Baby Bonus, Fertility, and Missing Women*

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August 16, 2023

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Abstract

Total fertility rates have declined across many developed countries. Many policies have been implemented in an attempt to reverse this trend. This paper presents novel causal evidence on the effects of one such policy—pro-natalist financial incentives offered in South Korea—on fertility, sex, and infant health. 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. 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 have a positive impact on birth weight for low-income families.

Keywords: pro-natalist policies; cash transfer, fertility, infant health, missing women, son preference.

JEL codes: H40, H75, I50, J13, J16, J18.

*I am grateful to Adriana Lleras-Muney, Kathleen McGarry, Youssef Benzarti, and Manisha Shah for their encouragement and guidance. I thank Libertad González, Rannveig Hart, Diederik Boertien, Brett McCully, Klaus Desmet, Daniel Millimet, Ömer Özak, James Lake, Michael Sposi, Gunnar Andersson, Paola Giuliano, Till von Wachter, Liyan Shi, and many seminar participants at UCLA, Collegio Carlo Alberto, Centre for Fertility and Health (NIPH Norway), Southern Methodist University, University of Oklahoma, University of Miami, DSE Winter School, 90th SEA meeting, 2021 Annual Conference of ESPE, 2021 CMES, 2021 EEA-ESEM Congress, 2021 and 2018 PAA meeting for helpful comments. This project was supported in part by the California Center for Population Research at UCLA with the 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 and the National Institutes of Health. All errors are mine.

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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). Policymakers have expressed growing concerns about the consequences of the resulting demographic imbalance (e.g., declining size of the labor force relative to the dependent population), which is further exacerbated by an aging population (Morgan, 2003; Frejka et al., 2010; Harper, 2014).[1] In this paper, I ask how financial incentives influence fertility. Furthermore, I investigate unintended consequences that will ultimately shape children's life course in the long run.

This paper provides novel causal evidence on the effects of pro-natalist cash transfers on sex ratio at birth and infant health as well as the number of births, leveraging temporal and spatial variation in the cash transfers and the universe of birth, death, and migrant registry records from South Korea. I investigate whether the introduction of a pro-natalist cash-transfer policy had an impact on birth rates, how elastic birth rates are with respect to cash transfers, and how the effect varies by birth order and mother's age. Next, I study the unintended consequences of the cash transfers on sex composition and infant health (birth weight, gestational age, and early-life mortality). Lastly, I explore potential mechanisms explaining the results: temporal adjustment of fertility, spatial sorting of families, heterogeneity of the cash transfer effect by parental characteristics, and selection into childbearing.[2]

South Korea—the empirical setting of this paper—offers a well-suited environment to study pro-natalist cash transfers, which provide cash awards to parents when they have a baby. There is rich variation in both implementation timing and the amount of cash transfers by *birth order* (first, second, and third), over *time* (year), and across *granular spatial units* (hereon referred to as districts).[3] 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 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 effects on birth rates and merge this data set with the universe of confidential birth registry records to study the effects on 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 effect on early-life mortality.

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

[2] An increase in the number of births may be driven by son preference (Basu and de Jong, 2010) or may simply reflect the temporal adjustments of childbearing (Andersen et al., 2018) and the spatial sorting of families (Tiebout, 1956). Access to better prenatal care from the *prospect* of receiving the cash transfer may improve health outcomes at birth (Almond, 2006; Aizer, 2011; Hoynes et al., 2015). An increase in the number of births may bring about poor "child quality" from the quantity-quality tradeoff as in Becker's human fertility model (Becker, 1960; Becker and Lewis, 1973; Becker and Tomes, 1976) or from families responding to the financial incentives who are negatively selected into childbearing in the dimensions that affect the children's outcomes. Changes in birth quality may affect children's long-run outcomes (Almond and Currie, 2011; Almond et al., 2018).

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

For causal identification, I include both district fixed effects and the city-by-time fixed effects in the estimating equations throughout this paper. The district fixed effects purge out 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 (e.g., a year or a month) as well as any aggregate shocks taking place at the national level, flexibly capturing local shocks changing over time; for instance, local labor, housing, and marriage market conditions. I further introduce time-varying district-level characteristics as control variables that are likely correlated with both cash transfers and birth outcomes.[4] Then, the residual variation in the cash transfer implementation timing and generosity is explained by some arguably random factors, such as the distribution of policymakers' subjective belief about the efficacy of and their political returns from the baby bonus and idiosyncratic errors in forecasting local fiscal capacity to operate local pro-natalist cash transfer programs. The key identification assumption is that this residual variation is orthogonal to all other determinants of birth outcomes, conditional on observed control variables as well as the rich set of fixed effects.

I find that the pro-natalist cash transfers 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 by 1.6 to 5% across birth orders. The birth rates increased for mothers in their prime age of childbearing. Leveraging the variation in the cash transfer generosity, I estimate the elasticity of birth rates with respect to the generosity of the baby bonus. I find that a 10% increase in the cash transfers raised birth rates by 0.58%, 0.34%, and 0.36% of the first, second, and third child, respectively. A back-of-the-envelope calculation implies that, in the absence of the cash transfers *ceteris paribus,* the total fertility rate would have been 4.7% lower than 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.[5]

I provide further evidence that the increase in the birth rates was driven by changes in the childbearing decisions of parent(s) at the margin of having an additional child. I uncover two new, important effects of these pro-natalist cash transfer programs. First, the cash transfers for a birth order affected only the birth rate of the corresponding birth order but not opf the others. 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 elasticities with respect to the cash transfers for a first or a third child are small in their magnitudes and statistically indistinguishable from zero. Second, the elasticities of cash transfers were positive only among mothers between the ages of 20 and 39, active in 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 on the sex ratio at birth. These cash transfers modulated the sex ratio which initially favored boys due to son preference, and decreased it

[4] I employ an accelerated failure time model and conduct a regression analysis and investigate the determinants of policy implementation timings and generosity in Section 2.3.

[5] Further, recovering the 2.1 replacement level would require a substantial increase in the cash transfer generosity by 10 to 16 times to a level equivalent to the median annual household income.

toward the natural sex ratio at birth. I document an unnaturally high male-to-female ratio at birth particularly among the third child (i.e., 121 boys for 100 girls according to the birth records from 2000 to 2015)—apparent evidence of "missing baby girls," arising likely from sex-selective abortion and infanticide.[6] The sex ratio at birth among the third children, however, showed a dramatic decline from a high sex ratio favoring boys in 2000 and reached a level consistent with the natural ratio (105 boys for 100 girls). Based on the universe of confidential birth registry records from 2001 to 2015, I find that higher cash transfers lowered the probability of a third child being a boy. The estimated elasticity implies that, in the absence of the cash transfers while 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 declined observed in 2015.

The cash transfers also had the unintended consequences on infant health. I estimate statistically significant negative effects of the cash transfers on birth weight and gestational age. These effects were concentrated among the higher-order births. For example, doubling the cash transfer generosity for a third child decreased the birth weight among the third children 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 study the longer-term effect of the cash transfer programs on early life mortality and 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 1 and 5.

I study potential mechanisms explaining the results of this paper. 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 and/or an unemployed father increased in cash-transfer generosity. Particularly given the wide gap in the employment rates between mothers and fathers, families with an unemployed father are likely low-income households. I provide evidence that this selection into childbearing in terms of income and other unobserved parental characteristics shaping infant health may explain why the effects of the pro-natalist cash transfers on birth weight and gestational age are negative. The results also hint that the effect of cash transfers on birth weight may be positive, at least among low-income families, if the change in gestational age arising from cash transfers were properly controlled. Lastly, I 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 existing literature in economics analyzing the effects of pro-natalist policies on fertility.[7] Lalive and Zweimüller (2009) found that the extension of parental leave in

[6]A plethora of papers repeatedly finds that the natural sex ratio at birth is 105 males to 100 females: e.g., Jacobsen et al. (1999). Furthermore, I use the 2015 Population Census, where 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 among the third child 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.

[7]Many policies to boost childbirth, 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

1990 in Austria increased the probability of women having an additional child, while Andersson and Duvander (2006) found no such effects in Sweden. There is a large literature finding small or no effects of U.S. tax policies and welfare programs benefiting families with children on fertility decisions (Whittington et al., 1990; Whittington, 1992; Crump et al., 2011; Rosenzweig, 1999; Kearney, 2004).[8] The previous literature on cash transfer for childbearing has mainly focused on difference-in-difference strategies and compared the fertility outcomes before and after cash transfer implementation and 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).[9] González (2013) and González and Trommlerová (2021) estimated a positive and persistent fertility effect of a universal child benefit in Spain based on a regression discontinuity design strategy. Hong et al. (2016) examined the same local government transfers in South Korea as in this paper, but for a shorter period of time from 2005 to 2011, and provide suggestive evidence that the local policies in South Korea may have increased the birth rates, not necessarily the completed fertility.[10]

This paper contributes to the aforementioned literature by offering a set of novel insights about pro-natalist cash transfers. Estimating the elasticities of birth rates 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: 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 is not transitory, resulting in increasing completed fertility. Albeit effective, the cash transfers alone may not be sufficient (or cost-effective) to raise the fertility rate back to the 2.1 replacement level.

Second, 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 Asian context.[11] Focusing on South Korea, Choi and Hwang (2020) show that son preference has been diminishing in recent years.[12] The findings of Yoo et al. (2016) also suggest a decline in son preference, but report that a decision to have a third child depends largely on the sex composition of the first and second children. Jayachandran (2017) estimated 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 are associated

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. (2023) for a literature review.

[8] Laroque and Salanié (2004) examined 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.

[9] 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.

[10] There is a related literature on abortion, which looks at the impact of a ban and legalization of abortion on the long-term outcomes of children. See Donohue and Levitt (2001) and Pop-Eleches (2006).

[11] See Bongaarts (2013) for a review on 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.

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

with higher male-to-female ratios. Anukriti (2018) studied the "Devirupak Scheme," which provided cash transfers based on the number of children and sex composition in India. She found that son preference intensified while total fertility declined. With fewer kids, families placed a higher value on having a son over the financial incentives associated with having girls.[13] 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 male-to-female ratio among the third children. I show that the baby bonus contributed to this decline, although the baby bonus was not designed to modulate the sex ratio, unlike the Devirupak Scheme in India. To the best of my knowledge, this paper is the first to show that a pro-natalist policy interacts with son preference and unintentionally alleviates sex-ratio imbalance in a cultural context where having a son is favored over having a daughter.

Third, this paper fills the gap in the literature by exploring the broader implications of financial incentives for having babies. There are growing interest and literature on the effects of pro-natalist policies on infant health, childhood and adulthood outcomes of the affected babies.[14] There are only a few papers that study the effects of pro-natalist cash transfers on a wide range of outcomes, other than the number of births. For example, González (2013) estimated the effects of the introduction of a universal child benefit in Spain on fertility, household consumption, and maternal labor supply. Yakovlev and Sorvachev (2020) found that child subsidies aimed at increasing fertility in Russia indeed increased the total fertility rates and produced substantial general equilibrium effects on the housing market and family stability. Milligan and Stabile (2011) showed that child benefits had positive effects on children's educational outcomes and mental health in the case of Canada. In contrast to these papers, I study whether the pro-natalist cash transfers affected early life outcomes of the babies born under this policy (i.e., birth weight, gestational age, and mortality) that are strong predictors for long-run outcomes (e.g., education attainment, labor market performance, and crime).[15] I find that the effect on infant health is negative and provide suggestive evidence that this is partly driven by the selection into childbearing by low-income families. Furthermore, this paper studies the effect of pro-natalist cash transfers on the sex ratio at birth, which will eventually shape the market conditions in the long run, such as the marriage market (Guilmoto, 2011).

The remainder of the paper is organized as follows. In Section 2, I provide institutional background on the pro-natalist policies in South Korea and the data sources. Section 3 describes the

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

[14] There is a large and robust literature on the effect of parental leave policies on children's outcome. Rossin (2011) and Stearns (2015) estimated positive effects of maternity leave on birth weight in the U.S., driven by more educated and working mothers in the former and disadvantaged mothers in the latter. Carneiro et al. (2015) show that the maternity leave benefit in Norway led to a decrease in the high school dropout rates and increased adulthood earning. Some find no effect of parental leave polices on children's outcomes in terms of 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 on maternity leave polices.

[15] There exists a large literature establishing the causal links between neonatal/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 (e.g., parental education, incarceration, income, and family structure) on a range of child's outcomes (Black et al., 2005; McCrary and Royer, 2011; Oreopoulos et al., 2008; Milligan and Stabile, 2011; Aizer and Doyle, 2015).

empirical strategies to identify the causal effects of the pro-natalist cash transfers on birth outcomes. 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 with pro-natalist cash transfer (Baby Bonus) policies and demographic and other relevant local characteristics, and the number of births.[16] To investigate the effects of cash transfers on sex, health outcomes, and early-life mortality, I merge this dataset 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 in South Korea, explain the sources for the main data sets and measurements, and investigate the determinants of the policy implementation timing and cash-transfer generosity.

2.1 Background

Before the 1960s, South Korea had a high fertility rate of above six children per woman. However, after pursuing one of the most fervent and successful family planning policies for over 20 years, the fertility rates have stayed below the 2.1 replacement level since 1983 (Lee and Choi, 2015). Fertility continued to decline until 2005 when the total fertility rates reached a historic low of 1.05 children per woman. In response to growing concerns arising from low fertility rates and the rapidly aging population, the national government of South Korea established the First Basic Plans for Low Fertility and Aged Society in 2006, followed by a series of revised plans after 5 years thereafter. The Plans outlined normative goals and operate at the national level.[17] In line with the administrative arrangements of the national and local governments (Local Autonomy Act, 1990), national government policies on welfare, generally speaking, are implemented at the macro-level

[16]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 222 districts that did not undergo redistricting and construct a balanced panel of districts. These districts belong to 15 cities (i.e., metropolitan cities and provinces). The final sample represents over 95% of the South Korean population. In Appendix, Figure A.1 plots the map of South Korea and shows 222 districts in 15 metropolitan cities and provinces (in different colors).

[17]There are a few areas the national government made progress. Paid parental leave was first implemented in 2001 with a monthly payment of 300,000 KRW (265 USD) up to 1 year, irrespective of income level. Benefits gradually increased over time reaching 1,000,000 KRW (883 USD) in 2011 for some income groups (Kim et al., 2022). Both mothers and fathers were eligible, but the total leave combining the mother's and father's should not exceed 1 year. The uptake among fathers have been very low, while the average total length of the maternity and paternity leaves combined has been consistently less than the full benefit duration of 1 year (Lee, 2022). Starting from 2018, the national government has offered pro-natalist cash transfers. During the time period this paper focuses, the national government did not implement pro-natalist policies or revise other welfare programs, public childcare, and healthcare that may affect childbearing decisions.

and may not vary across districts and cities.[18] Therefore, it is reasonable to assume the national pro-natalist policies indiscriminately affect districts uniformly.

As early as 2001, local governments started to adopt a pro-natalist cash transfer policy, which provided cash transfers to families with newborn babies. By 2012, all the districts have adopted this policy.[19] The structure of this policy (e.g., eligibility and transfer method) is virtually identical across districts. Every family with a newborn baby was eligible to receive baby bonus in their district of residence, unconditional on family earning 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 and verify that they had residence in the district.[20] Most beneficiaries received a one-time lump-sum transfer within a few weeks from claiming the benefit.[21] During the sample period of interest in this paper (i.e., 2001 to 2015), the baby bonus was the only pro-natalist policy implemented at the local level; it was well publicized to raise the public awareness in the form of public announcements, street posters, fliers, and mails.

Notwithstanding the common structure of the policy, the amount of cash transfers varied widely across districts and by birth orders. The cash transfers in 2015 ranged from 0 to 5.1 million KRW (or approximately 4,505 USD) for a first child, 0 to 7.54 million KRW (5,703 USD) for a second child, and 200,000 KRW (77 USD) to 18.8 million KRW (16,608 USD). In most cases, the cash-transfer generosity increased in birth order. In 2015, the average cash-transfer amounts were 770,000 KRW (or approximately 680 USD) for a first child, 1,060,000 KRW (or 926 USD) for a second child, and 2,660,000 KRW (2,350 USD) for a third child.[22] 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 its baby bonus scheme on an annual basis. Each renewal often resulted in increased generosity. Considering the ubiquitous concern regarding the declining fertility across districts and the national sentiment toward pro-natalism, the local baby bonus programs may have been a policy agenda convenient for local politicians to increase their public appeal as well as to boost fertility.

Figure 1 summarizes the local pro-natalist cash transfer policies, the total fertility rates, and

[18] The Plans of the national government "set abstract goals and directions, [and] did not specify guidelines for local policy formulations" (Kim, 2013).

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

[20] 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 given establishing residency in a new district is not trivial.

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

[22] On average, the cash transfer generosity in 2015 was modest, compared to the median monthly income of 3.5 million KRW for a 4-person household and to the average monthly female wage of 1.8 million KRW (only about 63% of the male wage). In this paper, I focus on the first, second, and third children because the number of families with more than 3 children is rare in South Korea. In most districts, cash transfer benefit does not increase after 3rd child, while a few district have more generous cash transfers for babies beyond the third child.

male-to-female sex ratio at birth. The top panel plots the fraction of districts with the cash transfer policy (dashed line) and the average amount of cash transfers conditional on having adopted the policy (solid line) for districts providing cash transfers to families. 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, baby bonus is available across all districts. Similar to the policy adoption rate, the average amount of cash transfers across districts increased over time. In the center and bottom panels, average total fertility rates and the male-to-female ratios at birth (solid lines) are plotted along with cash-transfer prevalence (dash line; reproduced from the top panel) over time. As more districts begin to offer bonus, the decline in the average fertility rates seem to stop and reverse the trend.. During the same period, the sex ratio at birth declined to the natural ratio of 1.05 boys.[23] I explore the extent to which these associations that the local cash transfers have with the fertility rate and the sex ratio at birth may be causal.

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 pro-natalist cash transfers. Because the policies are enacted and implemented at the local (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 child.[24] Based on the responses from local governments and cross-validation using alternative sources (e.g., administrative policy reports, online repository of local ordinances, and regulations and interviews), I build a yearly panel data set of districts from 2000 to 2015 with the amounts of baby bonus.

Panel A of Table 1 provides the summary statistics of cash transfers by birth order for selected years. The proportion of districts providing the pro-natalist cash transfers 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, minimum, and maximum values of cash transfer amounts in 1,000,000 KRW (883 USD) for districts with strictly positive cash transfers amounts (i.e., excluding zero). They show rich variation in cash transfer generosity over time (columns), across space (standard deviations, minimum, and maximum), and by birth order (rows).[25] The changes in cash transfer generosity over time can be explained both by more districts adopting

[23] Prior to 2000, the sex ratio at birth had been relatively stable at around 1.1 boys for each girl since 1996.

[24] There are several other papers that have collected the same set of information independently of this paper (Kim, 2013; Hong et al., 2016) from an online repository of local ordinances and regulations operated by the Ministry of the Interior of South Korea (`www.glis.go.kr`) and telephone surveys. The Ministry of Health and Welfare of South Korea has published the Annual Case Study of Local Government Population Policies since 2008. I further verify the accuracy of the official information I received from each district government by comparing it to these alternative sources and confirmed any discrepancies with individual government officials in charge of the pro-natalist cash transfers in their jurisdictions. Birth parities from first to third as they together constitute over 98.9% of the total births in South Korea during the sample period.

[25] To illustrate the spatial and temporal variation in the cash-transfer adoption timing and generosity, I present a series of maps plotting the cash-transfer generosity for each birth order in 2005, 2010, and 2015 in Figure A.2.

the policy with higher transfer amounts and those already offering baby bonus and increasing the generosity.

The second set of variables relates to the number of births in each district. To understand how the effects of cash transfers differ 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 a restricted access confidential birth registry records housed at the Bureau of Statistics of South Korea. The data spans the universe of births registered in Korea from 2000 to 2015, I count and export the number of births by birth order and mother's age group (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.

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 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.[26] Panel B of Table 1 reports the means and standard deviations of the total fertility rates and the order-specific birth rates computed across districts for selected years.

Lastly, I supplement the data set with district-level characteristics from various administrative data sources: KOSIS, Finance Integrated System, and the 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 controls. The demographic characteristics 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 (i.e. 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 how fiscally autonomous a local government

[26]Note that total fertility rate $TFR_{d,y}$ can be expressed as a function of the age- and order- specific birth rates. That is,

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

is.

Individual-Level Records: Birth Outcomes and Early-Life Mortality

I use restricted-access confidential birth registry records, spanning the universe of births registered in South Korea from 2001 to 2015, to study the effects of baby bonus on sex, birth weight measured in kilograms, and gestational age measured in weeks at the individual (birth) level. Each record includes detailed information on the newborn baby (e.g., date and place of birth, birth order, sex, birth weight, and gestational age) and the parent(s) (e.g., age, educational attainment level, occupation, and marital status).[27] The total sample size is 7,080,381 births of 1st, 2nd, and 3rd child,. Table A.1 reports the average birth weight, gestational age, and fraction of male births by birth order in different years. There are several notable patterns that 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 averages. Second, albeit the differences are subtle, the difference is larger for higher order births. Third, the fraction of boys among first births is stable at the natural sex ratio at birth and ranges narrowly 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 5. Based on these birth-death matched data, I define two indicator variables measuring early life mortality: one equal to one if the baby died before reaching the age of 1 (infant mortality); the other equal to one if the baby died before the age of 5 (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.

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 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

[27] While the records span the universe of births and includes a rich set of family characteristics, they do not include personal identifiers of parents, so which babies share the same parent(s) cannot be identified. Therefore, I cannot observe the fertility history of each mother. This also means that the sex composition of previous children cannot be observed. Educational attainment level and occupation are categorized differently in some years. By aggregating smaller categories, I create variables measuring educational attainment level and occupation, which are used as control variables and for heterogeneity analyses. 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/out-of-labor-force). Note that the occupation is measured at the time of childbirth. For instance, if a mother was on a maternity leave, then her occupation would be still recorded. Marital status is an indicator variable which takes the value of 1 if information for both parents is provided and the value of zero otherwise.

district-level policies. For causal analysis, it is important to understand, for instance, whether districts adopted the pro-natalist cash transfers early and offer more generous baby bonus when they had suffered from low fertility rates and thus been 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 τfollows 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 $T_\tau > 0$ 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:

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

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

Table 2 summarizes the results estimating Eq. 1 for baseline years from 2000 to 2006. Most of the observed demographic characteristics (e.g., 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 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 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 offer baby bonus. These districts may have been less concerned about the declining number of births because a greater number of the population had the potentials 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 affecting intra-houshold 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 cash-transfer generosity and estimate a straightforward 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 cash transfer amounts provided to families with a new baby of birth order p in district d in year y.[28] 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 (e.g., labor market conditions, which in turn affect local government budget); $X_{d,y}$ includes explanatory variables including demographic and local government characteristics in district d observed in year y.

Table 3 summarizes the results estimating Eq. 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 policy generosity changes over time. The right columns exclude district-year observations prior to policy adoption. Under this sample restriction, I focus on the intensive margin of cash transfers and study the factors that explain the differences in cash transfer amounts across districts and the generosity changed over time. When looking at both extensive and intensive margins together (Column 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 cross birth orders. In contrast, all of the estimated coefficients for these variables in Column 2, 4, and 5, lose their statistical significance. The 1-year lagged value of total fertility rate is positively correlated with the cash-transfer generosity only for first child, while this relationship is not statistically significant for 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 is more aggressive with their pro-natalist baby bonus programs.

While most of the coefficient estimates individually may not explain the variation in cash transfers, joint hypothesis testings indicate that these factors may be jointly correlated with the observed policy variation.[29] These local characteristics (e.g., fraction of female population, age composition, and cultural norm proxied by the 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.

[28] The inverse hyperbolic sine transformation approximates the natural logarithm of the cash transfer amounts and allows retaining observations with zero values (e.g., districts prior to the policy implementation).

[29] 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 post policy implementation. I cannot reject the null hypothesis at the 5% significance level for cash transfers for a second child, but reject the null hypothesis at the 5% significance level in case of cash transfers for first and third child.

3 Empirical Strategy

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

3.1 District-Level Analysis: Number of Births

Based on different implementation timing of the pro-natalist cash transfer policy in each district, I employ an event-study framework and semi-parametrically estimate the pro-natalist policy effects before and after based on the specification as 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 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 the metropolitan city-level shocks as well as the national-level shocks that may be correlated with birth rates and local policies (e.g., local labor and housing market conditions). $X_{d,y}$ is a set of district-level time varying characteristics.[30] $\left\{D_{p,d,y}^{(\tau)}\right\}_{\tau=L}^{U}$ 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.[31] $\epsilon_{p,d,y}$ is an error term. Event study coefficients $\left\{\gamma_p^{(\tau)}\right\}_{\tau=-L}^{U}$ measure the percent change in birth rates for p-th birth order τ years before and after the adoption of the cash transfer policy for 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 to each event year τ to different event years before and after policy implementation.

Estimation of Eq. 3 by OLS may not identify the average treatment effects of baby bonus in this empirical setting as the timings of policy implementation across districts are *staggered* over different years.[32] I follow Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021) and estimate Eq.

[30] 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 lag number of births of birth order $p' = p - 1$ when estimating the cash transfer 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 may potentially benefit from the pro-natalist cash transfers. As a result, the total observation is equal to 15 years from 2001 to 2015 times 222 districts, which is equal to 3,330.

[31] The maximum years before and after policy implementation $[L, U]$ are $[-10, 8]$ for the first child and $[-14, 11]$ for the second and third child.

[32] See Goodman-Bacon (2018), de Chaisemartin and D'Haultfœuille (2020), Sant'Anna and Zhao (2020), Callaway

3 using an estimation method robust to potential biases from staggered introduction of treatment under two identifying assumptions. The first assumption is the parallel trend assumption: i.e., in the absence of policy implementation, birth rates would have changed, on average, the same way across districts. Second, the adoption of baby bonus is not anticipated.[33]

Next, I exploit the rich variation in the cash transfer generosity to estimate the elasticity of birth rates with respect to cash incentives. Instead of taking the log transformation of the cash transfer 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 Eq. 3. Similarly, I introduce the same time-varying district-level characteristics as control variables. Coefficient β_p captures the effect of pro-natalist cash transfers on birth rate of birth order.[34]

For causal interpretation, the identification assumption is that, absent pro-natalist cash transfers, birth rates 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 (Eq. 5) is likely to hold in my analysis. Unlike a randomized control trial, 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 policymakers and may systematically explain the variation in cash transfer generosity. For example, districts with a relatively higher adult population, conditional on the total population, are more likely to have newly wedded couples who are active in childbearing decisions. At the same time, a higher fraction of the adult population (net of the elderly population) implies a larger tax base that allows for bigger budgets, which in turn are used to finance pro-natalist cash transfers. Therefore, omitting the proportion of adults in the population would overestimate the cash transfer effect on birth rates. The same is true for omitting marriage rates and factors related to the local government budget. Lastly, districts are geographically granular and the local labor and housing markets are usually defined in terms of a city (or a group of cities). The district fixed effects net out the time-invariant

and Sant'Anna (2021), and Borusyak et al. (2021) for recent literature on two-way fixed effects difference-in-difference estimators, which address the identification issues with a staggered treatment setup.

[33]The current state-of-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 Eq. 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 by the doubly robust difference-in-difference estimator proposed in Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021).

[34]In 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.

district-level differences that contribute to the differences in birth rates and the provision of baby bonus. The city-by-year fixed effects absorb shocks to these local market conditions, which would affect both people's childbearing decisions and local government's capacity to operate pro-natalist cash transfers. Conditional on these factors and the rich set of fixed effects, the residual variation in the cash transfer generosity is *plausibly exogenous.* For instance, policymakers across districts may have personal beliefs on the need and efficacy of pro-natalist cash transfers. In addition, it is impossible to perfectly forecast local government revenue for the next fiscal year and may under or overestimate its fiscal capacity to offer baby bonus. Such idiosyncratic factors explain the residual variation.

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

I investigate whether the baby bonus had any effects on the sex ratio at birth and infant health, which are important determinants of long -term individual and economy-wide outcomes (e.g., labor market performance and marriage market conditions). The sign of these effects are theoretically ambiguous. For instance, the cash transfers may adversely affect health outcomes at birth if cash transfers resulted in less investment per child due to increased number of births. They may improve infant health as cash transfers serve as an additional resource to take better care of newborn babies. This would be particularly the case for mothers who would have had children anyways in the absence of the cash transfers. With respect to the sex ratio at birth, while on a downward trajectory, son preference in South Korea remains strong.[35] The baby bonus may provide financial means to parents as they continue to have babies until they have at least one boy or simply more boys. It may compensate for the utility penalty associated with having girls and mitigate sex-selective abortion. To estimate the effects of pro-natalist cash transfers on the sex ratio at birth and infant health, I apply the same set of empirical strategies to the individual-level data based on the universe of birth records.

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

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

where H_i is a measure of an infant's sex and health (e.g., an indicator for a boy, birth weight, and gestational age) of baby i of birth order p born in district d in year-month $(y,\ m)$. The year-month-by-city fixed effects $\psi_{c(d),m,y}$ flexibly control for the month-to-month citywide shocks that affect birth outcomes.[36] In addition to the same set of control variables $X_{d,y}$ as in Eq. 4,

[35] The extent to which the sex ratio at birth deviates from its natural level of 105 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.

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

I leverage the parental information reported on each record and include a set of individual-level controls W_i including indicators for a child's birth order and parental educational attainment, age, marital status, and occupation types. ϵ_i is an error term. Then, event study coefficients $\left\{\gamma^{(\tau)}\right\}_{\tau=-L}^{U}$ measure change in the outcome of interest τ years before and after the adoption of the cash transfer policy.

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

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

where the same set of fixed effects and control variables are included, but the event-study dummies are replaced with $\sinh^{-1} CT_{p,d,y}$, the inverse sine transformed values of cash transfer generosity. Then, coefficient β measures the effect of cash transfers 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. The identification assumption is expressed as follows:

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

The use of registry records in my analysis is particularly advantageous to justifying the identification assumption (Eq. 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.[37]

4 Results

In this section, I present my estimation results in two parts. First, I discuss the district-level analysis investigating the effects of the pro-natalist cash transfers on the birth rates. The second part presents the estimation results on the effects of the cash transfers on the sex ratio at birth and infant health based on the individual birth registry records.

et al., 2015).

[37] In addition to an infant's sex and health outcomes, I use the birth-death matched records and estimate the effect of cash transfers on infant mortality. The matched records are only available for infants born between 2010 and 2013, during which time most districts had already adopted the policy. Because the cash transfer generosity often changed after the initial implementation, there still remains both cross-sectional and temporal variation in the cash transfer generosity, even when focusing on the samples between 2010 and 2013. Figure A.3 (bottom panels) in the Appendix plots the distribution of the residual variation in cash transfers for this time period while controlling for the observable characteristics and gradually adding the fixed effects. Comparing them to the top panels in which the all years are used, the residual variation is smaller, and the cross-sectional variation across districts is likely to contribute more to this variation.

4.1 District-Level Analysis: Birth Rates

Event Study Results.

I begin by presenting the event study results. For each birth order $p = 1, 2,$ and 3, I estimate Eq. 3 and Figure 2 plots the estimated event study coefficients $\left\{\gamma_p^{(\tau)}\right\}_{\tau=-5}^{5}$.[38] In the top panel, the changes in the birth rates of the first children relative to the birth rates 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 child stayed relatively constant until the policy implementation. However, the birth rates started to gradually increase since the cash transfer programs for a first child were implemented; all of the event study coefficients post implementation are positive and statistically different from zero. On average, the birth rates for the first child increased by 5.0%.

The middle panel and bottom panel of Figure 2 plot the event study coefficients for the second and third birth orders, respectively. The birth rates for both second and third children were relatively constant prior to the policy implementation. Thus, I conclude that the second and third child birth rates showed no pre-trend prior to when districts started to offer baby bonus. Upon the policy implementation, the birth rates started increasing. The birth rates for the second and third children increased by 1.6% and 4.6%, respectively.[39]

The event study results provide evidence that the birth rates across birth orders increased after districts started offering the baby bonus. The estimated increase in birth rates ranging from 1.6% to 5.0% is close to the increase in fertility by 5% estimated based on the 2007 introduction of a universal child benefit in Spain in González (2013). The increase in the birth rate for the first children implies not only a greater number of births of that corresponding birth order, but also an increase in the number of families who 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, who in turn became potential beneficiaries of the cash transfers for a third child. It is important to note that the estimated coefficients across birth orders tend to increase, likely due to the increase in cash transfer generosities over time within each district.

Next, further decomposing the birth rates into the 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

[38] The event-study coefficients are estimated for all 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 fewer number of districts as the event time is further away from event time zero. In Appendix, Figure A.4 extends the event window to $[-7, 7]$.

[39] In 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 child respectively.

of mothers who are prime for childbearing, between the ages of 20 to 39 and more likely to be actively making fertility decisions. The first-child birth rates increased across the age groups; the effect is particularly stronger among relatively younger mothers. Although modest in magnitude, the estimated changes after policy implementation for the second-child birth rates are positive and statistically significant among mothers between the ages of 30 and 34. Lastly, the third-child birth rates among mothers between the ages of 25 and 29 increased after the policy implementation. Mothers, who already have two children at a relatively younger age seem to respond to the cash transfers and have a third child. Also, third child birth rates increase among mothers between the ages of 35 and 39.

Based on the event study estimation 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 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 Appendix shows that the birth rates among mothers between the ages of 15 and 19 and between the ages of 40 and 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 terms of generosity (i.e., how much cash transfers were provided) across districts over time. In the next section, I leverage the variation in cash transfer generosity and estimate the elasticities of birth rates with respect to cash transfer generosity.

Elasticity of Birth Rates to Cash Transfers.

I report the results estimating Eq. 4 for each birth order in Table 4.[40] 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:

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

Thus, I can re-scale the coefficient β_p in Eq. 4 by adjustment factor $\rho(p)$ to compute elasticities. I evaluate the adjustment factors based on the average cash transfers in 2015 (i.e., $\bar{CT}_p = E\left[CT_{pdy}|y=2015\right]$).[41]

[40] In Appendix, I report the results estimating a naive specification without any fixed effects and control variables and gradually add the fixed effects and control variables in Table A.2.

[41] These average values are 0.34 for first child, 0.93 for second child, and 2.66 for third child, which translate to the values of adjustment factors equal to 0.3189, 0.6826, and 0.9362 for first, second, and third child, respectively. Alternatively, Bellemare and Wichman (2020) propose multiplying a large constant to a variable before taking the inverse hyperbolic since transformation. The implied elasticities based on their method (e.g., multiplying CT_p by

In Column 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 first child on birth rates for this birth order is positive and statistically significantly different from zero at the 0.1 percent significance level. The estimate implies that a 10% increase in the cash transfers for the first child increases the birth rate of the first child by 0.58% after applying the adjustment factor as shown in Eq. 9. Should parents be forward-looking and base their decision to have their first child on the cash incentives offered for higher birth orders, birth rates for the first child would be affected by cash transfers for the second and third child. To test whether this is the case, I additionally introduce cash transfers for the second and third child to the estimation. In Column 2, the coefficient estimates for 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 statistically not different from zero at the 5 percent significance level.

The results for the second child, reported in Column 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 in magnitude compared to the elasticity of birth rates for the first child birth with respect to cash transfers, implying that it takes a greater financial incentive to encourage families to have two children than on child. In Column 4, I additionally introduce the cash transfers provided for the first child and the third child. The coefficient estimate for cash transfers for the second child is robust to these additional control variables and change little from Column 3. None of the coefficient estimates of the cash transfers to the other birth orders are statistically different from zero at the 5% significance level. First, this result is in line with the intuition that the cash transfers provided for the first child should not matter for families at the margin of having a second child. Second, the cash transfers for the third child did not influence the families to alter their decisions about having a second child.

Lastly, the estimated coefficient in Column 5 implies that third child birth rates increase by 0.36% as the cash transfers for that birth rate goes up by 10%. Similar to the case of second birth before, the baby bonus provided to families with first and second children should not impact whether families already with two children decide to have an extra baby. In line with this intuition, the effects of cash transfers for the first and second child are statistically not different from zero.[42]

Figure 4 plots the elasticities of age-specific birth rates with respect to cash transfers. The effects of cash transfers are statistically zero for all birth orders among adolescents and those older than 40. Cash transfers affect the birth rates among 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 at the age group 30-35.

10,000) are very close to the elasticities I compute by multiplying an adjustment factor.

[42] The results are robust when using the birth rates and the cash transfers in levels. In Appendix, Table A.3 reproduces the results reported in Table 4 without taking 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 child, respectively.

For the higher order births, the elasticity to the cash transfers is higher among younger mothers (20-35) for the second child and among older mothers (25 to 39) for the third child.

Overall, the results demonstrate that cash transfers increase the birth rates across birth orders. These effects were birth order-specific. In other words, the cash transfers did not *inframarginally* 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, albeit 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 envelope calculation under the assumption that everything else is held constant implies 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 less children ever born by the female population in 2015 over their life cycle. This estimate falls within 3 to 5% increase in fertility estimated using the 2007 introduction of 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 would be exorbitant.[43]

4.2 Individual-Level Analysis: Infant's Sex, Birth Weight, and Gestation Age

Figure 5 reports the event study results estimating Eq. 6 using an indicator for a boy, birth weight, and gestational age. 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 are 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 the pro-natalist cash transfers. The average decline post policy implementation is about 0.3 percentage point, 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.[44]

The event study results thus far provide evidence that the cash transfers had statistically sig-

[43]The observed total fertility rate in 2015 is 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 3.4 million KRW) from 340,000 KRW for first child, about 17 times (or 15.7 million KRW) from 930,000 KRW for second child, and about 16 times (or 41.75 million KRW) from 2.7 million KRW. Note that the median annual household income (family size: 4) based on the 2015 Household Expenditure Survey was 42 million KRW.

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

nificant effects on an infant's sex at birth and health. However, the effects are estimated without accounting for the different levels of cash transfer generosity across districts and over time after policy implementation. Below, I estimate the elasticity to cash transfer generosity for each outcome of interest by exploiting the rich variation in cash transfer amount based on Eq. 7. Throughout my analysis, I include: the rich set of fixed effects and district-level time varying characteristics to purge out the differences in the outcomes variables arising due to regional differences; birth order of a child and parental characteristics (i.e., mother's and father's age, educational attainment level, occupation (including unemployment), and marital status) to level the baseline differences based on birth order and parental background.[45]

Sex Ratio at Birth.

The first two columns in Table 5 summarizes the results estimating the effect of the pro-natalist cash transfers 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 implies a 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: 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 decrease the probability of a male birth by 1.61 percentage points. The sex ratio at birth among the first children has been steadily at its natural ratio. The result implies that the cash transfer may have resulted in an unnatural sex ratio at birth with more girls. Thus, I explore how the effect is heterogeneous by birth order. I allow the cash transfer effect to vary across birth orders in Column (2). I find that the cash transfer did not affect the sex ratio among the first children and had a negative, but economically negligible effect on the sex ratio among the second children. Among the third children, the effect of cash transfer on the sex ratio is negative and statistically different from zero at the 0.1 significance level. Doubling the baby bonus for third children 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 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: e.g., changes in social norm (Chung and Gupta, 2007) and a reduced reliance on son due to increased old-age pension (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) corresponds to the baby bonus effect

[45] In 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 infant's sex at birth, Table A.5 for birth weight, and Table A.6 for gestational age.

on the sex ratio; the baby bonus explains about 53% of the decline in sex ratio at birth since 2000.

Health Outcomes: Birth Weight and Gestational Age

Column 3 in Table 5 reports the results estimating Eq. 7 for birth weight as the dependent variable. An increase in the amount of baby bonus led to a decrease in birth weight; this 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 third children. The estimated effects for the first and second child are not statistically significantly different from zero, while the estimated effect for the third child is statistically differently from zero at the 0.1 significant level. The coefficient estimate implies that doubling the cash transfers for third child would result in decreasing birth weight by 0.22% (or 7.2 grams) after applying the adjustment factor. The birth weight among the third children is on average higher than the lower order births; therefore, the cash transfers hardly made the third children worse off if compared to the first and second children's birth weight.[46] Still, the negative effect on birth weight is surprising. The literature estimating the income effect 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 EITC in the U.S. is associated with an increase in birth weight by 6.4 grams overall (by 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 parental characteristics that affect infant health. Another possible explanation is that the cash transfers may have affected other key determinants of birth weight. One of such factors is gestational age; the tight relationship between birth weight and gestation age is physiological and well documented.

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

[46] In Appendix, I explore whether the cash transfers had any impact on the incidences of low birth weight (less than 2500 grams; Column 1-2) and macrosonia (birth weight greater than 4,000 grams; Column 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.

effect on birth weight.

I further investigate whether the pro-natalist cash transfers had impacts on the early-life mortality. In Table 6, I summarize the results estimating the effects of the pro-natalist cash transfers on the 1-year (infant) and 5-year mortality, focusing on the birth cohorts born in years 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 statistically significantly not different from zero, while positive. In Column 2 where I allow the effect to differ by birth order, none of the estimated coefficients are statistically significant. Similarly, the results for 5-year mortality reported in Column 3-4 suggest that the cash transfers did not have any impact on the mortality in the long run. Overall, while the cash transfers had negative impact on health outcomes (birth weight and gestational age) observed at birth, the cash transfers did not lead to increasing early-lift mortality rates. Early-life mortality is arguably an extreme measure of infant health especially in this context where the infant mortality is already low. The results in this paper stipulates future research on the baby bonus effects on educational and labor market outcomes.

5 Mechanisms

The pro-natalist cash transfers increased birth rates, decreased birth weight and gestational age, and modulated the son preference. In this section, I discuss potential mechanisms explaining the main results of this paper.

5.1 Spatial Redistribution of Fertility: Migration

Kim (2023) studies the migration and commuting decisions in the context of South Korea and shows people choose to live in districts where there are greater local government expenditures, which include the baby bonus. I test whether potential beneficiary households moved across districts in response to yearly changes in the pro-natalist cash transfers based on the universe of resident registration records. A record is generated each time a household declares its residency.[47] 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 which are potential beneficiaries of pro-natalist cash transfers.[48] Using the universe of these registration records, I construct a panel data set of district pairs from 2001 to 2015 with the number of families who are potential beneficiaries for a first, second, and third child, and moved from one district to another. I estimate the following gravity equation:

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

[48] For instance, a household is identified as a potential beneficiary of pro-natalist cash transfers for a third child if this household has 4 members: there are two adults between the ages of 20 and 39 with an age difference less than or equal to 15 years; there are 2 children who are less than 19 years old and the older children is at least 15 years younger than the youngest parent.

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

where $F_{p,o,d,y}$ is the number of potential beneficiaries for pro-natalist cash transfers for 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 in 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 city-wide characteristics at each destination district (e.g., housing and labor market conditions); $X_{d,y}$ is a set of district-level time varying characteristics. Then, 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 Column 1, 2, and 3, respectively. In Column 1, the estimated coefficient is positive, but small in magnitude and statistically not 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 signifcant. The estimated effect of pro-natalist cash transfers in Column 3 is positive and statistically significant at the 1% significance level for the third child. Interpreting this estimate in terms of elasticity by applying the adjustment factor for the third card as explained in Eq. 9, a 10% increase in the cash transfers for third child increases the probability of households who are potential beneficiaries migrating to new districts by 0.2%. Notwithstanding the statistical significance, the magnitude of the effect is small. In Column (4), I test whether there exists a systematic correlation between the migration patterns among non-beneficiaries and cash transfer generosity. The result shows there is not.

Then, 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 the changes in the stock of potential beneficiaries. I replicate the main results in Table 4, while excluding adult population and net migration rate, thereby additionally loading the positive effect of migratory responses on birth rates onto the coefficients. Table A.8 in Appendix reports the results in Column 1, 2, and 4, respectively; Column 1, 3, and 5 are the same as the results reported 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 child in line with the gravity estimation results in Table 7. However, the estimates are not statistically significantly different from each other for all birth orders; this makes sense given the size of the estimated migratory effect.

5.2 Temporal Adjustment of Fertility

The positive effect of the cash transfers on birth rates may simply reflect the 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 shifts in fertility time is that potential beneficiaries may believe the cash transfer generosity would decrease 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 Figure 2 and A.4 showed that the increases in the birth rates across birth orders sustained for over 5 and 7 years after policy implementation. Furthermore, my results in Figure 3 serve as additional evidence ruling out this potential tempo effect. While the effect on first children was positive and statistically significant across mothers in their active age of childbearing, the effects second children for the middle, and third children for older mothers. I formally investigate the changes in mother's age and marriage duration at the time of childbirth. In Table 8, Column 1 and 2 report the results estimating Eq. 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 start having their first children and seem to have lengthened, if anything, the average marital duration until having a first child. The small, yet statistically significant decrease in mother's age among second child at the 1% significance level is likely driven by mothers choosing to have their second child with the assistance of cash transfers, who would otherwise have completed childbearing after having their first child. Despite this small decrease in mother's age for second child, the childbearing timing measured in terms of marriage duration was not affected by the baby bonus for all birth orders. All these evidences suggest that the fertility effect I identify 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 family, who are responding to the policy. The main specification (Eq. 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 aggregates and may masks heterogeneous effects. Further, the extent to which each characteristics contributes to the aggregated effect depends on its composition change. Thus, it is important to study changes in the composition of parents due to the baby bonus and its heterogeneity for policy implications as well as explaining the main results.

Changes in Composition. In Table 8, I report the estimated changes in parental characteristics. As mentioned earlier and reported in Column 1 and 2, the composition of mothers in terms of their age and childbearing timing relative to marriage year did not change with the cash transfers in a meaningful way. In terms of educational attainment (Column 3 and 4) while controlling for parental age, employment status and marital status, the cash transfers increased the fraction of parents with a college degree for all birth orders.[49] While these policies were well publicized and has a simple policy structure, the levels of policy awareness and of understanding it may differ by educational attainment level. Column 5 and 6 report the results on parental employment status. Holding the other parental characteristics constant (age, educational attainment level, marital status, husband's employment status), the cash transfers decreased the probability that a mother is employed for all birth orders, but especially stronger for the third child. The opportunity costs are lower for mothers, who are detached from the labor market.[50] At the same time, the share of working fathers among the second and third children also decreased in cash transfers. Doubling the cash-transfer generosity is associated with 0.18 and 0.16 percentage point decreases for the second and third children. Households with unemployed fathers, thus with relatively 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 Eq. 7 by pooling samples separately by mother's age and parental educational attainment level and employment status.[52] Figure 6 summarizes the results for birth weight (left panels) and gestational age (right panels). The top panels plot the estimated effects of cash transfers by pooling samples by mother's age groups. First, the cash transfer effect on birth weight is driven by relatively older mothers. The cash transfers did not affect the birth weight of the children whose mothers are between the ages of 20 and 24, irrespective of birth order. Among first and second child, the cash transfers had a statistically significant negative effect for mothers between the ages of 25 and 34, but not for mothers between the ages of 35 and 39. Across older mothers (ages: 25-39), birth weight among the third children decreased in cash transfers, and the magnitude of the effect is particularly large among 30- to 34-year-old mothers. Second, the cash transfer effect on gestational age for second child is driven by younger mothers (age: 20-34). The estimated coefficients suggest that the effects are not heterogeneous and similar across age groups for first and third child. The effects on gestational age for first child are not statistically significantly difference from zero at any meaningful significance level across all age groups. The effects for third child range narrowly from -0.0027 to

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

[50] The share of working mothers is noteworthy. It is only 31.8% for mothers among first child and decreases by 9.3% and 12.5% for second and third child. Contrastingly, the share of working fathers is 95.5% among 1st child and grows for families with 2 and more children.

[51] The birth records do not have information on family income; thus, I do not observe any direct income measures. The low female employment, 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 a observable, predetermined parental characteristics. When doing so, I still control for all the parental characteristics other than the one that stratifies the data. For example, when estimating the effects separately for working mothers and non-working mothers, I include the full set of indicators for birth order and parental characteristics (mother's and father's age, educational attainment level, marital status, and father's occupation), except the indicator for mother's occupation. I compare the estimated effects of pro-natalist cash transfers across parental characteristics based on the confidence intervals.

-0.0017.

In the middle panels, I report the estimation results by whether mother's or father's completed education attainment level is some college or above. None of the estimated coefficients for the first children in both left and right panels are statistically significant. The estimated cash transfer effects on birth weight among mothers without a college degree are -0.0017 and -0.0033 for the second and third children, respectively, and they are statistically significant at least at the 5% significance level. Among college graduate mothers, the magnitude of the effects is much smaller for both second and third children than the effects among mothers without a college degree; the effect on birth weight even becomes statistically indistinguishable from zero for second child. For third child, the estimate of -0.0014 on birth weight among college graduate mothers is different from the estimate among mothers without college when their confidence intervals are compared. Similarly, the results in the right panel suggests that the effects on gestational age are larger in magnitude for the second and third children with mothers without a college degree, compared to those with mothers with a college degree. There is no meaningful differences detected when comparing the cash transfer effects between fathers with and without a college degree for both birth weight and gestational age.

Lastly, the results by parental employment status are summarized in the bottom panels. The cash transfer effects on birth weight are similar across parental employment status for all birth orders. The effects among third child are consistently negative within a relatively tight range between -0.0032 (employed father) and -0.0021 (non-working mother). For gestational age, the cash transfer effects by father's employment status differ in a statistically significant way as the effect is considerably larger if a father was unemployed than if employed.

Discussion. The pro-natalist cash transfers had heterogeneous effects by mother's age, while there was no composition change in mother's age. The statistically negative effect among third child is explained partly by relatively older mothers. The statistically insignificant estimates for first and second child in the main result when pooling observations across mother's age mask the statistically significant negative effects among mothers between the ages of 25 and 34. The results over all suggest that the mothers who responding to pro-natatlist cash transfers may have been negatively selected in terms of unobservable characteristics that determine infant health and that the strength of this negative selection is positively correlated with cash transfer generosity.

Both the composition change and heterogeneous effects by mother's educational attainment level and by father's employment status explain the main results on birth weight and gestational age. An increase in the cash transfer generosity led to a higher fraction of parents with a college degree, but lowered the probability of being employed for both mothers and fathers. The cash transfer effects on birth weight and gestational age for second and third child differ between mothers with and without a college degree, but not between fathers with and without a college degree. These reduce-form estimates reflect other unobservable factors associated with mother's educational attainment level that contribute to infant health (e.g., prenatal care, health status, and life style). Although not possible to investigate further due to data limitation, the stronger negative effect of cash transfers on birth weight and gestational age among mothers without a college degree suggests that these mothers may have been more negatively selected in these unobserved factors compared to mothers with a college degree.

The cash transfer effect on gestational age is more negative for third child with an unemployed father than the effect for third child with a working father. Interestingly, the same pattern is not detected between mothers who work and who do not. The father's unemployment would generate a large negative income shock to a family.[53] The results imply that families, who responded to the cash transfers and had their third child, have been negatively selected in terms of income and that this negative selection is stronger among families with a non-working/unemployed father.

Contrarily, 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 cash transfer effect on birth weight after purging out the effect of cash transfer through gestational age 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 third child being a boy decrease with the pro-natalist cash transfers? There are many possible explanations. For instance, the cash transfers may compensate the utility penalty associated with having a girl, and a family keeps a girl whom they would not have aborted in the absence of the baby bonus. Another likely channel is that the preference for son is heterogeneous and that the families responding to the pro-natalist polices are indifferent to the infant's sex or even prefer a daughter to a boy. I cannot test these scenarios 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 in South Korea. It records information on age, gender, relation to the household head, birth district of each member in a household. Based on the Census data, I construct a household-level dataset, in which each household has at least one and at most three children and the youngest child was born after 2001. For all the children born after 2001, the cash transfer generosity they received is determined by their age, birth order, and birth district. Then, I estimate the effect of the pro-natalist cash transfers on the probability of having a boy separately by children's gender composition within a family, while

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

[54] This direct effect for unemployed fathers would be more positive if it is positive for employed fathers; it would be less negative or even positive for unemployed fathers, if negative for employed fathers. This result provides a suggestive evidence that the direct effect of cash transfers may be positive, especially for families who would benefit more from the financial assistance.

controlling for parental characteristics (i.e., age, educational attainment level, employment status, and marital status), the district fixed effects, and the city-by-year fixed effects.[55]

The estimation results are summarized in Table 9. In Column 1, although it is positive and large in magnitude, the estimated effect of the pro-natalist cash transfers on the probability of a first child being a boy among all families is not statistically significantly different from zero. In Column 2 to 4, I focus on the households with at least 2 children. Pooling all households, the estimated cash transfer effect on the probability of a second child being a boy is statistically indistinguishable from zero and close to zero in its magnitude. I separately estimate the effect by first child's sex in Column 3 and 4. While imprecisely estimated, the sign of the estimated effects is interesting: negative if a household already has a boy; positive if the first child is a girl.

In Column 5 to 8, I focus on the households with three children. In Column 5, the estimated cash transfer effect on the probability of a third child being a boy is -0.0256, statistically significant at the 5% significance level. This estimate is remarkably close to -0.0246, the estimated effect of the pro-natalist cash transfers on the sex of a third child based on the birth registry records reported in Table 5. Restricting the sample to households whose first and second children are both boys (Column 6) substantially increases the magnitude of the estimated cash transfer effect to -0.0892, which is statistically significant at the 1% significance level. These households are likely to have relatively weaker preference for son because they already have 2 boys. The strong negative effect of the cash transfers suggest two potential explanations: one, baby bonus is able to compensate the utility penalty for having a girl and keep a girl whom they would have aborted without the baby bonus; two, some of these families may prefer a daughter to a son and are choosing to have a third child for a daughter. For households with a boy and a girl (irrespective of the 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% significance 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 are more families having a third child with an intention to keep if the baby is a girl.

Lastly, the policy effect is reserved and turns positive to 0.0414 among households with two daughters (statistically significant only at the 10% significance level). For these families, the baby bonus does not seem to have offset a utility penalty associated with having a girl. The extent of such a penalty must be highest among families with two daughters when they plan to have a third child. Evidently, the average share of third child being a boy among these families is 0.617 or 161 boys per 100 girls in terms of sex ratio! The 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 (Eq. 7) as closely as possible.

6 Conclusion

In this paper, I study 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 combine the 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 been reduced by 4.7%. I provide evidence that these changes in birth rates are a result of increased completed fertility, not a temporary increase in the number of births from temporal adjustments in childbearing decisions and migratory responses of families making fertility decisions. In addition to the number of births, these cash transfers had unintended consequences on the sex ratio at birth and infant health. The effects are heterogeneous across parental characteritics and led to composition change. These pro-natalist cash transfers enabled parents who were unemployed with lower educational attainment levels to have a baby, who would not have done so without the baby bonus. There households are likely low-income families. The results suggest that this explain the negative effects of cash transfers on infant health. Furthermore, I provide a suggestive evidence that the effect of cash transfers on birth weight, if the changes in gestational age is controlled, may be positive, especially among low-income families. Lastly, I find that the cash transfers contributed to the shift of the male-skewed sex-ratio at birth towards its natural level.

The global total fertility rate has been declining and approaching the 2.1 replacement level. Low fertility rates are not a concern unique to South Korea. Many developed countries share this concern, which will likely be critical policy debate among today's developing countries with high, but rapidly declining fertility rates in their near future. This paper provides insights about the effects of pro-natalist cash transfers (baby bonus) to inform policy makers: these transfers increase completed fertility, but may not be sufficient to push the total fertility rates back up to the 2.1 replacement. In addition, the cash transfers may create unintended consequences of, for instance as explored in this paper, 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 (e.g., cash transfers and parental leave) and the long term implications of such policies on the outcomes of the children born as a result of these initiatives.

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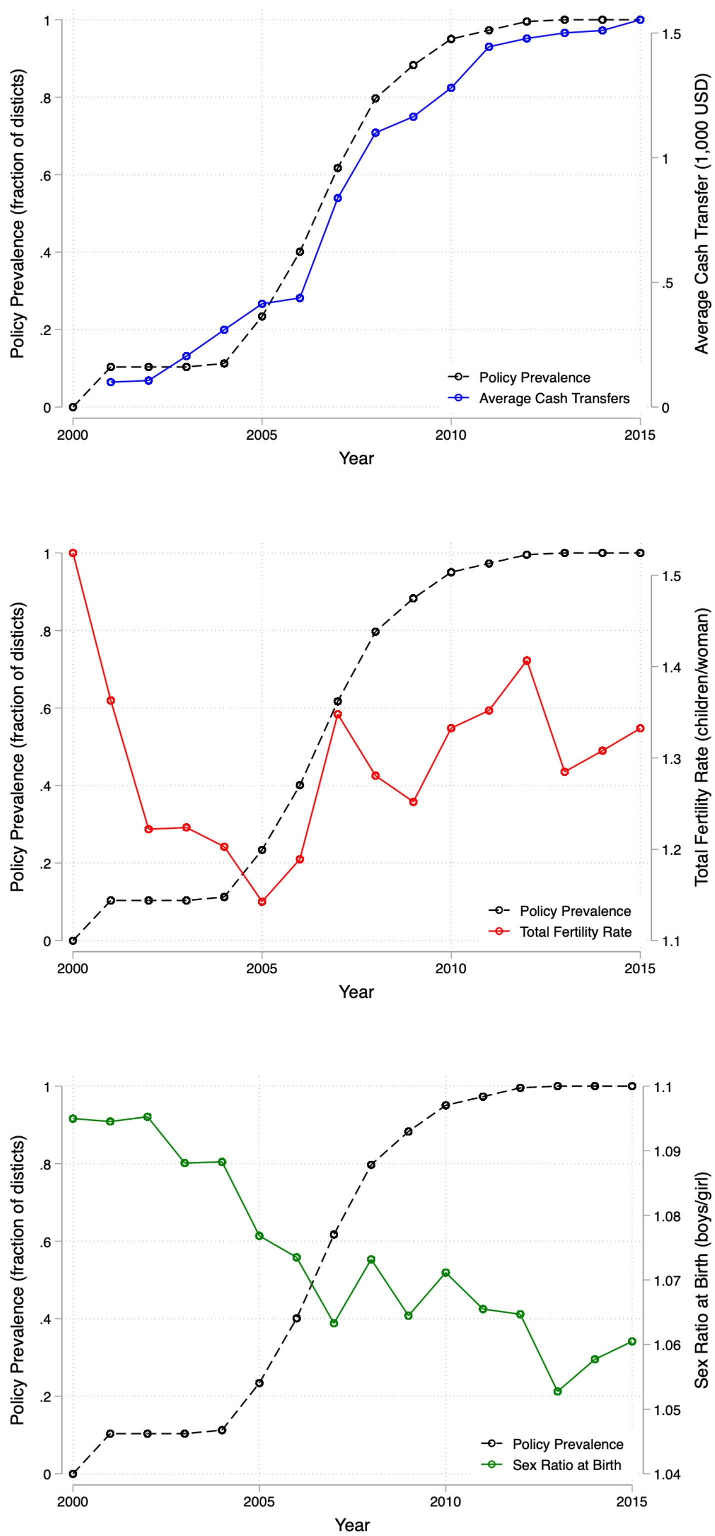

Figure 1: Baby Bonus, Total Fertility Rate, and Sex Ratio at Birth

Notes: This figure plots the fraction of districts (total of 222 districts) with pro-natalist cash transfers (Baby Bonus) and the average cash-transfer amounts 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
		-	(0.02)	(0.19)	(0.69)	(0.89)	(0.87)
		-	[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
		-	(0.02)	(0.30)	(0.97)	(1.21)	(1.23)
		-	[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
		-	(0.02)	(0.86)	(2.59)	(2.92)	(3.12)
		-	[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

Notes: This table summarizes the local pro-natalist cash transfers and birth rates for every three years from 2000 to 2015. For each birth order, Panel A reports the fraction of districts (out of 222 districts) with strictly positive pro-natalist cash transfers (1st row), average cash transfer amounts measured in 1 million KRW conditional on strictly positive pro-natalist cash transfers (2nd row), their standard deviations in parentheses (3rd row) and minimum and maximum values in brackets (4th 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 chilren per 1,000 women between the ages of 15 and 49. Standard deviations are reported in parentheses. In Appendix, TableA.1 reports the summary statistics of infant health outcomes (birth weight, gestational age, and sex at birth) based on the individual-level birth registry recrods.

	(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 Eq. 1. Each observation corresponds to a district, which had not implemented pro-natalist cash transfer policies 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 in the bottom. Heteroskedasticity robust standard errors are 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)
	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)
Totel 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 unit) provided for the first child in Column 1-2, for the second child in Column 3-4, and for the third child in Column 5-6 on the district-level characteristics based on Eq. 2. For each birth order (1st, 2nd, and 3rd), the observations in the left column correspond to all district-year pairs and the observations in 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 in the bottom. Clustered standard errors at the district level are reported in parentheses: * Significant at the 5 percent level, ** Significant at the 1 percent level, and *** Significant at the 0.1 percent level.

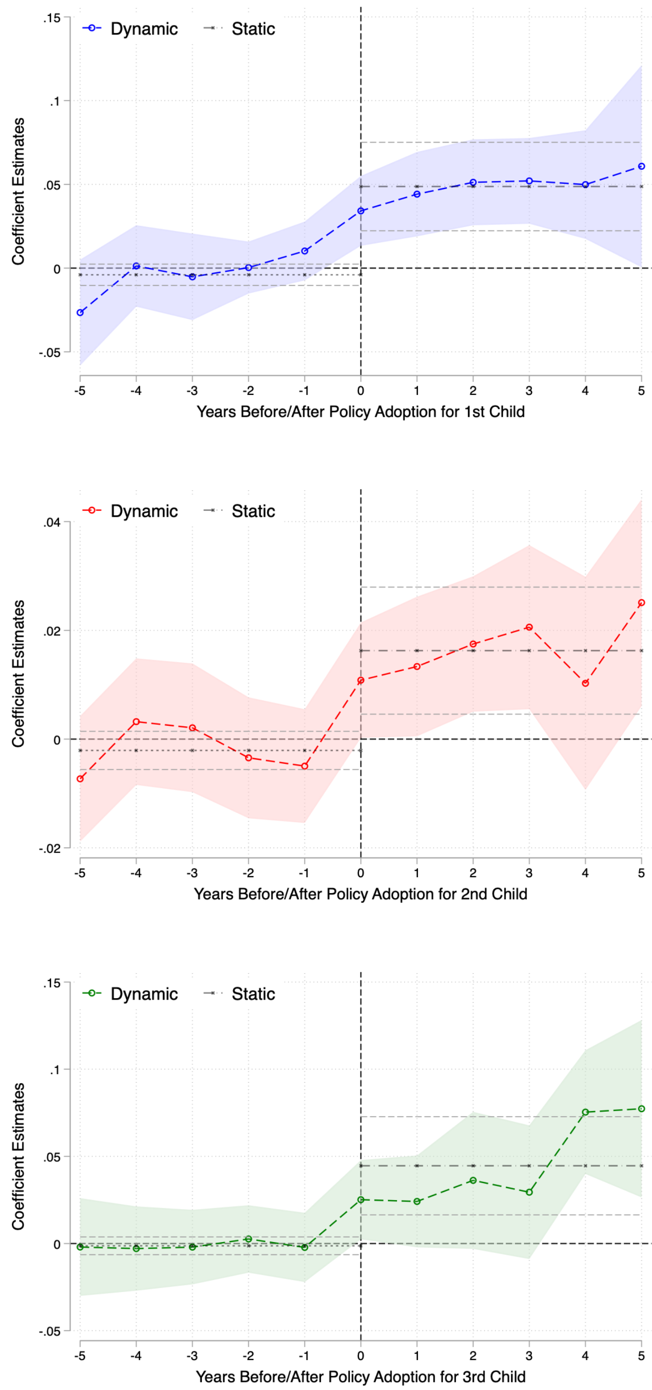

Figure 2: Parity-specific Birth Rates Before and After Policy Implementation

Notes: This event-study figure plots the estimated changes in the birth rates before and after pro-natalist cash transfer policy implementation for the 1st child (top in blue), 2nd child (middle in red), and 3rd child (bottom in green). The event-study coefficients are estimated based on Eq. 3 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients pre-/post-treatment periods are plotted in black dash-dotted lines. Error bars show 95% confidence intervals. Each observation corresponds to a district-year pair and is weighted by the female population between the ages of 15 and 49. Across each panel, the same set of fixed effects (i.e., 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 (between the ages of 20 and 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 2nd child (resp. the 3rd child) include the lagged number of births for the 1st child (resp. the 1st and 2nd child). Standard errors are clustered at the district level.

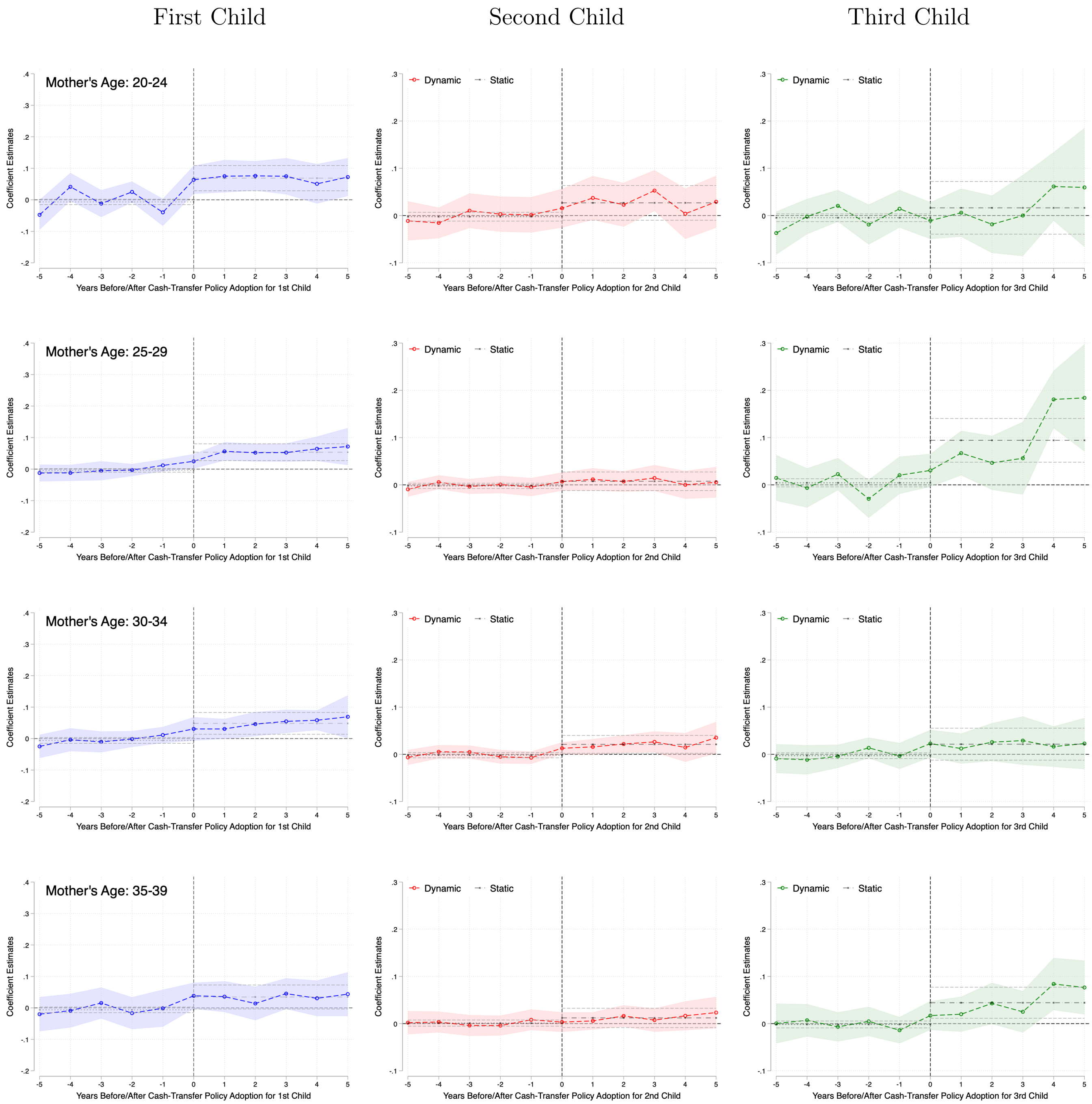

Figure 3: Birth Rates Before and After Policy Implementation (Ages: 20-39)

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 1st child (left in blue), 2nd child (center in red), and 3rd child (right in green). The event-study coefficients are estimated based on Eq. 3 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients pre-/post-treatment periods are plotted in gray dash-dotted lines. 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 (i.e., 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 (between the ages of 20 and 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 2nd child (resp. the 3rd child) include the lagged number of births for the 1st child by mothers in the same 5-year age group (resp. the 1st and 2nd child). Standard errors are clustered at the district level. In Appendix, the results for adolencent mothers and older mothers are presented in Figure A.8.

	(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 Cash Transfer on Birth Rates

Notes: This table reports the estimated effects of cash transfers on the birth rates for the 1st child (Column 1-2), the 2nd child (Column 3-4), and the 3rd child (Column 5-6) based on Eq. 4. For each birth order, the left column includes the inverse hyperbolic sine transformed (IHS) value of cash transfer amount for the corresponding birth order only; the right column includes the IHS values of cash transfers for the 1st, 2nd, and 3rd child as separate explanatory variables. Each observation corresponds to a district-year pair from 2001 to 2015 and is weighted by the female population between the ages of 15 and 49. Across columns, the same set of fixed effects (i.e., 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 (between the ages of 20 and 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, Column 3-4 (resp. 5-6) includes the lagged number of births for the 1st child (resp. the 1st and 2nd child) in log unit. In 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 percent level, ** Significant at the 1 percent level, and *** Significant at the 0.1 percent level.

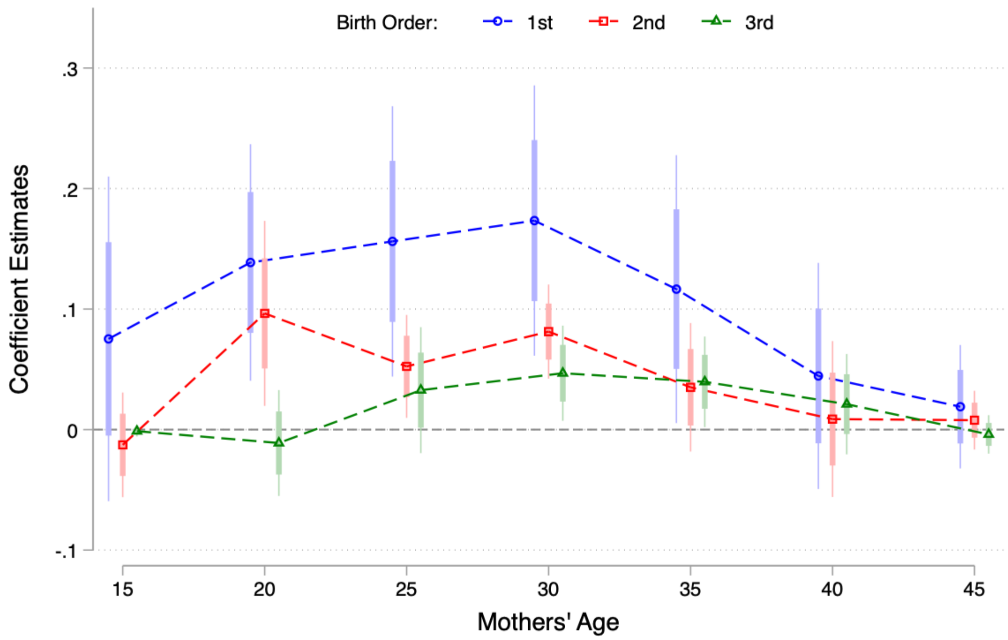

Figure 4: The Effect of Cash Transfer on Birth Rates by Mother's Age

Notes: This figure plots the estimated effects of the cash transfers on the age-specific birth rates for the 1st child (blue-circle), 2nd child (red-square), and 3rd child (green-triangle) by mother's 5-year age group (horizontal axis) based on Eq. 4. Standard errors are clustered at the district level. Error bars show 95 percent (thick) and 99.9 percent (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 (i.e., district fixed effects and city-by-year fixed effects). The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (between the ages of 20 and 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 2nd child (resp. the 3rd child) include the lagged number of births for the 1st child by mothers in the same 5-year age group (resp. the 1st and 2nd child).

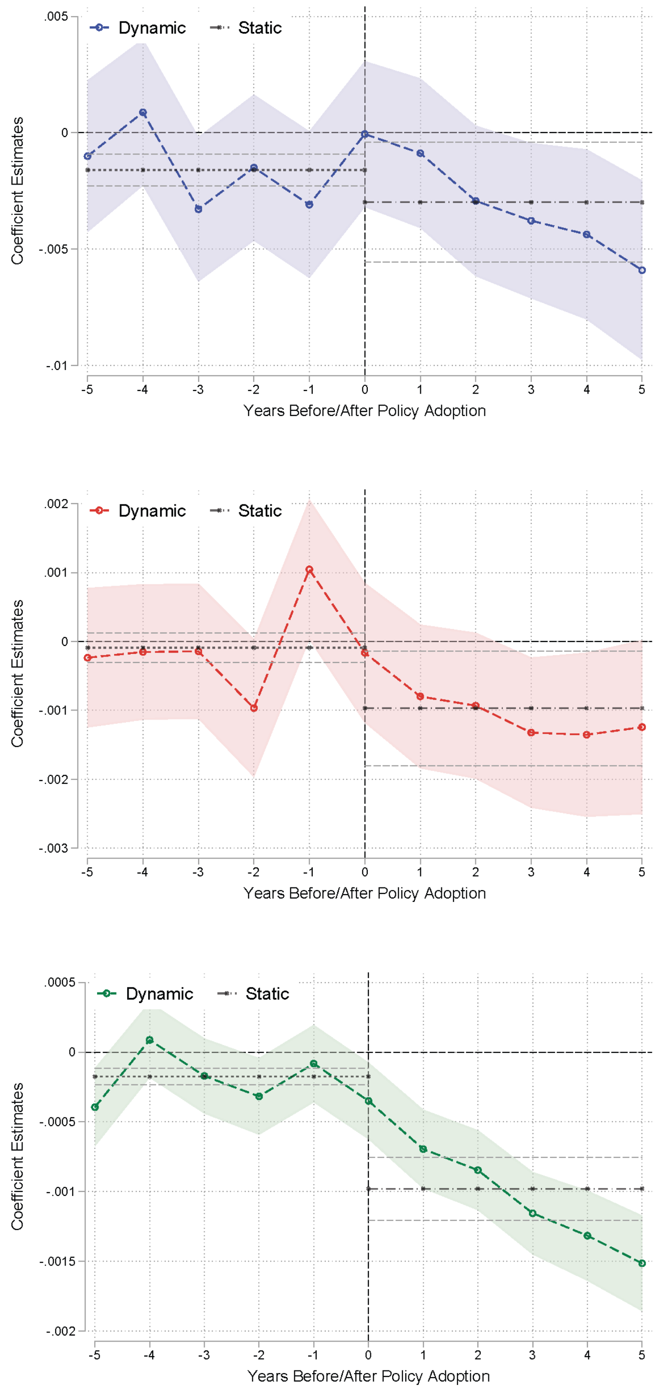

Figure 5: Infant's Sex at Birth, Birth Weight, and Gestational Age Before and After Policy Implementation

Notes: This event-study figure plots the estimated changes in the probability that an 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 Eq. 6 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients pre-/post-treatment periods are plotted in black dash-dotted lines. Standard errors are 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 (i.e., district fixed effects and city-by-month-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's educational attainment level, 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 (between the ages of 20 and 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.

	(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 Cash Transfer on Sex at Birth, Birth Weight, and Gestational Age

Notes: This table reports the estimated effects of cash transfers on indicator for boys (Column 1-2), log of birth weight (column 3-4), and log of gestational age (column 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-transfer effect is allowed to differ across birth parity. The mean values among 1st child are 0.513 percent for 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 (i.e., district fixed effects and city-by-month-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's educational attainment level, 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 (between the ages of 20 and 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. Standard errors are clustered at the district level and reported in parentheses: * Significant at the 5 percent level, ** at the 1 percent level, and *** at the 0.1 percent 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 Cash Transfer on Early Life Mortality

Notes: This table reports the estimated effects of cash transfers on early life mortality (indicator for those dead within one year since birth in Column 1-2 and within 5 years since birth in Column 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-transfer 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 (i.e., district fixed effects and city-by-month-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's age, educational attainment level, 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 (between the ages of 20 and 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. Standard errors are clustered at the district level and reported in parentheses: * Significant at the 5 percent level, ** at the 1 percent level, and *** at the 0.1 percent 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 Cash Transfers

Notes: This table reports the estimated effects of pro-natalist cash transfers on migration based on Eq. 10. The dependent variables in Column 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 non-beneficiaries of pro-natatlist cash transfers. Each observation corresponds to a destination-origin district-pair by year tuple from 2001 to 2015. The district origin-destinationl-level panel data set is constructed from the universe of resident registration records. Across columns, the same set of fixed effects (i.e., district-pair fixed effects, origin-district-by-year fixed effects, destination-city-by-year fixed effects) are included; the destination district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (between the ages of 20 and 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. Standard errors, threeway-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 percent level, ** Significant at the 1 percent level, and *** Significant at the 0.1 percent level.

	(1)	(2)	(3)	(4)	(5)	(6)
	Age	Years	College+		Employed	
	Mother	Married	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***	-0.0118***	-0.0027***
	(0.0388)	(0.0098)	(0.0025)	(0.0023)	(0.0026)	(0.0007)
3rd Child	-0.0199	0.0006	0.0279***	0.0019	-0.0934***	-0.0017**
	(0.0289)	(0.0108)	(0.0020)	(0.0014)	(0.0022)	(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: Compositional Changes in the Parental Characteristics

Notes: This table reports the estimated changes in the parental characteristics with cash transfers: parental age (Column 1), years of marriage (Column 2), education attainment level (Column 3 and 4), and employment status (Column 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 (i.e., district fixed effects and city-by-month-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's age (excluded in Column 1), educational attainment level (mother's excluded in Column 3 and father's in Column 4), occupation (mother's 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 (between the ages of 20 and 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. Standard errors are clustered at the district level and reported in parentheses: * Significant at the 5 percent level, ** at the 1 percent level, and *** at the 0.1 percent level.

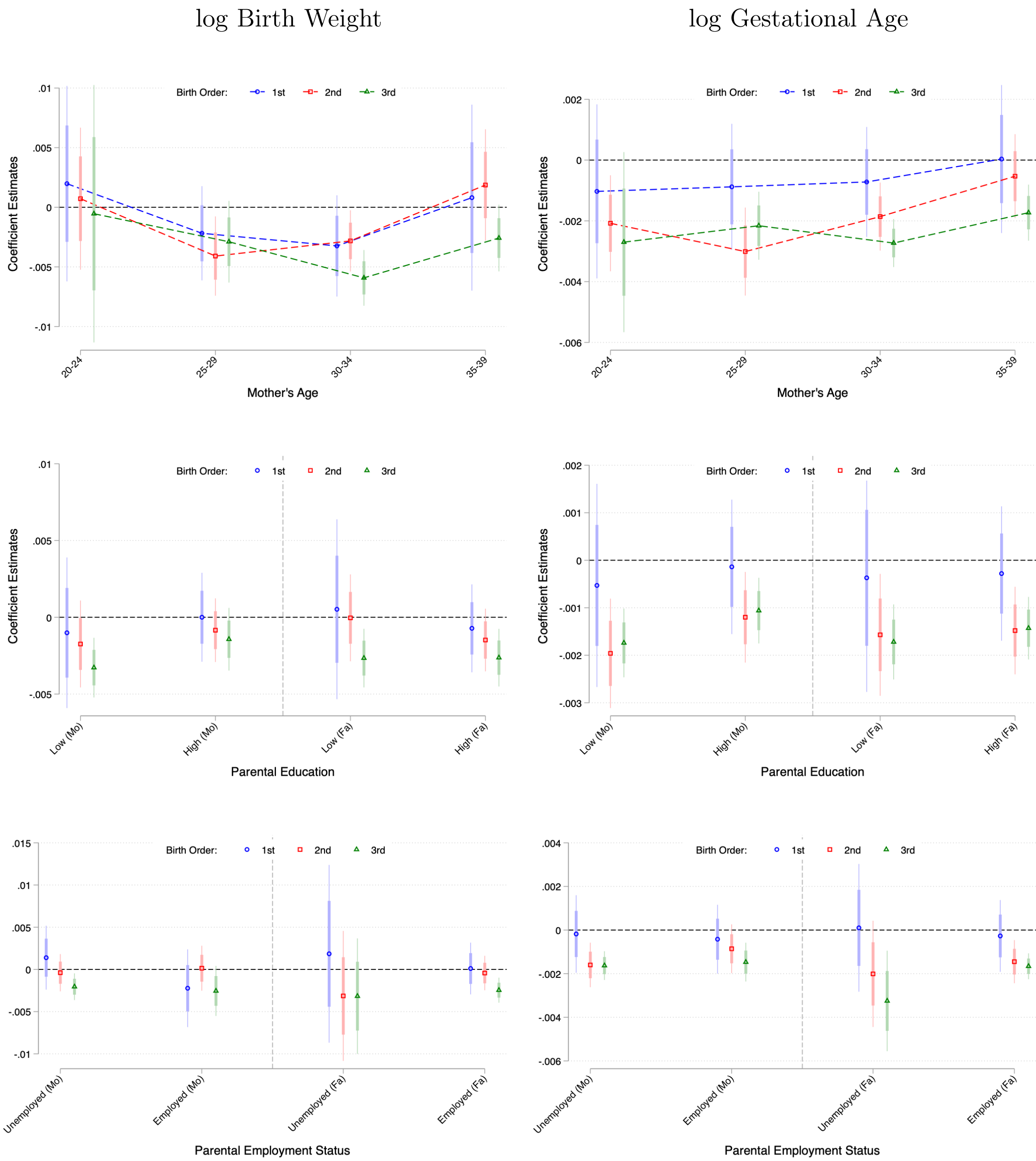

Figure 6: Heterogeneous Policy Effects on Infant's Sex and Health by Parental Age, Education, and Employment

Notes: This figure plots the parity-specific cash transfer effects on birth weight (left panels) and gestational age (right panels) by mother's age (top panels), parental educational attainment level (middle panels), and their employment status (bottom panels). The parity-specific effects are estimated based on Eq. 7, separately by pooling samples for each parental characteristics. Standard errors are clustered at the district level. Error bars show 95% (thick) and 99.9% (thin) confidence interval. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2001 to 2015. Across panels and groupings by parental characteristics, the same fixed effects (i.e., district fixed effects and city-by-month-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's age (excluded in the top panels) and father's age, their educational attainment level (mother's excluded in the middle panels when pooling observations by their educational attainment level and father's excluded in the middle panels when pooling observations by their educational attainment level), 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 (between the ages of 20 and 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.

	(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	5136,078	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 at Birth by Family Composition

Notes: This table reports the estimated effects of pro-natalist cash transfers on the probability that the 1st child is a boy (Column 1), that the 2nd child is a boy (Column 2-4), and that the 3rd child is a boy (Column 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 1, 2, or 3 children, whose youngest children was born in or after 2001. In each column, a different set of sample restrictions are applied: the observations are the households (HHs) with 1, 2, and 3 children in Column 1 (no restriction), the HHs with at least 2 children in Column 2, the HHs with at least 2 children, the 1st of whom is a boy, in Column 3, the HHs with 2 children, the 1st of whom is a girl, in Column 4, Across Column 5-8, the observations are the HHs with 3 children; in Column 6, the 1st and 2nd children are both boys; in Column 7, the 1st and 2nd children are a boy and a girl (vice versa); in Column 8, the 1st and 2nd children are both girls. Across columns, the same set of fixed effects (i.e., birth-district fixed effects and birth-city-by-birth-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's age, educational attainment level, 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 (between the ages of 20 and 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. Standard errors are clustered at the district level and reported in parentheses: * Significant at the 5 percent level, ** at the 1 percent level, and *** at the 0.1 percent level.

Appendix: Additional Figures and Tables

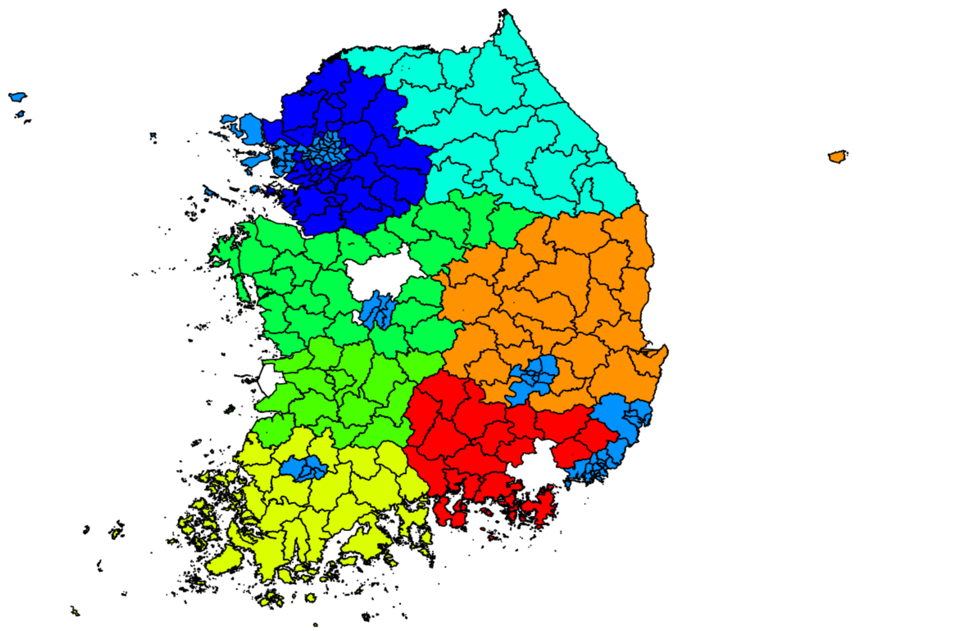

Figure A.1: Metropolitan Cities, Provinces, and Districts of South Korea

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

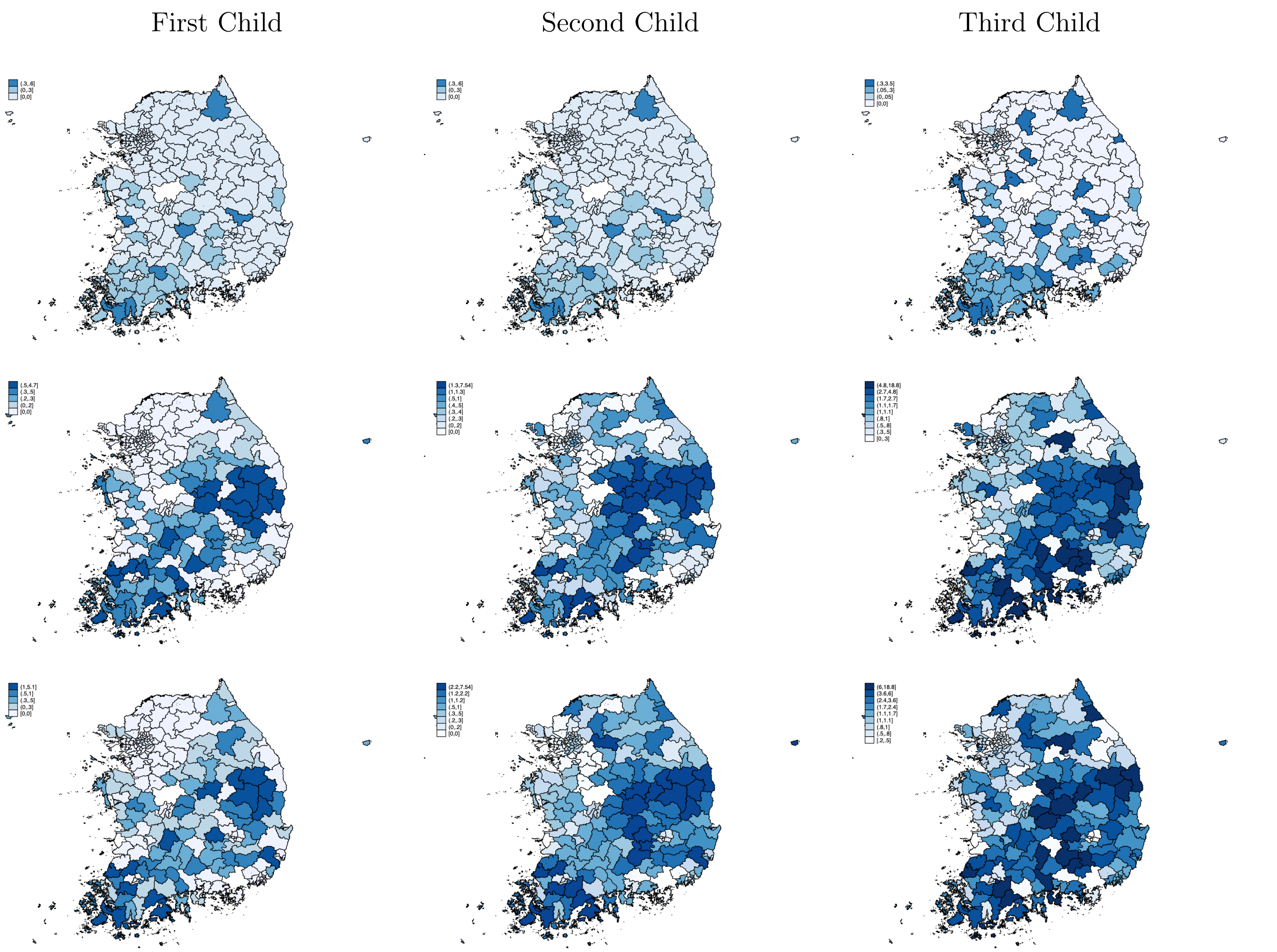

Figure A.2: Local Pro-natalist Cash Transfers across Districts (2005, 2010, 2015)

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 year 2005 (top), 2010 (middle), and 2015 (bottom). Districts in warmer blue have more generous cash trasnfer generosity.

	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

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

Notes: This table report 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.

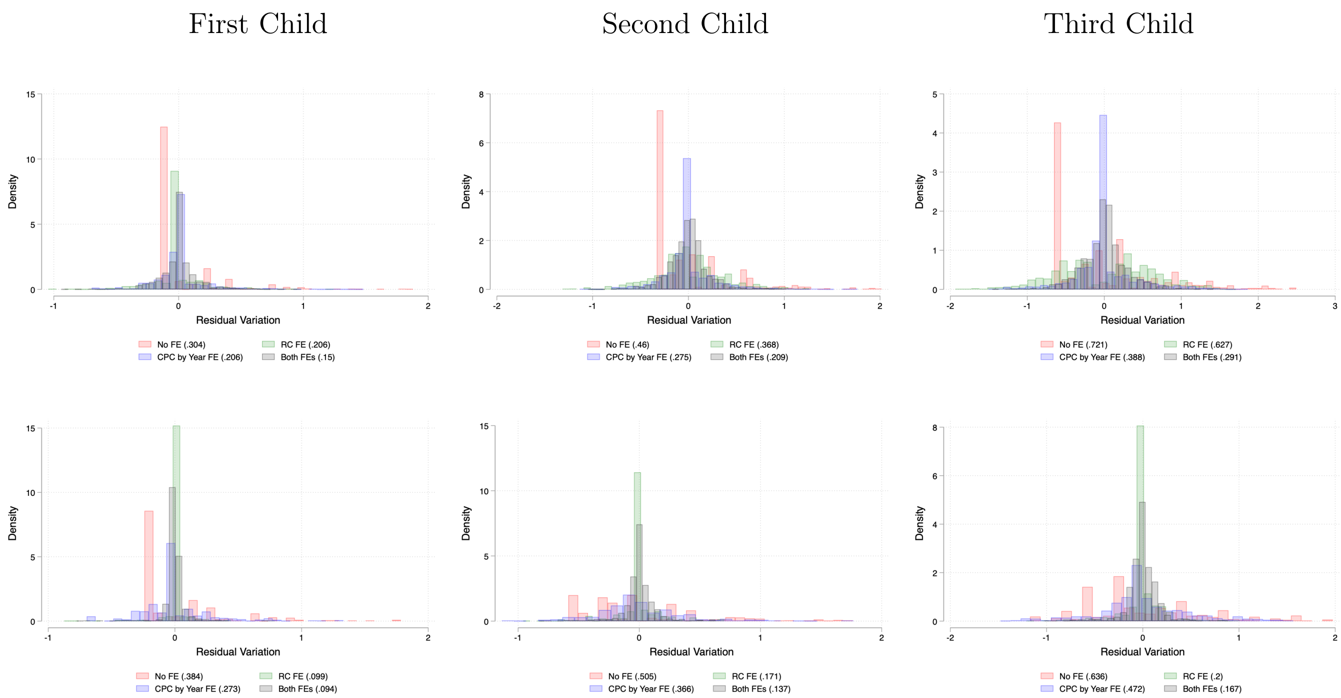

Figure A.3: Residual Variation in Local Pro-natalist Cash Transfers

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). Stardard errors are reported in parentheses.

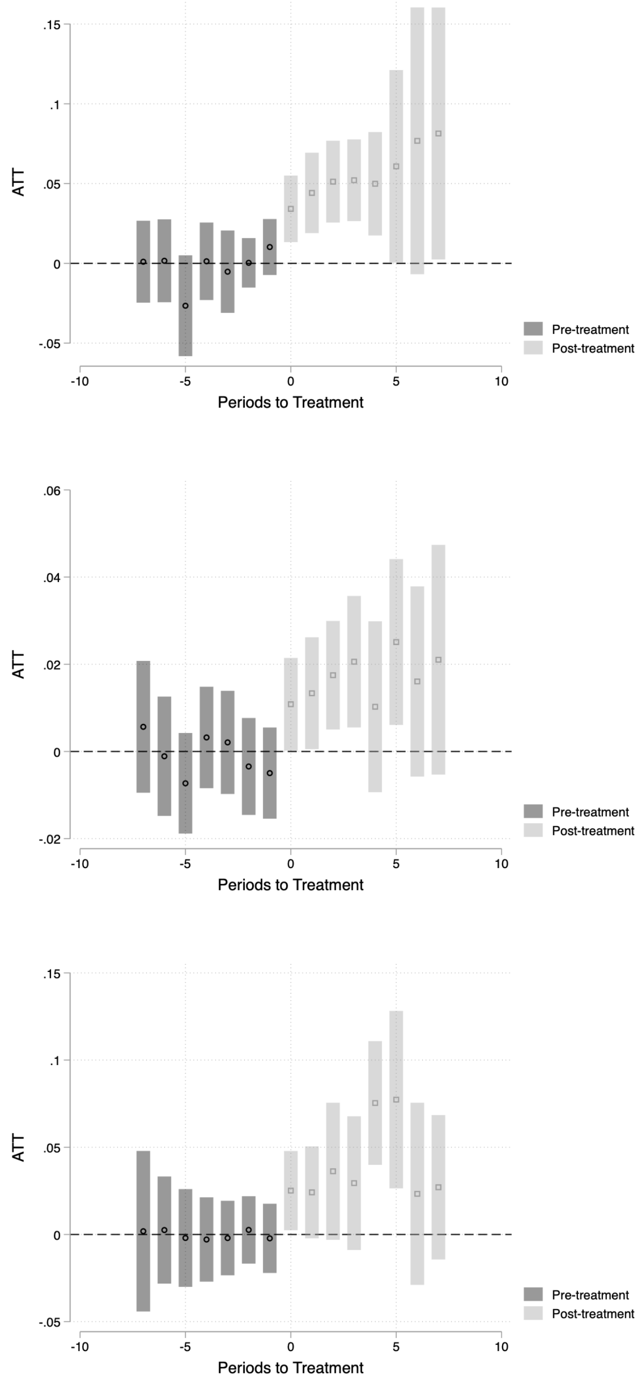

Figure A.4: Parity-specific Birth Rates Before and After Policy Implementation (Larger Window)

Notes: This event-study figure plots the estimated changes in the birth rates before and after pro-natalist cash transfer policy implementation for the 1st child (top in blue), 2nd child (middle in red), and 3rd child (bottom in green). The event-study coefficients are estimated based on Eq. 3 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients pre-/post-treatment periods are plotted in black dash-dotted lines. Error bars show 95% confidence intervals. Each observation corresponds to a district-year pair and is weighted by the female population between the ages of 15 and 49. Across each panel, the same set of fixed effects (i.e., 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 (between the ages of 20 and 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 2nd child (resp. the 3rd child) include the lagged number of births for the 1st child (resp. the 1st and 2nd child). Standard errors are clustered at the district level.

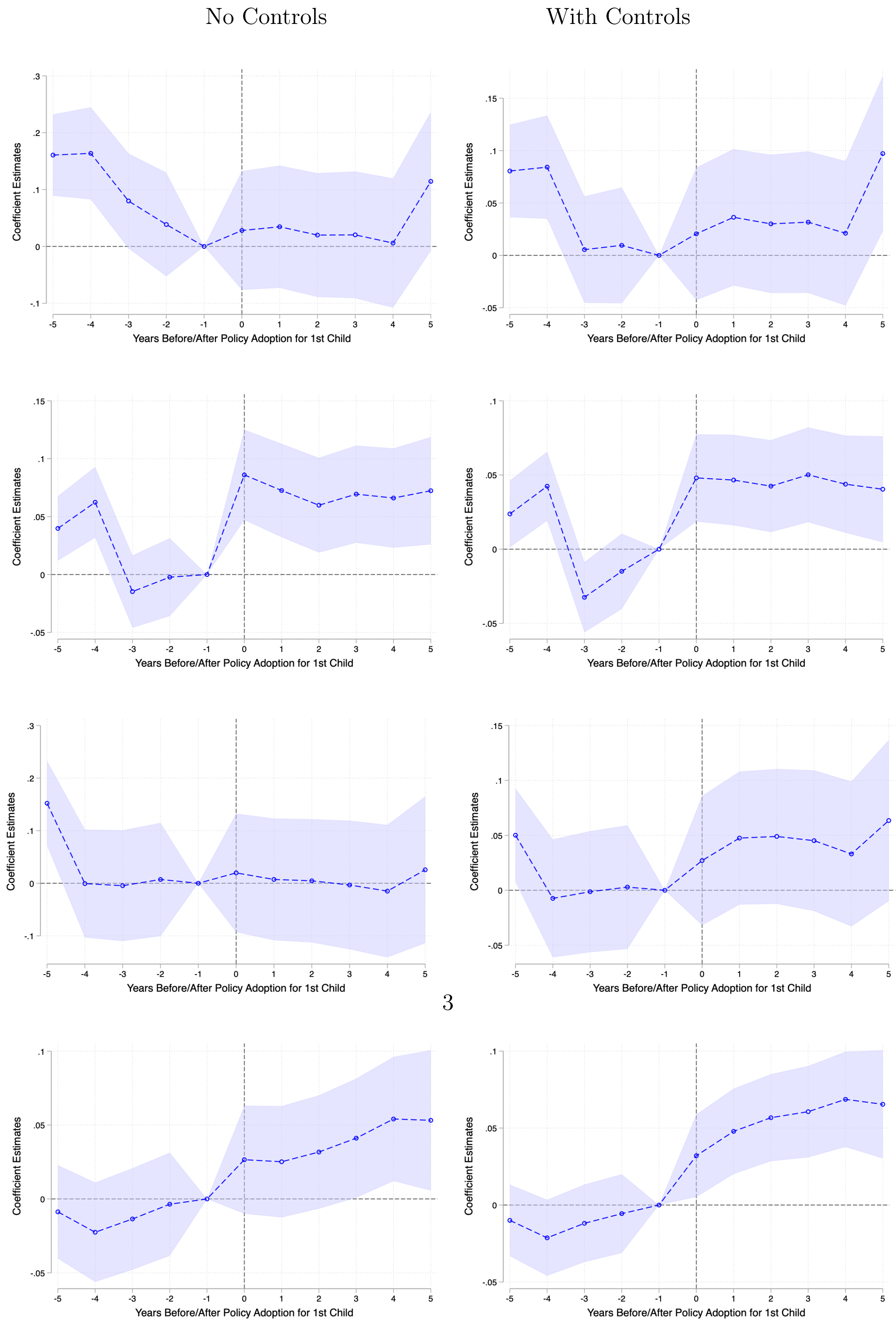

Figure A.5: Event Study Results for 1st Child Birth Rates

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.

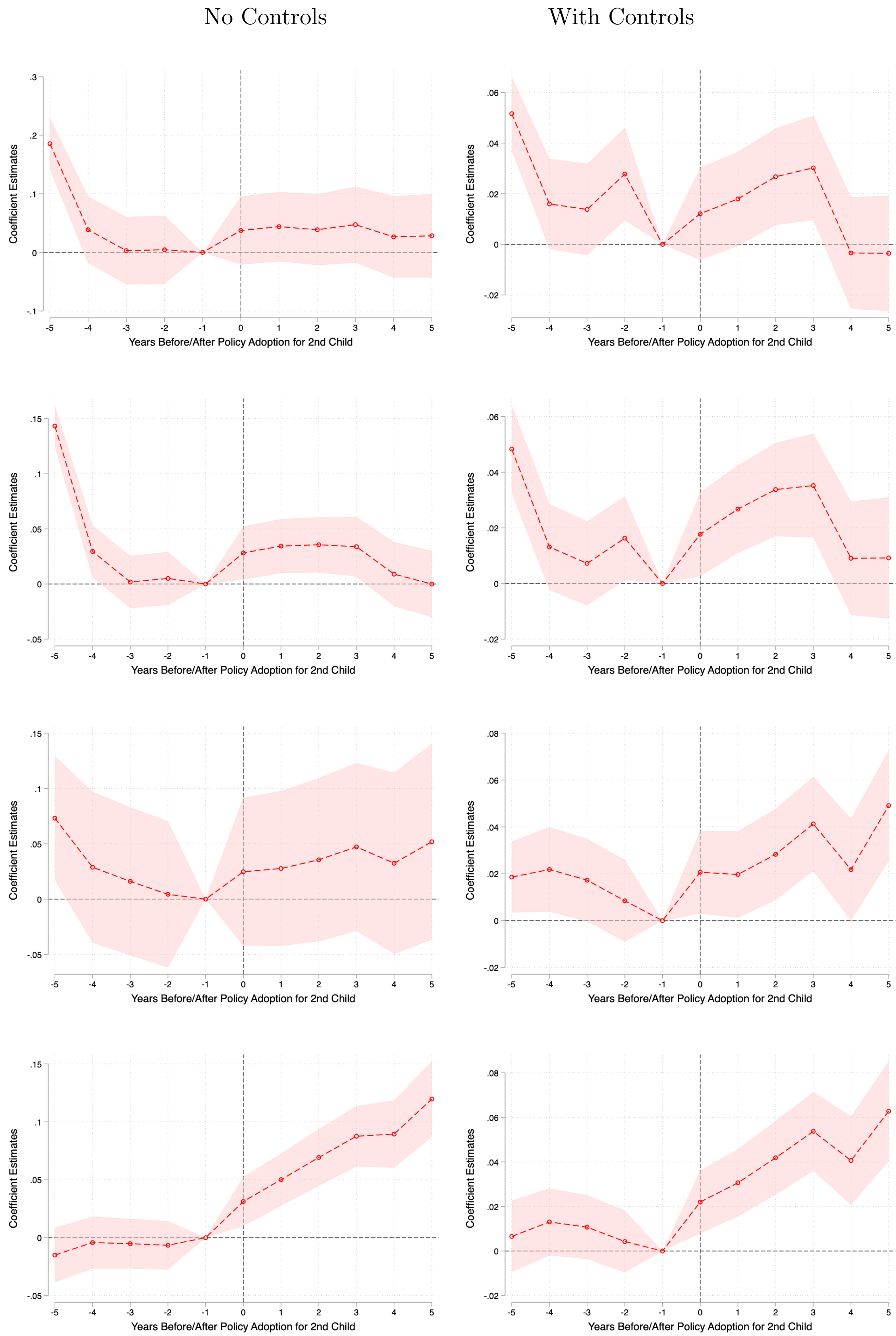

Figure A.6: Event Study Results for 2nd Child Birth Rates

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.

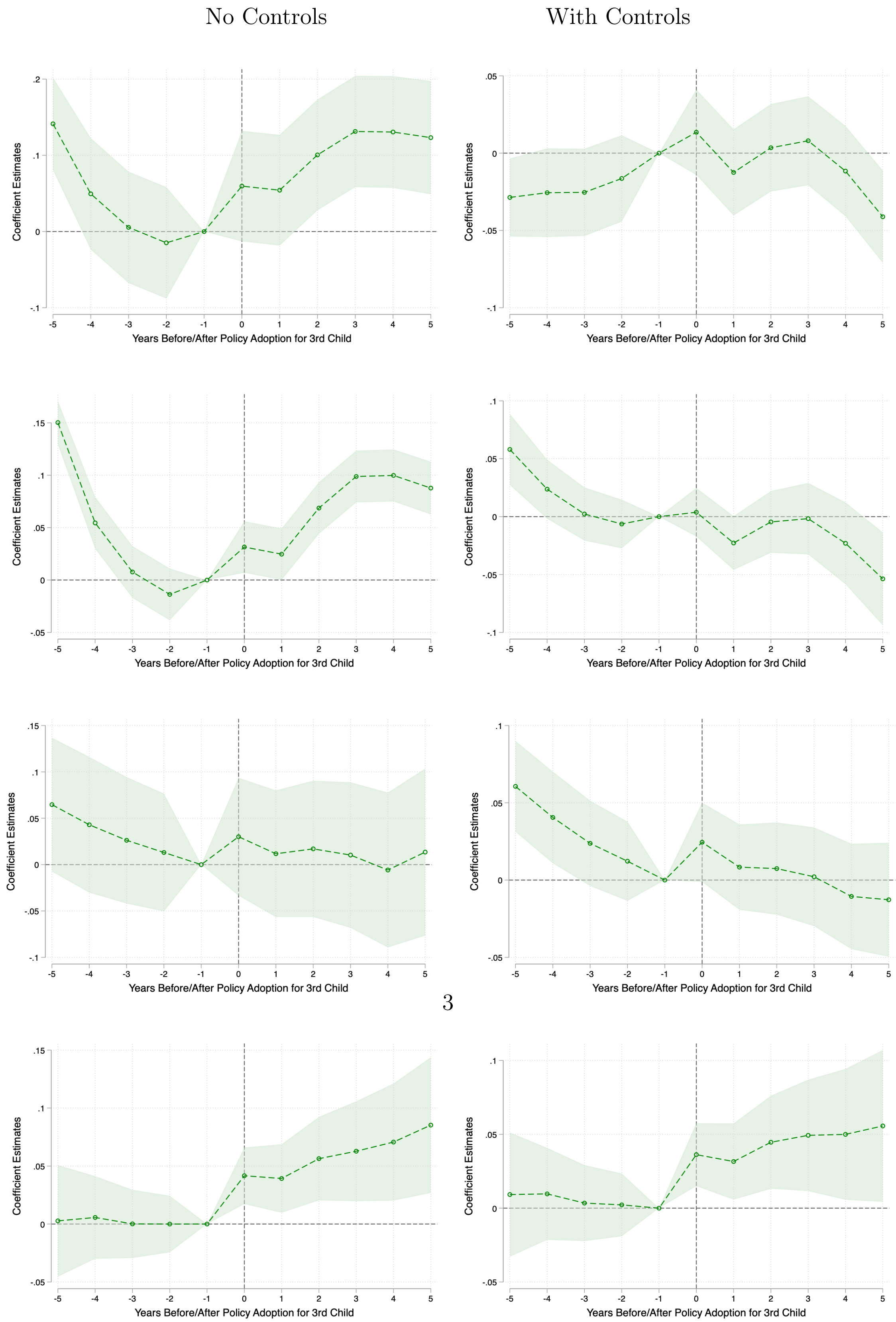

Figure A.7: Event Study Results for 3rd Child Birth Rates

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.

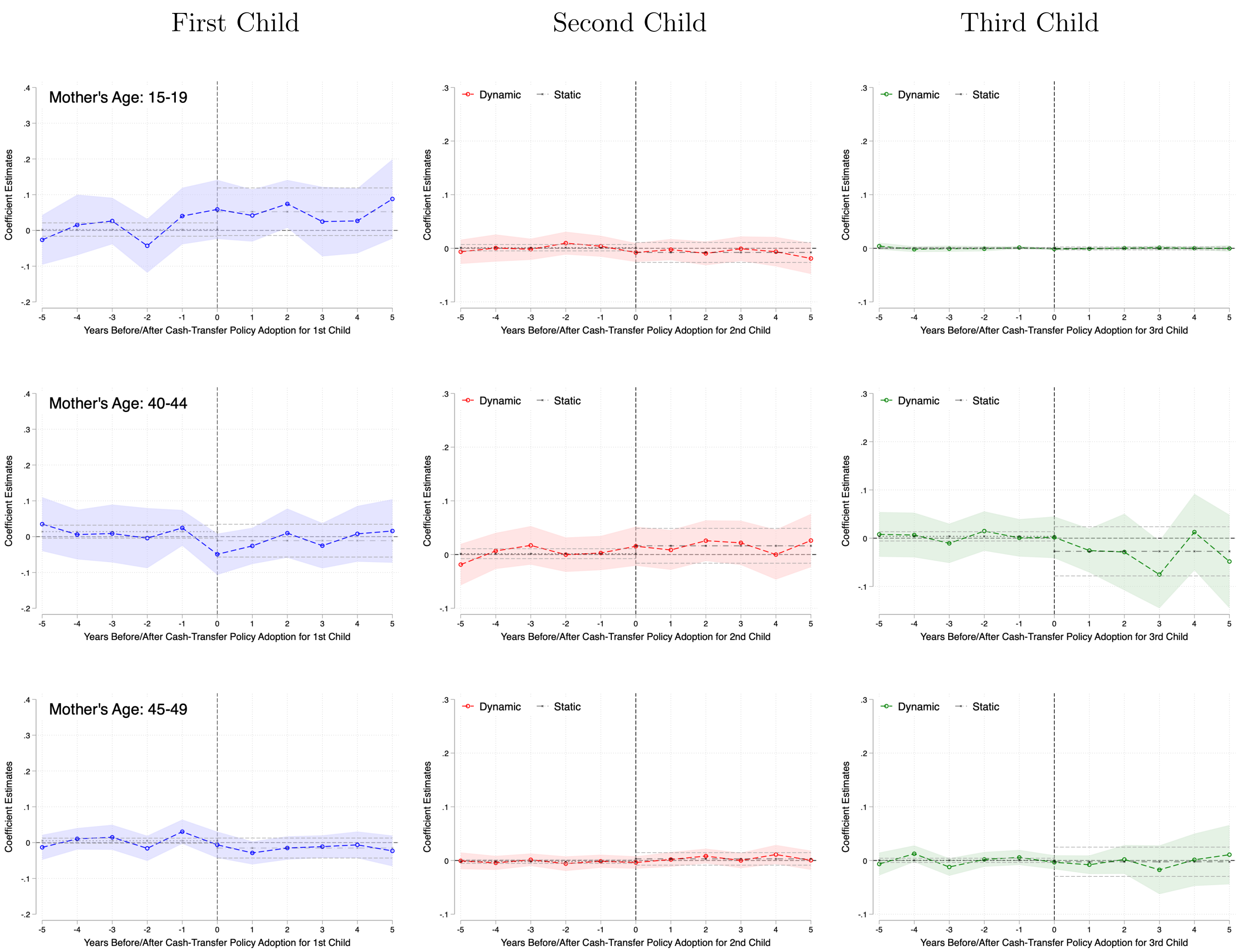

Figure A.8: Birth Rates Before and After Policy Implementation (Ages: 15-19, 40-44 and 45-49)

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 1st child (left in blue), 2nd child (center in red), and 3rd child (right in green) for adolescent (top panels) and older mothers (middle and bottom panels). The event-study coefficients are estimated based on Eq. 3 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). For each panel, the average values of the estimated coefficients pre-/post-treatment periods are plotted in gray dash-dotted lines. 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 (i.e., district fixed effects and city-by-year fixed effects) and district-level control variables are included. The control variables include total population, percentage of the female population, percentage of adult population (between the ages of 20 and 64), percentage of the elderly (older than 64), net migration rate (total inflow-outflow normalized by population), marriage rate, financial independence rate, and indicators for the gender and political party affiliation of the local government head. In addition, the estimations for the 2nd child (resp. the 3rd child) include the lagged number of births for the 1st child by mothers in the same 5-year age group (resp. the 1st and 2nd 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 1st child (Column 1-2), the 2nd child (Column 3-4), and the 3rd child (Column 5-6) based on Eq. 4. Each observation corresponds to a district-year pair from 2001 to 2015 and is weighted by the female population between the ages of 15 and 49. Across columns, the same set of fixed effects (i.e., district fixed effects and city-by-year fixed effects) and district-level control variables are included. The control variables include total population, percentage of the female population, percentage of adult population (between the ages of 20 and 64), percentage of the elderly (older than 64), net migration rate (total inflow-outflow normalized by population), marriage rate, financial independence rate, and indicators for the gender and political party affiliation of the local government head. In addition, Column 3-4 (resp. 5-6) includes the lagged number of births for the 1st child (resp. the 1st and 2nd child) in levels. For each birth order, the left column includes the cash transfer amount (in levels; measured in 1 million KRW) for the corresponding birth order only; the right column includes the cash transfers for the 1st, 2nd, and 3rd child (in levels; measured in 1 million KRW) as separate explanatory variables. 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.

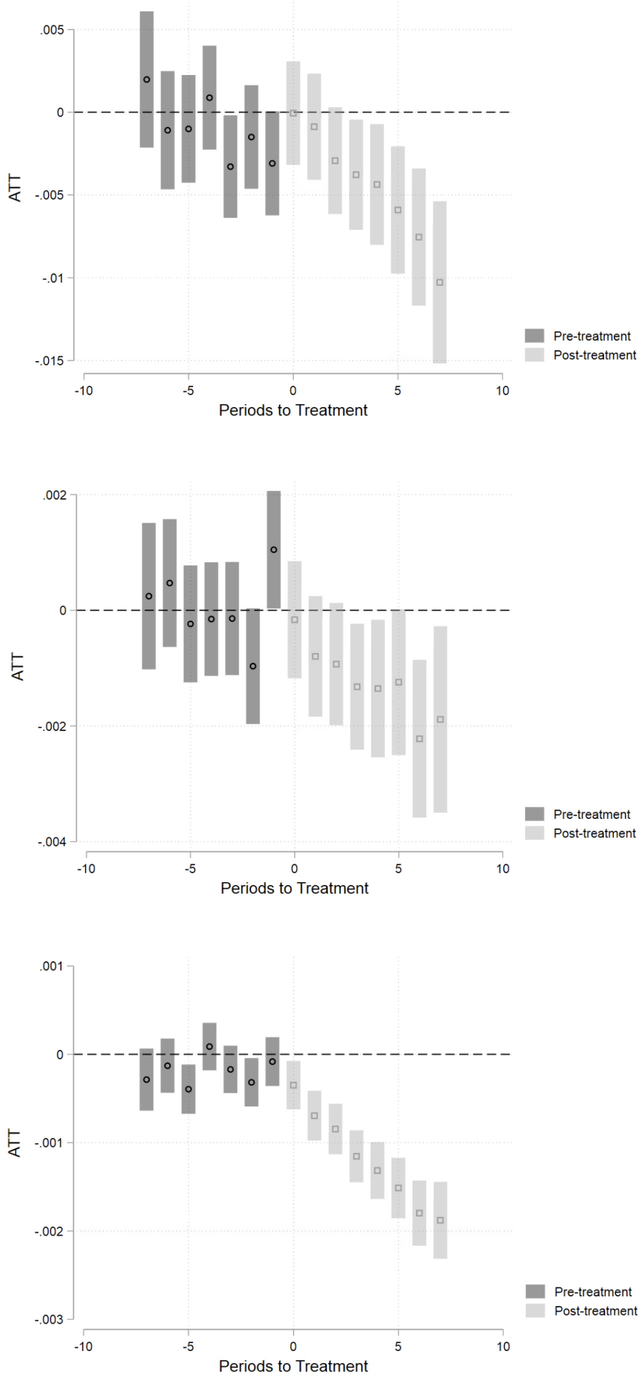

Figure A.9: Infant's Sex at Birth, Birth Weight, and Gestational Age Before and After Policy Implementation (Larger Window)

Notes: This event-study figure plots the estimated changes in the probability that an newborn is a boy (top), birth weight in log kilograms (middle), and gestational age in log weeks (bottom) before and after pro-natalist cash transfer policy implementation. The event-study coefficients are estimated based on Eq. 6 using the doubly robust difference-in-difference estimator (Sant'Anna and Zhao, 2020; Callaway and Sant'Anna, 2021). Standard errors are 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 (i.e., district fixed effects and city-by-month-year fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's educational attainment level, 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 (between the ages of 20 and 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.

	(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 Cash Transfer on Infant Sex at Birth

Notes: This table replicates the results reported in Table 5 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)	(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 Cash Transfer on Birth Weight

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)	(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 Cash Transfer on Gestational Age

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 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 Cash Transfer on Low Birth Weight, Macrosomia, and Preterm Births

Notes: This table reports the estimated effects of cash transfers on additional infant health outcomes (low birth weight in Column 1-2, macrosomia in Column 3-4, and preterm births in Column 5-6). For each dependent variable, the left column reports the estimated effects of cash transfers unconditional on birth order; in the right column, the cash-transfer effect is allowed to differ by birth order. The mean incidence rates of low birth weight, macrosomia, and preterm birth among the 1st children are 4.57%, 3.17%, and 4.75%, respectively. 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 (i.e., district fixed effects and city-by-month fixed effects) are included; family characteristics are controlled for: dummy variables for mother's and father's educational attainment level (no schooling, elementary school, middle school, high school, some college or above), age, occupation, and marital status. The district-level control variables include the total population, the percentage of the female population, the percentage of the adult population (between the ages of 20 and 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. Standard errors are clustered at the district level and reported in parentheses: * Significant at the 5 percent level, ** at the 1 percent level, and *** at the 0.1 percent 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		O		O

Table A.8: The Effect of Cash Transfer on Birth Rates Allowing Migratory Responses

Notes: This table reports the estimated effects of cash transfers on the birth rates (i.e., the cash-transfer elasticities of the birth rates for first child in Column 1-2, second child in Column 3-4, and third child in Column 5-6). Each observation corresponds to a district-year pair from 2001 to 2015 and is weighted by the female population of ages between 15 and 49. Across columns, the same set of fixed effects (i.e., 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 (between the ages of 20 and 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, Column 3-4 (resp. 5-6) includes the lagged number of births for the 1st child (resp. the 1st and 2nd child) in log unit. Column 1, 3, and 5. For each parity, the left column reports the results summarized in Table 4; the right column reports the coefficient estimate allowing migratory responses by excluding the percentage of the adult population and net migration rate from the set of control variables. Standard errors, clustered at the district level, are reported in parentheses: * Significant at the 5 percent level, ** Significant at the 1 percent level, and *** Significant at the 0.1 percent level.