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Schools as places of crime? Evidence from closing chronically underperforming schools☆

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ABSTRACT

We leverage the closing of chronically underperforming public schools in Philadelphia to estimate their impact on neighborhood crime. Employing a difference-in-differences strategy comparing monthly crime in blocks where school buildings closed to blocks where schools remained open or were never located, we find significant and substantive declines in crime following school closure. The decline in crime is driven by reductions in violent crime, is concentrated in blocks where high schools closed, during weekday hours when schools would have been in session, and is greatest in the blocks where more students exited following closures. While crime increased in blocks that enrolled a larger share of students displaced due to closures, the displacement of crime was significantly smaller in magnitude than the total crime reduction. These results suggest that closing schools with high rates of student misconduct and low educational performance led to a net reduction in crime in Philadelphia.

1. Introduction

Large public schools have long been considered locations of crime. Several facts support this claim. Criminal activity peaks in adolescence when most youth are still enrolled in secondary schools (Farrington, 1986). Crime rates are higher when schools are in session (Billings and Phillips, 2017; Cook et al., 2010; Jacob and Lefgren, 2003). And neighborhoods where schools are located have higher rates of crime during the daytime when students are present (Roncek and Faggianai, 1985; Roman, 2004; Weisburd et al., 2012). Yet, existing research is largely silent on the extent to which the presence of academically underperforming schools generates crime in surrounding neighborhoods.

From a theoretical perspective, the impact of schools on neighborhood crime is ambiguous. According to the influential routine activities theory (Cohen and Felson, 1979), most "criminal acts require convergence in space and time of *likely offenders*, *suitable targets* and the *absence of capable guardians* against crime" (p.88). It follows that the presence of students in school buildings could increase neighborhood crime by generating more potential victims to target by motivated offenders. The neighborhood effects literature also links the presence of schools to crime as a form of land use. Social interaction models suggest that crime rates will be higher in neighborhoods where schools are located, as they would produce more opportunities for peers to interact, and the criminal propensity of one youth to affect "his neighbor's decision to enter a life of crime" (Glaeser et al., 1996, p. 509). On the other hand, schools could reduce crime in neighborhoods if school officials (teachers, administrators, and school police officers) serve as capable guardians and offset the increase in likely offenders and unguarded victims in school settings. Also, schools may reduce crime in neighborhoods if their presence produces more positive socialization and collective efficacy among neighbors (Sampson and Raudenbush, 1999).

In this paper, we fill this important gap in the empirical literature by estimating the effect of academically underperforming schools on neighborhood crime. To do so, we leverage a district-level policy in Philadelphia which mandated the closing of 29 public schools at the end of the 2011–12 and 2012-13 school years – representing more than 10% of all public schools in Philadelphia (Steinberg and MacDonald, 2019). The closed schools in Philadelphia were among the lowest-performing,

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most under-enrolled, and served students with significantly worse behavior (truancy, violent misconduct and incidents involving law enforcement) than districtwide averages. Relying on a unique dataset that matches crimes to school locations and student enrollment patterns, we estimate the effect of underperforming schools on crime by comparing changes in monthly crime in block groups where schools closed to those where schools remained open or where schools were never located. We then examine whether any estimated change in monthly crime varied by the grade level of the closed schools, the hours that students attend schools (i.e., weekdays, 9am to 5pm), and changes in student enrollment following school closure. Finally, we estimate the displacement of crime following school closures by examining changes in crime in census block groups that received students from the recently closed schools. Doing so enables an assessment of whether the impact of closing underperforming schools on crime is attributable to the land use or the composition of students in school buildings.

We find that school closures led to a 15% decline in total crime and a 30% reduction in violent crime. The decline in violent crime is more precisely estimated, of which assaults are the most common type of violent offense. Further, the decline in crime is concentrated among neighborhoods where high schools closed, during school hours (weekdays, 9am-5pm), and in blocks that experienced the greatest decline in student enrollment following closure. These results suggest that a reduction in the presence of low-performing students with more problem behaviors following closures, rather than the land use effect of vacant school buildings, was a primary factor in the reduction in crime. We also find that crime increased more in areas that received a larger share of students displaced due to school closings. The displacement of crime to new blocks containing schools, however, is significantly smaller in magnitude than the reduction in crime from closing schools.

Our findings are consistent with social interaction models and routine activities theory and indicate that schools with higher rates of student misconduct and lower educational performance are generators of crime in neighborhoods. From a broader policy perspective, the results from this study are consistent with other research (Billings et al., 2019) suggesting that concentrating economically and educationally disadvantaged students together in the same schools increases crime in neighborhoods.

2. Related literature

A growing literature in urban economics and regional science examines the impact of various land uses on crime. Several papers show that the vacancy of commercial establishments or homes increases crime in neighborhoods (Chang and Jacobson, 2017; Ellen et al., 2013; Cui and Walsh, 2015). Research also finds that the demolition of large public housing complexes reduces crime (Aliprantis and Hartely, 2015; Sandler, 2017), and that the demolition of single-family public housing has either no effect on crime or reduces property crime (Spader et al., 2016). There is also evidence that the presence of alcohol outlets generate crimes in neighborhoods, whereas other forms of commercial establishments reduce crime (Han et al., 2016; Twinam, 2017). Existing evidence relies on the timing of changes in different forms of land use as a method for estimating causal effects on crime. Findings from these studies suggest that changes in the population size or social interactions, which are caused by changes in land use, impact crime by influencing the number of potential offenders or the surveillance of non-criminal bystanders to thwart crime.

Criminologists have argued that schools are also a form of land use associated with higher rates of crime in neighborhoods (see Weisburd et al., 2012, for a review). However, the criminology literature on the role of schools in neighborhoods is mostly cross sectional and provides little attention to the endogeneity of school location. The few studies that examine changes in crime in neighborhoods after schools close or open provide conflicting evidence. Brinig and Garnett (2009) find that closing neighborhood Catholic schools in Chicago was associated with an increase in crime. MacDonald et al. (2018) find that the opening of charter schools in Philadelphia has no association with changes in crime in neighborhoods compared to neighborhoods that never had schools, suggesting that adding a school to an area produces little meaningful change in neighborhood crime. While these two papers add insight into the effect of the presence of schools on neighborhood crime, they provide conflicting evidence on two different types of schools as measures of land use. Brinig and Garnett (2009) examine the effect of the presence of parochial schools on neighborhood crime, and MacDonald et al. (2018) examine the effect of opening new public charter schools on neighborhood crime. In both cases, the types of schools do not serve the lowest-achieving and most disadvantaged students with disproportionately high rates of problem behaviors, potentially an important source of crime generation in neighborhoods surrounding schools.

Various theoretical mechanisms explain why the presence of schools in a neighborhood may be related to crime. Criminal offending and victimization rates peak in the population during teenage years when most youth are still attending secondary schools (Farrington, 1986). Situational crime prevention, routine activities theory, and opportunity theory offer complementary theoretical explanations for why the presence of schools may be related to neighborhood crime. Routine activities theory and situational crime prevention both predict that schools could generate more crime in an area, as the presence of students in buildings and their travel to and from schools may facilitate more interactions between potential offenders and victims (MacDonald, 2015). Criminal opportunity theory would predict that the presence of a school could reduce the opportunity costs of crime (e.g., search costs and risk of detection) by congregating victims and offenders in places near schools (Cook, 1986). On the other hand, these theories could also predict that the presence of schools might reduce crime in neighborhoods. If the presence of teachers, administrators, and police in neighborhoods with schools facilitates effective place-guardians, then schools may reduce crime in neighborhoods by increasing surveillance and the opportunity costs of criminal offending.

The neighborhood effects literature suggests that schools as a form of land use might increase crime in neighborhoods. Glaeser et al. (1996) social interactions model, for example, predicts higher crime in neighborhoods with schools, since the presence of schools would increase the potential for criminal offenders to influence peers. The presence of schools in neighborhoods may then be an important contributor to explaining the high variance in crime across locations in cities. Schools may also generate crime in neighborhoods if their presence leads to more unsupervised youth hanging out around school properties and contributes to fewer opportunities for positive socialization. On the other hand, crime could be lower in neighborhoods with schools if their presence leads to more supervision of youth, positive social interaction, and facilitates collective efficacy among neighbors (Sampson and Raudenbush, 1999).

Existing evidence in the economics of crime literature suggests that the presence of students in schools affects crime. Examining the short-term effect of school attendance on crime, Jacob and Lefgren (2003) leverage variation in attendance due to teacher in-service days and find that the level of violent crime increases by 28% when school is in session while property crime decreases by 14%. Luallen (2006) relies on variation in school attendance generated by teacher strikes and finds that violent crime increases (by as much as 37%) and property crime decreases (by as much as 29%) during days when students attend school (referred to as incapacitation). Additional evidence that violent crime declines when students are not in school comes from Akee et al. (2014), who rely on variation in school attendance generated by teacher furlough days in Hawaii. They find that violent crime (i.e., juvenile assaults) declines when students are out of school, and that these effects are most pronounced in economically disadvantaged regions of the island.

Billings and Phillips (2017) also show that the effect of school attendance on violent crime is concentrated in neighborhoods where schools have lower academic achievement and a greater share of students

at risk for being arrested, suggesting that the concentration of underperforming students may be particularly important for generating crime in neighborhoods. Likewise, Billings et al. (2019) show that segregating more economically disadvantaged students in the same school increases total crime as students are more likely to be arrested together when committing crimes, providing empirical support for Glaeser et al. (1996) social interactions model.

Though these studies provide rigorous estimates of the presence of students in schools on the daily (i.e., short-term) effect of crime citywide or in specific neighborhoods, absent from this literature, however, is a direct examination of whether closing academically underperforming public schools materially impacts crime in neighborhoods over the longer term. This is an important omission in the literature, as public schools with low academic performance and more negative socialization may be one of the main epicenters of crime in neighborhoods. It is this question that we pursue in this paper, and one that contributes to a growing literature in economics on the effect of different forms of land use (public housing, commercial business, mixed use zoning) on crime. We view this paper as complementary of Billings and Phillips (2017), who estimate the effect of student attendance in under-performing schools on daily crime in neighborhoods, and Sandler (2017), who studies the effect of closing public housing complexes in Chicago on neighborhood crime.

3. School closings in Philadelphia

Urban school districts across the United States have increasingly relied on closing public schools as an education reform strategy to address declining student enrollment, fiscal constraints, poorly maintained school infrastructure, and low academic performance.[1] Between 2000 and 2010, nearly 70 urban school districts closed an average of 11 traditional public schools (Engberg et al., 2012). Closing public schools is controversial because it has a disproportionate impact on economically disadvantaged and minority students, and student mobility is associated with lower academic performance (de la Torre and Gwynne, 2009). While the impact of closing public schools on student achievement is mixed (Engberg et al., 2012; Brummett, 2014; Carlson and Lavertu, 2015; Steinberg and MacDonald, 2019), families voice concern that closing schools will cause crime to go up in neighborhoods as schools represent neighborhood anchors and their abandonment may lead to additional blight.

The School District of Philadelphia (SDP) presents an ideal setting for understanding the effect of academically underperforming schools on neighborhood crime.[2] Not only has student achievement in Philadelphia consistently been lower than Pennsylvania statewide averages, but Philadelphia students exhibit higher rates of truancy and serious misconduct (e.g., violence) than students in other Pennsylvania school districts (Lacoe and Steinberg, 2018). And, like Chicago, New York City and many other school districts located in large urban areas, Philadelphia has experienced the closing of both traditional and charter public schools. Between the 2006–07 and 2013-14 school years, 74 schools in Philadelphia closed, representing 19.4% of all schools that were ever open during this period.[3] Notably, of these 74 school closings, 36 school buildings became vacant for at least one academic year following school closure (see Fig. 1). It is this margin – building closure – that we focus on to understand the impact of chronically underperforming schools on neighborhood crime.

To address problems associated with declining student enrollment in traditional public schools, long-term fiscal deficits, and lagging school achievement relative to Philadelphia and statewide averages, the SDP mandated that five traditional public schools close at the end of 2011–12 school year and an additional 24 traditional public schools close at the end of the 2012-13 school year (for more detail, see Steinberg and MacDonald, 2019). We distinguish between two types of school building closings – those that closed due to the SDP's mandated closure policy and all other closings. To lend insight into the impact of closing schools on neighborhood crime, we focus on the subset of these 29 district-mandated school closures that resulted in a building closure – this includes 3 school buildings that became vacant at the end of the 2011-12 school year and 18 school buildings that became vacant at the end of the 2012-13 school year. The census block groups (of which there are 19) in which these 21 schools were located make up our treatment group for district-mandated school closings.

In addition to schools closed due to district mandate, an additional 45 schools closed during the school years 2006–07 through 2013–14 (no schools closed at the end of 2013–14); this includes 40 traditional and 5 charter public schools. Among these 45 school closings, 15 school buildings closed and remained vacant for at least one academic year. The census block groups (of which there are 13) in which these 15 school buildings were located make up our treatment group for all other (i.e., non-SDP mandated) school closings.

4. Data and descriptive statistics

We construct a panel dataset of all Philadelphia census block groups (n = 1816) from September 2006 through August 2016.[4] We link monthly crime incident data from the Philadelphia Police Department to each census block group.[5] Census block groups occupy more territory in areas where residential population and housing density is lower. The number of reported crimes per census block is effectively a rate per unit of residential population and better reflects the probability of exposure to crime than per-capita crime rates at geographic levels smaller than the city-level (Ihlanfeldt and Mayock, 2010; Aliprantis and Hartely, 2015).[6] The crime incident data includes the type of offense, the date, time and location (geocoded to the nearest latitude-longitude coordinate). We focus on three major crime categories created from these data that reflect criminal victimizations: (i) total crimes; (ii) violent crimes, which include assaults, homicides, robberies and rapes; and (iii) property crimes, which include burglaries and thefts.[7]

We supplement monthly crime counts at the census block group level with demographic and economic data from the American Community Survey (ACS). We merge the 2005–2009 5-year ACS estimates at the block group level to describe the demographic and economic

[1] Federal policies, including more recent school turnaround initiatives, have prompted many of the nation's largest urban school districts to endorse school closings as a means of offering students better educational options.

[2] In fall 2016, approximately 135,000 students were enrolled in traditional public schools, and an additional 65,000 students were enrolled in public charter schools located in Philadelphia. (Source: Pennsylvania Department of Education (http://www.education.pa.gov/Data-and-Statistics/Pages/Enrollment%20Reports%20and%20Projections.aspx). Charter schools located in Philadelphia operate as their own local education agency, and therefore are not part of the School District of Philadelphia, which operates all traditional public schools in the district.

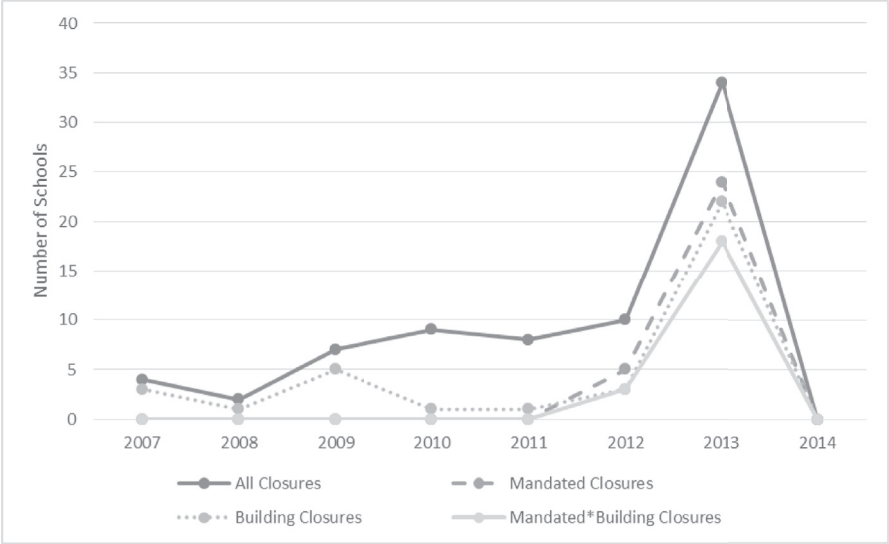
[3] Of the 74 school closings, 69 schools were traditional public schools and 5 were public charter schools. Of the 74 school closings, 38 school buildings were immediately occupied or taken over by another school so the school building itself never became vacant.

[4] Throughout the paper we refer to census block groups as neighborhoods. The results we present are substantively similar to results based on the larger area of census tracts as the unit of analysis.

[5] Monthly crime incident data retrieved from: https://www.opendataphilly.org/dataset/crime-incidents.

[6] Census block groups occupy more geographical territory when the residential population is lower. As a result, the counts of crime per block group are effectively a rate per residential population.

[7] Total crimes are measured by the total count of the following crimes: assault, aggravated assault with firearms, aggravated assault non-firearm, burglary residential, burglary non-residential, homicide, rape, robbery, other sex offenses, theft, theft from vehicle, and weapons offenses. Drug crimes are excluded from the total count of crime because they are victimless crimes.



Notes. Figure shows the distribution of school and building closures at the end of the 2006-07 through 2013-14 school years. Year indicates the spring of the school year (e.g., 2007 indicates the 2006-07 school year). *All Closures* indicates the count of all schools closed at the end of an academic year (n=74). *Mandated Closures* indicates the count of schools closed due to district mandate at the end of the 2011-12 and 2012-13 school years (n=29). *Building Closures* indicates the count of school buildings that became vacant (for at least one full academic year) following any school closure (n=36). *Mandated*Building Closures* indicates the count of school buildings that became vacant (for at least one full academic year) following district-mandated school closure (n=21). Of the 5 district-mandated closures at the end of the 2011-12 school year, 3 buildings became vacant; of the 24 district-mandated closures at the end of the 2012-13 school year, 18 buildings became vacant. Among the 74 school closures during this period, 5 schools were charter schools.

Fig. 1. Distribution of school and building closures in Philadelphia.

characteristics of block groups in the years prior to the mandatory school closures. We include the following block group level variables: race/ethnicity shares (the percent of the total population in a block group who are either white, black/African-American or Hispanic/Latino); educational attainment (the percent of the population 25 years and older in a block group who are not high school graduates, high school graduates (including high school equivalency), have some college education, have a bachelor's degree, or have an advanced degree (master's degree, professional school degree and/or a doctorate degree)); median household income; and the percent of low-income households (i.e., percent of households with less than $25,000 in annual income).

In addition to block-group level data, we include school-level data from the U.S. Department of Education, Common Core of Data (CCD) and the Pennsylvania Department of Education (PDE). The CCD includes school addresses, allowing us to link the location of all traditional and charter public schools in Philadelphia to their census block group, and information on the demographic characteristics of schools in Philadelphia, including total school enrollment, racial composition, and the share of a school's students receiving free or reduced-price lunch (a proxy for the poverty level of a school's students). School enrollment data from CCD include the count of students for each public school in Philadelphia, allowing us to examine enrollment changes in a census block group following school closures. We also merge school-level achievement data from PDE, and data on a school's behavioral climate, which includes the total arrest rate of a school's students, the total (per capita) number of out-of-school suspensions, the truancy rate and the (per capita) number of serious behavioral incidents.[8] Finally, we incorporate student-level data for all grade 3–12 students attending a traditional Philadelphia public school in the 2011–12 through 2013-14 school years.[9] This student-level data allows us to follow the post-closure school enrollment patterns of students displaced due to district-mandated closures at the end of the 2011–12 and 2012-13 school years, providing insight into the potential displacement effect of school closures on neighborhood crime.

Table 1 summarizes school-level characteristics for schools that were open in any year during the study period (*All Schools*); schools that always remained open during the study period (*Open Schools*); schools closed by district mandate (*District-Mandated Closure*), and schools that closed for other reasons (*Other Closure*). According to Table 1, schools closed by district mandate in 2011–12 and 2012–13 were systematically under-enrolled relative to districtwide averages, served a substantially higher percentage of Black students and a greater fraction of students receiving free or reduced-price lunch. Further, district-mandated closures served a substantially lower percentage of students deemed academically proficient in math or reading, and who had, on average, higher truancy rates and significantly more out-of-school suspensions, serious behavioral incidents and school-based arrests. These patterns are consistent with the district's policy mandating the closing of the lowest-performing and most under-enrolled traditional public schools in Philadelphia (Steinberg and MacDonald, 2019). We also find few differences in the observable school-level characteristics between district-mandated closures and schools that closed for other reasons.

Table 2 summarizes the socioeconomic characteristics and average monthly crime counts for all block groups in Philadelphia, for block groups with and without schools, and block groups that contained a district-mandated or other school closure. On average, crime rates are higher in block groups that contain schools compared to those without schools, confirming prior evidence on the disproportionate concentration

[8] The Pennsylvania Department of Education defines a serious behavioral incident as a "specific act or offense involving one or more victims or offenders … These include, but are not limited to, any behavior that violates a school's educational mission or climate of respect, or jeopardizes the intent of the school to be free of aggression against persons or property, drugs, weapons, disruptions, and disorder. Examples are incidents involving acts of violence, possession of a weapon, or the possession, use or sale of a controlled substance, alcohol, or tobacco by any person on school property, at school-sponsored events, and on school transportation to and from school." (Pennsylvania Information Management System, 2014).

[9] Student-level data provided to the authors by the School District of Philadelphia.

Table 1
School characteristics, by closure status.

School Characteristic	All Schools	Open Schools	Closures	
			District-Mandated	Other
Enrollment	592.1 (355.1)	612.8 (367.3)	437.4 (258.4)	539.9 (277.9)
Black	0.66 (.324)	0.63 (.332)	0.81 (.242)	0.82 (.210)
Hispanic	0.15 (.218)	0.15 (.226)	0.10 (.171)	0.11 (.155)
White	0.12 (.200)	0.14 (.212)	0.05 (.118)	0.03 (.039)
Other Race	0.08 (.107)	0.08 (.111)	0.04 (.057)	0.06 (.083)
Free/Reduced-Price Lunch	0.78 (.230)	0.77 (.233)	0.87 (.176)	0.83 (.221)
Student-Teacher Ratio	16.0 (6.02)	16.3 (6.43)	14.5 (2.57)	15.2 (3.10)
Charter School	0.25	0.29	0.00	0.11
Math Proficiency	0.51 (.221)	0.54 (.212)	0.34 (.168)	0.30 (.156)
Reading Proficiency	0.47 (.202)	0.50 (.197)	0.32 (.137)	0.29 (.144)
Truancy	0.29 (.209)	0.26 (.198)	0.42 (.177)	0.44 (.236)
Out-of-School Suspensions	0.25 (.287)	0.21 (.245)	0.39 (.279)	0.49 (.495)
Serious Behavioral Incidents	0.06 (.091)	0.05 (.092)	0.10 (.063)	0.11 (.084)
Incidents Involving Law Enforcement	0.019 (.025)	0.015 (.020)	0.038 (.034)	0.044 (.039)
Total Arrests	0.009 (.018)	0.007 (.014)	0.021 (.027)	0.027 (.030)
Schools	371	297	29	45

Notes. Mean (standard deviation) reported for the 2006–07 through 2012-13 school years; proportion reported for the share of schools that are charter schools. Among school closures, *District-Mandated* includes schools that were closed due to district mandate at the end of the 2011–12 and 2012-13 school years. *Other* includes non-district mandated closures occurring at the end of the 2006–07 through 2012-13 school years. *Other Race* includes Asian, American Indian, and students identifying as two or more races. *Out-of-School Suspensions, Serious Behavioral Incidents, Incidents Involving Law Enforcement*, and *Total Arrests* report the total count on a per-capita (school enrollment) basis; *Truancy* is the proportion of a school's students who are persistently absent. Sample includes all district schools, including traditional, charter, special education and vocational schools.

of crime among neighborhoods with schools (Weisburd et al., 2012). Further, block groups that contain closed schools have the highest crime rates among all block groups in Philadelphia. Though this evidence suggests that the absence of schools may be correlated with lower crime rates and lower rates of economic disadvantage, these descriptive patterns provide no insight into how closing underperforming schools and shifting student enrollment impacts neighborhood crime. In the sensitivity analysis section of the paper (section 6.1), we examine whether the estimated effects on crime are impacted by the choice of blocks included in comparison groups. We next describe our approach for uncovering both the direct effect of underperforming schools on crime and the potential displacement effect of building closure on crime.

5. Empirical approach

To estimate the effect of underperforming schools on neighborhood crime, we employ a difference-in-differences (DD) approach that compares changes in crime in census block groups with school buildings that close to block groups that did not have a school building close. We estimate variants of the following model:

$$Crime^{j}_{bmt} = \beta_0 + \beta_1\left(Closed^{mandated}_{bmt}\right) + \beta_2\left(Closed^{other}_{bmt}\right) + \gamma_b + \theta_{mt} + \varepsilon_{bmt} \quad (1)$$

In equation (1), *Crime* is the count of the *j*th crime type (i.e., total crime; violent crime; or property crime) reported in census block group *b* during month *m* in school year *t*. The binary variable $Closed^{mandated}_{bmt}$ indicates whether block group *b* contained a district-mandated school closure whose building closed as of month *m* in school year *t*.[10] The binary variable $Closed^{other}_{bmt}$ indicates whether block group *b* contained a non-district-mandated school closure whose building closed as of month *m* in school year *t*. The variable γ_b is a block group fixed effect that accounts for all time-invariant differences across census block groups; θ_{mt} represents month*year fixed effects, to control for secular trends that are common to all block groups; and ε_{bmt} is a random error term. We cluster the standard errors at the block group level to account for unmeasured dependence within blocks over time.

In alternative specifications of equation (1), we include the term $\gamma_b t$, which represents block group-specific (month*year) linear time trends. The inclusion of block-group specific linear time trends enables an assessment of the parallel trends assumption underlying this DD approach (Angrist and Pischke, 2009). Since the outcome variable in equation (1) is a nonnegative count, we estimate crime counts with a Poisson regression model. We report estimates of e^{β_1} and e^{β_2}, which are the incident-rate ratios estimating the effect of district-mandated school building closures and other (non-district mandated) school building closures, respectively, on the expected count of monthly crime. In equation (1), the null hypothesis (H_0) is $\beta = 0$; however, since we report incidence-rate ratios (e^{β_1} and e^{β_2}), the null hypothesis becomes $e^{\beta} = 1$.

To assess how crime evolved in the months leading up to and after school closings and to indirectly test the parallel trends assumption, we estimate an event study model as a non-parametric expansion of equation (1). As in equation (1), the event study model controls for block group and month*year fixed effects but replaces the closure indicators with month-specific treatment indicators; doing so allows for month-specific estimates of the effect of closures on crime in the twelve months prior to and following closures (Stevenson and Wolfers, 2006). We set September of the final school year in which the school building was open (i.e., twelve months before building closure) as the reference period and estimate month-specific effects of closures on crime in the eleven months leading up to and twelve months after school buildings were closed.

5.1. Student enrollment effect

We examine the extent to which the effect of school closings on crime varied by the decline in student enrollment in a block group following closure. If the effect on crime is increasing in the change in student enrollment, then as more students exit a block following closings, we should observe a greater decline in crime. First, we measure the change

[10] Since school closures occur between academic years, we set $Closed_{bmt} = 1$ as of September of the school year in which a school's building closed in block group b. For example, if a school's last year of operation was the 2012-13 school year and the school building was subsequently closed beginning in the 2013-14 school year, we set $Closed_{bmt} = 1$ as of September 2013, which is the first month in which the closed school would have been operating had it not been closed, and is therefore the first month in which the census block group experienced a closed school.

Table 2
Characteristics of census block groups, by closure status.

	All Block Groups	Block Groups: No Schools	Block Groups: Contain Schools		
			No Closures	District-Mandated Closures	Other Closures
Panel A: Socioeconomic Characteristics					
Black	47.3 (39.29)	47.3 (39.35)	42.5 (38.48)	67.5*** (36.14)	61.7*** (37.24)
White	35.1 (36.03)	35.4 (36.08)	37.9 (36.47)	20.0** (32.57)	20.2*** (28.54)
Hispanic	10.9 (20.04)	10.7 (19.56)	13.3 (23.18)	8.0 (19.55)	10.2 (18.40)
Median Income	38,729.9 (24,479.16)	39,482.5 (25,307.65)	36,142.7 (19,631.54)	30,347.7 (17,285.56)	31,660.6 (19,002.76)
Low-Income Rate	39.7 (22.77)	39.2 (22.67)	40.9 (23.81)	47.6 (16.88)	45.6 (22.65)
Less than H.S.	22.4 (16.80)	22.1 (16.72)	23.7 (17.82)	24.1 (14.22)	22.5 (15.75)
H.S.	36.0 (17.35)	36.1 (17.41)	35.4 (17.37)	36.4 (15.21)	37.1 (17.60)
Some College	21.1 (12.94)	21.1 (12.98)	20.7 (12.95)	21.3 (10.91)	22.6 (12.89)
Bachelors	12.2 (12.30)	12.2 (12.29)	12.4 (12.26)	13.0 (14.53)	11.2 (14.26)
Advanced Degree	8.3 (12.78)	8.5 (13.04)	7.8 (11.69)	5.2 (5.92)	6.6 (12.14)
Panel B: Monthly Crime Counts					
Total	9.43 (8.70)	9.01 (8.42)	11.31 (9.72)	12.96*** (9.62)	12.06*** (9.80)
Violent	2.16 (2.36)	1.99 (2.16)	2.86 (2.76)	3.88*** (3.58)	3.74*** (3.98)
Property	2.31 (3.47)	2.22 (3.30)	2.82 (4.53)	2.78 (3.60)	2.24*** (2.34)
Census Block Groups	1816	1518	232	27	40

Notes. In Panel A, mean (standard deviation) reported. Data are at the block group level from the American Community Survey (2005–2009 5-year estimates). *Black, White* and *Hispanic* are the percent of the total population in a block group who are either Black/African-American, White or Hispanic/Latino; *Median Income* is median household income (reported by the ACS in 2009 inflation adjusted dollars); *Low-Income Rate* is the percent of households with less than $25,000 in annual income; the educational attainment variables are the percent of the population 25 years and older who: are not high school graduates (*Less than H.S.*); are high school graduates, including high school equivalency (*H.S.*); have some college education (*Some College*); have a bachelor's degree (*Bachelors*); or have an advanced degree, including a master's degree, professional school degree and/or a doctorate degree (*Advanced Degree*). In Panel B, mean (standard deviation) count of crimes are at the year*month level and are reported for the 2006–2012 years (i.e., observations are at the block group*year*month level and consist of 12 observations per year across 7 pre-closure years). *Total* includes the count of all of the following crimes – assault, aggravated assault with firearms, aggravated assault non-firearm, burglary residential, burglary non-residential, homicide, rape, robbery, other sex offenses, theft, theft from vehicle, and weapons offenses. *Block Groups: No Schools* include block groups that never contained a school during the 2006–2012 years; *Block Groups: Contain Schools* include blocks groups that contained a school in any year during the 2006–2012 years. Since one block group contained both a district-mandated and an other (non-district mandated) school closure in the 2006–2012 years, the count of census block groups across the *Block Groups: No Schools* and *Block Groups: Contain Schools* categories does not equal 1,816, the count of all block groups in Philadelphia. Difference in means between block groups with closures and block groups with no closures, among *Block Groups: Contain Schools* categories, statistically significant at the *10%, **5% and ***1% levels.

in student enrollment following closure relative to enrollment in the pre-closure academic year as:

$$\Delta Enrollment_b = Enrollment_{b,PostClosure} - Enrollment_{b,PreClosure} \quad (2)$$

In Equation (2), $Enrollment_{b,PostClosure}$ is the count of students enrolled in schools located in block group b in the first post-closure school year (i.e., if a school closed in block group b at the end of the 2012-13 school year, then the post-closure year is the 2013-14 school year). $Enrollment_{b,PreClosure}$ Is the count of students enrolled in schools located in block group b in the final (pre-closure) school year in which a closed school was open (i.e., if a school closed in block group b at the end of the 2012-13 school year, then the pre-closure year is the 2012-13 school year). Then, we estimate a model according to the following form:

$$Crime^j_{bmt} = \beta_0 + \beta_1\left(Closed^{mandated}_{bmt}\right) + \beta_2\left(Closed^{mandated}_{bmt} * \Delta Enrollment_b\right) + \beta_3\left(Closed^{other}_{bmt}\right) + \beta_4\left(Closed^{other}_{bmt} * \Delta Enrollment_b\right) + \gamma_b + \theta_{mt} + \varepsilon_{bmt} \quad (3)$$

In equation (3), the moderating effect on crime is linear in $\Delta Enrollment_b$, which is the change in student enrollment among block groups experiencing a school building closing. The total effect of district-mandated closure is a linear combination of $\widehat{\beta}_1 + \widehat{\beta}_2(\Delta Enrollment_b)$; the total effect of other (non-district-mandated) closures is $\widehat{\beta}_3 + \widehat{\beta}_4(\Delta Enrollment_b)$. All other variables are defined as in equation (1).

5.2. Crime displacement

The mobility of students from closed schools may generate crime displacement to neighborhoods that receive these students. To examine the extent to which the change in crime varies as the concentration of displaced students attending a new school increases, we amend equation (1) and estimate variants of the following model:

$$Crime^j_{bmt} = \beta_0 + \beta_1\left(Closed^{mandated}_{bmt}\right) + \beta_2\left(Closed^{other}_{bmt}\right) + \beta_3(Displaced_{bmt}) + \gamma_b + \theta_{mt} + \varepsilon_{bmt} \quad (4)$$

In equation (4), *Displaced* is the fraction of students attending schools in block group b who were displaced from closed schools beginning in month m in school year t.[11] We restrict our analysis to the years when district-mandated school closings occurred (2011–12 and 2012-13 school years), since these are the only years in which we are able to follow displaced students' post-closure enrollment patterns.[12] In alternative specifications of equation (4), we measure *Displaced*: (i) as a continuous variable representing the fraction of enrolled students in block group b who were displaced due to district-mandated closures in the 2012–13 and 2013-14 school years; and (ii) non-parametrically with three indicator variables representing: blocks groups with less than 5 (and greater than zero) percent of displaced students; between 5 and 10 percent of displaced students; and with at least 10 percent of displaced

[11] The fraction of displaced students is the proportion of students attending all schools in a block group who were displaced due to district-mandated closures in the prior year. For example, if total student enrollment among all schools in a block group during the 2013-14 school year was 1,000, and 100 of these students were displaced due to district-mandated school closures in the 2012-13 school year, then the fraction of displaced students in that block group in the 2013-14 school year is 0.10.

[12] We leverage student-level data for grade 3–12 students attending a Philadelphia traditional public school in the 2011–12 through 2013-14 school years, allowing us to follow the enrollment patterns of students displaced due to district-mandated closures at the end of the 2011–12 and 2012-13 school years.

Table 3
Effect of school closures on crime.

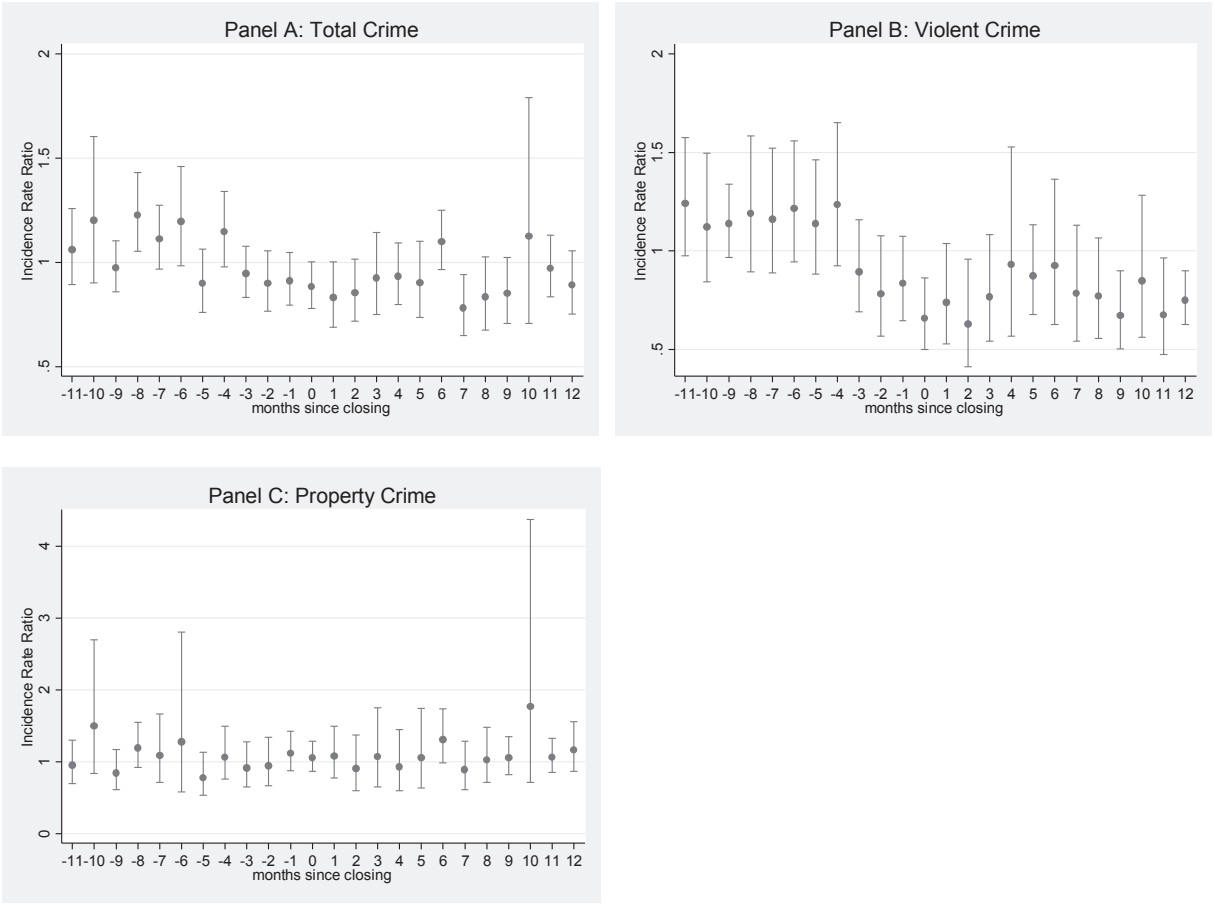
	Total Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Closed (Mandated)	0.851** (.060)	0.871** (.034)	0.693*** (.063)	0.712*** (.053)	1.12 (.148)	1.00 (.070)
Closed (Other)	0.775** (.077)	0.920 (.078)	0.831** (.075)	0.948 (.115)	0.823*** (.031)	0.872*** (.045)
P-Value from F-Test: *Mandated = Other*	.4449	.5551	.1564	.0428	.0265	.1090
Block Group Linear Time Trends		X		X		X
Mean (sd) of Dependent Variable	9.07 (8.98)		2.09 (2.32)		2.23 (3.38)	
Block Groups	1816	1816	1816	1816	1816	1816
Observations	217,920	217,920	217,920	217,920	217,920	217,920

Notes. Coefficients (incidence rate ratios) are reported with robust standard errors (clustered at the block group level). Observations are at the block group*year*month level. All regressions include block group and month*year fixed effects. Block group-specific linear time trends are at the month*year level. Coefficients statistically significant at the *10%, **5% and ***1% levels.

students. The reference group for the nonparametric estimates includes block groups that did not enroll any displaced students. All other variables are defined as in equation (1). If there is a negative spillover effect of displacing students following district-mandated closure, then β_3 will be positive.

6. Results

Table 3 summarizes the average effect of school building closures on neighborhood crime. We find that crime declines by 14.9% in block groups following district-mandated closure, relative to block groups in



Notes. Figures report coefficients (incidence rate ratios) with 95% confidence intervals. Observations are at the block group*year*month level for the 12 months before and 12 months after schools closed (n=67,192 block*month observations for the 1,816 unique block groups). All regressions include block group and month*year fixed effects. We center the month of closing – 0 months since closing – at September of the academic year in which the closed school did not reopen (e.g., if the school was closed at the end of the 2012-13 school year, which corresponds to June 2013 (or -3 months since closing), then 0 months since closing corresponds to September 2013). The reference month is 12 months prior to closing (i.e., September of the final school year in which the school building was open). The values -3, -2, and -1 months since closing correspond to summer months (i.e., June, July and August, respectively).

Fig. 2. Event study estimates of district mandated school closures on crime.

Philadelphia with no school building closures (see column 1, Table 3). This estimated effect corresponds to approximately 1.4 fewer monthly crimes in the block group and is robust to the inclusion of block group-specific linear time trends. Notably, nearly half of the reduction in total crime is due to the decline in violent crime. Violent crime declines by an estimated 30% – approximately 0.63 fewer violent crimes per month in block groups - following district-mandated closures (see column 3). In contrast, we find no effect of district-mandated closures on property crimes.

Event study estimates summarized in Fig. 2 confirm these findings. We observe substantial and immediate reductions in violent crime and null effects for property crime in block groups in the month when schools were mandated to be closed, relative to 12 months prior to closure (i.e., September of the final year in which a school building was open). Though the initial impact of school closings attenuates over the course of the school year, the event study estimates show that reductions in violent crime represent a downward shift – relative to the pre-closure period – in violent crime in the long-run. Closing academically underperforming schools appear to have a persistent effect on violent crime. We find no evidence of anticipatory effects or divergent crime trends in the months leading up to school closure, providing further validity for the difference-in-differences design.

In block groups following all other building closures, total crime declines by approximately 22% (see column 1, Table 3). These estimates, however, are not robust to the inclusion of block group-specific linear time trends, indicating the parallel trends assumption underlying the difference-in-differences approach is likely violated for other building closures (see column 2). This result further highlights the exogeneity of district mandated closure of underperforming schools with respect to pre-existing trends in neighborhood crime. We therefore focus our discussion on estimates of district-mandated closings on neighborhood crime.

6.1. Sensitivity tests

The estimates presented in Table 3 rely on a comparison group which includes all block groups in Philadelphia that contain schools (that did not close) and all block groups in Philadelphia that never contained schools. To the extent that crime displacement may occur as students move to their new schools following closures, these estimates may be overstated with the inclusion of block groups with schools that received displaced students. Specifically, the displacement of students from closed schools to block groups with open schools may result in shifting the location of crime between schools – from locations with closed schools to those with open schools; this means that blocks that don't contain building closures may be affected by the treatment (i.e., contamination). At the same time, our main estimates may also be sensitive to the choice of blocks included in the comparison group given the potential that block groups with and without closures may systematically vary on observable (and unobservable) dimensions that limit their appropriateness as a comparison group.

To assess the sensitivity of our main estimates, we construct five alternative comparison groups, including: (i) block groups with no schools; (ii) block groups with schools in 2006–07 (the first year of the sample period); (iii) block groups with schools ever in the study period; (iv) block groups with low-achieving schools[13]; and (v) block groups located in census tracts containing a building closure.[14] We then conduct balancing tests for each comparison group by estimating regressions of the following form:

$$Closed^{mandated}_{b,\ t=2005-09} = \beta_0 + X'_{b,\ t=2005-09}\Gamma + \varepsilon_{b,t=2005-09} \quad (5)$$

where $Closed^{mandated}_{b,\ t=2005-09}$ is an indicator for whether block group b contained a district-mandated building closure; **X** is a vector of demographic and economic characteristics of block group b from the 2005–2009 5-year American Community Survey (ACS), including race/ethnicity shares (the percent of total population in block group b who are white, black/African-American or Hispanic/Latino); the natural log of median household income; the low-income rate, which is the percent of households with less than $25,000 in annual income; and the percent of the population 25 years and older who have no more than a high school education (i.e., non-high school graduates or high school graduates, including high school equivalency).

Table 4 shows the results from these balancing tests and indicates that select demographic and economic characteristics of block groups jointly explain variation in the location of district-mandated school building closures for all comparisons except block groups with low-achieving schools and block groups located in census tracts containing a building closure. Indeed, there is a significant improvement in balance on these select characteristics when restricting the comparison set to block groups that contain schools that have similar academic performance as those closed by district mandate and to block groups located in census tracts containing a building closure.

Despite the results from the balancing tests, Table 5 shows that our main estimates (reported in Table 3) are robust to alternative comparison groups containing different block groups. Across the alternative comparison groups, the monthly count of total reported crime declines by approximately 11–15.6% (or 1.0–1.8 fewer monthly crimes) in block groups following district-mandated closure (see Panel A, columns 1–5). We again find that the decline in total crime is driven by a reduction in violent crime, by an estimated 29–31% across alternative comparison groups (approximately 0.61–1.07 fewer violent crimes per month) (see Panel B, columns 1–5). There remains no effect on property crime following district-mandated closure (see Panel C, columns 1–5).

We then estimate the effect of school closings among just the census tracts containing the largest round of district-mandated building closures

[13] To construct the comparison group containing block groups with low-achieving schools, we first calculate average achievement in the period prior to district-mandated closures for schools located in the same block group as: $Achievement_{b,\ t=2006-2010} = \frac{\sum_{t=1}^{T} \frac{Proficient^{math}_{bt}}{Tested^{math}_{bt}}}{T} + \frac{\sum_{t=1}^{T} \frac{Proficient^{reading}_{bt}}{Tested^{reading}_{bt}}}{T}}{2}$, where $Achievement_{b,\ t=2006-2010}$ is the academic achievement of students attending schools in block group b during the pre-closure period – 2006–07 through 2010-11 school years, and is measured as the proportion of students who are proficient in mathematics and reading among tested students attending schools in block group b during the pre-closure period (i.e., 2006–07 through 2010-11 school years); *Proficient* is the count of students attending schools in block group b during school year t who were academically proficient in either mathematics or reading; and *Tested* is the count of students attending schools in block group b during school year t who took the mathematics or reading exam. Then, we rank the 247 block groups (which are exclusive of the 19 district-mandated building closure blocks and 13 non-district-mandated building closure blocks) with achievement data in each of the 2006–07 through 2010-11 school years on the $Achievement_{b,\ t=2006-2010}$ measure. We then select the lowest-achieving non-closure blocks – those blocks in the bottom 35th percentile of achievement – for inclusion in the low-achieving comparison group (which includes 86 non-closure blocks). Among the 247 non-closure blocks, the mean (sd) of $Achievement_{b,\ t=2006-2010}$ is 0.52 (0.179); the mean (sd) of $Achievement_{b,\ t=2006-2010}$ among district-mandated building closure blocks is 0.33 (0.104); the mean (sd) of $Achievement_{b,\ t=2006-2010}$ among non-district-mandated building closure blocks is 0.34 (0.199); and the mean (sd) of $Achievement_{b,\ t=2006-2010}$ among the 86 non-closure blocks included in the low-achieving comparison group is 0.32 (0.088).

[14] There are 381 census tracts in Philadelphia, of which 27 census tracts contain at least one building closure (17 census tracts contain a district-mandated building closure and 12 census tracts contain a non-district-mandated building closure).

Table 4
Balancing tests - demographic and economic characteristics of block groups.

	Comparison Group					
	All Block Groups	Block Groups with No Schools	Block Groups with Schools in 2006–07 Year	Block Groups with Schools Ever in Study Period	Block Groups with Low-Achieving Schools	Block Groups Located in Census Tracts Containing Closures
Black	0.000 (.0002)	0.000 (.0002)	0.000 (.0012)	0.000 (.0011)	−0.000 (.0029)	−0.001 (.0022)
White	−0.000 (.0002)	−0.000 (.0003)	−0.001 (.0014)	−0.001 (.0012)	0.000 (.0036)	−0.002 (.0026)
Hispanic	−0.000 (.0002)	−0.000 (.0003)	−0.000 (.0014)	−0.001 (.0013)	−0.001 (.0028)	0.000 (.0027)
ln (Median Income)	0.005 (.0089)	0.005 (.0101)	0.042 (.0579)	0.038 (.0549)	0.027 (.1196)	0.067 (.0884)
Low-Income Rate	0.000 (.0002)	0.000 (.0002)	0.002 (.0014)	0.002 (.0013)	0.002 (.0028)	0.002 (.0023)
H.S./Less than H.S.	0.000 (.0001)	0.000 (.0001)	0.000 (.0001)	0.000 (.0007)	−0.000 (.0030)	−0.001 (.0011)
R^2	.0047	.0056	.0228	.0235	.0074	.0157
Overall F-Test (p-value)	.0960	.1076	.0717	.0652	.9617	.3681
Block Groups	1748	1486	276	293	115	145

Notes. Coefficients reported are from linear probability models with robust standard errors (clustered at the Census tract level). Dependent variable is an indicator for whether the block group contained a district-mandated school building closure. *Black* is the percent of the total population in a block group who are Black/African-American; *White* is the percent of the total population in a block group who are white; *Hispanic* is the percent of the total population in a block group who are Hispanic/Latino; ln*(Median Income)* is the log of median household income; *Low-Income Rate* is the percent of households with less than $25,000 in annual income; and *H.S./Less than H.S.* is the percent of the population 25 years and older who are not high school graduates (*Less than H.S.*) or who are high school graduates (including high school equivalency). *All block Groups* do not equal 1816 blocks because ACS data is missing for select block groups; *Block Groups with No Schools* do not equal 1547 blocks because ACS data is missing for select block groups; *Block Groups with Schools in 2006-07 Year* do not equal 282 blocks because ACS data is missing for select block groups; *Block Groups with Schools Ever in Study Period* do not equal 300 blocks because ACS data is missing for select block groups; *Block Groups with Low-Achieving Schools* do not equal 118 blocks because ACS data is missing for select block groups; *Block Groups Located in Census Tracts Containing Closures* do not equal 150 blocks because ACS data is missing for select block groups. Coefficients statistically significant at the *10%, **5% and ***1% levels.

Table 5
Effect of school closures on crime: Sensitivity analyses.

	Comparison Group					Time Period			
	Block Groups with No Schools	Block Groups with Schools in 2006–07 Year	Block Groups with Schools Ever in Study Period	Block Groups with Low-Achieving Schools	Block Groups Located in Census Tracts Containing Closures	2006–2013	2008–2013	2009–2014	2010–2014
Panel A: Total Crime									
Closed (Mandated)	0.844** (.060)	0.882* (.064)	0.881* (.064)	0.857** (.066)	0.891 (.064)	0.855** (.065)	0.859** (.061)	0.845** (.058)	0.861** (.058)
Closed (Other)	0.772*** (.077)	0.806** (.087)	0.794** (.078)	0.785** (.076)	0.803** (.076)	0.780** (.090)	0.852 (.106)	0.869 (.087)	0.837* (.082)
Mandated = Other	.4586	.4829	.3840	.4534	.3793	.5043	.9599	.8177	.8088
Mean (sd) of Dependent Variable	8.76 (8.89)	11.08 (9.67)	10.97 (9.51)	12.34 (9.87)	9.01 (7.43)	9.29 (8.70)	8.93 (8.51)	8.77 (8.99)	8.70 (9.10)
Panel B: Violent Crime									
Closed (Mandated)	0.690*** (.063)	0.701*** (.065)	0.703*** (.065)	0.709*** (.067)	0.696*** (.064)	0.660*** (.054)	0.655*** (.050)	0.678*** (.050)	0.698*** (.052)
Closed (Other)	0.829** (.075)	0.829** (.079)	0.839* (.076)	0.843** (.072)	0.839** (.072)	0.834* (.088)	0.989 (.080)	0.993 (.026)	0.926** (.028)
Mandated = Other	.1517	.2019	.1672	.1648	.1330	.0770	.0002	.0000	.0005
Mean (sd) of Dependent Variable	1.95 (2.20)	2.98 (2.99)	2.94 (2.95)	3.69 (3.47)	2.41 (2.74)	2.13 (2.34)	2.07 (2.27)	2.03 (2.25)	2.01 (2.23)
Panel C: Property Crime									
Closed (Mandated)	1.11 (.148)	1.14 (.152)	1.14 (.151)	1.09 (.145)	1.12 (.146)	1.16 (.192)	1.15 (.169)	1.12 (.129)	1.14 (.132)
Closed (Other)	0.822*** (.031)	0.851*** (.031)	0.828*** (.032)	0.811*** (.038)	0.819*** (.035)	0.845*** (.042)	0.853*** (.047)	0.816* (.092)	0.777* (.114)
Mandated = Other	.0277	.0353	.0225	.0360	.0289	.0681	.0579	.0458	.0393
Mean (sd) of Dependent Variable	2.16 (3.24)	2.66 (4.10)	2.64 (4.02)	2.32 (2.62)	1.89 (2.34)	2.28 (3.46)	2.26 (3.43)	2.23 (3.38)	2.22 (3.36)
Block Groups	1547	282	300	118	150	1816	1816	1816	1816
Observations	185,640	33,840	36,000	14,160	18,000	174,336	130,752	130,752	108,960

Notes. Each column within a panel is a separate regression. Coefficients (incidence rate ratios) are reported with robust standard errors (clustered at the block group level). Observations are at the block group*year*month level. All regressions include block group and month*year fixed effects. Coefficients statistically significant at the *10%, **5% and ***1% levels.

which occurred in a single year – the 2012-13 school year.[15] Given that some census tracts containing a district-mandated building closure in this single year also contained more comparison blocks (i.e., blocks without a building closure), we weight estimates of equation (1) by the percent of comparison blocks in a given tract; this way, tracts with more comparison blocks will contribute more to the estimated effect on crime.[16] Our main results are robust to restricting comparison to this single school closings event; violent crime declines by 26.2%, or 0.66 fewer violent crimes per month, following district-mandated closures in the 2012-13 school year (see Table A1).

Next, we assess the sensitivity of our main estimates to the time period examined. This allows us to assess whether the effect of district-mandated closure is sensitive to the choice of pre- and post-closure years included in the sample. Results summarized in columns 6–9 of

[15] Of the 17 census tracts that contained a district-mandated building closure occurring at the end of either the 2011–12 or 2012-13 school years, 14 census tracts contained a building that closed at the end of the 2012-13 school year.

[16] Of the 14 census tracts containing a district-mandated building closure in the 2012-13 year, two tracts contain two blocks (of which one tract contains two blocks with closures); three tracts contain four blocks; two tracts contain seven blocks; and seven tracts contain eight blocks.

Table 5 show that our main results are robust to alternative time periods, indicating that the choice of study years does not influence the estimated effect of district-mandated closure on neighborhood crime.

Finally, we re-estimated the primary specification using a spatial error regression model (Belotti et al., 2017).[17] If our primary estimates of school closings on crime are influenced by displacement of crime to neighboring blocks, this model will adjust standard errors for that form of spatial dependence. The results from the spatial error model suggest similar size effects.[18] Specifically, mandated closures are associated with a 26% reduction (2.35 fewer total crimes; $p < .0001$) in total monthly crimes at the block level. Similar to results based on our primary specification (see Table 3), the decline in total crime is driven by the reduction in violent crimes. Mandated closings are associated with an estimated 60% decline in monthly violent crime (1.36 fewer violent crimes; $p < .0001$). Consistent with our primary estimates in Table 3, property crime appears to increase by a less precisely estimated 10% (0.24 more property crimes; $p < .01$) following district-mandated closures. These estimates are larger than when we don't adjust for spatial autocorrelation, suggesting that, if anything, results based on our primary specification may be conservative with respect to the decline in crime following district-mandated closures.

6.2. Heterogeneity by school level

Prior evidence finds that high-school age students (i.e., ages 16 to 19) have the highest rate of offending in the population, and that victims of criminal activity are similar in age and demographics to offenders (Farrington, 1986). This suggests that closing high schools and displacing high-school age students from a neighborhood may lead to a larger (and differential) decline in crime relative to closing schools serving younger students. To explore this, we next examine whether the effect of building closure varies by the grade level of students – high school students compared to elementary/middle school students – displaced following district-mandated closure. Fig. 3 (and Table A2) summarize these results.[19]

Among district-mandated closings, we find that total crime declines more in block groups where high schools were closed than in block groups where elementary or middle schools were closed. The monthly count of total reported crime declines by 20.6% (1.8 fewer crimes) in block groups following district-mandated high school closings; this compares to a statistically insignificant decline of 9% in block groups following district-mandated closing of elementary or middle schools. Further, in block groups following district-mandated high school closings, the monthly count of violent crime declines by 40% (0.81 fewer violent crimes), while violent crime declines by 18.3% (0.37 fewer violent crimes) following district-mandated closure of elementary or middle schools (and is only marginally significant).[20] We again find no effect on property crime, and note that these estimates are robust to including only blocks groups that contain no schools as the comparison set.

6.3. Potential mechanisms

Next, we explore whether the decline in crime is driven by a student occupancy effect or property abandonment following school closing. To do so, we first examine the extent to which reductions in crime are concentrated during days and times when students were enrolled in school. Then, we examine the extent to which reductions in crime are driven by declines in the number of students enrolled in neighborhoods following school closures.

If the presence of students is the main driver of changes in crime patterns, then the decline in crime should be concentrated on weekdays and times – 9am to 5pm – when students displaced due to closure would otherwise be present. Table 6 summarizes results by the timing of reported crime. Among district-mandated closings, we find that the effect is concentrated almost entirely during weekdays between 9am and 5pm. Indeed, the monthly count of total reported crime declines by approximately 35% – 0.96 fewer crimes – in block groups following district-mandated closings during school hours and is robust to controls for block group linear time trends (see columns 1 and 2, Panel A, Table 6). Notably, there is no effect of district-mandated closures on crime during non-school hours – either during weekdays between 5pm and 9am or on weekends. We again find that the decline in total crime is concentrated among violent crime, and that this effect is almost entirely concentrated during school hours. Namely, the monthly count of violent crime declines by approximately 60% (or 0.40 fewer violent crimes) on weekdays between 9am and 5pm with no effect during weekday evening hours or on weekends. These estimates suggest that removing students from underperforming schools with more problem behaviors is likely the key driver of the reduction in crime following school closure. The removal of students from these blocks appears to be the driver of the observed reduction in crime and not the vacancy of the buildings following closure.

We next estimate the effects of district-mandated closures on crime by the change in student enrollment. Doing so provides a more precise assessment of the extent to which post-closure declines in student enrollment explain declines in crime. Table 7 summarizes the total effect on crime at three points along the distribution of the change in student enrollment in block groups containing district-mandated closures (Table A3 displays the incidence rate ratios from the full specification of equation (3)).

Table 7 shows that crime is linearly related to the number of students exiting a block group following school closure. Among block groups at the mean change in student enrollment (i.e., 389 fewer students), total crime declines by 14% and violent crime declines by 29%. Among block groups with one standard deviation above the mean decline in student enrollment (i.e., 621 fewer students), the magnitude of crime reduction is greater; namely, total crime declines by 19% and violent crime declines by 34%. This compares to block groups with, on average, 158 fewer students following closure, where total crime declines by a statistically insignificant 7.3% and violent crime declines by a statistically significant 23.2%. These findings are consistent with evidence summarized in Table 6, further suggesting that an enrollment effect of removing more youth from

[17] We first created a spatial weight matrix in which $w_{bj} = 1/n_b$, where n_b is the number of neighbors of census block b, if block b and block j were adjacent to each other and $w_{bj} = 0$ if block b and block j were not neighbors. We estimate variants of the following block group-level model:

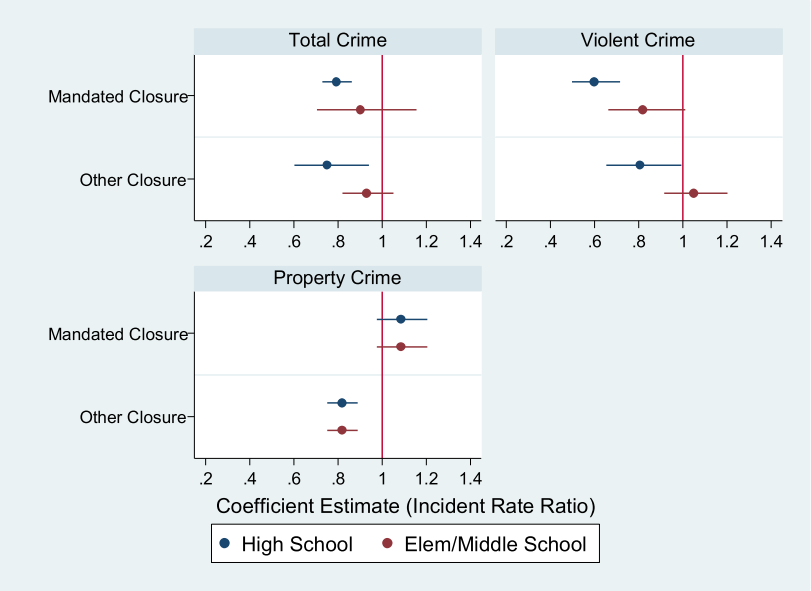
$$Crime^{i}_{bmt} = \beta_0 + \beta_1\left(Closed^{mandated}_{bmt}\right) + \beta_2\left(Closed^{other}_{bmt}\right) + \gamma_b + \theta_{mt} + \varepsilon_{bmt} + \lambda \sum_{j=1}^{1,816} w_b \varepsilon_{jt} \quad (6)$$

In equation (6), λ estimates the size of the spatial autocorrelation.

[18] We estimate equation (6) with a linear conditional mean function, while we estimate equation (1) with an exponential conditional mean function (since the count of crimes follows a Poisson distribution). To make results comparable across model specifications, we transform estimates from equation (6) into percentage changes (coefficient estimate/group mean) so they have similar interpretations as the incident-rate ratios (IRR's) estimated from the Poisson regressions in equation (1).

[19] A school is classified as a high school if it serves students in grades 9–12, and includes any school serving students in grades 1–12; we classify elementary and middle schools as schools whose highest grade offered is grade 8 (or lower). Among district-mandated school building closures, 8 were high schools and 13 were either elementary or middle schools. Among all other closures, 9 were high schools and 6 were either elementary or middle schools.

[20] The decline in crime among blocks that contain district-mandated closures of elementary or middle schools is not surprising, since prior evidence in Philadelphia finds that middle school students (i.e., grades 6–8) have higher rates of out-of-school suspensions for more serious misconduct (i.e., offenses occurring outside of the classroom but at school) than high school students (i.e., grades 9–12) (Lacoe and Steinberg, 2019).



Notes. Figure shows coefficient estimates (incidence rate ratios, with 95% confidence intervals) of the effect of closures, by school level and type of closure (i.e., district-mandated and other), for each of three crime types (total, violent, property). See Table A2 for a summary of the coefficient estimates.

Fig. 3. Effects of school closures on crime, by school level.

Table 6
Effect of school closures on crime, by timing of crime.

	Weekdays, 9am-5pm		Weekdays, 5pm-9am		Weekends	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total Crime						
Closed (Mandated)	0.652*** (.081)	0.776*** (.057)	0.940 (.054)	0.907* (.048)	1.04 (.051)	0.951 (.047)
Closed (Other)	0.735*** (.058)	0.925 (.106)	0.865 (.084)	0.893 (.091)	0.680 (.177)	0.935 (.062)
Mandated = Other	.4126	.1946	.4612	.8908	.1110	.8363
Mean (sd) of Dependent Variable	2.76 (3.45)		3.90 (4.26)		2.41 (2.89)	
Panel B: Violent Crime						
Closed (Mandated)	0.382*** (.059)	0.467*** (.050)	0.911* (.048)	0.838* (.078)	1.04 (.088)	0.908 (.097)
Closed (Other)	0.663** (.105)	0.894 (.145)	1.14* (.079)	1.097 (.142)	0.988 (.098)	0.935 (.086)
Mandated = Other	.0123	.0008	.0110	.0927	.7151	.8328
Mean (sd) of Dependent Variable	0.64 (1.16)		0.89 (1.22)		0.56 (.914)	
Panel C: Property Crime						
Closed (Mandated)	1.01 (.181)	1.049 (.090)	1.18* (.114)	0.976 (.073)	1.23 (.168)	0.962 (.149)
Closed (Other)	0.843*** (.048)	0.891* (.057)	0.798*** (.055)	0.830* (.080)	0.816** (.082)	0.905 (.061)
Mandated = Other	.3449	.1277	.0008	.1814	.0161	.7206
Mean (sd) of Dependent Variable	0.85 (1.57)		0.83 (1.43)		0.56 (1.10)	
Block Group Linear Time Trends		X		X		X
Block Groups	1816	1816	1816	1816	1816	1816
Observations	217,920	217,920	217,920	217,920	217,920	217,920

Notes. Coefficients (incidence rate ratios) are reported with robust standard errors (clustered at the block group level). Observations are at the block group*year*month level. All regressions include block group and month*year fixed effects. Block group-specific linear time trends are at the month*year level. Coefficients statistically significant at the *10%, **5% and ***1% levels.

Table 7
Heterogeneous effects of mandated school closures on crime, by change in student enrollment.

	−1 SD	Mean	+1 SD	
	$\Delta Enrollment_b = -158$ Students	$\Delta Enrollment_b = -389$ Students	$\Delta Enrollment_b = -621$ Students	P-Value: Difference in Effect, 158 to 621 Fewer Students
Total Crime	0.927 (.100)	0.867** (.061)	0.810*** (.039)	.1420
Violent Crime	0.768** (.086)	0.712*** (.062)	0.660*** (.042)	.0045
Property Crime	1.19 (.221)	1.12 (.141)	1.06 (.106)	.4478
Block Groups	1816	1816	1816	1816
Observations	217,920	217,920	217,920	217,920

Notes. Estimates (by crime type) are based on the linear combination of $\hat{\beta}_1 + \hat{\beta}_2$ ($\Delta Enrollment_b$) from equation (3), for different values of $\Delta Enrollment_b$ (i.e., the change in student enrollment in block group b following a district-mandated school closure); the mean (standard deviation) of $\Delta Enrollment_b$ is −389.4 (231.9) students. See Table A3 for estimates of full specification of equation (3) upon which these estimates are based. For all linear combination of coefficient estimates, robust standard errors (clustered at the block group level) are reported. Linear combination of coefficients statistically significant at the *10%, **5%, and ***1% levels.

Table 8
Crime spillovers, by fraction of displaced students.

	Comparison Group					
	All Block Groups		Block Groups with Schools in 2006–07 Year		Block Groups with Schools Ever in Study Period	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Total Crime						
Closed (Mandated)	0.851** (.060)	0.849** (.061)	0.886* (.065)	0.884* (.065)	0.885* (.064)	0.883* (.065)
Closed (Other)	0.775** (.077)	0.776** (.077)	0.807** (.087)	0.807** (.087)	0.794** (.078)	0.795** (.078)
Displaced	1.004* (.002)		1.006** (.003)		1.006** (.003)	
Displaced $^{0–5\%}$		0.968 (.019)		0.982 (.031)		0.988 (.029)
Displaced $^{5–10\%}$		1.067 (.053)		1.114* (.062)		1.092 (.062)
Displaced$^{10\%+}$		1.070 (.046)		1.094* (.058)		1.095* (.057)
0-5% = 5–10% = 10%+		.0329		.0111		.0333
Mandated = Other	.4378	.4584	.4625	.4847	.3678	.3840
Panel B: Violent Crime						
Closed (Mandated)	0.694*** (.063)	0.696*** (.063)	0.707*** (.065)	0.716*** (.067)	0.709*** (.065)	0.719*** (.067)
Closed (Other)	0.829** (.075)	0.830** (.075)	0.830* (.079)	0.833* (.080)	0.840* (.077)	0.844* (.076)
Displaced	1.008* (.004)		1.009** (.004)		1.008* (.004)	
Displaced $^{0–5\%}$		1.034 (.024)		1.059* (.032)		1.069** (.032)
Displaced $^{5–10\%}$		1.043 (.074)		1.129* (.070)		1.084 (.079)
Displaced$^{10\%+}$		1.183*** (.078)		1.224*** (.087)		1.231*** (.087)
0-5% = 5–10% = 10%+		.1172		.0946		.1075
Mandated = Other	.1666	.1730	.2257	.2538	.1868	.2150
Panel C: Property Crime						
Closed (Mandated)	1.12 (.148)	1.11 (.148)	1.14 (.152)	1.13 (.149)	1.14 (.151)	1.13 (.149)
Closed (Other)	0.823*** (.031)	0.824*** (.031)	0.851*** (.032)	0.850*** (.032)	0.829*** (.032)	0.828*** (.033)
Displaced	1.003 (.005)		1.005 (.006)		1.005 (.005)	
Displaced $^{0–5\%}$		0.947*** (.018)		0.947 (.031)		0.949* (.029)
Displaced $^{5–10\%}$		1.076 (.076)		1.078 (.084)		1.082 (.081)
Displaced$^{10\%+}$		1.070 (.095)		1.076 (.102)		1.077 (.101)
0-5% = 5–10% = 10%+		.0773		.0853		.0735
Mandated = Other	.0260	.0285	.0334	.0365	.0214	.0234
Block Groups	1816	1816	282	282	300	300
Observations	217,920	217,920	33,840	33,840	36,000	36,000

Notes. Coefficients (incidence rate ratios) are reported with robust standard errors (clustered at the block group level). *Displaced* is the fraction of students (0–100 percent) attending schools in a block group who were displaced from district-mandated school closures. The mean (standard deviation) of *Displaced* among block groups receiving students displaced due to district-mandated school closures is 3.22 (5.91) percent. In columns (2), (4), and (6), *Displaced* $^{0–5\%}$ is an indicator variable which includes block groups with greater than zero but less than five percent of enrolled students who were displaced due to district-mandated school closure; *Displaced* $^{5–10\%}$ is an indicator variable which includes block groups with between five and 10 percent of enrolled students who were displaced due to district-mandated school closure; *Displaced* $^{10\%+}$ is an indicator variable which includes block groups at least ten percent of enrolled students who were displaced due to district-mandated school closure; the omitted reference category includes block groups with zero displaced students. Observations are at the block group*year*month level. All regressions include block group and month*year fixed effects. Coefficients statistically significant at the *10%, **5% and ***1% levels.

blocks was the primary contributor to the relative reduction in crime.

6.4. Crime spillovers

In light of evidence that declines in crime are driven by the magnitude of students displaced from block groups following school closure, we examined the extent to which the receipt of displaced students was associated with increases in crime. Table 8 summarizes these results. Notably, any associated increases in crime following district-mandated closure in block groups that receive displaced students is unlikely to be equal in magnitude to the estimated decline in crime following building closure. This is because of the significant dispersion of youth across block groups following closure (see Figure A1).[21] Nonetheless, we find that block groups which enrolled higher fractions of displaced students, on average, experienced increases in crime, and that the associated increases were concentrated among violent crime. In blocks with a 10% increase in the fraction of enrolled students who were displaced due to closures (i.e., from 0% of displaced students in the pre-closure year to 10% of enrolled students who were displaced due to closures in the post-closure year), total and violent crime increased, on average, by 4% and 8%, respectively (see Table 8, column 1 of Panels A and B). The effects, however, are non-linear. In block groups where at least 10% of enrolled students were displaced due to district-mandated closure, violent crime increased by 18.3%, while there were no significant effects at lower thresholds of displaced students (see Table 8, column 2 of Panel B). The estimated spillover effects are robust to restricting the comparison group to either block groups with schools in 2006–07 (the first year of the sample period) or block groups with schools ever in the study period (see columns 3–6 of Table 8).

The estimated effect of closing schools could also be overstated if it were offset by spillovers onto neighboring blocks (i.e., census blocks located in the same census tract as a building closure). While our spatial error model examined whether the standard errors were impacted by spillovers to neighboring blocks, it did not directly test for crime spillovers to neighboring blocks. To do so, we estimate a variant of equation (1) which includes treatment indicators for neighboring blocks – those blocks located in the same census tract as a building closure – that are contemporaneous with the treatment indicators for blocks containing a

[21] In the year after district-mandated closures, 3.2 percent of students enrolled in schools at the block-group level, on average, were displaced by school closings in the previous year. In the 2012-13 school year, 1.7 percent of students enrolled in schools within a block group, on average, were displaced due to district-mandated closures at the end of the 2011-12 school year. Students displaced due to district-mandated closure at the end of the 2011-12 school year enrolled in schools located in 99 different block groups (of the 293 block groups that contain both schools and students). In the 2013-14 school year, 4.2 percent of students enrolled in schools within a block group, on average, were displaced due to district-mandated closures at the end of the 2012-13 school year. Students displaced due to district-mandated closure at the end of the 2012-13 school year enrolled in schools located in 162 different block groups.

building closure.[22] We find no change in total or violent crime among neighboring blocks following both district-mandated and non-district-mandated closures; in contrast, property crime declined by 10% in neighboring blocks following district-mandated closures (see Table A4). These results suggest that the effects of school closures on violent crime are highly localized to the blocks that contained district-mandated building closures, while building closure – and the subsequent displacement of students from closure blocks – resulted in positive spillovers onto neighboring blocks via the reduction in property crime. We find that our main estimates of closures on crime among closure blocks (reported in Table 3) are robust to these models which estimate spillover effects onto neighboring blocks.

Finally, we consider the net effect of district-mandated school closings on crime. To assess the net effect, we generate model-based predictions of the count of crime for each crime type (i.e., total, violent and property) among block groups that contained a district-mandated building closure and block groups that received at least one student displaced due to mandated closure; these estimates are relative to block groups that did not contain building closures and which received no displaced students following district-mandated closures (see Table 8).[23]

[22] The estimating equation is:

$$Crime^{j}_{bmt} = \beta_0 + \beta_1\left(Closed^{mandated}_{bmt}\right) + \beta_2\left(Closed^{other}_{bmt}\right) + \beta_3\left(Neighbor^{mandated}_{bmt}\right) + \beta_4\left(Neighbor^{other}_{bmt}\right) + \gamma_b + \theta_{mt} + \varepsilon_{bmt} \quad (7)$$

In equation (7), the binary variable $Neighbor^{mandated}_{bmt}$ equals one for block group b located in a census tract that contained a district-mandated school closure whose building closed as of month m in school year t; the binary variable $Neighbor^{other}_{bmt}$ equals one for block group b located in a census tract that contained a non-district-mandated school closure whose building closed as of month m in school year t; all other variables are defined as in equation (1). The comparison group includes block groups located in census tracts that did not contain a building closure.

[23] First, we re-estimate equation (4) and calculate marginal effects to predict the count of crime among block groups that contained: (i) district-mandated building closures (n = 19 unique block groups); (ii) between zero and 5 percent of enrolled students who were displaced due to district mandated closures (n = 163 unique block groups); (iii) between 5 and 10 percent of enrolled students who were displaced due to district mandated closures (n = 24 unique block groups); and (iv) at least 10 percent of enrolled students who were displaced due to district mandated closures (26 unique block groups). For each of these four types of block groups, we then calculated the change in the predicted count of crime, as follows:

$$\Delta Crime^{j,Group_k}_{bmt} = \left[E\left(Crime^{j}_{bmt}|Group_k = 1\right)*N^{Group_k}_{b}\right] - \left[E\left(Crime^{j}_{bmt}|Group_k = 0\right)*N^{Group_k}_{b}\right] \quad (8)$$

where $Group_k \in$ {Closed (mandated), $Displaced_{0\text{-}5\%}$, $Displaced_{5\text{-}10\%}$, $Displaced_{10\%+}$} and $N^{Group_k}_{b}$ is the count of unique block groups b among $Group_k$. The net effect of district-mandated closures on the jth crime type is calculated as: $NetEffect^{j}_{bmt} = \sum_{k=1}^{K} \Delta Crime^{j,Group_k}_{bmt}$. The net effect calculation is based only on the percent change in predicted crime counts among groups for which the parameter estimates from equation (4) are statistically different from zero (at the 10 percent level). However, results based on all parameter estimates from equation (4) produce net effects that are qualitatively the same (and are available upon request).

[24] Following Aliprantis and Hartley (2015), we also calculated the total effect of closures and displacements on crime (total, violent, and property) by taking the linear combination of parameter estimates shown in column 2 of Panels A, B and C in Table 8. The linear combination of these terms shows a total decline of 27.3% and 26.4% in total and violent crime, respectively, and no effect for property crime. However, unlike our calculation of the net effect of closures on crime in Philadelphia, calculations based on the linear combination of parameter estimates does not weight the total effect by the count of block groups contained in each group (i.e., blocks with district-mandated closures, etc.).

Overall, we find that district-mandated closings led to a net decline in crime. Specifically, following district-mandated school closings, total crime declined by 15.1%; violent crime declined by 2.3%; and property crimes declined by 5.3%.[24] We also calculated net effects among block groups containing low-achieving schools and find that district-mandated school closings led to a net reduction in crime. Specifically, total crime declined by 14.8% in block groups with the lowest achieving schools following closure.[25] The net reduction in crime citywide and around lower achieving schools following closures supports prior evidence that policies that dilute the social interactions among high crime risk peers – academically low-performing students with high rates of adverse behaviors – will affect the level and location of crime (Billings and Phillips, 2017).

7. Conclusion

This study demonstrates that academically underperforming schools have a meaningful impact on neighborhood crime. We show that the reduction in crime when underperforming schools close is due, in large part, to the decline in student enrollment in the neighborhood. Specifically, our findings provide support for a criminogenic effect of academically underperforming schools on neighborhood crime, with particularly meaningful reductions in violent crime. These findings are consistent with the literature in criminology and economics that disadvantaged schools may be crime producers. Indeed, our results point to a compositional effect of having more offenders and victims present in school buildings as the key mechanism by which closing underperforming schools reduces violent crime.

These findings are consistent with theories that predict crime to be higher with the presence of more students in an area by reducing the opportunity costs for crime and the potential for criminal socialization. This conclusion is informed by evidence that the reduction in violent crime occurred only during weekday hours when students would have otherwise attended schools in the absence of building closures, and is net of the displacement of students to higher-performing schools following the district-mandated closures. These findings are in direct contrast to those concerned that closing schools would lead to increases in crime and are consistent with other research suggesting that policies that encourage the "interaction of high crime-risk students with low crime-risk peers" may help reduce crime (Billings and Phillips, 2017, p. 24).

Further, to the extent that closing schools reduces the number of police in a neighborhood patrolling school grounds (i.e., reduction in the monitoring of crime), one might expect violent crime to rise given that police presence substantially lowers violent crime (Chalfin and McCrary, 2018). Yet, the estimated decline in crime not only supports the interpretation of a shift in criminal behavior as students exit a school neighborhood following closures but may also represent a lower-bound on the impact of chronically underperforming schools on neighborhood crime. Indeed, the crime increases that would occur with the loss of additional police patrolling school locations appears to be offset by a shift in the concentration of school-aged youth who are potential offenders and victims following school closures.

While removing more academically struggling youth from an area may reduce the overall opportunities for assaults and other violent offenses to occur, concern also exists around the potential displacement of crime as these youth are compelled to attend different schools following closure. Indeed, evidence that violent crime increases in neighborhoods where larger shares of enrolled students were displaced due to school closures is suggestive of a compositional effect of moving a sizeable share of students to a new school environment. However, the net effect of district-mandated closures suggests that crime drops when academically underperforming schools close.

[25] Among block groups containing the lowest-achieving schools in Philadelphia, we find there was a net increase of 4.8% in violent crimes.

Findings from this paper are based on closing among the most academic struggling schools located in some of the most economically disadvantaged communities in Philadelphia. The extent to which these environmental factors distinguish Philadelphia from other district settings may limit the generalizability of our findings. And, the conditions that may influence whether a school setting is criminogenic, including the infrastructure of the school campus and the built environment surrounding the school, may also limit the generalizability of our findings to other geographic and school settings. Yet, our findings highlight the deleterious effect that concentrating economic and academic disadvantage into schools has on crime, supporting prior evidence that segregating disadvantaged students together in the same school increases crime (Akee et al., 2014; Billings and Phillips, 2017; Billings et al., 2019). Therefore, policy efforts at the local and district levels which aim to desegregate underperforming schools may positively impact the economic opportunities for students from the most disadvantaged backgrounds and lead to reductions in neighborhood crime. Indeed, our findings reveal that a policy in Philadelphia of closing academically underperforming schools and dispersing students to other public schools helped reduce crime.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2019.04.001.

Appendix

Table A1
Effect of School Closures on Crime: Census Tracts Containing District-Mandated Building Closures in 2012–13 Year

	Total Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Closed (Mandated)	0.903 (.079)	0.894 (.113)	0.723*** (.078)	0.738** (.108)	1.19 (.177)	1.21 (.253)
Weights	No	Yes	No	Yes	No	Yes
Mean (sd) of Dependent Variable	9.45 (7.21)	9.32 (7.04)	2.55 (2.51)	2.53 (2.50)	1.86 (2.52)	1.76 (2.24)
Block Groups	86	84	86	84	86	84
Observations	10,320	10,080	10,320	10,080	10,320	10,080

Notes. Coefficients (incidence rate ratios) are reported with robust standard errors (clustered at the block group level). Observations are at the block group*year*month level. All regressions include block group and month*year fixed effects. Weights are calculated as: $weight_t = \frac{blocks_t^{comparison}}{blocks_t}$, where $blocks_t^{comparison}$ is the count of census block groups contained in census tract t that did not contain a district-mandated building closure in the 2012-13 school year, and $blocks_t$ is the count of all census block groups in census tract t. The count of total blocks in the even columns reflects the fact that one census tract contained two blocks with only building closures. Coefficients statistically significant at the *10%, **5% and ***1% levels.

Table A2
Effect of School Closures on Crime, by School Level

	Total Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: High Schools						
Closed (Mandated)	0.794*** (.034)	0.794*** (.034)	0.599*** (.055)	0.596*** (.055)	1.08 (.058)	1.08 (.058)
Closed (Other)	0.753** (.085)	0.753** (.085)	0.807** (.086)	0.805** (.086)	0.817*** (.035)	0.818*** (.035)
P-Value from F-Test:						
Mandated = Other	.6551	.6598	.0338	.0327	.0000	.0000
Mean (sd) of Dependent Variable	8.91 (8.94)	8.75 (8.89)	2.01 (2.26)	1.95 (2.20)	2.22 (3.30)	2.16 (3.23)
Block Groups	1620	1535	1620	1535	1620	1535
Observations	194,400	184,200	194,400	184,200	194,400	184,200
Panel B: Elementary & Middle Schools						
Closed (Mandated)	0.910 (.115)	0.903 (.114)	0.817* (.088)	0.817* (.088)	1.15 (.295)	1.15 (.294)
Closed (Other)	0.934 (.059)	0.929 (.059)	1.05 (.072)	1.05 (.073)	0.847*** (.051)	0.845*** (.052)
P-Value from F-Test:						
Mandated = Other	.8562	.8429	.0525	.0515	.2450	.2500
Mean (sd) of Dependent Variable	8.88 (8.89)	8.70 (8.83)	2.01 (2.21)	1.93 (2.14)	2.17 (3.32)	2.15 (3.24)
Block Groups	1712	1528	1712	1528	1712	1528
Observations	205,440	183,360	205,440	183,360	205,440	183,360

Notes. Each column within a panel reports a separate regression. Coefficients (incidence rate ratio) are reported with robust standard errors (clustered at the block group level). Observations are at the block group*year*month level. In columns (1), (3) and (5), the sample includes all blocks groups without schools and block groups that contain schools of the same school level (i.e., high schools or elementary/middle schools); in columns (2), (4) and (6), the sample restricts comparisons to block groups without schools. Panel A includes census block groups with high schools (i.e., schools serving grades 9–12); Panel B includes census block groups with elementary and middle schools (i.e., schools serving grades K-8). All regressions include block group and month*year fixed effects. Statistically significant at the *10%, **5% and ***1% levels.

Table A3
Heterogeneous effects of school closures on crime, by change in student enrollment.

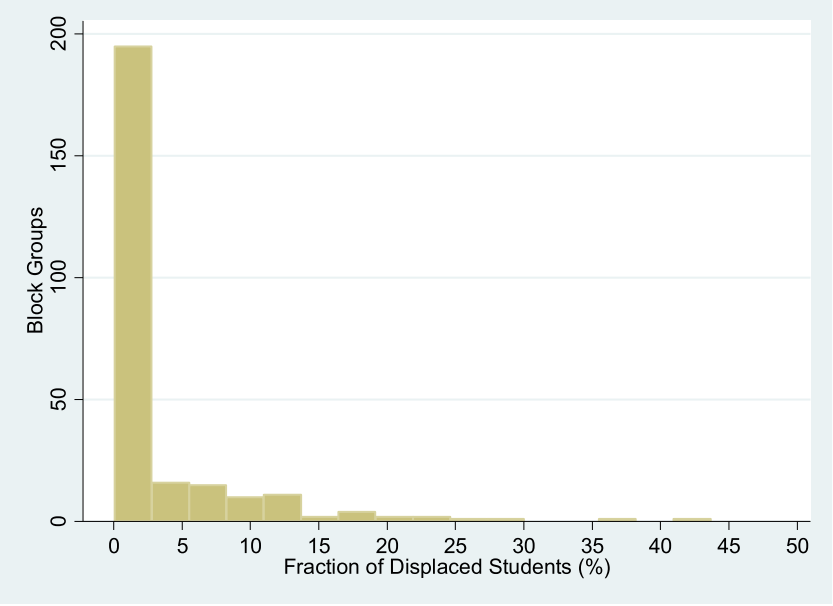
	Total Crime	Violent Crime	Property Crime
Closed (Mandated)	0.970 (.132)	0.809 (.105)	1.24 (.290)
Closed (Mandated)*$\Delta Enrollment_b$	1.000 (.0002)	1.000*** (.0001)	1.000 (.0003)
Closed (Other)	0.720*** (.082)	0.776*** (.075)	0.815*** (.036)
Closed (Other)*$\Delta Enrollment_b$	0.999** (.0001)	0.999** (.000)	0.999 (.000)
Block Groups	1816	1816	1816
Observations	217,920	217,920	217,920

Notes. Coefficients (incidence rate ratio) are reported with robust standard errors (clustered at the block group level). $\Delta Enrollment_b$ is the change in the count of enrolled students between the pre- and post-closure years among block groups containing school building closures. All regressions include block group and month*year fixed effects. Observations are at the block group*year*month level. Coefficients statistically significant at the *10%, **5% and ***1% levels.

Table A4
Spillover effects of school closures on neighboring blocks.

	Total Crime	Violent Crime	Property Crime
Closed (Mandated)	0.852** (.060)	0.694*** (.063)	1.11 (.147)
Closed (Other)	0.776** (.077)	0.832** (.075)	0.819*** (.031)
Neighbor (Mandated)	1.033 (.090)	1.025 (.075)	0.898** (.046)
Neighbor (Other)	0.978 (.038)	1.029 (.037)	0.996 (.037)
P-Value from F-Test:			
Mandated = Other	.4407	.1569	.0283
Mandated = Neighbor (Mandated)	.0840	.0008	.1386
Other = Neighbor (Other)	.0292	.0279	.0001
Mean (sd) of Dependent Variable	9.07 (8.98)	2.09 (2.32)	2.23 (3.38)
Block Groups	1816	1816	1816
Observations	217,920	217,920	217,920

Notes. Coefficients (incidence rate ratios) are reported with robust standard errors (clustered at the block group level). Observations are at the block group*year*month level. All regressions include block group and month*year fixed effects. Coefficients statistically significant at the *10%, **5% and ***1% levels.



Notes. Figure shows the fraction of enrolled students in a block group who were displaced due to district-mandated closures at the end of the 2011-12 or 2012-13 school years and who enrolled in schools in the 2012-13 and 2013-14 school years following closure, among blocks groups that received at least one displaced student. There were 181 unique block groups (of 293 that contain schools and students) that received at least one displaced student in either the 2012-13 or 2013-14 school years (a total of 261 block groups received at least one displaced student in either 2012-13 or 2013-14). In 2012-13, the mean (standard deviation) of the fraction of enrolled students, among the 99 blocks groups that received at least one displaced students, was 1.69 (3.46) percent. In 2013-14, the mean (standard deviation) of the fraction of enrolled students, among the 162 blocks groups that received at least one displaced students, was 4.15 (6.86) percent. Across the 2012-13 and 2013-14 years, the mean (standard deviation) of the fraction of enrolled students, among the 261 blocks groups that received at least one displaced students, was 3.22 (5.92) percent.

Fig. A1. Distribution of the Fraction of Students Displaced due to Mandated Closures.

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