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Abstract

This dissertation studies important social topics in South Korea. The first two chapters evaluate the effects of a pronatalist policy introduced by local governments in South Korea, a country with one of the lowest fertility rates worldwide. In the first chapter, possible selection into marriage is highlighted considering the low out-of-wedlock births in the country. Also, the policy effects are examined in two different dimensions: changes in the probability of having a child and the time-to-child delivery. The second chapter estimates two important key variables to address the effectiveness of this program. First is the unconditional distribution of the reservation price of fertility, which is the minimal compensation an agent must receive to induce her to have a child. Second is the infra-marginal ratio of spending, which shows the fraction of the program's budget spent on infra-marginal births. The last chapter investigates the effect of North Korea's repeated military provocations on the housing market in South Korea, focusing on the two nuclear weapon tests in 2009 and 2013.

Essays on Population and Housing Market in South Korea

by

Dahae Choo

B.A, Korea University, 2013
M.A, Korea University, 2016

Dissertation
Submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in Economics

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to my everything and more. Jungin Lee and Kyoungho Choo.

to my life-long friend and her family. Sarang Choo, Jonghoon Choi, and Siho Choi

Contents

List of Figures

List of Tables

Chapter 1

More Money, More Marriages, and More Babies? Evaluating Policy Impacts on Marriage and Fertility in South Korea

1.1 Introduction

The below-replacement fertility rate can be a threat to national security. This means the country will not sustain itself population-wide, not in the distant future. The severity of low fertility rates has been salient in many countries, so that governments in those countries implemented various pro-natalist policies to combat this issue. The decreasing rate of births in South Korea is shocking relative to those in the rest of the world; the total fertility rate reached 0.98 in 2018 (Statistics Korea[1]) as illustrated in Figure 1.1[2]. Local governments in South Korea initiated various pro-natalist policies to reverse the trend, including a cash transfer program called "baby bonus".[3] This policy has been under increasing scrutiny due to the seemingly irreversible low fertility rate, despite the growth in the resources allocated to the program. The aggregate budget for the program has increased 63.6% between 2016 and 2018.[4]

The goal of this paper is to evaluate the effect of the baby bonus program on the following fertility outcomes: the fertility decision (i.e., whether to have a child) and the time to have a child after marriage. Given social norms still dictate marriage as a pre-pathway for a child (Lee and Bauer 2013; Anderson and Kohler 2013),[5] I use the local government's marriage promotion budgets as a source of exogenous variation to explore potential selection issues related to marriage. Thus, my analysis accounts for both the marriage and the

[1] Statistics Korea is a central government organization for statistics which is part of Ministry of Strategy and Finance.

[2] The total fertility rate of South Korea has been continuously lower than that of other countries, as illustrated in Figure 1.1.

[3] The literal translation of the name is "childbirth celebration subsidy" or "childbirth encouragement subsidy", but I will call it coherently as baby bonus hereafter.

[4] The allocated budget for the baby bonus was about 230 billion KRW (roughly 230 million USD) in 2016, and it increased to 350 billion KRW (roughly 350 million USD) in 2018, according to the Annual Casebook of Population Policy of Local Governments.

[5] In addition, the out-of-wedlock birthrate in South Korea was just 1.9% in 2016 according to the OECD database.

childbearing decisions. In fact, the total fertility rate (TFR) and the marriage rate share very similar trends in South Korea. The TFR of the country dropped from 4.53% to 1.17% between 1970 and 2016, while the marriage rate showed a similar declining trend from 9.2% to 5.5% over the same period.[6]

This paper has three main contributions. First, within the context of the evaluation of pro-natalist policies, I am able to exploit the rich variation in local governments' transfer amounts, which allows me also to estimate the marginal effects of the policy at various transfer levels. Throughout the world, most pro-natalist policies are introduced and operated at a national—or state—level. Thus, the existing evaluation studies can only exploit variation for identification at that level and estimate the effect of the policies at limited transfer amounts. Examples include González (2013) using Spanish data, and Drago et al. (2011) and Sinclair et al. (2012) using Australian data. In contrast, local governments in South Korea are the autonomous entities operating the baby bonus programs, so the amounts of the financial subsidy vary significantly over locations. The estimation of marginal effects at different levels of the financial subsidy allows for a better understanding of the effectiveness of the baby bonus program, especially if the outcomes of interest do not change linearly with the level of the financial incentive. Furthermore, I am better able to find the level of transfer at which the policy becomes effective. By including all local governments ("districts") in South Korea, the analysis is free from the potential misrepresentation problem of true effects when including only certain regions in the analysis (e.g. Malak et al. 2019; Parent and Wang 2007; Yonzan et al. 2020; Joyce et al. 2004).

As the second contribution, this paper considers marriage as an intermediate decision for childbirth (selection into marriage). This is based on the fact that births outside

[6]The OECD family database.

of marriage rarely happen in South Korea. Most of the existing studies, which analyze developed countries, assess the effect of pro-natalist policies on the decision of unmarried women to start a family within a legal partnership (Francesconi and Van der Klaauw 2007), or focus on the sub-sample of married women (Cohen et al. 2013; Hong and Sullivan 2016). However, these contexts do not appear appropriate to explain the situation of countries where the marriage decision almost always precedes child delivery, like many Asian countries. Given that the baby bonus is unlikely to incentivize unmarried women to have babies, I drop the unmarried in analyzing the effect of the baby bonus program. However, estimating the effect of the baby bonus policy using only married women could yield biased estimates if the demand for children among married and unmarried women differ in a way such that this leads them to enter or not enter into matrimony in the first place. To account for this possibility, I employ a panel sample selection model, where the level of the marriage promotion budget is used as an exclusion restriction. Also, I delve into the policy impacts by the decomposition of the effects: direct and indirect effects of the baby bonus program. This helps us to understand the channel that the policy affects more substantially on childbirth–either via increasing marriage likelihood or via increasing childbirth directly. To the best of my knowledge, this is the first paper that considers the marriage decision as a source of selection bias, and, in turn, studies the pro-natalist policy and the marriage promotion policy together in one context.

The third contribution of this paper is to highlight two important aspects of the fertility decision: whether the baby bonus program ultimately leads females to have more children (i.e., "the quantum effect"), and the decision to have a child earlier than they would have otherwise (i.e., "the tempo effect"). While it is more common in the literature to focus on policy effects on the probability of having a child (Malak et al. 2019; Cohen et al. 2013), a mother's age at the first childbirth is another factor that needs to be addressed in the childbirth context. A rise in age at first delivery has been observed widely (Lien and Wang 2016), and its effect on the ultimate family size is negative (Gustafsson 2001) or negligible at best (Skirbekk et al. 2004). Aside from the benefits of inducing individuals to have children at younger ages, such as better overall health for newborn infants, it also increases the likelihood of them having more children in their life-cycle if their happiness of having a child is greater than their initial expectation (Björklund 2006). Thus, examining these two effects separately—the quantum (changes in birth probabilities) and the tempo effects (changes in time to child delivery)—allows me to give a more comprehensive understanding of the efficacy of the baby bonus program. To analyze the effects of the baby bonus program on fertility behavior, I use the following two econometric models. First, for estimating the quantum effects, I employ the panel correlated random effect models with sample selection proposed by Semykina and Wooldridge (2018). Second, I employ the Cox (1972) proportional hazard model and the frailty survival model to estimate the tempo effect. The primary data set I use consists of the five available waves of the Korean Longitudinal Survey of Women and Families, which includes individual demographic characteristics as well as their family's. I combine this data set with policy history data collected from various government-provided sources.[7] In addition, since one of the goals of this paper is to examine the potential selection issue described before, I require at least one variable to be used as an exclusion restriction. To this end, I use the province-level budget specifically used for the encouragement of marriage, which is constructed based on data acquired from the Annual Issue of Fertility and Family Policies, published by the Ministry of Health and Welfare.

The estimation results indicate that the baby bonus program has no substantial impact

[7]Sources are Municipal Ordinance Data Center, Nation Legislation Data Center, and the Annual Issue of Fertility and Family Policies.

on changing the probability of having a child once I account for selection bias, but the program induces a reduction in time-to-first delivery. Tests for selection bias indicate that it is necessary to correct for women's selection into marriage. After correction, the baby bonus yields no significant changes in birth likelihood, which is in line with findings in the literature–the minimal impact of pro-natalist benefits (Hong and Sullivan 2016; Milovanska-Farrington 2019). The marginal effects of the baby bonuses evaluated at each transfer level are close to zero in magnitude, having a small or no increase in birth probability with an additional 1,000 USD transfer. It also suggests that no substantial changes in birth likelihood can be achieved with an extra transfer of up to 7,000 USD, which is at least 7 times higher than the current average baby bonus. The decomposition of the policy effects shows that no significant direct or indirect effect of the baby bonus program is detected in increasing birth likelihood. Nevertheless, the previous results do not imply that the baby bonus program is entirely worthless, as I find evidence of tempo effects. I find that a baby bonus transfer of 1,000 USD reduces the time-to-first delivery after marriage by approximately 8 months, whereas time-to-the second childbirth after the first childbirth is unaffected. This is consistent with females deciding their family plans at the time of marriage. The baby bonus only facilitates their family plans and expedite first delivery by allowing them to rely on government resources.

Meanwhile, it is women's marriage decisions that are more responsive to external governmental resources, showing both changes in marriage likelihood and reduction in time-to-marriage. The marriage promotion budget per 10,000 residents increases marriage rates by 2.48% (0.5 percentage points) at the average level. To obtain conservative estimates of the marriage promotion budget, potential premarital conceptions are excluded as they could cause upward bias. After excluding those females, the marriage promotion budget leads to a 0.5 percentage point increase in the likelihood of marriage at the average level.

Females under the 50th percentile of household income show slightly higher responsiveness to marriage benefits by 0.1 percentage point. In addition to changes in marriage likelihood, I find tempo effects of the marriage promotion budget. I estimate that a 1,000 USD increase in the marriage promotion budget in a province reduces time-to-marriage by 3 months after females reach 20 years of age. Lastly, MPB also shows statistically significant indirect effects on birth likelihood by increasing birth likelihood by 0.23 and 3.9 percentage points. Collectively, my findings suggest that marriage promoting programs helped females' marriage decisions while achieving a higher total fertility rate via these programs has not been found at the time-horizon analyzed here.

The remainder of this paper is organized as follows. It begins with a review of related literature in Section 1.2. Then it explains the details of the baby bonus program in South Korea in Section 1.3. Sections 1.4 and 1.5 describe the data and the econometric framework used in the paper, respectively. The estimation results for the marriage promotion budget and baby bonus program follow in Section 1.6. Section 1.7 provides additional exercises, exploring various cases to gauge the efficacy of the policy effects. Section 1.8 concludes.

1.2 Literature Review

Becker (1960) first sheds light on the fertility decision in the economic context; children are consumption goods for parents so that a couple may have a child given the potential pleasure of having one subject to their budget constraint. Becker's theoretical framework provides the rationale for the adoption and the operation of pro-natalist benefits in many countries, as the shrinkage of the workforce emerges as a major challenge (Lundberg and Pollak 2007; Bloom et al. 2010). To tackle this problem, many countries introduce financial benefits to newborn children that could modify parents' budget resources and thus create a price effect of a child. This additional transfer would incentivize couples to newly enter into parenthood by relaxing their budget constraints.

Studies have attempted to examine the efficacy of pro-natalist benefits, with the main focus on changes in birth probability. In many countries, the tax schedule provides benefits in favor of a larger size of family, but this implicit way to lower the relative price of a child does not seem to work nor to have any significant impact (for a review, see Moffitt (1998) and Gauthier (2007)) despite some counter-examples found in Zhang et al. (1994) and Malkova (2018). Baughman and Dickert-Conlin (2009) estimate the effect of the U.S. Earned Income Tax Credit using birth certificate data between 1990 and 1999, but the expansion of the EITC has only a weak impact on specific groups of females. Crump et al. (2011) revisit Whittington et al. (1990) that studied the effect of the income tax personal exemption on fertility outcomes. They extend Whittington et al.'s study, evaluating not only the personal exemption but also the EITC and the Child Tax Credit. They cast doubt on the original finding that the pro-natalist package had a significant impact on fertility decisions. Similar works by Francesconi and Van der Klaauw (2007) studying the Working Families' Tax Credit in the United Kingdom, and by Haan and Wrohlich (2011) and Riphahn and Wiynck (2017), assessing German tax system document no substantial fertility response caused by pro-natalist tax benefits in those countries.

The more direct and explicit way of incentivizing childbirth is the cash transfer program or child allowance program introduced in some countries. Such policies seemed to be more effective in inducing more females into maternity, though effects are small in magnitude. Hong and Sullivan (2016) use 2% of the Korean census data to estimate the extent of fertility response caused by the inception of the baby bonus program while including only central parts of the country and higher-order births. They find a small increase in the birth probability for a third child–a 0.108% increase with a 1,000 USD transfer or higher. Using the census data but with more spans, Kim (2020) conducts a district-level analysis of the program and finds a 3% reduction in birth decreasing rate due to the program. With respect to the Spanish universal child benefit program that gives a one-time payment of 2,500 Euro to newborn children, González (2013) finds a 6% increase in the annual number of births mainly through the reduction of abortion rates. Risse (2010), Sinclair et al. (2012), and Drago et al. (2011), each examine the effects of the Australian baby bonus program introduced in 2004. They find an increase in the intention to have a child, and the expense for having an additional child is estimated between A$43,000 and A$126,000. Malak et al. (2019) and Milligan (2005) examine the effect of the child allowance programs in Quebec, Canada. Each finds about 8.6% and 8.7% increase with C$8,000 allowance, respectively. On the other hand, Milovanska-Farrington (2019) documents that 1% increase in family allowance yields 0.01% higher chance to give birth in Switzerland.

Despite weak or inconclusive evidence of the quantum effect, the literature often finds that pro-natalist benefits help females have a child earlier in life (the tempo effect). The benefit of having at least the tempo effect should not be overlooked given the importance of the age of mothers at childbirth in population composition (Björklund 2006), and the potential momentum for the next birth (Lutz and Skirbekk 2005). The study conducted by Parent and Wang (2007) examines the Cash Allowance Program in Quebec, finding strong short-run responses by increasing the birth probability. However, this increase in probability did not yield a higher fertility rate later in life, showing only the tempo effect in the short-run but not the quantum effect in the long run. Several papers also confirm that birth spacing between children is reduced with the pro-natalist benefits such as the universal dividend in Alaska, USA (Yonzan et al. 2020), and the expansion of the maternity-leave program in Germany (Arntz et al. 2017) and in Austria (Lalive and Zweimüller 2009).

It is hard to think that attempts to lower the opportunity cost of childbearing and, ultimately, to increase fertility rates have been very successful to date. It might be in part because parents would spend their additional resources in increasing the quality of a child rather than demanding more children (Becker and Lewis 1973). Another possible explanation concerning the discrepancy between the prediction of the theory and the observed policy effect could be the insufficient financial subsidy to make people have a child due to a decrease in the relative price. To that end, the baby bonus program in South Korea offers an attractive opportunity to examine the amounts that induce noticeable changes in female childbearing behavior. Nearly 230 different local governments operate an autonomous cash transfer program separately and have their own scheduled financial subsidies. Thus, exploiting this regional variability would reveal the responsiveness of fertility behavior at different levels of the transfer.

Meanwhile, the literature places considerable emphasis on how extramarital births affect women's marital status. It has been observed that births outside of marriage continue to grow in many Western countries, as illustrated in Figure 1.2.[8] Thus, most policies in Western countries are designed to help single mothers' economic stability and independence as they are economically less advantaged than dual-income couples. Yet, this unintentionally penalizes married couples (Alm et al. 1999; Bitler and Zavodny 2000), so the interest of the literature often has been on how the policies make unwed mothers detached from the formation of a legal partnership (e.g. Moffitt 1990; Yelowitz 1998; Lundberg and Pollak 2007; González 2007; Francesconi and Van der Klaauw 2007).

Figure 1.2 also shows the fact that childbirth outside marriage is rare in some countries

[8]OECD database: Share of births outside of marriage.

like South Korea and Japan, even in recent years. The channel previously addressed in the literature might not be relevant for those countries since marriage is a sort of prerequisite for childbirth due to social norms. In Figure 1.3, the co-movement of marriage and birth rates is observed for selected countries, which highlights a different trend that emerges in South Korea and Japan. This is not necessarily true in the countries that the literature concentrated on, like the United States, the United Kingdom, and Australia, as their TFR is stable regardless of the decreasing marriage trend. The seemingly different patterns suggest that a different approach and treatment may be required for the analysis of fertility for countries with low out-of-wedlock birth rates.

To that end, I shed light on the role of family formation, in the context of childbirth, especially for countries like South Korea, Japan, and Singapore (Jones 2007). Although a few papers studying fertility outcomes in Israel (Cohen et al. 2013) and in South Korea (Hong and Sullivan 2016) use married women, this may not fully account for females' decision-making process, as females are likely to select into marriage. If some unobserved factors related to a woman's childbirth decision affected the same woman's marriage in the first place, this would yield systematic differences among women based on their marital status. Thus, I examine the possibility that females select into marriage, given the potential selection bias in women's childbirth decisions.

1.3 Institutional Backgrounds

1.3.1 Baby Bonus Program

Most municipal ordinances state that the purpose of the baby bonus program is to actively deal with objects of public concern regarding low fertility rate and population aging, to ensure the sustainability of the districts.[9] It is local governments that become the primary operator of the baby bonus program as different local governments faced different priorities at hand. In rural areas, one of the most momentous problems was the population crisis. For instance, South Jeolla province launched the baby bonus in 2000 in response to a substantial reduction of the workforce in the province (Yoon 2000). Similarly, North Jeolla province initiated several population policies, including the baby bonus program. The central government imposed the population threshold of each province: a minimum of 2 million residents in the 2000s. It reduced financial subsidies and the number of local government administrative divisions if the minimum population threshold was not met. North Jeolla province was in danger of not meeting this population standard, so that it launched various population policies, including childbirth and migration promoting programs (Ji et al. 2001).

Despite general recognition of North Jeolla province as the first district that launched the program in 2000, the detailed information of how the program had been operated in the early stage is somewhat ambiguous.[10] This is because the local governments did not incorporate this program in their municipal ordinance, so the details of the program operation are not available. However, the authoritative interpretation made by the National Election Commission of South Korea (hereafter NEC) in 2005 helps to clarify the operation of the baby bonus program by the establishment of municipal law in each local district. A year prior to the 2016 local general election in South Korea, the NEC made an authoritative interpretation as to local governments' customary behavior; the provision without legislating the law is a violation of the election law (Lee 2005). After the interim suspension of the

[9]For instance, Jongro-gu, Seoul 2018.05.25 Municipal Ordinance 1266, and South Gyungnam 2019-01-31 Municipal Ordinance 4532.

[10]Most Korean papers refer to Hampyoung-gun or Nonsan-si as the first districts to have initiated the program. This ambiguity is caused by the fact that the early stage of the baby bonus program was an inside policy, not codified in the municipal ordinance. However, the Population Policy Casebook specifies Muan-gun and North Jeolla province as the first districts with the program.

program in 2005, local governments started incorporating the baby bonus programs in their municipal law. This codification clarifies the detailed operation of the baby bonus program afterward.[11]

Local governments in South Korea have expanded the program in two directions: extending the targeted birth orders (mainly, from third or higher parity births to second and first child) and increasing the transfer amount at each parity birth. Figure 1.4 highlights this increasing operation trend from 2007 to 2016. The acceleration of the adoption of the policy continued until recently, that most local governments launched the cash transfer program as of 2016. Initially, the targeted birth order of the program was on the third or higher parity births with about 55.41% of local governments providing financial subsidies in 2007. Specifically, the number of local governments with the baby bonuses for the third child increased from 128 (total number of districts: 231) to 225 (total number of districts: 228[12]) from 2007 to 2016. However, the target order became expanded to earlier parity births (first and second) as the baby bonus for higher parity births did not seem to be effective, given the country's decreasing TFR despite the widespread introduction of the policy. Thus, it is somewhat a recent trend that local governments start providing the baby bonus for the first and second child in addition to the existing benefit for the third- and higher-order births. Hence, 204 districts operated the baby bonus additionally for a firstborn in 2017, a substantial increase from 86 districts in 2007.

The average transfer amounts of the baby bonus increased substantially over the same period, as shown in Figure 1.5. The average total transfer was approximately 594 USD, 998 USD, and 2,208 USD[13] for a first-, second-, and third-order child, while for comparison, the

[11] It allows me to include the exact baby bonus amounts and track how other aspects of the program change as the paper covers the baby bonus program after 2007, which is after the codification of the baby bonus program in all districts.

[12] The total number of local governments decreased due to municipality mergers.

[13] All amounts are CPI-adjusted.

monthly salary calculated based on the minimum wage in South Korea was 907 USD in 2007. The amount for each parity increased to 815 USD, 1,233 USD, and 3,074 USD, and the monthly salary was 1,260 USD in 2016. The increase in the average transfer amounts might not look so notable at first glance, but that is because additional districts with the program tend to start providing small transfer amounts. Nonetheless, the average baby bonus became substantial, especially for higher-order births, and it can cover at least the medical expenses for child delivery, even for the first and second child.[14]

The baby bonus program is an unconditional[15] cash transfer program with no eligibility conditions except for the residency in the area. Most local districts require a certain length of residency, 209 local districts specified this residency requirement in their municipal ordinance in 2016. Table 1.1 summarizes the residency requirements that apply mostly for either of the caregivers[16] of a child, but the length varies across districts. For 2016, 61 local governments–the largest share–require a year of residency in the districts. The second-largest proportion, which is 54 local governments, asks for the primary caregiver of a child to be living in the district at the time of childbirth or birth registration. Also, the length is shorter in urban cities that provide smaller amounts of the baby bonus, whereas it becomes relatively long in rural areas to prevent taking advantage of the baby bonus.

In that regard, potential "baby bonus seekers" might exist due to the differences in transfer amounts. This could incentivize individuals who live in a place with lower baby bonus amounts to migrate to a district with a higher transfer or report their address

[14]The government approximately estimates medical expenses to be 500 USD for child delivery (Kim 2019).

[15]One city, Ansan, located in the Midwest part of South Korea, however, offered a conditional cash transfer program for a second child that requires their income to be below the 50th percentile in 2017. However, the time frame I use in this paper does not include this year.

[16]The eligible recipient of the cash benefit differs by district, and most of them define eligible recipients are either one of the parents or both parents of a child. However, all local governments include a proviso clause: When both of the parents are not able to live together due to work or personal reasons, then the benefit is provided to one of the parents.

differently.[17] The second–for instance, reporting a different address using their parents' address–may not be severe in survey data, but people still face an incentive to migrate if the difference between the two districts is high, as pointed out by Hong and Sullivan (2016). However, the potential baby bonus seekers are likely not to be problematic here for the following reasons. First, given that most newborns are a first or second child (781 out of 929 new births), it is less likely that the differentials in baby bonus across regions would be sufficient to cover the migration costs[18] as the average bonuses are only about 1,000 USD. Second, in order to explore the relationship between net migration and the baby bonus program in South Korea, I use data on the net migration flow of females aged between 25 and 50 across districts, obtained from Statistics Korea (KOSIS) from 2007 to 2016. I employ a fixed-effects model and estimate the effects of the baby bonus amounts on net migration flow. The intuition is driven by the fact that the baby bonus can be correlated with females' migration decisions to take advantage of financial incentives. Table 1.2 shows that no significant impacts of the baby bonus are detected on the net migration flow in Korean districts over time.[19] Third, as evidenced in Kim et al. (2018), the migration rate in South Korea shows a decreasing trend from 2006 to 2017, and most of the migration flows are still toward urban areas, concentrating in the Great-Seoul area (Seoul, Incheon, and Gyeonggi) that provides small amounts of the baby bonus.

Another important point concerning the operation of the program is that the payment of the baby bonus typically is made in two ways: one-time payment or subsidy form for

[17] Reporting a different address to take advantage of the baby bonus could be more problematic and severe if I was using administrative data as this information actually is used to provide the baby bonus to parents. However, the survey data I use is not directly related to baby bonus eligibility, thereby having less incentive for females to report a different residential address.

[18] The average moving expenses are estimated as 950 to 1,770 USD (Kim 2013; Nam 2016), which are higher than the baby bonus differentials across regions.

[19] The dependent variable is the net migration of females aged between 20 and 45 years old in a district, and the independent variables are the baby bonus amounts of the district. The net migration is obtained from KOSIS, the national statistical database, operated by Statistics Korea.

a certain length of months. The former form is common for the first and second births and in urban areas where incentives to take advantage of the baby bonus are relatively low. On the contrary, the latter is more commonly found when transfer amounts exceed 1,000 USD and for higher parity births in rural areas. Most births in my data are the first and second births, suggesting most females in the data would have faced a one-time payment baby bonus. Nevertheless, I follow the method of Cohen et al. (2013) that uses the present value[20] of the transfer amounts if the baby bonus is not paid at once to guarantee conservative estimates of the policy effects.

It is worth noting several important changes in the baby bonus program over the last few years. First, several upper-level governments also introduce the program that residents in the area would receive an additional transfer. I include this amount when constructing the corresponding baby bonus amount. North Chungcheong and North Gyeongsang provinces incorporated the program in 2007, whereas the four districts in Jeju province were integrated into the province-level program in the same year. As of 2018, 6 metropolitan cities (Busan, Daegu, Incheon, Gwangju, Daejeon, Ulsan) and 3 provinces (North Chungcheong, North Gyeongsang, Jeju) run an additional baby bonus program at the upper-level government. Second, the two districts–Jongro in Seoul and Goksung in South Jeolla–abolished the cash transfer, contrary to the increasing trend in the rest of the country. For instance, Jongro eliminated the provision for the first parity birth while Goksung entirely repealed the cash transfer program in 2017. The reason for the policy being abolished was, in fact, the intense debate regarding the effectiveness of the program not just in those regions but in the entire country.

[20] This value is computed using the CPI-index and adjusted based on the length of the provision of the transfer.

1.3.2 Marriage Promotion Budget

It is a recent phenomenon that South Korea is aware of the importance of marriage in relation to its population shrinkage, and some provinces–the upper-level local governments–operate programs promoting marriage and population in the province.[21] The purpose of these programs is to alleviate various constraints of singles in forming a marital union, thereby incentivizing marriage. Yet, these marriage promotion programs are at the governor's discretion of each province. Promoting marriage has not been recognized as one of the utmost social tasks in contrast to childbirth; thus, no law was enacted for the operation of the marriage promotion programs. For that reason, the local provinces with the program change over time, which seems to be related to changes in the governor of the local government. Nonetheless, only two provinces (Gyeonggi and South Jeolla) out of 17 provinces have never allocated budgets for marriage encouragement.

I construct a variable based on the Annual Casebook of Population Policy of Local Governments (the Population Policy Casebook), computing the budget used for the marriage promotion category per 10,000 residents. The Ministry of Health and Welfare collects all population policies operated by local governments for a comprehensive understanding of the population policy and publishes the Population Policy Casebook every year.

According to the Population Policy Casebook, policies categorized in marriage promotion do not directly target childbirth intentions but explicitly provide an incentive to form a marital union for unmarried singles. Thus, I compute the allocated budgets for the policies that directly target to increase the marriage rate in the province and use this

[21] Local governments do not keep detailed information about the marriage promotion programs as it is operated as a non-major policy, unlike the baby bonus program. These programs are not included in the municipal ordinance, but the central government collected all policy information that could be classified to be population-related and published those in the Population Policy Casebook. Also, there is no unconditional program given to newly married couples like the baby bonus program.

as an exogenous variation to correct for selection bias, as explained below. Such policies are blind meetings for the unmarried set up by local governments where whoever wishes to find a spouse can apply and meet a date. Also, provinces subsidize expenses when finding a spouse and getting married either internationally or domestically and provide a wedding venue to encourage cohabited couples or unmarried couples to enter into a formal marital union and health examination for couples at no cost.

1.4 Data

I use three datasets to construct a full set of information related to women's marriage and childbirth decisions, including females' demographic information, their eligible baby bonuses, and the marriage promotion budget. The Korean Longitudinal Survey of Women and Families is used to incorporate females' demographic information, and I combine this with 1) the baby bonus amounts that are acquired from municipal ordinances of each district and 2) the marriage promotion budget described in the province from the Population Policy Casebook published by the Ministry of Health and Welfare.[22]

1.4.1 Data

The Korean Longitudinal Survey of Women and Families (KLoWF), initiated in 2007, is a rotating panel survey conducted by the Korean Women's Department Institute. The panel data is a representative sample of the 2005 Population and Housing Census, having the same primary enumeration districts (EDs) used for the 2005 Census. The panel collected its first wave in 2007 and surveyed 9,068 households selected from households that have females

[22]The data collection process is described in the appendix.

aged between 19 and 64 and live in South Korea at the time of the survey. The data includes 9,997 women in its first-year sample, tracking their demographic characteristics biannually from 2008 to the latest wave in 2018. It targets to contain nearly 10,000 individuals in the sample; however, some households attrited in the subsequent waves. They are replaced by a household with similar characteristics from the same ED. About 70% of the original sample remain in the 2016 wave. I employ information from 2008 to 2016.

As its name suggests, the panel explicitly aims to establish a database of women's lives and ultimately support the evaluation of the female-targeted policies more effectively, which matches the purpose of this study. The data questionnaires include changes in place of residence, income and wage, and demographic information that can be further categorized into her family-related information and daily life information. The use of this panel data gives me three main advantages: First, it provides detailed information regarding females' residential addresses[23] where the female has lived. This is a piece of critical information to compute the policy variable of central interest–the baby bonus–as the amounts of the baby bonus are matched based on their residential addresses reported in the panel data. Second, the panel data ensures a larger number of females in the data due to its construction strategy. Each household has at least one female, while other data often has households without any female in their household. Lastly, the panel data includes comprehensive information regarding households' and individuals' characteristics, thereby allowing me to have the individuals' marriage and birth history as well as their essential information related to their family formation decisions.

I employ the unbalanced panel data with sampling weights[24] to maintain the largest

[23] This survey data includes addresses of the females at the district level that females report at the time of the survey. Often, other datasets reveal only their province-level address.

[24] The sampling weights are included in the data to make the sample resemble the census data. The panel describes the necessity of these weights as it is "*... needed for the differences in probabilities of selection, responses, and tracking.*"

sample size and refine the data as follows. First, women aged between 18 to 45 years old who never had childbirth experience before 2007[25] are included because the main interest of this paper is in the marriage decision and reproductive behavior of females. Second, I exclude the first wave from the analysis to have the same two-year gap between each wave, and also because the second wave covers the year of the first wave's survey.[26] Third, only birth outcome information is used for the latest wave (2018 data) to take timing lags in birth decisions into account. It is more reasonable to postulate that family plans are made at least a year before actual childbirths. Thus, the demographic information of the last wave is excluded, while the birth outcome of the year 2018 is attached to the demographic information of the 2016 wave. Lastly, I drop the observations who moved to a different city and got higher baby bonuses–11 out of 640 births initially from the data–since they could be potential baby bonus seekers. As a result, I end up with 7,411 observations for 3,085 women in my final data.

1.4.2 Summary Statistics for Quantum Effect

Table 1.3 reports the summary statistics[27] of the main analysis sample irrespective of their marital status. The proportion of married women is approximately 29%, and 8.37% of

[25] I exclude the females whose birth outcome appears prior to 2007 for the following reasons. First, females who married and had a child before 2007 made their lifetime decisions in the absence of the MPB and the baby bonus programs. Second, by focusing on the subgroup of females, it allows me to inspect further how it can lead females to form a family and have a child with particular attention to early parity births. In that sense, these subsample females could be thought of as the main drivers and targets of the policy to increase the country's fertility rate since they likely have at least a child once married. Third, this subgroup of females is relatively young and less wealthy. The different socio-demographic characteristics that these younger females possess would play a role in their family plan decision, and they are more likely to be free from the necessity of marriage and motherhood in their life. Thus, examining the policy effect on this subgroup can be more informative as to whether it can have a substantial influence on females' choice in the future.

[26] The first wave collects the income of a household that is earned the first half of the year 2007. This overlaps with the second wave that collects the income of a household earned in the year 2007.

[27] All amounts are in real terms and converted in 1,000 USD.

females experienced childbirth. The average baby bonus for a firstborn (BB1) is about 107 USD, and this becomes higher for a second child–488 USD baby bonus for a second child (BB2). The baby bonuses become substantial that a 1,790 USD transfer is given to the third (BB3) or higher parity births, and the sum of these baby bonuses (BB1 + BB2 + BB3) is 2,386 USD . The average budget used for marriage promotion in the province where individuals lived is roughly 1,164 USD per 10,000 residents, but these amounts varied significantly by year. This is because, as mentioned before, the budget allocation was purely at the discretion of the head of the district.

The summary statistics of demographic information are reported at the bottom of the table. The average woman is roughly 28.43 years old and completed at least a high school education, with an average of 60,440 USD household income. The next rows present the summary of her parents-related variables. About half of the females have a good relationship with their parents at age 15, whereas 6.06% of females have a bad relationship with their parents on average. Her mother received fewer years of education compared to her father, that 12.2% of her mothers completed at least college degrees. Meanwhile, 21.8% of her fathers earned at least a college degree.

Table 1.4 displays the summary statistics for married women with additional variables related to her husband. The married women have a child with 23% probability and face slightly higher average amounts of the baby bonus and marriage promotion budget relative to the full sample. The married sample is 33.88 years old, possesses similar years of education, 14.30 years spent at school, and household income, 58,740 USD on average. The proportion of married women having a good relationship with their parents is somewhat smaller than that of the full sample, 48.8%, but a larger proportion of the married women has a bad relationship with their parents at age 15, 7.95%. The proportions of the parents with a college degree or more are 4.88% for mothers and 10% for fathers, respectively. These proportions are much less than that of all females, which is consistent with the higher average age of the married subsample. Husbands are 36.75 years old. They completed similar years of education, 14.64 years, earned 3,674 USD monthly salary, and worked about 8.6 hours a day, on average. 55.9% of the married women live in their own house, and 10.1% of them live with either their parents or parents-in-law.

1.4.3 Summary Statistics for Tempo Effect

The data is reorganized to perform the survival analysis corresponding to the tempo effect. Table 1.5 shows the summary of three outcome variables: months to marriage, first delivery, and second delivery. The first column shows the summary statistics of females regardless of their age as to time-to-marriage after she becomes 20 years old. Among those who had never married before 2007, less than a fifth (17%) get married, and the average time-to-marriage measured in months is 102.853, meaning that it takes 8.57 years to nuptials after she became 20 years old.

The next three rows display the summary statics for the subgroup of females who became aged 20 between 1991 and 2011 as to the time-to-marriage, first delivery, and the second delivery. The average duration to marriage is about 105 months, having only 7% of females newly enter into marriage. Figure 1.6 shows that most women are married between 100 and 150 months after she aged 20 years old. As to child delivery, I observe that more than half of the married women do not give birth to their first child, and it takes approximately 40.3 months to first delivery on average. As illustrated in Figure 1.7 most first delivery is completed within 40 months after marriage. Only 37.55% of females give birth to their second child, and the average length is approximately 4 years from her first delivery. Figure 1.8 shows that the second child delivery mostly happens between 20 and 40 months after her first delivery. This contrasts with the first delivery that is completed relatively soon after her marriage. However, for those who have a second delivery, it is a little bit delayed to have sufficient time for child-rearing for her first child.

Table 1.6 describes the basic characteristics of the sample used for the survival analysis. Average women in the data are about 25 to 30 years old and completed high school education. Half of them had a good relationship with her parents at the age of 15. In contrast, less than a fifth of the females had a bad relationship. The proportion of the parents who earned at least a college degree is 20.5% for the father and 10.7% for the mother of the female. However, the proportion is almost halved when it comes to the child delivery data, potentially due to the increase in females' mean age. For the child delivery duration analysis, I incorporate husband-specific variables and whether she resides with either her parents or parents-in-law. Her husband has 32 years old on average, with similar years of schooling as her. He earns almost 4,000 USD a month with 7 to 9 hours of working a day. About half of them lived in their own house, and a fifth live with either side of their parents.

1.5 Empirical Strategy

I employ the panel model with sample selection proposed by Semykina and Wooldridge (2018) to estimate the effect of the baby bonus program. The model accounts for selection bias and also controls for individual heterogeneous effects while imposing a specific form of them. Next, the Cox (1972) proportional hazard model and the frailty model are used to gauge the effectiveness of the two policies on reducing time-to-marriage and time-to-child delivery.

1.5.1 Correlated Random Effects Model With Sample Selection

General Setup

I first specify how family plan decisions are made and highlight the potential source of bias from the basic model. The outcome variables are binary marital status, M_{ijt}, and childbirth decision, C_{ijt}, for individual i living in region j at time t. Let $\mathrm{MPB}_{j,t-1}$ be the amount of marriage promotion budget and $\mathrm{BB}_{i,t-1}$ be the amount of baby bonus. The marriage decision is modeled as:

$$\begin{aligned} \text{Married}^*_{ijt} &= \delta_1 \mathrm{MPB}_{j,t-1} + \delta_2 \mathrm{BB}_{i,t-1} + X_{i,t-1}\alpha + \omega^m t b^m_j + \tau^m_t + r^m_j + \mu^m_i + u^m_{ijt} \\ \mathrm{M}_{ijt} &= 1[\text{Married}^*_{ijt} > 0], \qquad t = 1, \cdots, T \end{aligned} \tag{1.1}$$

and the childbirth decision is modeled as:

$$\begin{aligned} \text{Childbirth}^*_{ijt} &= \theta \mathrm{BB}_{i,t-1} + X_{i,t-1}\beta + \omega^c t b^c_j + \tau^c_t + r^c_j + \mu^c_i + u^c_{ijt} \\ \mathrm{C}_{ijt} &= 1[\text{Childbirth}^*_{ijt} > 0], \qquad t = 1, \cdots, T \end{aligned} \tag{1.2}$$

where μ^k_i is a time-invariant individual effect for $k = \{m, c\}$ with m and c indicating that the variable is used in marriage and childbirth equations, respectively. r^k_j is a region-specific effect, and u^k_{ijt} represents idiosyncratic error terms. Year fixed-effects, τ^k_t, are included to take into account the declining fertility trend of the country and other potential common factors specific to that year. Additionally, It accounts for any pro-natalist policy changes implemented at the national level, which can potentially affect all females in South Korea. It is plausible to assume that each province has its distinctive attitudes toward marriage and childbirth shared by the residents. Women in urban cities like Seoul, the capital of South Korea, would likely delay marriage and forgo having children in lieu of their labor market opportunities, whereas rural areas would still attach to traditional life, having their family earlier than females in Seoul. These potential regional differences are controlled by the province-level fixed effects, r_j^k. The regional trend, b_j^k, which is assumed to be linear, is captured by allowing for different trends in urban and rural districts. The urban districts consist of Seoul and 7 other metropolitan cities. Finally, MPB, BB, and other control variables, X, are measured at time $t-1$ to account for the discrepancy in timing between conception and child delivery.

The variable of primary interest, $\mathrm{BB}_{i,t-1}$, which is the amount of the baby bonus that individual i faces at time $t-1$, captures the effect of the baby bonus on females' fertility decisions. The present value of the baby bonus a year prior to childbirth is used due to the discrepancy in timing between conception and child delivery. I first include a vector of baby bonuses (BB1, BB2, BB3), which specifies the baby bonus for the first, second, and third child. I also define the baby bonus variable as the sum of all baby bonuses (BB = BB1 + BB2 + BB3), to investigate whether the sum of the baby bonus affects women's birth decisions. Yet, the interpretation of the latter is valid under the assumption that females plan to have at least three children in their lifetime and account for the sum of the baby bonus, collectively.

Another key variable is $\mathrm{MPB}_{j,t-1}$, which is the present value of the marriage promotion budget in province j at time $t-1$. The inclusion of this variable examines the effect of the program on marriage decisions, and it also helps to control for the potential selection bias, as the observed birth outcomes for married women may not reflect the true baby bonus policy effect. If some unobserved factors affect females' marriage decisions in the first place, which could also affect their childbirth decisions, then these unobserved factors can affect both u^m_{ijt} and u^c_{ijt}, causing selection bias. To eliminate this selection bias in the estimation, an exclusion restriction is required. The marriage promotion budget is assumed to affect the marriage decision of females but is not directly related to women's childbirth decision since the policy directly targets marriage promotion but not childbirth.[28] Thus, it is reasonable to include this variable in the selection Equation (1.1) but exclude it from the outcome Equation (1.2), becoming an exclusion restriction variable.

$X_{i,t-1}$ is a vector of the women's demographic controls that could affect her marriage and childbirth decisions. It includes her personal characteristics: age and its square, and years of education. Her family background is accounted for by the following variables: income of her household and its square, the relationship with her parents at the age of 15, the education level of her parents, and whether she lives with her parents or parents-in-law. It also includes her husband's demographic information as this could be one of the most important determinants for childbirth decisions (Thomson et al. 1990; Sander 1992). Such demographic characteristics are age, years of education, monthly salary and its square, and hours worked of her husband. In addition, it contains a set of binary indicators whether she has first, second, and third child or not.

[28]These policies target increasing the chance to meet a spouse but do not directly aim to increase the fertility rate, and other policies related to childbirth are classified into other categories. Thus, it is reasonable to assume that this variable does not impact childbirth directly.

Correlated Random Effects With Sample Selection

One main difficulty arises in identifying the effect of the two policies using Equations (1.1) and (1.2): selection bias. To estimate the effect of the baby bonus program, I use married women data as unmarried women are unlikely to exhibit childbirth outcomes due to the conservative social norm. This could result in selection bias when females select into marriage based on unobserved factors, making a systematic difference based on females' marital status. I employ the panel correlated random effect model with sample selection (Semykina and Wooldridge 2018, the 'SW' model) to deal with this problem.

To describe the SW model, I start with the correlated random effects model (CRE model) (Chamberlain 1980), which is a sensible analogy to the fixed effects model to eliminate the individual-specific effects in nonlinear models. Presumably, time-invariant individual individual-specific characteristics would play an indispensable role in both marriage and childbirth decisions, and it could cause bias unless controlled. For instance, females who are raised from a more family-oriented household are more likely to get married and have children due to their preferable childhood experience. While taking mean deviations from both sides eliminates this effect in linear models (the fixed effects model), it is not feasible to follow the same strategy due to the nonlinearity of the binary choice model. In that circumstance, one of the available approaches to take care of this bias is by using the Mundlak (1978) device, which specifies the functional form of the individual effects as follows:

$$\mu_i^k = \psi^k + \bar{X}_i \eta^k + a_i^k \qquad \text{for } k = \{m, c\} \tag{1.3}$$

where ψ^k is the intercept, $\bar{X}_i = T^{-1} \sum_{r=1}^{T} X_{i,r-1}$, and $a_i^k \sim N(0, \sigma_{ak}^2)$ is independent of

$\mathbf{x}_i \equiv (X_{i1}, \ldots, X_{iT})$.[29] Let $v_{ijt}^k = a_i^k + u_{ijt}^k$, and this yields

$$\text{Married}_{ijt}^* = \psi^m + \delta_1 \text{MPB}_{j,t-1} + \delta_2 \text{BB}_{i,t-1} + X_{i,t-1}\alpha + \bar{X}_i \eta^m + \omega^m tb_j^m + \tau_t^m + r_j^m + v_{ijt}^m \tag{1.4}$$

$$\text{Childbirth}_{ijt}^* = \psi^c + \theta \text{BB}_{ij,t-1} + X_{i,t-1}\beta + \bar{X}_i \eta^c + \omega^c tb_j^c + \tau_t^c + r_j^c + v_{ijt}^c \tag{1.5}$$

A central problem in identifying the effect is selection bias, and I employ the panel bivariate probit with sample selection (the SW model) to eliminate this source of bias. This model is based on Van de Ven and Van Praag (1981), which is a special case of the sample selection when both selection and outcome are binary.[30] Under the Probit model, the relationship between the error terms can be denoted as:

$$v_{ijt}^c = \gamma v_{ijt}^m + e_{ijt} \tag{1.6}$$

where $\text{Var}(v_{ijt}^c) = 1$, $\text{Var}(v_{ijt}^m) = \sigma^2$, $\rho = \text{Corr}(v_{ijt}^c, v_{ijt}^m)$, $\gamma = \rho/\sigma$, and $e_{ijt}|\mathbf{x}_i, v_{ijt}^m \sim N(0, 1-\rho^2)$. Let Z_{ijt} denotes $(\text{MPB}_{j,t-1}, \text{BB}_{i,t-1}, X_{i,t-1})$. In the model, the proportions of individuals based on their marital and childbirth status can be expressed as:

$$P(C_{ijt} = 1, M_{ijt} = 1|\mathbf{x}_i) = \int_{-\infty}^{q_{ijt}} \Phi(w_{ijt})\phi(\nu)\mathrm{d}\nu \tag{1.7}$$

$$P(C_{ijt} = 0, M_{ijt} = 1|\mathbf{x}_i) = \int_{-\infty}^{q_{ijt}} [1 - \Phi(w_{ijt})]\phi(\nu)\mathrm{d}\nu \tag{1.8}$$

$$P(M_{ijt} = 0|\mathbf{x}_i) = 1 - \Phi(q_{ijt}) \tag{1.9}$$

[29] This implies that a_i^k is independent of all X_{ijt} regardless of t

[30] The exclusion restriction variable is not explicitly required in the case of bivariate probit with sample selection, as pointed out by Lee (2009). The outcome is not linearly related to covariates; thus, the multicollinearity of the inverse Mill's ratio should not be problematic in theory, unlike in the linear model case. Nonetheless, the inclusion of this variable facilitates the implementation of the model.

where $w_{ijt} = (\psi^c + \theta \mathrm{BB}_{i,t-1} + X_{i,t-1}\beta + \bar{X}_i\eta^c + \omega^c tb_j^c + \tau_t^c + r_j^c + \rho\nu)(1-\rho^2)^{-1/2}$, $q_{ijt} = \psi^m + \delta_1 \mathrm{MPB}_{j,t-1} + \delta_2 \mathrm{BB}_{i,t-1} + X_{ijt}\eta^m + \bar{X}_i\xi^m + \omega^m tb_j^m + \tau_t^m + r_j^m$, and ϕ and Φ represent the probability density function and the cumulative density function of a normal distribution, respectively.

Under the CRE framework in Equations (1.4) and (1.5), when $\mathrm{MPB}_{j,t-1}$, is not correlated with any of the idiosyncratic errors in both equations, the panel correlated random effect model with sample selection can be estimated consistently by maximizing the following sequential probit likelihood function:

$$\begin{aligned} L_{ijt} \equiv f(C_{ijt}, M_{ijt}|\mathbf{x}_i) = \left[\int_{-\infty}^{q_{ijt}} \Phi(w_{ijt})\phi(\nu)\mathrm{d}\nu\right]^{C_{ijt}M_{ijt}} \\ \times \left[\int_{-\infty}^{q_{ijt}} [1-\Phi(w_{ijt})]\phi(\nu)\mathrm{d}\nu\right]^{(1-C_{ijt})M_{ijt}} [1-\Phi(q_{ijt})]^{1-M_{ijt}} \end{aligned} \tag{1.10}$$

which maximizes the probability of each individual defined in Equation (1.7), (1.8), and (1.9) simultaneously. The average partial effects (APEs) show the effect of the covariate g on the outcome variable averaged across all cases in the sample. The APE of any continuous covariate g in $(\mathrm{MPB}, \mathrm{BB}, X)$ can be obtained as follows:

$$\widehat{\mathrm{APE}}_g = \left[\frac{1}{NTJ}\sum_{i=1}^{N}\sum_{t=1}^{T}\sum_{j=1}^{J}\phi(\hat{\psi}^c + \hat{\theta}\mathrm{BB}_{i,t-1} + X_{i,t-1}\hat{\beta} + \bar{X}_i\hat{\eta}^c + \hat{\omega}^c tb_j^c + \hat{\tau}_t^c + \hat{r}_j^c)\right]\hat{\beta}_g \tag{1.11}$$

where $\hat{\beta}_g$ is the parameter estimate of the covariate g. For binary covariates, the APE of covariate g is defined as:

$$\widehat{\text{APE}}_g = \frac{1}{NTJ}\sum_{i=1}^{N}\sum_{t=1}^{T}\sum_{j=1}^{J}\Big[\Phi(\hat{\psi}^c + \hat{\theta}\text{BB}_{i,t-1} + X^1_{i,t-1}\hat{\beta} + \bar{X}_i\hat{\eta}^c + \hat{\omega}^c tb^c_j + \hat{\tau}^c_t + \hat{r}^c_j) \tag{1.12}$$
$$- \Phi(\hat{\psi}^c + \hat{\theta}\text{BB}_{i,t-1} + X^0_{i,t-1}\hat{\beta} + \bar{X}_i\hat{\eta}^c + \hat{\omega}^c tb^c_j + \hat{\tau}^c_t + \hat{r}^c_j)\Big]$$

where $X^1_{i,t-1}$ have covariate g as one while $X^0_{i,t-1}$ covariate g to be zero.

The decomposition of the APE investigates which channel can more effectively affect females' childbirth decisions. The decomposition of the policy effects can be computed as:

$$TE: \quad \frac{\partial \Pr(C = 1|X, \text{BB})}{\partial \text{BB}} \tag{1.13}$$
$$IE: \quad \frac{\partial \Pr(M = 1|X, \text{BB}, \text{MPB})}{\partial \text{BB}} \times \Pr(C = 1|M = 1) \tag{1.14}$$
$$DE: \quad TE - IE \tag{1.15}$$
$$MIE: \quad \frac{\partial \Pr(M = 1|X, \text{BB}, \text{MPB})}{\partial \text{MPB}} \times \Pr(C = 1|M = 1) \tag{1.16}$$

The decomposition shows three potential channels that the baby bonus can have. First, Equation (1.13) shows the total effect of the baby bonus program (i.e., the TE) irrespective of the channel at which it modifies females' childbirth decisions. The TE corresponds to the changes in birth probability caused by the baby bonus program using Equation (1.5). Second, another potential channel that the baby bonus could have is by helping the formation of a family and inducing more females to have a child because of the marriage. This indirect effect of the baby bonus program (IE) is represented as in Equation (1.14). However, this is not directly obtained by the estimation but needs to be computed using Equation (1.14). The IE is the product of the changes in marriage likelihood caused by the BB and the proportion of childbirth among married females. Third, the direct effect of the baby bonus (DE) represents how the baby bonus program directly encourages women to enter into maternity because of the financial subsidies. This is simply the difference between IE and TE, as illustrated in Equation (1.15). Meanwhile, the MPB can also influence childbirth decisions as marriage is a pre-pathway to a child, and by definition, MPB can only have the indirect effect. Equation (1.16) shows the indirect effect of the MPB (MIE) is the product of changes in marriage likelihood due to the MPB and the proportion of childbirth among the married females.

Testing for Sample Selection

A test for selection bias informs whether the selection bias correction is necessary. Selection bias is caused by females selecting into marriage potentially based on their different demands for children. This can be tested using the null hypothesis, H_0: $\rho = 0$, which is equivalently to testing H_0: $\gamma = 0$ in Equation (1.6). Under the null hypothesis, the two error terms in the selection and outcome equations are not correlated as $\rho = 0$ means $v^c_{ijt} = e_{ijt}$. Semykina and Wooldridge (2018) proposed tests for selection bias to identify if correction is necessary: the Lagrange multiplier test and the variable addition test. The two tests are asymptotically equivalent, while the latter is easier to implement in practice. The procedure of the variable addition test is as follows:

(i) Estimate the selection equation on all observations using probit separately for each t and compute the inverse Mills ratio, $\hat{\lambda}_{ijt}$ for each i, j, and t. Equivalently, estimate the selection equation on all observations using probit including all t with time fixed effects and compute $\hat{\lambda}_{ijt}$.

(ii) Use t-test to test the significance of $\hat{\lambda}_{ijt}$ after estimating pooled probit for all t with the married sample.

1.5.2 Survival Analysis

I employ two models for the estimation of the policy effect in terms of how it affects the timing of the events: the Cox (1972) proportional hazard model and the frailty model. One of the key features of survival models is their acknowledgment of censoring. Often, the event of interest is not fully observed for some individuals within the time of the survey, and those individuals whose outcome is not fully observed are said to be censored.

Cox Model

The Cox model provides a reference model for survival analysis with the central assumption that time to an event is proportional to covariates. Here, the outcome variables, T, are defined as follows: duration to marriage from age 20, duration to first delivery from marriage, and duration to second delivery from the first delivery. Under this model, the rate of entry into an event is represented by the following equation:

$$h_i(t|\mathbf{s}) = h_0(t) \exp\left(\mathbf{s}_i' \chi\right) \tag{1.17}$$

where t is the realization of the time to occurrence of the events, T, for i and $h_0(t)$ is a baseline hazard rate that shows the risk for individuals with all covariates set to zero. $\exp(\mathbf{s}_i'\chi)$ indicates the relative impact of the observed characteristics, $\mathbf{s}$, on hazard rates, with $\mathbf{s}$ being the set of all covariates used in the sample selection model. One of the biggest advantages of the Cox model is that it can estimate the relative effects of covariates without specifying the baseline hazard. Since it is of interest to evaluate the effect of the policy variables, I do not specify the baseline hazard.

The original model assumes time to be measured continuously, which means no tied events, but it is rare that data is measured in that way. With the existence of tied events for almost every month[31], $m = 0, \cdots, D$, the Breslow (1974) approximation is widely used, which assumes all tied observations in a month m to leave precisely at the same time. With the Breslow approximation method, the partial likelihood function can be written as:

$$L(\chi) = \prod_{m=1}^{D} \frac{\exp\left(\sum_{i \in D_m} \mathbf{s}_i' \chi\right)}{\sum_{i \in R_m} \exp(\mathbf{s}_i' \chi)^{d_m}} \tag{1.18}$$

where R_m is the risk set at the mth month, which includes all individuals who have not experienced the event until month m. The event set, d_m, indicates the number of individuals who left exactly at month m in the data, and D_m is the event set at the mth distinct survival time. A set of parameters, χ, is obtained by maximizing the partial likelihood function in Equation (1.18).

Frailty Model

The frailty model incorporates cluster-specific random effects that modify the baseline hazard (Goldstein 2011; Snijders and Bosker 2011). For instance, individuals living in the same province may share the same unobserved characteristics, such as cultural and environmental aspects, and the lower-level local governments can share the same policy agenda given the same province (upper-level) governments. By the adoption of the frailty model, the possible correlation due to the hierarchy of local governments can be accounted for. The model introduces the random effect term, called frailty, and denoted here by B,

[31] It is impossible to know which event precedes the rest of the observations recorded in the same month, m, when the tied events are observed. The partial likelihood function needs to be modified depending on the assumption on which observation among all tied events in the month m precedes.

implying heterogeneity across clusters, which in turn allows different clusters to share the common baseline[32] risk. Within the model, individuals who live in the province, j, share the frailty terms, B_j, which simply modifies the baseline hazard depending on the area of residency.

$$\begin{aligned} h_{ij}(t|\mathbf{s}) &= B_j h_{ij} && (1.19) \\ &= B_j \exp{(\mathbf{s}'_{ij}\zeta)} p t^{p-1} && (1.20) \end{aligned}$$

where B_j is constant over time and creates the dependence within a district and is assumed to follow the Weibull distribution. The survival function can be easily computed based on the hazard function, so can the maximum likelihood function.

1.5.3 Identification Challenges

Two additional challenges exist in identifying the effect of the policies. First, there are females who give birth to their first child within 9 months after her marriage. Those individuals could have faced a different choice procedure in that childbirth led them to form a legal partnership for child-rearing. Those individuals are likely to consider marriage as a necessary condition for childbirth as well, thereby forming a family before childbirth. In that sense, the existence of these individuals does not necessarily mean that selection bias correction is not required. These females still gave birth within their own family, but the conception timing likely preceded their marriage date. Nonetheless, to obtain a conservative estimate of the policy variables, I conduct a robustness check by dropping those individuals from the sample to prevent this potential reverse channel that women could have faced.[33]

[32] The baseline risk is the estimated hazard when all covariates are zero.

[33] Dropping those females who gave birth to a child within 9 months after her marriage may yield the exclusion of premature infants, which is not necessarily to be premarital conception. Thus, this strategy

Second, a concern with the use of survival models is the discrepancy of entry time across the observed individuals in the data. For example, the Cox proportional hazard model sets all the entry time to be zero. This may cause a problem when the time window of the data is longer. Females entering into a marriage in the 1980s would inherently be different from females entering into a marriage in the 2020s as to when they want to deliver a child. Hence, I additionally divide samples into a similar cohort group who became aged 20 between 1992 and 2011 (20-year time window) to mitigate the potential consequences of pooling different cohorts.

1.6 Estimation Results

This section examines the impact of the baby bonus program and the marriage promotion budget, exploring various aspects of family formation decisions. Section 1.6.1 first provides the results of the test for selection bias. The estimation results of birth-related outcomes are provided in Section 1.6.2 Then, Section 1.6.3 investigates the tempo effect of the baby bonus program.

1.6.1 Testing for Selection Bias

Test results for selection bias under the SW model are reported in Table 1.7. Overall, results substantiate the presence of selection bias. The first four columns include a set of baby bonuses for each parity birth, and the next four columns have the sum of the baby bonus in the outcome equation.[34] Columns (1), (2), (5), and (6) do not include the baby bonus in the selection equation, but the rest of the columns do. Odd columns only control

obtains conservative estimates of the policy effects.

[34] The numbering of the columns corresponds to the specifications reported in Table 1.10.

women's demographic information, and even columns control for women's husband-related characteristics in addition to females' information.

Estimation results with husband-related controls support the necessity for the selection correction in the estimation. Test statistics are statistically significant at the 5% level when husband-related variables are included. Husband-related characteristics are considered to be one of the most important determinants for childbirth plans (Thomson et al. 1990; Sander 1992; Bertrand et al. 2015), as females would make a family plan collectively with their husbands, especially in the Asian context. Thus, it can be concluded that the necessity of selection bias correction is supported with the proper inclusion of the husband-related covariates in the estimation. Additionally, the signs of the test statics are all positive when statistically significant. It implies that unobserved factors that affect one's marriage and childbirth decisions, such as the attachment to traditional values, are positively correlated.

1.6.2 Quantum Effect

Marriage Decision

Table 1.8 and 1.9 show the estimation results of the policy package on women's marital status with and without correction for selection bias respectively. Table 1.8 reports the average partial effects (APEs) of the variables obtained using Equations (1.11) and (1.12). The first two columns of Table 1.8 is used to estimate the effects of a set of the baby bonus on childbirth decisions, which is reported in Table 1.10.[35] The next two columns are obtained similarly, but using the sum of the baby bonus amounts for childbirth estimations instead. The odd columns have the MPB only in the marriage estimations, whereas the

[35]In Equation (1.10), the log-likelihood function maximizes the marriage and childbirth outcomes at the same time. Even though Columns (2) and (4) in Table 1.8 have the same set of covariates in the estimation, the specifications used for the childbirth estimation are different.

even columns add the baby bonus variable in the marriage estimations. In Table 1.9, Column (1) estimates the effect of the MPB on marriage formation likelihood, and the next two columns consider the effects of the baby bonus program, either a set or the sum, on marriage likelihood.

Results suggest that females are responsive to external marriage incentives. The MPB is associated with a 0.5 percentage point increase, 2.48% increase with respect to the average marriage outcome, irrespective of specifications, which is the same as in the naive model in Table 1.9, showing a 0.5 percentage point increase in marriage likelihood at the average level. Specifically, Columns (1) and (3) postulate the case in which females are myopic; thus, she would only care about the policy immediately relevant to her, which is the MPB. When this is assumed, the estimated effects are 0.5 percentage points, leading to a 2.48% increase in the marriage rate. Next, Columns (2) and (4) explore whether the marriage decision is potentially affected by the baby bonus program. The inclusion of the baby bonus program in the marriage outcome equation accounts for two things. First, the baby bonus program can increase females' lifetime budgets if they decide to get married and eventually have children. Second, provided the increased lifetime budget due to the baby bonus program, this may lead to higher childbirth. In that sense, the inclusion of the baby bonus variable also examines whether the indirect effect of the baby bonus program on childbirth decisions would work via increasing marriage probability. However, no substantial effect of the BBs is detected in one's family formulation decisions, unlike the MPB, demonstrating that the baby bonus program is unlikely associated with females' marriage decisions.

Other variables show some interesting results, and particularly, the estimated effects are similar regardless of the specifications, both in Tables 1.8 and 1.9. First, more years of schooling[36] decreases the likelihood of being married by about 1.5 percentage points at the average level, which echos with the literature. Second, it is a less desirable relationship with her parents at age 15 that makes one be a married woman than having a good relationship with her parents at age 15. Females who experienced a relatively bad relationship with their parents have a higher chance of forming their own family by 6 percentage points. This contrasts with the result that a better relationship with her parents is not likely to affect females' marriage decisions. Lastly, her father's education level, but not the mother's education level, shows substantial impacts in explaining women's marriage decisions, suggesting that her youth experience may play a critical role in her marital status. Her father's education level is associated with less chance of being married, by 5.9 percentage points.

Birth Probability

Tables 1.10 and 1.11 show the birth probability outcome results with and without sample selection correction, respectively. In Table 1.10, the average partial effects (APEs) are computed using Equations (1.11) and (1.12). The specifications used for the marriage outcome estimation are reported at the bottom of Table 1.10. The first four columns use a set of the baby bonus for each parity birth, and the next four columns include the sum of these baby bonuses instead. Odd columns do not include husband-related variables, whereas the even columns additionally control for those variables. Table 1.11, on the other hand, first estimates the baby bonus effect using the full sample in columns (1) to (4), and

[36]The data includes women's laborforce-related variables such as monthly salaries and hours of work, which could be the most influential factors on the decision-making process for childbirth. However, I exclude these variables in the estimation, as females' labor market participation decision is endogenous with their childbirth decisions. Instead, females' education is used as a proxy variable for whether she works or not, reflecting females' labor market attachment, given the strong correlation between education attainments and labor force participation.

then the rest of the columns restricts the sample to be married women only.

The comparison between the APEs in Tables 1.10 and 1.11 demonstrates that the naive estimation of using married women would overstate the impact of the baby bonus program. Findings suggest that none of the BBs for each parity shows substantial impacts on one's childbirth decision under the SW model, while the sum of this could increase the birth likelihood by 0.6 to 0.7 percentage points, as reported in Columns (6) and (8) in Table 1.10. This means that only if females are assumed to account for baby bonus amounts at once and decide to have at least three children, the policy can have a substantial impact on their birth decisions. This contrasts with the results reported in columns (6) and (9) of Table 1.11, showing the ostensibly significant and substantial effect of the baby bonus for the first child (BB1) for married women. Also, the sum of the baby bonus amounts is statistically significant when it comes to the full sample, having a 0.3 percentage point increase at the average level. The APE of the BB1 child under the naive model is 5.9 percentage points, meaning that the baby bonus for the first child substantially increases the likelihood of having a child by almost 25.6% relative to the mean childbirth outcome, on average. My preferred estimation result is reported in Column (2) that includes the husband-related variables in the childbirth equation and excludes the baby bonus variables in the selection equation. This is comparable to the results reported in Column (6) of Table 1.11. After accounting for selection bias, the preferred specification shows that the statistical significance of the BB1 vanishes and the magnitude of the estimated effect decreases substantially.

In Figure 1.9, the predicted effects in terms of birth likelihoods are presented, based on the specifications reported in Columns (2) and (4) of Table 1.10. Thus, in addition to her demographic information, a set of the baby bonus amounts and husband-related variables are included in childbirth estimation, and the only marriage promotion budget is included in marriage estimation. Similarly, Figure 1.10 also shows the results using Columns (6) and (9) in the same table. The estimates of the figures are computed by exploiting estimates obtained by using Equations (1.11) and (1.12), while a specific value of the baby bonus amounts is used instead to get the predicted effects at each transfer level.[37] The difference in birth likelihood, which is the slope, can be interpreted as the extent of the policy effects obtained by an additional 1,000 USD transfer. However, figures indicate that the baby bonus program does not have a substantial impact on birth decisions at any transfer level considered. The baby bonus for the first child shows a marginal increase in birth likelihood, but the increase is less than 0.2 percentage points at maximum and shows almost no increase at all after 7,000 USD. Baby bonuses for the second and third children are even smaller and have an almost flat slope, meaning that the baby bonus amounts do not induce more females to have a second and third child. This means that females are not induced to have more children even if the transfer amounts are up to 7,000 USD. In Figure Figure 1.10, the birth probability increases with an additional transfer, but the increase is not bigger than 0.2 percentage points.

The results confirm that using married women may not fit the birth analysis context in the presence of selection into marriage in South Korea. The seemingly effective baby bonus program effect is likely driven by uncontrolled selection bias, even if there is any. Some unobserved factors affect one's marriage decision, thereby making a systematic difference between married and unmarried women; consequently, it causes bias in the estimation of childbirth outcomes. Furthermore, this unobserved determinant of marriage affects both decisions in the same direction, as evidenced in the test results in Section 1.6.1.

[37]The APE of the baby bonus is the average of these predicted effects evaluated at all transfer levels in the data.

One potential scenario for the source of this bias is the personal degree of attachment to the traditional norms and formation of a family. Asian family studies in Jones (2005); Suzuki (2008) extensively document that traditional family norms remain effective in East and South Asian countries than Western countries, but such values have been decreasing over time. The married group is more likely to be strongly attached to this social norm and Familism, so they are more likely to enter into the marriage in the first place. It is reasonable to conjecture that their initial desire for children would also be higher than unmarried women. Given this, the naive estimates in Table 1.11 would overstate the effect of the baby bonus due to selection bias. These women who married are more likely to possess some characteristics associated with a higher demand for children, but the naive model fails to account for this characteristic. This underscores the value of our use of the SW model in this context.

One interesting aspect to note is that the significance of many variables disappears when estimating the birth outcome for the SW model. Unlike marriage, estimates of childbirth decisions in Table 1.10 and 1.11 are quite different in not only magnitudes but also in statistical significance. This single MLE using Equation (1.10) maximizes the marriage decision and childbirth decision simultaneously, taking the effect of the variables on both decisions at the same time. Most parts of the explanatory power are absorbed in explaining the marriage decision and show a relatively weaker relationship with the childbirth decision when accounting for selection using the SW model.[38] It can be understood that the decision to have a child is likely to have been made already at the time of one's marriage. Certain unobserved characteristics associated with less fertile outcomes would have already affected one's marital decision so that it did not explain childbirth outcomes as much as expected.

[38]It is true that the power is not so large given the number of observations in my estimation. Nevertheless, it is apparent that the statistical significance of many variables in childbirth decisions disappears compared to the naive model, unlike in the models for the marriage decision.

Therefore, none of her observed youth backgrounds seem to affect the childbirth decision in Table 1.10. In that sense, those variables previously thought to be important determinants of childbirth plans (in Table 1.10) do influence the marriage decision first but not directly and substantially modify the childbirth decision.

Effect Decomposition

Table 1.12 summarizes the decomposition of the policy effects using the estimates of Columns (4) in Table 1.8 and Columns (8) in Table 1.10.[39] The upper part of the table shows the baby bonus program effect's decomposition while a set of the baby bonus is used in the regression. On the other hand, the bottom two rows report the decomposition of the effects, but the sum of the baby bonus amounts is included in the regression instead. The first column displays the estimated total effect of the baby bonus program on changes in birth likelihood, which is simply the estimates reported in Columns (4) for the BB1, BB2, and BB3, and (8) for BB in Table 1.10. The next column indicates the indirect effect of the baby bonus program using the estimates in Table 1.8 and Equation (1.14). The third column is the difference between the first two columns, examining the extent to which females are influenced due to the baby bonus *per se*.

The decomposition of the policy effects tells that females are more likely to be affected by the direct channel of the policy effects. In contrast, the direct effect itself is not large in magnitude except the BB1 and not statistically significant. The direct effects are almost 10 times higher than the indirect effects when it comes to the baby bonus for the first child. This is reasonable since my estimates suggest that most females are unlikely to account for the baby bonus when giving birth to a child. Given the absence of the substantial impact

[39] Note that the decomposition requires the baby bonus variable both in marriage and childbirth decision equations; therefore only Columns (4) and (8) can be used for this analysis.

of the policy effect in the first place, as shown in Section 1.6.2, it is hard to expect that the indirect channel of the baby bonus via marriage would work significantly. Yet, Table 1.12 shows that the MPB has significant impacts on females' birth decisions. In fact, this could be even higher than the baby bonus program's indirect effect, and it is statistically significant regardless of the specifications. In sum, the decomposition suggests that more substantial impacts on increasing childbirths have come from the marriage promotion rather than the baby bonus package.

1.6.3 Tempo Effects

Time-to-Marriage

I delve into the impact of the baby bonus program and the marriage promotion budget from a different angle to investigate whether it can reduce time to key events. Table 1.13 shows the policy impacts on the timing of marriage from age 20. The first three columns report the estimation results using the Cox model, and the next three columns are results obtained by the frailty model. First, I include the MPB that is an imminent policy that females are eligible for, then add the baby bonus amounts, which she could be eligible for in the following two columns. An 1,000 USD increase in the MPB may reduce time-to-marriage by 2.7% at best–about a 3-month reduction[40] ($102.853 \times 2.7\%$)–, where the estimates are statistically significant at the 10% level. When imposing different intercepts for individuals who live in the same districts (the frailty model), the marriage promotion budget does not provide any evidence of a statistically significant effect. Another variable, her father's education level, shows surprisingly large effects, delaying marriage by about 42%.[41]

[40] The average duration to month is reported in Table 1.5.

[41] Her father's education level is related to the different cohorts who became 20 years old in different decades, thereby raising the need to examine the effect at the cohort level, which I will examine next.

The seemingly weak impact of the MPB is likely to be driven by the discrepancy of the birth year across females, so I use the subsample of females who became aged 20 between 1991 and 2011 (the "cohort sample")[42]. In Table 1.14, the time-to-marriage is decreased by between 9.5 and 12.1%, while the mean duration to marriage is 105.301 months. This implies that the MPB program helped them formulate a marital union by 10 to 12.7 months earlier with a 1,000 USD MPB. The number is higher than the estimation results using the full samples, showing a substantial impact on women at prime marriage ages, as they are most likely to be the direct target of the program. Second and as expected, the role of the father's education vanishes with the cohort adjustment, while the importance of the woman's relationship with their parents' increases. Women who describe her bad relationship with her parents at age 15 would have delayed their marriage decision much more, by almost 54% delay, than those who had a better relationship, but it is only statistically significant at the 10% level and under the frailty model.

Time-to-Child Delivery

Table 1.15 and Table 1.16 explore whether the baby bonuses help in reducing the length to child delivery using the females aged 20 years old between 1991 and 2011. The first four columns use the Cox model, and estimation results using the frailty model are followed in the subsequent columns. The first two columns of each model include the policy variable that is immediately relevant to her, and the next subsequent policy variable is added in the following two columns. Finally, even columns additionally control for husband-related characteristics.

In Table 1.15, the baby bonus for the first child reduces the length of having a child

[42]The cohort is set to be a little bit longer than other studies that set the cohort to have 10-year window to ensure enough number of observations.

after her marriage, which is robust across specifications when husband-related variables are included. From Column (2), it takes 32.05 months[43] ($40.317 \times 20.5\%$) to her first delivery for women with a 1,000 USD baby bonus, such that duration to first birth is reduced by about 8 months with the program.[44] The frailty model also gives similar results, about an 8.2 to 8.6-month reduction. This estimate can be seen as large at first glance, but the subsidy could be sufficient to induce women to have a child sooner if they decide to have a child at the time of their marriage. The sign of age at marriage highlights that women would expedite their first delivery due to their shorter reproductive ages when they get into martial union later in their life. I include the baby bonus for the second child to see if the timing for the first child is also affected by not imminent but potentially eligible subsidies. However, the results again suggest that females care about the next eligible program, not collectively accounting for the policy package.

Next, Table 1.16 shows that the baby bonus for the second child does not change the timing of child delivery after the first delivery. I use the subsample of females aged 20 years old between 1991 and 2011. These results are quite robust regardless of the specifications.[45] The preferred estimation results in Columns (4) and (8), which assume females would only account for the succeeding eligible benefit, tell that the baby bonus for the second child would not affect their timing decision. This is likely because most married women decide to have at least one or two children at the time of their marriage. This makes sense because it is hard to expedite their second childbirth, given the time and energy required for their firstborn child-rearing. Therefore, additional governmental financial incentives could lead

[43]The mean duration is reported in Table 1.5.

[44]I estimate whether the baby bonus incentivizes all females who are married in the data to have their first child earlier. The results show that the baby bonus for the first child is associated with an 18.8% month reduction in time-to-delivery after marriage, which is a 10-month reduction (53.67×0.188).

[45]When it comes to the full sample ignoring the cohort effect, the estimation results still indicate that the time-to-second delivery from first would not be changed substantially with the baby bonus program. The estimated effect is 4% and statistically insignificant.

them to have a child earlier, while the timing of second delivery remains unchanged even with a more considerably higher amount of transfer.

1.6.4 Discussion

Adjusting these distinctive traits of married women would result in the dissipation of the policy effects, rendering the marginal effects essentially close to zero. However, it does not necessarily mean that the baby bonus program is useless as the policy does incentivize people to expedite the first delivery after marriage, approximately 8 months reduction. My preferred estimation result is reported in Column (2) of Table 1.10. According to the preferred specification, the baby bonus program would have helped in raising birth probability for the third child, but the effect is not higher than an additional 0.5 percentage point increase even with 7,000 USD transfer, which is 7 times higher than the average baby bonus for the third child. Yet, the sum of the baby bonus could have increased the birth likelihood by 0.6 to 0.7 percentage points, which can be valid if females plan to have three children in their life and account for these transfer amounts at the same time. Overall, the baby bonus does not seem to substantially raise the likelihood of having a child. This, in turn, would imply that the government's transfer was received by a woman who already planned to have a child regardless of the birth order. Yet, the policy still seems to have a marginal impact on helping females deliver their first child earlier. This can be a partial success of the policy in terms of health (Jacobsson et al. 2004; Liu et al. 2011) and the younger population pyramid, slowing down the speed of entry into an aging society Mathews (2009).

1.7 Additional Exercise

I conduct an extended analysis concerning whether the previous results remain under different situations. In Section 1.7.1, females who exhibit potential premarital conception are excluded to isolate the effect of the marriage promotion budget. Next, Section 1.7.2 focuses on the subgroup of females whose income is below the 50th percentile. Table 1.17 shows that selection bias largely exists in the specifications used in Section 1.7.1 and 1.7.2, respectively. Females seem to select into marriage even when I restrict the female sample, which showed a birth that is likely to be conceived before marriage. The test static is statistically significant when it comes to females with less wealth, buttressing the necessity of selection bias correction in childbirth estimations.

1.7.1 Premarital Births

Some females give birth to a child within a short time after their marriage. These females are not necessarily a counterexample of selection into marriage, as they still decide to be married before their childbirth. A social norm likely forces them to form a partnership to be a parent, so they are still considered to be attached to traditional norms. However, the existence of this subgroup could make the estimates of the policy variables biased as the channel of interest does not apply for this group. It is likely that the importance of the marriage promotion budget would be overstated if females do not take account of marriage before pregnancy. Thus, I drop 83 individuals (out of 2,677 females) that exhibit childbirth within 9 months after marriage and estimate the model using this subsample of these females.

Tables 1.18 and 1.19 report the estimation results of women's marriage and childbirth decisions, respectively, and the estimated results are very similar. Marriage likelihood is increased by 0.5 percentage points (2.48% increase) at the average level, and the baby bonus program does not seem to affect women's marriage decisions. The other variables show almost identical results in terms that her age, years of schooling, bad relationship with her parents, and father's education level are key determinants of females' marriage decisions. Yet, the bad relationship with her parents increases marriage likelihood by 7.8 percentage points, which is higher than in the main sample, and females whose fathers completed at least a college education have a lower chance of being married by 5.2 percentage points. As to childbirth decisions reported in Table 1.19, the effect of the baby bonus program is not statistically significant for each parity birth again, but the sum of this remains significant. Other than her and her husband's ages, there is weak evidence that longer hours of work are associated with a 1.3 percentage point increase in birth probability.

The use of this subsample aims to isolate the effect of the MPB as those females with premarital conception might not be affected by the MPB when they decided to get married. To that end, the existence of these females may or may not overstate the policy effect of the MPB in marriage outcome estimation. Therefore, dropping those females with potential premarital conception would give us more conservative estimates of the policy effects. However, even after excluding those females, the estimated effect of the MPB is still a 0.5 percentage point increase in marriage likelihood. Findings also show that the effect of the baby bonus program on the marriage decision is minimal. The baby bonus program is unlikely to have increased either the marriage rate or the birth rates.

1.7.2 Low Income Females

One of the key assumptions underlying the inception of pro-natalist policies is to provide financial subsidies to relax females' budget constraints to create a price effect. The theory predicts a bigger and statistically significant impact of the pro-natalist benefits for the less wealthy, as the financial incentives would be relatively greater for those females (Becker 1960; Becker and Lewis 1973). To investigate if this holds for females in South Korea, I focus on the subsample of females whose income is less than the 50th percentile, which results in 3,706 observations in my data.

Tables 1.20 and 1.21 show the estimation results using female samples with income under the 50th percentile. As to marriage decisions, the marriage promotion budget is, indeed, more effective for this female group, having a slightly higher effect for this subgroup of females. The chance to be married is increased by 0.6 percentage points, which corresponds to a 1.9% increase in marriage likelihood. The baby bonus, on the other hand, shows no substantial effects, and the estimated effects are smaller in magnitude than those of the main analysis in Section 1.6.2. The estimated effects of the baby bonus are almost close to zero, and the sum of the baby bonus amounts has an even smaller effect than the main sample. This suggests that the baby bonus program is not effective in creating price effects even for the females in more need, but this is not necessarily true for the MPB.

1.8 Conclusion

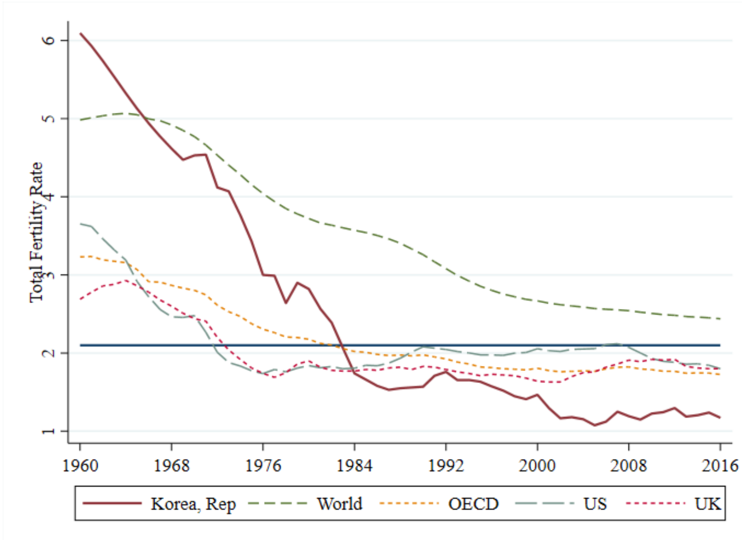
I examine the effect of the baby bonus operated by the local governments in South Korea in two directions: changes in birth likelihood and reduction in time-to-childbirth. In South Korea, marriage is likely a precondition to childbearing, so that the social norm is likely to force females to select into marriage. Some unobserved factors, such as more attachment to traditional life, affect females' decision to enter into marriage, thereby making females systematically different depending on her marital status. Presumably, married women would have had a higher attachment to Familism and traditional norms, which makes them have greater demands for children as well. When selection bias is accounted for, the overall estimation results suggest that the baby bonus has not been effective in increasing the probability of having a child. The baby bonuses for each parity birth do not seem to change her family plan, while the estimations without considering selection into marriage overstate the effect of the baby bonus for the first child. The decomposition of the quantum effects reveals that the baby bonus program does not have a significant direct or indirect impact, but the marriage promotion budget might have led to more maternity by helping the formation of a family. Yet, the baby bonus program seems to have a tempo effect: A 1,000 USD baby bonus for the first child shortened time-to-first delivery after marriage approximately by 8 months, while the baby bonus for the second child does not seem to affect the timing of their second delivery.

The findings also suggest that females' marriage decisions could be modified by the policy. The marriage likelihood due to the MPB increases by 2.48% increase with respect to the average marriage outcome at the average level even after isolating the effect of the MPB by excluding females with potential premarital conception. After excluding premarital conception, which could cause upward bias, the MPB still shows the policy effect. Also, females in more need of financial resources are more responsive to marriage benefits by 0.1 percentage points. In addition to changes in marriage likelihood, time-to-marriage is reduced by about 2.78 months with a 1,000 USD MPB. This contrasts with the baby bonus program, which only has the timing effect for their first child. These findings suggest that the ultimate goal of the policy packages that are increasing birth rates does not seem to be achieved; yet, marriage, a pre-requisite for childbirth in South Korea, is altered by marriage promoting programs.

This study has limitations in that the policy variable of the second interest, the marriage promotion budget, is not thoroughly investigated. It is not feasible to obtain the marriage promotion programs in detail because the marriage decision itself has not yet been a central interest for policy-makers. Therefore, the marriage policy's precise operation is not described in the paper due to insufficient information in the primary source data besides the allocated budget. Also, my research does not discuss the existence of the females who would have had a child in the absence of governmental benefits. However, when the large fraction of the potential mothers is the previous case, this creates a considerable expense of governmental budget without showing any policy effect. Given the weak impact of the baby bonus program, it might be the case in which the government paid to those females, which needs to be examined further.

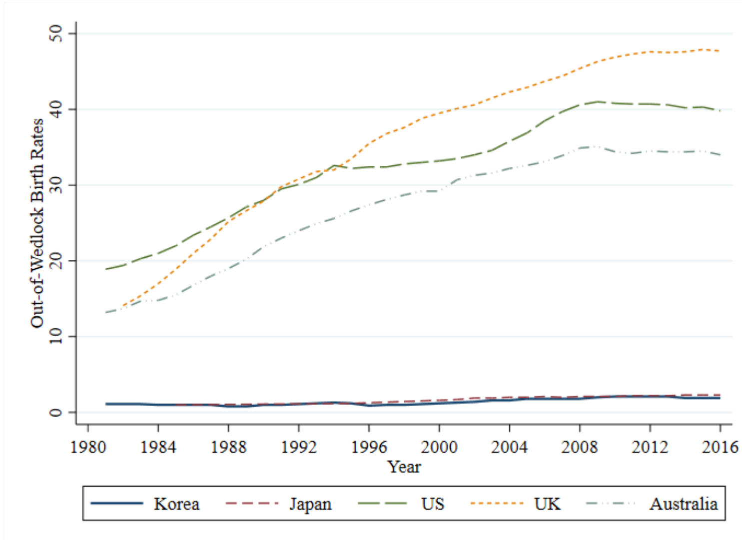
1.9 Figures

Figure 1.1: Total Fertility Rate



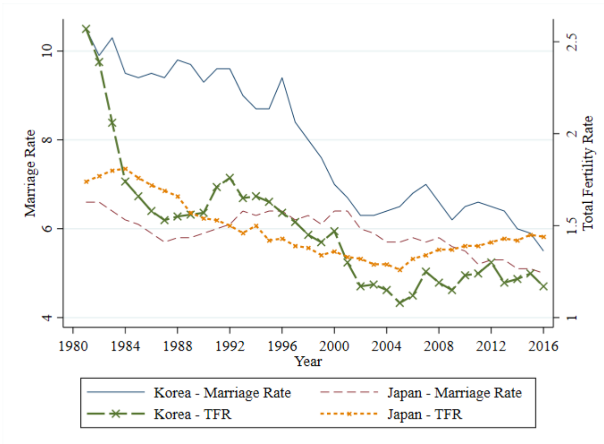
Source: the OECD family database. The blue horizontal line in Figure 1.1 represents the replacement level fertility, which is the total fertility rate at which a population replaces itself from one generation to the next generation solely without having migration.

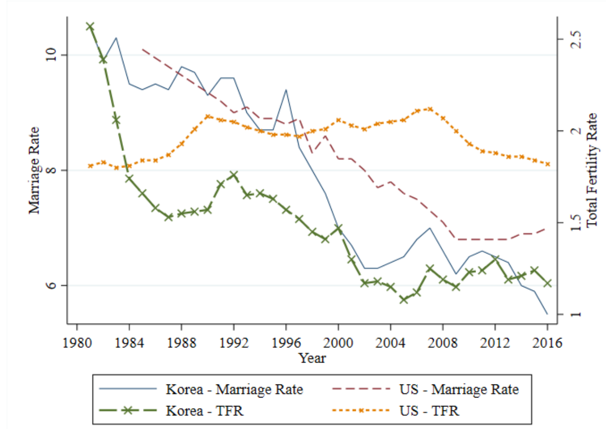
Figure 1.2: Out-of-Wedlock Birth Rates



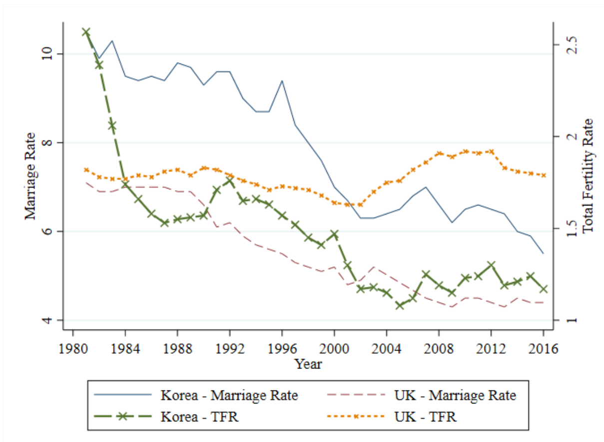
Source: the OECD family database.

Figure 1.3: Marriage Rate and Total Fertility Rate

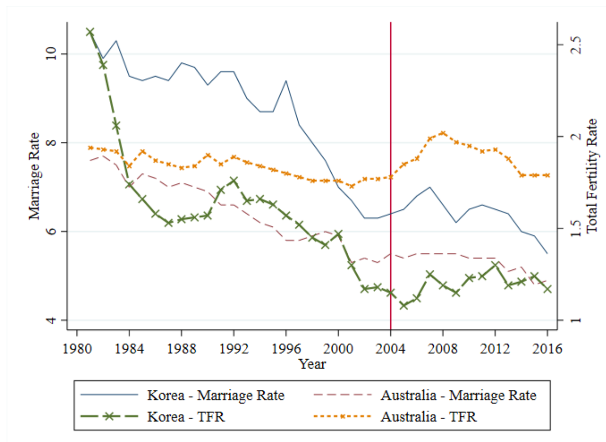




(a) South Korea - United States



(b) South Korea - United Kingdom



(c) South Korea - Australia

Source: the OECD family database. The red vertical line in Figure (1.3c) points out the inception of the baby bonus in Australia.

Figure 1.4: Districts with Baby Bonus Program

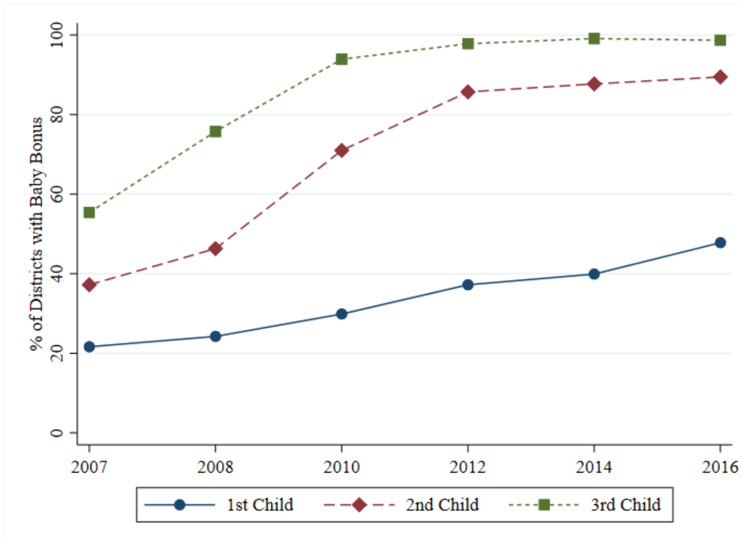
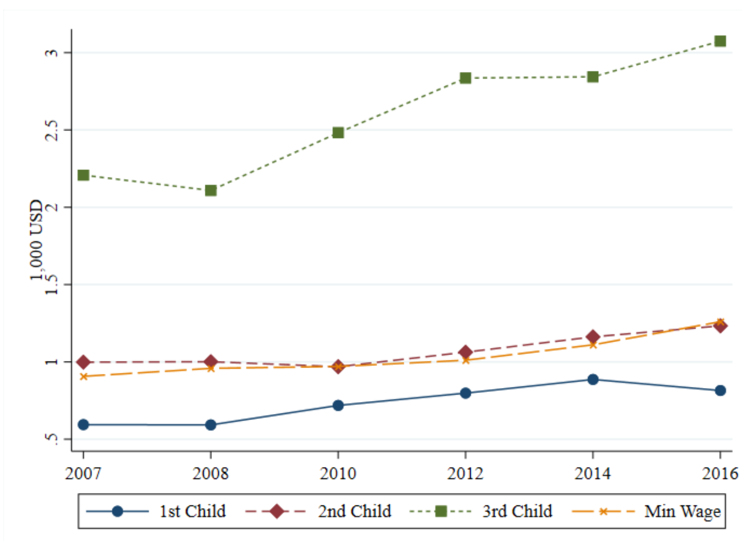


Figure 1.5: Average Transfer Amount



The amounts are averaged using the districts operating the baby bonus program. Thus, the rest of the districts without program are dropped in the figure.

Figure 1.6: Time to Marriage

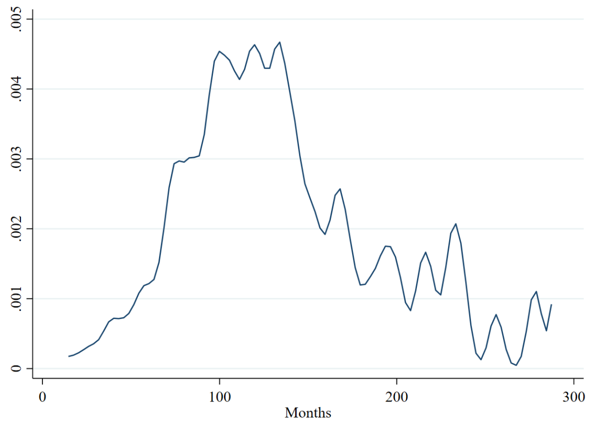


Figure 1.7: Time to First Delivery

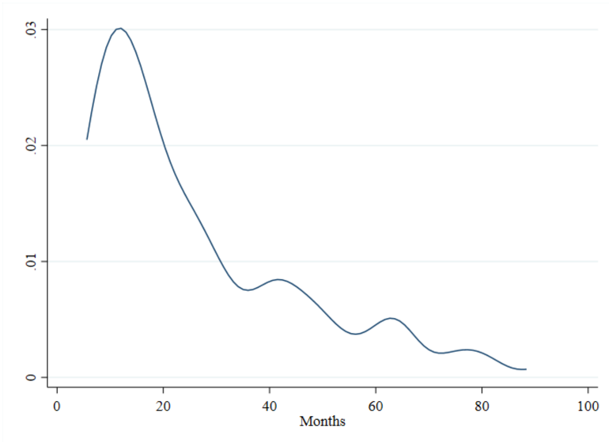
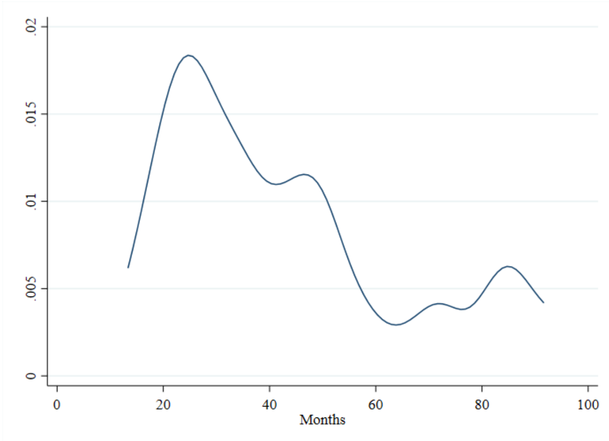
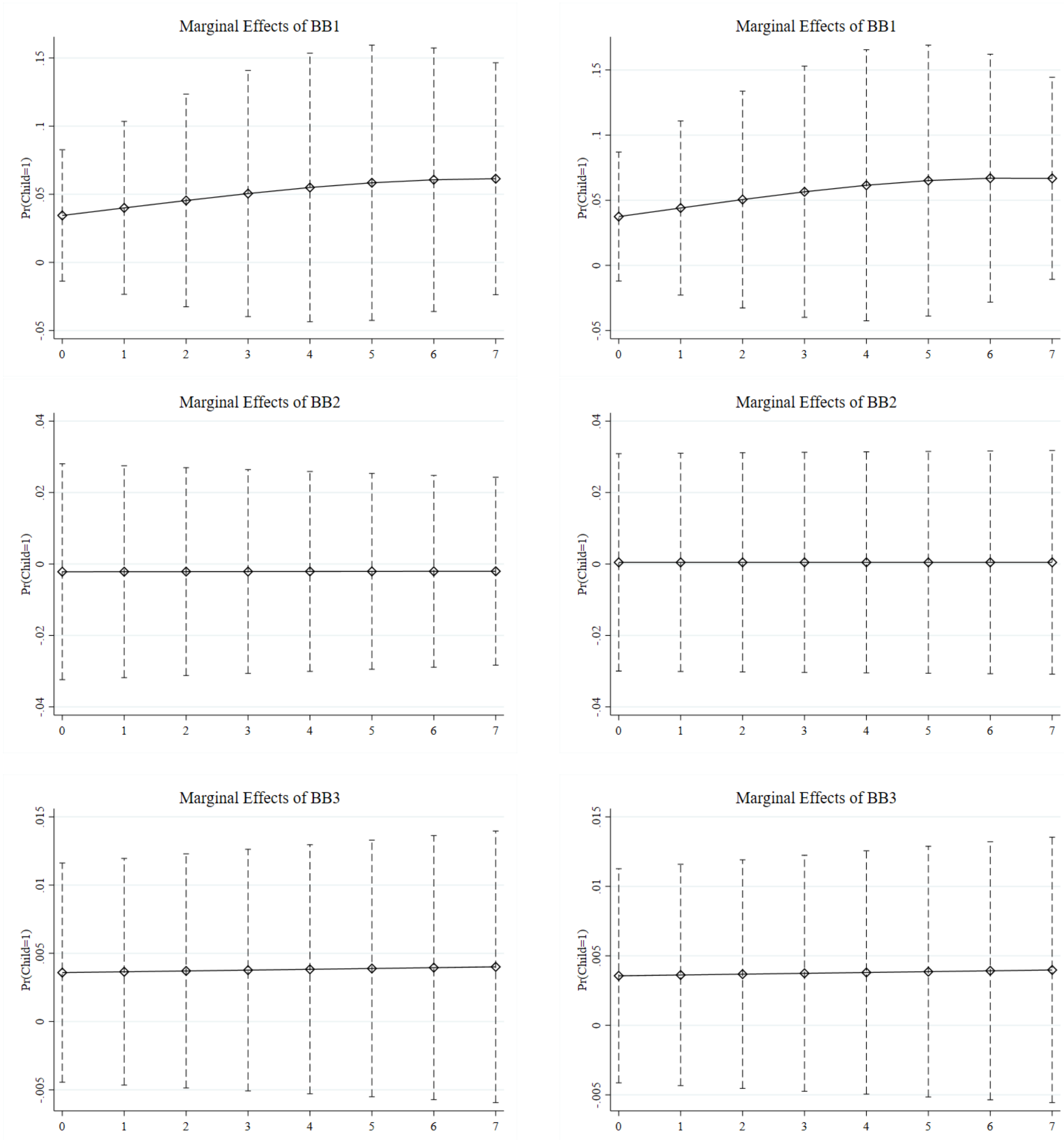


Figure 1.8: Time to Second Delivery



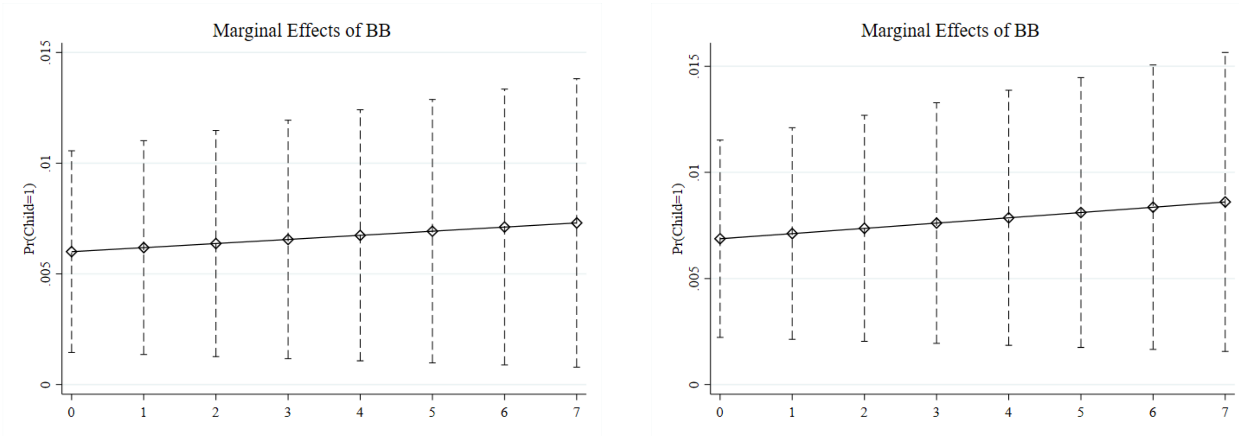
Figures are the smoothed hazard estimates at each specific month. Figure 1.6 is measured in months to marriage after females become 20 years old, Figure 1.7 is time to first delivery after marriage, and Figure 1.8 is time to second delivery after first delivery.

Figure 1.9: Marginal Effects of A Set of Baby Bonuses



Figures report the marginal effects of the baby bonus for each parity evaluated at each possible transfer level (measured in 1,000 USD). The left and right panels show the marginal effects of the BBs based on specifications used in Columns (2) and (4) in Table 1.10, respectively.

Figure 1.10: Marginal Effects of the Sum of Baby Bonuses



Figures report the marginal effects of the baby bonus for each parity evaluated at each possible transfer level (measured in 1,000 USD). The left and right panels show the marginal effects of the BBs based on specifications used in Columns (6) and (8) in Table 1.10, respectively.

1.10 Tables

Table 1.1: Eligibility Condition

	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
At the time of birth/registration	21	26	40	42	47	50	49	51	52	54
1 Month	2	3	4	5	7	7	7	9	12	8
90 Days	0	0	0	1	1	1	1	1	1	1
3 Months	13	16	21	22	24	24	24	25	23	23
180 Days	3	5	6	7	8	9	9	9	10	9
6 Months	27	29	35	40	50	52	52	50	46	48
10 Months	0	1	2	2	2	2	2	2	2	2
12 Months	31	45	53	59	51	50	55	55	59	61
Others	0	0	0	2	2	3	3	3	3	3
Not Available	131	103	67	48	36	30	24	20	17	16

The eligibility condition in terms of length of residency is reported. Four districts in Jeju island are combined as one district. Two districts in Others have different residency requirements for different parity births. The other requires two years of residency in the district to be eligible for the benefit. Not available indicate the districts where do not specify the residency requirements in their municipal ordinance or have no baby bonus program.

Table 1.2: Effect of Baby Bonuses on Migration

	Net Migration
BB1	0.065
	(0.050)
BB2	0.023
	(0.043)
BB3	-0.002
	(0.015)
Constant	-54.322**
	(27.341)
Observations	2,483

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Table shows the effect of the baby bonus package on the number of migrations in a district using the fixed effect model.
The net migration is obtained from KOSIS, the national statistical database, operated by Statistics Korea. KOSIS provides a net migration flow of the districts in South Korea. The yearly measured net migration flow is used for the estimation to examine whether the differences in the baby bonus amounts affect the migration flows across the districts.

Table 1.3: Summary Statistics of Quantum Effects

	2008	2010	2012	2014	2016	All Year
1[Married]	0.266	0.258	0.285	0.315	0.322	0.290
	(0.442)	(0.438)	(0.452)	(0.465)	(0.467)	(0.454)
1[Childbirth]	0.124	0.0968	0.0802	0.0694	0.0538	0.0837
	(0.329)	(0.296)	(0.272)	(0.254)	(0.226)	(0.277)
BB1	0.0801	0.106	0.121	0.107	0.121	0.107
	(0.345)	(0.406)	(0.423)	(0.404)	(0.413)	(0.400)
BB2	0.292	0.432	0.531	0.602	0.563	0.488
	(0.807)	(0.806)	(0.871)	(0.800)	(0.737)	(0.811)
BB3	1.020	1.785	2.289	1.996	1.831	1.790
	(2.183)	(2.114)	(2.480)	(2.092)	(1.920)	(2.197)
BB	1.392	2.323	2.941	2.705	2.515	2.386
	(2.970)	(2.962)	(3.319)	(2.878)	(2.715)	(3.013)
MPB	0.567	1.213	1.181	0.694	2.044	1.164
	(1.408)	(1.699)	(1.508)	(1.330)	(5.708)	(3.034)
Age	27.45	27.87	28.25	29.12	29.28	28.43
	(6.036)	(6.350)	(6.588)	(7.037)	(7.275)	(6.730)
Years of Schooling	14.60	14.72	14.80	14.84	14.89	14.78
	(1.923)	(1.892)	(1.860)	(1.889)	(1.792)	(1.872)
HH: Income	70.32	62.37	61.53	53.74	55.39	60.44
	(42.37)	(35.72)	(43.82)	(35.31)	(29.55)	(37.93)
P: Good Relationship	0.520	0.510	0.506	0.510	0.511	0.511
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
P: Bad Relationship	0.0632	0.0682	0.0581	0.0572	0.0568	0.0606
	(0.243)	(0.252)	(0.234)	(0.232)	(0.232)	(0.239)
P: Mother College +	0.0732	0.0975	0.120	0.137	0.175	0.122
	(0.260)	(0.297)	(0.325)	(0.344)	(0.380)	(0.328)
P: Father College +	0.158	0.189	0.219	0.238	0.276	0.218
	(0.365)	(0.391)	(0.414)	(0.426)	(0.447)	(0.413)
Observations	1408	1436	1446	1485	1636	7411

Summary statistics for all women in the data from 2008 and 2016. The last column displays the summary statistics of all year. Table displays means and standard errors in parentheses. All monetary amounts are reported in thousands of USD.

Table 1.4: Summary Statistics of Quantum Effects - Married Women

	2008	2010	2012	2014	2016	All Year
1[Childbirth]	0.369	0.305	0.184	0.173	0.163	0.230
	(0.483)	(0.461)	(0.388)	(0.379)	(0.370)	(0.421)
BB1	0.117	0.153	0.131	0.116	0.143	0.132
	(0.525)	(0.581)	(0.479)	(0.425)	(0.453)	(0.489)
BB2	0.356	0.475	0.517	0.652	0.608	0.533
	(0.876)	(0.957)	(0.947)	(0.886)	(0.812)	(0.897)
BB3	1.233	1.897	2.230	2.084	1.950	1.899
	(2.493)	(2.364)	(2.470)	(2.269)	(2.232)	(2.376)
BB	1.706	2.526	2.878	2.853	2.702	2.565
	(3.448)	(3.553)	(3.427)	(3.166)	(3.129)	(3.349)
MPB	0.613	1.075	1.302	0.663	3.112	1.447
	(1.451)	(1.619)	(1.656)	(0.943)	(7.972)	(4.247)
Age	31.26	32.61	33.73	34.79	35.95	33.88
	(4.559)	(4.453)	(4.278)	(4.319)	(4.092)	(4.622)
Years of Schooling	14.09	14.16	14.30	14.40	14.46	14.30
	(2.137)	(2.075)	(1.996)	(2.020)	(2.043)	(2.054)
HH: Income	70.72	62.30	55.93	53.29	54.78	58.74
	(36.48)	(32.11)	(28.58)	(43.64)	(27.61)	(34.68)
P: Good Relationship	0.487	0.472	0.476	0.494	0.505	0.488
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
P: Bad Relationship	0.0829	0.0916	0.0825	0.0748	0.0702	0.0795
	(0.276)	(0.289)	(0.275)	(0.263)	(0.256)	(0.271)
P: Mother College +	0.0535	0.0593	0.0461	0.0406	0.0474	0.0488
	(0.225)	(0.237)	(0.210)	(0.198)	(0.213)	(0.215)
P: Father College +	0.107	0.102	0.102	0.0940	0.0987	0.100
	(0.309)	(0.304)	(0.303)	(0.292)	(0.299)	(0.301)
H: Age	34.21	35.50	36.66	37.64	38.71	36.75
	(5.507)	(5.219)	(5.004)	(4.831)	(4.810)	(5.288)
H: Years of Schooling	14.52	14.57	14.64	14.78	14.85	14.69
	(2.357)	(2.298)	(2.211)	(2.104)	(2.164)	(2.220)
H: Monthly Salary	4.655	4.480	3.815	3.443	3.501	3.918
	(2.055)	(3.630)	(1.598)	(1.683)	(1.471)	(2.207)
H: Hours Worked	7.472	8.941	8.896	8.801	8.736	8.596
	(2.070)	(1.484)	(1.568)	(1.401)	(1.344)	(1.653)
HH: Own House	0.463	0.553	0.561	0.564	0.641	0.563
	(0.499)	(0.498)	(0.497)	(0.496)	(0.480)	(0.496)
HH: Live with Parents	0.0882	0.127	0.104	0.109	0.0968	0.105
	(0.284)	(0.333)	(0.306)	(0.312)	(0.296)	(0.306)
Observations	374	371	412	468	527	2152

Summary statistics for married women in the data from 2008 and 2016. The last column displays the summary statistics of all year. Table displays means and standard errors in parentheses.

Table 1.5: Time-to-Events Summary Statistics

	Full Sample	Aged 20 between 1991 and 2011		
Starting Time	Marriage Age 20	Marriage Age 20	First Delivery Marriage	Second Delivery First Delivery
Number of Observed Events (A)	419	112	339	184
Number of Total Events (B)	2542	1470	654	490
(A/B)%	16.48%	7.61%	51.83%	37.55%
Average Months to	102.853	105.301	40.317	47.616

Table shows the descriptive statistics of the time-to-event analysis. The first and second rows report the number of observed events and the number of total events, respectively. The third row reports the proportion of the observed events. The Last row presents the mean duration to the event from the starting time.

Table 1.6: Summary Statistics of Covariates for Tempo Effects

Starting Time	Marriage Age 20	First Delivery Marriage	Second Delivery First Delivery
MPB	0.0569	0.938	1.046
	(0.506)	(2.395)	(2.960)
BB1	0.0806	0.124	0.154
	(0.354)	(0.503)	(0.568)
BB2	0.262	0.397	0.484
	(0.666)	(0.768)	(0.881)
Age	25.29	29.82	30.40
	(4.301)	(3.743)	(3.996)
Age at Marriage		28.81	28.46
		(3.532)	(3.424)
Age at First Delivery			29.39
			(3.896)
Years of Schooling	14.85	14.36	14.30
	(1.744)	(1.986)	(2.003)
P: Good Relationship	0.507	0.492	0.490
	(0.500)	(0.500)	(0.500)
P: Bad Relationship	0.157	0.183	0.190
	(0.364)	(0.387)	(0.393)
P: Father College +	0.215	0.102	0.101
	(0.411)	(0.303)	(0.302)
P: Mother College +	0.107	0.0443	0.0445
	(0.309)	(0.206)	(0.206)
HH: Income	3.983	3.906	3.968
	(0.885)	(0.752)	(0.499)
H: Age		31.96	32.92
		(6.077)	(5.585)
H: Years of Schooling		14.59	14.69
		(2.798)	(2.559)
H: Monthly Salary		3.844	3.932
		(1.917)	(1.740)
H: Hours Worked		7.881	8.266
		(2.365)	(1.940)
HH: Own House		0.442	0.504
		(0.497)	(0.500)
HH: Live with Parents		0.148	0.150
		(0.360)	(0.357)
Observations	1470	654	494

Table reports the average of the variables using the subsample of the data used for the duration analysis. SE in parentheses.
For child delivery, husband-related variables are included in the regression. Also, the ages at the previous events are added.

Table 1.7: Tests for Selection Bias using the Main Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Main Data	0.570	1.430**	0.586	1.397**	0.556	1.416**	0.565	1.425**
	(0.594)	(0.600)	(0.577)	(0.575)	(0.588)	(0.592)	(0.573)	(0.582)
Outcome Equation								
BB1	YES	YES	YES	YES	NO	NO	NO	NO
BB2	YES	YES	YES	YES	NO	NO	NO	NO
BB3	YES	YES	YES	YES	NO	NO	NO	NO
BB1+BB2+BB3	NO	NO	NO	NO	YES	YES	YES	YES
Selection Equation								
MPB	YES	YES	YES	YES	YES	YES	YES	YES
BB1	NO	NO	YES	YES	NO	NO	NO	NO
BB2	NO	NO	YES	YES	NO	NO	NO	NO
BB3	NO	NO	YES	YES	NO	NO	NO	NO
BB1+BB2+BB3	NO	NO	NO	NO	NO	NO	YES	YES
Female Demographic	YES	YES	YES	YES	YES	YES	YES	YES
Husband-related	NO	YES	NO	YES	NO	YES	NO	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Weights	YES	YES	YES	YES	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Table reports the test results for selection bias using the specification in Table 1.10. The first section reports the test results, and the rest shows what variables are controlled for the test.

Table 1.8: Effects of MPB and BB on Marriage Decision WITH Selection Bias Correction

	All Sample			
	(1)	(2)	(3)	(4)
MPB	0.005***	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
BB1		0.010		
		(0.024)		
BB2		0.012		
		(0.013)		
BB3		-0.001		
		(0.005)		
BB				0.003
				(0.003)
Age	0.015***	0.015***	0.015***	0.015***
	(0.005)	(0.005)	(0.005)	(0.005)
Years of Schooling	-0.030**	-0.030**	-0.030**	-0.030**
	(0.013)	(0.013)	(0.013)	(0.013)
P: Good Relationship	-0.004	-0.004	-0.004	-0.004
	(0.018)	(0.018)	(0.018)	(0.018)
P: Bad Relationship	0.060*	0.060*	0.060*	0.060*
	(0.032)	(0.032)	(0.032)	(0.032)
P: Mother College +	0.033	0.033	0.033	0.033
	(0.038)	(0.038)	(0.038)	(0.038)
P: Father College +	-0.059**	-0.059**	-0.059**	-0.059**
	(0.029)	(0.029)	(0.029)	(0.029)
HH: Income	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Weights	YES	YES	YES	YES
Province FE	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	7411	7411	7411	7411
Pseudo Log Likelihood	-2.053	-1.942	-2.055	-1.945

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Bootsrapped SE in parentheses.

Dependent variable: marital status. Average partial effects refer to the average effect of a change in each covariate on the probability of being married.

Notes: Table reports the APE estimates of the marriage decision using Equations (1.11) and (1.12) and the main data. Odd columns include only immediate policy benefit (MPB), while even columns include all policy packages (MPB, BB) that she is eligible for.

Table 1.9: Effects of MPB and BB on Marriage Decision WITHOUT Selection Bias Correction

	All Sample		
	(1)	(2)	(3)
MPB	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)
BB1		0.013	
		(0.024)	
BB2		0.009	
		(0.013)	
BB3		-0.000	
		(0.005)	
BB			0.003
			(0.004)
Age	0.022***	0.022***	0.022***
	(0.001)	(0.001)	(0.001)
Years of Schooling	-0.015***	-0.015***	-0.015***
	(0.005)	(0.005)	(0.005)
P: Good Relationship	-0.004	-0.004	-0.004
	(0.018)	(0.018)	(0.018)
P: Bad Relationship	0.056*	0.056*	0.056*
	(0.032)	(0.032)	(0.032)
P: Mother College +	0.031	0.032	0.031
	(0.038)	(0.038)	(0.038)
P: Father College +	-0.058*	-0.058**	-0.058*
	(0.030)	(0.030)	(0.030)
HH: Income	0.001***	0.001***	0.001***
	(0.000)	(0.000)	(0.000)
Weights	YES	YES	YES
Province FE	YES	YES	YES
Regional Time Trend	YES	YES	YES
Year FE	YES	YES	YES
Observations	7411	7411	7411
Pseudo Log Likelihood	-1.728	-1.726	-1.727

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.

Dependent variable: marital status. Average partial effects refer to the average effect of a change in each covariate on the probability of being married.

Notes: Table reports the APE estimates of the marriage decision without sample selection bias correction using the main data. The first column includes only immediate policy benefit while the rest columns include all policy packages in time $t-1$.

Table 1.10: Effects of BB on Birth Likelihood WITH Selection Bias Correction

	Married Women							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BB1	0.014	0.035	0.016	0.038				
	(0.026)	(0.028)	(0.026)	(0.029)				
BB2	-0.002	-0.002	0.002	0.000				
	(0.017)	(0.017)	(0.017)	(0.018)				
BB3	-0.002	0.004	-0.002	0.004				
	(0.006)	(0.005)	(0.006)	(0.005)				
BB					0.000	0.006*	0.001	0.007**
					(0.003)	(0.003)	(0.003)	(0.004)
Age	-0.010	0.030***	-0.010	0.030***	-0.010	0.029***	-0.010	0.029***
	(0.024)	(0.011)	(0.024)	(0.011)	(0.024)	(0.011)	(0.024)	(0.011)
Years of Schooling	-0.003	-0.025	-0.003	-0.025	-0.003	-0.025	-0.003	-0.025
	(0.036)	(0.055)	(0.034)	(0.055)	(0.036)	(0.055)	(0.036)	(0.055)
P: Good Relationship	0.006	0.001	0.006	0.001	0.006	0.001	0.006	0.001
	(0.017)	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)	(0.019)	(0.018)
P: Bad Relationship	0.009	0.010	0.009	0.010	0.010	0.013	0.010	0.013
	(0.037)	(0.026)	(0.037)	(0.035)	(0.026)	(0.035)	(0.026)	
P: Mother College +	-0.017	-0.005	-0.015	-0.005	-0.016	-0.005	-0.016	-0.005
	(0.051)	(0.050)	(0.052)	(0.049)	0.013	0.023	0.013	0.023
P: Father College +	0.015	0.024	0.013	0.024	0.013	0.023	0.013	0.023
	(0.043)	(0.030)	(0.045)	(0.029)	(0.044)	(0.030)	(0.043)	(0.030)
HH: Income	-0.000	-0.001	-0.000	-0.001	-0.000	-0.001	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
H: Age		-0.030***		-0.030***		-0.029***		-0.029***
		(0.010)		(0.010)		(0.010)		(0.010)
H: Years of Schooling		0.017		0.017		0.021		0.021
		(0.059)		(0.058)		(0.058)		(0.058)
H: Monthly Salary		0.001		0.001		0.001		0.001
		(0.005)		(0.005)		(0.005)		(0.005)
H: Hours Worked		0.011*		0.011*		0.011*		0.011*
		(0.006)		(0.006)		(0.006)		(0.006)
HH: Own House		-0.003		-0.003		-0.002		-0.002
		(0.028)		(0.028)		(0.028)		(0.028)
HH: Live with Parents		-0.000		-0.000		-0.001		-0.001
		(0.064)		(0.063)		(0.063)		(0.062)
Selection Equation	(1)	(1)	(2)	(2)	(3)	(3)	(4)	(4)
Weights	YES	YES	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Husband-related	NO	YES	NO	YES	NO	YES	NO	YES
Observations	2152	2152	2152	2152	2152	2152	2152	2152
Pseudo Log Likelihood	-2.319	-2.192	-2.313	-2.186	-2.322	-2.195	-2.319	-2.192

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Bootsrapped SE in parentheses (500 repetitions).
Dependent variable: childbirth outcome. Average partial effects refer to the average effect of a change in each covariate on the probability of having a child.
Notes: Table shows the APEs of the policy packages on childbirth outcomes obtained by using Equations (1.11) and (1.12). Selection equation indicates the specification of the marriage decision variables reported in Table 1.8. A set of the baby bonus amounts is used in the first four columns, and the policy variable is replaced with the sum of the baby bonus in the following four columns. Odd columns control for the demographic characteristics of females, and even columns additionally control for her husband-related characteristics. All specifications include weights, province and year fixed effects, and regional time trend.

Table 1.11: Effects of BB on Birth Likelihood WITHOUT Selection Bias Correction

	All Sample				Married Women					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BB1	-0.001	-0.001			0.048	0.059*			0.059*	
	(0.010)	(0.010)			(0.036)	(0.035)			(0.035)	
BB2	0.007	0.007			-0.012	-0.017			-0.017	
	(0.007)	(0.007)			(0.024)	(0.023)			(0.022)	
BB3	0.002	0.002			-0.004	0.002			0.002	
	(0.002)	(0.002)			(0.008)	(0.007)			(0.007)	
BB			0.003*	0.003*			0.001	0.006		0.006
			(0.001)	(0.001)			(0.004)	(0.004)		(0.004)
MPB		0.000		0.000					0.000	-0.000
		(0.001)		(0.001)					(0.003)	(0.003)
Age	-0.002***	-0.002***	-0.002***	-0.002***	-0.021***	-0.011***	-0.021***	-0.011***	-0.011***	-0.011***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
Years of Schooling	0.002	0.002	0.002	0.002	0.008	-0.003	0.009	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.008)	(0.006)	(0.008)	(0.008)	(0.008)
P: Good Relationship	-0.002	-0.002	-0.002	-0.002	0.014	0.017	0.013	0.016	0.017	0.016
	(0.008)	(0.008)	(0.008)	(0.008)	(0.025)	(0.023)	(0.025)	(0.024)	(0.023)	(0.024)
P: Bad Relationship	-0.009	-0.009	-0.009	-0.009	-0.026	-0.029	-0.025	-0.027	-0.029	-0.027
	(0.014)	(0.014)	(0.014)	(0.014)	(0.036)	(0.037)	(0.036)	(0.037)	(0.037)	(0.037)
P: Mother College +	-0.018	-0.018	-0.018	-0.018	-0.065	-0.083*	-0.066	-0.083*	-0.083*	-0.083*
	(0.015)	(0.015)	(0.015)	(0.015)	(0.051)	(0.048)	(0.051)	(0.047)	(0.048)	(0.047)
P: Father College +	0.014	0.014	0.014	0.014	0.078**	0.109***	0.077*	0.107***	0.109***	0.107***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.039)	(0.040)	(0.039)	(0.040)	(0.040)	(0.040)
HH: Income	-0.000	-0.000	-0.000	-0.000	-0.000	-0.001	-0.000	-0.001	-0.001	-0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	0.120***	0.120***	0.120***	0.120***						
	(0.010)	(0.010)	(0.010)	(0.010)						
H: Age						-0.009**		-0.008**	-0.009**	-0.008**
						(0.003)		(0.003)	(0.003)	(0.003)
H: Years of Schooling						0.007		0.007	0.007	0.007
						(0.005)		(0.005)	(0.005)	(0.005)
H: Monthly Salary						0.003		0.003	0.003	0.003
						(0.004)		(0.004)	(0.004)	(0.004)
H: Hours Worked						0.008		0.008	0.008	0.008
						(0.006)		(0.006)	(0.006)	(0.006)
HH: Own House						0.007		0.007	0.007	0.007
						(0.022)		(0.022)	(0.022)	(0.022)
HH: Live with Parents						0.034		0.035	0.035	0.035
						(0.041)		(0.041)	(0.041)	(0.041)
Weights	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Province Time Trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Husband-related	NO	NO	NO	YES	NO	YES	NO	YES	NO	YES
Observations	7411	7411	7411	7411	2152	2152	2152	2152	2152	2152
Pseudo Log Likelihood	-0.971	-0.971	-0.971	-0.971	-0.639	-0.615	-0.640	-0.616	-0.615	-0.616

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.

Dependent variable: childbirth outcome. Average partial effects refer to the average effect of a change in each covariate on the probability of having a child.

Notes: Table depicts the APE of the variables on the childbirth outcome estimation results without accounting for selection bias caused by one's marital status using the main data. Columns (1) through (4) include all women samples in the estimation, and Columns (5) through (10) include married women in the estimation. A set of the baby bonus amounts is used in Columns (1), (2), (5), (6), and (9), and the policy variable is replaced with the sum of the baby bonus in the rest of the columns. From Columns (1) to (8), the odd columns control for the demographic characteristics of females, and even columns additionally control for her husband-related characteristics. All specifications include weights, province and year fixed effects, and regional time trend.

Table 1.12: Decomposition of the Quantum Effects of BB and MPB

	Total Effect	Indirect Effect	Direct Effect
BB1	0.038	0.0023	0.0357
	(0.029)	(0.113)	
BB2	0.000	0.0028	-0.0028
	(0.017)	(0.067)	
BB3	0.004	-0.0002	0.0042
	(0.004)	(0.067)	
MPB		0.0039***	
		(0.006)	
BB	0.007*	0.015	-0.008
	(0.004)	(0.016)	
MPB		0.0023***	
		(0.007)	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Bootsrapped SE in parentheses (500 repetitions).

Notes: The first column shows total effect of the baby bonus program on changes in birth likelihood using the estimates in Columns (4) and (8) in Table 1.10. The next column is the indirect effect of the baby bonus program, using Equation (1.14). The last column displays the direct effects of the baby bonus program in Equation (1.15). In the last row in each section, MPB only shows the indirect effect of the policy, which is obtained based on estimates in Columns (2) and (4) in Table 1.8.

Table 1.13: Time-to-Marriage from Age 20

	CPH			Frailty Model		
	(1)	(2)	(3)	(4)	(5)	(6)
MPB	0.027*	0.027*	0.027*	0.021	0.021	0.022
	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
BB1		0.149	0.162		0.158	0.129
		(0.136)	(0.158)		(0.111)	(0.136)
BB2			-0.017			0.034
			(0.104)			(0.085)
Age	-0.318***	-0.317***	-0.318***	-0.368***	-0.368***	-0.367***
	(0.032)	(0.032)	(0.032)	(0.027)	(0.027)	(0.027)
Years of Schooling	0.001	0.001	0.001	0.000	0.002	0.002
	(0.025)	(0.025)	(0.025)	(0.026)	(0.026)	(0.026)
P: Good Relationship	0.077	0.071	0.072	0.077	0.071	0.069
	(0.110)	(0.111)	(0.110)	(0.112)	(0.112)	(0.112)
P: Bad Relationship	-0.219	-0.222	-0.221	-0.231	-0.232	-0.234
	(0.161)	(0.161)	(0.161)	(0.159)	(0.159)	(0.159)
P: Father College +	-0.427**	-0.423**	-0.423**	-0.465**	-0.461**	-0.461**
	(0.177)	(0.177)	(0.177)	(0.181)	(0.182)	(0.182)
P: Mother College +	-0.029	-0.030	-0.030	-0.057	-0.054	-0.055
	(0.237)	(0.237)	(0.237)	(0.254)	(0.254)	(0.254)
HH: Income	-0.194	-0.195	-0.195	-0.192	-0.190	-0.190
	(0.190)	(0.191)	(0.191)	(0.183)	(0.183)	(0.184)
Constant				-11.306***	-11.353***	-11.352***
				(0.726)	(0.728)	(0.728)
Observed Events	419	419	419	419	419	419
Total Observations	2542	2542	2542	2542	2542	2542
Pseudo Log Likelihood	-2737.554	-2736.709	-2736.688	-2805.363	-2804.562	-2804.509

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$ SE in parentheses.

Dependent variable: time-to-marriage from age 20.

Notes: The first three columns employ the Cox model, and the rest three use the frailty model at the province and district level. Odd columns use the baseline covariates related to females' demographic information, and even columns add husband-related variables. Columns (3), (4), (7), and (8) examine whether females take the subsequent but not imminent policy (BB1) into account.

Table 1.14: Time-to-Marriage from Age 20 using the Cohort sample

	CPH			Frailty Model		
	(1)	(2)	(3)	(4)	(5)	(6)
MPB	0.121***	0.121***	0.121***	0.095***	0.095***	0.095***
	(0.027)	(0.027)	(0.027)	(0.022)	(0.022)	(0.022)
BB1		-0.024	-0.101		0.028	0.064
		(0.332)	(0.370)		(0.287)	(0.342)
BB2			0.079			-0.035
			(0.200)			(0.189)
Age	-0.144***	-0.144***	-0.143***	-0.194***	-0.194***	-0.194***
	(0.031)	(0.031)	(0.031)	(0.035)	(0.035)	(0.035)
Years of Schooling	0.007	0.007	0.007	0.022	0.022	0.022
	(0.046)	(0.046)	(0.046)	(0.052)	(0.052)	(0.052)
P: Good Relationship	-0.023	-0.021	-0.025	0.024	0.022	0.023
	(0.217)	(0.217)	(0.218)	(0.225)	(0.226)	(0.226)
P: Bad Relationship	-0.415	-0.414	-0.422	-0.538*	-0.539*	-0.533*
	(0.323)	(0.325)	(0.328)	(0.320)	(0.320)	(0.321)
P: Father College +	-0.460	-0.461	-0.464	-0.515	-0.514	-0.513
	(0.339)	(0.339)	(0.340)	(0.351)	(0.352)	(0.352)
P: Mother College +	0.144	0.144	0.143	0.154	0.155	0.154
	(0.446)	(0.446)	(0.447)	(0.463)	(0.463)	(0.463)
HH: Income	-0.002	-0.002	-0.002	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Constant				-13.472***	-13.478***	-13.477***
				(1.307)	(1.308)	(1.308)
Observed Events	112	112	112	112	112	112
Total Observations	1470	1470	1470	1470	1470	1470
Log Likelihood	-669.998	-669.995	-669.919	-846.808	-846.803	-846.785

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.
Dependent variable: time-to-marriage from age 20 using the cohort sample.
Notes: The first three columns employ the Cox model, and the rest three use the frailty model at the province and district level. Odd columns use the baseline covariates related to females' demographic information, and even columns add husband-related variables. Columns (3), (4), (7), and (8) examine whether females take the subsequent but not imminent policy (BB1) into account.

Table 1.15: Effect of Baby Bonus on Time-to-First Delivery From Marriage

	CPH				Frailty Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BB1	0.182***	0.205***	0.206***	0.209***	0.192**	0.213**	0.230**	0.204**
	(0.062)	(0.072)	(0.056)	(0.070)	(0.092)	(0.091)	(0.094)	(0.103)
BB2			-0.049	-0.012			-0.066	0.015
			(0.076)	(0.077)			(0.079)	(0.080)
Age	-0.093***	-0.484***	-0.092***	-0.484***	-0.147***	-0.654***	-0.146***	-0.654***
	(0.016)	(0.046)	(0.016)	(0.047)	(0.015)	(0.047)	(0.015)	(0.047)
Age at Marriage		0.429***		0.429***		0.592***		0.592***
		(0.046)		(0.046)		(0.048)		(0.048)
Years of Schooling	0.072**	0.042	0.074**	0.042	0.093***	0.050	0.094***	0.050
	(0.031)	(0.035)	(0.031)	(0.035)	(0.032)	(0.036)	(0.032)	(0.036)
P: Good Relationship	0.010	-0.037	0.011	-0.036	0.033	-0.024	0.033	-0.026
	(0.130)	(0.133)	(0.130)	(0.133)	(0.129)	(0.131)	(0.129)	(0.131)
P: Bad Relationship	-0.227	-0.073	-0.225	-0.073	-0.262	-0.013	-0.258	-0.014
	(0.164)	(0.163)	(0.165)	(0.163)	(0.167)	(0.169)	(0.167)	(0.169)
P: Father College +	0.239	0.169	0.235	0.167	0.308	0.194	0.307	0.196
	(0.187)	(0.202)	(0.188)	(0.203)	(0.212)	(0.211)	(0.212)	(0.211)
P: Mother College +	-0.148	-0.089	-0.144	-0.087	-0.190	-0.073	-0.194	-0.073
	(0.270)	(0.274)	(0.271)	(0.275)	(0.308)	(0.308)	(0.308)	(0.308)
HH: Income	0.215**	0.063	0.207**	0.062	0.307***	0.059	0.296***	0.060
	(0.105)	(0.098)	(0.105)	(0.098)	(0.106)	(0.106)	(0.106)	(0.107)
H: Age		0.004		0.004		0.005		0.005
		(0.012)		(0.012)		(0.013)		(0.013)
H: Years of Schooling		0.026		0.027		0.026		0.026
		(0.025)		(0.025)		(0.027)		(0.028)
H: Monthly Salary		0.045		0.044		0.042		0.043
		(0.030)		(0.030)		(0.031)		(0.031)
H: Hours Worked		0.011		0.011		0.007		0.006
		(0.030)		(0.030)		(0.029)		(0.029)
HH: Own House		0.182		0.182		0.192*		0.191*
		(0.115)		(0.115)		(0.116)		(0.116)
HH: Live with Parents		-0.181		-0.181		-0.190		-0.191
		(0.196)		(0.197)		(0.186)		(0.186)
Constant					-2.175***	-4.844***	-2.140***	-4.839***
					(0.679)	(0.766)	(0.680)	(0.766)
Observed Events	339	339	339	339	339	339	339	339
Total Observations	654	654	654	654	654	654	654	654
Log Likelihood	-1994.499	-1955.172	-1994.340	-1955.162	-1726.490	-1620.064	-1726.145	-1620.048

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.
Dependent variable: time-to-first delivery from marriage.
Notes: The first four columns employ the Cox model, and the rest use the frailty model at the province and district level. Odd columns use the baseline covariates related to females' demographic information, and even columns add husband-related variables. Columns (3), (4), (7), and (8) examine whether females take the subsequent but not imminent policy (BB2) into account.

Table 1.16: Effect of Baby Bonus on Time-to-Second Delivery From First Delivery

	CPH				Frailty Model			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BB2	0.178*	0.155*	0.187	0.166	0.122	0.118	0.151	0.127
	(0.097)	(0.092)	(0.124)	(0.120)	(0.099)	(0.091)	(0.129)	(0.124)
BB3			-0.005	-0.007			-0.016	-0.005
			(0.043)	(0.045)			(0.044)	(0.043)
Age	-0.084***	-0.326***	-0.084***	-0.325***	-0.108***	-0.553***	-0.108***	-0.553***
	(0.021)	(0.056)	(0.021)	(0.056)	(0.022)	(0.064)	(0.022)	(0.064)
Age at First Delivery		0.278***		0.277***		0.515***		0.515***
		(0.056)		(0.056)		(0.061)		(0.061)
Years of Schooling	-0.009	-0.003	-0.009	-0.003	0.006	-0.027	0.006	-0.027
	(0.044)	(0.056)	(0.044)	(0.056)	(0.047)	(0.054)	(0.047)	(0.054)
P: Good Relationship	0.059	0.152	0.059	0.152	0.060	0.097	0.060	0.099
	(0.185)	(0.187)	(0.185)	(0.187)	(0.187)	(0.184)	(0.187)	(0.185)
P: Bad Relationship	-0.115	-0.123	-0.115	-0.122	-0.174	-0.121	-0.171	-0.120
	(0.223)	(0.227)	(0.223)	(0.227)	(0.233)	(0.228)	(0.233)	(0.228)
P: Father College +	0.181	0.203	0.182	0.204	0.256	0.293	0.256	0.292
	(0.357)	(0.342)	(0.357)	(0.342)	(0.335)	(0.327)	(0.335)	(0.328)
P: Mother College +	-0.202	-0.262	-0.204	-0.265	-0.262	-0.375	-0.262	-0.374
	(0.577)	(0.563)	(0.579)	(0.565)	(0.529)	(0.509)	(0.528)	(0.509)
HH: Income	0.229	0.112	0.230	0.113	0.233	0.018	0.235	0.018
	(0.166)	(0.194)	(0.166)	(0.193)	(0.178)	(0.195)	(0.178)	(0.195)
H: Age		-0.036		-0.036		-0.051**		-0.051**
		(0.027)		(0.027)		(0.023)		(0.023)
H: Years of Schooling		-0.043		-0.043		-0.039		-0.038
		(0.047)		(0.047)		(0.045)		(0.045)
H: Monthly Salary		0.017		0.017		0.029		0.029
		(0.017)		(0.017)		(0.025)		(0.025)
H: Hours Worked		0.130***		0.130***		0.172***		0.172***
		(0.038)		(0.038)		(0.042)		(0.042)
HH: Own House		0.334**		0.334**		0.342**		0.342**
		(0.169)		(0.169)		(0.162)		(0.163)
HH: Live with Parents		0.129		0.130		0.076		0.077
		(0.245)		(0.245)		(0.251)		(0.251)
Constant					-4.780***	-7.894***	-4.777***	-7.893***
					(0.989)	(1.231)	(0.989)	(1.232)
Observed Events	184	184	184	184	184	184	184	184
Total Observations	490	490	490	490	490	490	490	490
Log Likelihood	-1008.659	-990.863	-1008.653	-990.853	-1050.707	-994.901	-1050.641	-994.895

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.

Dependent variable: survival analysis of time-to-second delivery from first delivery.

Notes: The first four columns employ the Cox model, and the rest use the frailty model at the province and district level. Odd columns use the baseline covariates related to females' demographic information, and even columns add husband-related variables. Columns (3), (4), (7), and (8) examine whether females take the subsequent but not imminent policy (BB3) into account.

Table 1.17: Tests for Selection Bias of the Subsample of Females

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Premarital Births	0.579	1.531**	0.588	1.424**	0.550	1.516**	0.530	1.490**
	(0.683)	(0.636)	(0.649)	(0.599)	(0.677)	(0.631)	(0.665)	(0.621)
Low Income Females	1.612**	1.763**	1.650**	1.765**	1.635**	1.785***	1.609**	1.769**
	(0.702)	(0.688)	(0.699)	(0.686)	(0.699)	(0.684)	(0.698)	(0.687)
Outcome Equation								
BB1	YES	YES	YES	YES	NO	NO	NO	NO
BB2	YES	YES	YES	YES	NO	NO	NO	NO
BB3	YES	YES	YES	YES	NO	NO	NO	NO
BB1+BB2+BB3	NO	NO	NO	NO	YES	YES	YES	YES
Selection Equation								
MPB	YES	YES	YES	YES	YES	YES	YES	YES
BB1	NO	NO	YES	YES	NO	NO	NO	NO
BB2	NO	NO	YES	YES	NO	NO	NO	NO
BB3	NO	NO	YES	YES	NO	NO	NO	NO
BB1+BB2+BB3	NO	NO	NO	NO	NO	NO	YES	YES
Female Demographic	YES	YES	YES	YES	YES	YES	YES	YES
Husband-related	NO	YES	NO	YES	NO	YES	NO	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Weights	YES	YES	YES	YES	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Table reports the results of the test for selection bias. The first, second, and third rows report the test results of the specification used in Table 1.19 and 1.21, respectively. The first section reports the test results, and the rest shows what variables are controlled for the test.

Table 1.18: Effects of MPB and BB on Marriage Likelihood WITH Selection Bias Correction excluding Premarital Conceptions

	All Sample			
	(1)	(2)	(3)	(4)
MPB	0.005***	0.005***	0.005***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
BB1		0.016		
		(0.023)		
BB2		0.012		
		(0.013)		
BB3		-0.004		
		(0.004)		
BB				0.002
				(0.003)
Age	0.015***	0.015***	0.015***	0.015***
	(0.005)	(0.005)	(0.005)	(0.005)
Years of Schooling	-0.029**	-0.029**	-0.029**	-0.029**
	(0.012)	(0.012)	(0.012)	(0.013)
P: Good Relationship	-0.009	-0.009	-0.009	-0.009
	(0.018)	(0.018)	(0.018)	(0.018)
P: Bad Relationship	0.078**	0.078**	0.078**	0.078**
	(0.032)	(0.032)	(0.032)	(0.032)
P: Mother College +	0.032	0.033	0.032	0.032
	(0.038)	(0.038)	(0.038)	(0.038)
P: Father College +	-0.052*	-0.052*	-0.052*	-0.052*
	(0.029)	(0.029)	(0.029)	(0.029)
HH: Income	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Weights	YES	YES	YES	YES
Province FE	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	7106	7106	7106	7106

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Dependent variable: marital status. Average partial effects refer to the average effect of a change in each covariate on the probability of being married.
Table reports the APE estimates of the marriage decision using Equations (1.11) and (1.12) and the subsample data. Odd columns include only immediate policy benefit while even columns include all policy packages in time $t-1$.

Table 1.19: Effects of BB on Birth Likelihood with Selection Bias Correction excluding Premarital Conceptions

	Married Women							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BB1	0.017	0.025	0.019	0.030				
	(0.037)	(0.022)	(0.026)	(0.023)				
BB2	0.002	0.003	0.005	0.006				
	(0.013)	(0.014)	(0.011)	(0.014)				
BB3	-0.001	0.007	-0.002	0.005				
	(0.006)	(0.005)	(0.004)	(0.005)				
BB					0.002	0.008***	0.002	0.009***
					(0.004)	(0.003)	(0.005)	(0.003)
Age	-0.015	0.030***	-0.012	0.030***	-0.014	0.030***	-0.014	0.030***
	(0.035)	(0.010)	(0.018)	(0.010)	(0.028)	(0.010)	(0.033)	(0.010)
Years of Schooling	0.004	-0.020	0.002	-0.020	0.003	-0.020	0.004	-0.020
	(0.027)	(0.014)	(0.016)	(0.014)	(0.022)	(0.014)	(0.026)	(0.014)
P: Good Relationship	0.008	0.001	0.006	0.001	0.007	0.001	0.008	0.001
	(0.027)	(0.016)	(0.019)	(0.016)	(0.024)	(0.016)	(0.026)	(0.016)
P: Bad Relationship	0.004	0.012	0.006	0.012	0.006	0.013	0.005	0.013
	(0.039)	(0.027)	(0.028)	(0.027)	(0.033)	(0.027)	(0.037)	(0.027)
P: Mother College +	-0.033	-0.001	-0.026	-0.000	-0.031	-0.001	-0.032	-0.001
	(0.083)	(0.041)	(0.052)	(0.041)	(0.071)	(0.041)	(0.083)	(0.041)
P: Father College +	0.037	0.027	0.029	0.027	0.033	0.026	0.034	0.026
	(0.100)	(0.029)	(0.060)	(0.028)	(0.083)	(0.028)	(0.097)	(0.029)
HH: Income	-0.000	-0.001	-0.000	-0.001	-0.000	-0.001	-0.000	-0.001
	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
H: Age		-0.032***		-0.032***		-0.032***		-0.032***
		(0.008)		(0.008)		(0.008)		(0.008)
H: Years of Schooling		0.015		0.015		0.017		0.017
		(0.034)		(0.034)		(0.034)		(0.034)
H: Monthly Salary		0.001		0.001		0.001		0.001
		(0.002)		(0.002)		(0.002)		(0.002)
H: Hours Worked		0.013*		0.013**		0.013*		0.013*
		(0.007)		(0.007)		(0.007)		(0.007)
HH: Own House		0.000		0.000		0.001		0.001
		(0.032)		(0.031)		(0.031)		(0.031)
HH: Live with Parents		-0.037		-0.037		-0.038		-0.038
		(0.047)		(0.047)		(0.047)		(0.047)
Selection Equation	(1)	(1)	(2)	(2)	(3)	(3)	(4)	(4)
Weights	YES	YES	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Husband-related	NO	YES	NO	YES	NO	YES	NO	YES
Observations	1970	1970	1970	1970	1970	1970	1970	1970
Pseudo Log Likelihood	-2.051	-1.940	-2.045	-1.934	-2.053	-1.942	-2.051	-1.941

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.

Dependent variable: childbirth outcome. Average partial effects refer to the average effect of a change in each covariate on the probability of having a child.

Notes: Table shows the APEs of the policy packages on childbirth outcomes obtained by using Equations (1.11) and (1.12) and the subsample data. Selection equation indicates the specification of the marriage decision variables reported in Table 1.18. A set of the baby bonus amounts is used in the first four columns, and the policy variable is replaced with the sum of the baby bonus in the following four columns. Odd columns control for the demographic characteristics of females, and even columns additionally control for her husband-related characteristics. All specifications include weights, province and year fixed effects, and regional time trend.

Table 1.20: Effects of MPB and BB on Marriage Likelihood WITH Selection Bias Correction using Females under the 50th Percentile of Household Income

	All Sample			
	(1)	(2)	(3)	(4)
MPB	0.006***	0.006***	0.006***	0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
BB1		-0.008		
		(0.023)		
BB2		0.018		
		(0.017)		
BB3		-0.007		
		(0.005)		
BB				-0.001
				(0.004)
Age	0.022***	0.022***	0.022***	0.022***
	(0.006)	(0.006)	(0.006)	(0.006)
Years of Schooling	-0.029**	-0.029**	-0.029**	-0.029**
	(0.013)	(0.013)	(0.013)	(0.013)
P: Good Relationship	-0.019	-0.019	-0.019	-0.019
	(0.022)	(0.022)	(0.022)	(0.022)
P: Bad Relationship	0.071**	0.071**	0.071**	0.071**
	(0.032)	(0.032)	(0.032)	(0.032)
P: Mother College +	0.040	0.040	0.040	0.040
	(0.050)	(0.050)	(0.050)	(0.050)
P: Father College +	-0.054	-0.054	-0.054	-0.054
	(0.037)	(0.037)	(0.037)	(0.037)
HH: Income	0.001*	0.001*	0.001*	0.001*
	(0.001)	(0.001)	(0.001)	(0.001)
Weights	YES	YES	YES	YES
Province FE	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Observations	3706	3706	3706	3706

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Dependent variable: marital status. Average partial effects refer to the average effect of a change in each covariate on the probability of being married.
Notes: Table reports the APE estimates of the marriage decision using Equations (1.11) and (1.12) and the female subsample with low income. Odd columns include only immediate policy benefit while even columns include all policy packages in time $t-1$.

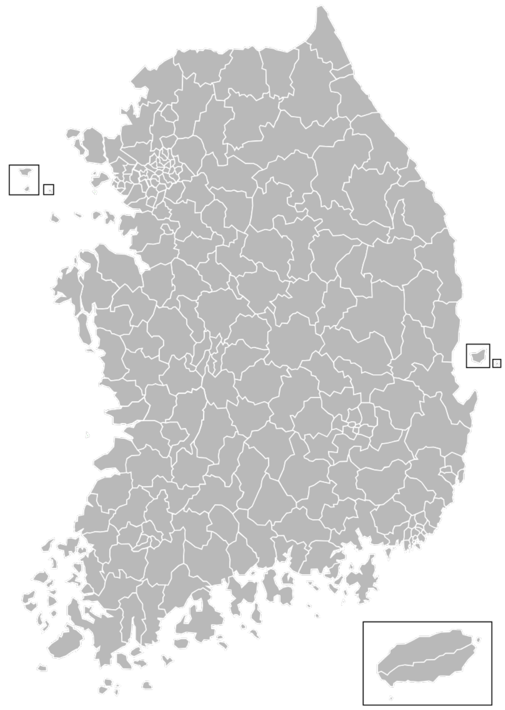
Table 1.21: Effects of BB on Birth Likelihood WITH Selection Bias Correction using Females under the 50th Percentile of Household Income

	Married Women							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BB1	0.005	0.018	0.003	0.016				
	(0.028)	(0.031)	(0.028)	(0.032)				
BB2	0.000	0.002	0.005	0.007				
	(0.019)	(0.020)	(0.019)	(0.021)				
BB3	0.000	0.003	-0.002	0.001				
	(0.005)	(0.005)	(0.005)	(0.006)				
BB					0.001	0.005	0.001	0.005
					(0.002)	(0.003)	(0.003)	(0.003)
Age	-0.008	0.027***	-0.008	0.027***	-0.008	0.027***	-0.008	0.027***
	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)
Years of Schooling	0.003	-0.010	0.003	-0.010	0.003	-0.010	0.003	-0.010
	(0.015)	(0.013)	(0.015)	(0.013)	(0.015)	(0.013)	(0.015)	(0.013)
P: Good Relationship	-0.015	-0.019	-0.015	-0.019	-0.016	-0.020	-0.016	-0.020
	(0.021)	(0.018)	(0.020)	(0.019)	(0.021)	(0.019)	(0.021)	(0.019)
P: Bad Relationship	-0.002	0.002	-0.002	0.002	-0.002	0.003	-0.002	0.003
	(0.028)	(0.028)	(0.027)	(0.028)	(0.027)	(0.028)	(0.028)	(0.028)
P: Mother College +	-0.042	-0.025	-0.040	-0.025	-0.042	-0.025	-0.042	-0.026
	(0.039)	(0.034)	(0.038)	(0.034)	(0.038)	(0.034)	(0.039)	(0.034)
P: Father College +	0.006	0.013	0.006	0.013	0.006	0.011	0.006	0.011
	(0.034)	(0.028)	(0.033)	(0.028)	(0.034)	(0.029)	(0.034)	(0.029)
HH: Income	0.001	-0.000	0.001	-0.000	0.001	-0.000	0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
H: Age		-0.024***		-0.024***		-0.024***		-0.024***
		(0.008)		(0.009)		(0.008)		(0.008)
H: Years of Schooling		0.055**		0.055**		0.057**		0.057**
		(0.024)		(0.024)		(0.024)		(0.024)
H: Monthly Salary		0.004		0.004		0.004		0.004
		(0.002)		(0.002)		(0.002)		(0.002)
H: Hours Worked		0.006		0.006		0.006		0.006
		(0.008)		(0.008)		(0.008)		(0.008)
HH: Own House		-0.014		-0.014		-0.013		-0.013
		(0.039)		(0.039)		(0.039)		(0.039)
HH: Live with Parents		-0.024		-0.024		-0.024		-0.024
		(0.077)		(0.077)		(0.077)		(0.077)
Selection Equation	(1)	(1)	(2)	(2)	(3)	(3)	(4)	(4)
Weights	YES	YES	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES	YES	YES
Regional Time Trend	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Husband-related	NO	YES	NO	YES	NO	YES	NO	YES
Observations	1163	1163	1163	1163	1163	1163	1163	1163
Pseudo Log Likelihood	-1.277	-1.214	-1.276	-1.213	-1.278	-1.215	-1.277	-1.214

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Dependent variable: childbirth outcome. Average partial effects refer to the average effect of a change in each covariate on the probability of having a child.
Notes: Table shows the APEs of the policy packages on childbirth outcomes obtained by Equations (1.11) and (1.12) and using females with income under the 50th percentile. Selection equation indicates the specification of the marriage decision variables reported in Table 1.20. A set of the baby bonus amounts is used in the first four columns, and the policy variable is replaced with the sum of the baby bonus in the following four columns. Odd columns control for the demographic characteristics of females, and even columns additionally control for her husband-related characteristics. All specifications include weights, province and year fixed effects, and regional time trend.

1.A Appendix

Government Structure in South Korea



Map of South Korea

South Korea is made up of 17 upper-level local governments which are further divided into two categories: (1) Larger cities including one special city (Seoul, the capital of South Korea), six metropolitan cities, and one special autonomous city (Sejong, the central administrative capital city), (2) nine provinces including Jeju, the special autonomous province. Metropolitan cities have its subordinate governments called "*gu*" (district) or "*gun*" (county) and provinces have its corresponding lower-level governments called "*si*" (city) or "*gun*" (county) depending on the population and history of the area. All these subordinate local governments (234 municipalities as of 2008) are municipal-level divisions of analysis in the paper, and these local governments are depicted in the map of South Korea.

Collection Process of the Policy Variables

It is essential to keep track of the precise baby bonus amounts across districts to identify the policy effect. Despite the extensive introduction of the program, there is no universal or central collection of how the baby bonus program is operated. Thus, I use two primary government-provided databases and the public disclosure request[46], to ensure the precise cash transfer amounts in the analysis.

Primary sources are Municipal Ordinance Data Center and National legislation Data Center where provide the entire history of all municipal ordinances, including enactments and revisions of the law. The baseline policy data is constructed on the basis of the municipal ordinance of all districts, so are the terms of eligibility conditions. However, as mentioned in Section 1.3, the early operation of the program is not properly documented in some districts before the correction order by the National Election Commission. This insufficient information is supplemented by public disclosure requests. I requested the baby bonus history of the district to each local government and obtained early operation of the baby bonus program. Finally, all policy information after 2007 is cross-validated with the Annual Issue Casebooks.

[46] http://www.elis.go.kr/, http://www.law.go.kr/, and https://www.open.go.kr/

Variable Names

Variable Name	Description
BB	Amounts of Baby Bonus that the female is eligible for
MPB	Marriage Promotion Budget per 10,000 residents
Age	Age of female
Years of Schooling	Years of education
HH: Income	Income of her household
P: Good Relationship	Female describes that the relationship with her parents at age 15 is good
P: Bad Relationship	Female describes that the relationship with her parents at age 15 is bad
P: Mother College +	The mother of the female completed at least college degree
P: Father College +	The father of the female completed at least college degree
H: Age	Age of her husband
H: Years of Schooling	Her husband's years of education
H: Monthly Salary	Her husband's monthly salary
H: Hours Worked	Her husband's hours worked per day
HH: Own House	She lives in her owned house
HH: Live with Parents	She lives with either her parents or her parents-in-law

Chapter 2

The fertility effects of pro-natalist policies: A density approach

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2.1 Introduction

Although low fertility rates are a problem in several developed countries, it is in Korea, however, where this problem takes its most salient form. Korea today has the lowest total fertility rate on the planet – 0.98 in 2018. The country displayed below-replacement levels of total fertility since the 1980s, and, at this pace, forecasters expect that population decline will begin in 2028.[2,3]

A decrease in birth rates in South Korea has been a troubling phenomenon. Low birth rates and an aging population are concerning since a higher ratio of elders to prime-aged workers strains resources to fund health care and the social security net. South Korea's government recognizes the country's low fertility rate as one of its most crucial contemporary challenges. Thus, as a response to the observed low levels of fertility, the government has recently initiated and expanded a variety of pro-natalist policies such as maternity leave, parental leave, and other childcare benefits. South Korea is then a great laboratory to study whether pro-natalist policies can meaningfully achieve its goals of increasing fertility. This is what we do in this paper: We study the effects of the Korean fertility program called the "baby bonus".[4]

Operated by local governments, the baby bonus program provides parents with a monetary transfer in the event that they have a child. The program today is the main policy instrument used by Korean policymakers of local administration to deal with the country's low fertility rates. As of 2018, about 98% of the local governments participate in

[2]Source: Statistics Korea, a central government organization for statistics, a subsidiary of the Ministry of Strategy and Finance.

[3]OECD family database reports that the total fertility rate of South Korea has been lower than 2.1–the replacement level–since 1983.

[4]The literal translation of the program's name is the "Childbirth celebration grant". We adopt the convention in this literature and refer to the program as the "Baby bonus program".

the program.

There is evidence in the literature that pro-natalist policies can raise the birth rates (Cohen et al. 2013; Whittington et al. 1990; Milligan 2005; González 2013). What we ask in this paper, beyond estimating the program's effects on fertility, are the following questions: How large benefit levels would have to be for the program to have fertility effects of the magnitudes expected by the policymakers? How large would the program's budget have to be to achieve this goal? How much of the program's budget is currently spent on infra-marginal births, that is, births that would occur even if the transfer to those parents were set to zero?

Our main strategy to achieve this is by estimating the *reservation price of fertility* as a function of the program's benefit level under a fairly general framework. In our framework, the reservation price of fertility is defined as the minimum amount that a female needs to be compensated for additional childbirth. We provide the estimation strategy for the unconditional distribution of the reservation price of fertility by employing the binary-choice models typically used in this literature. We characterize how one can take estimates from the conditional probability of having a child – conditional on observable pre-determined characteristics and baby bonus amounts – and use them to recover the unconditional distribution of the reservation price of fertility. To the best of our knowledge, we are the first to study the fertility effects of pro-natalist policies through the lenses of the distribution of the reservation price of fertility. We are also the first to report estimates of this distribution.

Our main identifying assumption is that, conditional on a rich set of covariates that are associated with childbirth decisions such as age, marital status, and other household pre-determined characteristics, couples that were exposed to higher levels of incentives from variation in the program's generosity across similar municipalities are comparable to one another, so the variation in the treatment, the baby-bonus amount across these individuals, is as good as random. Different forms of this assumption, exploiting variation in the policy's generosity across time and jurisdictions, are often used in this literature (Milligan 2005; Cohen et al. 2013; González 2013; Parent and Wang 2007; Riphahn and Wiynck 2017).

Additionally, we report another object that we define as the *Infra-marginal ratio of spending*, representing the fraction of the program's budget that is spent on infra-marginal births, that is, births that would have occurred in the absence of any financial incentive. We show how this object is identified and can be estimated using the same regression tools that are typically used to estimate the program's treatment effect. To the best of our knowledge, we are the first to report estimates of the Infra-marginal ratio of spending.

The part of the budget spent on payments to infra-marginal births does not contribute to boosting fertility but nevertheless generates a financial burden to the local governments. Governments could, in principle, attempt to avoid this burden by means of second or third-degree price discrimination. However, due to both fairness concerns – treating otherwise identical taxpayers with different policy benefits – and also a lack of technology to implement such schemes, fine-tuned designs of price discrimination are, in practice, unfeasible. As a result, governments are trapped in a difficult situation: If they want to incentivize fertility, they must also commit to spending resources on infra-marginal births. Our results suggest that the share of Infra-marginal births in Korea is over 95%. As a result, the fraction of the Korean pro-natalist program's budget that is spent on infra-marginal births is estimated to be over 75%.

We also revisit some results in this literature (Milligan 2005; Cohen et al. 2013) and show that this high share of infra-marginal births is not a result due to peculiarities of the

Korean setting, but part of a general problem associated with these types of policies. Our estimates suggest that in Quebec and in Israel, the share of Infra-marginal births was over 90%, with some of our estimates above 95%. We conclude that these programs, at the current level of benefits associated with them, act mainly as a lump-sum transfer to parents that would already have chosen to have a child anyway.

These results are consistent with our estimates of the reservation price of fertility distribution. Our estimates show that only a small fraction of females in Korea find themselves near the margin of indifference of having a child. These are the individuals that can be induced to have a child by means of small financial incentives. As a result, unless the program's generosity increases substantially, pro-natalist programs such as the Korean baby bonus are bound to provide payments mostly to infra-marginal parents: Those that were already planning to have the child even in the absence of the benefit.

This paper is organized as follows. Section 2.2 provides a literature review, and Section 2.3 describes the baby bonus program and the data used in the paper. Section 2.4 and 2.5 explain the model and empirical strategies to examine the effect of the pro-natalist policy in South Korea, respectively. Empirical results are provided in Section 2.6. Finally, Section 2.7 concludes the chapter.

2.2 Literature Review

Childbirth is perceived as an economic decision since Becker (1960). Under this research paradigm, parents decide to have a child based on a comparison of the utility gained from having a child and expected costs associated with raising a child. Thus, factors that affect the costs or the benefits of having a kid should, in theory, influence fertility decisions. The literature has then searched for evidence of these types of fertility responses to economic incentives in a variety of settings. The most eminent challenge to these types of exercises is to find an exogenous variation to identify these effects.

Sometimes, pro-natalist policies are incidental. They occur as a byproduct of the tax code, which is designed with tax deductions for families with more children (Moffitt 1998; Gauthier 2007). In the USA, for example, the Earned Income Tax Credit (EITC) in the United States comes with a built-in pro-natalist component since the program's benefit level increases with family size.

Estimates of the fertility effects of the tax incentives, embedded in the EITC, show only a weak impact on a subgroup of females (Baughman and Dickert-Conlin 2009). Crump et al. (2011) also studies the personal exemption in the United States together with the EITC and the Child Tax Credit, but they conclude these pro-natalist incentives do not seem to increase birth rates. In other developed countries, evidence of the fertility effects of tax incentives is also found to be weak or mixed. See **?** for the case of France, Francesconi and Van der Klaauw (2007) for the case of the United Kingdom, and Haan and Wrohlich (2011) and Riphahn and Wiynck (2017) for the case of Germany. All fail to find substantive fertility responses to the incentives present in the tax code.

On the other hand, the literature finds that more direct and salient pro-natalist schemes – such as cash transfer programs – do display more noticeable fertility effects. For instance, the Allowance for Newborn Children (ANC) in Quebec, Canada, is examined by Milligan (2005) and Malak et al. (2019). Milligan (2005) find that a CAD $1,000 child allowance yields an increase in the fertility rate of around 16.9%, while Malak et al. (2019) estimate 8.6% increase in fertility with a CAD $8,000 child allowance. González (2013) studies a similar cash benefit program in Spain in 2007. All mothers with a newborn child were eligible for a one-time payment of 2,500 Euros. Using a regression discontinuity design, he finds a sizable impact of the program of around 6% on the annual number of births. Similarly, in 2004, the Australian government initiated a cash transfer program, called the 'baby bonus.' The federal government announced a lump sum payment of AUD $ 3,000 baby bonus to a newborn child. Drago et al. (2011) and Risse (2010) find increases in self-reported intention to have a child after the introduction of the policy. A 1% increase in Switzerland's family allowance also shows 0.01% in birth likelihood (Milovanska-Farrington 2019). Also, Hong and Sullivan (2016) evaluate the Korean baby bonus program with limited inclusion of the districts, and they report a marginal increase in birth rates by 0.1% as a response to a USD $1,000 baby bonus transfer.

2.3 Data

2.3.1 Baby Bonus Program

You: The baby bonus is an in-cash transfer to all legal residents with a newborn or adopted child, operated by local governments in South Korea. The goal of the baby bonus program is to actively deal with objects of public concern in response to the population aging and ultimately ensure the districts' sustainability. The population crisis is more severe in rural areas, which made South Jeolla province initiate a cash transfer to newborn babies as a population-targeted policy package (Yoon 2000). This cash transfer program has been widely adopted as many other local places faced a similar situation with a large and rapid population decline. From 2006, districts started incorporating the program into their municipal ordinances, having a more formal form of the program operation.

Tables 2.2 and 2.3 summarize the amount and number of local governments with the baby bonus program. Over time, the average amounts of the baby bonus have risen in most places because the amounts were perceived to be too small relative to the expenses related to raising a child.[5]

In Table 2.3, we show the evolution of the number of districts that implemented the program at the different parity births (first child, second child, and third child). We can see the total number of local governments for which the program is present in some form or another has continuously increased over the two decades. As of 2018, about 98% of the local governments have a cash transfer program given to the third of higher-order births. It is also notable that the program's expansion is made in the way that the early parity births (the first and second child) became the recipients of the benefit. A little less than 50% of local governments have the baby bonus program for the first child, which is approximately twice as large compared to ten years ago.

In terms of eligibility criteria, some of the districts have residency requirements, which we discuss in detail below. Except for residency requirements, the program's eligibility criteria are close to universal: All mothers, regardless of income, age, or occupation, are entitled to claim it. In its early stages of operation, most municipalities did not specify these residency requirements, as illustrated in Table 2.4. However, the controversy over the baby bonus seekers who migrated into a district with a higher baby bonus became intense, so the local governments started incorporating a proviso as to who could be the beneficiaries of the program in the district. As a result, 61 out of 225 local governments stated that the newborn child's primary caregiver is required to live in the district at least a year before childbirth in 2016.

[5]To put the Baby Bonus values in perspective, the monthly cost of preschool in South Korea is estimated to be roughly between 526 to 1004 USD in 2010 (Shin and Kim 2010).

2.3.2 Data

Despite the widespread presence of the baby bonus program across the country, there is no unified dataset that contains information about the policy. Thus, we collect the transfer amounts and their operation details for all districts from the municipal ordinances. Municipal Ordinance Data Center and National legislation Data Center are the two data sources for which all enactment and revision of the municipal ordinances are kept. The amounts of the baby bonus for each year are collected from those two sources, and this is cross-validated using the Population Policy Casebook.

We combine the baby bonus information we collected with the Korean Longitudinal Survey of Women and Families (KLoWF). The KLoWF is a rotating panel data operated by the Korean Women's Department Institute since 2007 and collected biannually from 2008 to 2018. The dataset includes detailed demographic information such as age, education, marital status, number of children, place of residence, and income. Given the purpose of the panel data,[6] households without any female members are excluded from the sample. The panel uses the same primary enumeration districts (EDs) used for the 2005 Census of South Korea to construct the sample and targets to have nearly 10,000 households. Due to attrition, approximately 70% of the original household in the first wave remains in the data.

We refine the females aged between 19 and 30 years old with less than two children, namely the affected population as in Cohen et al. (2013).[7] Those females are still in their prime reproductive age and thus more likely to be responsive to the program's incentives.

We construct a policy variable – labeled as cash transfer – which is the potential benefit

[6] The panel data states its goal *to keep track of females' information and ultimately support the policy evaluation related to the females.*

[7] About 85.3% of females in South Korea reported their lifetime desired number of children less than two in the 2015 Census.

level a female is eligible as compensation for her next childbirth from the local government that she resides. Given the discrepancy in timing between the conception and actual child delivery, it is assumed that females would account for the baby bonus amount at the time of child planning.

As a means of preventing the baby bonus seekers, a subsidy form of transfers became popular when it comes to higher-order births. In most rural areas, the average amounts for the third or higher-order child are significantly higher than the urban areas. Those governments included a safeguarding condition that the transfer is provided for specific months (or years) specified in the municipal ordinance. The benefit is forfeited if the recipients migrate to another district, so the recipients must stay in the district. To account for the potential recipients' discounting the benefit, we use the present value of the baby bonus amount whenever the benefit is not offered at one-time.[8] Second, while the baby bonus's primary district unit is city-level governments, some upper-level governments also introduced the baby bonus program. We simply add the eligible baby bonus amounts for those provinces in addition to the district's transfer, as females would take those two amounts into account when making their fertility decisions.

Table 2.5 reports the summary statistics of the data. Approximately 13% of females are married, and 7.2% of the females give birth to a child the two-year window before the interview, which we set to be our period of analysis, on average. They are, on average, eligible for a USD $135 baby bonus for the subsequent childbirth. Females are about 24.24 years old and have around USD $63,361 annual household income. For those females that are married, we observe that females' husbands are about 32 years, completed 14.46 years of schooling, earn USD $3,951 monthly salary, and work about 8.36 hours a day on average.

[8] We discount these benefits using the inflation rate by the Korean CPI index reported by Statistics Korea.

2.4 Framework

We aim to estimate the fertility effects of the baby bonus policy. To do that, we need some form of exogenous variation in the policy that can be used to infer its effects. Following the literature, we use the within and cross-time variation in transfer generosity to identify the policy effects on fertility. In this section, we discuss both the statistical assumptions and the restrictions on economic behavior we impose in our analysis. We show that, under these assumptions, we can infer the policy effects and a series of other relevant objects that are typically not estimated in this literature, such as the unconditional distribution of the reservation price of fertility and the fraction of the program's expenditures that are spent on infra-marginal births.

Let Y_i be the binary indicator that a female i had a child in the observation period of interest. We assume that the period of interest is short enough that only a single childbirth decision can be made.[9] Let X be a set of predetermined covariates required to ensure that in provinces treated with different levels of the policy, individuals are comparable. Let $Y_i(d)$ be the potential outcome that female i would have had if the treatment variable – the level of the baby bonus policy – were to be set at d. The fundamental problem of causal evaluation is that the causal effect of changing the baby bonus policy from one level, say d, to another level, say d', is defined as the difference between two potential outcomes $Y(d') - Y(d)$ that are never jointly observed in the data. The observed outcome of interest is linked to the potential outcomes through the observation equation:

$$Y_i = Y_i(D_i)$$

[9] In our empirical analysis, we used a two-year window. That window is supposed to include both the time from conception until childbirth but also the time required to move from the decision to have children until conception as well. The frequency of two or more children in a two-year window in Korea is quite rare, so little is lost by assuming that a single decision can be made in such an interval.

where D_i is the level of the baby bonus treatment that individual i was exposed. It is instructive to think about the special case in which there are only two possible levels of the policy: Either it is zero, so the individual is a control, or it is (normalized to) one. In that case, the observation equation becomes:

$$Y_i = Y_i(1)D + Y_i(0)(1 - D)$$

For those individuals that are treated, we observe fertility levels in the state of the world in which they are treated (but we do not observe $Y(0)$ for them). In contrast, for the individuals that are not treated, we observe fertility levels in the state of the world in which they are not treated (but $Y(1)$ is missing for them).

Our goal lies in estimating functionals of the difference of potential outcomes such as the average treatment effect $ATE \equiv E[Y(1) - Y(0)]$ and the average treatment effect on the treated $ATT = E[Y(1) - Y(0)|D = 1]$. To do that, we follow the literature (Milligan 2005) and assume that, after controlling for a rich set of predetermined characteristics X – variations in the policy level are independent of potential outcomes:

Assumption 1. (Conditional Exogeneity / Ignorability) $Y(d) \perp\!\!\!\perp D \mid X$.

Assumption 1 states that differences in the outcome distributions across individuals that share the same levels of the covariates X can be unambiguously attributed to the policy. Under this assumption – and assuming that no set of covariates completely determine the treatment level status (common support assumption)–, it is well known that the effects of interest such as the ATE and the ATT are identified. These effects can then be estimated through regression or re-weighting procedures. For the discussion that follows, it is helpful to assume that we are looking at a population that shares the exact same values of X, thus controlling for predetermined characteristics is unnecessary. In this sub-population, since the distribution of X is degenerate, there is no need to carry the dependency on the value of X when writing a (conditional) probability or an expectation. In Section 2.5, we will explicitly bring back the role of covariates when we discuss estimation and empirical exercise.

Our goal in the discussion that follows is to highlight that, under the same set of assumptions that this literature has used to identify the fertility effects of these pro-natalist policies, we can also identify another important object: The infra-marginal ratio of spending. That share is defined as the fraction of the program's budget spent on payments to couples that would have made precisely the same fertility choice of having a child even if they were not encouraged to do so by the program's incentives.

This object is of obvious relevance to policymakers since that part of the program's budget acts essentially as a lump-sum transfer to parents without the behavioral change that the program is attempting to induce. Note that since fertility levels are binary, there are only four potential combinations of $Y(0)$ and $Y(1)$ for any possible value of the policy. These are given in Table 2.1.

Table 2.1: Principal Strata

	$Y(0) = 0$	$Y(0) = 1$
$Y(1) = 0$	Never-takers	Defiers
$Y(1) = 1$	Compliers	Always-takers

Borrowing the terminology from Angrist and Imbens (1994), we call an individual an "always-taker" if they would choose to have a child whether or not they were incentivized to do so.[10] For these individuals, both $Y(1)$ and $Y(0)$ are equal to one. We call an individual

[10]It is worthwhile to note that our setting differs from Angrist and Imbens (1994) in a very important way. In Angrist and Imbens (1994), there are three objects: the treatment, the outcome, and the instrument. For us, there are only two: baby bonus and fertility. We choose to use their terminology because in our

a "never-taker" if they would choose not to have a child regardless of whether they were incentivized to do so. For these individuals, both $Y(1)$ and $Y(0)$ are equal to zero.

The third case is precisely the set of individuals that the program is mostly interested in. For the "compliers", the choice would be to not have a child $Y(0) = 0$ if they were not incentivized to do so. However, if they were incentivized to do so, then the choice would be different. They would instead choose to have a child ($Y(1) = 1$). Compliers are the individuals for which the baby bonus incentive indeed changes fertility choices.

There is also a fourth possibility (although it is ruled out by a model in which individuals are rational). We call an individual a "defier" if they would choose to have a child if they were not incentivized to do so, but would instead choose to have one if they weren't ($Y(1) = 0$ but $Y(0) = 1$). We will follow the literature (Angrist and Imbens 1994) and assume from now on the absence of defiers in our analysis.

Equipped with the definitions of these four groups, we can now study how the program's budget will be allocated. Our interest lies in the fraction of the budget that will be spent on changing fertility.

To begin, note that the policy only pays individuals that choose to have children when they are treated. Thus, whenever $Y(1) = 0$, no money is spent on that individual. Therefore, the share of the program's budget that is spent on never takers or defiers is, by construction, zero.

Note also that the entirety of the program's budget will be then spent on either compliers or always takers. Since all individuals in the treated group are treated with the exact same level of benefits, the share of the budget spent on individuals for which the

setting, the stratification of individuals based on the values of the pair of $Y(1)$ and $Y(0)$ is analogous to theirs, although the rest of our setting is different.

program effectively induced a behavioral change is then given by the share of compliers in the group composed of compliers and always takers. Finally, note that in the absence of defiers, we have that:

$$ATE = E[Y(1) - Y(0)] = 1 \times Pr[Y(1) - Y(0) = 1] = Pr[Complier]$$

This follows from applying the law of iterated expectations to the left-hand side of the equation and using the fact that the difference between $Y(1)$ and $Y(0)$ is only non-zero for the complier group. In other words, the effect of the program on average fertility is given by the size of the group of individuals for which the program induced them to have a child.[11]

Thus, in the special case that the policy only takes two values, as long as the program's effect (ATE) is identified, we can immediately identify the share of the program's budget that is spent on individuals that do have a behavioral response to the incentive and the share of the program's budget that is spent on a lump-sum transfer to individuals that would have made the exact same choice if they were offered no incentive anyway. These are given by $ATE/Pr[Y = 1|D = 1]$ and $Pr[Y(0) = 1]/Pr[Y = 1|D = 1]$. Note that the denominator of these expressions is the fertility level for the group of individuals that are treated with the policy. This is an object directly observed in the data.

When the policy takes only two possible values, the share of the budget that is spent on infra-marginal coincides with the share of infra-marginal births itself. However, these objects become different when the policy takes more than only two values, which is typically the case. Now, we show how to obtain the share of the budget spent on infra-marginal births. To begin, note that the total spending associated with this policy up to a constant

[11] In the general case in which defiers are present, we have that $ATE = 1 \times (Pr[Compliers - Pr[Defiers])$, so the average treatment effect is a lower bound for the proportion of compliers.

of proportionality (given by population size) equal to the expected spending of this policy on a randomly selected individual.[12] Thus, we can study the expected spending instead, which is given by $E[YD]$. Using and add and subtract strategy, we can decompose this object into two components:

$$E[YD] = E[Y(0)D] + E[(Y - Y(0))D].$$

The two terms on the right-hand side of this equation are both positive, and they add to the expectation of the program's spending on a randomly selected individual of the population. Note that, since $Y(0)$ is a binary random variable, the first term $E[Y(0)D]$ is the spending on individuals for which $Y(0)$ is already one. These are individuals that receive the benefit but would have chosen to have a child without the program. The second term $E[(Y - Y(0))D]$ is spending on individuals for which $Y(d)$ is equal to one but $Y(0)$ is equal to zero.[13] Those are the compliers, individuals that were induced to have children because of the program. Dividing these terms by the total spending of the program, which is the left-hand side of this equation, we get the share of the budget that is spent on infra-marginal births and the share that is spent on the compliers, that is, the share of the budget that is spent on births that were induced by the program. This is a useful decomposition, but it is still written in terms of counterfactual objects such as $Y(0)$. Now, we are going to write the object of interest in terms of objects that we see in the data, Y, D, and X.

[12] We can write the total spending on this program as $\sum_i Y_i D_i$, where the sum is over every individual in this population. Multiplying and dividing by the population size yields the relationship between the program's expenditure and its average spending per individual in the population.

[13] Simple algebra using the fact that $Y - Y(0)$ is a binary variable yields the useful characterization $E[(Y - Y(0))D] = Pr[Complier]E[D|Complier]$. This equation states that the spending per capita on compliers is given by the proportion of compliers times the mean value of the benefit, conditional on the population of compliers.

Remark 1. Under Assumption 1 and under linearity of the conditional mean of Y given D and X, we can write the share of the budget that is spent on infra-marginal individuals as:[14]

$$1 - \frac{Cov(Y, D|X = x)}{Var[D|X = x]} \frac{E[D^2]}{E[YD]}.$$

The proof of this result is straightforward and thus omitted. It is useful to note that the ratio $Cov(Y, D|X = x)/Var(D|X = x)$ coincides with the coefficient associated with D from the regression of Y on D and X, and that $E[YD]$ can be written as $Pr[Y = 1]E[D|Y = 1]$, which is the proportion of births in the data times the mean benefit level associated with those births. Since all objects in the expression above are functionals of the joint distribution of observed data (Y, D, X), the share of the budget that is spent on infra-marginal individuals is identified.[15]

2.4.1 A Behavioral Roy Model of Fertility

In this section, we show that, under the same functional form assumptions typically used in this literature to estimate the program effects, one can, in fact, identify the entire distribution of willingness to have a child. To do that, we will only add one economic assumption on the individual's behavior:

Assumption 2. (Roy's rationality) A female will choose to have a child in the period we analyze if, and only if, their utility of doing so is greater than the utility of the alternative.[16]

[14] When the functional form for the conditional mean of Y given X and D is non-linear, such as in a probit or a logit model, the formula becomes slightly different but the object of interest – the infra-marginal share of spending – is still identified.

[15] Both $Cov(Y, D|X)$ and $Var[D|X]$ are, in principle, functions of value of X that the conditional covariance and the conditional variance is being evaluated. But the ratio of these two objects is constant under linearity of the conditional mean of Y given D and X, which was assumed to be true to derive this result. As a result, we do not need to be explicit about which value of X we are conditioning in this case.

[16] We believe this assumption is appropriate in the Korean setting, given that most pregnancies are planned.

Thus, for any given level of the policy D we can write the individual's choice as:

$$Y = Y(D) = D - \epsilon > 0, \tag{2.1}$$

where Y is the indicator of whether or not the individual decides to have a child in the period we analyze, and ϵ is defined as the individual's valuation (in money metric units) of the dis-utility of having a child. It is defined as the difference in the utility of not having a child in the analyzed period and the utility of having a child. In the absence of any financial incentives, the proportion of individuals that will choose to have a child is then given by the proportion of individuals for which the dis-utility of having a child is negative. The random variable ϵ thus measures precisely how much greater is the utility of not having a child compared to the alternative of having one. When ϵ is, for example, minus 1000 dollars, then the individual will be willing to have a child and would still be willing to do so if the environment were to change the relative costs and benefits to have a child up to 1000 dollars. If, by contrast, by means of taxes, subsidies, or changes in the costs of raising a child, either the costs of having a child or the benefits of not having one were to change by more than one thousand dollars, the individuals would change her optimal choice of fertility to not having a child.

Similarly, on the right side of the real line, ϵ measures the *reservation price of fertility*. That is, it measures the exact change in the incentives of the two alternatives that are required to induce individuals with a willingness not to have a child given by ϵ to change their choice and instead decide to have one.

The reservation price of fertility is then the required financial incentive that is

In other settings, for example, in the study of teenage pregnancy, Roy's rationality is likely to be inappropriate.

needed to induce an individual to have a child. It is the fertility analog to the worker's reservation wage in labor economics. The worker's reservation wage is the smallest financial compensation that is enough to induce labor market participation. The reservation price of fertility, similarly, is the smallest financial incentive that is high enough to induce someone to have a child.

Individuals with ϵ lower than zero will have a child without any financial incentives to do so. We refer to them in our analysis as the always-takers, or the infra-marginal individuals, since they are below the margin of indifference threshold. A pro-natalist policy such as the baby bonus that pays couples a cash amount given by d dollars will induce a change in fertility in this population by precisely the proportion of individuals a reservation price of fertility between zero and the cash incentive level d.[17]

The baby bonus program changes the net benefit of having a child, but it does not change the utility of the *status quo* option. Thus, the agent would make a choice of having a or another child based on the potential utility of both scenarios at a point in time. The change in the net utility of having a child induced by the baby bonus must then be given by the size of the financial incentive.

This discussion highlights two important conditions regarding the childbirth decisions of the agents, which can be summarized as follows. To be induced by the program to have a child, it must be the case that the reservation price of the fertility of the agent lies between zero and the cash grant level d. That is, it must be the case that:

$$Y(d) - Y(0) = 1 \Longleftrightarrow 0 < \epsilon < d$$

[17]Note that, under Roy's rationality assumption, no individual would ever be a defier. That is, no one would think that the net utility of having a child is positive when the government pays them zero for doing so, but suddenly it becomes negative when the government adds to the net utility of having a kid a strictly positive transfer.

This pair of inequalities states that for those females who would have a child because of the program, the net utility of having a child is *negative* in the absence of the program. This must be the case because, if it were not, the female would decide to have a child even without receiving any benefit from the government. Second, for these agents, the reservation price of fertility ϵ must be between 0 and d in order for the utility of having a child to become positive when exposed to the incentive.

The fraction of the individuals that will choose to have a child in the absence of the program can be written as a function of ϵ:

$$\Pr[Y = 1|D = 0] = \Pr[\epsilon < 0] = F_\epsilon(0) \tag{2.2}$$

where $F(\cdot)$ is the CDF of the distribution of the reservation price of fertility ϵ.[18] That is, the proportion of individuals that would have a child without the government's incentive is given by the proportion of individuals for which the reservation price of fertility is below zero, and thus the utility of having a child is already greater than the utility of not having one. Given that the total number of females in the population N, the number of births in the analyzed period, in the absence of any pro-natalist policy, can be obtained as $N_{births} = N \times F_\epsilon(0)$.

Also, the corresponding fraction of the individuals of the exact same population that would choose to have a child in the counterfactual scenario in which the pro-natalist policy is present and offers a cash grant level given by d if the individual chooses to have a child

[18] Under Assumption 1, we can write $Pr[Y = 1|D = d, X = x] = Pr[Y(d) = 1|D = d, X = x] = Pr[Y(d) = 1|X = x] = Pr[\epsilon < d|X = x] = F_\epsilon(d|X = x)$. Dropping the dependency on X since we are looking at a sub-population that for which X is degenerate yields Equation (2.2).

can be expressed as:

$$\Pr[Y = 1|D = d] = \Pr[\epsilon < d] = F_\epsilon(d) \tag{2.3}$$

Finally, the fraction of the agents who decide to have a child as a result of the baby bonus program is the difference between those two fractions defined earlier.

$$F_\epsilon(d) - F_\epsilon(0) = Pr[0 < \epsilon < d] \tag{2.4}$$

The left-hand side of the equation above, $F_\epsilon(\delta) - F_\epsilon(0)$, is the causal effect of the program on expected fertility levels, the ATE. The additional births caused by the baby bonus program can be computed by the product of the total number of females and the changes in births induced by the baby bonus program.

Additionally, the first-order Taylor approximation of the left-hand side of the equation above with respect to d around zero gives us another useful insight into the changes in the number of births induced by the program.

$$N(F_\epsilon(d) - F_\epsilon(0)) \approx N f_\epsilon(0) d \tag{2.5}$$

This tells that the exact number of additional births induced by the program can be approximated by the product of the total number of females, the density of females who are indifferent between having or not having a child, and the size of the cash benefit. Thus, to a first-order of approximation, to find the effects of a pro-natalist policy, we only need two key pieces of information: How large is the incentive d and how many individuals are at the margin of indifference of having a child $f_\epsilon(0)$. If we believe that the proportion of individuals at the margin of indifference between having a child is small, which is likely not an unreasonable assumption, then large incentives are going to be required to substantially move fertility levels.

It is also useful to note the subtle differences between the change in fertility rates as defined above and the object called *total fertility rate*, which is a related but distinct object. The object we study in this paper is the fertility rate of females of different ages (and different cohorts) at a given point in time. In contrast, the *total fertility rate* is the sum of the fertility rates of *a single cohort* over different points in time, that is, over the course of the female's childbearing years. The total fertility rate is designed to answer how many children, on average, females of a certain cohort will have by the end of their childbearing years. In contrast, the objects we study in this paper – the observed fertility – are designed to answer how many children will be born at a certain point in time, across mothers of all cohorts. In Appendix 2.A.2, we further discuss the similarities and differences between these objects. It is worth point out, however, that our results – particularly the ones about the costs of doubling fertility – are robust to whether we look at a single cohort over time or all cohorts in a given period.

2.5 Empirical Strategy

In this section, we discuss how to connect our theoretical considerations with the regression models typically estimated in this literature. The main strategy to achieve this is by assuming the density distribution of the error terms for a regression model, and we estimate the density of the *reservation price of fertility* based on the specified error assumption. The distribution of the reservation price of fertility, denoted as $F_\epsilon(d) \equiv Pr[\epsilon < d]$, represents the proportion of females who are willing to have a child in the analyzed period when provided by the cash transfer amount of d. Hence, estimating the distribution function $F_\epsilon(d)$ and its density $f_\epsilon(d) = \frac{\partial F_\epsilon(d)}{\partial d}$ allows us to identify the share of the infra-marginal and marginal females and thus to examine the various aspects of the policy effects such as the fraction of the budget that acts as a lump-sum transfer to parents and how larger the benefits would have to be to achieve desired levels of fertility.

Thus, far, we simplified our analysis by assuming that we were looking at groups of individuals characterized by identical values of the pre-determined covariates X but different levels of the policy variable D. Thus, it is implicit in the previous discussion that all probabilities and means are given a certain value of X. Now that we are discussing estimation in practice, we are going to bring the covariates back to highlight how we implement our methods.

We are going to estimate models for the conditional probability of having a child in the analyzed period Y as a function of pre-determined characteristics X and the key policy variable D, the baby bonus amount. We assume as it is standard in this literature that controlling for a rich set of characteristics that determine fertility choices, variation in the baby bonus generosity is as good as randomly assigned and thus identify the policy effects on fertility. Importantly, we want to highlight that, under the same functional form assumptions used to estimate these models, we can recover a series of relevant objects that are useful to understand the trade-offs associated with pro-natalist policies.

The models we estimate take the following form:

$$Pr[Y = 1|D, X] = E[X\beta + D > u], \tag{2.6}$$

where D and X are the baby bonus program and the covariates related to her childbirth decisions, respectively, u is an idiosyncratic unobserved error term, and A is the indicator function that takes the value one when the event A occurs and zero otherwise. Note that this is essentially a re-statement of equation one with two key distinctive features: (i) We now explicitly include the role of pre-determined covariates in explaining fertility decisions, and (ii) we applied the expected value operator applied to both sides of Equation (2.1) to write it in terms of a conditional probability. Note, also, that we can think of the reservation price of fertility of an individual as $u - X\beta$, since in the probability specification above, this is precisely the level that the incentive D must take for the individual to become indifferent between having and not having a child. Thus, there is a direct connection between the term $u - X\beta$ in our regression specifications and the reservation price of fertility ϵ that we defined in our theoretical analysis. Note, also, that in the special case in which every individual in a population shares the same values of X, then the distribution of u is, up to a location shift, the same as the distribution of ϵ. In the general case in which X is non-degenerate in the population, the relationship between u and ϵ is through the equation $u - X\beta = \epsilon$, so that Equation (2.6) above coincides with Equations (2.1) and (2.3). Thus, the reservation price of fertility is now decomposed into an idiosyncratic unobserved component u and a systematic and observable component $X\beta$.

Definition 1. An agent characterized by a vector of (X, D, u) is at the margin of indifference between having a child if $u = X\beta + D$.

Under Equation (2.1), an agent would be indifferent between having a child when $D = \epsilon$. Substituting one equation into the other, we obtain the relationship between the structural object ϵ, the reservation price of fertility, and the reduced-form one u, which is the unobserved term in the binary-choice model. The relationship is $u - X\beta = \epsilon$, and thus, $X\beta$ captures the part of the structural object ϵ that is explained by the covariates X, whereas u captures the remaining idiosyncratic part of the reservation price of fertility ϵ that is not explained by covariates.

One important restriction implied by Equation (2.6) above is that we are assuming that the systematic component of the reservation price of fertility takes a linear form. This assumption can be relaxed using re-weighting methods. In Appendix 2.A.3, we report the results we obtained when using re-weighting methods. For simplicity, we will assume for now that the functional form of the relationship between X and the conditional probability of Y is correctly specified.

We are interested in studying the distribution of the reservation price of fertility. Dividing both sides by the standard deviation of the error term, we obtain the following expression:

$$Pr[Y = 1|D, X] = E[X\frac{\beta}{\sigma} + \sigma^{-1}D > \frac{u}{\sigma}], \tag{2.7}$$

If we assume that the distribution of ϵ is normal, then Equation (2.7) above yields a constrained Probit specification, where the constraint is that the coefficient on the baby bonus has to be larger than zero (since its interpretation is the inverse of the standard deviation of the idiosyncratic component of the reservation price of fertility). If, instead, we assume that the distribution of the error term takes another parametric functional form, we get a different binary choice model. Regardless of the functional form assumption, for all standard parametric distributional assumptions on the error term one uses, we can readily obtain estimates of the distribution of the reservation price of fertility once the error term is specified.

The distributional assumptions implicit in standard binary choice models such as

Logit, Probit, and the linear probability model impose restrictions on the shape of the distribution of the unobserved component of the reservation price of fertility, u. The Logit model assumes that u takes the form of a logistic distribution, whereas the Probit model assumes that u takes the form of a normal distribution. Although these assumptions do restrict the class of distributions that the unobserved component of the reservation price of fertility might take, they still leave quite a lot of flexibility for the distribution of ϵ, which is the key object of interest for us. For example, if in the presence of a single discrete covariate, the distribution of u under the Probit model is by assumption normal, whereas the distribution of ϵ, the reservation price of fertility, is going to belong to the class of a mixture of normals. A mixture of normals is a much more flexible class of distributions that can present asymmetries, multi-modality, and a large array of shapes and forms. Similarly, although the linear probability model imposes that the conditional distribution of the reservation price of fertility is uniform, the unconditional distribution of the reservation price of fertility most likely will not be uniform. Thus, although the model does place restrictions on the shape of u, it still leaves quite a lot of flexibility for the distribution of ϵ, which is the object we are interested in studying.

2.5.1 Local-Constant Approximation

The simplest functional form for $E[X\beta + D > u]$ is a linear function of its left-hand side of the inequality's argument. This functional form is obtained when u has a Uniform distribution with a lower bound at zero and an unknown standard deviation σ_u. This yields the constrained linear probability model:

$$Pr[Y = 1|D, X] = X\beta\frac{\sigma_1}{\sigma_u} + \frac{\sigma_1}{\sigma_u}D \tag{2.8}$$

where σ_u is the standard deviation of the unobserved error term u, and σ_1 is the standard deviation of the standard Uniform $(0, 1)$ distribution.[19] Under the linear probability model, σ_1/σ_u is the marginal effect of a change in the baby bonus amount (D) on the probability change in having a child, X is a vector of covariates that are associated with females' childbirth decisions, and u is an idiosyncratic error that is assumed to be uncorrelated with the baby bonus conditional on X.

It is worth pointing out that the model naturally imposes the constraint that the coefficient on the baby bonus variable should be positive: It is a ratio of two standard deviations. A useful implication of this fact is that we can then use estimates of the marginal effect of the baby bonus on the outcome to back out the magnitude of the heterogeneity in u across individuals with a similar level of observable covariates. Moreover, simple algebra using the law of total probability yields the useful result that, in the linear probability model, we have:

$$\frac{\sigma_1}{\sigma_u} = f_\epsilon(0) \tag{2.9}$$

Thus, the coefficient associated with the baby bonus variable in a linear probability model can be interpreted as *the density of the agents at the margin of indifference* between having and not having a child. This means that we can obtain the density of the females whose reservation price of fertility is sufficiently close to zero by inspecting the coefficient on the baby bonus variable in a regression of the outcome Y on baby bonus D and covariates X.

[19] Note that, in contrast with the normal distribution, the standard uniform distribution does not have a unit variance. This is why σ_1 shows up on the expression for the coefficient on D.

2.5.2 Normal Distribution

In Section 2.5.1, we employ the assumption that the unobserved error term u has a Uniform distribution. If, instead, we assume that the idiosyncratic error u belongs to the class of normal distributions, with $u \sim N(0, \sigma_u^2)$, we arrive at the constrained Probit model. Under the Probit model, childbirth decisions of females are governed by the baby bonus program and the vector of covariates as:

$$\begin{aligned} Pr[Y = 1|D, X] &= E[X\beta + D \geq u] \\ &= \Phi(X\frac{\beta}{\sigma_u} + \sigma_u^{-1}D), \end{aligned} \tag{2.10}$$

where Φ is the cumulative distribution function of the standard normal distribution. Note that, under the normality assumption for the distribution of u, the interpretation of the coefficient on the baby bonus variable is as the inverse of the standard deviation of the unobserved heterogeneity in the reservation price of fertility, conditional on observable characteristics X. We will use this fact when interpreting our results. In the appendix, we show how to move from coefficient estimates from the conditional probability Probit model to estimates of the unconditional distribution of the reservation price of fertility.

2.6 Estimation Results

2.6.1 Local Approximation

Table 2.6 reports the results from linear regression models. They can be interpreted as the best linear approximations to the conditional probability of fertility, given the level of covariates. Alternatively, they can be viewed as the results implied by the assumption that

u, the unobserved component of the reservation price of fertility, belongs to the class of uniform distributions.

In the table, the first three columns report estimation results using different covariates associated with childbearing decisions. We first start considering the simplest case where we estimate the effects of the cash incentive controlling for the female's age, marital status, and current family size. Since in a given year and given family size, the value of the baby bonus displays nearly no variation within a district, the variation that is identifying the policy effects, in this case, is across districts' differences in the program's benefit. Intuitively, we are comparing females with the same age, same marital status, with the same number of kids at the beginning of the two-year window of analysis, but that live in places with different values of the baby bonus incentive.

We include the covariates that are commonly used in the literature (Milligan 2005; Parent and Wang 2007; Malak et al. 2019). In the second column, we add the female's income and her husband's age. Then, in the third column, we add the husband's education, income, and hours worked. In the next three columns, heterogeneous impacts of the baby bonus for each birth order are allowed. To do so, we include the interaction term between the cash transfer amount and the number of children the female already had when entering the period of our analysis. Given the strong correlation between the number of children and female's age, interaction terms between those two are included.

In examining the estimates in Table 2.6, we find evidence that the cash transfer program has a statistically significant impact on females' fertility decisions. A USD $1,000 baby bonus leads to a 1.7 percentage point increase in the birth outcome. The effect becomes a little larger when we control household income and husband-related variables, varying from 2 to 2.4 point increase in birth probability. The estimated effects are similar across different model specifications. Females with a USD $1,000 transfer still face a 2.1 to 2.5 percentage point higher chance of giving birth when we allow the heterogeneous impact of the baby bonus per each birth order.

The elasticities of fertility implied by our estimates range between 0.032 and 0.046. To put these numbers in perspective, Baughman and Dickert-Conlin (2009) studied the fertility effects of the Earned Income Tax Credit in the United States during 1990 and 1999 and found the elasticities to be between 0.009 and 0.022. Whittington et al. (1990), on the other hand, when studying the fertility effects of the dependent tax exemption on fertility as well during the period from 1913 to 1984, found fertility elasticities of around 0.127 to 0.248. When it comes to demographic heterogeneity of these effects, the estimates for white women are typically on the lower end of the distribution, whereas secular Jews are on the upper end of the range of estimates. Baughman and Dickert-Conlin (2009) estimate an elasticity of 0.009 for white women, whereas Cohen et al. (2013) estimates an elasticity of 0.325 for the Secular Jews and a third child during the period 1999 to 2005.

One consequence of imposing the rationality assumption is that we obtain a different perspective on the coefficient on the baby bonus variable: We can use it to back out the standard deviation of the idiosyncratic component of the reservation price of fertility. Our estimates suggest that the interval length – that is, the difference between the upper bound and the lower bound of the support of u – is of the order of 58 thousand dollars.[20] The corresponding estimated standard deviation of u, which we denote by σ_u, is around 17 thousand dollars. This represents the standard deviation of the reservation price of fertility conditional on X. It is useful to note how our estimates of σ_u are considerably larger

[20] We can obtain estimates of this object because under the linear probability model the coefficient associated with the baby bonus variable is a ratio of two standard deviations, both of which are functions of the boundary of the support of different uniform random variables. The first is the standard deviation of u, which is the object we are interested in. The other is the standard deviation of the standard uniform distribution. This object is known.

than the baby bonus values we observe. That is, there is much more heterogeneity in the willingness to have children across individuals relative to the magnitude of the financial incentives in place by the program that we observe in Table 2.2.

2.6.2 Detection of the Infra-marginal Females

In Table 2.6, we display our estimates of the Infra-marginal ratio, the proportion of the program's budget that is spent on births of always-takers.[21] That is, births of those that receive the benefit but would have decided to have a child without the benefit anyway. The results suggest that most of the budget are poorly targeted expenditures, that is, expenditures that do not contribute to additional childbirths. Our estimate suggests that the infra-marginal share of expenditures with this program is over 75%. That is, out of every dollar spent on the program, only 25 cents are directed to females who were induced to have a child because of the incentive. In terms of births, our estimate suggests that at least 95% of the births observed in the groups treated by the baby bonus policy are infra-marginal. That is, these births would have occurred even in the absence of the program's incentive.

Why do we observe such a large Infra-marginal ratio? Our results suggest that the value of the transfer is simply too small. If that is the case, only females near the margin of the indifference of having a child will change their fertility plans as a response to incentives of such magnitude. On the other hand, given the program's eligibility criteria, each and every female that was already planning to have a child is allowed to claim the benefit. As

[21] **?** estimate a related by different parameter. They define the benefit-to-cost ratio for different subgroups of females, examining the cost of a policy relative to the number of babies born in a particular group. They measure the benefit to cost ratio by setting the benefit to be the product of the elasticity of the baby bonus and the coefficient of the baby bonus. Then, this effect of the baby bonus program is divided by the cost of the policy.

a result, most of the program's budget will inevitably be spent on individuals that did not actually change their fertility plans. Our result suggests that over 75% of the program's budget is spent without changing fertility decisions. [22]

Table 2.7 contains a cross-country comparison of the ratio between the Korean program and other cash transfer programs. For comparison, we choose Quebec, Canada and Israel, studied by Milligan (2005) and Cohen et al. (2013), respectively. Before we being, it is important to point out that for these other settings, we only have enough information to report the share of *infra-marginal births*. The information contained in these papers is not detailed enough to allow us to back out the *infra-marginal share of spending*. The reason for that is that we do not observe all terms in the expression we obtained in our Remark 1 discussed above.

To obtain estimates of the share of infra-marginal births in these other settings, we use the regression results reported in Table 6 in Milligan (2005) and Table 3 in Cohen et al. (2013). For the first comparison, we consider the simplest specification of each paper. Then, the second and third columns additionally control for the female and family-related variables, respectively.

Is this large Infra-marginal ratio of births we observe in Korea is a country-specific result? Our findings suggest that it is not. Large Infra-marginal ratios do not seem to be a result of Korea's particular setting but instead part of a more general phenomenon. The ratio is above 0.90 irrespective of the countries analyzed and specifications used, while the minimum is estimated at around 0.920 in Quebec.

[22] If one were successful in implementing third-degree discrimination, our results suggest that the program's budget could decline by at least a factor of 4 and still retain the exact same fertility effects.

2.6.3 Density Estimation

In Table 2.8 and 2.9, estimation results of the Probit and Logit regression are reported. We follow the literature and report Average Partial Effects as opposed to the Probit and Logit coefficient themselves. As before, the first column includes the baby bonus, female's age, marital status, and the number of children before the period we analyze. Column (2) additionally controls for family income and age of the female's husband, and Column (3) controls a full set of covariates with interaction terms. In the next three columns, again, we account for different family structures, so the impacts of the transfer can be heterogeneous for each parity birth.

Observing the estimates reported in Table 2.8 and 2.9, we note that they are quite similar. The average partial effect of the cash transfer variable is estimated to be between 0.8 and 1.3 percentage points. These estimates correspond to an implied 2.5 to 3.4% increase in the birth outcome probability – the mean of the dependent variable – for the Probit regression and a 2.8 to 3.7% increase for the Logit regression. To put these numbers in perspective, in studying the effects of the Quebec ANC program, Milligan (2005) report estimates ranging from 5.6 to 12.0% using the Canadian Census data between 1991-1996. On the other hand, Malak et al. (2019) examine the same ANC program during the same period, and they estimate a change in the birth probability to be around 8.6%.

We can again use the estimates of the parameters to infer the implied standard deviation of the idiosyncratic component of the reservation price of fertility. The coefficient associated with the baby bonus variable in our baseline specification – not reported in the table since in there we displayed average partial effects – is approximately 0.0838. Using this estimate, we get an estimate of σ_u of 11.93 thousand dollars. This should be interpreted as the standard deviation of the reservation price of fertility conditional on observed covariates X. To put this number in perspective, recall that an interval around the mean of a normal distribution will contain 95% of the draws when the length of the interval is around 3.92 –which is twice 1.96 – standard deviations. Thus, the length of the interval on the space of the reservation price of fertility – for females with the exact same value of observable characteristics – that covers 95% of females is approximately 46.76 thousand dollars. In other words, our estimates suggest that there is considerable heterogeneity in the reservation price of fertility for females with similar characteristics. For some, it will take little to induce them to have a child; for others, it will take quite a lot. This implies that the conditional density of the reservation price of fertility, conditional on observable characteristics, should have a large spread and, as a result, small changes in the baby bonus D are unlikely to induce many females to choose to have a child. For example, a baby bonus level of one thousand dollars amounts to less than 10% of a standard deviation of u.

Now, we take the estimates from these conditional probability models and use them to recover estimates of the distribution of the reservation price of fertility. The results from this exercise are displayed in Figure 2.1 and Figure 2.2. The different panels report the results obtained using different model specifications (columns the tables from which we obtained the estimates to construct these figures). The figures on the left show the CDF of the reservation price, while those on the right display the PDF of it.

Recall that the reservation price of fertility is the required transfer amount to induce a female to have an additional child. Hence, the CDF at zero is the fraction of the infra-marginal females – the ones who would have a child irrespective of the program. From Figures 2.1 to 2.2, we find that the CDF of the reservation price of fertility is small at zero, which is simply another way to say that fertility levels are indeed low in Korea. More importantly, the current level of the program's generosity moves the relevant threshold to childbearing only so slightly relative to the spread of the distribution of the reservation price of fertility. We use a black vertical reference line to denote the zero of the X-axis. The red line that nearly touches the black reference line denotes the average baby bonus incentive observed in the data. The total fertility effects of the program are the difference between the height of the CDF of the reservation price of fertility when it crosses the red reference line and the height of the CDF of the reservation price of fertility when it crosses zero, the black reference line. Alternatively, looking at the density function is the area underneath the PDF in between the two reference lines. Since the mass of females for which the reservation price of fertility is in between zero and the program's benefit level is small, the program only adds a small fraction of the females into parenthood.

Using the results from the baseline specification of the Probit model, we obtained an estimated standard deviation for the unconditional distribution of the reservation price of fertility of 14.61 thousand dollars. This value is, as expected, higher than the estimated standard deviation of the unobserved component of the reservation price of fertility, σ_u. To put this number in perspective, the baby bonus value for the first children in 2016 was 815 dollars. This suggests that the size of the baby bonus incentive for the first child in 2016 amounts to around 5% of one standard deviation of the reservation price of fertility ϵ. In other words, compared to the heterogeneity in reservation price of fertility among females in Korea, going from zero dollars to 815 dollars of the benefit will not cover a lot of mass under the area of the density function.

2.6.4 Baby Bonus Benefit Levels, Fertility Rates, and the Program's Budget

Although our estimates use a short window – two years – and condition on the current family size, we can use them to obtain implied effects on total fertility over the course of a female's childbearing years. To highlight the policy effect from a different point of view, we estimate the required budget expansion for doubling the current birth rate. Doubling fertility is a reasonable benchmark as it would bring Korea very close to the *replacement level of fertility*.

The replacement level of fertility is defined as a level of the *Total Fertility Rate* of 2.1. This is roughly the required number of children per female that would ensure that on average, in a large population, the females would produce enough daughters that survive up to and through their childbearing years, so the population is stable without migration flows.[23] Figures 2.3 and 2.4 depict estimates of the program's expected expenditure associated with different values of the program's benefit level. In Figure 2.3, the estimates reported in Columns (1) through (3) are used, while we use the estimates in Columns (4) through (6) for the next figure.

These figures collectively indicate that a tremendous increase in budget expenditure is required to reach anywhere near the replacement level of fertility. The rate of increase in budget expenditure is much faster than that of increase in birth rates. This means that when the government attempts even a small increase in transfer amounts, it will hopelessly add an enormous financial burden. In fact, as Table 2.10 illustrates, the increase in the

[23] The total fertility rate is one of the key indicators of fertility. Its is proportional to an *un-weighted* average of fertility rates using equal weights for females of different age groups, as opposed to weights proportional to the proportion of females at each age group. Its exact definition and how it relates to the outcomes of interest in this paper are discussed in detail in Appendix 2.A.1.

expenditure for the program needs to be at least 43 times. When it comes to estimates from Probit and Logit models, the required increase in the budget is at least 50 times the current level.

It is also useful to note that the confidence intervals for this object are asymmetric. The normal distribution is a poor approximation to the finite sample sampling distribution of this object since it is written as a non-linear transformation of the parameters and the data. As a result, the distribution of our estimates presents a quite long right-tail but a short left-tail. Thus, we can be very confident that if the budget required to double fertility is actually lower than our estimates, it is likely to be close to our estimates. On the other hand, if we underestimated how much it takes to double fertility, we might have underestimated it by quite a lot. This asymmetry is displayed in the unequal distance of our lower and upper bounds from our point estimate in our 95% confidence interval. Bringing in the uncertainty of our estimates into consideration reinforces our point that making large changes in fertility rates, such as the one required in Korea, by means of a pro-natalist policy, will likely require expenditures that are much larger than the budget allocated currently to this type of program.

One might be tempted to interpret our results as being artificially coming from the fact that we are defining a short interval to analyze fertility. This is not the case. Since we observe females of different ages and we can compute age-specific fertility rates and age by family size baby bonus effects, we can relate our results to total fertility defined as fertility at the end of the childbearing years. In Appendix 2.A.2, we discuss how the *total fertility rate* relates to the outcomes we use in our binary-choice models.[24] We also show below some complementary piece of information: Graphical evidence of the effects of the baby

[24] In an alternative exercise, we use the *total fertility rate* as our outcome of interest. The results in terms of the budget expansion required to double the total fertility rate are similar to the ones we report here and available through request.

bonus policy in the *total number of observed births* in Korea since the program's inception. This evidence should ease the concerns that our results could be in a way mechanically driven by the way we define our outcome variable.

It is useful to compare the spending and fertility effects of the baby bonus with a setting under which the government is able and willing to implement third-degree discrimination. If the government were to be perfectly informed about the female's reservation price, then it could transfer to each agent an amount exactly equal to her reservation price of fertility, preventing all infra-marginal spending. In fact, the negative reservation price in Figures 2.1 and 2.2 implies that those females are even willing to pay a tax in order to have a child. The density to the right of zero displays the share of females that must be compensated in order to be induced to childbirth. Thus, theoretically, the government can collect a tax from the infra-marginal females and transfer it to those females requiring financial incentives using the collected tax.

Naturally, we do not expect governments to implement such a scheme. More than just an inability to observe the agents' reservation price of fertility, third-degree discrimination means that the government would have to treat otherwise identical citizens with different benefits. Fairness concerns would prevent such a strategy from being implemented.

We can summarize our findings as a fundamental difficulty associated with universal and fair fertility programs. If one wishes to achieve a higher fertility rate by means of larger transfers, any percentage increase k in the program's benefit d will induce an increase in fertility by approximately by k times the elasticity of aggregate fertility to benefit levels. However, that will come at an increase in the program's budget of the order of k squared, since the government must pay for not only the just induced births but also all of those that were already induced before. This means that the increase in the program's budget grows at a rate much faster than the program's effect on fertility outcome, making any attempts to increase the program's effect by increasing its benefit bound to be limited by a dramatic rise in the program's cost. At small values of d, the program cannot possibly affect many individuals since there will be not many individuals that are close to being indifferent between having and not having a child. For small values of d, it is also true that most of the program's budget will be spent on individuals that would have a child without the program. However, large values of d are going to be realistically infeasible since the program's budget will grow at the rate of the square of the benefit d.

This delicate balance that policymakers must strike can be clearly visualized when we observe aggregate fertility levels in Korea. In Figures 2.5 and 2.6, we plot the evolution of births in the country, from 1981 until 2016. These data are obtained from the Vital Statistics from the national statistical database (KOSIS). We combine the information of the births, the location of the birth, and the parity order, to compute the implied aggregate expenditure by the government on the baby bonus program each year.

In examining the trends in Figures 2.5 and 2.6, we note that there is a smooth downward trend in aggregate fertility, the total number of newborns, in Korea, that dates back to the 1980s. We also observe that around 2004, the government expenditures with the pro-natalist program start. Since then, the expenditure has risen from zero to twenty million, and then to two hundred million.[25] Yet still, there is barely any noticeable change in the aggregate fertility rate in the country. This is despite the fact that the expenditures on fertility incentives increased tenfold multiple times.

The evidence from aggregate fertility suggests that there are no noticeable changes in

[25] The allocated budget for the program is reported by about 230 billion KRW (roughly 230 million USD) in 2016, according to the Annual Casebook of Population Policy of Local Governments published by the Ministry of Health and Welfare.

the number of births of the second or third child. There is some evidence that the downward trend on the births of the first child slowed down to a certain extent after the introduction of the program. This is consistent with the results of our previous estimates of the fertility responses to the policy, which we estimate to be small but still different from zero. Looking at the total number of births irrespective of birth-order, we get a picture very similar to the one we obtained for the second and third child. It is worth noticing that, the program's expenditures with the third child grew by more than tenfold between 2006 and 2016. In the same period, the number of children associated with this expenditure – that is, births of a third child – barely changed at all. The number of births of third child is in fact, quite similar to the levels that were observed since 1986, with the difference that between 1986 and 2003, there policy was not in place, so there was no expenditure associated with it.

It is useful to point out that since these graphs represent the total number of births observed in Korea in this period, they bypass the need to carefully consider the effects of the window of observation we used in our main regression exercises. We are counting here each and every birth, from each and every female, over the course of the years. Although the program's expenditure rises dramatically since its conception in 2004, there's barely any noticeable change in the total number of births in the country. We don't observe a noticeable change in the level of the series, neither on its derivative, its trend.

It is theoretically clear that the effect of a cash transfer that is conditional on having a child should have a greater than or equal to zero effect on the likelihood that a female decides to have a child.[26] However, there is no reason to expect that simply because the effect is weakly positive, and it will be large enough to be economically meaningful. Figures

[26]The effect on the total number of children can be negative if the income effect associated with the baby bonus transfer dominates the substitution effect. However, the income effect does not exist for those at the corner, that is, those that would have otherwise chosen not to have children. For further details on this discussion, see Milligan (2005).

2.5 and 2.6 suggest that, despite the enormous changes in expenditure with this type of program in Korea in the last 15 years, there no evidence that the fertility responses were anywhere close to the levels that policymakers expect from this type of policy. We can only conclude that, by and large, the expenditure associated with this policy consists mostly of transfer to parents that would have chosen to have another child anyway. There is simply no way to fix that by means of increases in the cash transfer's benefit level since the implied change in the expenditure is going to rise much faster than the change in fertility, which is another phenomenon that is absolutely clear in these graphs.

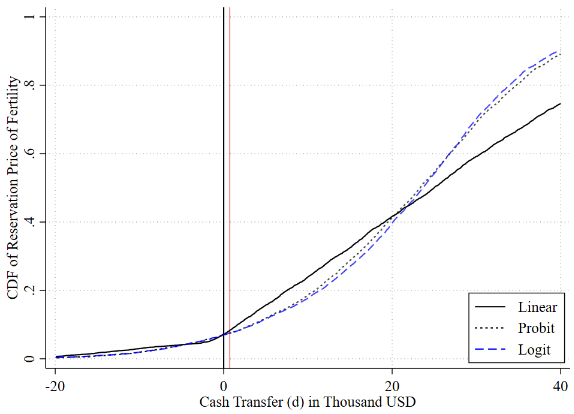
2.7 Conclusion

We study the fertility effects of a pro-natalist Korean policy called the "baby bonus" program. Our findings suggest that although the program does increase the likelihood that a female will have a child, over 75% of the baby bonus budget is spent on infra-marginal females, that is, those that would still have chosen to have a child even in the absence of the program. We revisit some results in this literature and conclude that our findings are not specific to the Korean setting but instead part of a more widespread phenomenon. We estimate the share of births in the treated group that would still be observed even in the absence of the policy for similar programs implemented in Quebec and in Isreal. Our estimates of the share of infra-marginal births were greater than 90 percent in every country we country analyzed.

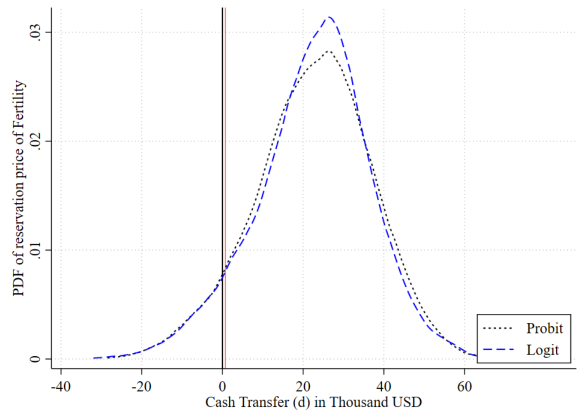
We use estimates from conditional choice probabilities obtained from standard binary choice models to report estimates of the *unconditional distribution of the reservation price of fertility*, which is the exact amount of monetary incentives required to induce a female to have a child. Our estimates of the distribution of the reservation price of fertility highlight a delicate balance that governments must strike: On the one hand, they want the program to induce an effect on birth rates. On the other hand, to do so, they must pay all of those that decide to have a child the exact same amount (a no third-degree discrimination assumption). As a result, the program's spending must grow at a rate that is much faster than the program's effect on fertility. Our results suggest that pro-natalist programs such as the Korean baby bonus are bound to have only minuscule effects on fertility levels. This suggests that alternative approaches in order to raise fertility might be more fruitful in achieving the policymakers' goals.

2.8 Figures

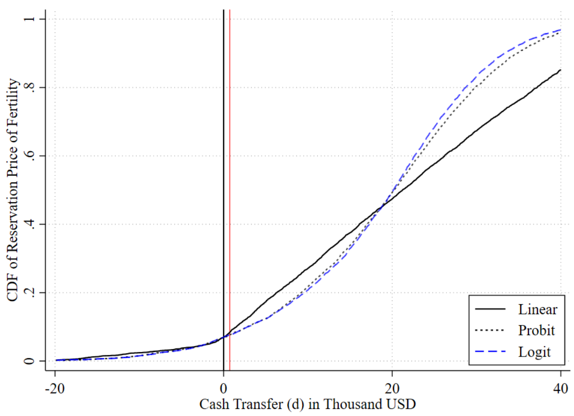
Figure 2.1: Reservation Price of Fertility Distribution



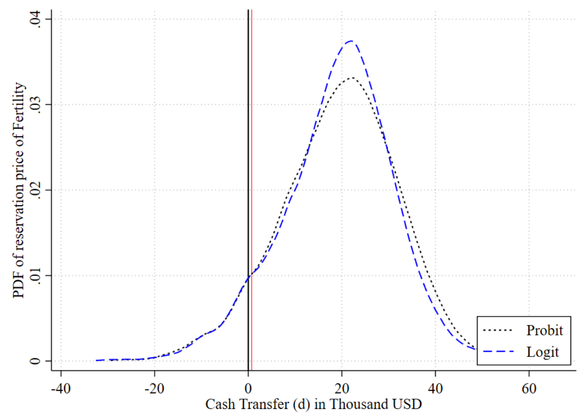
(a) CDF of Column (1)



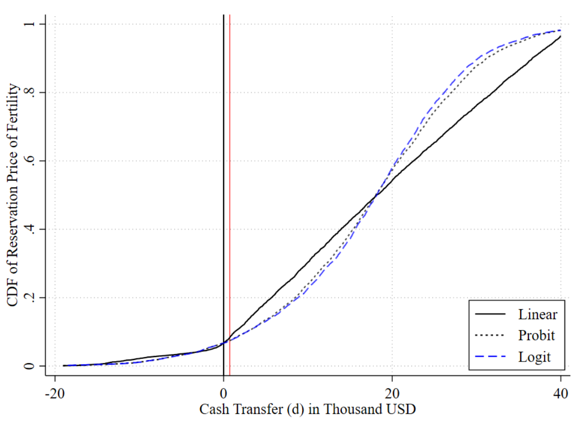
(b) PDF of Column (1)



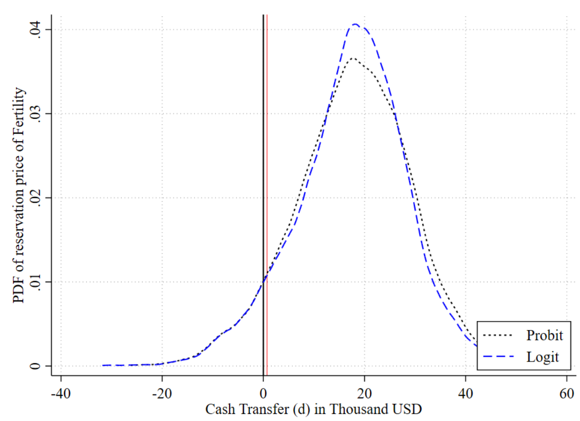
(c) CDF of Column (2)



(d) PDF of Column (2)



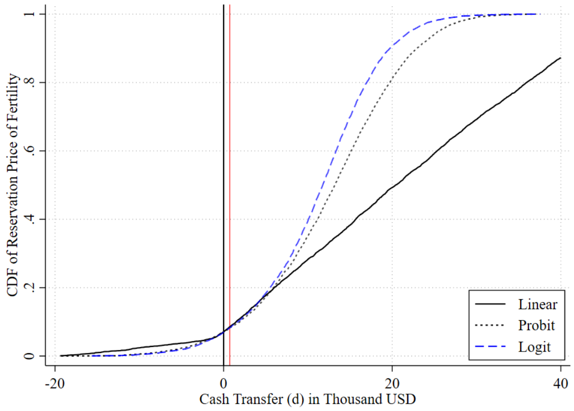
(e) CDF of Column (3)



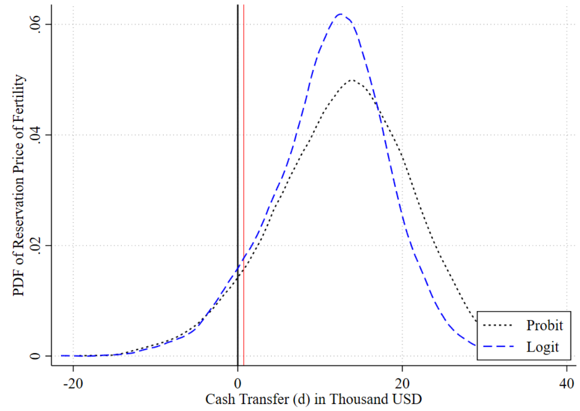
(f) PDF of Column (3)

Notes: Left panels show the CDF of reservation price using the estimation results of the first three columns in Table 2.8 and Table 2.9. Local linear approximation in solid black line is added for reference. Right panels show the analytical PDF of reservation price from the estimated CDF. The first vertical line in black indicates the zero transfer amount. The second vertical line in red shows the current baby bonus at the average level. Cash transfer is measured in 1,000 USD.

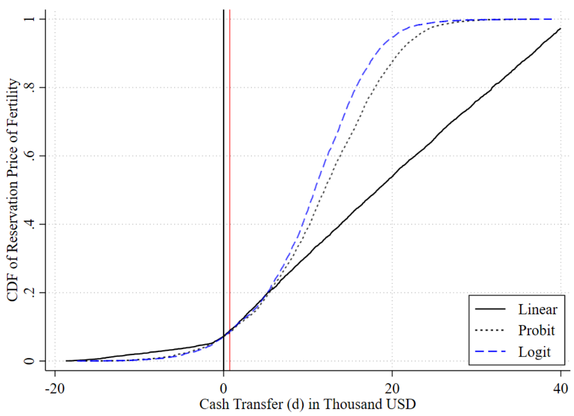
Figure 2.2: Reservation Price of Fertility Distribution – Alternative Specifications



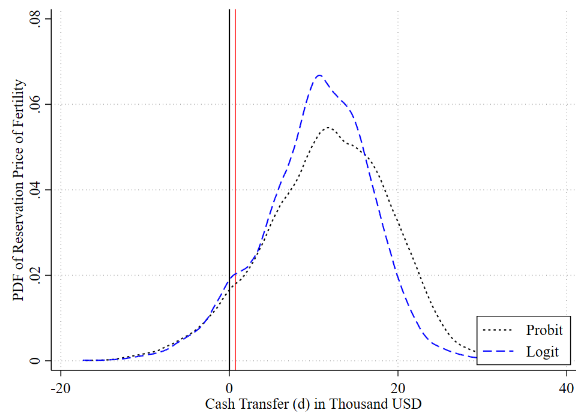
(a) CDF of Column (4)



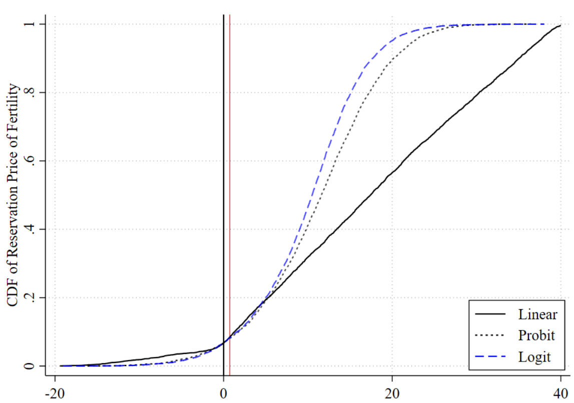
(b) PDF of Column (4)



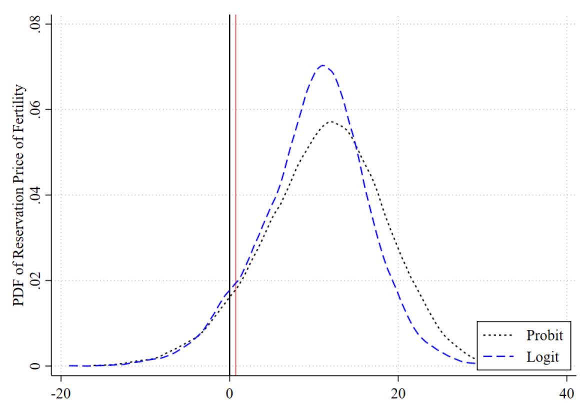
(c) CDF of Column (5)



(d) PDF of Column (5)



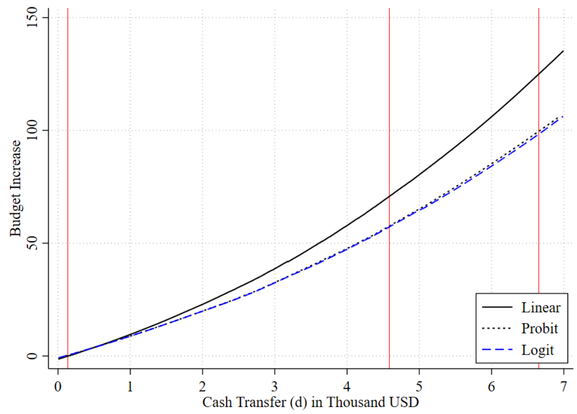
(e) CDF of Column (6)



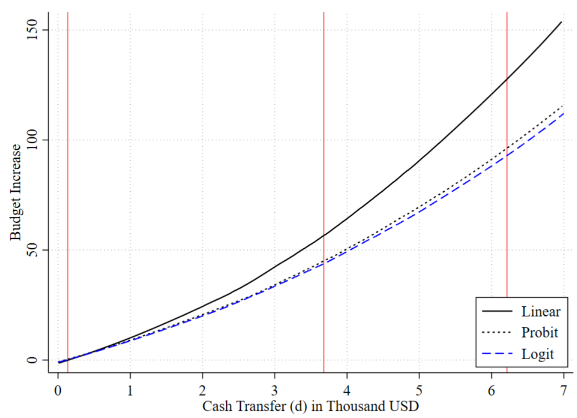
(f) PDF of Column (6)

Notes: Left panels show the CDF of reservation price using the estimation results of the first three columns in Table 2.8 and Table 2.9. Local linear approximation in solid black line is added for reference. Right panels show the analytical PDF of reservation price from the estimated CDF. The first vertical line in black indicates the zero transfer amount. The second vertical line in red shows the current baby bonus at the average level. Cash transfer is measured in 1,000 USD.

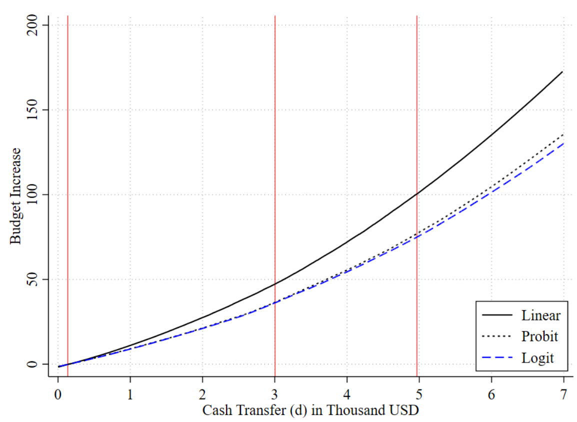
Figure 2.3: Baby Bonus Benefit Level and The Program's Budget



(a) Estimate in Column (1)



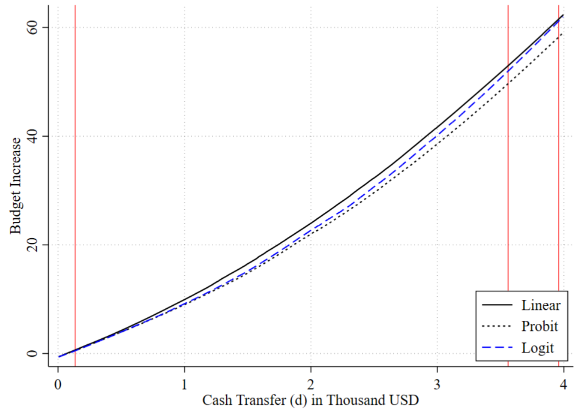
(b) Estimate in Column (2)



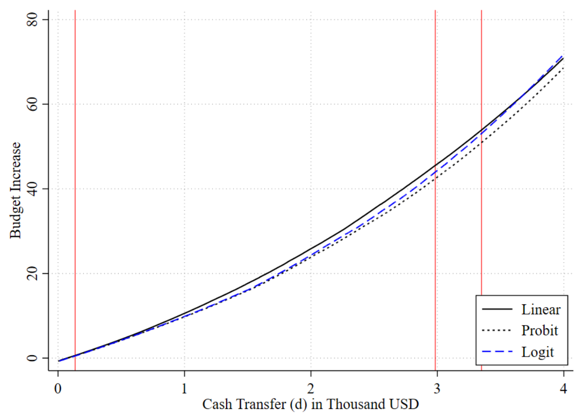
(c) Estimate in Column (3)

Notes: Figures show the required budget increase relative to the current budget expenditure to double the current fertility rate, close to the replacement level fertility, using the estimation results of the first three columns in Table 2.6, Table 2.8, and Table 2.9. The X-axis shows the cash transfer amount until the birth rate doubled, and the Y-axis shows the increase in budget expenditure with that transfer level. The first vertical line indicates the current average level of the baby bonus. The next two vertical lines show the required budget increase under the local approximation and Logit model, respectively.

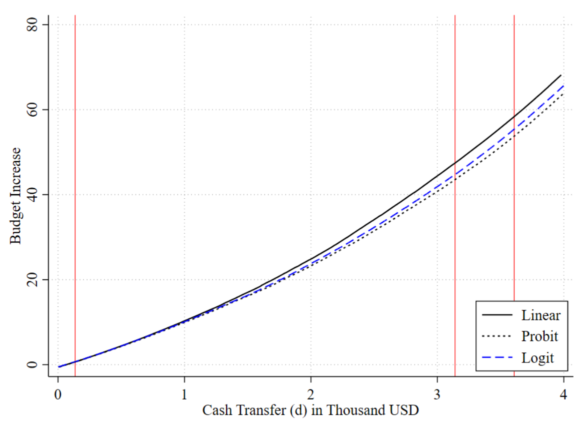
Figure 2.4: Baby Bonus Benefit Level and The Program's Budget – Alternative Specifications



(a) Estimate in Column (4)



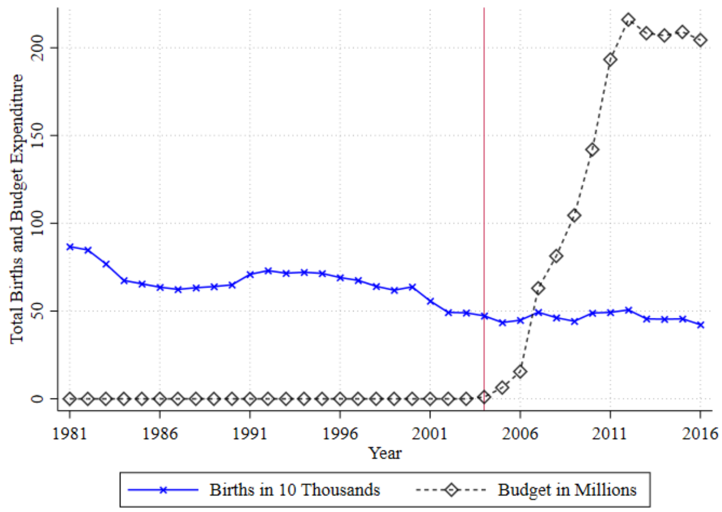
(b) Estimate in Column (5)



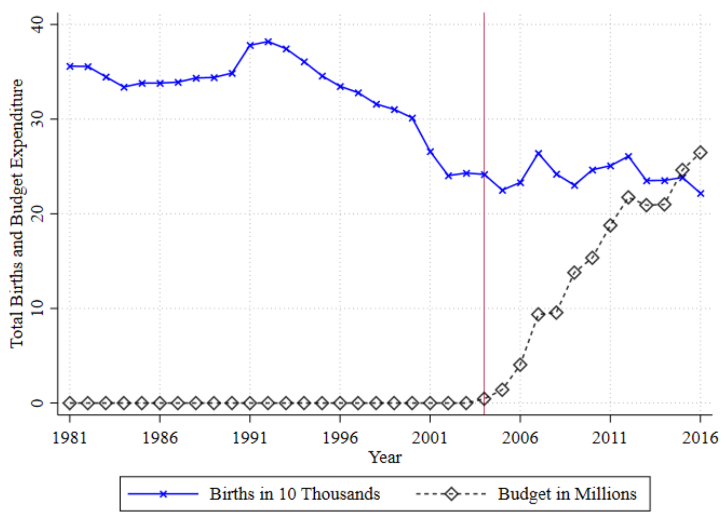
(c) Estimate in Column (6)

Notes: Figures show the required budget increase relative to the current budget expenditure to double the current fertility rate, close to the replacement level fertility, using the estimation results of the first three columns in Table 2.6, Table 2.8, and Table 2.9. The X-axis shows the cash transfer amount until the birth rate doubled, and the Y-axis shows the increase in budget expenditure with that transfer level. The first vertical line indicates the current average level of the baby bonus. The next two vertical lines show the required budget increase under the local approximation and Logit model, respectively.

Figure 2.5: Total Number of Births and Estimated Budget Expenditure (1981-2016)



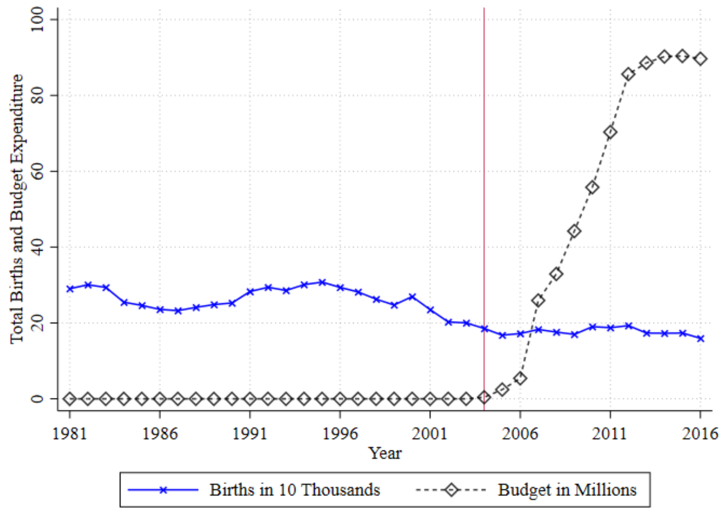
(a) All Children



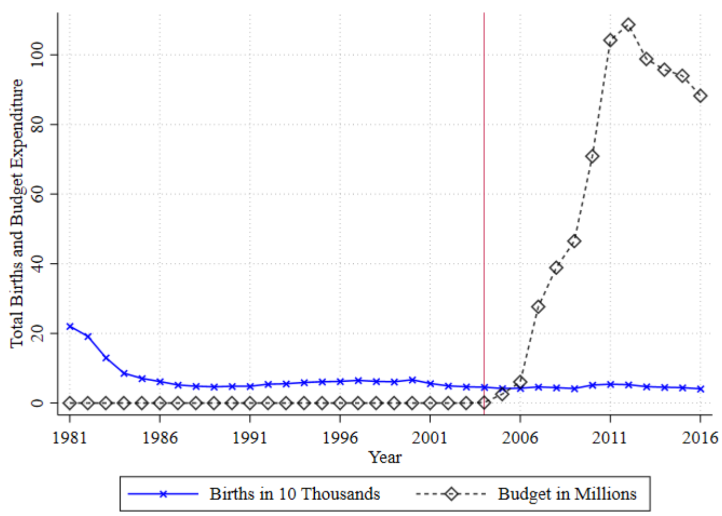
(b) First Child

Source: Vital Statistics from the KOSIS (the national statistical database).
Notes: The total number of births are in 10,000. The budget expenditure is in 1,000,000,000 KRW (roughly, 1,000,000 USD). The first vertical line indicates the first introduction of the baby bonus in the municipal ordinance in 2004.

Figure 2.6: Total Number of Births and Estimated Budget Expenditure (1981-2016)



(a) Second Child



(b) Third Child

Source: Vital Statistics from the KOSIS (the national statistical database).
Notes: The total number of births are in 10,000. The budget expenditure is in 1,000,000,000 KRW (roughly, 1,000,000 USD). The first vertical line indicates the first introduction of the baby bonus in the municipal ordinance in 2004.

2.9 Tables

Table 2.2: Average Baby Bonuses (USD)

Year	2007	2008	2009	2010	2011
First Child	594.495	592.793	685.296	718.73	732.004
Second Child	998.489	1001.45	947.909	969.123	1062
Third Child	2207.79	2109.08	2270.5	2481.97	2725.64
Minimum Wage	906.854	958.463	971.286	970.814	991.66
Year	2012	2013	2014	2015	2016
First Child	798.204	890.273	886.695	806.114	815.138
Second Child	1063.18	1151.49	1162.29	1182.13	1233.33
Third Child	2835.78	2878.6	2843.85	2886	3074.9
Minimum Wage	1010.91	1049.62	1110.77	1174.38	1260.27

All amounts are CPI-adjusted and converted in USD.
Notes: Table shows the average cash transfer amount when the program is introduced. First child, second child, and third child denote the average cash transfer amounts for each parity births in that year.
The minimum wage denotes the monthly salary computed based on the minimum wage each year, assuming individuals would work for 52 hours per week.

Table 2.3: Proportion of Districts with Baby Bonus Program (%)

Year	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
First Child	21.65	24.24	27.71	29.87	36.80	37.23	39.74	39.91	42.98	47.81
Second Child	37.23	46.32	64.07	71.00	77.49	85.71	87.77	87.72	88.60	89.47
Third Child +	55.41	75.76	86.58	93.94	96.54	97.84	99.13	99.12	99.12	98.68
Total Districts	228	228	228	228	228	228	225	225	225	225

Notes: Table shows the proportion of districts with the baby bonus for each parity birth. Two districts in Jeju island are combined as one district. The number of total districts decreased due to municipality mergers.

Table 2.4: Eligibility Condition

	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
0 Months (No Requirement)	21	26	40	42	47	50	49	51	52	54
1 Month	2	3	4	5	7	7	7	9	12	8
90 Days	0	0	0	1	1	1	1	1	1	1
3 Months	13	16	21	22	24	24	24	25	23	23
180 Days	3	5	6	7	8	9	9	9	10	9
6 Months	27	29	35	40	50	52	52	50	46	48
10 Months	0	1	2	2	2	2	2	2	2	2
12 Months	31	45	53	59	51	50	55	55	59	61
Others	0	0	0	2	2	3	3	3	3	3
Not Available	131	103	67	48	36	30	24	20	17	16

Notes: The eligibility condition in terms of length of residency is reported. Four districts in Jeju island are combined as one district. Two districts in Others have different residency requirements for different parity births. The other requires two years of residency in the district to be eligible for the benefit. Not available indicate the districts where do not specify the residency requirements in their municipal ordinance or have no baby bonus program.

Table 2.5: Summary Statistics

	Mean	SD	Min.	Max.
$1[\text{Childbirth}]_t$	0.072	0.258	0.000	1.000
Cash Transfer	0.135	0.472	0.000	9.584
Age	24.239	3.494	19.000	30.000
Unmarried	0.870	0.336	0.000	1.000
# Children_{t-1}	0.066	0.247	0.000	1.000
Income	63.361	37.519	0.000	793.113
Observations	4839			
H: Age	32.162	4.127	21.000	56.000
H: Years of Schooling	14.458	2.071	0.000	22.000
H: Monthly Salary	3.951	1.734	0.000	13.109
H: Hours Worked	8.361	1.924	1.429	15.857
Observations	629			

Notes: Table shows the summary statistics of the data of the all year between 2008 and 2016 (biannual). For the husband-related variables, the summary statistics of the married are included. Monetary values are reported in 1,000 USD and CPI-adjusted.

Table 2.6: Local Linear Approximation Using Uniform Distribution

	(1)	(2)	(3)	(4)	(5)	(6)
Cash Transfer	0.017**	0.020***	0.024***	0.021**	0.024***	0.025***
	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)
Age	0.006***	0.006***	0.006***	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unmarried	-0.387***	-0.765***	-0.627***	-0.387***	-0.769***	-0.631***
	(0.013)	(0.070)	(0.113)	(0.013)	(0.071)	(0.114)
# Children$_{t-1}$	-0.069***	-0.064***	-0.073***	0.006	-0.162	-0.114
	(0.017)	(0.017)	(0.018)	(0.210)	(0.212)	(0.213)
Income		-0.050	-0.121		-0.050	-0.120
		(0.085)	(0.087)		(0.085)	(0.087)
H: Age		-0.012***	-0.012***		-0.012***	-0.012***
		(0.002)	(0.002)		(0.002)	(0.002)
H: Years of Schooling			0.002			0.002
			(0.005)			(0.005)
H: Monthly Salary			0.023***			0.023***
			(0.005)			(0.005)
H: Hours Worked			0.004			0.004
			(0.005)			(0.005)
# Children$_{t-1}\times$ Cash Transfer				-0.011	-0.009	-0.003
				(0.015)	(0.015)	(0.015)
# Children$_{t-1}\times$ Age				-0.002	0.004	0.002
				(0.007)	(0.007)	(0.007)
Constant	0.277***	0.652***	0.523***	0.275***	0.657***	0.527***
	(0.031)	(0.075)	(0.115)	(0.031)	(0.076)	(0.116)
Infra-marginal Ratio (Births)	0.968	0.962	0.956	0.966	0.960	0.955
Infra-marginal Ratio (Spending)	0.828	0.798	0.765	0.791	0.764	0.753
Observations	4839	4839	4839	4839	4839	4839
R-squared	0.256	0.261	0.264	0.256	0.261	0.264

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.
Dependent variable: Childbirth outcome in the next two years.
Notes: Table reports estimation results using the linear regression. The first three columns estimate the effect of the cash transfer, while the next three columns allow heterogeneous effects for each birth order. Cash transfer and monthly salary are reported in 1,000 USD, while household income is measured in 1,000,000 USD. The infra-marginal ratio of birth is reported at the bottom of the table.

Table 2.7: Local Linear Approximation – Country Comparison

	Country		Model 1	Model 2	Model 3	
Infra-marginal Ratio (Births)	Korea	Table 6	0.968***	0.962***	0.956***	
			(0.021)	(0.023)	(0.023)	
	Quebec, Canada	Table 6	0.947	0.928	0.920	Milligan (2005)
	Israel	Table 3	0.990	0.976	0.976	Cohen et al. (2013)
Infra-marginal Ratio (Spending)	Korea	Table 6	0.828***	0.798***	0.765***	
			(0.103)	(0.106)	(0.108)	
Control Variables	Benefit		YES	YES	YES	
	Female-related		NO	YES	YES	
	Family-related		NO	NO	YES	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Bootstrapped SE in parentheses.
Notes: The Infra-marginal ratio is reported at the bottom of the table, showing the share of the infra-marginal females. The first, second, and third-row report the infra-marginal ratio of birth in South Korea, Quebec in Canada, and Israel, respectively. The values are computed using the implied percentage increase in birth probability reported in Milligan (2005) and Cohen et al. (2013).

Table 2.8: Effects of the Cash Transfer Program - Probit Regression

	(1)	(2)	(3)	(4)	(5)	(6)
Cash Transfer	0.008	0.009*	0.010**	0.011**	0.012**	0.013**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Age	0.009***	0.009***	0.009***	0.007***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unmarried	-0.283***	-0.685***	-0.650***	-0.273***	-0.653***	-0.596***
	(0.026)	(0.110)	(0.186)	(0.026)	(0.119)	(0.203)
# Children$_{t-1}$	-0.020***	-0.019**	-0.021***	-0.006	-0.009	-0.011
	(0.008)	(0.008)	(0.008)	(0.016)	(0.014)	(0.014)
Income		-0.072	-0.148		-0.076	-0.153
		(0.091)	(0.103)		(0.092)	(0.104)
H: Age		-0.004***	-0.004***		-0.004***	-0.004***
		(0.001)	(0.001)		(0.001)	(0.001)
H: Years of Schooling			-0.001			-0.000
			(0.002)			(0.002)
H: Monthly Salary			0.006**			0.006**
			(0.003)			(0.003)
H: Hours Worked			0.001			0.001
			(0.002)			(0.002)
Implied %	0.027	0.031	0.034	0.025	0.030	0.033
Observations	4839	4839	4839	4839	4839	4839
Log Likelihood	-837.936	-832.468	-830.361	-835.208	-830.411	-828.332

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.
Dependent variable: Childbirth outcome in the next two years.
Notes 1: Implied % stands the implied % for Increase in Birth Probability
Notes 2: The average partial effects of Probit model estimation results are reported in the table. The APE shows the changes in birth probability caused by the covariate at the average level. The first three columns estimate the effect of the cash transfer, while the next three columns allow heterogeneous effects for each birth order. Cash transfer and monthly salary are reported in 1,000 USD, while household income is measured in 1,000,000 USD.

Table 2.9: Effects of the Cash Transfer Program - Logit Regression

	(1)	(2)	(3)	(4)	(5)	(6)
Cash Transfer	0.008*	0.009**	0.010**	0.012**	0.013***	0.013***
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
Age	0.009***	0.009***	0.009***	0.007***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unmarried	-0.269***	-0.679***	-0.641***	-0.252***	-0.636***	-0.573***
	(0.026)	(0.108)	(0.184)	(0.025)	(0.120)	(0.202)
# Children$_{t-1}$	-0.018**	-0.017**	-0.019***	-0.005	-0.006	-0.008
	(0.007)	(0.007)	(0.007)	(0.013)	(0.012)	(0.012)
Income		-0.072	-0.181		-0.076	-0.186
		(0.098)	(0.116)		(0.097)	(0.115)
H: Age		-0.003***	-0.004***		-0.003***	-0.003***
		(0.001)	(0.001)		(0.001)	(0.001)
H: Years of Schooling			-0.001			-0.000
			(0.002)			(0.002)
H: Monthly Salary			0.006**			0.006**
			(0.003)			(0.003)
H: Hours Worked			0.001			0.001
			(0.002)			(0.002)
Implied %	0.029	0.034	0.037	0.028	0.032	0.035
Observations	4839	4839	4839	4839	4839	4839
Log Likelihood	-845.567	-839.632	-837.306	-841.617	-836.373	-834.096

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.
Dependent variable: Childbirth outcome in the next two years.
Notes 1: Implied % stands the implied % for Increase in Birth Probability
Notes 2: The average partial effects of Logit model estimation results are reported in the table. The APE shows the changes in birth probability caused by the covariate at the average level. The first three columns estimate the effect of the cash transfer, while the next three columns allow heterogeneous effects for each birth order. Cash transfer and monthly salary are reported in 1,000 USD, while household income is measured in 1,000,000 USD.

Table 2.10: Required Budget to Double Fertility

	Model 1	Model 2	Model 3
Linear Model	67.719	55.710	43.908
	[30.228, 514.471]	[29.062, 335.058]	[25.332, 137.189]
Probit	96.561	91.143	82.851
	[51.237, 785.501]	[50.507, 390.584]	[43.718, 246.965]
Logit	103.968	91.319	82.715
	[54.659, 420.911]	[55.140, 327.164]	[48.696, 226.701]
	Model 4	**Model 5**	**Model 6**
Linear Model	60.762	46.443	46.641
	[21.482, 409.086]	[31.946, 168.389]	[24.299, 200.459]
Probit	63.057	55.414	55.928
	[61.905, 1150.584]	[35.954, 418.771]	[32.433, 221.431]
Logit	59.779	52.800	50.227
	[74.342, 1491.573]	[35.968, 283.345]	[33.326, 151.944]

Bootstrapped 95% confidence interval in brackets.
Notes: Coefficients displayed report the estimates of how many times larger the program's budget should be to double the fertility rate in Korea, based on the estimates in Table 2.6, 2.8, and 2.9, respectively.

2.A Appendix

2.A.1 Deriving the Conditional and Unconditional Distributions of the Reservation Price of Fertility

Simulating the Unconditional Distribution of the Reservation Price of Fertility

One way to obtain the distribution of the reservation price of fertility is by performing analytical integration of the derivative of the conditional mean function over the distribution of the covariates X. However, to avoid dealing with multiple integrations, we instead rely on a numerical procedure to obtain draws from the implied distribution of the reservation price of fertility. In this section, we explain our procedure and prove that it generates draws from a distribution that coincides with the distribution of interest, the distribution of the reservation price of fertility.

To begin, assume that the parameters β and σ_u of the binary-choice model known. Naturally, in the actual empirical exercise, these objects are going to be replaced by their estimates.

1. Generate for each observation i a random, independent draw from the standarized distribution of the error term u implied by the binary choice model, denote it by $\tilde{u}_i$. For example, in the probit model, generate a draw from a standard normal random variable.
2. Compute $\tilde{\epsilon}_i \equiv \sigma_u \tilde{u}_i - X_i\beta$
3. End

Algorithm 2.A.1: Drawing from the distribution of the reservation price of fertility

The distribution of $\tilde{\epsilon}$ coincides with the distribution of the reservation price of fertility ϵ, for a given value of the vector of parameters (β, σ_u). Thus, by plotting the distribution of the simulated draws $\tilde{\epsilon}$, we are plotting an estimate of the distribution of the reservation price of fertility that is implied by the functional form assumptions of the binary-choice model and the parameter estimates.

The CDF of the reservation price of fertility, $F_\epsilon(d)$ measures the proportion of individuals that would choose to have a child if the value of the baby bonus were to be set externally to d. In other words, it computes $Pr[\epsilon < d]$, the fraction of individuals for which the reservation price is lower than the point of evaluation, d. Our goal now is to show that $Pr[\epsilon < d]$ should coincide at every point of evaluation d with the distribution we obtain from the algorithm above.

To begin, we fix an arbitrary point of evaluation d and note that:

$$Pr[\tilde{\epsilon} < d] = Pr[-X\beta + \sigma_u \tilde{u} < d]$$

Using the law of iterated expectations, we get that:

$$\begin{aligned} Pr[\tilde{\epsilon} < d] &= E_X[Pr[-X\beta + \sigma_u \tilde{u} < d|X]] \\ &= E_X[Pr[\tilde{u} < X\frac{\beta}{\sigma_u} + \sigma_u^{-1} d|X]] \end{aligned}$$

Using the ignorability assumption, we can write the right-hand side of this equation as:

$$\begin{aligned} Pr[\tilde{\epsilon} < d] &= E_X[Pr[\tilde{u} < \sigma_u^{-1} D - X\frac{\beta}{\sigma_u}|X, D = d]] \\ &= E_X[Pr[Y(D) = 1|X, D = d]] \\ &= E_X[Pr[Y(d) = 1|X]] \\ &= Pr[Y(d) = 1] = F_\epsilon(d), \end{aligned}$$

where in the first equality we used ignorability, the second we used the functional form assumption on u, the third we used ignorability, and the last we used the law of iterated expectations.

Thus, at any point of evaluation d, the distribution obtained from the simulated draws $\tilde{\epsilon}$ will coincide with the distribution of the reservation price of fertility. Naturally, in finite samples, we do not observe the true values of β or σ_u, so in practice, they must be replaced by their consistent estimators. But, provided that the functional form assumption of the binary-choice model is correctly specified and that the parameter estimates approach their true values as the sample size gets large, the estimates of the distribution of the reservation price of fertility obtained from this procedure will, up to a simulation error, approximate the true values of the unconditional distribution of the reservation price of fertility.

2.A.2 Fertility Rates – Objects and Their Measurement

There is a large literature in Demography dedicated to the measurement of population and the forces that lead to population changes over time (births, deaths, and migration flow).

A key object of interest in this literature is the *Total Fertility Rate*. This object aims to measure, in a certain sense, the number of children women of a certain group have, on average, in the course of their childbearing years. The total fertility rate is defined as the answer to the following question: If we followed a hypothetical female from the beginning until the end of her childbearing years – assuming that she would certainly live until the end of her childbearing years – and in each and every year we were to attribute to her the fertility rate that is observed by the current fertility associated with that age group, then what is the expected number of children that she would have had at the end of her childbearing years?

To understand how this object is defined, recall that we use Y as the binary indicator that a female had a child in our sample in the period we analyze. Thus, for the female i, Y_i will be one if she had children in the last twelve months and zero otherwise. We observe a random sample of the population and observe the age of the female. We are going to use A_i to denote the female's age at the time of our survey and N_g the number of females with age A_i equal to g. Using these definitions, then the *Total Fertility Rate* (TFR) is given by:

$$\textit{Total Fertility Rate} = \sum_g \bar{Y}_g$$

where $\bar{Y}_g = \frac{1}{N_g} \sum_i Y_i A_i = g$ is the average of the outcome Y for the females of age A equal to g. It is useful to multiply and divide by the total number of childbearing years, which we will denote by T so that we can write the total fertility rate as:

$$\textit{Total Fertility Rate} = T \sum_g \bar{Y}_g \frac{1}{T}$$

Note that the *Total Fertility Rate* is closely related to a *unweighted average* of fertility outcomes of females of different age groups. It is useful to note that, by the law of total expectation, the sample average of our key variable of interest, Y_i, is closely related to the total fertility rate as defined above. Note that:

$$\bar{Y} = \sum \bar{Y}_g \widehat{Pr}[A_i = g]$$

That is, the mean fertility observed in a one year period is equal to an *weighted average* of age-specific fertility rates, weighted by the proportion of the population that belongs to each age group. In contrast, the *Total Fertility Rate* puts identical weights to each age group. Another subtle difference is that the total fertility rate aims to count the total number of kids over the childbearing years, not the average number of years in which a female had a child. As a result, it behaves as a sum instead of an average, and thus the T, the total number of childbearing years, shows up outside of the summation.

Thus, except for these minor differences in the way that different age groups are treated in one measure versus another, there is a direct relationship between the total fertility rate and the outcome we study in our paper (the conditional mean of Y). For example, if a policy such as the baby bonus were to increase the observed fertility rate by one hundred percent at each age group, the effect of that policy on the key dependent variable of the models we study in this paper would be to increase the mean of Y by 100%. Note also that the policy would have the exact same effect on the *total fertility rate*: It would double it as well.

As a robustness exercise, we computed the implied effects of each and every counterfactual exercise we performed in our paper, measuring the changes in the TFR as opposed to the mean of Y as we do in our main empirical analysis. As expected, we obtain near-identical results since both objects attempt to measure the same phenomenon. The changes in the weighting scheme alter our estimates just a little, but the key take-away from our results remain the same. Importantly, the required budget needed to double the TFR, as opposed to the one needed to double the mean observed fertility in the sample, are nearly the same. These estimates are available through request.

2.A.3 Robustness Check 1

In this section, we report the results of several empirical exercises aimed to verify the robustness of our findings to the choice of controls and to functional form restrictions on the relationship between the outcome Y and the vector of covariates.

First, we use a propensity score re-weighting model to control for the differences in the distribution of observable characteristics across provinces in Korea. This works as an alternative to directly controlling for these attributes in our binary-choice models. In another exercise, we estimate the fertility effects of the baby bonus programs, varying the set of controls.

Since, in our setup, there are several treated and several control units with different characteristics, we must first choose a reference group. We choose Gyeonggi-do, a province geologically surrounding the capital city of Korea, as a reference group. The province is populated with 13.53 million residents and is the largest province with many megacities and rural areas. We use a binary choice model in which the outcome is whether or not the individual lives in Gyeonggi-do, and estimate this model separately for each pair of provinces, including Gyeonggi-do and one other province at the time. This allows us to compute province-specific propensity score weights that balance the observable characteristics (proportion of unmarried, age, etc.) across all provinces in Korea. By including the weighting, we make all other groups comparable to the reference province. In possession of these weights, we can re-estimate the models using the weights to ensure that provinces that face different levels of the policy are comparable (through the re-weighting scheme) in terms of their observable characteristics.

Table 2.11 displays the reweighting regression results. When the samples are re-weighted to be similar to the reference province, we observe that slightly larger effects of the cash transfer. The estimated effects are varying from a 2.3 to a 3 percentage point increase. The effects become larger when we include the interaction terms in the last three columns. In terms of the Infra-marginal ratio of births, we estimate it to be larger than 94%. Thus, our re-weighting estimation results reinforce the findings we obtain under the linear separability assumption we use in our baseline specification.

Now, we examine whether our results are sensitive to the choice of the controls. To do that, we re-estimate our models, including an additional set of covariates commonly used in this literature (Milligan 2005; Parent and Wang 2007; Malak et al. 2019). In these specifications, we assume that childbirth decisions are determined by females' years of schooling, heterogeneity across provinces, and year fixed effects in addition to the variables we used in our baseline exercise.

Table 2.12 reports the results of this exercise. Our estimates suggest that a USD $1,000 cash transfer leads to a 1.5 to 2.2 percent increase in birth probability. The proportion of infra-marginal births is estimated to be greater than 96%. The ratio is slightly higher than that of the main results, thereby supporting that our estimation results are robust to the inclusion of these controls.

2.A.4 Robustness Check 2

Our estimates of the reservation price of fertility distribution are obtained as a by-product of the functional form assumptions implied in different binary-choice models. Whenever one uses a linear probability model, the assumption that the conditional mean of the outcome given the covariates is linear implies that the unobserved error term u is uniform. Similarly, a Probit model assumes that the unobserved term u is normally distributed. We take advantage of the fact that the conditional distribution of the error terms is known in each binary-choice model and the fact that we can integrate out the covariates to obtain estimates of the unconditional distribution of the reservation price of fertility.

Naturally, our estimates of the distribution of the reservation price of fertility are only as good as our assumptions on the shape of the distribution of the unobserved error term u. In our main exercises, we use linear probability models, Logit, and Probit, which correspond to three distributional assumptions on u: Uniform, Normal, and Logistic.

Each distinct assumption we make yield a different distribution of the reservation price of fertility, as expected. However, the key findings, thus far, seem to be quite robust to the parametric restriction we impose on u. In this section, we further investigate this issue. To do that, we re-estimate our binary-choice models, using different functional forms on the distribution of the error term. Our goal is to see how much our results change when we deviate from the commonly used specifications in this literature.

We use the complementary Log-Log (cloglog) and Log-Log (loglog) models. Under the cloglog and loglog models, the conditional probability of the outcome Y given the covariates and the baby bonus is given by:

$$\text{cloglog}: Pr[Y=1|D,X] = 1-\exp\left(-\exp\left(\sigma_u^{-1}D + X\frac{\beta}{\sigma_u}\right)\right)$$
$$\text{loglog}: Pr[Y=1|D,X] = \exp\left(-\exp\left[-(\sigma_u^{-1}D + X\frac{\beta}{\sigma_u})\right]\right)$$

The inverse of this conditional probability distribution is a mapping that takes a childbirth probability on the $(0,1)$ interval and maps it into an index between $(-\infty,\infty)$ characterized by the covariates X and baby bonus, D. A key property of this mapping is that it approaches the extremes slower than the mapping associated with the Probit or the Logit distribution. Thus, these binary-choice models are used when the probability of an event is either very small or very large. They can better account for the case in which the distribution of u is asymmetric.

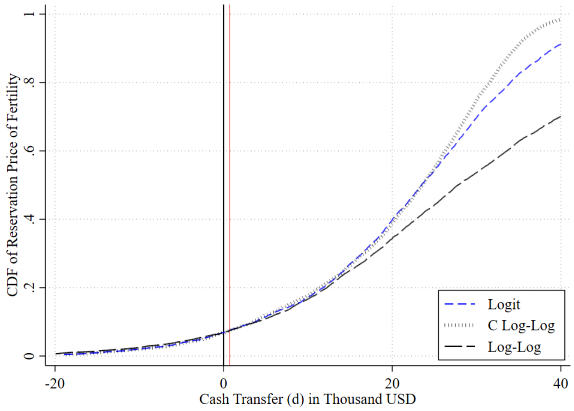
Figures 2.7 and A2 report comparison the cloglog, loglog, and Logit model for reference.

The general pattern we see is that, for values close to the baby bonus values observed in the data, the fertility effects of the policy are pretty much the same regardless of the functional form assumption we impose on the error term u. At larger values, as expected, we start to notice some differences. The cloglog behaves similarly to the Logit, so the results are quite similar to the ones we report as our baseline estimates. The loglog specification, however, displays a smaller density at the peak but a much thicker right tail. As a result, the CDF of the reservation price of fertility grows slower than in our baseline results.

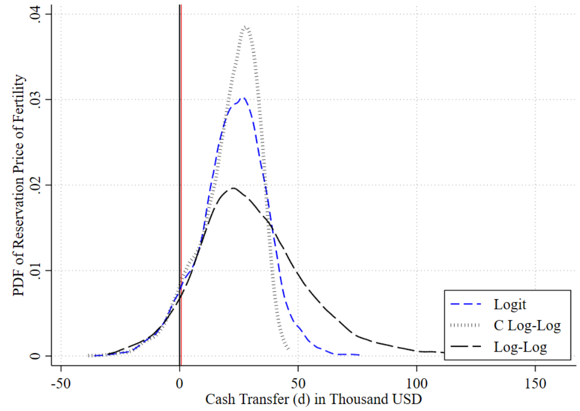
Thus, we can conclude that, although our error distribution assumptions are not innocuous for understanding the policy effects, particularly at very large values of the policy, the general message of our results remains largely robust to the distributional assumptions we make on the error term u. This is even more so on the range of values that are closer to the ones we observe in the data. It is useful to note that the estimates of the CDF of the reservation price of fertility distribution are still quite similar up to 20 thousand dollars, which is over 20 times larger than the mean value of the policy observed in the data. More importantly, when they differ, they differ in a way that makes our results even more extreme. Compared to our baseline specification, the CDF of the reservation price of fertility distribution grows *slower* as we increase the baby bonus level, making attempts to increase fertility by means of increasing the policy benefit level even harder to be successful if the Log-Log specification is correct.

2.B Appendix Figures

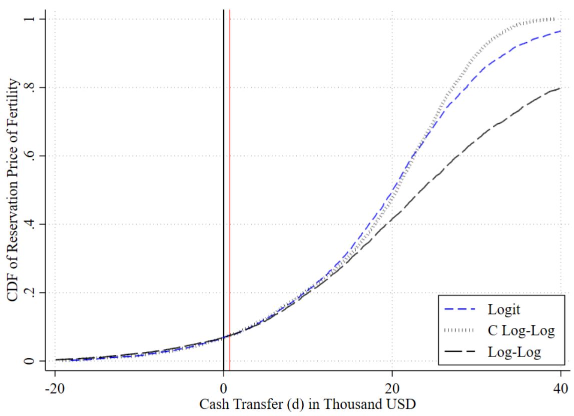
Figure 2.7: Reservation Price of Fertility Distribution Estimates Using C Log-Log and Log-Log



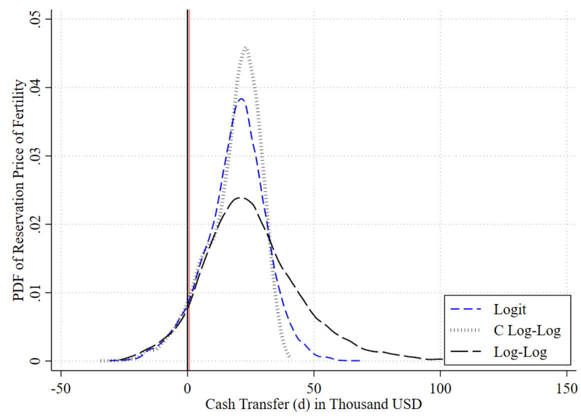
(a) CDF of Column (1)



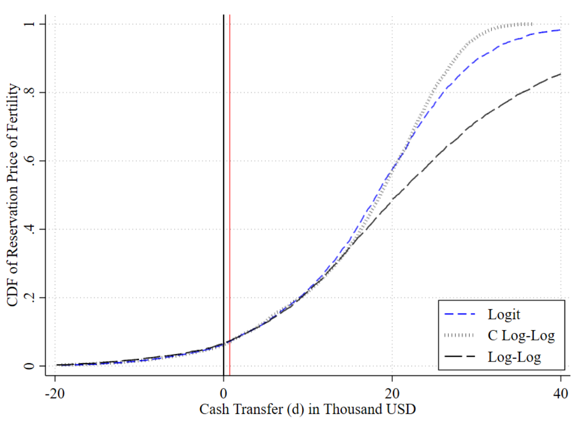
(b) PDF of Column (1)



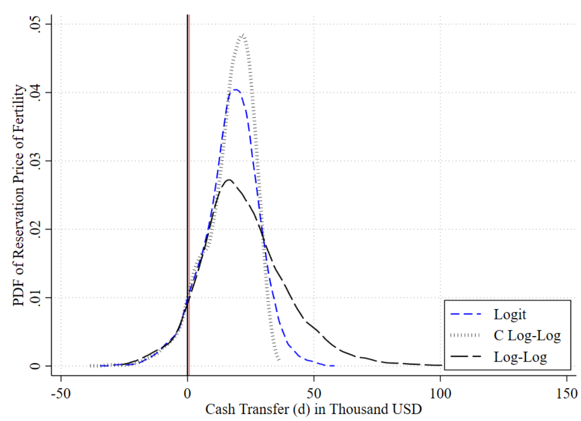
(c) CDF of Column (2)



(d) PDF of Column (2)



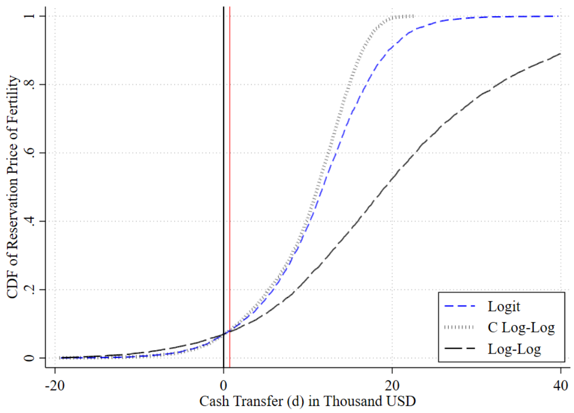
(e) CDF of Column (3)



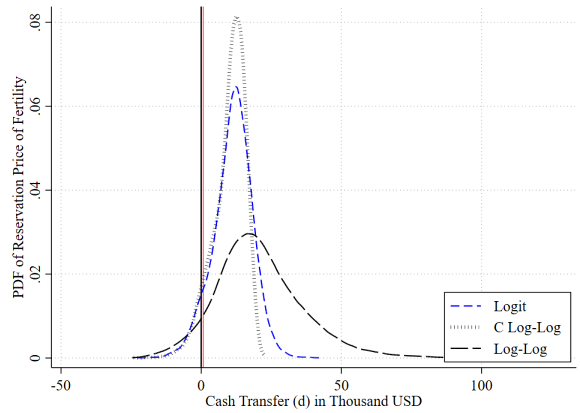
(f) PDF of Column (3)

Notes: Columns refer to the columns in Table 2.9. Left panels show the CDF of reservation price using the estimation results using different error term distributions. Logit regression is added for reference. Right panels show the analytical PDF of reservation price from the estimated CDF. The first and second vertical lines indicate the zero and the current baby bonus at the average level, respectively.

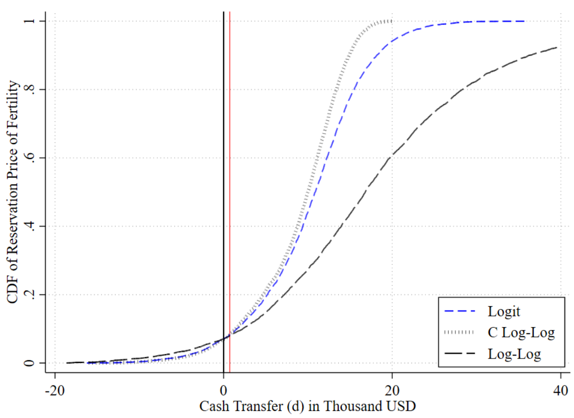
Figure A2: Reservation Price of Fertility Distribution Estimates Using C Log-Log and Log-Log – Alternative Specifications



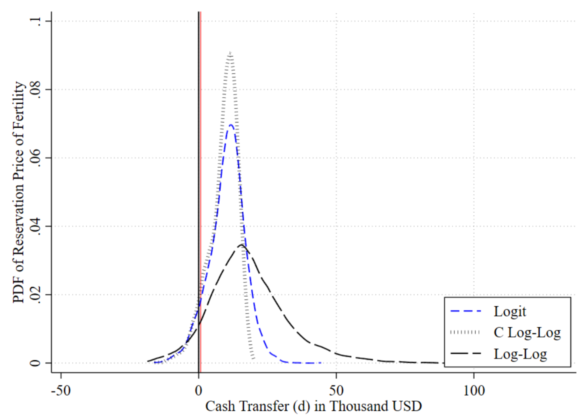
(a) CDF of Column (4)



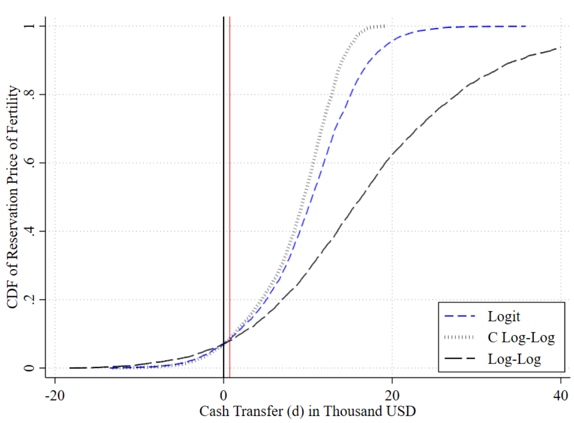
(b) PDF of Column (4)



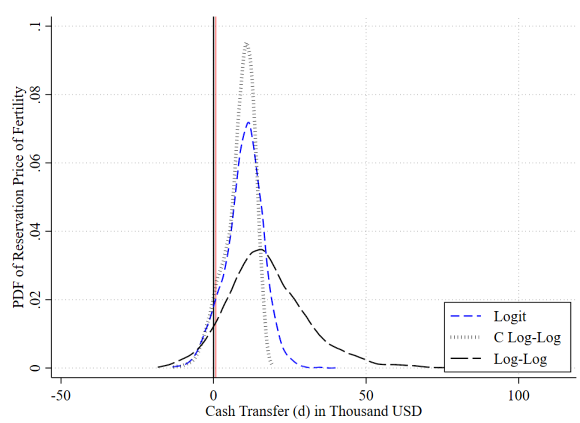
(c) CDF of Column (5)



(d) PDF of Column (5)



(e) CDF of Column (6)



(f) PDF of Column (6)

Notes: Columns refer to the columns in Table 2.9. Left panels show the CDF of reservation price using the estimation results using different error term distributions. Logit regression is added for reference. Right panels show the analytical PDF of reservation price from the estimated CDF. The first and second vertical lines indicate the zero and the current baby bonus at the average level, respectively.

2.C Appendix Tables

Table 2.11: Local Linear Approximation Using Reweighting

	(1)	(2)	(3)	(4)	(5)	(6)
Cash Transfer	0.023***	0.027***	0.030***	0.031***	0.034***	0.036***
	(0.006)	(0.006)	(0.007)	(0.008)	(0.008)	(0.008)
Age	0.006***	0.006***	0.006***	0.006***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unmarried	-0.405***	-0.748***	-0.538***	-0.403***	-0.756***	-0.547***
	(0.013)	(0.068)	(0.111)	(0.013)	(0.069)	(0.113)
# Children$_{t-1}$	-0.098***	-0.095***	-0.100***	-0.059	-0.238	-0.178
	(0.017)	(0.017)	(0.017)	(0.195)	(0.197)	(0.198)
Income		-0.085	-0.139		-0.085	-0.138
		(0.085)	(0.086)		(0.085)	(0.086)
H: Age		-0.011***	-0.011***		-0.011***	-0.011***
		(0.002)	(0.002)		(0.002)	(0.002)
H: Years of Schooling			0.005			0.005
			(0.005)			(0.005)
H: Monthly Salary			0.019***			0.019***
			(0.005)			(0.005)
H: Hours Worked			0.008*			0.008
			(0.005)			(0.005)
# Children$_{t-1}\times$ Cash Transfer				-0.017	-0.017	-0.013
				(0.013)	(0.013)	(0.013)
# Children$_{t-1}\times$ Age				-0.001	0.005	0.003
				(0.007)	(0.007)	(0.007)
Constant	0.294***	0.636***	0.434***	0.291***	0.646***	0.444***
	(0.031)	(0.072)	(0.113)	(0.031)	(0.074)	(0.115)
Infra-marginal Ratio (Births)	0.957	0.950	0.943	0.952	0.945	0.940
Observations	4839	4839	4839	4839	4839	4839
R-squared	0.265	0.269	0.272	0.265	0.269	0.272

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.
Dependent variable: Childbirth outcome in the next two years.
Notes: Table reports estimation results using the reweighting. The first three columns estimate the effect of the cash transfer, while the next three columns allow heterogeneous effects for each birth order. Cash transfer and monthly salary are reported in 1,000 USD, while household income is measured in 1,000,000 USD. The Infra-marginal Ratio (Births) is reported at the bottom of the table.

Table 2.12: Local Linear Approximation Using More Covariates

	(1)	(2)	(3)	(4)	(5)	(6)
Cash Transfer	0.015*	0.019**	0.022***	0.017*	0.021**	0.021**
	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)	(0.010)
Age	0.005***	0.006***	0.005***	0.005***	0.006***	0.005***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unmarried	-0.376***	-0.737***	-0.553***	-0.376***	-0.743***	-0.558***
	(0.013)	(0.070)	(0.114)	(0.013)	(0.071)	(0.115)
# Children$_{t-1}$	-0.070***	-0.065***	-0.073***	-0.019	-0.183	-0.137
	(0.017)	(0.017)	(0.018)	(0.210)	(0.213)	(0.213)
Income		-0.077	-0.131		-0.077	-0.131
		(0.089)	(0.089)		(0.089)	(0.089)
H: Age		-0.011***	-0.012***		-0.012***	-0.012***
		(0.002)	(0.002)		(0.002)	(0.002)
Years of Schooling		-0.001	-0.002		-0.001	-0.002
		(0.002)	(0.002)		(0.002)	(0.002)
H: Years of Schooling			0.004			0.004
			(0.005)			(0.005)
H: Monthly Salary			0.022***			0.022***
			(0.005)			(0.005)
H: Hours Worked			0.006			0.006
			(0.005)			(0.005)
# Children$_{t-1}\times$ Cash Transfer				-0.006	-0.005	0.001
				(0.015)	(0.015)	(0.015)
# Children$_{t-1}\times$ Age				-0.002	0.004	0.002
				(0.007)	(0.007)	(0.007)
Constant	0.291***	0.655***	0.496***	0.290***	0.663***	0.502***
	(0.037)	(0.084)	(0.119)	(0.037)	(0.085)	(0.120)
Year FE	YES	YES	YES	YES	YES	YES
Province FE	YES	YES	YES	YES	YES	YES
Infra-marginal Ratio (Births)	0.972	0.965	0.960	0.971	0.964	0.960
Observations	4839	4839	4839	4839	4839	4839
R-squared	0.267	0.271	0.274	0.267	0.271	0.274

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. SE in parentheses.

Dependent variable: Childbirth outcome in the next two years.

Notes: Table reports estimation results using more covariates: years of schooling, province fixed-effects, and year fixed effects. The first three columns estimate the effect of the cash transfer, while the next three columns allow heterogeneous effects for each birth order. Cash transfer and monthly salary are reported in 1,000 USD, while household income is measured in 1,000,000 USD. The Infra-marginal Ratio (Births) is reported at the bottom of the table.

Chapter 3

Threat or Empty Threat: Perception toward North Korea's Nuclear Provocation

3.1 Introduction

North Korea has been one of the most reclusive regimes worldwide, but what gained its global recognition is continuous military provocations to the world. Since the Korean War (1950-1953), North Korea has continuously used military threats mainly, seeking external aid and internal cohesion (Lee and Yoo 2020). Nevertheless, the increased geopolitical tension has been producing various consequences not just on the Korean peninsular but globally.

The military tension created by North Korea has been aggravated as the country added nuclear weapons to its provocative military package while there have been olive branches between the two Koreas. As illustrated in Table 3.1, the former President of South Korea, Kim, Dae-Jung, and the former Supreme Leader of North Korea, Kim, Jung-Il, held the first and historic inter-Korean summit in 2000. Despite increased hope for reconciliation after the first summit, North Korea conducted the first nuclear weapon test in 2006, and the second and third nuclear weapon tests were conducted in 2009 and 2013, respectively. The military provocation had been intensified after the succession of the supreme leader position by Kim Jong-un. The country claimed that it was successful in conducting three nuclear tests in 2016 and 2017 until it stopped before the summit in 2018.

There is no doubt that unpredictable inter-Korean relations have impacted various aspects of life in South Korea. Among all, what literature often finds is a substantial impact on financial markets in South Korea (Lee and Yoo 2020; Dibooglu and Cevik 2016; Pyo 2021; Huh and Pyun 2018). They agree that negative events such as missile launches and military confrontations led to a sudden drop in stock prices right after the event (Dibooglu and Cevik 2016), but its impacts seem to be weakening or vanishing as the event of analysis, which relates to a negative shock on the market, becomes the recent one (Pyo 2021; Huh

Table 3.1: Inter-Korean Summits and Major Nuclear Conflicts

Date	Description
6/13/2000	**2000 Inter-Korean summit**
10/9/2006	North Korea conducted the first nuclear weapons test (Supreme Leader Kim, Jung-Il).
10/2/2007	**2007 Inter-Korean summit**
5/25/2009	North Korea conducted **the second nuclear test** at the Punggye-ri Nuclear Test Site (Supreme Leader Kim, Jung-Il).
2/12/2013	**The third underground nuclear test** was conducted at Punggye-ri nuclear test site (Supreme Leader Kim, Jung-Il).
1/6/2016	North Korea conducted the fourth nuclear test at the Punggye-ri nuclear test site (Supreme Leader Kim, Jung-Eun).
9/9/2016	The fifth nuclear test was conducted at the Punggye-ri nuclear test site.
9/3/2017	North Korea claims to have conducted a 6th nuclear test.
2018	**April, May, and September 2018 Inter-Korean summits**

source: https://beyondparallel.csis.org

and Pyun 2018).

This paper aims to contribute to the literature by investigating the effects of North Korea's nuclear tests on housing markets in South Korea in addition to the financial markets and by asking two specific questions, shedding light on the perceived risk as an environmental amenity that residents enjoy. In doing so, I focus on two nuclear tests that were conducted in 2009 and 2013.[1] First, it is important to examine whether any of North Korea's nuclear tests have an impact on housing prices because of the increased risk bearing in the Korean Peninsula. If it were to have any impact whatsoever, this effect could differ city by city; the effect can be greatest and be limited to the cities and towns near the Military Demarcation Line, the border between South and North Korea, or the effect could be widespread all over the places in South Korea. Another possibility is that that the effect would be most significant among the cities with military bases or the cities where there had

[1]The first and the last three nuclear weapon tests are excluded in analysis for the following reasons. The second inter-Korean summit was followed about a year after the first nuclear test, which makes the period of analysis confounded. In a similar sense, it is hard to isolate the effects of each nuclear test when it comes to the last three.

never been conquered by North Korea were not affected by any military threat. In this paper, I focus on the effects on the border cities first, as they are the ones who would be front line with a lot of military bases. Thus, there would be the most obvious first target if another war were to occur. Then, other possible scenarios will be examined in the following discussion.

Secondly and perhaps more interestingly, as North Korea has repeatedly conducted nuclear tests, the perception toward this military provocation would not have the same effect for different orders of the missile tests, like the financial markets (Pyo 2021). If residents in affected areas considered it is an empty threat or a political show to gain the predominance of negotiation position, the negative price effect of the repeated military provocation would not have shown the same effect for the later nuclear tests. As opposed to this, the repetition of military conflicts could result in a slumping real estate market if the perception of those who lived in the neighboring area had aggravated and consolidated by the multiple threats.

Findings suggest that the second nuclear test in 2009 may have had an impact on the housing market both in terms of the real estate price and the volume of transactions in the border cities. The individual housing unit analysis shows a 1.8 to 3.4% decline in housing sales price after the second provocation, while it takes about six months to a year until we observe a statistically significant effect. Using the localities as a unit of analysis, I observe that the second provocation also substantially impacts the total number of housing transactions in which a reduction in housing sales is almost as 20% low a year after the nuclear test. This lagged response of the second provocation is likely caused by the lock-in problem (Beron et al. 1997) as real estate transactions require time to search and individuals are locked in the initial deal. Also, the gradual change in housing price can be interpreted in relation to the Bayesian belief; residents near the border are likely to update their subjective risk assessment in a way that the nuclear test would not threaten their life considerably.

The third nuclear test in 2013, however, does not seem to have altered the perceived risk among the residents in the border cities as opposed to the second test. There is no statistical evidence that housing sales price or the total number of transactions were affected after the provocation, implying that nuclear tests had been gradually considered an empty threat among potential buyers. The empty threat hypothesis that can be confirmed by the result is consistent with the literature (Zhu et al. 2016) in which changes in perceived risk tend to have only temporal impacts rather than the persistent effect on the affected area.

The paper organizes as follows. Section 3.2 explores the related literature. In Section 3.3 and 3.4, data and empirical methods used in the paper will be provided. The discussion of the empirical results is in Sections 3.5 and 3.6. Lastly, Section 3.7 concludes and discusses the implications of these results.

3.2 Literature Review

Risks borne in the region, whether it is nature-driven or human-driven, entail economic costs that the homeowners need to pay. This hedonic valuation of safety has been studied as to the spatial impacts caused by events as these could alter the subjective assessment of risks toward the area of residence. Natural disasters such as hurricanes, earthquakes, wildfires, and landslides are one of the evident determinants of housing price as it affects the hedonic evaluation of the residential areas. Beron et al. (1997) use the Loma Prieta earthquake in 1989 to estimate the hedonic price in the San Francisco area, finding about

30% drop after the earthquake. Cheung et al. (2018) document that Oklahoma experienced heterogeneous impacts on housing values for different intensity scales of earthquakes. A drop in residential property values varies by 3 to 9%. Naoi et al. (2009) examines whether the earthquake risks in Japan affect housing prices in the earthquake-susceptible areas due to the alteration of subjective assessment after massive earthquakes. They find a 13% decrease in housing value and a 16% decrease in rent after the earthquakes.

Other types of natural disasters are associated with a decline in property values. Mueller et al. (2009) study the effect of housing price change due to the repeatedly occurring forest fires in California. Due to changes in public perception of that risk factor, the housing price is dropped by 10% and 23% for the first and second wildfires, respectively. Hallstrom and Smith (2005) identify the significant damage in housing values after hurricane Andrew, which resulted in a 19% decline in housing price. Ortega and Tapınar (2018) also find a price penalty in the flood zone in New York City, varying 8 to 22% drop due to the storms.

While most of the literature focuses on the economic consequences of natural disasters that may affect subjective assessments of risk, another strand of recent literature focuses on the information shock that does not alter any residential amenity except the perceived risk. The majority of them exploit the 2011 Fukushima Nuclear Accident, examining whether the increased awareness of nuclear risk affects the housing valuation near the nuclear power plants. For instance, Boes et al. (2015) illustrate the association between the perceived risk caused by the massive coverage of the Fukushima nuclear accident and housing price near the nuclear power plants in Switzerland. In similar works by Tanaka and Zabel (2018) and Zhu et al. (2016), both also shed light on changes in the public perceptions of environmental hazards caused by the Fukushima nuclear accident in the US and China. All of them find a temporary drop in housing valuation, varying from 2.3 to 18% decline, but it recovered almost a year after the accident.

In line with the literature, this paper aims to evaluate the effects of the perceived risks shared by the residents on housing valuation. The credibility of threat can influence the expectation of residents in the area especially susceptible to a hazard as the occurrence of missile-firing became more frequent, thereby leading to the decrease in housing prices in the neighboring area. It is of interest to investigate how much the change in perception toward the risk affects the housing price because this particular event makes every other thing remain unchanged. Other hazard types such as earthquakes, hurricanes, and even rocket launches do inevitably destroy certain areas so that it sometimes might not be feasible to distinguish the price effect due to the perception changes and due to the change in house values. For that matter, we have a great advantage to extract the effect of one event of interest, leaving all other things constant. Therefore, this research would like to examine the effect of missile tests tested in North Korea by taking advantage of those facts.

The reason why this North Korea's nuclear test is special and unique, especially in the hedonic price analysis, in comparison with the existing literature, is that this unexpected external shock can alter the perception of people toward the potential risk of their residential area while leaving every other physical characteristic unchanged. Hence, it is beneficial that the nuclear weapon test helps us evaluate the effect arisen from the sole characteristic change in the area susceptible to a certain risk. Because no missile has hit any part of the South Korean territory, all the physical properties remain the same, and that enables us to detour the typical problem that the hedonic analysis faces.

3.3 Data

The Ministry of Land, Infrastructure, and Transport in South Korea provides publicly available real estate transaction data covering entire transactions of the country. Among all types of real estate transactions, I exclude non-residential type sales such as land acquisition and commercial real estate but include apartment complex, office-house, one-family house (denoted by "*House 1*"), and multiplex house (denoted by "*House 2*"). Unfortunately, the data does not fully provide the exact address of one-family houses and multiplex houses; instead, it contains up until the first digit of the street number. Thus, I geocode the addresses based on the first digit and the street names for those housing types and use exact addresses otherwise.

The outcome of primary interest is the sale price of a residential unit that happened in 2009 and 2014. Specifically, the pre-treatment prices are set by all housing sales within three months before each provocation. I use three different periods for the post-treatment price to account for the time delay in reflecting individuals' risk evaluation: three-month transactions within 90, 180, and 360 days after the provocation. When it comes to the locality-level analysis, all variables associated with each locality are measured in a quarter.

I use a different set of localities for the control group.[2] First, the control group A consists of 13 localities that are located on the other side of two border provinces, Gyeonggi-do and Gangwon-do, when using localities close to the borderline. Second, another definition of the treatment group is housing sales within 30km from the border. The control group B, which is the corresponding control group to this treatment group, is set as all real estate transactions 100km outside but located in the same provinces. Lastly, using the first treatment group, I exploit another treatment group, the control group C, for

[2]The detail of the control groups is in the appendix.

a robustness check. Those localities are selected based on the synthetic control method; whenever the weight of localities is greater than 0.2, I include them in the control group.[3]

Table 3.2 reports the summary statistics of the data used for the housing unit analysis. The average price of the residential units is higher in the border cities. In the estimation, four residential types are included: apartment complex, office-residential complex, house 1, and house 2. Most of the transactions that about 60 to 82% transactions are apartment complex sales in both events. The second-largest portion, about 15 to 20%, is house 2 sales, and the office-residential complex and house 1 occupy 5 to 10% of the sales. Lastly, more than 70% of housing transactions are house sales in Gyeonggi-do, which is the most populated province in South Korea.

When it comes to the locality-level analysis, I additionally include locality-specific characteristics obtained from the Korean Statistical Information Service, the national database of South Korea. In Table 3.3, the descriptive statistics of the data show the information obtained by averaging the sales data at the locality level. The volume of the transactions is 643 and 730, while the average prices are 152 and 171 in 2009 and 2013, respectively. Apartment complexes are the most frequently transacted housing type, about 63%, and followed by one house 1, house 2, and office-residential complex.

Other locality-specific control variables are the total number of births, deaths, marriage, male population, population, and net migration of localities. The average total number of births is 155 and 149, and that of deaths are 77 and 84 in 2009 and 2013. The average number of marriages is 105 and 100 in the same period. Also, the male and total population are increasing over the period, while the net migration decreases to 151 from 203.

[3] The detailed description of the control sets is illustrated in the appendix.

3.4 Empirical Model

The main goal of this paper is to measure the devaluation of the properties in localities in which may have affected by North Korea's nuclear weapon tests. Bayesian economics states that individuals update their beliefs, and this updated belief would be reflected in the market price of housing units (Tanaka and Zabel 2018). To that end, it is reasonable to postulate that the nuclear weapon tests may influence individuals' subjective risk toward property, and the effects can be heterogeneous across the country based on the condition of the residential unit locations. For instance, localities near the borderline may behave more actively and evidently because they are likely the first target and the front line of the military conflict between South Korea and North Korea. On the other hand, localities with U.S. military bases could fear more given the importance of U.S. military power on the peninsula since the Korean war.

Yet, there are several challenges when identifying the effects of interest; first, given the country's size, just about 1% of the U.S. area[4], South Korea as a whole could be affected by the military provocations. Additionally, even if all parts of the territory were to be affected by the test, the extent to which subjective risk assessments of local environmental risks vary may differ across the country. To investigate this possibility, the definition of treatment variable is changed to the distance to the borders.

Second, as pointed out by Beron et al. (1997), real estate transactions require time to search a property and to close the deal, which causes the *lock-in* problem. Because of this, the real estate market would require substantial time to fully update people's subjective risk evaluation so that the market price would not change immediately after the provocations.

[4]The area of South Korea is 100,363km^2, whereas the United States total area is 9,833,520km^2. To put the numbers in perspective, the country is as almost big as the state of Kentucky.

In that regard, I use three different sets of outcomes: 90, 180, and 360days real estate price after the provocation.[5]

Third, the choice of the control group is the last concern. It is essential to have comparable housing units to the treatment units to gauge the effects of interest. To make this valid, I focus on residential unit sales in the same provinces in the most northern part of South Korea. This allows the characteristics of housing units in the data as much similar as the treated units, as they share the same historical and administrative environments. In addition, I exploit another control group that is chosen by the synthetic control method, which forces the pre-trend of housing sales similar to the treatment group. This robustness check will allow us to examine whether the results are robust to the change in the control group in use.

3.4.1 Difference-in-Differences

Estimation of the effects lends to a difference-in-differences estimator. The empirical model for log price of residential transactions can be denoted as:

$$\ln \text{Price}_{ilt} = \tau_1 \text{Post}_t \cdot \text{Treat}_i + \alpha \text{Post}_t + \beta \text{Treat}_i + \mathbf{x}_{it}\theta + r_l + \varepsilon_{ilt} \tag{3.1}$$

where r_l denotes neighborhood fixed-effects that observe all time-invariant characteristics associated with the housing sale price. Given that each province has its distinctive real estate market properties, this fixed effect would absorb the heterogeneity shown across the provinces used in the estimation. $\mathbf{x}_{it}$ a set of property covariates that may affect the

[5] According to Ko et al. (2019), almost 95% of real estate properties are sold in a year after posting. Thus, by having 360 days after the provocation, almost all property transactions with the updated subjective risks would be covered in the analysis.

housing price, such as house type, year built, square footage, and floor level. To account for the heterogeneity across the localities, all standard errors are clustered at the locality-level.

The variable of primary interest is the interaction term between post-period indicator variable, Post_t, and treated area indicator variable, Treat_i, which shows the average treatment effects on the treated (ATT). Also, Treat_i measures the location-specific effects whether the housing units are located near the border or not.[6] Post_t shows the post-nuclear test effects. As noted earlier, the treatment period is set differently, focusing on three-time periods: 90, 180, and 360 days after the provocation.

3.4.2 Generalized Difference-in-Differences

The expansion of this discussion can be made by including all regions of South Korea in the analysis. South Korea is geographically not a big country in which the country is just as big as Kentucky in the U.S. This implies that the entire region of South Korea may be affected by the provocation. To assess this hypothesis, I redefine the treatment variable as the distance to the borderline and explore if it is one of the determinants of housing price. The underlying assumption is that residents living closer to the border may have experienced a more significant change in their subjective risk assessment after the provocation. This can be expressed as:

$$\ln \text{Price}_{ilt} = \tau_1 \text{Post}_t \cdot \text{Distance}_i + \alpha \text{Post}_t + \beta \text{Distance}_i + \mathbf{x}_{ilt}\theta + r_l + \varepsilon_{ilt} \tag{3.2}$$

where all things unchanged but the treatment variable. In our new specification, Treat_i is the distance to the border and Post_t shows the post-nuclear test effects. Additionally, r_l

[6] Since the treatment group and control group are defined differently in some cases, which does not coincide with the locality and the treatment status, I use the subscript of i instead of l.

denotes province fixed-effects, and $\mathbf{x}_{it}$ a set of covariates related to housing price.

I conduct a locality-level analysis using the generalized difference-in-differences for the following two reasons. First, while the difference-in-difference estimation method is the workhorse causal inference in many settings, the method can focus on only two types of periods and two types of units. This may not be the most informative way to understand the treatment effects when the post-treatment periods are more than one period. Exploiting the advantage of the data in hand, which covers the entire transactions between 2006-2016, I expand the time period of analysis and examine the price trajectories of housing units after the provocation. Second, changing the unit of analysis to the locality-level highlights another aspect of real estate markets: the total number of housing sales transactions. This is part of essential information, revealing how the real estate market has been affected by the nuclear weapon tests. While the decrease in price may be hard to emerge in the short run, the volume of transactions can instantaneously respond to an external stimulus such as the nuclear tests. Therefore, adding this outcome variable in analysis allow us to examine the real estate market more thoroughly.

The model for this locality-level analysis can be written as follows:

$$\ln y_{lt} = \mathbf{z}_{lt}\tau_2 + \overline{\mathbf{w}}_{lt}\theta + \lambda_t + \gamma_l + \bar{\epsilon}_{lt} \tag{3.3}$$

where λ_t time effects, γ_l locality effects, and $\bar{\epsilon}_{lt}$ locality-specific errors. The outcome variable, y_{lt} either $\overline{\text{Price}}$ or Transactions, is mean price or total number of transactions of locality l. Also, Locality and time period covariates, $\mathbf{z}_{lt}$, and locality-specific covariates, $\mathbf{w}_{lt}$ are included.

3.5 Estimation Results

3.5.1 Baseline Estimation Results

First, I examine the effects of each nuclear weapon test on housing prices using all transaction records. I use cities located in the other parts of the two provinces, Gyeonggi-do and Gangwon-do, as a control group (control group A), which is illustrated in Figure 3.1. The slashed area denotes the treated localities, and grey colored area shows the control localities. As they are both located in the same province, the heterogeneity that may arise due to administrative and environmental differences can be minimized.[7] To account for the remaining heterogeneity across the localities in the same province, I report the standard errors clustered at the locality level. The first, second, and third columns of each set show the result using 90, 180, and 360 days after the provocations, respectively.

In Table 3.4, estimation results of housing market change due to the nuclear tests in 2009 are presented. In the first three columns, the control group A is used while the next three use the alternative control group, the control group B, using sales transactions within 30km and outside 100km in Gyeonggi-do and Gangwon-do.[8] The results indicate that when using the control group A, the sale prices in the border cities after the provocation increase after 90 days, but the increased effect disappears afterward. About a year later the provocation, the sales price decreases by 3.4%. When it comes to the next three columns using the distance to the border, the overall results show similar implications; first, it takes time to fully reflect individuals' adjusted subjective assessment in the housing market. The

[7] The reason to exclude the localities located in the middle of those two sets includes Seoul, the capital of South Korea, bringing complexity into the analysis due to its specificity.

[8] In the literature, it is more common to use the distance measure to differentiate the treatment group and control group. Here, we have this as an alternative one for the coherence of the unit of analysis as the locality will be the unit of analysis in the next discussion.

housing price increases by 1.5% initially then drop by 1.8%.

Next, Table 3.5 displays changes in real estate sales price caused by North Korea's nuclear tests in 2013. Similar to the previous table, the first, second, and third columns of each control unit show price changes in the real estate market the 90, 180, and 360 days after the test. Regardless of the control groups in use and times analyzed, the nuclear tests by North Korea do not seem to affect the real estate market meaningfully. It is quite different from what we observe in the previous result: the gradual but mild residential unit price decrease in the real estate market.

Collectively, it suggests that individuals' subjective risk assessment toward the local environmental risks they face would overreact to the nuclear test first and fade away afterward. Residents in the border cities would have considered the nuclear weapon tests as an empty threat after the third test in 2013. As the nuclear tests do not affect residents' lives at all except the increased military conflict, people may not value the newly updated information as they regard this as a political show rather than an actual threat.

3.5.2 Effects on South Korea

One might raise the question that South Korea as a whole may be affected given the country's small size. This question can be overruled because the country's housing sales index shows an increasing trend after the provocations as in Figure 3.2. The sales indexes do not move downward after the two nuclear provocations, and those even show a substantial increase in the housing sales prices. This implies that the housing market as a whole is unlikely to be affected by the military conflict. Nevertheless, it is worth examining whether living close to North Korea hamper the environmental amenity that the residents enjoy. To that end, I exploit another treatment variable, which is the distance to the border, and assess the impacts on the real estate market in South Korea after the military provocation.

Table 3.6 shows that housing prices are unlikely affected by the nuclear weapon tests. The first three columns show the estimation results focusing on the 2009 nuclear test, while the next three illustrate the results of the 2013 nuclear test. It shows a large decline in housing prices 90 days after the nuclear test in 2009. As previously noted, it is perhaps caused by the time lag to reflect the updated information, thus, not relevant to the nuclear tests. After allowing substantial time lag to reflect the updated information, distance to the border is not associated with the housing price. Thus, the estimation results also support that the country as a whole does not experience a large change in subjective risk assessment due to the nuclear test.

3.5.3 Locality-Level Analysis

So far, each housing sales transaction is treated as an independent unit, but now we consider each locality as a unit of analysis. For this, the housing transactions are averaged at the locality level on a quarterly basis. One quarter before the event and five quarters after the event are included to track the housing price trajectories and the volume of real estate transactions.

Figures 3.3 and 3.4 illustrate the estimation result using this new data. The upper panel of Figure 3.3 shows estimation results focusing on the housing price change in 2009, while the bottom shows the result of the 2013 provocation. What it shows is a temporal decrease in housing prices about a year after the second provocation, although this is statistically significant only at the 10% level. When choosing a different control group based on the choice set obtained from the synthetic control method, there is no significant change in housing price. Similar to what we observe in the previous sections, the housing market in terms of its price is not affected when it comes to the third nuclear test in 2013, irrespective of the control group in use. This substantiates the previous discussion that the housing market would not react much even after the military provocations were intensified.

As opposed to what we observe for the housing price change, it is the volume of housing transactions that may be affected by the provocation in 2009. The total number of transactions decreases by 20 to 40% about a year quarter later, which is statistically significant at the 95% level. Collectively, it suggests that the only 2009 nuclear test had a substantial impact on real estate markets in South Korea, not only in terms of sales prices but also in terms of willingness to transact a house, but this is not necessarily true for the nuclear test in 2013.

3.6 Additional Exercise

3.6.1 Estimation Results using Alternative Control

To begin, it is essential to check if the common trend assumption, which is the central assumption that validates the difference-in-differences, holds. Figure 3.5 shows the housing price trends of treated localities (in red) and control localities (in blue) for each provocation in 2009 and 2013, respectively. As shown in the figures, the housing sales in the treated and control areas do not move together. Therefore, I use an alternative control group C, localities chosen by the synthetic control method. As illustrated in Figure 3.6, those localities consist of more metropolitan cities than the previous control groups. Changing the control group ensures the common trend between the treatment and control groups by its construction strategy as in Figure 3.7.

In Table 3.7, I examine whether the previous finding still holds even with the change of cities used for comparison to the treatment group. The first three columns show the estimation results focusing on the 2009 nuclear test, while the next three illustrate the results of the 2013 nuclear test. When choosing the alternative control group, we observe a continuous decrease in housing sales price irrespective of the time used for the control group. For the second nuclear test in 2009, we do observe a more apparent decrease in housing sales price, whereas that does not necessarily be true for the third test in 2013. The second nuclear test leads to a decline in housing price, varying 2.1 to 7.5%; yet, the third one in 2013 shows a 1.6% decrease about a year after the provocation.

3.6.2 Effects on U.S. military bases

It is worth exploring the last possibility that the cities with U.S. military bases may have experienced higher environmental risks. Contrary to what its name suggests, the Korean war was ended in a cease-fire in 1953 between the United States and North Korea, not between South Korea and North Korea.[9], which propose a possibility that the potential military target that North Korea may be interested in is indeed the U.S. military bases in South Korea. Thus, I choose two cities with the U.S. military base, which is not a metropolitan city or overlaps with any metropolitan cities within 100km radius. Two latticed areas in Figure 3.8 denote the localities used in the analysis, while dotted areas show other localities with the base but not included in the analysis.

I report the estimation results in Tables 3.8 and 3.9. The first three shows the estimation results of the 2009 nuclear test 90, 180, and 360 days after the provocation. The next three shows corresponding results of the 2013 nuclear provocation. When it comes to

[9]China also participated in the Korean Armistice Agreement.

the city of Gunsan, no substantial impact is found because of the provocation. Although there is a significant price decline in Wonju after the 2009 nuclear test, varying from 6.8 to 11%, its impact was not evident in 2013. This substantiates the previous finding in which the provocation had a minimal impact only in 2009.

3.7 Conclusion

The purpose of this study is to examine the impact of North Korea's continuous military threats on the perception of the residents in South Korea especially focusing on how it changes the valuation of the houses of the residents. When it comes to the discussion of the hedonic price analysis, the estimation of the sole effect coming from one's perception changes, how they access the housing value located in the area susceptible to risk, would be other contribution because almost no papers have succeeded in disentangling the effect from the hedonic price change caused by physical condition changes.

There is weak evidence that North Korea's military provocation may have had a substantial impact on the housing market in South Korea, but its impact does not last. This is consistent with the literature: the temporal decline in real estate price due to the change in perceived risk (Zhu et al. 2016; Tanaka and Zabel 2018). The second nuclear test leads to a decline in housing sales price in the border cities by 1.8 to 3.4%, but the third provocation does not show any substantial impact. Also, we do observe a substantial decrease in housing sales, showing a 20 to 40% drop in sales transactions.

One problem that has not been addressed is the bidding and sorting. The bidding and sorting is an important issue in addressing the hedonic valuation, as pointed out by Yinger (2015). However, this problem has not been addressed because of the nature of the amenity of interest and the limitation of the data. The changes in perceived risk are measured as the binary variables, making thereby the bidding and sorting caused by the perceived risk not feasible. In future research, the distance to the border will be used as a proxy variable for the perceived risk and investigate whether this amenity factor affected residents' sorting into different areas with less risk susceptible areas.

3.A Appendix

3.A.1 Synthetic Control Method

In this subsection, the construction of an alternative control group, the control group C, will be explained. The control group C is chosen based on the weights ($\mathbf{w}$) obtained by the Synthetic Control Methods (SCM) proposed by Abadie et al. (2015). The SCM constructs an alternative control group using the weights from the set of potential comparisons from the untreated units after the intervention. To begin, let us define each locality as $l = 1, \ldots, (L+1)$, while setting the treated locality to be $l = 1$. Then, the outcome of each locality is denoted as $Y_l = (Y_{l1}, , Y_{lT})$, and the observed outcome can be written as two components:

$$Y_{lt} = Y_{lt}^N + \omega_{lt} D_{lt}$$

where Y_{lt}^N is the outcome variable that would have observed in the absence of the intervention. ω_{lt} is the treatment effect, and D_{lt} is an indicator variable, showing the treatment status after T_0. The treatment-free outcome, Y_{lt}^N, is the observed outcome before the intervention, but this is not observed for the treated locality after the intervention.

The goal of the SCM method is to obtain a set of weights $\mathbf{w} = (w_2, \ldots, w_{L+1})'$ such taht $w_l \geq 0$ and $\sum_2^{L+1} w_m = 1$, which denotes the contribution to the synthetic control unit. This can be written as:

$$\hat{Y}_{1t}^N = \sum_2^{L+1} w_l Y_{lt}$$

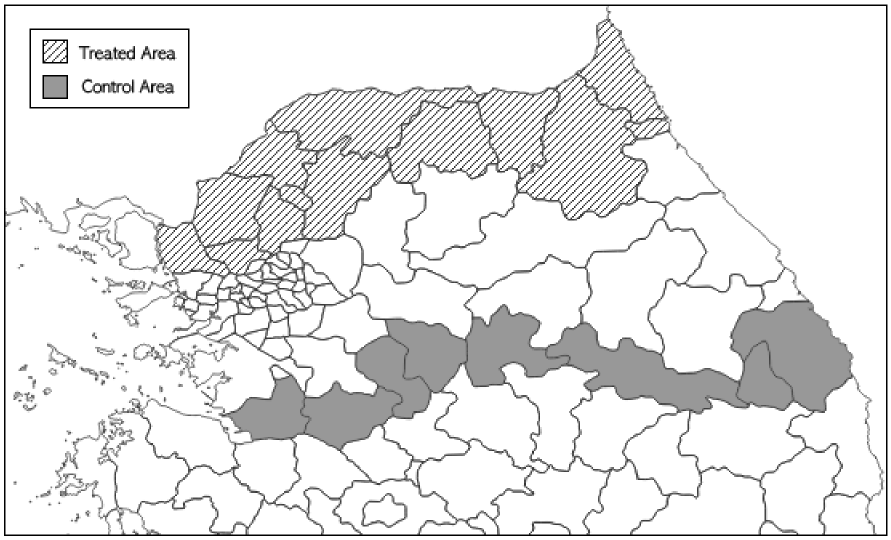
This weight can be obtained by minimizing the distance in the observed and unobserved predictors:

$$\sqrt{(X_1 - X_0\mathbf{w})'V(X_1 - X_0\mathbf{w})}$$

where X_1 is a $k \times 1$ vector that contain the values of the predictors for localities $l = 1, , L+1$. Meanwhile, X_0 is a $k \times L$ matrix containing the values of the predictors for the J untreated units. V is a $k \times k$ positive definite diagonal matrix that assigns the weights of the covariates.

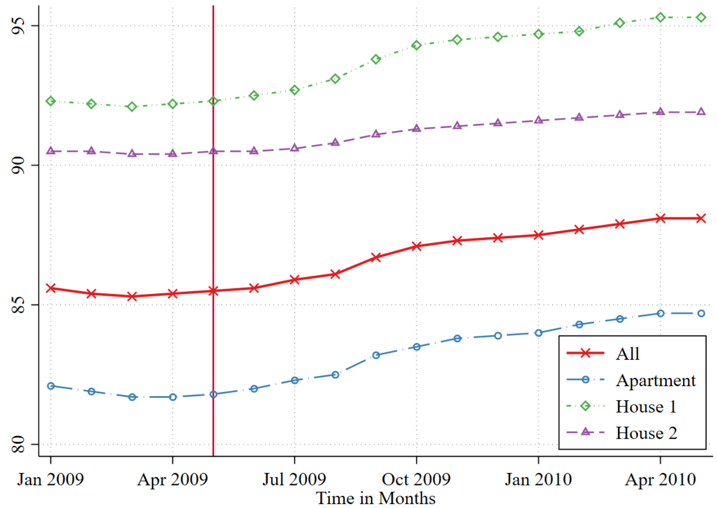
3.B Figures

Figure 3.1: Border Cities and Control Group A

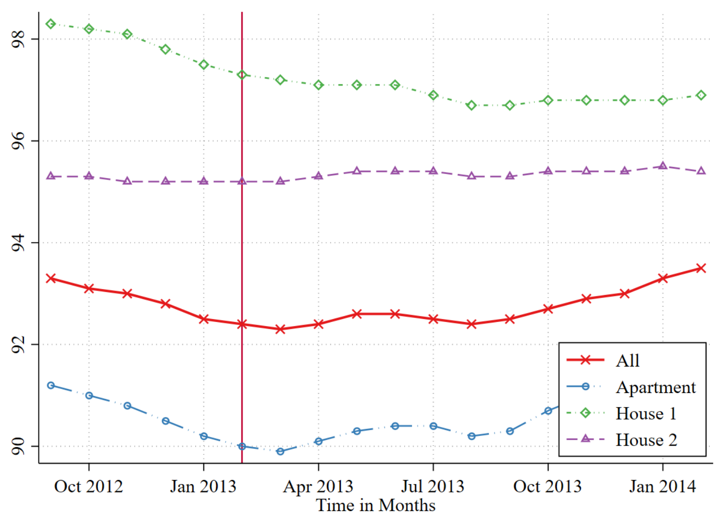


Notes: Graph shows the treated and control areas.

Figure 3.2: Housing Sales Price Index



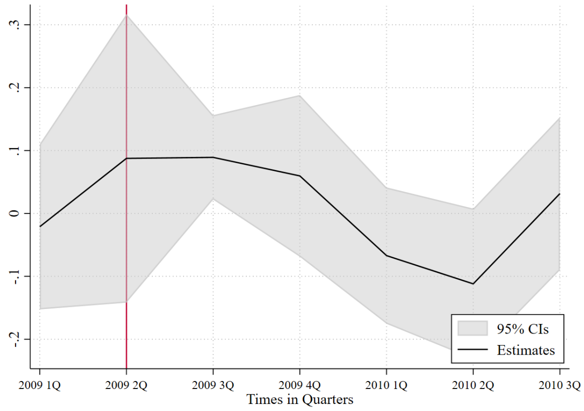
(a) Housing Sales Price Index 2009



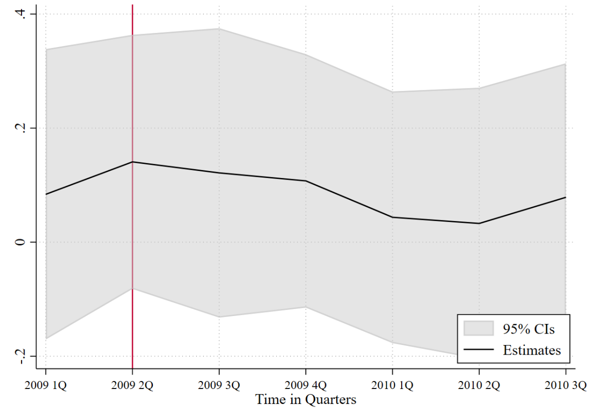
(b) Housing Sales Price Index 2013

Notes: Graphs show housing sales price index of South Korea in 2009 (upper) and 2013 (bottom). The thick red line includes all housing types, and the blue line shows the index of apartment complexes. The green and purple lines show that of the house 1 and house 2, respectively.

Figure 3.3: Effects on Housing Price using Localities

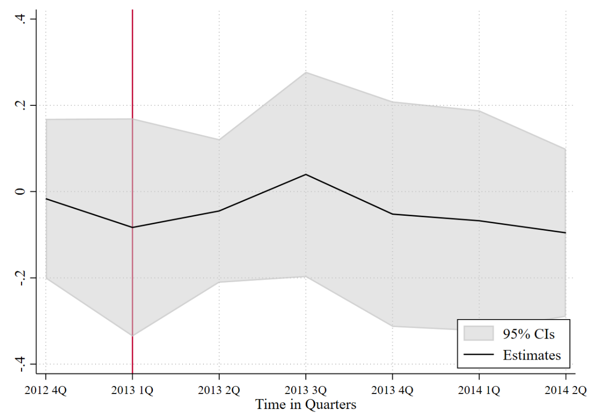


(a) Housing Price 2009 - Control A



(b) Housing Price 2009 - Control C

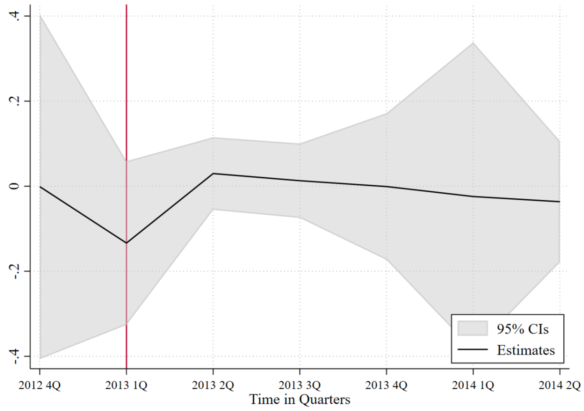
(c) Housing Price 2013 - Control A



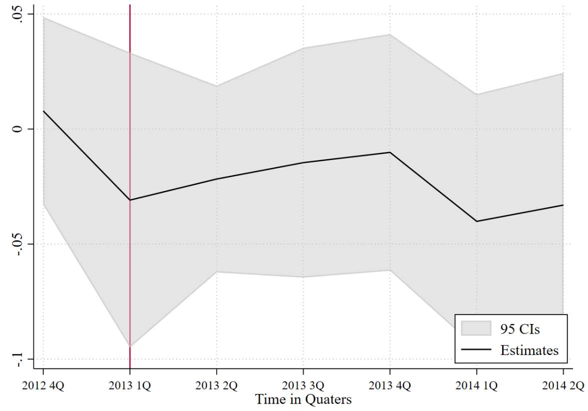
(d) Housing Price 2013 - Control C

Notes: Graphs display the estimation results of housing sales price at the locality-level using the generalized difference-in-differences estimator. Two upper panels show the results of the nuclear provocation in 2009, while the bottom two illustrate that of the nuclear provocation in 2013.

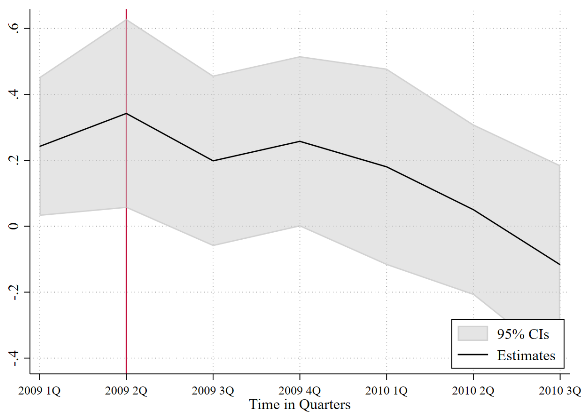
Figure 3.4: Effects on Transaction Volume using Localities



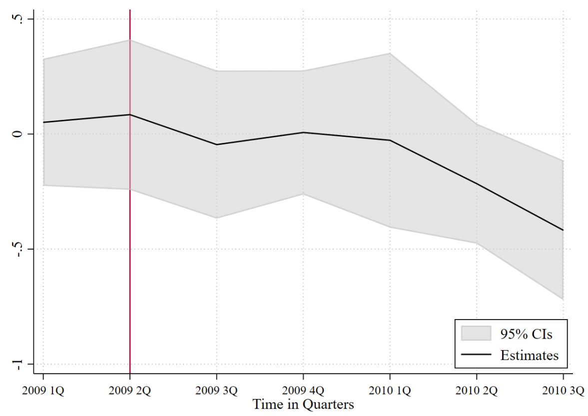
(a) Transaction Volume 2009 - Control A



(b) Transaction Volume 2009 - Control C



(c) Transaction Volume 2013 - Control A

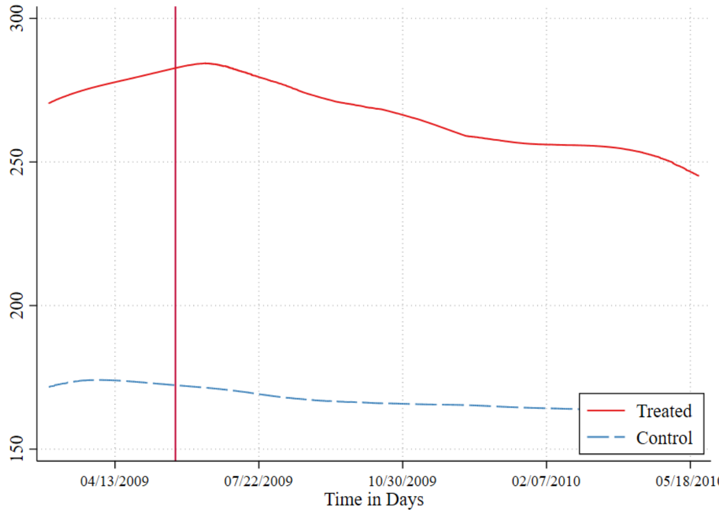


(d) Transaction Volume 2013 - Control C

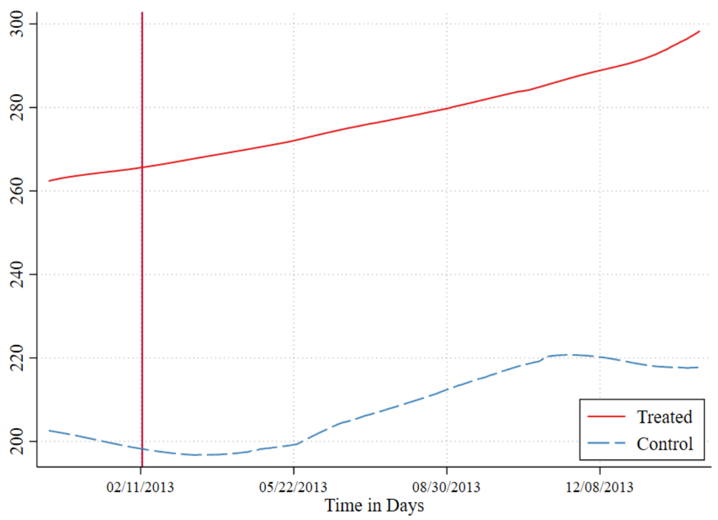
Notes:

Notes: Graphs display the estimation results of housing sales transactions at the locality-level using the generalized difference-in-differences estimator. Two upper panels show the results of the nuclear provocation in 2009, while the bottom two illustrate that of the nuclear provocation in 2013.

Figure 3.5: Common Trend Assumption



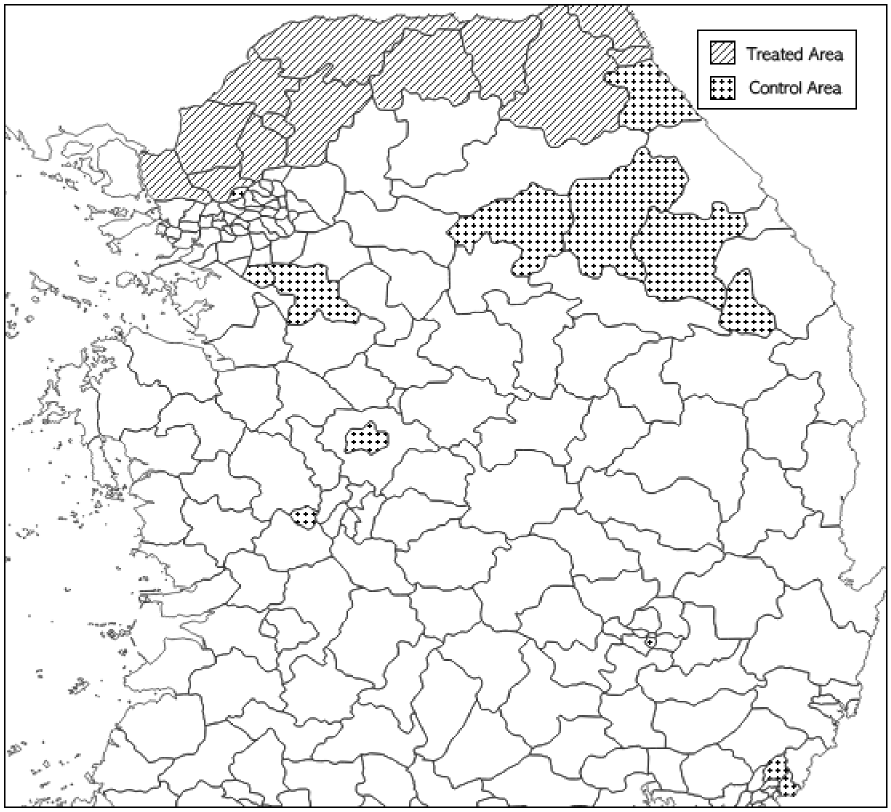
(a) Common Trend 2009



(b) Common Trend 2013

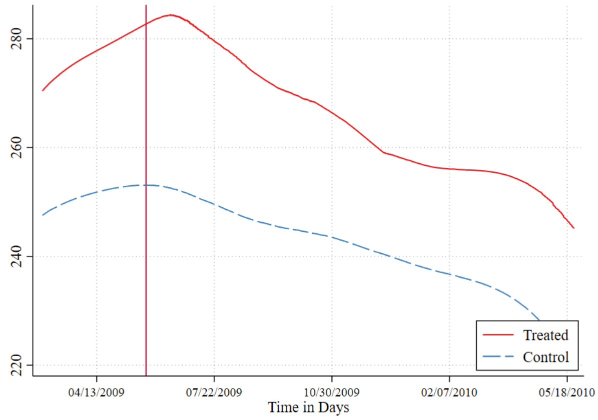
Notes: Graphs show the trend of housing sales price in the treated and control areas in 2009 (top) and 2013 (bottom). The red line is the housing price of the treated and the blue dotted line is that of the control.

Figure 3.6: Border Cities and Control Group C

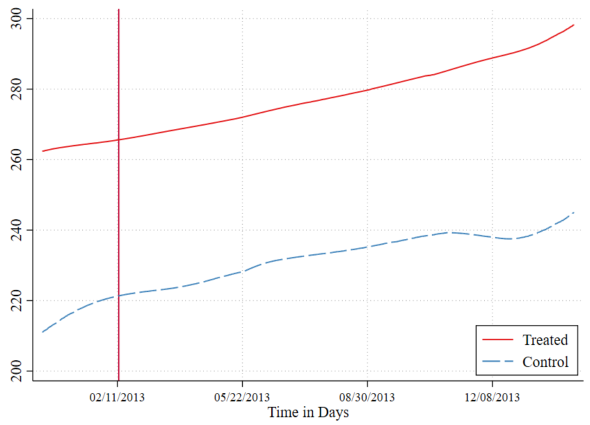


Notes: Graph shows the treated and control areas.

Figure 3.7: Common Trend Assumption - Alternative Control Group



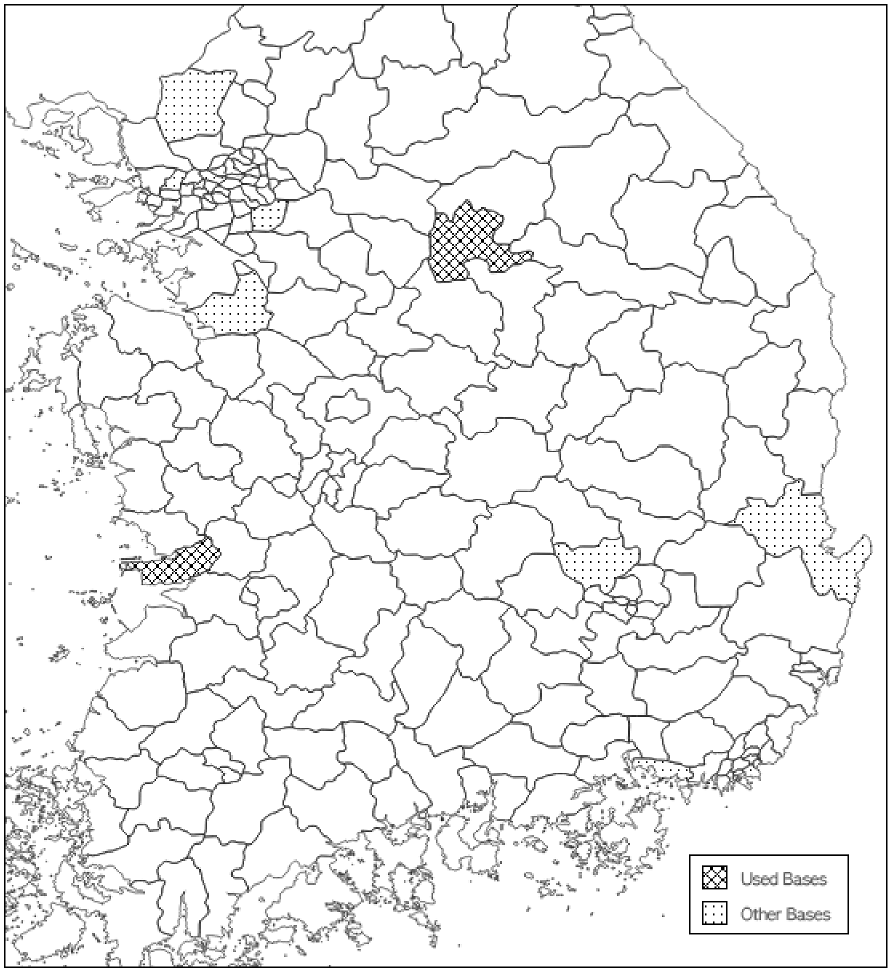
(a) Common Trend 2009



(b) Common Trend 2013

Notes: Graphs show the trend of housing sales price in the treated and control areas in 2009 (top) and 2013 (bottom). The red line is the housing price of the treated and the blue dotted line is that of the control.

Figure 3.8: Cities with US Military Bases



Notes: Graph shows the the US military bases in South Korea. Only X-checked areas are used in the paper.

List of Treatment Groups

Treatment	List of Localities	Control
Group A	**(Gyeonggi-do)** Goyang-si, Gimpo-si, Dongducheon-si, Bucheon-si, Yangju-si, Paju-si, Pocheon-si, **(Gangwon-do)** Goseong-gun, Sokcho-si, Yanggu-gun, Yeoncheon-gun, Inje-gun, Cheorwon-gun, Hwacheon-gun	Group A, C
Group B	Transactred houses ranged witihn 30 km in Gyeonggi-do and Gangwon-do	Group B

List of Control Groups

Control	List of Localities	Treatment
Group A	**(Gyeonggi-do)** Hwaseong-si, Osan-si, Pyeongtaek-si, Anseong-si, Icheon-si, Yeoju-gun, **(Gangwon-do)** Yangpyeong-gun, Wonju-si, Yeongwol-gun, Taebaek-si, Samcheok-si	Group A
Group B	Transactred houses ranged between 100 km and farther from the border in Gyeonggi-do and Gangwon-do	Group B
Group C 2009	**(Gangwon-do)** Hoengseong-gun, Yangyang-gun, Taebaek-si, Pyeongchang-gun, Jeongseon-gun, **(Gyeonggi-do)** Suwon-si, Yongin-si, **(Seoul)** Eunpyeong-gu, **(Busan)** Haeundae-gu, **(Daegu)** Suseong-gu, **(Chungcheongbuk-do)** Cheongju-si, Danyang-gun, **(Chungcheongnam-do)** Gyeryong-si	Group A
Group C 2013	**(Gangwon-do)** Gangwon-gun, Samcheok-si, **(Gyeonggi-do)** Gwangju-si, Gunpo-si, **(Seoul)** Jungnang-gu, **(Busan)** Yeongdo-gu, **(Chungcheongnam-do)** Buyeo-gun, **(jeollanam-do)** Gurye-gun, **(Gyeongsangbuk-do)** Gyeongju-si, Yeongju-si, Ulju-gun, **(Gyeongsangnam-do)** Namhae-gun, Tongyeong-si, Geochang-gun, **(Jeju-do)** Jeju-si	Group A

3.C Tables

Table 3.2: Summary Statistics 1

	2009 Nuclear Test											
	90 Days				180 Days				360 Days			
	Treat		Control		Treat		Control		Treat		Control	
	Before	Post	Before	Post	Before	Post	Before	Post	Before	Post	Before	Post
Price	279.6	290.0	183.9	175.7	279.6	242.8	183.9	177.9	279.6	240.8	183.9	177.7
	(109.9)	(112.1)	(79.93)	(78.24)	(109.9)	(126.4)	(79.93)	(77.97)	(109.9)	(109.7)	(79.93)	(79.50)
Apartment	0.667	0.632	0.816	0.827	0.667	0.652	0.816	0.783	0.667	0.603	0.816	0.777
Office-House	0.0949	0.0924	0.00525	0.00407	0.0949	0.0898	0.00525	0.00673	0.0949	0.124	0.00525	0.00704
House 1	0.0534	0.0535	0.0902	0.0842	0.0534	0.0553	0.0902	0.0975	0.0534	0.0570	0.0902	0.103
House 2	0.185	0.222	0.0888	0.0843	0.185	0.202	0.0888	0.112	0.185	0.216	0.0888	0.113
Gangwon-do	0.0624	0.0511	0.182	0.203	0.0624	0.0680	0.182	0.232	0.0624	0.0809	0.182	0.252
Gyeonggi-do	0.938	0.949	0.818	0.797	0.938	0.932	0.818	0.768	0.938	0.919	0.818	0.748
Observations	12755	9989	8378	7128	12755	11512	8378	7131	12755	8751	8378	6962
	2013 Nuclear Test											
	90 Days				180 Days				360 Days			
	Treat		Control		Treat		Control		Treat		Control	
	Before	Post	Before	Post	Before	Post	Before	Post	Before	Post	Before	Post
Price	262.9	269.5	214.3	209.9	262.9	279.1	214.3	222.7	262.9	287.9	214.3	227.9
	(95.74)	(98.77)	(86.60)	(87.75)	(95.74)	(103.7)	(86.60)	(86.73)	(95.74)	(105.4)	(86.60)	(94.40)
Apartment	0.720	0.719	0.782	0.746	0.720	0.763	0.782	0.758	0.720	0.752	0.782	0.787
Office-House	0.0551	0.0529	0.0138	0.0101	0.0551	0.0340	0.0138	0.0121	0.0551	0.0521	0.0138	0.00945
House 1	0.0524	0.0551	0.0819	0.108	0.0524	0.0389	0.0819	0.0899	0.0524	0.0446	0.0819	0.0812
House 2	0.173	0.173	0.122	0.136	0.173	0.164	0.122	0.140	0.173	0.151	0.122	0.123
Gangwon-do	0.0593	0.0487	0.252	0.251	0.0593	0.0488	0.252	0.203	0.0593	0.0456	0.252	0.222
Gyeonggi-do	0.941	0.951	0.748	0.749	0.941	0.951	0.748	0.797	0.941	0.954	0.748	0.778
Observations	8107	7898	6675	5564	8107	13422	6675	8083	8107	15698	6675	10788

Table shows the summary statistics of housing transactions related to the 2009 and 2013 nuclear provocations. The mean and standard error of the housing price is reported in the table, whereas the proportion of all other variables is presented.

Table 3.3: Summary Statistics 2

	2009 Nuclear Test				
Variable	Obs	Mean	Std. Dev.	Min	Max
Transaction	200	643.15	979.35	17	5911
Price	200	152.835	72.654	53.384	337.473
% Apartment	200	0.631	0.199	0.056	0.959
% Office House	200	0.019	0.049	0	0.258
% House 1	200	0.176	0.137	0.013	0.694
% House 2	200	0.175	0.128	0.009	0.733
Gangwon-do	200	0.48	0.501	0	1
Gyeonggi-do	200	0.52	0.501	0	1
# Births	200	155.04	202.958	9	816
# Deaths	200	77.41	69.978	11	315
# Marriage	200	105.205	140.911	6	692
# Male	200	95622.33	119003.7	11089	465232
# Population	200	189345	238592.3	21297	941876
Net Migration	200	203.18	789.783	-1071	6482
	2013 Nuclear Test				
Variable	Obs	Mean	Std. Dev.	Min	Max
Transaction	200	730.685	1020.449	11	5642
Price	200	171.209	70.474	78.455	328.751
% Apartment	200	0.628	0.198	0.176	0.927
% Office House	200	0.013	0.025	0	0.121
% House 1	200	0.188	0.148	0.019	0.667
% House 2	200	0.171	0.117	0	0.557
Gangwon-do	200	0.48	0.501	0	1
Gyeonggi-do	200	0.52	0.501	0	1
# Births	200	149.01	189.136	3	758
# Deaths	200	84.44	78.979	9	382
# Marriage	200	100.74	135.746	4	723
# Male	200	101780.9	123960.9	12066	492083
# Population	200	201626.7	248778.9	22762	999116
Net Migration	200	151.82	643.979	-1156	4401

Table shows the summary statistics of housing transactions related to the 2009 and 2013 nuclear provocations averaged at the locality level.

Table 3.4: Effects of 2009 Nuclear Test on Housing Price of Border Cities

	Control Group A			Control Group B		
	(1)	(2)	(3)	(4)	(5)	(6)
Border * Post	0.061***	0.003	-0.034***	0.015*	-0.018**	-0.012
	(0.006)	(0.007)	(0.007)	(0.008)	(0.009)	(0.009)
Border	-0.196***	-0.187***	-0.202***	0.010	0.041***	0.014
	(0.047)	(0.045)	(0.026)	(0.014)	(0.014)	(0.014)
Post (90 days)	-0.014***			0.018***		
	(0.005)			(0.006)		
Post (180 days)		0.016***			0.007	
		(0.005)			(0.006)	
Post (360 days)			0.030***			0.003
			(0.005)			(0.007)
Constant	4.437***	3.809***	4.734***	4.404***	4.382***	4.079***
	(0.079)	(0.045)	(0.042)	(0.140)	(0.304)	(0.081)
Observations	38250	39776	36846	20787	21503	19284
R-squared	0.665	0.659	0.660	0.774	0.748	0.766

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Outcome variable: log-transformed price in 2009
The first three columns use border cities, and the next three use the distance measure to the border to define treatment and control groups. In each set of columns, the first, second, and third columns display the results using 90, 180, and 360 days after the provocation.

Table 3.5: Effects of 2013 Nuclear Test on Housing Price of Border Cities

	Control Group A			Control Group B		
	(1)	(2)	(3)	(4)	(5)	(6)
Border * Post	0.010	0.006	0.002	0.002	0.003	0.009
	(0.007)	(0.006)	(0.006)	(0.009)	(0.008)	(0.008)
Border	0.031	0.051	-0.473***	0.041***	0.051***	0.040***
	(0.054)	(0.043)	(0.024)	(0.014)	(0.012)	(0.012)
Post (90 days)	0.008			0.020***		
	(0.005)			(0.007)		
Post (180 days)		0.027***			0.028***	
		(0.004)			(0.006)	
Post (360 days)			0.048***			0.032***
			(0.004)			(0.006)
Constant	4.632***	4.898***	4.499***	5.271***	4.501***	4.186***
	(0.054)	(0.041)	(0.068)	(0.073)	(0.277)	(0.202)
Observations	28244	36287	41268	15942	20517	23496
R-squared	0.639	0.655	0.658	0.712	0.718	0.726

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Outcome variable: log-transformed price in 2013
The first three columns use border cities, and the next three use the distance measure to the border to define treatment and control groups. In each set of columns, the first, second, and third columns display the results using 90, 180, and 360 days after the provocation.

Table 3.6: Effects of Nuclear Test on Housing Prices of All Regions in South Korea

	Nuclear Test 2009			Nuclear Test 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance * Post	-0.433*	-0.161	0.026	-0.123	0.003	-0.176
	(0.220)	(0.354)	(0.297)	(0.225)	(0.161)	(0.133)
Post	0.054***	0.025	-0.001	0.017	0.027***	0.055***
	(0.013)	(0.029)	(0.023)	(0.014)	(0.010)	(0.008)
Distance	-0.053	0.061	-0.297	-0.122	-0.126	-0.104
	(0.362)	(0.421)	(0.308)	(0.291)	(0.321)	(0.307)
Observations	100365	99233	92209	67754	88672	101575
R-squared	0.726	0.709	0.726	0.691	0.695	0.695

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Outcome variable: log-transformed price
The first three columns examine the housing price related to the 2009 nuclear test and the next three show the results of the 2013 nuclear test. In each set of columns, the first, second, and third columns display the results using 90, 180, and 360 days after the provocation.

Table 3.7: Effects of Nuclear Tests on Housing Price of Border Cities using Alternative Control Group

	Nuclear Test 2009			Nuclear Test 2013		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat * Post	-0.028***	-0.021***	-0.075***	0.001	0.005	-0.016***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Treat	-0.132**	-0.648***	-0.602***	-0.928***	-0.073	-0.064
	(0.054)	(0.021)	(0.022)	(0.029)	(0.045)	(0.051)
Post (90 days)	0.074***			0.019***		
	(0.004)			(0.005)		
Post (180 days)		0.072***			0.030***	
		(0.004)			(0.005)	
Post (360 days)			0.071***			0.072***
			(0.004)			(0.004)
Constant	4.079***	3.778***	4.553***	3.990***	3.949***	4.054***
	(0.068)	(0.043)	(0.017)	(0.071)	(0.061)	(0.058)
Observations	49182	49780	48712	29247	37930	42166
R-squared	0.694	0.679	0.692	0.671	0.685	0.674

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Outcome variable: log-transformed price
The first three columns examine the housing price related to the 2009 nuclear test and the next three show the results of the 2013 nuclear test. In each set of columns, the first, second, and third columns display the results using 90, 180, and 360 days after the provocation.

Table 3.8: Effects of Nuclear Tests on Housing Price of Military Base in Gunsan

	2009 Nuclear Test			2013 Nuclear Test		
	(1)	(2)	(3)	(4)	(5)	(6)
Base * Post	-0.009	-0.024	0.016	-0.029	-0.033	-0.024
	(0.026)	(0.023)	(0.024)	(0.032)	(0.030)	(0.028)
Base	-0.229	-0.270	-0.154	-0.369	-0.306	1.043**
	(0.268)	(0.215)	(0.391)	(0.420)	(0.211)	(0.419)
Post (90 days)	0.034			0.068**		
	(0.023)			(0.027)		
Post (180 days)		0.067***			0.052**	
		(0.021)			(0.025)	
Post (360 days)			0.101***			0.063***
			(0.021)			(0.023)
Constant	3.756***	5.037***	3.286***	3.693***	5.214***	4.234***
	(0.296)	(0.278)	(0.163)	(0.346)	(0.490)	(0.396)
Observations	5420	6133	5870	3138	3572	4022
R-squared	0.512	0.518	0.517	0.559	0.563	0.546

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Outcome variable: log-transformed price
The first three columns examine the housing price related to the 2009 nuclear test and the next three show the results of the 2013 nuclear test. In each set of columns, the first, second, and third columns display the results using 90, 180, and 360 days after the provocation.

Table 3.9: Effects of Nuclear Tests on Housing Price of Military Base in Wonju

	2009 Nuclear Test			2013 Nuclear Test		
	(1)	(2)	(3)	(4)	(5)	(6)
Base * Post	-0.110***	-0.074***	-0.068***	-0.020	-0.010	0.005
	(0.015)	(0.014)	(0.014)	(0.016)	(0.014)	(0.014)
Base	0.048	-0.124**	0.024	0.079	-0.126**	-0.042
	(0.059)	(0.059)	(0.054)	(0.063)	(0.062)	(0.057)
Post (90 days)	0.030***			0.025**		
	(0.010)			(0.012)		
Post (180 days)		0.057***			0.045***	
		(0.009)			(0.010)	
Post (360 days)			0.097***			0.058***
			(0.009)			(0.010)
Constant	4.160***	3.855***	4.006***	3.890***	5.593***	5.092***
	(0.378)	(0.178)	(0.092)	(0.161)	(0.294)	(0.302)
Observations	6742	7438	7303	5480	6316	7193
R-squared	0.499	0.506	0.517	0.480	0.503	0.488

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$. Robust SE in parentheses.
Outcome variable: log-transformed price
The first three columns examine the housing price related to the 2009 nuclear test and the next three show the results of the 2013 nuclear test. In each set of columns, the first, second, and third columns display the results using 90, 180, and 360 days after the provocation.

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More Money, More Marriages, and More Babies? Evaluating Policy Impacts on Marriage and Fertility in South Korea

The fertility effects of pro-natalist policies: A density approach, with Hugo Jales (R&R, Journal of Asian Economics)

Threat or Empty Threat: Perception toward North Korea's Nuclear Provocation

Works in Progress

Gender and Racial Bias in Texts: the Case of Ratemyprofessor.com, with Maria Zhu

The Effect of School Closings on Child Welfare at Home, with Maria Zhu