CESifo WORKING PAPERS

11215
2024

July 2024

Baby Bonus, Fertility, and Missing Women

Wookun Kim

CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
· from the CESifo website: https://www.cesifo.org/en/wp

CESifo *Working Paper No. 11215*

Baby Bonus, Fertility, and Missing Women

Abstract

This paper presents novel causal evidence on the effects of pro-natalist cash transfers on fertility, sex ratio at birth, and infant health. In the context of South Korea, I exploit rich spatial and temporal variation in cash transfers provided to families with newborn babies and the universe of birth-, death-, and migrant-registry records. I find that the total fertility rate in 2015 would have been 4.7% lower without the cash transfers. Surprisingly, the cash transfers had an unintended consequence of correcting the unnaturally male-skewed sex ratio at birth. The cash transfers led to reductions in gestational age and birth weight, but no change in early-life mortality. A rich heterogeneity analysis suggests that negative selection into childbearing may explain the health effects and that cash transfers may increase birth weight for low-income families.

JEL-Codes: H400, H750, I500, J130, J160, J180.

Keywords: pro-natalist policies, cash transfer, fertility, infant health, sex ratio at birth, son preference.

Wookun Kim
Department of Economics
Southern Methodist University
3300 Dyer Street, Suite 301
USA – Dallas, TX 75205
wookunkim@smu.edu

July 2, 2024
Click here for the most recent version:
https://smu.box.com/s/infvfw8h4mdpxiaqlw7g2dq2t7kdr9ng
I am grateful to Adriana Lleras-Muney and Kathleen McGarry for their encouragement and guidance. I thank the journal's editor, Maya Rossin-Slater, and anonymous referees for their insightful comments and constructive feedback. I benefited from discussions with Libertad González, Rannveig Hart, Diederik Boertien, Hyun-A Kim, Brett McCully, Klaus Desmet, Daniel Millimet, Gunnar Andersson, Paola Giuliano, Youssef Benzarti, Manisha Shah, and many seminar participants at Sungkyunkwan University, Collegio Carlo Alberto, Centre for Fertility and Health (NIPH Norway), Korea Institute of Public Finance, Southern Methodist University, University of Oklahoma, University of Miami, UCLA, and DSE Winter School as well as at the 90th SEA meeting, 2021 Annual Conference of ESPE, 2021 CMES, 2021 EEA-ESEM Congress, and 2021 and 2018 PAA meetings for their helpful comments. This project was supported in part by the California Center for Population Research with a grant (T32HD007545; P2CHD041055) from the Eunice Kennedy Shriver National Institute of Child Health and Human Development. The content is solely my responsibility and does not represent the official views of the NICHD or the National Institutes of Health. All errors are mine.

1 Introduction

Total fertility rates have dramatically declined in much of the developed world, and women are having fewer than two children on average (Strulik and Vollmer, 2015). Policy makers have expressed growing concerns about the consequences of the resulting demographic imbalance (for example, declining size of the labor force relative to the dependent population), which is exacerbated by an aging population (Morgan, 2003; Frejka et al., 2010; Harper, 2014).[1] It is important to understand not only the consequences of these policies for fertility but also how pro-natalist incentives shape infant outcomes directly and indirectly through selection into childbearing.

South Korea—the empirical context of this paper—has experienced a rapid decline in fertility, with a total fertility rate below the replacement level of 2.1 since the early 1980s. During the same time, the low fertility rates coincided with unnaturally high male-to-female sex ratios at birth, especially among babies of higher birth orders. In the early 2000s, some local governments started to provide cash rewards to families with newborn babies, and this policy (called pro-natalist cash transfers or the baby bonus) became ubiquitous in 2012. Pro-natalist cash transfers have also been implemented in other countries, such as Canada, Israel, Japan, and Spain. In this paper, I provide novel causal evidence on the effects of pro-natalist cash transfers on fertility, sex ratio at birth, and infant health. To do so, I leverage plausibly exogenous variation in the baby bonus and the universe of birth-, death-, and migrant-registry records from South Korea.

South Korea represents an ideal environment to study the effects of the baby bonus. There is rich variation in both implementation timing and the generosity of the baby bonus by birth order (first, second, and third), over time (year), and across granular spatial units (districts).[2] I leverage this plausibly exogenous variation to identify the causal effects of pro-natalist cash transfers on birth outcomes. I construct a yearly panel data set of districts from 2000 to 2015 with cash-transfer amounts and the number of births by birth order and mother's age to estimate the baby bonus's effects on birth rates by birth parity and mother's age. I merge this data set with the universe of confidential birth-registry records to study the effects on the sex ratio at birth and infant health. Furthermore, I use confidential birth-death matched registry data for children born between 2010 and 2013 and investigate the baby bonus's effect on early-life mortality.

For causal identification, I include both district fixed effects and city-by-time fixed effects in the estimating equations throughout this paper. The district fixed effects purge any time-invariant district-level characteristics, such as baseline demographic composition and sticky social norms. The city-by-time fixed effects absorb any trends and changes common across districts within each city for each time unit (for example, a year or a month) as well as any national-level shocks. These fixed effects capture local shocks that change over time—for instance, local labor, housing, and marriage

[1] Jones (2022) emphasizes the importance of policies related to increasing fertility and shows that we may converge on an empty planet—that is, a world in which "knowledge and living standards stagnate for a population that gradually vanishes."

[2] Districts in South Korea are the smallest administrative units with self-governing authorities and sub- administrative units in 17 metropolitan cities and provinces (for example, Seoul Metropolitan City and Gyeonggi Province), both referred to as cities.

market conditions. I further introduce time-varying district-level characteristics as control variables that are likely correlated with both cash transfers and birth outcomes. The residual variation in the pro-natalist cash-transfer generosity and implementation timing is explained by arguably random factors, such as the distribution of policy makers' subjective beliefs about the efficacy of and political returns from the baby bonus and the distribution of idiosyncratic errors in forecasting local fiscal capacity to operationalize local baby-bonus programs. The key identification assumption is that this residual variation is orthogonal to all other determinants of birth outcomes, conditional on the observed control variables and the rich set of fixed effects.

I find that the baby bonus increased birth rates. Based on a heterogeneity-robust event-study framework (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021), I estimate a statistically significant increase in birth rates of 1.6% to 5% across birth orders. The birth rates increased for mothers in their prime age of childbearing. Leveraging the variation in cash-transfer generosity, I estimate the elasticity of birth rates with respect to the generosity. I find that a 10% increase in the cash transfers raised birth rates by 0.58%, 0.34%, and 0.36% for the first, second, and third births, respectively. A back-of-the-envelope calculation suggests that in the absence of the cash transfers, ceteris paribus, the total fertility rate would have been lower by 4.7% compared to the observed total fertility rate in 2015, which corresponds to approximately 562,439 fewer children ever born over the life cycle of the 2015 female population.[3]

I uncover two new, important effects of these pro-natalist cash-transfer programs. First, the cash transfers for a given birth order affected only the birth rate of the corresponding birth order. For example, the elasticity of second-child birth rates with respect to the cash transfers for a second child is positive and statistically significantly different from zero, but the elasticity of those rates with respect to the cash transfers for a first or third child are small and statistically indistinguishable from zero. This result implies that the increase in birth rates was driven by changes in the childbearing decisions of parent(s) at the margin of having an additional child. Second, the elasticities of cash transfers were positive only among mothers between the ages of 20 and 39, who were actively making childbearing decisions. The pro-natalist cash transfers had no impact on the birth rates of adolescents and mothers older than 40.

The baby bonus had an unintended, surprising consequence for the sex ratio at birth. It modulated the sex ratio, which initially favored boys because of son preference, toward the natural sex ratio at birth. First, I document an unnaturally high male-to-female ratio at birth particularly among third births (namely, 121 boys for 100 girls according to the birth records in 2000)—apparent evidence of missing baby girls due to sex-selective abortion.[4] The sex ratio at birth among third births, however, showed a dramatic decline to a level consistent with the natural ratio (105 boys for

[3] Reaching the 2.1 replacement level would require an increase in cash-transfer generosity of 10 to 16 times to a level equivalent to the median annual household income.

[4] A plethora of papers find that the natural sex ratio at birth is 105 males to 100 females—for example, Jacobsen et al. (1999). I use the 2015 Population Census, from which I observe family composition and compute the probability of having a boy after two daughters. The implied sex ratio, in this case, is 180 boys to 100 girls. The sex ratio for third children is 105 boys to 100 girls for families with one son and one daughter and 101 boys to 100 girls for families with two sons.

100 girls) in 2015. Based on the universe of confidential birth-registry records from 2000 to 2015, I find that the baby bonus lowered the probability of a third child being a boy. The estimated elasticity with respect to cash-transfer generosity implies that in the absence of the cash transfers, holding everything else constant, the sex ratio at birth among the third children born in 2015 would have been 124.7 boys per 100 girls. The cash transfers explain about 53% of the total decrease in the sex ratio at birth from 2000 to 2015.

The baby bonus also had unintended consequences for infant health. I estimate statistically significant negative policy effects on birth weight and gestational age. These effects were concentrated among higher-order births. For example, doubling the cash-transfer generosity for a third child decreased the birth weight of a third child on average by 7.2 grams and the gestational age by 0.2%. The decrease in gestational age resulted in a higher incidence of preterm births. By matching confidential death records with the birth-registry records for the children born between 2010 and 2013, I am able to study the longer-term effect of the cash-transfer programs on early-life mortality, and I find no evidence that the children born in districts where parents received different amounts of the cash transfers were more or less likely to die before reaching the age of one or five.

I study potential mechanisms explaining the main results. I provide evidence that the increase in birth rates was a result of more children ever born by women, not a mere reflection of the temporal adjustment of childbearing timing and the spatial sorting of families expecting newborn babies and relocating to places with more generous cash transfers. I find that the probability that a baby has an unemployed mother or an unemployed father increased in cash-transfer generosity. Particularly given the wide gap in employment rates between mothers and fathers, families with an unemployed father are likely low-income households. I provide evidence that selection into childbearing on the basis of income and other unobserved parental characteristics shaping infant health may explain why the policy effects on birth weight and gestational age were negative. The results also suggest that, at least among low-income families, the direct effect of the baby bonus on birth weight may have been positive if the indirect effect—through its effect on gestational age—is properly controlled for. Last, using the 2015 Population Census, in which the sibling composition of each household is observed, I replicate the main result for the sex ratio at birth and show that the decline of the sex ratio at birth among third children is driven by families with at least one boy before having their third child.

This paper builds upon the literature in economics analyzing the effects of pro-natalist policies on fertility.[5] Lalive and Zweimüller (2009) find that the extension of parental leave in 1990 in Austria increased the probability of women having an additional child, while Andersson and Duvander (2006) find no such effect in Sweden. There is a large literature finding little to no effect on fertility decisions of U.S. tax policies and welfare programs benefiting families with children (Whittington et al., 1990;

[5] Many policies to boost fertility, such as cash transfers, parental leave, and tax benefits, have been proposed and implemented around the world. The U.N. Population Division (2011) documents that 40 out of 47 countries with low fertility had pro-natalist policies as of 2010; the majority of these countries provided cash incentives. Fleckenstein and Lee (2012) detail pro-natalist policy changes in Britain, Germany, South Korea, and Sweden; Frejka et al. (2010) summarize pro-natalist policies implemented in East Asia. See Gauthier (2007) and Hart et al. (2024) for a literature review.

Whittington, 1992; Crump et al., 2011; Rosenzweig, 1999; Kearney, 2004).[6] The literature on pro-natalist cash transfers has mainly focused on difference-in-differences strategies and compared the fertility outcomes before and after policy implementation, often based on a one-time change in the policy, while using unaffected regions or ineligible families as a control group (Milligan, 2005; Boccuzzo et al., 2008; Cohen et al., 2013; Riphahn and Wiynck, 2017; Malkova, 2018; Malak et al., 2019).[7] González (2013) and González and Trommlerová (2021) estimate positive and persistent fertility effects of a universal child benefit in Spain based on a regression discontinuity design. Hong et al. (2016) examine the same local-government transfers in South Korea as in this paper but for a shorter period—from 2005 to 2011—and provide suggestive evidence that the policies may have increased birth rates but not necessarily completed fertility.

This paper contributes to the aforementioned literature by offering several novel insights. Estimating the elasticities of birth rates with respect to cash-transfer generosity by birth orders and female age groups, I find that parents at the margin of having an additional child were incentivized by the baby bonus: there was no inframarginal effect. Only the mothers who were in their prime age for childbearing responded to the cash transfers. The effect of pro-natalist cash transfers was not transitory and increased completed fertility. Though effective, the cash transfers alone may not be a sufficient (or cost-effective) way to raise the fertility rate back to the 2.1 replacement level.

Second, this paper fills a gap in the literature on pro-natalist policies by estimating cash- transfer effects on the sex ratio at birth and infant health. Only a few papers have studied pro-natalist cash transfers' effects on outcomes other than number of births.[8] For example, the literature has investigated cash transfers' effects on household consumption and maternal labor supply in Spain (González, 2013), on the housing market and family stability in Russia (Yakovlev and Sorvachev, 2020), and on children's educational and mental health outcomes in Canada (Milligan and Stabile, 2011). My contribution to this literature is twofold. First, I study whether pro-natalist cash transfers affected babies' early-life outcomes (namely, the sex ratio at birth, birth weight, gestational age, and mortality) that shape individual long-run outcomes and economy-wide conditions (for example, education attainment, marriage market, labor productivity, and crime).[9] Second, I find that the effect on infant health is negative and provide suggestive evidence that this is partly driven by

[6]Laroque and Salanié (2004) study the effect of the French tax system on fertility. See Hotz et al. (1997) and Hoynes (1997) for a broad review of earlier works on related topics.

[7]Malkova (2018) uses an event-study framework to estimate the effects of Russia's 1981 expansion in maternity benefits, which provided both maternity leave and small cash transfers, and finds that fertility rates increased.

[8]There is a large and robust literature on the effects of parental leave policies on children's outcomes. Some studies find that maternity leave increased birth weight in the U.S., driven by more educated and working mothers (Rossin, 2011)and disadvantaged mothers (Stearns, 2015). Carneiro et al. (2015) show that the maternity leave benefit in Norway led to a decrease in high school dropout rates and increased adulthood earning. Some find no effect of parental leave polices on children's education and labor market outcomes (Liu and Skans, 2010; Rasmussen, 2010; Baker and Milligan, 2010; Dustmann and Schönberg, 2012; Dahl et al., 2016). See Rossin-Slater (2017) for a comprehensive review of maternity leave polices.

[9]A large literature establishes the causal links between infant and childhood health factors and later outcomes (Behrman and Rosenzweig, 2004; Almond et al., 2005; Black et al., 2007; Oreopoulos et al., 2008; McCrary and Royer, 2011; Case et al., 2002). Many works have estimated the effects of family characteristics (for example, parental education, incarceration, income, and family structure) on a range of child outcomes (Black et al., 2005; McCrary and Royer, 2011; Oreopoulos et al., 2008; Milligan and Stabile, 2011; Aizer and Doyle, 2015).

selection into childbearing by low-income families.

Last, this paper contributes to the literature on son preference. There is little evidence on the effect of pro-natalist financial incentives on the sex ratio. Several papers document son preference and male-skewed sex ratios at birth, especially in Asia.[10] Jayachandran (2017) finds a causal relationship between desired family size and son preference in India. Ebenstein (2010) finds that regions with higher fines for violating the one-child policy in China were associated with higher male-to-female ratios. Anukriti (2018) studies the Devirupak Scheme, which provided cash transfers based on the number of children and sex composition in India. She finds that son preference intensified, whereas total fertility declined.[11] In South Korea, Choi and Hwang (2020) show that son preference has been diminishing in recent years.[12] Yoo et al. (2016) also find a decline in son preference but report that the decision to have a third child depends largely on the sex composition of the first and second children. Adding to this literature, I document that much of the decline in the unnaturally male-skewed sex ratio at birth since 2000 is driven by the decline of the sex ratio among third births. I show that the baby bonus contributed to this decline, although it was not designed to do so, unlike the Devirupak Scheme in India. To the best of my knowledge, this paper is the first to show that a pro-natalist policy interacted with son preference and unintentionally alleviated sex-ratio imbalance in a culture in which having a son is favored over having a daughter.

The remainder of the paper is organized as follows. In Section 2, I provide institutional background on the local baby-bonus programs in South Korea and the main data sources. Section 3 describes the empirical strategies to identify the causal effects of the baby bonus on fertility, sex ratio at birth, and infant health. Section 4 presents the results based on the district-level analysis of the number of births and the individual-level analysis of the sex ratio at birth and infant health. In Section 5, I explore potential mechanisms explaining the main results. Section 6 concludes.

2 Background and Data

I construct a yearly panel data set of 222 districts in South Korea from 2000 to 2015 on local baby-bonus policies, demographic and other relevant local characteristics, and the number of births.[13] To investigate the baby bonus's effects on the sex ratio at birth, infant health, and early-life mortality, I merge this data set with confidential administrative birth-registry data that span the universe of births from 2000 to 2015 and death records for the cohorts born between 2010 and 2013. In this section, I provide background information about the local pro-natalist cash-transfer policies

[10] See Bongaarts (2013) for a review of son preference and fertility decisions. González (2018) documents the skewed sex ratio among Indian immigrants in Spain and finds no difference in infant health by gender among the immigrant population.

[11] Anukriti and Kumler (2019) find that tariff shocks in India resulted in increased fertility and decreased the sex ratio at birth.

[12] Chung and Gupta (2007) argue that the trend in Korea is due to a country-wide change in social norms.

[13] During the sample period, some districts were merged or split. Because the policy information for these districts no longer exists, I restrict the sample to the 222 districts that did not redistrict and I construct a balanced panel of districts. These districts belong to 15 cities (that is, metropolitan cities and provinces). The final sample represents over 95% of the population. In the appendix, Figure A.1 plots a map of South Korea and shows 222 districts in 15 metropolitan cities and provinces (in different colors).

in South Korea, explain the sources for the main data sets and measurements, and investigate the determinants of the baby-bonus generosity and implementation timing.

2.1 Background

Before the 1960s, the total fertility rate in South Korea was high: above six children per woman. However, after pursuing one of the most fervent and successful family planning policies for over 20 years, the fertility rate has stayed below the 2.1 replacement level since 1983 (Lee and Choi, 2015). Fertility continued to decline until 2005, when the total fertility rate reached a historic low of 1.05. In response to growing concerns arising from the low fertility rate and the rapidly aging population, the national government established the First Basic Plans for Low Fertility and Aged Society in 2006, followed by a series of revised plans every 5 years thereafter. The plans outlined normative goals and operated at the national level.[14] In line with the administrative arrangements of the national and local governments (Local Autonomy Act, 1990), the national government's policies on welfare, generally speaking, are implemented nationwide and do not vary across districts and cities. Therefore, regardless of where people lived, they had equal access to the national pro-natalist policies.

As early as 2001, some local governments started to adopt a pro-natalist cash-transfer policy, which provided cash transfers to families with newborn babies. By 2012, all districts adopted this policy.[15] The structure of this policy (for example, eligibility and transfer method) was virtually identical across districts. Every family with a newborn baby was eligible to receive a baby bonus in their district of residence, unconditional on family earnings or employment status. To receive cash transfers, the parent(s) of a newborn baby simply had to register their baby's birth at a civic center in their district of residence.[16] Most beneficiaries received a one-time lump-sum transfer within a few weeks from claiming the benefit.[17] During the sample period of interest in this paper (that is,

[14]The plans of the national government "set abstract goals and directions, [and] did not specify guidelines for local policy formulations" (Kim, 2013). The national government made progress in a few areas. Paid parental leave was first implemented in 2001 with a monthly payment of KRW 300,000 (USD 265) up to one year, irrespective of income level. Benefits gradually increased over time, reaching KRW 1,000,000 (USD 883) in 2011 for some income groups (Kim et al., 2022). Both mothers and fathers were eligible, but the total leave for mothers and fathers could not exceed one year. The uptake rates among fathers have been very low, while the average combined length of maternity and paternity leaves has been consistently less than the full benefit duration of one year (Lee, 2022). Since 2018, the national government has offered pro-natalist cash transfers. During the period this paper focuses on, the national government did not implement other pro-natalist policies or revise other welfare programs, public childcare, and health care that may affect childbearing decisions.

[15]Each local government adopted this policy independently from the national government and thus financed its baby-bonus program using its own budget, which is the sum of local income tax revenue and intergovernmental transfers from the national government. Income tax rates in South Korea are nationally determined and do not vary by district. Intergovernmental transfers are determined following a complex formula in accordance with the national law. See Kim (2023) for a detailed discussion.

[16]Government officials often check the length of residency to prevent people from gaming the policy. Similarly, local governments can rescind the cash transfers if they identify fraudulent cases. However, these measures are precautionary and local government officials attest that they rarely witness such instances, especially since establishing residency in a new district is not trivial.

[17]A few districts with a generous baby bonus implemented an installment-payment scheme and spread the bonus out over a year or two.

2001 to 2015), the baby bonus was the only pro-natalist policy implemented at the local level; to raise public awareness, it was publicized through public announcements, street posters, fliers, and mail.

Notwithstanding the common structure of the policy across districts, the amount of the baby bonus varied widely across districts and by birth orders.[18] The amount in 2015 ranged from KRW 0 to 5.1 million (or approximately USD 4,505) for a first child, KRW 0 to 7.54 million (USD 5,703) for a second child, and KRW 200,000 (USD 77) to KRW 18.8 million (USD 16,608) for a third child. In most districts, the cash-transfer generosity increased in birth order. In 2015, the average cash-transfer amounts were KRW 770,000 (or approximately USD 680) for a first child, KRW 1,060,000 (or USD 926) for a second child, and KRW 2,660,000 (USD 2,350) for a third child.[19] In most districts, the cash-transfer amounts for all birth orders increased over time. After adopting the policy, the local legislative council members and the local governing head renewed the locality's baby-bonus scheme annually. Increased generosity often came with each renewal. Considering the ubiquitous concern regarding declining fertility across districts and the national sentiment of pro-natalism, the local baby-bonus programs may have been convenient for local politicians seeking to increase their public appeal and to boost fertility.

Figure 1 summarizes the local pro-natalist cash-transfer policies, total fertility rate, and male-to-female sex ratio at birth. The top panel plots the fraction of districts with the baby-bonus policy (dashed line) and the average amount of baby bonus conditional on having adopted the policy (solid line) across districts and birth parities. A small group of districts first implemented the cash-transfer policy in 2001, and the number of districts with the policy increased dramatically from 2005. By 2012, the baby bonus was available in all districts. Similarly to the policy adoption rate, the average amount of the baby bonus increased over time. In the center and bottom panels, average total fertility rate and male-to-female ratios at birth (solid lines) are plotted along with policy prevalence (dashed line; reproduced from the top panel) over time. As a greater number of districts began to offer a baby bonus, the decline in the average fertility rate seems to have stopped and reversed. During the same period, the sex ratio at birth declined to the natural ratio of 1.05 boys per girl.[20] I explore the extent to which these associations between local pro-natalist cash transfers and fertility rate and sex ratio at birth may be causal.

[18] I focus only on the first, second, and third children because the number of families with more than three children is economically small in South Korea; during the sample period, the first, second, and third newborn babies together constituted over 98.9% of all births. In most districts, the cash-transfer benefit did not increase after the third child, while a few district had more generous cash transfers for babies beyond the third child.

[19] The average cash-transfer generosity in 2015 for each birth parity was 22% (first child), 30% (second child), and 76% (third child) of the median monthly income of KRW 3.5 million for a four-person household and 43% (first child), 59% (second child), and 148% of the average female monthly salary of KRW 1.8 million (about 63% of the male monthly salary).

[20] From 1996 to 2000, the sex ratio at birth was relatively stable at around 1.1 boys for each girl.

2.2 Data

District-Level Variables: Cash Transfers and Birth Rates

The district-level data set comprises three key components. The first component pertains to the local baby-bonus programs. Because the policies are enacted and implemented at the district level, I filed an Official Information Disclosure Act request to each district and obtained information on the amount awarded to parents for their first, second, and third children. Based on the responses from local governments and cross-validation using alternative sources (for example, administrative policy reports, online repository of local ordinances and regulations, and interviews), I built a yearly panel data set of districts from 2000 to 2015 with the amounts of the baby bonus.

Panel A of Table 1 provides the summary statistics of the baby-bonus programs by birth order for selected years. The proportion of districts providing a baby bonus increased over time, eventually reaching 41%, 88%, and 100% in 2015 for a first, second, and third child, respectively. Panel A also reports the mean, standard deviation, and minimum and maximum values of cash-transfer amounts in KRW 1 million (USD 883) for districts with strictly positive cash-transfer amounts (that is, excluding the districts without a baby bonus for the corresponding birth order). They show rich variation in cash-transfer generosity over time (columns), across space (standard deviations, minimum, and maximum), and by birth order (rows).[21] The changes in cash-transfer generosity over time can be explained both by more districts adopting the policy with higher transfer amounts and those already offering baby bonus increasing the generosity.

The second set of variables relates to the number of births in each district. To understand how the effects of the baby bonus differed across female age groups, I need to know how many children were born by birth order in each female age group. Such detailed information is not publicly available. Therefore, I use the restricted-access confidential birth-registry records housed at the Bureau of Statistics of South Korea. The data span the universe of births registered in Korea from 2000 to 2015. I count the number of births by birth order and mother's age group (in 5-year intervals from 15 to 49) in each district-year pair. Together with the female-population data from the resident-registration database maintained by the Ministry of Interior and Safety, I construct birth rates specific to the birth order, both for the entire female population and for each age group. Panel B of Table1 reports the means and standard deviations of the total fertility rates and the parity-specific birth rates computed across districts for selected years.[22]

Last, I supplement the data set with district-level characteristics from various administrative data sources: the Korea Statistical Information System, Finance Integrated System, and National Election Commission of South Korea. These variables are used in two ways. First, I investigate the determinants of pro-natalist cash-transfer policy adoption and generosity. Second, throughout my analysis, I include these observable characteristics as control variables. The demographic char-

[21] Figure A.2 in the appendix presents a series of maps plotting cash-transfer generosity for each birth order in 2005, 2010, and 2015.

[22] In the appendix, Section A provides a detailed discussion of the construction of birth rates specific to age and birth order as well as the correspondence between these measures and the total fertility rate.

acteristics include total population, fraction of female population, proportion of adult population (between the ages of 25 and 60), proportion of the elderly, marriage rate, and net migration rates (that is, net inflows per 1,000 people). Local government characteristics include the gender and political party of the local-government head and the financial-independence rate, which measures the fiscal autonomy of each local-government.

Individual-Level Records: Gender and Infant Health, and Early-Life Mortality

I use the restricted-access confidential birth-registry records, spanning the universe of births registered in South Korea from 2001 to 2015, to study the effects of the baby bonus on the sex ratio at birth, birth weight measured in kilograms, and gestational age measured in weeks. Each record includes detailed information on a newborn baby (for example, the date and place of birth, birth order, gender, birth weight, and gestational age) and the parent(s) (for example, age, educational attainment, occupation, and marital status).[23] The total sample size is 7,080,381 births of first, second, and third children.

Table A.1 in the appendix reports the average birth weight, gestational age, and fraction of male births by birth order for selected years. Several notable patterns arise from the summary statistics. First, the average birth weight and gestational age were slightly lower across birth orders in 2015 compared to their 2000 average values. Second, though the differences are subtle, the difference was larger for higher-order births. Third, the fraction of boys among first births was stable at the natural sex ratio at birth and ranged between 0.512 and 0.515, which corresponds to 105 to 106 boys for 100 girls. Fourth, the fraction of boys among second and third births consistently decreased over time. The decline is particularly striking among third births: from 0.587 in 2000 to 0.513 in 2015 (from 143 boys to 105 boys for 100 girls).

For the cohorts born between 2010 and 2013 (1,711,947 births), their birth records are matched with death records of all the babies who died before reaching the age of five. Based on these birth-death matched data, I define two indicator variables measuring early-life mortality: one equal to 1 if the baby died before reaching the age of one (infant mortality), the other equal to 1 if the baby died before the age of five (under-five mortality). On average, about 1.7 and 2.3 children per 1,000 births born between 2010 and 2013 died before their first and fifth birthdays, respectively.

[23] The records do not include personal identifiers of parents; therefore, I cannot observe the fertility history of each mother and the sex composition of the siblings. Educational attainment and occupation are categorized differently in some years. By aggregating smaller categories, I create variables measuring educational attainment and occupation. The educational attainment levels used in this paper are no schooling, elementary school, middle school, high school, and some college or above; the occupation categories are professional service workers, office workers, sales/retail service workers, farmers/fishers, technicians, menial workers, and no occupation (unemployed or out of labor force). Occupation is measured at the time of childbirth. For instance, if a mother was on maternity leave, then her occupation would still be recorded. I construct an indicator variable for those employed based on the occupation categories. Marital status is an indicator variable that takes the value of 1 if information for both parents is provided and 0 otherwise.

2.3 Determinants of Policy Implementation Timing and Generosity

To estimate the causal effects of these pro-natalist cash transfers, I exploit the temporal and cross-sectional variation arising from local governments' decisions to adopt the policies and to change the cash-transfer generosity thereafter. These decisions are hardly random. Local governing heads and district-council members, who are locally elected, are responsible for designing and executing district-level policies. For causal analysis, it is important to understand, among other things, whether districts adopted the pro-natalist cash transfers early and offered a more generous baby bonus when they had suffered from low fertility rates and thus were keen on raising birth rates. I conduct statistical analyses to formally investigate the determinants of policy-implementation timing and generosity.

First, I study local characteristics that determined how long it took for districts to adopt the pro-natalist cash-transfer policy for some baseline years. Following a standard approach in the survival-analysis literature, I assume that the time until policy adoption T_τ since baseline year τ follows a Weibull distribution with shape parameter $\rho > 0$ (without loss of generality, assume scale parameter $\kappa = 1$ for simplicity). I derive the hazard function $\lambda(T_{d,\tau}|X_{d,\tau})$, which captures the instantaneous probability that a district adopts the pro-natalist cash-transfer policy after $T_\tau > 0$ years conditional on baseline year τ as a function of local characteristics $X_{d,\tau}$ observed in year τ and a log-normally distributed stochastic error term ϵ_τ. After applying some algebraic operations, I obtain the following equation:

$$\ln T_{d,\tau} = \alpha X_{d,\tau} + \epsilon_{d,\tau}. \tag{1}$$

This equation corresponds to an accelerated failure time model. Equation 1 sheds light on which variables explain how long it took for districts to implement the pro-natalist cash transfers. For example, did districts with lower fertility rates adopt the policy early? In addition to the observed district characteristics, city fixed effects are introduced to purge the effects of citywide economic shocks and market conditions (for example, labor and housing) that commonly affect the districts within each city.

Table 2 summarizes the results estimating equation 1 for baseline years from 2000 to 2006. Most of the observed demographic characteristics (for example, population, total fertility rates, fraction of female population, fraction of elderly population, and net migration rate) that may shape both policy decisions and birth rates do not individually explain the timing of policy adoption. In particular, I cannot reject the null hypothesis that the estimated effect of the total fertility rates is equal to zero across all baseline years. The sign of the estimates flips depending on the baseline years. Holding everything else constant, the baseline total fertility rates do not predict the adoption timing. While a low fertility rate is a common concern across districts, districts with lower fertility rates did not implement a baby-bonus program earlier than those with higher fertility rates. The estimated coefficients for marriage rate are consistently negative, which may imply that districts with more newly married couples adopted policies earlier. Districts with a higher fraction of adults in the population tended to take longer to start providing a baby bonus. These districts may have been less concerned about the declining number of births because more people had the potential for childbearing. Districts with conservative local governing heads appear to have implemented the policies later; this likely reflects the tendency of conservative parties to put less emphasis on policies influencing intrahousehold decisions. The estimated coefficients for financial-independence rate are consistently negative and statistically significantly different from zero across columns. Holding everything else constant, as the financial independence of a local government increases, it is more likely to adopt a pro-natalist cash-transfer policy early. Therefore, the local-government budget seems to play an important role in determining the ability of local governments to implement their own policies.

Second, I investigate the extent to which local characteristics explain the variation in the baby-bonus generosity and estimate a specification as follows:

$$\sinh^{-1} CT_{p,d,y} = \phi_d + \psi_{c(d),y} + \pi X_{d,y} + \epsilon_{d,y}, \tag{2}$$

where the dependent variable $\sinh^{-1} CT_{p,d,y}$ is the inverse hyperbolic sine transformation of the amount of baby bonus provided to families with a new baby of birth order p in district d in year y.[24] District fixed effects ϕ_d capture all time-invariant local characteristics. City-by-year fixed effects $\psi_{c(d),y}$ capture time-variant city-level determinants of the cash-transfer generosity (for example, labor market conditions, which in turn affect local government budget); $X_{d,y}$ represents explanatory variables including demographic and local government characteristics in district d observed in year y.

Table 3 summarizes the results estimating equation 2 by birth order p. For each birth order, the left column includes all districts and years from 2001 to 2015. The results in this column capture both the extensive margin of policy adoption and the intensive margin of changes in generosity over time. The right columns exclude district-year observations prior to policy adoption. Under this sample restriction, I focus on the intensive margin of baby bonus and study the factors that explain the differences in cash-transfer amounts across districts and the changes in generosity over time. When looking at both the extensive and intensive margins together (Columns 1, 3, and 5), the results indicate that the cash-transfer amounts were lower in districts with a higher fraction of females and adults in the population across birth orders. In contrast, all of the estimated coefficients for these variables in Columns 2, 4, and 5 lose their statistical significance. The one-year lagged value of total fertility rate is positively correlated with the cash-transfer generosity only for a first child, while this relationship is not statistically significant for a second and third child. The results imply that the cash-transfer generosity did not change systematically with the past total fertility rate; it is not the case that districts with lower fertility rates were more generous with their baby-bonus programs.

While most of the coefficient estimates individually might not explain the variation in cash transfers, the results of a joint-hypothesis test indicate that these factors may be jointly correlated

[24]The inverse hyperbolic sine transformation approximates the natural logarithm of the baby-bonus amount and allows me to retain observations with zero values (for example, districts prior to the policy implementation).

with the observed policy variation.[25] These local characteristics (for example, fraction of female population, age composition, and cultural norms proxied by party identification and gender of local leaders) may affect birth outcomes. Thus, I control for these time-varying district-level factors and include the district fixed effects and the city-by-year fixed effects throughout my analysis.

3 Empirical Strategy

In this section, I present empirical strategies to identify the baby bonus's effects on the number of births using the district-level data set and its effects on the sex ratio at birth and infant health using the individual-level confidential registry records. I leverage the temporal and cross-sectional variation in the generosity and policy-implementation timing of cash transfers and introduce a rich set of fixed effects and control variables to purge key confounding forces.

3.1 District-Level Analysis: Number of Births

Based on the different implementation timing of the pro-natalist cash-transfer policy in each district, I employ an event-study framework and semiparametrically estimate the policy effects on birth rates before and after implementation based on the specification that follows:

$$\ln BR_{p,d,y} = \phi_d + \psi_{c(d),y} + \delta X_{d,y} + \sum_{\tau=L}^{U} \gamma_p^{(\tau)} D_{p,d,y}^{(\tau)} + \epsilon_{p,d,y}, \tag{3}$$

where the dependent variable $\ln BR_{p,d,y}$ is the log of birth rates for birth order p in district d in year y. District fixed effects ϕ_d capture all time-invariant district-level determinants of birth rates. City-by-year fixed effects $\psi_{c(d),y}$ flexibly capture the year-to-year changes in city-level shocks as well as the national-level shocks that may be correlated with birth rates and local policies (for example, local labor and housing market conditions). $X_{d,y}$ is a set of district-level time-varying characteristics.[26] $\left\{D_{p,d,y}^{(\tau)}\right\}_{\tau=L}^{U}$ is a set of dummy variables indicating whether or not the number of years since district d implemented the cash-transfer policy for birth order p is equal to τ in year y.[27] $\epsilon_{p,d,y}$ is an error term. Event study coefficients $\left\{\gamma_p^{(\tau)}\right\}_{\tau=-L}^{U}$ measure the percent change in the birth rates for the p-th birth order τ years before and after the adoption of the cash-transfer policy for

[25] For each specification, I test the joint significance of all the covariates and report the p-values. I reject the null hypothesis that all the coefficients are zero at the 0.1% significance level across birth orders when all district-year pairs are considered. The p-values increase when pooling observations after policy implementation. I cannot reject the null hypothesis at the 5% significance level for cash transfers for a second child, but I do reject it for first and third children.

[26] The set includes the same variables used to study the determinants of the implementation timing (excluding total fertility rates) and generosity. Additionally, I include the one-year lag number of births of birth order $p' = p - 1$ when estimating the cash transfer's effect on the birth rates of birth order $p = 2, 3$. For instance, the lag number of births for the first birth order is included when the birth rates of the second child are the dependent variable. These additional variables together with the fraction of female population, proxy the number of families and parents who might benefit from the pro-natalist cash transfers. As a result, the total observation is equal to 3,330 district-year pairs (15 years from 2001 to 2015 times 222 districts).

[27] The maximum numbers of year before and after policy implementation $[L, U]$ are $[-10, 8]$ for $p = 1$ and $[-14, 11]$ for $p = 2, 3$.

the corresponding birth order. Because birth rates capture the number of babies born during each calendar year, the baby bonus is expected to affect the babies born in the year when the district first adopted the cash transfers: $\tau = 0$. The identification comes from comparing the difference in birth outcomes between districts with different implementation timing for each event year τ across different event years before and after policy implementation.

Estimation of equation 3 by OLS might not identify the average treatment effects of the baby bonus in this empirical setting, as the timings of policy implementation across districts are staggered over different years.[28] I follow Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) and estimate equation 3 using an estimation method robust to potential biases from the staggered introduction of the treatment under two identifying assumptions. The first assumption is the parallel-trend assumption: in the absence of policy implementation, birth rates would have changed, on average, the same way across districts. Second, the adoption of the baby bonus was not anticipated.[29]

Next, I exploit the rich variation in the cash-transfer generosity to estimate the elasticity of birth rates with respect to the generosity. Instead of taking the log transformation of the generosity $CT_{p,d,y}$, I take the inverse hyperbolic sine transformation to include observations from pre-policy-implementation years when $CT_{p,d,y} = 0$. I estimate the following equation:

$$\ln BR_{p,d,y} = \phi_d + \psi_{c(d),y} + \delta X_{d,y} + \beta_p \sinh^{-1} CT_{p,d,y} + \epsilon_{p,d,y}, \tag{4}$$

where ϕ_d and $\psi_{c(d),y}$ are the same set of district fixed effects and city-by-year fixed effects as in equation. 3. Similarly, I introduce the same time-varying district-level characteristics as control variables. Coefficient βp captures the effect of pro-natalist cash transfers on birth rate of the p-th birth order.[30]

For causal interpretation, the identification assumption is that absent pro-natalist cash transfers, the birth rates would vary across districts within a city in a given year for reasons that are uncorrelated with the pro-natalist cash transfers. That is,

$$E\left[CT_{p,d,y} \times \epsilon_{p,d,y} | \phi_d, \psi_{c(d),y}, X_{d,y}\right] = 0. \tag{5}$$

I argue that this identification assumption (equation 5) is likely to hold in my analysis. Unlike

[28] See Goodman-Bacon (2018), de Chaisemartin and D'Haultfœuille (2020), Sant'Anna and Zhao (2020), Callaway and Sant'Anna (2021), and Borusyak et al. (2021) for recent studies using two-way fixed-effects difference-in-difference estimators, which address the identification issues with a staggered treatment setup.

[29] The current state-of-the-art estimators in this literature do not allow refinements in the two fixed effects (treatment unit-level and time level). To partial out the effects of time-varying metropolitan-city shocks and the control variables, I first estimate equation 3 without the event-study dummy variables and obtain the residuals. Then, I estimate the changes in the birth rates before and after policy implementation using the doubly robust difference-in-differences estimator proposed in Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021). Accordingly, I bootstrap the standard errors clustered at the district level. Instead of a universal base period, I use a varying base period to capture the treatment effect.

[30] In the appendix, Figure A.3 (top panels) plots the residual variation in cash-transfer generosity after controlling for the observable district-level demographic and local-government characteristics and by gradually adding the district fixed effects and the city-by-year fixed effects using the full years from 2001 to 2015.

randomized controlled experiments, the cash-transfer generosity is not randomly assigned to each district in different years. Instead, it is determined through a legislative process. In Section 2.3, I studied district-level demographic and local-government characteristics that are relevant to policy makers and may systematically explain the variation in cash-transfer generosity. Conditional on these factors and the rich set of fixed effects, the residual variation in the cash-transfer generosity is plausibly exogenous. For instance, policy makers across districts may have personal beliefs about the need for and efficacy of pro-natalist cash transfers. In addition, it is impossible to perfectly forecast local-government revenue for the next fiscal year, and policy makers may under- or overestimate the government's fiscal capacity to offer a baby bonus. Such idiosyncratic factors explain the residual variation.

3.2 Individual-Level Analysis: Sex Ratio at Birth and Infant Health

I estimate the baby bonus's effects on the sex ratio at birth and infant health, which are important determinants of long-term individual and economy-wide outcomes (for example, labor market performance and marriage market conditions). The signs of these effects are theoretically ambiguous. For instance, the baby bonus may adversely affect health outcomes at birth if cash transfers result in less investment per child by increasing the number of births. Or they may improve infant health, as cash transfers serve as an additional resource to take better care of newborn babies. With respect to the sex ratio at birth, while on a downward trajectory, son preference in South Korea remains strong.[31] The baby bonus may provide financial means to parents as they continue to have babies until they have at least one boy. Or it may compensate for the utility penalty associated with having girls and mitigate sex-selective abortion. To estimate the baby bonus's effects on the sex ratio at birth and infant health, I apply the same set of empirical strategies to the universe of birth records.

First, I estimate the changes in birth weight, the probability of a baby being a boy, and gestational age before and after the policy implementation as follows:

$$H_{i,p,d,y} = \phi_d + \psi_{c(d),y,m} + \delta X_{d,y} + \omega W_i + \sum_{\tau=L}^{U} \gamma^{(\tau)} D_{p,d,y}^{(\tau)} + \epsilon_{i,p,d,y}, \tag{6}$$

where $H_{i,p,d,y}$ is a measure ofinfant outcome (namely, gender, birth weight, gestational age, and early-life mortality) of baby i of birth order p born in district d in year-month (y, m). Year-month-by-city fixed effects $\psi_{c(d),m,y}$ flexibly control for the month-to-month citywide shocks that affect birth outcomes.[32] In addition to using the same set of control variables $X_{d,y}$ as in equation 4, I leverage the parental information reported on each record and introduce a set of individual-level controls

[31] The extent to which the sex ratio at birth deviates from its natural level of 105 boys for 100 girls is especially pronounced among families when they first had daughters as opposed to sons. According to the 2015 Population Census, which covers about 20% of the population, the sex ratio is 181 boys for 100 girls among third children when their older siblings are both girls. This number drops to 101 boys for 100 girls if their older siblings are both boys.

[32] Rich individual records provide me with enough power to introduce the year-month-by-city fixed effects, which capture seasonality in birth weight and gestational age (Darrow et al., 2009; Bodnar and Simhan, 2008; Boland et al., 2015).

W_i including indicators for a child's birth order and parental educational attainment, age, marital status, and occupation types. $\epsilon_{i,p,d,y}$ is an error term. Event-study coefficients $\left\{\gamma^{(\tau)}\right\}_{\tau=-L}^{U}$ measure the change in the outcome of interest τ years before and after the adoption of the cash-transfer policy.

To estimate the effects of cash-transfer generosity on the sex ratio at birth and infant health, I estimate the following specification:

$$H_{i,p,d,y} = \phi_d + \psi_{c(d),y,m} + \delta X_{d,y} + \omega W_i + \beta \sinh^{-1} CT_{p,d,y} + \epsilon_{i,p,d,y}, \quad (7)$$

where the same set of fixed effects and control variables as in equation 6 are included, but the event-study dummy variables are replaced with $\sinh^{-1} CT_{p,d,y}$, the inverse-sine-transformed values of cash-transfer generosity. Coefficient β measures the baby bonus's effect on the outcome of interest. The source of identifying variation remains virtually the same as in the district-level analysis: time-series variation in cash-transfer amounts within each district and the spatial variation across districts within each year-month-by-city pair.[33] The identification assumption is expressed as follows:

$$E\left[CT_{p,d,y(m)} \times \epsilon_i | \phi_d, \psi_{c(d),m}, X_{d,y(m)}, W_i\right] = 0. \quad (8)$$

The use of registry records is particularly advantageous to justifying the identification assumption (equation 8). For instance, the rich parental information W_i allows me to flexibly account for the effects of parental characteristics without assuming a constant marginal effect for each of these observed factors.

4 Results

In this section, I present my estimation results in two parts. First, I discuss the district-level analysis investigating the baby bonus's effects on birth rates. The second part presents the estimation results regarding the sex ratio at birth and infant health based on the individual birth-registry records.

[33] Early-life mortality outcomes are only available for babies born between 2010 and 2013, during which time most districts had already adopted the policy. Because most of the districts already started providing a baby bonus by 2010, the event-study framework would only provide insights on the year-to-year changes in early-life mortality (years) after the cash transfer was implemented. However, there still remains both cross-sectional and temporal variation in cash-transfer generosity for the samples between 2010 and 2013. In the appendix, Figure A.3 (bottom panels) in the appendix plots the distribution of the residual variation in cash transfers for this period while controlling for the observable characteristics and gradually adding the fixed effects. While smaller when comparing them to the top panels in which all years are used, there still remains meaningful residual variation. The cross-sectional variation across districts is likely to contribute more to this variation.

4.1 Birth Rates by Birth Order and Mother's Age

Event Study Results.

I begin by presenting the event-study results. For each birth order $p = 1, 2,$ and 3, I estimate equation 3 and plot the estimated event-study coefficients $\left\{\gamma_p^{(\tau)}\right\}_{\tau=-5}^{5}$ in Figure 2.[34] In the top panel, τ =−5 the changes in the birth rates of the first births before and after policy implementation are plotted along with the 95% confidence intervals. Prior to the policy implementation ($\tau < 0$), none of the estimated coefficients are statistically different from zero at the 5% significance level. The birth rates of the first births stayed relatively constant until the policy implementation. After the baby bonus became available for a first child, the birth rates started to gradually increase; all of the event-study coefficients after implementation are positive and statistically different from zero. On average, the birth rates of first births increased by 5.0%. The middle and bottom panels of Figure 2 plot the event-study coefficients for the second and third births, respectively. The birth rates of both birth orders were relatively constant prior to the policy implementation. Thus, I conclude that the birth rates of these birth orders showed no pre-trend. Upon the policy implementation, the birth rates started to increase. The birth rates for the second and third births increased by 1.6% and 4.6%, respectively.[35]

Comparing the results to the previous literature, the estimated increase in birth rates ranging from 1.6% to 5.0% is close to the increase in fertility by 5% estimated in response to the 2007 introduction of a universal child benefit in Spain by González (2013). The increase in the birth rate of the first births implies not only a greater number of births of the corresponding birth order but also an increase in the number of families that would benefit from cash transfers provided for a second child. Similarly, the increase in the birth rates for the second children implies that providing the cash transfers to families having a second child increased the number of families with two children, which in turn became potential beneficiaries of the cash transfers for a third child. Importantly, the estimated coefficients across birth orders tend to increase, likely because of the increase in cash-transfer generosity over time within each district.

Next, further disaggregating the birth rates into age-specific birth rates, I estimate the changes in the birth rates before and after the policy implementations for different age groups of mothers. Figure 3 plots the event-study coefficients estimated for each birth order (separated by columns) and each age group of mothers (5-year intervals; top to bottom panels). First, I focus on age groups of mothers who are of prime childbearing age, between 20 and 39 years, and more likely to be actively making fertility decisions. The first-child birth rates increased across the age groups; the effect is particularly strong among relatively young mothers. Although modest in magnitude, the estimated

[34] The event-study coefficients are estimated for all the event-study dummies. The figure plots a subset of these estimates for event-time window $[-5, 5]$ because the event-study coefficients within this window are estimated using a greater number of districts. The estimated coefficients of event time are based on the fewer number of districts as the event time goes beyond zero. In the appendix, Figure A.4 extends the event window to $[-7, 7]$.

[35] In the appendix, Figure A.5, A.6, and A.7 plot the event study coefficients estimated without any district-level control variables (left) and with the control variables (right) by gradually adding the district fixed effects and city-by-year fixed effects (cross rows) for first, second, and third children respectively.

changes after policy implementation for the second-child birth rates are positive and statistically significant among mothers aged 30 to 34. Last, the third-child birth rates among mothers aged 25 to 29 increased after the policy implementation. Mothers who already had two children at a relatively young age seem to have responded to the cash transfers and had a third child. Also, the third-child birth rates increased among mothers aged 35 to 39.

Based on these results, I conclude that the pro-natalist cash transfers increased the birth rates across birth orders. However, there is large heterogeneity of the baby bonus's effect by birth order and mother's age. The cash transfers for a specific birth order increased the birth rates of that birth order among the mothers most likely at the margin of having babies of the corresponding birth order. Figure A.8 in the appendix shows that the birth rates among mothers aged 15 to 19 or 40 to 49 did not change before and after the policy implementation across birth orders. The event-study results are estimated using the variation in policy-implementation timing. Therefore, they do not take into account the fact that these cash transfers varied in generosity across districts and over time. In the next section, I leverage the variation in the cash-transfer generosity and estimate the elasticities of birth rates with respect to the amount of the baby bonus.

Elasticity of Birth Rates with Respect to Cash-Transfer Generosity.

I report the results estimating equation 4 for each birth order in Table 4.[36] Because the dependent variables are measured in log units and I take the inverse hyperbolic sine transformation of cash-transfer generosity, the estimated coefficients approximate the elasticities of birth rates with respect to cash transfers. To obtain the exact values, the estimated coefficients must be adjusted as follows:

$$e_{BR_p,CT_p} = \frac{\partial \ln BR_p}{\partial \ln CT_p} = \beta_p \times \underbrace{\frac{\bar{CT}_p}{\sqrt{\bar{CT}_p^2+1}}}_{\rho(p)}, \text{ where } \beta_p = \frac{\partial \ln BR_p}{\partial \sinh^{-1} CT_p} = \frac{\partial \ln BR_p}{\partial CT_p / \sqrt{\bar{CT}_p^2+1}}. \tag{9}$$

Thus, coefficient β_p in equation 4 can be re-scaled by adjustment factor $\rho(p)$ to compute elasticities. I evaluate the adjustment factors based on the average amount of the baby bonus provided for each birth order in 2015 (that is, $\bar{CT}_p = E\left[CT_{pdy} | y = 2015\right]$).[37]

In Columns 1 and 2, the dependent variable is the log of first-child birth rates. In the first column, the estimated effect of the cash transfer provided to a first child on the first birth-order birth rates is positive and statistically significantly different from zero at the 0.1% significance level. The estimate implies that a 10% increase in the cash transfers for the first child increases

[36] In the appendix, I report the results estimating a naive specification without any fixed effects and control variables and gradually introducing them in Table A.2.

[37] These average values are 0.34 for a first child, 0.93 for a second child, and 2.66 for a third child, which translate into values of adjustment factors equal to 0.3189, 0.6826, and 0.9362, respectively. Alternatively, Bellemare and Wichman (2020) propose multiplying a large constant to a variable before applying the inverse hyperbolic sine transformation. The implied elasticities based on their method (for example, multiplying CT_p by 10,000) are very close to the elasticities I obtain from rescaling the estimated coefficients by the adjustment factors.

the birth rate of the first child by 0.58% after applying the adjustment factor as in equation 9. Should parents be forward-looking and base their decision to have their first child on the cash incentives offered for higher birth orders, the birth rates for the first births would be affected by the generosity of the baby bonus provided to second and third children. To test whether this is the case, I additionally introduce cash transfers for second and third children to the estimation. In Column 2, the coefficient estimates for the baby bonus awarded to families having their first child does not change in a meaningful way, whereas both of the estimated effects of the cash transfers for higher-order births are not statistically different from zero at the 5% significance level.

The results for the second child, reported in Columns 3 and 4, also show that the cash transfers increased birth rates. According the estimate in Column 3, a 10% increase in the cash transfers for the second child raised the birth rate for second children by 0.34%. This estimate is smaller than the elasticity of birth rates for the first-child birth with respect to cash transfers, implying that it requires greater financial incentives to encourage families to have the second child. In Column 4, I additionally introduce the cash transfers provided for the first and third children. The coefficient estimate for cash transfers for the second child is robust to these additional control variables and changes little from Column 3. None of the coefficient estimates of the cash transfers for the other birth orders are statistically different from zero at the 5% significance level. This result is in line with the intuition that the cash transfers provided for the first child should not matter for families who already have a child. The cash transfers for the third child did not influence the families to alter their decisions about having a second child.

The estimated coefficient in Column 5 implies that the third-child birth rates increased by 0.36% as the cash transfers for that birth rate rose by 10%. Similarly to the case of second birth, the baby bonus provided to families with first and second children should not affect whether families already with two children decide to have another baby. In line with this intuition, the effects of cash transfers for the first and second children are not statistically different from zero.[38]

Figure 4 plots the elasticities of age-specific birth rates with respect to cash transfers. The effects of cash transfers are statistically equivalent to zero for all birth orders among adolescents and those older than 40. Cash transfers affect the birth rates among the women who are most likely active in their childbearing decisions. The cash-transfer elasticity of birth rates for the first child is positive across younger and older mothers who are still in their prime age for childbearing and peaks for those aged 30–35. For the higher-order births, the elasticity with respect to cash-transfer generosity is higher among younger mothers (20–35) for the second child and among older mothers (25–39) for the third child.

Overall, the results demonstrate that cash transfers increase the birth rates across birth orders. These effects were specific to birth order. In other words, the cash transfers did not *inframarginally*

[38]The results are robust when using the birth rates and the cash transfers in levels. In the appendix, Table A.3 reproduces the results reported in Table 4 without undertaking any transformation. Based on levels, the implied benefit elasticities are close to the elasticities estimated using the log transformed values of birth rates. Evaluating at the 2015 average birth rates for each birth order, a 10% increase in the cash transfers increased the birth rate by 0.3%, 0.3%, and 0.2% for first, second, and third children, respectively.

affect the fertility decisions but instead led to an increase in total fertility by encouraging families at the margin of having an extra child to choose to have a baby. The implied benefit elasticities ranging between 0.03 and 0.06 are, though relatively small, within the range of the benefit elasticities of fertility with respect to various forms of financial incentives in other developed countries (Zhang et al., 1994; Gauthier and Hatzius, 1997; Milligan, 2005; Cohen et al., 2013; Malak et al., 2019).

A back-of-the-envelope calculation under the assumption that everything else is held constant finds that in the absence of these cash transfers, the total fertility rate in 2015 would have been reduced by 4.7%, which corresponds to approximately 562,439 fewer children ever born to the female population in 2015 over their life cycle. This estimate falls within the 3% to 5% range for increase in fertility estimated using the 2007 introduction of a universal child benefit in Spain (González, 2013; González and Trommlerová, 2021). Although I find that the pro-natalist cash transfers were effective in raising the birth rate, the implied amount of cash transfers to raise the total fertility rate back to the 2.1 replacement level is exorbitant.[39]

4.2 Sex Ratio at Birth and Infant Health

Event-Study Results.

Figure 5 reports the event-study results estimating equation 6 using an indicator for a boy, birth weight, and gestational age as the dependent variable.[40] In the top panel, the changes in the probability of an infant being a boy before and after the policy implementation are plotted along with the 95% confidence intervals. Before the policy implementation, none of the estimated event-study coefficients were statistically different from zero at the 5% significance level. After the policy implementation, the fraction started to decline. On the flip side, the fraction of girls increased after the local governments started offering a baby bonus. The average decline after the policy implementation is about 0.3 percentage points, statistically significantly different from zero at the 5% significance level. The estimated event-study coefficients for birth weight are plotted in the middle panel. Prior to the reform, birth weight on average did not change much except one year before the policy implementation, when birth weight increased. However, birth weight decreased after the policy implementation by about 0.1%, statistically significantly different from zero at the 5% significance level. In the bottom panel, the decrease in gestational age during the post-policy period is more apparent, while the magnitude of the decline is similar to that of birth weight.

Below, I present the results estimating the elasticity of each outcome of interest with respect to cash-transfer generosity by exploiting the variation in cash-transfer generosity based on equation 7.[41]

[39] 39The observed total fertility rate in 2015 was 1.33 children per woman, about 58% of the 2.1 replacement level. Assuming no other changes in the economy, the cash transfers would have to increase by about 10 times (or KRW 3.4 million) from KRW 340,000 for first children, about 17 times (or KRW 15.7 million) from KRW 930,000 for second children, and about 16 times (or KRW 41.75 million) from KRW 2.7 million for third children. The median annual household income (family size: four) based on the 2015 Household Expenditure Survey was KRW 42 million.

[40] In the appendix, Figure A.9 extends the event window to $[-7, 7]$.

[41] Throughout my analysis, I include the rich set of fixed effects and district-level time-varying characteristics to purge the differences in the outcome variables arising because of regional differences as well as including birth

Elasticity of Sex Ratio at Birth with Respect to Cash-Transfer Generosity.

The first two columns in Table 5 summarize the results estimating the baby bonus's effect on the probability of an infant being a boy. During the sample period of 2001 to 2015, the average fraction of boys among first children is 0.51, which is equivalent to the sex ratio of 105.4 boys per 100 girls. Across the columns, the estimated coefficients of the dummy variables for second- and third-order births are positive and statistically significantly different from zero at least at the 1% significance level. There were significantly more boys than girls among the third children relative to the natural sex ratio at birth in the beginning of the sample period: 125 boys per 100 girls.

The estimated effect of the cash transfers in Column 1 is negative and statistically significant. Restricting this effect to be uniform across birth orders, doubling the cash-transfer amount decreases the probability of a male birth by 1.61 percentage points. The sex ratios at birth among the first births have been steadily at the natural sex ratio. The result implies that the cash transfer may have resulted in more baby girls being born below the natural sex ratio at birth among the first births. I explore how the effect is heterogeneous by birth order and allow the cash-transfer effect to vary by birth orders in Column 2. I find that the cash transfer did not affect the sex ratio at birth among the first births and had a negative but economically negligible effect on the sex ratio among the second births. Among the third births, the effect of cash transfers on the sex ratio is negative and statistically different from zero at the 0.1% significance level.[42] Doubling the baby bonus provided for a third child would reduce the sex ratio by 2.3 percentage points.

Without the baby bonus and holding everything else constant, the sex ratio among the third children born in 2015 would have been 124.7 boys per 100 girls. The difference between this counterfactual sex ratio at birth in 2015 and the observed sex ratio at birth in 2000 (142.1 boys per 100 girls) can be attributed to macro-level forces other than the cash transfers—for example, changes in social norms (Chung and Gupta, 2007) and a reduced reliance on sons because of increased old-age pensions (Ebenstein, 2014). The difference between the counterfactual sex ratio without the financial incentives and the observed sex ratio at birth in 2015 (105.3 boys per 100 girls) can be attributed to the baby bonus's effect on the sex ratio. The baby bonus explains about 53% of the decline in the sex ratio at birth since 2000.[43]

order of a child and parental characteristics (mother's and father's age, educational attainment, occupation [including unemployment], and marital status) to level the baseline differences based on birth order and parental background. In the appendix, I report the estimation results without any fixed effects and control variables and gradually introduce the district fixed effects, city-by-year fixed effects, district-level control variables, and indicators for parental characteristics in Table A.4 for the probability of a baby being a boy, Table A.5 for birth weight, and Table A.6 for gestational age.

[42] Figure A.10 in the appendix plots the changes in the probability of a baby being a boy before and after adoption of the baby bonus by birth parity. The event-study results also indicate no change among first births, an economically insignificant decrease among second births, and a statistically significant decline among third births.

[43] For second births, the counterfactual sex ratio in 2015 without cash transfers is 105.51. The cash transfers explain 42% of the decline from 107.47 in 2010 to 104.08 in 2015.

Elasticities of Birth Weight and Gestational Age with Respect to Cash-Transfer Generosity.

Column 3 in Table 5 reports the results estimating equation 7 for birth weight as the dependent variable. An increase in the amount of the baby bonus led to a decrease in birth weight; the estimate of -0.0016 is statistically different from zero at the 0.1% significance level. In Column 4, I allow the effect of cash transfers on birth weight to vary across birth orders. The negative effect estimated in Column 1 is solely driven by the decrease in birth weight among the third births. The estimated effects for the first and second children are not statistically significantly different from zero, while the estimated effect for the third child is statistically different from zero at the 0.1% significant level. The coefficient estimate implies that doubling the cash transfers for third children would result in decreasing birth weight by 0.22% (or 7.2 grams) after applying the adjustment factor. Because the average birth weight of a third child is higher than that of lower-order births, the baby bonus may not have made the third birth worse off if compared to the first and second births.[44]

Still, the negative effect on birth weight is surprising. The literature estimating the effect of income on birth weight has shown a positive effect. For instance, Hoynes et al. (2015) find that an increase in income of $1,000 (in 2009) via the Earned Income Tax Credit in the U.S. is associated with an increase in birth weight of 6.4 grams overall (2.8 grams for non-Hispanic white mothers). On the one hand, since the cash- transfer programs in South Korea are not income-tested, the comparison is misleading. On the other hand, it hints that the effect of extra income may be heterogeneous by subgroups of population, who are responding to the policy, and that there may be negative selection into childbearing along the lines of the parental characteristics that affect infant health. Another possible explanation is that the cash transfers affected other key determinants of birth weight. One such factor is gestational age; the tight relationship between birth weight and gestation age is physiological and well documented.

The estimated effects of baby bonus on gestational age are reported in Table 5 (Columns 5 and 6). The estimate in Column 5 is negative and statistically different from zero. When this effect is allowed to differ by birth order in Column 6, I find that the estimated effect in Column 5 is driven by the changes in gestational age with respect to the cash transfers among the second and third births but not the first. Doubling the cash-transfer generosity would result in decreasing gestational age by 0.1% for the second child and 0.2% for the third. The average gestational age for these children is lower than that for first children. I investigate whether the reduction in gestational age is associated with more preterm births in Table A.7 (Columns 5–6). The results indicate that doubling the baby bonus leads to a 5% to 6% increase in the incidence of preterm births among second and third children. The results suggest that the cash-transfer effect on gestational age and incidence of preterm birth partly explains the negative effect on birth weight.[45]

[44]In the appendix, I explore whether the cash transfers had any impact on the incidences of low birth weight (less than 2500 grams; Columns 1–2) and macrosomia (birth weight greater than 4,000 grams; Columns 3–4) in Table A.7. The cash transfers increased the incidence of low birth weight among first and second children but not among third children, while the incidence of macrosomia decreased among third children.

[45]Table A.8 replicates Columns 3–6 while controlling for the gender of the child. The results suggest that the

I now investigate whether the baby bonus affected early-life mortality. In Table 6, I summarize the results estimating the effects of the pro-natalist cash transfers on one- and five-year mortality, focusing on the birth cohorts born between 2010 and 2013 whose death records were matched with their birth records. In Column 1, the estimated effect of the cash-transfer policy on infant mortality is positive but not statistically significantly different from zero. In Column 2, in which I allow the effect to differ by birth order, none of the estimated coefficients are statistically significant. Similarly, the results for five-year mortality reported in Columns 3–4 suggest that the cash transfers did not affect mortality in the long term. Overall, while a greater amount of cash transfers was associated with lower newborn health (birth weight and gestational age), the baby bonus did not increase early-life mortality. Early-life mortality is arguably an extreme measure of infant health, especially in this context, in which infant mortality is already low. The results in this paper call for future research on the baby bonus's effects on educational and labor market outcomes.

5 Mechanisms

The baby bonus increased birth rates, decreased birth weight and gestational age, and modulated son preference. In this section, I discuss potential mechanisms explaining these results.

5.1 Spatial Redistribution of Fertility: Migration

Kim (2023) studies migration and commuting decisions in South Korea and shows people choose to live in districts where there are greater local-government expenditures, which include the baby bonus. Using the universe of resident registration records, I test whether potential beneficiary households moved across districts in response to yearly changes in the pro-natalist cash transfers. A record is generated each time a household declares its residency.[46] Each record includes information on the prior and current place of residency, the date of registration, and the demographic characteristics (sex, age, and indicator for household head) of every member of the household. Based on the family composition, I identify households that are potential beneficiaries of pro-natalist cash transfers.[47] I then construct a panel data set of district pairs (origin and destination) from 2001 to 2015 with the number of families that are potential beneficiaries to receive cash transfers for a first, second, and third child and moved from one district to another. I estimate the following gravity equation:

$$\ln F_{p,o,d,y} = \phi_{o,d} + \phi_{o,y} + \phi_{c(d),y} + \delta X_{d,y} + \kappa_p \sinh^{-1} CT_{p,d,y} + \xi_{p,o,d,y}, \tag{10}$$

changes in the sex ratio at birth do not explain the strong negative effects of the baby bonus on birth weight and gestational age.

[46]The Resident Registration Law (1962) requires households to register with their new district of residency within 14 days of moving.

[47]For instance, a household is identified as a potential beneficiary of pro-natalist cash transfers for a third child if it has four members, if there are two adults between the ages of 20 and 39 with an age difference less than or equal to 15 years, and if there are two children less than 19 years old and the older children are at least 15 years younger than the youngest parent.

where $F_{p,o,d,y}$ is the number of potential beneficiaries for pro-natalist cash transfers for the p-th birth order who moved from origin-district o to destination-district d in year y; district-pair fixed effects $\phi_{o,d}$ capture the time-invariant factors that vary at the bilateral level, such as distance and similarity of cultural norms; origin fixed effects $\phi_{o,y}$ capture all time-varying characteristics of each origin district including the pro-natalist cash transfers and the city-level factors; destination-city fixed effects $\phi_{c(d),y}$ capture all citywide characteristics at each destination district (for example, housing and labor market conditions); $X_{d,y}$ is a set of district-level time varying characteristics. A positive value of κ_p implies that potential beneficiaries of pro-natalist cash transfers moved to places offering relatively higher cash transfers.

Table 7 reports the estimation estimated values of κ_p for first, second, and third birth orders in Columns 1, 2, and 3, respectively. In Column 1, the estimated coefficient is positive but small and not statistically different from zero. This implies that the generosity of the cash transfers provided to families for their first child does not systematically explain the migration decisions of people who may benefit from these transfers. In Column 2, the coefficient estimate becomes slightly larger but is still not statistically significant. The estimated effect of pro-natalist cash transfers in Column 3 is positive and statistically significant at the 1% level for the third child. Interpreting this estimate in terms of elasticity by applying the adjustment factor for the third card as explained in equation 9, a 10% increase in cash transfers for the third child increases the probability that households that are potential beneficiaries will migrate to a new district by 0.2%. Notwithstanding the statistical significance, the effect is small. In Column 4, I test whether there is a systematic correlation between the migration patterns among non-beneficiaries and cash-transfer generosity. The result shows there is not.

Are the positive effects of cash transfers on birth rates partly, if not entirely, driven by the spatial sorting of families across districts, instead of changes in the number of children being born? The results presented in Table 4 are estimated while holding the number of potential beneficiaries constant as explained in Section 3.1. Therefore, the estimated coefficients exclude the effects of cash transfers on birth rates through changes in the stock of potential beneficiaries. I replicate the main results in Table 4 while excluding adult population and net migration rate from the set of control variables, thereby loading the positive effect of migratory responses on birth rates onto the coefficients. Table A.9 in the appendix reports the results in Columns 1, 2, and 4, respectively; Columns 1, 3, and 5 show the same results as in Table 4. Comparing the estimates across columns for each birth order, the estimates in the right columns are larger for the second and third children, in line with the gravity estimation results in Table 7. However, for all birth orders, the estimates are not statistically significantly different from each other; this makes sense, given the small size of the estimated migratory effects.

5.2 Temporal Adjustment of Fertility

The positive effect of the cash transfers on birth rates may simply reflect changes in the timing of fertility (tempo effect), not the total number of children ever born by women (Andersen et al., 2018).

A possible explanation for cash transfers influencing fertility time is that potential beneficiaries may believe the cash transfers will decrease in amount or get repealed if they do not act quickly. However, the overall generosity of these cash transfers did not decrease, as shown in Figure 1. The event-study results in Figures 2 and A.4 showed that the increases in the birth rates across birth orders were sustained for over five and seven years after policy implementation. Furthermore, my results in Figure 3 serve as additional evidence ruling out this tempo effect. While the effect on first children is positive and statistically significant across mothers in their active age of childbearing, the effects on second children for the middle, and third children are positive among older mothers. I formally investigate the changes in mother's age and marriage duration at the time of childbirth. In Table 8, Columns 1 and 2 report the results estimating equation 7 using mother's age and years married as dependent variables while allowing the cash-transfer effect to vary by birth order. The cash transfers had no impact on when mothers have their first children and seem to have lengthened, if anything, the average marital duration until having a first child. The small yet statistically significant increase (at the 1% significance level) in mother's age when having a second child is likely driven by mothers' choice to have their second child with the assistance of cash transfers, when they would otherwise have stopped bearing children after their first child.[48] Despite this small increase in mother's age for second child, the childbearing timing measured in terms of marriage duration was not affected by the baby bonus for any birth order. All this evidence suggests that the fertility effect I identified is not a mere reflection of temporal adjustment of childbearing but corresponds to additional births per woman.

5.3 Composition Changes and Heterogeneity Analysis

The effects of pro-natalist cash transfers on the sex ratio at birth and infant health may be heterogeneous across characteristics of families, which are responding to the policy. The main specification (equation 7) includes a set of fixed effects for the observed parental characteristics, notably mother's and father's age, educational attainment level, and occupation categories (including unemployment/not working). While this strategy accounts for the baseline differences in the outcome variables across parental characteristics, the estimated effect is aggregated and may mask heterogeneity. Further, the extent to which each characteristic contributes to the aggregated effect depends on its composition change. Thus, to understand the main results and their policy implications, it is important to study changes in the composition of parents due to the baby bonus and its heterogeneity.

Changes in Composition. In Table 8, I report the estimated changes in parental characteristics. As mentioned earlier and reported in Columns 1 and 2, the composition of mothers' age and childbearing timing relative to marriage year did not change with the cash transfers in a meaningful way. Regarding educational attainment (Columns 3 and 4), when controlling for parental age, employ-

[48] Doubling the baby bonus generosity increased the average mother's age among the second children only by about 2.3 months and the marriage duration until the first child by about seven days.

ment status, and marital status, the cash transfers increased the fraction of parents with a college degree for all birth orders.[49] While the policy was publicized and had a simple structure, levels of awareness and understanding of it may differ by educational attainment. Columns 5 and 6 report the results for parental employment status. Holding the other parental characteristics constant (age, educational attainment, marital status, husband's employment status), the cash transfers decreased the probability that a mother is employed for all birth orders but especially for the third child. The opportunity costs are lower for mothers, who are detached from the labor market.[50] At the same time, for the second and third children, the share of working fathers also decreased in cash transfers. Doubling the cash-transfer generosity is associated with 0.18 and 0.16 percentage point decreases in that share for the second and third children, respectively. Households with unemployed fathers—and thus with lower income—may have found a generous baby bonus attractive and responded to the policy.[51]

Heterogeneity. To investigate heterogeneous effects of pro-natalist cash transfers, I estimate equation 7 by pooling samples separately by mother's age, parents' educational attainment, and their employment status.[52] Figure 6 summarizes the results for sex ratio at birth (left panels) birth weight (middle panels), and gestational age (right panels).

The top panels plot the estimated effects of cash transfers by mother's age group. First, the cash transfers' effect on the probability of a baby being a boy is statistically indistinguishable from zero for the first birth order across all age groups. The estimated effects are negative and statistically significantly different from zero for higher-order births only among older mothers. Similarly, the negative effect of cash transfers on birth weight is driven by older mothers. Second, the cash transfers did not affect the birth weight of the children with mothers aged 20 to 24, irrespective of birth order. The cash transfers had a statistically significant negative effect for mothers aged 30 to 34 for the first birth order, 25 to 34 for the second, and 25 to 39 for the third. Third, the cash transfers' effects on gestational age are small for the first birth order and not statistically significant. Third, the cash transfers' effects on gestational age are small for the first birth order and not statistically significant. The effects are negative and statistically significantly different from zero among younger mothers (aged 20–34) for the second birth order across all age groups. For the third birth order, the estimated effects suggest that the effects are consistently negative, statistically significantly different

[49] However, in the absence of a baby bonus, the fraction of mothers with a college degree was 5% and 13% lower for second and third children, respectively, compared to the mothers of first children. Considering their average generosity, the cash transfers for second and third children did not level the difference in educational attainment among mothers with different numbers of children.

[50] The share of working mothers based on the birth-registry data is noteworthy. It was only 31.8% for mothers having their first child and decreased by 9.3 percentage points and 12.5 percentage points for those having second and third children. In contrast, the share of working fathers was 95.5% for first children and grew for families with two or more children.

[51] The birth records do not include information on family income. Low female employment in South Korea, especially among mothers, implies that father's employment is likely the main income source.

[52] Throughout the heterogeneity analysis, the estimation is done by stratifying samples based on observable, predetermined parental characteristics. When doing so, I still control for all the parental characteristics other than the one that stratifies the data. I compare the estimated effects of cash transfers across parental characteristics based on the point estimates and their confidence intervals.

from zero, and not heterogeneous across the age groups, ranging narrowly from −0.0027 to −0.0017.

In the middle panels, I report the results by whether parental completed education attainment is some college or above. The negative effect on the probability of a baby being a boy is driven particularly by parents with a college degree across birth orders. It is the third-order births that had negative effects for both educational attainment levels. Next, none of the estimated coefficients for birth weight and gestational age for first children are statistically significantly different from zero. The estimated effects on birth weight among mothers without a college degree are −0.0017 for second births and −0.0033 for third, and they are statistically significant at least at the 5% significance level. The magnitude of the effects is much smaller among college-graduate mothers for both second and third children than among mothers without a college degree; the effect on birth weight becomes statistically indistinguishable from zero for second children. For third children, the estimated effect of −0.0014 on birth weight among mothers with a college degree is different from the estimate among mothers without a college degree when their confidence intervals are compared. Similarly, the results for gestational age suggest that the effects are larger for second and third children for mothers without a college degree, compared to those with a college degree. There is no meaningful difference detected when comparing the effects between father's educational attainment levels for either birth weight or gestational age.

Last, the results by parental employment status are summarized in the bottom panels. For both mothers and fathers, the effects of cash transfers on the probability of a baby being a boy are negative for second births. The magnitude of the negative effects is much larger for third births. The effects on birth weight are similar across parental employment status for all birth orders. The effects for third births are consistently negative within a relatively tight range between −0.0032 (employed father) and −0.0021 (nonworking mother). For gestational age, the effects by father's employment status differ in a statistically significant way, as the estimated effect is considerably larger if a father is unemployed than if employed.

Discussion. The pro-natalist cash transfers had heterogeneous effects on the sex ratio at birth and newborn health by mother's age, while there was no composition change in mother's age. The statistically significant negative effects on the sex ratio at birth and birth weight for third births are explained partly by older mothers. The statistically insignificant estimates for the sex ratio at birth, birth weight, and gestational age for first and second births in the main results mask the heterogeneous effects by mother's age.

Both the composition change and heterogeneous effects by parental educational attainment and employment status explain the main results. An increase in cash-transfer generosity led to a higher fraction of parents with a college degree but lowered the probability of mothers and fathers being employed. The policy effects on the sex ratio at birth for second births are negative only among parents with a college degree, while they are statistically significant for third births irrespective of parental educational attainment. The effects on birth weight and gestational age for second and third births differ between mothers with and without a college degree but not between fathers with and without a college degree. These estimates reflect other unobservable factors associated with mother's educational attainment that contribute to infant health (for example, prenatal care, health status, and lifestyle). Although not possible to investigate further because of data limitations, the stronger negative effects of cash transfers on birth weight and gestational age among mothers without a college degree suggest that these mothers may have been more negatively selected in these unobserved factors compared to mothers with a college degree.

The effect on gestational age is more negative for third children with unemployed fathers than for third children with working fathers. The same pattern is not detected for mothers who work versus mothers who do not. Father's unemployment generates a large negative income shock to a family.[53] The composition and heterogeneity results imply that families who responded to the cash transfers and had a third child were negatively selected in terms of income and that this negative selection was stronger among families with a nonworking or unemployed father.

In contrast, the effect on birth weight did not differ by father's employment status. To proceed, I make a few plausible assumptions: there is a strong positive physiological relationship between birth weight and gestational age, and this correlation does not differ by father's employment status. Under these assumptions, the direct effect of the baby bonus on birth weight, if its effect through gestational age were controlled for, must be heterogeneous. The pro-natalist cash transfers mitigate the decrease in birth weight due to lower gestational age for families with unemployed fathers (or low-income families).[54]

5.4 Son Preference

Why did the probability of a third child being a boy decrease with the pro-natalist cash transfers? There are many possible explanations. For instance, in South Korea, where son preference is strong, the cash transfers may have compensated for the utility penalty associated with having a girl, and families thus kept girls whom they would not have aborted in the absence of the baby bonus. Another likely channel is that son preference is heterogeneous and that the families responding to the pro-natalist polices were indifferent to infants' sex or even preferred daughters to boys. I cannot test these conjectures with the birth-registry records because I cannot link the records by mothers (or fathers) to observe their full childbearing history and observe the children's gender composition within a family.

Instead, I use the 2015 Population Census of South Korea to shed light on how the baby bonus interacted with son preference. The census surveys about 20% of all households. It records information on age, gender, relation to the household head, and birth district of each member in a household. Based on the census data, I construct a household-level data set in which each household

[53] Given the wide gender wage gap and low labor force participation rate of mothers in South Korea, the extent of the income shock is plausibly larger, compared to countries with a smaller gender wage gap and higher labor force participation of mothers.

[54] I cannot determine the sign of the direct effect of the baby bonus on birth weight. However, this paper provides suggestive evidence that the direct effect is positive, especially for families who benefit more from the financial assistance.

has at least one and at most three children and the youngest child was born after 2001. For all children born after 2001, the size of the cash transfer they received is determined by their age, birth order, and birth district. Then, I estimate the effect of the cash transfers on the probability of having a boy separately by children's gender composition within a family while controlling for parental characteristics (age, educational attainment, employment status, and marital status), district fixed effects, and city-by-year fixed effects.[55]

The estimation results are summarized in Table 9. In Column 1, although it is positive and large, the estimated effect of the cash transfers on the probability of a first child being a boy among all families is not statistically significantly different from zero. In Columns 2 to 4, I focus on households with at least two children. Pooling all households, the estimated effect of cash transfers on the probability of a second child being a boy is statistically indistinguishable from zero and close to zero in magnitude. I separately estimate the effect by first child's sex in Columns 3 and 4.

In Columns 5 to 8, I focus on households with three children. In Column 5, the estimated effect on the probability of a third child being a boy is –0.0256, statistically significant at the 5% level. This point estimate is remarkably close to –0.0246, the effect on the probability of a third birth being a boy when estimated using the birth-registry records, as reported in Table 5. Restricting the sample to households whose first and second children are both boys (Column 6) substantially increases the estimated effect of cash transfers to –0.0892, which is statistically significant at the 1% level. These households are likely to have had weaker son preference because they already had two boys.

The strong negative effect of the cash transfers suggests two potential explanations: first, the baby bonus compensated for the utility penalty for having and keeping a girl whom they would have aborted without the baby bonus; second, some of these families may have preferred a daughter to a son and were choosing to have a third child for a daughter. For households with a boy and a girl (irrespective of birth order) in Column 7, the estimated effect on the probability of a third child being a boy is –0.0440, statistically significant at the 5% level. The average probability of a third child being a boy for families with at least one boy before having the third child was already at the natural sex ratio or slightly below. A further reduction in the sex ratio with the cash transfers implies there were more families having a third child with the intention to keep it if it was a girl.

Last, the policy effect is reserved and turns positive to 0.0414 among households with two daughters (statistically significant only at the 10% level). For these families, the baby bonus does not seem to have offset the utility penalty associated with having a girl. The extent of the penalty must have been highest among families with two daughters when they planned to have a third child. The average share of third children who were boys among these families was 0.617, or 161 boys per 100 girls. Cash-transfer generosity did not affect this extremely skewed sex ratio among families with strong son preference.

[55] The estimating equation here follows the main individual-level specification (equation 7) as closely as possible.

6 Conclusion

In this paper, I studied the causal effects of pro-natalist cash transfers provided to families with newborn babies on birth rates, sex ratio at birth, and infant health. I combined rich temporal and spatial variation in the implementation timing and generosity of cash transfers for each birth order with confidential birth-registry records to identify causal estimates. The baby bonus increased birth rates across birth orders. In the absence of cash transfers, the total fertility rate in 2015 would have fallen by 4.7%. I provided evidence that these changes in birth rates are a result of increased completed fertility, not a temporary increase in the number of births due to temporal adjustments in childbearing decisions or migratory responses of families making fertility decisions.

In addition to affecting the number of births, these cash transfers had unintended consequences for the sex ratio at birth and infant health. The effects are heterogeneous across parental characteristics and led to composition changes. The cash transfers enabled parents who were unemployed and had lower educational attainment to have a baby. These households are likely low-income families. The results suggest that this negative selection may explain the negative effects of cash transfers on infant health. Furthermore, I provided suggestive evidence that the effect of cash transfers on birth weight, if the changes in gestational age were controlled for, may have been positive, especially among low-income families. Last, I found that the cash transfers contributed to the shift of the male-skewed sex ratio at birth toward its natural level.

The global total fertility rate has been declining and approaching the 2.1 replacement level. Many developed countries share a concern about low fertility rates, and the issue will likely be critical in today's developing countries with high fertility rates that will rapidly decline in the near future. This paper provides insights about the effects of pro-natalist cash transfers (the baby bonus) to inform policy makers: these transfers increase completed fertility but might not be sufficient to push total fertility rates back up to the replacement level. In addition, the transfers may have unintended consequences—for instance, modulating the sex ratio at birth and influencing infant health by inducing negative selection into childbearing. Further research is required to better understand the interactive effects of different policy options (for example, cash transfers and parental leave) and the long-term implications of such policies for the outcomes of the children born as a result.

References

Aizer, A. and J. J. Doyle (2015). Juvenile incarceration, human capital, and future crime: Evidence from randomly assigned judges. *The Quarterly Journal of Economics 130*(2), 759–803.

Almond, D., K. Y. Chay, and D. S. Lee (2005). The costs of low birth weight. *The Quarterly Journal of Economics 120*(3), 1031–1083.

Andersen, S., N. Drange, and T. Lappegård (2018). Can a cash transfer to families change fertility behaviour? *Demographic Research 38*, 897–928.

Andersson, G. and A.-Z. Duvander (2006). Gender equality and fertility in Sweden. *Marriage & Family Review 39*, 121–142.

Anukriti, S. (2018). Financial incentives and the fertility-sex ratio trade-off. *American Economic Journal: Applied Economics 10*(2), 27–57.

Anukriti, S. and T. J. Kumler (2019, apr). Women's worth: Trade, female income, and fertility in India. *Economic Development and Cultural Change 67*(3), 687–724.

Baker, M. and K. Milligan (2010). Evidence from maternity leave expansions of the impact of maternal care on early child development. *Journal of Human Resources 45*(1), 1–32.

Behrman, J. R. and M. R. Rosenzweig (2004). Returns to birthweight. *Review of Economics and Statistics 86*(2), 586–601.

Bellemare, M. F. and C. J. Wichman (2020). Elasticities and the inverse hyperbolic sine transformation. *Oxford Bulletin of Economics and Statistics 82*(1), 50–61.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2005). The more the merrier? The effect of family size and birth order on children's education. *The Quarterly Journal of Economics 120*(2).

Black, S. E., P. J. Devereux, and K. G. Salvanes (2007). From the cradle to the labor market? The effect of birth weight on adult outcomes. *The Quarterly Journal of Economics 122*(1), 409–439.

Boccuzzo, G., M. D. Zuanna, G. Caltabiano, and M. Loghi (2008). The impact of the bonus at bith on reproductive behaviour in a lowest-low fertility context: Friuli-Venezia Giula (Italy). *Vienna Yearbook of Population Research 6*, 125–147.

Bodnar, L. M. and H. N. Simhan (2008). The prevalence of preterm birth and season of conception. *Paediatric and Perinatal Epidemiology 22*(6), 538–545.

Boland, M. R., Z. Shahn, D. Madigan, G. Hripcsak, and N. P. Tatonetti (2015). Birth month affects lifetime disease risk: A phenome-wide method. *Journal of the American Medical Informatics Association 22*(5), 1042–1053.

Bongaarts, J. (2013). The implementation of preferences for male offspring. *Population and Development Review 39*(2), 185–208.

Borusyak, K., X. Jaravel, and J. Spiess (2021). Revisiting event study designs: Robust and efficient estimation.

Callaway, B. and P. H. Sant'Anna (2021). Difference-in-differences with multiple time periods. *Journal of Econometrics 225*(2), 200–230.

Carneiro, P., K. V. Løken, and K. G. Salvanes (2015, apr). A flying start? maternity leave benefits and long-run outcomes of children. *Journal of Political Economy 123*(2), 365–412.

Case, A., D. Lubotsky, and C. Paxson (2002). Economic status and health in childhood: The origins of the gradient. *American Economic Review 92*(5), 1308–1334.

Choi, E. J. and J. Hwang (2020). Transition of son preference: Evidence from south korea. *Demography 57*(2), 627–652.

Chung, W. and M. D. Gupta (2007). The decline of son preference in South Korea: The roles of development and public policy. *Population and Development Review 33*(4), 757–783.

Cohen, A., R. Dehejia, and D. Romanov (2013). Financial incentives and fertility. *The Review of Economics and Statistics 95*(1), 1–20.

Crump, R., G. S. Goda, and K. J. Mumford (2011). Fertility and the personal exemption. *The American Economic Review 101*(4), 1616–1628.

Dahl, G. B., K. V. Løken, M. Mogstad, and K. V. Salvanes (2016, oct). What is the case for paid maternity leave? *Review of Economics and Statistics 98*(4), 655–670.

Darrow, L. A., M. J. Strickland, M. Klein, L. A. Waller, W. D. Flanders, A. Correa, M. Marcus, and P. E. Tolbert (2009). Seasonality of birth and implications for temporal studies of preterm birth. *Epidemiology 20*(5), 699–706.

de Chaisemartin, C. and X. D'Haultfœuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–2996.

Dustmann, C. and U. Schönberg (2012, jul). Expansions in maternity leave coverage and children's long-term outcomes. *American Economic Journal: Applied Economics 4*(3), 190–224.

Ebenstein, A. (2010). The "missing girls" of China and the unintended consequences of the one child policy. *Journal of Human Resources 45*(1), 87–115.

Ebenstein, A. (2014). Patrilocality and missing women.

Fleckenstein, T. and S. C. Lee (2012). The politics of postindustrial social policy: Family policy reforms in Britain, Germany, South Korea, and Sweden. *Comparative Political Studies 47*(4), 601–630.

Frejka, T., G. W. Jones, and J.-P. Sardon (2010). East Asian childbearing patterns and policy developments. *Population and Development Review 36*(3), 579–606.

Gauthier, A. (2007). The impacts of family policies on fertility in industrialized countries: A review of the literature. *Population Research Policy Review 26*(3), 323–346.

Gauthier, A. H. and J. Hatzius (1997, nov). Family benefits and fertility: An econometric analysis. *Population Studies 51*(3), 295–306.

González, L. (2013). The effect of a universal child benefit on conceptions, abortions, and early maternal labor supply. *American Economic Journal: Economic Policy 5*(3), 160–188.

González, L. (2018, may). Sex selection and health at birth among indian immigrants. *Economics & Human Biology 29*, 64–75.

González, L. and S. K. Trommlerová (2021). Cash transfers and fertility. *Journal of Human Resources 58*(3), 783–818.

Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. *NBER Working Paper 25018*.

Harper, S. (2014). Economic and social implications of aging societies. *Science 346*(6209), 587–591.

Hart, R. K., J. Bergsvik, A. Fauske, and W. Kim (2024). *Handbook of Labor, Human Resources and Population Economics.*, Chapter Causal Analysis of Policy Effects on Fertility.

Hong, S. C., Y.-I. Kim, J.-Y. Lim, and M.-Y. Yeo (2016). Pro-natalist cash grants and fertility: A panel analysis. *The Korean Economic Review*.

Hotz, V. J., J. A. Klerman, and R. J. Willis (1997). Chapter 7 the economics of fertility in developed countries. In *Handbook of Population and Family Economics*, pp. 275–347. Elsevier.

Hoynes, H., D. Miller, and D. Simon (2015). Income, the earned income tax credit, and infant health. *American Economic Journal: Economic Policy 7*(1), 172–211.

Hoynes, H. W. (1997). *Fiscal Policy: Lessons from Economic Research*, Chapter Work, Welfare, and Family Structure: What Have we Learned? Cambridge, Mass.: MIT Press.

Jacobsen, R., H. Møller, and A. Mouritsen (1999). Natural variation in the human sex ratio. *Human Reproduction 14*(12), 3120–3125.

Jayachandran, S. (2017). Fertility decline and missing women. *American Economic Journal: Applied Economics 9*(1), 118–139.

Kearney, M. S. (2004). Is there an effect of incremental welfare benefits on fertility behavior? *Journal of Human Resources XXXIX*(2), 295–325.

Kim, D.-R. (2013). Local government policy diffusion in a decentralised system: Childbirth support policy in South Korea. *Local Government Studies 39*(4), 582–599.

Kim, K., S.-H. Lee, and T. J. Halliday (2022, sep). Paid childcare leave, fertility, and female labor supply in South Korea. *Review of Economics of the Household*.

Kim, W. (2023). Migration, commuting, and the spatial distribution of public spending.

Lalive, R. and J. Zweimüller (2009). How does parental leave affect fertility and return to work? *The Quarterly Journal of Economics 124*(3), 1363–1402.

Laroque, G. and B. Salanié (2004). Fertility and financial incentives in France. *CESifo Economic Studies 50*(3), 423–450.

Lee, S. and H. Choi (2015). Lowest-low fertility and policy responses in South Korea. In *Low and Lower Fertility*, pp. 107–123. Springer International Publishing.

Lee, Y. (2022, jan). Is leave for fathers pronatalist? a mixed-methods study of the impact of fathers' uptake of parental leave on couples' childbearing intentions in South Korea. *Population Research and Policy Review 41*(4), 1471–1500.

Liu, Q. and O. N. Skans (2010, jan). The duration of paid parental leave and children's scholastic performance. *The B.E. Journal of Economic Analysis & Policy 10*(1).

Local Autonomy Act (1990). Act no. 4310. The Congress of South Korea.

Malak, N., M. M. Rahman, and T. A. Yip (2019, mar). Baby bonus, anyone? examining heterogeneous responses to a pro-natalist policy. *Journal of Population Economics 32*(4), 1205–1246.

Malkova, O. (2018). Can maternity benefits have long-term effects on childbearing? Evidence from Soviet Russia. *The Review of Economics and Statistics 100*(4), 691–703.

McCrary, J. and H. Royer (2011). The effect of female education on fertility and infant health: Evidence from school entry policies using exact date of birth. *American Economic Review 101*(1), 158–195.

Milligan, K. (2005). Subsidizing the stork: New evidence on tax incentives and fertility. *The Review of Economics and Statistics 87*(3), 539–555.

Milligan, K. and M. Stabile (2011). Do child tax benefits affect the well-being of children? Evidence from Canadian child benefit expansions. *American Economic Journal: Economic Policy 3*(3), 175–205.

Morgan, P. S. (2003). Is low fertility a twenty-first-century demogrphic crisis? *Demography 40*(4), 589–603.

Oreopoulos, P., M. Stabile, R. Walld, and L. L. Roos (2008). Short-, medium-, and long-term consequences of poor infant health. *Journal of Human Resources 43*(1), 88–138.

Rasmussen, A. W. (2010, jan). Increasing the length of parents' birth-related leave: The effect on children's long-term educational outcomes. *Labour Economics 17*(1), 91–100.

Resident Registration Law (1962). Act no. 1067. The Congress of South Korea.

Riphahn, R. T. and F. Wiynck (2017, jun). Fertility effects of child benefits. *Journal of Population Economics 30*(4), 1135–1184.

Rosenzweig, M. R. (1999). Welfare, marital prospects, and nonmarital childbearing. *Journal of Political Economy 107*(S6), S3–S32.

Rossin, M. (2011, mar). The effects of maternity leave on children's birth and infant health outcomes in the united states. *Journal of Health Economics 30*(2), 221–239.

Rossin-Slater, M. (2017, aug). Maternity and family leave policy.

Sant'Anna, P. H. and J. Zhao (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics 219*(1), 101–122.

Stearns, J. (2015, sep). The effects of paid maternity leave: Evidence from temporary disability insurance. *Journal of Health Economics 43*, 85–102.

Strulik, H. and S. Vollmer (2015). The fertility transition around the world. *Journal of Population Economics 28*, 31–44.

The U.N. Population Division (2011). *World fertility policies 2011.* New York: United Nations.

Whittington, L. A. (1992). Taxes and the family: The impact of the tax exemption for dependents on marital fertility. *Demography 29*(2), 215.

Whittington, L. A., J. Alm, and E. Peters (1990). Fertility and the personal exemption: Implicit pronatalist policy in the United States. *The American Economic Review 80*(3), 545–556.

Yakovlev, E. and I. Sorvachev (2020). Short- and long-run effects of sizable child subsidy: Evidence from Russia. *SSRN Electronic Journal*.

Yoo, S. H., S. R. Hayford, and V. Agadjanian (2016). Old habits die hard? Lingering son preference in an era of normalizing sex ratios at birth in South Korea. *Population Research and Policy Review 36*(1), 25–54.

Zhang, J., J. Quan, and P. van Meerbergen (1994). The effect of tax-transfer policies on fertility in canada, 1921-88. *The Journal of Human Resources 29*(1), 181.

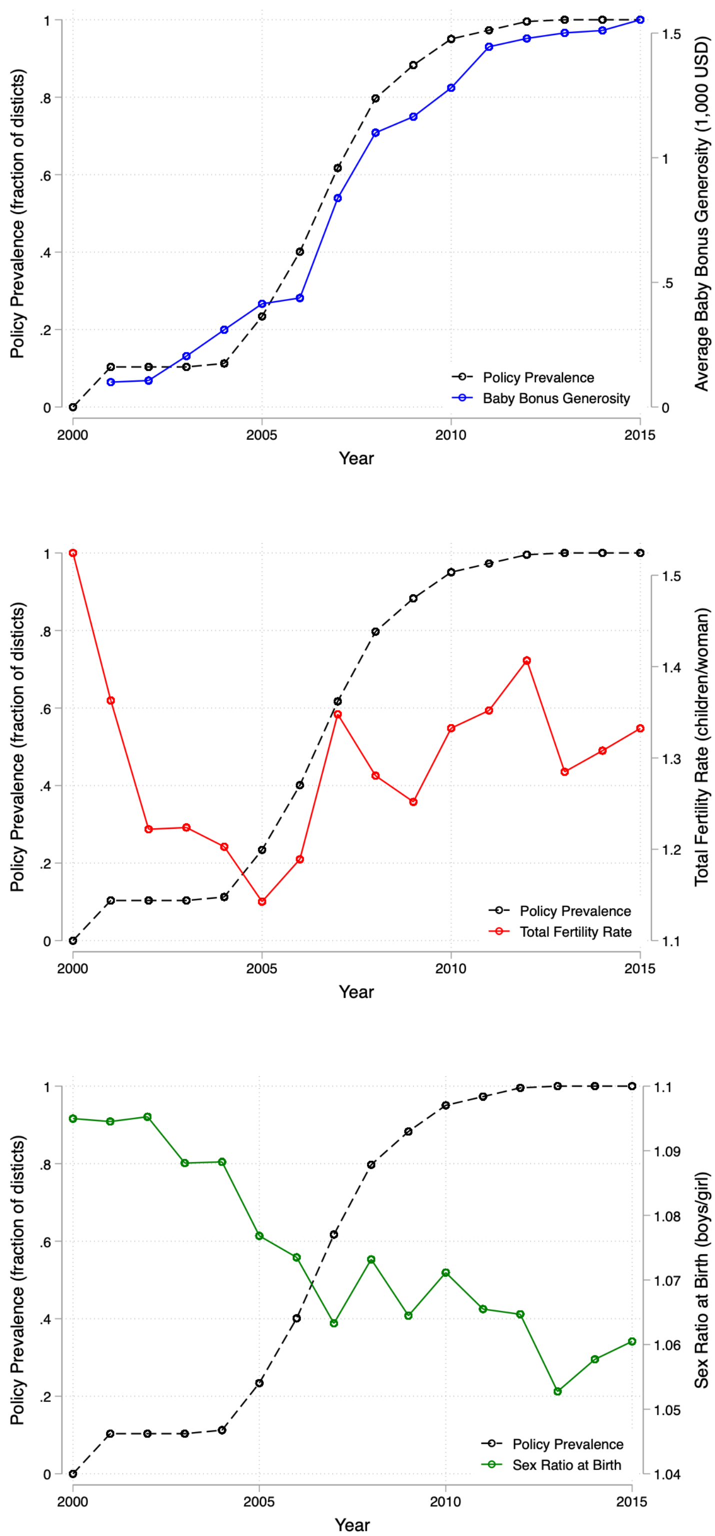

Figure 1: Baby Bonus, Total Fertility Rate, and Sex Ratio at Birth

Notes: This figure plots the fraction of districts (222 total) with pro-natalist cash transfers (baby bonus) and the average amount of baby bonus taken across birth parities and districts with strictly positive cash-transfer generosity, measured in 1 million Korean won (KRW) in the top panel, the average total fertility rates across districts in the middle panel, and the average male-to-female ratio at birth in the bottom panel over time from 2000 to 2015.

		Year					
		2000	2003	2006	2009	2012	2015
A. Pro-natalist Cash Transfers							
1st Child	%	0.00	0.10	0.20	0.34	0.41	0.44
	Mean	-	0.20	0.35	0.59	0.77	0.77
	S.D.	-	(0.02)	(0.19)	(0.69)	(0.89)	(0.87)
	Min/Max	-	[0.20,0.30]	[0.05,1.44]	[0.05,4.70]	[0.05,5.10]	[0.05,5.10]
2nd Child	%	0.00	0.10	0.25	0.69	0.89	0.88
	Mean	-	0.20	0.38	0.75	0.99	1.06
	S.D.	-	(0.02)	(0.30)	(0.97)	(1.21)	(1.23)
	Min/Max	-	[0.20,0.30]	[0.05,2.40]	[0.10,6.50]	[0.05,7.54]	[0.05,7.54]
3rd Child	%	0.00	0.10	0.40	0.88	1.00	1.00
	Mean	-	0.20	0.58	1.91	2.51	2.66
	S.D.	-	(0.02)	(0.86)	(2.59)	(2.92)	(3.12)
	Min/Max	-	[0.20,0.30]	[0.05,5.00]	[0.20,18.80]	[0.20,18.80]	[0.20,18.80]
B. Birth Rates							
Total Fertility Rate		1.52	1.22	1.19	1.25	1.41	1.33
		(0.23)	(0.20)	(0.21)	(0.25)	(0.28)	(0.27)
Birth Rates of Birth order:	1st	20.97	16.46	16.50	17.19	18.99	17.73
		(7.87)	(6.18)	(7.30)	(7.64)	(9.65)	(10.04)
	2nd	19.41	14.48	12.80	13.38	14.97	13.74
		(7.70)	(5.66)	(5.59)	(5.69)	(7.21)	(7.39)
	3rd	5.00	3.51	3.39	3.43	4.23	3.68
		(2.33)	(1.70)	(1.70)	(1.80)	(2.31)	(2.15)

Table 1: Summary Statistics of Baby Bonus and Fertility

Notes: This table summarizes the local pro-natalist cash transfers and birth rates for every three years from 2000 to 2015. For each birth parity, Panel A reports the fraction of districts (out of 222 districts) with strictly positive cash transfers (first row), average cash-transfer amounts measured in 1 million KRW conditional on strictly positive pro-natalist cash transfers (second row), their standard deviations in parentheses (third row), and minimum and maximum values in brackets (fourth row). Panel B reports the average total fertility rates measured by number of children per woman and parity-specific birth rates measured by number of children per 1,000 women between the ages of 15 and 49. Standard deviations are reported in parentheses. In the appendix, Table A.1 reports the summary statistics of newborns' health outcomes (birth weight, gestational age, and sex ratio at birth) based on the individual-level birth-registry records.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline					
	2000	2001	2002	2003	2004	2005
Log(Population)	-0.0311	-0.0337	-0.0545	-0.100	-0.125	-0.0498
	(0.0283)	(0.0351)	(0.0431)	(0.0528)	(0.0726)	(0.0842)
Total Fertility Rate	0.0376	-0.140	0.351	0.0238	-0.350	0.953
	(0.195)	(0.185)	(0.221)	(0.265)	(0.413)	(0.512)
% Female Population	1.047	-3.203	0.691	-0.0681	-5.486	7.066
	(3.248)	(3.167)	(3.498)	(4.293)	(6.127)	(7.100)
% Adult Population	3.280*	4.131*	6.440**	6.925**	7.479*	8.212*
	(1.459)	(1.856)	(2.024)	(2.492)	(3.311)	(3.584)
% Elderly Population	0.0689	1.526	1.849	1.548	2.311	1.697
	(1.370)	(1.448)	(1.539)	(1.867)	(2.526)	(2.533)
Net Migration Rate	0.0005	-0.0003	-0.0016	0.0012	-0.0024	-0.0031
	(0.0010)	(0.0011)	(0.0014)	(0.0018)	(0.0024)	(0.0028)
Marriage Rate	-3.317	-2.352	-5.149	-6.155	-8.447	-5.965
	(2.919)	(3.671)	(5.228)	(6.141)	(7.346)	(16.49)
Financial-Independence Rate	-0.0043**	-0.0045**	-0.0044**	-0.0063**	-0.0083**	-0.0073*
	(0.0014)	(0.0014)	(0.0016)	(0.0020)	(0.0026)	(0.0029)
Conservative Local Gov't Head	0.0312	0.0078	0.149*	0.174*	0.218*	0.305**
	(0.0440)	(0.0409)	(0.0650)	(0.0822)	(0.0983)	(0.100)
Female Local Gov't Head			0.0297	0.279	0.372	0.495
			(0.0469)	(0.209)	(0.301)	(0.428)
Observations	222	199	199	199	197	170
R^2	0.875	0.279	0.299	0.293	0.318	0.393
p-value	0.0239	0.0052	0.0057	0.0036	0.0004	0.0553

Table 2: Determinants of Baby-Bonus Adoption Timing

Notes: This table reports the estimated coefficients from regressing the log of the number of years until a district implements the local pro-natalist baby-bonus policy since a given baseline year (annually from 2000 to 2005) on the district-level characteristics observed in the same baseline year based on equation 1. Each observation corresponds to a district that had not implemented a pro-natalist cash transfer policy prior to each baseline year. City fixed effects are included across columns. A p-value testing the null hypothesis that all the coefficients are jointly equal to zero is reported at the bottom. Heteroskedasticity-robust standard errors are reported in parentheses: * significant at the 5% level, ** significant at the 1% level, and *** significant at the 0.1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
	First Child		Second Child		Third Child	
	All	Ex. Zero	All	Ex. Zero	All	Ex. Zero
Log(Population)	0.0291	-1.647	-0.252	-0.718	-0.398	-1.154
	(0.149)	(1.134)	(0.229)	(0.724)	(0.346)	(0.667)
Total Fertility Rate (Lag)	0.404***	0.377*	0.219	0.122	0.210	0.0364
	(0.100)	(0.155)	(0.116)	(0.149)	(0.173)	(0.213)
% Female Population	-7.263*	-7.722	-9.270**	-14.51	-10.15*	-16.27
	(2.801)	(7.378)	(3.432)	(7.428)	(4.923)	(10.47)
% Adult Population	-1.535	2.809	-6.688**	-2.903	-8.206*	-2.350
	(1.859)	(5.613)	(2.445)	(5.090)	(3.869)	(6.148)
% Elderly Population	2.784	1.848	1.069	5.312	-0.450	2.764
	(1.732)	(4.864)	(2.296)	(4.294)	(3.225)	(5.866)
Net Migration Rate (Lag)	0.0001	0.0004	0.0012**	0.0010	0.0011*	0.0013
	(0.0002)	(0.0008)	(0.0004)	(0.0009)	(0.0005)	(0.0007)
Marriage Rate (Lag)	-8.797***	-4.287	-13.09***	-4.253	-9.300*	-11.70
	(2.370)	(7.248)	(3.351)	(8.590)	(4.321)	(7.826)
Financial-Independence Rate	0.0007	-0.0012	0.0027	0.0030	0.00312	0.00578
	(0.0013)	(0.0080)	(0.0020)	(0.0048)	(0.0033)	(0.0054)
Conservative Local Gov't Head	-0.0174	-0.0320	0.0050	-0.0464	0.0050	-0.0024
	(0.0134)	(0.0332)	(0.0281)	(0.0514)	(0.0313)	(0.0366)
Female Local Gov't Head	-0.0139	0.0274	0.0410	0.00448	0.106	-0.0031
	(0.0138)	(0.0565)	(0.0386)	(0.0484)	(0.0748)	(0.0807)
Observations	3,330	908	3,330	1,693	3,330	2,045
R^2	0.763	0.824	0.803	0.856	0.837	0.875
p-value	0.0000	0.0291	0.0000	0.0683	0.0001	0.0269

Table 3: Determinants of Baby-Bonus Generosity

Notes: This table reports the estimated coefficients from regressing the amount of pro-natalist cash transfers (in log units) provided for the first child in Columns 1–2, the second child in Columns 3–4, and the third child in Columns 5–6 on the district-level characteristics based on equation 2. For each birth order (first, second, and third), the observations in the left column correspond to all district-year pairs and the right column excludes observations prior to policy adoption. A p-value testing the null hypothesis that all the coefficients are jointly equal to zero is reported at the bottom. Clustered standard errors at the district level are reported in parentheses. * significant at the 5% level, ** significant at the 1% level, and *** significant at the 0.1% level.

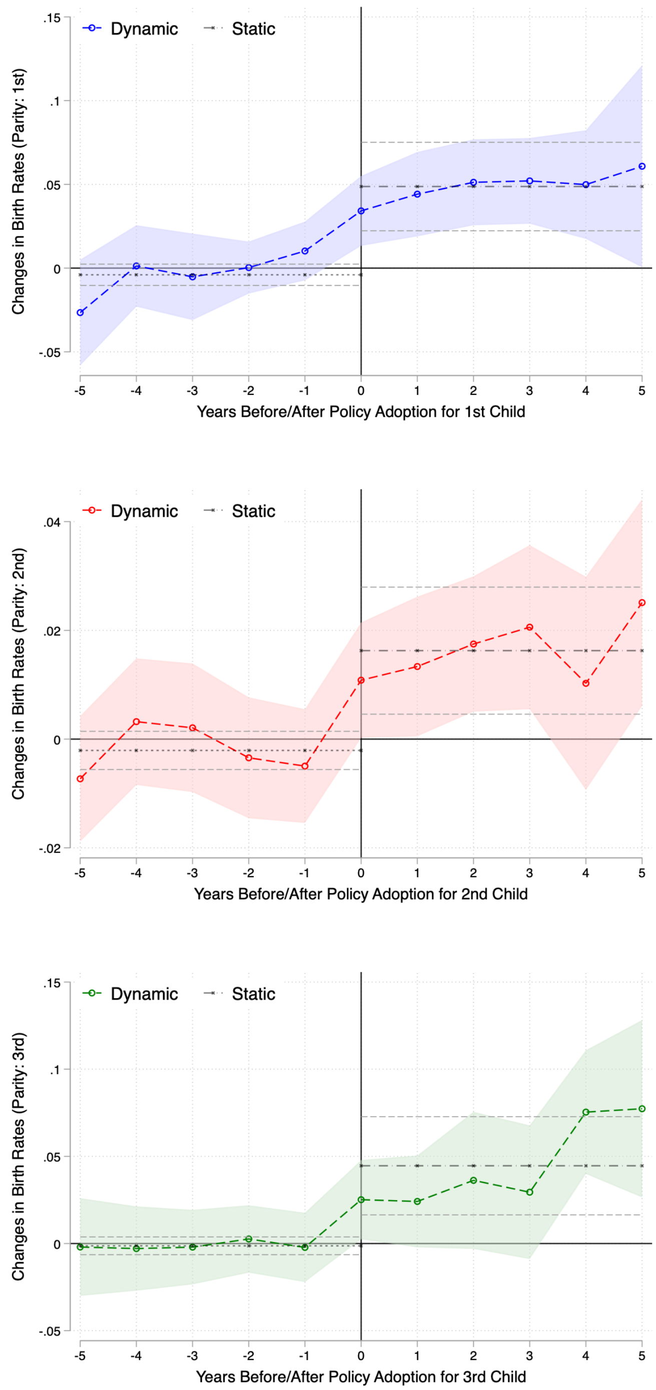

Figure 2: Parity-Specific Birth Rates before and after Baby-Bonus Adoption

Notes: This event-study figure plots the estimated changes in the birth rates before and after pro-natalist cash-transfer policy implementation for the first child (top, in blue), second child (middle, in red), and third child (bottom, in green). The event-study coefficients are estimated based on equation 3 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in black dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a district-year pair and is weighted by the female population aged 15 to 49. Across each panel, the same set of fixed effects (that is, district fixed effects and city-by-year fixed effects) and district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child (resp. the first and second child).

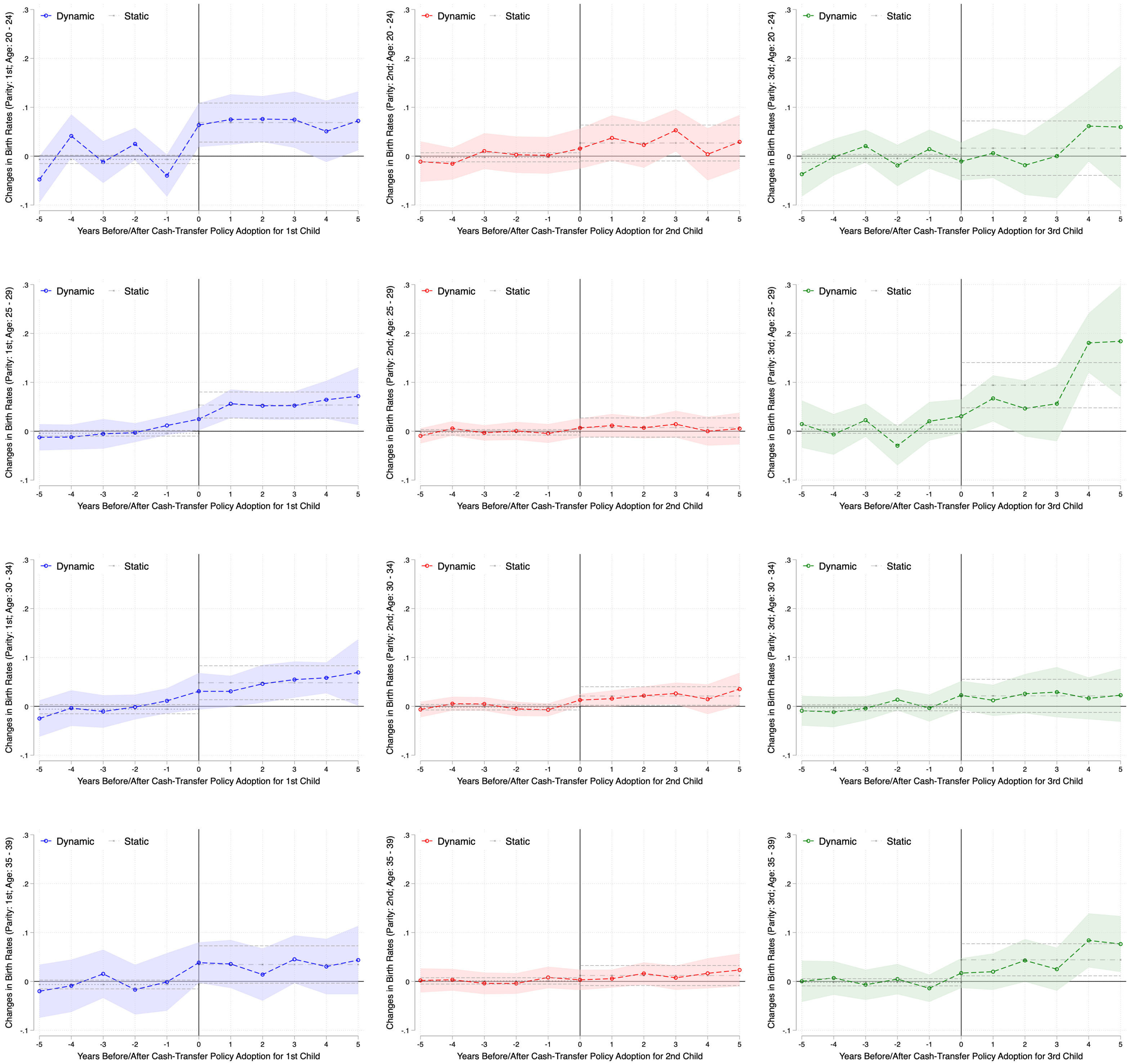

Figure 3: Parity-Specific Birth Rates before and after Baby-Bonus Adoption (Ages: 20–39)

Notes: This event-study figure plots the estimated changes in the age-specific birth rates before and after pro-natalist cash-transfer policy implementation for the first child (left, in blue), second child (center, in red), and third child (right, in green). The event-study coefficients are estimated based on equation 3 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in gray dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a district-year pair and is weighted by the female population of each age group. Across each panel, the same set of fixed effects (that is, district fixed effects and city-by-year fixed effects) and district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child by mothers in the same 5-year age group (resp. the first and second child).

	(1)	(2)	(3)	(4)	(5)	(6)
	log Birth Rates					
	First Child		Second Child		Third Child	
$\sinh^{-1}$ Cash Transfer for						
First Child	0.182***	0.204***		-0.0189		0.00434
	(0.0371)	(0.0390)		(0.0216)		(0.0254)
Second Child		-0.0532	0.0504***	0.0488**		-0.0422
		(0.0276)	(0.00940)	(0.0162)		(0.0229)
Third Child		0.0295		0.00798	0.0394***	0.0560***
		(0.0163)		(0.00806)	(0.00959)	(0.0121)
Observations	3,330	3,330	3,330	3,330	3,330	3,330
R^2	0.951	0.952	0.970	0.970	0.958	0.958

Table 4: The Effect of Baby Bonus on Parity-Specific Birth Rates

Notes: This table reports the estimated effects of cash transfers on the birth rates for the first child (Columns 1–2), the second child (Columns 3–4), and the third child (Columns 5–6) based on equation 4. For each birth order, the left column includes the inverse-hyperbolic-sine-transformed value of cash-transfer amount for the corresponding birth order only; the right column includes those values for the first, second, and third children as separate explanatory variables. Each observation corresponds to a district-year pair from 2001 to 2015 and is weighted by the female population aged 15 to 49. Across columns, the same set of fixed effects (district fixed effects and city-by-year fixed effects) and the same district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, Columns 3–4 (resp. 5–6) include the lagged number of births for the first child (resp. the first and second children) in log units. In the appendix, Table A.3 replicates the same results in levels without taking the transformations. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** significant at the 1% level, and *** significant at the 0.1% level.

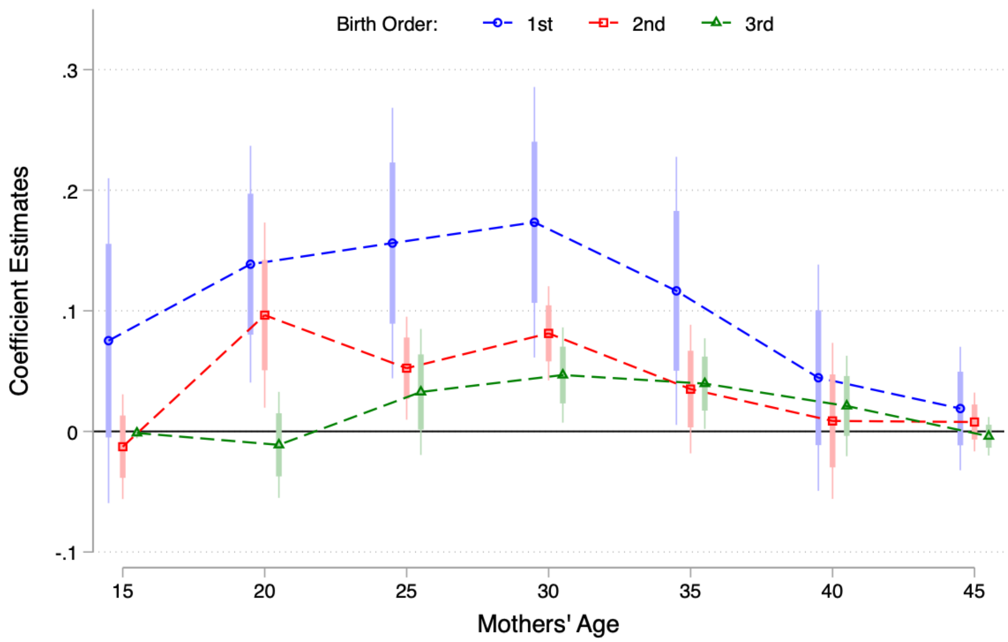

Figure 4: The Effect of Baby Bonus on Parity-Specific Birth Rates by Mother's Age

Notes: This figure plots the estimated effects of the cash transfers on the age-specific birth rates for the first child (blue circles), second child (red squares), and third child (green triangles) by mother's 5-year age group (horizontal axis) based on equation 4. Standard errors are clustered at the district level. Error bars show 95% (thick) and 99.9% (thin) confidence intervals. Each observation corresponds to a district-year pair from 2001 to 2015 and is weighted by the female population of each age group. Across each point estimate, the same set of fixed effects (district fixed effects and city-by-year fixed effects) is included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child by mothers in the same 5-year age group (resp. the first and second children).

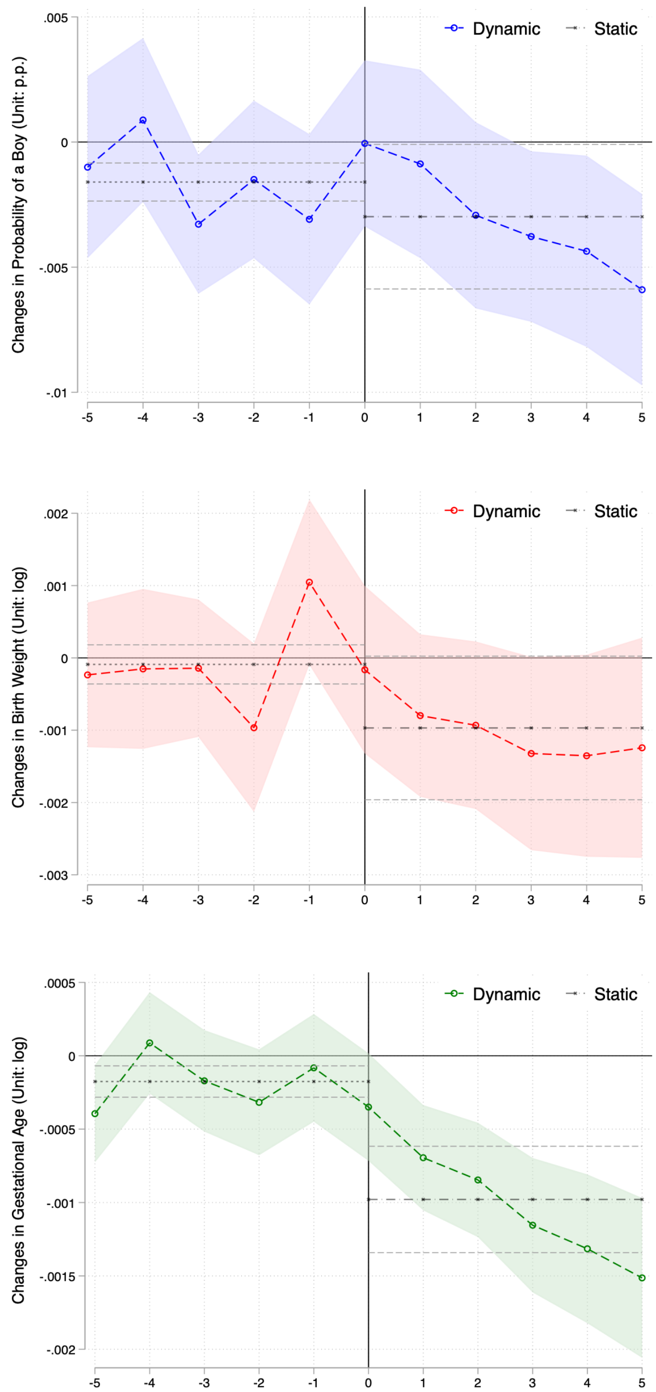

Figure 5: Sex Ratio at Birth and Infant Health before and after Baby-Bonus Adoption

Notes: This event-study figure plots the estimated changes in the probability that a newborn is a boy (top, in blue), birth weight in log kilograms (middle, in red), and gestational age in log weeks (bottom, in green) before and after pro-natalist cash-transfer policy implementation. The event-study coefficients are estimated based on equation 6 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in black dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across panels, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included and family characteristics are controlled for: dummy variables for mother's and father's educational attainment, age, occupation (including unemployment), and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate.

	(1)	(2)	(3)	(4)	(5)	(6)
	Indicator for Boy		log Birth Weight		log Gestational Age	
$\sinh^{-1}$ Cash Transfer	-0.0161***		-0.0016***		-0.0016***	
	(0.0015)		(0.0004)		(0.0002)	
×1st Child		-0.0027		0.0001		-0.0002
		(0.0025)		(0.0009)		(0.0005)
×2nd Child		-0.0041**		-0.0005		-0.0015***
		(0.0013)		(0.0006)		(0.0003)
×3rd Child		-0.0246***		-0.0024***		-0.0017***
		(0.0019)		(0.0004)		(0.0002)
2nd Child	0.0025***	0.0012**	0.0047***	0.0046***	-0.0108***	-0.0108***
	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0001)	(0.0001)
3rd Child	0.0385***	0.0436***	0.0137***	0.0142***	-0.0103***	-0.0101***
	(0.0019)	(0.0022)	(0.0005)	(0.0005)	(0.0002)	(0.0002)
Observations	6,488,097	6,488,097	6,488,097	6,488,097	6,488,097	6,488,097

Table 5: The Effect of Baby Bonus on Sex Ratio at Birth and Newborn Health

Notes: This table reports the estimated effects of baby bonus on indicators for boys (Columns 1–2), log of birth weight (Columns 3–4), and log of gestational age (Columns 5–6). For each dependent variable, the left column reports the estimated effect of cash transfers unconditional on birth parity; in the column to the right, the cash transfers' effect is allowed to differ across birth parity. The mean values among first children are 0.513% for the indicator for boys, 3.192 kilograms for birth weight, and 39.074 weeks for gestational age. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across columns, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included, and family characteristics are controlled for: dummy variables for mother's and father's educational attainment, age, occupation including unemployment, and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)
	1×(dead before 1 year)		1×(dead before 5 year)	
$\sinh^{-1}$ Cash Transfer	0.0026		-0.0028	
	(0.0178)		(0.0222)	
×First Child		0.0297		0.0149
		(0.0295)		(0.0343)
×Second Child		-0.0028		0.0016
		(0.0203)		(0.0243)
×Third Child		0.0076		-0.0073
		(0.0231)		(0.0273)
Second Child	0.0392***	0.0426***	0.0429***	0.0428***
	(0.0074)	(0.0295)	(0.0093)	(0.0095)
Third Child	0.0865***	0.0837***	0.1055***	0.1113***
	(0.0190)	(0.0234)	(0.0229)	(0.0278)
Observations	1,711,947	1,711,947	1,711,947	1,711,947

Table 6: The Effect of Baby Bonus on Early-Life Mortality

Notes: This table reports the estimated effects of cash transfers on early-life mortality (indicator for those dead within 1 year since birth in Columns 1–2 and within 5 years since birth in Columns 3–4). Estimated coefficients are measured in percentage points. For each dependent variable, the first and second columns report the estimated effects of cash transfers unconditional on birth order; in the third and fourth columns, the cash transfers' effects are allowed to differ by birth order. The mean values of 1-year and 5-year mortality rates are 0.14% and 0.20%, respectively. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2010 to 2013. Across columns, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included, and family characteristics are controlled for: dummy variables for mother's and father's age, educational attainment, occupation including unemployment, and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)
	log # Potential Beneficiaries			Non-Beneficiaries
	First Child	Second Child	Third Child	
$\sinh^{-1}$ Cash Transfer for				
First Child	0.0092			0.0147
	(0.0131)			(0.0138)
Second Child		0.0122		-0.0099
		(0.0106)		(0.0138)
Third Child			0.0209**	0.0011
			(0.0076)	(0.0080)
Observations	596,495	281,005	249,238	602,631

Table 7: Migratory Response of Families to Baby Bonus

Notes: This table reports the estimated effects of pro-natalist cash transfers on migration based on equation 10. The dependent variables in Columns 1, 2, and 3 are the log of the numbers of households identified as potential beneficiaries of pro-natalist cash transfers for a first, second, and third child, respectively. The dependent variable in Column 4 is the log of the number of households identified as nonbeneficiaries of pro-natalist cash transfers. Each observation corresponds to a tuple of destination by origin district by year from 2001 to 2015. The district origin-destination-level panel data set is constructed from the universe of resident registration records. Across columns, the same set of fixed effects (district-pair fixed effects, origin-district-by-year fixed effects, destination-city-by-year fixed effects) is included; the destination-district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors, three-way-clustered at the origin-district-by-year level, destination-district-by-year level, and district-pair level, are reported in parentheses. * significant at the 5% level, ** significant at the 1% level, and *** significant at the 0.1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Mother's Age	Marriage Duration	College+		Employed	
			Mother	Father	Mother	Father
$\sinh^{-1}$ Cash Transfer for						
1st Child	-0.0667	0.0600*	0.0147***	0.0152***	-0.0364***	-0.0010
	(0.0617)	(0.0253)	(0.0038)	(0.0042)	(0.0073)	(0.0013)
2nd Child	0.117**	-0.0064	0.310***	0.0114***	-0.0066	-0.0027***
	(0.0388)	(0.0098)	(0.0025)	(0.0023)	(0.0034)	(0.0007)
3rd Child	-0.0199	0.0006	0.0279***	0.0019	-0.0118***	-0.0017**
	(0.0289)	(0.0108)	(0.0020)	(0.0014)	(0.0026)	(0.0005)
2nd Child	0.847***	2.247***	-0.0487***	-0.0099***	-0.0934***	0.0032***
	(0.0123)	(0.0060)	(0.0010)	(0.0007)	(0.0022)	(0.0003)
3rd Child	1.974***	4.619***	-0.133***	-0.0309***	-0.125***	0.0016**
	(0.0297)	(0.0125)	(0.0018)	(0.0014)	(0.0036)	(0.0005)
Observations	6,488,100	6,438,047	6,483,519	6,449,188	6,488,097	6,488,097
Mean Dependent Variable (1st Child)	28.999	1.808	0.6368	0.6606	0.3176	0.9554

Table 8: Composition Changes in Parental Characteristics

Notes: This table reports the estimated changes in the parental characteristics with cash transfers: mother's age (Column 1), years of marriage (Column 2), education attainment (Columns 3 and 4), and employment status (Columns 5 and 6). Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across columns, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included, and family characteristics are controlled for: dummy variables for mother's and father's age (excluded in Column 1), educational attainment (mother's attainment excluded in Column 3 and father's in Column 4), occupation (mother's occupation excluded in Column 5 and father's in Column 6), and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

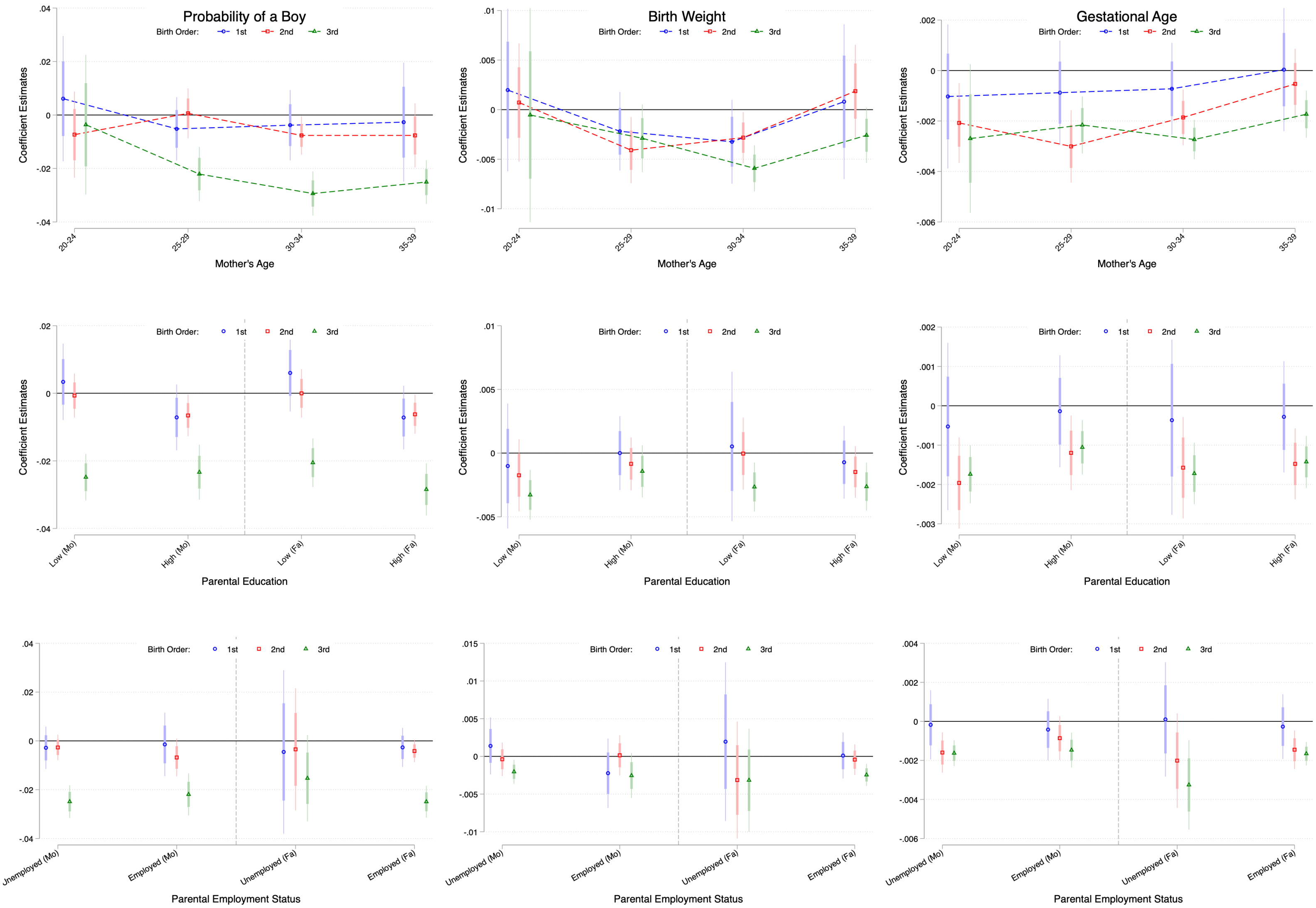

Figure 6: Heterogeneous Policy Effects on Sex Ratio at Birth and Newborn Health by Parental Age, Education, and Employment

Notes: This figure plots the parity-specific cash-transfer effects on the sex ratio at birth (left panels), birth weight (middle panels) and gestational age (right panels) by mother's age (top panels), parents' educational attainment (middle panels), and their employment status (bottom panels). The parity-specific effects are estimated based on equation 7, separately by pooling samples for each parental characteristic. Standard errors are clustered at the district level. Error bars show 95% (thick) and 99.9% (thin) confidence intervals. Across panels and groupings by parental characteristics, the same fixed effects (district fixed effects and city-by-month-year fixed effects) are included and family characteristics are controlled for: dummy variables for mother's age (excluded in the top panels) and father's age, their educational attainment (mother's attainment excluded in the middle panels when pooling observations by their educational attainment and father's excluded in the middle panels when pooling observations by their educational attainment), and mother's and father's occupation (excluded in the bottom panels when pooling by the corresponding parent's employment status) and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate.

	(1)	(2)	(3)	(4)
	$P(1st = B)$	$P(2nd = B)$	$P(2nd = B)$	$P(2nd = B)$
$\sinh^{-1}$ Cash Transfer	0.0234	0.0001	-0.0085	0.0064
	(0.0143)	(0.0078)	(0.0136)	(0.0141)
Observations	516,078	395,359	196,792	198,562
HH Sample Restrictions:				
# of Children	1, 2, 3	2, 3	2, 3	2, 3
Sex of 1st Child	-	-	Boy	Girl
	(5)	(6)	(7)	(8)
	$P(3rd = B)$	$P(3rd = B)$	$P(3rd = B)$	$P(3rd = B)$
$\sinh^{-1}$ Cash Transfer	-0.0256*	-0.0892**	-0.0440*	0.0414
	(0.0126)	(0.0311)	(0.021)	(0.0239)
Observations	69,417	16,100	26,089	27,189
Sample Restrictions:				
# of Children	3	3	3	3
Sex of 1st & 2nd Child	-	Both Boys	Boy and Girl	Both Girls

Table 9: The Effects of Baby Bonus on Sex Ratio at Birth by Family Composition

Notes: This table reports the estimated effects of pro-natalist cash transfers on the probability that the first child is a boy (Column 1), that the second child is a boy (Columns 2–4), and that the third child is a boy (Columns 5–8). The data source is the 2015 Population Census of South Korea, which surveys 20% of the population. Each observation corresponds to a household with one, two, or three children whose youngest child was born in or after 2001. In each column, a different set of sample restrictions is applied: the observations are the households (HHs) with one, two, and three children in Column 1 (no restriction); the HHs with at least two children in Column 2; the HHs with at least two children, the first of whom is a boy, in Column 3; the HHs with two children, the first of whom is a girl, in Column 4. Across Columns 5–8, the observations are the HHs with three children; in Column 6, the first and second children are both boys; in Column 7, the first and second children are a boy and a girl (vice versa); in Column 8, the first and second children are both girls. Across columns, the same set of fixed effects (birth-district fixed effects and birth-city-by-birth-year fixed effects) is included and family characteristics are controlled for: dummy variables for mother's and father's age, educational attainment, employment status, and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

Appendix

A Construction of Birth Rates

Following the convention in demography, birth rates for birth order p in district d in year y $BR_{p,d,y}$ is defined as follows:

$$BR_{p,d,y} = \frac{NB_{p,d,y}}{fpop_{d,y}} \times 1000,$$

where $NB_{p,d,y}$ equals the number of the p-th order children born in district d in year y; $fpop_{d,y}$ is the female population of ages between 15 and 49 living in d in year y. Likewise, I define age-specific birth rates $BR_{a,p,d,y}$ as

$$BR_{a,p,d,y} = \frac{NB_{a,p,d,y}}{fpop_{a,d,y}} \times 1000,$$

where $NB_{a,p,d,y}$ equals the number of p-th order children born in district d in year y by mothers who belong to age group a (5-year intervals from 15 to 49); $fpop_{a,d,y}$ is the female population in age group a living in d in year y. Note that total fertility rate $TFR_{d,y}$ can be expressed as a function of the age- and order- specific birth rates. That is,

$$TFR_{d,y} = \sum_{\forall a,p} BR_{a,p,d,y} \times \frac{5}{1000}.$$

B Additional Figures and Tables

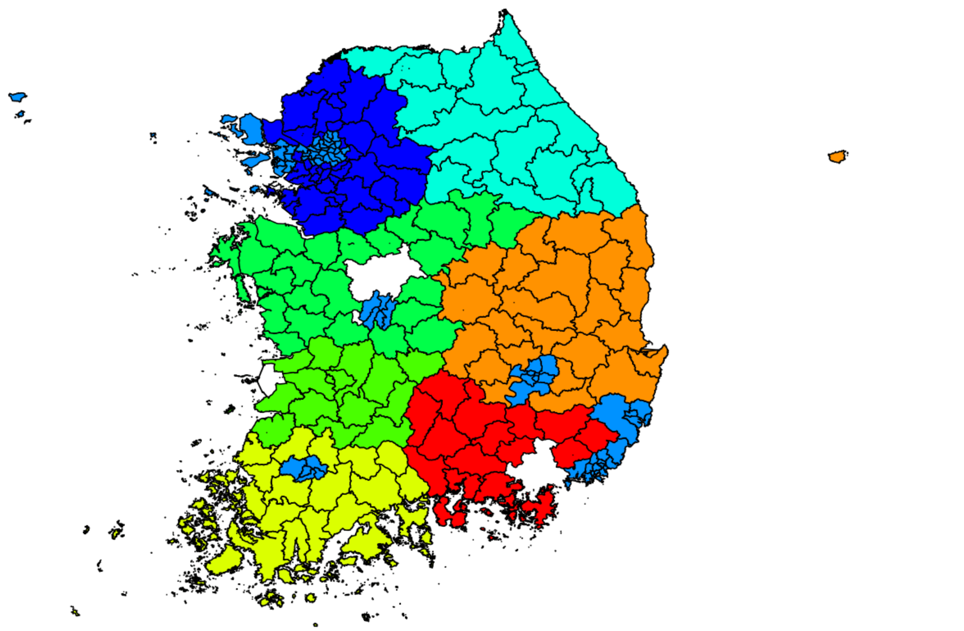

Figure A.1: Metropolitan Cities, Provinces, and Districts of South Korea

Notes: This figure map plots 222 districts in South Korea, which constitutes 15 metropolitan cities and provinces (refer to as cities in the main text) in different colors. Note that light blue indicates the districts located in 7 metropolitan cities including Seoul, Busan, Daegu, Daejeon, Gwangju, Incheon, and Ulsan.

First Child Second Child Third Child

Figure A.2: Local Pro-natalist Cash Transfers across Districts (2005, 2010, 2015)

Notes: This figure presents a set of maps plotting the pro-natalist cash transfers for the 1st child (left), 2nd child (center), and 3rd child (right) across districts for years 2005 (top), 2010 (middle), and 2015 (bottom). Darker blue indicates more a greater amount of cash transfers.

	Year					
	2000	2003	2006	2009	2012	2015
A. Fraction of Male Births						
1st Child	0.515	0.512	0.514	0.513	0.513	0.515
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
2nd Child	0.518	0.516	0.515	0.513	0.512	0.510
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
3rd Child	0.587	0.574	0.548	0.533	0.521	0.513
	(0.492)	(0.495)	(0.498)	(0.499)	(0.500)	0.500)
	(0.483)	(0.492)	(0.482)	(0.490)	(0.488)	(0.499)
B. Birth Weight						
1st Child	3.246	3.256	3.236	3.219	3.206	3.202
	(0.443)	(0.452)	(0.450)	(0.449)	(0.450)	(0.458)
2nd Child	3.257	3.268	3.248	3.232	3.217	3.208
	(0.440)	(0.450)	(0.449)	(0.453)	(0.460)	(0.464)
3rd Child	3.303	3.292	3.262	3.238	3.229	3.212
	(0.483)	(0.492)	(0.482)	(0.490)	(0.488)	(0.499)
C. Gestational Age						
1st Child	39.409	39.280	39.182	39.019	38.918	38.818
	(1.450)	(1.562)	(1.585)	(1.588)	(1.600)	(1.644)
2nd Child	39.065	38.852	38.731	38.521	38.399	38.298
	(1.465)	(1.571)	(1.555)	(1.570)	(1.586)	(1.590)
3rd Child	39.085	38.803	38.650	38.404	38.297	38.172
	(1.576)	(1.679)	(1.666)	(1.691)	(1.672)	(1.719)

Table A.1: Summary Statistics (Birth Weight, Gestational Age, Sex Ratio)

Notes: This table reports the mean fraction of male births (Panel A), birth weight in kilograms (Panel B), and gestational age in weeks (Panel C) for the 1st, 2nd, and 3rd child based on the universe of confidential birth registry records for every three years from 2000 to 2015 . Standard deviations are reported in parentheses.

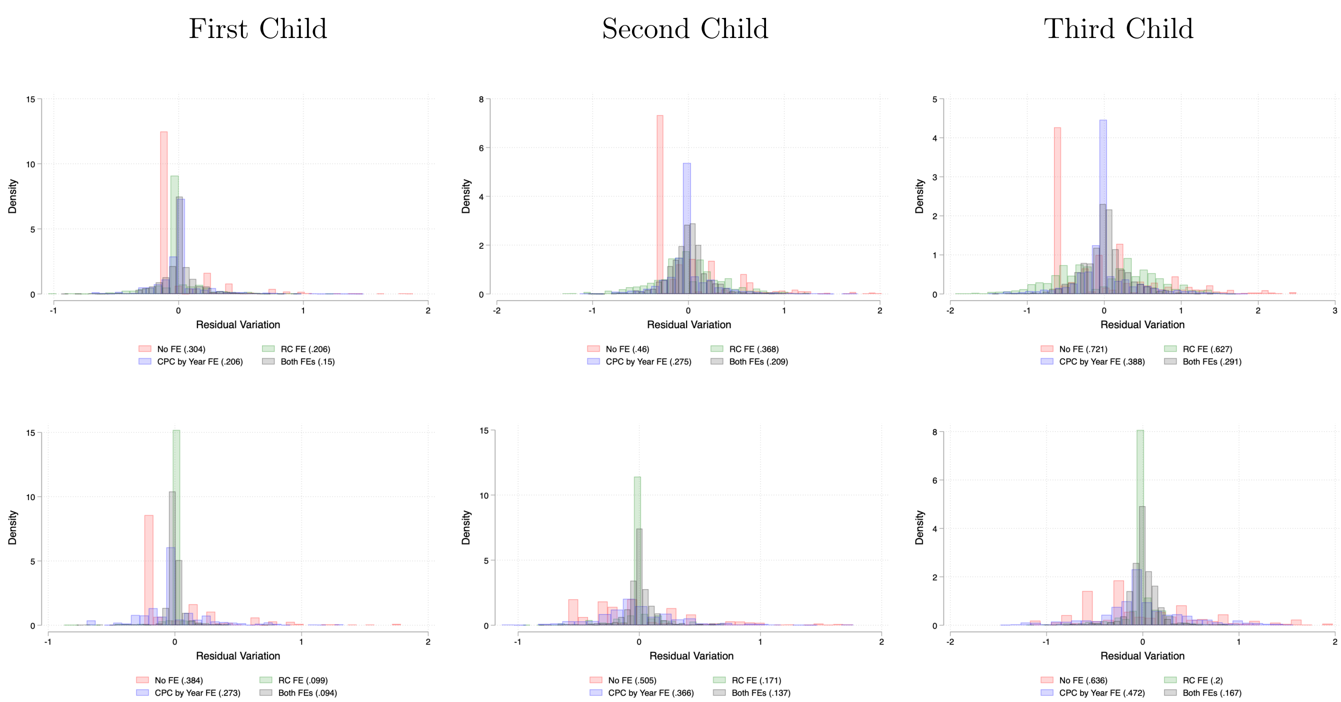

Figure A.3: Residual Variation in Baby Bonus Generosity

Notes: This figure plots the histogram of the inverse hyperbolic transformed values of local pro-natalist cash transfers for 1st child (left), 2nd child (center), and 3rd child (right). The top panels use all the sample periods (i.e., 2001-2015) used for estimating the baby bonus effects on fertility and infant health, except mortality; the bottom panels use only the sample periods for which the the birth-death matched administrative data set is available (i.e., 2010-2013). Each panel contains the histogram of the inverse hyperbolic sine transformed cash transfers residualized by a constant ("No FE" in red), district fixed effects ("RC FE" in green), city-by-year fixed effects ("CPC by Year FE" in blue), and both fixed effects together ("Both FEs" in gray). Standard deviations are reported in parentheses.

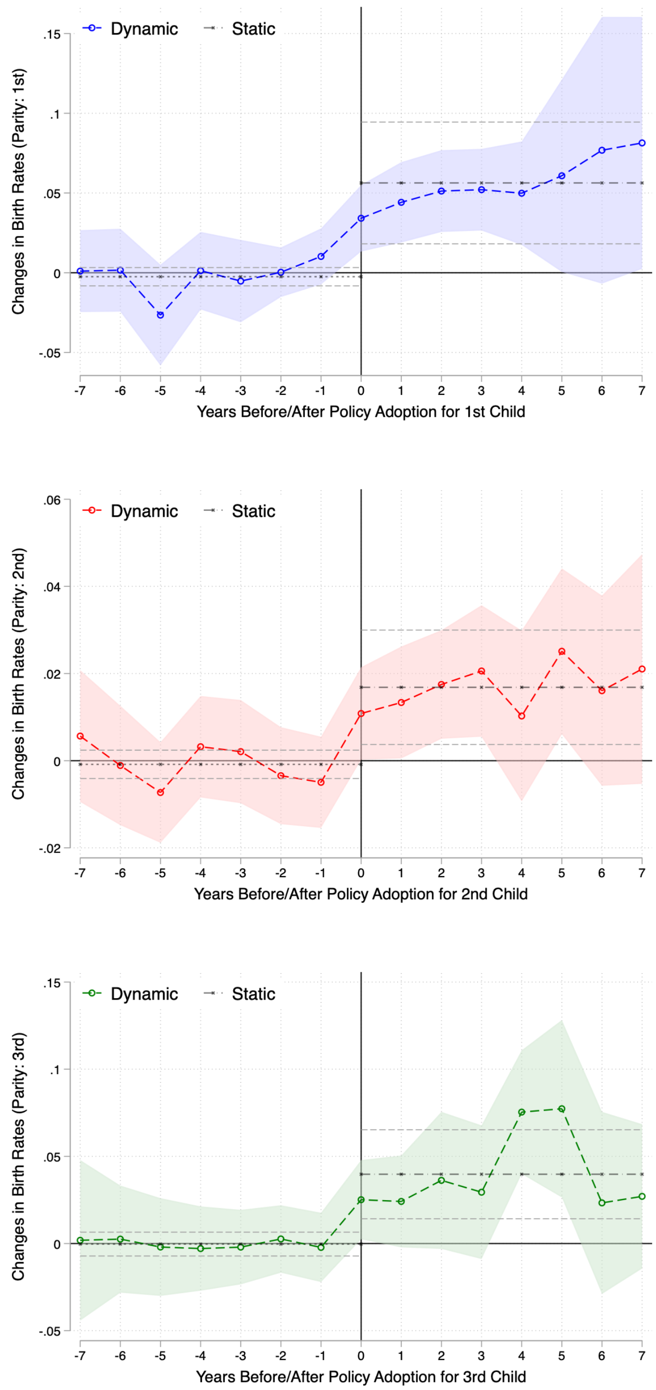

Figure A.4: Parity-specific Birth Rates Before and After Baby Bonus Adoption (Larger Window)

Notes: This event-study figure plots the estimated changes in the birth rates before and after pro-natalist cash-transfer policy implementation for the first child (top, in blue), second child (middle, in red), and third child (bottom, in green). The event-study coefficients are estimated based on equation 3 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in black dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a district-year pair and is weighted by the female population aged 15 to 49. Across each panel, the same set of fixed effects (that is, district fixed effects and city-by-year fixed effects) and district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child (resp. the first and second child).

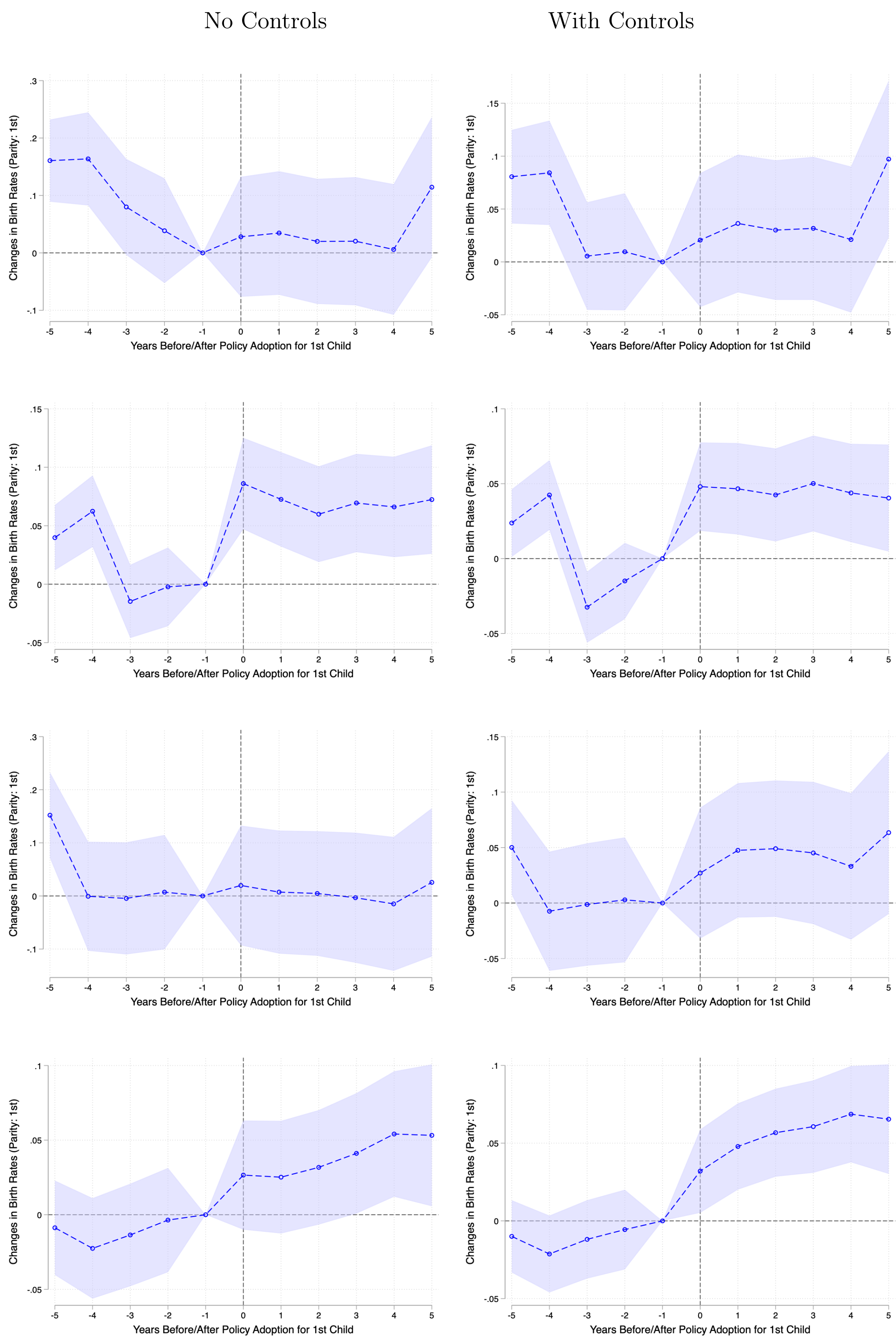

Figure A.5: Event Study Results for 1st Child Birth Rates

Notes: This figure presents a set of results estimating Eq. 3 for the 1st child without any control variables (i.e., district-level time vary characteristics) in the left panels and with the district-level control variables in the right panels. The top panels plot the estimation results without any fixed effects; the second top panels include district fixed effects; the panels second from the bottom includes city-by-year fixed effects; the bottom panels include the set of both fixed effects.

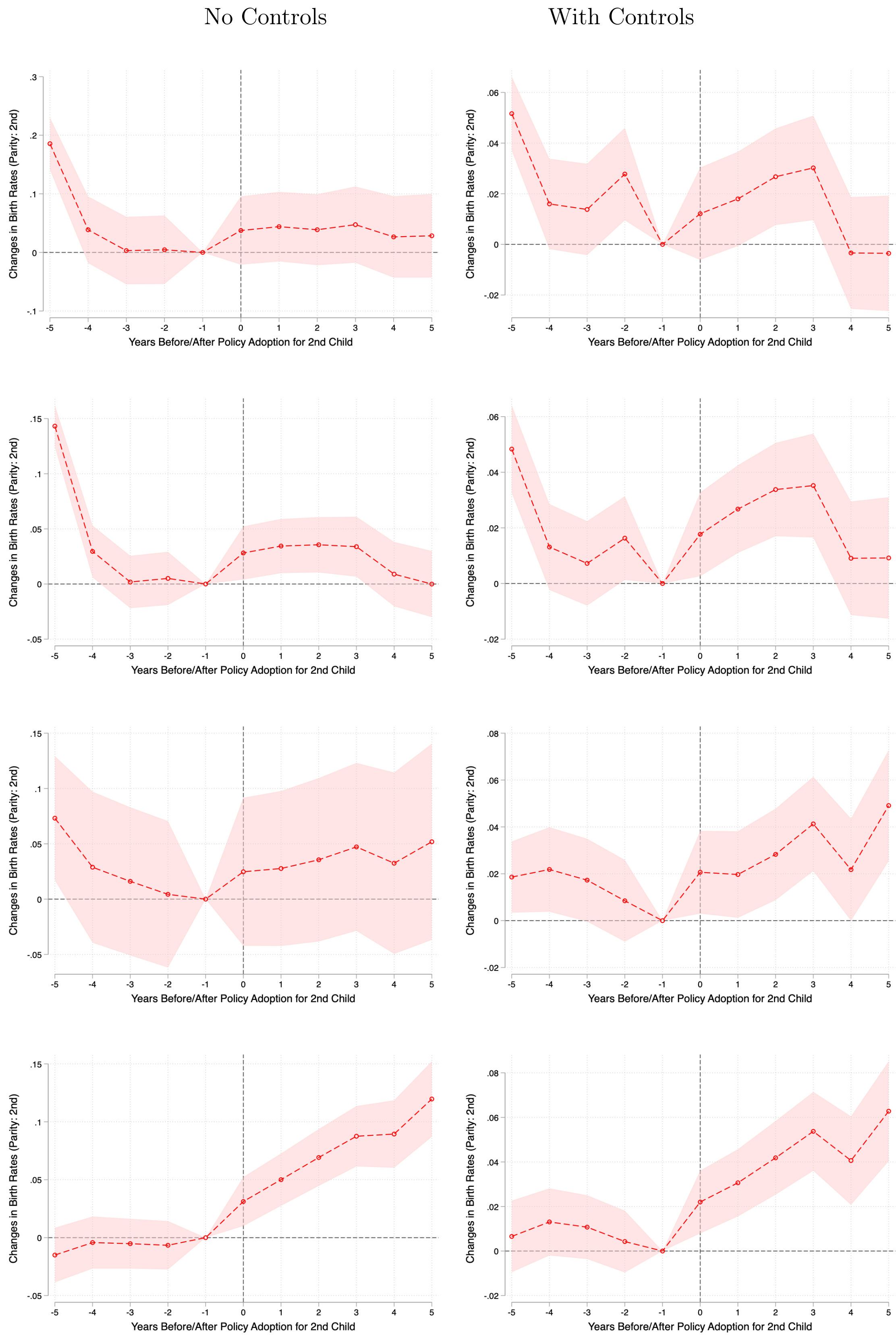

Figure A.6: Event Study Results for 2nd Child Birth Rates

Notes: This figure presents a set of results estimating Eq. 3 for the 2nd child without any control variables (i.e., district-level time vary characteristics) in the left panels and with the district-level control variables in the right panels. The top panels plot the estimation results without any fixed effects; the second panels include district fixed effects; the panels second from the bottom includes city-by-year fixed effects; the bottom panels include the set of both fixed effects.

No Controls With Controls

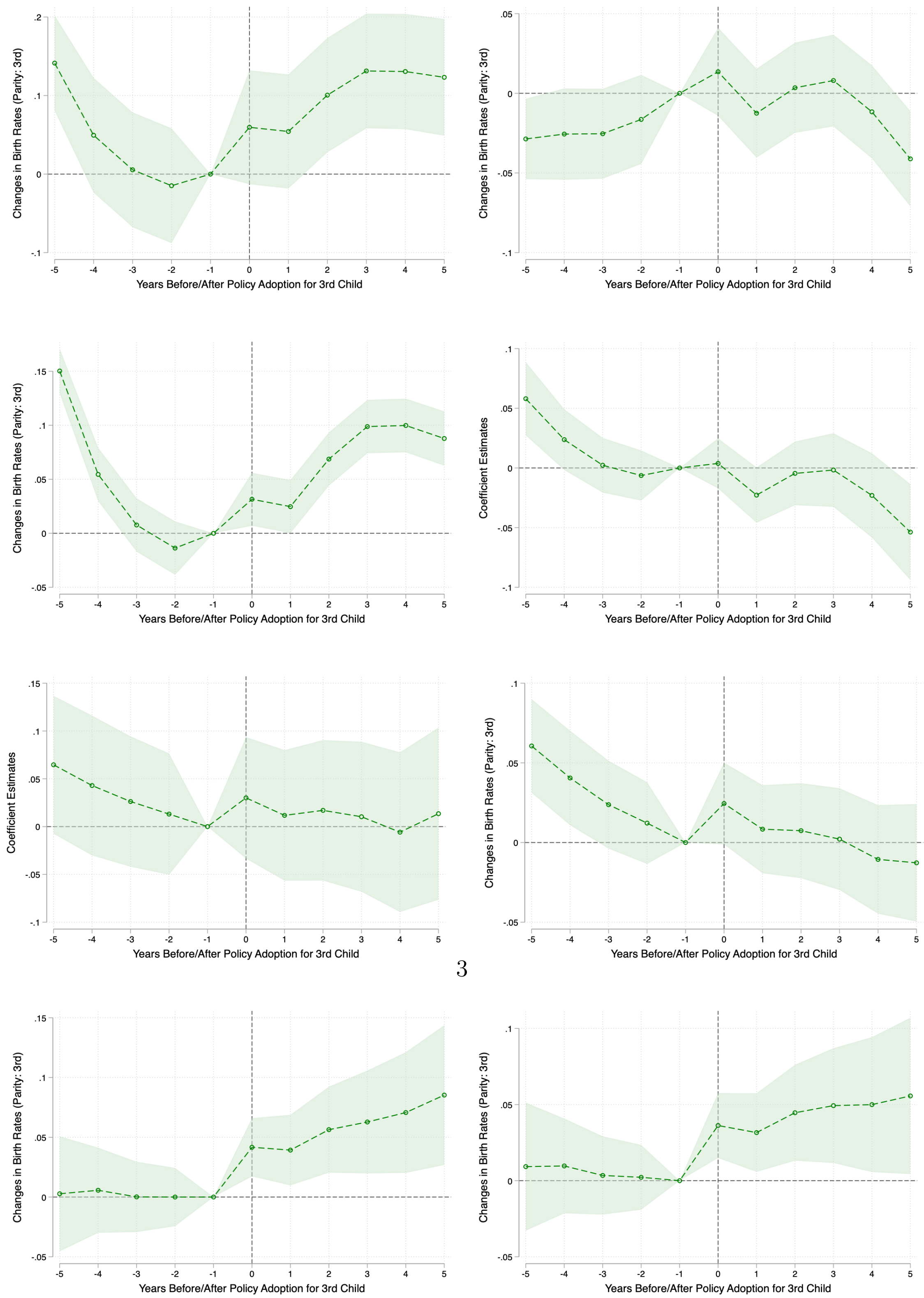

Figure A.7: Event Study Results for 3rd Child Birth Rates

Notes: This figure presents a set of results estimating Eq. 3 for the 3rd child without any control variables (i.e., district-level time vary characteristics) in the left panels and with the district-level control variables in the right panels. The top panels plot the estimation results without any fixed effects; the second top panels include district fixed effects; the panels second from the bottom includes city-by-year fixed effects; the bottom panels include the set of both fixed effects.

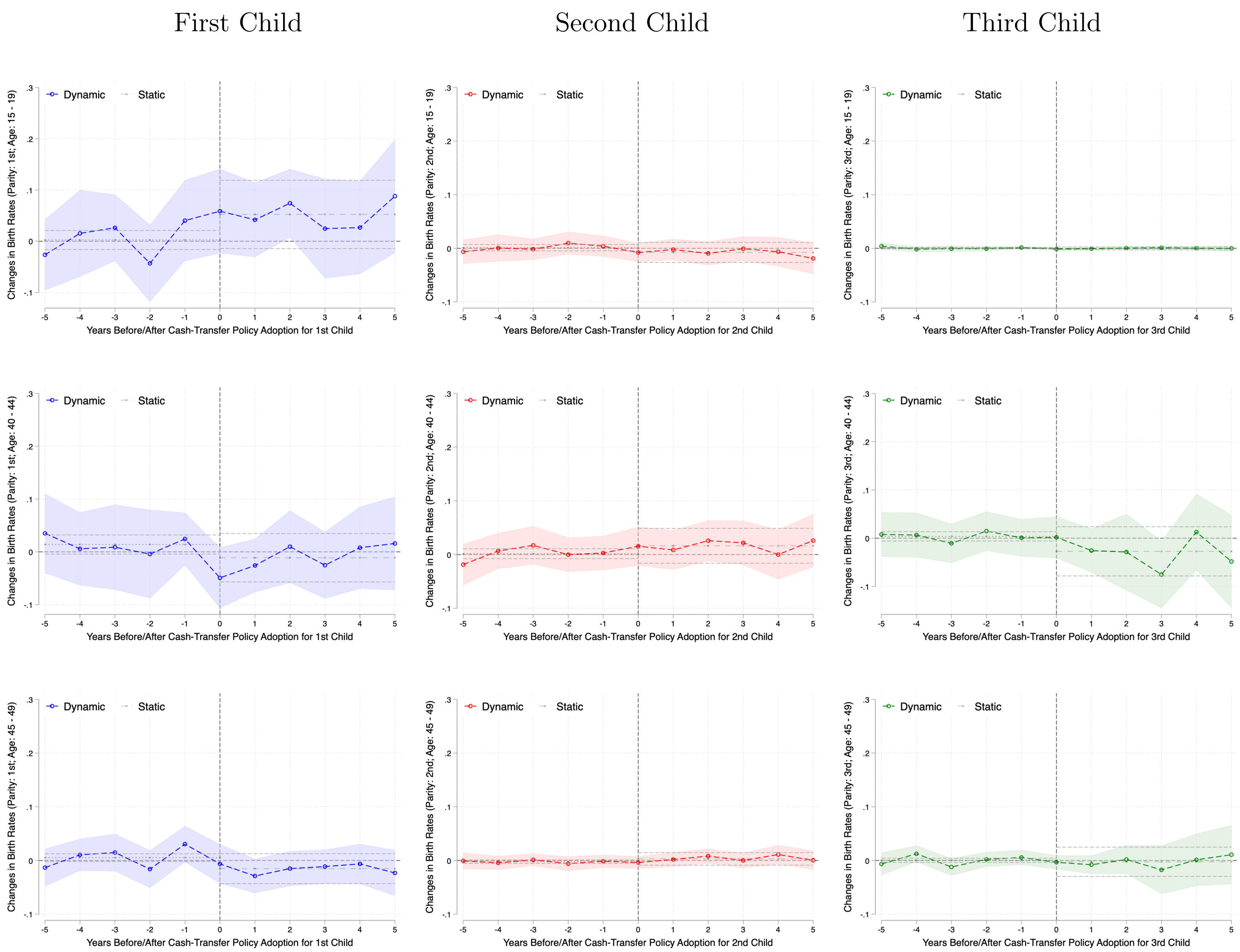

Figure A.8: Birth Rates Before and After Baby Bonus Adoption (Ages: 15-19, 40-44 and 45-49)

Notes: This event-study figure plots the estimated changes in the age-specific birth rates before and after pro-natalist cash-transfer policy implementation for the first child (left, in blue), second child (center, in red), and third child (right, in green). The event-study coefficients are estimated based on equation 3 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in gray dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a district-year pair and is weighted by the female population of each age group. Across each panel, the same set of fixed effects (that is, district fixed effects and city-by-year fixed effects) and district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child by mothers in the same 5-year age group (resp. the first and second child).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				A. Birth Order: 1st			
$\sinh^{-1}$ Cash Transfer for 1st Child	-0.0433	0.193***	-0.00221	0.191***	0.162***	0.162***	0.182***
	(0.0769)	(0.0348)	(0.0737)	(0.0467)	(0.0412)	(0.0410)	(0.0371)
Observations	3,330	3,330	3,330	3,330	3,330	3,330	3,330
R-squared	0.001	0.865	0.218	0.916	0.931	0.931	0.951
				B. Birth Order: 2nd			
$\sinh^{-1}$ Cash Transfer for 2nd Child	-0.0190	-0.0128	-0.00173	0.120***	0.0532***	0.0532***	0.0504***
	(0.0269)	(0.0155)	(0.0474)	(0.0245)	(0.00972)	(0.00977)	(0.00940)
Observations	3,330	3,330	3,330	3,330	3,330	3,330	3,330
R-squared	0.000	0.811	0.306	0.926	0.968	0.969	0.970
				C. Birth Order: 3rd			
$\sinh^{-1}$ Cash Transfer for 3rd Child	0.134***	0.0374***	0.0774	0.0803***	0.0389***	0.0385***	0.0394***
	(0.0206)	(0.00920)	(0.0438)	(0.0135)	(0.00934)	(0.00966)	(0.00959)
Observations	3,330	3,330	3,330	3,330	3,330	3,330	3,330
R-squared	0.040	0.878	0.555	0.942	0.957	0.957	0.958
District FE		O		O	O	O	O
City-by-Year FE			O	O	O	O	O
District-level control variables:							
Demographic Characteristics					O	O	O
Local Gov't Characteristics						O	O
Marriage and Net Migration Rates							O

Table A.2: The Effect of Cash Transfer on Birth Rates

Notes: This table replicates the results reported in Table 4 for the 1st (Panel A), 2nd (Panel B), and 3rd (Panel C) child by gradually adding fixed effects and district-level control variables. In column 1, the effects of baby bonus on birth rates are estimated without any fixed effects and control variables. In Column 2, the district fixed effects are included. In Column 3, the city-by-year fixed effects are included. Column 4 reports the estimated effects while including both sets of fixed effects. Starting from Column 5 to 7, district-level time varying characteristics are gradually introduced: demographic characteristics (total population, age and gender composition, lagged number of births for the 1st child (Panel B only), lagged number of births for the 1st and 2nd child (Panel C only) in Column 5, local government characteristics (financial independence rate and indicators for the gender and political party affiliation of the local government head) in Column 6, and lagged marriage and net migration rate in Column 7. Standard errors are clustered at the district level and reported in parentheses: * Significant at the 5 percent level, ** Significant at the 1 percent level, and *** Significant at the 0.1 percent level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Birth Rates (# Birth/1,000 Women)					
	First Child		Second Child		Third Child	
Cash Transfer for						
1st Child	1.582***	1.790***		-0.0583		0.0314
	(0.366)	(0.385)		(0.240)		(0.0829)
2nd Child		-0.291*	0.374***	0.371*		0.00287
		(0.137)	(0.100)	(0.156)		(0.0557)
3rd Child		0.0631		0.0142	0.0341*	0.0311
		(0.0582)		(0.0310)	(0.0138)	(0.0163)
Observations	3,330	3,330	3,330	3,330	3,330	3,330
R^2	0.976	0.976	0.976	0.976	0.957	0.957

Table A.3: The Effect of Cash Transfer on Birth Rates in Levels

Notes: This table reports the estimated effects of cash transfers on the birth rates for the first child (Columns 1–2), the second child (Columns 3–4), and the third child (Columns 5–6) based on equation 4. For each birth order, the left column includes the cash-transfer amount for the corresponding birth order only; the right column includes the cash-transfer amounts for the first, second, and third children as separate explanatory variables. Each observation corresponds to a district-year pair from 2001 to 2015 and is weighted by the female population aged 15 to 49. Across columns, the same set of fixed effects (district fixed effects and city-by-year fixed effects) and the same district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, Columns 3–4 (resp. 5–6) include the lagged number of births for the first child (resp. the first and second children) in log units. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** significant at the 1% level, and *** significant at the 0.1% level.

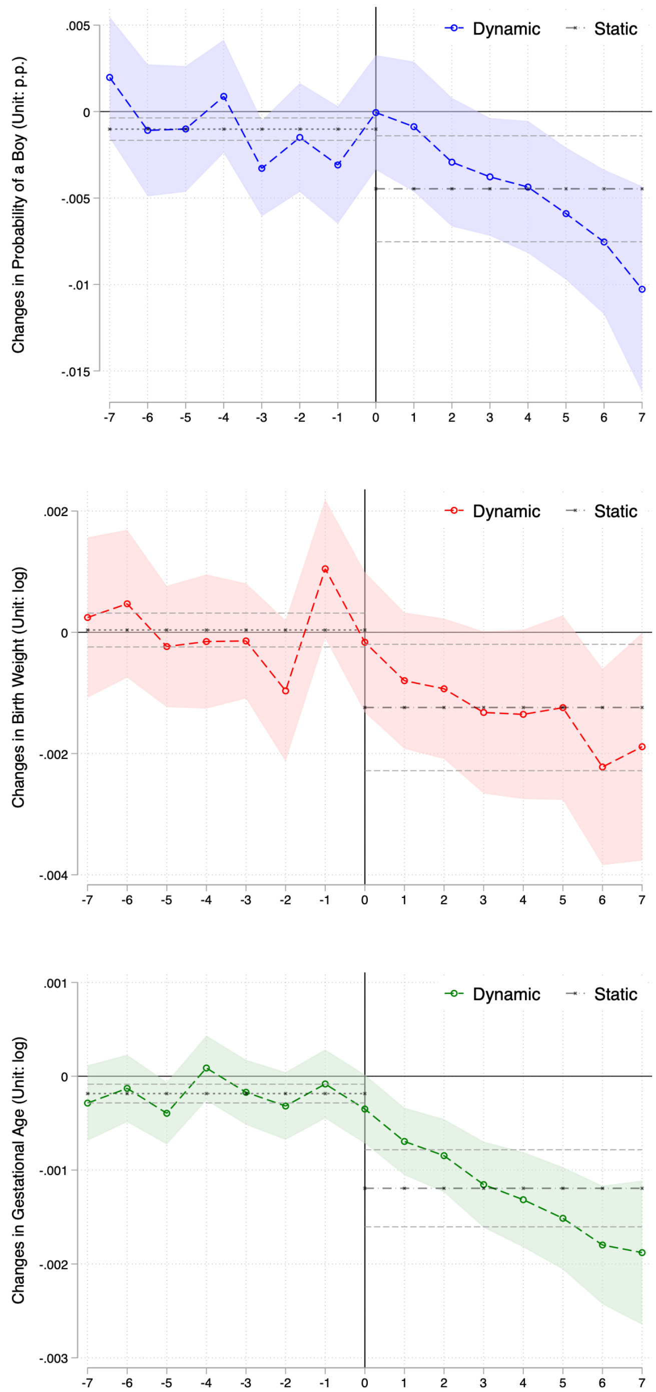

Figure A.9: Sex Ratio at Birth and Infant Health before and after Baby-Bonus Adoption (Larger Window)

Notes: This event-study figure plots the estimated changes in the probability that a newborn is a boy (top, in blue), birth weight in log kilograms (middle, in red), and gestational age in log weeks (bottom, in green) before and after pro-natalist cash-transfer policy implementation. The event-study coefficients are estimated based on equation 6 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in black dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across panels, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included and family characteristics are controlled for: dummy variables for mother's and father's educational attainment, age, occupation (including unemployment), and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate.

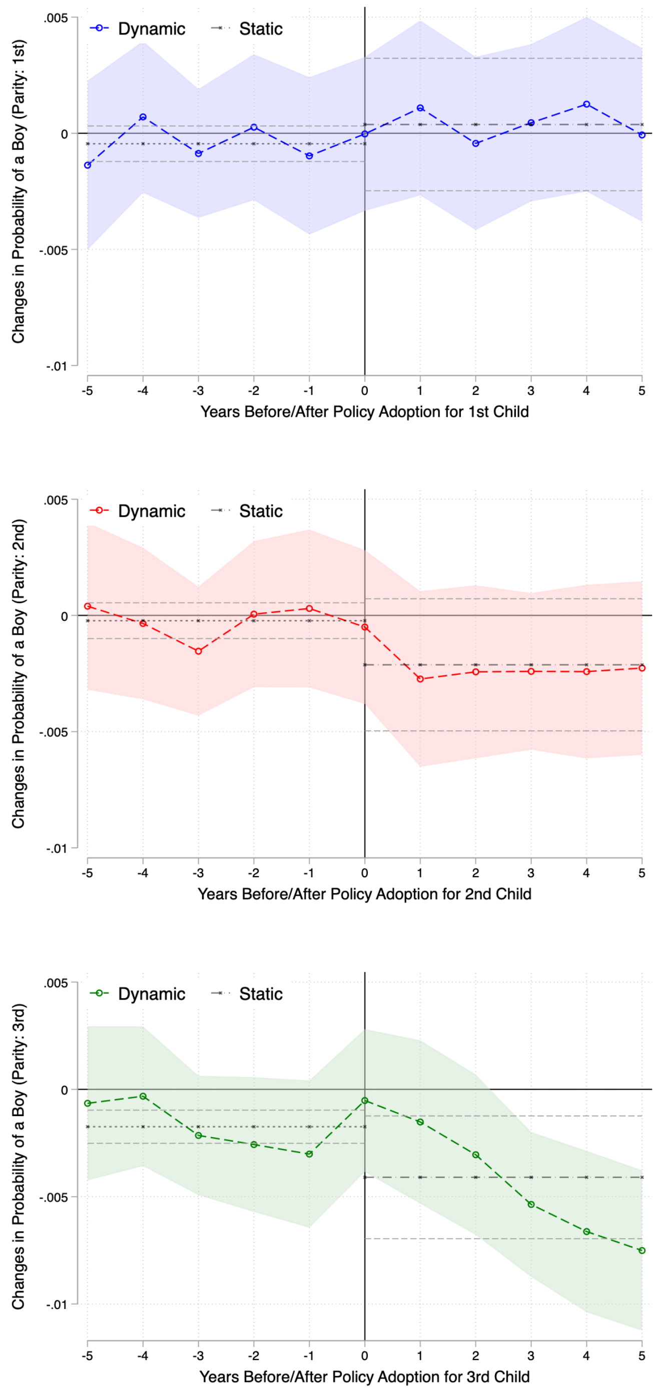

Figure A.10: Sex Ratio at Birth before and after Baby-Bonus Adoption by Parity

Notes: This event-study figure plots the estimated changes in the probability that a newborn is a boy among the first (top, in blue), second (middle, in red), and third (bottom, in green) children before and after pro-natalist cash-transfer policy implementation. The event-study coefficients are estimated based on equation 6 using the doubly robust difference-in-differences estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients in pre- and post-treatment periods are plotted in black dash-dotted lines. Standard errors are bootstrapped and clustered at the district level. Error bars show 95% confidence intervals. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across panels, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included and family characteristics are controlled for: dummy variables for mother's and father's educational attainment, age, occupation (including unemployment), and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\sinh^{-1}$ Cash Transfer for								
1st Child	-0.0010	-0.0054**	-0.0022	-0.0027	-0.0026	-0.0027	-0.0027	-0.0027
	(0.0016)	(0.0020)	(0.0024)	(0.0025)	(0.0025)	(0.0025)	(0.0025)	(0.0025)
2nd Child	-0.0037***	-0.0067***	-0.0037**	-0.0041**	-0.0040**	-0.0041**	-0.0041**	-0.0041**
	(0.0010)	(0.0011)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)	(0.0013)
3rd Child	-0.0247***	-0.0262***	-0.0245***	-0.0246***	-0.0245***	-0.0246***	-0.0246***	-0.0246***
	(0.0020)	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0019)
Observations	6,488,101	6,488,101	6,488,101	6,488,101	6,488,097	6,488,097	6,488,097	6,488,097
District FE		O	O	O	O	O	O	O
City-by-Year FE			O	O	O	O	O	O
District Characteristics				O	O	O	O	O
Parental Characteristics:								
Age					O	O	O	O
Education Attainment Level						O	O	O
Occupation							O	O
Marital Status								O

Table A.4: The Effect of Baby Bonus on Sex at Birth

Notes: This table replicates the results reported in Column 2 of Table 5 by gradually introducing fixed effects and control variables. The mean probability of a newborn being a boy among first children is 0.513%. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\sinh^{-1}$ Cash Transfer for								
1st Child	-0.0098***	-0.0148***	-0.0002	-0.0005	0.0004	0.0001	0.0001	0.0001
	(0.0012)	(0.0018)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)	(0.0009)
2nd Child	-0.0129***	-0.0144***	-0.0002	-0.0004	-0.0002	-0.0005	-0.0005	-0.0005
	(0.0012)	(0.0012)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
3rd Child	-0.0104***	-0.0110***	-0.0018***	-0.0019***	-0.0020***	-0.0025***	-0.0024***	-0.0024***
	(0.0007)	(0.0007)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Observations	6,488,101	6,488,101	6,488,101	6,488,101	6,488,097	6,488,097	6,488,097	6,488,097
District FE		O	O	O	O	O	O	O
City-by-Year FE			O	O	O	O	O	O
District Characteristics				O	O	O	O	O
Parental Characteristics:								
Age					O	O	O	O
Education Attainment Level						O	O	O
Occupation							O	O
Marital Status								O

Table A.5: The Effect of Baby Bonus on Birth Weight

Notes: This table replicates the results reported in Column 4 of Table 5 by gradually introducing fixed effects and control variables. The mean birth weight among first children is 3.192 kilograms. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\sinh^{-1}$ Cash Transfer for								
1st Child	-0.0053***	-0.0106***	-0.0005	-0.0004	-0.0002	-0.0003	-0.0002	-0.0002
	(0.0008)	(0.0012)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
2nd Child	-0.0086***	-0.0111***	-0.0015***	-0.0015***	-0.0013***	-0.0015***	-0.0015***	-0.0015***
	(0.0007)	(0.0008)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
3rd Child	-0.0071***	-0.0078***	-0.0016***	-0.0016***	-0.0016***	-0.0017***	-0.0017***	-0.0017***
	(0.0004)	(0.0004)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Observations	6,488,101	6,488,101	6,488,101	6,488,101	6,488,097	6,488,097	6,488,097	6,488,097
District FE		O	O	O	O	O	O	O
City-by-Year FE			O	O	O	O	O	O
District Characteristics				O	O	O	O	O
Parental Characteristics:								
Age					O	O	O	O
Education Attainment Level						O	O	O
Occupation							O	O
Marital Status								O

Table A.6: The Effect of Baby Bonus on Gestational Age

Notes: This table replicates the results reported in Column 6 of Table 5 by gradually introducing fixed effects and control variables. The mean gestational age among first children is 39.074 weeks. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Birth Weight < 2.7kg		Birth Weight > 4kg		Gestational Age < 37 weeks	
$\sinh^{-1}$ Cash Transfer	0.0013**		-0.0010***		0.0003***	
	(0.0005)		(0.0004)		(0.0006)	
×1st Child		0.0022*		-0.0004		0.0003
		(0.0011)		(0.0010)		(0.0014)
×2nd Child		0.0018**		0.0007		0.0037***
		(0.0007)		(0.0006)		(0.0007)
×3rd Child		0.0009		-0.0021***		0.0035***
		(0.0005)		(0.0005)		(0.0007)
2nd Child	-0.0033***	-0.0033***	-0.0013***	-0.0015***	0.0051***	0.0050***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0003)	(0.0003)
3rd Child	-0.0035***	-0.0033***	0.0094***	0.0097***	0.0096***	0.0095***
	(0.0005)	(0.0005)	(0.0004)	(0.0004)	(0.0005)	(0.0006)
Observations	6,488,097	6,488,097	6,488,097	6,488,097	6,488,097	6,488,097

Table A.7: The Effect of Baby Bonus on Low Birth Weight, Macrosomia, and Preterm Births

Notes: This table reports the estimated effects of baby bonus on the probabilities of low birth weight (Columns 1–2), macrosomia (Columns 3–4), and preterm birth (Columns 5–6). For each dependent variable, the left column reports the estimated effect of cash transfers unconditional on birth parity; in the column to the right, the cash transfers' effect is allowed to differ across birth parity. The mean incidence rates among first children are 4.57% for low birth weight, 3.17% for macrosomia, and 4.75% for preterm birth. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across columns, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included, and family characteristics are controlled for: dummy variables for mother's and father's educational attainment, age, occupation including unemployment, and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)
VARIABLES	log Birth Weight		log Gestational Age	
$\sinh^{-1}$ Cash Transfer	-0.0011**		-0.0016***	
	(0.0004)		(0.0002)	
×1st Child		0.0002		-0.0003
		(0.0009)		(0.0005)
×2nd Child		-0.0004		-0.0015***
		(0.0006)		(0.0003)
×3rd Child		-0.0017***		-0.0018***
		(0.0004)		(0.0002)
2nd Child	0.0046***	0.0046***	-0.0108***	-0.0108***
	(0.0003)	(0.0003)	(8.95e-05)	(9.54e-05)
3rd Child	0.0125***	0.0129***	-0.0102***	-0.0100***
	(0.0005)	(0.0005)	(0.000167)	(0.0002)
Indicator for Boy	0.0304***	0.0304***	-0.0033***	-0.0033***
	(0.0001)	(0.0001)	(3.71e-05)	(3.72e-05)
Observations	6,488,097	6,488,097	6,488,097	6,488,097
R-squared	0.015	0.015	0.042	0.042

Table A.8: The Effects of Baby Bonus on Birth Weight and Gestational Age Controlling for Baby's Gender

Notes: This table replicates the results reported in Columns 3-6 of Table 5 by additionally controlling for baby's gender and reports the estimated effects of cash transfers on log of birth weight (column 1-2) and log of gestational age (column 3-4). For each dependent variable, the left column reports the estimated effect of cash transfers unconditional on birth parity; in the column to the right, the cash transfers' effect is allowed to differ across birth parity. Each observation corresponds to a birth, and the total observations span the universe of births in South Korea from 2001 to 2015. Across columns, the same set of fixed effects (district fixed effects and city-by-month-year fixed effects) is included, and family characteristics are controlled for: dummy variables for mother's and father's educational attainment, age, occupation including unemployment, and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** at the 1% level, and *** at the 0.1% level.

	(1)	(2)	(3)	(4)	(5)	(6)
			log Birth Rates			
	1st Child		2nd Child		3rd Child	
$\sinh^{-1}$ Cash Transfer for						
1st Child	0.182***	0.177***				
	(0.0371)	(0.0376)				
2nd Child			0.0504***	0.0577***		
			(0.0094)	(0.0097)		
3rd Child					0.0394***	0.0411***
					(0.0096)	(0.0095)
Observations	3,330	3,330	3,330	3,330	3,330	3,330
Controlling for Migration	O	X	O	X	O	X

Table A.9: The Effect of Baby Bonus on Birth Rates Allowing Migratory Responses

Notes: This table reports the estimated effects of cash transfers on the birth rates for the first child (Columns 1–2), the second child (Columns 3–4), and the third child (Columns 5–6) based on equation 4. For each birth order, the left column includes the same set of fixed effects (district fixed effects and city-by-year fixed effects) and the same district-level control variables are included. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (aged 20 to 64), the percentage of the elderly (older than 64), the net migration rate (lag), the marriage rate (lag), indicators for the gender and political-party affiliation of the local-government head, and the financial-independence rate. In addition, Columns 3–4 (resp. 5–6) include the lagged number of births for the first child (resp. the first and second children) in log units; the right column reports the estimated effect of cash transfers while allowing migratory responses by excluding the percentage of the adult population (aged 20 to 64) and the net migration rate (lag) from the set of district-level control variables. Standard errors are clustered at the district level and reported in parentheses. * significant at the 5% level, ** significant at the 1% level, and *** significant at the 0.1% level.