Effect of public transit on crime: evidence from SEPTA strikes in Philadelphia

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Abstract

Objectives We use the temporary closings of subway stations in Philadelphia to examine the effects of public transit on crime in the nearby communities.
Methods The Southeastern Pennsylvania Transportation Authority (SEPTA), a regional public transportation authority in Philadelphia, has experienced two labor strikes occurring in 2009 and 2016. During these two strikes, public transit was disrupted for nearly 1 week. We used the closings of 47 subway stations during strikes to evaluate the place-based effect of public transit on crime. We also examined whether these effects varied with the ridership level in each station.
Results Total crime decreased by 38% within 100 m of the stations when subway stations were closed due to the strike and by 10% within 500 m. The primary drivers of the decrease were violent crimes, including assault and robbery. However, there is no obvious relationship between the closings of stations and the change in property crimes and mischief. In addition, crimes in stations with higher ridership declined less than those in stations with medium ridership when strikes occurred.
Conclusions Public transit in Philadelphia appears to be associated with elevated violent crime in the surrounding community. Areas around subway stations may require greater security to control crime during its operational hours. Passengers may serve as guardians to deter some crimes when the public transit is operational.

Keywords Place-based criminology · Natural experiment · Transit security

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Introduction

Crime may concentrate in small geographical units (Taylor 1997; Weisburd 2015; Braga et al. 2010). The public often views public transit stations as crime hotspots. Public transit stations are populated by anonymous riders who represent easy targets under specific circumstance (Smith and Clarke 2000; Loukaitou-Sideris et al. 2001). Public transit may also provide potential criminals improved access to neighborhoods and provide cover for them (Poister 1996; Block and Block 2000). However, there are also reasons to believe that public transit might cause neighborhood crime to fall, rather than rise. Despite the increased access, public transit may also export criminals living within the neighborhood to commit their crimes elsewhere (Phillips and Sandler 2015). Moreover, the existence of public stations may decrease the journey-to-work cost and increase the job accessibility of neighborhood residents, thereby increasing the opportunity cost of crime (Ihlanfeldt 2003; Liggett et al. 2003). Also, additional police may patrol the station areas, and cameras and other security equipment may be installed, which make areas safer (Brantingham and Brantingham 1993; Draca et al. 2011; Klick and Tabarrok 2005). Therefore, the correlation between public transit and crimes may vary from different neighborhoods depending on the relative magnitudes of the above factors.

A number of studies have been conducted to test the effect of public transit on crime, which gives us a general picture of how the crime rate may vary with long-term or short-term changes in public transit. Some cross-sectional studies suggest that certain types of crime tend to cluster around public transit stations (Block and Block 2000; Loukaitou-sideris 1999). However, a few quasi-experimental studies using long-term changes in public transportation suggest no relationship between transit and crime (Billings et al. 2011; Ridgeway and MacDonald 2017).

This research used labor strikes of the Southeastern Pennsylvania Transportation Authority (SEPTA), a regional public transportation authority in Philadelphia, to explore the effects of public transit on crime. SEPTA has experienced two labor strikes occurring in 2009 and 2016. During these two strikes, public transit services including bus, subway, and electric trolleybus were disrupted for nearly 1 week in Philadelphia. These events provide a quasi-experimental context to explore the causal effect of public transit on crime. Moreover, since the effects of public transit on crime may result from changes in the spatial distribution of passengers or potential perpetrators, this study tries to address what attributes of neighborhoods may affect the relationship between public transit and crime.

The remainder of this article has six sections. The "Theoretical framework and prior literature" section reviews the theoretical framework and prior literature on public transit and crime. The "Data and methods" section presents the data and methods. The "Results" section presents empirical results and a robustness check. The "Discussion" section discusses the findings and policy implications of our findings.

Theoretical framework and prior literature

Routine activity theory indicates that structural changes in routine activity patterns can influence crime rates by affecting three elements, i.e., (1) motivated offenders, (2)

suitable targets, and (3) capable guardians (Cohen and Felson 1979). We use this theoretical framework to explain how public transit may influence crime.

First, public transit stations may provide a favorable environment for potential offenders. The operation of public transit can provide cover for them because offenders can act as normal riders and their crimes may not be easily detected as suspicious activity. Also, the physical features of stations can provide easy exit and entry before and after the crime (Block and Block 2000; Loukaitou-Sideris et al. 2001). Some scholars suggest that commercial and transitional areas are more likely to attract offenders than residential areas (Poister 1996). Because transit lines may lead to the expansion of commercial areas and areas with mixed land use patterns, transit extensions could make certain neighborhoods more attractive to potential offenders.

More importantly, criminals may be willing to travel a certain distance to commit a crime. Public transit is supposed to increase the mobility of people wishing to travel to distant locations (Poister 1996; Liggett et al. 2003). Consequently, on the one hand, as the percentage of the neighborhood served by transit increases, the average cost of travel from outside the neighborhood to locations within the neighborhood declines, which causes more crime by outsiders. On the other hand, transit increases the average speed of travel to other neighborhoods, and may thereby reduce the probability that local criminals will commit crimes in the home neighborhoods (Ihlanfeldt 2003).

Second, public transit can also increase the targets' suitability because they gather crowds of passengers and save the amount of time necessary for a criminal to find victims (Brantingham and Brantingham 1993; Liggett et al. 2003). In addition, because passengers typically live away from the area, they are often stationary and unguarded, making them easier prey for criminals (Smith and Clarke 2000). Finally, the gathering of large number of people at transit locations could also generate extra litter and other signs of social disorder. According to the broken windows theory, this may signal that an area is uncared for and cause more serious crime to flourish (Kelling and Wilson 1982).

Third, riders as well as shop owners, managers, or employees around stations may also have a deterrent effect in reducing crime (Brantingham and Brantingham 1993; Smith and Clarke 2000). In the long-term, the opening of a public transit station usually implies increased security equipment and additional police patrol (Billings et al. 2011). On the one hand, routine activity theory indicates that this may increase the capable guardianship in certain communities and lead to substantial decreases in crime rates. On the other hand, the broken windows theory also posits that crime could decrease since these structures could remove signs of social disorder and may signal to potential offenders that public cares about their community.

Therefore, according to the theoretical framework, public transit could affect crime in either direction. Moreover, the theories could also explain why the true effect of public transit on crime may depend on factors such as whether changes in public transit are temporary or permanent, as well as the relative magnitudes of certain neighborhood characteristics. For example, because we cannot expect that the temporary closure of public stations will influence land use or substantial investments in certain neighborhoods, short-term disruption of public transit cannot have an impact on crime through these mechanisms. In addition, the relative magnitudes of the effect of public transit could also vary with neighborhood characteristics, since public transit could interact with the tendency of a neighborhood to import or export crime. Therefore, the increased mobility caused by public

transit cannot raise the crime rate unless the affected neighborhood has a strong tendency to import crime.

Much of the earlier work on public transit and crime is cross-sectional studies. By analyzing robbery data occurring on the street from October 1995 through October 1996, Block and Block (2000) found higher street robbery rates around transit stops in Chicago and New York than in other parts of these cities. Loukaitou-Sideris et al. (2001) used a stratified random sample of sixty bus stops in downtown Los Angeles, and found that certain urban forms and bus stop characteristics such as street layouts, environmental disrepair and desolation, and physical form elements can have an impact on the relationship between a bus stop and crime. However, the limitations of cross-sectional studies are that they cannot isolate public transit from other rival causal factors. Just as Block and Block (2000) admitted in their paper, the transit stops were more likely to be located near bars and other businesses that may be sources of crime, raising the question of whether it is transit or other nearby land use are responsible for more crime.

Some researchers investigated the relationship between crime and public transit using a quasi-experimental approach. Prior studies aimed to estimate the long-run effects of public transit access by measuring changes in crime occurring before and after the construction of a new rail station or rail lines. Poister (1996) presented a preliminary time series analysis of crime incidence in suburban neighborhoods surrounding two rapid rail stations in GA. His study showed that some reported crime increased when the stations opened, but the increases were followed by a decreasing crime trend to earlier levels over the next several months. Similarly, using the opening of the Green Line train in Los Angeles, Liggett et al. (2003) found that the transit line had no significant impact on crime trends and that public transit had not transported crime from the inner city to the suburbs. However, lack of a proper control group prevents these time-series studies from excluding the possibility that events other than the change in public transit may influence crime trends as well. Billings et al. (2011) estimated the effect of the announced and actual opening of a light rail transit system in Charlotte, NC on neighborhood crime. After implementing a before-and-after methodology using two alternate transit corridors to control for differences between neighborhoods, they found that light rail did not increase crime around stations. Instead, a decrease in property crimes was found once the station locations were announced, which remained relatively stable after the operation. Finally, using the train schedule changes in Washington, DC as exogenous shocks, Jackson and Owens (2011) showed that although there was little effect of expanded public transit service on DUI arrests, areas where bars are within walking distance to transit stations experienced decrease in DUI arrests, replacing them with other alcohol-related arrests.

Overall, previous literature on long-term effects tends to support the idea that transit expansion will not bring crime. However, this conclusion cannot be directly used to show that greater security or police place-based interventions during the operation of public transit are not necessary. First, our theoretical framework suggests that the long-term extension of public transit could influence land use in certain neighborhoods. It may be the economic development that was spurred around public transit stations offset the negative effect of public transit on crime. Second, it is also possible that existing crime trends could influence the placement and timing of public transit construction. This may mute our ability to detect the causal effect of public transit on crime.

Some recent studies used temporary openings and closings in the public transit system to evaluate its impact on crime, which represent the most similar research to

ours. Phillips and Sandler (2015) examined whether public transit access affected crime based on temporary maintenance-related closures of stations in the Washington, DC rail transit system. They found that closing one station reduced crime by 5%, and the effect was larger at stations that tend to import crime. However, the conclusion that the short-term disruption of public transit is associated with a decrease in crime rates does not always hold. For example, Ridgeway and MacDonald (2017) assessed the effect of the Los Angeles Metro Rail system on crime in neighborhoods. For short-term effects, they evaluated crime around the periods of public transit disruptions due to labor strikes in Los Angeles. The results are consistent with their findings on the long-run effect of new transit station openings; there is no evidence that the opening or disruption of a public transit station resulted in changes in crime in the surrounding neighborhoods. Using an adjusted empirical method, however, DeAngelo et al. (2019) found that strikes in Los Angeles are associated with an increase in certain crimes, indicating that aggravated assaults rose by 18.7% and aggregate property crimes increased by 5.7% in the affected area within the period of strikes in Los Angeles.

Although studies using strikes in Los Angeles show that the operation of public transit does not appear to be associated with a decrease in crime, we cannot simply generalize this conclusion to other cities. One reason is that Los Angeles did not have a rail transit system until the 1990s (Wachs 1993). Many people in Los Angeles still rely on automobiles and an expansive network of freeways as their dominant mode of transportation. Therefore, the closing of public transit in Los Angeles influenced a relatively small proportion of residents. However, it is possible that the disruption of public transit may have more significant effect on people's daily life in cities such as Philadelphia, where a large portion of residents has relied heavily on public transportation for decades. This could also result in a detectable change in the spatial distribution of crime.

This research uses a quasi-experimental approach to study the effects of public transit on crime in Philadelphia, a city having its rapid transit lines operated for more than a century (Cheape 1980). Following Ridgeway and MacDonald's (2017) and DeAngelo et al.'s (2019) research design, we use the SEPTA strikes in Philadelphia as an external shock to detect the causal relationship between temporary disruption of public transit and crime. In our study, we do not expect that public transit can affect crime by changing land use or substantial investments in infrastructure or security equipment in such a short period. Moreover, considering their studies did not assess whether the relationship between public transit and crime varies with ridership; this paper adds these two variables to examine whether the effect of station closures on crime depends on these factors.

Data and methods

Crime data

The Philadelphia Police Department publishes detailed incident-level reported crime data (Philadelphia Police Department 2017). For every crime incident, the data indicate the type of offense as well as the corresponding date, latitude, and longitude. We group offense types into the following categories: "Total," "Assault," "Robbery," "Burglary,"

"Theft/Larceny," and "Mischief."[1] We include data both from 2009 and 2016; years in which public transit was interrupted at some time because of strikes.

SEPTA subway system in Philadelphia

The SEPTA is a state-created public transportation authority that operates the bus, rapid transit, commuter rail, light rail, and electric trolleybus services for Philadelphia and the surrounding counties of Delaware, Montgomery, Bucks, and Chester.

Several unions represent employees working in different divisions of SEPTA. Among them, Transportation Workers Union (TWU) Local 234 represents more than 4700 members who work in the City Transit Division. This division operates almost all of Philadelphia's public transit, including all subway, bus, and trolley services within Philadelphia. TWU twice went on strike since 2006 because of failed negotiations with SEPTA. The first strike began on November 3, 2009 and ended on November 8, 2009. The second strike lasted from November 1, 2016 to November 7, 2016. SEPTA was forced to shut down almost all the busses, trolleys, and subways within Philadelphia during both strikes. This affected the system's 400,000 daily riders (SEPTA 2009, 2016a, b, c, d).

Both strikes happened because SEPTA and the union were not able to reach an agreement on wages, pensions, and work conditions, and they ended as soon as a tentative contract was accepted by both parties. On October 28, 2009 and October 26, 2016, both 6 days before the strikes started, SEPTA posted an announcement on its official website saying that the current contract between SEPTA and the TWU Local 234 would expire 6 days later. Notably, it disclosed that while negotiation was ongoing, customers should be prepared for the possibility of a stoppage of public transit if a strike occurred (SEPTA 2009, 2016a, b, c, d).Therefore, some citizens in Philadelphia could be aware of the possibility of a strike in the future. During both strikes, several local media outlets in Philadelphia released daily reports on the process of negotiation and the chaos generated from the strike (Lin et al. 2009; Sasko 2016). Moreover, SEPTA regularly updated service interruption information on its website until the public transportation services resumed (SEPTA 2016a, b, c, d). This media attention and information disclosures suggest uncertainty about the strike end date.

Because both strikes happened due to the failure of reaching a new agreement between SEPTA and TWU Local 234, crime was not an ingredient in the strike decisions. Consequently, for our analyses, it is reasonable to believe that openings and closings of public transit stations due to strikes are exogenous shocks to the community, providing us a quasi-experimental context to determine the causal effect of public transit on crime.

This analysis focuses on crimes around 47 stations along two subway lines in Philadelphia, i.e., the Broad Street Line and the Market-Frankford Line. Both lines play an integral part in Philadelphia's daily public transportation. On an average

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[1] The "assaults" category includes both aggravated assault and other assaults. The "robbery" category includes robberies with or without firearms. The "theft/larceny" category include motor vehicle thefts, thefts from vehicle, and other thefts. The "mischief" category includes receiving stolen property, disorderly conduct, gambling violations, liquor law violations, prostitution/commercialized vice, public drunkenness, vagrancy/loitering, vandalism/criminal mischief, and driving under the influence. The "Total" category includes all of the above offenses as well as other rare crime types such as homicide, rape, and arson.

weekday, the Broad Street Line and the Market-Frankford Line carry around 140,000 and 180,000 passengers, respectively (SEPTA 2014; SEPTA 2016a, b, c, d). During both strikes, all stations along these two lines were closed, blocking all public access to these lines and stations. Combining the crime timing and locations with the station closings, we aimed to examine whether the stoppage of public transit influenced crime in the surrounding area. We considered an area to be affected by public transit if it was within a given radius around a station. We used radii of 100 m, 200 m, 300 m, 400 m, and 500 m and reported the results for each of these. Figure 1 shows the geographical distribution for a 200-m buffer radius around each station. Buffers around each station can overlap when considering larger radii. To avoid counting a specific crime located in the overlapping area multiple times, we assigned each crime to its nearest station. For example, consider a crime occurring in a location 250 m from station A and 450 m from station B, it will be labeled as a crime near station A within the buffer radius of 300 m, 400 m, and 500 m. But it is not counted as a crime near station B within the buffer radius of 500 m.

We included a crime in the original analysis only if it occurred between 1 month before and 1 month after one of the strikes and occurred in an area within the buffer radius of a station. Therefore, for each radii, our data include 6251 (47×133) observations.
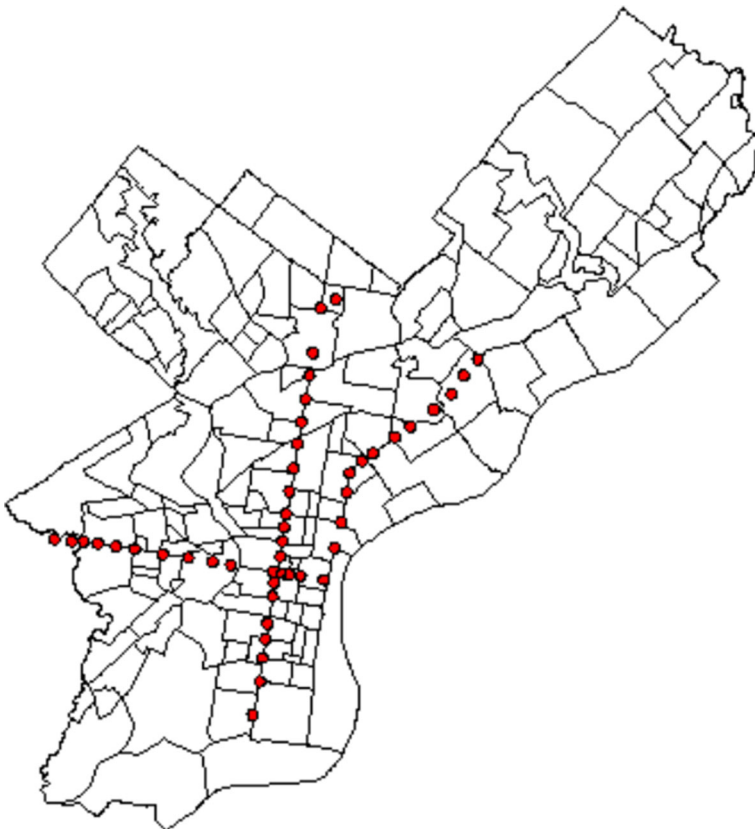

Fig. 1 Station buffers with a 200-m buffer radius

Table 1 presents the average number of crimes committed around all 47 stations in samples of different radii. The total crime row of Table 1 shows that, on average, 23 crime incidents occurred within a 100-m radius around transit stations; areas within 200-m radius around transit stations report 46 crime incidents; areas within 500-m radius around transit stations have 129 crimes in a typical day. In addition, the descriptive statistics also suggest that crime density is much higher in areas close to transit stations than that far away from stations. For example, the areas within 200-m radius are 4 times larger than the areas within 100-m radius, but the crime count within 200-m radius are only twice as that within 100-m radius.

Poisson regression models

The following Poisson regression model estimates whether the closings of the transit stations due to strikes had an effect on crime:

$$y_{it} \sim \text{Poisson}(\lambda_{it})$$

$$\log(\lambda_{it}) = \beta_0 + \beta_1 \text{strike}_t + \beta_2 I(\text{year}(t) = 2016) + \beta_3 I(\text{year}(t) = 2009)t$$
$$+ \beta_4 I(\text{year} = 2016)t + \eta_i, \tag{1}$$

where y_{it} is the observed number of crimes reported in buffer i on day t, and λ_{it} is the expected crime count in buffer i on day t, strike_t is a 0/1 indicator of whether the strike was active at time t. The model also includes a fixed effect for the year, making 2009 the reference year. β_3 and β_4 model the crime trend, allowing slopes of crime trends to

Table 1 Descriptive statistics for key crime variables within a given radius of transit stations

		Radius around stations				
Crime type		100 m	200 m	300 m	400 m	500 m
Total crime	Mean	22.9	45.8	72.4	102.9	128.5
	SD	9.3	11.6	15.1	19.0	21.9
Assault	Mean	2.6	5.6	9.8	14.7	19.1
	SD	2.0	3.0	3.9	4.9	5.5
Robbery	Mean	1.1	2	3.3	4.7	5.6
	SD	0.9	1.4	1.7	2.1	2.3
Burglary	Mean	0.3	1.0	2.2	3.8	4.9
	SD	0.6	1.0	1.7	2.9	3.1
Theft/Larceny	Mean	4.8	10.3	16.5	22.8	28.1
	SD	2.6	4.2	5.4	6.2	6.9
Mischief	Mean	10.2	20.1	31.3	44.5	46.2
	SD	5.6	7.4	9.6	11.9	12.1
Number of observation days		133	133	133	133	133

differ in 2009 and 2016. η_i is a station fixed effect. Our quantity of interest is $\exp(\beta_1)$ which measures how many times larger (or smaller) the crime rate is with the closing of a nearby station due to the strike. Since the model does not differentiate the effect of strikes in 2009 and 2016, $\exp(\beta_1)$ represents the average effect over both periods. We computed robust standard errors for all estimates because of extra-Poisson variation (i.e., overdispersion) in the crime counts.

One threat to the validity of significance tests based on regression models can be sensitive to distributional assumptions such as overdispersed counts, auto-correlation, and clustering (Wang and De Gruttola 2017). Therefore, we use a permutation test as a non-parametric approach to conduct a significance test. This test has already been applied in other criminological research (Ridgeway and MacDonald 2017; Moyer and Ridgeway 2018; Faraji et al. 2018).

The general idea of permutation tests is to randomly permute the treatment label, recompute the test statistic, and generate a reference distribution for the parameters of interest. In our analysis, for each permutation, we randomly selected two periods in 2009 and 2016, respectively, labeled them as strike periods, recomputed model (1) using crime data 1 month before and after the selected periods, and obtained the coefficient of interest. We repeated this randomization of strike periods 1000 times, refitted the model, and generated the null reference distribution using the 1000 collected estimates. The permutation p value is the fraction of test statistics that are as or more extreme than the estimates obtained from the correctly labeled strike periods.

In addition, because the relationship between public transit and crime may depend on station attributes and neighborhood characteristics, we further considered two factors in our analysis to assess the heterogeneous effects of public stations on crime, i.e., (1) the spatial distribution of passengers, and (2) the local crime levels.

The spatial distribution of passengers can locally affect crime in two ways. On the one hand, passengers can be potential victims of crimes (Smith and Clarke 2000). If the spatial distribution of passengers influences crime mainly through this channel, we expect that stations of higher ridership, which contain more victims on normal days, would experience a more significant drop in crime during strike times. On the other hand, however, passengers can serve as capable guardians to deter offenders from committing certain crimes in public (Cohen and Felson 1979). Consequently, if this is true, the crime rate in stations of higher ridership would increase rather than decrease during strike times due to the lack of sufficient surveillance.

The SEPTA Open Records Office, per a Freedom of Information Act request, provided ridership survey data on the Broad Street Line and Market-Frankford Line from 1992 to 2016. Every few years, SEPTA randomly selected several weekdays and weekends to count the number of boards and leaves for each station in these two lines. This survey data shows that levels of ridership in each station which remains consistent across all years. Our daily observations also demonstrate that daily ridership is relatively stable in most stations. "Hot" stations are full of riders almost every day. We graded each of the 47 stations as low, medium, or high level of ridership.[2] Since during

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[2] We labeled 11 stations with the greatest number of passenger boards as stations of high level of ridership. All these stations have over 7000 boards on an average weekday. And we labeled 10 stations with the smallest number of passenger boards as stations of low level of ridership. All these stations have fewer than 3000 boards on an average weekday.

the strike times, all stations were equally empty of riders, if the distribution of passengers is a key important factor, we expect that the crime change in the stations of high ridership would be different from that in the stations of low ridership.

Therefore, to examine whether crime changes around a subway station during strikes are dependent on the ridership of a station:

$$y_{it} \sim \text{Poisson}(\lambda_{it})$$

$$\log(\lambda_{it}) = \beta_0 + \beta_1 \text{strike}_t + \beta_2 \text{low ridership}_i + \beta_3 \text{high ridership}_i + \beta_4 \text{low ridership}_i \quad (2)$$

$$\times \text{ strike}_t + \beta_5 \text{high ridership}_i \times \text{strike}_t + \beta_6 I(\text{year}(t) = 2016)$$

$$+ \beta_7 I(\text{year}(t) = 2009)t + \beta_8 I(\text{year} = 2016)t,$$

In model (2), we only focus on areas within the buffer radius of 100 m around a station, so that y_{it} is the number of crimes reported in the 100-m buffer around station i at day t. The coefficients of the interaction terms (β_4, β_5) are of primary interest. They capture whether the effect of strikes differs by the ridership of a station. $\exp(\beta_4)$ measures how many times larger the change of crime rate is in a station of low ridership during a strike compared with those stations of medium ridership. $\exp(\beta_5)$ measures how many times larger the change of crime rate is in a station of high ridership during a strike compared with those stations of medium ridership.

Results

The main effect of strikes on crime

Figure 2 and 3 show time series plots of the daily crime count for buffers with a 100-m radius around 47 stations 2 weeks before through 2 weeks after the strikes in 2009 and 2016, respectively. As can be seen in Fig. 2, we find that crime decreased sharply during
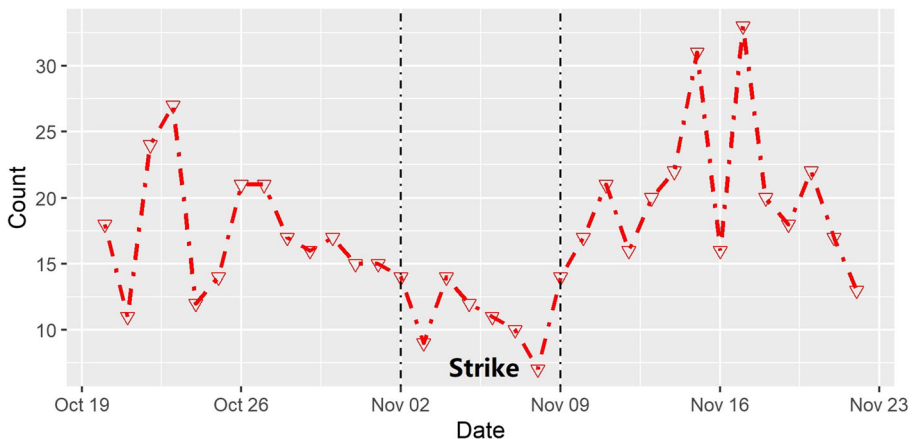

Fig. 2 Total daily crime count within 100 m of transit stations before, during, and after the 2009 strike
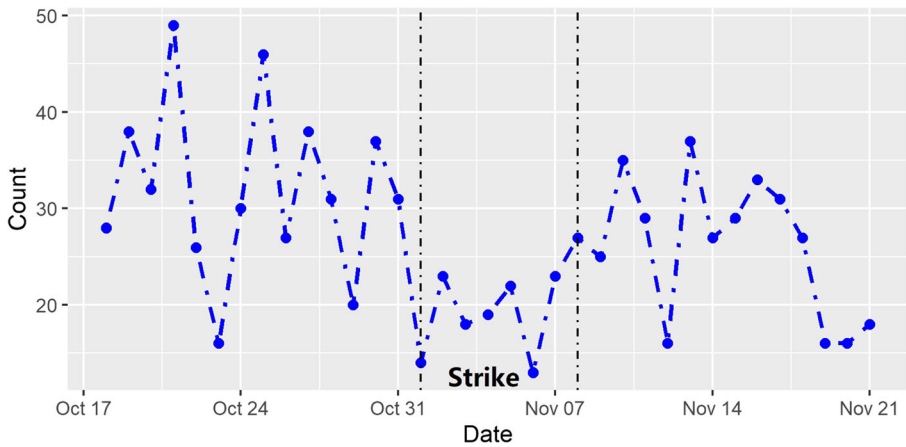
Fig. 3 Total daily crime count within 100 m of transit stations before, during, and after the 2016 strike

the first 2 days of the strike and then increased on the third day. But the local maximum of the crime count during the strike was still smaller than days outside the strike period. After that, crime continued to decrease and reached its local low of fewer than 5 crimes per day. However, crime increased dramatically as soon as the strike in 2009 ended, and the daily crime count remained more than 15 afterwards. Figure 3 also shows that the daily crime count decreased sharply from more than 30 to fewer than 10 on the first day of the strike in 2016. After that, crime fluctuated at a lower level for the next 5 days and increased sharply right after the disruption in public transit came to an end. Both figures indicate that the crime count dropped and increased in affected areas when a nearby public station closed and reopened, signaling a relationship between crime and public transit operation.

Figure 4 shows the permutation test null distribution for what model (1) would estimate to be the percent change in total crime attributable to strikes in areas within 100 m, assuming strikes and crime are in fact not related. As can be seen in the figure, after randomly shuffling the strike periods, estimated effects between a decrease of 20% or an increase of 20% could reasonably be by chance. However, the test statistic obtained in our
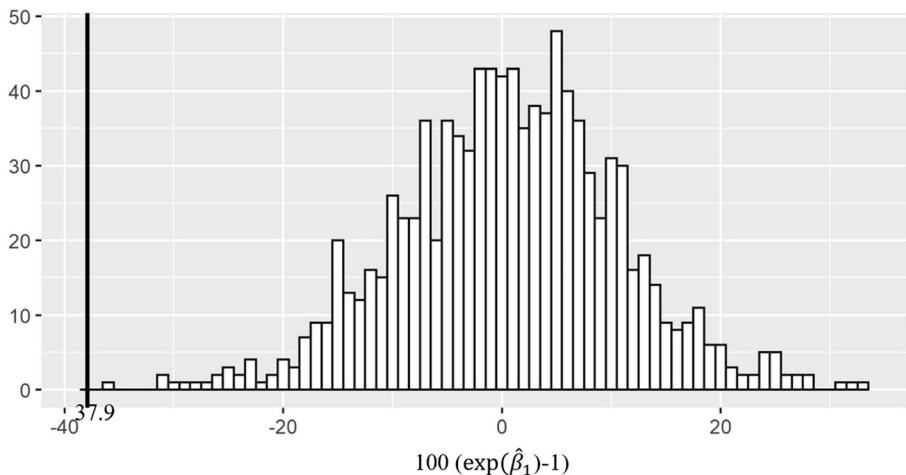

Fig. 4 Null distribution for the effect of periods on crime within 100 m

Table 2 Percent change in crime for areas within a given radius of a transit station (and permutation test p value) associated with station closings due to strikes

	Average crime count per day within 300 m of stations	Radius around stations				
		100 m	200 m	300 m	400 m	500 m
Total crime	72	−37.9	−22.4	−15.4	−12.8	−9.9
		<0.001*	0.006*	0.034*	0.04*	0.12
Assault	10	−60.9	−36.2	−15.2	−18.6	−17.5
		0.002*	0.012*	0.194	0.06	0.056
Robbery	4	−59.5	−18.4	−23.2	−22.1	−17.5
		0.022*	0.372	0.118	0.11	0.138
Burglary	3	−19.0	52.9	10.7	−15.5	−17.0
		0.736	0.154	0.608	0.382	0.262
Theft/larceny	17	−25.7	−14.2	−13.1	−11.4	−10.4
		0.08	0.148	0.136	0.148	0.17
Mischief	32	−16.9	−12.6	−6.5	−3.1	4.6
		0.228	0.238	0.39	0.638	0.666

For each crime type and for each radius, we show the estimated percent change in crime, computed as $100\left(\exp(\hat{\beta}_1)-1\right)$, and the permutation test p value. The second column shows the average number of crimes per day within 300 m of the station areas to give the reader an idea of the change of crimes that occur during the strikes

original data is −37.9%, with a permutation p value less than 0.001, indicating that the estimated coefficient is outside normal random variation and is statistically significant.

We repeated the analysis using all crime categories and buffers with various radii. Table 2 shows the percent change in crime attributable to the closings of subway stations for each of the crime categories using permutation p values. We used different sizes of the radius around each station in order to assess the range of the station's effect. We found that the total crime count decreased after the stations were closed during strikes. For example, the 300 m column in Table 2 shows that the total crime count decreased by 15.4% within the 300-m buffers. Considering the average crime count per day within 300 m of stations is 72, this shows that strikes were associated with 11 fewer crimes within a 300-m radius around the stations.

In addition, the closings of subway stations did not affect all crime categories to the same extent. We find strong evidence that rates of violent crimes including assault and robbery were substantially lower when stations were closed due to a strike. Within 100 m of a station, the percentage of assault significantly declined by 61% and the percentage of robbery significantly declined by 60%. However, the results demonstrate that property crimes, such as burglary and theft, as well as mischief did not significantly change after a nearby public transit station was closed.

Results demonstrating a consistent gradient effect depending on the doses of treatment can provide evidence of a causal relationship (Wakeford 2015; Moyer and Ridgeway 2018; Faraji et al. 2018). When arguing for the causal effect, we expect to observe a stronger effect of the station closings in the areas closest to them, and a smaller effect as the radius is expanded to include more areas further away from the stations. As can be seen in Table 2, the effect of public transit decreased when we expand the radius of the buffers. For example, the percentage change of total crime declined from 37.9% in the 100-m radius to 12.8% in

the 400-m radius and becomes no longer significant in the 500-m radius. The percentage change of assault declined from 60.9% of the 100-m radius to 36.2% in the 200-m radius and becomes statistically insignificant when we consider a radius beyond 200 m. The percentage change of robbery is only significant with the 100-m buffer; it becomes insignificant once we consider a radius of more than 100 m. These findings support the conclusion that the closings of public transit stations caused certain crimes to decline.

Heterogeneous effects

Table 3 shows the relative change in crime rates from the model (2), with and without a strike compared with the stations with medium levels of ridership. The results show that the relative change in total crime in stations with low ridership was a 36.9 decrease compared with the stations with medium levels of ridership, and the relative change in total crime in stations with high ridership was an 52.9 increase compared with the stations with medium levels of ridership. We found similar results in crime categories including assault, theft, and mischief. This appears to support the theory that passengers may serve as capable guardians to deter certain crimes. When a strike causes all stations to be equally empty of riders, stations with a higher level of ridership on normal days may experience a relative increase in certain crimes because they lose more guardians.

Robustness

One potential threat to a simple before-after time-series design in our analysis is that the changes in crime can be attributable to other events in Philadelphia or seasonality. One solution to address this concern is to add appropriate control groups to the time series analysis and conduct difference-in-differences analyses. In addition, our original research design uses a sample including crimes that occurred between 30 days before and 30 days after strikes. In this section, we replaced our original sample with samples including crimes that happened 15 days before and after strikes and 60 days before and after strikes, respectively. Finally, we added more control variables to control for days of week effects and allow the linear trend to be different before, during, and after the strike.

Our first difference-in-differences analysis compares crime activity within the 100-m radius of a station in the same time periods of different years. For example, the 2009

Table 3 Heterogeneous effect of the SEPTA strikes on crime within 100-m radius

Crime type	Low ridership stations	95% CI	High ridership stations	95% CI
Total crime	− 36.9*	(− 46.9, − 27.8)	52.9*	(36.6, 70.9)
Assault	− 36.1*	(− 57.4, − 6.8)	63.4*	(22.9, 116.4)
Theft/larceny	− 29.9*	(− 47.3, − 7.9)	108.5*	(71.4, 153.5)
Mischief	− 38.9*	(− 52.5, − 2.2)	34.6*	(11.6, 61.2)

For each crime type and for each interaction term, we show the estimated percent change in crime computed as $100\left(\exp\left(\hat{\beta}_1\right)-1\right)$, and a 95% confidence interval. Asterisk marks estimates with p values less than 0.05. Standard errors used in the 95% confidence intervals and p values were robust standard errors to accommodate overdispersion. Since the total number of robberies and burglaries is much smaller than other crime categories, the heterogenous effects analysis is underpowered for these two crime types. We excluded them in this effect heterogeneity analysis

strike began on November 3, 2009 and ended on November 8, 2009, and our sample in 2009 includes crime from October 4, 2009 to December 8, 2009 within 100-m radius of transit stations. We treat this sample as our treatment group and included crime that occurred from October 4, 2008 to December 8, 2008 within 100-m radius of transit stations as our control group. We constructed the same control group for the 2016 strike. Figure 5 demonstrates the time trend of daily crime count of our treatment and control group. Importantly, we visually detect that the crime trends in treatment and control areas are almost parallel before both strikes. However, while the crime trend experienced a sharp decrease during strike years right after the strike started and increased when the strike ended in both years, the crime counts in control years remained relatively stable.

To assess if the strike year and non-strike year had parallel trends before the strike period, we applied the following model to test crime happened before the strike period:

$$y_{it} \sim \text{Poisson}(\lambda_{it})$$

$$\log(\lambda_{it}) = \beta_0 + \beta_1 \text{strike\_year}_t + \beta_2 t + \beta_3 \text{strike\_year}_t \times t + \eta_i, \qquad (3)$$

where y_{it} is the observed number of crimes reported in buffer i on day t, and λ_{it} is the expected crime count in buffer i on day t. t is a simple linear time trend. The strike _ year$_t$ is a 0/1 indicator whether this day is within a strike year. η_i is a station fixed effect. The parallel trend assumption can be tested by checking the statistical significance of the estimated β_3. If this parameter is statistically different from zero, then this counters the assumption that trends in crime are parallel between the strike periods. Table 4 presents the results of β_3 for each of the dependent variables we wish. The estimated parameter for β_3 is statistically insignificant for crime in each comparison group. Therefore, we have confidence that both strike years had parallel trends in dependent variables.

Having verified the key assumption of difference-in-differences design, we used the following Poisson regression model to estimate whether the closings of the transit stations had an effect on crime:

$$y_{it} \sim \text{Poisson}(\lambda_{it})$$

$$\begin{aligned}
\log(\lambda_{it}) = {} & \beta_0 + \beta_1 \text{strike}_{\text{year}(t)} + \beta_2 \text{strike}_{\text{period}(t)} + \beta_3 \text{strike}_{\text{year}(t)} \times \text{strike}_{\text{period}(t)} \\
& + \beta_4 I(\text{year}(t) = 2008) + \beta_5 I(\text{year}(t) = 2009) \\
& + \beta_6 I(\text{year}(t) = 2015) + \beta_7 I(\text{year}(t) = 2008 \ or \ 2009)t \\
& + \beta_8 I(\text{year}(t) = 2008 \ or \ 2009)I(t \geq 30)t \\
& + \beta_9 I(\text{year}(t) = 2008 \ or \ 2009)I(t \geq 36)t \\
& + \beta_{10} I(\text{year}(t) = 2015 \ or \ 2016)t \\
& + \beta_{11} I(\text{year}(t) = 2015 \ or \ 2016)I(t \geq 30)t \\
& + \beta_{12} I(\text{year}(t) = 2015 \ or \ 2016)I(t \geq 37)t + \alpha_{\text{Weekday}(t)} + \eta_i \qquad (4)
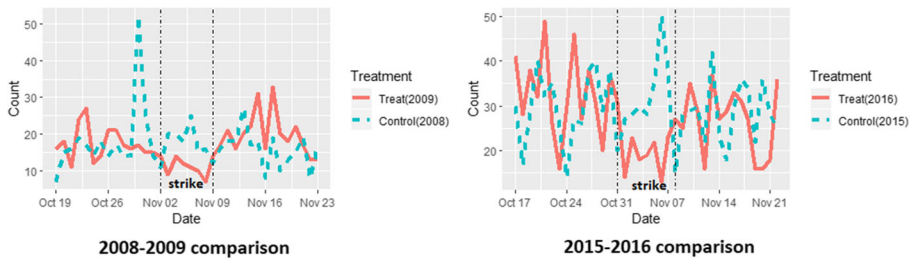\end{aligned}$$

Fig. 5 Time series of total daily crime count during strike years and non-strike years

where y_{it} is the observed number of crimes reported in 100-m buffer i on day t, and λ_{it} is the expected crime count in buffer i on day t, $\text{strike}_{\text{year}(t)}$ is a 0/1 indicator of whether this year was a strike year (2009 or 2016). $\text{strike}_{\text{period}(t)}$ is a 0/1 indicator of whether this period was a strike period. For 2008 and 2009, $\text{strike}_{\text{period}(t)} = 1$ when the date is between November 3 and November 8. For 2015 and 2016, $\text{strike}_{\text{period}(t)} = 1$ when the date is between November 1 and November 7. The model also includes a fixed effect for the year, making 2016 the reference year. β_7 through β_{12} model the secular crime trend in both years, allowing slopes of crime to differ in different time periods both in 2008–2009 and 2015–2016 comparison groups. The indicators for $t \geq 30$, $t \geq 36$, and $t \geq 37$ capture the periods after the strikes begin and after they end. α controls for day of week effects and η_i is a station fixed effect. Our quantity of interest is $\exp(\beta_3)$ which measures how many times larger (or smaller) the crime rate was during the strike period in the strike year relative to that during the same period in the year before the strike year.

Table 5 shows the results of our first difference-in-differences analysis. We found that total crime decreased by 47.2% during strike periods in strike years relative to that in non-strike years. This estimate is larger than our original estimate. Similar to our main results, we found this decrease in crime was mostly driven by the drop of violent crimes, and there is no evidence that property crime declined during the strike. We repeated the analysis by replacing our original sample with samples including crimes that happened 15 days before and after strikes and 60 days before and after strikes, respectively. This exercise yields estimates that are remarkably similar to those

Table 4 Evaluation of the parallel trend assumption for each outcome

Year	2008–2009			2015–2016		
	Estimate	Standard error	p value	Estimate	Standard error	p value
Total crime	− 0.013	0.008	0.12	− 0.008	0.006	0.20
Assault	− 0.006	0.017	0.73	− 0.006	0.017	0.71
Robbery	− 0.022	0.023	0.33	− 0.049	0.025	0.06
Burglary	− 0.046	0.029	0.12	− 0.057	0.045	0.20
Theft/larceny	− 0.013	0.014	0.36	0.003	0.013	0.83
Mischief	− 0.019	0.013	0.15	− 0.005	0.009	0.58

Each point estimate is obtained from a separate regression using model (3). For each crime type and for each strike year, we show the estimate of β_3, its robust standard errors to accommodate overdispersion, and p values

Table 5 Effect of strike periods on crime in strike years relative to non-strike years

Sample period	30 days		15 days		60 days	
	% change in crime	95% CI	% change in crime	95% CI	% change in crime	95% CI
Total crime	− 47.2*	(− 58.9, − 32.5)	− 46.3*	(− 58.7, − 30.4)	− 44.9*	(− 57.0, − 29.7)
Assault	− 62.3*	(− 82.4, − 24.1)	− 60.5*	(− 83.7, − 27.2)	− 62.7*	(− 82.5, − 25.3)
Robbery	− 71.6*	(− 90.1, − 3.0)	− 62.9*	(− 87.4, − 5.2)	− 72.6*	(− 90.3, 3.4)
Burglary	− 6.9	(− 83.9, 38.1)	41.3	(− 75.6, 645.6)	4.3	(− 81.8, 432)
Theft/larceny	− 31.3	(− 55.7, 5.7)	− 32.4	(− 57.3, 6.1)	− 28.3	(− 53.2, 8.9)
Mischief	− 31.8	(− 52.8, 2.4)	− 34.8	(− 55.5, 5.0)	− 27.5	(− 49.9, 4.0)

Sample period refers to the time before and after a strike included in our sample. For each crime type and for each sample period, we show the estimated percent change in crime computed as $100\left(\exp(\hat{\beta}_1)-1\right)$, and a 95% confidence interval. Asterisk marks estimates with p values less than 0.05. Standard errors used in the 95% confidence intervals and p values were robust standard errors to accommodate overdispersion

generated from the original sample and thus provides us with confidence that the results generated from our original analysis are robust.

Our second difference-in-differences analysis uses areas within 100-m radius of a transit station as our treatment areas and uses areas more than 100 m but less than 200 m away from the station as our control areas. These "100 m–200 m" doughnuts areas are near the 100-m buffer areas but do not have a transit station. After verifying the parallel trend assumption, we used the following Poisson regression model to estimate whether the closings of the transit stations due to strikes had an effect on crime in treatment areas relative to control areas:

$$y_{it} \sim \text{Poisson}(\lambda_{it})$$

$$\begin{aligned} \log(\lambda_{it}) = {} & \beta_0 + \beta_1 \text{strike}_t + \beta_2 \text{treat}_i + \beta_3 \text{treat}_i \times \text{strike}_t + \beta_4 I(\text{year}(t) = 2016) \\ & + \beta_5 I(\text{year}(t) = 2009)t + \beta_6 I(\text{year}(t) = 2009)I(t \geq 30)t \\ & + \beta_7 I(\text{year}(t) = 2009)I(t \geq 36)t + \beta_8 I(\text{year}(t) = 2016)t \\ & + \beta_9 I(\text{year}(t) = 2016)I(t \geq 30)t + \beta_{10} I(\text{year}(t) = 2016)I(t \geq 37)t \\ & + \alpha_{\text{Weekday}(t)} + \eta_i \end{aligned} \tag{5}$$

where y_{it} is the number of crimes reported in buffer or doughnut i on day t, strike_t is a 0/1 indicator of whether the strike was active at time t. treat_i is a 0/1 indicator of whether this area is a treatment area (buffer area) or a control area (doughnut area). The model also includes a fixed effect for the year, secular crime trend in both years, days of week effects, and station fixed effects. $\exp(\beta_3)$ measures how many times larger (or smaller) the crime rate is with the closing of a nearby station in treatment areas due to the strike relative to control areas.

Table 6 shows the results of the second difference-in-differences analysis. We found that total crime decreased by 32.2% during strike periods in 100-m buffer areas relative to that in 100–200-m doughnut areas near a station. This estimate is smaller but still

Table 6 Effect of station closings for areas with an accessible station relative to areas without a station

Sample period	30 days		15 days		60 days	
	% change in crime	95% CI	% change in crime	95% CI	% change in crime	95% CI
Total crime	− 32.2*	(− 47.6, − 12.6)	− 30.8*	(− 47.0, − 9.8)	− 31.0*	(− 46.7, − 11,1)
Assault	− 52.3*	(− 78.1, − 6.7)	− 58.6*	(− 80.9, − 15.5)	− 54.6*	(− 81.3, − 0.7)
Robbery	− 69.2*	(− 89.5, − 21.0)	− 63.8*	(− 88.1, − 2.5)	− 68.7*	(− 89.1, − 21.2)
Burglary	− 58.4	(− 91.0, 36.9)	− 43.8	(− 88.3, 103.2)	− 57.5	(− 91.1, 38.7)
Theft/Larceny	− 13.8	(− 46.6, 38.6)	− 10.8	(− 45.9,46.8)	− 4.4	(− 40.2,52.2)
Mischief	− 7.7	(− 37.6, 36.2)	− 7.5	(− 38.4, 38.6)	− 7.6	(− 36.6, 34.6)

Sample period refers to the time before and after a strike included in our sample. For each crime type and for each sample period, we show the estimated percent change in crime computed as $100\left(\exp\left(\hat{\beta}_1\right)-1\right)$ and a 95% confidence interval. Asterisk marks estimates with p values less than 0.05. Standard errors used in the 95% confidence intervals and p values were robust standard errors to accommodate overdispersion

close to our original estimate. Finally, using other samples including 15 days before and after strikes and 60 days before and after strikes generates the similar results.

Discussion

This study examined the effect of public transit on crime in Philadelphia using the closings of subway stations due to the external shock of strikes. Our results show that the closings of subway stations are associated with a significant decrease in total crime and in violent crimes including assault and robbery. In addition, we find that the effect of station closings decreases with distance from the station, which is consistent with a causal effect. There was no significant relationship between the disruption of public transit and the change in property crime and mischief. Finally, our robustness check provides us with confidence that our estimates are not an artifact of the research design. Notably, the smaller estimates generated from the difference-in-differences analysis by comparing areas with a station to nearby areas without a station suggest that crime was not displaced, at least, to nearby areas.

Our results generated from the analysis in Philadelphia contrast with previous research conducted in Los Angeles that found that station closings due to strike did not cause crime to decrease (DeAngelo et al. 2019; Ridgeway and MacDonald 2017). One potential explanation for this is that Philadelphia is much more transit-dependent than Los Angeles. Specifically, as mentioned by DeAngelo et al. (2019), the rail system in Los Angeles served approximately 100,000 daily riders, but in Philadelphia with one-third of the population of Los Angeles, this number is more than 320,000 (SEPTA 2014; SEPTA 2016a, b, c, d). Not only does this transit dependence shape normal Philadelphia citizens, but it influences potential criminal offenders as well. Several criminological theories may offer the framework to discuss these influences. First, routine activity theory suggests that public transit may create criminal opportunities during its operation. It is possible that subway stations can provide cover for potential offenders because they can pretend to be normal riders. Also, the physical features of

subway stations can provide easy exit and entry for criminals, further lowering the potential cost of crime (Cohen and Felson 1979; Block and Block 2000; Loukaitou-Sideris et al. 2001). Second, according to the broken windows theory, public transit may increase crime by gathering transient people in small geographical units, and this may generate extra litter and other kinds of social disorder. Consequently, the signal that an area is uncared for could cause more serious crime to flourish (Kelling and Wilson 1982; Ridgeway and MacDonald 2017).

The heterogeneity analysis indicates that subway stations of lower ridership on normal days had a larger decrease in crime after the strikes made all stations empty. However, stations of high ridership on normal days experienced a relative increase in crime. This finding counters the proposition that public transit generates crime by bringing more potential victims. In contrast, it appears to support the theory that passengers may serve as capable guardians, deterring certain offenders from committing crimes in public. Moreover, our main results also found that violent crimes such as assaults and robbery experienced the most significant drop during strikes. Both crimes are less likely to occur when a large group of guardians are present.

Therefore, we tend to believe that in Philadelphia, public transit increases crime not by transporting crowded victims. Instead, certain physical features, as well as potential social disorders in subway stations, could be main factors to blame. The unique environment in transit stations make them attractive to potential offenders who are dependent on public transit to commit crime as long as the capable guardianship is missing. When strikes occurred, these potential offenders can no longer enter these stations and therefore crime will be decreased.

Our findings suggest that greater security or police place-based interventions around subway stations may be needed to minimize the potential negative effects of public transit on crime, especially during operational hours. Considering that our research shows an obvious decreasing effect when moving to a larger radius away from the subway station, security measures and police interventions may not need to be too extensive. Place-based interventions focusing on crime and disorder may potentially reduce crime as long as it is confined to a small area within a 400-m radius. Also, it is worthwhile to mention that our findings cannot be used as evidence to support the argument that the public should resist public transit expansion in the metropolitan areas. In the long run, transit expansion may reduce crime if it changes the built environment of neighborhoods through increasing the job accessibility of neighborhood residents, changing the land use of nearby neighborhoods, and leading to larger investments in policing and security equipment. Since our study only focuses on the temporary disruption of public transit on crime, these long-term effects cannot be assessed in our analysis.

Our research has some limitations. First, because our analysis relies on reported incident-level crime data, it is possible that the decrease in violent crime partially resulted from less intense policing and citizen reporting of offenses around subway stations when they were closed during strikes. However, compared with serious crimes such as robbery and assault, mischief such as gambling, vagrancy, and littering is usually considered to be more influenced by intense policing and reporting. But, we do not find that strikes significantly cause these lesser crimes to drop. Second, the relationship between public transit and crime may vary depending on neighborhood characteristics such as land use characteristics, property density, and demographic structure. Our analysis aims to examine the average effect of transit on crime in all subway stations.

It is possible that the decline in crime occurred only in a portion of subway stations located in neighborhoods with certain socio-economic features. Future research should examine these heterogeneous effects and aim to answer the question of under what conditions public transit may increase or decrease crime at the street segment level.

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