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Preventing School-Based Arrest and Recidivism Through Prearrest Diversion: Outcomes of the Philadelphia Police School Diversion Program

Naomi E. S. Goldstein, Rena Kreimer, Siying Guo, TuQuynh Le, Lindsey M. Cole, Amanda NeMoyer, Stephanie Burke, George Kikuchi, Kevin Thomas, Fengqing Zhang

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Preventing School-Based Arrest and Recidivism Through Pre-Arrest Diversion: Outcomes of the Philadelphia Police School Diversion Program

Abstract

Objectives: Created to combat the school-to-prison pipeline, the Philadelphia Police School Diversion Program offers voluntary community-based services to eligible youth accused of minor school-based offenses in lieu of arrest. This study evaluated Program effectiveness in accomplishing goals related to reductions in school-based arrests, serious behavioral incidents, and recidivism.

Hypotheses: We expected the annual number of school-based arrests in Philadelphia schools to decrease over the Program's first five years and predicted that the annual number of serious behavioral incidents would not increase. Further, we expected that diverted youth—compared to youth arrested in schools the year before Program implementation—would have significantly lower rates of recidivism arrests in the two years following their school-based incidents.

Method: Using a quasi-experimental design, we examined data from 2,302 public school students (67.0% male; 76.1% Black; age range: 10–22 years) who were either diverted from arrest through the Diversion Program or arrested in Philadelphia schools in the year prior to Diversion Program implementation. We compared rate of recidivism arrest, number of arrests, and time to arrest between diverted and arrested youth. We also used district-wide descriptive statistics to examine five-year trends in school-based arrests and serious behavioral incidents.

Results: Since Program implementation, the annual number of school-based arrests in Philadelphia has declined by 84% and the number of serious behavioral incidents has declined by 34%. Diverted youth demonstrated less recidivism than arrested youth in the two years following their initial incident; however, after propensity score matching, we no longer observed these differences.

Conclusions: Findings indicate that a pre-arrest diversion program can safely reduce school-based arrests and suggest a need for future research regarding the role of demographic and incident-related characteristics in recidivism outcomes.

Keywords: juvenile justice, diversion, pre-arrest diversion, school-to-prison pipeline, school policing, intervention program, juvenile arrest, recidivism

Public Significance Statement: Reductions in school-based arrest with no subsequent increase in recidivism following implementation of the Philadelphia Police School Diversion Program provide evidence that pre-arrest diversion programs can play a major role in safely dismantling the school-to-prison pipeline and may encourage replication of the program in other jurisdictions.

Preventing School-Based Arrest and Recidivism Through Pre-Arrest Diversion: Outcomes of the Philadelphia Police School Diversion Program

Zero-tolerance policies and harsh disciplinary practices in school districts across the United States have contributed to the development of a *school-to-prison pipeline,* with schools serving as a primary referral source to the juvenile justice system (Teske, 2011). The increasing presence of police officers within schools appears to further the flow of students into this pipeline, as school-based law enforcement has been linked to higher rates of exclusionary discipline (e.g., suspension, expulsion) and arrest of students (Fisher & Hennessy, 2016; Petteruti, 2011). Importantly, school-based police referrals and arrests often serve as responses to minor infractions (e.g., marijuana possession, disruptions, disorderly conduct)—*not* responses to students endangering school safety (Bahena et al., 2012; Whitaker et al., 2019). This criminalization of normative adolescent misbehavior (Reyes, 2006; Theriot, 2009) disproportionately affects students of color (Losen & Skiba, 2010) and, by increasing rates of contact with police and/or arrest, it contributes to many negative short- and long-term consequences for youth (e.g., Bernburg & Krohn, 2003; Kirk & Sampson, 2013; Wiley & Esbensen, 2016).

Further, although these police practices were designed to improve school safety (Counts et al., 2018), research suggests they often fail to fulfill their protective goals and, in some cases, students feel *less* safe after their implementation (e.g., Lacoe, 2015; Theriot, 2013). Consequently, recent reform efforts have sought to alter the role of school-based law enforcement and reduce the number of students arrested for misbehavior. One such effort, the Philadelphia Police School Diversion Program (“Diversion Program”), implemented citywide in 2014, was designed to substantially reduce the number of school-based arrests while maintaining

school safety (Goldstein et al., 2019). This paper presents results of a Diversion Program evaluation to assess accomplishment of these objectives.

School-Based Law Enforcement and Arrests

Although the specific justification for police presence in schools has changed over time to align with public concerns (e.g., student drug use, school violence), the past several decades have seen a marked and consistent rise in the use of school-based law enforcement officers (Ryan et al., 2018), especially in high schools, urban schools, and schools with at least 1,000 students (James & McCallion, 2013). Across all U.S. public schools, one security or law enforcement personnel exists for every 580 students (Gray & Lewis, 2015). Notably, as the number of school-based police officers increases in a given school, so does the likelihood that the school will refer traditional disciplinary problems to law enforcement and that students will be arrested for their misbehavior, even when those behaviors are relatively minor (Brady et al., 2007; Hirschfield, 2008).

During the 2015–2016 academic year, U.S. schools referred more than 290,600 students to law enforcement, resulting in more than 52,000 arrests for school-related incidents (U.S. Department of Education, 2018; U.S. Department of Education, 2019). Importantly, although overall youth arrests have largely decreased in recent years (Hockenberry & Puzzanchera, 2020), the proportion of law enforcement referrals originating from schools has increased (e.g., Puzzanchera & Hockenberry, 2013; Hockenberry & Puzzanchera, 2020). Critical analysis of these data indicates troubling racial and ethnic disparities in referral and arrest rates, given similar rates of misbehavior across racial and ethnic groups (Finn & Servoss, 2014; Gregory et al., 2010). Although Black students comprise just 15.4% of the total public school population, they account for 30.1% of students referred to law enforcement and 35.0% of students subjected

to school-based arrest (U.S. Department of Education, 2018). Black male students appear particularly impacted by this disparity, as they make up just 7.9% of the public school population but experience 12.8% of school-based arrests, compared to White male students, who comprise 25.2% of public school students but only experience 8.0% of school-based arrests (U.S. Department of Education, 2018).

Police contact and arrest have been linked to negative outcomes for youth, including diminished school engagement, poorer academic performance, reduced participation in prosocial activities, exclusion from prosocial peers, and greater association with delinquent peers—all of which, together, contribute to heightened risk for future delinquency and arrest (Liberman et al., 2014; Wiley et al., 2013; Wiley & Esbensen, 2016). In addition to the negative effects on individual youth, arresting students for minor infractions and removing them from the school environment does not appear to improve school safety—the primary objective of school-based law enforcement initiatives. Instead, the presence of school-based security or law enforcement officers has often failed to demonstrate a relationship with students' feelings of safety and, worse, has been linked to increased levels of fear among students (Bachman et al., 2011; Lacoe, 2015; Schreck & Miller, 2003).

Diversion Programs

In response to growing recognition of the negative consequences associated with juvenile justice involvement, programs that seek to keep young people out of the system have grown increasingly prevalent (Wilson et al., 2018; Wilson & Hoge, 2013). Most of these programs focus on diverting youth *after* arrest, a strategy that can be useful to prevent later stage juvenile justice involvement but which fails to spare youth from the often-traumatic experience of arrest and from other collateral consequences of arrest, including a record that can carry into adulthood

(ACLU of Florida, n.d.; Tallon et al., 2018). In contrast, *pre-arrest* diversion programs—sometimes referred to as "true diversion" programs (Wilson & Hoge, 2013 p. 498)—serve to prevent and protect youth from the negative effects and collateral consequences of arrest, detainment, processing, and court proceedings that occur before post-arrest diversion programs typically begin (Kirk & Sampson, 2013; Liberman et al., 2014; Tallon et al., 2018).

Pre-arrest diversion programs are typically police driven. During an initial police contact, when an officer might otherwise arrest a youth, instead, the officer warns, releases, or refers the eligible youth to a diversion program, thereby avoiding arrest and, often, formal justice system processing (Wilson & Hoge, 2013). The specific components and structure of pre-arrest diversion initiatives vary widely. For example, in some jurisdictions, youth receive simple warnings with no further follow up, whereas locations with more formal diversion programs might require youth to complete certain tasks (e.g., community service, an in-person or online course, substance use counseling) and undergo a period of supervision to ensure adherence. In these formal programs, successful completion usually finalizes the diversion expectations, and no further justice involvement occurs. In contrast, failure to comply with program requirements can result in a voiding of the diversion opportunity, resulting in charges and potential adjudication (Models for Change Juvenile Diversion Workgroup, 2011).

Despite their different structures, pre-arrest diversion programs typically share common goals, including preventing youths' entry into the juvenile justice system, providing at-risk youth with needed services, reducing system costs, and helping youth avoid the negative consequences associated with justice system involvement—perhaps, most importantly, an increased risk for recidivism (Mears et al., 2016; Models for Change Juvenile Diversion Workgroup, 2011). Proponents of labeling theory assert that diversion programs ensure youth are not deemed

"delinquent," which can often cause stigma, limit access to beneficial opportunities, and create expectations of further misbehavior, thereby increasing youths' risk for recidivism (Schwalbe et al., 2012; Wilson & Hoge, 2013). Additionally, during formal system processing and supervision, youth frequently encounter other justice-involved youth from whom they might learn deviant behaviors and attitudes; therefore, diverting youth can help reduce this likelihood (Hoge, 2016; Wilson & Hoge, 2013).

Determining whether diversion programs, as a general approach, meet their stated goals can be difficult because of variability in program characteristics and implementation, including youth eligibility criteria (Mears et al., 2016). This challenge seems to contribute to equivocal meta-analytic findings (e.g., Lipsey, 2009; Schwalbe et al., 2012; Wilson & Hoge, 2013). Rather than provide support for all forms of diversion for youth of all risk levels, meta-analyses have indicated that certain types of programming (e.g., caution and warning alone, family-based treatment) have been linked to reduced recidivism and that, for low-risk youth, pre-arrest diversion efforts are more effective than post-arrest diversion programs (Farrell et al., 2018; Wilson & Hoge, 2013). Such support has facilitated the increasing use of diversion initiatives to address low-level offense behavior among youth; however, more rigorous research must examine the outcomes of such initiatives (Mears et al., 2016). The current study seeks to address this need by evaluating the extent to which one large-scale program—the Philadelphia Police School Diversion Program—meets its primary objectives.

Current Study: Philadelphia Police School Diversion Program

The Philadelphia Police Department (PPD) initiated the Philadelphia Police School Diversion Program in 2014, after an internal review of police records revealed that local schools were one of the city's largest source of referrals to police for youth arrests (K. Bethel, personal

communication, October 24, 2019) and that the School District of Philadelphia's (SDP) student arrest rate was three to 25 times higher than the rate of most other Pennsylvania school districts (Youth United for Change & Advancement Project, 2011). Aimed at dismantling the school-to-prison pipeline within the city, the Diversion Program prohibits police from arresting students with no delinquency history who have been accused of specified minor offenses (e.g., disorderly conduct, marijuana possession) on school property and, instead, facilitates their referral to voluntary, community-based prevention services (Goldstein et al., 2019).

Specifically, when a school safety officer or other school staff member reports an incident to the PPD, school police officers respond and call the Diversion Program Intake Center to determine whether the student in question meets Diversion Program eligibility criteria based on age (i.e., at least 10 years old), lack of previous adjudications or open cases, and incident type. All eligible youth avoid arrest and remain in school throughout this process—they are never taken to a police station or intake center. Diverted youth and their families subsequently receive a home visit from a Philadelphia Department of Human Services (DHS) social worker who assesses social service needs and refers youth and/or families to voluntary, free services (e.g., academic support, mentoring, drug and alcohol counseling) through existing programs funded by DHS (Goldstein et al., 2019). Importantly, youth are automatically diverted based on objective criteria and have no further obligations as part of the Diversion Program—even youth who decline service participation are neither arrested nor otherwise sanctioned. These components of the Diversion Program were designed to prevent net widening, so that diverted youth and their families are not subjected to justice involvement simply by nature of their treatment needs and potential disinterest in service participation (Mears et al., 2016). However, once youth have been enrolled in the Diversion Program, they cannot be diverted from school-

based arrest through this program again. See Goldstein and colleagues (2019) for a full program description.

Creators of the Philadelphia Police School Diversion Program sought to substantially reduce school-based arrest rates city-wide (i.e., to reduce the number of school-based arrests by 50% within three years of program implementation) while maintaining school safety (i.e., no increase in the number of serious behavioral incidents in Philadelphia schools). They also sought to enhance youth outcomes and community safety by reducing recidivism. By preventing initial entry into the juvenile justice system, the program creators expected to maintain adolescents' normal developmental trajectories and reduce their risk of future arrest. Therefore, the current study sought to evaluate the Diversion Program's effectiveness in 1) reducing the number of arrests in Philadelphia schools, 2) reducing or maintaining—but not increasing—the number of serious behavioral incidents in Philadelphia schools, and 3) reducing recidivism rates of diverted youth relative to comparable youth arrested in Philadelphia schools in the year prior to implementation of the Diversion Program.

Method

Participants

The PPD provided electronic, deidentified archival data from 2,302 public school students (67.0% male) who were either diverted through the Diversion Program since its inception (*diverted youth sample*) or arrested in Philadelphia schools in the year prior to Diversion Program implementation (*arrested youth sample*). Additionally, the SDP provided school enrollment data for all students attending Philadelphia schools. School enrollment data were missing for 38 students, and therefore, we excluded these youth from our analyses.

Diverted Youth Sample

Although the Diversion Program has been in place for five years, we limited our diverted sample to those youth diverted within the program's first three years so that we could examine two years of recidivism data. Thus, the diverted youth sample (n = 1,281) included all youth enrolled in the Diversion Program between September 2014 and June 2017. Police automatically enrolled youth if they committed one of several designated summary or low-level misdemeanor delinquent acts (i.e., non-firearm weapon possession, marijuana possession, disorderly conduct, trespassing, or vandalism) on school grounds, were at least 10 years of age (the minimum age of arrest in Pennsylvania), and had no previous or pending delinquency adjudications identified during a police record check. Diverted students (69.2% male) were between 10 and 22 years of age (M = 14.82, SD = 2.17) at the time of their school-based incident and diversion. Data indicated most youth were Black (73.9%), followed by Hispanic (14.4%), White (9.4%), and other race/ethnicity (2.3%). Throughout the three school years examined in this study, police diverted youth for incidents at 212 Philadelphia schools (including 168 SDP public schools and 30 public charter schools), diverting between 1 and 56 students (M = 5.41, SD = 8.85) through the program at each school.

Arrested Youth Sample

The arrested youth sample (n = 1,021) included all SDP-affiliated students arrested for school-based incidents in the academic year preceding Diversion Program implementation (i.e., the 2013-2014 school year). Arrested students (65.7% male) were between 10 and 19 years of age (M = 15.21, SD = 1.97) at the time of their school-based arrest. Data indicated most youth were Black (78.9%), followed by Hispanic (10.5%), White (5.3%), and other race/ethnicity (5.3%). During the examined academic year, police arrested youth for incidents at 144

Philadelphia schools (including 111 SDP public schools and 21 public charter schools), arresting between 1 and 77 students (M = 6.83, SD = 11.72) per school.

Quasi-Control Sample

To more accurately compare outcomes between arrested youth and diverted youth, we created a quasi-control sample from youth in the arrested sample who would have been eligible for the Diversion Program if it had been available at the time of their school-based arrest. Thus, other than experiencing a school-based arrest instead of diversion, all criteria for the quasi-control sample mirrored those of youth included in the diverted sample (i.e., incident type, age, and absence of previous delinquency finding or pending adjudication). The quasi-control sample consisted of 531 students (61.8% male) who were between 10 and 19 years of age (M = 15.50, SD = 1.82) at the time of their school-based arrest ("initial arrest"). Data indicated most youth were Black (76.7%), followed by Hispanic (10.9%), White (7.7%), and other race/ethnicity (4.7%). During the examined academic year, police arrested youth in the quasi-control group for incidents at 101 Philadelphia schools (including 78 SDP public schools and 15 public charter schools), arresting between 1 and 52 students (M = 5.14, SD = 8.25) per school. To reiterate, these students were arrested for school-based offenses in the year prior to Diversion Program implementation but would have been eligible for diversion if the program had existed at the time.

Comparison to School District of Philadelphia Student Population

Demographic characteristics of Philadelphia school students have largely remained consistent over the course of the Diversion Program's operation. Specifically, the district is fairly evenly split between male (51.8%) and female (48.2%) students (SDP District Performance Office, 2020). Black students comprise the largest racial/ethnic group, representing approximately 51.6% of the student body, whereas Hispanic/Latinx students and White students

represent approximately 19.4% and 13.9% of the student body, respectively (SDP District Performance Office, 2020). In comparison, our samples are largely male (67.0%), and Black students represent a much larger proportion (i.e., more than 75%). Such differences parallel observed disparities between youth with and without histories of police contact and/or arrest, with the former group including a disproportionate number of young Black men (e.g., Crutchfield et al., 2012; Puzzanchera, 2020).

Procedures and Measures

PPD staff routinely collected data related to the Diversion Program's objectives at the individual, school, and district levels as part of a longitudinal program evaluation. More specifically, in accordance with data licensing agreements between the PPD and the research institution, a designated PPD liaison prepared and transferred deidentified youth data to the research team via encrypted files at scheduled intervals. PPD officials extracted all data for long-term youth outcome measures directly from agency records, thereby eliminating issues of participant attrition or response bias. Study procedures adhered to Drexel University's Institutional Review Board policies.

School-Based Data

The PPD Diversion Program liaison provided annual rates of school-based arrests in the city of Philadelphia after obtaining them from the SDP Office of School Safety, as well as annual rates of serious behavioral incidents reported in SDP schools. Serious behavioral incidents represent all student-related incidents (e.g., theft, vandalism, assault) reported by individual public schools to the school district and recorded by the SDP's Office of School Safety. Some, but not all, of these serious behavioral incidents were reported to the PPD in accordance with an SPD-PPD memorandum of understanding. Finally, the SDP provided information about the out-

of-school suspensions, expulsions, and disciplinary transfers that occurred in public schools each academic year from 2013-2014 to 2017-2018.

Youth Demographic and Incident Data

Since the inception of the Diversion Program, PPD staff have maintained a database of all youth enrolled in the program, including their demographic information (i.e., age at initial incident, race/ethnicity, gender, school attended), date of diversion, and the primary (i.e., most serious) type of offense committed (referred to throughout this manuscript as "incident type"). The PPD Office of Research and Analysis extracted data from this database and transmitted a deidentified version to the researchers.

For the arrested youth sample, the PPD and SDP worked together to provide the researchers with a deidentified list of all students arrested at Philadelphia schools in the 2013-2014 school year that could be tracked for purposes of identifying recidivism arrests. Then, to determine which youth would comprise the quasi-control sample (i.e., arrested youth who would have met eligibility criteria for the Diversion Program if it had existed at the time), the PPD worked with the First Judicial District of Pennsylvania to identify students in the full arrested sample who had neither a prior adjudication nor a pending case at the time of the school-based arrest. The PPD then shared with the research team the demographic data (i.e., age, race/ethnicity, gender, school attended) and arrest information (i.e., arrest date, incident type) contained in records of these quasi-control sample youth.

In this study, demographic data for all youth included: gender (male, female); race/ethnicity (White, Black, Hispanic, other); age at arrest or diversion; and school attended. Of note, rather than creating two variables to represent youths' race and ethnicity, PPD recordkeeping procedures combine these demographic factors into one variable. Additionally,

the race/ethnicity data originally provided by the PPD included several additional categories (i.e., White, Black, Hispanic, American Indian/Alaskan Native, Asian, Multiracial, unknown, and other); however, due to very low frequencies, we combined all groups other than White, Black, and Hispanic into a single "other race/ethnicity" category.

For both diverted and arrested youth, PPD provided Uniform Crime Reporting (UCR) codes for the incident that led to police referral; the PPD also identified the three broader categories (non-firearm weapon possession, marijuana possession, or other) into which each UCR code should be classified. For youth in the diverted and quasi-control samples, other incidents included disorderly conduct, vandalism, and trespassing. For youth in the full arrested sample, other incidents included those three types of offenses plus several additional offenses (e.g., offenses against persons, property offenses, firearm offenses, non-marijuana drug offenses). Before conducting analyses, we dichotomized primary incident type (i.e., non-firearms weapon possession and marijuana possession/other) for simplicity of presentation because marijuana possession and other offense types demonstrated similar patterns in our primary outcomes.

Youth Recidivism Arrest

For the purposes of this study, we defined recidivism as any arrest following enrollment in the Diversion Program (for the diverted youth sample) or any *additional* arrest following the relevant school-based arrest (for the arrested youth sample and quasi-control sample). To determine whether examined youth incurred a subsequent arrest, the PPD Office of Research and Analysis identified, on an annual basis, *all* arrests—whether incurred as a juvenile or adult—for all youth examined in this study. The office then provided the research team with the date of each arrest. Based on the date of arrest, research staff calculated time to recidivism (i.e., days

between school-based incident and recidivism arrest). The current analyses examined two-year recidivism data for all youth.

Method of Analysis

We calculated descriptive statistics of baseline characteristics separately for youth in the diverted, arrested, and quasi-control samples. We then examined differences between the diverted youth sample and each of the other two samples' baseline characteristics using two-sample independent *t*-tests for continuous variables and chi-square tests for categorical variables. Additional analyses were then conducted in steps, as described in the next several paragraphs.

To identify differences in recidivism from baseline to post-Diversion Program implementation, we first used chi-square tests to compare binary recidivism outcomes (i.e., was the youth arrested, no/yes) between our full arrested youth sample and our diverted youth sample at 3, 6, 9, 12, 15, 18, 21, and 24 months post-diversion or school-based arrest. Of note, we chose to examine recidivism at 3-month time points throughout our 2-year follow up period to provide ongoing, time-specific information about patterns of reoffending to identify when youth may be at highest risk of recidivism. Such information could inform the development and implementation of time-specific recidivism prevention efforts for youth diverted through the program. We then used Tweedie compound Poisson general linear models to examine sample differences in number of arrests at each of the same nine time points. We selected this model to account for exact zeros in the number of arrests (Zhang, 2013). We also conducted a Cox proportional-hazards regression to determine whether we observed differences in time to recidivism between the diverted youth sample and the full arrested youth sample. We then performed the same series of analyses to compare the diverted youth sample with the quasi-control sample, as we created this subsample to be more similar to the diverted youth sample

with regard to initiating incidents and previous justice involvement. These sets of analyses will generate meaningful information for system stakeholders inquiring about outcome differences between arrested and diverted youth in their broadest categorizations and also provide useful context for subsequent analyses that use statistical methods to create more equivalent samples for comparison.

Next, to more closely approximate the effect of diverting youth in lieu of arrest on recidivism outcomes, we utilized propensity score matching techniques. These techniques statistically generated matched samples of diverted youth and youth from the quasi-control sample that were largely equivalent across relevant covariates, including age at initial incident, gender (male, female), race/ethnicity (White, Black, Hispanic/Latino, other), and incident type (non-firearm weapon possession, marijuana possession/other). Propensity score matching can be used to reduce potential bias from otherwise unobserved group differences (i.e., confounding) in nonexperimental studies, thereby providing more precise estimates of an independent variable's effects (D'Agostino, 1998). Researchers regularly use this strategy for matching and comparing youth with and without justice involvement (e.g., Beardslee et al., 2019; Kirk & Sampson, 2013; Ward et al., 2014). In the current study, we utilized an R program that matched cases based on a 1:1 nearest neighbor ratio (Ho et al., 2007; R Core Team, 2019). After generating the matched samples, we again conducted the series of analyses previously described. We first used chi-square tests to compare binary recidivism outcomes between samples, then we conducted Tweedie compound Poisson general linear model to examine sample differences in number of arrests at each time point, and finally, conducted a Cox proportional-hazards regression to determine whether we observed sample differences in time to recidivism.

Results

Descriptive Data

When the Philadelphia Police School Diversion Program began in May 2014, city police implemented the program simultaneously across all Philadelphia schools, including the 214 public schools in the SDP (serving nearly 132,000 students). In the prior school year (i.e., 2013-2014), 1,580 arrests occurred across all Philadelphia schools. During the first year of the Diversion Program, 724 school-based arrests took place, representing a 54% reduction. The annual number of school-based arrests declined annually thereafter, with a 68% reduction from year one to three and an 84% reduction from year one to year five (2018-2019, the last year for which data are currently available). See Figure 1, which displays school-based arrest rates by year.

Importantly, as school-based arrests declined, so did the number of serious behavioral incidents within SDP schools. Specifically, 6,359 annual behavioral incidents occurred in the 2013-2014 school year; this number decreased to 5,615 in the Diversion Program's third year (a 12% reduction from pre-Diversion Program numbers) and further decreased to 4,192 in the 2018-2019 school year (a 34% total reduction from the 2013-2014 school year). Similarly, although 25,262 total exclusionary discipline incidents (i.e., out-of-school suspensions, expulsions, and disciplinary transfers) occurred for public school students 10 years of age or older in the 2013-2014 school year, this total decreased to 19,006 incidents in the Diversion Program's third year (a 25% reduction from pre-Diversion Program numbers) and further decreased to 14,919 incidents in the 2017-2018 school year (the last year for which such data were available)—a 41% reduction from the 2013-2014 school year.

In total, police diverted 2,036 students through the Philadelphia Police School Diversion Program in the program's first five years, with 1,281 diverted in the first three years, the period

examined in this study's analyses. Within this three-year diverted sample, youth were most often diverted for non-firearm weapon possession (48.5%), followed by marijuana possession (37.9%), and then "other" incident types (13.6%). In contrast, youth in the full arrested sample were most often arrested for other offenses (69.7%), followed by non-firearm weapon possession (16.4%), and marijuana possession (13.9%). In the quasi-control sample, youth were most commonly arrested for "other" offenses (45.5%), followed by non-firearm weapon possession (29.8%), and then marijuana possession (24.7%). The diverted and arrested samples differed significantly by age, $t(2300) = 4.53$, $p < .001$, $d = 0.19$, 95% CI [0.11, 0.27], race/ethnicity, $\chi^2(3) = 36.99$, $p < .001$, $d = 0.28$, 95% CI [0.19, 0.37], and incident type, $\chi^2(2) = 719.87$, $p < .001$, $d = 2.02$, 95% CI [1.87, 2.17]. The two samples did not differ significantly by gender, $\chi^2(1) = 2.92$, $p = .087$, $d = 0.07$, 95% CI [-0.01, 0.15]. Further, the diverted sample and quasi-control sample differed significantly by age, $t(1810) = 6.35$, $p < .001$, $d = 0.33$, 95% CI [0.23, 0.43]; gender, $\chi^2(1) = 9.28$, $p = .002$, $d = 0.15$, 95% CI [0.05, 0.23]; race/ethnicity, $\chi^2(3) = 12.64$, $p = .005$, $d = 0.18$, 95% CI [0.08, 0.28]; and incident type, $\chi^2(2) = 216.54$, $p < .001$, $d = 0.85$, 95% CI [0.74, 0.96]. See Table 1 for the baseline characteristics of the diverted, arrested, and quasi-control samples.

Comparisons between Diverted Youth Sample and Full Arrested Youth Sample

Within two years of their initiating incident, 26.7% of diverted youth had experienced a recidivism arrest, compared to 33.5% of youth in the full arrested sample. Chi-square analyses comparing recidivism arrest (no, yes) between these two samples at 3, 6, 9, 12, 15, 18, 21, and 24 months post-school-based incident revealed that youth in the diverted sample were significantly less likely than youth in the full arrested sample to have experienced a recidivism arrest at each time point. Detailed statistics are presented in Table 2.

Among youth with at least one recidivism arrest in the 24-month period following the initiating incident, diverted youth demonstrated a longer time to recidivism than youth in the full arrested sample; diverted youth: *M* = 335.31 days, *SD* = 204.95; arrested youth: *M* = 289.32 days, SD = 204.73; hazard ratio = 0.76, 95% CI [0.65, 0.88], *z* = -3.62, *p* < .001. Additionally, on average, diverted youth had fewer recidivism arrests within two years (*M* = 0.34, *SD* = 0.82, range = 0–7) than did youth in the full arrested sample (*M* = 0.56, *SD* = 1.03, range = 0–12). Tweedie compound Poisson general linear models comparing number of arrests at 3, 6, 9, 12, 15, 18, 21, and 24 months post-initiating incident also revealed that diverted youth had significantly fewer arrests than youth in the full arrested sample at each examined time point. See Table 3 for detailed statistics.

Comparisons between Diverted Youth Sample and Quasi-Control Sample

As noted, 26.7% of diverted youth experienced a recidivism arrest within two years of their initiating incident, compared to 31.9% of youth in the quasi-control sample. Chi-square analyses comparing recidivism arrest (no, yes) between these two samples at 3, 6, 9, 12, 15, 18, 21, and 24 months post-school-based incident revealed that youth in the diverted sample were significantly less likely than youth in the quasi-control sample to have experienced a recidivism arrest at 3, 9, 12, 15, and 24 months (see Figure 2). We did not observe significant differences at the remaining three time points examined. Detailed statistics are presented in Table 2.

Among youth with at least one recidivism arrest in the 24-month period following the initiating incident, diverted youth demonstrated a longer time to recidivism than youth in the quasi-control sample; diverted youth: *M* = 335.31 days, *SD* = 204.95; arrested youth: *M* = 274.37 days; *SD* = 204.61; hazard ratio = 0.81, 95% CI [0.67, 0.98], *z* = -2.21, *p* = .027. Additionally, on average, diverted youth had fewer recidivism arrests within two years (*M* = 0.34, *SD* = 0.82,

range = 0–7) than did youth in the quasi-control sample (M = 0.58, SD = 1.18, range = 0–12). Tweedie compound Poisson general linear models comparing number of arrests at 3, 6, 9, 12, 15, 18, 21, and 24 months post-initiating incident identified significant between-group differences at 3, 6, 9, 12, and 15 months; we did not observe significant differences in the remaining three time points examined. See Table 3 for detailed statistics and Figure 3 for a graphical representation.

Comparisons after Propensity Score Matching

As described previously, we matched the diverted and quasi-control samples based on their distribution of propensity scores. As shown in Table 4, the two matched samples did not differ significantly by gender, age, race/ethnicity, or incident type.

Following propensity score matching, we conducted the same series of analyses to more accurately estimate the relationship between youth diverted status and recidivism outcome. Chi-square analyses comparing recidivism arrest (no, yes) between these matched samples at 3, 6, 9, 12, 15, 18, 21, and 24 months post-school-based incident revealed no significant between-group differences at any examined time point. Detailed statistics are presented in Table 5. Among youth with at least one recidivism arrest in the 24-month period following the initiating incident, there was no significant difference in time to recidivism between the matched samples of diverted and arrested youth; hazard ratio = 1.11, 95% CI [0.89, 1.38], z = 0.91, p = .363. Finally, Tweedie compound Poisson general linear models comparing number of recidivism arrests at 3, 6, 9, 12, 15, 18, 21, and 24 months post-initiating incident did not identify significant differences between the matched samples at any examined time point. See Table 6 for detailed statistics.

Discussion

This study sought to determine whether the Philadelphia Police School Diversion Program met several goals, and results revealed promising findings. First, for the first five years

of the Diversion Program's operation, the number of school-based arrests across the city declined annually, each year exceeding the targeted 50% reduction from the pre-Diversion school year—and culminating in an 84% reduction in year five. Second, this decline in school-based arrests was accompanied by a 34% reduction in serious behavioral incidents reported by Philadelphia schools from the year before Diversion Program implementation to year five of the program, suggesting that diverting youth in lieu of arrest did not worsen school safety. Third, when looking at raw numbers and basic statistical comparisons, youth diverted from arrest via this program had a lower rate of recidivism arrest throughout the two years following their initial incidents than students arrested for school-based offenses in the year prior to Diversion Program implementation. Importantly, we recognize that these basic statistical comparisons should not be used to draw conclusions about causal impacts of the Diversion Program on youths' likelihood of recidivism arrest. Instead, these analyses provide important context for system stakeholders and—when combined with results following propensity score matching—suggest that they should avoid relying on comparisons between all arrested and all diverted youth to determine program effectiveness. Specifically, once we applied propensity score matching techniques to make the diverted youth sample more similar to the quasi-control sample with respect to demographic and incident-related variables, these groups no longer demonstrated significant differences in recidivism rates. This finding suggests that the relationship between diversion and subsequent arrest is complicated and potentially confounding variables must be considered during further exploration.

The substantial overall reduction in arrests across Philadelphia schools likely reflects the effective implementation of the Philadelphia Police School Diversion Program, as the program was specifically designed to achieve this outcome and the school district made no other major

changes to the code of conduct or to school-related policing during this time. This success suggests that program-driven policy changes targeted relevant youth and incident types, that police officer trainings effectively conveyed the mission of the Diversion Program, that stakeholders demonstrated adequate buy-in to program, and that relevant actors competently carried out the program's policies and procedures.

Accompanying reductions in serious behavioral incidents and exclusionary discipline across Philadelphia schools indicate that, when remaining in school, diverted students did not frequently exhibit serious misbehavior and disrupt school climate with illegal actions. These findings align with a study of another diversion program in which investigators observed small decreases in disciplinary referrals among diverted students (Fader et al., 2015). It is possible that with increasing awareness of the Diversion Program and recognition that police would no longer arrest most students and immediately remove them from school, school personnel may have less frequently reported behavioral incidents to the Office of School Safety—particularly those, like disorderly conduct, which require subjective judgment to determine severity—and, instead, addressed these issues internally. Teachers and other school personnel may also underreport rates of student misbehavior in response to policies designed to discourage the use of exclusionary discipline and to dismantle the school-to-prison pipeline (e.g., Graham, 2019; Taylor, 2015). However, despite potential concerns related to underreporting, it is unlikely that the sustained drop in behavioral incidents we observed would have occurred if students' serious misbehaviors had, in actuality, become more frequent. Thus, these findings provide support that schools can respond to students' misbehaviors using alternatives to arrest without fearing a subsequent increase in unsafe behaviors on school grounds.

On average, diverted youth were less likely than arrested youth to experience an arrest within two years of their initial school-based incident; however, once we used propensity score matching techniques to improve the equivalence of the two samples, we no longer observed significant between-group differences. Importantly, avoiding arrest has several additional benefits beyond a potential reduction in recidivism. For example, by avoiding arrest, diverted youth are likely able to avoid the "delinquent" label that can often accompany formal juvenile justice system processing and lead to stigmatizing attitudes toward youth (Mears et al., 2016; Sullivan et al., 2010). These young people may also have less exposure to antisocial peers in the justice system and be less frequently excluded from prosocial peers and other positive influences in the school setting than arrested youth. Additionally, considerable stress can arise from arrest and the subsequent police and court processing—including potential requirements accompanying court supervision and the fear of punishment for failure to comply with court conditions (Cox, 2012; Feierman et al., 2016). These consequences often occur even when youth are arrested for minor, school-based misbehavior (e.g., Wolf, 2013). Thus, by avoiding arrest and its accompanying stressors, diverted youth may be better able to stay on normal developmental trajectories (e.g., graduating high school; Kirk & Sampson, 2013) that eventually result in desistance from offending behavior.

Similarly, among youth with a recidivism arrest within 24 months, diverted youth tended to have a longer time to recidivism compared to arrested youth overall—but we no longer observed significant group differences after propensity score matching. Of note, the Diversion Program provides access to 90 days of voluntary prevention services for all diverted youth, which may serve as a protective mechanism, and future research should examine the effect of service acceptance and participation on recidivism risk. Additionally, youth are often suspended

or expelled from school in the immediate aftermath of a school-based arrest (Cuellar & Markowitz, 2015)—and perhaps following the school-based incidents that give rise to diversion. Students' removal from a structured environment and increased free time in the community may contribute to an increased likelihood of delinquent behavior and arrest (Monahan et al., 2014) and, therefore, this factor should be explored in the future to determine its impact on recidivism.

Although diverted youth consistently demonstrated a lower recidivism rate than arrested youth at each examined time point over the course of two years and a longer time to recidivism when it did occur, these differences were no longer statistically significant after propensity score matching based on age at initial incident, gender, race/ethnicity, and initial incident type. Such findings indicate that these demographic and incident-related characteristics may play an important role in the risk for future recidivism. These results largely align with prior research identifying gender differences in delinquent behavior and arrest (e.g., Fergusson & Horwood, 2002; Miller et al., 2010) and disproportionate rates of police contact and arrest for racial/ethnic minority youth (Dillard, 2013). Importantly, though the Diversion Program employs objective eligibility criteria as a strategy for preventing demographic-related disparities at the point of school-based arrest, it was not designed to address such disparities in any subsequent *community-based* arrests. As a result, youths' demographic characteristics may still have served as significant predictors of recidivism arrest and, by controlling for these factors via propensity score matching, we may have hindered our ability to observe between-group differences for diverted and arrested students. Along those lines, it is important to reiterate that our outcome measures were based on recidivism *arrest* and do not necessarily reflect increased delinquent behavior (Hirschfield, 2018; Skiba et al., 2002; Wolf, 2013). Initiatives like the Diversion Program might be used as part of a strategy to reduce racial and ethnic disparities in juvenile

justice outcomes; however, findings related to racial and ethnic differences in recidivism arrest suggest that additional, supplemental strategies will likely be necessary.

Positive outcomes related to reducing school-based arrests have considerable implications for both youth and their communities. For youth, avoiding arrest prevents exposure to the negative collateral consequences associated with justice system involvement. This point becomes particularly apparent when remembering the fact that *all* youth in the quasi-control sample acquired an arrest record—even the 68% of the group that did not incur a recidivism arrest in the two years following their initial school-based incident. Thus, all youth in that sample, likely, face the adverse collateral consequences of that arrest record, including those related to educational and employment opportunities, housing restrictions, and court costs and fees that can haunt them throughout adolescence and into adulthood (e.g., Kirk & Sampson, 2013; Wiesner et al., 2010). In contrast, just 26.7% of youth in the diverted sample acquired an arrest record within two years of their initial incident, allowing 73.3% of this group to continue on the typical adolescent trajectory of desistance from future offending behavior (Lussier et al., 2015) without long-term suffering for potentially normative adolescent misbehavior.

In addition to youth directly benefiting from the positive outcomes of the Diversion Program, the greater school and Philadelphia communities have also benefited. For example, as previous scholars and advocates have suggested (Children's Defense Fund, 2013; Kim, 2012; Theriot & Orme, 2016), fewer arrests in school likely contributes to fewer disruptions in school, fewer students removed from school, and less contention among students and between students and school police. Additionally, although differences in recidivism arrest between the diverted and quasi-control samples were not statistically significant after propensity score matching, these results suggest that implementing the Diversion Program did not *increase* recidivism rates, an

important outcome when considering safety in the larger community. Finally, as fewer youth are referred to and processed in the justice system, communities benefit via direct and indirect cost savings. Direct benefits likely include reduced costs associated with police processing, court processing, community-based supervision, and any detention or residential placement that would have stemmed from the averted arrests (e.g., Wolf, 2013). Further, in Philadelphia, added cost savings stem from the fact that all associated service referrals and provision occurred through existing, DHS-funded programs (Goldstein et al., 2019). Communities and their juvenile justice agencies can reinvest such savings into more preventative and diversionary programming aimed at further reducing youths' justice system involvement. Indirect benefits include improved youth trajectories, increased engagement in school, and improved job prospects for youth later in life.

Limitations

Recognizing the potential implications of the current findings, we also acknowledge that these results must be interpreted within the context of study limitations. First, this study focused on a Diversion Program operating within one police department and one school district. Further, because the Diversion Program was implemented in all city schools simultaneously, we were unable to perform a true experiment using randomization. Instead, we used a quasi-control sample, comprised of youth arrested in 2013-2014 who would have been eligible for the Diversion Program if it had existed at the time, to make important outcome comparisons. Although we initially considered using a five- or ten-year period from which to draw the quasi-control sample, we ultimately decided to include only this one year of data, as the SDP revised its code of conduct in the summer of 2012 to eliminate zero-tolerance policies. We concluded that discipline practices prior to that change would not serve as an appropriate baseline for comparison in this study and that it could take up to a full school year following this code of

conduct change for discipline practices to stabilize across the district. Thus, we compared post-implementation data to data from the year immediately prior to implementation and took care to ensure identical inclusion criteria for the two samples.

Additionally, to facilitate analysis, the examined incident categories were rather broad (i.e., non-firearm weapon possession, drug possession/other), which prevented a more detailed examination of the ways in which initial incidents might influence risk of future recidivism arrest. Our comparison samples also differed from our diverted sample, particularly, in regard to incident type: both the full arrested and quasi-control samples had larger proportions of "other" offenses than the diverted sample. Though such differences may have been expected between the full arrested and diverted samples given the design of the Diversion Program, it is unclear why they existed between the quasi-control and diverted samples, as we used the same eligibility criteria to create those groups. Importantly, however, using propensity score matching to create more equivalent groups should have removed the effects that such differences may have had on recidivism findings. Future research should investigate potential explanations for these group differences—including any possible systemic factors—and include finer tuned information on incident type, distinguishing, for example, between various types of weapons within the category of non-firearm weapon possession (e.g., knife, pepper spray, scissors).

Although baseline group differences limited our initial statistical analyses comparing the diverted sample to both the full arrested sample and the quasi-control sample, we developed our analysis plan to best produce information that is accessible and useful to both scholars and practitioners. System stakeholders have expressed interest in comparisons between arrested and diverted youth in their broadest categorizations. Starting there and then creating progressively more equivalent groups for comparison—first, by generating a quasi-control sample of students

who would have been eligible for the Diversion Program if it had existed at the time of their arrest and, second, by using propensity score matching techniques—as a step-by-step process provided important context for final recidivism arrest outcomes. Specifically, showing how we no longer observed significant differences after comparing the most equivalent groups showcases the ways in which propensity score matching techniques impacted the results and highlights areas for further investigation (e.g., age at incident, incident type).

We also acknowledge that, ideally, our analyses would be able to examine why school personnel refer some students engaging in particular behaviors to law enforcement but do not refer other students who engage in those same behaviors. However, because schools cannot collect data about the times that school personnel choose *not* to call police or report behavioral incidents to the Office of School Safety, we could not address this issue in our analyses. As a result, although we utilized propensity score matching to better control for group differences, additional unmeasured selection factors may have further distinguished our matched diverted and quasi-control samples. Having such additional information could be particularly useful to identify potential racial/ethnic biases impacting decisions about whether school personnel refer youth to law enforcement, resulting in diversion or arrest.

Finally, beyond diverting youth from arrest, the Philadelphia Police School Diversion Program offers intensive preventative services to diverted youth and their families (see Goldstein et al., 2019). These services are not mandatory and, at the time of this study, data on youths' service participation were unavailable for analysis. Future research should seek to determine the ways in which service participation might provide a protective short-term function and contribute to long-term prevention of later arrest among diverted youth.

Future Directions

In addition to addressing the identified limitations, further study of this program should examine whether and how diversion from arrest may be linked to other important youth outcomes, such as school progress (e.g., suspension, expulsion, attendance, graduation), academic achievement, health outcomes, and future employment over a longer time period. Such investigation should focus not only on diverted youth at the individual level, but also consider potential impacts on attendance and academic performance among other students at the school level (e.g., Steinberg & Lacoe, 2017). Future research should also evaluate how the program has fulfilled other identified goals, such as reducing racial and ethnic disparities in school-based arrest.

Prospective studies might use time to recidivism arrest findings to test out effectiveness of targeted, time-specific recidivism prevention efforts for diverted youth. Additionally, future analyses might focus more specifically on the incidents leading to youths' recidivism arrests—for example, identifying factors associated with subsequent school-based arrests compared to subsequent community-based arrests. Such inquiry could contribute to understanding why matched diverted youth did not demonstrate reduced risk of overall recidivism arrest despite decreasing annual rates of school-based arrests and, potentially, provide additional data to evaluate the program's ability to improve school and/or public safety. Finally, results suggesting that the program reduced the number of school-based arrests without a corresponding increase in serious incidents will likely stimulate interest in this program and might inspire other jurisdictions to replicate and evaluate their own implementations of the Diversion Program to reduce the number of school-based arrests. Further investigation of the short- and long-term cost savings of diverting youth might enhance the appeal of replication to other jurisdictions.

Conclusion

An evaluation of the Philadelphia Police School Diversion Program produced promising results, suggesting that such pre-arrest, school-based diversion opportunities can contribute to considerable reductions in school-based arrests without compromising school or public safety. Findings related to future risk for arrest suggest that further research should investigate the role of demographic and incident-related characteristics in these outcomes. Replication of these efforts in other jurisdictions would, likely, further bolster the evidence that pre-arrest diversion programs can play a major role in safely dismantling the school-to-prison pipeline, helping youth stay on a positive trajectory as they mature into adulthood.

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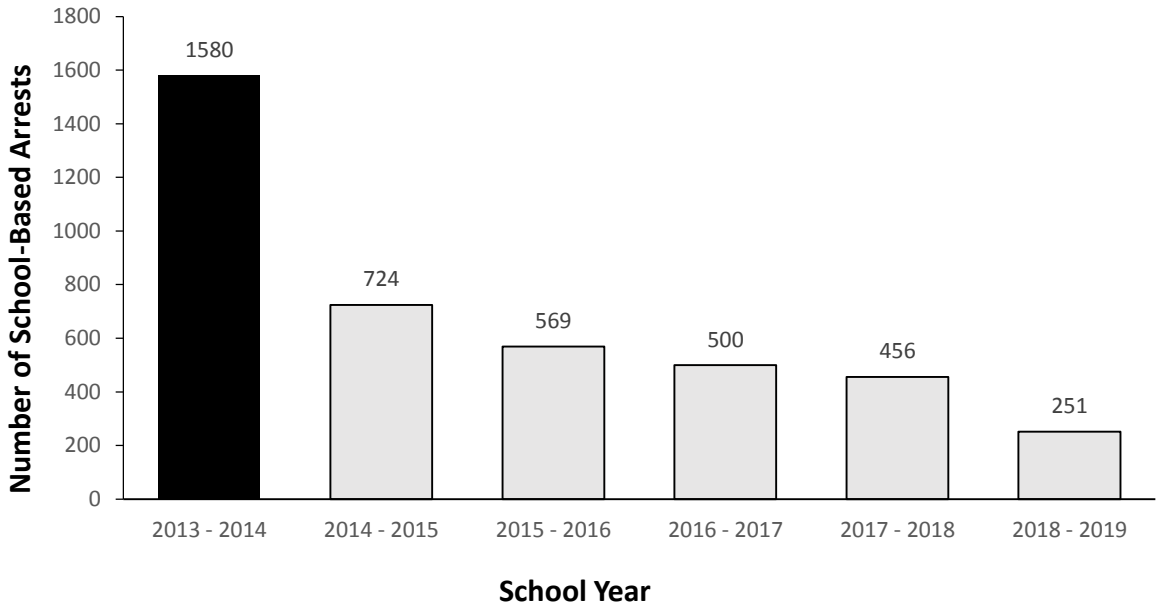
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Figure 1

Annual Number of School-Based Arrests in Philadelphia Schools from 2013 to 2019



Note. The 2013-2014 academic year represents the school year before implementation of the Philadelphia Police School Diversion program. Over the course of the five years of the Diversion Program (i.e., from 2013-2014 to 2018-2019), school-based arrests have decreased by 84%.

Figure 2

Binary Recidivism Arrest Outcomes at 3-Month Time Points for Diverted and Quasi-Control Samples

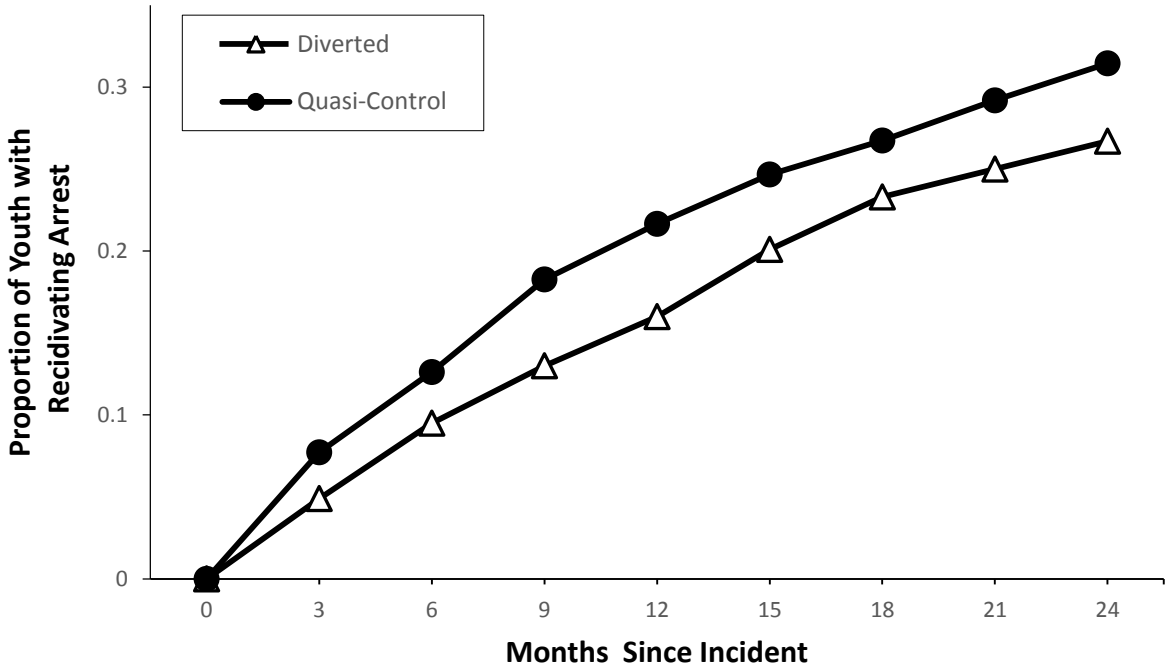


Figure 3

Number of Recidivism Arrests at 3-Month Time Points for Diverted and Quasi-Control Samples

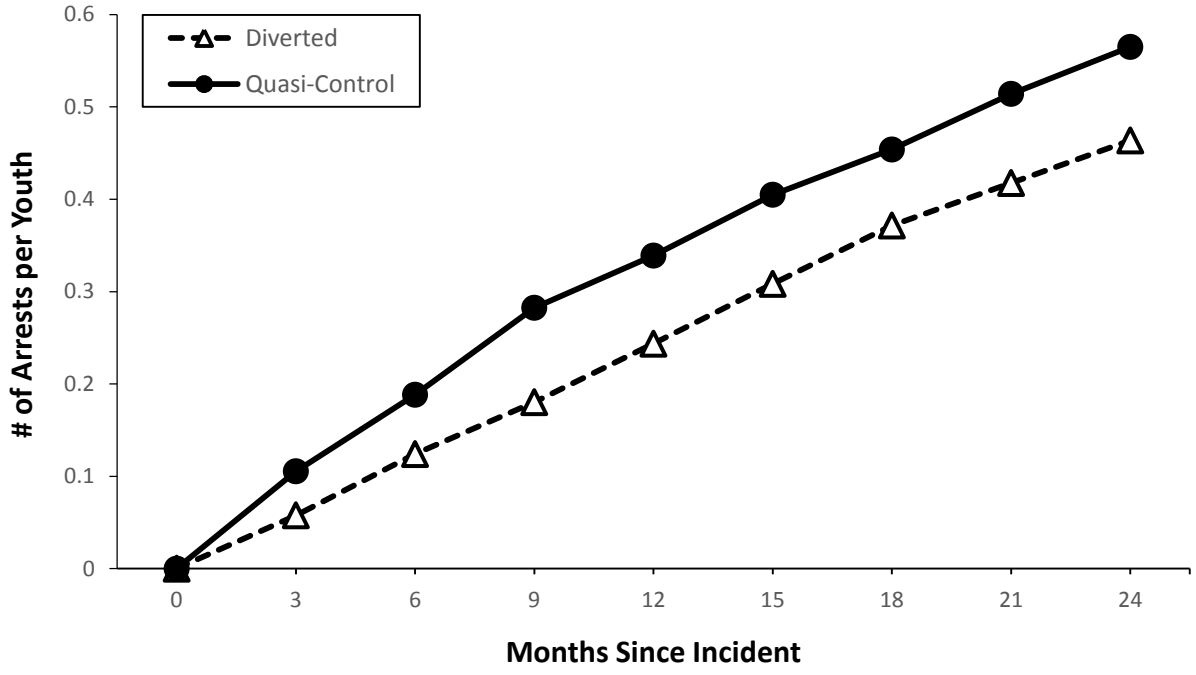


Table 1

Comparison of Baseline Characteristics of Participants Across Diverted and Arrested Samples

Variable	Diverted sample (n = 1,281)	Arrested sample (n = 1,021)	p^1	Quasi-control sample (n = 531)	p^2
Age, M (SD)	14.82 (2.17)	15.21 (1.97)	< .001	15.50 (1.82)	< .001
Gender, *n* (%)			.087		.002
Female	395 (30.8%)	350 (34.3%)		203 (38.2%)	
Male	886 (69.2%)	671 (65.7%)		328 (61.8%)	
Race/ethnicity, *n* (%)			< .001		.005
White	121 (9.4%)	54 (5.3%)		41 (7.7%)	
Black	946 (73.9%)	806 (78.9%)		407 (76.7%)	
Hispanic	185 (14.4%)	107 (10.5%)		58 (10.9%)	
Others	29 (2.3%)	54 (5.3%)		25 (4.7%)	
Incident Type, *n* (%)			< .001		< .001
Marijuana possession	484 (37.8%)	146 (14.3%)		131 (24.7%)	
Weapon possession	620 (48.4%)	176 (17.2%)		158 (29.8%)	
Other	177 (13.8%)	699 (68.5%)		242 (45.5%)	

Note: The *p*-value column reflects the *p*-value of the χ^2 or *t*-test evaluating differences in demographic variables across the diverted and arrested samples (p^1) and the diverted and quasi-control samples (p^2).

Table 2

Chi-Square Results Examining Proportion of Youth Samples with Recidivism Arrest at 3-Month Time Points

	Diverted Youth (n = 1,281)	Arrested Youth (n = 1,021)	Comparing Diverted & Arrested Samples				Quasi-Control (n = 531)	Comparing Diverted & Quasi-Control Samples			
	Recidivism Arrest	Recidivism Arrest	χ^2	p-value	d	95% CI	Recidivism Arrest	χ^2	p-value	d	95% CI
3 Months	63 (4.9%)	76 (7.4%)	5.95	.015	0.10	[0.02, 0.18]	41 (7.7%)	4.95	.026	0.10	[0.01, 0.20]
6 Months	122 (9.5%)	132 (12.9%)	6.37	.012	0.11	[0.02, 0.19]	67 (12.6%)	3.52	.061	0.09	[-0.004,0.18]
9 Months	167 (13.0%)	182 (17.8%)	9.76	.002	0.13	[0.05, 0.21]	97 (18.3%)	7.84	.005	0.13	[0.04, 0.22]
12 Months	205 (16.0%)	222 (21.7%)	12.01	.001	0.14	[0.06, 0.23]	115 (21.7%)	7.87	.005	0.13	[0.04, 0.22]
15 Months	258 (20.1%)	259 (25.4%)	8.62	.003	0.12	[0.04, 0.20]	131 (24.7%)	4.30	.038	0.10	[0.01, 0.19]
18 Months	299 (23.3%)	287 (28.1%)	6.56	.010	0.11	[0.03, 0.19]	142 (26.7%)	2.18	.140	0.07	[-0.02, 0.16]
21 Months	320 (25.0%)	321 (31.4%)	11.48	.001	0.14	[0.06, 0.22]	155 (29.2%)	3.23	.072	0.08	[-0.01, 0.18]
24 Months	342 (26.7%)	342 (33.5%)	12.25	<.001	0.15	[0.06, 0.23]	167 (31.5%)	3.96	.046	0.09	[0.002, 0.19]

Note. For all chi-square analyses, df = 1.

Table 3

Tweedie Compound Poisson Results Examining Number of Recidivism Arrests Across Samples at 3-Month Time Points

	Comparing Diverted & Arrested Samples						Comparing Diverted & Quasi-Control Samples					
	Estimate	SE	*t*-value [a]	*p*-value	*d*	95% CI	Estimate	SE	*t*-value [b]	*p*-value	*d*	95% CI
3 Months	-0.49	0.21	-2.34	.020	-0.10	[-0.18, -0.01]	-0.60	0.24	-2.46	.014	-0.12	[-0.22, -0.02]
6 Months	-0.36	0.14	-2.47	.014	-0.10	[-0.18, -0.02]	-0.42	0.17	-2.42	.016	-0.12	[-0.22, -0.02]
9 Months	-0.33	0.12	-2.70	.007	-0.11	[-0.20, -0.03]	-0.45	0.14	-3.12	.002	-0.16	[-0.26, -0.06]
12 Months	-0.25	0.11	-2.26	.024	-0.10	[-0.18, -0.01]	-0.33	0.13	-2.47	.014	-0.13	[-0.23, -0.03]
15 Months	-0.22	0.10	-2.28	.023	-0.10	[-0.18, -0.01]	-0.27	0.12	-2.28	.023	-0.12	[-0.22, -0.02]
18 Months	-0.18	0.09	-1.97	.049	-0.08	[-0.17, 0.00]	-0.20	0.11	-1.79	.074	-0.09	[-0.19, 0.01]
21 Months	-0.19	0.09	-2.22	.026	-0.09	[-0.18, -0.01]	-0.21	0.11	-1.91	.057	-0.10	[-0.20, 0.00]
24 Months	-0.18	0.08	-2.17	.030	-0.09	[-0.17, -0.01]	-0.20	0.10	-1.89	.059	-0.10	[-0.20, 0.00]

[a] For all *t*-values in this column, $df = 2300$. [b] For all *t*-values in this column, $df = 1810$.

Table 4

Statistical Comparisons of Relevant Covariates Across Samples Before and After Propensity Score Matching

	Before Matching			After Matching		
	Diverted Youth (n = 1,821)	Quasi-Control (n = 531)	Mean Difference	Diverted Youth (n = 531)	Quasi-Control (n = 531)	Mean Difference
Age at initial incident	-0.10	0.23	0.32	0.23	0.23	< -0.01
Male gender	0.69	0.62	-0.07	0.63	0.62	-0.01
Female gender	0.31	0.38	0.07	0.37	0.38	0.01
Black	0.74	0.77	0.03	0.77	0.77	-0.01
Hispanic	0.14	0.11	-0.03	0.12	0.11	-0.01
Other race/ethnicity	0.02	0.05	0.03	0.04	0.05	0.01
Non-firearm weapons possession	0.49	0.30	-0.19	0.29	0.30	0.01

Table 5

Chi-Square Results Comparing Diverted and Arrested Youth Samples on Proportion with Recidivism Arrest After Propensity Score Matching

	Diverted Youth (n = 531)	Arrested Youth (n = 531)				
	Recidivism Arrest	Recidivism Arrest	χ^2	p-value	d	95% CI
3 Months	30 (5.6%)	41 (7.7%)	1.51	.22	0.08	[-0.04, 0.20]
6 Months	53 (9.9%)	67 (12.6%)	1.59	.21	0.08	[-0.04, 0.20]
9 Months	79 (14.9%)	97 (18.3%)	1.97	.16	0.09	[-0.03, 0.21]
12 Months	95 (17.9%)	115 (21.7%)	2.14	.14	0.09	[-0.03, 0.21]
15 Months	120 (22.6%)	131 (24.7%)	0.52	.47	0.04	[-0.08, 0.16]
18 Months	138 (25.9%)	142 (26.7%)	0.04	.84	0.01	[-0.11, 0.13]
21 Months	147 (27.7%)	155 (29.2%)	0.23	.63	0.03	[-0.09, 0.15]
24 Months	155 (29.2%)	167 (31.5%)	0.54	.46	0.05	[-0.08, 0.17]

Note. For all chi-square analyses, df = 1.

Table 6

Tweedie Compound Poisson Results Comparing Diverted and Arrested Youth on Number of Recidivism Arrests After Propensity Score Matching

	Estimate	SE	*t*-value	*p*-value	*d*	95% CI
3 Months	-0.41	0.33	-1.26	.21	-0.08	[-0.20, 0.04]
6 Months	-0.36	0.22	-1.59	.11	-0.08	[-0.20, 0.04]
9 Months	-0.33	0.18	-1.84	.07	-0.09	[-0.21, 0.03]
12 Months	-0.27	0.16	-1.65	.10	-0.09	[-0.22, 0.03]
15 Months	-0.19	0.14	-1.36	.17	-0.05	[-0.17, 0.07]
18 Months	-0.14	0.13	-1.07	.29	-0.02	[-0.14, 0.10]
21 Months	-0.15	0.13	-1.20	.23	-0.03	[-0.15, 0.09]
24 Months	-0.16	0.12	-1.31	.19	-0.05	[-0.17, 0.07]

Note. For all analyses, $df = 1060$.