Essays on Health Economics

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Abstract

This dissertation consists of three essays on Health Economics. Chapter 1 analyzes the effects of abortion costs for minors on abortions, sexual behavior, and births. We exploit a 2015 change in parental involvement (PI) laws in Spain as a natural experiment in costs, together with rich population-level data on abortions and births. Using the exact date of teenager birth, we first document a decrease in abortions by 17-years-olds using a difference-in-difference comparison with 18-years-olds, consistent with the law that targeted Spanish minors. Using bunching methods from the Public Finance literature, we show evidence of temporal displacement. Some 17-years-old delayed their abortion and waited until they turned 18 and thereby avoided involving their parents. Second, we consider how the law change may have influenced health-related behaviors, finding implicitly that sexual behaviors changed so as to reduce the likelihood of becoming pregnant before turning 18 (and thereby internalized the cost of parental involvement). This is seen in the permanent shift in the number of abortions at age 18 that exists after removing the temporal displacement abortions around the age 18 threshold and an increase in the number of births to mothers who were pregnant at age 17. This paper finds that an important dimension of risky youth behavior responds to incentives contained in parental notification laws.

Chapter 2 analyzes the effects of abortion costs on sex-selection by exploiting a 2010 abortion liberalization in Spain and the difference in son-preferences by nationality and child order documented in the literature. We show using a difference-in-difference comparison a significant increase in the fraction of boys for Chinese parents giving birth to their third child or above relative to children born of Spanish parents. Consistent with the literature, we do not find any effect on the fraction of boys for the first or the second child. Using the provincial number of abortion centers per person as a measure of access to abortion, we show, at the correlation level, that the effects come from those provinces with higher access to abortions. Finally, we find suggestive evidence that birth outcomes

of Chinese girls who are the third children, and thus are now more likely to be "wanted" after the reform, improve. Gestational weeks increase, and the chance of being born prematurely decrease although our evidence suffers from lack of power.

Finally, chapter 3 analyzes the effects of a universal, unconditional cash transfer announcement on birth outcomes by exploiting the 2007 *cheque bebé* policy in Spain that provided 2,500 euros per child to all mothers giving birth immediately after its announcement (Jul 2007). We use a difference-in-difference analysis comparing those born before and after the announcement. By exploiting the timing of the policy announcement we can avoid the composition effects caused by the incentives to have children generated by the policy. We show that the birth weight of those children born after the policy announcement (Sept-Dec) significantly improved relative to those born before (Apr-Jun) using previous years to control for the seasonal effects. Moreover, we provide suggestive evidence that those who are more vulnerable, as measured by the average municipality income level, parents' marital status, or parents' age, experience the most substantial improvements on birth weight.

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Dedication

To those who suffer the non-senses of this world.

With the hope of not damaging anyone.

Chapter 1:

Parental Involvement in Abortion Decisions and Teenager Responses: Evidence from Spain

1.1 Introduction

Despite the high stakes of a decision like having a child, the literature has documented, perhaps surprisingly, that women's abortions respond to costs that seem relatively small in comparison to the costs of having an unwanted child. This is all the more surprising especially because legal abortions are the last safe mechanism to avoid an unwanted child. Fischer et al. (2018), for example, show that abortions fell, and births rose in Texan counties that no longer had an abortion provider within 50 miles.

One common abortion regulation that may appear to impose a small cost is parental involvement (PI) laws. PI laws are any combination of laws that require minors to involve one (or both) of their parents into the abortion process.[1] Besides, understanding PI laws is important because minors are a group who are particularly vulnerable to unwanted children. An unwanted child is going to impose a constraint on minors' schooling and working choices, which might end up having a considerable impact on minors' present and future wellbeing. One of the main challenges that the literature has faced in order to understand the effects of PI laws on abortions is the lack of population-level data on abortions.[2] This paper contributes to this literature by bringing an unusually rich-population

[1]These laws can take many forms, like requiring the teens to obtain parental consent in order to abort or requiring them to inform their parents despite not needing their consent to abort.

[2]Dennis et al. (2009) in their literature review of the effects of PI laws argue that *" the biggest difficulty in evaluating parental involvement laws is the lack of population-based data on abortions"*.

level data set on abortions to understand the impact of PI laws on minors.

We exploit the 2015 abortion reform in Spain, which increased the abortion costs for minors. Specifically, the reform forced 16-17-year-old women to obtain parental consent (and thus, to inform their parents) to abort. Before the reform, 16-17 years old did not need parental consent and could avoid informing their parents with their doctor's permission.[3] Importantly, there was no other change for any other age group regarding abortion. Our data allow us to explore the effects of the law change on abortions, risky sexual behavior leading to pregnancies, and births.

To find the effects on abortions we use a difference-in-difference strategy where we compare 17 years old abortions with the 18 years old ones. We show that 17 years old abortions decreased relative to the 18 years old ones. The effects are driven by Spanish women as opposed to non-Spanish. Meanwhile the effects are significant for students and not significant for non-students. We then compare 16 years-old abortions with the 15 years-ones. We do not find any effect for the 16 years old, which is consistent with the notion that younger minors are more likely to involve their parents regardless of the regulatory environment.

Besides the overall effects on abortion, the jump in the abortion cost at age 18 might cause a temporal displacement of the procedure around the 18th birthday. Women aborting immediately after turning 18 do not have to pay the fixed cost of involving their parents, which before the 18th birthday they have to incur. This cost structure generates an incentive for women who are going to turn 18 during the first 14 weeks of pregnancy to wait until they turn 18 to abort.[4] Using the fact that we know the exact day of birth of the woman and the exact day of the abortion, we analyze the possibility that the notch generated at the cost structure causes bunching on abortions around the woman's 18th-

[3]The law stated that 16-17 years old did not have to inform their parents if *"the minor can convince the doctor that informing her parents would cause a serious conflict leading to violence, threats or coercion"*. That is, we exploit a policy change where PI costs for 16-17 years old increased, but they already existed before (i.e., before the law change, it was already "more costly" for minors to abort relative to 18 years old).

[4]Abortion is liberalized during the first 14 weeks of pregnancy in Spain. This is why the relevant age to know whether a woman was affected by the PI law is her age at gestational week 14.

years-old birthday by bringing the bunching methods from the Public Finance literature. We show the existence of this mechanism, showing that some women who can prefer to delay their abortions until they turn 18 (with the corresponding costs of aborting at a later gestational week) in order to avoid involving their parents. Unfortunately, we do not have enough power to see if this mechanism becomes stronger after the PI law reform.

There are two possible explanations of the decrease in abortions. First, there are fewer pregnancies. Minors internalize the new costs of terminating an unwanted pregnancy, and thus of getting pregnant, and modify their sexual behavior to avoid unwanted pregnancies. Second, there are more births.[5] We provide evidence of the existence of both of these channels.

First, we explore the possibility that minors internalize the higher abortion costs and modify their sexual behavior to avoid an unwanted pregnancy. We show that there is a discontinuous jump on the number of abortions at age 18 once we remove all the temporal displacement from the bunching. The size of this jump could be caused by 17 and 18 years-old differences. However, the size of this jump becomes larger after the 2015 reform when, to the best of our knowledge, nothing else changed differently for 17-years-old and 18-years-old. We interpret this as a permanent effect of the policy on the sexual behavior of minors.

Second, we analyze the effects on fertility by taking advantage that we know the exact day of birth of the mother and the exact day in which she gives birth. Using a difference-in-difference strategy, we show that births of Spanish women who were 17-years-old during the first 14 weeks of their pregnancy, increased after the reform relative to those who were 18-years-old. Fertility increases for both students and non-students.[6] We do not find any effect for non-Spanish.

To assess the importance of risky sexual-behavior relative to fertility, we combine our

[5] A third possibility is that these abortions disappear from our data because minors abort illegally or migrate to another country to abort. We will discuss more on these possibilities later.

[6] However, we do not find any effects for those women whose professional activity is missing.

abortion and birth data to construct a conceptions data. By following the same strategy as the fertility analysis, we show that 17 years old conceptions significantly decrease relative to 18 years old after the PI cost increase. The absolute magnitude of the decrease is, as expected, smaller than the one in abortions. Results are driven by Spanish women.

There have been several studies on the effects of PI laws on abortions, most of which have used US data. Some of the early studies have analyzed PI laws changes within states while most recent studies have analyzed the effects of PI laws across states. One of the main challenges these studies have faced is the lack of population level abortion data and the impossibility to follow minors who travel across states to avoid the PI laws. This paper contributes to the literature by, first, bringing an unusually rich-population level data set on abortions. Second, an advantage of analyzing Spain is that to avoid the PI laws minors need to travel abroad, which is more costly than traveling across states.

The possibility that minors delay abortion until they turn 18 had, to the best of our knowledge, only been documented by Colman and Joyce (2009). We contribute to this literature by bringing in the Public Finance bunching methods. Regarding the effects of PI laws on minors sexual behavior to avoid unwanted pregnancies the effects documented by the literature have been mixed. We contribute to this literature by finding implicitly that sexual behaviors changed to avoid unwanted pregnancies. Finally, our positive effects on fertility are consistent with Myers and Ladd (2017). They find that teen births increase with PI laws when those laws forces teens to travel long distances in order to avoid them (as opposed to when they can travel to a nearby state to avoid them). This situation is more similar to the Spanish case, where it is not easy for teens to avoid PI laws.

The remaining of the paper is organized as follows. First, section 1.2 discusses the related literature. Section 1.3 discusses in detail the abortion policy change. Section 1.4 describes the data. Section 1.5 analyzes the effects on abortion discussing the identification strategy and presenting the results (section 1.5.1 for the difference-in-difference analysis and section 1.5.2 for the bunching analysis). Section 1.6 discusses the identification strategy and presents the results for the indirect effects (section 1.6.1 for the sexual behavior analysis, section 1.6.2 for the fertility analysis, and section 1.6.3 for the conceptions analysis). Finally, section 1.7 concludes with a discussion.

1.2 Literature Review

We organize the discussion of the related literature in three complementary parts. First, we discuss the literature on the effects of the PI laws on teenagers' abortions. Most of this literature has studied changes in US PI laws. Second, we discuss the literature on the behavioral responses of teenagers modifying their risky sexual behavior as a result of the PI laws in order to avoid unwanted pregnancies. Finally, we present the literature on the effects of PI laws on teenagers' fertility.

On the effects of PI laws on minor abortions several studies in the US have analyzed their effects finding, in general, that PI laws reduce minor abortions and that minors out-of-state travel to abort to avoid these laws increases. Some of the literature has analyzed PI laws changes within states by comparing the minors' abortions with the abortions of women over 18. An early study, for example, is Cartoof and Klerman (1986), which analyzes the April 1981 Massachusetts law change, which required unmarried women under age 18 to obtain parental or judicial consent before having an abortion. They find that during the 20 months after the law went into effect, minors abortions in the state were reduced to half. For women aged 18 or older, this change did not happen. They find that out-of-state travel by minors to abort explained most of the decline. Moreover, non-resident minors stopped going to the state to abort. Another example is Ellertson (1997), who analyzes the effect of PI laws in three states: Minnesota (1981), where she finds a 26% decrease in in-state abortions, Missouri (1985) 20% decrease, and Indiana(1982) 17% decrease. She finds suggestive evidence that minors out-of-state travel to abort increased in Missouri, but data was incomplete. She finds no evidence that minors' birth rates increase.

Other studies analyze the effects of PI laws across states. An early study is Ohsfeldt and

Gohmann (1994) who use data from 1984, 1985, and 1988 from around 30 states. They find that PI laws caused a significant reduction in minors' abortions. A recent study is Joyce et al. (2019) who document that PI laws enacted before the mid-1990s are associated with around 15%-20% decrease in minors' abortions. PI laws enacted after do not have any effect on minors' abortions. They find large differences in these effects by state.

We contribute to this literature by analyzing the effects of minors abortions in Spain when PI laws become stricter. Moreover, our rich data set allows us to explore heterogeneous effects in order to understand better who are the minors (if any) that are affected by these laws (eg., minors who live with their parents or not, immigrants or not, or minors who have their own income source). Understanding what kind of minors are affected is essential in order to understand the effects of the policy thoroughly. This is something that the existing literature has not analyzed in detail because of data limitations.

Another relevant dimension that this paper analyzes is the possibility that minors who are going to turn 18 during their pregnancy delay their abortion until they turn 18. Analyzing this requires very precise data since the exact day of abortion and the exact day of birth of the woman are required. A paper that has documented the existence of this behavioral response to a PI law is Colman and Joyce (2009) who analyzes the effects of the parental notification requirement that went into effect in Texas in 2000. They find that the proportion of abortions carried out at age 18 increased by 6% among minors who conceived at age 17 years and eight months, and by 13% among those who conceived at 17 years and nine months. As a result, second-trimester abortions increased for this group. We contribute to this literature by analyzing this channel in the case of Spain and by bringing the bunching methods from the public finance literature.[7]

Second, the evidence on the effects of PI laws on minors sexual behavior to avoid unwanted pregnancies has been mixed. Levine (2003) and Klick and Stratmann (2007) provide evidence that PI laws reduce risky sexual behavior among minors. Levine (2003)

[7]See, for example, Kleven and Waseem (2013) or Chetty et al. (2011).

uses data from the National Survey of Family Growth from 1988 to 1995. He uses a "triple difference" using state variation and women's age variation and documents that PI laws caused a reduction in abortion for minors (but nor for older women), but they had no effects on births. He finds some evidence suggesting an increase in contraceptive use but no reduction in sexual activity. However, Dennis et al. (2009) argue that Levine (2003)'s design is not convincing because of data limitations. Klick and Stratmann (2007) uses gonorrhea rates as a measure of risky sex among teens and reports that PI laws lead to a decrease in gonorrhea rates for teens. However, Colman et al. (2013) argue that gonorrhea rates are underreported. They replicate Klick and Stratmann (2007) and by introducing new evidence based on rates of chlamydia and self-reported sexual behaviors conclude that there is no evidence that PI laws affect teens' sexual behavior. Sabia and Anderson (2016) revises Colman et al. (2013) using a different data set and exploiting additional state policy variation find an increase in the probability that sexually active minor teen females use birth controls after the implementation of PI laws.

We contribute to this literature by proposing a new method to evaluate minors' sexual behavioral changes. In particular, we propose to analyze whether once we exclude all the temporary displacement effects caused by minors waiting to turn 18 to abort, we still observe a "jump" on the abortions happening after minors turn 18 relative to the trend observed before they turn 18. We then analyze whether this "jump" increases after the PI laws become stricter. If this is the case, we will then interpret this permanent effect of the policy as an effect driven by minors changing their sexual behavior in order to avoid being pregnant before turning 18.

Finally, regarding the effects of PI laws on fertility, a recent study is Myers and Ladd (2017). They use double and triple-difference estimation strategies and allow the effects to vary with the distances minors have to travel to avoid the PI laws. They find that before 1992, PI laws did not cause an increase in teen births. This is consistent with Levine (2003), as mentioned above, and with Kane and Staiger (1996). However, they find that after

1992 when those laws forces teens to travel long distances in order to avoid them, PI laws caused an increase in teen birth by an average of 3%. The effects are increasing in PI laws' avoidance distance. This situation is closer to the Spanish case, where it is not easy for teens to avoid PI laws. We contribute to this literature by analyzing the effects in the case of Spain. Our precise data allows us to zoom in and compute whether the woman was 17-years-old during the first 14 weeks of their pregnancy, which is the relevant age to know whether the woman was affected by the PI laws or not.

1.3 Institutional Context and Policy Change

We are going to analyze the effects of the policy reform that took place in Spain in 2015. Before discussing this policy reform, we provide some context on the recent abortion legislation in Spain.

The first recent abortion reform was the Law 9/1985, approved on July 5th, 1985. Abortion was allowed only under three circumstances. First, if there was a physical or psychological risk for the pregnant woman, abortion was allowed at any moment of the pregnancy period. Second, if the woman had been a victim of rape abortion was allowed during the first 12 weeks of pregnancy. Finally, if there were physical or psychological malformation of the fetus abortion was allowed during the first 22 weeks of pregnancy. Under this law, 16-17 years old women were treated as any other minor, and they needed parental consent in order to abort.

On July 5, 2010, the socialist party (PSOE) government implemented the second reform (Law 2/2010), liberalizing abortion in the first 14 weeks of pregnancy. Within the first 14 gestational weeks a woman could abort without providing any justification three days after being informed of her rights and the existing subsidies for mothers. After the 14th gestational week, if there was a risk to the mother or the fetus, she could abort until week 22. If the fetus had an illness incompatible with life, she could abort at any moment of the pregnancy.

Beyond liberalizing abortion, the law also changed the abortion conditions for the 16-17 years old minors. The law stated that the consent to abort belonged to the 16-17 years old as if she was over 18 years old. However, at least one of the parents or legal guardians of the minor had to be informed unless *"the minor can convince the doctor that informing her parents would cause a serious conflict leading to violence, threats or coercion"* in which case the requirement to having to inform one of the parents or legal guardians could be dropped. The law did not change for women who were 15 years old or younger, who continued to require parental consent to abort.

The third, and most recent reform, that this paper exploits took place on September 21, 2015 (Law 11/2015). After the November 2011 general elections, there was a change of government in Spain, and PP took power. One of their electoral promises had been to change the 2010 abortion law and go back to the 1985 law or even a more restrictive one. However, they did not manage to reach consensus among themselves, and the Minister responsible for the project ended up resigning on September 23rd, 2014.

On February 18th, 2015, PP presented a new law in Congress. On September 19th, 2015, the Senate approved the law, and on September 21st, 2015, the law entered into effect. The 2015 law change ended up implementing only the following change to the 2010 law: *"for the abortion of the underage it will be necessary not only her consent but also their parents' consent"*. That is, the law ended the possibility of the 16-17 years old to abort without informing their parents. Notice that the law was announced in advanced and thus its implementation was anticipated given that televisions and newspapers covered it in detail.

Figure 1.7.1 summarizes the abortion policy changes for 16-17 years old women described in this section and our available abortion data.

1.4 Data

We use two sources of data, one for abortions and one for births, for our analysis. For each data set we first describe the data collection process and what information it contains and then we present some summary statistics to characterize the minors who are aborting or giving birth.

1.4.1 Abortion Data

We use population-level data from the Spanish Ministry of Health. The data covers the universe of abortions that took place in Spain between 2011 and 2017. The doctor performing the abortion has to report the abortion to the regional authorities by filling a standardized form with information about the procedure and the woman. We have access to the data contained in these standardized forms.

The data is rich relative to the usual abortion data, and, importantly for this project, it includes the exact day in which the abortion happened and the exact day of birth of the woman. With this information, we can construct the difference between the abortion day and the 18th birthday, positive sign meaning the woman aborted after turning 18, and negative sign meaning she aborted before. This information is important for analyzing the existence of bunching in the number of abortions around the day that women turn 18th. The data also includes information about the gestational weeks at which the abortion is performed. This is important because by combining it with the exact day of the abortion we can estimate the conception day. Doing the same with the birth data we can construct a conception data in Spain and analyze what happened to the overall conceptions when the PI laws changed.

Moreover, we have information about the woman, which allows to check for heterogeneous effects. In particular, we have information, for example, on the woman's nationality, whether she lives alone, with her parents or with her partner, whether she has her own income source. Finally, we have geographical information about where the woman lives. In particular, we have information on the province and the municipality where the woman lives.

Table 1.7.1 provides some summary statistics for the abortions data for women aged

15-18 years old for the 2011-2017 period. Together, the 15-18 years old only represent 7.33% of the total abortions that took place in Spain between 2011-2017. Of those, more abortions come from older women, with 11.15% of the abortions coming from the 15 years old, increasing to 41.51% from the 18 years old. Fewer of those who are older are students or still live with their parents. An overwhelming majority of 15 years old, 92%, are students while only 57% of the 18 years old are.[8] Around 70 % of the 15-17 years old lived with their parents while only 63% of the 18 years old did. Finally, around 70% of the abortions were from women born in Spain with Spanish nationality. In terms of the characteristics of the abortions, the average gestational weeks at the time of the abortion is decreasing on age (9.14 weeks for 15 years old and 8.42 for 18 years old), the total number of abortions per woman is increasing in age (from 0.07 to 0.184) and the total number of living children per woman too (from 0.04 to 0.08).

Unfortunately, we have little information to gauge directly the extent of the abortion cost increase by the reform. In other words, we do not have population-data on whether 16-17 years old took advantage of the possibility of not involving their parents that the law allowed. Nonetheless, a November 2014 report by *ACAI* (*Asociación de clínicas acreditadas para la interrupción del embarazo*) which is an association of abortion clinics in Spain suggests that a considerable percentage of minors were avoiding to involve their parents in the abortion procedure. During the discussion of the 2015 reform *ACAI* lobbied to prevent the change by arguing that the minors not involving their parents were a "minority". The report analyzes the 16-17 years old abortions performed in their clinics between January 2014 and September 2014. They performed 25,394 abortions during this period, which represents around 35% of the total abortions performed in Spain during this period. Among the abortions performed by the *ACAI* clinics, 913 of them were to 16-17 years old. This represents around 37% of the total 16-17 years old abortions performed in Spain during the time of the analysis. 12.38% of the 16-17 years old abortions (113 abortions) performed in

[8]Education is compulsory in Spain until age 16.

the *ACAI* clinics were carried out without the parents being informed. This is suggestive evidence that a considerable percentage of minors were benefiting from the possibility of not involving their parents during the abortion process.

1.4.2 Birth Data

Population-level birth data comes from the *National Statistical Institute* (*INE*). The data contains the universe of births that take place in Spain. The information appears as it appears in the official national registry. All families have to register the newborn babies within eight days of birth.

Importantly for this project and the period 2007-2017 the data includes the exact day in which the mother gives birth, the exact day in which the mother was born and the gestational weeks at the moment of birth.[9] The time and date of birth are set by the health center that assisted with the delivery.

The data also includes some birth outcomes such as birth weight (in grams), the weeks of gestation at birth, whether the newborn lived more than 24 hours or not, whether the child was born alive or not. Moreover, it includes some information about the parents' characteristics such as their education level, whether they are married or not, their immigration status, or the job category of the parents among others. Finally, it includes geographical information like the province and county of registration of the child.

Table 1.7.2 provides some summary statistics for the births data for women aged 15-18 years old for the 2012-2017 period. Together, the 15-18 years old only represent 1.22% of the total births that took place in Spain between 2012-2017. Of those, more births come from older women, with 7.15% of the births coming from the 15 years old, increasing to 46.23% from the 18 years old. Around 70% of the births came from women with Spanish nationality born in Spain, around 47% of the 15 years old births came from women who

[9]The data is publicly available, with the exception of the exact day of birth and the exact day in which the mother was born. This data was provided by for this project.

were studying while only 25% of the 18 years old were studying.[10] The fraction of women being married increases from 2.68% for the 15 years old to 10% for the 18 years old. Fathers are considerably older than the mothers (for the 15 years old the average father's age is 21.58 while for the 18 years old is 23.53). Finally, the percentage of fathers who are studying is also considerably lower than the percentage of mothers (20% for the 15 years old and only 9% for the 18 years old).

1.5 Direct Effects on Abortion

We first focus on the direct effects of the PI law change on the abortions of the affected minors (16 and 17 years old women). In particular, we explore the overall effects, and the heterogeneous effects across different groups of minors, and the temporary effects (existence of bunching) on those women who are turning 18 during their pregnancy. For those the temporary effects, we explore both the bunching caused by the previous PI laws, and its change after the policy change.

1.5.1 Overall Effects

1.5.1.1 Identification Strategy

In order to estimate the effect of the policy change on the number of abortions of the 16-17 years old women, we use a *difference-in-difference* strategy. Given that the policy change only affected those women aged 16-17 years old and did not cause any change to women of other ages, we can compare what happens to the number of abortions of those women aged 17, the treated group, with those who are just slightly older, 18 years old, before and after the policy change. Similarly, we can compare the treated 16 years old to the control 15 years old.

[10] By law education is compulsory in Spain until age 16. Most of the 15 years old who report not being studying report sharing the household work, and a minority report being working even though minimum legal working age in Spain is 16 years old.

The main assumption of the *difference-in-difference* identification strategy is the parallel trends assumption. If the policy change had not taken place, the number of abortions of the 17 years old women would have evolved in the same way as the number of abortions of the 18 years old women. In particular, the fraction of abortions of the 17 years old relative to the 17-18 years old population would have stayed roughly constant if the policy change had not taken place. Since the abortion policy change was, to the best of our knowledge, the only policy change that affected 17 and 18 years old (and 15 and 16 years old) differently at the time of the policy change this assumption seems plausible.[11] In the results section, we plot the evolution of the fraction of abortions of the 17 years old relative to the 17 and 18 years old in order to evaluate the this assumption.

We do the same analysis with the 16 years old relative to the 15 years old. We analyze the 16 years old separately from the 17 years old because there is evidence suggesting that minors aged 16 and younger are more likely to involve their parents regardless of the regulatory environment, hinting at possible different effects.[12]

In particular, we are estimating the following regression:

$$age17_{imt} = \alpha + \beta \times POST_{imt} + \mu_t + \eta_m + \epsilon_{imt} \tag{1.1}$$

where $age17_{imt}$ is an indicator variable equal to 1 if woman i who aborted in month m and year t was 17 years old at the time of the abortion and equal to 0 if she was 18 years old. $POST$ is equal to 1 if the abortion took place after the policy change and 0 otherwise. μ_t are year fixed effects, and η_m are month fixed effects. The coefficient of interest to see the effect of the policy change on the abortions of the 17 years old (or 16 years old depending

[11] Notice that implicit to this assumption there is the assumption that the population growth is constant. Ideally, we would like to have good population data in order to normalize the number of abortions by the appropriate cohort size, but unfortunately, we do not have this data. Assuming that the population evolves smoothly seems a reasonable assumption.

[12] See Henshaw and Kost (1992) or Reddy et al. (2002).

on the specification) is β. We also take advantage of the rich data set in order to look for heterogeneous effects by analyzing different groups (those who live alone versus those who live with their parents, or small towns versus big towns for example) differently. Understanding the heterogeneous effects is relevant to fully understand the effects of the policy.

1.5.1.2 Results

A. Abortion Effects on 17 years old

Figure 1.7.2 plots the fraction of abortions of the 17 years old relative to the abortions of the 17-18 years old in each month-year between 2011-2017. The figure shows, perhaps noisily, that after the policy reform, the fraction of abortions of the 17 years old decreased.

Table 1.7.3 provides the main results of the difference-in-difference analysis from equation 1.1. The law led to a significant decrease in the abortions of the 17 years old relative to the abortions of the 17-18 years old. 17 years old abortions represented around 41% of the total abortions from the 17-18 years old, and the the policy led to an absolute decrease of 3.98 percentage points, which is statistically significant at the 5% level. In the same table, we can see that the decrease comes mainly from Spanish women (their decrease is 5.28 absolute percentage points which is significant at the 5% level).[13] For the other women, we still observe a decrease, but it is smaller (2.56 percentage points), and it is not statistically significant. Table A.1.1 in the appendix shows that results are robust to comparing the 17 years old women with 18 and 19 years old.

We now turn to explore heterogeneous effects across different groups to understand the characteristics of the women who were most affected by the policy in Tables 1.7.4 and 1.7.6. In Table 1.7.4, the point estimate for those who are living alone or with their partner is the same as for those who are living with their parents. However, only the results for those living with their parents are significant, probably due to the lack of power. This

[13] We define Spanish women as women born in Spain with Spanish nationality.

result is the same for Spanish women. For non-Spanish women results are only significant for those who live alone or with their partner. In table 1.7.5, results are driven by students, regardless of nationality. Finally, in table 1.7.6 we cans see that results are driven by small municipalities (population below 50,000) as opposed to large cities.

B. Abortion Effects on 16 years old

We now analyze the effects of the policy change on the 16 years old women following the same method as before. We will compare the 16 years old with the 15 years old and, separately, with the 18 years old. Figure 1.7.3 plots the fraction of abortions of the 16 years old relative to the abortions of the 15-16 years old in each month-year between 2011-2017. The figure does not suggest any effect of the policy change on the 16 years old.

Tables 1.7.7 and 1.7.8 provide the results of this analysis. None of the coefficients of these tables are statistically significant, and thus they suggest that the policy did not have an effect for the 16 years old women (neither for the Spanish nor for the non-Spanish). In table 1.7.7, we compare them to 15 years old, a group they are more likely to be more similar to, and we obtain negative coefficients, but none of them is statistically significant. In table 1.7.8, when we compare them with the 18 years old abortions, the coefficients become positive (opposite sign to what we would expect), but they are small, and none of them is statistically significant.

Tables A.1.2-A.1.3 in the appendix explore the heterogeneous effects for the 16 years old women relative to the 15. Effects are noisier than for the 17 years old because of the lower number of observations, but as before, they seem to suggest that the policy had an effect in small municipalities. In particular in municipalities below 10,000 inhabitants. As opposed to 17 years old, where the main effect for Spanish women came from those who lived with their parents, for the 16 years old the effect comes from those who live alone or with their partner (around 18% of the 15 and the 16 years old women who abort report to live alone). For them, the fraction of 16 years old abortions relatives to 16 and 15 drops 11 absolute percentage points, which is statistically significant at the 1% level. We cannot look for heterogeneous effects among students because 15 years old, who are the natural control for 16 years old women, are not allowed to work and must be studying by law.

1.5.2 Temporal Effects. Bunching

Now consider a woman who gets pregnant at 17 but will turn 18 within the first 14 weeks of pregnancy. She can wait until she turns 18 to abort to avoid involving her parents. This section investigates if a significant number of women do so.

We first discuss how the PI laws generate a cost structure that can create incentives for bunching around aborting the 18th birthday or the days immediately after. These incentives apply only to those turning 18 within 14 weeks of pregnancy.[14] After discussing theoretically the incentives that could lead to bunching and the effects of the policy change, we discuss the identification strategy to quantify this bunching. Finally, we present the results of this analysis.

1.5.2.1 Motivation

We assume that the abortion costs for those women around 18 years old have two components: (i) gestational weeks, and (ii) the costs of involving their parents or making their case in front of the doctor to avoid telling their parents. We will refer to the later component as the C_i costs, which are specific to each woman i. We assume that the costs of aborting are increasing in the number of gestational weeks. The higher the gestational weeks at which the abortion is performed, the larger the costs the woman face. One can think of these costs as psychological costs caused by the uncertainty of waiting or costs of a more painful abortion procedure. We also assume that the cost function for individual i, $f_i(w)$ is continuous in w, gestational weeks. PI costs is a fixed cost that women pay only if they abort before turning 18. Notice that these PI costs can be heterogeneous across

[14]Here we consider 14 weeks of pregnancy for simplicity. In reality, the woman can still abort when she is 15-22 weeks pregnant if there is a risk for the mother or the fetus. Anecdotal evidence suggests that the psychological risk for the mother is a way that some women use when they want to abort after week 14.

women: some women would always tell their parents for support, some will considerate it a moderate cost while for those with complicated relationships with their parents or devoted catholic parents, it can be an enormous cost.

Therefore, the total abortion costs (TAC_i) for a particular woman as a function of w, the gestational weeks, and whether she aborts before or after turning 18 looks like:

$$TAC_i(w) = \begin{cases} C_i + f_i(w) & \text{if abortion day} < \text{18 birthday} \\ f_i(w) & \text{if abortion day} \geq \text{18 birthday} \end{cases}$$

Figure 1.7.4 illustrates an example of the effects of the PI costs for those women who turn 18 during their first 14 weeks of pregnancy. Each woman wants to minimize the abortion costs. Let T_i be the minimum week of pregnancy at which waiting to abort after the 18th birthday dominates for woman i. First, note that $T_i < 14$ because for simplicity we assume that abortion is only legal until week 14. Second, if the woman discovers that she is pregnant before T_i, she aborts immediately. Before T_i, $f_i(w)$ is low enough that even adding C_i, involving her parents, is still better than waiting to turn 18 to abort. However, at T_i, $f_i(w)$ is high enough that a reduction for not paying C_i is better. The decision rule is then straightforward: if pregnancy is discovered after T_i, wait and abort on 18th birthday.

Notice that in this set up there is no way to go to an earlier gestational week. Once T_i is crossed there is no way to decide to abort before week T_i. Specifically, if the woman learns that she is pregnant and doubts for a while what she should do and during this doubting time week T_i is crossed then the optimal thing to do is to wait until turning 18 despite having learned about the pregnancy slightly before T. This makes the bunching even stronger.

Suppose, for example, that the difference between the gestational weeks at which the woman turned 18, and T is equal to 4 weeks. If a woman turns 18 at week 12 of her pregnancy that would imply that $T = 8$. Women are very likely to know that they are pregnant by week 8 and thus, according to this simple model, they will decide to abort before turning 18. However, if a woman turns 18 at week 8, then $T = 4$, and thus there is a chance that she does not know that she is pregnant before T and when she learns it she will wait to turn 18 in order to abort. Again, she will abort immediately after turning 18 in order to minimize the abortion costs. This incentive structure creates a notch and will create bunching on the number of abortions immediately after women turn 18 and a missing mass of abortions "immediately" before women turn 18. Notice, however, that we do not expect a complete hole in the mass before turning 18 since for some women the PI costs are non-existing since they would have involved their parents no matter what.

We can incorporate the policy reform into this analysis. The policy reform, if anything, caused an increase in the PI costs for some women. Before the reform, it is possible to avoid involving the parents by making a case to the doctor. After the policy reform, there is no way to avoid involving the parents. That some women tried to avoid telling their parents shows that for these women involving them had higher costs. We can make the PI costs depend on the period:

$$TAC_i(w,t) = \begin{cases} C_i(t) + f_i(w) & \text{if abortion day} < 18 \text{ birthday} \\ f_i(w) & \text{if abortion day} \geq 18 \text{ birthday} \end{cases}$$

where:

$$C_i(t) = \begin{cases} C_{1i} & \text{if } t = 0 \text{ (before the reform)} \\ C_{2i} & \text{if } t = 1 \text{ (after the reform)} \end{cases}$$

where $C_{2i} > C_{1i}$ for some women i. For those women who see involving their parents as a support the law will continue to be irrelevant.

Figure 1.7.5 shows this analysis graphically. The increase in PI costs (C) decreases T_i, $T_{1i} < T_{2i}$. Women who find out that they are pregnant between T_{1i} and T_{2i} now wait instead of aborting immediately as they would have done before the reform. Bunching should thus increase as a result of the policy change.

We can do the same analysis for the 15 and 16 years old. Before the policy change, there is a PI cost decrease at 16 birthday since there is a chance for them to avoid involving their parents. Therefore, if this mechanism is relevant, we should observe some bunching on the number of abortions around their 16th birthday before the policy change. After the 2015 reform, in contrast to the 17 and 18 years old, there is no difference in abortion costs anymore between 16 years old and 15 years old. Therefore, any existing bunching should disappear.

1.5.2.2 Identification Strategy

To estimate excess bunching at the notch, we follow Kleven and Waseem (2013). We construct a variable, $\Delta 18_i = \text{abortion day}_i - \text{18th birthday day}_i$, to know when the abortion took place relative to the 18th birthday. Notice that $\Delta 18_i = 0$ if the abortion happens the same day as the woman turns 18, $= -1$ if it happens one day before she turns 18, $= +1$ if one day after turning 18, and so on. In order to estimate bunching we compare the empirical distribution with an estimated counterfactual distribution. To estimate the counterfactual density, we fit a flexible polynomial to the empirical density where we exclude observations in a range $[\Delta 18_L, \Delta 18_U]$ around the notch point $\Delta 18^* = 0$. Because $[\Delta 18_L, \Delta 18_U]$ contains observations that are affected by bunching responses, excluding them allows us to use the polynomial to estimate what would have been the density had the cost not increase at 18.

We group $\Delta 18_i$ variable into small bins indexed by j. Each bin specifies d_j, a range of number of days between abortion day and 18th birthday. So, the number of women in bin j is

$$c_j = \sum_i \mathbf{1}\{\Delta 18_i \in d_j\}$$

Let the indices of bins starting from $\Delta 18_L$ and ending at $\Delta 18_U$ be $\{L, L+1, ..., U\}$. We run the following regression:

$$c_j = \sum_{k=0}^{p} \beta_k \times j^k + \sum_{l=L}^{U} \gamma_l \times \mathbf{1}\{j = l\} + \nu_j \tag{1.2}$$

where p is the order of the polynomial. We estimate the counterfactual distribution using the predicted values from equation (1.2) excluding the contribution of the dummies in the excluded range, i.e., $\hat{c}_j = \sum_{k=0}^{p} \beta_k \times j^k$. We then estimate bunching as the difference between the observed and counterfactual bin counts in $[\Delta 18_L, \Delta 18_U]$. The excess bunching is the difference in $[\Delta 18^*, \Delta 18_U]$, $\hat{B} = \sum_{j=\Delta 18^*}^{\Delta 18_U} (c_j - \hat{c}_j)$, and the missing mass is the difference in $[\Delta 18_L, \Delta 18^*]$, $\hat{M} = \sum_{j=\Delta 18_L}^{j<Delta18^*} (\hat{c}_j - c_j)$.

Following Kleven and Waseem (2013), we calculate standard errors using a bootstrap procedure by random resampling of residuals in (1.2). Standard errors of each variable are then defined as the standard deviation in the distribution of that variable.

This method relies on plausible determining the excluded range $[\Delta 18_L, \Delta 18_U]$. Because bunching above the notch is considerably sharp, we can find $\Delta 18_U$ visually. $\Delta 18_L$ is less obvious because of the heterogeneity of T_i; different women may start waiting from different points. To estimate $\Delta 18_L$, we exploit the fact that the missing mass below the notch has to be equal to excess bunching above the notch, i.e., $\hat{M} = \hat{B}$.

1.5.2.3 Results

We pool the entire period 2011-2017, i.e., before and after the reform together, for power to estimate excess bunching in figure 1.7.6. On the horizontal axis, we have $\Delta 18$, which is binned at five days. On the vertical axis, we have the total number of abortions corresponding to each bin. The notch happens at $\Delta 18 = 0$. There is a jump on the number of abortions when the women turn 18 and a missing mass just before that. The excess mass lasts for three bins (15 days after women turn 18). We estimate this excess mass as $\hat{B} = 212.4$ extra abortions, which is statistically significant because the standard error is

33.56.

Following Kleven and Waseem (2013), we normalize this quantity. We compute $\hat{b}$ by dividing $\hat{B}$ by the counterfactual quantity at the notch bin. The excess mass represents 0.92 of the counterfactual at the notch bin, i.e., compared to the counterfactual the number of abortions almost doubled. Figure 1.7.8 repeats the same analysis with 10 days bins. Results are less noisy. There is still a statistically significant excess mass although the point estimate decreases to 164 with standard error 27.92.

Figure 1.7.7 analyzes the periods before and after the reform separately. Results become considerably noisier due to the lack of power. As a result, we cannot tell whether bunching became stronger after the PI costs increase. Figure 1.7.9 repeats the analysis with 10 days bins. Results become less noisy, but they are still considerably noisy to conclude that this mechanism became more important after the reform.

Figure A.2.3 in the appendix does a placebo analysis and shows that, as expected, there is no bunching on the number of abortions around 19th, 20th, and 25th birthdays. This shows that the effects we find around the 18th birthday are not "birthday effects", but that they are caused by the different incentives (PI laws) that 17 and 18 years old face.

Figures A.2.4 and A.2.5 in the appendix show that there was no bunching around the 16th birthday neither before nor after the reform. This provides evidence that the 15 years old who could, did not delay their abortions until turning 16 before the policy reform to try to avoid involving their parents by making their case in front of the doctor.

1.6 Indirect Effects

Given the direct effects documented above on the minors' abortions, a natural follow up question is what happens to those missing abortions. There are four possibilities: minors traveling for abortions, aborting illegally, fewer pregnancies, and higher births. First, although it is possible to travel for abortions, in our case, teenagers have to travel to a foreign country which is considerably more costly than traveling across states in the US.

Second, although we cannot completely rule out illegal abortions, from our understanding of the Spanish context this is unlikely since there have been no reports about it.

We address the two remaining possibilities in turn: fewer pregnancies and more births. By computing conception data and analyzing the effects of the reform on conceptions we discuss the relative importance of both responses.

1.6.1 Permanent Effects. Changes in Sexual Behavior

If women are forward-looking, they anticipate the PI costs increase if they need an abortion before the 18th birthday, and thus modify their behavior in to avoid unwanted pregnancies. Note that this is true even before the reform since minors still had to make the case to the doctors or inform their parents. But the law change sharpened the incentives to avoid an unwanted pregnancy before turning 18.

Ideally, we would like to have data on women's sexual behavior. Unfortunately, we do not have this data, but our precise data on abortions permits an indirect method to evaluate this channel by measuring the permanent effects on abortions. Specifically, if minors change their sexual behaviors, we should observe a discontinuous jump in abortions after 18 once we exclude all the observations around the 18th birthday. Excluding the observations around the 18th birthday essentially removes the temporary effects in section 1.5.2. By testing whether the jump becomes more significant after the policy change we can answer whether the reform changed minors' sexual behavior.

One concern of this approach is that at the 18th birthday threshold there are other discontinuous changes, for example, drinking age. Indeed, this is a problem to interpret the jump before the policy change as causal. However, at the time of the reform, to the best of our knowledge, nothing else changed at this threshold. As a result, we can interpret the increase in the jump as the causal effects from the reform. In other words, if after excluding all the temporary displacement the jump increases, it can be interpreted as women modifying their sexual behavior (either by using more contraceptives or decreasing the number of sexual relationships, we cannot distinguish among those) in order to avoid becoming pregnant while being 17 years old.

1.6.1.1 Identification Strategy

We use an analysis similar to a *Regression Discontinuity* (*RD*) method, where the running variable is $\Delta 18_i$. However, we note that the woman can choose $\Delta 18_i$ and therefore the analysis is not an RD. However, we borrow this method to measure the size of the jump after *excluding* the observations around the threshold. Specifically, we exclude the abortions that took place 50 days, 75, days or 100 days before and after the woman turns 18 and analyze if there is a jump in the number of abortions, and if the size of this jump changes when the PI laws changed (2015 reform).

Let $d \in \{0, \pm 1, \pm 2, ...\}$ be all possible values of $\Delta 18_i$. Let the period, T, take two values, $\{0, 1\}$ where 0 indicates the period before the reform, and 1 indicates the period after the reform. Similar to section 1.5.2, we define the count variable

$$Y_{dT} = \sum_i \left\{ \Delta 18_i = d \,\&\, \text{abortion day}_i = T \right\}$$

The main assumption of this identification strategy is that nothing else changed at the time of the policy change affecting differently those women below 18 than those above 18. Therefore, we are assuming that if the policy change had not taken place, the size of the jump would have stayed constant.[15] We estimate the following regression:

$$\begin{aligned} Y_{dT} = \alpha + \beta_1 \times a18_d + \beta_2 \times a18_d \times POST_T + \beta_3 \times d + \beta_4 \times d \times POST_T + \\ + \beta_5 \times a18_d \times d + \beta_6 \times a18_d \times d \times POST_T + \beta_7 \times POST_T + \epsilon_{dT} \end{aligned} \tag{1.3}$$

[15]Notice that when we did the bunching analysis in section 1.5.2 we were analyzing the temporary effects of women waiting to turn 18 in order to abort. Here we try to exclude these women and see if there is still a jump in the number of abortions when the woman turns 18 years old.

where $POST_T = 1$ if $T = 1$ (after the reform), and $= 0$ if $T = 0$ (before the reform). $a18$ is a dummy equal to 1 if $d \geq 0$. Our coefficient of interest is β_2, the coefficient on $a18_d \times POST_T$, which measures the change in the jump occurring after the policy. As mentioned before, we exclude those abortions that happened 50, 75 or 100 days before and after turning 18.

1.6.1.2 Results

Figures 1.7.10-1.7.12 show the main results when we exclude abortions that happened 50, 75, and 100 days before and after the woman turns 18 years old. In the horizontal axis, we have d, with binned at 15 days, and on the vertical axis, we have the absolute number of abortions that took place in that bin (Y_{dT}). We plot the counts for the whole period 2011-2017, and then we divide them into two periods, before ($POST_T = 0$)and after ($POST_T = 1$) the reform. These figures show how a linear polynomial predicts well the increase in the number of abortions as women get older for women aged 16-20 years old and hence aborted within 730 days before and after turning 18 years old. We can see how even after excluding all the potential temporary effects, there is a discontinuous jump at age 18 relative to what we would have predicted. The relative size of this jump gets larger after the policy change.[16]

Table 1.7.9 provides the regression analysis from equation 1.3 which confirms the visual analysis from the figures above. Results are stable when we exclude 50, 75, and 100 observations before and after aborting the day one turns 18. The policy change increased the size of the jump by about 10%, which is statistically significant at the 5% level. Before the policy change, the jump was around 12%, significant at the 1% level, but we cannot interpret this as causal. These results suggest that a considerable percentage of women modified their behavior in order to avoid becoming pregnant after the law change that

[16]The number of abortions in the before and after figures have been normalized for the length of each period (4 years 9 months and 21 days before the reform, and 2 years 3 months and 9 days after the reform). The counts in the vertical axis are normalized to "per year" abortions.

forced them to inform their parents in case of abortion.

Section A.3 in the appendix does a placebo test by doing the same analysis around ages 19 and 20. The interaction coefficients ($a20xPOST$ and $a19xPOST$) are not significant for any of the specifications as expected.[17,18]

1.6.2 Effects on Fertility

1.6.2.1 Identification Strategy

In order to test if the policy change increases the number of births of 16-17 years old, we use a *difference-in-difference* approach. As in the analysis of the effects on the number of abortions, the policy design allows for a *difference-in-difference* analysis where women aged 17 (or 16) are the treated group and women aged 18 (or 15) are the control group. However, while we can use the woman's age as reported from the abortion data to define her treatment status, we can not do the same for the birth data. This is because her treatment status is defined by her age at 14 weeks of pregnancy, not at birth. The age at 14 weeks of pregnancy defines her abortion possibilities, which were the subject of the reform. As a result, besides using the woman's birthday and her exact day of giving birth, we also use information on gestational weeks to construct her age at 14 weeks of pregnancy. The treatment group consists of those who are 17 at 14 weeks of pregnancy and the control group of those who are 18 at 14 weeks of pregnancy. We can estimate the age of the woman when she was 14 weeks pregnant by using the exact day in which the woman gave birth, the exact day in which the woman was born, and the gestational weeks at which the woman gave birth.[19]

Besides the nuance constructing the treated group, there is a concern regarding selec-

[17] $a19$ is equal to 1 if $\Delta 19 \geq 0$ and 0 otherwise. $a20$ is equal to 1 if $\Delta 20 \geq 0$ and 0 otherwise.

[18] However, the coefficients on $a19$ and $a20$ (the original discontinuous jump before the policy change around age 19 and age 20) are significantly negative. We do not have any proper interpretation of why this is the case.

[19] We perform the same analysis defining age 17 at 22 weeks of pregnancy since during the first 22 weeks of pregnancy the woman can abort if there is a physical or psychological risk for the mother.

tion into treatment shortly after the policy change. Those being in the treated group (17 years old during the first 14 weeks of pregnancy) in the treated period between September 21st, 2015 (day of the policy change) and December 2015 were already pregnant at the moment of the abortion law change and could have aborted without parental consent by aborting before the policy reform. The fact that they had not aborted despite the fact that the policy reform had been announced in advance suggests some selection into being pregnant during this period. Because of that, we will divide the treated period into two and analyze separately the periods Sept 21, 2015 - Dec 2015 and Jan 2016 onward. For the Jan 2016 onward, we may expect that, if anything, fertility for 17 years old increases relative to 17 years old. However, for the Sept, 2015 - Dec 2015 period, the effects of the policy are less clear.

Following the same logic as before in the direct effects on abortion, we will analyze what happens to the fraction of 17 years old women at gestational week 14 giving birth relative to the whole population of 17-18 years old at gestational week 14 population. The assumption, again, is that this fraction would have stayed constant in the absence of the policy change.

Therefore, we are estimating the following regression:

$$age17_{imt} = \alpha + \beta \times POST_{imt} + \mu_t + \eta_m + \epsilon_{imt} \tag{1.4}$$

where:

$$POST_t = \begin{cases} 1 & \text{if 14 weeks pregnancy day} \geq \text{Jan, 2016} \\ 0 & \text{if 14 weeks pregnancy day} < \text{Sept 21, 2015} \end{cases}$$

$$age17_{mt} = \begin{cases} 1 & \text{if woman's age at 14 weeks pregnancy=17} \\ 0 & \text{if woman's age at 14 weeks pregnancy=18} \end{cases}$$

where $age17_{imt}$ refers to woman i who is 14 weeks pregnant in year t and month m. μ_t are year fixed effects, and η_m are month fixed effects. The coefficient of interest is β. The period Sept 21, 2015 - Dec 2015 is excluded from these regressions for the reasons discussed before. Another regression using this period as the treated period and excluding the period from Jan 2016 onward is estimated separately.

1.6.2.2 Results

Tables 1.7.10-1.7.11 show the main results regarding the fertility effects. They exclude the women who were 14 weeks pregnant between September 21st, 2015, and December 31st, 2015. Therefore, the treated period includes the women who were 14 weeks pregnant in 2016 and 2017. Table 1.7.10 shows that when we analyze all the women in our sample, there were no effects on the fraction of births for 17 years old women at 14 weeks of pregnancy relative to the population of 17-18 years old women. The result is the same for Spanish versus non-Spanish women.

We look for heterogeneous responses on births. First, Table 1.7.11 divides the sample by whether the mother is still a student or not. We do not find any big differences by whether the mother is still studying or not, but we find strong statistically significant positive effects among women who are still studying (9.06 absolute percentage points. 17 years old represent around 46% of all the births of the 17-18 years old in this population group) and significant effects among those who are not studying (4.49 absolute percentage points). Table 1.7.12 explores the reason why we find significantly positive effects among students and non-students when we did not find any effects when we analyzed the overall effects. This table divide the sample by whether the information on the mother's profession (including whether the mother is a student or not) is missing or it is not missing. We can see that when we exclude those observations with missing information on the mother's profession, we find a positively significant effect at the 5% level (point estimate 0.0583). That is the fraction of births of mothers who were 17 years old in the 14th week of pregnancy increase relative to the population of these mothers and those who were 18 at week 14th.[20]

Second, Table 1.7.13 explores the existence of heterogeneous responses across women with different education level.[21] At age 16, Spanish students who are doing well in school should obtain the "ESO" level. Therefore, all women aged 17 or older who do well in school should have this title. Not having it is a sign of not having progressed adequately in school. We divide our sample by whether women have obtained this education level or not. We find strong positive effects on fertility for those women who do not have the title (6.16 absolute percentage points, significant at the 5% level. 17 years old represent around 43% of all the births of the 17-18 years old in this population group). Women with Spanish nationality mainly drive these effects (effects of 10.9 absolute percentage points significant at the 1% level). For women born abroad and without Spanish nationality, the signs become negative (opposite to what we would expect), but they are not significant. Finally, for those women with the "ESO" level, we do not observe any effects neither for Spanish women nor for non-Spanish.

Tables A.4.3-A.4.5 in the appendix show that the main results are robust to defining age 17 at 22 weeks of pregnancy instead of 14. As discussed before, abortion is allowed until week 22 of pregnancy if there is a risk for the mother or the fetus, and a doctor certifies it.

Tables A.4.1-A.4.2 in the appendix provide the results when we estimate our model considering the treated from September, 21st 2015-Dec 2015. However, as discussed before, this is hard to interpret since those women were already pregnant when the policy was announced and decided not to abort despite the policy having been announced in

[20]Notice that those with missing values from Table 1.7.12 are considered neither students nor non-students and thus are excluded from this table.

[21]Unfortunately, we do not have the same covariates (whether they live with their parents or not, for example) that we had in the case of the abortion data.

advanced. We can see a significant negative coefficient for Spanish women, but a positive significant coefficient for non-Spanish women on the effects of the policy on the fraction being of mothers being 17. However, again, it is hard to read much into these numbers.

1.6.3 Effects on Conceptions

We combine the abortions data set with the births data set to construct the total number of conceptions that took place in Spain.[22] As we find some suggestive evidence that risky sexual behaviors leading to pregnancies decrease but births increase, we pool the abortion and birth data together to find the effect on conceptions. This allows us to understand the relative importance of our two indirect effects.

1.6.3.1 Identification Strategy

We use a similar *difference-in-difference* stratey as in the fertility data. We estimate the month-year of the conception of each pregnancy by combining the exact day of the abortion or birth with the number of gestational weeks.

We then follow the same strategy as in the fertility data to construct a dummy equal to 1 if the woman was 17 years old at the 14 week of pregnancy and 0 if she was 18.[23]

We then run the same regression as in the fertility analysis, but with the universe of conceptions that took place in Spain and including conception year and month fixed effects. The $POST$ dummy, which is the variable of interest, is equal to 1 if the 14 weeks of pregnancy was before October, 2015 and 0 if it was before.

1.6.3.2 Results

Table 1.7.14 show the main results. 17 years old conceptions significantly fell (2.23 absolute percentage points) as a result of the PI law reform relative to the 18 years-old ones. The results are driven by Spanish women. The point estimates of these magnitudes

[22]Note that $Total\,Conceptions = Births + Abortions$.
[23]Note that for abortions happening before gestational week 14 this is an hypothetical exercise.

are, as expected, smaller in absolute terms than the abortion drop. However, the results are still significantly negative which suggests the importance of the risky sexual-behavior responses from minors.

1.7 Conclusion

This paper contributes to the literature on the effects of parental involvement laws on minors' abortions and their responses by bringing a rich population-level data on abortions in Spain. By exploiting a law change that increased the abortion costs for 16 and 17 years old women without affecting women of other ages we have documented that forcing minors to involve their parents decreases the abortions of the 17 years old women relative to the abortions of the 18 years old. These results are mainly driven by women who are still studying and from women who live in small towns as opposed to big cities. We do not find any effects on the 16 years old abortions consistent with existing evidence that younger minors are more likely to involve their parents regardless of the regulatory environment. Moreover, by bringing in the bunching methods from the Public Finance literature we document the existence of bunching on the number of abortions happening immediately after women turn 18. This provides evidence that some women delay their abortions, with the corresponding risks and costs of aborting at a later gestational week, in order to avoid involving their parents.

After documenting the effects on 17 years abortions we turn to explore other behavioral responses. If there are less abortions it has to be the case that there are either less pregnancies or that there are more births (or a combination of both). We provide suggestive evidence that there are less pregnancies by exploiting the discontinuous jump on the number of abortions at age 18 once we exclude all the temporal displacement from the bunching. This suggests that minors modify their "risky" sexual behavior to avoid unwanted pregnancies when the costs of abortions increases. Despite that, we document for some groups, including those who are still studying, the number of births increases suggesting that some minors ended up with an unwanted child as a result of the reform. Overall, however, the total number of conceptions decrease suggesting the importance of this response channel.

This paper has thus provided evidence that minors respond to laws that force them to involve their parents. Being aware of that is important to have an informed debate on whether to implement or not to implement these laws. A relevant dimension that this paper has not been able to study is the effects on minors well-being of involving their parents in an abortion process. Trying to quantify this effect and understanding whether it is positive or negative is another relevant dimension to take into account to fully understand the effects of these laws.

Figures and Tables

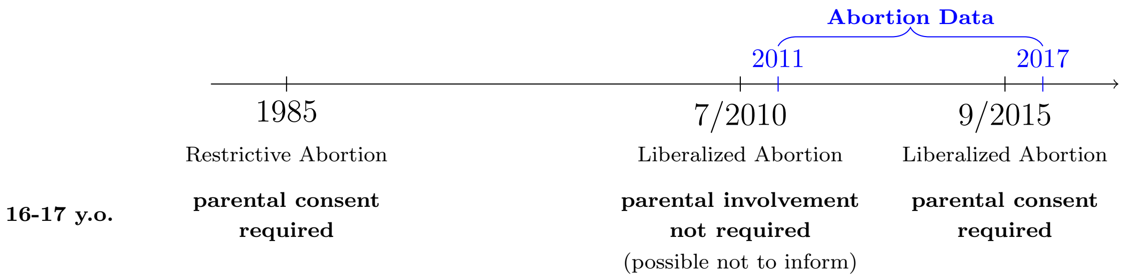


Figure 1.7.1: Policy Figure

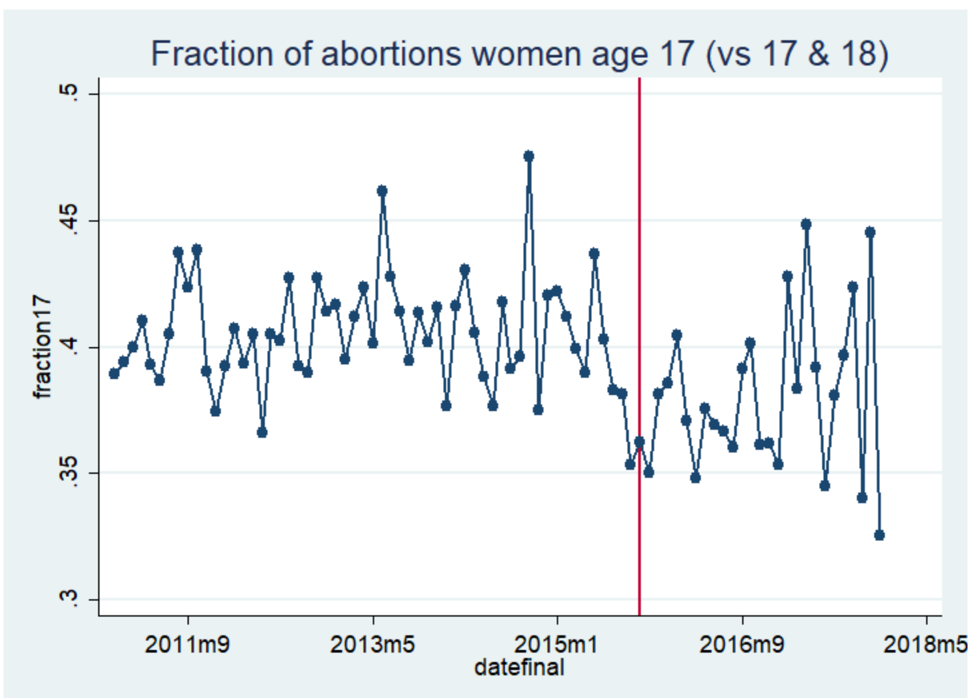


Figure 1.7.2: Fraction of abortions of women aged 17 relative to the abortions of the 17-18 years old women by month-year.

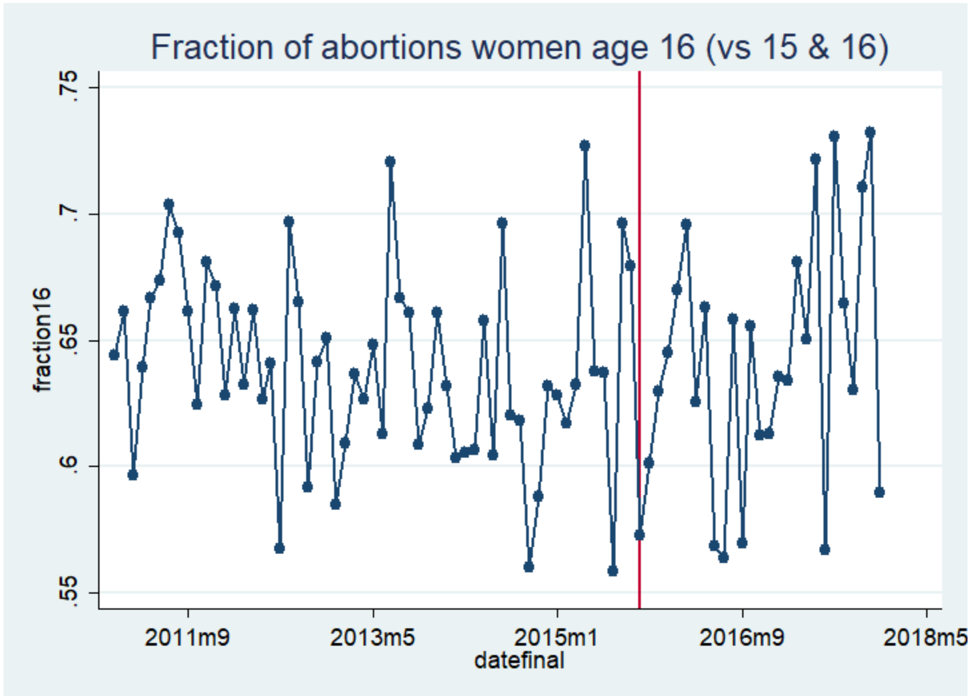


Figure 1.7.3: Fraction of abortions of women aged 16 relative to the abortions of the 15-16 years old women by month-year.

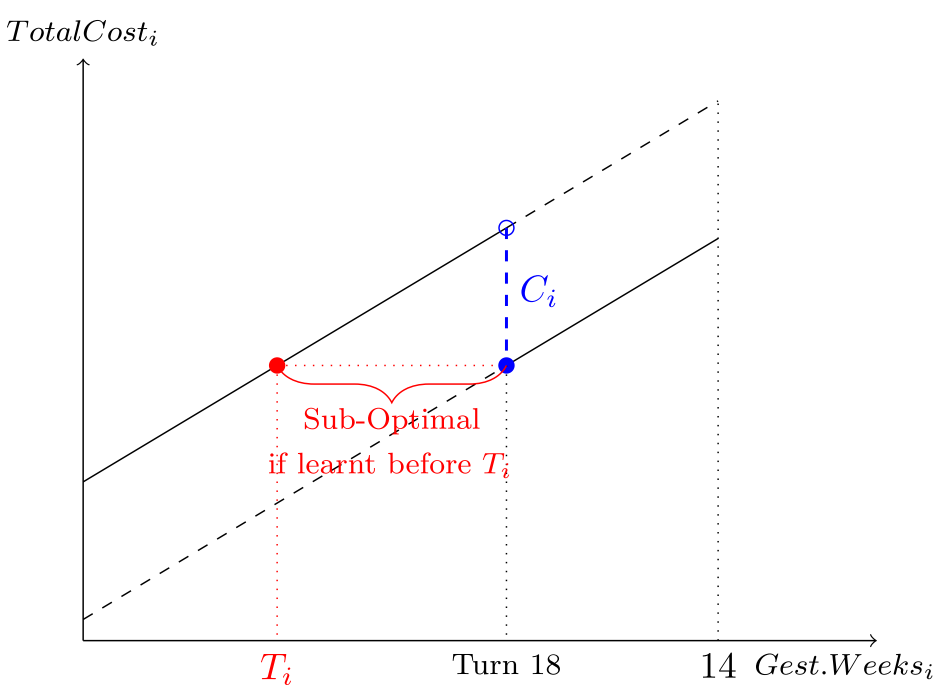


Figure 1.7.4: Total Abortion Cost

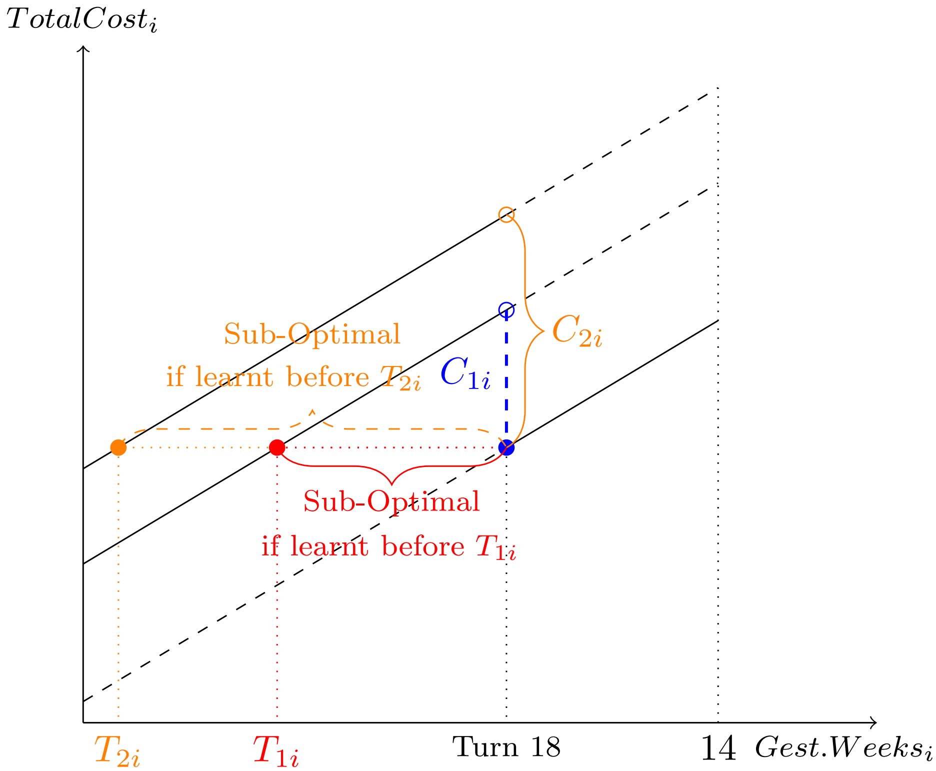


Figure 1.7.5: Total Abortion Cost. Policy Change

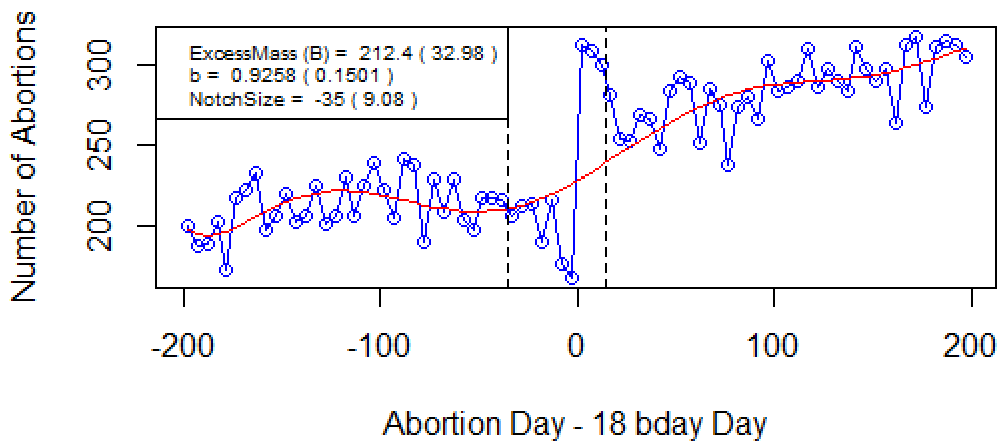
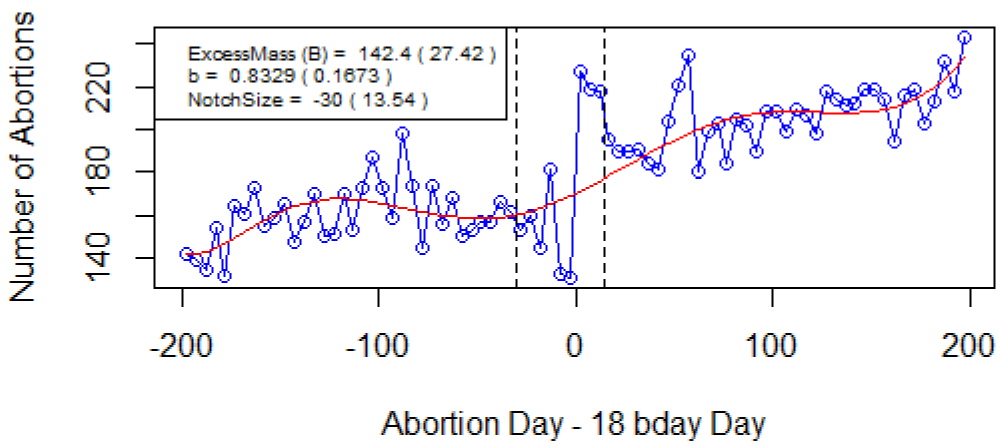
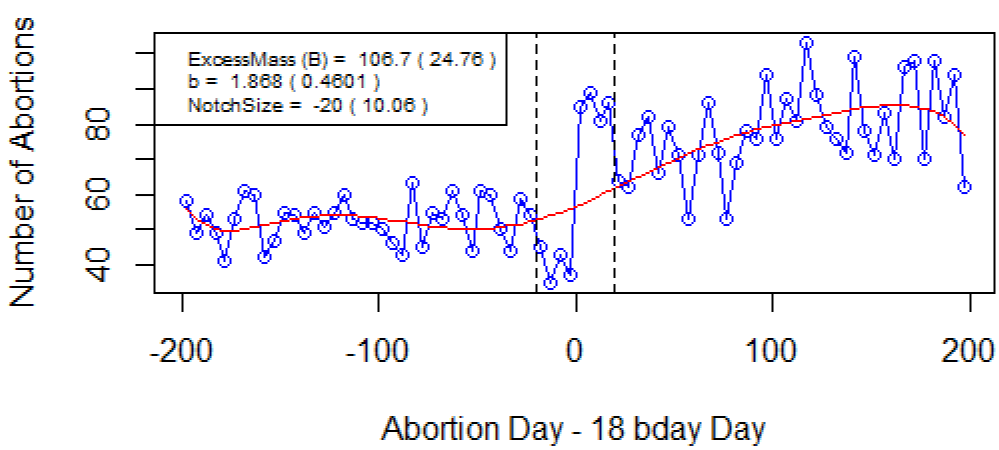


Figure 1.7.6: Bunching Analysis. All 2011-2017. Binwidth 5 days.

Notch is at $\Delta 18 = 0$. Binwidth 5 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days, polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.



(a) Before the policy reform



(b) After the policy reform

Figure 1.7.7: Bunching Analysis. Before and After the Reform. Binwidth 5 days.

Notch is at $\Delta 18 = 0$. Binwidth 5 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days (19 for the after), polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.

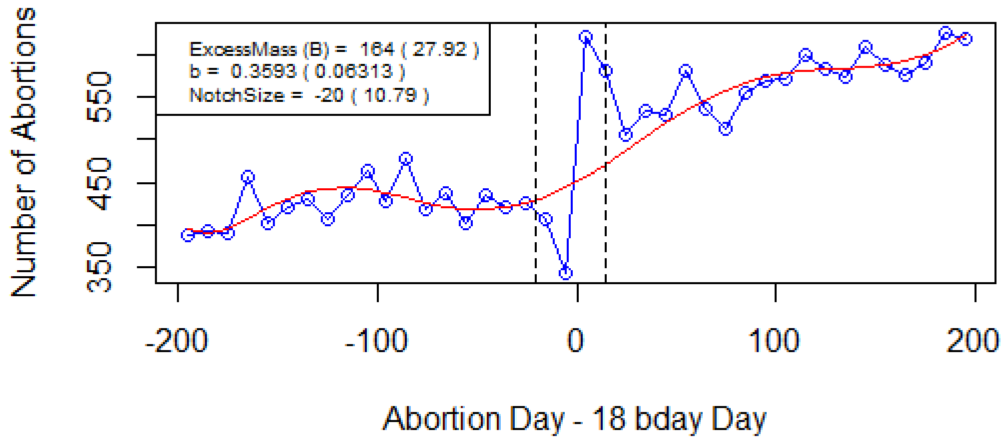
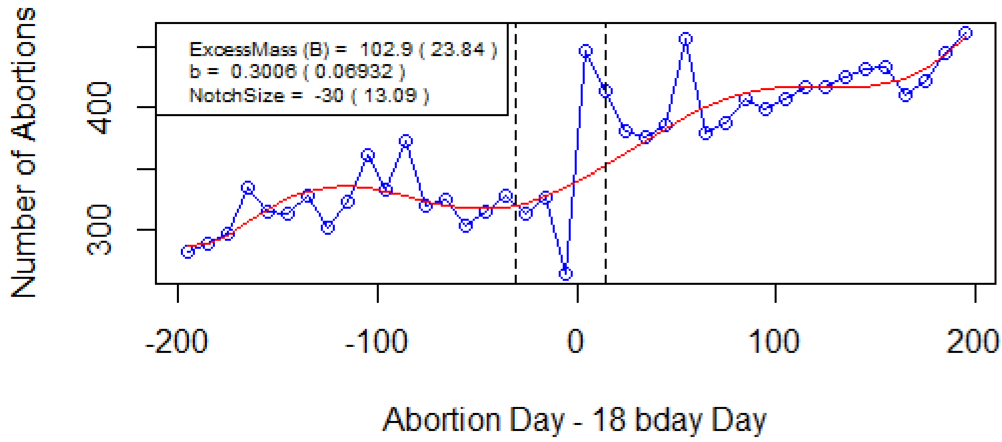
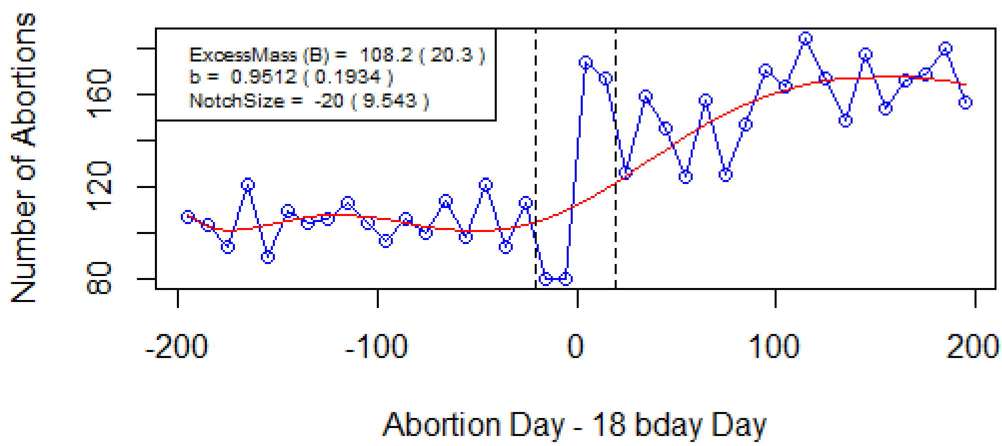


Figure 1.7.8: Bunching Analysis. All 2011-2017. Binwidth 10 days.

Notch is at $\Delta 18 = 0$. Binwidth 10 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days, polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.



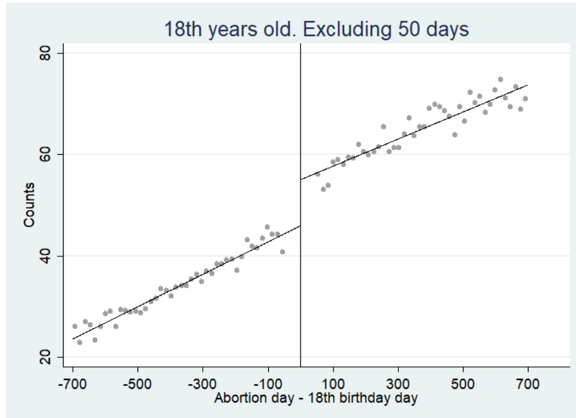
(a) Before the policy reform



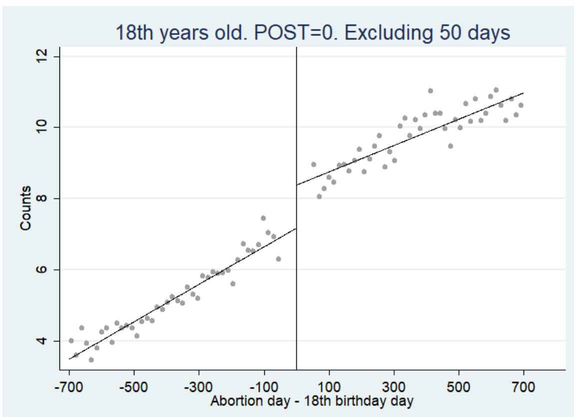
(b) After the policy reform

Figure 1.7.9: Bunching Analysis. Before and After the Reform. Binwidth 10 days.

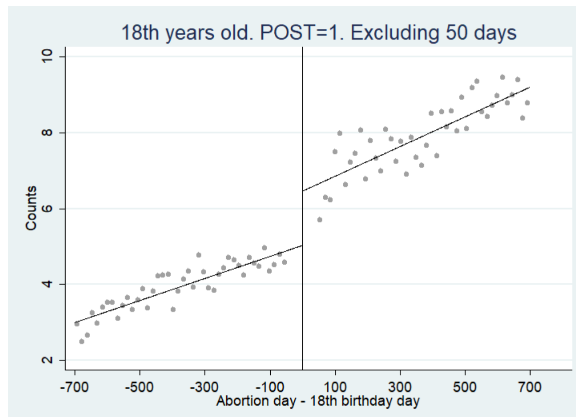
Notch is at $\Delta 18 = 0$. Binwidth 10 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days $(19$ for the after), polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.



(a) All



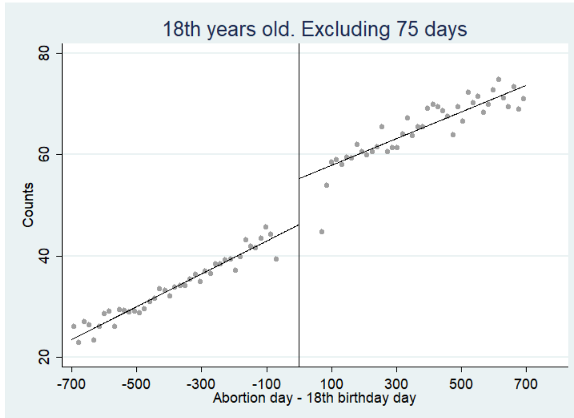
(b) Before



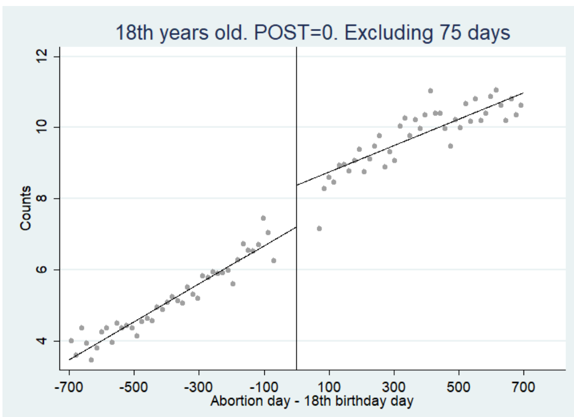
(c) After

Figure 1.7.10: Permanent Effects. Excluding 50 observations before and after threshold.

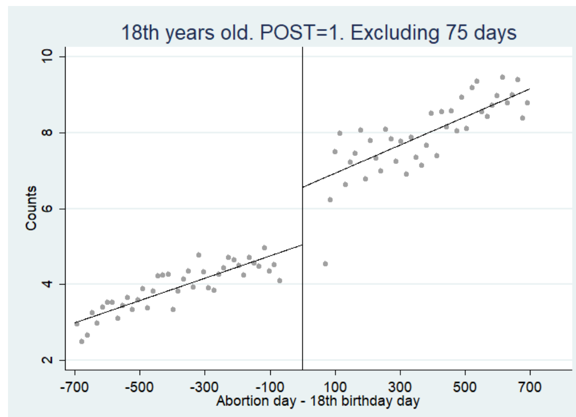
We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).



(a) All



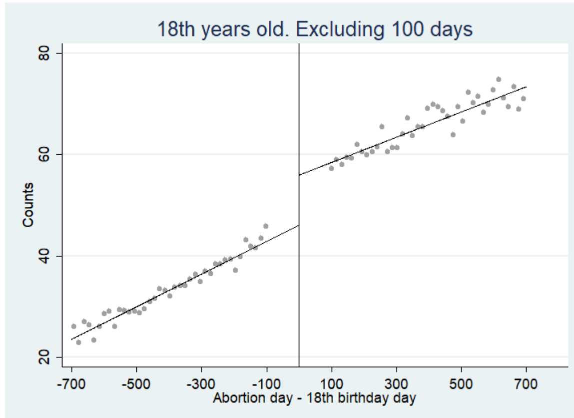
(b) Before



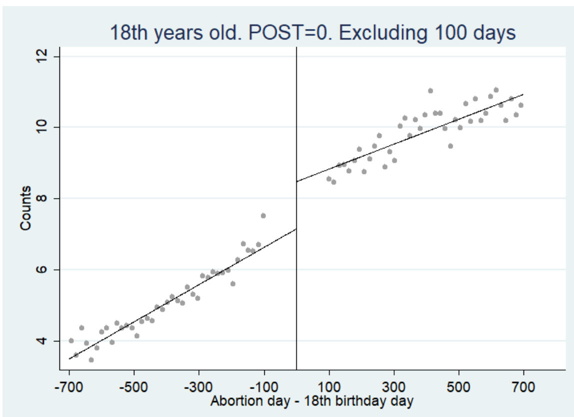
(c) After

Figure 1.7.11: Permanent Effects. Excluding 75 observations before and after threshold.

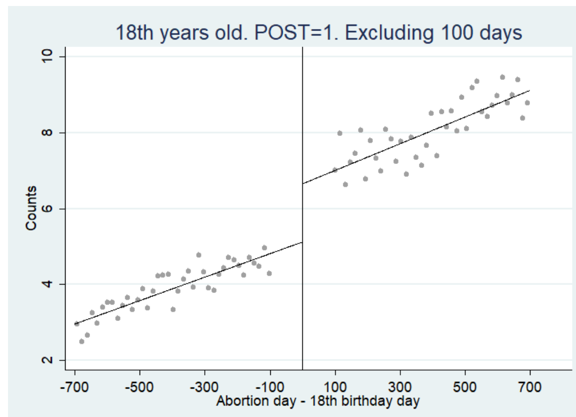
We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).



(a) All



(b) Before



(c) After

Figure 1.7.12: Permanent Effects. Excluding 100 observations before and after threshold

We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).

Table 1.7.1: Summary Statistics on abortions by age

	15 y.o.	16 y.o.	17 y.o.	18 y.o.	15-18 y.o.
Woman has ESO or more	0.532	0.646	0.728	0.787	0.714
	(0.499)	(0.478)	(0.445)	(0.409)	(0.452)
Spanish	0.734	0.736	0.724	0.695	0.716
	(0.442)	(0.441)	(0.447)	(0.460)	(0.451)
Student	0.926	0.836	0.737	0.576	0.711
	(0.262)	(0.370)	(0.440)	(0.494)	(0.453)
Lives with her parents	0.758	0.739	0.710	0.636	0.691
	(0.428)	(0.439)	(0.454)	(0.481)	(0.462)
Own Income	0.0157	0.0305	0.0606	0.150	0.0866
	(0.124)	(0.172)	(0.239)	(0.357)	(0.281)
Gestational Weeks	9.148	8.929	8.649	8.420	8.665
	(3.909)	(3.708)	(3.418)	(3.232)	(3.471)
Abortion paid by the Gov	0.748	0.748	0.746	0.750	0.749
	(0.434)	(0.434)	(0.435)	(0.433)	(0.434)
Total number abortions	0.0737	0.0822	0.118	0.184	0.133
	(1.318)	(0.303)	(0.371)	(0.455)	(0.581)
Number of living children	0.0406	0.0407	0.0801	0.124	0.0860
	(1.305)	(0.216)	(0.307)	(0.374)	(0.533)
Observations	5858	10444	14436	21816	52554

mean coefficients; sd in parentheses

Table 1.7.2: Summary Statistics on fertility by age

	15 y.o.	16 y.o.	17 y.o.	18 y.o.	15-18 y.o.
Mother has ESO or more	0.141	0.176	0.249	0.337	0.272
	(0.348)	(0.381)	(0.432)	(0.473)	(0.445)
Spanish	0.724	0.740	0.720	0.692	0.711
	(0.447)	(0.438)	(0.449)	(0.462)	(0.453)
Mother Student	0.470	0.384	0.325	0.253	0.312
	(0.499)	(0.486)	(0.468)	(0.435)	(0.463)
Married	0.0268	0.0364	0.0623	0.104	0.0747
	(0.161)	(0.187)	(0.242)	(0.305)	(0.263)
Number of Children	0.0166	0.0529	0.0785	0.121	0.0896
	(0.128)	(0.243)	(0.281)	(0.349)	(0.303)
Gestational Weeks	38.53	38.65	38.78	38.83	38.76
	(2.288)	(2.260)	(2.191)	(2.159)	(2.195)
Father's Age	21.58	21.47	22.36	23.53	22.73
	(6.015)	(5.083)	(5.018)	(5.231)	(5.265)
Father Student	0.206	0.155	0.128	0.0909	0.119
	(0.404)	(0.362)	(0.334)	(0.287)	(0.324)
Observations	2168	5029	9118	14026	30341

mean coefficients; sd in parentheses

Table 1.7.3: Effects on the fraction of 17 years old abortions relative to 17-18 abortions

	All	Spanish	Non-Spanish
POST	-0.0398**	-0.0528**	-0.0256
	(0.0165)	(0.0179)	(0.0279)
Abortion Year FE	X	X	X
Abortion Month FE	X	X	X
Province FE	X	X	X
Y mean	.4	.41	.37
Observations	35399	25199	9594
Adjusted R-squared	.001	.0006	.0046

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.4: Effects on the fraction of 17 years old abortions relative to 17-18 abortions by who they live with

	All		Spanish		Non-Spanish	
	Alone/Part	Parents	Alone/Part	Parents	Alone/Part	Parents
POST	-0.0477	-0.0417*	-0.0437	-0.0546**	-0.0726*	-0.0188
	(0.0290)	(0.0232)	(0.0361)	(0.0255)	(0.0383)	(0.0413)
Abortion Year FE	X	X	X	X	X	X
Abortion Month FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	.35	.43	.37	.43	.31	.42
Observations	11041	22885	7210	16998	3641	5510
Adjusted R-squared	.0238	.0013	.02	.0012	.0277	.0029

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.5: Effects on the fraction of 17 years old abortions relative to 17-18 abortions by whether they are studying or not

	All		Spanish		Non-Spanish	
	Student	Non-Student	Student	Non-Student	Student	Non-Student
POST	-0.0658***	0.00665	-0.0701***	-0.0145	-0.0879**	0.0499
	(0.0172)	(0.0262)	(0.0189)	(0.0339)	(0.0360)	(0.0367)
Abortion Year FE	X	X	X	X	X	X
Abortion Month FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	.46	.29	.46	.31	.47	.25
Observations	22222	12596	16429	8371	5387	4037
Adjusted R-squared	.0029	.0059	.0026	.0053	.009	.0049

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.6: Effects on the fraction of 17 years old abortions relative to 17-18 abortions by population size

	<10,000	10,000-50,000	50,000-500,000	>500,000
POST	-0.0950*	-0.122**	0.0185	-0.0320
	(0.0537)	(0.0466)	(0.0257)	(0.0395)
Abortion Year FE	X	X	X	X
Abortion Month FE	X	X	X	X
Province FE	X	X	X	X
Y mean	.42	.41	.4	.37
Observations	5034	9057	14216	7092
Adjusted R-squared	-.0012	.0043	.001	.003

Standard errors in parentheses clustered at the province level.

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.7: Effects on the fraction of 16 years old abortions relative to 16 and 15 years old abortions

	All	Spanish	Non-Spanish
POST	-0.0287	-0.0287	-0.0189
	(0.0226)	(0.0276)	(0.0558)
Abortion Year FE	X	X	X
Abortion Month FE	X	X	X
Province FE	X	X	X
Y mean	.64	.64	.64
Observations	15725	11821	3683
Adjusted R-squared	.0023	.0029	.0009

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.8: Effects on the fraction of 16 years old abortions relative to 16 and 18 years old abortions

	All	Spanish	Non-Spanish
POST	0.0116	0.00512	0.0200
	(0.0168)	(0.0232)	(0.0374)
Abortion Year FE	X	X	X
Abortion Month FE	X	X	X
Province FE	X	X	X
Y mean	.32	.34	.29
Observations	31392	22484	8389
Adjusted R-squared	.0019	.0014	.0035

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.9: Effects on Log number of abortions. Excluding different observations around the threshold. P(1). Bandwidth 730 days

	Excl. 50	Excl. 75	Excl. 100
a18	0.125***	0.117***	0.132***
	(0.0213)	(0.0233)	(0.0251)
a18xPOST	0.108**	0.125**	0.104**
	(0.0440)	(0.0467)	(0.0499)
Observations	2724	2624	2524
Adjusted R-squared	.86	.86	.86

Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.10: Effects on fertility for those women who were not pregnant when the policy came into effect. 14 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015

	All	Spanish	Non-Spanish
POST	0.0186	0.0342	-0.0269
	(0.0167)	(0.0223)	(0.0302)
Year FE	X	X	X
Month FE	X	X	X
Prov FE	X	X	X
Y mean	.39	.4	.37
Observations	19675	12762	4986
Adjusted R-squared	.0022	.0032	.0013

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.11: Effects on fertility for those women who were not pregnant when the policy came into effect. 14 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015. By whether the mother is a student or not

	All		Spanish		Non-Spanish	
	Student	No-Student	Student	No-Student	Student	No-Student
POST	0.0906**	0.0449**	0.115*	0.0744**	0.0291	-0.0183
	(0.0418)	(0.0214)	(0.0601)	(0.0239)	(0.0689)	(0.0438)
Year FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Prov FE	X	X	X	X	X	X
Y mean	.46	.37	.45	.39	.48	.34
Observations	4508	11818	2585	8065	1079	3009
Adjusted R-squared	.0049	.0042	.0125	.0047	-.0058	.0057

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.12: Effects on fertility for those women who were not pregnant when the policy came into effect. 14 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015. By whether the mother's profession (incuding being a student) is missing or not

	All		Spanish		Non-Spanish	
	NotMiss	Missing	NotMiss	Miss	NotMiss	Miss
POST	0.0583**	-0.0131	0.0861***	-0.0686	-0.00952	0.0840
	(0.0183)	(0.0469)	(0.0244)	(0.0545)	(0.0423)	(0.0733)
Year FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Prov FE	X	X	X	X	X	X
Y mean	.4	.36	.41	.38	.38	.33
Observations	16379	3296	10694	2068	4096	890
Adjusted R-squared	.0035	.0024	.0055	.0075	-.0008	.0042

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.13: Effects on fertility for those women who were not pregnant when the policy came into effect. 14 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015. By education level

	All		Spanish		Non-Spanish	
	No-ESO	ESO	No-ESO	ESO	No-ESO	ESO
POST	0.0616**	0.00397	0.109***	-0.000913	-0.0831	-0.0491
	(0.0267)	(0.0365)	(0.0309)	(0.0487)	(0.0529)	(0.0700)
Year FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Prov FE	X	X	X	X	X	X
Y mean	.43	.35	.44	.36	.41	.33
Observations	8791	4139	6317	2496	1910	1080
Adjusted R-squared	.0013	.0029	.0043	.0107	-.0059	.0102

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 1.7.14: Effects on the fraction of 17 years old conceptions (defined as being 17 at gestational week 14) relative to 17-18 conceptions

	All	Spanish	Non-Spanish
POST	-0.0223**	-0.0293**	-0.0000996
	(0.00956)	(0.0129)	(0.0247)
Conception Year FE	X	X	X
Conception Month FE	X	X	X
Province FE	X	X	X
Y mean	.4	.41	.38
Observations	59093	40440	15694
Adjusted R-squared	.0008	.001	.0009

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Chapter 2:

The Effect of Liberalizing Abortion on Sex-selection: Evidence from Spain

2.1 Introduction

Son preferences in China and among Chinese immigrants to the West have been widely documented in the literature.[1] A regular pattern in all these studies is that the boy-birth percentage is normal for the first child, but it increases significantly at higher birth order. A channel that is likely to play a considerable role in sex ratios at birth is sex-selective abortions. Sex-selective abortions, beyond the existence of parents' preferences for sons, require two technologies. First, one that reveals the gender of the fetus before the abortion is performed. Second, one that facilitates the abortion of the fetus once the gender is known.

This paper focuses on understanding the effects of reducing the costs performing an abortion on sex-selection in a situation where ultrasound technology to identify the gender of the fetus is available. Specifically, we study the effects of the abortion liberalization reform that took place in Spain in 2010 on Chinese immigrants' sex-selection.[2] Before this reform, abortion was only allowed in Spain under some circumstances certified by the doctors or the police. After the reform, women could freely abort without providing any justification.[3]

[1]See, for example, Abrevaya (2009) or Almond et al. (2013) among others.

[2]*Law 2/2010*.

[3]Anecdotal evidence suggests that before the reform, the law was "loosely" enforced, and women were aborting under the "psychological risk for the mother" category.

The main challenge to estimate the effects of a change in the availability or the costs of these technologies is that such a change could be driven by a different underlying demand for boys. A cross-sectional study on the effects of the change in technology or costs would yield an overestimate the effects of these changes since the regions where the changes have happened would also be the regions with higher demand for sons.

To identify the causal effect of the abortion liberalization, we use three sources of variation. First, we exploit the exogenous variation in the access to sex-selective abortion caused by the abortion liberalization reform. The reform had nothing to do with Chinese immigrants' preferences for sex-selection. Second, we exploit the variation in son-preferences associated with a nationality that has been widely documented in the literature. Son-preferences, as discussed before, exist among Chinese and Chinese immigrants, but they have not been found among Spaniards. Third, we exploit the variation in the demand for boys associated with higher birth orders within Chinese that the literature has consistently documented. Finally, we add another source of variation and construct a measure of access to abortion based on the number of abortion centers per inhabitant in each province. We explore, at the correlation level, whether our results are driven by those provinces with higher access to abortion.

First, we use a difference-in-difference with children from Chinese mothers being the treated group and children from Spanish mothers being the control group. We run our analysis separately for children who are the first child, children who are the second child, and children who are the third child or above. As expected from the son-preference pattern found in the literature, we do not find any effects from the abortion liberalization on the fraction of boys born from Chinese mothers giving birth to their first or their second child. However, we find a significant, but noisy, increase in the fraction of boys from those Chinese mothers giving birth to their third child or above caused by the reduction in the costs of abortion.

Second, in order to target those with stronger preferences, we use the same difference-in-difference as before, but comparing those children with both parents being Chinese with those with both parents being Spanish. By excluding those few children with a Chinese mother, but non-Chinese father, we focus on those parents who are more likely to have stronger son-preferences. The effects on the fraction of boys for those mothers giving birth to their third child or above are now more stable than before across the different specifications.

Third, we add a third difference consisting on the access to abortion measure. We show that the increase in the fraction of boys comes mainly from those provinces with higher access to abortion, which reinforces our sex-selection via abortion channel story.

Finally, our previous results suggest that the number of "unwanted" Chinese girls from those mothers giving birth to their third child or above decrease given that the fraction of boys at birth increase. We thus analyze if Chinese girls' birth outcomes improve for the high order births following the abortion liberalization reform.[4] Although the results are noisy, we find some suggestive evidence of an improvements in birth outcomes for those Chinese girls who are the third child or above. The fraction being premature significantly decreases, and gestational weeks significantly increases, suggesting some improvements on Chinese girls' birth outcomes. However, results are too noisy to get a clear picture of the effect of the reform on Chinese girls' birth outcomes.

A limited literature has studied the effects of both the availability of ultrasound technology and the abortion costs on sex-selection. On the availability of ultrasound technology, there have been very few studies, Bhalotra and Cochrane (2010) and Chen et al. (2013), for example, study the effects of ultrasound access on the fraction of boys born in India and China. They find that access to a technology that reveals the gender of the fetus increases the fraction of boys at birth. On the effects or decreasing abortion costs on sex-selection a similar paper to ours is Lin et al. (2008). They exploit the legalization of

[4]Notice that Chinese boys' birth outcomes could also improve since now those women who prefer boys know that if they have a child, the child will be a boy since otherwise, they will abort. This might make them take extra care during the entire pregnancy even before knowing the gender of the fetus.

abortion in Taiwan in the 1980s and document an increase in the fraction of boys at birth following the reform. Moreover, they document a decrease in female mortality. Other papers that have analyzed the effects of access to ultrasound technology on girls' outcomes have documented an improvement in those.[5] This paper contributes to this literature by analyzing the effects a plausibly exogenous change in the costs of abortions, given that Chinese immigrants are a minority in Spain and thus did not have any influence on the abortion reform, among Chinese immigrants to the West.

The remaining of the paper is organized as follows. First, section 2.2 discusses the related literature. Section 2.3 discusses in detail the abortion policy change. Section 2.4 describes the data. Section 2.5 discusses our identification strategy. Section 2.6 presents our results. Finally, section 2.7 concludes with a discussion.

2.2 Literature Review

This paper builds on the widely documented evidence on son preferences in China, and its persistence among Chinese immigrants to the West. An empirical regularity of all these studies is that the boy-birth percentage is normal for the first child, but it increases significantly at higher birth order children, especially for the third child and above. Abrevaya (2009) shows an unusual high boy-birth percentage, mainly among third and fourth children born in the US from Chinese and Asian Indian mothers. Almond and Edlund (2008) document male-biased sex ratios among children born in the US with Chinese, Korean, and Indian parents. The fraction of boys increase at higher birth orders if the parents had not had a previous son. Almond et al. (2013) document a similar pattern among Asian immigrants in Canada.

In the case of Spain, the existence of sex-selection has been shown by González (2018). She shows an extremely son-biased sex ratio at birth among Indian immigrants, especially at higher birth orders. Moreover, she documents that children of Indian parents have poor

[5] See, for example, Hu and Schlosser (2015) or Anukriti et al. (2018).

health outcomes during infancy, but she finds no gender gap in infant health.

Beyond the existence of son preferences, a necessary condition to have sex-selective abortions is that the gender of the fetus is known at the time of the abortion. In this paper, we exploit an abortion law reform that liberalized abortion during the first 14 weeks of pregnancy. Therefore, for this reform to have an effect on sex-selection, we need that the gender of the fetus is known by gestational week 14. In Spain, ultrasound is the primary method to determine the sex of the fetus. Many studies have shown that accuracy rates of ultrasound in determining the fetus gender after week 12 are quite high. Whitlow et al. (1999) document that the test is 90% accurate in determining the sex of the baby at 14 weeks, 79% at 13 weeks, 75% at 12 weeks, and 46% accurate at 11 weeks. Efrat et al. (1999) finds that the test is correct by more than 98% by 12 weeks.

A paper that has exploited the variation in access to the technology that reveals the gender of the fetus is Bhalotra and Cochrane (2010). They exploit the differential introduction of diagnostic ultrasound in India between 1972 and 2005, together with variation across families in the incentive to conduct sex selection depending on the sex of previous births and birth order. They find that that improved local access to ultrasound technology caused a substantial increase in the fraction of boys at birth in higher-order births for those women who had not had the desired number of boys. Another paper that exploits the differential introduction of diagnostic ultrasound is Chen et al. (2013). They study it in China during the 1980s and, similarly to Bhalotra and Cochrane (2010), document an increase in the fraction of boys associated with the introduction of diagnostic ultrasound.

A similar paper to ours that has studied the impact of the access to abortion on sex ratios at birth is Lin et al. (2008). They exploit the legalization of abortion that took place in Taiwan in 1985/86 and the variation in demand for boys associated with higher birth orders. They find that the legalization of abortion significantly increased the fraction of males born. The effect came entirely from third and higher-parity births. They also show that another effect of the legalization of abortion was a 20% decrease in female mortality and an increase in fertility for older mothers.

The literature has also documented that parental preferences for boys affect in gender differences in parents' investments and medical care allocation. This ends up causing gender differentials in children's nutrition, morbidity, and mortality outcomes among others (see, for example, Rosenzweig and Schultz (1982), Barcellos et al. (2014), or Rose (1999)).

Some studies have documented that access to ultrasound technology improves the surviving girls' outcomes. Access to ultrasound technology, together with the possibility of aborting, allow parents to sex-select by terminating an unwanted pregnancy. Girls who are born when these technologies are available are thus more likely to be wanted. Hu and Schlosser (2015) documents that, in India, an increase in prenatal sex-selection, measured by high sex-ratios at birth, lead to a reduction in girls' malnutrition among the surviving girls. Anukriti et al. (2018) show that the increase in ultrasound availability lead to a reduction in under-5 mortality of girls after birth in India. In this paper, we study the effects of changing the abortion costs, when ultrasound technology is available, on Chinese girls birth outcomes.

2.3 The Abortion Policy Change

This paper exploits the 2010 abortion policy reform that took place in Spain. Before describing this reform, we provide some context on the previous abortion law that was in place before this reform.

Before the 2010 law change, abortion in Spain was restricted by law.[6] In case of carrying an abortion without following the procedures established by the law, the mother and the doctor faced potential prison penalties. Abortion was regulated by *Law 9/1985*, which had been approved on July 5th, 1985. Under this law, abortion was only allowed under three circumstances: (1) physical or psychological risk for the pregnant woman, (2) rape, and (3) physical or psychological malformation of the fetus. Under case (1) abortion was

[6] *Law 9/1985*

allowed at any moment of the pregnancy period, under case (2) abortion was allowed during the first 12 weeks of pregnancy, and under case (3) abortion was allowed during the first 22 weeks of pregnancy. A medical report certifying that the conditions were satisfied, and a police report in the case of rape, were required in order for the woman to abort.

On March 3rd, 2010, a new abortion law was published (*Law 2/2010*). The law was implemented on July 5th, 2010. The new abortion law, implemented by the socialist party, represented the liberalization of abortion in Spain. It allowed women to freely decide to abort during the first 14 weeks of pregnancy without the need to provide any justification. The only requirement was that women who are considering to abort need to be informed about their rights and the public subsidies that exist to help mothers if they decide to have the child. Starting three days after being informed, they can abort. In case that the process carried some risk for the mother or the fetus, the law allowed the woman to abort during the first 22 weeks. After week 22 abortion was only allowed if a severe and incurable illness or an illness incompatible with life were detected to the fetus. In this case, abortion was allowed at any moment of the pregnancy. For these last two possibilities of aborting beyond week 14, a medical report was required.

Anecdotal evidence suggests that the law before the 2010 reform was not strictly enforced, and that women were aborting by claiming psychological risk for the mother, which is case (1) listed above. In 2007, for example, there were 11.49 abortions/1,000 women in Spain. In 2009, the ratio was roughly the same (11.41). The ratio slightly increased in 2010 to 11.71, and reached its maximum in 2011 with 12.47 abortions per 1,000 women. In 2016 the ratio had decreased to 10.36.[7] That is, immediately after the reform, the number of abortions per 1,000 women increased a bit, but considerably less than if the law had been strictly enforced. However, the effects of the reform could potentially be different for different groups of the population.[8]

[7]See the report *Interrupción Voluntaria del Embarazo Datos definitivos correspondientes al año 2016* report from the Ministry of Health in Spain.

[8]Unfortunately, our individualized data on abortions only starts in 2011.

2.4 Data

For our analysis, we use two sources of data: birth data and data on the number of abortion centers in Spain.

2.4.1 Birth Data

We use birth data that contains data on the child gender, the parents' nationality, and birth outcomes come from the *National Statistical Institute*. This is a population-level data set, which contains information on the universe of births that take place in Spain. Importantly, the information appears as it appears in the official national registry. All families have to register the newborn babies within eight days of birth.

The data includes the year and month of birth. The time and date of birth are set in the documentation provided by the health center that assisted with the delivery. Other information available coming from standardized forms filed at the registry includes the gender of the child, whether it was a single or multiple birth, the birth order (first child, second child, third child or above). Unfortunately, the data does not include the previous children's gender. However, it includes the country of nationality of the mother and the father, the country of birth of the mother and the father and the previous child country of birth. This allows us to see the effect for children whose mother and father are Chinese (we would expect a stronger effect for them than children with only one of the parents being Chinese) or for children whose parents are Chinese and were born in China versus children whose parents were not born in China.

The data also includes some birth outcomes like the birth weight (in grams), the weeks of gestation at birth, whether the newborn lived more than 24 hours or not, or whether the child was born alive or not. Moreover, it includes some information about the parents' characteristics like their age, whether they are married or not, their immigration status, or the job category of the parents among others. Finally, it includes geographical information like the province and county of registration of the child.

Starting in 2007, there is information on parents' education level and whether the child was born by cesarean section or not. Unfortunately, this information is not available before 2007. There is no information on Apgar scores for any of the years.

Figures 2.7.1 and 2.7.2 address the concern on whether there are enough births in Spain from Chinese mothers to study the effects of the abortion liberalization on sex-selection among them. Figure 2.7.1 provides the total number of births that took place in Spain between 2000 and 2016 and separates them by their birth order. We can see how the number of births from Chinese mothers start from a considerably low number in 2000 and have an increasing trend until they stabilize around 2008. Then they decrease slightly around 2013. Figure 2.7.2 zooms in for those births from Chinese mothers which are third birth or above. We observe a similar trend: births start from just being around 100 per year and increase until they reach around 600 per year. Because of this evolution of the number of births and the availability of the education covariates, this paper will use the births that took place in Spain between 2007 and 2016. Figures B.1.1 and B.1.2 in the appendix plot the ratio of Chinese births in Spain relative to all the births that took place in Spain during this period.

For our analysis, we exclude those mothers giving birth to twins since it is unclear how to interpret birth order in these cases. Table 2.7.1 provides some summary statistics dividing the resulting sample into those children whose mother and father have Spanish nationality ("Spanish") and those children whose mother and father have Chinese nationality ("Chinese"). For the 2007-2016 period, our period of interest, there were a total of 3,361,963 births (excluding twins) in Spain from children with both parents having Spanish or Chinese nationality. Out of this, there were 36,342 births from Chinese, which represents 1.08% of the births. The fraction of Chinese and Spanish parents who are married is more or less the same (around 64%), but both Chinese fathers and mothers tend to be younger than the Spanish ones. Chinese fathers and mothers are considerably less educated than Spanish fathers and mothers. 83% of Chinese mothers and fathers report having less than High school while only 31% of the Spanish mothers and 40% of the Spanish fathers report that. Finally, Chinese mothers have more children than the Spanish ones. The number of previous children born alive on average is 0.68 for Chinese mothers while it is only 0.57 for the Spanish mothers. Out of the total number of Children born from Chinese parents, 13% represents the third child or above of the mother while this is only the case for 8% of the Spanish mothers.

2.4.2 Abortion Clinics Data

We use data from the Spanish Ministry of Health. Doctors performing an abortion in Spain have to report the abortion to the regional authorities by filling a standardized form with information about the procedure and the woman. The Ministry of Health has information on every abortion performed in Spain and every clinic or hospital that performed at least one abortion during a particular year.

Unfortunately, we only have access to this individual-level-micro data from 2011 onward. However, each year the Ministry of Health publishes a report with some summary statistics of the abortions performed in Spain during that particular year. Importantly for this project, these reports also include a list of all the centers that performed at least one abortion in that year. These reports, which are available from before 2011, allow us to know the number of clinics that performed abortions in each province each year from 2007 to 2016, the relevant period for this study. We can then combine this with data on the number of inhabitants in each province-year to construct the ratio *clinics*/*population* as a measure of access to abortion.

Figures 2.7.3 - 2.7.6 plot the map of Spain colored based on the average number of abortion centers per 100,000 inhabitants within each province for the whole period 2007-2016, and the value of this ratio for 2007, 2010, and 2016. Figure 2.7.3 shows that the number of centers per 100,000 inhabitants on average goes from 0 in some provinces to 0.88

centers per 100,000 inhabitants in the provinces with the highest ratios. The higher ratios are concentrated in the regions of Catalonia, Valencia, Balearic Islands, Basque Country, Asturias, and a province in Galicia (Oursense). Figures 2.7.4 - 2.7.6 show the evolution of our measure of access to abortion by plotting the map in 2007, 2010, and 2016. We can see how, for example, the access of abortions measures for those in the highest bin decrease slightly in 2010 relative to 2007 but increases again in 2016 ending with a higher ratio than in 2007.

Table 2.7.2 takes advantage that we have individual abortions data in the post-reform period. It provides, at the descriptive level, the ratio of the number of abortions over the number of abortions plus the number of births in 2011-2016 by child order and nationality (Chinese women versus Spanish women). For those women who have zero living children, and thus if they end up giving birth, the child born would be their first child, abortions represented 15% of the abortions plus births in this period. This percentage is the same for both Chinese and Spanish women. For those Spanish women with a living child, the rate decreases slightly to 11%, while for Chinese women, it increases up to 20%. For those women with two or more living children (thus, if the pregnancy ends up in birth the child born would be the third or above), the percentage increases for Spanish women up to 35%, and it increases considerably for Chinese women up to 65%. That is, in the post-reform period (2011-2016), abortions from Chinese women with two or more living children represented 65% of the Chinese women abortions plus pregnancies that took place in that period.

Notice that if Chinese women follow a simple rule of aborting if they expect a girl as a third child then 50% of them will abort the first pregnancy, and then 50% of those will abort the second attempt and so on. That is, some of these abortions are likely to come from the same women trying multiple times to have a boy. Section B.2 in the appendix provides some suggestive evidence on the post-reform behavior among Chinese women. In particular, it plots the cumulative distribution functions (CDF) of the number of previous abortions, and of the gestational weeks at abortion by nationality and child-order. Figure B.2.1 shows that conditional on the number of living children, Chinese women have performed more previous abortions than Spanish women. Moreover, Chinese women with two living children have performed more previous abortions than Chinese women with zero living children.

Figure B.2.2 plots the CDF of the gestational weeks at abortion for Chinese women with zero living children versus Chinese women with two living children, and Spanish women with zero living children versus Spanish women with two living children. This figure divides the sample into two periods (2011-2013 and 2014-2016). Figure B.2.3 complements this by plotting the CDF(Gestational Weeks) in 2011-2013 versus 2014-2016 for Chinese women with two living children. Together, these figures explore the possibility that Chinese women with two living children abort earlier in the most recent period (2014-2016) due to the possible availability of non-invasive prenatal testing.

All these patterns are consistent with the interpretation that Chinese women use abortions to sex-select.

2.5 Identification Strategy

2.5.1 Difference-in-Difference

The main concern to study the effect of reducing abortion costs on sex-selection is that the reduction of the abortion cost might be endogenous. The region's strong preferences might cause the reduction of the cost of abortion (or liberalization of abortion) to sex-select. If that is the case, a cross-sectional comparison would overestimate the real effect of reducing abortion costs on the fraction of boys born.

The empirical strategy used in this paper uses three sources of variation to overcome the endogeneity problems and estimate the causal effect of reducing abortion costs on sex selection. First, it exploits the variation caused by the liberalization of abortion that took place in Spain in 2010. Given that births for Chinese immigrants account for 1% of total births, the abortion reforms in Spain are highly unlikely driven by their preferences. Second, it exploits the variation in son's preferences associated with nationality. Son preferences exist in China and persist among Chinese immigrants as discussed before. These preferences for sons have not been documented among Spanish parents. Third, we also exploit the variation in birth order within nationality. Research has shown, as discussed before, that, within Chinese, no sex selection exists for the first child, but sex selection becomes pronounced at higher birth order. Parents are more likely to sex-select in a given birth if their desire to have another child in order to have a boy is lower. That is likely to happen in higher birth orders either because of the total number of children's preferences, financial constraints or biological constraints caused by the mother's age.

The identification strategy used to exploit the variations available is a difference-in-difference (DD) comparison with children with Chinese mothers being the treated group and children with Spanish mothers being the control group. Mothers giving birth to their first child, second child, and third child or above are analyzed separately in order to exploit the different sex-selection preferences within Chinese mothers suggested by the literature.[9] Based on all this discussion, we would expect that if there is any effect, it should appear in the mothers giving birth to their third child or above. No-effect or a smaller effect would be expected for mothers giving birth to their first or second child.

Given that the liberalization of abortion took place in July 2010, we would expect the effects (if any) to start appearing around December 2010. To be conservative, we define the treated period as starting in January 2011. This should bias the results downwards and give a conservative estimate. The period of the analysis is 2007-2016 for the reasons discussed in section 2.4.

Therefore, our difference-in-difference will estimate the following regression for mothers giving birth to their first, second, and third or above child:

[9] Third child and higher birth parities are merged in the *third or above* category for power reasons.

$$y_{imt} = \alpha + \gamma POST_t + \lambda motherChinese_i + \delta(POST_t * motherChinese_i) + \mu_t + \eta_m + \epsilon_{imt} \tag{2.1}$$

where

$$POST_t = \begin{cases} 1 & \text{if } 2011 \leq \text{birth year} \leq 2016 \\ 0 & \text{if } 2007 \leq \text{birth year} \leq 2010 \end{cases}$$

$$motherChinese_i = \begin{cases} 1 & \text{if mother has Chinese nationality} \\ 0 & \text{if mother has Spanish nationality} \end{cases}$$

and y_{imt} is the gender of child i born in month m in year t. μ_t are birth-year fixed effects (included in all specifications), η_m are birth-month fixed effects (included in all specifications). Some of the specifications will include other controls (mother and father's education, mother and father's age, and the civil status of the parents). Finally, only single births are considered (i.e., twins are excluded) since the main outcome of interest is the gender of the child. The main coefficient of interest is δ for those children whose mothers are giving birth to their third child or above. The δ for those children whose mothers are giving birth to their first or second child can be used as a placebo test. If our hypothesis is correct, we would expect no effect on them.

2.5.2 Difference-in-Difference-in-Difference with Abortion Clinics-population ratio

Ideally, we would like to show that the number of abortions increased for those Chinese mothers giving birth to their third child or above and at a gestational week where the gender is known. This would allow us to make a stronger argument that the increase in sex-selection (if it exists) comes via the abortion channel. Unfortunately, we do not have access to this precise data before 2011, and therefore, we would not have any year before the abortion liberalization reform to estimate our difference-in-difference.

However, we have data on the number of clinics performing abortions each year in each province. We can then construct a *clinics/population* ratio in each province as a measure of access to abortion. We can then see whether the increase in the fraction of boys happens in provinces with a higher *clinics/population* ratio. This, as a correlation, would reinforce the evidence of our argument that the increase sex-selection after the abortion liberalization happens via abortions channel. Note that we are not claiming a causal effect of the number of abortion clinics on sex-selection. A causality argument would have at least three potential problems. First, the number of clinics in each province are likely driven by different abortion demand. Second, Chinese immigrants could be choosing where to live based on the number of abortion clinics available, which would lead to a selection problem. Third, it could just be heterogeneous responses. Easier access to abortion might be correlated with characteristics that affect what kind of immigrants a province gets and this can be correlated with gender preferences and thus, we would have, again, a selection problem. Understanding the causal effects of the number of abortion clinics on sex-selection goes beyond the scope of this paper. The first problem is less of a concern because Chinese are just a minority and are not likely to affect the number of clinics in each province. However, the last two problems pose a serious challenge for selection bias.

To estimate these effects we are going to add another interaction with the ratio *clinics/population* and so we are going to estimate the following regression:

$$\begin{aligned} y_{imt} = \quad & \alpha + \gamma POST_t + \lambda Chinese_i + \delta(POST_t * Chinese_i) + \beta_1 ratio_{it} + \beta_2(POST_t * ratio_{it}) \\ & + \beta_3(Chinese_i * ratio_{it}) + \beta_4(POST_t * Chinese_i * ratio_{it}) + \mu_t + \eta_m + \epsilon_{imt} \quad (2.2) \end{aligned}$$

$Chinese_i$ is a dummy equal to 1 if both the father and the mother of the child have Chinese nationality, and equal to 0 if they both have Spanish nationality.[10] We estimate

[10] We estimate this regression defining Chinese as both the father and the mother having Chinese nationality to gain precision.

this regression separately for mothers giving birth to their first, second, and third child or above. Again, we would expect that if there are any effects, they should come from those Chinese women giving birth to their third child or above in those provinces with more access to abortion. Our coefficient of interest is the coefficient of the interaction of the *clinics/inhabitants* ratio with the $POST_t * Chinese_i$ variable (β_4 for those women giving birth to their third child or above.

2.5.3 Chinese Girls Birth Outcomes Difference-in-Difference-in-Difference

To estimate the effects of the abortion liberalization on Chinese girls' birth outcomes, we add the gender of the child interaction to regression (2.1).

$$\begin{aligned} y_{imt} = \quad & \alpha + \gamma POST_t + \lambda Chinese_i + \delta(POST_t * Chinese_i) + \beta_1 girl_i + \beta_2(POST_t * girl_i) \\ & + \beta_3(Chinese_i * girl_i) + \beta_4(POST_t * Chinese_i * girl_i) + \mu_t + \eta_m + \epsilon_{imt} \quad (2.3) \end{aligned}$$

$girl_i$ is an indicator equal to 1 if the child is a girl and equal to 0 is he is a boy. To gain precision, we define $Chinese_i$ as both the father and the mother having Chinese nationality (vs. both the father and the mother having Spanish nationality). Our coefficient of interest that captures the effects of the reform on Chinese girls' birth outcomes for different outcomes of interest (fraction being premature, or low birth weight, among others) is β_4. Our main identification assumption is that, beyond the abortion reform, nothing else changed differently for the Chinese gender gap in birth outcomes relative to the Spanish one for those being the third child and above.

2.6 Results

2.6.1 Effect on the fraction of boys born

This section looks at the effect of the reform on Chinese relative to Spanish parents. Tables B.3.1 and B.3.2 in the appendix look more closely on the mechanisms and show the reform effects on the fertility of Chinese mothers. Results show that the number of births from Chinese parents giving birth to their third child or above increased relative to the number of births representing the first and the second child. Similarly, the number of births from Chinese mothers who are 35 years old or older increased relative to those who are younger. For this group, giving birth is more costly, and after the reform, they had the certainty that if they gave birth, it would be a boy. These effects on fertility are consistent with our sex-selection results.

2.6.1.1 Children with Chinese Mother versus Spanish Mother

Figures 2.7.7 - 2.7.9 plot the fraction of boys for children born from Chinese mothers relative to children born from Spanish mothers between 2007 and 2016, excluding those mothers who give birth to twins. Figure 2.7.7 plots it for mothers giving birth to their first child, figure 2.7.8 plots it for mothers giving birth to their second child, and figure 2.7.9 plots it for mothers giving birth to their third child or above. In the three figures, we can see how, because of the different number of births between Spanish mothers and Chinese mothers, the fraction of boys from Chinese mothers are much noisier than the fraction of boys from Spanish mothers.

Despite this noisiness, we can see how, as expected, the abortion liberalization had no effect on the fraction of boys born from Chinese mothers giving birth to their first child relative to those born from Spanish mothers. The policy change had no clear effect either on the fraction of boys born from Chinese mothers giving birth to their second child even though it becomes noisier than when we analyze the first child. Finally, we can see (figure 2.7.9) a clear jump after the abortion liberalization on the fraction of boys born from Chinese mothers giving birth to their third child or above. The fraction of boys increases significantly for this group relative to the Spanish control group except for 2014, where the fraction of boys from Chinese mothers drops significantly to then return to the previous levels. Unfortunately, we do not have any explanation for this sudden decrease in this particular year.

This increase in the fraction of boys from mothers giving birth to their third child or above and no effect for lower-order children is what we would have expected based on our discussion of the sex-selection literature in section 2.2. Therefore, our results show that when abortion costs decrease (in this case via a liberalization of abortions) sex-selection among Chinese immigrants in Spain giving birth to their third child or above (i.e., those whom the existing literature have shown to have a preference for sex-selection) increases. Finally, figure 2.7.10 plots in the same figure the fraction of boys for Chinese mothers giving birth to their first, second, and third child or above.

Tables 2.7.3 - 2.7.5 put numbers to the previous figures by providing the results of estimating regression 2.1. All three tables have the same structure: column (1) provides the basic results when only birth year and birth month fix effects are included, in column (2) some controls including father and mother age, mother civil status, and father and mother education level dummies are added. Finally, column (3) includes province fix effects. This allows us to observe whether our results are very sensitive to the inclusion of covariates, which would generate some doubts into the validity of the results. Standard errors are clustered at the province level in all the regressions.

Table 2.7.3 provides the results for mothers giving birth to their first child, and table 2.7.4 provides the results for mothers giving birth to their second child. As shown before in the figures, for these groups, there is no sex-selection either before nor after the abortion liberalization reform. The fraction of boys from Chinese mothers is not statistically significantly different from the fraction of boys from Spanish mothers. Table 2.7.5 estimates our analysis for mothers giving birth to their third child or above. Confirming the visual analysis of the previous figure, there was no sex-selection from Chinese mothers before the abortion liberalization reform (the coefficient on the dummy indicating whether the mother is Chinese or Spanish is not significant), but the policy reform caused a statistically significant increase of around three absolute percentage points on the fraction of boys from Chinese mothers relative to Spanish mothers. This coefficient is roughly stable to the inclusion of controls or province fixed effects.

2.6.1.2 Children with Chinese Father (and Mother)

In order to gain more power given the limited number of mothers with Chinese nationality, we now try to restrict the sample to those children with both father and mother being Chinese (relative to those with father and mother being Spanish). By doing this, we exclude those children whose mother is Chinese, but whose father is not Chinese (this represents around 4%, 1,677, of the births from Chinese mothers). We would expect to find higher effects of reducing the abortion costs for this group since we would expect stronger sex-selection preferences if both the father and the mother are Chinese.

Figures 2.7.11 - 2.7.13 plot the fraction of boys for children born from Chinese mothers and fathers relative to children born from Spanish mothers and fathers between 2007 and 2016 excluding those mothers who give birth to twins. These figures provide a very similar story to the one provided before when we analyzed the fraction of boys for those Children with a Chinese mother and did not exclude those whose father was not Chinese. We observe a huge jump on the fraction of boys for those Chinese parents where the mother is giving birth to her third child or above, but we continue to observe a drop in 2014.

Tables 2.7.6 - 2.7.8 provide the results of the regression analysis. Tables 2.7.6 and 2.7.7 provide similar results than before: there is no sex-selection among Chinese immigrants giving birth to their first or second child, and the decrease in abortion costs had no effect on them. Table 2.7.8 provides the regression results for those mothers giving birth to their third child or above. The results are similar to before but more stable. We obtain a significant positive effect of around 3.3 absolute percentage points. The effects are significant at the 5% level in all the specifications while before when we did not include controls, they were only significant at the 10% level. Table B.4.1 in the appendix shows similar results when the period considered is from 2000 to 2016.[11]

Finally, tables B.5.1 and B.5.2 in the appendix provide the results of estimating a difference-in-difference analysis considering only births from Chinese parents (both father and mother) and excluding those from Spanish parents. The treated group is those being the third child or above, and the control group is those being the first and the second child in table B.5.1, and only the first child in table B.5.2. Both tables provide similar results to the previous analysis showing that the fraction of boys from Chinese parents for the third child or above increases relative to the first and second children. Because of the smaller sample size (children from Spanish parents are excluded), results only become significant when we gain precision by adding controls.

2.6.1.3 *Adding The Abortion Centers Difference*

We now turn to add another difference, the access to abortion measure, in order to make the abortion channel argument stronger. We implement the identification strategy discussed in section 2.5.2. As discussed, we measure abortion access at the province level with the ratio of the number of abortion centers that performed at least one abortion in a given year in that province over the province population that year (*clinics*/*population*). We use two different *clinics*/*inhabitants* ratios: the current one, and assign each province its average during the 2007-2016 period.

First, we use the current value of the *clinics*/*population* ratio in each year for each province. Table 2.7.9 provides the results of this analysis. As before, we analyze women giving birth

[11] No controls are added in this table since education controls are not available before 2007. However, the simple specification with birth year, birth month and province fixed effects lead to similar results as when the 2007-2016 period is considered.

to their first, second, and third child or above separately. We continue defining *Chinese* as children with both father and mother having Chinese nationality. The results show that the effects of the abortion liberalization reform on sex-selection for Chinese women giving birth to their third child or above come from those provinces with higher access to abortion. The coefficient on the interaction between *POSTxChinese* with *clinics/population* is positive and statistically significant. Moreover, it is stable to the inclusion of control variables. This reinforces our story that the effects we are finding on sex-selection come via the abortion channel.

Second, we assign to each province its average *clinics/population* value during the 2007-2016 period. Table 2.7.10 provides the results of using this access to abortion measure. The increase in the sex-selection effects are driven by those provinces with higher access to abortion since the coefficient on the interaction between *POSTxChinese* and the average of *clinics/population* within each province is positive and statistically significant for those women giving birth to their third child or above.

2.6.2 Effects on Chinese Girls Birth Outcomes

The abortion liberalization, as discussed before, reduces the abortion costs for those parents who do not want to have a girl. So far, we have shown that less "unwanted" girls are born from those Chinese parents giving birth to their third child or above. A plausible hypothesis is that the birth outcomes from those Chinese girls being the third child or above improve provided that now they are more likely to be "wanted".[12] This section presents the results of exploring this possibility.

Tables 2.7.11 - 2.7.14 estimate equation 2.3 separately for those women giving birth to their first, second, and third child or above. We focus on the effects of the abortion liberalization on Chinese girls' birth outcomes. In particular, we focus on birth weight,

[12]Notice that Chinese boys birth outcomes could also improve since now parents who only want boys know that if they have a child, the child will be a boy with probability one since if she is a girl, they will abort.

gestational weeks, the fraction of low birth weight (less than 2500 grams at birth), extreme low birth weight (less than 1500 grams at birth), premature (born before 37 weeks of pregnancy), and whether the child lives more than 24 hours or not.

The tables show that children from Chinese parents have better birth outcomes than children from Spanish parents. They are significantly less likely to be low birth weight, extreme low birth weight (except those who are the second child), and premature than children from Spanish parents regardless of whether they are the first, the second, or the third child or above. Moreover, they have a significantly higher birth weight. However, we only find significant positive effects on living more than 24 hours and on gestational weeks for those Chinese children who are the third child or above.

We explore the existence of a Chinese gender gap relative to a Spanish gender gap on birth outcomes. For those being the first child, we only find a 10% statistically significant difference in the fraction being premature. In contrast, for those giving birth to their second child, we only see it on the extreme low birth weight fraction. Therefore, for those giving birth to their first or second child, we do not find any clear pattern suggesting the existence of a Chinese gender gap on birth outcomes. For those giving birth to their third child or above, we find the presence of the Chinese gender gap on the fraction living more than 24 hours. This gap is statistically significant at the 5% level.

We do not find any conclusive effects of the abortion liberalization policy change on Chinese girls' birth outcomes. Based on the sex-selection results from the previous sections, we would expect to find stronger effects, if any, on those Chinese girls being the third child or above. For those Chinese girls being the third child or above, we find some suggestive evidence of an improvement in birth outcomes following the reform. The policy change significantly increased the gestational weeks (Table 2.7.14), and reduced the fraction of Chinese girls being premature (only significant at the 10% level. Table 2.7.13), but had no significant effect on the birth weight, or the low birth weight fraction, or the fraction living more than 24 hours. For those children who are the first or the second child, we only find a significant effect on the fraction of Chinese girls living more than 24 hours and on the gestational weeks of those mothers giving birth to their first child. Contrarily to what we expected, both of these effects are negative.

Finally, tables B.5.3 and B.5.4 in the appendix provide the results of estimating the analysis considering only births from Chinese parents (both father and mother) and excluding those from Spanish parents. We add the child order difference comparing those Chinese girls who are the third child or above with those who are the first or the second in table B.5.3, and only with those who are the first child in table B.5.4. Similar to the previous results, we find positive and significant effects on the gestational weeks for those Chinese girls who are the third child or above relative to those who are the first or the second child. We do not find significant effects on the other birth outcomes analyzed.

Overall, results are too noisy to conclude that the policy change improved the birth outcomes of those Chinese girls being the third child or above. Unfortunately, we do not have enough power to reach a definite conclusion on this important channel.

2.7 Conclusion

This paper contributes to the literature on the effects of decreasing abortion costs on Chinese parents' sex-selection and surviving girls' birth outcomes. By exploiting an abortion liberalization in Spain, different son-preferences by nationality (Spanish versus Chinese) and birth order we have documented that the liberalization caused an increase in the fraction of boys born of Chinese parents giving birth to their third child or above. Results are stable to the inclusion of controls. Consistent with the literature on son-preferences, we find no effects for those women giving birth to their first or their second child. Moreover, we document, at the correlation level, that the effects come mainly from those provinces with higher access to abortion. This is measured by the ratio of the number of abortion centers that performed at least an abortion in a given year in a given province relative to the population in that province in that year. These findings are consistent with our interpretation.

After documenting the increase in the fraction of boys born following the reform, we turn to explore any potential effects on the birth outcomes of the girls born from Chinese parents giving birth to their third child. Those girls are now more likely to be "wanted" than before. Unfortunately, our results are too noisy to get a definite conclusion. Still, we find some suggestive evidence of an improvement via an increase in gestational weeks, and a decrease in the fraction being premature for those Chinese girls who are the third child or above. However, we do not have enough power to see if the policy had any effects on the fraction living more than 24 hours.

This paper has thus provided evidence that Chinese immigrants' sex-selection responds to the abortion regulations of the country they live in. Understanding this and understanding how this sex-selection happened (if it happened) before reducing the costs of abortion is important to understand all the effects of an abortion reform that reduces the costs of aborting.

Figures and Tables

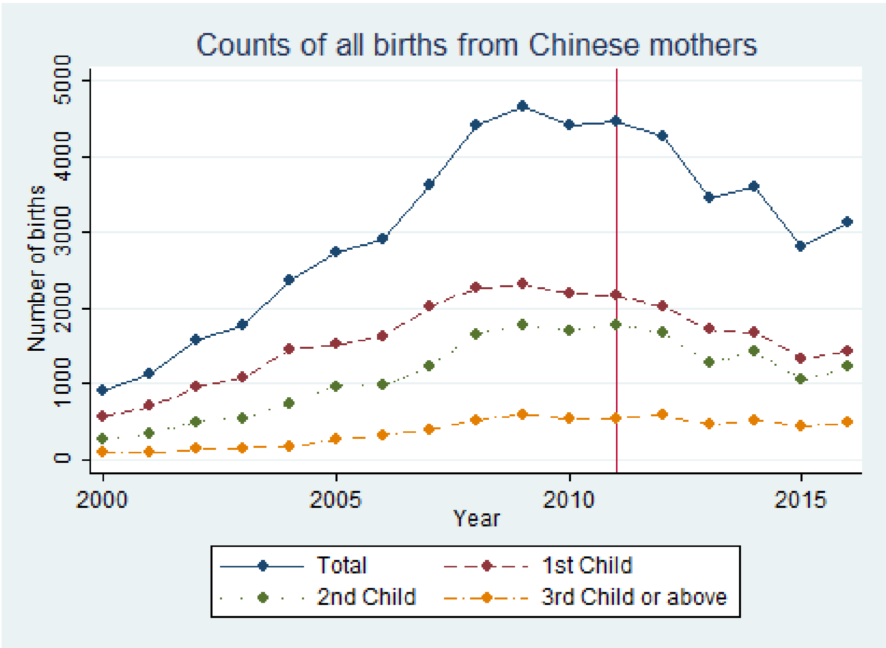


Figure 2.7.1: Counts of all births from Chinese mothers by birth order

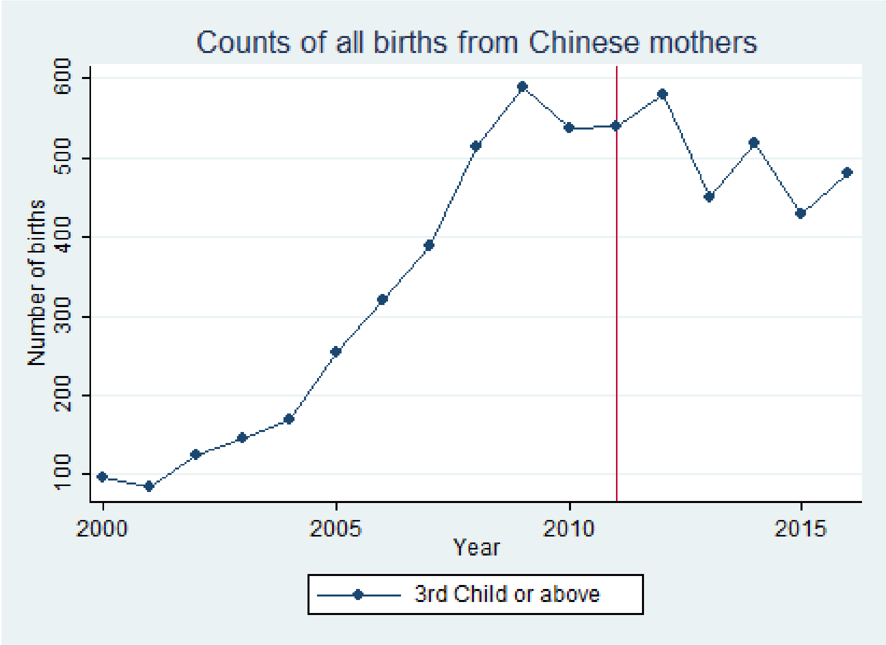


Figure 2.7.2: Counts of all births from Chinese mothers giving birth to their third child or above

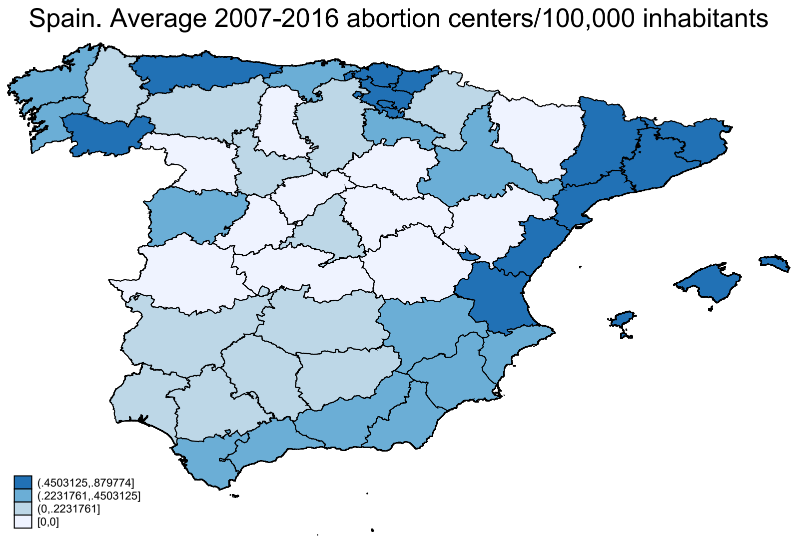


Figure 2.7.3: Average number of abortion centers per 100,000 inhabitants between 2007-2016

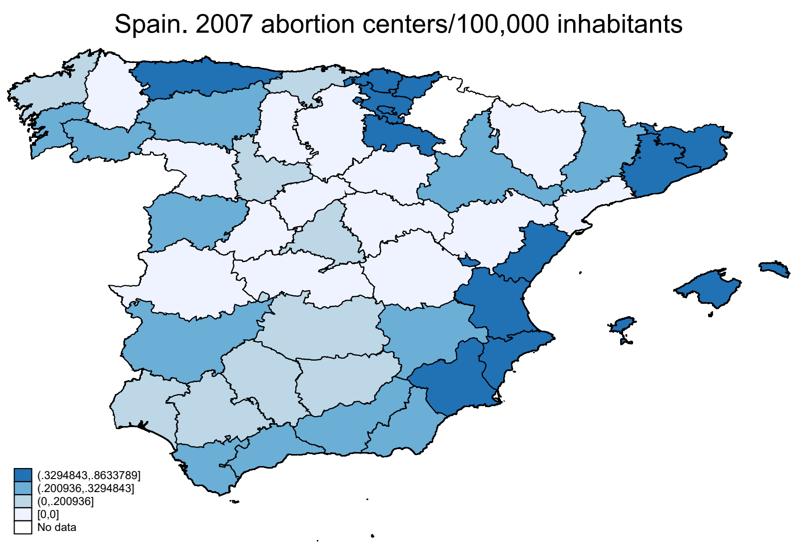


Figure 2.7.4: Number of abortion centers per 100,000 inhabitants in 2007

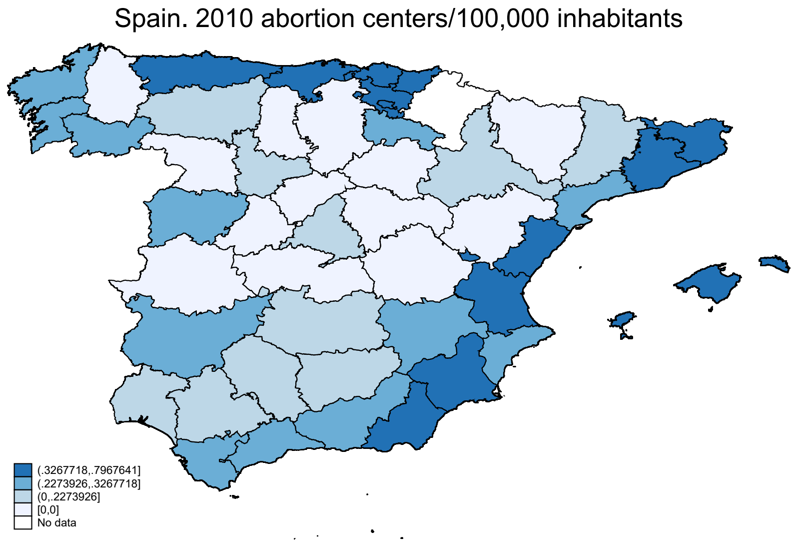


Figure 2.7.5: Number of abortion centers per 100,000 inhabitants in 2010

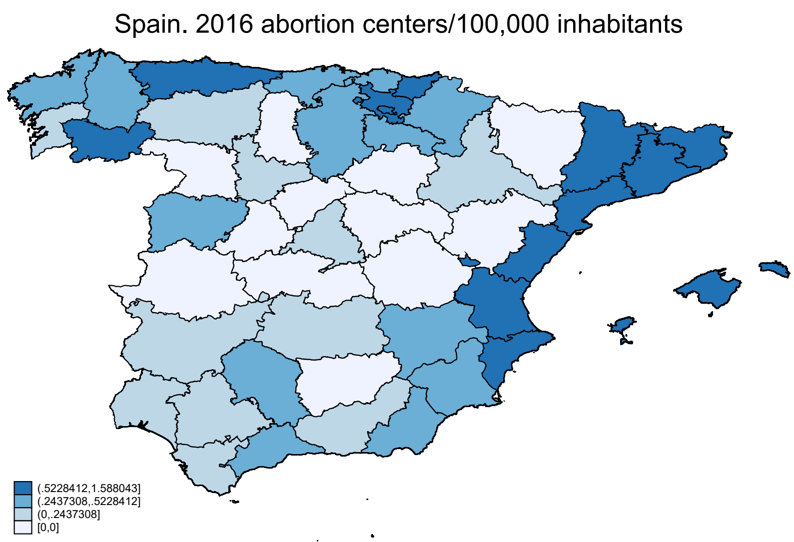


Figure 2.7.6: Number of abortion centers per 100,000 inhabitants in 2016

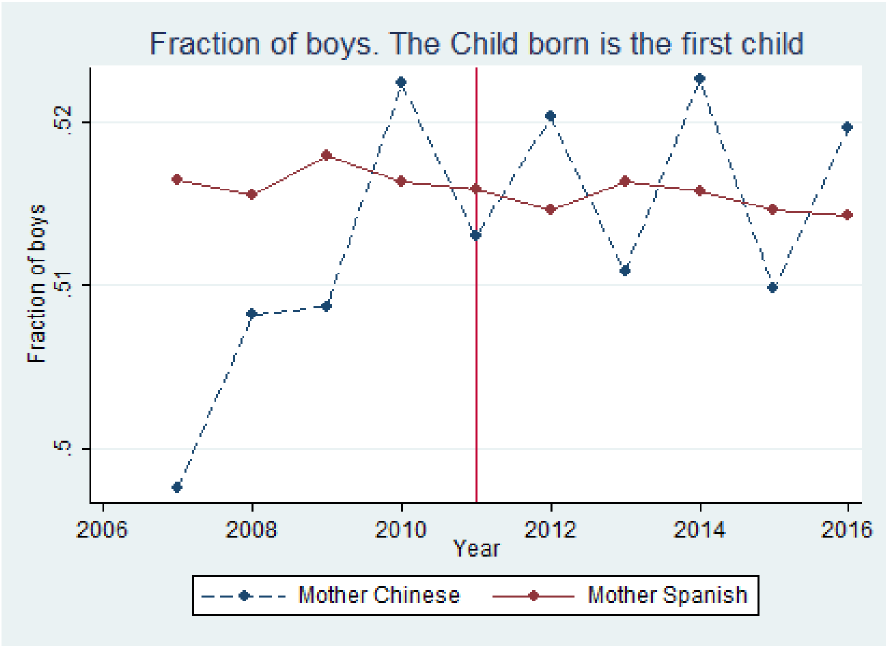


Figure 2.7.7: Fraction of boys born in Spain from mothers with Chinese and Spanish nationality giving birth to one child who is their first child

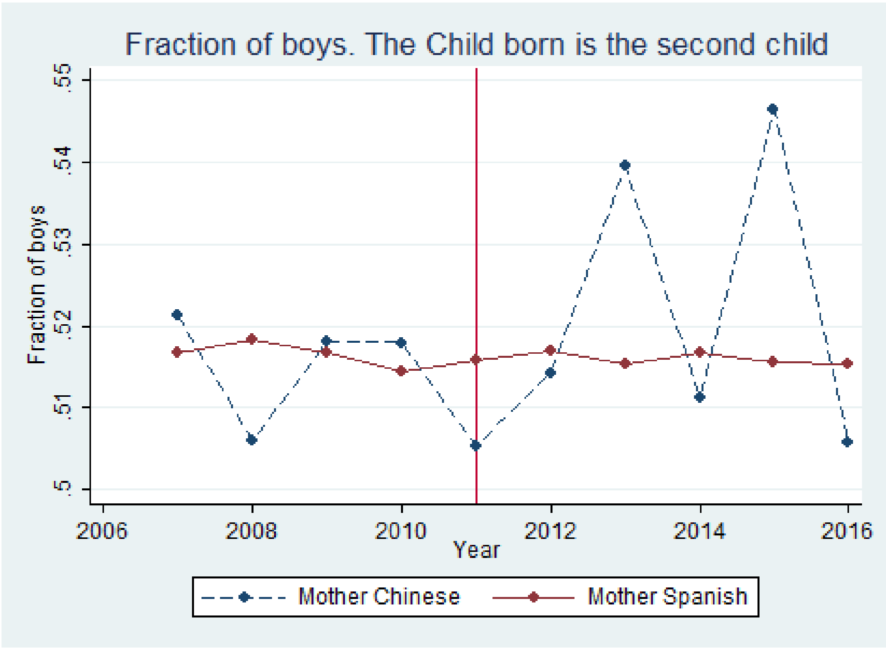


Figure 2.7.8: Fraction of boys born in Spain from mothers with Chinese and Spanish nationality giving birth to one child who is their second child

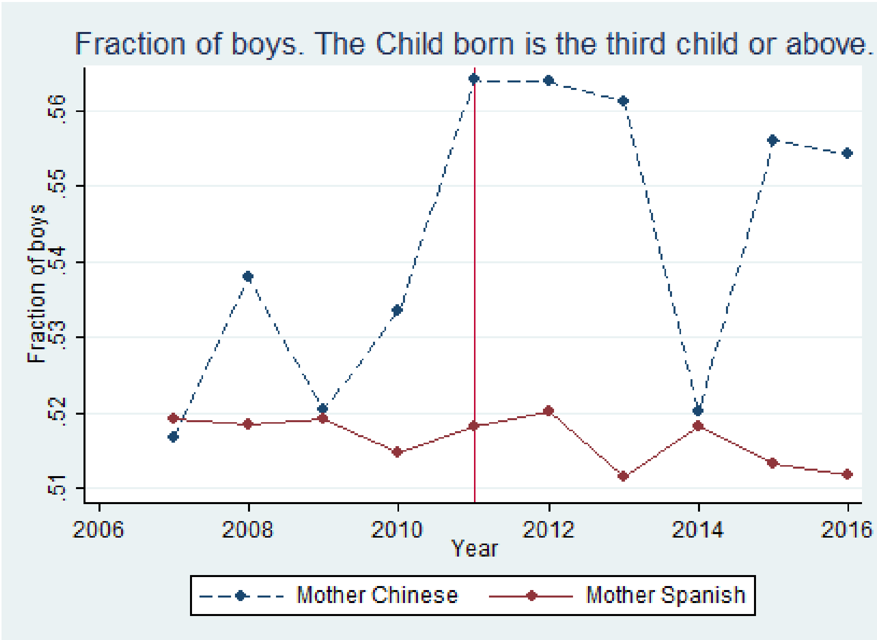


Figure 2.7.9: Fraction of boys born in Spain from mothers with Chinese and Spanish nationality giving birth to one child who is their third child or above

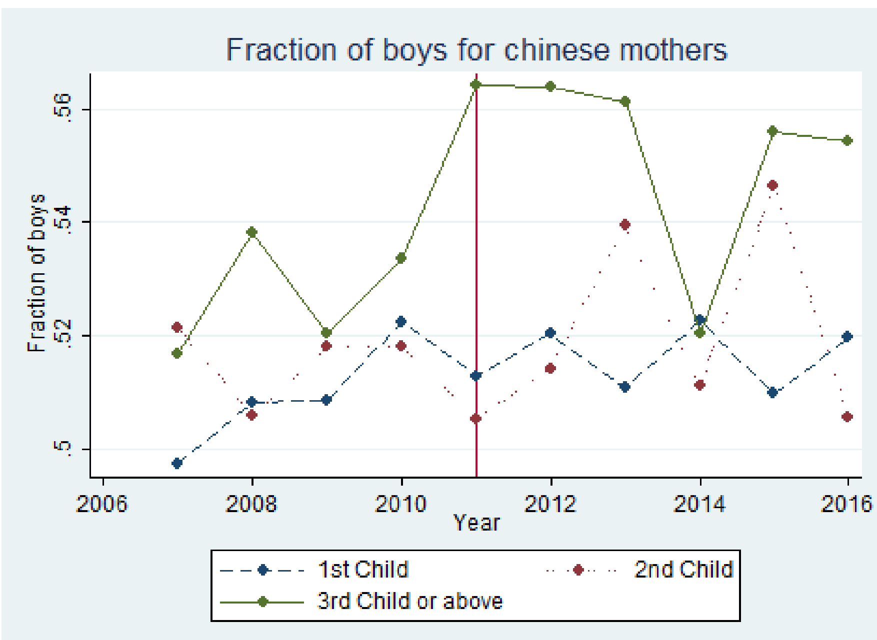


Figure 2.7.10: Fraction of boys born in Spain from mothers with Chinese nationality giving birth to one child

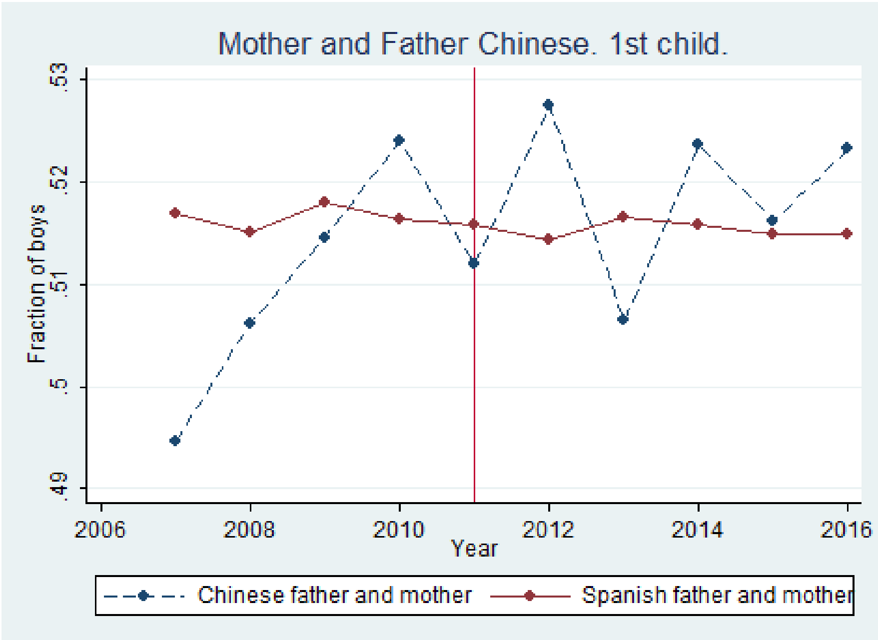


Figure 2.7.11: Fraction of boys born in Spain from mothers and fathers with Chinese and Spanish nationality giving birth to one child who is their 1st child

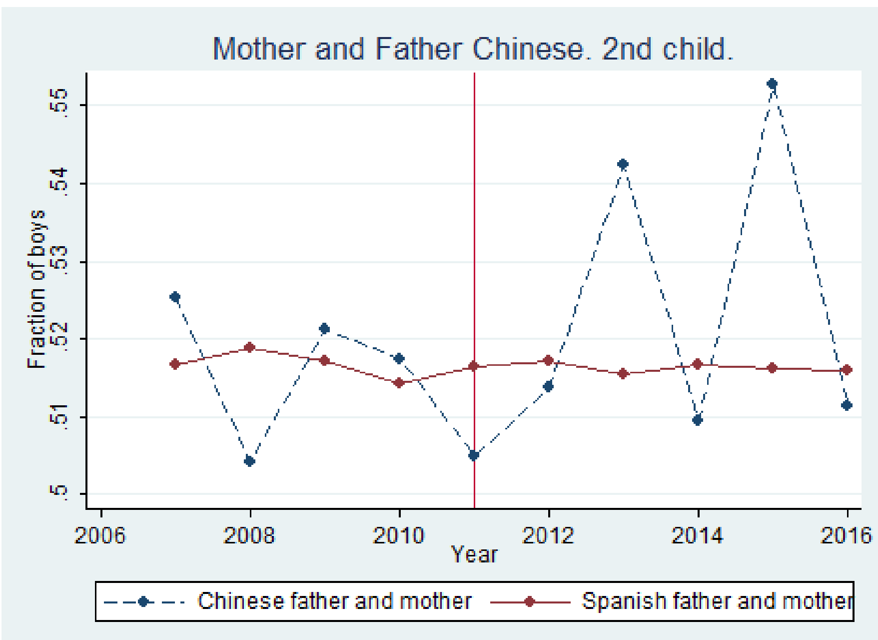


Figure 2.7.12: Fraction of boys born in Spain from mothers and fathers with Chinese and Spanish nationality giving birth to one child who is their 2nd child.

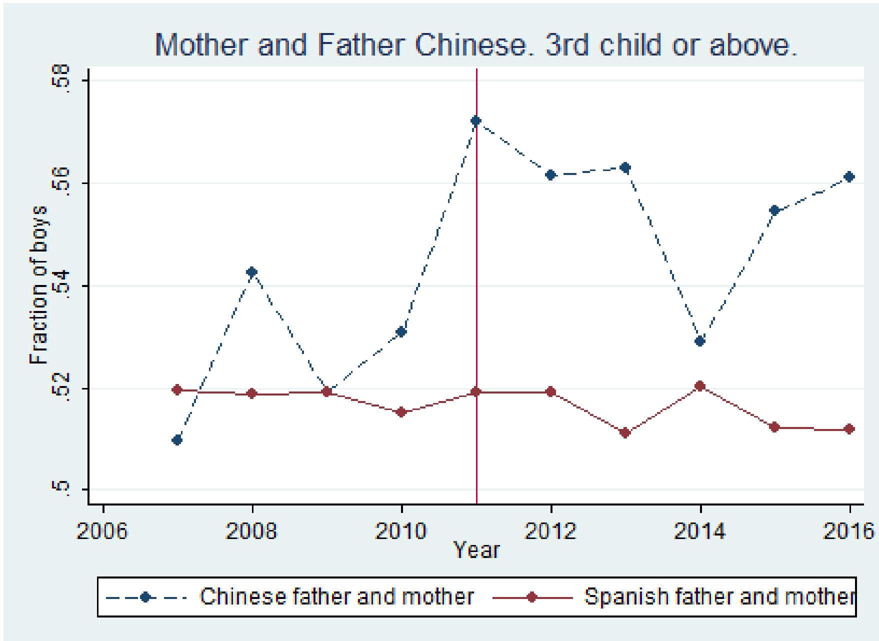


Figure 2.7.13: Fraction of boys born in Spain from mothers and fathers with Chinese and Spanish nationality giving birth to one child who is their 3rd child or above

Table 2.7.1: Summary Satistics. Children with mother and father with Spanish nationality and children with mother and father with Chinese nationality. Excluding twins. 2007-2016

	Spanish	Chinese
Married	0.641	0.639
	(0.480)	(0.480)
Mother's age	32.32	28.52
	(5.020)	(4.916)
Father's age	34.42	31.03
	(5.515)	(5.301)
Mother has less than High School	0.312	0.831
	(0.463)	(0.375)
Mother college or more	0.385	0.0362
	(0.487)	(0.187)
Father has less than High School	0.407	0.833
	(0.491)	(0.373)
Father college or more	0.268	0.0303
	(0.443)	(0.171)
Number Previous Children born alive	0.571	0.677
	(0.750)	(0.783)
Child born is the first	0.540	0.483
	(0.498)	(0.500)
Child born is the second	0.377	0.385
	(0.485)	(0.487)
Child born is the third or above	0.0828	0.132
	(0.276)	(0.338)
Observations	3,325,621	36,342

mean coefficients; sd in parentheses

Table 2.7.2: Number of Abortions/(Number Abortions + Number Births) by woman's nationality and woman's living children (2 living children means that if the pregnancy had ended up in birth the child would have been the 3rd child of the woman). Post Reform. 2011-2016

	Chinese Women	Spanish Women
0 living children	15%	15%
1 living child	20%	11%
2 or more living children	65%	35%

Table 2.7.3: Difference-in-Difference. Mother with Chinese nationality (relative to mothers with Spanish nationality). Mothers for whom the child born is the first child. 2007-2016.

	(1)	(2)	(3)
mother_chinese	-0.00709	-0.0115	-0.0112
	(0.00599)	(0.00878)	(0.00881)
POST	-0.00195	-0.00102	-0.00118
	(0.00138)	(0.00160)	(0.00159)
POSTxmother_chinese	0.00790	0.00529	0.00532
	(0.00787)	(0.0102)	(0.0102)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	1949299	1709055	1709055
Adjusted R-squared	0	0	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.4: Difference-in-Difference. Mother with Chinese nationality (relative to mothers with Spanish nationality). Mothers for whom the child born is the second child. 2007-2016

	(1)	(2)	(3)
mother_chinese	-0.000990	0.00328	0.00304
	(0.00487)	(0.00544)	(0.00553)
POST	-0.00168	-0.000720	-0.000671
	(0.00177)	(0.00212)	(0.00214)
POSTxmother_chinese	0.00349	-0.00603	-0.00600
	(0.00612)	(0.00694)	(0.00698)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	1330519	1208861	1208861
Adjusted R-squared	0	0	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.5: Difference-in-Difference. Mother with Chinese nationality (relative to mothers with Spanish nationality). Mothers for whom the child born is the third child or above. 2007-2016

	(1)	(2)	(3)
mother_chinese	0.00975	0.00648	0.00724
	(0.00953)	(0.00947)	(0.00940)
POST	-0.00703**	-0.0103**	-0.0103**
	(0.00322)	(0.00406)	(0.00412)
POSTxmother_chinese	0.0275*	0.0310**	0.0308**
	(0.0144)	(0.0153)	(0.0153)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	305721	270098	270098
Adjusted R-squared	.0001	0	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.6: Difference-in-Difference. Father (and mother) with Chinese nationality (relative to Father and mother with Spanish nationality). Mothers for whom the child born is the first child. 2007-2016

	(1)	(2)	(3)
chinese	-0.00636	-0.00971	-0.00937
	(0.00506)	(0.00830)	(0.00833)
POST	-0.00177	-0.000620	-0.000804
	(0.00147)	(0.00165)	(0.00166)
POSTxchinese	0.00905	0.00510	0.00516
	(0.00719)	(0.00889)	(0.00889)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	1812892	1630119	1630119
Adjusted R-squared	0	0	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.7: Difference-in-Difference. Father (and mother) with Chinese nationality (relative to Father and mother with Spanish nationality). Mothers for whom the child born is the second child. 2007-2016

	(1)	(2)	(3)
chinese	-0.000136	0.00417	0.00396
	(0.00516)	(0.00498)	(0.00505)
POST	-0.00118	-0.000440	-0.000365
	(0.00163)	(0.00197)	(0.00199)
POSTxchinese	0.00376	-0.00411	-0.00409
	(0.00651)	(0.00672)	(0.00677)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	1268917	1163452	1163452
Adjusted R-squared	0	0	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.8: Difference-in-Difference. Father (and mother) with Chinese nationality (relative to Father and mother with Spanish nationality). Mothers for whom the child born is the third child or above. 2007-2016

	(1)	(2)	(3)
chinese	0.00842	0.00654	0.00706
	(0.0101)	(0.00999)	(0.00985)
POST	-0.00714**	-0.0116**	-0.0114**
	(0.00353)	(0.00413)	(0.00419)
POSTxchinese	0.0325**	0.0334**	0.0333**
	(0.0150)	(0.0161)	(0.0161)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	280154	253129	253129
Adjusted R-squared	.0001	.0001	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.9: Effect on the Fraction of Boys using the current fraction of abortion centers to population in each Province. 2007-2016

	1st child		2nd child		**3rd child**	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	0.000240	0.000451	-0.00216	-0.00253	-0.0110**	-0.0130**
	(0.00210)	(0.00198)	(0.00224)	(0.00220)	(0.00484)	(0.00470)
chinese	-0.00776	-0.00908	-0.00858	-0.00994	0.0323	0.0305
	(0.00705)	(0.00728)	(0.00854)	(0.00839)	(0.0204)	(0.0201)
POSTxchinese	0.0140	0.0139	0.00172	0.00202	-0.00628	-0.00531
	(0.0119)	(0.0120)	(0.0144)	(0.0143)	(0.0219)	(0.0220)
POSTxratiocenters	-0.00495	-0.00470	0.000782	0.000872	0.0139	0.0150
	(0.00330)	(0.00330)	(0.00416)	(0.00409)	(0.0115)	(0.0112)
ratio_centers	0.00211	0.00190	0.00407	0.00398	-0.0131	-0.0137
	(0.00410)	(0.00413)	(0.00563)	(0.00560)	(0.0143)	(0.0141)
chinesexratiocenters	0.00697	0.00737	0.0246	0.0244	-0.0772	-0.0756
	(0.0225)	(0.0224)	(0.0223)	(0.0222)	(0.0577)	(0.0576)
POSTxchinesexratiocenters	-0.0140	-0.0143	-0.00146	-0.00130	**0.111****	**0.110****
	(0.0261)	(0.0261)	(0.0270)	(0.0270)	(0.0535)	(0.0535)
Birth Year FE	X	X	X	X	X	X
Birth Month FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Controls		X		X		X
Observations	1803063	1803063	1261128	1261128	278071	278071
Adjusted R-squared	0	0	0	0	.0001	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.10: Effect on the Fraction of Boys using the average centers/100,000 inhabitants in each Province during 2007-2016 as the access measure. 2007-2016

	1st child		2nd child		3rd child	
	(1)	(2)	(3)	(4)	(5)	(6)
POST	0.0000736	0.000537	-0.00176	-0.00204	0.000681	-0.0127**
	(0.00211)	(0.00192)	(0.00205)	(0.00209)	(0.00410)	(0.00424)
chinese	-0.000529	-0.00174	-0.0134	-0.0146*	0.0273	0.0252
	(0.00879)	(0.00898)	(0.00823)	(0.00801)	(0.0171)	(0.0168)
POSTxchinese	0.00776	0.00771	0.00728	0.00746	-0.00718	-0.00626
	(0.0144)	(0.0144)	(0.0122)	(0.0122)	(0.0209)	(0.0210)
POSTxaverageratiocent	-0.00526**	-0.00511*	0.00169	0.00171	0.0108	0.0118
	(0.00262)	(0.00262)	(0.00267)	(0.00266)	(0.00804)	(0.00781)
average_ratiocent	0.00383*	0.00373*	0.00000572	0.000264	-0.00863	-0.00907
	(0.00192)	(0.00190)	(0.00249)	(0.00247)	(0.00598)	(0.00595)
chinesexaverageratiocent	-0.0152	-0.0150	0.0318**	0.0314**	-0.0435	-0.0422
	(0.0157)	(0.0155)	(0.0126)	(0.0126)	(0.0342)	(0.0339)
POSTxchinesexaverageratiocent	0.00358	0.00312	-0.00704	-0.00673	0.0973**	0.0965**
	(0.0245)	(0.0246)	(0.0222)	(0.0223)	(0.0420)	(0.0419)
Birth Year FE	X	X	X	X	X	X
Birth Month FE	X	X	X	X	X	X
Controls		X		X		X
Observations	1812892	1812892	1268917	1268917	280154	280154
Adjusted R-squared	0	0	0	0	.0001	.0001

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.11: Effect on Chinese girls birth outcomes. Child born is the 1st Child. 2007-2016

	Less2500g	Less1500g	Premature	Livemore24h
chinese	-0.0423***	-0.00683***	-0.0277***	-0.000106
	(0.00378)	(0.000674)	(0.00321)	(0.000366)
girl	0.0118***	-0.000403**	-0.00716***	0.0000437
	(0.000627)	(0.000199)	(0.000549)	(0.0000392)
girlxchinese	-0.00365	0.000474	0.0108*	0.000333
	(0.00696)	(0.000813)	(0.00557)	(0.000382)
POST	0.00524***	0.00176***	-0.00264**	-0.000378***
	(0.00130)	(0.000404)	(0.00129)	(0.0000885)
POSTxchinese	0.00134	0.00260**	-0.00221	-0.000768
	(0.00429)	(0.00112)	(0.00475)	(0.000909)
POSTxgirl	-0.000360	0.000380	0.0000169	-0.0000724
	(0.000816)	(0.000260)	(0.000719)	(0.0000651)
POSTxgirlchinese	-0.00246	-0.000157	0.00115	-0.00102
	(0.00848)	(0.00162)	(0.00650)	(0.000975)
Birth Year FE	X	X	X	X
Birth Month FE	X	X	X	X
Controls	X	X	X	X
Province FE	X	X	X	X
Y mean	.0675	.007	.059	.9997
Observations	1591816	1591816	1630119	1630119
Adjusted R-squared	.0042	.0007	.0024	.0001

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.12: Effect on Chinese girls birth outcomes. Child born is the 2nd Child. 2007-2016

	Less2500g	Less1500g	Premature	Livemore24h
chinese	-0.0283***	-0.00153	-0.0107**	-0.000508
	(0.00452)	(0.00181)	(0.00453)	(0.000626)
girl	0.0112***	0.000155	-0.00597***	0.000112**
	(0.000727)	(0.000152)	(0.000550)	(0.0000403)
girlxchinese	-0.00424	0.00414*	0.000466	0.000716
	(0.00660)	(0.00243)	(0.00617)	(0.000625)
POST	-0.00364**	0.000860**	-0.0103***	0.000277*
	(0.00124)	(0.000304)	(0.00192)	(0.000144)
POSTxchinese	0.00578	-0.000468	-0.00757*	-0.000402
	(0.00449)	(0.00175)	(0.00385)	(0.00107)
POSTxgirl	-0.000462	-0.000462*	0.000936	-0.0000578
	(0.000838)	(0.000237)	(0.000952)	(0.0000507)
POSTxgirlchinese	-0.00120	-0.00313	-0.000961	-0.00151**
	(0.00764)	(0.00236)	(0.00608)	(0.000685)
Birth Year FE	X	X	X	X
Birth Month FE	X	X	X	X
Controls	X	X	X	X
Province FE	X	X	X	X
Y mean	.0475	.0046	.0506	.9997
Observations	1133827	1133827	1163452	1163452
Adjusted R-squared	.0044	.0004	.0025	.0001

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.13: Effect on Chinese girls birth outcomes. Child born is the 3rd Child or above. 2007-2016

	Less2500g	Less1500g	Premature	Livemore24h
chinese	-0.0492***	-0.00575***	-0.0378***	0.000437***
	(0.00539)	(0.00144)	(0.00775)	(0.0000813)
girl	0.0132***	0.000382	-0.00504***	0.000177**
	(0.00171)	(0.000501)	(0.00137)	(0.0000756)
girlxchinese	0.00386	0.00283	0.0114	-0.000171**
	(0.00861)	(0.00246)	(0.0104)	(0.0000780)
POST	-0.00546**	0.000353	-0.0136***	-0.000160
	(0.00220)	(0.000716)	(0.00305)	(0.000196)
POSTxchinese	0.0178**	0.00195	0.0124	-0.00137
	(0.00648)	(0.00225)	(0.00937)	(0.00107)
POSTxgirl	-0.00109	-0.000121	0.000204	-0.0000960
	(0.00177)	(0.000593)	(0.00205)	(0.000136)
POSTxgirlchinese	-0.0130	0.00203	-0.0199*	0.000699
	(0.0106)	(0.00394)	(0.0117)	(0.00140)
Birth Year FE	X	X	X	X
Birth Month FE	X	X	X	X
Controls	X	X	X	X
Province FE	X	X	X	X
Y mean	.0582	.006	.0672	.9996
Observations	244261	244261	253129	253129
Adjusted R-squared	.0094	.0008	.0053	.0001

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.7.14: Effect on Chinese girls birth weight and gestational weeks. 2007-2016

	1st child		2nd child		3rd child	
	BWeight	GestWeek	BWeight	GestWeek	BWeight	GestWeek
chinese	153.6***	0.0567	127.1***	-0.0127	203.0***	0.210**
	(8.113)	(0.0340)	(15.62)	(0.0684)	(18.55)	(0.0682)
girl	-111.6***	0.0876***	-128.8***	0.0902***	-123.0***	0.0639***
	(1.392)	(0.00419)	(1.439)	(0.00675)	(3.646)	(0.0129)
girlxchinese	16.57	0.0197	20.49	-0.0413	-7.826	-0.141
	(13.62)	(0.0333)	(15.69)	(0.0707)	(23.81)	(0.0952)
POST	-12.63**	-0.0452***	11.65**	0.0258	32.31***	0.0454*
	(4.194)	(0.0114)	(3.490)	(0.0168)	(6.197)	(0.0241)
POSTxchinese	-17.56	0.0541	-3.782	0.0582	-60.22**	-0.249**
	(12.34)	(0.0352)	(16.88)	(0.0492)	(17.33)	(0.0839)
POSTxgirl	-1.049	-0.00367	0.865	-0.0127*	1.564	0.00355
	(1.008)	(0.00517)	(1.887)	(0.00725)	(3.904)	(0.0214)
POSTxgirlchinese	1.740	-0.0765*	0.877	0.0767	17.42	0.293**
	(14.38)	(0.0416)	(17.48)	(0.0757)	(25.27)	(0.111)
Birth Year FE	X	X	X	X	X	X
Birth Month FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	3198.5	39.21	3287.02	39.07	3274.55	38.86
Observations	1591809	1417988	1133826	980819	244260	206653
Adjusted R-squared	.0196	.0046	.0263	.0065	.0303	.0079

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Chapter 3:

On the effect of income transfers (announcements) on birth outcomes: Evidence from Spain

3.1 Introduction

Understanding the determinants of birth outcomes is important since if birth outcomes are improved, later-life health outcomes of the benefited children may also benefit (Barker (1992); Black et al. (2007)). Understanding the relationship between income and birth outcomes is thus relevant since, beyond being interesting per se, a natural channel to explore to improve those is to provide unconditional cash transfers to parents during the pregnancy period or soon after the child is born. These payments, or their expectation, could improve birth outcomes via several channels like reducing parents' stress or improving nutrition, among others.

This paper estimates the effect on birth outcomes of in-utero exposure to the knowledge that a cash transfer will take place soon after the child is born. In particular, we study the Spanish *Cheque Bebé* that took place in Spain in 2007. We exploit the fact that the policy was a universal cash transfer policy, consisting of 2,500 euros per child, announced by surprise. It was announced that the parents of those children born from the day of the announcement onward (July 3, 2007) would receive a one-time payment of 2,500 euros.

Understanding the effects of a universal cash transfer on birth outcomes has two main challenges. First, since the policy was universal, it is unclear how to construct an appropriate control group. Second, the income transfer is likely to affect fertility (even via abortion decisions) and thus create a composition effect. Some parents might now decide to have a child because of the cash transfer while they would not have had it before.[1] Those who decide to have a child now are likely to be different than those who were having a child before the policy change. In order to address this second challenge, we take advantage of the timing of the policy. The surprise announcement factor, together with the fact the income transfer would be paid to all births happening from the moment of the announcement onward, implies that for some months, birth outcomes were affected by the expectation of receiving a cash transfer, but not by parents fertility response to the policy.

Because of this, together with the evidence that nutritional changes occurring on the third trimester of pregnancy are the ones most likely to affect birth weight, we compare the birth outcomes of interest of those born between April-June with those born between September-December 2007.[2] Moreover, because of the evidence of seasonal effects, we use the previous years, 2003-2006, to control for differences between those born between Apr-Jun and those born between Sept-Dec. That is, we estimate a difference-in-difference analysis between 2003-2007 where those born between April-June are the "control" group, and those born between Sept-Dec are the "treated" group. 2007 is the "treated" period.

First, we estimate the difference-in-difference with the universe of births taking place in Spain. Consistent with the findings of the literature, we show effects on birth weight, but not on gestational weeks. These effects are small. We do not find any impact on the low birth weight fraction, the fraction being premature, or the fraction living more than 24 hours.

Second, we notice that the income cash transfer is not likely to affect everyone equally. We would expect that if it has an effect, it should affect those with lower resources more since, for them, the 2,500 euros represent a higher income shock. We then try to target those groups who are more vulnerable. Unfortunately, we do not have parents' income or education information at the individual level.[3] To try to target those who are more

[1] González (2013) shows that one of the effects of the policy was indeed to increase fertility.

[2] Note that the earliest the composition effect could happen is via the abortion decisions channel. Births happening in December 2007 are too early for these decisions to affect them.

[3] Information on parents' education starts in 2007.

vulnerable, we first use the average income level at the municipality level. We show that the effects on birth weight are higher in those municipalities at the lowest 10th percentile of the income distribution. We do not find any significant effect for those municipalities at the top 10% of the income distribution. However, positive and significant effects are found for those at the top 25%. No effects are found in the other birth outcomes studied.

We then try to target at the individual level by comparing those parents who are married to those who are not, and those young parents (between 18 and 25) with those who are older. Consistent with our story, effects on birth weight are significant and stronger for those single women and those women who are younger. For those single women, the fraction of premature births decreases slightly. Overall, these results provide suggestive evidence that those who are more vulnerable are more likely to benefit the most. More precise data would be needed to reach a final conclusion.

This paper contributes to the literature that tries to understand the effects of unconditional cash transfers on birth outcomes. For example, Amarante et al. (2016) study an income transfer program in Uruguay and show a decrease in the fraction of low birth weight. Almond et al. (2011) use the introduction of the *Food Stamp Program* (*FSP*), arguing that it represented an exogenous increase in income for the poor. They found that pregnancies exposed to the FSP program three months before birth yielded deliveries with increased birth weight.

The remaining of this paper is organized as follows. First, section 3.2 describes the 2,500 euros per child policy. Section 3.3 discusses the background literature. Section 3.4 describes the data. Section 3.5 discuss the identification strategy. Section 3.6 shows the results. Finally, section 3.7 concludes with a discussion.

3.2 The 2,500 euros per child policy

The 2,500 euros per child policy, known as *cheque bebé* in Spain, was a temporary program that consisted of an unconditional one-time cash transfer of 2,500 euros per child born or adopted between July 2007 and December 2010. The cash transfer was universal (it did not depend on the family income or any other family characteristic).

The policy started in 2007, which was a moment in which Spain was running a budget surplus. Its explicit goals, as stated in the law and announced by the government, were: (1) help parents with childbirth expenditure, (2) increase fertility, and (3) maintain the living standards of low-income families. We now describe the policy in detail.

3.2.1 Timing, credibility, and coverage

On July 3, 2007, the Spanish president, Jose Luis Rodriguez Zapatero, announced the policy by surprise during the "State of the Nation" address in Congress. It was the star proposal of the address. It was breaking news on TV and the next day the announcement appeared in the front pages of the main newspapers. The fact that there were elections coming (on May 2008) caused that the other political parties classified the measure as electioneering. The important thing for this paper, however, is that the policy was announced by surprise. Because of this, it did not give any incentives to modify the day of birth. This is so because at the announcement day, on July 3, it was announced that the policy would benefit those children born after July 3 (at the end, the policy benefited those born after July 1, 2007, for logistical reasons) which implies that there was no time to modify the day of birth to benefit from the beginning of the policy.

Ten days later, on July 13, the government sent the law to Congress for debate and asked to process it urgently. The same day, during the regular press conference after the government meeting, the government announced that the law would be approved in November 2007 and that the first baby bonuses would be paid in December 2007. To make the announcement even more credible, it was announced that people could start filing for the bonus on July 16. Importantly, all these steps were covered in detail by the press.

The proposal became a law (as announced) on November 15, 2007, and the first baby bonuses were paid on late November (slightly earlier than expected).[4] Three years later, on May 10, 2010, the government announced the end of the policy during the first round of budget cuts caused by the economic slowdown. It was announced that the baby bonus would not be paid to children born after December 31, 2010. That is, the cancellation date of the policy was announced with almost seven and a half months of anticipation. This created an incentive for those births planned for the beginning of January 2011 to be moved to the end of December 2010 to receive the 2,500 euros payment.[5]

3.2.2 The beneficiaries, the time of payment, and the take-up

The subsidy was paid to all mothers giving birth on or after July 1, 2007, until December 31, 2010, who satisfied the following two conditions: (1) giving birth in Spain, and (2) having legally resided in Spain continuously for at least the two years before giving birth. The second requirement had to be satisfied by the mother (not by the father). If the father satisfied the second requirement, but the mother did not, the subsidy would not be paid. Moreover, the subsidy was paid to the mother. Importantly, the program eligibility and the amount of money paid was independent of any other family characteristic such as income. The same conditions were required for receiving the subsidy in case of adoption.

The first subsidies, as mentioned before, were paid in late November. Therefore, those families whose children were born before late November 2007 started to receive their payments by late November, which implies, for example, that those who gave birth in July started receiving the payment five months after giving birth. Once the program was implemented the payment was expected to take place within one month of giving birth.[6]

The application for the subsidy was considerably simple since mothers only had to

[4] Law 35/2007 (November 15, 2007).

[5] Borra et al. (2019) use this to analyze the effects of scheduling birth early on infant health. They find that a significant number of births were shifted from early January 2011 to late December 2010, affected babies had about 250 grams lower birth weight, and they suffered significantly more hospitalizations during the first 15 months of life.

[6] Unfortunately, it is not possible to know whether the mother applied/received the subsidy and, if she had received it when the payment took place.

file a form to apply. To have a sense of the take-up, since there will be no individual information on whether a specific mother received the payment or not, it is worth checking the total number of baby bonuses that the tax authorities in Spain reported having paid. In 2007 (the year of the implementation of the baby bonus), they reported having paid 161,983 baby bonuses, which represented a total expenditure of 404.95 millions of euros. The 161,983 baby bonuses paid (one per child) represents approximately 80% of the total number of births that took place in Spain between July 1 (beginning of the policy) and December 31 of 2007. This 80% needs to be taken as a lower bound since not all births that took place in Spain during that period were eligible for receiving the subsidy (only those from mothers who satisfied the conditions mentioned above were eligible). In 2008, a year where the policy was already consolidated, the tax authorities reported having paid 491,557 baby bonuses which represented 95% of the total births that took place in Spain in 2008.[7] It is important to notice that 2007 was the year of the policy implementation, and therefore the policy was not widely known to everyone. The high take-up of 2007 and the almost full take-up of 2008, a year in which the policy was consolidated, shows that the application costs were very low and that people knew about the existence of the policy, which is consistent with the broad coverage that it had in the media.

3.2.3 Putting the 2,500 euros magnitude in perspective and giving birth in Spain

To put the 2,500 euros payment in perspective, we can compare it with the monthly earnings in Spain in 2007. In 2007 the monthly gross minimum wage for a full-time job in Spain was 570.60 euros. The baby bonus payment was thus equivalent to 4.4 times the monthly wage of a low-wage full-time worker in Spain. About 20 percent of working women earned the minimum wage or below, and the median female gross monthly earnings in 2007 were about 1,190 euros. Therefore, the baby bonus payment more than doubled the median female gross monthly earnings.[8]

[7] Memoria 2007 and 2008, Agencia Tributaria (www.aeat.es)
[8] 2007 *Wage Structure Survey*

Maternity care in Spain is mainly provided by the publicly funded and publicly run National Health Service, which is highly valued. Mothers with private insurance (many public servants who have the chance to opt for private healthcare and some wealthy families) tend to give birth in private clinics as long as there are no birth complications. The average length of hospital stay and guidelines of patient care are similar in private hospitals and the National Health Service, but private hospitals carry out more c-sections (Redondo et al. (2013)). Because of this system, the 2,500 euros will not be used to pay any better treatment or necessary procedure, and so this will not be a potential channel in case of finding an effect on birth outcomes.

3.3 The baby bonus and Infant Health. Background Literature

The previous section describing the baby bonus policy has tried to make two crucial points: (1) the policy announcement was credible, and (2) people knew about the policy during the first months of implementation as the high take-up levels show. These two points are important because they are necessary conditions to argue that the policy announcement made people believe that they would receive a positive income shock when their child was born. Because of the payment timing (after birth) and the variables of interest (birth outcomes), this policy is different from a policy that made the payment during the pregnancy period. Here, instead, what happens during pregnancy is the knowledge that a payment transfer will be made in the future. Economic theory tells us that if nobody were credit constrained, the timing of the payment would not matter. Therefore, as long as people knew for sure that this payment would happen, the timing of the payment would be irrelevant. However, if people are credit constrained, then the payment timing would be crucial since credit-constrained people, who are those who are more likely to benefit more from a cash transfer, would not be able to use these extra resources during pregnancy. Unfortunately, we do not have information on whether people are credit constrained or not. It is important to notice that having a child represents a considerable shock in terms of financial expenses when the child is born for those with fewer resources. Therefore, even if parents were credit constrained it is possible that they were making an extra effort to save some money every month to pay for all those extra expenses. If they know that they will receive a cash transfer when the child is born, they would not have to save this money. This is an example of a potential channel of how the announcement of the payment could benefit credit constrained families.[9] In the remaining of this section, we briefly discuss the biomedical and the economic literature of birth weight determinants. It is important to keep in mind this timing of the payment and notice that some channels, like a potential stress reduction, do not necessarily require the payment to be made, but just its announcement.

Maternal nutrition and maternal physical and mental health during pregnancy are identified as major determinants of birth outcomes by the biomedical and the economic research literature. Following Kramer (1987a, 1987b), birth weight can be decomposed into that related to the gestation length, and growth conditional on gestation length (intrauterine growth, IUG). The medical literature has documented the effect of poor maternal nutrition and health or cigarette smoking on IUG. However, less is known about the determinants of gestational length. Moreover, there is evidence that nutritional changes occurring on the third trimester of pregnancy are the ones most likely to affect birth weight.[10]

Some of the evidence on the determinants of birth weight comes from economic literature. Almond and Mazumder (2011) show that maternal fasting during Ramadan has negative effects on birth weight (around 40 grams). This is evidence that moderate changes in maternal nutrition and potentially other maternal lifestyle aspects like change in sleep or work patterns can affect birth weight. There is also evidence suggesting that exposure to violence and maternal stress reduce gestational length and increase low birth weight incidence (Camacho (2008); Aizer et al. (2016); Aizer (2011)). Knowing that

[9] Unfortunately, we will have no data to disentangle different potential channels. This is just a thought exercise with the only goal of illustrating in the abstract how this announcement could benefit credit constrained families.

[10] See the literature review from Rush et al. (1980).

shortly after giving birth, the mother going to receive a one-time considerable payment of 2500 euros could potentially reduce, at least in part, maternal stress originated in economic needs and the expenditure shock that happen when a child is born. Importantly, this channel would work independently of whether the family is credit-constrained or not, and it might have a bigger effect on those families who are credit constrained since the 2,500 euros are likely to represent a higher percentage of their income. Therefore, the baby bonus could, in theory, reduce maternal stress, and thus, according to this literature, it could potentially reduce low birth weight incidence and increase gestational length. Unfortunately, we will not have information on the parents' stress level at the moment of giving birth to assess if this channel plays an active role or not in this context.

Other evidence comes from studies that analyze government welfare and transfer programs. Some studies analyze programs that specifically aimed at improving the nutritional and health status of pregnant women. For example, Bitler and Currie (2005) and Hoynes et al. (2011) analyze the Special Supplement Nutrition Program for Women, Infants, and Children, which provides food and nutritional advice to pregnant women. Both papers find that the low birth weight incidents are reduced mainly through the IUGR channel rather than the gestational length channel. Parental care utilization seems to have a role in these results.

There is not much evidence of the effects of unconditional cash transfers on birth outcomes. This is relevant since there is no reason to believe that unconditional cash transfers would have similar effects to conditional cash transfers or transfer of specific products like food. Amarante et al. (2016) analyze a generous social assistance program - Plan de Atencion Nacional a la Emergencia Social (PANES)- that was implemented in Uruguay between April 2005 and December 2007. They exploit the fact that the program assignment depended on a discontinuous function of a baseline income score. They find that the program led to a sizable reduction in the incidence of low birth weight caused by a faster intrauterine growth rather than longer gestational length. Almond et al. (2011) use the introduction of the Food Stamp Program (FSP). The goal of the FSP was to increase the nutrition of the poor, but they argue that the FSP treatment represents an exogenous increase in income for the poor. They find that pregnancies exposed to FSP three months before birth yielded deliveries with increased birth weight, with the largest gains at the lowest birth weights. Currie and Cole (1993) focus on participation in the US Aid to Families with Dependent Children (AFDC) program. They find no significant effect on low birth weight even though mothers benefiting from the program were also more likely to receive Medicaid, Food Stamps, and housing subsidies, which could potentially improve birth outcomes.

It is important to notice that the unrestricted cash transfer could affect birth outcomes through other channels than the ones mentioned above. The policy might affect fertility (which was the main goal of the policy) since families who otherwise would have decided not to have a child now might decide to have it because of the 2,500 euros payment. This is likely to create a composition effect since these families might be different from the ones who were having a child without the policy, and thus the birth outcomes of their children might just be different because of that. To address this issue, we exploit the fact that this composition effect will appear only after those deciding on having a child because of the policy announcement have the child (this could happen through the decision to abort or not). González (2013) analyzes the effects of this baby bonus policy on fertility and early maternal supply. She uses a regression discontinuity-type design and finds that the 2,500 euros benefit increased fertility (she estimates that the policy caused an increase of the number of births around 6 percent), in part through a reduction in abortions. Ninety percent of abortions take place at less than 13 weeks of gestation, which implies that a reduction in the incidence of abortions after the policy announcement (July 3rd, 2007) would lead to an increase in the number of births starting in January 2008. This implies that during some months after the policy announcement (before January 2008), the composition effect will not exist.

Finally, an unconditional cash transfer could have a negative effect on birth outcomes through the increase in consumption of products that affect birth outcomes negatively, such as alcohol or cigarettes.

In short, economical and medical literature suggest the importance of maternal nutrition (and small changes to it) on birth weight (especially on the third trimester of pregnancy). Maternal stress and smoking during pregnancy seem to be relevant, with the effect coming, at least in part, through a change in gestational length. These are all channels potentially affected by a cash transfer announcement during pregnancy. Finally, previous unconditional cash transfer payments have found birth weight improvements, mainly coming through the IUG channel.

3.4 Data

Data on birth outcomes come from the *National Statistical Institute*. It is population-level data. That is, we have data on the universe of births in Spain. Importantly, the information appears as it appears in the official national registry. All families have to register the newborn babies within eight days of birth.

The data includes the year and month of birth. The time and date of birth are set in the documentation provided by the health center that assisted with the delivery. Importantly for this project, other information available coming from standardized forms filed at the registry, include the gender of the newborn, the birth weight (in grams), the weeks of gestation at birth, or whether the newborn lived more than 24 hours or not.

Moreover, it includes some information about the parents' characteristics like their age, their nationality, whether they are married or not, or the job category of the parents among others. Finally, it includes geographical information like the province and county of inscription of the child and the municipality size. Starting in 2007 there is information on parents' education level and whether the child was born by cesarean section or not. Unfortunately, this information is not available before 2007. There is no information on Apgar scores for any of the years.

Table 3.7.1 provides the summary statistics between 2003 and 2007 dividing the data between those born between April and June, and those born between September and December, which will be our "treated" group.[11] The table shows that overall, around 70% of the parents are married, 85% of them have the Spanish nationality, and the average number of previous children born alive is 0.57. 55% of the births represent the first child of the mother, 35% the second, and only 9% of the births represent the third child or above or the mother. Finally, fathers' average age is 33 years old while mothers' average age is 30.7 years old, and around 27% of the mothers report to work at home. Students are a minority among parents representing less than 1% of the parents' occupations among the births that took place between 2003 and 2007 in Spain.

Moreover, Table 3.7.1 also tests the difference between those born between April-June with those born between September-December. The table shows that, as expected and documented in the literature, those born between April and June are different than those born between September and December.[12] Those children born between September and December are more likely to be married, to have parents with Spanish nationality, or that the child born is the second child of the mother in comparison to those born between April and June. Those children born between April and June are also more likely to have a mother who is a student, parents who work at home or that the child born is the first or the third or above.[13]

3.5 Identification Strategy

Ideally, we would like to estimate the effects of the cash transfer announcement at different moments of pregnancy to see if the moment of the announcement, that is, the mo-

[11] We describe our identification strategy in detail in section 3.5.

[12] We discuss this in more detail when we discuss the identification strategy. See, for example,Buckles and Hungerman (2013) or Alba Ramírez et al. (2014).

[13] Unfortunately, "unemployment" is not among the different job categories and thus we cannot know whether parents are unemployed. "Working home" might include some parents who are unemployed.

ment in which the family learns that they will have extra-economic resources when the mother gives birth, matter in the variables of interest as the literature discussed above suggest. However, the main problem to do that (and to estimate any effect of this policy) is the lack of a clear control group given the universality of the cash transfer.

One of the potential effects of the baby bonus, as discussed before and studied in González (2013), is to affect the decision of having a child. Families who did not want to have a child (either because they could not afford it or for any other reason) could potentially decide to have a child now because of the 2,500 euros per child policy. If this happens, then we would have a composition effect, which would cause that families having a child after these decisions affect births would not be comparable to those families having a child before these decisions have any effect. This would imply that our estimates on the effects of the cash transfer on birth outcomes would include both effects: (1) the effects of the cash transfer on those children who would have been born independently of the policy, and (2) the composition effect if the birth outcomes of those whose decision depend on the cash transfer existence are different from the birth outcomes of those whose decision does not depend on the cash transfer existence. We want to estimate the effects of the cash transfer announcement without the composition effect since otherwise, the interpretation can be misleading. Importantly, the change in the decision of having a child or not having it because of the cash transfer could happen through abortions (deciding to abort vs deciding not to abort and have the child) and therefore the composition effect could start showing up as early as January 2008 as discussed before and showed in González (2013).

One possibility would be to use the end of the policy given that it was announced on May 10, 2010, that the baby bonuses would not be paid after December 2010. This implies that during January most of the women who were giving birth had taken their decision thinking that the baby bonus would be paid and so we would not face a composition effect problem (everyone around the threshold of December 2010 took their decision of having a child believing that the 2,500 euros per child would be paid). However, using the end of the policy (comparing those born before January 2011 with those born in and after January 2011) would have, at least, two main problems. First, as discussed above and already studied by Borra et al. (2019), the end of the policy created an incentive to schedule births earlier to receive the money. Second, it would be a story about "expecting a cash transfer at the moment of deciding to have a child and during the first weeks of pregnancy and then discovering that the cash transfer would not be paid" versus "expecting a cash transfer at the moment of deciding to have a child and during the entire pregnancy". This can be potentially very different than a story of "expecting and getting a cash transfer" versus "never expecting any cash transfer", which is the interest of this paper.

The above discussion points out that one of the main problems to estimate the effect of the policy on birth outcomes is to find an appropriate control group. Moreover, the fact that the outcomes of interest are outcomes at birth implies that we cannot use a *Regression Discontinuity* design comparing those born immediately before July 2007 with those born immediately after July 2007 since the cash transfer was announced on July 3, 2007. Therefore, even if those born after July 1 received the cash transfer there would have been no time for birth outcomes to be affected by the announcement.

Because of all that, and the evidence from the medical literature discussed before, that the birth weight is more responsive to nutritional changes affecting the third trimester of pregnancy, we propose to exploit the before and after difference created by the policy change comparing those births that happened in April-June with those happening in September-December 2007. By choosing this treated group, we avoid the composition effect factor. Moreover, we exclude July and August in order to give some time for the policy to have an effect on birth outcomes. The main identifying assumption is that both groups' birth outcomes would have evolved in the same way in the absence of the baby bonus policy. To defend this identification strategy, we are going to use data on the previous years starting in 2003. That is, we suggest to estimate the following difference-in-difference:

$$y_{imt} = \alpha + \gamma TREAT_m + \lambda year2007_t + \delta(TREAT_m * year2007_t) + \mu_t + \epsilon_{imt} \tag{3.1}$$

where

$$TREAT_m = \begin{cases} 1 & \text{if birth month = 9,10,11,12} \\ 0 & \text{if birth month = 4,5,6} \end{cases}$$

and,

$$year2007_t = \begin{cases} 1 & \text{if birth year=2007} \\ 0 & \text{if birth year =2003,2004,2005,2006} \end{cases}$$

Where y_{imt} is the outcome of interest of baby i born in month m in year t. The coefficient of interest is δ, and the simplest specification includes birth year fixed effects. In other specifications, some controls, province fixed effects, and province x birth year fixed effects are added.

The main reason not to compare before and after treatment in 2007 without using previous years is that there is evidence showing the existence of seasonality on family's characteristics. This literature argues that this seasonality on family's characteristics might explain seasonality effects on later outcomes.[14] This has been shown in the case of Spain by Alba Ramírez et al. (2014). Because of that evidence, it would not be surprising to find differences in levels in the variables of interest. Using previous years' data allows us to control for this seasonality.

3.6 Results

First we show results for the difference-in-difference analysis analyzing the entire population given the universality of the income transfer. Next, we try to target those who are

[14]See, for example, Buckles and Hungerman (2013).

more likely to be vulnerable based on our limited data since those are the ones who we would expect to benefit more from the policy. Moreover, in the appendix we show the evolution of the birth counts and provide some raw data graphs on the birth outcomes of interest.

3.6.1 Difference-in-Difference Results

Table 3.7.2 provides the results of estimating regression 3.1 on some covariates. Because of our discussion on how the policy was implemented, announced by surprise in July, we would expect to find no effects on these covariates since, to the best of our knowledge, nothing else changed in 2007 that affected those children born in April-June differently to those born in September-December. Moreover, December 2007 is already too early to observe any composition effect change based on those parents who decide to have the children because of the universal income transfer while before in the absence of this policy would not have had the child. Table 3.7.2 shows the effects on some of the covariates (fraction being married, mother's and father's age, indicator on whether the father and the mother are students). As expected, they did not change differently in 2007 for those in our "treated" and "control" groups despite some of them having different levels. This confirms the lack of composition effect that we had expected and reinforces our identification strategy.

Figure 3.7.1 plots yearly our birth outcomes of interest (birth weight, gestational weeks, fraction being low birth weight, fraction being premature, and fraction living more than 24 hours) for those born between April and June (not affected by the policy in 2007, but affected from 2008 to 2010) and those born between September and December (affected by the policy between 2007 and 2010). The figure shows how the birth weight, the gestational weeks, the fraction being low birth weight, and the fraction being premature evolve similarly for those children born between April-June and those born between September-

December in all the years except 2007.[15] The only exception is for the fraction living more than 24 hours. For this variable both groups seem to follow different paths. The only variable where the universal income transfer seem to have a minor effect is on children's birth weight. In 2007, the birth weight for those children born between April-June decreases slightly relative to 2006 while for those born between September-December, and thus affected by the income transfer announcement, it increases slightly. For the rest of the variables we do not observe any clear effect at the figure level.

Tables 3.7.3 - 3.7.8 estimate regression 3.1 and implement our difference-in-difference identification strategy. We focus on the effects of the universal cash transfer announcement on birth outcomes like birth weight (in grams), gestational weeks, the fraction being low birth weight (<2500g), the fraction being extreme low birth weight (<1500g), the fraction being premature, and the the fraction living more than 24 hours. Each outcome is analyzed in a separate table, and all the tables have the same structure. All the specifications include birth year fixed effects. Column (1) is the simplest specification, column (2) adds some controls including parents' age, civil status or nationality. Column (3) adds province fixed effects. This allows us to compare the stability of the coefficients across the different specifications. If the coefficients change considerably when controls are added this would cast some doubt to our results. Finally, standard errors are clustered at the province level in all the regressions.

Results are noisy and we only find significant effects on children's birth weight. Table 3.7.3 shows that the policy slightly improve birth weight (around 8 grams, the average birth weight is around 3,226 grams). This result is stable across the different specifications. For all the other birth outcomes that we analyze we find the sign we would expect suggesting an improvement (with the exception of the fraction living more than 24 hours, where we find a negative sign), but non of these coefficients are significant. Finding effects on birth weight, but not on the gestational weeks (IUG channel) is consistent with the pre-

[15]Notice that the figure also includes 2008-2010. In those years, both groups received the 2500 euros and thus there was no difference among them.

vious findings of the literature on the effects of unconditional cash transfer payments as discussed in section 3.3.

The results presented so far in this section, however, analyze the effects on the universe of births that took place in Spain in the period of interest. It is unclear that we should expect to find any effects when analyzing the universe of births since for some mothers, as discussed before, the announcement that they will receive 2,500 euros had no-effect. We would expect that the effects, if any, should appear among those children whose parents have a lower socioeconomic status. It is for this specific group that the channels discussed before, like potential reduction of stress, are likely to play a role. We next try to target these subgroups with the limited variables available in our data.

3.6.2 Heterogeneous Effects. Trying to Target the "Most Vulnerable"

Unfortunately, we do not have individual information on parents' income, and the individual information on parents' educational level only starts in 2007. Therefore, it cannot be used since we would not have any pre-treatment year, which given the differences in levels between those born in Apr-Jun versus those born between September-December shown before is critical. We start our attempt to target those who are more vulnerable and thus benefit the most from the universal income transfer program by using the average income level at the municipality level. We then compare those who are married to those who are not married, those mothers who are between 18 and 25 years old versus those who are older, and those who live in a municipality below 50,000 inhabitants to those who live in larger towns.

First, we start by comparing the effects in those municipalities with lower income per capita with those with higher income per capita.[16] In our birth outcome data, we have information about the municipality in which the birth was registered for all municipalities with a population above 10,000 inhabitants. Unfortunately, we do not have this data for

[16]We use the municipality income per capita estimation data from *Fundación de Estudios de Economía Aplicada* (*FEDEA*) for 2007 elaborated by Miriam Hortas-Rico and Jorge Onrubia (2015).

those municipalities with a population below 10,000 citizens. In our period of interest, around 16% of births (371,507 births) were registered in towns below 10,000 inhabitants, and thus we do not have this information for them.

Table 3.7.9 provides the results of estimating the effects on birth weight for municipalities with different average income level. In particular, for those municipalities at the 5th percentile (income below €12,083.46), 10th percentile (income below €12,954.67), 25th percentile (income below €14,857.71), top 25% (income above €20,473.15), and top 10% (income above €24,138.04). Consistent with our interpretation, we do not find any significant effects in those municipalities at the top 10% of income level, and results seem stronger at the bottom 10% of income level. However, results are also significant at the top 25% of income level. Unfortunately, our measure is at the municipality level and this might not be precise enough. Table 3.7.10 provides the results for the other outcomes of interest (gestational weeks, fraction being low birth weight, fraction being premature, and fraction living more than 24 hours) for those municipalities at the bottom 25% of income and those at the top 25% of income. Similarly to when we looked at the entire population, we do not find any effects either at the bottom of the income distribution at the municipality level for our birth outcomes of interest.

Second, Tables 3.7.11 - 3.7.15 try to target the most vulnerable groups by using the covariates available in our births data set. In particular, we compare those parents who are married with those who are not married expecting to find larger effects among those who are not married, those who are young (between 18 and 25) with those who are older expecting that the younger ones economic situation is more precarious, and those who live in small municipalities (less than 50,000 inhabitants) with those who live in larger municipalities. For this last comparison we do not have any clear prior.

We show larger effects for those groups who are more likely to be vulnerable. For birth weight, we find significantly effects on those children whose parents are not married, young parents, and large municipalities. For the other outcome of interest where we had not found any effect when we analyzed the entire population we now find positive effects on gestational weeks for those parents who are young (significant at the 10% level), and significant negative effects on the fraction being premature for those parents who are not married, and for those who live in larger municipalities. Finally, for those who are not married we also document a small negative effect on the fraction of children living more than 24 hours. This is the opposite sign to what we would have expected.

All this analysis provides suggestive evidence that most vulnerable groups' birth outcomes are likely to benefit from a credible cash transfer announcement. Unfortunately, our data faces limitations to try to target these groups at the individual level and provide more precise estimates and answers to this important question.

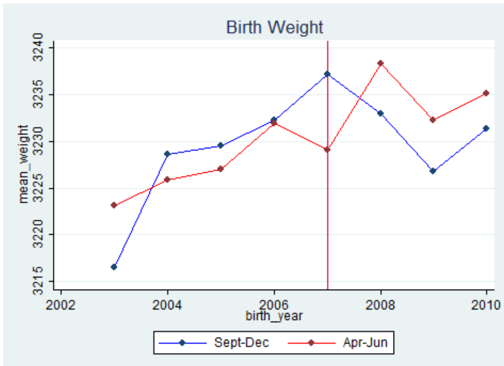
3.7 Conclusion

This paper contributes to the literature on the effects of unconditional cash transfers on birth outcomes. By exploiting a universal cash transfer announcement to all the mothers giving birth after its announcement and exploiting its particular timing that allows us to avoid any potential composition effect, we document an increase in the birth weight for those children born after the policy announcement relative to those born before and controlling for seasonality. Consistent with other studies, we do not find any significant effects on gestational weeks.

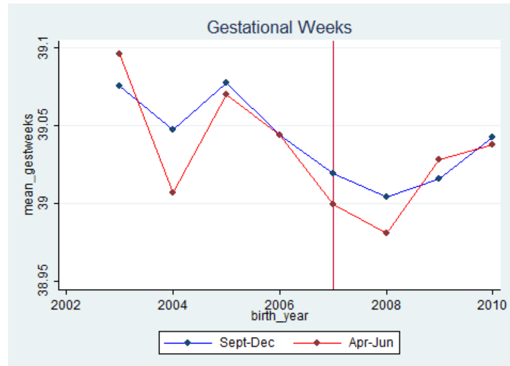
After documenting this increase, we try to target those who are more vulnerable since they are the ones more likely to benefit the most from the 2,500 euros cash transfer announcement. Using the limited covariates in our data set, we provide suggestive evidence that those who are more vulnerable experienced the larger impacts on birth weight. We target them, although imprecisely, by using the average income at the municipality level, whether parents are married or their age group.

This paper has thus provided evidence of the positive effects of unconditional cash transfers on birth outcomes. Fully understanding how different groups, specially the most vulnerable ones, are affected by this kind of policies is a crucial step towards improving birth outcomes.

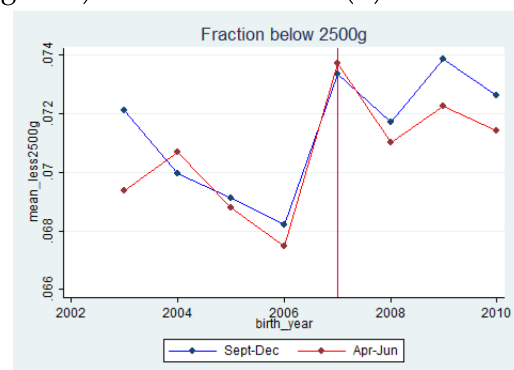
Figures and Tables



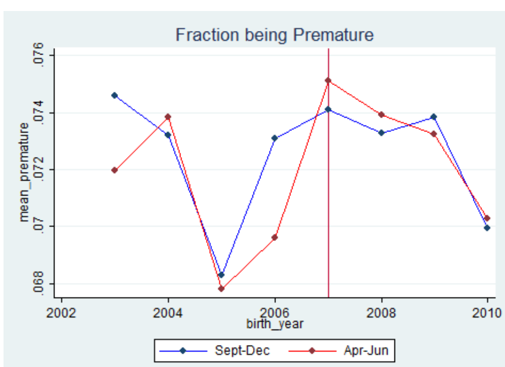
(a) Birth Weight (in grams)



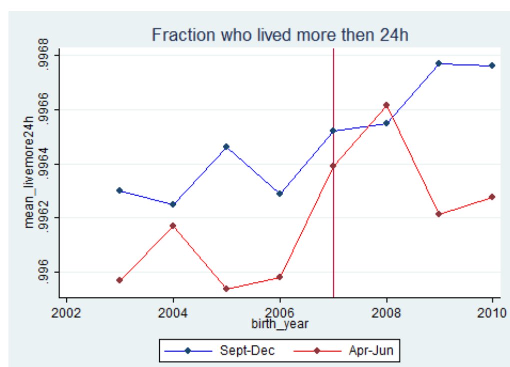
(b) Gestational Weeks



(c) Fraction with birth weight below 2500g



(d) Fraction born premature (<37 gestational weeks)



(e) Fraction who lived more than 24h

Figure 3.7.1: Difference-in-Difference

The figure shows the average of different outcome variables for the "treated" group (those born between September and December) and the "control" group (those born between April and June) from 2003 to 2010. The vertical line corresponds to the treatment year (2007).

Table 3.7.1: Summary Statistics. Children born between April-June, and children born between September-December. 2003-2007.

	Apr-Jun		Sep-Dec		
	Mean	SD	Mean	SD	Diff.
Married	0.743	0.437	0.719	0.450	0.024***
Father's age	33.346	5.717	33.350	5.840	-0.004
Mother's age	30.735	5.190	30.712	5.299	0.023*
Mother Spanish nationality	0.851	0.356	0.837	0.369	0.014***
Father Spanish nationality	0.864	0.343	0.852	0.355	0.012***
Mother Student	0.008	0.088	0.008	0.091	-0.001***
Father Student	0.002	0.043	0.002	0.043	-0.000
Mother works home	0.273	0.446	0.279	0.449	-0.006***
Father works home	0.003	0.055	0.004	0.059	-0.000***
Number Previous Children born alive	0.575	0.790	0.575	0.802	0.001
Child born is the first	0.553	0.497	0.559	0.496	-0.006***
Child born is the second	0.352	0.478	0.342	0.474	0.010***
Child born is the third or above	0.095	0.293	0.098	0.298	-0.003***
Birth Weight	3228.035	516.350	3229.545	517.879	-1.510
Low Birth Weight (<2500g)	0.069	0.253	0.069	0.254	-0.001
Gestational Weeks	39.044	1.898	39.053	1.916	-0.009**
Premature	0.072	0.258	0.073	0.259	-0.001*
Live more than 24h	0.996	0.062	0.996	0.060	-0.000*
Observations	568,698			787,460	

Table 3.7.2: Effects on some covariates. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	Married	MothAge	FathAge	MothStudent	FathStudent
TREAT	-0.0227***	0.00878	0.0202**	0.000580**	0.0000339
	(0.00119)	(0.0109)	(0.00881)	(0.000205)	(0.000107)
TREATxyear2007	-0.00124	-0.0261	0.00456	-0.000262	0.000194
	(0.00206)	(0.0182)	(0.0219)	(0.000528)	(0.000255)
year2007	-0.0672***	0.342***	0.411***	0.00632***	0.00101***
	(0.00352)	(0.0273)	(0.0305)	(0.000578)	(0.000170)
Birth Year FE	X	X	X	X	X
Province FE	X	X	X	X	X
Y mean	.73	30.71	33.33		.86
Observations	1356158	1356158	1334374	1303146	1278198
Adjusted R-squared	.0205	.0154	.0097	.002	.0002

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.3: Effects on Birth Weight. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	(1)	(2)	(3)
TREAT	-0.251	-0.545	-1.570
	(1.303)	(1.388)	(1.543)
TREATxyear2007	8.160**	7.886**	7.560**
	(3.646)	(3.524)	(3.583)
year2007	7.357**	3.979	3.165
	(2.975)	(3.073)	(2.922)
Birth Year FE	X	X	X
Controls		X	X
Province FE			X
Y mean	3226.99	3226.99	3226.99
Observations	1292418	1272262	1272262
Adjusted R-squared	.0001	.0055	.0089

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.4: Effects on Gestational Weeks. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	(1)	(2)	(3)
TREAT	0.00709	0.00784	0.00657
	(0.00770)	(0.00761)	(0.00795)
TREATxyear2007	0.0127	0.0132	0.0123
	(0.0162)	(0.0158)	(0.0161)
year2007	-0.0809***	-0.0668***	-0.0663***
	(0.0169)	(0.0172)	(0.0184)
Birth Year FE	X	X	X
Controls		X	X
Province FE			X
Y mean	39.04	39.04	39.04
Observations	1173417	1155227	1155227
Adjusted R-squared	.0002	.0021	.0046

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.5: Effects on fraction being Low Birth Weight (<2500g). Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	(1)	(2)	(3)
TREAT	0.000858*	0.000693	0.000715
	(0.000477)	(0.000495)	(0.000522)
TREATxyear2007	-0.00111	-0.000987	-0.000955
	(0.00130)	(0.00125)	(0.00126)
year2007	0.00347**	0.00281*	0.00283*
	(0.00147)	(0.00147)	(0.00144)
Birth Year FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.07	.07	.07
Observations	1356158	1334374	1334374
Adjusted R-squared	0	.001	.0015

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.6: Effects on fraction being Extreme Low Birth Weight (<1500g). Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	(1)	(2)	(3)
TREAT	-0.000165	-0.000182	-0.000185
	(0.000187)	(0.000188)	(0.000190)
TREATxyear2007	-0.000110	-0.000154	-0.000157
	(0.000417)	(0.000423)	(0.000423)
year2007	0.000798**	0.000437	0.000446
	(0.000306)	(0.000320)	(0.000323)
Birth Year FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.01	.01	.01
Observations	1356158	1334374	1334374
Adjusted R-squared	0	.0003	.0003

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.7: Effects on fraction being Premature. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	(1)	(2)	(3)
TREAT	0.00152**	0.00138**	0.00136*
	(0.000652)	(0.000620)	(0.000685)
TREATxyear2007	-0.00254	-0.00238	-0.00257
	(0.00163)	(0.00165)	(0.00164)
year2007	0.00251	0.00101	0.00161
	(0.00187)	(0.00184)	(0.00201)
Birth Year FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.07	.07	.07
Observations	1173417	1155227	1155227
Adjusted R-squared	.0001	.0005	.0018

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.8: Effects on fraction living more than 24h. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	(1)	(2)	(3)
TREAT	0.000310**	0.000332**	0.000338**
	(0.000142)	(0.000133)	(0.000132)
TREATxyear2007	-0.000311	-0.000260	-0.000252
	(0.000254)	(0.000244)	(0.000242)
year2007	0.000631**	0.000815***	0.000832***
	(0.000230)	(0.000230)	(0.000231)
Birth Year FE	X	X	X
Controls		X	X
Province FE			X
Y mean	1	1	1
Observations	1356158	1334374	1334374
Adjusted R-squared	0	.0002	.0005

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.9: Effects on Birth Weight for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007. By Municipality Income level

	All	Low 5%	Low 10%	Low 25%	Top 25%	Top 10%
TREAT	-1.515	-12.13	0.978	-6.500	-3.263	-5.539
	(1.481)	(11.03)	(9.204)	(5.407)	(2.224)	(3.566)
TREATxyear2007	9.486**	32.32	50.88**	20.25*	8.416*	10.97
	(4.042)	(25.55)	(17.40)	(11.57)	(4.879)	(7.687)
year2007	6.977**	-52.71	-75.11***	-6.597	7.964**	7.382*
	(3.189)	(47.11)	(14.38)	(7.866)	(2.930)	(4.106)
Birth Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	3225.12	3269.28	3252.32	3247.55	3218.03	3218.53
Observations	1095275	2311	5958	51458	683327	348042
Adjusted R-squared	.0028	.006	.0043	.0026	.003	.0036

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.10: Effects on Gestational Weeks, Low Birth Weight, fraction being Premature for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007. By Municipality Income level

	GestWeeks		less2500g		premature		livemore24h	
	Low 25%	Top 25%	Low 25%	Top 25%	Low 25%	Top 25%	Low 25%	Top 25%
TREAT	0.0166	-0.00219	0.00535*	0.00172*	-0.00129	0.00153	-0.0000224	0.000404*
	(0.0169)	(0.0117)	(0.00295)	(0.000987)	(0.00262)	(0.000963)	(0.000399)	(0.000226)
TREATxyear2007	0.0580	0.00867	-0.00579	-0.00180	-0.00208	-0.00156	-0.00104	-0.0000599
	(0.0550)	(0.0242)	(0.00712)	(0.00238)	(0.00831)	(0.00227)	(0.000729)	(0.000447)
year2007	-0.0530	-0.0968***	0.00874*	0.00240	-0.00181	0.00426**	0.000287	0.000646
	(0.0455)	(0.0252)	(0.00463)	(0.00196)	(0.00707)	(0.00208)	(0.000388)	(0.000415)
Birth Year FE	X	X	X	X	X	X	X	X
Province FE	X	X	X	X	X	X	X	X
Y mean	39.12	39.01	.07	.07	.07	.07	1	.99
Observations	45773	627821	53895	708999	45773	627821	53895	708999
Adjusted R-squared	.0033	.0027	.0008	.0006	.0011	.0013	.0005	.0006

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.11: Effects on Birth Weight for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	Married		$17 < \text{Parents age} < 26$		Munsize $< 50{,}000$	
	Yes	No	Yes	No	Yes	No
TREAT	-2.104	5.794**	-1.593	-1.543	-0.969	-1.213
	(1.776)	(1.949)	(5.135)	(1.346)	(1.655)	(1.601)
TREATxyear2007	5.581	11.88**	22.59*	6.669	3.623	10.90**
	(3.965)	(4.711)	(11.27)	(4.123)	(3.324)	(4.613)
year2007	8.601**	15.88**	7.655	5.098	7.393*	6.942**
	(2.957)	(4.688)	(8.396)	(3.142)	(3.962)	(3.021)
Birth Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	3238.14	3196.47	3195.35	3230.06	3238.71	3219.16
Observations	945215	347203	72144	1071089	517558	774860
Adjusted R-squared	.0037	.0018	.002	.0032	.0032	.0027

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.12: Effects on Gestational weeks for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	Married		17 < Parents age < 26		Munsize < 50,000	
	Yes	No	Yes	No	Yes	No
TREAT	-0.00197	0.0327**	0.0221	0.0000350	0.0113	0.00199
	(0.00790)	(0.0119)	(0.0245)	(0.00719)	(0.00765)	(0.00968)
TREATxyear2007	0.00534	0.0173	0.0887*	0.0101	-0.00195	0.0216
	(0.0133)	(0.0278)	(0.0453)	(0.0154)	(0.0123)	(0.0215)
year2007	-0.0906***	-0.0313	-0.0893**	-0.0802***	-0.0378*	-0.107***
	(0.0188)	(0.0244)	(0.0411)	(0.0186)	(0.0204)	(0.0194)
Birth Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	39.06	39.01	39.05	39.04	39.08	39.02
Observations	856713	316704	65142	975080	463250	710167
Adjusted R-squared	.0033	.0024	.0049	.0027	.0042	.0028

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.13: Effects on the fraction being Low birth Weight (<2500g) for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	Married		17 < Parents age < 26		Munsize < 50,000	
	Yes	No	Yes	No	Yes	No
TREAT	0.00161**	-0.00264**	-0.000487	0.00130**	0.000956	0.000796
	(0.000607)	(0.000888)	(0.00207)	(0.000531)	(0.000649)	(0.000779)
TREATxyear2007	-0.0000800	-0.00263	-0.00305	-0.000526	-0.0000559	-0.00182
	(0.00142)	(0.00225)	(0.00470)	(0.00154)	(0.00153)	(0.00166)
year2007	0.00314**	-0.0000368	-0.00147	0.00369**	0.00534**	0.00204
	(0.00154)	(0.00240)	(0.00421)	(0.00166)	(0.00162)	(0.00176)
Birth Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	.07	.08	.08	.07	.06	.07
Observations	988399	367759	76339	1120362	551522	804636
Adjusted R-squared	.0006	.0005	.0007	.0006	.0006	.0005

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.14: Effects on the fraction being Premature for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	Married		17 < Parents age < 26		Munsize < 50,000	
	Yes	No	Yes	No	Yes	No
TREAT	0.00219**	-0.00156	-0.00190	0.00202**	0.00153	0.00149*
	(0.000751)	(0.00116)	(0.00355)	(0.000660)	(0.000918)	(0.000845)
TREATxyear2007	-0.000397	-0.00725**	-0.0106	-0.00224	-0.000482	-0.00431*
	(0.00173)	(0.00258)	(0.00769)	(0.00166)	(0.00136)	(0.00217)
year2007	0.00180	0.00330	0.00515	0.00263	0.000795	0.00482**
	(0.00203)	(0.00300)	(0.00693)	(0.00191)	(0.00250)	(0.00214)
Birth Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	.07	.08	.08	.07	.07	.08
Observations	856713	316704	65142	975080	463250	710167
Adjusted R-squared	.0015	.001	.0017	.0013	.0014	.0014

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7.15: Effects on the fraction living more than 24h for different subgroups. Children born between April-June, and children born between September-December ("treated"). 2003-2007.

	Married		17 < Parents age < 26		Munsize < 50,000	
	Yes	No	Yes	No	Yes	No
TREAT	0.000177	0.000945**	-0.000379	0.000321*	0.0000399	0.000498**
	(0.000147)	(0.000333)	(0.000471)	(0.000172)	(0.000142)	(0.000202)
TREATxyear2007	-0.0000595	-0.00102*	0.000374	-0.000317	-0.000188	-0.000373
	(0.000262)	(0.000568)	(0.00117)	(0.000286)	(0.000189)	(0.000390)
year2007	0.000483**	0.00172**	-0.0000795	0.000677**	0.0000960	0.00100**
	(0.000214)	(0.000592)	(0.000925)	(0.000237)	(0.000189)	(0.000381)
Birth Year FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	1	.99	1	1	1	.99
Observations	988399	367759	76339	1120362	551522	804636
Adjusted R-squared	.0003	.001	0	.0003	.0004	.0007

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

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Appendix A: Chapter 1 Supplementary Materials

A.1 Difference-in-Difference. Supplementary Tables

A.1.1 Effects on 17 years old abortions relative to 18 and 19 years old

Table A.1.1: Effects on the fraction of 17 years old abortions relative to 17, 18, and 19 years old abortions

	All	Spanish	Non-Spanish
POST	-0.0239**	-0.0309**	-0.0176
	(0.0101)	(0.0116)	(0.0189)
Abortion Year FE	X	X	X
Abortion Month FE	X	X	X
Province FE	X	X	X
Y mean	.4	.41	.37
Observations	60238	42148	17061
Adjusted R-squared	.0012	.001	.0039

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

A.1.2 Effects on 16 years old Abortions. Heterogeneity

Table A.1.2: Effects on the fraction of 16 years old abortions relative to 16 and 15 years old abortions by who they live with

	All		Spanish		Non-Spanish	
	Alone/Part	Parents	Alone/Part	Parents	Alone/Part	Parents
POST	-0.0663**	-0.00443	-0.110***	0.00684	0.0782	-0.0378
	(0.0285)	(0.0291)	(0.0272)	(0.0329)	(0.0568)	(0.0738)
Abortion Year FE	X	X	X	X	X	X
Abortion Month FE	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	.66	.63	.66	.64	.67	.63
Observations	3658	11565	2628	8843	996	2552
Adjusted R-squared	.0032	.0016	.0041	.0019	.0179	.0036

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.1.3: Effects on the fraction of 16 years old abortions relative to 16 and 15 years old abortions by population size

	<10,000	10,000-50,000	50,000-500,000	>500,000
POST	-0.136**	-0.0140	-0.0122	-0.000687
	(0.0585)	(0.0495)	(0.0312)	(0.0768)
Abortion Year FE	X	X	X	X
Abortion Month FE	X	X	X	X
Province FE	X	X	X	X
Y mean	.65	.63	.64	.65
Observations	2244	4230	6359	2892
Adjusted R-squared	.0057	.0023	.0018	-.002

Standard errors in parentheses clustered at the province level.

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

A.2 Bunching Supplementary Materials

A.2.1 Bunching Robustness

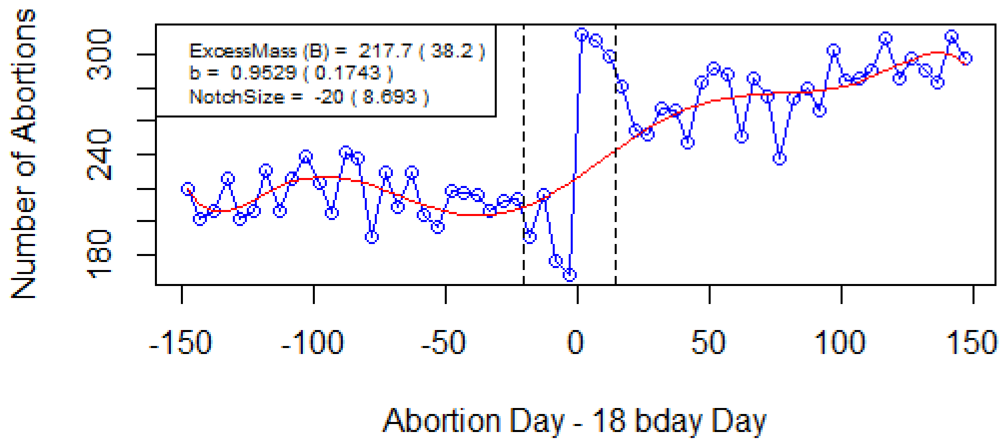
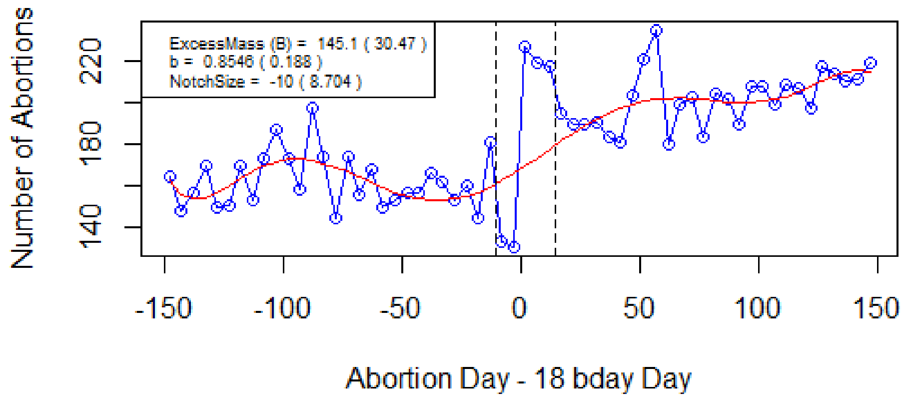
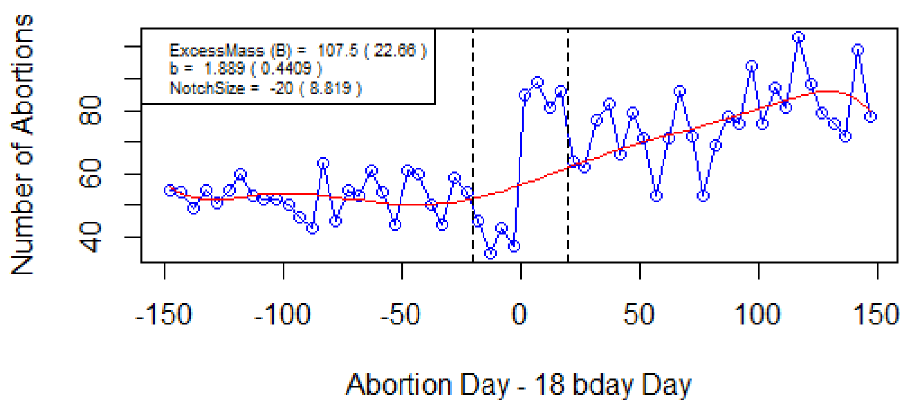


Figure A.2.1: Bunching Analysis. All 2011-2017.

Notch is at $\Delta 18 = 0$. Binwidth 5 days, bandwith 150 days before and after turning 18, $\Delta 18_U = 14$ days, polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.



(a) Before the policy reform

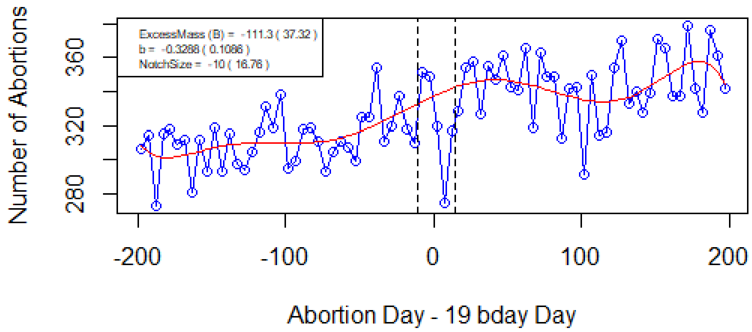


(b) After the policy reform

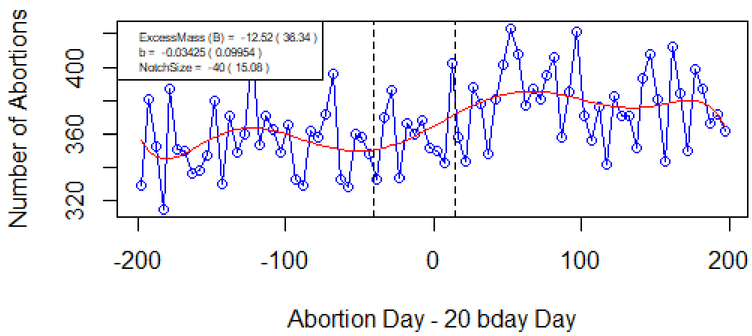
Figure A.2.2: Bunching Analysis. Before and After the Reform

Notch is at $diff18 = 0$. Binwidth 5 days, bandwith 150 days before and after turning 18, $\Delta 18_U = 14$ days $(19$ for the after), polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.

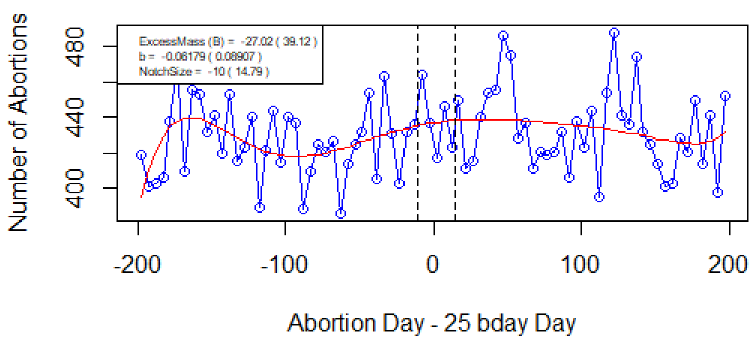
A.2.2 Placebo Test. Bunching around 19th, 20th, and 25th birthdays



(a) Abortions around 19th birthday. 2011-2017



(b) Abortions around 20th birthday. 2011-2017

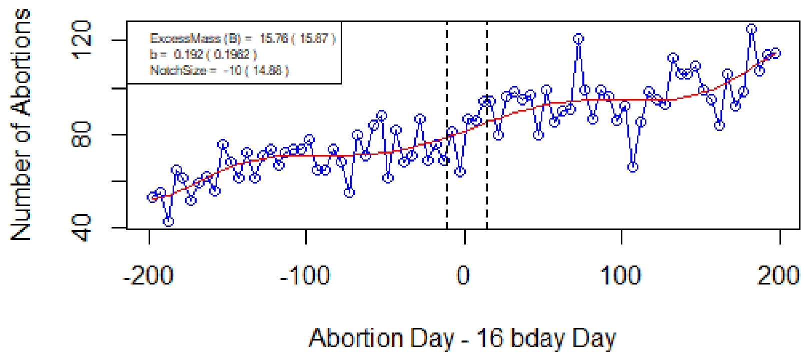


(c) Abortions around 25th birthday. 2011-2017

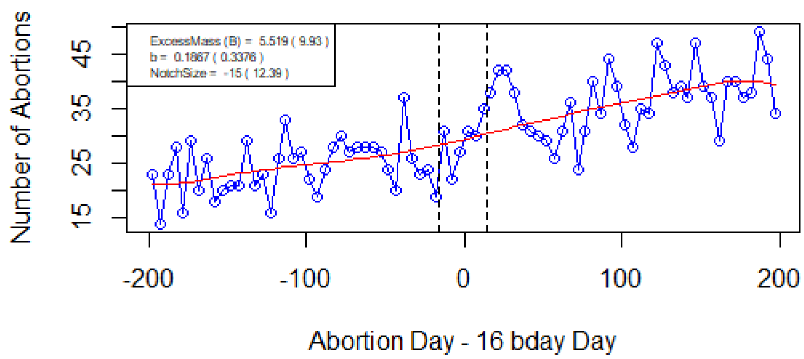
Figure A.2.3: Bunching Analysis. Placebo test

Number of abortions around 19th, 20th, and 25th birthday. Notch is at $diff = 0$. Binwidth 5 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days, polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.

A.2.3 Bunching around 16th birthday



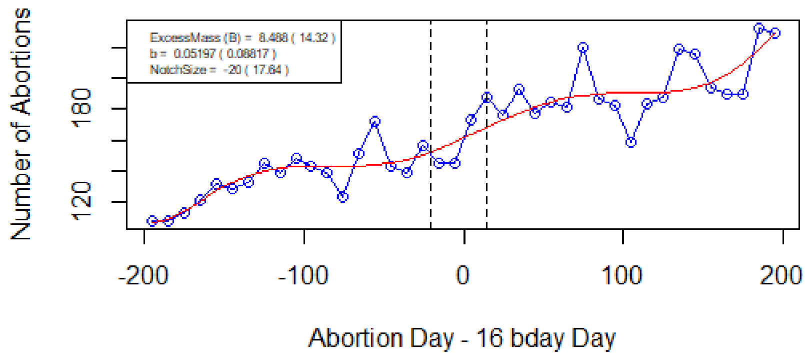
(a) Abortions around 16th birthday. Before the reform



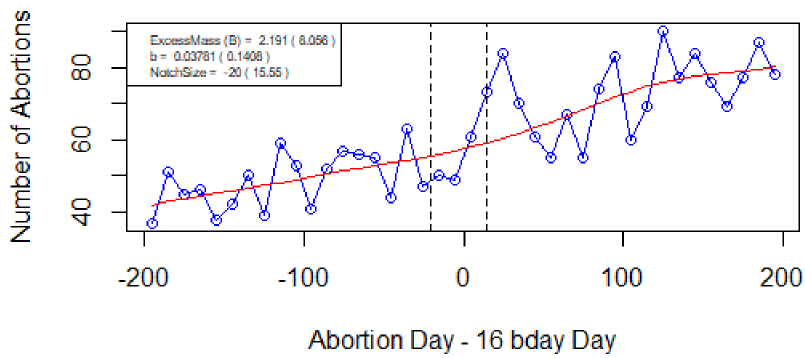
(b) Abortions around 16th birthday. After the reform

Figure A.2.4: Bunching Analysis. All 2011-2017. Number of abortions around 16th birthday.

Notch is at $\Delta 18 = 0$. Binwidth 5 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days, polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.



(a) Abortions around 16th birthday. Before the reform

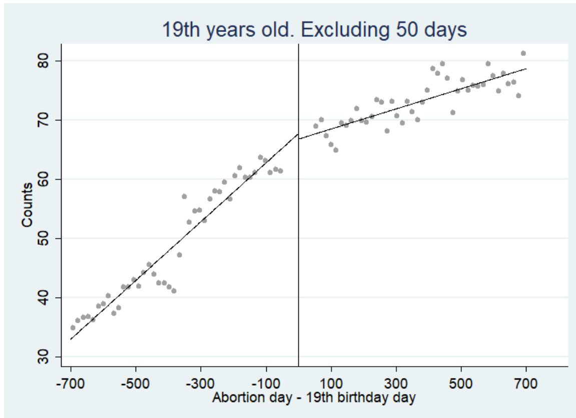


(b) Abortions around 16th birthday. After the reform

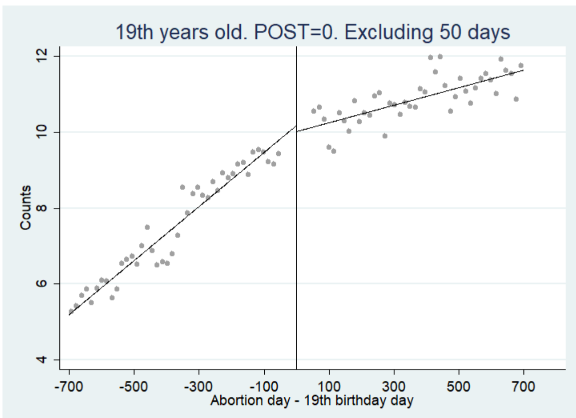
Figure A.2.5: Bunching Analysis. Number of abortions around 16th birthday. Before and After the Reform.

Notch is at $\Delta 18 = 0$. Binwidth 10 days, bandwith 200 days before and after turning 18, $\Delta 18_U = 14$ days, polynomial of order 7th, number of bootstraps =100. b is calculated as the ratio of excess mass relative to the counterfactual value at the notch point. Standard errors in parenthesis.

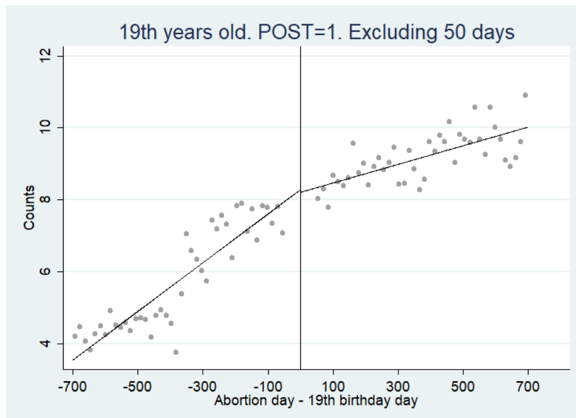
A.3 Permanent Effects. Placebo Test



(a) All



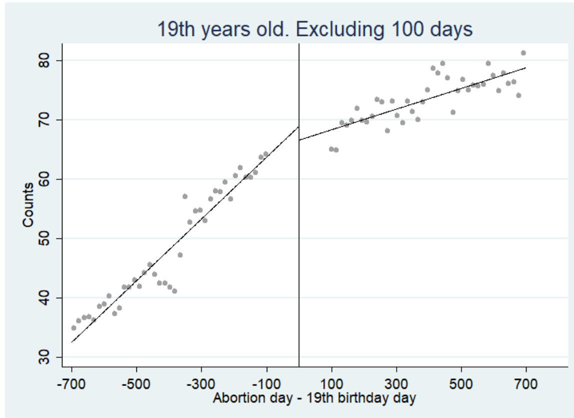
(b) Before



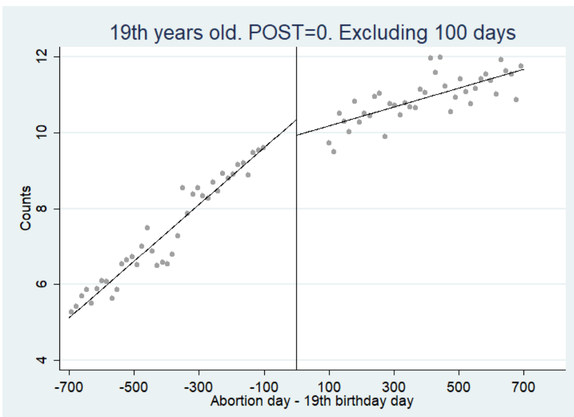
(c) After

Figure A.3.1: Permanent Effects. Placebo Test (abortions around 19th years old birthday).

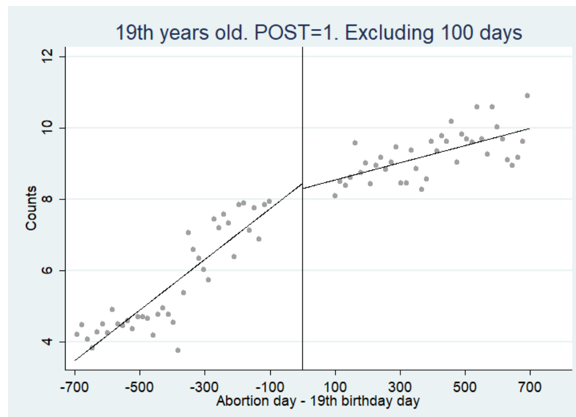
Excluding 50 observations before and after threshold. We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).



(a) All



(b) Before



(c) After

Figure A.3.2: Permanent Effects. Placebo Test (abortions around 19th years old birthday).

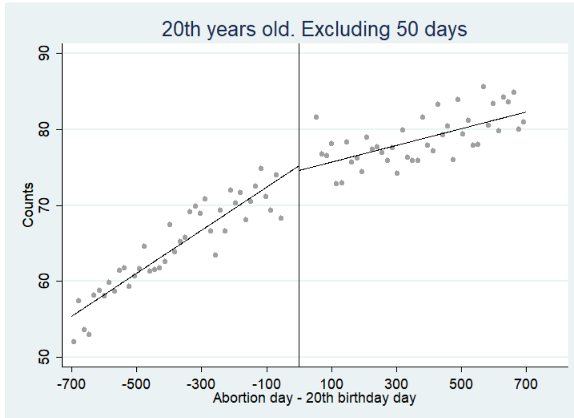
Excluding 100 observations before and after threshold. We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).

Table A.3.1: Effects on Log number of abortions. Placebo Test. Excluding different observations around the threshold (aborting around 19 years old birthday). P(1). Bandwidth 730 days

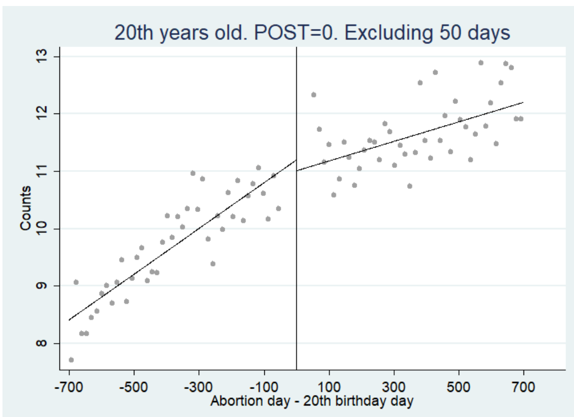
	Excl. 50	Excl. 75	Excl. 100
a19	-0.0531**	-0.0752***	-0.0865***
	(0.0195)	(0.0207)	(0.0224)
a19xPOST	0.0161	0.0369	0.0363
	(0.0366)	(0.0400)	(0.0433)
Observations	2724	2624	2524
Adjusted R-squared	.86	.86	.86

Standard errors in parentheses

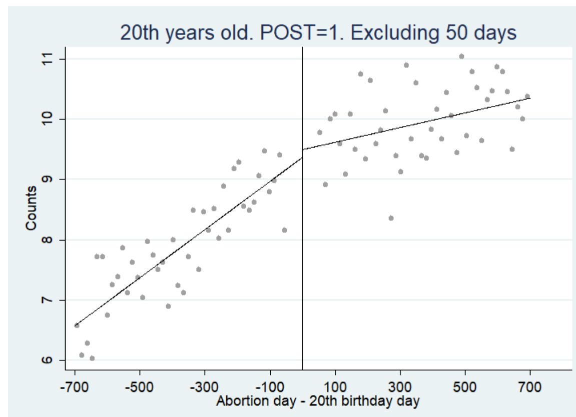
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$



(a) All



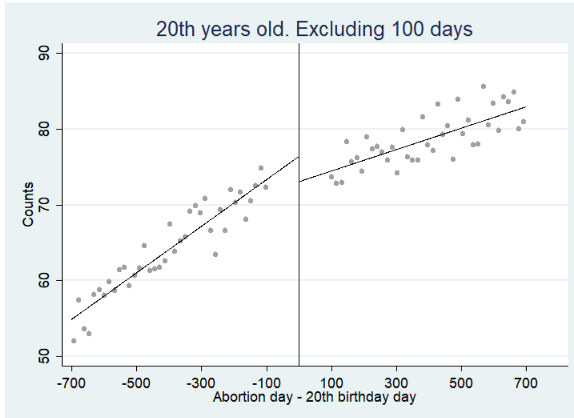
(b) Before



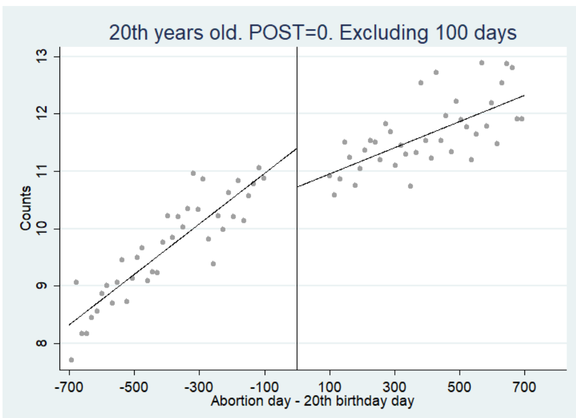
(c) After

Figure A.3.3: Permanent Effects. Placebo Test

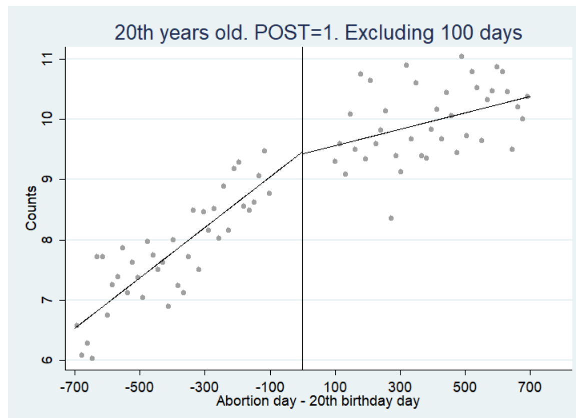
Excluding 50 observations before and after threshold (abortions around 20th years old birthday). We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).



(a) All



(b) Before



(c) After

Figure A.3.4: Permanent Effects. Placebo Test

Excluding 100 observations before and after threshold (abortions around 20th years old birthday). We normalize the number of abortions by the different period length to "per year" abortions. This takes into account that the post-reform period (POST=1) is shorter (Sept, 21st 2015- 2017) than the before-reform period (POST=0) (2011-Sept 20, 2015). We normalize the Y axis counts to counts per year (i.e. POST=0 counts at each bin multiplied by 365/1754 and POST=1 counts by 365/833).

Table A.3.2: Effects on Log number of abortions. Placebo Test. Excluding different observations around the threshold (aborting around 20 years old birthday). P(1). Bandwidth 730 days

	Excl. 50	Excl. 75	Excl. 100
a20	-0.0231	-0.0436**	-0.0651**
	(0.0179)	(0.0185)	(0.0202)
a20xPOST	0.0221	0.0365	0.0461
	(0.0326)	(0.0350)	(0.0386)
Observations	2724	2624	2524
Adjusted R-squared	.86	.86	.86

Standard errors in parentheses

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

A.4 Difference-in-Difference Fertility Effects

A.4.1 14 weeks of pregnancy during 2012-2015

Table A.4.1: Effects on fertility for those women who were pregnant when the policy came into effect. 14 weeks of pregnancy during 2012-2015

	All	Spanish	Non-Spanish
POST	-0.0186	-0.0579**	0.0869**
	(0.0202)	(0.0264)	(0.0304)
Year FE	X	X	X
Month FE	X	X	X
Prov FE	X	X	X
Y mean	.39	.4	.38
Observations	15236	9843	3935
Adjusted R-squared	.0026	.0034	.0037

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4.2: Effects on fertility for those women who were pregnant when the policy came into effect. 14 weeks of pregnancy during 2012-2015. By education level

	All		Spanish		Non-Spanish	
	No-ESO	ESO	No-ESO	ESO	No-ESO	ESO
POST	-0.0332	0.0132	-0.0386	-0.0636	-0.0190	0.188**
	(0.0329)	(0.0462)	(0.0420)	(0.0575)	(0.0613)	(0.0892)
Year FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Prov FE	X	X	X	X	X	X
Y mean	.42	.34	.43	.34	.41	.32
Observations	7109	3405	5074	2057	1574	896
Adjusted R-squared	.001	.0004	.0001	.0085	-.002	.0203

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

A.4.2 Defining age 17 at 22 weeks of pregnancy (instead of 14). 2012-2017 omitting Sept 21-Dec 2015

Table A.4.3: Effects on fertility for those women who were not pregnant when the policy came into effect. 22 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015

	All	Spanish	Non-Spanish
POST	0.00120	0.00845	-0.0101
	(0.0128)	(0.0154)	(0.0336)
Year FE	X	X	X
Month FE	X	X	X
Prov FE	X	X	X
Y mean	.39	.4	.37
Observations	18587	12080	4725
Adjusted R-squared	.0014	.0022	.0013

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4.4: Effects on fertility for those women who were not pregnant when the policy came into effect. 22 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015. By whether the mother's profession (incuding being a student) is missing or not

	All		Spanish		Non-Spanish	
	NotMiss	Missing	NotMiss	Miss	NotMiss	Miss
POST	0.0492**	-0.00648	0.0697**	-0.0404	0.0182	0.0123
	(0.0171)	(0.0430)	(0.0220)	(0.0639)	(0.0414)	(0.0593)
Year FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Prov FE	X	X	X	X	X	X
Y mean	.4	.33	.4	.35	.38	.32
Observations	15664	2923	10224	1856	3938	787
Adjusted R-squared	.0037	.0014	.0052	.0098	.0033	-.0104

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4.5: Effects on fertility for those women who were not pregnant when the policy came into effect. 22 weeks of pregnancy during 2012-2017 omitting Sept 21-Dec 2015. By whether the mother is a student or not

	All		Spanish		Non-Spanish	
	Student	No-Student	Student	No-Student	Student	No-Student
POST	0.0882**	0.0330*	0.136**	0.0434**	0.0475	0.0270
	(0.0342)	(0.0177)	(0.0502)	(0.0200)	(0.0625)	(0.0428)
Year FE	X	X	X	X	X	X
Month FE	X	X	X	X	X	X
Prov FE	X	X	X	X	X	X
Y mean	.46	.37	.45	.39	.48	.34
Observations	4448	11168	2534	7651	1088	2842
Adjusted R-squared	.0061	.005	.0105	.0055	.0123	.0135

Standard errors in parentheses clustered at the province level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix B: Chapter 2 Supplementary Materials

B.1 Ratio Chinese Births in Spain relative to All Births in Spain

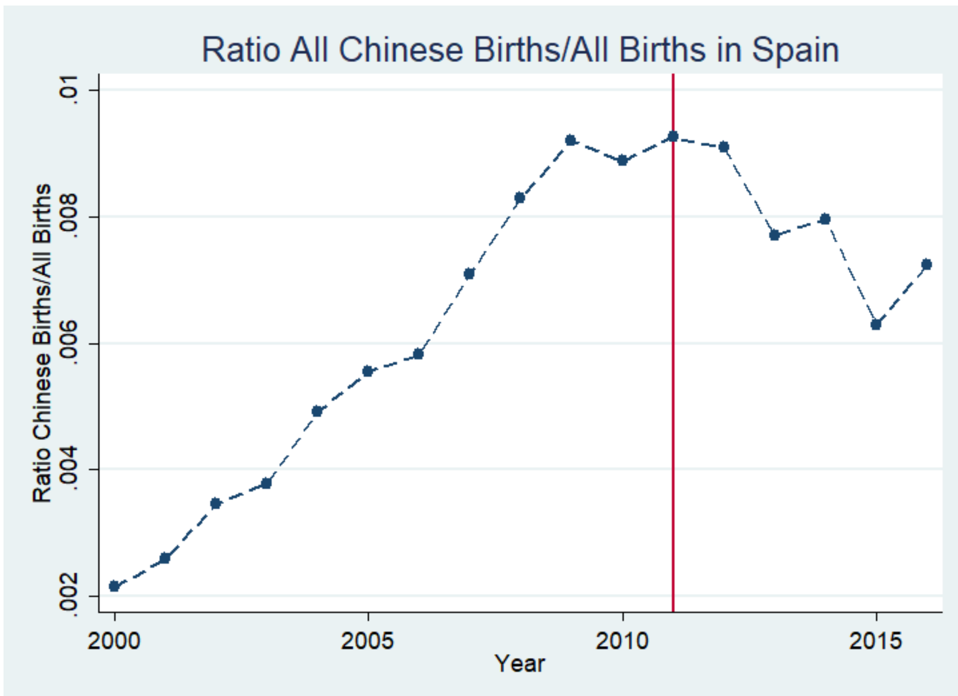


Figure B.1.1: Ratio All Chinese Births relative to All Births in Spain

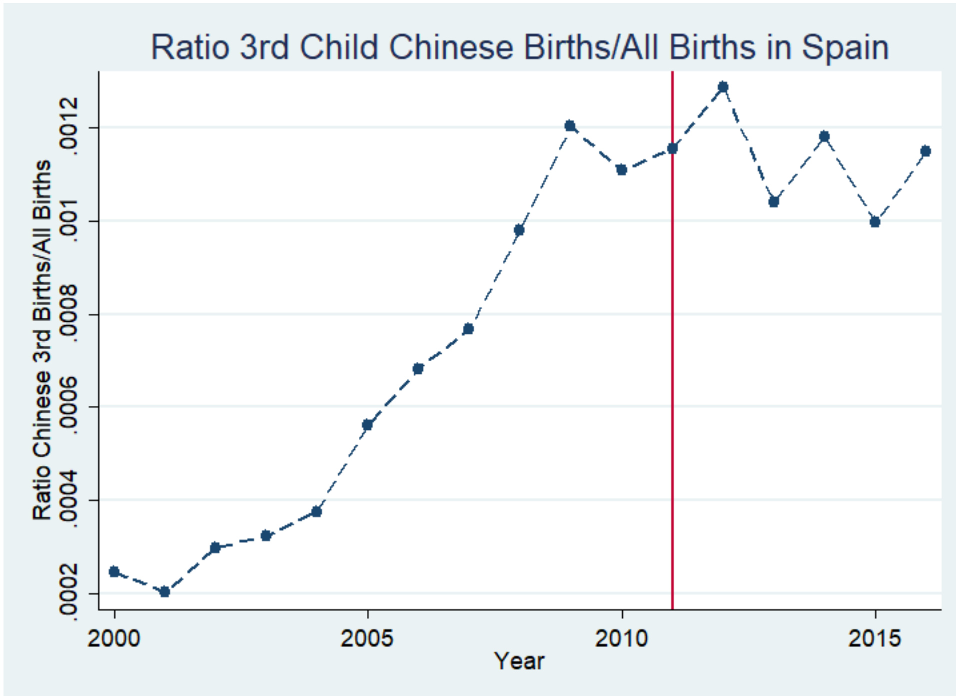
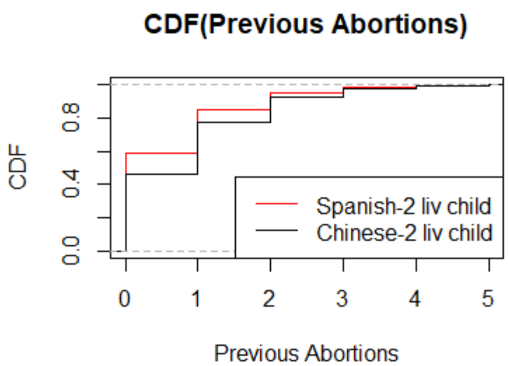


Figure B.1.2: Ratio 3rd Child or Above Chinese Births relative to All Births in Spain

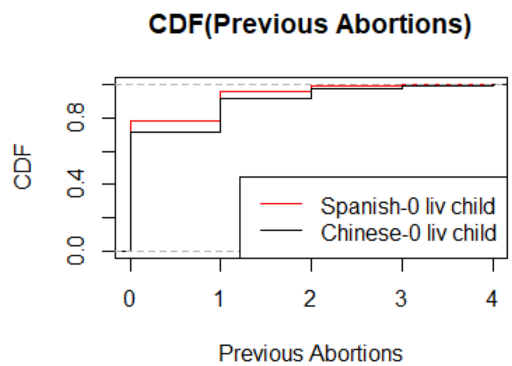
B.2 Post Reform Abortion Data Figures. 2011-2016

This section uses the rich individual-level data on abortions to provide some suggestive evidence on the post-reform behavior among Chinese women. Unfortunately, this data is only available in the post-reform period starting in 2011.

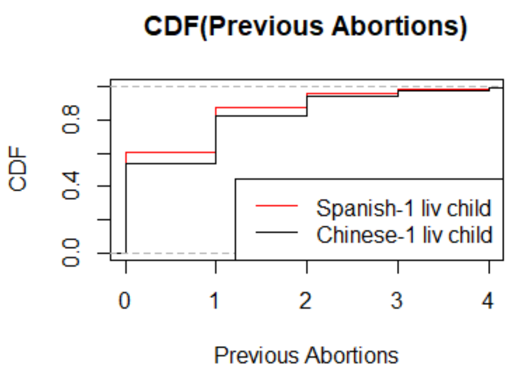
B.2.1 CDF Number of Previous Abortions by Nationality and Child Order



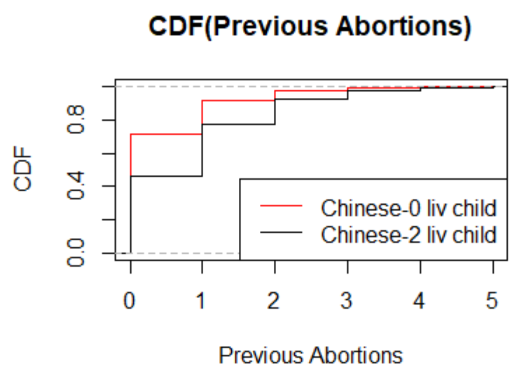
(a) 2 living children



(b) 0 living children



(c) 1 living child

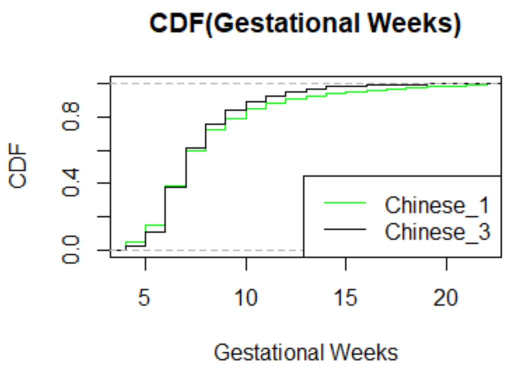


(d) Chinese 0 vs 2 living child

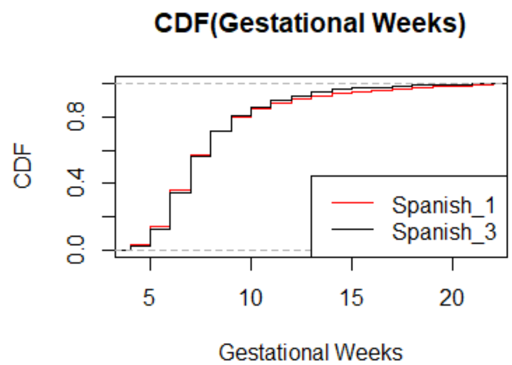
Figure B.2.1: CDF (Number Previous Abortions) by child order and nationality

Cumulative Distribution Function (CDF) of the number of previous abortions by nationality (Chinese versus Spanish) and child order (women with 0 living children, women with 1 living child, and women with 2 living children. Sub-figure B.2.1d compares Chinese women with 0 living children and Chinese women with 2 living children. In this sub-figure Spanish women are not included.

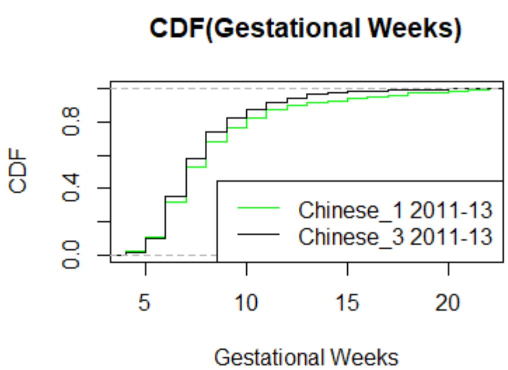
B.2.2 CDF Gestational Weeks at Abortion by Nationality and Child Order



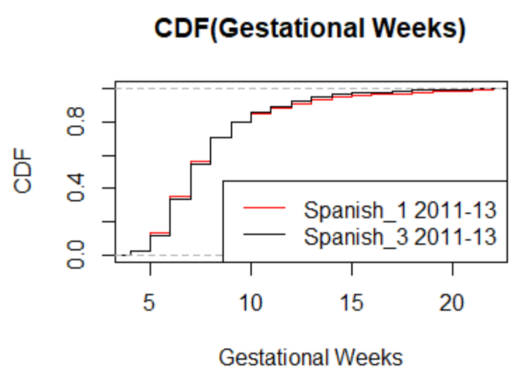
(a) Chinese 0 vs 2 living children 2011-2016



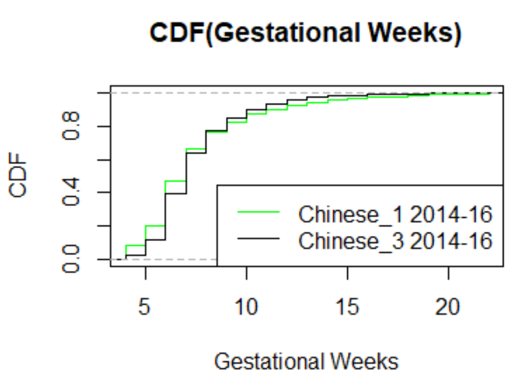
(b) Spanish 0 vs 2 living children 2011-2016



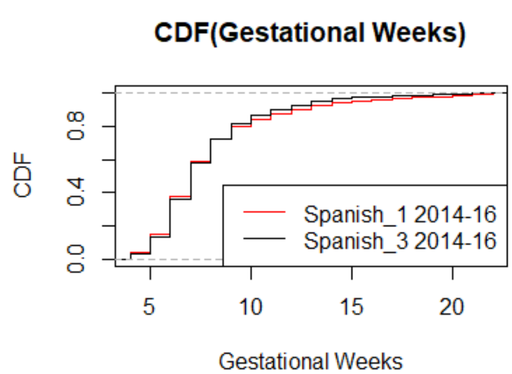
(c) Chinese 2011-2013



(d) Spanish 2011-2013



(e) Chinese 2014-2016



(f) Spanish 2014-2016

Figure B.2.2: CDF (Gestational Weeks) by nationality and period. 0 living children vs 2 living children

Cumulative Distribution Function (CDF) of the gestational weeks at abortion by nationality and period (2011-2013 and 2014-2016). Women with 0 living children versus 2 living children within nationality in all sub-figures.

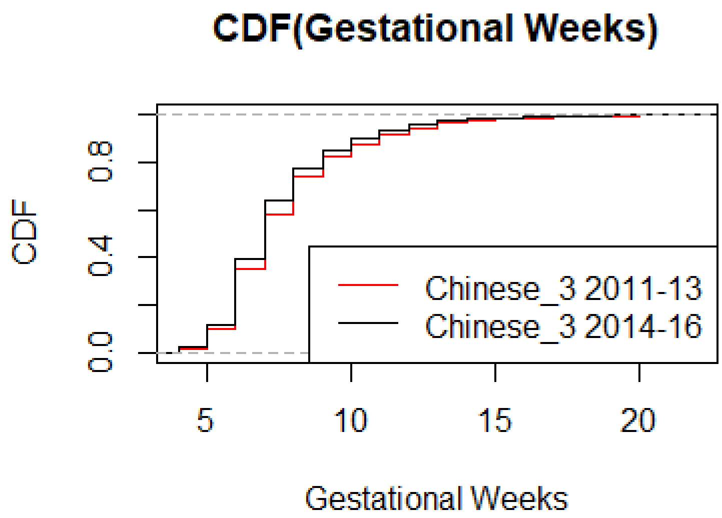


Figure B.2.3: CDF(Gestational Weeks) Chinese Women with 2 living children. 2011-2013 versus 2014-2016

B.3 Effects on Chinese Parents Fertility

Tables B.3.1 and B.3.2 show the effect of the reform on the fertility of Chinese mothers. Table B.3.1 looks at those giving birth to their third child and above (relative o those giving birth to first and second child). Table B.3.2 looks at mothers who are 35 years old or older (relative to younger ones). For both groups, giving birth is more costly and after the reform they had the certainty of giving birth to a boy since otherwise they would abort. In particular, we estimate the following regression:

$$y_{gmt} = \alpha + \beta_1 third\_child_{gmt} + \beta_2 third\_child_{gmt} \times POST_{mt} + \beta_3 POST_{mt} + \mu_t + \eta_m + \epsilon_{mt}$$

where y_{gmt} are the number of births from Chinese parents in group g (third child or above versus first and second, 35 years and older versus 34 years and younger), month m and year t. $third\_child$ is an indicator equal to one for the third child or above counts and equal to zero for the counts of the first or second children. $POST_{mt}$ is a dummy equal to one if the period is 9 (and 6) months after the abortion reform and zero otherwise. We do a similar analysis using the birth counts of Chinese women who are 35 or older versus younger.

Table B.3.1: Effects on the number of births being the third child or above (relative to being 1st or 2nd). Defining the $POST$ variable starting 6 and 9 months after the policy change. Only children from Chinese father and mother included. 2007-2016

	6 months after	9 months after
third_child	-259.0***	-258.9***
	(4.165)	(4.015)
third_childxPOST	55.96***	57.39***
	(6.024)	(5.961)
POST	-49.69***	-22.01**
	(8.409)	(11.008)
Birth Year FE	X	X
Birth Month FE	X	X
Y mean	153.2	153.2
Observations	240	240
Adjusted R-squared	.9594	.9603

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table B.3.2: Effects on the number of births being from 35 years old or older relative to younger mothers. Defining the $POST$ variable starting 6 and 9 months after the policy change. Only children from Chinese father and mother included. 2007-2016

	6 months after	9 months after
age35	-258.3***	-257.9***
	(4.332)	(4.193)
age35xPOST	52.32***	53.07***
	(5.961)	(5.907)
POST	-47.87***	-19.85*
	(8.425)	(11.158)
Birth Year FE	X	X
Birth Month FE	X	X
Y mean	153.2	153.2
Observations	240	240
Adjusted R-squared	.9621	.9625

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

B.4 Parents with Chinese Nationality relative to Spanish nationality. 2000-2016

Table B.4.1: Difference-in-Difference. Father (and mother) with Chinese nationality (relative to Father and mother with Spanish nationality). By mother's child order. 2000-2016

	1st child	2nd child	3rd child
chinese	0.00300	0.000799	0.0125
	(0.00304)	(0.00367)	(0.00775)
POSTxchinese	-0.000345	0.00280	0.0286**
	(0.00641)	(0.00521)	(0.0131)
POST	-0.00267	-0.00175	-0.00499
	(0.00195)	(0.00185)	(0.00367)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Province FE	X	X	X
Y mean	.52	.52	.52
Observations	3204157	2180956	499168
Adjusted R-squared	0	0	0

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

B.5 Only Parents with Chinese Nationality. Third Child vs First (and Second)

B.5.1 Effects on the Fraction of Boys

We estimate the following regression:

$$y_{imt} = \alpha + \beta_1 third_{imt} + \beta_2 POST_{mt} \times third_{imt} + POST_{mt} + \mu_t + \eta_m + \epsilon_{imt}$$

were y_{imt} is the gender of child i, born in month m, and year t. $third$ is a dummy equal to one if the child is the third child or above and equal to zero if she is the first or the second (or only the first in table B.5.2). We estimate this regression only in children from Chinese parents (both father and mother) and the coefficient of interest is β_2.

Table B.5.1: Effects on the Fraction of Boys. Diff-in-Diff. Only Chinese father and mother sample (i.e. no Spanish included in the regression). 3rd child vs 1st and 2nd child

	(1)	(2)	(3)
third	0.0127	0.00219	0.00345
	(0.0118)	(0.0104)	(0.0107)
POSTxthird	0.0256	0.0321*	0.0327*
	(0.0172)	(0.0165)	(0.0167)
POST	0.0136	0.0228	0.0215
	(0.0125)	(0.0200)	(0.0205)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	36342	26144	26144
Unadjusted R-squared	.001	.002	.0044
Adjusted R-squared	.0004	.0006	.0011

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table B.5.2: Effects on the Fraction of Boys. Diff-in-Diff. Only Chinese father and mother sample (i.e. no Spanish included in the regression). 3rd child vs 1st child (2nd children are excluded)

	(1)	(2)	(3)
third	0.0150	0.00935	0.0103
	(0.0119)	(0.0120)	(0.0122)
POSTxthird	0.0241	0.0304*	0.0308*
	(0.0176)	(0.0174)	(0.0176)
POST	0.0299	0.0495**	0.0483**
	(0.0198)	(0.0187)	(0.0191)
Birth Year FE	X	X	X
Birth Month FE	X	X	X
Controls		X	X
Province FE			X
Y mean	.52	.52	.52
Observations	22346	15177	15177
Unadjusted R-squared	.0018	.0035	.007
Adjusted R-squared	.0008	.0011	.0012

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

B.5.2 Effects on Birth Outcomes

Table B.5.3: Effects on Chinese Girls' Birth Outcomes. Diff-in-Diff. Only Chinese father and mother sample (i.e. no Spanish included in the regression). 3rd child vs 1st and 2nd child

	B.Weight	GestWeeks	Less2500g	Less1500g	Premature	Livemore24h
third	79.13***	-0.00170	-0.0163**	-0.00210	-0.0110	0.000430
	(21.53)	(0.0746)	(0.00594)	(0.00182)	(0.00830)	(0.000475)
girl	-101.2***	0.0801**	0.00789*	0.00223*	-0.000633	0.000594
	(11.58)	(0.0348)	(0.00435)	(0.00121)	(0.00377)	(0.000462)
girlxthird	-29.39	-0.148	0.00974	0.000932	0.00692	-0.000517
	(23.41)	(0.0913)	(0.00937)	(0.00287)	(0.0110)	(0.000453)
POST	-13.30	0.146**	-0.00775	0.00235	-0.0374***	0.000219
	(17.13)	(0.0589)	(0.00591)	(0.00178)	(0.00808)	(0.000746)
POSTxthird	-40.73*	-0.275**	0.0137*	0.000884	0.0146	-0.000864
	(24.17)	(0.0965)	(0.00745)	(0.00234)	(0.0101)	(0.00146)
POSTxgirl	2.949	-0.00461	-0.00319	-0.00177	-0.000526	-0.00130**
	(9.579)	(0.0411)	(0.00507)	(0.00122)	(0.00393)	(0.000597)
POSTxgirlxthird	15.99	0.299**	-0.0121	0.00387	-0.0196	0.00177
	(28.79)	(0.135)	(0.0122)	(0.00426)	(0.0130)	(0.00151)
Birth Year FE	X	X	X	X	X	X
Birth Month FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	3351.2588	39.0756	.0372	.004	.0502	.999
Observations	24161	16884	24161	24161	26144	26144
Unadjusted R-squared	.0218	.0143	.0061	.0035	.0061	.0039
Adjusted R-squared	.0181	.0089	.0023	-.0002	.0027	.0004

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table B.5.4: Effects on Chinese Girls' Birth Outcomes. Diff-in-Diff. Only Chinese father and mother sample (i.e. no Spanish included in the regression). 3rd child vs 1st child (2nd children are excluded)

	B.Weight	GestWeeks	Less2500g	Less1500g	Premature	Livemore24h
third	101.1***	-0.0702	-0.0186**	0.000345	-0.00456	0.0000768
	(22.18)	(0.0675)	(0.00671)	(0.00192)	(0.00748)	(0.000543)
girl	-93.57***	0.107**	0.00775	0.000308	0.00368	0.000365
	(13.19)	(0.0326)	(0.00655)	(0.000776)	(0.00542)	(0.000391)
girlxthird	-37.28	-0.177*	0.00960	0.00276	0.00254	-0.000288
	(26.14)	(0.103)	(0.0111)	(0.00282)	(0.0117)	(0.000363)
POST	-50.37**	0.140**	-0.00338	0.00315*	-0.0310**	0.000411
	(20.35)	(0.0613)	(0.00753)	(0.00174)	(0.0122)	(0.000644)
POSTxthird	-27.60	-0.251**	0.0141*	-0.000468	0.00939	-0.000664
	(22.43)	(0.0987)	(0.00793)	(0.00259)	(0.0103)	(0.00153)
POSTxgirl	0.721	-0.0699	-0.00284	-0.000189	0.0000651	-0.00112
	(13.76)	(0.0440)	(0.00867)	(0.00139)	(0.00660)	(0.000978)
POSTxgirlxthird	20.39	0.359**	-0.0122	0.00244	-0.0198	0.00153
	(30.33)	(0.129)	(0.0152)	(0.00437)	(0.0134)	(0.00172)
Birth Year FE	X	X	X	X	X	X
Birth Month FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Province FE	X	X	X	X	X	X
Y mean	3335.1727	39.1095	.0383	.0034	.0499	.999
Observations	14030	9922	14030	14030	15177	15177
Unadjusted R-squared	.0269	.0187	.0077	.0059	.0068	.0055
Adjusted R-squared	.0205	.0096	.0012	-.0006	.0008	-.0005

Standard errors in parentheses clustered at the province level

$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix C: Chapter 3 Supplementary Materials

C.1 Birth Counts

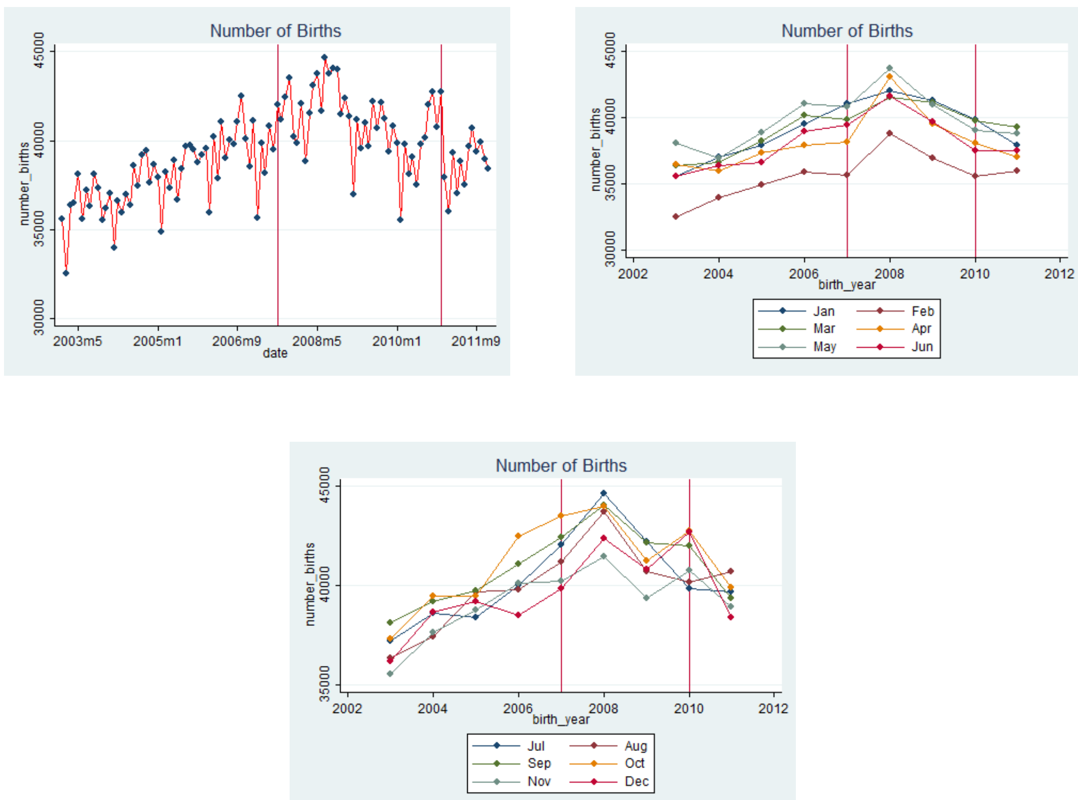


Figure C.1.1: Number of Births

The top-left graph shows the total number of births that took place in spain in each month-year starting in 2003 and until 2011. For the other two graphs, each line corresponds to the number of births that took place in Spain in a particular month in different years (from 2003 until 2011). The first vertical line corresponds to the year that the policy was announced (2007) while the second vertical line corresponds to the end of the policy year (2010). The policy was announced in July and it ended in December.

C.2 Raw Data

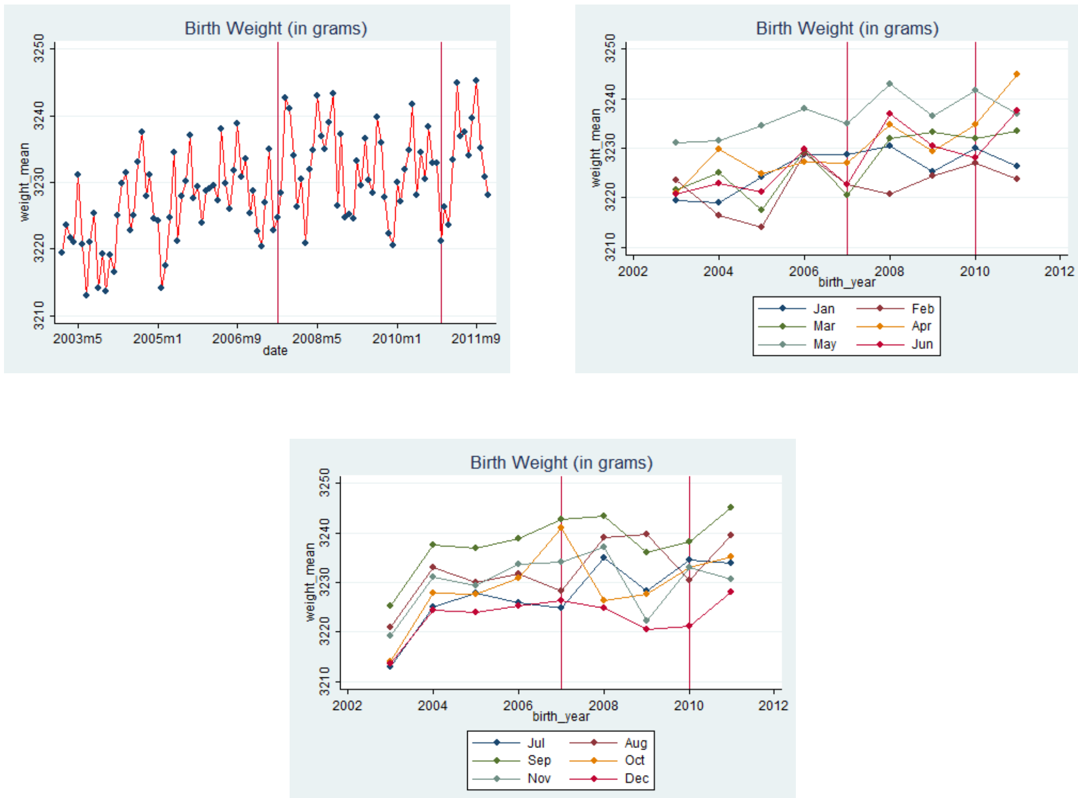


Figure C.2.1: Birth Weight

The top-left graph shows the average birth weight (in grams) for each month-year starting in 2003 and until 2011. For the other two graphs, each line corresponds to the average birth weight (in grams) of babies born in Spain in a particular month in different years (from 2003 until 2011). The first vertical line corresponds to the year that the policy was announced (2007) while the second vertical line corresponds to the end of the policy year (2010). The policy was announced in July and it ended in December.

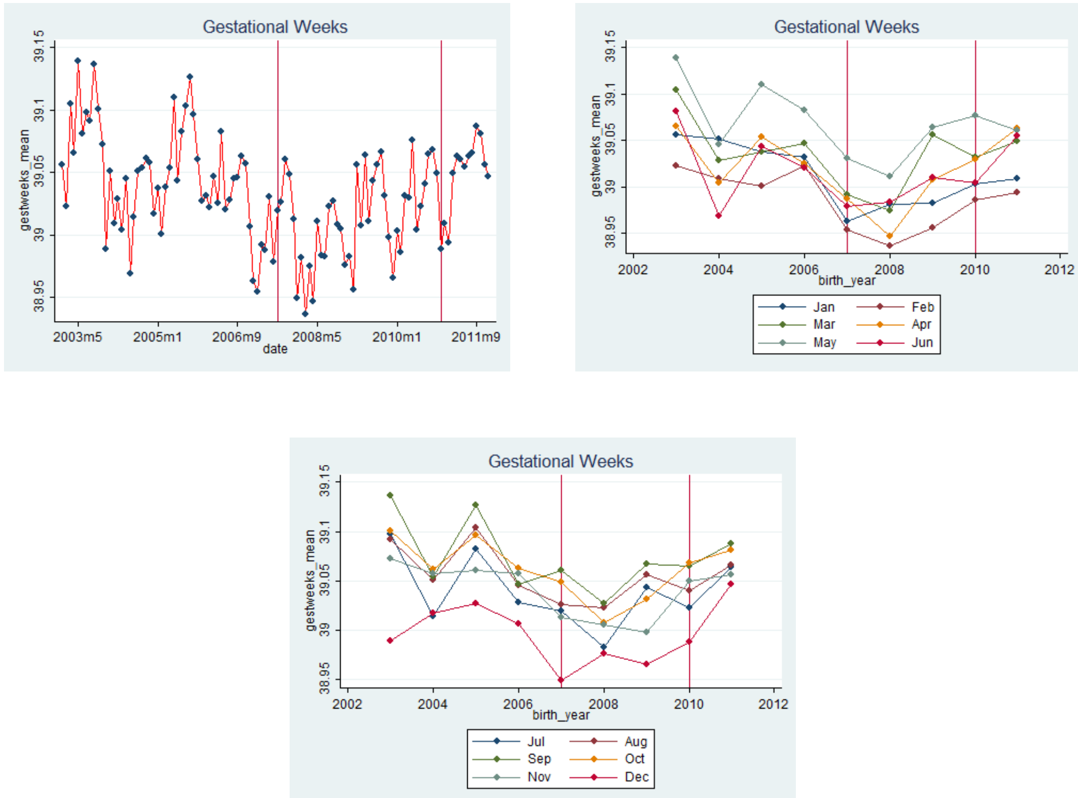


Figure C.2.2: Gestational Weeks

The top-left graph shows the average gestational weeks for each month-year starting in 2003 and until 2011. For the other two graphs, each line corresponds to the average gestational weeks of babies born in Spain in a particular month in different years (from 2003 until 2011). The first vertical line corresponds to the year that the policy was announced (2007) while the second vertical line corresponds to the end of the policy year (2010). The policy was announced in July and it ended in December.

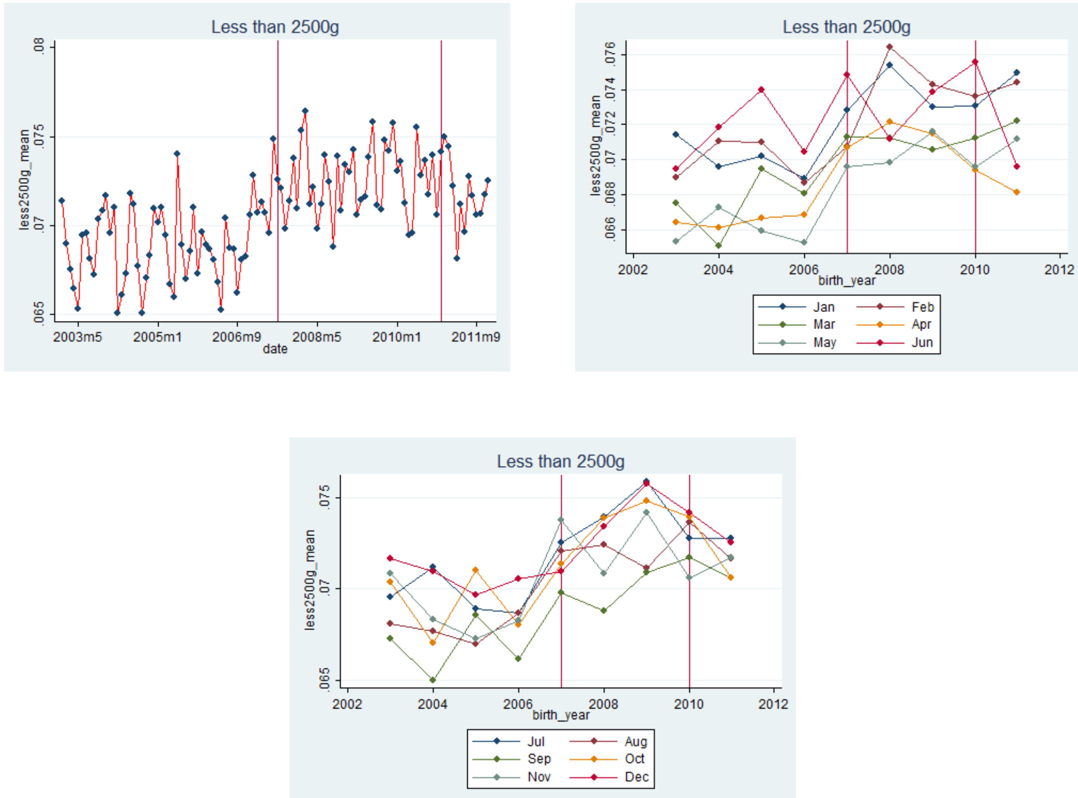


Figure C.2.3: Fraction born with less than 2500g

The top-left graph shows the fraction of children who were born in Spain with less than 2500 grams for each month-year starting in 2003 and until 2011. For the other two graphs, each line corresponds to the fraction of children who were born with in Spain with less than 2500 grams in a particular month in different years (from 2003 until 2011). The first vertical line corresponds to the year that the policy was announced (2007) while the second vertical line corresponds to the end of the policy year (2010). The policy was announced in July and it ended in December.

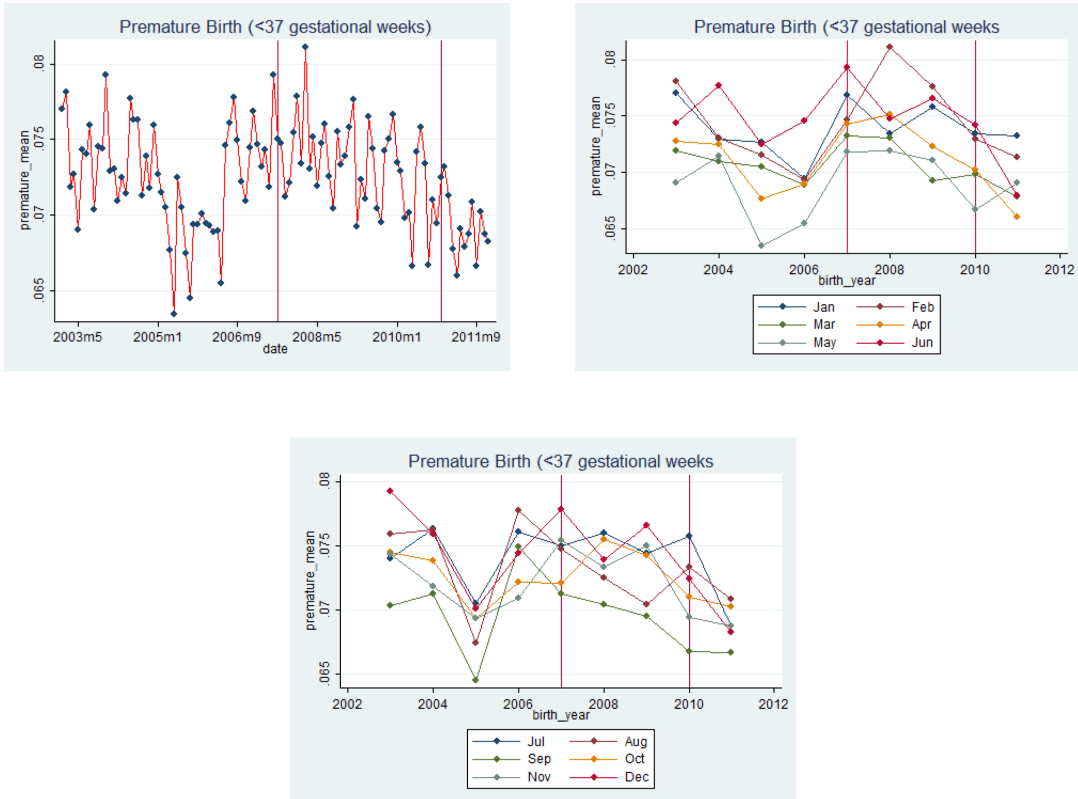


Figure C.2.4: Fraction Born Premature

The top-left graph shows the fraction of children who were born being premature (defined as pregnancies of less than 37 weeks) in Spain for each month-year starting in 2003 and until 2011. For the other two graphs, each line corresponds to the fraction of children who were born premature in Spain in a particular month in different years (from 2003 until 2011). The first vertical line corresponds to the year that the policy was announced (2007) while the second vertical line corresponds to the end of the policy year (2010). The policy was announced in July and it ended in December.

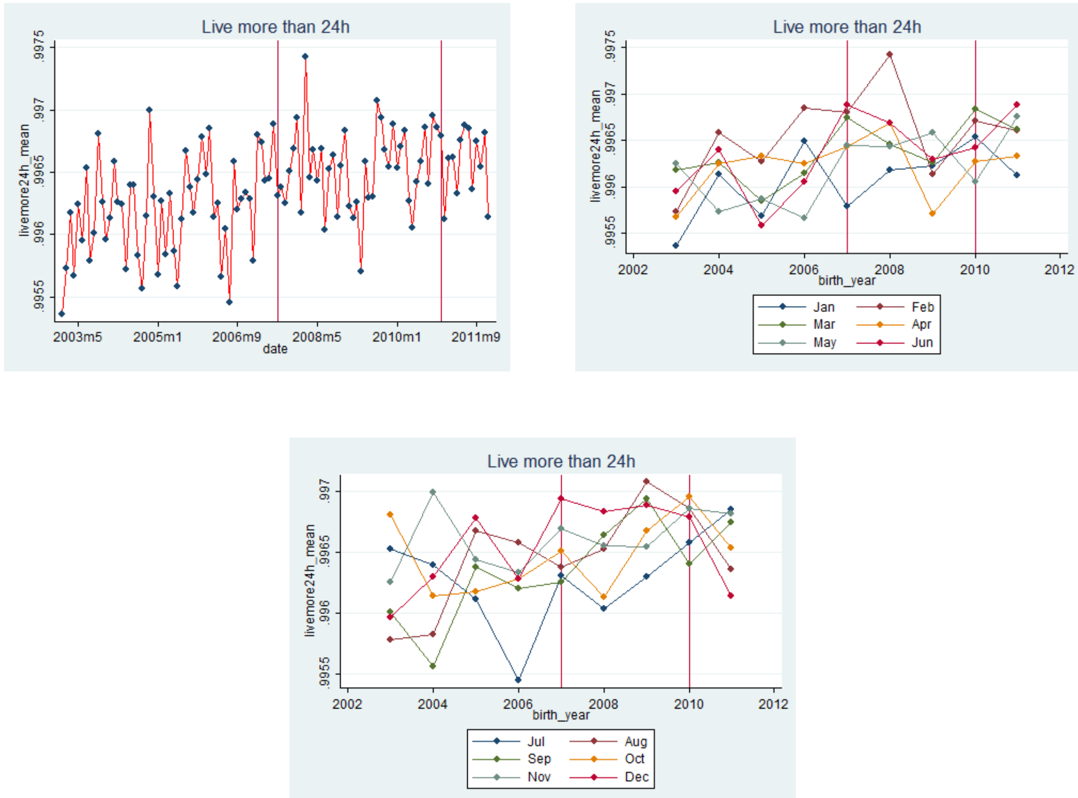


Figure C.2.5: Fraction who lived more than 24h

The top-left graph shows the fraction of children who lived more than 24 hours in Spain for each month-year starting in 2003 and until 2011. For the other two graphs, each line corresponds to the fraction of children who lived more than 24 hours in Spain in a particular month in different years (from 2003 until 2011). The first vertical line corresponds to the year that the policy was announced (2007) while the second vertical line corresponds to the end of the policy year (2010). The policy was announced in July and it ended in December.