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Bicycle Infrastructure and Traffic Congestion: Evidence from DC's Capital Bikeshare

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Abstract

This study explores the impact of bicycle-sharing infrastructure on urban transportation. We estimate a causal effect of the Capital Bikeshare on traffic congestion in the metropolitan Washington, D.C., area. We exploit a unique traffic dataset that is finely defined on a spatial and temporal scale. Our approach examines within-city commuting decisions as opposed to traffic patterns on major thruways. Empirical results suggest that the availability of a bikeshare reduces traffic congestion upwards of 4% within a neighborhood. In addition, we estimate heterogeneous treatment effects using panel quantile regression. Results indicate that the congestion-reducing impact of bikeshares is concentrated in highly congested areas.

JEL Codes: L91, H40, Q53, R41, R53
Keywords: Traffic congestion, public transportation, bicycle-sharing, pollution, automobile externalities

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1 Introduction

Tailpipe emissions from transportation constitute 27% of greenhouse gas emissions in the United States.[1] The effect of automobile pollution is amplified further by increases in congestion in urban areas, which exacerbate both private and public damages. Schrank et al. (2012) estimate national congestion costs arising from time loss and wasted fuel at more than \$120 billion in 2011, while annual CO_2 emissions attributable strictly to congestion are 56 billion pounds. In addition, 56 billion pounds of CO_2 emissions translates to over \$1 trillion in social costs.[2]

In response to these concerns, government agencies have imposed highway tolls, built high-occupancy vehicle lanes, invested in public transit infrastructure, imposed fuel economy standards, and relied on voluntary information campaigns in an effort to reduce vehicle miles traveled (VMTs), alleviate congestion, and mitigate the associated environmental damages. A new mechanism to reduce urban traffic congestion that is currently gaining traction for its purported cost-effectiveness, environmental-friendliness, and positive health impacts is the adoption of citywide bicycle-sharing systems (bikeshares). This infrastructure provides an alternative to driving for short trips and extends the existing network of public transit within a metropolitan area. Further, bicycling infrastructure augments the environmental bona fides of densely populated urban areas (Kahn, 2010). If bikeshares reduce traffic congestion, they may provide a low-cost policy lever to reduce automobile externalities in urban areas.

Bicycle-sharing programs have seen substantial uptake in European cities such as Amsterdam, Paris, Copenhagen, and London, but only recently have U.S. cities adopted these transportation systems (Nair et al., 2013).[3] Of note, Washington, D.C., Minneapolis-St. Paul, Boston, San Francisco, and New York City have installed city-wide bikeshares. It is thus worth examining the policy importance of environmental benefits of a bicycle-sharing program in urban areas. Specifically, we focus on metropolitan Washington, D.C.'s, Capital Bikeshare, which was introduced in 2010. Schrank et al. (2012) show that the Washington area ranks first in pounds per automotive commuter for CO_2 emissions produced during congested travel, at 631 pounds per commuter annually. This estimate, of course, ignores any local pollutants that contribute to ambient air quality. Further, a journalist at the *Washington Post* notes, "Capital Bikeshare ... funded the original bikes and the docking stations

[1] http://www.epa.gov/climatechange/ghgemissions/sources.html.

[2] These estimates are the emissions arising solely from congested travel, as opposed to free-flowing traffic.

[3] A typical bikeshare system works as follows. A user registers for an annual, multiday, or (infra-)day membership at one of many bikeshare "stations," which house the bicycles not in use. The user is then given a key, physical or numerical, to unlock a bicycle for transportation and she is allowed to return it to any other station within the bikeshare system. The user pays according to a rate structure based on the elapsed time of the trip.

with federal grants earmarked for local programs that mitigate congestion and improve air quality."[4] In particular, Capital Bikeshare leveraged federal money through the Congestion Mitigation and Air Quality Improvement Program (CMAQ), which funds programs in air quality nonattainment areas for ozone, carbon monoxide, and particulate matter that reduce congestion related emissions.[5] In order to assess these environmental benefits, however, a causal link between bikeshares and traffic congestion must be established; identifying this effect is the focus of our paper.

Specifically, we examine the impacts of introducing additional transportation options into an existing transit network within a large metropolitan area. Focusing on the introduction of the Capital Bikeshare program, we examine the effects of bikeshare station locations on traffic congestion. The expansion of the bikeshare program over 2011 and 2012 allows for identification of changes in congestion within a neighborhood over time. Bikeshare station locations are matched with micro-level traffic data on a finely defined spatial and temporal scale within the city. These data provide an advantageous approach to examining within-city commuting decisions as opposed to examining changes in traffic patterns on major thruways and arterial highways. To the best of our knowledge, this is the first paper in the economics literature to examine the causal effect of large-scale bicycle-sharing infrastructure on motor vehicle traffic, with implications for environmental and health benefits.

Empirically, we develop a framework to capture the effect of bikeshare systems on traffic congestion in multiple ways. Fixed effects models controlling for time-invariant unobservables in a neighborhood allow us to explore the effect of the bikeshare systems on traffic congestion, while highlighting the bias from endogenous selection of bikeshare locations. Our causal specifications, using the presence of a bikeshare station as a treatment, utilize propensity score matching on observable socioeconomic characteristics, pre-treatment traffic congestion, and public transportation infrastructure to mitigate the effect of selection bias. Estimates from our preferred models indicate a 4% reduction in congestion due to the presence of a bikeshare, which translates to roughly $24 million in private and $850,000 in public benefits within our sample. Further analysis explores heterogeneity in the impact of bikeshare stations through the use of quantile regression. Our results suggest that the reduction in congestion is concentrated in areas with relatively high congestion.

In the next section, we provide a brief history of bikeshare programs and institutional details of the Capital Bikeshare program. We then outline the relevant literature as it

[4]Badger, Emily. "Why DC's bikeshare is flourishing while New York's is financially struggling." *The Washington Post*, 1 April 2014. http://www.washingtonpost.com/blogs/wonkblog/wp/2014/04/01/why-dcs-bikeshare-is-flourishing-while-new-yorks-is-financially-struggling/. Last Accessed: October 13th, 2014.

[5]Fact Sheets on Highway Provisions, http://www.fhwa.dot.gov/safetealu/factsheets/cmaq.htm. Last Accessed: October 13, 2014.

relates to traffic congestion and environmental quality. In Section 4, we describe the data. We present our quasi-experimental strategy and empirical models in Section 5 and discuss matching and estimation results in Sections 6 and 7, respectively. Policy implications are discussed in Section 8, followed by robustness checks in Section 9 and conclusions in the final section.

2 Institutional background on bikesharing

Bikesharing programs allow members to check out bicycles from stations located in public spaces, and return them to other stations when their ride is complete. Modern systems generally require members to purchase a membership for a specified time (e.g., a daily or annual membership). Members use a key to unlock bicycles at any station, and they can return them to an empty dock at a station near their end destination. Rides that last less than a given amount of time (typically 30 minutes) are free, while overage fees are incurred for longer trips.

Bikesharing systems continue to grow rapidly in North America and are providing new transportation opportunities for residents and visitors in major cities (Martin and Shaheen, 2014). Bikesharing systems are meant to encourage short to medium distance rides, ideally complementing existing public transit, providing an alternative to walking to and from a major transit center, or linking two routes that do not overlap (Pucher, 2005). Shaheen (2012) proposes some potential benefits of bikesharing including increased mobility, consumer transportation cost savings, reduced transportation infrastructure costs, reduced traffic congestion, reduced fuel use, increased use of public transit (Martin and Shaheen, 2014), public health improvements, and greater environmental awareness.

The Washington D.C. Capital Bikeshare program was introduced in the fall of 2010, beginning with 400 bicycles and 49 stations and quickly growing to over 100 stations and more than 1,000 bicycles by the end of 2010. The growth in ridership, shown in Figure 1 relative to the trends in the number of stations in use between 2010 and 2012, reflects the overall increase in cycling as a transport mode in the metropolitan D.C. area. According to the 2012 American Community Survey, the share of bicycle commuters in D.C. in 2012 was 4.1%, up from 3.1% in 2010 and 1.2% in 2000.

Between 2010 and 2012, the first 3 years of the program, the Capital Bikeshare expanded dramatically. Figure 1 shows that the number of stations in use each month increased from 104 to nearly 190 at the end of 2012 and ridership increased by almost 100,000 trips between the peak in summer 2011 and summer 2012. The expansion in stations over this time period allowed the Capital Bikeshare system to double the number of bicycles in service, from 82 stations in October 2010 to 168 in October 2012. At its monthly peak, in September 2012, all users spent a combined 65,000 hours on Capital Bikeshare bicycles. In addition, Figure 2 indicates that the amount of bike lanes in the D.C. metropolitan area remained fairly constant over the time of our study so we are able to concentrate on the bikeshare program itself, rather than impact of changes in accompanying infrastructure.

3 The Economics of Traffic Congestion

Recently, the economics literature on transportation has focused on motor vehicle drivers' behavior. Particularly, research has characterized consumer responses to changes in the price of gasoline as well as the incidence and distributional implications of taxing gasoline to curb its negative externalities (Bento et al., 2009; West and Williams III, 2005, 2007). This research informs the policy-relevant debate concerning the optimal mechanism (e.g., Pigouvian taxation of gasoline) to reduce environmental damages that arise from motor vehicles. In addition, researchers have examined how variability in gasoline prices affects speed and congestion on freeways (Burger and Kaffine, 2009), carpooling behavior (Bento et al., 2013), and the substitutability of other modes of public transportation (Currie and Phung, 2007; Spiller et al., 2012). While this research sheds light on consumers' decision-making process within the existing transportation infrastructure for motor vehicles, it leaves open the question of how consumer behavior changes with the addition of a new, purportedly environmentally friendly, transportation option.

The effect of adding a bikeshare network to an existing transit system can be examined similarly to the introduction of a rail line, for example, as it effectively reduces the relative cost of transportation. This additional transportation option is particularly relevant in urban areas where public transit is more cost-effective and may, in the short run, circumvent the fundamental law of road congestion (Duranton and Turner, 2011), which posits that VMTs increase proportionally with additional vehicle lanes and exhibit no response from additional public transit service. Among research at the intersection of motor vehicle traffic and public transit, no studies have provided an estimate of the impact of bicycle-sharing programs on traffic congestion nor their ability to augment existing public transit in an urban area.

A parallel literature explores the investment in public transit infrastructure and finds that despite garnering a large fraction of public support, only a small number of commuters actually use public transportation. As such, several studies have concluded that investment in public transit does little to reduce traffic congestion and thus fails to reap the corresponding environmental benefits (Rubin et al., 1999; Winston and Langer, 2006; Winston and Maheshri, 2007; Duranton and Turner, 2011; Lin Lawell et al., 2016; Beaudoin and Lawell, 2016). Specifically, Beaudoin and Lawell (2016) find no evidence that increased transit supply improves air quality at the margin, conditional on existing urban travel regulations. Still, Beaudoin et al. (2014) provide evidence that a 10% increase in public transit capacity reduces traffic congestion by 0.8%, though this effect is stronger in densely populated cities. Further, Anderson (2014) shows that commuters most likely to support public transit are those who would otherwise commute along highly congested motorways. Using a transit strike in Southern California as a natural experiment, he finds that average highway travel delays increase by about 0.2 minutes per mile, a 47% increase. The evaluation of a bikeshare system's effect on congestion is particularly relevant within this context as it provides a low-cost alternative to larger capital investment and could increase the efficiency of existing transit options by improving accessibility in a metropolitan area.

The literature on the local environmental effects of congestion is somewhat sparse in comparison to the research on public investment. Parry et al. (2007) provide an extended review of the ways in which motor-vehicle trips produce environmental and public externalities, including the cost-effectiveness of policies designed to curb these externalities. Overall, the analyses summarized in Parry et al. (2007) indicate that the local environmental effects of traffic congestion are substantial. More relevant to our context, Beaudoin et al. (2015) survey the literature on how traffic congestion maps to emissions, while Barth and Boriboonsomsin (2009) and Berechman (2010) show specifically that lower-speed vehicle transportation emits more greenhouse gases. Thus, policies designed to reduce congestion would have the highest marginal impact on greenhouse-gas mitigation from the transportation sector.

Additionally, several papers utilize novel identification strategies to uncover the forces that map transportation policies to environmental and health outcomes. Specifically, Cutter and Neidell (2009) examine the effect of "Spare the Air" information campaigns, in which commuters are asked to voluntarily forgo motor vehicle trips on days when local ambient pollution levels are dangerous. This voluntary mechanism, designed to mitigate the incidence of exposure to ozone in central California, was found to reduce traffic volume and increase public transit usage. Sexton (2012), however, provides an empirical counterpoint to Cutter and Neidell (2009) by examining general equilibrium effects induced by free-fare days for public transportation. Sexton shows that free-fare days increase motor-vehicle traffic, as well as the corresponding local pollution, due to an unintended reduction in relative costs of driving. In contrast, Chen and Whalley (2012) show that the introduction of an urban rail line in Taipei induced a 5 to 10% reduction in carbon monoxide, suggesting a substantial decrease in tailpipe emissions. Finally, Currie and Walker (2011) study the effect of reduced congestion induced by the introduction of E-ZPass on infant mortality and birthweight. They provide convincing evidence of the positive public health spillovers of reduced congestion on local environmental quality.

Overall, research in this vein suggests that a reduction in congestion improves local air quality; however, there may be perverse incentives for commuters that work against the desired social optimality of policy designs. In this paper, we build on this literature by examining an altogether different type of transportation policy with implications for environmental and health outcomes through its effect on traffic congestion. Although support for bicycle-sharing programs often touts environmental benefits, ex ante predictions of the effect of bicycle-sharing programs on traffic congestion are mixed.

4 Data

4.1 Transportation Choice Data

The Capital Bikeshare, which began in September 2010, serves the metropolitan Washington, D.C., area. It is funded in part by the District of Columbia, City of Alexandria, and Arlington County and operated by a private company. In the fall of 2013, Capital Bikeshare expanded to Montgomery County in Maryland. Uptake in ridership and the number of station locations increased substantially over its first three years of operation. Figure 3 shows the number of rides initiated at Capital Bikeshare stations over our study period, demonstrating an obvious increase in ridership, particularly during peak hours.

We are first concerned with the locations of bikeshare stations, analyzing the impact arising from the existence of stations. The geographic locations of these stations are publicly available from Capital Bikeshare. Our data include 165 stations located throughout the metropolitan area. It is important to note that some bikeshare stations were established after the first period of our dataset, generating observations of traffic congestion in the same block group before and after a station is established. In Table III we present the number of stations in operation during different months over the range of our sample as well as the number of treated block groups. Of the block groups that have a bikeshare station established at some point, 29% have a station established after the first time period in our analysis.

A key component of our analysis is access to traffic data that are finely disaggregated on both a spatial and temporal scale. Furthermore, we use observations of city streets and arterial roads, rather than only major highways. INRIX[6] traffic data were obtained through partnership with the CATT Lab at the University of Maryland, College Park. Archived real-

[6]INRIX is a private company that collects information about roadway speeds in real time from anonymous mobile phones, connected cars, trucks, delivery vans, and other fleet vehicles equipped with GPS locator devices.

time information on traffic speed by road segments was obtained through the Vehicle Probe Project (VPP) Suite within the Regional Integrated Transportation 5 Information System (RITIS). The unit of observation for raw speed data is a road segment that is identified by its Traffic Message Channel (TMC) code and exhibits a much richer geographic span than standard traffic monitors that capture flow at various points along major highways. Studies of the quality of INRIX data (e.g., Zhang et al. (2015), Coifman and Kim (2013)) indicate that it is a reliable source for traffic conditions, particularly during times of high traffic volume such as the morning commute.

Each TMC road segment is characterized by the latitudinal and longitudinal coordinates of its start and end points and tends to be less than one-half of a mile, with a mean and median length of 0.37 and 0.24 miles, respectively, in our sample. These road segments cover a more comprehensive range of within-city traffic patterns than was covered in previous research. To manage the spatial nature of our dataset, we construct road midpoints as the geographic midpoint between start and end points. These midpoints define a road observation.

Recall that the primary variable of interest is traffic congestion. We construct a normalized metric of traffic congestion by comparing observed speed to a reference speed (defined by INRIX) that is a typical historical speed for a road segment. Congestion for a particular segment is defined as follows:

$$\mathrm{CONG}_j = \frac{\mathrm{Speed}_j^R}{\mathrm{Speed}_j^O}, \tag{1}$$

where Speed_j^R is a constant reference speed for free-flowing traffic on road segment j determined by the prevailing speed limit and historical speed patterns and Speed_j^O is the observed speed at any point in time. Given this definition, congestion is decreasing in observed speed. We measure congestion using speeds aggregated to 30-minute intervals, focusing on morning rush-hour commuting times from 6:00 am through 10:00 am. In addition, our analysis includes observations from April, May, September, and October of 2011 and 2012, resulting in 1,384 time periods across 2,790 road segments. The start date for our dataset is based on implementation of the bikeshare program and installation of stations. Specific months are chosen to capture times of the year in which cycling is a reasonable commuting option and commuters have typical work schedules. Summary statistics for our dependent variable congestion are shown in Table I.

4.2 Census Block Groups

An important component of our analysis is the spatial nature of the data, as bikeshare stations and speed (and thus congestion) are observed at particular geographic coordinates. It is therefore necessary to establish a geographic link between the variables.

We use U.S. Census block groups as the geographic unit of observation. Census block groups make up the second smallest geographic area defined by the U.S. Census. The smallest designation, census blocks, is unsuitable for the analysis because of our treatment of road segments. Because we use the midpoint of each road as the designation of its location, census blocks are small enough that a road may pass through several blocks but, of course, its midpoint is in only one. This problem is largely alleviated by using a slightly larger geographic designation. In addition, the size of census blocks makes it likely that individuals move across blocks for their commuting choice (e.g., an individual might walk to a bikeshare station in a bordering block or bike through a block that borders that of the usual car commute), so that such a small area may not accurately capture the relationship of transportation mode choices. Our study area includes 305 block groups.

The question becomes one of the relationship between aggregate demand for each mode of transportation in a particular block group. Through the use of GIS software, bikeshare stations are easily linked to the block group in which any particular station is located. For congestion data, we aggregate to the block group level by taking mean congestion for all road segment midpoints located within a particular block group. Figure 4 shows block group boundaries of the study area with road segment midpoints overlaid. Bikeshare stations (as of the final time period of our sample) and Metrorail stations are displayed in the same geographic boundaries in Figure 5. Our sample includes 560,798 observations of block group-time combinations. As shown in Table I, 15.9% of our sample observations are block groups with bikeshare stations.

4.3 Adjacent Block Groups

As an additional component in the empirical analysis, we consider the impact of bike stations that are close to road segments, but perhaps are in different census geographies. Individuals are not confined to a particular census block. An individual may, for example, forgo a car trip and use a bikeshare station in an adjacent block group. It is therefore possible that the impact we are trying to identify is not confined solely to transportation demand within the same block group. In addition, geographic space is continuous and census block groups are somewhat arbitrary to individuals. Thus we are concerned about stations that may be located in different block groups, though they may be very close to the area of measured road congestion. For each block group, we measure the stations that are in neighboring block groups (those that share a border). We also measure the stations that are in neighboring blocks, obtaining a better measure of those stations that are close to measured congestion, but perhaps just across a block group border. Across our sample of block groups and time periods, 48% of our observations have a bikeshare station in a bordering block group, and 25.9% have a bikeshare station in a bordering block.

Table II displays summary statistics for our bikeshare station count measures. The variable $Stations\_Adj_j$ denotes the number of bikeshare stations in all block groups that share a border with block group j. Similarly, $Stations\_Kadj_j$ denotes the number of bikeshare stations in all blocks that share a border with block group j. Summary statistics show that capturing bikeshare stations in adjacent locations could be considerably important. Note also that relatively few observations have multiple stations. While Table II summarizes bikeshare station counts based on observations in the data, it is also worth considering whether block groups had a station at any point over the sample period. Of the 416 block groups, 19.7% have a bikeshare station at some point in time. Alternatively, 56% of block groups have a station within a neighboring block group at some point in time and 31.5% have a station within a neighboring block.[7]

5 Empirical Strategy

Our general empirical approach seeks to identify a causal effect of bikeshare programs on traffic congestion. Existence of a bikeshare station may impact traffic congestion in several ways. Automobile drivers may opt to use the bikeshare to avoid traffic congestion, or any other utility increase associated with biking relative to driving, including potential time savings. A bikeshare option may also extend the existing network of public transportation, again making driving a less attractive option. If all bikeshare users are commuters that have switched from individually driving, one would expect a decrease in congestion as the result of fewer cars on the road. Although, if all bikeshare users previously commuted via the city's rapid transit or bus system, or are simply using it to augment a commute that already takes place via public transportation, one would expect no decrease in automobile traffic. At the same time, it could be the case that additional bikeshare users on the road could interfere with automobile travel and increase traffic congestion, particularly if the increase in riders is due to commuters substituting away from other modes of public transportation.[8]

[7]These percentages are slightly larger than those in Table II since some block groups are treated for only a portion of the dataset.

[8]A 2012 survey of Capital Bikeshare members provides evidence that bikeshare users may not be predominatly substituting from driving, but there is a considerable reduction in miles driven among

Our analysis seeks only to estimate the combined impact of these mechanisms on traffic congestion. Rather than structurally modeling transportation choices, we take a reduced form approach to identifying changes in congestion as a result of the bikeshare program.

In this section, we first develop a series of models controlling for unobservable effects at the block group level to assess the relationship between traffic congestion and the availability of bikeshare stations. Within these models, we note the potential bias induced by nonrandom siting of bikeshare stations. To correct for this, we develop a treatment effect model that eliminates this bias using propensity score matching. Specifically, we use propensity score matching to generate control and treatment samples that can be used with standard panel fixed-effect estimation. Within the latter framework, we estimate an average treatment effect on the treated (ATT) of the bikeshare on congestion. We also explore heterogeneity in the treatment effect using quantile regression while controlling for unobserved spatial and temporal heterogeneity.

5.1 Panel Data Model

To estimate the impact of the bikeshare program on traffic congestion, we take a reduced form approach and estimate several different linear equations. The first general model examines the relationship between the existence of bikeshare stations and the average level of traffic congestion in a block group. We estimate the natural log of traffic congestion as

$$\ln CONG_{jhdmt} = \alpha + \delta_j + \nu_h + \mu_m + \upsilon_t + \gamma Rain_d + \beta Station_{jhdmt} + \varepsilon_{jhdmt}, \tag{2}$$

where $CONG_{jhdmt}$ denotes average congestion among all road segments with midpoints in block group j during half-hour period h, day d, month m, and year t. The variable $Rain_d$ is an indicator variable equal to 1 if precipitation is observed on that day.[9] The parameters δ_j, ν_h, μ_m, and υ_t represent block group, half-hour period, month, and year dummy variables, respectively. The variable $Station_{jhdmt}$ indicates whether a station exists in block group j in time period $\{h, d, m, t\}$. We are therefore primarily interested in the coefficient β.

Two additional specifications each include an additional variable to indicate the presence of a station in an adjacent area. These specifications use the same definition of average congestion as (2) and are otherwise identical except for a single additional variable. In the second specification we include $Station\_Adj_{jhdmt} = 1$ if there is a bikeshare station in an

member of the program. The report can be found at http://www.capitalbikeshare.com/assets/pdf/cabi-2012surveyreport.pdf.

[9]This specification assumes no variation in weather across block groups. In addition, we are unable to obtain historical weather observations at half-hour periods so there is no variation in this variable over the course of a single day.

adjacent census block group. In the third specification, $Station\_Kadj_{jhdmt} = 1$ if there is a bikeshare station in an adjacent census block.

We refer to the above empirical framework, including specifications with variables to indicate nearby stations, as the fixed-effects panel data (FEPD) model.

5.2 Treatment Effect Model

In our empirical framework we recognize the potential selection bias when examining the impact of the existence of bikeshare stations. It is likely the case that stations were established in areas of high congestion for two reasons: these are the locations that are most in need of bikeshare stations from the city's perspective and these are likely the locations with the highest demand for bikeshares. Alternatively, stations require ample sidewalk space and may be best suited to areas of residential and commercial concentration, rather than commuting corridors. In either case, it is well known that endogenous selection of treatment (i.e., bikeshare stations) of this nature will generate biased estimates. Therefore, we use propensity score matching techniques to properly identify the causal impact of bikeshare stations. The following section outlines our approach to and justification for matching.

Define treatment status T to be equal to 1 (treatment) if a block group contains a station and equal to 0 otherwise (untreated). Define the potential outcome[10] in the treatment condition as $CONG^1$ and the potential outcome in the untreated condition as $CONG^0$. The goal of the analysis is to identify the average treatment effect on the treated (ATT),

$$ATT = E[CONG^1 - CONG^0 \mid T = 1]. \tag{3}$$

The econometric strategy discussed in the previous section essentially estimates

$$E[CONG^1 \mid T = 1] - E[CONG^0 \mid T = 0]. \tag{4}$$

Equation (4) is only an unbiased estimator of the ATT if $E[CONG^0 \mid T = 1] = E[CONG^0 \mid T = 0]$, which is unlikely to be true given, for example, the decision to locate bikeshare stations in areas of high congestion. To the extent that this is true, we expect FEPD estimates to be biased upwards.

The matching technique we employ is based on the more plausible assumption that

$$E[CONG^0 \mid X, T = 1] = E[CONG^0 \mid X, T = 0]. \tag{5}$$

[10] In the empirical analysis that follows, we use the natural log of congestion as the outcome variable.

Equation (5) states that observed $E[CONG^0 \mid X, T = 0]$ offers a proper counterfactual for the unobserved $E[CONG^0 \mid X, T = 1]$. By conditioning on the set of variables X, we can create a counterfactual sample of block groups with no stations (untreated) that are observably similar to those that do have a station (treated). In addition to observed variables X as matching covariates, we impose the restriction that no control block groups are adjacent to a treated block group. This final restriction is an effort to avoid a violation of the stable unit treatment value assumption (SUTVA). Rather than matching on the multidimensional set of covariates X, we use propensity scores, $P(X)$, which denotes the predicted probability that a particular block group will have a bikeshare station conditional on X.

Note that we combine the identifying power of the FEPD estimator with that of matching, following the approach of Ho et al. (2007), Ferraro and Miranda (2017), and Wichman and Ferraro (2017).[11] The fixed effect specification alone controls for any congestion shocks that are time-invariant or specific to a particular block group. Our matching strategy becomes important in the context of the implicit assumptions of the model. The econometric framework assumes homogeneous treatment effects and similar time trends across block groups. By matching observations based on block group descriptors and pre-treatment congestion, we make these assumptions more plausible. Thus we use propensity score matching to process the data and generate a set of control observations that are similar to the treated block groups. We then apply the FEPD estimator to the matched samples to obtain unbiased estimates.

6 Pre-Processing Data Using Propensity Score Matching

Based on the potential selection problem in previous specifications, we employ matching techniques in an attempt to obtain unbiased coefficient estimates and identify a causal impact. In the context of our quasi-experimental approach, recall that selection into treatment may occur if the decision to establish a bikeshare station in a particular location is dependent on the level of congestion in that location. Our matching technique therefore seeks to establish a set of control block groups with no bikeshare stations that are similar to those block groups that do have bikeshare stations, so that our analysis has a sound counterfactual. We then estimate linear regressions identical to the specification in Equation (2) on our constructed sample of matched observations.

Our matching approach rests on the policy guidelines that determined the location of

[11] See Smith and Todd (2005) for an extensive discussion of this approach.

bikeshare stations. Without access to an explicit set of rules or methods to site stations, there is reason to believe that the siting process depended on both socioeconomic variables and current traffic congestion. In an August 2010 application for funding from the Transportation Investments Generating Economic Recovery II (TIGER II) Competitive Grant Program administered by the U.S. Department of Transportation, the Metropolitan Washington Council of Governments indicated a criterion for expanding the Capital Bikeshare conditional on improving transportation options for underserved populations. In particular, the report reads, "[Capital Bikeshare] will bring an affordable, convenient, and healthy travel option directly to 30% of the region's households and population and provide access to 45% of the region's jobs, particularly in areas where low-income and/or transit-dependent populations are concentrated."[12] However, a policy report in Georgetown's Public Policy Review shows that bikeshare stations are located predominantly in wealthy areas of metropolitan Washington with stations located sparsely throughout impoverished neighborhoods.[13] Since station locations are not independent of socioeconomic neighborhood characteristics nor transit accessibility, this suggests that a matching approach along socioeconomic and pre-treatment traffic patterns is a viable strategy to reduce bias from any nonrandomness in the siting of a bikeshare station.

The set of covariates X used to estimate and predict propensity scores is shown in Table IV. We use block group socioeconomic data to help predict the probability that a station is sited in a particular block group. We calculate mean congestion and the standard deviation of congestion in two months prior to the bikeshare program being implemented, in an effort to match the trend in the dependent variable.

We use socioeconomic and pre-treatment traffic patterns as matching variables based on policy makers' stated objectives. Since the bikeshare program operates as part of a larger transportation system and may act as a substitute or complement to existing public transportation, however, there is reason to believe that additional variables my factor into the decision to locate a bikeshare station in a particular location. As additional sources of endogenous station siting, we examine the role of the Washington Metropolitan Area Transit Authority Metrorail, a rapid transit system, and Metrobus, a city-wide bus system. We also measure the aggregate distance of dedicated bicycle lanes, an obvious complement to the bikeshare system.

[12] A Regional Bike-sharing System for the National Capital Region, Metropolitan Washington Council of Governments, August 23, 2010, p. 12. http://www.mwcog.org/uploads/committee-documents/bV5YWlxe20100820155649.pdf. Last accessed, October 13, 2014.

[13] Johnson, Kristine. "Capital Bikeshare in low-income areas: The question no one is asking." Georgetown Public Policy Review, Domestic Policy, Energy & Environment. September 2, 2014. http://gppreview.com/2014/09/02/capital-bikeshare-low-income-areas-question-one-asking/. Last accessed, October 13, 2014.

Our analysis proceeds with two separate matching approaches. The first approach follows from the explicit objectives of the bikeshare program and includes socioeconomic variables and pre-treatment traffic patterns. We refer to this as matching specification 1. A second propensity score matching approach, matching specification 2, includes the distance to the nearest Metrorail station, the number of Metrobus stops in a block group, and total miles of dedicated bicycle lanes, in addition to the full set of covariates in matching specification 1.

While our dataset includes observations across block groups and time periods, our matching approach concerns only block groups. This is based on the following assumption: given that a station is to be established in a particular block group, the timing of its establishment is exogenously determined. Note that the decision to build a station is not a decision each period, but instead a onetime decision to locate a semipermanent station in a block group. There are instances of removal of a station in our dataset, but no instances of removal followed by re-establishment.

We construct a subsample of the full dataset of treated block groups and untreated block groups, in which the untreated block groups are chosen as the most observationally similar to the treated block groups. To accomplish this we use a block group's propensity score $P(X)$, the probability that a block group has a station conditional on the set of observed covariates X. We estimate a probit model to predict $\hat{P}(X)$. Due to the desire to match on covariates before treatment, we use publicly available U.S. Census data from 2009 to construct X.

To define a match, we use caliper matching techniques to find a valid control block group. The caliper approach differs only slightly from nearest neighbor matching. With caliper matching, a treated observation may have more than one corresponding control block group. This allows us to take advantage of multiple control observations that may all be very good counterfactuals, rather than having to choose only the closest. Caliper matching also removes outliers and inliers from the dataset. Treatment observations for which there is no untreated observation with a propensity score within the caliper range are dropped from the sample. In contrast, nearest-neighbor matching forces the best match, regardless of how close of a match it may be. The choice regarding the size of the caliper is left primarily to the researcher. As its objective is to remove poor matches, we use a relatively small caliper and define its size as 0.05 times the observed standard deviation of the predicted propensity scores. This is equivalent to a caliper equal to 0.0083.[14] We address the somewhat arbitrary choice of caliper size in our robustness checks. To avoid issues that arise from having to choose a particular order of matching (Rosenbaum, 1995), we match with replacement in all specifications.

[14]Alternatively, nearest neighbor matching results in a median absolute difference in the predicted propensity score of matches equal 0.025, with matches that differ up to 0.276 in predicted propensity scores.

6.1 Matched Samples

We estimate propensity scores as the basis for matching. The use of congestion as a covariate, however, forces us to consider the time periods allowed in our sample. To avoid matching on our dependent variable, only observations from 2011 onward are used in the estimation sample. In this way, our treatment and control groups are matched based on observed characteristics of the block groups, as well as congestion before the possibility of treatment. The drawback to this approach is that we lose identification power in some cases derived from observing the same block group with and without a station.

Coefficients from the probit regressions indicate that among the socioeconomic variables, Med_Inc, Per_Educ, and Per_Own are significant predictors in the first matching specification.[15] The pre-treatment congestion variables show no statistically significant impact in predicting station locations. In the second matching specification, Med_Inc and Per_Educ are again statistically significant. Each of the three additional variables, $Dist\_to\_Metro$, Agg_BL_Length, and Bus_Count, are statistically significant. To assess the performance of our matching technique we analyze block group descriptors in the treatment and control samples. The objective of matching is to obtain balanced samples in which covariates are similarly distributed across samples. We refer to the set of unmatched observations as the full sample.

6.2 Covariate Balance

Looking first at the full sample, Table V shows mean covariate values for block groups with bikeshare stations (treatment group) along with mean covariate values for block groups without stations, those that would act as counterfactuals using the full sample. For each covariate, we conduct a hypothesis test with the null hypothesis that both means are the same. The p-value from each of these tests is reported in the third column. In addition, we perform a Kolmogorov-Smirnov test (K-S test) for each covariate. The K-S test examines the empirical distribution functions of the treatment and control observations to test whether the samples were drawn from the same distribution. Again, the p-values correspond to a test with a null hypothesis that the treatment and control samples were drawn from the same distribution. Finally, we also report the ratio of sample variances of the two samples. From Table V, the p-value and K-S p-value suggest that sample selection bias likely exists in the full sample. With the exception of Sq_Mi and Pop, a test of means leads us to reject the hypothesis that sociodemographic covariates have identical means across the treatment and control samples at the 5% significance level. Similar results are clear for the K-S test

[15] Full estimation results are available from the authors upon request.

of the covariate distributions. Pre-treatment congestion is already fairly well balanced for May and September, which is expected in light of insignificant estimates in our treatment prediction model. Finally, variables indicating alternative public transportation options are poorly balanced. Identical conclusions of imbalance are drawn from the K-S test.

Table VI displays similar statistics for covariate balance in the first matched sample. The full sample includes 56 treated block groups. Using a caliper of 0.05σ, where σ is the standard deviation of predicted propensity scores, the number of treated block groups decreases to 39, as block groups are dropped from the analysis for not having a sufficiently close match. Since caliper matching was done with replacement, these block groups are matched to 36 unique control block groups from the full sample control of 334. While a comparison of means suggests fairly well-balanced samples, formal hypothesis tests support considerable improvement from matching, as mean comparison p-values are insignificant for all covariates. The K-S tests show similar results and offer additional support for balanced samples. The variables $House\_Age$ and Sq_mi continue to be significantly different, though these variables were insignificant in propensity score estimation. Variance ratios show modest improvement beyond the full sample. Balance on pre-treatment congestion measures also improves. In general, results suggest that our matching approach is effective at balancing covariates across treatment and control samples, thus reducing any bias that may be present in estimation.

Covariate balance from our alternative matching specification, which includes covariates indicating other public transportation options, is shown in Table VII. Note that prior to matching balance on the three transportation infrastructure covariates is extremely poor. Examination of the matched samples shows that they are balanced well, as we are unable to reject different means and different distributions, respectively. However, matching on the additional covariate leads us to lose balance (though not to the point of significant differences) on most of the other covariates. The variables $House\_Age$ and Sq_mi, however, are again significantly different in mean and distribution across treatment and control. Overall, matched samples remain fairly well-balanced, particularly on significant predictors, and indicate the efficacy of our propensity score matching approach.

7 Results

In the following section we present estimated treatment effects. Baseline results are reported for our two matching specifications that vary based on the set of matching covariates. We then further examine the role of treatment in surrounding block groups. We include several robustness checks that reinforce baseline results. Finally, we explore the possibility of heterogeneity in treatment effects across our sample. In all estimated models we cluster standard errors, though we remain agnostic regarding whether standard errors should be clustered based on block groups or time periods, i.e. spatially or temporally. In the results that follow, we report standard errors robust to clustering on time and standard errors robust to multi-way (time and block group) clustering (Cameron et al. (2011).[16]

7.1 Matched Samples Estimates

In Table VIII we report regression results using the unmatched sample. We expect these coefficients to exhibit bias due to the selection of block groups into treatment. However, we report estimates for comparison to matched results and to demonstrate the importance of controlling for selection bias. Results indicate a negligible impact of a bikeshare station on traffic congestion, with statistical significance in only one specification. The coefficient in column 2 of Table VIII indicates a decrease in congestion of 0.34% when a station is present.

Before turning to regression results using samples developed from propensity score matching, we provide suggestive evidence of a treatment effect in Figure 6. Controlling for block group fixed effects, we plot the trend in congestion over time, in which time is defined as number of periods before or after a bikeshare is installed in a block group. We separately estimate trends for treated observations before and after treatment, and estimate similar trends for the matched block groups. The gray line shows the trend for treated block groups, along with 95% prediction intervals. Note a drop in congestion after treatment. The congestion trend for control observations, denoted with the dark line, indicates very little change after treatment. Neither trend has a statistically significant slope. While congestion levels are not significantly different across treatment and control groups, Figure 6 suggests some impact of treatment. We explore this congestion reduction more formally with model estimates below.

To put congestion reduction in context, consider a 1.0% reduction in traffic congestion. Relative to mean congestion in our sample of 1.23, for a road segment with a reference speed of 40 mph, this change in congestion corresponds to a change in actual speed of 0.98%. Alternatively, a 1% change in congestion corresponds to a reduction from median congestion

[16] The proper level for clustering should be determined based on correlation among regressors and correlation among the errors within a cluster. Our treatment variable, siting of a bikeshare station, is highly correlated with block group since there are few instances of removing stations from locations. Given the nature of the bikeshare program, treatment is also correlated with time since many of the bikeshare stations were built as part of major expansions of the program. In considering the error term, there is reason to expect some temporal correlation in congestion. However, the temporal portion of our panel is not a true time series, as a subsequent period could be the next half-hour period, the next day, or the next month. Of course, one may also expect spatial correlation in the error term at any time period, suggesting that clustering on time is the proper specification. Standard errors clustered on block group alone are omitted. They are consistently close to, but less than, multi-way clustered standard errors.

to the 47^{th} percentile.

Average treatment effect on the treated (ATT) estimates, reported in Table IX, are negative and statistically significant across specifications when standard errors are clustered on time. All estimates contain block group and time fixed effects as outlined earlier. In addition, we run weighted least squares estimation, weighting on the aggregate road mileage in a block group.[17] The impact of the presence of a station is interpreted as a decrease in congestion of 1.8% in our preferred specification in which we control for stations in adjacent block groups. This estimate is significant under both cluster specifications.[18] Our results indicate that the presence of a bikeshare station reduces traffic congestion. One conjecture is that this is the result of substitution away from automobile commutes. In addition, we see that non-random siting of stations is an important consideration in measuring the impact of the bikeshare program. Considerably larger negative impacts in the matched sample suggest that stations may be located in high-congestion areas and are thus positively correlated with traffic congestion. With propensity score matching, we remove block groups that were previously serving as poor counterfactuals and identify a causal relationship between bikeshare stations and reductions in traffic congestion. We compare coefficient estimates to those from the FEPD model in which observations are not matched. Relative to unmatched sample estimates, coefficients from the matched sample are significantly different at the 1% level for all three specifications.

From the coefficients on $Station\_Adj$ and $Station\_KAdj$, our results indicate a congestion-increasing effect of stations in neighboring geographic areas, slightly larger than the direct congestion-reducing impact. A potential explanation for these positive coefficients is a spillover effect, in which there is substitution from adjacent block groups among drivers that seek to avoid bike traffic.[19] Thus the increase in congestion could be the result of car traffic that would otherwise have been in a different block group. Note that the estimated impact of a station in a neighboring block group is larger than that of a station in a neighboring block. This may be the result of using only an indicator variable for the presence of stations when the distribution of the number of stations in an adjacent block group is more positively skewed than the number of stations in an adjacent block. Still, these results suggest that the geographic spillover of traffic due to the presence of bikeshare stations could be a significant factor in analyzing traffic congestion impacts. We discuss these impacts further below. In

[17]We use weighted least squares since there is considerable variation in the number of observations used to aggregate congestion in each block group.

[18]Reported standard errors do not account for the fact that propensity scores are predicted. However, we calculate bootstrapped standard errors that do account for the variance of predicted propensity scores. The small increase in standard errors is small enough to retain statistical significance.

[19]Evidence of similar spatial substitution among automobile drivers as a response to traffic policy can be found in Wolff (2014).

requiring that our control sample is made up of block groups with no adjacent treated block groups, we rule out the possibility that this effect is the result of endogenous concentration of stations.

Table X reports results from estimation using samples matched on variables denoting other public transportation options, as well as the previously used set of covariates. Estimates indicate a congestion-reducing effect that is considerably larger than that of the baseline model. As expected, the siting of bikeshare stations within close proximity to Metrorail stations potentially implies close proximity to congested commuting areas and thus generates an upward bias in the unmatched estimates. A similar logic applies to stations in areas with a high volume of public bus commuting. Based on our modified matching specification, results indicate that bikeshare stations reduce congestion by approximately 4%. Estimates are robust to different standard error cluster specifications.

Given the additional variables in our matching approach, the estimated coefficients on *Station_Adj* and *Station_Kadj* shrink and become substantially smaller than the coefficient on *Station*. This suggests the possibility that the large positive impact of adjacent treatment in the first matching specification may be the result of spatial correlation between bikeshare locations and other public transportation. We further explore the effect of adjacent treatment in the next section, before discussing robustness checks.

7.2 Treatment in Adjacent Locations

We estimate two additional specifications to examine the impact of having a bikeshare station in close proximity but outside of the block group. First, recall that in the propensity score matching step we avoid violating SUTVA by restricting the set of potential control observations to block groups that are not adjacent to treated block groups. Here, we drop that restriction and force each treated block group to have at least two control matches: one block group that is adjacent to a treated block group (block) and one block group that is not adjacent to a treated block group (block). While this may cause concern in estimating the impact of a bikeshare station, it offers an improved estimate of adjacent treatment since our sample now includes as controls untreated observations that are adjacent to a treated block group. We match on the full set of covariates and find a statistically significant coefficient on the variable *Station* of -0.0288 when controlling for adjacent block group treatment and -0.0289 when controlling for adjacent block treatment, both only slightly smaller than our baseline specification. Full results are shown in Table XI. More importantly, the coefficients on *Station_Adj* and *Station_Kadj* fall to 0.0045 and 0.0047, respectively, and are insignificant when standard errors are clustered on both time and block group. Better identification indicates that there is little or no congestion spillover from adjacent treatment.

Estimates in our main results suggested a congestion-increasing spillover effect due to a bikeshare station in a neighboring location. Identification in that scenario is based on congestion levels in treated block groups that are adjacent to a treated block group relative to other treated blocks that are not adjacent to a treated block group. Moreover, our propensity matching technique does not address this portion of identification. Bikeshare stations tend to be more densely located in places with heavy commutes. This clustering of stations creates a spatial distribution of stations in which those block group that are treated are more likely to also have a station in an adjacent block group. In our main results, positive coefficients on $Station\_Adj$ and $Station\_Kadj$ are largely driven by this spatial correlation.

It is also important to note that our definition of adjacent treatment is based on boundaries of blocks and block groups. These entities vary in size and shape, and may be problematic in capturing spatial relationships. We therefore redefine adjacent treatment as equal to 1 if there exists a bikeshare station with r kilometers of the block group. We estimate the model for various values of r, ranging from $0.5 - 3$km. Results are shown in Table XII The coefficient on *Station* is consistently negative and significant across most specifications, although becomes statistically 0 when a distance of 2.75 or 3.0 is used. Interestingly, the coefficient on adjacent treatment is negative and significant when adjacent treatment is defined as a bikeshare station within a small distance of the block group. This effect becomes smaller and eventually insignificant when the definition of treatment is based on the existence of a station at a larger and larger distance. These estimates suggest that previously discussed coefficients on $Station\_Adj$ and $Station\_Kadj$ may be capturing the combined negative impact of treatment in close proximity and a positive effect of treatment at slightly farther distances. The impact of nearby stations is seemingly confounded by a definition of adjacent treatment based only on the boundaries of blocks and block groups.

7.3 Robustness Checks

We conduct a number of robustness checks to explore alternative possibilities that may explain our results. Each of the following specifications build off of the second matching specification and we focus on our preferred specification, controlling for stations in adjacent block groups. In general, model estimates are consistent throughout.

To address potential confounding variables, we run a simple falsification test. We estimate the model on observations prior to program implementation and assign treatment status based on whether the block group is treated with a bikeshare station in the future. In Table A1, results show no significant impact of a station. In addition, we achieve strong covariate balance in this specification. Similarly, to avoid any confounding variables related to removal of bikeshare stations, we re-estimate our model with the exclusion of treated block groups that had stations established and then removed. Regression results, in Table A2, are nearly identical to our previous estimates.

Next, we consider the possibility that drivers may react to changing traffic conditions by substituting towards different time periods. This is particularly important in the context of results from Lin Lawell et al. (2016), in which intertemporal substitution due to driving restrictions leads to an increase in emissions. If this were the case, some time periods may experience an increase (or a smaller decrease) in congestion. We divide our sample into four subsamples based on the four one-hour-periods in our data and estimate a separate regression for each period. Results are reported in Table A3 for the specification that controls for bikeshare stations in adjacent block groups. Coefficients indicate a congestion-reducing impact of a bikeshare station, though the impact is of statistically significantly greater magnitude later in the morning. For the later two time periods, the estimated $Station$ coefficients are -0.0537 and -0.0495, respectively. For the earlier time periods, the estimated $Station$ coefficients are -0.0228 and -0.0377, respectively. Thus we cannot rule out the possibility that drivers are substituting towards earlier times, but such a mechanism does not outweigh the effect of a bikeshare station.

Another potential issue in our analysis is that a block group's reference speed, which is used to calculate congestion via equation (1), changes over time. The standard deviation of reference speed has a mean of 1.32 over road segments, with the 20^{th} and 80^{th} percentiles equal to 0.47 and 1.91, respectively. If the change in reference speed over time is related to treatment, the relationship between treatment and congestion may be misidentified. Still, the change in reference speed over time is important to capture other trends in traffic patterns. To address this issue, we examine changes in reference speed for each road segment. We fit a trend line for each road segment and observe that the slope of this trend line is positive for 65%, negative for 29%, and flat for the remaining observations. Importantly, the distribution of trend line slopes is nearly identical for road segments in control and treatment block groups, and we find a correlation between $Station$ and slope of the trend line to be 0.013. We can therefore conclude that changes in the reference speed are independent of treatment. To further examine this issue, we estimate our model using observed speed as the dependent variable, rather than congestion. The coefficient on $Speed$ is positive and significant, suggesting that the previously discussed reduction in congestion is the effect of increases in speed. Full results for regressions with $Speed$ as the dependent variable are reported in Table A4.

Regarding other potential confounding variables, we also consider the impact of rain and station outages. The presence of rain may have a direct impact of increasing traffic congestion due to road conditions and reducing the willingness to use a bicycle. Thus we do not expect to observe the same congestion-reducing effect on these days. we estimate the model on a sample in which we drop all observations on days when rain is observed. While this leads to a reduction in observations equal to approximately half of the sample, coefficient estimates are nearly identical with no statistical difference. We also consider the impact of station outages, times at which a station has zero bicycles. In such cases our measured variable, the existence of a station, is a poor indication of a transportation option. We observe all times when a particular station is empty, so we drop any observations in which the time-period overlaps a period of station outage. This reduces our sample by approximately 12%. Results continue to indicate the congestion-reducing impact of a bikeshare station and are not statistically different from our baseline specification.[20]

Finally, we examine our use of a caliper equal to 0.05 standard deviations to determine the degree to which the somewhat arbitrarily chosen caliper size may influences our estimates. We report sensitivity analysis for our preferred specification that controls for stations in adjacent block groups. We re-estimate the model with matched samples based on a caliper ranging from 0.01 to 0.10 standard deviations, using both matching specifications. Coefficient estimates on *Station*, along with standard errors, are shown in Table A5. We present results for the second matching specification, which includes socioeconomic variables, pre-treatment congestion, and public transportation variables. Estimates of the congestion impact of a bikeshare station range from -3.04%, when using a relatively large caliper equal to 0.10, to -4.8% when a caliper of 0.01 standard deviations is employed, though they are fairly stable in terms of identifying a congestion-reducing effect of a bikeshare station.

7.4 Heterogenous Treatment Effects

An extension of our empirical model examines whether the impact of a bikeshare station on congestion is uniform across congestion levels. To do so, we use quantile regression in which the marginal impact of a bikeshare station varies with the congestion quantile. The propensity matching approach discussed earlier is primarily focused on matching the conditional expectation of the dependent variable, which fits well with a least squares regression of our linear equation. Still, Kolmogorov-Smirnov tests in the earlier covariate balance discussion suggest that the distributions, rather than simply the means, are matched well. Thus we are confident in applying quantile regression to our matched samples.

[20] Regression results for specifications in which days with rain or times with station outages are dropped are available from the authors upon request.

Given our panel data set, we follow the empirical specification of Canay (2011),

$$Q_{\ln Cong}(\tau|X) = Station_{jhdmt}\beta(\tau) + Station\_Adj_{jhdmt}\beta_{Adj}(\tau) + \gamma Rain_d + \delta_j + \nu_h + \mu_m + \upsilon_t, \quad (6)$$

where $Q_y(\tau|X)$ indicates the τ^{th} conditional quantile of variable y, conditional on the design matrix X. This is nearly identical to Canay (2011), except that we add time fixed effects in addition to spatial fixed effects. The concept, however, remains the same in that time and location level effects are independent of the congestion quartile. Heterogeneity exists only in the impact of a bikeshare station. The method developed by Canay (2011) finds consistent estimates of the β parameter that can used to estimate fixed effects. In a second step, the dependent variable is transformed by calculating deviations from fixed effects. The transformed variable becomes the dependent variable for quantile regression to estimate the quantile functions $\beta(\tau)$ and $\beta_{Adj}(\tau)$. We also estimate equation (6) with a $\beta_{KAdj}(\tau)$ to correspond with our earlier specification.

We fit conditional quantile functions for nine congestion deciles, using our second matching specification. Coefficient estimates with 95% confidence interval bounds from quantile regression are plotted in Figure 7. The two sets of confidence intervals for each point represent different standard error cluster definitions.[21] The horizontal dashed line is drawn at the coefficient estimate, -0.0404, from the linear regression model. We find that the congestion-reducing effect is concentrated at higher levels of congestion. The impact of a bikeshare station ranges from having a negative impact on congestion of approximately -3%, with a steady increase in the magnitude of the effect as congestion increases. For block groups that have high levels of congestion, however, bikeshare stations appear to reduce congestion upwards of -5.4%. Estimates are highly significant in both cluster specifications.

Coefficient results are intuitively satisfying. Given our congestion measure, low levels of congestion likely indicate a lower bound at which traffic flows freely since a further reduction in congestion would be generated by an increase in observed speed. Therefore, we may not expect to see as much of a congestion-reducing impact. A statistically significant congestion-reducing impact of bikeshare stations is stronger in congested areas, as there is considerable opportunity to reduce congestion through the availability of alternative transportation.

[21] Standard errors are calculated using a bootstrap method, following Krinsky and Robb (1986), using 1000 simulations.

8 Discussion

In general, coefficient estimates from matched samples imply a causal link between the presence of bikeshare stations and congestion reduction. Given evidence of improved covariate balance in several matching models, our regression results suggest that there is self-selection of block groups into treatment. The FEPD estimates are biased by observations of bikeshare stations that are placed in high-congestion areas, thus generating a positive correlation between the presence of a station and congestion levels. Matching prevents low-congestion block groups with no stations from serving as the counterfactual for high-congestion block groups that are selected into treatment, i.e. have a station established. Thus coefficients estimated from matched samples present a more accurate estimate of the causal impact of bikeshare stations, and we see considerable evidence that bikeshare stations are effective at reducing congestion in their immediate areas. In the context of extremely large costs of congestion estimated by Schrank et al. (2012), even a small reduction in congestion of approximately 4% generates considerable welfare improvement.

An examination of the adjacency coefficients relative to those on *Station* in our baseline model suggests the possibility of other important effects of the presence of bikeshare stations. However, further analysis reveals that these coefficient estimates could be the result of two mechanisms. First, our matching approach seeks to estimate a causal effect of treatment, where treatment is defined as a bikeshare station in a particular location. This leads to a less than ideal identification strategy for the impact of stations in neighboring locations. While a modified matching technique suggests no effect of nearby stations, we are less confident in estimates of the direct treatment effect due to a less restricted control group. A second explanation for our baseline findings related to adjacent treatment is a simplified definition of adjacent treatment based on treatment in a bordering locality. Due to considerable variation in the geographic size and shape of census block and block groups, this creates only a rough approximation of the spatial distribution of stations. A more precise definition of nearby bikeshare stations reveals a congestion-decreasing effect of both treatment and adjacent treatment.

Our estimated causal effects can be used to calculate rough estimates of the benefits that accrue to commuters due to the bikeshare program. Consider a 4% reduction (from our baseline estimates) in traffic congestion among the 19.7% of block groups that have a bikeshare station. This would reduce annual congestion costs for Washington area automobile commuters by approximately \$57 per commuter, and total costs by \$182 million (Schrank et al., 2015). This figure represents the private economic benefits that accrue to commuters through shorter travel times and reduction of wasted fuel. In terms of social benefits, a 4% reduction in traffic congestion for our study area would imply an annual benefit of roughly \$1.28 million from reductions in congestion-induced CO_2 emissions.[22] These estimates, however, ignore any local environmental benefits from improved air quality through, for example, reduced NO_x emissions. Further, these numbers also ignore private cost-savings from mode-switching and any health benefits that may accrue to bicycle commuters. Thus, the true monetary benefits are potentially much greater than the back-of-the-envelope statistics reported here. We focus only on the direct impact of a bikeshare station on the block group in which it is located, ignoring any impact on surrounding areas. Though our baseline models show a congestion-increasing impact of treatment in adjacent areas, subsequent analysis demonstrates no impact of bikeshares in surrounding areas or a further congestion-decreasing effect, in which case the calculations above can be interpreted as conservative estimates.

Overall, there appear to substantial private and public economic benefits for urban communities from adopting bikesharing infrastructure. Importantly, these aggregate numbers compare favorably to Capital Bikeshare's total operating costs of \$5.8 million in fiscal year 2014, with a 70% cost recovery ratio without government intervention (DDOT, 2015).

9 Conclusion

In this analysis we present causal evidence of the impact of bikeshare programs on traffic congestion. Though the marginal impact is somewhat small, it translates into considerable gains in social welfare. Using a unique dataset of city roads, we construct a finely spatially defined measure of congestion that allows us to examine congestion effects at a disaggregated geographic level. A panel dataset of half-hourly traffic observations at the census block group level suggests that the existence of a bikeshare station in a block group reduces traffic congestion. To control for non-randomness of station locations, we use a propensity score matching approach to identify the causal effect of the presence of a station. A comparison of estimates from matched samples to those from the full sample of observations suggests a selection bias in the placement of bikeshare stations.

Our empirical results indicate that the average treatment effect of the presence of bikeshare stations is an approximately 4% reduction in traffic congestion. Our results are robust to various caliper values in propensity score matching. Estimates are also robust to heteroskedasticity and autocorrelation when standard errors are clustered spatially and temporally. Regarding policy, our analysis indicates the effectiveness of a bikeshare program in reducing congestion.

[22] This estimate is obtained by multiplying the implied reduction in congestion-induced CO_2 emissions from Eisele et al. (2013) by a social cost of carbon estimate of \$41.4, taken from USSCC (2015).

Our model takes a reduced from approach to identifying a treatment effect. We are therefore unable to determine the degree to which commuters are substituting away from driving or away from other modes of public transportation. A statistically significant treatment effect, however, indicates that at least a portion of the increase in bikeshare usage is the result of substitution away from automobile commuting. Further research should explore the consumer's transportation decision to measure transportation substitution patterns. This point has important implications for policy. If the bikeshare competes with other public transportation, such as light rail, local governments may be in a position to reallocate funding towards a bikeshare system that requires considerably less capital investment. Still, if the bikeshare serves as a complement to existing public transportation, by extending the network, it could increase the marginal benefits to the consumer of using light rail or bus. Given evidence for the congestion-reducing impact of the bikeshare program, a structural model of transportation decisions that can disentangle choice patterns could speak more to the optimal public funding for various transportation options.

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Tables

Table I: Summary Statistics

	mean	std. dev.	10^{th} percentile	90^{th} percentile
Congestion (by road)	1.247	0.476	0.968	1.586
Congestion (by block group)	1.216	0.262	1.021	1.442
Station	0.159	-	-	-
Station_Adj	0.480	-	-	-
Station_KAdj	0.259	-	-	-

Notes: Congestion is defined at the Census block group level. Station is the average number of stations in a Census block group. Station_Adj is the number of stations in an adjacent Census block group. Station_KAdj is the number of stations in an adjacent Census block.

Table II: Frequency Distribution of Station Counts

	0	1	2	3	4	5	6	7+
Stations (max = 6)	471,763	63,486	16,016	6,358	416	2,433	326	-
Stations_Adj (max = 23)	291,894	72,748	54,288	41,657	23,776	17,050	9,766	49,619
Stations_Kadj (max = 5)	415,826	97,124	28,294	9,463	6,889	3,202	-	-

Table III: Date of Installation

	Apr. 2010	May 2010	Sep. 2010	Oct. 2010	Apr. 2011	May 2011	Sep. 2011	Oct. 2011
Stations	95	99	101	101	143	149	160	160
Treated Block Groups	40	42	42	45	53	53	56	56

Notes: This table presents the number of stations (block groups) installed (treated) in each month of the sample analyzed.

Table IV: Variables for Propensity Score Matching

Variable	Description
Med_Inc	median household income
Price	median price of housing (rented and owned units)
Pop	block group population
Per_Educ	percentage of population +25 years with bachelor's degree or further education
Per_Own	percentage of housing units owned by tenant
House_Age	median age of housing units
Sq_Mi	square mile area
Cong_Mean_May	mean congestion in May 2010
Cong_SD_May	standard deviation of congestion in May 2010
Cong_Mean_Sep	mean congestion in September 2010
Cong_SD_Sep	standard deviation of congestion in September 2010
Dist_to_Metro	distance from center of block group to nearest Metrorail station
Agg_Bikelane	aggregate distance of dedicated bicycle lanes
Bus_Count	count of Metrobus stops in the block group

Table V: Covariate Balance: Full Sample, Unmatched

Variable	Treatment Mean	Control Mean	Variance Ratio	p-value	K-S p-value
Med_Inc	83695.625	94296.912	1.774	0.052	0.019
Price	3951.310	4687.899	1.331	0.026	0.031
Pop	1554.143	1391.771	0.981	0.100	0.027
Per_Educ	0.634	0.540	1.104	0.010	0.016
Per_Own	0.450	0.596	1.386	0.000	0.001
House_Age	54.982	55.614	0.492	0.410	0.016
Sq_mi	0.180	0.192	1.751	0.376	0.087
Cong_Mean_May	1.257	1.262	1.187	0.440	0.900
Cong_SD_May	0.153	0.156	1.831	0.438	0.220
Cong_Mean_Sep	1.250	1.260	1.318	0.352	0.636
Cong_SD_Sep	0.142	0.156	2.292	0.186	0.228
Dist_to_Metro	283.424	1055.266	7.285	0.000	0.000
Agg_BL_Length	691.085	271.081	0.207	0.001	0.000
Bus_Count	8.089	5.614	0.258	0.017	0.211

Notes: This table presents balance statistics for our full, unmatched sample. Treatment (control) mean is the covariate mean for all treated (control) block groups. Variance ratio is the treatment mean divided by the control mean. P-value is the result of a two-sided t-test comparing treatment and control means. K-S p-value is the test statistic for the Kolmogorov-Smirvnov equality-of-distribution tests.

Table VI: Covariate Balance: Matching Specification 1 (Caliper = 0.008)

Variable	Treatment Mean	Control Mean	Variance Ratio	p-value	K-S p-value
Med_Inc	82988.361	89836.714	1.038	0.280	0.797
Price	4166.841	4241.700	1.189	0.456	0.534
Pop	1614.750	1505.171	1.373	0.311	0.344
Per_Educ	0.536	0.562	0.662	0.331	0.267
Per_Own	0.513	0.533	1.885	0.384	0.368
House_Age	55.194	50.114	0.611	0.117	0.017
Sq_mi	0.180	0.195	0.666	0.351	0.020
Cong_Mean_May	1.297	1.267	0.830	0.278	0.348
Cong_SD_May	0.173	0.163	1.213	0.389	0.536
Cong_Mean_Sep	1.293	1.250	0.997	0.161	0.077
Cong_SD_Sep	0.158	0.152	1.193	0.414	0.884

Notes: This table presents balance statistics for our matching with replacement using a caliper width of 0.05 standard deviations of the propensity score (approximately 0.008). Observations are matched on socioeconomic variables and pre-treatment traffic patterns. Treatment (control) mean is the covariate mean for all treated (control) block groups. Variance ratio is the treatment mean divided by the control mean. P-value is the result of a two-sided t-test comparing treatment and control means. K-S p-value is the test statistic for the Kolmogorov-Smirvnov equality-of-distribution tests.

Table VII: Covariate Balance: Matching Specification 2 (Caliper = 0.008)

Variable	Treatment Mean	Control Mean	Variance Ratio	p-value	K-S p-value
Med_Inc	88770.045	102033.588	1.767	0.235	0.132
Price	4430.313	5246.449	1.160	0.203	0.058
Pop	1461.591	1460.941	0.492	0.499	0.436
Per_Educ	0.531	0.582	1.167	0.295	0.647
Per_Own	0.587	0.582	2.058	0.475	0.568
House_Age	61.773	52.000	1.096	0.029	0.002
Sq_mi	0.143	0.252	1.205	0.039	0.001
Cong_Mean_May	1.275	1.308	0.515	0.293	0.505
Cong_SD_May	0.152	0.263	4.467	0.046	0.218
Cong_Mean_Sep	1.275	1.314	1.110	0.264	0.711
Cong_SD_Sep	0.144	0.250	12.172	0.053	0.122
Dist_to_Metro	480.331	621.841	1.624	0.144	0.310
Agg_BL_Length	326.140	207.954	0.630	0.167	0.437
Bus_Count	7.682	6.941	0.735	0.334	0.574

Notes: This table presents balance statistics for our matching with replacement using a caliper width of 0.05 standard deviations of the propensity score (approximately 0.008). Observations are matched on socioeconomic variables, pre-treatment traffic patterns, distance to the nearest Metrorail station, the number of Metrobus stops, and total miles of dedicated bicycle lanes. Treatment (control) mean is the covariate mean for all treated (control) block groups. Variance ratio is the treatment mean divided by the control mean. P-value is the result of a two-sided t-test comparing treatment and control means. K-S p-value is the test statistic for the Kolmogorov-Smirvnov equality-of-distribution tests.

Table VIII: Presence of Bikeshare Stations: FEPD Unmatched Sample

	1	2	3
Station	0.0009	-0.0034	-0.0007
(cluster on Time)	(0.0008)	(0.0015)***	(0.0010)
(Two-way cluster)	(0.0078)	(0.0078)	(0.0076)
Station_Adj		0.0131	
(cluster on Time)		(0.0025)***	
(Two-way cluster)		(0.0078)	
Station_Kadj			0.0070
(cluster on Time)			(0.0015)***
(Two-way cluster)			(0.0057)
Adjusted R^2	0.4622	0.4623	0.4622
Observations	652,052	652,052	652,052

Notes: The tables shows regression results using the full unmatched sample of observations. Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (15-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table IX: Presence of Bikeshare Stations: Matching Specification 1

	1	2	3
Station	-0.0083	-0.0182	-0.0147
(cluster on Time)	(0.0025)***	(0.0031)***	(0.0026)***
(Two-way cluster)	(0.0138)	(0.0129)	(0.0132)
Station_Adj		0.0367	
(cluster on Time)		(0.0034)***	
(Two-way cluster)		(0.0166)**	
Station_Kadj			0.0255
(cluster on Time)			(0.0026)***
(Two-way cluster)			(0.0156)*
Adjusted R^2	0.5566	0.5573	0.5569
Observations	105,432	105,432	105,432

Notes: This table shows regression results using a sample of matched observations, where observations are matched on socioeconomic variables and pre-treatment traffic patterns . Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 15-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table X: Presence of Bikeshare Stations: Matching Specification 2

	1	2	3
Station	-0.0373	-0.0404	-0.0385
(cluster on Time)	(0.0045)***	(0.0050)***	(0.0046)***
(Two-way cluster)	(0.0152)***	(0.0158)***	(0.0150)***
Station_Adj		0.0263	
(cluster on Time)		(0.0045)***	
(Two-way cluster)		(0.0177)*	
Station_Kadj			0.0096
(cluster on Time)			(0.0036)***
(Two-way cluster)			(0.0109)
Adjusted R^2	0.5029	0.5032	0.5029
Observations	62,246	62,246	62,246

Notes: This table shows regression results using a sample of matched observations, where observations are matched on socioeconomic variables, pre-treatment traffic patterns, distance to the nearest Metrorail station, the number of Metrobus stops, and total miles of dedicated bicycle lanes. Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (15-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table XI: Presence of Bikeshare Stations: Matching Specification 2 (Include controls that are adjacent to treated observation.)

	1	2	3
Station	-0.0285	-0.0288	-0.0289
(cluster on Time)	(0.0034)***	(0.0035)***	(0.0034)***
(2-way cluster)	(0.0159)*	(0.0159)*	(0.0157)*
Station_Adj		0.0045	
(cluster on Time)		(0.0025)*	
(2-way cluster)		(0.0102)	
Station_Kadj			0.0047
(cluster on Time)			(0.0027)*
(2-way cluster)			(0.0097)
Adjusted R^2	0.5492	0.5492	0.5492
Observations	119,160	119,160	119,160

Notes: This table shows regression results using a sample of matched observations, where observations are matched on socioeconomic variables, pre-treatment traffic patterns, distance to the nearest Metrorail station, the number of Metrobus stops, and total miles of dedicated bicycle lanes. Unlike results reported above, in this sample we include control observations that are adjacent to treated observations. Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table XII: Presence of Bikeshare Stations: Treatment within Radius

Distance (km)	Station	SE	Station_Adj	SE
0.50	-0.0583***	(0.0123)	-0.0368	(0.0177)**
0.75	-0.0582***	(0.0123)	-0.0437	(0.0173)**
1.00	-0.2256***	(0.0323)	-0.2138	(0.0333)***
1.25	-0.1149***	(0.0100)	-0.0916	(0.0216)***
1.50	-0.0647***	(0.0070)	-0.0477	(0.0230)**
1.75	-0.0647***	(0.0070)	-0.0410	(0.0227)*
2.00	-0.0648***	(0.0070)	-0.0389	(0.0238)
2.25	-0.0649***	(0.0070)	-0.0281	(0.0243)
2.50	-0.0649***	(0.0070)	-0.0281	(0.0243)
2.75	0.0018	(0.0270)	0.0485	(0.0415)
3.00	0.0015	(0.0269)	0.0676	(0.0598)

Notes: Each row presents results from a regression specification in which treatment status is defined as the presence of a station within the block group (as before), but adjacent treatment is defined as having a station within a specified distance of the block group's border. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Figures

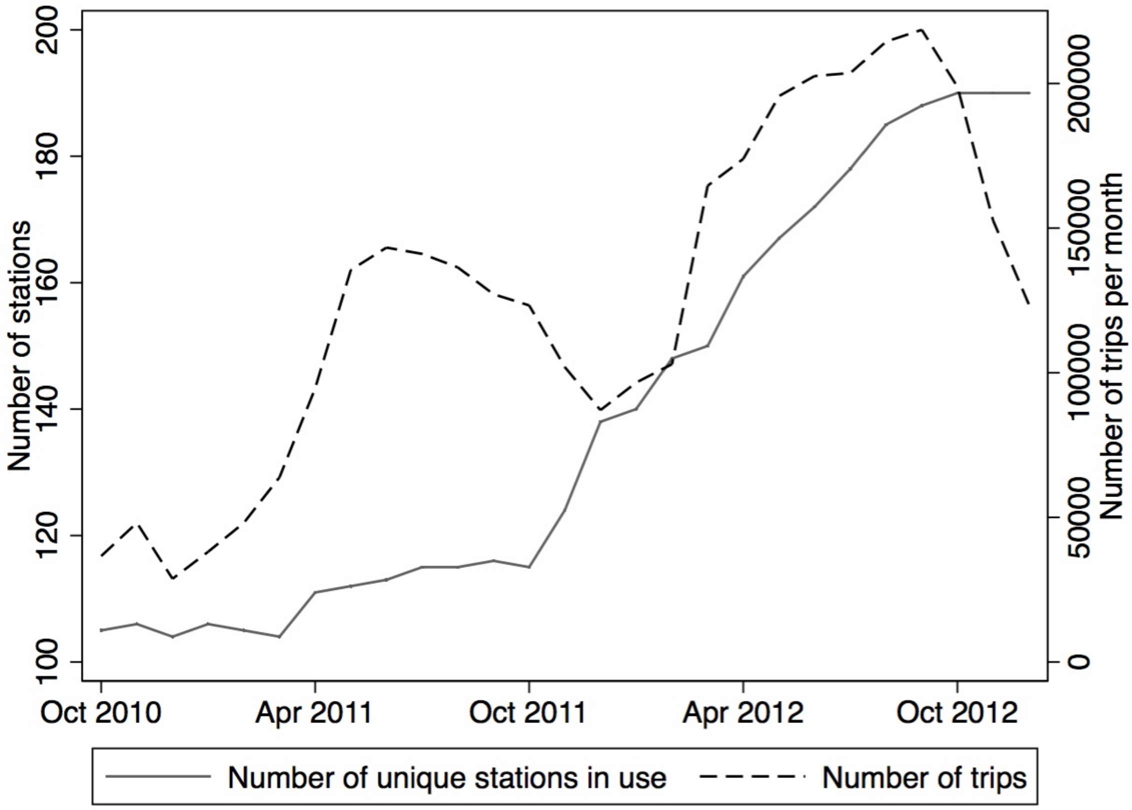

Figure 1: Trends in number of Capital Bikeshare trips and number of stations in use between October 2010 and December 2012

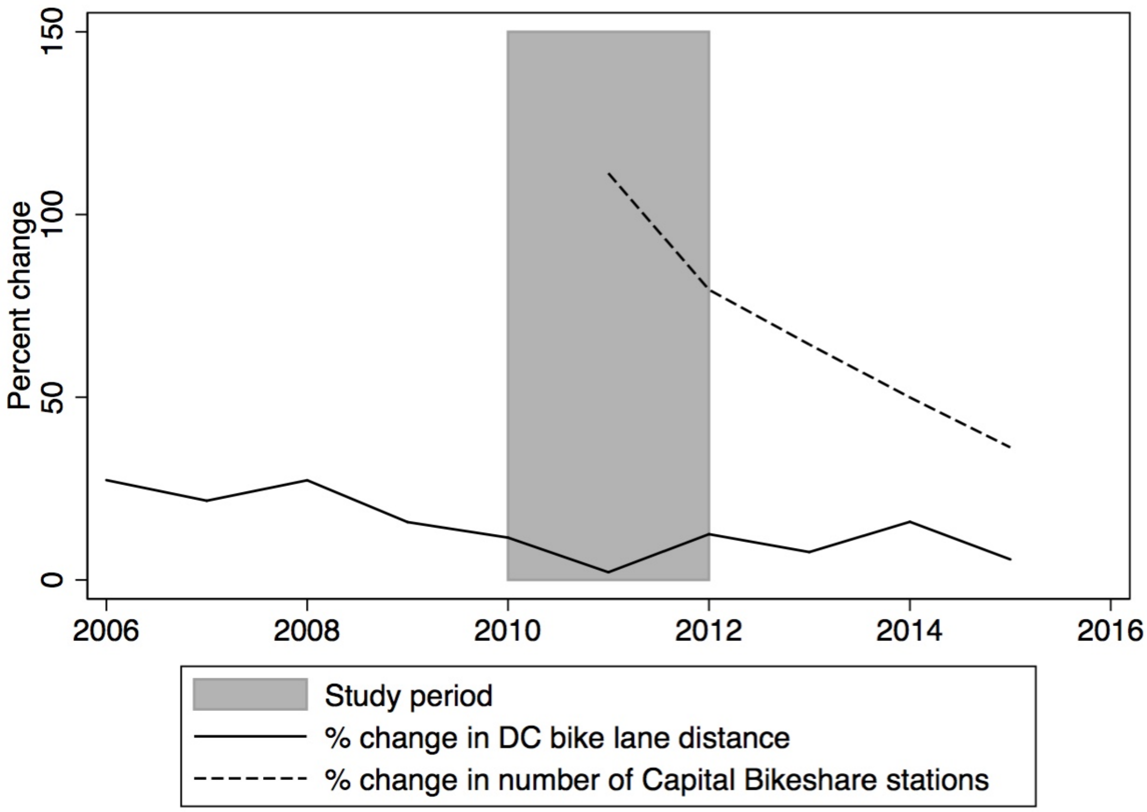

Figure 2: Trends in the percent change of bicycle lanes vs. number of Capital Bikeshare stations throughout the sample.

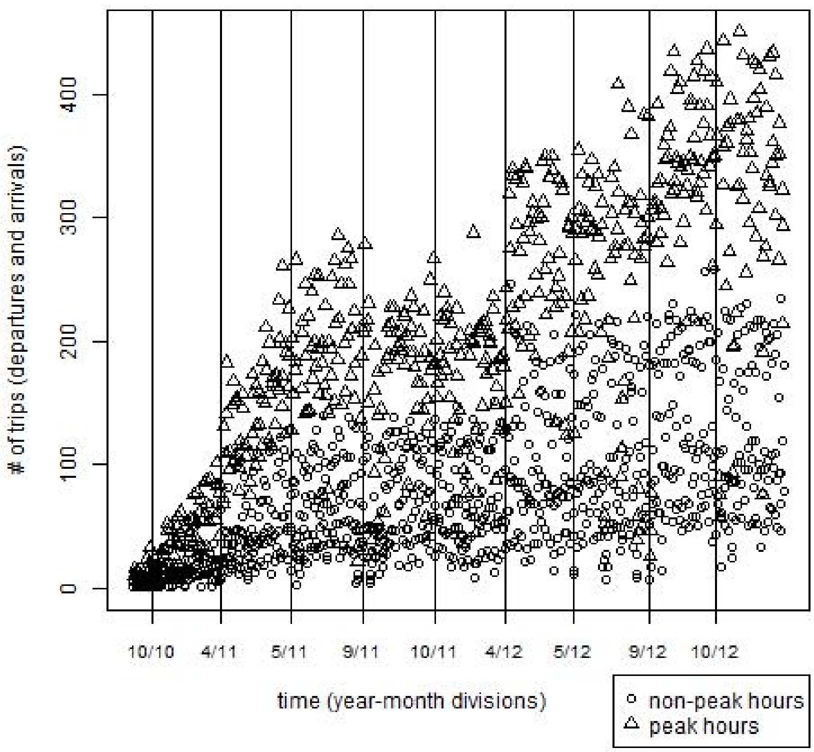

Figure 3: Total count of bikeshare trips made (departures and arrivals) throughout the sample by time of day.

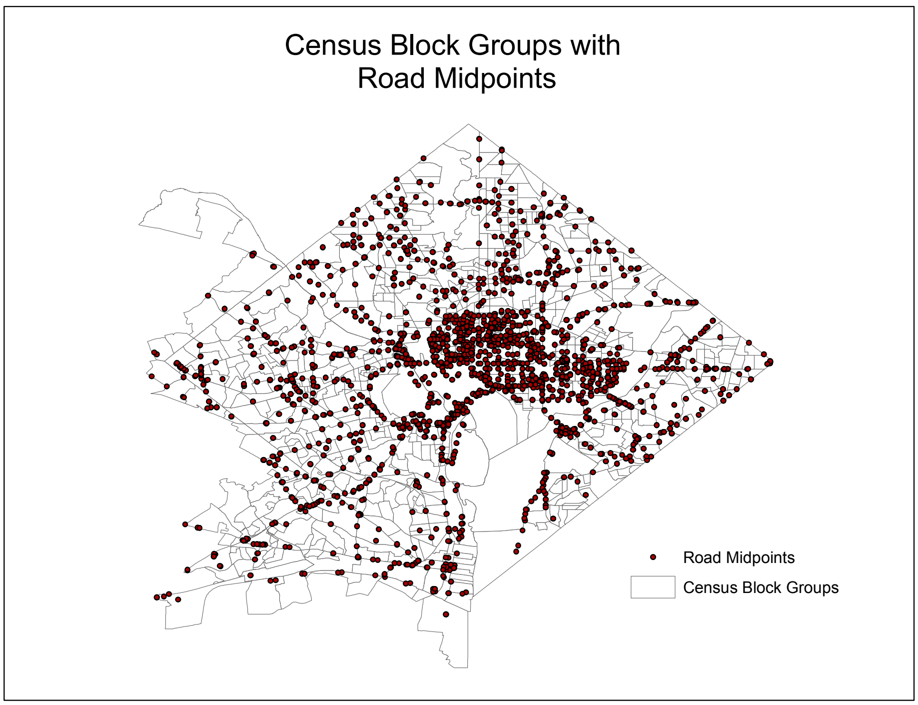

Figure 4: Census Block Groups with Road Midpoints

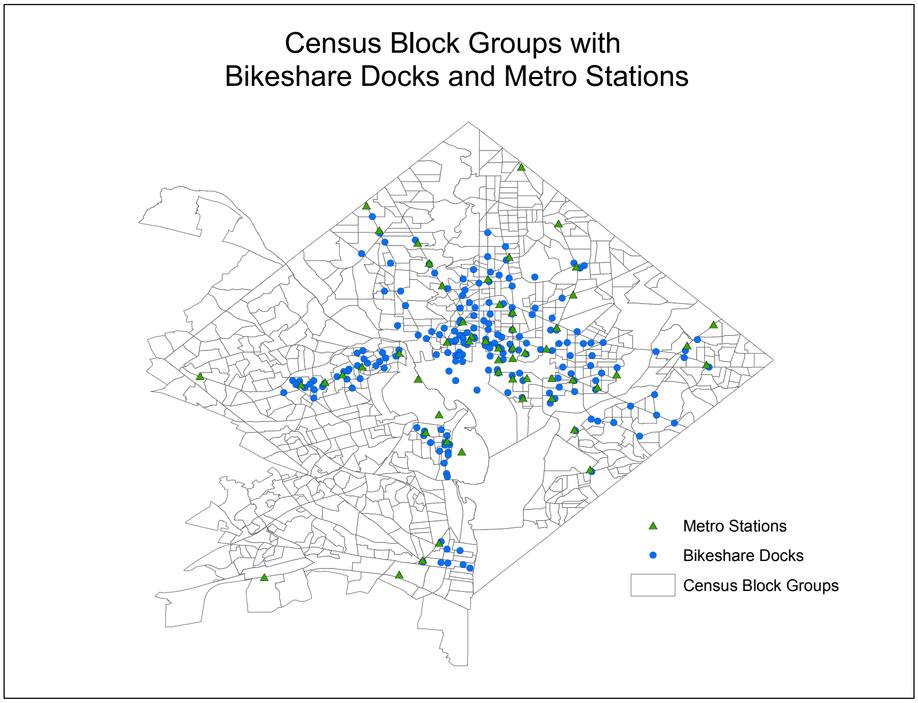

Figure 5: Census Block Groups with Bikeshare Stations and Metrorail Stations

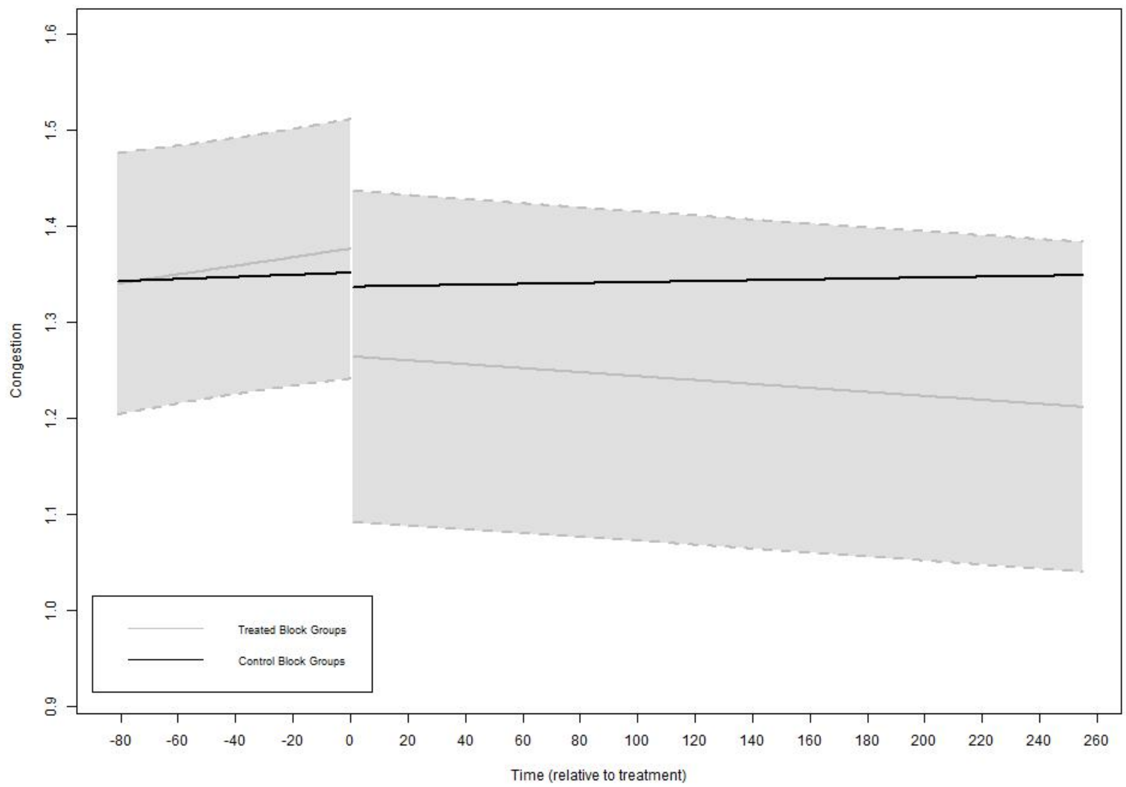

Figure 6: Congestion Trend: Treated vs. Control Block Groups

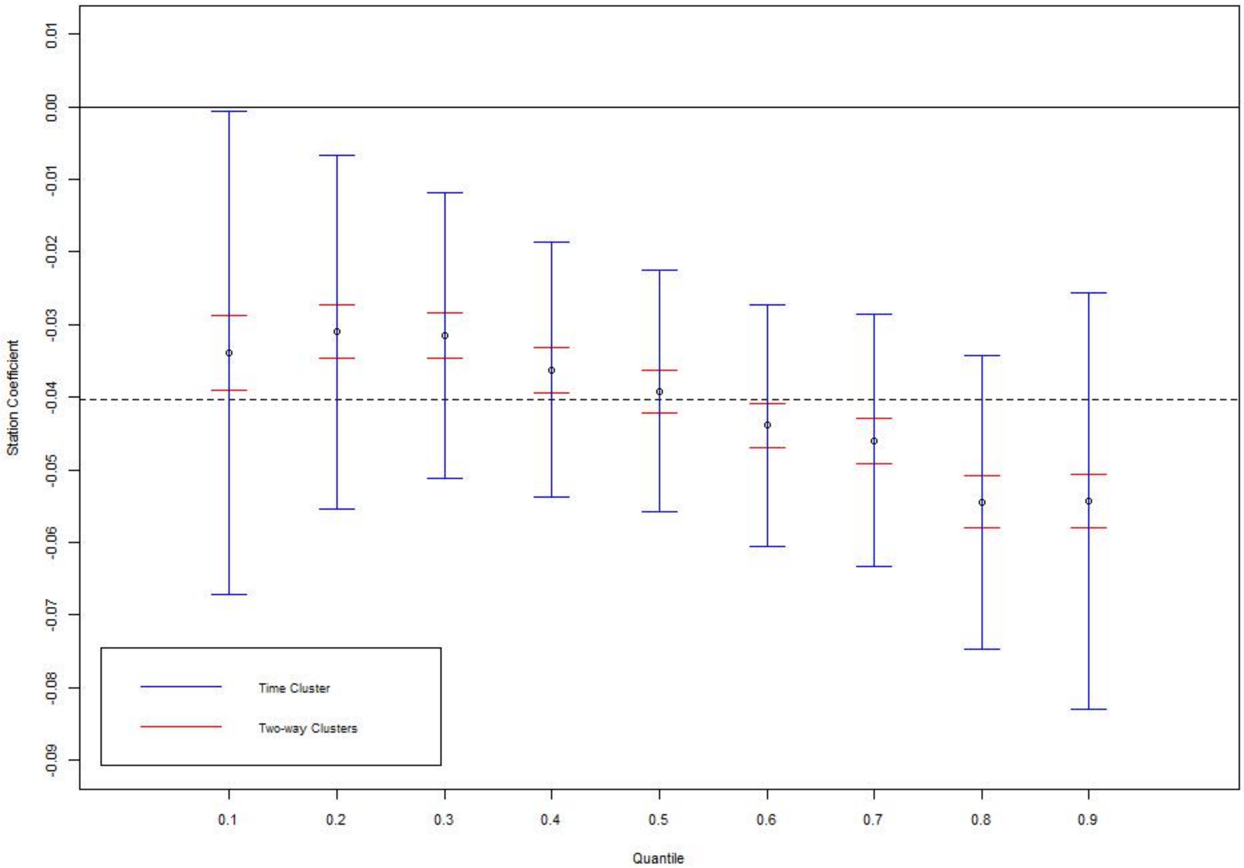

Figure 7: Quantile Regression: Station Coefficients (Control for Neighboring Block Groups)

Appendix

Table A1: Presence of Bikeshare Stations: Pre-Treatment Falsification Test

	1	2	3
Station	0.0599	-0.0119	0.0135
(cluster on Time)	(0.0137)*	(0.0162)	(0.0202)
(2-way cluster)	(0.0663)	(0.0539)	(0.0363)
Station_Adj		0.0723	
(cluster on Time)		(0.0274)**	
(2-way cluster)		(0.0716)	
Station_Kadj			0.0633
(cluster on Time)			(0.0332)*
(2-way cluster)			(0.0835)
Adjusted R^2	0.1181	0.1182	0.1213
Observations	15,829	15,829	15,829

Notes: This table shows regression results using observations prior to implementation of the bikeshare program. Observations are matched based on the full matching specification. Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table A2: Presence of Bikeshare Stations: Exclude BGs with Removal

	1	2	3
Station	-0.0379	-0.0409	-0.0390
(cluster on Time)	(0.0046)***	(0.0050)***	(0.0047)***
(2-way cluster)	(0.0152)**	(0.0158)**	(0.0150)**
Station_Adj		0.0251	
(cluster on Time)		(0.0045)***	
(2-way cluster)		(0.0172)	
Station_Kadj			0.0092**
(cluster on Time)			(0.0035)
(2-way cluster)			(0.0109)
Adjusted R^2	0.5024	0.5027	0.5025
Observations	62,246	62,246	62,246

Notes: This table shows regression results excluding any block groups that were treated at one time but later had a station removed and became untreated. Observations are matched based on the full matching specification. Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table A3: Presence of Bikeshare Stations: Time Subsamples

	6:00 - 7:00 A.M.	7:00 - 8:00 A.M.	8:00 - 9:00 A.M.	9:00 - 10:00 A.M.
Station	-0.0228	-0.0377	-0.0537	-0.0495
(cluster on Time)	(0.0038)***	(0.0079)***	(0.0018)***	(0.0072)***
(2-way cluster)	(0.0119)*	(0.0139)**	(0.0216)**	(0.0212)**

Notes: This table shows regression results using subsamples based on time of day. Observations are matched based on the full matching specification and we control for treatment in adjacent block groups. Each column presents results from a different regression specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table A4: Presence of Bikeshare Stations: Dependent Variable Speed

	1	2	3
Station	0.0183	0.0203	0.0188
(cluster on Time)	(0.0042)***	(0.0046)***	(0.0042)***
(2-way cluster)	(0.0084)**	(0.0088)**	(0.0086)**
Station_Adj		-0.0171	
(cluster on Time)		(0.0039)***	
(2-way cluster)		(0.0097)*	
Station_Kadj			-0.0038
(cluster on Time)			(0.0021)*
(2-way cluster)			(0.0071)
Adjusted R^2	0.7121	0.7121	0.7121
Observations	59,929	59,929	59,929

Notes: This table shows regression results using a sample constructed from the full matching specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. Each column presents results from a different regression specification. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.

Table A5: Caliper Robustness

Caliper Size	Coef.	SE (Time Cluster)	SE (Two-Way Cluster)
0.010	-0.04868	(0.0106)***	(0.0242)**
0.015	-0.04174	(0.0075)***	(0.0195)**
0.020	-0.02737	(0.0052)***	(0.0181)*
0.025	-0.03329	(0.0044)***	(0.0171)**
0.030	-0.04051	(0.0053)***	(0.0165)***
0.035	-0.04051	(0.0053)***	(0.0165)***
0.040	-0.04051	(0.0053)***	(0.0165)***
0.045	-0.03985	(0.0049)***	(0.0159)***
0.050	-0.04042	(0.0050)***	(0.0158)***
0.055	-0.04042	(0.0050)***	(0.0158)***
0.060	-0.03729	(0.0050)***	(0.0162)**
0.065	-0.03425	(0.0049)***	(0.0164)**
0.070	-0.03425	(0.0049)***	(0.0164)**
0.075	-0.03583	(0.0050)***	(0.0166)**
0.080	-0.03358	(0.0049)***	(0.0166)**
0.085	-0.03274	(0.0047)***	(0.0166)**
0.090	-0.03035	(0.0045)***	(0.0168)**
0.095	-0.03035	(0.0045)***	(0.0168)**
0.100	-0.03035	(0.0045)***	(0.0168)**

Notes: This table shows regression results using samples constructed from the full matching specification. The dependent variable is the natural log of motor vehicle traffic congestion for each 30-minute interval in our sample defined at the block group level. Each row indicates the size of the caliper used to match observations in generating the samples. Caliper size is measured in standard deviations of the estimated propensity scores. All models include hour-of-day, month-of-year, and station fixed effects. Standard errors presented in parentheses are clustered on time (30-minute interval) or two-way clustered at the time and station level. *, **, and *** indicate statistical significance at the 0.1, 0.05, and 0.01 level, respectively.