Flourishing Opportunities:
Four essays in Applied Econometrics

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Abstract

This thesis comprehends four essays investigating strategies to fight against poverty. The first essay explores a series of police operations to pacify the slums of Rio de Janeiro to understand the impacts of intrauterine exposure to violence on birth outcomes. One argues that pregnancies starting before, but ending around the pacification dates are 'quasi-randomly' exposed to exogenous shocks of violence during pregnancy. The results show that each month pregnant women are exposed to pacification increases birth weights by 4 grams and reduces the probability of low birth weight (< 2500 grams) by 1.2 percent compared to pregnancies ending just before pacifications. A second essay uses Brazilian legislative change making it mandatory for private hospitals to publicly disclose information about physicians' performance. The results show a reduction in scheduled C-sections by 4.8 percent; which two-thirds originating from physicians anticipating to information disclosure. The third essay proposes an empirical strategy to estimate bullying effects on labour and schooling outcomes when "true" bullying is observed inaccurately. The estimates show that high-school bullying decreases University attendance by 3.4 percent and increases the probability of being not in education, employed or in training after high-school by 2.8 percent. Estimations neglecting misreport implicates in impacts two-thirds smaller. And finally, the fourth essay shows that poor households increase their participation in social groups after receiving Bolsa Família. The strategy explores households registered in Cadastro Único, and performs a propensity score difference-in-difference framework to minimize selection bias. Becoming a recipient of Bolsa Família increases .09 standard deviations the number of social affiliation and increase from 6.1 to 8.9 percent the probability of engaging in social groups. Altogether, this thesis implicates that investing in early stages of life harvest significant benefits to disadvantaged children, it also shows that victims of bullying need sustained support after high school, and that conditional cash transfers foster social engagement.

ACKNOWLEDGEMENTS

If there is any relevant result in the lines following, if there is any interesting questioning or sensible reasoning in this thesis, it is a product of the reckless capacity of my wife to listen to my endless frustrations. Her promptness, I must say, to feel my distress even though this thesis was not formally hers and to challenge every idea since its beginning granted her the first paragraph of my work. She plays a preeminent role in all my achievements. *Vanessa, meu bem, eu te amo*.

By convention, parents cannot write their child's thesis; otherwise mine would have definitely tried. I will always recollect the silence of my mother weeping in the telephone before receiving the news about my application to Cambridge, or my father spending some energy trying to remember the name of Stephen Hawking. My mother-in-law and my brother-in-law for being guided in their first trip abroad, which, I hope, was one of the greatest experiences in their lives.

My brothers, my rest. They build the most relaxing and unbreakable shelter against the storms I passed through along these years. However distant physically, the same computer used to write this thesis put us together many times for endless talks and distracting laughs. *Marney, Sandro, Sandrinho, Shailo, Marta e Luiz amo muito todos vocês*.

During all these years, I learnt to call my supervisor, Dr. Flavio Comim, as a friend. As could not be different, Prof. Comim witnessed my pursuit for possible, and plausible, ways to test my hypotheses, and shared the surprise when every result came out (despite of most of them being incorrect). Certainly, we spent more time thinking about what was wrong than on the sparse moments of congruence. When I had no arguments in the opposite direction, the next line was written.

Several ears listen to the construction of this thesis. To name a few; Daniel Santos, Caio Piza, Anna Vignoles, Rodrigo Soares, Jane Cooley Fruehwirth, and Karen Marcous. Great improvement and valuable questioning arose from our conversations. Of similar importance, this thesis would not be possible without the generous financial help from *Cambridge Overseas Trust* from the University of Cambridge and from the *Coordination for Research from the Brazilian Ministry of Education* (CAPES).

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Finally, listing all friends and experiences I had during my stay in Link House certainly would exceed the word limit set by the University. Starting from the first cohort; Toshiasu, Bart, Maria, Nazareno, Mahed, Hao and the wardens Sam and Steve have kindly made our adaptation period imperceptible. Jackie, DJ, Fei, Tao, Mel and Lil only improved what could not be better. And those who are still in the house; Antonios, Jee-youn, Daniele, Yessica, Chiara, Ilhame, Marta, Gary and Anne by (in)voluntarily make me laugh every night at the "magic" table. To all those who left a line in the chapter "My life in Cambridge", remember, you are all here, inside my heart.

DECLARATION

I declare that the contents of this thesis are original and have not been submitted in whole or in part for consideration for any other degree or qualification in this, or any other university. This dissertation contains fewer than 60,000 words including tables, appendices, references, footnotes.
Ildo José Lautharte Junior
November 2017

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Chapter 1

Introduction

"The most valuable of all capital is that invested in human beings; and that capital the most precious part is the result of the care and influence of the mother"

Alfred Marshall, (1890).

Diedre lives in the slums of Rio de Janeiro. Despite of having scarce resources and being unable to choose a life she values, the future of the child in her womb is by far the biggest of her concerns. She is a clever woman, she knows that "we should never underestimate the power of a good infancy", but fancies which means would make possible to break the chains of poverty that drag her down. Her hope, above all, rests on effective solutions to fight against poverty.

Armed gangs control and dispute the slum where she lives for trafficking since a time she cannot recall. Bullets dash in the sky whenever the police approaches, gifting everyone with a sleepless night. Physicians keep telling her that exposure to violence during pregnancy harms the development of her baby and that it prevents him to acquire the skills needed to succeed in life; "But violence has become part of our landscape, Dr. Just look to the horizon". Sometimes she wonders what would have happened to birth outcomes if she had moved to a peaceful area while pregnant. Sadly, just in Rio de Janeiro, this is the reality for 1,093 million people (IPP, 2015) which is only a small fraction of the 860 million living in favelas across the world, according to the United Nations (2015).

Nonetheless, finding ways to minimize such damage is problematic; for example, reading about the benefits of vaginal delivery, Diedre decided to have a normal birth once the benefits for babies seem incontestable. However, the physician is far more knowledgeable about her clinical condition, and she has no information about the quality of the physician who will perform her delivery. Perhaps being able to search for the best professional possible could overcome the influence of experiencing a violent environment during pregnancy. "For good or bad, someone has to take care of me when it happens".

The prospects regarding education are also disappointing. Low quality seems to be the norm at schools around her house. In response, Diedre is convinced to read books every night to her baby, to reduce her workload to participate during his cognitive and emotional development, and to stimulate creativity and curiosity in all ways possible. But what can she do to prevent her child to suffer bullying? And what if he does not tell her about such episodes to evade further retaliation from bullies or isolation from friends?

While contemplating these obstacles and considering asking for help is when Diedre realizes how limited is her social network. Her friends likely suffer from similar problems, and she does not have connections and resources to participate in community groups to demand social support and ask for help. "I fell myself isolated, like an island".

This thesis proposes four public policies to relief poverty of people like Diedre. Naturally, she is just an anecdote, but that represents the conditions of millions across the world. It asks: what would have happened to birth outcomes if peace knocks on the door of a pregnant woman living in violence areas; what if hospitals provide more information about physicians' quality on delivery choices; which are the consequences of misreporting bullying to young adults; and whether conditional cash transfers incentivize social engagement. By no means, this thesis intents to present an exhaustive and exclusive list of solutions to poverty, but mostly, it symbolizes the need for credible evidence to design public action. The conclusions indicate we have good reasons to be optimistic.

The chapter two "*Babies and 'Bandidos': Birth Outcomes in pacified slums of Rio de Janeiro*" analyses the effects of intrauterine exposure to violence by exploring a series of police operations to pacify the slums of Rio de Janeiro. These operations introduced permanent 'Pacifying Police Units' (UPPs) in the heart of slums to patrol and maintain security within a delineated 'pacification boundaries'. Official crime records in these areas demonstrate, to name a few, sustained reductions in homicides (55 percent), robberies (55 percent), sharp increases in the apprehension of drugs and guns at pacification dates.

The argument is that pregnancies starting before, but ending 'quasi-randomly' around pacification dates are exposed to exogenous shocks on violence levels that is convenient empirically. This study contrasts with previous evidence by exploring the relation between *reductions* in crime and birth outcomes instead of using the occurrence of homicides and civil wars near to where expectant mothers live. In general, the results suggest that each month of pregnancy experiencing pacification increases birth weights in 4 grams and reduces the probability of low birth weight by 0.2 percent. Alternative measures of exposure, the inclusion of lags and forwards and a placebo test confirm these findings.

The chapter three "*Information Disclosure, Informed Mothers and Delivering Babies*" disserts on policies at the time of birth. It estimates what would have happened to C-section rates if information about physician's quality is disclosed to patients. Nonetheless, a common challenge to test such hypothesis empirically is that if in one hand mothers with complicated pregnancies can use the information to select the best physician possible, physicians can anticipate to information disclosure "gaming" with their C-section rates. The aim of this chapter is to separate the influence of physicians anticipating to information disclosure from patients searching for the most suitable treatment by using a legislative change in Brazil.

The legislation was enacted on 6 January 2015 and made it mandatory for private hospitals to publicly disclose information about physicians' performance on C-sections. However, a particular characteristic in the law makes possible to identify physicians' anticipation response; the legislation provided 180 days as an 'adaptation period' where any information needed to be disclosed to patients. Therefore, physicians had the advantage of knowing which information would be disclosed 6 months in advance.

Using data of 2.5 million births in Brazil from January 2014 to December 2015, chapter three compares the trends in delivery rates in private and public hospitals before and after the legislation, and includes the difference in trends before and after patients had access to information (on 7 July 2015). The results indicate a reduction in scheduled C-sections by 4.8 percent which two-thirds originates from physicians' anticipatory response. One proposes robustness checks using emergency C-sections, which usually emerges from unpreventable complications during the pregnancy; a falsification test using breech deliveries as an outcome highly correlated with C-section rates but not mentioned in the legislation, and re-estimating the effects for physicians-patients as a placebo group. All these tests corroborate that physicians' anticipatory response play a preeminent role on delivery choice.

In chapter four "*The effects of (misreported) bullying on labour and school outcomes of young adults*" estimates bullying effects on labour and schooling outcomes when 'true' bullying is imperfectly observed. The empirical strategy uses the reduced-form coefficients from longitudinal regressions to minimize the distance between 'true' and observed bullying impacts using a transformation matrix. The identification comes from students shifting among bullying histories across longitudinal regressions.

The estimating results show that high-school bullying decreases University attendance by 3.4 percent and increases the probability of being not in education, employed or in job training after high-school by 2.8 percent. For bullying involving direct interaction between bully and victim (e.g. a fight) there is a relatively similar effect for boys and girls, while acts not requiring interaction (e.g. gossip) have exclusive effects on girls. Moreover, the higher the frequency of bullying the stronger the impacts on labour and schooling outcomes.

The last chapter "*On the social capital consequences of conditional cash transfers: Evidence from Bolsa Família*" estimates the impacts of the conditional cash transfer Bolsa Família on social participation. The biggest concern while testing this hypothesis is on previous differences in observables and unobservable characteristics of households that influence participation in social capital and in Bolsa Família. Or putting simply, Bolsa Família is not randomly assigned to households.

To minimize the influence of selection bias, one initial adjustment proposes to match households according to their probability to participate in Bolsa Família. The estimation of such probability, i.e. the propensity score, includes variables used as criteria to select participants; per capita income, family composition and dummies for location. A second adjustment runs several regressions by level of per capita income (R$200, R$140 and R$100). And finally, one restricts the sample to households registered in Cadastro Único; a national registry or disadvantaged families that is mandatory to be eligible to any social program. The identification arises from comparing the differences in the trend of social affiliations of treatment and matched controls before and after receiving Bolsa Família.

Participation in social groups increases by .103 after households receive Bolsa Família compared to matched households in the control group. This effect seems to be "pro-poor", i.e.; it rises comparing households with lower levels of per capita income. Further estimations by social group demonstrate that affiliations concentrate on political movements, business associations, labour unions, and education groups. Additionally, access to informal credit ("Credit Fiado") also increases from 5.1 to 6.7 percent for households receiving Bolsa Família. Perhaps surprisingly, these increases in social participation are not followed by changes in the "number of friends you can count in hard times". If one assumes that the corrections attenuate the bias, it is possible to say that not considering the selection of beneficiaries reduces these effects by at least one-fourth.

These findings have crucial policy implications. If Diedre were exposed to a less violent environment during her pregnancy, there would be immediate benefits on birth outcomes or her child. Or providing her more information about physicians' performance would have decreased the probability of having an unnecessary C-section. Without sustained focus, people misreporting being victims of bullying have problems in engaging at University and in the job market after high school. And finally, social capital must be seen as a complementary channel where conditional cash transfers operate.

Chapter 2

Babies and 'Bandidos': Birth outcomes in pacified slums of Rio de Janeiro

What if violence goes through the womb and affects birth outcomes? In fact, a recent body of literature exploring eruptions in armed conflicts and local crime concludes that violence strongly predicts birth weight, low birth weight and prematurity of those exposed in utero (e.g., Koppensteiner and Manacorda, 2016; Mansour and Rees, 2012; Akresh et al., 2012). Yet, no evidence exists on the consequences of *reducing* violence on birth outcomes. Possibly, such absence reflects the rarity of an exogenous decline on violence levels during pregnancy or reasonable counterfactuals to perform estimations. This paper discussses this gap exploring a series of police operations to pacify the slums of Rio de Janeiro to analyse the effects of intrauterine exposure to sharp reductions in violence on birth outcomes.

Brazil persistently tops the list of one of the most violent countries in the world, according to UNODC (2014). The city of Rio de Janeiro, in particular, has 31 victims of homicide per 100,000 population each year (ISP, 2015). Under the gravity of this situation, the government of Rio de Janeiro conducted police policies to 'pacify' its slums. These 'pacifications' employed police squads specially trained to occupy areas of restrict access, and involved military apparatus to re-control the territory from armed groups and trafficking. But differently from retaking control and returning to its headquarters, the culmination point of each operation was installing 'pacifying police units' (or *Unidade de Policia Pacificadora* (UPPs)) in the heart of favelas to sustain security after the troops are gone. Most importantly, a particular characteristic of these operations is a precise 'pacification bounder' where each UPPs should patrol.

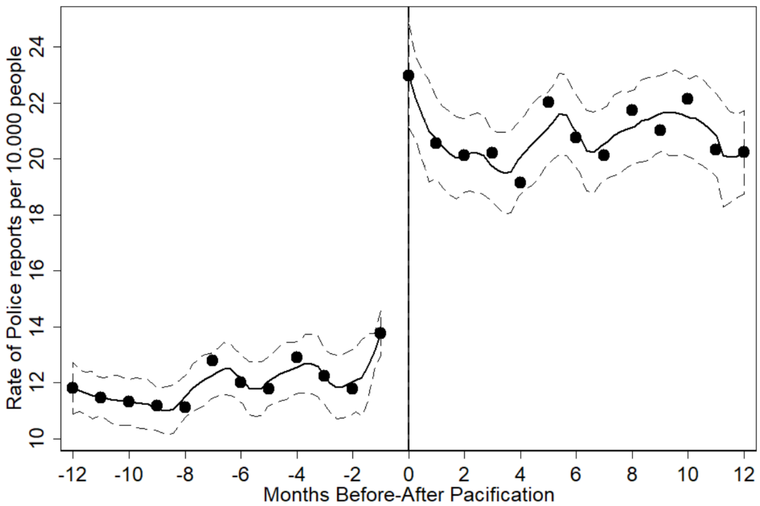


Figure 2.1: Rate of police reports in favelas before and after pacification

Notes: Figure 2.1 shows the rate of police report per 10.000 living inside the pacification boundaries. The rate of police reports is based on pacification dates shown in Table 6.1 in the supplementary materials. The population size inside pacification boundaries comes from the Brazilian Census 2010 and was calculated by the IPP (2015). The local linear regressions before and after pacification dates use local polynomial models with normal kernels and one-month bandwidth. The confidence intervals are 95 percent.

One can check the shock on violence caused by pacifications observing the number of police reports per 10,000 persons living inside pacification boundaries in Figure 2.1. Remarkably, Figure 2.1 presents a discontinuous jump in police reports at pacification dates that remains stable across the following year post-pacification. As we present in more detail in Section II, this discontinuity originates mainly from increases in flagging, apprehension of drugs and guns, residents reporting aggression, being threated or sexual assault. During the same period, homicides decrease 55 percent and homicides caused by the police reduced to basically zero. This relative success paved the way to the implementation of 37 UPPs along 2008 to 2015, benefitting more than 700,000 people living in more than nine thousand square meters of pacified areas (IPP, 2015).

Based on Figure 2.1, I argue that pregnancies starting before but ending neighboring pacification dates experience a 'quasi-random' shock on the intrauterine exposure to violence that configures a convenient opportunity to estimate its effects on birth outcomes . To circumvent recurrent biases faced by previous studies, I estimate if there is any significant discontinuity in birth outcomes at the pacification dates for women living in the same street inside pacification boundaries before and after pacification. Therefore, I fit a regression discontinuity model at pacification dates and boundaries.

There are some merits of using this identification strategy. A straightforward attractiveness is that the data allows to overlap home addresses during pregnancy and official pacification boundaries to assigning mothers experiencing pacification. So, one measures the intention-to-treat (ITT) effect of mothers experiencing abrupt reductions in crime during pregnancy. As long the government advertised pacifications up to 15 days before it started (see Table 6.1), major concerns on negative selection of pregnancies would be lessened once pacifications were improbable to be anticipated. Furthermore, because pacification effects are measured at street level, one can also argue that pregnancies ending just before pacification are unlikely to be systematically different from pregnancies ending just after pacification other than in the exposure to violence. To my knowledge, this is the only study with these advantages.

Another particularity of this study relative to previous evidence is the source of variation in the exposure to violence during pregnancy. While the literature conventionally relies on peaks of homicides (Mansour and Rees, 2012; Koppensteiner and Manacorda, 2016), terrorist attacks (Camacho, 2008; Lauderdale, 2006) or wars (Akresh, et al., 2012; Bundervoet et al., 2009; Minoi and Shemyakine, 2014), the 'quasi-experiment' of pacification goes in the opposite direction evaluating the impacts of intrauterine exposure to reductions of violence. Would the impacts of pacification on birth outcomes be similar to eruptions of violence? Little, if any, evidence has been provided to this question and this paper provides a first approach to help in this discussion.

To test this hypothesis, I use restricted geocoded data on the *System of Information of Living Births* (SINASC) over the period from 1^{st} January 2007 to 31^{st} December 2015 from the city of Rio de Janeiro. This data provides information about the characteristics of mothers, current and previous pregnancies and birth outcomes of around 700,000 births. Information of dates of birth, length of pregnancy and conception dates permit to identify mothers experiencing, or not, pacification and in what trimester of gestation. Following previous literature, particular attention is on birth weight and low birth weight, but natural extensions include preterm birth and the number of prenatal visits.

The main implication of our results is that intrauterine exposure to less crime improves birth outcomes. More precisely, each month experiencing pacification increases birth weight in 4 grams and reduces low birth weight in 0.2 percent. Nevertheless, there is a high heterogeneity by trimester of gestation. Pregnancies exposed during first trimester increase birth weight by 30 grams and reduce the incidence of low birth weight from 0.8 to 1.5 percent compared to pregnancies living in the same street but ending before pacification dates. For those experiencing pacification during the third trimester, birth weights increase 50 grams, yet it was not possible to detect any effect on low birth weight. When one compares changes in the trends in birth outcomes of mothers living in opposite sides of pacification boundaries before and after it occurs, a similar picture corroborates these conclusions.

Indeed, to claim causal effects of intrauterine exposure to less crime on birth outcomes necessarily requires that birth outcomes change through reduction in crime inside pacification borders, and not through unobservables associated with the shock. To provide a more convincing case for my findings, I perform a placebo test estimating the regressions for mothers residing near but outside pacification boundaries. I assume that pregnancies geographically close share similar shocks on the unobservables that could be mistakenly attributed to pacification. The placebo test indicates that mothers living outside pacification boundaries do not present significant changes in birth outcomes at pacification dates.

But which are the mechanisms that the reduction of crime by the pacification of favelas improves birth outcomes? However difficult to answer this question, three possibilities may translate these results; one psychosocial, another structural and a final behavioral. The psychosocial channel is that pacification reduces stress and anxiety experienced by pregnant women affecting their birth outcomes. There is a vast body of studies suggesting that psychological factors constraint fetal growth and induce premature birth by restricting oxygen and nutrients taken by the fetus (Masi et al., 2007; Kramer, 1987; Camacho, 2008; Torche, 2011; Wadhwa et al., 1993). A validation test using the association between prematurity and birth weight, confirms that pacification decreases the probability of premature birth (< 37 weeks) by 7.3 percent. Therefore, this paper joins the recent economic literature arguing on the implications of stress and anxiety on birth outcomes (Camacho, 2008).

The sudden presence of police in favelas could also alter the services available for pregnant women. In this matter, further estimations show a reduction in the share of pregnancies with less than four prenatal visits by 2.9 percent at pacification dates. This result may indicate that women have better access to prenatal care or there is a higher supply of health services in pacified favelas - we are not able to provide evidence which one predominates. What can be said is that while further investigations do not detect any change in the probability of delivering in a hospital, there is indication that the later women start their pregnancy after pacification dates the higher is the probability of using prenatal services. A final interpretation for our results is that women living in pacified areas change their nutritional and health behavior. If reductions in stress and anxiety caused by pacification encourage women to smoke less, to avoid drinking alcohol and caffeine, both could be pointed as possible channels for our estimates.

2.1 Literature review

Empirical studies have recently documented a negative association between intrauterine exposure to violence and birth outcomes. In general, this conclusion rises applying two main empirical strategies: exploring short-term unpredictable and extreme peaks of violence in the proximities, or using long-term eruptions of armed conflicts and wars. The identification usually derives from comparing birth outcomes of fetuses experiencing these shocks with others that, by change or because they live in non-violent regions, did not experience such events.

Belonging to the first group, the most similar study to mine comes from Koppensteiner and Manacorda (2016). They estimate the effects of homicide incidence on birth outcomes in Brazil. Their identification compares expectant mothers exposed to homicides during pregnancy to mothers residing in the same area but not exposed to homicides. The authors find that one standard deviation increase in homicides during the first trimester of pregnancy reduces birth weight in around 2 grams and increases the probability of low birth weight by .6 percent. Nonetheless, these estimations consider cities smaller than 5.000 citizens.

Yet, performing the same strategy for Fortaleza, now a city of 4 million citizens, Koppensteiner and Manacorda (2016) find that one standard deviation increase in homicides during the first trimester of pregnancy reduces birth weight in 0.41 grams, and increases low birth weight in .3 percent: i.e., 15 percent the magnitude found for small municipalities. The authors argue that this reduction reflects higher average of violence in Fortaleza compared to small cities which makes the impact of additional homicides less prominent. However, it is still not clear whether such differences reflect heterogeneous capacity of mothers to select themselves to have babies in safer neighbors or during calmer periods. Or if homicides are a noisy proxy of violence in bigger municipalities when crime, and poverty, are likely to be geographically concentrated.

Additional evidence of intrauterine exposure to violence comes from terrorist attacks. Camacho (2008) is the first study in economics measuring the effect of prenatal stress on child birth outcomes exploring landmines explosions in Colombia as a random exogenous variation in the level of prenatal stress. The results show that children born with at least one landmine explosion during each trimester of pregnancy weigh on average 27 grams less than those births experiencing any explosion. In a similar study, Lauderdale (2006) uses the upsurge in anti-Arab sentiment following the terrorist attacks of September 11, 2001 in the United States to instrument prenatal stress of Arabic-named women during pregnancy. Comparing birth outcomes of Arabic-named women with Arabic-named expectants one year earlier, he finds that the first group is 34 percent more likely to have a low birth weight and 1.5 time more likely to have a premature birth during the semester after September 11. For other ethnic groups, there is no significant effect . Similar evidence using terrorist attacks is found by Smits et al., (2006) and Eskenazi et al., (2007).

A second group of evidence explores long-term armed conflicts. These studies rely on the temporal and spatial variation of violence to compare pregnancies living in areas of conflict and non-conflict. The type of conflicts ranges from escalation of homicides in Mexico during the war against drugs (Brown, 2014), fatalities caused by Israeli security force (Mansour and Rees, 2012), war in Eritrea-Ethiopia (Akresh, et al., 2012) and Burundi (Bundervoet et al., 2009), and conflicts in Cote d'Ivore (Minoi and Shemyakine, 2014). The models usually control for area of residence, birth cohort, individual and household characteristics. Not surprisingly, women experiencing violence during the first trimester of pregnancy have lower birth weight and higher probability of low birth weight compared to pregnancies not exposed.

In broader terms, it is possible to say that exposure to sporadic and erratic violence tend to produce smaller effects on birth outcomes than long-term, and perhaps more brutal, conflicts. For example, Camacho (2008) finds that a single landmine bomb decreases birth weight by 8.7 grams, but sequential bombings during pregnancy decrease it by 27.7 grams. Additionally, if estimates from Koppensteiner and Manacorda (2016) and Mansour and Rees (2012) suggest a reduction of approximately 2 grams in birth weight for a one standard deviation increase in homicides, long-term escalation of homicides in Mexico is associated with decreases in birth weights of 75 grams (Brown, 2014). It is also usual to find stronger effects on the first trimester of pregnancy (Koppensteiner and Manacorda, 2016; Camacho, 2008) and smaller, yet less frequent, effects during the third trimester (Mansour and Rees, 2012).

2.2 The Pacification of favelas and descriptive statistics

2.2.1 The pacification of favelas in Rio de Janeiro

According to the Institute Pereira Passos (2015), the city of Rio de Janeiro has 1,093 million people living in 1,035 favelas (approximately 20 percent of its entire population). It should not be a surprise that these communities, most commonly, have unideal provision of public services, a great share of informal housing, and lack access by the police. Their proximity of touristic areas and rich neighborhoods also makes drug trafficking a profitable activity. Altogether, these conditions foster armed gangs to fill the vacuum left by the state establishing parallel laws for those living in the territory, solving disagreements internally and even providing bank services (World Bank, 2011). At some point in time, some favelas had become unreachable to the state.

In an effort to change such scenario, the government of Rio de Janeiro introduced in December 2008 a pilot program to pacify the favela Dona Marta (located in the upper class neighborhood 'Botafogo'). However, instead of undertaking the historical approach of confronting the armed groups and leaving the area, the focus of these operations is to install permanent police stations named '*Unidade de Policia Pacificadora*' (UPP) - or 'Pacifying Police Units' - in the heart of favelas. The government declares that "Pacifying Police Units aim to recover the territories under control of illegal armed groups, to restore the monopoly of the state in the use of force and to reduce criminal levels, especially of lethal violence" (Diário Oficial, 2015; ISP, 2015).

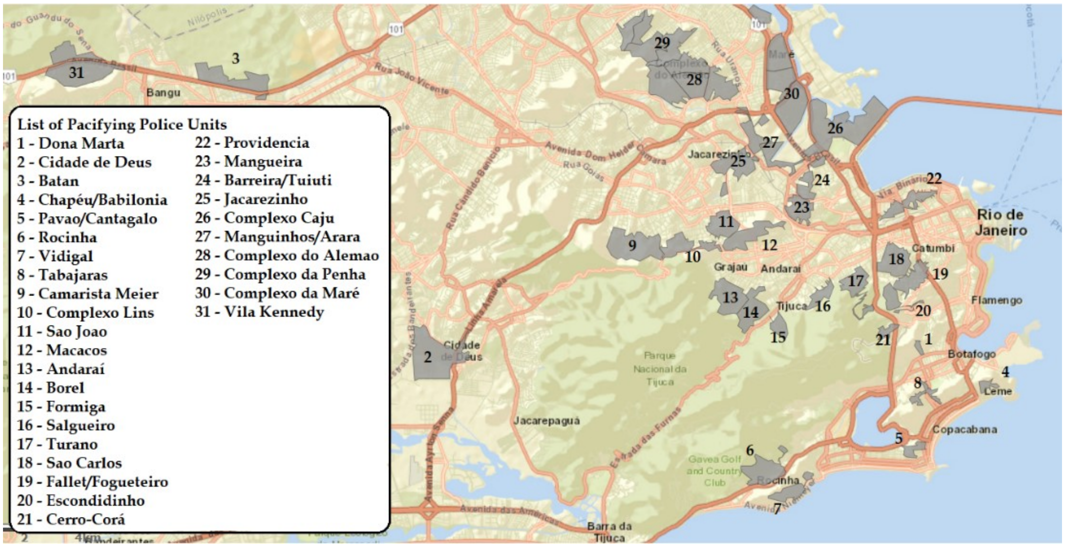


Figure 2.2: Pacification boundaries of UPPs in Rio de Janeiro

Notes: This map demonstrates 31 areas with Pacifying Police Units (UPP) in Rio de Janeiro. The UPP boundaries are in grey and do not represent a single UPP. In Complexo do Alemão (Nº 28) and Complexo da Penha (Nº 29) there are four UPPs. Table 6.1 in the supplementary materials shows descriptive statistics for each UPP area. *Source*: Instituto Pereira Passos, State of Rio de Janeiro, Brazil (2015).

Under its apparent success[1] , the UPP Dona Marta paved the way to the installation of additional 36 UPPs by the end of 2015, which operate in 196 favelas where nearly 1.5 million people live. The location of each UPP along with their respective names are shown in Figure 2.2. The first set of favelas being pacified were Dona Marta, Batan, Cidade de Deus, Chapéu/Babilonia, Pavão/Cantagalo, and Tabajaras (respectively numbers 1, 2, 3, 4, 5, 8) - most of them in the richer south. The biggest favelas are Rocinha (Nº 6), Complexo do Alemão (Nº 28), Complexo da Penha (Nº 29) and Complexo da Maré (Nº 30). Because the pacification of Vila Kennedy (Nº 31) occur late 2015, that is the only UPP excluded from our empirical analysis. A list of favelas, along with population size and number of communities is shown in Table 6.2 in the Supplementary Materials. Altogether, UPPs account for more

[1] In a survey made by Fundação Getúlio Vargas (2013) after the pacification of *Dona Marta* and *Cidade de Deus*, 66 percent of the population in these areas approved the pacification programs, and 93 percent felt safer. Another evidence from this survey is that 70 percent of residents in non-pacified areas would like to have an UPP in their community (FGV, 2013).

than 9,500 police officers patrolling over 9.446.047m^2 of occupied areas (IPP, 2015).

In the process of pacification, confrontation with armed gangs across occupations was far from homogeneous. Critically violent areas demanded preparation similar to a war. For example, the occupation of 'Complexo do Alemão' and 'Complexo da Penha', likely the most violent favelas in Rio de Janeiro, involved 2,700 armed officers, war apparatus of armoured tanks and helicopters, and was broadly broadcasted by national and international media. Earlier estimates from the police indicated around 600 drug dealers living in these communities, and by the end of the operation, the police seized at least 50 marine rifles, and more than 40 tons of drugs - including 200 kg of crack-cocaine (OGlobo, 2010). On the other hand, in 'Rocinha' (n 6), police did not fire a single shot to retake control (OGlobo, 2011).

Along with permanent policing, another decisive characteristic of pacifications is their 'pacification boundaries'. That is, when the pacification of a favela was over, the government decrees official boundaries where each UPP must patrol and maintain security once special forces leave. Consider, for example, the case of UPP Pavão/Cantagalo in Figure 2.3. The grey area outlines its borders which includes 127,953m^2 with 10.338 residents (IPP, 2015, from the Brazilian Census 2010). The community of Pavão/Cantagalo was occupied on December 23rd, 2009, and shares its borders with the neighborhoods *Copacabana* and *Ipanema*.

2.2.2 Crime data

This paper argue that pacifications offer an exogenous variation in crime levels experienced by pregnancies residing inside pacification boundaries, however what is the empirical evidence? A natural way to check whether there was a shock in crime inside occupied favelas is comparing crime rates at the pacification dates. Table 2.1 presents several crime statistics one year before and after pacification dates, and an includes additional column testing the mean differences between both periods. To observe police activities and crime incidence separately, Panel A shows rates of police actions while

Figure 2.3: Example of pacification borders of Pavão/Cantagalo

Notes: The pacification border of UPP Pavão/Cantagalo comes from the Diírio Oficial (2009). All boundaries for 37 UPPs in this paper are presented individually in the Supplementary Materials

Source: Instituto Pereira Passos, State of Rio de Janeiro, Brazil (2015).

Panel B displays crime rates. Individual plots showing monthly changes are shown in the *supplementary materials*.

Panel A in Table 2.1 reveals strong increases in the rates of police reports, flagging, and apprehension of drugs after pacification dates. Particularly, 'flagging' and 'apprehension of drugs' increase approximately four fold. While in the year preceding pacification there were 5.45 flagging per 100,000 persons in UPP areas, it reaches almost 18 in the following year. Similarly, the rate of drugs apprehended goes from 9.31 to 31.81 cases per 100,000 persons in the same period. Figures 6.19 in the *supplementary materials* show a positive jump in these variables just after police arrived in favelas.

Higher presence of the police can be seen in the increase of twice as many mandates (line 6) than before pacification, and on the police causing almost zero homicides (line 4). In the meanwhile, there are shy, but still significant, increases in the rate of 'Bodies found' (line 7) and reports of people missing (line 8). Although I acknowledge that flagging and police mandates do not inform which crimes were foiled - whether violent or non-violent - and 'drugs apprehended' does not account for quantity, Table 2.1, along with figures in

Table 2.1: Police activities and crime rates before and after pacification dates and inside UPP boundaries

	Year before pacification (A)		Year after pacification (B)		Time difference (B) - (A)
	Mean	Std. dev	Mean	Std. dev	
Panel A-Police activities per 100.000 persons					
01. Police reports	135.9	(7.95)	231.7	(10.0)	95.80***
02. Flagging	5.459	(1.51)	17.94	(2.06)	12.48***
03. Apprehension of drugs	9.360	(1.94)	31.81	(4.05)	22.45***
04. Homicides cause by police	1.619	(0.78)	0.270	(0.27)	- 1.34***
05. Police being killed	0.046	(0.15)	0.042	(0.07)	- .004
06. Police mandates	1.527	(0.71)	3.217	(1.14)	1.69***
07. Bodies found	0.216	(0.24)	0.441	(0.26)	.225***
08. Reports of people missin	2.082	(0.69)	2.591	(0.76)	.509**
Panel B-Crimes per 100.000 persons					
09. Homicide	3.362	(1.11)	1.566	(0.61)	-1.79***
10. Reports of attempt murder	1.943	(0.67)	2.762	(0.96)	.819**
11. Reports of threat	14.56	(1.86)	33.98	(4.10)	19.42***
12. Reports of stealing	23.67	(2.41)	30.50	(1.61)	6.83***
13. Reports of lethal violence	21.13	(2.79)	52.75	(6.42)	31.62***
14. Reports of robberies	22.65	(2.40)	15.30	(2.51)	- 7.35***
15. Reports of sexual assault	1.326	(0.60)	2.078	(0.85)	.752***

Notes: Table 2.1 demonstrates crime rates per 100,000 persons living inside pacification boundaries. A full map of boundaries are shown in Figure 2.2 and figures for individual UPPs are presented in the *supplementary materials*. Population sizes inside these areas was measured by Instituto Pereira Passos (IPP, 2015) using the Brazilian Census 2010. Column (A) displays the rates one year before pacification, and column (B) is for one year after pacification dates. Column (B) - (A) calculates the difference between rates in both periods, clustering the standard errors by favela. 'Police reports' compromise all crimes and police actions reported.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

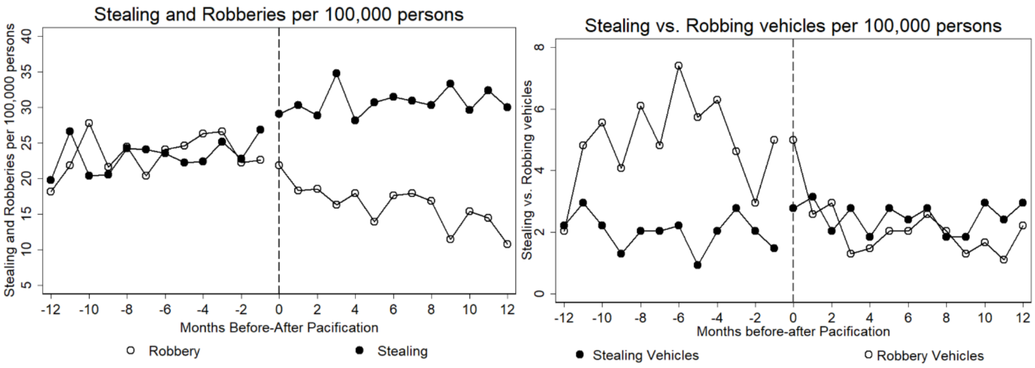
the *supplementary materials*, indicate significant increases in policing after pacification dates inside UPP boundaries.

Perhaps the most distinct indication that pacifications reduced crime in UPP areas comes from homicide rates (in line 9) and from reports of treat (line 10), lethal violence (in line 13) and sexual assault (in line 15). The rate of homicide per 100,000 persons living in UPP areas decreases around 55 percent comparing both periods, and seems to sustain the rate around 1.5 during the year following the pacification (see Figure 6.19). Interestingly, there is a strong increase in the rate of persons reporting being threat, suffering lethal violence, and being victims of sexual assault after pacification. Regarding the first two, there are 3 times more people reporting being threatened or violented to the police, and almost twice as many people reporting sexual assault. A plausible explanation for this increase is the inaccuracy of crime recording before pacification, which implicates that these results merely demonstrate better recording practices when UPPs start operating. But we should also consider other possibilities.

Low reporting rates before pacification may also rely on the 'law of silence' imposed by armed groups in favelas (UNODC, 2013). As drug dealers prefer police away from their territory, the population was not allowed to report crime - the community 'leader' solves disagreements in his/her own manner (World Bank, 2011). Moreover, similar increases in reporting rates have already been found by Monteiro (2013) using a different source of data. Another more optimistic possibility is that the higher reporting rates in Table 2.1 reflect trust in the UPP police (UNODC, 2014).

But which kind of violence has been foiled by UPPs? Along with the drastic increase in drug apprehension and flagging, and decreases in homicide rates, further evidence on which types of crimes are being prevented arise from comparing stealing and robbery. This comparison is useful because robbery and stealing involve unauthorized taking of property from another but while the first involves the use of physical force or violence, the second does not. Therefore, it provides us some indication whether UPPs have different impacts on violent or non-violent crimes. Figure 2.4 accounts for this fact showing monthly rates of stealing and robbery per 100,000 persons inside the pacification boundaries. Because we have information about vehicles separately, I include it as an additional category in Figure 2.4 (B).

Figure 2.4 (A) shows opposite trends for robberies and stealing after pacification. Before pacification both rates present a very similar pattern, staying around 23 reports per 100,000 persons. However, if on one hand the rate of people reporting stealing in the year following pacification increases to approximately 33 reports, robbery shows a steady decrease along the pacified year. A similar conclusion emerges comparing stealing and robbery ratres of vehicles. Figure 2.4 (B) demonstrates a higher rate of robberies of vehicles



(a) Stealing vs. Robbery (b) Stealing vs. Robbery of vehicles

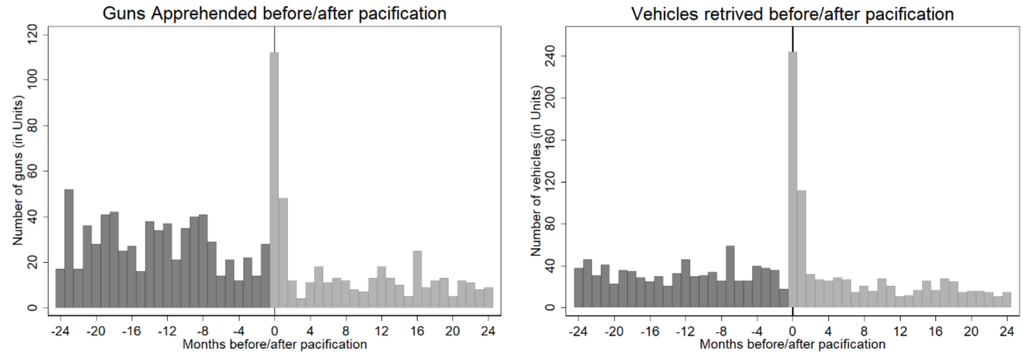
Figure 2.4: Stealing vs. Robbery in pacified favelas

Notes: The plots (A) and (B) in Figure 2.4 compute the rate of stealing and robbery per 100,000 persons living inside UPP boundaries before and after pacification. Each UPP boundary is shown in Figure 2.3. Table 6.1 in the Supplementary Materials presents the pacification dates used. *Supplementary materials* also present further descriptions and figures of the crime data.

(5 per 100,000 persons) than stealing (around 2) in UPP boundaries before pacifications occurs and such difference is entirely abbreviated after pacifications due to the decline in the rates of robberies. Therefore, UPPs seem to have a higher rate of success preventing more violence crimes as robbery - which usually involves physical force, or gun pointing - but less success in non-violent crimes as stealing.

Another evidence that pacifications represent a shock on crime levels comes from guns apprehended and vehicles retrieved in Figure 2.5. Two implications can be seen: one is that when the pacification occurs there is a peak of at least 5 folds from the historical average of guns apprehended and vehicles retrieved, and second, both remain in lower levels than before pacification. These results reinforce the argument that pacifications represent a discontinuous shock in policing inside these communities.

Taken these results together, it seems fair to say that pacifications accomplished their aim of 'reducing lethal violence' and establishing state's sovereignty. Based on the results from Table 2.1 and Figures 2.4 and 2.5, there is a nonnegligible increase in drugs and guns apprehended, flagging, and a reduction in the rate of robberies and homicides per 100,000 persons



(a) Guns apprehended (b) Vehicles retrieved

Figure 2.5: Guns apprehended and Vehicles retrieved in pacified favelas

Notes: This bar graph plots the monthly total number of guns apprehended and vehicles retrieved before and after pacification. The UPP areas used to calculate these numbers are presented in Figure 2.2.

after pacification. Some evidence suggests that UPPs are more likely to foil violent (homicides or robberies) than non-violent crimes (stealing). And persons living in favelas seem to be more likely to report crimes (threat, sexual assault and lethal violence) to the police.

2.2.3 Birth data

This paper uses a restricted-use geocoded data of Rio de Janeiro from the System of Information on Life Births (SINASC) from January 1, 2007 to December 31, 2015. The data contains all births registered (nearly 700,000 births), and provides information about several birth outcomes (birth weight, congenial malformation, gender, if twins), pregnancy characteristics (Length of pregnancy[2], delivery method, prenatal visits, place of delivery, emergency C-section), previous pregnancies (if had an abortion, number of previous normal births and C-sections), and mothers' characteristics (age, race, married, level of education, occupation and number of children). The data also provides unique identification codes for the hospital of birth that are useful to control for hospital fixed effects.

[2]The length of pregnancy is presented in ranges; less than 22 weeks, from 22 to 27 weeks, from 28 to 31 weeks, from 32 to 36 weeks, from 37 to 41 weeks and more than 42 weeks of gestation.

An important information in SINASC is the residential addresses of mothers during pregnancy. With this information, one overlays their addresses with official pacification boundaries (around 7,800 streets) shown in Figure 2.2 to identify women residing in each pacified area. Such information also allows identifying mothers living in the proximities but outside pacification boundaries and perform some robustness checks. The identification of mothers who experienced pacifications while pregnant comes from conception dates generated by subtracting the length of pregnancy from dates of birth. Nevertheless, we test the validity of this assignment rule exploring an alternative measure of pacification exposure.

Table 2.2 summarizes birth outcomes and mothers' characteristics for births ending no more than eight months before pacification. The column "Inside UPP boundaries" reports descriptive statistics for pregnancies residing inside UPP boundaries and the column "Outside UPP boundaries" relates to pregnancies living in the opposite side of pacification boundaries (Table 6.1 in the appendix lists these neighborhoods). Apart from prematurity data, the descriptive statistics are for non-premature births (> 37 weeks).

Table 2.2 shows that before pacification mothers living inside pacification boundaries have babies 13.1 grams lighter than those living outside. Additionally, pregnancies in pacification areas have higher probability of low birth weight, preterm birth and have higher percentage among those with no more than four prenatal visits. Mothers in pacified boundaries are younger, more likely to be single and black, and also have more children than mothers in non-pacified areas. There is a significant difference in the percentage of professional mothers - which may reflect imbalances in education and income - but a surprisingly similar incidence of very premature births and abortions.

2.3 Identification Strategy

The pacification of favelas in Rio de Janeiro produced abrupt reductions on crime (see Section II) by re-introducing permanent 'pacifying police units'

Table 2.2: Descriptive statistics inside and outside UPP boundaries before pacification

	Outside UPP boundaries		Inside UPP boundaries	
	Mean	Std. dev	Mean	Std. dev
Birth outcomes				
Birth weight (in grams)	3272.2	(468)	3259.1	(472)
Low birth weight (< 2500 grams)	.037	(.191)	.049	(.216)
Premature (< 37 weeks)	.102	(.303)	.107	(.309)
Very premature (< 32 weeks)	.018	(.134)	.018	(.133)
Up to 4 prenatal visits	.065	(.248)	.099	(.300)
Delivery in a hospital	.964	(.187)	.994	(.077)
Mothers' characteristics				
# of children	.764	(1.05)	.991	(1.30)
Single	.635	(.481)	.721	(.448)
Mother's age (in years)	27.2	(6.58)	25.1	(6.68)
If twins	.013	(.117)	.008	(.092)
% of C-section	.598	(.490)	.415	(.493)
% of black mothers	.047	(.212)	.083	(.276)
% of male	.514	(.500)	.519	(.500)
% of congenital malformation	.004	(.067)	.007	(.083)
% of mothers who had an abortion	.269	(.444)	.272	(.445)
% of mothers working in public service	.018	(.133)	.008	(.090)
% of professional mothers	.171	(.377)	.030	(.171)
% of technical-high-school mothers	.063	(.243)	.024	(.153)
% of repair or housekeeping	.546	(.498)	.658	(.474)
# of pregnancies	8,670		7,853	

Notes: Table 2.2 demonstrates descriptive statistics for mothers living inside and outside UPP boundaries eight months before pacification. We use eight months before pacification as a baseline, because our main estimations compare non-premature births (> 37 weeks) starting before pacification but ending eight months before or after the police arrived. Naturally, statistics for premature and very premature births are exceptions, and include all births in both areas. The variables for mothers' occupation come from the Brazilian Occupation Codes 2002. The number of births per month in each favela is used as weights.

(UPPs) in areas previously dominated by armed gangs and trafficking. Most particularly, these operations demarked official boundaries of action where officers should patrol and maintain security. Exploring this exogeneous shock in a delineated area and date offers a sensible opportunity to estimate the effects of intrauterine exposure to violence on birth outcomes. If one can observe any discontinuity in birth outcomes of mothers living inside UPP boundaries following the discontinuity in crime due to pacification, there is a plausible case to claim causal effects of crime on birth outcomes.

Assume that DOP_f represents dates of pacification in the pacified favela f, and let DOC_{if} and DOB_{if} be the date of conception and date of birth for pregnancies i residing in f. It is important to observe that DOP_f considers pacification dates instead of UPPs' inauguration dates[3]. Intrauterine exposure to less crime occurs if a pregnancy starts before the pacification dates ($DOC_{if} < DOP_f$) but has date of birth at least as later as the pacification dates ($DOB_{if} \geq DOP_f$). We argue that pregnancies *starting* before but *ending* around pacification dates are 'quasi-randomly' exposed to discontinuous reductions in crime. Therefore, one considers an indicator variable to represent pregnancies exposed to pacification during pregnancy as;

$$P_{if} = 1[DOC_{if} < DOP_f | DOB_{if} \geq DOP_f], \tag{2.1}$$

P_{if} equals one for pregnancies experiencing the pacification of the favela f and assumes the value zero when mothers started their pregnancy and gave birth before the pacification dates ($DOC_{if} < DOP_f | DOB_{if} < DOP_f$). To maintain congruence with previous literature (Camacho, 2008; Mansour and Rees, 2002; Koppensteiner and Manacorda, 2016; among others), one splits P_{if} in three dummy variables to represent the trimester of gestation mothers experienced pacification. The variable $P_{if(1)}$ identifies exposure during the first trimester of pregnancy if the conception dates are up to 2 months before pacification dates, $P_{if(2)}$ considers exposure during second trimester when mothers start their pregnancy from 3 to 5 months before pacification, and finally, $P_{if(3)}$ relates to exposure during the third trimester if pregnancies start 6 to 9 months before pacification dates. Such division allows us to observe heterogeneities in the effects of pacification in birth outcomes. Figure 2.6 illustrates in more detail how mothers are allocated according to this strategy.

The estimating equation is given by;

[3] I argue that the operation dates represent when the reduction in crime occurs. Due to budgetary constraints, allocation of staff and construction of the police station there were cases where troops remained a long time in the favela until the UPP was finally inaugurated (for example, approximately 5 months in Complexo do Alemão and Complexo da Penha).

Before Pacification: $DOC_{if} < DOP_f$				After pacification
$DOB_{if} < DOP_f$	$DOB_{if} > DOP_f$			$DOC_{if} \geq DOP_f$
$P_{if} = 0$	$P_{if(3)}$	$P_{if(3)}$	$P_{if(1)}$	
-12 -11 -10	-9 -8 -7 -6	-5 -4 -3	-2 -1	0

Conception month before the pacification

Figure 2.6: Allocation of treatment and control groups

$$
\begin{aligned}
BO_{isf} = \alpha + \gamma_1 P_{if(1)} + \gamma_2 P_{if(2)} + \gamma_3 P_{if(3)} + \\
\beta X_i + t + \rho_{hospital} + \rho_{MOB} + \rho_{streets} + \varepsilon_{isf}
\end{aligned}
\tag{2.2}
$$

where BO_{isf} represents the birth outcome of interest for the expectant mother i, living in the street s and favela f. For example, when BO_{isf} is birth weights the interpretation of γ_1,γ_2,and γ_3 - the parameters of interest - is changes in birth weights for mothers experiencing pacification during the first, second and third trimester of gestation relative to mothers ending their pregnancies up to 3 months before pacification dates. X_i controls for mother's age, if married, if twins, malformation, number of children and abortions, if black, and dummies for occupational groups[4]. Eq. (2.2) also includes a time trend variable t centered in pacification dates, and fixed-effects for hospital of birth ($\rho_{hospital}$), month of birth (ρ_{MOB}), and the street mothers lived during their pregnancy ($\rho_{streets}$). The inclusion of street fixed-effects $\rho_{streets}$ makes the identification of pacification arise from changes in birth outcomes of women living in the same street before and after pacification dates. If unobservable factors at street level plague credible interpretation of pacification effects, our model has the advantage of circumventing these biases. Finally, ε_{isf} is the error term.

As Eq. (2.2) identifies the effects of intrauterine exposure to violence exploring pregnancies ending around pacification dates, one should assume that observable and unobservable characteristics do not present any discontinuity at the threshold. Perhaps the most powerful way to check for discon-

[4] Our data provides codes for mother's occupation, the model includes separate dummies for each occupational group, 'public service', 'professional' and 'technical worker'. Professional is defined as professionals demanding Diploma, while technical workers are defined as having high school.

tinuities in observables is plotting average characteristics of mothers *ending* their pregnancy before or after pacification dates but starting before police arrives. Plots for several variables are shown in Section 5.A in the *supplementary materials*, and support the assumption of non-discontinuity. This assumption seems reasonable even for unobservables once the government of Rio de Janeiro set pacification dates based on predetermined strategic location of favelas, levels of lethal violence and budget constraints - thus, birth outcomes were never used as a decision rule.

Another fair questioning is whether mothers are able to assign themselves to the treatment group by choosing when to start or postpone having a child. Here, we constraint our estimations for pregnancies starting at least one month before pacification dates to minimize sample selection. Because announcements of pacification occur up to 15 days prior the operations, I argue that mothers belonging to treatment and control groups were already pregnant and could not anticipate to the pacification. Excluding pregnancies starting after pacification dates reduces biases rising from migration since it is more likely to occur after pacification takes place. Robustness checks provide additional evidence on these hypothesis.

A final source of concern is how far a pregnancy should end before pacification dates in order to compose the control group. If an unnecessary large bandwidth is selected the comparison of pregnancies to distant in time may exacerbate the influence of unobservables and produce uninformative conclusions, but at the same time considering pregnancies ending too early may compromise the consistency of our estimations by reducing sample sizes. I confront this issue including in the control mothers ending their pregnancy from eight to three months before pacification dates.

2.4 The impacts of pacifying favelas on birth outcomes

Tables 2.3 and 2.4 present estimation results on the effects of pacification of favelas in Rio de Janeiro on birth weight, and the probability of low birth weight (< 2,500 grams). All estimations compare pregnancies with more than 37 weeks of gestation starting before, but ending around the pacification dates. The estimates of interest are shown by the coefficients 'First', 'Second' and 'Third' trimesters of gestation, as defined in Section IV. These variables represent the stage of pregnancy which the pacification occurs and are useful to capture heterogeneities in the effects of in utero exposure to lower levels of crime, as vastly used in the referred literature. Columns '[1]', '[2]' and '[3]' consider pregnancies *ending* 8, 6, and 3 months before pacification dates as a control group to check the sensibility of the estimating results. Column [4] is a falsification test and includes a dummy for pregnancies ending one trimester before pacification and another dummy for pregnancies starting one trimester after pacification. Standard errors are clustered at conception dates and shown in parenthesis.

There are at least two main findings in Table 2.3. First, intrauterine exposure to pacification increases birth weight of pregnancies living inside pacification boundaries relative to mothers living in the same street but giving birth just before the police arrived. In more concrete terms, the coefficient 'First trimester' in column '[1]' shows that pregnancies experiencing pacification during the first trimester increase their birth weight by 29.14 grams, while for 'Third trimester' birth weight increases 52.43 grams. There is no evidence of pacification effects for the second trimester of gestation in any model. Overall, Table 2.3 indicates that reductions in crime levels led by the pacification of favelas affected the birth weight of mothers in their first and third trimester of gestation.

A second finding in Table 2.3 is that pregnancies ending one trimester before pacification do not show any significant change in their birth weights relative to pregnancies ending before them and living in the same street. It is what one would expect to find since pregnancies ending before the pacification do not experience any reduction in crime rates as shown previously in Section II. In complement, Table 2.3 shows that women starting their pregnancies during the trimester following pacifications also present insignificant changes in birth weights. That is an intriguing result. Once the focus of pacifications is to control favelas permanently and better security in the

Table 2.3: The effects of pacification on birth weight (clustered standard erros by conception month)

	Birth weight in grams			
	Inside UPP boundaries			
	[1]	[2]	[3]	[4]
One trimester before pacification	-	-	-	- 10.54
	-	-	-	(12.22)
First trimester	29.14^{***}	31.97^{***}	30.94^{***}	16.99^{*}
	(8.97)	(8.58)	(9.93)	(9.66)
Second trimester	18.86	24.80	22.25	8.23
	(14.03)	(16.10)	(16.02)	(15.54)
Third trimester	52.43^{***}	55.69^{***}	55.95^{***}	35.39^{***}
	(6.82)	(7.52)	(9.14)	(7.07)
One trimester after pacification	-	-	-	12.74
	-	-	-	(11.00)
Includes controls and time trend?	yes	yes	yes	yes
Fixed effects:				
Hospital of birth	yes	yes	yes	yes
Month of birth	yes	yes	yes	yes
Pacification dates	yes	yes	yes	yes
Street of residence	yes	yes	yes	yes
# of pregnancies	18,726	16,527	13,325	24,241
R^2	.24	.25	.27	.23

Notes: All estimations in Table 2.3 use pregnant women living in 31 UPP areas shown in Figure 2.2 and listed in Table 6.1 in the appendix. Premature deliveries with less than 37 weeks of gestation are excluded, and constitute a separate analysis. Weights for the number of births per UPP boundary are used in all regressions. Results are presented for three control groups: Column '[1]' considers as controls mothers ending their pregnancy up to 8 months before pacification dates, column '[2]' uses mothers ending their pregnancy 6 months before pacification dates and column '[3]' uses mothers ending their pregnancies 3 months before pacification. Mothers in the treatment group are unaltered in all regressions. The variables 'First trimester', 'Second Trimester' and 'Third Trimester' represent dummy variables equal to one for mothers experiencing pacification during their first, second and third trimester of gestation, according to their conception dates and dates of birth. Standard errors are clustered by month of conception and shown in parentheses.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

long run, it seems reasonable to expect that the impacts of reducing crime on birth weights continue in the long-run - nevertheless the results from Table 2.4 do not support this conclusion.

Table 2.4: Linear probability estimates of the pacification of favelas on low birth weight (clustered stadandar errors at conception month)

	Low birth weight (< 2,500 grams) Inside UPP boundaries			
	[1]	[2]	[3]	[4]
One trimester before pacification	-	-	-	.004
	-	-	-	(.005)
First trimester	-.013***	-.015***	-.010**	-.008*
	(.004)	(0.003)	(0.004)	(0.004)
Second trimester	-.006	-.009	-.007	-.003
	(.006)	(.007)	(.007)	(.006)
Third trimester	-.011	-.013*	-.013	-.005
	(.007)	(.007)	(.008)	(.005)
One trimester after pacification	-	-	-	.001
	-	-	-	(.007)
Fixed effects:				
Hospitals of birth	yes	yes	yes	yes
Month of birth	yes	yes	yes	yes
Pacification dates	yes	yes	yes	yes
Street of residence	yes	yes	yes	yes
# of pregnancies	18,726	16,527	13,325	24,241
R^2	.05	.06	.06	.05

Notes: Estimations in Table 2.4 use pacification borders of 31 UPP areas shown in Figure 2.2. These results are conditioned to children that were born and premature deliveries with less than 37 weeks of gestation are excluded. Regressions weight for the number of births per UPP boundary. Column '[1]' uses as controls mothers giving birth up to 8 months before pacification dates, column '[2]' 6 months before pacification dates and column '[3]' 3 months before pacification. As in previous regressions, the treatment group are unaltered in all regressions. Standard errors are clustered by month of conception and shown in parentheses.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

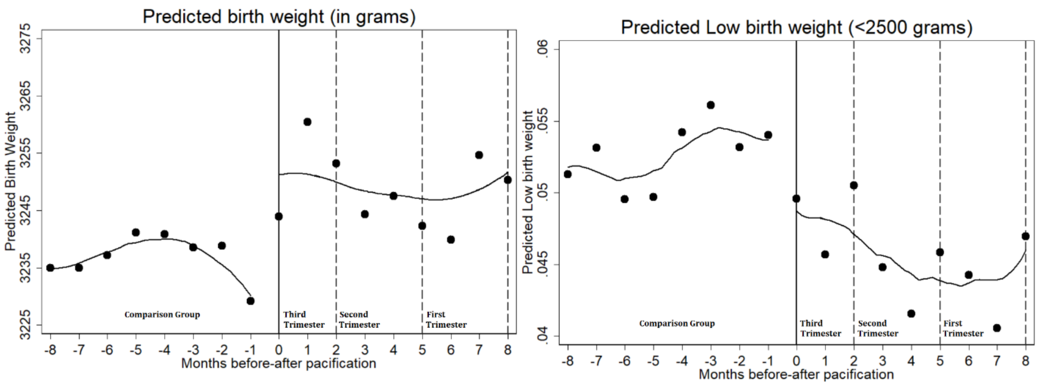
*** Significance level at $\rho < .01$.

Results from Table 2.4 suggests that experiencing pacification also reduces the probability of low birth weight (< 2,500 grams). Pregnancies living in UPP boundaries exposed to pacification since first trimester are 0.8-1.5 percent less likely to give birth to babies with less than 2,500 grams. Differently from the results for birth weight, the pacification effects are exclusive to first trimester, but similarly to Table 2.3, these impacts remain unaltered using alternative control groups. The results in column [4] including mothers not-exposed to pacification during the gestation strengthens the evidence that intrauterine exposure to less crime through pacification of favelas in the main channel of decreases in low birth weight inside UPP boundaries.

Figure 2.7 illustrates the results from Tables 2.3 and 2.4 plotting the average predicted birth weight (A) and low birth weight (B) before and after pacification. Both figures show that women living inside UPP boundaries have lower predicted birth weight and higher predicted low birth weight before pacification. In figure A, for example, the average birth weight is around 3,235 grams before pacification, but pregnancies ending 1-2 months after pacification - which I assign as 'Third trimester' - increase it to around 3,255 grams, and increase again 6-8 months ('First Trimester'). The reduction in the incidence of low birth weight is even clearer checking figure B. After 3 months of pacification (i.e. women experiencing pacification in the first trimester of gestation), the incidence of low birth weight reduces from 5.5 percent to less than 4.5 percent[5].

But why the reduction in crime caused by pacification would improve birth outcomes? Presumably there are three distinct, but complementary, arguments to answer this question: one is psychosocial, another is structural and a third is behavioral. Regarding the first, the link is that pacification reduces stress and anxiety experienced by pregnant women living inside UPP areas by reducing crime and violence. Massive evidence suggests that both factors impair fetal growth, leads to premature delivery by releasing catecholamines, stimulates placental hypo perfusion, and restricts the oxygen and nutrients taken by the fetus (Masi et al., 2007; Kramer, 1987; Alexlrod and Reisine, 1984; Rondó et al., 2003; Omer, 1986; Copper et al., 1996). Previous empirical work in economics usually justifies the ef-

[5]To formally check whether mothers are able to sort themselves to receive the treatment, one uses the McCrary test (McCrary, 2008) to observe jumps in the density of the forcing variable around the pacification dates. The test demonstrates that women do not manipulate to start their pregnancy around the pacification dates. This result indicates that the strategy to include as a treatment only women that started pregnancy before the pacification dates but ending after police arrived is a reasonable strategy to minimize biases arising from sorting around the threshold.



(a) Predicted birth weight (b) Predicted Low birth weight

Figure 2.7: Predicted birth weight (A) and Low birth weight (B)

Notes: Figure 2.6 shows predicted values for birth weight and low birth weight for pregnant women living inside UPP boundaries before and after pacification. These regressions use local linear estimations with bandwidths equal to two months and normal kernels. These predictions are based on Eq. (2.2) using birth weights and a linear probablity model for low birth weight as outcomes. The map of UPP boundaries is in Figure 2.2 and pacification dates can be found in Table 6.1 in the appendix.

fects of intrauterine exposure to violence, terrorist attacks or war, based on this argument (Camacho, 2008; Torche, 2011; Wadhwa et al., 1993; are some examples). Therefore, a plausible channel to interpret our results is that the sharp decrease in crime levels after pacification reduces maternal stress and anxiety in UPP areas that reflects in better birth outcomes, especially during the first and third trimester of pregnancy.

Lower crime may also reflect on longer gestational length. Messer at al., (2006) found that heterogeneities in crime exposure during pregnancy leads to differences in preterm birth rates among racial groups, and other studies argue that feeling unsafe during pregnancy increases the risk of preterm birth (Dole et al., 2003; Glynn et al., 2001). Under these circumstances, reductions in the incidence of preterm births should be considered as a mechanism which less crime affects birth weight of women living inside UPP boundaries after pacification.

Another explanation is structural. Putting simply, safer neighbors can provide better services for pregnant women and doing so improve birth outcomes. If one expects that pacification expands the provision of health services, because dwellers can search for prenatal care whenever necessary, or because it is safer for professionals to work during different working shifts, prenatal care in favelas may improve due to pacification. For instance, it is not clear which of these channels play a major role in the evidence presented so far. Proxies for maternal stress and anxiety are simply non-existent in our data set. And as we excluded premature pregnancies from our estimations in Tables 2.3 and 2.4, one cannot equally infer whether our results reflect changes in gestational length.

To provide evidence on these points, we proceed as follows. I generate dummies for premature (< 37 weeks) and very premature births (< 32 weeks) to estimate the effects of pacification on the probability of prematurity using linear probablity models of Eq. 2.2. If violent neighborhoods predispose women to have premature births, as the literature suggests, estimations instrumenting reductions in violence using the pacification of favelas may unveil reductions in preterm births. We also include dummies for 'having from 0 to 4 prenatal visits' and 'delivery in a hospital' to check if women had more access to health care after pacification. In the case there is a positive shock on prenatal services in pacified favelas, it must be followed by a decrease in the incidence of pregnancies with few prenatal visits and by an increase in deliveries in a hospital. Thus, we re-estimate Eq. (2.2) substituting birth outcomes by these variables and present the results in Table 2.5.

Table 2.5 implicates that experiencing pacification in the third trimester of pregnancy reduces the probability of having preterm births relative to pregnancies ending just before pacification. Still according to Table 2.5, being exposed to pacification during the third trimester reduces premature and very premature births by 7.3 and 2.6 percent, respectively. Additionally, due to the well-known association between prematurity and birth weight (Kramer, 1987), these regressions for premature births also provide a 'validation test' for the results presented in Table 2.3. Results from estimations using alternative control groups (either six or three months) led to equivalent conclusions and were suppressed.

Table 2.5 shows a reduction of at least 1.9 percent point in the share of

Table 2.5: The effects of pacification on prematurity and prenatal visits (clustered std. errors)

	Prematurity				Prenatal visits		Delivery in	
	Less than 37 weeks		Less than 32 weeks		(< 4 visits)		a hospital	
	[1]	[2]	[1]	[2]	[1]	[2]	[1]	[2]
One trimester bef. pacification	-	-.018	-	-.008	-	-.008	-	-.001
	-	(.022)	-	(.006)	-	(.009)	-	(.001)
First trimester	-.008	-0.015	-.009	-0.01	-.029***	-.027***	.001	.001
	(.054)	(.049)	(.014)	(.012)	(.004)	(.005)	.000	(.001)
Second trimester	-.016	-0.023	-.009	-0.01	-0.01	-0.01	.000	.000
	(.045)	(.045)	(.009)	(.009)	(.006)	(.007)	.000	(.001)
Third trimester	-.073*	-.074**	-.026***	-.026***	-.019**	-.019*	.000	.000
	(.035)	(.033)	(.005)	(.006)	(.008)	(.009)	(.000)	(.001)
One trimester aft. pacification	-	-.039	-	-.009	-	-.040***	-	.001*
	-	(.074)	-	(.022)	-	(.007)	-	(.000)
# of pregnancies	20,842	27,022	20,842	27,022	18,347	23,759	18,726	24,241
R^2	0.27	0.24	0.28	0.23	0.28	0.26	0.71	0.67

Notes: All estimations include fixed-effects for hospital of birth, month of birth, pacification dates and streets. The UPP boundaries used to identify women in the treatment group are shown in Figure 2.2, and individual boundaries are shown in the Supplementary Materials. The regressions weight by the number of births in each UPP boundary. Differently from previous estimations, 'Prematurity' includes all pregnancies, but estimations for the number of prenatal visits constraint the sample for nonpremature births (those having at least 37 weeks of gestation) to keep congruence with previous estimations. Standard errors clustered by conception dates and are presented in parentheses.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

pregnant women having up to four prenatal visits after pacification. On the other side, one is not able to detect any significant change in hospital deliveries. If in one hand these results must reflect increases in the provision of health services when the government re-controls favelas, on the other hand pacification might also intensify the capacity searching for care regardless time and location. Possibly, increase in health services explain why the effects are also significant for pregnancies starting after pacification for estimations considering 'Prenatal visits' and 'Deliveries in a hospital'. Overall, because more prenatal visits are associated with heavier babies (Jewell and Triunfo, 2006; Guilkey et al., 1989; Wheby et al., 2009), it seems fair to believe that our results are partially driven by better access of prenatal services after pacification.

A final explanation why pacifications may affect birth outcomes is behavioral. It is likely that high levels of violence stimulate bad habits if stressed and anxious women are more likely to smoke cigarettes or drink alcohol/caffeine (McAnarney and Stevens-Simons, 1990). Crime can also constraint nutritional behavior because it limits shops and supermarkets to open to avoid thievery and robbery. Unfortunately, our data does not provide any opportunity to test these additional channels in more detail.

2.5 Difference-in-difference estimates

One could be concerned with potential placement effects. To try to account for that, I estimate pacification effects by comparing changes in the trends in birth outcomes from mothers living in opposite sides of UPP boundaries before and after pacification dates. Here, what is 'opposite side' requires additional explanation because the regressions are likely to provide uninformative estimates if unnecessarily remote pregnancies are used as a control group. To tackle this problem, I ponder two criteria to assign controls: naturally, they should live outside UPP borders but in a neighborhood where there is an UPP in its official limits . A second criterion considers women living in neighborhoods without UPPs by sharing boundaries. The first criteria is useful for UPP boundaries that are within large neighborhoods (as UPP Dona Marta in Figure 2.2) while the second criteria encompasses UPPs that their boundaries overlays the neighborhoods (as UPP Complexo do Alemão). A list of UPPs and their control neighborhoods is in Table 6.1 in the appendix.

Considering these cases, I estimate the following equation;

$$\begin{aligned} BO_{isf} = \alpha + \beta X_i + t + \rho_{hospital} + \rho_{MOB} + \rho_{streets} + \beta_1 UPP_f + \beta_2 P_{if} + \\ + \gamma_1 (UPP_f \times P_{if(1)}) + \gamma_2 (UPP_f \times P_{if(2)}) + \gamma_3 (UPP_f \times P_{if(3)}) + \varepsilon_{isf} \end{aligned} \tag{2.3}$$

where a difference between Eq. (2.3) and Eq. (2.2) is the inclusion of UPP_f which equals one for mothers living in UPP boundaries and zero for those in the proximities. The second difference is the inclusion of interactions between UPP_f and $P_{if(1)}$, $P_{if(2)}$ and $P_{if(3)}$ to identify pacification effects per trimester of gestation. For example, $(UPP_f \times P_{if(1)})$ represents changes in the trends for mothers experiencing pacification during from the first trimester of gestation relative to changes in the trends for mothers outside pacification borders. And a final difference is the absence of street fixed effects. It is because they would be absorbed by the UPP_f impacts. The remaining variables are kept the same. Table 2.7 presents the results from Eq. (2.3).

Results in Table 2.7 portrait analogous conclusions to previous estimates; i.e. intrauterine exposure to lower crime levels affects positively birth outcomes. In more exact terms, the coefficient "First trimester × UPP" in Panel A shows that pregnancies experiencing pacification during first trimester increase their birth weight by 26,32 grams, which mimics the estimation of 30 grams in Table 2.3. When pregnancies exposed to pacification during third trimester also present from 27 to 29 grams change in birth weight, that is significant, but smaller compared to the 55 grams in Table 2.3.

Differently from the previous evidence, an additional concern to interpret the results from difference-in-difference in Table 2.7 is crime displacement. Once the pacifications influence the cost of practicing crime inside UPP borders in a certain point of time, bandits can observe such changes in costs and adapt their behaviour accordingly. This reasoning implicates that crime could have gone to areas around the pacification borders which would influence the level of violence faced by mothers living nearby increasing their exposure to violence during pregnancy. It means that the estimates in Table 2.6 should be seen carefully because if crime displacement occurs from pacified areas to neighbour areas used as controls, the coefficients would be upward biased.

For estimates using low birth weight and prematurity, the conclusions remain aligned with previous findings; low birth weight decreases 1.3 percent for exposure during first trimester, prematurity decreases 7,3 percent and very prematurity decreases 2.6 percent when pacification occurs during third trimester. Therefore, all conclusions from section 2.4 remain unaltered when one compares the trends of pregnancies living inside and close to pacification boundaries.

2.6 Robustness checks

An implication for the results shown so far is that intrauterine exposure to less crime due to pacification of favelas increases birth weight and reduces low birth weight of women living inside UPP boundaries. There is also evidence that lower incidence of preterm births and access to prenatal care are

Table 2.6: Difference-in-difference estimations of pacification effects on birth outcomes (clustered standard erros at conception month)

Panel A:- Birth weight and low birth weight	Birth weight (in grams)		Low birth weight (< 2,500 grams)	
	[1]	[2]	[1]	[2]
One trimester before pacification × UPP		.519		.004
		(12.45)		(.007)
First trimester × UPP	26.32**	23.64*	-.013**	-.010*
	(12.31)	(11.51)	(.005)	(.005)
Second trimester × UPP	18.69	17.25	-.010*	-.007
	(14.13)	(12.15)	(.005)	(.005)
Third trimester × UPP	29.22**	27.52**	-.009*	-.005
	(12.82)	(11.00)	(.005)	(.004)
Post-pacification	-8.60	-5.28	.004	.001
	(8.54)	(8.20)	(.003)	(.003)
UPP	-33.83***	-30.83***	.011**	.009**
	(9.26)	(8.47)	(.005)	(.004)
One trimester after pacification × UPP		22.90		-.002
		(14.51)		(.008)
Panel B: Prematurity	Premature (< 37 weeks)		Very premature (< 32 weeks)	
	[1]	[2]	[1]	[2]
One trimester before pacification × UPP		-.013		-.006
		(.016)		(.004)
First trimester × UPP	-.021	-.036	-.010	-.013
	(.059)	(.057)	(.016)	(.014)
Second trimester × UPP	-.031	-.046	-.013	-.015
	(.043)	(.050)	(.010)	(.011)
Third trimester × UPP	-.072**	-.088**	-.026***	-.029***
	(.031)	(.041)	(.006)	(.007)
Post-pacification	.015	.023	.006	.007
	(.011)	(.018)	(.004)	(.004)
UPP	.014	.026	.005	.007
	(.011)	(.024)	(.004)	(.006)
One trimester after pacification × UPP		-.031		-.005
		(.073)		(.020)

Notes: Table 2.6 demonstrates difference-in-difference estimations of pacification effects on birth outcomes. UPP is a dummy variable equal one for mothers living in UPP boundaries and zero otherwise. The variables 'First', 'Second', and 'Third' Trimester represent dummies for the time pacification occur during pregnancy. Regressions weight by the number of births in each neighbourhood, the include the same set of controls of previous estimations. Standard errors clustered by conception month and presented in parentheses.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

two possible mechanisms which the coefficients operate. However, although the identification strategy minimizes some concerns comparing mothers living in the same street using different compositions of controls and including lags and forwards, there are still concerns whether it mirrors errors in conception dates, if coinciding shocks in unobservables are leading us to mistakenly attribute to pacification the effects of other factors, or if mothers are selecting themselves to receive the treatment. This section provides indication that each of these concerns are not influencing our major conclusions.

The assumption that pregnancies starting before but ending 'quasi-randomly' around the pacification dates may originate bias if conception dates are measured with error. Moreover, if such error affects the probability of assigning women to treatment and control groups correctly, it should be a threat to our identification strategy. To offer an alternative measure, I propose using the number of months between dates of birth and official pacification dates. Both seem to be arguably more reliable measures than length of pregnancy, especially for women in favelas with limited access to hospitals and prenatal care. Now, instead of dummies for trimesters of pregnancy, the model replaces P_{if} by the number of months mothers experienced pacification until birth (i.e. the diffenrece between the date of birth and the pacification dates) and zero for women delivering before pacification dates. The estimating results using such alternative measure of exposure are shown in Table 2.7 Panel A.

Furthermore, to test for biases generated by unobservables at the threshold I propose a placebo test. I suppose that mothers living close but outside UPP boundaries are likely to experience shocks on unobservables similarl to those experienced by mothers living inside UPP areas. For that, I perform the placebo test estimating the impacts of reductions in crime at pacification dates for mothers living outside UPP boundaries. If this placebo test shows that mothers living outside UPP boundaries present a discontinuity in their birth outcomes at pacification dates, there are good reasons to believe that our interpretation of pacification effects is incorrect. Table 2.7 demonstrates the results for the placebo test in Panel B.

Both panels in Table 2.7 reinforce the credibility of our previous conclu-

Table 2.7: Alternative measure of intrauterine exposure to violence and placebo test (clustered standard erros at conception month)

	PanelA: # of months		**Panel B: Placebo test**			
	Inside UPP boundaries		Outside UPP boundaries			
	Birth Weight (in grams)	Low Birth Weight (< 2,500 grams)	Birth Weight (in grams)		Low birth weight (< 2,500 grams)	
	[1]	[2]	[1]	[2]	[1]	[2]
Months exposed to pacification	4.011***	- .002**	-	-	-	-
	(1.142)	(0.001)	-	-	-	-
One trimester bef. pacification	-	-	-	8.098	-	.004
	-	-	-	(15.79)	-	(.005)
First trimester	-	-	-7.63	-7.04	.002	.004
	-	-	(13.74)	(14.95)	(.004)	(.004)
Second trimester	-	-	8.78	7.29	-.004	-.003
	-	-	(8.63)	(11.21)	(.005)	(.005)
Third trimester	-	-	-10.07	-2.99	.010	.009
	-	-	(8.49)	(10.33)	(.008)	(.007)
One trimester aft. pacification	-	-	-	14.210	-	- .006
	-	-	-	(21.88)	-	(.006)
Includes controls?	yes	yes	yes	yes	yes	yes
All fixed effects?	yes	yes	yes	yes	yes	yes
# of pregnancies	17,668	17,668	21,085	27,280	21,085	27,280
R-squared	0.24	0.22	0.30	0.31	0.29	0.29

Notes: All estimation for 'Inside UPP's boundaries' use the same UPP boundaries from the standard estimations in Eq. (2.2). 'Outside UPP's boundaries' estimate pacification effects on pregnant women living in the proximities but in the opposite side of the UPP boundaries. The areas considered in this group can be checked in the Table 6.1 in the appendix, as a general rule control areas encompass neighbourhoods adjacent to pacification borders. The dependent variable 'Months exposed to pacification' measures the number of months each women experience pacification whil pregnant based on the difference between the date of birth and pacification dates. Pregnancies ending before pacification are assigned in the control group. The remaining variables have the same definitions as before. Weights for the number of births in each area are used in all estimatation. Standard errors are clustered by conception dates and presented in parentheses.
* Significance level at $\rho < .10$
** Significance level at $\rho < .05$
*** Significance level at $\rho < .01$.

sions. Table 2.7 shows that using months of exposure depicts an equivalent picture for pacification effects in Tables 2.3 and 2.4 each additional month experiencing pacification increases birth weight by 4.011 grams and reduces 0.2 percent low birth weight. Considering a constant impact across months, these results involve that being exposed to pacification since first trimester increases birth weights from 24.06 to 32.08 grams and reduces the probability of low birth weight from 1.2 to 1.6 percent. Therefore, similar to the conclusions from Tables 2.3 and 2.4 In Panel B, the placebo test confirms that there are no pacification effects for mothers living outside UPP boundaries.

What can be seen as an additional test, we plot several graphs showing the monthly characteristics of mothers before and after pacification dates. These graphs check whether mothers' composition changes through pacification dates and there is no indication that is the indeed case. Overall, based on our estimations using alternative measures of exposure, the placebo test and graphical analysis one could therefore be more confident that our results operate through intrauterine exposure to pacification.

2.7 Conclusions

This paper estimates the effects of intrauterine exposure to violence on birth outcomes exploring the pacification of slums in Rio de Janeiro. These pacifications aimed to retake control of favelas dominated by armed gangs and trafficking introducing permanent pacifying police units (UPPs) patrolling and maintaining security inside 'pacification boundaries' (See Figure II). Using microdata of crime records from Rio de Janeiro, we show that these pacifications promoted abrupt decreases in crime rates - especially in violent crimes - inside pacification boundaries. To name a few, apprehensions of drugs and guns, along with flagging, increased by at least 4 folds, homicides decreased approximately 55 percent, and three times more people report crimes to police compared to pre-pacification periods. We argue that comparing pregnancies starting before but ending around pacification dates provides a 'quasi-randomly' exogenous variation on crime in UPP boundaries that is useful to estimate the effects of reducing violence on birth outcomes.

Unlikely previous work, our study focuses on the effects of a discontinuous decrease on crime caused by pacification is a geographically delineated urban area. Such characteristic contrasts with the evidence using peaks of homicides and terrorist attacks (Mansour and Rees, 2012; Koppensteiner and Manacorda, 2016; Camacho, 2008), or armed conflicts (Akresh, et al., 2012; Bundervoet et al., 2009; Minoi and Shemyakine, 2014) to instrument intrauterine exposure to violence. Another important distinction is that we identify violence effects comparing pregnancies living in the same street, inside pacification boundaries, before and after the police arrived. Including street fixed-effects enables to circumvent time-invariant unobservable factors at street level associated with birth outcomes.

The evidence indicates that reducing violence in favelas affects positively birth outcomes of those exposed in-utero. Per se, intrauterine exposure during the first trimester increases birth weight in around 30 grams, and by 50 grams for mothers exposed during third trimester. Our findings also show that mothers exposed during the first trimester reduce by 0.8-1.5 percent the chances of giving birth to babies with less than 2,500 grams. Additional estimations comparing changes in the trends in birth outcomes of mothers living in opposite sides of UPP boundaries before and after pacification corroborate these conclusions.

To test if these estimations truly reflect intrauterine exposure to pacification, I perform some robustness checks. Regression including lags and forwards to account pregnancies starting one trimester after or ending one trimester before pacification dates does not show any significant change in birth outcomes. Another robustness check verifies if measurement error in conception dates compromises our capacity to identify pregnancies experiencing pacification. Similarly, re-estimations using the number of gestational months exposed to pacification (i.e. dates of birth minus pacification dates) does not alter the interpretation of our findings. Finally, we perform a placebo test estimating pacification effects for mothers living in the other side of pacification boundaries. The idea underlying the placebo test is that if we are estimating exposure to pacification, pregnancies residing outside pacification boundaries should not demonstrate similar changes in birth outcomes than mothers inside UPP boundaries. Confirming our findings, the placebo test does not detect any effect of pacification on mothers living outside pacification boundaries.

There are three possible channels to interpret our findings. A first channel is that the abrupt reduction in crime caused by pacifications reduces stress and anxiety experienced by women living inside UPP areas. Considerable amount of evidence indicates that intrauterine exposure to violence leads to preterm births, weakens fetal growth, and reduces the nutrients absorbed by the fetus (Kramer, 1987 presents an ample literature review; Rondó et al., 2003; Omer, 1986; Copper et al., 1996 are additional references). Reducing violence in favelas may also improve the provision of health care services, including prenatal care, access to hospitals and nutrition. Another explanation is behavioral. It is possible that less violence may improve nutritional habits because less stressed mothers would be less likely to drink alcohol, consume caffeine or smoke cigarettes which are known to be harmful for birth outcomes (Kramer, 1987).

Given to data limitations, this paper provides evidence on reduction of prematurity and better access to prenatal care. Additional estimations show that pacifications decrease preterm births (< 37 weeks) by 7.3 percent and very preterm births (< 32 weeks) by 2.6 percent for pregnancies exposed during third trimester to pacification. Due to the strong association between length of pregnancy and birth weights, both results can be seen as a validation test for the results considering birth weight. After pacification, the probability of having no more than four prenatal visits also decreases in at least 1.5 percent, but it was not possible to detect any change in the probability of having birth in a hospital or clinic. One interpretation for these results is better accessibility to health services in pacified favelas.

These findings have some crucial policy implications. The robust impact of pacification on birth outcomes broaden the perspective on the benefits of decreasing violence in poor communities, not only to prevent potential causalities or insecurity among residents but promoting its value on fighting against inequalities at birth. For example, taken the fourth richest neighborhoods in Rio de Janeiro together (Leblon, Jardim Botanico, Ipanema and Lagoa), the average birth weight is 63.2 grams higher, and 2.8 percent lower incidence of low birth weight than pacified favelas. According to our estimates, the introduction of UPPs lead to 50 percent decrease in these gaps in birth outcomes. Similarly, the gap in few prenatal visits of 10,63 percent would decrease up to 25 percent after pacifications. Although these measures do not exclusively reflect pacification effects but also other mechanisms, it still demonstrates a more encompassing impact of reducing crime in poor areas.

Chapter 3

Information disclosure, informed mothers and delivering babiers

A pregnant woman relies on the expertise of physicians to recommend the procedure she would choose if she had the same medical knowledge regarding her clinical condition. However, a wide medical knowledge gap between the physician and the patient has long been reported in the literature (Arrow, 1963). Several public policies have aimed to reduce this gap, helping patients to distinguish the quality of different health services by disclosing information or elaborating ranks of quality. For instance, if a patient becomes aware that a physician is endorsing an unjustified procedure, she may not consent, penalize him by spreading a bad reputation, or choose a different professional (Rochaix, 1989; Dranove, 1988). Another aim of these policies is that patients with complicated conditions will have discretionary power to search for the most suitable health service, or refuse to start a treatment without all necessary information. In such cases, the merits of additional information can hardly be exaggerated.

However, physicians also respond to information disclosure. If hospitals with doubtful quality suspect that disclosing information may penalize their reputation and reduce their market-share, they have incentives to artificially "game" with the information before releasing it to the public. For example, physicians can avoid treating complicated pregnancies, or perform unrecommended treatments to complicated patients, in order to artificially increase their quality and be pooled with high-quality physicians (Dranove et al., 2003). Thus, while mothers with pregnancy complications can explore the information about physicians to select the most suitable treatment for themselves, physicians may refuse the most complicated patients because they drive their performance down and provide negative signals to future patients. Overall, it is challenging to separate the effect of patients selecting physicians from the physicians' anticipatory response to information disclosure.

This paper explores a peculiar change in the Brazilian legislation as a quasi-experiment to identify the responses of physicians from the access to information by patients. The legislation was enacted on 6 January 2015, and made it compulsory for private hospitals to disclose information about delivery rates (C-sections and normal births) and antenatal care (partographs and "expectant cards"), at the physician level. The key element is that the legislation gave 180 days to private hospitals to adapt themselves before any information was disclosed to the public. Thus, physicians had the privilege of knowing what information must be disclosed 180 days in advance: this gap provides a plausible opportunity to estimate the anticipatory responses of physicians to public information disclosure. Patients themselves could only access information on the 181^{st} day after the change in the legislation (i.e. on 7 July 2015): this fact is used to estimate the influence of patients accessing information. Few previous studies have separated the response of physicians from the physician-patient relation, and even fewer have estimated the impacts of information disclosure on delivery methods. In this paper, I am able to test these hypotheses.

The simplicity of the information disclosed in the legislation is another advantage of this research, compared with previous studies. Previous literature argues that the impact of information depends on the capacity of patients to understand it (Epsein, 2009), if the information is already known (Dranove et al., 2003), or relevant for their purposes (Jin and Sorentin, 2006). In the Brazilian legislation, the information disclosed is the percentage of C-sections and normal births performed by physicians in the previous twelve months. Therefore, no top-down ranking of quality or judgement about the ideal percentage of C-sections are disclosed. And for cases where the expectant mother insists on having a C-section, unjustified given her clinical condition, a formal contract "clearly" stating the risks of unnecessary C-sections must be signed by the physician and the patient. This formality guarantees that patients are informed by physicians about the risks of unnecessary procedures. The intention of the legislation is that better-informed mothers are more likely to make better choices with their physician about the most suitable delivery method, and that physicians treating informed mothers may recommend fewer unnecessary C-sections.

Using detailed data from the Brazilian National Health Service for 2.5 million births, from 1 January 2014 to 31 December 2015, the identification strategy compares changes in the trends in delivery rates in private hospitals before and after the legislation change (on 6 January 2015) with changes in the trends in delivery rates of public hospitals during the same period. As patients only had access to information 181 days later, the model also compares alterations in the trends of delivery rates in private and public hospitals before and after patients had access to information (on 7 July 2015). So, difference-in-difference estimates identify the anticipatory response of physician by measuring changes in trends during the 'adaptation period' of 180 days, while the triple-difference coefficient captures the impact of patients accessing information. By comparing these coefficients, it is possible to identify whether physicians anticipated to information disclosure by recommending fewer C-sections, and whether patients impaired or fostered the change in C-section rates by accessing information.

Two assumptions motivate this analysis. The first assumes that patients do not react to information that they do not have (Dranove et al, 2003, make a similar assumption). Specifically, if pregnant women respond to the legislation change by altering their delivery choices, even without access to information, the estimations of anticipatory effects would be invalid. The second assumption considers that the change in the legislation is uncorrelated with unobserved trends in pregnancies when controls for the characteristics of hospitals and patients are included in the model. This means that legislation did not affect the composition of the expectant mothers hospitalized before and after the legislation, which seems reasonable given that pregnancies last on average for nine months. Further checks on the plausibility of these assumptions are discussed in section 3.6.

Results from triple-difference estimations indicate that information disclosure reduced scheduled C-section rates by 4.8 percent in private hospitals when compared with public hospitals. Two-thirds of this reduction reflects physicians' anticipation effects that occurred after the change in legislation, but before disclosing information to patients. This can be seen by C-section rates in private hospitals decreasing by 3.4 percent during the 180 days following the change in legislation, while the patients' access to information provoked an incremental decrease of 1.4 percent. Estimates for emergency C-sections do not indicate changes in the trends between public and private hospitals for any period. Given that emergency C-sections generally arise in response to safety measures or because of unpreventable complications, it seems reasonable to believe that the impact of legislative change on scheduled C-sections occurs mostly in unnecessary procedures.

Additional evidence comes from regressions per quartile of C-sections and hospital ownership type. Regarding the first, the results indicate that private hospitals with limited structure to accommodate normal deliveries present insignificant reductions in the C-section rates during the adaptation period. This conclusion arises since private hospitals in higher quartiles presenting insignificant changes in the trends of C-section rates also have fewer rooms for prenatal, normal birth, before birth and are twice less likely to have "Medical Ethic Committees" and committees to "Review Medical Reports" than those in lower quartiles. Because private hospitals in upper quartiles have at least 92 percent of C-sections during the year preceding the legislative change, another possible constrained to reduce C-sections is the absence of know-how by physicians to perform normal births.

Regressions per ownership type suggest that non-profit and smaller for-profit hospitals have stronger reductions in C-section rates during the adaptation period compared to bigger for-profit hospitals[1] . As non-profit status relates to quality signaling (Arrow, 1963; Glaeser and Shleifer, 2001; Jones et al., 2017), information disclosure may force non-profit hospitals to practice further reductions in C-section rates expecting that their patients become more quality elastic when accessing information, or to maintain the reputation of non-profit oriented by signaling lower C-section rates than profit-oriented hospitals. Additionally, the stronger competition for patients may explain why C-sections decreased more in small than in big for-profit hospitals because these hospitals may compete for better-informed patients reducing current C-section rates. Overall, these constraints in structure, signalling and in the market, suggest limitations in reducing C-sections rates in private hospitals after legislative change.

These conclusions are consistent using two alternative empirical approaches and after proposing two robustness checks. The first group estimates monthly diff-in-diff regressions to test whether there is any significant change in the trends between private and public hospitals before legislative change. The second approach tests the difference in the coefficients from a diff-in-diff model during the adaptation period and information disclosure. The conclusion that two-thirds in the reduction of C-section corresponds to physicians anticipating to information disclosure remain unaltered after using both approaches.

Finally, the first robustness check explores physician-mothers as a placebo group, to whom the information disclosed was likely to be irrelevant or already known. This idea has already been explored by the previous literature, but in another context (Johnson and Rehavi, 2016). The placebo test indicates that when mothers already had medical knowledge, the effects of information disclosure on C-section rates are insignificant. A second test explores breech deliveries in a falsification test. I argue that breech deliveries are unpreventable by physicians and patients, and at the same time are strongly associated with having a C-section (as used by Jensen and Wust,

[1]The identification of small for-profit hospitals comes from hospitals participating in the program "Simples". In order to be enrolled in this program, private hospitals must declare annual revenues lower than R$ 3.6 million Brazilian Reais (1.5 million US dollars). The advantage of this scheme relies on a simplified bureaucracy to pay taxes.

2015). If the model is correct, the incidence of breech deliveries should not change in response to changes in the legislation or patients' access to information. Similar to the placebo test, the falsification test suggests insignificant changes in the trends of breech deliveries between private and public hospitals.

The paper proceeds as follows: Section 3.1 briefly discusses the literature of information disclosure and summarizes its previous findings; Section 3.2 describes the legislation, Section 3.3 explains the triple-difference framework; and Section 3.4 discusses the National Health Service data sources. Section 3.5 presents the results, Section 3.6 includes the robustness checks and Section 3.7 concludes.

3.1 Information disclosure and procedure choice

A rich literature in health economics studies the implications of public disclosure of information on the interaction between physicians and patients. In fact, there is consistent evidence that disclosing previously unknown information about hospitals' quality allows patients to shift away from low-quality hospitals and to make better choices among physicians and hospitals of heterogeneous quality. In general, this literature can be divided into three main groups.

The first category of papers investigates changes in the market share of hospitals after mandatory disclosure of information or publication of quality rankings. In these cases, the identification is usually achieved by comparing the difference in trends in the outcomes before and after disclosing information, relative to the areas where it was not mandatory. One conclusion from these studies is that hospitals anticipate the loss of market share caused by public information disclosure by artificially improving their quality through selecting healthier patients (Dafny and Dranove, 2008; Dranove and Sfekas, 2008; Dranove et al., 2003). Another important implication is that low-ranked hospitals generally lose market share to a greater extent than high-ranked hospitals.

Perhaps the major practical difficulty in this literature is to identify whether the gains in market share reflect hospitals "gaming" with information, or whether patients choose to avoid low-ranked hospitals. If high-ranked hospitals attract more complicated patients, while healthier patients avoid being treated by low-quality hospitals, then the gains in information disclosure can be attributed to selection bias of patients towards hospitals. In short, it is not possible to be certain whether information disclosure or selection is responsible for changes in the patient-physician relation. Secondly, the evidence of information disclosure is hardly generalizable. The estimations commonly compare intra-state variation in the US (specially, New York and Pennsylvania), and the identification is based on the average effects within states where quality rankings are adopted, relative to averages in states without such ranks.

The second group investigates the response of patients to information disclosure. The focus is on how publicized quality rankings affect the patients' choice of hospitals and physicians. The general finding is that patients are more likely to choose high-quality hospitals after the information becomes publicly available (Pope, 2009; Bundorf et al., 2009, Santos et al., 2017; Beaulieu, 2002; Scanlan et al., 2002; Dafny and Dranove, 2005; Jin and Sorensen, 2005; Chernew et al., 2008). Dranove and Sfekas (2009) observed that cardiac surgery patients avoided low-quality hospitals, rather than searching for high-quality ones. The effects of information disclosure seem weaker for critical medical procedures (Epstein, 2009), and for relatively healthy patients than less healthy patients (Varkevisser et al., 2012, Wedig and Tai-Seale, 2002).

Finally, the third group constructs theoretical models to describe patient-physician interactions when physicians recommend treatments with unclear benefits for patient's health - the supply-induced demand argument. The level of inducement by physicians in these models depends on the cost of the treatment, the severity of the illness in question, and the "diagnostic skills" of patients (Dranove, 19988; Rochaix, 1989). Naturally, the average patient has less medical knowledge than the average physician; therefore, these models indicate that patients in critical clinical conditions are more likely to comply with a recommendation, because the severity of the illness increases the marginal utility of treatment (Dranove, 1988). On the other hand, if physicians recommend costly or unnecessary procedures to relatively healthy and informed patients, it is less likely that patients will consent to the treatment recommended. For a fixed level of illness, these models show that providing new information to patients decreases the capacity of physicians to induce demand recommending unnecessary procedures.

A persistent challenge in these last two groups in the literature is to isolate the influence of unobserved confounders associated with the information disclosed. For example, there is evidence indicating that the effects of information depend on the extent to what it is already known by patients, its complexity, and relevancy for the patient's purposes (Dranove and Sfekas, 2009; Cutler et al., 2004; Marshall et al., 2000): all of these factors are generally unobserved by the econometrician. Recent evidence using physicians treating physicians shows that physician-patients have significantly lower probability of having a C-section delivery and better health outcomes than non-physician patients (Johnson and Rehavi, 2016). More importantly, the models of physician-patient interaction explicitly demonstrate that empowering patients with information about physicians, influences physicians' behaviour allowing them to anticipate the negative effects of information disclosure (Scanlon et al., 2002; Dafny and Dranove, 2005; Wedig and Tai-Seale, 2002).

This paper contributes to this literature by exploring a plausible exogenous variation in the physician-patient interaction caused by a change in the Brazilian legislation. The legislation was enacted on 6 January 2015, but had a peculiarity of providing an "adaptation period" of 180 days before private hospitals had to disclose their C-section and normal birth rates to the public. Thus, patients had legal support to access information only after 7 July 2015. In other words, one assumes that during the first 180 days following the legislative change, physicians could anticipate potential losses from publicized information by artificially changing their percentages of C-sections. In turn, I consider that from 6 January to 7 July 2015, patients would not change their behaviour in response to the information they do not have.

There are several advantages in using this change in legislation to identify the effects of information disclosure. Firstly, the information disclosed is particularly simple. It seems reasonable to believe that interpreting percentages of C-sections requires a basic understanding of proportions, and does not require any particular medical knowledge. Secondly, the relevance of the information disclosed. Pregnant women seeking a physician for normal delivery might consider useful to be informed about physicians' percentage of C-sections before making a choice. Moreover, in the case that they do not search for information, the legislation prevents patients from being uninformed. In the situation where a patient insists on undergoing a C-section, contradicting her clinical condition, the physician and the patient must sign a formal contract stating "clearly" the risks of an unnecessary C-section. Thirdly, the simplicity of information disclosed also makes it possible to use physician-patients as a placebo group to test our model with patients who already know the information. And finally, I propose a falsification test, using breech deliveries as a highly predictable variable for C-sections, but which is not encompassed by the legislation. Such tests are further discussed in Section VI-C.

3.2 Legislative change and information disclosure

Brazil is an anecdotal case for studying the 'epidemic of C-sections'. According to the C-section rate advised by WHO (15%), there were 1.250 million unnecessary procedures in Brazil from 2014 to 2015 in our sample. First, there is a great discrepancy between figures for public and private hospitals: 45 percent of deliveries in public hospitals occur by C-section, which is already high, but half the rate in the private sector (88.4 percent, see Table 3.2). And secondly, it seems that C-sections in Brazil are excessively frequent among women from more affluent backgrounds. For example, studies from Potter et al, (2001), Béhague et al., (2002); and Barros et al., (1996) demonstrate that women with higher incomes, more educated, better caredfor, in their first pregnancy and, even more paradoxically, in lower-risk are more likely to have a C-section. Faisal-Cury and Menezes (2006) and Béhague et al., (2002) add to this list that previous birth experiences play an important role influencing current delivery choice.

Table 3.1: Marginal effects of mother's characteristics on delivery choice during the baseline (Clustered standard errors at hospital level)

	Dependent variable: 1 = C-section; 0 = Normal Birth			
	Public Hospitals		Private Hospitals	
Teenage mother	- .029***	(.003)	- .033***	(.012)
Previous normal Birth	- .499***	(.007)	- .383***	(.034)
Number of children	.002	(.002)	.027***	(.005)
Mother with diploma	- .013**	(.005)	- .023	(.019)
Unemployed	-.016	(.024)	.019	(.031)
Housewife	- .029***	(.009)	- .001	(.007)
Mother's age	.014***	(.000)	.003***	(.001)
Twins	.376***	(.013)	.057***	(.012)
Number of previous abortions	- .000	(.003)	.005*	(.002)
First pregnancy	.274***	(.006)	.070***	(.011)
Single mother	- .019**	(.008)	- .003	(.005)
Lives in another city	.039***	(.008)	- .004	(.008)
Black mother	- .029***	(.009)	- .008	(.010)
# of patients	992,873		386,645	
Pseudo R^2	.131		.105	

Notes:Table 3.1 demonstrates marginal effects from probit regressions of mother's characteristics on the probabliity of having a C-section. These regressions weight for the number of births per month in each hospital. A detailed explanation of the dataset is shown in Section 3.4. Regressions are for deliveries occurring from 1 January 2014 until 5 January 2015, one day before the legislative change. The column "Private hospitals" consider hospitals not operating within the National Health System (SUS).

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

To assess which mother's characteristics are associated with the probability of having a C-section, I present in Table 3.1 Probit estimation of C-section delivery and mother's characteristics during the baseline. This exercise demonstrates three major mother's characteristics presenting stronger association with C-section deliveries. In one hand, mothers in their first pregnancy or expecting twins have a strong inclination to perform a C-section. These coefficients seem to play a far more important role determining C-section rates than, for example, mother's age, number of abortions, number of children, or mothers with a diploma. On the other hand, mothers who had previous normal deliveries are less likely to have a C-sections. Although it is hard to reaffirm the list of covariates provided by previous studies, Table 3.1 indicates that the delivery choice seems to depend on unobservable characteristics of mothers correlated with the reasons for choosing a normal delivery in previous pregnancies or with the determinants of starting the first pregnancy.

Dissatisfied with this scenario, on 6 January 2015 the Brazilian government enacted a legislation making it compulsory for hospitals, health insurance plans and physicians in the private sector to disclose information about their performance in birth deliveries in the previous 12 months. Specifically, the legislation demanded that patients should be informed of hospital's and physician's percentages of C-sections and normal deliveries. Patients are in charge of asking this information to hospitals and hospitals have to provide it in up to 15 days. The primary motivation was to reduce the incidence of unnecessary C-sections by guaranteeing that uninformed mothers were given information about their probability of undergoing a C-section.

To track the performance and decisions made by physicians, the legislation also established:

(i) The exhibition of partographs showing all information requested by the World Health Organization[2] (See Figure 6.20 in the appendix);

(ii) The completion of the 'card of expectant mothers', with indicators and observations about the evolution of pregnancy conditions (See Figure 6.20 in the appendix);

(iii) If expectant mothers still opt for an unrecommended C-section, the physician must 'clearly' redact a formal document to the patient, stressing all risks provenient from an unnecessary C-section. This document must be signed by both physician and patient;

(iv) If a request for information remains unanswered for more than 15 days, the government can charge hospitals or physicians up to R$25.000 Brazilian Reais (around $7,000-$9,000 US dollars);

(v) These demands in the legislation start to be implemented 180 days after its publication on 6 January 2015.

Points (i) and (ii) outline the information and how it should be presented to patients. It is important to note that all enforcement is done by patients, pregnant or not, requesting the information directly to hospitals. If by any reason private hospitals refuse or delay the provision of information, the point (iv) in the law is triggered. Perhaps it may be easier for patients to comprehend percentages of C-sections than partographs and indicators that require some medical knowledge. However, points (iii) and (iv) specify that physicians must prove formally and clearly that the risks of unnecessary C-sections are acknowledged by the patient. Therefore, there is a strong predilection in the legislation towards patients receiving the information.

Of great empirical interest is point (v). The time gap between the announcement of the legislation and when patients could legally access the information gave 180 days for private hospitals to change their behavior, anticipating the potential harms that high percentage of C-section would have on their market share. This paper explores this time-gap in the law as a credible source of variation in the interaction between physician and patient, in order to isolate the response of physicians to public disclosure of information.

3.3 Estimation strategy

The change in the legislation imposing private hospitals to reveal the percentages of C-sections per physician offers a plausible exogenous variation in the physician-patient relation to identify the impacts of public information disclosure on delivery choices. A crucial characteristic for identification is that, under this legislation, patients were legally permitted to request information only after an adaption period of 180 days following its approval. Therefore, an analysis exploring the time-gap between when the law was enacted (6 January 2015) and when patients were legally allowed to access information (7 July 2015), provides a credible opportunity to separate the effects of physicians anticipating themselves to public information disclosure from the influence of patients accessing information.

The most straightforward approach in this context is to compare the differences in trends of delivery methods in private and public hospitals before and after the legislation changed. However, as the effects of patients accessing information is of particular interest, I include in the model an additional difference in the trends between private and public hospitals before and after patients had access to information. Thus, in addition to the variation between trends before and after the law changed, the estimations also consider a discontinuity when patients had access to information. This approach configures a triple-differences (DDD) estimation of the effect of disclosing information on the behaviour of physicians and patients.

To represent the triple differences, I use a linear probability model of birth outcomes, adjusted for changes in the observed characteristics of hospitals and patients. The DDD model takes the form:

$$\begin{aligned} BO_{iht} = {} & \alpha + \beta_1 M_{it} + \beta_2 H_{ht} + \lambda_{hospital} + \lambda_{month} + trend + \\ & \gamma_1 PRIV_h + \gamma_2 LAW_t + \gamma_3 DISCLOSURE_t + \gamma_4 (PRIV_h \times LAW_t) + \\ & \gamma_5 (PRIV_h \times LAW_t \times DISCLOSURE_t) + \varepsilon_{iht}, \end{aligned} \tag{3.1}$$

where BO_{iht} represents the birth outcome of interest; LAW_t equals one for post-legislation periods (after 6 January 2015); $PRIV_h$ equals one for private hospitals h, and $PRIV_h \times LAW_t$ interacts both. $DISCLOSURE_t$ is a dummy for periods after 7 July 2015, when patients were able to access information of physicians; $PRIV_h \times LAW_t \times DISCLOSURE_t$ interacts these dummies. M_{it} includes a set of controls for mothers' characteristics: age, years of education, race, marital status (= 1 if single), if she comes from another city, if she is unemployed, if she gave birth to twins, and the number of children. And importantly, M_{it} also controls for conditions in previous deliveries: number of abortions, previous caesarians, and a dummy for women experiencing their first pregnancy. H_{ht} includes controls for hospital characteristics: total number of rooms for surgery, rooms for clinical medicine, and for complex procedures; also, whether the hospital has an obstetric centre, neonatal centre, and the number of after-birth rooms and rooms for prenatal. Finally, all estimations consider hospital fixed-effects ($\lambda_{hospital}$) to account for unobserved time-invariant characteristics of hospitals, such as structural and physician quality (as argued by Chernew et al., 2008); λ_{month} controls for seasonal variations in birth outcomes, trend is a monthly time trend variable and ε_{iht} is an error term.

It is important to note why Eq. (3.1) is not saturated. In a saturated model, there would be interactions of LAW_t and $PRIV_h$ with $DISCLOSURE_t$. However, as long LAW_t equals one for periods after legislation change, i.e. from 6 January 2015 onwards and zero otherwise, and $DISCLOSURE_t$ equals one from 7 July 2015, the interaction $DISCLOSURE_t \times LAW_t$ equals $DISCLOSURE_t$. Similarly, the interaction $DISCLOSURE_t \times PRIV_h$ produces the same vector as the triple difference $PRIV_h \times LAW_t \times DISCLOSURE_t$. These results reflect the fact that our third difference occurs on time dummies. To lessen the concerns regarding the arbitrariness of our model, I also present estimates from an alternative approach considering these difficulties.

An advantage of estimating triple-difference models is to demand weaker assumption compared to difference-in-difference models. The model proposed in Eq. 3.1 accounts for the unobserved trends in C-sections across private and public hospitals and for the trends in C-section rates for private hospitals before and after the legislative change. Once one assumes it as an advantage, triple difference strategies provides a more robust approach than difference-in-differences that would only account for changes before and after the legislation change.

Detailed data allows us to investigate a set of important birth outcomes using this framework. Firstly, I begin observing changes in the probability of having a C-section. A second set of outcomes present coefficients for scheduled and emergency C-sections[3], to examine whether the impacts of information disclosure depend on the form of delivery. If emergency C-sections happen under unforeseeable conditions when information disclosure should be less relevant, it is plausible to believe that the legislative change must have a larger effect on scheduled C-sections, but little or no effect on nonpreventable complications that lead to emergency surgeries. Thirdly, to check for heterogeneities on the effects of information disclosure, I include separate estimations per quartile of C-section during the baseline and per type of private hospital (Non-profit, For Profit and "Simples"). To my knowledge, no previous paper has evaluated the effects of information disclosure on a similar set of outcomes.

The empirical analysis concentrates primarily on two parameters of interest. The 'physician's parameter' or "anticipation effects" γ_4 subtracts the difference in the trends in public hospitals from the difference in trends of private hospitals, but restricts itself to the 180 days following the change in the legislation. Specifically, γ_4 shows whether physicians anticipated to the legislation change in terms of delivery choice. The second parameter of interest, γ_5 captures incremental changes in the trends in birth outcomes between public and private hospitals when patients had access to information about physicians. Throughout the paper, I compare γ_4 and γ_5 to check whether the response of physicians is weaker or stronger according to access to information by patients.

Note that while the coefficient γ_4 identifies the response of physicians to information disclosure, the coefficient γ_5 hardly identifies the exclusive maternal response to legislation change. This is because in the first patients do not have access information and physicians were aware that information

[3]I consider scheduled caesarians to be births occurring by C-section before labour has started, whereas emergency caesarians are those carried out after the start of labour and induced by physicians.

that would be disclosed, but in the second, patients accessing information might change physicians and mothers' behaviour which the parameter cannot distinguish. Therefore, when I report γ_5 as the effects of information disclosure it does not implicate that *mothers* are the main and exclusive channel of change in delivery choice.

A persistent caveat for interpreting γ_4 and γ_5 as an evidence of causality is that the information disclosed could be correlated with time-invariant and time-variant unobservable features of hospitals. Regarding the time-invariant, note that Eq. (3.1) includes hospital fixed-effects exactly with the purpose of addressing the influence of hospitals' time-invariant unobservable characteristics affecting delivery choice. Thus, any remaining bias must arise from the correlation between time-varying characteristics in the residual ε_{iht} and the change in the legislation. For example, private hospitals practising fewer C-sections may have disclosed their information before the date stipulated by the legislation, in order to influence patients' decisions and obtain a larger market share. Patients could also estimate C-section rates before they were indicated by the legislation by using alternative proxies (availability of rooms, doulas among the staff, experiences of friends, etc.).

A standard way to control for these biases is to include the price of C-sections in Eq. (3.1). Because the correlation between prices and hospital quality is probably strong and positive, not including price as a control may lead the coefficients γ_4 and γ_5 to be downward biased. As our data does not provide the price paid by patients for their deliveries, I introduce some additional measures to avoid bias. Firstly, given that physicians are constrained from recommending unnecessary procedures by the hospitals' ethical concerns and their own reputation (see Evans, 1974), I include two dummies equal to one for the month when hospitals declare having "Medical Ethics Commitees" and "Commitees for Reviewing Medical Reports" and zero otherwise. Secondly, the model includes a dummy for teaching status, and another for months when hospitals participated in the "National Evaluation of Health Services Program" (PNASS)[4] . Thirdly, to control for heterogeneous

[4]The PNASS evaluates the general structure in the hospital, the labour conditions, in-

quality in pregnancy care, I include a national certificate named "Hospital Friend of Children", provided by the Brazilian government to hospitals demonstrating "excellency in child care and deliveries". Hopefully, these controls will minimize the bias due time-variant unobservables.

Finally, triple-difference estimates consider some assumptions. First, the model assumes no parallel shock on deliveries and birth outcomes at the time the legislation was enacted. In the case that a shock in birth outcomes occurred at the same time as the legislation, the model would mistakenly attribute the effects of such shock to the change in legislation and information disclosure. The second assumption considers that patients do not change their behaviour unless they have access to information. This assumption means that the impact measured by γ_4 does not represent changes in patients' behaviour in anticipation of the information disclosure but only from physicians' response. I checked the plausibility of these assumptions by estimating the model for emergency C-section and for a falsification test using breech deliveries, which are highly associated with C-section rates but are not mentioned in the legislation; I also propose a placebo test exploring the medical knowledge of physician-patients, to whom the information disclosed is arguably already known.

3.4 Data Description

The data for patients originates from the Sistema de Informações sobre Nascidos Vivos (SINASC) from 1 January 2014 to 31 December 2015. This data provides a rich source of information from approximately 6 million births (3 million per year) that occurred in Brazil. One of the main advantages of SINASC is that it provides a comprehensive amount of information about current and previous pregnancies, which might influence the decision of physicians to recommend a C-section.

Table 3.2 shows descriptive statistics in the baseline from outcomes and patient characteristics. The first two columns show the means and standard

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dices of risk performance and patient satisfaction in yearly basis.

Table 3.2: Descriptive statistics for birth outcomes and pregnancy characteristics in the baseline

	Private hospitals		Public Hospitals	
	Mean	SD	Mean	SD
Birth outcomes				
% of C-section	.884	(.320)	.408	(.491)
% of normal births	.116	(.319)	.589	(.491)
% scheduled C-section	.598	(.490)	.124	(.330)
% emergency C-section	.284	(.451)	.284	(.451)
Patients' characteristics				
% previous C-section	.375	(.484)	.293	(.455)
# of prenatal visits	9.56	(8.51)	7.65	(8.89)
Mother's age (in years)	29.48	(5.85)	24.72	(6.51)
% of teenage mothers	.034	(.181)	.185	(.388)
% of first pregnancy	.514	(.499)	.619	(.485)
% of twins	.028	(.165)	.019	(.138)
# of children	.623	(.815)	1.222	(1.49)
% with previous abortion	.202	(.519)	.226	(.570)
% of mothers without diploma	.057	(.232)	.274	(.446)
% of single mother	.268	(.442)	.452	(.497)
% live in a different city	.359	(.479)	.248	(.432)
% of black patients	.041	(.199)	.055	(.229)
# of patients in the baseline	379,398		1,044,467	

Notes: The sample is from the SINASC for January 2014 - December 2015 and consists of public and private hospitals not operating within the National Health Service (SUS).

deviations for private hospitals not operating within the National Health Service (SUS), while the last two columns show the means and standard deviations for public hospitals. As one may expect, there is a very large discrepancy in C-section rates between private and public hospitals; 88.4 percent of births in private hospitals are C-sections, while in public hospitals the figure is 40.8 percent. Table 3.2 also demonstrates that most of the difference in C-section rates arises in scheduled C-sections: 59.8 percent in private hospitals against 12.4 percent in public hospitals.

The remaining rows in Table 3.2 show pregnancy characteristics. Births in private hospitals are around 10 percent more likely than in public hospitals to be born from mothers who have previously had a C-section, and the number of prenatal visits is larger (around two more visits in private hospitals). Patients in private hospitals are on average more likely to be married, have fewer children, and are around five years older than their public counterparts. For instance, there is a significant difference in the educational levels of patients. Almost 1 out of 3 patients in public hospitals do not have an undergrad diploma, against approximately 6 percent in private hospitals. The percentages of black patients are relatively similar, but 12 percent more patients from private hospitals come from cities other than that of the hospital. A general indication from Table 3.2 is that differences in delivery choices might reflect differences in patients' characteristics between private and public hospitals.

Another important advantage of SINASC is the opportunity to match unique identifiers of hospitals with other datasets from SUS. Therefore, to control for many potential cofounders associated with hospital quality in pregnancy care, I match SINASC data with the Cadastro Nacional de Estabelecimentos de Saúde (CNES), which provides several characteristics of hospitals per month. The list is too extensive to present in detail, but in this paper, I include: number of rooms for specific purposes, centers of obstetrics, surgery and cardiology, national certificates of quality, and participation in national programs of quality evaluation, among other characteristics.

Table 3.3 presents means and standard deviations from baseline characteristics of hospitals used in this paper. The first two columns show the means and standard deviations for private hospitals, while the last two columns refer to public hospitals. Table 3.3 shows that public hospitals have significantly more rooms for prenatal, waiting rooms before delivery, and more than double the number of shared rooms for new-borns.

Another great difference between private and public hospitals is in terms of teaching status and certificates: 27.3% public hospitals conduct some teaching activity, against 3.3% private hospitals; 33.3% have the certificate 'Hospital Friend of Children' for 'excellence in pregnancy and child care', while only 0.4% of private hospitals have this same certificate. Private hospitals have a higher tendency to have a Medical Ethics Committee: 65.4% against 52.3% in public hospitals. The remaining characteristics are rel-

Table 3.3: Descriptive statistics for public and private hospitals in the baseline

	Private hospitals		Public Hospitals	
	Mean	SD	Mean	SD
Hospital structure				
# of rooms for prenatal	.74	(.962)	1.79	(1.81)
# of rooms for normal birth	2.24	(5.46)	2.56	(8.85)
# of rooms for surgeries	1.73	(1.98)	.85	(1.03)
# of rooms for before birth	1.45	(1.84)	5.12	(4.07)
# of shared rooms for new-borns	9.95	(21.4)	21.00	(27.2)
# of hospitals with an Obstetric Unit	.82	(.379)	.97	(.162)
# of hospitals with a Neonatal Unit	.53	(.499)	.68	(.464)
# of hospitals with a Surgery Unit	.97	(.155)	.96	(.176)
Average of rooms for surgery	25.00	(24.1)	28.62	(44.8)
Average of rooms for clinic	23.41	(33.2)	33.29	(47.2)
Average of rooms for complex operations	26.18	(28.3)	27.30	(33.0)
Average number of births per month	259.5	(296.0)	229.1	(189.)
Quality variables and certificates				
% Teaching Status	.033	(.180)	.279	(.448)
% Certificate: "Hospital friend of Children"	.004	(.068)	.333	(.471)
% Only low complexity operations	.028	(.166)	.021	(.144)
% "Medical Ethics Committee"	.654	(.475)	.523	(.499)
% "Committee to Review Medical Reports"	.575	(.494)	.553	(.497)
% participated in PNASS	.216	(.412)	.189	(.391)
# of hospitals	669		2,981	

atively similar: 2-3% of hospitals perform only low-complexity operations, around 20% participated in PNASS, and around 55% have a Committee to Review Medical Reports.

3.5 The effects of information disclosure on C-section rates

3.5.1 Triple difference estimations without controls and fixed effects

The logic underlying triple-difference coefficients from Eq. (3.1) can be checked in Table 3.4. Column (A) shows the percentages of C-sections for the pre-legislation period, and column (B) calculates those percentages after the change in the legislation. The column 'Time-diff' calculates the difference across time - i.e. subtracting columns (A) from (B) - while the row 'Private-Public diff' measures the difference in C-section rates between private and public hospitals at a point in time. Panel A considers post-treatment to be the period after information disclosure (from 7 July 2015 onwards), while in Panel B considers post-treatment after the change in legislation, but before the information was disclosed to patients (from 6 January to 6 July 2015).

Table 3.4: DDD estimates of the impact of information disclosure on C-section rates (Standard errors clustered at hospital level

	% of C-sections		
	Before	After	Time-Diff
	(A)	(B)	(B) - (A)
A. Information disclosure, 7 July 2015.			
1. Private hospitals	.8834	.8513	-.0319
	(.0077)	(.008)	(.0026)
2. Public hospitals	.4083	.4031	-.0052
	(.0053)	(.0056)	(.0022)
3. Private-public difference	.4750	.4482	
	(.0094)	(.0100)	
Difference-in-difference			-.0267
			(.0040)
B. Legislation change, 6 January 2015.			
4. Private hospitals	.8834	.8577	-.025
	(.0077)	(.0088)	(.003)
5. Public hospitals	.4083	.3996	-.0087
	(.0053)	(.0053)	(.0018)
6. Private-public difference	.4750	.4581	
	(.0094)	(.0103)	
Difference-in-difference			-.0168
			(.0039)
Triple Difference (DDD)			-.0098
			(.0031)

Notes: The table contains the percentage of C-sections for the group identified. Standard errors are clustered at hospital level and are given in parentheses. Public and private hospitals are defined in columns (A) and (B) respectively, Panel A calculates averages before the legislation and after patients have access to information, Panel B measures the rate of C-sections before and after the legislation changed. The triple difference (DDD) is the difference between the difference-in-difference from Panel A and B.

An estimate of λ_1 in Eq. (3.1) can be seen in column (A), row 3. This coefficient shows .475 parcents' difference between private and public hospitals before the legislation changed. This estimation can also be checked by comparing the means for C-sections in the baseline in Table 3.2. Similarly, $\lambda_2 = -.0087$, in the column 'Time diff', row 5, shows that there is a negative trend in C-sections for public hospitals before and after the change in legislation. The 'Time diff' column, row 2, estimates λ_3 as equal to $-.0052$, which represents a decline in the trends of public hospitals before and after patients have access to information. As one could expect, altogether these coefficients suggest significant private-public differences in C-sections before and after the legislation changed.

The interaction variables in Eq. (3.1) are estimated in the last two rows of Table 3.4. The difference between columns (B) and (A) in row 6 estimates the diff-in-diff coefficient λ_4 in Eq. (3.1), which represents the difference in trends in C-section rates of private hospitals before and after the legislation, compared with changes in the trends in C-section rates of public hospitals for the same period. Thus, Table 3.4 indicates a significant decline of 1.68 parcent in C-section rates in private hospitals after the legislation changed and before patients had access to information. I consider this a preliminary indication that physicians anticipated information disclosure by reducing C-section rates.

Finally, the triple-difference coefficient λ_5 captures changes in the trends of C-sections specific to private hospitals after patients had access to information relative to changes in trends of public hospitals; the estimate simply subtracts the diff-in-diff in Panel B from that in Panel A. According to Table 3.4, there was a significant decline of -0.98 percentage point in C-section rates after information was disclosed to patients on 7 July 2015. It is important to note that the triple-difference compares trends in C-section rates between private and public hospitals before and after information disclosure, but excludes periods before the legislative change: i.e. the estimations measure incremental changes in the trends of private hospitals after patients could access information. Therefore, if we assume that the reaction of physicians to information disclosure is constant after the change in legislation, the further decline in the trend of C-sections in private hospitals represents a plausible estimation of patient's empowerment due to information disclosure. Otherwise, it shows that patients accessing information changes the physician-patient relation, and intensifies the reduction in C-section rates by 0.98 percent.

3.5.2 Triple-difference estimates with controls, fixed-effects and per type of C-section

A clear limitation of the DDD estimations presented in Table 3.4 is that they do not account for changes in the observable characteristics of patients that could potentially affect C-sections rates. To check whether the results change when introducing covariates, I estimate Eq. (1) using a full set of controls for hospital and patient characteristics and present the results in Tables 3.5 and 3.6. Table 3.5 estimates the impacts of legislative change on the probability of having a C-section, while Table 3.6 separates the estimations by emergency and scheduled C-section. As complementary evidence, Figure 3.1 plots predicted probabilities of having a C-section to provide visual evidence that private and public hospitals had common trends before the legislative change.

All columns in Tables 3.5 and 3.6 control for hospitals and patients listed in Table 3.2 and 3.3, fixed effects for the month of birth and hospital of birth, and a monthly time-trend variable. Columns [2] add specific state trends and columns [3] add specific trends for hospitals. Standard errors are clustered at hospital (in parentheses) and at state (in brackets) level and weights consider the number of births in the hospital per month. Clustering the standard errors at state level helps to check whether hospitals in the same area adopt similar delivery choices and to account for potential regional differences in patients' propensity to have a C-section.

The estimated effects of information disclosure in Table 3.5 indicate a reduction in the probability of having a C-section by 1.9 percent during the adaptation period when physicians had 180 days to disclose information to patients. An estimative which is slightly larger than the one presented in Table 3.4. Moreover, there is an additional decline of 0.8 parcents after patients start having access to the information on 7 July 2015. These coef-

Table 3.5: DDD estimates of the effects of legislation change and information disclosure on C-section (Clustered standard errors at (hospital) and [state] level)

	For all types of C-sections		
	[1]	[2]	[3]
LAW	.011	.011	.011
	(.006)*	(.006)*	(.006)*
	[.005]*	[.005]*	[.005]*
DISCLOSURE	.004	.004	.004
	(.004)	(.004)	(.004)
	[.002]	[.002]	[.002]
PRIV	.536	.533	.394
	(.009)***	(.034)***	(.509)
	[.012]***	[.027]***	[.772]
LAW × PRIV	-.019	- .019	- .019
	(.006)***	(.006)***	(.006)***
	[.006]***	[.006]***	[.007]***
DISCLOSURE × LAW × PRIV	- .008	-.008	- .008
	(.003)**	(.003)**	(.003)**
	[.004]**	[.004]**	[.004]**
Covariates for patients and hospitals	yes	yes	yes
FE: Hospitals and month of birth	yes	yes	yes
Includes State trends?	no	yes	no
Includes Hospital trends?	no	no	yes
Number of patients	2,588,767	2,588,767	2,588,767
Number of hospitals	3,626	3,626	3,636
R-squared	.34	.34	.34

Notes: The standard errors are clustered at hospital (in parenthesis) and state (in brackets) level. The regressions include fixed effects for hospitals, months and states. The dummy variable 'LAW' equals one for the period after the legislation changed, i.e. from 6 January 2015, onwards; 'Disclosure' is a dummy variable for periods of information disclosure from 7 July 2015, when the information for physicians and private hospitals was finally released and verifiable by patients; the treated dummy 'PRIV' denotes private hospitals not operating within the National Health System (SUS).

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

ficients indicate that at least two thirds of the total decline in C-sections reflects physicians anticipating to information disclosure. This conclusion remains unaltered when one adds specific trends for states and hospitals in columns [2] and [3].

One reason to be careful interpreting the results in Table 3.4 is that mothers in private hospitals may already have all information needed to decide which physician will perform her delivery but they are actually opting to have a C-section. This would be the case because their preferences are for C-section deliveries rather than lack of information even acknowledging the risks involved. That would also help to explain why the legislative change only reduced in 1.6% the C-section rates after information disclosure.

Further evidence that estimations from Table 3.5 reflect the effects of information disclosure can be checked in Figure 3.1. Figure 3.1 plots predicted probabilities of having a C-section for private and public hospitals before and after the legislative change. Note that, for periods before the law, the trends in C-section rates between public and private hospitals had a similar evolution across the year preceding the legislative change. However, after 6 January 2015 (the first vertical line), there is a sharp reduction in C-section rates of private hospitals that is not followed by public hospitals; I attribute such decrease during the adaptation period to private hospitals anticipating to information disclosure. Similarly, the change on C-section rates during the "Information disclosure" represents the incremental effects produced by patients accessing information about physician's performance.

Another possible way to check the credibility of our results is estimating the effects of information disclosure on emergency and scheduled C-sections. Why? Because if emergency C-sections arise under critical and unpreventable circumstances, it is plausible to assume that legislative change does not affect the rate of emergency procedures. Moreover, as scheduled C-sections are more susceptible to represent a recommendation from the physician or patient's request, one could also expect that the effects of information disclosure may be concentrated on scheduled procedures. Another possible reason to perform separate regressions is that the margin for procedure choice may be wider for healthy patients scheduling a C-section than

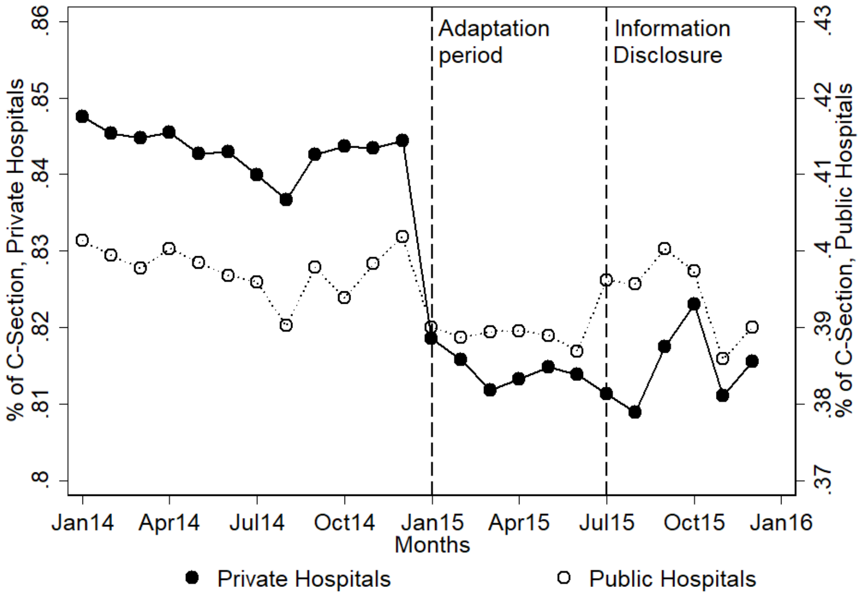


Figure 3.1: Predicted C-section in Public and Private hospitals
Notes: Figure 3.1 plots the average predicted C-section per month from estimates in column [1] in Table 3.4. The percentage of C-section of private hospitals is presented in the left axis, while the right axis shows the percentage of C-section of public hospitals. The vertical line on Jan 15 represents the date when the legislation changed (6 January 2015), and the vertical line on Jul 15 marks when patients started accessing information of physicians.

for unhealthy patients that the clinical condition might demand an emergency intervention (Rochaix, 1989).

The results from Table 3.6 point exactly to this reasoning. If in one hand, it is not possible to detect any change in the trends of emergency C-sections in private hospitals after the legislation change compared to public hospitals, one the other hand scheduled C-sections reduced by 3.4 percent during the adaptation period and declined again by 1.4 percent after patients had access to information. Overall, these results qualify our previous findings by indicating that the effects of information disclosure concentrate on the reduction of scheduled procedures.

In fact, the absence of impacts on emergency C-sections in Table 3.6 motivates us to question whether the reduction in C-section occurs among unnec-

Table 3.6: DDD estimates per type of C-section (Clustered Standard errors at (hospital) and [state] level)

	Type of C-section					
	Emergency			Scheduled		
	[1]	[2]	[3]	[1]	[2]	[3]
LAW	-.001	-.001	-.001	.015	.015	.015
	(.006)	(.006)	(.006)	(.006)***	(.006)***	(.006)***
	[.006]	[.006]	[.006]	[.005]***	[.005]***	[.005]***
DISCLOSURE	.004	.004	.004	.002	.002	.002
	(.006)	(.006)	(.006)	(.005)	(.005)	(.005)
	[.003]	[.003]	[.003]	[.004]	[.004]	[.004]
PRIV	.620	.614	1.692	-.185	-.185	-.197
	(.014)***	(.027)***	(1.10)	(.030)***	(.030)***	(.030)***
	[.011]***	[.023]***	[1.16]	[2.55]	[1.31]	[1.15]
LAW × PRIV	-.008	-.008	-.007	-.034	-.034	-.035
	(.014)	(.014)	(.014)	(.010)***	(.010)***	(.010)***
	[.007]	[.007]	[.007]	[.010]***	[.010]***	[.010]***
LAW × PRIV × DISCLOSURE	.000	.000	.000	-.014	-.014	-.014
	(.010)	(.010)	(.011)	(.005)***	(.005)***	(.005)***
	[.005]	(.005)	[.006]	[.005]***	[.005]***	[.005]***
Covariates: patients and hospitals	yes	yes	yes	yes	yes	yes
FE: Hospital and month of birth	yes	yes	yes	yes	yes	yes
Includes State trends?	no	yes	no	no	yes	no
Includes Hospital trends?	no	no	yes	no	no	yes
# of patients		2,144.009			2,380.154	
R^2	.23	.23	.23	.53	.53	.53

Notes: The standard errors are clustered at hospital (in parenthesis) and state (in brackets) level. The regressions include fixed effects for hospitals, months and states. The dummy variable 'LAW' equals one for the period after the legislation changed, i.e. from 6 January 2015, onwards; 'Disclosure' is a dummy variable for periods of information disclosure from 7 July 2015, when the information for physicians and private hospitals was finally released and verifiable by patients; the treated dummy 'PRIV' denotes private hospitals not operating within the National Health System (SUS).
* Significance level at $\rho < .10$
** Significance level at $\rho < .05$
*** Significance level at $\rho < .01$.

essary procedures. That can be the case if better-informed patients diminish the power of physicians to generate demand for their own services by recommending unnecessary scheduled C-sections. Dranove (1988) argued that physicians have less discretion to induce demand when patients improve their own 'diagnostic skills', which is precisely the purpose of the legislation. And still according to Dranove (1988), knowledgeable patients would only accept a treatment with uncertain benefits if there are higher levels of clinical complications. Thus, at the margin, physicians must increase their threshold of health complications to recommend an unnecessary scheduled C-section.

Physicians may also reduce scheduled C-sections on the basis of 'Defensive medicine'. Defensive medicine is when physicians recommend a procedure with no benefits to the patient, but do so to protect himself from liability (Kessler et al., 1996; Currie and MacLeod, 2008). Patients more conscious of their clinical conditions may be more likely to sue physicians for malpractice, negligence or unnecessary treatments. So, physicians' best strategy for minimizing their liability is to perform C-sections only in cases where there is a smaller margin for doubt.

3.5.3 The effects of information disclosure per quartile

According to the results presented so far, physicians anticipate information disclosure by performing -1.9 percent fewer C-sections, and the disclosure to patients reinforce this reduction by -0.8 percent. These effects almost double for scheduled C-section, and are undetectable for emergency deliveries. A potential explanation for the decline during the adaptation period is that private hospitals anticipated to future information disclosure by reducing current C-sections.

A plausible reason why this occurs is that private hospitals may expect that patients attribute high percentages of C-sections to bad performance, or consider it as an indication of over-recommend procedures that are not necessarily beneficial for the foetus. Following the argument of Dranove et al. (2003) for cardiac surgery, hospitals with high rates of C-section disproportionately have incentives to decrease their rates, thus becoming less distinguishable from hospitals with lower C-section rates. Therefore, when the information is disclosed, patients will be less able to avoid over-recommenders: it becomes harder to punish bad physicians by searching for a second professional, because their C-section rates are relatively similar.

To analyze this issue, I calculate the quartiles of C-section rates per hospital during the baseline and re-estimate Eq. (1) for each quartile. These regressions demonstrate whether private hospitals in higher quartiles (with higher C-section rates) responded differently from private hospitals in lower quartiles (with low C-section rates) to legislative change. Figure 3.2 (A) plots the coefficients of $PRIV_h \times LAW_t$ and (B) shows the coefficients of

$PRIV_h \times LAW_t \times DISCLOSURE_t$ for each quartile. Note that (A) represents the anticipatory effects of hospitals and (B) shows the impacts of patients accessing information about physician's performance.

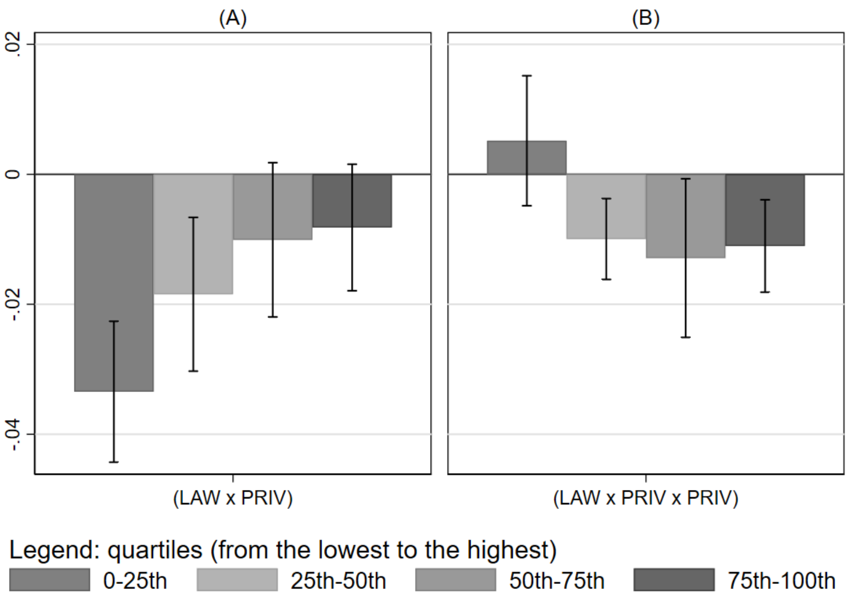


Figure 3.2: The effects of information disclosure per quartile

Notes: Figure 3.2 demonstrates estimation results per quartile of C-section before legislative change. The coefficients of $PRIV_h \times LAW_t$ are presented in (A) and $PRIV_h \times LAW_t \times DISCLOSURE_t$ in (B). The characteristics of hospitals is each quartile is shown in Table 3.7.

Surprisingly, Figure 3.2 (A) shows that private hospitals in lower quartiles had stronger reductions in C-section rates than private hospitals in higher quartiles during the adaptation period. In more explicit terms, there is no indication that private hospitals with the higher C-section in the baseline (i.e. belonging to the third and fourth quartiles) reduced their C-section rates on the 180 days following the law. The adaptation effects are significant only for hospitals in the first and second quartiles, and therefore with lower C-section rates before legislative change. It is the opposite conclusion that we would expect based on the reasoning above.

But why private hospitals in lower quartiles reduce their C-section rates more expressively than private hospitals in higher quartiles during the adap-

Table 3.7: Characteristics of Private hospitals per quartile of C-section rates in the baseline

	Quartiles (from the lowest to the highest)							
Hospital Characteristics	0-25th		25th-50th		50th-75th		75th-100th	
% C-Section	.741	(.438)	.885	(.320)	.929	(.258)	.972	(.166)
% of Normal Births	.259	(.438)	.115	(.319)	.070	(.255)	.027	(.163)
# of rooms for prenatal	1.22	(1.33)	.571	(.592)	.498	(.655)	.535	(.686)
# of rooms for normal birth	.976	(1.05)	1.55	(1.49)	.510	(.626)	.578	(.566)
# of rooms for surgeries	.915	(.279)	.819	(.385)	.777	(.416)	.757	(.429)
# of rooms for before birth	2.42	(2.11)	1.25	(1.41)	.856	(1.48)	1.02	(1.76)
# of shared rooms for new-borns	9.03	(14.9)	26.1	(39.1)	5.59	(9.61)	5.30	(8.39)
% "Hospital friend of Children"	.014	(.121)	.001	(.043)	0	(0)	0	(0)
% "Medical Ethics Committee"	.922	(.269)	.643	(.479)	.565	(.496)	.452	(.498)
% "Review Medical Reports"	.863	(.343)	.572	(.495)	.502	(.500)	.351	(.477)
# of hospitals	108		95		148		294	
# of patients	98,139		98,242		104,315		100,310	

tation period? A candidate to answer this question lays on the lack of structure provided by private hospitals belonging to high quartiles to perform normal deliveries. Table 3.7 demonstrates that hospitals in higher quartiles (third and fourth) had fewer rooms for prenatal, normal birth, and for before birth than hospitals in lower quartiles (specially first quartile). It also shows that 92.2 percent of hospitals in the first quartile have a "Medical Ethic Committees" and 86.3 percent have "Review Medical Reports", while these percentages are 45.2 and 35.1 respectively for hospitals in the fourth quartile and slightly higher for hospitals in the third quartile. Additionally, there are no hospitals in the third and fourth quartiles holding the certificate "Hospital Friend of Children". Thus, even though physicians could foresee losses in market-share or reputation due to over-recommending C-sections, their actual capacity to reduce C-sections rates seems to be constrained by the structure of private hospitals to accommodate normal births.

Another possible reason for the results in Figure 3.3 (A) is that physicians in higher quartiles lack the skills to perform normal births. Although our data does not provide us any opportunity to test this argument empirically, there seems a plausible case to suggest that physicians in the fourth quartile have less experience in performing normal births than physicians in lower quartiles when only 2.7 percent of their deliveries during all 2014 were normal against 25.9 percent among hospitals in the first quartile. Altogether, these constraints might explain why the impacts of information disclosure are relatively small. As more than 88 percent of births in private sector are delivered by C-section and the legislative change only reduced it in - 4.8 percent, the results from Figure 3.3 implicate that lack of structure and known-how to perform normal births prevent information disclosure to produce sharper reductions in C-section rates.

Figure 3.2 (B) indicates an opposite conclusion compared to (A): or simply, private hospitals in higher quartiles present stronger reductions in C-section rates when patients had access to information. An explanation for this result comes from Rochaix (1989). The intuition underlying her supply-induced demand model is that the patient has an expectation for her treatment, and waits for the physician's recommendation. If the distance between the physician's recommendation and the patient's expectation is large enough, patients have incentives to search for a second opinion. So, if an expectant mother checks the previous performance of physicians, verifies her partographs and observes no complications during her pregnancy, she would be less likely to search for another physician if he reduces the gap between what he recommends and what she expects by performing a standard vaginal delivery.

3.5.4 The effects of information disclosure per hospital ownership

The major motivation of the legislative change is that better-informed patients are able to distinguish hospitals and physicians from heterogeneous quality. For instance, due to information disclosure, hospitals can reduce their C-section rates aiming to signal better quality or because they are facing stronger competition for patients in the market. In this section I explore the link between quality signaling and non-profit status, especially under asymmetric information (Arrow, 1963; Glaeser and Shleifer, 2001; Jones et al., 2017), to measure the effects of information disclosure per ownership type.

In fact, when Glaeser and Shleifer (2001) asked why entrepreneurs open a non-profit firm one suggestion was that firms use this status to signal quality for their services. This answer originates from the idea that non-profit status "softens" the incentives to maximize profits and allows hospitals to distribute them to improve quality. Yet, the strong incentive to maximize profit would discourage for-profit hospitals to invest on unobservable quality (Jones et al., 2017). Regarding our context, even assuming that a C-section and a normal birth produce the same profit, for-profit hospitals would be still more resistant to reduce C-section rates than non-profit hospitals because several C-sections can be scheduled on the same day (implicating in high opportunity costs to perform a normal birth) and reduce the workload of physicians (and the respective extra costs).

Similarly, patients may prefer non-profit hospitals because they expect them to hire better physicians by offering higher salaries. Or feel protected by the belief that the delivery choice in non-profit hospitals are not profit-oriented. Therefore, if the non-profit status is used to signal quality by hospitals and low C-section rates are attributed to better performance by patients, it seems reasonable to suspect that non-profit hospitals have stronger incentives to reduce C-sections when information is disclosed.

In an attempt to capture how information disclosure affects delivery choice by hospital ownership, I divide the sample of private hospitals by "Non-Profit", "For Profit" and "Simples". The group "Simples" is composed by for-profit hospitals reporting annual gross profit up to R$ 3,6 million Brazilian Reais (or approximately 1.5 million US dollars). "Simples" simplifies the bureaucracy when hospitals are paying their taxes but does not reduce the amount paid. The inclusion of "Simples" in the analysis tests whether information disclosure affects for-profit hospitals with different budget sizes. The estimation results of anticipatory effects and information disclosure effects for "Non-profit", "Profit" and "Simples" are in Figure 3.3.

Figure 3.3 shows that Non-profit and Simples hospitals have the highest reduction in C-section rates during the 180 days of adaptation following legislative change. In the case of non-profit hospitals, information disclosure forces further reductions in C-section rates because their patients demand higher quality (Glaeser and Shleifer, 2001) or become more quality elastic when access information. Jones et al., (2017) argues in a similar direction

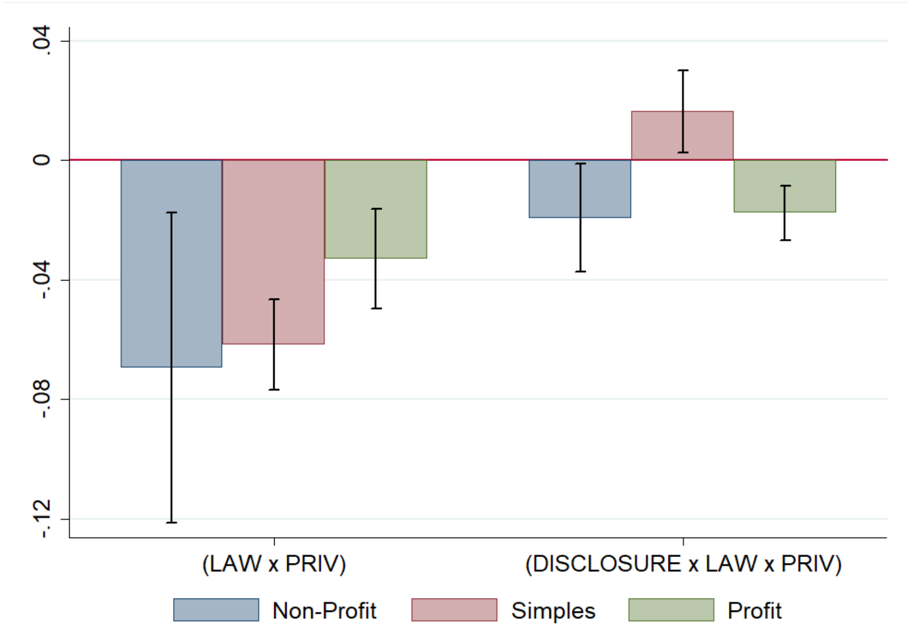


Figure 3.3: The effects of information disclosure per hospital ownership
Notes: The figure depicts the coefficients of $PRIV_h \times LAW_t$ and $PRIV_h \times LAW_t \times DISCLOSURE_t$ for "Non-profit", "Profit" and "Simple" hospitals and confidence intervals of 90 percent. The last category "Simples" represents micro for-profit hospitals with annual gross income lower than R$ 3,6 million Brazilian Reais.

for changes in hospital status. Another explanation is that non-profit hospitals aim to maintain the reputation of non-profit oriented by signaling lower C-section rates. This idea is stressed by Glaeser and Shleifer (2001). Therefore, non-profit hospitals reduce C-section rates to confirm higher quality disclosing lower levels of C-section rates.

However, non-profit hospitals are not the only ones reducing C-section rates during the adaptation period. Figure 3.3 shows that the reduction in Simples hospitals is as large as in non-profit hospitals and almost double than for-profit hospitals not belonging to Simples. Such difference in the anticipatory effects between "Simples" and "non-Simples" hospitals may rely on market competition. If we consider that hospitals belonging to simples are smaller and face stronger competition than bigger hospitals facing weaker competition, an explanation is that Simples hospitals, with smaller annual profit than For-profit hospitals, compete for future better-informed patients reducing current rates of C-sections. Jones et al., (2017) demonstrates the hospitals facing a minimal degree of competition are less likely to change their status after information disclosure compared to hospitals under strong competition. Another evidence for this point is that "For-Profit" hospitals are the only ones having a significant reduction in C-section rates during information disclosure.

3.6 Robustness Checks

3.6.1 Alternative approach and monthly difference-in-difference estimates

To provide a convincing case for the results so far, we also present estimations using two alternative strategies. The first re-estimates Eq. (1) replacing the dummy variable LAW_t by another dummy $ADAPT_t$ equal to one for the adaption period (i.e. from 6 January to 6 July 2015) and zero otherwise. This approach simply replaces the triple difference $PRIV_h \times LAW_t \times DISCLOSURE_t$ in Eq. (1) by the interaction $PRIV_h \times DISCLOSURE_t$, and tests whether the coefficients from $PRIV_h \times ADAPT_t$ and $PRIV_h \times DISCLOSURE_t$ are statistically significant.

The estimating results from this approach are shown in Table 3.8. The second piece of evidence comes from a set of Diff-in-Diff estimations comparing the trends between private and public hospitals in each month. If the coefficients comparing the trends between private and public hospitals only become statistically significant after the legislative change, we have additional arguments that our results are causal. Figure 3.2 plots the Diff-in-Diff coefficients per month.

The results from Table 3.8 and Figure 3.5 do not change the interpretation of our previous findings. Note that $\lambda_4 - \lambda_5$ equals to the triple difference coefficients in Tables 3.5 and 3.6, and we only accept the null hypothesis that $\lambda_4 - \lambda_5 = 0$ for emergency C-sections. Figure 3.5 shows that the difference in trends in C-section between private and public hospitals only start

Table 3.8: Alternative estimates of the effects of legislation change on C-section (Clustered standard errors)

Estimating regression:	$BO_{iht} = \alpha + \beta_1 M_{it} + \beta_2 H_{ht} + \lambda_{hospital} + \lambda_{month} + trend + \gamma_1 PRIV_h + \gamma_2 ADAPT_t + \gamma_3 DISCLOSURE_t + \gamma_4(PRIV_h \times ADAPT_t) + \gamma_5(PRIV_h \times DISCLOSURE_t) + \varepsilon_{iht},$					
			Per type of C-section			
	All C-sections		Emergency		Scheduled	
	Coef.	Std. Error	Coef.	Std. Error	Coef.	Std. Error
Adapt	.011*	(.006)	-.001	(.006)	.015***	(.006)
Private	.536***	(.008)	.620***	(.013)	- .191***	(.030)
Disclosure	.014*	(.009)	.003	(.009)	.017**	(.007)
Adapt × Private	- .019***	(.006)	-.008	(.014)	- .034***	(.010)
Disclosure × Private	- .026***	(.006)	-.008	(.016)	- .048***	(.012)
F-test H_0: $\lambda_4 - \lambda_5 = 0$	4.83**		0.00		8.09***	
P-Value	.028		.976		.004	
# of patients	2,588,767		2,144,009		2,380,154	
# of hospitals	3,626		3,626		3,636	
R^2	.34		.23		.53	

Notes: The standard errors are clustered at hospital level. The regressions include fixed effects for hospitals and months of birth. The dummy variable $ADAPT_t$ equals one for the period after the legislation changed, i.e. from 6 January 2015, and before information disclosure 6 July 2015; 'Disclosure' is a dummy variable for periods of information disclosure from 7 July 2015; the treated dummy 'PRIV' denotes private hospitals not operating within the National Health System (SUS). All regressions include controls for hospital and patient characteristics shown in Table 3.2 and 3.3.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

to be significant after legislative change on January 2015. Therefore, both results reinforce the conclusion that private hospitals are performing fewer C-sections after information disclosure.

3.6.2 The effects of information disclosure comparing only private hospitals

Why physicians did not reduce C-section rates more during the adaption period? I combine the alternative approach in section 3.6.1 with the evidence from section 3.5.3 indicating that the capacity of physicians to recommend fewer C-sections is limited by the availability of appropriate structure to compare private hospitals. Private hospitals lacking physical facilities to perform normal deliveries (fewer rooms for normal births, fewer waiting rooms for expectant mothers, insufficient obstetricians or obstetric centres,

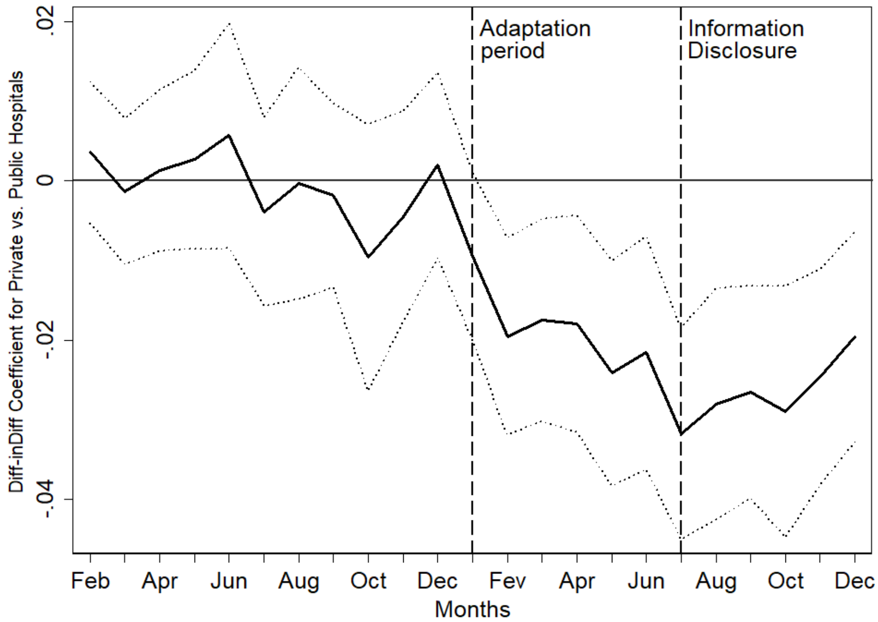


Figure 3.4: Difference-in-difference coefficients per month

Notes: Figure 3.4 plots the coefficients from the interaction between $PRIV$ and monthly dummies from February 2014 to December 2015. These estimations include all controls for hospitals and patients, but naturally excludes the time-trend variable. Confidence intervals are for 10 percent significance level.

for example) may face higher costs for reducing C-section rates at least in the short-run. This section tests whether structure constrained physicians to anticipate to information disclosure. For that, I proceed as follows.

One estimates the impacts of the legislative change by comparing private hospitals from different quartiles. So, regressions compare the changes in the trends of C-section rates of private hospitals in the 0-25th, 25th-50th and 50th-75th quartiles with the changes in the trends of private hospitals in the 75th-100th quartile. As hospitals in the 75th-100th quartile seem to be the most constraint to perform normal deliveries, therefoore I believe it is reasonable to consider them as the control group. The results of this exercise are shown in Table 3.9.

In intuitive terms, physicians with better structure to perform normal deliveries anticipate to information disclosure more intensively. This inter-

Table 3.9: Effects of information disclosure for private hospitals of heterogeneous structure for normal births (clustered standard errors at hospitals level)

	Dependent variable: C-section		
	Control group: Private hospitals, 75^{th}-100^{th} quartile		
	Treatment group: Private hospitals in the		
	0-25^{th} quartile	25^{th}-50^{th} quartile	50^{th}-75^{th} quartile
	(A)	(B)	(C)
PRIV	- 0.698***	- 0.117***	- 0.011
	(0.031)	(0.020)	(0.007)
ADAPT	- 0.002	- 0.003	0.002
	(0.008)	(0.009)	(0.005)
DISCLOSURE	- 0.012	- 0.006	- 0.013*
	(0.009)	(0.009)	(0.007)
DISCLOSURE × PRIV	- 0.008	- 0.013*	0.004
	(0.007)	(0.008)	(0.007)
ADAPT × PRIV	- 0.028***	- 0.016***	- 0.003
	(0.004)	(0.005)	(0.005)
FE: Hospitals and MOB	Yes	Yes	Yes
Covariates: hospitals and patients	Yes	Yes	Yes
# of hospitals	468	455	508
# of observations	368,638	371,979	365,350
R^2	0.22	0.09	0.07

Notes: Standard errors are clustered at hospital level. The regressions include only private hospitals. The assignment of hospitals to each quartile uses the percentage of C-section rates during the baseline, from 1st January 2014 until 6th January 2015.
* Significance level at $\rho < .10$
** Significance level at $\rho < .05$
*** Significance level at $\rho < .01$.

pretation comes from estimates in column (A) showing that private hospitals in the 0-25^{th} quartile reduce C-sections by 2.8 percent relative to private hospitals in the 75^{th}-100^{th} quartile. For estimates in column (B), this result diminishes to 1.6 percent and vanishes completely comparing private hospitals in the two highest quartiles in column (C).

Another possible explanation for the results in Table VII is that physicians in the 75^{th}-100^{th} quartile do not have the experience and skills to perform normal births. Although our data does not provide any opportunity to test this argument empirically, it seems a plausible case given that physicians in this group performed only 2.7 percent normal births during the entire 2014 against 25.9 percent among physicians in the 0-25^{th} quartile.

3.6.3 Falsification and placebo tests

The preceding analyses indicate that the change in legislation and the subsequent public disclosure of information to patients decreased the probability of C-section deliveries in private hospitals. However, a persisting concern is that our results are only valid if patients do not change their behaviour before accessing the information about physicians' performance. Nonetheless, it is possible that patients respond to legislative change by speculating the C-section rates and then selecting better physicians. Another identification concern is distinguishing the impacts of information obtained from alternative sources (Jin and Sorensen, 2005). Thus, depending on the correlation between unobserved factors and the information disclosed, our impact estimates may mistakenly attribute to legislative change changes in trends that are in fact caused by other factors.

To provide empirical evidence that unobservable factors do not drive our results, I propose a falsification and a placebo test, based on previous evidence in the literature. The rationale for the falsification test is that if patients modify their behavior in response to the legislation change, then there must be similar changes for outcomes highly associated with the probability of having a C-sections, but which were not encompassed by the legislation. For that purpose, I explore breech deliveries because they are a non-preventable complication highly associated with C-sections, but not mentioned in the law (Jensen and Wüst, 2015). Finding no effects of legislation and information disclosure on the incidence of breech deliveries would be an evidence against the argument that patient's anticipation is the real source of our results. Thus, the falsification test simply replaces the dependent variable by an indicator for breech deliveries.

The placebo test uses physician-patients as a placebo group. The argument is that the delivery choices of highly informed physician-patients would be unaffected by additional information, as they probably already know it. I assume that there is less difference in discerning power between physicians and physician-patients than between physicians and non-physician patients, because the medical knowledge gap is narrower in the

Table 3.10: Placebo and falsification tests (Cluster standard errors)

	Panel A: FALSIFICATION TEST	
	Dependent variable: Breech delivery	
	Coef.	Std. error
Law	- .000	(.000)
Private	- .001*	(.001)
Disclosure	- .000	(.000)
Law × Private	.000	(.001)
Disclosure × Law × Private	.000	(.001)

	Panel B: PLACEBO TEST			
	Dependent variable: C-section			
	Physicians-patients (N=19,778)		Non-physician patients (N=2,568,989)	
	Coef.	Std. error	Coef.	Std. error
Law	- .021	(.039)	.011*	(.006)
Private	.943***	(.033)	.535***	(.009)
Disclosure	- .038	(.031)	.004	(.004)
Law × Private	.003	(.026)	- .019***	(.006)
Disclosure × Law × Private	.000	(.034)	- .007**	(.003)

Notes: Standard errors are clustered at hospital levels. DDD regressions include the same set of covariates for hospitals and patients (see the list in Table 3.1 and II). Fixed effects for hospital of birth and month of birth are also included in all regressions. Physicians-patients are identified using the list provided in Table A.I in the appendix.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

former comparison (regarding this point, see Johnson and Rehavi, 2016). Then, the placebo test constitutes in re-estimating Eq. (3.1) separately for physician-patients and other patients. Table 3.10 presents the estimation results for the falsification (Panel A) and placebo tests (Panel B).

The falsification tests show zero effects of the law on breech delivery. Similarly, the results from the placebo test in Panel B show that the legislation change and access to information have insignificant effects in physician-patients. The effects of information disclosure on delivery choice concentrate on non-physician patients.

The results from the falsification and placebo test suggest that patients did not anticipate to legislation change, because pregnancy conditions strongly associated with C-sections were not affected. Similarly, physician-patients with similar medical knowledge to the physicians, yet less likely than non-physician patients to be influenced by information disclosure, present insignificant changes in C-section trends in response to legislation change. Therefore, based on these tests, there is no indication that our results are an artefact of patients anticipating to information disclosure, or that access to alternative sources of information is leading our conclusions.

3.7 Conclusions

Do physicians anticipate public disclosure of information by recommending fewer C-sections? Does this effect change when patients start to have access to information? These questions essentially address the importance of the knowledge gap between the physician and the patient, as regards treatment choice. In other words, physicians are far more knowledgeable than patients about their own health conditions, which enables them to recommend treatments not necessarily beneficial to the patient's health. Recent policies have required the public disclosure of information about physicians and hospitals, with the intention that better-informed patients can use the information to choose better health services.

Nonetheless, physicians can anticipate the consequences of information disclosure by "gaming" their performance: they can artificially enhance their quality by refusing patients whose clinical condition is likely to impact on their performance. To some extent, they can also recommend treatments for a specific aim that is not beneficial for the patient. Consequently, when the information is finally disclosed to the public, low-performing physicians are pooled with high-performing physicians, making it harder for patients to distinguish between them. Therefore, in order to determine the effectiveness of policies that aim to empower patients with more information about the quality of health services, it is essential to understand whether physicians anticipate information disclosure.

This paper explores a peculiar change in Brazilian legislation in order to identify the anticipation effects of physicians to information disclosure. On 6 January 2015, the legislation made it compulsory for private hospitals to disclose information about their delivery performance (mainly C-section rates). The attractive feature of this legislation is that it provided 180 days for physicians to "adapt" themselves before disclosing any information to patients. Thus, physicians were fully aware of what information they should disclose 180 days in advance. I propose using this 180 days of "adaptation period" as an exogenous variation in the physician-patient relation, to estimate the anticipatory responses of physicians to information disclosure.

The identification strategy compares changes in trends of C-section rates of private hospitals before and after the legislation was enacted with changes in the trends of C-section rates of public hospitals (which did not have to disclose information) over the same period. To account for the influence of patients obtaining information, I also compared changes in the trends of C-section rates between private and public hospitals before and after patients had access to information. Therefore, I estimate triple-difference regressions using the "adaptation period" after the legislation, as well for when patients were legally allowed to request information. Both comparisons aim to separate the effects of physicians anticipating information disclosure from patients accessing information.

This paper presents three main findings. First, the strongest and most robust result shows that two-thirds of the decline in C-sections reflects physicians anticipating to public information disclosure. Scheduled C-sections decreased in private hospitals by 4.8 percentage point after the legislation compared to their public counterparts: 3.4 percentage point of this difference corresponded to physicians anticipating information disclosure, and 1.4 percentage point was the result of patients accessing the information. One explanation for this result is that physicians predicted losses in market share because if they acquired bad reputation for being an over-recommender or profit-oriented, and therefore reduced the rate of C-sections performed. Second, there were no changes in the trends of emergency C-sections: this may indicate that the reduction in C-sections occurred among unnecessary procedures represented by scheduled C-sections. Specifically, as emergency procedures are performed due to safety concerns and for unpredictable reasons, it is reasonable to assume that emergency C-sections are less responsive to information disclosure than scheduled C-sections.

Third, further results show that lack of structure to perform normal deliveries constrained private hospitals to reduce C-sections rates. Quartile estimations demonstrate a tendency of hospitals from lower quartiles to reduce C-sections more pronouncedly than hospitals belonging to higher quartiles during the adaptation period. Such unintuitive result reflects the fact that private hospitals in lower quartiles having more rooms for prenatal, normal births, before birth and shared rooms for new-borns than hospitals in higher quartiles. Moreover, hospitals in lower quartiles have are twice more likely to have "Medical Ethics Committee" and a committee to "Review Medical Reports" than hospitals in higher quartiles. At the same time, private hospitals in higher quartiles present stronger reductions in C-section rates only when patients had access to information. Fourth, larger reductions in C-section during the adaptation period occur in non-profit and smaller hospitals. These results reflect non-profit hospitals signaling quality to better-informed patients and because smaller hospitals, with smaller annual profit than For-profit hospitals, compete for future better-informed patients reducing current rates of C-sections.

In addition to the result for emergency C-sections, four pieces of evidence support a causal interpretation of these findings. First, estimates replacing the triple differences by interactions capturing the adaptation period and the information disclosure separately implicates on the same conclusions. Secondly, diff-in-diff estimates for changes in the trends between private and public hospitals per month confirm that only after legislative change there is a significant decrease in the trends of C-section in private hospitals compared to public hospitals.

The third evidence proposes a test employing physician-patients as a placebo group. The idea is that physician-patients would not be affected by additional information once they already have that knowledge provided by the information disclosure. The notion that physician-patients do not change their behaviour to the same extent as non-physician patients has empirical support from the literature (Johnson and Rehavi, 2016). The results from the placebo test show that it is not possible to conclude that the effects of information disclosure on physician-patients are different from zero.

Finally, a falsification test used an outcome that is highly correlated with having a C-section, but was not mentioned in the legislation. For this purpose, I used breech deliveries, because they cannot be prevented by physicians' efforts to avoid C-sections, or used by patients when estimating physician's performance. The results of the falsification test indicate that there is not changes in the trends of breech deliveries after the change in the legislation and information disclosure. Both tests suggest that the results are not driven by patients anticipating to information disclosure or by alternative sources of information.

Chapter 4

The effects of (misreported) bullying on labour and schooling outcomes of young adults

Despite bullying being a systematic phenomenon at schools, identifying when it actually occurs is still challenging. Much of this difficulty originates from victims actively deliberating whether to report bullying to others, heterogeneous concepts of what bullying is among informants, and from inconsistencies in alternative reports. Since the first attempts to study the topic by Olweus (1978) and Pikas (1975), there has been a clear concern about 'underreporting by the students' (Olweus, 1994, p.1184) echoed by the idea that the 'variables involved seem difficult to measure' (Pikas, 1975).

Arguably, students can rationally avoid reporting bullying because they anticipate future retaliation from bullies or social isolation from friends scared of becoming the next victim. Smith and Shu (2000) estimated that around 30% of victims do not tell anyone about bullying episodes. Moreover, Hanish and Kochenderfer-Ladd (2004) suggest that victims misreport because they are hesitant to look embarrassed in front of a strange interviewer reporting humiliation. The slogan 'Don't suffer in silence' from UK anti-bullying campaigns explicitly acknowledges misreporting among victims (Department for Education, 1994).

Another challenge in identifying victims of bullying is that informants can have different notions of what should be reported while answering questionnaires (Arora and Thompson, 1987; Erikson et al., 2015; Ahmad and Smith, 1994; Shakoor et al., 2011). Some informants may report aggression as bullying, whereas others may consider it as a 'fight'; some could report social exclusion as bullying, others as a simple enmity. Additionally, specific cases of bullying engineered to be unrevealed to the victim, parents and teachers, and maintain the perpetrator unknown (as spreading false rumours), may amplify these difficulties (Björkqvits et al. 1992).

Using alternative reports to classify victims is similarly problematic. Past investigations have indicated that less than one third of students tell their parents about bullying episodes at school, and less than half report teachers (Whitney and Smith, 1993; Rigby and Barnes, 2002). Some authors also argue that parents are less likely to report bullying using questionnaires than their children (Stockdale et al., 2002), and there are also evidence suggesting that teachers over-report bullying (Hanish et al., 2004; Houndoumadi and Pateraki, 2001). Overall, all these facts create reservations regarding evidence that the incidence of bullying declines across high-school years (Olweus (1993, 1994), Smith et al., (1999) and Boulton and Underwood (1992) among others), and suggests that this decrease may reflect increases in misreporting of bullying.

This paper proposes an estimation strategy similar to Card (1996) and Kane et al., (1999) to estimate bullying effects when 'true' incidence of bullying is imperfectly observed. The first step saves reduced-form coefficients from longitudinal regressions for each high-school year (2004, 2005 and 2006). These regressions include dummies generated to represent the bullying history of students throughout high-school. For example, the bullying history '001' refers to students reporting being victims only in the third year of high-school, '111' equals one for students reporting bullying in all high-school years, and so forth. The second step uses the coefficients from the first step to minimize the distance between observed and 'true' bullying via a transformation matrix. The identification of bullying effects comes from students 'switching' among bullying histories across longitudinal regressions.

To minimize the most serious fears regarding the selection of victims, the empirical strategy incorporates some desirable features. First, it includes fixed-effects of students to deal with selection based on unobservable characteristics; second, all models include geographic area fixed-effects[1] for cases where local unobservables influence bullying rates; third, detailed data allows controlling for special learning difficulties, and physiological characteristics for students negatively selected in terms of these characteristics. And fourth, the analysis incorporates OLS and 2SLS regressions to compare estimated impacts with other standard techniques. Despite these corrections, it is still difficulty to argue that the bullying effects are isolated from selection bias.

Besides proposing a strategy to tackle non-classical measurement error bias in bullying reports, this paper contributes to the literature in some important points. To the extent that the definition of bullying involves 'exposition to negative actions repeatedly and over time' (Olweus, 1997), I test the heterogeneous effects of 'negative actions' requiring direct interaction between victims and bullies ('calling names', 'physical aggression', 'stealing' and 'yelling at') and of indirect forms of bullying not requiring bully-victim interaction ('gossip', 'sending hurtful messages' - for a detailed discussion see Björkqvist et al., 1992); this paper also explores 'repeatedly and over time' to estimate the effects of different frequencies of bullying (from 'daily bullying' to 'it varies'). Finally, this is the first study to estimate the effects of bullying on University attendance, and on the probability of students being not in education, employed or in training (NEET) two-three years after high school. The outcome "University attendance" was chosen to observe whether students stay studyng after living higher school after suffering bullying in school environment. Similarly, NEET among young represents one of the most recent concerns in the literature.

[1] I assume "geographic areas" assume the survey stratum that could not be identified but represent as a codified area where students live. Therefore, students living in the same area have the same stratum id.

Results from the approach indicate consistent negative effects of bullying on labour and schooling outcomes. They show that victims of high-school bullying decrease University attendance by 3.4 percentage points compared to non-victims, and increase the probability of being NEETs by 2.6 percentage points. To check the influence of non-classical measurement error bias, I also present OLS and 2SLS regressions instrumenting bullying reports from parents using bullying history profiles, restricting the sample for 2004 only. This exercise shows that OLS coefficients are two-thirds of those from the strategy proposed, and three times smaller than 2SLS coefficients. The fact that our coefficients lay between OLS and 2SLS aligns with using 2SLS and OLS coefficients as upper and lower boundaries for the true coefficient as discussed in the previous literature (Aigner, 1987; Bollinger, 1996; Black et al., 2000; Kane et al., 1999).

Another result shows heterogeneous impacts based on the interaction between bully and victims. Students suffering 'aggression', 'stealing' and other forms of direct interaction have from 3% to 4% lower probability of University attendance and a higher probability of NEET. These results are similar for boys and girls. For instance, bullying requiring no interaction between the victim(s) and bullies (e.g. gossip and spreading false rumours) presents significant effects on University attendance twice as bigger than direct bullying, but only for girls. Therefore, it does not indicate that boys are majorly affected by direct bullying, however it agrees with previous findings that girls suffer more severely from the effects of indirect bullying than boys (Björkqvist et al., 1992).

A last finding relates to 'repeatedly and over time'. A clear result from our estimations is that the higher the frequency of bullying episodes, the larger is the effect on University attendance and being a NEET. As one could expect, 'daily' and 'weekly' bullying present strong and significant effects while less frequent forms become insignificant in relation to both outcomes.

Anti-bullying polices can benefit from these results in several ways. The first contribution is to unveil the undesirable implications of considering observed bullying to design policies. Crucial benefits can be gained by focusing on those who report themselves as non-victims. Secondly, it seems vital to equip students, parents and teachers with equivalent and transparent concepts of what bullying is. In our sample, there is a disappointing proportion of less than one third of parents reporting bullying when their children acknowledge being victims. Third, bullying as direct confrontation seems to affect all students similarly, but indirect bullying shows stronger effects on girls. Fourth, students victimized daily and weekly may receive additional attention. And fifth, anti-bullying policies should provide continuous support to minimize the effects of bullying on later labour and schooling outcomes.

Besides this introduction, the structure of the paper is as follows: Section 4.1 presents the challenges in measuring bullying; Section 4.2 proposes the empirical strategy; Section 4.3 describes the Longitudinal Study of Young People in England (LSYE); and the results are presented in Section 4.4. Finally, Section 4.5 outlines the main conclusions.

4.1 The challenges of measuring bullying

Probably the most decisive obstacle in identifying bullying is the active role played by students considering whether to inform being victims. Students may rationally avoid telling others because they expect retaliation from the bullies, who are likely to intensify future attacks, or isolation from friends who fear becoming new targets. Past evidence indicates that students also feel humiliated when reporting bullying in front of an interviewer (Smith et al., 2004; Hanish and Kochenderfer- Ladd 2004; Olweus, 1978). Thus, deciding to report bullying can result in resistance from students who are skeptical about the chances of their situation changing, feeling helplessness in the anonymity (Bijttebier and Vertommen, 1998; Naylor et al., 2001).

Avoiding self-report problems exploring alternative sources of information does not seem a promising solution, since a majority of students do not tell their parents and teachers about bullying episodes. Whitney and Smith (1993) and Rigby and Barnes (2002) argue that only one out of three students report bullying to their parents, and one out of two tell teachers. Other studies indicate that students are more likely to admit victimization than their parents (Stockdale et al., 2002) and are less likely to report bullying than teachers (Hanish et al., 2004; Houndoumadi and Pateraki, 2001). However, if pupils share unsustainable and critical episodes with others (as suggested by Shakoor et al., 2011) or if teachers and parents are more likely to report only drastic changes in the behaviour of students, using these reports would exacerbate the issue of identifying critical victims among those who are simply victims of bullying.

Another issue involved in using alternative reports is that teachers and parents can have conflicting ideas of what bullying is, and consequently report inconsistent scenarios. Some can report physical aggression as bullying, others can consider it as 'fight'. Some teachers may understand that unkind gossip at school should be reported as bullying, whereas others understand it as animosity. In fact, Hazler et al. (2001) show that 83% of non-bullying situations are diagnosed as bullying by teachers when a physically unfair match is involved. And Boulton (1996; 1997) estimates that one out of four teachers do not recall 'name calling', 'spreading rumours' or 'social exclusion' as genuine forms of bullying. These inconsistencies demonstrate that deciding to report bullying also depends on the definition implemented by the person answering the questionnaire (Boulton and Underwook, 1992).

Instead of using questionnaires, a possible approach is to observe the typical characteristics of victims. In general terms, this research indicates that victims are smaller, and physically weaker (Olweus, 1978), more insecure and socially isolated (Bernstein and Watson, 1997), have higher levels of anxiety, depression, and mental problems than non-victims (Takizawa et al., 2014; Lereya et al., 2015). However, the most serious difficulty in this approach is to know whether such characteristics are the determinants or the cause of bullying (Fekkes et al., 2006).

Few studies in economics estimate the impacts of early bullying on later outcomes. The first is from Brown and Taylor (2008), who explore the National Child Development Study from England and identify bullying using the question 'Bullied by other kids?', answered by parents when pupils were 7 and 11 years old. Their conclusion shows a negative association between bullying and future grades and earnings. Another study from Le et al. (2005)

presents evidence that conduct disorders during childhood (including bullying) affect school dropping out rates and unemployment among victims. And Sarzosa and Urzua (2015) address non-random selection of victims and provide evidence that early bullying increases stress, depression, probability of smoking, and mental health.

Finally, Erikson et al. (2015) recognize measurement error and selection bias of victims as a threat to consistent estimations and inventively instrument bullying using the proportion of peers whose parents have a criminal conviction. In general terms, their OLS regressions suggest that bullying reduces 9^{th} grade GPA by .13 points, while 2SLS coefficients of bullying impacts reach -1.141 and become insignificant. These authors argue that the strong association between bullying reports and grades makes it difficult to interpret the results: teachers potentially misclassify victims of bullying based on their grades.

The most consistent contribution of this paper to the existing literature is to propose an empirical strategy to estimate the impacts of bullying when true bullying is observed with error. The approach explores 'jumps' of students among bullying histories in each longitudinal year to identify the impacts of bullying. More simply, the model saves reduced-form coefficients from longitudinal regressions to minimize the distance between true and observed bullying coefficients. Another important contribution is to investigate bullying effects on University attendance and on students being not in education, employed or in training (NEET) after high-school completion. A last contribution is to estimate the impacts of different forms of bullying, and by frequency of bullying episodes.

4.2 Empirical strategy

The main argument of this paper relies on the importance of non-classical measurement error on the estimation of bullying effects. Measurement error occurs for several reasons: students can avoid telling others about episodes of bullying because they fear retaliation from bullies, expect isolation from friends hesitant to become next targets, or feel ashamed to report humiliation. Some students may also be unaware that they are victims given that bullying can take indirect forms as spreading derogatory rumours or exclusion from social groups. Similarly, parents and teachers can choose not to report bullying, because they see social isolation as enmity, and physical bullying as aggression or a fight.

This paper proposes a strategy based on Card (1996) to estimate bullying effects when "true" bullying is observed with error. In short, the identification explores switches of students among bullying profiles across longitudinal years to address problems originating from misreporting. It represents an *average treatment on the treated* approach based on the baseline model considering students outcomes in 2008 (two years after high school) on student characteristics as follows:

$$Z_{i2008} = \gamma_0 + \gamma bullied_i + X_i\delta + \tau_s + \tau_y + \varepsilon_i, \tag{4.1}$$

where Z_{i2008} represents the outcome of interest (i.e. NEET and University attendance) for student i in 2008; $bullied_i$ is a bullying indicator equal to one for victimization and zero otherwise. X_i is a set of characteristics from parents (mother's age, if the mother is black, single, if the father works, household size, among others) and students (age, sex, if they were born in UK, if their parents are divorced and scores in GCSE Exams). Importantly, X_i includes controls for special learning needs: attention deficit disorder, memory difficulties, numeracy problems, dyslexia, literacy, and physical problems[2]. Finally, ε_i is an unobserved component of Z_{i2008} . The primary goal is to estimate γ, the parameter of interest.

A straightforward advantage of using Eq. (4.1) is the possibility of including fixed-effects for survey stratum[3] τ_s and high school years τ_y, which

[2]The special learning variables consider; 'Attention deficit' includes: 'Attention deficit', 'hyperactivity disorder' and 'ADH'. The variable 'Memory difficulties' consider 'memory difficulties', 'general' or 'unspecified' learning difficulties. 'Numeracy problems' relates to problems with mathematics, 'Dyslexia' relates to 'Reading difficulties, especially in english'; 'Literacy problems' consider problems in 'literacy', 'expression', 'interaction', and 'communication'. Finally, 'physical problems' are 'Deafness', and 'sigh problems'.

[3]The sample considers survey stratum geographic areas that could not be identified but

allow controlling for time-invariant unobserved characteristics potentially correlated with the incidence of bullying. If one assumes that bullying is determined by time-invariant characteristics of students, the region where they live or time, the inclusion of these fixed-effects provides a reliable estimate of bullying effects when there is no error within bullying reports or when the measurement error is a fixed-effect itself.

To illustrate the implications originating from non-classical measurement error for bullying estimates, consider that the researcher observes 'true' bullying $bullied_i^*$ only using imperfect reports of victimization $bullied_i$. In this case, the relationship between $bullied_i^*$ and $bullied_i$ can be written as:

$$bullied_i^* = \lambda bullied_i + \eta_i \tag{4.2}$$

where η_i is the measurement error in bullying reports. The discussion so far suggests that the accuracy of bullying reports is influenced by either unobservable characteristics of students, parents and teachers or the form of bullying. To check the extend the correlation between $bullied_i$ and η_i in Eq. (4.2) influences bullying estimates, I substitute Eq. (4.2) into Eq. (4.1):

$$Z_{i2008} = \gamma_0 + (\gamma/\lambda)bullied_i^* + X_i\delta + \tau_s + \tau_y + (\varepsilon_i - (\gamma/\lambda)\eta_i), \tag{4.3}$$

A primary implication is that $\mathbb{E}[(\varepsilon_i - (\gamma/\lambda)\eta_{it}), bullied_i^*] = 0$ cannot be assumed because, as shown in Eq. (4.2), $bullied_i^*$ is correlated with η_{it}. It is possible to calculate the attenuation bias from Eq. (4.3) as:

$$\hat{\gamma} = (\gamma/\lambda)[1 - cor(\eta_i, bullied_i^*)], \tag{4.4}$$

where $cor(bullied_i^*, \eta_i)$ is the correlation between the measurement error η_i and $bullied_i^*$, and λ is the correlation between 'true' and observed bullying. Eq. (4) unveils important information: first, for a given $cor(bullied_i^*, \eta_i)$, the higher correlation between 'true' and observed bullying leads to more accurate estimates of bullying effects. In intuitive terms, a better measure of

represent an identified area where students live. Therefore, sutdents with the same stratum live in the same "survey area" bullying reduces the influence of misreporting on impacts estimations. Second, for a given λ, the higher the correlation between the measurement error and the 'true' bullying, the stronger is the bias towards zero (attenuation bias). In an ideal scenario, when perfect measures of bullying are available ($\lambda = 1$) and the correlation of measurement error and true bullying is null $cor(bullied_i^*, \eta_{it} = 0)$, OLS estimates of Eq. (4.2) will produce estimations of bullying effects without the influence of measurement error.

To see what determines λ, consider $Pr(bullied_i = 1|bullied_i^* = 0) = p_0$ the probability of students mistakenly reporting being 'bullied' when it actually has not occurred. In additional, consider $Pr(bullied_i = 1|bullied_i^* = 1) = p_1$ as the probability of correctly assigning $bullied_i$ to victims. Thus, p_0 refers to the 'false positive' and $(1 - p_1)$ is the 'false negative' rates in bullying reports[4].

Now, assume the true probability of bullying in the population equals $Pr(Bullied_i^* = 1) = b^*$, and the observed probability of bullying as $b = P_1 b^* + p_0(1 - b^*)$. So, one can also write $bullied_i = p_1 bullied_i^* + p_0(1 - bullied_i^*)$. Based on these equations, it is possible to estimate the correlation between $bullied_i^*$ and $bullied_i$ as:

$$\lambda = \frac{b^*(p_1 + p_0 - b)}{b(1-b)} - \frac{(b - p_0)(1 - b^*)}{b(1-b)} \times cor(\eta_i, bullied_i^*), \qquad (4.5)$$

As one could expect, Eq. (4.5) indicates that the correlation between 'true' and observed bullying depends on p_0 and p_1, the probability of 'true' and observed bullying, and on the correlation between measurement error and 'true' bullying. If there is 100% probability of assigning victims as victims and 0% probability of assigning victim to non-victims, $p_1 = 1$ and $p_0 = 0$, true and observed bullying are equal ($b^* = b$), and $\lambda = 1 - cor(\eta_{it}, bullied_{it}^*)$. In this case, the estimations of bullying effects would be consistent. Another result from Eq. (4.5) is that in a scenario where $cor(\eta_{it}, bullied_{it}^*)$ is positive

[4]One might wonder whether the probability of a false positive (p_0) is smaller or larger than a false negative $(1 - p_1)$. One could speculate that students are more likely to hide victimization than reporting being victims when it actually did not happen. However, if someone uses reports from parents, part of the literature suggests that parents over-report bullying. Thus, it is possible to have both scenarios: students underreporting bullying $(1 - p_1) > p_0$, and parents over-reporting it, $(1 - p_1) < p_0$

and high enough, OLS estimates can have the opposite sign.

A plausible solution for attenuation bias and selection bias in Eq. (4.2) is finding an instrument that is correlated with observed bullying, but does not directly influence the outcomes of interest. However, the correlation between $bullied_{it}$ and η_{it} makes instrumental variable strategies ineffective in terms of solving measurement error bias. To see why, assume Eq. (4.2) when λ equals 1; $bullied^{*}_{it} = bullied_{it} + \eta_{it}$. When observed bullying equals one, the measurement error would be $\eta_{it} \geq 0$, yet if it is zero, $\eta_{it} \leq 0$. This negative covariance between observed bullying and the measurement error makes IV techniques to produce upward biased estimations of bullying effects (Aiger, 1987; Bollinger, 1996; Black et al., 1999; and Kane et al., 1999). Therefore, considering that OLS estimations suffer from attenuation bias and 2SLS estimates are upward bias, some authors suggest using OLS as a lower bound and 2SLS as the upper bound for the 'true' coefficient.

This paper proposes a two-step procedure to estimate 'true' bullying effects addressing the bias originating from non-classical measurement error. The first step is inspired on Card (1996) and involves estimating reduced-form coefficients for each longitudinal year (2004, 2005 and 2006) in the data considering student's outcomes in 2008. To do so, I generate dummies for bullying histories of students exploring victimization reports across three longitudinal years. So, I generate eight exclusive dummies representing each bullying history; $B_{iv} = V(B_{i000}, B_{i001}, B_{i011}, B_{i010}, B_{i100}, B_{i101}, B_{i110}, B_{i111})$. For example, B_{i001} is a dummy variable equal to one for students reporting being a victim of bullying only in the third longitudinal year, and non-victim in the first and second years. B_{i111} equals one when students report being bullied in all three years, and zero otherwise. All estimations consider B_{i000} as the omitted category (when students always declare being non-victims) and Section IV presents descriptive statistics for each bullying history in Table 4.3.

Now the model uses a similar approch used by Card (1996) to identify the effects of being unionized on wages. In the case of Card (1996) the estimations are based on the survey years, but in our estimates, we consider more convininet to make separte regressions per high school years. The estimations assume an additional relation between students fixed-effects and bullying histories:

$$\tau_i = \omega_0 + \sum_{v=2}^{V} \phi_v B_{iv} + \xi_{iv}, \tag{4.6}$$

where $\mathbb{E}[\xi_{iv}, B_{iv}] = 0$. Finally, the first step substitutes Eq. (5) in Eq. (1) and saves the reduced-form coefficients from the longitudinal regressions in the following:

$$\begin{aligned} Z_{i2008} = {} & \gamma_{02004} + \gamma bullied_{i2004} + X_{i2004}\delta + \tau_s + \tau_y + \phi_{001}B_{i001} + \phi_{011}B_{i011} \\ & + \phi_{010}B_{i010} + \phi_{100}B_{i100} + \phi_{101}B_{i101} + \phi_{110}B_{i110} + \phi_{111}B_{i111} + \varepsilon_{i2004}, \end{aligned} \tag{4.7}$$

$$\begin{aligned} Z_{i2008} = {} & \gamma_{02005} + \gamma bullied_{i2005} + X_{i2005}\delta + \tau_s + \tau_y + \phi_{001}B_{i001} + \phi_{011}B_{i011} \\ & + \phi_{010}B_{i010} + \phi_{100}B_{i100} + \phi_{101}B_{i101} + \phi_{110}B_{i110} + \phi_{111}B_{i111} + \varepsilon_{i2005}, \end{aligned} \tag{4.8}$$

$$\begin{aligned} Z_{i2008} = {} & \gamma_{02006} + \gamma bullied_{i2006} + X_{i2006}\delta + \tau_s + \tau_y + \phi_{001}B_{i001} + \phi_{011}B_{i011} \\ & + \phi_{010}B_{i010} + \phi_{100}B_{i100} + \phi_{101}B_{i101} + \phi_{110}B_{i110} + \phi_{111}B_{i111} + \varepsilon_{i2006}, \end{aligned} \tag{4.9}$$

where Eq. (6) performs OLS estimations for 2004, Eq. (7) estimates for 2005, and Eq. (8) for 2006. All estimations use 'University attendance' and 'NEET' in 2008 as dependent variables. The option for these two outcomes is based on the importance of attending university for skills development and on the incidence of NEET among teenagers outlined in the recent literature. $bullied_{iw}$ is a dummy variable equals to one if parents report bullying and zero otherwise for the respective longitudinal year; dummies for bullying histories B_{iv} come from students' reports and X_i is the set of covariates. Finally, τ_s is the survey stratum fixed-effect, while ε_{iw} is the error-term in each wave w.

The second step minimizes the distance between observed and true bullying using the reduced-form coefficients from the first step. Explicitly, it uses the estimator $B^* = (T^T\hat{\Sigma}^{-1}T)^{-1}(T^T\hat{\Sigma}^{-1}\hat{\pi})$, and calculates the standard errors from the square root of $V = (T^T\hat{\Sigma}^{-1}T)$. Here, $\hat{\pi}$ stacks $3\times(h+k)$ reduced-form coefficients from Eq. (6), (7) and (8); $\hat{\Sigma}$ is a $(3\times(h+k))\times(3\times(h+k))$ block diagonal matrix, where each estimated covariance matrix from Eq. (6), (7) and (8) represents a block diagonal; h is the number of bullying histories; and k indicates the number of covariates.

The matrix T transforms the reduced-form coefficients into 'true' bullying impacts. The essential idea underlying T is to explore 'swifts' of students among bullying histories for each longitudinal year to estimate 'true' bullying impacts. In simple terms, the strategy uses "jumps" to different bullying histories $V(.)$ in each regression to identify the impacts of bullying on labour and schooling outcomes.

Analytically, T stacks the transformation matrixes $\begin{pmatrix} t_w & 0_{((h+1)\times k)} \\ 0_{(k\times(h+1))} & I_{(k\times k)} \end{pmatrix}$ from $w = 2004, 2005$, and 2006; I_k is a $k\times k$ identity matrix, $0_{((h+1)\times k)}$ is a $(h+1)\times k$ matrix of zeros, and t_w is a $h\times(h+1)$ matrix. Thus, using the longitudinal year 2004, the configuration of t_{2004} is composed by the multiplication of two matrixes:

$$\Omega'_{2004} = \begin{pmatrix} 1 \\ \rho_{001} \\ \rho_{010} \\ \rho_{100} \\ \rho_{101} \\ \rho_{110} \\ \rho_{011} \\ \rho_{111} \end{pmatrix} \text{ and } \Phi_{2004} = \begin{pmatrix} 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 & 0 & 0 & 0 \\ 1 & 0 & 0 & 1 & 0 & 0 & 0 & 0 \\ 1 & 0 & 0 & 0 & 1 & 0 & 0 & 0 \\ 1 & 0 & 0 & 0 & 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 0 & 0 & 0 & 1 & 0 \\ 1 & 0 & 0 & 0 & 0 & 0 & 0 & 1 \end{pmatrix}$$

t_{2004} simply multiplies Ω_{2004} and Φ_{2004}: $t_{2004} = \Omega_{2004}\times\Phi_{2004}$. ρ_v in the matrix Ω represents the probability of the bullying history v calculated from our sample. An identical procedure is employed to construct the transformation matrixes for 2005 and 2006. For instance, the difference between t_{2004}, t_{2005} and t_{2006} lays in the first column of Φ; i.e. $\Phi'_{2005}[,1] = \begin{pmatrix} 1 & 0 & 1 & 0 & 0 & 1 & 1 & 1 \end{pmatrix}$

and $\Phi'_{2006}[,1] = \begin{pmatrix}1 & 1 & 0 & 0 & 1 & 0 & 1 & 1\end{pmatrix}$. Once t_w is known, T is a $(3\times(h+k))\times(h+k)$ matrix that stacks $\begin{pmatrix} t_{2004} & 0_{((h+1)\times k)} \\ 0_{(k\times(h+1))} & I_{(k\times k)}\end{pmatrix}$, $\begin{pmatrix} t_{2005} & 0_{((h+1)\times k)} \\ 0_{(k\times(h+1))} & I_{(k\times k)}\end{pmatrix}$, $\begin{pmatrix} t_{2006} & 0_{((h+1)\times k)} \\ 0_{(k\times(h+1))} & I_{(k\times k)}\end{pmatrix}$.

In summary, there are $(8+k)$ parameters to estimate in the second step; the true bullying effect (γ) and another 7 parameters from 'true' bullying histories of students ($B^*_{i001}, B^*_{i011}, B^*_{i010}, B^*_{i100}, B^*_{i110}, B^*_{i101}, B^*_{i111}$) plus k covariates. These parameters come from 21 sample moments (3 waves × 7 bullying histories), 7 proportions of bullying histories and k covariates; compromising $(28+k)$ sample moments.

4.3 Data Description

The data for this paper comes from the *Longitudinal Study of Young people in England* (LSYE). All estimations use questionnaires of bullying from the longitudinal years of 2004, 2005 and 2006, while the dependent variables are University attendance and on not in education, employed or in training (NEET) in 2008. The LSYE encompasses information about households, parents and students across high-school years, and include unique identifiers for students, and schools.

Some characteristics make LSYE a useful survey for studying later impacts of high school bullying. First, the sample of students is large. LSYE interviews around 13,000 students per longitudinal year. This advantage is especially relevant once one compares the limitation in sample size in previous studies. Second, students and parents report victimization, allowing a longitudinal perspective of bullying misclassification[5]. Finally, the victimization questionnaires provide several facets and frequencies of bullying. Altogether, these advantages make it possible to estimate unaddressed ques-

[5]Questions about victimization status are not exactly the same for parents and students. Specifically important for measurement error, the LSYE explicitly uses the word "bullying" with parents while for students this was intentionally omitted. According to some previous studies, such exclusion suggests a positive impact on reporting victimization.

Table 4.1: Students' victimization questionnaire and type of bullying

Student's questionnaire: **"Have any of these things happened to you at your school in the last 12 months?"**	Type of Bullying
01. Called names by other pupils at his/her school	Victim and direct
02. Sent offensive or hurtful text messages or emails	Victim and indirect
03. Shut out from groups of other pupils or from joining in things	Victim and indirect
04. Made to give other pupils his or her money or belongings	Victim and direct
05. Threatened by other pupils with being hit or kicked or with other violence	Victim and direct
06. Actually being hit or kicked or attacked in any other way by other pupils	Victim and direct
07. Any other sort of bullying	Victim
08. No, none of these things have happened in the last 12 months	Non-victim
09. Don't know	Non-victim
10. Don't want to answer	Non-victim

tions in the literature.

All estimations use fixed-effects for the survey stratum. The survey strata were selected using the proportion of pupils in receipt of free school meals in the area. Within each stratum, school selection probabilities were calculated based on the number of pupils in Year 9 from the six major minority ethnic groups highlighted above. Within each stratum maintained schools were ordered and thus implicitly stratified by region then by school admissions policy before selection.

Table 4.1 shows the question and the group of answers used to assign students as 'victims' and 'non-victims'. Table 4.1 also includes an additional column indicating which subcategory the answer belongs to. Answers assigned with "direct bullying" report cases requiring direct interaction between the victim(s) and the bully(ies), and cases of "indirect bullying" do not depend on such interactions, but instead explore environmental or social channels to harm the victims. Naturally, both subcategories are included as general "bullying". Following these criteria, I consider Questions 1, 4, 5 and 6 as 'direct', and Questions 2 and 3 as 'indirect' bullying.

One can dispute the assignment of some questions in Table 4.1. For example, 'being called by names' (Question 1) represents direct bullying, while 'sent offensive text messages' (Question 2) has been assigned as indirect bullying. To justify this choice, I argue that calling someone names demands concrete interaction between the victim and the bully, while 'text messages' uses an intermediary device through which bullying occurs. Another issue is whether 'shut out from groups of friends' (Question 3) is not more closely

Table 4.2: Cross tabulating bullying reports of parents and their children

Panel A: Bullying	Bullying reports for parents		# of
Bullying reports of children	Non-Victims	Victims	Children
Non-victim	23,211	3,939	27,150
	(85.49%)	(14.50%)	
Victim	7,775	6,823	14,598
	(53.26%)	(46.73%)	
Panel B: Indirect bullying			
Non-victim	29,366	1,387	3,753
	(95.48%)	(4.52%)	
Victim	8,579	2,416	1,995
	(78.02%)	(21.98%)	
Panel C: Direct bullying			
Non-victim	31,029	1,775	32,804
	(94.58%)	(5.41%)	
Victim	6,269	2,675	8,944
	(70.91%)	(29.09%)	

Notes: The cross tabulation of bullying status uses answers from reports of parents and their children to the question: "Have any of these things happened at your school in the last 12 months?". Results for indirect bullying in Panel B and direct bullying in Panel C use the subcategories generated in Table 4.1. All percentages are calculated relative to the total number of children in the row.

aligned with direct bullying. In fact, if someone manipulates peers to exclude the victim by direct intimidation, Question 3 has a better match with direct bullying. However, exploring the environment through the consent of others appears to be more closely related to indirect bullying. Assuming it as direct bullying does not change our results in any significant way.

An additional advantage of the LSYE is the possibility of comparing bullying reports from parents and their children. An arguably simple method to check this overlap is by crossing students and parents' answers using the definitions shown in Table 4.1. Table 4.2 investigates this issue and demonstrates the number and percentages of students for each combination of answers.

The results from Table 4.2 indicate two scenarios. First, if on one hand there is a high overlap between parents and children for children declaring

Table 4.3: The rate of agreement of bullying reports between parents and children across time

Bullying histories, according to Students	Bullying history, according to parents' reports 000	100	110	010	101	001	011	111	# of students
000	**.77**	.10	.03	.04	.01	.03	.01	.02	15,474
100	.50	**.26**	.08	.04	.04	.02	.01	.06	5,442
110	.34	.18	**.19**	.07	.05	.02	.02	.14	4,095
010	.55	.11	.09	**.11**	.01	.03	.03	.07	2,808
101	.38	.16	.11	.04	**.09**	.06	.02	.13	1,449
001	.57	.10	.03	.04	.05	**.11**	.03	.07	1,563
011	.44	.11	.07	.09	.03	.07	**.08**	.12	1,569
111	.21	.13	.14	.05	.06	.04	.04	**.33**	4,911
# of parents	20,679	5,190	2,901	1,875	1,164	1,257	810	3,435	37,311

Notes: The bullying histories of parents and students are measured concatenating their bullying reports for the longitudinal waves 1, wave 2 and wave 3 respectively. The percentages present the number of parents agreeing relative to the total number of students in a bullying history profile.

themselves as non-victims of bullying, the percentage of parents reporting bullying when their children report being victims is generally low. As an example, Panel B demonstrates that 95.48% of children reporting being non-victims of indirect bullying have parents who report the same. However, the percentage of parents reporting their children as victims of indirect bullying represents only 21.98% of the children reporting being victims. The second scenario from Table 4.2 is that children report being victims two-three times more frequently than their parents. Overall, parents appear to have an accurate idea for children not experiencing bullying, but a far less precise notion when their children are victims of bullying.

The third advantage of the LSYE is the opportunity to observe bullying reports across time. To explore this advantage, one creates dummy variables equal to one for victims and zero otherwise for each longitudinal year, and then these dummies have been concatenated to generate bullying histories for parents and their children - $B_{hist} = B_{2004}B_{2005}B_{2006}$. Table 4.3 demonstrates the ratio of parents reporting the same bullying history as their children within each bullying history profile. If parents and their children provide the same bullying reports, i.e. always reporting bullying when the other reports, the diagonal in Table 4.3 should be equal to 1 and the off-diagonals equal to 0.

The most consistent evidence from Table 4.3 is that the rate of parents reporting the same bullying history as their children decreases for episodes during the later longitudinal years. For now, consider just the diagonals from the column '100' to '011' in Table III; while .26 parents report the same bullying history as their children in '100', for the subsequent columns, the figure drops from .19 in '110', to .08 in column '011'. This result is in accordance with previous investigations indicating that parents are less likely to provide consistent reports as their children become older, or find it difficult to observe their behaviour during the later years of high school (Olweus, 1993,1994; Whitney and Smith, 1993; Rigby, 1996; Boulton and Underwood, 1992).

As argued for Table 4.2, Table 4.3 reinforces the argument that parents are better at identifying never-victims than always-victims. If for never-victims '000' .77 parents never reported bullying, only .33 parents identify always-victims '111'. Additionally, among always-victims in the row '111', .21 of parents never reported bullying and another .32 only report bullying during the first or second years of high-school. Overall, Table 4.3 indicates that parents are more likely to provide similar reports of bullying when children are younger. In particular, switches of students from non-victims to victims are hard for parents to identify.

Finally, Table 4.4 provides descriptive statistics regarding outcomes and control variables for victims and non-victims according to students' reports in 2004. In short, the results show that victims are attending University less frequently and are not studying, employed or in training (NEET) more frequently during 2008 that non-victims of bullying. Table 4.4 demonstrates that students reporting being non-victims have a slightly higher probability of being from divorced families, and obtain a lower percentage on national exams. Nonetheless, the biggest discrepancies are among victims being more likely to have issues such as dyslexia, literacy, numeracy, or physical problems, memory difficulties, and attention deficit disorder. The remaining variables are similar between these two groups.

Table 4.4: Descriptive statistics for victims and non-victims in 2004

	Victims		Non-Victims	
	Mean	SD	Mean	SD
Dependent variables, longitudinal wave 2008				
University attendance	.441	(.496)	.530	(.499)
Not in education, employed or in training (NEET)	.102	(.302)	.074	(.261)
Control Variables, longitudinal wave 2004				
Male (=1)	.496	(.500)	.498	(.500)
Born in the United Kingdom	.921	(.269)	.917	(.274)
Student's age (in years)	14.326	(.476)	14.326	(.476)
Mother's age (in years)	41.262	(5.65)	41.389	(5.60)
% White mother	.710	(.453)	.634	(.481)
Household size	4.404	(1.41)	4.573	(1.46)
% Single family	.079	(.270)	.076	(.266)
% Divorced family	.123	(.329)	.084	(.278)
% of scores in the national exam	.194	(.336)	.218	(.351)
% Dyslexia	.052	(.223)	.035	(.185)
Special learning needs				
% Literacy problems	.045	(.208)	.028	(.167)
% Numeracy problems	.016	(.126)	.007	(.085)
% Physical problems	.016	(.126)	.012	(.113)
% Memory difficulty	.021	(.145)	.013	(.113)
% Attention deficit	.010	(.103)	.006	(.083)
# of Schools	654		654	
# of Students	6,597		9,173	

Notes: The bullying histories of parents and students are measured concatenating their bullying reports for the longitudinal waves 1, wave 2 and wave 3 respectively. The percentages present the number of parents agreeing relative to the total number of students in a bullying history profile.

4.4 First-stage estimates

As previously mentioned, I use the set of bullying histories of young students as instruments for mother's bullying reports in a instrumental strategy framework. But before hand, it might be worthed to note in each circunstances these instruments would be invalid. The most straightforward case is if mothers adapt their reports based on the *bullying history* of students. Mothers also have a good notion of whether the incidence of bullying in their children has increased or decreased along the high school, so it seems unlikely the mothers would adapt current bullying reports to future student reports of bullying. Another portential source of problems is whether bullying histories of students directly influence the outcome of interest ("University Attendance" or "NEET"). In this case, one hopes that the inclusion of a rich set of covariates and the differentiation in the reduced-form estimation would minimize any potential bias coming from this point.

To provide evidence that our instruments are credible, Table 4.5 shows estimates of the relationship between mothers' bullying reports and students bullying histories during the first year of high school, our set of instruments. Here, as before, I consider the bullying history "001" as a dummy variable representing if students report being bullied only in the final year of high school, while "101" is a dummy variable showing if students reported being bullied in the first and third year of high-school.

The results from Table 4.5 suggest a statistically significant relation between students bullying histories and mothers' bullying reports, which reinforces the idea that mothers and students have a similar notion of what is bullying incidence. It is important to note that the correlation is stronger for students reporting bullyinh during the first years of high school, specifically for "111", "110" and "100" while starts to be insignificant and reduce in size for students reporting bullying later in high school.

Table 4.5: First-stage estimations for the first year in the high school (2004) (Standard errors are clustered at school level)

Students' bullying histories	Coefficients
001	0.023
	(0.024)
011	0.094***
	(0.025)
111	0.419***
	(0.013)
110	0.326***
	(0.015)
100	0.233***
	(0.015)
101	0.287***
	(0.026)
010	0.062***
	(0.018)
Observations	14,854
R^2	0.16

Notes: The bullying histories of parents and students are measured concatenating their bullying reports for the longitudinal waves 1, wave 2 and wave 3 respectively. The percentages present the number of parents agreeing relative to the total number of students in a bullying history profile.
* p<.10, ** p<.05, *** p<.01

4.5 The effects of bullying on labour and schooling

Table 4.6 shows the estimated effects of high-school bullying on University attendance, and not being in education, employed or in training (NEET). All covariates and bullying histories are from the longitudinal years 2004, 2005 and 2006. The dependent variable "University attendance" equals one for students attending University in 2008, and zero otherwise; NEET equals one for students not in education, employed or in training and zero otherwise, also in 2008. Standard errors are clustered at school level, and all regressions include fixed-effects for the survey stratum.

The 'OLS' columns run OLS regressions using bullying reports of parents and covariates from 2004 alongside outcomes from 2008 (i.e. Eq. (6) in Section III). The '2SLS' columns apply two-stage instrumental variables instrumenting bullying reports of parents using the bullying histories of students. Finally, the 'MDE' columns show the results for the minimum distance estimator proposed in Section 4.2.

Table 4.6: The effects of bullying on University attendance and NEET (clustered std. errors)

	Attending University			Not in education, employed or in training, NEET		
	OLS	MDE	2SLS	OLS	MDE	2SLS
Bullying	-.023**	-.034***	-.092***	.009	.026***	.068***
	(.011)	(.013)	(.026)	(.007)	(.008)	(.017)
Includes covariates	yes	yes	yes	yes	yes	yes
Bullying histories	yes	yes	IV	yes	yes	IV
FE: survey stratum	yes	yes	yes	yes	yes	yes
# of students	8,198		8,198	10,998		10,998

Notes: Table 4.5 reports regression estimates for bullying projected to the probability of attending University and not in education, employed or in training (NEET) in 2008. Bullying is a dummy variable equal to one when students have suffered bullying of any form in the previous 12 months, and zero otherwise. The 'OLS' columns use bullying reports from 2004 as a treatment variable, and the '2SLS' columns explore bullying histories of students as instruments for parents' bullying reports. IV indicates that 2SLS used bullying histories of students as instruments for bullying reports of parents. The 'MDE' columns show the results of minimum distance models presented in Section III. All regressions include controls for household size, the student's age, sex, whether the student was born in the UK, mother's age, whether the mother is single, working full-time, divorced, and GCSE exam scores. A set of special learning needs are also considered: attention deficit disorder, literacy problems, numeracy problems, physical problems, and memory difficulties. Standard errors are clustered at school level and reported in parentheses.

* Significance level at $\rho < .10$

** Significance level at $\rho < .05$

*** Significance level at $\rho < .01$.

The results from Table 4.6 indicate significant and adverse impacts of high-school bullying on University attendance and NEET. The 'MDE' column shows that for victims of bullying during high-school the probability of attending University decreases by -3.4 percent compared to non-victims. This estimate is 1.47 larger than the OLS coefficient of -2.3% and around one-third of the instrumental variable estimates in the 2SLS column (-9.2%).

Another result from Table 4.6 is that bullying increases the probability of students being NEETs by 2.6 percent. The OLS regression in this case is not able to detect any effect of bullying, however the '2SLS' column demonstrates coefficients around three times larger than MDE estimations. More importantly, the evidence that MDE estimations lay between OLS and 2SLS presents a positive evidence that our approach addresses biases originating from measurement error.

If on one hand the measurement error in binary variables makes OLS estimates biased toward zero due to attenuation bias (as noted by Aigner, 1973), on the other hand Black et al. (2000), Kane et al. (1999) and Card (1996) demonstrate that the same circumstances cause 2SLS estimates to be upward biased. Therefore, one could use OLS estimates as a lower bound and 2SLS as an upper bound to identify the 'true' coefficient when bullying is a binary variable and reported with error. Evidence from Table 4.6 points exactly to this result.

A similar pattern for OLS and 2SLS estimates is found by Eriksen et al., (2015). These authors identify bullying effects on GPA in the 9^{th} grades by inventively using the proportion of parents with criminal convictions to instrument bullying reports. Their OLS estimates show that bullying lowers 9^{th} grade GPA by .139 points, for instance the 2SLS coefficient increases to -1.141 and becomes insignificant. Furthermore, these authors suggest that a strong association between bullying reports and GPA, or a misclassification of victims based on their grades may explain why the 2SLS coefficients become insignificant and difficult to interpret. Our estimations circumvent biases from misclassification exploring the variation in bullying histories of students for longitudinal regressions, thus obtaining bullying coefficients that are approximately three times smaller than 2SLS estimates.

A variety of channels may explain how being a victim of bullying is a strong predictor of labour and schooling outcomes for young adults. Certainly, the most consolidated evidence comes from psychology that emphasizes the association between early bullying and later damages on emotional skills. This literature indicates that victims of bullying have higher levels of anxiety, are more likely to have depression, and have more mental health problems than non-victims (Takizawa et la., 2014; Bond et al., 2001; Lereya et al., 2015 and all the references therein). Wolke et al. (2013) demonstrate that victims have difficulties staying in the same job for extended amounts of time, suggesting higher instability than non-victims. Victims of bullying are also associated with lower self-esteem and weaker physical health (Fekkes et al., 2006; Rigby, 2003). At the same time, numerous studies demonstrate the crucial role of these traits in relation to job market and schooling decisions (Borghans et al., 2008; Heckman and Rubinstein, 2001; Duckworth and Seligman, 2005; Noftle and Robins, 2007; Waddell, 2006).

Another possible reason why victims have worse outcomes than non-victims is that they expect to suffer new episodes of bullying at University and in the job environment. Glew et al. (2005) observed that bullying often makes students feel as if they do not belong to the school. Based on their evidence, it is plausible that victims of high-school bullying may foresee that bullying is likely to occur in the workplace and at University, thus avoiding these environments.

4.6 Further results

4.6.1 The effects of direct and indirect bullying

This section addresses the distinction made in the literature between direct and indirect forms of bullying. The first form requires interaction between the victim(s) and the perpetrator(s): some examples include stealing, kicking, and yelling at them. The second form does not demand interaction but includes acts exploring social and environmental manipulation (such as social isolation, anonymous phone messages, and spreading false rumours). A detailed discussion regarding both types of bullying can be seen in Björkqvist et al., (1992).

To estimate the effects of both forms, I use the criteria shown in Table 4.1 Section 4.3 to create one dummy for indirect and direct bullying. So, I re-estimate Eq. (6), (7) and (8) in the first step, and the minimum distance estimator replaces 'bullying' for the 'Indirect' and 'Direct' exclusive dummies. Table 4.7 shows the results relating to the impacts of indirect and direct bullying on University attendance and NEET. As long there is evidence of heterogeneous impacts of indirect and direct bullying per gender, Table 4.7

also presents results for boys and girls in separated columns.

Table 4.7: Minimum distance estimations for direct and indirect bullying (clustered std. errors)

	All Sample		Girls		Boys	
	University	NEET	University	NEET	University	NEET
Indirect bullying	-.005	-.001	-.068***	-.029***	-.001	-.011
	(.009)	(.006)	(.015)	(.010)	(.008)	(.007)
Direct bullying	-.041***	.018***	-.031***	.047***	-.043***	.027***
	(.008)	(.006)	(.010)	(.007)	(.012)	(.010)
Includes covariates	yes	yes	yes	yes	yes	yes
Bullying histories yes	yes	yes	yes	yes	yes	yes
FE: survey stratum	yes	yes	yes	yes	yes	yes
# of students	24,594	24,594	24,594	24,594	24,594	24,594

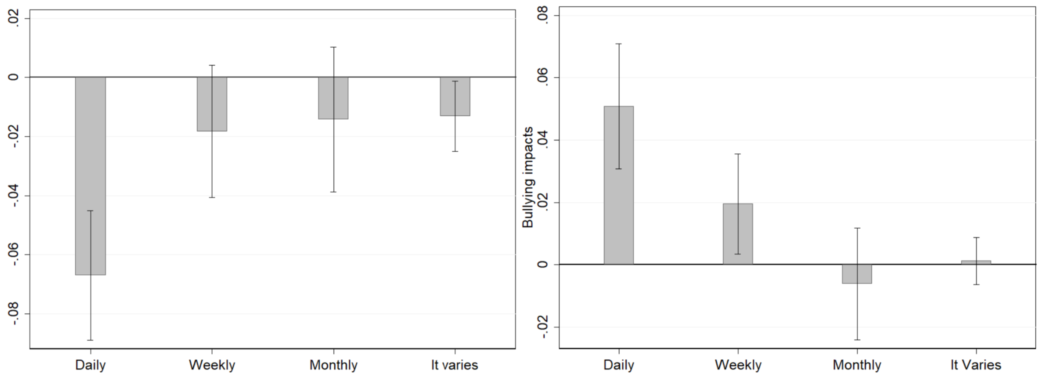
Notes: All results are from minimum distance estimations. Fixed-effects for strata in the sample and bullying histories are included. The variables used in the definitions of indirect and direct bullying can be checked on Table 4.1. Standard errors are clustered by school and reported in parentheses.
*** Significance level at $\rho < .01$.

Table 4.7 demonstrates consistent heterogeneities for indirect bullying effects per gender. The minimum distance estimates for girls show that victims of indirect bullying during high school have -6.8 percent lower probability of University attendance, more than double the effects of direct bullying (-3.1%). For boys, the impacts are statistically insignificant. Surprisingly, indirect bullying decreases the probability of girls to be NEET by 2.9 percentage point.

If on one hand being a victim of indirect bullying affects girls disproportionally, direct bullying seems to have a similar impact on both boys and girls. These findings are aligned with the evidence that girls suffer more than boys from indirect bullying (Björkqvist et al., 1992), but there is no indication that boys suffer significantly more than girls from direct bullying.

4.6.2 What is "repeated" bullying?

This section qualifies "repeated and over time" bullying. For this aspect, I generate four exclusive dummies per frequency of victimization: ‘daily’, ‘weekly’, ‘monthly’ and ‘It varies’. Figure 4.1 plots the coefficients and standard errors of minimum distance estimations including the frequencies of bullying. Figure (A) presents the results for University attendance, and the results for NEET are in Figure (B).



(a) (b)

Figure 4.1: The impacts of bullying by frequency or victimization

Notes: Figure 4.1 plots the coefficients from the MDE regressions in Section 4.2. 'Daily' equals to one when parents report bullying 'Every day' and equals zero otherwise, while 'weekly' considers the responses 'A few times a week' and 'Once or twice a week'. 'Monthly' equal one for parents reporting bullying 'Once every two weeks' and 'Once a month' and zero otherwise, and 'It varies' includes reports of 'Less often than this'. All estimations control for a set of covariates and fixed-effects and cluster standard errors at school level. In borth figures, confidence intervals are 90%.

As one could expect, the higher the frequency of victimization the lower the probability of University attendance and the higher the probability of being NEET after high-school. Notably, the effects of daily bullying at least twice as large as weekly bullying for both outcomes; for example, it lowers University attendance by -6.7 percent in comparison to the insignificant -1.8 parcents of weekly bullying. And for NEET, the results suggest an even clearer decline by frequency. The impact of 'daily' and 'weekly' is bullying is 5.1, and 1.3 percent respectively, while for 'monthly' and 'it varies' it becomes insignificant.

Overall, the results from Figure 4.1 demonstrate a decrease in the impacts of bullying for less frequent episodes. Similar evidence has been found for negative psychological consequences of bullying (Olweus, 1996; Hazler et al., 1991), for decreases in the grades achieved by Italian students (Ponzo, 2013), and higher unemployed rates among Finish (Varhama and Björkqvist, 2005). If one defines 'repeated' bullying when the impacts start to be significant, the evidence of Figure 4.1 indicates 'repeated' bullying occurring at least on a 'weekly' basis.

4.7 Conclusions

Economists, psychologists and educationalists have long reinforced the difficulties in observing bullying at schools. In general, the most common approaches to identify victims are limited to asking students directly, applying victimization questionnaires, and requesting that parents and teachers report episodes of bullying in school. However, there are good reasons to suspect that our capacity to identify victims of bullying is compromised during the use of these approaches.

Firstly, victims may avoid telling others because they fear retaliation if bullies start a reprisal after parents or teachers act for remediation. Alternatively, friends, afraid of becoming new targets, start isolating the victims. Another limitation of directly asking students arises when bullying inherently aims to maintain the act unknown and shield the perpetrator's identity. These situations can involve manipulating the social environment, for instance spreading false rumours, exclusion from social groups, or by sending anonymous text messages to the victim. It is challenging for students, parents and teachers to report such cases as they are not sure whether they have actually occurred. Secondly, students may feel embarrassed about reporting humiliation to a strange interviewer.

Thirdly, the concept of what bullying is may change among informants. Some informants may report aggression as bullying, while others consider it as a 'fight'; some may attribute social exclusion as bullying, others as enmity. Therefore, the association between reported bullying and other unobservable factors makes usual techniques ineffective to obtain consistent estimations of bullying effects.

This paper proposes an empirical strategy to estimate the late effects of bullying when 'true' bullying is imperfectly observed. It minimizes the distance between true and observed bullying by exploring switches of students among bullying profiles across longitudinal years. The first step saves reduced-form coefficients from longitudinal regressions in 2004, 2005 and

2006, while the second step uses a transformation matrix to minimize the distance between 'true' and observed bullying. This transformation matrix 'turns on' for active bullying profiles in a particular longitudinal year, and 'turns off' otherwise.

Some precautions have also been taken to prevent non-random selection of victims. Primarily, regressions control for unobservable characteristics of students with the same bullying history and include fixed-effects for the survey stratum. Another measure is to include a set of controls for special learning difficulties and psychologic indicators arguably correlated with bullying incidence. Finally, I compare the estimations from our approach with OLS, instrumental variable regressions, and using different definitions of bullying and frequencies of attacks to provide evidence that our results are credible.

Four main findings emerge from this analysis. Firstly, the estimations show that victims of high-school bullying have -3.4 parcents less probability of going to University than non-victims. Secondly, the probability of being not in education, unemployed or in training (NEET) increases by 2.6 parcents for victims of bullying. Relating these effects with standard techniques shows that OLS coefficients are two-thirds of the coefficients in our approach, while 2SLS coefficients are around three times larger. The fact that our estimations lay between OLS and 2SLS is a good indication that the measurement error bias is, at least partially, addressed (Bollinger, 1996).

The third finding is that direct forms of bullying such as 'calling names', 'physical aggression', 'stealing' and 'yelling at' have negative effects between 3 and 4 parcents and is relatively similar for boys and girls. However, indirect forms of bullying including gossip, sending hurtful messages, and social exclusion have an exclusive effect on girls' outcomes. This evidence is aligned with the literature that indicates girls using and suffering more from verbal and social bullying, however there is no evidence that boys suffer more than girls in terms of direct bullying. Fourth, these effects are strengthened when bullying occurs more frequently. Overall, although high-school bullying negatively affects the labour and schooling outcomes of young adults, these effects depend on the form and frequency of bullying.

These results have direct implications for anti-bullying policies. Perhaps the most appealing consequence is that observed decreases in bullying incidence can reflect increases in misreporting by victims rather than actual decreases in bullying. This concern can be envisioned by observing the decreases in bullying incidence along high-school years observed in the literature (Olweus, 1993; Boulton and Underwood, 1992). Another implication is that polices focusing on students reporting bullying may incur in great lost by not monitoring reported non-victims that are reluctant or afraid to expose themselves as victims. Further benefits may arise when anti-bullying policies work with parents, teachers and students towards a clear definition of what constitutes bullying. Anti-bullying programs have a lot to gain dealing with these difficulties.

Chapter 5

On the social capital consequences of conditional cash transfers: Evidence from Bolsa Família[1]

A vital feature of social capital is to enable individuals to access opportunities and resources otherwise unavailable (Dasgupta, 1988; Narayan and Pritchett, 1996). Communities socially connected are more likely to overcome imperfections in contract enforcement (Bowles and Ginti, 2002), reduce the asymmetry of information in the job market (Ioannides and Loury, 2004; Granovetter, 1973), lessen the adverse selection and moral hazard in the credit market (Munshi, 2011), and reduce the costs to monitor governments (Putnam, 2000). To understand the mechanisms generating social capital for the poor seem a crucial step forward to pave the road out of poverty.

At the same time, conditional cash transfers have been one of the most used international tools to fight against poverty (Ariel and Norbert, 2009). For instance, despite strong evidence indicating improvements on child and adult health outcomes (Attanasio et al., 2005; Gertler, 2004; Paxson and

[1]This chapter received *Review & Resubmit* from *Journal of Development Studies*

Shady, 2010), schooling (Glewwe and Kassouf, 2012; Schultz, 2004; Glewwe and Olinto, 2004; Macours and Vakis, 2008), crime enrolment (Chioda et al., 2016), labor supply (de Brauw et al., 2015) among others, little is known whether conditional cash transfers programs affect social capital. This paper helps to fulfill this gap by estimating whether becoming a recipient of Bolsa Família, the Brazilian conditional cash program, increases social participation.

One reason why Bolsa Família would increase social capital is by its supplementary services of "Acquaintanceship" and "Ties strengthening". These services foster "strength familial and social ties, incentivize social participation and work towards the feeling of belong and identity within the community" (MDS, 2015). A second channel is that beneficiaries attending health checks could use it as a space for interaction with other beneficiaries. Or enjoying better health enables beneficiaries to participate in further social activities. Bolsa Família could also generate social participation by enforcing the conditionals of school attendance. For example, when schools increase the costs of minimum attendance by providing teaching of bad quality (uninteresting classes for children) or inadequate structure (insufficient seats or classrooms), households receiving Bolsa Família would be more likely to monitor school activities and engage in strategies to fix these problems.

However, simply comparing social participation of recipients and non-recipients of Bolsa Família almost surely produces mistaken conclusions. It reflects the fact that previous differences in observable and unobservable characteristics of households may influence participation in Bolsa Família and social groups that would be erroneously attributed to Bolsa Família. Ideally, to avoid these difficulties would require a random allocation of Bolsa Família to households and then comparing their changes in social capital participation for treatment and control groups. In order to avoid selection bias, this study proposes three adjustments..

First, households are matched according to their probability to participate in Bolsa Família using their propensity score. The propensity score includes covariates crucial in the allocation of Bolsa Família (per capita income and household size, among others) along with dummies per region.

Then, the change in social affiliation rates of beneficiaries before and after Bolsa Família are compared vis-á-vis the change in social affiliation rates of non-beneficiaries before and after Bolsa Família; i.e. it is carried out a propensity score matching difference-in-difference model. A second measure to reduce selection bias re-estimates the model per category of per capita income. The argument is that if Bolsa Família prioritizes households with lower per capita income, estimations matching households with similar per capita income may lessen the observable and unobservable differences between treatment and controls.

Finally, a third adjustment takes into account that households must register into Cadastro Único (Cad. Único) before being eligible to Bolsa Família. Registration is voluntary and free. Thus, if households in Cad. Único have unobservable characteristics that influence the eligibility to Bolsa Família and social affiliation, comparing treated and matched controls in Cad. Único seem to be less susceptible to bias. This strategy has already been used by previous works (de brauw et al., 2014; 2015; 2015b) and robustness checks are carried out to assess its plausibility.

One tests this hypothesis by using data of households for 2005 and 2009 from the Bolsa Família Evaluation Survey (AIBF). AIBF provides information about family composition, income, health conditions, house and neighborhood structure, welfare programs and participation in a list of 11 social groups. Some families were excluded from our analysis because of attrition in locating their addresses (2,553 families) or were not registered in Cad. Único in both waves (1,646 households).

The results indicate that receiving Bolsa Famí increases participation in social groups. In more specific terms, after receiving Bolsa Família, households increase the number of social affiliations by at least .105 percent compared to matched controls. These impacts tend to be larger the lower is the per capita income of households included in the sample. And estimates for the probability of initiating participation on social groups demonstrate a positive effect of Bolsa Família of 6.1 percentage points. Importantly, if it is plausible to consider that the adjustments proposed minimize selection bias, unmatched and unrestricted estimations indicate that selection overestimates the impacts of Bolsa Família on social capital by at least one-fourth.

One also estimates by type of social group. This exercise shows that increases in social affiliation concentrate on: Political Movements by 1.3 percent, Business associations by 1.9 percent, Labour Unions by 3.5 percent, and Education groups by 2.1 percent. Additionally, households receiving Bolsa Família have from 5.1 to 6.7 percent higher probability of accessing informal credit (Credit "Fiado"), but additional estimations are not able to detect any impact on personal networking (measured by the "Number of friends you can count in hard times"). These results implicate that the poorer the households the stronger is social participation, but the effects on the probability of starting participating in social groups seem homogeneous for different groups of per capita income.

Perhaps the most important questioning to these conclusions is whether socially connected households or random shocks that increase the participation in Bolsa Família and social capital are producing artificial coefficients. If socially connected households exert higher pressure in the government to be registered in Cad. Único or households experiencing a drought or a death of a relative search for social support, the probability of participating in Bolsa Família and social groups would similarly increase. Nonetheless, when one re-estimates the models considering how families discovered Bolsa Família[2] and by the occurrence of life and personal shocks[3] , the conclusions remain unaltered. Based on this evidence, I believe there are good reasons to suggest that Bols Família increases social capital participation.

In addition to this introduction, this chapter has the following structure; section II examines the main characteristics of Bolsa Família, and proposes why it influences social participation; section III explains the identification strategy and section IV demonstrates the characteristics of the Bolsa

[2]The AIBF survey asks; "How did you know about Bolsa Família?", and households could answer; "City Council", "Relatives", "Friends", "Neighbors", "Television", "Radio" and "Newspapers", and "Schools", "Social Assistants" and "Clinics". The estimations grouped "Relatives", "Friends" and "Neighbors" as "Personal Networks"; "Television", "Radio" and "Newspapers" as "Media", and "Schools", "Social Assistants" and "Clinics".

[3]The list of shocks considers households reporting; plagues, droughts, living in a divided community, if the husband or the wife dies, divorce, or the occurrence of any social and political discrimination against a member in the household between 2005 and 2009.

Família evaluation survey (AIBF). Finally, Section V examines the implications of the empirical estimates, Section VI shows further results and section VI concludes.

5.1 Bolsa Família and social capital

5.1.1 Bolsa Família: eligibility and main characteristics

The program Bolsa Família started from several modifications from previous smaller social programs. Perhaps its main antecessor is *Programa Bolsa Escola* initiated in 1995 in the city of Brasilia, Brazil's capital. During 1995 and 2003 there were several social programs for different purposes, "Bolsa Gas", "Cartao Alimentacao" and "PETI" for young adults. In 2003 the these programs were unified and transfermed in whats is known today as Bolsa Faília.

There has been some modifications in Bolsa Família in along these years. In 2008 the "*Benefício Vinculádo ao Adolescente*", a benefit attached to household with teenagers with 16 or 17 years old, was included as part of the cash transfer. The payment per teenager is approximately R$ 30. General estimates indicate that approximately 56 million people benefit from Bolsa Família (or 25 percent of the population in 2010). The costs are around 0.5 of Brazilian GDP.

Bolsa Família aims to alleviate poverty by two main channels - (i) the provision of cash transfers to households under certain thresholds of income and (ii) the fulfillment of conditionals. The amount of cash transferred depends on the number of children between 0 and 17 years old and on the per capita income in the households. The conditions focus on prenatal and postnatal care, periodic visits to monitor health and attendance rates for teenagers at school age.

Before being considered as a beneficiary of Bolsa Famía, all households must register in "Cadastro Único" (Cad. Único), a national list of disadvantaged families. The registration is free and works as a national dataset of all poor household which provides characteristics of household and families. The federal government combines maps of poverty to the information in Cad. Único to classify how many and which household should receive Bolsa Família.

The most important criteria to become beneficiary is per capita income. Households earning less than R$100 are automatically eligible for cash transfers due to below the poverty line. "Poor" households are classified by per capita income lower than R$140 per month. The amount of cash transferred is adjustable based on different family compositions (number of children less than five; pregnant or breastfeeding women) and to the level of per capita income. In general, households selected to participate in Bolsa Família receive between R$22 and R$200 per month.

Similar to other CCTs, Bolsa Família has conditionals on the spheres of education and health. In terms of education, children and teenagers between 6 and 15 years must have at least 85 percent of attendance on primary and secondary schools; for older teenagers, the program requests 75 percent of school attendance. In terms of health, local hospitals should provide; full assistance for the vaccination of children, perform periodical medical screenings, and prenatal care for women between 14 and 44 years.

5.1.2 Why would Bolsa Família generate social capital?

Perhaps the strongest reason why Bolsa Família generates social capital is through its supporting programs. In fact, two programs are central; Acquaintanceship Services and Ties strengthening Service (Serviços de Convivência e Fortalecimento de Vínculos). The Ministry of Social Development justifies these services to "strength familial and social ties, incentivize social participation and work towards the feeling of belong and identity within the community" (MDS, 2015).

In the same context, the city council makes accessible to beneficiaries centers named Centro de Referência de Assistência Social (CRAS) that promote activities and meetings within the community, inform about opportunities for social engagement, and spaces for social acquaintanceship between Bolsa Família beneficiaries and the community (MDS, 2015). Therefore, additional to cash transfers and conditionals, participants of Bolsa Família are incentivized to engage on social capital activities.

Another channel which Bolsa Família can influence social capital is through the enforcement of health and education conditionals. Besides the fact that participants gather periodically to have health checks or prenatal care, better health services would enable beneficiaries to participate in additional groups and associations that promote healthier activities. A similar argument applies to education. If schools increase the opportunity costs for households fulfill the conditionals by offering insufficient places or teaching of bad quality, participants of Bolsa Família will be more likely to engage in activities attempting to correct such problems.

5.2 Empirical strategy and estimation

A key empirical issue is how to disentangle the influence of observables and unobservables from the impact of Bolsa Família on beneficiaries' social affiliations. In this matter, we propose four adjustments. The first estimates a participating equation using a probit model including a rich set of covariates (see Table 1.A in the appendix the estimation results for the propensity score and Figure 1.A for its densities for the treatment and control groups). This exercise allows matching treatment and control households sharing similar probabilities of participating in Bolsa Família given their observed characteristics.

A second step performs a difference-in-difference strategy to the matched sample. It compares the change in social affiliation rates of treatment and matched controls before and after Bolsa Família started. The difference-in-difference model is in the following;

$$SC_{ht} = \alpha_0 + \alpha Bolsa\ Família_h + \beta Post + \rho(Bolsa\ Família_h \times Post) + X_{ht}\Phi + \varepsilon_{ht}, \tag{5.1}$$

Where SC_{ht} is the outcome of interest and represent either the number of affiliation in social groups or a dummy variable equal to 1 if households report being affiliated to at least one and zero otherwise. In an effort to follow referential studies (as Putnam, 2000), SC_{ht} proxies for the decision of households h in time t to join an social group and does not take into account the size of these groups or intensity of participation. $Bolsa\ \ Família_h$ equals 1 to treated households and zero otherwise, Post denotes a time dummy for 2009 and ($Bolsa\ \ Família_h \times Post$) interacts both. One advantage of Eq. (1) is controlling for time-invariant unobservable characteristics at household's level that may influence affiliation in social groups and participation in Bolsa Família. Finally, the parameter of interest is ρ.

A key issue, however, is how to disentangle the influence of selection on the observables and unobservables on Îş from the true impact of Bolsa Família on Social capital. Our first adjustment tackles selection on the observables by matching recipients and non-recipients based on their propensity to participate in Bolsa Família. In explicit terms, we use propensity score matching diff-in-diff models to match household receiving Bolsa Família to the most similar non-recipient household, regarding the propensity score.

For that we estimate logit models including variables central for program participation, i.e.; monthly per capita income, number of children under 3, number of children from 5 to 17, non-work income, if the household is isolated, 4 dummies per each region in Brazil (South, North, Northeast, and Southeast), if the household's address is in urban areas, and if living in favelas. The appendix provides the results for propensity score in Table 1A and, as an evidence of common support, the appendix also shows the densities of propensity scores for treatment and control households in Figure 1A.

A second adjustment to address selection on the observables restricts the sample of households by per capita income - the main decision variable in the allocation of Bolsa Família. The first threshold compromises households earning less than R$200 monthly per capita income, the second R$140 and, finally, the third considers households with less than R$100 per capita income. These definitions originate from the official thresholds used by the Brazilian government.

But even though the model compares households presenting close values of propensity score, it seems similarly central to consider why there is still eligible household not receiving Bolsa Família in 2005 and 2009? In other terms, there might be biases arising from unobservables that make eligible households less likely to participate in Bolsa Família that also influence social capital. Before proceeding by proposing a solution to this problem, it is necessary to underpin some points regarding the selection of beneficiaries.

In general terms, the selection of recipients of Bolsa Família involves three steps. In a first stage, the federal government (particularly the Ministry of Social Development) sets quotas for the number of beneficiaries in each city. These quotas are based on "poverty maps" built from national data sets and fiscal constraints. Some authors suggest that including quotas for cities increases the effectiveness of cash transfers by prioritizing the poorest households and by addressing potential moral hazard issues among local authorities (Barros et al., 2008; Lindert et al., 2007). Taken these into account, the second step consists of local governments registering potential beneficiaries in a national registry called Cadastro Único (Cad. Único)[4]. The registration is free and voluntary, and administered at city level. Thus, to be eligible to Bolsa Família all households have, beforehand, to be in included in Cad. Único. Finally, the federal government, in a third step, uses Cad. Único to assign which households should receive the cash transfers.

Now, back to the original question, one reason poor families may not receive Bolsa Família in 2005 and 2009 is because the number of eligible households exceeds the quotas in their city. Actually, de Janvry et al., (2005) demonstrate that it is indeed the case to a great majority of cities in Brazil. In our database, for example, there are 8,225 households (or 63.88 percent) declaring to be non-beneficiaries of Bolsa Família but registered in Cad. Único in 2005[5]. In cases when the number of eligible households exceeds the quota stipulated, the federal government prioritizes households with the lowest per capita income and higher number of children aged between 0 and 17 years.

An additional argument for eligible households to be excluded of partic-

[4]The Cadastro Único performs automatic validation checks with other data sources to verify the validity of all the information provided.

[5]In 2009 there are 2,667 households (or 35.38 or our sample) declaring being registered in Cad. Único but do not receive Bolsa Família.

ipating in Bolsa Familia is the heterogeneous effort made by municipalities to register poor families in Cad. Único (Lindert et al., 2007). If cities making no effort to registry eligible families in Cad. Único are also socially uncoherent to demand additional action from the government, the influence of unobservables on Cad. Único, and ultimately on Bolsa Família, will be incorrectly attributed to the effects of Bolsa Família on social capital affiliation.

The third adjustment tries to account for selection on unobservables by exploring the registration into Cad. Único. We argue that if households that voluntarily registered in Cad. Único present similar unobservable characteristics that influence their eligibility to Bolsa Família and the level of social affiliations, restricting the estimations to households in Cad. Único would lessen biases originating from selection on unobservables at household's level. Previous literature proposes a similar approach in a different context (de brauw et al., 2015; 2014; 2013). Moreover, additional regressions explore how households discovered about Bolsa Família (from the city council, schools, hospitals, friends, relatives, neighbours, TV, Radio, or newspapers) to check whether our conclusions alter by source of information.

Table 5.1 presents balancing tests for the number of social capital affiliations, and the determinants of participation in Bolsa Família. To observe the influence of matching on the balancing test, Panel A simply presents the averages of control and treatment groups while panels B, C, D and E match households according to their propensity score for different levels of per capita income. Panels A and B consider the full sample, that is it; households registered and unregistered in Cad. Único. And Panels C, D and E restrict the sample to households registered. That would give us an indication about the differences of restricting the sample to Cad. Único households.

Not surprisingly, Panel A in Table 5.1 shows significant differences between the averages of treatment and control groups when households are unmatched. This result reinforces the claim for matching to account for selection on observables. Panel B indicates that matching reduces the differences between treatment and control groups but significant differences remain between both groups. Differences between treatment and control groups only become insignificant in Panels C, D, and E when one restricts

Table 5.1: Balancing tests for treatment and controls in the baseline (clustered standard errors)

		Determinants of participation in Bolsa Família		
A. Unmatched (full sample)	Social Capital	Per capita income	Children under 3	Children 5 to 17
Controls	.584	230.44	.217	4.04
Treatment	.534	110.53	.400	4.76
Diff(Treatment - Controls)	.050**	-119.91***	.182***	.72***
B. Matched (full sample)				
Controls	.568	134.36	.423	4.86
Treatment	.534	110.53	.400	4.76
Diff (Treatment - Controls)	-.034*	-23.83***	-.023	-.09**
C. Matched (Households in Cad. Único): Per capita income ≤ R$200				
Controls	.556	78.75	.424	4.82
Treatment	.526	78.38	.418	4.83
Diff (Treatment - Controls)	-.031	.37	.006	.009
D. Matched (Households in Cad. Único): Per capita income ≤ R$140				
Controls	.548	64.75	.437	4.88
Treatment	.507	64.28	.437	4.89
Diff (Treatment - Controls)	- .041	- .47	-.000	.014
E. Matched (Households in Cad. Único): Per capita income ≤ R$100				
Controls	.552	53.66	.449	4.89
Treatment	.511	53.30	.453	4.91
Diff (Treatment - Controls)	-.042	- .35	.004	.02

Notes: Unmatched estimations in Panel A simply compare the averages between treatment and controls. Panels B, C, D and E use the propensity score to match household in the treatment and control groups. "Full Sample" considers all households surveyed in the 2005 and "Households in Cad. Único" restricts to households registered in Cadastro Único either in 2005 or 2009 and excludes those never registered. In all Pansl, the control are households not receiving Bolsa Família from 2005 to 2009, while the treatment group starts receiving only in 2009. The row Diff(Treatment - Control) tests the null hypothesis that the difference in averages of treatment and control groups are zero clustering the standard errors by sector (the proxy for communities). Additional variables included in the propensity score (dummies per region, if the household is isolated, if lives in a slum, non-wage income, and if lives in a urban area) were not shown to keep clarity and do not change our conclusions.

* Signifiance level at $\rho < .10$

** Signficance level at $\rho < .05$

*** Signficance level at $\rho < .01$

the sample to households registered in Cad. Único and earning less than R$200 per capital income.

Overall, the strategy tackles non-random selection by comparing the difference in the trend of social affiliations of households in Cad. Único and with close propensity scores before and after receiving Bolsa Família. So, the treatment group is composed by households receiving Bolsa Família in 2009 but not in 2005 (2,875 households) and the control households do not participate in both periods. The sample excludes households not registered in Cad. Único or that already received Bolsa Família in 2005 (4,705 households, or 30.52 parcents). Additional checks the sensibility of our estimates by how families discovered about Bolsa Família and by personal or neighbourhood shocks between the two waves.

5.3 Data

This study explores panel data from Bolsa Família Evaluation Survey (or AIBF) to estimate the impacts of Bolsa Família on social capital participation. The first wave was conducted in 2005 and comprehends 15,426 households participating or not in Bolsa Família, and registered or not in Cad. Único. Reflecting the geographical concentration poverty in Brazil, the survey oversamples households in the North, Northeast, and Centre-West, and subsamples the Southeast and South . The follow up survey re-interviews 11,433 household from the original sample (i.e. an attrition rate of 6.5 percent per year (de brauw et al., 2007)).

The AIBF collects information from income, education, labour (including child labour), participation in social programs and health expenditure (including anthropometric measures and vaccines). There is a special concern to survey whether the household is registered in Cad. Único, if receives Bolsa FamÍlia, the amount of cash transferred in the last 1.5 year, how did the household discover about Bolsa Família, a set of questions relating to the conditionalities and self-evaluations regarding changes in the household conditions after becoming a beneficiary. This paper assigns treatment and control groups based on answers to these questions (See section V.a).

A whole set of questions inform the materials of walls, floor, ceiling and the number of toiles and bedrooms in the house. There are also variables about the environment where the households are situated; if there exist pavement on the street, sewage, if the government collects trash, if it is a rural area, if live in a favela and which region the household is located. AIBF presents questionnaires on personal and environmental shocks (for example, if the husband or wife died, or there was a flood or drought) between 2005 and 2009. These variables are important to our aims because they might influence the participation in Bolsa Família by provoking negative shocks on income and by also encouraging the engagement in the community. This advantage gives us an opportunity to perform additional robustness checks.

Table 5.2 provides means and standard deviations for the full sample, for households registered in Cad. Único, where the treatment and control groups are derived, and for the attrition. One can observe that 31,9 percent of all households surveyed in AIBF in 2005 were receiving Bolsa Família, while a slightly higher percentage (35,2 percent, in the Column "Restricted Sample") received Bolsa Família among those registered in Cad. Único. Regarding Cad. Único, more than 85 percent of households were already included in the national registry in 2005.

As expected, Table 5.2 shows that the treatment group has lower levels of per capita income and bigger household sizes when compared to the control group. This evidence may reflect the focus of Bolsa Família on poorer households with more children. In general terms, households belonging to the treatment group have fewer rooms and toilets, are more likely to be in favelas and in urban areas than control households. Only 0.6 percent of households in the treatment group have a computer, against 3 percent in control households, while also present lower percentages of possessing radio and television.

A natural concern on longitudinal data comes from attrition in the following up surveys. If one assumes that the absence of a household in the sample is associated with observable and unobservable characteristics that influence participation in Bolsa Família and engagement in social affiliations, there are good reasons to believe that attrition bias impairs any es-

Table 5.2: Descriptive statistics of households during the baseline

		Sample of households registered in Cad. Único in 2005			
	Full Sample	All Households	Treament	Control	Attrition
Bolsa Família	.319	.352	0	0	.266
	(.466)	(.478)	(0)	(0)	(.442)
Cadastro Único	.856	.948	.924	.924	.774
	(.351)	(.222)	(.265)	(.265)	(.418)
Per Capital Income	166.8	140.5	109.8	181.4	231.9
	(467)	(434)	(205)	(356)	(547)
Households Size	4.50	4.62	4.77	4.23	4.23
	(1.86)	(1.83)	(1.86)	(1.70)	(1.97)
Female household head	.365	.366	.355	.367	.371
	(.482)	(.482)	(.479)	(.482)	(.483)
Black household head	.116	.118	.123	.112	.103
	(.320)	(.322)	(.329)	(.315)	(.304)
Live in a slum	.057	.059	.068	.054	.067
	(.232)	(.237)	(.252)	(.227)	(.251)
Urban areas	.196	.198	.234	.162	.180
	(.397)	(.398)	(.423)	(.368)	(.384)
Isolated household	.638	.639	.634	.648	.620
	(.481)	(.480)	(.482)	(.478)	(.485)
# of rooms	5.07	5.01	4.89	5.22	5.02
	(1.72)	(1.67)	(1.65)	(1.71)	(1.81)
# of toilets	.841	.817	.770	.899	.950
	(.511)	(.487)	(.502)	(.469)	(.544)
Has a Radio	.239	.232	.227	.241	.261
	(.427)	(.422)	(.419)	(.428)	(.439)
Has a Television	.877	.875	.860	.896	.877
	(.328)	(.331)	(.347)	(.305)	(.329)
Has a Computer	.0273	.0162	.00626	.0307	.056
	(.163)	(.126)	(.0789)	(.173)	(.230)
# of households	11,427	10,270	2,875	3,776	3,991

Notes: The full sample considers households surveyed in both waves excluding households in the attrition. The restricted sample is formed only by households registered in Cad. Único in either 2005 or 2009. The columns "Treatment" and "Controls" restrict the sample to households not receiving Bolsa Família in the baseline. And finally, the column "attrition" is composed by households not surveyed in the following up.

timation of Bolsa Família effects on social capital. In short, it is central to consider the underlying circumstances that lead to attrition. Here, AIBF has three main determinants for not including households in the follow up survey; firstly, difficulties to locate new addresses; secondly, lack of security; and thirdly, mistrust from the community.

The single most important reason for attrition was the impossibility of locating the addresses of 2,553 households. According de brauw et al., (2011), households living in favelas were specially challenging to track once there is a higher incidence of informal settlement. In addition to that, the follow up wave occurred exclusively in 268 cities covered by the first wave and did not try to localize households moving to cities outside these cities. For all the sample, not tracking households' addresses represents approximately 30 percent of all the attrition; and out of it, 1,256 households moved to municipalities not covered in the 2009 survey.

The second cause of attrition relates to safety in some communities. Even though violence was being expected while planning AIBF (MDS, 2015), insecurity drastically curbed the collection of information. This problem was especially pronounced in areas where drug dealers claimed to control the territory. The final reason why households refuse to participate in the follow up survey was mistrust in the enumerators. There existed a suspicious that enumerators actually wanted to steal information from Bolsa Família's cards and use the cash for their own benefit. To soften such issues, enumerators were advised to ask a neighbor to support AIBF when households were not convinced about its veracity. This measure did not let attrition due to refusal to reach more than 1 percent of households (MDS, 2015).

From column "Attrition" in Table 5.2 it is possible to see that the attrition group was less likely to receive Bolsa Famía or to be registered in Cad. Único in the baseline (e.g. 77,4 percent were registered in Cad. Único against 92,4 percent in the treatment group). They also show the highest per capita income (R$231.9), the highest number of toilets and the highest probability of having a computer at home.

An especially convenient characteristic of AIBF is to provide the information about participation of households in social groups. The survey defines participation if the head of the household reports affiliation or participation in formal or informal of groups of people that gather at least twice per year. A list of social groups with their respective participation rates for treatment and control groups is shown in Table 5.3.The variable "Number of Social Capital Affiliations" adds up all social groups that households report to participate, while "If any social capital affiliation" is a dummy variable equal to zero for households without affiliation and one if the households has at least one affiliation.

Table 5.3: Affiliation in social groups for treatment and control households in the baseline

	Restricted sample of households in the Cad. Único			
"Do the head of household participate"	Treatment		Controls	
"in one of these groups?"	Mean	Std. dev	Mean	Std. dev
# of Social capital affiliations	.532	(.849)	.584	(.890)
If any Social Capital affiliation	.754	(.431)	.780	(.415)
Neighbourhood Groups	.046	(.211)	.044	(.207)
Education Groups	.027	(.163)	.032	(.178)
Cooperatives	.013	(.115)	.012	(.110)
Religious groups	.232	(.422)	.267	(.443)
Business Associations	.004	(.070)	.006	(.082)
NGO	.004	(.070)	.005	(.076)
Political Movement	.006	(.082)	.011	(.106)
Cultural Association	.005	(.073)	.010	(.099)
Labour Unions	.075	(.264)	.076	(.265)
Communitarian Activity	.103	(.304)	.103	(.304)
Another social group	.011	(.105)	.013	(.114)
# of observations	2,609		3,371	

Notes: Table 5.3 calculates participation rates in social organizations for households registered in Cad. Único, a national registry for social programs. The row "Number of Social Capital Affiliation" presents the averages of social affiliations for treatment and control groups in the Baseline and the row "If any social affiliations" shows the percentage of households with at least one social affiliation. Social groups are defined as formal or informal groups of people gathering at least two times per year. Each participation corresponds to the respective last 12 months. For communitarian activities, the definition in the questionnaire is vague, it considers any type of work toward the community and does not specify the kind of activity and nature that should be considered.

Table 5.3 shows a slightly lower participation of households belonging to the treatment group in practically all social organizations during the baseline. The number of social capital affiliations reproduces Table 5.2 Panel A the for unmatched treatment and control groups. Furthermore, a clear evidence from Table 5.3 is a strong participation of households in religious groups. There are 23,2 percent of treated and 26,7 percent of control households participating in religious groups, it is the highest participation rate than any other social group in the list. The second highest participation rate is 10,3 percent for communitarian activities.

On the other hand, business associations, NGOs and cultural associations have no more than 1 percent of household participating in the baseline. The biggest differences arise from affiliation in political movements where controls have also double affiliations than controls (i.e. 1.1 percent against 0.6 percent), and cultural associations and educational groups where households control have twice more participation than treatment households.

5.4 The effects of Bolsa Família on social capital

Table 5.4 presents the estimation effects of Bolsa Família on social capital. Panel A considers the number of social affiliations and Panel B shows the impacts on the probability of participating in any social group. Columns (1) and (2) perform differences-in-differences regressions without matching treatment and control households. Column (1) includes the full sample of households, while column (2) restricts to households registered in Cad. Único either in 2005 or 2009. The columns (3) to (6) re-estimate the diff-in-diff model matching treatment and control households based on their propensity score values. Such regressions allow us to have an idea about the role of selection bias on our results.

Table 5.4 also shows separate results by different levels of monthly per capita income. Column (3) estimates for all levels, column (4) is for households earning less than R$200 of per capital income, column (5) considers households earning no more than R$140 and column (6) limits to R$100 monthly per capita income. These regressions are useful to test the sensibility of Bolsa Família impacts when the level of poverty of treatment and control households vary. Standard errors are clustered at sector level as a proxy for communities.

Table 5.4: The effects of Bolsa Família on social capital affiliation (clustered standard errors)

	Differences-in-differences					
			Matched and restricted sample			
	Unmatched		Levels of per capita income			
	Full	Restricted	All			
PANEL A:	Sample	Sample	levels	< R$200	< R$140	< R$100
# of Social affiliations	(1)	(2)	(3)	(4)	(5)	(6)
Baseline mean	0.499	.498	.498	.492	.482	.482
	(.848)	(.845)	(.845)	(.832)	(.820)	(.814)
Post	-.123***	-.108***	-.132***	-.151***	-.160***	-.171***
	(.024)	(.026)	(.024)	(.028)	(.034)	(.038)
Bolsa Família	-.023	-.026	-.037	-.031	-.041	-.043
	(.022)	(.024)	(.026)	(.028)	(.031)	(.035)
Post × Bolsa Família	.112***	.107***	.077**	.105***	.128***	.137***
	(.027)	(.028)	(.031)	(0.035)	(0.040)	(.045)
R^2	.06	.05	.01	.01	.01	.01
PANEL B: If any social affiliation						
Baseline mean	0.696	.705	.705	.706	.700	.695
	(.459)	(.455)	(.455)	(.456)	(.457)	(.460)
Post	-.160***	-.148***	-.134***	-.132***	-.135***	-.131***
	(.013)	(.014)	(.013)	(.018)	(.022)	(.027)
Bolsa Família	-.030***	-.034***	-.025**	-.011	-.017	.003
	(.011)	(.011)	(.012)	(.013)	(.015)	(.017)
Post × Bolsa Família	.089***	.081***	.065***	.063***	.075***	.061**
	(.015)	(.015)	(.017)	(.021)	(.025)	(.030)
R^2	.07	.07	.01	.01	.01	.01
# of households	14,929	12,838	12,838	9,511	7,273	5,572

Notes: The dependent variable in panel A is the number of social capital affiliations and in panel B is a dummy variable equal to one when households report participating in at least one social activity listed in Table 5.3 and zero otherwise. The AIBF surveys 258 cities in 24 states in Brazil and the baseline is in 2005 and the second wave occurs in 2009. Column (1) performs diff-in-diff estimates for the full sample while column (2) restricts the sample to households registered in Cad. Único. Columns (3) to (6) provide the results from propensity score matching diff-in-diff regression for households registered in Cad. Único. Columns (3) does not limit per capita income, but column (4) limits to R$200 monthly per capita income, column (5) to R$140 and column (6) to R$100 per capita income. All columns comprehend the cofounders presented in Table 5.2 and robust standard errors are clustered at sector level and shown in parenthesis.
$*$ Signifiance level at $\rho < .10$
$**$ Signficance level at $\rho < .05$
$***$ Signficance level at $\rho < .01$

The estimation results from Table 5.4 indicate that receiving Bolsa Família stimulates social capital participation. In panel A columns (3) to (6), household participating in Bolsa Família increase the number of social affiliations from .077 to .137, compared to matched households not receiving Bolsa Família in both periods. These impacts tend to be larger while considering for households presenting lower per capita incomes (Columns (5) and (6)). For example, when households earning less than R$100 per capita income monthly receive Bolsa Família, their number of social affiliations increase by .16 standard deviations compared to households .09 standard deviations for households on column (3).

Panel B tells practically the same story. The results suggest that participation in Bolsa Família increases the probability of affiliation in social groups from 6.1 to 7.5 parcents. However, differently from panel A, Bolsa Família impacts do not have a clear pattern for different per capita income thresholds. Perhaps it indicates that Bolsa Família increases the intensity of social participation according to per capita income but incentivizes primary participation similarly across households. The fact that Bolsa Família still affects social capital after constraining the sample to Cad. Único households, matching treatment and controls, and performing separate estimates by per capita income, seems a good indication that our conclusions are credible.

A possible way to qualify our results is speculating the extent of selection bias. One way to tackle this issue is comparing the coefficients from column (1), the baseline model, with estimations from column (2), which only considers households registered in Cad. Único, and from columns (3) that performs propensity score matching diff-in-diff for Cad. Único households. From this exercise, it is possible to observe that restricting the estimations to Cad. Único households in column (2) reduces Bolsa Família effects from 4.4 to 8.9 parcents than unrestricted-unmatched models in column (1).

For instance, a stronger reduction occurs when one compares the unrestricted-unmatched model in column (1) with restricted-matched estimations in column (3). In this case, the impact of Bolsa Família becomes from 26.9 to 31.25 percent smaller. Therefore, if it is plausible to assume that the three adjustments proposed help to minimize selection bias in Bolsa Família effects, results from our regressions indicate that the selection process of beneficiaries overestimate the impacts of Bolsa Família on social capital by at least one-fourth.

To provide better understating about which social groups households are engaging after receiving Bolsa FamÍlia, Table 5.5 re-estimates matching diff-

in-diff models for a list of 11 social groups. Each row represents the diff-in-diff coefficient ($Post \times Bolsa$
$Família$) from a separate regression using a dummy variable per social group as dependent variable at time. Four groups seem to increase social affiliation, to wit; Political Movements (line 03) by 1.3 parcent, Business associations (line 05) by 1.9 parcent, Labour Unions (line 06) by 3.5 parcents, and Education groups (line 08) by 2.1 parcents.

Table 5.5: Propensity Score Matching Diff-in-Diff coefficients per type of social affiliation (Clustered Standard Errors)

		Households registered in Cad. Único and per capita income < R$200	
List of social capital groups	Control means	Post × Bolsa Família	Clustered Std. Errors
01. Communitarian activity	0.094	.006	(.018)
02. Religious Group	0.241	.025	(.023)
03. Political Movement	0.013	.016***	(.005)
04. Cooperative	0.008	- .012	(.008)
05. Business Association	0.007	.019***	(.006)
06. Labour Union	0.084	.035**	(.016)
07. Neighbourhood Association	0.052	.008	(.015)
08. Education Group	0.030	.021***	(.008)
09. NGO	0.005	.003	(.004)
10. Cultural Association	0.005	.003	(.003)
11. Another group	0.014	.012*	(.007)

Notes: The estimates in Table 5.5 is for households earning less than R$200 of per capita income monthly, and only for households registered in Cad. Único. Each row represents a separate propensity score matching diff-in-diff regression at the time. The coefficient $Post \times Bolsa \quad Família$ represents the changes in the participation rates of households receiving Bolsa Família relative to the changes in participation rates of households not receiving the benefit. Standard errors are clusters at sector level and shown in parentheses.
$*$ Signifiance level at $\rho < .10$
$**$ Signficance level at $\rho < .05$
$***$ Signifiance level at $\rho < .01$

The mechanism under which households receiving Bolsa Família engage in these social groups might be diverse. These groups can provide the social environment necessary for poor households to share innovation. Or the higher probability of affiliation in labour unions, business association and education groups could represent a further interest in using social groups as safety nets during economic instability. Another mechanism is that house-

Table 5.6: Estimations of Bolsa Família's effects on alternative measures of social capital (Clustered Standard Errors)

	Panel A: "Number of friends you can count on hard times				Panel B: "Have you ever used credit "Fiado" (1 = if yes)			
	All levels (1)	< R$200 (2)	< R$140 (3)	< R$100 (4)	All levels (1)	< R$200 (2)	< R$140 (3)	< R$100 (4)
Post	.047	-.003	.43	-.80	-.14***	-.13***	-.14***	-.14***
	(.31)	(.68)	(1.10)	(.69)	(.014)	(.018)	(.022)	(.028)
Bolsa Família	-1.53	-1.39	-1.47	-.58	-.027*	-.008	-.007	.006
	(.96)	(.92)	(1.10)	(.73)	(.014)	(.015)	(.017)	(.019)
Post × Bolsa Família	-.341	-.411	-.832	.358	.057***	.051**	.067**	.061*
	(.35)	(.70)	(1.12)	(.74)	(.018)	(.022)	(.026)	(.031)
# of households	11,506	8,479	6,469	4,936	12,838	9,511	7,273	5,572
R^2	.00	.00	.00	.00	.01	.01	.01	.01

Notes: The coefficient Post × Bolsa Família represents the impacts of Bolsa Família in each social capital participation. Standard errors are clusters at community level and shown in parentheses.

holds receiving Bolsa Família are better able to avoid exploitative political relationships (Hunter and Sugiyama, 2014; Fried 2012) and engage into political movements to demand further than basic needs.

It is fair to question the results presented so far arguing that they do not account for personal networking, since they are not necessarily intermediated by social groups (e.g. Charles and Kline, 2006; Putnan, 2000, Granovetter, 1973, Kramarz and Skans, 2014). To provide further evidence on this issue, one provides two additional results. The first comes from estimating the effects of Bolsa famí on the number of friends "you can count in hard times", while the second includes the probability of using credit "Fiado" in the previous 12 months as a dependent variable. Credit "Fiado" represents an informal transaction where individuals engage in a credit operation without a formal contract. Such transaction is commonly based on "mutual trust" between the borrower and the lender, does not have any legal support and the payment is informally established by the two. One assumes that households need to have higher social capital with the lender in order to access a form of credit based on mutual trust. Table 5.6 presents the estimation results for both outcomes using a propensity score matching diff-in-diff.

Table 5.6 panel A indicates that the effect of Bolsa Família on social group affiliations does not seem to be followed by increases in the number of friend that households "can count in hard times", our proxy for personal networks. Panel A shows insignificant coefficients for all estimates and levels of per capita income. This result may reflect that the question "How many friends you can count in hard times?" was done only in 2009, while for 2005 was obtained retrospectively during the interviews in in 2009. So, this fact can originate memory bias in the number of friends reported for 2005.

On the other hand, Panel B Table 5.6 demonstrates that Bolsa Família increases the probability of accessing credit "Fiado" in at least 5 percent. Similar to our previous results, higher access to informal credit arises for when one compares households in lower per capita income categories in columns (3) and (4). Along with increases in the social capital, another explanation for higher access to information credit is that households' participants of Bolsa Família have a continuous inflow of cash. Borrowers can be using this fact as a collateral in informal credit contracts.

5.5 Further results

5.5.1 How did you know about Bolsa Família?

Previous evidence indicates that Bolsa Família increases the number and the probability of affiliations in social capital groups, and intensify the use of informal credit. However, there is still the possibility that simultaneity between social capital and participation in Bolsa Família is biasing these conclusions. If socially connected communities are more favorable to redistributive policies and exert higher pressure on the local governments to register poor households in Cad. Único, the probability of participating in Bolsa Família would similarly increase. In this scenario, participation in Bolsa Família is a consequence of higher social capital and connected communities rather than the opposite as our model assumes.

Here, one process as follows. As argued above, municipalities led several aspects of implementation which reflects in high heterogeneity in this service (Lindert et al., 2008). So, firstly I explore a question in the AIBF asking; "How did you know about Bolsa Família?", where the responses include; "City Council", "Relatives", "Friends", "Neighbors", "Television", "Radio" and "Newspapers", and "Schools", "Social Assistants" and "Clinics". Secondly,

Table 5.7: Estimating effects by different source of information about Bolsa Família (clustered standard errors)

	"How did you know about Bolsa Família?"			
Panel A:	City	Personal		Schools, Clinics,
# of social affiliations	Council	Networks	Media	Social workers
Post	-.160***	-.155***	-.154***	-.154***
	(.030)	(.029)	(.029)	(.028)
Bolsa Família	-.040	-.037	-.051*	-.070**
	(.031)	(.030)	(.031)	(.029)
Post × Bolsa Família	.198***	.107**	.148***	.088**
	(.056)	(.042)	(.050)	(.043)
Panel B: If has any social affiliation				
Post	-.136***	-.135***	-.136***	-.136***
	(.018)	(.018)	(.018)	(.017)
Bolsa Família	-.003	-.013	-.011	-.028*
	(.014)	(.014)	(.015)	(.014)
Post × Bolsa Família	.086***	.049**	.068**	.090***
	(.031)	(.025)	(.027)	(.026)
# of observations	6,708	7,196	6,876	7,082

Notes: Each column of Table 5.7 considers a different group of responses to "How did you discovered about Bolsa família?". It also uses answers provided in both waves, so households responding to have known about Bolsa Famía from the City council and watching "Television" are included in both regressions. The estimations match treatment and control groups earning less than R$200 per capita income based on the value of the propensity score. Standard errors are clustered by sector.

I grouped; "Relatives", "Friends" and "Neighbors" as "Personal Networks"; "Television", "Radio" and "Newspapers" as "Media", and "Schools", "Social Assistants" and "Clinics" and, thirdly, re-estimate the matching models for each group of answer. The results are presented in Table 5.7.

In general, Table 5.7 indicates that even when one compares households discovering Bolsa Família from the same source, the effects of Bolsa Família on social capital affiliation persist significant. Table 5.7 also shows that the highest impacts come from comparing treatment and control groups who discovered Bolsa Família through the City council where there is an increase in the number of social affiliations by .198 (or .12 standard deviations) compared to matched controls. The same comparison lead to an increase of 8.9 parcents in the probability of having a social affiliation, according to Panel

B. Note, these regressions compare households discovering Bolsa Família from the same sources, so if unobservables at municipality level affects social affiliation and participation of households in Bolsa Família similarly, the diff-in-diff estimates cancelled out selection bias at municipality level.

Finally, the lowest impacts of Bolsa Família arise from the column "Personal Networks", i.e. when households discovered Bolsa Família because of relatives, friends or neighbors. Treatment households who discovered Bolsa Família from relatives, friends and neighbors present an increase in the number of friends by .107 and by 4.9 percent in Panel B. Both coefficients are only half of the estimates for households discovering about Bolsa família from the City council. These relative smaller effects might indicate that households discovering about Bolsa Família from relatives, friends or neighbors have stronger social connections making the marginal impacts of Bolsa Família smaller.

5.5.2 Experiencing shocks and migration

Another relevant questioning for our conclusions is whether our results are not an artefact of random shocks influencing Bolsa Família and social participation in the same way. For example, households experiencing a flood or a drought between the two waves may face negative shocks on per capita income that rise the probabilities of receiving Bolsa família in the second wave and incentivize these households to look for support from social capital groups. In these circumstances, participation in social capital groups and the likelihood of receiving Bolsa Família would have a positive correlation even though the actual reason for higher social engagement is experiencing the shock.

On this matter, one explores a list of shocks occurring between 2005 and 2009. A first group considers households that reported suffering; plagues, droughts, or living in a divided community. One argues that these shocks may influence per capita income and participation in Bolsa Família because plagues and droughts affect the crops of families while a divided neighborhood may reduce social affiliations. A second group includes; if the husband or the wife dies, divorce, or the occurrence of any social and political discrimination against a member in the household. Table 5.8 shows the coefficients for these groups, and includes in a third panel for households that migrated between the waves. Standard errors are clustered in parenthesis.

Table 5.8: The effects of Bolsa Família per type of shock and migration (clustered standard errors)

	# of Social Affiliations		If any social affiliation	
Panel A: There was any plague or drought in the crops, or live in a divided community?				
Yes ($N = 3,120$)	.175***	(.066)	.100***	(.035)
No ($N = 7,081$)	.073*	(.041)	.049*	(.025)
Panel B: Did your husband or wife died? Divorced? Did anyone suffer social or political discrimination?				
Yes ($N = 1,573$)	.139*	(.079)	.119**	(.055)
No ($N = 8,628$)	.093**	(.038)	.052**	(.023)
Panel C: Did the household move from 2005 to 2009?				
Yes ($N = 2,416$)	.172*	(.088)	.063	(.060)
No ($N = 7,785$)	.119***	(.041)	.063**	(.025)

Notes: Table 5.8 presents the estimations results for matching diff-in-diff for environmental shocks in Panel A, to life shocks in Panel B only for households earning less than R$200 per capita income monthly. To provide additional checks, one also includes in Panel C separate regressions for households who moved, but stayed in the same city, between the two waves. Standard errors are clustered in parenthesis.

The estimation results from Panel A and B in Table 5.8 show that, regardless experiencing shocks, households still have higher number and probability of participating in social capital groups after receiving Bolsa Família. It is worthy to mention that the household experiencing shocks have approximately twice larger effects than otherwise. Panel C demonstrates that either households who moved or not have significant increases in social capital, .172 and .119 respectively, but only those who did not move present higher probability of participation in their first social group. Therefore, even considering the source of information, whether there was any personal and environmental shock or migration among households, it is still significant the effects of Bolsa Família on social capital.

5.6 Conclusions

Despite conditional cash transfers aim to promote economic relief, whether households participating in such programs engage on social activities has been overlooked in empirical evaluations. However, the Brazilian conditional cash transfer program Bolsa Família has supplementary services to "strength familial and social ties, incentivize social participation and work towards the feeling of belong and identity within the community" (MDS, 2015) that make a strong case to hypothesize that the program may induced to social participation.

In an ideal scenario, to test this hypothesis would require comparing the social capital of households randomly assigned to treatment and control groups. In other terms, the effect of Bolsa Família on social capital could be measured by randomly allocating households from non-recipient to recipient status and observing whether there are any significant differences in the social capital among these two groups. Not surprisingly, that is not the case for Bolsa Família. This paper applies three adjustments to minimize the influence of selection bias of Bolsa Família beneficiaries. To account for selection on the observables, the first adjustment performs matching diff-in-diff estimates comparing treatment and control households matched by their values of the propensity score before and after receiving Bolsa Família. The propensity score is calculated considering the probability of receiving Bolsa Família during the baseline conditioned on variables used for selection into the program (per capita income, number of children and region). A second adjustment restricts the estimates by levels of per capita income. Thresholds of per capita income were based on the criteria used by the government to select beneficiaries, to wit; households under R$200 per capita income, under R$140 and under R$100. These estimations allow comparing households in treatment and control groups that are more likely to participate in Bolsa Família as per capita income decreases.

A third adjustment aims to reduce the influence of selection on unobservables. For that, one restricts the estimations to households registered in Cadastro Único, a national registry of disadvantaged families. It considers that the registration is voluntarily and free and assumes that households registered in Cad. Único have similar unobservables that affect eligibility into Bolsa Família and social affiliations. A similar approach has been used in past research (de brauw et al., 2015; 2014; 2013). Additional robustness checks check the validity of this assumption re-estimating our equations by how families discovered about Bolsa Família.

Using a two-wave panel with information about characteristics of households, neighborhood and affiliation in social groups, our estimations are able to uncover several findings. The most consistent finding is that households increase social participation by at least .09 standard deviations after receiving Bolsa Família relative to matched households also registered in Cad. ÃŽnico but not receiving the benefit. The impacts tend to increase the lower the per capita income in the household, reaching .16 standard deviations for households earning less than R$100 per capita income monthly. Another important result is that regressions not accounting for selection bias, i.e. unmatched and unrestricted to Cad. Único households, overestimate the effects of Bolsa Família by at least one-fourth.

Likewise, this paper performs estimation per type of social affiliation. These exercise shows that social affiliations increase on Political Movements by 1.3 parcent, Business associations by 1.9 parcent, Labour Unions by 3.5 parcents, and Education groups by 2.1 parcents. Additional regressions quality these conclusions demonstrating that households receiving Bolsa Família have from 5.1 to 6.7 percent higher probability of accessing informal credit ("Fiado Credit"), but it is not possible to detect any changes in the "number of friends you can count in hard times". These results indicate that the increases in the affiliation on social organization were not followed by a wider personal networking.

Robustness checks use responses from "How did you know about Bolsa

Família?"; "City Council", "Relatives", "Friends", "Neighbors", "Television", "Radio", "Newspapers", "Schools", "Social workers" and "Clinics" to re-estimate our adjusted model per source of information regarding Bolsa Família. In general terms, even comparing households discovering about Bolsa Família using the same source, the effects on social capital persist. And final robustness check argues that if environmental experiences (plague or drought) affects negatively per capita income, increasing the probability of participating in Bolsa família, and at the same time induce households to search for social support, our estimates would be capturing the association between Bolsa Família and social capital caused by these shocks.

Estimations considering households by personal (Husban or Wife died, Divorce, political and social discrimination) and environmental shocks (plagues, droughts, divided community) demonstrate that regardless if households experience shocks, they still have higher number and probability of participation in social capital groups after receiving Bolsa Família. In fact, experiencing a shock produces a twice as larger effect than otherwise. Stratifying the estimations by households who moved or not also lead to the same conclusion.

Chapter 6

Final remarks

This thesis provides empirical evidence for designing policies against poverty. Four specific ideas are covered; the importance of in-utero experiences on birth outcomes, that it is possible to reduce unnecessary C-section by promoting information disclosure about physicians' performance, how much is lost if any attention is spent to help students not reporting being victims of bullying, and finally, that the conditional cash transfer Bolsa Família influences engagement in the community. The intention is not to provide a "magic" list of policies. The real focus is on suggesting evidence driven policies to flourish opportunities.

There are several contributions along this thesis. The second chapter is the first attempt in economics to estimate the impacts of intrauterine exposure to reductions in crime on birth outcomes. The methodological innovation relies on exploring the pacification dates of delineated areas in favelas of Rio de Janeiro to overcome selection bias arising from families selecting the safest place possible to have their baby, or by deciding when is the best time to start a pregnancy. The conclusions demonstrate that each month of intrauterine exposure to pacification increases birth weights by 4 grams and reduces the probability of low birth weight by 1.2 percent.

Patients rely on physicians' expertise to recommend the most suitable service given their clinical conditions. Based on this notion, governments have been promoting disclosure of information about physicians' performance arguing that patients are empowered to select the best physician possible. However, such policies commonly do not account that physicians also respond information disclosure. This thesis uses in chapter three a legislative change in Brazil making mandatory for private hospitals to disclose information about physician's performance and shows that it reduced C-section rates by 4.8 parcents. For instance, two-thirds of this effect originates from physicians anticipating to information disclosure. Special care should be taken on the anticipatory responses of agents while designing such type of policies.

Another barrier in designing public policies is that people tend to misreport discrimination, prejudice and humiliation (Heckman, 1998). In the case of bullying, students might misreport being victims because are afraid of future retaliation of bullies, isolation from friends or are hesitant to look embarrassed reporting being victims. Asking a third individual may incur in heterogeneous notions of what bullying is and provide inconsistent reports. Chapter four tackles this problem using longitudinal regressions of bullying reports to minimize the distance between observed and true bullying and estimates the effect of bullying on schooling and labour after higher school. The results show that high-school bullying decreases University attendance by 3.4 parcent and increases the probability of being not in education, employed or in training after high-school by 2.8 percent. Neglecting misreporting cut such estimates by approximately two-thirds.

Finally, socially connected families have better changes to emerge out of poverty (Putnam, 2000). Yet there is still fairly limited evidence of what types of public policies generate social capital. Chapter five proposes to test whether the Brazilian conditional cash transfer program Bolsa Família affects social participation. To account that Bolsa Família is not assigned randomly, chapter five adjusts the estimations by matching households with similar propensity scores, imposing thresholds of per capita income, and by restricting the sample to households registered in Cadastro Único. In general, the results suggest that social affiliations increase by .09 standard deviation, or 6.1 parcents, for households receiving Bolsa Família compared to matched controls.

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Supplementary Materials

Babies and Bandidos: Birth Outcomes in Pacified Slums of Rio de Janeiro

Table 6.1: Pacification dates per favela in Rio de Janeiro

Pacifying Police Units (UPPs)	Announcing dates	Pacification Dates	Control neighbourhood for each UPP
Santa Marta		19/12/2008	Botafogo
Batam	12/17/2008	16/02/2009	Padre Miguel and Realengo
Cidade de Deus		18/02/2009	Curicica, Pechincha, Gardênia Azul and Freguesia
Chapéu-Mangueira	05/12/2009	10/06/2009	Leme
Pavão-Pavãozinho		23/12/2009	Ipanema and Copacabana
Tabajaras		23/12/2009	Botafogo and Copacabana
Providência		21/03/2010	Santo Cristo, Cidade Nova, Saúde, Centro and Gamboa
Borel	04/26/2010	28/04/2010	Grajaú, Tijuca and Andaraí
Formiga	05/03/2010	28/04/2010	Grajaú, Tijuca and Andaraí
Salgueiro		28/04/2010	Grajaú, Tijuca and Andaraí
Andaraí		15/06/2010	Andaraí, Méier, Grajaú, Engenho de Dentro
Turano		15/08/2010	Rio Comprido and Tijuca
São João		15/10/2010	Engenho Novo, Sampaio
Alemão		28/11/2010	Complexo do Alemão
Penha		28/11/2010	Complexo da Penha
Mangueira		19/06/2011	Mangueira
Rocinha	11/04/2011	13/11/2011	Leblon, Gávea and Sao Conrado
Vidigal	11/04/2011	13/11/2011	Leblon, Gávea and Sao Conrado
Arará/mandela		11/10/2012	São Cristovão, Maracana, Maria da Graça, Higienópolis, Benfica and Cachambi
Jacarezinho		11/10/2012	São Cristovão, Maracana, Maria da Graça, Higienópolis, Benfica and Cachambi
Manguinhos		11/11/2012	São Cristovão, Maracana, Maria da Graça, Higienópolis, Benfica and Cachambi
Barreira/Tuiuti		03/03/2013	São Cristovão
Cajú		03/03/2013	Cidade Universitária
Cerro-Corá		29/04/2013	Laranjeiras
Camarista Meier		07/10/2013	Lins de Vasconcellos, Méier, Água Santa, Engenho de Dentro
Lins Vasconcelos		07/10/2013	Lins de Vasconcellos, Méier, Água Santa, Engenho de Dentro

Notes: There is a time gap between the pacification dates and the implementation of Pacifying Police Units (UPP) in the favelas. We opt to use pacification dates instead of the implementation of UPPs because when the pacifications took place, the police, army and marine stay in the territory policing until the government had resources and the bureaucracy was solved to implement the pacifying police Units. However, to be included in the sample, we use the official limits of UPPs.

Table 6.2: Characteristics of pacified favelas in Rio de Janeiro

List of Pacifying Police Units (UPPs)	# of UPPs	# of Communities	Population size	# of Households
Santa Marta	1	1	3,908	1,176
Batam	1	6	12,811	3,734
Cidade de Deus	1	11	47,795	14,742
Chapeu Mangueira	1	2	3,739	1,178
Pavao-Pavaozinho	1	2	10,338	3,268
Tabajaras	1	6	4,239	1,400
Providencia	1	4	4,889	1,465
Borel	1	6	12,811	3,734
Formiga	1	1	4,312	1,279
Salgueiro	1	2	3,345	926
Andarai	1	6	9,685	2,993
Turano	1	12	12,215	3,438
Sao Joao	1	5	7,035	1,952
Complexo do Alemão	4	15	60,555	18,226
Penha	4	11	48,559	13,060
Mangueira	1	5	14,589	4,311
Rocinha	1	2	71,080	23,970
Vidigal	1	2	10,371	3,448
Arara/mandela	2	13	44,051	13,143
Jacarezinho	1	18	39,041	11,538
Manguinhos	2	13	44,051	13,143
Barreira/Tuiuti	1	4	13,667	4,472
Caju	1	9	16,117	5,122
Cerro-Corá	1	3	2,805	779
Camarista Meier	2	17	20,550	5,685
Lins de Vasconcellos	2	17	20,550	5,685
Total	36	193	543,108	163,867

Notes: All information of UPPs comes from Census 2010 and was constructed by Institute Pereira Passos.

List of Maps of UPP boundaries

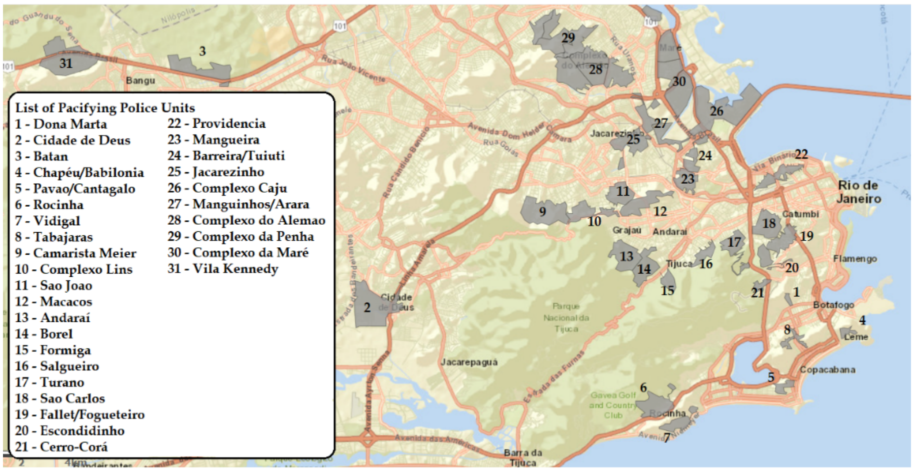


Figure 6.1: Pacified Areas

Figure 6.2: Upp Dona Marta, Upp Tabajaras and Chapéu Babilôlia

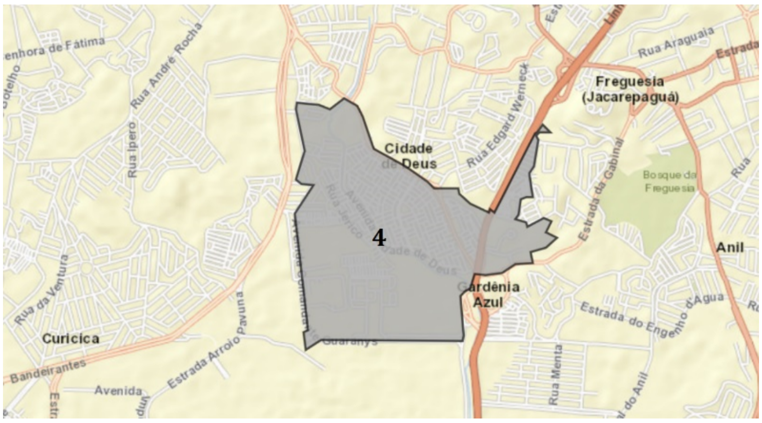


Figure 6.3: Upp Cidade de Deus

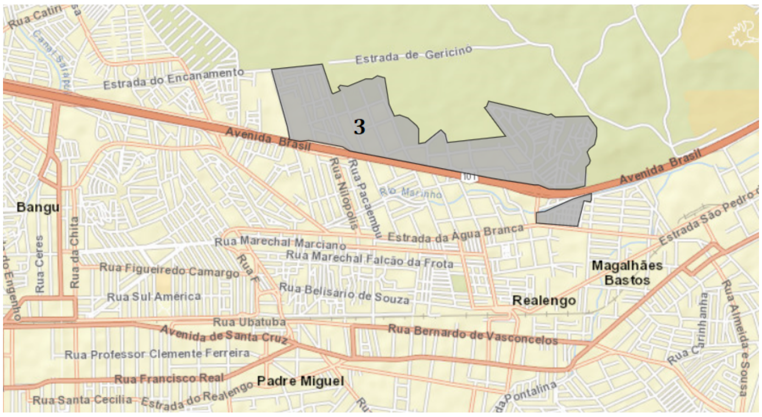


Figure 6.4: Upp Baran

Figure 6.5: Upp Pavão-cantagalo

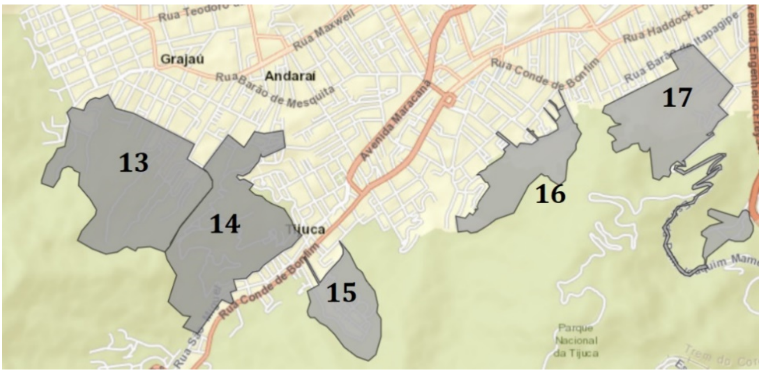


Figure 6.6: Upp Andaraí, Borel, Salgueiro and Turano

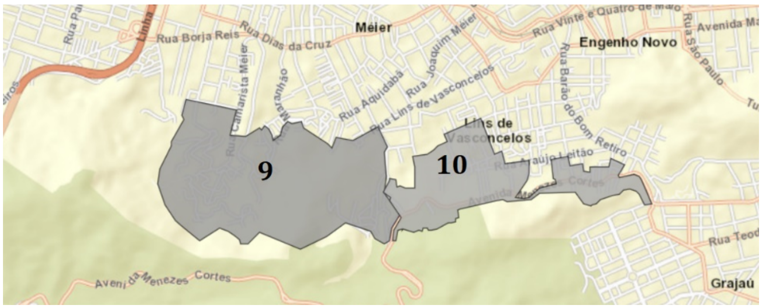


Figure 6.7: Upp Lins and Camarista Meier

Figure 6.8: Upp Cerro-Corá

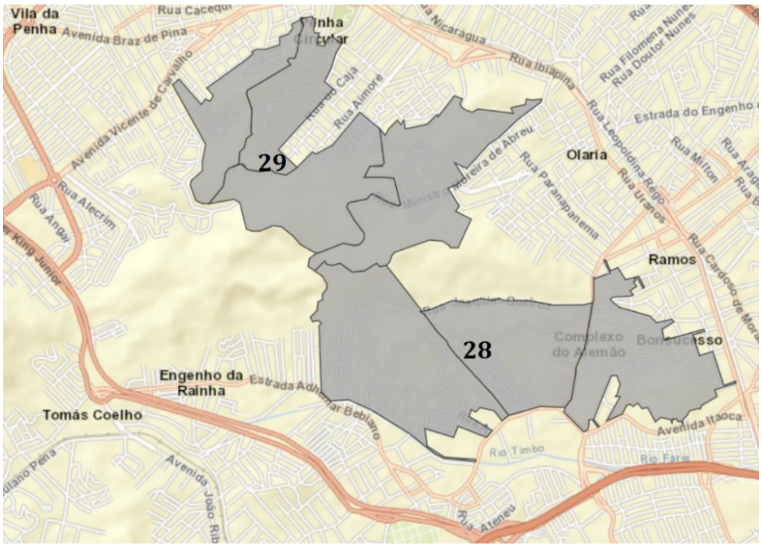


Figure 6.9: Upp Compleo do Alemão e Penha

Figure 6.10: Upp Providência

Figure 6.11: Upp Rocinha and Vidigal

Figure 6.12: Upp Mangueira

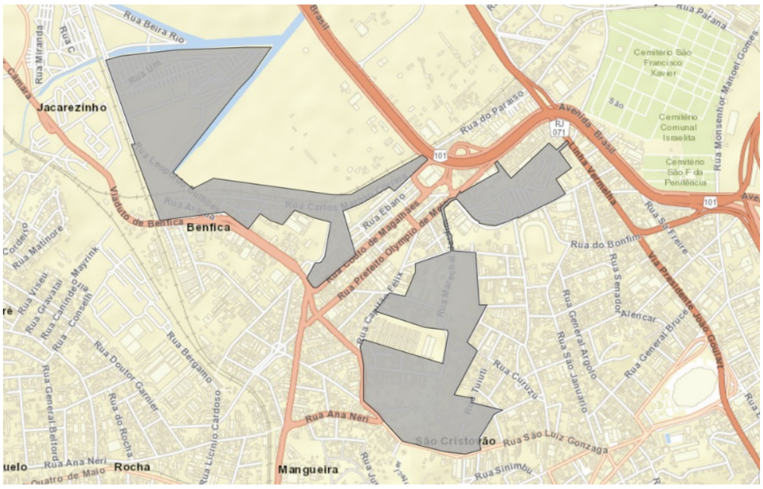


Figure 6.13: Upp Barreira Vasco and Arará mandela

Figure 6.14: Upp Cajú

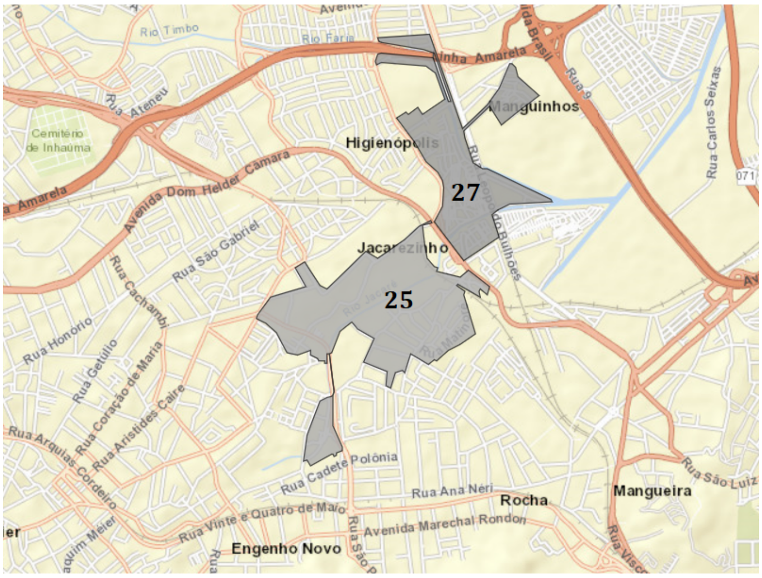


Figure 6.15: Upp manguinhos

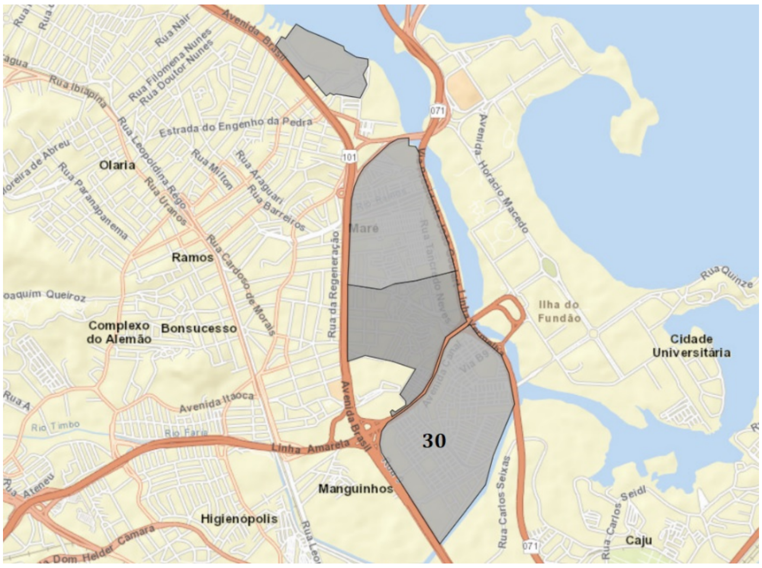


Figure 6.16: Upp Complexo da Maré

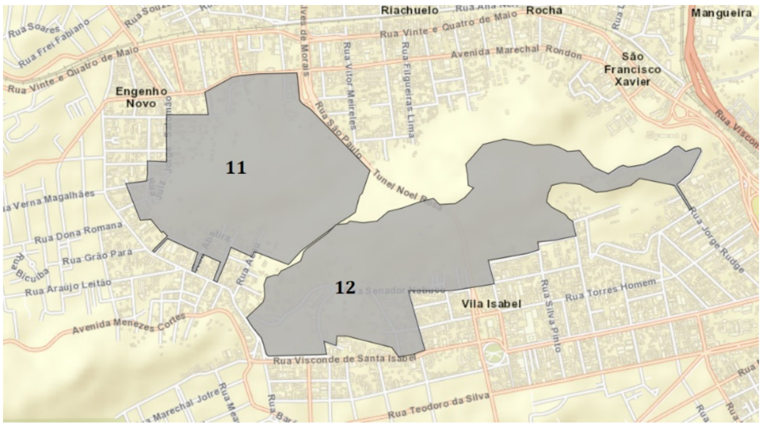


Figure 6.17: Upp Macacos and São João

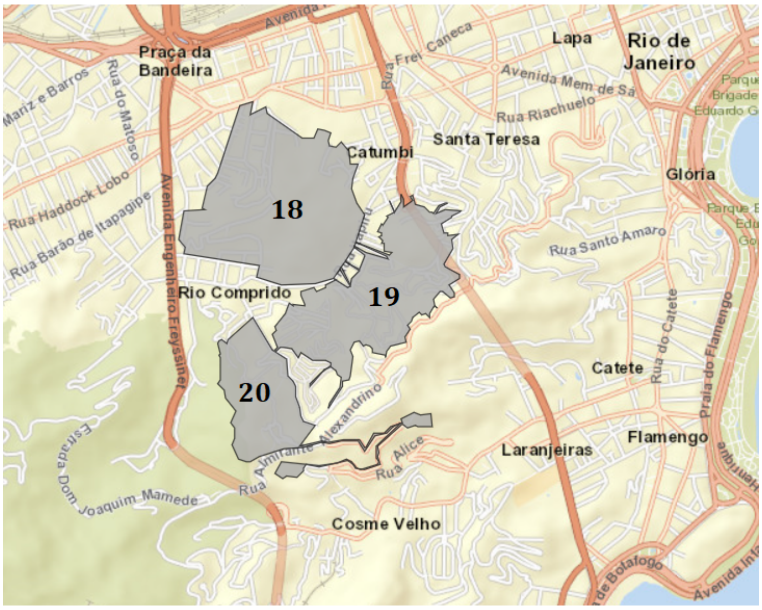
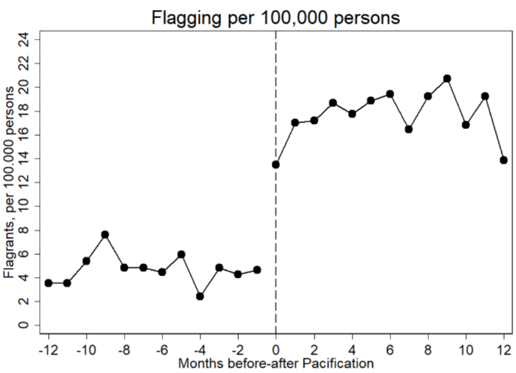


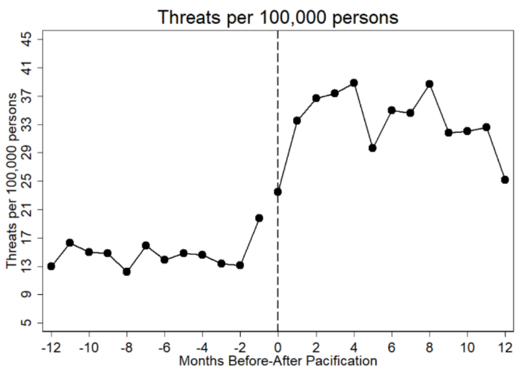
Figure 6.18: Upp Complexo do Alemão and São Carlos

Plots of crime levels before and after pacification

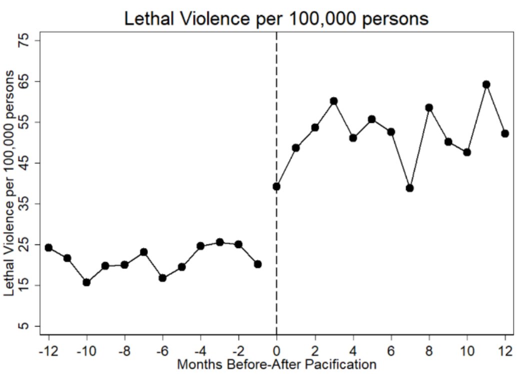
The plots add up the crime rates for 38 UPPs. The pacification borders are show in the appendix together with the pacification dates. Some plots are not displayed due space the reader is led to the Supplementary Materials for further description of the crime data.



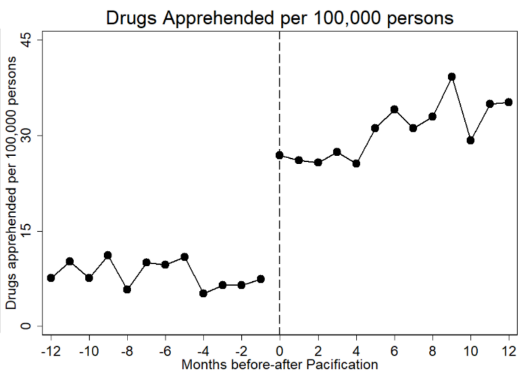
(a)



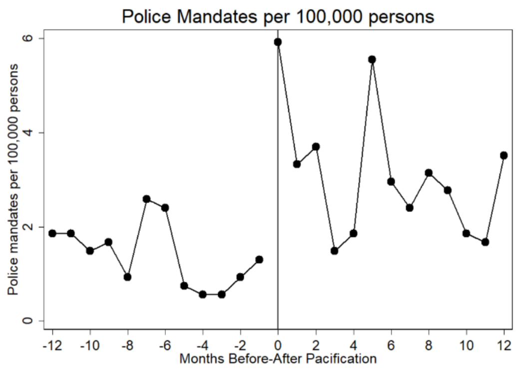
(b)



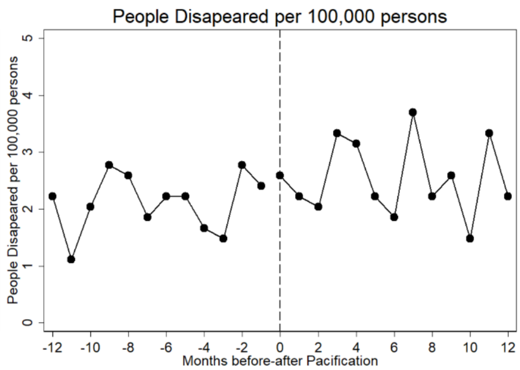
(a)



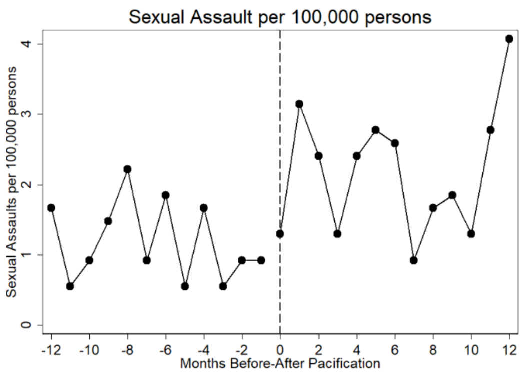
(b)


Police Mandates per 100,000 persons
Police mandates per 100,000 persons
Months Before-After Pacification

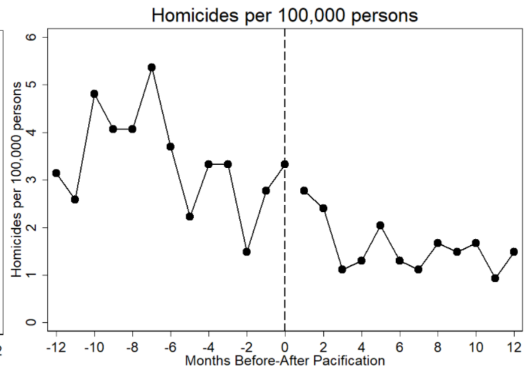
(a)


People Disapeared per 100,000 persons
People Disapeared per 100,000 persons
Months before-after Pacification

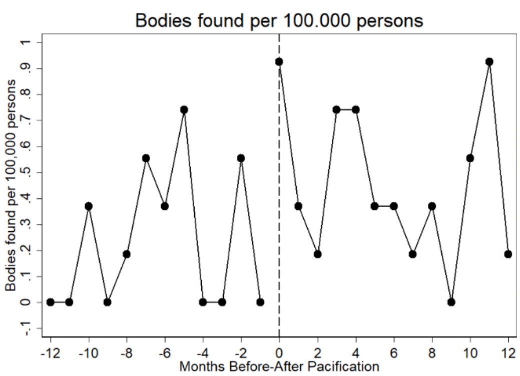
(b)


Sexual Assault per 100,000 persons
Sexual Assaults per 100,000 persons
Months Before-After Pacification

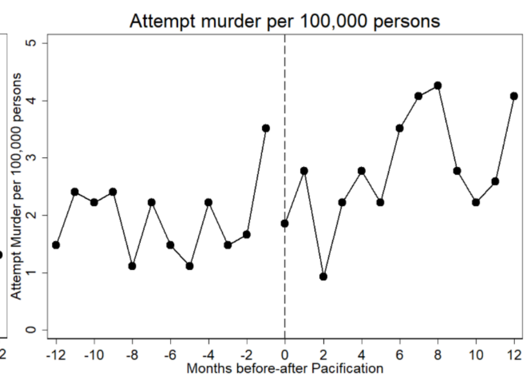
(a)


Homicides per 100,000 persons
Homicides per 100,000 persons
Months Before-After Pacification

(b)

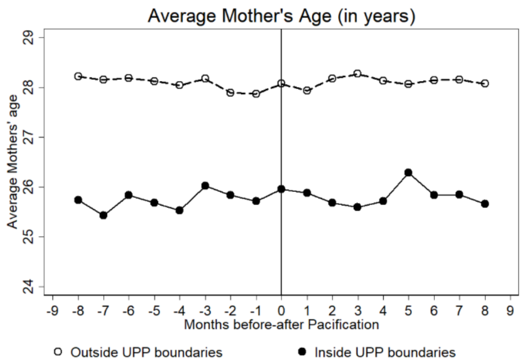

Bodies found per 100.000 persons
Bodies found per 100,000 persons
Months Before-After Pacification

(a)

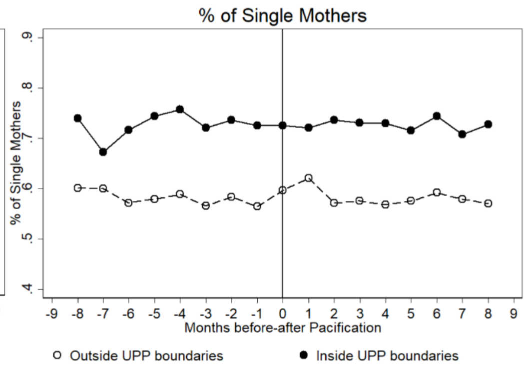

Attempt murder per 100,000 persons
Attempt Murder per 100,000 persons
Months before-after Pacification

(b)

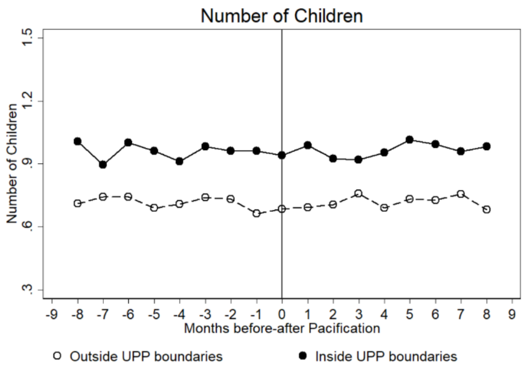
Checking for discontinuities in the observable characteristics



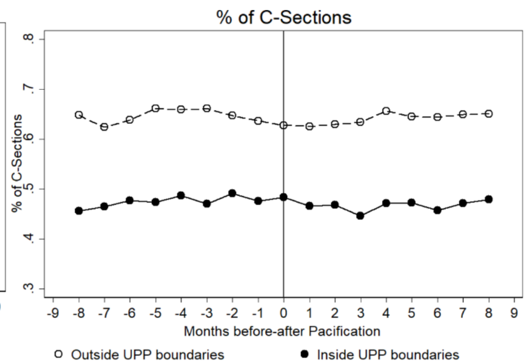
(a)



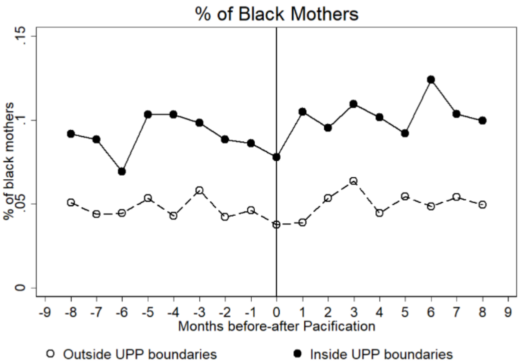
(b)



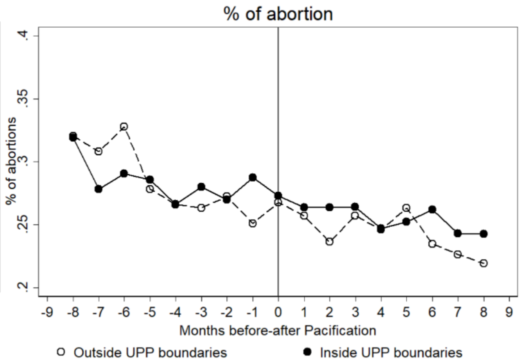
(a)



(b)



(a)



(b)

Figure 6.19: Mother characteristics before and after the pacification dates

Notes: The figure VII test whether there is any additional discontinuity in the characteristics of mothers before and after the pacification of favelas. The panels show (from the left-top to the right): the number of children of expectant women, age of mothers (in years), the incidence of twins, incidence of malformation in the fetus, percentage of women with previous abortions and percentage of mothers with any education. The continuous lines represent the fitted regressions from second order polynomials and bandwidth equal to 2 months for 12 before the pacification (our threshold) and 12 after the pacification. The dashed lines represent the 95% confidence interval.

Infromation Disclosure Informed Mothers and Delivering Babies


ANS Agência Nacional de Saúde Suplementar

CARTÃO DA GESTANTE

Nome

Endereço

Bairro | Município | UF

Telefone

Nome da Operadora

Registro ANS

Agendamento

Data	Hora	Nome do profissional	Sala

Carta de Informação à Gestante

O parto é um momento muito especial, que marca uma transição na vida da mulher e da família. Representa o primeiro encontro com o bebê. Por isso, é importante a mulher vivenciar este momento plenamente.

Pense em tudo o que gostaria ou não gostaria que acontecesse em seu parto, quem você gostaria de ter ao seu lado, como gostaria de aliviar a dor.

Quando for escolher o médico para realizar o seu pré-natal, você pode buscar informações importantes junto a sua operadora pelos canais de comunicação disponíveis, como o percentual de cesarianas e de parto normais realizados pelo médico e a maternidade. A operadora deverá lhe fornecer um número de protocolo e responder à sua solicitação em 15 dias úteis de forma presencial, por carta ou por e-mail.

Além disso, esclareça as dúvidas com seu médico. Assim você participará de forma ativa de todas as decisões sobre o seu parto.

O final da gestação, entre a 37ª e a 42ª semana, também é fundamental para a completa formação do bebê dentro do útero, para que após o nascimento ele possa se adaptar ao mundo exterior, física e mentalmente com mais facilidade e tranquilidade. Muitos recém-nascidos acabam sofrendo de problemas respiratórios devido a cesáreas agendadas e realizadas antes do início do trabalho de parto, em uma fase em que o bebê ainda não está pronto para nascer. Isto pode aumentar algumas complicações, tais como problemas respiratórios agudos e outras morbidades neonatais, quando comparados àqueles nascidos com 39 semanas ou mais. Em consequência, levar a necessidade de internação em uma UTI neonatal. Esta situação interfere no início do aleitamento materno e na adequada adaptação do recém-nascido à vida extrauterina, ocasionados pela internação.

O parto cesáreo é procedimento cirúrgico que deve observar claras indicações médicas.

Todas as mulheres têm o direito a um acompanhante de sua livre escolha no pré-parto, parto e pós-parto imediato. Planos hospitalares com obstetrícia com acomodação enfermaria, quarto semi-privativo, quarto privativo, ou qualquer outra acomodação, devem cobrir as despesas da paramentação (roupas higienizadas necessárias para entrar no centro cirúrgico), alimentação e acomodação do acompanhante.

Dúvidas, reclamações e sugestões, entre em contato com o Disque ANS 0800 701 9656.

ANS Agência Nacional de Saúde Suplementar

Idade: Estado Civil:

Peso anterior :

Altura (cm) :

Antecedentes familiares:

☐ Hipertensão Arterial

☐ Diabetes ☐ Gemelar

Exames	Data	Resultado	Data	Resultado	Data	Resultado
ABO-RH						
Toxoplasmose						
HBsAg (1º e 3º trimestre)						
Glicemia de Jejum						
TOTG (2h/75g)						
Anti-HIV						
HB/Ht						
VDRL						
Urina-EAS						
Urina Cultura						
Coombs Indireto						
Outros						

Eletroforese de Hemoglobina — Padrão ☐ AA; Heterzigose ☐ AS ☐ AC; Homozigoze ☐ SS ☐ AC

Malária *(somente para região Amazônica)*

Teste Rápido	Data	Resultado	Data	Resultado
Sífilis				
HIV				
Hepatite B e C				

Suplementação Sulfato ferroso

Sim ○ 1º mês ○ 2º mês ○ 3º mês ○ 4º mês ○ 5º mês ○ 6º mês ○ 7º mês ○ 8º mês ○ 9º mês ○

Não ○ 1º mês ○ 2º mês ○ 3º mês ○ 4º mês ○ 5º mês ○ 6º mês ○ 7º mês ○ 8º mês ○ 9º mês ○

Suplementação Ácido fólico

Sim ○ 1º mês ○ 2º mês ○ 3º mês ○ 4º mês ○ 5º mês ○ 6º mês ○ 7º mês ○ 8º mês ○ 9º mês ○

Não ○ 1º mês ○ 2º mês ○ 3º mês ○ 4º mês ○ 5º mês ○ 6º mês ○ 7º mês ○ 8º mês ○ 9º mês ○

Ultrassonografia

Data	IG DUM	IG USG	Peso fetal	Placenta	Líquido	Outros

	1ª	2ª	3ª	4ª	5ª	6ª	7ª	8ª	9ª	10ª	11ª	12ª
Data												
Queixa												
IG Semanas												
Peso (Kg) / IMC												
Edema												
Pressão Arterial (mmHG)												
Altura Uterina (cm)												
Apresentação Fetal												
BCF / Mov. Fetal												
Toque, se indicado												

Participou de atividades educativas

Sim ○ Não ○

Data

Data

Data

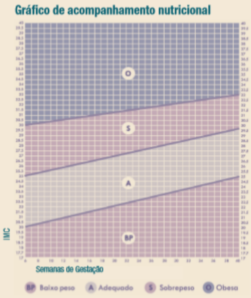
Realizou visita à maternidade

Sim ○ Não ○

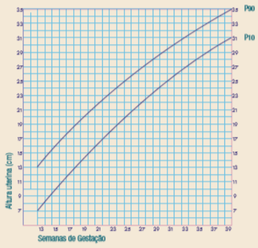
Data

Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta	Obs., diagnóstico e conduta
Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura	Assinatura

Gráfico de acompanhamento nutricional



Curva de altura uterina / idade gestacional



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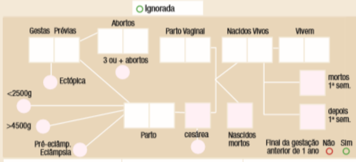
DUM	/ /	Tipo de gravidez	Risco habitual ○
DPP	/ /	○ Única	Gravidez Alto Risco ○
DPP (USG)	/ /	○ Gemelar	Gravidez Planejada Não ○ Sim ○
		○ Tripla ou mais	
		○ Ignorada	



	Não	Sim		Não	Sim	Antecedentes clínicos	Não	Sim
Diabetes	○	○	Cardiopatia	○	○			
Infecção Urinária	○	○	Tromboembolismo	○	○	Cir.pelv.uterina	○	○
Infertilidade	○	○	Hipertensão Arterial	○	○	Outros	○	○
						Outros	○	○

Gestação Atual

	Não	Sim		Não	Sim		Não	Sim
Fumo (nº de cigarros)			Anemia	○	○	Hipertensão arterial	○	○
Álcool	○	○	Inc. Istmocervical	○	○	Pré-eclâmpsia/eclâmp.	○	○
Outras drogas	○	○	Ameaça parto premat.	○	○	Cardiopatia	○	○
Violência doméstica	○	○	Isoimunização Rh	○	○	Diabetes gestacional	○	○
HIV/Aids	○	○	Oligo/polidrâmio	○	○	Uso de insulina	○	○
Sífilis	○	○	Rut.prem.membrana	○	○	Hemorragia 1º trim.	○	○
Toxoplasmose	○	○	CIUR	○	○	Hemorragia 2º trim.	○	○
Infecção Urinária	○	○	Pós-datismo	○	○	Hemorragia 3º trim.	○	○

Vacina antitetânica		Hepatite B	Imunizada ○
Sem informação de imunização ○		1ª dose	/ /
Imunizada há menos de 5 anos ○		2ª dose	/ /
Imunizada há mais de 5 anos ○		3ª dose	/ /
1ª dose / /	2ª dose / /	Influenza	/ /
3ª dose / /	reforço / /	Difteria	/ /
Coqueluche (dTpa) / /		Tétano	/ /

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Consulta odontológica

18	17	16	15	14	13	12	11	21	22	23	24	25	26	27	28
48	47	46	45	44	43	42	41	31	32	33	34	35	36	37	38

Legenda

* - Mancha branca ativa	Ca - Lesão cavitada ativa	PF - Prótese fixa
0 - Mancha branca inativa	Ci - Lesão cavitada inativa	RE - Restauração estética
A - Ausente	E - Extraído	SP - Selamento provisório
Ae - Abrasão/erosão	H - Hígido	T - Traumatismo
Am - Amálgama	M - Restauração metálica	X - Extração indicada

Presença de gengivite/periodontite Não ○ Sim ○ data / /

Plano de tratamento (por consulta)

Tratamento realizado (para o cirurgião dentista)

Data	Dente	Procedimentos realizados	Ass.CD
/ /			
/ /			
/ /			
/ /			
/ /			
/ /			
/ /			

Necessidade de encaminhamento para referência (para o cirurgião dentista)

Especialidade	Tratamento necessário	Encaminhamento	Retorno	Plano cuidado (contra-refer.)

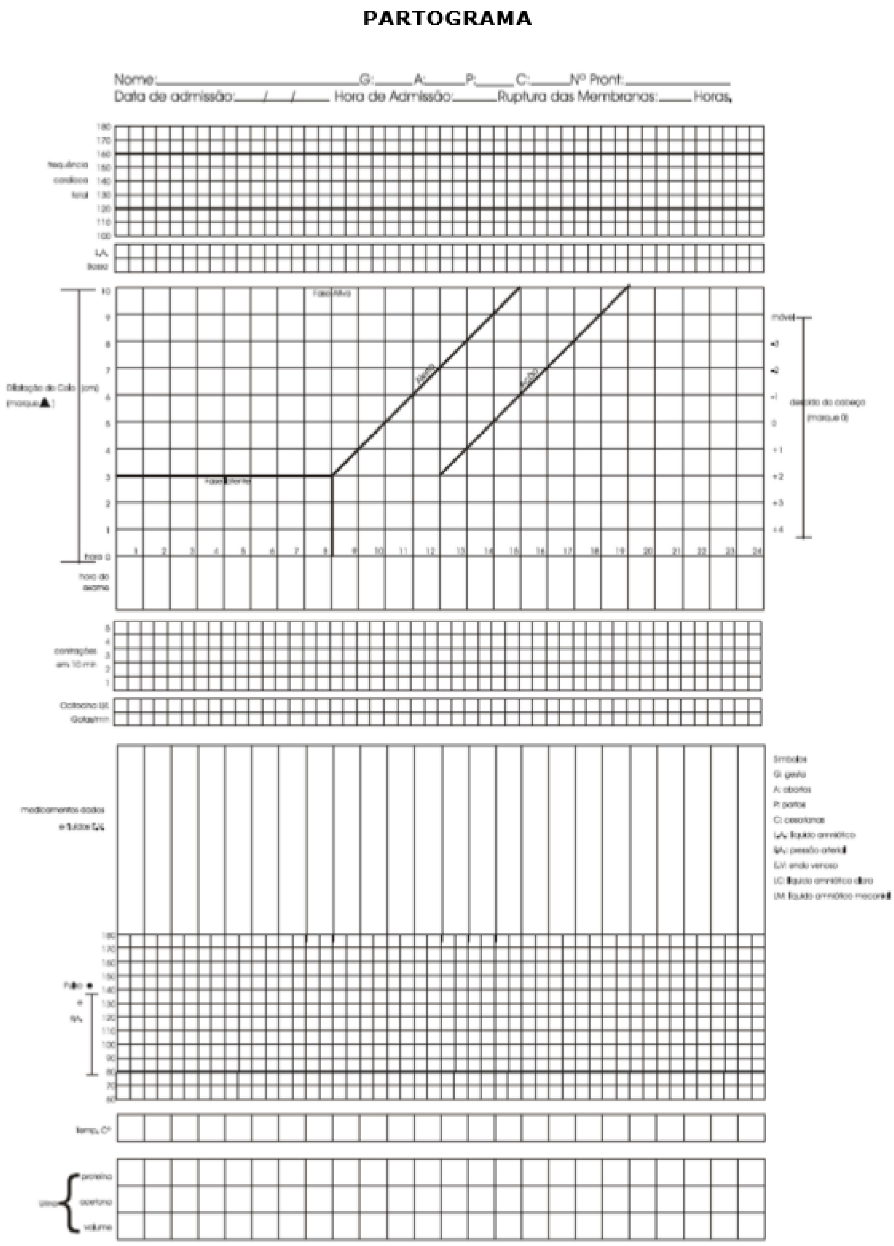

Figure 6.20: Partogram

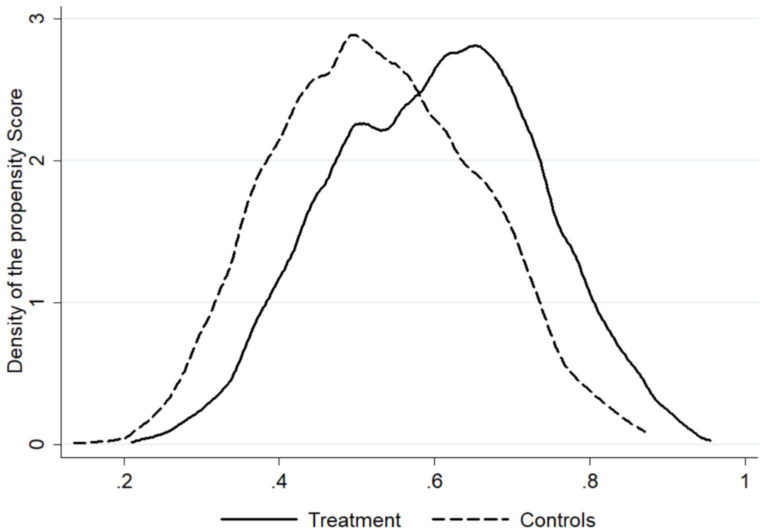

Figure 6.21: Propensity score for treatment and controls