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Essays on Applied Microeconomics

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Aos meus, com carinho.

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"If you try the best you can

The best you can is good enough"

Thom Yorke

ABSTRACT

This thesis is composed by three unrelated chapters in applied microeconomics. In the first chapter I estimate the effect of hosting the Summer Olympic Games on country's subsequent sports performance. In the second chapter I use the flash flood that occurred in the Brazilian state of Santa Catarina in 2008 to estimate the existence of spatial spillovers from natural disasters in geographically linked areas. The final chapter tests the "mobile guardianship" hypothesis on criminal activity through a quasi-experiment caused by the introduction of the ninth digit in mobile phones in some municipalities of the Brazilian state of São Paulo. The first chapter examines the effect of hosting the Summer Olympic Games on future country's sport success, computing the total number of Olympic medals in the subsequent Games. In order to control for the endogeneity produced by the hosting decision, we use the Synthetic Control Method (SCM), which constructs a weighted country that works as a synthetic counterfactual. The main finding of this paper is that, although decreasing over time, the ex-host effect does not fade away immediately after hosting for some host countries (as Australia and Canada), and that the effect is negligible for middle income countries (as Mexico and Greece). Despite the high costs associated with hosting mega-events such as the Olympic Games, the results shed some light on an important benefit generated from investments in sports. In the second chapter I use a flash flood that occurred in the Brazilian state of Santa Catarina in 2008 to investigate the existence of spatial spillovers from natural disasters in geographically linked areas. For that, I estimate a difference-in-differences model that explicitly allows for the existence of spatial interactions within affected and non-affected regions. The results show that municipalities directly affected by the flood suffered a 8.47% decrease in GDP per capita on the year of the disaster. Three years after the flood however GDP per capita rebounded back to pre-disaster levels in all sectors but the Agricultural sector. Finally, the spatial estimations show that spillovers exist and are economically relevant. The final chapter presents prelimary evidence on how cell phone technology shocks affects crimes. If the phone guardianship hypothesis for crime drop is true, one should expect that exogenous shocks in mobile technology have impact on crime. The present study tests the discontinuity in the number of mobile access and its effects on a range of crimes and on victimization. Using data from Secretaria Estadual de Segurança Pública de São Paulo (SSP) and Brazilian Ministry of Health I estimate the average effect through a temporal difference-in-differences. The results suggest that the ninth digit have a significant impact on homicides and bodily injury, but no effect on vehicle and property thefts. The results provide an insight into the relationship between mobile technology and crime, in addition to supporting the expansion of technology-based policies to deter crime.

Keywords: Olympic Games. Synthetic control. Natural disaster. Spatial spillover. Ninth digit. Mobile phones. Crime.

RESUMO

Esta tese é composta por três artigos não relacionados em microeconomia aplicada. O primeiro artigo estima o efeito de sediar os Jogos Olímpicos sobre a performance esportiva futura de um país. O segundo capítulo explora as fortes chuvas ocorridas em Santa Catarina em 2008 para investigar a existência de *spillovers* espaciais em regiões geograficamente relacionadas. No último capítulo é testada a hipótese da “segurança móvel” sobre a atividade criminal através de um quasi-experimento introduzido pela regra de adição do nono dígito aos telefones móveis em alguns municípios do estado de São Paulo. O primeiro capítulo examina o efeito de sediar os Jogos Olímpicos sobre a performance esportiva futura dos países-sede, computando o número de medalhas olímpicas conquistadas nas edições subsequentes. A fim de lidar com a endogeneidade relacionada à decisão de sediar os Jogos Olímpicos, utiliza-se o método de Controle Sintético, o qual constrói um país sintético através de um média ponderada de outros países como contrafactual. Os resultados mostram que, embora decrescentes ao longo do tempo, o efeito *ex-host* não desaparece imediatamente após sediar para alguns países (como Austrália e Canadá) e que o efeito é nulo para países de renda média (como México e Grécia). Apesar dos elevados custos associados a realização dos Jogos Olímpicos, este trabalho discute um importante benefício gerado através do investimento em esportes. O segundo capítulo utiliza as fortes chuvas ocorridas em Santa Catarina, no ano de 2008, para investigar a existência de *spillovers* espaciais em decorrência do desastre natural em regiões geograficamente relacionadas. Utiliza-se o método de diferença-em-diferenças que explicitamente permite a existência de interações espaciais entre regiões afetadas e não afetadas. Os resultados mostram que os municípios diretamente afetados sofreram uma queda de cerca de 8,47% no PIB per capita no ano do desastre. Três anos depois, no entanto, o PIB per capita retorna ao nível pré-disastre em todos os setores da economia, com exceção do setor agrícola. Por fim, os estimadores espaciais mostram que *spillovers* existem e são economicamente relevantes. O último capítulo mostra evidências preliminares de como choques sobre tecnologias de telefones móveis afetam criminalidade, baseado na teoria da “vigilância móvel”. Se a hipótese da "vigilância móvel" é verdadeira, espera-se que choques exógenos sobre a tecnologia de telefones móveis tenham impacto sobre a criminalidade. Utilizando os dados da Secretaria Estadual de Segurança Pública de São Paulo (SSP) e do Ministério da Saúde, estima-se o efeito médio do tratamento através de estimadores de diferenças em diferenças. Os resultados sugerem que a inclusão do nono dígito teve impacto significante sobre homicídios e lesão corporal dolosa, mas sem evidências para roubos ou furtos de veículos e propriedades. Tais evidências sugerem o uso intensivo de políticas baseadas em tecnologia para o combate ao crime.

Palavras-chave: Jogos Olímpicos. Controle sintético. Desastres naturais. Efeitos de *spillover*. Nono dígito. Telefones móveis. Crime.

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*

1 The Olympic Spirit Boost: A Synthetic Control Approach

1.1 Introduction

Hosting the Summer Olympic Games (henceforth referred to as the Games[1]) can generate manifold benefits and advantages to a country in terms of positive and lasting legacy[2]. However, due to the large amount of resources required to the viability of the Games and the diffused tangible and intangible benefits produced, there is no consensus among economists about the net effect for the host country considering the multidimensionality of several indicators that might be altered at the national level (GLYNN, 2008). As argued by Coates and Humphreys (2003), this difficulty in measuring the magnitude of the benefits make it a hard task to justify public expenditures involved in the feasibility of such events.

The empirical literature concerned to measure the impacts of hosting the Olympic Games on social and economic indicators is large, including topics on labor market (BAADE; MATHESON, 2002; HOTCHKISS; MOORE; ZOBAY, 2003), tourism and migration (KANG; PERDUE, 1994; LYBBERT; THILMANY, 2000), investment in infrastructure (ESSEX; CHALKLEY, 1998; FRIEDMAN et al., 2001; NEWMAN, 2007; ZHANG; ZHONG; YI, 2015), trade and output (BAYAR; SCHAUR, 2014; BRÜCKNER; PAPPA, 2015; MATHESON, 2006; MEHROTRA, 2011; ROSE; SPIEGEL, 2011), and local development (BILLINGS; HOLLADAY, 2012; GRIEVE; SHERRY, 2012), to name a few. Another branch of the literature is intertwined with its impacts on the level of national pride, prestige and general feel-good factor (ALLISON; MONNINGTON, 2002; FORREST; SIMMONS, 2003; KAVETSOS; SZYMANSKI, 2009; HILVOORDE;

[1] Just as the Summer Olympic Games, the Winter Olympic Games take place every four years. I restrict the analysis to only the Summer Olympic Games because the larger data availability and scale of the event.

[2] Although the Summer Olympic Games are held in a single host city, it is easy to argue the existence of spillovers for athletes within a country. See, e.g., Johnson and Ali (2000) and Owen (2005).

ELLING; STOKVIS, 2010). Whether these impacts can justify the public expenditure used in the viability of such events is of great importance and may be a definitive argument in favour of hosting.

An immediate impact for the country whose city hosts the Games also considered in literature is the increase in the number of Olympic medals awarded (BALMER; NEVILL; WILLIAMS, 2003; JOHNSON; ALI, 2000). If this impact is lasting or not may be reflected in how current athletes are capable to benefit from Olympic venues' legacy or in the countries' ability to attract the interest of new athletes over time. In other words, investments in sports are likely to reduce "performance depreciation" for ongoing athletes and provide infrastructure and financial support to increase the chances of medal success for future competitors. To this extent, the ex-host effect is defined as the effect of hosting the Games on future sport success, computed by the country's medal count in subsequent Games. On the one hand, the existence of an ex-host effect allow to shed some light on the mechanisms explaining the host effect, given that some explanations - as crowd effect[3] - are more akin with transitory effects while others - as investment effect - are more connected with permanent shocks. On the other hand, the ex-host effect is relevant because an Olympic breakthrough may be an additional argument in favour of hosting the Games. The first recognized paper that access the ex-host effect is Bernard and Busse (2004). The authors estimate the determinants of Olympic success and find that both large population and high GDP per capita are highly correlated with Olympic medal wins. Moreover, the addition of lagged medal shares suggest that sport investments for the hosting country may potentially increase the chances of gain more medals in subsequent editions. Also employing a dynamic panel estimation method, Contreras and Corvalan (2014) find that the ex-host effect fades away immediately after hosting. These divergent results are partly one of the difficulties of using longitudinal data to identify the ex-host effect since one can not observe what would happen to a host country in absence of treatment.

Estimates of the causal effect of hosting the Games on subsequent total medal

[3] Several studies have found positive correlation between the psychological and behavioural states previous to competitive sport events and home advantage factors, such as crowd effect, territoriality and judging bias. For a detailed discussion of home advantage effect see Carron, Loughhead and Bray (2005), Pollard and Pollard (2005) and Unkelbach, Memmert et al. (2010).

awarded through the comparison of actual hosting cities with those that never hosted may be misleading because cities are not randomly assigned to host the Olympic Games, but determined by unobservable characteristics. Billings and Holladay (2012) and Rose and Spiegel (2011) use countries whose cities bid unsuccessfully for host the Games as counterfactuals[4]. However, this procedure can reduce dramatically the sample of countries used as controls and unable us to perform a precise analysis. Another caveat to access causal interpretation for the estimators is that controlling for confounding time-varying may be difficult since many of these covariates are indeed being altered at the national level (ANGRIST; PISCHKE, 2008).

These shortcomings is exactly what this paper aim to disentangle by using the synthetic control estimators (ABADIE; GARDEAZABAL, 2003; ABADIE; DIAMOND; HAINMUELLER, 2010; ABADIE; DIAMOND; HAINMUELLER, 2015) to identify the ex-host effect. Intuitively, the method constructs a weighted average of control countries that best resemble pre-treatment characteristics for the treated unit, i.e., this artificially constructed country is much similar to the host country in the pre-treatment periods than any of the control country on their own. An important feature of the method is that only one treated unit is required. Besides, one can access the individual treatment effect, differently of the average treatment effect of the intervention. To our knowledge, this is the first paper that has attempted to give the ex-host effect estimators a causal interpretation. This paper advance in literature by using a transparent and flexible model that relaxes the assumption that confounding factors are time invariant (fixed effects) or share a common trend (difference-in-differences), given the effect of unobservable confounding factors is allowed to vary with time.

Zhang, Zhong and Yi (2016) and Miyoshi and Sasaki (2016) use the synthetic control method to access the hosting impact on air quality for the 2008 Summer Olympic Games in Beijing and on labour market outcomes for the 1998 Winter Olympic Games in Nagano, respectively. However, no formal inference procedure is presented to check the statistical significance of their SCM estimators. In this paper, I analyse several test

4 There is a vast literature in causal inference that use runners-up as natural counterfactuals. See, for example, Anagol and Fujiwara (2014), Greenstone and Moretti (2004) and Lee (2001).

statistics widely used in the SCM literature to check if there is enough evidence to reject the null hypothesis of no effect. More specifically, I follow the inference procedure used in Abadie, Diamond and Hainmueller (2010), that consists in estimating p-values through permutation test and implement two tests proposed in Ando (2015). The latter tests consist in comparing the sizes of the average treatment effects in relation to the distribution of average placebo effects and on the overall average treatment effect. Also, a novel and formal inference procedure is carried out following Imbens and Rubin (2015) and Firpo and Possebom (2016), which establishes the hypotheses that guarantee the validity of Fisher's Exact Hypothesis Testing Procedure, a scheme that uses the empirical distribution of a test statistic to check whether there is sufficient evidence to reject the null hypothesis of no effect. According to Firpo and Possebom (2016), test statistics that use the SCM outperforms test statistics frequently used in the evaluation literature.

I analyse six Summer Olympic Games[5] and take advantage by using the bid winner announcement period as the time of the treatment in order to count for any effect that may occur between the announcement and the time the Games take place. The results of this paper show that, although the ex-host effect decreases over time, it does not fades away immediately after hosting for most host countries and that the effects are negligible for developing economies. These results support the findings in Bernard and Busse (2004), suggesting that countries with the higher GDP per capita are likely to invest in sports and benefit from Olympic legacy after hosting the Games.

The paper is structured as follows. In section 1.2 I provide a brief overview of the Olympic candidature process, then explain the data set used to access the causal ex-host effect and the selected case studies. Sections 1.3 and presents the Synthetic Control Method and formalizes the inference procedure. Section 1.4 presents and discuss the estimated results along with placebo studies and alternative tests, and section 1.5 concludes.

[5] In the subsection 1.2.3 I explain all excluded case and also the data limitation that restricts our analysis.

1.2 Olympic Games and Data

In this section I present the main information used to conduct the empirical analysis of the ex-host effect for a number of Games. I briefly describe how the candidature process is carried out and how the bid city is selected for hosting the Games. I also discuss which case studies were selected to this study, given data limitation and suitability of the synthetic control method.

1.2.1 *Olympic bid and host cities*

The Olympic Games are considered the main sporting event in the world, composed of a large number of modalities in different sports and athletes from different nationalities. The modern format of the Summer Olympic Games was introduced in 1896 in Athens and, although originally designed as an event that transcends the nationalist character, the Games have played important social and political roles for the host cities, for the host region and for the country (MATHESON, 2006; GLYNN, 2008; HILVOORDE; ELLING; STOKVIS, 2010; GRIEVE; SHERRY, 2012). Since then, many cities have struggled to host the Olympic Games so as to catalyse positive development of tangible and intangible long-term legacies. The candidature process begins with the National Olympic Committees (NOCs) appointing which cities[6] from its territory to put forward bids for hosting and members of the International Olympic Committee (IOC) vote by secret ballot and announce the bid winner. Typically, the host city election occurs seven years prior the Games, but the whole candidature process is launched 10 years before the Games take place.

The current Olympic candidature process consists of two stages: (a) the invitation phase[7] and (b) the formal candidature process. The invitation phase focuses on the dialogue between the IOC and future candidate cities with the purpose of sharing best practices and construction of a solid project that best meets the city's long-term development aspi-

[6] Since 1960, only one city per country is allowed to be candidate. In contrast, for the Games in 1956, six cities from the USA have bid for the election.

[7] The invitation phase protocol was introduced as a result of the Olympic Agenda 2020 and has already been launched for the 2024 bidding process.

rations (International Olympic Committee, 2014, p. 12). Following the invitation phase, cities that decided to bid for the Games enroll the official candidature process. During this phase, candidate cities put together their Games vision, concept and legacy perspectives, presenting a plan that ensure have the necessary legal, institutional and financial mechanisms in place to host the Olympic Games. The last track analyses how candidate cities will deliver the Games and ensure a sustainable legacy. Finally, during the host city election, the cities bidding for host make a final presentation to the IOC Session and the IOC members vote by secret ballot in a multi-round scheme to choose the host city. The elected host city then signs the Host City Contract with the IOC and is able to implement all infrastructure, financing and bureaucratic project[8].

The bid cities and round-by-round host city votes counts for the Games between 1968 and 2004 are presented in table A.1 in appendix A. The candidate city that receive the lowest number of votes in each round is withdraw from the dispute and the remaining cities follow to the next round of votes. The vote process ends when there are only two cities disputing or one city receives at least 50% of total votes in each round. It is interesting to note that in some cases the elected host city did not receive the highest sum of votes in the previous rounds and that the difference of votes is smaller in the final round. Also, there was a single city (Los Angeles) bidding to host the Games in 1984 and only one round of votes for the Games in 1968, 1980 and 1988.

As argued by Rose and Spiegel (2011), some unobservable differences between bid and non-bid countries may lead to the effect on the outcome of interest. Countries with more developed economy, political and institutional arrangements have systematically been chosen as hosts, suggesting that countries with higher GDP per capita are more likely to host the Games. Indeed, of 30 editions of the Games held between 1989 and 2012, 90% were held in cities whose country had high economic status. Even in developing economies hosting the Games, they also experienced episodes that boosted their economic activity. In fact, the 1968 Mexico Games was the first held in a developing country. The country

[8] In 2001, the IOC and the International Paralympic Committee (IPC) signed an agreement that guaranteed the automatic inclusion to host the Paralympic Games for the winner bid of the Olympic Games.

has experienced an average of 6% increase in GDP per capita between 1940 and 1980, an episode referred as the "Mexican Miracle". In its turn, South Korea has experienced a large economic development after 1970s, associated with an even more formidable advance in education, one of the pillars of this sort of performance that is both successful and stable (ZIMELIS, 2011). It is worth to mention the fact that China experienced above average economic performance since the 1990s. All these factors reinforce the importance of economic and social characteristics as fundamental to the choice decision of the host country.

1.2.2 *Data sources*

To access the causal ex-host effect the data consists of a unbalanced panel covering 127 countries observed during the time interval of 1948 and 2012[9]. The chosen time period allows us to observe the countries' medal count evolution from the first Games after the disruption during the World Wars until the last Games played. Further, data prior to this period are not readily reliable and are limited for some countries in database. In order to apply the synthetic control method, I set the treatment period as the announcement of the bid winner for a given Olympic Games. As argued by Billings and Holladay (2012), some impacts may occur between announcement and actual hosting which are related to the preparation, organization and implementation of the Games and are likely to create a temporary influx of economic activity. As I am interested in the long-term effect, taking into account such temporary effect, our focus is to compare the host country and its counterfactual before the announcement and after actual playing of the Games. Our treatment enables the comparison of the time trend prior any treatment effect and after all policy and institutional changes that culminate in the hosting period. The choice of the treatment period is consistent with Brückner and Pappa (2015), which confirm that macroeconomic effects associated with hosting the Games occur well in advance before the actual Games.

[9] The sample is unbalanced due the fact that participating nations increased during the analysed period from 59 in 1948 to 205 in 2012, as well to the the fact that some countries did not compete in at least one of the Games.

Tabela 1.1: Synthetic Control Setup

Variable	*Description*	*Source*
Outcome variable	Count medal	IOC Database
Predictors	Olympic team size	IOC Database
	Real GDP per capita (deflated CPI 2005)	Penn World Table
	Population growth	Penn World Table
	Human capital index	Penn World Table
	Life expectancy	World Bank Open Data
	Secondary scholl enrollment	Barro and Lee (2013)
	Polity IV index	Marshall and Jaggers (2002)
Treated units	6 host countries between 1968 and 2004	
Intervention period	Games prior the winner bid announcement	
Donor pool	OECD countries	
	All countries	

Note: The table presents the variables and data sources used in the study. Also I present basic information about the synthetic control setup.

Table 1.1 describes the variables and data sources used in this study. The outcome of interest is the number of medals awarded by a country in the Summer Olympic Games, observed every four years.[10]. Data on medal count is publicly available from the IOC Database, as well the size of national teams for all Games. A list of socioeconomic indicators with observed characteristics in pre-treatment period for both treated country and countries in donor pool are used in this study. I choose a set of covariates used in the cross-country growth literature: countries per capita gross domestic product (GDP) and population growth[11], both obtained from the *Penn World Tables 8.0*; secondary school enrollment from Barro and Lee (2013); and the *Polity IV* Index, a variable with information on the level of democracy for countries (MARSHALL; JAGGERS, 2002). Additionally, I include in the analysis two variables that capture the level of human development of

[10] I simple sum the number of gold, silver and bronze medals, not distinguishing the weight that each medal has to form the Olympic ranking as the current lexicographical criterion suggests. Other authors propose alternatives methods to rank countries as the allometric approach (MORTON, 2002), efficiency frontiers (MELLO et al., 2008) and the mean point of a constrained convex set (SITARZ, 2013), to name a few.

[11] Conclusions remain unchanged if I use alternative variables for population as total population count or a log-linearized version of it.

nations: population life expectancy, from World Bank Open Data, and the human capital index obtained from Feenstra, Inklaar and Timmer (2015).

1.2.3 *Selected case studies*

Table 1.2 presents basic information about the six selected Games used to estimate the ex-host effect. Among the selected countries, only Greece and Australia had hosted the Summer Olympic Games, in 1896 and 1956, respectively. In order to implement the synthetic control method to our comparative case studies it is necessary to observe the outcome of interest in both pretreatment and post-treatment periods. The first Games played after the World Wars disruption was held in London in 1948. I chose an average time period length of 20 years (equivalent to 5 editions of the Games) before the intervention to adjust the synthetic control for each case. Therefore, the first Olympic Games analysed here was the one held in Mexico in 1968[12]. The timing of the treatment is particularly important since the size of the pre-intervention period (T_0) is fundamental to check if the synthetic control estimators close resemble the pretreatment characteristics of the treated unit. As stated by Billings and Holladay (2012), the selection bid process and the timing of the Games themselves divides the analysis into three respective time periods: prior the host city winner announcement; the time between the announcement and the actual playing of the Olympics; and after the Olympic year. Since some effects may occurs during the time between the announcement and the hosting, the intervention year is settle to the closest Olympic year relative to the bid winner announcement.

The reasons I exclude the other four Games from our study are: (i) the Games in 1972 and 1980, held in Munich (West Germany) and Moscow (USSR), respectively, were not analysed here due their unstable political regime during the time analysed, that is, the division of Germany between 1945 and 1990 and the dissolution of the countries that were part of the Eastern block in 1991. So, I can not track these treated units for a time horizon sufficiently large after the treatment in order to capture the effect by comparing with its counterfactual as well as disentangle which component of the estimated effect is

[12] Variable values prior to 1950 are not systematically collected by most database. For example, the Penn World Table time series is collected from the year of 1950.

Tabela 1.2: Selected Games used for comparative case studies

	City (Country)	Bid winner Announcement	Intervention Year	Olympic Year	Analysed period
1	Athens (Greece)	Aug. 5,1997	1996	2004	1976-2012
2	Sydney (Australia)	Aug. 23, 1993	1992	2000	1972-2012
3	Barcelona (Spain)	Oct. 16, 1986	1988	1992	1964-2012
4	Seoul (South Korea)	Aug. 30, 1981	1980	1988	1960-2012
5	Montreal (Canada)	May 5, 1970	1968	1976	1948-2000
6	Mexico City (Mexico)	Oct. 18, 1963	1964	1968	1948-1988

Note: The table presents the country and city hosting the Games. “Bid winner announcement” refers to the year that the International Olympic Committe chose the host city; “Intervention Year” refers to the year before the treatment period; “Olympic Year” is the actual year the Games was played. The “Analysed period” is the sample period of study.

due to hosting or to the specific political regime[13]; (*ii*) Although important and widely analysed in previous studies, the Games in 1984 and 1996, both held in the United States, are not easily suitable in a synthetic control framework since the US has always had the highest total medal count, and I could not gain any information studying the US case, specially because I would not be able to construct a good synthetic control to mimic its performance before treatment period; (*iii*) Lastly, the Summer Olympics Games in Beijing in 2008, and London in 2012, were not analysed due to lack of additional information to conduct a counterfactual analysis in post-treatment period. Following the chronological order of the Games, I also analyse the editions of 1976 in Montreal, 1988 in Seoul, 1992 in Barcelona, 2000 in Sydney and 2004 in Athens (see table 1.2).

The donor pool is composed by all countries that, along with the hosting countries, participated in all Games during the analysed period and were not affected by the intervention. To account for similarity, a natural pool of potential comparison units would be members of the Organization for Economic Cooperation and Development (OECD). As discussed in subsection 1.2.1 countries with higher socioeconomic status are more likely to host the Games, and homogenize the sample only with OECD countries provide a better comparison group.

[13] Some papers try to precisely predict the economic performance of these countries after the intervention (political events) using the synthetic control approach. See, e.g., Abadie, Diamond and Hainmueller (2015) and Kennedy (2014).

1.3 Empirical Methodology

The causal effect of one action or treatment relative to another involves the comparison of the potential outcomes for the treated and control units. Estimates of the causal effect of hosting the Games on future sport success comparing actual host cities with those that never hosted is likely to be biased upward due the fact that host cities are presumably those with more capability to invest in infrastructure to promote national sport. Though, "controlling" for nations GDP help to minimize such bias but is not convincing in terms of causal identification.

As discussed by Cavallo et al. (2013), one can access longitudinal data to overcome the problem of the non-observed variables by assuming that they are time-invariant. However, this assumption only holds if countries in control group present similar pre-treatment trend relative to those affected by hosting the Games (IMBENS; WOOLDRIDGE, 2008).

One methodological strategy that relax the similar trend assumption is to adopt the synthetic control estimators, developed by Abadie and Gardeazabal (2003), Abadie, Diamond and Hainmueller (2010) and Abadie, Diamond and Hainmueller (2015), that allows the effects of confounding unobservable characteristics of countries to vary with time. The assignment to the treatment is based on the Olympic bid process, where the intervention takes place once the IOC select the host city.

1.3.1 *Synthetic Control Method*

In this section I present the synthetic control method (SCM) more in detail. Under this approach, one can observe the potential outcome of the hosting country in the absence of treatment through a weighted combination of potential untreated countries. Such construction tries to mimic the most important characteristics of the treated country in the pre-intervention period. After the intervention takes place (i.e., a country hosts the Games), the synthetic control is used to estimate the counterfactual situation.

Suppose that there is available data to $J+1$ countries[14] indexed by j. Without loss

[14] The more generic term used is *unit*. It refers to regions, states or, in our case, countries.

of generality, let the first country be the one that hosts the Summer Olympic Games and the J remaining countries serve as potential comparisons group. Let $j = 1$ is the treated country and units $j = 2$ to $j = J + 1$ are the control units that constitute the “donor pool”[15]. Additionally, assume that the sample is a balanced panel where all countries are observed at the same time periods $t = 1, \ldots, T_0, T_{0+1}, \ldots, T$. The pre-intervention periods are labelled as $t = 1, \ldots, T_0$ and the post intervention periods as $t = T_{0+1}, \ldots, T$.

Following Abadie and Gardeazabal (2003), Abadie, Diamond and Hainmueller (2010) and Abadie, Diamond and Hainmueller (2015), let Y_{jt}^N be the number of Olympic medals that would be observed for country j in time t in the absence of intervention for $j = 2, \ldots, J + 1$ and time $t = 1, \ldots, T$. Now, let Y_{jt}^I be the outcome that would be observed for the country j at time t if country $j = 1$ hosted the Games and its effects from period $t = T_{0+1}, \ldots, T$. Define

$$\alpha_{jt} = Y_{jt}^I - Y_{jt}^N \tag{1.1}$$

as the intervention effect for country j at time t and D_{jt} as a dummy variable that equals 1 if country j host the Games in period t and 0 otherwise. Therefore, this notation allow us to write the observed outcome for country j at time t as

$$Y_{jt} = Y_{jt}^N - \alpha_{jt} D_{jt}. \tag{1.2}$$

Since only $j = 1$ is exposed to intervention from periods $t = T_{0+1}, \ldots, T$, it follows that:

$$D_{jt} = \begin{cases} 1 & \text{if } j = 1 \text{ and } t > T_0 \\ 0 & \text{otherwise} \end{cases}$$

The parameters of interest are $(\alpha_{1T_{0+1}}, \ldots, \alpha_{1T})$. For $t > T_0$, Y_{1t}^I is always observable. Therefore, I need only calculate Y_{1t}^N in order to estimate the causal effect of host the

15 The term *donor pool* is widely used in the statistical matching literature and was first used in the SCM by Abadie, Diamond and Hainmueller (2010). Refers to a reservoir of potential comparison countries that were not affect by the intervention.

Games on the total number of Olympic medals in the subsequent Games as described in equation (1.1).

Let $W = (w_2, \ldots, w_{J+1})'$ be a $(J \times 1)$ generic vector of weights such that $w_j \geq 0$ for $j = 2, \ldots, J+1$ and $\sum_{j=2}^{J+1} w_j = 1$. Each particular value of W is equivalent to choose a synthetic control, that is, a vector of weights such that the characteristics of the host country are close resembled by the characteristics of synthetic control. Now define X_1 as a $(K \times 1)$ vector of observed characteristics of the treated country, and X_0 as a $(K \times J)$ matrix of the same variables for the countries in the donor pool. Both X_1 and X_0 may include pre-intervention values of the outcome variable[16]. For any particular choice of W, the difference of the pre-intervention characteristics of the treated unit and the synthetic control is given by $X_1 - X_0 W$.

Now suppose that there exists a set of optimal weights $W^* = (w_2^*, \ldots, w_{J+1}^*)$ that minimize the size of the vector $X_1 - X_0 W$. Likewise, the choice of $W^*(V^*) = [w_2^* \ldots w_{J+1}^*]'$ is derived by the solution to the minimization problem:

$$W^*(V) = arg \min_{W \in \mathcal{W}} (X_1 - X_0 W)' V (X_1 - X_0 W) \tag{1.3}$$

where $\mathcal{W} = \{W = [w_2^* \ldots w_{J+1}^*]' \in \mathbb{R}^J : w_j \geq 0 \, \text{for each} \, j = 2, \ldots, J+1 \, \text{and} \, \sum_{j=2}^{J+1} w_j^* = 1\}$ is a convex set and V is a diagonal semidefinite $(K \times K)$ matrix whose trace equals one. Furthermore, V is obtained by minimizing the difference of outcome variable of the treated unit and the synthetic control in the pre-intervention periods just as:

$$V^* = arg \min_{V \in \mathcal{V}} (Y_1 - Y_0 W^*(V))'(Y_1 - Y_0 W^*(V)) \tag{1.4}$$

where $\mathcal{V}$ is a set of diagonal positive semidefinite $(K \times K)$ matrix whose trace also equals one. Finally, the synthetic control estimator of the causal effect of the treatment is obtained by the comparison between the outcome variable of the treated unit and the synthetic

[16] As discussed in Ando and Sävje (2013), since no unit is affected by the intervention in the pre-intervention periods, the specific combination of the synthetic control could be adjusted using the observed outcomes from these periods.

control at the post-intervention periods:

$$\hat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}. \tag{1.5}$$

for the treated country and for each $t = 1, \dots, T$.

1.3.2 *Significance of Estimated Effects*

The use of traditional inference procedures in comparative case studies is a difficult task given the small-sample nature in some contexts, the presence of only one treated unit[17] are more than one treated unit and the absence of random treatment assignment. These restrictions prevent the direct application of statistical tests on the null hypothesis of no effect. In a synthetic control framework, a number of falsification tests are the pillars for the quantitative and qualitative inference of the estimators.

The first attempt to intuitively access the significance of the estimated effects in a synthetic control framework was proposed by Abadie and Gardeazabal (2003). Such inference procedure verifies graphically whether the synthetic control estimated effect for the treated unit is large relative to the empirical distribution of the estimated effects for the units not exposed to the intervention. Basically, it consists in a permutation test in which the vector of estimated effects $\hat{\alpha}_{1t} = [\hat{\alpha}_{1T_{0+1}}, \dots, \hat{\alpha}_{1T}]$ is compared with the empirical distribution of $\hat{\alpha}_{jt} = [\hat{\alpha}_{jT_{0+1}}, \dots, \hat{\alpha}_{jT}]$ for $j = 2, \dots, J+1$ and time periods $t = 1, \dots, T$. If the absolute value $|\hat{\alpha}_{1t}|$ is different of the distribution of $|\hat{\alpha}_{jt}|$, then the null hypothesis of no intervention effect must be rejected. In other words, if the treatment effect for a host country is large than the most placebo effects, then the ex-host effect may be plausible.

However, Abadie, Diamond and Hainmueller (2015) noticed that the value of $|\hat{\alpha}_{1t}|$ can be different when compared to the distribution of $|\hat{\alpha}_{jt}|$ for some $t = T_{0+1}, \dots, T$ but not for other time periods, what can be a misleading procedure to reject the null hypothesis.

[17] Cavallo et al. (2013) and Dube and Zipperer (2013) developed different ways to apply the synthetic control method when more than unit are affected by the intervention, the so called pooled intervention effect.

To overcome this problem, the authors propose using the empirical distribution of

$$RMSPE_j = \frac{\sum_{t=T_{0+1}}^{T}(Y_{jt} - \hat{Y}_{jt}^N)^2/(T - T_0)}{\sum_{t=1}^{T_0}(Y_{jt} - \hat{Y}_{jt}^N)^2/T_0}, \tag{1.6}$$

where RMSPE means root mean squared prediction errors. The proposed p-value is calculated by

$$p = \frac{\sum_{j=1}^{J+1} \mathbb{1}(RMSPE_j \geq RMSPE_1)}{J+1}, \tag{1.7}$$

where $\mathbb{1}(.)$ is an indicator function. One should reject the null hypothesis if p is less than some significance level, such as the value of 0.1. However, plausible p-values are not readily available due the small number of placebo units in each case and the existence of unit-specific transitory shocks that should be mean zero but are not averaged out in the estimator of the SCM. Following the idea of Mideksa (2013) for inference procedure, Ando (2015) proposes two placebo tests. The first test is based on the comparison of average estimated treated effects and placebo effects across years to minimize noise from unit-specific transitory shocks. Then the sizes of average treatment effects are compared to the empirical distribution of average placebo effects for all control units in all Games. The second test examines the average of average treatment effects across different Games is significantly different of zero, called overall treatment effects[18].

1.3.3 *Inference Procedure*

Although the SCM is widely used in a variety of empirical studies, no formal theory behind its inference procedure had been developed. Test the significance of the estimated intervention effect is of fundamental importance for the host decision. In this section I follow Imbens and Rubin (2015) and Firpo and Possebom (2016) and present the conditions in which the inference procedure of the synthetic control is valid. Also, I establish the general hypothesis to guarantee a causal effect of our estimators. The first is the *stable unit treatment value assumption* (SUTVA):

[18] For more detail on how implement these two tests see Ando (2015).

Assumption 1. The potential outcome vectors $Y_j^I = [Y_{j1}^I \dots Y_{jT}^I]'$ and $Y_j^N = [Y_{j1}^N \dots Y_{jT}^N]'$ for any country $j = 1, \dots, J+1$ do not vary with the treatments assigned to other countries and, for each country, there are no different forms or versions of intervention which lead to different potential outcomes vectors.

The first element of assumption 1 refers to the non-interference component of SUTVA - the treatment status applied to one country does not affect the outcome of others countries, i.e., no spill-over effects in space. The second element in SUTVA, the non hidden variations of treatments, requires that the country receiving treatment level do not receive different forms of that treatment, i.e., there is only a single dose treatment.

Assumption 2. The choice of which unit will be treated (i.e., which region is our region 1) is random *conditional on the choice of the donor pool.*

Assumption 2 requires that the treatment assignment for a host city is completely random. It seems not to be the case. Historically, the Olympic Games have been allocated to bid cities in developed countries and strong institutional and economic means to organize and create favourable conditions to the good functioning of the Games. Billings and Holladay (2012) states that cities bidding to host the Game are quite different from those not bidding in a variety of unobservable characteristics that influence the ability to host and generate benefits from the Games. I chose a pool based countries on observable variables that best approximates the characteristics of the host countries. To this end, I chose as donor pool countries members of the OECD. Firpo and Possebom (2016) discuss that assumption 2 can be interpreted as imposing only random treatment assignment conditional on observables, a standard condition in the evaluation literature also known as *ignorability* or *unconfoundness*.

Assumption 3. The potential outcomes $Y_j^I = [Y_{j1}^I \dots Y_{jT}^I]'$ and $Y_j^N = [Y_{j1}^N \dots Y_{jT}^N]'$ for each region $j = 1, \dots, J+1$ and time period $t = 1, \dots, T$ are fixed but a priori unknown quantities.

As a consequence of assumption 3 the potential outcomes vectors are considered fixed and the only random component is the vector of treatment assignments (IMBENS;

RUBIN, 2015, p. 86). The forth and last assumption states the sharp null hypothesis, that of no effect whatsoever of the treatment assignment:

Assumption 4. I establish the exact null hypothesis as $H_0 : Y_{jt}^{I} = Y_{jt}^{N}$ for each region $j = 1, \ldots, J+1$ and time period $t = 1, \ldots, T$.

Under these assumptions, the p-value in equation 1.7 is valid and known as *Fisher's Exact P-value* (FEP). By these procedure, Fisher et al. (1960) refers to access the null hypothesis of no intervention effect, that is, the null hypothesis under which the vector of potential outcome for the units are identical.

After establishing the conditions that guarantee the validity of the inference procedure, Firpo and Possebom (2016) also implemented an informative R function[19] that presents graphically the statistical significance of the estimated intervention effect through confidence sets and provides a reliable tool to qualitatively test the effect. Firpo and Possebom (2016) analyse the size and power of several test statistics through Monte Carlo experiments and find that those tests that use the SCM outperforms test statistics frequently used in the evaluation literature. The construction of a confidence set can shortly described as it follows. Assuming a constant intervention effect, $c \in \mathbb{R}$, a γ-confidence interval can be defined as $CI_{\gamma,\theta} = \{c \in \mathbb{R} : p_{\theta_c} > \gamma\} \subseteq CS_{\gamma,\theta}$, where $\gamma \in (0,1) \subset \mathbb{R}$, θ is an observed test statistic and $CS_{\gamma,\theta}$ is a confidence set containing all intervention effect function whose associated sharp null hypothesis are not rejected by the inference procedure. Under assumptions 1-3 , a γ-confidence set for the linear intervention effect can be written as

$$\tilde{CS}_{\gamma,\theta} = \left\{ \begin{array}{c} f \in \mathbb{R}^{\{1,\ldots,T\}} : f(t) = \tilde{c}(t - T_0)\mathbb{1}(t \geq T_0 + 1) \\ \text{and } p_{\theta_c} > \gamma \end{array} \right\} \subseteq CS_{\gamma,\theta}, \tag{1.8}$$

where $\gamma \in (0,1) \subset \mathbb{R}$ and $\tilde{CS}_{\gamma,\theta}$ is a confidence set that contains all linear in time intervention effects whose associated sharp null hypotheses are not rejected by the inference procedure.

[19] For a detailed explanation on how implement confidence sets in original gap plots for the synth package, see the author's web page (`https://goo.gl/4Jvd2W`).

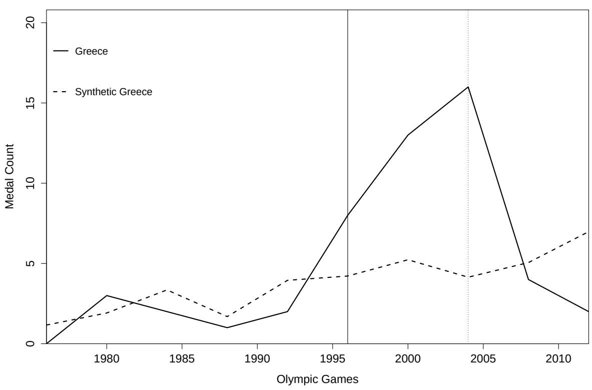
1.4 Results

In this section I present the main results of the causal ex-host effect for six countries whose cities hosted the Summer Olympic Games between 1968 and 2004. As discussed in subsection 1.2.3, the chosen comparative case studies analysed in this study reflect the fact that data is not readily available in some cases.

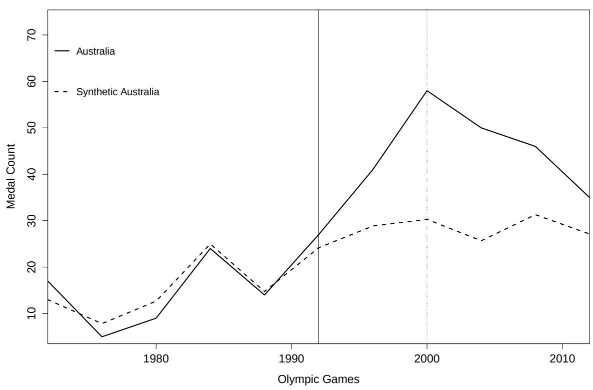
1.4.1 *Estimated effects*

The solution of the minimization in (1.3)-(1.4) for all comparative cases studies are presented in table A.2 (in Appendix A). The donor pool is composed by all countries members of OECD. Since the synthetic control is a convex set of weights, we must have positive weights with sum equal to one. For example, the synthetic Australia (see column 2 in table A.2) is a weighted average of France, Sweden and New Zealand, in this order of importance, while all other countries in donor pool obtain weights equal to zero.

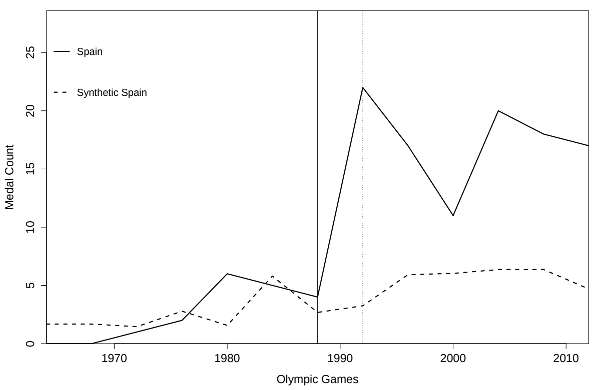
Before presenting the estimated ex-host effects, let us first compare how similar are the outcome of interest and the selected covariates values in the pretreatment period between the treated and the synthetic countries. Reliable causal synthetic control estimators require that the counterfactual unit resemble the main characteristics of the treated country in the period before the intervention. Table A.3 shows the covariates used in estimation of the synthetic control in all cases. The results indicate that the values of most pre-treatment covariates of the synthetic unit are closer to those of the treated country than the average values of the sample of countries in the donor pool. In particular, these results highlight an important feature of the synthetic control method since the treated and synthetic units are reasonably comparable, by excluding those units that do not present similar characteristics in the pretreatment period. Figure 1.1 presents graphically the levels and trends of the total number of medals for the host countries (solid line) and for the synthetic country (dashed line). The solid vertical line represents the time of intervention, i.e., the bid winner announcement time and the dotted vertical line represents the actual Olympic hosting. As can be seen, hosting the Games seems to have a decreasing but lasting effect on the number of medals awarded in the subsequent editions for most cases. In particular, the effect is sizable for Australia, Barcelona and South Korea: these countries experienced a large increase in terms of Olympic success in comparison of their synthetic controls. Differently, the size of the effect is negligible for Greece and Mexico. For these two host countries there are no relevant difference between the number of medal won in relation to their synthetic versions.



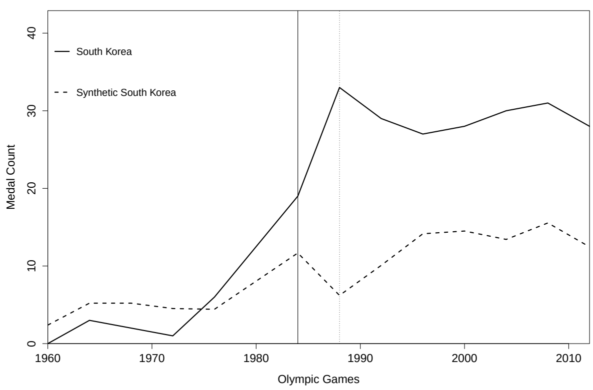
(a) Athens (GRE) 2004



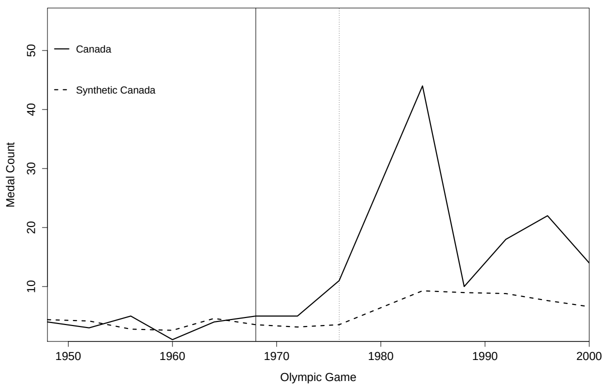
(b) Sydney (AUS) 2000



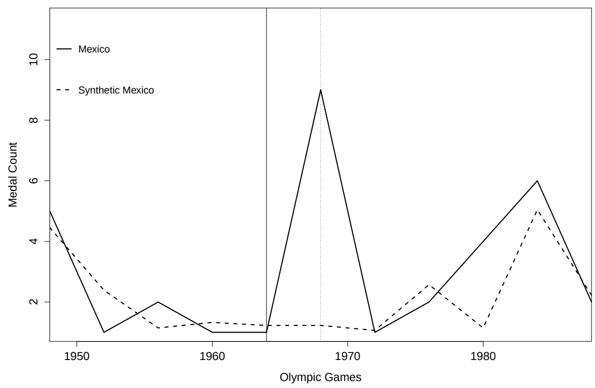
(c) Barcelona (ESP) 1992



(d) Seoul (KOR) 1988



(e) Montreal (CAN) 1976



(f) Mexico City (MEX) 1968

Figura 1.1: Olympic Medal Count: Treated and Synthetic Control Units Notes: The solid and dashed lines represents, respectively, the host countries and the synthetic countries. The Olympic medal counts is the sum of gold, silver and bronze medals. The solid vertical lines indicates the intervention time, i.e., the bid winner announcement time and the dotted vertical line represents the actual Olympic hosting.

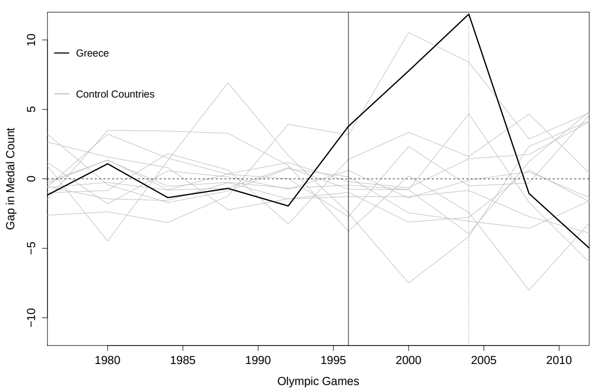
Table 1.3 summarizes the post-treatment gaps between the treated countries and the synthetic units. The gaps underline the graphical analysis presented in figure 1.2. On the one hand, the total medal count for Greece is on average 46% smaller than in the synthetic control. On the other hand, the gaps for South Korea indicates that the Olympic success on medal awarded is 120 % higher when compared with the synthetic control unit on average. Australia, Spain and Canada also presents higher number of medals count, with an average increase of 58.5 %, 185.7 % and 167 %, respectively.

Tabela 1.3: Post-Treatment gaps in the Synthetic control

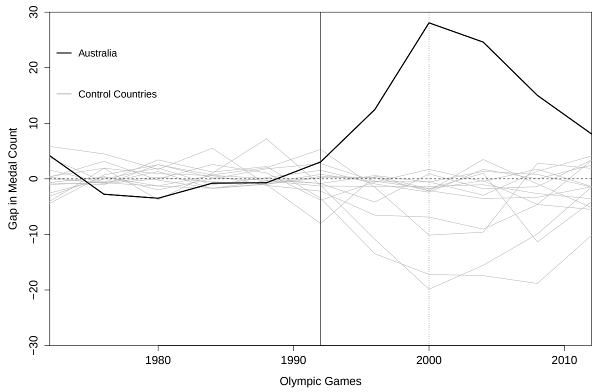
	Average		**Minimum**		**Maximum**	
Host	Gap	Percent	Gap	Percent	Gap	Percent
Athens (CAN)	-3.019	-46.120	-4.983	-71.360	-1.056	-20.880
Sydney (AUS)	15.910	58.490	8.082	30.020	24.610	96.940
Barcelona (ESP)	10.720	185.700	4.967	82.340	13.640	262.000
Seoul (KOR)	15.470	119.900	12.850	90.750	18.920	187.800
Montreal (CAN)	12.360	167.000	1.024	11.400	34.720	374.200
Mexico City (MEX)	0.589	45.970	-0.573	-22.260	2.855	249.300

Note: *Gap* is defined as the difference between the medal count for the treated unit and the synthetic unit. *Percent* is calculated by dividing *Gap* by the medal count for the synthetic control unit multiplied by 100. Average, minimum and maximum values are calculated over the post-treatment period.

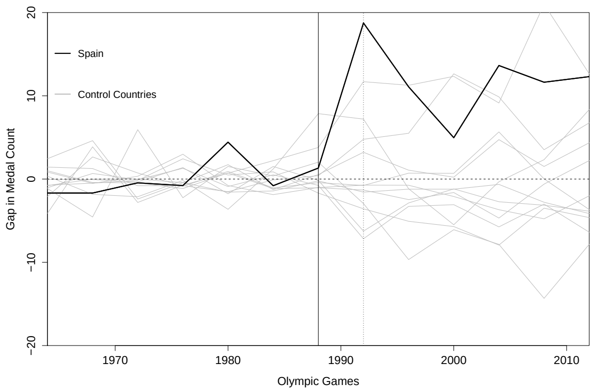
The minimum and maximum gaps values and difference percentage are presented in following columns of table 1.3. Nonetheless, the results should be taken cautiously. For example, even though Mexico experienced an average increase of 46% in medal counts in the post-treatment period it represents an average increase of less than one medal awarded (0.589). This is because the very limited Olympic performance in the pretreatment period and a small leverage can induce high percentage increase. Also, the sign of the gap is negative in some periods and positive in others, and an inference procedure based in these estimates should be misleading to reject the null hypothesis of no intervention effect.



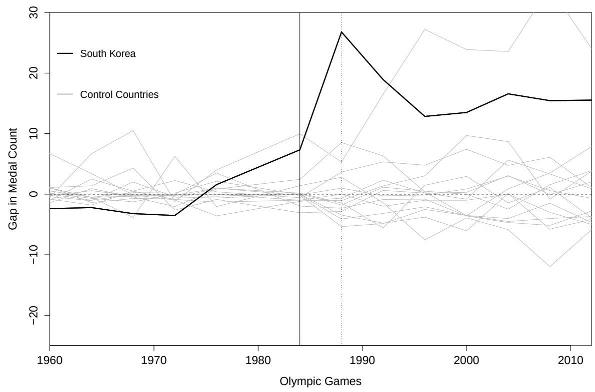
(a) Athens (GRE) 2004



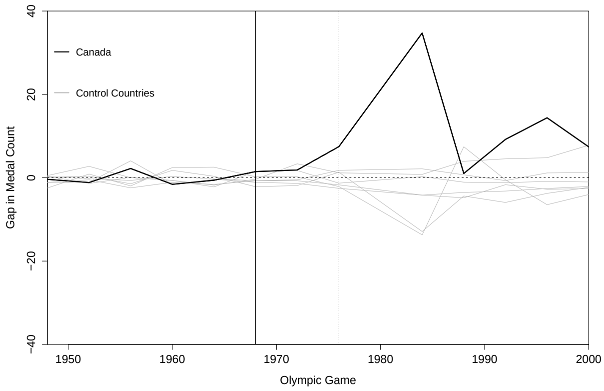
(b) Sydney (AUS) 2000



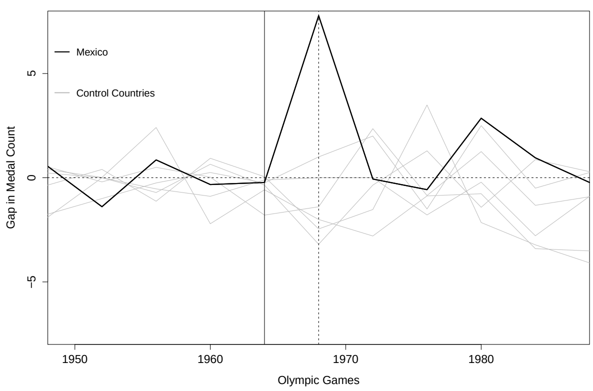
(c) Barcelona (ESP) 1992



(d) Seoul (KOR) 1988



(e) Montreal (CAN) 1976



(f) Mexico City (MEX) 1968

Figura 1.2: Gaps in Olympic Medal Count. Notes: Bold lines represents the gaps for treated countries. The gray lines correspond to the gaps for all countries in donor pool. I present only countries in donor pool with RMSPE values less than RMSPE of the treated unit

1.4.2 *Placebo tests*

Although the SCM has been widely used in the evaluation literature, no formal theory behind its inference procedure had been developed, a fact that casts doubt on the significance of the treatment effect estimators. Nonetheless, it has not prevented to develop alternative inference strategies to test whether the estimated effects are statistically significant. In this subsection, I conduct the placebo tests developed by Abadie, Diamond and Hainmueller (2010) and Abadie, Diamond and Hainmueller (2015), and two tests presented in Ando (2015). Finally, I introduce the inference procedure presented in subsection 1.3.3, which allows us to apply a new way to estimate confidence sets for the synthetic control estimators.

First, to test whether the estimate ex-host effects could be driven by chance, original placebo tests procedure developed by Abadie, Diamond and Hainmueller (2010) consists in the application of the synthetic control method to all units in donor pool that did not experienced the intervention during the period of analysis. Then, the distribution of placebo gaps is compared with actual treated unit. This is meant to access whether the treatment effect for the treated country is large relative to the effects for other countries chosen at random. After calculating all placebo estimates, the time trends of estimated treatment effect is analysed graphically. Figure 1.2 represents the placebo gaps. In each case, I exclude countries with five times higher RMSPE than treated unit, so to comparing with those countries with good fit in the pretreatment period. The results suggest that the ex-host effect is highly plausible for Australia, Spain and South Korea because their total medal count is larger than most of the placebo countries in the post-treatment period, and in less extent for Canada. Abadie, Diamond and Hainmueller (2015) noticed a problem with this strategy since the gap values of the treated unit can be different when compared to the distribution of placebo units for some $t = T_{0+1}, \ldots, T$ but not for other time periods, what can be a misleading procedure to reject the null hypothesis. The authors show that a large post-intervention RMSPE is not indicative of a large intervention effect if the pre-intervention RMSPE is also large. So, the ratio of the post-reunification RMSPE by its pre-reunification RMSPE is, in essence, used as an empirical p-value. Figure A.1 (appendix A) show the RMSPE ratio for all case studies. According to this inference procedure, only Canada ratio stands out in the distribution of ratios: post-treatment RMSPE is about 200 times the RMSPE for the pre-treatment period. In other words, if I pick a country at random from the entire sample, the likelihood of obtaining a ratio as high as this one would be $1/18 \approx 0.055$. RMSPE ratio values for the other cases are: Greece, 0.83; Australia, 0.13; Spain, 0.15, South Korea, 0.19; and Mexico, 0.57[20].

Although the original placebo procedure provides useful information about the likelihood of estimated treatment effects under the null hypothesis of no effect, plausible p-values are not readily available in general due to the limited number of placebo units in one case study - as in Montreal and Mexico City - and the existence of unit-specific transitory shocks that should be mean zero but are not averaged out in the estimator of the SC method. Ando (2015) proposes two test statistics[21] to investigate the significance of the intervention impact. The first test consists in compare the sizes of average treatment effects relative to the distribution of average placebo effects for all placebo units in all case studies, where the total number of average placebo effects are increased by summing placebo trials in different case studies. It consists in estimate all treatment effects $\hat{\alpha}_{g,t}$,

[20] I reject the null hypothesis of no effect if the value of p is less than some pre-specified significance level, such as the value of 0.1.

[21] The implementations of these two test are described in the online Appendix of Ando (2015). For more detail see `https://goo.gl/eOmVAe`.

and placebo effects $\hat{\eta}_{g,i,t}$ in all case studies, with g indicating one specific case study, i representing a control unit in donor pool and $t \in \{T_{0+1}, \ldots, T\}$. Then, I calculate the average treatment effects for the treated unit $\bar{\hat{\alpha}}_g$, and placebo units $\bar{\hat{\eta}}_{g,i}$ for all case studies in the post-intervention period. I use the distribution of $\bar{\hat{\eta}}_{g,i}$ for significance tests on $\bar{\hat{\alpha}}_g$, assuming that they follow the common distribution under the null hypothesis

The second test proposed by Ando (2015) check whether the overall average treatment effect is sufficiently large in comparison to the distribution of corresponding placebo estimates that are calculated with randomly chosen placebo units. The first step consist in calculate the overall treatment effect $\tilde{\alpha} = (\sum_{g=1}^{G} \bar{\hat{\alpha}}_g)/G$ and the overall placebo effects, $\tilde{\gamma} = (\sum_{g=1}^{G} \bar{\hat{\gamma}}_{i,g})/G$, where G is the number of case studies. Then, I replicate the previous step M times and create a distribution of overall average placebo effects $\tilde{\gamma}_m$, with $m = 1, \ldots, M$. Finally, the distribution of $\tilde{\gamma}_m$ is used for a significance test on $\tilde{\alpha}$.

The results of the two tests are graphically presented in figure 1.3. Taking into account all estimated placebo effects in all case studies ($N = 144$), I observe that the distribution of the average effect for all units have mean around zero, indicating that the treatment assignment has no systematic effects on the control units. Regarding the magnitude of the average effects observed in in part (a) of figure 1.3, I observe that average estimated effect in Spain, Canada, South Korea and Australia are larger than 95% of placebo effects (CDF > 0.95). On the other hand, the average estimated effect in Greece and Mexico are smaller than 5% of the estimated average placebo effects, so I do not have enough evidence to reject the null hypothesis of no ex-host effect for these two countries. The part (b) of figure 1.3 presents the second test, based on the overall average treatment effect. It shows that the overall average treatment effect is larger than the threshold line at $CDF = 99.5\%$ when I implement a number of repetitions $M = 10,000$. So, it is unlikely that the magnitude of this overall average treatment effect is generated by random errors. From figure 1.3, I can attest that regarding the results previously presented Greece and Mexico have no significant ex-host effect. Also, I can observe a highly heterogeneous size effects for the host countries and the analysis of the average estimated effects can be misdealing in terms of host decision.

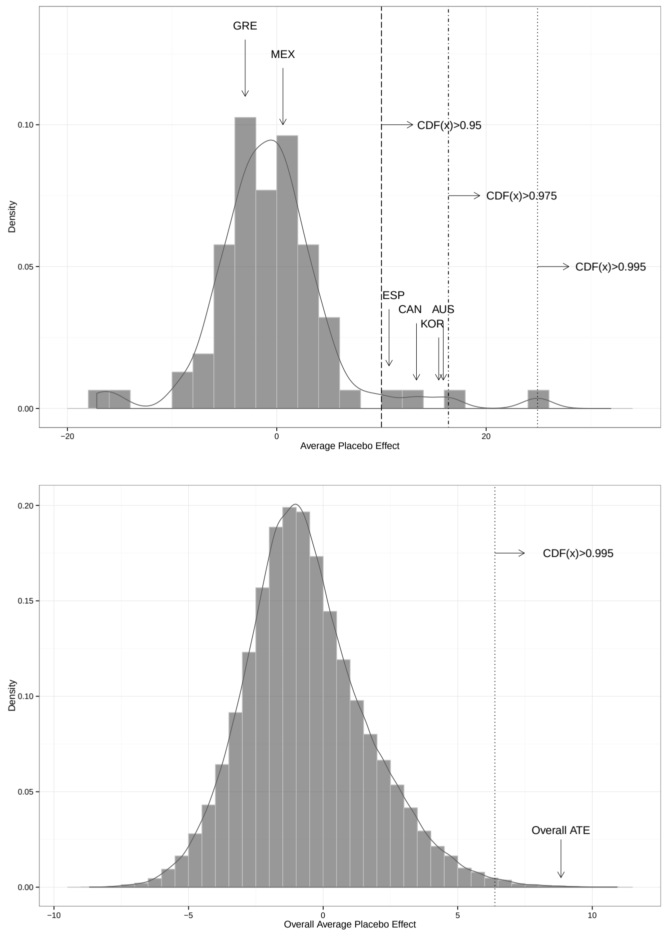
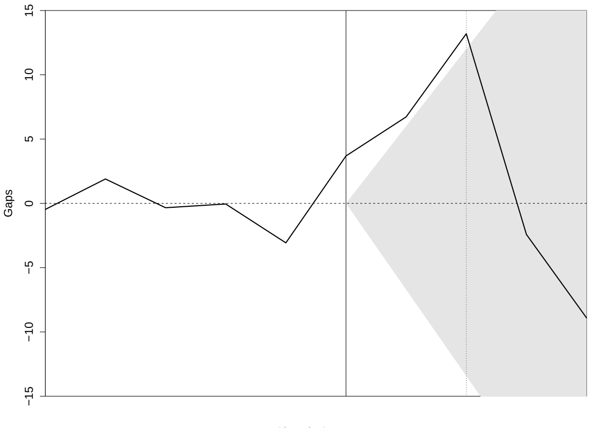


Figura 1.3: (A) Histogram of average placebo effects. Notes: Number of observation $N = 144$. CDF(x) is the empirical cumulative distribution function of average placebo effects; (B) Histogram of overall placebo effects. Notes: Number of Monte Carlo repetitions $M = 100,000$. CDF(x) is the empirical cumulative distribution function of overall average placebo effects.

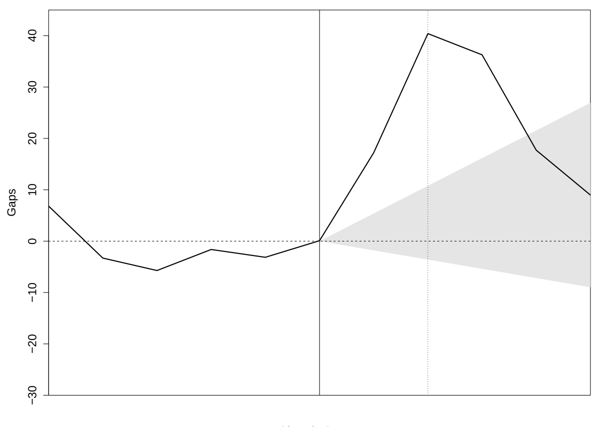
For a more formal analysis, I rely on the inference procedure proposed by Firpo and Possebom (2016). I construct a 88.9%-Confidence set[22] that contains all linear in time intervention effects whose associated null hypotheses are not rejected when I use the RMSPE test statistic. The results are shown in the figure 1.4. As a result of the empirical studies, I conclude that the ex-host effect does not cease right after hosting the Games, but lasts for one more edition to Australia, South Korea and Canada. The null hypothesis of no effect is indisputable not rejected for the cases of Greece, Spain and Mexico. This analysis emphasizes the potential gains of using confidence sets for precise analysis of the SCM estimators when I use robust statistical tests, as the RMSPE, compared to other

[22] Firpo and Possebom (2016) argue that at least 20 countries are required to build a 90%-Confidence set. This is not the case when I compare the treated countries with the pool of countries members of the OECD. Thus, a 88.9%-Confidence set is the closest to the usual 90%-Confidence set.

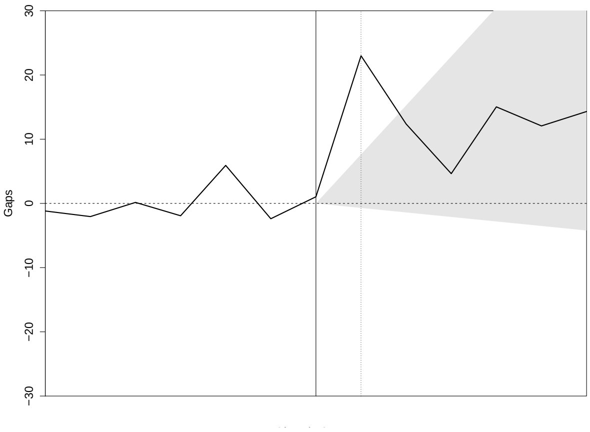
tests used in the evaluation literature.



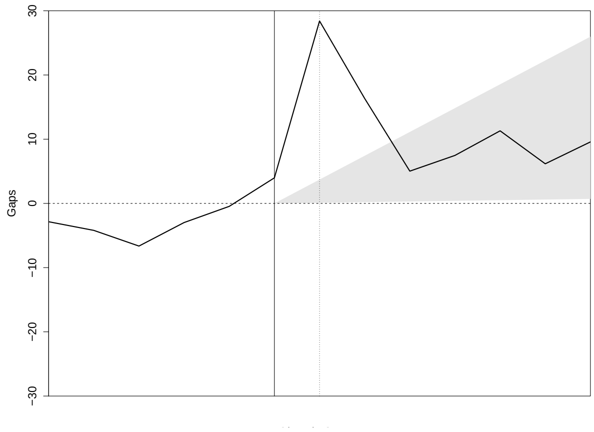
(a) Athens (GRE) 2004



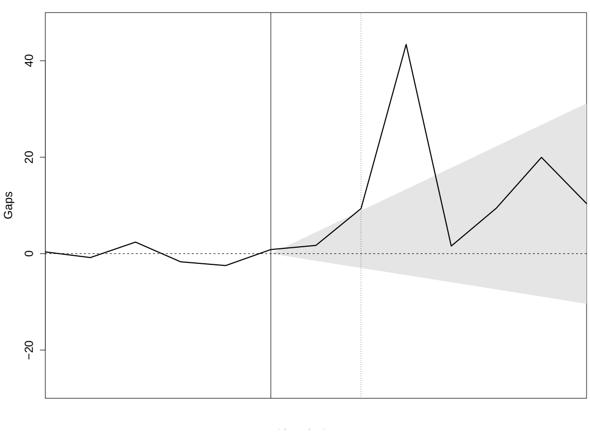
(b) Sydney (AUS) 2000



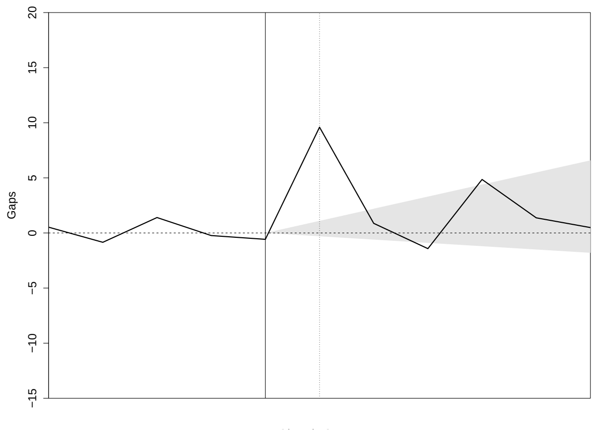
(c) Barcelona (ESP) 1992



(d) Seoul (KOR) 1988



(e) Montreal (CAN) 1976



(f) Mexico City (MEX) 1968

Figura 1.4: Confidence Set for Linear in Time Intervention Effects. Notes: Bold lines represents the gaps for treated countries and the gray area corresponds correspond the 88.9%-Confidence Sets for linear in time intervention effects when we use the RMSPE as test statistics.

1.5 Conclusion

This paper examines how hosting the Summer Olympic Games has influence on the number of medals won in the subsequent Games, known as the ex-host effect, for six quantitative case studies. Using synthetic control estimators I find that the ex-host effects are highly heterogeneous, positive for some host countries (Australia and Canada) and negligible for others (Greece and Mexico). I employ a variety of tests statistic widely used in the SCM literature that, although informative, casts doubt about the significance of the estimated effect. A new and compelling inference procedure developed by Firpo and Possebom (2016) is tested in order to be a definitive inference to the rejection of the null hypothesis. Through confidence sets and an informative graphic, one can easily observe the statistical significance of the estimators.

I stress out the fact that the estimated ex-host effect may be driven by the "durability effect" of athletes, that is, the ability of competing at high level for several Games, in which case his or her performance (and hence Olympic future success) is only a matter of funding. However, I argue that hosting the Games can create a Olympic breakthrough in terms of increased visibility and investments in sports, both for those that present greater chance of medal won and less traditional sports.

2 Natural disasters, economic growth and spatial spillovers: Evidence from a flash flood in Brazil

2.1 Introduction

In this paper we use a flash flood that occurred in the Brazilian state of Santa Catarina in 2008 to investigate the existence of spatial spillovers from natural disasters in geographically-linked areas. For that, we compare the GDP trajectory of municipalities affected by the flash flood to the trajectory of municipalities not affected by the flood in the years immediately before and after the occurrence of the disaster using a difference-in-differences model that explicitly considers temporal dynamics (AUTOR; KATZ; KEARNEY, 2008; HUSBY et al., 2014) and allows for the existence of spatial interactions within affected and unaffected regions (along the lines of Delgado and Florax (2015)). While the literature analyzing the economic effects of natural disasters is quite large, spatial interactions – to the author's knowledge – have been largely overlooked and neglected in previous studies. Our study therefore aims at answering the two following questions: first, what were the economic effects of this natural disaster on directly affected municipalities? Second, and more importantly, how much (if any) neighbouring regions were indirectly affected by it?

The direct impacts of environmental catastrophes have for long been in the research agenda of empirical economists, specially considering the huge effects that tsunamis, hurricanes, earthquakes and flooding may cause to economies all over the world. For instance, according to the according to the United Nations Office for Disaster Risk Reduction (UNISDR) and the Hyogo Framework for Action reports, between 2005 and 2015 around 700 thousand people have lost their lives, 1.4 million have been wounded and nearly 23 million have been made homeless as consequence of natural disasters. The total economic loss amount to more than $1.3 trillion. In the United States, the Hurricane Katrina, one of the strongest storms to impact the coast of the country in the last 100 years, left millions of homeless and an economic impact of approximately $150 billion (NEUMAYER; PLÜMPER; BARTHEL, 2014).

Those massive shocks led many to wonder about the true effect of a natural disaster. From a theoretical perspective, the notion that environmental disasters might have permanent long-run effects on income is not obvious (HSIANG; JINA, 2014). Income response to shocks may depend on the type and magnitude of the environmental disruption (KAHN, 2005) as well as on the level of development and institutional arrangement. More developed societies for instance may experience lower human and economic losses when compared to less well-off societies (KAHN, 2005; NOY, 2009; TOYA; SKIDMORE, 2007).

Following this rationale, the literature has laid out four competing possibilities for the growth trajectory succeeding a shock. The first one, known as the “no recovery” possibility, argues that a natural disaster may result in damage to infrastructure and physical capital, which hinders capital accumulation and thus negatively affects the economic growth rate. In addition, the disaster can also alter business expectations, scaring away new investments, and may trigger the migration of skilled and educated workers to unaffected areas (BARONE; MOCETTI, 2014), making post-disaster output to be permanently lower than its pre-disaster trajectory. A second possibility, the “recovery to trend” hypothesis, argues that a short-run negative shock exists but income levels should converge in the long-run back to their pre-disaster trend. According to Strobl (2011) and Yang (2008), the rebound back to pre-disaster trend may occur because individuals and wealth will migrate into devastated locations due to increases in the marginal product of capital when capital and labor become relatively scarce after the disaster due to destruction and mortality.

A third possibility, the “build back better” hypothesis, states that the shock may generate incentives for gradually building new and more efficient infrastructure compared to the capital that was destroyed in the disaster, leading to a positive net effect on long-run income levels (CUARESMA; HLOUSKOVA; OBERSTEINER, 2008). A final possibility, known as "creative destruction" hypothesis, argues that income may increase in the short-run due to greater inflow of financial aid arising from donations and loans and to increases in demand for goods and services as destroyed capital is replaced with new assets. The environmental disruption may even provide the impetus to adopt new technologies, leading to improvements in total factor productivity (SKIDMORE; TOYA, 2002).

From an empirical perspective, however, the debate is far from settled. Cross-country evidence provides mixed empirical support for the hypotheses considered. Cavallo et al. (2013), for instance, found no statistical relation between catastrophic natural disasters and economic growth, neither in the short nor in the long-run; the only exception occurring in extremely large disasters that were followed by radical political revolutions. More recently, Hsiang and Jina (2014), who analyzed 6,700 tropical cyclones during 1950-2008, showed that there was a long-term contraction in the income of affected countries.[1] The analysis based on cross-country data, however, may be subject to bias. First, with few exceptions, natural disasters occur in very specific areas, making the assumption that the entire country has been affected a little implausible. Second, since in the cornerstone of these analysis lies the assumption that treated and untreated countries can be compared, substantially unobserved - and difficult to control for - heterogeneity across countries may prevent one from obtaining a clear identification of causal effects. This has motivated researchers to focus on investigating the economic impacts of natural hazards from a regional perspective, analyzing specific events and using more disaggregate data.

Along this line, Xiao, Wan and Hewings (2013) recently investigated the economic effects of the 1993 flood that occurred in the Midwest region of the United States using a matching algorithm to select non-flooded control counties and an ARIMA intervention model. They showed that the flood caused severe economic disturbances, mostly in the short-run, even though a persistent contraction in the agricultural sector was observed. Barone and Mocetti (2014) examined two large-scale earthquakes that occurred in two different Italian regions in 1976 and 1980, one in the Friuli region (north of the country)

[1] See also Kahn (2005); Noy (2009); Toya and Skidmore (2007).

and other in the Irpina region (south of the country). Using a synthetic control approach, the authors showed that, discounting financial aid, both quakes had a negative impact in the short-term GDP. In the long-run, however, their findings indicate a positive effect in one case and a negative effect in the other one. While the Friuli region experienced increases in its GDP per capita, the Irpina region suffered a 12% decrease in GDP per capita. The authors suggest that differences in institutional quality between the regions may explain post disaster behaviors.

Using nightlight data from satellite images, Elliott, Strobl and Sun (2015) evaluated the impact of typhoons on economic activity in the coastal area of China. The results show that typhoons generated heavy short-term losses to the local economy. Their estimates also show a net economic loss of US$ 28.34 billion from 1992 to 2010. Tanaka (2015) investigated the economic impact of an earthquake that struck the Japanese city of Kobe in 1995. Using plant-level data and a methodology based on a difference-in-differences strategy with matching, the author showed that the plants that survived in Kobe's most devastated districts experienced severe negative shocks in employment and value added growth during the first three years that followed the quake.

While the main motivation of these cross-country and cross-regional papers has been to investigate the positive/negative effects of natural disasters on economic outcomes,[2] issues related to spatial interactions have been largely overlooked. This is especially important in cross-regional analysis, since economies interact with each other through commercial transactions, labor mobility, technology diffusion and/or sharing of infrastructure assets (ERTUR; KOCH, 2007; LESAGE; FISCHER, 2008; SARDADVAR, 2012). It is reasonable therefore to expect that neighbouring regions also suffer the consequences of a natural disaster, even indirectly. For instance, the destruction of important infrastructures, such as bridges and highways, might compromise the economic growth of all economies that share its use in some way (CRESCENZI; RODRÍGUEZ-POSE, 2012). Additionally, when a region suffers a negative shock, their ability to transact goods

[2] Recently, researchers started using micro-level data to investigate the long-run effects and intergenerational transmission of disasters on individuals' outcomes. See, for instance, Caruso (2015); Imberman, Kugler and Sacerdote (2012); Vigdor (2008).

and disburse funds for new investments is reduced, compromising also the performance of neighbouring areas who would eventually benefit from spillover effects. Therefore, ignoring spatial interactions by treating regions as islands may cause the model to (potentially) underestimate the economic consequences of natural disasters.

Our study aims to contribute to this literature by explicitly considering the spatial interactions of natural disasters. We evaluate therefore not only the direct effect of the disaster, but allow our difference-in-differences model to capture also the indirect effects of the shock on neighbouring regions. In order to do so, we apply the spatial extension of the difference-in-differences estimator recently proposed by Delgado and Florax (2015). As case study, we investigate the economic impact of the flash flood that occurred on the northern coast of the Brazilian state of Santa Catarina in 2008. According to the state's Civil Defense, this was the worst disaster in the state history, with over 1.5 million people affected, 32,853 displaced, 5,617 made homeless and 135 killed. This is an interesting case to be evaluated, since it occurred in a relatively rich region of a developing country[3]. In addition to considering spillover effects, our paper is among the few that use disaggregate data at the municipality level to evaluate how each of the three economic sectors – agriculture, industry and services – responds to the flash flood.

Our difference-in-differences results showed that municipalities directly affected by the flood suffered a 10% decrease in GDP per capita in the year following the disaster. Three years after the flood, however, GDP per capita rebounded back to pre-disaster levels; the only exception occurring in the agricultural sector, which experienced a decrease of about 22% in the first year after the shock and a statistically significant decrease of about 8% three years after the flood. Regarding our spatial estimations, our results show that spillovers exist and are economically relevant. We find an indirect impact of the flash flood of about -1.4% to -3.6%. In this way, ignoring the spatial spillovers leads to an underestimation of disasters effects and can generate sub-optimal recovery policies.

[3] To our knowledge, the study of Ribeiro et al. (2014) is the only one that assesses the economic impact of this event. Using a synthetic control method, the authors show that the flash flood caused a monthly reduction of 5.13% in industrial production in the state of Santa Catarina. A drawback of their analysis is the use of aggregate data at the state level, which can lead to inaccurate estimates, especially since the disaster occurred in a very specific and concentrated area of the state.

Our study is structured as follows: in section 2.2 we describe the study area and provide details about the natural disaster to be evaluated. Section 2.3 presents the empirical strategy and section 2.4 describes the data and the variables used in the analysis. In section 2.5 we present the results and in section 2.6 we discuss the main empirical findings and policy implications.

2.2 The 2008 Flash Flood

Our study area comprises the Brazilian state of Santa Catarina, located in the southern part of Brazil. With an area of 95,736.165 km^2, the state has a population of 6.8 million inhabitants and is one of the most developed regions in the country, with a GDP per capita of R$ 29,350 (IBGE, 2013). Compared to other Brazilian states, Santa Catarina has the lowest illiteracy rate (3.2%), the lowest infant mortality rate (9.49%), the highest life expectancy (78.77 years) and the lowest levels of income inequality (Brazilian Demographic Census 2010).

The state is located in a geographic area highly vulnerable to natural hazards. From 1992 to 2012, according to official records provided by Brazilian Atlas of Natural Disaster (2012), the state registered 4,999 natural disasters, 34% of which being flash floods, followed by dry spells and droughts (30%), windstorms (13%), hail (11%) and floods (9%). The flash floods and floods[4] occur predominantly in the basin of the Itajaí River (northeastern region of Santa Catarina) and its potential for destruction is amplified due to increasing urbanization pressures on the natural environment (STEVAUX; MARTINS; MEURER, 2009). These events often occur in summer and spring, due to increases in humidity that facilitates tropical convections and to the mesoscale convective complexes (MCC), specifically favoring the occurrence of flash floods (Brazilian Atlas of Natural Disaster 2012).

In November 2008, the state of Santa Catarina was hit by a series of heavy rains that resulted in a catastrophic flash flood, impacting more than seventy cities concentra-

[4] The flash floods differ from floods due to their sudden occurrence, being caused by heavy rains (short time), having quick shifts and are more associated with hilly areas.

ted in the northeastern area of the state. In the affected municipalities, like Blumenau and Joinville, total rainfall of November 2008 almost surpassed 1,000 mm, much above Novembers' historic average of 150 mm. In the case of Blumenau, for instance, such volume exceeded the total rainfall observed in October 2008 by 355%. Figure 1 gives us a clear picture of the magnitude of the phenomenon. The figure plots the monthly rainfall observed in November 2008 and the maximum historic values recorded by Epagri, a Santa Catarina government think tank, and the National Water Agency (ANA) for five affected cities.

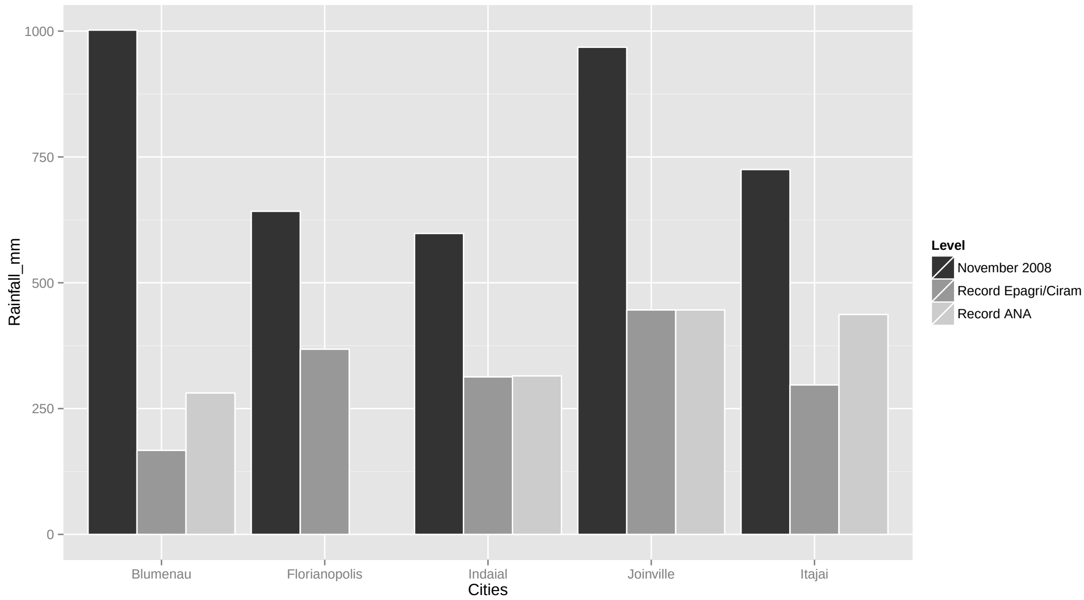


Figura 2.1: Cumulative Rain Level in Nov/2008 compared with the previous monthly records.

Source: Epagri/Ciram and National Water Agency (ANA).

According to Stevaux, Martins and Meurer (2009), the event was a result of two simultaneous weather events: a high pressure anticyclone that spread in the southern Atlantic Ocean coast and a cyclonic vortex of low pressure, which caused the ascension of air masses and the formation of rain clouds. The 2008 flash flood caused immense human damage due to landslides. According to the Civil Defense of Santa Catarina, 1.5 million people were affected by the event, 32,853 were displaced, 5,617 were homeless and 135 were killed. The severity of the 2008 event was such that it accounts for 80.3% of deaths from all flash floods that have occurred in the state during the period from 1992 to 2012 (Brazilian Atlas of Natural Disaster 2012).

In terms of economic damage, the natural disaster occurred in an area with high industrial concentration and affected major urban centers like Florianópolis, Blumenau, Joinville and Itajaí, cities that concentrate 34.4% of the state GDP and 22.8% of its population (Brazilian Demographic Census 2010). Regarding infrastructure, the floods and landslides caused interceptions on roadways and highways, disrupted the Brazil-Bolivia gas pipeline (Gasbol), interrupted business in the Itajaí harbor, caused the destruction of agricultural assets and deteriorated most residential capital around the affected region. In the state, 63 municipalities declared state of emergency and 14 declared a state of public calamity. Figure 2 shows the geographical distribution of these municipalities.

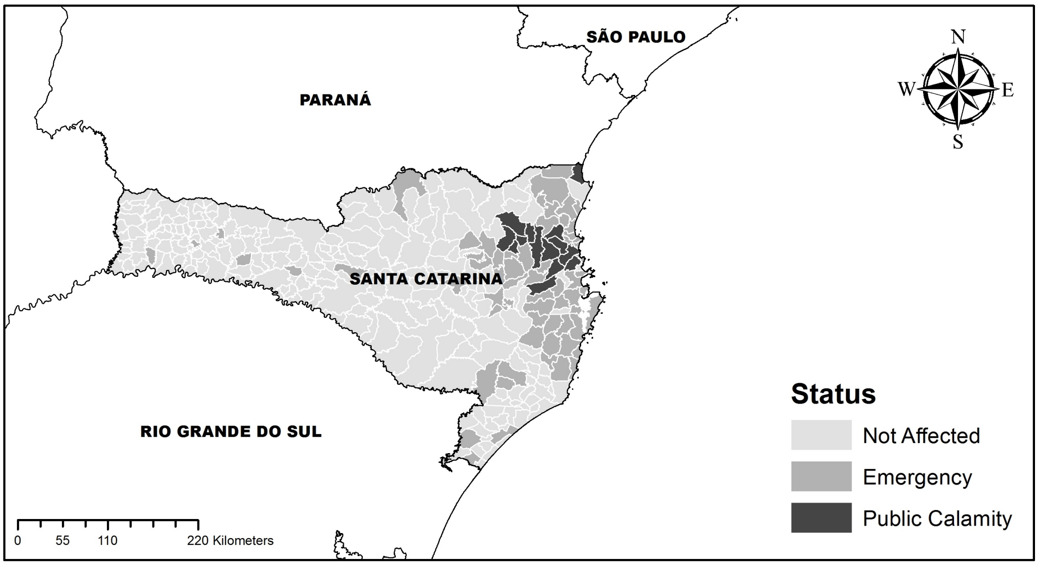


Figura 2.2: Geographical Distribution of Affected Municipalities.

Source: Own elaboration based on information from the Civil Defense of SC.

Due to its tremendous socioeconomic consequences and the amount of lives affected, the November 2008 disaster is considered by the World Meteorological Organization (WMO) and by the United Nations (UN) as the worst catastrophe in the history of the state of Santa Catarina.

2.3 Empirical Strategy

In this section, we first present the empirical strategy we adopt to identify the impact of the 2008 Santa Catarina flash flood on GDP per capita of affected municipalities. We then describe the method through which we investigate the existence of potential spillover effects.

2.3.1 *Direct effects*

Regarding the direct effects of the 2008 Santa Catarina flash flood on GDP, we start with a standard municipal-level fixed-effects model that calculates the difference between the GDP before and after the flash flood for treated and untreated municipalities. This strategy, widely used in all areas of empirical economics, has recently been used to measure the economic impact of unanticipated natural disasters on a regional perspective by Husby et al. (2014) and Tanaka (2015). Our basic specification is given by the following equation:

$$y_{it} = \phi Flood_{i2008} + \sum_{k>2008} \eta_k Flood_{ik} + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it}, \tag{2.1}$$

where y_{it} is the log of GDP per capita of municipality i at year t, $Flood_{i2008}$ is a dummy variable that assumes the value of 1 if municipality i was affected by the natural disaster in 2008 and 0 otherwise, and $Flood_{ik}$, where $k > 2008$, are dummy variables representing treatment effects for k years after 2008. X_{it} is a vector of controls and μ_i and λ_t are, respectively, municipality and year fixed effects. ε_{it} is an error term, clustered at the municipality level in all estimations to allow for an arbitrary covariance structure within municipalities over time.

The municipality fixed effect included in the model nonparametrically controls for municipality time-invariant unobservable characteristics, such as municipalities' fixed geographical aspects. The time fixed effect nonparametrically controls for yearly differences in GDP level common to all municipalities, such as macroeconomic aspects that can affect GDP. Finally, the vector of municipality characteristics, X_{it}, controls for time-varying characteristics that might be correlated with the shock and with municipalities' GDP.[5]

The parameters of interest are given by ϕ and η_k, for $k > 2008$. ϕ represents the causal effect of the shock on GDP for the first year directly affected by the treatment.

[5] In the next section we specify in more detail the variables included in this vector.

It calculates therefore the difference between the average of the outcome of interest for the first year after the shock and the average of this outcome before the shock for treated and untreated municipalities. In a similar manner, η_k allows us to evaluate the time heterogeneity of the impact (temporal perspective), assessing whether the impact of the natural disaster is temporary or persistent.

As it is widely known, to interpret these parameters as causal we must rely on the assumption that there is no time-varying unobserved variable that is simultaneously correlated with our treatment and outcome variables, hence excluding the possibility of omitted-variable bias (ANGRIST; PISCHKE, 2008). Although this assumption may seem strong for several empirical applications, we highlight that the natural disaster we analyze is hardly anticipated. Municipalities can predict rainfall a few days/hours before it happens, but not months before. Hence, simple comparison between affected and unaffected regions may deliver the causal effect of interest.

We note however that a few natural disasters, such as floods and flash floods, may impact certain regions more frequently than others. It might be possible that these regions invest more in disaster preparedness, alleviating the potential impacts of a flood. If these investment differences change between municipalities but are fixed across time, the municipality fixed effect we include in the model should be sufficient to allow for a causal interpretation of the estimates. On the other hand, if investment changes across the time span we consider in our analysis, then we might face problems in identifying the isolated impact of the 2008 flash flood. We note however that preparedness tends to alleviate destruction, making our estimates likely to be a lower bound of the potential effect.

As it follows, although we cannot directly test if trajectories differ substantially between treated and control municipalities, since we cannot observe the treated group in the absence of treatment (ANGRIST; PISCHKE, 2008), we can test the robustness of our estimates to the existence of dynamic changes that might coincide with the occurrence of the flood. For that, we consider estimating model 2.1 with additional dummies indicating years before the disaster. We check therefore whether causes happen before consequences, in line with Granger (1969), by allowing the model to have heterogeneous anticipatory effects (leads), denoted by $Flood_{ik}$, where $k < 2008$, in addition to the heterogeneous post-treatment effects (lags) already included in the model (AUTOR, 2003). We estimate therefore:

$$y_{it} = \sum_{k<2008} \omega_k Flood_{ik} + \phi Flood_{i2008} + \sum_{k>2008} \eta_k Flood_{ik} + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it}. \quad (2.2)$$

If the model we estimate in equation 2.1 incorrectly attributes pre-existing trends in the outcome to our treatment effect, then dummies indicating years before the occurrence of the flash flood should matter in equation 2.2 and anticipatory effects, captured in ω_k, should be significant.

2.3.2 *Indirect effects*

Moving to estimates of potential spillover effects, we relax an important assumption required for the validity of the basic difference-in-differences estimator which is the Stable Unit Treatment Value Assumption (SUTVA). This assumption implies that potential outcomes for the unit i are unrelated to the treatment status of units j (Angrist, Imbens and Rubin (1996), Delgado and Florax (2015)). However, as aforementioned, we expect to find significant spillover effects within the municipalities studied because of spatial dependence between local economies. In fact, there is strong evidence in favor of a positive spatial relation in the economic growth of Brazilian regional economies (Lima and Neto (2016), Özyurt and Daumal (2013), Resende (2011)).

Following this rationale, we apply the spatial extension of the difference-in-differences estimator recently proposed by Delgado and Florax (2015). This strategy allows us to explicitly consider the local spatial dependence of the treatment variable, so that the outcome of municipality i depends not only on their own treatment, but also on the treatment status of close neighbors. This framework has been increasingly adopted in the impact evaluation literature (CHAGAS; AZZONI; ALMEIDA, 2016; DUBÉ et al., 2014; HEC-

KERT; MENNIS, 2012) to measure spatial treatment effects. The extension we consider is given by the following equation:

$$
\begin{aligned}
y_{it} = \phi Flood_{i2008} + \delta \sum_{j=1}^{n} w_{ij} Flood_{j2008} + \sum_{k>2008} \eta_k Flood_{ik} + \\
\sum_{j=1}^{n} w_{ij} (\sum_{k>2008} \theta_k Flood_{jk}) + \gamma X_{it} + \mu_i + \lambda_t + \varepsilon_{it}.
\end{aligned}
\tag{2.3}
$$

This equation includes a spatial lag of the treatment dummy (second term on the right hand side) as well as spatial lags for post-treatment dummies (fourth term on the right hand side). The coefficient ϕ measures the average direct treatment effect (ADTE) and the coefficient δ multiplied by the average proportion of treated neighbors measures the average indirect treatment effect (AITE), or the spillover effect of the flash flood. In this case, the control group is composed of municipalities that are neither directly nor indirectly treated (DELGADO; FLORAX, 2015).

The terms w_{ij} are elements of the spatial weight matrix W, which captures the neighborly relationship between municipality i and municipality j. In the present study, we use two types of spatial matrices: I) Binary Contiguity: w_{ij} takes the value of 1 if i borders j and 0, otherwise; II) k-Nearest Neighbors: w_{ij} takes the value of 1 if j is one of the k-nearest neighbors of i and 0, otherwise.

2.4 Variables and Data

The data we use to investigate the direct and indirect effects of the 2008 flash flood on the economic performance of the Santa Catarina municipalities consists of a balanced panel for the years between 2006 and 2010. Since our study area is often affected by natural disasters, we choose this small time interval to eliminate two less intense shocks that might affect/confound our estimates. In 2004, for instance, the southern Brazilian states were hit by the “Catarina” hurricane, which affected about 40 municipalities in the State of Santa Catarina and caused four deaths. In January 2011 another flash flood occurred in the state and affected 83 cities and caused six deaths.

2.4.1 *Treatment definition*

An important issue when estimating the effects of disasters is related to the criteria used to define municipalities affected by the shock. A common choice has been to use rainfall levels and their deviations from historic averages as a measure of disaster exposure. As we argued in section 2.2, however, the 2008 flash floods were characterized by a combination of heavy rain, flooding and landslides. Since these floods and landslides were responsible for most of the damage, and these are not perfectly predicted by rainfall variation within municipalities, using rainfall as proxy for exposure may be inadequate. We choose therefore to use a more objective measure of disaster exposure: areas declared under a state of public calamity. Accordingly, the Brazilian Ministry of Integration establishes a few objective criteria that must be satisfied for cities to declare a state of public calamity: I) 10 or more individuals killed, or 100 individuals affected; II) 10 or more public health/education facilities destroyed; and III) economic loss above 8.33% of the municipality's net current revenue. Hence, due to these strict criteria, we expect only municipalities that were heavily affected by the floods to be considered as treated.

Aside from those who declared a state of public calamity, a few municipalities geographically far from the most heavily affected region (northeastern state) declared state of emergency. This is a milder signalling of a natural hazard, but more prone to endogeneity since no objective criteria has to be satisfied for those who declare an emergency status. There is in fact evidence that the recognition of emergency status by the federal government is related to partisan position (political alignment) of the municipality mayor (CAVALCANTI, 2016), as a mechanism to facilitate federal transfers. In that regard, we add political controls (as described below) to test for this heterogeneity and perform sensitivity analysis. As a robustness exercise, we also define treatment based on a measure of human damage; we consider treated municipalities as those who had homeless or dead individuals.[6]

[6] There are 25 municipalities matching this criterion.

2.4.2 *Outcome and control variables*

The dependent variable is the municipality GDP per capita, constructed annually by the Brazilian Institute of Geography and Statistics (IBGE). The choice of covariates was based on the empirical literature of regional growth (Crescenzi and Rodríguez-Pose (2012), Lall and Shalizi (2003), LeSage and Fischer (2008)). From IBGE we use data on population size, urbanization rates[7] and the share of agricultural and manufacturing sector on the GDP. We also include public investment in capital as a proxy for physical capital investment, which is measured annually by the Department of National Treasury (STN), subordinated to the Brazilian Ministry of Finance. Related to urban infrastructure we use information on the proportion of households with access to piped water, access to electricity and garbage collection. This information is made available annually by the primary care information system (SIAB), from the Ministry of Health.

In addition to the socioeconomic variables that might determine our outcome variable, we consider adding covariates related to local politics. Following Barone and Mocetti (2014), we include election turnout as proxy for civic engagement, which is a measure of institutional quality. In addition, as partisan alignment has been shown to affect local growth (NOVOSAD; ASHER, 2016), we add two dummy variables that link the political alignment between local and state (federal) governments. This variable takes value 1 when the mayor and the governor (president) are from the same party and 0 otherwise. All political variables were obtained from the Brazilian Electoral Superior Court (TSE).

In table 2.1 we present descriptive statistics for the variables used in our analysis. We note that municipalities affected by the flash flood are richer when compared to unaffected municipalities. Also, they present higher volume of public investment, better urban infrastructure and are more industrialized.

7 Since the urban population size is only available for the years 2007 and 2010, values for the other years were estimated via interpolation since it has a steady trend.

Tabela 2.1: Descriptive Statistics

	Treated		Control	
	Mean	SD	Mean	SD
GDP per capita (1000)	26.639	17.268	20.909	10.18
Population (1000)	58.388	80.722	18.751	45.933
Public Investment (in Million)	33.268	52.222	9.652	22.551
Share of Agriculture	0.050	0.047	0.265	0.177
Share of Manufacturing	0.344	0.114	0.229	0.148
Urbanization Rate	0.773	0.208	0.557	0.240
% Served by Piped Water	0.663	0.227	0.553	0.249
% Served by Garbage Collection	0.914	0.110	0.650	0.253
% Served by Electricity	0.991	0.006	0.979	0.035
Alignment with President	0.100	0.302	0.099	0.299
Alignment with Governor	0.386	0.490	0.387	0.487
Electoral Turnout	0.885	0.041	0.890	0.051

Note: SD corresponds to the standard deviation. The GDP per capita and public investment are deflated to R$ of 2000. Electoral turnout is the ratio between the number of voters who attended the elections and the total electorate.

2.5 Results

In this section we present the direct and indirect effects of the 2008 Santa Catarina's flash flood on GDP per capita of the affected municipalities. We also present the effects taking into account the three economic sectors. At the end of the section we access the spatial spillover effects on geographically linked regions.

2.5.1 *Direct effects of the 2008 flash flood*

Table 2.2 presents the direct effect estimates of the Santa Catarina flash flood on GDP of the affected municipalities. As stated above, the empirical strategy consists in estimating the presence of either short-run or persistent effects on GDP of the affected units. Column (1) refers to the model with time and municipality fixed effects, while in columns (2) and (3) we include socioeconomic and political control variables, respectively.

According to the column (1) the flash flood led to an average drop of about 10% on GDP per capita in the year that municipalities were hit by the disaster. The same intense and significant result was obtained for the second year after the flash flood. Only in 2010 the GDP per capita rebounded back to the patterns observed for the unaffected municipalities. These findings suggest the existence of short-term negative effects on eco-

Tabela 2.2: Impact of Natural Disasters on GDP: Benchmark Specification.

	(1)	(2)	(3)
$Flood_{2008}$	-0.1045***	-0.1027***	-0.0847***
	(0.027)	(0.028)	(0.028)
$Flood_{2009}$	-0.1053***	-0.0978***	-0.0771***
	(0.031)	(0.031)	(0.034)
$Flood_{2010}$	-0.0328	0.0113	0.0211
	(0.041)	(0.043)	(0.046)
Time Fixed Effects	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓
Socioeconomic Controls		✓	✓
Political Controls			✓
Observations	1465	1465	1465
Adjusted R^2	0.9259	0.9319	0.9326
F-Statistic	62.14	65.84	65.91

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. The socioeconomic and political controls are reported in Table 1. Non-dichotomous control variables are in logarithm format.

nomic activity, with a quick recovery to pre-disaster levels. This is along the lines of the "recovery to trend" hypothesis, also observed in the recent cross-regional literature reported in Elliott, Strobl and Sun (2015), Husby et al. (2014) and Xiao, Wan and Hewings (2013). Results are quantitatively the same when we add socioeconomic controls in our main specification (column 2), and present marginal changes when political controls are added (column 3). The reduction of the magnitude of the damage observed in column (3) may indicate that the political alignment between the executive power of the different entities of the federation can facilitate the reception of transfers, political windfalls and donations, in line with the work of Cavalcanti (2016).

In order to capture any pre-trend differences between treated and control units, table 2.3 presents the results considering anticipatory effects, as depicted in equation 2.2. Here we consider the specification with one lead and two lags time periods. Column (1) represents the model with time-fixed effect, municipality fixed-effects, and socioeconomic control variables; and column 2 controls for political variables.

For both specifications, the coefficients measuring anticipatory effects are not statistically significant. Put in another way, one can argue that there are no significant diffe-

Tabela 2.3: Impact of Natural Disasters on GDP growth: Leads and Lags Specification.

	(1)	(2)
$Flood_{2007}$	-0.0386	-0.0384
	(0.024)	(0.024)
$Flood_{2008}$	-0.1213***	-0.1031***
	(0.036)	(0.037)
$Flood_{2009}$	-0.1163***	-0.0956***
	(0.040)	(0.043)
$Flood_{2010}$	-0.0072	0.0026
	(0.050)	(0.053)
Time FE	✓	✓
Municipality FE	✓	✓
Socioeconomic Controls	✓	✓
Political Controls		✓
Observations	1465	1465
Adjusted R^2	0.9319	0.9326
F-Statistic	65.62	65.7

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. The time-varying controls are reported in Table 1. Non-dichotomous control variables are in logarithm format.

rences between the GDP trajectories for control and later affected municipalities. If there was another unobserved factor leading to a drop in the GDP per capita of the affected municipalities, then it would have had contemporaneous effects on the same economies, which is hardly credible. The lagged coefficients for 2009 and 2010 have the same pattern as the results obtained in table 2.2.

In table 2.4 we present the results of two robustness tests. In the first test we use as alternative treatment status municipalities that suffered some human damage, defined as rather homeless or dead people as consequence of the flood. This criterion is less restrictive than the state of public calamity declaration and comprises a larger number of affected municipalities. In the second robustness test, the control group is composed of municipalities with observable characteristics similar the affected ones' (columns 3 and 4). The matching procedure consists in estimating the propensity score for each municipality[8] before the parametric estimation (HO et al., 2007). To improve balance between treated

[8] For more details about this methodology, see Caliendo and Kopeinig (2008). We employ a nearest neighbour algorithm for the construction of a new control group, composed of 14 municipalities.

and control units, the matching estimation is useful to reduce the model dependence, remove outliers and minimize potential selection bias.

Tabela 2.4: Impact of Natural Disasters on GDP growth: Robustness Checks.

	Human Damaged		Matched Sample	
	(1)	(2)	(3)	(4)
Flood_{2008}	-0.0897***	-0.0713***	-0.0777**	-0.0763**
	(0.023)	(0.024)	(0.038)	(0.039)
Flood_{2009}	-0.0878***	-0.0691***	-0.0658	-0.0472
	(0.025)	(0.027)	(0.044)	(0.049)
Flood_{2010}	0.0017	0.0097	-0.0030	0.0143
	(0.032)	(0.033)	(0.059)	(0.061)
Time FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Socioeconomic Controls	✓	✓	✓	✓
Political Controls		✓		✓
Observations	1465	1465	140	140
Adjusted R^2	0.9322	0.9327	0.9721	0.974
F-Statistic	66.1	66	113.8	114.4

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. The time-varying controls are reported in Table 1. Non-dichotomous control variables are in logarithm format. In column (1) and (2) the treated municipalities are those who have had some human damage, while in column (3) and (4) the treatment and control groups are composed after the matching estimation.

According to table 2.4, changes in treatment criteria and the composition of the control group do not change the results for the year the flash flood occurred, even tough the results are quantitatively smaller. It is noteworthy that the negative impact disappears one year before the natural disaster took place (columns 3 and 4) mainly because the sample size is drastically reduced after matching.

2.5.2 *Heterogeneity by intensity of damage and by economic sectors*

The use of public calamity declaration as a proxy for disaster exposure may be tricky because it does not reveal the intensity of the damage. Even if one knows whether the municipality was affected by the disaster there is not a clear picture of the size of the damage. An intuitive way to obtain some insights regarding the destruction level is creating an alternative treatment group composed of the municipalities affected by the flood, but at a lower magnitude than those that claimed public calamity. As discussed in section 2.4, when a natural event occurs in a particular region the local government can claim for emergency or public calamity status. Thus, the use of the state of emergency dummies in equation 2.1 as treatment status is a reasonable strategy to attain the differences in shock intensity[9]. One can expect that municipalities that have enacted state of emergency also suffer from the shock, but less intensively than those who claimed public calamity. Table 2.5 shows the results of this empirical exercise.

Tabela 2.5: Impact of Natural Disasters on GDP growth: the extent of the damage.

	(1)	(2)
Emergency_{2008}	-0.0356**	-0.0324*
	(0.017)	(0.017)
Emergency_{2009}	-0.0213	-0.0211
	(0.017)	(0.018)
Emergency_{2010}	-0.0056	-0.0075
	(0.019)	(0.021)
Calamity_{2008}	-0.1148***	-0.0966***
	(0.029)	(0.029)
Calamity_{2009}	-0.105***	-0.085**
	(0.033)	(0.036)
Calamity_{2010}	0.0079	0.0168
	(0.0439)	(0.0470)
Time FE	✓	✓
Municipality FE	✓	✓
Socioeconomic Controls	✓	✓
Political Controls		✓
Observations	1465	1465
Adjusted R^2	0.9321	0.9327
F-Statistic	65.37	65.39

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. The time-varying controls are reported in Table 1. Non-dichotomous control variables are in logarithm format.

Municipalities that have enacted state of emergency status presented a 3.24 % reduction in the GDP per capita in the year the flash flood occurred, while those that enacted public calamity presented a drop of 9.66% (see column 2). It is noteworthy that the effect fades away immediately in the year following the disaster in the case of emer-

[9] The causal interpretation of the state of emergency dummies should be done with caution since, as discussed in section 2.4, this variable is potentially endogenous.

gency status. This evidence suggests the existence of heterogeneous effects in the disaster exposure and the use of emergency and calamity status resemble this fact.

The results presented so far show that the 2008 flash flood intensively harmed the economic activity of the affected municipalities. An important aspect for damage management policy design and financial aid plans is to understand how these effects spread through different economic sectors. Although some quantitative research is focused in measuring the economic impact of natural disasters on economic outcomes, little is known about their impact on distinct economic sectors. This gap in literature hampers the development of better mechanisms to promote the reestablishment of affect areas. In this sense, the shock affects the economic sectors in different ways and to test this hypothesis we re-estimated equation 2.1 considering the GDP per capita in different economic sectors: agriculture, manufacturing and services. Table 2.6 below summarizes the results.

Tabela 2.6: Impact of Natural Disasters on GDP growth: different sectors.

	Agriculture	Industry	Services
	(1)	(2)	(3)
Flood_{2008}	-0.1949***	-0.0952***	-0.0441**
	(0.048)	(0.039)	(0.020)
Flood_{2009}	-0.2066***	-0.0785	-0.0352
	(0.065)	(0.049)	(0.028)
Flood_{2010}	-0.0816**	-0.0524	0.0194
	(0.041)	(0.061)	(0.027)
Time FE	✓	✓	✓
Municipality FE	✓	✓	✓
Socioeconomic Controls	✓	✓	✓
Political Controls	✓	✓	✓
Observations	1465	1465	1465
Adjusted R^2	0.9466	0.9868	0.9969
F-Statistic	84.64	357.4	1535

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. We exclude the sharing of agriculture and manufacture from the time-varying controls. Non-dichotomous control variables are in logarithm format.

All economic sectors are negatively affected by the natural disaster, although in different magnitudes. The agricultural sector is the most damaged by the flash flood[10].

[10] As the share of manufacturing and services in the economy of the damaged areas is much higher

In the year of the disaster, the agricultural GDP of the treated municipalities is reduced on average by 19.49%. This figure is twice as large as the impact size on manufacturing GDP, and approximately four times the size on GDP of the tertiary sector.

Due to the nature of the shock, this result is quite expected. The floods can destroy completely the agricultural production systems, and it is almost impossible to protect the farms and agricultural assets from shocks like these. As mentioned in introduction, the study of Xiao, Wan and Hewings (2013), which investigated the economic consequences of a 1993 Midwest flood, also found that the impacts on the agricultural sector were negative and long-lasting. Additionally, this evidence is also consistent with the finding of a recent report of the Food and Agriculture Organization of the United Nations (2015), which found that, in developing countries, the agricultural sector absorbs about 22% of the economic damage caused by natural disasters.

2.5.3 *Spatial Spillovers*

The evidence shown above reveals that the flash flood caused a negative economic impact on the affected municipalities, but one may consider the existence of spillover effect in neighbouring areas. Due to the regional linkage between economic activity, it is reasonable to expect that some spatial effects were neglected by the previous results. A particular region may be directly affected by the disaster (when the event occurs within its own boundary) or may be affected indirectly (when the event occurs in the vicinity of the region). Additionally, the SUTVA is unlikely to hold in studies focused on the impact of disasters in a regional perspective. Following the methodology proposed by Delgado and Florax (2015), table 2.7 shows the estimated results from equation 2.3.

Columns (1), (2) and (3) show model specifications similar to those estimated in section 5.1, while column (4) presents the specification that considers the spatial lags of the covariates. This model is widely used in spatial econometric literature[11] and is known

than the share of agriculture, the absolute impact of the disaster is greater in the first two sectors. Considering the average GDP of the affected municipalities and the coefficients described in table 6, it is estimated that the shock caused (per municipality) a reduction of R$ 3.71 million in agriculture, R$ 56.17 million in industry and R$ 49.7 million in services.

11 Besides adding spatial lag in the covariates, another option would also include the spatial lag in the

as SLX model (VEGA; ELHORST, 2015).

Tabela 2.7: The Indirect Impact of Natural Disasters on GDP growth: spatial difference-in-differences specification.

	(1)	(2)	(3)	(4)
Flood_{2008}	-0.1098***	-0.1076***	-0.0895***	-0.0758***
	(0.0274)	(0.0281)	(0.0282)	(0.0250)
Flood_{2009}	-0.1192***	-0.1098***	-0.0888***	-0.0808***
	(0.0320)	(0.0319)	(0.0341)	(0.0307)
Flood_{2010}	-0.0450	-0.0001	0.0100	0.0165
	(0.0414)	(0.0440)	(0.0467)	(0.0425)
W*Flood_{2008}	-0.0950	-0.0910	-0.1092	-0.1335
	(0.0863)	(0.0890)	(0.0894)	(0.0943)
W*Flood_{2009}	-0.2521***	-0.2361***	-0.2492***	-0.2734***
	(0.0964)	(0.0936)	(0.0936)	(0.1012)
W*Flood_{2010}	-0.2204*	-0.2220**	-0.2223**	-0.2361*
	(0.1163)	(0.1097)	(0.1122)	(0.1213)
Time FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Socioeconomic Controls		✓	✓	✓
Political Controls			✓	✓
Neighborhood Controls				✓
Observations	1465	1465	1465	1465
Avg. Prop. of Treated Neighbors	0.0526	0.0526	0.0526	0.0526
R^2	0.9266	0.9326	0.9333	0.9333
F-Statistic	62.18	65.9	66.04	61.18

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. The time-varying controls are reported in Table 1. Non-dichotomous control variables are in logarithm format. We use the binary contiguity matrix (weighted) in all estimations.

As it can be observed, the results related to the direct impact remain unchanged when compared to the benchmark specification (see table 2.2). However, there is evidence that a particular economy is also affected indirectly by the shock: the lags' coefficients for the neighbour treatment variable are negative and statistically significant. Thus, one year after the event, municipalities that have a neighbour affected by the 2008 flash flood showed a decline in its GDP per capita of about 1.4% (0.27x0.0526)[12] and that a contraction of 1.24% (0.2361x0.0526) occurred two years after the event. These findings

dependent variable, specification known as Spatial Durbin Model (SDM). However, the consideration of global spatial dependence on a difference-in-differences framework is still a developing point (DELGADO; FLORAX, 2015)

12 As discussed in section 3, the average indirect treatment effect (AITE) is measured as the multiplication between the spatial lag coefficient and the respective average proportion of treated neighbours. Using the binary contiguity matrix, we observed that this value is 0.0526 (Table 9).

reveal that the municipalities indirectly affected by the natural hazard did not suffer the consequences immediately, only in years after the shock (2009 and 2010).

There are two possibilities that may help explain this result. Firstly, the intensity of economic interactions among directly affected and neighbouring areas can have some degree of time seasonality. For example, such interactions may occur predominantly in the early months of the year, so that in November 2008 (month that the flash flood occurred), most of the interactions had already been materialized. Secondly, it is likely that the economic effect of a drop in the demand for goods and services from neighbours can only be felt with a certain delay.

Tabela 2.8: The Indirect Impact of Natural Disasters on GDP growth: different spatial weight matrices.

	(1)	(2)	(3)	(4)
	$k = 2$	$k = 4$	$k = 6$	$k = 8$
Flood_{2008}	-0.0742***	-0.0775***	-0.0832***	-0.0813***
	(0.0225)	(0.0239)	(0.0258)	(0.0290)
Flood_{2009}	-0.0737***	-0.0724***	-0.0768***	-0.0700**
	(0.0288)	(0.0292)	(0.0314)	(0.0342)
Flood_{2010}	0.0229	0.0275	0.0213	0.0264
	(0.0402)	(0.0398)	(0.0412)	(0.0448)
W*Flood_{2008}	-0.0922	-0.1511*	-0.2466***	-0.3102***
	(0.0620)	(0.0863)	(0.1033)	(0.1217)
W*Flood_{2009}	-0.1320**	-0.2205**	-0.3306***	-0.3900***
	(0.0584)	(0.0913)	(0.1217)	(0.1461)
W*Flood_{2010}	-0.0929	-0.1818**	-0.2564*	-0.2730*
	(0.0599)	(0.0911)	(0.1320)	(0.1519)
Time FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Socioeconomic Controls	✓	✓	✓	✓
Political Controls	✓	✓	✓	✓
Neighbohood Controls	✓	✓	✓	✓
Avg. Prop. of Treated Neighbors	0.05631	0.0538	0.0552	0.0516
Observations	1465	1465	1465	1465
R^2	0.934	0.9336	0.933	0.9339
F-Statistic	63.47	62.1	60.65	60.62

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. We use robust standard errors that were grouped at the municipal level. The standard deviations are presented in parentheses. The dependent variable is the logarithm of GDP per capita. The time-varying controls are reported in table 1. Non-dichotomous control variables are in logarithm format. We use the k-nearest neighbor matrix (weighted) in all estimations.

Finally, table 2.8 shows that indirect impacts are insensitive to specifications with different spatial matrices (k-Nearest Neighbor), indicating robustness. Thus, the evidence showed in this section indicates that spillover effects are far from negligible and neglecting them may lead to undermined results of the real economic impacts of natural disasters. Further, aid-relief policies should take into account the potential indirect effects of natural disasters.

2.6 Concluding Remarks

This paper assessed both direct and indirect effects caused by the flash flood that occurred in the Brazilian state of Santa Catarina. In order to deal with the endogeneity of the treatment definition, which is municipalities that were affected by the disaster, we used as objective criteria of disaster exposure those municipalities declared under a state of emergency or public calamity status. We estimated contemporary and dynamic effects of the flash flood on the economic performance of the affected areas. Also, we allowed the existence of spillover effects on unaffected but geographically related areas in the state.

We found that municipalities directly affected by the flash flood had a significant drop of 8.5% on GDP per capita immediately after the disaster and a decrease of 7.71% in the following year. The economic performance of the municipalities rebounded to the pre-disaster level only after two years. When considering different economic sectors we observed that the agriculture sector does not present the same recovery pattern and has more permanent consequences. The results for spatial spillovers suggest that indirect effects are far from negligible due the economic and social interactions between municipalities. These results are particularly important for aid policies aimed at mitigating overall loses, which should also consider the neighboring affected regions.

3 Mobile Guardianship and Crime Deterrence: Evidences from a Natural Experiment in Brazil

3.1 Introduction

The decline in crime rates observed in many industrialized countries from the mid-1990s to 2000s has led to a growing interest of criminologists and policy makers regarding the causes of the phenomenon. A range of relationships analyzed by theoretical and empirical researchers have been suggested as forces that could drive the crime drop, including the increase in police numbers (EVANS; OWENS, 2007), the growth in the prison population (BUONANNO; RAPHAEL, 2013; ZIMRING, 2011), more efficient security policies (LEVITT, 1997; MARVELL; MOODY, 1996; TRAVIS; WAUL, 2002), gun law reforms (LOTT, 2013), economic conditions and its impact on unemployment rate (FREEMAN, 2001), demographic changes (BLUMSTEIN; RIVARA; ROSENFELD, 2000), the access to drug markets (LEVITT, 2004), as well as factors associated with the legalization of abortion (DONOHUE; LEVITT, 2001). The conclusions drawn from these studies are sensitive to estimation methodology and tend to have little external validity. Besides, there is no a single factor associated with the decrease in criminal activity.

In addition to the reasons explaining the crime drop, some authors suggest that the increase in private security in public spaces is associated with a significant reduction in the number of crimes (BROOKS, 2008; COOK; MACDONALD, 2011). Based on the security hypothesis framework, Farrell et al. (2011) discuss that changes in the quantity and quality of private security have played a major part in driving crime falls in most developed societies. Linked to routine activity and crime opportunity theories, Klick, MacDonald and Stratmann (2012) suggest a novel and underappreciated link that may have contributed to the crime drop: the growth and improvements of mobile phone technology. Using available mobile phone data for the US, the authors found a negative association between mobile phones and violent crimes, although data limitation and the lack of natural experiment or instrumental variables are a critical barrier for causal interpretation.

In the classical work of Becker (1968), offenders commit a crime if the expected benefit of such activity exceed the associated costs. The expected benefits include any monetary or physical reward obtained by committing a crime. The expected cost associates the likelihood of being punished and the utility loss of the punishment. Most security policies are focused on that side of the equation, i.e., increasing the likelihood of offenders have been punished by committing illegal activities. In this sense, mobile phones provide additional surveillance and change criminal's perceived risk of apprehension.

This study contributes to the literature by using a quasi-experiment to bring preliminary evidences of the effect of mobile phone use on crime: the implementation of the ninth digit to mobile phone numbers in Brazil. This change was meant to eliminate the shortage of available numbers and increase the numbering capacity for new users. Conducted by Brazil's National Telecommunication Agency (ANATEL), the change in mobile dialing consisted to add the digit 9 before the current eight-digit numbers. The introduction of the ninth digit was first implemented in the municipalities of the state of São Paulo with area code 11 on July 2012, while other area codes in the state remained with the eight-digit numbers[1].

The introduction of the ninth digit caused an unexpected and significant reduction in the number of accesses by users as some calls were not completed, either because systems adaptation or some widespread misunderstandings about the new rule. This episode has provided the environment for a natural experiment to identify the impact of the mobile phone on crime. If the "mobile guardianship" hypothesis for crime drop is true (FARRELL; TILLEY; TSELONI, 2014), one should expect that exogenous shocks on mobile technology have impact on crime. Here I assume linearity in the sense that a reduction in the number of mobile access may cause an increase in the number of offenses.

[1] According to ANATEL's schedule, all mobile phones in Brazil will be standardized with the ninth digit by the end of 2016.

The empirical strategy is based in two different (and complementary) approaches to estimate the causal effect of the reduced number of mobile calls on crime and victimization. First, I apply a temporal difference-in-differences estimator to compare the trajectory of criminal activities between the municipalities affected by the adoption of the ninth digit and other municipalities in the state, but with different area codes during the period between January 2012 and June 2013. This time period allows us to disentangle the effects of other policies in the state of São Paulo during the 2000s, as the adoption of dry laws (BIDERMAN; MELLO; SCHNEIDER, 2010), which restricted recreational consumption of alcohol by imposing mandatory closing hours for bars and restaurants in the São Paulo metropolitan region, or the nationwide firearms legislation control that restricted the legal firearms possession (CERQUEIRA; MELLO et al., 2013), for example.

Second, this paper uses a regression discontinuity approach to estimate the local treatment effect on homicides. In particular, I compare the incidence of homicides before the implementation of the ninth digit with the incidence on the first day the new rule was applied. Falsification tests are conducted on the basis of untreated municipalities.

The results show that the ninth digit caused a significant impact on homicides and bodily injury, but only persistent for the latter. The results for rape show that there is a positive impact on the number of victims in the month following the introduction of the ninth digit. For vehicle and property theft, however, there is no significant effect. These finds are closely related to Klick, MacDonald and Stratmann (2012), which hypothesize that mobile phones have the largest impact for violent crimes and a lesser impact for property crimes. The results have clear implications for security policies such as the expansion of mobile technology and other technology-based instruments to deter crime.

The rest of the paper is organized as follows. Section 3.2 presents a simple model that links mobile use and crime, as well as the institutional background for changes in numbering plan. Section 3.3 establishes the empirical strategy used to access the causal effect on crime. Section 3.4 describes the data. Section 3.5 contains the empirical results and discuss possible explanation for them. Section 3.6 offers some concluding remarks.

3.2 Background and Numbering Plan in Brazil

In this section I briefly present a simple pathway linking the usage of mobile and crime deterrence. Next, I will show how the changes in numbering plan have occurred in Brazil, beginning in the state of São Paulo for the municipalities with specific code area. It is argued that the exogenous change provides a reliable scenario to obtain some evidences of mobile drop calls effect on crime.

3.2.1 *Theoretical link: a simple model*

The routine activity theory suggests that the mechanism by which crime occurs is based on the convergence of three basic factors: (i) *suitable targets*; (ii) *potential offenders*; and (iii) *lack of capable guardianship* (COHEN; FELSON, 1979). Within this context, Klick, MacDonald and Stratmann (2012) argue that mobile phones provide additional surveillance against potential offenders and increase capable guardianship for suitable targets of crime. The mechanisms which explain how the mobile phones prevent crime are twofold. First, carrying a mobile phone could increase the likelihood of a victim provide quicker reporting of crimes and, in some cases, real time detailed information[2]. Second, the mobile phones can be used to anticipate the offender's decision and change the perceived risk of apprehension, increasing the probability of being punished.

The growth and the widespread usage of the mobile phones increases the probability of reporting crimes in a relatively short time interval since it has occurred. In this sense, the mobile technology greatly reduces the cost of reporting a crime, providing more detailed information on crimes occurred (*ex-post* actions) and real threats of the victimization (*ex-ante* actions). Although the relationship between the usage of mobile phones and crime reduction is in contact with the modern discussions of the private security expansion policies in preventing illegal activities, Klick, MacDonald and Stratmann (2012) point out the lack of reliable data on mobile, strong instrumental variables or natural experiments to isolate the true causal effect. Thus, there is no definitive answer to this

[2] Spelman and Brown (1981) found that fast reporting to the police combined with fast police response are of crucial importance to the likelihood of arresting the offender at or near the crime scene.

relationship in crime literature and studying the signal of the effect is of great relevance as benchmark for new security policies based in communication technologies.

In this part I present the structure of a fairly simple model following Doleac and Sanders (2013)[3], assuming that offenders engage in crime if the expected benefits are greater than the associated expected costs. Let the expected cost of crime be a function of the length of the sentence if arrested (T) and the likelihood of capture (P), which is a function of victim reporting crimes by using mobile phones (M). Also, there a set of factors associated with the expected costs such as law and social enforcement (F). An individual can attempt a crime if:

$$\mathbb{E}[Benefit_{crime}] > \mathbb{E}[Cost(T, P(M, F))_{crime}]. \tag{3.1}$$

In equilibrium, it is expected $\partial C/\partial P$ and $\partial P/\partial M$ to be positive. Greater mobile availability is likely to allow real time communication and detailed information about the crime and the criminal, increasing the probability of being captured and hence the expected cost of committing crime.

Although the mobile phones have attested to be instrumental in reducing crime, they have also played a part in creating it. For example, mobile phone can increase benefits for offenders as instruments for organized crime. Also, the mobile phones are attractive targets for the thieves as their small size also makes them relatively attractive targets. If it is true, then we should expect the benefits as function of the mobile availability $\mathbb{E}[Benefit(M)_{crime}]$, with $\partial B/\partial M > 0$. Measure the magnitude of the effect between benefits and costs is a hard task, and the results should be interpreted as the net effect of the mobile deterrence on crimes. Know the signal of the effect is an empirical question. To this end, I benefit from the adoption of ninth digit in the mobile phones for municipalities with code areas 11 to isolate the causal effect.

A possible caveat regarding the relationships above is due to the fact that the

[3] In their paper, Doleac and Sanders (2013) investigate how ambient light shifts induced by Daylight Saving Time (DST) affects crimes. The authors suggest that more light means witness as more likely to spot criminals committing crimes and more likely to being apprehended and punished.

addition of the ninth digit rule did not change the format for the public utility service phone, such as the 190 police emergency number. In fact, calls to the police were not affected during the analysis period even though the data from the São Paulo Military Police point out that only 13% of daily number of calls become police reports. Moreover, statistics show that the average response time exceeds 5 minutes, which may compromise the likelihood of punishment to the criminal. In this sense, the use of the regression discontinuity design is justified since it allows to smooth out potential confounders that may affect homicides. In the following subsection, I present the change in the mobile dialing plan in Brazil.

3.2.2 *Changes in Brazilian numbering plan*

The Brazilian telephone numbering plan was established in the year of 1998 by the National Telecommunication Agency (ANATEL), which regulates telecommunication services for fixed and mobile phones and other institutional aspects. The user access code for mobile phones was set in the format $[N_{10}N_9]$ $N_8N_7N_6N_5 + N_4N_3N_2N_1$, with the first two digits representing the area code[4] and the last 8 digits the current local number. On January 2012, ANATEL announced a change in the mobile phones dialing plan. All mobile phone numbers would be gradually changed from current 8 to 9 digits by including the digit 9 after the two digit area code and before the current number, $[N_{10}N_9]$ $\mathbf{9}N_8N_7N_6N_5 + N_4N_3N_2N_1$. This change was meant to increase capacity from the current 44 million to 90 million users in the metropolitan area of São Paulo.

Figure 3.1 below shows the area codes in the state of São Paulo. The ninth digit was first introduced in municipalities in the São Paulo metropolitan region (area code 11) on July, 2012. The remaining code areas in the state of São Paulo (area codes 12 to 19) received the ninth digit one year later, on August 2013. It is expected that by the end of 2016 the ninth digit will be implemented in all Brazilian territory in order

4 Area codes - or Direct Distance Dialing (DDD) - were ordered, according to ANATEL, by the development level of the Brazilian states, not directly related to the geography of each region. The codes that start by 1 were directed to the state of São Paulo, the one with the highest population density in 1969, year of creation of the area codes. The state has 9 area codes, ranging from 11 to 19. The codes beginning in 2 were allocated to Rio de Janeiro, and those starting with 3 to Minas Gerais. Currently, there are 67 different area codes in Brazil. For further information, see `http://goo.gl/IIayCv`.

to standardize the numbering plan in the country and enable the expansion of mobile technology. The shift to the ninth digit occurred gradually in favor of the network and the user's adaptation. From the date of implementation to mid-October 2012 calls were intercepted with a message reminding users of the new rule. On January 2013, however, only calls with the 9 digits were completed.

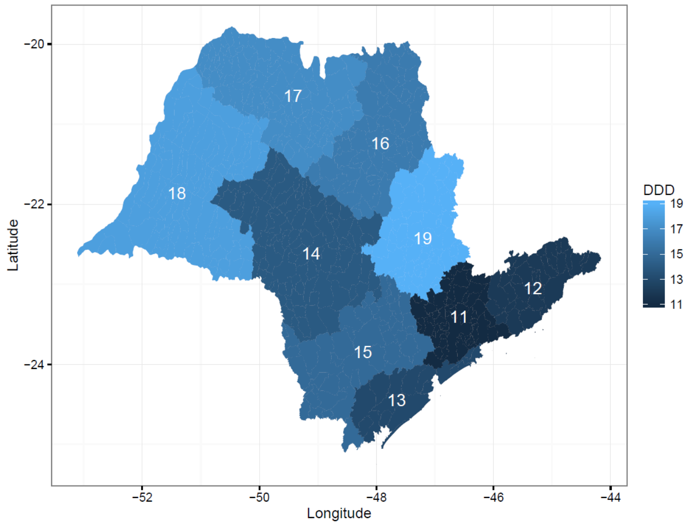
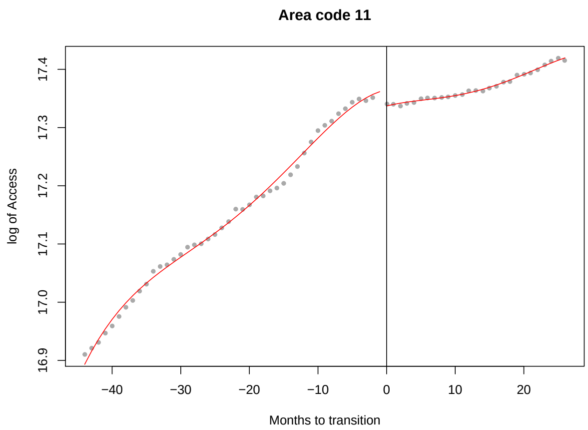


Figura 3.1: State of São Paulo divided by area codes

ANATEL's number of mobile calls data show that during the period of the transition and the adaptation there was a significant drop in the number of mobile access in the municipalities affected by the addition of the ninth digit[5]. The identifying strategy followed here exploit the fact that during the inclusion of the ninth digit there was a significant drop in the number of calls, which may unexpectedly influence criminal activities in the sense of lack of capable guardianship. I refer to the three expected cutoffs as the following: transition (I) refers to the date of introduction of the ninth digit (during this period all calls with 8 or 9 digits were completed); transition (II), in which all calls with 8 digit were intercepted with a message reminding users of the change; and transition (III), where the use of nine digits was mandatory.

The plot of the number of calls is depicted in figure 3.3. This discontinuity can be exploited using the regression discontinuity approach (IMBENS; LEMIEUX, 2008;

[5] Despite the gradual change, operators faced some troubles in adapting all systems to the new technology (http://goo.gl/p76aD0). Users and companies that depend on mobile service reported disorders generated by the new rule (http://goo.gl/3UIav4).



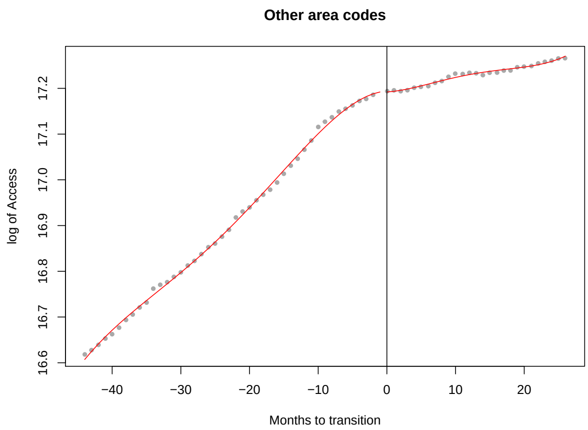


Figura 3.3: Discontinuity in the number of calls: Treated and Untreated Municipalities

LEE; LEMIEUX, 2010; CALONICO; CATTANEO; TITIUNIK, 2014). The graphs show linearized number of calls centered on the transition month. The graph on the left, which take into account municipalities in area code 11, shows that points on the right of cutoff are slightly shifted below. The graph on the right, on the other hand, presents that points evolved smoothly around the cutoff. The results considering all cutoffs are presented in table 3.1. Only for transition (II) there was a significant drop of about 1.6% in the number of calls for the treated municipalities. It represents a drop of nearly 1 million of calls during the month of October 2012. For unaffected municipalities, results are not different of zero. Results remain unchanged in relation to the bandwidth selectors choice.

Tabela 3.1: Monthly estimates of local linear RDD on number of access

	Area code 11			Other area codes		
Transition	(I)	(II)	(III)	(I)	(II)	(III)
$Digit9_{LATE}$	-0.0106	-0.0157***	0.0108	-0.0107	-0.0048	-0.0063
	(0.0101)	(0.0039)	(0.0080)	(0.0078)	(0.0114)	(0.0032)
Bandwidth	CCT	CCT	CCT	CCT	CCT	CCT
Polynomial order	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Observations	432	432	432	432	432	432

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered at the municipality level. The outcome variable is the log of user's access. (CCT) bandwidth refers to Calonico, Cattaneo and Titiunik (2014).

3.3 Empirical Strategy

The empirical strategy used in this study to access the effect of the inclusion of the ninth digit on several categories of crimes follow two approaches. The first consists to estimate the difference in monthly level of crimes before and after the introduction of the new digit for both affected and non-affected municipalities through a difference-in-differences setup. The second approach estimates the immediate impact on homicides for treated municipalities through a regression discontinuity design (RDD). I start with the estimation of the following benchmark model:

$$Crime_{itm} = \gamma Digit9_{ik=0} + \sum_{k>0} \delta_k Digit9_{ik} + \Psi X_{itm} + \omega_m + \eta_i + \phi_t + \epsilon_{itm}, \tag{3.2}$$

where i is the indicator for municipality, t for year and m for month. $Digit9_{ik=0}$ assumes value 1 if municipality i was affect by the introduction of the ninth digit in the intervention period (on October 2012, which presented significant drop in the number of calls as discussed in subsection 3.2.2). $Digit9_{ik}$ for $k > 0$ captures treatment effects for k months after October 2012. The parameters γ and δ_k are the parameters of interest, which capture the average treatment effect for municipalities that received the new digit. The dependent variable $Crime_{itm}$ measures several crime categories related to violence, such as the number of homicides victims, rape, theft and robbery. η_i, ω_m and ϕ_t are dummies controlling for municipalities, month and year fixed effects, respectively. These fixed effects are included to control for seasonality and specific factors that affects criminality in a given year and/or month. For all regression, the term error ϵ_{itm} is heteroskedasticity robust and clustered on municipality level (BERTRAND; DUFLO; MULLAINATHAN, 2002).

The vector of characteristics X_{itm} takes into account other factors affecting criminal activity. For example, municipality GDP per capita is included in the vector characteristics since it may account for some of the increase in the mobile penetration, as well as the capability of the municipalities adopt other policies that may deter crime. Following Biderman, Mello and Schneider (2010), I also include the demographics characteristics such as population, the ratio of male population in ages 15 to 30 years and urbanization ratio. It also includes state-level enforcement variables as the presence of a municipal police force and monthly data on the number police investigation, the number of guns apprehended and the number of arrests.

To identify the parameters of interest, one have to attest that outcome variable trends are similar for both treated and control groups, the Common Trend Assumption (ANGRIST; PISCHKE, 2008). Such hypothesis can be tested through the inclusion of anticipatory effects, $Digit9_{ik}$, for k periods preceding the intervention. The model can now be written as:

$$Crime_{itm} = \sum_{k<0} \tau_k Digit9_{ik} + \gamma Digit9_{ik=0} + \sum_{k>0} \delta_k Digit9_{ik} + \Psi X_{itm} + \omega_m + \eta_i + \phi_t + \epsilon_{itm} \quad (3.3)$$

Equation 3.3 also captures anticipatory effects of the intervention by adding leads and lags to the benchmark specification. If the trends in the outcome variable for treated and control groups are similar, then it is expected that the anticipatory effect is null. Another assumption to validate the estimates is the Stable Unit Treatment Value Assumption (SUTVA). Such hypothesis attests that the vector of potential outcome for municipality i is not directly associated with treatment status of municipality j, that is, there is no spillover effect in space (ANGRIST; IMBENS; RUBIN, 1996; IMBENS; RUBIN, 2015). If SUTVA is not observed estimators tend to be biased and inconsistent. It is hard to argue, however, that SUTVA is valid in this case since municipalities close to the border code area may be also affected. It may reflect economic and social interaction with municipalities in the treated area. An exercise to check SUTVA validity consists in restrict the sample with neighbor municipalities in both treated and control groups (see figure 3.1).

The second strategy identify the local average treatment effect of transition to the ninth digit on the number of homicides using a regression discontinuity design (RDD). In particular, the approach consists in comparing the evolution of violent deaths around the transition day for municipalities in the area code 11. For that end, the following model is estimated:

$$log(Homic_{it}) = \varphi \mathbb{1}(Digit9_{it} \geq 0) + g(Digit9_{it}) + \mu_{it}, \tag{3.4}$$

with $Homic_{it}$ corresponding to the number of victims in municipality i in day t. $Digit9_{it}$ is the running variable, defined as the number of days in relation to the implementation to the ninth digit. Negative values for the running variable refers to the pre-treatment period and positive values is related to post-treatment period. The function $g(.)$ controls for unobserved factors that evolved smoothly over time and are unrelated to the dialing plan change. The random term of error is μ_{it}.

Estimators are obtained nonparametrically using a Sharp RDD and accessed using local-polynomial point estimators (SKOVRON; TITIUNIK, 2015). I use two optimal data-driven bandwidth selectors vastly used in literature, (IK) Imbens and Kalyanaraman (2011), (CCT) Calonico, Cattaneo and Titiunik (2014), and an alternative cross-validation method (CV) proposed by Ludwig and Miller (2005).

3.4 Data

Throughout this paper, I analyze the introduction of the ninth digit effect on the incidence of criminal activity in the state of São Paulo. In order to obtain credible estimates, I restrict the analysis for the period between January 2012 and July 2013. The choice of this time horizon is intended to minimize the effect of any other security policies that may affect crime. Furthermore, this period allows us to examine the presence of any seasonality and observe pre- and post- treatment periods of comparison.

The choice of the state of São Paulo for this study is justified in figure 3.4. Like many industrialized countries and with a GDP per capita of US$23,700 in 2010 (IBGE, 2010), São Paulo has experienced a drop in the number of violent crimes during the period between 2004 and 2014. The solid line represents the homicide rate per 100,00 inhabitants in Brazil, and the dashed line depicts the rate for the state of São Paulo. It can be seen that homicide rate in Brazil increased slightly during the period, presenting a small drop

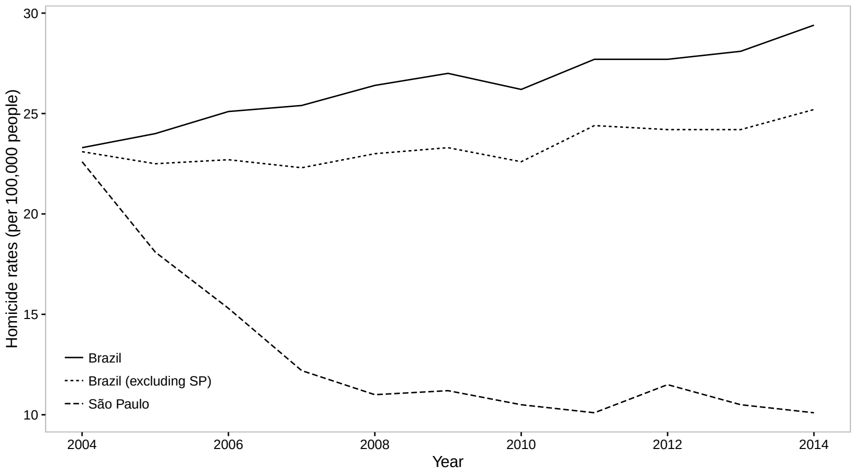


Figura 3.4: Homicide rate in Brazil and São Paulo

Source: Data from *Secretaria de Estado da Segurança Pública* (2016). Homicide rates per 100,000 inhabitants.

in the year of 2010. The same pattern is observed if excluding São Paulo (dotted line in graph). On the other hand, the homicide rate for São Paulo fell sharply between 2004 and 2008. After this period, it remained steady until 2014. Interesting to notice is that the homicide rate for São Paulo showed a slight increase in 2012, year the introduction of the ninth digit.

Data used in this study come from different sources. The dependent variable is a collection of crime categories obtained from the *Secretaria Estadual de Segurança Pública de São Paulo* (SSP). Criminal statistics are reported at the municipal-level such as homicides, bodily injury, rape, vehicle and property thefts. Such data are available since January 2011 and are published monthly. Also from SSP, it is possible to collect data on police productivity referred to the number police investigation, the number of guns apprehended and the number of arrests. From the Brazilian Bureau of Statistics (IBGE) comes information on municipal-level policies such as the establishment of the police forces and the existence of municipal police departments. Demographic information are extracted from São Paulo's State Foundation for Statistical Analysis (*Fundação Sistema Estadual de Análise de Dados* - SEADE). The data presents a variety of social and economic aspects of the state and municipalities, collected in different lengths of time.

For the regression discontinuity approach is used daily municipality-level homicides compiled by the *Sistema de Informações sobre Mortalidade* (SIM), implemented by the Brazilian Ministry of Health. Death causes are distinguishable following International Classification of Diseases (ICD-10)[6].

Tabela 3.2: Summary Statistics - Treated and Control Municipalities

	Area code 11		*Other area codes*		*Area code 11 (excluding SP)*	
Variables	Pre 9th digit	Post 9th digit	Pre 9th digit	Post 9th digit	Pre 9th digit	Post 9th digit
GDP per capita	33.690	36.182	22.382	24.090	33.484	35.980
	(29.242)	(30.491)	(17.814)	(20.128)	(29.428)	(30.690)
Population	337.358	339.283	35.024	35.251	162.092	163.301
	(1,408.927)	(1,414.613)	(82.582)	(83.208)	(217.269)	(218.469)
% Male population 15-30 years old	0.2617	0.2606	0.2511	(0.2504)	0.2619	0.2608
	(0.0143)	(0.0144)	(0.0262)	(0.0259)	(0.0144)	(0.0144)
Urbanization rate	0.9114	0.9127	0.8424	(0.8444)	0.9102	0.9115
	(0.1439)	(0.1428)	(0.1384)	(0.1378)	(0.1447)	(0.1436)
Homicides	1.1952	1.2989	0.7178	0.6302	1.1989	1.3020
	(2.0513)	(2.2523)	(3.1010)	(2.8215)	(2.0673)	(2.2697)
Bodily injury	34.7315	33.9991	48.6027	48.1127	34.7754	34.0656
	(14.9343)	(15.3426)	(34.1810)	(33.7140)	(15.0449)	(15.4502)
Rape	3.1692	2.9785	2.9805	2.9923	3.1836	2.9881
	(3.6196)	(3.2391)	(6.9563)	(7.1169)	(3.6463)	(3.2636)
Vehicle thefts	25.0128	25.9881	7.5270	7.9317	24.3776	25.3305
	(20.8773)	(21.9222)	(12.6088)	(12.9668)	(20.4047)	(21.4427)
Property thefts	106.0005	106.7681	85.2382	85.7791	104.0599	104.7532
	(41.2502)	(41.8489)	(65.4614)	(64.1196)	(38.5068)	(38.9477)

Note: Data are collected from Secretaria Estadual de Segurança Pública de São Paulo (SSP), Brazilian Bureau of Statistics (IBGE) and Department of Statistics of the State of São Paulo (SEADE). All means (except for population variable) are computed using population as a weight per 100,000 inhabitants. GDP per capita is calculated in Reais (R$) in constant prices of 2010.

Summary statistics of municipalities where the ninth digit was implemented (area code 11) and those that remain with the 8 digits (other area codes) are in table 3.2. GDP per capita is higher for municipalities in are code 11 than for other municipalities in the state, as well as the population size. Urbanization rate and the proportion of male population between 15 and 30 years old are quite similar between these two groups. The crime rates tend to be greater for area code 11 municipalities, except for bodily injury. Even excluding the city of São Paulo from the sample of treated group, mean values remains unchanged.

To obtain pre- and post-treatment period, I use nine-month periods to/from treatment. The period analyzed is inserted between January 2012 and July 2013. This is

[6] More details of released ICD-10 codes in World Health Organization page `http://apps.who.int/classifications/icd10/browse/2016/en`.

important for seasonality analysis and minimization of confounders that may affect crime. Other municipalities in the state (those with code areas 12 to 19) received the ninth digit in August 2013, the second wave of changes in the mobile numbering plan. In this sense, it is ensured that the control group is not affect by exogenous shocks during the studied period.

3.5 Results and Discussion

This section presents the main results of the ninth digit introduction impact on criminal activity in the state of São Paulo. First, I present and discuss the results for the dynamic difference-in-differences estimators, which considers the signal of the effect and the persistence over time. The results are expanded for several categories of crime. Further, I explore the immediate impact on homicides victims using the regression discontinuity design.

3.5.1 *Main estimates*

Table 3.3 presents the results for homicides victims considering the second phase of the introduction of the ninth digit, since it had a significant drop in the number of mobile connections.[7] Column (1) shows the estimate for the basic difference-in-differences model, without municipality fixed effects and without control variables. The dummy treatment variable is equal to 1 for municipalities where the ninth digit took place in July 2012. As can be seen, there was a significant positive impact on homicides of 0.61 homicides for each 100,000 inhabitants.

In columns (2)-(3) are presented the estimates for the temporal model in the equation 3.2. Column (2) presents the dynamic effect without control variables. The effect remains significant and positive only for the first month after treatment. Column (4) adds municipality the control variables. The results remain unchanged, with a increase in homicides in the first month after introduction of the ninth digit. The drop on the

[7] The estimation results for the other two transition phases (I and III) are available upon request to the author. Nonetheless, the estimated results do not show evidence of impact on crime.

Tabela 3.3: Estimates of the impact of ninth digit introduction on Homicides

	(1)	(2)	(3)
$Digit9(t)$	1.0151***	0.5031*	0.5133*
	(0.2882)	(0.3011)	(0.3020)
$Digit9(t+1)$	0.8764**	0.3643	0.3745
	(0.4254)	(0.4081)	(0.4083)
$Digit9(t+2)$	0.8987**	0.3866	0.3968
	(0.3494)	(0.3560)	(0.3564)
Month FE	✓	✓	✓
Year FE	✓	✓	✓
Municipality FE		✓	✓
Controls			✓
Observations	12,255	12,255	12,255
F-Statistic	2.565	1.414	1.411

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors are clustered at municipality level and are presented in parentheses. The dependent variable is the homicide rate per 100,000 inhabitants.

number of calls is likely to reduce mobile deterrence for victims of crime only in the month that municipalities experienced the change in mobile dialing. In the months following the treatment, the impact of the new rule on homicides is not different of zero.

The results for bodily injury are presented in table 3.4. Again, the results are present from the simple (1) to the full specification (3). In all specifications, the ninth digit introduction caused a drop in bodily injury for treated municipalities, relative to control municipalities. In column (3), the model with fixed effects and control variables, the drop is significant with magnitude of -10,62 per 100,000 inhabitants, which means a drop of 30.6%. One month after treatment $(t + 1)$, the size of the impact is smaller but still significant.

The mechanisms that explain the results for homicide and bodily injury are relevant for security policies, mainly because these types of crime are likely to occur among strangers and most plausibly deterred by the use of mobile phones (ORRICK; PIQUERO, 2015). As mobile phones increase surveillance and apprehension, an expansion of this te-

Tabela 3.4: Estimates of impact of ninth digit introduction on Bodily Injury

	(1)	(2)	(3)
$Digit9(t)$	-23.713***	-10.596***	-10.6209***
	(2.770)	(2.376)	(2.3801)
$Digit9(t+1)$	-16.600***	-3.483*	-3.5080*
	(2.371)	(1.919)	(1.9233)
$Digit9(t+2)$	-15.792***	-2.675	-2.7001
	(2.470)	(2.113)	(2.0972)
Month FE	✓	✓	✓
Year FE	✓	✓	✓
Municipality FE		✓	✓
Controls			✓
Observations	12,255	12,255	12,255
F-Statistic	20.53	7.821	7.782

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors are clustered at municipality level and are presented in parentheses. The dependent variable is the rate of bodily injury per 100,000 inhabitants.

chnology would increase the costs of crime perceived by potential offenders. One possible explanation for the previous results is as the following. A potential homicide victim could avoid the murder if she could use the mobile phone in a threatening situation. Once homicide data are rarely under-reported this reduced communication lower the likelihood of deter criminals. On the other hand, the difficulty of communication caused fewer reports of bodily injury which helps improve statistics.

Another category of crime within this context is rape, defined by the Brazilian penal code as "the act of embarrass someone by violence or serious threat, to have sexual intercourse or to perform or allow him to practice other libidinous acts". In many countries rape statistics are unreliable or misleading. In Brazil, rape is severely under-reported and official data is not accurate[8].

The results for rape are presented in table 3.5. The result in the basic model

[8] Security policies aimed at protecting women exist, but are still quite inefficient in Brazil. One possible explanation for under-reporting is that women often feel guilty and ashamed, so they do not report being raped, especially as they know the extent of existing impunity. See more in `http://goo.gl/3UIav4`

(column 1) demonstrate no effect on rape for the first month the new rule took place. The temporal estimation, however, presents a significant increase in rape in the first period after treatment. In the month following, however, the effect is null.

Tabela 3.5: Estimates of impact of ninth digit introduction on Rape

	(1)	(2)	(3)
$Digit9(t)$	0.4444	0.4237	0.4141
	(0.4599)	(0.4739)	(0.4734)
$Digit9(t+1)$	1.0126*	0.9918**	0.9822**
	(0.5206)	(0.4984)	(0.4921)
$Digit9(t+2)$	-0.2280	-0.2488	-0.2582
	(0.5602)	(0.5822)	(0.5838)
Month FE	✓	✓	✓
Year FE	✓	✓	✓
Municipality FE		✓	✓
Controls			✓
Observations	15,480	15,480	15,480
F-Statistic	1.831	1.613	1.603

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors are clustered at municipality level and are presented in parentheses. The dependent variable is the rate rape per 100,000 inhabitants.

Other categories of crimes that are less likely to predict (such as theft or robbery) are not easily deterred by the use of mobile phones (KLICK; MACDONALD; STRATMANN, 2012). The estimates for vehicle and property theft and robbery are presented in table 3.6. According to the results, there is no effect for these types of crime when compared treated and control municipalities.

3.5.2 *Robustness checks*

The key assumption for any difference-in-difference estimation is that the outcome in treatment and control groups would follow the same time trend in the time period before treatment. In some aspects, the common trend assumption is not easy to verify and the most widely strategy used to check its validity is to use pretreatment data to show that

Tabela 3.6: Estimates of impact of ninth digit introduction on Vehicle and Property theft

	Vehicle theft		Property theft	
	(1)	(2)	(3)	(4)
$Digit9(t)$	0.8217	0.9409	4.2699	4.2120
	(1.2708)	(1.2706)	(3.5383)	(3.5563)
$Digit9(t+1)$	-0.3500	-0.2307	-2.2403	-2.2981
	(1.0287)	(1.0205)	(2.4370)	(2.4553)
$Digit9(t+2)$	1.5752	-1.4558	-0.1395	-0.1974
	(1.3581)	(1.3521)	(2.8659)	(2.8881)
Month FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓
Controls		✓		✓
Observations	15,480	15,480	15,480	15,480
F-Statistic	42.01	41.78	34.44	34.25

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors are clustered at municipality level and are presented in parentheses. The dependent variable is the rate of vehicle and property theft per 100,000 inhabitants.

the trends are similar (AUTOR, 2003). Estimated results of equation 3.3 are presented in table 3.7.

From table 3.7 we observe that for all categories of crime there are no anticipatory effects, i.e., the inclusion of the ninth digit did not caused any change in crime patterns in the pretreatment period. This is also true for the phase (I) treatment and consistent with discontinuity exploited in the subsection 3.2.2. Consistent with previous results, the treatment effect in the period t is significant for homicides and bodily injury.

With population around 11,3 million (IBGE, 2010) São Paulo is the most populous municipality in Brazil and the 12th largest city proper by population in the world. It also has the largest economy by GDP in Latin America and Southern Hemisphere. Economic and social interaction, thus, makes the mobile phone an important component in people routine. In order to check if result presented in last section were driven mainly by the city of São Paulo, table 3.8 report the results excluding the most populous city in the sample.

Tabela 3.7: Estimates of impact of ninth digit introduction: Anticipatory effects

	Homicides (1)	Injury (2)	Rape (3)	Vehicle (4)	Property (5)
$Digit9(t-2)$	-0.1586	-2.9711	0.4880	0.8584	1.9946
	(0.3115)	(1.9907)	(0.6624)	(1.2631)	(4.1138)
$Digit9(t-1)$	-0.1804	-4.8934	0.2088	-1.0433	5.2950
	(0.2761)	(2.1546)	(0.4861)	(1.2100)	(3.3610)
$Digit9(t)$	0.4490*	-10.7169***	0.4813	-2.7001	4.7786
	(0.2179)	(2.4086)	(0.4775)	(1.3102)	(3.6965)
$Digit9(t+1)$	0.3320	-3.8565**	1.0494**	-0.1538	-1.8528
	(0.4061)	(1.9713)	(0.5107)	(1.0263)	(2.4497)
Month FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
Observations	12,255	12,255	12,255	12,255	12,255
F-Statistic	1.409	7.784	1.602	41.78	34.25

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors are clustered at municipality level and are presented in parentheses. All dependent variable are rates per 100,000 inhabitants.

As can be seen in table above, excluding the city of São Paulo does not change significantly previous results. The impact size - for the model with fixed effects and control variables - are slightly lower compared to the results with the inclusion of São Paulo. This corroborates to the fact that the impact on crime was homogeneous for the region with area code 11 and negligible for control regions.

3.5.3 *RDD results*

Figure 3.6 presents the main findings for log of homicides for treated and untreated municipalities. In the panel on the left are the results for municipalities where the ninth digit for mobile phone numbers were implemented. Following Smith (2016), the outcome variable is demeaned to minimize the persistent day-of-week effects.

It can be observed a slightly shift upward, implying in a moderate increase in the

Tabela 3.8: Estimates of impact of ninth digit introduction: Excluding São Paulo

	Homicides (1)	Injury (2)	Rape (3)	Vehicle (4)	Property (5)
$Digit9(t)$	0.5138*	-10.4967***	0.4154	0.9147	4.3082
	(0.3115)	(2.3937)	(0.4787)	(1.2937)	(3.6154)
$Digit9(t+1)$	0.3751	-3.4163*	0.9846*	-0.1880	-2.0769
	(0.4145)	(1.9410)	(0.5047)	(1.0315)	(2.5041)
$Digit9(t+2)$	0.3993	-2.6262	-0.2550	-1.4054	-0.0348
	(0.3613)	(2.1204)	(0.5913)	(1.3733)	(2.9456)
Month FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes
Observations	12,236	12,236	12,236	12,236	12,236
F-Statistic	1.411	7.773	1.603	40.46	34.25

Note: $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$. Standard errors are clustered at municipality level and are presented in parentheses. The dependent variable is the rate of bodily injury per 100,000 inhabitants.

number of homicides once users could not complete mobile calls. In the panel on the right I consider contemporaneous homicide levels for the untreated municipalities. There is no significant discontinuity around the cutoff for other area codes.

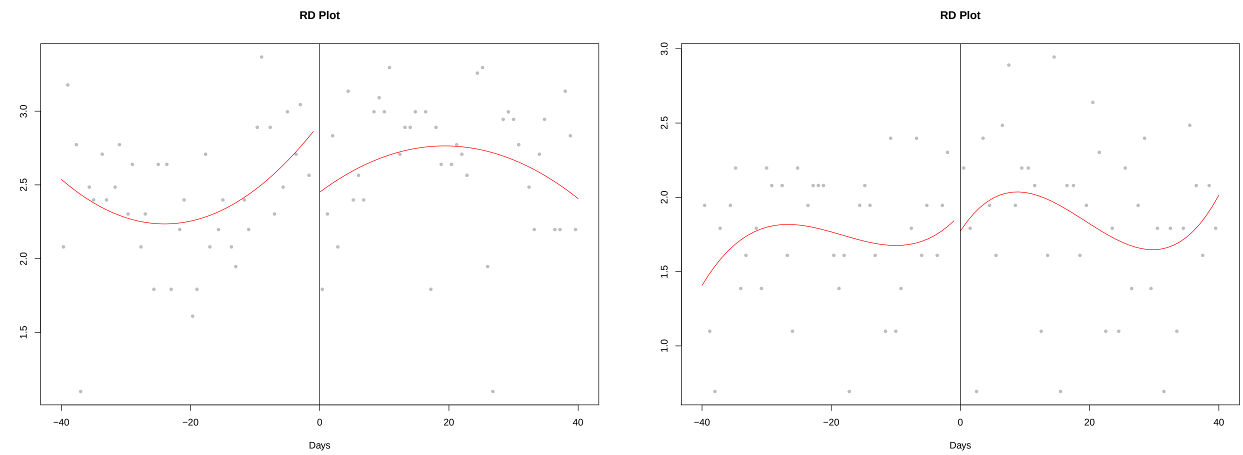


Figura 3.6: Implementation of the ninth digit

The detailed estimates are presented in table 3.9. Considering all bandwidth selection - IK, proposed by Imbens and Kalyanaraman (2011); CCT by Calonico, Cattaneo and Titiunik (2014); and CV, a cross-validation method proposed by Ludwig and Miller (2005) - procedures the results for both treated and untreated municipalities. Even though the impacts for both groups are negative, they are not statistically different from zero. These findings support the fact that the impact of the drop in the mobile calls caused by the implementation of the ninth digit does not occur immediately after the transition date.

Tabela 3.9: Estimates of Local Linear RDD on Homicides

	Area code 11			Other area codes		
$Digit9_{LATE}$	-0.4450	-0.2818	-0.2818	-0.5243	-0.5412	-0.5412
	(0.5704)	(0.5704)	(0.6180)	(0.2961)	(0.2961)	(0.3440)
Bandwidth	CCT	IK	CV	CCT	IK	CV
Polinomyal order	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	Uniform	Uniform	Uniform	Uniform	Uniform	Uniform
Observations	4067	4067	4067	2716	2716	2716

Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors are clustered at the municipality level. The outcome variable is the log of homicides. Bandwidth codes: (CCT) Calonico, Cattaneo and Titiunik (2014); (IK) Imbens and Kalyanaraman (2011); (CV) Cross-Validation Ludwig and Miller (2005).

3.6 Concluding Remarks

In addition to the factors discussed by theoretical and empirical economists to explain the decline in the crime rate in most of industrialized countries, some scholars have focused in the study of other potential correlations based on the security hypothesis. In this sense, a potential and underappreciated link is related to the increase of mobile phone technology and crime deterrence.

In this paper, I present the first attempt to estimate the causal effect of the use of mobile phone on crime. A natural experiment induced by the introduction of the ninth digit for some municipalities in the state of São Paulo is used to estimate the signal and size of the effect of interest. At our benchmark estimate, the drop in the number of mobile calls caused monthly homicide rates per 100,000 inhabitants to increase by around 0.5, which means nearly 50% increase. For bodily injury rate, there was a fall of 10.62 in the first month, which means a drop of 30.6%, and a drop of 3.5 in the second month after treatment. For rape, I find an increase of 0.98 in the rate per 100,000 inhabitants only one month after treatment, but no immediate effects. The results for vehicle and property theft are not statistically significant, supporting the findings in Klick, MacDonald and Stratmann (2012), which hypothesize that mobile phones have the largest impact for violent crimes. The regression discontinuity estimates show, however, no immediate impact of mobile calls drop on crime.

According to Farrell, Tilley and Tseloni (2014), phone guardianship is unlikely to prove to be a major contributor to the crime drop. Nonetheless, the results of this study are consistent to the modern discussions in the crime literature and appealing toward the use and expansion of private security instruments in crime prevention.

This study paves the way for further studies interested in estimating the effect of the mobile phones use on crime through different instruments and empirical strategies in favor of external validity. Noteworthy, shutdown in telecommunications services in specific contexts or judicial bans to the use of chat platform can be also explored as instruments to access the causal effect.

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Apêndices

APÊNDICE A – The Olympic Spirit Boost

Tabela A.1: Bid cities and round-by-round of host city elections.

Year	IOC Session	Bid cities	Round				
			1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}
2004	Aug. 5, 1997	**Athens (GRE)**	**32**	-	**38**	**52**	**66**
		Rome (ITA)	**23**	-	**28**	**35**	41
		Stockholm (SWE)	**20**	-	**22**	20	-
		Cape Town (RSA)	16	**62**	19	-	-
		Buenos Aires (ARG)	16	44	-	-	-
2000	Aug. 23, 1993	**Sydney (AUS)**	**30**	**30**	**37**	**45**	
		Beijing (CHN)	**32**	**37**	**40**	43	
		Manchester (GBR)	**11**	**13**	11	-	
		Berlin (GER)	**9**	9	-	-	
		Istanbul (TUR)	7	-	-	-	
1996	Aug. 18, 1990	**Atlanta (USA)**	**19**	**20**	**26**	**34**	**51**
		Athens (GRE)	**23**	**23**	**26**	**30**	30
		Toronto (CAN)	**14**	**17**	**18**	22	-
		Melbourne (AUS)	**12**	**21**	16	-	-
		Manchester (GBR)	**11**	5	-	-	-
		Belgrade (YUG)	7	-	-	-	-
1992	Oct. 16, 1986	**Barcelona (ESP)**	**29**	**37**	**47**		
		Paris (FRA)	**19**	**20**	23		
		Belgrade (YUG)	**13**	**11**	5		
		Brisbane (AUS)	**11**	**9**	10		
		Birmingham (GBR)	**8**	8	-		
		Amsterdam (NED)	5	-	-		
1988	Aug. 30, 1981	**Seoul (KOR)**	**52**				
		Nagoya (JPN)	27				
1984	May 18, 1978	**Los Angeles (USA)**	-				
1980	Oct. 13, 1974	**Moscow (USSR)**	**39**				
		Los Angeles (USA)	20				
1976	May 5, 1970	**Montreal (CAN)**	**25**	**41**			
		Moscow (USSR)	**28**	28			
		Los Angeles (USA)	17	-			
1972	Apr. 25, 1966	**Munich** (GER)	**29**	**31**			
		Madrid (ESP)	**16**	16			
		Montreal (CAN)	**6**	13			
		Detroit (USA)	6	-			
1968	Oct. 18, 1963	**Mexico City (MEX)**	**30**				
		Detroit (USA)	14				
		Lyon (FRA)	12				
		Buenos Aires (ARG)	2				

Notes: "Year" correspond the actual hosting Games; "IOC Session" refers to bid winner announcement date; "Bid cities" represents the cities bidding for host the Games, and "Rounds" indicates the number of rounds and the share of votes each city received.

Tabela A.2: Synthetic Weights for Host Countries (OECD Countries)

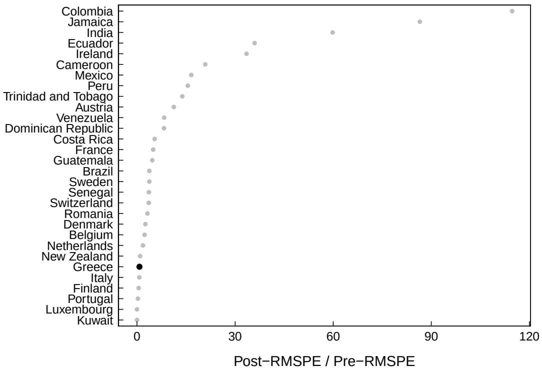
Athens 2004	Sydney 2000	Barcelona 1992	Seoul 1988	Montreal 1976	Mexico 1968
AUT	AUT	AUT	AUT	AUT (0.006)	AUS
BEL (0.007)	BEL	BEL	BEL	BEL (0.057)	AUT
DEN	DEN	DEN	CHI (0.003)	DEN (0.027)	BEL
FIN	FIN	FIN	DEN	FRA (0.101)	DEN
FRA	FRA (0.713)	FRA (0.112)	FIN	IRL (0.005)	FRA (0.082)
IRL (0.688)	IRL	IRL	FRA (0.347)	LUX (0.246)	IRL
ITA	ITA	ITA	IRL	NZL (0.545)	LUX
LUX	LUX	LUX	LUX	NOR (0.008)	NZL
MEX	MEX	NED	NED	POR	POR (0.918)
NED (0.168)	NED	NZL	NZL	SWE	SWE
NZL	NZL (0.076)	POR (0.888)	NOR	TUR (0.003)	
POR (0.134)	POR	SWE	POR (0.649)		
SWE	SWE (0.209)	SWI	SWE		
SWI	SWI		SWI		
			TUR		

Notes: The synthetic control is composed by the country weights from the minimization problem. Since the synthetic control is a convex set of weights, we must have positive weights with sum equal to one. We highlight the countries that receive positive values. The remaing countries received weights equal to zero.

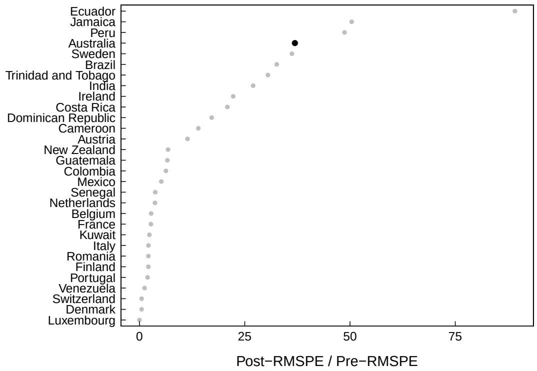
Tabela A.3: Covariates Balance in the pre-intervention period

	Athens (GRE)			**Sydney (AUS)**		
Variables	Treated	Synthetic	Sample mean	Treated	Synthetic	Sample mean
Count Medal	2.66	2.72	7.11	16.00	16.09	6.37
Olympic team size	65.16	73.22	112.37	216.50	213.31	109.33
Real GDP per capita	13293.98	15212.13	19912.98	20808.50	19273.30	18216.00
Log of Population	1.00	0.71	0.95	1.18	1.47	0.93
Human Capital index	2.52	2.86	2.67	3.20	2.42	2.60
Life expectancy	75.33	74.11	74.53	74.67	74.85	73.58
Secondary enrollment	1728303.76	1647843.84	3734336.20	5551702.48	7010227.11	3140041.15
Polity IV index	9.00	9.96	8.99	10.00	8.81	8.63
	Barcelona (ESP)			**Seoul (KOR)**		
Variables	Treated	Synthetic	Sample mean	Treated	Synthetic	Sample mean
Count Medal	2.57	2.52	6.23	5.17	5.57	4.33
Olympic team size	144.85	48.60	95.42	89.00	94.97	79.36
Real GDP per capita	10825.98	8848.69	16355.37	2715.77	9182.95	13318.80
Lof of Population	1.55	1.06	0.85	1.49	1.21	0.82
Human Capital index	1.96	1.90	2.53	2.20	1.81	2.38
Life expectancy	63.80	69.78	72.84	60.58	68.86	70.08
Secondary enrollment	2419161.49	1165728.25	2389412.93	3560270.74	1837865.27	1312364.22
Polity IV index	1.28	2.21	9.08	-1.33	0.43	8.11
	Montreal (CAN)			**Mexico City (MEX)**		
Variables	Treated	Synthetic	Sample mean	Treated	Synthetic	Sample mean
Count Medal	3.67	3.67	5.92	2.00	2.11	7.26
Olympic team size	109.83	60.88	80.46	70.60	56.62	95.69
Real GDP per capita	12536.67	12526.74	9482.80	4946.47	3942.75	9021.44
Log of Population	1.25	0.34	0.75	1.55	0.99	0.74
Human Capital index	2.62	2.62	2.17	1.45	1.53	2.25
Life expectancy	71.75	70.58	68.53	58.06	64.23	69.91
Secondary enrollment	3665956.29	733657.69	7811192.06	650850.07	356164.09	977922.06
Polity IV index	10.00	9.69	7.74	-6.00	-7.61	7.58

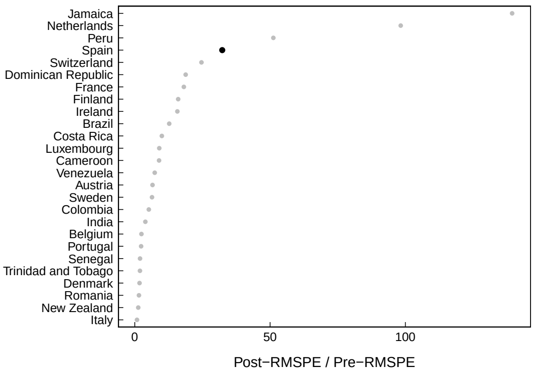
Note: The table shows all covariates used in the construction of the synthetic control units. For each case study, “Treated” refers to the pretreatment covariates mean for the treated unit, “Synthetic” is the mean of pretreatment covariates for the synthetic control unit and “Sample mean” is the mean for all countries in donor pool.



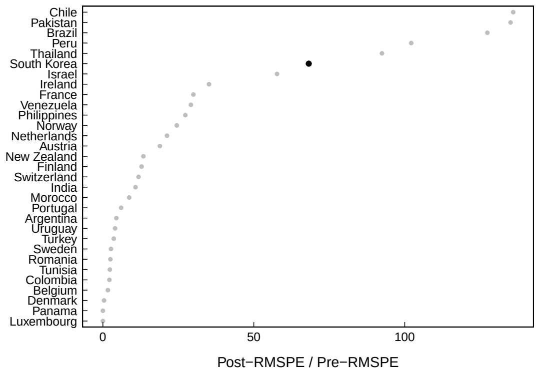
(a) Athens (GRE) 2004



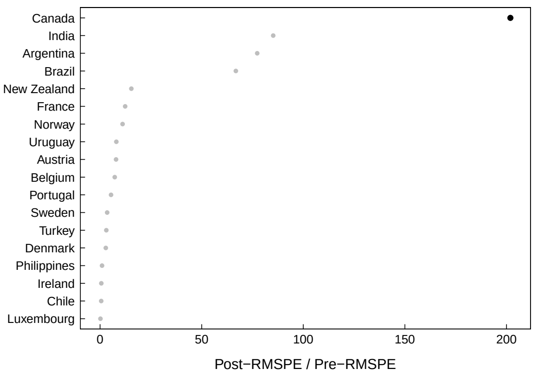
(b) Sydney (AUS) 2000



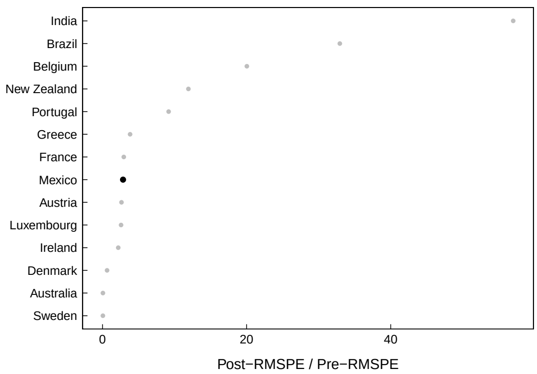
(c) Barcelona (ESP) 1992



(d) Seoul (KOR) 1988



(e) Montreal (CAN) 1976



(f) Mexico City (MEX) 1968

Figura A.1: Ratio of Post treatment RMSPE to Pre treatment RMSPE