Does Public Transit Spread Crime? Evidence from Temporary Rail Station Closures[1]

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Abstract

We test whether public transit access affects crime using a novel identification strategy based on temporary, maintenance-related closures of stations in the Washington, DC rail transit system. The closures generate plausibly exogenous variation in transit access across space and time, allowing us to test the popular notion that crime can be facilitated by public transit. Closing one station reduces crime by 5% in the vicinity of stations on the same train line. Most of this effect remains after controlling for decreased ridership, indicating that a decrease in the availability of victims does not drive most of our results. We find suggestive evidence that crime falls more at stations that tend to import crime, i.e. stations where perpetrators are less likely to live. We also see larger decreases at stations on the same line when the transit authority closes stations that tend to export crime. These heterogeneous effects suggest that the response of perpetrators to increased transportation costs contributes to the decrease in crime.

Keywords: public transit, crime, transportation costs

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1. Introduction

Crime rates tend to be higher in urban areas. In the United States, overall rates of violent crime and property crime per person are twice as high in Metropolitan Statistical Areas as in rural areas. In MSAs theft is 135 percent, murder is 53 percent, and robbery is 800 percent more frequent than in rural areas (U.S. Census, 2012). Several theories compete to explain this fact. One leading theory claims that densely populated areas encourage crime by lowering transportation costs to committing crime. Density either groups together potential victims in crowds or brings potential victims and potential perpetrators into close proximity (Glaeser and Sacerdote, 1999). Public transit may play an important role in both the level and spatial distribution of urban crime by allowing perpetrators to travel to affluent neighborhoods to commit property crimes. Popular opinion and public safety professionals tend to espouse this theory. For instance, District of Columbia Metropolitan Police Chief Cathy Lanier states:

> I can tell you the mobility factor is huge in terms of who your victims are and where they come from. And who your suspects [are], where they come from. And with mass transportation, if you look just at the way the metro lines run around the city, and I can tell you when the metro is down on the weekends for track work and certain lines are down I promise you my robberies go down. Every time they say track work, I'm good. (Lanier, 2013)

However, little rigorous evidence supports the idea that public transit spreads crime. Crime rates closely reflect public transit routes in many cities (e.g. Block and Block, 2000), but rigorous studies examining the effect of public transit on crime generally do not support the idea that public transit either increases crime or transfers crime from poor to rich areas (Billings, Leland, and Swindell, 2011; Jackson and Owens, 2011; Ihlanfeldt, 2003).

We investigate the effect of public transit on crime using a natural experiment in the Washington, DC metropolitan area. Over the past several years, the Washington Metropolitan

Area Transit Authority (WMATA) has engaged in extensive renovation. As a result of the construction and maintenance work, various train stations in the WMATA system have been closed for a series of consecutive days for reasons unrelated to crime in the surrounding neighborhood. This provides a natural opportunity to exploit variation in train service across time and stations to measure the effect of transit service on crime in the vicinity of the train station. While the station closures are selectively targeted for weekends and holidays, we demonstrate that conditional on time controls (day of the week-hour fixed effects, month-year fixed effects, and a holiday dummy) and station fixed effects, these closures generate believably exogenous variation in public transit access. For instance, lagged crime rates do not correlate with closures, conditional on our controls. Thus, we use these closures to identify the effect of train service on crime.

We find that closing one station reduces the aggregate level of crime across the rail system within ¼ mile of stations. This effect is concentrated entirely at other stations on the same line as the closed station, where crime falls by 5%. We do not detect a significant change in crime at stations on other lines. We are not able to measure the pure effect of removing public transit on the closed station itself because the closed station potentially experiences multiple confounding effects beside changes in travel time.[3] If we make the conservative assumption that the pure travel time effect on the closed station is zero, then aggregate crime across the entire rail system falls by .14 crimes per hour, or about 2% of the mean crime rate. Dynamic spillovers of crime to nearby time periods compensates for part of this drop, but crime still falls by about 1% even taking into account such dynamic effects.

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[3] For instance, the closures replace trains with buses that bridge passengers through the closed section of rail. The requirement to shift modes increases travel time for all stations on the same line, including the closed station, but crime at the closed station itself may also change as crowds of potential victims move from closed train stations to open bus stations.

Stations closures change not only the level but also the spatial distribution of crime. We find this on three dimensions. First, the observed effects follow the network structure of the rail system. As noted above, crime only falls at stations on the same line. Also, we find evidence that being disconnected from a larger number of stations on the line leads to larger drops in crime. Second, public transit appears to redistribute crime from neighborhoods with many potential perpetrators to neighborhoods with few potential perpetrators. Crime falls more when WMATA closes a station where more offenders tend to live, presumably "trapping" potential perpetrators who are then unable to commit a crime elsewhere. We also find suggestive evidence that crime falls more at stations where offenders do not tend to live, i.e. neighborhoods that tend to be targets. Third, public transit affects the pattern of crime within neighborhoods. Closures reduce crime most significantly within ¼ mile of the station, though stations closures may also decrease crime up to 1 mile from the station. Station closures thus represent a real reduction of crime in the neighborhood rather than a simple local redistribution. This result suggests that the concentrations of crime typically observed at public transit stations are in fact "new" crime in the neighborhood rather than a simply concentration of existing crime.

In principle, the effects we observe could result from changes in victim, perpetrator, or police behavior. The available evidence makes a victim behavior explanation unlikely. We use data on rail ridership to control for changes in the availability of victims caused by station closures. This reduces the observed effect only slightly, suggesting that the main mechanism in not a change in the availability of victims. Additionally, thefts from automobiles account for roughly half of the observed decrease in crime. Since this type of crime does not require a present victim and because park and ride trips are uncommon for the stations we study, a large change in theft from cars points to changes in perpetrator rather than victim behavior. We cannot completely rule out changes in police behavior due to a lack of data, though both this explanation and a victim behavior mechanism are less consistent the observed effects of closures on the spatial distribution of crime. It appears most likely that public transit facilitates crime by decreasing transportation costs for perpetrators.

The implications of these findings apply both to public transit and beyond transit itself. Little existing evidence demonstrates whether criminals travel to commit crimes or just commit crimes in their local areas. This paper shows that a temporary increase in transportation costs changes the amount and spatial distribution of crimes in a pattern that is consistent with criminals travelling to commit crimes. Our results have the most direct implications for police response to a transit policy change such as a temporary increase in service hours. Police should deploy more resources during hours of expanded operation or near newly opening train stations. Permanent changes in transit access, such as the construction of additional stations, may change the spatial distribution of crime. Policymakers should account for such effects on crime while also considering potential feedback of local economic benefits on crime rates. More broadly, we demonstrate that perpetrators travel to commit crimes but that such travel is costly. These facts have implications for public safety and crime control efforts. Local crime prevention efforts in low-income areas of a city may have city-wide effects, since perpetrators do not just commit crimes in their local area. These spillover effects may be positive if police efforts reduce the total number of criminals or negative if the intervention simply displaces crime. Policy changes which would appear to affect crime only in one location can affect the level and distribution of crime throughout a city.

2. Theoretical Background and Empirical Literature

Why are crime rates higher in urban areas? Various theories explain this fact using implications of rational criminal behavior (Becker, 1968). One explanation is that criminals face high transportation costs and the close quarters and transportation infrastructure of cities allow criminals to travel easily to locations where the return to crime is high. Another possibility is that dense crowds increase the return to crime by increasing the rate at which a criminal comes in contact with an attractive target. Glaeser and Sacerdote (1999) summarize these theories succintly, "A natural explanation for why cities have a high return from crime is that costs of transport for crime are extremely high…Urban density should lower transport costs, increase the returns per crime, and increase the overall crime level." Various literatures indirectly support the theory that transportation costs matter for crime by showing that crime is fundamentally local: house prices respond to local crime rates and crime risk (Gibbons, 2004; Linden and Rockoff, 2008; Pope, 2008; Pope and Pope, 2012; Congdon-Hohman, 2013); high local crime rates reduce the number of retail businesses nearby (Rosenthal and Ross, 2010); residents leave (Cullen and Levitt, 1999; Foote, 2013) and return (Glaeser and Gottlieb, 2006) to cities in response to crime rates; and police presence leads to localized crime reduction (Draca, Machin, and Witt, 2011; Klick and Tabarrok, 2005; Di Tella and Schargrodsky, 2004). The results of these different academic literatures on crime all support the conclusion that crime is local. As such, these empirical results indirectly support a theory of crime in which transportation costs affect rational criminal behavior.

If transportation costs matter for crime, then access to public transit should affect both crime rates and the distribution of crime over geographic areas. Public transit stations attract dense crowds of people, which should raise the return to crime. Public transit also reduces transportation costs to people wishing to travel to distant locations to commit crimes. Thus, the reduction in access to public transit that we study should affect observed crime. The effect of scattering crowds of potential victims is clear. Crime should fall near public transit stations that are closed since reduced access to public transit leaves fewer potential victims available.

On the other hand, increasing transportation costs of potential criminals has more complicated effects. As shown by Ihlanfeldt (2003), extending the Becker model of rational criminal behavior to public transit and a spatial environment quickly generates more nuanced predictions. We demonstrate these results formally in an appendix, but we summarize the results here.

Reducing public transit access (e.g. by closing a public transit station) at a location has two countervailing effects on crime at that location: decreasing access for outsiders who wish to commit crimes near the station but also "trapping" locals who wish to commit crimes elsewhere, who now may commit local crimes. Thus, the effect of station closures on crime should depend on whether a station tends to import or export crime.

Reducing access to public transit at one station also increases transportation costs for all stations on the same train line. Given the above discussion of heterogeneous effects, a station closure could affect crime throughout a train line in two different ways. A station closure could simply redistribute crime across stations, moving it from locations far from perpetrators' residences to locations near where they live. In this case, transportation costs affect only where a crime is committed, but the overall crime rate is unaffected. On the other hand, station closures could reduce the overall level of crime across the entire transit line if the higher opportunity cost of committing a crime pushes potential perpetrators into non-criminal activity (e.g. market work or leisure). Thus, the theory generates two testable hypotheses. Does reducing access to public transit lower the average crime rate across stations on the same transit line? Does the reduction in crime depend on whether a station tends to import or export crime? Our analysis will focus on answering these questions.

Despite the predictions of existing theory, studies that directly test the effect of public transit on crime provide little support for the idea. Geographic patterns of crime often reflect public transit routes (Block and Block, 2000), but this correlation may reflect factors other than the causal effect of access to public transit on crime. Studies aiming to isolate the effect of public transit on crime generally measure changes in crime occurring before and after construction of new rail stations. These studies find no increase in crime from access to transit itself (Billings, Leland, and Swindell, 2011) or a redistribution of crime from affluent to low-income areas (Ihlanfeldt, 2003), which opposes standard predictions. These studies estimate the long-run effects of public transit access; thus the estimates incorporate more than just a change in transportation costs. For instance, neighborhood demographics may change as house prices rise (Gibbons and Machin, 2005; Billings, 2011) in response to reduced transportation costs for other activities such as job search (Holzer, Quigley and Raphael, 2003; Phillips, 2014). Differing from the rest of the literature, Jackson and Owens (2011) study an extension of rail transit hours and find some effect on alcohol-related crime; however, they focus on only alcohol-related crime and find a change in the composition of (fewer DUIs; more other alcohol-related crime) but not the level of crime near transit stations. Thus, the existing empirical literature finds little support for the theory that public transit can spread crime. We now test this proposition in a new setting with a new identification strategy.

3. Data

3.1. Crime Data

The main analysis uses daily, geo-coded data on reported crimes made available by the Washington Metropolitan Police Department (DC Metropolitan Police Department, 2013c). We use data for all crimes reported in the District of Columbia from January 1, 2011 to October 7, 2013 for 8 types of crime: assault, sexual assault, robbery, arson, burglary, stolen auto, theft, and theft from auto. We drop homicides from our dataset because they do not include time of day. As with nearly all studies of crime, we focus on reported crime making it very likely that our data underestimate actual crime levels. However, this typically leads to underestimating the effect of an event on crime due to a simple reduction in the scale of the outcome variable. Even recognizing that reported crime will not differ from actual crime by a simple scaling factor, we are only concerned about the differences between measured and actual crime if the measurement error in reported crime is correlated with station shutdowns. This would most plausibly occur if shutdowns lead the police to focus on other locations, making it more difficult to report a crime. However, in this case the lack of police would also remove a deterrent to crime. The existing literature agrees that a reduced police presence increases *reported* crime (e.g. Draca, Machin, and Witt, 2011). At most, the reporting and deterrence effects of police offset one another in reported crime rates (Vollard and Hamed, 2012). Thus, our results would overestimate the effect of changes in perpetrator behavior only if the police shift resources *away* from closed stations to other stations. While this cannot be fully addressed without data on police presence, the police should be able to shift resources to all stations, reducing crime on both the same line and on other lines, which we do not observe.

Aside from changes in policing, transit closures may affect the probability of crime reporting by shifting the location of the crime. This could decrease the probability that a crime is reported if it makes crime harder to predict; however, if criminals are shifted from optimally chosen locations this would increase the probability of a crime being reported. It is possible that changes in reporting affect our results, but on balance it seems safe to assume that using reported crimes most likely leads to accurate or conservative estimates of the effects of station closures.

Each crime lists not only the date, time, and type of crime but also its geo-coded block location. We combine the crime data with geo-coded locations of transit stations to measure crime in the neighborhood of each station. For each date-hour time period and station in the sample we measure the number of crimes committed within ¼ mile of the station. For some specifications, however, we also make use of “rings” around each station of various radii. For instance, we define the half-mile ring around a station as the number of crimes occurring between ¼ and ½ mile from the station. In general, we multiply these values by 100 so that they can be interpreted as a percent of a crime. Table 1 provides summary statistics. The first column of Panel A lists the summary statistics for the full sample period. In a typical hour, 2.7 percent of a crime (0.027 crimes) occurs within ¼ mile of a station. Property crimes account for 95% of all crimes near rail stations. An average of 5.8 percent of a crime occurs every hour between ¼ and ½ mile and 21% percent of a crime between ½ and 1 mile. If we rescale these numbers to account for the larger land area in further out rings, the density of crimes per square mile falls as one travels outward from 0.138 to 0.098 to 0.089 for these concentric rings.

3.2. Ridership Data

We also use ridership data to gauge the mechanism by which station closures affect crime. For this purpose, we use administrative data obtained from the Washington Metropolitan Area Transit Authority (WMATA) via a public records request. The data list the total number of entries and exits for every hour and every station in the WMATA system during our sample period. The ridership data covers all stations in the WMATA system and every hour of 2011-

2013. However, we limit the ridership data to cover the same time period as our crime data. We also exclude stations outside the borders of the District of Columbia from our analysis. As shown in Table 1, ridership averages 682 riders per hour for our sample.

3.3. Station Closure Data

We combine the crime and ridership data with a novel dataset on maintenance-motivated station closures and delays in the WMATA rail system. We code the station and timing of the closures directly from WMATA news releases from the organization's website, www.wmata.com. Our data covers the 41 stations and 4 train lines inside the District of Columbia and includes a total of 4,897 station-hour closures. The vast majority of these have occurred since 2011 when WMATA began a large-scale maintenance program on the rail system. While this maintenance program has included several components (delays, single-tracking, etc.), we focus solely on instances in which rail access to a station is completely eliminated and replaced by shuttle buses. During the closures, passengers on the line in question must get off the train at the previous station, board a shuttle bus, and ride that bus to the closed station or the first open station beyond the closure. Because of this setup, station closures generate significant delays of a half hour or more for any trip passing through the station. In relation to the model in the appendix, a closure at any one station maps to an increased travel time for individuals at any other station on the same line. Importantly, these closures are motivated solely by maintenance needs, a factor which should be unrelated to crime levels. WMATA announces the closures we use for identification several weeks ahead of time and in the immediate run-up to closure advertises via large signs and public address announcements. The fact that the closures can be anticipated will be important when we discuss potential dynamic effects.

Measuring closures presents a pair of issues that need to defined carefully. First, some stations have multiple lines, which are not always closed at the same time. Define T_{itl} as an indicator of whether maintenance shuts down line l of station i for any part of hour t. There are four rail lines in our data (Red, Orange, Blue, and Green).[4] We measure the extent to which an individual station was closed as the fraction of lines through the station that are closed:

$$T_{it} = \frac{1}{L_i} \sum_{l} T_{itl}$$

where L_i is the number of lines travelling through station i. Thus, T_{it} equals 1 if all lines are closed in station i for at least part of hour t.

Second, as discussed in the theory section station, closures naturally affect any station at which there are delays as a result of the closure. Thus, we need multiple treatment variables, representing not only the closure of a particular station but also closures of connected stations. We can measure the extent of closures throughout the whole rail system:

$$\bar{T}_{(i)t} = \frac{1}{I-1} \sum_{j \neq i} T_{jt}$$

where I is the number of stations in the system. $\bar{T}_{(i)t}$ measures the fraction of the entire system, other than station i, that is closed during hour t. Since spillovers of crime are most likely to affect directly connected stations, we measure this separately for stations on the same line(s) as station i and stations not on the same line(s).

3.4. Complementary Data Sources

We use a small number of complementary data sources. As the theory above indicates, station closures may have heterogeneous effects depending on the tendency of a particular

[4] During the sample period, the Silver line was under construction. There was also a Yellow line in operation, though within the District of Columbia, it follows the same path as the Green line, only diverging after leaving DC for Virginia. Thus, we consider the Green and Yellow lines to be identical.

location to be a source or a destination for those committing crimes. We measure tendency of a location to be a source for perpetrators by measuring the number of juvenile arrests for which the arrestee has residence near the station as a percentage of the population near the station. Data limitations do not allow us to measure home locations for the crimes reported in our dataset; however, DC MPD data on juvenile arrests do include police service area of residence for all juvenile arrests during the same time frame as our main dataset (e.g. Metropolitan Police Department, 2013a). We use this to calculate the total number of juvenile arrests in each police service area (PSA) and divide the number of arrests by PSA population estimates calculated by NeighborhoodInfo DC (a partnership involving the Urban Institute) using Census population counts. Since parts of the quarter-mile radius circle around a given train station may be in multiple PSAs, we compute juvenile arrestees per capita near a station as a weighted average of all overlapping PSAs, weighting by the fraction of crimes near that station committed in each PSA during 2012. In summary, we calculate the juvenile arrests per hundred people for those living near each train station and use this as a measure of the tendency of a train station to be a source of those committing crimes. An average station will have 2 arrests assigned to those living near the station per 100 residents.

Finally, we control for whether the day in question is a holiday or a weekend. We code this variable as an indicator for days on which there is no S&P 500 listing.

4. Identification Strategy

4.1. Regression Framework

To identify the effect of public transit on crime, we exploit variation in transit access generated by maintenance-related closures of transit stations in Washington, DC. From 2011-2013 the Washington Metropolitan Area Transit Authority conducted extensive track maintenance that required fully shutting down rail stations and replacing the trains with buses, leading to extensive delays for travelers moving along those lines. We use these station closures as a natural experiment to examine whether crime falls at one station when a closely connected station is shut down. The following regression equation provides our main empirical framework:

$$C_{it} = \alpha + \beta T_{it} + \gamma_1 Same\bar{T}_{(i)t} + \gamma_2 Other\bar{T}_{(i)t} + \delta X_t + \eta_i + \epsilon_{it} \qquad (1)$$

C_{it} is the number of crimes within one quarter mile of station i during hour t; T_{it} is the fraction of station i's lines that are closed during hour t. The next two variables measure closures throughout the system. $Same\bar{T}_{(i)t}$ is the fraction of stations that are closed on lines passing through station i; $Other\bar{T}_{(i)t}$ is the fraction of stations that are closed on lines not passing through station i. X_t is a vector of date and hour controls; η_i is a station fixed effect; and ϵ_{it} is an error term. Our preferred specification includes fixed effects for the day of the week interacted with the hour of the day, fixed effects for month interacted with year, and a dummy for holidays and weekends.

Our coefficient of interest is γ_1, which measures the effect on crime rates on the entire line when a station is closed. This coefficient could be zero if closures simply redistribute crime from stations far from perpetrator residences to nearby stations. On the other hand, closures could reduce crime throughout the train line ($\gamma_1 < 0$) if closures simply reduce the number of available potential victims near train stations or if the relevant alternative to committing a crime far away is a non-criminal activity (e.g. work or leisure). The coefficient γ_2 measures a similar response to station closures on other lines. If closures have a smaller effect on travel times for stations on other lines, then γ_2 would be closer to zero. If crime were displaced from closure lines to non-closure lines γ_2 would be positive. The coefficient β identifies the effect of closing a transit station on crime at the same station. While seemingly straightforward, this effect is difficult to interpret. In addition to reducing perpetrator mobility and the number of potential available victims, a station closure also simply moves a crowd of people from an enclosed rail station to an open bus stop where they may be easier targets for criminals. This extra effect (among others) makes the own station effects difficult to interpret and we focus our attention instead on how closures affect other stations where this confounding effect is not present.

Even an unbiased average treatment effect may not fully characterize the effect of public transit on crime if public transit redistributes crime across stations. First, if transportation costs for perpetrators matter, then crime may fall more in affluent locations that tend to import crime when connected stations close down. We test for this effect by including the interaction of the closure variables in equation (1) with a proxy for the propensity of local residents to commit crimes. We use the number of juvenile residents arrested per capita for this purpose. We would expect that crime would fall most in locations where few arrestees live. This represents heterogeneity by *crime location*. If many potential criminals reside in a particular location, then a transit closure elsewhere may lead to no drop in crime because perpetrators are local. On the other hand, a location with few potential criminal residents will experience a larger drop in crime because perpetrators tend to commute into the neighborhood. Second, the effect of closures may be heterogeneous with respect to the *closure location.* We would expect that if a location where many potential criminals reside experiences a station closure, then this should decrease crime at connected stations more than if the closure happened in a location where few potential criminals reside. Thus, there are heterogeneous effects by whether perpetrators tend to live at the *closure location*. To measure heterogeneous effects by the tendency of the closure location to export crime, we modify our treatment variable to be weighted by the juvenile arrestees measure:

$$\overline{NT}_{(i)t} = \frac{1}{I-1} \sum_{j \neq i} (N_j * T_{jt})$$

where N_j is juvenile arrests per capita at station j. Heterogeneous effects by the number of residents in the closure location who are likely to commit crimes can then be tested by including this variable in equation (1).

4.2. Exogeneity

Our empirical strategy identifies the effect of public transit on crime rates by testing whether crime falls at other stations when a connected station is closed for maintenance. This involves comparing crime for station-hours affected by closures to station-hours not affected by closures. In the present context, the maintenance motivation of the station closures provides a strong *a priori* reason to trust that the timing and location of station closures by WMATA would not correlate with unobservable counterfactual crime rates. However, selection bias may still exist if the particular stations needing maintenance also happen to be low (or high) crime areas or if WMATA times maintenance for down times when ridership and crime are both low. Figure 1 displays the closure data across the course of the week, demonstrating the need to control for selective timing of station closures. The black dashed line shows the average proportion of stations closed during each hour of the week. Station closures occur almost entirely during weekends and holidays, with a typical closure occurring during late Friday evening, all day Saturday and Sunday, and sometimes a holiday Monday.[5] Panel B of Table 1 confirms that closures occur on weekends and holidays. The first column shows variable means for open stations on lines with some maintenance-related closures. The second column shows the same for open stations on lines with no maintenance closures. We omit the closed stations themselves because, as described above, these stations are subject to confounding effects other than changes

[5] The WMATA rail system does close late at night every day of the week; however, we code our closure variable only for maintenance-related closures. Thus, it takes on a value of zero during the week (when WMATA avoids closing stations for maintenance) and late at night on weekends (when stations would be closed anyway). Given that all stations open and close for the day at the same time, day-of-week X hour fixed effects partial out variation due to regular nightly closures.

in transit access. Since we focus on how closing one station affects other stations on the same line, the relevant comparison to ensure baseline balance is between two categories of open stations, those on lines with closures and those on lines without closures. The third column measures the simple difference between these two columns. Station-hours on lines with closures are 63 percentage points more likely to occur on weekends and holidays, a large difference which is statistically significant.

Clearly, WMATA targets station closures for these low-ridership times, which would introduce bias if low ridership times are also low crime times. WMATA could also plausibly target stations, times of the week, or months of the year with low ridership and low crime rates. Thus, our main specification in equation (1) includes a holiday/weekend dummy, day of the week-hour fixed effects, month-year fixed effects, and station fixed effects. Our regression strategy will assume that closures generate exogenous variation in transit access conditional on these controls. Figure 1 plots the crime data to verify that this assumption is believable. In this figure, we measure crime as the number crimes at a station in a given hour relative to the station average (i.e. removing station fixed effects). The red line shows average crime rates for stations during weeks with no station closures. The blue and green lines show crime rates during weeks when some stations are closed, with the green line showing crime rates for station-hours affected by closures and blue showing crime rates for station-hours not affected by closures. Our identification strategy measures the effect of public transit on crime by comparing crime rates during maintenance-related closures to the red line counterfactual of crime during other weeks. Importantly, from Tuesday to Thursday there should be no difference in crime rates between weeks with closures and weeks without closures because closures are not in effect. Figure 1 bears this out. Crime during weeks with closures and crime during weeks without closures follow each other closely from Tuesday to Thursday. This provides strong evidence that, conditional on trends in crime over the course of a typical week and station fixed effects, station closures are as good as randomly assigned to station-hours. A large difference appears Friday evening, which we will return to when discussing dynamic responses to station closures.[6] Overall, the visual evidence supports our conclusion that, conditional on our control variables, stations affected by closures are ex-ante similar to those unaffected.

We provide quantitative evidence of baseline balance in the second and third row of panel B in Table 1. We measure lagged crime for a particular station-hour as average crime at the same station in the previous Tuesday through Thursday (when closures do not occur). Lagged crime rates differ between stations on lines with closures (2.9 percent of a crime per hour) and those without (2.7). This difference is statistically significant; however, crime is actually *higher* on lines during closure weeks, which would bias our results toward zero. Furthermore, in column (5) we add our combination of fixed effects, and the difference disappears. Likewise, when we compare ridership in the days before closures, ridership is significantly higher on lines where closures occur. However, the positive difference points to underestimating the true effect of closures, and the difference shrinks from 101 to 26 passengers when we add our control variables.

With this evidence in hand, we can reasonably use crime rates at the same time of the week (but different weeks) as a counterfactual. Thus, we interpret the gap between the green and red lines from late Friday evening through Monday as the effect of public transit on crime rates. During station closures, crime rates are lower late Friday evening, late Saturday evening, and in the middle of the day Monday relative to crime in other weeks; they are higher during the middle

[6] In section 5.5 we demonstrate that this spike appears to be due to a dynamic response of potential victims, who alter commuting patterns in anticipation of planned maintenance. While this could in principle generate bias by contaminating the control group, we demonstrate in section 5.5 that this does not change our main results.

of the day Saturday; and no difference appears on Sunday. Of course, we can only measure each of these hourly differences noisily, so we embed this identification strategy into a regression framework, presented above, to measure the average effect of public transit on crime across all observations.[7]

Our approach differs from most of the literature in that we focus on many, short, maintenance-related changes in access to public transit. The most popular approach, as discussed above, involves testing how crime responds to construction of new transit stations (Ihlanfeldt, 2003; Billings, Leland, and Swindell, 2011). Our approach does bear similarity, though, to that of Jackson and Owens (2011), who study whether extending evening hours for the DC rail system affected alcohol-related crimes. Both their study and the present study benefit from exploiting high frequency data and changes in transit service over the course of the week motivated by factors unrelated to crime. This provides a context where variation in transit access can more reasonably be considered exogenous.

4.3. Station Closures and Access to Transit

If the station closures we exploit can be accepted as exogenous, we still must show that these closures reduce access to public transit. We can test this directly using ridership data from WMATA. In Table 2 we examine the relationship between station closures and train ridership using a regression akin to equation (1) but with ridership as the dependent variable. We measure ridership as the sum of entries and exits at a particular station in a particular hour. The first column simply examines the relationship between station closures and ridership. For pure mechanical reasons, closing a station decreases its ridership nearly to zero. In particular, ridership falls by 227 riders per hour to an average ridership during closures of 33 riders per

[7] The non-closure times on weekends (blue line) reflect times when stations would be closed anyway and thus are not defined as closed due to maintenance.

hour.[8] The second column of Table 2 includes effects on ridership at other stations in the system. The coefficient of -483 on own line closures indicates that ridership would fall by 483 riders per hour at a station if all other stations on the same line were closed. A typical closure, though, closes only 11 percent of the line. Such a closure would decrease ridership at all other stations on the line by about 50 riders per hour. We do not observe similar spillover effects onto ridership at stations on other lines. The point estimate for spillovers onto other lines is negative but statistically insignificant and small (a decrease of about 9 riders per hour for a typical closure). Including variables for spillovers onto other stations also decreases the measured effect on the closed station itself from -227 to -172. The coefficient decreases because closures tend to occur in groups of stations on the same line, and the own-station coefficient was previously partially picking up the effect of closing nearby stations. The final two columns of Table 2 simply demonstrate that closures affect entries and exits symmetrically, limiting both the ability of people to leave the station and enter the station, as would be expected.

Thus, we conclude that closing train stations reduces access to public transit, reducing ridership and raising transportation costs. Of course, higher transportation costs could affect both perpetrators and victims, but the ridership results demonstrate that station closures limit transportation options for both of these groups. Moreover, the results demonstrate that closing a train station generates not only a mechanical reduction in ridership at the station itself but also fewer riders at other stations on the same line. As discussed above, we do not focus on how closures affect crime at the closed station itself because station closures represent a package of changes that go beyond a simple shock to access to public transit. However, other stations on the same line do not face these confounding changes, and the ridership results indicate that station

[8] Average ridership is not exactly zero due to a small number of instances in which closures were announced but apparently not carried out.

closures reduce access to public transit at other stations on the same line. We also demonstrate that closures only measurably affect public transit access at stations on the same train line as the closure. Thus, the ridership results generate a testable prediction: station closures should only affect crime at stations on the same line and not at stations on other lines.

5. Results

5.1. Main Effect on Crime

We first estimate the average effect of closing a transit station on crime within ¼ mile of the station and other stations as in equation (1). Table 3 provides the results of this estimation. In the first column, we examine simply the effect of closing a transit station on crime near the closed station itself. This specification includes our main set of controls: station fixed effects, day of the week-hour fixed effects, month-year fixed effects, and a weekend-holiday dummy. The coefficient of 0.14 indicates that crime actually rises by 0.14 percent of a crime if a station is closed for the full hour. Relative to an average crime rate of 2.7 percent of a crime per hour, this represents about a 5 percent increase in crime; however, this effect is statistically insignificant. Also, as discussed above it is difficult to interpret this as an effect of altering access to transportation because closing a transit station may affect the immediate area not only by limiting transit access but also by dispersing, concentrating, or shifting crowds of people. As such we do not take a stand on the interpretation of this coefficient but include this variable in all specifications.

Station closures potentially affect crime not only at the closed station itself but also at directly connected stations. In particular, transit access at these stations will be significantly affected without other confounding changes that could exist at the closed station itself. The second column of Table 3 tests whether crime falls at stations on the same line as the closed station and/or at stations on other lines. The coefficient of -1.28 indicates that crime at a station would fall by 1.28 percent of a crime per hour, or 47 percent of the mean, if all other stations on the same line are closed. Of course, shutting down all stations on the same line extrapolates beyond our sample. A typical shutdown involves shutting down 11 percent of other stations on a line (about 2 stations), meaning that a typical shutdown reduces crime at connected stations by 5 percent of the mean crime rate. On the other hand, closures do not affect crime at stations on other lines in a statistically detectable manner. The measured coefficient is smaller and positive (0.43) but statistically insignificant. While the theory is ambiguous about whether public transit affects total crime committed near transit stations or simply redistributes crime from one station to another, these results indicate that reducing access to public transit actually reduces the overall level of crime across the train system.[9]

The third column of Table 3 tests the results for robustness to including date fixed effects. This specification uses only variation within days when at least one station closes for at least one hour. The more limited variation generates larger standard errors but still results in a large negative coefficient of -1.86. If anything, controlling for time effects at a more detailed level generates larger effects. Switching from a linear model to a Poisson model also has little effect on the qualitative results. The coefficient of -0.535 indicates that totally closing a transit line decreases the log of the count of the number of crimes by -0.535 points, which corresponds to a 71 percent ($e^{-0.535} - 1 \approx 0.71$) decrease in the crime. If anything, the our preferred model underestimates the effect of closures.

Finally, we can run a placebo test using lagged crime rates. If maintenance-related closures truly provide an exogenous shock to transit availability, then closures should not be

[9] Overall crime in the city could rise, though, even as crime near train stations falls if closures simply shift crime to locations more than ¼ mile from the station. We address this issue in section 5.6.

correlated with where and when crime occurs before the closures happen. We test this proposition by estimating our main model using a one week lag of the crime variable as our outcome. In this placebo test, the coefficient of 0.613 on the same line closures variable has a positive sign, is smaller in magnitude, and is statistically insignificant. The other closure variables are likewise statistically insignificant. The data are consistent with our claim that, conditional on the included control variables, maintenance related closures are exogenous to crime rates.

5.2. Conditioning on Ridership

As discussed above, we posit that transit closures affect crime near stations by reducing access to the neighborhood. Thus far, the data shows that ridership falls (Table 2) and crime, in turn, also falls. In this sense, the reduction in ridership represents a "first stage" for the effect of transit closures on crime. However, reduced transit access could reduce crime either by increasing the time cost to a distant perpetrator of committing a crime or by simply reducing the number of available victims crowded around the station. In the final column of Table 3, we run our main specification controlling for contemporaneous ridership in an attempt to separate out these two mechanisms. Controlling for ridership, which endogenously responds to station closures, can generate bias so the results must be interpreted with caution. However, since most riders are potential crime victims rather than perpetrators, ridership allows us to control for the availability of victims. As shown in the results, ridership correlates strongly with crime; 1,000 additional passengers are associated with an additional 58% of a crime committed near the station. Controlling for ridership reduces the coefficient on own-line closures from -1.28 to -0.99, indicating that the number of available victims explains part of the reduction in crime caused by station closures. However, the effect of own-line closures remains large, negative, and statistically significant even after controlling for ridership. Reduced ridership only explains 23% of the observed drop in crime. A typical closure with no drop in the number of available victims would lead to a 4% drop in crime (rather than 5% with ridership falling). Our results cannot simply be simply explained by a change in the availability of victims.

5.3. Heterogeneous Effects

If perpetrator behavior drives the results we observe, then closing stations should have heterogeneous effects. First, closing nearby stations should not reduce crime at stations that tend to be the home station for those committing crimes. If anything, cutting off potential targets should increase crime at these stations. On the other hand, reductions in crime should be concentrated at stations that are targets, i.e. stations that are a sink rather than a source for those committing crimes. Table 4 tests this theory. The first column simply reproduces our preferred specification for the main effects. The second column then tests for how treatment effects of closing a station interact with the tendency of a station to export crime. We observe the likely criminal population residing at a station using data on juveniles residing nearby who were arrested. We measure, roughly speaking, the percentage of population residing nearby that was arrested for a juvenile crime.[10] As can be seen, the coefficient on closures at other stations on the same line remains negative and increases in magnitude to -1.46. This coefficient has a straightforward interpretation as well; it indicates that crime would fall by 1.46 percent of a crime per hour at a station where none of the population living there was arrested for a juvenile crime. This change matches the expectation from theory that station closures will have a greater effect on crime in neighborhoods where those committing crimes are less likely to reside. This

[10] Specifically, we count the number of juvenile arrests in which the arrestee lived near the station, divide it by the nearby residential population, and multiply by 100. The percentage interpretation helps with intuition, though of course repeat offenders make this interpretation only approximately true. Results are qualitatively similar if we use measures of the fraction of the population on parole or historical crime rates for the interactions.

changing coefficient occurs because the interaction of own line closure and the parolees measure has a positive coefficient, 0.116. Interpreting the point estimate literally would indicate that a station with 6% of the nearby population arrested for a juvenile crime (2 s.d. above the mean) would have only half the drop in crime experienced by a station with no juvenile offenders (-1.46 + 6 * 0.116 = -0.76 versus -1.46). This interaction coefficient is not statistically significant, so these results should be interpreted cautiously. At least, the data are consistent with the prediction that closures should have less effect in neighborhoods where perpetrators are likely to live and more effect where they are not likely to live.

The theory also predicts heterogeneous effects of station closures according to the station that is closed. If a station closes in a neighborhood where perpetrators tend to live, then this should decrease crime to a greater extent elsewhere than shutting down a station where perpetrators are less likely to live. We test this in column three of Table 4 by replacing the "fraction closed" treatment variables with treatment variables where closures are weighted by juvenile arrestees per capita near the closure station. The coefficient of -0.698 on the "own lines" version of this variable indicates that crime would fall by about 4 percent of a crime per hour if the entire line was closed *and* each closed station had 6 percent of its residents arrested. This effect is much larger in magnitude than the effect averaged over closures at all types of stations. The results suggests that closures decrease crime more when the closed station is in a neighborhood where perpetrators are more likely to live. Altogether, the results on heterogeneous effects from the first panel of Table 4 suggest that public transit affects not only affects the overall level of crime but also its spatial distribution, as predicted by theory.

Stations closures should also affect crime more significantly when the closure cuts off the station from many other stations on the same line. For instance, if a line has 10 stations and the

9th station is closed, this should more dramatically affect station 10 than station 8. Station closures propagate through the rail network, and the location of closures in the structure of that network should matter for the observed effects on crime. Our main measure of the effect of closures, the fraction of stations closed on a line, does not consider where on a line the closure occurs and thus does not include such heterogeneity. To capture such network effects, we construct a new variable which measures the fraction of stations on the same line which are disconnected from that station by a closure. Recall the example of a line with 10 stations which are numbered 1 through 10 in order of their arrangement on the line. If the 9th station is closed, station 10 loses access to 90% of stations on the line (all but itself), station 9 loses access to 90% of stations on the line, and stations 1 through 8 lose access to only 20% of stations on the line. We construct this variable for all stations-hours in our sample.

The fourth column of Table 4 displays results replacing our standard treatment variable, the fraction of stations on the line that are actually closed, with this measure of the fraction of stations on the line which are disconnected. As is evident, we still find a strong negative effect of closures on crime at other stations on the same line. The coefficient of -0.586 indicates that disconnecting one station from all other stations on the line causes crime to fall by about 0.6 percent of a crime per hour, i.e. 22% of average crime rates. The coefficient on the new disconnection variable is not directly comparable to our previous coefficients using the fraction closed variable. However, we can compare them by measuring the drop in crime each implies would occur during a typical closure. In our data, the average closure disconnects a station from 39% of stations on the same line. Thus, a typical closure would lead to a drop in crime equal to about 8% of mean crime rates. Recall that when using the fraction of closed stations, our results indicated that a typical closure leads to a 5% drop in crime. Thus, using a measure that takes into account how closures affect stations differently depending on their location along the line, we measure a larger drop in crime. The stronger relationship suggests that, as expected, closures have larger effects at stations that become isolated by the closure.

5.4. Types of Crime

The data allow us to measure the effect of closing stations on crime rates separately for different types of crime. We can use this information to further examine the mechanism driving the observed drop in crime. If victim behavior drives the effects we observe, then station closure effects should be most pronounced for crimes requiring a present victim (e.g. assault and robbery). On the other hand, if we observe a drop in crimes with no present victim (e.g. theft from cars, burglary) then we would most reasonably conclude that the relevant mechanism is how transit closures affect perpetrator behavior.

Our data include 8 different categories of crime: other theft (50% of all crimes), theft from auto (24%), robbery (11%), burglary (5%), motor vehicle theft (5%), assault with a deadly weapon (4%), arson (< 1%), and sex crimes (< 1%). Table 5 presents the results of our main specification split out by category of crime. As is apparent, "theft from auto" drives most of our results. Station closures lead to 0.0128 fewer crimes per hour, and 0.0065 of these (51%) are thefts from vehicles. No other category registers a statistically significant drop in crime (though some of the point estimates are large). This evidence provides a strong indication that perpetrator behavior rather than victim behavior drives our results. Reducing public transit access could foreseeably reduce the number of potential victims of robbery, assault, or other types of theft; however, it seems less likely that a temporary change in transit access would appreciably change opportunities for breaking into automobiles.

Though less plausible than a potential direct effect on the number of robbery victims, it is possible that shutting down a train station could reduce the number of cars available to break into if cars near train stations belong to people who park at a WMATA parking lot and then ride the train to work. However, this is unlikely in our sample. The stations we observe are urban stations inside the District of Columbia where few passengers park. Only 15% of riders at the stations in our sample "park-and-ride" (WMATA, 2008). According to the WMATA website, 31 of the 42 stations in our sample have no WMATA parking and 4 others do not have all-day parking. At the stations with no parking, only 10% of riders park-and-ride, likely because the only parking near most stations in our sample is city street parking which residents of other neighborhoods can use for at most 2 hours. At stations with no WMATA parking, changes in ridership should not affect the availability of automobiles to break into. If we exclude the 11 stations with WMATA parking and focus on these stations where parking and riding is rare, neither our main results nor the results for theft from automobiles qualitatively change.

5.5. Temporal Spillovers

We observe significant but temporary variation in access to public transit. If potential criminals or potential victims pay attention to public announcements about future station closures, then we might expect changes in crime rates outside the time of the closure itself. Perpetrators could re-allocate criminal activity to times before or after closures when the opportunity cost of committing a crime is lower, or victims could alter commuting patterns to avoid long delays. We saw evidence of such dynamic effects in Figure 1, which shows that crime rates exhibit a large spike on Friday afternoon and evening just prior to station closings.

To analyze such dynamic effects, we make use of standard event study methods. First, we group event-time by day, so the time component indicates how many days it has been since a closure occurred. We do this using a maximum operator, $\max_{j=1,..,24}\{Same\bar{T}_{(i),t+24+j}\}$, as a righthand side variable that, in this example, records whether other stations on the same line will be closed at any time between 25 and 48 hours in the future. Following our previous framework, we create these lagged and lead closure variables, grouped by day, for the closed station, closures of other stations on the same line, and closures on other lines. We interact all of these closure variables with a dummy for whether the entire system is open at time t: $Open_t = (1 - T_{it}) * (1 - Same\bar{T}_{(i)t}) * (1 - Other\bar{T}_{(i)t})$. To understand this interaction, consider a typical closure beginning on Friday evening. The coefficient on $Open_t * \max_{j=1,..,24}\{Same\bar{T}_{(i),t+24+j}\}$ will measure the change in crime for hours when the entire transit system is open at stations that will have closures at other stations on their lines in between 25 and 48 hours in the future. For a typical weekend closure, this essentially measures the average change in crime on Wednesdays before closures. The $Open_t$ interaction is important because station closures tend to run for many consecutive hours over a weekend. A regression with uninteracted lags would split a drop in crime observed on Sunday into a contemporaneous effect, the effect of a one day lag of Saturday closures, and the effect of a two day lag of Friday closures. The interacted lagged closure variables cleanly separate times with contemporaneous closures from times with no contemporaneous closures. Altogether, we run the following regression.

$$C_{it} = \alpha + \beta T_{it} + \gamma_1 Same\bar{T}_{(i)t} + \gamma_2 Other\bar{T}_{(i)t} + \sum_{k=-4}^{4} \beta_k Open_t * \max_{j=1,..,24}\{T_{i,t+24*k+j}\}$$

$$+ \sum_{k=-4}^{4} \gamma_{1k} Open_t * \max_{j=1,..,24}\{Same\bar{T}_{(i),t+24*k+j}\}$$

$$+ \sum_{k=-4}^{4} \gamma_{2k} Open_t * \max_{j=1,..,24}\{Other\bar{T}_{(i),t+24*k+j}\} + \delta X_t + \psi Z_{it} + \eta_i + \epsilon_{it} \qquad (2)$$

Most terms are defined in the preceding paragraph or our main regression specification above. The only new element is Z_{it} which represents six variables, two for each closure variable, indicating station-hours that more than 5 days before or 5 days after a closure. Altogether this specification sets up an event study with hours between 4 and 5 days before the closure as the omitted category, measuring all effects relative to this range.

Figure 2 displays the γ_{1k}'s from this specification. The blue line shows results for how closures affect crime at other stations on the same line. As above, the contemporaneous effect of station closures on crime at connected stations is negative, large, and statistically different from zero. Changing the control group via this event study actually increases the magnitude of the measured effect to -2.6. The dynamic effects are not different from zero at any point. We measure a positive point estimate of 1.0 the day before closures, as we might expect from Figure 1. However, this effect is not significantly different from zero. The closest we come to measuring a dynamic effect in this specification is actually 1 day *after* the closure, where closures continue to have a negative (though diminishing) effect.

Grouping observations into days adds statistical precision but it may hide dynamic effects of station closures if, as suggested by Figure 1, crime spikes only for a few hours prior to closures. We test this in a more direct, though somewhat ad-hoc, manner by simply testing for treatment effects 5 hours before and 5 hours after closures. We can test this in a regression framework as follows:

$$C_{it} = \alpha + \beta_0 T_{it} + \beta_1 Open_t * LagT_{it} + \beta_2 Open_t * LeadT_{it}$$

$$+\gamma_{10} Same\bar{T}_{(i)t} + \gamma_{11} Open_t * LagSame\bar{T}_{(i)t} + \gamma_{12} Open_t * LeadSame\bar{T}_{(i)t}$$

$$+\gamma_{20} Other\bar{T}_{(i)t} + \gamma_{21} Open_t * LagOther\bar{T}_{(i)t} + \gamma_{22} Open_t * LeadOther\bar{T}_{(i)t}$$

$$+\delta X_t + \eta_i + \epsilon_{it} \qquad (3)$$

We continue to use a maximum operator to group sets of hours for precision purposes. Here we include five hours before and after closures, i.e. $LeadT_{it} = \max(T_{it+1}, T_{it+2}, T_{it+3}, T_{it+4}, T_{it+5})$.

Table 6 displays the results. The first column displays our main specification for comparison. The second column includes leads and lags of the station closure variables, essentially averaged over 5 hours before and after closures. The first thing to notice is that though including these dynamic effects changes the control group, this does not significantly change the main results. Crime still drops during closures. Moving to dynamic effects, the coefficient of 3.40 on the lead of closures on a station's own line indicates that crime increases by 3.40 percent of a crime per hour in the five hours leading up to a station closure. Thus, we observe a large and statistically significant increase in crime prior to station closures. This quantifies the spike in crime during Friday evenings in Figure 1. However, the dynamic effects measured in column 2 of table 6 are statistically noisy. The magnitude of the leading effect of own station closures is very large but also has a very large confidence interval. This latter result owes itself to the fact that while a station closure covers many hours of a weekend, there are only a handful of hours when the system is fully operational but just about to experience station closures.

Potential dynamic effects that reallocate crime across time matter for whether station closures actually reduce the overall level of crime. Crime falls during closures but possibly increases just before closures and falls just after closures. To get a sense of whether crime falls on net, it is useful to consider a typical weekend closure. A typical closure involves 42 hours of maintenance-motivated closures and 13 hours each of "just before" and "just after" maintenance-motivated closures.[11] If we assume only a contemporaneous effect of station closures, then we

[11] By far the most common schedule involves closing early from 10 p.m. Friday to Saturday at 3 a.m. and shutting down all hours Saturday and Sunday. The station would be closed regardless of maintenance from 3 a.m. to 7 a.m.

would predict that closing a full train line would reduce crime by about half of a crime per weekend per station (42*-1.28/100). On the other hand, for the most conservative measurement of the total reduction in crime, one could assume that crime rises before closures, falls during closures, and is not changed after closures. In this instance, we would predict a drop in crime of only 0.08 crimes per station per weekend ([-1.24*42 + 3.40*13 – 0*13]/100). In a perhaps more reasonable scenario, we can allow for both lagging and leading effects according to the point estimates from column 2 of Table 6. In this case, the total reduction in crime from shutting down a full train line would be about 0.3 crimes per station per weekend ([-1.24*42 + 3.40*13 – 1.74*13]/100). Of course, if we take the estimates of Figure 2 literally, the total crime reduction would actually be greater than assuming no dynamic effects.

As with the contemporaneous effects, we can test whether the observed dynamic effects appear to be caused by victim or perpetrator behavior. To this end, we extend our dynamic analysis to ridership in the final two columns of Table 6. The first of these columns simply displays our main results for ridership. The final column replicates our dynamic analysis but with ridership as the outcome. As shown before, ridership at the station falls contemporaneously when stations on the same line are closed. Of greater interest, closing down the whole line increases ridership by 631 people just before station closures. This increase prior to closures matches the dynamic effects of closures on crime. This spike in ridership prior to closures suggests that, while perpetrator behavior most likely drives most of the contemporaneous effects observed earlier, changes in the availability of victims may explain a significant part of the dynamic effects that we observe.

Saturday and Sunday. In our accounting, the early morning hours between maintenance-related closures count in both lagging and leading effects.

In sum, uncertainty in the dynamic estimates does not allow for a precise accounting of the total drop in crime due to station closures. We prefer the estimates in column (2) of Table 6, which indicate that dynamic effects reduce the measured net drop in crime by 40% (from 0.5 crimes averted per station to 0.3). However, we can establish certain facts in nearly all specifications: crime is reallocated across time in response to temporary closures; this reallocation does not likely fully offset the fall in crime during closures; and the reaction of potential victims to station closures likely explain at least some of the observed dynamic effects. These results relate to the literature on the displacement effects of policing measures (e.g. Jacob, et. al. 2007), demonstrating that displacement of victim behavior can increase crime before or after a policy is in effect just as much as displacement of perpetrator behavior. More importantly for our purposes, allowing for dynamic effects does not alter the main conclusion of this paper. The results continue to support the idea that closing stations disrupts criminal activity.

5.6. Spatial Spillovers

Thus far, we have focused on measuring the effect of rail station closures on crime within ¼ mile of other stations. However, the effect of transit closures could also stretch beyond the immediate vicinity of train stations. First, closures could affect crime rates in the neighborhood further from the station either positively or negatively. Reducing access to transit could reduce crime immediately next to the station by simply dispersing those perpetrators, raising crime rates in the surrounding neighborhood. On the other hand, station closures could have simply have a similar but less pronounced negative effect on crime further from stations.

Table 7 investigates this possibility by replicating our main specification for three different areas. The first column simply replicates our main result but changes the outcome to percent of a crime per square mile, showing that crime falls within ¼ mile of a station when

WMATA closes other stations on the same line for maintenance. The coefficient of -6.56 indicates that the number of crimes per square mile falls by 6.56 percent of a crime per station per hour ($6.56 \approx 1.28/\frac{\pi}{16}$). The second and third columns expand this specification to concentric rings around the station. The second column shows that, if anything, station closures still reduce crime between ¼ and ½ of a mile from the station. This effect, though, is less intense and statistically insignificant. The effect almost totally dissipates in the third column with no evidence of crime reduction from ½ to 1 mile from the station. Importantly, though, we do not observe increased crime in the surrounding neighborhood. Station closures do not appear to simply disperse crime into the surrounding neighborhood; instead they truly decrease the total amount of crime in the neighborhood with the greatest effect near the station.

One final spatial spillover could be that station closures re-direct crime to neighborhoods very far from transit stations. Already, we have shown this to be unlikely given that most of Washington, DC is within 1 mile of a train station and we do not observe crime spilling over to areas within 1 mile of stations. However, we do have data on crime that is not within 1 mile of train stations. These crimes cannot be assigned to any one station in a meaningful way, so we simply aggregate them at the daily level. We then check whether daily crime counts more than one mile from train stations rise on days with station closures, conditional on our time controls. The final column of Table 7 reports these results. We find a positive coefficient; however, the effect is small and statistically insignificant.[12] For comparison, consider the predictions of the results for all columns of Table 7 together in the event of a shutdown of all stations in the WMATA rail system. Crime would fall by 54 percent of a crime per hour within ¼ mile of

[12] Another interpretation of this result assumes that no effect is possible beyond one mile from stations and that any value here represents non-random selection of day-hours for station closures. Not surprisingly given the results, controlling for positive selection using crime trends far from stations would lead to larger measured effects.

stations (-6.56 per station*42 stations*$\frac{\pi}{16}$ sq. mi. per station), 54 percent of a crime between ¼ and ½ mile, and 20 percent of a crime between ½ and 1 mile. The final column indicates that across all parts of the city more than 1 mile from a train station, 13 percent of a crime would be added. Thus, falling crime within 1 mile of stations outweighs any increase in crime more than 1 mile from stations.

6. Conclusion

We study the effect of public transit on crime using temporary, maintenance-related rail station closures in Washington, DC as a source of variation in public transit access. We find strong evidence that closing down stations reduces crime at other connected stations but no evidence of changes at stations on other lines. Closing one station leads to a 5% reduction in crime at other stations on the same line, which amounts to a 2% decrease in crime across the entire rail network. This matches patterns in access to transit as ridership falls at stations on the same line but not stations on other lines. Closures also shift the spatial distribution of crime. We find suggestive evidence that crime falls more in neighborhoods where few previous offenders live and also falls more when the closed station is in a neighborhood where many previous offenders live. Crime reductions also follow the structure of the rail network, falling more in stations that become disconnected from a larger part of their train lines. Finally, we rule out the possibility that crime falls near stations simply because it is displaced to locations further from stations. If anything, crime falls in areas further from train stations.

We investigate the mechanisms behind the drop in crime and conclude that changes in perpetrator behavior likely drive most of the decrease. Controlling for ridership as a proxy for the availability of victims reduces the size of the measured drop in crime only from 5% to 4%. A decrease in theft from automobiles accounts for the majority of the drop in crime, and few riders "park-and-ride" at the stations at our sample. Also, the heterogeneous effects mentioned above point to a mechanism driven by perpetrator behavior. On the other hand, we find evidence of dynamic effects driven by victim behavior. Riders time their commutes just prior to delays, leading to spikes in both ridership and crime just before station closures. This effect offsets some, but not all, of the drop in crime that occurs during closures.

These results differ from the existing literature. The most closely related studies find no effect of station construction on crime (Billings, Leland and Swindell, 2011), a counterintuitive redistribution of crime from wealthy to poor areas (Ihlanfeldt, 2003), and a redistribution between DUI's and other alcohol-related crimes (Jackson and Owens, 2011). Thus, we provide the first evidence that matches the popular theory that transit may spread crime both by reducing transportation costs for perpetrators and by generating crowds of potential victims. Our results differ from the literature mainly due to our use of temporary, maintenance-related station closures to identify the effect of transit on crime. Construction of new transit lines (e.g. Billings, Leland, and Swindell, 2011; Ihlanfeldt, 2003; Liggett, Loukaitou-Iseris, and Iseki, 2003) are relatively permanent policies with one-time temporal variation. Our approach provides an alternative source of identification that is arguably less susceptible to bias than previous approaches. Additionally, previous estimates use permanent variation in transit access, such as new station construction or changes to operating hours, and thus estimate the long-run responsiveness of crime to transit access. This long run elasticity includes both the direct effect of transit access on perpetrator behavior and general equilibrium effects of police response and changing neighborhood demographics. For instance, previous research demonstrates that house prices rise when stations are built (Gibbons and Machin, 2005; Billings, 2011) which likely leads to demographic changes which could in turn affect crime rates. We effectively isolate short-run changes, such as transportation access, from long-run changes in the neighborhood. Another way our results can be reconciled with the existing literature is if victims and perpetrators respond differently to changes in transportation costs in the short run and long run. Perhaps perpetrators can adjust transportation habits in the long run while this is impossible in the short run. Further research is needed to separate out the direct effect of lowering transportation costs from demographic change and public policy response in the long run. This further study would more directly inform cost-benefit analysis of building public transit stations.

Our results provide support for the theory that perpetrators respond sensitively to transportation costs. A relatively modest change in public transit leads to noticeable changes in perpetrator behavior and a spatial redistribution of crime toward the residences of those committing crimes. Closures only affect crime on the same line, suggesting that the affected perpetrators are unlikely to change trains to commit a crime. Closures matter far more for crime within ¼ mile of a station than for crime further afield, suggesting that the affected perpetrators do not walk large distances to commit a crime. All of these results fit within a version of Becker's model of rational criminal behavior modified to include a spatial dimension and transportation costs (Becker, 1968). Our results suggest perpetrators who respond sensitively to the opportunity cost of travel time on many different dimensions. We provide direct evidence that shocks to transportation costs can affect criminal behavior. In doing so, we directly confirm the commonly held belief that high transportation costs to criminal behavior make crime a local phenomenon.

Knowing that perpetrators respond sensitively to transportation costs matters for policing strategy. As the quotation in the introduction indicates, police consider public transit to be an important factor in the geography of crime. Our results indicate that this evaluation is correct.

Law enforcement may then naturally proceed to a “hot spots” policing strategy that allocates resources to narrowly defined areas around train stations. Our results would both support this strategy as well as suggest practical improvements. For instance, when targeting a part of the city for extra resources, police may wish to avoid simply pushing crime into other nearby areas. Our results suggest that targeting one location for extra police resources may very well displace crime to other locations which are nearby, either by foot or on public transit. Targeting many connected locations simultaneously would be more effective. Policing strategy ought to account for the fact that public transit can spread crime, both by redistributing it across space and by enabling crimes that would not have otherwise occurred.

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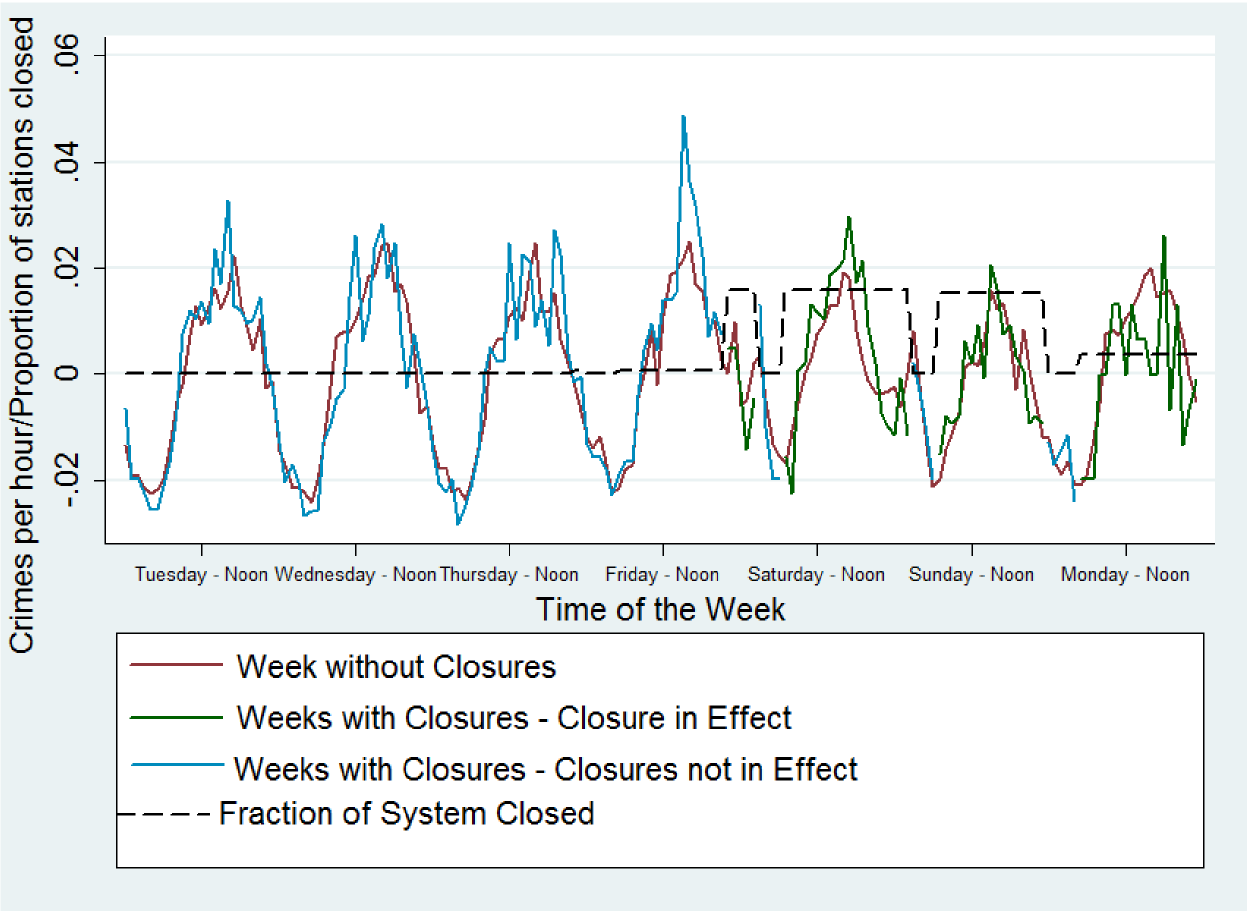
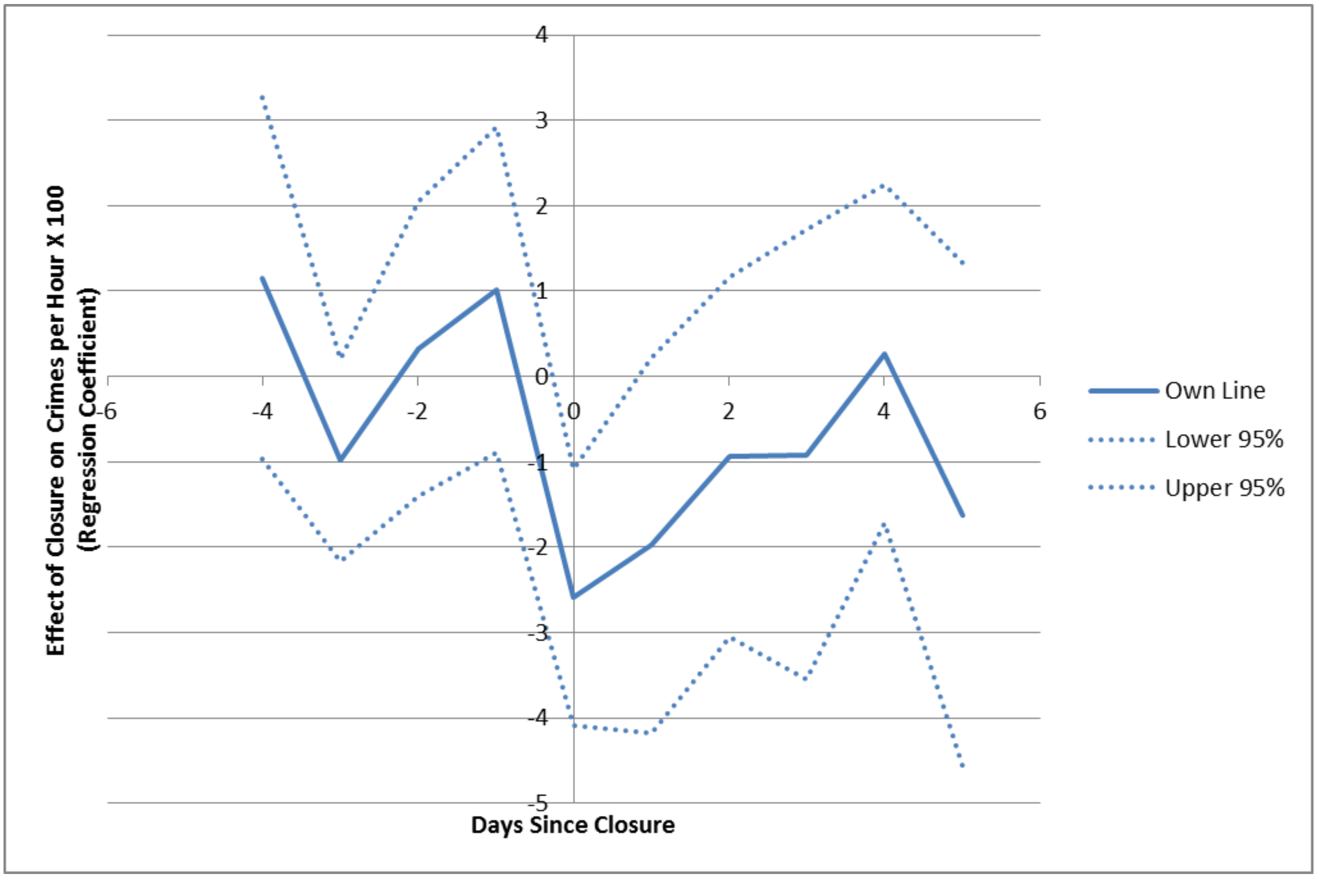
Figure 1. Station closures and crime throughout the week



Station-hour observations are defined as being in a week with no closure if there is no closure in the entire WMATA system within 84 hours of the observation; defined as being in a week with a closure (blue line) if the station is open and the observation is within 84 hours of but at least 1 hour from a closure at a different station on the same line; and defined as exposed to a closure (green line) if the station is open and a different station on the same line is closed. The fraction of system closed variable averages over all days, not just those with closures.

Figure 2: Dynamic Spillovers of Station Closures



This figure reports coefficient estimates and confidence intervals for the γ_{1k}'s from equation (2). Five days before closure is the omitted category. The regression also includes: variables for 6+ leads and lags of a closure, contemporaneous and dynamic effects of own station and other lines closures, station FE, and our standard time controls. Standard errors are clustered at the station level.

Table 1: Summary Statistics

A. Outcomes and Treatment Variables

	All	Entire System Open	Part of System Closed	Station Closed	Other Stations Closed
Number of crimes x 100:					
-All, zero to quarter mile	2.7	2.7	2.9	2.4	2.9
-Violent, zero to quarter mile	0.1	0.1	0.1	0.1	0.2
-Property zero to quarter mile	2.6	2.6	2.8	2.2	2.8
-All, quarter to half mile	5.8	5.8	6.1	4.6	6.2
-All, half to one mile	21.0	20.9	22.8	16.9	23.3
Number of riders	682	706	382	124	400
Percent of line-hours closed:					
-Station	0.4%	0.0%	6.0%	91.7%	0.0%
-System	0.4%	0.0%	6.0%	5.0%	6.0%
-Own line(s)	0.4%	0.0%	5.8%	12.4%	5.4%
-Other line(s)	0.4%	0.0%	6.0%	0.5%	6.3%
N	1019088	943740	75348	4897	70451

B. Baseline Balance

Line Status: Station Status:	Some Closures Open	No Closures Open	Difference No Controls	Difference With Controls
Holiday/Weekend	0.92	0.29	0.63*** (0.03)	--
Lagged Avg. Crimes per Hour	2.9	2.7	0.2** (0.1)	0.0 (0.1)
Lagged Avg. Riders per Hour	951	845	101** (42)	26*** (9)
Fraction Parolee	1.9	2.0	-0.1 (0.1)	--
Parking Available	0.22	0.26	-0.04** (0.02)	--
N	33528	980663		

Unit of observation is station-day-hour. Sample period covers January 1, 2011 - October 7, 2013. Lagged crimes per hour and lagged ridership per hour are computed as the average of hours at the same station from the previous Tuesday through Thursday. All listed quantities are means, except for sample sizes and when otherwise noted. The final two columns of panel B list the coefficient on a dummy for whether any other stations on the same line are closed. In the second to last column, the regression includes only this variable and a variable for if the station itself is closed. The final column also includes dowXhour fixed effects, monthXyear fixed effects, station fixed effects, and a holiday-weekend dummy. Standard are errors in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors are clustered two ways, at the station level and the date level.

Table 2: Main Effect on Ridership

Dependent Variable:	Ridership	Ridership	Entries Only	Exits Only
Fraction Station Closed	-227***	-172***	-91***	-81**
	(65)	(64)	(32)	(32)
Fraction of Line Closed	--	-483***	-237***	-246***
		(138)	(66)	(73)
Fraction of Other Lines Closed	--	-85	-26	-59
		(172)	(83)	(90)
Holiday/Weekend Dummy	-431***	-426***	-213***	-212***
	(76)	(76)	(37)	(39)
Station FEs	Yes	Yes	Yes	Yes
DOW X Hour FEs	Yes	Yes	Yes	Yes
Month X Year FEs	Yes	Yes	Yes	Yes
Date FEs	No	No	No	No
N	1019004	1019004	1019004	1019004

Unit of observation is station-date-hour. Standard are errors in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors are clustered two ways, at the station level and the date level.

Table 3: Main Effect on Crime

	Own Station	Spillovers	Date FE	Poisson	Lagged Outcome	Controlling for Ridership
Fraction Station Closed	0.141	0.297	0.336	0.112	-0.148	0.397*
	(0.244)	(0.245)	(0.236)	(0.116)	(0.297)	(0.236)
Fraction of Line Closed	--	-1.28***	-1.86*	-0.535***	0.613	-0.999**
		(0.462)	(0.877)	(0.186)	(0.797)	(0.461)
Fraction of Other Lines Closed	--	0.434	0.498	0.214	0.793	0.483
		(0.88)	(1.92)	(0.288)	(0.706)	(0.845)
Holiday/Weekend Dummy	-0.454**	-0.446**	--	-0.172**	-0.088	-0.198
	(0.178)	(0.181)		(0.077)	(0.114)	(0.183)
Ridership (thousands)	--	--	--	--	--	0.582***
						(0.155)
Station FEs	Yes	Yes	Yes	Yes	Yes	Yes
DOW X Hour FEs	Yes	Yes	Yes	Yes	Yes	Yes
Month X Year FEs	Yes	Yes	No	Yes	Yes	Yes
Date FEs	No	No	Yes	No	No	No
N	1019004	1019004	126966	1019004	1011990	1019004

Unit of observation is station-date-hour. Standard are errors in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the number crimes within 1/4 mile of the station X 100 except the Poisson model which uses the simple crime count. The outcome is contemporaneous except in the fifth column in which it is lagged by one week. All models estimated by ordinary least squares except column (4), which uses a Poisson model. Standard errors are clustered two ways, at the station level and the date level. The third column restricts the sample to those days where at least one station is closed.

Table 4. Heterogeneous Effects

Dependent Variable:	Crime	Crime	Crime	Crime
Model:	Main Effects	Heterogeneity by Own Station Arrestees	Heterogeneity by Other Station Arrestees	Heterogeneity by Network Location
Fraction Station Closed	0.297	0.525	0.299	0.715**
	-0.457	-0.472	-0.231	-0.312
Fraction Own Line Closed	-1.28***	-1.46**	--	--
	-0.462	-0.625		
Fraction Own Line Disconnected	--	--	--	-0.586***
				-0.209
Fraction Other Lines Closed	0.434	1.2	--	0.41
	-0.88	-1.09		-0.871
Own Station Arrestees *Fraction Station Closed	--	-0.12	--	--
		-0.15		
Own Station Arrestees *Fraction Own Line Closed	--	0.116	--	--
		-0.002		
Own Station Arrestees *Fraction Other Lines Closed	--	-0.388	--	--
		-0.491		
Fraction Own Line Closed - Weighted by Arrestees at Closure	--	--	-0.698***	--
			-0.165	
Fraction Other Line Closed - Weighted by Arrestees at Closure	--	--	-0.229	--
			-0.341	
Station FEs	Yes	Yes	Yes	Yes
Date/Hour Controls	Yes	Yes	Yes	Yes
N	1019004	1019004	1019004	1019004

Unit of observation is station-date-hour. Standard errors are in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variables are the number crimes within 1/4 mile of the station X 100 and total entries plus exits from the station. Standard errors are clustered two ways, at the station level and the date level. Date/hour controls include DOWXhour FE, monthXyear FE, and a holiday-weekend dummy.

Table 5. Main Effects by Type of Crime

	Type of Crime							
	Arson	**ADW**	**Burglary**	**Motor Vehicle Theft**	**Theft from Auto**	**Other Theft**	**Robbery**	**Sex Crimes**
Fraction Station Closed	-0.00192	0.0325	0.00491	-0.0388	0.153	0.0417	0.122	-0.0160**
	(0.00122)	(0.0451)	(0.0370)	(0.0707)	(0.180)	(0.143)	(0.101)	(0.00667)
Fraction Other Stations on Line Closed	0.00374	-0.205	-0.195	0.109	-0.652***	-0.134	-0.218	0.0105
	(0.00536)	(0.125)	(0.129)	(0.159)	(0.236)	(0.426)	(0.172)	(0.0426)
Fraction Stations on Other Lines Closed	0.0115	0.0980	-0.214	0.146	-0.303	0.511	0.272	-0.0859*
	(0.0218)	(0.207)	(0.211)	(0.157)	(0.00463)	(0.789)	(0.358)	(0.0486)
Holiday/Weekend Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Station FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Day of Week X Hour FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month X Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1019004	1019004	1019004	1019004	1019004	1019004	1019004	1019004

Unit of observation is station-date-hour. Standard errors are in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variable is the number crimes within 1/4 mile of the station of a particular type of crime X 100. Standard errors are clustered two ways, at the station level and the date level.

Table 6. Temporal Spillovers

Dependent Variable	Crime	Crime	Ridership	Ridership
Fraction Station Closed	0.297	0.292	-172***	-170***
	(0.457)	(0.244)	(64)	(65)
Fraction Own Line Closed	-1.28***	-1.24***	-483***	-481***
	(0.462)	(0.474)	(138)	(138)
Fraction Other Lines Closed	0.434	0.510	-85	-74
	(0.880)	(0.888)	(172)	(172)
Own Station - Lead	--	-0.716	--	-185***
		(0.639)		(63)
Own Station - Lag	--	0.807*	--	337**
		(0.483)		(95)
Own Line - Lead	--	3.40**	--	631***
		(1.38)		(203)
Own Line - Lag	--	-1.74	--	-801***
		(1.38)		(266)
Not Own Line - Lead	--	-1.47	--	239*
		(1.44)		(136)
Not Own Line - Lag	--	2.06	--	240
		(1.36)		(237)
Station FEs	Yes	Yes	Yes	Yes
Date/Hour Controls	Yes	Yes	Yes	Yes
N	1019004	1018668	1019004	1018668

Unit of observation is station-date-hour. Standard errors are in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. The dependent variables are the number crimes within 1/4 mile of the station of a particular type of crime X 100 and number of entries plus exits at the station. Standard errors are clustered two ways, at the station level and the date level, for all columns.

Table 7. Spatial Spillovers

Dependent Variable:	Crime Density	Crime Density	Crime Density	Crime
Radius (miles):	< 1/4	1/4 < r < 1/2	1/2 < r < 1	> 1
Fraction Station Closed	1.5	0.509	-0.247	--
	(1.25)	(0.6)	(0.337)	
Fraction Own Line Closed	-6.56***	-2.2	-0.205	--
	(2.35)	(2.23)	(1.76)	
Fraction Other Lines Closed	2.14	-0.111	0.29	--
	(4.48)	(2.43)	(2.17)	
Fraction of Whole System Closed	--	--	--	12.5
				(29.7)
Station FEs	Yes	Yes	Yes	No
Date/Hour Controls	Yes	Yes	Yes	Yes
N	1019088	1019088	1019088	24264

Unit of observation is station-date-hour, except for the final column which is at the date-hour level. Standard errors are in parentheses; *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively. Standard errors are clustered two ways, at the station level and the date level, for all but the final column, which only clusters at the date level. The dependent variable is crimes X 100 per sq. mi. in all but the final column, which is crimes X 100. The radius differs over the columns, indicating different distances from the station for which crimes are counted in the dependent variable.

Theory Appendix

While the focus of this paper is empirical, a simple model can illustrate the different ways in which public transit may affect the decision to commit a crime. The model is a simple version of the Becker (1968) economic model of crime with a geographic dimension added in a manner similar to Ihlanfeldt (2003). Note that this model focuses on how mass transit can reduce the opportunity cost of committing a crime for perpetrators, though transportation costs can also affect victim behavior.

Consider a person i who lives at home location h and is deciding whether to work[13], commit a crime in the home neighborhood, or commit a crime at another station s. If he works, he is paid an hourly wage $w_h \epsilon_i$, where w_h is the mean wage for workers from location h, and ϵ_i is an idiosyncratic component of the wage that varies from person to person and is non-negative. Rather than working, an individual can commit a crime at home or at location s. Committing the crime has an expected return (net of losses due to law enforcement) of v_h at home and v_s at the other station. Criminal activity requires C hours to commit the crime and t_{hs} hours to travel from home to the crime scene (out of some finite time budget). We assume that the travel time is zero when committing a crime at home but positive when committing a crime elsewhere so that $t_{hh} = 0$ and $t_{hs} > 0$ when $s \neq h$.

The individual will choose to commit a crime somewhere if the return to working over the same period of time is lower than the return to crime either at home or at the other station. Formally, a crime is committed in the city if:

$$v_s \geq (C + t_{hs}) w_h \epsilon_i \quad \text{or} \quad v_h \geq C w_h \epsilon_i$$

More succinctly, the individual decides to work if:

[13] We follow convention and assume that work is the alternative activity to committing a crime, but "work" could equally represent any activity other than crime that gives person i positive utility.

$$\epsilon_i \geq \max\left\{\frac{v_s}{(C+t_{hs})w_h}, \frac{v_h}{Cw_h}\right\}$$

Given a distribution $F(\cdot)$ of ϵ_i we can characterize the probability of working or committing a crime:

$$\Pr[Work] = 1 - F\left[\max\left\{\frac{v_s}{(C+t_{hs})w_h}, \frac{v_h}{Cw_h}\right\}\right]$$

$$\Pr[Commit\ Crime] = F\left[\max\left\{\frac{v_s}{(C+t_{hs})w_h}, \frac{v_h}{Cw_h}\right\}\right] \quad (A.1)$$

The effect of cutting off access to public transit can be simulated in the model by increasing the travel time from one location to another, t_{hs}. Assuming that the density is well defined, we can analyze station closures on the overall crime level of the city, both at home and at the distant station, as a comparative static. It clearly follows from (A.1) that if relevant margin is that between crime at home and working $\left(\frac{v_h}{Cw_h} > \frac{v_s}{(C+t_{hs})w_h}\right)$, then removing access to public transit does not affect overall crime levels.

$$\frac{\partial \Pr[Commit\ Crime]}{\partial t_{hs}} = \frac{\partial}{\partial t_{hs}} F\left[\frac{v_h}{Cw_h}\right] = 0$$

The decision of committing crime or working only depends on local returns to crime, which are unaffected. Higher transportation costs simply shifts crime from one location to another. On the other hand, overall crime levels can fall if the relevant margin is individuals choosing between crime far from home and working. In this case,

$$\frac{\partial \Pr[Commit\ a\ Crime]}{\partial t_{hs}} = \frac{\partial}{\partial t_{hs}} F\left[\frac{v_s}{(C+t_{hs})w_h}\right] < 0$$

The increased transportation cost shifts people to working rather than simply a different type of crime in this case. Assuming that there are many individuals considering committing a crime with work ability distributed according to $F(\cdot)$, the results imply that the overall level of crime may either decrease or be unchanged by transit station closures.

To examine why the effect on crime overall is ambiguous, we can decompose crime into that committed at home and that committed at another station. The individual will commit a crime at the other station s if the return to crime at s is higher than both the return to working and also the return to committing a crime at home:

$$v_s \geq (C + t_{hs})w_h\epsilon_i \text{ and } v_s - (C + t_{hs})w_h\epsilon_i \geq v_h - Cw_h\epsilon_i$$

More succinctly, a crime will be committed at location s if:

$$\epsilon_i \leq \min\left\{\frac{v_s - v_h}{t_{hs}w_h}, \frac{v_s}{(C + t_{hs})w_h}\right\} \qquad (A.2)$$

Then, (A.1) and (A.2) can characterize the probability of committing crimes at distant station s and home station h as:

$$\Pr[Commit\ crime\ at\ other\ s] = F\left[\min\left\{\frac{v_s - v_h}{t_{hs}w_h}, \frac{v_s}{(C + t_{hs})w_h}\right\}\right]$$

$$\Pr[Crime\ at\ home\ h] = F\left[\max\left\{\frac{v_s}{(C + t_{hs})w_h}, \frac{v_h}{Cw_h}\right\}\right] - F\left[\min\left\{\frac{v_s - v_h}{t_{hs}w_h}, \frac{v_s}{(C + t_{hs})w_h}\right\}\right]$$

It directly[14] follows that:

$$\frac{\partial \Pr[Crime\ at\ other]}{\partial t_{hs}} \leq 0$$

$$\frac{\partial \Pr[Crime\ at\ home]}{\partial t_{hs}} \geq 0$$

Thus, cutting off public transit at home station h increases crime committed at home but decreases crime committed elsewhere. Shutting down a transit station can lead to two different scenarios. In some situations, it may simply re-distribute crime across stations by shifting crime

[14] The second result follows from noticing that it is impossible for $\frac{v_s}{(C+t_{hs})w_h} > \frac{v_h}{Cw_h}$ and $\frac{v_s - v_h}{t_{hs}w_h} < \frac{v_s}{(C+t_{hs})w_h}$ to both hold at the same time.

from distant locations to the area near the closed station. In other situations, the decrease in crime at distant stations may outweigh the increase in crime at home leading to both a redistribution of crime and a drop in the overall level.

To approach this model with our data, though, we have to take into account the reality that any closure affects travel times for any trip traveling through that station. A transit station closure will make it not only harder to leave but also harder to get in. The effects of the model run both ways. To be concrete, suppose that the world consists only of two stations with people making the same decision. Suppose that the number of people at station h considering committing a crime is N_h and the number of people at station s considering crime is N_s. Then, the amount of crime at station h, C_h, will be:

$$C_h = N_s * \Pr[Commit\ crime\ at\ other\ station] + N_h * \Pr[Commit\ crime\ at\ home]$$

Consider a station closures (as in our data) that disrupts travel in both directions. This type of station closure will have an ambiguous effect on total crime at both stations because of the countervailing effects on locally generated crime and crime imported through the transit system:

$$\frac{\partial C_h}{\partial t_{hs}} = N_s * \frac{\partial \Pr[Crime\ at\ other]}{\partial t_{hs}} + N_h * \frac{\partial \Pr[Crime\ at\ home]}{\partial t_{hs}}$$

These theoretical results generate several implications that are important for the empirical exercises to follow:

- Stations closures could in principle either decrease the overall level of crime or simply shift it from one location to another leaving the level unchanged
- Closing a public transit station at a location has two countervailing effects on crime at that location: decreasing access from outsiders who wish to commit crimes but also "trapping" locals who wish to commit crimes elsewhere, who now commit local crimes

- Because of these countervailing effects, a closure at one location could affect crime rates at any other connected station, and the direction of the effect on crime is ambiguous.
- Whether crime falls at any given station affected by a closure (at the station or a connected station) depends in part on whether a station tends to export or import crime. If the closed station has a large population of potential criminals (large N_h), then a reduction in public transit may increase crime at that station. However, if a reduction in public transit access cuts off the station from others with large numbers of potential criminals (large N_h) then the transit closure may lower crime at that station