UNIVERSITY of PENNSYLVANIA

Department of Criminology

Working Paper No. 2015-14.0

Effect of Rail Transit on Crime: A Study of Los Angeles from 1988-2014

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Abstract

Neighborhoods often resist public transit expansion for fears that it will increase crime by attracting transient populations and prospecting criminals. On the other hand, transit may reduce crime by altering economic development and other positive features of neighborhoods. Studies examining public transit and crime have either been cross sectional or examined the impact of public transit expansion in a limited number of locations. We improve on previous research by examining the effect that the Los Angeles Metro Rail system had on crime in neighborhoods. Analyzing data on crimes reported to the police over 27 years we are able to assess the change in crime in the neighborhoods surrounding stations as they opened compared to neighborhoods not exposed to new transit stations. We also capitalize on the fact that during this period Los Angeles experienced two of the nation's longest transit strikes. These interruptions provide a natural experiment that we use to test for the effect of transit on crime neighborhoods. We find no evidence that new transit station openings or a disruption in transit due to strikes result in changes in crime in surrounding neighborhoods.

1. Introduction

The public often resists public transit expansion because of fears that it will generate additional crime in neighborhoods (Poister, 1996). This fear seems reasonable. After all, transit systems may bring more transient people to neighborhoods and increase the criminal offending population in an area. Transit systems may also generate more disorder in neighborhoods, increasing blight and signal that an area is unguarded and crime is tolerated (Wilson and Kelling, 1982). On the other hand, transit may lead to reductions in crime in an area, by spurring economic development and raising property values (Bowes and Ihlanfeldt, 2001).

A number of studies find that transit stops are hot spots for crime. There appears to be specific features of the built environment around high-crime transit stops including: vacant buildings, graffiti, check cashing stores, and alley ways (Levine, Wachs, and Shirazi 1986; La Vigne, 1997; Loukaitou-Sideris, 1999; Loukaitou-Sideris et al., 2001). Research also suggests that street robberies tend to cluster near transit stops (Block and Block, 2000). While there exists an extensive body of literature on how different design features of transit locations are correlated with the variation in crime (Smith and Clarke, 2000), most of this literature is cross sectional. A few quasi-experimental studies that examine crime before and after the opening of transit compared to other locations provide little evidence that transit brings crime (MacDonald, 2015). Yet, the quasi-experimental studies remain inconclusive because they tend to focus on only a few isolated transit expansions. Less is known about the effect of increasing transit access on crime

over sustained periods of time and entire municipalities. A sustained expansion of public transit may exert lasting effects on crime if it produces a noticeable difference to an area's criminal opportunity environments (Cook, 1986; Clarke, 1995).

In this paper we improve on the prior literature in five ways. First, we examine the effect of the entire expansion of the Metro Rail transit system in Los Angeles, the second largest city in the United States. We are, therefore, able to assess the effect that a substantial increase in access to rail transit has on crime for a major city. Second, we rely on a long time series. This allows us to analyze the effects of rail transit expansion on crime across nearly three decades, and not just the short-term effect of transit on crime. Third, we are able to carefully construct comparisons of the change in crime around rail transit stops that open compared to areas that could have had transit stations. This allows us to more carefully estimate the effect of transit on crime. Fourth, twice during the study period the labor union representing transit drivers went on strike. This means that for two extended periods in two different years rail transit in Los Angeles abruptly ceased operation. This provides two natural experiments in which we can examine what happens to crime in areas that have established transit when the system is no longer running.

We begin with a brief discussion of the existing literature on the effect of transit on crime, followed by a more detailed description of the Metro Rail transit system in Los Angeles. Subsequent sections discuss the data and methods of our analysis, the results, and our conclusions.

2. Prior Literature

Several cross sectional studies have found that bus and other public transit stops are hot spots for crime.[1] Levine et al. (1986) found from an onsite survey of three high crime bus stops the presence of drug dealing, a crowded sidewalk, and a nearby high school, suggesting the surrounding environment may be the source for why some transit stops appear to be crime hot spots. Loukaitou-Sideris (1999) examined ten of the highest crime bus stops in Los Angeles that accounted for 18% of all reported crime around bus stops, finding that they were more likely than other stops to be situated in poor, high-crime neighborhoods, on busy intersections in commercial areas, and near vacant land or crowded sidewalks. Loukaitou-Sideris et al. (2001) in a more expansive study of crime around 60 bus stops in Los Angeles found that crime rates were higher at bus stops near an alley, a check cashing store, a liquor store, those with visible signs of graffiti or litter, and those located near "undesirable establishments." Undesirable establishments were, however, the only factor in their multivariate regression model that predicted crime rates. Block and Block (2000) found higher street robbery rates around transit stops in Chicago and New York than other parts of these cities. They noted that the transit stops were also more likely to be located near bars and other businesses that may be sources of crime, raising the question of whether it is transit or other nearby land-uses that are responsible for more robberies. Kooi (2013) found no association between transit locations (bus stops) and crime rates on blocks, after comparing blocks with similar levels of poverty. While these studies provide good descriptive

[1] MacDonald (2015) provides a thorough review of this literature. Here we summarize this review.

evidence of crime around transit locations, they do not tell us whether transit is the cause of crime. Transit may be endogenously related to other land-uses that generate crime.

A few quasi-experimental studies address the question of whether transit causes crime by examining what happens to crime before and after the opening of new transit stations. These studies focus on rail transit. Rail transit has expanded in certain parts of the country, carries more passengers than buses, and could have more of an effect on neighborhood crime rates. Poister (1996) examined the change in crime around two station stops before and after they opened in the suburbs of Atlanta, and found a small and inconsistent relationship between transit openings and crime. There was a significant increase in some offenses and a reduction in others around one transit location in the first month after it opened. In another station opening there was a small increase in one offense category. Over the longer follow-up of 15 months there was no overall impact on crime. Liggett and colleagues examine what happens to crime in neighborhoods after the opening of several Metro Rail "Green Line" transit stations in Los Angeles that connected poor inner-city neighborhoods to more affluent suburban neighborhoods (Liggett, Loukaitou-Sideris, and Iseki 2003). They compared the crime rates around fourteen station stops for the five years before and after their opening relative to the local city or larger jurisdiction in which each station was located. They found that crimes increase in six out of the fourteen station areas relative to the adjacent areas in which each station was situated. All six locations were within the city of Los Angeles. Ihlanfeldt (2003) examined the expansion of transit locations in Atlanta crime over four years (1991-1994) when rail service was expanded, and found that crime increased near downtown and decreased in the suburbs. Billings, Leland, and Swindell (2011) estimated the effect of the announced and actual opening of a light rail transit system in Charlotte, North Carolina on neighborhood crime by comparing changes in crime in neighborhoods within one mile of the new light rail to neighborhoods located near a proposed future expansion area. They found that the announced opening of the light rail was associated with a reduction in crime, but that the actual opening of rail had no impact. This work suggests that it may be economic development that was spurred around light rail lines that reduced crime, and that the operation of the rail had no consequence.

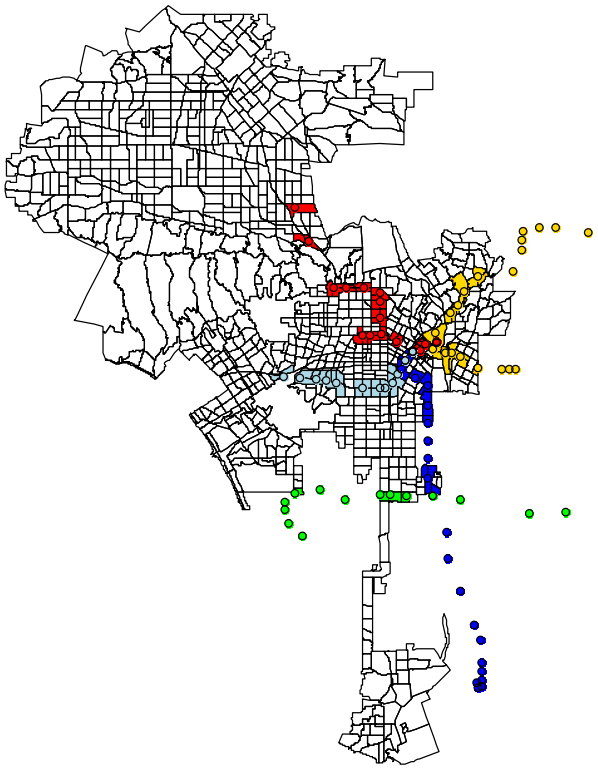
In general the literature paints an inconsistent picture of whether transit impacts crime in neighborhoods. Most of the cross sectional studies examine how the variation in the built environment around transit stations is associated with crime, not whether transit itself causes crime to increase in an area (MacDonald, 2015). The quasi-experimental research provides limited evidence to support the idea that transit causes crime. However, this literature has notable limitations. First, the studies tend to focus on only short-term changes in crime after the opening of transit. It is possible that it takes criminals time to update their daily routines and adjust to the benefits that transit may provide for providing easier crime targets. Additionally, if transit results in other changes to the land-use or population of neighborhoods it is possible that transit could impact crime over a long-term. Second, studies tend to focus on only a few locations in which transit expanded. This may mute the ability to detect the effect of transit on crime. Third, rail transit is fundamentally different than bus transit in reshaping the land-use around transit locations and increasing passenger capacity. This may mean that rail transit is more fundamental for reshaping the crime environment of places.

A strong belief persists in the public that transit expansion will bring crime. In the case of the Metro Rail in Los Angeles this can be seen from several expansions. In 2012 Culver City became the newest community to be connected to the Metro Rail system. Culver City police indicated that crime had yet to change. A Culver City police lieutenant stated "In general we have not had any measurable increase in crime related to … the increased number of people that are now in our city as a result of taking the Expo line" (Simpson, 2013). The fact that this topic was on the consciousness of the police is indicative of the fear that transit expansion has.

3. Metro Rail in Los Angeles

While Los Angeles is not known as a transit city, over the past 25 years Los Angeles has made a major investment in rail transit with the development of the Metro Rail system. In 1961 the Los Angeles Metropolitan Transit Authority ended service to the last of the remaining Pacific Electric rail lines (Boarnet, 2012). For the next thirty years Los Angeles was the largest city in the U.S. without a rail transit system. Due to a variety of factors, including the passage of several sales taxes dedicated to transit development, Los Angeles County began planning for the development of rail transit in the 1980s. In 1990 Metro Rail opened the Blue Line, the first in a series of new commuter rail lines (Boarnet, 2012). By 2012 Los Angeles had expanded its rail service to six lines (Blue 1990; Red/Purple 1993; Green 1995; Gold 2003; Expo 2012) covering 87 miles of service and carrying more than 300,000 daily riders (Metro, 2015). Thus, Los Angeles went from having no rail commuter transit in 1988 to the expansive system in 2012 shown in Figure 1. The transit system also connects many impoverished areas with relatively high crime to affluent areas in the western and central parts of Los Angeles. Thus, the expansion of Metro Rail offers a unique opportunity to estimate the effect of rail transit on changes in neighborhood crime.

Figure 1: Los Angeles Neighborhoods and Metro Rail Stations



Note: Polygons represent LAPD reporting districts in 2005. The circles represent the locations of Metro Rail stations color-coded according to the rail line serving that station. Several stations are located outside of the Los Angeles city limits.

Operation of the system has been largely continuous since 1990. However, labor disputes caused two major disruptions to rail transit in Los Angeles. From September 16 to October 17, 2000 and then again from October 14 to November 18, 2003 transit workers went on strike, resulting in complete shutdowns of the system. These present natural experiments to examine the effect of transit on crime in Los Angeles. During this time period four of the major Metro Rail lines were operating and carrying an estimated 220,000 passengers on an average weekday[2]. Prior research shows strong effects of the strike on freeway speeds and length of commutes during peak commuting hours (Lo and Hall, 2006; Anderson, 2014). These studies show that the strikes substantially disrupted the city's transportation system.

[2] http://media.metro.net/projects_studies/research/images/reports/mta_bus_regional_weekday_travel_patterns.pdf

4. Methods

Data

The crime data for examining the effect of transit on crime consist of the quarterly crime reports produced by the Los Angeles Police Department (LAPD) from 1988 to 2014, a time span which encompasses the creation of the entire Los Angeles Metro Rail system. Data were compiled from two sources, LAPD archival data kept at the Los Angeles Public Library and incident-level data acquired from the LAPD directly. The archival data consist of roughly 2,300 pages of tables reporting the number of crime incidents by year, quarter, crime type, and reporting district. RDs are LAPD's neighborhood area designation and, similar to census tracts, they occupy more territory when the residential population is lower. This means the counts of crime per reporting district are effectively a rate per residential population (Cook and MacDonald, 2011). We scanned the archival records from the library and used character recognition software to digitize them. The pages included column and row sums by RD by crime type by year, which we used as checksums to verify the accuracy of the digitization.

The Los Angeles Times reported on the undercounting of crime in 2005 (Poston, 2014). The archival data scanned showed a dramatic drop in crime during this year. The LAPD audited and reclassified all of its 2005 crime incidents. Therefore, we relied on the audited crime incident data from 2005 onward provided by the LAPD. We focus our analysis on seven crime categories, aggravated assault, burglary or theft from a vehicle, burglary, grand theft auto, grand theft person[3], homicide, and robbery. These crimes were consistently documented throughout the 27-year study period. For the analysis of the transit strike we used incident level data from 2000 and 2003 rather than the quarterly data so that we could capture the precise effect of the start and end dates of the transit strikes.

LAPD has frequently renumbered, merged, and split RDs over the study period. Relying on shape files provided by the LAPD, historic maps, and a crosswalk used in Grogger (2002), we relabeled all RDs to the 2005 boundaries, merging some 2005 RDs in order to align with RDs that were not merged in earlier years. This resulted in 939 RDs. Data from 2009 onward were geocoded directly into the 2005 RD locations.

Data on transit station openings were collected from historical records posted on the Los Angeles Metro website (metro.net) and the associated historical archives from its recent 25 year anniversary commemoration (metro25.squarespace.com).[4]

Models

Since many of the 939 RDs in Los Angeles are far from places that will ever have train stations, we focused the analysis on the 281 RDs that have a boundary within 1km of a station

[3] Grand theft person is a misdemeanor listed under California PC 487(c) and involves the stealing of property of any value directly off of another person without force or threat of force.

[4] All of the data, including the scanned archival data, historical RD maps, RD crosswalks, station maps and timelines are available from this study's GitHub site, https://github.com/gregridgeway/LAPDcrimedata.

that will open at some time during the study period. We considered RD *i* to have a station in quarter *t* if there is a station open within 200m of the RD boundary[5] in quarter *t*.

The first model we estimate takes the form

$$\log(\lambda_{it}) = \beta_0 + \beta_1 I(\text{distance}_{it} < 200) + \alpha_i + \gamma'_{\text{division}(i)} ns_{15}(t) \quad (1)$$

where $y_{it} \sim \text{Poisson}(\lambda_{it})$ is the count of crimes in RD *i* in quarter *t*. β_1 captures the treatment effect of having a nearby station. This is essentially a model for a stepped wedge design (Hussey and Hughes, 2007). Model (1) also includes a fixed effect for the RD and 15 natural spline parameters[6] for each division to capture the local smoothed time trend over the 108 quarters, thus controlling for secular trends that are specific to regions of Los Angeles.

This and all subsequent models were estimated using maximum quasi-Poisson likelihood and clustered standard errors within RDs to allow for dispersion and dependence within RDs.

Classical approaches for conducting significance tests can be sensitive to distributional assumptions such as overdispersed counts, auto-correlation, and clustering. Even robust standard error estimates depend on asymptotics, of which we are unsure whether our sample sizes offer adequate approximations. Permutation tests offer a non-parametric alternative to generating a reference distribution for the parameters of interest. The general approach is to randomly permute the treatment label (having a station in our case) on the units of observation (the RDs). For each permutation of the treatment labels we re-compute the test statistic. The permutation p-value is the fraction of test statistics that are as or more extreme than the original observed test statistic. We repeatedly exchanged the station openings between the RDs, such that a station that actually had a station open in 2003 will be randomly assigned to have another RD's station assignment. We repeated this randomization of station openings 10,000 times, refit the model shown in (1), and formed the null reference distribution using the 10,000 collected estimates of β_1.

It is possible that crime changes around rail station openings are dependent on the existing crime level in neighborhoods. To examine this possible effect modification, we estimated a second model according to the following form:

$$\begin{aligned}\log(\lambda_{it}) = \beta_0 + \beta_1 I(\text{distance}_{it} < 200) + \beta_2 I(\text{distance}_{it} < 200) * \text{low}_{it} \\ + \beta_3 I(\text{distance}_{it} < 200) * \text{high}_{it} + \gamma'_{\text{division}(i)} ns_{15}(t) + \alpha_{\text{division}(i),t}\end{aligned} \quad (2)$$

In model (2) each RD is assigned to low, medium, or high crime groups depending on whether they were in the lowest, middle, or highest tertile of total crimes/km^2 in the two quarters before a station opened nearby. Model (2) includes a station/crime level interaction term to assess whether the effect of station differs by the recent crime level in the neighborhood. The model also includes a separate time trend by division as well as a quarter by division term.

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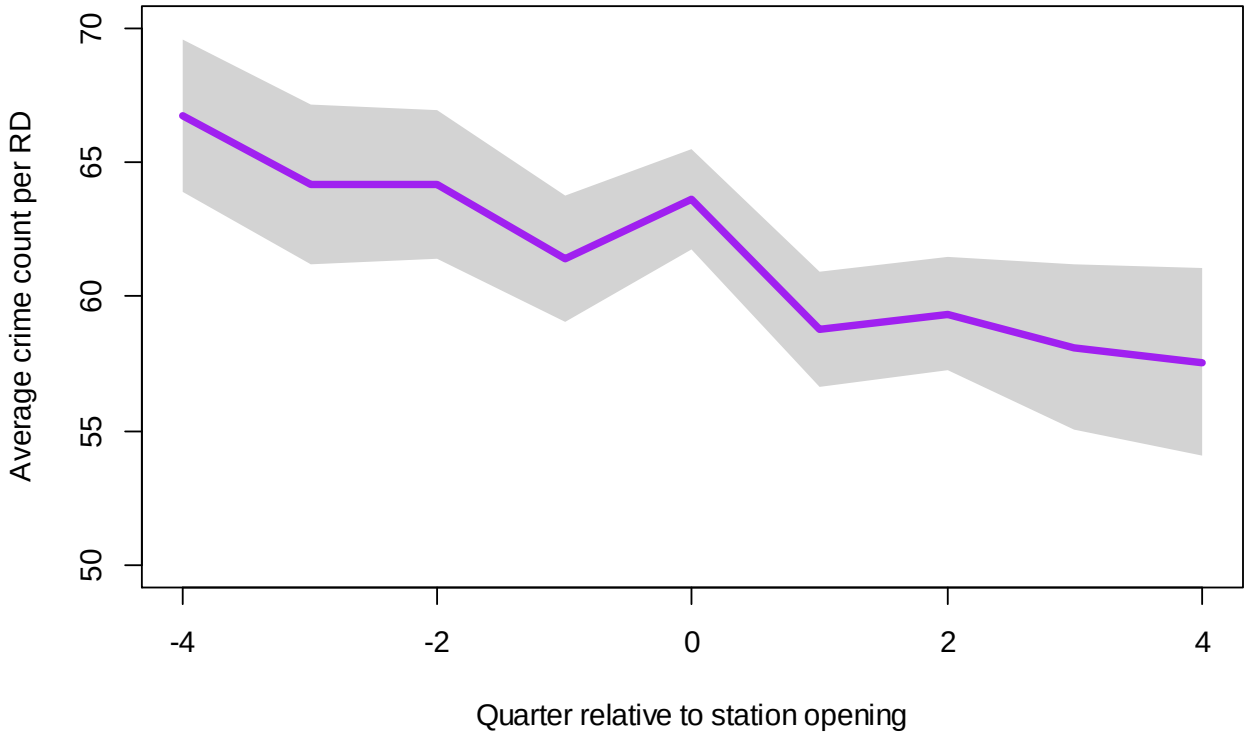
[5] 200m from the boundary was selected so that the majority of residents in the RD would be within a typical walking distance to the Metro Rail station.

[6] $ns_{15}(t)$ represents a set of 15 natural spline basis functions.

A key identifying assumption of models (1) and (2) is that station openings are independent of existing crime trends during this time period. Our estimates of station openings on crime could be biased if station locations are determined based on recent crime trends. Even though this is unlikely given that many years of planning and construction precede a station opening, we test for this possibility directly by selecting each RD only the year before and year after a station opened within 200m of the RD.[7] This way the effect of station opening on crime nearby is only determined relative to crime in the period just before the opening. Figure 2 provides a visual guide to this analysis. In Figure 2 for each of the 116 RDs that eventually have a station opening, we shifted that RDs' time series so that the quarter containing the station opening occurs in quarter 0. While this plot shows the known crime decline, there is also a modest increase between quarter -1 and the quarter with the station opening, an increase from about 62 crimes to 64 crimes (about 3%) on average per RD.

Figure 2: Trend in the four quarters before and after the station opening



To assess whether the change in crime rates at quarter 0 is real or a result of normal variation in crime rates, we estimated this model as

$$\log(\lambda_{it}) = \beta_0 + \beta_1 I(\text{distance}_{it} < 200) + \alpha_i + ns_{15}(t) \quad (3)$$

[7] For some RDs, this will be from a period in the mid-1990s while for other RDs this will be a sequence from the late 2000s.

In model (3) the effect of transit on crime is estimated for the year after a station opens relative to the year before. We extend this specification and consider the effect of the distance from the station on crime in the year after relative to the year before.

$$\log(\lambda_{it}) = \beta_0 + \beta_1 \text{distance}_{it} + \alpha_i + ns_{15}(t) \quad (4)$$

In model (4) $\exp(\beta_1) - 1$ will capture the percent increase in crime for each additional meter an RD is from a station, measuring the effect of proximity to station on crime.

For the permutation test for this analysis, we randomly selected a block of nine quarters from each RD's 108 quarter time series to replace the actual nine quarters spanning that spanned the station's actual opening. This effectively permutes the timing of a station opening. For each permutation we refit models (3) and (4). Even though there is a declining crime trend through the study period, that trend will be present in any nine quarter sequence of crime counts for an RD. The permutation test answers whether the parameter estimates from the actual observed timing of station openings is extreme according to the reference distribution.

2000 and 2003 Los Angeles Transit Strikes

We use the 2000 and 2003 strikes to test the impact of rail transit on crime. For each strike period we selected the RDs that were within 1 km of a transit station (*n*=184 in 2000, *n*=212 in 2003). We labeled the RDs that were within 200m of a station as "transit" RDs (*n*=65 in 2000, *n*=77 in 2003). Finally, we included daily crime counts from the one month before the strikes began through one month following the end of the strikes[8]. We estimated this model according to the following form:

$$\begin{aligned}\log(\lambda_{it}) = {} & \beta_0 + \beta_1 \text{transit}_i + \beta_2 \text{strike}_t + \beta_3 \text{transit}_i * \text{strike}_t + \\ & \beta_4 I(\text{year}(t) = 2003) + \beta_5 I(\text{year}(t) = 2000)t + \beta_6 I(\text{year}(t) = 2003)t\end{aligned} \quad (5)$$

The primary parameter of interest is $\exp(\beta_3)$, which captures the change in crime in RDs with stations during a strike relative to when the transit system was operational compared to those without station access. This parameter can be expressed as a ratio of rate ratios (RRR), a form of difference in difference estimator, as shown in (6)

$$\exp(\beta_3) = \frac{\lambda_{\text{transit,strike}}}{\lambda_{\text{transit,}\overline{\text{strike}}}} \Big/ \frac{\lambda_{\overline{\text{transit}},\text{strike}}}{\lambda_{\overline{\text{transit}},\overline{\text{strike}}}} \quad (6)$$

We did not differentiate the effect of the strike in 2000 and 2003 so that $\exp(\beta_3)$ represents the average effect over both periods. The model also allows for the level of crime and the trend in crime to vary by the year of the strike. Since we are looking at a short window of about 90 days in each of the periods, we used a linear time trend, allowing slopes to differ in 2000 and 2003.

[8] Specifically, August 14, 2000 through November 11, 2000 and September 12, 2003 through December 19, 2003

5. Results

Table 1 shows the estimated effect of transit stations on crime from model (1). Overall we find an estimated 2.6 percent reduction in the number of crimes following a station opening, but the precision of the estimate cannot distinguish the effect from 0. The stations tend to be associated with crime reduction across all categories except homicide, the rarest crime incident and the one with which we have the least amount of precision. However, across all crime categories, all confidence interval includes 0, suggesting that the magnitude of the effect of rail stations on crime is smaller than we have precision to detect. In summary, we see no evidence that opening of transit stations affects crime in neighborhoods.

Table 1: Effect of transit stations on crime

Crime type	Average crime count per RD per year	% crime increase	95% CI	Permutation p-value
Total	216.9	-2.6	(-6.2, 1.2)	0.21
Assaults	39.8	-3.9	(-9.4, 1.9)	0.17
Burglary/theft from vehicle	58.3	-3.6	(-9.9, 3.1)	0.34
Burglary	34.5	-2.2	(-8.1, 4.2)	0.51
Auto theft	46.1	-3.8	(-9.3, 2.0)	0.17
Grand theft person	4.5	-6.9	(-19.0, 7.1)	0.35
Homicide	0.9	4.6	(-8.6, 19.7)	0.51
Robbery	32.8	-0.9	(-7.9, 6.6)	0.77

Note: The average crime count per RD per year gives the average across the study period to give perspective on the scale of the effects. These average counts were computed across the entire 27 year period, but note that crimes counts were twice as high in the first half of the study period than the latter half of the study period. The permutation p-values shown here are of similar magnitude to what we would obtain using standard methods.

Figure 3 shows the modeled time trend for total crime marginalizing over the RD fixed effect. The figure visually shows the minimal effect stations appear to have on crime.

Figure 3: Modeled trend in the average crime count per RD with and without a station

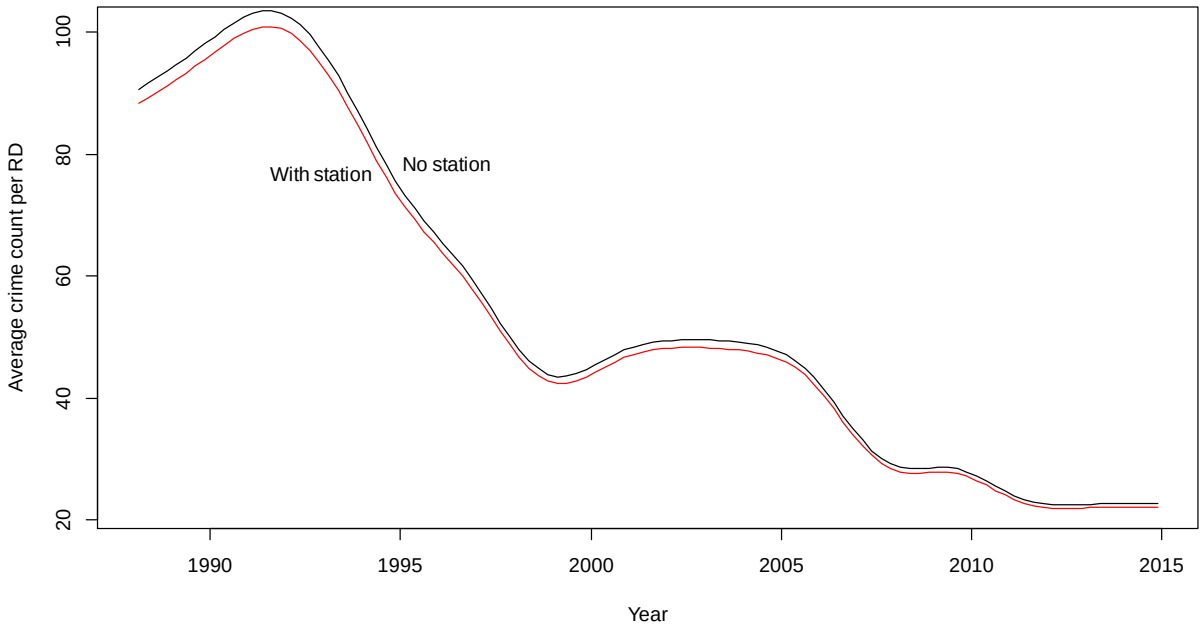


Table 2 shows the relative change in crime rates from model (2) with and without a station relative to the RDs with medium levels of crime, computed as $\exp(\beta_2) - 1$ and $\exp(\beta_3) - 1$. The results show that any differential effects of rail stations on crime were below our power to detect them. The 95% confidence interval for thefts from vehicle is the only one that does not overlap 0. However, we also computed a non-parametric permutation test p-value by randomly reassigning station openings to RDs and found that no coefficient was unusually large.

Table 2: Differential effect of station opening on high and low crime neighborhoods

Crime type	% increase in crime in low crime RDs	95% CI	Permutation p-value	% increase in crime in high crime RDs	95% CI	Permutation p-value
Total	-10.3	(-27.3, 10.8)	0.35	-5.9	(-28.2, 23.4)	0.70
Assaults	-2.3	(-30.0, 36.4)	0.88	-3.4	(-32.8, 38.9)	0.88
Theft from vehicle	-22.0	(-37.7, -2.4)	0.12	-9.2	(-31.9, 21.3)	0.61
Burglary	-14.1	(-31.9, 8.4)	0.18	8.8	(-19.7, 47.2)	0.61
Auto theft	-0.3	(-20.1, 24.4)	0.98	4.7	(-21.1, 39.0)	0.77
Grand theft person	-4.6	(-45.8, 68.0)	0.87	-11.7	(-51.1, 59.4)	0.69
Homicide	-2.4	(-30.0, 36.3)	0.89	8.5	(-22.8, 52.5)	0.69
Robbery	-5.8	(-32.3, 31.2)	0.67	-24.8	(-47.4, 7.4)	0.13

Table 3 shows the percent increase in crime attributable to having an accessible station, computed as $\exp(\beta_1) - 1$ from model (3). Table 3 also shows $\exp(\beta_1) - 1$ from model (4), the percent increase in crime for each additional kilometer separating the RD from a station. This table also shows permutation test p-values. Unlike the earlier tests, these were calculated by randomly selecting a different consecutive eight quarter sequence of crime count outcomes from within the same RD, re-estimating the model on the shuffled data, and computing the probability of observing a $\beta_1$as or more extreme than the one estimated on the original dataset.

Table 3: Effect of station presence and distance on crime for RDs with accessible stations comparing the year before and after station openings

Crime type	% increase in crime when station opens	95% CI	p-value	% crime increase per km away from station	95% CI	p-value
Total	2.7	(-3.1, 8.9)	0.43	0.1	(-0.1, 0.4)	0.50
Assaults	-0.6	(-9.9, 9.6)	0.91	0.0	(-0.5, 0.6)	0.97
Burglary/theft from vehicle	5.6	(-3.0, 14.8)	0.40	0.0	(-0.4, 0.4)	0.95
Burglary	1.5	(-13.1, 18.5)	0.85	0.5	(-0.2, 1.2)	0.26
Auto theft	6.6	(-5.8, 20.7)	0.23	0.1	(-0.3, 0.5)	0.83
Grand theft person	-8.7	(-28.6, 16.7)	0.51	1.2	(0.2, 2.3)	0.18
Homicide	-27.7	(-59.9, 30.4)	0.31	0.0	(-3.5, 3.5)	0.98
Robbery	0.6	(-9.0, 11.3)	0.92	0.1	(-0.6, 0.7)	0.88

The results suggest that any effect of being close to a rail station or having an accessible rail station is smaller than the precision of our estimates, showing no effect of transit distance on crime.

Figure 4 visually shows the results of the analysis of the two labor strikes. The two vertical lines in each of the two plots mark the start and end of the strikes. The trends in the plot show the results of the model fit to the total crime per RD. In both 2000 and 2003 the RDs without stations a relatively smooth linear trend throughout the period, before, during, and after the strike. The RDs with stations, on the other hand, show a clear jump in the average crime counts during the strike. This offers evidence counter to the claim that stations attract crime.

Figure 4: Modeled trend in the average crime count per RD with and without a station

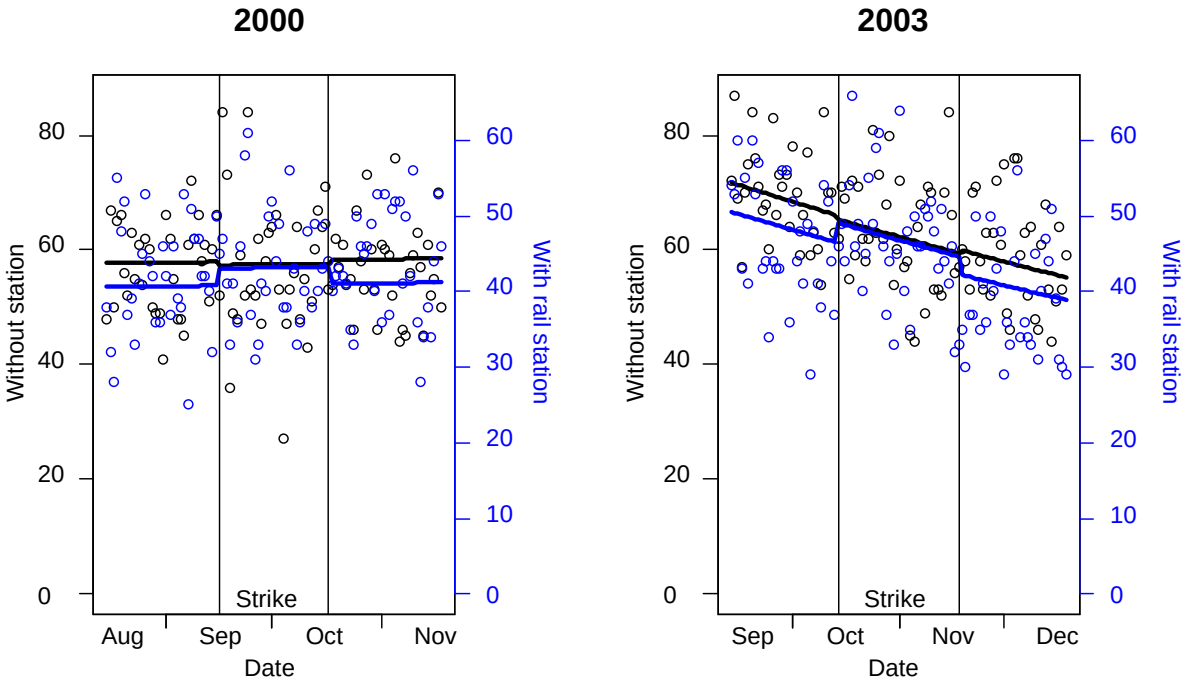


Table 4 shows the estimated RRR and the associated permutation test p-values, computed by randomly shuffling the assignment of transit to RDs. We find that across most crime types the RRR is close to 1.0 indicating that the transit strike, if it had an effect, affected both transit accessible RDs and non-transit accessible RDs equally. The effect on total crime approaches the common standard of statistical significance, but the direction suggests that, if anything, crime increases when the Metro Rail stations close.

Table 4: Ratio of rate ratios estimating the effect of the transit strike on crime

Crime type	RRR	95% CI	Permutation p-value
Total	1.07	(0.99, 1.14)	0.068
Assaults	1.02	(0.87, 1.21)	0.79
Burglary/theft from vehicle	1.10	(0.98, 1.24)	0.15
Burglary	1.13	(0.95, 1.34)	0.18
Auto theft	1.06	(0.92, 1.21)	0.46
Grand theft person	1.06	(0.66, 1.70)	0.83
Homicide	2.22	(0.76, 6.51)	0.17
Robbery	1.00	(0.84, 1.19)	0.99

Robustness Check

After examining the expansion of Metro Rail using several models that exploit the phased roll out of the system and two labor strikes that disrupted Metro Rail operation for extended periods, essentially we find that rail transit expansion had no effect on crime in Los Angeles.

The key threat to the validity of the analysis is the possibility that recent crime trends influenced the placement and timing of station openings. This is unlikely as planning for lines and station openings occurs years if not a decade in advance of opening. Nonetheless, we checked that the crime trends just before station openings did not differ between RDs without stations and those about to receive a station. We examined the seven time points at which nine or more stations opened in the same quarter. We estimated the interaction between time and an indicator of an RD about to receive a station. Table 5 shows the estimated interaction effects.

Table 5: Estimate of the interaction effect of time and station opening in the two years leading up to the station opening

Station Opening	Coefficient	SE	p-value
Blue 1990	0.029	0.107	0.78
Red 1993	-0.018	0.115	0.88
Red 1996	0.014	0.089	0.88
Red 1999	-0.019	0.086	0.82
Gold 2003	-0.036	0.144	0.80
Gold 2009	0.021	0.169	0.90
Expo 2012	-0.088	0.094	0.35
Combined	-0.012	0.007	0.10

This analysis suggests that total crime trends in RDs without stations and those about to have a station appear to be approximately parallel in the two years leading up to the station opening. The estimate using data combined across all seven line openings suggests that crime was declining slightly more rapidly in advance of the station opening. Although the magnitude is small the direction of the bias is toward finding an effect of the station decreasing crime.

6. Conclusions

Transit plays a vital role in public infrastructure spending in many regions. The majority of funding for public transit typically comes from sales taxes, grants, and publicly issued bonds. A relatively small fraction (less than 10%) of the Los Angeles County Metropolitan Transit system is paid for out of fare revenues.[9] However, the maintenance and expansion of public transit has been justified as a basis for reducing traffic congestion, improving economic development, and reducing the spatial mismatch between employment opportunities and low income household locations. Yet, neighborhoods often resist public transit expansion for fears that it will increase crime in neighborhoods. If crime is a big concern, arguably it deserves a more central role in planning for and promoting public transit expansion. Criminal opportunities may indeed be created by the expansion of transit, if it becomes easier for criminals to travel to wealthier neighborhoods where there are more lucrative crime environments (Clarke, 1995). Transit may also increase crime by shifting the number of transient people to areas, generating extra litter and other signs of disorder that signal an area is uncared for (Wilson & Kelling,

[9] http://media.metro.net/projects_studies/funding/images/2012_funding_sources_guide.pdf

1982). If more people take public transit to commute to work there also may be more potential victims traveling in relatively unguarded environments. In contrast, transit expansion may reduce crime if it changes the built environment of neighborhoods through increasing economic development, changing residential populations, or increasing the number of people on streets who act as guardians (Cohen & Felson, 1979).

As reported here, the creation and expansion of Metro Rail in Los Angeles had no impact on crime. The expansion of a major metropolitan rail system that brought hundreds of thousands of passengers a day to neighborhood station stops that would have in the past been required to travel via bus or car had no appreciable crime effect. Rail transit appears to have no crime increase or reduction effects. It appears that the concern that crime is a consequence of transit expansion is overstated in the context of Los Angeles. Of course crime can and does occur on and near transit systems, but it suggests that crime on transit may produce no major consequence for overall neighborhood crime patterns.

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