Essays in Urban and Development Economics

A Thesis Submitted to the University of Dublin, Trinity College in Application for the Degree of Doctor of Philosophy

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Dublin, 2022

Declaration, Online Access and the General Data Protection Regulation

I declare that this thesis has not been submitted as an exercise for a degree at this or any other university and it is entirely my own work. Chapter 3 of this thesis is co-authored with Mathias Allemand, Martina Kirchberger, Sveta Milusheva, Carol Newman, and Brent Roberts.

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Summary

This thesis consists of three essays at the intersection of urban, environmental, transport, development and labour economics. Each chapter tackles important questions regarding the role of new active mobility transport systems on the environment and housing markets of cities, and the potential non-cognitive training to improve the labour market outcomes of urban low-skilled workers. Using principally empirical methods in a causal inference framework, the thesis makes extensive use of secondary spatial data and primary survey data.

Chapter 1 examines the impact of bike-share on air pollution. Combining the universe of bike-share trips with high-resolution, ground measures of the concentration of air pollutants in New York City between 2009 and 2019, it estimates the causal effect of cycling infrastructure on air quality. A routing algorithm, leveraging the location and usage data of bike-share stations, is used to map areas where road traffic is expected to decrease after the introduction of bike-share. By employing quasi-experimental methods, the analysis yields causal estimates of the effects of bike-share on air quality. I find that the deployment of bike-share is associated with a 3% reduction in black carbon and a 13% reduction in nitric oxide concentrations, both pollutants associated with road traffic. Back-of-the-envelope valuation of the health and mortality benefits associated with the reduction in nitric oxide concentrations suggests that bike-share prevented up to $327 million in social damages. In addition, I investigate potential mechanisms and show that the introduction of bike-share is associated with a decrease in short taxi trips in areas served by bike-share, which I interpret as suggestive evidence that bike-share substituted road traffic.

Chapter 2 investigates whether the initial deployment of bike-sare in New York City between May 2013 and June 2015 increased real-estate transaction prices of properties close to bike-share stations. Given the potential of bike-share systems to act as a local amenity (by providing new transport options to commuters), agents may increase their valuation of property units near bike-share stations. 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 away from 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.

Chapter 3 reports the results of a randomised controlled trial testing the impact of non-cognitive training on the labour market outcomes of low-skilled workers in a developing country. Non-cognitive skills are increasingly recognized as important determinants of labour market outcomes. To what extent specific skills can be affected in adulthood remains an open question. In this study, low-skilled workers employed on the construction site of a new railway in Dakar, Senegal were randomly assigned to receive a training intervention designed to affect conscientiousness-related skills. The findings show that treated workers were significantly more likely to stay in their job and have higher wages nine months after the intervention. The findings suggest that non-cognitive skills can be affected even later in the life cycle and can have substantial labour market returns.

Acknowledgements

Thanks everyone — really, great job!

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Introduction

The human species is an inherently social animal. In the past ten thousand years, this social instinct has driven some groups of humans to form ever-growing societies, leading to the concentration of ever-larger populations in confined geographical spaces: cities. Cities and the concentration of human activity have led to many important developments, but has come with its own set of challenges. Dealing with the negative externalities of packing thousands upon thousands of people in a couple of square kilometres has been a constant since the very birth of the *res urbanae*, the urban question.

Cities in the twenty-first century keep evolving, opening new challenges to urban dwellers and policy-makers. Heirs to almost a century of car-centred urban planning, cities have come to realise that individual, motorised transport based on fossil fuels could only go so far in providing a safe, healthy and equitable space for millions to thrive. In light of the growing urgency in dealing with climate change, new evidence on the devastating impacts of air pollution on human health, and the need to build more inclusive societies, cities have been building new infrastructure and promoting new ways of living. This is especially noticeable in the realm of urban transport: the past twenty years have seen cities investing massively in more sustainable, public transport, and re-thinking their position towards individual motorised vehicles. Because of the importance of the transport sector in greenhouse gases and air pollution emissions (around 30% of total emissions, EPA (2023) and Karagulian et al. (2015)), and its role in fostering opportunities by either the jobs it provides directly or by connecting people to employment, transportation policy is key to the development of cities in the twenty-first century.

The present thesis proposes to explore this theme through three chapters. [contribution?] The first two investigate the role of a new transport system developed in the past twenty years, bike-share systems. Bike-share systems are publicly available bicycles docked at stations. In both chapters, I study

New York City's (NYC) bike-share program opened in 2013, the largest in North America with more than 800 stations and 60 thousand daily trips on average in 2022. This represents a very large cycling intervention in the United States' largest and densest city.

In the first chapter, I investigate the potential impact of the bike-share system on the concentration of air pollutants. I use the NYC Community Air Survey (NYCCAS) from the NYC Health Department to measure the change in the concentration of four air pollutants associated with traffic from internal combustion engines. The NYCCAS is a uniquely suited data set as it maps, for every year since 2009, the annual average concentration of six pollutants for more than nine thousand 300-by-300 metre cells. This unique spatial granularity lets me precisely match the extent of the bike-share system to NYCCAS cells, and compute the effect of bike-share on the concentration of air pollutants. To do so, I use the universe of bike-share trips taken on the system from May 2013 until December 2019, and define the areas of the city where we would expect road traffic to decrease due to bike-share substitution. I define these areas by mapping the hypothetical car route running between pairs of bike-share stations. This defines the area treated by bike-share, i.e., the area where we would expect air pollution to decrease due to the arrival of the bike-share system. Using a two-way fixed effects design, I estimate the change in pollution due to the introduction of bike-share. The results indicate that bike-share reduced the concentration of nitric oxide by up to XX% and black carbon by XX% with respect to pre-intervention mean concentrations. Using back-of-the-envelop calculations, I estimate that the reduction in nitric oxide concentrations saved up to $320 million in social damages.

Finally, I explore the mechanisms at play and test whether there is a discernible change in traffic after the arrival of bike-share. Using th universe of taxi trips, the estimation indicates that short taxi trips, those most susceptible to be substituted by bike-share, decreased faster than long taxi trips in areas where bike-share was implemented. I interpret this result as suggestive evidence that bike-share substituted trips away from taxis, which may explain the reduction in the concentration of air pollution observed.

This chapter makes several important contributions to the literature on transport and the environment. It is the first study to use long-run, observational data to assess the impact of cycling on air quality. Previous studies relied on assumed substitution rates between cycling and other transport modes to evaluate the environmental impact of cycling policies. By using ground measures of air pollution in a quasi-experimental setting, and by explicitly testing the substitution hypothesis, this chapter is able to deliver a robust estimate of the effect of bike-share on air quality. Second, by analysing ten years of air pollution data, I can capture changes in mobility patterns that short-run studies may not: because commuting habits may take some time to materialise, multi-year studies are best equipped to identify the impacts of transportation policies.

In the second chapter, I test whether the NYC bike-share system had an impact on real-estate transaction prices. Analysing the impacts of transport policies on housing prices is a tried and tested method in urban economics to estimate the value premium brought by the policy change. In this chapter, I focus on the first wave of bike-share station openings, from May 2013 to June 2015. I first geocode the universe of real-estate transactions from the NYC Department of Finance, and then match each transaction to a bike-share station. I define the transactions within 150 metres of a bike-share station as treated, and those between 150 and 500 metres as the control transactions. This empirical strategy builds upon Shr, Hsu, et al. (2023) and their study on the short-run impacts of bike-share on rents in Taiwan. Using bike-share station, quarter and building class fixed effects, I find that residential properties transacted in the treated area were 6.1% more expensive than their counterparts in the control area after the opening of the system. I perform additional analyses and find that evidence for a causal impact of bike-share on property prices is mixed: the event study shows ambiguous parallel trends pre-treatment and modest evidence of post-treatment effects. On the other hand, the estimates remain mostly stable when varying the treatment and sample distances around bike-share stations. Evidence of impacts on commercial properties is inconclusive, however. Overall, these results should be interpreted with a degree of caution, but also suggest some potential impacts might have taken place.

This study contributes to the literature in a couple of regards. First, it provides the first medium-run estimates of the impacts of bike-share on real-estate transactions. Previous studies focused on short-term impacts and rental markets. Second, it is the first to evaluate those impacts outside Asia and for United States' largest and densest city, and North America's largest bike-share system. Lastly, it adds to the rich literature on the impact of transport policies on real-estate markets by analysing a large cycling intervention. [improve]

Finally, the last chapter takes a slightly different perspective on transport infrastructure in cities. In this study, we study the impact of a psychological training intervention on low-skilled workers employed on a railway construction site in Dakar, Senegal. We perform a randomised controlled trial and randomly assign workers to a training intervention designed to affect conscientiousness-related skills, a class of non-cognitive skills thought to be important in the workplace. The training consists of a two-hour session with a trained professional followed by short phone reminders over eight weeks. The main aim of the intervention is to help workers foster new behaviours and ensure these changes endure. We examine the impact of the training on labour market outcomes such as wages and employment status, and find that treated workers were more likely to be employed in the construction company and received on average higher wages. We interpret this as hard evidence that the intervention had an impact. We then examine the psychometric data and find that several conscientiousness indicators improve as a result of the intervention. At the same time, we also document the challenges related to administering self-reported psychometric questionnaires to low-skilled workers in a developing context: our measures are often top-coded and we faced translation issues.

This chapter contributes to three strands of the literature. First, we add to the literature on soft skills and their labour market impacts. Our experiment is the first, to our knowledge, to target different facets of conscientiousness and focus on the impact on labour market outcomes. Second, we contribute to the literature documenting the specific characteristics of labour markets in developing settings, characterised by significant search and matching frictions and the high turnover for low-earnings jobs, among others. Finally, we contribute to the debate on whether psychological traits may be changed in adulthood.

[do I need concluding remarks? if yes, what kind? "The remainder of the thesis is organised as follows. . . "?]

Chapter 1

Cycling Towards Cleaner Cities? Evidence from New York City's Bike Share System

1.1 Introduction

Air quality is a central issue in cities: with a majority of the world's population living in urban areas, increased exposure to air pollutants has negative impacts on health, and is associated with substantial social and economic costs (Carozzi and Roth, 2023). Road transportation is an important emitter of urban air pollutants, contributing close to 40% of concentrations (EEA, 2021), and leading many cities to implement novel policies aiming to lessen the burden of motor vehicles. Bike share is one such intervention, a network of automated stations where public bicycles are rented for short periods and returned. With over three thousand systems installed worldwide in the past 20 years, bike share has become a ubiquitous sight in cities (Meddin et al., 2022). By making cycling more convenient and cheaper, bike share has the potential to replace trips previously made by internal combustion engine (ICE) vehicles,[1] thus improving air quality. Despite its popularity and potential, very little is known about the impact of bike share on road transportation and air pollution.

In this paper, I identify the causal effects of bike share on air pollution concentrations in New York City (NYC) between 2009 and 2019. While previous research has examined the impact of other policies on pollution

[1]This includes cars, taxis, buses and motorcycles that run on fossil-fuel engines.

abatement from road transportation, such as low-emission zones (Wolff, 2014; Jiang et al., 2017; Zhai and Wolff, 2021), congestion charge (Tonne et al., 2008; Green et al., 2020) and the opening of new subway lines (Chen and Whalley, 2012; Gendron-Carrier et al., 2022), the impact of cycling on air pollution has received much less attention.

Causal evidence on the environmental impact of bike share is scarce.[2] A recent set of papers use a difference-in-differences approach to estimate the impact of bike share on a related outcome, traffic congestion (Wang and Zhou, 2017; Hamilton and Wichman, 2018). While congestion is correlated with air quality, these studies do not estimate the change in air pollution concentrations, which is a key outcome for assessing the health impacts of bike share. Shr, Yang, et al. (2022) are the first to estimate the impact of bike share directly on the air quality. They combine air quality data with a bike share expansion in Taiwan's second-biggest city in a difference-in-differences setting, and find no or very small reductions in air pollution concentrations one year after the expansion.

My study adds to the existing literature by providing the first estimates of the long-run effects on air quality of a bike share program. It does so by leveraging the deployment and expansion of the largest bike share system in North America, in the most populous and densest city in the United States (US), New York City. Using high-resolution, observational air quality data over ten years, I rely on a rich data set of air pollution concentrations to estimate the contribution of bike share to air quality. To the best of my knowledge, it is the first study that provides causal estimates on the long-term impacts of New York City's bike share system.

The main challenges in causally estimating the relationship between bike share and air quality are (1) the availability of high-resolution pollution data over long periods and (2) a credible identification strategy. I address these challenges by employing the NYC Community Air Survey as my measure of air quality, and combining it with the staggered rollout of NYC bike share stations to identify areas treated by bike share. The NYC Community Air

[2] A geographic and engineering literature evaluates the impact of bike share on pollution and the environment, but it uses non-causal methods. The main issue of these studies is their reliance on hypothetical rates of substitution towards cycling from motor vehicles due to bike share, which may lead to overestimating the impact of bike share on the emission of air pollutants. See Fishman et al. (2014), Ricci (2015), Médard de Chardon (2016), Zhang and Mi (2018), Qiu and He (2018), and Zheng, Gu, et al. (2019).

Survey is a unique data set that reports the annual average concentration of six pollutants for 9,760 300-metre by 300-metre cells covering the entirety of the city's extent, from 2009 to 2019. The reported concentrations for each cell are derived from year-round observations at over one hundred monitoring stations, of which 80% are placed randomly.[3] These 300-metre cells are taken as the spatial units of observation.

While bike share is not implemented randomly,[4] I show that the timing and spatial extent of each roll-out wave are as good as random, conditional on a set of covariates. The staggered rollout of bike share provides quasi-random variation in the substitution of car traffic, allowing me to estimate the causal effect of bike share on air pollution concentrations using a difference-in-differences strategy.

Key to this identification strategy is the definition of areas within the city where bike share affected pollution concentrations. I identify areas where bike share is most susceptible to reducing pollution using the universe of bike share trips and a routing algorithm. The universe of trips is used to determine yearly pairs of stations, which are defined by at least one observed bike share trip between the two stations in a given year.[5]

Assuming that road traffic reduction is the channel through which bike share might impact air pollution, I compute the typical *car* journey between stations for each pair using a routing algorithm, effectively capturing the areas where *fewer* cars are susceptible to have been driven due to bike share. Aggregating the journeys at the 300-metre-cell level, I obtain the spatial extent of bike share's influence on car traffic.[6] By mapping areas where concentrations are most likely to be impacted by the deployment of bike share, I ensure that the treatment and control groups are defined as closely as possible to the true areas of influence of bike share, and add variation in the spatial extent of the

[3]The NYCCAS has been used in epidemiological and environmental studies (Savitz et al., 2014; Johnson et al., 2016), including the evaluation of transport policies — see for example Fry et al. (2020) and Lovasi et al. (2022).

[4]To ensure success, city planners choose to deploy bike share in busy areas first, where a large demand for transportation guarantees sufficiently high usage.

[5]For example, suppose there are three stations in the network, A, B and C. Assume that only three trips were taken in a year: one from A to B, another from A to C, and one from B to A. The pairs of stations for that year will thus be A–B, A–C, and B–A. Notice that B–C, C–A and C–B are missing and thus *not considered station pairs*.

[6]I also investigate the intensive margin of bike share by imputing the number of bike share trips for each journey and aggregating at the cell level. I obtain the intensity of bike share's influence on car traffic for each 300-metre cell.

treated area that is less correlated with the locations of bike share stations.

I use the staggered rollout of bike share stations and the associated yearly changes in the spatial "footprint" of bike share on car traffic as my treatment variable in the difference-in-differences strategy. In effect, I compare air pollution concentrations in cells where car traffic is most likely impacted by bike share with cells not likely affected, before and after the deployment and expansion of bike share. Conditional on parallel trends in concentrations prior to treatment and no omitted policy affecting the concentrations, this strategy delivers an average treatment effect on the treated.

I find that concentrations of pollutants associated with road transportation reduce by 3% for black carbon (a subset of particulate matter), and 13% for nitric oxide, with respect to pre–bike-share mean concentrations. Interestingly, I do not find an effect on particulate matter (PM 2.5) concentrations, an air pollutant widely studied in the literature. Results also contrast with Shr, Yang, et al. (2022) who find no effect of bike share deployment on nitric oxides and only a slight decline in carbon monoxide, which they attribute to substitution away from two-wheeled ICE vehicles. My results highlight that the likely source substitution is key in explaining the evolution of different pollutants. These results are robust to alternative specifications, and hold when using the Borusyak et al. (2022) difference-in-differences estimator accounting for variation in treatment timing and heterogenous treatment effects (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). Back-of-the-envelope calculations show that the social benefit from the reduction in nitric oxide reaches up to $327 million since bike share was introduced. Brought back to an average bike share trip, riders pay on average $1.81 per trip while reducing social damages by $3.31.

These findings are highly relevant when set in the context of the significant health impact of air pollution. In the US, between 100 and 200 thousand yearly deaths are associated with air pollution (Burnett et al., 2018). Furthermore, excessive concentrations of air pollutants are linked to asthma incidence and crises, costing upwards of $135 million per year in emergency room visits alone (Anenberg et al., 2018; Qin et al., 2021).[7] Air pollution is also negatively associated with cognitive performance, crime rates, labour supply, labour

[7] Air pollution also affects other chronic respiratory diseases, leading to the yearly loss of an estimated 371 thousand disability-adjusted life years in the US (Murray et al., 2020). Other medical outcomes affected include newborn birth weights and mortality (Currie et al., 2014).

productivity, and decision-making ability (Aguilar-Gomez et al., 2022; Klingen and van Ommeren, 2021). As this literature highlights, the benefits associated with improving air quality are large and broad in scope.

Road transportation is one the largest sources of air pollution: in NYC, up to 30% of concentrations of local air pollutants are attributed to ICE vehicles (Matte et al., 2013). Reducing the impact of road traffic has become a priority in pollution abatement strategies of cities. The goal is for transportation users to reduce the number of trips or kilometres driven by ICE vehicles, either by increasing the cost they pay for using road transportation or by increasing the availability of less polluting modes of transportation. Bike share is one such alternative mode of transportation: by implementing it on a large scale, NYC effectively made cycling easier, more convenient and cheaper in vast areas of the city. This sudden change in alternatives has the potential to divert road transportation users to cycling, reducing kilometres driven and congestion, and ultimately decreasing air pollutants associated with ICE vehicles (Leroutier and Quirion, 2022).

This paper examines the evidence on the substitution mechanism using the universe of taxi trips and finds that, in areas served by bike share, short taxi trips (most similar to typical bike share trips) decrease faster than longer ones, indicating that bike share did substitute trips away from road transportation. This result echoes the research linking bike share and congestion, which finds that congestion decreased in areas where bike share is introduced (Wang and Zhou, 2017; Hamilton and Wichman, 2018).

This paper contributes to two main strands of the literature. First, it extends the environmental and urban economics literature on the impacts of transportation policies on air quality. Studies have investigated the impact of low-emission zones (Wolff, 2014; Jiang et al., 2017; Zhai and Wolff, 2021), congestion charge (Tonne et al., 2008; Green et al., 2020), and subway lines (Chen and Whalley, 2012; Gendron-Carrier et al., 2022). Bike share is often portrayed as an instrument to reach pollution abatement objectives, despite the limited evidence (DeMaio, 2009; Médard de Chardon, 2016). This paper aims to fill the gap in our understanding of the environmental impacts of bike share, and expand our knowledge of the impact of transport policies on air quality.

Second, an emerging "cycling" literature examines the effects of bike share on road traffic and air quality. Papers studying the impact on road traffic combine bike share deployment and data on congestion,[8] and find that bike share and other micromobility modes reduce congestion (Wang and Zhou, 2017; Hamilton and Wichman, 2018; Asensio et al., 2022).[9] Studies investigating the impact of bike share on air pollution rely on hypothetical rates of substitutions from ICE vehicles to bike share, limiting their accuracy (Fishman et al., 2014; Ricci, 2015; Médard de Chardon, 2016; Zhang and Mi, 2018; Qiu and He, 2018; Zheng, Gu, et al., 2019). To the best of my knowledge, Shr, Yang, et al. (2022) are the first to use causal inference methods to evaluate the impact of bike share on air quality, but find small effects on carbon monoxide and no effects on nitric oxides concentrations in Taiwan. My paper contributes to this literature by quantifying the causal effect on air pollution concentrations of the largest bike share program in North America, using high-resolution, observational measures of air quality over ten years. By presenting evidence that bike share substituted taxi service, my study also highlights that the source of substitution is key to understanding the potential effects that bike share may have on air quality.

The rest of the paper is organised as follows: section 1.2 develops the conceptual framework linking bike share to air pollution, section 2.3 the empirical strategy and section 2.2 presents the data, section 2.4 presents the results, section 1.6 explores the substitution mechanism, and section 2.5 concludes.

1.2 Conceptual framework

This section introduces a simple model of transport choice that will formalise the relationship between bike share and the propensity to choose cycling as a mode of transport. I then describe the main channel through which bike share may change air pollution concentrations (other channels are presented later in section 1.6.3).

1.2.1 Transport mode choice

The toy model described below is adapted from McFadden (1974b) and McFadden (1974a), as presented in Small and Verhoef (2007). For a given trip within the city, each individual n faces a menu of transport options $j = 1, \ldots, J$

[8]Congestion is commonly measured by travel speeds of road vehicles.

[9]These studies also find little evidence of traffic and congestion displacement.

(e.g., walking, public transport, taxi). Each option j is associated with costs c (e.g., ticket purchase, purchase or rent of a vehicle), travel time t and ease of access a (i.e., how far does the individual need to travel on foot to access mode j) for a given pair of origin-destination locations l. Utility is composed of a systematic utility $V(\cdot)$ which incorporates the characteristics of transport options as described above, and an unobservable component of utility ϵ_{jn} which captures idiosyncratic individual preferences. Individuals choose the transport option j that maximises their total utility:

$$U_{jln} = V(c_{jl}, t_{jl}, a_{jl}) + \epsilon_{jn} \tag{1.1}$$

The introduction of bike share abruptly increases the systematic utility of cycling in areas served by bike share: cycling is cheaper and more accessible. In other words, for areas L where bike share is implemented at time T, the utility of cycling ($j = J$) increases: $V_{t<T}(c_{JL}, t_{JL}, a_{JL}) < V_{t \geq T}(c_{JL}, t_{JL}, a_{JL})$. For individuals to switch to cycling after bike share, the utility associated with cycling after T needs to be greater than utilities for all other transport options:

$$\begin{aligned}
&\text{at } t > T: && U_{JLn} > U_{jLn} && \text{for all } j \neq J \\
&= && V_{JL} + \epsilon_{Jn} > V_{jL} + \epsilon_{jn} && \text{for all } j \neq J \\
&= && \epsilon_{Jn} - \epsilon_{jn} > V_{jL} - V_{JL} && \text{for all } j \neq J
\end{aligned}$$

If that condition is satisfied, and $j_{t<T} \neq J_{t<T}$ (i.e., the individual was not already cycling before bike share), then the individual will have switched to cycling thanks to bike share.

1.2.2 Substitution and impact on air quality

Having established the conditions that enable an individual to switch to cycling due to the availability to bike share, we now turn to how that translates into improving air quality. The change in air pollution is highly dependent on the mode of transport used in $t < T$: if the individual switched from a polluting mode of transport (i.e., an ICE vehicle), replacing those polluting trips with cycling will have an impact on air pollution concentrations. The substitution effect of bike share towards cycling is operative on air quality conditional on the previous mode of transport. On the other hand, if the individual switched to cycling from public transport or walking, the effect will be minimal.[10]

The key takeaway from this simple model is that the substitution mechanism implies that pollution reduces in areas where less polluting vehicles are driven after the implementation of bike share. In section 2.3, I use this spatial property of the substitution effect to construct a credible measure of where ICE traffic might have reduced thanks to bike share. This is used as the treatment variable in the empirical analysis to identify the causal impact of bike share on air pollution.

1.3 Empirical strategy

There are several empirical challenges to estimating the causal impact of bike share on air pollution concentrations. The first challenge is that air quality depends on a multitude of factors other than bike share. In addition, other changes that could be happening at the same time with the expansion of bike share could also change pollution concentrations. In this section, I present the identification challenges and how my empirical strategy overcomes these challenges.

The main challenge to identifying the causal relationship between bike share and air pollution is that bike share stations are not randomly placed across the city. From a city planning perspective, this makes sense: one has to ensure that the bike share program will be successful by reaching a large enough transport market. In the case of NYC, that meant that bike share was deployed first in Manhattan south of 60th street and downtown Brooklyn.[11] In subsequent years, bike share was expanded north on the island of Manhattan,

[10] Bike share may still have an indirect positive effect by decongesting public transport, which becomes more attractive to some ICE vehicle users who now switch to now less-congested public transport, thus reducing emissions. Another form of indirect substitution occurs when bike share and public transit are taken as complementary modes of transport: if bike share helps connect transportation hubs (e.g., railway or subway stations) to final destinations, a composite bike-share–public-transit transportation good might become a more attractive alternative to ICE vehicle users, inducing substitution.

[11] The highest concentration of transportation trips in NYC is located in the southern third of the island of Manhattan, which contains Midtown and Lower Manhattan, the city's most important business districts. While that area as a whole is fairly heterogenous, it does contain the major business centres and transportation hubs (Pennsylvania Station, Grand Central Station, Port Authority Bus Terminal), and generates an important share of NYC's transport demand.

and, across the East River, deeper into Brooklyn and Queens (gradual rollout is mapped in Figure 1.11).

The second identification challenge is that NYC is a large, heterogenous city, constantly evolving. A myriad of policies that might affect pollution concentrations are enacted every year, which could contaminate the estimate of the impact of bike share.

I address these challenges using a staggered difference-in-differences (DD) strategy. The empirical strategy compares areas affected by bike share with areas less affected, before and after the deployment of bike share. It takes advantage of the gradual rollout of the bike share program to sequentially identify treated and control areas. It then aggregates the estimated treatment effect of bike share over the several expansion waves to yield an overall average treatment effect.

I further define the treatment indicator by carefully identifying the areas where bike share is expected to affect pollutant concentrations the most, making it is less sensitive to the placement of bike share stations. As discussed below in greater detail, I define the treatment areas as areas where fewer cars are expected to drive, thus covering a larger tract of land than the stations themselves. This approach exploits the fact that bike share's *area of influence* changed over the years at a seemingly random pace.

More specifically, recall that the units of analysis are 300 by 300-metre cells, for a total of 9,760 cells across NYC. Each of these cells will be considered treated if it lies on the route between two bike share stations that exchanged a bike during a given year. In other terms, the treated cells are the cells through which ICE traffic is expected to decrease, which, because cars tend to be constrained by the road network, includes many cells where bike share stations are not accessible. This constitutes the first element that introduces variation on the spatial scale that is unrelated to the placement of bike share stations.

Furthermore, the expansion of the bike share network was carried out in a staggered and irregular fashion. The history of NYC's bike share deployment makes for a fitting illustration. Bike share's first deployment in NYC was originally scheduled for Summer 2012, but due to major software issues it was delayed first to Fall 2012 and then Spring 2013. At the same time, the spatial extent of the initial launch was significantly scaled down as a result of extensive damages from Hurricane Sandy in October 2012.[12] The first expansion of the system was similarly pushed back by a year to 2015 because of remaining software issues.[13] Thus, while the *order* through which areas of the city received bike share is arguably not random, the *exact timing* (i.e., the year it received bike share) and the *precise spatial extent* of the extension (e.g., why stop at 59th street and not 79th street?) are relatively random. Combined with the fact that the treatment definition used in this study is a superset of the area where stations are deployed based on car itineraries, the precise timing and extent of the treatment variable provides enough variation to act as a valid treatment definition for a DD setting. In section 1.5.3, I perform several tests to back up these claims.

In addition to a credible treatment, the empirical strategy relies on two main assumptions. First, control and treatment areas must follow parallel trends in terms of pollution concentrations before the deployment of bike share. In other words, the difference in concentrations between the treated and control group should be stable before treatment. This central DD assumption is necessary in order to take the post-treatment trend of the control group as a credible counterfactual for the post-treatment trend of the treatment group had bike share not been implemented. I test for parallel trends in section 1.5.1.2 using a dynamic DD specification.

Second, staggered DD assumes that no other concomitant policy that could have affected pollution concentrations was enacted at the same time and place as/where bike share was rolled out. This assumption requires that there are no omitted and concurrent policies that might explain the change in air quality. This means that treated areas (the areas where bike share is expected to lower traffic emissions) should not have been subjected to policies that could have reduced air pollution. While many policies and programs are enacted every year in NYC, it appears unlikely that these policies would have targeted the precise areas where bike share is expected to reduce pollution, and not

[12] Originally planned to extend up to 79th street, the launch finally covered areas south of 59th street only.

[13] The considerable reworking of plans and schedule was reported at the time by the press (see for example https://archive.ph/jZl4 and https://www.wnyc.org/story/284420-breaking-citibank-is-sponsor-of-nyc-bike-share-citibike/), and confirmed during several private conversations I had with people involved in the planning of NYC's bike share program. Apart from major software issues and Hurricane Sandy, they also noted that the initial lack of enthusiasm for the program on the part of Bill De Blasio (NYC's new mayor in 2014) decreased available resources for further expansions in the short run.

the others. For example, the city introduced a new type of taxi in 2013, the boro taxi. Launched to increase transport options in neighbourhoods outside Manhattan, boro taxis can only pick up passengers North of 100th street in Manhattan and everywhere else in the other boroughs. While the launch date closely matches the opening of the bike share program, the boro taxis program, if it affected traffic and pollution, affected it in areas different from the ones I code as treated by bike share. It is therefore unlikely that a policy followed the same spatial and temporal pattern as bike share's extension.

One of the central requirements for this estimation strategy is the precise identification of areas where pollution concentrations might have been affected by bike share. Using the bike share trips dataset, I construct a new variable to capture the impact of bike share on air pollution. As described in section 1.2, the impact of bike share on air quality depends on the substitution away from ICE vehicles as a result of bike share. If there is any substitution away from ICE vehicles, we would expect air pollution concentrations to decrease in areas where fewer vehicles are driven because of bike share.

To identify those areas, I use the bike share trip data to first compute the unique origin-destination pairs of stations (i.e., pairs of stations that exchanged at least one bike during a given year). Using the `r5r` package in R, a routing algorithm able to take into account the road network at different periods (Pereira et al., 2021), for each pair of stations I compute the optimal car route that would have been driven if the bike trip between these stations would have been made by car. For each year, there are on average 24K unique origin-destination pairs of bike share stations, totalling 500K computed car routes.[14]

This exercise produces the optimal car routes between unique pairs of bike share stations for each year. Next, I intersect these car routes (including a 150-meter buffer on both sides) with the grid cells, identifying the cells crossed by a car route. Finally, I input the number of bike share trips associated with each pair of stations to the car routes, and sum the number of bike share trips for each cell that are crossed by car routes. I end up with a dataset of cells that measure the total bike share traffic, but in areas where cars would have been

[14] The r5r routing algorithm is well-suited to this application for several reasons. First, contrary to the Google Maps API, it takes into account the road network in place at a given time, letting me compute realistic routes across my sample period. Second, it is run on a local computer, which greatly reduces computational time. Finally, it is free and open-source, which makes the algorithm and processing transparent and reproducible.

driven. In other words, I now have a yearly variable that captures where (at the extensive margin) we would expect air pollution to reduce after bike share, and, where we would expect it to reduce the most (at the intensive margin) as captured by the number of bike share trips. The result is displayed as a map in Figure 1.12.[15]

The econometric specification used to estimate the staggered DD is described by equation 1.2. It is a standard two-way fixed effects (TWFE) model with multiple treatment periods:

$$Y_{ct} = \beta \cdot Treat_{ct} + \phi_t + \gamma_c + C_{ct_0} + \varepsilon_{ct} \tag{1.2}$$

Y_{ct} denotes the concentration of a given pollutant in cell c at time t; $Treat_{ct}$ is a binary variable indicating whether cell c is treated by bike share at time t; ϕ_t and γ_c are year and cell fixed effects, respectively, which control for any invariant cell characteristics and time trends; C_{ct_0} is a vector of controls for cell c at period t_0 right before treatment; ε_{ct} is the error term. Standard errors are clustered at the community district level,[16] and in a robustness check I follow Conley (1999) to compute standard error robust to spatial dependence.

The coefficient of interest is β, which represents the average treatment effect on the treated for the whole post-treatment period. In other words, it is the average change in pollution concentration for a cell being crossed by a car route. If the treatment is continuous (i.e., the number of bike share trips), β is interpreted as the effect of an additional bike share trip on pollution concentration for an average treated cell.

The average treatment effect is a valuable metric, but one might, however, also be interested in the dynamic effect of treatment with respect to time. The dynamic DD specification, also known as an event study, plots the treatment effect for all periods. The dynamic specification also allows us to test for differential pre-tends between groups: by plotting the difference between

[15] Alternatively, I define treatment for a given cell during a given year as (1) being within 300 metres of a bike share station, or (2) being contained within the smallest convex polygon encompassing all stations. The first alternative definition is based on the proximity to the bike share system, while the second is also based on proximity but incorporates areas *between* treated areas as being treated, even though they might be far from a station. Both, however, do not model the footprint of cars that might be substituted as a consequence of bike share expansion.

[16] Community districts are local, within-borough neighbourhood advisory boards. There are 59 such districts across NYC, plus 12 non-district areas (e.g., parks, beaches, airports), for a total of 71 clusters.

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 1.3:

$$Y_{ct} = \sum_{k=-9}^{-2} \beta_k \cdot Treat_{ck} + \sum_{k=0}^{6} \beta_k \cdot Treat_{ck} + \phi_t + \gamma_c + C_{ct_0} + \varepsilon_{ct} \tag{1.3}$$

where k denotes the relative time to the first year of treatment, and the other terms are 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_k denote the relative difference between treatment and control groups compared to the period right before treatment.

A recent strand of the econometric literature has brought into question the TWFE estimator with multiple treatment times. In particular, Goodman-Bacon (2021) shows that TWFE reports biased average treatment effects, and those distortions are particularly pronounced when (1) the size of the control group is small, and (2) the average treatment effect varies over time. Several robust estimators have since been proposed in the literature, with notable contributions including de Chaisemartin and D'Haultfœuille (2020), Callaway and Sant'Anna (2021), Sun and Abraham (2021), and Borusyak et al. (2022). While my setting does not suffer from a small control group, I cannot rule out that the average treatment effect varies across cohorts. To confirm the results obtained from the TWFE model, I use the estimator developed by Borusyak et al. (2022). This estimator has several advantages compared to alternative robust estimators, the main one being its high efficiency in computing standard errors.

1.4 Data

This section describes the data used to carry out the main analysis and the testing of the substitution mechanism.

1.4.1 Air pollution

Spatial-temporal concentrations of air pollutants are the main outcome variables of the analysis. The New York City Department of Health and Mental

Hygiene (NYCDOH) provides yearly, high-resolution air pollution data covering the city's territory, divided into 9,670 300-metre square cells since 2009. This subsection describes these data.

The project known as the New York City Community Air Survey (NYCCAS) has been conducted by the NYCDOH and Queens College since December 2008. It aims to evaluate air quality at the street level by measuring six major air pollutants: fine particulate matter (particles smaller than 2.5 micrometres, i.e. PM 2.5), black (or elemental) carbon (BC), nitric oxide (NO), nitrogen dioxide (NO_2), ozone (O_3) and sulfur dioxide (SO_2).[17] Up to 150 monitors are distributed around the city every year, with 80% percent of sites selected randomly, while the remainder are purposefully chosen near areas of interest and to guarantee minimal coverage of neighbourhoods. The data collected from the monitors is then processed and used to calibrate a land-use regression (LUR) model which in turn produces the final raster grid dataset. Each of the 9,670 300-metre cells measures the annual average concentration of a given pollutant in that cell. Details on air pollutant measurements and the LUR model are presented in Clougherty et al. (2013) and Matte et al. (2013).

Several features make the NYCCAS a relatively unique air quality dataset. First, it offers measurement for a wide range of air pollutants. Second, pollution concentrations are given at a high spatial resolution (300-metre cells, when satellite-based resolutions range from 1 to 2 kilometres, see for example Gollin et al. (2021) or Gendron-Carrier et al. (2022)), enabling much more detailed analysis within the city. Finally, the NYCCAS is continuously available before and after the start of bike share, making pre and post-bike-share comparisons possible.

I focus on a subset of pollutants measured by the NYCCAS. Two criteria were used to select pollutants: (1) the pollutant should be associated with ICE vehicles, and (2) it should be measured close to its emission source. Criterium (1) ensures that the pollutant is relevant to the main mechanism (i.e., substitution), while (2) is necessary to narrow down the potential area of influence of bike share. The selected pollutants are nitrous oxides (NO and NO_2), and particulate matter (PM 2.5 and BC). Nitrous oxides are gases and common markers of ICE vehicle traffic, with 30% of concentrations attributed to on-road vehicles. Nitrous oxides have a relatively steep concentration

[17] O_3 is measured during the Summer season only. SO_2 was measured in Winter only until 2017, when its measurement was discontinued due to concentrations below detection limits.

gradient, which means that concentrations decrease faster the further away from the emission source. PM 2.5 captures all the particles that are smaller than 2.5 micrometres (about 1/20th the diameter of a human hair). Its health impacts have been extensively studied, which makes it a popular pollutant to focus on. BC (also called soot or elemental carbon) is a subset of PM associated with Diesel engine emissions.

1.4.2 Bike share

The bike share data is publicly available on the website of the service provider, Citi Bike.[18] The data consists of the universe of bike share trips made on the system since its start in May 2013, as required by the service agreement with the city of New York. Each trip is characterised by origin and destination station (unique ID, name and geographic coordinates), departure and arrival timestamps, and an indicator variable for subscriber riders. If the rider is a subscriber, year of birth and gender are reported. Several cleaning operations are made on trip data to remove potentially problematic observations: temporary and service stations are removed using identified keywords; round trips (i.e., identical origin and destination station) are removed; and trips under three minutes and over six hours are dropped. The final dataset contains about 100 million trips from May 2013 to December 2019.[19] The gradual rollout of the bike share stations is mapped in Figure 1.11.

1.4.3 Spatial and time-varying variables

I collect several spatially distributed and time-varying control variables. The location and type of cycling lanes are obtained from the New York City Department of Transportation (NYCDOT). A large amount of data cleaning and manual processing was necessary in order to correctly classify changes in cycling-lane types. Cycling lane lengths per type were then computed for each 300-metre cell.

The American Community Survey (ACS) is used to gather socio-demographic variables. I use the ACS 5-year, which collects data on a rolling 5-year basis and delivers estimates at the census tract resolution. Census tract values are then imputed to the 300-metre cells using the areal interpolation functions

[18] `https://ride.citibikenyc.com/system-data/`, accessed 2022-10-20.

[19] I described the building of the bike share treatment variable using these data in section 2.3.

from the `areal` R package (Prener and Revord, 2019).[20] Variables extracted from the ACS include population, median household income, and level of tertiary education.

Zoning and building information are sourced from the New York City Department of City Planning (NYCDCP) and its PLUTO dataset, which provides detailed land use and geographic data at the tax lot level. These data are also aggregated at the 300-metre cell level using areal interpolation.

Finally, I obtain the map of community districts from the NYC Open Data portal, and assign each cell to one of the 59 community districts (and 12 remaining areas, usually parks) its area covers the most.

1.4.4 Summary statistics

The variables used in the analysis are summarised in Table 1.1. The yearly counts of cells crossed by a car route (main treatment variable) are displayed in Table 1.2.

[20] Each cell takes the weighted average of census tracts with weights given by the share of each tract's area covering the cell.

Table 1.1: Summary statistics

	Mean	SD	Min	Max
Treatment group				
Bike share trips	184,432.4	425,979.5	0	3393072
NO (parts per billion)	22.36	9.27	8.89	71.85
NO2 (parts per billion)	23.28	4.44	11.25	47.46
PM 2.5 (microgram per cubic metre)	9.45	1.61	5.99	17.63
BC (absorption)	1.11	0.3	0.47	2.61
College graduates (count)	592.12	679.39	0	4,815.52
Population	1,589.34	1,161.32	0	7,311.22
Population over 25	1,149.99	880.62	0	5,832.03
Median household income (2019 $)	69,168.22	44,223.59	0	272,587.4
Protected cycle lane (ft)	318.34	579.58	0	3,522.82
Painted cycle lane (ft)	425.09	710.55	0	4,540.39
Cycle route (ft)	156.77	384.82	0	2,946.44
At-least-painted cycle lane (ft)	743.43	887.22	0	5,729.1
Built surface (sq ft)	1,451,731	1,744,487	0	22,156,429
Office area (sq ft)	296,846.4	1,079,304	0	13,503,541
Residential area (sq ft)	784,423.6	821,027.6	0	5,625,712
Commercial area (sq ft)	641,170.4	1,386,116	0	19,326,407
Retail area (sq ft)	78,493.02	169,277.3	0	3,790,754
Other floor area (sq ft)	2.79	16.26	0	1,047.57
Control group				
Bike share trips	0	0	0	0
NO (parts per billion)	14.86	5.22	5	80.52
NO2 (parts per billion)	16.62	4.15	5.38	32.27
PM 2.5 (microgram per cubic metre)	7.94	1.3	5.36	13.2
BC (absorption)	0.78	0.24	0.17	3.23
College graduates (count)	132.32	158.72	0	1,910.09
Population	734.29	837.89	0	7,394.62
Population over 25	492.46	551.1	0	4,916.74
Median household income (2019 $)	63,418.2	37,975.29	0	293,774.6
Protected cycle lane (ft)	101.97	373.93	0	6,913.87
Painted cycle lane (ft)	105.04	368.01	0	3,974.35
Cycle route (ft)	53.82	233.34	0	3,086.08
At-least-painted cycle lane (ft)	207.01	525.26	0	7,911.46
Built surface (sq ft)	382,765.3	516,905.7	0	49,915,505
Office area (sq ft)	16,346.77	52,503.43	0	1,046,949
Residential area (sq ft)	284,622.9	338,853.6	0	2,919,417
Commercial area (sq ft)	93,149.03	250,471.1	0	10,218,426
Retail area (sq ft)	17,338.82	46,285	0	1,103,965
Other floor area (sq ft)	0.75	6.83	0	992.49

Table 1.2: Treatment yearly summary

Year	Cell on car route	Count	Percent
2013	0	8510	92.79
	1	661	7.21
2014	0	8512	92.81
	1	659	7.19
2015	0	8165	89.03
	1	1006	10.97
2016	0	7903	86.17
	1	1268	13.83
2017	0	7599	82.86
	1	1572	17.14
2018	0	7547	82.29
	1	1624	17.71
2019	0	7442	81.15
	1	1729	18.85

1.5 Results

In this section, I report the average treatment effect of bike share on air pollution concentrations obtained from the TWFE estimation. Event studies plotting the dynamic impact of bike share on concentrations are presented, for both standard TWFE and the Borusyak, Jaravel and Spiess (BJS) estimator. Tests on the validity of the empirical setting are then presented.

1.5.1 TWFE results

1.5.1.1 TWFE Average treatment effects

The results are presented for each selected pollutant in turn in Tables 1.3 to 1.6. Standard errors are clustered at the community-district level.[21] My preferred specification includes cell and year fixed-effects with baseline controls (Column 2).

Table 1.3 shows the impact on NO concentrations for a cell being on a car route between bike share stations. In my preferred specification with baseline controls (Column 2), concentrations of NO decrease on average by 2.7 parts per billion for cells on a car route. This coefficient represents a 13.4% decrease

[21] Conley (1999) standard errors robust to spatial dependence are reported in Appendix 1.B.

Table 1.3: Effect of bike share on NO concentrations

	NO	
	(1)	(2)
On-car-route	-2.5360***	-2.7281***
	(0.8595)	(0.8543)
Baseline controls		✓
Cell FE	✓	✓
Year FE	✓	✓
Mean concentration pre-treat.	20.322	20.353
% mean concentration pre-treat.	-12.479	-13.404
Observations	91,710	90,898
R^2	0.906	0.908
Within R^2	0.049	0.066

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.4: Effect of bike share on NO2 concentrations

	NO2	
	(1)	(2)
On-car-route	-1.1489***	-1.2554***
	(0.2771)	(0.2759)
Baseline controls		✓
Cell FE	✓	✓
Year FE	✓	✓
Mean concentration pre-treat.	19.950	20.007
% mean concentration pre-treat.	-5.759	-6.275
Observations	91,710	90,898
R^2	0.978	0.979
Within R^2	0.081	0.123

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

with respect to mean concentrations for the whole sample before 2013. Table 1.4 reports the average treatment effect of bike share on NO_2 concentrations. Bike share significantly reduces NO_2 concentrations in areas where fewer cars are likely to be driven. The decrease in NO_2 represents about 6.2% of pre-treatment concentrations. The impact on BC is reported in Table 1.5. For BC also, concentrations reduce in areas where fewer cars are expected to be driven due to bike share. The coefficient represents a 2.8% decrease compared to BC concentrations before the first implementation of bike share in 2013. Finally, the effect on PM 2.5 is shown in Table 1.6. The effect of bike share on PM 2.5 concentrations is indistinguishable from zero, but has the expected negative sign. From these results, it appears that bike share has reduced the concentration of NO, NO_2 and BC by 2.8 to 13.4%, but had no effect on PM 2.5 concentrations.

Table 1.5: Effect of bike share on BC concentrations

	BC	
	(1)	(2)
On-car-route	-0.0253*	-0.0280**
	(0.0128)	(0.0129)
Baseline controls		✓
Cell FE	✓	✓
Year FE	✓	✓
Mean concentration pre-treat.	1.015	1.017
% mean concentration pre-treat.	-2.494	-2.757
Observations	91,710	90,898
R^2	0.956	0.956
Within R^2	0.006	0.011

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Tables 1.3 to 1.6 investigate the extensive margin of the impact of bike share, with the treatment being an indicator variable taking the value of 1 if the cell is crossed by a car route, and zero otherwise. These results do not take into account the intensity of bike share activity and produce an average treatment effect on the treated, irrespective of the level of treatment. Next, I investigate the impact of treatment intensity on pollution concentrations.

Table 1.6: Effect of bike share on PM concentrations

	PM	
	(1)	(2)
On-car-route	-0.0097	-0.0320
	(0.0686)	(0.0688)
Baseline controls		✓
Cell FE	✓	✓
Year FE	✓	✓
Mean concentration pre-treat.	9.433	9.441
% mean concentration pre-treat.	-0.103	-0.339
Observations	91,710	90,898
R^2	0.978	0.979
Within R^2	0.000	0.018

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Recall from section 2.3, I compute, for each car route between pairs of bike share stations, the number of bike share trips for that pair. I then aggregate the number of bike share trips for each cell. In Tables 1.7 to 1.10, I report the effect of the number of bike share trips in cells crossed by car routes. I present the results for both the number of bike share trips measured in 10,000 and the inverse hyperbolic sine transformation of the number of bike share trips.[22]

For NO (Table 1.7), 10,000 additional bike share trips through a cell crossed by a car route reduce concentrations by 0.086 parts per billion, or 0.4% of the pre-2013 mean NO concentration. For a 1% increase in bike share trips, NO concentration would decrease by 0.003 parts per billion, or 0.015% of pre-2013 mean concentrations. The impact of an additional 10,000 bike share trips is smaller for NO_2 but still statistically significant (Table 1.8): concentrations would reduce by 0.13% with respect to the pre-2013 mean. The impact of a 1% increase in bike share trips would result in a 0.006% decrease from the pre-treatment mean. BC concentrations would reduce by

[22]The inverse hyperbolic sine (IHS) is a type of log transformation, particularly suited for variables with a large share of zeros (MacKinnon and Magee, 1990; Bellemare et al., 2013). The IHS value is given by $IHS(x) = ln\left(x + \sqrt{x^2 + 1}\right)$. Its interpretation is the same as a typical log transformation: the coefficient divided by 100 represents the change in the outcome variable when the independent variable increases by 1%.

Table 1.7: Effect of bike share on NO concentrations

	NO			
	(1)	(2)	(3)	(4)
Trips (10K)	-0.0839***		-0.0860***	
	(0.0115)		(0.0114)	
Trips (IHS)		-0.2758***		-0.2947***
		(0.0850)		(0.0845)
Baseline controls			✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean concentration pre-treat.	20.322	20.322	20.353	20.353
% mean concentration pre-treat.	-0.413	-1.357	-0.423	-1.448
Observations	91,710	91,710	90,898	90,898
R^2	0.927	0.909	0.929	0.911
Within R^2	0.257	0.077	0.279	0.097

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.8: Effect of bike share on NO2 concentrations

	NO2			
	(1)	(2)	(3)	(4)
Trips (10K)	-0.0253***		-0.0263***	
	(0.0034)		(0.0033)	
Trips (IHS)		-0.1119***		-0.1218***
		(0.0256)		(0.0254)
Baseline controls			✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean concentration pre-treat.	19.950	19.950	20.007	20.007
% mean concentration pre-treat.	-0.127	-0.561	-0.131	-0.609
Observations	91,710	91,710	90,898	90,898
R^2	0.981	0.979	0.982	0.980
Within R^2	0.188	0.103	0.231	0.147

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

0.1% for ten thousand additional bike share trips, and 0.003% for a 1% increase. Finally, PM concentrations decrease by 0.035% for 10,000 additional bike share trips (statistically significant), but a 1% increase in bike share trips does not significantly reduce concentrations.

Table 1.9: Effect of bike share on BC concentrations

	BC			
	(1)	(2)	(3)	(4)
Trips (10K)	-0.0010***		-0.0010***	
	(0.0002)		(0.0002)	
Trips (IHS)		-0.0032***		-0.0035***
		(0.0012)		(0.0012)
Baseline controls			✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean concentration pre-treat.	1.015	1.015	1.017	1.017
% mean concentration pre-treat.	-0.097	-0.316	-0.100	-0.342
Observations	91,710	91,710	90,898	90,898
R^2	0.958	0.957	0.958	0.957
Within R^2	0.046	0.014	0.052	0.019

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

1.5.1.2 TWFE Dynamic effects

I now report the estimates for the dynamic TWFE specification. In this specification, I estimate the impact of bike share on pollution concentrations for every period relative to treatment. Plotting the results yields an event study, which lets us evaluate the parallel trends before treatment assumption (i.e., no statistically significant differences in the trends of pollution concentrations between the control and treatment group before the treatment), and observe the dynamics of the effect over time. The estimates plotted control for baseline covariates, with standard errors clustered at the community district level.

Figure 1.1 plots the dynamic effect of being crossed by a car route between two bike share stations on the concentration of NO. In the period leading up to treatment (the left half of the plot), we notice no statistically significant

Table 1.10: Effect of bike share on PM concentrations

	PM			
	(1)	(2)	(3)	(4)
Trips (10K)	-0.0031***		-0.0033***	
	(0.0011)		(0.0011)	
Trips (IHS)		-0.0036		-0.0057
		(0.0067)		(0.0067)
Baseline controls			✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Mean concentration pre-treat.	9.433	9.433	9.441	9.441
% mean concentration pre-treat.	-0.033	-0.038	-0.035	-0.060
Observations	91,710	91,710	90,898	90,898
R^2	0.979	0.978	0.979	0.979
Within R^2	0.033	0.001	0.055	0.020

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

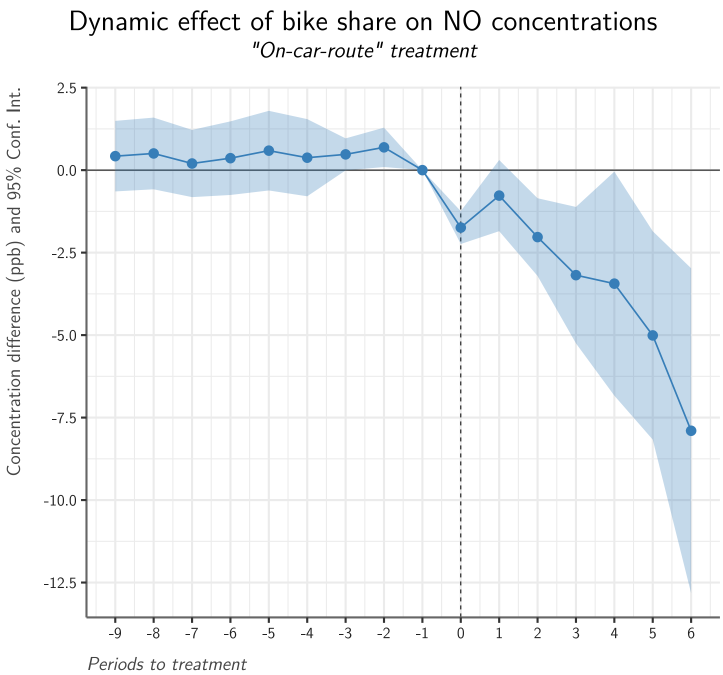


Figure 1.1: Dynamic effect of bike share on NO concentrations

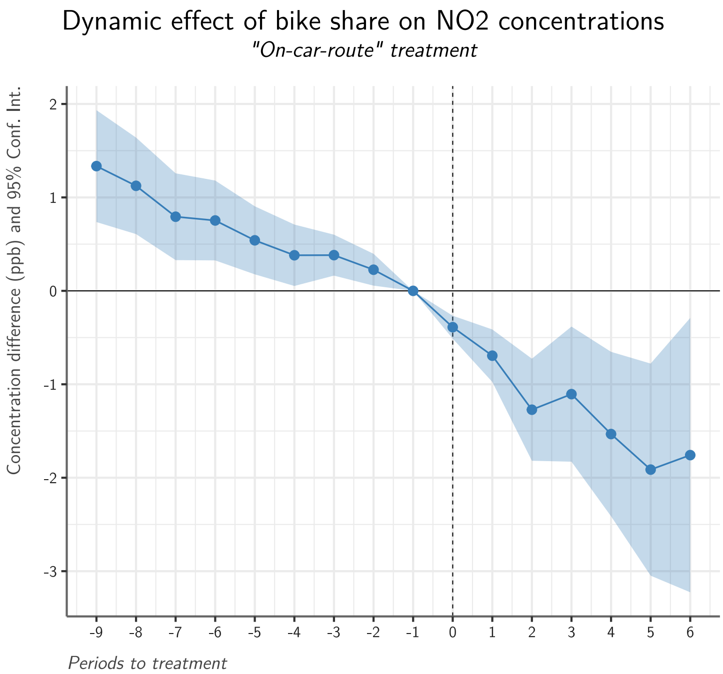


Figure 1.2: Dynamic effect of bike share on NO_2 concentrations

differences in pollution concentrations between the treated and control group, providing good support for the parallel trends assumption. A decreasing trend after treatment indicates that treated cells see their NO concentrations drop after bike share is implemented and are crossed by car routes. The effect gets larger and is persistent over time. Turning to NO_2 in Figure 1.2, we also notice and significant decrease post-bike-share introduction. In the period leading to treatment, however, treated cells displayed a statistically significant difference in NO_2 concentration with control cells, which does not support the parallel trends assumption. Dynamic effects of bike share on BC are displayed in Figure 1.3. The parallel trends assumption seems reasonably supported by the data, and there is a statistically significant and persistent decrease in BC concentrations after bike share introduction. Finally, there are no discernible patterns of the impact of bike share on PM 2.5 concentrations (Figure 1.4).

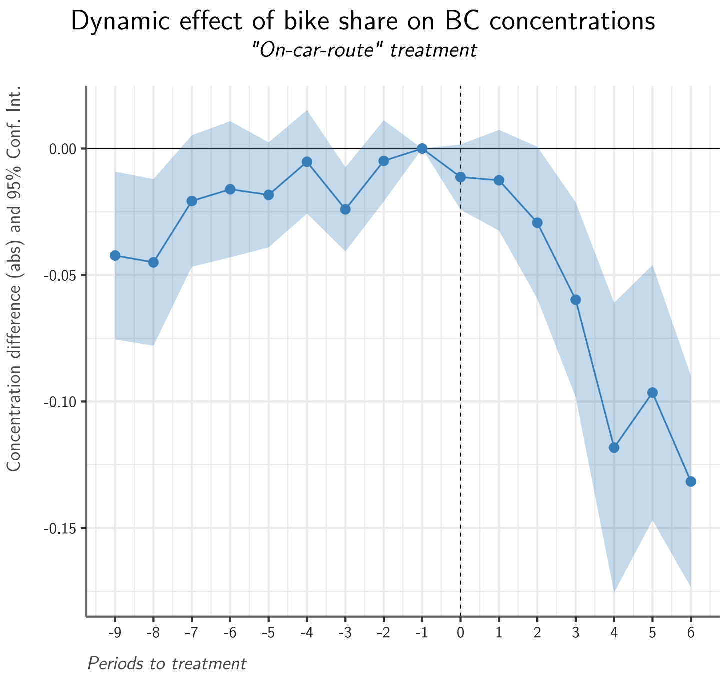


Figure 1.3: Dynamic effect of bike share on BC concentrations

1.5.2 Borusyak, Jaravel and Spiess (2022) Dynamic effects

Under some conditions, TWFE estimates have been shown to be biased for settings with multiple treatment periods. While the present setting does not suffer from the worst pitfalls of TWFE (e.g., small or non-existent never treated), TWFE is still subject to potential bias if the ATT varies over time. To mitigate these concerns, I report the dynamic effects of bike share on pollution concentrations using the Borusyak et al. (2022) (BJS) estimator.

Figure 1.5 shows the dynamic effect of being crossed by a car route for cells at each period relative to treatment on the concentration of NO. In the pre-treatment period, we notice a slight negative pre-trend for the treated group, which may indicate that treated cells were not following a parallel trend in NO concentration before treatment. In the post-period, we see a decrease, persistent in time, for the treated group. NO_2 (Figure 1.6) now displays reasonable parallel trends between groups prior to treatment, and a significant and persistent decrease in NO_2 concentration for the treated group after treatment. BJS dynamic effects (Figure 1.7) confirm TWFE results for BC,

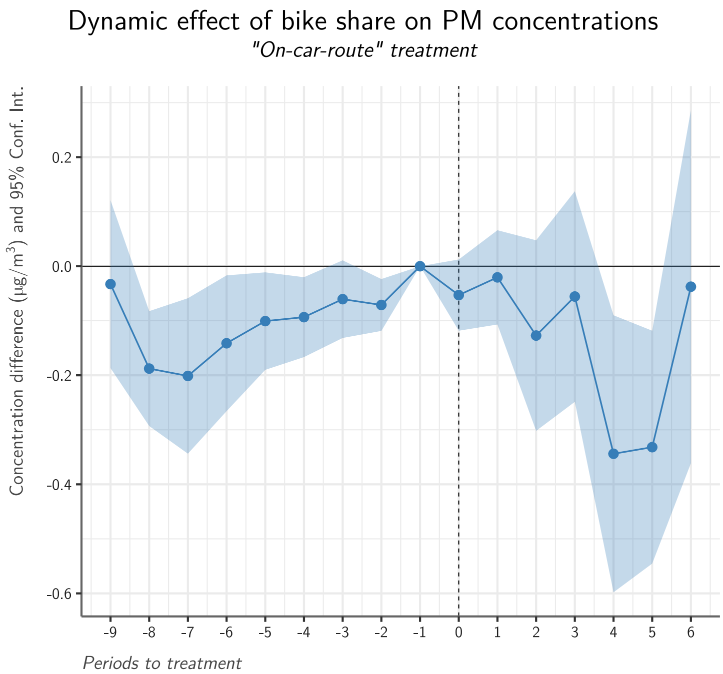


Figure 1.4: Dynamic effect of bike share on PM 2.5 concentrations

with pre-trends consistent with the parallel trend assumption and a negative and persistent effect of bike share on BC concentrations. Finally, the effects of bike share on PM 2.4 (Figure 1.8) remain indistinguinshable from zero.

1.5.3 Testing treatment exogeneity

DD estimations rely on the assumption that treatment status and timing are orthogonal to covariates. I now turn to the testing of the exogeneity assumption, both on the cross-sectional level and the temporal level. To check for the cross-sectional exogeneity of treatment, I run a linear probability model on the treatment status, using a battery of covariates averaged for years before 2013. The estimating equation takes the form of

$$Y_i = \beta_0 + \beta X_{i\bar{t}} + \gamma_b + \varepsilon_i \tag{1.4}$$

where Y_i is the binary treatment status (0 if never treated, 1 if eventually treated) of cell i, $X_{i\bar{t}}$ the vector of covariates average over the pre-treatment

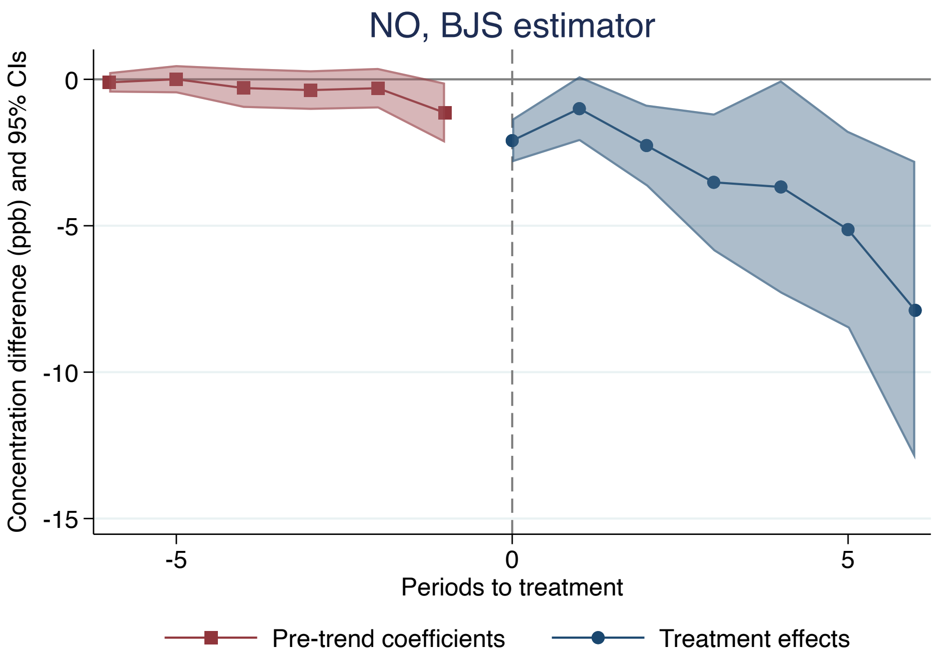


Figure 1.5: Dynamic effect of bike share on NO concentrations, BSJ estimator

period $\bar{t}$ (i.e., before 2013), β the vector of coefficients associated with the covariates, and γ_b are borough fixed effects.

Results of the linear probability model are reported in Table 1.11. Magnitudes are in general fairly small, but a handful of covariates are systematically associated with a higher probability of a cell having a car route crossing it. In preferred specifications with borough fixed effects (columns 3 and 4), higher levels of residential built area, office area and general built area are associated with a higher probability of treatment. Bike lane length seems to be associated with a higher probability of treatment, but the different types take varying statistical significance depending on the specification.[23] Population and median income are predictors of treatment in specifications without borough fixed effects, but their predictive power vanishes when adding them. Finally, higher counts of college graduates per cell are associated with a higher probability of treatment.

Table 1.12 estimates the same model as in 1.4, with the year of treatment

[23] These measures of bike lanes are highly correlated to each other, and might be relatively collinear.

Table 1.11: Effects of baseline covariates on probability of treatment

	Probability of being treated			
	(1)	(2)	(3)	(4)
Constant	0.1003***	0.1003**		
	(0.0092)	(0.0475)		
Other floor area (sq ft)	0.0007	0.0007	0.0017	0.0017
	(0.0015)	(0.0029)	(0.0020)	(0.0018)
Retail area (sq ft)	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Residential area (sq ft)	0.0000	0.0000	0.0000*	0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Office area (sq ft)	0.0000***	0.0000*	0.0000*	0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Commercial area (sq ft)	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Built area (sq ft)	0.0000	0.0000	0.0000*	0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
At-least-painted cycle lanes (ft)	-0.0012	-0.0012	0.0000***	0.0000
	(0.0010)	(0.0009)	(0.0000)	(2.8020)
Cycle route (ft)	0.0001***	0.0001*	0.0001	0.0001**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Painted cycle lanes (ft)	0.0014	0.0014	0.0001***	0.0001
	(0.0010)	(0.0009)	(0.0000)	(2.6784)
Protected cycle lanes (ft)	0.0015	0.0015*	0.0000	0.0000
	(0.0010)	(0.0009)	(0.0000)	(2.8896)
Population over 25	-0.0002***	-0.0002	-0.0002	-0.0002
	(0.0001)	(0.0002)	(0.0002)	(0.0001)
Population	0.0001***	0.0001	0.0001	0.0001
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Median household income	0.0000***	0.0000**	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
College graduates	0.0005***	0.0005***	0.0003**	0.0003***
	(0.0000)	(0.0001)	(0.0001)	(0.0001)
Borough FE			✓	✓
Standard-errors	Robust	Cluster CD	Cluster CD	Conley (0.59km)
Observations	9,089	9,089	9,089	9,089
Squared Correlation	0.277	0.277	0.391	0.391
Pseudo R^2	0.288	0.288	0.506	0.506
BIC	6,473.067	6,473.067	4,568.309	4,568.309

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Robust indicate heteroskedasticity-consistent standard errors, *Cluster CD* denote standard errors clustered at the community district, and *Conley* are standard errors robust to spatial dependence computed following Conley (1999).

Table 1.12: Effects of baseline covariates on timing of treatment

	Year of treatment			
	(1)	(2)	(3)	(4)
Constant	9,199.0699***	9,199.0699***		
	(82.7379)	(380.8176)		
Other floor area (sq ft)	-5.8446	-5.8446	-13.6360	-13.6360
	(14.7006)	(26.0137)	(15.8736)	(14.1585)
Retail area (sq ft)	0.0003	0.0003	0.0003	0.0003
	(0.0007)	(0.0011)	(0.0008)	(0.0007)
Residential area (sq ft)	0.0026	0.0026	0.0043*	0.0043**
	(0.0064)	(0.0052)	(0.0025)	(0.0021)
Office area (sq ft)	0.0011***	0.0011	0.0009*	0.0009**
	(0.0004)	(0.0007)	(0.0005)	(0.0005)
Commercial area (sq ft)	0.0012	0.0012	0.0032	0.0032
	(0.0064)	(0.0052)	(0.0025)	(0.0022)
Built area (sq ft)	-0.0027	-0.0027	-0.0043*	-0.0043**
	(0.0064)	(0.0052)	(0.0025)	(0.0021)
At-least-painted cycle lanes (ft)	15.9987	15.9987	-0.6707***	-0.6707
	(33.4240)	(28.2850)	(0.1279)	(29,801.1007)
Cycle route (ft)	-0.5449***	-0.5449	-0.4542	-0.4542**
	(0.1906)	(0.4194)	(0.3013)	(0.2287)
Painted cycle lanes (ft)	-15.3254	-15.3254	-0.3133*	-0.3133
	(33.4210)	(28.4746)	(0.1598)	(30,109.0169)
Protected cycle lanes (ft)	-13.5124	-13.5124	0.1876	0.1876
	(33.4204)	(28.3052)	(0.1434)	(29,933.4526)
Population over 25	1.6746***	1.6746	1.6433	1.6433
	(0.6230)	(2.3883)	(1.7218)	(1.1214)
Population	-0.9936***	-0.9936	-0.9489	-0.9489
	(0.3711)	(1.3676)	(0.9755)	(0.6694)
Median household income	0.0065***	0.0065*	0.0017	0.0017
	(0.0008)	(0.0034)	(0.0031)	(0.0014)
College graduates	-3.6981***	-3.6981***	-2.3044**	-2.3044***
	(0.3124)	(1.0816)	(0.9850)	(0.5169)
Borough FE			✓	✓
Standard-errors	Robust	Cluster CD	Cluster CD	Conley (0.59km)
Observations	9,089	9,089	9,089	9,089
R^2	0.013	0.013	0.391	0.391
Within R^2			0.112	0.112

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Robust indicate heteroskedasticity-consistent standard errors, *Cluster CD* denote standard errors clustered at the community district, and *Conley* are standard errors robust to spatial dependence computed following Conley (1999).

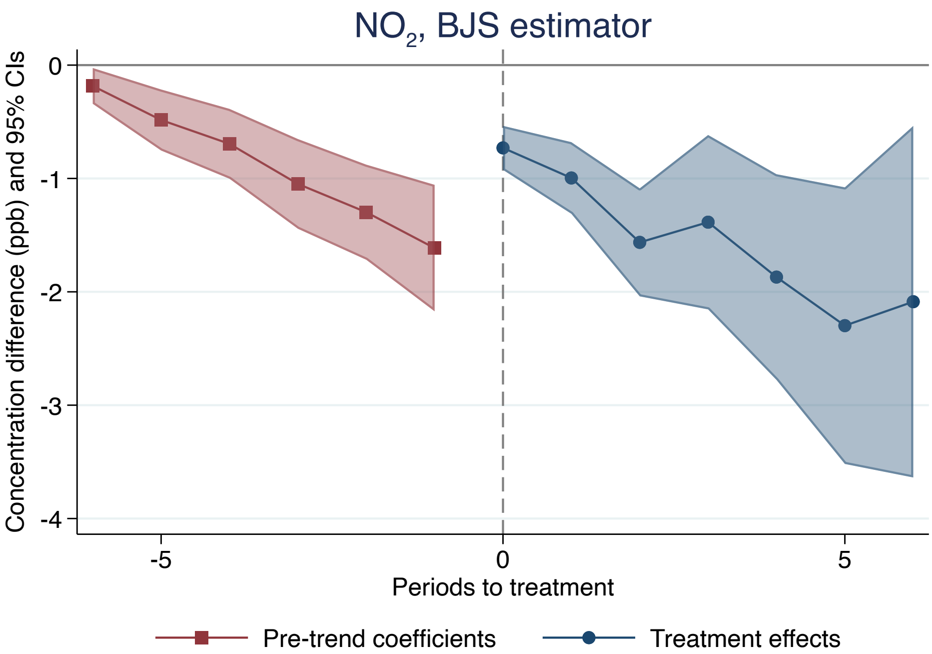


Figure 1.6: Dynamic effect of bike share on NO_2 concentrations, BSJ estimator

now taking the role of the dependent variable. The goal of this estimation is to check whether covariates are good predictors for the timing of treatment. Negative coefficients are interpreted as covariates that make treatment occur earlier. The extent of residential area is thus associated with slightly later treatment, while total built area is a predictor of earlier treatment. Cycle routes and painted cycle lanes are also associated with earlier treatment, while protected lanes do not appear to be a predictor of treatment. In preferred specifications with borough fixed effects, population and median income are not statistically significant predictors of treatment timing. Finally, college graduates are associated with earlier treatment.

Given their statistical significance in predicting both treatment status and the timing of treatment, I control for residential area, office area, total built area, at-least-painted cycle lanes, median household income and count of college graduates in my specifications.

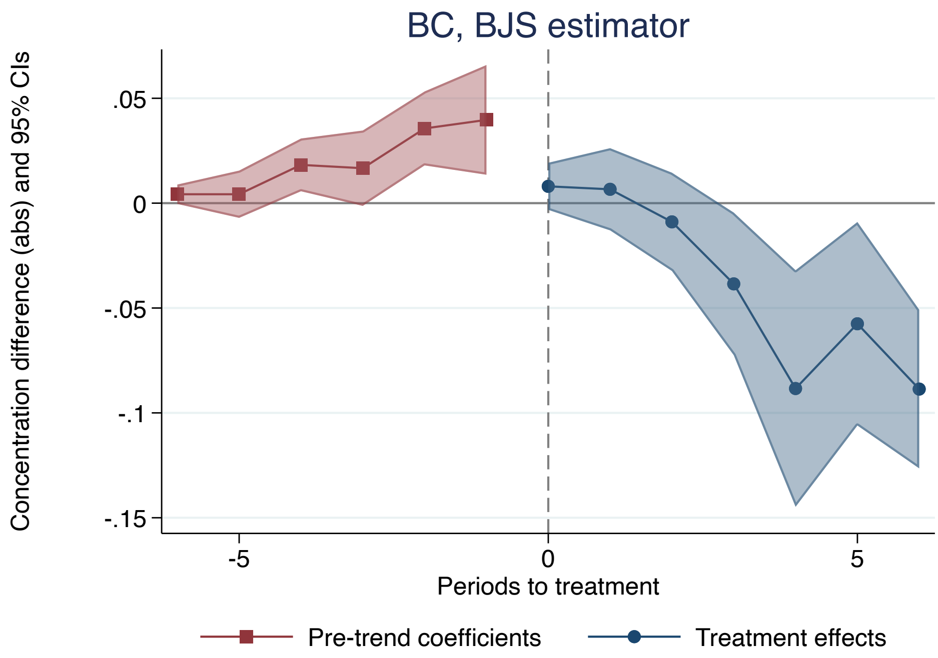


Figure 1.7: Dynamic effect of bike share on BC concentrations, BSJ estimator

1.5.4 Robustness checks

I perform the same analysis with two alternative treatment definitions: (1) the cell is within 300 metres of a bike share station, (2) the cell is within the smallest convex polygon that includes all bike share stations. These treatment definitions are less well suited to capturing the areas where fewer cars might have been driven due to bike share, and are more closely associated with the areas of bike share implementation, which might make treatment less exogenous to other factors. Conditional on these limitations, I find comparable results using the alternative treatment definitions (see Appendix 1.C).

Finally, I compute standard errors robust to spatial dependence following Conley (1999) (Appendix 1.B). For most specifications, this improves statistical significance.

1.5.5 Discussion

Results presented in this section indicate that bike share has reduced the concentration of NO by up to 13.4% and BC by up to 2.7% with respect

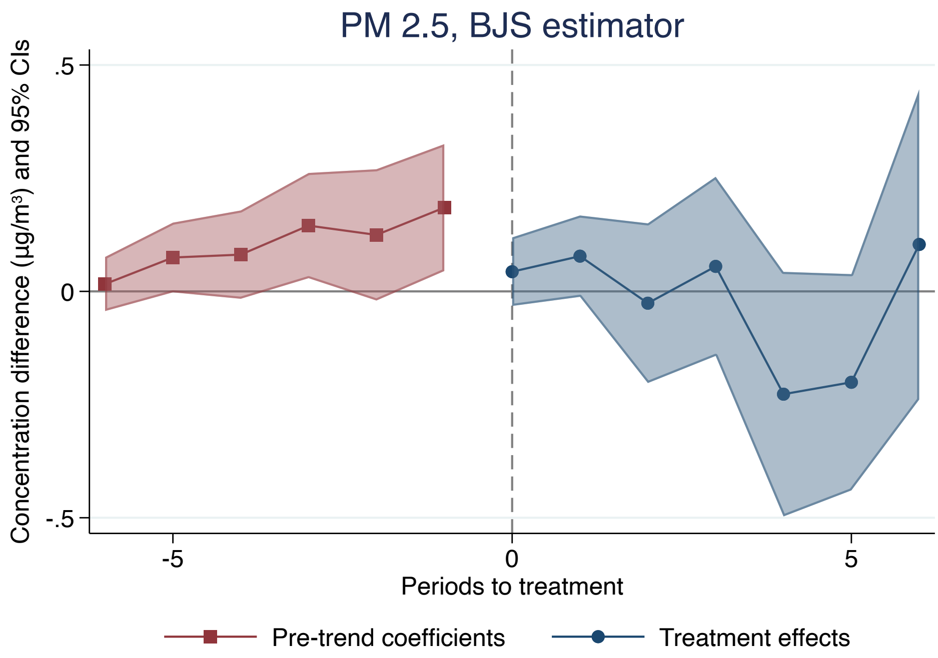


Figure 1.8: Dynamic effect of bike share on PM 2.5 concentrations, BSJ estimator

to pre-2013 mean concentrations in NYC. The dynamic estimates shown in the event study plots provide support for the parallel trends assumption for both pollutants, and lead me to conclude that the decreasing effect of bike share on NO and BC is likely causal. The pre-treatment trends displayed by NO_2 in its event study plot call for caution when interpreting the statistically significant negative coefficients as causal. When analysing NO_2 with the BJS estimator, however, the parallel trends assumption seems better supported by the graphical evidence. The impact of bike share on NO_2 is thus uncertain, although it is important to note that NO and NO_2 are often highly correlated and have similar emission sources. As we have seen, the impact on PM 2.5 of bike share is relatively noisy, and we cannot conclude that bike share had a significant impact on PM 2.5 concentrations in NYC.

Comparing these results with related literature is challenging, as most studies focus on other pollutants such as PM 2.5, PM 10 and carbon monoxide (CO). Low-emission-zones studies have found that PM 10 decreases by 5.5 to 9% (Wolff, 2014; Zhai and Wolff, 2021), while Jiang et al. (2017) finds very limited impacts on nitrogen oxides. Some of the literature on congestion charge has found significant decreases in NO_2 and PM 10 (Tonne et al., 2008), while others registered an increase in NO_2 but a decrease in NO (Green et al., 2020). Finally, subway and urban railway expansions have been associated with a reduction of aerosol particulates (similar to PM) of 4% (Gendron-Carrier et al., 2022) and CO by 5 to 15 percent (Chen and Whalley, 2012). The results I obtain, although for different pollutants, are of similar magnitude.

Shr, Yang, et al. (2022) is the only other study examining the impact of bike share on air pollution using causal inference. Their setting is a two-period DD in Taiwan's second-biggest city. They find moderate decreases in CO of around 2.0%, but no statistically significant decrease in NO or NO_2. They note that CO is the only pollutant they observe of which a significant share may be attributed to two-wheeled ICE vehicles. This suggests that some substitution might have occurred for this class of vehicles, and highlights that the impact of bike share on air quality is highly dependent on the transport modes it substitutes. As we will see in the next section, I argue that taxis play an important role in NYC's transport environment, and find suggestive evidence that bike share substitutes taxi service.

Using concentration-response values of NO on health outcomes (mortality, emergency department visits and hospitalisations) and monetary values associated with those outcomes, I perform back-of-the-envelope calculations to estimate the health and mortality costs saved by the deployment of bike share. I find that bike share reduced costs by \$327 million, and that an average bike share trip is associated with a \$3.31 decrease in health costs, while end-users pay \$1.81 per trip on average. If costs and benefits where perfectly internalised and no other costs existed, bike share riders should receive \$1.50 per ride for the net social benefit they create. I detail the monetary valuation of the health benefits associated with the reduction in NO due to bike share in Appendix 1.D.

1.6 Mechanism

In section 1.2, I discussed the main mechanism through which bike share might have impacted air pollution concentrations, which is the substitution away from internal-combustion-engine (ICE) vehicles in favour of cycling. In this section, I explore the available evidence to support this mechanism. I start by reviewing the literature and policy reports before turning to the data and analysing the evolution of taxi trips in areas served by bike share.

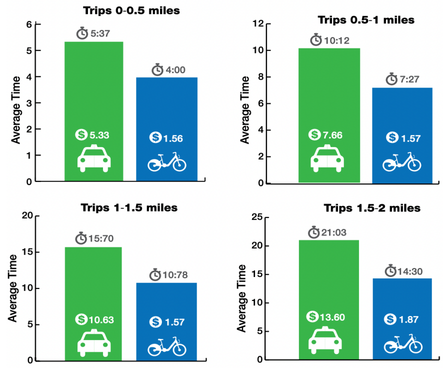
1.6.1 Introduction

The main hypothesis for bike share to reduce pollution is that the introduction of bike share reduces the relative price of (and increases the accessibility to) cycling. This change in the relative attractiveness of the cycling transport option will induce some trips to be substituted away from other transport modes. If these previous transport modes were ICE vehicles, then bike share will have reduced emissions associated with these vehicles. In the previous section, we saw that the concentrations of key pollutants generated by ICE decreased in areas where fewer vehicles are likely to have been driven due to bike share. Importantly, the previous section has shown a decrease in a harmful *by-product* of traffic, air pollution. Here, I examine there is direct evidence that traffic itself decreased.

To tackle this question, I turn to taxis. Taxis are a useful measure of traffic and ICE vehicles for several reasons. First, taxi trips have been identified by previous research to be good proxies for overall road traffic (Castro et al., 2012; Kong et al., 2016; Kan et al., 2019). Second, taxi riding is a popular mode of transport in NYC. In 2014, taxis made on average 485 thousand trips per day, transporting 236 million passengers per year (New York City Taxi and Limousine Commission, 2014).

Taxi rides in NYC also exhibit many characteristics that make them good substitution candidates for bike share. First, an average taxi ride is similar to an average bike share ride: 55% of taxi trips are less than 3 kilometres long, while around 80% of bike share trips are less than 3 kilometres. Rider demographics are also reasonably comparable, with 70% of taxi riders below 35 years old, while the median age of a bike share subscriber is 33. In addition, bike share was implemented in areas with heavy taxi use: 95% of traditional "yellow" taxi pick-ups occurred in Manhattan below 96th street, an area that bike share served by 2016. Finally, the 2019 NYC Mobility Report presents a compelling case study, comparing taxi service and bike share in Midtown (see Figure 1.9). In Midtown, where close to 50% of all counted vehicles were taxis or ride-hailing cars, a bike share ride was consistently faster than a taxi ride for trips ranging from 0.5 to 2 miles, and came at a fraction of the taxi fare (see Figure 1.9(b)), further hinting towards the substitution potential of bike share for taxi service.

(a) Location of Midtown



(b) Fares and travel time comparison

Figure 1.9: Bike share vs Taxi service in Midtown, 2019 NYC Mobility Report

Beyond similar characteristics and substitution potential, previous research has revealed a direct substitution relationship between bike share and taxi rides. Molnar and Ratsimbazafy (2017) show that, following the outage of a bike share station, taxi pickups increase in its vicinity. Moreover, they estimate the long-run substitution of taxis due to bike share of 3 to 4%, with taxi drivers operating in the bike share area seeing a relative decline in revenue.

1.6.2 Testing the substitution mechanism

Based on the anecdotal and empirical evidence presented above, I further investigate the relationship between bike share and taxi service using the universe of taxi trips from 2009 to 2019 provided by the NYC Taxi & Limousine Commission (NYC T&LC). There are on average 170 million taxi trips per year, for a total of about 1.1 billion trips during my study period. Each trip is characterised by origin and destination locations, start and end time and date, distance driven, fares, and other variables. Using these data, I identify taxi trips most likely to be substitutable by bike share. I use the taxi trip's travel distance, and define substitutable trips as those which are five kilometres or less, based on the fact that 85% of bike share trips are less than five kilometres. I aggregate short (i.e., less than five kilometres) and long taxi trips at the taxi zone of pickup.[24] I then specify a dynamic staggered DD estimation that captures the effect of bike share deployment on the level of short and long taxi trips. The estimating equation is given by:

$$Y_{itd} = \sum_{k=-9}^{-2} \beta_k \cdot Treat_{ik} + \sum_{k=0}^{6} \beta_k \cdot Treat_{ik} + \phi_t + \gamma_i + \eta_{it} + \varepsilon_{it} \tag{1.5}$$

where Y_{itd} is the number of taxi trips of length d (i.e., short or long) in taxi zone i in month t. Unlike the main analysis, the treatment variable $Treat_i$ is defined, for each taxi zone, as being within 300 metres of a bike share station. In the present context, it is most likely that the impact of bike share on taxi pickups will be most prominent close to bike share stations. Finally, ϕ_t, γ_i and η_{it} denote time, taxi-zone, and borough-time fixed effects, respectively, while ε_{it} serves as the error term.

The β_k coefficients are plotted in Figure 1.10, separately for short trips (green, circles) and long trips (orange, triangles). In the periods leading to treatment, there are no significant differences in taxi pickups for both short and long taxi trips between taxi zones close to bike share stations and the others. Following treatment, areas served by bike share see a decrease in the number of pickups. Notably, the decrease is larger for short trips compared to long trips. I interpret this as suggestive evidence that bike share reduces taxi trips taken, and more so for taxi trips that are most similar to bike share trips.

1.6.3 Other mechanisms

While I expect substitution to be the main channel through which bike share impacts pollution, other channels may come into play. I identify three additional channels through which bike share may affect air pollution: the crowding effect, the efficiency externality and the strength-in-numbers virtuous cycle.

The crowding effect describes the effect of increasing the number of bikes on the streets. Although they take much less space per traveler, bikes may still create congestion on streets, especially due to lower travel speeds which

[24]There are 263 taxi zones defined by the T&LC, see Figure 1.13. Their size varies, with smaller zones in southern Manhattan and larger ones in Staten Island, likely related to the level of taxi traffic in each zone.

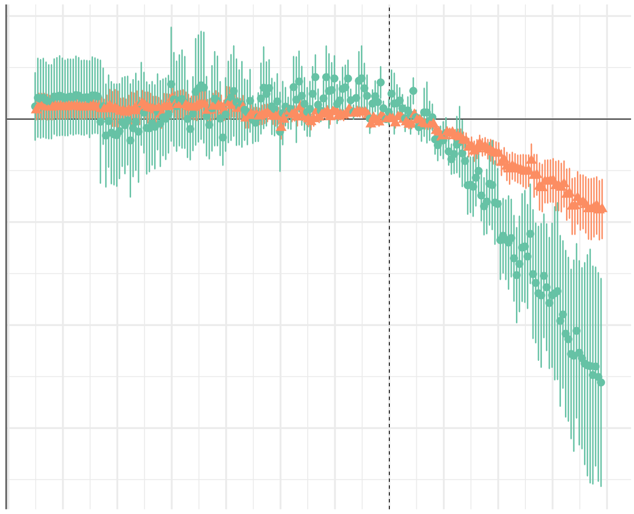


Figure 1.10: Dynamic effect of bike share on yellow taxi pickups

may force other vehicles to slow down, creating congestion and increasing pollution emissions. Bike lanes, by removing lanes for other road vehicles, may have a similar effect. On the other hand, the crowding effect may lead ICE vehicles to drive alternative routes to avoid newly congested areas, displacing air pollution rather than reducing it. Evidence from Hamilton and Wichman (2018), however, indicates that congestion tends to decrease in areas served by bike share, and suggests limited road traffic displacement to adjacent areas. Importantly, the crowding out effect would go against me finding an impact of bike share on air pollution.

Usage of bike share varies across time and space. Popular origin stations experience a high demand for bikes, while docks at popular destination stations are sometimes full. To ensure a sufficient level of service, bikes need to be rebalanced between stations. This need for efficiency, however, may create externalities. Rebalancing in NYC is done with "bike trains" (an e-bike that can carry 12 to 16 bikes) and ICE vehicles. The trips made by the latter, especially during highly congested periods, increase emissions and congestion, creating an efficiency externality. While little data is available on the scope and frequency of rebalancing, I expect the induced increase in driving and congestion to have a relatively small effect on air pollution due to its relatively small footprint compared to total traffic. Increased air pollution due to station rebalancing would bias the estimates of bike share downwards.

Finally, increasing the number of bike trips made in the city through bike share may create a virtuous cycle: by making bikes more ubiquitous and accessible, the attitude of ICE vehicles towards bikes may change over time. More cyclists on the streets create a "safety-in-numbers" effect and lessen the perceived danger of cycling for potential cyclists, a crucial factor for cycling take-up identified by the literature (Pucher and Buehler, 2012). More bike trips also mean a shift in the perception of cycling, contributing to making cycling a more acceptable transport option. This all leads, at the margin, to more people switching to cycling, further decreasing air pollution if their previous modes of transport were ICE vehicles. The virtuous cycle initiated by bike share would overestimate the effect of bike share if the increase in non–bike-share cycling was spatially correlated with the bike share area.

1.7 Conclusion

In this paper, I investigate the effect of the implementation and gradual rollout of the NYC bike share program on air pollution concentrations. Leveraging the gradual expansion of the system and a treatment variable capturing areas most likely to be affected by bike share, I find that bike share reduced concentrations of NO_2 by up to 13.4% and BC by up to 2.7% compared to pre-implementation mean concentrations. The decrease in NO concentrations is associated with up to $327 million reductions in health and mortality costs, and make riding a bike share a net social benefit.

These effects differ from estimates made in the previous literature. In particular, Shr, Yang, et al. (2022) find no impact of bike share on nitrogen oxides and only limited decrease in carbon monoxide in Taipei's second-largest city. The disparity between the two results might stem from different likely sources of substitution. In NYC, the analysis of taxi trips suggests that the substitution away from taxi service might explain the decrease in pollution levels. In Taipei, as Shr, Yang, et al. (2022) have noted, most of the substitution may come from public transport riders, pedestrians and some two-wheeled-vehicle riders (hence the decrease in carbon monoxide). This highlights the importance of identifying the source of substitution, as it will determine the environmental impact of sustainable transport policies and micromobility services.

Appendix 1.A Additional maps

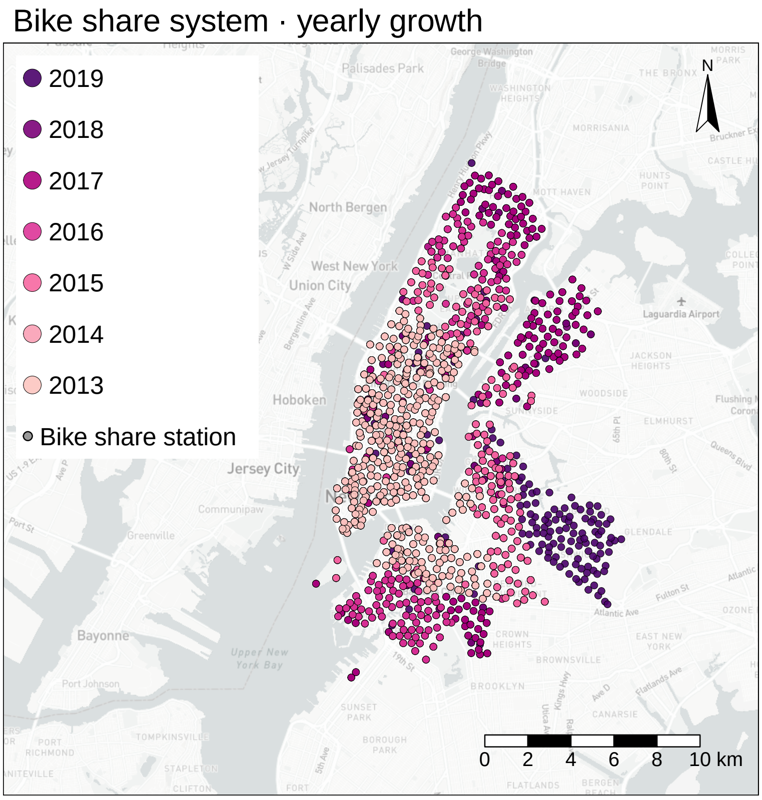
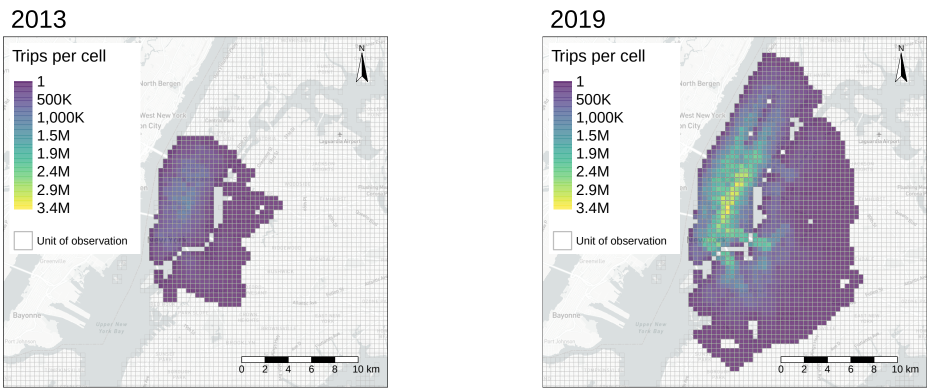


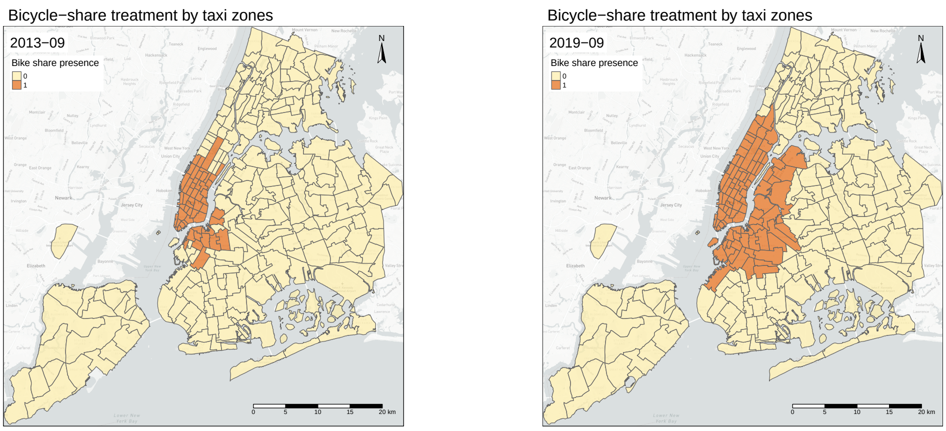
Figure 1.11: Gradual rollout of bike share stations in NYC



(a) 2013

(b) 2019

Figure 1.12: Spatial extent of treatment and imputed trips per cell at bike share implementation and last study period



(a) 2013

(b) 2019

Figure 1.13: Map of taxi zones in proximity of bike share stations

Appendix 1.B Conley standard errors

I compute standard errors robust to spatial dependence following Conley (1999) for all specifications.

Table 1.13: Effect of bike share on NO concentrations

	NO			
	(1)	(2)	(3)	(4)
On-car-route	-2.5360***	-2.7281***	-1.0262***	-0.6398***
	(0.3540)	(0.3507)	(0.1957)	(0.1996)
Baseline controls		✓	✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Year-Community district FE			✓	
Year-Borough FE				✓
Mean concentration pre-treat.	20.322	20.353	20.353	20.353
% mean concentration pre-treat.	-12.479	-13.404	-5.042	-3.144
Observations	91,710	90,898	90,898	90,898
R^2	0.906	0.908	0.960	0.937
Within R^2	0.049	0.066	0.013	0.010

Conley (0.59km) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.14: Effect of bike share on NO2 concentrations

	NO2			
	(1)	(2)	(3)	(4)
On-car-route	-1.1489***	-1.2554***	-0.2010***	-0.4141***
	(0.0967)	(0.0955)	(0.0647)	(0.0629)
Baseline controls		✓	✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Year-Community district FE			✓	
Year-Borough FE				✓
Mean concentration pre-treat.	19.950	20.007	20.007	20.007
% mean concentration pre-treat.	-5.759	-6.275	-1.005	-2.070
Observations	91,710	90,898	90,898	90,898
R^2	0.978	0.979	0.994	0.985
Within R^2	0.081	0.123	0.012	0.026

Conley (0.59km) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.15: Effect of bike share on BC concentrations

	BC			
	(1)	(2)	(3)	(4)
On-car-route	-0.0253***	-0.0280***	-0.0078	-0.0097**
	(0.0057)	(0.0057)	(0.0052)	(0.0047)
Baseline controls		✓	✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Year-Community district FE			✓	
Year-Borough FE				✓
Mean concentration pre-treat.	1.015	1.017	1.017	1.017
% mean concentration pre-treat.	-2.494	-2.757	-0.771	-0.952
Observations	91,710	90,898	90,898	90,898
R^2	0.956	0.956	0.979	0.970
Within R^2	0.006	0.011	0.001	0.002

Conley (0.59km) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.16: Effect of bike share on PM concentrations

	PM			
	(1)	(2)	(3)	(4)
On-car-route	-0.0097	-0.0320	-0.0091	0.0538***
	(0.0262)	(0.0260)	(0.0174)	(0.0184)
Baseline controls		✓	✓	✓
Cell FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Year-Community district FE			✓	
Year-Borough FE				✓
Mean concentration pre-treat.	9.433	9.441	9.441	9.441
% mean concentration pre-treat.	-0.103	-0.339	-0.096	0.569
Observations	91,710	90,898	90,898	90,898
R^2	0.978	0.979	0.992	0.984
Within R^2	0.000	0.018	0.003	0.016

Conley (0.59km) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix 1.C Alternative treatment definitions

In this appendix, perform the TWFE estimations using alternative treatment definitions.

1.C.1 Stations within 300m of cell

Table 1.17: Effect of bike share on NO concentrations

	NO		
	(1)	(2)	(3)
Station	-3.8915***	-2.1059***	-1.5368***
	(1.1872)	(0.5868)	(0.5721)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	20.353	20.353	20.353
% mean concentration pre-treat.	-19.120	-10.347	-7.551
Observations	90,898	90,898	90,898
R^2	0.910	0.960	0.937
Within R^2	0.089	0.028	0.021

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.18: Effect of bike share on NO2 concentrations

	NO2		
	(1)	(2)	(3)
Station	-1.4994***	-0.4007***	-0.5309***
	(0.3277)	(0.1384)	(0.1807)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	20.007	20.007	20.007
% mean concentration pre-treat.	-7.494	-2.003	-2.654
Observations	90,898	90,898	90,898
R^2	0.979	0.994	0.985
Within R^2	0.122	0.018	0.028

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.19: Effect of bike share on BC concentrations

	BC		
	(1)	(2)	(3)
Station	-0.0404**	-0.0170*	-0.0153
	(0.0162)	(0.0098)	(0.0093)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	1.017	1.017	1.017
% mean concentration pre-treat.	-3.978	-1.668	-1.506
Observations	90,898	90,898	90,898
R^2	0.957	0.979	0.970
Within R^2	0.015	0.002	0.003

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.20: Effect of bike share on PM concentrations

	PM		
	(1)	(2)	(3)
Station	-0.1002	-0.0942**	0.0090
	(0.0788)	(0.0419)	(0.0514)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	9.441	9.441	9.441
% mean concentration pre-treat.	-1.061	-0.997	0.095
Observations	90,898	90,898	90,898
R^2	0.979	0.992	0.983
Within R^2	0.022	0.007	0.014

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

1.C.2 Cells within the convex hull

Table 1.21: Effect of bike share on NO concentrations

	NO (1)	NO (2)	NO (3)
Convex polygon	-2.7534**	-0.1855	-0.3325
	(1.0736)	(0.6438)	(0.5012)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	20.353	20.353	20.353
% mean concentration pre-treat.	-13.528	-0.911	-1.633
Observations	90,898	90,898	90,898
R^2	0.907	0.959	0.936
Within R^2	0.058	0.008	0.008

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.22: Effect of bike share on NO2 concentrations

	NO2 (1)	(2)	(3)
Convex polygon	-1.1882***	0.0989	-0.2136
	(0.3380)	(0.1864)	(0.2059)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	20.007	20.007	20.007
% mean concentration pre-treat.	-5.939	0.494	-1.067
Observations	90,898	90,898	90,898
R^2	0.979	0.994	0.985
Within R^2	0.100	0.010	0.016

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.23: Effect of bike share on BC concentrations

	BC (1)	(2)	(3)
Convex polygon	-0.0379**	-0.0170***	-0.0167*
	(0.0143)	(0.0051)	(0.0093)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	1.017	1.017	1.017
% mean concentration pre-treat.	-3.729	-1.669	-1.638
Observations	90,898	90,898	90,898
R^2	0.957	0.979	0.970
Within R^2	0.015	0.002	0.004

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 1.24: Effect of bike share on PM concentrations

	PM		
	(1)	(2)	(3)
Convex polygon	-0.0353	0.0399	0.0748
	(0.0802)	(0.0378)	(0.0535)
Baseline controls	✓	✓	✓
Cell FE	✓	✓	✓
Year FE	✓	✓	✓
Year-Community district FE		✓	
Year-Borough FE			✓
Mean concentration pre-treat.	9.441	9.441	9.441
% mean concentration pre-treat.	-0.374	0.423	0.792
Observations	90,898	90,898	90,898
R^2	0.979	0.992	0.984
Within R^2	0.018	0.004	0.018

Clustered (Community district) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Appendix 1.D Monetary valuation of benefits

In this section, I perform a back-of-the-envelope calculation of the health monetary value associated with the observed decrease in NO concentrations. The impact of NO on health endpoints (e.g., emergency department (ED) visits, mortality) is given by concentration-response (CR) values found in the epidemiological literature, which are then converted to monetary values using survey and regulatory data for each endpoint. I conclude by benchmarking these benefits with the cost paid by end-users.

1.D.1 Valuing health benefits

I start with the average treatment effect of bike share on NO in my preferred specification, found in Table 1.3 column 2. I then collect CR values of NO on (1) mortality and (2) asthma ED visits and hospitalisations. The CR value on mortality is obtained from Meng et al. (2021), a meta-analysis using studies in 398 cities around the world. I use the US-specific estimates for total mortality for a 10μg increase in NO. From Zheng, Ding, et al. (2015) I obtain the CR value of NO for visits to the ED and hospitalisation of people affected by asthma.

In order to compute the changes in health outcomes attributable to the treatment, we need to apply the CR values to health outcomes before treatment. I obtain mortality at the community-district level from the NYCDOH Vital Statistics data sets,[25] and asthma ED visits and hospitalisations at the community-district level from the NYCDOH Environment & Health Data Portal.[26] I take the yearly average of the three outcomes over the five years prior to the first bike share implementation in 2013.

The TWFE estimator provides the average treatment effect on the treated group, which is the total effect over the whole period for all units treated at some point. In order to compute health benefits, I select community districts that fall in the area of the treated group. Community districts are fairly large geographical units and some of them are barely covered by the treatment area. To avoid overestimating health benefits, I select only those that have more than 30% of their area treated by the bike share treatment.

[25] https://www.nyc.gov/site/doh/data/data-sets/death-micro-sas-datasets.page, accessed 2022-11-21.

[26] https://a816-dohbesp.nyc.gov/IndicatorPublic/, accessed 2022-11-21.

Summing up baseline outcomes for all eventually treated community districts, I can now compute how much bike share changed those outcomes in the post-treatment period. I use the formula described in 1.6 to compute the change in outcomes attributable to treatment:

$$\Delta Out = \frac{CR}{10} ATT \cdot Out_{\bar{t}} \tag{1.6}$$

where Out is the outcome of interest, CR the concentration-response value, ATT the average treatment effect on the treated, and $Out_{\bar{t}}$ the average outcome in period before treatment $\bar{t}$. Computing the changes for mortality, ED visits and hospitalisations, I find that bike share avoided 33 deaths, 1,122 ED visits and 412 hospitalisations through the reduction in NO concentrations.

To transform these health outcomes into monetary values, I obtain the value of statistical life from EPA (2010), and the average cost of ED visits and hospitalisation from the Blewett et al. (2021). I convert all dollar amounts to 2019 equivalents using the Consumer Price Index retroactive series from the US Bureau of Labor Statistics.[27]

Finally, I multiply the changes in health outcomes by their monetary value. The deaths avoided by bike share are valued at $320 million, and the prevented ED visits and hospitalisation of asthma-affected people amount to $1.2 and $6.2 million, respectively. In total, the back-of-the-envelope calculation shows that the deployment of bike share in NYC since 2013 saved a total of $327 million due to the reduction in NO concentrations.

1.D.2 Benchmarking monetary benefits

How large are these monetary benefits? To shed light on this question, I compare the monetary benefits associated with NO decrease with the price paid by end-users. Combining trip record data and monthly reports, I find that the mean price per ride weighted by the number of trips is around $1.81.[28]

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[27] Stewart (1999), https://www.bls.gov/cpi/research-series/r-cpi-u-rs-home.htm, accessed 2022-11-21.

[28] A majority of trips are made by annual subscribers, but there exists no published figure on the number of trips per annual member. Using a selection of busy and slow bike share months, I collect the number of trips made by members, which I divide by the number of active members that month to get an average number of trips per member for that month. Dividing the price of an annual subscription by 12 (i.e., month-equivalent price) and by the average number of trips by members yields the average price per trip for members that month. For casual trips (no membership), I use the price of a single 45-minute trip. This approach likely underestimates the average price per trip for casual rides as it does not take into account

The total number of trips taken on the bike share system during the sample period amounts to 98,952,256 trips. Dividing the monetary benefits obtained in the previous section by the total number of trips yields the average monetary benefit per trip of \$3.31.

For an average bike share trip, end-users pay \$1.81 but provide \$3.31 in health benefits. If costs and benefits were perfectly internalised and no other costs or benefits existed, end-users should be paid the benefits they produce minus the cost to run the service, which we can assume is represented by the average cost per ride. In the present case, end-users should be paid $3.31 - 1.81 = \$1.50$ per ride for the health benefits they provide.

overtime charges. However, the share of casual trips being low (15% at most), this should not affect the estimates substantially.

Chapter 2

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

2.1 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 a 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 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 and real-estate market in the medium-run. Previous studies have shown the existence of a rent premia around bike-share stations six months after implementation, but it remains unclear if these effects translate into higher transaction prices in the medium to long run.

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. I perform several additional analyses exploring the validity of that estimate.

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 transport 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, with estimates for price premia ranging from 2% to 8% 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. This analysis is important for a several 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, if for example buyers and renters have different utility functions, or if the increase in in sale prices takes time to materialise. In order to make the comparison with previous studies easy, I build upon the treatment definitions and estimation strategy set forth in Shr, Yang, et al. (2022).

The results of this paper are important in several regards. First, they confirm that cycling policies behave similarly to other transportation policies: they have an impact on real estate sale prices, even in a city where only about 1% of workers commute to work by bicycle.[1] 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

[1] U.S. Census Bureau (2021). Sex of Workers by Means of Transportation to Work, American Community Survey 1-year estimates (Table B08006). Retrieved from `https://censusreporter.org` (accessed 2023-08-01).

value due to bike-share documented here may be partially captured by city government through property taxes, which could go towards financing these investments, and improve policymakers' and voters' support for them (see analysis and thorough discussion of the value creation brought by the opening of Second Avenue metro extension in New York City in Gupta et al. (2022)).

The remainder of the paper is organised as follows: section 2.2 discusses data sources, the cleaning of the data sets and the generation of the treatment variable; section 2.3 defines the empirical strategy used to evaluate the impact of bike-share on property prices; section 2.4 present the results, including additional analysis and robustness checks; section 2.5 concludes.

2.2 Data

This section describes the data used in the paper. I begin by describing the outcome data, which assembled from New York City's official transaction records, the cleaning operations performed on the data, and the sample creation process. I then move on to describing the bike-share data, and the procedure to define control and treatment property units.

2.2.1 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 gather transaction records spanning from January 2011 to April 2015, and execute 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 stored in a geographical information system database, I determine the location of a transaction as the centroid of the polygon.[2]

[2] 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.

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), and therefore keep only transactions that took place between January 1, 2011 (two and half years before the first opening of stations) and June 30, 2015 (right before the next spatial expansion of the system).

Next, I retain only sales with non-zero prices and non-missing surface areas, deflate the sale price to June 2015 levels, and compute the surface area per unit.[3] I identify price outliers, first using the definition in Gupta et al. (2022) (sale prices greater than $400 thousand and less than $10 million), but also price-per-square-foot outliers (greater than $50 and less than $20 thousand).[4] Finally, I extract building attributes (residential/commercial, elevator, condo, etc: see subsection 2.2.4 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,[5] and distance to the nearest park.[6]

The analyses of this paper all exclude price and price-per-square-foot outliers. I will mainly concentrate the impact of bike-share on residential units, but will also present results for commercial units in the main text and the appendix.

2.2.2 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[7] to identify the opening (and occasionally closing) date of each station. The first wave is defined by

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[3] 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.

[4] 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.

[5] 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).

[6] 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.

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

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 364 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 2.3), 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. I end up with 360 stations, of which 331 opened in May 2013, 27 in June 2013 and 2 in March 2014.

2.2.3 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: 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 had 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): therefore, the station matched 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.[8]

By allowing for multiple matches as described above (and after cleaning transactions as outlined earlier in subsection 2.2.1), I end up with about eleven thousand transaction-station pairs[9] out of 3830 unique transactions, with a mean of 2.9 matches per transactions and a maximum of 15.[10] 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 2.1 illustrates how treatment and control status is attributed, and how transactions may act as controls for multiple bike-share stations. Figure 2.2 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 indicator variables.

Some specifications also use the entire universe of transactions that took place in the sample period. I construct one last variables to accommodate

[8]Note that cases 2.2 and 2.3 imply that I do not exploit the potential cumulative effect of multiple treatment instances by several bike-share stations in the vicinity. While potentially important, accounting for repeated or cumulative treatment is not straightforward in practice: I reserve this analysis for future work.

[9]This figure includes both residential and commercial properties, but excludes price and price-per-square-foot outliers. Among those, about 8,800 are transaction-station pairs of residential units.

[10]That means a transaction may serve as control for up to 15 bike-share stations. This is not the norm, however: more than 57% of transactions are matched to a single station, and 77% to less than five stations.

Figure 2.1: 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: they are represented by a diamond shape.

these additional observations: an indicator variable that captures whether a transaction is within the sample area of any bike-share station (i.e., within 500 metres). I do not compute the distance to matched bike-share stations for transactions beyond 500 metres, but do compute distances to amenities and extract the same unit attributes. I apply the same filtering criteria (ranges of sale prices, non-missing surface data, etc) described in section 2.2.1 to the transactions beyond 500 metres. For these transactions, the post-period is defined as after the first opening of bike-share station, i.e. after May 2015.

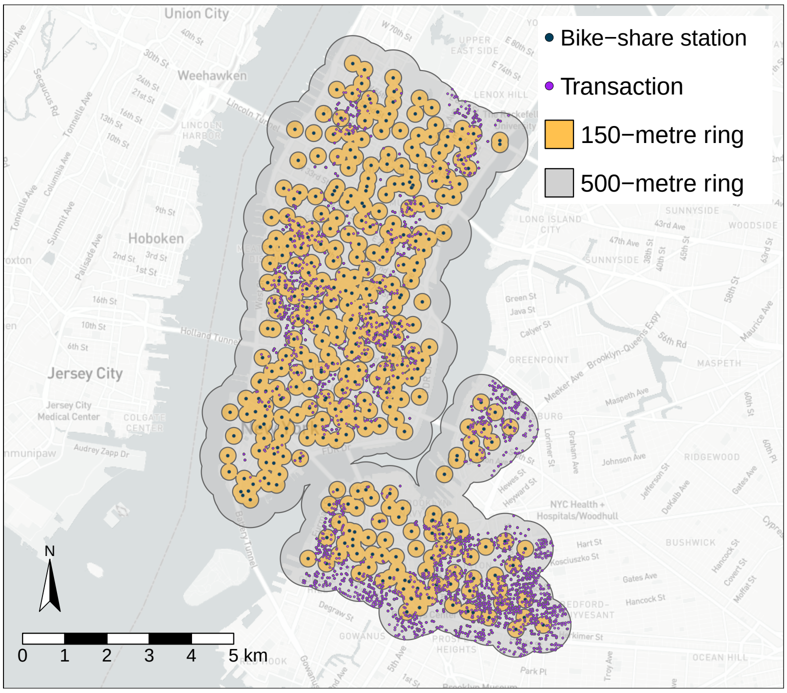


Figure 2.2: Overview of the study area, treatment and control areas, and location of transactions. *Notes*: Transactions are represented by purple points, and are restricted to residential and non-outlier units. The yellow area represents the treatment rings (i.e., within 150 metres of a bike-share station), 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 2.6 for a detailed map).

2.2.4 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 2.1: Summary statistics, residential properties, whole sample period

	Mean	SD	Min	Median	Max	Miss
Panel A: Within 500 metres of a bike-share station (N = 8,889)						
Sale price (2015 $)	3,159,445	2,414,492	400,021.4	2,466,912	9,935,151	0
Log sale price (2015 $)	14.64	0.86	12.9	14.72	16.11	0
Sale price per sqft (2015 $)	3,518.88	3,693.91	113.53	1,971.47	19,858.97	0
Residential units (count)	8.37	44.61	1	3	1,681	0
Commercial units (count)	0.42	0.85	0	0	15	0
Total units (count)	8.79	44.81	1	3	1,684	0
Built surface (sqft)	7,385.82	34,799.22	0	3,694	1,231,250	0
Land surface (sqft)	2,451.29	7,553.66	0	2,000	298,550	0
Final surface (sqft)	7,422.86	34,800.77	680	3,740	1,231,250	0
Surface per unit (sqft)	1,262.45	907.93	191.62	1,003.88	9,155	0
Building age	99.47	27.5	0	110	217	0
Year built	1,913.35	27.48	1,798	1,901	2,015	0
Dist. to bus stop (m)	100.81	62.03	7.57	91.11	479.44	0
Dist. to subway entrance (m)	292.54	166.23	12.08	262.6	1,073.82	0
Dist. to bike-share station (m)	308.82	129.51	2.09	327.57	499.93	0
Dist. to park (m)	345.05	220.83	2.51	303.73	1,137.15	0
Sale quarter	9.77	4.96	1	10	18	0
Panel B: Beyond 500 metres of a bike-share station (N = 65,795)						
Sale price (2015 $)	916,098.2	1,008,826	400,000	635,885.4	9,983,196	0
Log sale price (2015 $)	13.5	0.56	12.9	13.36	16.12	0
Sale price per sqft (2015 $)	854.07	1,273.69	51.11	522.26	19,905.42	0
Residential units (count)	3.35	7.77	0	2	594	0
Commercial units (count)	0.1	0.46	0	0	12	0
Total units (count)	3.45	7.9	0	2	594	0
Built surface (sqft)	3,626.09	7,987.84	0	2,250	890,134	0
Land surface (sqft)	3,367.46	3,390.27	0	2,500	382,704	0
Final surface (sqft)	3,644.9	7,989.31	300	2,260	890,134	0
Surface per unit (sqft)	1,360.44	737.46	132	1,200	31,494	0
Building age	73.19	30.4	0	82	213	8
Year built	1,939.8	30.36	1,800	1,930	2,015	0
Dist. to bus stop (m)	2,652.64	3,969.35	6.72	597.07	21,983.22	0
Dist. to subway entrance (m)	2,472.79	3,910.65	5.47	770.84	22,533.2	0
Dist. to park (m)	474.54	316.77	0	417.59	2,308.86	0
Sale quarter	10.38	5.09	1	11	18	0

Notes: Panel A reports summary statistics for residential property transactions within 500 metres of a bike-share station (preferred sample), while panel B summarises transactions beyond 500 metres of bike-share stations. Price and price-per-square-foot outliers have been removed for both samples.

Table 2.2: Balance table treated vs control ring, residential properties, 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
Dist. to bus stop (m)	103.89	61.26	93.18	49.87	-10.70***	0.00
Dist. to subway entrance (m)	291.07	159.62	295.86	201.74	4.79	0.56
Dist. to bike-share station (m)	346.00	100.78	98.95	34.37	-247.04***	0.00
Dist. 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. *Dist. to bike-share station* is reported as the hypothetical distance to the matched bike-share station were the system already implemented: in reality, no pre-period transaction is near a bike-share station, since they were not yet built.

Table 2.3: Balance table, residential properties, categorical variables, whole sample period

		Treated ring 0 (N= 1,386)		Control ring 1 (N= 7,503)		Beyond sample 2 (N=65,795)		Total	
		N	Pct.	N	Pct.	N	Pct.	N	Pct.
Post-period	0	679	49.0	4014	53.5	31137	47.3	35830	48.0
	1	707	51.0	3489	46.5	34658	52.7	38854	52.0
Treated (treatment ring × post)	0	679	49.0	7503	100.0	0	0.0	8182	11.0
	1	707	51.0	0	0.0	0	0.0	707	0.9
Sample ring	0	0	0.0	0	0.0	65795	100.0	8889	11.9
	1	1386	100.0	7503	100.0	0	0.0	65795	88.1
Elevator	0	1329	95.9	7312	97.5	65055	98.9	73696	98.7
	1	57	4.1	191	2.5	740	1.1	988	1.3
Walkup	0	886	63.9	5000	66.6	58702	89.2	64588	86.5
	1	500	36.1	2503	33.4	7093	10.8	10096	13.5
Condo	0	1380	99.6	7453	99.3	65782	100.0	74615	99.9
	1	6	0.4	50	0.7	13	0.0	69	0.1
Coop	0	1370	98.8	7383	98.4	65750	99.9	74503	99.8
	1	16	1.2	120	1.6	45	0.1	181	0.2
Rental	0	682	49.2	4183	55.8	56715	86.2	61580	82.5
	1	704	50.8	3320	44.2	9080	13.8	13104	17.6

Notes: Columns *Treated ring* summarise the data for transactions within 150 metres of bike-share stations, *Control ring* for transactions between 150 and 500 metres from bike-share stations, and *Beyond sample* for transactions beyond the sample area of bike-share, i.e. more than 500 metres away from any station. The *Post-period* for the latter transactions is defined as the sale taking place after the initial deployment of bike-share in May 2013. The *Total* columns reports counts and share across the whole sample (treated and control rings, and beyond sample).

Table 2.4: Balance table, residential and commercial building class categories, whole sample period

	Treated ring 0 (N= 1,898)		Control ring 1 (N= 9,723)		Beyond sample 2 (N=71,088)		Total	
Building class category	N	Pct.	N	Pct.	N	Pct.	N	Pct.
01 One Family Dwellings	164	8.6	833	8.6	24533	34.5	25530	30.9
02 Two Family Dwellings	320	16.9	2018	20.8	25261	35.5	27599	33.4
03 Three Family Dwellings	180	9.5	1203	12.4	6876	9.7	8259	10.0
07 Rentals - Walkup Apartments	495	26.1	2480	25.5	7067	9.9	10042	12.1
08 Rentals - Elevator Apartments	47	2.5	125	1.3	724	1.0	896	1.1
09 Coops - Walkup Apartments	5	0.3	23	0.2	26	0.0	54	0.1
10 Coops - Elevator Apartments	8	0.4	57	0.6	16	0.0	81	0.1
11a Condo-Rentals	1	0.1	1	0.0	10	0.0	12	0.0
13 Condos - Elevator Apartments	2	0.1	9	0.1	0	0.0	11	0.0
14 Rentals - 4-10 Unit	161	8.5	714	7.3	1279	1.8	2154	2.6
17 Condo Coops	3	0.2	40	0.4	3	0.0	46	0.1
21 Office Buildings	116	6.1	386	4.0	685	1.0	1187	1.4
22 Store Buildings	245	12.9	966	9.9	2010	2.8	3221	3.9
27 Factories	19	1.0	155	1.6	637	0.9	811	1.0
29 Commercial Garages	105	5.5	495	5.1	1212	1.7	1812	2.2
30 Warehouses	27	1.4	218	2.2	749	1.1	994	1.2

Notes: Columns *Treated ring* summarise the data for transactions within 150 metres of bike-share stations, *Control ring* for transactions between 150 and 500 metres from bike-share stations, and *Beyond sample* for transactions beyond the sample area of bike-share, i.e. more than 500 metres away from any station. The *Total* columns reports counts and share across the whole sample (treated and control rings, and beyond sample).

2.2.5 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 2.3, 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 2.3 motivates the analysis to follow.

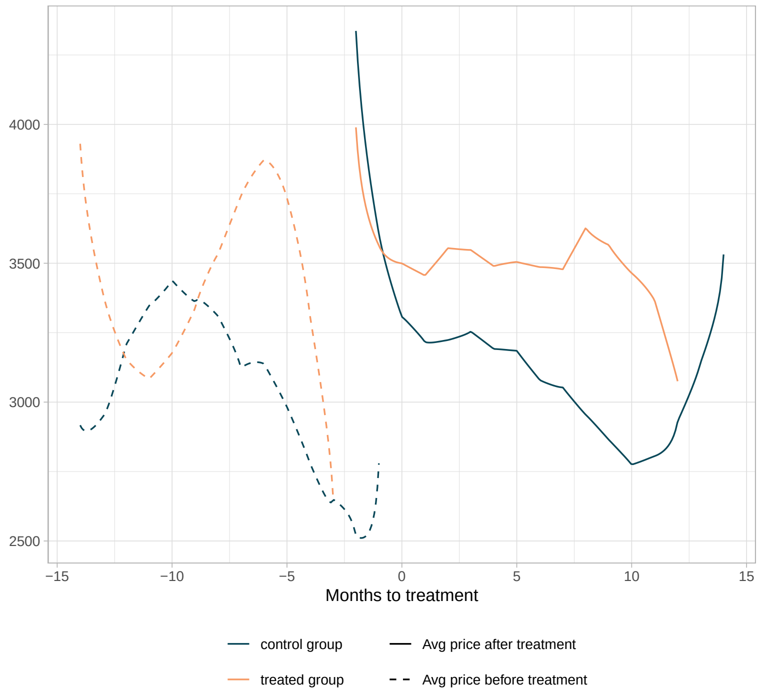


Figure 2.3: Local polynomial regression of sales prices on periods to treatment. *Notes*: Periods to treatment are given in months. The dashed lines are pre-treatment, and solid lines post-treatment results of a local polynomial regression of sales prices on periods to treatment. Yellow lines identify 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 a three-month bandwidth.

2.3 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, Yang, et al. (2022).

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. I also use the entire universe of transactions in the sample period in alternative estimations: the control group in that case is arguably not as well defined, but the estimation does inform on us the general price dynamics with respect to distance to bike-share stations.

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 transaction prices. Moreover, to be a significant threat to identification, other policies would have to exactly follow the spatial and temporal pattern of bike-share stations' implementation, which I control for to the best of my abilities using distances to amenities and to transport infrastructure.

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:

$$\begin{aligned} ln(P_{ijct}) = {} & \delta_{<150} D_{ij}^{<150} \times Post_{ijt} + \lambda_{<150} D_{ij}^{<150} \\ & + \delta_{<500} D_{ij}^{<500} \times Post_{ijt} + \lambda_{<500} D_{ij}^{<500} + \beta' X_{it} + \gamma' Z_{it} \\ & + \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct}, \end{aligned} \tag{2.1}$$

where P_{ijct} is the real sale price (base June 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. As mentioned, this model estimates the impact of bike-share while using the full universe of transactions as controls, including transactions that may be on the other side of the city. While arguably not the best selection of a control group, this estimation lets us discern the general price patterns with respect to distance to bike-share stations.

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:

$$\begin{aligned} ln(P_{ijct}) = \delta_{<150} D_{ij}^{<150} \times Post_{ijt} + \lambda_{<150} D_{ij}^{<150} + \beta' X_{it} + \gamma' Z_{it} \\ + \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct}, \end{aligned} \tag{2.2}$$

The coefficient of interest in both models 2.1 and 2.2 is $\delta_{<150}$, which represents the average treatment effect of bike-share on the treated transactions. Concretely, it is the percent change in sale prices for a transaction within the treatment ring (i.e., within 150 metres) of a bike-share station after the opening of the station. In an alternative specification, 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 on transaction prices. I also perform robustness checks that vary the distance of treated and sample rings, and add 50-metre buffers between treated and control rings in which transactions are dropped.

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 2.3:

$$\begin{aligned} ln(P_{ijct}) = \sum_{k=-6}^{-2} \beta_k \cdot D_{ij}^{<150} + \sum_{k=0}^{6} \beta_k \cdot D_{ij}^{<150} + \beta' X_{it} + \gamma' Z_{it} \\ + \kappa_j + \varphi_c + \tau_t + \kappa_j \times t + \varepsilon_{ijct}, \end{aligned} \tag{2.3}$$

where k denotes the relative time to the first year of treatment, the other terms being the same as in the previous specifications. 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.

2.4 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, perform robustness checks, and test whether bike-share stations had an impact on the sale prices of commercial properties. Finally, I discuss the results in light of previous research.

2.4.1 Residential properties

2.4.1.1 Hedonic model

I briefly present the results of a hedonic model on residential properties 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 2.5. Columns 1 to 3 use the universe of transactions across NYC during the sample period (2011-01-01 to 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 dependent variable for all specifications is the log sale price,[11] 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 characteristics. Standard errors are clusters at the Neighbourhood Tabulation

[11] 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).

Areas (NTA) level for columns 1 to 3, and at the bike-share station level for columns 4 to 6.

Table 2.5: 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, 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, and might be clustered together 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 to one 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.

2.4.1.2 Treatment effect of bike-share stations

Table 2.6 displays the results of estimation equation 2.1 and 2.2. 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.

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

Table 2.6: 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.

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, I 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 may 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).

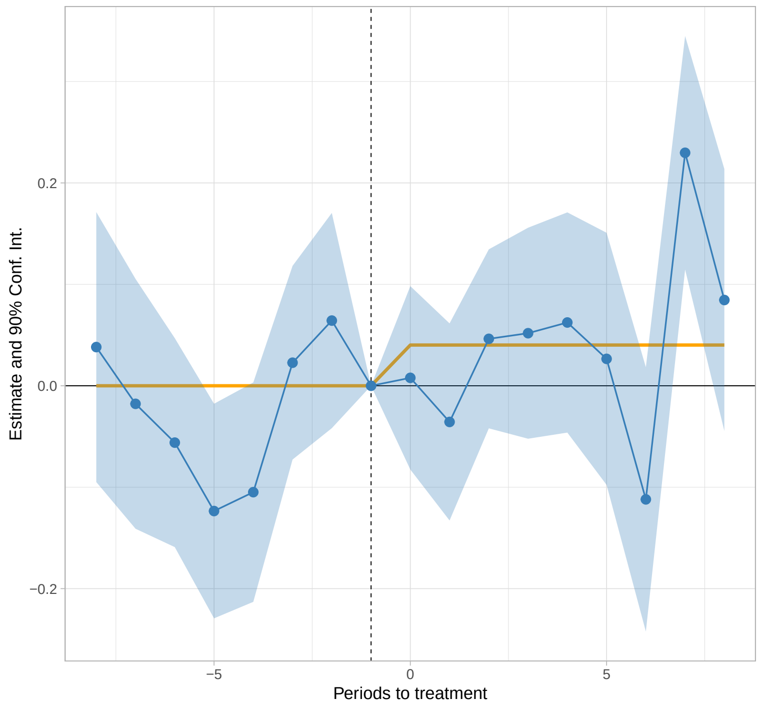


Figure 2.4: Dynamic effect of treatment on transaction price (event study). *Notes*: Periods to treatment are given in quarters. The shaded area indicates the coefficients' 90% confidence intervals. The orange line represents the post-period aggregated effect. The estimation only includes transactions within 500 metres of a bike-share station and includes the full set of covariates and fixed effects (see equation 2.3).

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.[12] Therefore, I decide not to report results with more granular fixed effects, as those would restrict too severely the number of observations used by the estimation.[13]

Figure 2.4 depicts the results from estimating equation 2.3, 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 strong support for the parallel trends assumption and post-treatment effects displayed in table 2.6.

I check the consistency of this result in Table 2.7. 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 2.4% (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.

2.4.1.3 Robustness checks: alternative rings

In this subsection, I perform the analysis laid out in equation 2.2 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 add, for each combination of the previous two dimensions, a buffer ring between treatment and control of 0 (baseline) and 50 metres. In specifications with a

[12]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.

[13]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 adding finer levels of fixed effects, the statistical significance of the treatment effect further improves.

Table 2.7: 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.

50-metre buffer, I exclude from analysis transactions that are in a 50-metre 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 2.8. All reported results follow the specification used in column 6 of table 2.6, 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 column 6 in table 2.6 (i.e., the baseline case).

Table 2.8: 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 2.2.3 and figure 2.1).

Table 2.8 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.[14] On the other hand, the estimations for the 400-metre sample ring are inconclusive.[15] 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.

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

[15]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 (see section 2.2.3 and figure 2.1).

2.4.2 Commercial properties

I now turn to transactions of commercial properties. Due to the important differences that exist between building-class categories in the realm of commercial properties, I perform the analysis for the difference categories separately. In the main text, I only present the results for offices and retail properties (table 2.9). 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 2.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 high density employment, retail and residential areas. Second, there seem to be fewer reasons to believe that agents transacting these types of properties include "bikeability" in their decision function, and they 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 2.9 show 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 2.C.1 presents the results of a hedonic model on all types of commercial property.

2.4.3 Discussion

Overall, the results of the analyses presented above point towards possible 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

Table 2.9: 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.

discuss its implications.

In section 2.4.1.2, 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 2.6, columns 1 and 2), but found a treatment effect of 6.1% on sale prices when 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 arguably 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 2.4. 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 several additional analyses. First, I replace the treatment indicator variable with a continuous measure of distance to treatment in table 2.7. The results are consistent with those of table 2.6, 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 2.8 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. For example, urban rail stations typically increase property prices by 3 to 10%. Studies on bike-share systems showed a reduction of the subway premium by up to 30% after the introduction of bike-share. Of particular interest to the present study are the estimated effects of bike-share on rents determined by Shr, Yang, et al. (2022) 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. Finally, 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 (Daniele et al., 2022).

2.5 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 found 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 held for an important subset of different ring sizes, but did 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 would 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.

Appendix 2.A Building decades

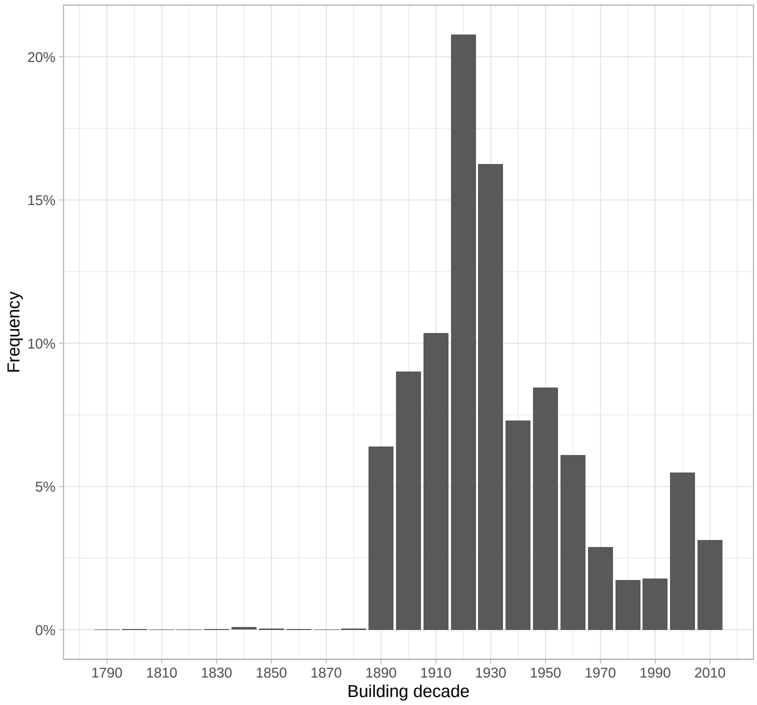


Figure 2.5: Building decade of transactions in the main sample data set (i.e., residential units, no price and price-per-square-feet outliers).

Appendix 2.B Detailed treatment map

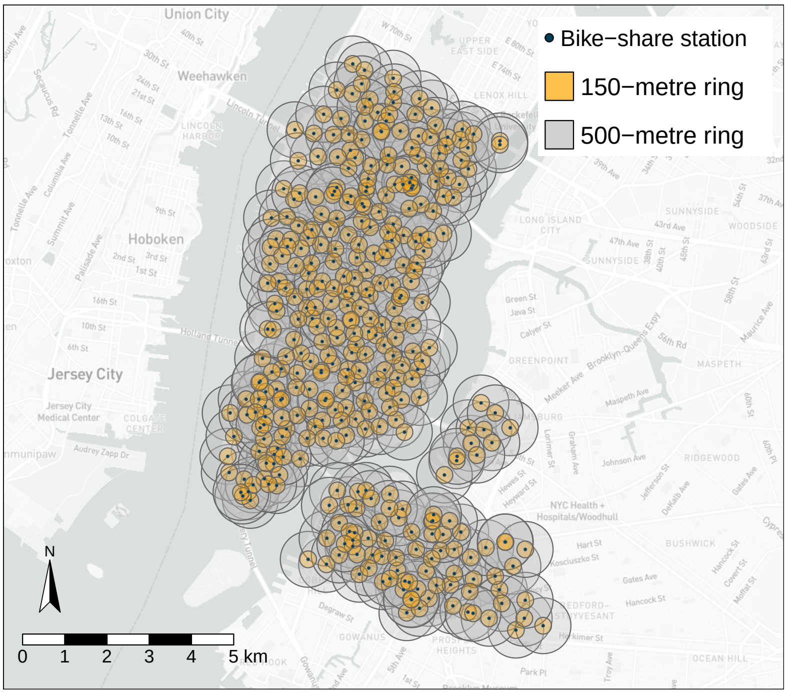


Figure 2.6: Overview of the study area and treatment and control rings, not merged. *Notes*: The yellow areas represent the treatment rings (i.e., within 150 metres of a bike-share station), while the grey areas indicate the control rings (i.e., between 150 and 500 metres from a bike-share station).

Appendix 2.C Additional analyses on commercial properties

2.C.1 Hedonic regressions

Table 2.10: Hedonic model — offices and retail transactions

	Log sale price (2015 $)							
Building class category →	Offices (1)	Retail (2)	Offices (3)	Retail (4)	Offices (5)	Retail (6)	Offices (7)	Retail (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 2.11: 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.

2.C.2 Treatment regressions, placebo analysis

Table 2.12: 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.

Chapter 3

Conscientiousness and Labor Market Returns: Evidence from a Field Experiment in West Africa

Joint with Mathias Allemand, Martina Kirchberger, Sveta Milusheva, Carol Newman, and Brent Roberts.

3.1 Introduction

A large body of evidence highlights a link between the Big Five personality traits (agreeableness, conscientiousness, extroversion, neuroticism and openness) and outcomes in the work place (Roberts, Kuncel, et al., 2007; Soto, 2019; Borghans et al., 2008; Bowles et al., 2001; Fletcher, 2013; Nyhus and Pons, 2005).[1] One such trait, conscientiousness, which relates to being punctual, hard working and responsible, emerges as a key trait for successful labor market outcomes, such as job performance and productivity (Barrick et al., 2001; Cubel et al., 2016; Dudley et al., 2006).[2] It has long been thought that the Big Five personality traits are immutable in adulthood and not amenable to intervention efforts. However, recent theories in psychology propose that mindfulness and behavioral change techniques can help to shift beliefs and

[1]In these studies, the Big Five personality traits have been found to be predictors of income, long-term unemployment, job stability, job performance, job satisfaction, extrinsic and intrinsic career success and financial security.

[2]Conscientiousness is a spectrum of constructs describing individual differences in the propensity to be self-controlled, responsible to others, hardworking, orderly, and rule abiding (Roberts, Jackson, et al. 2009; Roberts, Lejuez, et al. 2014). It includes inter-related facets such as industriousness, organization, self-control, responsibility, persistence, decisiveness, conventionality, and punctuality.

behaviors and modify skills to increase traits such as conscientiousness. This can be achieved by creating awareness about personality and its importance and by teaching behavior change skills (Allemand and Flückiger, 2017; Roberts, Hill, et al., 2017a).[3]

To what extent interventions that target specific traits can be effective in improving labor market outcomes remains an open question. Between 230 million and 450 million new workers are expected to enter the labor force in Africa by 2030 (WEF, WB and ADB, 2017; WEF, 2017). This places considerable pressure on young people to find and maintain jobs, particularly in the presence of substantial search and matching frictions and high rates of turnover documented in low-income countries (Abebe, Caria, Fafchamps, Falco, Franklin, and Quinn, 2021; Abebe, Caria, Fafchamps, Falco, Franklin, Quinn, and Shilpi, 2020; Banerjee and Sequeira, 2020; Donovan et al., 2021). McKenzie, 2021 highlights the potential for non-traditional training incorporating psychology and focusing on essential non-cognitive skills to improve labor market outcomes. A number of recent studies have demonstrated positive impacts of comprehensive skills training programs, for example, on productivity (Adhvaryu et al., forthcoming) or earnings (Chioda et al., 2021). Soft-skills training programs could be important for jobs that require minimal skills but a high level of conscientiousness.

This paper examines the impact of a targeted conscientiousness training intervention for low-skilled workers on employment, wages and job retention. To do this, we combine methods and insights from both economics and psychology. The training program is based on a recent conceptual intervention model, the Sociogenomic Trait Intervention Model (STIM) by Roberts, Hill, et al., 2017a, and a recent intervention program developed by Stieger, Wepfer, et al., 2020 for Swiss participants, which we adapted to our particular context. Our sample consists of 386 workers employed at baseline in the construction of a new express train in Dakar, Senegal. The training consisted of an initial two-hour in-person session, conducted on company premises during work hours by a trained professional, external to the company, on how to be more conscientious at work, followed by a series of weekly short phone calls to workers over eight weeks reminding them of different ways to behave more

[3]For example, initial evidence from high-income contexts shows that psychological interventions using smartphone apps can help people to change personality traits in desired directions in adulthood (Stieger, Wepfer, et al., 2020; Stieger, Flückiger, et al., 2021).

conscientiously at work. Workers were randomly assigned to the treatment and control groups. The control group did not receive any additional training beyond the standard training protocols implemented by the company when new workers take up employment and periodically throughout the duration of their contract.

The framework for the intervention builds on the assumption that a trait like conscientiousness is a system of continuous and varying trait-related behaviors and experiences that can be manifest as a skill (i.e., the momentary, optimal expression of a trait). Hence, the main aim of the intervention is to help people change the behaviors and experiences associated with a domain of conscientiousness in a way that ensures that the change is enduring. This could be by showing certain conscientiousness-related behaviors and experiences more frequently or more intensively (for example, trying to become more punctual at work). To achieve this, the STIM uses behavioral activation theory, a form of cognitive behavior therapy used to treat depression (Lejuez et al., 2001; Magidson et al., 2014).[4] The intervention aims to motivate and to activate the participants by changing and promoting conscientiousness-related behaviors and experiences. We target four important skill-based facets of conscientiousness that are specifically important in the context of work: industriousness, punctuality, responsibility and orderliness (Roberts, Lejuez, et al., 2014). Changes in these skills may have positive effects on labor market outcomes. If participants demonstrate more conscientiousness-related behaviors and experiences, such as punctuality, responsibility, or orderliness, in a work context, and this behavior is observed and valued by their supervisors, they are more likely to be retained in their posts and see their improved performance reflected in higher wages.[5]

We find that receiving conscientiousness skills training increases job retention and wages nine months after the end of the intervention. Workers in the treatment group were 10% more likely to remain employed by the construction company and their last reported monthly earnings were about

[4] Behavioral activation to treat depression is a method to re-motivate and reactivate depressed patients. The conditions and consequences that trigger and maintain the depressive behavior must be identified and changed. Depressive behavior is replaced step-by-step with potentially rewarding activities.

[5] Over the longer-term, the positive reinforcement that conscientiousness-related behaviors by workers are rewarded by supervisors could in turn encourage further conscientious behavior leading to a positive cycle that could lead to longer-term positive effects such as influencing whether someone is promoted or finds alternative employment.

40 USD higher than those of the control group (a 20% increase from average baseline earnings). We interpret these labor market impacts as "hard" evidence that our training had an impact. Examining shifts in self-reported measures of conscientiousness-related skills, we find respondents are more likely to find it easier to "work towards reaching one's goals", to "live up to your responsibilities", to "focus on one's most important goals" and to "fulfill one's duties and obligations". We treat these results as suggestive evidence that the training affected conscientiousness-related skills; the reliability of our scales of self-reported measures, while acceptable by conventional standards, is low due to high average reported response scores and/or other response biases. Our study therefore also highlights the difficulty of using standard questions to capture essential skills across very different contexts.

The paper contributes to three strands of the literature. First, our paper contributes to a recent literature on the effects of various soft-skills training programs on labor market outcomes that finds substantial effects on individuals' ability to maintain a job (Barrera-Osorio et al., 2021), returns to the firm with no effect on wages (Adhvaryu et al., forthcoming), effects on both individual earnings and firm profits (Chioda et al., 2021), positive effects on firm profits (Campos et al., 2017) or positive effects only for women (Acevedo et al., 2020). Groh et al., 2016, on the other hand, do not find effects on hours worked, income or employment.[6] These programs typically train workers on a combination of skills, such as communication, writing, time management, negotiation, and emotional regulation. We add to this literature by providing evidence on the effectiveness of targeting one of the key traits for labor market outcomes (conscientiousness).[7] A recent paper by Bryan et al., 2021 found that a theology education program which promoted evangelical Protestant Christian values had a positive impact on income that appears to be due to increased grit, which can be considered one facet of conscientiousness. Our experiment is the first, to our knowledge, to target different facets of conscientiousness and focuses on the impact on labor market outcomes.

Second, we contribute to a recent literature that points out distinctive features of labor markets in low-income countries, such as the high prevalence

[6] Aghion et al., 2019 further highlight the role of complementarities between soft skills of low-skilled workers and a firm's other assets.

[7] We also relate to a large literature on the effectiveness of worker training programs. For examples and overviews, see McKenzie, 2017, McKenzie, 2021, Alfonsi et al., 2020, Card et al., 2011 or Attanasio et al., 2011.

of non-salaried work (Bandiera et al., 2022), significant search and matching frictions (Abebe, Caria, Fafchamps, Falco, Franklin, and Quinn, 2021; Abebe, Caria, Fafchamps, Falco, Franklin, Quinn, and Shilpi, 2020; Banerjee and Sequeira, 2020) or the high turnover for low-earnings jobs (Donovan et al., 2021). Our evidence suggests that improving conscientiousness might play an important role in increasing job tenure.

Third, we contribute to an ongoing debate about whether particular traits can be changed in adulthood and answer this question in the context of low-skilled workers in a low-income country (Allemand and Flückiger, 2017; Roberts, Hill, et al., 2017a). Most of the current evidence stems from research in high-income countries with high-skilled samples (Stieger, Wepfer, et al., 2020; Stieger, Flückiger, et al., 2021). Our experiment allows us to examine whether conscientiousness-related skills can be activated in this particular setting using a simple and low-threshold intervention and also allows us to test the reliability of psychological interventions in vastly different contexts from which they were originally designed.

The rest of the paper is organized as follows. In Section 3.2 we present the setting for our study, and describe the experiment and the data. Section 3.3 presents the results and discusses possible mechanisms and potential caveats to the study. Section 3.4 concludes.

3.2 Setting and field experiment

3.2.1 Setting and background

The study took place from April 2019 to May 2020. The setting for our experiment was Dakar, Senegal. Dakar's workforce is, on average, aged 30 years old, 49% have attended middle school, and 72% are employed in low-skilled occupations or are self-employed (Agence Nationale de la Statistique et de la Démographie Sénégal and ICF, 2020). We worked with a company that forms part of a joint venture involved in the construction of a new 36 km long railway express train connecting the city center of Dakar with Diamniadio, a new city established outside Dakar, and in a second phase with the new international airport. The express train is one of the flagship projects of the government's five-year strategic plan which highlighted the need for improving transport in the Greater Dakar Area. The first phase of construction started in 2017 and was completed in 2020.

3.2.2 Field experiment

Workers employed at the company responsible for the construction of the new express train in Dakar were recruited for the study. Having a sample of workers employed by the same company allows us to hold unobservable determinants of conscientiousness constant, such as how motivating supervisors are, company work atmospheres and management styles. To select our sample, the company provided us with a list of workers in low-skilled positions that were working on different portions of the tracks of the express train. There were a total of 386 workers on these lists, which formed our sample. Almost two-thirds of the sample were manual workers, barrier workers, and security agents. The remainder were other types of workers on the construction site that were classified by the company as having a low level of qualifications.

Following recruitment to the study, we administered a baseline survey in April 2019 to collect information on worker characteristics and personality traits. After the baseline, half of the workers were randomly selected to receive the behavioral activation intervention focused on conscientiousness. All workers selected for the intervention took part.[8]

The intervention aims to: (a) present information about the importance and benefits of the four important aspects of conscientiousness at work; (b) increase the motivation to change conscientiousness states; (c) provide instructions to activate conscientiousness states; and (d) prompt behavioral practice using reminders to activate the conscientiousness states.[9] The intervention consisted of two components. The first component was a group training session with an average of 24 participants that lasted 2 hours, for eight groups in total. The length of the session and the number of participants were similar to other training sessions that workers received on safety and security. The session was hosted by a professional consultant with experience delivering training of this kind in Senegal.[10] The training was conducted in the local language, Wolof, to ensure that workers fully understood all of the material. During the session, the trainer explained different concepts regarding non-cognitive skills to workers in the treatment group, in particular conscientiousness, with an

[8]It is possible that there were spill-over effects between the treatment and control groups (given that workers were assigned to different locations on the construction site depending on the company's needs). This, however, would bias the results against finding an effect.

[9]Full details of the content of the training session are provided in Appendix 3.A.

[10]The consultant has previously led several training programs in Senegal in personal development, conflict management, judicial defense and youth support.

emphasis on how improvements in such skills can lead to long-term benefits for the worker. The control group did not receive any additional training beyond the standard training provided by the company to all workers, which includes an initial security training at the start of the contract and periodic sessions throughout the duration of the project.

The second component consisted of weekly reminders for eight weeks via short phone calls (less than 1 minute) to activate non-cognitive skills change. Calls were made by a survey company. Reminders were randomized across weeks and all workers received the same set of reminders each week. During each call, workers were given personal skills reminders, such as, "Make sure not to leave your place of work at any time without replacement", that target conscientiousness. Phone calls were also conducted in Wolof.

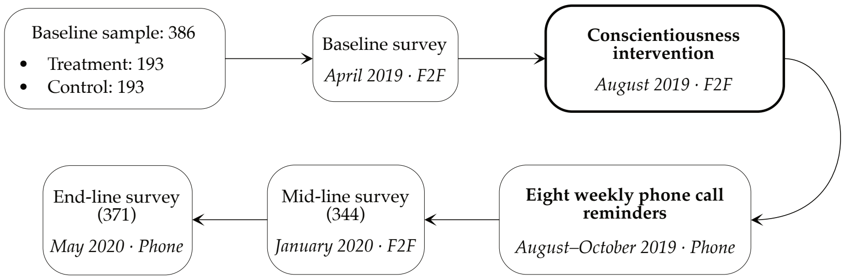
A mid-line survey was conducted in January 2020, and an end-line survey was conducted by phone in May 2020. Our sample started with the 386 individuals interviewed at baseline. At mid-line, 344 answered our survey (10.9% attrition), while at end-line we were able to reach 371 respondents (3.1% attrition).[11]

Despite the fact that this light touch intervention has a relatively low "dose", it is expected that the repeated reminders function as triggers that initiate the conscientiousness states in the daily life of the workers and instigate change processes. Skill change can be best elicited through repeating behaviors that differ from typical, trait-like behavior (Allemand and Flückiger, 2017; Roberts, Hill, et al., 2017a; Wrzus and Roberts, 2017). The accumulation of conscientiousness-related behaviors and experiences should eventually lead to more habitual behaviors and experiences, and personality change through bottom-up processes. Demonstrating more conscientiousness-related behaviors in the workplace, such as punctuality, responsibility, or orderliness, is desirable from both the employer's and the individual's perspective. A more punctual and responsible worker is more likely to be retained by the company and receive a higher wage. This may in turn lead workers to feel that they are living up to the expectations placed on them, promoting even more conscientiousness-related behaviors and experiences in the future.

Figure 3.1 summarizes the timeline of the intervention and surveys. Ap-

[11] Appendix Table 3.4 compares baseline characteristics of attrition and non-attrition groups. We do not find any systematic differences between these groups after controlling for multiple hypothesis testing. We also do not find differences in attrition between the treatment and control groups.

Figure 3.1: Timeline of intervention



Note: This figure shows the timeline of the intervention for the treatment group. The number of respondents is in parenthesis. *F2F* indicates the survey or intervention was done face-to-face, while phone surveys are coded with *Phone*. See Appendix Figure 3.2 for a more detailed version.

pendix Table 3.5 shows some basic characteristics of the sample for treatment and control workers. The average age of participants in our study was 36-37 years.[12] The vast majority were male and only around one-fifth had completed middle school. For most, this was not their first formal job and about one-tenth of the sample were recent migrants. We do not find any statistically significant difference in baseline traits or household-level variables across the treatment and control groups with the exception of worker age: treatment workers were slightly younger than control workers but this difference is only marginally statistically significant at conventional levels.

Following Cumming (2014), we also report Cohen's d measures and their 95% confidence intervals in the last columns of Appendix Table 3.5 for the differences between treatment and control groups. We find that Cohen's d are all smaller than 0.2[13] and that the confidence intervals include zero, suggesting that the differences between groups are not significantly different from each other. While some of the household-level baseline characteristics are different when looking at them individually, Cohen's d is always below 0.2. Further, applying a correction for multiple hypothesis testing eliminates any statistical significance in group differences. We therefore conclude that our sample is balanced across the treatment and control groups on these baseline characteristics. Throughout the paper, we report our results with and without

[12] Our sample is slightly older than the general workforce in Dakar. This is due to the company recruiting workers with some prior experience.

[13] This is the conventional cut-off value for Cohen's d below which effects are considered small or very small (Sawilowsky, 2009).

baseline controls.

3.3 Results and discussion

This section presents our key results. We start by showing the effects of the intervention on the probability of being employed, the probability of still working at the construction company and on wages. We then discuss possible mechanisms and potential limitations of our study.

3.3.1 Main results

Table 3.1 shows the results of a simple ordinary least squares regression of the following equation:

$$y_{i,t=3} = \beta_0 + \beta_1 T_i + \beta_2 X_{i,t=1} + \varepsilon_{i,t=3} \tag{3.1}$$

where $y_{i,t=3}$ is the outcome of interest for worker i at end-line, T_i is an indicator for whether the worker is in the treatment or the control group, $X_{i,t=1}$ are baseline individual and household controls which include sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings and household income, and ε_i is a statistical noise term. We show the unconditional results in columns (1) and (3) and add the controls in columns (2) and (4).[14]

Columns (1) and (2) of Table 3.1 show that our treatment did not significantly increase the probability of employment at end-line.[15] However, workers in the treatment group were about 10% more likely to keep their job in the construction company.[16] These results are similar when we add baseline controls in column (4).[17] [18]

[14] To account for multiple hypothesis testing we also compute sharpened q-values which control for the false discovery rate. They are computed following Anderson (2008) and correct for multiple hypothesis testing within groups of outcomes. The results are presented in Appendix Table 3.6. All of our results on labor market outcomes are robust to this correction.

[15] At end-line, 72% of the sample were still employed and 51% were still employed at the construction company.

[16] The magnitudes and significance of the treatment coefficients are robust to a logit specification.

[17] We do not find any differences in the probability of remaining in the company by baseline occupations (see Appendix Table 3.7).

[18] About two-thirds of workers who left the company stayed in the construction sector, with an additional 15% of workers employed in wood and metal work, carpentry and craftsmanship (see Appendix Table 3.8).

Table 3.1: Labor market outcomes at end-line

	(1) Employed	(2) Employed	(3) Still at company	(4) Still at company
Treated	0.0613	0.0580	0.103**	0.109**
	(0.187)	(0.213)	(0.048)	(0.039)
Constant	0.694***	0.895***	0.456***	0.373**
	(0.000)	(0.000)	(0.000)	(0.010)
Standardized coeff.	0.137	0.130	0.206	0.217
Baseline controls		✓		✓
N	371	371	368	368
R-sq	0.00472	0.0428	0.0106	0.0344

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income.

We next turn to last-month's earnings at end-line.[19] Column (1) in Table 3.2 shows that the treatment group received significantly higher earnings at end-line. Their monthly earnings rose by a sizeable amount of 22,545 CFA francs (41.51 in 2020 USD) compared to the control group.[20] The positive impact on wages is robust to the inclusion of baseline controls in column (2). In column (3) we additionally control for earnings at baseline (baseline outcome) and the result holds, increasing slightly in magnitude.

In light of the higher probability of retention within the company shown in Table 3.1, we next explore to what extent the earnings impacts are driven by keeping a job at the construction company by controlling for whether a worker is still employed by the company in column (4). Table 3.2 shows that workers employed by the construction company did earn significantly higher wages, which explains part of the result, but there was also an additional effect of the training of 15,759 CFAF (29.02 in 2020 USD) that workers earned, irrespective of where they were working at end-line.

[19] Appendix 3.C provides detailed information on how earnings are defined.

[20] This represent a 20% increase from average baseline earnings. The average wage of workers at end-line was 84,146 CFAF (155.13 USD).

Table 3.2: Earnings at end-line

	(1) Last earnings	(2) Last earnings	(3) Last earnings	(4) Last earnings
Treated	22544.7***	23816.3***	24408.6***	15759.7**
	(0.005)	(0.003)	(0.001)	(0.018)
Still at company				73271.0***
				(0.000)
Constant	72751.5***	44534.9**	21716.9	-1626.4
	(0.000)	(0.045)	(0.304)	(0.930)
Standardized coeff.	0.289	0.306	0.313	0.202
Mean outcome control gr.	112644.92	112644.92	112644.92	112644.92
Baseline controls		✓	✓	✓
Baseline outcome			✓	✓
N	370	370	370	367
R-sq	0.0210	0.0599	0.169	0.378

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household,total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: earnings measured at baseline.

3.3.2 Mechanisms

We now turn to potential mechanisms that could explain the improvements in the labor market outcomes that we observe. We consider three possible mechanisms. First, the training could have directly affected conscientiousness-related skills and this in turn affected labor market outcomes by making workers capable of being more responsible and hard-working and thereby more productive. Second, the training could have affected other qualities, such as agreeableness or openness, and these led to participants keeping their jobs. In turn, longer tenure allowed workers to acquire conscientiousness skills and these impacted their earnings. Third, it is possible that the firm kept track of who was trained and on the basis of this, retained workers and paid them higher wages. We discuss each of these potential channels in turn.

First, to measure the direct effect of the intervention on conscientiousness, we measured conscientiousness-related skills before the training at baseline and after the training at mid-line using a 32-item questionnaire measuring skill-based versions of four facets of conscientiousness: responsibility, industriousness, organization and punctuality (Soto et al., 2022). Using face-to-face interviews, each respondent was asked to answer a set of questions on "how easy or difficult it is to" behave in a particular way relating to each of the scales. Responses were recorded using a five-point scale ranging from one (very difficult) to five (very easy). Details on the conscientiousness-related skill measures used are provided in Appendix 3.D and Appendix Table 3.10.

While not perfect, the reliability metrics of the psychometric scales (Cronbach's alpha and the interitem covariance, reported in Appendix Table 3.11) are acceptable, and the reliability of the overall conscientiousness skill scale is satisfactory (Taber, 2018). Several factors might explain the imperfect scores of the scales: translation issues,[21] cultural differences, social desirability bias or the fact that we asked the questions in an interview format rather than have respondents self-report. More generally, this highlights that psychometric surveys, often designed with university students in Western cultures in mind, may not be easily translatable to different contexts, especially in developing countries and for low-skilled workers.

With these caveats in mind, we found that the training had an impact on four of the 32 conscientiousness items and report the results in Table 3.3. Specifically these are: how easy or difficult is it for you to (1) work towards reaching your goals; (2) focus on your most important goals; (3) live up to your responsibilities; and (4) fulfill your duties and obligations.

We did not find a statistically significant effect on any of the other conscientiousness skills or the overall conscientiousness skill scale (see Appendix Tables 3.12 to 3.20). The high value of the constant term in all specifications indicates very high response scores, saturated close to their maximum value of five. This might be because respondents feel pressured into making positive judgements about themselves to appear socially desirable or signal their willingness to keep their job at the construction company. Such high baseline scores leave little room for change/improvement in these measures as a result of the intervention.

[21] The measures we used were translated from English into French and then into the local language Wolof, which may affect reliability.

Table 3.3: Conscientiousness skills

	(1) Reach goals	(2) Focus on goals	(3) Responsible	(4) Fulfill duty
Treated	0.268*	0.162*	0.298*	0.162*
	(0.059)	(0.081)	(0.009)	(0.011)
Constant	3.941***	4.666***	4.030***	4.566***
	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.202	0.192	0.281	0.274
Mean outcome control gr.	3.47	4.33	4.15	4.53
N	343	343	343	343
R-sq	0.074	0.028	0.084	0.063

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels. Personality skills are measured at baseline and midline. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. All specifications include baseline controls and skill measured at baseline. Baseline controls include sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Reach goals* and *Focus on goals* both measure the industriousness skill, while *Responsible* and *Fulfill duty* measure the responsibility skill. Appendix Table 3.12 presents specifications excluding the control variables.

Despite this, the results suggest that individuals who received the training report at least some increased levels of conscientiousness skills along some of the facets. Magnitudes of the effects range between a 0.16 point increase on the five-point scale for "focusing on one's most important goals" and a 0.30 point increase for "living up to one's responsibilities". These increases are robust to the inclusion of skill scores at baseline and the same set of controls as in Tables 3.1 and 3.2.[22] We also explored other margins that could have been

[22] It should be noted that we lose statistical significance when we adjust the *p*-values using the sharpened *q*-values (see Appendix Table 3.6). This is not surprising given the large number of traits considered. As such, these findings should be treated as providing suggestive evidence of the impact of the training on conscientiousness-related behaviors. We also performed a principal component analysis on the four facets of conscientiousness to reduce the dimensionality, extracting either the first principal component of each facet and then examining the impact of the treatment, or combining the first principal component of each facet into a summary measure. These measures similarly suffer from a high average value at baseline and lack of variation in the measured traits leading to very high *p*-values.

affected by the treatment such as hours worked per week, having a second occupation or self-reported levels of absenteeism. We do not find that the treatment affected any of these margins.[23]

Second, an alternative mechanism is that the intervention affected other personality domains. For example, it could be that workers in the treatment group felt that the company had chosen them specifically to invest in. Because of this, they behaved more respectfully towards their supervisors and colleagues. This in turn raised their tenure which allowed them to acquire better levels of conscientiousness which was then reflected in their labor market outcomes. To examine this mechanism, we explored whether any of the other Big Five domains (agreeableness, extroversion, neuroticism and openness) shifted in significant ways. We do not find any clear evidence supporting this alternative mechanism (all coefficients are insignificant when we control for multiple hypothesis testing), although it should be noted that these scores also suffer from high baseline values. Further, we re-estimate the earnings equation (Table 3.2) and include an interaction term between remaining in the company and treatment (see Appendix Table 3.9). The interaction term is not statistically significant suggesting that the effect of training was the same whether an individual is employed by the construction company or not. This suggests that the effects are not working exclusively through longer tenure at the company.

Third, we cannot rule out that the company kept track of who we trained and kept workers because they knew they were trained, although this information was not shared with the company. While this could contribute to the employment retention effects, positive wage effects for workers who left the company would not be driven by this. This suggests that our results are not totally driven by the company keeping track of trained workers. Moreover, observations from the field do not support this hypothesis. Supervisors tend to be in close contact with their workers, and closely monitor their behavior. Given how important it is for the company to be able to rely on workers to perform responsibly in these roles, it is unlikely that the company would not take into account the actual performance of workers in their performance

[23]Hours worked are already quite high at baseline, with a mean of 61.4 hours worked and a standard deviation of 11.3 making it difficult for workers to increase hours or take a second job. Further, most workers employed at the company take the company bus to the construction site and back home, so that there is not much scope for an individual worker to adjust their hours worked.

evaluations.

Overall, the evidence is supportive of a mechanism whereby our intervention affected worker conscientiousness and this in turn affected their labor market outcomes, although more research is needed to better measure conscientiousness skills in such contexts.

3.3.3 Potential limitations

We note two limitations to our study. First, end-line data collection took place after the onset of the COVID-19 pandemic and it is possible that our intervention interacted with the pandemic. For example, given that the training aimed to make individuals more conscientious, it is possible that they were more careful during the pandemic and lost their jobs due to wanting to stay at home rather than run the risk of exposure to the virus by going to work. On the other hand, more conscientious individuals might have felt a greater responsibility to go to work. To measure whether the intervention affected the impact of COVID-19 on the behavior of the workers, the end-line survey asked a number of questions on how the pandemic impacted their livelihoods. One set of questions asked respondents to compare current incomes and expenditures to before the start of the pandemic, specifically in February 2020. We do not find any significant differences in the extent to which treated and control individuals reported lower incomes, lower transfers and gifts, higher health expenditures, lower savings or more borrowing (see Appendix Tables 3.21 and 3.22).[24] We find that individuals in the control group reported having taken more measures to prevent the spread of the virus but do not find any differences in the rate at which treatment and control individuals displayed symptoms, or lost their main source of income as a result of COVID-19. Overall, this suggests that the intervention did not interact with the pandemic in a significant way.

A second concern relates to the lack of an active control group. For the first part of the intervention, the training session, it is worth noting that general security training sessions were common for all workers at the company. Workers were brought together in similarly sized groups to the conscientiousness training session for these security training sessions. For the phone reminders, providing messages that were different in content to

[24] We only find a small statistically significant difference for education expenditures, which is higher for the control group.

the conscientiousness messages would constitute a different treatment arm rather than an active control group. Moreover, reminders relating to different aspects of the general security training that all workers received would look similar to the phone reminders that the treatment group received. As such, an active control group was not included in the study.

Finally, it is worth mentioning that our results capture relatively medium-term impacts. While beyond the scope of this paper, future research should also seek to measure longer-term impacts.

3.4 Conclusion

We used a randomized controlled trial to test the impact of a conscientiousness training intervention for low-skilled workers in an urban developing country setting on employment, wages and job retention. The training program was based on recent work in the field of psychology (Roberts, Hill, et al., 2017a; Stieger, Wepfer, et al., 2020) and was designed to affect conscientiousness traits among workers. We found that providing conscientiousness training significantly affected the probability that the workers were still employed by the company at end-line and their level of earnings.

To our knowledge, this is one of the first studies that tests a psychological intervention of this kind in the field. The developing country context for the study is particularly relevant given the importance of job creation and the problem of job turnover in these settings. Our study highlights the potential for psychological training of this kind to improve labor market outcomes for low-skill workers. By making our training materials fully available, we hope to facilitate further research on this topic.

Furthermore, while we find some suggestive evidence that the training affected the conscientiousness skills of the treated workers, our study also underlines the challenges associated with using standard, self-reported measures to capture personality skills across different contexts, with different types of populations, in different languages and across different levels of literacy. Future research that provides guidance on the appropriate instruments to use in settings outside university campuses is vital for being able to pin down the precise channels through which interventions aimed at affecting non-cognitive skills in such contexts work.

Appendix 3.A Details of the Experiment

3.A.1 Curriculum of the psychological intervention

1. Introduction and goals of the group training session

Duration: 5 min

- Introduction to group training session and warm-up
- Present outline and the goals of the session
- Present expectations (e.g., willingness to participate in group discussions)

2. Present information about the importance and benefits of conscientiousness at work

Duration: max. 25 min

2.1. Present facts about conscientious behaviors at work

Present information about factors and skills that can make people more successful or less successful at work. On the one hand, research has shown that skills ["keeping one's work place clean" "following through with commitments"] and experiences [setting and achieving goals] are important for success at work and satisfaction with the job. On the other hand, research has also shown that a lack of skills or willingness to learn the skills is related to job-related difficulties [being repeatedly late to work]. This group session will introduce and discuss the importance and benefits of four important skills in the context of work and beyond.

Present definitions of the four "non-cognitive" skills (i.e., industriousness, punctuality, responsibility, and orderliness (Roberts, Kuncel, et al., 2007); [please simplify the definitions to fit the needs of the participants]:

- *Industriousness* captures the tendencies to work hard, aspire to excellence, and persist in the face of challenges.
- *Punctuality* reflects the simple tendency to show up on time to previously scheduled appointments. Being punctual appears important when considering one's ability to plan, work hard to get somewhere, avoid temptations that might lead one to be late, care enough to meet other people on time, and understand the rules and conventions surrounding one's social group.

- *Responsibility:* On the high end of the spectrum, responsibility reflects the tendency to follow through with promises to others and follow rules that make social groups work more smoothly. On the low end, it reflects the tendency to be an unreliable partner in achievement settings and to break one's promises.

- *Orderliness* encompasses the overarching tendency to be "prepared", which includes tendencies toward neatness, cleanliness, and planfulness on the positive side, or disorderliness, disorganization, and messiness on the negative end of the spectrum.

2.2. Transfer of the definitions to real-life work context

Transfer the definitions to the everyday work life contexts of the participants to make them more comprehensible and realistic to participants [provide specific examples and scenarios from the job as barrier workers to motivate the discussion].

- What does it specifically mean to be *hardworking* at my workplace/in my job? (e.g., invest efforts into work; complete tasks; help site vehicles to cross; observe all rules; secure train passages; remove all stones etc.; monitor equipment; take tours through the allocated area; work as hard as everyone else on the site)

- What does it specifically mean to be *punctual* at my workplace/in my job? (e.g., show up on time; be on time in the morning at the bus pick-up point; be punctual for meetings with supervisor)

- What does it specifically mean to be *responsible* at my workplace/in my job? (e.g., to follow work-related rules; consult the supervisor in case of a delay or absences; inform the supervisor in case of a problem)

- What does it specifically mean to be *orderly* at my workplace/in my job? (e.g., keep the workplace in order; keep the torch clean in the building barracks, workplace or surroundings of the workplace; do not leave your own uniform e.g., helmets, vests, etc. unattended)

3. Increase the motivation to change conscientiousness

Duration: max. 20 min

3.1. Discuss possible rewards of being conscientious

Discuss real or hypothetical situations/scenarios in the everyday work life of the participants, showing how certain skills lead to *better work outcomes*. The goal of this task is to motivate participants' behaviors. It should be made clear what participants would gain when they habitually show conscientious behaviors:

- What are possible positive consequences or rewards (for the individual, for others, for the workplace) of being *hardworking* at the workplace? [getting a raise; helping the company complete the work better or faster]
- What are possible consequences or rewards (for the individual, for others, for the workplace) of being *punctual* at the workplace? [not losing your job; getting another job with the same company]
- What are possible consequences or rewards (for the individual, for others, for the workplace) of being *responsible* at the workplace? [helping the entire organization to be better thus making the company successful leading to keeping your job and future jobs]
- What are possible consequences or rewards (for the individual, for others, for the workplace) of being *orderly* at the workplace? [avoiding accidents and not hurting yourself or others]

3.2. Discuss possible costs of low conscientiousness

Discuss real or hypothetical situations/scenarios in the everyday work life of the participants, showing how missing or poorly trained skills leads to *poor work outcomes*. The goal of this task is to motivate participants' behaviors by pointing to possible costs [please provide realistic examples and scenarios]:

- What are possible negative consequences or costs (for the individual, for others, for the workplace) of being *lazy* at the workplace? [less likely to receive a recommendation for another job; less likely to be given other opportunities]
- What are possible negative consequences or costs (for the individual, for others, for the workplace) of being *unpunctual* at the workplace? [making other coworkers in a previous shift have to stay late]

- What are possible negative consequences or costs (for the individual, for others, for the workplace) of being *unreliable* at the workplace? [slow-down work for everyone; less likely to be asked to continue working for the company]

- What are possible negative consequences or costs (for the individual, for others, for the workplace) of being *disorganized* and *messy* at the workplace? [higher likelihood of hurting yourself or someone else in the workplace]

4. Provide instruction on how to activate conscientious behaviors
Duration: max. 40 min

4.1. Behavioral activation tasks / behavioral "experiments" in daily work life

To pursue their specific goals and to perform the four important skills in the context of work, participants have to practice and repeat the goal-related behaviors in the same work context repeatedly so that the contexts may elicit the behaviors [the specific behavioral activation tasks / "behavioral experiments" based on the list in the appendix can be used for the discussion].

4.2. Possible barriers to perform the four important skills

Discuss possible barriers that may hinder participants to perform the skills (e.g., individual barriers, work-related barriers).

- What are possible barriers to being *hardworking* at the workplace? How can these barriers be overcome?

- What are possible barriers to being *punctual* at the workplace? How can these barriers be overcome?

- What are possible barriers to being *responsible* at the workplace? How can these barriers be overcome?

- What are possible barriers to being *orderly* at the workplace? How can these barriers be overcome?

4.3. Possible resources to perform the four important skills

Discuss possible resources that may help participants to perform the skills (e.g., social support).

- What are possible resources for being *hardworking* at the workplace? How can these resources be used?

- What are possible resources for being *punctual* at the workplace? How can these resources be used?
- What are possible resources for being *responsible* at the workplace? How can these resources be used?
- What are possible resources for being *orderly* at the workplace? How can these resources be used?

4.4. Identification of goals and goal setting

Participants specify one specific goal for each of the four skills (i.e., industriousness, punctuality, responsibility, and orderliness) they would like to pursue in the next weeks. For each of the four specific goals, participants have to generate one *specific goal* in the form of an *if-then plan* (e.g., "If I see a car right in front of the barrier, then I check all the rules before I let the car pass" [industriousness]; "If I have a meeting with my supervisor, then I will do everything I can to be there on time" [punctuality], "If I want to leave the workplace, then I will ask the supervisor first" [responsibility]; "If I start to work, then I will keep things tidy" [orderliness]) [please provide simple examples from the daily life work context]. This task also may help to identify specific situations in which participants can perform specific goal-related behaviors: What are typical work-related situations ("if") in which participants perform the behaviors and the four skills, respectively ("then")?

The goal setting process should follow the SMART criteria:

- *Specific:* Goals must be clearly defined (not vague, but as precise as possible).
- *Measurable:* Goals must be measurable.
- *Attainable/attractive:* The goals must be attainable and desirable for the person.
- *Realistic:* The goal set must be possible and feasible.
- *Timely:* It must be possible to set a reasonable time limit to achieve the goal including time limits for smaller steps.

Behavior Activation through Reminders

Prompt behavioral practice by weekly reminders to activate conscientiousness behaviors

Procedure

Every week each participant will get a short phone call with 4 reminders targeted toward each of the four skills. For each phone call, 4 behavioral activation tasks / "behavioral experiments" (1 task per facet of conscientiousness) can be randomly [or, sequentially] selected from the list below.

Appendix: Behavioral Activation Tasks / "Behavioral Experiments"

Here is a set of broad and specific behavioral activation tasks / "behavioral experiments" [this list can be expanded with more specific tasks depending on the participants' daily work life].

Industriousness

Broad reminders

- Try to have high standards and work toward them.
- Try to go above and beyond of what is required.
- Try to work as hard as the majority of people around you.
- Try to give the highest quality in everything you do.
- Try to do more than what is required.
- It's important to set goals and achieve them.
- Complete the tasks you start.
- Persist at tasks after meeting setbacks or failures.
- Try to work extra hard on a project to make sure that it is done right.
- Complete the projects you start.
- Finish what you start.
- Put your mind on the task at hand.
- Get things done quickly.
- Always know what you are doing.
- Do not let yourself get distracted.

- Do not postpone decisions.
- Finish what you start, e.g., checking a vehicle, cleaning a rail, touring through an area for monitoring, unloading a truck.
- Get things done quickly.
- Do not let yourself get distracted, e.g., by another vehicle while checking one vehicle.

Specific reminders

- Help site vehicles to cross the rail:
  - Try to give the highest quality in everything you do, e.g., check all the rules before you let a car pass (headlights on, rear red lights on, no children, no people in the load area, the car has a numbered red badge, etc.)
  - Put your mind on the task at hand, e.g., when checking a vehicle crossing the rail.
- Secure train passages:
  - Try to give the highest quality in everything you do, e.g., check that there are no stones, sand or holes in the rail at all.
  - Put your mind on the task at hand, e.g., when a train is arriving, and you prevent pedestrians and vehicles from passing.
- Monitoring equipment:
  - Put your mind on the task at hand, e.g., touring through an area to monitor small railway equipment.
- Unloading trucks:
  - Try to give the highest quality in everything you do, e.g., try to unload the equipment of a truck as properly as you can.
  - Put your mind on the task at hand, e.g., when you are unloading a truck.

Punctuality

- Do not forget meetings.
- Keep up with required work.
- Get to appointments with your supervisor on time.
- Do not miss the bus; be at the picking up point on time.
- Return phone calls in timely fashion.

Responsibility

- Try to carry out your obligations to best of your ability.
- Go out of your way to keep your promises.
- If you are running late, call ahead to notify those who are waiting for you.
- If you are running late, call ahead and inform the supervisor.
- If you want to leave the workplace, ask the supervisor first.
- Unloading trucks: In event of a problem, register the truck number and call the supervisor.
- If you need to call in sick, do so before your shift so your supervisor can get a replacement.

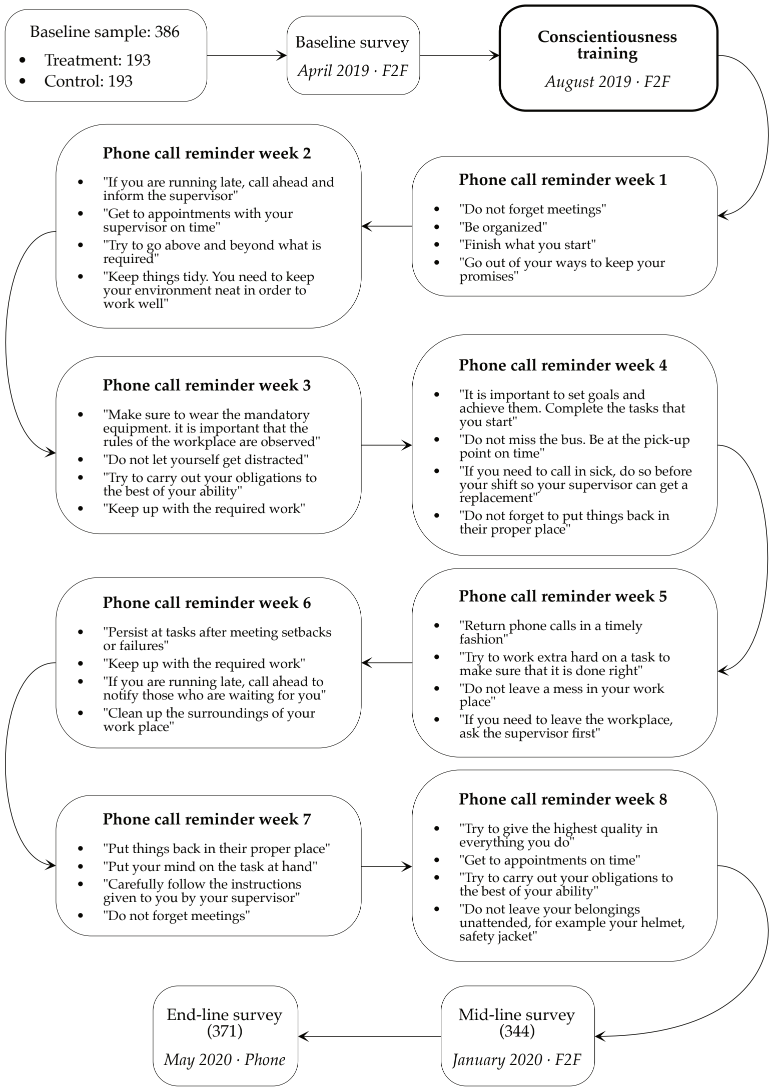
Orderliness

- Keep your environment neat in order to work well.
- Be organized.
- Do not forget to put things back in their proper place.
- Clean up the surroundings of your workplace.
- Do not leave a mess in your work place.
- Tidy up your work place.

- Put things back in their proper place.
- Do not leave your belongings unattended, e.g., orange project jacket, helmet.
- Keep things tidy.
- See that rules are observed, e.g., do not make fire around the workstation, do not wear earphones on your ears at the workstation, wear your mandatory equipment.

3.A.2 Detailed timeline

Figure 3.2: Detailed timeline of the intervention and surveys, including phone call reminders' contents



Note: This Figure expands on Figure 3.1 and shows the main conscientiousness reminders for each follow-up phone call. The number of respondents is in parenthesis. *F2F* indicates the survey or intervention was done face-to-face, while phone surveys are coded with *Phone*.

Appendix 3.B Additional tables

Table 3.4: Sample characteristics at baseline for attrition vs non-attrition groups

	(1)		(2)		(3)		t-test	Cohen's D		
	Non-attrition		Attrition		Total		Difference			
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)	Effect size	95% CI	
Individual-level variables										
Last earnings	332	1.11e+05	54	1.16e+05	386	1.12e+05	-4475.682	-0.066	-0.353	0.222
		[3228.205]		[14796.793]		[3453.839]				
Female	332	0.096	54	0.019	386	0.085	0.078*	0.279	-0.009	0.567
		[0.016]		[0.019]		[0.014]				
Age	332	36.352	54	37.296	386	36.484	-0.944	-0.105	-0.393	0.183
		[0.483]		[1.366]		[0.457]				
Middle school	332	0.244	54	0.111	386	0.225	0.133**	0.319	0.030	0.607
		[0.024]		[0.043]		[0.021]				
First formal job	330	0.124	38	0.053	368	0.117	0.072	0.223	-0.113	0.559
		[0.018]		[0.037]		[0.017]				
Recent migrant	325	0.098	37	0.054	362	0.094	0.044	0.152	-0.188	0.492
		[0.017]		[0.038]		[0.015]				
Reach goals	332	3.518	54	3.370	386	3.497	0.148	0.101	-0.187	0.388
		[0.079]		[0.220]		[0.075]				
Focus on goals	332	4.398	54	4.315	386	4.386	0.083	0.086	-0.202	0.374
		[0.052]		[0.137]		[0.049]				
Up to responsabilities	332	4.220	54	3.926	386	4.179	0.294*	0.274	-0.015	0.562
		[0.057]		[0.171]		[0.055]				
Fulfill duty	332	4.503	54	4.593	386	4.516	-0.090	-0.108	-0.396	0.179
		[0.046]		[0.107]		[0.042]				
Household-level variables										
# Beds in household	332	4.792	54	5.185	386	4.847	-0.393	-0.145	-0.433	0.143
		[0.149]		[0.369]		[0.138]				
# Children in household	332	4.452	54	4.500	386	4.459	-0.048	-0.013	-0.301	0.274
		[0.203]		[0.395]		[0.183]				
# Adults in household	332	6.349	54	6.889	386	6.425	-0.539	-0.141	-0.429	0.146
		[0.203]		[0.610]		[0.194]				
Household debt	332	26731.928	54	37962.963	386	28303.109	-1.12e+04	-0.193	-0.481	0.095
		[3064.672]		[9661.929]		[2963.524]				
Household savings	332	83734.940	54	48148.148	386	78756.477	35586.792**	0.350	0.061	0.638
		[5752.068]		[10876.423]		[5209.624]				
Household income	332	53448.795	54	49351.852	386	52875.648	4096.943	0.140	-0.147	0.428
		[1631.964]		[3439.215]		[1484.070]				

Note: The *Attrition* group includes all individuals who missed at least one round (either the mid-line or end-line survey, or both). *Recent migrant* is defined as living for three years or less in current location and moved from outside Dakar. *Middle school* indicates whether an individual has completed middle school. Applying multiple hypothesis correction, any statistical significance in group difference disappears.

Table 3.5: Sample characteristics at baseline

		(1)		(2)		(3)	t-test	Cohen's D		
		Control		Treatment		Total	Difference			
Variable	N	Mean/SE	N	Mean/SE	N	Mean/SE	(1)-(2)	Effect size	95% CI	
Individual-level variables										
Last earnings (in th.)	193	112.645	193	111.230	386	111.937	1.415	0.021	-0.179	0.220
		(5.618)		(4.034)		(3.454)				
Female	193	0.088	193	0.083	386	0.085	0.005	0.018	-0.181	0.218
		(0.020)		(0.020)		(0.014)				
Age	193	37.326	193	35.642	386	36.484	1.684*	0.188	-0.012	0.388
		(0.644)		(0.645)		(0.457)				
Middle school	193	0.212	193	0.238	386	0.225	-0.026	-0.062	-0.261	0.138
		(0.030)		(0.031)		(0.021)				
First formal job	182	0.110	186	0.124	368	0.117	-0.014	-0.043	-0.247	0.162
		(0.023)		(0.024)		(0.017)				
Recent migrant	179	0.084	183	0.104	362	0.094	-0.020	-0.068	-0.275	0.138
		(0.021)		(0.023)		(0.015)				
Reach goals	193	3.472	193	3.523	386	3.497	-0.052	-0.035	-0.235	0.164
		(0.106)		(0.105)		(0.075)				
Focus on goals	193	4.332	193	4.440	386	4.386	-0.109	-0.113	-0.313	0.086
		(0.073)		(0.066)		(0.049)				
Up to responsibilities	193	4.145	193	4.212	386	4.179	-0.067	-0.062	-0.262	0.137
		(0.079)		(0.076)		(0.055)				
Fulfill duty	193	4.528	193	4.503	386	4.516	0.026	0.031	-0.168	0.231
		(0.057)		(0.062)		(0.042)				
Household-level variables										
# Beds in household	193	4.850	193	4.845	386	4.847	0.005	0.002	-0.198	0.201
		(0.206)		(0.184)		(0.138)				
# Children in household	193	4.596	193	4.321	386	4.459	0.275	0.076	-0.123	0.276
		(0.283)		(0.232)		(0.183)				
# Adults in household	193	6.290	193	6.560	386	6.425	-0.269	-0.071	-0.270	0.129
		(0.286)		(0.264)		(0.194)				
Household debt	193	29274.611	193	27331.606	386	28303.109	1943.005	0.033	-0.166	0.233
		(4418.191)		(3961.204)		(2963.524)				
Household savings	193	74093.264	193	83419.689	386	78756.477	-9326.425	-0.091	-0.291	0.109
		(7054.669)		(7671.283)		(5209.624)				
Household income	193	53756.477	193	51994.819	386	52875.648	1761.658	0.060	-0.139	0.260
		(2140.630)		(2059.713)		(1484.070)				

Note: Last earnings is given in thousand CFA francs, Senegal's local currency. *Recent migrant* is defined as living for three years or less in current location and moved from outside Dakar. *Middle school* indicates whether an individual has completed middle school. Correcting for multiple hypothesis eliminates any statistical significance between group differences.

Table 3.6: Correction for multiple inference of the treatment effects

	Bivariate	Baseline controls	Baseline outcomes
***Panel A* · Labour market outcomes**			
Employed			
Unadjusted p-value	0.187	0.213	
Sharpened q-value	0.078	0.077	
Still at company			
Unadjusted p-value	0.048	0.039	
Sharpened q-value	0.051	0.042	
Last earnings			
Unadjusted p-value	0.005	0.003	
Sharpened q-value	0.017	0.010	
***Panel B* · Conscientiousness skills**			
Reach goals			
Unadjusted p-value	0.027	0.048	0.059
Sharpened q-value	0.371	0.916	1.000
Focus on goals			
Unadjusted p-value	0.096	0.082	0.081
Sharpened q-value	1.000	1.000	1.000
Responsible			
Unadjusted p-value	0.006	0.008	0.009
Sharpened q-value	0.187	0.268	0.219
Fulfill duty			
Unadjusted p-value	0.010	0.013	0.011
Sharpened q-value	0.187	0.268	0.219

Note: Each cell contains p- or q-values for the multiple regressions presented in Table 3.1 to 3.3. In Panel A, we correct for testing three hypotheses (three outcome variables and one treatment), while in Panel B we correct for 32 hypotheses (all conscientiousness items). The sharpened q-values are calculated using the Stata code from Anderson (2008).

Table 3.7: Occupations at baseline of workers still employed in the construction company vs workers not employed in the company at end-line

	(1)		(2)		T-test
	Not in company		Still in company		Difference
Variable	N/n	Mean/SE	N/n	Mean/SE	(1)-(2)
Security agent helper	181	0.304	187	0.385	-0.081
	[55]	(0.034)	[72]	(0.036)	
Mason	181	0.127	187	0.176	-0.049
	[23]	(0.025)	[33]	(0.028)	
Iron worker	181	0.105	187	0.053	0.051*
	[19]	(0.023)	[10]	(0.016)	
Form setter	181	0.149	187	0.118	0.032
	[27]	(0.027)	[22]	(0.024)	
Carpenter	181	0.017	187	0.000	0.017*
	[3]	(0.010)	[0]	(0.000)	
Laborer	181	0.271	187	0.251	0.019
	[49]	(0.033)	[47]	(0.032)	
Driver help	181	0.006	187	0.000	0.006
	[1]	(0.006)	[0]	(0.000)	
Topographer/Topographer helper	181	0.006	187	0.005	0.000
	[1]	(0.006)	[1]	(0.005)	
Flag holder	181	0.000	187	0.005	-0.005
	[0]	(0.000)	[1]	(0.005)	
Specialized laborer/worker	181	0.017	187	0.000	0.017*
	[3]	(0.010)	[0]	(0.000)	
Unspecified worker	181	0.000	187	0.005	-0.005
	[0]	(0.000)	[1]	(0.005)	

Note: This table compares the occupations at baseline of those, at end-line, that remained in the construction company with those that did not. In square brackets, the number of individuals per occupation either still in the construction company or not.

Table 3.8: Activity sector for workers who left the construction company

	Freq.	Pct.
Construction	53	64.63
Wood and metal work, carpentry, craftmanship	12	14.63
Dealer/Salesman/Retail sales	8	9.76
Electrician	1	1.22
Driver	1	1.22
Cleaner	1	1.22
Security/Guardian/Soldier	1	1.22
Agriculture, farming, fisherman	3	3.66
Daily worker	1	1.22
Tyre mechanic*	1	1.22
Total	82	100.00

Note: Sector measured at end-line. Out of the 181 respondents not working in the company at end-line, 82 had other jobs, 97 were unemployed and 2 had left the labour market. * The French expression was *vulgarisateur*.

Table 3.9: Earnings at end-line with interaction

	(1) Last earnings	(2) Last earnings	(3) Last earnings
Treated	24408.6***	15759.7**	23697.7**
	(0.001)	(0.018)	(0.013)
Still at company		73271.0***	80942.9***
		(0.000)	(0.000)
Treated × Still at company			-15480.5
			(0.244)
Constant	21716.9	-1626.4	-5341.4
	(0.304)	(0.930)	(0.776)
Standardized coeff.	0.313	0.202	0.304
Mean outcome control gr.	112644.92	112644.92	112644.92
Baseline controls	✓	✓	✓
Baseline outcome	✓	✓	✓
N	370	367	367
R-sq	0.169	0.378	0.378

Note: Columns 1 and 2 are columns 3 and 4 from Table 2 and added to help comparison with main results. p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household,total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: earnings measured at baseline.

Appendix 3.C Details on earnings computation

Earnings were measured in different ways in the three survey rounds. At baseline, all workers were asked both their last wage and, as a consistency check, the range where their last wage fell. At mid-line, due to survey constraints, we could only ask whether their wage had risen or fallen compared to baseline, and by how much it rose or fell. At end-line, workers still employed at the construction company were again asked whether their wage varied and by how much, while workers employed elsewhere were asked the amount they earned and the range where that amount fell, just as in baseline.

From this, we computed interpolated wages at end-line for workers still at the construction company. First, we checked that the exact wage had been reported at baseline: if that was missing, we used the range to infer the exact wage, using "midpoint estimates", i.e., the median point in each range category. For example, if a worker did not report the exact wage, but indicated that her wage was in the third category, which corresponds to a wage between 130,000 and 180,000 CFAF, then we infer that her exact wage was $\frac{180000-130000}{2} = 155,000$.

Next, using the answers to the items "did your wage rise or fall since baseline?" and "by how much did it rise or fall?" we interpolated mid-line wages. The amount by which wages rose or fell was also given as a range, so we also applied the midpoint estimates to infer an exact amount for the increase or decrease. For instance, if baseline wage was 145,000 CFAF and the worker reported an increase between 5,001 and 10,000 CFAF, the interpolated wage for mid-line would be $145000 + \frac{10000-5001}{2} = 147,499.5$. We applied the same procedure for end-line wages of workers still working in the construction company. For workers who had left the construction company, we proceeded in the same fashion as in baseline: we used the exact wage, and if missing we inferred the amount using the midpoint of the range category.

Appendix 3.D Details on conscientiousness measures

3.D.1 Description of conscientiousness questions

At baseline and midline, we measure conscientiousness using a 32-item questionnaire. Four conscientiousness scales are defined, and each item measures a specific one of them. These four scales are responsibility, industriousness, organizational and punctuality. Table 3.10 describes each item and its associated scale. Each respondent was asked in face-to-face interviews to answer the questions by using a five-point scale: (1) very difficult, (2) fairly difficult, (3) neither easy nor difficult, (4) fairly easy, and (5) very easy.

Table 3.10: Conscientiousness traits: list of all items

Conscientiousness trait	How easy or difficult is it to...
Organizational	Keep things tidy and in order
	Follow a schedule
	Use a method to follow-up
	Organize schedule
	Clean up after yourself
	To keep personal spaces organized
	Keep things well organized
	Keep things in order
Industriousness	Make plans
	Work to reach your goals
	Fix clear goals
	Set high standards
	Change ways of working towards a goal after a setback
	Work hard to succeed
	Focus on most important goals
	Make plans to reach a goal
	Keep trying after failing
Responsibility	Be there when others need me
	That other people depend on me
	Keep promises
	Fulfill my promises and engagements
	Live up to responsibilities
	Respect engagements
	That other people are counting on me
	Fulfill my duties and obligations
	Manage responsibilities
Punctuality	Show remorse if late at a meeting
	Go to meeting early
	Avoid being late to a meeting or work
	Avoid procrastination
	Be punctual
	Be late to meetings

3.D.2 Reliability of scales

Table 3.11: Conscientiousness traits: reliability of scales

Scale	Round	Cronbach's alpha	Interitem covariance	Number of items
All traits	Baseline	0.793	0.106	32
	Midline	0.785	0.073	32
Organisational	Baseline	0.588	0.091	8
	Midline	0.495	0.034	8
Industriousness	Baseline	0.615	0.21	9
	Midline	0.684	0.219	9
Responsibility	Baseline	0.707	0.155	9
	Midline	0.58	0.079	9
Punctuality	Baseline	0.428	0.144	6
	Midline	0.343	0.065	6

Note: Each of the 32 conscientiousness question relates to one of the four conscientiousness traits. This table reports the reliability of each of these traits, as measured by Cronbach's α and interitem covariance, as well as the overall reliability.

3.D.3 Impact of treatment on conscientiousness skills

Table 3.12: Conscientiousness skills: alternative specifications

	(1) Reach goals	(2) Focus on goals	(3) Responsible	(4) Fulfill duty
Treated	0.316**	0.152*	0.317***	0.165***
	(0.027)	(0.096)	(0.006)	(0.010)
Constant	3.811***	4.509***	4.178***	4.669***
	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.238	0.180	0.299	0.274
N	343	343	343	343
R-sq	0.0143	0.0081	0.0224	0.0194

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; Specification excludes all baseline controls.

Table 3.13: Conscientiousness traits: Organisational skill I

	(1) Tidy	(2) Follow schedule	(3) Follow-up	(4) Organize schedule
Treated	0.00316	-0.0131	0.0732	-0.00418
	(0.941)	(0.739)	(0.498)	(0.938)
	[1.000]	[1.000]	[1.000]	[1.000]
Constant	4.734***	4.686***	3.874***	4.299***
	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.00811	-0.0366	0.0730	-0.00844
Mean outcome control gr.	4.58	4.77	4.18	4.5
Baseline controls	✓	✓	✓	✓
Trait at baseline	✓	✓	✓	✓
N	343	343	343	343
R-sq	0.0307	0.0153	0.0578	0.0486

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.14: Conscientiousness traits: Organisational skill II

	(1) Clean up	(2) Organized personal space	(3) Keep organized	(4) Keep tidy
Treated	0.0393	-0.0217	0.00960	-0.0189
	(0.582)	(0.620)	(0.845)	(0.634)
	[1.000]	[1.000]	[1.000]	[1.000]
Constant	4.146***	4.660***	4.737***	4.740***
	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.0596	-0.0547	0.0213	-0.0525
Mean outcome control gr.	4.72	4.77	4.84	4.8
Baseline controls	✓	✓	✓	✓
Trait at baseline	✓	✓	✓	✓
N	343	343	343	343
R-sq	0.0570	0.0100	0.0461	0.0132

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.15: Conscientiousness traits: Industriousness skill I

	(1) Make plans	(2) Work goals	(3) Clear goals	(4) High standards	(5) Change after setback
Treated	-0.149	0.268*	0.104	-0.0210	-0.0376
	(0.221)	(0.059)	(0.285)	(0.822)	(0.777)
	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Constant	4.312***	3.941***	3.557***	4.478***	4.161***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	-0.131	0.202	0.117	-0.0248	-0.0308
Mean outcome control gr.	3.96	3.47	4.26	4.22	3.93
Baseline controls	✓	✓	✓	✓	✓
Trait at baseline	✓	✓	✓	✓	✓
N	343	343	343	343	343
R-sq	0.0781	0.0738	0.0404	0.0153	0.0398

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.16: Conscientiousness traits: Industriousness skill II

	(1) Work hard	(2) Focus on goals	(3) Plan to reach goal	(4) Keep trying after failure
Treated	0.115	0.162*	0.0491	-0.155*
	(0.391)	(0.081)	(0.673)	(0.087)
	[1.000]	[1.000]	[1.000]	[1.000]
Constant	3.632***	4.666***	3.818***	4.213***
	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.0931	0.192	0.0461	-0.187
Mean outcome control gr.	4.15	4.33	3.75	4.37
Baseline controls	✓	✓	✓	✓
Trait at baseline	✓	✓	✓	✓
N	343	343	343	343
R-sq	0.0448	0.0278	0.0463	0.0365

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.17: Conscientiousness traits: Responsibility skill I

	(1) Present for others	(2) Dependable	(3) Keep promises	(4) Fullfill engagements	(5) Up to responsabilities
Treated	-0.0104	-0.0901	-0.0139	-0.0181	0.298***
	(0.935)	(0.396)	(0.743)	(0.694)	(0.009)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.219]
Constant	3.362***	3.162***	4.348***	4.781***	4.030***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	-0.00887	-0.0900	-0.0355	-0.0431	0.281
Mean outcome control gr.	3.99	4.27	4.83	4.77	4.15
Baseline controls	✓	✓	✓	✓	✓
Trait at baseline	✓	✓	✓	✓	✓
N	343	343	343	343	343
R-sq	0.0532	0.0868	0.0514	0.0207	0.0844

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.18: Conscientiousness traits: Responsibility skill II

	(1) Respect engagements	(2) Others count on me	(3) Fullfill duty	(4) Manage responsibilities
Treated	0.0393	-0.0487	0.162**	0.0334
	(0.361)	(0.592)	(0.011)	(0.578)
	[1.000]	[1.000]	[0.219]	[1.000]
Constant	4.811***	4.034***	4.566***	4.401***
	(0.000)	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.0994	-0.0580	0.274	0.0609
Mean outcome control gr.	4.75	4.3	4.53	4.61
Baseline controls	✓	✓	✓	✓
Trait at baseline	✓	✓	✓	✓
N	343	343	343	343
R-sq	0.0528	0.0511	0.0629	0.0296

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.19: Conscientiousness traits: Punctuality skill I

	(1) Remorse late	(2) Meet earlier	(3) Avoid being late
Treated	-0.000120	0.0373	-0.0164
	(0.999)	(0.540)	(0.852)
	[1.000]	[1.000]	[1.000]
Constant	4.457***	4.550***	4.661***
	(0.000)	(0.000)	(0.000)
Standardized coeff.	-0.000137	0.0670	-0.0204
Mean outcome control gr.	4.16	4.66	4.41
Baseline controls	✓	✓	✓
Trait at baseline	✓	✓	✓
N	343	343	343
R-sq	0.0410	0.0314	0.0315

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Table 3.20: Conscientiousness traits: Punctuality skill II

	(1) Avoid procrastination	(2) Punctual	(3) On time at meetings
Treated	0.0148	0.0135	0.154
	(0.919)	(0.778)	(0.192)
	[1.000]	[1.000]	[1.000]
Constant	4.103***	3.832***	3.914***
	(0.000)	(0.000)	(0.000)
Standardized coeff.	0.0111	0.0299	0.140
Mean outcome control gr.	3.85	4.79	4.25
Baseline controls	✓	✓	✓
Trait at baseline	✓	✓	✓
N	343	343	343
R-sq	0.0243	0.0890	0.0614

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Mean outcome control gr.*: mean level of the outcome variable for the control group at baseline. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income. *Baseline outcome*: trait measured at baseline.

Appendix 3.E COVID-19 related questions

Table 3.21: Impact of treatment on COVID-19 outcomes: bivariate regressions

Panel A	(1) Protective measures	(2) Effective measures	(3) Display symptoms	(4) Contract change	(5) Lost income	(6) Income decrease	(7) HH income decrease
Treated	-0.404***	-0.414***	0.0341	0.0224	-0.0694	-0.0443	-0.00855
	(0.003)	(0.002)	(0.509)	(0.678)	(0.160)	(0.374)	(0.856)
	[0.020]	[0.020]	[1.000]	[1.000]	[0.787]	[1.000]	[1.000]
Constant	3.809***	3.749***	0.412***	0.520***	0.367***	0.667***	0.721***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Std. coeff.	-0.310	-0.325	0.0689	0.0448	-0.147	-0.0925	-0.0189
N	371	371	368	346	365	371	371
R-sq	0.0241	0.0265	0.00119	0.000503	0.00543	0.00214	0.0000900

Panel B	(8) Transfers received decrease	(9) Food expend. increase	(10) Health expend. increase	(11) Educ expend. increase	(12) Transfers sent increase	(13) Savings decrease	(14) Borrowing increase
Treated	0.000756	-0.0343	-0.00924	-0.0391*	0.0249	0.0330	0.0155
	(0.988)	(0.459)	(0.800)	(0.088)	(0.374)	(0.521)	(0.723)
	[1.000]	[1.000]	[1.000]	[0.542]	[1.000]	[1.000]	[1.000]
Constant	0.372***	0.290***	0.148***	0.0710***	0.0656***	0.557***	0.219***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Std. coeff.	0.00156	-0.0770	-0.0264	-0.177	0.0925	0.0667	0.0369
N	371	371	371	371	371	371	371
R-sq	0.000000611	0.00148	0.000174	0.00787	0.00214	0.00112	0.000341

Note: *p*-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened *q*-values in square brackets.

Table 3.22: Impact of treatment on COVID-19 outcomes: full-controls regressions

Panel A	(1) Protective measures	(2) Effective measures	(3) Display symptoms	(4) Contract change	(5) Lost income	(6) Income decrease	(7) HH income decrease
Treated	-0.412***	-0.425***	0.0278	0.0217	-0.0916*	-0.0500	-0.0137
	(0.002)	(0.001)	(0.594)	(0.693)	(0.056)	(0.314)	(0.771)
	[0.017]	[0.017]	[1.000]	[1.000]	[0.287]	[1.000]	[1.000]
Constant	3.882***	3.831***	0.550***	0.715***	0.441***	0.897***	0.871***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Std. coeff.	-0.316	-0.335	0.0562	0.0434	-0.194	-0.104	-0.0303
Baseline controls	✓	✓	✓	✓	✓	✓	✓
N	371	371	368	346	365	371	371
R-sq	0.0589	0.0615	0.0304	0.0111	0.111	0.0521	0.0417

Panel B	(8) Transfers received decrease	(9) Food expend. increase	(10) Health expend. increase	(11) Educ expend. increase	(12) Transfers sent increase	(13) Savings decrease	(14) Borrowing increase
Treated	0.00368	-0.0462	-0.0188	-0.0361	0.0227	0.0241	0.0131
	(0.942)	(0.322)	(0.600)	(0.117)	(0.424)	(0.640)	(0.764)
	[1.000]	[1.000]	[1.000]	[0.475]	[1.000]	[1.000]	[1.000]
Constant	0.292**	0.524***	0.492***	0.120*	0.112	0.705***	0.258**
	(0.036)	(0.000)	(0.000)	(0.058)	(0.151)	(0.000)	(0.032)
Std. coeff.	0.00760	-0.104	-0.0538	-0.164	0.0845	0.0486	0.0312
Baseline controls	✓	✓	✓	✓	✓	✓	✓
N	371	371	371	371	371	371	371
R-sq	0.0412	0.0315	0.0695	0.0384	0.0128	0.0445	0.0493

Note: p-values in parentheses, *, **, *** denote significance at 10%, 5% and 1% levels; sharpened q-values in square brackets. *Baseline controls*: sex, age, completed middle school, number of beds in household, number of children in household, number of adults in household, total (formal and informal) household debt, total household savings, household income.

Conclusion

This thesis presents three chapters exploring the topics of urban transportation, environmental impact and the labour market characteristics of urban low-skilled workers. In this concluding chapter, I summarise their findings and highlight the policy implications they suggest.

This first chapter tested whether the implementation and expansion of New York City's bike-share program improved ground measures of air quality. Using a staggered difference-in-difference strategy, I found that the concentration of air pollutants associated with road traffic decreased in areas where we would expect fewer cars to be driven due to bike-share. I estimate that the reduction in the concentration of nitric oxide saved up to $320 million in social damages. Finally, I show that short taxi trips (those most likely to be substituted by bike-share) reduced faster in areas where bike-share was implemented, suggesting that bike-share substituted some taxi trips, providing a plausible channel for the observed improvement in air quality.

These results have several policy implications. First, they show that bike-share may be an effective way to improve air quality. Cities are burdened by excessive concentrations of air pollutants which have a wide array of negative consequences on both human health and the economy. By showing that the introduction and expansion of a bike-share system can lead to improved air quality, this study provides policymakers with a quantitative measure of the magnitude of the change and the economic benefits it may deliver. Importantly, by empirically showing a change in car traffic through the analysis of taxi trips, this chapter showed that there can be substitution between motorised vehicles and cycling. Prior to this study, it remained unclear if bike-share could lead to substitution away from motorised traffic. With this finding comes an important provision, however: the substitution away from motorised vehicles is inherently dependent on how these vehicles are used. In New York City, taxis are a popular transport mode for short-distance trips, and bike-share has a clear competitive advantage in this type of travelling when traffic is very dense. Bicycles are able to use cycling paths and roll past stuck cars in traffic jams, making bike-share more attractive than taxis in these situations. However, taxis are not always such a prominent mode of urban transport. In cities where a high share of commuters already use public transport, switching to bike-share will not have the same potential impact on air quality.[25] The results of this chapter thus also highlight that the impact of cycling policies will depend greatly on what commuters are substituting away from when they switch to cycling, and represents a fascinating area of study for future research (see for example [leroutier2023]).

The second chapter examines whether bike-share stations are valued in the real-estate market. Using the universe of transactions in New York City, it shows that residential properties within 150 metres of a bike-share station were 6.1% more expensive than their counterpart between 150 and 500 metres away from bike-share stations. While the evidence this study presents is not clear-cut, the positive impact on sale prices is in line with the results of other studies on bike-share and rents, and the literature on transport infrastructure in general. Assuming bike-share does have an impact on sale prices has implications for urban policy-makers. First, the sale-price premium around bike-share stations demonstrates that urban dwellers value the service and are willing to pay a higher price for living near a station. Second, it shows that the bike-share system creates value that may be captured by property taxes. This is important for policymakers who have to justify the cost of investing in those new systems. This chapter takes a step in that direction by estimating the value created by the bike-share program.

In the third chapter, we run a randomised controlled trial on low-skilled workers employed on the construction site of a new railway in Dakar, Senegal. We randomly assign workers to attend a training session designed to affect conscientiousness-related skills. We find that the workers who received the training and the following short phone reminders over eight weeks are more likely to be retained in the construction, and received on average higher wages. By showing that non-cognitive skills may be changed in adulthood and that their improvement leads to improve labour market outcomes, we provide novel evidence that can be used by policymakers and other stakeholders to

[25] Provided that public transport runs on clean energy.

design better, more efficient training programs. The training intervention we implemented was relatively low-cost compared to other, skill-based training, representing a potentially more efficient use of resources by constrained governments or non-governmental organisations willing to improve the labour market outcomes of low-skilled workers. Second, we also highlight the challenges of using psychometric measures with low-skilled workers in developing countries. Understanding the psychological features of these populations is key to designing effective policies: researchers and authorities should dedicate steer resources towards improving our knowledge in that domain.

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