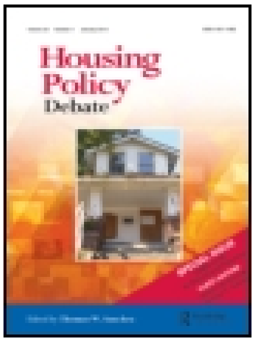

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Investigating Transit-Induced Displacement Using Eviction Data

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ABSTRACT
This article uses eviction data to test the transit-induced displacement hypothesis—that the placement of new transit stations will lead to elevated property values, gentrification, and displacement. We use a case study of four cities in the United States that built or extended rail lines between 2005 and 2009: Newark, New Jersey; San Diego, California; Seattle, Washington; and St. Louis, Missouri. We employ a combination of propensity score matching and difference-in-differences modeling to compare eviction filing rates in gentrifiable neighborhoods near new transit stations with a set of similar neighborhoods not close to the station. We find very limited evidence that new transit neighborhoods experienced heightened rates of evictions compared with the controls. In three of the four cities, the effect of the opening of the station on eviction rates was insignificant. Eviction rates did spike in St. Louis immediately following the opening of the line, but this time period also coincided with the financial crisis.

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public transit; residential mobility; evictions; propensity score matching; difference-in-differences

The transit-induced gentrification and displacement hypothesis posits that the increased accessibility and economic development that accompany new transit investments will be capitalized into nearby property values, leading to gentrification and a disproportionate exodus of lower income residents (Dawkins & Moeckel, 2016; Padeiro, Louro, & da Costa, 2019; Rayle, 2015; Zuk, Bierbaum, Chapple, Gorska, & Loukaitou-Sideris, 2018). Such an outcome would contradict the improved mobility benefits that new public transit may provide lower income and autoless residents, raising concerns about the social equity implications of transit and forming a paradox in their investments (Rayle, 2015; Revington, 2015). Indeed, new rail transit projects have been criticized for promoting a neoliberal urban development agenda, emphasizing economic growth above all else, potentially at the expense of the most vulnerable residents (Culver, 2017; Olesen, 2020).

The empirical evidence on the relationship among transit, gentrification, and displacement has been less alarming than the critical literature may suggest. Many of these studies have occurred at the neighborhood or census tract scale, finding minimal support for the notion that transit and gentrification are definitively linked. Rather, metropolitan and local contextual factors generate mixed trajectories for neighborhoods near new transit stations (Baker & Lee, 2019; Bardaka, Delgado, & Florax, 2018; Deka, 2017; Dong, 2017; Hess, 2020; Kahn, 2007; Nilsson & Delmelle, 2018; Padeiro et al., 2019; Pollack, Bluestone, & Billingham, 2010). Fewer studies have looked at the residential movements that shape neighborhood-scale outcomes, but—like residential mobility analyses in the broader gentrification literature—these studies have found no evidence that lower income residents disproportionately move out of new transit neighborhoods (Boarnet, Bostic, Burinskiy, Rodnyansky, & Prohofsky, 2018; Delmelle & Nilsson, 2020; Rodnyansky, 2018).

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Quantifying displacement has long confounded researchers as the data sets used to measure this phenomenon are widely critiqued as inadequate to capture its complex spatial and temporal nature (Newman & Wyly, 2006; Rayle, 2015). In this article, we examine the connection between transit and displacement from an alternate vantage point, using rates of evictions as a metric for testing the transit-induced displacement hypothesis. Eviction rates have been used in a limited number of studies with respect to gentrification (Chum, 2015; Sims & Iverson, 2019) and they provide a counter indicator from more traditionally employed residential mobility data sets. Eviction data are by no means a panacea to the challenges surrounding quantifying displacement; rather, they provide another piece of evidence, which—collectively, with other forms of scholarship—helps to paint a portrait of the effects of transit on surrounding neighborhoods and residents. Using four cities across the United States that built or extended rail transit systems between 2006 and 2009, we compare eviction filing rates in new transit neighborhoods with those of otherwise similar neighborhoods in the same city using a combination of propensity score matching and difference-in-differences estimations. We find limited differences between transit neighborhoods and their corresponding set of control neighborhoods, adding to the growing literature that has thus far been unable to quantify a significant impact of transit on local neighborhoods.

Background

The theoretical relationship between new transit investments and evictions centers on the potential for transit to spur gentrification, which, in turn, may lead to increases in displacement. Evictions can be considered a form of direct displacement that occurs when a tenant is unable to afford increased rent, as one example (Chum, 2015; Marcuse, 1985); it is a type of involuntary move. This literature review first covers the transit-gentrification evidence, followed by the connection between gentrification and evictions.

Transit, Gentrification, and Displacement

The conceptual connection between transit and gentrification is relatively straightforward. Transit brings about improvements in accessibility and is often accompanied by strategic transit-oriented development (TOD) surrounding stations. If these improvements are considered desirable amenities by residents, then increased competition for housing nearby will drive up property values and rents. Those willing and able to afford the location premium of these newly invested-in urban locations will out-bid lower income residents unable to keep up with rising costs of housing, leading to population turnover in new transit neighborhoods (Padeiro et al., 2019; Rayle, 2015; Zuk, Bierbaum, Chapple, Gorska, & Loukaitou-Sideris, 2018). Transit-induced gentrification is also often associated with newly built urban infill on previously underutilized or vacant land, especially in designated TOD zones in the area immediately surrounding transit stations (Bhattacharjee & Goetz, 2016). This type of new-built gentrification may result in an indirect type of displacement whereby nearby residents experience a loss of place (emphasizing dis*place*ment), but are not necessarily undergoing a physical, direct displacement (Davidson & Lees, 2010).

The empirical evidence in support of the transit-induced displacement hypothesis has been tenuous at best. Although there is a general consensus in the literature that new transit stations often generate price premiums on nearby properties (Billings, 2011; Bowes & Ihlanfeldt, 2001; Hamidi, Kittrell, & Ewing, 2016; Ke & Gkritza, 2019), the contextual sensitivity of these effects has also been emphasized (Higgins & Kanaroglou, 2018). The type of station, demand for housing, extent and type of surrounding TOD, and location within the city all have the potential to accentuate or dissipate these effects (Duncan, 2011; Hess & Almeida, 2007; Zhong & Li, 2016).

Not surprisingly then, the evidence that new transit stations lead to neighborhood-level changes, including gentrification, has also been mixed, underscoring the same contextual considerations that the price capitalization literature has revealed (Kahn, 2007; Padeiro et al., 2019). Studies have shown significant heterogeneity in the number of neighborhoods undergoing changes between metropolitan areas: those with stronger population and economic growth such as San Diego, California; Minneapolis, Minnesota; Denver, Colorado; and San Francisco, California have higher rates of neighborhood change or gentrification compared with more stagnant cities like Buffalo, New York, or Baltimore, Maryland (Baker & Lee, 2019; Bardaka et al., 2018; Nilsson & Delmelle, 2018). Countergentrification, or declining neighborhood income levels, near new transit stations were found in neighborhoods in Portland, Oregon, a city often lauded for its progressive land use and transit policies (Baker & Lee, 2019). Some of the variation in the findings across the literature can be attributed to their differing research designs. A failure to utilize a set of control neighborhoods to construct counterfactuals to transit neighborhoods is a limitation across many of them (Padeiro et al., 2019).

Very few studies have used a disaggregate approach to study the relationship between transit, neighborhood gentrification, and displacement. Delmelle and Nilsson (2020) used the Panel Study on Income Dynamics to test the hypothesis that lower income residents have disproportionately moved out of new transit neighborhoods across the United States since 1970. The authors found, as did other residential mobility studies on gentrification, that lower income residents have a higher likelihood of leaving their neighborhood overall, but the opening of a new transit station had no impact on the probability of exiting. Given the heterogeneity in the land price and neighborhood change studies described above, it is likely that a nation-wide analysis masks the instances where a disproportionate exit does occur, resulting in a null effect. What that study does show, however, is that elevated mobility rates for lower income residents have not been the norm across all cities and station locations. Rodnyansky (2018) drew the same conclusion using tax record data for the city of Los Angeles, California; he found no evidence of a disproportionate exit of low-income residents in neighborhoods surrounding transit stations.

One argument for some of the null effects of transit on neighborhoods or residential movements could be made in the context of location efficiency, or the combined transportation and housing costs of residing in a particular place. Several studies have reported on lower transportation costs associated with living near transit—this reduction could theoretically offset any increases in rents, enabling residents to stay in place (Hamidi et al., 2016; Renne, Tolford, Hamidi, & Ewing, 2016).

The paucity of disaggregate studies on transit's role in gentrification and displacement calls for additional research. The residential mobility and tax data sets used thus far are subject to the same criticism facing quantitative analyses of displacement in the gentrification literature, which has similarly been unable to statistically capture displacement effects (Rayle, 2015): they were not designed to study displacement and simplify this complex process into a single movement in time.

Gentrification and Evictions

As an alternative to residential mobility data, the use of eviction data to measure displacement has received some attention in the literature. Sims (2016) makes the case that spatially analyzing rates of evictions across an urban area can reveal locations with higher than expected rates, which may be indicative of places undergoing restructuring housing and labor markets, or strategic actions of landlords. This serves to deemphasize the behaviors of residents relative to the underlying structural forces generating these spatial patterns. Given that transit investments are a place-based tool often used to produce economic development, and are consequently charged with causing displacement in areas surrounding stations, this perspective is particularly appealing for studying the transit-induced displacement hypothesis.

However, it is not without limitations. If renters are displaced because of increasing rents, this may occur largely without significant increases in evictions, particularly in cities where eviction judgments are relatively infrequent because of landlord–tenant and eviction law. Furthermore, if renters leave because of increased rents, they are likely to leave once their lease expires rather than exposing themselves to the consequences of eviction. Even eviction filings, rather than forced removal through eviction judgments, can have significant negative consequences for renters in an already difficult search for affordable housing. Many landlords are hesitant or even refuse to rent out their properties to tenants with evictions on their records (Desmond, 2012; Immergluck, Ernsthausen, Earl, & Powell, 2019). Finally, many evictions are handled informally outside of the court process; these are not captured in eviction statistics and thus fully measuring the extent of evictions is a challenge (Desmond, 2016). In short, evictions or eviction filings are not an all-encompassing measure of displacement. However, they provide a measure to study variations in involuntary moving rates within cities.

Spatial analyses on eviction rates have yielded some insights into where rates are generally highest. Comparing metropolitan areas, evictions are higher in those where housing costs have outpaced lower income wages and where competition for housing is greatest (Seymour & Akers, 2019). Within cities, analyses have revealed an enduring occurrence of evictions in high-poverty, largely minority neighborhoods, constituting one of the key reasons that low-income residential mobility rates are perpetually high in these neighborhoods (Desmond, 2012). These neighborhoods possess older housing that is costlier to maintain, and landlords may charge higher rents or evict tenants in anticipation of housing code violations or vacancies (Seymour & Akers, 2020).

Sims (2016) identified four geographies of evictions in Los Angeles between 1994 and 1999. Gentrifying neighborhoods were one, whereas other eviction hot spots occurred in pregentrifying or nongentrifying neighborhoods. These latter three geographies can be understood by invoking processes that generate spatial patterns of urban inequalities, including the influx of foreign capital into housing markets, predatory lending activity, and—importantly for this research—"local development-oriented growth machines," (p. 51) an umbrella term that may encapsulate transit and associated TOD.

Seymour and Akers (2019) linked the practice of purchasing foreclosed or auctioned housing by investors as a pathway to increased concentrations of evictions. With respect to gentrification and evictions in Toronto, Canada, Chum (2015) found that neighborhoods in early stages of gentrification—those undergoing social changes such as increases in artists and more highly educated residents, but without significant increases in income or owner-occupied housing—have the highest eviction rates. Neighborhoods in much later stages of gentrification did not have increased levels of evictions, potentially because the most vulnerable residents had already left.

In sum, although processes of evictions are complex and involve decisions of multiple actors (landlords, tenants, courts, etc.), uncovering a link between new transit developments and increases in evictions may provide a theoretically appealing framework from which to study the transit-induced displacement hypothesis compared with quantitative approaches based solely on residential mobility data.

Data and Methodology

This research uses block-group data from The Eviction Lab at Princeton University.[1] The data include the number of eviction judgments in which renters were ordered to leave in a given block group and year. They only count a single address which received an eviction judgment per year. The data also contain eviction filings in the block group including multiple cases filed against the same address in the same year. From these numbers, an eviction rate is calculated as the ratio of the number of renter-occupied households in an area that received an eviction judgment in which renters were ordered to leave. It also includes an eviction filing rate, calculated as the ratio of the number of evictions filed in an area over the number of renter-occupied homes in that area. The data come merged with socioeconomic and demographic characteristics of the block group based on the 2000 U.S. Census summary file 1 (for 2000–2004 data), the 2009 American Community Survey 5-year estimates (for 2005–2009), and the 2015 American Community Survey 5-year estimates (for 2011–2016).

Although this database is a convenient source of eviction data that are longitudinal and collected for multiple U.S. cities, it nevertheless has some limitations. For instance, as it only captures evictions that have formally worked their way through the court system, it misses many evictions that are settled informally with tenants and landlords. In many cases, residents also vacate their units after the landlord has made an initial filing with the court, prior to a judgment or removal by law enforcement (Desmond, 2016; Immergluck et al., 2019). Compared with locally compiled eviction records for specific cities, the Eviction Lab data set also appears to report lower eviction rates, according to Aiello et al. (2018). Given these possible underestimations of evictions or tenants being forced to leave because of the threat of eviction, we focus this analysis on eviction filing rates as opposed to actual eviction judgments. However, we also estimate our models with the latter data to test the robustness of our results.

Rail station locations and opening year data come from the Center for Transit-Oriented Development (CTOD) and have been supplemented by the authors where gaps were found. We use stations that opened between 2005 and 2011 to observe changes in rates at least 5 years before and after the opening of stations. These include the stations presented in Table 1.[2] Since one of the four cities is missing eviction data for the year 2000, we restrict our analysis to the years 2001–2016.

To estimate the potential impact of the opening of rail transit stations on eviction rates, we apply a quasiexperimental approach that addresses some of the limitations of previous transit-gentrification studies (Padeiro et al., 2019). Specifically, we use a difference-in-differences estimation, which compares changes in an outcome—eviction filing rates—over time between a population that has received a treatment (the opening of a light rail station) and a population that has not (a comparison or control group). To define our treatment—or transit—neighborhoods, we select block groups that intersect a 0.25-mile buffer around a station. The size of the buffer is simply used as a selection tool—the entire block group that intersects any portion of this buffer is the unit of analysis for our treatment cases. The use of block groups as the unit of analysis is partially because this is the smallest geography available from the Eviction Lab. It also more closely approximates neighborhoods than do the larger census tracts more commonly used in studies of neighborhood change (e.g., gentrification) and displacement around transit stations (Baker & Lee, 2019; Delmelle & Nilsson, 2020; Gruve-Cavers & Patterson, 2015; Heilmann, 2018; Kahn, 2007; Nilsson & Delmelle, 2018; Pathak, Wyczalkowski, & Huang, 2017). Often, census tracts are used because of their availability over time, but some studies that examine more recent changes have utilized block groups as the unit of analysis (Bardaka et al., 2018; Dong, 2017; Hess, 2020). It is worth noting that the use of block groups may still mask changes that occur at a more localized scale. Data on smaller geographies such as census blocks are limited to the decennial census.

Since we are investigating potential displacement from transit-induced gentrification, we only include treatment and potential comparison block groups that could be considered gentrifiable. Our operational definition follows that of Freeman (2005), where a neighborhood is considered gentrifiable if it is low income and has previously experienced disinvestment. Unlike Freeman (2005), we do not require a neighborhood to be in the central city to be considered gentrifiable (following Ding, Hwang, & Divringi, 2016). Based on this definition, we include block groups that have a 2000 median household income of less than the median for the metropolitan area in 2000 and that have a share of housing built within the past 20 years that is lower than the share found in the entire metropolitan area.

Table 1. Cities and light rail lines included in the study.

City	System, line	Year opened
Newark, NJ	Newark Light Rail, Broad Street Extension	2006
San Diego, CA	San Diego Trolley, Green line	2005
Seattle, WA	Sound Transit, Central Link	2009
St. Louis, MO	MetroLink extension	2006

Furthermore, neighborhoods that receive rail stations are not selected at random; hence, we need to be careful in selecting a comparison group to avoid causing bias in the estimated effect of the treatment (Billings, 2011; Gertler, Martinez, Premand, Rawlings, & Vermeersch, 2016; Heilmann, 2018). We use propensity score matching to identify neighborhoods that had a similar probability of receiving a light rail transit station, but did not (Dong, 2017; Pathak et al., 2017). Other approaches used to select control neighborhoods for studies regarding social and economic impacts of transit stations have involved selecting neighborhoods along planned (but not implemented) lines (Billings, 2011; Heilmann, 2018) and distance-based approaches (Bardaka et al., 2018; Pathak et al., 2017). The combination of using difference-in-differences estimations and propensity score matching allows us to achieve similarity in observed and at least time-invariant unobserved characteristics (Gertler et al., 2016; Thoemmes & Kim, 2011). However, there is also a risk of introducing bias by using matched difference-in-differences approaches, especially when matching is performed on pretreatment levels of the outcome variable and/or on time-varying covariates with low serial correlation (Daw & Hatfield, 2018).

For matching, we use block-group characteristics from the 2000 census summary file 1 data. To minimize potential bias, we do not include the outcome variable in the matching procedure. We include time-variant characteristics, however, as neighborhoods are generally slow to change (Nilsson & Delmelle, 2018; Wei & Knox, 2014), and so their characteristics tend to have strong serial correlation. Similar to Pathak et al. (2017), we include a range of variables in the matching procedure: population density, distance to the city center, median household income, poverty rate, racial composition, median home value, share of renter-occupied housing units, median gross rent, and rent burden. Block groups within a quarter-mile of a new or existing rail transit station are excluded from the potential set of matches, as are first-order, or adjacent, neighbors of treatment block groups, to avoid potential spatial spillovers.

Although the above helps us find control neighborhoods that are similar to the treatment neighborhoods in terms of housing and demographic and socioeconomic characteristics, we also need to consider what factors are important in shaping where transit stations are located. Hence, for each city we run a stepwise logistic regression[3] to find the characteristics most influential in predicting the likelihood of a neighborhood being treated (i.e., receiving a station), and use these in the matching procedure. We also restrict matches to be within the same counties as the treated neighborhoods to control for differences in local housing programs and eviction processes. However, in some cases, the balance of treatment and control block groups is uneven between neighboring counties (e.g., see Newark and St. Louis in Figure 1) which may affect the results if eviction processes in the two counties are vastly different. An optimal matching algorithm with a one-to-one matching ratio is applied to each city (or the county or counties within which the city and the light rail line reside). Optimal matching algorithms find the matched samples with the smallest average absolute distance across all matched pairs, compared with greedy algorithms that minimize the distance within each matched pair without minimizing the total distance within matched pairs. Although the two approaches generally produce the same set of matched samples, optimal matching is sometimes marginally better in producing closely matched pairs (Gu & Rosenbaum, 1993; Ho, Imai, King, & Stuart, 2011).

The resulting set of control neighborhoods (or block groups) from the one-to-one matching procedure is shown in Figure 1, together with treatment neighborhoods and stations included in the analysis, for each of the four study areas. Note that not all block groups within a quarter-mile of the light rail stations are included, only those that meet the gentrifiable criteria outline earlier. The control block groups are their matched comparisons.

With data on the treatment and control groups identified above, we estimate the following difference-in-difference model by city:

$$ER_{it} = \alpha + \beta_1 Treat_i + \beta_2 Post_t + \beta_3 Treat_i \times Post_t + \gamma X_t + \varepsilon_{it} \qquad (1)$$

where i denotes the neighborhood (proxied by block groups) and t the year. The dependent variable, ER_{it}, is the neighborhood eviction filing rate. $Treat_i$ and $Post_t$ are included to control for initial between-group differences and time period differences, respectively. Our coefficient of interest, the difference-in-difference estimator β_3, is given by the interaction between $Treat_i$ and $Post_t$. It measures whether

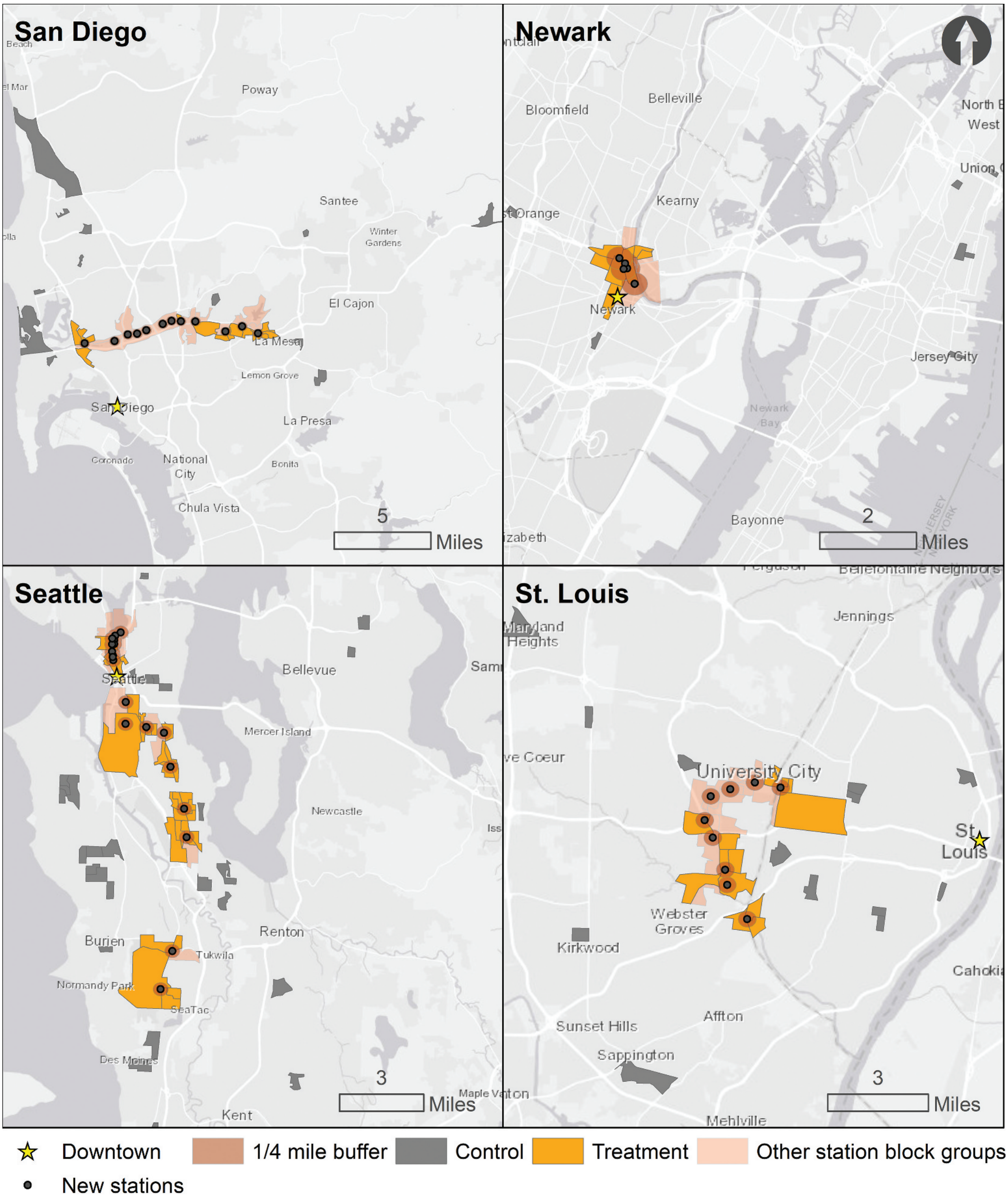

Figure 1. Study areas.

neighborhoods that received treatment experienced significantly higher or lower eviction filing rates after the opening of the rail transit stations compared with the neighborhoods in the control group. Finally, X_t is a vector of block-group fixed effects to control for neighborhood unobservables.

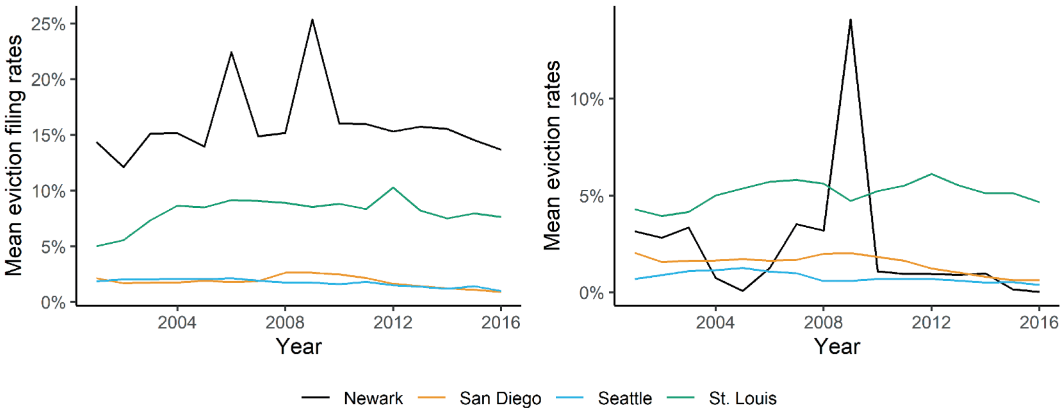

Figure 2. Mean eviction filing rate by treatment versus control block groups.

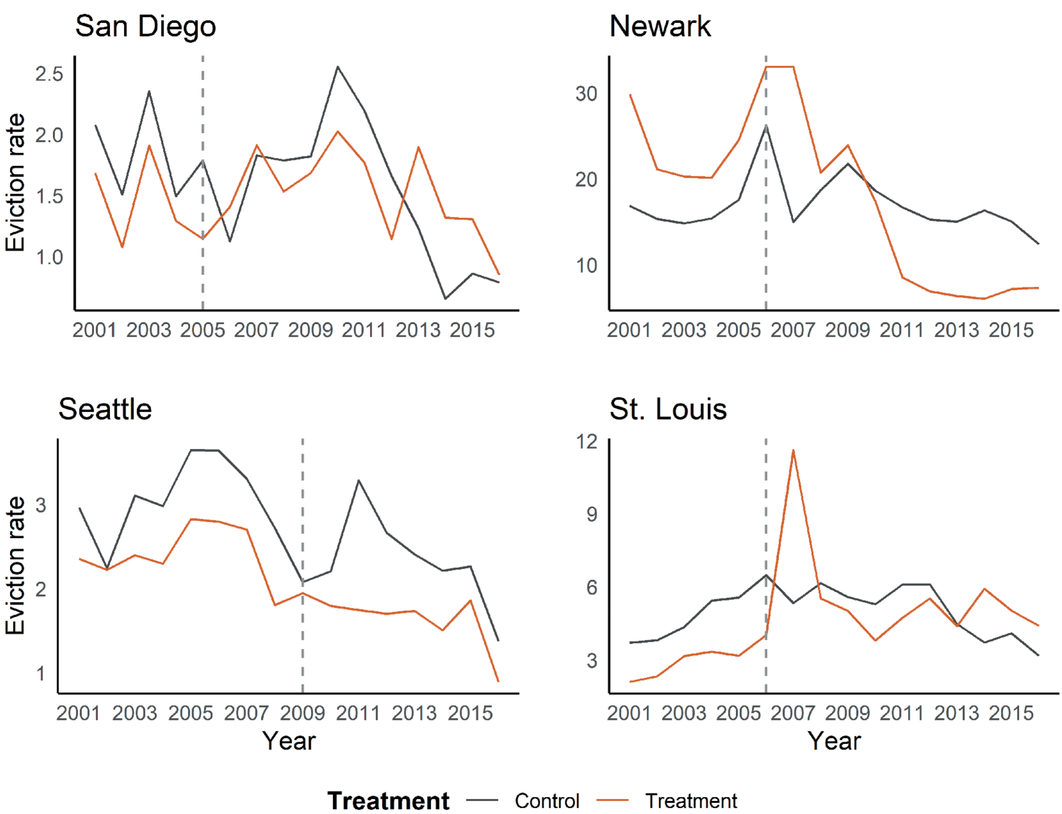

Figure 3. Eviction filing and eviction judgment rates across cities.

Results

Eviction Trends in Case Study Cities

To place the results of our comparison between transit and control gentrifiable neighborhoods in context, we begin with an overview of general eviction trends across the four selected case study cities during our study time period. The graphs in Figure 2 show the mean eviction filing rates and eviction rates for the four cities. Newark stands out for its high filing rates and for its very sharp peak in eviction rates following the housing market crash in 2008 and 2009. This city has the second highest share of renters in the country (75% as of 2017) and was hit especially hard by predatory lenders pursuing subprime opportunities in high-poverty, minority neighborhoods (Troutt, 2017). This led to a high rate of foreclosures following the housing market crash and may help to explain the spikes in Figure 2. St. Louis has the second-highest rate of evictions of the four cities. It was similarly hit hard by foreclosures in the wake of the Great Recession which increased the number of renters and rental properties (Moskop & Cambria, 2016). Although the Protecting Tenants at Foreclosure Act of 2009 (Public Law 111–22, 2009) could explain some of the drop in Newark in 2010, St. Louis foreclosures continued to increase until 2012.

Difference-in-Differences Modeling Results

We now turn to the model results to compare eviction trends in treatment (transit) neighborhoods with a set of controls. Table 2 shows the characteristics of the treatment and control groups in the four cities. Overall, the characteristics of the two groups are similar within each city as a result of the matching procedure, although there are some minor differences in racial makeup in San Diego and Seattle.

Although similarity across a wide range of socioeconomic, demographic, and housing characteristics in the preperiod is important, it is not required for controls to constitute a valid counterfactual (Gertler et al., 2016). The assumption of parallel (or equal) trends in the preperiod is required. We test the validity of the parallel trend assumption using two methods. First, we plot the annual changes in eviction rates for the treatment and control group across the entire period, with a focus on the period before the light rail line opened (indicated by the dashed vertical lines in Figure 3). Although the pretrends generally follow similar patterns, it is difficult to assess by visual inspection the significance of minor differences across years.

Therefore, we perform an additional test, applied by Canales, Nilsson, and Delmelle (2019), Galiani, Gertler, and Schargrodsky (2005), and Wan, Ha, Yoshida, and Zhang (2016), in which we estimate the model in Equation (1) without the post, treatment, and interact terms but with neighborhood and year fixed effects, using only observations from the pretreatment time period. This reduced model is estimated for the treatment and control group separately. We then perform a significance test on the difference between the estimated coefficients of the year fixed effects (using 2001 as the reference year). No statistically significant difference is detected between the year fixed effects from the treatment and control groups in any of the cities (see Table 3). That is, we find no evidence of a divergence in trends between the treatment and control groups in the years before the light rail opened in each respective city. This is true even for San Diego and Seattle, where visual inspection shows some divergence in trends the year before the opening of the stations in each city (see Figure 3).

Having satisfied the parallel trends assumption, we move on to the results of the differences-in-differences model described in Equation (1). The results, shown in Table 4, suggest some differences in eviction filing rates between the treatment and control groups, on average, as indicated by the significance of the *treatment* coefficient in all four cities. Newark experienced a significant average increase in eviction filing rates in both groups in the time period after the light rail line opened. This is to be expected given the aforementioned city-wide trends and the spike in evictions during that time (see Figure 2).

Seattle saw significant declines in evictions in both treatment and control groups after the opening of its new transit line in 2009, the continuation of a downward trend that had started a couple of years before. The only city that experienced a significant change in eviction filing rates in the light rail neighborhoods post opening was St. Louis. However, after the 1-year dramatic increase in 2007, eviction filing rates fell again to continue following similar rates to those in the control

Table 2. Mean characteristics in 2000 by treatment versus control block groups.

	Mean (Standard error)		
	Treatment	Control	Difference-in-means
San Diego			
Population density (1,000/sq mile)	8.92 (1.49)	7.43 (1.84)	1.49
Median household income ($1,000)	35.34 (1.30)	37.15 (1.58)	− 1.80
Median home value ($1,000)	207.61 (21.56)	225.70 (19.74)	− 18.09
Poverty rate (%)	19.51 (2.07)	17.89 (2.90)	1.61
Renters (%)	58.69 (4.59)	48.97 (7.15)	9.72
Rent ($)	794.61 (32.98)	898.11 (60.86)	− 103.49
Rent burden (%)	35.52 (2.48)	31.66 (2.48)	3.85
Black (%)	4.34 (0.65)	2.00 (0.84)	2.34**
White (%)	68.70 (1.84)	78.53 (3.05)	− 9.67**
Hispanic (%)	15.96 (0.97)	9.92 (1.26)	6.04***
Newark			
Population density (1,000/sq mile)	17.79 (5.54)	31.53 (7.91)	− 13.74
Median household income ($1,000)	31.02 (5.45)	23.67 (4.05)	7.35
Median home value ($1,000)	60.55 (23.24)	66.07 (31.30)	− 5.51
Poverty rate (%)	35.16 (6.90)	31.81 (4.59)	3.34
Renters (%)	82.57 (5.97)	89.17 (2.56)	− 6.59
Rent ($)	572.57 (74.29)	565.00 (52.19)	7.57
Rent burden (%)	24.99 (2.68)	33.92 (2.99)	− 8.93**
Black (%)	45.02 (11.93)	20.19 (12.16)	24.82
White (%)	19.48 (6.41)	27.45 (9.48)	− 7.97
Hispanic (%)	25.61 (6.13)	48.27 (12.00)	− 22.65
Seattle			
Population density (1,000/sq mile)	8.89 (1.36)	10.15 (1.13)	− 1.26
Median household income ($1,000)	33.14 (1.9)	32.01 (1.91)	1.12
Median home value ($1,000)	141.24 (13.22)	141.52 (13.01)	− 0.28
Poverty rate (%)	20.59 (2.22)	23.08 (2.47)	− 2.49
Renters (%)	59.42 (4.81)	66.01 (4.18)	− 6.58
Rent ($)	591.88 (34.38)	622.11 (32.72)	− 30.23
Rent burden (%)	28.64 (1.31)	29.85 (0.89)	− 1.21
Black (%)	18.50 (1.68)	15.86 (2.47)	2.64
White (%)	30.66 (4.16)	41.95 (2.90)	− 11.28**
Hispanic (%)	9.31 (1.06)	11.76 (1.22)	− 2.44
St. Louis			
Population density (1,000/sq mile)	5.71 (1.26)	7.32 (0.99)	− 1.60
Median household income ($1,000)	31.14 (3.38)	32.09 (2.39)	− 0.94
Median home value ($1,000)	107.56 (21.13)	126.39 (22.02)	− 18.82
Poverty rate (%)	14.01 (3.18)	16.47 (3.04)	− 2.45
Renters (%)	44.98 (6.34)	56.76 (5.66)	− 11.77
Rent ($)	482.92 (46.10)	525.84 (37.85)	− 42.92
Rent burden (%)	23.44 (2.65)	25.07 (1.38)	− 1.63
Black (%)	17.32 (5.27)	18.57 (5.30)	− 1.25
White (%)	65.79 (7.45)	70.93 (6.07)	− 5.14
Hispanic (%)	1.85 (0.31)	1.80 (0.27)	0.05

Note. ***, **, and * indicate statistical significance at the $p < .01$, $p < .05$, and $p < .10$ significance level..

neighborhoods. Both the treatment and control neighborhoods in this city were majority White and comprised close to 50% renters (lower than compared with Seattle and Newark) in the year 2000 (see Table 2).

The between-group estimator, *treatment*, is significant for the case of St. Louis, suggesting that there were, on average, higher eviction filing rates in the treatment neighborhoods. One plausible explanation is the spike in rates in 2007 that can be observed in Figure 3, which shows elevated rates for both the treatment and control neighborhoods, but a much more dramatic spike for the transit neighborhoods. Once this peak dropped, transit neighborhoods had much lower rates of evictions compared with the control group. This, of course, raises the question why transit neighborhoods saw such a sharp spike in evictions in 2007, compared with the controls. The spike in evictions occurs just after the introduction of the transit system, but also while the housing market bubble was peaking, leading to its crash. Although vacancy rates increased and rents decreased during the Great

Table 3. Test for difference in pretreatment year fixed effects (reference year = 2001).

Year dummy	Treatment	Control	*p* value
San Diego			
2002	− 0.6085 (0.4507)	− 0.5728 (1.0222)	.9745
2003	0.2242 (0.6893)	0.2757 (1.2565)	.9714
2004	− 0.3935 (0.4813)	− 0.5857 (1.0277)	.8656
Newark			
2002	− 8.7228 (11.9250)	1.5242 (7.2261)	.6057
2003	− 9.5842 (10.3885)	− 2.0442 (5.6845)	.5243
2004	− 9.6914 (10.7553)	− 1.4771 (6.5351)	.5140
2005	− 4.3085 (13.3489)	0.6828 (7.4087)	.6947
Seattle			
2002	− 0.1296 (0.4330)	− 0.7196 (0.4455)	.3423
2003	0.0425 (0.4189)	0.1400 (0.4901)	.8798
2004	− 0.0568 (0.4903)	0.0168 (0.5818)	.9228
2005	0.4715 (0.4560)	0.6803 (0.8110)	.8225
2006	0.4409 (0.5401)	0.6715 (0.6771)	.7900
2007	0.3446 (0.5055)	0.3393 (0.6141)	.9947
2008	− 0.5462 (0.4702)	− 0.2431 (0.5179)	.6648
St. Louis			
2002	0.2323 (0.6030)	0.1023 (1.1070)	.9179
2003	1.0646 (0.7301)	0.6453 (1.2251)	.7688
2004	1.2400 (0.8092)	1.7269 (1.0828)	.7187
2005	1.0661 (0.7621)	1.8523 (1.2964)	.6012

Note. Standard errors are given in parentheses.

Table 4. Difference-in-differences estimation results.

	San Diego	Newark	Seattle	St. Louis
Treatment	− 2.2835***	20.9660***	2.9351***	− 4.6694*
	(0.4970)	(4.3646)	(0.7251)	(2.6166)
Post	− 0.1353	4.2069**	− 0.7756***	0.6593
	(0.1960)	(1.9326)	(0.1521)	(0.8423)
Treatment × Post	0.1436	− 1.0363	0.0274	2.1643*
	(0.2760)	(2.8066)	(0.2084)	(1.2191)
Neighborhood Fixed Effects (FEs)	Yes	Yes	Yes	Yes
N	442	185	963	378
Adjusted R^2	0.57	0.77	0.48	0.20

Note. Standard errors are given in parentheses.
***, **, and * indicate statistical significance at the $p < .01$, $p < .05$, and $p < .10$ significance level..

Recession (Joint Center for Hosuing Studies, 2011), many lost their jobs and fell into economic hardship, which could explain the peaks in 2007–2008 in harder hit cities such as St. Louis and Newark. Generally, across both groups in all four cities, eviction filing rates fell toward the end of the study period, when the U.S. economy had mostly recovered.

In summary, across this four-city sample, we do not find strong evidence of a significant effect from the opening of a transit station in a neighborhood on neighborhood eviction filing rates, as the difference-in-differences estimator, *treatment* × *post*, is insignificant for three out of four cities and only significant at the 10% significance level in St. Louis. We also ran the model with the actual eviction (judgment) rate as the dependent variable. The results remain qualitatively the same, with only some weak evidence (again at the 10% significance level) of a similar magnitude in St. Louis.[4]

Discussion and Conclusions

Quantifying the effects of transit on neighborhood gentrification and displacement has proved challenging despite a considerable amount of rhetoric surrounding the subject. Like the gentrification literature more broadly, studies on this relationship have been subject to criticism: the neighborhood-scale gentrification analyses have not consistently utilized a set of control neighborhoods to compare transit trends with (Padeiro et al., 2019), and residential mobility studies have been scarce and use data that was not designed for studying displacement (Rayle, 2015). In this analysis, we used eviction data to determine whether neighborhoods (proxied by census block groups) near new transit stations in four U.S. cities (Newark, San Diego, Seattle, St. Louis) experienced elevated eviction filing rates after the station opened compared with a set of similar neighborhoods in each city that were not close to the transit station. We found very minimal evidence that eviction filing, or the actual eviction judgment rate, was higher following the opening of a new station; this was only significant in the case of St. Louis, where transit neighborhoods underwent a significant spike in evictions just as the new transit lined opened. However, this time period also corresponded to the financial crisis of 2007–2008. Following this brief spike, eviction levels fell again and remained at around the same rates as in the control neighborhoods. Unfortunately, this analysis is unable to disentangle the role that transit played in elevating eviction rates in these neighborhoods from impacts of the financial crisis.

Setting aside the St. Louis case, Newark, San Diego, and Seattle did not experience significant increases in evictions or eviction filings in gentrifiable neighborhoods near new transit stations compared with similar gentrifiable neighborhoods elsewhere in the city. The lack of significant findings adds to a growing body of literature that has been unable to quantify the expected impact of transit on surrounding neighborhoods, gentrification, and residential mobility. This is not to say that transit-induced gentrification and displacement never occur, but the evidence increasingly paints a more nuanced picture of this relationship than the popular discourse on the subject suggests. From a policy perspective, this suggests that the benefits of transit may outweigh the gentrification fears that have, in some cities, been used as a rationale for protesting plans (Allison, 2017; Rayle, 2015). Other studies have emphasized that gentrification pressures stemming from transit may be more acute when coupled with other amenities that make a neighborhood prime for gentrification to begin with (Delmelle, Nilsson, & Schuch, 2020), but that the impacts of transit alone are unlikely to transform a neighborhood. The contextual circumstances need to be emphasized when developing specific policy recommendations for each place; there is likely no one-size-fits-all policy applicable across all cities and station locations, given varying housing market pressures.

Eviction data have theoretical advantages compared with residential mobility or tax record data, which have been used previously to test the transit-induced displacement hypothesis; however, they too suffer from multiple limitations as an indicator of displacement. Although block group-level data are more detailed than the commonly used census tract data to study gentrification and displacement, this geography may mask more localized changes and be too large to detect effects. Furthermore, given the limited availability of eviction data dating back further in time, we are also unable to capture potential announcement effects – that is, house price capitalization that may be occurring prior to opening of the stations in certain neighborhoods because of speculations made by investors following the announcement of the rail transit investment (Billings, 2011). In addition, effects might differ depending on station type, such as walk-n-ride/TOD versus park-n-ride/transit-adjacent developments, as shown in related studies (e.g., Nilsson and Delmelle, 2018). Unfortunately, we did not have enough observations per city to run the analysis by type of station. Other limitations, perhaps better suited for in-depth case studies focusing on a single city, include controlling for share of public housing in both treatment and control neighborhoods, job density, and more political and social variables such as the presence and strength of community-based organizations.

Finally, evictions represent one small instance of displacement effects that may be felt by residents. New transit neighborhoods with their associated newly built developments may not physically displace residents if they were constructed on underutilized or vacant land, but they may represent exclusionary forms of displacement if they are priced above what existing residents could afford to pay. They also bring about changes in demographics which may lead to a loss-of-place feeling. These more experiential forms of displacement (Atkinson, 2015; Elliott-Cooper, Hubbard, & Lees, 2019) move beyond Marcuse's (1985) monetarily oriented forms of displacement and remain an aspect often overlooked in quantitative studies. Even if low-income residents are able to stay in a neighborhood, the influx of newer, higher income residents may result in the loss of political power and decision-making or a shift in the behaviors, norms, and values of the neighborhood (Hyra, 2015). Therefore, this analysis does not claim that displacement does not occur in new transit neighborhoods, but rather, that rates of evictions and eviction filings do not differ significantly across similar neighborhoods in the same city.

One hypothesis for the lack of evidence for elevated rates of gentrification and displacement around new transit stations in the literature is the notion that increasing housing costs are offset by decreases in transportation costs, also referred to as location efficiency (Hamidi et al., 2016; Renne et al., 2016). This is likely a more plausible scenario in cities with well-connected transit systems compared with those that open a single light rail line in an otherwise auto-dominated city, but it nonetheless deserves further scrutiny for its role in explaining the limited impacts of transit revealed in the literature thus far.

Notes

1. A project directed by Matthew Desmond and designed by Ashley Gromis, Lavar Edmonds, James Hendrickson, Katie Krywokulski, Lillian Leung, and Adam Porton. The Eviction Lab is funded by the JPB, Gates, and Ford Foundations as well as the Chan Zuckerberg Initiative. More information can be found at evictionlab.org
2. Several lines that opened between 2005 and 2011 are not included in the analysis. These include the Blue Line opened in Charlotte, North Carolina, in 2007, because of missing evictions data prior to 2004; the MAX Green Line in Portland, Oregon, which opened in 2009, because of too much overlap with existing stations; the Valley Metro Rail opened in Phoenix, Arizona, in 2008, because of missing evictions data post2005; and Santa Clara Valley Transportation Authority's Vasona line in San Jose, California, opened in 2005, because of missing evictions data between 2002 and 2013.
3. For this step, we use backward selection for our stepwise regression—meaning we start with a predictive model containing all the variables used for the matching procedure as predictors of receiving a light rail station (a dummy variable coded 1 if considered a station neighborhood). Then the algorithm iteratively eliminates predictors one at a time, at each step considering whether the model selection criterion Bayesian Information Criterion (BIC) will be improved by adding back in a variable removed at a previous step. The resulting model contains a subset of predictors that are most contributive in predicting the outcome variable (i.e., receiving a light rail station; RDocumentation (2020) and references therein).
4. These results are available from the authors upon request.

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