

Under the Radar or Under Arrest: How Is Adolescent Boys' First Contact With the Juvenile Justice System Related to Future Offending and Arrests?

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This study examined the extent to which being arrested during adolescence was associated with subsequent self-reported offending and court-recorded arrests. We also examined whether the way in which the justice system processed adolescents was related to the nature of these associations. The sample included 532 boys who had been arrested (“justice-system-involved”) and 99 boys who had never been arrested despite engaging in similar illegal behaviors (“no-justice-system-contact”). Data included official arrest records and youths’ self-reported illegal behavior at a baseline interview and a follow-up 6 months later. To reduce group differences at baseline, we calculated matching weights with 2 dozen variables and used these weights in all analyses. Results demonstrated that the groups differed in their rate of change in self-reported offending between the 2 interviews and in their likelihood of being arrested during the study period. The no-justice-system-contact group self-reported the same amount of offending at baseline and the follow up, whereas the justice-system-involved youth who received the most lenient disposition (i.e., sanction and dismiss) decreased their self-reported violent, theft or property, and total offending, and the justice-system-involved youth who received the most punitive disposition (i.e., adjudication) increased their self-reported violent offending. All justice-system-involved youth were more likely to be arrested during the study period than the no-justice-system-contact youth, even after accounting for self-reported offending. Thus, even though some justice system interventions were associated with less subsequent offending, involvement with the juvenile justice system during adolescence, in and of itself, is a significant risk factor for repeated contact with the system.

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are those of the authors and do not necessarily reflect the official position or policies of the funding agencies.

The data used in this article came from the Crossroads study. Data from the Crossroads study have been used in a variety of prior studies to examine different questions related to adolescent development, adolescent delinquency, and involvement with the juvenile justice system. Part of the data in this article was collected as part of the first author’s dissertation project and was presented at the annual meeting of the American Psychology-Law Society in 2015. We thank the undergraduate research assistants who worked on this project and the study participants and their families.

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Public Significance Statement

The findings show that arrested youth are processed in a variety of ways, and the way in which a youth is processed is associated with whether the youth desists or continues to offend in the future. However, because all justice-system youth were more likely than similarly delinquent no-contact youth to be arrested during the study, findings suggest that it is difficult to break away once you are “in” the system—regardless of whether behavior actually improves.

Keywords: juvenile justice system-involved youth, delinquency, recidivism, adolescent offenders

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The proclivity to engage in antisocial and illegal behavior increases sharply between childhood and adolescence and naturally declines thereafter (Gottfredson & Hirschi, 1987; Sampson & Laub, 1993). Although some adolescents who violate the law are arrested, there is a substantial proportion of youth who engage in the same illegal behaviors but are never arrested (Erickson & Empey, 1963; Farrington, Jolliffe, et al., 2003; Gold, 1966). In theory, involvement with the juvenile justice system should be associated with better outcomes because the fundamental tenets of the juvenile justice system are to rehabilitate youth and reduce recidivism (Steinberg & Schwartz, 2000). In reality, however, almost all available empirical evidence suggests that contact with the justice system is actually related to more offending or police contacts (Bernburg & Krohn, 2003; Gatti, Tremblay, & Vitaro, 2009; McAra & McVie, 2007).

One understandable criticism of this line of work is that some empirical studies have presented an “unfair” comparison of arrested and nonarrested youth without comprehensively controlling for selection effects. With this limitation in mind, it is unclear whether the sustained or increased criminal behavior among justice-system-involved youth is because of involvement with the justice system itself or because of the preexisting risk factors that led to the arrest in the first place. We designed the present study to overcome this limitation by specifically recruiting a sample of youth who had a history of engaging in similar illegal behavior but who differed in one important respect: some youth were arrested for their behaviors while others were not. With this sample of arrested and nonarrested youth, we examined the extent to which youths’ first contact with the juvenile justice system was associated with subsequent self-reported illegal behavior and court-recorded official arrests, and whether the type of the justice system involvement was related to the nature of the findings.

Background Research

Many prior studies have found that involvement with the justice system during adolescence is a risk factor for later antisocial behavior, offending, and/or justice system contact (Bernburg & Krohn, 2003; Gatti et al., 2009; McAra & McVie, 2007; Ward, Krohn, & Gibson, 2014). What is unclear, however, is whether these offending outcomes are because of the impact of being arrested or because of differences among juveniles that predate (and influence) their arrest. Based on prior work, we know that adolescents are selected into the justice system based on their existing risk factors (e.g., sociodemographic, antisocial predisposition, low IQ, and impulsivity) and criminal history (e.g., chro-

nicity and severity of offending). In general, arrested youth tend to have a richer history of prior antisocial behavior, higher impulsivity and hyperactivity, greater sensation seeking, more aggression, more substance use, greater affiliation with delinquent peers, more parental conflict, more parental disengagement, and more academic difficulties than nonarrested peers (Farrington, 2009; Farrington, Loeber, & Tfofi, 2012; Gatti et al., 2009; Kirk & Sampson, 2013). Research also shows that being Black or poor increases the odds of being arrested (Beckett, Nyrop, Pflingst, & Bowen, 2005), even after controlling for self-reported prior delinquency (Brownfield, Sorenson, & Thompson, 2001).

Relevant for the present study is that these same risk factors could also influence the type of sanctions (i.e., community service hours vs. placement with other arrested youth) that youth receive from the justice system, in addition to whether youth continue to break the law after their first arrest. Without a statistical correction for these potential selection effects, it is difficult to determine whether involvement with the justice system, above and beyond preexisting risk factors, increases risk for later offending and arrests.

A handful of similar prior studies have attempted to control for possible selection effects with propensity score matching. One study of arrested adolescents found that youth who were processed in court had greater odds of being arrested later in the study than matched adolescents who were diverted (Petitclerc, Gatti, Vitaro, & Tremblay, 2013). Two studies used propensity score matching to compare arrested and nonarrested youth and found that arrested youth had higher rates of subsequent offending and arrests (Lieberman, Kirk, & Kim, 2014; Wiley, Slocum, & Esbensen, 2013). However, none of these studies specifically recruited youth who engaged in the same types of illegal behavior or compared the effect of varying degrees of justice system contact to no intervention at all.

Severity and Type of Justice System Contact

Justice system interventions vary greatly, even within the same jurisdictions. Although justice system decision makers may strive to deliver the same sanctions for youth who break the same laws, many charges can be processed in more than one way (Snyder & Sickmund, 2006). These sanctions range from more formal processing in court to more informal case handling at the local probation department. On the one hand, more punitive interventions, such as court hearings, time in detention, and formal probation, might expose adolescents to more serious offenders and unstable living environments, and cause them to spend time away

from positive influences like prosocial peers, family, and traditional schools. On the other hand, these interventions may also connect adolescents with necessary services (mental health, educational, and substance use), structure, and supervision. More lenient interventions, such as diversion programs, may also connect adolescents with needed services, but without disruptions to their family or school environments and without increasing their exposure to more deviant youth. Because many diversion programs include the possibility of charge dismissal if terms are satisfied, diversion programs may protect youth against long-term damage to their reputation or social standing. Given that different justice system processing styles likely lead to vastly different experiences for arrested youth, the impact of justice system involvement likely depends on the characteristics of the involvement. Some interventions might be associated with improvements in behavior while others may not.

Studies examining varying degrees of justice system contact have typically found that more punitive processing styles and sanctions (e.g., court appearances, supervised probation, and placement) are associated with worse outcomes than less punitive sanctions (e.g., diversion; Bernburg & Krohn, 2003; Gatti et al., 2009; Petittlerc et al., 2013). However, no prior studies have comprehensively controlled for the fact that the youth who receive the most serious and punitive sanctions might have failed previous justice system interventions and/or committed more serious crimes, or differed in other critical respects, and no prior studies have compared different degrees of justice system contact to no contact at all.

Developmental Considerations

It is also unknown whether the age of the adolescent influences the nature of the association between justice system involvement and later behavior. Because of their developmental immaturity, a child may be especially vulnerable to adverse experiences during early adolescence (Paus, 2009; Smetana & Villalobos, 2009; Susman & Dorn, 2009). One study found that being convicted of a crime during early adolescence was associated with more academic difficulties than convictions during later adolescence (De Li, 1999). These findings are intriguing, but the precise reason for this association is not clear. The worse outcomes associated with an earlier conviction could have been because of the differences among individuals that led to an earlier (or later) conviction and not specifically because of the timing of the conviction (i.e., omission of critical confounding variables). Many prior studies have shown that youth who commit crimes at younger ages have more severe dispositional and background risk factors than youth who start showing antisocial behavior later in adolescence (Fairchild, van Goozen, Calder, & Goodyer, 2013; Frick & Viding, 2009; Moffitt, 2018). It is also unclear whether a similar pattern of results would be observed for nonacademic outcomes.

The Present Study

We designed the present study to overcome the limitations of prior work by comparing the behavior and arrest records of adolescents whose illegal behavior came to the attention of law enforcement to the behavior and arrest records of adolescents who self-reported similar delinquent behavior but were never arrested.

In particular, we examined whether justice-system-involved youth self-reported more offending, and were more likely to be arrested, 6 months after their first arrest than no-justice-system-contact youth who had previously engaged in similar illegal behavior but were never arrested. We also examined whether the way in which the justice system processed an adolescent, or the age of the adolescent, was related to the nature of the associations.

We naturally restricted many preexisting differences among justice-system-involved youth by excluding youth with prior arrests and only including youth who had been charged with an offense of moderate severity in a single jurisdiction. This methodology increased the probability that the variations in sanctions were tested within crimes of similar severity and that the sanctions were not be influenced by previous contact with the justice system. In addition, we used a statistical weighting technique to further reduce the influence of preexisting differences and confounding variables.

We hypothesized that the nature of the associations would vary based on how the youth was processed and the age of youth. For example, we hypothesized that more lenient interventions (e.g., diversion) would be associated with lower offending and that more punitive sanctions (e.g., court-ordered probation; adjudication) would be associated with higher offending. We also hypothesized that youth who received no justice system intervention at all (i.e., no justice-system-contact youth) would demonstrate no change in their behavior. Additionally, youth who were processed in the most punitive way would have the highest rearrests rates. We also hypothesized that the magnitude of the effects would be largest among younger adolescents (~13 to 14 years old), and that the size of the effects would gradually diminish across age.

Method

Sample

Justice-system-involved sample. The justice-system-involved sample consisted of 532 adolescent boys enrolled in the California site of the Crossroads Study (<http://sites.uci.edu/crossroadsinfo/>). Crossroads is an ongoing longitudinal study examining the experiences, behaviors, and development of youth involved with the juvenile justice system. See the study website for a list of prior publications with the justice-system involved sample (<https://sites.uci.edu/crossroadsinfo/publications/academic-publications/>). Boys were eligible to participate in the Crossroads study if they had recently been arrested for the first time, were between the ages of 13 and 17 years old ($M = 15.48$ years, $SD = 1.22$), and had committed an eligible offense (e.g., vandalism, theft, assault; see Table 1 in the online supplemental materials). Eligible charges included moderate offenses that had similar probabilities of being diverted and formally processed in court. Charge selection was based on a review of historical records over the 5-year period immediately before the commencement of the study. Crossroads investigators also limited recruitment to Hispanic (74.6%), White (23.1%), and Black (2.3%) adolescent boys (based on court records) who were available for a baseline interview within 6 weeks of receiving their case disposition. Almost all youth (97.2%) committed their eligible offense within 12 months before the baseline interview ($M = 135.44$ days before baseline; $SD = 82.67$; range: 0 to 570 days). Crossroads personnel identified the justice-system involved sample through a collaborative process with the probation

department. From 2011 to 2013, the probation department provided research staff a list of newly arrested juveniles. Research staff used these lists, along with court records, to determine whether each juvenile met the criteria for study inclusion (e.g., eligible charge, within age range, and no prior history of arrests). After project staff identified eligible youth, researchers contacted each youth and their parents via the telephone, e-mail, or a house visit. Research staff obtained all contact information from the probation department and/or publicly accessible information sources. Only 52 youth (6.3%) and 148 parents (17.9%) declined to participate (total contacted $N = 825$). Because of limited or nonworking contact information, project staff was unable to reach 93 potential participants (11.3%) within the timeframe.

No-justice-system-contact sample (i.e., “no-contact”). From November 2012 to April 2014, project staff also recruited a no-contact sample in the California site of the Crossroads study using a variety of strategies (e.g., modified snowball sampling, targeted community sampling). First, project staff invited enrolled participants to nominate same aged peers (13 to 17 years) who might be interested in participating in a “similar” study (also referred to as peer nomination sampling; Schreibeis-Baum et al., 2016). This strategy is very similar to snowball sampling (Goodman, 1961) or chain referral (Penrod, Preston, Cain, & Starks, 2003) and especially useful for identifying populations that might be difficult to locate (Fedina & DeForge, 2017; Shaghagh, Bhopal, & Sheikh, 2011). To protect the privacy of potential no-contact participants, we did not give existing participants any information about study eligibility for the peer (no-contact) sample (the similar study). Nominators provided basic contact information and written permission to contact their friends. Project staff also gave enrolled participants a study flyer that could be distributed to friends or schoolmates. Second, similar to other studies that have recruited demographically similar comparison groups (e.g., Milojevich, Levine, Cathcart, & Quas, 2018), research staff members distributed study flyers throughout the community (e.g., schools, community centers, and coffee shops). The flyer invited interested youth (and their parents) to call the project coordinator for more study details. By using broad and targeted sampling strategies, we increased the probability of finding youth who fit the study eligibility requirements for the no-contact sample.

Research personnel screened all nominated youth for whom we had working contact information and all interested youth who responded to the flyer ($N = 507$). The screener consisted of questions that assessed age, sex, whether the youth ever had contact with law enforcement or been under justice system surveillance (When was the last time you were . . . arrested/on probation/in court [for something other than a traffic violation]; been required by law enforcement to attend a class or program?), and whether youth had recently (i.e., within the last year) engaged in any of the eligible illegal behaviors (e.g., When was the last time you . . . got into a physical fight at school or another public place; attacked someone with the idea of seriously hurting them; vandalized property; did graffiti or engaged in tagging?). We selected eligible behaviors for the no-contact screener to align with the eligible charges for the California site of the Crossroads study (see Table 1 in the online supplemental materials). Youth were eligible for the no-contact sample if they had no prior arrests and if they self-reported engaging in at least one eligible behavior in the past 12 months. The screener identified 124 (24.5%) eligible partici-

pants. Study ineligibility was because of youth identifying as a female (24.1%), youth being outside the eligible age range (16.7%), youth having been arrested in the past (33.7%), youth not self-reporting engagement in any of the eligible behaviors in the time-frame (23.2%), and youth not identifying with any of the included race or ethnicities (2.2%). Of the eligible youth, 100 were enrolled in the study as part of the no-contact sample (80.6%). We used official records to confirm that enrolled no-contact youth had no prior charges officially filed in the county. We excluded one eligible youth because we discovered that he had previously been arrested during the official record review. The remaining nonparticipation among eligible youth ($n = 23$) was because of parent refusal, youth refusal, and inability to schedule the baseline interview during the recruitment period. The final no-contact sample consisted of 99 youth who self-reported committing similar crimes as the justice-system-involved sample but were never arrested ($M = 15.80$ years, $SD = 1.24$; 71.72% Hispanic, 24.24% White, 3.03% Black, 1.01% Other).

Procedures (Both Samples)

Parents and legal guardians provided consent and youth provided assent before all study procedures. After the consent and study orientation process, youth participated in a baseline interview with a research staff member (approximately 2–3 hr). We conducted the interviews at the most convenient location for the participant (e.g., participants’ homes, local coffee shops) on laptop computers with computer-assisted interview software. When necessary, research staff interviewed youth in treatment, residential, or other justice system facilities. Interviewers read questions aloud to minimize comprehension difficulties, and participants had the choice of responding audibly or using a keypad to respond privately. Research staff conducted one follow-up interview (referred to as “the follow up”) approximately 6 months after the baseline interview ($M = 5.99$ months after baseline; $SD = 0.24$ months), which consisted of an interview battery that was similar to the baseline interview. Retention was excellent: 96% ($N = 96$) of no-contact youth and 97.5% ($N = 519$) of justice-system involved youth completed the 6 month follow up interview. The research team utilized many strategies to enhance retention. First, we asked for several pieces of primary contact information at the end each interview. Second, we asked for contact information for several close family members and friends who would know how to contact the youth if we could not find them with the primary contact information. Third, we started trying to locate participants many weeks before the targeted interview date so we had plenty of time to reach the boys before the interview window expired. Fourth, we asked participants to call our laboratory if any contact information changed before we contacted them. Fifth, we adequately compensated research participant for their time. Sixth, we were extremely flexible when scheduling the interviews so were able to meet participants at the day, time, and location that were most convenient for the participant. Lastly, perhaps most importantly, the interviewers treated participants with respect and warmth, which ensured that the interview experience was positive.

We encouraged honest responding from the participants by alerting them to our confidentiality policy, which included a Privacy Certificate from the Department of Justice. The Privacy Certificate protected participants because it legally prohibited the

research staff from disclosing identifiable or incriminating information revealed during the interviews (even if the data were subpoenaed). As discussed with the participants, research staff only broke confidentiality if a boy reported abuse or a serious plan to hurt himself or another person. The University of California Irvine's Institutional Review Board approved all study procedures and materials (Study Name: Crossroads: Formal vs. Informal Processing in the Juvenile Justice System; Protocol Number: HS # 2010-7867).

Measures

We used official court records to divide the justice-system-involved youth into four groups. The four groups were (listed in order of increasing severity): sanction and dismiss ($n = 109$), diversion ($n = 163$), court-ordered probation ($n = 172$), and adjudication ($n = 88$). Both sanction and dismiss and diversion cases fall under the larger category of informal processing, which means they were diverted from court and handled at the probation department. Informally processed youth were supervised by the probation department only. Sanction and dismiss cases had less supervision, fewer sanctions, and shorter terms than diversion programs. For the most part, informally processed youth were off probation as soon as they completed the terms of their probation, which could be completed in as short as 4 to 6 weeks for sanction and dismiss youth (6 months maximum), and 3 to 6 months for diverted youth. In contrast, court-ordered probation and adjudication fall under the larger category of formal processing, in which youth were petitioned and processed through the formal court system. Formally processed youth were typically supervised by both the court and probation in some capacity for 6 to 12 months (with the possibility of even longer sentences). Please see Tables 2 and 3 in the online supplemental materials for descriptive information about initial dispositions for the justice-system-involved groups. Please see Table 4 in the online supplemental materials for information about initial charges for the groups.

Outcome Variables

We used self-reported offending and official arrest records as outcomes because both data sources have strengths and weaknesses and using both captures complementary dimensions of criminal behavior (Thornberry & Krohn, 2000).

Self-report of offending. We measured self-reported illegal behavior with a revised version of the Self-Report of Offending scale (SRO; Huizinga, Esbensen, & Weiher, 1991). At both interviews, youth reported whether they had engaged in 24 different illegal behaviors in the past 6 months. With these items, we created four variables for each time-point: a total offending score that indicated the number of different behaviors the youth engaged in during the recall period (max 24); a binary violent offending variable (10 items; e.g., carjacking, rape, robbery or armed robbery, fighting, assault, gang violence; 0 = no; 1 = yes); a binary theft or property offending variable (9 items; e.g., vandalism, arson, breaking and entering, stolen goods, auto theft; 0 = no; 1 = yes); and a binary drug dealing offending variable (2 items; sold marijuana, sold other illicit drugs; 0 = no; 1 = yes). We used a total offending variety score, rather than a frequency score, because variety scores are less vulnerable to recall bias than fre-

quency scores (Osgood, McMorris, & Potenza, 2002), variety scores are sensitive to criminal behavior heterogeneity (Sweeten, 2012), and variety scores are highly correlated with measures of seriousness and frequency of antisocial behavior (Monahan & Piquero, 2009).

Official (re-)arrests. We used official court and probation records to determine whether youth were (re-)arrested between baseline and the follow up. We only included new charges in the rearrest outcome; we did not include probation or technical violations (e.g., failure to attend school or services). We created a binary variable indexing whether participants were arrested at least once because very few participants were arrested more than once during the six month follow up period (~7%). We also created binary variables to indicate whether youth were arrested for at least one person or violent offense (e.g., assault, battery, robbery), theft or property offense (e.g., burglary, petty theft, vandalism), or drug offense (e.g., possession of a controlled substance, drug dealing).

Matching Variables

All matching variables were measured at the baseline interview.

Demographics. Youth self-reported their date of birth, race/ethnicity (coded: 0 = non-White; 1 = White), whether their biological parents were still married (0 = no; 1 = yes), and a proxy for socioeconomic status (10-point scale measuring the highest education of participants' mother or father [whoever was highest], ranging from 1 = grade school to 10 = professional or graduate degree).

Intelligence quotient (IQ). We measured IQ with the vocabulary and matrix reasoning subscales of the Wechsler Abbreviated Scale of Intelligence (Wechsler, 1999).

Psychosocial maturity. We used the total score (30 items) from the Psychosocial Maturity Index to measure psychosocial maturity (self-reliance, identity, and work orientation; Greenberger & Sørensen, 1974). Higher scores were indicative of greater psychosocial maturity ($\alpha = .88$).

Callous-unemotional traits. We used the 24 items from the Inventory of Callous-Unemotional traits to measure callous-unemotional traits (e.g., limited empathy and guilt; Kimonis et al., 2008). Higher scores were indicative of more callous-unemotional traits ($\alpha = .77$).

Impulse control. We used the Impulse Control scale from the Weinberger Adjustment Inventory to measure impulse control (eight items; Weinberger & Schwartz, 1990). Higher scores were indicative of more impulse control ($\alpha = .75$).

Life-time variety of offending. We used the SRO to measure lifetime offending (Huizinga et al., 1991). At baseline, youth reported whether they had ever engaged in the 24 different illegal behaviors described previously (0 = no; 1 = yes). We calculated a variety score for lifetime offending by summing the 24 binary items. Higher scores indicated greater lifetime offending ($\alpha = .82$).

Substance use. We assessed substance use with an adapted version of the Substance Use and Abuse Inventory (Chassin, Rogosch, & Barrera, 1991). Youth stated the frequency with which they used tobacco, alcohol, marijuana, and other illicit drugs (e.g., ecstasy, cocaine) in the previous 6 months using an 8-point scale, ranging from 0 (*not at all*) to 7 (*everyday*). Because of low

base-rates, we used a binary indicator of any other (nonmarijuana) drug use instead of the 8-point scale.

Parent criminal behavior. We used a modified version of the Association with Deviant Peers scale to measure parent criminal behavior (Thornberry, Lizotte, Krohn, Farnworth, & Jang, 1994). Items asked participants to state whether either parent had engaged in 13 illegal behaviors in the past 6 months (e.g., stolen something worth more than \$100, carried a gun, been in a fight). See Table 5 in the online supplemental materials for more information about this scale. Higher scores were indicative of more parent criminal behavior ($\alpha = .66$).

Parental involvement. We measured parental involvement (e.g., whether the parent knows where his or her adolescent child goes at night, whether parents know who the adolescent spends time with) with an adapted version of the Parental Monitoring Inventory (Steinberg, Lamborn, Dornbusch, & Darling, 1992). Higher scores were indicative of more parental involvement ($\alpha = .84$).

Peer delinquency. We used 13 items from the Association with Deviant Peers scale to measure Peer delinquency (Thornberry et al., 1994). Items asked youth to state the proportion of friends that had engaged in 13 illegal behaviors over the previous 6 months (same behaviors used to assess parent criminal behavior; e.g., carried a gun, been in a fight). See Table 5 in the online supplemental material for more information about this scale. Higher scores were indicative of greater affiliation with delinquent peers ($\alpha = .92$).

Proportion of friends arrested. Youth stated whether any of their best friends (up to 5) had ever been arrested. The total number of friends arrested was divided by the total number of friends identified, creating a variable that indexed the proportion of best friends that had been arrested.

Grades. Youth self-reported their grades in school using an 8-point scale: 1 = *mostly below Ds*; 2 = *mostly Ds*; 3 = *about half Cs and half Ds*; 4 = *mostly Cs*; 5 = *about half Bs and half Cs*; 6 = *mostly Bs*; 7 = *about half As and half Bs*; 8 = *mostly As*.

Extracurricular activity engagement. Youth stated whether they were involved in any extracurricular activities (e.g., sports, school clubs, school government, and music/band; 0 = not enrolled in any; 1 = enrolled in at least one).

School disciplinary practices (suspensions and expulsion). Youth self-reported the number of times that they had been suspended from school in the past 6 months, and whether they had ever been expelled (0 = never; 1 = expelled at least once).

School truancy. We used five items to measure school truancy (e.g., late for school, cut or skipped school, and in trouble for missing too many days; Cernkovich & Giordano, 1992; Eccles, Wigfield, & Schiefele, 1998). Using a 5-point scale (0 = *never*; 1 = *1–2 times*; 3 = *3–6 times*; 7 = *7–9 times*; 10 = *10 or more times*), boys reported the frequency with which each item occurred over the past 6 months. Higher scores were indicative of more school truancy ($\alpha = .70$).

Neighborhood disadvantage. We used Geographic Information System (GIS) to link each participant's home address to 2010 Census Tracts. Census tract-level data was then linked with data from the American Community Survey, 2007–2011. Consistent with prior work (Chung & Steinberg, 2006; Fagan, 2008; Mulvey et al., 2010), we created a latent disadvantage variable with the following indicator variables: proportion of people at or below

poverty in the housing tract, proportion of single person households in the housing tract, proportion of unemployed persons in the tract, and proportion of households on public assistance in the tract. Next, we estimated factor scores and then saved these values as a new variable. Higher scores were indicative of more disadvantageous neighborhoods.

Neighborhood impressions. We measured the physical and social characteristics (e.g., empty beer bottles on the streets) of the participants' neighborhoods with 21 self-reported items (Sampson, 1997; Sampson & Raudenbush, 1999; Sampson, Raudenbush, & Earls, 1997). Higher scores were indicative of poorer neighborhoods ($\alpha = .93$).

Plan of Analysis

The goal of the present study was to examine whether youths' first official encounter with the juvenile justice system was associated with subsequent offending and arrests. We weighted the data with two dozen matching variables (see Measures section for descriptions of matching variables) to reduce the impact of selection effects. The inclusion of weights increased our ability to statistically isolate the effects of justice system processing, because the weights help to minimize the preexisting differences among groups. The weighting strategy utilized in the present study is similar to propensity score matching, an analytic strategy used in prior studies with arrested and nonarrested youth and recommended by others (see Apel & Sweeten, 2010; Kirk & Sampson, 2013; Liberman et al., 2014; Ward et al., 2014), but appropriate when comparing more than two groups (McCaffrey et al., 2013).

To accommodate the five groups, which included four justice-system-involved groups and one no-contact group, data were matched and weighted by the inverse probability of group membership, conditioned on the matching variables (McCaffrey et al., 2013). To calculate these weights, binary indicators of the five groups (no-contact, sanction and dismiss, diversion, court-ordered probation, and adjudication) were regressed on the 24 matching variables in five conditional binary logistic regressions. Next, we calculated each participant's predicted probability and inverse probability (1/predicted probability) of belonging to each group. Last, we created a final weighting variable in which each participant was weighted by the inverse probability of belonging to the group that he was assigned. We truncated the weighting variable at the 99th percentile (7 scores were truncated).

Using the matching weights generated in the previous step, we examined group differences at the baseline and follow up interviews. We controlled for baseline values of the outcome variable when examining group differences at the follow up (essentially estimating the change in the outcome variable). Group contrasts were only examined and reported if the omnibus test of group differences (main effect) was significant. We chose the type of regression models based on distributional properties of the outcome variables. We used negative binomial regressions for total offending and logistic regressions for theft or property offending, violent offending, drug dealing, and official arrests. In addition, we used population-average generalized estimating equations (GEE) to directly examine and plot within-group change between interviews. After the primary analysis, we also examined whether any of the associations varied by participants' age at baseline. We tested age interactions by repeating the previously described anal-

ysis and including the main effect of age and a product term between age and a nominal variable representing the five groups. We used robust *SEs* and conducted all analyses in Stata 14 (StataCorp, 2015).

Sensitivity analyses. To supplement the primary analyses, we examined treatment effects of justice system processing in three additional models. First, we examined the main effect of justice system involvement on the outcomes using multivariate models with imputed data sets (imputation model described below). In these models, we regressed the outcome variables on the 24 matching variables, baseline values of the outcomes, and the nominal variable representing the five groups. When the main effect of justice system involvement was significant, we rotated the groups to obtain all possible contrasts. Consistent with primary models, we chose the type of regression model based on distributional properties of each outcome variable. We also examined treatment effects of justice system involvement using T-effects with robust *SEs* and the inverse probability weighted regression adjustment (command in Stata). Using T-effects, we regressed the outcome variables on baseline values of the same variable, and included the 24 matching variable in the matching component of the analysis. We used the no-contact group as the control or comparison group. The precise treatment model was dependent on the distribution of the outcome variable (i.e., poisson model for total offending; logit models for theft or property offending, violent offending, and drug dealing). We would have used a negative binomial model for total offending (similar to the primary analysis); however, negative binomial models are not currently supported in T-effects (StataCorp, 2015). Finally, as an additional supplemental analysis, we compared the no-contact youth to a combined group of all justice-system-involved youth.

Missing data. All participants had complete data on initial arrest status (arrested or not) and processing type (sanction and dismiss, diversion, court-ordered probation, and adjudication). Approximately 89% of participants ($n = 561$) had complete data on the 24 matching variables. The adjudicated group was more likely than the other groups to be missing data on at least one matching

variable (odds ratios [ORs] from 2.07 to 3.40; $p < .05$). Because maximum likelihood estimation (default for logistic regressions) eliminates cases with missing data on the predictor variables (in our case, the matching variables), we imputed 50 data sets with chained equations (Stata's MICE procedure) to ensure that the 70 participants with missing data on the matching variables could be included. We only imputed missing data on the matching variables; we did not impute data on the outcome variables. As described earlier, follow up retention rates were excellent. Over 96% of participants in each group had follow up data. There were no group differences in likelihood of having missing data on any of the outcome variables.

Results

Factors Associated With Justice System Involvement

Matching variables. Differences between the justice-system-involved groups and no-contact youth on the 24 matching variables are presented in bivariate regressions in Table 6 of the online supplemental materials (using the raw data). As shown in this table, the five groups differed on seven matching variables before weighting. Compared with the four justice-system-involved groups, the no-contact youth had higher IQs, higher parent criminal behavior, better grades in school, were more likely to be involved in extracurricular activities, had fewer school suspensions, and were less likely to have been expelled from school. However, when the weights were included, there were no group differences on any of the matching variables. See online supplemental materials (Table 6) for more information.

Total self-reported offending. The main effect of justice-system-involvement for total offending was not significant at baseline or the 6 month follow up (see Table 1), although the sanction and dismiss group significantly decreased in total offending between the interviews (see Figure 1).

Self-reported theft or property offending. Although the main effect of justice system processing for theft or property

Table 1
Differences in Offending Among Justice System Processing Groups and No-Contact Youth

Outcome variable	Main effect of group differences $\chi^2(p)$	No-contact ($n = 99$)	Sanction and dismiss ($n = 109$)	Diversion ($n = 163$)	Court-ordered probation ($n = 172$)	Adjudicated ($n = 88$)
		Estimated value	Estimated value	Estimated value	Estimated value	Estimated value
Total offending						
Baseline	2.31 (.678)	1.77	1.60	1.77	1.70	1.33
Follow up	6.52 (.164)	1.85	1.43	1.78	2.12	2.33
Theft or property offending						
Baseline	3.79 (.435)	.54	.51	.49	.46	.38
Follow up	11.39 (.023)	.42 ^b	.23 ^a	.33 ^{ab}	.42 ^b	.39 ^b
Violent offending						
Baseline	4.67 (.323)	.44	.47	.46	.39	.33
Follow up	11.48 (.022)	.37 ^{ab}	.30 ^a	.36 ^a	.38 ^a	.53 ^b
Drug dealing						
Baseline	2.61 (.626)	.23	.14	.14	.17	.14
Follow up	1.12 (.890)	.16	.17	.16	.17	.12

Note. Analytic sample sizes: Baseline: $N = 631$; follow up: $N = 613$. Negative binomial models used to estimate total offending. Binary logistic regression models used to estimate theft or property offending, violent offending, and drug dealing. Weighted by inverse probability of belonging to the group that the youth was assigned (conditioned on all matching variables). Group contrasts only estimated when main effect of justice system involvement was significant ($p < .05$). Estimated values in the same row with the same superscript letters are not significantly different from each other.

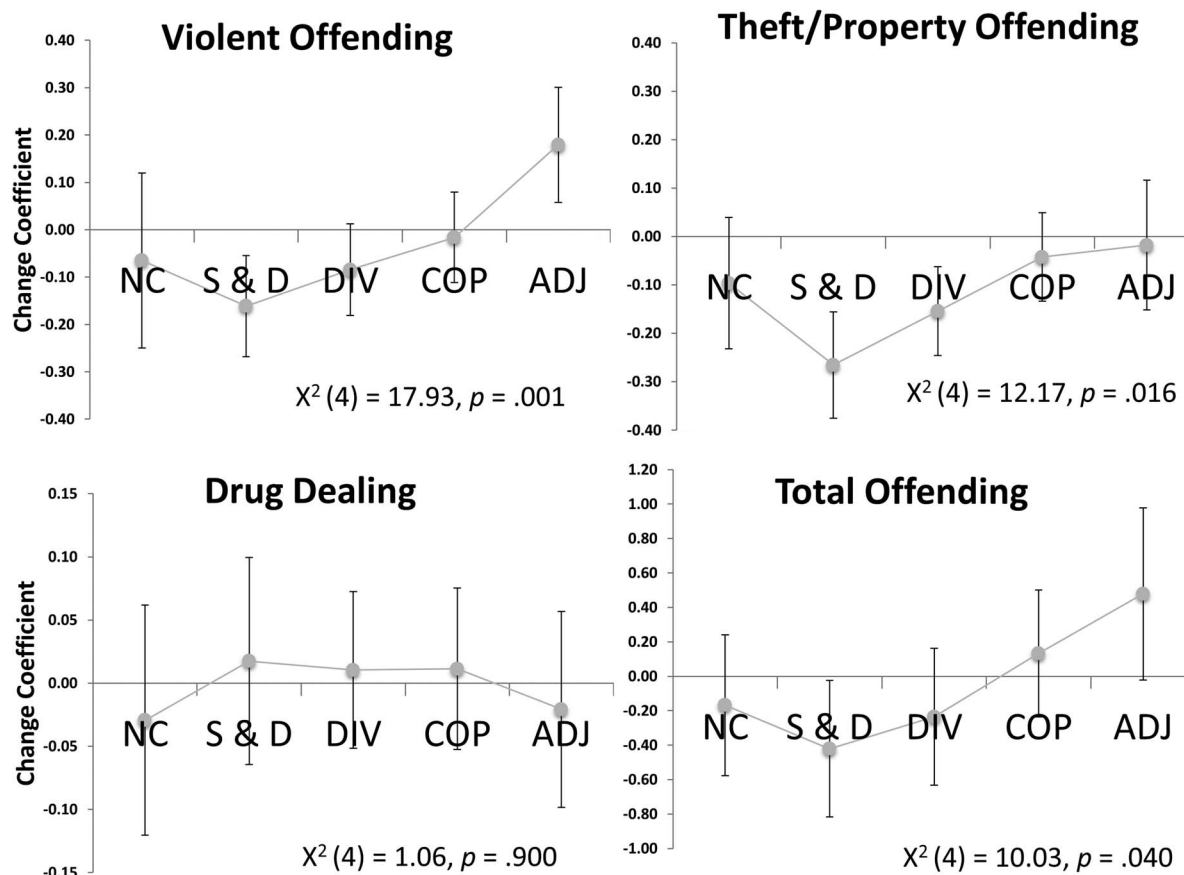


Figure 1. Figure 1 displays the rate of change between the baseline and the follow up interview for each of the offending outcomes by processing group. Values obtained from generalized estimating equations. A value of 0 indicates no change between the two interviews. Values less than 0 indicate a decrease between the two interviews. Values greater than 0 indicate an increase between the two interviews. Error bars crossing the X-axis (at 0) indicate that the coefficient is not significantly different than 0. The χ^2 represents the main effect of group change. NC = No-contact; S & D = Sanction and Dismiss; DIV = Diversion; COP = Court-Ordered Probation; ADJ = Adjudication.

offending was not significant at the baseline interview, processing group differences were observed at the 6 month follow up (see Table 1). Specifically, the sanction and dismiss group was less likely to engage in theft or property offending than the no-contact, court-ordered probation, and adjudication groups at the follow up (see Table 1), representing small-medium effects (ORs from 1.8 to 3.2; Chen, Cohen, & Chen, 2010). Indeed both the sanction and dismiss and diversion groups significantly decreased their theft or property offending between the interviews (see Figure 1). The no-contact youth did not significantly differ from the diversion, court-ordered probation, or adjudication group at the follow up.

Self-reported violent offending. There were no group differences in violent offending at baseline (see Table 1). However, group differences were present at the follow up (see Table 1). Specifically, adjudicated youth were significantly higher in violent offending than court-ordered probation, diversion, and sanction and dismiss youth (see Table 1) at the follow up, representing small-medium effects (OR from 2.0 to 3.1; Chen et al., 2010). The sanction and dismiss group significantly decreased their violent

offending between the interviews while the adjudicated group significantly increased their violent offending between the interviews (see Figure 1).

Self-reported drug dealing. There were no group differences in drug dealing at the baseline or the follow up interview (see Table 1; Figure 1).

(Re-)arrest during the follow-up. Despite self-reporting similar offending as the justice-system-involved youth, none of the no-contact youth were arrested during the study period. However, approximately 14% of sanction and dismiss youth ($n = 15$), approximately 19% of diversion youth ($n = 31$), approximately 28% of court-ordered probation youth ($n = 48$), and approximately 23% of adjudicated youth ($n = 20$) were rearrested before the 6 month follow up interview. Because none of the no-contact youth were arrested, we used a firths logistic regression with penalized maximum likelihood estimation (Firth, 1993; Heinze & Schemper, 2002). These models are designed to accommodate outcomes with low base rates and complete separation. Firths logistic regression (at least in Stata 14) does not allow noninteger probability weights, so we could not include the matching weights in this analysis.

Table 2
Differences in Court Records of Arrests Among Justice System Processing Groups and No-Contact Youth in Multivariate Models (N = 614)

Variable	Any arrest			Any arrest, controlling for concurrent self-reported offending		
	Coef.	95% CI	<i>p</i>	Coef.	95% CI	<i>p</i>
Justice system processing group^a						
Sanction and dismiss	3.23	[.38, 6.08]	.027	3.29	[.42, 6.15]	.025
Diversion	3.43	[.61, 6.26]	.017	3.47	[.63, 6.31]	.017
Court-ordered probation	4.02	[1.21, 6.84]	.005	4.09	[1.26, 6.91]	.005
Adjudicated	3.53	[.69, 6.38]	.015	3.60	[.74, 6.46]	.014
Baseline weighting variables						
Race	.37	[-.35, 1.10]	.312	.46	[-.27, 1.19]	.220
Age	-.18	[-.39, .03]	.097	-.16	[-.37, .06]	.160
IQ	-.02	[-.04, .00]	.099	-.02	[-.04, .01]	.132
Psychosocial maturity	-.21	[-.86, .44]	.523	-.25	[-.91, .40]	.448
Callous-unemotional	.00	[-.04, .03]	.823	-.01	[-.05, .03]	.585
Impulse control	-.25	[-.58, .07]	.123	-.24	[-.57, .09]	.147
Lifetime offending	.08	[-.02, .18]	.111	.05	[-.05, .16]	.313
Marijuana use	.07	[-.03, .17]	.184	.06	[-.04, .16]	.260
Alcohol use	.12	[-.01, .25]	.060	.11	[-.02, .25]	.085
Tobacco use	.09	[-.01, .20]	.077	.11	[.01, .22]	.038
Other illicit drug use	.19	[-.42, .79]	.544	.13	[-.48, .73]	.679
Parent education	-.14	[-.27, -.02]	.026	-.15	[-.28, -.02]	.019
Parent criminal behavior	-.11	[-.245, 2.22]	.923	.06	[-.233, 2.44]	.963
Parental involvement	-.39	[-.78, .00]	.051	-.39	[-.78, .01]	.055
Bio parents married	-.06	[-.56, .44]	.804	-.09	[-.59, .42]	.740
Peer delinquency	-.35	[-.83, .13]	.159	-.46	[-.95, .03]	.067
Proportion of friends arrested	.09	[-.69, .87]	.828	.17	[-.62, .96]	.675
Grades	-.03	[-.17, .12]	.718	-.05	[-.19, .10]	.539
Extracurricular activities	-.12	[-.60, .36]	.630	-.12	[-.61, .36]	.618
Suspensions	.01	[-.08, .09]	.851	.00	[-.09, .09]	.967
Expulsion	.34	[-.15, .83]	.170	.33	[-.16, .82]	.186
School truancy	-.09	[-.23, .05]	.214	-.08	[-.22, .07]	.295
Neighborhood disadvantage	-.11	[-.38, .16]	.435	-.13	[-.40, .15]	.368
Neighborhood impressions	.30	[-.18, .77]	.219	.30	[-.18, .79]	.220
Self-report of offending at follow up						
Theft or property				.22	[-.31, .75]	.409
Violence				.45	[-.06, .96]	.085
Drug dealing				.20	[-.47, .87]	.557

Note. Coef. = β coefficient; 95% CI = 95% confidence interval. Models estimated with firths logistic regressions and imputed data.

^a Compared with no-contact.

Accordingly, we included all of the matching variables with the imputed data sets as independent variables. Results indicated that all of the processing groups were significantly more likely to be rearrested during the study period than the no-contact youth (see Table 2), representing fairly large effects (*ORs* > 5; Chen et al., 2010). Additionally, the court-ordered probation group was significantly more likely to be rearrested than the sanction and dismiss and the diversion groups ($t = 2.23, p = .025$; $t = 2.03, p = .042$, respectively), representing small-medium effects (*ORs* from 1.8 to 2.2; Chen et al., 2010). None of the significant group differences in rearrests became nonsignificant if we additionally controlled for concurrent theft or property offending, violent offending, and drug dealing (see Table 2). Base rates among the groups were too small to analyze the arrest charge categories, but see Table 3 for descriptive information regarding the type of rearrests.

Age interactions. None of the age interactions were significant, indicating that the pattern of results was similar for youth of different ages (all *p* values > .10). See Table 7 in the online

supplemental materials for more information about the age by processing group interactions.

Sensitivity Analyses

Results generated in the multivariate models were similar to the results generated in the primary analysis (see Table 4). The sanction and dismiss group was less likely to engage in theft or property offending at the follow up than the no-contact (see Table 4), diversion ($p = .045$, not presented in table), court-ordered probation ($p = .012$, not presented in table), and adjudication ($p = .025$, not presented in table) groups. Additionally, treatment effects from the T-effects analyses are presented in Table 8 in online supplemental materials, and results were also similar to the results generated in the primary analysis. For example, compared with the no-contact group, the adjudicated group was significantly higher on violent offending at the follow up. Finally, when the justice system youth were combined and compared with the no-contact

Table 3
 Descriptive Statistics for Arrests During the Study by Justice System Processing Group

Processing group	N ^a	Number of people arrested during study	Arrested for violent/person offense	Arrested for property or theft offense	Arrested for drug-related offense	Arrested for other offense ^b	Any probation violation ^c
		N (% within group)	N (% within group)	N (% within group)	N (% within group)	N (% within group)	N (% within group)
Sanction and dismiss	106	15 (14.15%)	2 (1.89%)	11 (10.38%)	1 (.94%)	4 (3.77%)	0 (.00%)
Diversion	162	31 (19.14%)	7 (4.32%)	16 (9.88%)	8 (4.94%)	7 (4.32%)	1 (.62%)
Court-ordered probation	171	48 (28.07%)	9 (5.26%)	23 (13.45%)	11 (6.43%)	19 (11.11%)	7 (4.09%)
Adjudicated	88	20 (22.73%)	8 (9.09%)	11 (12.50%)	1 (1.14%)	4 (4.55%)	12 (13.64%)

Note. Arrest variables include new charges and do not include probation violations. Youth could have more than one arrest between baseline and follow up interview (categories are not mutually exclusive).

^a Represents the number of adolescents within group with complete official record data. ^b Other offenses include charges such as disorderly conduct, carrying a weapon, and driving under the influence. ^c Probation violations include violations that were officially petitioned in court.

youth, the only difference was that the justice system group was significantly higher on violent offending at the follow up (see Table 9 in online supplemental materials).

Discussion

Although prior studies have found that justice system involvement in adolescence is associated with continued or increased offending (e.g., Gatti et al., 2009), most prior studies have not comprehensively controlled for selection effects. We designed the present study to overcome this limitation by specifically recruiting a sample of youth who engaged in similar illegal behaviors; however, only some youth were arrested. To further reduce the possibility of findings being influenced by selection effects and preexisting differences among groups, the present study utilized a specialized weighting technique with two dozen matching variables.

We found that the juvenile justice system in the site of the present study used a variety strategies to process adolescents who were recently arrested for the first time. Our data showed that the way in which an adolescent's first arrest was handled was predictive of his subsequent self-reported illegal behavior and court-recorded arrests. Youth who experienced the most lenient justice system processing (sanction and dismiss) reported less violent, theft or property, and total offending at the follow up compared with baseline, though youth who received the most severe processing (adjudication) reported higher violent offending at the follow up than the baseline. In contrast, the no-contact group reported similar offending at baseline and the follow up (see Figure 1). These findings suggest that more lenient and informal justice system interventions may be associated with behavioral improvements in some domains, but the positive effects might diminish, and eventually reverse, as processing becomes more punitive. This is somewhat consistent with prior studies that have found that youth who undergo the most punitive justice system processing have the highest rearrest and subsequent offending rates (Gatti et al., 2009; Petittler et al., 2013; Ward et al., 2014). It was also notable that the outcomes associated with justice system processing were similar for youth of different ages, suggesting that adjudication was associated with worse outcomes for 13- through 17-year-old adolescent boys and that sanction and dismiss was associated with positive outcomes for boys across this age range.

The results in the present study appeared to follow a linear pattern, with the best outcomes for youth who received the lightest sanctions

and the worst outcomes for youth who received the harshest treatment (see Figure 1). One possibility is that the "gentle warning" and minimal justice system involvement in the sanction and dismiss group facilitated behavioral improvement. The jurisdiction in the present study also may have embraced a therapeutic approach for the sanction and dismiss group, as a meta-analysis of juvenile justice interventions found that programs that emphasize therapeutic goals are more effective at reducing recidivism than programs that emphasize supervision and control (Lipsey, 2009). Unfortunately, we do not have specific details about the nature of the monitoring or the probation officer relationship within the sanction and dismiss group. However, we know that the justice system assigned a variety of sanctions to the sanction and dismiss group. For example, probation officers asked sanction and dismiss youth to write an apology letter to victims and/or parents, write an essay on an assigned topic (e.g., sports, lessons learned from case, "why I should not steal," "why it is wrong to destroy property"), volunteer in the community, pay a relatively small restitution to victims (<\$350), and/or participate in specialized legal awareness anger management, substance abuse, or counseling programs.

In contrast, adjudicated youth may have had higher violent offending as a result of different processes and developmental phenomena. First, structural changes in their life, such as disruptions in school or day-to-day life, or exposure to unsafe settings, could have influenced their propensity to engage in violence (Beck & Rantala, 2016). Second, adjudicated youth may have suffered reputational damage (i.e., labeling) as a result of official court involvement (Becker, 2003). Stereotypes and public labeling might reduce prosocial and socially normative opportunities (Matsueda, 1992), which could force adjudicated youth into using nonconventional tactics, such as violence and other criminal behavior, to survive. Insofar as people are motivated to behave in ways that are consistent with others' expectations of them (Harter, Stocker, & Robinson, 1996; Rosenthal & Jacobson, 1968), adjudication and court involvement also may have caused youth to develop self-delinquent identities and/or caused them to act in ways consistent with delinquent stereotypes. Additionally, peers are highly influential during adolescence (Brown & Larson, 2009; Steinberg & Monahan, 2007) and the type of peers with whom adolescents affiliate is a strong correlate of youths' own behavior and attitudes (Brechwald & Prinstein, 2011; Potat, 2007). Adjudicated youth may have increased their violent offending because

Table 4
Differences in Offending Among Justice-System-Involved Youth and No-Contact Youth in Multivariate Regression Models (N = 614)

Variable	Total offending			Theft or property offending			Violent offending			Drug dealing offending		
	Coef.	95% CI	p	Coef.	95% CI	p	Coef.	95% CI	p	Coef.	95% CI	p
Justice system processing group ^a												
Sanction and dismiss	-.33	[-.70, .03]	.074	-1.26	[-2.01, -.51]	.001	-.02	[-.72, .69]	.966	-.45	[-1.46, .57]	.390
Diversion	-.15	[-.48, .18]	.362	-.57	[-1.23, .08]	.084	.28	[-.36, .92]	.395	-.46	[-1.38, .45]	.320
Court-ordered probation	.04	[-.28, .36]	.806	-.41	[-1.05, .24]	.219	.25	[-.39, .90]	.444	-.30	[-1.17, .58]	.504
Adjudicated	-.08	[-.45, .29]	.671	-.38	[-1.13, .36]	.315	.68	[-.06, 1.42]	.071	-.69	[-1.76, .38]	.208
Race	.02	[-.27, .31]	.895	-.12	[-.71, .48]	.698	-.21	[-.79, .37]	.474	.20	[-.58, .97]	.619
Age	-.08	[-.17, .01]	.090	-.16	[-.34, .02]	.081	-.24	[-.41, -.07]	.005	.07	[-.21, .34]	.620
IQ	.00	[-.01, .01]	.640	.00	[-.02, .02]	.814	-.02	[-.03, .00]	.071	.01	[-.01, .04]	.331
Psychosocial maturity	-.02	[-.30, .26]	.903	.01	[-.59, .60]	.982	.00	[-.55, .54]	.988	-.11	[-.97, .74]	.794
Callous-unemotional	.03	[.02, .05]	<.001	.04	[.01, .07]	.016	.00	[-.03, .03]	.821	.07	[.03, .12]	.002
Impulse control	-.03	[-.17, .11]	.645	.00	[-.28, .28]	.993	-.09	[-.36, .17]	.490	.16	[-.25, .58]	.439
Lifetime offending	.11	[.06, .17]	<.001	.20	[.10, .31]	.000	.12	[.02, .22]	.017	.20	[.07, .33]	.003
Marijuana use	.04	[.00, .08]	.052	.02	[-.06, .11]	.596	-.06	[-.14, .03]	.193	.14	[.03, .26]	.017
Alcohol use	.06	[.00, .11]	.042	.08	[-.04, .20]	.171	.04	[-.07, .16]	.459	.00	[-.15, .16]	.978
Tobacco use	-.04	[-.09, .00]	.055	-.07	[-.16, .03]	.188	-.05	[-.14, .05]	.327	-.02	[-.13, .10]	.796
Other illicit drug use	.19	[-.06, .43]	.137	.25	[-.30, .80]	.372	-.01	[-.56, .53]	.958	.43	[-.22, 1.09]	.193
Parent education	.03	[-.02, .08]	.224	.03	[-.07, .14]	.499	.04	[-.06, .13]	.476	.08	[-.07, .22]	.308
Parent criminal behavior	-.21	[-1.20, .77]	.671	-1.10	[-3.27, 1.06]	.318	-.90	[-2.98, 1.18]	.398	-.57	[-3.22, 2.08]	.675
Parental involvement	-.11	[-.28, .05]	.183	-.20	[-.54, .14]	.251	-.37	[-.70, -.05]	.024	-.20	[-.67, .27]	.403
Bio parents married	.02	[-.23, .19]	.850	-.07	[-.50, .36]	.750	-.03	[-.44, .37]	.869	-.07	[-.68, .55]	.835
Peer delinquency	-.28	[-.08, .48]	.006	.56	[.11, 1.01]	.015	.57	[.13, 1.00]	.011	.53	[-.01, 1.07]	.055
Proportion of friends arrested	-.06	[-.41, .29]	.728	-.10	[-.84, .65]	.801	.41	[-.29, 1.12]	.248	.11	[-.82, 1.04]	.816
Grades	.03	[-.03, .09]	.269	-.03	[-.16, .10]	.674	.03	[-.09, .15]	.648	-.07	[-.24, .10]	.402
Extracurricular activities	-.04	[-.26, .17]	.687	-.25	[-.69, .18]	.257	.14	[-.27, .56]	.503	-.05	[-.65, .55]	.875
Suspensions	.02	[-.02, .07]	.280	.04	[-.05, .12]	.371	.01	[-.07, .10]	.776	.01	[-.13, .14]	.939
Expulsion	.07	[-.16, .29]	.569	-.04	[-.52, .44]	.871	.18	[-.26, .63]	.426	.49	[-.15, 1.13]	.132
School truancy	-.02	[-.08, .03]	.431	-.05	[-.17, .07]	.435	.03	[-.09, .14]	.671	-.14	[-.31, .03]	.102
Neighborhood disadvantage	.03	[-.09, .15]	.588	.06	[-.18, .30]	.606	.12	[-.11, .34]	.308	.15	[-.20, .49]	.409
Neighborhood impressions	-.11	[-.31, .10]	.305	-.26	[-.69, .17]	.233	-.17	[-.58, .23]	.395	-.42	[-1.02, .18]	.169
Baseline value of outcome	.01	[-.05, .07]	.656	.73	[.29, 1.17]	.001	.85	[.44, 1.26]	<.001	.79	[.06, 1.51]	.034

Note. Coef. = β coefficient; 95% CI = 95% confidence interval. Negative binomial regressions used to estimate total offending. Binary logistic regressions used to estimate theft or property offending, violent offending, and drug dealing offending. Models estimated with imputed data on matching variables.

^a Compared with no-contact.

of greater exposure to delinquent peers (Dishion, McCord, & Poulin, 1999; Dishion, Spracklen, Andrews, & Patterson, 1996). Our data do not address which specific features of justice system processing may make youth more—or less—likely to reoffend in the future, but our data clearly support the need for such research.

It was interesting that the middle groups, diversion and court-ordered probation, were not significantly different from each other or the no-contact group at the follow up (although the diversion group reported lower theft or property offending at the follow up than they reported at the baseline, while these other groups did not demonstrate a significant change between the interviews). The similarities between the diversion and court-ordered probation could be because of the similar justice system strategies used to monitor these groups. The primary difference between the two groups was that the court was in charge of monitoring the progress of the court-ordered probation group, while the probation department was in charge of monitoring the diverted group. However, both groups were given community supervision with an assigned probation officer and received sanctions such as community service hours, attending school, random drug tests, avoiding certain people (e.g., cooffenders, witnesses, and victims), substance use programs, counseling, anger management, and curfews. One difference between the groups was that the court-ordered probation

group was typically given longer probation terms (3 to 48 months, with most receiving a 12 month sentence) than the diverted group (2 to 6 months, with most receiving about a 6 month sentence). Additionally, many of the diverted youth were given a writing assignment (49%), while very few (1%) of the court-ordered probation group were given one. In contrast, a larger proportion of the court-ordered probation group was required to pay a restitution fee (92% of court-ordered probation youth compared with 60% of diverted youth), and in cases when the diverted group was given a restitution payment requirement, it was typically a much smaller amount (<\$350 for diverted group; up to \$6,600 for court-ordered probation group). Finally, while a handful of court-ordered probation youth were committed for a few days, none of the diverted youth were committed.

Although the magnitude and direction of the results for the self-reported outcomes were different across justice system groups, all justice-system-involved youth were more likely to be arrested during the study period than the similarly delinquent no-contact youth. It was interesting that there was a slightly linear pattern in the prevalence of rearrests rates among the justice system groups. A larger percent of court-ordered probation and adjudicated youth were arrested (28 and 23%, respectively) than diverted youth (19%), and all of these groups had higher preva-

lence rates than the sanction and dismiss group (14%). None of the no-contact youth were arrested during the study period. These high rates of rearrests are consistent with general recidivism statistics in the state of the present study. For example, the California Department of Corrections and Rehabilitation found that 74.2% of youth released from the Division of Juvenile Justice were rearrested within 3 years of being released (California Department of Corrections and Rehabilitation, 2017). The differences between the justice-system-involved groups and the no-contact group in the present study are particularly striking considering that youth in the justice-system-involved groups generally did not self-report more offending than the no-contact youth at the follow up.

These findings are consistent with the idea that youth who received more punitive processing would be subjected to more intense and more frequent contact by their probation officers, and the intensity and frequency of probation officer contact could be related to the timing of the re-arrest. Indeed it is possible that being under justice system surveillance increases the risk of arrest simply because those under surveillance are more likely to have their illegal behavior detected and reported (Bechtold, Monahan, Wakefield, & Cauffman, 2015; Matthews, Krivelyova, Stephens, & Bilchik, 2013; Ryan, Abrams, & Huang, 2014). For example, one study of youth on probation found that the best predictor of time until first probation violation was the number of times that the probation officer had contacted that youth or his family (Bechtold et al., 2015).

It is also important to consider that diverted, court-ordered probation, and adjudicated youth likely had probation officers who monitored their progress at school, which means school personnel may have been aware of their legal status. As such, school administrators might have been more likely to report youth who were known to have an arrest record. Similarly, it is possible that neighbors, peers' parents, and other community members who were aware of an adolescent's prior encounters with law enforcement were more likely to report his illegal behavior (or perceive his behavior as being worthy of legal involvement) than an adolescent who has never been arrested. The present study did not examine the mechanisms responsible for the higher arrest rates among the justice-system-involved sample, but this would be an important area for future research.

Although the present study demonstrated that results for the four justice-system-involved groups were similar in a variety of analytic models, it was interesting that the supplemental analyses combining the justice system groups revealed that important group differences were masked when processing heterogeneity was not taken into consideration. If these were the only analyses in the present study, the conclusions would have been misleading, and the positive outcomes associated with the sanction and dismiss group would have been lost. This is important for future researchers to consider when designing similar studies and conducting similar analyses or when drawing conclusions about the effectiveness of the juvenile justice system.

Despite the many similarities among the groups, several group differences were observed at the baseline interview before matching weights were included. Compared with all other groups, the no-contact youth had higher IQs, received better grades in school, were more likely to be involved in extracurricular activities, had fewer school suspensions, and were less likely to have been

expelled from school. This is consistent with other studies that have found academic difficulties among arrested adolescents (e.g., Kirk & Sampson, 2013; Liberman et al., 2014; Wiley et al., 2013). One surprising difference between the no-contact youth and the justice-system youth was in regard to parent criminal behavior. In our sample, the no-contact youth reported more parent criminal behavior than the justice system groups. There are many potential explanations for this finding. First, parents of no-contact youth may actually have more prolific criminal histories than parents of justice system youth. Perhaps the enhanced criminal sophistication among the no-contact youths' mothers and fathers allowed these parents to teach their children how to avoid law enforcement detection. It is important to keep in mind that no-contact youth were intended to represent (to some extent) the population of adolescents who broke the law but got away with it. Another potential explanation is that no-contact youth were more comfortable disclosing parental criminal engagement or that the justice-system-involved-youth were less comfortable revealing parental illegal behavior. Finally, the differences in parental criminal behavior could be a function of sampling—perhaps the parents who were willing to allow their arrested youth to participate in the study were less likely to have criminal histories. Although we (unfortunately) have no way of knowing the true reason for the observed finding, it is worth noting that the magnitude of the differences was fairly small. Nonetheless, the difference among the groups reinforces the importance of using matching weights in addition to the targeted recruitment strategy.

Limitations and Directions for Future Research

It is important to consider the findings in the context of the study limitations. First, the results in our study are correlational and not necessarily indicative of causal pathways. It is also important to emphasize that the analysis focused on community-based supervision practices, as very few participants spent time in placement or other secure facilities. Next, the results were specific to one jurisdiction in California. Future research should examine whether the same patterns are evident in other jurisdictions. Although a justice-system-involved sample was recruited in the other two Crossroads sites, a no-contact sample was only recruited in the California site.

Additionally, the present study only obtained data on two measurement occasions that were 6 months apart. It is unknown whether the findings will be sustained long-term, particularly long after probation sentences have been completed. Furthermore, the findings only generalize to boys who were arrested for moderately severe, but not victimless, crimes, and boys who had no prior encounters with the juvenile justice system. It is unlikely that these findings would generalize to adolescents who commit serious offenses (e.g., murder), minor status offenses (e.g., truancy), or youth with prior justice-system processing experiences. It is also important to keep in mind that many of the participants in the no-contact sample were nominated by (and, thus, peers of) the arrested sample, which limits the extent to which the no-contact sample represents the population of nonarrested youth in general. Nonetheless, the no-contact sample was intended to represent youth who could have been arrested but were not; thus, providing a proxy for a counterfactual outcome for the arrested youth.

Furthermore, because none of the no-contact youth were arrested during the study period, we had to use a specific regression model that could handle the complete separation of the outcome variable. Although we were unable to use the matching weights in this analysis, it is unlikely that doing so would have changed the results. Additionally, we used two dozen matching variables in the present study; however, there are other important matching variables to consider for future studies (e.g., adolescent physical maturity, mental health diagnoses, and attitudes about delinquency). Moreover, we used self-report of offending for some of the outcomes, which is subject to recall errors and reporting bias. However, an individual's true antisocial behavior is likely more correlated with self-report of offending than with official arrest records because a number of external factors (e.g., whether the behavior is detected or reported, whether charges are filed) influence whether an illegal act will lead to an arrest (Maxfield, Weiler, & Widom, 2000). We also used variety scores in addition to binary indicators of specific illegal behavior to minimize recall issues (Hindelang, Hirschi, & Weis, 1981; Osgood et al., 2002). It is easier for youth to remember *if* they attacked someone with a weapon or *if* they set property on fire in the last 6 months, than it is to remember *how many times* they attacked someone with a weapon or *how many times* they set property on fire during this period.

All things considered, the present study offers many avenues for future investigations. Future research should dedicate time and resources to gaining a comprehensive understanding of the specific features of different justice system processing experiences, programs, and interventions. Additionally, future research should examine whether the findings in the present study are sustained long-term and whether similar patterns are observed for nonoffending outcomes (e.g., school performance or academic attainment, psychological health, and substance use). Future research should also examine whether there are certain subgroups of adolescents for whom the outcomes associated with justice system processing are worse (or better). We examined age as a moderator but there are many other potential moderators worth exploring (e.g., race/ethnicity, parental criminal behavior and justice system experience parental involvement). We also did not explore the mechanisms linking justice system involvement with future behavior. Future research should examine potential mediators that explain why contact with the justice system may be associated with subsequent behavioral change and sustained legal contact (e.g., peer delinquency, probation officer monitoring, labeling, or stigmatization). It is also important to keep in mind that the present study only included boys, and future research should examine these issues with adolescent girls.

Developmental Considerations

In many countries, antisocial risk-taking (e.g., vandalism, theft, and fighting) is normative among teenagers and generally peaks in mid-to-late adolescence (Duell et al., 2018). Indeed, convergent evidence from a variety of data sources has shown that adolescents are more likely to engage in risky, dangerous, and antisocial behavior than children or adults (e.g., Blum & Nelson-Mmari, 2004; Steinberg, 2008), and self-report surveys indicate that many adolescents have done something illegal in their lifetime (Farrington, 2009). Despite this peak in antisocial risk taking in adolescence, the overwhelming majority of teens who break the law

will naturally desist from antisocial behavior before adulthood (Eisner, 2002; Hirschi & Gottfredson, 1983; Moffitt, 2018). Empirical research consistently shows that only a small percent (5–10%) of adolescents continue to offend beyond their early 20s (Moffitt, 2018; Monahan, Steinberg, Cauffman, & Mulvey, 2009, 2013). Given that the vast majority of adolescent delinquency is transient and exploratory, punitive justice system interventions may be unnecessary and ineffective at best, and dangerous at worst, for the majority of first-time arrested youth who commit moderately serious crimes.

Conclusion

The present study compared a sample of youth who were recently arrested for the first time to a sample of adolescents who engaged in similar illegal behaviors but were never arrested. Results suggested that more lenient justice system interventions for youth with no prior history of arrests might deter offending in the short-term, while a more punitive processing approach might actually exacerbate it. However, even after accounting for self-reported offending, all justice-system-involved youth were more likely to be rearrested than similarly delinquent youth who were never under justice system surveillance. This finding of increased risk for arrest for all processed youth is critical because repeated or sustained involvement with the justice system may lead to increasingly severe sanctions (i.e., extended stays in placement or secure facilities), which could undermine any potential positive impact of initial justice system interventions. This issue becomes especially salient as youth reach the age of majority and transition into the adult criminal system, where arrest records are public and permanent. Taken together, the data in the present study suggest that the default policy should be to keep the justice system's involvement to a minimum for first-time arrested youth who commit crimes of moderate severity.

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