, ranging from 0=not at all true to 3=definitely true. After 12 items were reverse scored, a total score for all 24 items was calculated. The total score was the sum of the responses on all items, with higher scores indicating that youth self-reported more callous-unemotional traits ($\alpha = .77$).

School truancy. As described previously, school truancy was measured with 5 youth self-report questions. Self-report questions assessed truancy from school over the past 6 months: *How many times have the following things happened to you over the past six months…I was late for school, I cut or skipped school, I was absent from school, I got in trouble for missing too many days, I had to go to truancy court*. Youth responded to these 5 questions with a 5-point likert scale (0=never,1=1-2 times, 3=3-6 time, 7=7-10 times, 10=10 or more times) and all items were averaged together. School truancy at baseline was used in the propensity score matching.

Parental involvement. As described previously, parental involvement was assessed with an adapted version of the Parental Monitoring Inventory (Steinberg et al., 1992), and the baseline values were used in the matching analyses. Five items assessed parental effort, five items measured parental knowledge, and four items measured parental monitoring. Sample items include *How much does* (primary caregiver) ***try to know*** *who you spend your time with* (effort)? *How much does* (primary caregiver) ***really know*** *who you spend your time with* (knowledge)*? How often do you have a set time to be home on school nights* (monitoring)? I used the

composite score, which combined information from all three scales, for the propensity score matching analysis.

Parental antisocial behavior. Parental antisocial behavior was assessed with a scale that was previously described, the Association with Deviant Peers scale (Thornberry et al., 1994). However, instead of asking about peers, the prompt was, *Now I'm going to ask you the same questions I just went through, but this time I'd like you to think about your parents, and whether they have done these things*. This scale measured the degree of parents' antisocial *behavior* and required youth to answer whether either of his parents engaged in any of 13 possible antisocial and illegal behaviors in the past 6 months (e.g., stolen something worth more than $100, hit or threatened to hit someone, carried a gun). Youth responded "Yes" or "No" to all 13 items. "Yes" responses were given 1 point and "No" responses were given 0 points, and the total score is the mean of the 13 items (α =.66).

Neighborhood characteristics. Neighborhood characteristics was assessed by 21 self-report questions that measured the physical and social disorder in adolescents' neighborhoods (Sampson & Raudenbush, 1999; Sampson et al., 1997; Sampson, 1997). Adolescents responded on a 4-point likert scale (1=never, 2=rarely, 3=sometimes, 4=often) to *How often does each of the following occur within your neighborhood*. Sample items include empty beer bottles on the streets or sidewalks, gangs hanging out, and needles or syringes. Items were averaged together and higher scores indicate worse neighborhoods (α = .93). Previous research with adolescent offenders found that this measure has similar internal consistency (α = .92; Chung & Steinberg, 2006).

Peer delinquency. As described previously, peer delinquency at baseline was used in the propensity score matching analysis. In particular, I used the composite of all four subscales to

match the contact and no-contact youth: peer antisocial behavior, peer antisocial influence, proportion of close friends who have been arrested, and the proportion of close friends who have spent time in jail.

IV. Results

Plan of Analysis

Propensity score matching. The ultimate goal of the present study is to the estimate the effect of the justice system on adolescent development. Ideally, the gold standard for assessing treatment effects is a randomized control trial (e.g., Austin, Grootendorst, & Anderson, 2007; Campbell & Stanley). If contact with the justice system was assigned completely at random, we would have a natural (randomized) experiment and would not need to adjust for pre-existing differences between those who are arrested and those who have never been arrested. However, because it is assumed that youth are not randomly assigned to be arrested, and we cannot randomly assign youth who violate the law to be either arrested or not, a randomized control trial is not possible in the contemporary research context. As such, modern researchers who want to investigate the impact of an arrest typically rely on observational data. In observational data, there may be important differences between those who are arrested by the police and those who avoid law enforcement detection; participants are naturally selected and sorted, either by self or others, into treatment groups (Austin et al., 2007). These *selection effects* can severely confound the estimation of treatment effects.

As such, a statistical technique that accounts for selection effects is needed for observational studies. Propensity score matching is a technique that can approximate random assignment to treatment (in this case, the "treatment" group is the group who was exposed to a justice system intervention) by matching on many variables that are related to treatment

assignment (Austin et al., 2007; Dehejia & Wahba, 2002; Rosenbaum & Rubin, 1983). This helps balance the groups, in our case, arrested and non-arrested youth, with respect to the measured and unmeasured variables. It is important to note that many differences between the contact and no-contact samples in the present study were naturally constrained due to the recruitment methodology (e.g., restricted age range, specific eligibility criteria). With propensity score matching, no-contact and arrested youth are matched holistically based on their conditional *probabilities* (i.e., "propensities") of being arrested (based on a combination of demographic, psychosocial, behavioral, and individual characteristics that are known to be related to being arrested; Rosenbaum & Rubin, 1983; Farrington, 2009). As such, the first step in propensity score matching is to estimate a conditional binary logistic regression, with whether youth were in the arrested group or no-contact group as the outcome variable. This regression is estimated with all of the matching variables included as covariates in the model (Rosenbaum & Rubin, 1983). There is much debate about how to select variables to be used in the matching analysis (e.g., Austin et al., 2007). I included variables that were theoretically related to treatment assignment (i.e., being arrested), many of which were also strongly related to the outcome variables (i.e., true "confounders"). The risk associated with including variables that are associated with outcome variables is that it can decrease your ability to form matches, however, high quality matches were indeed formed in the present study (see below). In Monte Carlo simulation studies, results indicated that including true or potential confounding variables in the matching analysis was related to greater overall accuracy in the estimation of treatment effects (Austin et al., 2007).

The predicted values generated from the conditional binary logistic regression represent the *propensity score* (i.e., the probability of being arrested). This propensity score (predicted probability of arrest) is the variable used to form the matches between the groups. In this

context, we can assume that individuals who have the same probability of having justice system contact (that is, individuals with the same "propensity score") have been randomly assigned to be arrested or not, thus creating a quasi-randomized experiment.

After the propensity scores (probability of receiving juvenile justice system contact, given the values of the matching variables) were estimated and saved as a new variable in the data set, I checked whether the propensity scores for no-contact youth overlapped with the arrested youth. This is the first step in determining whether it is possible to form high-quality matches between the two groups. As described in the next section, the propensity score distributions indeed overlapped and I was able to form high quality matches. The next step is to yoke youth in the two groups (arrested and no-contact) who have the most similar propensity scores.

Two data sets were generated. One matched data set with no-contact and informal youth and one matched data set with no-contact and formally processed youth. Parallel analyses in these data sets were used to investigate whether different intensities of involvement with the juvenile justice system have different effects on academic outcomes, substance use, and antisocial and illegal behavior.

To ensure the best possible matches between arrested and no-contact youth, I used nearest-neighbor matching with replacement. With this technique, no-contact youth were matched to the arrested youth who had the most similar probability of being arrested (most similar propensity score), regardless of whether the arrested youth was already used in a match. The disadvantage to this approach is that arrested youth can be matched to more than one no-contact youth. The advantage of this approach is that better overall matches are produced (e.g., Dehejia & Wahba, 2002). Although matching with replacement has the inherent risk of putting

more weight on some youth's data (usually individuals at the extremes), informal youth were matched about 1.37 times (*sd* = 1.19) and formal youth were matched, on average, 1.48 times (*sd* = 0.48). The median and mode for formal and informal youth was 1 match. Matching on the specific propensity score (as opposed to matching within stratified propensity score quintiles) is the recommended approach and has been shown in Monte Carlo simulation studies to be better at creating balance between the groups (Austin et al., 2007).

After the matches were made, I confirmed the success of the matching algorithms. To do this, I investigated differences between the arrested and no-contact samples pre- and post-matching. As shown in Tables 7 and 8, the non-significant differences in the matched data sets suggest that the propensity score pairings correctly diminished group differences ("selection effects").

Table 7. Differences Between No-Contact and Informal Youth in Unmatched and Matched Data Sets.

Differences between no-contact and informal samples in unmatched and matched data sets.

		Mean		t-test	
		No-contact	Formal	t	$p > \|t\|$
Age	Unmatched	15.85	15.44	2.77	0.01**
	Matched	15.83	15.76	0.41	0.68
Hispanic	Unmatched	0.72	0.76	-0.92	0.36
	Matched	0.71	0.59	1.67	0.10
Black	Unmatched	0.03	0.01	1.69	0.09
	Matched	0.03	0.09	-1.80	0.07
Highest ed of parents	Unmatched	5.67	4.97	2.49	0.01**
	Matched	5.67	5.78	-0.33	0.74
Bio parents married	Unmatched	0.40	0.39	0.24	0.81
	Matched	0.41	0.29	1.67	0.10
Prior offending	Unmatched	0.15	0.15	0.07	0.94
	Matched	0.15	0.14	0.59	0.56
Substance use	Unmatched	2.35	2.24	0.42	0.68
	Matched	2.38	2.15	0.78	0.44
IQ	Unmatched	95.91	90.66	3.69	0.00***
	Matched	96.33	96.01	0.17	0.87
Maturity of judgment	Unmatched	0.25	-0.05	2.58	0.01**
	Matched	0.25	0.26	-0.10	0.92
Callous-unemotional traits	Unmatched	25.04	26.59	-1.68	0.09
	Matched	24.97	25.10	-0.11	0.91
Truancy	Unmatched	2.13	1.88	1.20	0.23
	Matched	2.13	1.86	1.17	0.25
Parental involvement	Unmatched	-0.24	0.03	-2.34	0.02*
	Matched	-0.24	-0.24	0.00	1.00
Parental antisocial behavior	Unmatched	0.10	0.06	3.83	0.00***
	Matched	0.10	0.10	0.25	0.80
Neighborhood characteristics	Unmatched	1.79	1.85	-0.94	0.35
	Matched	1.78	1.76	0.27	0.79
Peer delinquency	Unmatched	0.00	-0.08	0.80	0.42
	Matched	-0.01	0.03	-0.34	0.73

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

Table 8. Differences Between No-Contact and Formal Youth in Unmatched and Matched Data Sets.

Differences between no-contact and formal samples in unmatched and matched data sets.

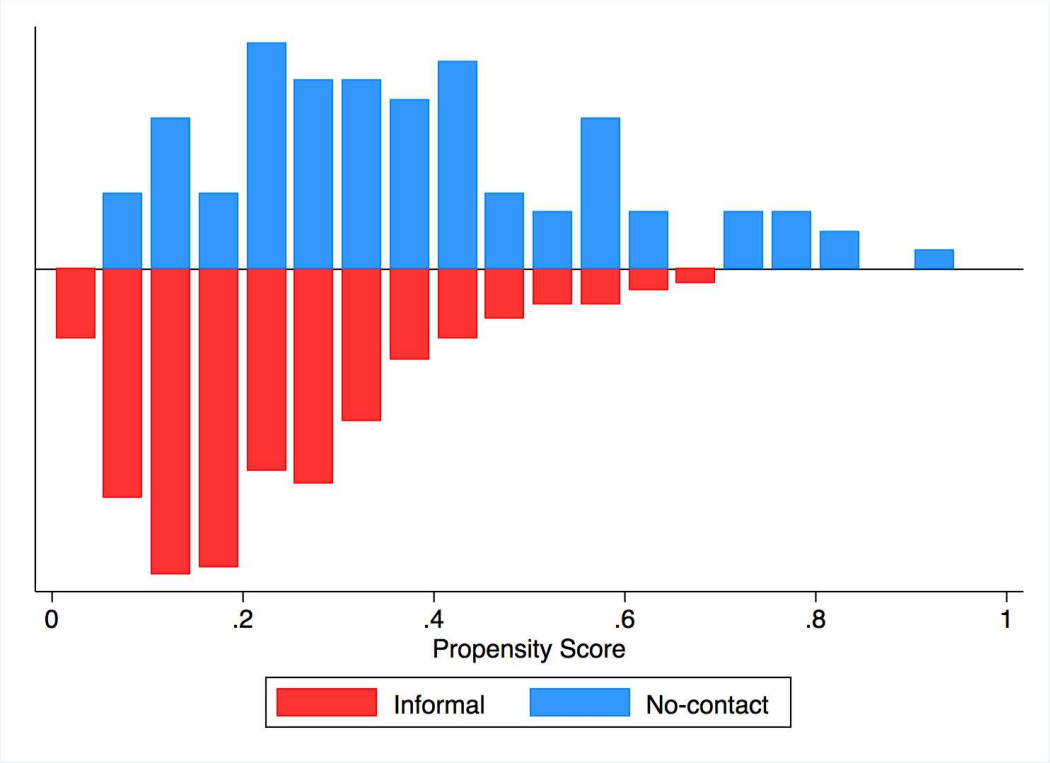
		Mean		t-test	
		No-contact	Formal	t	$p > \|t\|$
Age	Unmatched	15.85	15.54	2.24	0.03*
	Matched	15.83	15.80	0.19	0.85
Hispanic	Unmatched	0.03	0.01	1.24	0.21
	Matched	0.03	0.01	1.01	0.32
Black	Unmatched	0.72	0.80	-1.79	0.07
	Matched	0.71	0.76	-0.82	0.42
Highest ed of parents	Unmatched	5.67	4.88	2.78	0.01**
	Matched	5.67	5.66	0.03	0.98
Bio parents married	Unmatched	0.40	0.31	1.69	0.09
	Matched	0.41	0.39	0.29	0.77
Prior offending	Unmatched	0.15	0.17	-1.06	0.29
	Matched	0.15	0.15	-0.20	0.84
Substance use	Unmatched	2.35	2.56	-0.74	0.46
	Matched	2.38	2.36	0.08	0.94
IQ	Unmatched	95.91	89.36	4.35	0.00***
	Matched	96.33	96.21	0.06	0.95
Maturity of judgment	Unmatched	0.25	-0.04	2.47	0.01**
	Matched	0.25	0.25	-0.01	0.99
Callous-unemotional traits	Unmatched	25.04	26.67	-1.76	0.08
	Matched	24.97	26.70	-1.50	0.14
Truancy	Unmatched	2.13	2.08	0.21	0.83
	Matched	2.13	2.31	-0.62	0.54
Parental involvement	Unmatched	-0.24	0.06	-2.45	0.02*
	Matched	-0.24	-0.25	0.06	0.95
Parental antisocial behavior	Unmatched	0.10	0.07	2.11	0.04*
	Matched	0.10	0.13	-1.36	0.17
Neighborhood characteristics	Unmatched	1.79	1.86	-0.92	0.36
	Matched	1.78	1.72	0.86	0.39
Peer delinquency	Unmatched	0.00	0.09	-0.70	0.48
	Matched	-0.01	0.07	-0.69	0.49

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

The last step is to address the research aims with the data sets derived from the matching analyses. Specifically, a series of models were used to investigate whether (Aim 1), for whom (Aim 2), and how (Aim 3) contact with the juvenile justice system is related to each of the outcome variables. The specific type of regression model varied for different outcome variables, based on the residual distribution of the variable (see Table 10). Importantly, all of the analyses described below were conducted two times. No-contact youth were matched and compared to informally processed youth and no-contact youth were matched and compared to formally processed youth.

Matches between no-contact and informal youth. The propensity scores were similarly distributed for no-contact youth (m = 0.38, sd = 0.19, range: 0.07 to 0.92) and informal youth (m = 0.23, sd = 0.14, range: 0.01 to 0.68). See Table 9 for propensity score descriptives and see Figure 17 for histograms of the propensity scores. Other than four no-contact youth who had missing data on the covariates used to estimate the propensity score, all no-contact youth were used to form a match. As a result, 95 no-contact youth formed high quality matches with informally processed youth. Of the informally processed youth, 69 individuals were used to form a match: 56 were paired once, 9 were paired twice, 2 were paired 3 times, 1 was paired 6 times, and 1 informally processed youth was used in 9 matches (this informal youth had a particularly low probability of being arrested). The average difference in propensity scores between the matched no-contact and informal youth was 0.01 (sd = 0.03; range: 0.00 to 0.24). Only 7 of the matched informally processed youth were nominators of any of the members of the no-contact sample.

Figure 17. Propensity Score Distributions for No-contact and Informal Youth.



Matches between no-contact and formal youth. The propensity scores were similarly distributed for no-contact youth (*m* = 0.38, *sd* = 0.19, range: 0.07 to 0.92) and formally processed youth (*m* = 0.25, *sd* = 0.14, range: 0.02 to 0.84). See Table 9 for propensity score descriptives and see Figure 18 for a visual display of the propensity scores. Ninety-five no-contact youth and 64 formally processed youth were used in the matching analysis. Of the formally processed who were used in a match, 43 were used in one pairing, 14 were used twice, 4 were used 3 times, and 3 were used 4 times. The average difference in propensity scores between the matched no-contact and formally processed pairs was 0.00 (*sd* = 0.01; range 0.00 to 0.03). Only 8 of the matched formally processed youth nominated a peer who eventually enrolled and became a member of the no-contact sample.

Figure 18. Propensity Score Distributions for No-contact and Formal Youth.

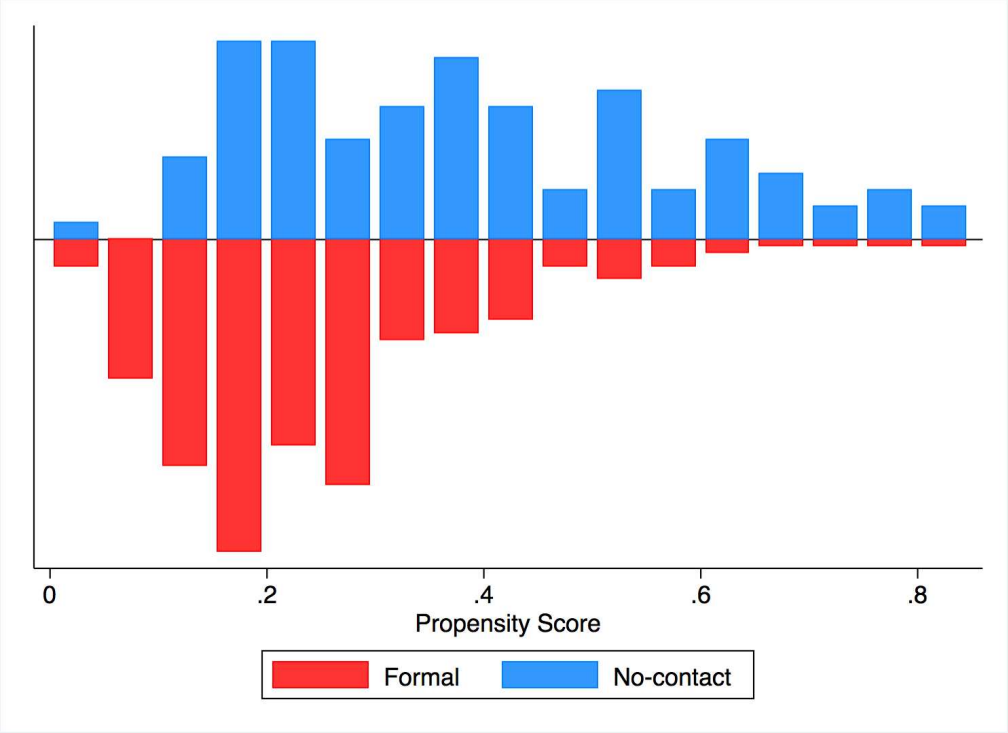


Table 9. Propensity Scores by Group.

Propensity Score by Group.

	Mean (sd)	Median	Range
No-contact	.38 (.19)	.32	.07 to .92
Informal	.23 (.14)	.21	.01 to .68
Formal	.25 (.14)	.22	.04 to .84

Aim 1. Examine whether contact with the juvenile justice system contributes to decreases in school achievement and school attachment and increases in school misconduct, truancy, substance use, and delinquent behavior. I addressed Aim 1 using a variety of models. First, I estimated a series of Generalized Linear Models (GLM) wherein the outcome variables were the individual measures of academic outcomes, substance use, and delinquency

and the predictor was juvenile justice contact (either formal youth or informal youth) relative to no-contact youth. GLM is a flexible extension of linear models (e.g., ordinary least squares regression) that can accommodate variables with many different residual distributions (i.e., normal, exponential, inverse gaussian, poisson, binomial, and multinomial). See Table 10 for the models that corresponded to each outcome variable at baseline and the follow up. Using these models, I investigated 3 different variables for each outcome:

1. Whether the degree of change between baseline and follow up was significantly different for the arrested sample compared to the no-contact sample, controlling for baseline values (i.e., the change or difference between baseline and the follow up; see column 2 in Tables 11 and 12)
2. Whether there were significant differences between the arrested sample and the no-contact sample at baseline (see column 3 in Tables 11 and 12)
3. Whether there were significant differences at the 6 month follow up, controlling for baseline values (see column 4 in Table 11 and 12). This autoregressive approach should produce estimates very similar to the change score estimated in the first analysis.

In addition to these 3 characteristics of each outcome variable (i.e., change score, baseline scores, and follow up scores), I was also interested in whether there was significant within group change between baseline and the follow up (see columns 5 and 6 in Table 11 and Table 12). Naturally, I also tested whether the within group change estimates were significantly different in the two groups (which produces inferences that are very similar to the analysis that used change scores as the outcome variable; column 7 in Table 11 and Table 12). Including an interaction between time and the no-contact versus arrested group variable allows an

investigation of whether there are significant differences in within group change between arrested and non-arrested youth. For each outcome variable, this essentially asks the question, "Does the degree of change between baseline and the follow up differ for arrested and non-arrested youth?" To test these within group change models, I used Response Profile analyses (also called Covariance Pattern Models) with an unstructured error covariance, which is a type of multilevel model that can accommodate the correlated residuals associated with longitudinal data. This type of analysis is appropriate when there are less than 5 time-points, when there is a single categorical variable covariate, and when the design is balanced (Fitzmaurice, Laird, & Ware, 2011; Xavier, 2008).

Box plots for the change scores for all outcome variables are presented in Figures 19 to 28. The predicted values for the models in Aim 1 are presented in Figures 29 to 38. Importantly, I investigated the effect of formal processing, compared to no-contact, and the effect of informal processing, compared to no-contact, separately. In each model, I controlled for the baseline value of the outcome variable (except when the outcome variable was baseline levels; column 2 in Table 11 and Table 12).

It is important to note that I do not present results with school dropout or school expulsions as outcome variables. Fortunately, only a handful of participants dropped out of school or were expelled between baseline and the follow up, thus, there is not enough statistical variability to estimate differential likelihoods of dropping out of school or being expelled. Specifically, in the matched and weighted data sets, 5 formally processed, 1 informally processed, and 0 no-contact youth reported being expelled in the previous 6 months at the follow up interview. In regard to dropping out of school, in the weighted data sets, 4 informally

processed, 0 formally processed, and 0 no-contact youth dropped out of school between baseline and follow up.

Outcome change scores box plots for no-contact youth and the matched sample of informally processed and formally processed youth. Figures 19 to 28.

Figure 19. Box Plot: Change in Offending Frequency

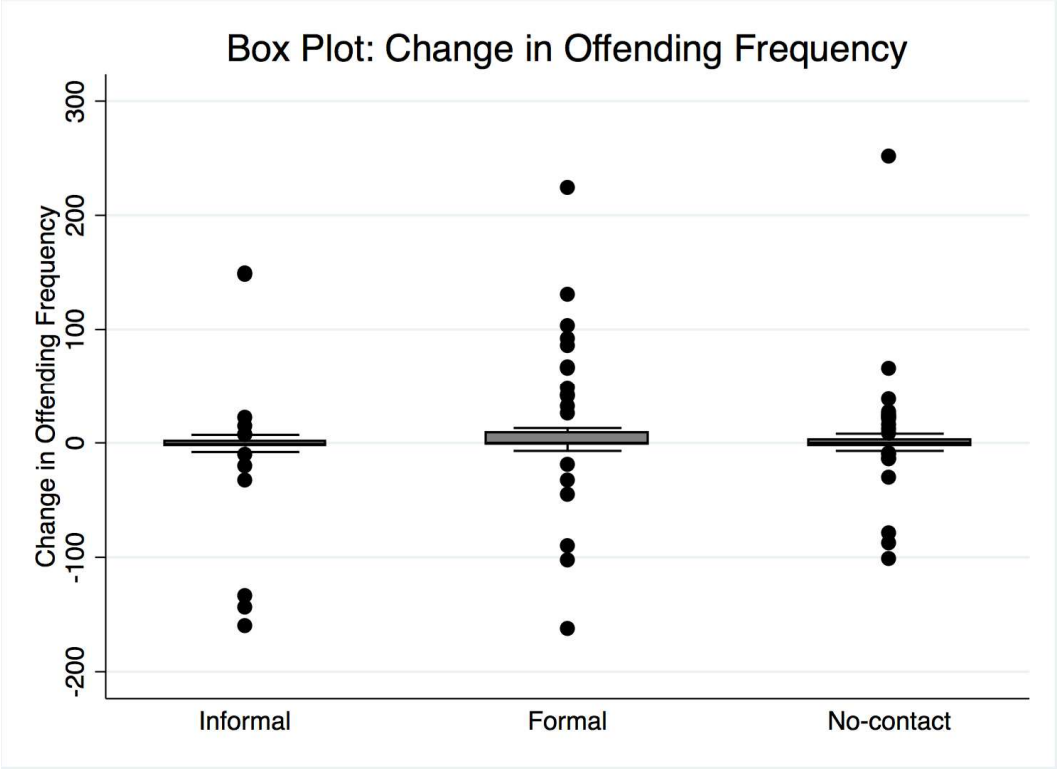


Figure 20. Box Plot: Change in Offending Variety.

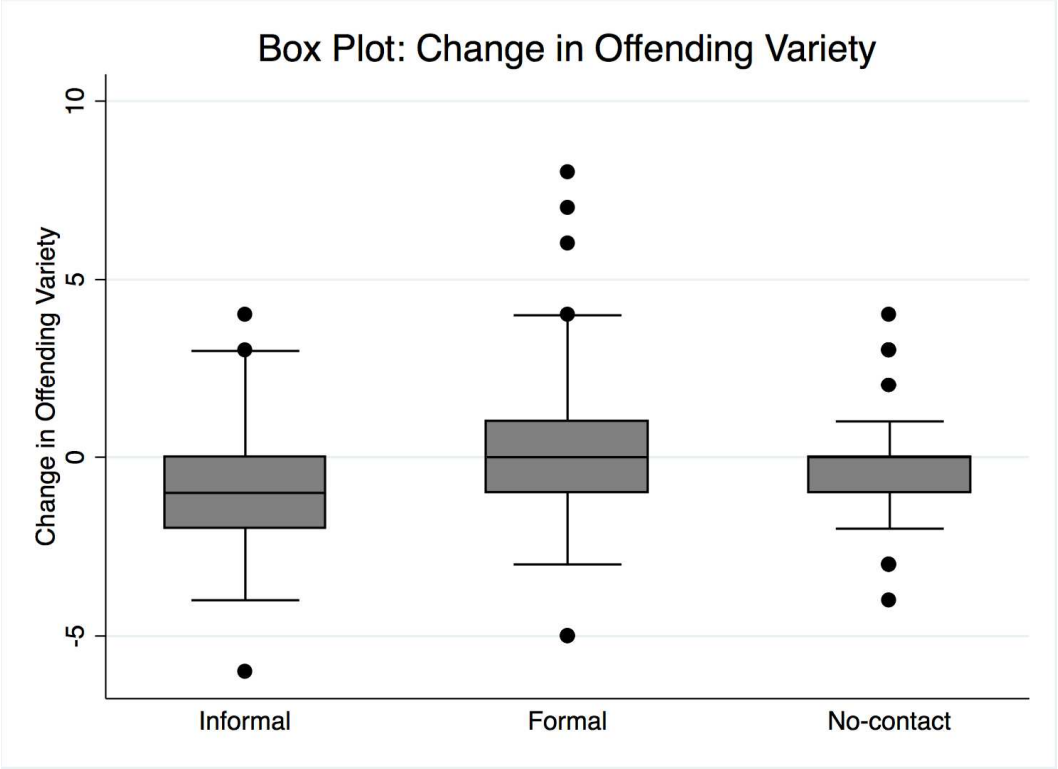


Figure 21. Box Plot: Change in Substance Use Frequency

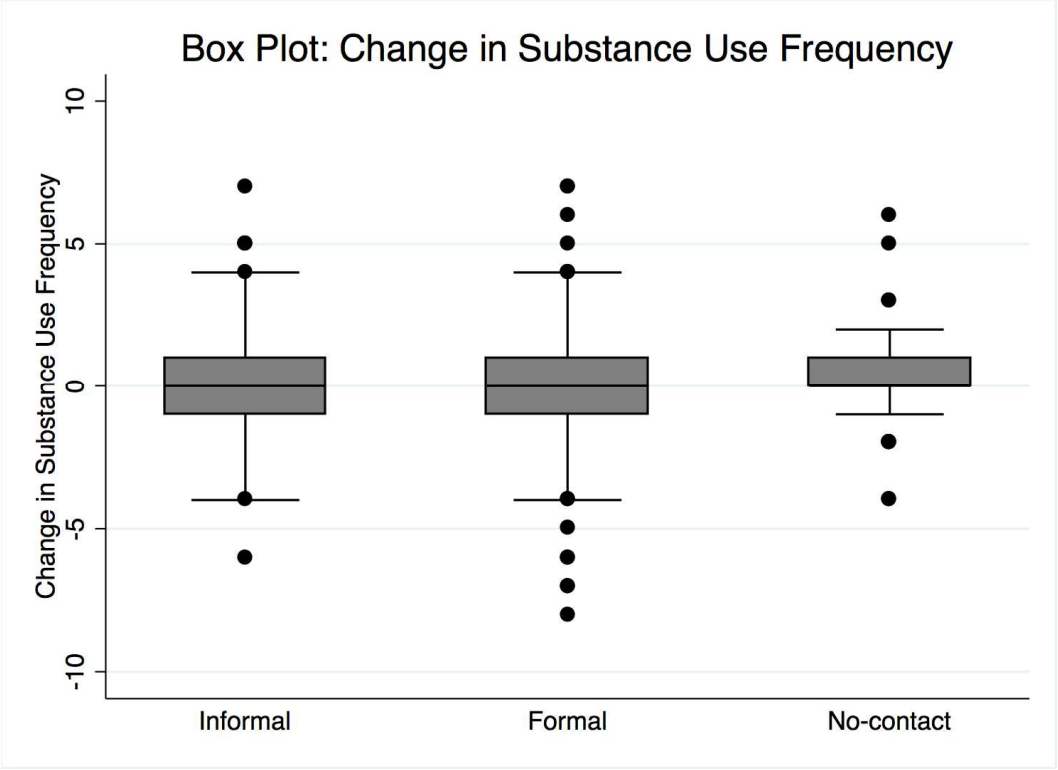


Figure 22. Box Plot: Change in Substance Use Variety.

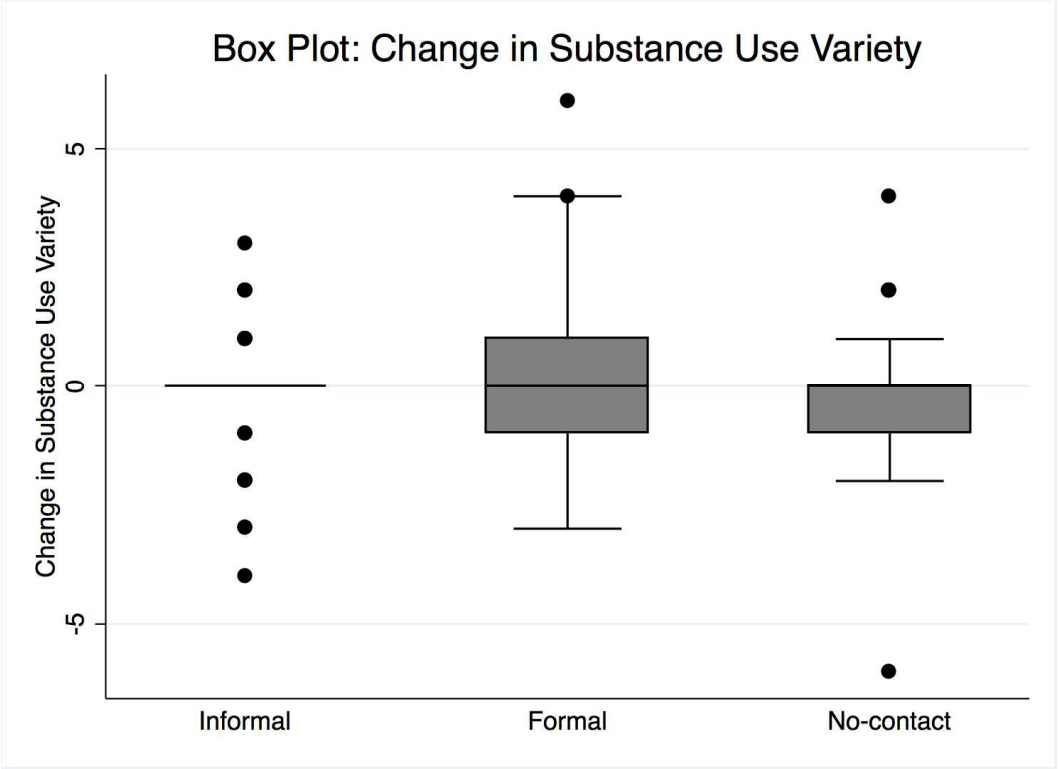


Figure 23. Box Plot: Change in School Bonding

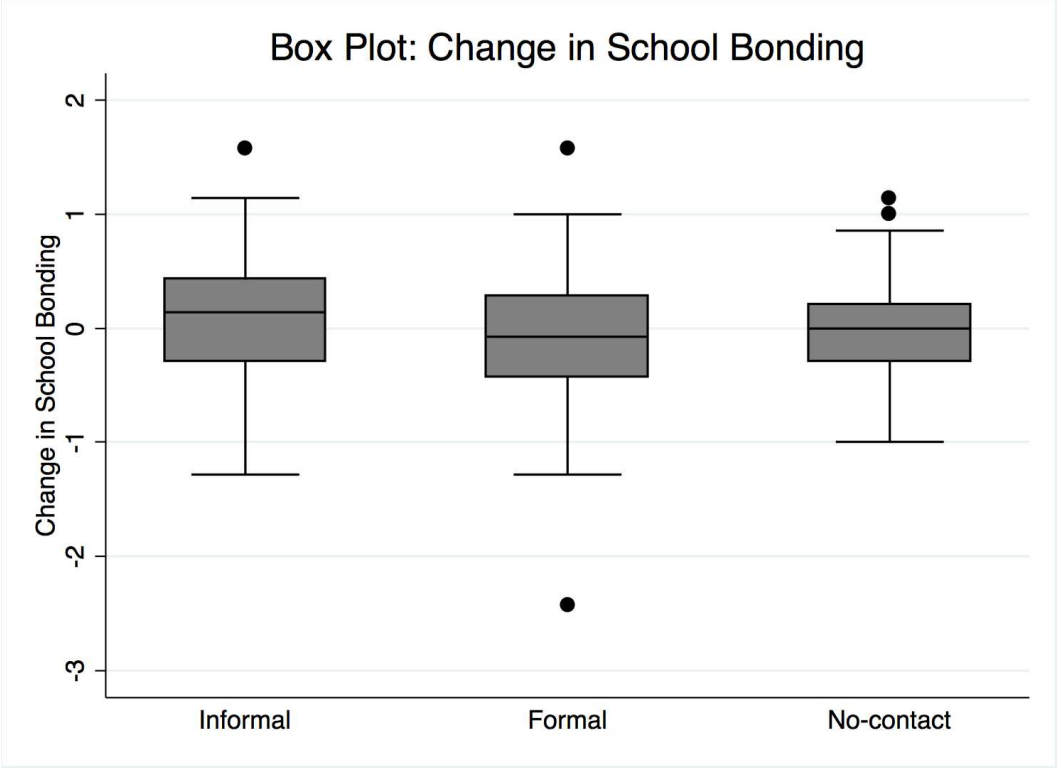


Figure 24. Box Plot: Change in Teacher Bonding

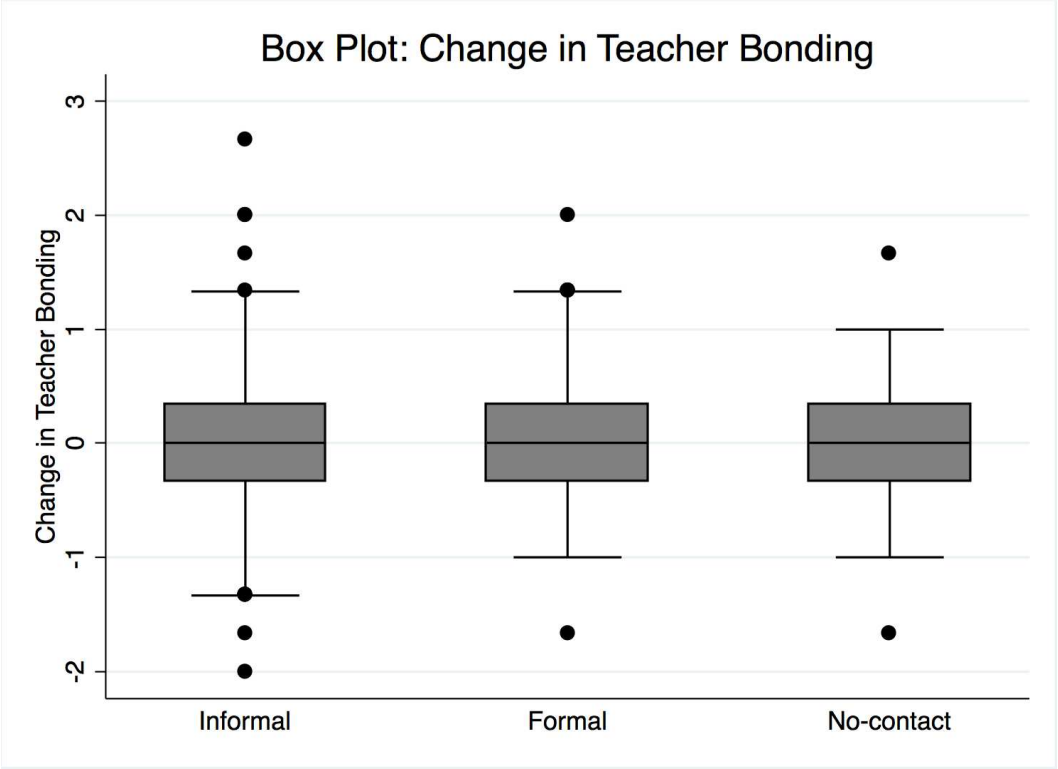


Figure 25. Box Plot: Change in Grades

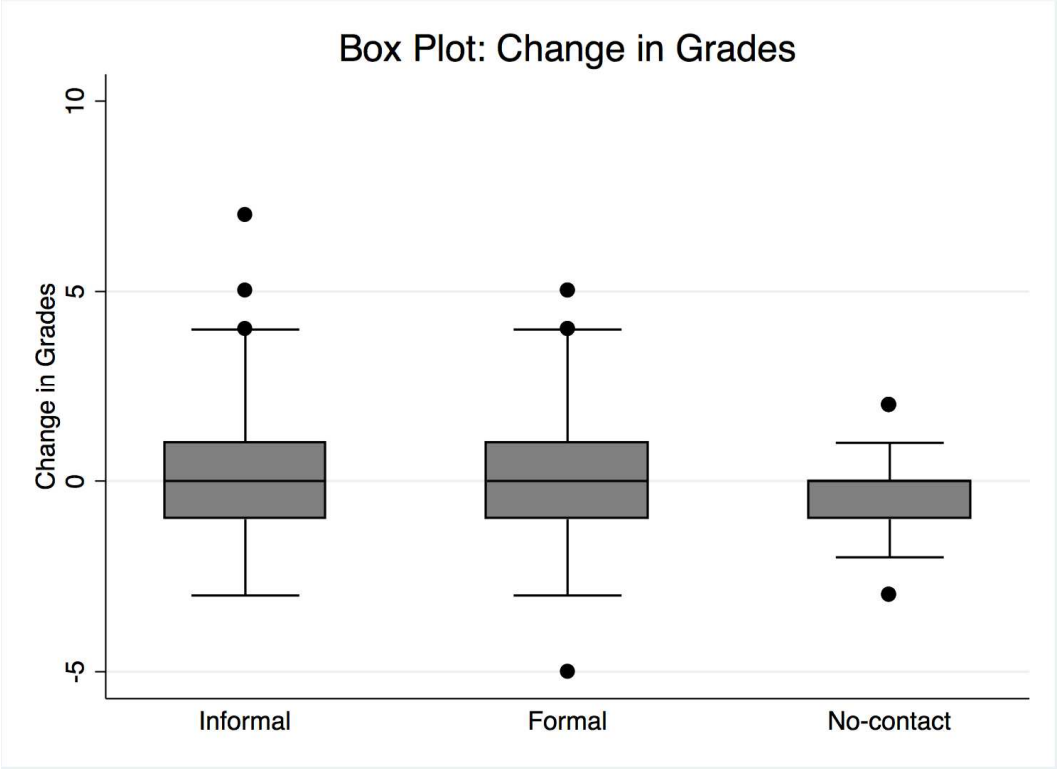


Figure 26. Box Plot: Change in School Misconduct

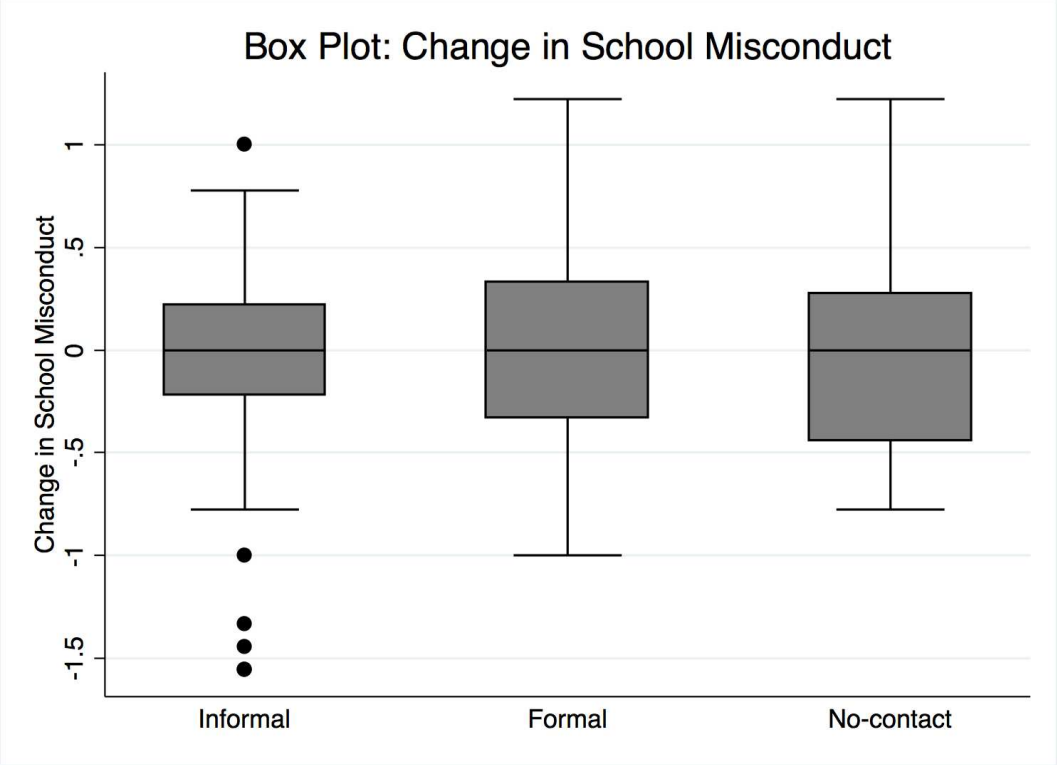


Figure 27. Box Plot: Change in School Truancy

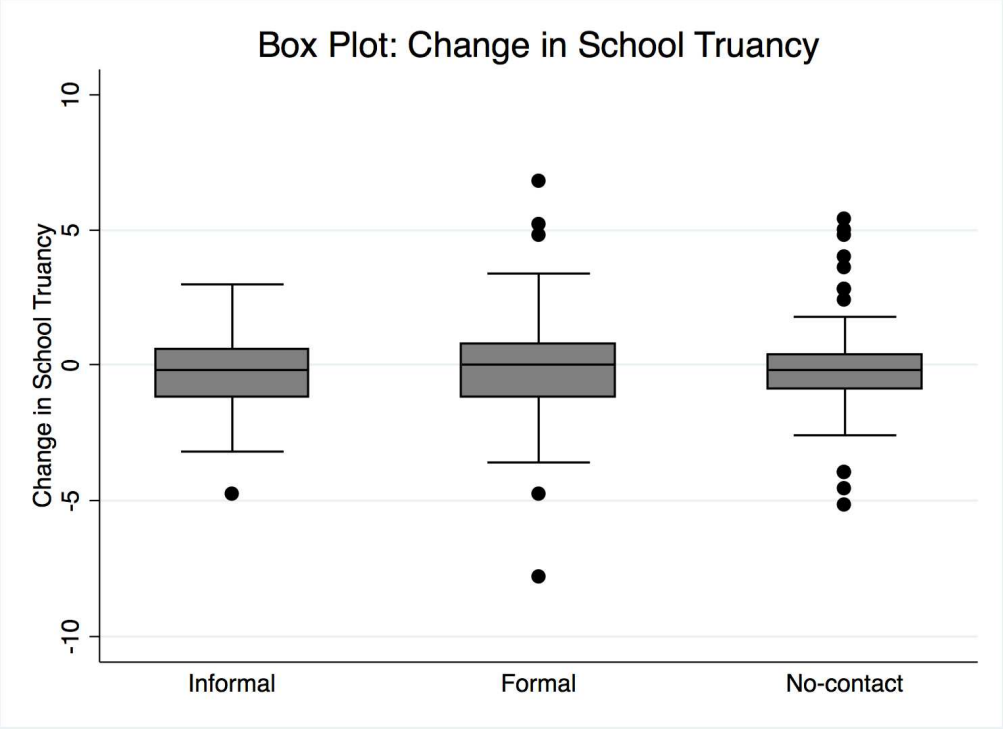
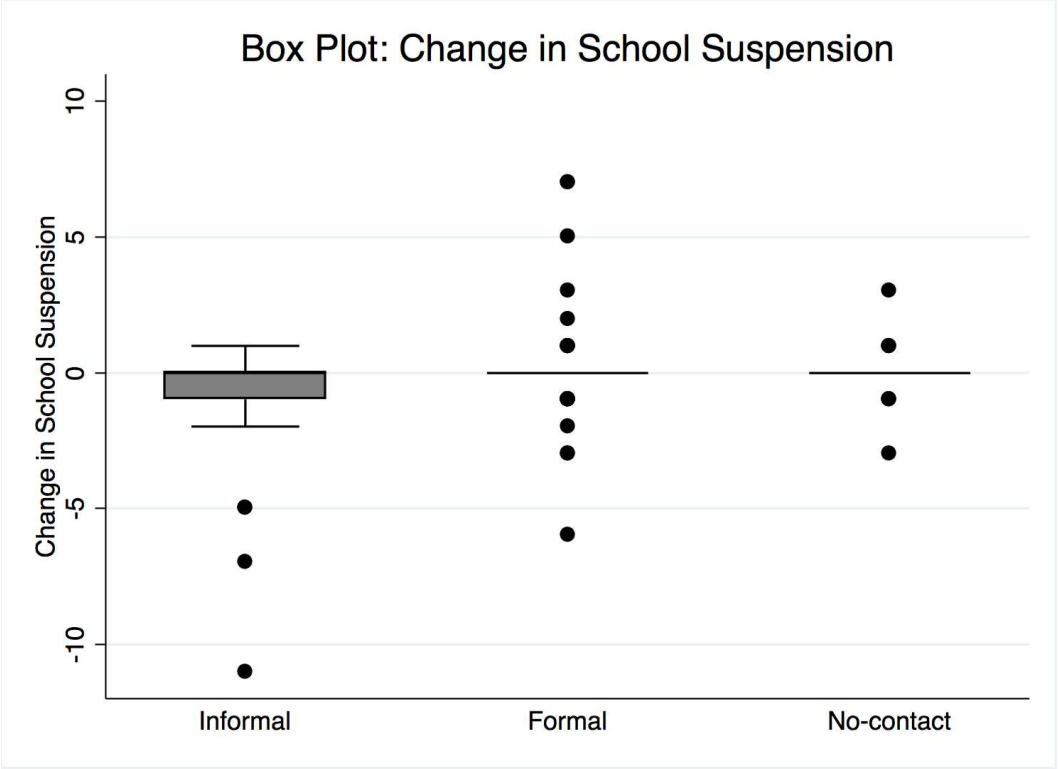


Figure 28. Box Plot: Change in School Suspension



Self-report of offending (no-contact and informal youth). Although it was only at a trend level, the degree of change in self-report of offending frequency between interviews was greater for informal youth than no-contact youth. Informal youth significantly declined in offending frequency between baseline and the follow up, while no-contact youth did not experience any significant change during this period. Also, although no-contact youth were significantly lower in offending frequency at baseline, no-contact youth were significantly higher than informal youth at the follow up. In regard to offending variety, there was a significant difference in the degree of change between the interviews, with informal youth decreasing more than no-contact youth. Although there was no difference in offending variety at baseline, no-contact youth were significantly higher at the follow up interview. Indeed this difference was due to the significant decline in offending variety among informal youth. There was no

difference between baseline and the follow up for the no-contact youth. See Table 11 and Figures 29 and 30.

Self-report of offending (no-contact and formal youth). When no-contact youth were compared to formally processed youth with respect to offending, the opposite pattern emerged. Although no-contact and formally processed youth did not differ in offending frequency at the baseline interview, formal youth engaged in significantly more offending than no-contact youth at the follow up interview. Indeed formally processed youth significantly increased in offending frequency between baseline and the follow up while no-contact youth did not change during this period. In contrast to offending frequency, there were no differences between no-contact and formally processed youth in offending variety at baseline, at the follow up, or in the degree of change between the interviews. Furthermore, neither group exhibited any significant within group change in offending variety between baseline and the follow up. See Table 12 and Figures 29 and 30.

Likelihood of (re-)arrest according to official records (no-contact and informal youth). Approximately 14.44% of matched and weighted informally processed youth were re-arrested between baseline and the follow up. None of the no-contact youth were arrested between baseline and the follow up. Because all youth in this comparison who were arrested in the recall period were informally processed (i.e., complete separation), I used a Firth logistic regression (1993) to overcome the inestimable maximum likelihood estimates that would have been associated with a typical binary logistic regression. Results indicated that informal youth were significantly more likely to have an arrest between baseline and the follow up than no-contact youth (z = -2.42, p = .016). Importantly, informally processed youth were still more likely to be re-arrested if I controlled for concurrent self-report of offending variety (z = -2.09, p = .036) or

concurrent self-report of offending frequency ($z = 2.10$, $p = .036$). Of the re-arrests for informal youth, 46% were vandalism, 38% were drug-related, and 15% were receiving, buying, or concealing stolen property.

Likelihood of (re-)arrest according to official records (no-contact and formal youth). In the matched and weighted data set of formally processed youth and no-contact youth, 17.2% of formally processed youth were re-arrested between baseline and the follow up. As previously stated, none of the no-contact youth were arrested between baseline and the follow up. Because formal status perfectly predicts likelihood of arrest, I used a Firth logistic regression (1993) to overcome the biased maximum likelihood estimates. Results indicated that formally processed youth were much more likely than no-contact youth to be arrested in the recall period ($z = -2.57$, $p = .010$), and this is true regardless of whether I control for self-report of offending variety ($z = -2.19$, $p = .028$) or self-report of offending frequency ($z = -2.24$, $p = .025$). Of the re-arrests, approximately 44% were vandalism, 6% were resisting arrest, 38% were drug-related (possession of controlled substance, purchasing tobacco, possession of alcohol), 6% were inflicting pain on an elderly or dependent person, and 6% were driving without a license.

Substance use (no-contact and informal youth). When no-contact and informal youth were compared with respect to their substance use variety and substance use frequency, there were no differences in the degree of change between interviews, no differences at baseline or the follow up, and no within group changes between baseline and the follow up for either no-contact youth or informal youth. See Table 11 and Figures 31 and 32.

Substance use (no-contact and formal youth). Similarly, there were no differences between no-contact and formal youth in substance use variety or frequency at baseline, at the follow up, or in the rate of change between the two interviews. There were also no significant

within group change between baseline and the follow up. However, there was a trend for formal youth to have a higher substance use variety score at the follow up than no-contact youth. See Table 12 and Figures 31 and 32.

Likelihood of moving to continuation school (no-contact and informal youth). Of the matched and weighted sample of informally processed youth, 3.4% of informal youth moved from a traditional junior high school or high school at baseline to a continuation school at the follow up interview. Of the no-contact youth, 1.7% of youth moved from a traditional junior high school or high school at baseline to a continuation school at the follow up interview. According to a binary logistic regression, there was no group difference in the likelihood of moving to a continuation school (z = -0.58, p = .559).

Likelihood of moving to a continuation school (no-contact and formal youth). Although only 1.7% of no-contact youth were transferred to an alternative or continuation school between baseline and the follow up, 13.2% of formal youth moved from a traditional junior high school or high school at baseline to a continuation school at the follow up interview. According to a binary logistic regression with the matched pairs, formally processed youth were significantly more likely than no-contact youth to be moved to a continuation school between the interviews (z = -2.06, p = .039).

School and teacher attachment (no-contact and informal youth). There were no differences between informally processed youth and no-contact youth on either of the school attachment variables. Specifically, the two groups did not differ on their baseline values, follow up values, or the degree of change between the two interviews. There was also no significant within group change between baseline and the follow up. See Table 11 and Figures 33 and 34.

School and teacher attachment (no-contact and formal youth). There were no differences between no-contact and formally processed youth in school or teacher attachment at baseline, at the follow up, or in the rate of change between the two interviews. There were also no significant within group changes between baseline and the follow up in school or teacher bonding (although there was a trend for no contact youth to increase in school attachment between the two interviews). These findings stay the same if I control for formally processed adolescents' greater likelihood of being transferred to an alternative or continuation school. See Table 12 and Figures 33 and 34.

Self-report of grades in school (no-contact and informal youth). Compared to informally processed youth, no-contact youth did not significantly differ in their degree of change in school grades between baseline and the follow up. Although there was a trend for no-contact youth to have higher grades at the baseline interview, there was no difference at the follow up interview. There was also no significant within group change for no-contact or informal youth. See Table 11 and Figure 35.

Self-report of grades in school (no-contact and formal youth). In regard to self-reported grades in school, there were no significant differences in the degree of change between the two interviews. Although no-contact youth were significantly higher than the matched formally processed youth at baseline, this group difference was eliminated at the follow up interview. Indeed formally processed youth significantly improved in their grades in school between baseline and the follow up. This finding is true if I control for likelihood of switching to a continuation or alternative school. There was no within group change for no-contact youth. See Table 12 and Figure 35.

School misconduct (no-contact and informal youth). With respect to school misconduct, the change between baseline and the follow up was significantly different for informally processed youth and no-contact youth. Specifically, informal youth were more likely to decrease in school misconduct than no-contact youth, although the rate of change between the interviews was not significant for informal youth. Interestingly, no-contact youth engaged in more school misconduct than informal youth at baseline and at the follow up. No-contact youth did not change in their level of school misconduct between baseline and the follow up. See Table 11 and Figure 36.

School misconduct (no-contact and formal youth). When no-contact youth and formally processed youth were compared with respect to school misconduct, no significant differences were observed. Specifically, there were no differences at baseline, at the follow up, or in the rate of change between the interviews. There was also no significant within group change for either group. These findings remain the same if I control for likelihood of switching to a continuation school. See Table 12 and Figure 36.

Truancy (no-contact and informal youth). The were no truancy differences at baseline, the follow up, or in the degree of change between the interviews. Furthermore, informal and no-contact youth did not demonstrate any significant within group change between the interviews. See Table 11 and Figure 37.

Truancy (no-contact and formal youth). No-contact and formally processed youth did no significantly differ in their rate of change in frequency of truancy days between the two interviews. There were also no differences at baseline, at the follow up, or within either group. These findings remain the same if I control for likelihood of switching to a continuation school. See Table 12 and Figure 37.

Suspension (no-contact and informal youth). At the baseline interview, informally processed youth experienced more school suspensions than no-contact youth. However, at the follow up interview, this pattern reversed: there was a trend for no-contact youth to have slightly more suspensions than informally processed youth. Importantly, this is due to informally processed youth having significantly fewer school suspensions at the follow up interview compared to the baseline interview. No-contact youth reported the same amount of school suspensions at baseline and the follow up interview. See Table 11 and Figure 38.

Suspension (no-contact and formal youth). Formally processed youth received significantly more suspensions than no-contact youth at the baseline interview; however, there were no differences at the follow up interview. Formal and no-contact youth did not differ in their degree of change between the two interviews, and there was no significant within group change between baseline and the follow up. See Table 12 and Figure 38.

Table 10. Type of Analysis Used to Model the Outcome Variables at Baseline and Follow Up.

Type of regression model used to model outcome variables at baseline and at the follow up

Outcome Variable	Type of outcome/Analysis
Offending frequency	Count (Poisson/ negative binomial)
Offending variety	Count (Poisson/ negative binomial)
Substance use frequency	Ordered categorical (ordered logit)
Substance use variety	Count (Poisson)
School attachment	Continuous (linear regression)
Teacher attachment	Continuous (linear regression)
Grades	Ordered categorical (ordered logit)
School misconduct	Continuous (linear regression)
Truancy	Continuous (linear regression)
Suspensions	Count (Poisson)
Re-arrest	Dichotomous (binary logit)
School transfer	Dichotomous (binary logit)

Table 11. Aim 1: Differences Between No-Contact and Informally Processed Youth.

Aim 1: Differences between no-contact and informally processed youth (matched sample).

Outcome variable	Effect of no-contact vs. informal in rate of change between BL and FU[1] *t* (*p*)	Baseline differences[2] *estimate* (*p*)	Follow up differences (controlling for baseline value)[2] *estimate* (*p*)	Response Profile/Covariance Pattern Models[3] Within group change: No-contact *z* (*p*)	Within group change: Informal *z* (*p*)	Interaction between time and no-contact vs. informal *z* (*p*)
Δ in Offending frequency (Figure 29)	1.65 (.100)	***z* = -6.52, *p* <.001**	***z* = 19.58, *p* <.001**	0.26 (.798)	**-2.34 (.019)**	1.75 (.080)
Δ in Offending variety (Figure 30)	**2.64 (.009)**	*z* = 0.63, *p* = .532	***z* = 10.41, *p* <.001**	-0.83 (.408)	**-4.11 (<.001)**	**2.13 (.033)**
Δ in Substance use frequency (Figure 31)	-0.20 (.840)	*z* = 1.40, *p* = .160	*z* = 0.50, *p* = .617	0.82 (.411)	1.31 (.189)	-0.24 (.808)
Δ in Substance use variety (Figure 32)	0.46 (.648)	*z* = 1.65, *p* = .098	*z* = 0.64, *p* = .523	-0.26 (.792)	-0.33 (.744)	0.02 (.985)
Δ in School attachment (Figure 33)	0.93 (.356)	*t* = 1.39, *p* = .165	*t* = 0.93, *p* = .356	1.71 (.087)	0.73 (.465)	0.79 (.431)
Δ in Teacher attachment (Figure 34)	-0.04 (.965)	*t* = -0.37, *p* = .711	*t* = -0.04, *p* = .965	0.93 (.351)	0.93 (.352)	0.08 (.936)
Δ in Grades (Figure 35)	-0.43 (.670)	*z*=1.96; *p*=.056	*z*= 0.03; *p*=.978	-0.35 (.726)	1.36 (.175)	-1.17 (.242)
Δ in School	**2.17 (.032)**	***t* = 37.01, *p* = .027**	***t* = 2.17, *p* = .032**	-0.05 (.960)	-1.61 (.108)	1.03 (.305)

misconduct (Figure 36)						
Δ in Truancy (Figure 37)	1.57 (.118)	t = 1.17, p = .245	t = 1.57, p = .118	-0.13 (.895)	-1.73 (.084)	1.04 (.299)
Δ in Suspensions (Figure 38)	1.65 (.101)	***t* = -5.35, *p* <.001**	t =1.76, p = .079	0.16 (.870)	**-4.58 (<.001)**	**3.30 (.001)**

[1] Change scores closely paralleled normal distributions; as such, linear regressions were utilized to examine whether the rate of change differed for formal and no-contact youth, controlling for baseline values of the outcome variable.

[2] See Table 10 for the type of analysis used to model differences at baseline and the follow up.

[3]Reponse profiles/covariance pattern models (repeated measures mixed models) were used to investigate whether there was significant change within either group. The interaction term indicates whether the difference in slopes is significant.

Table 12. Differences Between No-Contact and Formally Processed Youth.

Aim 1: Differences between no-contact and formally processed youth (matched sample).

				Response Profile/Covariance Pattern Models[3]		
				Within group change		
	Effect of no-contact vs. formal in rate of change between BL and FU[1] *Estimate t* (*p*)	Baseline differences[2] *Estimate z or t* (*p*)	Follow up differences (controlling for baseline value)[2] *estimate* (*p*)	No-contact *z* (*p*)	Formal *z* (*p*)	Interaction between time and no-contact vs. informal *z* (*p*)
Δ in Offending frequency (Figure 29)	-1.46 (.146)	*z*=-1.46; *p*=.143	***z*=-7.54; *p*<.001**	0.30 (.766)	**2.56 (.010)**	-1.45 (0.148)
Δ in Offending variety (Figure 30)	-1.14 (.258)	*z*=-0.87; *p*=.383	*z*=-1.35; *p*=.176	-0.59 (.556)	0.76 (.447)	-0.94 (.346)
Δ in Substance use frequency (Figure 31)	1.54 (.125)	*z*=-091; *p*=.361	*z*=1.38; *p*=.169	0.53 (.595)	-1.19 (.233)	1.19 (.235)
Δ in Substance use variety (Figure 32)	-1.06 (.289)	*z*=0.37; *p*=.709	*z*=-1.65; *p*=.100	-0.22 (.823)	1.38 (.168)	-1.07 (.284)
Δ in School attachment (Figure 33)	0.43 (.664)	*t*=0.34; *p*=.735	*t*=0.43; *p*=.664	1.69 (.090)	1.30 (.194)	0.42 (.677)
Δ in Teacher attachment (Figure 34)	0.40 (.690)	*t*=-0.30, *p*=.768	*t*=0.40, *p*=.690	1.09 (.274)	0.69 (.490)	0.36 (.719)
Δ in Grades (Figure 35)	-0.50 (.620)	***z*=2.65; *p*=.008**	*z*=-0.53; *p*=.598	-0.26 (.796)	**2.22 (.026)**	-1.68 (.093)
Δ in School	-0.01 (.989)	*t*=0.09; *p*=.929	*t*=-0.01; *p*.989	-0.03 (.976)	-0.00 (.998)	-0.02 (.983)

misconduct (Figure 36)						
Δ in Truancy (Figure 37)	-0.60 (.549)	t=-0.62; p=.537	t=-0.60; p=.549	-0.10 (.920)	0.25 (.803)	-0.24 (.810)
Δ in Suspensions (Figure 38)	-1.57 (.119)	***z*=-2.40; *p*=.016**	***z*=-3.07; *p*=.002**	0.10 (.921)	1.60 (.110)	-0.97 (.332)

[1] Change scores closely paralleled normal distributions; as such, linear regressions were utilized to examine whether the rate of change differed for formal and no-contact youth, controlling for baseline values of the outcome variable.
[2] See Table 10 for the type of analysis used to model differences at baseline and the follow up.
[3]Reponse profiles/covariance pattern models (repeated measures mixed model) were used to investigate whether there was significant change within either group. The interaction term indicates whether the difference in slopes is significant.

Figure 29: Offending Frequency at Baseline and the Follow Up.

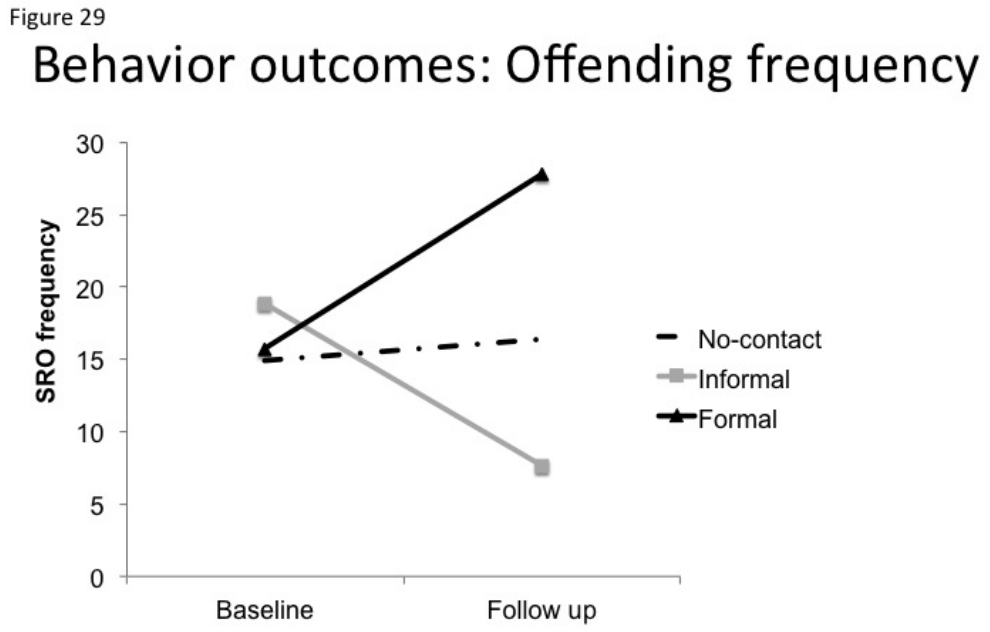


Figure 30: Offending Variety at Baseline and the Follow Up.

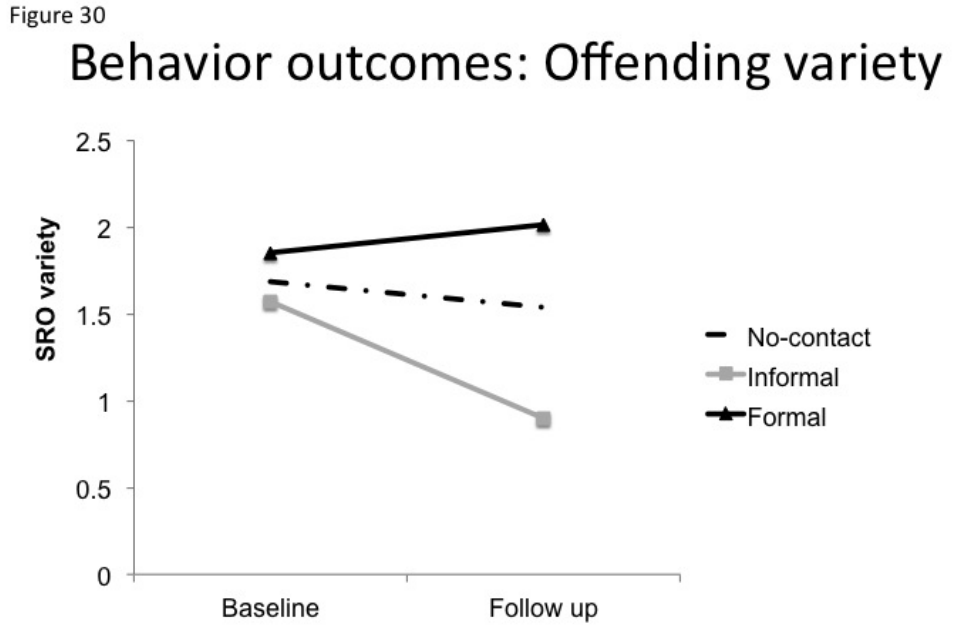


Figure 31: Substance Use Frequency at Baseline and the Follow Up.

Figure 31

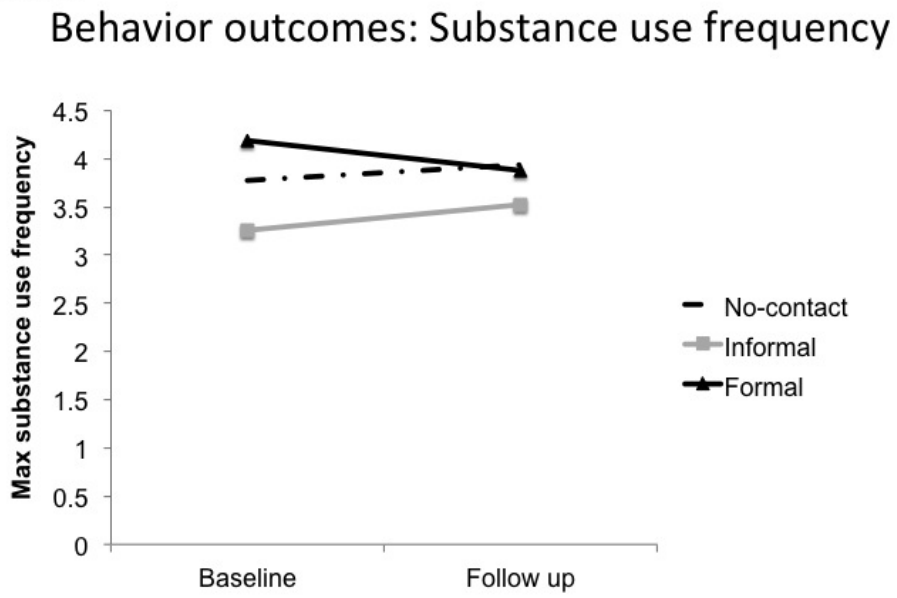


Figure 32: Substance Use Variety at Baseline and the Follow Up.

Figure 32

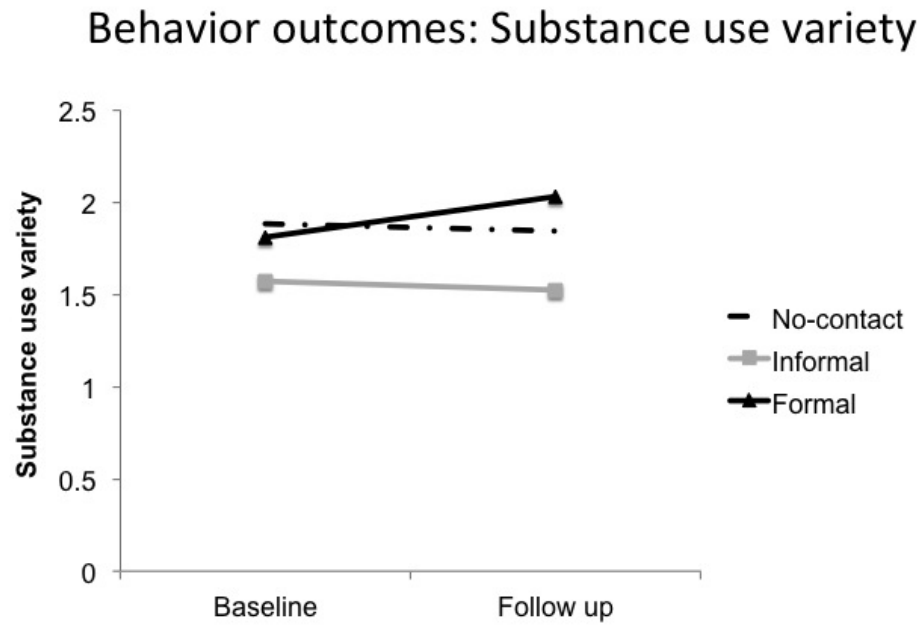


Figure 33. School Attachment at Baseline and the Follow Up.

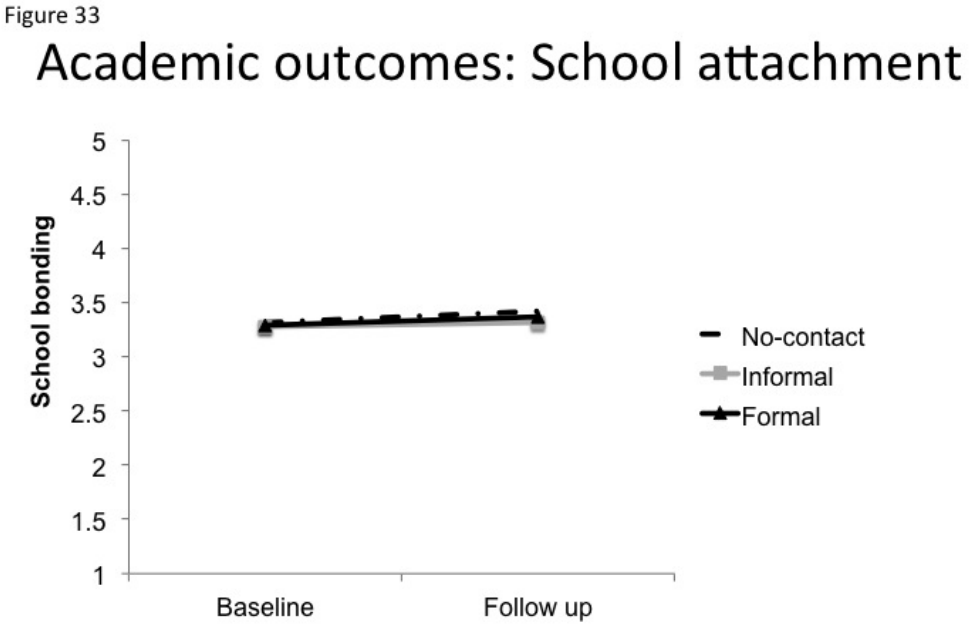


Figure 34: Teacher Bonding at Baseline and the Follow Up.

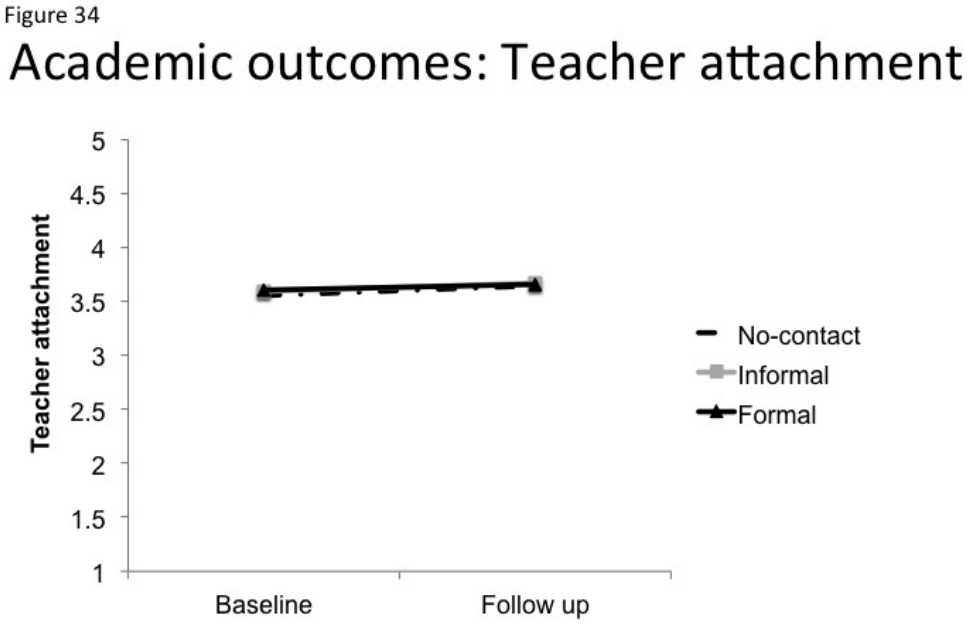


Figure 35 Grades in School at Baseline and the Follow Up.

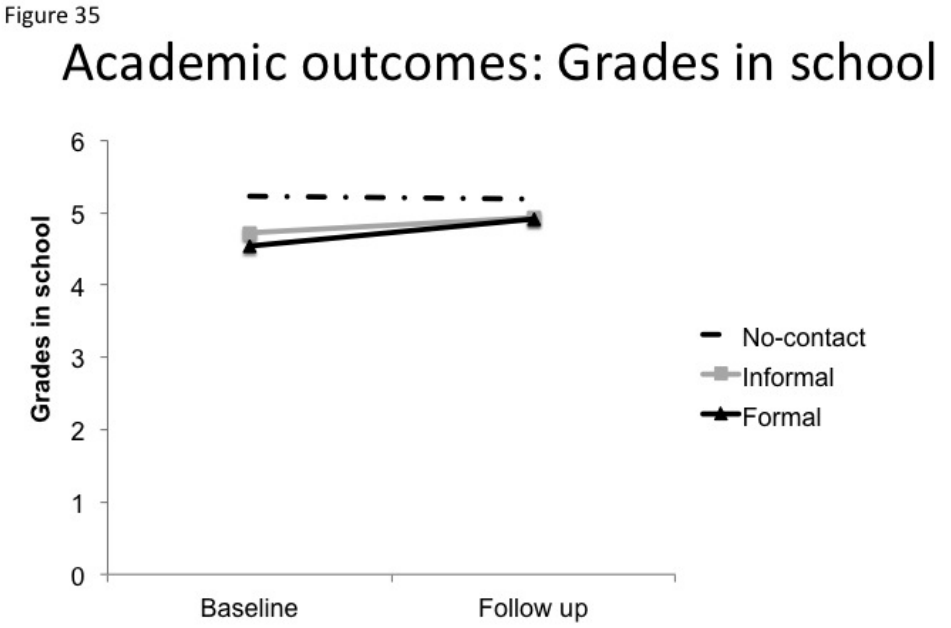


Figure 36. School Misconduct at Baseline and the Follow Up.

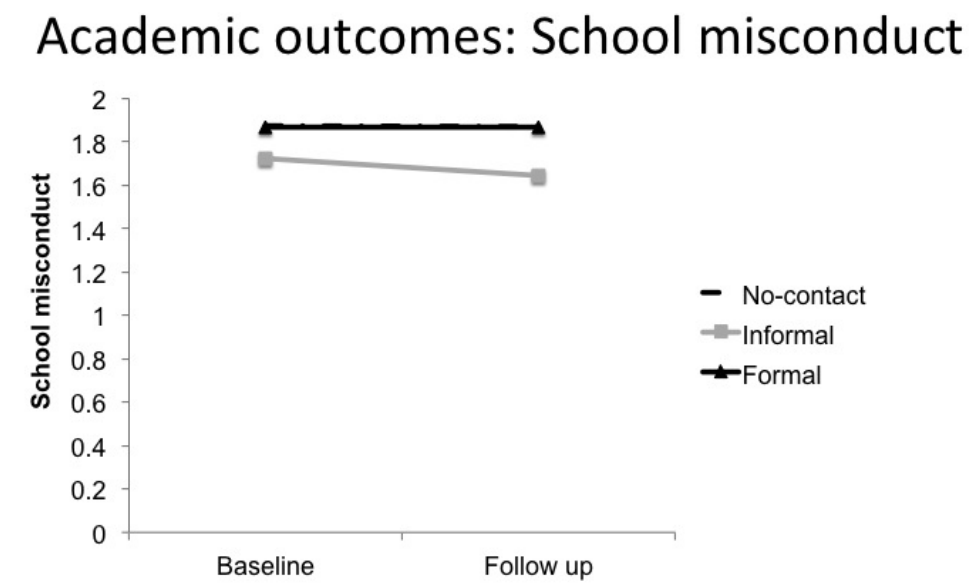


Figure 37. School Truancy at Baseline and the Follow Up.

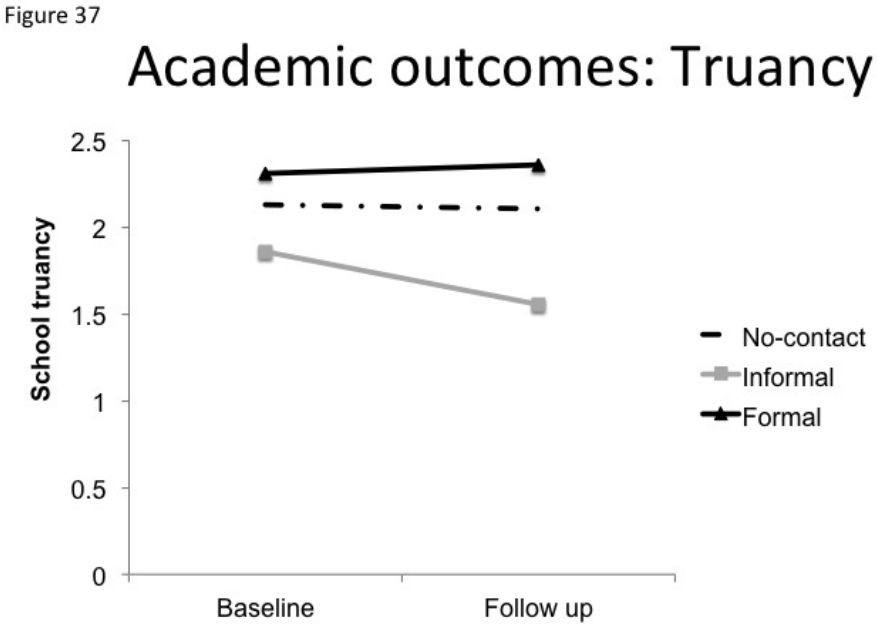
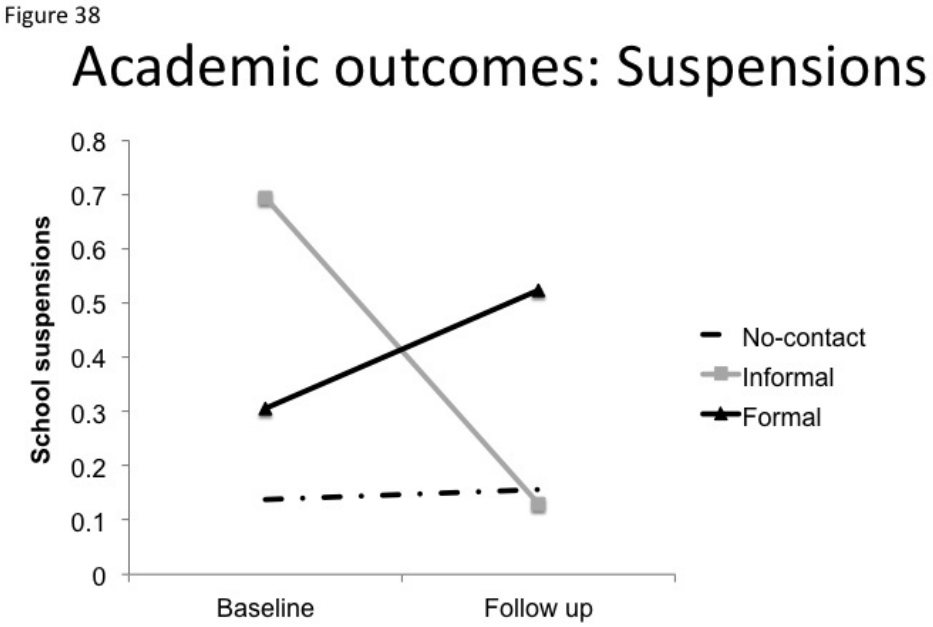


Figure 38. School Suspensions at Baseline and the Follow Up



Aim 2: Identify whether the relations between juvenile justice system contact and later academic achievement and delinquent behavior vary as a function of youth's age. To address Aim 2, I first created an interaction term between age (centered) and the dummy coded juvenile justice contact variables (two variables: [1] informal vs. no-contact; [2] formal vs. no-contact). Second, I re-ran the GLMs described in Aim 1 including as predictors the main effects of juvenile justice contact (i.e., the matched sample of no-contact and either formally processed or informally processed youth), age, the new contact X age interaction term, and the baseline value of the outcome variable (the outcome variable was the rate of change between baseline and the follow up; see column 2 in Table 11 and Table 12). Significant interaction terms indicate that the relation between justice system contact and the outcome variable differs depending on a youth's age. Post hoc probing was used to discern the nature of the interaction term. See Table 13 for the interaction term parameter estimates.

Differential effects of contact by age of youth (no-contact and informal youth). The next analysis in the present study tested whether any of the relations in Aim 1 varied by age of youth. As such, I repeated the analyses presented previously with an age by justice system contact interaction term (informal versus no-contact youth). This interaction term was only significant when grades in school was the outcome (see Table 13). Post hoc probing of the interaction term indicated that age was not related to change in school grades for no-contact youth. However, for informally processed youth, older youth experienced higher rates of change (i.e., more improvement) in school grades between baseline and the 6-month follow up interview. None of the other interaction terms were significant (see Table 13).

Differential effects of contact by age of youth (no-contact and formal youth). When no-contact youth were compared to formally processed youth, the age interaction terms were

significant when teacher attachment, grades in school, and substance use variety were the outcome variables (see Table 13). Specifically, for formally processed youth, older youth were more likely to increase in teacher attachment between baseline and the follow up than younger youth (age was not related to change in teacher bonding for no-contact youth). Although the interaction terms for substance use variety and grades in school were indeed significant—which indicates that the relation between age and the rate of change between baseline and the follow up was significantly different for no-contact and formally processed youth—when I post hoc probed these interaction terms by looking within groups, the age betas were not statistically significant for either group (which could be power issues, discussed in the next section). However, based on the direction of the age parameters, it appears that older formally processed youth may be more likely to decrease in substance use frequency and may be more likely to improve in grades than younger formally processed youth. For no-contact youth, the direction of the parameters (although not technically significant) suggests that older youth may potentially do worse on these outcomes at the follow up compared to baseline.

Table 13. Aim 2: Age Interactions for Primary Analyses

Aim 2: Age interaction terms for informally and formally processed youth with no-contact youth.

Outcome variable	Age X informal β (*p*)	Description interaction term (post hoc probing)	Age X formal β (*p*)	Description of interaction terms (post hoc probing)
Δ in Offending frequency	-0.02 (.791)		0.11(.322)	
Δ in Offending variety	-0.09 (.296)		0.15 (.149)	
Δ in Substance use frequency	-0.03 (.733)		0.15 (.131)	
Δ in Substance use variety	-0.03 (.733)		**0.25 (.014)**	*Although interaction term is significant, age is not related to grades within either group. However, the age estimate is negative for formal youth and positive for no-contact youth*
Δ in School attachment	-0.06 (.560)		0.58 (.560)	
Δ in Teacher attachment	-0.07 (.473)		**-0.25 (.010)**	*Among formal youth, older youth more likely to increase in teacher bonding; age is not related to teacher bonding for no-contact youth*
Δ in Grades	**-0.26 (.004)**	For informally processed youth, older youth have higher rates of change in grades. Age is not related to change in grades for no-contact youth	**-0.21 (.027)**	*Although interaction term is significant, age is not related to grades within either group. However, the age estimate is positive for formal youth and negative for no-contact youth*
Δ in School misconduct	0.10 (.282)		-0.06 (.563)	
Δ in Truancy	0.04 (.659)		-0.00 (.990)	
Δ in Suspensions	-0.36 (.367)		-0.02 (.851)	

Notes. Bold typeface indicates statistical significance.

Aim 3: Investigate whether any effect of juvenile justice contact on behavioral outcomes (Aim 1) is attributable to changes in youth (i.e., expectations for future success), parents (i.e., parental involvement) or peers and social context (i.e., increases in peer delinquency). I used a structural equation modeling framework to examine whether I could identify any mediating variables that might explain how justice system contact affects the behavioral outcomes in Aim 1 (Kline, 2011; Little, Card, Bovaird, Preacher, & Crandall, 2007). Specifically, I tested whether any proportion of the effect of justice system contact (either formal or informal compared to no-contact) on the outcome variables described in Aim 1 was transmitted through any of the mediator variables. Before estimating the significant indirect effect of any potential mediating variable, most researchers suggest that you first consider the direct effects of the treatment group on the outcomes (Aim 1), the direct effect of the treatment group on the mediator variables, and the direct effects of the mediators on the outcome variables (Baron & Kenny, 1986; Little et al., 2007). As such, I present all of these estimates before I show the significant indirect effects.

Each mediator was tested separately for each outcome variable. See Table 14 for the direct effect of contact with the justice system (dichotomous variable: either formal or informal vs. no-contact) on the mediators, see Table 15 and Table 16 for the direct effects of the mediators on the outcomes, and see Table 17 and Table 18 for the indirect effects of contact with the justice system transmitted through each of the mediator variables. In the indirect effect models, two equations were modeled simultaneously:

- Change in outcome variable predicted by change in the mediator, the justice system contact variable, and the baseline value of the dependent variable
- Change in mediator predicted by the justice system contact variable, and the

baseline value of the mediator variable

Indirect effects of mediators (no-contact and informal). First, I show the direct effect of informal versus no-contact status on each of the mediators in Table 14. Results indicated that no-contact youth had higher scores on the change between baseline and the follow up on school aspirations, school expectations, and success expectations. Specifically, although informal youth did not change on success expectations ($z = 0.63$, $p = .531$), no-contact youth had higher success expectations at the follow up compared to their scores at the baseline interview ($z = 2.57$, $p = .010$). There was no significant within group change (for no-contact or informal youth) on school aspirations or school expectations. Furthermore, informally processed youth decreased more in peer delinquency than no-contact youth. Although both groups, on average, associated with fewer delinquent peers at the follow up compared to baseline, informal youth decreased more than no-contact youth in the recall period (no-contact slope: $z = -2.35$, $p = .019$; informal slope: $z = -5.44$, $p < .001$; interaction: $z = 1.97$, $p = .049$). See Table 14. In regard to the direct relation between change in the mediators and change in the outcome variables, there were many significant effects (see Table 15). Results indicated that youth who increased between baseline and the follow up in self-esteem felt more attached to their school and participated in more school misconduct at the follow up. Youth who had more delinquent peers at the follow up also engaged in more offending (frequency and variety) and were less likely to form close attachments to their teachers. Youth whose parents increased in monitoring between baseline and the follow up engaged in less offending (frequency); however, youth whose parents increased in monitoring concurrently engaged in more substance use, more school misconduct, were truant from school on more days, and received poorer grades in school. Youth who perceived more opportunities within their neighborhood at the follow up were truant and

suspended from school on fewer days. Youth who placed less value on future success at the follow up engaged in more offending (frequency and variety), used fewer substances (variety), and felt more attached to their teachers. Youth who had higher expectations for future success felt more attached to their teachers. Individuals who had higher school aspirations at the follow up engaged in less offending (frequency) and had better relationships with their teachers; however, individuals who had higher school aspirations also engaged in more substance use (variety). Youth who expected to attend more school engaged in less offending (frequency). Youth who reported feeling more attached to their teachers also self-reported feeling more attached to their school in general and received fewer days of school suspension. Youth who increased in school truancy between baseline and the follow up engaged in more offending (variety), more school misconduct, and reported lower levels of teacher attachment at the follow up. See Table 15.

Although there were many direct relations between the mediators and the outcome variables, none of the potential mediators satisfied all of the requirements to partially or fully explain any of the significant effects of contact with the justice system on the outcome variables tested in the first aim.

Indirect effects of mediators (no-contact and formal). As mentioned in the previous section, the first set of analyses tested whether formal and no-contact youth differed in the degree of change on any of the mediator variables. Compared to formally processed youth, no-contact youth increased more in school aspirations and school and work success expectations between baseline and the follow up. Specifically, although formally processed youth did not change on success expectations (z = -0.90, p = .368), no-contact youth significantly increased in their expectations for future success between baseline and the follow up (z = 2.42, p = .016;

interaction term: z =2.40, p = .016). There was no significant within group change in school aspirations. Furthermore, there was a significant difference in the degree of change between baseline and the follow up in parental monitoring. Although there was no change for no-contact youth (z = -1.11, p =.268), formally processed youth, on average, had parents who significantly decreased in monitoring between baseline and the follow up (z = -4.88, p = <.001). See Table 14 for the direct effect of formal status on the mediators.

The next set of analyses investigated whether change between baseline and the follow up on any of the mediator variables was simultaneously related to change on any of the outcome variables during this period for no-contact and formally processed youth. The results indicated that several of the mediators were related to the outcome variables (See Table 16). Youth who increased in self-esteem between baseline and the follow up felt more attached to their teachers and more attached to their school but they also engaged in more school misconduct. Youth who reported associating with more delinquent peers at the follow up engaged in more offending (variety and frequency), more substance use (frequency and variety), more school misconduct, and more school truancy. Youth whose parents increased in monitoring between baseline and the follow up were likely to improve their grades in school during this period. Individuals who perceived fewer opportunities within their neighborhood engaged in more offending (frequency and variety), had poorer grades in school, and were suspended from school on more days. Youth who placed less value on future education and work success at the follow up engaged in more offending (frequency and variety). Individuals who had higher expectations for future success felt more attached to their teachers, had better grades in school, engaged in less school misconduct, and were truant from school on fewer days. Youth who expected to attend more years of schooling engaged in less offending (frequency and variety), were truant from school on

fewer days, and had better grades in school. Youth who aspired to attend more years of school and individuals who self-reported feeling more attached to their teachers had better grades in school. Youth who felt closer to their school engaged in more substance use (variety), more school misconduct, and were truant from school on more days. Finally, youth who were truant from school on more days engaged in more offending (frequency and variety), more substance use (frequency), and more school misconduct during the recall period; however, youth who were truant on more days also reported feeling more attached to their school. See Table 16. Although many of the mediators were significantly related to the outcome variables, only three were significant mediators in the pathway between contact with the justice system (formal versus no-contact) and the outcome variables. Specifically, although formal did not change on their school and work expectations, no-contact youth increased slightly, which, in turn, was related to slight improvement in their grades in school, less school misconduct, and less school truancy.

Table 14. Direct Relation Between Justice System Contact and Mediators

Direct relation between justice system contact and mediators

	No-contact vs. informal youth z (*p*)	No-contact vs. formal youth z (*p*)
Δ in Self-esteem	1.86 (.063)	0.51 (.608)
Δ in Peer delinquency	**2.22 (.027)**	-0.36 (.717)
Δ in Parental involvement	0.44 (.663)	**2.44 (.015)**
Δ in Neighborhood climate	-0.68 (.494)	1.40 (.162)
Δ in School aspirations	**2.07 (.038)**	**2.47 (.014)**
Δ in School expectations	**2.06 (.039)**	1.82 (.069)
Δ in Success value	1.00 (.317)	1.62 (.105)
Δ in Success expectations	**2.45 (.014)**	**4.02 (<.001)**
Δ in Teacher attachment	-0.13 (.897)	0.10 (.920)
Δ in School attachment	0.72 (.474)	0.10 (.916)
Δ in Truancy	1.41 (.158)	-0.72 (.471)

Note. Each mediator tested separately; Bold typeface indicates statistical significance

Table 15. Direct Relation Between Mediators and Outcome Variables (No-Contact and Informal Youth).

Direct relation between mediators and outcome variables in matched sample of no-contact and informally processed youth.

	Mediators *z* (*p*)										
	Δ Self-estm[1]	**Δ** Peer delinq.[2]	**Δ** Parent involv.[3]	**Δ** Neigh clim.[4]	**Δ** Succ. value[5]	**Δ** Succ. Exp.[6]	**Δ** Sch asp.[7]	**Δ** Sch exp.[8]	**Δ** Teach attach[9]	**Δ** School attach[10]	**Δ** Truan.[11]
Δ Offend. freq.	-0.60 (.549)	**3.07 (.002)**	**-0.91 (.363)**	-0.34 (.731)	**-5.21 (<.001)**	-0.55 (.584)	**-1.99 (<.047)**	**-2.47 (.013)**	-1.64 (.102)	0.61 (.544)	1.47 (.142)
Δ Offend. variety	-0.29 (.773)	**3.46 (.001)**	1.72 (.086)	-0.26 (.795)	**-2.52 (.012)**	0.92 (.360)	-0.23 (.821)	-0.80 (.421)	-0.84 (.400)	-0.50 (.616)	**2.80 (.005)**
Δ Sub use freq	-1.74 (.081)	1.37 (.170)	1.31 (.191)	1.38 (.167)	0.50 (.620)	0.60 (.551)	1.05 (.293)	0.66 (.509)	-1.34 (.180)	-1.74 (.082)	-0.23 (.817)
Δ Sub use variety	0.33 (.741)	-1.05 (.293)	**2.23 (.026)**	1.59 (.111)	**2.13 (.033)**	0.92 (.358)	**3.75 (<.001)**	1.18 (.236)	-0.15 (.880)	0.39 (.699)	**3.07 (.002)**
Δ School attach	**2.16 (.031)**	-1.24 (.216)	-0.17 (.866)	1.12 (.262)	1.22 (.223)	0.60 (.548)	0.87 (.385)	0.08 (.938)	**4.32 (<.001)**	----	1.11 (.269)
Δ Teach attach	1.45 (.146)	**-3.78 (.001)**	0.61 (.542)	1.64 (.101)	**3.93 (<.001)**	**2.15 (.031)**	**3.37 (.001)**	1.23 (.219)	----	**4.70 (<.001)**	**-2.12 (.034)**
Δ Grades	-0.45 (.650)	0.38 (.702)	**-2.29 (.022)**	1.37 (.170)	-0.38 (.707)	0.30 (.767)	-0.43 (.670)	0.94 (.345)	0.03 (.978)	0.06 (.955)	-1.84 (.066)
Δ School miscon.	**2.58 (.010)**	0.59 (.557)	**2.52 (.012)**	-0.24 (.807)	-0.63 (.528)	0.39 (.693)	-0.42 (.674)	-0.69 (.491)	-1.68 (.093)	0.22 (.814)	**7.71 (<.001)**
Δ Truancy	1.85 (.065)	0.15 (.878)	**2.49 (.013)**	**-1.96 (.050)**	0.13 (.895)	0.46 (.647)	0.17 (.864)	0.38 (.702)	0.80 (.005)	0.22 (.824)	----
Δ Susp.	1.71 (.088)	0.95 (.341)	-0.02 (.986)	**-5.22 (<.001)**	-1.17 (.244)	0.43 (.666)	-0.68 (.495)	0.16 (.877)	**-2.85 (.004)**	-1.67 (.095)	0.27 (.784)

Notes. All mediators and outcome variables represent the change between baseline and follow up interview.
Bold typeface indicates statistical significance ($p < .05$)
[1] Self-esteem; [2] Peer delinquency; [3] Parental involvement; [4] Neighborhood climate; [5] Success value; [6] Success expectations; [7] School aspirations; [8] School expectations; [9] Teacher attachment; [10] School attachment; [11] School truancy

Table 16. Direct Relation Between Mediators and Outcome Variables (No-Contact and Formal Youth).

Direct relation between mediators and outcome variables in matched sample of no-contact and formally processed youth.

	Mediators *z* (*p*)										
	Δ Self-estm[1]	Δ Peer delinq.[2]	Δ Parent involv.[3]	Δ Neigh clim.[4]	Δ Succ. value[5]	Δ Succ. Exp.[6]	Δ Sch asp.[7]	Δ Sch exp.[8]	Δ Teach attach[9]	Δ School attach[10]	Δ Truan.[11]
Δ Offend. freq.	0.36 (.719)	**4.71 (<.001)**	-0.20 (.841)	**-2.40 (.016)**	**-3.19 (.001)**	-0.04 (.964)	-0.29 (.773)	**-2.94 (.003)**	-0.47 (.635)	0.41 (.681)	**3.81 (<.001)**
Δ Offend. variety	-0.72 (.472)	**6.86 (<.001)**	-0.07 (.940)	**-2.47 (.014)**	**2.69 (.007)**	-1.94 (.052)	-0.50 (.619)	**-3.11 (.002)**	-0.69 (.262)	0.35 (.725)	**4.02 (<.001)**
Δ Sub use freq	-0.64 (.520)	**4.90 (<.001)**	-0.03 (.973)	-0.68 (.498)	-0.31 (.759)	1.12 (.262)	0.89 (.372)	-1.47 (.142)	0.11 (.911)	0.16 (.871)	**2.63 (.008)**
Δ Sub use variety	-0.84 (.400)	**4.63 (<.001)**	-0.57 (.567)	-0.47 (.636)	0.36 (.720)	0.24 (.808)	0.46 (.646)	-0.01 (.992)	-0.40 (.686)	**2.18 (.029)**	**2.97 (.003)**
Δ School attach	**2.93 (.003)**	-1.16 (.246)	0.49 (.622)	1.07 (.287)	1.44 (.150)	-0.98 (.326)	0.35 (.725)	1.46 (.145)	0.10 (.917)	----	**3.23 (.001)**
Δ Teach attach	**2.55 (.011)**	-1.00 (.319)	0.86 (.387)	1.90 (.057)	-0.72 (.472)	**2.02 (.043)**	0.41 (.680)	0.00 (.997)	----	0.24 (.809)	0.50 (.615)
Δ Grades	0.49 (.624)	-1.05 (.293)	**2.61 (.009)**	**1.96 (.051)**	-0.79 (.430)	**3.42 (.001)**	**2.25 (.025)**	**3.60 (<.001)**	**2.32 (.020)**	0.54 (.593)	-0.29 (.775)
Δ School miscon.	**1.95 (.051)**	**2.73 (.006)**	0.47 (.636)	-0.28 (.778)	-0.69 (.448)	**-3.43 (<.001)**	0.27 (.791)	-1.02 (.308)	-1.20 (.228)	**3.15 (.002)**	**9.47 (<.001)**
Δ Truancy	0.57 (.571)	**2.07 (.038)**	1.12 (.263)	-0.76 (.446)	-0.16 (.870)	**-3.18 (.001)**	0.29 (.770)	**-2.09 (.037)**	-1.54 (.123)	**2.13 (.033)**	----
Δ Susp.	0.74 (.459)	0.86 (.392)	-0.54 (.588)	**-2.00 (.046)**	0.04 (.970)	-1.14 (.254)	1.19 (.235)	0.75 (.455)	1.51 (.131)	-0.79 (.428)	0.009 (.999)

Notes. All mediators and outcome variables represent the change between baseline and follow up interview. Bold typeface indicates statistical significance ($p < .05$)

[1] Self-esteem; [2] Peer delinquency; [3] Parental involvement; [4] Neighborhood climate; [5] Success value; [6] Success expectations; [7] School aspirations; [8] School expectations; [9] Teacher attachment; [10] School attachment; [11] School truancy

Table 17. Indirect Effects of Justice System Contact on Outcome Variables Through Mediators (No-Contact and Informal Youth)

Indirect effects on outcomes through mediators for models comparing no-contact and informally processed youth.

	Mediators *z* (*p*)										
	Δ Self-estm[1]	Δ Peer delinq.[2]	Δ Parent involv.[3]	Δ Neigh clim.[4]	Δ Succ. value[5]	Δ Succ. Exp.[6]	Δ Sch asp.[7]	Δ Sch exp.[8]	Δ Teach attach[9]	Δ School attach[10]	Δ Truan.[11]
Δ Offend. freq.	-0.84 (.401)	1.75 (.081)	-0.41 (.685)	0.34 (.736)	-0.98 (.326)	-0.75 (.452)	-1.45 (.148)	-1.59 (.111)	0.04 (.969)	-0.48 (.631)	0.98 (.327)
Δ Offend. variety	-0.73 (.466)	1.79 (.073)	0.42 (.675)	0.30 (.762)	-0.93 (.350)	0.60 (.549)	-0.17 (.861)	-0.77 (.442)	0.04 (.969)	-0.39 (.696)	1.25 (.213)
Δ Sub use freq	-1.28 (.200)	1.19 (.236)	0.41 (.679)	-0.61 (.540)	0.44 (.656)	0.59 (.557)	0.94 (.349)	0.63 (.529)	0.04 (.969)	-0.66 (.508)	-0.23 (.822)
Δ Sub use variety	0.27 (.787)	-1.02 (.306)	0.43 (.669)	-0.63 (.530)	0.91 (.365)	0.83 (.406)	1.82 (.069)	1.03 (.305)	0.04 (.970)	0.34 (.732)	1.28 (.200)
Δ School attach	1.29 (.197)	-1.13 (.261)	-0.22 (.828)	-0.59 (.552)	0.74 (.459)	0.51 (.609)	0.82 (.415)	0.09 (.926)	-0.04 (.969)	----	0.85 (.395)
Δ Teach attach	1.10 (.269)	**-2.03 (.042)**	-0.04 (.970)	-0.71 (.476)	0.95 (.343)	1.56 (.118)	1.70 (.089)	1.01 (.312)	----	0.36 (.719)	-1.11 (.267)
Δ Grades	-0.40 (.686)	0.46 (.644)	-0.50 (.618)	-0.57 (.568)	-0.37 (.713)	-0.31 (.754)	-0.43 (.666)	0.86 (.390)	-0.83 (.407)	0.05 (.956)	-1.04 (.299)
Δ School miscon.	1.34 (.180)	0.27 (.791)	0.39 (.698)	0.30 (.763)	-0.55 (.585)	0.14 (.889)	-0.37 (.709)	-0.61 (.544)	0.13 (.897)	0.26 (.796)	1.39 (.165)
Δ Truancy	1.17 (.242)	-0.09 (.930)	0.39 (.698)	0.67 (.504)	0.13 (.897)	0.30 (.762)	0.21 (.833)	0.41 (.681)	0.24 (.813)	-0.18 (.856)	----
Δ Susp.	1.12 (.261)	0.68 (.496)	-0.15 (.881)	0.70 (.482)	-0.73 (.468)	0.32 (.751)	-0.63 (.527)	0.24 (.814)	0.13 (.897)	-0.66 (.509)	0.18 (.856)

Notes. All mediators and outcome variables represent the change between baseline and follow up interview. Bold typeface indicates statistical significance ($p < .05$)

[1] Self-esteem; [2] Peer delinquency; [3] Parental involvement; [4] Neighborhood climate; [5] Success value; [6] Success expectations; [7] School aspirations; [8] School expectations; [9] Teacher attachment; [10] School attachment; [11] School truancy

Table 18. Indirect Effects of Justice System Contact on Outcome Variables Through Mediators (No-Contact and Formal Youth)

Indirect effects on outcomes through mediators for models comparing no-contact and formally processed youth.

	Mediators *z* (*p*)										
	Δ Self-estm[1]	Δ Peer delinq.[2]	Δ Parent involv.[3]	Δ Neigh clim.[4]	Δ Succ. value[5]	Δ Succ. Exp.[6]	Δ Sch asp.[7]	Δ Sch exp.[8]	Δ Teach attach[9]	Δ School attach[10]	Δ Truan.[11]
Δ Offend. freq.	0.32 (.746)	-0.36 (.718)	-0.02 (.980)	-1.19 (.232)	-1.45 (.146)	0.21 (.833)	-0.25 (.806)	-1.54 (.124)	-0.26 (.799)	0.10 (.919)	-0.71 (.479)
Δ Offend. variety	-0.41 (.683)	-0.36 (.717)	0.09 (.925)	-1.25 (.212)	-1.36 (.173)	-1.61 (.108)	-0.45 (.656)	1.56 (.118)	-0.28 (.780)	0.10 (.921)	-0.71 (.478)
Δ Sub use freq	-0.41 (.681)	-0.36 (.718)	-0.15 (.877)	-0.66 (.507)	-0.28 (.776)	0.97 (.334)	0.81 (.416)	-1.07 (.285)	0.11 (.916)	0.09 (.928)	-0.70 (.487)
Δ Sub use variety	-0.43 (.666)	-0.36 (.718)	-0.42 (.674)	-0.37 (.714)	0.31 (.753)	0.45 (.650)	0.48 (.628)	0.04 (.970)	-0.24 (.809)	0.10 (.917)	-0.70 (.484)
Δ School attach	0.21 (.834)	0.61 (.544)	0.47 (.639)	0.85 (.398)	1.06 (.288)	-0.98 (.325)	0.35 (.729)	0.99 (.324)	0.10 (.918)	----	-0.70 (.482)
Δ Teach attach	0.30 (.761)	0.71 (.480)	0.81 (.416)	1.05 (.295)	-0.62 (.534)	1.80 (.072)	0.76 (.446)	0.00 (.997)	----	-0.19 (.846)	-0.44 (.658)
Δ Grades	0.23 (.818)	0.28 (.780)	1.63 (.104)	1.04 (.297)	-0.72 (.470)	**2.67 (.008)**	1.67 (.095)	1.47 (.142)	0.09 (.925)	0.28 (.781)	0.29 (.771)
Δ School miscon.	0.21 (.834)	-0.69 (.490)	0.47 (.638)	-0.27 (.786)	-0.64 (.524)	**-2.52 (.012)**	0.26 (.791)	-0.81 (.417)	-0.10 (.920)	0.10 (.916)	-0.72 (.473)
Δ Truancy	0.20 (.843)	-0.67 (.501)	1.03 (.302)	-0.63 (.527)	-0.19 (.850)	**-2.37 (.018)**	0.30 (.765)	1.13 (.260)	-0.10 (.920)	0.10 (.916)	----
Δ Susp.	0.20 (.840)	-0.54 (.586)	-0.36 (.722)	-1.13 (.259)	-0.02 (.981)	-0.86 (.387)	1.08 (.282)	0.67 (.503)	0.10 (.920)	-0.10 (.917)	-0.21 (.834)

Notes. All mediators and outcome variables represent the change between baseline and follow up interview. Bold typeface indicates statistical significance ($p < .05$)

[1] Self-esteem; [2] Peer delinquency; [3] Parental involvement; [4] Neighborhood climate; [5] Success value; [6] Success expectations; [7] School aspirations; [8] School expectations; [9] Teacher attachment; [10] School attachment; [11] School truancy

Power analysis. Although post-hoc power analyses are not widely accepted, these analyses can provide information that aids in the interpretation of the results. In general, observed statistical power greater than .80 ($\alpha = .05$) is considered acceptable (Cohen, 1988; Cohen, Cohen, West, & Aiken, 2003). With the matched data sets, we had an adequate sample size to find true effects in Aim 1 and Aim 2. However, it does not seem that the sample, at present, is large enough to test all of the indirect effect effects in Aim 3. In general, a sample size of about 200 (and a ratio of about 20 cases for every estimated parameter) is necessary for the SEM meditational models in Aim 3 (Kline, 2011). See Table 19.

Table 19. Observed Statistical Power.

Observed Statistical Power (**β**) *in Aim 1 and Aim 2 (analyzed using Soper's 2014 Post-hoc statistical power calculator for multiple regression).*

	Aim 1		Aim 2 (models with age interaction term)	
	Informal vs. no-contact **β**	Formal vs. no-contact **β**	Informal vs. no-contact **β**	Formal vs. no-contact **β**
Δ in Offending frequency	1.00	0.55	0.99	0.60
Δ in Offending variety	1.00	0.92	1.00	0.99
Δ in Substance use frequency	0.78	0.99	0.71	0.99
Δ in Substance use variety	0.99	0.96	0.99	0.95
Δ in School attachment	0.99	0.99	0.99	0.99
Δ in Teacher attachment	0.99	0.99	0.99	0.99
Δ in Grades	0.99	0.99	0.99	0.99
Δ in School misconduct	0.99	0.98	0.99	0.98
Δ in Truancy	0.99	0.99	0.99	0.99
Δ in Suspensions	1.00	0.72	1.00	0.99

V. Discussion

Many studies have found that arrested youth are more likely than non-arrested youth to be convicted as adults and experience diminished academic and occupational attainment, reduced adult earnings, and unemployment (Bernburg & Krohn, 2003; Bushway & Reuter, 2002; De Li, 1999; Gatti et al., 2009; Hjalmarsson, 2008; Hirschfield, 2004; Kirk & Sampson, 2012; Sweeten, 2006). However, an important limitation in prior work is the possibility of selection effects, with arrested youth likely to have very different psychological and behavioral profiles pre-justice system contact than non-arrested youth (Beckett et al., 2005; Brownfield et al., 2001; Gatti et al., 2009; Hirschfield et al., 2006; Kirk & Sampson, 2012). As such, any later observed differences in arrested and non-arrested individuals' achievement and behavior could be spurious, with both the arrest and later outcomes explained by a combination of measured and unmeasured variables that preceded or contributed to the police contact. This leaves us wondering whether the observed maladjustment is due to the type of adolescent who comes to the attention of law enforcement or due the type of justice system interventions that arrested youth experience.

The present study overcomes these limitations by specifically recruiting a sample of youth who engaged in the same type of illegal behavior, however, only some youth were arrested as a result of their crime. To further reduce the possibility of selection effects, the present study utilized a statistical technique designed to approximate random assignment to treatment, propensity score matching. The results indicate that, when selection effects are taken into consideration, contact with the juvenile justice system does not have a universally harmful effect on adolescent behavior and academic outcomes. In fact, the results demonstrate that informal processing actually deters future offending, school misconduct, school truancy, and school

suspensions. However, both informal and formal youth, regardless of their actual antisocial and illegal behavior, are more likely than no-contact youth to be arrested during the study period.

Primary Results and Ties to Prior Literature

Compared to their behavior before the arrest, after six months of informal probation, diverted youth engaged in less antisocial and illegal behavior, less school misconduct, and were truant and suspended from school on fewer days. No-contact youth, individuals who were not exposed to any intervention from the justice system, did not change on any of these variables between the two interviews. These results suggest that some aspects of informal processing for first-time juvenile offenders, at least in the short-term, might serve as specific deterrents of future offending and misconduct (for a review of deterrence theories, see Morris, 1966; Zimring & Hawkins, 1973). However, the present study generated some unsettling findings: if likelihood of recidivism was only measured with official arrest data, the opposite conclusion would have been made. Indeed one of the most striking findings in the present study is that, although informal youth self-reported *less* offending than no-contact youth at the follow up, informal youth were much more likely to have an arrest during this recall period. This is true after controlling for the frequency and variety of an individual's actual illegal behavior. Perhaps most noticeable is that informal youth were arrested for very minor offenses—for example, possession of marijuana and vandalism. As such, if the only outcome variable in the present study was presence of an arrest between baseline and the follow up according to official data, I may have erroneously concluded that informal processing *leads* to subsequent offending, as evident by informal participants' greater likelihood of having a re-arrest. In reality, the self-report data support the opposite conclusion. Although informal processing might have a positive influence on juvenile offenders' actual behavior, the system continues to arrest them.

It is important to include self-report in addition to official evidence of law-breaking because most illegal behavior among adolescents goes undetected by law enforcement (Farrington et al., 2003). Self-reports are likely to capture these undetected illegal acts and, therefore, may serve as a more accurate measure of law-breaking behavior (Maxfield et al., 2000). As mentioned previously, there is a large discrepancy between rates of adolescent offending and adolescent arrests and the correlation between official reports of arrest and youth self-report is often relatively low (Brame et al., 2012; Farrington, 2009; Moffitt, 1993; Monahan et al., 2009; OJJDP). Consequently, using both self-reports and official arrest data allows us to capture two unique measures of antisocial and illegal behavior.

I argue that an individual's true antisocial behavior is less correlated with official reports of arrest than with youth self-report of offending because a number of external factors influence the chances that an offense will lead to an arrest. These factors include whether the crime is detected, whether it is reported to law enforcement, whether police decide to accept and file the charges, and whether there is enough evidence for a charge to be sustained. Police have a great deal of discretion when determining whom to arrest (Black & Reiss, 1970; Kraus & Hasleton, 1982). Although it is true that a youth's recollection of his antisocial and illegal behavior over the previous 6 months might not be a perfect representation of his actual behavior, his self-report of offending is likely to be a better proxy for true behavior than official arrest records. Policies based on an individual's actual likelihood for desistance should be based on self-report data. Conversely, policies based on whether one context leads to subsequent intervention by justice system should be based on official court data.

Contrary to informal processing, formal processing had mixed effects on youth. The data suggest that matched formally processed youth might improve in their grades in school, but

formal youth also increased in offending frequency. Matched formal youth were also more likely to be transferred to a continuation or alternative school than no-contact youth. If I control for whether youth were transferred to a continuation or alternative school in the recall period, results still indicate that formally processed youth significantly improved in their grades, while no-contact youth did not change in their average self-reported grades in school. Indeed being transferred to an alternative or continuation school was not related to grades in school. Furthermore, like informally processed youth, formally processed youth were much more likely than no-contact youth to be arrested between baseline and the follow up interview, regardless of their self-reported offending during this same period. Formally processed youth had the highest odds of being re-arrested. This finding is consistent with much prior work that has demonstrated that arrested youth are significantly more likely than non-arrested youth to have an adult arrest record, and that youth who experience harsh justice system sanctions are most at risk for future arrests (e.g., Gatti et al., 2009). There is also meta-analytic data that show that individuals exposed to justice system interventions that focus on discipline and programs that emphasize (or dramatize) the negative consequences of crime, features that are more common in formal sanctions than informal ones, have higher rates of subsequent arrests than individuals who attend programs that do not have these qualities (Lipsey, 2009).

One of the most interesting findings observed in the present study is that informally processed youth had better behavioral outcomes than both no-contact youth and formally processed youth. Diversion programs were originally designed to provide juvenile offenders with an opportunity to receive needed services in a way that does not attach the stigmatizing criminal label that is typically associated with formal processing, an idea most influenced by labeling theory (Klein, 1986; Schur, 1971; Lundman, 1976). An underlying assumption is the

acknowledgement that some justice system interventions may cause more harm than good. This assumption is especially true for low-level offenders who commit relatively minor crimes—behaviors that are common in adolescence.

One of the most apparent features of diversion is the huge variability in the types of experiences that are available to diverted youth (Schwalbe, Gearing, MacKenzie, Brewer, & Ibrahim, 2012). Indeed some youth may receive very few (if any) services, while other youth may receive a host of intensive therapeutic resources. In the present data, of the matched informally processed youth, 4.21% were referred to tutoring or other academic related counseling, 15.79% were referred to individual counseling or therapy, 2.11% were referred to group counseling, 10.53% were referred to family counseling, and 1.05% were referred to job skills. Youth could be referred to any combination of these services or none of these services. These figures are very similar for the formally processed youth: Of the matched formally processed youth, 6.32% were referred to tutoring or other academic related counseling, 12.63% were referred to individual counseling or therapy, 8.42% were referred to group counseling, 5.26% were referred to family counseling, and 4.21% were referred to job skills. Despite the huge variability in the type of "diversion" interventions that informal youth may be exposed to, a recent meta-analysis of 28 studies published between 1983 and 2009, which included over 19,000 juvenile offenders in 33 independent studies, found that diversion programs that basically do nothing more than "warn and release" are just as effective at reducing recidivism as intensive diversion programs that subject youth to a variety of psychosocial interventions (Schwalbe et al., 2012). Still, the finding that informally processed youth showed the biggest decline—of all three groups—in frequency and variety of self-reported offending is consistent with Petrosino et al.'s (2010) meta-analysis that combined data from 29 studies and found that diversion was a stronger

predictor of desistance than both formal processing and "doing nothing" ("no-contact" in the present study).

Limitations

It is encouraging to discover that some types of informal justice system interventions might reduce the likelihood of future law breaking behavior and might also improve an individual's behavior in school. However, like all studies, there are important limitations here that need to be considered. Most importantly, the results presented in this study are specific to the context of one site in southern California. It is unknown whether the same patterns would emerge in other jurisdictions. Future research should replicate the findings presented here in different geographic locations. Furthermore, although a series of power analyses indicated that I was able to detect small and medium effects in Aim 1 and Aim 2, the sample size in the present study may not have been sufficient to detect small effects in the meditational models in Aim 3. Although the sample size in the present study is almost identical to a similar study that matched arrested and non-arrested youth (Kirk & Sampson, 2012), future research should replicate the findings in a larger sample of matched pairs of arrested and non-arrested youth, and among youth who committed more serious offenses.

It is important to keep in mind that the findings presented here only generalize to male first time offenders who commit relatively minor crimes and thus have a low probability of being caught. It is unlikely that these findings would generalize to adolescent offenders who commit serious offenses (like murder) or status offenses (like running away). The present study does not support a net-widening approach nor do the data suggest that informal processing would have positive effects on youth whose behavior would never have been called into question by law enforcement. Moreover, the present study only recruited male first time offenders. It is

unknown whether there will be positive effects after the 2nd, 3rd,…nth arrest. This is especially worrisome given that formal and informal youth, regardless of actual self-reported illegal behavior, are more likely to be arrested between baseline and the follow up than matched no-contact youth. It is also unknown whether these results would generalize to female first-time offenders. Last, the present study only used data from two measurement occasions that were six months apart. It is unknown whether the positive effects of contact with the justice system will be sustained long-term. The long-term effects of prolonged justice system involvement are also unknown.

Study Strengths

Despite these limitations, there is much methodological strength in the present study. First, no prior study has specifically recruited a comparison group of non-arrested youth that engaged in the same types of illegal behavior as a sample of arrested youth. Although prior work *controls* for measures of prior delinquency, it is likely that non-arrested youth engage in statistically fewer, less severe, or qualitatively different behaviors. As such, the ranges of prior offending variables are not overlapping, and as a result, controlling for prior delinquency does not completely account for pre-existing differences and possible selection effects. The present study carefully recruited individuals who were never arrested but who committed the same types of crimes as their arrested counterparts. Furthermore, a specialized statistical technique (propensity score matching) was used to boost comparability between the juvenile justice contact sample and the no-contact sample. This technique helps account for the fact that youth are likely to have different risk and protective factors that increase or decrease their likelihood of being "caught" (i.e., selection effects), which are also related to the outcome variables of interest. Although one prior study was able to statistically match arrested and non-arrested youth (Kirk &

Sampson, 2012), no other prior studies were able to match these two groups. Moreover, multiple entry points along the justice system pipeline were measured in the present study—formal processing and informal processing. This means that the present study was well positioned to answer two important questions: whether any contact with the justice system leads to positive or negative outcomes, and whether different degrees of involvement with the juvenile justice system has differential effects on adolescents' behavior. The present study also tested new outcome variables by looking at how arrested and non-arrested youth differ in their school and teacher attachment, school misconduct, truancy, and substance use. Another strength of the present study is its attention to a variety of mediators, such as school and teacher bonding, the value that youth place on future success, adolescents' expectations for future success, and the amount of school that youth would like to attend and the amount of school that youth expect to attend.

Furthermore, the present study addressed the study aims with multiple different types of analyses and found consistent results across the different models. For example, I looked at change scores, compared differences at baseline to differences at the follow up, and used a longitudinal method to look at within group change. The main message was the same regardless of the type of analysis: for the same degree of self-reported illegal behavior, formal and informal youth are much more likely to be arrested than no-contact youth. Finally, the most noteworthy strength of the present study is that recidivism was measured with both youth self-reports and official arrest records, which led to two conflicting conclusions. This is the first study, to my knowledge, that examined this question with two different indicators of recidivism.

Hypotheses Not Confirmed

It was very interesting that none of the study hypotheses were confirmed. However, this should not be surprising given the limitations in prior work discussed throughout this dissertation. Although many studies have found that arrested youth are more likely than non-arrested youth to suffer from later maladjustment, these relations could be spurious, with both the arrest and later outcomes explained by the same individual or contextual variables that facilitated the arrest. This is consistent with the third variable problem, such as the propensity to engage in illegal behavior and the lack of self-control, that Gottfredson and Hirschi have been arguing since at least the 1980s (see Gottfredson & Hirschi, 1987). Because the present study specifically recruited non-arrested youth who violated the same laws as the arrested sample, and statistically matched arrested and non-arrested youth on a variety of variables known to be related to both likelihood of arrest as well as the outcome variables, the present study permitted a more nuanced test of whether contact with the juvenile justice system in and of itself is related to subsequent maladjustment.

I was, however, surprised to find that informally processed and formally processed youth did not differ from no-contact youth and did not exhibit any within group change in their substance use six months after the arrest. This is especially interesting given that many (if not all) justice system youth have a probation term that requires youth to submit to random drug testing. Furthermore, many youth involved in the justice system are referred to substance use treatment. In fact, the majority of adolescents in treatment for a substance use problem are referred by the juvenile justice system (Substance Abuse and Mental Health Services Administration Treatment Episode Data Set, 2008). In the present study, not surprisingly, formal and informal youth were much more likely than no-contact youth to report attending any substance use treatment between baseline and the follow up interview. At the follow up, 1.6% of

no-contact youth, compared to 15.2% of formal youth and 13.2% of informal youth, reported receiving drug or alcohol treatment in the previous 6 months. It was also surprising that the three groups, in general, did not differ in school and teacher attachment, regardless of whether they were referred to a continuation school, between baseline and the follow up.

However, formal and informal processing did have some positive effects on school outcomes. Recall that formal processing was related to an improvement in grades in school and informal processing was related to decreases in a variety of school misconduct. The positive effects of formal and informal processing on school engagement and performance could be due to the fact that almost all youth on probation have a probation term that requires them to attend school and follow all school rules, which means that probation officers likely monitoring attendance and behavior at school (Mayer, 2005). As a result, juvenile offenders' behavior at school is monitored by their parents as well as the court. This double monitoring and threat of punishment might encourage arrested youth to be more engaged in school. It would be important to follow up with these youth in 12, 18, and 24 months to see whether the positive effects on school outcomes are sustained after probation sentences are terminated. It is also possible that the positive effects are due to the justice system linking arrested youth in need of services to tutoring or educational services, mental health services, substance use treatment, social skills training, family counseling, or other behavior-enhancing treatment.

One finding that was both encouraging and discouraging concerned the lack of change in informally and formally processed individuals' expectation for future success. On the one hand, this finding is encouraging because I hypothesized, based on labeling theory and self-identifying as a delinquent, that arrested youth would significantly decline in their expectations for future success between baseline and the follow up. On the other hand, this lack of change is

discouraging because their matched no-contact counterparts significantly increased in their expectations for future success during this period. This increased expectation for future success for no-contact youth, in turn, was related to small improvements in grades in school, closer relationships with their teachers, lower school misconduct, and less school truancy. To the extent that slight improvements in expectations for future success are normative as individuals age during adolescence, justice system involvement may halt development in this domain. This is consistent with some aspects of labeling theory, which suggest that justice system involvement may cause a reduction in adolescents' real and perceived perceptions of future opportunities (Becker, 1963; Lemert, 1951). Future research should continue to follow arrested and non-arrested youth to see whether the gap in future success expectations continues to widen or whether arrested youth eventually catch back up to their non-arrested counterparts.

One particularly curious finding was with regard to the age interactions. For both informally and formally processed youth, the analyses presented previously indicate that older youth improve more in grades in school than younger youth. Age was not related to changes in school grades for no-contact youth. This could be due to the type of adolescent who is arrested for the first time as a 13 year old, for example, compared to the type of adolescent who is arrested for the first time as a 17 year old. Indeed much prior work has indicated that individuals who start engaging in crime at younger ages are likely to have a host of risk factors (Farrington, 2009). In fact, the age at which an adolescent is first arrested is one of the most robust predictors of whether that individual will desist from crime or recidivate as an adult (Farrington, 2009). In the presented study, of the informally processed and formally processed youth, younger youth came from worse neighborhoods and had parents who were less effective monitors than older youth.

Implications, Policy Recommendations, and Conclusion

Although the majority of the hypothesized indirect effects were not significant in the present study, the mediation analyses revealed some interesting results that can be used to improve justice system interventions. Given that increases in peer delinquency were strongly related to increases in offending frequency and offending variety, substance use frequency and variety, and school misconduct, justice system interventions should not only be sensitive to grouping like-minded delinquents together, but interventions should also try to connect juvenile offenders to positive peer influences and help offenders develop interpersonal skills to foster healthy friendships with pro-social peers. In addition to peer delinquency, the value that youth attribute to future success was also strongly related to future offending. If interventions encourage offenders to not only aspire success but also to value it, juvenile delinquents may be more likely to desist from crime. Finally, it is important for the justice system to require and ensure that youth stay enrolled in school and attend school on a regular basis. Being truant from school was related to more offending, more substance use, and less school attachment. Of course, given that the present study only had two measurement occasions, it is unclear whether the proposed mediators actually caused changes in these outcome variables or vice versa. The present study can only demonstrate that they changed simultaneously.

Conclusion. As a result of justice system interventions, juvenile offenders might engage in less offending and less school misconduct but they also may be transferred to alternative or continuation schools and continue to be arrested for minor offenses. More severe contact with the justice system, i.e., formal processing, is related to worse outcomes than informal processing. Notwithstanding the limitations in the present study, the most important take-away message is that diversion, at least in one site for low-level offenders, might deter future offending, however,

it will also lead to subsequent run-ins with the law. The increased likelihood of subsequent arrests undermines any potentially positive impact of informal processing and leads to the revolving door of the justice system. The first contact with the justice system might have a positive impact on juvenile offenders' behavior; however, it is unlikely that there will be positive effects as youth are channeled deeper into the justice system and they become increasingly "ensnared" in the criminal lifestyle (Moffitt, 1993). This issue becomes especially salient as juvenile offenders reach the age of majority and they transition into the adult criminal system, where arrest records are public and permanent.

Policy recommendations. The data in the present study indicate that formal processing for this population does not deter future offending. It leads to subsequent offending and subsequent contact with the justice system and causes youth to be transferred out of traditional schools and into alternative or continuation schools. Prior research consistently shows that having an arrest record or being transferred to an alternative school substantially reduces the likelihood of subsequent educational and occupational attainment and success. As such, the default policy should be to divert low-level first-time offenders and keep the justice system's involvement to a minimum.

VI. References

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Appendix 1.

Eligible Charges In Orange County

Code	**Description**
HS 11350	DRUG POSSESSION
PC 148(A)	RESISTING ARREST
PC 148(A)(1)	RESISTING ARREST
PC 240/242	ASSAULT/BATTERY
PC 241	ASSAULT
PC 242	BATTERY
PC 243.1	BATTERY
PC 243.6	BATTERY
PC 243.6	BATTERY
PC 243(A)	BATTERY
PC 243(B)	BATTERY
PC 243(C)	BATTERY
PC 243(C)(1)	BATTERY
PC 243(C)(2)	BATTERY
PC 243(E)(1)	BATTERY
PC 243.2(A)	BATTERY
PC 243.2(A)(1)	BATTERY
PC 243.5(A)(2)	BATTERY
PC 415	FIGHTING IN PUBLIC
PC 459	BURGLARY
PC 459-460 (B)	BURGLARY IN THE SECOND DEGREE
PC 488	PETTY THEFT
PC 594	VANDALISM

Project ID#_____

Appendix 2

Peer Locator Sheet and Consent to Contact

You mentioned some friends during our interview today. We'd like to contact them to see if they might be interested in participating in a similar, paid research study. If you think this is something your friends might want to do, would you provide some information so we can contact them?

Name	**Age**	**Address**	**Telephone/cell phone number**

Consent to Contact

I hereby give the Crossroads research staff permission to contact the people listed above.

You can change your mind at any time if you decide that you no longer want us to contact these people. Participation will be entirely voluntary and your friends do not have to participate. We will not tell your friends anything about your involvement with the Crossroads study, other than you gave us their name. Also, only friends who meet our eligibility requirements will be able to participate in the study.

Signature of Subject: Date:

_____ _____

Printed name of Subject: Witness:

_____ _____

Appendix 3

Dissertation Measures and Individual Items

Measures in this document are sorted according to the order in which they are presented in the dissertation. They appeared in a different order when the interview was administered.

OUTCOME VARIABLES

Recidivism – Self Report of Offending

We are now going to spend some time talking about things you may or may not have done. You will see that some of the things I will ask you about have to do with illegal activity you may have been involved with at some point. I want to remind you that the things we talk about will be kept confidential. If you tell me of a past crime that you committed, I will not reveal this information to anyone – even if you were not charged for this crime. There are two exceptions to this promise of confidentiality:

1) if you tell me about a specific plan you have to commit a crime in the future, and
2) if you tell me that someone is in jail for a crime that you committed.

Other than those two situations, this information will be kept confidential

Have you ever...	Have you done this in the past six months?	How many times have you done this in the past six months?	How old were you when you first did this?	How old were you the last time you did this?	Thinking about the last time you did this, was anyone with you at the time? (1) Yes (5) No
Destroyed or damaged property that did not belong to you? (SRORow1) (1) Yes (5) No					
Have you ever purposely set fire to a house, building, car, or vacant lot? (SRORow2) (1) Yes (5) No					
Have you ever					

entered or broken into a building to steal something? (SRORow3) (1) Yes (5) No						
Have you ever stolen something from a store (shoplifted)? (SRORow4) (1) Yes (5) No						
Have you ever bought, received, or sold something that you knew was stolen? (SRORow5) (1) Yes (5) No						
Have you ever used checks or credit cards illegally? (SRORow6) (1) Yes (5) No						
Have you ever stolen a car or motorcycle to keep or sell? (SRORow7) (1) Yes (5) No						
Have you ever sold marijuana? (SRORow8) (1) Yes (5) No						
Have you ever sold other illegal drugs (cocaine,						

crack, heroine)? (SRORow9) (1) Yes (5) No						
Have you ever carjacked someone? (SRORow10) (1) Yes (5) No						Did you have a gun the last time you did this? (1) Yes (5) No
Have you ever driven while you were drunk or high? (SRORow11) (1) Yes (5) No						
Have you ever been paid by someone for having sexual relationship with them? (SRORow12) (1) Yes (5) No						
Have you ever forced someone to have sex with you? (SRORow13) (1) Yes (5) No						Did you have a gun the last time you did this? (1) Yes (5) No
Have you ever killed someone? (SRORow14) (1) Yes (5) No						Did you have a gun the last time you did

						this? (1) Yes (5) No
Have you ever shot someone (where bullet hit the victim)? (SRORow15) (1) Yes (5) No						
Have you ever shot AT someone (where you pulled the trigger)? (SRORow16) (1) Yes (5) No						
Have you ever taken something from another person by force, using a weapon? (SRORow17) (1) Yes (5) No						Did you have a gun the last time you did this? (1) Yes (5) No
Have you ever taken something from another person by force, without a weapon? (SRORow18) (1) Yes (5) No						
Have you ever beaten up or physically attacked someone so badly that they probably needed a doctor? (SRORow19) (1) Yes (5) No						

Have you ever been in a fight? (SRORow20) (1) Yes (5) No						
Have you ever beaten up, threatened, or physically attacked someone as part of a gang? (SRORow21) (1) Yes (5) No						Did you have a gun the last time you did this? (1) Yes (5) No
Have you ever carried a gun? (SRORow22) (1) Yes (5) No						
Have you ever broken into a car to steal from it? (SRORow23) (1) Yes (5) No						
Have you ever gone joyriding? (SRORow24) (1) Yes (5) No						

Substance Use

I would like to spend some time now talking about your use of alcohol and drugs over the past six months and over your lifetime. Remember that all this information is confidential. (CARD20)

What is the most that you have EVER used:		If used: In the past six months:	
alcohol (such as beer, wine, wine coolers, hard liquor, vodka, gin, or	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month	How often have you had alcohol to drink?subuse1	(1) None/not used in lifetime (2) 1-2 times (3) Less than

whiskey?)subuse0	(4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How often did you have five or more drinks at one time? Subuse3	1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day (1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Marijuana or hashish to get high? SubEv1	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times did you use marijuana or hashish? SubRec1	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Sedatives or tranquilizers to get high (this includes sleeping pills, barbiturates, seconal, valium, librium,	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month	How many times have you used sedatives or tranquilizers to get high? (this includes sleeping pills,	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month

xanax, quaaludes, etc.)? SubEv2	(6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	barbiturates, seconal, valium, Librium, xanax, ativan, Quaaludes, ect) SubRec2	(5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Stimulants or amphetamines to get high (like diet pills, benzadrine, methamphetamine)? SubEv3	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used stimulants or amphetamines? (like diet pills, benzadrine, methamphetamine) SubRec3	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Cocaine (including powder, crack, free base, coca leaves, or paste)? SubEv4	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used cocaine (including power, crack, free base, coca leaves or paste)? SubRec4	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Opiates (such as heroin, codeine, demoral, morphine, percodan, methodone, darvon, Opium, dilaudid, or talwin)? SubEv 5	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used opiates (such as heroin, oxycontin, codeine, demoral, morphine, percodan, methodone, darvon, opium, dilaudid or talwin)? SubRec5	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per

			week (8) 4-5 times per week (9) Every day
Ecstasy to get high? SubEv6	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used ecstasy to get high? SubRec6	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Hallucinogens (these include acid, LSD, mescaline, peyote, DMT, psilocybin, mushrooms, PCP, angel dust, etc.)? SubEv7	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used hallucinogens to get high? (these include acid, LSD, mescaline, peyote, DMT, psilocybin, mushrooms, PCP, angel dust ect.) SubRec7	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Used inhalants (like glue, cleaning fluids, gasoline, toluene, or paint)? SubEv8	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used inhalants to get high? (like glue, cleaning fluids, gasoline, toluene or paint). SubRec8	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day

Amyl nitrate, odorizers or rush? SubEv9	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used amyl nitrate, odorizers, or rush to get high? SubRec9	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Medications that the doctor prescribed to you to get high? SubEv10	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used medications that the doctor prescribed to you to get high? SubRec10	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Someone else's prescription medications to get high? SubEv11	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	How many times have you used someone else's prescription medications to get high? SubRec11	(1) None/not used in lifetime (2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day
Any other drugs to get high?	(1) None/not used in lifetime	How many times have you used any	(1) None/not used in lifetime

SubEv12 Specify:	(2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day	OTHER drugs to get high? SubRec12 Specify which ones:	(2) 1-2 times (3) Less than 1X/month (4) Once per month (5) 2-3 times per month (6) Once per week (7) 2-3 times per week (8) 4-5 times per week (9) Every day

Teacher Attachment

(If subject is currently enrolled in school)
Please tell me the number that indicates how much you agree or disagree with the following statements about school and teachers. (CARD6)

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree	NA
Most of my teachers treat me fairly (s0sch8)	(1)	(2)	(3)	(4)	(5)	(7)
I care what my teachers think of me. (s0sch9)	(1)	(2)	(3)	(4)	(5)	(7)
I like my teachers. (s0sch13)	(1)	(2)	(3)	(4)	(5)	(7)

School Attachment

These questions refer to the school most often attended in the past six months (if tied, most recently), according to the School Calendar. [EXCLUDING CORRESPONDENCE/INTERNET SCHOOLS]

I am going to ask you some questions about what it is (was) like at XXX School. Please say how much you agree or disagree with each statement. (CARD9)

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
I am happy when I am there. (s#scheval6)	(1)	(2)	(3)	(4)	(5)
I enjoy being there. (s#scheval16)	(1)	(2)	(3)	(4)	(5)

I feel like I am a part of that school. (s#scheval36)	(1)	(2)	(3)	(4)	(5)
I like school	(1)	(2)	(3)	(4)	(5)

School Achievement

I'd like to ask you some questions about school.
Are you currently enrolled in school now? (By enrolled, I mean signed up to go to school) (s0Enrld)
Interviewer: If Subject is home-schooled, code YES. "School" here means any kind of formal education, not just high school. If subject is taking "on-line" classes, consider the subject enrolled.

(1) Yes
(5) No
If No:
Have you dropped out of school? (s0DropOut)
(1) Yes
(5) No
At any time in the past six months, did you attend school or take classes toward a certificate or degree?
Interviewer: "School" here means any kind of formal education or on-line classes, not just high school. Do NOT count test preparation classes (e.g. SAT prep courses) or therapeutic interventions (e.g. anger management, traffic school)
(1) Yes
(5) No
If No: Skip School Calendar

If Yes (to currently enrolled):
What is the name of the school you are currently enrolled in? (s0NowSchName)
_____
Interviewer: Enter a short label to be used on the School Calendar to identify this school. Use 'HOME' if currently home-schooled.
Have you been enrolled at this school throughout the past six months? (s0SameSch)
(1) Yes
(5) No

Academic Achievement

What are/were your grades like in school? (s#Dem29) (CARD7)
Interviewer: If subject is currently dropped out, refer to the last grades received in school.

(1) Mostly A's
(2) About half A's and half B's
(3) Mostly B's
(4) About half B's and half C's

(5) Mostly C's
(6) About half C's and half D's
(7) Mostly D's
(8) Mostly below D's
(97) NA/ No grades given

School Calendar

For each school fill out the chart below:

School Name	What type of school is…? (What type of degree were you working toward?) (s0PIType) *Interviewer: For internet/correspondence schools, rate based on degree program that subject is/was pursuing when enrolled*	What type of school is…? (How did/do you complete the course-work?) (s0PIMedium) *Interviewer: Use 1 for typical high schools (daytime attendance) and colleges where subject lives at home or off campus. Use 2 for non-detention boarding schools or colleges where subject lives on campus.*
1)	(1) Elementary School (2) Middle School (Jr. High) (3) High School (4) GED Class (5) Vocational Training (6) Associate's Degree Program (7) 2-year college (8) 4-year college (9) Masters Program (10) Doctoral (M.D./Ph.D.) Program (11) Law Degree Program (96) Other (specify) _____	(1) Traditional (day school, non-residential) (2) Traditional (residential/boarding) (3) Night School (4) Correspondence/Internet (5) Home schooling (6) Detention-based (7) Other (Specify) _____
2)	(1) Elementary School (2) Middle School (Jr. High) (3) High School (4) GED Class (5) Vocational Training (6) Associate's Degree Program (7) 2-year college (8) 4-year college (9) Masters Program (10) Doctoral (M.D./Ph.D.) Program (11) Law Degree Program	(1) Traditional (day school, non-residential) (2) Traditional (residential/boarding) (3) Night School (4) Correspondence/Internet (5) Home schooling (6) Detention-based (7) Other (Specify) _____

	(96) Other (specify) _____	
3)	(1) Elementary School (2) Middle School (Jr. High) (3) High School (4) GED Class (5) Vocational Training (6) Associate's Degree Program (7) 2-year college (8) 4-year college (9) Masters Program (10) Doctoral (M.D./Ph.D.) Program (11) Law Degree Program (96) Other (specify) _____	(1) Traditional (day school, non-residential) (2) Traditional (residential/boarding) (3) Night School (4) Correspondence/Internet (5) Home schooling (6) Detention-based (7) Other (Specify) _____
4)	(1) Elementary School (2) Middle School (Jr. High) (3) High School (4) GED Class (5) Vocational Training (6) Associate's Degree Program (7) 2-year college (8) 4-year college (9) Masters Program (10) Doctoral (M.D./Ph.D.) Program (11) Law Degree Program (96) Other (specify) _____	(1) Traditional (day school, non-residential) (2) Traditional (residential/boarding) (3) Night School (4) Correspondence/Internet (5) Home schooling (6) Detention-based (7) Other (Specify) _____
5)	(1) Elementary School (2) Middle School (Jr. High) (3) High School (4) GED Class (5) Vocational Training (6) Associate's Degree Program (7) 2-year college (8) 4-year college (9) Masters Program (10) Doctoral (M.D./Ph.D.) Program (11) Law Degree Program (96) Other (specify)_____	(1) Traditional (day school, non-residential) (2) Traditional (residential/boarding) (3) Night School (4) Correspondence/Internet (5) Home schooling (6) Detention-based (7) Other (Specify) _____

School Misconduct (Truancy)

How often have you skipped school without permission in the past six months? (s0SchMC2)

(1) Not at all
(2) Once or twice
(3) Several times
(4) Often/Many times

How many times have the following things happened to you over the past six months? (CARD8) *Interviewer: If the subject was only in school for part of the time, the responses should refer to when the subject was in school.*	
I was late for school. (s0truant1)	(0) Never (1) 1-2 times (3) 3-6 times (7) 7-9 times (10) 10 or more times
I cut or skipped classes. (s0truant2)	(0) Never (1) 1-2 times (3) 3-6 times (7) 7-9 times (10) 10 or more times
I was absent from school. (s0truant3)	(0) Never (1) 1-2 times (3) 3-6 times (7) 7-9 times (10) 10 or more times
I got in trouble at school for missing too many days. (s0truant4)	(0) Never (1) 1-2 times (3) 3-6 times (7) 7-9 times (10) 10 or more times
I had to go to truancy court. (s0truant5)	(0) Never (1) 1-2 times (3) 3-6 times (7) 7-9 times (10) 10 or more times

School Misconduct

Have you ever been suspended from school? (S0Dem30)

(1) Yes
(5) No
If Yes:
*How many times (were you suspended)? (S0Dem31)*_____
How old were you the first time (you were suspended)? (S0Dem32) _____

Have you ever been expelled from school? (S0Dem33)

(1) Yes

(5) No

If Yes:

How many times (were you expelled)? (S0Dem34) _____

How old were you the first time (you were expelled)? (S0Dem34b) _____

School Misconduct

How often have you EVER done each of these things? (Remember that your answer will be kept ABSOLUTELY CONFIDENTIAL.) (CARD10)

Have you ever….	How often have you…in the last 6 months?	Did you ever…before you were 11 years old?
Copied homework or a class assignment off somebody else. (S0SchMca3) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No
Cheated on a test. (S0SchMca4) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No
Gotten in trouble for disturbing the class. (S0SchMca5) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No
Purposely damaged school property. (S0SchMca6) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No
Been kicked out of class. (S0SchMca7) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No
Been sent to the principal's office. (S0SchMca8) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No
Received detention. (S0SchMca9) (1) Yes (5) No	(1) Not at all (2) Once or Twice (3) Several Times (4) Often/Many times	(1) Yes (5) No

In the past 6 months, have you been suspended from school? (s#susp6m)

(1) Yes

(5) No
If Yes:
How many times? ___ (s#susp6mct)

In the past 6 months, have you been expelled from school? (s#exp6m)
(1) Yes
(5) No
If Yes:
How many times? ___ (s#exp6mct)

MEDIATING VARIABLES

Neighborhood Climate

In this next part, we are interested in your feelings about work, school and the future. Please tell me the number that indicates how much you agree or disagree with the following statements. (CARD16)

	Strongly Disagree	Disagree	Neither Agree nor Disagree	Agree	Strongly Agree
In my neighborhood, it's pretty easy for a young person to get a good-paying, honest job. (s#sch40)	(1)	(2)	(3)	(4)	(5)
Most of my friends have graduated or will graduate from high school. (s#sch41)	(1)	(2)	(3)	(4)	(5)
In my neighborhood, it's hard to make much money without doing something illegal. (s#sch42)	(1)	(2)	(3)	(4)	(5)
College is too expensive for most of the people in my neighborhood. (s#sch43)	(1)	(2)	(3)	(4)	(5)
I'll never have as much opportunity to succeed as people from other neighborhoods. (s#sch44)	(1)	(2)	(3)	(4)	(5)
My chances of getting ahead and being successful are not very good. (s#sch45)	(1)	(2)	(3)	(4)	(5)

School Aspirations

How far would you LIKE to go in school? (s#Sch46)
(1) Drop out before graduation
(2) Graduate from high school
(3) Go to a business, technical school or junior college
(4) Graduate from college
(5) Go to graduate or professional school

School Expectations

How far do you THINK you will go in school? (s#Sch47)

(1) Drop out before graduation
(2) Graduate from high school
(3) Go to a business, technical school or junior college
(4) Graduate from college
(5) Go to graduate or professional school

Perceptions of Opportunities

Value of Future Work and School Success

Now I'd like to ask you some questions about your long range goals and your chances of achieving these goals. (CARD21)

How important is it to you...	Not at all important	Not too important	Somewhat important	Pretty important	Very important	NA- Already achieved
To have a good job or career? (s0Opp01a)	(1)	(2)	(3)	(4)	(5)	(97)
To graduate from college? (s0Opp02a)	(1)	(2)	(3)	(4)	(5)	(97)
To earn a good living? (s0Opp03a)	(1)	(2)	(3)	(4)	(5)	(97)

Expectations for Future Work and School Success

What do you think your chances are... (CARD22)	Poor	Fair	Good	Very Good	Excellent	NA- already achieved
To have a good job or career? (s0Opp01b)	(1)	(2)	(3)	(4)	(5)	(97)
To graduate from college? (s0Opp02b)	(1)	(2)	(3)	(4)	(5)	(97)
To earn a good living? (s0Opp03b)	(1)	(2)	(3)	(4)	(5)	(97)

Rosenberg Self-Esteem

The next set of questions deals with your general feelings about yourself. Please indicate how much you agree or disagree with each statement. (CARD27)

	Strongly Disagree	Disagree	Agree	Strongly Agree
I feel that I'm a person of worth, at least on an equal plane with others. (s0SE1)	(0)	(1)	(2)	(3)
I feel that I have a number of good qualities. (s0SE2)	(0)	(1)	(2)	(3)
All in all, I am inclined to feel that I am a failure. (s0SE3)	(0)	(1)	(2)	(3)
I am able to do things as well as most other people. (s0SE4)	(0)	(1)	(2)	(3)
I feel I do not have much to be proud of. (s0SE5)	(0)	(1)	(2)	(3)
I take a positive attitude toward myself. (s0SE6)	(0)	(1)	(2)	(3)
On the whole, I am satisfied with myself. (s0SE7)	(0)	(1)	(2)	(3)
I wish I could have more respect for myself. (s0SE8)	(0)	(1)	(2)	(3)
I certainly feel useless at times. (s0SE9)	(0)	(1)	(2)	(3)
At times, I think I am no good at all. (s0SE10)	(0)	(1)	(2)	(3)

Association with Deviant Peers

I am going to ask some questions about your friends. (CARD14)

During the past six months, how many of your friends have…	None of them	Very few of them	Some of them	Most of them	All of them
Purposely damaged or destroyed property that did not belong to them? (s0PDel1)	(1)	(2)	(3)	(4)	(5)
Hit or threatened to hit someone? (s0PDel2)	(1)	(2)	(3)	(4)	(5)
Sold drugs? (s0PDel3)	(1)	(2)	(3)	(4)	(5)
Gotten drunk once in a while? (s0PDel4)	(1)	(2)	(3)	(4)	(5)
Gotten high on drugs (s0PDel5)	(1)	(2)	(3)	(4)	(5)
Carried a knife? (s0PDel6)	(1)	(2)	(3)	(4)	(5)
Carried a gun? (s0PDel7)	(1)	(2)	(3)	(4)	(5)
Owned a gun? (s0PDel8)	(1)	(2)	(3)	(4)	(5)
Gotten into a physical fight?	(1)	(2)	(3)	(4)	(5)

(s0PDel9)					
Been hurt in a fight? (s0PDel10)	(1)	(2)	(3)	(4)	(5)
Stolen something worth more than $100? (s0PDel11)	(1)	(2)	(3)	(4)	(5)
Taken a motor vehicle or stolen a car? (s0PDel12)	(1)	(2)	(3)	(4)	(5)
Gone in or tried to go into a building to steal something? (s0PDel13)	(1)	(2)	(3)	(4)	(5)
Suggested that you should go out drinking with them? (s0PDel14)	(1)	(2)	(3)	(4)	(5)
Suggested or claimed that you have to get drunk to have a good time? (s0PDel15)	(1)	(2)	(3)	(4)	(5)
Suggested or claimed that you have to be high on drugs to have a good time? (s0PDel16)	(1)	(2)	(3)	(4)	(5)
Suggested that you should sell drugs? (s0PDel17)	(1)	(2)	(3)	(4)	(5)
Suggested that you should steal something? (s0PDel18)	(1)	(2)	(3)	(4)	(5)
Suggested that you should hit or beat someone up? (s0PDel19)	(1)	(2)	(3)	(4)	(5)
Suggested that you should carry a weapon? (s0PDel20)	(1)	(2)	(3)	(4)	(5)

How many close friends do you have? (Relatives are OK. Include your girlfriend/ boyfriend if you have one, if you consider him/her to be a close friend.) (s0NFrnd)
Interviewer: We want at least one name here. If subject says he/she has no 'close' friends, then ask about anyone they consider a 'friend'. Boyfriends/girlfriends should be included if they are 'close' friends.
FrNames [1] _____
FrNames [2] _____
FrNames [3] _____
FrNames [4]_____
FrNames [5] _____

Tell me some more about your close friends. (CARD10)

	Friend 1	Friend 2:	Friend 3:	Friend 4:	Friend 5:
How old is					

friend? (s0AgeFr#)					
Male or female? (s0Genfr#)	(1) Male (2) Female	(1) Male (2) Female	(1) Male (2) Female	(1) Male (2) Female	(1) Male (2) Female
How often do you have contact with him/her? (s0ContFr#) [Card 3]	(1) Daily (2) 3-6 times a week (3) 2 times per week (4) 1 time per week (5) Less weekly, more than monthly (6) 1 time per month (7) Less than monthly	(1) Daily (2) 3-6 times a week (3) 2 times per week (4) 1 time per week (5) Less weekly, more than monthly (6) 1 time per month (7) Less than monthly	(1) Daily (2) 3-6 times a week (3) 2 times per week (4) 1 time per week (5) Less weekly, more than monthly (6) 1 time per month (7) Less than monthly	(1) Daily (2) 3-6 times a week (3) 2 times per week (4) 1 time per week (5) Less weekly, more than monthly (6) 1 time per month (7) Less than monthly	(1) Daily (2) 3-6 times a week (3) 2 times per week (4) 1 time per week (5) Less weekly, more than monthly (6) 1 time per month (7) Less than monthly
Ever been arrested? (s0ArrFr#)	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No
Ever been in jail or in detention? (s0JaiFr#)	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No
Ever been in a mental hospital? (s0HosFr#)	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No
Ever used drugs? (s0DruFr#)	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No	(1) Yes (5) No
Is he/she one of the same people you mentioned as a co-offender for (charge in month/year)? (s0CoofFr#)	(1) Yes Identify: (5) No	(1) Yes Identify: (5) No	(1) Yes Identify: (5) No	(1) Yes Identify: (5) No	(1) Yes Identify: (5) No

Parental Involvement

You mentioned earlier that [PCareName] is the main person who is responsible for raising you.
Interviewer: Time Frame=right now. Responsible person means a legal guardian/parent who is responsible for raising the subject. It cannot be a boyfriend, girlfriend, or peer.
NOTE: The youth's residence in a facility (if applicable) does not preclude answering this series of questions.
NOTE: These answers should reflect the level of monitoring that characterizes the majority of the time in the recall period. If it is split evenly, this should reflect the time during which there were no intervening circumstances influencing the level of monitoring. For example, some parents have distinguished the level of monitoring before the youth was on probation vs. while he/she was on probation.

(Card 11)
How much does [PcareName] try to know who you spend time with? (s#Pmonit1)
(1) Doesn't try at all
(2) Tries a little bit
(3) Tries a lot
(4) Tries extremely hard

How much does [PcareName] really know who you spend time with? (s#Pmonitb1)
(1) Doesn't know at all
(2) Knows a little bit
(3) Knows a lot
(4) Knows everything

How much does [PcareName] try to know how you spend your free time? (s#Pmonit2)
(1) Doesn't try at all
(2) Tries a little bit
(3) Tries a lot
(4) Tries extremely hard

How much does [PcareName] really know how you spend your free time? (s#Pmonitb2)
(1) Doesn't know at all
(2) Knows a little bit
(3) Knows a lot
(4) Knows everything

How much does [PcareName] try to know how you spend your money? (s#Pmonit3)
(1) Doesn't try at all
(2) Tries a little bit
(3) Tries a lot
(4) Tries extremely hard

How much does [PcareName] really know how you spend your money? (s#Pmonitb3)

(1) Doesn't know at all
(2) Knows a little bit
(3) Knows a lot
(4) Knows everything

How much does [PcareName] try to know about where you go right after school or work is over for the day? (s#Pmonit4)
(1) Doesn't try at all
(2) Tries a little bit
(3) Tries a lot
(4) Tries extremely hard

How much does [PcareName] really know about where you go right after school or work is over for the day? (s#Pmonitb4)
(1) Doesn't know at all
(2) Knows a little bit
(3) Knows a lot
(4) Knows everything

How much does [PcareName] try to know about where you go at night? (s#Pmonit5)
(1) Doesn't try at all
(2) Tries a little bit
(3) Tries a lot
(4) Tries extremely hard

How much does [PcareName] really know about where you go at night? (s#Pmonitb5)
(1) Doesn't know at all
(2) Knows a little bit
(3) Knows a lot
(4) Knows everything

Do you live with [PcareName]? (s0PmLive)
(1) Yes
(5) No

How often do you have a set time to be home on school or work nights? (s#Pmonit7) (CARD12)
(1) Never
(2) Sometimes
(3) Usually
(4) Always
(97) NA (don’t live with caretaker)

How often do you have a set time to be home on weekend nights? (s#Pmonit8)
(1) Never
(2) Sometimes
(3) Usually

(4) Always
(97) NA (don't live with caretaker)

How often does [PcareName] know what time you will be home when you've gone out. (s#Pmonit9)
(1) Never
(2) Sometimes
(3) Usually
(4) Always
(97) NA (don't live with caretaker)

If [PcareName] is not at home, how often do you leave a note, call, or communicate with [PcareName] in some way about where you are going? (s#Pmonit10)
(1) Never
(2) Sometimes
(3) Usually
(4) Always
(97) NA (don't live with caretaker)

- **School and teacher attachment (described previously)**

- **School truancy (described previously**)

MATCHING VARIABLES

Demographics

What is your date of birth?

In what country were you born? (s0Dem19)
(1) United States
(96) Other (specify-s0Dem20) _____

Do you consider yourself to be Latino or Hispanic? (s0Dem21)
(1) Yes
(5) No
[IF YES] To which Latino or Hispanic group would you say you belong? (s0Dem22)
(1) Mexican American (include 'Mexican' and 'Chicano' here)
(2) Cuban American
(3) Puerto Rican American
(96) Other (specify-s0Dem23) _____

Do you consider yourself to be White, African American/Black, Asian, Native American, or some other race? (s0Dem24)

(1) White
(2) African American/Black
(3) Asian
(4) Native American
(96) Other (specify-s0Dem25) _____

To which ethnic group would you say you belong? _____ (s0Dem25)

Educational background of parents (SES)

How far did your mother/female guardian go in school? (s0rel169)
(1) Some grade school
(2) Finished grade school
(3) Some high school
(4) High school diploma
(5) Business or trade school
(6) Some college or graduate of 2-year college
(7) College graduate (4-year college)
(8) Some graduate or professional school beyond college
(9) Professional or graduate degree
(97) NA - Single parent household

How far did your father/male guardian go in school? (s0rel170)
(1) Some grade school
(2) Finished grade school
(3) Some high school
(4) High school diploma
(5) Business or trade school
(6) Some college or graduate of 2-year college
(7) College graduate (4-year college)
(8) Some graduate or professional school beyond college
(9) Professional or graduate degree
(97) NA - Single parent household

Family Criminality

	What is the relationship to you of the family member who was involved in criminal activity (s0famcrim2)?	Was this person arrested (s0famcrim3)?	Was this person jailed (s0famcrim4)?	Did this person live at your home address when they were involved in criminal activity (s0famcrim5)?

Family Member 1				
Family Member 2				
Family Member 3				
Family Member 4				
Family Member 5				

Has anyone in your family ever committed a crime? (I won't ask for their names) (s0famcrim1).
Interviewer: Include grandparents, parents, brothers, and sisters (even if they don't live at home), or any other relatives living at home, birth or otherwise.

(1) Yes
(5) No

If Yes:

Relationship Codes:

11. Biological Father
12. Biological Mother
13. Biological Sister
14. Biological Brother
15. Biological Grandmother
16. Biological Grandfather
17. Stepfather
18. Stepmother
19. Stepsister
20. Stepbrother
21. Adoptive Father
22. Adoptive Mother
23. Adoptive Sister
24. Adoptive Brother
25. Wife
26. Husband
27. Son
28. Daughter
29. Aunt
30. Uncle
31. Female Cousin
32. Male Cousin
33. Nephew
34. Niece
35. Live-in boyfriend/girlfriend
36. Female Friend
37. Male Friend
38. Boyfriend (not live-in)
39. Girlfriend (not live-in)
40. Male Roommate
41. Female Roommate
42. Professional Relationship
43. Foster mother
44. Foster father
45. Foster brother
46. Foster sister
47. Mother of my child (if no other category applies)
48. Father of my child (if no other category applies)
49. Stepson (non-biol. child in subject's care)
50. Stepdaughter (non-biol. child in subject's care)
51. Fiancé(e)
52. Foster daughter
53. Foster son
95. Other relative
96. Other (not biologically related)
97. NA

Academic Achievement

What are/were your grades like in school? (s#Dem29) (CARD7)
Interviewer: If subject is currently dropped out, refer to the last grades received in school.

(3) Mostly A's

(4) About half A's and half B's
(3) Mostly B's
(4) About half B's and half C's
(5) Mostly C's
(6) About half C's and half D's
(7) Mostly D's
(8) Mostly below D's
(97) NA/ No grades given

Maturity of Judgment

Weinberger Adjustment Inventory

Please respond to each statement by thinking about how you usually have felt or acted in the past six months by selecting one of the choices. (CARD19)

	False	Somewhat False	Not Sure	Somewhat True	True
Doing things to help people is more important to me than almost anything else. (s#wai1)	(1)	(2)	(3)	(4)	(5)
I'm the kind of person who will try anything once, even if it's not that safe. (s#wai2)	(1)	(2)	(3)	(4)	(5)
People who get me angry better watch out. (s#wai3)	(1)	(2)	(3)	(4)	(5)
I should try harder to control myself when I'm having fun. (s#wai4)	(1)	(2)	(3)	(4)	(5)
I often go out of my way to do things for other people. (s#wai5)	(1)	(2)	(3)	(4)	(5)
I can do things as well as other people can. (s#wai6)	(1)	(2)	(3)	(4)	(5)
I do things without giving them enough thought. (s#wai7)	(1)	(2)	(3)	(4)	(5)
I enjoy doing things for other people, even when I don't receive anything in return. (s#wai8)	(1)	(2)	(3)	(4)	(5)
If someone tries to hurt me, I make sure I get even with them. (s#wai9)	(1)	(2)	(3)	(4)	(5)
I like to do new and different things that many people would consider weird or not really safe.	(1)	(2)	(3)	(4)	(5)

(s#wai10)					
I become 'wild and crazy' and do things other people might not like. (s#wai11)	(1)	(2)	(3)	(4)	(5)
If someone does something I really don't like, I yell at them about it. (s#wai12)	(1)	(2)	(3)	(4)	(5)
Before I do something, I think about how it will affect people around me. (s#wai13)	(1)	(2)	(3)	(4)	(5)
When I'm doing something fun (like partying or acting silly), I tend to get carried away and go too far. (s#wai14)	(1)	(2)	(3)	(4)	(5)
I say the first thing that comes into my mind without thinking enough about it. (s#wai15)	(1)	(2)	(3)	(4)	(5)
I pick on people I don't like. (s#wai16)	(1)	(2)	(3)	(4)	(5)
I try very hard not to hurt other people's feelings. (s#wai17)	(1)	(2)	(3)	(4)	(5)
I lose my temper and 'let people have it' when I'm angry. (s#wai18)	(1)	(2)	(3)	(4)	(5)
I make sure that doing what I want will not cause problems for others. (s#wai19)	(1)	(2)	(3)	(4)	(5)
I stop and think things through before I act. (s#wai20)	(1)	(2)	(3)	(4)	(5)
I say something mean to someone who has upset me. (s#wai21)	(1)	(2)	(3)	(4)	(5)
I think about other people's feelings before I do something they might not like. (s#wai22)	(1)	(2)	(3)	(4)	(5)
When someone tries to start a fight with me, I fight back. (s#wai23)	(1)	(2)	(3)	(4)	(5)

Future Outlook Inventory

Even though you may act differently depending on the situation, the purpose of these questions is to understand what you are usually like. Please listen carefully and select the choice that is most true of you. (CARD24)

	Never True	Rarely True	Often True	Always True
I will keep working at difficult,	(1)	(2)	(3)	(4)

boring tasks if I know they will help me get ahead later. (s0Foi001)				
I live each day as if it's my last. (s0Foi002)	(1)	(2)	(3)	(4)
I think about how things might be in the future. (s0Foi003)	(1)	(2)	(3)	(4)
I tend to get caught up in the excitement of the moment. (s0Foi004)	(1)	(2)	(3)	(4)
I make lists of things to do. (s0Foi005)	(1)	(2)	(3)	(4)
Before making a decision, I weigh the good vs. the bad. (s0Foi006)	(1)	(2)	(3)	(4)
The future is very vague and uncertain to me. (s0Foi007)	(1)	(2)	(3)	(4)
I will give up my happiness now so that I can get what I want in the future. (s0Foi008)	(1)	(2)	(3)	(4)
I make decisions on the spur of the moment. (s0Foi009)	(1)	(2)	(3)	(4)
I would rather save my money for a rainy day than spend it now on something fun. (s0Foi010)	(1)	(2)	(3)	(4)
I can't really plan for the future because things change so much. (s0Foi011)	(1)	(2)	(3)	(4)
I always seem to be doing things at the last minute. (s0Foi012)	(1)	(2)	(3)	(4)
I don't plan, I take each day as it is. (s0Foi013)	(1)	(2)	(3)	(4)
I can see my life 10 years from now. (s0Foi014)	(1)	(2)	(3)	(4)
I usually think about the consequences before I do something. (s0Foi015)	(1)	(2)	(3)	(4)

The Psychosocial Maturity Inventory

Please listen carefully and indicate the number that shows how much you agree with each statement. (CARD23)

	Strongly Disagree	Slightly Disagree	Slightly Agree	Strongly Agree
Hard work is never fun. (s0psm01)	(1)	(2)	(3)	(4)
I don't like to tell my ideas about God when I know				

others disagree with me. (s0psm02)	(1)	(2)	(3)	(4)
I'm the sort of person who can't do anything really well. (s0psm03)	(1)	(2)	(3)	(4)
If something more interesting comes along, I will usually stop any work I'm doing. (s0psm04)	(1)	(2)	(3)	(4)
It's not very practical to decide what kind of job you want because that depends so much on other people. (s0psm05)	(1)	(2)	(3)	(4)
I can't really say what my interests are. (s0psm06)	(1)	(2)	(3)	(4)
If you haven't been chosen as the leader, you shouldn't suggest how things should be done. (s0psm07)	(1)	(2)	(3)	(4)
I can't think of any kind of job that I would like a lot. (s0psm08)	(1)	(2)	(3)	(4)
I find it hard to stick to anything that takes a long time to do. (s0psm09)	(1)	(2)	(3)	(4)
In a group I prefer to let other people make the decisions. (s0psm10)	(1)	(2)	(3)	(4)
My life is pretty empty. (s0psm11)	(1)	(2)	(3)	(4)
I hate to admit it, but I give up on my work when things go wrong. (s0psm12)	(1)	(2)	(3)	(4)
You can't be expected to make a success of yourself if you had a bad childhood. (s0psm13)	(1)	(2)	(3)	(4)
I can't seem to keep people as friends for very long. (s0psm14)	(1)	(2)	(3)	(4)
I often don't get my most important work done because I've spent too much time on other work. (s0psm15)	(1)	(2)	(3)	(4)
Luck decides most things that happen to me. (s0psm16)	(1)	(2)	(3)	(4)
I act like something I'm not a lot of the time. (s0psm17)	(1)	(2)	(3)	(4)

I seldom get behind on my work. (s0psm18)	(1)	(2)	(3)	(4)
The main reason that I'm not more successful is that I have bad luck. (s0psm19)	(1)	(2)	(3)	(4)
I never know what I am going to do next. (s0psm20)	(1)	(2)	(3)	(4)
I tend to go from one thing to another before finishing any one of them. (s0psm21)	(1)	(2)	(3)	(4)
When things go well for me, it is usually not because of anything I myself actually did. (s0psm22)	(1)	(2)	(3)	(4)
I change the way I feel and act so often that I sometimes wonder who the "real" me is. (s0psm23)	(1)	(2)	(3)	(4)
I often don't finish work that I start. (s0psm24)	(1)	(2)	(3)	(4)
I feel very uncomfortable if I disagree with what my friends think. (s0psm25)	(1)	(2)	(3)	(4)
Nobody knows what I'm really like. (s0psm26)	(1)	(2)	(3)	(4)
I often leave my homework unfinished if there are a lot of good TV shows on that evening. (s0psm27)	(1)	(2)	(3)	(4)
It is best to agree with others, rather than say what you really think, if it will keep the peace. (s0psm28)	(1)	(2)	(3)	(4)
I am not really accepted and liked. (s0psm29)	(1)	(2)	(3)	(4)
No one should expect you to do work that you don't like. (s0psm30)	(1)	(2)	(3)	(4)

Resistance to Peer Influence

For each question, decide which sort of person you are most like. Then decide if that is sort of true or really true for you. For each line make only ONE of the four choices. Choose that statement that describes the type of person that you are most like:

	Really	Sort of				Sort of	Really

	True for Me (1)	True for Me (2)				True for Me (3)	True for Me (4)
s0RPI01	❍	❍	Some people go along with their friends just to keep their friends happy	Or	Other people refuse to go along with what their friends want to do, even though they know it will make their friends unhappy	❍	❍
s0RPI02	❍	❍	Some people think it's more important to be an individual than to fit in with the crowd	Or	Other people think it is more important to fit in with the crowd than to stand out as an individual	❍	❍
s0RPI03	❍	❍	For some people, it's pretty easy for their friends to get them to change their mind	Or	For other people, it's pretty hard for their friends to get them to change their mind	❍	❍
s0RPI04	❍	❍	Some people would do something that they knew was wrong just to stay on their friends' good side	Or	Other people would not do something they knew was wrong just to stay on their friends' good side	❍	❍
s0RPI05	❍	❍	Some people hide their true opinion from their their friends if they think their friends will make fun of them because of it	Or	Other people will say their true opinion in front of their friends, even if they know their friends will make fun of them because of it	❍	❍
s0RPI06	❍	❍	Some people will not break the law just because their friends say that they would	Or	Other people would break the law if their friends said that they would do it	❍	❍
s0RPI07	❍	❍	Some people change the	Or	Other people act the same	❍	❍

			way they act so much when they are with their friends that they wonder who they "really are"		way when they are alone as they do when they are with their friends		
s0RPI08	❍	❍	Some people take more risks when they are with their friends than they do when they are alone	Or	Other people act just as risky when they are alone as when they are with their friends	❍	❍
s0RPI09	❍	❍	Some people say things they don't really believe because they think it will make their friends respect them more	Or	Other people would Not say things they Didn't really believe Just to get their friends To respect them more	❍	❍
s0RPI10	❍	❍	Some people think it's better to be an individual even if people will be angry at you for going against the crowd.	Or	Other people think it's better to go along with the crowd than to make people angry at you	❍	❍

Parental Antisocial Behavior and Influence

Now I am going to ask you some of the same questions I just went through, but this time I'd like you to think particularly about your parents, and whether or not they have done these things.

During the past six months, has either of your parents…	Yes	No
Purposely damaged or destroyed property that did not belong to them? (s0SDel1)	(1)	(5)
Hit or threatened to hit someone? (s0SDel2)	(1)	(5)
Sold drugs? (s0SDel3)	(1)	(5)
Gotten drunk once in a while? (s0SDel4)	(1)	(5)
Gotten high on drugs (s0SDel5)	(1)	(5)
Carried a knife? (s0SDel6)	(1)	(5)
Carried a gun? (s0SDel7)	(1)	(5)
Owned a gun? (s0SDel8)	(1)	(5)

Gotten into a physical fight? (s0SDel9)	(1)	(5)
Been hurt in a fight? (s0SDel10)	(1)	(5)
Stolen something worth more than $100? (s0SDel11)	(1)	(5)
Taken a motor vehicle or stolen a car? (s0SDel12)	(1)	(5)
Gone in or tried to go into a building to steal something? (s0SDel13)	(1)	(5)
Suggested that you should go out drinking with them? (s0SDel14)	(1)	(5)
Suggested or claimed that you have to get drunk to have a good time? (s0SDel15)	(1)	(5)
Suggested or claimed that you have to be high on drugs to have a good time? (s0SDel16)	(1)	(5)
Suggested that you should sell drugs? (s0SDel17)	(1)	(5)
Suggested that you should steal something? (s0SDel18)	(1)	(5)
Suggested that you should hit or beat someone up? (s0SDel19)	(1)	(5)
Suggested that you should carry a weapon? (s0SDel20)	(1)	(5)

Neighborhood Characteristics

(NCIntro) You mentioned earlier that you lived at [homename] for the longest time period in the past six months.

Thinking about the neighborhood around [place lived in the most]…How often does each of the following occur within your neighborhood? (CARD15)

	Never	Rarely	Sometimes	Often
1. Cigarettes on the street or in the gutters? (s#NeiCon01)	(1)	(2)	(3)	(4)
2. Garbage in the streets or on the sidewalk? (s#NeiCon02)	(1)	(2)	(3)	(4)
3. Empty beer bottles on the streets or sidewalks? (s#NeiCon03)	(1)	(2)	(3)	(4)
4. Boarded up windows on buildings? (s#NeiCon04)	(1)	(2)	(3)	(4)
5. Graffiti or tags? (s#NeiCon05)	(1)	(2)	(3)	(4)
6. Graffiti painted over? (s#NeiCon06)	(1)	(2)	(3)	(4)
7. Gang graffiti? (s#NeiCon07)	(1)	(2)	(3)	(4)
8. Gangs (or other teen groups) hanging out? (s#NeiCon08)	(1)	(2)	(3)	(4)
9. Abandoned cars? (s#NeiCon09)	(1)	(2)	(3)	(4)
10. Empty lots with garbage? (s#NeiCon10)	(1)	(2)	(3)	(4)
11. Condoms on sidewalk? (s#NeiCon11)	(1)	(2)	(3)	(4)
12. Needles or syringes? (s#NeiCon12)	(1)	(2)	(3)	(4)
13. Political messages in graffiti? (s#NeiCon13)	(1)	(2)	(3)	(4)
14. Adults hanging out on the street? (s#NeiCon14)	(1)	(2)	(3)	(4)
15. People drinking beer, wine or liquor? (s#NeiCon15)	(1)	(2)	(3)	(4)
16. People drunk or passed out? (s#NeiCon16)	(1)	(2)	(3)	(4)

17. Adults fighting or arguing loudly? (s#NeiCon17)	(1)	(2)	(3)	(4)
18. Prostitutes on the streets? (s#NeiCon18)	(1)	(2)	(3)	(4)
19. People smoking marijuana? (s#NeiCon19)	(1)	(2)	(3)	(4)
20. People smoking crack? (s#NeiCon20)	(1)	(2)	(3)	(4)
21. People using needles or syringes to take drugs? (s#NeiCon21)	(1)	(2)	(3)	(4)

- **Wechsler Abbreviated Scale of Intelligence (assessed and scored with paper and pencil)**
- **Prior offending (measure described previously)**
- **Prior substance use (measure described previously)**
- **Peer delinquency (measure described previously)**
- **Peer school misconduct (measure described previously)**
- **Parental involvement (described previously)**