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Evaluation of the Philadelphia Police School Diversion Program: Long-Term Outcomes and Sustained Impact

Final Technical Report

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Abstract

Schools are a primary referral source to the juvenile justice system, helping create and perpetuate the school-to-prison pipeline. Seeking to dismantle this pipeline in the city, the Philadelphia Police Department (PPD) partnered with the School District of Philadelphia (SDP) and the Philadelphia Department of Human Services (DHS) to develop and operate the Philadelphia Police School Diversion Program. This evaluation examined long-term outcomes for diverted youth and sustained program impacts over five years. From a full sample of 3,616 diverted and arrested students, this study used a quasi-experimental design to compare data for diverted youth (quasi-experimental group; $n = 1,281$) and similar youth arrested in schools in the year before the program's implementation (quasi-control group; $n = 531$). Results revealed a significantly lower five-year recidivism rate and a significantly lower four-year out-of-school suspension rate among diverted youth than among propensity score matched arrested youth. Matched diverted and arrested youth samples did not differ significantly in their likelihood of school dropout or on-time graduation in the four years following their referring school-based incident. Additionally, examined relationships were not significantly moderated by youths' race/ethnicity or special education status. Program impacts exceeded target thresholds over time, with an 84% reduction in the number of school-based arrests from baseline to year 5 of Diversion Program operation, a 34% reduction in the number of serious behavioral incidents from baseline to year 5, and an 89% service referral acceptance rate among diverted youth and families with whom DHS made contact following diversion. Overall, results indicate the Diversion Program was successful in meeting its primary goal of substantially reducing the number of school-based arrests city-wide without endangering school or community safety.

Executive Summary

Background and Hypotheses

Zero-tolerance policies and harsh disciplinary practices have made schools a primary referral source to the juvenile justice system, helping create and perpetuate the school-to-prison pipeline. Seeking to dismantle this pipeline in the city, the Philadelphia Police Department (PPD) partnered with the School District of Philadelphia (SDP) and the Philadelphia Department of Human Services (DHS) to develop and operate the Philadelphia Police School Diversion Program. The current evaluation examined long-term outcomes for diverted youth and sustained program impacts over time.

It was hypothesized that the Diversion Program, in lieu of school-based arrest, would improve youths' long-term outcomes by 1) reducing the probability of recidivism arrest, 2) improving school-related behavioral and academic outcomes, and 3) narrowing disparities by race/ethnicity and special education status in recidivism arrest and school-related outcomes. It was also predicted that early program successes would be sustained over five years, with at least a 50% reduction in the annual number of school-based arrests from the year prior to Diversion Program implementation, at least a 15% reduction in the annual number of serious behavioral incidents in schools from that baseline year, and at least an 80% acceptance rate by youth and families of voluntary, no-cost, community-based prevention service referrals offered through this program.

Method

Sample. In the baseline academic year before Diversion Program implementation, police arrested 1,580 youth in Philadelphia schools; they diverted 2,036 youth through the Diversion Program in its first five years of operation. This study used a quasi-experimental design to

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compare data for diverted youth with SDP records (quasi-experimental group; $n = 1,281$) to data for similar youth arrested in schools in the year before the program's implementation (quasi-control group; $n = 531$). Diverted youth in the quasi-experimental sample were largely male (69%), predominantly Black (74%), and between 10 and 22 years of age (M = 14.82, SD = 2.17) at the time of diversion. Similarly, youth in the quasi-control arrested sample were largely male (62%), predominantly Black (77%), and between the 10 and 19 years of age at the time of their school-based arrest (*M* = 15.50, *SD* = 1.82).

Procedures. This project involved cross-system matching, integration, and analysis of individual-level, deidentified police, school, and human services records, as well as acquisition of aggregate school-based arrest and serious behavioral incident data.

Method of Analysis. Chi-square tests, Tweedie compound Poisson general linear models, Cox proportional-hazards regression, propensity score matching, logistic regression, and mixed-effects logistic regression were used to examine program effectiveness by comparing moderate- and long-term police and school outcomes of diverted and arrested youth up to five years after a school-based incident. To evaluate sustained program outcomes over time, descriptive data were compared against the proposed program target rates for school-based arrests, serious behavioral incidents, and acceptance of service referrals.

Results

Results indicated that, compared to youth in the quasi-control arrested sample, a smaller proportion of diverted youth experienced a recidivism arrest in the five years following their referring school-based incident. Additionally, comparing these groups after propensity score matching revealed significant differences in likelihood of recidivism arrest in the five-year follow-up period and in likelihood of out-of-school suspension in the four-year follow-up period.

However, these matched samples did not differ significantly in likelihood of school drop-out or on-time graduation in the four years following their referring school-based incident. Additionally, examined outcomes did not differ significantly by race/ethnicity or special education status.

Program impacts exceeded target thresholds over time, with an 84% reduction in the number of school-based arrests from baseline to year 5 of the Diversion Program, a 34% reduction in the number of serious behavioral incidents from baseline to year 5, and an 89% service referral acceptance rate among diverted youth and families with whom DHS made contact.

Discussion

Results indicate the Diversion Program was successful in meeting its primary goal of substantially reducing the number of school-based arrests city-wide without endangering school or community safety. Although findings suggest the program may not have had the anticipated effects of disproportionately *improving* police and school outcomes for Black youth and youth enrolled in special education, the program appears to substantially reduce the rates of arrest and recidivism for all youth. Importantly, many existing diversion programs disproportionately benefit White youth, and results from the current evaluation suggest that is not the case with the Philadelphia Police School Diversion Program.

Additionally, although important outcomes related to school discipline differed between diverted and arrested youth, the fact that no significant between-group differences emerged in long-term academic achievement outcome analyses suggests that diverted youth may benefit from additional school-based support to prevent school dropout and promote on-time graduation.

In sum, the Diversion Program offers a promising model to other jurisdictions seeking policy and

practice changes aimed at shrinking their school-to-prison pipelines.

EVALUATION OF THE PHILADELPHIA POLICE SCHOOL DIVERSION PROGRAM

Background and Overview

In the early 1990s, school districts across America began adopting a "zero-tolerance" approach to student discipline, mandating the imposition of predetermined punishments (e.g., suspension, expulsion, referral to law enforcement) when students violate school rules, regardless of mitigating circumstances (Curran, 2019). Proponents of zero-tolerance approaches argued that severely punishing disruptive students would create a safer educational environment more conducive to learning and deter other students from similar misbehaviors (American Psychological Association [APA] Zero Tolerance Task Force, 2008). Throughout the 1990s and 2000s, as increasing numbers of public schools adopted zero-tolerance approaches to discipline, the numbers of suspensions and arrests in schools rose dramatically. For example, the number of suspensions across the country nearly doubled—from 1.7 million in 1974 to 3.3 million in 2006 (National Center for Education Statistics, 2008; Wald & Losen, 2003). Additionally, the number of students arrested and referred to juvenile court for school infractions increased even more sharply—in some districts, the number of students arrested in schools increased by as much as 1,248% between the mid-1990s and 2004 (Teske, 2011; Wald & Losen, 2003).

Importantly, though zero-tolerance and similar policies were typically justified by the idea of deterrence, empirical studies found that school violence and disruption did *not* decrease during the rise of zero-tolerance policies (e.g., APA Zero Tolerance Task Force, 2008) and that such practices actually were linked with *increased* rates of future misconduct (APA Zero Tolerance Task Force, 2008; Costenbader & Markson, 1998; Raffaele-Mendez, 2003). In fact, by removing students from schools for even minor forms of misbehavior, zero-tolerance approaches also remove youth from important protective factors associated with the school environment,

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including supportive peer and adult relationships and involvement in prosocial extracurricular

activities (Mahoney, 2014; Weber et al., 2016). Thus, exclusionary discipline puts youth at

higher risk for future negative outcomes, including poor academic performance, school dropout,

and justice involvement (e.g., Mittleman, 2018; Monahan et al., 2014).

With zero-tolerance policies facilitating an increase in the use of harsh disciplinary

practices—and the increasingly widespread practice of employing school-based police officers—

schools have become a primary referral source to the juvenile justice system (Teske, 2011).

Thus, these practices have contributed to the development of a "school-to-prison pipeline,"

which pushes youth out of a normative, developmentally appropriate environment (i.e., school)

and *into* an often-punitive justice system. For example, U.S. schools referred more than 290,600

students to law enforcement during the 2015-2016 school year, resulting in more than 52,000

arrests for school-related incidents (U.S. Department of Education, 2018; U.S. Department of

Education, 2019). Instead of primarily addressing serious and/or violent school behaviors,

school-based police referrals and arrests frequently serve as responses to *minor* and *nonviolent*

infractions (e.g., class disruption, marijuana possession; Bahena et al., 2012; Whitaker et al.,

2019). When school personnel and law enforcement criminalize normative adolescent

misbehavior in this way (Reyes, 2006; Theriot, 2009) youth often experience negative outcomes,

including feeling less connected to school, performing more poorly in academics, participating in

fewer prosocial activities, being excluded by prosocial peers, and associating more often with

delinquent peers—each of which contributes to an increased risk for future delinquency and

arrest (Liberman et al., 2014; Wiley et al., 2013; Wiley & Esbensen, 2016).

Further compounding the many consequences of arrest, school administrators typically

impose additional disciplinary sanctions on students arrested in schools. For example, arrested

students can also be suspended, expelled, and transferred for a single incident, resulting in many missed school days and a disrupted education. Many arrested students are transferred to alternative education programs, which typically lack the academic standards and rigor of traditional educational institutions (Lehr et al., 2009; Vogell, 2017). Further, students sent to alternative schools drop out at higher rates and are more likely to experience poverty as adults than students from traditional secondary schools (Lehr et al., 2009; Vogell, 2017). Taken together, these findings indicate that, rather than protecting students, zero-tolerance practices trigger wide-ranging negative consequences—often contributing to a greater likelihood of further justice system involvement.

Additionally, the disproportionate impacts of school-based arrests and discipline practices on students of color and youth with disabilities (Hoffman, 2014; Jordan, 2015; U.S. Department of Education Office for Civil Rights, 2014) present serious concerns about bias, inequality, and disparate treatment. Although Black students constitute just 15% of the total public-school population in the United States, they account for 30% of students referred to law enforcement and 35% of students arrested in schools (U.S. Department of Education Office for Civil Rights, 2018). Further, students receiving special education services account for approximately 33% of students who come into contact with the juvenile justice system, despite constituting only 14% of the public-school population in the United States (Kim et al., 2021; National Center for Education Statistics, 2021).

Philadelphia Context

With approximately 119,492 students in 215 district-operated schools,[1] the School

District of Philadelphia (SDP) is the 8th largest urban school district in the United States and

faces some of the most significant socioeconomic and academic obstacles in the country. An

overwhelming majority of students come from families with low incomes and from historically

underserved racial and ethnic minority groups (i.e., more than 70% are Black or Hispanic;

District Performance Office, 2020). More than 18,000 SDP students (14%) have been diagnosed

with physical and/or learning disabilities severe enough to require special educational services

and more than 11,000 students (8%) have limited English proficiency. Many SDP students also

struggle with trauma outside of school; an estimated two out of three students in the SDP have

experienced trauma in the form of poverty, violence, familial instability, or abuse (Hardy, 2014).

Although the SDP formally eliminated its zero-tolerance policy in 2012, it has

experienced significant challenges to maintaining a positive, healthy, and safe climate across all

schools. In the 2013-2014 school year, there were 6,579 reported serious behavioral incidents,

16,629 suspensions, and 1,580 arrests in Philadelphia schools (School District of Philadelphia,

2015). Historically, Philadelphia has accounted for approximately 30% of school-based arrests in

the Commonwealth of Pennsylvania (Goldstein et al., 2019). According to SDP data, students

from several marginalized racial/ethnic groups—particularly Black students—often face harsher

punishments than their peers. Black students are more likely to face suspension and expulsion for

engaging in the same behaviors as White students, and they have a greater probability of being

arrested for disciplinary violations. During the 2013-2014 school year, Black youth accounted

for 80% of school-based arrests—despite representing just 58% of the SDP student body.

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[1] In addition to the 215 district-operated schools, the School District of Philadelphia includes 86 charter schools and 25 alternative education schools. With these additional schools and other/cyber charter schools, total school enrollment amounts to 202,944 students (School District of Philadelphia, 2021).

Additionally, youth with Individualized Education Plans (IEPs) were over-represented among students arrested in schools during the 2013-2014 academic year; approximately 32% of arrested youth had an IEP, whereas only 14% of SDP students had one.

Reform Efforts

Increased understanding of how zero-tolerance policies contribute to the school-to-prison pipeline has recently gained national attention, resulting in calls to reform existing school-based arrest practices that result in an overwhelming number of students entering the juvenile justice system for engaging in normative adolescent misbehavior (e.g., Langberg & Fedders, 2013; Teske & Huff, 2011). Schools across the country have been encouraged to abandon zero-tolerance policies to reduce exacerbation of racial and ethnic disparities in school discipline, arrest rates, and long-term involvement with the justice system (Gregory et al., 2017; Heitzeg, 2014). Although many programs exist to divert youth from further justice system involvement *after* arrest (i.e., before adjudication and disposition), these programs do not spare youth the negative consequences *of* arrest, including a record that often follows them into adulthood (Kirk & Sampson, 2013). In contrast, pre-arrest diversion programs prevent youth from entering the justice system altogether, thereby avoiding the negative effects and collateral consequences of arrest, detention, processing, and any subsequent court proceedings.

School-based, *pre-arrest* diversion programs can be a powerful tool for preventing students' entry into the school-to-prison pipeline. Such programs allow youth to remain in school, where they maintain connections to prosocial individuals and continue to engage in prosocial activities. As a result, diverted students benefit from an opportunity for a "second chance" to stay on a typical, positive adolescent trajectory, and relevant agencies can link youth to appropriate services to address underlying needs. Pre-arrest school-based diversion programs

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aim to shift policy and practice away from reliance on severe punishments and towards approaches based on the premise that misbehaving students need support rather than dismissal.

The Philadelphia Police School Diversion Program

In 2013, the Philadelphia Police Department (PPD) reviewed 10 years of arrest and incident data and found that Philadelphia schools were the largest referral source for juvenile arrests in the city. In response, PPD leadership sought to develop an alternative model to addressing youth misbehavior. Recognizing that arresting students for low-level summary and misdemeanor delinquent acts committed on school property was not in the best interest of students or the community, the PPD, SDP, and Philadelphia Department of Human Services (DHS) agreed that such incidents should be addressed by school administrators and supportive service providers rather than police—thus developing the framework for the Philadelphia Police School Diversion Program.

Implemented city-wide in May 2014, this pre-arrest, school-based program diverts all first-time offending youth, 10 years or older, who commit specified school-based minor misdemeanor or summary offenses (e.g., disorderly conduct, marijuana possession, weapon possession other than firearm) on school property. In lieu of arrest, the Diversion Program connects each diverted student with a DHS social worker who conducts a home visit and refers the student and their family to voluntary, free, community-based services as needed. Importantly, students face no consequences—including arrest, suspension, or expulsion—if they decline offered services.

The main goals of the Diversion Program are to: 1) reduce the number of students arrested in schools, 2) improve school retention rates by reducing suspensions, expulsions, and disciplinary transfers, 3) address disparities in school-based arrests and disciplinary practices,

and 4) provide access to community-based services for youth and their families. A preliminary OJJDP-funded, short-term outcome evaluation of the Philadelphia Police School Diversion Program (2014-JZ-FX-K003) revealed that, in the second year of the program's operation, school-based arrests city-wide were down 64% from the year prior to program implementation. Additionally, within 1.5 years of program implementation, only 13% of youth diverted through the program were subsequently arrested—substantially below the recidivism rate for youth arrested in school in the year prior to Diversion Program implementation. As a result, the Diversion Program has been widely cited and recognized for its potential as a mechanism for dismantling the school-to-prison pipeline (Goldstein et al., 2019). Other jurisdictions across the country have inquired about replicating the Diversion Program; however, questions about the moderate- and long-term programmatic-, justice- and academic-related outcomes for diverted youth remained. The current study was designed to answer those pressing questions to inform policy and practice in Philadelphia, as well as replication efforts in other jurisdictions.

Research Objectives, Questions, and Hypotheses

A primary objective of the current study was to examine moderate- and long-term outcomes for youth diverted through the Philadelphia Police School Diversion Program to determine whether the program was meeting its stated goals over time. First, we asked: Does the Diversion Program keep youth out of the justice system? Specifically, we hypothesized that diverted youth would have a lower five-year recidivism arrest rate than comparable youth who were arrested in schools in the year before Diversion Program implementation and that, when youth do incur a recidivism arrest, diverted youth would have a longer time to arrest than those comparable arrested youth.

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Second, we inquired: Does the Diversion Program keep youth in school? We hypothesized that diverted youth would have fewer disciplinary sanctions (e.g., suspensions, disciplinary transfers) than comparable arrested youth in the five years after their referring school-based incident.[2] We also hypothesized that diverted youth would demonstrate greater academic success and achieve higher school attainment than comparable arrested youth in the five years following their referring school-based incident.

Third, we asked: Does the Diversion Program narrow disparities in youth outcomes by race/ethnicity and special education status? We hypothesized that disparities in recidivism, school discipline, and academic outcomes would be narrower among diverted youth than among comparable arrested youth at the five-year post-incident point.

In addition to examining moderate- and long-term *individual-level* outcomes for diverted youth, this study also sought to investigate whether initial *program-related* outcomes were sustained over time. For example, Diversion Program leaders aimed for a 50% reduction in the annual number of school-based arrests following program implementation. In its second year, school-based arrests had already decreased by 64% compared to 2013-2014 (i.e., the year before Diversion Program implementation), and this study examined Philadelphia school-based arrest rates over the course of five years of Diversion Program operation to determine whether initial reductions were maintained. Similarly, to identify whether the Diversion Program met goals related to keeping schools safe and providing high levels of service access to youth and their families, for each of the first five years of program operation, we examined the rate of serious school-based behavioral incidents in Philadelphia schools and the rate at which youth and

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[2] Given COVID-19-related school closures from March 2020 through June 2021, we needed to adjust the examined time frame of all hypotheses related to school outcomes from five years to four years following the referring school-based incident.

families accepted the voluntary community-based service referrals they received as part of the Philadelphia School Diversion Program.

Study Methods

Samples

We collected electronic, deidentified data for a total of 2,302 students attending Philadelphia public schools who were either arrested in schools during the 2013-2014 academic year or diverted through the Philadelphia Police School Diversion Program for a school-based incident in the 2014-2015, 2016-2017, or 2017-2018 academic years.

Diverted Youth Sample

After city-wide implementation of the Diversion Program, any student who committed one of several summary or low-level misdemeanor delinquent acts (e.g., marijuana possession, disorderly conduct, vandalism) on Philadelphia school grounds, was at least 10 years of age (i.e., minimum age of arrest in Pennsylvania), and had no previous delinquency adjudications or open court cases was automatically diverted in lieu of arrest. For short- and moderate-term outcomes (i.e., up to 2-years post-incident), our quasi-experimental sample included all youth diverted through the Diversion Program between September 2014 and June 2017 (n = 1,281); this group of youth is a subsample of all 1,391 youth diverted in these years, limited to those with SDP data.[3] The quasi-experimental sample of students were largely male (69%) and were between ages 10 and 22 (M = 14.82, SD = 2.17) at the time of their school-based diversion. Most of these youth were Black (74%), and additional racial/ethnic groups represented included: Hispanic

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[3] Youth in charter, parochial, and private schools who meet Diversion Program eligibility criteria can also be diverted in lieu of arrest; however, the SDP could not access and provide us with their data; thus, those youth were excluded from the quasi-experimental diverted youth sample.

(14%), White (9%), and any other race/ethnicity (2%); the administrative data we acquired included race and ethnicity as a single, mutually exclusive variable.

To investigate long-term impacts of the Diversion Program, we compared outcomes between diverted and arrested youth for follow-up periods after a referring school-based incident of five years (for police outcomes) and four years (for school outcomes). With these long follow-up periods, sufficient time had elapsed from the date of the referring incident *only* for youth diverted during the 2014-2015 academic year. As a result, for evaluation of long-term outcomes, our quasi-experimental sample included youth with SDP records who were diverted through the Diversion Program between September 2014 and June 2015 ($n = 427$).

Full Arrested Youth Sample

To make initial comparisons to our diverted youth sample, we collected data for all students who were arrested in SDP schools in the academic year prior to Diversion Program implementation (i.e., 2013-2014; $n = 1,021$); this group of youth is a subsample of all 1,580 youth arrested in this baseline year, limited to those with SDP data.[4] Arrested students were also largely male (66%) and their ages ranged between 10 and 19 years ($M = 15.21$, $SD = 1.97$) at the time of their school-based arrest. Like the diverted youth sample, most arrested youth were Black (79%); additional racial/ethnic groups represented included: Hispanic (11%), White (5%), and any other race/ethnicity (5%).

Quasi-Control Arrested Youth Sample

To generate a more equivalent comparison group, we created a quasi-control sample from youth in the full arrested sample who would have been eligible for the Diversion Program if it

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[4] As with the quasi-experimental diverted sample, if students were arrested in a charter, parochial, or private school, the SDP could not access and provide us with their data; thus, they were excluded from the examined arrested sample.

had existed when they were arrested in school. Thus, criteria for the quasi-control arrested sample mirrored those of youth in the diverted sample (i.e., incident type, age, and absence of previous delinquency finding or pending adjudication). Our final quasi-control sample was comprised of 531 students, was largely male (62%), and included students ages 10 through 19 years ($M = 15.50$, $SD = 1.82$) at the time of their school-based arrest. Most youth in the quasi-control arrested sample were Black (77%); additional racial/ethnic groups represented included: Hispanic (11%), White (7%), and any other race/ethnicity (5%).

Measures

Youth Demographic and Incident Data

The PPD maintains a Diversion Program database that includes demographic information (e.g., gender, race/ethnicity, age at diversion, school attended), diversion date, and the type of incident leading to diversion for all youth diverted through the program. That database was the source of relevant police data for all youth in our quasi-experimental diverted sample. Data for the full arrested youth sample came from police records of students who were arrested in Philadelphia schools during the 2013-2014 academic year; these data similarly included demographic information, arrest date, and the type of incident leading to arrest. From this full arrested youth sample, the PPD identified, in consultation with the First Judicial District of Pennsylvania (i.e., the Philadelphia County Court System, which includes juvenile court), those youth who should be included in the quasi-control arrested sample based on whether they would have qualified for the Diversion Program at the time of their referring incident; of the youth arrested in school in 2013-2014, they reviewed the arrest type and whether the youth had a previous delinquency finding or an open case at the time of the school-based incident. For all diverted and arrested youth included in the study samples, the PPD tracked any and all

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subsequent arrests (i.e., recidivism arrests) as a child or adult, including the date of that arrest.

Demographic data for youth examined in this study included: gender (male, female),

race/ethnicity (Black, Hispanic, White, other)[5], age at arrest or diversion, and school attended.

In addition to demographic data, the PPD provided incident-related data for youth across

all samples. Specifically, the PPD shared Uniform Crime Reporting (UCR) codes for the school-

based incident that led to police referral and resulted in either diversion or arrest. We also

received guidance from the PPD to classify UCR codes into three broad categories (i.e., non-

firearm weapon possession, marijuana possession, other). Example "other" incidents included

disorderly conduct, vandalism, and trespassing for youth in the quasi-experimental diverted and

quasi-control arrested samples. Additionally, other incidents for youth in the full arrested sample

included those same offenses plus several others (e.g., more serious offenses against persons,

firearm offenses, and possession of large quantities of drugs) that would have made them

ineligible for the Diversion Program if it had been in operation at the time of their referring

incident. Ultimately, for simplicity of presentation, we dichotomized youths' referring incident

type (i.e., non-firearms weapon possession, marijuana possession/other), because marijuana

possession and offenses in the "other" category tended to display similar patterns in primary

outcomes.

School-Related Data

Given that all youth in our samples were SDP students, school-related data for the current

study originated from a comprehensive database maintained by the SDP Office of Research and

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[5] Rather than identifying race and ethnicity as two separate variables, PPD records combined them into a single variable. In addition to Black, Hispanic, and White, PPD records included the following categories: American Indian/Alaskan Native, Asian, Multiracial, unknown, and other). Given concerns about disseminating potentially identifiable information by using data from categories with very low frequencies, we combined all groups other than Black, Hispanic, and White into a single "any other race/ethnicity" category.

Evaluation as part of routine procedure. This database includes information related to youths' academic performance, grade progression, and experiences of formal discipline (e.g., suspension, disciplinary transfer). Thus, to address our second research question, focused on whether the Diversion Program keeps youth in school, we "examined three dichotomous (no/yes) outcomes: 1) out-of-school suspension associated with the incident that led to diversion or arrest (incident-related suspension), 2) any suspension within the 365 days following the referring incident (post-incident suspension), and 3) any referral for expulsion or disciplinary transfer within 365 days following the referring incident (post-incident referral for expulsion or disciplinary transfer)" (Goldstein, NeMoyer, et al., in press). Of note, code of conduct and other SDP policy changes in 2012 encouraged school personnel to utilize disciplinary transfer in lieu of expulsion. Given that both outcomes similarly result in permanent school removal, we combined these two forms of referrals into a single variable: referral for permanent school removal.

Additionally, "we categorized youths' suspensions as related to their referring incidents if they met one of the following scenarios: 1) the suspension was imposed within one school day of the incident that led to diversion or arrest ($n = 1,053$) or 2) the suspension was imposed within seven calendar days of the referring incident *and* the corresponding SDP-reported suspension reason matched the PPD-reported incident type ($n = 158$)" (Goldstein, NeMoyer, et al., in press). Then, to ensure a comprehensive classification of incident-related suspensions, we pinpointed any suspensions not meeting either of the two scenarios presented above that occurred within three school days of a referring incident. Two senior research staff reviewed, together, the narratives provided by the SDP and PPD for the suspension and incident, respectively, to jointly determine whether they were equivalent ($n = 87$).

Other Data

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The PPD Diversion Program liaison provided annual rates of school-based arrests in the city of Philadelphia, 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. Additionally, the SDP provided aggregate information about the number of out-of-school suspensions, expulsions, and disciplinary transfers that occurred in its public schools each academic year from 2013-2014 through 2017-2018.

Finally, the DHS Division of Juvenile Justice Services oversees the procedures through which diverted youth receive a home visit from a social worker and a possible referral to voluntary community-based services. This agency maintains records detailing whether: 1) initial contact was made with the parent/guardian of the diverted youth; 2) the DHS social worker completed a home visit, and 3) the youth and parent/guardian accepted the voluntary, no-cost, intensive prevention services referral offered through the Diversion Program. This information was used to examine whether the Diversion Program met its goals of providing high levels of service access to youth and their families.

Procedures

The Drexel research team worked in partnership with administrators and research staff from the PPD, SDP, and DHS to extract relevant study data from agency databases to inform the short-, moderate-, and long-term Diversion Program evaluation. For all diverted and arrested youth, the SDP created a unique study ID number to use in place of identifying information (e.g., names, full dates of birth). The SDP then shared student names and ID numbers with the PPD and DHS (per an existing Memorandum of Understanding) so that those agencies could similarly deidentify their data, while still allowing the research team to match deidentified individual-level

data across databases. The research team carried out all study procedures in accordance with Drexel University Institutional Review Board policy and obtained data licensing agreements from the PPD, SDP, and DHS.

Method of Analysis

Recidivism Outcome Analyses

When examining outcomes related to recidivism arrest, we first examined *moderate-term* outcomes using a two-year follow-up period that allowed us to include three cohorts of diverted youth (i.e., youth diverted in the 2014-2015, 2015-2016, and 2016-2017 school years) in the quasi-experimental sample. Subsequently, we performed similar analyses using a five-year follow-up period to examine *long-term* outcomes, thereby limiting our quasi-experimental sample to the one cohort of diverted youth (i.e., youth diverted in the 2014-2015 school year) for whom five years' worth of follow-up data were available.

Initially, for our moderate-term analyses, we used chi-square tests to compare binary recidivism arrest outcomes (no/yes) between the diverted youth sample and the full arrested youth sample at two years following the date of diversion or school-based arrest. Then, to examine between-sample differences in *time to recidivism arrest*, we conducted a Cox proportional-hazards regression. We also examined sample differences in number of arrests at the two-year follow-up point using Tweedie compound Poisson general linear models to account for exact zeros in the number of arrests (Zhang, 2013). We subsequently performed the same series of analyses to compare the diverted youth sample with the quasi-control sample, which we created using the Diversion Program eligibility criteria to better approximate the diverted youth sample.

Next, we used propensity score matching techniques to make the samples more equivalent in terms of age, gender, race/ethnicity, and incident type, thereby better controlling for external factors when estimating the effect of diverting youth in lieu of arrest on recidivism outcomes (D'Agostino, 1998). This technique has often been utilized to reduce potential bias from unobserved group differences in nonexperimental studies, comparing youth with and without justice involvement (e.g., Beardslee et al., 2019; Kirk & Sampson, 2013; Ward et al., 2014). To execute this approach, we used an R program to match cases based on a 1:1 nearest neighbor ratio (Ho et al., 2007; R Core Team, 2019). After generating the matched samples, we conducted the same series of analyses used to compare the diverted youth sample to both the full arrested and quasi-control samples. Namely, we used chi-square tests for binary recidivism outcomes, Tweedie compound Poisson general linear modeling for number of arrests, and Cox proportional-hazards regression for time to recidivism.

To examine the ways the relationship between diversion/arrest and recidivism outcomes might differ based on youths' race/ethnicity (non-Black, Black) or IEP status (no, yes), we conducted moderation analyses.[6] To do so, we conducted, largely, the same series of analyses described above, first, while including an interaction term between diversion/arrest and youths' race/ethnicity and then again, while including an interaction term between diversion/arrest and youths' IEP status. Instead of chi-square tests, we used logistic regression models to examine disparities in binary recidivism outcomes. Otherwise, we followed the same steps, using Cox proportional-hazards regression to examine time to recidivism and Tweedie compound Poisson general linear models to examine number of arrests within each follow-up period.

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[6] We chose to dichotomize race/ethnicity as Black youth compared to youth from all other racial/ethnic groups because the vast majority of students across all study samples were Black. Further, including the additional categories for race/ethnicity (i.e., White, Hispanic, other) as separate groups prevented the generation of samples with similar propensity scores (i.e., matched samples).

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School Discipline Outcome Analyses

For our school discipline outcome analyses, we first examined *moderate-term* outcomes using a one-year follow-up period with three cohorts of diverted youth (i.e., youth diverted in the 2014-2015, 2015-2016, and 2016-2017 school years) in the quasi-experimental diverted youth sample. We then performed the same analyses using a four-year follow-up period to examine *long-term* outcomes with one cohort of diverted youth (i.e., youth diverted in the 2014-2015 school year) in the quasi-experimental sample.

For our moderate-term school discipline analyses, we examined outcomes between matched samples of diverted and arrested youth, generated through propensity score matching (Austin, 2011; Morgan, 2018). These matched samples were largely equivalent across gender (male, female), race/ethnicity (Black, others), age at arrest or diversion, incident type (marijuana possession, others), and total number of suspensions in the year prior to the referring incident (0, 1, 2 or more).

To account for the hierarchical structure of our data (i.e., youth nested within schools), we used mixed-effects logistic regression (Bates et al., 2011; Gibbons et al., 2010; Kwok et al., 2008) to examine the effect of the youths' school-based diversion or arrest on each of three school disciplinary outcomes (i.e., incident-related suspension, post-incident suspension, and post-incident referral for expulsion or disciplinary transfer). Next, we added to the model the covariates used to create the matched samples to determine whether any of these variables predicted school disciplinary outcomes. Then, to investigate whether the main effects of diversion were moderated by any relevant covariates, we examined interactions between youths' diversion or arrest and each covariate as separate mixed-effects logistic regression models (Hayes & Rockwood, 2017).

Additionally, recognizing that rates of exclusionary discipline largely trended downward over the years of the Diversion Program's operation, we performed a cohort analysis among diverted youth to determine whether this trend influenced our results. Specifically, we used chi-square tests of independence to examine whether diversion year was associated with each of our school discipline outcomes (i.e., incident-related suspension, post-incident suspension, post-incident referral for expulsion or disciplinary transfer).

Finally, to examine the ways in which the relationship between diversion/arrest and school discipline outcomes might differ based on youths' race/ethnicity (non-Black, Black) or IEP status (no, yes), we performed moderation analyses. To do so, we conducted, largely, the same series of mixed-effects logistic regression analyses described above. We did so, first, while incorporating an interaction term for diversion/arrest and youths' race/ethnicity and, second, while incorporating an interaction term for diversion/arrest and youths' IEP status.

Academic Achievement Analyses

We examined long-term academic achievement outcomes by comparing rates of school dropout and on-time graduation (i.e., graduated within four years of 9th grade) between youth diverted from arrest for a school-based incident in the 2014-2015 school year (i.e., our long-term quasi-experimental sample) and comparable youth arrested for similar offenses in the 2013-2014 school year (i.e., our quasi-control sample). To do so, we used mixed effects logistic regression models to account for the hierarchical nature of the data (i.e., nested youth within schools). In any instance where models failed to converge on an estimate, we used logistic regression without nesting. In addition to simply comparing youth from the quasi-experimental and quasi-control samples, we used to propensity score matching to generate more equivalent groups based on gender, race/ethnicity, age at incident, and IEP status. We then conducted the same mixed effects

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logistic regression analyses as before, comparing academic outcomes between the two matched samples.

Finally, to examine the ways in which the relationship between diversion/arrest and academic achievement outcomes might differ based on youths' race/ethnicity (non-Black, Black) or IEP status (no, yes), we performed moderation analyses. To do so, we conducted, largely, the same series of mixed-effects logistic regression analyses described above. We did so, first, while incorporating an interaction term for diversion/arrest and youths' race/ethnicity and, second, while incorporating an interaction term for diversion/arrest and youths' IEP status.

Results

Program-Level Outcomes

In May 2014, Philadelphia police implemented the Diversion Program simultaneously across all Philadelphia schools (Goldstein et al., 2019). Across Philadelphia, in the year before Diversion Program implementation (i.e., the 2013-2014 academic year), 1,580 school-based arrests occurred (Goldstein, Kreimer, et al., 2021). As described by Goldstein, Kreimer, and colleagues (2021), there was a 54% reduction in Philadelphia school-based arrests during the Diversion Program's first year of operation. This trend continued, as the annual number of school-based arrests declined each year, until there was an 84% reduction in school-based arrests from the year before program implementation to its fifth year of operation (i.e., the 2018-2019 academic year). See Goldstein, Kreimer, and colleagues (2021) for a figure depicting the annual number of Philadelphia school-based arrests by year.

In addition to the annual decreases in number of school-based arrests, over the first five years of the Diversion Program's operation, the number of serious behavioral incidents within SDP schools also declined. As described by Goldstein, Kreimer, and colleagues (2021), 6,359

serious behavioral incidents occurred in 2013-2014, and this number decreased to 4,192 in the 2018-2019 school year, representing a 34% reduction. Similarly, the use of exclusionary discipline (i.e., out-of-school suspensions, expulsions, disciplinary transfers) in SDP schools decreased by 41% in the Diversion Program's first four years of operation.

In total, police diverted 2,036 students through the Diversion Program in the program's first five years. In this same five-year period, DHS successfully contacted 74% of diverted youth and their parents/guardians, with the annual percentage increasing from 66% in the 2014-2015 academic year to 77% in the 2018-2019 academic year. Importantly, youth who could and could not be contacted following diversion did not differ significantly in terms of demographic characteristics (i.e., age, race, gender) or the type of referring incident. Of the families with whom DHS made contact, 89% accepted a referral for the voluntary community-based services, programs that include components such as mentoring, academic support, and referrals for substance use treatment.

Recidivism Outcomes

Moderate-Term Recidivism Outcomes

Goldstein, Kreimer, and colleagues (2021) authored a paper reviewing *moderate-term* outcome findings related to youths' recidivism arrests.[7] Results indicated that 27% of diverted youth had experienced a recidivism arrest in the two years following their referring school-based incident, as compared to 34% of youth in the full arrested youth sample and 32% of youth in the quasi-control sample. A summary of moderate-term outcome analysis results is presented below.

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[7] Of note, the Philadelphia Police Department and School District of Philadelphia provided additional data after Diversion Program publications cited in this document were released or in press. These updates led to minor data refinements. As a result, numbers presented in this report may differ slightly from those values reported in previous or in press publications.

Full arrested sample comparisons. Chi-square analysis revealed that diverted youth were significantly less likely than youth in the full arrested sample to have experienced a recidivism arrest within the two-year follow-up period after their initiating incident, $\chi^2 = 12.25$, $df = 1$, $p < .001$. Among youth with at least one recidivism arrest in the two-year follow-up period, diverted youth had a longer time to recidivism arrest than youth in the full arrested sample; diverted youth: $M = 301.79$ days, $SD = 199.84$; arrested youth: $M = 289.32$ days, SD = 204.73; hazard ratio = 0.76, 95% CI [0.65, 0.88], $z = -3.62$, $p < .001$. Further, on average, diverted youth had fewer recidivism arrests during the 2-year follow-up period ($M = 0.46$, $SD = 0.98$, range = 0–9) than full arrested sample youth ($M = 0.55$, $SD = 1.00$, range = 0–10). Results of Tweedie compound Poisson general linear models indicated that diverted youth had significantly fewer arrests than full arrested sample youth during the two-year follow-up period, $B = -0.17$, $p = .040$, $d = -0.09$, 95% CI: [-0.17, -0.004].

Quasi-control sample comparisons. Results of chi-square analysis demonstrated that diverted youth were significantly less likely than quasi-control arrested sample youth to have had a recidivism arrest within the two years following their referring school-based diversion or arrest, $\chi^2 = 3.96$, $df = 1$, $p = .046$. Among youth with at least one recidivism arrest in the two-year follow-up period, diverted youth spent a longer time with no recidivism arrests than youth in the quasi-control sample; diverted youth: $M = 301.79$ days, $SD = 199.84$; arrested youth: $M = 274.72$ days; $SD = 203.60$; hazard ratio = 0.81, 95% CI [0.67, 0.98], $z = -2.21$, $p = .027$. Further, on average, diverted youth had fewer recidivism arrests ($M = 0.46$, $SD = 0.98$, range = 0–9) than did youth in the quasi-control sample ($M = 0.56$, $SD = 1.11$, range = 0–10) during the two-year follow-up period, however, this difference was not statistically significant, $B = -0.19$, $p = .074$, $d = -0.09$, 95% CI: [-0.19, 0.01].

Matched sample comparisons. Chi-square analysis with the youth samples generated via propensity score matching revealed no significant differences between diverted and arrested youth in the likelihood of experiencing a recidivism arrest during the 2-year follow-up period, χ^2 = 1.15, df = 1, p = .283. Among youth with at least one recidivism arrest during follow-up, we did not observe a significant difference in time to recidivism between the matched samples (hazard ratio = 1.13, 95% CI [0.91, 1.41], z = 1.11, p = .269). Further, Tweedie compound Poisson general linear models did not produce significant differences between the matched samples in terms of number of recidivism arrests in the two years after a referring school-based diversion or arrest, B = 0.07, p = .594, d = 0.03, 95% CI: [-0.09, 0.15].

Please see Goldstein, Kreimer, and colleagues (2021) for figures associated with these reported findings.

Long-Term Recidivism Outcomes

Given the longer follow-up period of five years, the quasi-experimental sample for long-term outcome analyses was limited to the one cohort of diverted youth for whom five years had passed since their initial incident and five years' worth of follow-up recidivism data were available (i.e., the 2014-2015 diverted cohort). Within five years of their referring school-based incident, 40% of youth in that cohort had experienced a recidivism arrest, compared with 52% of youth in the full arrested sample and 47% of youth in the quasi-control sample. A summary of additional long-term outcome analysis results is presented below.

Full arrested sample comparisons. Chi-square analysis comparing recidivism arrest (no, yes) between the diverted and full arrested samples revealed that youth in the diverted sample were significantly less likely than youth in the full arrested sample to have experienced a recidivism arrest in the five years following their school-based incident, χ^2 = 15.95, df = 1, p <

.001. Among youth with at least one recidivism arrest in the five-year follow-up period, diverted youth demonstrated a shorter mean time to recidivism arrest than youth in the full arrested sample (diverted youth: M = 563.74 days, SD = 451.48; arrested youth: M = 592.43 days, SD = 479.20; hazard ratio = 0.72, 95% CI: [0.60, 0.85], z = -3.79, p < .001). Additionally, on average, diverted youth had fewer recidivism arrests within five years (M = 1.13, SD = 2.00, range = 0–16) than did youth in the full arrested sample (M = 1.32, SD = 1.97, range = 0–17). However, Tweedie compound Poisson general linear models comparing number of arrests in the five years following a referring school-based incident did not reveal a significant difference between diverted youth and youth in the full arrested sample, B = -0.15, p = .100, d = -0.09, 95% CI: [-0.21, 0.02].

Quasi-control sample comparisons. Chi-square analysis comparing recidivism arrest (no, yes) between the diverted and quasi-control arrested samples revealed no significant difference in likelihood of a recidivism arrest in the five-year follow-up period, χ^2 = 3.77, df = 1, p = .052. Among youth with at least one recidivism arrest in the five years following the referring incident, diverted youth demonstrated a longer time to recidivism than youth in the quasi-control sample (diverted youth: M = 563.74 days, SD = 451.48; arrested youth: M = 558.11 days, SD = 476.47; hazard ratio = 0.82, 95% CI: [0.67, 1.00], z = -1.99, p = .047). Additionally, on average, diverted youth had fewer recidivism arrests within five years (M = 1.13, SD = 2.00, range = 0–16) than did youth in the quasi-control sample (M = 1.20, SD = 1.93, range = 0–12). However, Tweedie compound Poisson general linear models comparing number of arrests in the five-year follow-up period post-referring incident did not identify significant between-group differences, B = -0.06, p = .581, d = -0.04, 95% CI: [-0.16, 0.09].

Matched sample comparisons. Following propensity score matching, we another conducted chi-square analysis comparing recidivism arrest (no, yes) during the five-year follow-up period between our matched samples of diverted and arrested youth. Results revealed significant between-group differences, $\chi^2 = 4.10$, $df = 1$, $p = .043$, such that diverted youth were less likely than matched arrested youth to experience a recidivism arrest in the five years following their school-based incident. Among youth who did incur a recidivism arrest during the five-year follow-up period, there was a significant difference in time to recidivism between the matched samples (hazard ratio = 0.80, 95% CI: [0.65, 0.99], z = -2.02, $p = .043$), indicating a longer time to future arrest for diverted youth than for arrested youth following their school-based incident. Finally, Tweedie compound Poisson general linear models comparing number of recidivism arrests in the five years following an initiating incident did not identify significant differences between the matched samples, $B = -0.10$, $p = .397$, $d = -0.06$, 95% CI: [-0.20, 0.08].

Long-Term Recidivism Disparities

To address our third research question, asking whether the Diversion Program narrowed disparities based on race/ethnicity and special education status, we performed moderation analyses to determine whether the effects of diversion on each of our individual-level, long-term youth outcomes was moderated by either youths' race/ethnicity or their IEP status. We present results from the moderation analyses focused on long-term *recidivism* outcomes below.

Full arrested sample comparisons. We conducted logistic regression analyses including diverted/arrested status, dichotomized race/ethnicity (non-Black, Black), and their interaction term as predictors of experiencing a recidivism arrest within the five-year follow-up period. Results revealed no significant interaction between youths' diverted/arrested status and their race/ethnicity, OR = 1.64, 95% CI: [0.95, 2.89], $p = .081$. Similarly, logistic regression analyses

examining whether youths' IEP status (no, yes) moderated the relationship between diversion/arrest and long-term recidivism arrest outcomes revealed no significant interaction, OR = 1.16, 95% CI: [0.72, 1.88], p = .550. Using Cox proportional hazards regression, we examined whether youths' race/ethnicity or IEP status moderated the relationship between diversion/arrest and time to recidivism; results indicated no significant moderating effect for race/ethnicity (hazard ratio = 1.49, 95% CI: [0.95, 2.32], z = 1.74, p = .081) or for IEP status (hazard ratio = 1.17, 95% CI: [0.83, 1.65], z = 0.89, p = .375). Similarly, Tweedie compound Poisson general linear models comparing *number* of recidivism arrests within the five years following a referring school-based incident suggested no significant interactions between either diversion/arrest and race/ethnicity, B = 0.11, p = .635, or diversion/arrest and IEP status, B = 0.13, p = .497.

 Quasi-control arrested sample comparisons. Again, we conducted logistic regression analyses to investigate whether youths' race/ethnicity or special education status moderated the relationship between diversion/arrest and recidivism arrest during the five-year follow-up period. As with the full arrested sample, results indicated no significant interaction effect for either race/ethnicity, OR = 1.44, 95% CI: [0.78, 2.70], p = .250, or IEP status, OR = 1.08, 95% CI: [0.63, 1.87], p = .775, in the long-term follow-up period. Additionally, Cox proportional hazards regression analyses revealed no significant interactions between diversion/arrest and race/ethnicity (hazard ratio = 1.31, 95% CI: [0.79, 2.15], z = 1.04, p = .296) or between diversion/arrest and IEP status (hazard ratio = 1.08, 95% CI [0.73, 1.61], z = 0.40, p = .686) on time to recidivism. Finally, Tweedie compound Poisson general linear models comparing number of arrests in the five years following the referring school-based incident did not reveal a significant interaction between either diversion/arrest and race/ethnicity, B = -0.01, p = .967, or between diversion/arrest and youths' IEP status, B = 0.10, p = .671.

Matched sample comparisons. After using propensity score matching techniques to make our diverted and quasi-control arrested samples more equivalent on the basis of demographics and incident type, we conducted another set of logistic regression analyses to determine whether the relationship between diversion/arrest and recidivism arrest outcomes depended on race/ethnicity or IEP status. Results indicated that neither youth's race/ethnicity, OR = 1.10, 95% CI: [0.56, 2.17], p = .779, nor their IEP status, OR = 1.01, 95% CI: [0.55, 1.85], p = .985, significantly interacted with diversion/arrest in its relationship with recidivism arrest (no, yes) during the five years following a referring school-based incident. An additional Cox proportional hazards regression revealed no significant interactions between diversion/arrest and race/ethnicity (hazard ratio = 1.05, 95% CI: [0.61, 1.81], z = 0.19, p = .852) or between diversion/arrest and IEP status (hazard ratio = 1.01, 95% CI: [0.65, 1.57], z = 0.05, p = .962) in the relationship with time to recidivism arrest. Similarly, Tweedie compound Poisson general linear models comparing number of arrests during the 5-year follow up period did not reveal significant interactions between diversion/arrest and either youth's race/ethnicity, B = -0.17, p = .572, or their IEP status, B = 0.04, p = .883.

School Discipline Outcomes

Moderate-Term Discipline Outcomes

Findings of *moderate-term* outcome analyses related to youths' school discipline experiences were published by Goldstein, NeMoyer, and colleagues (in press)[8] and focused on comparisons between the matched diverted and arrested samples generated via propensity score matching to better account for potentially unobserved confounding variables. Descriptive results

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[8] As noted above, the Philadelphia Police Department and School District of Philadelphia provided additional data after Diversion Program publications cited in this document were released or in press. These updates led to minor data refinements. As a result, numbers presented in this report may differ slightly from those values reported in previous or in press publications.

showed that 68% of diverted youth and 72% of arrested youth were suspended as a result of the incident that led to either diversion or arrest. Additionally, 38% of diverted youth and 48% of arrested youth were suspended for a new incident within 365 days of their initial referring incident (i.e., post-incident suspension). Finally, 10% of diverted youth and 13% of arrested youth were referred for permanent school removal via expulsion or disciplinary transfer for a new incident within 365 days of their initial incident (i.e., post-incident referral for permanent school removal). See Goldstein, NeMoyer, and colleagues (in press) for a figure depicting these frequencies.

Incident-related suspensions. Results from an initial mixed-effects logistic regression analysis with no covariates did not identify a significant difference in the probability of incident-related suspension between the matched samples of diverted and arrested youth, $OR = 0.83$, 95% CI: [0.60, 1.15], $p = .264$. Incorporating covariates (i.e., age, gender, race/ethnicity, incident type, number of pre-incident suspensions) into the model did not reveal any significant relationships with incident-related suspension. Then, to examine whether youths' diversion year might be linked to their likelihood of incident-related suspension, we conducted a cohort analysis among our matched diverted youth sample. We did not observe a significant relationship between cohort year and incident-related suspension, $\chi^2 (df = 2) = 0.12$, $p = .941$.

Finally, we conducted moderation analyses to explore whether the examined covariates significantly interacted with youths' diverted or arrested status to influence the likelihood of incident-related suspension. Indeed, youths' age at diversion or arrest significantly moderated the relationship between diversion/arrest and incident-related suspension: as youths' age at diversion or arrest increased, so did the *difference* in likelihood of incident-related suspension between diverted and arrested youth, $OR = 0.84$, 95% CI: [0.71, 0.99], $p = .041$. Importantly, regardless

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of age, arrested youth consistently demonstrated a higher likelihood of incident-related suspension.

We also observed a significant moderating effect of gender, such that, for female students (but not male students), diversion or arrest was significantly linked to incident-related suspension: arrested girls were more likely than diverted girls to be suspended for their referring incident, OR = 0.32, 95% CI: [0.16, 0.63], p = .001. Further, among arrested youth, female students were significantly more likely than male students to be suspended for their referring incident, OR = 2.19, 95% CI: [1.31, 3.76], p = .003. No similar significant gender differences were observed among diverted youth, OR = 0.74, 95% CI: [0.47, 1.17], p = .198, nor were there significant differences between diverted and arrested boys, OR = 1.19, 95% CI: [0.80, 1.79], p = .385. We did not observe moderating effects for the remaining covariates (i.e., race/ethnicity, incident type, number of pre-incident suspensions).

Post-incident suspensions. An initial mixed-effects logistic regression analysis with no covariates indicated that diverted youth were less likely than arrested youth to have at least one post-incident suspension, OR = 0.67, 95% CI: [0.49, 0.91], p = .011. We then incorporated covariates (i.e., gender, race/ethnicity, age, incident type, number of pre-incident suspensions) into the model. Results revealed that age at diversion or arrest was significantly associated with post-incident suspension, OR = 0.81, 95% CI: [0.74, 0.88], p < .001, such that, as youth grew older in age at their school-based incident, they became less likely to receive a suspension in the year after that incident.

Additionally, race/ethnicity was significantly linked to post-incident suspension, OR = 1.84, 95% CI: [1.27, 2.68], p = .001, as Black youth were, on average, more likely to experience this school discipline outcome in the year following a diversion or arrest. Youths' history of

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suspensions in the year *prior* to diversion or arrest was also significantly associated with *post-incident* suspension: youth with no suspensions in the year before the referring incident were less likely than youth with at least two pre-incident suspensions to be suspended in the year following the examined school-based incident, OR = 2.74, 95% CI: [1.94, 3.88], $p < .001$. No other covariates demonstrated significant relationships with youths' likelihood of post-incident suspension. Of note, even when incorporating selected covariates into the analysis, we continued to observe a significant relationship between diversion and arrest and post-incident suspension, OR = 0.63, 95% CI: [0.46, 0.86], $p = .004$.

Moderation analyses with our selected covariates revealed no significant moderators of the relationship between diversion/arrest and post-incident suspension. Additionally, to examine whether youths' diversion year might be linked to their likelihood of post-incident suspension, we conducted a cohort analysis among our matched diverted youth sample. We did not observe a significant relationship between cohort year and post-incident suspension, $\chi^2(df = 2) = 3.78, p = .151$

Post-incident referral for school removal. Results from an initial mixed-effects logistic regression analysis with no covariates did not identify a significant difference in the probability of receiving a post-incident referral for expulsion or disciplinary transfer between the matched samples of diverted and arrested youth, OR = 0.65, 95% CI [0.42, 1.01], $p = .053$. After incorporating relevant covariates (i.e., gender, race/ethnicity, age, incident type, number of pre-incident suspensions) into the model, we found that youth with two or more pre-incident suspensions had a higher likelihood of post-incident referral for school removal than youth with no pre-incident suspensions, OR = 2.15, 95% CI [1.29, 2.67], $p = .004$. No other examined

covariates demonstrated significant relationships with post-incident referral for permanent school removal.

Importantly, after we incorporated all examined covariates into the analysis, youths' diverted or arrested status was significantly linked to a post-incident referral for permanent school removal, OR = 0.61, 95% CI [0.38, 0.95], p = .028. It appears that controlling for these covariates improved our ability to detect the effect of diverting youth in lieu of arrest for school-based incidents. The observed relationship suggested that diverted youth were less likely to receive a post-incident referral for permanent school removal than arrested youth when covarying these demographic variables, incident type, and suspension history. None of our selected covariates significantly influenced the relationship between diversion/arrest and likelihood of post-incident referral for permanent school removal.

Finally, when examining whether youths' diversion year was linked to their likelihood of post-incident referral for school removal, our cohort analysis among the matched diverted sample identified a significant relationship, $\chi^2(df = 2) = 19.93$, $p < .001$. Further investigation revealed that, although youth diverted in the 2014-2015 and 2015-2016 school years demonstrated a similar number of post-incident referrals for permanent school removal (i.e., 21 and 22 students per year, respectively), only one student diverted in 2016-2017 received a referral for permanent school-removal.

See Goldstein, NeMoyer, and colleagues (in press) for figures and tables associated with these reported findings.

Long-Term Discipline Outcomes

When examining long-term school discipline outcomes, we limited our quasi-experimental sample to the cohort of diverted youth (i.e., the 2014-2015 diverted cohort), for

whom four years' worth of follow-up school data were available. In brief, 52% of youth in the quasi-experimental diverted sample received a suspension in the four years after their referring incident, compared to 55% of youth in the quasi-control sample. An initial mixed effect logistic regression analysis without covariates did not indicate any significant between-group differences in the probability of a post-incident suspension between diverted youth and youth in the quasi-control arrested sample, $OR = 0.85$, 95% CI: [0.61, 1.17], $p = .314$.

Matched sample comparisons. After using propensity score matching techniques to make our quasi-experimental diverted and quasi-control arrested samples more equivalent in demographics and pre-incident suspension experiences, we conducted another mixed-effects logistic regression analysis without covariates. Results indicated significant between-group differences in the likelihood of post-incident suspension, $OR = 0.64$, 95% CI: [0.42, 0.96], $p = .034$, with diverted youth significantly less likely than arrested youth to receive a suspension during the four-year period following their referring school-based incident.

We then incorporated covariates (i.e., gender, race/ethnicity, age, incident type, number of pre-incident suspensions) into the model. Results revealed that youths' age at diversion or arrest, race/ethnicity, and pre-incident suspension history were all significantly linked to this outcome. Specifically, the older in age youth were at the time of their school-based incident, the *less* likely they were to receive a suspension in the four years following that incident, $OR = 0.61$, 95% CI: [0.52, 0.72], $p < .001$. Additionally, race/ethnicity was significantly linked to post-incident suspension, with Black youth, on average, more likely to experience a suspension in the four years following a school-based diversion or arrest, $OR = 2.38$, 95% CI: [1.38, 4.25], $p < .001$. A history of suspension in the year *prior* to diversion or arrest was also significantly associated with *post*-incident suspension: youth with no suspensions in the year before the

referring incident were less likely than both youth with one pre-incident suspension, OR = 2.84, 95% CI: [1.56, 5.26], p = .001, and youth with at least two pre-incident suspensions, OR = 3.81, 95% CI: [2.26, 6.54], p < .001, to be suspended in the four years following the examined school-based incident.

Importantly, after we incorporated all examined covariates into the analysis, youths' diverted or arrested status was no longer significantly linked to their likelihood of suspension in the four years following their school-based incident, OR = 0. 67, 95% CI [0.42, 1.04], p = .078. Additionally, examined covariates did not significantly moderate the relationship between diversion/arrest and likelihood of long-term post-incident suspension.

Long-Term Discipline Disparities

To address our third research question, asking whether the Diversion Program narrowed disparities based on race/ethnicity and special education status, we performed moderation analyses to determine whether the effects of diversion on *long-term school discipline* outcomes varied with either youths' race/ethnicity or their IEP status. As with other long-term academic outcomes, for these analyses, we examined a four-year follow-up period and limited our quasi-experimental sample to the cohort of youth diverted in the 2014-2015 academic year (i.e., the only group of diverted youth for whom sufficient time had passed to generate four years of follow-up data).

Quasi-control comparisons. We conducted mixed-effects logistic regression analyses including youths' diversion/arrest, dichotomized race/ethnicity (non-Black, Black), and their interaction term as predictors of receiving a suspension in the four years after a referring school-based incident. Results did not suggest that youths' race/ethnicity moderated the relationship between diversion/arrest and long-term post-incident suspension, OR = 0.75, 95% CI: [0.36,

1.54], $p = .426$. Similarly, using mixed-effects logistic regression analyses to examine whether youths' IEP status (no, yes) moderated the relationship between diversion/arrest and long-term post-incident suspension, results revealed no significant interaction, OR = 0.72, 95% CI: [0.37, 1.42], $p = .341$.

Matched sample comparisons. After creating more equivalent comparison groups through propensity score matching, we again conducted mixed-effects logistic regression analyses with the matched samples of diverted and arrested youth. The first analysis included youths' diversion/arrest, dichotomized race/ethnicity (non-Black, Black), and their interaction term as predictors of receiving a suspension in the four years after a referring school-based incident. Results did not suggest that youths' race/ethnicity moderated the relationship between diversion/arrest and long-term post-incident suspension, OR = 0.70, 95% CI: [0.22, 2.21], $p = .542$. Similarly, using mixed-effects logistic regression analyses to examine whether youths' IEP status (no, yes) moderated the relationship between diversion/arrest and long-term post-incident suspension, results revealed no significant interaction, OR = 0.66, 95% CI: [0.25, 1.74], $p = .405$.

Academic Achievement Outcomes

When considering the potential effect of the Diversion Program on youths' academic achievement, we focused our inquiries on two relevant outcomes: school dropout and on-time graduation (i.e., graduating from school within 4 years of entering 9th grade). We also, once again, limited our quasi-experimental sample to the cohort of diverted youth (i.e., the 2014-2015 diverted cohort), for whom four years' worth of follow-up school data were available.

Long-Term Academic Outcomes

Descriptively, 33% of youth in the diverted sample dropped out of school in the four years after their referring incident, compared with 29% of youth in the quasi-control arrested

sample. Additionally, 40% of youth in both the diverted sample and the quasi-control arrested sample graduated on time within four years of their referring school-based incident.

School dropout. An initial mixed-effects logistic regression analysis with no covariates indicated a significant difference in likelihood of long-term school dropout between diverted youth and youth in the quasi-control sample, OR = 1.37, 95% CI: [1.00, 1.87], p = .047, with diverted youth more likely than arrested youth to drop out of school in the four years following their referring school-based incident. However, after using propensity score matching techniques to make our samples more equivalent in demographic characteristics and IEP status, results of another mixed-effects logistic regression analysis without covariates did *not* indicate significant between-group differences in the likelihood of school dropout during the four-year follow-up period, OR = 1.34, 95% CI: [0.96, 1.86], p = .084.

On-time graduation. An initial logistic regression analysis with no covariates indicated no significant differences in likelihood of long-term school dropout between diverted youth and youth in the quasi-control sample, OR = 0.97, 95% CI: [0.73, 1.29], p = .838. Then, after using propensity score matching techniques to make our quasi-experimental diverted and quasi-control arrested samples more equivalent in demographic characteristics and IEP status, we conducted another logistic regression analysis without covariates. Results did not indicate significant between-group differences in the likelihood of on-time graduation during the four-year follow-up period, OR = 1.04, 95% CI: [0.76, 1.44], p = .789.

Long-Term Academic Disparities

To address our research questions regarding disparities, we performed moderation analyses to determine whether the effects of diversion on *long-term academic* outcomes varied with either youths' race/ethnicity or their IEP status. As with other long-term academic

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outcomes, for these analyses, we examined a four-year follow-up period and limited our quasi-experimental sample to the cohort of youth diverted in the 2014-2015 academic year (i.e., the only group of diverted youth for whom four years of follow-up data were available). Additionally, these analyses focused on comparisons between the matched diverted and arrested samples generated via propensity score matching to better account for potentially unobserved confounding variables.

School dropout. We conducted a mixed-effects logistic regression analysis including youths' diversion/arrest, dichotomized race/ethnicity (non-Black, Black), and the interaction term of these two variables as predictors of school dropout in the four years after a referring school-based incident. Results did not suggest that youths' race/ethnicity moderated the relationship between diversion/arrest and long-term school dropout, $OR = 1.38$, 95% CI: [0.66, 2.88], $p = .396$. Similarly, using mixed-effects logistic regression analyses, youths' IEP status (no, yes) did not significantly moderate the relationship between diversion/arrest and long-term school dropout, $OR = 1.20$, 95% CI: [0.60, 2.39], $p = .608$.

On-time graduation. To examine students' likelihood of on-time graduation (i.e., graduating within 4 years of ninth grade) in the four-year follow-up period, we conducted logistic regression analyses with the matched samples of diverted and arrested youth. First, we included youths' diversion/arrest, their dichotomized race/ethnicity (non-Black, Black), and the interaction term between these two variables as predictors of long-term on-time graduation. Results did not suggest that youths' race/ethnicity moderated the relationship between diversion/arrest and on-time graduation, $OR = 0.74$, 95% CI: [0.36, 1.51], $p = .411$. Similarly, youths' IEP status (no, yes) did not significantly moderate the relationship between diversion/arrest and on-time graduation, $OR = 0.89$, 95% CI: [0.48, 1.65], $p = .711$.

Discussion

Implications of Findings

Program-related results of this evaluation suggest that the Diversion Program successfully met its primary goal of substantially reducing the number of school-based arrests across Philadelphia without endangering school safety. These findings offer promise that a reform grounded largely in a specific policy change—namely, that police officers will not arrest students with no delinquency history for certain school-based behaviors—can safely and substantially contribute to dismantling the school-to-prison pipeline. Such a policy-based change required few new resources (e.g., personnel, programs) and contrasts substantially with more typical diversion programs that often require intensive intervention programming or the addition of many new staff members (Tallon et al., 2017). The Diversion Program's 89% service referral acceptance rate among contacted families—for *voluntary* community-based services—is higher than compliance rates typically observed for mandated juvenile probation conditions (e.g., NeMoyer et al., 2014; Vidal & Woolard, 2017), suggesting that providing youth and family with opportunities for services and supports to meet their needs at first indication of system contact may be more productive than requiring participation by threatening future justice involvement for noncompliance. Of course, participation rates between voluntary and mandated services should be compared in future research, but for now, these results suggest that youth and families are very willing to accept local, no-cost service referrals when given the choice.

By reducing the number of arrests in schools, the Diversion Program prevents many youth from obtaining a criminal justice record that can follow them throughout adolescence and into adulthood, interfering with higher education and employment opportunities, precluding military service for many, and excluding them from public housing (e.g., Boucai, 2007;

Cauffman et al., 2020; Johnson, 2015). More immediately, widespread diversion from arrest will reduce the number of young people who experience the stress and trauma of the arrest and booking process—not to mention of further justice system processing, including the potential for detention, court hearings, and post-adjudication placement. Additionally, diverting in lieu of arrest can help maintain youths' relationships with peers and adults at school, which serve as a critical foundation for students' engagement and success at school (e.g., Allen et al., 2018; Hirschfield & Gasper, 2011; Quin, 2017). More specifically, diverting youth from arrest can prevent the stigma, "delinquent" labeling, and negative attitudes often observed among peers and school personnel who have witnessed or learned about a student's school-based arrest (Cox et al., 2012; Mears et al., 2016; Sullivan et al., 2010). By maintaining these important relationships with prosocial peers and adults in school while avoiding extended time with young people in the justice system, diverted youth may be more likely to follow a typical adolescent trajectory that leads to desistance from offending behavior and keeps them at school and in the community.

Diversion Program outcomes should also provide indirect benefits to the school environment and the greater community. As an example, reducing school-based arrests should also reduce the number of disruptions in school generated by police arresting students, the number of students removed from school, and the risk for antagonistic relationships between students and school police. Additionally, when fewer youth are arrested and managed through the justice system, public agencies spend less money on court processing, long-term supervision, and facility placements that may have been associated with an arrest. In several states and many counties, agencies can then reinvest taxpayer dollars into more preventative programming and initiatives.

Recidivism Outcomes

Overall, diverted youth had a lower recidivism arrest rate throughout the two- and five-year follow-up periods, with the most notable difference in recidivism arrest likelihood between the *matched* diverted and arrested samples in the five-year follow-up period. Of note, compared to youth in the full arrested sample, diverted youth unexpectedly demonstrated a *shorter* time to recidivism arrest; in contrast, compared to youth in the quasi-control sample, diverted youth demonstrated a *longer* time to recidivism arrest. These diverging findings may reflect the delinquent histories among some youth in the full arrested sample or more serious nature of the charges levied against these youth. Thus, many of the young people in the full arrested sample may have been detained, adjudicated, and placed for their offenses, thereby restricting their opportunities to incur a subsequent arrest in the community. In contrast, we limited the quasi-control sample to those arrested youth who would have been eligible for the Diversion Program if it had existed at the time of their school-based offenses. These youth could not have had a delinquency history or open court case, and they could only have been charged with specified summary or misdemeanor offenses, decreasing the likelihood that they would have been removed from the community during processing for those charges.

Beyond differences in recidivism arrests, it is also important to highlight that *all students* in the quasi-control arrested sample were burdened with an arrest record as a result of their referring school-based incident—even the majority of youth in the group who did not go on to receive a subsequent arrest in the five years following their initial incident. As a result, all youth in that sample were at risk of, and likely experienced, many negative collateral consequences of such a record (e.g., limits on housing, education, and employment; costly fines and fees). In contrast, just 40% of diverted youth acquired an arrest record within five years of their referring incident, thereby allowing 60% of the sample to maintain a more typical adolescent trajectory.

Discipline Outcomes

In addition to its primary goal of reducing school-based arrests, the Diversion Program was designed to "keep youth in school" (Goldstein, NeMoyer, et al., 2021). We hypothesized that diverted youth would be less likely than arrested youth to experience exclusionary school discipline in the short term (i.e., directly for the incident that led to diversion or arrest), in the moderate term (i.e., in the year following a referring incident), and in the long term (i.e., in the four years following a referring incident). Given that the Diversion Program represents a change to *police* policy—with schools maintaining their discretionary autonomy for school-related discipline—it may not be surprising that diverted and arrested youth did not differ significantly in their rates of incident-related suspension. School personnel who call police in response to a student's behavior may be inclined to impose additional discipline on that student, such as an out-of-school suspension, regardless of whether the police referral resulted in diversion or arrest. Of note, during development of the Diversion Program, stakeholders wondered whether school staff would potentially utilize *more* exclusionary discipline with diverted students in an effort to ensure accountability without an arrest. This concern did not come to fruition; diverted youth demonstrated a descriptively, though not significantly, lower rate of incident-related suspension.

Although likelihood of incident-related suspension did not differ significantly between diverted and arrested youth, the strength of the relationship between youths' diversion/arrest and likelihood of incident-related suspension depended on youths' age at the time of incident and on youths' gender. Specifically, diversion in lieu of arrest appears to be most beneficial for older youth and for female students. It may be that elementary and middle schools employ more consistent discipline approaches, regardless of whether students' behaviors result in diversion or arrest, whereas personnel at the high school level have greater discretion and may be more

inclined to remove arrested students from school for their misbehaviors, perhaps because school decision makers view older, arrested students as greater risks of danger to others in the school environment (Curran et al., 2019; Watson & Stevenson, 2020). Additionally, girls may disproportionately benefit from diversion with respect to incident-related suspension because being arrested violates perceived gender norms, a phenomenon that results in particularly harsh punishments (Moore & Padavic, 2010; Spivak et al., 2014). Although our sample size distribution precluded the ability to examine gender and race as simultaneous moderators, Black girls are most frequently punished for these types of norm-violating behaviors (Annamma et al., 2019; Morris & Perry, 2017)—an important disparity given that Philadelphia public schools, and public schools in many other urban districts, consist of predominantly Black students.

Diverted youth were significantly less likely than matched arrested youth to be suspended in the one year following their referring school-based incident. By avoiding an arrest, diverted youth may have been better able to stay in school and maintain prosocial relationships within their educational environment, as peers, teachers, and school administrators may have been less likely to perceive youth as troublemakers (Bernburg et al., 2006; Kirk & Sampson, 2013), and school personnel may subsequently have been more lenient in response to future misbehavior (Wolf & Kupchik, 2017). Importantly, though diversion/arrest was *initially* significantly linked to post-incident suspension over the four-year follow-up period, once we compared the matched samples of diverted and arrested youth, we no longer observed a significant relationship. This change may reflect the nuanced nature of the relationships between and among youths' demographic factors and IEP status, police contact, and exclusionary school discipline experiences. Additionally, it may reflect the smaller sample size (i.e., the quasi-control sample and only one cohort of diverted youth) included in our long-term analyses, given the need for

four years of follow-up data. Additional longitudinal Diversion Program data collection and analysis may be able to provide a larger sample to investigate the Diversion Program's ability to keep youth in prosocial environments over time and serve not only as a "second chance" for justice system involvement but also as a "second chance" to stay on typical trajectories in school settings.

Other factors linked to youths' moderate- and long-term suspension included age at incident, race/ethnicity, and history of pre-incident suspension. Older youth at the time of a referring incident were more likely to experience future suspension, perhaps reflecting perceptions among school personnel that certain behaviors (e.g., marijuana possession, disorderly conduct) are normative for older youth, but more concerning for younger youth; norm violations for other demographic characteristics have historically been associated with more punitive responses in a variety of contexts (e.g., Moore & Padavic, 2010; Spivak et al., 2014). Additionally, racial/ethnic differences in suspension outcomes over time may reflect some combination of differential rates of *referral* for discipline and differential administrator *responses* to discipline referrals, likely stemming from implicit biases (e.g., Skiba et al., 2011). Finally, the link between previous suspension history and subsequent suspensions aligns with extant research highlighting that any instance of suspension increases the likelihood of future exclusionary discipline (Heitzeg, 2014; Mittleman, 2018). Youth with a history of suspensions— especially more than one—may receive a "troublemaker" label from school personnel, regardless of whether they were diverted or arrested, which likely decreases the chances that teachers or other school personnel will offer leniency for future misbehavior.

Academic Achievement

Although we initially found significantly higher rates of school dropout among diverted youth than quasi-control arrested youth, we did not observe a significant relationship between the *matched* diverted and arrested samples and either school dropout or on-time graduation during a four-year follow-up period. It is possible that some youth in the quasi-control arrested sample who were not included in the matched arrested sample were disproportionately at risk for confinement following their school-based arrest. Given that confined youth typically attend school in their long-term placement facilities—most of which are outside of Philadelphia—such youth would likely be listed in SDP records as having been transferred from the district rather than having dropped out. Future research that includes court data would help to refine arrested youth samples to better examine these long-term academic achievement outcomes.

Despite this sampling nuance, the *graduation rate* among diverted and arrested students did not differ significantly. Importantly, the 40% graduation rate for *both* diverted and arrested students was dramatically lower than the graduation rate for the general SDP population, which averages approximately 69% when including students attending alternative schools, and 78% when not including alternative school students (Erden-Akcay, 2020). It may be that youth in both the diverted and arrested samples already had similarly elevated risks for school dropout and not graduating on time, given similarities in factors such as academic performance, residence in disadvantaged neighborhoods, history of behavioral health disorders, challenges with familial relationships, and tendency to engage in delinquent behavior (Gubbels et al., 2019; Pharris-Ciurej et al., 2012; Wodtke et al., 2011). Thus, these youths' diversion or arrest experiences may not play a major incremental role in their long-term school enrollment and academic achievement. Perhaps, rather than serving as a protective factor against school dropout and/or a facilitator of on-time graduation, school-based diversion might serve as a potential indicator for

school personnel about youth who could benefit from additional, school-based supports aimed at preventing dropout and promoting on-time graduation. Importantly, although the Diversion Program was developed to link diverted students and their families to relevant and helpful services, those services are *community-based*, offered through the Philadelphia Department of Human Services, and identified based on a broad assessment of youth and family needs, which may or may not include youths' risk for school dropout and academic challenges that interfere with on-time graduation.

Addressing Disparities

Given consistently observed racial/ethnic and IEP-based disparities in justice system, school discipline, and academic achievement outcomes, Diversion Program developers expressed hope that diverting youth in lieu of arrest for school-based offenses in a predominantly Black school district might help reduce those disparities. Results of our moderation analyses did not suggest that the Diversion Program disproportionately *improved* police and school outcomes for Black youth or youth enrolled in special education. However, the program appears to substantially reduce the rates of arrest, recidivism, and exclusionary discipline for all youth. In that way, the Philadelphia Police School Diversion Program sits apart from many other diversion initiatives, which often disproportionately benefit White youth (Annie E. Casey Foundation, 2020). We also recognize that moderation effects of race/ethnicity may have been obscured by our need to dichotomize the potential moderator into Black and non-Black labels to preserve power. Collapsing several different racial/ethnic categories (e.g., White, Hispanic, Asian/Asian American, more than one race/ethnicity) into one likely increased variability and limited our ability to observe significant effects. Future work in this and other jurisdictions may be better

able to investigate the ways in which the Diversion Program—or a similar initiative—may work to reduce existing racial/ethnic disparities in justice system and school-related outcomes.

Policy and Practice Recommendations

Results of our evaluation of the Philadelphia Police School Diversion Program can inform policy and practice recommendations in two areas: 1) quality improvement and sustainability of the existing initiative and 2) potential program replication in new jurisdictions. First, given overall findings that the Diversion Program accomplished its primary goals of safely reducing rates of school-based arrests and linking youth and families with community-based services, program administrators may leverage study findings to engender support for their continued efforts. Second, based on the results of this evaluation, stakeholders from the police department and school district might work collaboratively to identify new avenues for accomplishing Diversion Program goals. For example, given that diverted youth were not significantly less likely than arrested youth to be suspended for the incident that led to police referral, school administrators may wish to focus training efforts and policy changes on encouraging school staff to use alternatives to suspension (e.g., restorative justice practices) in response to diversion-eligible behaviors. This suggestion may be particularly timely, as the use of restorative justice practices has considerably increased across the SDP in recent years. Additionally, given that diverted youth demonstrated poorer on-time graduation outcomes than the general SDP student population, Diversion Program administrators might work with school leadership and personnel to develop procedures for linking youth to school-based academic supports as part of the diversion process. In this way, though diversion itself may not serve as a protective factor against dropout and for on-time graduation, it *could* serve as a meaningful intervention point to foster students' progress toward academic success.

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Additionally, results of this evaluation can provide critical information for jurisdictions seeking to replicate the Diversion Program in their own schools and communities. For example, among the many U.S. jurisdictions that have inquired about the Diversion Program, replication efforts have been limited. It may be that some localities have reservations about instituting widespread diversion in lieu of school-based arrest for fear of potentially compromising school and/or community safety. However, results of the current evaluation suggest that, after Diversion Program implementation in Philadelphia, district-wide metrics of school safety and exclusionary discipline improved, as did several individual youth outcomes. Further, the fact that diverted youth demonstrated a significantly *lower* rate of recidivism arrest than similar arrested youth in the five years following an initial school-based incident indicates that community safety was not compromised—and may have been improved—in the aftermath of Diversion Program implementation. Thus, these empirical findings should promote the diffusion of this innovation to new areas of the country.

Importantly, though jurisdictions seeking to replicate the Diversion Program will likely adapt it to best fit their local contexts, there are several critical components of the program that should be retained. First, similar school-based diversion programs should prescribe the automatic diversion of all youth that meet established eligibility criteria—this program element removes the opportunity for implicit bias to influence police decisions about which youth to divert. Second, a *true diversion* model (Wilson & Hoge, 2013) should be used; once youth are diverted, their cases should be closed and they should not be expected to complete any requirements as part of the diversion process. Although an important component of the Philadelphia Police Diversion Program involves connecting youth and their families to community-based services for identified needs, any replication efforts that include similar services should offer them on a

strictly voluntary basis—youth should not be sanctioned in any way if they choose not to participate. The true diversion approach of Philadelphia's Diversion Program avoids noncompliance via *legal* behaviors (e.g., not attending counseling sessions)—or technical violations, in juvenile justice terms—leading to unnecessary justice involvement and its accompanying collateral consequences. Third, given the interdisciplinary nature of the Diversion Program, it will be crucial for stakeholders from multiple public agencies (e.g., police departments, school districts, social service providers) within a given jurisdiction to work collaboratively to develop and implement a local iteration of the Police School Diversion Program. See Goldstein and colleagues (2019) for a detailed description of the development process of the Philadelphia Police School Diversion Program.

Limitations

We recognize several study limitations. For example, because the Diversion Program was implemented across all Philadelphia schools simultaneously, we were unable to conduct a randomized controlled trial (RCT), often considered the gold standard for assessing intervention effectiveness. However, given current understanding of the negative consequences of arrest, randomly assigning youth and/or schools to "diversion" or "arrest" conditions would raise serious ethical concerns. Instead, we developed a quasi-control sample of youth arrested in the year before the Diversion Program who would have been eligible for diversion if it had been available. We also utilized statistical techniques to further equalize our samples to improve the accuracy and meaningfulness of comparisons, and we examined cohort-based differences to distinguish between effects of diversion/arrest status from period effects in outcomes over time. Additionally, this evaluation focused on a diversion program developed and implemented within one city, in conjunction with one police department and one school district—a fact which may

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raise concerns about generalizability of findings. However, Philadelphia is the country's sixth largest city, and the SDP is the eighth largest urban school district in the United States (School District of Philadelphia, 2019), both of which have populations similar to those of other major cities and school districts in the country.

Additional limitations include the broad categories we used to distinguish youths' offense type and race/ethnicity, which we did to preserve power for our analyses, and the absence of data we would have liked to incorporate into the analyses, including potentially relevant covariates (e.g., socioeconomic status) and related outcomes (e.g., incident-related referral for permanent school removal, self-reported future offending behaviors). Although we were somewhat restricted by a reliance on administrative data, typically, the same can be said for police and school stakeholders who might wish to implement and evaluate the Diversion Program in their own jurisdictions, increasing the external validity of our procedures.

Future Research

Beyond addressing identified limitations, future work related to our evaluation of the Philadelphia Police School Diversion Program might include examining additional, potentially relevant youth outcomes that could be influenced by diversion in lieu of arrest. For example, poor health outcomes have frequently been observed among justice-involved youth; however, it is unclear whether diversion at such an early stage of juvenile justice processing, compared to arrest, could contribute to improved health-related outcomes for youth. Additionally, if diverted or arrested students experience a recidivism arrest after their referring incident, examining court processing outcomes for those future arrests may allow investigators to determine whether diversion reduces the effects of cumulative disadvantage for Black youth in the justice system.

Further, longitudinal tracking of youth in our diverted and arrested samples over even longer periods of time would allow us to map out and compare trajectories of offending behavior and arrest, identifying whether true diversion acts as a turning point *away* from future delinquency and possibly illuminating malleable risk and protective factors that may influence the effects of diversion in lieu of arrest. Finally, although results of the current evaluation suggest that diverted youth are less likely than arrested youth to be suspended from school in years following their referring incident, it will be important to elicit feedback directly from diverted students about how they perceive their school climate and whether their perceptions are impacted by their interactions with school personnel during and after the incident that led to their diversion.

Conclusions

Over the course of four years, our evaluation of the Philadelphia Police School Diversion Program has produced several important findings. First, the Diversion Program appears to be meeting its primary goals, reducing the number of school-based arrests in Philadelphia without increasing the number of serious behavioral incidents in city schools, and referring diverted youth and their families to voluntary, community-based services. Additional findings indicated that school-based diversion appears to help students avoid subsequent out-of-school suspensions and recidivism arrests over the long term; further, diversion does not appear to change—for better or for worse—youths' risks for school dropout or on-time graduation compared to school-based arrest. However, by protecting diverted youth from incurring an arrest record, the Diversion Program protects diverted youth from the considerable negative collateral consequences of such a record. Results of this evaluation can be used to improve and sustain the

Diversion Program in Philadelphia and to lay the foundation for replication efforts in other

jurisdictions across the country.

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