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RAFAEL ALFENA ZAGO
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BY THE COMMITTEE CONSISTING OF

Dr. Joan Hamory

Dr. Gregory Burge

Dr. Daniel Hicks

Dr. Chunbei Wang

Dr. Fabio Costa Morais de Sá e Silva

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Abstract

This work brings three studies that evaluate public policies in Brazil. First, I study a recent infrastructure improvement program in São Paulo City, to estimate the causal impact of improved public lighting on nighttime crime. Secondly, I estimate the impact of immigrant influxes on local labor markets, taking advantage of a natural experiment generated by a massive wave of forced migration of Venezuelans to Roraima, a state in northern Brazil, from 2016 onward. Finally, I provide arguments to answer how new technology-based companies such as Uber impact traffic-related outcomes such as traffic deaths and accidents with fatalities.

Chapter 1

Let There Be Light! Improved Public Lighting and Nighttime Crime: Evidence from Brazil

This paper studies a recent infrastructure improvement program in São Paulo City, Brazil, to estimate the causal impact of improved public lighting on nighttime crime. Exploiting the phased roll-out of the program - as well as its unexpected abandonment - to alleviate selection bias, I find that lighting improvements reduce nighttime crime and "non-homicide" crimes, a measure that includes property and violent crimes, both in the districts that received lights and in neighboring districts. Yet, there is some evidence that daytime crime rises as a result of the improved lighting, possibly due to an increase in total economic activity in the areas that received the lamps. Overall, it appears that better public lighting is an important catalyst for reducing crime in dense urban areas in middle-income countries.

Keywords: Public Lighting, Nighttime crime, Brazil, Urban Areas.

1.1 Introduction

The existing literature on crime prevention is primarily based on the "rational choice" perspective, despite the shortcomings of this approach.[1] This theory holds that a potential criminal will choose to commit a crime if the expected utility from committing the offense is higher than the expected utility from using his/her time and resources for another activity (Becker, 1968). Because of this, research on methods to deter crime usually focuses on factors that can function either through decreasing the benefits of or increasing the costs of crime

[1] This approach has been criticized for its failings in measuring the non-pecuniary benefits of crime, for assuming equivalent costs and benefits for different types of crimes, and for not taking into account the unpredictable events that drive some crime, among other things.

(Fajnzylber, Lederman, and Loayza, 2002). Not in total opposition to this, the "situational crime prevention" theory focuses on the settings for crime, and advocates for changes in the physical environment in which crimes occur to reduce the opportunity for them to happen (Clarke, 1997; Mayhew and Clarke, 1980; Heal and Laycock, 1986; Atkins, Husain, and Storey, 1991).[2]

Viewed through the lens of either of these theories, the *ex ante* impact of improved public infrastructure such as street lights on crime is ambiguous. On the one hand, enhanced visibility at night could deter crime through increasing the expected cost of committing it (for instance, by increasing the probability that the criminal is caught). Moreover, the public investment could act as a boon to social cohesion, community pride and positive image of the area, also negatively affecting criminal activity there Taub, Taylor, and Dunham, 1984; Fowler and Mangione, 1986; Lavrakas and Kushmuk, 1986; Taylor and Gottfredson, 1986; Wilson and Kelling, 1982; Painter and Farrington, 1999.[3]

However, it could be instead that improvements in lighting help potential offenders to successfully commit crimes, either during the day or at night. For nighttime crime, an increase in the number of people leaving their homes at night could increase the number of empty homes available for burglary and the number of potential victims on the streets (Painter and Farrington, 1997). Increased visibility could allow the criminal better judgment of the valuables carried by and defenses available to potential victims, as well as better perception of the proximity of potential witnesses. Furthermore, if newly illuminated areas attract criminal activities at nighttime, it could be the case that these areas also attract more attention for such activities during daylight hours.

Although the literature in economics on the relationship between public lighting and crime is not new, there is a paucity of studies in the last two decades, particularly examining areas outside the United States and the United Kingdom.[4,5] In the U.S., findings of studies focused on public lighting improvements across numerous cities are mixed, likely

[2]This theory differentiates itself from classical theories of modern criminology in the sense that the latter focus either on explaining criminal behaviour and how certain groups of society would be more inclined to commit crimes (Clarke, 1997; Gottfredson and Hirschi, 1990), or on how to deal with the criminal (Clarke, 1997; Wilkins, 1990). For more information on the origins of the "situational crime prevention" theory, see Jeffery, 1977 and Newman, 1972.

[3]Note that this mechanism would predict reductions not only in nighttime crime, but also in daytime crime. I return to this shortly.

[4]Studies using U.S. data primarily focus on relighting programs undertaken in response to a wave of crime that took place around the country in the 1960s (see below). The U.K. endured a power crisis in the 1970s, which lead authorities to reduce public lighting there by 50% over a two-year period as part of an emergency plan. During this time, several areas reported increases in crime (Painter, 1996), and several studies using U.K. data seek to understand the relationship between subsequent relighting and crime (see below).

[5]Several studies in the economics literature on crime have attempted to demonstrate the effectiveness of other public policies designed to combat crime, including studies of bike patrols in commuter lots in Vancouver (Barclay et al., 1996), police presence in municipalities in the U.S. (Levitt, 2002; McCrary, 2002) and in Buenos Aires, Argentina (Di Tella and Schargrodsky, 2004), and dry laws in São Paulo, Brazil (Biderman, De Mello, and Schneider, 2009).

owing to impediments to causal identification (Farrington and Welsh, 2002).[6] In the U.K., research evaluating relighting programs that were implemented across London during the 1970s, 1980s, and 1990s generally finds reductions in crime, as well as reductions in the fear of crime and disorder, and increases in the usage of public streets (Painter, 1994), though studies examining such investments in other parts of the U.K. have mixed results (Poyner and Webb, 1991, Shaftoe, 1994, Poyner and Webb, 1997, Herbert and Moore, 1991, Davidson and Goodey, 1991, Burden and Murphy, 1991, and Ditton et al., 1992).

Yet, nearly all of these studies in the U.S. and the U.K. utilize simple before and after comparisons, or linear regressions with controls and interactions, to study the relationship between public lighting and crime.[7] The use of this approach does not allow one to distinguish the effects of the relighting programs from other policies, programs, and events that may also affect crime. In addition, this approach can mask the impact of time trends on criminal activity, as well as possible variations in crime over different periods (Painter and Farrington, 1997; Campbell and Cook, 1979).

Recent work attempts to better achieve causal identification. A randomized experiment in New York City found reductions in outdoor nighttime crime of 36 percent, and in overall indices of crime of 4 percent, in communities that received temporary streetlights (Chalfin et al., 2019). In a rare example of research outside of a high-income country, Arvate et al., 2018 use an instrumental variables strategy to evaluate the effect of a rural electrification program (*Luz Para Todos* - "Light for All") on violent crime in northeastern Brazil. The results indicate a decrease of 92 cases of outdoor homicides per 100,000 inhabitants as a result of the policy.

The present study seeks to shed further light on this issue (pun intended!), by exploiting unusual features of a recent public lighting improvement program in São Paulo City, Brazil to achieve causal identification. Using a differences-in-differences approach that takes into account both the phased roll-out and early abandonment of the program, I find that improved public lighting decreases nighttime cell phone thefts and robberies, and homicides, with cell phone robberies being reduced by 26% ($p<0.05$) and homicides by 29% ($p<0.05$). Total crime and "non-homicide crimes" at nighttime each fall by approximately 11% monthly ($p<0.05$ in both cases). Furthermore, I find positive spillovers of the lighting program on neighboring districts (particularly for cell phone thefts and homicides). There is some evidence that total and daytime crimes increase, possibly due to increased overall economic activity or other policy changes going on at the same time as the infrastructure improvement program.

The rest of the paper is organized as follows: Section 2 discusses the context in which

[6] Cities studied include Kansas City, Missouri (Wright et al., 1974), Atlanta, Georgia (Atlanta Traffic Engineering Dept and United States of America, 1974), Milwaukee, Wisconsin (Department of Intergovernmental Fiscal Liaison, 1973; Department of Intergovernmental Fiscal Liaison, 1974), Portland, Oregon (Inskeep and Goff, 1974), Harrisburg, Pennsylvania (Harrisburg Police Department, 1976), New Orleans, Louisiana (Sternhell, 1977), Fort Worth, Texas (Lewis, Sullivan, et al., 1979) and Indianapolis, Indiana (Quinet and Nunn, 1998).

[7] See for instance Atkins, Husain, and Storey, 1991; Painter and Farrington, 1999; Painter and Farrington, 1997.

the lighting improvement program was developed, and how it was rolled out. Section 3 describes the data and the estimation strategy. Section 4 presents results of the analysis and a set of robustness checks. Section 5 discusses the mechanisms driving the results, and Section 6 concludes.

1.2 The Programa LED nos Bairros

In 2014, the mayor of São Paulo, Fernando Haddad, proposed an infrastructure improvement plan that aimed, if implemented, to change the paradigm of the city. One central idea was to revitalize public lighting by replacing every street lamp in the city with an LED (Light-Emitting Diode) bulb and by expanding the overall lighting net. Among other advantages, LED technology is more efficient (and thus lower cost), can better control light distribution, and is less likely to be damaged by electrical disturbances (Philips and AES Serviços TC, 2013).

One of the requirements imposed by city administrators prior to implementation of this program was that the replacement of bulbs should begin in certain "priority zones", defined as regions with higher social and economic vulnerabilities and higher population densities. The public authorities believed that the new lighting system would decrease crime rates, especially for crimes facilitated by darkness. It was believed that because citizens living in these areas were more likely to move around either on foot or using public transportation, they would benefit greatly from better lighted sidewalks (ILUME, 2015). The program thus would begin in places that had high crime rates, high social vulnerability and high demand for public lighting (Chiovetti, 2018).

Following this requirement, the first districts to receive the new LED bulbs were chosen based on an index entitled the *Índice Paulista de Vulneravilidade Social* (IPVS). Created by the *Fundação Sistema Estadual de Análise de Dados* (SEADE) in 2010, the index included socioeconomic and demographic characteristics of districts.[8]

The mayor announced the launch of public bidding to undertake this program in May of 2015 (Santiago, April 22, 2015). However, due to fraud suspicions, operations for a full-scale program were suspended (and remain so). Instead, a pilot program known as "Programa LED nos Bairros" (PLNB) was launched in December of 2015 with the installation of LED lamps in 2 districts. Following the successful completion of installation in these districts, 7 more districts received the new lighting technology between December 2015 and June 2016[9]. A second phase of the program, launched in June 2016, aimed to include 11 more districts,

[8]Elements of the index include: average income per capita, average income of female-headed households, percentage of households with income at or below 1/2 the minimum wage, percentage of households with a literate household head, percentage of household heads aged 10-29, percentage of female-headed households where the head is aged 10-29, average age of household head, and percentage of kids aged 0-5.

[9]The first phase included *Heliópolis* (1,300 bulbs replaced) in the district of Sacomã, *Jardim Monte Azul* (534) in Jardim São Luis, Brasilândia (9,400), Lajeado (6,800), Sapopemba (11,300), Raposo Tavares (5,340), Jardim Ângela (11,200), Jardim Helena (5,900) and Pedreira (6,600) (ILUME, January, 2016).

but ended up completing only 5 more due to insufficient budget[10]. Figure 3.3 displays the districts where lights were installed ("treated"), the districts that were chosen for inclusion in the program but where installation was never actually conducted ("would be treated"), and districts that were not included in the program ("non-treated").

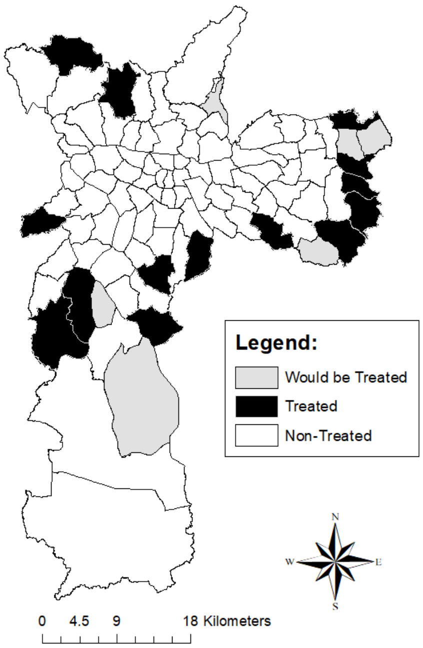

Figure 1.1: Treatment Status by District for Programa LED nos Bairros, 2015-2016

Although all of the districts that received the new LED lamps are on the outskirts of the city, almost every region in the city had at least one district considered for the program, except for the center[11]. In the analysis that follows, the 14 districts that received the program will be considered the analysis "treatment" group. Although all other districts in São Paulo will be considered the control group in some estimations, the main analysis will exploit the 6 districts that were announced to be in the program but did not ultimately receive lights as a better defined control group.

[10]The second phase aimed to include Cidade Tiradentes (6,200), Guaianases (5,300), part of Jabaquara (2,500), Iguatemi (7,200), part of Grajaú (3,300), Perus (5,500), São Rafael (8,000), Socorro (6,000), Jaçanã (5,300), Itaim Paulista (9,800) and Vila Curuçá (7,800) (Secretaria Especial de Comunicação, May 3, 2016), but ended up delivering to only Cidade Tiradentes, Guaianases, Perus, Iguatemi and Jabaquara (partially) (Chiovetti, 2018; ILUME, January, 2016)

[11]This is, among other factors, because some of the streets in the center of the city already had LED lamps.

1.3 Data and Estimation Strategy

1.3.1 Data

Several sources of data were used in the following analysis. These sources can be divided into two categories: a) data related to the lighting improvement program and characteristics of the districts; and b) data related to crimes.

Data on District Characteristics and the PLNB Program

Information related to the PLNB is available on the São Paulo city hall website, including the list of treated districts and the lamp installation schedule (for the first phase of the program). Reports, news and social media posts were used to confirm this list, as well as to generate the installation schedule for the second phase of the program.[12] The city hall website also includes information on studies that were conducted before the release of the Public-Private Partnership, and details of the lamps that would be installed in each district. Further information on program development was obtained through personal interviews with program authorities and consultation of a book launched by the Services Secretary in 2018 that presents some details on the program (Chiovetti, 2018). Zonal-level IPVS data was extracted from the *SEADE*'s website, and combined with files publicly provided by the city hall website (GeoSampa, 2018) in order to generate district-level data.

Table 3.1 presents summary statistics for selected characteristics of the districts of São Paulo in 2014, one year before the start of the PLNB program. Statistics are presented first for the full set of districts in the city (column 1), and then separately for those that received lamps during late 2015 and 2016 ("treated districts", column 2), those that did not receive lamps through the PLNB ("control districts", column 3), and districts that were selected to receive lamps but did not due to unexpected program abandonment ("would be treated districts", column 4, which is a subset of the districts in column 3). Columns 5 and 6 test for statistically significant differences between columns 2 and 3, and columns 2 and 4, respectively.

From this table, one can see that prior to the launch of the lightning improvement program, the districts that received lamps were more populous and had higher population density, on average, than those that did not receive lamps. Treated districts also had lower high school enrollment, and lower formal employment rates, although penetration of social programs was fairly similar across the two groups. Revenue from taxes on services[13] was significantly lower for the districts that received lamps, representing approximately 32.2% of the total revenues from the same source in the non-treated group.

[12] I define months that each district began to receive the LED lamps as those that are reported by at least two news articles or media posts.

[13] This tax represents 5% of service provided by companies and self-employed persons, and thus is a good proxy variable for economic activity at the district level.

Table 1.1: District Characteristics, 2014

	(1) All Districts	(2) Treatment	(3) Control	(4) Would Be Treated	(5) (2)-(3)	(6) (2)-(4)
Total population	119,936	195,377	107,056	172,300	88,321***	23,077*
	(71,685)	(75,280)	(62,574)	(107,499)	(-16)	(-1.9)
Fraction male	.47	.48	.47	.48	.0091***	.0016*
	(.012)	(.006)	(.013)	(.0082)	(-9.1)	(-1.7)
Area (in km^2)	16	17	16	25	1.7	-7.3***
	(27)	(7.6)	(29)	(31)	(-.74)	(2.9)
Population/km^2	.011	.012	.011	.011	.0016***	.0016**
	(.0052)	(.0049)	(.0052)	(.0058)	(-3.7)	(-2.2)
Total number of lamps (1)	6,224	7,696	5,973	8,185	1,723***	-489
	(2,513)	(2,342)	(2,456)	(3,950)	(-8.5)	(1.2)
Lamps/km^2	618	494	639	531	-144***	-37
	(323)	(171)	(337)	(191)	(5.4)	(1.5)
High school enrollment rate (2)	.7	.51	.73	.63	-.22***	-.12***
	(.35)	(.083)	(.37)	(.24)	(7.7)	(5.8)
Spots in social programs	359	378	356	351	23	27
	(322)	(445)	(297)	(382)	(-.84)	(-.46)
Formal employment rate	.66	.099	.75	.22	-.65***	-.13***
	(.94)	(.077)	(.99)	(.32)	(8.6)	(4.8)
Taxes on services (3)	8,065,781	2,885,665	8,961,110	4,034,694	-6,075,445***	-1,149,029
	(14,783,408)	(5,888,212)	(15,651,003)	(8,032,951)	(5)	(1.2)
IPVS value (4)	2.5	3.6	2.4	3.4	1.2***	.18***
	(.89)	(.52)	(.81)	(.74)	(41)	(3.7)
Nº of Districts	96	14	82	6	96	20

Notes: Columns (1) through (4) depict the means and standard deviations (in parentheses) for selected district characteristics. Column (4) contains a subset of districts in column (3), namely those districts who were chosen for program inclusion but did not receive any lamps due to early abandonment of the program. Columns (5) and (6) show mean differences, respectively, between columns (2) and (3), and columns (2) and (4), with standard errors in parentheses. Superscripts *, ** and *** represent statistical significance at p<0.1, p<0.05, and p<0.01, respectively.

(1) Total number of lamps and, consequently, Lamps/km^2 use data from the year 2016, the only source of data available.

(2) The high school enrolment rate is calculated by dividing the number of students enrolled in high school by the population of individuals aged 15-19.

(3) Data on taxes on services are available only for 2014 and 2105 and the results are calculated with values in 2014 as the base year. In addition, this measure is missing for 1 district in columns 1 and 3.

(4) Values of IPVS are fixed and derived from 2010.

Although it could just mean that there are more commercial places in non-treated areas, it mostly reflects the types of economic activity in each area, with more developed activities being located in non-treated districts, suggesting economic vulnerabilities in the treated districts. Note that although the total number of lamps is higher in treated districts, the number of lamps per km^2 is lower in treated areas than in non-treated areas, suggesting a need for the PLNB in those selected districts. Finally, as expected, the IPVS is 1.2 point (50%) higher for the districts that received the new LED lamps.[14]

[14] The index classifies areas according to their socioeconomic vulnerability into 7 groups, being 1 the group with lower vulnerabilities and 7 those areas part of the group with higher vulnerabilities. Non-classified areas

Importantly, differences between the districts that received lamps and those that should have but did not due to early abandonment of the program (column 6) are generally smaller and less statistically significant than when compared to the full control group. In particular, the sets of districts that were chosen for program inclusion are similar in size, in initial lamp density, and in measures of economic activity (sales tax revenues). With respect to population density, high school enrollment rate and the share of the population in formal employment, although differences are statistically significant, magnitude differences are substantially smaller compared to those in column (5). The group of districts that were among the ones chosen to receive the new bulbs but did not is indeed more similar to the group that actually received the LED lamps. I exploit this fact in the following analysis.

Data on Crime

Data on crime is taken from the website of the Public Safety Secretary for the Government of São Paulo (SSP, 2017). For five categories of crimes (cell phone thefts, cell phone robberies, vehicle thefts, vehicle robberies, and homicides), the PSS makes available monthly records. The data was collected from victims who accessed the website to register the crimes. Each record includes information on the nature of the crime, as well as the date, time, location (including GPS) , and an indicator on the period (night, morning, and afternoon) of its occurrence. For crimes related to vehicles, detailed information on the vehicle is also included. The present study uses available crime data for the years 2013-2017, three years before and one year after the start of the PLNB program.[15]

Despite its level of detail, a key limitation of this data is that not all crimes committed are actually registered in this system. In 2018, for example, it is estimated that 52% of robberies against a person in the city of Sao Paulo, 64% thefts against a person, 13% of vehicle robberies, and 22% of vehicle thefts were not reported to the police (Amâncio, 2018). Under-reporting could be attributed to lack of belief that a stolen item will be recovered, a belief that the value of the item is not sufficiently high to cover the costs (financial, temporal, and/or emotional) of reporting and pursuing the matter, or a feeling of distrust in public authorities (Amâncio, 2018). The under-reporting of crime likely contributes to several months with zero crimes recorded in the district-level data, and presents a problem for estimation, in particular to the extent that there are unobservables that are possibly correlated with different reporting rates across districts.[16]

receive a zero value and are dropped from the calculus on the average IPVS for each district.

[15]Duplicates in the data (the same crime counted twice or more) are eliminated based on the crime registration number. In the case of homicides, they are eliminated based on the ID number of the victim, which is also provided in the public data set. Besides, for instance, if a cell phone robbery was followed by a homicide, although the crime is counted for both categories of crimes separately, they are not double counted in total nor "non-homicides crimes".

[16]Appendix Figure A3.1 plots the distributions of crime, by category. Crime counts are skewed to the right. This right-skewness may be a result of a large amount of months with zero nighttime crimes registered. Yet, Appendix Table A3.4 suggests that this is unlikely to be the case, showing that the frequency of months with zero crimes registered at night across crimes is no larger than 12% (for cell phone robbery), with the exception of homicides, (where approximately 70% of months see zero crimes registered). In order to alleviate

Table 1.2: Summary Statistics on Crime Prior to Announcement of the PLNB

	(1) All Districts	(2) Treatment	(3) Control	(4) Would Be Treated	(5) (2)-(3)	(6) (2)-(4)
Nighttime Cell Phone Thefts	16.63	7.81	18.13	7.00	-10.32***	0.808*
	(26.16)	(5.63)	(27.94)	(4.29)	(1.39)	(0.48)
Nighttime Vehicle Thefts	12.47	10.49	12.81	11.26	-2.319***	-0.774
	(8.54)	(7.42)	(8.67)	(5.88)	(0.46)	(0.63)
Nighttime Cell Phone Robberies	28.04	32.56	27.27	26.60	5.293***	5.961**
	(26.56)	(27.46)	(26.33)	(20.57)	(1.42)	(2.32)
Nighttime Vehicle Robberies	21.91	33.35	19.95	31.97	13.39***	1.376
	(18.96)	(21.45)	(17.79)	(19.81)	(0.99)	(1.90)
Nighttime Non-Homicide Crimes	76.77	81.94	75.89	75.45	6.047**	6.488
	(51.80)	(47.89)	(52.40)	(38.91)	(2.78)	(4.11)
Total Non-Homicide Crimes	161.35	153.20	162.74	156.29	-9.539*	-3.083
	(96.91)	(87.02)	(98.45)	(78.79)	(5.20)	(7.67)
Nighttime Homicides	0.66	1.17	0.57	1.04	0.595***	0.125
	(1.23)	(1.66)	(1.12)	(1.37)	(0.07)	(0.14)
Nighttime Crime	77.36	82.99	76.40	76.43	6.592**	6.565
	(51.96)	(48.32)	(52.51)	(39.27)	(2.79)	(4.15)
Total Crime	162.27	154.89	163.53	157.94	-8.636*	-3.043
	(97.05)	(87.59)	(98.54)	(79.44)	(5.21)	(7.72)
Nº of District-Months	2,784	406	2,378	174	2,784	580
Nº of Districts	96	14	82	6	96	20

Notes: Data is presented at the level of district-month, and includes observations from from January 2013 to May 2015. Columns (1) to (4) present the mean (and standard deviation) of number of nighttime crimes reported. Columns (5) and (6) show mean differences from columns (2) and (3) and columns (2) and (4), respectively, with standard errors in parenthesis. "Total Non-Homicide Crimes" and "Total Crime" include crimes committed both during the day and at night. Superscripts *, ** and *** represent statistical significance at p<0.1, p<0.05, and p<0.01, respectively.

Table 3.2 presents summary statistics[17] on nighttime crime from January 2013 to May 2015[18], prior to the announcement of the bidding process for the lighting program.[19] Results from t-tests of mean differences in column (5) suggest that crime is generally more frequent in districts that ultimately received lights, compared to those that do not. The total amount

concerns of under-reporting for homicides, I compare the total number of homicides reported by the PSS with data from the National Department of Health (DATASUS) (see Appendix Figure A3.2). Although the figure suggests that indeed homicides are understated in the PSS data, both data sets seem to follow the same trends pre and post PLNB program implementation for treated and "would be treated" groups. Furthermore, regressions of total homicides across both data sets result in very similar coefficients (not shown), supporting the evidence described in the main results below.

[17]"Non-Homicide crimes" include property and violent crimes as defined by the FBI (FBI, 2016a; FBI, 2016b).

[18]Appendix Table A3.2 provides an extended set of summary statistics through December 2015, and Appendix Table A3.3 provides summary statistics on daytime crimes from January 2013 to May 2015.

[19]As the program announcement, even prior to the start of implementation, could affect crime rates (Barclay et al., 1996, *in*), the data extracted before this period can better depict baseline crime levels.

of nighttime crime is, on average, 6.6% higher in treated districts than in the non-treated ones before the announcement of the program, while total crimes committed (regardless of time of day) are slightly lower in treated districts. Importantly, there is very little evidence of differences in crime rates between districts that were chosen for lamps and received them and districts that were chosen for lamps but did not ultimately receive them (column 6), which is the comparison I use in the main analysis.

1.3.2 Estimation Strategy

This paper seeks to estimate the causal impact of improved public lighting on nighttime crime in São Paulo, Brazil. Additional analysis, derived from this main one, will explore possible spatial displacement of crime due to the program, as well as changes in daytime crime.

The key identification issue in estimating the causal impact of improved public lighting on crime is one of omitted variables. As described above, districts were not randomly selected for program inclusion; instead, those considered more socially and economically vulnerable were the ones chosen to receive the new LED lamps. In order to alleviate this estimation problem, I exploit the timing of the phased roll-out to different districts, including a treatment indicator and district fixed effects in the basic regression specification. The fixed effects absorb district characteristics that do not change over time, limiting omitted variables to those that vary within districts over time. These regressions are summarized by:

$$crime\_night_{imt} = \gamma_0 + \gamma_1 crime_{i(m-1)t} + \gamma_2 crime\_day_{imt} + \gamma_3 D_{imt} + \gamma_4 time + X_{it}\gamma_5 + \theta_i + \theta_m + \theta_t + \xi_{imt}$$

where $crime\_night_{imt}$ is the count of crimes reported in district i, month m and year t that occurred at nighttime; $crime_{i(m-1)t}$ is the number of crimes reported in the previous month, included to account for possible inertia or learning effects of criminal activity in a given district (Fajnzylber, Lederman, and Loayza, 2002); D_{imt} is an indicator for the district having received LED lamps during or before that month; $time$ is a linear time trend capturing aggregate trends over time; and X_{it} is a set of time-varying district-level control variables including high school enrollment, formal employment and total population. θ_i, θ_m, and θ_t are respectively district, month and year fixed effects. Results from Hosmer–Lemeshow goodness-of-fit tests suggest that the data suffers from overdispersion. For this reason, results from both linear and Poisson regressions, in which the log count of nighttime crime is regressed on the vector of covariates[20], will be presented.

My primary specification additionally includes $crime\_day_{imt}$ as a control, representing the count of daytime crimes reported in the district-month-year. This measure is included to control for unobserved changes related to overall crime that are happening in the districts over time, such as new business openings (increasing overall economic activity) and other

[20] In order to take into account the presence of zeroes, for the Possion specifications, I use $\ln(crime\_day_{imt} + 1)$ and $\ln(crime_{i(m-1)t} + 1)$ on the RHS of the equation

policy changes (such as changes in policing in these districts). However, to the extent that the lighting program also affects day crime, inclusion of this measure may generate a problem in estimation. This could happen, for example, if the new lights increased economic activity during both night and daytime, for instance by attracting new businesses that are also open during the day (Dinkelman, 2011). I return to this issue in Section 4.1.2 below.

In order to further limit concerns of omitted variables, I present regression results that limit the districts included in the control group to only those that were supposed to receive the LED lamps but did not - the "would be treated" group. I call the sample including only districts that were chosen to receive the LED lights the "restricted sample". As already seen in Tables 3.1 and 3.2, the districts that received the lights and those that were chosen to receive lights but did not receive them are very similar according to their observable characteristics - indeed, the only reason the latter group did not receive the new LED lamps was due to unexpected program abandonment. Appendix Figure A2.3 provides additional evidence of these similarities, showing that trends in crime in the treated and "would be treated" districts are almost identical prior to the launch of the program. By using these districts as a better identified control group, it is expected that the endogeneity generated from the non-random choice of districts to receive the program would be drastically attenuated and we should expect, therefore, more reliable results.

I present the results of this restricted sample first and foremost, but, given sample size limitations, I additionally present results using the full set of districts that did not receive the improved LED lights as a control group, in what I refer to as the "full sample". Appendix Figure A2.4 shows relatively similar trends in crime - although very different levels of crime - prior to the launch of the PLNB across this larger set of control districts and the treated districts, suggesting the estimates using this full sample still provide useful results. Thus, γ_3 can still be interpreted as the effect of the program on the monthly number of cases registered in each category of crime.

1.4 Results and Robustness Checks

1.4.1 Results

Table A3.1 reports estimates from linear (odd columns) and Poisson (even columns) regressions using the equation specified above. Panel A considers the restricted sample (districts who were supposed to receive lamps only), and Panel B considers the full sample (all districts).

Across the restricted sample presented in Panel A, we see negative point estimates for nearly all categories of crime across both regression models, and statistically significant negative impacts for total nighttime crime, "non-homicide crimes", cell phone robberies, and homicides.

The magnitude of impacts is also substantial. The results presented in Panel A suggest that receiving improved LED lights reduces total nighttime crimes by 11.1% overall ($p<0.05$) in the linear model, and results are also negative in the Poisson model with a 1.8% ($p<0.01$) decrease (columns 1 and 2). "Non-Homicide Crimes" (columns 3 and 4) were reduced by 10.9% ($p<0.05$) and 1.8% ($p<0.01$) in linear and Poisson specifications respectively. Cell phone thefts (columns 5 and 6) have negative point estimates, respectively 9.4% and 2.6% for the two different specifications, though results are not statistically significant in either model. On the other hand, the magnitude of the coefficient for cell phone robberies in the linear model suggest that the PLNB may have reduced this type of crime by 25.7% ($p<0.05$), while the magnitude reaches 4.4% ($p<0.05$) in the Poisson specification. The estimate for vehicle robberies, although non significant, points to a decrease in cases by around 0.4% in both models. Finally, results for homicides (columns 13 and 14) points to a 29% and 22% reductions in cases ($p<0.05$ for both specifications)[21]. Thus, even when changing the model specification, results seem to sustain previous findings: as a response to the PLNB, crimes were reduced in treated districts.

Table A3.1, Panel B presents results from linear (odd columns) and Poisson (even columns) regressions using the full sample of districts in São Paulo. The results are generally similar to those discussed in Panel A in terms of direction of estimated effects, with the improved LED lights having a negative impact on cell phone thefts and robberies, vehicle robberies, and homicides. In fact, results from the fixed-effect model using the full sample suggest a decrease of 3.5% of total crimes and 3.3% of "non-homicide" ones, although not statistically significant. On the other hand, for these two categories of crime, coefficients derived from the Poisson model point to reductions of 1.7% ($p<0.01$) and 1.8% (p.0.01) respectively. Cell phone thefts (columns 5) are highly negatively impacted by the new LED lamps, with a 26.4% reduction in such crimes monthly in treated districts ($p<0.01$). In addition, while coefficients from the fixed-effect model indicate non-significant reductions in cell phone robberies (-5.9%), the Poisson model suggest a 10% decrease in this category of crimes ($p<0.01$). Finally, results for homicides suggest decreases of 25.1% and 6% respectively for the fixed-effect and Poisson models. Therefore, if compared to the fixed-effect model, the Poisson specifications present identical directions for crimes, except for cell phone thefts and vehicle robberies. (Though recall that point estimates were aligned in direction in the restricted sample, discussed above.)

Taken together, the results presented in Table A3.1 provide compelling evidence that the lighting improvement program reduced crime in treated districts.

Spatial Spillovers

The crime literature suggests that criminals generally target their activities in particular areas, such as transportation nodes, or areas where their surroundings are familiar (e.g. near their homes) (Barclay et al., 1996). To the extent that infrastructure changes affect these

[21] Percentages for the coefficients derived from the Poisson specifications are calculated by: $(e^{\gamma_3} - 1) \times 100$.

Table 1.3: Effect of Improved Lighting on Nighttime Crimes

	Total Crime		Non-Homicide Crimes		Cell Phone Thefts		Vehicle Thefts		Cell Phone Robberies		Vehicle Robberies		Homicides	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Restricted Sample														
LED lamps	-8.486**	-0.019***	-8.254**	-0.019***	-0.661	-0.026	0.152	0.001	-6.825**	-0.045**	-0.136	-0.004	-0.301**	-0.248**
	(3.360)	(0.006)	(3.340)	(0.006)	(0.474)	(0.022)	(0.643)	(0.017)	(3.021)	(0.019)	(1.182)	(0.016)	(0.121)	(0.120)
Nº of Nighttime Crimes$_{(m-1)}$	0.508***	0.103***	0.512***	0.105***	0.183***	0.051***	0.096**	0.016	0.399***	0.120***	0.320***	0.038***	-0.043	-0.056
	(0.067)	(0.007)	(0.067)	(0.008)	(0.033)	(0.016)	(0.037)	(0.015)	(0.021)	(0.020)	(0.052)	(0.011)	(0.032)	(0.049)
Nº of Day Time Crimes	0.548***	0.153***	0.544***	0.154***	0.132***	0.069***	0.048	0.065***	0.835***	0.513***	0.573***	0.325***	0.090***	0.143***
	(0.092)	(0.008)	(0.091)	(0.009)	(0.017)	(0.021)	(0.042)	(0.013)	(0.069)	(0.022)	(0.051)	(0.023)	(0.028)	(0.051)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.707		0.708		0.363		0.094		0.798		0.525		0.018	
Pseudo-R^2		0.024		0.025		0.045		0.034		0.206		0.058		0.086
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Panel B: Full Sample														
LED lamps	-2.651	-0.017***	-2.537	-0.018***	-4.784***	0.010	0.535	0.019	-1.618	-0.106***	-0.506	0.017	-0.143	-0.061
	(2.706)	(0.004)	(2.695)	(0.004)	(1.145)	(0.013)	(0.385)	(0.013)	(1.284)	(0.013)	(1.065)	(0.012)	(0.109)	(0.088)
Nº of Nighttime Crimes$_{(m-1)}$	0.284***	0.096***	0.285***	0.096***	0.142**	0.046***	0.207***	0.061***	0.353***	0.141***	0.342***	0.064***	-0.005	-0.013
	(0.063)	(0.004)	(0.063)	(0.004)	(0.069)	(0.007)	(0.020)	(0.007)	(0.026)	(0.009)	(0.031)	(0.006)	(0.018)	(0.034)
Nº of Day Time Crimes	0.627***	0.137***	0.626***	0.141***	0.559***	0.131***	0.117***	0.100***	0.857***	0.509***	0.595***	0.276***	0.034**	0.077**
	(0.092)	(0.006)	(0.092)	(0.006)	(0.102)	(0.009)	(0.023)	(0.007)	(0.051)	(0.010)	(0.038)	(0.010)	(0.016)	(0.037)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.575		0.576		0.415		0.162		0.759		0.473		0.009	
Pseudo-R^2		0.039		0.040		0.098		0.057		0.212		0.103		0.137
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96	96	96	96	96	96	96	96

Notes: The level of analysis is the district-month level. Separate regressions are run for each different type of crime. Coefficients for fixed-effect and Poisson specifications are depicted respectively on odd and even columns within crime category. Regressions include controls for the log number of teenagers enrolled in high school across districts, log of population and log of formal employees per year, as well as month, year and district fixed effects. Robust standard errors are in parenthesis, clustered at the district level for the fixed-effect models. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

areas by pushing crime out, the impact on neighboring areas is unclear - criminals may move their activities to these neighboring areas (nearby to where they were working before, thereby increasing crime in neighboring areas), or may move elsewhere altogether (thereby also decreasing crime in neighboring areas). Because of that, I now investigate if the PLNB created spillover impacts (positive or negative) for neighboring districts. If the program was indeed effective in reducing crimes in neighboring areas, taking into account only decreases in crimes in treated districts would underestimate the total, and real, impact of the program.

In order to study this question, I add to the main regression model (presented in the equation above) an indicator variable that equals 1 if any given district neighbors a treated district and that district that it neighbors has started being treated. Since five out of six "would be treated" districts neighbor at least one treated district, the entire sample is included in the estimation. Table 3.3 shows the results. Estimates suggest that crime is generally reduced in areas neighboring those that received improved lighting - coefficients are negative for the most part, and statistically significantly so for cell phone thefts, vehicle robberies and homicides. Results suggest that the program reduced approximately total and "non-homicide crimes" by about 0.9% and 0.8% respectively (not statistically significant). For cell phone thefts (column 3) and homicides (column 7), estimates indicate a reduction of respectively 25.4% and 26.3% of cases monthly in neighboring districts ($p<0.01$ and $p<0.05$, respectively). Finally, the new LED bulbs seem to have diminishing vehicle robberies by 5.5% in neighboring districts ($p<0.1$)

Therefore, two findings seem to be important to be highlighted when comparing the main results found in Table A3.1 with the ones in Table 3.3. First, with the exception for coefficients for cell phone and vehicle thefts and vehicle robberies, all other estimates for neighboring districts are lower than those for treated districts, which is reasonable given that we should expect the treatment effect to be stronger in districts that actually received the treatment. However, it is not entirely a puzzle having coefficients for neighboring districts greater in magnitude than those for treated districts given a possible treatment effect without treatment already found in the literature.[22] Secondly, the reduction of both total and "non-homicide" crimes in neighboring districts represent around 7% of the reduction of nighttime crimes in treated districts for both categories. At the same time, reductions in homicides in neighbor districts are around 50% of the effect of the PLNB on treated districts, while cell phone thefts seem to be reduced more in neighboring districts, 4.6 cases ($p<0.01$), than in treated ones. Therefore, estimating the effect of the program only on treated districts seems to underestimate the real treatment effect of PLNB.

Daytime Crime

Regressions to this point employ daytime crime as a control measure, as a proxy for other policies or circumstances that are changing that may affect overall crime at the district

[22] One example of that is a decrease of auto thefts registered in Vancouver that were registered only as an effect of a publicity campaign on TV about the future program to be implemented (Barclay et al., 1996).

Table 1.4: Effect of Improved Lighting on Nighttime Crimes in Neighboring Districts

	Total Crime	Non-Homicide Crimes	Cell Phone Thefts	Vehicle Thefts	Cell Phone Robberies	Vehicle Robberies	Homicides
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
LED lamps	-2.9	-2.7	-6.5***	.62	-.95	-.88	-.19*
	(3.013)	(3.000)	(1.588)	(0.416)	(1.293)	(1.029)	(0.107)
Neighbor District	-.66	-.59	-4.6***	.24	1.8	-1.1*	-.15**
	(2.670)	(2.659)	(1.562)	(0.379)	(1.138)	(0.599)	(0.067)
Nº of Nighttime Crimes$_{(m-1)}$	.28***	.28***	.14**	.21***	.35***	.34***	-.0061
	(0.063)	(0.063)	(0.068)	(0.020)	(0.025)	(0.031)	(0.017)
Nº of Day Time Crimes	.63***	.63***	.56***	.12***	.86***	.59***	.034**
	(0.092)	(0.092)	(0.102)	(0.023)	(0.051)	(0.038)	(0.016)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.575	0.576	0.416	0.162	0.759	0.473	0.010
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96

Notes: The model is derived from the main estimation explained in section 3. The "Neighbor District" variable is added as an indicator variable that equals 1 if any given district neighbors a treated district and that district that it neighbors has started being treated. For more information, see Table A3.1. Robust standard errors are in parenthesis and clustered at the district level. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table 1.5: Effect of Improved Lighting on Daytime Crimes

	Total Crime	Non-Homicide Crimes	Cell Phone Thefts	Vehicle Thefts	Cell Phone Robberies	Vehicle Robberies	Homicides
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Treated VS Would be Treated							
LED lamps	5.148^{***}	5.164^{***}	1.128	1.041	3.133^{**}	0.840	-0.037
	(1.656)	(1.650)	(0.856)	(0.663)	(1.200)	(1.543)	(0.121)
Nº of Daytime Crimes$_{(m-1)}$	0.678^{***}	0.680^{***}	0.397^{***}	0.694^{***}	0.687^{***}	0.475^{***}	0.007
	(0.033)	(0.033)	(0.068)	(0.139)	(0.027)	(0.059)	(0.035)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.581	0.583	0.463	0.529	0.657	0.284	0.005
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20
Panel B: Full Sample							
LED lamps	6.116^{**}	6.145^{**}	-2.286	1.207^{***}	3.900^{***}	0.038	-0.064
	(2.903)	(2.880)	(2.010)	(0.431)	(0.942)	(0.931)	(0.088)
Nº of Daytime Crimes$_{(m-1)}$	0.384^{***}	0.384^{***}	0.207^{***}	0.471^{***}	0.636^{***}	0.397^{***}	-0.013
	(0.066)	(0.066)	(0.050)	(0.066)	(0.016)	(0.043)	(0.019)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.277	0.278	0.119	0.312	0.578	0.215	0.002
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96

Notes: See Table A3.1. Robust standard errors are in parenthesis and clustered at the district level. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

level. Yet, it could be the case that the infrastructure improvement program also impacted crimes committed during the day, either positively or negatively. For instance, improved lighting during the nighttime may increase economic activities in the district - by increasing the flow of customers, or increasing the number of businesses locating there. This may increase crime during the day, due to an increase in the number of potential targets for criminal activity. Alternatively, as Section 4.1.1 concludes that the program decreased crime in neighboring districts, it could be that it similarly lowers crime in the treated districts even during daytime hours (if for instance, criminals are deterred by the nighttime changes and opt to move their activities elsewhere).

In Table 3.4, I estimate the relationship between the lighting program and daytime crimes. Note that these estimates should be interpreted with caution. By using day crime as the dependent variable, there is no other independent variable that accounts for time variant changes happening in each district that may impact crime.[23] Interestingly, the estimates in Table 3.4 suggest a positive relationship between receipt of improved LED lamps and crime, in both the restricted and full samples of districts. In particular, it looks as though vehicle thefts and cell phone robberies increased as LED lights were installed, suggesting that either criminals moved their activities to the daytime (temporal displacement of crime), or that something else was changing in these districts over time that increased crime there (such as increased economic activity, or a policy change that increased daytime crime).

Table 3.5 provides estimates of the relationship between lighting program receipt and nighttime crime, without the daytime crime control (so re-estimation of the results in Table A3.1 without the daytime crime control). As in Table A3.1, odd columns depict the results of the fixed-effect specification, while even ones show the coefficients resulted from the Poisson specification. Estimates using the restricted sample of districts (Panel A) and the full sample of districts (Panel B) suggest somewhat mixed results. Consistent across the two panels and regression models, we do see a positive relationship between nighttime crime and the relighting program in particular for vehicle thefts, and a negative relationship for homicides.

The findings derived from Table 3.5 seem to clarify the ones in Table 3.4, with the increase in crimes during day time possibly being caused by an increase of potential victims due to a growth of economic activity (day and night) in the treated districts as a response to the PLNB. The suggested higher number of vehicle thefts may also be a result of more vehicles parked in those areas, a consequence of a higher economic activity generated by the program. Thus, in absolute terms, crimes in general seem to be increasing due to the PLNB, but nighttime crimes are falling relative to day time crime due to the brighter lights. The evidence suggests not only that the PLNB has changed the environment of treated districts at night, but also created an increase in economic activity during day and night in these districts. However, it is not possible to entirely rule out some weak evidence that crimes committed under natural light are increasing due to compensations of the decreases found during the night (temporal displacement).

[23]The lack of a proxy variable used for this purpose comes from the fact that it is extremely hard to find district-level data on a monthly basis.

Table 1.6: Effect of Improved Lighting on Nighttime Crimes - Without Day Crime Controls

	Total Crime		Non-Homicide Crimes		Cell Phone Thefts		Vehicle Thefts		Cell Phone Robberies		Vehicle Robberies		Homicides	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Restrict Sample														
LED lamps	-0.286	0.006	-0.171	0.007	-0.385	-0.017	0.345	0.010	0.007	0.079***	0.648	0.013	-0.305**	-0.254**
	(2.388)	(0.007)	(2.353)	(0.007)	(0.545)	(0.022)	(0.694)	(0.017)	(1.928)	(0.025)	(1.231)	(0.022)	(0.125)	(0.119)
Nº of Nighttime Crimes$_{(m-1)}$	0.720***	0.169***	0.723***	0.171***	0.228***	0.058***	0.099**	0.020	0.730***	0.420***	0.468***	0.096***	-0.048	-0.065
	(0.020)	(0.008)	(0.020)	(0.008)	(0.035)	(0.016)	(0.038)	(0.016)	(0.018)	(0.026)	(0.045)	(0.018)	(0.031)	(0.049)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.613		0.616		0.334		0.083		0.692		0.354		0.013	
Pseudo-R^2		0.023		0.023		0.044		0.034		0.161		0.034		0.085
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20	20	20	20	20	20	20	20
Panel B: Full Sample														
LED lamps	2.378	0.002	2.492	0.002	-5.491***	0.033**	0.753*	0.030**	3.404***	-0.000	-0.256	0.041**	-0.145	-0.065
	(3.231)	(0.005)	(3.211)	(0.005)	(1.636)	(0.014)	(0.405)	(0.013)	(1.135)	(0.018)	(1.060)	(0.016)	(0.111)	(0.088)
Nº of Nighttime Crimes$_{(m-1)}$	0.478***	0.144***	0.479***	0.146***	0.260***	0.066***	0.225***	0.068***	0.684***	0.426***	0.462***	0.103***	-0.006	-0.017
	(0.059)	(0.004)	(0.059)	(0.004)	(0.088)	(0.007)	(0.019)	(0.007)	(0.014)	(0.011)	(0.034)	(0.008)	(0.018)	(0.034)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.383		0.385		0.166		0.116		0.632		0.324		0.009	
Pseudo-R^2		0.038		0.039		0.097		0.056		0.170		0.088		0.137
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96	96	96	96	96	96	96	96

Notes: See Table A3.1. Robust standard errors are in parenthesis and clustered at the district level for the fixed-effect models. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

1.4.2 Robustness Checks

In this section, several alternative models are run in order to provide extra evidence to support the results found in the previous section. First, the outcome measure is changed to the rate of crime for every 100,000 residents[24], and the regression is run using the linear model only. Table 3.6 depicts the results for this estimation strategy. Estimates are similar to those presented in Table A3.1. The signs are exactly the same as the one in the main estimation strategy, for both the restricted sample and the full sample. This supports the previous finding that the program negatively impacted crimes committed during nighttime, except for vehicle thefts, which never present negative coefficients. Coefficients for total and "non-homicide crimes" in Panel A suggest reductions respectively of about 19.9 and 19.5 cases per 100,000 inhabitants, significant at 5% level, which represent reductions of 18.5% and 18.3% respectively (results even higher than the ones presented in Table A3.1.

The second important aspect to take into account for the robustness of the estimation is the fact that, although LED lamps accounted for only 2% of the lighting system of the city in March of 2014 (as cited in section 2), it means that some of the districts used in the main estimation strategy as the control group cannot be entirely considered as non-treated ones. The fact that these districts already had part of their territory illuminated by LED lamps means that the effects observed on the districts contemplated by the PLNB could already have happened in those districts, even in lower magnitudes. If the changes in the environment caused by better infrastructure were enough to generate changes in behavior by the criminals and the population (as observed in the previous section), it would generate doubts on the validity of the results for including in the non-treated group districts that should be considered at least as partially treated ones.

Additionally, some districts that indeed received the new lamps through the PLNB were only partially treated. Not the entire lighting system of of these districts were replaced by LED lamps. This is the case of *Jardim São Luis*, *Sacomã*, *Jabaquara* and *Iguatemi*. The reasons that led to this condition of partially treated districts vary. In *Sacomã*, for example, the first district that received the LED lamps in December of 2015, the goal was to illuminate a specific vulnerable area known as *favela de Heliópolis*, where months before the installation of the LED lamps, female residents protested against the lack of lighting and the fear they felt by walking on the streets of their neighborhood. On the other hand, in *Jabaquara* and *Iguatemi*, lack of resources of the city hall was the main reason for incomplete implementation of the new LED technology.

Because of these factors, I re-estimate the main model, this time restricting the sample by dropping all of these *partially treated districts* on both sides (treated and control groups). Thus comparing only "full treated" regions with "full non-treated" ones. Although we should expect stronger results once we remove this districts, the fact that the sample size is now about 1/3 lower than the one used in the main estimation strategy could affect the statistical significance of the results. However, the direction of the estimates should not change and

[24]Because of that, population was removed as a control on the RHS of the equation.

still point to a reduction in crimes (except for vehicle thefts, as already highlighted in section 4), following the trends of the previous specifications. Panels A and B in Table 3.7 depict the results.

In both panels, the directions of the results are indeed exactly the same as in the main estimation strategy (Table A3.1), as we should expect. The only change for this evaluation is the magnitude and significance of the results. While in Panel B, results are not statistically significant, except for cell phone thefts, following the same pattern as the one presented in the same panel in Table A3.1, when the control group is narrowed, not only is the previous pattern confirmed, but magnitudes of statistically significant coefficients increase. Estimates point to a reduction of 13.9% and 13.76% respectively in total and "non-homicide crimes" by month, significant at 5% level. Cell phone thefts and robberies appear to have been reduced respectively in 0.89% (not significant) and 35.9% (significant at 5% level). Vehicle robberies decreased by 3.67% (not significant) and homicides were reduced by 38.32% ($p<0.05$).

Table 1.9 depicts coefficients resulted from a model where the treatment dummy (D_{imt}) is interacted with a measure of light intensity[25]: a) the percentage of bulbs replaced in each district (panels A and B), and b) the amount of bulbs per km^2 (panels C and D). Thus, estimates in Panels A and B should be interpreted as the average monthly effect of a 1% increase in replaced bulbs on crime i, while the ones in Panels C and D should be interpreted as the average effect of one extra LED lamp per km^2 on crime i per month.

Coefficients presented in Table 1.9 follow the same pattern observed previously. Aggregate crimes are highly affected by the replacement of bulbs using the two different measures of intensity. Furthermore, besides vehicle thefts[26] and robberies, all the other types of crimes report decreases in cases across every panel and specifications, although not statistically significant.

In a final robustness check presented in Table 3.8, I run placebo tests in order to confirm that patterns and results presented are indeed evidences of causality of the PLNB on crimes and not derived solely by chance. In order to do so, I first randomly select 14 districts (out of the 96) in the sample to be the treated ones. Then, I follow the chronological order of treatment assignment in order to, once again, randomly assign the time of treatment among the selected placebo districts[27]. By doing so, I create fake treatment and control groups and re-create Panel B of Table A3.1. Finally, I drop the fake treated districts of the sample and randomly select 6 districts to be part of the fake "would be treated" group and re-create Panel A of Table A3.1. Results are depicted in Table 3.8.

None of the estimates are significant. Although, results point to a decrease in aggregate

[25] Both measures include not only the original number of lamps that were replaced by LED lamps, but those that were added to the lighting system of each treated district. Because of that, the percentage of replaced bulbs may surpass the 100% mark for some districts.

[26] Although in Panel A the coefficient for vehicle thefts turns to be negative (column (8) with the addition of day crimes in the set of controls).

[27] The chronological order is the following: three districts received the new LED lamps in December of 2015, one in February, two in March, two in April, one in May and five in June of 2016.

Table 1.7: Effect of Improved Lighting on Rate of Nighttime Crimes

	Total Crime (1)	Non-Homicide Crimes (2)	Cell Phone Thefts (3)	Vehicle Thefts (4)	Cell Phone Robberies (5)	Vehicle Robberies (6)	Homicides (7)
Panel A: Treated VS Would be Treated							
LED lamps	-19.862**	-19.534**	-1.321	0.366	-18.541*	-1.069	-0.492*
	(8.654)	(8.617)	(1.819)	(1.603)	(9.203)	(2.708)	(0.276)
Nº of Nighttime Crimes$_{(m-1)}$	0.577***	0.580***	0.249***	0.104**	0.415***	0.295***	-0.036
	(0.085)	(0.083)	(0.043)	(0.046)	(0.019)	(0.042)	(0.040)
Nº of Day Time Crime	0.493***	0.490***	0.168***	0.022	0.947***	0.636***	0.068**
	(0.115)	(0.113)	(0.019)	(0.028)	(0.113)	(0.068)	(0.024)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.710	0.711	0.399	0.073	0.814	0.496	0.011
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20
Panel B: Full Sample							
LED lamps	-4.759	-4.598	-4.087***	1.771**	-5.379	-0.497	-0.398
	(3.231)	(3.208)	(1.436)	(0.687)	(3.782)	(1.795)	(0.271)
Nº of Nighttime Crimes$_{(m-1)}$	0.472***	0.474***	0.109**	0.191***	0.394***	0.357***	-0.001
	(0.078)	(0.078)	(0.055)	(0.025)	(0.030)	(0.042)	(0.026)
Nº of Day Time Crime	0.580***	0.578***	0.462***	0.065	0.926***	0.700***	0.037*
	(0.088)	(0.087)	(0.072)	(0.042)	(0.059)	(0.068)	(0.019)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.652	0.654	0.321	0.114	0.793	0.505	0.009
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96

Notes: Robust standard errors are in parenthesis and clustered at the district level. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table 1.8: Effect of Improved Lighting on Nighttime Crimes - Treated vs Non-treated Districts

	Total Crime	Non-Homicide Crimes	Cell Phone Thefts	Vehicle Thefts	Cell Phone Robberies	Vehicle Robberies	Homicides
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Treated VS Would Be Treated							
LED lamps	-10.626^{**}	-10.386^{**}	-0.621	0.158	-9.550^{**}	-1.173	-0.228^{**}
	(3.859)	(3.870)	(0.670)	(0.750)	(3.420)	(1.104)	(0.088)
Nº of Nighttime Crimes$_{(m-1)}$	0.539^{***}	0.543^{***}	0.144^{***}	0.108^{**}	0.376^{***}	0.375^{***}	-0.022
	(0.076)	(0.076)	(0.042)	(0.040)	(0.018)	(0.051)	(0.037)
Nº of Day Time Crimes	0.537^{***}	0.532^{***}	0.140^{***}	0.031	0.907^{***}	0.725^{***}	0.071^{**}
	(0.113)	(0.112)	(0.022)	(0.036)	(0.079)	(0.047)	(0.031)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.704	0.704	0.373	0.092	0.806	0.525	0.014
Nº of Months	885	885	885	885	885	885	885
Nº of Districts	15	15	15	15	15	15	15
Pannel B: Full Treated vs Full Non-treated							
LED lamps	-0.886	-0.794	-2.470^{**}	0.549	-1.422	-0.425	-0.100
	(2.926)	(2.910)	(0.969)	(0.420)	(1.681)	(1.302)	(0.082)
Nº of Nighttime Crimes$_{(m-1)}$	0.422^{***}	0.422^{***}	0.105^{*}	0.204^{***}	0.376^{***}	0.360^{***}	0.003
	(0.056)	(0.057)	(0.055)	(0.025)	(0.024)	(0.038)	(0.017)
Nº of Day Time Crimes	0.478^{***}	0.478^{***}	0.272^{***}	0.087^{***}	0.826^{***}	0.665^{***}	0.025
	(0.080)	(0.079)	(0.099)	(0.025)	(0.037)	(0.053)	(0.017)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.550	0.551	0.150	0.138	0.762	0.486	0.010
Nº of Months	3,599	3,599	3,599	3,599	3,599	3,599	3,599
Nº of Districts	61	61	61	61	61	61	61

Notes: Robust standard errors are in parenthesis and clustered at the district level. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table 1.9: Effect of Improved Lighting on Nighttime Crimes - Intensity Measures

	Total Crime	Non-Homicide Crimes	Cell Phone Thefts	Vehicle Thefts	Cell Phone Robberies	Vehicle Robberies	Homicides
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: % Replaced Bulbs $\times D_{imt}$ - Treated VS Would Be Treated							
Intensity of Replacement	-6.1**	-5.9**	-.36	-.06	-4.7*	-.5	-.013
	(2.819)	(2.782)	(0.440)	(0.584)	(2.643)	(1.229)	(0.157)
Adjusted-R^2	0.706	0.707	0.362	0.094	0.797	0.525	0.015
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20
Panel B: % Replaced Bulbs $\times D_{imt}$ - Full Sample							
Intensity of Replacement	-2.2	-2.1	-4.2***	.41	-1.2	-.81	-.018
	(2.850)	(2.837)	(1.062)	(0.403)	(1.357)	(1.254)	(0.091)
Adjusted-R^2	0.575	0.576	0.414	0.162	0.759	0.473	0.009
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96
Panel C: $lamps/km^2 \times D_{imt}$ - Treated VS Would Be Treated							
Intensity of lamps	-.012**	-.011**	-.00059	.00087	-.01**	.00066	-.00039*
	(0.005)	(0.005)	(0.001)	(0.001)	(0.005)	(0.002)	(0.000)
Adjusted-R^2	0.706	0.707	0.362	0.095	0.797	0.525	0.017
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20
Panel D: $lamps/km^2 \times D_{imt}$ - Full Sample							
Intensity of lamps	-.0033	-.0031	-.008***	.0011*	-.0032	.0001	-.00027*
	(0.005)	(0.005)	(0.002)	(0.001)	(0.002)	(0.001)	(0.000)
Adjusted-R^2	0.575	0.576	0.414	0.162	0.759	0.473	0.009
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96

Notes: Robust standard errors are in parenthesis and clustered at the district level. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table 1.10: Effect of Improved Lighting on Nighttime Crimes - Placebos

	Total Crime	Non-Homicide Crimes	Cell Phone Thefts	Vehicle Thefts	Cell Phone Robberies	Vehicle Robberies	Homicides
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Treated VS Would Be Treated							
LED lamps	1.473	1.496	-1.564	-0.761	1.160	1.034	-0.012
	(4.113)	(4.077)	(1.574)	(0.832)	(1.994)	(1.898)	(0.160)
Nº of Nighttime Crimes$_{(m-1)}$	0.302***	0.304***	0.119***	0.223***	0.374***	0.431***	-0.088***
	(0.070)	(0.072)	(0.017)	(0.046)	(0.029)	(0.056)	(0.027)
Nº of Day Time Crimes	0.533***	0.532***	0.479***	0.137***	0.823***	0.527***	0.072**
	(0.011)	(0.012)	(0.009)	(0.025)	(0.059)	(0.032)	(0.034)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.667	0.667	0.653	0.162	0.755	0.446	0.026
Nº of Months	1,180	1,180	1,180	1,180	1,180	1,180	1,180
Nº of Districts	20	20	20	20	20	20	20
Panel B: Full Sample							
LED lamps	-1.292	-1.267	-2.115	-0.157	1.026	0.204	0.019
	(3.289)	(3.282)	(1.491)	(0.455)	(1.695)	(1.267)	(0.126)
Nº of Nighttime Crimes$_{(m-1)}$	0.284***	0.285***	0.143**	0.207***	0.352***	0.342***	-0.004
	(0.063)	(0.063)	(0.069)	(0.020)	(0.026)	(0.031)	(0.017)
Nº of Day Time Crimes	0.626***	0.625***	0.559***	0.118***	0.854***	0.595***	0.035**
	(0.091)	(0.091)	(0.102)	(0.023)	(0.051)	(0.038)	(0.016)
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted-R^2	0.575	0.576	0.414	0.162	0.759	0.473	0.009
Nº of Months	5,664	5,664	5,664	5,664	5,664	5,664	5,664
Nº of Districts	96	96	96	96	96	96	96

Notes: Robust standard errors are in parenthesis and clustered at the district level. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

of crimes in the Panel B, when we move to Panel A, results does not follow the pattern observed so far and remain statistically not significant. I also apply a Monte Carlo simulation running the same experiment 100 times, selecting different “fake treatments”, and, in the case of Panel A, “would be treated”, for each different category of crime. Even though within the sample of coefficients derived from the simulation, some of them are reported to be statistically significant at 10% level (maximum of 23 for vehicle robberies in Panel B, and 16 for homicides and vehicle robberies in Panel A), none of the averages of the coefficients are significant at any significance level. Therefore, results are highly robust to different specifications and provide strong evidences that the PLNB indeed was responsible for the decreases in crimes.

1.5 Discussion

The PLNB had a significant impact on “non-homicide” and total crimes (results driven in particular by the “non-homicide crimes”), but it seems that different types of crimes are affected differently by the program. Taking into account results presented above and the high percentage of under-reporting of cases of cell phone robberies and thefts (highlighted in Section 3), these kinds of “non-homicide crimes” appear to be highly affected by the program (mainly robberies). This may be the case due to the fact that most of these crimes are committed by surprise, with the victim not noticing the criminal’s position and, therefore, not having time to protect from the act. Thus, better lights would improve the ability of people walking on the streets to identify possible offenders, and inhibit the criminal’s action by creating more difficulties for hiding (before and after the crime). Potential witnesses also may influence the decision of committing these types of crimes, since, for the criminal, it will be more difficult to get away without being caught or recognized by someone. These hypotheses, raised by the “situational crime prevention” theory, can be confirmed by several testimonies of treated districts residents:

> “Now I can walk down the stairs on the streets [where I live] because I can even see if there is someone hiding around”, resident of Lajeado (Simao Pedro, March, 2016).

> “The difference in our lives is huge. I’ve spent years here, with my baby son, without being able to be out of my house at night due to the lack of lighting. When he was sick, it was horrible. Now we can be outside, stay later on the streets and go back home safer”, resident of Raposo Tavares (Jornal Zona Leste News, May, 11 2016).

> “It got 70% better. In the past it was way darker. Now, the streets are brighter, what makes it easier to see if someone is coming”, resident of Cidade Tiradentes (Alencar, August, 25 2016).

These are just some of several testimonies given by residents of the PLNB districts that provide context in favor of the hypotheses highlighted in section 1. Indeed there are more people on the streets at night enjoying the public spaces like squares and even in local businesses until later in the night. Some of the testimonies point out that the neighborhood is now “more alive” (Gomes, September, 29 2016) or even “more beautiful” (Prefeitura de São Paulo, October, 5 2016), what may be perceived as an increase in the pride of the population and in the sense of community. This mechanism also helps to explain what may be happening in those districts that received the new LED lamps that is driving the number of crimes related to cell phones downwards.

On the other hand, estimates for crimes related to vehicles do not point to significant reductions in these types of crimes. Better lights could in fact discourage criminals, but, alone, this policy does not seem to be enough to generate significant results. In order to understand what has happened in the districts with the PLNB, we first need to look at the logic behind the opportunities that open space for these specific types of crimes related to vehicles. One of the main reasons that creates “hot spots” for vehicle thefts is unattended vehicles in areas near major roadways, with easy transit access (Clarke et al., 1996; Barclay et al., 1996). The period of the day that people leave their cars unattended is mostly when they go to work. They normally park their cars for several hours on the streets and only come back in the end of the day so they can go back home. Therefore, within this interval, criminals would have time to act with no fear. Results in Table A3.1 and Table A3.3 confirm this assumption by showing that vehicle thefts are indeed committed more frequently during day time for all the three different groups. Thus, vehicle thefts committed at night could still be affected by the new lights, but given that the frequency of vehicle thefts is higher during day time, and that the “hot spots” are still the same, it is reasonable not to expect drops in results for vehicle thefts committed at night.

Another factor that pushes criminals to commit crimes related to vehicles is the ease with which they can get rid of them after the crime (Laycock and Webb, 2005). Because of that, aiming to act in decreasing the high rates of vehicle thefts and robberies in the state, in January of 2014, the State Assembly of the state of São Paulo approved a law that restricted the number of car shops authorized to resell car parts. According to the law, only certified car shops would have the authorization to keep their businesses and all the car parts sold would have to be registered informing the public authorities and the customer about their origin (Assembleia Legislativa do Estado de São Paulo, January, 2 2014). Therefore, shops considered illegal by the public authorities would have to close their doors. As an immediate consequence, several car shops were closed (G1 São Paulo, July, 1 2015) in the capital, São Paulo. If we expect that this law indeed reduced the number of crimes related to vehicles (both thefts and robberies), which seems to be the case (Ribeiro and Carvalho, February, 25 2018; Pagnan, May, 24 2019), it would then absorb most of the impact on these crimes[28], and the LED lamps would not generate significant results, for vehicle thefts or robberies

[28]Although the law was approved more than one year before the PLNB, we should expect that it takes time to make it work. Thus, supervision and the following closing of illegal car shops would not have an immediate effect and, in fact, would continue to have effects over time. Consequently, the results for vehicle related crimes could be influenced by a late response of the law.

committed at night, which is consistent with the results shown in this paper.

Finally, we come to homicide, which is not considered a crime of opportunity. It is normally motivated by other reasons such as fights (for instance, in bars and parties), revenge, drug trafficking, or premeditated executions (Secretaria de Segurança Pública do Estado de São Paulo, 2019). It also may be motivated by domestic alterations and by the police (Moraes et al., September, 26 2019). High magnitudes related to this specific type of crime are mostly explained by the already low number of cases registered in comparison to other crimes.

On the other hand, three possible mechanisms may explain the observed decreases in homicides. First, simply by reducing violent crimes (as seen for vehicle and cell phone robberies), the probability of homicides as a consequence of these types of crimes is also reduced. Secondly, if reductions in crimes in total also result in both less trafficking and more criminals being arrested, less criminals inclined to commit homicides are on the streets. Third, mental health of residents could have increased as a consequence of the improvement in quality of life caused by improvements in lighting in the neighborhood, as supported by residents testimonials. If this is true, a decrease in cases of homicides makes sense.

1.6 Conclusion

The biggest challenge in the literature on crime and infrastructure, particularly with regard to lighting improvements, is to develop a study that provides technically supported evidence of causality. This paper addresses this question by investigating the effects of a lighting improvement program that occurred in the city of São Paulo, in 2015 and 2016, named "Programa LED nos Bairros". By exploiting the phased roll-out of the program and a pool of districts that were supposed to receive the program but did not as a control group, it is possible to mitigate concerns of endogeneity and thus present persuasive estimates of the impact of the PLNB on crime.

Data was collected for all crimes registered in the city on a monthly basis from 2013 to 2017. Estimates indicate a large negative impact of the new LED lamps on crime, particularly for cell phone robberies (25.7%) and homicides (58.9%). Aggregate measures of "non-homicide" and total crimes also suggest significant reductions in cases as a result of the program, at 11.1% and 10.9% respectively. Neighboring districts seem to be affected by the program, with almost all categories of crime studied, except for vehicle thefts and cell phone robberies, being reduced (significant for cell phone thefts, vehicle robberies and homicides) in what is interpreted as a positive "spillover effect". Finally, although carefully interpreted, I find weak evidence of daytime crime increasing.

The robustness of these findings is confirmed by using multiple estimation strategies, variations in the definition of the outcome measure, different control groups and running placebo tests. All the results suggest that the main empirical strategy applied in this study in fact provides evidence of causality between the PLNB and reductions in crimes observed in treated districts. The main limitation of this paper is, however, the impossibility to obtain monthly data on each district and, therefore, not being able to attest the effect of the program on day crimes, although providing a few signs of weak temporal displacement.

In brief, results suggest that better light infrastructure has an important role in combating crime through environmental changes that affect the behaviour of both potential criminals and victims. As stated by the "crime prevention theory", the complexities of different types of crimes are such that the same measure may work for some, while not affecting others even within the same type of crime for different goods (for example cell phone and vehicle thefts). Although not homogeneous across crime categories, an improved public lighting system can be an effective way to reduce certain types of crimes committed at night in both treated and neighboring areas.

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Appendix

Table A1.1: Summary Statistics 2013 - November, 2015

	(1) All Districts	(2) Treated Districts	(3) Non-Treated Districts	(4) Would Be Treated	(5) (2)-(3)	(6) (2)-(4)
Cell Phone Thefts Day	28.43	15.30	30.67	16.25	-15.36***	-0.944
	(31.38)	(10.69)	(33.15)	(10.97)	(1.51)	(0.89)
Cell Phone Thefts Night	17.21	8.13	18.76	7.20	-10.63***	0.931**
	(26.93)	(5.78)	(28.75)	(4.34)	(1.30)	(0.44)
Vehicle Thefts Day	25.15	14.38	26.99	19.67	-12.61***	-5.294***
	(19.18)	(11.64)	(19.61)	(22.32)	(0.91)	(1.29)
Vehicle Thefts Night	12.75	10.54	13.12	11.76	-2.585***	-1.223**
	(8.71)	(7.38)	(8.87)	(5.95)	(0.42)	(0.58)
Cell Phone Robberies Day	21.26	23.48	20.88	22.95	2.595***	0.525
	(18.38)	(19.53)	(18.15)	(17.72)	(0.90)	(1.57)
Cell Phone Robberies Night	31.33	36.97	30.37	31.50	6.605***	5.478**
	(27.81)	(29.14)	(27.46)	(24.54)	(1.35)	(2.30)
Vehicle Robberies Day	14.62	23.26	13.15	27.53	10.11***	-4.272**
	(14.41)	(19.01)	(12.90)	(22.36)	(0.68)	(1.66)
Vehicle Robberies Night	21.44	32.23	19.60	31.50	12.63***	0.726
	(18.22)	(20.83)	(17.08)	(19.12)	(0.86)	(1.68)
Non-Homicide Crimes Day	87.79	74.80	90.01	85.03	-15.21***	-10.23***
	(53.30)	(45.01)	(54.28)	(45.42)	(2.59)	(3.72)
Non-Homicide Crimes Night	80.61	85.80	79.72	80.70	6.079**	5.095
	(53.31)	(48.45)	(54.05)	(41.99)	(2.60)	(3.84)
Total “Non-Homicide Crimes”	168.40	160.60	169.73	165.73	-9.134*	-5.135
	(100.53)	(90.28)	(102.13)	(82.60)	(4.91)	(7.26)
Homicides Day	0.38	0.69	0.33	0.76	0.366***	-0.0653
	(0.89)	(1.17)	(0.83)	(1.40)	(0.04)	(0.10)
Homicides Night	0.63	1.14	0.55	1.07	0.593***	0.0741
	(1.20)	(1.62)	(1.09)	(1.42)	(0.06)	(0.13)
Total Crime Day	88.11	75.42	90.28	85.69	-14.86***	-10.27***
	(53.30)	(45.10)	(54.29)	(45.65)	(2.59)	(3.73)
Total Crime Night	81.18	86.84	80.21	81.71	6.625**	5.129
	(53.47)	(48.83)	(54.18)	(42.44)	(2.61)	(3.88)
Total Crime	169.29	162.26	170.50	167.40	-8.239*	-5.143
	(100.68)	(90.75)	(102.25)	(83.34)	(4.92)	(7.31)
Nº of Months	3,360	490	2,870	210	3,360	700
Nº of Districts	96	14	82	20	96	20

Notes: Columns (1) to (4) depict the mean coefficients with standard deviation in parenthesis for all the different types of crimes and the time of the day they were committed. Columns (5) and (6) show mean differences from respectively columns (2) and (3) and columns (2) and (4) with standard errors in parenthesis. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table A1.2: Summary Statistics on Daytime Crimes Prior to Announcement of the PLNB

	(1) All Districts	(2) Treated Districts	(3) Non-Treated Districts	(4) Would Be Treated	(5) (2)-(3)	(6) (2)-(4)
Cell Phone Thefts Day	27.46	14.68	29.64	15.49	-14.96***	-0.812
	(29.45)	(10.14)	(31.07)	(10.73)	(1.56)	(0.94)
Vehicle Thefts Day	25.03	14.52	26.82	20.02	-12.30***	-5.498***
	(19.22)	(11.96)	(19.65)	(24.37)	(1.01)	(1.51)
Cell Phone Robberies Day	19.07	20.37	18.85	19.31	1.513	1.057
	(17.32)	(17.51)	(17.28)	(14.04)	(0.93)	(1.50)
Vehicle Robberies Day	14.79	23.40	13.32	27.50	10.08***	-4.103**
	(14.30)	(17.75)	(13.07)	(22.60)	(0.74)	(1.75)
Non-Homicide Crimes Day	84.58	71.27	86.86	80.84	-15.59***	-9.571**
	(51.09)	(42.40)	(52.10)	(45.06)	(2.73)	(3.92)
Homicides Day	0.39	0.71	0.34	0.79	0.376***	-0.0731
	(0.91)	(1.18)	(0.84)	(1.44)	(0.05)	(0.11)
Total Crime Day	84.91	71.90	87.13	81.51	-15.23***	-9.608**
	(51.09)	(42.52)	(52.10)	(45.28)	(2.73)	(3.93)
Nº of Months	2,784	406	2,378	174	2,784	580
Nº of Districts	96	14	82	20	96	20

Notes: Data on the table is presented on a month level and the period ranges from Janury 2013 to May 2015. Columns (1) to (4) of Table 1 depict the mean of monthly number of each category of crime committed per district on the period of the day with standard deviation in parenthesis. Columns (4) and (5) shows mean difference from respectively columns (2) and (3) and (2) and (4) h standard errors in parenthesis.Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table A1.3: Frequency of Months With Zero Crimes Registered at Night

	(1) Total Crime	(2) Non-Homicide Crimes	(3) Cell Phone Robbery	(4) Cell Phone Theft	(5) Vehicle Robbery	(6) Vehicle Theft	(7) Homicide
Not Zeros	99.65	99.58	88.11	98.28	95.82	98.68	30.24
Zeros	0.35	0.42	11.89	1.72	4.18	1.32	69.76
Total	100.00	100.00	100.00	100.00	100.00	100.00	100.00
Nº of Months	5,760	5,760	5,760	5,760	5,760	5,760	5,760

Figure A1.1: Kernel Densities by Crime and Period of the Day.

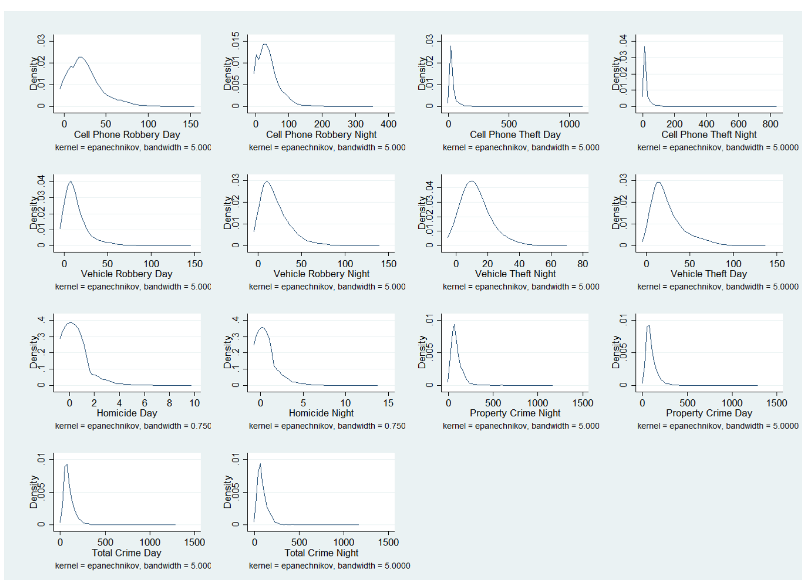


Figure A1.2: Total Homicides by Groups by Data sets

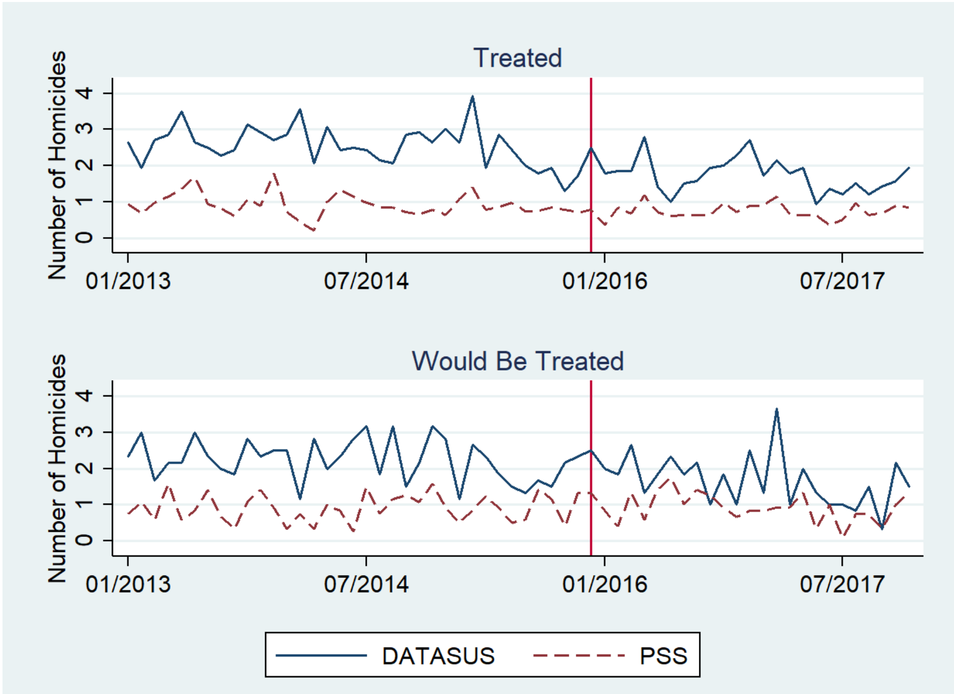


Figure A1.3: Mean of Night Crimes Over Time by Group

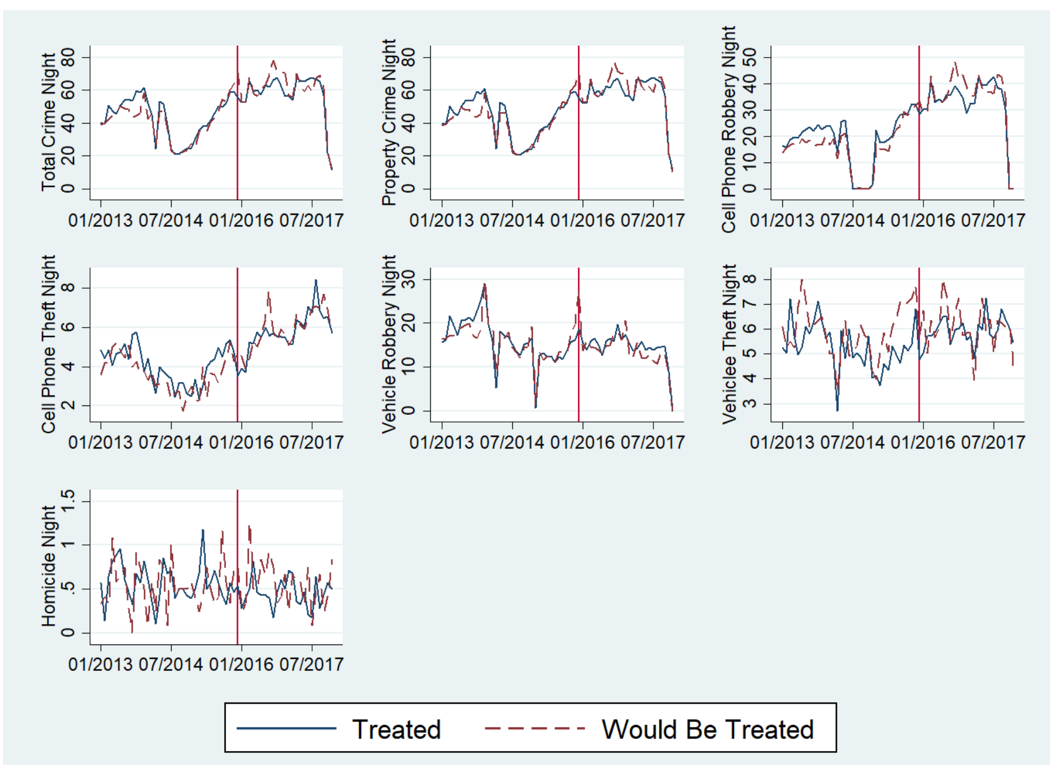


Figure A1.4: Mean of Night Crimes Over Time by Group

Chapter 2

Impacts of Forced Immigration: The Venezuelan *Diaspora* and the Brazilian Labor Market

This paper estimates the impact of immigrant influxes on local labor markets, taking advantage of a natural experiment generated by a massive wave of forced migration of Venezuelans to Roraima, a state in northern Brazil, from 2016 onward. In 2018 alone, Venezuelan immigrants increased Roraima's population by at least 8%. Estimates indicate that a 1 percentage point increase in the share of migrants in the state population due to immigration resulted in 1.1% higher earnings for native workers in the formal sector, 0.3% fewer hours worked weekly, and 2.3% higher wages. All categories of workers experienced increases in earnings and wages due to migration, except those in agriculture and fishing. Two mechanisms seem to explain these findings: the higher demand for goods and services created by the massive entry of Venezuelan migrants, and a wage premium for natives. Instead of affecting earnings and wages, the intensification of competition for jobs in the state is expressed in fewer hours worked and increased dismissals, with a 0.4% higher probability of termination. Results are confirmed by employing a Synthetic Control Method.

Keywords: Natural Experiment; Venezuela; Brazil; Wages; Migration; Native workers; Synthetic Control Method.

2.1 Introduction

The topics of immigration and forced migration have received increasing attention in the academic and policy-making communities in recent years. War and economic hardship have displaced millions of individuals around the world; according to the "Independent", 44,500 refugees seek asylum daily, and the total number of people planning to immigrate has reached 68.5 million (Sommerlad, June 20, 2018). The impacts of immigration have been studied more extensively in the United States and Europe due to their status as major immigrant destinations. However, many immigrants choose to settle in other nations, especially those nearby to their home countries, possibly constrained by limited resources and lack of knowledge of opportunities further afield. The present study examines the impacts of a recent massive influx of immigration from Venezuela to northern Brazil.

A key area of study in the immigration literature is the effects on native labor markets. Economists have developed several strategies in an attempt to causally and accurately estimate the impact of migrants on job market outcomes of locals since the 1990s[1]. Effects have been found to vary according to the skill and educational composition of the migrants compared to the native population, although it is not obvious that the two groups will compete for the same jobs (Altonji and Card, 1991). Short run impacts may also differ from long-run impacts, as over time population changes increase the demand for food, services and other economic activities, pushing local business and industries to increase their demand for labor (Altonji and Card, 1991; Jeaeger, 1996).

A recent set of studies uses natural experiments to estimate the effect of immigration on native labor markets. One of the most famous events studied is the "Mariel boatlift", a mass emigration of Cubans who traveled from Cuba's Mariel Harbor to the United States between April 15th and October 31st of 1980. Migration increased the Miami labor force by 7%, and because the immigrants were relatively unskilled, less skilled occupations and industries had the highest percentage increase in labor supply (Card, 1990). Although early studies showed that the city of Miami was able to rapidly absorb the increase in labor supply, and hence there was no impact on native wages or on unemployment rates (Card, 1990), later work revisiting this natural experiment provides evidence that in fact the wage of high school less than high school in Miami in fact dropped by 10% to 30% (Borjas, 2017).

In South America, the issue of local labor market impacts of immigration has recently come to attention because of the Venezuelan *diaspora*. Its effects have been studied in Colombia (Caruso, Canon, and Mueller, 2019; Bahar, Ibáñez, and Rozo, 2021; Bonilla-Mejía

[1]Early studies of the impacts of immigration on labor markets relied on spatial variation in immigrant inflows as a source of identification. However, some argue that this approach suffers from endogeneity, as migrants tend to go to places with better job opportunities and higher wages. One attempt to alleviate this problem is to use past share of immigrants as an instrumental variable for current immigration (Altonji and Card, 1991). Yet, the biggest disadvantage of this method is that the share of historical immigrants is fixed, preventing the inclusion of controls to address potential sources of local heterogeneity (e.g. city fixed effects). An alternative method developed was the "shift-share IV". The main goal was to generate variation at the local level by exploiting variation in national inflows (Card, 2001; Card, 2009). However, the use of this instrument has been criticized due to likelihood of producing less negative results as compared to other approaches (David A. Jeaeger and Stuhler, 2018) and changing directions of impact across different time periods within the same country (Borjas, 1999).

et al., 2020; Santamaria, 2020)[2], Ecuador (Olivieri et al., 2020b; Olivieri et al., 2020a)[3], and Brazil (Ryu and Paudel, 2021). From 2016 to 2018, over 62,000 Venezuelans sought refugee status in Roraima State, Brazil[4], increasing its population by more than 8% in 2018 alone. This massive inflow of immigrants into northern Brazil is a consequence of the recent deep political and economic crisis in Venezuela.

Using a Synthetic Control Method (SCM) followed by a Differences-in-Differences strategy, Ryu and Paudel, 2021 find that, although the Venezuelan migration lowered labor force participation and employment rate, it had no significant impact on hourly wages in Roraima State, Brazil. However, results could be explained by the fact that the authors use a dataset that does not allow to differentiate migrants from natives[5]. The current paper improves upon that work estimating the effects of the Venezuelan *diaspora* to Brazil using both a natural experiment that exploits the actual variability in the number of migrants who arrived in Roraima State, and a SCM that uses information from the other 25 states plus the Federal District to construct a "Synthetic Roraima" that confirms the findings. Most importantly, the analysis is performed using microdata from an administrative company registry ("Relação Anual de Informações Sociais" - RAIS), a data set that allows the differentiation of natives from migrants.

I find that a one percentage point increase in the share of migrants in the state population caused by the massive influx of Venezuelans to Roraima State increased monthly earnings and hourly wages of natives, respectively, by 1.1% ($p < 0.01$) and 2.3% ($p < 0.01$). However, I also find that, as a result, natives work 0.3% ($p < 0.01$) fewer hours per week and are 0.4% ($p < 0.01$) more likely to be terminated from employment. Upon desegregating effects by level of education and occupation, I find evidence of heterogeneous responses across groups, with workers in lower-skilled occupations such as agriculture and fishing experiencing decreases in earnings and wages, and also that the probability of termination only increases for men.

This research aims to put South America in the spotlight in the immigration literature by providing evidence on how the forced migration crisis in Venezuela affects the local labor market of another less-developed country. The rest of the paper is organized as follows: Section 2 provides a historical background of the events in Venezuela that generated the conditions for immigration to Brazil, Section 3 presents the data and the estimation strategy;

[2]Employing an instrumental variable approach, Caruso, Canon, and Mueller, 2019 find that a 1 percentage point increase in immigration from Venezuela reduces the wages of the informal sector in the country by 10 percentage points in urban areas. However, other recent studies do not find any effect on the labor market outcomes in Colombia

[3]These studies find that not only have young, low-educated Ecuadorian workers in high-inflow regions experienced increases in informality (5%) and a 13% decrease in earnings compared to native workers with similar characteristics located areas with low or non influxes of Venezuelans, but that Venezuelans are experiencing significant occupational downgrading relative to their employment prior to emigration to Ecuador.

[4]This is likely a lower bound on the actual number of people who migrated to northern Brazil at this time. According to the Country's Ministry of Interior, at least 199,365 Venezuelans passed through the area in 2017 and 2018, but not all remained in northern Brazil, and others entered the country without being detected

[5]The literature has shown that immigrants tend to earn lower wages upon arrival than locals.

Section 4 discusses the main results, and Section 5 concludes.

2.2 The Venezuelan *Diaspora*

The twenty-first century did not begin peacefully in Venezuela. Coming from a series of coup attempts in the 1990's, the country elected Hugo Chavez as president in 1998, who was himself the leader of a 1992 coup attempt. Chavez was elected on a platform to bring a "Bolivarian Revolution" to the country, with the implementation of a new constitution aimed at implementing socialist and populist economic and social policies. To do this, the president would take advantage of high oil prices[6]. In 2001 alone, Chavez was able to pass 49 laws in Congress that aimed to reallocate land and wealth.

However, in response to a 2003 coup attempt, Chavez increased media controls[7], and expropriated companies (including Exxon Mobil and ConocoPhilips), among other political actions, raising local and international concern. As oil prices dropped in 2010, the country's economic problems intensified. In an attempt to control high inflation (21.07% monthly), in 2012 he promoted a control on the prices of basic goods such as fruit juice, toothpaste, disposable diapers, beef, milk, and corn (Neuman, 2012), and suggested that companies that did not follow the controls would be expropriated.

In April 2013, Nicolas Maduro was elected as presidential successor after Chavez died of cancer. The inflation rate was reaching 43% monthly, the price of oil again started to drop significantly[8], and political opposition grew. At the end of 2014, inflation had reached more than 50%. The government announced cuts in public spending, and the social programs started by Chavez were put at risk. In 2016, the government increased petrol prices for the first time in 20 years. Unrest and discontent led to violent protests that killed several Venezuelans in the second half of 2016 and 2017[9].

In 2018, "the UN warned of a migration 'crisis', estimating that economic woes and food and medical shortages [had] caused more than two million Venezuelans to leave their country since 2014. Most [were] settling in nearby Peru, Ecuador, Colombia and Brazil, leading to tensions in the region" (BBC, 2018). Venezuelans who lived near the Brazilian border were migrating to the neighboring country largely on foot (Governo do Brasil, 2020). On the other side of the country, Venezuelans were migrating to neighboring Colombia. Almost 1.3 million Venezuelan migrants were registered in Colombia in June 2019, as well as 768,000

[6] According to OPEC website, Venezuela's oil revenues account for about 98% of export earnings (OPEC, 2018).

[7] In 2007, the government refused to renew the terrestrial broadcasting license of RCTV channel, a news channel critical of the President, causing massive protests and strong international condemnation.

[8] See Figure A3.1 for the relationship between oil prices and Venezuelan seeking refugee status in Brazil.

[9] In September 2016, hundreds of thousands of people participated in a protest in Caracas calling for the removal of President Maduro, accusing him of responsibility for the economic crisis. In 2017, several people died in confrontations with security forces during mass protests demanding early presidential elections and the revoking of a planned constituent assembly to replace the National Assembly.

in Peru, 288,000 in Chile, 263,000 in Ecuador, and 130,000 in Argentina (IOM, June 7, 2019)[10]. However, the latter countries do not share borders with Venezuela (see Figure 3.3), and migrants go to these destinations by plane or first enter Colombia or Brazil. Other countries, including Mexico, the United States, and Spain, also host significant numbers of refugees and migrants from Venezuela.

Figure 2.1: South American Countries Receiving Venezuelan Migrants

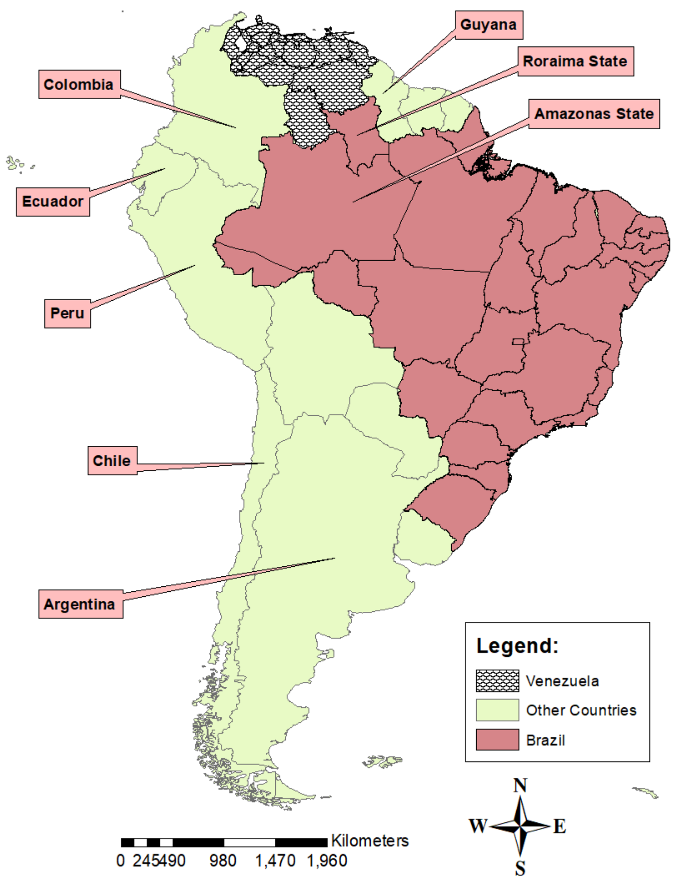


Figure 2.2 reports the number of refugee requests by Venezuelans in Brazil over time (Federal Police, 2019). These figures are likely a lower bound on the total number of people crossing the border, since some do not register their presence or request refugee status. However, Figure 2.2 shows that although there is some evidence of an increase in 2015 and 2016[11], there is a clear massive influx of Venezuelan refugees starting in 2017.

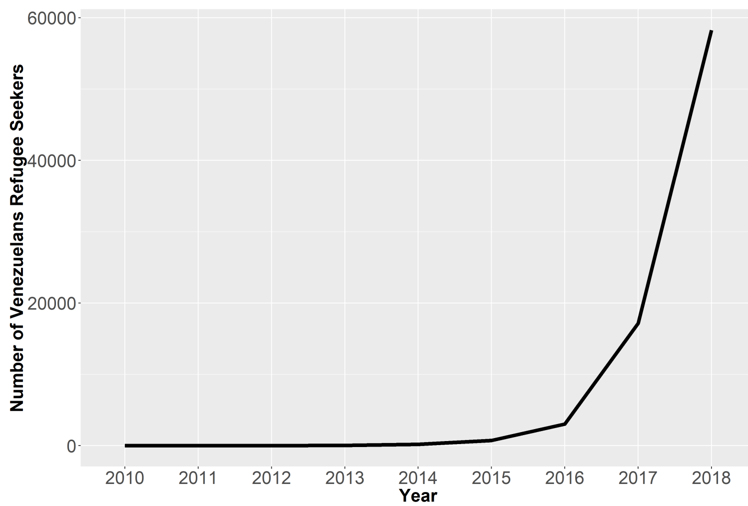
The northern state of Roraima is by far the main destination of Venezuelans who enter Brazil (see Figure 3.1), especially for those traveling on foot (Jornal Nacional, 2019; Marchao,

[10]Although Guyana also shares a border with Venezuela, "populated areas in Venezuela are far removed from urban centers in Guyana, saving the country from a mass invasion of people seeking help. (Caribbean Life, March 5, 2019)."

[11]It is hard to identify the "tipping point" for more intense migration in 2016. In that year, the petrol barrel reached its lowest price in more than a decade, at $34.7. See Figure A3.2 in the Appendix.

2019; Governo do Brasil, 2018). A considerable number of Venezuelans eventually go on to settle in the state of Amazonas, one of the main industrial centers of the country; however, they are first entering the country through Roraima and only then (often after some delay) trying to reach Amazonas[12].

Figure 2.2: Number of Refugee Requests by Venezuelans, 2010-2018



Source: Brazilian Federal Police (2019)

According to the 2010 census, Roraima is 13th, among 27 states plus the Federal District, in the Human Development Index ranking in Brazil. With a population of over half a million inhabitants in 2018, it averaged a nominal household income per capita of approximately \$310.71 monthly[13] (IBGE, 2020). Venezuelans cross the border in the city of Pacaraima (see Figure 3.2), with a population of 15,580 inhabitants in 2018. Monthly earnings on the city average \$411.07[14] (1.7 the minimum salary in Brazil) and 46.5% of the population earn up to half the minimum salary (\$120.90 monthly)[15].

From 2017 through October 2018, 176,259 Venezuelans migrated to Brazil through Pacaraima (Ministério da Casa Civil, 2018). This number accounts for 14.78% of the population of Roraima, 22.7% of the population of the capital city of Boa Vista, and incredibly 1,131.32% of the population of Pacaraima town. More than half of these immigrants stayed in Brazil, many in Pacaraima or Boa Vista. According to a survey conducted with Venezuelan immigrants in the cities of Boa Vista and Pacaraima between January 25th and March 8th of 2018 (DTM, April 30, 2018), 23% of them came from Bolvar, the closest Venezuelan state to the Brazilian border, 24% from Monagas, and 28% from Anzoategui[16] (see Figure

[12] Although Amazonas also borders Venezuela, the Amazon Forest makes crossing this part of the border an almost impossible task. The only road (Troncal 10 in Venezuela and BR-174 in Brazil) connecting both countries is the one that goes from Santa Elena de Uairén (Venezuela) to Pacaraima (Roraima, Brazil).

[13] 2018 values.

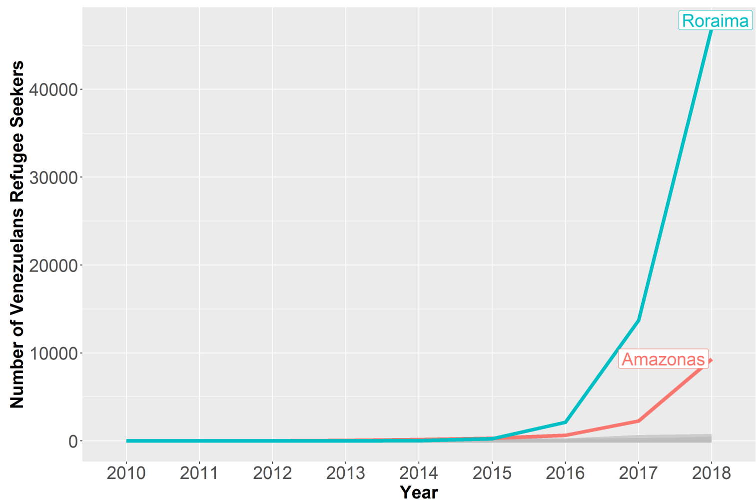
[14] 2017 values.

[15] *Ibdem.*

[16] Although another Venezuelan state also borders the west of Roraima, the Amazon Forest makes crossing

3.2). These three states account for 75% of the Venezuelan migrants surveyed. From the sample of 3,516 interviews (2,420 in Boa Vista and 1,096 in Pacaraima), the main reported reasons for migration from Venezuela to Brazil were economic reasons (job) (67%), lack of access to food and medical services (22%) and escape from violence (7%). Furthermore, 42% reported that they would face hunger if they returned to Venezuela, 32% said that they would not have employment, and about 1% feared persecution.

Figure 2.3: Number of Venezuelans Refugee Requests by State, 2010-2018



Source: Brazilian Federal Police (2019)

Venezuelan immigrants look either to stay in Brazil, or use the country as a route to go elsewhere. The same survey conducted by DTM reported that 48% of the interviewees wanted to stay in Brazil, while 52% sought to eventually continue to another country (primarily Argentina). Among those who wanted to stay, 22% wanted to stay in Roraima, while 59% wanted to go to Amazonas state. Among migrants, 12% had only primary education, 50% had a secondary level education, 28% had a college degree, and 1% had a post-graduate degree[17]. Of the sample, 58% of the interviewees were men, while 41% were women. Approximately 57% reported being unemployed[18]. Among the employed, 33% worked in the service sector, 31% in commerce, and 13% in construction. Finally, 83% earned less than the Brazilian minimum salary at the time of survey.

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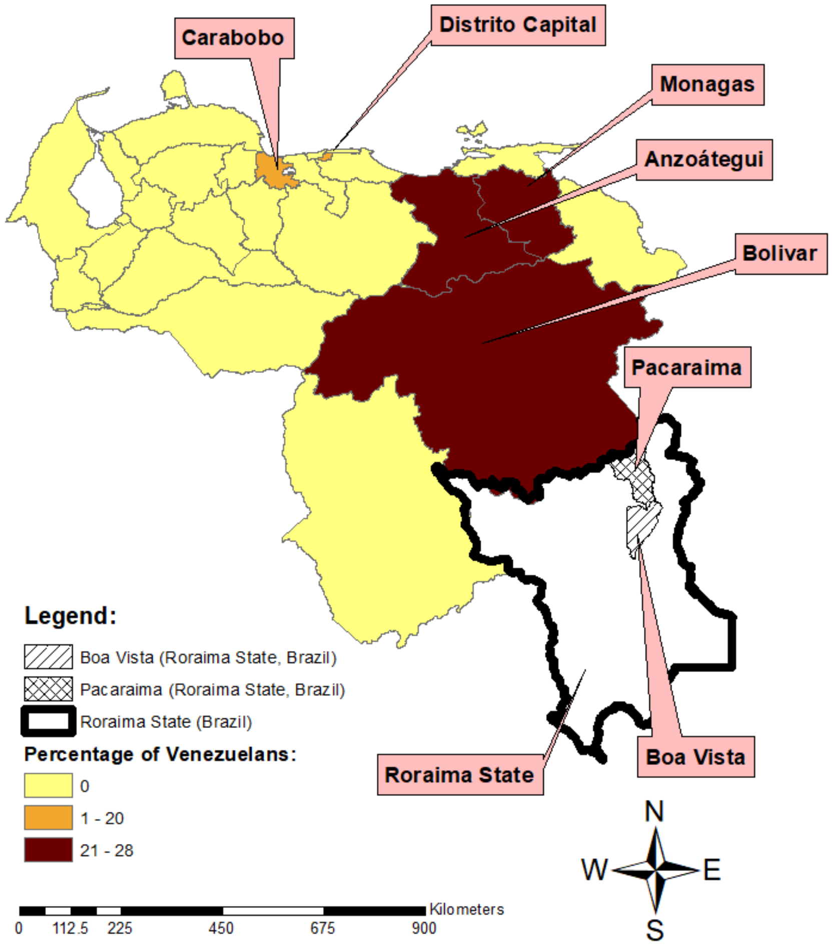
this part of the border an almost impossible task. See Footnote 9 for more information

[17]Fewer than 1% reported no schooling.

[18]Note that he distribution of schooling levels among the unemployed is extremely similar to the full sample.

Figure 2.4: States of Origin of Venezuelans Who Migrate to Roraima through Pacaraima



Source: DTM (2018) (DTM, April 30, 2018)

Given the forced nature of Venezuelan migration to Roraima starting in 2017, as evidenced by their relatively close origins to the Venezuela-Brazilian border, their method of crossing the border often on foot, and their stated reasons for immigration, it is possible to argue that most Venezuelans migrating to Roraima do so because they do not see any other alternative. Thus, in my analysis, this phenomenon is treated as a natural experiment.

2.3 Data and Estimation Strategy

2.3.1 Data

In this investigation, two main sources of data were used: data on formal labor market outcomes and social characteristics of workers in Brazil and data related to Venezuelan migration to the country. I explain both data sources in detail here.

Brazilian Labor Market Outcomes and Workers Characteristics

Data that describe the characteristics of formal workers in Roraima were extracted from *Relação Anual de Informações Sociais* (RAIS), an administrative registry created to meet the control, statistics, and information needs of government entities[19]. Because companies have a legal obligation to report annually to RAIS, the database provides information on every worker in the formal sector in Brazil[20], and can be desegregated by state and city levels. The variables contained in the data set include, among others, an indicator of whether the employee is employed at December 31st, as well as hiring and dismissals[21] according to gender, age group, education level, length of service and earnings, broken down into occupational, geographic, and sectoral levels. The database contains information on the number of employees in a company, their weekly hours worked, and nationality.

Table 2.1: Summary statistics Roraima, 2012-2015

	(1) Brazilians	(2) Venezuelans	(3) Other Nationalities	(4) (2)-(1)	(5) (2)-(3)
Real Earnings (in R$)	1,602.50	947.93	2,468.11	-654.56***	-1,520.17***
	(2,158.79)	(897.04)	(3,092.50)	(-5.14)	(-8.21)
Weekly Wkd. Hours	39.60	41.23	39.98	1.63***	1.25**
	(6.80)	(6.06)	(8.21)	(4.06)	(2.36)
Real Wage (in R$/h)	12.00	6.29	20.30	-5.71***	-14.01***
	(29.16)	(9.37)	(37.46)	(-3.31)	(-6.26)
Months Employed	56.20	9.99	37.17	-46.21***	-27.18***
	(84.72)	(15.55)	(72.01)	(-9.24)	(-6.34)
Age	35.05	31.71	39.48	-3.33***	-7.77***
	(11.21)	(9.84)	(12.82)	(-5.04)	(-9.34)
Observations	525,031	287	818	525,318	1,105

Notes: The means presented in columns (1)-(3) are drawn from individual-level RAIS data from 2012-2015. Column (4) indicates the difference between column (2) (Venezuelans) and column (1) (Brazilians), with t-test in parenthesis. Column (6) indicates the difference between column (2) (Venezuelans) and column (3) (Migrants from Other Nationalities) with t-test in parenthesis.

Table 3.1 provides summary statistics of individual-level RAIS data for 2012-2015 (before intensification of Venezuelan migration to Roraima). The first three columns present

[19]Originally, RAIS was created to control the entry of foreign labor in Brazil and the records of collection and granting of benefits by the Ministry of Social Security.

[20]About 7.4 million establishments that filled RAIS in 2009.

[21]"Admission" means every entry of a worker into the establishment during the year, whatever its origin, and by "dismissal" during that year, every departure of a person whose employment relationship with the establishment ended during the year for any reason (dismissal, retirement, death), either on the initiative of the employer or the employee. Inflows and outflows through transfers are included, respectively, in admissions and dismissals.

mean values for Brazilians (1), Venezuelans (2), and migrants from other nationalities (3). Column (4) presents the difference between means for Venezuelans and Brazilians, and column (6) presents the difference between means for Venezuelans and migrants from other nationalities. First, it is possible to observe that, prior to the *diaspora*, the number of Venezuelans working in Roraima state in the formal sector is very low (287), indicating that there are not many Venezuelans working in the state or that most of them do not work in the formal sector. Second, Venezuelans earn 41% ($p < 0.01$) less than native workers and about 62% ($p < 0.01$) less than migrants from other countries. However, workers from Venezuela work about 4% more hours weekly than Brazilians ($p < 0.01$), and 3% more than migrants from other nationalities ($p < 0.05$). In other words, Venezuelans work about 6.5 and 5 hours more than Brazilians and other migrants monthly. Consequently, wages for Venezuelans are 46% ($p < 0.01$) lower than those for Brazilians and 69% ($p < 0.01$) lower than those of other migrants. Finally, Venezuelans average about 10 months employed, compared to 56 and 37 for Brazilians and other migrants respectively, and are 3 and 7 years younger than Brazilians and migrants from other countries.

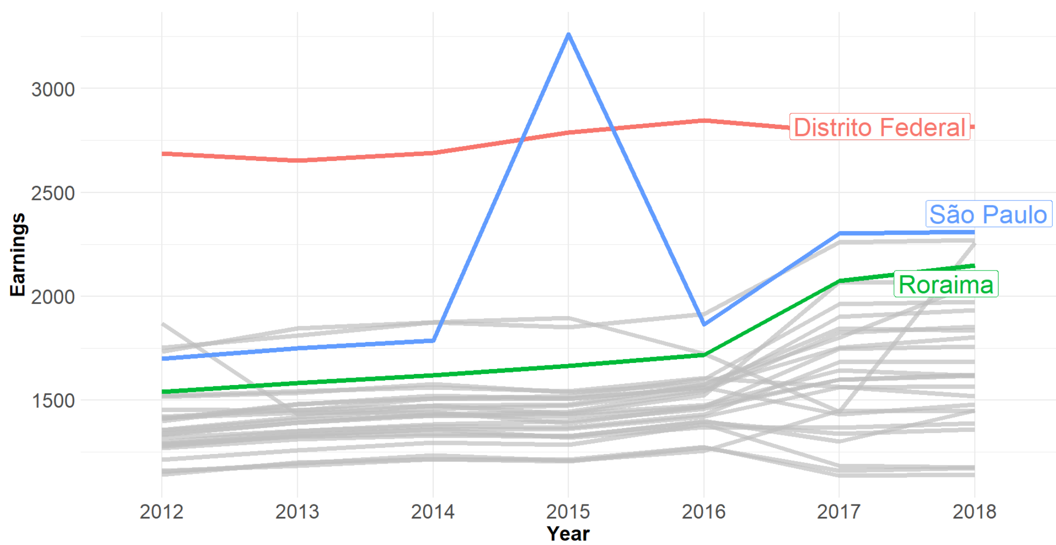
Table 2.2: Workers Characteristics in Roraima, 2012-2015

Characteristics	Brazilians	Venezuelans	Other Nationalities
Panel A: Social			
Gender:			
Female	47.8	34.15	33.99
Ethnicity:			
Asian	1.22	3.48	1.59
Black	1.21	1.05	13.45
Indigenous	0.35	0.00	0.00
Parda	38.54	55.40	32.76
White	8.34	13.94	11.98
Non Identified	11.41	24.74	15.16
Panel B: Education			
College or More	22.37	12.20	38.63
Some College	3.18	3.83	3.06
High School	53.15	70.73	40.22
Less Than High School	21.29	13.24	18.09
Panel C: Labor Market			
Occupational Sectors:			
Police/Army/Firefighter	1.09	0.00	0.00
Public Sector	6.34	6.62	7.09
Arts & Sciences	14.57	3.83	30.20
Agriculture/Fishing	1.29	0.70	0.73
Manufacturing	12.80	16.72	19.07
Management Services	25.33	20.21	16.75
Commerce	28.50	43.55	20.29
Repair/Maintenance	1.57	2.09	2.20
Mid-Level Technician	8.51	6.27	3.67
Individuals	525,031	287	818

Notes: Frequencies depicted in the table are drawn from individual-level RAIS data from 2012-2015.

Table 3.2 provides additional summary statistics on the social, educational and labor market characteristics of formal workers in Roraima. Panel A summarizes demographic information. While the Brazilian population is fairly divided between male and females, migrants (both from Venezuela and other countries) are primarily men. The majority of workers in Roraima self-reports as either “Parda”[22] or white, with 13.45% of migrants from other nationalities being black. Panel B shows that more than half of native workers have only a high school education, while about 22% have a college or more. Venezuelans working in Roraima, on the other hand, are primarily low-skilled, with more than 70% of them reporting only high school education and 13% reporting less than high school education. Migrants from other countries, however, are balanced between those with college or more and high school education (39% and 40% respectively). Finally, panel C shows that Venezuelans occupy primarily positions in manufacturing (17%), Management Services (20%) and commerce (40%), while migrants from other nationalities have 30% workers in Arts & Sciences, compared with 14.6% Brazilians and only 3.87% Venezuelans. Therefore, Table 3.2 shows that before the massive entry of migrants in 2016, Venezuelan workers were mainly less educated than Brazilians and migrants from other countries and were employed in low-skilled occupations in Roraima state.

Figure 2.5: Average Earnings by State, 2012-2018



Figures 3.4 and 3.5 show monthly earnings and weekly hours worked for native workers in the formal sector from 2012 to 2018 by state in Brazil. Figure 3.4 shows that, earnings grew over the study period across almost the country, and in 2018, Roraima is among the 5 states with highest average earnings in the country. The Brazilian capital, “Distrito Federal”, far exceeds every other state in earnings (excepts for São Paulo in 2015), likely due to the high concentration of higher-earning public jobs. In addition, Figure 3.5 shows that Roraima is one of the states where employees work fewer hours in the country. Finally, Figure 2.7

[22]“Parda” represents mixed-race, in particular a descent from black and white, black and indigenous or white and indigenous parents.

indicates that Roraima was 5th in the ranking of states with higher real wages for natives in the formal sector before 2016, but became the second state in terms of real wages in 2017 and remained so in 2018.

Figure 2.6: Average Worked Hours Weekly by State, 2012-2018

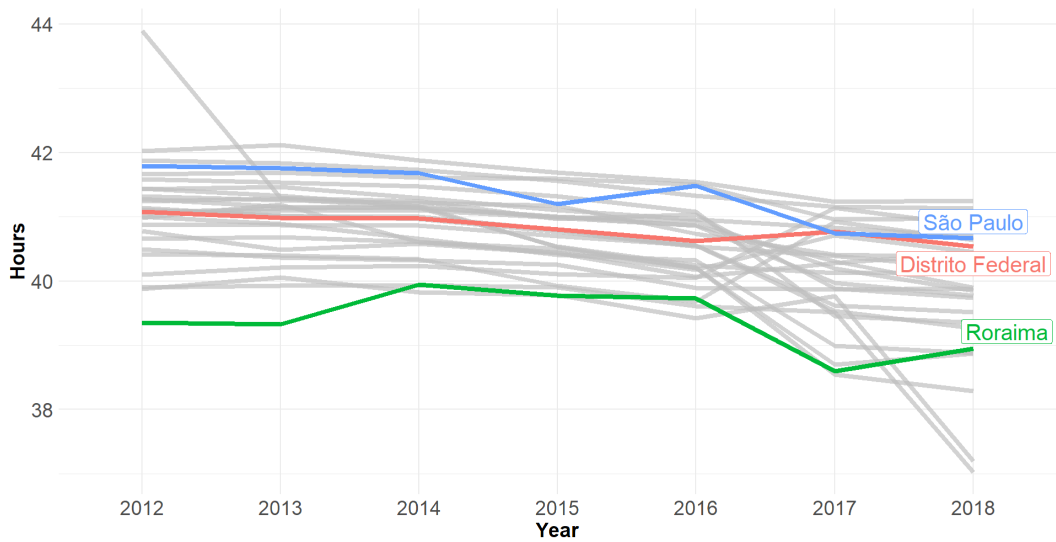
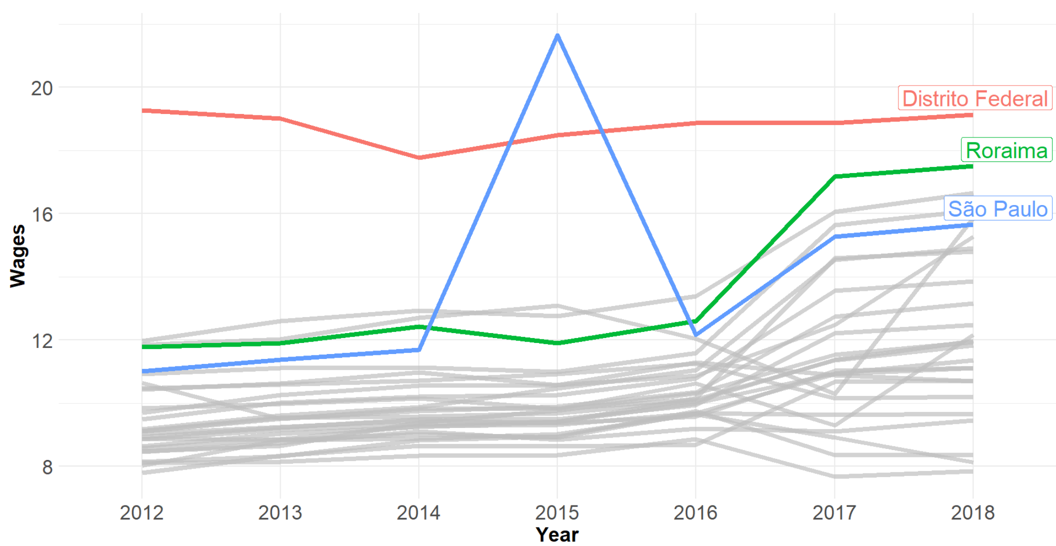
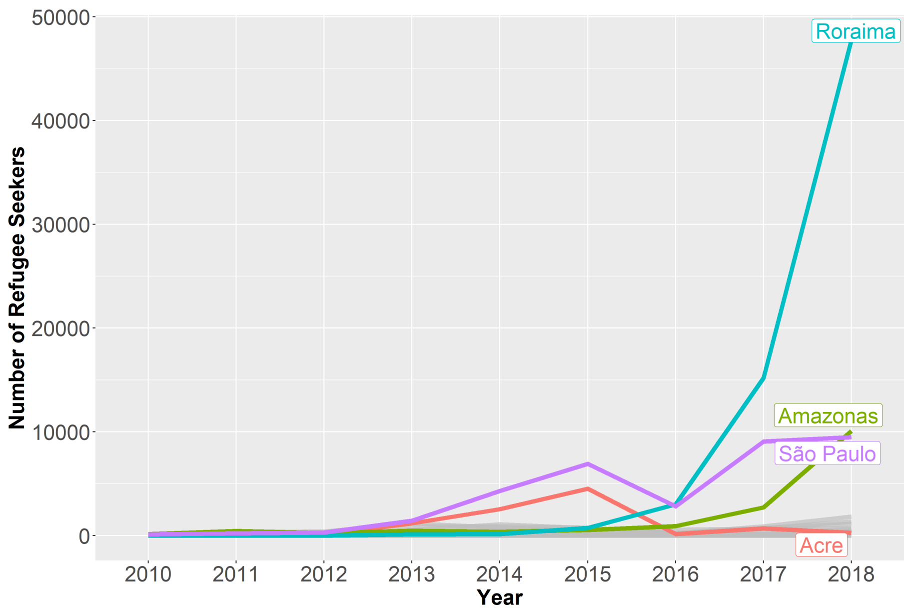


Figure 2.7: Average Real Wages by State, 2012-2018



Venezuelan Migrants

Data on the number of Venezuelans crossing the border to Brazil are derived from reports published by the Brazilian Federal Police and the "Ministério da Casa Civil". These two institutions are responsible for registering every person who enters the country, including details on their location, date of entry, nationality and other characteristics. However, despite all this effort by local authorities, it is still difficult to measure the exact number of people who migrate to the country and stay in Brazil, given the long border and the various ways people can cross it. I use the number of migrants who requested refugee status in each state during 2010-2018 as a proxy measure of the number of Venezuelans migrating to Brazil. The country has created mechanisms to facilitate the new life of migrants, including a temporary residence allowance for Venezuelans. In fact, "under Brazilian law, while their asylum requests are being processed, [Venezuelans] cannot be deported, are entitled to a work permit, and are allowed to enroll children in school" (Human Rights Watch, 2011).



Source: Federal Police

Figure 2.8: Number of Refugee Requests by State Over Time

Figure 2.9 shows the number of migrants who requested refugee status in Brazil of all nationalities. If we compare these numbers with Figure 3.1, it is clear that most of migrants requesting a refugee status in Brazil (if not all of them, particularly in 2017 and 2018) are Venezuelans, and are located in Roraima, followed by Amazonas or São Paulo. However, Roraima receives by far the highest number of Venezuelan migrants compared to the rest of the country.

2.3.2 Estimation Strategy

The natural experiment created by the massive inflow of migrants from Venezuela to Brazil lends itself to a straightforward estimation strategy. Regressions are performed on individual-level data, appropriately weighted to maintain population representativeness. The regression equation is as follows:

$$y_{itc} = \beta_0 + \beta_1 Immigrants_t + \beta_2 time + \beta_3 X_{itc} + \theta_t + \theta_c + \xi_{it}$$

where the dependent variable y is either log monthly earnings, log weekly hours worked, log wages[23] of individual i, year t, and city c, or an indicator that equals one if the worker has been terminated from the company; $time$ is a time trend that captures the evolution of the outcomes over time; $Immigrants_t$ is the percentage points increase of the share of Venezuelan migrants in Roraima[24] in year t; X_{itc} is a set of individual-level control variables including education, gender, ethnicity, hours worked[25] age, days off of work, company type, contract type, legal nature, type of hiring, size of company, occupation, time in employment, and indicators for disability and whether the employer was working in the company in the end of December of that year; θ_y and θ_c are year and city fixed effects and ξ_{iyc} is the error term. The coefficient of interest is β_1, which is the percentage change in the outcome of interest as a result of a one percentage point increase in the share of migrants in the state of Roraima population as a consequence of Venezuelan migration.

In what follows, I use this regression specification to provide estimates of the relationship between Venezuelan immigration and the labor market outcomes of individuals living only in Roraima state. However, it is important to highlight some of the features of this approach. Although migration to Roraima can be considered a natural experiment due to the bordering location of Roraima and the push factors from Venezuela previously described, migration further into Brazil is likely to be much more selected. Recall that a share of Venezuelans who migrated to Roraima, when asked about their final destination, answered that they ultimately desired to settle in Amazonas, a state that shares a border with Roraima and which is a manufacturing center in Brazil. Besides the selection problem with Venezuelans settling in Amazonas, migrants coming from countries other than Venezuela to states with better economic opportunities than Roraima, like São Paulo (See Figure 2.7), can also play an important role in results for the entire country. Thus, only specifications for Roraima should generate consistent and unbiased estimators, given the observed forced migration from Venezuela[26]. In addition, to alleviate remaining concerns about the fact that migrants can still select their destinations in the country, I use a Synthetic Control Method

[23] This variable is constructed by multiplying the number of weekly hours worked by 4 and dividing monthly earnings by this number.

[24] The variable is constructed as follow: $(\frac{\text{number of migrants in Roraima year } t}{\text{population of Roraima in year } t})*100$.

[25] Except when it is the dependent variable.

[26] It should also be noted that the Brazilian government, together with the army, started an operation named "Operação Acolhida" - *Operation Welcome* - as an answer to the massive migration of Venezuelans in the country. This program included the provision of financial aid to Roraima, as well as resettlement plans for some immigrants to other states in Brazil; together, these suggest that estimates from this analysis may be, in fact, a lower bound of the true effect of the Venezuelan migration. In addition, this operation did not choose randomly the ones who would be moved, nor their destination. It only moved those who wanted to and to the states/cities they chose to go. This would also create a non-randomness factor when dealing with the effect of migration in the entire country. However, the number of people transported to other states reached only 5,482 (OIM, 2019) (see Figure A2.3 in the Appendix), which would likely lead to a much more diffuse impact on other states.

(SCM) following Abadie, Diamond, and Hainmueller, 2010[27] taking advantage of the fact that migration intensified in 2016 only in Roraima. Thus, I use information on the other 25 states and the Federal District from 2012 to 2018 to construct a "Synthetic Roraima", which generates a better control group to compare with the trends of the outcome variables in Roraima state.

However, it could be the case that Venezuelans are leaving Roraima for other countries. If this is the case, the number of people in Roraima would not raise by as much as estimated, and using the number of refugee requests as a proxy variable for the actual number of migrants in the state could overestimate the impact of the "diaspora" on the labor market outcomes. This concern is addressed by Figure A2.4 in the Appendix. It shows the difference between the arrivals and departures of Venezuelans in the state through all border control posts (land borders, ports, and airports). It is possible to see that the net amount of Venezuelans in the state is highly positive during the study period. However, internal migration could be an issue if Venezuelans leave the state to other states in the country. An alternative way to measure it is to look at the number of Venezuelans working in different states. Figure A2.5 shows the number of Venezuelans working in formal jobs in each different state in the country[28]. Even though the number of migrants increase in some states such as Amazonas, Paraná, Santa Catarina, Rio Grande do Sul and São Paulo, they do not seem high enough that could invalidate the use of the number of refugee requests as a proxy variable[29]. Also, we should expect that most of them are Venezuelans transferred from Roraima by the *Operação Acolhida*.

2.4 Results and Discussion

This section is subdivided into four. First, the primary results are presented. The remaining subsections explore the heterogeneity of these results by education level, occupation group, gender, and ethnicity of the individual.

2.4.1 Primary Results

Table A3.1 reports the results of regressions using the specification described above. The coefficient of interest (β_1) is represented in the first row of the table.

The coefficient of interest in column (1) indicates that a 1 percentage point increase in the share of the migrants in the state population caused by the entry of Venezuelans

[27] Fore more information on the SCM, see Abadie and Gardeazabal, 2003 and Abadie, Diamond, and Hainmueller, 2015.

[28] It still could be the case that Venezuelans who are leaving Roraima are not in the formal sector. However, given the financial situation of those who entered the country through Roraima, it is hard to expect that a high number of migrants are able to leave the state of entry to other states.

[29] Although it is still possible that Venezuelans are departing the Roraima to other states through land.

increases the monthly earnings of native formal workers by 1.1% ($p < 0.01$). Therefore, if we consider that during 2018 alone 46,974 Venezuelans requested refugee status in Roraima[30], which represents a 8.14% increase in the population of the state, monthly earnings increased by 8.95% for natives in the formal sector due to Venezuelan immigration in that year. This represents an increase of approximately $R\$143.50$ on average for native workers in the state[31]. Column (2) of Table A3.1 estimates the impact of the Venezuelan immigration on weekly hours worked. In Roraima, a 1 percentage point increase in the share of migrants in the state population due to Venezuelan migration reduces weekly working hours by 0.3% ($p < 0.01$) - approximately 28.5 minutes per month. Finally, column (3) depicts the results when the dependent variable is logarithmic wages. In Roraima, wages increase by about 2.3% ($p < 0.01$) in response to a 1 percentage point increase in the share of migrants in the state population resulting from Venezuelan migration. However, column (4) shows that natives are 0.4% ($p < 0.01$) more likely to be terminated from employment.

These results raise important findings. First, one possible explanation for the higher earnings and wages is that the increased demand for goods and services created by the massive entry of Venezuelans into the state could have generated additional benefits for native workers. If this is true, the results show that this effect overcame the one created by the competition for jobs in the formal sector[32]. Second, this competition for jobs does not cause lower earnings/wages for Brazilians, but results in terminations and lower worked hours weekly. Third, there is a wage premium for native workers in the formal sector. Using average wages in the state, it implies that native wages in Roraima increased by about R$ 2.25 per hour due to Venezuelan immigration in 2018 alone (about $ 0.45)[33]. This third hypothesis is strengthened by the results shown Table 3.3, that indicate that newly hired native workers are earning 1.4% ($p < 0.01$) more, with 3.7% ($p < 0.01$) increase in wages, but are working about 1.2% (0.01) fewer hours. Although relevant and highly significant, these results can conceal important heterogeneity between subgroups of the population. The following sections investigate the heterogeneity between groups by education, occupation, gender, and ethnicity.

[30] These are the official numbers of people who actually requested the refugee status, rather than the total number who entered the country. As already highlighted, it is hard to measure with precision the exact number of people who crossed the border and actually stayed in the country or in Roraima. Because of that, these results are likely a lower bound of the true effect.

[31] This calculation takes into account the average of R$ 1,602.50 for real earnings, as shown in Table 3.1.

[32] Data shows that in 2018, the state economy became about 9% more diversified, compared to the period of 2010 to 2017. Between 2017 and 2018, Roraima stood out as the state with the highest increase in planted area (28.9%) in the country. In addition, from 2015 on, Roraima's seasonally adjusted index of retail sales is above the national level, with a growth trend over the entire period, but that spikes specifically in 2018. Finally, exports increased by 20% in the state from 2016 to 2019, and the primary destination of exports became Venezuela, with an increase in exports of US$ 84.3 million to the country from 2015 to 2019. (FGV DAPP, 2020)

[33] This calculation takes into account the average of R$ 12.00 for wage shown in Table 3.1 for Brazilians.

Table 2.3: The Effect of Venezuelan Migration on Formal Labor Market Outcomes of Native Workers in Roraima

	Earnings (1)	Hours (2)	Wages (3)	Pr[Termination] (4)
$(Migrants/Population) \times 100$	0.011***	-0.003***	0.023***	0.004***
	(0.001)	(0.000)	(0.001)	(0.001)
Yearly Time	0.007***	0.004***	-0.010***	-0.002***
	(0.001)	(0.000)	(0.001)	(0.001)
Asian = 1	-0.602***	-0.635***	-0.693***	0.075***
	(0.007)	(0.003)	(0.007)	(0.007)
Black = 1	-0.560***	-0.650***	-0.668***	0.064***
	(0.007)	(0.003)	(0.007)	(0.006)
Indigenous = 1	-0.606***	-0.615***	-0.700***	0.046***
	(0.007)	(0.004)	(0.008)	(0.008)
Parda = 1	-0.567***	-0.645***	-0.681***	0.090***
	(0.006)	(0.003)	(0.007)	(0.005)
White = 1	-0.512***	-0.675***	-0.618***	0.058***
	(0.006)	(0.003)	(0.007)	(0.005)
Male = 1	0.107***	-0.004***	0.090***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
Weekly Hours Worked	0.006***		-0.033***	-0.000***
	(0.000)		(0.000)	(0.000)
Months in Employment	0.002***	-0.000***	0.002***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)
Individuals	1,248,651	1,266,073	1,266,073	1,266,691
Adjusted-R^2	0.662	0.250	0.660	0.192

Notes: The set of control variables included in the estimation, but not depicted on the table, are education, age, days off of work, company type, contract type, legal nature, type of hiring, size of company, occupation, time in employment, and indicators for disability and whether the employer was working in the company in the end of December of that year. All specifications include year and city fixed effects. Robust standard errors are depicted in parenthesis. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

Table 2.4: Effects of Venezuelan Migration on Newly Hired Native Workers

	Earnings	Hours	Wages
	(1)	(2)	(3)
(Migrants/Population)*100	0.014***	-0.012***	0.037***
	(0.001)	(0.001)	(0.001)
Observations	408,053	412,122	412,122
Adjusted-R^2	0.408	0.213	0.366

Notes: The set of control variables included in the estimation, but not depicted on the table, are education, age, days off of work, company type, contract type, legal nature, type of hiring, size of company, occupation, time in employment, and indicator for disability. All specifications include year and city fixed effects. Robust standard errors are depicted in parenthesis. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively.

2.4.2 Results by Level of Education

Table 3.4 presents results from separate regressions for each education level. Column (1) suggests that, in Roraima, all categories of workers experienced increases in earnings as a result of Venezuelan immigration, with growth of 0.6% ($p < 0.1$), 1% ($p < 0.01$), 1.5% ($p < 0.01$), and 1.7% ($p < 0.01$) respectively to those with College Completion or More, Some College, High School Completion, and Less than High School. In Roraima, workers with at least some college are working about 2.6% more hours ($p < 0.01$), while those with less than high school are working 0.4% ($p < 0.01$) fewer hours in response to a 1 percentage point increase in the share of migrants in the state population.

Furthermore, column (3) sheds light on an interesting aspect. Although native workers with some college education earn more due to an increase in both hours worked and real wages by 2.8% ($p < 0.01$), those with high school education experience increases in earnings specifically due to increases in wages by 3% ($p < 0.01$). Additionally, the wages of those with less than high school education are increasing at the same rate as the earnings, even though the hours worked are decreasing. Finally, column (4) shows that although the probability of termination increases for all levels of education, it is higher in magnitude for those with complete High School education, who are 1% more likely to be terminated as a result of Venezuelan migration.

Therefore, the results suggest a wage premium for native workers as a result of Venezuelan migration regardless of their level of education. However, it also indicates that, although experiencing increases in wages, low and middle skilled workers are working fewer hours weekly, likely as a consequence of a higher supply of workers in the labor market that creates a pressure in the job market. The hypothesis is strengthened by the increase in the probability of termination for all groups, but specially for workers with only High School education. Even Venezuelans with higher education could compete for jobs with low middle-skilled due to the need, xenophobia, difficulty in the language, and other factors that make it difficult to settle in a new country.

Table 2.5: The Effects of Venezuelan Migration on Formal Labor Market Outcomes of Native Workers in Roraima - by Level of Education

	Earnings (1)	Hours (2)	Wages (3)	Pr[Termination] (4)
College Completion or More	0.006*	0.026***	-0.003	0.009**
	(0.003)	(0.002)	(0.004)	(0.003)
Some College	0.010***	0.026***	0.028***	0.002***
	(0.001)	(0.000)	(0.001)	(0.001)
High School Completion	0.015***	-0.000	0.030***	0.010***
	(0.001)	(0.001)	(0.002)	(0.001)
Less Than High School	0.017***	-0.004***	0.017***	0.005***
	(0.002)	(0.001)	(0.002)	(0.001)

Notes: Coefficients and standard errors shown are on (Migrants/Population)$\times$100. The set of control variables included in the estimation, but not depicted on the table, are gender, ethnicity, hours worked (except in column (2)) age, days off of work, company type, contract type, legal nature, type of hiring, size of company, occupation, time in employment, and indicators for disability and whether the employer was working in the company in the end of December of that year (except in column (4)). All specifications include year and city fixed effects. Robust standard errors are depicted in parenthesis. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively. Separate regressions a run for each level of education.

2.4.3 Results by Occupation

This subsection presents results segregated by occupation. Column (1) in Table 3.5 report the estimates for monthly earnings. It shows that in Roraima there are only two occupations that are negatively affected by the migration of Venezuelans: Army/Police/Firefighters, and Fishing/Agriculture. Respectively, earnings are dropping by 1.2% ($p < 0.01$), and 0.9% ($p < 0.05$). On the other hand, the earnings increase for natives in the Public Sector, Arts & Sciences, Management Services, and Commerce, respectively by 0.9% ($p < 0.01$), 0.5% ($p < 0.01$), 1.3% ($p < 0.01$) and 1.2% ($p < 0.01$). Column (2) shows that manufacturing workers are the only ones experiencing an increase in weekly working hours, 0.4% ($p < 0.01$). All other categories, except Army/Police/Firefighters and "Fishing/Agriculture", and Repair/Maintenance, are experiencing significant decreases in weekly working hours. Besides, column (3) shows that the group of workers most negatively affected by the migration in terms of wages are the ones who work in agriculture or fishing, with drops in wages by 1.2% ($p < 0.05$).

Finally, column (4) shows that workers in Arts & Sciences, Commerce and Mid-Level Technicians are less likely to be terminated, respectively, by 0.8% ($p < 0.01$), 0.5% ($p < 0.01$) and 0.8% ($p < 0.01$). On the other hand, those in the Public Sector, in Manufacturing and Management Services are, respectively, 3.7% ($p <$).01), 0.3% ($p < 0.1$) and 1.2% ($p < 0.01$)

more likely to be terminated. These results confirm the ones presented in Table 3.3, that the massive migration of Venezuelans generated a wage premium for natives, expect for those working in low-skilled occupations in agriculture and fishing. Thus, it less educated workers in more vulnerable sectors of the economy are experiencing competition for jobs and earning less specially due to reductions in wages both for Roraima.

Table 2.6: The Effects of Venezuelan Migration on Formal Labor Market Outcomes of Native Workers in Roraima - by Occupation

	Earnings	Hours	Wages	Pr[Termination]
	(1)	(2)	(3)	(4)
Army/Police/Firefighters	-0.012***	-0.000	-0.011***	0.006
	(0.004)	(0.000)	(0.003)	(0.004)
Public Sector	0.009***	-0.002***	0.013***	0.037***
	(0.003)	(0.001)	(0.003)	(0.002)
Arts & Sciences	0.005***	-0.007***	0.007***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.001)
Fishing/Agriculture	-0.009**	0.004	-0.012**	0.008
	(0.004)	(0.003)	(0.006)	(0.006)
Manufacturing	0.002	0.004***	0.039***	0.003*
	(0.002)	(0.001)	(0.002)	(0.002)
Management Services	0.013***	-0.002***	0.025***	0.012***
	(0.001)	(0.000)	(0.001)	(0.001)
Commerce	0.012***	-0.001***	0.023***	-0.005***
	(0.001)	(0.000)	(0.001)	(0.001)
Repair/Maintenance	0.006	-0.002	0.068***	0.004
	(0.005)	(0.002)	(0.007)	(0.005)
Mid-Level Technician	0.003	-0.012***	0.019***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.002)

Notes: Coefficients and standard errors shown are on (Migrants/Population) × 100. The set of control variables included in the estimation, but not depicted on the table, are gender, ethnicity, hours worked (except in column (2)) age, days off of work, company type, contract type, legal nature, type of hiring, size of company, education, time in employment, and indicators for disability and whether the employer was working in the company in the end of December of that year (except in column (4)). All specifications include year and city fixed effects. Robust standard errors are depicted in parenthesis. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively. Separate regressions a run for each occupation.

2.4.4 Results by Gender and Ethnicity

Table 3.6 separates the results by gender and ethnicity. Column (1) in panel A shows that the increase in earnings is higher for women than for men (respectively, 1.5% ($p < 0.01$) and 1% ($p < 0.01$)). Furthermore, columns (2) and (3) show that although women work fewer hours (0.5% ($p < 0.01$) compared to 0.01% ($p < 0.05$), the wage premium for men is higher (2.9% ($p < 0.01$) compared to 2.4% ($p < 0.01$). Panel B of the same table reports results by ethnicity in the state. *Parda* and Indigenous are earning respectively about 1.4% ($p < 0.01$) and 1.2% ($p < 0.05$) more per month for every 1 percentage point increase in the share of migrants in the state population as a result of Venezuelan migrants. In addition, native whites and *Parda* work respectively 3.6% ($p < 0.01$) and 0.2% ($p < 0.01$) fewer hours during the week, and are experiencing wage premiums of 3.6% ($p < 0.01$) and 4.2% ($p < 0.01$) respectively. Finally, column (4) shows that only men are affected in terms of increasing probability of termination. While it increases by 0.7% ($p < 0.01$) for males, it does not change for females. In terms of ethnicity, only Indigenous are affected, with a 3% ($p < 0.01$) lower probability of termination.

Table 2.7: The Effects of Venezuelan Migration on Formal Labor Market Outcomes of Native Workers in Roraima - by Gender and Ethnicity

	Earnings (1)	Hours (2)	Wages (3)	Pr[Termination] (4)
	Panel A: By Gender			
Male	0.010***	-0.001**	0.029***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Female	0.015***	-0.005***	0.024***	0.001
	(0.005)	(0.001)	(0.001)	(0.001)
	Panel B: By Ethnicity			
Black	0.003	-0.006	0.020***	0.002
	(0.006)	(0.004)	(0.008)	(0.005)
White	0.004	-0.013***	0.036***	(0.002)
	(0.003)	(0.003)	(0.004)	-0.000
Parda	0.014***	-0.002***	0.042***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Asian	-0.011	0.001	-0.003	-0.011
	(0.008)	(0.003)	(0.009)	(0.008)
Indigenous	0.012**	0.002	0.007	-0.030***
	(0.010)	(0.006)	(0.012)	(0.009)

Notes: Coefficients and standard errors shown are on (Migrants/Population) × 100. The set of control variables included in the estimation, but not depicted on the table, are gender (except in Panel A), ethnicity (except in Panel B), hours worked (except in column (2)) age, days off of work, company type, contract type, legal nature, type of hiring, size of company, occupation, time in employment, and indicators for disability and whether the employer was working in the company in the end of December of that year (except in column (4)). All specifications include year and city fixed effects. Robust standard errors are depicted in parenthesis. Superscripts *, ** and *** represent significance ate 10%, 5% and 1% respectively. Separate regressions a run for each gender and ethnicity.

2.5 Robustness Checks

Although Venezuelans are forced to migrate to Roraima, there may still be some selection of places that some of them chose as a destination. It could be the case that characteristics such as the level of employment, education, the past share of Venezuelans, among others, could lead migrants to choose Roraima as a place to live. If this is the case, the estimation could be affected by endogenous selection. To mitigate these concerns, I use a Synthetic Control Method (SCM) following Abadie, Diamond, and Hainmueller, 2010[34] that takes advantage of the fact that migration intensified in 2016 only in Roraima. To create a synthetic control group comparable to the state of Roraima, I generate a weighted combination of the other 25 states of the country plus the Federal District (Ryu and Paudel, 2021) using mean annual data of the following variables: age of workers, share of men, "Pardos", workers with at least a college degree, workers in manufacturing, commerce, and agriculture, companies with 1,000 or more employees, and values from 2012 to 2015 of the dependent variables[35].

Figure 2.9: Time series of synthetic Roraima versus Roraima

The trends for both the observed and synthetic groups are shown in Figure 2.9, while Figure 2.10 presents differences between both groups for each dependent variable. Both figures confirm the findings of the main estimation strategy. Earnings and wages in Roraima are especially following a similar trend compared to the synthetic control group, and as soon as the first wave of Venezuelans hit the state in 2016, both surged. Although noisier, the hour graph also confirms the drop in weekly working hours in 2016. However, the synthetic

[34]Fore more information on the SCM, see Abadie and Gardeazabal, 2003 and Abadie, Diamond, and Hainmueller, 2015.

[35]See figure A2.6 for the weights used in the pool of donor states

group predicts an even lower number of hours in 2018 compared to what is observed in Roraima. Thus, the SCM provides the same results as those found exploiting the variation of the number of refugee requests in the state of Roraima, strengthening the hypothesis that the Venezuelan migration was indeed a natural experiment.

Figure 2.10: Differences between Synthetic Roraima and Roraima

2.6 Conclusion

This article estimates the impacts of the migration of a huge number of Venezuelans to Roraima state, Brazil, from 2016 by exploiting the variation in the number of refugee requests from Venezuelans in the state. Due to the forced nature of migration as a result of a deep economic and political crisis in Venezuela, the phenomenon created a natural experiment. The results of the main estimate (and confirmed by the use of an SCM) indicate two possible effects. First, an increase in the demand for goods and services in the state. Secondly, the competition for jobs created by the influx of migrants. This is expressed mostly in two ways: a wage premium for natives, lower weekly hours worked, and a higher probability of dismissals.

The results suggest that, on average, a 1 percentage point increase in the share of migrants in Roraima population per year due to migration increases the monthly earnings and wages of native workers in the formal sector by 1.1% and 2.3%, respectively. On the other hand, natives work 0.3% fewer hours during the week and are 0.4%more likely to be terminated from employment.

“The effect of immigration on the wage structure depends crucially on the differences between the skill distributions of immigrants and natives. The direct effect of immigration is most likely to be felt by those workers who had similar capabilities" (Borjas, 2017). Recent surveys suggest that the wave of Venezuelan migrants arriving in Brazil is mainly made up of people with up to secondary education (62%), and therefore it should be expected that the main direct effects of the massive increase in the labor force will come from this specific group of the population. However, careful attention should be paid to the fact that even high-skilled migrants could be working in low-skilled jobs due to several reasons, such as not speaking the language, xenophobia, etc. Therefore, dividing the results by level of education and occupations proved to be more informative in a better understanding of the formal labor market in Roraima. Although earnings and wages are growing for all educational groups (except for wages for native workers with at least a college degree), only less than high school are working fewer hours due to the Venezuelan migration. Besides, workers with up to high school education are 1% more likely to be terminated from employment, the highest rate among the other groups.

In addition results desegregated by occupation provide a more in-depth analysis on how migration has affected natives in the formal sector in Roraima. Decreases in wages by 1.2% for workers in fishing and agriculture shows that this is the sector where competition manifests in terms of wages and not worked hours nor probability of termination. Finally, by looking at heterogeneity across gender and ethnic groups, I find that, although both males and female native workers are experiencing increases in both earnings and wages, only males are suffering with a 0.7% higher likelihood of dismissal. Besides, while wages increased for blacks (2%), whites (3.6%), and *pardos* (4.2%), only indigenous were affected in terms of the likelihood of dismissal (-3%).

This article adds to the literature in three aspects. First, it solves the problem of not differentiating nationals from foreigners, which was not solved in previous works. Second, it provides credible arguments that allow me to treat Venezuelan *diaspora* as a natural experiment and, because of that, result in a clear identification strategy. Finally, the remaining concerns of endogeneity are addressed using a synthetic control method. Although I attempt to attenuate this type of concern, the main limitation, however, is the impossibility of excluding the possibility of internal migration of both natives and Venezuelans. Future research should focus on efforts to mitigate these concerns.

Building on the literature studying the effects of migration on developed countries, this article redirects the focus to the importance of impacts in less developed countries. This is important not only due to their global economic and political importance, but also to shed light on an important phenomenon happening in the developing world.

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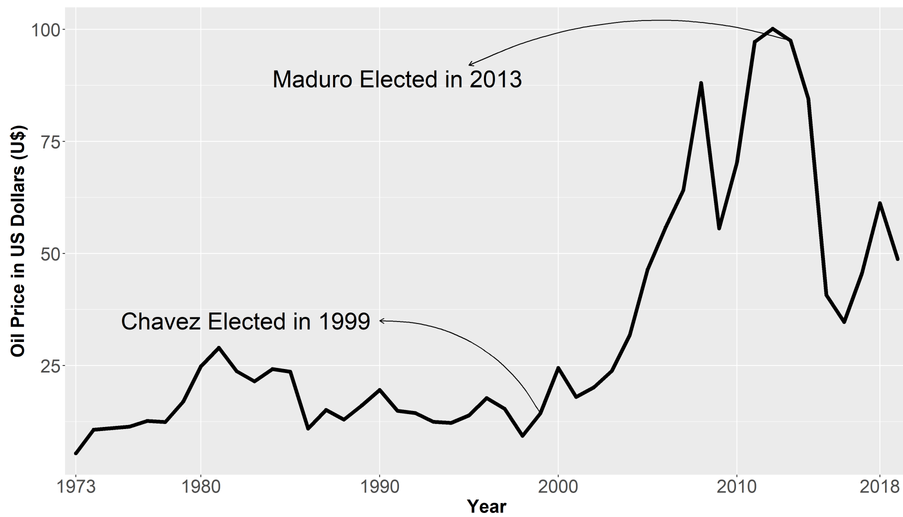
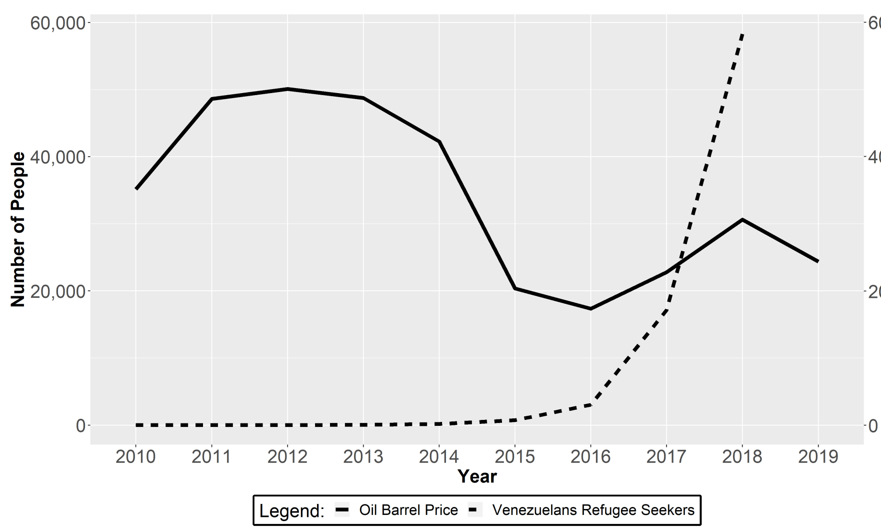
Appendix



Source: U.S Energy Information Administration

Figure A2.1: Costs of Venezuela Crude Oil (Dollars per Barrel)



Source: Brazilian Federal Police and U.S Energy Information Administration

Figure A2.2: Venezuela's Crude Oil Prices and Venezuelans Migrating to Brazil

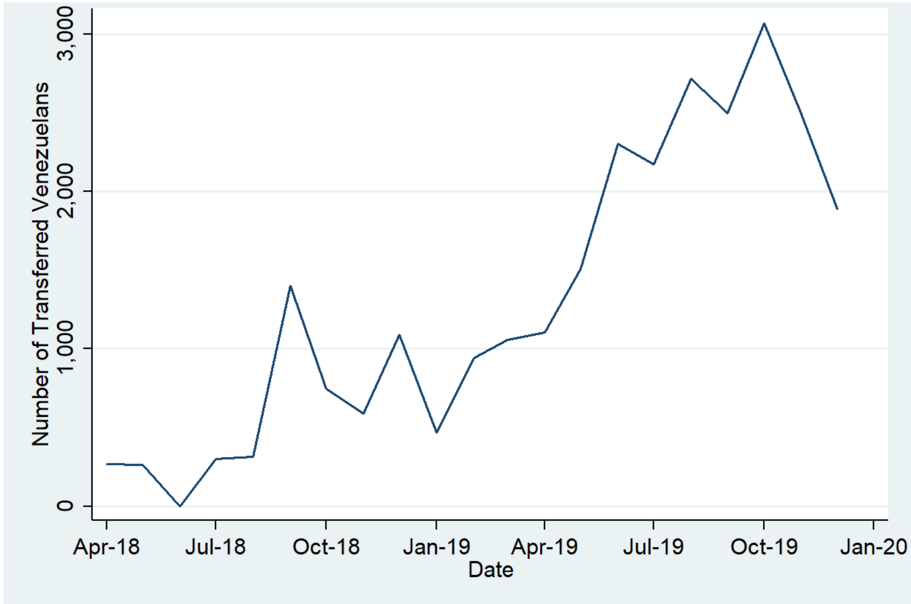


Figure A2.3: Number of Venezuelans Transferred from Roraima by Operação Acolhida, 2018 - 2019

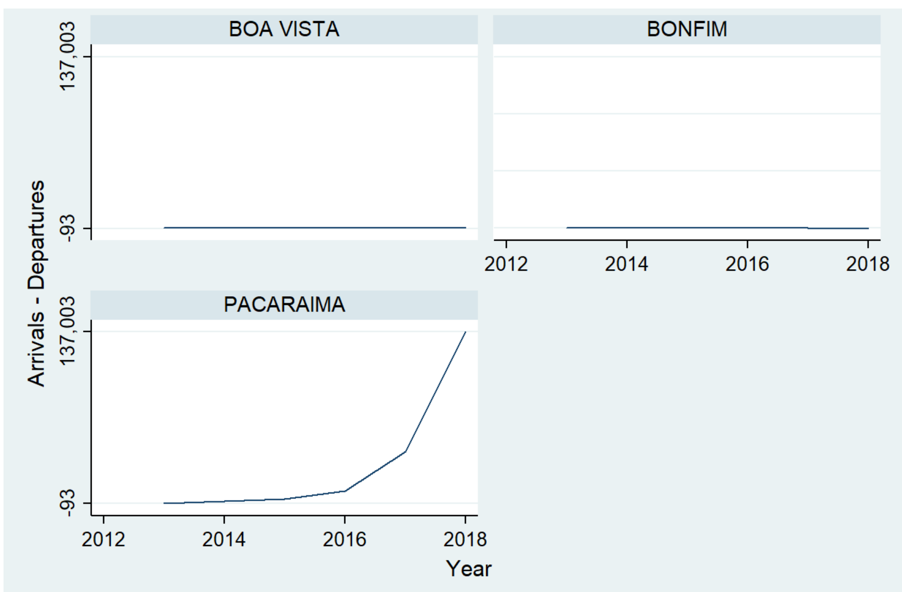


Figure A2.4: Net Amount of Venezuelans in Roraima, 2012 - 2018

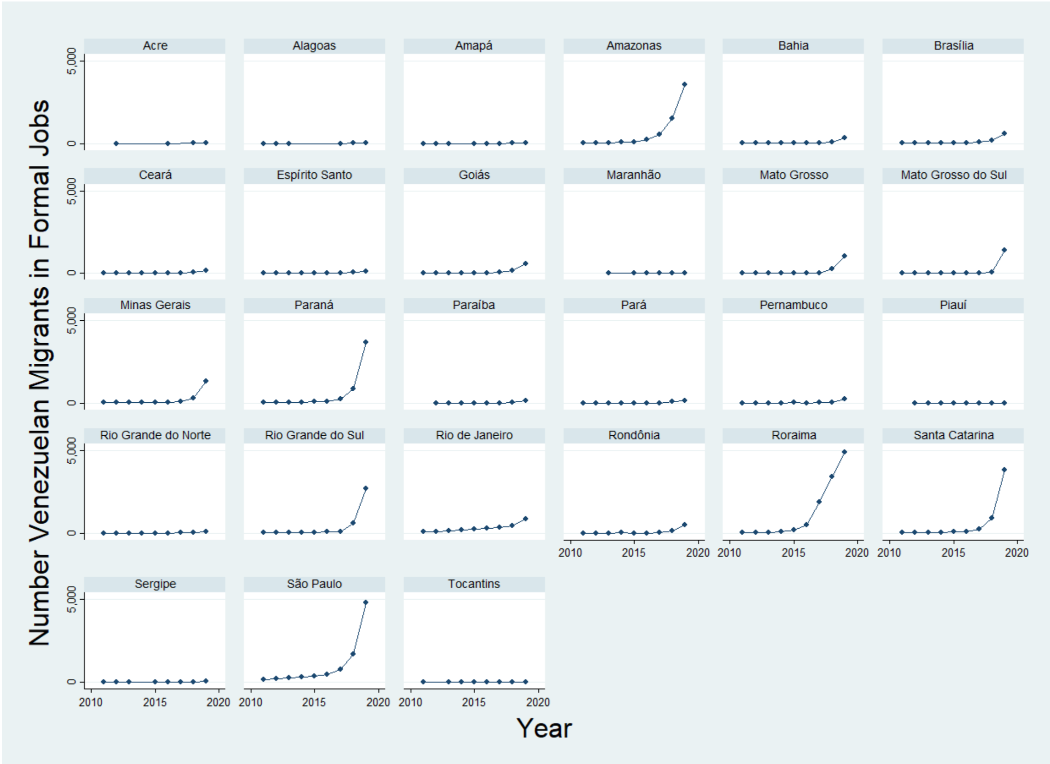


Figure A2.5: Number of Venezuelans in Formal Jobs by state, 2011 - 2019

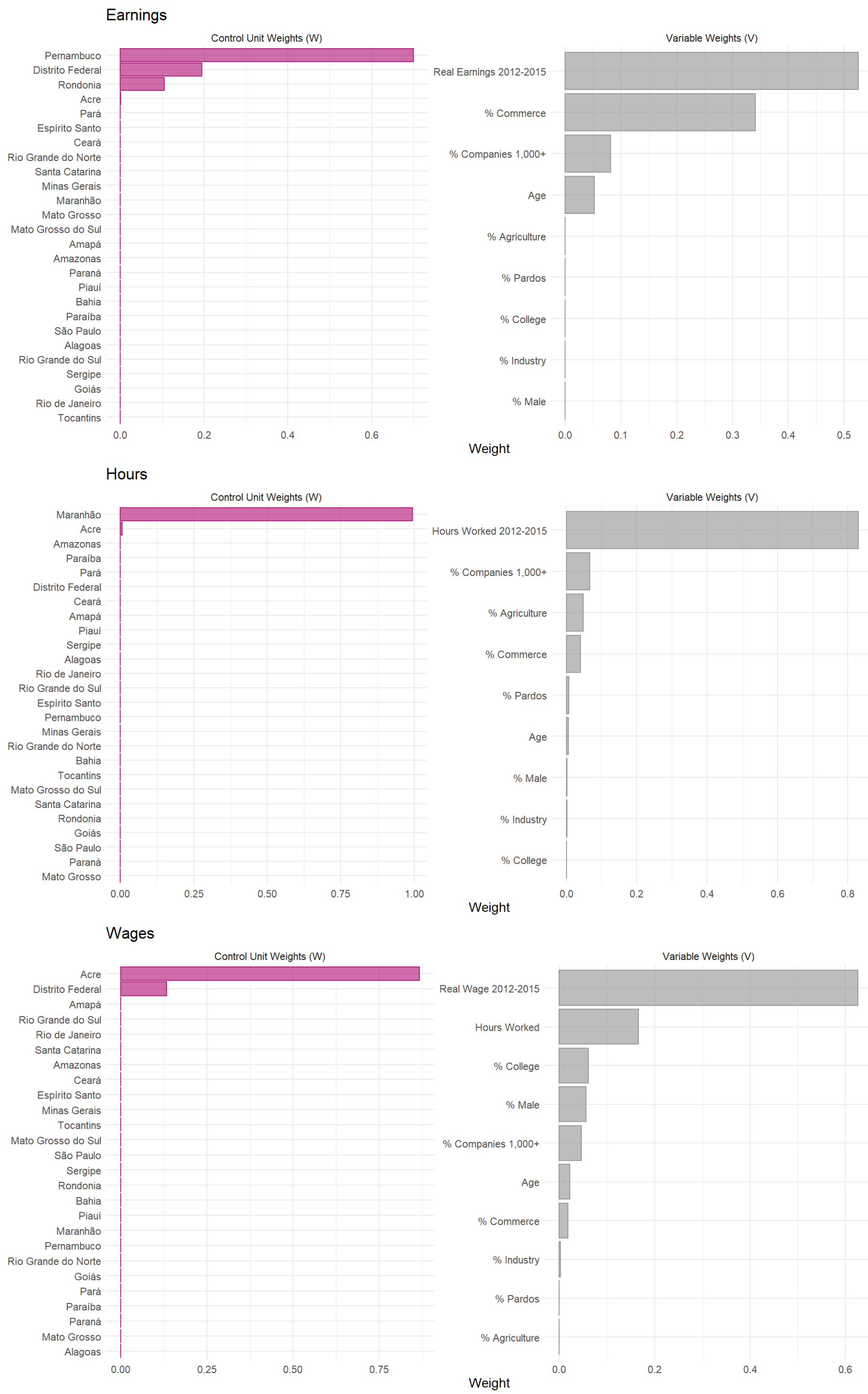


Figure A2.6: Synthetic Roraima Weights

Chapter 3

New Technology-Based Companies: The Impact of Uber on Traffic Safety in São Paulo, Brazil

The impact of ride-sharing companies on society is still unclear. This paper provides arguments to answer how new technology-based companies impact traffic-related outcomes such as traffic deaths and accidents with fatalities. Using data from 2015 to 2018 reported by police and fire departments, I develop a difference-in-difference strategy that takes advantage of the variation in the timing of Uber entry in cities in the state of São Paulo, Brazil. The results suggest that an initial downward effect on deaths from traffic accidents and accidents with fatalities is not sustained over time, and the average monthly effect of Uber on both outcomes is null. On the other hand, the availability of ride-sharing companies simultaneously decreases the number of alcohol-related tickets by 7.5% and increases the number of accidents with fatalities involving at least one car by 6.9%. This apparent discrepancy reflects both the new transportation alternative provided by the company for consumers of alcoholic beverages and the fact that riding as an Uber driver became an important option against sub- and unemployment in a context of long-lasting economic crisis, increasing the number of cars on the streets both extensively and intensively.

Keywords: Uber, Traffic Deaths, Alcohol-Related Tickets, Brazil, Differences-in-Differences.

3.1 Introduction

Although governments are still struggling to deal with regulations for new services provided by emerging ride-sharing companies such as Uber and Lyft (Rogers, 2015), this new sector of the modern economy is changing the behavior of most of its users. Being not only the pioneer but also the largest company in the sector, Uber has been the main target of this kind of research. The company, founded in March 2009, in San Francisco, CA, currently provides services in more than 700 cities around the world, in more than 60 different countries, and counts more than 3 million partner drivers in these nations. The number of users exceeds 93 million people, and the average number of daily trips by the company exceeds 17 million (Uber, 2019b). The value of the ride-hailing company is estimated to be 82.4 billion dollars (De La Merced and Conger, 2019) and, although the numbers may vary, Uber's market share in the US ride-hailing market is estimated between 65% and 69%[1]. (Iqbal, 2019).

By offering a new option of transportation, the impact of new ride-sharing companies on different social outcomes is driving economists to develop new studies to better understand this phenomenon. The primary impact of Uber's entry into the market is heavily felt, especially on other means of transportation. There is a body of knowledge available that suggests that Uber is a complement to rail transit and a substitute for bus rider, with stronger effects identified in large cities with lower ridership prior to Uber entry (Hall, Palsson, and Price, 2018). Furthermore, it seems that Uber has decreased taxi passengerhip (Nie, 2017), although it does not affect taxi drivers' wages (Cramer and Krueger, 2016; Berger, Chen, and Frey, 2018). The advent of new ride-sharing companies can also reduce the incidence of certain types of crime, such as DUI (driving under the influence), assault arrests, and disorderly conduct (Dills and Mulholland, 2018). Finally, Uber reduces the number of drunk driving accidents, traffic deaths (Dills and Mulholland, 2018), and hospitalizations (Barreto, Neto, and Carazza, 2021).

However, although the presence of Uber may take drunk drivers off the streets, being a partner driver has become an alternative to unemployment and/or subemployment in a context of economic crisis in developing countries. Therefore, the increase in both the number of cars on the streets and the time these new partners spend driving during their shift may play an important role in estimating the effect of Uber on accidents with fatalities and traffic deaths. To provide additional evidence on the role transportation technology plays in the modern economy, this article estimates the effect of Uber on accidents with fatalities and traffic deaths in cities in São Paulo State, Brazil. I use a difference-in-differences strategy that exploits monthly variation in Uber's entry in each city, comparing outcomes in areas that have received Uber's services with areas that have not using, among other sources, data reported by both police and firefighters departments from 2015 to 2018. This study aims to fill the literature gap on estimating the effect of Uber on traffic-related outcomes in a developing country context.

The biggest challenge faced is to understand both what drives Uber to enter a specific city and, among those cities, which would be reached by the ride-sharing company before the other. This issue is addressed here by controlling for observed and unobserved factors, improving the control group, changing assumptions about the distribution of the dependent variables, and restricting the analysis to a single state. By focusing on several cities in a

[1]Lyft is in the second position of market share of ride-sharing companies in the United States, but does not provide services in Brazil at the moment. The biggest rivals of Uber in the country are, respectively, "99" and "Cabify".

single state, it is also possible to overcome some of the heterogeneity concerns across areas directly related to both the decision to enter and traffic-related outcomes.

The present work also investigates possible heterogeneous effects across sex of those who die in traffic, the size of the city where Uber is present, the period of the day (day or night), days of the week, and locations (streets or highways) of accidents with fatalities. These estimates contribute to understanding whether Uber's effect vary conditional on the characteristics of drivers, accidents, and cities where Uber is available. In addition, I also provide analyses of the effect of Uber availability on other traffic-related outcomes, such as tickets, the number of licenses emitted for paid activities, and the number of vehicle registrations, that provide insights on how the presence of Uber affects the outcomes of interest.

Contrary to what has been suggested, this study provides evidence that factors other than population, income, and education are highly correlated with Uber's entry into a certain city-market. Compared to Barreto, Neto, and Carazza, 2021, which finds that Uber entry in an earlier period (2011-2016) in cities across Brazil reduces quarterly traffic deaths and traffic-related hospitalization rates by 10% ($p < 0.01$) and 16% ($p < 0.05$) respectively, this paper builds on that work and finds that the effect Uber plays on traffic-related outcomes is highly dependent on the economic scenario, and is not sustained over time. However, although it does not affect traffic deaths and accidents with fatalities, Uber availability reduces overall tickets by 6% ($p < 0.05$), and this reduction is mainly driven by alcohol-related tickets, which decline by approximately 7.5% ($p < 0.05$) per month in cities where the company provides its services. The presence of the ride-sharing company also increases the number of monthly vehicle registrations by 0.2% ($p < 0.1$) and increases accidents with fatalities with at least one car involved by 6.9% ($p < 0.1$), suggesting not only more cars on the streets, but also more hours of heavy traffic. Thus, these findings suggest that although Uber availability significantly reduces the number of drunk drivers on the streets, having Uber as an alternative means of generating/supplementing income in a fragile economy (boosting the informal job market) creates an effect both intensively and extensively on the number of cars on the streets that offsets its positive impacts.

The rest of the paper is organized as follows: Section 2 describes the data; Section 3 presents the estimation strategy; Section 4 describes the results; Section 5 depicts a series of robustness checks; Section 6 discusses the results, and Section 7 concludes.

3.2 Data

3.2.1 Uber Entry

Data on when Uber entered each city was collected on the company's website (Uber, 2019a). Uber, concerned about advertising its service for every new location, records on its website the day, month, and year that the service became available in each city nationwide and, therefore, specifically in the state of São Paulo. This information is regularly updated for every city in which the service is launched, providing a reliable source of information on the beginning of the company's operations at each of the different locations. From 2014 to 2019, Uber was officially providing services in 40 cities in the state. Figure 3.1 shows the year in which each city received the services of the ride-sharing company. Therefore, Uber arrives first in the regions surrounding the capital of the state, near the coastal area, and only in 2018 will it begin its expansion to the inner part of the state, with some exceptions. However, most of the state still lacks Uber services in 2018[2].

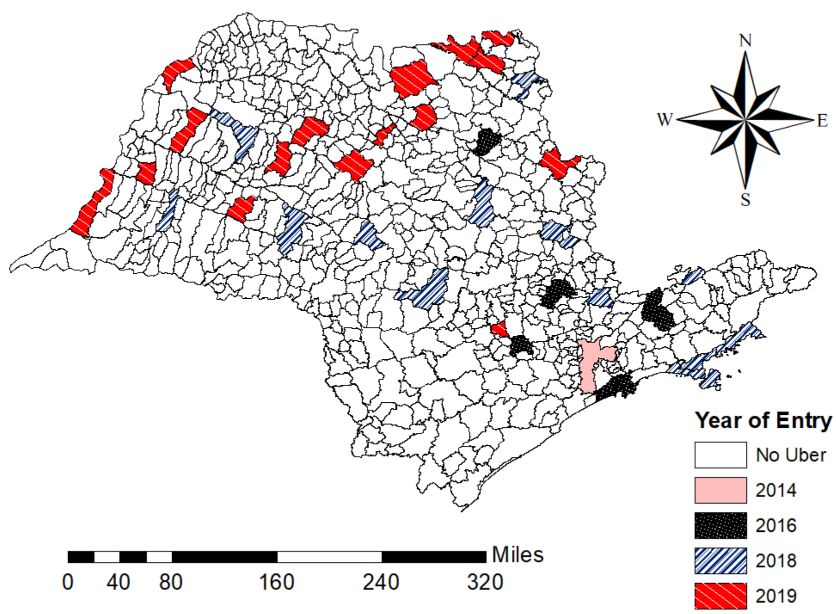


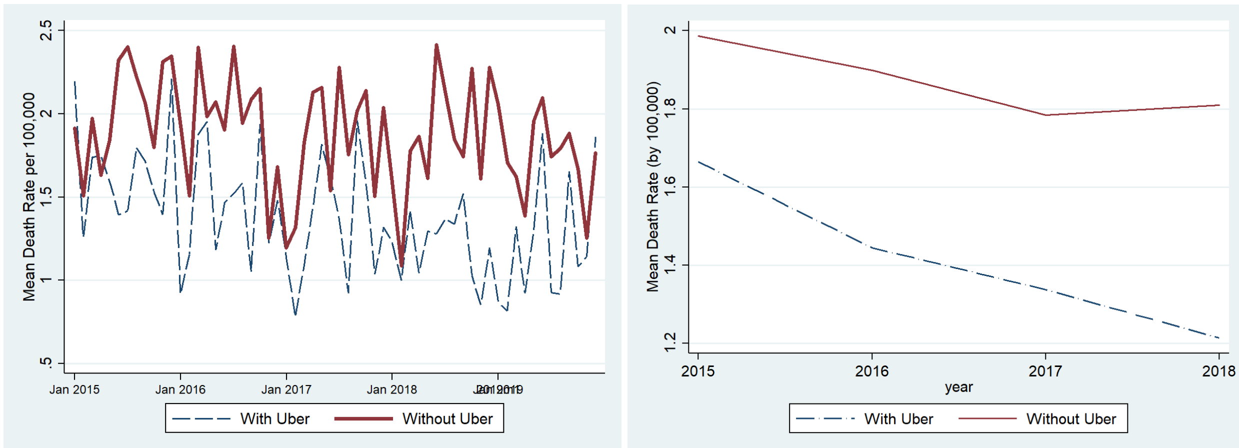
Figure 3.1: Map of When Uber Entered Each City

Figure 3.2 shows the average monthly death rates (per 100,000 residents) in cities with and without Uber during the study period both by month (Figure 3.2a) and year (Figure 3.2b). Although very cyclical, months prior to the end of each year reached peaks in death rates for both categories of cities[3], both figures suggest that, in addition to being similar between groups, there is an overall downward trend of death rates in traffic. However, it becomes clearer in Figure 3.2b that in cities with Uber during some time during the study period there is a marked decrease in death rates from 2016 onward, when Uber spread more throughout the state, reaching its lowest number in 2018. On the other hand, in cities

[2]A city that officially does not count with Uber, but borders one that does, can receive drives originally from the latest, but cannot be the origin of a new one.

[3]This phenomenon happens mainly due to holidays (Christmas and New's Year Eve), when people travel specially by car, what increases the number of cars on the streets in coastal areas and in highways. Consequently, the probability of crashes resulting in deaths is higher during December and January

without Uber, although also observed, the decline does not occur with the same intensity, reaching its lowest number in 2017 with a slight increase in 2018.



(a) By Month

(b) By Year

Figure 3.2: Death Rate by Uber Presence

3.2.2 Traffic Deaths and Accidents with Fatalities

The government of São Paulo has been reporting accidents with fatalities and actual traffic deaths within state limits (Governo do Estado de São Paulo, 2019) every month since 2015. Therefore, the sample used for the present research contains monthly observations of the number of traffic deaths registered in all 645 cities in the state from 2015 to 2018. Data are publicly available and contain important pieces of information such as the location of the accident that led to death, with longitude and latitude coordinates, exact date (year, month, and day), day of the week, period of the day, if the fatal accident happened on the streets of the city or on the highway, the vehicles involved in the accident, among others. Regarding the characteristics of the victim, the data present information on age and sex and whether the victim was a driver, a passenger, or a pedestrian.

Using the coordinates provided by the data, Figure 3.3 shows the intensity with which traffic fatalities occur in the state of São Paulo from 2015 to 2018. By showing the sum of deaths for this time period in each city, it seems clear that, although some cities in the north and mideast of the state present a higher number of fatalities in traffic during the study period, as we approach the east side of the state, where the capital, São Paulo City and other large cities with higher population are located, the intensity in which fatal crashes occur is higher compared to the rest of the state. On the other hand, when death rates are taken into account, instead of the absolute numbers of deaths, the situation seems to change. Cities with a higher population seem to have relatively lower traffic fatality rates per 100,000 residents.

Data on accidents with fatalities and traffic fatalities are merged with two other data sets. The *Departamento Estadual de Trânsito de São Paulo* (DETRAN - SP), which acts similarly to the Department of Motor Vehicles (DMV) in the United States, reports monthly data on the number of new driver licenses emitted, informing if the driver license is specifically for a driver who will perform any type of paid activity with it, traffic tickets and the reason why the ticket was received, and new registered vehicles in each city of the state by type of fuel. Except for the number of registered vehicles, available only from 2016 onward, the other two variables are available for the entire study period (DETRAN, 2021).

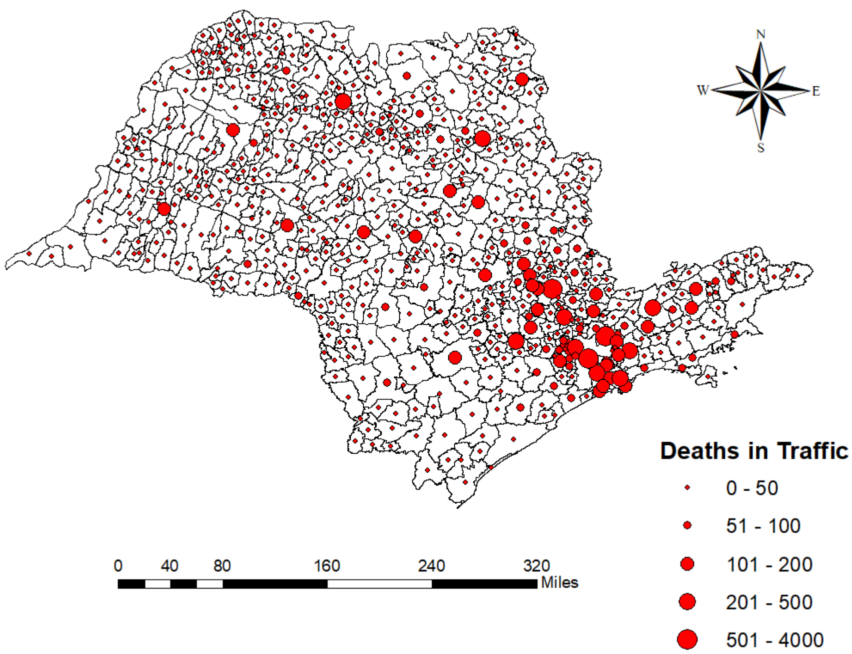


Figure 3.3: Traffic Deaths by City in São Paulo State, 2015-2018

Finally, yearly information on the characteristics of cities, also publicly available, is extracted from *Fundação Sistema Estadual de Análise de Dados* (SEADE), a state government institution that collects and reports socioeconomic and demographic data for cities and regions within the state of São Paulo. Among several available variables, for the purpose of this research, I use population, area (in km^2), sex ratio (number of men for every 100 women), percentage of the population 60 plus years old, percent of the population who lives in urban areas, the number of cars, buses, motorcycles, percent of the population in formal jobs, average real earnings of workers who have a formal job and the amount of students enrolled in undergraduate programs, public spending in transportation in 2011 and the percent of residents with at least a bachelor's degree in 2010 (SEADE, 2019). Table 3.1 provides summary statistics for both the characteristics of the cities in the state of São Paulo and the traffic related variables mentioned above in 2015, when Uber was available only in the city of São Paulo.

Table 3.1: Summary statistics - 2015

	(1) Total sample	(2) Cities with Uber	(3) Cities without Uber	(4) (2)-(3)
Traffic Deaths (per 100,000)	1.96	1.66	1.98	-0.316
	(5.93)	(1.84)	(6.10)	(-1.12)
Accidents w/ Fatalities (per 100,000)	1.80	1.53	1.81	-0.280
	(5.25)	(1.60)	(5.40)	(-1.12)
License for PA (per 100.00)	3,762.38	6,862.39	3,562.54	3,299.8***
	(7,415.68)	(8,726.51)	(7,278.67)	(9.38)
Tickets (per 100,000)	364.78	463.99	358.38	105.6***
	(181.90)	(168.05)	(180.92)	(-12.29)
Undergraduate Students (per 100,000)	909.19	3,961.02	712.46	3,248.6***
	(2,226.11)	(3,201.54)	(1,994.10)	(32.64)
Cars (per 1,000)	308.37	355.62	305.32	50.29***
	(74.92)	(84.59)	(73.23)	(14.26)
Bus (per 1,000)	4.58	3.34	4.66	-1.321***
	(2.92)	(1.49)	(2.97)	(-9.53)
Motorcycle (per 1,000)	111.61	174.13	107.58	66.55***
	(48.72)	(53.78)	(45.52)	(30.29)
Pop. Density	306.59	463.77	296.46	167.3***
	(1,215.44)	(624.23)	(1,243.29)	(2.89)
Population (in millions)	0.05	0.20	0.04	0.159***
	(0.11)	(0.23)	(0.09)	(31.21)
Sex Ratio (men per 100 women)	102.19	96.20	102.57	-6.368***
	(17.19)	(4.84)	(17.62)	(-7.80)
% Population 60+	14.74	14.26	14.77	-0.510***
	(3.25)	(2.49)	(3.29)	(3.29)
% Urban Population	85.73	96.71	85.02	11.69***
	(13.76)	(2.54)	(13.89)	(18.18)
% Formal Employment (FE)	22.80	26.85	22.54	4.308***
	(13.23)	(7.26)	(13.48)	(-6.85)
% Bachelor's Degree	8.59	13.84	8.25	5.592***
	(3.91)	(4.38)	(3.63)	(31.87)
Average Earnings in FE (in R$)	2,013.80	2,341.57	1,992.67	348.9***
	(459.66)	(531.57)	(446.49)	(16.18)
Public Spending in Transportation in 2011 (in millions R$)	2.90	9.87	2.57	7.305***
	(13.11)	(17.82)	(12.75)	(8.90)
Nº of Months	7,728	468	7,260	7,728
Nº of Cities	644	39	605	644

Notes: Table 1 shows the mean coefficients of characteristics with standard deviation in parenthesis on the city level for the year of 2015. Column (1) shows characteristics of cities that would get Uber at some point during the study period (2015-2018), while column (2) shows characteristics for cities that would not. Column (4) shows mean differences from columns (2) and (3) and the t-statistic values are in parentheses. Data on cities' characteristics is extracted on the yearly level. Sex Ratio is measured by the number of men for every 100 women. Data for Spending in Public Transportation in 2011 is presented in 2011 values. São Paulo city is not included in the sample. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

Column (4) of Table 3.1 represents the mean differences between the cities that Uber covered some time during the study period and those that did not (with t-tests in parentheses). When population is taken into account (death rates), there are no significant differences between the two groups in terms of both traffic deaths and accidents with fatalities[4]. Cities

[4]The similarities between accidents with fatalities and traffic deaths show that it is uncommon that a fatal accident results in more than one victim.

with Uber have a significantly higher number of driver licenses emitted for paid activity (PA) and given traffic tickets. Furthermore, Uber is present in cities with significantly higher population and population density, a relatively lower male population (about 6 less men per 100 women), and a lower percentage of the population of 60 years and older. In addition, Uber areas present substantially more students enrolled in undergraduate courses and a higher percentage of the population with at least a bachelor's degree in 2010, suggesting that not only the education level is higher for the population living in these areas, but also that the number of higher educational institutions is higher in those cities. Cities that would be covered by Uber also count significantly more cars and motorcycles and fewer buses per 1,000 residents than the rest of the state, suggesting both a surplus of private means of transportation and a deficit in public transportation options in cities where Uber would be present some time during the study period[5], although public spending on transportation is substantially higher in Uber cities, probably due to being highly correlated with the size of the city in terms of population. Finally, not only does a higher percentage of the population work in formal jobs (approximately 4%), but the earnings in the formal sector are also significantly higher in Uber cities (17.5%).

Therefore, Table 3.1 suggests that cities that would count on Uber at some time during the study period are socially and economically different from those that would not[6]. However, in terms of deaths and accidents with fatalities rates, both groups are similar.

3.3 Estimation Strategy

The effect of Uber on traffic deaths is estimated using a difference-in-difference strategy exploiting variation in the date of Uber entry in each city in the state of São Paulo. The advantage of such a model comes primarily from the fact that Uber enters different city markets on different dates. Therefore, because the data provide the number of traffic deaths monthly for each city in the state both before and after Uber is available for cities that would and would not receive Uber some time between 2015 and 2018, the difference-in-difference estimate allows a comparison of how the trend in traffic deaths has changed as a result of Uber entering a city with those markets where the company is (still) absent.

Although widely used in the field of social sciences to derive causality (Bertrand, Duflo, and Mullainathan, 2004), given the possibility of controlling for invariable differences between the units of analysis (attenuating endogeneity problems generated from the comparison of different units), the difference-in-differences method still suffers from two main problems. First, it must be the case that pretreatment trends do not differ between the treated and control groups (Angrist and Pischke, 2008). Second, serial correlation can emerge as an inconsistency when estimating standard errors (Bertrand, Duflo, and Mullainathan, 2004). These issues will be addressed separately.

[5]Since only the capital of the state, São Paulo City, has an internal rail system of transportation, the number of buses per 1,000 people turns to be a good indicator for public transportation options.

[6]All differences are statistically significant at 1% level.

Since in most cities not all Uber services such as “Uber Black”or “Uber Pool”[7] are offered and because “UberX” accounts for the highest proportion of ridership, the focus of the estimate is on the latter. Estimates are derived from the following model[8]:

$$Y_{cym} = \beta_0 + \beta_1 UberX_{cym} + \beta_2 Population + \beta_3 trend + X_{cy}\beta_4 + \theta_c + \theta_m + \theta_y + \xi_{cym} \quad (3.1)$$

Where the dependent variable is $\ln(\text{outcome} + 1)$ in city c, year y, month m; $UberX_{cym}$ is equal to one for month m in which the ride-sharing service is available in city c in year y; *Population* is the natural log of population (in millions) of city c in year y; *trend* is a time trend that captures the evolution of the outcome variable independently of Uber presence; X_{cy} is a set of control variables that include population density, sex ratio (men per 100 women), share of population 60 plus years old, urbanization, formal employment, number of students enrolled in undergraduate courses, earnings in formal employment, and car, bus and motorcycles per 1,000 residents; θ_c, θ_m and θ_y are, respectively, city, month and year fixed effects, and ξ_{cmy} is the error term. To avoid issues with heteroskedasticity, robust standard errors are clustered at the city level.

However, before turning to the discussion of the results, I check the parallel pretrend assumption discussed above. To do so, I created a series of time dummies that indicate the distance from the month m to the month that UberX was implemented in the city c. Thus, by regressing the outcome variable on these dummies, the model measures not only the effect of having UberX available over time, but also the preentry differences between cities with and without its services. Therefore, if no differences are found in traffic deaths comparing the months before Uber becomes available in both treated and control groups, there is no violation in the parallel pre-trend assumption and the use of the control group is valid for the use of the difference-in-difference method. Thus, the following strategy is used:

$$Y_{cym} = \alpha_0 + \sum_{j=-12}^{18} \alpha_{1,j}\gamma_{t+j} + \alpha_2 Population + \alpha_3 trend + X_{cy}\beta_4 + \phi_c + \phi_m + \phi_y + \eta_{cym} \quad (3.2)$$

where the dependent variable is again $\ln(\text{deaths} + 1)$ or $\ln(\text{accidents with fatalities} + 1)$ in the city c, year y, month m; γ_{t+j} is a series of dummy variables that equal one for each time distance j (in months) from Uber entry month t in city c. As highlighted above, *Population* is the natural log of population (in millions) of city c in year y, *trend* is a time trend that captures the evolution of the outcome variable independently of Uber's presence, X_{cy} is a set of control variables, and ϕ_c, ϕ_m and ϕ_y are, respectively, fixed effects of city, month, and year, and η_{cmy} is the error term.

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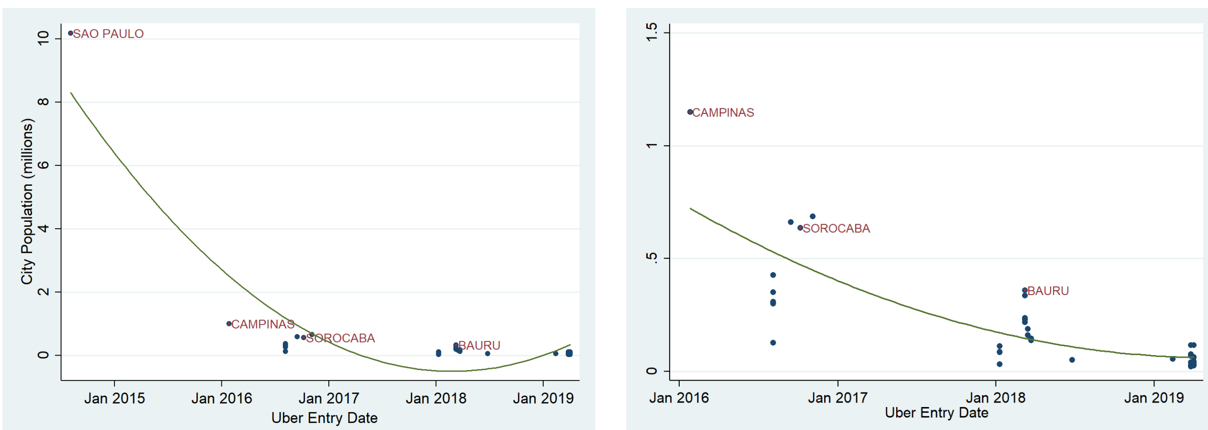
[7] According to the company website, “Uber Black matches riders with [...] drivers driving luxury vehicles for a higher price.”, while “UberPool gives passengers the option to share a ride for a more affordable price.”

[8] The described equation is the main estimation strategy used in this research. Results are consistent even after dropping the set of controls.

3.3.1 Uber's Decision of Entry

However, even by passing the parallel pre-trend test, there might still be some important time-varying characteristics affecting not only traffic deaths, but also the decision of UberX to enter a specific market. The literature suggests that Uber's decision is highly correlated with two main observable variables: population and education (Hall, Palsson, and Price, 2018; Cramer and Krueger, 2016). Figure 3.4, which shows a fitted line of a quadratic regression of the Uber entry date on the mean population of the city during the study period, seems to reaffirm what has been found in the literature on the importance of population in Uber's decision to enter a market. In fact, the company focuses first on providing its services in cities with a larger population. Because São Paulo city is the largest in the state in terms of population size and can drive the results by being an outlier in the data, I drop it out of the sample and reestimate the same graph (Figure 3.4b). Although the slope changes, Figure 3.4b still shows a negative relationship between population and the date the company decided to provide services in that city.



(a) With São Paulo City (b) Without São Paulo City

Figure 3.4: Uber Entry Date by City Population

Some may also argue that other factors might affect not only the decision of the company to enter a city but also the actual use of its services when it is available. Income and quality of work play an important role in the accessibility of a private ride service such as UberX (Greenwood and Wattal, 2017; DiMaggio et al., 2004), especially in a developing context. In fact, the profile of Uber users in Brazil appears to be defined as middle-class formal workers and students up to 36 years of age (40% up to 26 years of age), with about 70% owning a car (Coelho et al., 2017). The characteristics of the population and their respective way of life can also differ in terms of frequency of use of services. In Brazil, 45% use Uber for recreational activities, 22.1% to return home, and 15.1% to work (Coelho et al., 2017). Consequently, cities with a younger population and more college students may be the places where UberX is most widely used (Warschauer, 2004). Additionally, alternative means of transportation may be a factor not only for Uber's decision to enter a city, but also for the actual use of UberX as one of the options. About 50% of the company users in Brazil report that a taxi would be the alternative to Uber, while 30% would use public transport instead (Coelho et al., 2017).

Table 3.2: Linear Regressions Predicting Whether and When Uber Enters a City

	Did UberX Entry	Date UberX Enter
	(1)	(2)
Population	0.78557***	-3.52469***
	(0.04590)	(0.12000)
Pop. Density	-0.03621***	-0.31061***
	(0.00169)	(0.02039)
Sex Ratio	0.00050***	-0.00256
	(0.00003)	(0.00181)
% Population 60+	0.00161**	-0.00837*
	(0.00051)	(0.00419)
% Urbanization	0.00262***	-0.02136**
	(0.00009)	(0.00781)
% Formal Employment (FE)	0.00002	0.01062***
	(0.00007)	(0.00158)
Undergraduate Students	0.01721***	0.02528***
	(0.00081)	(0.00539)
Avg. Real Earnings in FE	0.02683**	-1.22901***
	(0.00854)	(0.06837)
Cars (per 1,000)	-0.14824***	0.97914***
	(0.00907)	(0.07296)
Buses (per 1,000)	-0.01216***	0.39083***
	(0.00248)	(0.02562)
Motorcycles (per 1,000)	0.12450***	-0.11918***
	(0.00419)	(0.03349)
Observations	30,912	1,872
Adjusted-R^2	0.23	0.93
F-Statistic	259.538	2,783.469

Notes: Data on when Uber entered each city was collected by the author by accessing Uber blog. Information on cities' characteristics is missing for one of the cities. This is the reason why the number of observations is reduced. variables are logged, expect for sex ratio, share of population 60 plus and urbanization. Population is measured in millions and population density in km^2. Sex ratio measures the number of men for every 100 women in the population of the city; degree of urbanization is the percentage of people living in urban areas as opposed of rural ones. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

To investigate whether there are significant relationships between Uber's decision and any other observable factors that vary over time other than population, I ran two different linear models: 1) first, predicting whether UberX enters a city, and 2) between cities that UberX actually entered, predicting when Uber would enter (Hall, Palsson, and Price, 2018). The results are shown in Table 3.2. The coefficients in column 1 provide evidence that there are several aspects, in addition to population and education, not highlighted in the literature that may be correlated with Uber's decision to enter a city. Although the probability of entering Uber increases by approximately 78% with a 1% increase in the population of a city, the company also appears to be inclined to provide services in areas with lower population densities. In addition, a higher share of the population of 60+ years with a higher proportion of men also seems to attract the company. The company also seems to be more interested in more urbanized areas with a greater presence of college students. In economic terms, although the company is attracted to cities with higher levels of income, the quality of jobs does not seem to affect this entry decision. Finally, Uber is especially interested in cities with a lower rate of cars and buses per 1,000 residents and higher rates of motor vehicles, which indicates that the company may benefit not only from a deficit in private and public means of transportation, but also in providing safer rides (in contrast to motorcycle rides).

Furthermore, among the sample of cities where Uber has decided to provide its services, population, population density, share of the population of 60+ years old, urbanization, average earnings, and motorcycles per 1,000 residents seem to have an inverse relationship with the date that Uber became available. In other words, the larger these variables are in a city, the sooner the company will be available. On the other hand, significant and positive relationships are found for the share of the population in formal employment, amount of college students, rate of cars and buses per 1,000 inhabitants, reaffirming that Uber enters first in cities with a deficit in private and public means of transportation, but also indicating that, among cities, the company decides to provide its services. The higher the deficit in buses (the most common public means of transportation) per population, the sooner the company will be present at these locations.

These results provide evidence that indeed Uber's decision is correlated to many other factors beyond education and population, the two most important ones specifically highlighted in the literature. Because of that, besides passing the previous parallel pretrend test, a model that aims to derive an unbiased estimator of UberX effect on death in traffic should include a series of control variables that account not only for what may influence the dependent variable, but also, and most important, for the factors that drive the nonrandomness of Uber's decision whether to enter a city and the timing it enters a city.

Finally, in 2016, the Federal Government approved a law that strengthened penalties for drivers driving under the influence of alcohol (G1, 2018). Although this law came into force in 1997 nationwide, it has been strengthened over the years. During the study period, it has been enforced twice. First in 2016, with an increase of approximately 53% of the fine applied to drivers caught driving under the influence of alcohol, and the same penalty being applied also to those drivers who refuse to take the breathalyzer test. Second, in 2018, with a penalty of 8 years in prison for drunk drivers who cause traffic deaths or 5 years if there are injured individuals in a car accident caused by a drunk driver. Since changes in the law may also have affected the behavior of drivers in drinking and driving and may be responsible for differences in fatal crashes from this period on, I reestimate the model described in the previous section adding the set of control variables that were significant in Uber's decision to enter a city in Table 3.2, with an indicator variable that equals one from the year 2016 onward that addresses the above-cited changes in the law.

3.4 Results

Then we turn our attention to the analysis of the results derived from Equation 2 depicted in the previous section. Figure 3.4 shows the regression results for both traffic deaths and accidents with fatalities with $-12 \leq j \leq 18$ (respectively, 12 months before UberX entry and 18 months after UberX entry)[9,10]. When taking into account the results in the analysis of pretrends, it is first noted that before Uber enters a city, the results for traffic deaths, accidents with fatalities, total, alcohol-related, and total DUI tickets are not statistically significant (see also Table A3.1 in the Appendix). On the other hand, when the number of car registrations is the dependent variable, 3 out of 10 months in a 95% confidence interval before the company's entry are statistically significant at a 95% confidence interval, which may indicate an early reaction to Uber presence (specifically 2 months) before the company's entry. Thus, the parallel pretrend assumptions of the differences-in-differences model discussed in the previous section can be validated (Angrist and Pischke 2008; Bertrand et al. 2004).

Table 3.3 reports the results for the model specified in Equation 1 for the outcome variables. The first row presents the coefficient of interest (β_1) that can be interpreted as the monthly average effect (in percent) of having UberX available on the outcome of interest. On average, UberX has no effect on both traffic deaths and accidents with fatalities. On the other hand, UberX negatively affects the overall number of tickets given by approximately 6% ($p < 0.05$). Most importantly, columns (4) and (5) shows that the overall decrease in tickets appears to be driven specifically by tickets applied to drivers under the influence of alcohol, which decreases by 7.5% ($p < 0.05$) per month, while overall DUI tickets[11] are not affected. Furthermore, the indicator variable for the change in dry law in 2016 points to significant increases in both general and alcohol-related tickets, suggesting more police presence and more operations to inhibit drivers from driving under the influence of alcohol. However, UberX is negatively affecting both outcomes, providing evidence that the availability of ride-sharing company services actually contributes to inhibiting the habit of drinking and driving and possibly preventing traffic deaths caused by this behavior. Finally, the increase in the number of cars registered by 0.2% monthly ($p < 0.1$) indicates a higher number of cars on the streets (extensive increase) as an effect of UberX availability.

[9]Due to data availability, only 10 months prior to the entry of the company is showed for New Car Registrations.

[10]Results are shown with the set of controls previously discussed in Equation 2. The removal of the set of control variables produces similar results.

[11]Types of tickets applied to drivers in these situations include driving the vehicle with temporarily no physical or mental capacity or trust/give vehicle to a person with no physical/mental state of safely driving, what accounts for the use of other drugs or substances.

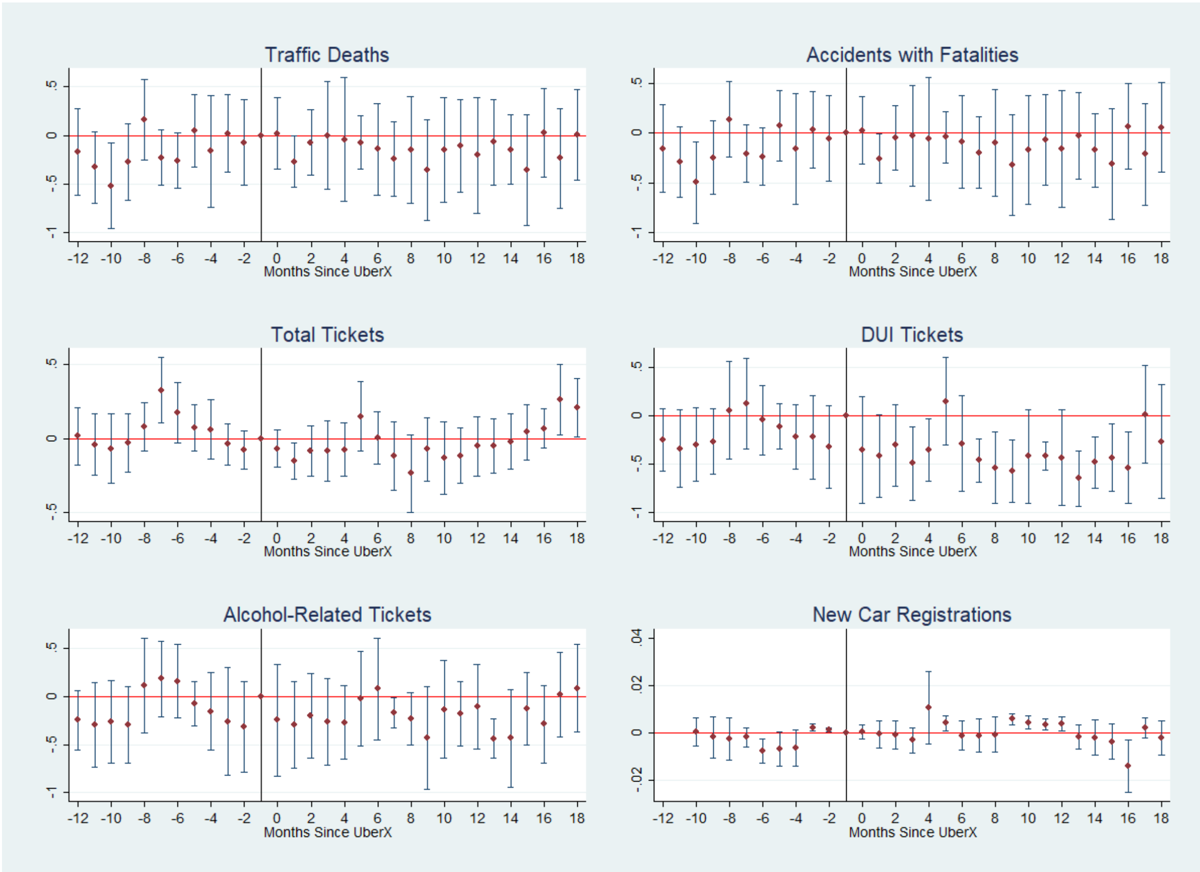


Figure 3.5: Monthly Effect of Uber on Traffic-Related Outcomes with 95% CI

In addition, UberX entry reduces traffic deaths and accidents with fatalities one month after entry by approximately 27% ($p < 0.05$) and 26% ($p < 0.05$), respectively. However, although producing negative estimates, the significance of the coefficients disappears in the following months, even when it reaches its lowest value in magnitude in the 9th month. Results are similar for tickets. However, the reductions are statistically significant for the first and 8th months, by 15.2% ($p < 0.05$) and 23.7% ($p < 0.1$), respectively. For alcohol-related tickets, significant reductions occur in 7th and 8th months, 16.8% ($p < 0.05$) and 23.1% ($p < 0.1$), respectively. On average, UberX does not affect DUI tickets in general, but the results are negative and statistically significant, especially from the 7th month of availability of UberX, with reductions of 45.6% ($p < 0.01$), 53.3% ($p < 0.01$) and 56.8% ($p < 0.01$) 7, 8 and 9 months after UberX availability, respectively. Finally, when analyzing new car registrations, the coefficients are statistically significant 5 months after Uber enters a city, with an increase of 0.4% ($p < 0.01$). The increase is especially intensified 9 months after the company provides services, with positive and significant effects of 0.6% ($p < 0.01$) and 0.4% ($p < 0.05$) in both 9th and 12th months[12]. Thus, Figure 3.5 shows that the effects of UberX on the traffic-related outcomes analyzed do not sustain over time, with a high variance of coefficients.

[12]See results in Table A3.1 in the Appendix.

Table 3.3: Effect of Uber on Traffic-Related Outcomes

	Traffic Deaths	Accidents w/ Fatalities	Tickets	Total DUI Tickets	Alcohol-Related Tickets	New Car Reg.
	(1)	(2)	(3)	(4)	(5)	(6)
UberX	-0.005	0.002	-0.060**	-0.059	-0.075**	0.002*
	(0.033)	(0.035)	(0.026)	(0.061)	(0.034)	(0.001)
Population	-12.401***	-13.866***	-5.205	49.570***	0.931	-0.534**
	(4.163)	(3.945)	(5.903)	(7.409)	(6.902)	(0.208)
Dry Law 2016	-0.009	-0.005	0.156***	0.155***	0.068***	-0.002***
	(0.015)	(0.014)	(0.022)	(0.025)	(0.020)	(0.001)
Pop. Density	0.667*	0.686*	2.464***	2.551***	-0.773	0.904***
	(0.400)	(0.373)	(0.950)	(0.845)	(0.738)	(0.037)
Sex Ratio	-0.019	-0.015	0.080**	0.083***	-0.082***	-0.003
	(0.016)	(0.014)	(0.036)	(0.029)	(0.026)	(0.002)
% Pop. 60+	0.003	0.001	0.046	0.080**	-0.098***	-0.000
	(0.017)	(0.016)	(0.042)	(0.040)	(0.035)	(0.002)
% Urbanization	0.013*	0.010	-0.003	0.002	0.056***	0.001**
	(0.007)	(0.006)	(0.018)	(0.013)	(0.012)	(0.001)
% Formal Employment (FE)	0.000	0.000	0.001	-0.001	-0.001	-0.000**
	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.000)
Enrollment in Undergraduate	-0.012	-0.011	-0.003	0.009	0.014	0.000
	(0.007)	(0.007)	(0.008)	(0.010)	(0.012)	(0.000)
Earnings in FE (in R$)	0.027	0.017	0.141	-0.196**	0.057	0.002
	(0.048)	(0.044)	(0.095)	(0.086)	(0.081)	(0.004)
Cars (per 1,000)	0.114	0.120	0.362	0.011	1.019***	0.618***
	(0.129)	(0.119)	(0.351)	(0.305)	(0.237)	(0.016)
Buses (per 1,000)	0.023	0.024	0.176**	0.096	0.079	-0.001
	(0.040)	(0.037)	(0.085)	(0.083)	(0.067)	(0.003)
Motorcycles (per 1,000)	0.002	-0.025	0.867***	-0.307*	0.068	0.188***
	(0.091)	(0.084)	(0.300)	(0.185)	(0.161)	(0.011)
Observations	30,912	30,912	30,912	30,912	30,912	24,472
Cities	644	644	644	644	644	644
Adjusted-R^2	0.004	0.004	0.044	0.026	0.048	0.893
F-Statistic	4.211	4.534	44.595	13.579	21.941	5034.078

Notes: City, month and year fixed effects are included in every estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

3.5 Robustness Checks

3.5.1 Different Specifications

Although the conditions for the use of differences-in-differences are satisfied, it could still be the case that traffic deaths and accidents with fatalities are not normally distributed. If this is the case, violating the Gauss-Markov theorem could lead to biased estimates. Because of that, I use different specifications to run the model described in Equation 1. I assume four different assumptions about the dependent variable distribution and run these models separately. The first is an OLS. Second, I use a negative binomial (NB) model that, contrary to a Poisson specification, allows the conditional variance of the dependent variable to exceed its conditional mean. Third, I use a Poisson Quasi-Maximum Likelihood Estimation (QMLE)

(Simcoe, 2008). Since its assumptions are different from the Negative Binomial and Poisson models, "assumptions of the model are not violated if the distribution of the dependent variable is not Negative Binomial or Poisson"(Greenwood and Wattal, 2017). Finally, to account for the high frequency of zeros in the sample, I run a regression with an inverse hyperbolic sine transformation of the dependent variables. Results are shown in Table 3.4 and are consistent with those in Table 3.3.

Table 3.4: Effect of Uber on Traffic Deaths and Accidents with Fatalities - Different Specifications

	OLS	NB	QMLE	IHS	OLS	NB	QMLE	IHS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A				
	Traffic Deaths				Accidents with Fatalities			
UberX	-0.088	-0.038	0.005	-0.004	-0.038	0.003	0.021	0.004
	(0.122)	(0.040)	(0.033)	(0.043)	(0.117)	(0.041)	(0.035)	(0.045)
Dry Law 2016	-0.274***	-0.143***	-0.040	-0.077*	-0.246***	-0.080	-0.051*	-0.067*
	(0.095)	(0.042)	(0.030)	(0.042)	(0.083)	(0.110)	(0.028)	(0.038)
Observations	30,912	30,048	30,048	30,912	30,912	30,048	30,048	30,912
Cities	644	626	626	644	644	626	626	644
Adjusted-R^2	0.011			0.004	0.013			0.004
χ^2		382.482	60.706			160.873	76.793	
				Panel B				
	Total Tickets				Total DUI Tickets			
UberX	-123.873***	-0.068***	-0.106***	-0.060**	2.047	0.064**	0.028	-0.074
	(40.164)	(0.013)	(0.028)	(0.027)	(2.745)	(0.029)	(0.094)	(0.067)
Dry Law 2016	-43.831***	-0.185***	0.018	-0.300***	1.192	0.248***	0.106***	-0.023
	(13.107)	(0.007)	(0.017)	(0.073)	(0.773)	(0.022)	(0.027)	(0.082)
Observations	30,912	30,912	30,912	30,912	30,912	30,816	30,816	30,912
Cities	644	644	644	644	644	642	642	644
Adjusted-R^2	0.078			0.040	0.112			0.022
χ^2		5,437.776	125.134			1,055.995	295.582	
				Panel C				
	Alcohol-Related Tickets				New Car Registrations			
UberX	-1.186**	0.039	-0.088	-0.080**	852.994*	0.001*	0.005***	0.002*
	(0.518)	(0.024)	(0.059)	(0.039)	(493.053)	(0.001)	(0.001)	(0.001)
Dry Law 2016	-1.590***	-0.407***	0.137***	-0.339***	1891.862***	0.076***	-0.003***	0.020***
	(0.299)	(0.015)	(0.024)	(0.074)	(412.784)	(0.002)	(0.000)	(0.003)
Observations	30,912	30,720	30,720	30,912	24,472	24,472	24,472	24,472
Cities	644	640	640	644	644	644	644	644
Adjusted-R^2	0.081			0.046	0.668			0.893
χ^2		.	501.111			141,002.424	97,462.533	

Notes: City, month and year fixed effects are included in every estimation except for the QMLE. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

3.5.2 Different Control Groups

The next concern is that although fixed effects and control variables account for observed and unobserved differences between treated and untreated groups, the cities that ended up covered by UberX during the study period are not similar to those that did not. Thus, one may think that they are not a good counterfactual. Because of that, two different strategies are used to improve the characteristics of the control group. First, I restrict the pool of cities in the control group only to the cities that would receive UberX in 2019 (results presented in Table 3.5).

Secondly, I predict the probability of receiving UberX based on the two most important factors highlighted in the literature, population and education, and rerun the main model for four different minimum probability values ($Pr[UberX = 1|educ, pop] \geq 0.05$, $Pr[UberX = 1|educ, pop] \geq 0.10$, $Pr[UberX = 1|educ, pop] \geq 0.20$ and $Pr[UberX = 1|educ, pop] \geq 0.50$). See the results in Table 3.6. Although the results for new car registrations remain positive and significant (now at 1% level), the drastic reduction in the sample size decreases the precision of estimates for total, DUI and alcohol-related tickets in Table 3.5, with an increase in standard errors. The same effect is observed for alcohol-related tickets in Table 3.6, while the other traffic-related outcomes present coefficients similar to those shown in Table 3.3.

Table 3.5: Effect of Uber on Traffic Deaths and Accidents with Fatalities: Cities that would receive UberX in 2019 as the Control Group

	Traffic Deaths	Accidents w/ Fatalities	Tickets	Total DUI Tickets	Alcohol-Related Tickets	New Car Reg.
	(1)	(2)	(3)	(4)	(5)	(6)
UberX	0.051	0.047	-0.035	0.031	0.083	0.003^{***}
	(0.045)	(0.045)	(0.033)	(0.085)	(0.054)	(0.001)
Dry Law 2016	-0.128	-0.110	0.166^{**}	0.289	0.271^{*}	0.000
	(0.091)	(0.086)	(0.081)	(0.175)	(0.138)	(0.001)
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,872	1,872	1,872	1,872	1,872	1,482
Cities	39	39	39	39	39	39
Adjusted-R^2	0.029	0.027	0.250	0.063	0.171	0.900
F-Statistic	3.747	3.199	44.465	11.336	18.461	3542.792

Notes: City, month and year fixed effects are included in every estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

Table 3.6: Effect of Uber on Traffic Deaths and Accidents with Fatalities - Control Group Based on $Pr[UberX = 1|educ, pop]$

	$Pr \geq 0.05$	$Pr \geq 0.1$	$Pr \geq 0.2$	$Pr \geq 0.5$	$Pr \geq 0.05$	$Pr \geq 0.1$	$Pr \geq 0.2$	$Pr \geq 0.5$
Panel A								
	Traffic Deaths				Accidents with Fatalities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UberX	-0.006	0.003	-0.005	0.033	-0.001	0.007	0.000	0.032
	(0.034)	(0.035)	(0.036)	(0.041)	(0.036)	(0.036)	(0.037)	(0.041)
Dry Law 2016	-0.029	-0.043	-0.117*	0.000	-0.024	-0.042	-0.110	0.000
	(0.034)	(0.057)	(0.070)	(.)	(0.033)	(0.056)	(0.068)	(.)
Observations	9,348	4,476	2,964	2,160	9,348	4,476	2,964	2,160
Cities	195	95	62	45	195	95	62	45
Adjusted-R^2	0.008	0.012	0.020	0.027	0.008	0.012	0.020	0.025
F-Statistic	3.050	2.776	2.374	3.029	3.290	2.987	2.823	3.130
Panel B								
	Total Tickets				Total DUI Tickets			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UberX	-0.070***	-0.044	-0.060**	-0.069**	-0.044	-0.023	-0.021	-0.009
	(0.026)	(0.027)	(0.027)	(0.030)	(0.062)	(0.068)	(0.077)	(0.078)
Dry Law 2016	0.135***	0.195***	0.132**	0.000	0.270***	0.349***	0.300**	0.000
	(0.035)	(0.053)	(0.066)	(.)	(0.059)	(0.093)	(0.125)	(.)
Observations	9,348	4,476	2,964	2,160	9,348	4,476	2,964	2,160
Cities	195	95	62	45	195	95	62	45
Adjusted-R^2	0.095	0.156	0.184	0.228	0.061	0.096	0.101	0.081
F-Statistic	23.942	20.286	23.461	35.869	10.867	8.786	9.654	13.568
Panel C								
	Alcohol-Related Tickets				New Car Registrations			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UberX	-0.039	0.008	0.002	0.058	0.003***	0.003***	0.003***	0.003***
	(0.035)	(0.039)	(0.049)	(0.046)	(0.001)	(0.001)	(0.001)	(0.001)
Dry Law 2016	0.189***	0.272***	0.256**	0.000	-0.000	0.000	0.001	0.000
	(0.051)	(0.073)	(0.097)	(.)	(0.001)	(0.001)	(0.001)	(.)
Observations	9,348	4,476	2,964	2,160	7,408	3,556	2,354	1,710
Cities	195	95	62	45	195	95	62	45
Adjusted-R^2	0.094	0.147	0.156	0.167	0.905	0.903	0.900	0.898
F-Statistic	16.685	16.943	15.432	24.626	2896.791	1836.127	1982.462	2345.229

Notes: City, month and year fixed effects are included in every estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

3.5.3 Different Data Set

The following concern could be that the data set used in this research differs from others. If there are omitted cases from the data set, especially if the omission does not occur randomly across cities, it could also generate biases in the estimations. To account for this possibility, I use an alternative data set (DATASUS) provided by the Brazilian Ministry of Health (Ministério da Saúde, 2020). Data identify the main cause of death according to the International Classification of Diseases (ICD-10). From that, I select all traffic-related deaths[13] and rerun the main estimation. Results are presented in Table 3.7. The coefficient of interest (β_1) is very similar to that presented in Table 3.3, even when the set of control

[13]Following Barreto, Neto, and Carazza, 2021, classifying as traffic-related deaths the following ICD-10 groups: Pedestrian traumatized in transport accident, cyclist traumatized in transport accident, cyclist traumatized in transport accident, tricycle occupant traumatized in transport accident, car occupant traumatized in transport accident, pickup truck occupant traumatized in transport accidents, bus occupant traumatized in transport accidents

variables is not taken into account, and remains statistically nonsignificant. Consequently, it seems plausible to reject the hypothesis that there are omitted traffic-related deaths in the original data set and not reject the hypothesis that the effect of UberX on traffic-related deaths is null.

3.5.4 Heterogeneity of Traffic Deaths

After reassuring the main findings presented in Table 3.3, the following subsections are dedicated to investigate whether different groups of cities, individuals, and accidents are affected by the presence of the ride-sharing company in different ways.

By Victim's Characteristics

As mentioned above, the profile of Uber users is well defined and described as middle-class formal job workers and students up to 36 years of age (Coelho et al., 2017). Because of that, we could expect that this group may be the most benefited by the presence of UberX. If this is true, it could be the case that only specific groups of individuals would have significant effects as a result of UberX availability. Furthermore, there may be heterogeneous effects based on gender differences in the use of ride-sharing services. More than 80% of the sample of people who die in traffic are made up of men, and consequently this is the part of the sample that is expected to benefit the most from the presence of UberX. Women, on the other hand, may be more reluctant to take advantage of the UberX service because they do not trust the service or because they fear being the victim of violence or harassment (Gragnani and Balago, 2016). Because of that, I now run the main estimation with traffic deaths as the dependent variable segregating the sample first by gender and second by age. Following the profile of Uber users, two age groups are defined as victims between 18 and 39 years of age and those 40 years and older.

Although the results are negative for both genders (1.9% and 0.3% for men and women, respectively), but stronger for men, none of them are statistically significant. The same happens for different categories of age. Most surprisingly, the direction of the coefficient for victims between 18 and 39 years of age, those who are more similar to the profile of people who actually use Uber, is positive (increase of 0.9%), while the coefficient is negative for victims 40 years and older (1.8% decrease).

By Accident Characteristics

Now, I investigate whether having UberX affects traffic deaths differently according to either location, period of the day, or day of the week the fatal accident. First, although most of the requested rides are expected to be within the city limits, Uber is available, given the proximity of cities in the state of São Paulo, it is possible for Uber users to request a ride

Table 3.7: Effect of Uber on Traffic-Related Deaths Using a Different Dataset (DATASUS)

	Traffic-Related Deaths	
	(1)	(2)
UberX	-0.013	-0.013
	(0.054)	(0.054)
Population	3.011	5.099
	(4.479)	(5.353)
Dry Law 2016		-0.000
		(0.013)
Pop. Density		-0.078
		(0.408)
Sex Ratio		-0.011
		(0.012)
Pop. 60 plus		-0.028*
		(0.016)
Urbanization		0.001
		(0.006)
Perc. Pop. in Formal Employment		-0.000
		(0.000)
Enrollment in Undergrad		-0.002
		(0.007)
Avg. Real Earnings in Formal Employment		-0.008
		(0.034)
Cars per 1,000 inhab.		0.122
		(0.128)
Buses per 1,000 inhab.		-0.033
		(0.035)
Motorcycles per 1,000 inhab.		0.069
		(0.070)
Observations	30,912	30,912
Cities	644	644
Adjusted-R^2	0.002	0.002
F-Statistic	4.528	3.022

Notes: City, month and year fixed effects are included in every estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

Table 3.8: Effect of Uber on Traffic Deaths by Victim's Characteristics

	By Gender		By Age	
	Male	Female	18-39	40 plus
	(1)	(2)	(3)	(4)
UberX	-0.019	-0.003	0.009	-0.018
	(0.037)	(0.030)	(0.026)	(0.032)
Dry Law 2016	0.009	0.003	-0.000	-0.007
	(0.014)	(0.009)	(0.010)	(0.014)
Observations	30,912	30,912	30,912	30,912
Cities	644	644	644	644
Adjusted-R^2	0.003	0.001	0.002	0.004
F-Statistic	4.238	1.970	2.014	4.634

Notes: City, month and year fixed effects are included in every estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

with a different city as destination. However, the latter could be either a city where Uber is also available or not. Thus, Uber could affect not only traffic deaths on the streets of the available cities, but also on the highways and, to some extent, on the streets of neighboring cities.

To understand the mechanisms (if any) in which Uber is affecting traffic death, I use three different subsets of traffic deaths as representations of some possible hypotheses. First, if Uber is indeed taking drunk drivers out of the streets, we should expect traffic deaths during the weekend and at night, periods when alcoholic beverages are mostly consumed and when most accidents involving drunk people occur (Acselrad et al., 2012), to decrease as a response to having UberX available in the city[14]. Second, if Uber is indeed a response to unemployment[15], we could expect that there would be more cars on the streets of cities more frequently, which can increase the probability of accidents and, consequently, fatalities in traffic specifically caused by cars[16].

Furthermore, if Uber is also a response to underemployment, people who work during

[14]Previous researches do not find evidences of UberX affecting overall fatalities (Greenwood and Wattal, 2017), but specifically alcohol-related ones (DUI). Since the data do not provide the reason for the fatal accident, I use deaths that occur at night and late at night, on weekends (Fridays to Sundays), on the streets, when most drunk drivers would be on the streets as a proxy for deaths caused by DUI

[15]The state of São Paulo, as the entire country, presented high rates of unemployment over the study period: respectively 12.7%, 12.4% and around 12% in 2017, 2018 and 2019 (Loschi, 2020).

[16]I also take advantage of the fact that the data classifies the accident by type of vehicle involved. Since almost 70% of UberX reported having a car (Coelho et al., 2017), if, in fact, more people are letting their cars in the garage due to UberX, it is expected that fewer accidents with cars could occur as a result of having available ride-sharing services. Due to that, I remove all accidents that do not count with at least one care involved (see column (8)).

the day are also driving at night and on weekends to increase their monthly (or daily) income. If this is the case, having more cars at night and on weekends should lead us to expect that traffic deaths during this period could also be increased by an increase in the probability of accidents. The estimates for each different type of accident may help shed light on which mechanism is stronger, and therefore what is the net effect on the outcome variable of interest. Finally, I select only accidents that resulted in victims of collisions, crashes, or overruns (column (9)).

Table 3.9: Effect of Uber on Traffic Deaths by Accident Characteristics

	By Location		By Period		By Day of the Week		Other definitions		
	Streets	Highways	Night	Day	Weekend	Week Days	WNS	Car Involved	Type
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
UberX	-0.022	0.017	0.041	-0.036	0.015	-0.018	0.021	0.069*	0.019
	(0.042)	(0.032)	(0.029)	(0.031)	(0.031)	(0.034)	(0.025)	(0.036)	(0.036)
Dry Law 2016	0.005	0.002	0.020	-0.014	0.004	-0.012	0.012	0.072***	-0.014
	(0.011)	(0.013)	(0.013)	(0.011)	(0.013)	(0.012)	(0.008)	(0.014)	(0.014)
Observations	30,912	30,912	30,912	30,912	30,912	30,912	30,912	30,912	30,912
Cities	644	644	644	644	644	644	644	644	644
Adjusted-R^2	0.013	0.001	0.005	0.003	0.003	0.002	0.006	0.032	0.002
F-Statistic	6.239	2.399	4.870	2.874	3.276	2.810	2.939	14.986	3.260

Notes: "WNS" stands for Weekend, at Night on the Streets. City, month and year fixed effects are included in every estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

Not entirely surprising, no effect is found for deaths occurring on highways, which indicates that even though some drivers may be crossing city limits to complete their drives, the percentage of such cases within the pool of requests by Uber users is not enough to generate significant impacts for deaths in highways connecting two close cities. Although in the specifications almost none of the coefficients are statistically significant, there is a negative direction for deaths that occur on the streets (2.2%), during the day (3.6%) and on weekdays (1.8%), but signs are positive for deaths at night (4.1%) and on weekends (1.5%). When analyzing only deaths that occur on weekends, at night and on the streets ("WNS") in column (7), although the coefficient is not statistically significant, it is positive (2.1%), which suggests that although drunk drivers have been taking out of the streets, as shown in Table 3.3, with more cars on the streets caused by more Uber drivers, there is a higher probability of accidents with fatalities. This suspicion is reinforced by the findings in column (8). Traffic deaths resulting from accidents with at least one car involved increased by about 6.9% ($p < 0.1$) per month in cities where UberX is available. The fact indicates that UberX may be increasing the number of vehicles on the road at certain times (specifically at night on the weekend) and, consequently, increasing probability of accidents with fatalities involving at least one car.

3.6 Discussion

After careful analysis, the findings above suggest that, although UberX availability reduces traffic deaths, accidents with fatalities, and total tickets issued in the first month, these effects are not sustained over time, and it takes at least 7 to 8 months so a city feels the impact of having UberX available on other traffic-related outcomes, such as alcohol-related and total DUI tickets, and these effects occur at the same time as there are more car registrations.

Some reasons seem to explain the lag response of the traffic-related outcomes studied. First, it takes time so the company increases the supply of drivers in the city. As soon as there are enough drivers to meet the demand for rides, from the 9^{th} month on, it seems that the impacts are stronger. Second, although the company advertises its services in every city it enters, it may be the case that the biggest share of the population does not receive this information with the same speed, thus not being aware of the new services provided and consequently not making use of it up to a certain month. Third, even after being aware of the new services, if residents are risk averse and take the time to build confidence in the company, they may not use the services for some time until it is proven safe to ride UberX. With the same rationale, if transportation is a habit and, because of that, it is difficult to change, it also takes time for changes in behaviors to be translated into actions. Finally, if, when launched, UberX is still more expensive than other available transportation options (especially public ones), the service will only be used by the part of the population that can afford it. Therefore, even though the prices of ride sharing could be lower than those of other private means of transportation, such as taxis, what would happen during its first months of service would be merely a substitution of taxis to UberX (Nie, 2017). Consequently, the entry of the company into the market may not affect traffic-related outcomes in the first months of operation[17].

In addition, in a context of long-lasting economic crisis, as was the Brazilian economy from 2016 on, being an Uber driver would be an option for those in need of a job as an alternative to unemployment and underemployment. When working as a driver for Uber becomes one of the preferred and maybe easiest alternatives to generate/supplement income, a great amount of cars on the streets would make traffic heavier during both days and nights. Simply due to the fact that the number of vehicles on the streets is higher, the likelihood of accidents with and without fatalities would increase accordingly. The findings in Table 3.9 strengthen this hypothesis by indicating a higher number of accidents involving at least one car that are possibly caused by UberX drivers being on the streets more often in periods with a previously lower number of vehicles before the company entered a city. However, the findings indicate that not only UberX is pushing cars to be more often on the streets, but also that there are more cars on the streets. If residents buy more cars as a form of investment so that they can drive as a paid activity, we would not only have an intensive increase in cars on the streets, but also an extensive one.

As a direct consequence of more Uber drivers, both intensively and extensively, as shown in both Tables 3.3 and 3.9, the supply of services would be greater and, since the price charged by the company is entirely based on supply and demand at a given time, the prices charged by the ride tend to decrease when supply exceeds demand[18]. Consequently, a higher share of

[17]For more information, see the framework developed by Jackson and Owens, 2011 of how public transportation affects DUI

[18]In the U.S, it was found that, without including a tip, Uber's prices are less expensive than taxi in 19

the population would have the possibility to use its services, since after 7-8 months it is more likely that the population has not only been informed about it, but also already adapted to the service. Therefore, if not only increases the surplus of the upper class by pushing private transportation prices down, Uber is also becoming an alternative, especially to the public transportation system to the middle and lower classes, residents would substitute or even complement public transportation means by/with UberX services (Hall, Palsson, and Price, 2018). Although buses theoretically take cars out of the streets, given their potential to transport several people at a time, Uber, given its lowest prices caused by the increase in the supply of drivers and its higher comfort and convenience in comparison to public transportation, may be seen as an alternative to a bad (or lack of) public transportation system with increasing fares. Consequently, putting back on traffic a large number of cars increases the interaction between vehicles, and therefore increases the likelihood of crashes and fatalities derived from them.

Finally, since there are no background checks of drivers working for the company, as there are for taxi drivers[19], for example, the qualifications of Uber drivers are at least questionable and can lead to less skilled drivers on the streets, which can also increase the probability of accidents involving cars, as suggested in Table 3.9. In addition, since Uber's drivers work with their private vehicles, on one hand, they may even be more careful about its maintenance or appearance in order to increase their probabilities of receiving rides based on public evaluation. However, given that there are no mandatory inspections annually[20], as is the case for taxis, vans, etc., the higher probability of having mechanical issues and/or system failures also increases the likelihood of injuries and fatalities in traffic.

Therefore, more people using the services of the ride-sharing company, prevents specifically alcohol-related tickets and possibly traffic deaths resulted from it[21]. However, although some private vehicles are left in their garages, the economic deprivations resulting from the downturn of the economic activities in the country increases both the number of vehicles on the streets (both extensively and intensively) and the number of less prepared drivers, which may offset the decrease in the number of drunk drivers on the streets and explain the null effect of UberX on overall traffic deaths and accidents with fatalities on average. Therefore, the net effect of the UberX effect on both accidents with fatalities and the resulting traffic deaths is not straightforward and seems to depend not only on the strength of each mechanism, but also on how long the service is available and on the overall state of

of 21 cities studied, while if including a tip to the taxi driver, Uber is cheaper in all of 21 cities (Silverstein, 2014). In Brazil, UberX fares are estimated to be 15% to 50% cheaper than taxis (Higa, 2015).

[19]In Brazil, where this study is set, taxi drivers must go through a course that is made of 28 hours in total, divided in 14 hours of human relationship topics, 8 hours of defensive driving, 2 hours of first aid and 4 hours of basic mechanics and electrics.

[20]In September of 2019, Uber was granted with a judicial decision that suspended the need for any vehicle inspection for drivers who work with Uber in São Paulo. The allegation was that the cars that drive with Uber are private and, for them, the Brazilian Traffic Code does not require the same inspections as for vehicles in the private transportation and rental sectors. The decision was published on Uber's blog and is publicly available

[21]On the other hand, if drinking is a normal good, the availability of ride-sharing options, by decreasing the costs of drinking, would also increase its demand and the net effect of this phenomenon on fatal crashes is, therefore, not obvious.

the economy.

3.7 Conclusion

This article estimates the effect of Uber on traffic-related outcomes by exploiting the variation in the date of entry of the ride-sharing company in cities in the state of São Paulo, Brazil. The results suggest that having UberX available, although it does not affect overall traffic deaths or accidents with fatalities monthly on average, it reduces the number of tickets issued by 6%, mainly as a result of a decrease of 7.5% per month in alcohol-related tickets. In addition, having UberX available also increases the number of new car registrations and accidents involving at least one car by 0.2% and 6.9%, respectively. Furthermore, the results indicate that it takes about 7 to 8 months for UberX to produce significant impacts on traffic-related outcomes, although the impacts do not sustain overtime.

Results suggest that even after controlling for changes in the law for drinking and driving, having UberX available reduces alcohol-related tickets by 7.5%. Therefore, having UberX available saves approximately R$ 3,400 (U$ 700) in tickets every year in a given city in São Paulo State on average, what translates into R$ 185,000 (about U$ 37,000) state wide savings in a given year only in prevented tickets. Besides, if less drunk drivers on the streets also result in fewer traffic deaths caused by it, having UberX would also save in court costs, loss of income caused by deaths and/or imprisonment, medical costs, etc.

On the other hand, since being an Uber driver has become an alternative to unemployment and underemployment in the context of a long-lasting economic crisis, the decrease in alcohol-related tickets (and likely fatalities in traffic) is counterbalanced by an overcrowding effect of more cars on the streets longer hours during the day, increasing the probability of accidents with fatalities involving at least one car by 0.2%. These findings also should draw the attention of policy makers not only on the necessity of quickly adapting to the new sharing economy but also by showing that the driving market is highly correlated to the overall economy.

Although UberX seems to be a viable alternative/complement means of transportation for both other private and public options, especially for consumers of alcoholic beverages, a downturn in economic activity can increase the supply of workers on the driving market in a way that counteracts the benefits of having UberX. Although prices may decrease in response to increased drivers' supply, which can expand services to a larger share of the population, it also translates into a higher probability of accidents and deaths resulting from it. Furthermore, had the economy been in a different and more stable situation, these "new drivers" would probably have higher earnings and better quality jobs in other occupations.

Finally, although the present work has shown that having UberX available specifically prevents drinking and driving behavior, there is evidence in the literature that having the ride-sharing company available also increases alcoholic consumption (Burgdorf, Lennon, and

Teltser, 2020). It reaffirms not only that UberX is a viable option for these consumers, but also that restaurants and bars may benefit from this alternative not only because UberX allows for higher consumption of alcoholic beverages by those who would drink less because they would have to drive, but also to attract those who would not drink at all. In addition, it shows that viable alternative means of transportation with lower prices, greater convenience and more comfort may contribute to a change in drinking and driving behavior.

Given the results presented and what has been found in the literature, there are several mechanisms by which Uber may impact accidents with fatalities and traffic deaths, and the net direction of change is not obvious. In practice, Uber seems to have changed not only the way consumers behave in relation to public and private means of transportation, but also the way authorities should think about urban mobility. Its implementation is still recent, and, because of that, its effects are not entirely understood. This work aims to contribute to the growing literature on the impact of new technological services on social outcomes (Dills and Mulholland, 2018; Jackson and Owens, 2011; Greenwood and Wattal, 2017; Brazil and Kirk, 2016; Cramer and Krueger, 2016) and specifically fill the gap on how and why the impacts of these services differ in developing countries (Barreto, Neto, and Carazza, 2021).

However, the main limitation of this work is the lack of information on the cause of accidents with fatalities and traffic deaths resulting from them. The attempt to approximate the conditions of cases of alcohol-related deaths does not produce significant results, while alcohol-related tickets are shown to be reduced as a consequence of UberX availability. Besides, it is left to future research to measure other companies' externalities (both positive and negative) that may affect social welfare in different and nontrivial ways.

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Appendix

Table A3.1: Effect of Uber on Traffic-Related Outcomes by Month

	Traffic Deaths	Accidents w/ Fatalities	Tickets	Total DUI Tickets	Alcohol-Related Tickets	New Car Reg.
	(1)	(2)	(3)	(4)	(5)	(6)
6 months before Uber	-0.261*	-0.236	0.173*	-0.044	0.160	-0.008***
	(0.146)	(0.148)	(0.103)	(0.183)	(0.194)	(0.003)
5 months before Uber	0.045	0.075	0.074	-0.110	-0.077	-0.007*
	(0.190)	(0.183)	(0.080)	(0.119)	(0.118)	(0.004)
4 months before Uber	-0.163	-0.158	0.061	-0.221	-0.152	-0.006
	(0.294)	(0.283)	(0.101)	(0.170)	(0.205)	(0.004)
3 months before Uber	0.018	0.031	-0.039	-0.221	-0.257	0.002***
	(0.203)	(0.197)	(0.072)	(0.221)	(0.287)	(0.001)
2 months before Uber	-0.071	-0.052	-0.078	-0.321	-0.312	0.001***
	(0.226)	(0.219)	(0.067)	(0.218)	(0.239)	(0.000)
1 month before Uber: Omitted						
Month Uber Entered	0.023	0.028	-0.068	-0.352	-0.242	0.000
	(0.186)	(0.171)	(0.065)	(0.282)	(0.297)	(0.002)
1 months after Uber	-0.268**	-0.258**	-0.152**	-0.415*	-0.292	-0.001
	(0.133)	(0.126)	(0.063)	(0.217)	(0.231)	(0.003)
2 months after Uber	-0.072	-0.047	-0.085	-0.304	-0.200	-0.001
	(0.171)	(0.165)	(0.087)	(0.216)	(0.226)	(0.003)
3 months after Uber	0.001	-0.030	-0.084	-0.492**	-0.266	-0.003
	(0.284)	(0.258)	(0.103)	(0.195)	(0.229)	(0.003)
4 months after Uber	-0.040	-0.059	-0.076	-0.352**	-0.268	0.011
	(0.324)	(0.314)	(0.092)	(0.166)	(0.194)	(0.008)
5 months after Uber	-0.071	-0.042	0.149	0.149	-0.022	0.004***
	(0.141)	(0.130)	(0.120)	(0.231)	(0.249)	(0.002)
6 months after Uber	-0.139	-0.092	0.002	-0.285	0.080	-0.001
	(0.241)	(0.237)	(0.089)	(0.253)	(0.271)	(0.003)
7 months after Uber	-0.242	-0.195	-0.119	-0.461***	-0.168**	-0.001
	(0.193)	(0.185)	(0.117)	(0.116)	(0.080)	(0.004)
8 months after Uber	-0.146	-0.098	-0.237*	-0.538***	-0.231*	-0.001
	(0.279)	(0.272)	(0.133)	(0.189)	(0.140)	(0.004)
9 months after Uber	-0.353	-0.320	-0.073	-0.573***	-0.427	0.006***
	(0.265)	(0.260)	(0.110)	(0.164)	(0.271)	(0.001)
10 months after Uber	-0.149	-0.171	-0.130	-0.415*	-0.131	0.004***
	(0.273)	(0.277)	(0.124)	(0.246)	(0.259)	(0.001)
11 months after Uber	-0.107	-0.068	-0.115	-0.414***	-0.177	0.004***
	(0.242)	(0.231)	(0.096)	(0.072)	(0.170)	(0.001)
12 months after Uber	-0.203	-0.157	-0.053	-0.431*	-0.106	0.004**
	(0.303)	(0.300)	(0.103)	(0.254)	(0.226)	(0.002)
Observations	29,319	29,319	29,319	29,319	29,319	23,225
Cities	614	614	614	614	614	614
Adjusted-R^2	0.003	0.004	0.041	0.025	0.041	0.892

Notes: All estimations include the following set of controls: population, dry law 2016, population density, sex ratio, share of population 60 plus years old, urbanization, formal employment, students in enrolled in undergraduate courses, earnings in formal employment, and car, bus and motorcycles per 1,000 residents. City, month and year fixed effects are included in the estimation. Robust standard errors are in parentheses and clustered at the city level. Superscripts *, ** and *** represent significance at 10%, 5% and 1% respectively.

Table A3.2: Frequency of Traffic Deaths by Days of the Week

	Observations	Percentage	Cumulative Percentages
Sunday	6,891	19.83	19.83
Saturday	6,709	19.30	39.13
Friday	4,981	14.33	53.46
Monday	4,240	12.20	65.66
Thursday	4,195	12.07	77.73
Wednesday	4,002	11.52	89.25
Tuesday	3,736	10.75	100.00
Total	34,754	100.00	-
N	34,754	-	-

Table A3.3: Frequency of Traffic Deaths by Period of the Day

	Observations	Percentage	Cumulative Percentages
Night	11,712	33.70	33.70
Afternoon	7,448	21.43	55.13
Late Night	6,684	19.23	74.36
Morning	6,083	17.50	91.87
N/A	2,827	8.13	100.00
Total	34,754	100.00	-
N	34,754	-	-

Table A3.4: Frequency of Traffic Deaths by Location

	Observations	Percentage	Cumulative Percentages
Streets	18,628	53.60	53.60
Highways	15,053	43.31	96.91
N/A	1,073	3.09	100.00
Total	34,754	100.00	-
N	34,754	-	-

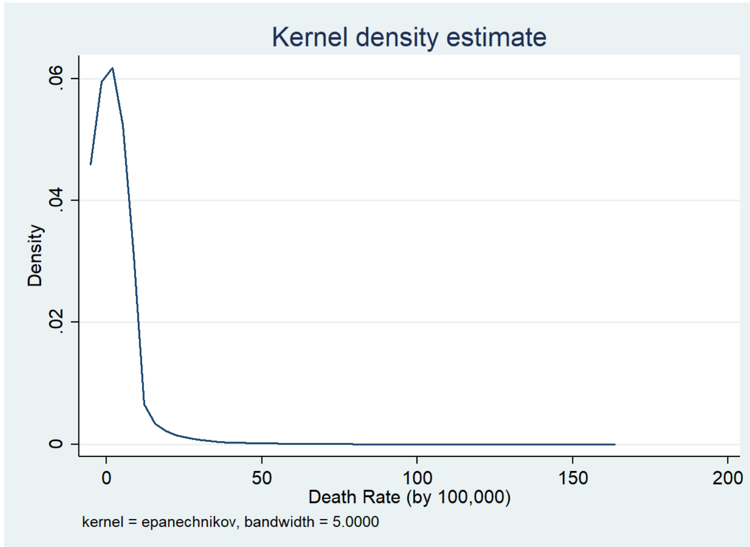

Figure A3.1: Kernel Density of Monthly Death Rates in Traffic

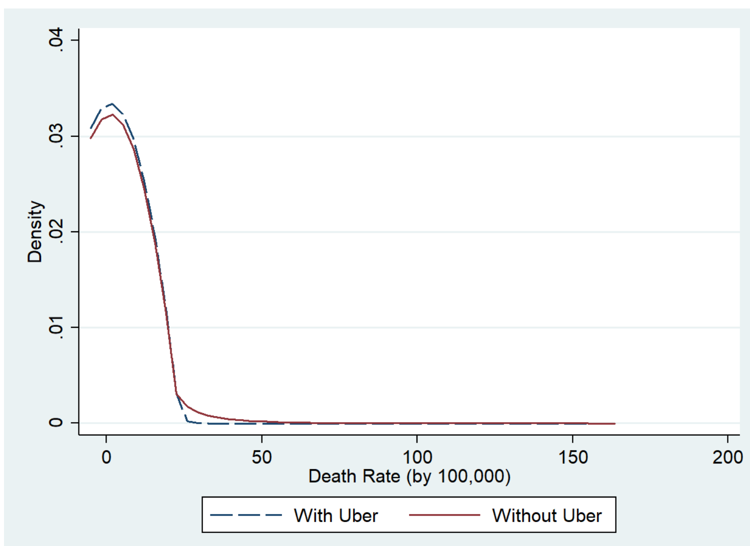

Figure A3.2: Kernel Density of Monthly Death Rates in Traffic by Treatment Group