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Does Pro-Natalist Cash Transfer Work? Evidence from Local Programs in South Korea

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Abstract

This paper estimates the effects of pro-natalist cash transfers (baby bonus) on birth outcomes. I exploit rich spatial and temporal variation in these cash transfers and administrative data on the universe of births and deaths in South Korea. The total fertility rate in 2015 would have been 3% lower without the cash transfers. The cash-transfer elasticities of birth rates vary widely across birth order and mother's age. These financial incentives encouraged working mothers to have second and third children. I observe a decrease in gestational age among these working mothers, which in turn led to an overall reduction in birth weight. There is no evidence of changes in early life mortality, but the cash transfers shifted the male-skewed sex ratio towards its natural level.

Keywords: cash transfer, pro-natalist, birth weight, neonatal, missing women, son preference.
JEL codes: H4, H75, I5, J13, J16, J18

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1 Introduction

Total fertility rates have dramatically declined in much of the developed world, and women are having, on average, fewer than 2 children (Strulik and Vollmer, 2015). Policy makers have expressed growing concerns about the consequences of the resulting demographic imbalance (e.g., declining size of labor force relative to dependent population), which is further exacerbated by an aging population (Morgan, 2003; Frejka et al., 2010; Harper, 2014).[1] In response, many policies to boost childbirth, such as cash transfers, parental leave, and tax benefits, have been proposed and implemented around the world.[2] Despite years of research and policy debate, our understanding of how these policies work remains limited. Do these pro-natalist measures increase the number of children ever born per woman? Do they shape any other aspects of birth that will impact children's life course?

To adress these questions, I estimate the effects of pro-natalist cash transfers on the number of births, neonatal health, and sex composition at birth in South Korea. First, I investigate whether or not the introduction of pro-natalist cash-transfer policy had an impact on birth rates, how elastic they are with respect to cash incentives, and variation by birth order and mother's age. Second, I study the effects of the cash transfers on birth quality (birth weight, gestational age, and early-life mortality) and sex composition. An increase in the number of births may be driven by son preference (Basu and de Jong, 2010), or may simply reflect the changes in the timing of childbearing (Andersen et al., 2018). The cash transfers may affect neonatal health outcomes as well as the number of births. 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).[3] Changes in birth quality would affect children's long run outcomes (Almond and Currie, 2011; Almond et al., 2018). I compare birth outcomes of babies born in districts that adopted cash-transfer programs in different years with varying degrees of cash-transfer generosity.

South Korea—the empirical environment of this paper—provides an ideal setting to study pro-natalist cash transfers, which provide cash subsidies to parents when they have a baby. There is rich variation in both pro-natalist policy implementation timing and the amounts of cash transfers by *birth order* (first, second, and third), over *time* (year), and across *granular spatial units* (hereon

[1] Jones (2020) emphasizes the importance of policies related to increasing fertility and shows that, without such measures, we may converge to an *empty planet* in which "knowledge and living standards stagnate for a population that gradually vanishes".

[2] The U.N. Population Division (2011) documents that 40 out of 47 countries with low fertility had pro-natalist polices 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 Gautheir (2007) for a literature review.

[3] Amarante et al. (2016) find that a social assistance program in the form of cash transfer reduced the incidence of low birth weight for poor families in Uruguay. The literature testing the quantity-quality model of fertility (Becker and Lewis, 1973) has found mixed or no evidence of tradeoffs between family size (or the number of children) and birth quality (Black et al., 2005; Angrist et al., 2010; Millimet and Wang, 2011; Liu, 2014; Mogstad and Wiswall, 2016).

referred to as districts).[4] I leverage this rich time-series and cross-sectional variation to identify the causal effects of pro-natalist cash transfers on various outcomes associated with births. I construct a yearly panel data set of districts from 2000 to 2015 with pro-natalist cash transfer amounts and the number of births (decomposed by birth order and mother's age) to estimate the cash-transfer effects on birth rates and merge this data set with the universe of confidential birth registry records to study the effects on neonatal health (birth weight and gestational age) and the sex of newborn babies. Furthermore, I use confidential birth-death matched registry data for children born between 2010 and 2013 and investigate the cash-transfer effect on early-life mortality.

For causal identification, I include both district fixed effects and the city-by-time fixed effects to the estimating equations throughout this paper. The district fixed effects purge out any time-invariant district-level characteristics such as baseline demographic composition. The city-by-time fixed effects absorb any trends and changes common across districts within each city in a given time (e.g., a year or a month) as well as any aggregate shocks and changes taking place at the national level. Because districts are relatively granular with respect to their geographic size, these fixed effects capture region-specific shocks for each time period; 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. Then, the key identification assumption is that all other determinants of birth outcomes, net of observed control variables as well as the rich set of fixed effects, are orthogonal to cash transfer implementation timings and generosity.

Exploiting plausibly exogenous variation in the pro-natalist cash transfers across space and over time, I find that the cash transfers increased birth rates. Based on an event study framework, I estimate a statistically significant increase in the birth rates across birth orders. The effect of cash transfers for a first child was largest among younger mothers (ages 25-29). For mothers between the ages of 30 and 34, having a second child was the most pronounced effect. Finally, for those between the ages of 35 and 39, I observed that cash transfers influenced having a third child. Leveraging the variation in the cash transfer generosity, I estimate the elasticity of birth rates with respect to cash incentives. I find that a 10% increase in the cash transfers raised birth rates by 0.6%, 0.4%, and 0.4% for first, second, and third children, 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 3.0% lower than the observed total fertility rate in 2015.

I provide further evidence that the increase in birth rates were driven by changes in the child-bearing decisions of parent(s) at the *margin* of having an additional child. I uncover two new, important effects of these transfer programs. First, the cash transfers for a birth order affected only the birth rate at the corresponding birth order, but not 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

[4]Districts in South Korea are the smallest administrative units with self-governing authorities, and they form 17 metropolitan cities and provinces such as Seoul and Gyeonggi (hereon referred to as cities).

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 females older than 40.

The cash transfers resulted in unintended consequences on neonatal health. Based on the universe of confidential birth registry records from 2000 to 2015, I estimate statistically significant negative effects of cash transfer on gestational age. Conditional on gestational age, the direct effect of cash transfers on birth weight is positive and large. Birth weight increases by 9.01 grams among second children and 3.06 grams among third children. 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 a parent(s) received varying amounts of the cash transfers were more or less likely to die before reaching the age of 1 and 5.

The cash transfers had another unintended consequence on sex at birth. These cash transfers modulated the sex-ratio imbalance favoring boys from son preference. 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"—from sex selective abortion and infanticide.[5] The sex ratio among third children over time, however, showed dramatic decline from a high sex-ratio favoring boys in 2000 and reached a level consistent with the natural sex-ratio at birth. I find that the pro-natalist cash transfers decreased 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 at the 2015 level, the sex ratio among third children born in 2015 would have been 114.3 boys per 100 girls.

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 being born by each woman, not a mere reflection of temporal adjustment of childbearing timing and the spatial redistribution of families who were going to have babies into places with more generous cash transfers. I conclude that the selection is most plausible mechanism explaining the cash-transfer effects on neonatal health. I find that the probability that a baby has a mother who is working (employed) increases with cash-transfer generosity. However, these mothers are more likely to have time constraints and have a shorter pregnancy duration on average relative to those who are not working. This selection explains why the cash-transfer effect on gestational age is negative, which ultimately lowers birth weight. Furthermore, I verify that this selection does not explain why cash transfers reduced sex-ratio at birth favoring boys: the effects of cash transfers on the sex of newborn babies are the same for working and stay-home mothers.

[5] A plethora of papers repeatedly find that the natural sex-ratio 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 2 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.

This paper builds upon the existing literature in economics analyzing the effects of pro-natalist policies on fertility. Lalive and Zweimüller (2009) find that the extension of parental leave in 1990 in Austria increased the probability of women having an additional child, while Andersson and Duvander (2006) find no such 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).[6] 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; Malkova, 2018).[7] Hong et al. (2016) examine 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 fertility rate.[8]

This paper is closely related to the large aforementioned literature studying the effects of pro-natalist policies on the number of children. In this paper, I bring several innovations to the literature. First, my research offers a set of new important insights about pro-natalist cash transfers, thanks to the rich variation in cash-transfer generosity as well as implementation timings. By estimating the elasticities of birth rates to cash transfers by female age groups and birth orders, I find that a parent(s) at the margin of having an additional child are incentivized by the cash bonus: no inframarginal effect. Similarly, only the mothers who are in their prime age for childbearing respond to the cash transfers.

Second, this paper contributes to the literature on son preference. Several papers document son preference and male-skewed sex ratio at birth, especially in Asia.[9] Focusing on South Korea, Choi and Hwang (2020) show that son preference has been diminishing in recent years.[10] The findings in Yoo et al. (2016) also suggest declines in son preference, but report that a decision to have a third child depends largely on the sex composition of first and second children. Based on a survey, Jayachandran (2017) estimate 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 with higher male-to-female ratios. Anukriti (2018) studies "Devirupak Scheme", which provided cash transfers based on the number of children and sex composition in India. She found that fertility fell, but son preference intensified. With fewer kids, families placed higher value on having a son over the financial incentives associated with having girls. I document that much of the decline in the naturally skewed male-to-female ratio since 2000 is driven by the

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

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

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

[9] See Bongaarts (2013) for a review on son preference and fertility decisions.

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

decline of the sex ratio among third children. I show that the pro-natalist cash transfers reduced this ratio. To the best of my knowledge, this paper is the first to show that a pro-natalist policy interacts with son preference and unintentionally resolves sex ratio imbalance in a cultural context where having sons is favored over having daughters.

Third, this paper fills the gap in the literature by exploring the broader implications of financial incentives for having babies. There are a few papers that study the effects of pro-natalist policies on a wide range of outcomes, other than the number of births. For example, González (2013) employs a regression discontinuity design and estimates the effects of the introduction of a universal child benefit on a range of fertility, household consumption, and maternal labor supply.[11] Yakovlev and Sorvachev (2020) find 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. In contrast to these papers, I study whether or not the pro-natalist cash transfers had any effects on the 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).[12] Furthermore, this paper studies the effect of cash transfer on 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 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 neonatal health outcomes and sex. 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 local information on cash transfer policies and demographic and other relevant local characteristics, and the number of births.[13] To investigate the effects of cash transfers on neonatal health outcomes, early-life mortality and sex at birth, 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

[11] Lalive and Zweimüller (2009) find that extended parental leave increases the probability of having an additional child and reduce return to work.

[12] 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).

[13] During the sample period, some districts were merged or split. Because the policy information for these districts no longer exists, I restrict the sample to 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.

pro-natalist cash transfer policies in South Korea, explain the data sources 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 about low fertility rates and the rapidly aging population, the national government in 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. It is important to note that the Plans only outlined normative goals and operate at the national level. In line with the administrative arrangements of the national and local governments (Local Autonomy Act, 1990), national government policies on welfare, generally speaking, operate at the macro-level and may not vary across districts and cities.[14] Therefore, it is reasonable to assume national welfare policies indiscriminately influence districts uniformly.

As early as 2001, local governments adopted a pro-natalist cash transfer policy independently from the national government, which provided cash transfers to families with newborn babies. Since 2012, this policy has been adopted by all the districts and become ubiquitous. While the structure of this policy (e.g., eligibility and transfer method) are virtually identical across districts, the amount of cash transfers varies widely by districts and birth order. Because these transfers are local policies, local governments finance them using their budget, which is the sum of local income tax revenue and intergovernmental transfers from the national government.[15] To receive cash transfers from the local government, the parent(s) of a newborn baby simply register their baby's birth at a civic center in their respective district of residence. During the process of birth registry, local government officials verify the parent(s)' residence using their resident registration numbers (e.g., analogous to social security number in the U.S.).[16]

Figure 1 summarizes the local pro-natalist cash transfer policy, the total fertility rates, and male-to-female sex ratio at birth. The top panel plots the number of newborn babies over time (dashed line) and the average amount of cash transfers conditional on having adopted the policy (solid line) for districts providing cash transfers to families. In the center and bottom panel, average total fertility rates, the male-to-female ratios at birth (solid lines) and cash-transfer prevalence (dash line; as in the top panel) are plotted over time. As districts increasingly enacted this policy,

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

[15]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 (2020).

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

national fertility trends appeared to steadily reversed. During the same period, the sex ratio at birth declined to the natural sex ratio at birth of 1.05.[17] Figure 1 suggests that the local cash transfers may be responsible for reversing the declining fertility rate and lowering the male-favored sex ratio at birth. In order to identify the causal effects of these cash transfers on a range of birth outcomes, I construct a panel dataset of districts with local policy, demographic, and governmental characteristics whereby I merge restricted-access confidential birth and death registry records with the district level information.

2.2 Data

District-Level Variables: Cash Transfers and Birth Rates

The district-level data set comprises of three key components. The first component pertains to variables pertaining to the local cash transfer policies. I filed an Official Information Disclosure Act request to each district to disclose information on the amount awarded to parents for their first, second, and third child, as stipulated by the cash transfer policy in each district.[18] Based on responses by officials and cross-validation using alternative sources (e.g., administrative policy reports, online repository of local ordinances, and regulations and telephone survey), I build a yearly panel data set of districts from 2000 to 2015 with the amounts of pro-natalist cash transfers at each respective birth order.

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, reaching 41%, 89%, and 100% for a first, second, and third child, respectively. Panel A also reports the means and standard deviations of cash transfer amounts in 1,000,000 KRW or approx. 1,000 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), and by birth order (rows). In particular, changes in cash transfer generosity over time can be explained both by more districts adopting the policy with higher transfer amounts and that those already already executing the policy increased generosity.

The second set of variables relate to the number of births in each district. The Korean Statistical Information Service (KOSIS)–an official government online portal–publishes annual district-level total fertility rates and the number of births for each birth order (first, second, third). To understand how the effects of cash transfers differ across female age groups, I need to know how many children

[17] As mentioned earlier, the total fertility rates before 2000 had been monotonically decreasing. Prior to 2000, the sex ratios had been relatively stable at around 1.1 boys for each girl since 1996.

[18] 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 local pro-natalist cash transfers through a telephone survey. This paper focuses on birth parities from first to third as they together constitute over 98.9% of the total births in South Korea during the sample period. Similarly, in most cases, the amount of the cash transfer awarded to families remains the same after a third child.

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 record data housed at the Bureau of Statistics of South Korea. Since 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 and year. 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 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 child 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 child born in district d in year y by mothers whose age falls in age group a (5-year intervals from 15 to 49); $fpop_{a,d,y}$ is the female population in age group a living in d in year y. Note that total fertility rate $TFR_{d,y}$ can be expressed as a function of the age- and order- specific birth rates. That is,

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

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 observables to control for local demographics and government characteristics.

The demographic characteristics I include total population level, fraction of female population, proportion of adult population (between the ages of 25 and 60), marriage rate, and net migration rates (i.e. net inflows per 1,000 people). Government characteristics include the gender of the local government head and the financial independence rate, which measures how fiscally autonomous a local government is in the absence of intergovernmental transfers from the national government.

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 2000 to 2015, to study the effects of pro-natalist cash transfers on neonatal outcomes including birth weight measured in kilograms, gestational age measured in weeks, and sex. Each administrative record includes detailed demographic information on the newborn baby (e.g., date and place of birth and birth order) and the parent(s) (e.g., age, marital status, educational attainment level, and occupation).[19] The total sample size is 7,081,285 births. Table A.1 reports the average birth weight, gestational age, and fraction of male births by birth order for selected years.

For the cohorts born between 2010 and 2013, their birth records are matched with death records should they have died before reaching the age of 5. The sample size for this subset of the data is 1,711,949 births. 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. These estimates are considered low among developed countries.

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 district-level policies. In this section, I formally investigate the determinants of policy implementation timing and generosity.

First, I employ an accelerated failure time framework to understand the local factors that determined how long it took for a district 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 duration of how long it takes a district to adopt the pro-natalist cash transfer T_τ since any baseline year τ has 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 since a baseline year τ as a function of baseline local characteristics $X_{d,\tau}$ and a log-normally distributed stochastic error term ϵ_τ. After applying some algebraic operations, I obtain the following equation:

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

[19] 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.

This equation corresponds to an accelerated failure time model. The interpretation of the equation itself 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 that affect the districts within each city uniformly.

Table 2 summarizes the results estimating Eq. 1. The baseline year changes from 2000 to 2006 and the number of observations decrease across columns as some districts started adopting the cash transfer program. Most of the observed demographic characteristics (e.g., population, total fertility rates, fraction of female population, fraction of elderly population, and net migration rate) do not explain the timing of policy adoption. In particular, all of the estimated effects of the total fertility rates are statistically not different from zero and the sign of the estimates flips depending on the baseline years. While none of estimates are statistically significant, the estimated coefficients for marriage rate are consistently negative, which implies that districts with more newly married couples adopted policies earlier, conditional on population size and age composition. Districts with a higher fraction of adults in the population, while holding the proportion of the elderly constant, tended to have a longer duration of time preceding adoption of the cash transfers. These districts may have been less concerned about the declining fertility rates because a greater number of the population had the potentials for childbearing. Districts with conservative local governing heads implemented the policy later. 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.

Second, I investigate the extent to which local characteristics explain the generosity of cash transfers. I 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 sin transformation of the cash transfer amounts provided to families with a new baby of birth order p in district d in year y. 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 turns affect local government budget); $X_{d,y}$ includes the demographic and local government characteristics of district d observed in year y.

Table 3 summarizes the results estimating Eq. 2 by birth order p. For each birth order, the first column includes all years and districts. The results are driven by both the extensive margin of policy adoption and the intensive margin of policy generosity changes over time. The second column excludes district-year observations with zero cash transfers (mostly prior to policy adoptions). Under this sample restriction, I can focus on the intensive margin of cash transfers and study their determinants. When looking at both intensive and extensive margins together (Column

1, 3, and 5), the results indicate that the across birth orders, the cash transfer amounts were lower in districts with a higher fraction of females and adults in the population. In contrast, all of the respective estimates in the other columns, which exclude zeros, lose their statistical significance.

While each estimate 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.[20] These local characteristics (e.g., fraction of female population, age composition, and cultural norm proxied by party identification of the local leaders) are likely to be correlated with birth outcomes. Thus, I control for these factors and include the district fixed effects and the city-by-year fixed effects throughout the rest of my analysis.

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 neonatal health and sex using the individual-level confidential registry records. I leverage the temporal and cross-sectional variation in cash transfers 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 for each district, I conduct an event study and semi-parametrically estimate the pro-natalist policy effects before and after based on the specification as follows:

$$\ln BR_{p,d,y} = \phi_d + \psi_{c(d),y} + \delta X_{d,y} + \sum_{\tau=-7}^{7} \gamma_p^{(\tau)} D_{p,d,y}^{(\tau)} + \epsilon_{p,d,y}, \tag{3}$$

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 (e.g., local cultural norm). City-by-year fixed effects $\psi_{c(d),y}$ flexibly capture the year-to-year changes in the city-level shocks as well as the national-level shocks that are 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.[21] $\left\{ D_{p,d,y}^{(\tau)} \right\}_{\tau=-7}^{7}$ is a set of dummy variables indicating whether or not the

[20]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 1% significance across birth orders. However, I cannot reject the null when excluding zero cash transfer cases.

[21]These covariates include local demographics and government characteristics. They are the same set of variables used to study the determinants of the implementation timing (excluding total fertility rates) and generosity. In addition, I include a set of lag number of births of birth order $p' < p$ when estimating the cash transfer effect on the birth rates of birth order p. For instance, the lag number of births for the first birth order is included when the birth rates of 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

number of years since district d implemented the cash transfer policy for birth order p is equal to τ in year y.[22] $\epsilon_{p,d,y}$ is an error term.

Event study coefficients $\left\{\gamma_p^{(\tau)}\right\}_{\tau=-7}^{7}$ 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 *relative* to the leave-out year. Birth rates count babies born during each calendar year. This means that the babies born in the year when the district first adopted the cash transfers (i.e., $\tau = 0$) would have been affected by the policy. Therefore, I set the leave-out year to be $\tau = -1$ and equivalently restrict $\gamma_p^{(-1)} = 0$. Based on the estimated event study coefficients, I investigate whether there was a trend prior to the policy implementation and how birth rates change following implementation.

Next, I exploit the rich variation in 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 to estimate elasticities, I take the inverse hyperbolic sine transformation $\sinh^{-1} CT_{p,d,y}$. By preventing dropping observations where the cash transfer amount equals to zero, this formulation allows me to estimate the coefficient of interest using the full history of birth rates I observe in the dataset. 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. Elasticity β_p measures the percent change in the birth rate of birth order p with respect to a 1% increase in pro-natalist cash transfer generosity for the corresponding birth order.

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. The district-level fixed effects purge out any permanent local factors that determine cash transfers and birth rates. Furthermore, as studied in Section 2, the observed time-varying local characteristics and rich set of fixed effects explain about 78 to 85 percent of the variation in cash transfers.

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 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.

to 3,330.

[22] Some observations for districts that adopted their policies relatively later may be more than 7 years before the policy adoption. I group all of these cases as $\tau = -7$. This means $D_{pdy}^{(-7)}$ is an indicator for $\tau \leq -7$. Similarly, $D_{pdy}^{(7)}$ is an indicator for $\tau \geq -7$.

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 local government budget. Lastly, districts are geographically granular and local labor and housing markets are usually defined in terms of a city (or a group of cities). The city-by-year fixed effects absorbs shocks to these local market conditions, which would affect both people's childbearing decision and local government's capacity to operate pro-natalist cash transfers.

Throughout the district-level observations, each observation is weighted using the number of women in the population between the ages of 15 and 49. There are two ways the errors are correlated. First, the errors may be correlated within geographical groupings, which correspond to cities in this case, for each year (Moulton, 1990). Second, there is a concern regarding the serial correlation within a panel dimension, as explained in Bertrand et al. (2004). Therefore, standard errors are two-way clustered by district and city-year pair.

3.2 Individual-Level Analysis: Neonatal Health and Sex

In addition to the number of births, I investigate whether the pro-natalist cash transfers had any effects on neonatal health and sex of infants, which are important determinants of long term individual 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 neonatal health if cash transfers resulted in less investment per child due to increased number of births. They may improve neonatal health as cash transfers serve as an additional resources to take better care of new babies. With respect to the sex of a child, while on a downward trajectory, the son preference in South Korea still remains strong.[23] On the one hand, the cash transfers may provide financial means to parents as they continue to have babies until they have at least one boy or simply more boys. On the other hand, cash transfers may compensate the utility penalty associated with having girls.

To estimate the cash transfer effects on neonatal health and sex, I use individual records of the universe of births in South Korea and estimate

$$H_i = \phi_d + \psi_{c(d),y,m} + \beta \sinh^{-1} CT_{p,d,y} + \omega W_i + \delta X_{d,y} + \epsilon_i, \tag{6}$$

where dependent variable H_i is a measure of neonatal health and sex (e.g., birth weight, gestational age, indicator for boy) of baby i of birth order p born in district d in year-month m. District fixed effects ϕ_d capture all the permanent local factors. City-by-month fixed effects $\psi_{c(d),m}$ flexibly control for the month-to-month citywide shocks that affect birth outcomes.[24] In addition to the

[23] The extent to which sex ratio deviates from its natural level of 105 is especially pronounced among families when they first had daughters as opposed to sons. According 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.

[24] Rich individual records provide me with enough power to introduce the month-by-city fixed effects. Furthermore, being able to do so is important due to seasonality in birth weight and pregnancy duration (Darrow et al., 2009; Bodnar and Simhan, 2008; Boland et al., 2015).

same set of control variables $X_{d,y(m)}$ as in Eq. 4, I leverage the parental information reported on each record and include a set of individual-level controls W_i including indicators for child's birth order and parental educational attainment (no schooling, elementary school, middle school, high school and some college or above), age, marital status, and occupation. ϵ_i is an error term.

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 city-month pair. The identification assumption is expressed as follows:

$$E\left[CT_{p,d,y(m)} \times \epsilon_i | \phi_d, \psi_{c(d),m}, X_{d,y(m)}, W_i\right] = 0. \tag{7}$$

The use of registry records in my analysis is particularly advantageous to justifying the identification assumption described above (Eq. 7). The outcomes of interest are birth weight and gestational age, which heavily depend on parental characteristics and inputs during pregnancy. The large number of observations and the rich parental information allow me to flexibly account for the effects of parental characteristics without assuming a constant marginal effect for each of these observed factors.

While the data used in the individual-level analysis correspond to individual births (thus not panel), there is still a clear panel structure defined in terms of geography (districts and cities) and time (birth dates, months, and years). In terms of statistical inference, I cluster standard errors by districts and city-month pairs in the spirit of Moulton (1990) and Bertrand et al. (2004). Statistical inference results are robust to alternative clustering options.

4 Results

In this section, I present my estimation results in two parts. In the first part, I discuss the district-level analysis investigating the effects of the pro-natalist cash transfers on birth rates. The second part presents the estimation results on the effects of the cash transfers on neonatal health outcomes and sex of children based on the confidential birth registry records spanning the universe of births in South Korea.

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 plot the estimated event study coefficients $\left\{\gamma_p^{(\tau)}\right\}_{\tau=-7}^{7}$. In the top panel, the changes in birth rates of first child relative to the birth rates 1 year prior to 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 transfers for first child were implemented;

all of the event study coefficients post implementation are positive and, except for the year of implementation, statistically different from zero. On average, the birth rates for the first child increased by approximately 8.0 %.[25]

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 the second child were decreasing prior to when the cash transfers were provided. While only a few estimates are statistically different from zero, this downward trend appears to be salient. Upon the policy implementation, the trend reversed and biirth rates started increasing. Relative to the birth rates one year prior to the policy implementation, the birth rates for 2nd child increase by 2.5 %.[26] None of the event study coefficients for years before the policy implementation, shown in the bottom panel, are statistically different from zero. Thus, I conclude that the third child birth rates showed no pre-trend prior to when districts started to offer cash transfers. Thereafter, birth rates increased starting in the year of policy implementation and continue to increase over time. The average increase in the birth rates post-implementation is about 5.5 %.[27]

The event study results above provide evidence that birth rates across birth orders increased after districts started offering cash transfers conditional on child births. The increase in the birth rate for the first child implies not only a greater number of births of that corresponding birth order, but also an increase in the number of mothers who would benefit from cash transfers provided for the second child. Similarly, the reversal in the downward trend in the birth rates for the second children implies that proving the cash transfers to families having a second child increased the number of families with two children, who in turn became potential beneficiaries of cash transfers for the third child. It is important to note that the estimated coefficients across birth orders tend to increase likely from increases in cash transfer generosities over time within districts after their policy adoptions. While the average increase in birth rates were largest for the first child, followed by the third, the per dollar increase in birth rates decrease monotonically if the average cash transfer amount of each birth order are taken into account (i.e., \$577 for first child, \$843 for second child, and \$2,042 for third child).

Next, further decomposing the birth rates into age-specific birth rates, I estimate the changes in birth rates before and after the policy implementations for different age groups of mothers. Figure 4 plots the event study coefficients estimated for each birth order (separated by columns) and each age group of mothers (5 year intervals). First, I focus on age groups of mothers that are prime for childbearing, between the ages of 25 to 39, and more likely to be actively making fertility decisions. First child birth rates increased among relatively younger female population between the ages of

[25]This estimate corresponds to the average of the event study coefficients post-implementation ($\tau \geq 0$). The dashed horizontal lines from 0 to 7 are the lower and upper bounds of the 95% confidence interval.

[26]If the downward trend is taken into account, the magnitude of the increase in birth rates would be larger.

[27]The estimated pre-trends show that the decline in the fertility rates observed in the beginning of the 21st century is driven by the decline in second child births, not those of first and third. In Appendix, Figure A.1 and A.2 plot event study coefficients estimated without any fixed effects and controls and only with fixed effects, respectively. Overall, these figures show the importance of accounting for city-wide economic shocks and permanent local factors to identify the event study coefficients.

25 and 34. Second child birth rates, among mothers between the ages of 30 and 34 increased, while birth rates among younger (25-29) and older (35-39) mothers stayed relatively constant before and after the policy implementation. Lastly, third child birth rates among mothers between the ages of 35 and 39 increased after the policy implementations.[28]

Based on the event study estimation results discussed above, I conclude that the pro-natalist cash transfers increased the birth rates across birth orders. However, these increases do not arise uniformly across age groups of mothers. 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. Furthermore, Figure A.3 shows that the birth rates among mothers between the ages of 15 and 19 and between the ages of 45 and 49 did not change before and after policy implementation across birth orders. The event study results above are estimated using the variation in policy implementation timing; therefore, they do not take into account the fact that these cash transfers varied widely across districts over time in terms of generosity (i.e., how much cash transfers were provided). In the next section, I leverage the variation in cash transfer generosity in addition to policy implementation timing 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.[29] Because the dependent variables are measured in log units and I take the inverse hyperbolic sine transformation, the estimated coefficients approximate the elasticities of birth rates with respect to cash transfers. In order to obtain the exact values, the estimated coefficients must be adjusted as follows:

$$e_{BR_p,CT_p} = \frac{\partial \ln BR_p}{\partial \ln CT_p} = \beta_p \times \underbrace{\frac{\bar{CT}_p}{\sqrt{\bar{CT}_p^2+1}}}_{\kappa(p)}, \text{ where } \beta_p = \frac{\partial \ln BR_p}{\partial \sinh^{-1} CT_p} = \frac{\partial \ln BR_p}{\partial CT_p / \sqrt{\bar{CT}_p^2+1}}. \tag{8}$$

Thus, I can re-scale the coefficient β_p in Eq. 4 by adjustment factor $\kappa(p)$ to compute elasticities. I evaluate the adjustment factors based on the average cash transfers in 2015 (i.e., $\bar{CT}_p = E\left[CT_{pdy}|y = 2015\right]$).[30]

In Column (1) and (2), the dependent variable is the log of first child birth rates. According to the first column, the estimated effect of the cash transfer provided to first child on birth rates

[28]While the estimated coefficients prior to policy implementation are upward trending, none of these estimates are significantly different from zero at the 5% significance level. Third child birth rates among younger mothers had been decreasing prior to the policy implementation and continued to decrease after a statistically insignificant increase immediately after policy implementation.

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

[30]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 10,000) are very close to the elasticities I compute by multiplying an adjustment factor.

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 0.6% after applying the adjustment factor as shown in Eq. 8. 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 introduce cash transfers for the second and third child as additional control variables. In Column (2), while the coefficient estimates for cash transfers allocated to families having their first child does not change in a meaningful way and stays statistically significant, both of the estimated effects of the cash transfers for higher order births are statistically not different from zero at the 5 significance level.

The results for the second child, reported in Column (3) and (4), also show that the cash transfers increased the 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 0.4%. 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). Furthermore, none of the coefficient estimates of the cash transfers to the other birth orders are statistically different from zero at the 10% 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 results based on the third child birth rates are qualitatively similar to the results discussed above. The estimated coefficient in Column (5) implies that third child birth rates increase by 0.4% as the cash transfers for that birth rate goes up by 10%. Similar to the case of second birth before, the cash transfers provided to families with one or two children should not impact whether the 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.

Overall, the results demonstrate that cash transfers increase the birth rates across birth orders. These effects were birth order-specific, however, in the sense that the cash transfers provided only affected the birth rates at the corresponding birth order. In other words, the cash transfers did not *inframarginally* affect the fertility decisions, but instead lead to an increase in total fertility by encouraging families at the margin of having an extra child to choose to have a baby. Figure 4 plots the elasticities of age-specific birth rates with respect to cash transfers. The effects of cash transfers are statistically zero across 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 implied elasticities of 0.06 and 0.04 are, albeit relatively small, within the range of the elasticities of fertility with respect to various forms of financial incentives in other developed countries (Cohen et al., 2013). A back of envelope calculation implies that in the absence of these cash transfers, the total fertility rate in 2015 would have been reduced by 3%, which corresponds to approximately 428,274 less children.[31]

4.2 Individual-Level Analysis: Birth Weight, Gestational Age, and Sex

Neonatal Health Outcomes: Birth Weight and Gestational Age

Column (1) in Table 5 reports the results estimating Eq. 6 for birth weight as the dependent variable. An increase in the amount of pro-natalist cash transfers led to a decrease in with weight; this estimate of -0.0014 is statistically different from zero at the 0.1 significance level. In addition, the coefficients estimates of the indicators for second child, third child, and a boy are all positive and significant. They imply that male babies on average weigh 3.1 % (100 grams) more than female babies; a second child and a third child are 0.5% (16 grams) and 1.3% (43 grams) heavier than a third child, who on average weights about 3228 grams. In Column (2), 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 linked to cash transfers among third children. The estimated effects for the first and second child are not significantly different from zero, while the estimated effect for the third child is statistically differently from zero at the 0.1 significant level.

Column (3) and (4) report the results introducing the log of gestational age as an additional explanatory variable to Column (1) and (2). The estimated effects of gestational age on birth weight across columns are tightly estimated, positive, and large in magnitude: a 1% increase in gestational age increases birth weight by 2.2%. Restricting the effect of cash transfers such that the effect is the same across birth orders in Column (3), the estimated coefficient is positive. Compared to Column (1), the sign of the coefficient changes from negative to positive: controlling for gestational age, the cash transfers increased birth weight. Allowing this effect to differ by birth order in Column (4), I find that birth weight among the second and the third child increased with respect to cash transfers, while the cash transfers did not affect the birth weights among first children.

The magnitudes of these estimates are consistent with other papers estimating the income effect on birth weight. For instance, Hoynes et al. (2015) find that an increase in income of $1,000 (in 2009) is associated with an increase in birth weight by 6.4 grams overall in the U.S. (by 2.8 grams for non-Hispanic white mothers). Transforming my estimates to semi-elasticities, I find that a $1,000 increase in cash transfers on average increased the birth weights by 9.01 grams among second children and 3.06 grams among third children. In Appendix, I explore whether the cash transfers had any impact on the incidences of low birth weight (less than 2500 grams) in Table A.3. I find that the cash transfers reduced the incidence of low birth weight among second and third children, but not among first children.[32]

[31] This estimate serves as a lower bound because the basic computation does not take into account the dynamic aspect of cash transfers that changes the distribution of families at "risk" of having an extra child.

[32] I also study the effect of cash transfer on overweight babies (weighing more than 4,000 grams). The results are shown in Table A.4. The probability of babies being overweight is reduced for the third child by cash transfers, but increased for second children.

The discrepancy between the first two columns and the second two columns in Table 5 corresponds to the indirect (or mediated) effect of cash transfers on birth weight *through* gestational age. The sign change of the estimates from negative to positive implies that the indirect impact is negative and large enough to dominate the direct effect of cash transfers shown in Column (3) and (4). Combining it with the result on the positive effect of gestational age on birth weight, I postulate that the cash transfers must have reduced gestational age among higher order births. I report the effect of cash transfers on gestational age in Table 6.

In Column (1), the estimated effect of the cash transfers on gestational age is negative and statistically significant. When this effect is allowed to differ across birth order in Column (2), I find that the estimated effect observed in Column (1) is driven by the changes in gestational age among second and third children, but not first children with respect to the cash transfers. These decreases are statistically different from zero at the 0.1 percent significance level. If there is a "backdoor" effect of cash transfers on gestational age through birth weight, these estimates would combine both the direct and indirect effects.

Given the findings above (i.e., positive effects of cash transfers and gestational age on birth weight), the direct effect would be more negative once after controlling for birth weight. In Column (3) and (4), I show the estimation results with birth weight as a control variable and find that the estimated cash transfer effects do not change compared to the results in the first two columns without including birth weight in the estimation. The results overall indicate a causal relationship beyond correlation between gestational age and birth weight: a change in gestational age causes birth weight to change.

This structural relationship further clarifies how the cash transfers affects these neonatal health outcomes. While these financial incentives improve birth weight of a baby, they negatively affect the gestational age and shorten pregnancy duration, which in turn lowers birth weight. Ultimately, the indirect effect (-) dominates the direct effect of cash transfers on birth weight (+) and the overall effect turns out to be negative. Later in Section 5, I further discuss the structural relationship between cash transfers, birth weight, and pregnancy duration and study the mechanism explaining why these pro-natalist cash transfers resulted in lowering gestational age, on average. Here, I further investigate whether the pro-natalist cash transfers had impacts on the early-mortality either directly or via changes in the neonatal health outcomes.

In Table 7, I summarize the results estimating the effects of the pro-natalist cash transfers on the infant mortality in Column (1)-(4), focusing on the birth cohorts from 2010 to 2013 whose death records were matched with their birth records. In Column (1), the estimated effect of the cash transfer policy on infant mortality is positive: although small in magnitude, infant mortality increased with cash transfers, which ultimately led to a decrease in birth weight. In Column (2) where I control for birth weight and gestational age, the coefficient still remains positive, but the magnitude is halved consistent with the direction of bias. However, in both columns, the estimated effects are not statistically different from zero. Neonatal health outcomes are key determinants of infant mortality, as a 10% increase in birth weight and gestational age decreases infant mortality by 0.19 and 0.75 percentage points, respectively. All the estimated effects in Column (3) and (4), which allow the effect of the cash transfers to vary across birth order without and with birth weight and gestational age, are not significantly different from zero.

The results looking at 5-year mortality reported in Column (5) and (8) for the same 2010-2013 cohorts also indicate that the cash transfers did not have any impact on the mortality in the long run. The estimated coefficients are statistically indistinguishable from zero and quantitatively close to zero. Furthermore, although the neonatal health measures have statistically significant effects on mortality, the extent of their effects is largely reduced. The gender differences in mortality rates are statistically different. Boys are 0.06 and 0.0007 percentage points more likely to die before reaching the age of 1 and 5 years, respectively; however, these estimates are arguably small in terms of magnitude.[33]

Sex Ratio at Birth

Families in South Korea historically have had a strong preference for a son. This tendency has resulted in an unnaturally skewed sex ratio favoring boys, especially since the introduction of ultrasound in 1980s. However, the sex ratio started to decline since 2000, as shown in Panel C of Figure 1, and has gradually moved towards the natural sex ratio. In Panel C of Table A.1, I report the average sex ratio for every three years from 2000 for each birth order. The sex ratio among first children was already at the natural level and has not changed much since then. The decline observed in Figure 1 is driven by the decline in the sex ratio among the second and particularly third children. Here, I investigate whether the pro-natalist cash transfers had any effects on the sex ratio.

Table 8 summarizes the results estimating the effect of the pro-natalist cash transfers on the probability that a baby is male. During the sample period of 2000 to 2015, the average proportion 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 in front of the dummy variables for second and third order births are positive and statistically significantly different from zero at least at the 1% level. In other words, there are more boys being born than girls. Furthermore, the increase in the sex ratio is higher for third children (105.9 for second children vs. 120.9 for third children).

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 1.81 percentage points. Allowing this effect to vary across birth orders in Column (2), I find that the cash transfer did not affect the sex ratio among first children and had a positive, but negligible effect on the sex ratio among second children. Among 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 cash transfers provided for third children reduces the sex ratio by 2.76 percentage points. According to a back of the envelop calculation, this means that

[33] Analyzing the mortality and morbidity of mixed-gender twins in the U.S., Zhao et al. (2017) find that infant mortality among male twins is 0.25 percentage points higher than female twins.

increasing the cash transfers by 171% would lower the unnaturally male-favored sex ratio for third children to the sex ratio among first children.

Without cash transfers and holding everything else constant in 2015, the sex ratio among third children born in 2015 would have been 114.3 boys per 100 girls. This counterfactual sex ratio is lower than the observed average taken over the sample period (2000 to 2015). This change can be attributed to forces other than the cash transfers: e.g., changes in social norm as discussed in Chung and Gupta (2007). The difference between the counterfactual sex ratio without the financial incentives and the observed sex ratio of 105.3 boys per 100 girls in 2015 demonstrates to the effect of the local pro-natalist cash transfers on sex ratio.

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 mechanisms explaining the main results of this paper.

Spatial Redistribution of Fertility: Migration

Kim (2020) 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 includes these local pro-natalist cash transfer. 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 available through the Micro-Data Integrated Service provided by Statistics Korea. A record is generated each time a household declares its residency.[34] 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.[35] 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:

$$\ln F_{p,o,d,y} = \phi_{o,d} + \phi_{o,y} + \phi_{c(d),y} + \delta X_{d,y} + \kappa_p \sinh^{-1} CT_{p,d,y} + \xi_{p,o,d,y}, \tag{9}$$

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 origin-district d in year y; district-pair fixed effects $\phi_{o,d}$

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

[35] 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.

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; 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 9 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 did not affect the migration decisions of people who may benefit from these transfers. In Column (2) and (3), the effects of pro-natalist cash transfers are positive and statistically at the 1% significance level for the second child and 0.1% for the third child. Applying the adjustment factors, as explained in Eq. 8, the results imply that a 1% increase in the cash transfers for second and third child increases the probability of households who are potential beneficiaries migrating to new districts by 2.3% and 2.8%, respectively. In Column (4), I test whether there exists a systematic correlation between the migration patterns among non-beneficiaries and cash transfer generosity. The results imply there is not.

Then, are the positive effects of cash transfers on birth rates partly, if not entirely, driven by the spatial redistribution of families across districts instead of changes in the number of children being born? My findings suggest that this is not the case. The results presented in this paper are estimated while holding the number of potential beneficiaries constant as explained in Section 3.1. Therefore, the estimated coefficients in Table 4 exclude the effects of cash transfers on birth rates through the changes in the stock of potential beneficiaries. I replicate Column (1), (3), and (5) in Table 4 excluding female population, lag number of lower birth-order births, and net migration rate, thereby additionally loading the effect of migratory responses on birth rates onto the coefficient. Table A.5 in Appendix reports the results in Column (2), (4), (6) respectively; Column (1), (3), and (5) are the same as the results reported in Table 4. Comparing the first and second column for each birth order, the estimates in the second columns are larger as expected. However, allowing the migratory response leads to large and statistically significant difference only for only second and third child in line with the gravity estimation results in Table 9.

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. In Table 10, Column (1) and (2) report the results estimating Eq. 6 using the log of mother's age and the inverse hyperbolic sine transformed 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 babies and also had little impact on marital duration prior to the birth their first child. The small, yet statistically significant decrease in mother's age among second and third children are likely driven by mothers choosing to have their second or third child with the assistance of cash transfers, who would otherwise have completed childbearing after having their first and second child. Furthermore, my results in Figure 3 serve as evidence ruling out this potential tempo effect. The effect on first children was positive and statistically significant among younger mothers, second children for the middle, and third children for older mothers. This suggests that the effects I identify correspond to additional births, not simply as a temporal adjustment of childbearing, mother's age and years married.[36]

Structural Relationship between Cash Transfers, Gestational Age, and Birth Weight

The estimated effects of cash transfers CT on birth weight BA and gestational age GA shed light on a structural relationship between neonatal health outcomes and the pro-natalist cash transfers. Based on the estimation results, I express their structural relationship as follows:

$$\ln BW = \beta_C \sinh^{-1} CT + \beta_G \ln GA + \eta_B \tag{10}$$

$$\ln GA = \gamma_C \sinh^{-1} CT + \eta_G \tag{11}$$

where coefficients β_C and β_G capture the direct effects of cash transfers and gestational age on birth weight; γ_C captures the effect of cash transfers on gestational age; η_B and η_G are stochastic errors. Table 5 and Table 6 report the estimates for all of these structural parameters: $\hat{\beta}_C = 0.003$, $\hat{\beta}_G = 2.200$, and $\hat{\gamma}_C = -0.002$. Substituting Eq. 11 into Eq. 10, I obtain the expression below:

$$\ln BR = \pi_C \sinh^{-1} CT + \eta_B + \beta_G \eta_G, \tag{12}$$

where $\pi_C = \beta_C + \beta_G \gamma_C$ is the reduced-form effect of cash transfers on birth weight. This reduced-form effect combines the direct effect of cash transfers on birth weight β_C and the indirect effect via gestational age $\beta_G \gamma_C$. The implied value of the reduced-form parameter $\tilde{\pi}_C = -0.0014$, which matches the estimated value of the same parameter reported in Column (1) of Table 5.What this result indicates is that all other determinants of birth weight conditional on gestational age are jointly orthogonal to cash transfer.

[36] While the event study coefficients post-implementation in Figure 3 does not take the generosity of cash transfers into account, the consistently positive and significant estimates over 8 years provide additional evidence supporting the quantum effect of cash transfers.

Selection: Working Mother

A puzzle remains: why did the pro-natalist cash transfers shorten pregnancy duration? One potential mechanism is selection. The pro-natalist cash transfers affect the childbearing decisions of mothers at the margin of having one more child. In particular, among those at the margin, cash transfers may have been particularly valuable if they were employed as their labor market-related opportunity costs of childbearing are high. The financial assistance enables these mothers to have an additional child, but with a shorter pregnancy duration than the mothers who are not working, thus have fewer time contraints. I formally test this hypothesis by estimating the effect of the cash transfers on the likelihood of births by working mothers. That is, I estimate Eq. 6 with an indicator for mother's employment status (1 if employed) as the dependent variable. Note the month-by- city fixed effects purge out the month-to-month city-level changes in labor market conditions. Column (3) of Table 10 reports the results. Across birth orders, the pro-natalist cash transfers increased the probability of working mothers to have babies. As the cash transfers for the third child double, mothers who had their third child are 1.6 percentage point more likely to be working.

This selection may also explain the results on birth weight and sex ratio. In Column (4), I show that the positive effect of cash transfers on birth weight estimated in the previous section in fact applies primarily to working mothers' infants. I additionally introduce a set of terms interacting the cash transfers by birth order and mother's working status. The coefficients inform whether the cash transfers had differential effects on birth weight for working mothers. On the one hand, all of the estimated coefficients of the effect of cash transfers become smaller in magnitude (relative to Column (4) of Table 5) and lose statistical significance. On the other hand, the estimated coefficients for the interaction terms are positive at the 5% significance level for fifrst children and 0.1% for higher order births. Lastly, I test if the observed selection effect for working mothers explains the reduction in the male-skewed sex ratio from cash transfers. The story is that working mothers are more likely to be educated and less attached to son preference. If this is the case, the sex ratio reflected a more natural ratio after the implementation of the policy not because the cash transfers compensated the perceived utility penalty associated with daughters, but because mothers who value daughters more highly increased their childbearing activities. In Column (5), though, I verify that this is not the case. The estimates for the cash transfers do not change much from the results in Column (2) of Table 7. The effect of the interaction terms, estimating whether the cash transfers had differential effect on the probability of a male birth for working mothers, are not statistically different from zero.

6 Conclusion

In this paper, I study the causal effects of pro-natalist cash transfers provided to families with newborn babies on birth rates, neonatal health outcomes, and sex ratio at birth. 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 pro-natalist cash transfers increased birth rates across birth orders. The elasticity of birth rates with respect to cash-transfer generosity is largest among first children and decreases with birth order. In the absence of cash transfers, the total fertility rate in 2015 would have been reduced by 5%, which corresponds to about 0.4 million less children ever born by the female population in the same year. I provide evidence that these changes in birth rates are a result of increased 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 neonatal health and sex ratio at birth. These pro-natalist cash transfers enabled working mothers to have a child, who would have not have done so with out the financial aids. However, working mothers are more likely to have a shorter pregnancy duration. As a result, the cash transfers ended up decreasing gestational age on average. Conditional on pregnancy duration, cash transfers increased birth weight, an effect which is concentrated among infants with working mothers. Lastly, I find that the cash transfers shifted 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 fertility, particularly valuable amongst working mothers. 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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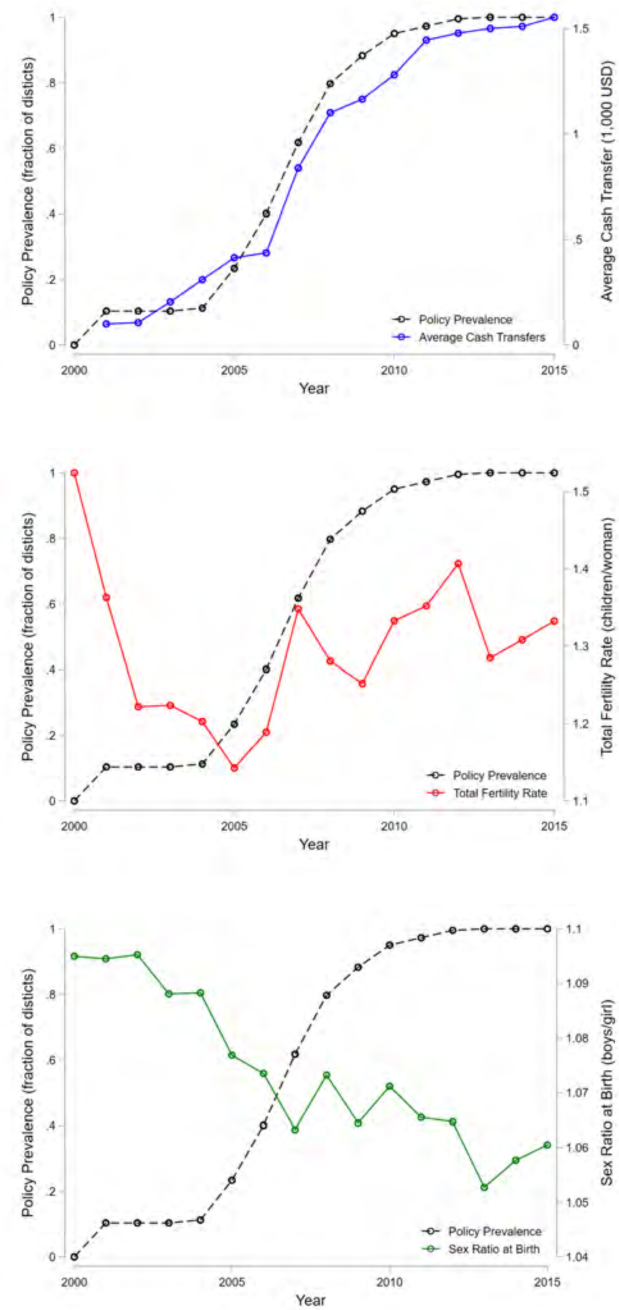

Figure 1: Local Pro-Natalist Cash Transfer Policies and Total Fertility Rates

Notes: This figure plots the fraction of districts (total of 222 districts) with pro-natalist cash transfers and the average cash-transfer amounts measured in 1,000,000 KRW (top), the average total fertility rates measured by children per woman (middle), and sex ratio at birth measured by the number of boys per girl (bottom) over time from 2000 to 2015. Note that 1,000,000 KRW approximately converts to 1,000 USD.

		Year					
		2000	2003	2006	2009	2012	2015
A. Pro-Natalist Cash Transfers							
Cash Transfer							
First 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)
Second 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)
Third 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)
B. Birth Rates							
Total Fertility Rate	mean	1.52	1.22	1.19	1.25	1.41	1.33
		(0.23)	(0.20)	(0.21)	(0.25)	(0.28)	(0.27)
First Child Birth Rate	mean	20.97	16.46	16.50	17.19	18.99	17.73
		(7.87)	(6.18)	(7.30)	(7.64)	(9.65)	(10.04)
Second Child Birth Rate	mean	19.41	14.48	12.80	13.38	14.97	13.74
		(7.70)	(5.66)	(5.59)	(5.69)	(7.21)	(7.39)
Third Child Birth Rate	mean	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 policies, fertility rates, and birth rates for first, second, and third child for every three years from 2000 to 2015. For cash transfers by birth order, the table reports the fraction of districts (out of 222 districts) with strictly positive pro-natalist cash transfers and average cash transfer amounts measured in 1,000,000 KRW (excluding zero). Note that 1,000,000 KRW approximately converts to 1,000 USD. Total fertility rate and birth rates are measured by children per woman and children per 1,000 women, respectively. Standard deviations are reported in parentheses.

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline					
	2000	2001	2002	2003	2004	2005
log(population)	-0.0318	-0.0323	-0.0582	-0.0948	-0.114	-0.0497
	(0.0287)	(0.0355)	(0.0431)	(0.0532)	(0.0723)	(0.0860)
log(total fertility rates)	0.112	-0.168	0.486	0.0425	-0.382	0.953
	(0.311)	(0.263)	(0.270)	(0.320)	(0.486)	(0.576)
% Female Population	1.421	-2.976	1.013	-0.0841	-5.388	5.566
	(3.291)	(3.153)	(3.473)	(4.270)	(6.076)	(7.252)
% Adult Population	3.415*	4.092*	6.691**	6.870**	7.635*	8.283*
	(1.490)	(1.894)	(2.041)	(2.487)	(3.321)	(3.642)
% Elderly Population	0.0616	1.455	1.921	1.564	2.436	2.141
	(1.333)	(1.426)	(1.522)	(1.847)	(2.489)	(2.508)
Net Migration Rate	0.000457	-0.000282	-0.00169	0.00133	-0.00246	-0.00304
	(0.00104)	(0.00105)	(0.00136)	(0.00173)	(0.00242)	(0.00284)
Marriage Rate	-3.433	-2.435	-5.300	-6.605	-8.717	-5.822
	(2.915)	(3.670)	(5.206)	(6.151)	(7.304)	(16.97)
Conservative Local Gov't Head	0.0295	0.00660	0.152*	0.168*	0.215*	0.290**
	(0.0436)	(0.0408)	(0.0652)	(0.0841)	(0.100)	(0.103)
Financial Independence Rate	-0.428**	-0.452**	-0.431**	-0.624**	-0.862***	-0.681*
	(0.134)	(0.138)	(0.157)	(0.202)	(0.257)	(0.286)
Observations	222	199	199	199	197	170
R^2	0.275	0.278	0.300	0.287	0.313	0.385
p-value	.0285	.0072	.0026	.0032	.0005	.0785

Table 2: Determinants of Policy 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 policy since a given baseline year (each column annually from 2000 to 2005) on the district-level characteristics observed in that baseline year. Each observation corresponds to a district, which had not implemented pro-natalist cash transfer policies prior to each baseline year. A p-value testing the null hypothesis that all the coefficients are jointly equal to zero is reported. 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)	0.0706	-1.907	-0.224	-0.706	-0.386	-1.092
	(0.133)	(1.138)	(0.206)	(0.780)	(0.319)	(0.685)
% Female Population	-8.131***	-7.840	-8.879**	-14.12	-9.628*	-16.07
	(2.408)	(7.337)	(3.233)	(7.232)	(4.613)	(10.17)
% Adult Population	-2.980	1.306	-7.729***	-4.187	-9.343**	-2.816
	(1.770)	(5.274)	(2.242)	(4.809)	(3.428)	(5.795)
% Elderly Population	2.036	-0.191	0.177	4.303	-1.373	2.480
	(1.542)	(4.914)	(2.117)	(4.186)	(2.963)	(5.618)
Net Migration Rate	0.0002	0.0005	0.0009*	0.0003	0.0003	0.0005
	(0.0002)	(0.0009)	(0.0004)	(0.0008)	(0.0005)	(0.0008)
Marriage Rate	-4.188	11.42	-8.122*	9.723	-4.288	-6.027
	(2.429)	(9.529)	(3.400)	(10.74)	(3.931)	(9.026)
Conservative Party Local Gov't	-0.0142	-0.0394	0.0089	-0.0471	0.0072	-0.0021
	(0.0128)	(0.0338)	(0.0251)	(0.0486)	(0.0295)	(0.0343)
Financial Independence Rate	0.137	-0.279	0.291	0.274	0.367	0.605
	(0.127)	(0.772)	(0.182)	(0.479)	(0.305)	(0.561)
Observations	3,552	908	3,552	1,693	3,552	2,045
R^2	0.739	0.814	0.798	0.856	0.835	0.875
p-value	0.0014	.1374	0.0000	.0618	.0001	.0531

Table 3: Determinants of Cash Transfer Generosity

Notes: This table reports the estimated coefficients from regressing the amount of pro-natalist cash transfers 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 (measured in 1,000,000 KRW or approximately 1,000 USD) on the district-level characteristics. For each birth order (first, second, and third), the sample in the first column correspond to all district-year pairs and the samples in the second column excludes observations prior to policy adoption. For each year, a p-value testing the null hypothesis that all the coefficients are jointly equal to zero is reported. 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.

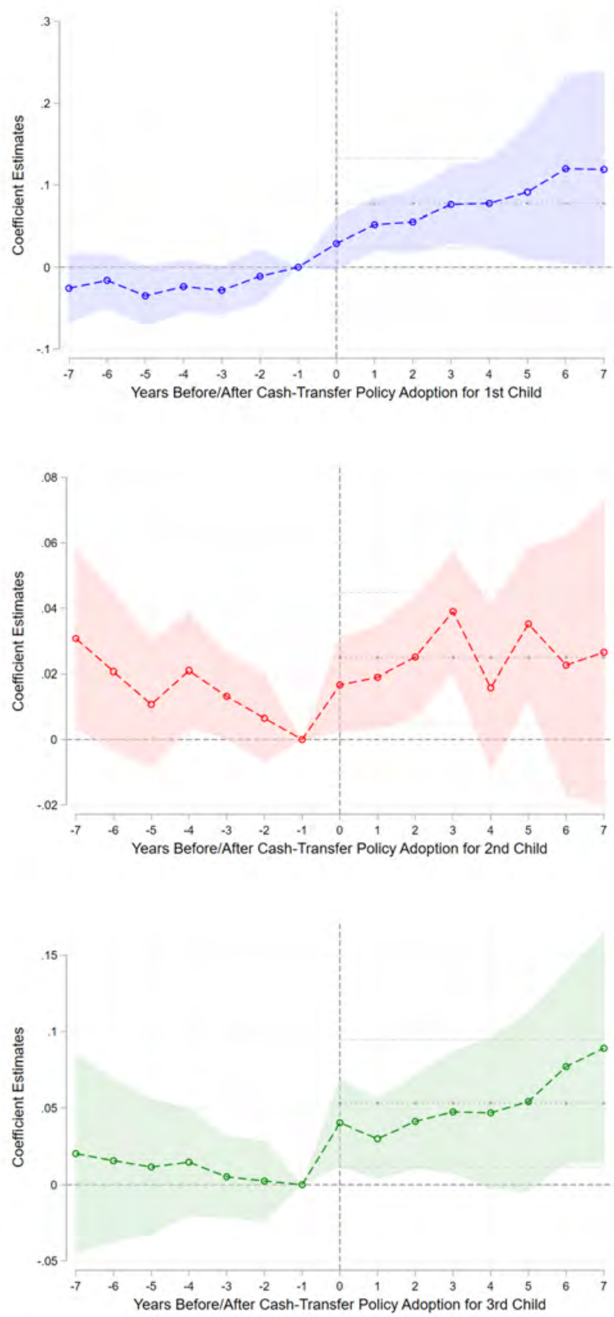

Figure 2: Birth Rates by Birth Parity Before and After Local Policy Implementation

Notes: This event-study figure plots the changes in the birth rates before and after pro-natalist cash transfer policy implementation for the first child (top-left), second child (top-right), and third child (bottom). 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 control variables include the log of the total population, 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child (resp. the first and second child). Error bars show 95% confidence intervals. Standard errors are twoway-clustered at the district level and city-by-year level.

First Child Second Child Third Child

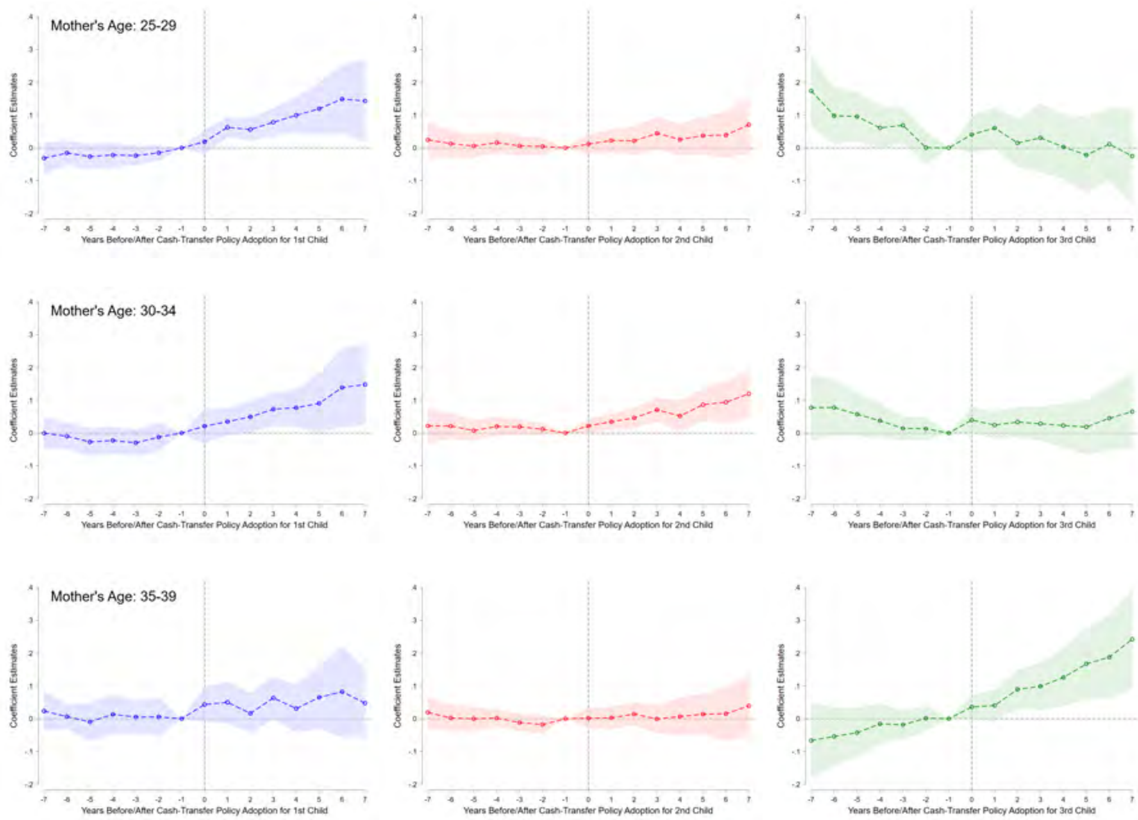

Figure 3: Birth Rates Before and After Policy Implementation (Ages: 25-39)

Notes: This event-study figure plots the changes in the age-specific birth rates before and after pro-natalist cash transfer policy implementation for the first child (left), second child (middle), and third child (right). The vertical axes are scaled the same across the panels. 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 the log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child (resp. the first and second child). Error bars show 95% confidence intervals. Standard errors are twoway-clustered at the district level and city-by-year level.

	(1)	(2)	(3)	(4)	(5)	(6)
			log Birth Rates			
	First Child		Second Child		Third Child	
$\sinh^{-1}$ Cash Transfer for						
First Child	0.189***	0.198***		-0.0186		0.00524
	(0.0393)	(0.0422)		(0.0220)		(0.0248)
Second Child		-0.0401	0.0565***	0.0536**		-0.0363
		(0.0310)	(0.00942)	(0.0168)		(0.0223)
Third Child		0.0315		0.00924	0.0419***	0.0559***
		(0.0177)		(0.00846)	(0.00921)	(0.0120)
Observations	3,330	3,330	3,330	3,330	3,330	3,330
R^2	0.946	0.946	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 (i.e., the cash-transfer elasticities of the birth rates for the first child in Column 1-2, the second child in Column 3-4, and the 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 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 the log of 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 (total inflow minus outflow normalized by population), marriage rate, 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 first child (resp. the second child). For each birth order, the first column includes the inverse hyperbolic sine transformed (IHS) value of cash transfer amount for the corresponding birth order only; the second column includes the IHS values of cash transfers for the first, second, and third child as separate explanatory variables. Standard errors, twoway-clustered at the district level and city-by-year 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.

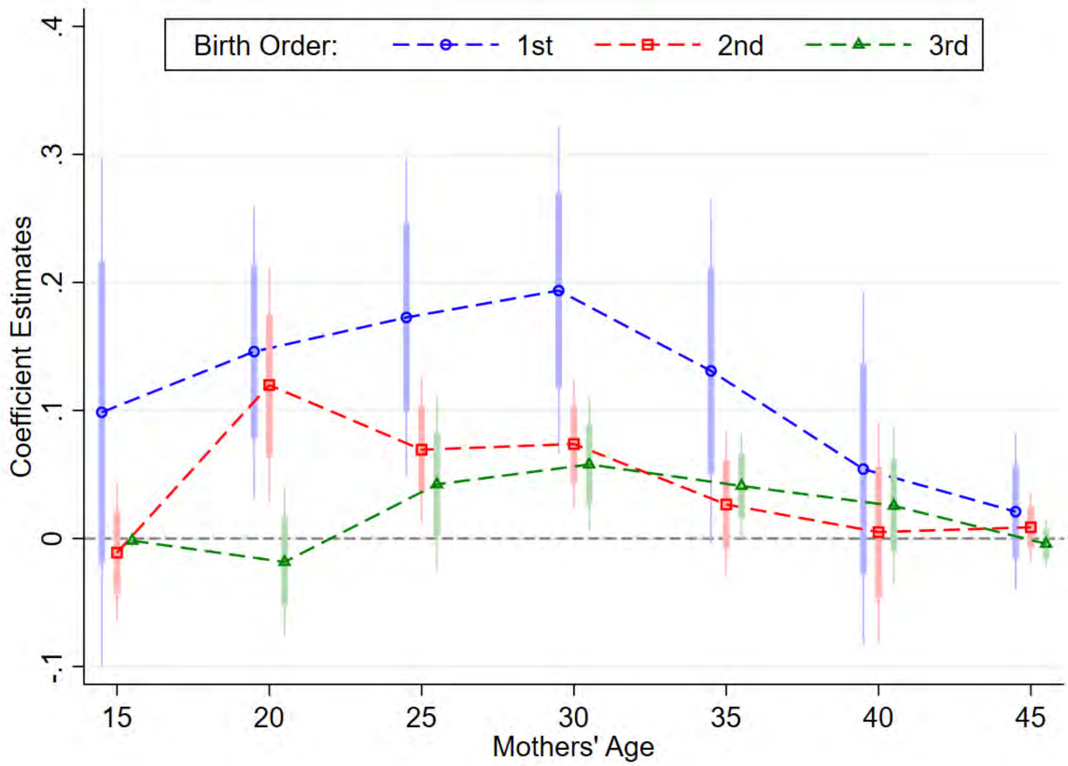

Figure 4: The Effect of Cash Transfer on Birth Rates by Mother's Age

Notes: This figure plots the estimated effects of the cash transfers on the age-specific birth rates for the first child (blue-circle), second child (red-square), and third child (green-triangle) by each mother's age group (horizontal axis). 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) and district-level control variables are included. The control variables include the log of 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 elderly (older than 64), the net migration rate (total inflow minus outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child (resp. the first and second child). Error bars show 95 percent (thick) and 99.9 percent (thin) confidence intervals. Standard errors are twoway-clustered at the district level and city-by-year level.

	(1)	(2)	(3)	(4)
	log Birth Weight			
$\sinh^{-1}$ Cash Transfer	-0.0014***		0.0030***	
	(0.0004)		(0.0004)	
×First Child		0.0001		0.0009
		(0.0009)		(0.0009)
×Second Child		-0.0003		0.0037***
		(0.0006)		(0.0006)
×Third Child		-0.0023***		0.0026***
		(0.0004)		(0.0004)
Second Child	0.0046***	0.0044***	0.0277***	0.0275***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Third Child	0.0132***	0.0136***	0.0342***	0.0344***
	(0.0005)	(0.0005)	(0.0004)	(0.0004)
Boy	0.0305***	0.0305***	0.0377***	0.0377***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Gestational Age			2.200***	2.200***
			(0.0091)	(0.0091)
Mean Dependent Variable (First child)	3.228	3.228	3.228	3.228
Observations	7,081,285	7,081,285	7,081,285	7,081,285

Table 5: The Effect of Cash Transfer on Birth Weight

Notes: This table reports the estimated effects of cash transfers on birth weight. Column (1) reports the estimated effects of the cash transfers unconditional on birth order; in Column (2), the cash-transfer effects are allowed to differ by birth order. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2000 to 2015. Column (3) and (4) include the log of gestational age as an additional control to Column (1) and (2). 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 log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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)
	log Gestational Age			
$\sinh^{-1}$ Cash Transfer	-0.0020***		-0.0017***	
	(0.0002)		(0.0002)	
×First Child		-0.0003		-0.0004
		(0.0005)		(0.0004)
×Second Child		-0.0018***		-0.0018***
		(0.0003)		(0.0003)
×Third Child		-0.0022***		-0.0018***
		(0.0002)		(0.0001)
Second Child	-0.0105***	-0.0105***	-0.0113***	-0.0112***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Third Child	-0.0096***	-0.0094***	-0.0118***	-0.0117***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Boy	-0.0033***	-0.0033***	-0.0083***	-0.0083***
	(0.00004)	(0.00004)	(0.00004)	(0.00004)
log Birth Weight			0.1650***	0.1650***
			(0.0007)	(0.0007)
Mean Dependent Variable (First child)	39.109	39.109	39.109	39.109
Observations	7,081,285	7,081,285	7,081,285	7,081,285

Table 6: The Effect of Cash Transfer on Gestation Age

Notes: This table reports the estimated effects of cash transfers on gestational age (log weeks). Column (1) reports the estimated effects of cash transfers unconditional on birth order; in Column (2), the cash-transfer effects are allowed to differ by birth order. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2000 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 log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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)	(7)	(8)
	1×(dead before 1 year)				1×(dead before 5 year)			
$\sinh^{-1}$ Cash Transfer	0.0023		0.0011		-0.00003		-0.00004	
	(0.0181)		(0.0184)		(0.0002)		(0.0002)	
×First Child		0.0293		0.0299		0.0001		0.0002
		(0.0289)		(0.0292)		(0.0003)		(0.0003)
×Second Child		-0.0313		-0.0056		0.00001		-0.00001
		(9.0300)		(0.1273)		(0.7148)		(0.3530)
×Third Child		0.0073		0.0073		-0.0001		-0.0001
		(7.1564)		(8.2741)		(0.5599)		(0.2906)
Second Child	-0.0394	0.0428	-0.0433	-0.0394	0.0004	0.0004	-0.0004	-0.0004
	(14.7253)	(46.5371)	(38.4995)	(0.2223)	(0.0358)	(0.1273)	(0.5708)	(0.6734)
Third Child	0.0866	0.0838	0.0115	0.00756	0.0011	0.0011	0.0003	0.0003
	(25.4684)	(11.2664)	(47.2467)	(9.7606)	(0.5656)	(0.2212)	(0.1619)	(0.0743)
Boy	(0.0271)	0.0271**	0.0576	0.0576***	0.0004***	0.0004***	0.0008***	0.0007*
	0.0183)	(0.0093)	(0.0315)	0.0159	(0.0001)	0.0001077	(0.0001)	(0.0003)
Log Birth Weight			-1.8586***	-1.8586***			-0.0204***	-0.0204
			0.1035	0.2775			(0.0046)	(0.0046)
Log Gestational Age			-7.4524***	-7.4524***			-0.0794***	-0.0794***
			0.8443	0.5315			(0.0086)	(0.0119)
Mean Dependent Variable (First child)	0.1653	0.1653	0.1653	0.1653	0.2299	0.2299	0.2299	0.2299
Observations	1,711,949	1,711,949	1,711,949	1,711,949	1,711,949	1,711,949	1,711,949	1,711,949

Table 7: 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 1-4 and within 5 years since birth in Column 5-8). 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. 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 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 log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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)
	Boy	
$\sinh^{-1}$ Cash Transfer	-0.0181***	
	(0.0016)	
$\times$First Child		-0.0027
		(0.0025)
$\times$Second Child		-0.0042**
		(0.0013)
$\times$Third Child		-0.0276***
		(0.0021)
Second Child	0.0025***	0.0012**
	(0.0004)	(0.0004)
Third Child	0.0421***	0.0474***
	(0.0020)	(0.00023)
Mean Dependent Variable (First child)	0.5132	0.5132
Observations	7,081,285	7,081,285

Table 8: The Effect of Cash Transfer on Sex Ratio

Notes: This table reports the estimated effects of cash transfers on indicator for boys. The first column reports the estimated effects of cash transfers unconditional on birth parity; in the second column the cash-transfer effects are allowed to differ across birth parity. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2000 to 2015. The average number of male children per 100 female children is 105.4 for first children, 105.9 for second children, and 120.9 for third childreb. Across columns, the same set of fixed effects (i.e., district fixed effects and city-by-month fixed effects) are included; the 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 log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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)
	log # Potential Beneficiaries			Non-Beneficiaries
	First Child	Second Child	Third Child	
$\sinh^{-1}$ Cash Transfer for				
First Child	0.00926			0.00622
	(0.0124)			(0.0131)
Second Child		0.0339**		-0.000500
		(0.0106)		(0.0129)
Third Child			0.0301***	0.00179
			(0.00715)	(0.00705)
Observations	619,074	297,830	264,991	624,848

Table 9: Migratory Response of Families to Cash Transfers

Notes: This table reports the estimated effects of pro-natalist cash transfers on the log of number of households moving into districts. Each observation corresponds to a district-pair by year tuple from 2001 to 2015. 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 district-level control variables include the log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. 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. 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)
	log Mother's Age	log Years Married	Mother Working	log Birth Weight	Boy
$\sinh^{-1}$ Cash Transfer					
×First Child	-0.0021	-0.0110	0.0376***	-0.0005	-0.0013
	(0.0012)	(0.0070)	(0.0071)	(0.0010)	(0.0035)
×Second Child	-0.0046***	0.0380***	0.0109**	0.0016*	-0.0070**
	(0.0006)	(0.0050)	(0.0034)	(0.0007)	(0.0020)
×Third Child	-0.0143***	0.0427***	0.0159***	0.0010	-0.0285***
	(0.0009)	(0.0036)	(0.0026)	(0.0006)	(0.0025)
Mother Working×$\sinh^{-1}$ Cash Transfer ×1st Child				0.0022*	-0.0031
				(0.0011)	(0.0031)
×Second Child				0.0039***	-0.0001
				(0.0006)	(0.0021)
×Third Child				0.0020***	-0.0012
				(0.0006)	(0.0019)
Mean Dependent Variable (1st child)	28.999	1.807	0.682	3.228	0.5132
Observations	7,080,381	7,028,130	7,080,381	7,081,285	7,080,381

Table 10: Timing of Childbearing and Selection of Working Mothers into Childbearing

Notes: This table reports the estimated effects of cash transfers on various outcomes associated with each birth: the log of mother's age, the inverse hyperbolic sine transformed value of years married, indicator for a working mother, log of birth weight, and indicator for a male birth in each column, respectively. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2000 to 2015. Across columns, the same set of fixed effects (i.e., district fixed effects and city-by-month fixed effects) are included; the individual characteristics are controlled for: the log of gestational age, the log of birth weight (excluded in Column 4), an indicator for a male birth (ex. in Col. 5), dummy variables for mother's and father's education attainment level (no schooling, elementary school, middle school, high school, some college or above), age, occupation, and marital status (Column (1) excludes the fixed effects for mother's age). The district-level control variables include the log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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.

Appendix: Additional Figures and Tables

	Year					
	2000	2003	2006	2009	2012	2015
			A. Birth Weight			
First Child	3.246	3.256	3.236	3.219	3.206	3.202
	(0.443)	(0.483)	(0.450)	(0.449)	(0.450)	(0.458)
Second 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)
Third 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)
			B. Gestational Age			
First 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)
Second Child	39.065	38.852	38.731	38.521	38.399	38.298
	(1.465)	(1.571)	(1.555)	(1.570)	(1.586)	(1.580)
Third Child	39.085	38.803	38.650	38.404	38.297	38.172
	(1.577)	(1.679)	(1.666)	(1.691)	(1.672)	(1.719)
			C. Fraction of Male Births			
First 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)
Second 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)
Third 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)

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

Notes: This table report the mean birth weight in kilograms (Panel A), pregnancy duration in weeks (Panel B), and fraction of male births (Panel C) for the first, second, and third child for every three years from 2000 to 2015 based on the universe of confidential birth registry records. Standard deviations are reported in parentheses.

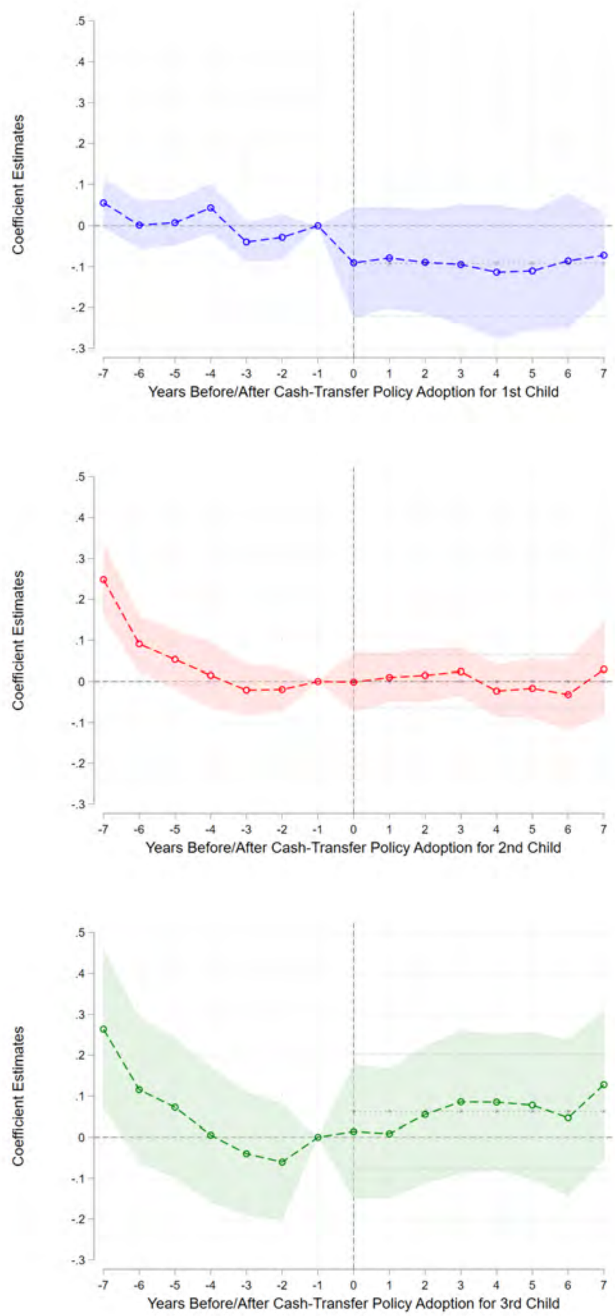

Figure A.1: Birth Rates Before and After Local Policy Implementation (Naive)

Notes: This event-study figure plots the changes in the birth rates before and after pro-natalist cash transfer policy implementation for the first child (top-left), second child (top-right), and third child (bottom). Each observation corresponds to a district-year pair and is weighted by the female population of each age group. In each regression, no fixed effects and control variables are included. Error bars show 95% confidence intervals. Standard errors are twoway-clustered at the district level and city-by-year level.

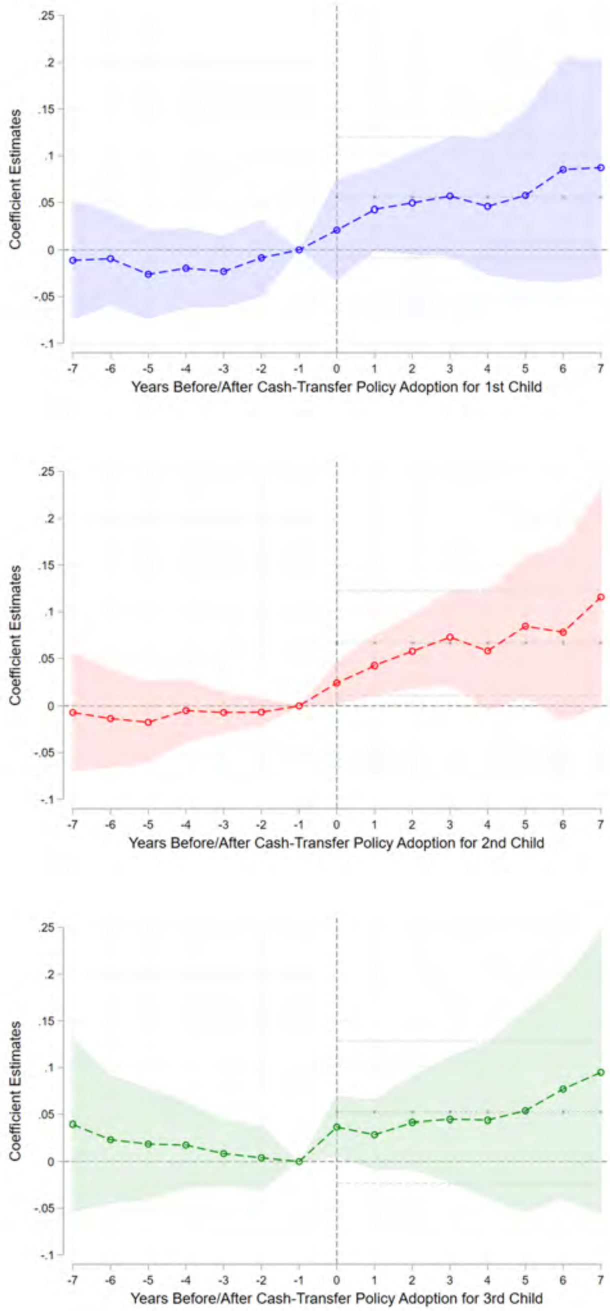

Figure A.2: Birth Rates Before and After Local Policy Implementation (Fixed Effects Only)

Notes: This event-study figure plots the changes in the birth rates before and after pro-natalist cash transfer policy implementation for the first child (top-left), second child (top-right), and third child (bottom). 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. But, no time-varying district level control variables are included. Error bars show 95% confidence intervals. Standard errors are twoway-clustered at the district level and city-by-year level.

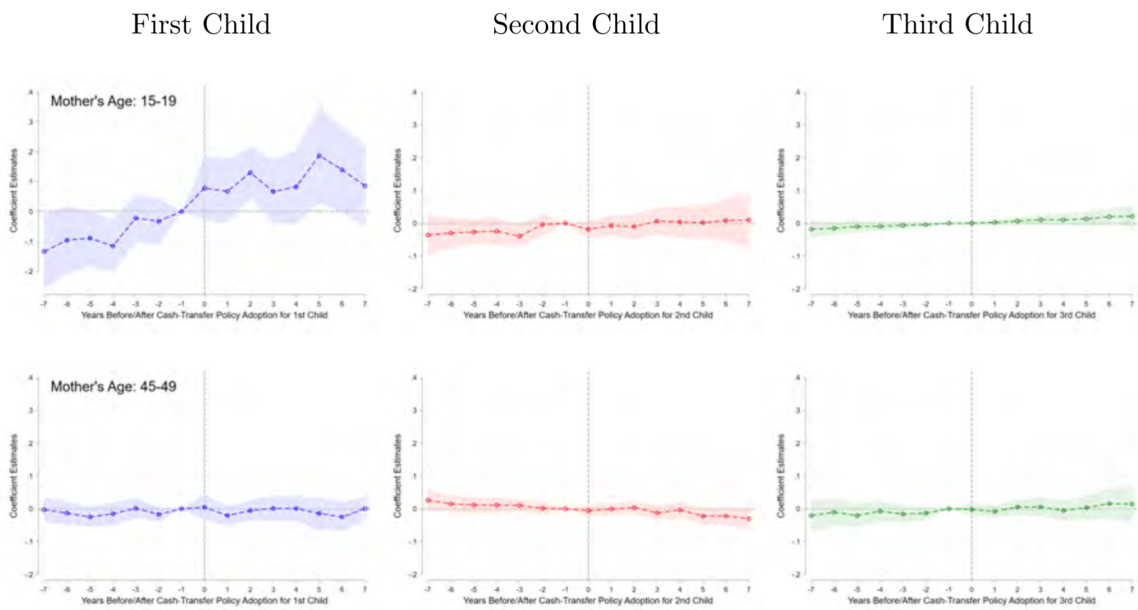

Figure A.3: Birth Rates Before and After Policy Implementation (Ages: 15-19 and 45-49)

Notes: This event-study figure plots the changes in the age-specific birth rates before and after pro-natalist cash transfer policy implementation for the first child (left), second child (middle), and third (right). 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 the log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. In addition, the estimations for the second child (resp. the third child) include the lagged number of births for the first child (resp. the first and second child). Error bars show 95% confidence intervals. Standard errors are twoway-clustered at the district level and city-by-year level.

	(1)	(2)	(3)
	Naive	+FE	+Control
	A. First Child		
Cash Transfers	-0.0433	0.191***	0.189***
	(0.0802)	(0.0453)	(0.0393)
	B. Second Child		
Cash Transfers	-0.0190	0.120***	0.0565***
	(0.0365)	(0.0241)	(0.00942)
	C. Third Child		
Cash Transfers	0.134***	0.0803***	0.0419***
	(0.0317)	(0.0134)	(0.00921)
Observations	3,330	3,330	3,330

Table A.2: Cash Transfer Effects on Birth Rates (Naive, FE, and Full)

Notes: This table reports the results estimating the elasticity of birth rates for the first (Panel A), second (Panel B), and third (Panel C) children with respect to pro-natalist cash transfers without any fixed effects and time-varying district-level controls in Column (1), adding a set of district fixed effects and city-by-year fixed effects in Column (2) and both the fixed effects and the control variables in Column (3). 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. The control variables include the log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. In addition, Column (3) in Panel B (resp. Panel C) includes the lagged number of births for the first child (resp. the first and second child). Standard errors, twoway-clustered at the district level and city-by-year 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)
	1 × (Birth Weight < 2.7 kilograms)			
$\sinh^{-1}$ Cash Transfer	0.0013**		-0.0035***	
	(0.0005)		(0.0006)	
×First Child		0.0023*		0.0015
		(0.0011)		(0.0012)
×Second Child		0.0018**		-0.0026**
		(0.0006)		(0.0008)
×Third Child		-0.0010*		-0.0044***
		(0.0005)		(0.0005)
Second Child	-0.0034***	-0.0034***	-0.0291***	-0.0291***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Third Child	-0.0034***	-0.0032***	-0.0269***	-0.0262***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Boy	-0.0092***	-0.0092***	-0.0172***	-0.0172***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
log Gestational Age			-2.4528***	-2.4529***
			(0.0092)	(0.0092)
Mean Dependent Variable (First child)	0.04644	0.04644	0.04644	0.04644
Observations	7,081,285	7,081,285	7,081,285	7,081,285

Table A.3: Incidence of Low Birth Weight

Notes: This table reports the estimated effects of cash transfers on the incidence of low birth weight (less than 2.5 kilograms). Column (1) reports the estimated effects of cash transfers unconditional on birth order; in Column (2), the cash-transfer effects are allowed to differ by birth order. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2000 to 2015. Column (3) and (4) include log gestational age as an additional control to Column (1) and (2). 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 log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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)
	1 × (Birth Weight > 4.0 kilograms)			
$\sinh^{-1}$ Cash Transfer	-0.0010*		-0.0004	
	(0.0004)		(0.0004)	
×First Child		-0.0004		-0.0003
		(0.0011)		(0.0010)
×Second Child		0.0009		0.0015**
		(0.0006)		(0.0008)
×Third Child		-0.0022***		-0.0015**
		(0.0005)		(0.0005)
Second Child	-0.0015***	-0.0018***	0.0019***	0.0017***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Third Child	0.0091***	0.0097***	0.0122***	0.0128***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Boy	0.0178***	0.0178***	0.0189***	0.0189***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Gestational Age			0.3258***	0.3258***
			(0.0040)	(0.0040)
Mean Dependent Variable (First child)	0.0317	0.0317	0.03171	0.0317
Observations	7,081,285	7,081,285	7,081,285	7,081,285

Table A.4: Incidence of Over Birth Weight

Notes: This table reports the estimated effects of cash transfers on the incidence of over birth weight (greater than 4 kilograms). Column (1) reports the estimated effects of cash transfers unconditional on birth order; in Column (2), the cash-transfer effects are allowed to differ by birth order. Each observation corresponds to a birth and the total observations span the universe of births in South Korea from 2000 to 2015. Column (3) and (4) include log gestational age as an additional control to Column (1) and (2). 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 education 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 log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. Standard errors, twoway-clustered at the district level and city-by-month 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)
	log Birth Rates					
	First Child		Second Child		Third Child	
$\sinh^{-1}$ Cash Transfer for						
First Child	0.189***	0.205***				
	(0.0393)	(0.0417)				
Second Child			0.0565***	0.0994***		
			(0.00942)	(0.0201)		
Third Child					0.0419***	0.0622***
					(0.00921)	(0.0125)
Observations	3,330	3,330	3,330	3,330	3,330	3,330

Table A.5: The Effect of Cash Transfer on Birth Rates (Excluding/Including Migratory Response)

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 control variables include the log of 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 (total inflow-outflow normalized by population), marriage rate, indicators for the gender and political party affiliation of the local government head, and the financial independence rate. In addition, Column 3 (resp. 5) includes the lagged number of births for the first child (resp. the first and second child). Column 1, 3, and 5 correspond to the same columns in Table 4. For each birth order, the second column (2,4, and 6) excludes the percentage of female population, net migration rate, and lagged number of lower-order births from the set of control variables. Standard errors, twoway-clustered at the district level and city-by-year 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.