# The impact of bike-share on real-estate transaction prices

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**Abstract**

Bike-share programs have been introduced in more than two thousand cities around the world, but little is know about their impact on cities. Given their potential to act as an local amenity (providing new transport options to commuters), agents may increase their valuation property units nearby bike-share stations. In this paper, I test this hypothesis in New York City using the universe of real-estate transactions. I find that transactions within 150 metres of a bike-share station are sold at prices up to 5.8% higher than properties between 150 and 500 metres of the same station. This result suggests that bike-share is valued by urban dwellers and that it initiated important value creation.

**Keywords:** Real estate, Urban transportation, Cycling

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# 1 Introduction

In the past thirty years, cities around the world have invested significantly in policies to encourage cycling. Praised for its potential to reduce traffic congestion, relieve pressure on public transport and curtail air pollution, the implementation of cycling policies has been widespread. On the one hand, cities have improved and expanded their cycling infrastructure (cycling paths, bike parking, etc), making cycling safer and more convenient. On the other hand, they have also sought to make cycling more accessible by providing public bike-share schemes to their inhabitants. More than two thousand such programs now exist around the world, providing an estimated 66 million trips in North America alone in 2021.

The advent of an affordable and practical cycling option thanks to bike-share programs has the potential to significantly change the transport landscape of cities. From previous research, we know that these changes in mobility options may have important impacts on commuting patterns, neighbourhood composition, and real-estate prices [citations]. Despite their popularity in the past two decades, there is a notable scarcity of research on the impact that bike-share programs may have on cities.

In this paper, I test whether the initial roll-out of North America's largest bike-share scheme in New York City in May 2013 had an impact on real-estate transaction prices. I geocode the universe of real-estate transactions from the New York City Department of Finance and match each of them to a bike-share station opened in the first wave (i.e., between May 2013 and June 2015). I assign transactions within 150 metres of a bike-share station to the treatment group (or ring), and transactions between 150 and 500 metres to the control group. I use a two-ring difference-in-differences approach, comparing transactions in the treatment ring with those in the control ring, before and after the opening of a bike-share station, and including station fixed effects. I find that transaction prices in the treatment ring increased by 5.8% after bike-share was implemented compared to transactions in the control ring.

This study contributes to a large body of research documenting the effects of transportation on real-estate prices, which showed (in the majority of cases) a positive relationship between access to infrastructure and prices. Important contributions for urban rail include Ahlfeldt et al. (2015), Baum-Snow and Kahn (2005), Dewees (1976), Gupta et al. (2022), Heblich et al. (2020), Hess and Almeida (2007), and Zhou, Chen, et al. (2021), and estimate price premiums between 3% and 10% for properties around rail stations. The evidence on high-capacity bus lines is similar: from a 2% to up to a 8% premium for properties around bus rapid transit (BRT) systems (see Munoz-Raskin (2010) and Zhang and Yen (2020)). Highways have also been shown to have a positive effect on nearby residential property prices (see for example Cohen and Schaffner (2019) and Levkovich et al. (2016)).

Recently, the literature started investigating the impact of cycling and cycling infrastructure on real-estate prices. A first set of studies (El-Geneidy et al., 2016; Li and Joh, 2017; Qiao et al., 2021) relies on cross-sectional analyses and do not employ causal inference methods. On the other hand, Chu et al. (2021), Pelechrinis et al. (2017), Shr et al. (2022), and Zhou, Li, et al. (2022) employ quasi-experimental methods to recover the causal impact of bike-share on real-estate prices. Pelechrinis et al. (2017) uses aggregated price data at the ZIP code level for the city of Pittsburg, which arguably does not control optimally for the characteristics and location of properties. Looking at free-floating bike-share systems in China, Chu et al. (2021) and Zhou, Li, et al. (2022) find that they decrease the price premium of subway stations. Finally, Shr et al. (2022) investigate the impact of a docked bike-share system on rents in Taiwan's second largest city, focusing on six months prior and after the deployment. They find that rents increase by 2% for properties with 150 metres of bike-share stations relative to those between 150 and 500 metres. These results suggest that bike-share programs have a positive impact on real-estate prices for units in their vicinity, but say little about their potential long-term effects.

My paper contributes to this literature by using property-level transaction prices to investigate the largest bike-share program in North America over a four year period around the initial launch, which is important for a couple of reasons. First, it remains unclear if a bike-share systems in the largest and densest city in the United States has effects similar to those reported in the previous literature for Asian urban areas. Second, transport habits may be sticky and individuals may take time to adjust to new transport options: as such, one might expect that commuting changes brought by cycling policies take time to materialise into real-estate transaction prices, motivating a long-term analysis. Finally, rental and sale markets might differ in how they respond to changes in cycling policies.

These results are important in several regards. First, they show that cycling policies behave similarly to other transportation policies: they have an impact on real estate markets, even in a city where only about 1% of workers commute to work by bicycle. Second, it highlights the importance of taking into account the distributional impacts of transport policies, as they may change neighbourhood attractiveness and eventually price out some segments of the population. Third, the increase in property value due to bike-share documented here may be partially captured by the city through property taxes, which could go towards financing these investments, and improve policymakers' and voters' support for them (Gupta et al., 2022).

## 2 Data

This section describes the data used in the paper. I begin by describing the outcome data, which originates from the NYC transaction records, the primary operations performed on the data, and the sample creation process. I then transition to describing the bike-share data, and how I define control and treatment units.

## 2.1 Real-estate transaction records

To assess the impact of bike share on real-estate prices, I utilise the universe of transaction records collected by the New York City Department of Finance (NYCDF). These records report key variables such as sale price, surface area, tax lot identifiers, and building classes for every real estate transaction in NYC since 2003. I have gathered transaction records spanning from January 2011 to April 2015, and executed a series of cleaning and transformation procedures to ensure the data is ready for analysis.

First, I geocode each transaction using tax lot identifiers. The NYCDF maintains its own property identifier, uniquely locating every piece of property in the city. Using their Digital Tax Map, which associates each property identifier with a geographical polygon, I determine the location of a transaction as the centroid of the polygon.[1]

As mentioned above, I concentrate on the first wave of bike-share implementation in NYC (i.e., stations opened between May 2013 and April 2014) to keep the analysis manageable. The estimation strategy is based on a two-ring approach (see the [identification strategy] section), using transactions up to 500 metres away from first-wave bike-share stations, and I retain transactions within that range (significantly reducing computational complexity). Approximately 80 thousand transactions fall within that spatial-temporal range — I outline the precise construction of the treatment variable in subsection 2.3.

Next, I retain only sales with non-zero prices and non-missing surface areas, deflate the sale price to December 2015 levels, and compute the surface area per unit.[2] I identify price outliers, first using the definition in (Gupta et al., 2022) (sale prices greater than 400 thousand dollars and less than ten million dollars), but also price-per-square-foot outliers (greater than $50 and less than 20 thousand dollars).[3] Finally, I extract building attributes (residential/commercial, elevator, condo, etc: see subsection 2.4 for the complete list of attributes) using the building class category reported for each transaction. I also compute distances to main amenities for each transaction: distance to the nearest subway entrance and bus stop,[4] and distance to the nearest park.[5]

[1] Most polygons are identified using a borough + tax block + tax lot identifier. Condos and other communal property types are uniquely identified at the borough + tax block level only, and their location is therefore the centre of the tax block.

[2] Surface area is given for the whole building, not the transacted unit: I take the surface area per building unit in order to correctly measure the size of a transacted unit.

[3] The later outlier definition is based on anecdotal evidence of top and bottom prices in NYC (see for example https://therealdeal.com/new-york/2018/03/21/these-are-nycs-most-expensive-homes-by-price-per-square-foot/ and https://www.nyrentownsell.com/blog/best-price-per-sq-feet-in-ny-to-rent-and-buy/, both accessed 2023-07-13). The range is wide by design, as its goal is to remove to most unlikely prices per square foot.

[4] Historical data on subway entrances and bus stops locations is collected by Baruch College at the City University of New York, and freely available at https://www.baruch.cuny.edu/confluence/display/geoportal/NYC+Mass+Transit+Spatial+Layers+Archive (accessed 2023-07-13).

[5] The current location of parks is provided by the New York City Parks at https://nycopendata.socrata.com/Recreation/Parks-Properties/enfh-gkve (accessed 2023-07-13). I was not able to find historical locations of parks — the assumption is that the location of new parks is not correlated with the deployment of bike-share stations, which is plausible.

The analysis presented later will concentrate on residential units excluding price and price-per-square-foot outliers, but results including outliers and commercial units are reported in the appendix.

### 2.2 Bike-share data

This paper estimates the impact of the first wave of bike-share stations on real-estate prices. I use the universe of bike-share trips[6] to identify the opening (and occasionally closing) date of each station. The first wave is defined by the first spatial extent of the system, i.e. the initial area of the city that the bike-share system covered. The bike-share system in NYC was launched in May 2013 and the subsequent spatial expansion took place in July 2015. During that period, stations opened in three different months: May and June 2013, and March 2014. Upon visual inspection, it appears that some stations were closed and others opened within the same calendar month, and very near to each other. Since the estimation strategy relies on bike-share station fixed effects (see the section 3), it is critical to identify the correct set of stations, so I match those that opened and closed within a month and within 50 metres of each other as the same station.

### 2.3 Treatment construction

As detailed later in the paper, the estimation strategy compares real-estate transactions close to a bike-share station (within 150 metres) to those further away (between 150 and 500 metres from the station), before and after the opening of the station. Each transaction thus has to be matched with one (or more, depending on the case) bike-share station. In this subsection, I detail the steps and decisions made in matching transactions to stations.

I start by computing, for each transaction, all the bike-share stations within 500 metres. Each transaction may be matched to multiple stations: indeed, it is not uncommon for a transaction to fall within multiple 500-metre rings around bike-share stations, with a median of ten stations matched and a maximum of 17. At this stage, every row in the dataset is a transaction-station pair, with as many rows per transaction as it matches bike-share stations, and includes a measure of distance to the station (between zero and 500 metres by construction). However, not all matches are valid for estimation: indeed, a transaction cannot be treated by one station and act as a control for another. I therefore select station matches according to the following algorithm:

**Case 1** The transaction matches only one station: keep that match.

**Case 2.1** The transaction matches multiple stations, and all are further than 150 metres (i.e., the transaction is always a control): keep all matches. This allows the transaction to act as a control for multiple bike-share stations.

[6] Available on the bike-share provider's website: https://citibikenyc.com/system-data (accessed 2023-07-13).

**Case 2.2** The transaction matches multiple stations, and all are within 150 metres (i.e., the transaction is always treated): keep the earliest and closest matched station (in that order). If the transaction is impacted by treatment, the first station within 150 metres likely started affecting its value first, and it is probable (if the stations within 150 metres opened at the same time) that the closest one has the most impact.

**Case 2.3** The transaction matches multiple stations, some of them within 150 metres, some of them between 150 and 500 metres: keep the station (1) within 150 metres, (2) opened earliest, and (3) closest (in that order). Once treated, a station should not be considered a control (it would violate SUTVA), and the station matches between 150 and 500 metres are discarded. If there are multiple stations within 150 metres, the same criteria as in Case 2.2 are used.[7]


Figure 1: Treatment construction illustration. The green transactions (i.e., within the 150-metre, yellow ring) are treated, while the red ones are controls (500-metre, grey ring). Upward triangles are transactions matched to bike-share station 54, while downward triangles are transactions matched to station 21. As described in the algorithm, some stations (diamond shape) are used as controls by both stations.

By allowing for multiple matches as described above (and after cleaning transactions as outlined earlier in subsection 2.1), I end up with about 20 thousand transaction-station

[7]Note that cases 2.2 and 2.3 imply that I do not exploit the potential cumulative effect of multiple bike-share stations in the vicinity. While potentially important, accounting for repeated or cumulative treatment is not straightforward in practice.

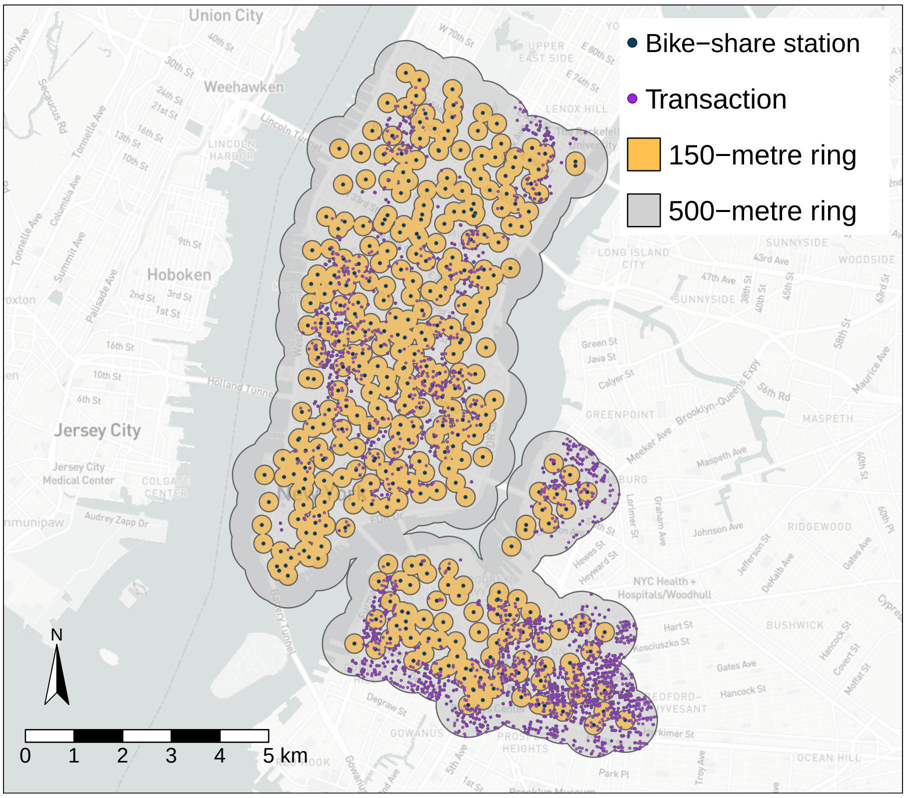


Figure 2: Overview of the study area, treatment and control areas, and location of transactions. Transactions are represented by purple points. The yellow area represents the treatment rings (i.e., within 150 metres of a bike-share station), while the grey area represents the controls rings (i.e., between 150 and 500 metres from a bike-share station). Note that for clarity, I merged the overlapping areas into a single area: in practice, each stations has its own individual treatment and control ring (see figure B.1 for a detailed map).

pairs. Transactions which are within 150 metres of their matched stations are coded as within the treatment ring (those who are between 150 and 500 metres are coded as belonging to the control ring), and transactions taking place after the opening of their matched station are coded as post-period (those before as pre-period). As detailed below, the treatment effect will be identified by the interaction between the treatment ring and post-period dummies.

## 2.4 Descriptive statistics

Here I report summary statistics for the variables used in the estimation, as well as balance tables by treated vs control rings before the opening of bike-share.

Table 1: Summary statistics, numeric variables

| | Mean | SD | Min | Median | Max | Miss |
|---|---|---|---|---|---|---|
| Sale price (2015 $) | 3,138,831 | 2,419,373 | 400,920.9 | 2,430,962 | 1e+07 | 0 |
| Log sale price (2015 $) | 14.63 | 0.86 | 12.9 | 14.7 | 16.12 | 0 |
| Sale price per sqft (2015 $) | 3,501.5 | 3,692.28 | 112.86 | 1,955.51 | 19,761.28 | 0 |
| Residential units (count) | 8.42 | 45.2 | 0 | 3 | 1,681 | 0 |
| Commercial units (count) | 0.42 | 0.85 | 0 | 0 | 15 | 0 |
| Total units (count) | 8.84 | 45.4 | 0 | 3 | 1,684 | 0 |
| Built surface (sqft) | 7,425.46 | 35,257.67 | 0 | 3,680 | 1,231,250 | 0 |
| Land surface (sqft) | 2,463.39 | 7,666.63 | 0 | 2,000 | 298,550 | 0 |
| Final surface (sqft) | 7,464.32 | 35,259.12 | 680 | 3,712 | 1,231,250 | 0 |
| Surface per unit (sqft) | 1,258.99 | 903.44 | 191.62 | 1,003 | 9,155 | 0 |
| Building age | 99.51 | 27.3 | 0 | 110 | 217 | 3 |
| Year built | 1,913.32 | 27.28 | 1,798 | 1,901 | 2,015 | 3 |
| Distance to bus stop (m) | 100.45 | 61.07 | 7.57 | 91.28 | 479.44 | 0 |
| Distance to subway entrance (m) | 293.28 | 166.4 | 12.08 | 264.06 | 1,073.82 | 0 |
| Distance to bike-share station (m) | 307.13 | 129.75 | 2.09 | 324.61 | 499.93 | 0 |
| Distance to park (m) | 347.12 | 222.98 | 2.51 | 303.73 | 1,137.15 | 0 |
| Sale quarter | 9.78 | 4.95 | 1 | 10 | 18 | 0 |

Table 2: Balance table treated vs control ring, numeric variables, pre-treatment period

| | Control ring 0 (N=3871) | | Treated ring 1 (N=669) | | | |
|---|---|---|---|---|---|---|
| | Mean | Std. Dev. | Mean | Std. Dev. | Diff. in Means | p-value |
| Sale price (2015 $) | 2,971,322.44 | 2,348,748.57 | 3,309,722.13 | 2,407,715.54 | 338,399.69*** | 0.00 |
| Log sale price (2015 $) | 14.56 | 0.87 | 14.70 | 0.85 | 0.14*** | 0.00 |
| Sale price per sqft (2015 $) | 3,484.51 | 3,628.14 | 3,759.74 | 3,955.44 | 275.23* | 0.09 |
| Residential units (count) | 11.10 | 64.93 | 8.12 | 24.06 | -2.97** | 0.03 |
| Commercial units (count) | 0.47 | 0.90 | 0.55 | 1.05 | 0.08* | 0.08 |
| Total units (count) | 11.57 | 65.14 | 8.67 | 24.69 | -2.90** | 0.04 |
| Built surface (sqft) | 9,369.99 | 49,368.65 | 7,619.01 | 24,514.66 | -1,750.98 | 0.16 |
| Land surface (sqft) | 2,831.98 | 11,061.97 | 2,262.38 | 2,144.94 | -569.60*** | 0.00 |
| Final surface (sqft) | 9,451.63 | 49,368.31 | 7,625.89 | 24,512.91 | -1,825.74 | 0.14 |
| Surface per unit (sqft) | 1,218.60 | 888.58 | 1,272.60 | 917.00 | 54.00 | 0.16 |
| Building age | 98.83 | 25.17 | 101.43 | 23.54 | 2.60*** | 0.01 |
| Year built | 1,912.99 | 25.11 | 1,910.40 | 23.49 | -2.59*** | 0.01 |
| Distance to bus stop (m) | 103.74 | 60.57 | 92.49 | 49.11 | -11.25*** | 0.00 |
| Distance to subway entrance (m) | 291.77 | 159.61 | 297.35 | 202.16 | 5.58 | 0.50 |
| Distance to bike-share station (m) | 345.16 | 100.95 | 98.80 | 34.40 | -246.36*** | 0.00 |
| Distance to park (m) | 337.13 | 215.64 | 311.22 | 206.45 | -25.91*** | 0.00 |
| Sale quarter | 5.78 | 2.73 | 5.76 | 2.71 | -0.02 | 0.84 |

*Note*: Significance codes: *: 0.1, **: 0.05, ***: 0.01.

Table 3: Balance table treated vs control ring, categorical variables, whole sample period

| | | Control ring 0 (N=7245) | | Treated ring 1 (N=1370) | | Total | |
|---|---|---|---|---|---|---|---|
| | | N | Pct. | N | Pct. | N | Pct. |
| Post-period | 0 | 3871 | 53.4 | 669 | 48.8 | 4540 | 52.7 |
| | 1 | 3374 | 46.6 | 701 | 51.2 | 4075 | 47.3 |
| Treated (treatment ring × post) | 0 | 7245 | 100.0 | 669 | 48.8 | 7914 | 91.9 |
| | 1 | 0 | 0.0 | 701 | 51.2 | 701 | 8.1 |
| Elevator | 0 | 7066 | 97.5 | 1314 | 95.9 | 8380 | 97.3 |
| | 1 | 179 | 2.5 | 56 | 4.1 | 235 | 2.7 |
| Walkup | 0 | 4832 | 66.7 | 877 | 64.0 | 5709 | 66.3 |
| | 1 | 2413 | 33.3 | 493 | 36.0 | 2906 | 33.7 |
| Condo | 0 | 7195 | 99.3 | 1364 | 99.6 | 8559 | 99.3 |
| | 1 | 50 | 0.7 | 6 | 0.4 | 56 | 0.7 |
| Coop | 0 | 7130 | 98.4 | 1354 | 98.8 | 8484 | 98.5 |
| | 1 | 115 | 1.6 | 16 | 1.2 | 131 | 1.5 |
| Rental | 0 | 4048 | 55.9 | 676 | 49.3 | 4724 | 54.8 |
| | 1 | 3197 | 44.1 | 694 | 50.7 | 3891 | 45.2 |

Table 4: Balance table treated vs control ring, building class categories, whole sample period

| | Control ring 0 (N=7245) | | Treated ring 1 (N=1370) | | Total | |
|---|---|---|---|---|---|---|
| Building class category | N | Pct. | N | Pct. | N | Pct. |
| 01 One Family Dwellings | 789 | 10.9 | 162 | 11.8 | 951 | 11.0 |
| 02 Two Family Dwellings | 1955 | 27.0 | 317 | 23.1 | 2272 | 26.4 |
| 03 Three Family Dwellings | 1180 | 16.3 | 179 | 13.1 | 1359 | 15.8 |
| 07 Rentals - Walkup Apartments | 2391 | 33.0 | 488 | 35.6 | 2879 | 33.4 |
| 08 Rentals - Elevator Apartments | 117 | 1.6 | 46 | 3.4 | 163 | 1.9 |
| 09 Coops - Walkup Apartments | 22 | 0.3 | 5 | 0.4 | 27 | 0.3 |
| 10 Coops - Elevator Apartments | 53 | 0.7 | 8 | 0.6 | 61 | 0.7 |
| 11a Condo-Rentals | 1 | 0.0 | 1 | 0.1 | 2 | 0.0 |
| 13 Condos - Elevator Apartments | 9 | 0.1 | 2 | 0.1 | 11 | 0.1 |
| 14 Rentals - 4-10 Unit | 688 | 9.5 | 159 | 11.6 | 847 | 9.8 |
| 17 Condo Coops | 40 | 0.6 | 3 | 0.2 | 43 | 0.5 |

## 3 Empirical strategy

Identifying the impact of bike-share on real-estate transaction prices is lined with several empirical challenges. First, transaction prices are determined by a multitude of well-known factors (size, unit attributes, distance to amenities, etc.). Second, other dynamics might be at play that could determine the evolution of real-estate prices concurrently to bike-share stations. In this section, I discuss these challenges and how I address them with my estimation strategy.

The primary obstacle to determining the causal relationship between bike share and real-estate transaction prices is the non-random placement of bike share stations throughout the city. This deliberate selection process is logical from a city planning perspective, as it aims to ensure the success of the bike share program by targeting areas with a significant transportation market. In the case of NYC, this resulted in the initial deployment of bike share stations in Manhattan south of 60th Street and downtown Brooklyn.

The second challenge lies in the vast and diverse nature of NYC, which is continually evolving. Each year, numerous policies are implemented that may impact real-estate transaction prices, potentially confounding the estimation of the bike share's impact.

I address these challenges using a two-ring difference-in-differences (TRDD) strategy. The empirical strategy compares transactions taking place close to bike-share stations (inner or treatment ring) to those further away (outer or control ring), before and after the first deployment of bike-share. The two-ring construction identifies treatment (ring from zero to 150 metres around the station) and control (ring from 150 to 500 metres) transactions for each bike-share station, and estimates the difference between treatment and control transactions after bike-share implementation, net of station and time fixed effects.

The TRDD addresses the concerns around the identification by carefully selecting a credible control group of transactions (the outer ring). By focusing on potential impacts at a very small geographical scale, this identification strategy gives more ex-ante credibility to the parallel trends assumption required by difference-in-differences: everything else equal, it is reasonable to assume that transactions in the control and treatment ring follow similar sale-price trends in the absence of bikes share. I will provide evidence of this in later sections using an event study design.

The second main assumption of difference-in-differences is that no other concomitant policy that could have affected sale prices was enacted at the same time and place where bike-share was rolled out. I address this assumption by narrowing down the sample period to four and a half years (January 2011 to June 2015), which should limit the number of potential policies that might affect the estimates. Moreover, to be a significant threat to identification, other policies would have to exactly follow the spatial and temporal pattern of bike-share stations, which I control for to the best of my abilities.

The econometric specification used to estimate the TRDD is described by the following equation:

$$\begin{aligned} ln(P_{ijct}) =&\delta_{<150}D_{ij}^{<150} \times Post_{ijt} + \lambda_{<150}D_{ij}^{<150} + \beta' X_{it} + \gamma' Z_{it} \\ &+ \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct}, \end{aligned} \tag{1}$$

where $P_{ijct}$ is the real sale price (base December 2015) of transaction $i$, matched to bike-share station $j$, at time $t$; $D < 150_{ij}$ is a dummy variable which equals one if unit $i$ is within 150 metres of station $j$ (i.e., in the treatment ring), and zero otherwise (i.e., in the control ring); $Post_{ijt}$ is a dummy variable indicating whether a unit is transacted after the opening of bike-share station $j$; $X_{it}$ is a vector of unit attributes (elevator, building age, etc); $Z_{it}$ is a vector of distances to nearby (dis)amenities (subway station, bus stop); $\kappa_j$ and $\tau_t$ are station and year-month fixed effects, respectively; $\kappa_j \times t$ are station-specific linear time trends (some specifications); $\varepsilon_{ijct}$ standard errors clustered at station level.

The coefficient of interest is $\delta_{<150}$, which represents the average treatment effect of bike-share on the treated transactions. In practice, it is the average change in log sales for a transaction within the treatment ring of a bike-share station after the opening of the station. Alternatively, I replace the treatment ring dummy $D < 150_{ij}$ by a continuous measure of distance (in hundreds of metres) to the matched bike-share station $D_{ij}$. The coefficient now reports, for a transaction, the average effect (in percent) of being 100 metres further from its matched bike-share station.

Finally, I also run a dynamic TWFE model in order to investigate the dynamic effect of bike-share with respect to the timing of treatment. The dynamic DD specification, also known as event study, plots the treatment effect for all periods. The dynamic specification also allows us to test for differential pretends between groups: by plotting the difference between treatment and control in the pre-treatment period, we will be able to evaluate the validity of the parallel trends assumption.

The specification for the dynamic DD is given in equation 2:

$$\begin{aligned} ln(P_{ijct}) = &\sum_{k=-6}^{-2} \beta_k \cdot D_{ij}^{<150} + \sum_{k=0}^{6} \beta_k \cdot D_{ij}^{<150} + \beta' X_{it} + \gamma' Z_{it} \\ &+ \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct}, \end{aligned} \tag{2}$$

where $k$ denotes the relative time to the first year of treatment, the other terms being the same as in the previous specification. The coefficients of interest are $\beta_k$, which are then plotted against relative time. In this setting, the reference period is relative time $k = -1$, therefore the plotted $\beta_k$s denote the relative difference between treatment and control groups compared to the period right before treatment.

# 4 Results

This section presents the results of the analysis. First, I report the results for the hedonic regressions, then the impact of the opening of bike-share stations on sale prices, and additional results using the distance to the station as the treatment variable.

Columns 1 to 3 of Table 5 display the results for the hedonic model. The goal of the hedonic model is to predict the sale price with unit and building attributes as accurately as possible. Good predictions from the hedonic model serve as both (1) a check that the data is sound and reliable (behaves predictably), and (2) a test of whether the unit attributes are correctly specified and constructed.

In Column 1 of Table 5, I regress the log sale price (in real terms, based on December 2015 prices) on a unit's surface area, building age, and distances to bus stops, subway entrances, and the nearest park. Most of the independent variables have the expected sign: the larger a unit's surface, the closer to a subway station and a park, the more expensive it is. Two variables seem to behave less obviously: the closer to a bus stop and the younger a building, the lower the transaction price. This, however, may make sense: everything else being equal, being closer to a bus stop probably means being closer to busy roads, a disamenity. Older buildings are associated with cachet and prestige, or might be located in areas where architecture was preserved, which may be associated with higher transaction prices.

In Column 2, I introduce bike-share station and year-quarter fixed effects, and allow the slope of year-quarter to vary across stations. From section 2, you will recall that each transaction is matched with at least one bike-share station (multiple if the transaction only falls into control rings). Introducing station and time fixed effects controls for space-specific and time-specific unobservable factors. The coefficient of unit surface remains stable, while the magnitude of distances to subway and parks decreases. Interestingly, building age is not a statistically significant predictor of transaction price. Adding building class categories in effect compares transactions within the same category, which might share common, unobservable traits associated with price. I do that in Column 3, and the magnitude of unit surface increases: every additional hundred square feet increase the price by 2.4%. The magnitude and significance of the other explanatory variables remain stable.

Moving on to Columns 4 to 6, I introduce the treatment and post-period dummies, our main coefficients of interest. There are no controls in Column 4, and the model is estimated with bike-share station and year-quarter fixed effects, allowing each station to have time-varying slopes. The main coefficient of interest, the interaction between the treatment and post-period dummies, is positive but not statistically significant at conventional levels (p-value 0.168). Controlling for unit attributes and distance to amenities in Column 5 does not sensibly change either the magnitude or the statistical significance of the coefficient (p-value 0.148). When adding building class fixed effects, however, the coefficient turns statistically significant (p-value 0.0869). Under the model

Table 5: Treatment impact on transaction sale prices

| | Log sale price (2015 $) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Post-period | | | | -0.1617*** | -0.1759*** | -0.1595*** |
| | | | | (0.0604) | (0.0617) | (0.0583) |
| Treated ring | | | | 0.0065 | 0.0137 | -0.0175 |
| | | | | (0.0246) | (0.0234) | (0.0241) |
| Treated ring × Post-period | | | | 0.0481 | 0.0499 | 0.0581* |
| | | | | (0.0349) | (0.0345) | (0.0338) |
| Surface per unit (100s sqft) | 0.0089*** | 0.0092*** | 0.0244*** | | 0.0091*** | 0.0243*** |
| | (0.0015) | (0.0009) | (0.0011) | | (0.0009) | (0.0011) |
| Building age (10s years) | 0.0142** | -0.0029 | -0.0017 | | -0.0032 | -0.0019 |
| | (0.0056) | (0.0036) | (0.0026) | | (0.0036) | (0.0026) |
| Distance to bus stop (100s m) | 0.0716*** | 0.0891*** | 0.0910*** | | 0.0921*** | 0.0927*** |
| | (0.0189) | (0.0162) | (0.0153) | | (0.0166) | (0.0156) |
| Distance to subway (100s m) | -0.1046*** | -0.0378*** | -0.0328*** | | -0.0374*** | -0.0327*** |
| | (0.0165) | (0.0073) | (0.0069) | | (0.0073) | (0.0069) |
| Distance to park (100s m) | -0.1634*** | -0.0379*** | -0.0307*** | | -0.0386*** | -0.0312*** |
| | (0.0119) | (0.0073) | (0.0070) | | (0.0074) | (0.0071) |
| Bike-share station FE | | Yes | Yes | Yes | Yes | Yes |
| Sale year-quarter FE | | Yes | Yes | Yes | Yes | Yes |
| Building class category FE | | | Yes | | | Yes |
| *Varying Slopes* | | | | | | |
| Sale year-quarter (Bike-share station) | | Yes | Yes | Yes | Yes | Yes |
| Mean outcome pre-period | 3,022,065 | 3,022,065 | 3,022,065 | 3,021,188 | 3,022,065 | 3,022,065 |
| Observations | 8,612 | 8,612 | 8,612 | 8,615 | 8,612 | 8,612 |
| $R^2$ | 0.234 | 0.637 | 0.680 | 0.622 | 0.638 | 0.681 |
| Within $R^2$ | | 0.041 | 0.088 | 0.002 | 0.043 | 0.090 |
| RMSE | 0.753 | 0.519 | 0.487 | 0.529 | 0.518 | 0.486 |

*Note*: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the bike-share station level.

in Column 6, the impact of having a bike-share station within 150 metres of a property increases its transaction price by 5.8% with respect to property transacted between 150 and 500 metres of the same bike-share station. By adding building class category fixed effects, we effectively compare transactions within the same building category, within the same bike-share station 500-metre radius, and within a given year-quarter, controlling for unobservable common factors within each of these dimensions. Since building categories within a given area might share a lot of characteristics in common, which are unobservable with the current data, comparing within building class might thus be important to remove heterogeneity and sharpen the estimation of our coefficient of interest. Moreover, the coefficient for the treated ring is statistically insignificant, indicating that bike-share stations are not placed in more ex-ante expensive areas with respect to their entire 500-metre radius.

The data would allow for more disaggregated fixed effects, using for example year-month of sale (instead of year-quarter), or building class (instead of building class category). While the appeal of employing more detailed fixed effects is clear, it is important to note that they also bring their own set of disadvantages. In particular, allowing for finer levels of fixed effects would greatly reduce the number of observations used in the estimation. The model would compare observations within each fixed-effect

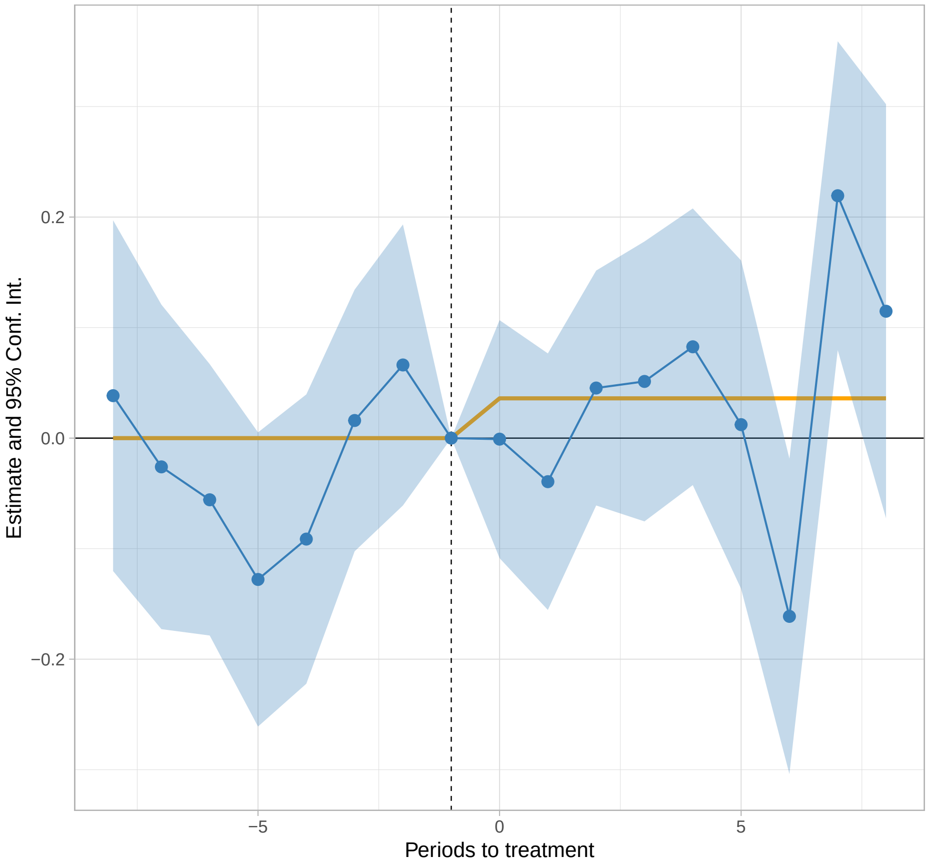


Figure 3: Dynamic effect of treatment on transaction price (event study). The shaded area indicates the coefficients' 95% confidence intervals. The orange line represents the post-period aggregated effect.

"cell", i.e., it would only compare, with each other, the observations that share the same values across all fixed effects. As we introduce more granular fixed effects, it becomes increasingly likely that a given observation (transaction) is unique in a given fixed-effect cell, and has no other observation to be compared with. In this case, the estimation ignores that observation and the coefficients are estimated using fixed-effect cells with at least two observations.[8] Therefore, I decide not to introduce more granular fixed effects, as those would restrict the number of observations used by the estimation.[9]

I check the consistency of this result in Table 6, Columns 3 to 5. Replacing the binary treatment variable with a measure of distance to the matched bike-share station, I find that every additional hundred metres away from the station reduces transaction prices by 2.1% (preferred specification, Column 5). This is in line with the result obtained with the dichotomous variable, and shows there exists a downward-sloping gradient of

[8]It also follows that the model may underestimate the standard errors of the estimated coefficients, which would lead us to over-reject the null hypothesis.

[9]With the current set of fixed effects as in column 6, there are about five thousand observations (out of eight thousand) that belong to a fixed-effect cell with more than two observations. Introducing building class and year-month fixed effects lowers the usable observations down to two thousand.

Table 6: Additional specifications and continuous treatment

| | Log sale price (2015 $) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Post-period | 0.0870*** | 0.1332*** | -0.0938 | -0.1047 | -0.0843 |
| | (0.0253) | (0.0243) | (0.0693) | (0.0693) | (0.0645) |
| Treated ring | 0.1408*** | 0.1050** | | | |
| | (0.0491) | (0.0416) | | | |
| Treated ring × Post-period | 0.0532 | 0.0548 | | | |
| | (0.0472) | (0.0403) | | | |
| Distance to bike-share station (100s m) | | | 0.0025 | 0.0002 | 0.0066 |
| | | | (0.0084) | (0.0080) | (0.0078) |
| Distance to bike-share station (100s m) × Post-period | | | -0.0191* | -0.0200* | -0.0213** |
| | | | (0.0113) | (0.0111) | (0.0102) |
| Surface per unit (100s sqft) | | 0.0084*** | | 0.0091*** | 0.0243*** |
| | | (0.0015) | | (0.0009) | (0.0011) |
| Building age (10s years) | | 0.0130** | | -0.0030 | -0.0018 |
| | | (0.0056) | | (0.0036) | (0.0026) |
| Distance to bus stop (100s m) | | 0.0810*** | | 0.0917*** | 0.0928*** |
| | | (0.0187) | | (0.0167) | (0.0158) |
| Distance to subway (100s m) | | -0.1059*** | | -0.0372*** | -0.0326*** |
| | | (0.0168) | | (0.0072) | (0.0069) |
| Distance to park (100s m) | | -0.1644*** | | -0.0391*** | -0.0314*** |
| | | (0.0120) | | (0.0075) | (0.0072) |
| Bike-share station FE | | | Yes | Yes | Yes |
| Sale year-quarter FE | | | Yes | Yes | Yes |
| Building class category FE | | | | | Yes |
| *Varying Slopes* | | | | | |
| Sale year-quarter (Bike-share station) | | | Yes | Yes | Yes |
| Mean outcome pre-period | 3,021,188 | 3,022,065 | 3,021,188 | 3,022,065 | 3,022,065 |
| Observations | 8,615 | 8,612 | 8,615 | 8,612 | 8,612 |
| $R^2$ | 0.009 | 0.244 | 0.622 | 0.638 | 0.681 |
| Within $R^2$ | | | 0.002 | 0.043 | 0.090 |
| RMSE | 0.857 | 0.748 | 0.529 | 0.518 | 0.486 |

*Note*: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the bike-share station level.

transaction prices around bike-share stations.

## 4.1 Discussion

The results displayed above indicate that the introduction of bike-share stations had a positive impact on residential real-estate transaction prices. Properties within 150 metres of a station saw their transaction price increase by 5.8% on average, compared to property transactions further away (between 150 and 500 metres), and within a given building class category. Moreover, every additional hundred metres away from a bike-share station reduces transaction price by 2.1% on average, compared to before the opening of the system.

These impacts on real-estate transaction prices are relatively significant, but within the range we would expect from this type of intervention (see [citations]). Of particular interest to us are the estimated effects of bike-share on rents determined by Shr et al. for Taiwan. They find that six months after the opening of bike-share, rents increase on average by 1.71% for units within 150 metres of stations. While of the same order of magnitude, my results are relatively larger. [Should I try to give some ex-post rationalisations?].

[Should I discuss the limited statistical significance of the main coefficient more in-depth?]

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# A Building decades

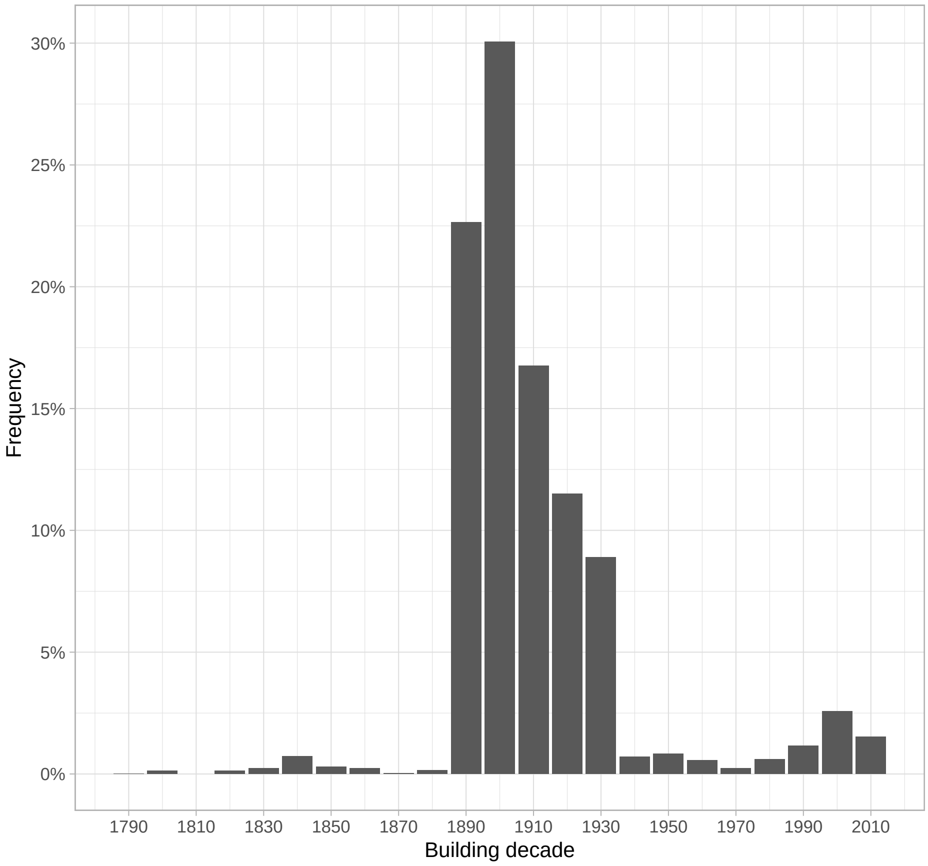


Figure A.1: Building decade of transactions in main sample data set (i.e., residential units, no price and price-per-square-feet outliers).

# B Detailed treatment map

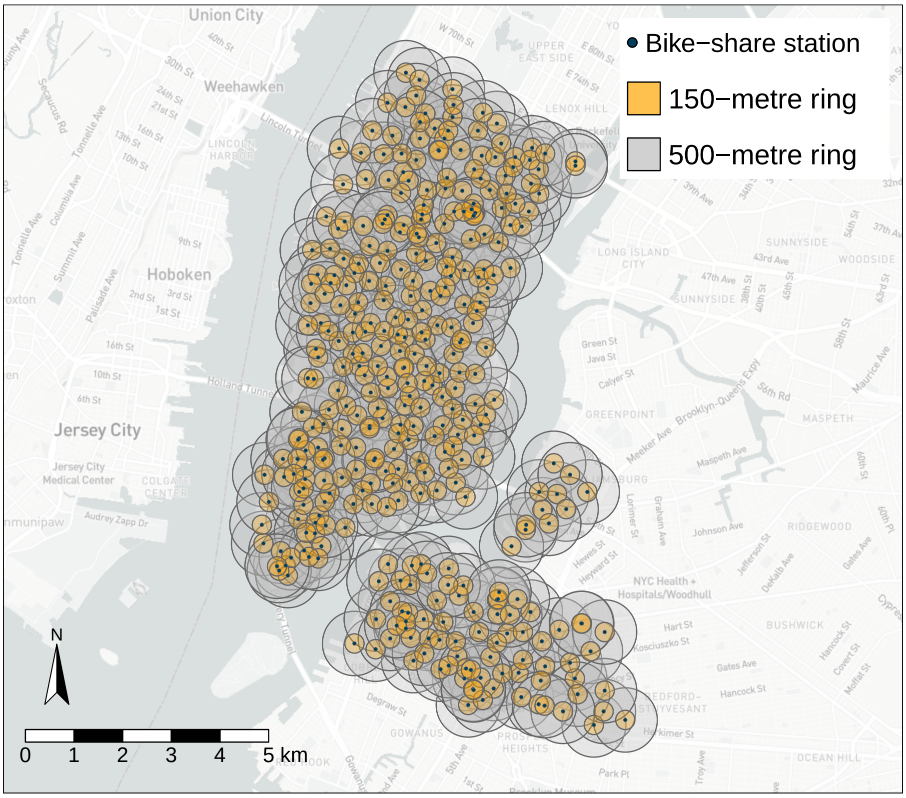


Figure B.1: Overview of the study area and treatment and control rings. The yellow areas represent the treatment rings (i.e., within 150 metres of a bike-share station), while the grey areas indicate the controls rings (i.e., between 150 and 500 metres from a bike-share station).