The impact of bike-share on real-estate transaction prices in New York City*

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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 6.1% higher than properties between 150 and 500 metres of the same station, or $185,755 higher for an average transaction. 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. 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 6.1% after bike-share was implemented compared to transactions in the control ring, which represents an increase of $185,755 for an average transaction price.

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 Dewees (1976), Baum-Snow and Kahn (2005), Hess and Almeida (2007), Ahlfeldt et al. (2015), Heblich et al. (2020), Zhou, Chen, et al. (2021), and Gupta et al. (2022), 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 an 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 Levkovich et al. (2016) and Cohen and Schaffner (2019)).

Recently, the literature started investigating the impact of cycling and cycling infrastructure on real-estate prices. One set of studies (El-Geneidy et al., 2016; Li and Joh, 2017; Qiao et al., 2021) rely on cross-sectional analyses and do not employ causal inference methods. On the other hand, Pelechrinis et al. (2017), Chu et al. (2021), Zhou, Li, et al. (2022), and Shr, Yang, 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, Yang, 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 to and after the deployment. They find that rents increase by 1.7% 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 system 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 medium-term (multi-year) analysis, which has not been done in previous studies. Finally, rental and sale markets might differ in how they respond to changes in cycling policies. In order to make the comparison with previous studies easy, I build upon the treatment definitions and estimation strategy set forth in Shr, Hsu, et al. (2023).

The results of this paper 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

[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.

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]

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, a total of 360 [check] 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

[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.

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

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.

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]

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 pairs [check]. 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). Figure 1 illustrates how treatment and control status is attributed, and how transactions may act as controls for multiple bike-share stations. Figure 2 provides an overview of treated and control areas and transactions. As detailed below, the treatment effect will be identified by the interaction between the treatment ring and post-period dummies.

[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 1: Treatment construction illustration. *Notes*: The green symbols represent treated transactions (i.e., within the 150-metre, yellow ring), while the red symbols 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 are used as controls by both stations: those are represented by a diamond shape.

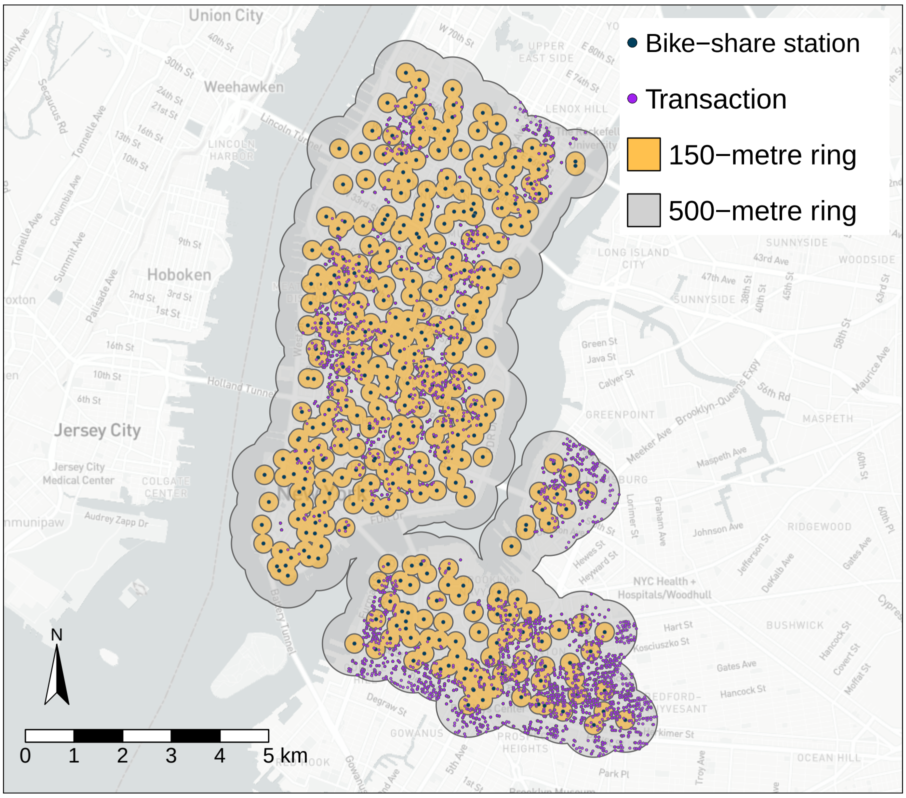

Figure 2: Overview of the study area, treatment and control areas, and location of transactions. *Notes*: Transactions are represented by purple points. The yellow area represents the treatment rings (i.e., within 150 metres of a bike-share station), while the grey area represents the control rings (i.e.,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

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. [should I show both full sample and within sample area?]

Table 1: Summary statistics, numeric variables

	Mean	SD	Min	Median	Max	Miss
Sale price (2015 $)	1,183,105	1,455,369	4e+05	673,843.3	9,983,196	0
Log sale price (2015 $)	13.64	0.7	12.9	13.42	16.12	0
Sale price per sqft (2015 $)	1,171.24	1,948.77	51.11	563.3	19,905.42	0
Residential units (count)	3.95	17.11	0	2	1,681	0
Commercial units (count)	0.13	0.53	0	0	15	0
Total units (count)	4.08	17.23	0	2	1,684	0
Built surface (sqft)	4,073.58	14,206.07	0	2,400	1,231,250	0
Land surface (sqft)	3,258.42	4,123.63	0	2,500	382,704	0
Final surface (sqft)	4,094.56	14,207.76	300	2,400	1,231,250	0
Surface per unit (sqft)	1,348.77	760.42	132	1,176	31,494	0
Building age	76.32	31.25	0	84	217	8
Year built	1,936.65	31.23	1,798	1,930	2,015	0
Distance to bus stop (m)	2,348.91	3,816.24	6.72	299.55	21,983.22	0
Distance to subway entrance (m)	2,213.29	3,738.27	5.47	626.58	22,533.2	0
Distance to bike-share station (m)	308.82	129.51	2.09	327.57	499.93	65795
Distance to park (m)	459.12	309.77	0	399.12	2,308.86	0
Sale quarter	10.3	5.08	1	11	18	0

Notes: The sample reported here includes transactions outside the sample areas of bike-share stations (i.e., beyond 500 metres).

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

	Control ring 0 (N=4014)		Treated ring 1 (N=679)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p-value
Sale price (2015 $)	2,992,981.78	2,349,601.20	3,319,213.45	2,414,041.15	326,231.66***	0.00
Log sale price (2015 $)	14.57	0.87	14.70	0.85	0.13***	0.00
Sale price per sqft (2015 $)	3,503.77	3,632.46	3,778.58	3,972.99	274.81*	0.09
Residential units (count)	10.98	63.92	8.13	23.91	-2.85**	0.04
Commercial units (count)	0.47	0.89	0.54	1.05	0.07*	0.08
Total units (count)	11.45	64.13	8.68	24.54	-2.78**	0.04
Built surface (sqft)	9,283.55	48,620.97	7,600.34	24,341.75	-1,683.21	0.16
Land surface (sqft)	2,809.18	10,872.52	2,260.12	2,132.94	-549.06***	0.00
Final surface (sqft)	9,360.91	48,620.97	7,607.12	24,340.01	-1,753.78	0.15
Surface per unit (sqft)	1,220.12	888.96	1,270.99	917.76	50.87	0.18
Building age	98.81	25.19	101.41	23.73	2.60***	0.01
Year built	1,913.01	25.12	1,910.42	23.68	-2.59***	0.01
Distance to bus stop (m)	103.89	61.26	93.18	49.87	-10.70***	0.00
Distance to subway entrance (m)	291.07	159.62	295.86	201.74	4.79	0.56
Distance to bike-share station (m)	346.00	100.78	98.95	34.37	-247.04***	0.00
Distance to park (m)	334.56	213.42	309.88	205.70	-24.67***	0.00
Sale quarter	5.78	2.73	5.77	2.70	-0.01	0.92

Notes: 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=7503)		Treated ring 1 (N=1386)		Total	
		N	Pct.	N	Pct.	N	Pct.
Post-period	0	4014	53.5	679	49.0	4693	6.3
	1	3489	46.5	707	51.0	4196	5.6
Treated (treatment ring × post)	0	7503	100.0	679	49.0	8182	11.0
	1	0	0.0	707	51.0	707	0.9
Elevator	0	7312	97.5	1329	95.9	73696	98.7
	1	191	2.5	57	4.1	988	1.3
Walkup	0	5000	66.6	886	63.9	64588	86.5
	1	2503	33.4	500	36.1	10096	13.5
Condo	0	7453	99.3	1380	99.6	74615	99.9
	1	50	0.7	6	0.4	69	0.1
Coop	0	7383	98.4	1370	98.8	74503	99.8
	1	120	1.6	16	1.2	181	0.2
Rental	0	4183	55.8	682	49.2	61580	82.5
	1	3320	44.2	704	50.8	13104	17.6

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

	Control ring 0 (N=7503)		Treated ring 1 (N=1386)		Total	
Building class category	N	Pct.	N	Pct.	N	Pct.
01 One Family Dwellings	833	11.1	164	11.8	25530	34.2
02 Two Family Dwellings	2018	26.9	320	23.1	27599	37.0
03 Three Family Dwellings	1203	16.0	180	13.0	8259	11.1
07 Rentals - Walkup Apartments	2480	33.1	495	35.7	10042	13.4
08 Rentals - Elevator Apartments	125	1.7	47	3.4	896	1.2
09 Coops - Walkup Apartments	23	0.3	5	0.4	54	0.1
10 Coops - Elevator Apartments	57	0.8	8	0.6	81	0.1
11a Condo-Rentals	1	0.0	1	0.1	12	0.0
13 Condos - Elevator Apartments	9	0.1	2	0.1	11	0.0
14 Rentals - 4-10 Unit	714	9.5	161	11.6	2154	2.9
17 Condo Coops	40	0.5	3	0.2	46	0.1

2.5 Descriptive evidence

Before presenting the empirical strategy and the results of the statistical analysis, I present in this section descriptive evidence that may point towards bike-share having an impact on property prices. In figure 3, I plot the results of a local polynomial regression of sale prices on periods to treatment (in months), reminiscent of an event study. Prior to treatment, treated and control transactions show relatively similar price trends, while after treatment the treated group displays a relatively higher level. While not a statistical test, figure 3 motivates the analysis to follow.

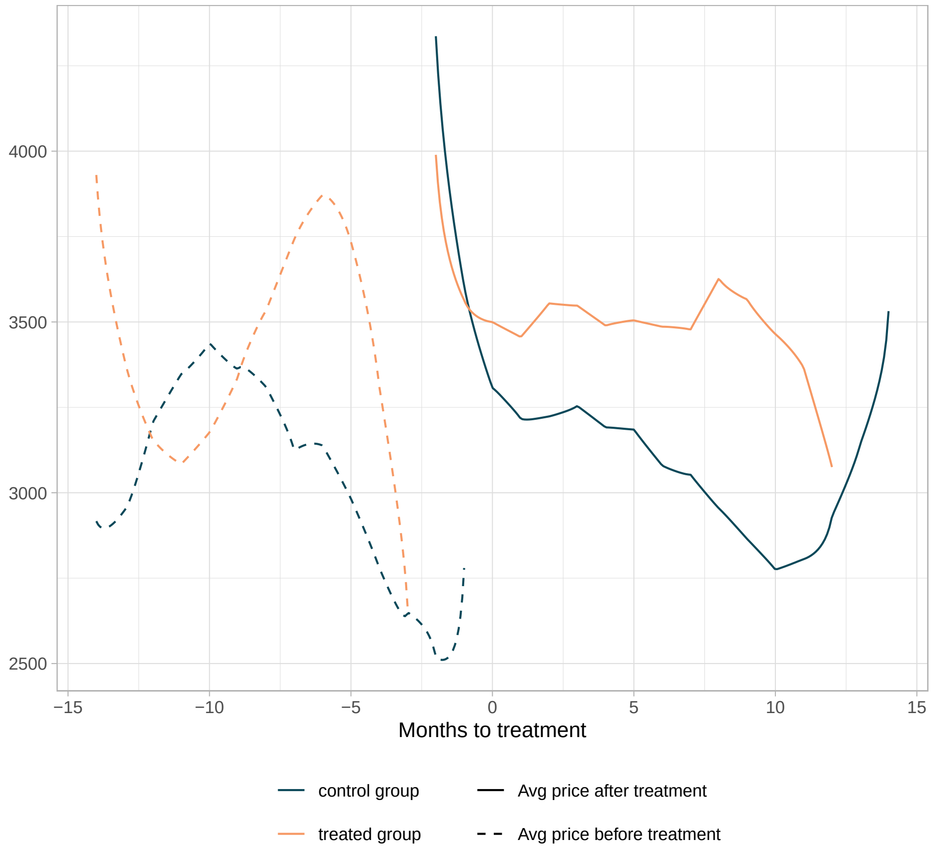

Figure 3: Local polynomial regression of sales prices on periods to treatment. *Notes*: Periods to treatment are given in months. The dashed lines are pre-treatment, and solid lines post-treatment results of a local polynomial regression of sales prices on periods to treatment. Yellow lines identify the treated transactions (i.e., within 150 metres of a bike-share station), blue ones the control group (i.e., between 150 and 500 metres of a bike-share station). The local polynomial regressions use an Epanechnikov kernel and three-month bandwidth.

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. These primary treatment definitions and this estimation strategy build upon the ones outlined in Shr, Hsu, et al. (2023).

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.

To explore the impact of bike-share on sale prices, I specify the following model, which uses the universe of transactions and controls for within bike-share-station sample areas (or ring; less than 500 metres away) and within bike-share-station treatment areas (or ring; less than 150 metres away). This specification is formalised in the following equation:

$$
\begin{aligned}
ln(P_{ijct}) = {} & \delta_{<150} D_{ij}^{<150} \times Post_{ijt} + \lambda_{<150} D_{ij}^{<150} \\
& + \delta_{<500} D_{ij}^{<500} \times Post_{ijt} + \lambda_{<500} D_{ij}^{<500} + \beta' X_{it} + \gamma' Z_{it} \\
& + \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct},
\end{aligned} \tag{1}
$$

where P_{ijct} is the real sale price (base December 2015) of transaction i, matched to bike-share station j, at time t; $D_{ij}^{<150}$ is an indicator variable which is equal to 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); $D_{ij}^{<500}$ is an indicator variable similar to $D_{ij}^{<150}$ but for transactions within 500 metres of a bike-share station (i.e., the sample 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); κ_j and τ_t are station and year-month fixed effects, respectively; $\kappa_j \times t$ are station-specific linear time trends (some specifications); ε_{ijct} standard errors clustered at station level.

The econometric specification used to estimate the TRDD (i.e., when restricting the sample to transactions within 500 metres of bike-share stations) is described by this second equation:

$$
\begin{aligned}
ln(P_{ijct}) = {} & \delta_{<150} D_{ij}^{<150} \times Post_{ijt} + \lambda_{<150} D_{ij}^{<150} + \beta' X_{it} + \gamma' Z_{it} \\
& + \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct},
\end{aligned} \tag{2}
$$

The coefficient of interest in both models 1 and 2 is $\delta_{<150}$, which represents the average treatment effect of bike-share on the treated transactions. Concretely, it is the average change in log sales for a transaction within the treatment ring (i.e., within 150 metres) of a bike-share station after the opening of the station. Alternatively, I replace the treatment ring dummy $D_{ij}^{<150}$ by a continuous measure of distance (in hundreds of metres) to the matched bike-share station D_{ij}. The coefficient in this case reports, for a transaction, the average effect (in per cent) of being 100 metres further away 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 3:

$$ln(P_{ijct}) = \sum_{k=-6}^{-2} \beta_k \cdot D_{ij}^{<150} + \sum_{k=0}^{6} \beta_k \cdot D_{ij}^{<150} + \beta' X_{it} + \gamma' Z_{it} \\ + \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct}, \tag{3}$$

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 β_k, which are then plotted against relative time. In this setting, the reference period is relative time $k = -1$, therefore the plotted β_ks denote the relative difference between treatment and control groups compared to the period right before treatment. In this model, the sample is also restricted to transactions within 500 metres of bike-share stations.

4 Results

In this section, I first concentrate on residential properties and present the results of a hedonic model, regressing the sale price of transactions on unit attributes and distances to (dis)amenities. I then explore the potential treatment effects of bike-share stations on residential transaction prices. In the second subsection, I test whether bike-share stations had an impact on the sale prices of commercial properties, and check the potential heterogeneity of these effects across types of commercial properties. Finally, I discuss the results in light of previous research.

4.1 Residential properties

4.1.1 Hedonic model

I briefly present the results of the hedonic models in this subsection. The main goal of this analysis is to validate the selection of the sample of interest, the construction of the variables (unit characteristics and amenities), and the overall soundness of the real-estate data set. Moreover, showing that prices react in a sensible manner to unit attributes and amenities strengthens the credibility of the analysis of bike-share treatment effects.

The results of the hedonic model are displayed in Table 5. Columns 1 to 3 use the universe of transactions across NYC during the sample period (2011-01-01 to Jun 2015-06-30), while columns 4 to 6 restrict the sample to transactions that took place within the sample area of bike-share stations (500 metres). The depend variable for all specifications is the log sale price,[8] which is regressed on units' surface area, building age, and distances to bus stops, subway entrances, and the nearest park. From column 1 to 3 and 4 to 6, I add fixed effects that control for temporal, spatial and building-class unobservable

[8]Prices are deflated to June 2015 levels using the Consumer Price Index for New York-Newark-Jersey City from the Bureau of Labor Statistics, available at https://www.bls.gov/regions/northeast/data/xg-tables/ro2xgcpiny.htm (accessed 2023-07-26).

characteristics. Standard errors are clusters at the Neighbourhood Tabulation Areas (NTA) level for columns 1 to 3, and at the bike-share station level for columns 4 to 6.

Table 5: Hedonic model of residential transactions' sale prices

	Log sale price (2015 $)					
	(1)	(2)	(3)	(4)	(5)	(6)
Surface per unit (100s sqft)	0.0098***	0.0076***	0.0205***	0.0090***	0.0090***	0.0242***
	(0.0021)	(0.0011)	(0.0012)	(0.0015)	(0.0008)	(0.0011)
Building age (10s years)	0.0413***	-0.0146***	-0.0020	0.0155***	-0.0019	-0.0013
	(0.0082)	(0.0025)	(0.0020)	(0.0054)	(0.0035)	(0.0025)
Distance to bus stop (100s m)	-0.0024*	0.0042**	0.0024	0.0694***	0.0853***	0.0882***
	(0.0013)	(0.0021)	(0.0018)	(0.0182)	(0.0161)	(0.0151)
Distance to subway (100s m)	-0.0015	-0.0066***	-0.0033*	-0.1061***	-0.0375***	-0.0320***
	(0.0013)	(0.0021)	(0.0018)	(0.0161)	(0.0071)	(0.0067)
Distance to park (100s m)	-0.0435***	-0.0045*	-0.0037**	-0.1623***	-0.0361***	-0.0291***
	(0.0076)	(0.0023)	(0.0019)	(0.0118)	(0.0071)	(0.0068)
Neighbourhood FE (187)		Yes	Yes			
Sale year-quarter FE (18)		Yes	Yes		Yes	Yes
Building class category FE (11)			Yes			Yes
Bike-share station FE (333)					Yes	Yes
Varying Slopes						
Sale year-quarter (Neighbourhood)		Yes	Yes			
Sale year-quarter (Bike-share station)					Yes	Yes
Standard-Errors	Neighbourhood			Bike-share station		
Mean outcome pre-period	3,040,182	3,040,182	3,040,182	3,040,182	3,040,182	3,040,182
Observations	74,667	74,667	74,667	8,889	8,889	8,889
Adjusted R^2	0.148	0.635	0.727	0.231	0.604	0.650
Within Adjusted R^2		0.026	0.104		0.039	0.084
RMSE	0.649	0.423	0.366	0.752	0.519	0.488

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the neighbourhood-tabulation-area level in columns 1 to 3, and at the bike-share-station level for columns 4 to 6. Numbers in parenthesis next to "FE" indicate the number of fixed-effect levels for that variable.

Most of the explanatory variables have the expected sign across specifications: the larger a unit's, the closer to a subway station and a park, the higher the sales price. The coefficients for building age and distance to bus stops are less stable, but make sense in their respective specifications. In a model with no spatial or temporal fixed effects, older buildings are associated with higher sale prices. This first reverses when controlling for time of sale and neighbourhood (younger buildings are associated with higher prices), and then disappears when controlling for building-class categories. 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 It makes sense that the impact of a building age on sale price thus disappears when comparing transactions within a given area. Distance to bus stops does not seem to explain sale prices very well when using the universe of transactions. However, being closer is a strong negative predictor of sale price for transactions within the bike-share station sample areas. A way to think about these results is that, ceteris paribus, a unit closer to a bus stop is probably closer to busy roadways, which are a clear disamenity.

Overall, the results from the hedonic model for both the universe of transactions and transactions within bike-share stations' sample areas indicate that the data set is reliable and that unit attributes and distances to amenities behave sensibly. In the next subsection, I present the results of the effect of bike-share stations on sale prices.

4.1.2 Treatment effect of bike-share stations

Table 6 displays the results of estimation equation 2 and 1. Columns 1 and 2 use the universe of transactions and control for locations of transactions within a treatment ring (i.e., within 150 metres of a bike-share station) and within a sample ring (i.e., within 500 metres of a bike-share station). In columns 3 to 6, the sample is restricted to transactions within sample rings.

Table 6: Treatment impact on residential transaction sale prices

	Log sale price (2015 $)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated ring × Post-period	0.0402	0.0530	0.0554	0.0573	0.0517	0.0611*
	(0.0472)	(0.0511)	(0.0474)	(0.0396)	(0.0329)	(0.0328)
Treated ring	0.0058	-0.0381	0.1324***	0.0975**	0.0128	-0.0183
	(0.0323)	(0.0327)	(0.0487)	(0.0416)	(0.0235)	(0.0240)
Sample ring × Post-period	-0.0371	-0.0045				
	(0.0525)	(0.0472)				
Sample ring	0.1340**	0.1025**				
	(0.0590)	(0.0506)				
Post-period	-0.0194	-0.0499	0.0884***	0.1348***	-0.1635***	-0.1459**
	(0.1048)	(0.1009)	(0.0247)	(0.0235)	(0.0615)	(0.0580)
Surface per unit (100s sqft)	0.0076***	0.0205***		0.0085***	0.0090***	0.0241***
	(0.0011)	(0.0012)		(0.0015)	(0.0008)	(0.0011)
Building age (10s years)	-0.0146***	-0.0020		0.0144***	-0.0022	-0.0014
	(0.0025)	(0.0020)		(0.0054)	(0.0035)	(0.0025)
Distance to bus stop (100s m)	0.0042*	0.0024		0.0781***	0.0880***	0.0897***
	(0.0021)	(0.0018)		(0.0180)	(0.0164)	(0.0153)
Distance to subway (100s m)	-0.0065***	-0.0032*		-0.1075***	-0.0370***	-0.0319***
	(0.0021)	(0.0018)		(0.0163)	(0.0071)	(0.0067)
Distance to park (100s m)	-0.0048**	-0.0039**		-0.1635***	-0.0368***	-0.0296***
	(0.0022)	(0.0018)		(0.0119)	(0.0072)	(0.0069)
Neighbourhood FE (187)	Yes	Yes				
Sale year-quarter FE (18)	Yes	Yes			Yes	Yes
Building class category FE (11)		Yes				Yes
Bike-share station FE (333)					Yes	Yes
Varying Slopes						
Sale year-quarter (Neighbourhood)	Yes	Yes				
Sale year-quarter (Bike-share station)					Yes	Yes
Standard-Errors	Neighbourhood		Bike-share station			
Mean outcome pre-period	3,040,182	3,040,182	3,040,182	3,040,182	3,040,182	3,040,182
Observations	74,667	74,667	8,889	8,889	8,889	8,889
Adjusted R^2	0.636	0.727	0.008	0.241	0.605	0.650
Within Adjusted R^2	0.027	0.105			0.040	0.086
RMSE	0.423	0.366	0.854	0.747	0.518	0.487

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the neighbourhood-tabulation-area level in columns 1 to 2, and at the bike-share-station level for columns 3 to 6. Numbers in parenthesis next to "FE" indicate the number of fixed-effect levels for that variable.

Columns 1 and 2 show no statistically significant effect of being treated by bike-share on transaction prices. Transactions in sample rings (i.e., within 500 metres of a bike-share station) are about 10% more expensive in the preferred specification (column 2). This specification, which includes neighbourhood fixed effects, indicates that the bike-share system might have been located in the more affluent areas within neighbourhoods.

Moving on to Columns 3 to 6, I reduce the sample to only include transactions within 500 metres of bike-share stations (i.e., within the sample rings). The specifications gradually add covariates (unit attributes and distance to amenities) and fixed effects. In column 3, 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* 0.243). Controlling for unit attributes and distance to amenities in Column 4 does not sensibly change either the magnitude or the statistical significance of the coefficient (*p-value* 0.148). Column 5 adds bike-share station and quarter fixed effects: the magnitude of the coefficient remains stable and its p-value improves (0.118). Adding these fixed effects, the coefficient for being in a treated ring turns statistically insignificant. In column 6, I add building-class category fixed effects and the coefficient of interest turns statistically significant (*p-value* 0.063). Under this specification, the impact of having a bike-share station within 150 metres of a property increases its transaction price by 6.1% with respect to property transacted between 150 and 500 metres of the same bike-share station, or $185,755 for an average transaction price. 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 in each of these dimensions. Since building categories within a given area might share a considerable number of common characteristics, which are unobserved in 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 sample area (i.e., 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 the current fixed effects would greatly reduce the number of observations used in the estimation. The model would compare observations within each fixed-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.[9] Therefore, I decide not to report more granular

[9] 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.

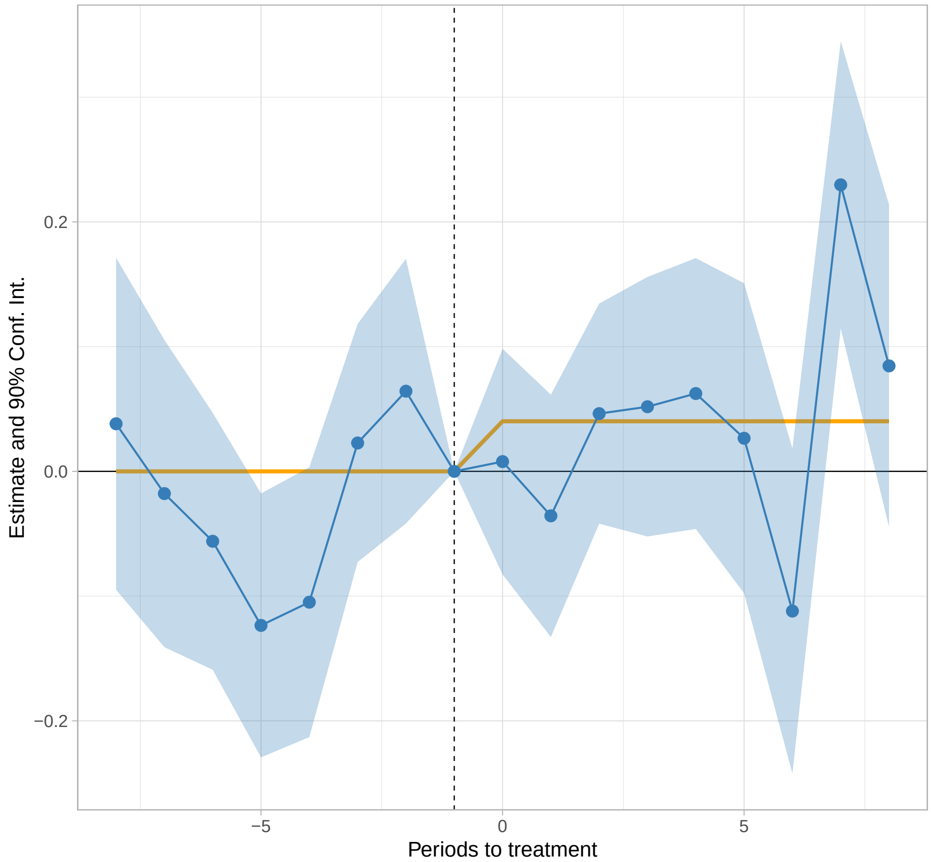

Figure 4: Dynamic effect of treatment on transaction price (event study). *Notes*: Periods to treatment are given in quarters. The shaded area indicates the coefficients' 90% confidence intervals. The orange line represents the post-period aggregated effect. The estimation only includes transactions within 500 metres of a bike-share station and includes the full set of covariates and fixed effects (see equation 3).

fixed effects, as those would restrict the number of observations used by the estimation.[10]

Figure 4 depicts the results from estimating equation 3, an event study of the effect of bike-share with respect to treatment periods. Indicative of evidence for parallel trends, pre-treatment periods display coefficients indistinguishable from zero (except for period -5): there are (almost) no statistical differences in price trends between treated and control transactions before treatment occurs. After treatment, there is a slight upward trend in the coefficients, indicating that prices in treated increased faster compared to control areas. However, the coefficient at period 7 is the only statistically significant one at the 10% significance level. Overall, the evidence from the event study does not provide very compelling evidence for the results reported in table 6.[decide]

I check the consistency of this result in Table 7. Replacing the binary treatment

[10]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. Note that when doing this, the statistical significance of the treatment effect further improves.

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 2.4% (preferred specification, Column 3), or $73,816 for an average transaction price. This is in line with the result obtained with the indicator variable, and shows there exists a downward-sloping gradient of transaction prices around bike-share stations.

Table 7: Continuous treatment

	Log sale price (2015 $)		
	(1)	(2)	(3)
Dist. to bike-share station (100s m) × Post-period	-0.0219**	-0.0228**	-0.0243**
	(0.0110)	(0.0108)	(0.0100)
Dist. to bike-share station (100s m)	0.0033	0.0014	0.0076
	(0.0083)	(0.0079)	(0.0077)
Post-period	-0.0723	-0.0828	-0.0608
	(0.0687)	(0.0687)	(0.0640)
Surface per unit (100s sqft)		0.0090***	0.0241***
		(0.0008)	(0.0011)
Building age (10s years)		-0.0020	-0.0013
		(0.0035)	(0.0025)
Distance to bus stop (100s m)		0.0877***	0.0899***
		(0.0165)	(0.0155)
Distance to subway (100s m)		-0.0369***	-0.0318***
		(0.0071)	(0.0068)
Distance to park (100s m)		-0.0372***	-0.0298***
		(0.0073)	(0.0070)
Bike-share station FE (333)	Yes	Yes	Yes
Sale year-quarter FE (18)	Yes	Yes	Yes
Building class category FE (11)			Yes
Varying Slopes			
Sale year-quarter (Bike-share station)	Yes	Yes	Yes
Mean outcome pre-period	3,040,182	3,040,182	3,040,182
Observations	8,889	8,889	8,889
Adjusted R^2	0.589	0.605	0.650
Within Adjusted R^2	0.001	0.041	0.086
RMSE	0.529	0.518	0.487

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the bike-share-station level. Numbers in parenthesis next to "FE" indicate the number of fixed-effect levels for that variable.

4.1.3 Robustness checks: alternative rings

In this subsection, I perform the analysis laid out in equation 2 for alternative definitions of rings. I use sample-ring distances of 400, 500 (baseline) and 600 metres; treatment rings of 125, 150 (baseline) and 175 metres; and, for each combination of the previous two dimensions, a buffer ring between treatment and control of 0 (baseline) and 50 metres.

Table 8: Alternative rings

	Without buffer			With 50m buffer		
Treatment ring in metres →	125	150	175	125	150	175
Sample ring 400m						
Treated ring × Post-period	0.0498	0.0485	0.0147	0.0506	0.0485	0.0284
	(0.0350)	(0.0339)	(0.0341)	(0.0351)	(0.0340)	(0.0364)
	[0.1561]	[0.1539]	[0.6664]	[0.1504]	[0.1545]	[0.4357]
Observations	7,635	6,078	4,724	6,746	5,278	4,117
Adj. R^2	0.6562	0.6569	0.6828	0.6575	0.6597	0.6844
Sample ring 500m						
Treated ring × Post-period	0.0610*	0.0611*	0.0357	0.0631*	0.0678**	0.0519
	(0.0350)	(0.0328)	(0.0317)	(0.0353)	(0.0333)	(0.0336)
	[0.0825]	[0.0635]	[0.2616]	[0.0747]	[0.0423]	[0.1230]
Observations	11,414	8,889	6,673	10,525	8,089	6,066
Adj. R^2	0.6542	0.6499	0.6805	0.6547	0.6522	0.6819
Sample ring 600m						
Treated ring × Post-period	0.0565	0.0590*	0.0424	0.0557	0.0635**	0.0544*
	(0.0344)	(0.0315)	(0.0302)	(0.0345)	(0.0320)	(0.0316)
	[0.1015]	[0.0623]	[0.1609]	[0.1068]	[0.0480]	[0.0865]
Observations	16,090	12,413	9,230	15,201	11,613	8,623
Adj. R^2	0.6499	0.6460	0.6778	0.6502	0.6481	0.6787

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the bike-share-station level are given in parenthesis. *p-values* are given in square brackets. When using 50-metre buffers, the observations within a 50-metre ring beyond the end of the treatment ring are dropped (e.g., if the treatment ring is 125 metres, observations located between 125 and 175 metres are dropped). The number of observations decreases as the treated area grows because fewer transactions can act as controls for multiple stations (see section 2.3 and figure 1).

In specifications with a 50-metre buffer, I exclude from analysis transactions that are in a 50-meter ring outside the treatment ring. For example, if the treatment ring is 125 metres, transactions that are between 125 and 175 metres are excluded.

The results are summarised in table 8. All reported results follow the specification used in column 6 of table 6, with sale year-quarter, bike-share station and building-class category fixed effects, full set of covariates (unit attributes and distances to amenities), and standard errors clustered at the bike-share-station level. Column 2, second panel corresponds to the results of table 6 (i.e., the baseline case).

Table 8 shows that the results obtained earlier hold for seven out of twelve models in sample rings of 500 and 600 meters, with magnitudes remaining relatively stable.[11] On the other hand, the estimations for the 400-metre sample ring are inconclusive.[12]

[11] In addition, in the 600-metre sample ring, the treatment effect coefficients for the two 125-metre treatment ring models are significant at just over 10%.

[12] This might be explained by the loss in power when reducing the diameter of the sample ring, as shown by the declining number of observations. The number of observations also decreases within a given size of sample ring when the treatment rings grows: this happens because fewer transactions can act as controls for multiple stations. These controls appeared as many times as they were controls, but if they switch to treated as the treatment ring grows, they only appear once, decreasing the number of observations despite the sample ring remaining the same.

Overall, these results suggest that there is a treatment effect of bike-share stations on residential property prices between 125 and 175 metres away from stations, compared to transactions within a 500 to 600-metre radius around the station.

4.2 Commercial properties

I now turn to transactions of commercial properties. Due to the important differences that exist between building-class categories in the commercial properties realm, I perform the analysis for the difference categories separately. In the main text, I only present the results for offices and retail properties (table 9). The other categories of commercial properties are commercial garages, factories and warehouses, and the results of treatment regressions on these categories can be found in appendix C.2. I do not expect these latter categories to be impacted by the implementation of the bike-share system for several reasons. First, bike-share stations were not opened near industrial areas where factories and warehouses are located. The primary goal of the system was to serve "busy" areas made up of workplaces, offices, retail and residential zoning. Second, there seem to be fewer reasons to believe that agents transacting these types of properties include "bikeability" in their decision function, and most probably give disproportionately more weight to other factors.

Starting from the hypothesis that bike-share had a positive impact on office and retail property prices, the coefficients in table 9 have the expected sign, but fall short of statistical significance at conventional levels in the preferred specification (columns 5 and 6). Interestingly, subway stations appear to be an important disamenity for both retail and office properties within bike-share-station sample areas: the closer the subway entrance, the lower the price.

As expected, the results for other categories of commercial properties (commercial garages, factories and warehouses) are also not statistically significant. The results on these other categories may be interpreted as a successful placebo analysis, showing that bike-share did not affect the sale prices of properties we would not expect it would.

Appendix C.1 presents the results of a hedonic model on all types of commercial property.

4.3 Discussion

Overall, the results of the analyses presented above point towards approximate/possible [better word?] evidence that the first wave of bike-share implementation did have an impact on residential transaction prices in NYC. I now review this evidence and discuss its implications.

In section 4.1.2, I reported the results of estimating the treatment effect of bike-share on residential transaction prices. I found no statistically significant impacts on sale prices of being in a treated ring after intervention when using the universe of transactions (table 6, columns 1 and 2), but found a treatment effect of 6.1% on sale prices when

Table 9: Treatment impact on commercial transaction sale prices, by building class category

	Log sale price (2015 $)					
Building class category →	Offices	Retail	Offices	Retail	Offices	Retail
	(1)	(2)	(3)	(4)	(5)	(6)
Treated ring × Post-period	0.5181**	0.2587	0.2049	0.2589	0.3098	0.1870
	(0.2323)	(0.1758)	(0.2888)	(0.1753)	(0.4191)	(0.2397)
Treated ring	-0.2149	-0.3021**	-0.3234	-0.3115***	-0.5807	-0.2781
	(0.2336)	(0.1194)	(0.2552)	(0.1197)	(0.4115)	(0.1778)
Post-period	-0.0583	0.3162***	0.4725	0.3937**	-0.1954	0.4713*
	(0.1017)	(0.0722)	(0.3440)	(0.1904)	(0.9012)	(0.2643)
Surface per unit (100s sqft)	0.0010***	0.0016***	0.0017***	0.0054***	0.0017***	0.0070***
	(0.0002)	(0.0006)	(0.0003)	(0.0008)	(0.0004)	(0.0011)
Building age (10s years)	0.0260	0.0659***	0.0194	-0.0220	0.0184	-0.0356
	(0.0236)	(0.0138)	(0.0429)	(0.0168)	(0.0838)	(0.0234)
Distance to bus stop (100s m)	0.2728	0.0622	-0.0368	-0.1523*	-0.2521	-0.1480
	(0.1727)	(0.0871)	(0.2910)	(0.0904)	(0.4665)	(0.1183)
Distance to subway (100s m)	0.0526	0.0579**	0.3327**	0.0393	0.4851***	0.0772**
	(0.0610)	(0.0248)	(0.1398)	(0.0266)	(0.1830)	(0.0374)
Distance to park (100s m)	-0.0973*	0.0174	0.1387*	0.0112	0.2059*	0.0008
	(0.0563)	(0.0297)	(0.0822)	(0.0254)	(0.1146)	(0.0386)
Bike-share station FE			Yes	Yes	Yes	Yes
Sale year-quarter FE (18)			Yes	Yes	Yes	Yes
Varying Slopes						
Sale year-quarter (Bike-share station)					Yes	Yes
# Bike-share station	–	–	189	284	189	284
Mean outcome pre-period	2,989,510	2,950,488	2,989,510	2,950,488	2,989,510	2,950,488
Observations	492	1,211	492	1,211	492	1,211
Adjusted R^2	0.052	0.050	0.402	0.327	-0.149	0.271
Within Adjusted R^2			0.157	0.075	0.198	0.121
RMSE	1.194	1.017	0.719	0.742	0.564	0.639

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the bike-share-station level. "# [FE variable]" indicate the number of fixed-effect levels for that variable.

restricting the sample to transactions within 500 metres of a bike-share station, which represents an increase of $185,755 for an average transaction. Narrowing down the sample to transactions closer to the treated areas hopefully improves the comparability between treated and control groups: transactions within 500 metres have a higher probability of following similar price trends than transactions further away.

Parallel trends are the central assumption in a difference-in-differences analysis. The specific assumptions required for a spatial ring method difference-in-difference were laid out by Butts (2023), who shows that parallel trends need to hold *locally* within the sample ring for inference to be valid. Again, this is more likely to happen when restricting the sample to observation in the near vicinity of the intervention. I explore this assumption in figure 4. Unfortunately, the evidence from the event study is somewhat ambiguous: there is weak evidence of parallel trends pre-treatment, and modest evidence for post-treatment dynamic treatment effects.

Despite this notable drawback, I perform a battery of additional analyses. First, I replace the treatment indicator variable with a continuous measure of distance to treatment in table 7. The results are consistent with those of table 6, showing a clear downward-slopping price gradient around bike-share stations after implementation: for every additional hundred metres away from a station, prices decrease on average by 2.4%, or $73,816 for an average sale price.

I then test whether the results are sensitive to the choice of distances used to construct the treatment and sample rings. Table 8 reports the results for 18 combinations of sample and treatment ring sizes, with and without buffers between rings. The estimates for 500 and 600-metre sample rings are for the most part consistent with the main results, showing treatment effects between 5.4% and 6.8%.

Finally, I turn to impacts on commercial properties, but do not find strong evidence of any treatment effect on office or retail units, and reassuringly no impacts on commercial units less likely affected by the intervention such as commercial garages, factories and warehouses.

On the whole, the evidence for a causal impact of bike-share stations is mixed. While some estimations show statistically significant treatment effects, some tests failed to produce supporting evidence for key identifying assumptions. Overall, these results should be interpreted with a degree of caution, but also suggest some potential impacts might have taken place.

With respect to previous research, the effects on sale prices reported here are relatively large but within the range for transport infrastructure interventions. Urban rail stations typically increase property prices by 3 to 10%. Studies on bike-share showed a reduction of the subway premium by up to 30% after the introduction of bike-share. Of particular interest to us are the estimated effects of bike-share on rents determined by Shr, Hsu, et al. (2023) 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, which could be explained by the fact that transaction prices include the present value of all future rents, exceeding the increase seen at the rent level.

There are several ways the analysis could be expanded and improved. First, I focused in this paper on the first wave of bike-share implementation, from May 2013 to June 2015. In subsequent years, the system grew both spatially and in usage. Exploiting these later extensions would let us use more transactions, in more areas, and possibly enable a repeated-transaction analysis. Expanding the time frame also comes with its own set of challenges, most notably how to handle variation in treatment timing and heterogenous treatment effects as pointed out by the difference-in-differences literature (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021). Second, the addition of rent data could provide larger sample sizes and more precise estimates. Property transactions, even in a metropolis like NYC, are still relatively less frequent than leases. When examining very small geographic areas, the number of observations in each fixed-effect cell quickly reduces, and some bike-share stations are not used in the computation of the final estimates. Rent data could provide more observation per fixed-effect cell at higher time frequencies, potentially enabling the use of more granular fixed-effects. More precise data on unit attributes (surface area, number of rooms, number of bathrooms, etc.) and distances to amenities may help improve the estimates. While adding building-class-category fixed-effects does go some ways towards controlling for unobserved factors, better unit-level data would do a better job at accounting for features impacting sale price, improving the precision of the estimates. Other interesting avenues for future research include investigating the interplay between the bike-share network and public transit: for example, do prices increase more in areas further away from subway stations? Similarly, studying the effects on market access of bike-share systems could provide very interesting insight into the role of cycling in urban transport networks.

5 Conclusion

In this paper, I analysed the impact of the first wave of bike-share station openings in NYC on real-estate transaction prices. I used the universe of transactions carried out in NYC between January 2011 and June 2015, which were geocoded and matched to their closest bike-share station. Using the sample of transactions within 500 metres of bike-share stations, I find that properties within 150 metres of bike-share stations were about 6.1% more expensive than those between 150 and 500 metres away, representing an increase of $185,755 for an average transaction.

I then performed additional analyses, which provided mixed evidence on the treatment effect of bike-share on transaction prices. The results hold for an important subset of different ring sizes, but do not materialise for commercial properties.

These results contribute to the emerging literature on the impacts of cycling on cities. They are broadly in line with previous studies showing a positive impact of bike-share systems on real-estate prices. Future research would benefit from better data and could explore longer time frames, which might be important to identify long-run effects. The impact of cycling on market access (as pioneered by Daniele et al. (2022)) and its implication for real-estate markets is another promising area of research.

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A Building decades

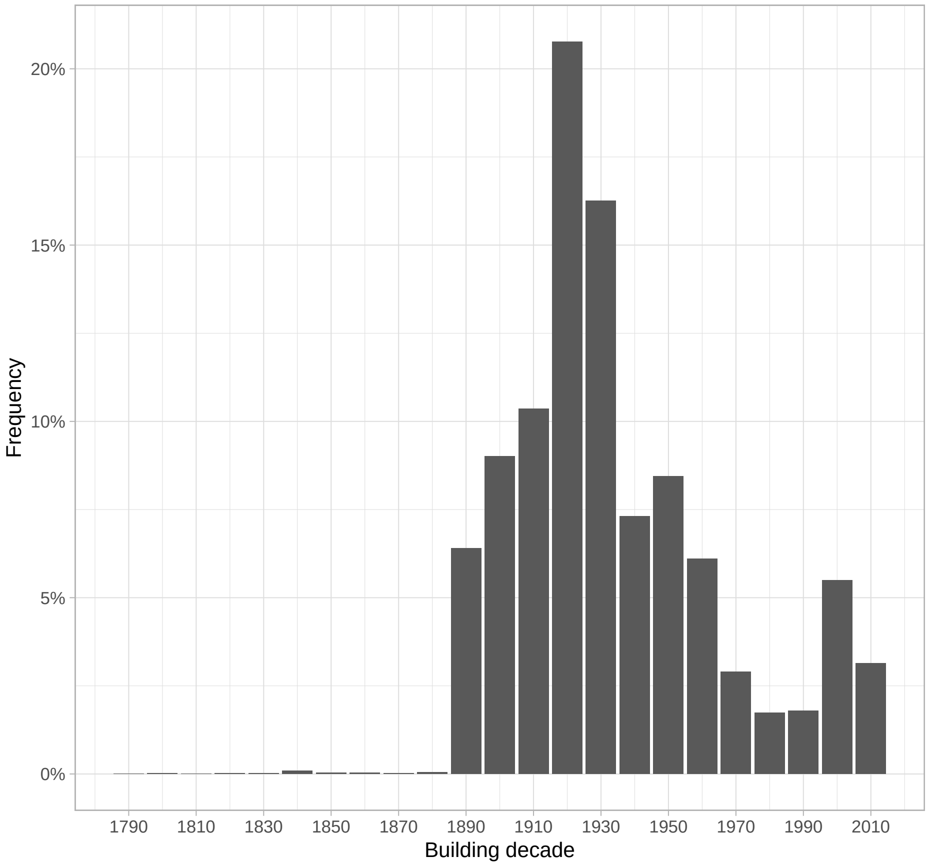

Figure A.1: Building decade of transactions in the main sample data set (i.e., residential units, no price and price-per-square-feet outliers).

B Detailed treatment map

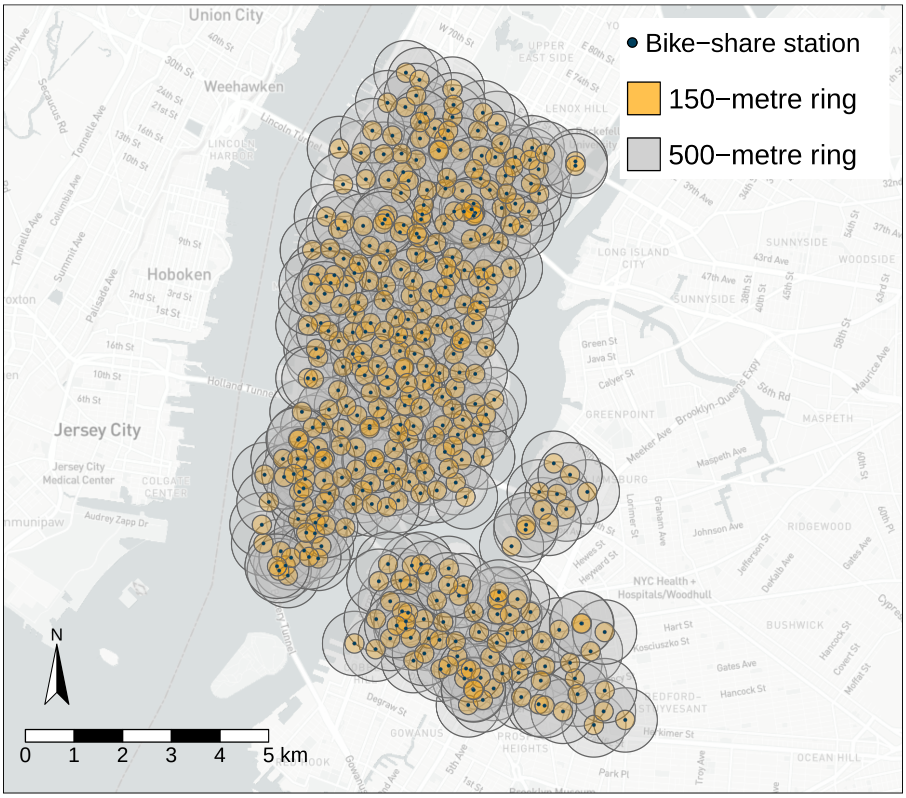

Figure B.1: Overview of the study area and treatment and control rings, not merged. *Notes*: The yellow areas represent the treatment rings (i.e., within 150 metres of a bike-share station), while the grey areas indicate the control rings (i.e., between 150 and 500 metres from a bike-share station).

C Additional analyses on commercial properties

C.1 Hedonic regressions

Table C.1: Hedonic model — offices and retail transactions

	Log sale price (2015 $)							
Building class category →	Offices	Retail	Offices	Retail	Offices	Retail	Offices	Retail
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Surface per unit (100s sqft)	0.0017***	0.0016	0.0017***	0.0016	0.0017***	0.0016	0.0018***	0.0071***
	(0.0006)	(0.0015)	(0.0005)	(0.0014)	(0.0006)	(0.0015)	(0.0004)	(0.0011)
Building age (10s years)	0.0367	0.0501**	-0.0616***	-0.0654***	-0.0767***	-0.0594***	0.0476	-0.0355
	(0.0313)	(0.0235)	(0.0192)	(0.0123)	(0.0244)	(0.0151)	(0.0822)	(0.0234)
Distance to bus stop (100s m)	-0.0067*	-0.0098***	-0.0150	0.0033	-0.0118	0.0009	-0.2205	-0.1677
	(0.0037)	(0.0029)	(0.0162)	(0.0091)	(0.0212)	(0.0102)	(0.4861)	(0.1179)
Distance to subway (100s m)	-0.0077**	-0.0020	-0.0075	-0.0122	-0.0079	-0.0117	0.4931***	0.0775**
	(0.0038)	(0.0032)	(0.0160)	(0.0081)	(0.0205)	(0.0090)	(0.1816)	(0.0367)
Distance to park (100s m)	-0.1279***	-0.0593***	-0.0043	-0.0022	-0.0118	0.0015	0.1586	0.0017
	(0.0328)	(0.0180)	(0.0360)	(0.0135)	(0.0415)	(0.0146)	(0.1141)	(0.0397)
Neighbourhood FE			Yes	Yes	Yes	Yes		
Sale year-quarter FE (18)			Yes	Yes	Yes	Yes	Yes	Yes
Bike-share station FE							Yes	Yes
Varying Slopes								
Sale year-quarter (Neighbourhood)					Yes	Yes		
Sale year-quarter (Bike-share station)							Yes	Yes
# Neighbourhood	–	–	153	183	153	183	–	–
# Bike-share station	–	–	–	–	–	–	189	284
Standard-Errors	Neighbourhood						Bike-share station	
Mean outcome pre-period	3,035,532	3,094,887	3,035,532	3,094,887	3,035,532	3,094,887	2,989,510	2,950,488
Observations	1,175	3,212	1,175	3,212	1,175	3,212	492	1,211
Adjusted R^2	0.206	0.115	0.591	0.500	0.580	0.519	-0.143	0.268
Within Adjusted R^2			0.078	0.043	0.089	0.041	0.202	0.117
RMSE	1.285	1.236	0.854	0.900	0.795	0.855	0.572	0.642

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the neighbourhood-tabulation-area level in columns 1 to 6, and at the bike-share station level for columns 7 to 8. "# [FE variable]" indicate the number of fixed-effect levels for that variable.

Table C.2: Hedonic model — commercial garage, factory and warehouse transactions

	Log sale price (2015 $)											
Building class category →	Garages	Factories	Warehouses	Garages	Factories	Warehouses	Garages	Factories	Warehouses	Garages	Factories	Warehouses
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Surface per unit (100s sqft)	0.0024***	0.0014**	0.0005*	0.0016***	0.0014**	0.0005**	0.0015***	0.0015**	0.0005**	0.0030***	0.0074***	0.0018***
	(0.0007)	(0.0006)	(0.0002)	(0.0004)	(0.0005)	(0.0002)	(0.0004)	(0.0006)	(0.0003)	(0.0009)	(0.0022)	(0.0005)
Building age (10s years)	0.0128	-0.0126	0.0282	-0.0155	-0.0328	-0.0025	-0.0153	-0.0365	-0.0107	0.0203	-0.0067	-0.0707*
	(0.0210)	(0.0274)	(0.0238)	(0.0164)	(0.0346)	(0.0170)	(0.0203)	(0.0345)	(0.0186)	(0.0671)	(0.0696)	(0.0382)
Distance to bus stop (100s m)	-0.0099***	-0.0069**	-0.0116***	0.0179	-0.0243	-0.0188	0.0209	-0.0193	-0.0125	-0.3056	-0.1726	-0.0637
	(0.0025)	(0.0033)	(0.0032)	(0.0118)	(0.0247)	(0.0164)	(0.0145)	(0.0314)	(0.0215)	(0.5219)	(0.4458)	(0.4031)
Distance to subway (100s m)	0.0013	-0.0079	0.0025	-0.0293***	0.0026	0.0047	-0.0318**	0.0058	0.0064	-0.0978	0.0257	0.0010
	(0.0027)	(0.0056)	(0.0037)	(0.0112)	(0.0183)	(0.0133)	(0.0132)	(0.0187)	(0.0167)	(0.1641)	(0.1143)	(0.0479)
Distance to park (100s m)	-0.0236	-0.0053	-0.0374	0.0222	-0.0275*	-0.0242	0.0267	-0.0192	-0.0315	0.1016	0.0505	0.0198
	(0.0259)	(0.0135)	(0.0243)	(0.0147)	(0.0166)	(0.0250)	(0.0175)	(0.0171)	(0.0259)	(0.2422)	(0.0642)	(0.1346)
Neighbourhood FE				Yes	Yes	Yes	Yes	Yes	Yes			
Sale year-quarter FE				Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bike-share station FE										Yes	Yes	Yes
Varying Slopes												
Sale year-quarter (Neighbourhood)							Yes	Yes	Yes			
Sale year-quarter (Bike-share station)										Yes	Yes	Yes
# Neighbourhood	–	–	–	169	104	123	169	104	123	–	–	–
# Sale year-quarter	–	–	–	18	18	18	18	18	18	18	17	17
# Bike-share station	–	–	–	–	–	–	–	–	–	113	62	117
Standard-Errors					Neighbourhood						Bike-share station	
Mean outcome pre-period	2,845,757	3,251,103	3,110,593	2,845,757	3,251,103	3,110,593	2,845,757	3,251,103	3,110,593	2,695,724	2,848,955	2,998,746
Observations	1,063	811	989	1,063	811	989	1,063	811	989	245	174	242
Adjusted R^2	0.149	0.130	0.142	0.509	0.284	0.421	0.496	0.282	0.432	9.231	-0.861	2.445
Within Adjusted R^2				0.085	0.131	0.092	0.074	0.144	0.103	1.584	0.033	0.620
RMSE	1.172	1.065	1.110	0.809	0.891	0.845	0.736	0.822	0.773	0.424	0.666	0.276

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the neighbourhood-tabulation-area level in columns 1 to 9, and at the bike-share-station level for columns 10 to 12. "# [FE variable]" indicate the number of fixed-effect levels for that variable.

C.2 Treatment regressions, placebo analysis

Table C.3: Treatment impact on sale prices — commercial garage, factory and warehouse transactions

	Log sale price (2015 $)								
Building class category →	Garages	Factories	Warehouses	Garages	Factories	Warehouses	Garages	Factories	Warehouses
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated ring × Post-period	0.4878	-1.0683**	-0.4246	0.2818	0.0856	-1.2943**	0.0184	0.3940	-2.1961**
	(0.3543)	(0.4767)	(0.5278)	(0.3499)	(0.9439)	(0.5214)	(0.7088)	(1.3875)	(0.8649)
Treated ring	0.1286	0.7584**	0.1720	-0.2244	0.0273	0.4632	-0.0444	-0.3329	1.2072*
	(0.2374)	(0.2986)	(0.3721)	(0.2524)	(0.7015)	(0.3719)	(0.4706)	(0.8909)	(0.6361)
Post-period	-0.4632***	0.5025***	0.1817	0.2908	0.7233*		0.2754	0.4847	
	(0.1499)	(0.1362)	(0.1634)	(0.6291)	(0.3772)		(1.1324)	(0.4803)	
Surface per unit (100s sqft)	0.0019***	0.0049***	0.0006***	0.0013***	0.0069***	0.0007***	0.0030***	0.0076***	0.0013**
	(0.0005)	(0.0008)	(0.0001)	(0.0004)	(0.0015)	(0.0001)	(0.0009)	(0.0024)	(0.0005)
Building age (10s years)	0.0500*	-0.0300	-0.0268	0.0522	-0.0002	-0.0730***	0.0253	-0.0133	-0.0866**
	(0.0280)	(0.0353)	(0.0302)	(0.0352)	(0.0501)	(0.0259)	(0.0873)	(0.0781)	(0.0340)
Distance to bus stop (100s m)	-0.0636	0.3537*	0.2850**	0.0147	0.0290	-0.3148**	-0.3104	-0.1716	-0.7590***
	(0.2489)	(0.2083)	(0.1186)	(0.2258)	(0.3974)	(0.1436)	(0.6193)	(0.4315)	(0.2792)
Distance to subway (100s m)	-0.0653*	-0.1336***	-0.1090***	-0.1302	0.0200	0.0167	-0.0973	0.0183	0.0012
	(0.0372)	(0.0281)	(0.0381)	(0.0908)	(0.0670)	(0.0371)	(0.1759)	(0.1130)	(0.0451)
Distance to park (100s m)	-0.1439***	-0.0385	-0.0021	0.0842	0.0160	-0.0113	0.1032	0.0385	-0.0164
	(0.0477)	(0.0328)	(0.0381)	(0.1083)	(0.0534)	(0.0804)	(0.2735)	(0.0724)	(0.1455)
Bike-share station FE				Yes	Yes	Yes	Yes	Yes	Yes
Sale year-quarter FE				Yes	Yes	Yes	Yes	Yes	Yes
Varying Slopes									
Sale year-quarter (Bike-share station)							Yes	Yes	Yes
# Bike-share station	–	–	–	113	62	117	113	62	117
# Sale year-quarter	–	–	–	18	17	17	18	17	17
Mean outcome pre-period	2,695,724	2,848,955	2,998,746	2,695,724	2,848,955	2,998,746	2,695,724	2,848,955	2,998,746
Observations	245	174	242	245	174	242	245	174	242
Adjusted R^2	0.189	0.316	0.093	0.674	0.272	0.716	5.113	-1.068	2.015
Within Adjusted R^2				0.057	0.120	0.336	1.292	-0.075	0.733
RMSE	1.182	0.962	0.924	0.505	0.725	0.342	0.424	0.664	0.248

Notes: Significance codes: *: 0.1, **: 0.05, ***: 0.01. Standard errors clustered at the bike-share-station level. "# [FE variable]" indicate the number of fixed-effect levels for that variable.