Childbirth, Baby Bonus, Maternal Mental Health*

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February 26, 2025

Abstract

We study how maternal mental health impacts of childbirth, the role of pro-natalist cash transfers in shaping maternal health, and future fertility consequences of maternal mental health. Using claims-level data from South Korea's universal healthcare system, we find that diagnoses of mental health conditions increase by approximately 80-163% following the first and second births. However, we observe little evidence that pro-natalist cash transfers mitigate these adverse mental health effects. As possible mechanisms, we find that the adverse mental health effects are greater among high-income families, and these families tend to spend more on children. Child penalties in the labor market do not explain our results. Lastly, we find that experiencing poor mental health after childbirth is negatively associated with the likelihood of having an additional child. Overall, our findings offer new insights into the interplay among motherhood penalties, pro-natalist cash transfers, and fertility.

JEL Classification: J13; J18; I10; H51; H75

Keywords: Maternal Mental Health; Childbirth; Child Penalty; Pro-natalist Cash Transfers; Fertility

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1 Introduction

Fertility rates are declining sharply across the world, with significant reductions observed in regions such as Europe, Asia, and Africa. This trend is projected to continue into the future (Vollset et al., 2020). Coupled with a rapidly aging population, these demographic shifts are expected to have profound implications for economic growth, government budgets, and welfare systems (Harper, 2014; Jones, 2022; Maestas et al., 2023; Doepke et al., 2023). Under the presumption that the declining fertility rates are due to increases in costs of childbirth, governments have introduced pro-natalist policies aimed at alleviating the burden of childbirth on families (Hart et al., 2024). A more comprehensive understanding of the multifaceted nature of these costs and the interconnected effects of policy interventions is essential to better understand fertility choices and effectively address broader demographic challenges.

This study examines maternal mental health impacts of childbirth and the potential influence of pro-natalist policies in South Korea (Korea). We focus on mental health for three reasons. First, mental health affects individual well-being directly and indirectly by influencing socioeconomic outcomes such as productivity and social interactions (Bartel and Taubman, 1986; Chatterji et al., 2011; Biasi et al., 2020; Ringdal and Rootjes, 2022). Second, parental well-being significantly impacts children's outcomes (Farahati et al., 2003; Mühlenweg et al., 2015; Reupert and Maybery, 2016; Bütikofer et al., 2024). Third, psychological distress may influence future childbearing decisions. Alongside the physical demands of childbirth, women often assume extensive caregiving responsibilities and adjust their labor market participation to balance work and child-rearing. These adjustments, coupled with the motherhood penalty in the workforce (Guryan et al., 2008; Kuziemko et al., 2018; Kleven et al., 2021, 2024), can contribute to a decline in mental well-being.

Korea serves as a preferred empirical context and offers several key advantages. First, total fertility rates in Korea have remained below the replacement level of 2.1 since the early 1980s, dropping to just 0.72 children per woman in 2023—the lowest in the world. This provides a unique opportunity to explore the mental health costs of childbirth and their dynamic effects on subsequent fertility decisions in the low fertility setting. Second, the local (district) governments in Korea began awarding "baby bonus" as early as 2001 to families with newborns. We leverage the plausibly exogenous variation in the timing of these baby bonus implementations across districts over time to examine their impact on maternal mental health. Third, we can access the claims-level data from Korea's universal healthcare system, which enable the identification of childbirth events and their timing, as well as the ability to track individual healthcare utilization for mental health and fertility histories over time. Instead of relying on the subjective evaluations of mental health commonly used in the literature, we address measurement challenges by utilizing health insurance claims data. Based on these claims, we construct a monthly panel dataset of mothers who gave birth between 2005 and 2012, tracking their medical histories from three years before to three years after childbirth.

To identify the effects of birth and baby bonus, we trace changes in maternal mental healthcare utilization before and after childbirth among mothers, and compare these changes between those

with and without baby bonus benefits by employing a modified dynamic event-study design type approach, following (Kleven et al., 2019, 2021). Using healthcare claims of mothers identified from a 2% random sample of National Health Insurance (NHI) enrollees, we document the following four sets of findings.

First, we find evidence that childbirth negatively impacts mothers' mental health, particularly following the first and second births. Approximately two years after childbirth, mothers are 0.5 percentage points more likely to seek mental healthcare. Compared to the baseline (pre-childbirth) rate of healthcare utilization, this represents about a 80–163% increase in mental health-related incidences after childbirth. However, for third births, we do not observe any statistically significant changes in maternal mental health.

Second, we find little evidence that the baby bonus program improved maternal mental health. As a type of cash transfer, the baby bonus could potentially help mothers better manage their (mental) health, potentially yielding positive effects on maternal well-being. However, our findings indicate that the baby bonus did not mitigate the rise in mental health issues among mothers following their first and second births.

Third, we examine heterogeneous effects of birth and baby bonus by household income levels. Household income can improve household members' mental health (Kim and Koh, 2021). Since raising children can cause significant financial distress to households, childbearing can worsen maternal mental health and baby bonus may mitigate such adverse maternal mental health impacts of birth. However, we document evidence that the adverse mental health impacts were greater among mothers with a higher income level. This result suggests that the financial burden of child-rearing might not be a leading mechanism through which childbearing worsens maternal mental health. Consistent with this, we document little evidence of heterogeneous effects of baby bonus by households' income levels.

Lastly, we provide suggestive evidence of a negative relationship between worsened maternal mental health and future fertility decisions. Using mother-level data on healthcare utilization for mental health following the first (or second) childbirth and the subsequent birth of the second (or third) child, we find that mothers who experienced mental health issues after their first childbirth are less likely to have a second child. This relationship becomes less pronounced when examining the transition from the second birth to a third child. Additionally, the baby bonus appears to have little effect in mitigating the negative association between maternal mental health issues and the likelihood of having another child.

This paper contributes to two strands of economic literature.

First, this study contributes to the literature on the consequences of childbirth and the motherhood penalty by providing new evidence on maternal mental health impacts of childbirth. As a leading cause of costs of childbirth, previous studies have primarily examined the labor market effects of motherhood and changes in intra-household allocation following childbirth (Guryan et al., 2008; Kuziemko et al., 2018; Anderson and Nix, 2022; Kleven et al., 2024). However, evidence on the impact of childbirth on maternal mental health remains limited, which suggests that the costs

of child birth and child-rearing can be under-estimated without considerations of the adverse maternal mental health consequences. Notable exceptions include Chuard (2023) and Ahammer et al. (2024), both of which estimate the mental health costs of parenthood in Austria (with the latter also covering Denmark). Our study differs in several key aspects. While these works focus on the transition into parenthood, we examine the effects of childbirth on maternal mental health across different birth parities. To the best of our knowledge, this is the first study to provide evidence on the impact of childbirth on maternal mental health in a lowest-low fertility context. Additionally, we explore how pro-natalist policies shape maternal mental health and, in turn, influence subsequent fertility decisions.

Second, this study contributes to the literature on the impacts of pro-natalist policies by providing novel evidence on the interplays between a cash-transfer program and the adverse mental health impact of birth. A robust body of literature also explores the effects of pro-natalist policies, which is one of the most frequently used policy levers, on fertility and children's outcomes (Malak et al., 2019; González, 2013; González and Trommlerová, 2023; Anukriti, 2018; Hoynes et al., 2015; Kim, 2024). However, to the best of our knowledge, there has been limited evidence of whether such programs can mitigate the motherhood penalty. This study fills this gap in the literature by shedding new light that governments' cash transfers might not effectively mitigate maternal mental health consequences of childbirth. As a possible explanation, our heterogeneity analyses suggest that households' financial burden of raising children might not be the leading mechanism of the negative relationship between childbirth and maternal mental health.

In Section 2, we describe the data and the background of our empirical setting. In Section 3, we discuss our empirical strategy. In Section 4, we present our empirical findings. In Section 5, we conclude.

2 Data and Institutional Background

2.1 Institutional Background

Universal Healthcare System and Mental Healthcare in South Korea South Korea's healthcare system is founded on a universal, single-payer National Health Insurance (NHI) program that has been compulsory for all citizens since 1989 (Kwon, 2009; OECD, 2012). As of 2022, 97.1% of the population is enrolled in the system, while the remaining 2.9% receive coverage through the Medical Aid program (Kim, 2023). Funded by contributions from employees, employers, and government subsidies, this system provides comprehensive coverage. Although cost-sharing through co-payments and deductibles exists, healthcare providers, public and private alike, are subject to standardized reimbursement schedules and government-regulated pricing, which keep out-of-pocket costs relatively low while sustaining high levels of service utilization.

Mental healthcare is also covered by the NHI. The coverage includes psychiatric and psychological outpatient consultations and treatments, inpatient services, and prescription medications, thereby providing a strong institutional foundation for access. Nonetheless, the mental healthcare

utilization is relatively low compared to countries with similar economic development and universal coverage schemes. One contributing factor is cultural stigma surrounding mental illness, which discourages individuals from seeking timely help or acknowledging mental health concerns (Sung-kil and In-sok, 2017; Chung and Yang, 2020). Efforts to reduce stigma and increase awareness are ongoing in South Korea, yet some sociocultural barriers persist, potentially affecting care-seeking behavior (Ng, 1997; Park and Jeon, 2016; Kim, 2021). Additionally, although insurance is comprehensive, co-payments for mental health treatments can be higher than for other medical services, creating financial disincentives to seek care (Jang et al., 2018).[1]

Persistent Decline in Fertility and Local Pro-natalist Casth Transfers—Baby Bonus Before the 1960s, South Korea's total fertility rate was high, averaging more than six children per woman. However, following over two decades of active and highly effective family planning initiatives, the fertility rate fell below the 2.1 replacement level in 1983 and has ranked the lowest in terms of total fertlity rates since 2018 (Lee and Chien, 2016).[2] Beginning in 2001, some local governments adopted pro-natalist cash-transfer policies, providing financial incentives to families with newborns.[3] By 2012, every district had implemented this policy, which featured consistent eligibility criteria and benefit structures across regions. All families with newborns were entitled to receive the baby bonus—regardless of income or employment status—as long as their district offered the benefit for that particular birth order. To receive the baby bonus, parents simply needed to register their child's birth at their district's civic center.

The top panel of Figure 1 displays the proportion of districts providing a baby bonus for first births (blue circles), second births (red squares), and third births (green diamonds) each year from 2000 to 2015. Prior to 2005, only about 10% of districts offered the baby bonus for any birth order. By 2015, policy coverage increased to 44% for first births, 88% for second births, and 100% for third births. Prior to 2005, the baby bonus was available only in 10% of the districts for all birth orders. In 2015, the policy prevalence reaches 44%, 88%, and 100% for first, second, and third child, respectively.[4]

[1] In contrast, many European countries and Canada exhibit higher rates of mental health treatment utilization, aided by more robust anti-stigma campaigns, stronger community-based mental health networks, and policies that more evenly align psychiatric and non-psychiatric cost-sharing structures (OECD, 2021).

[2] In the mid-2000s, concerns over South Korea's declining fertility and rapidly aging population intensified. In response, the national government introduced the First Basic Plans for Low Fertility and Aged Society in 2006. These plans established national-level objectives aimed at addressing demographic challenges. Because South Korea's administrative framework ensures that its national pro-natalist initiatives are accessible nationwide, we assume that the impacts of the national policies are uniform across the country.

[3] South Korea's administrative hierarchy consists of a national government, followed by first-level subdivisions that include 17 provinces (e.g., Seoul, Gyeonggi, and Busan), each of which oversees a second tier of local jurisdictions (hereon, referred to as districts). We exclude districts that underwent administrative mergers or splits during our sample period (Sejong, Changwon, Chungju, and Jeju), as policy data for these areas are not available. Our analysis therefore focuses on 222 districts in South Korea.

[4] Kim (2024) examines the determinants of policy adoption and concludes that local public budgets drive implementation decisions, whereas demographic characteristics including past fertility trends do not explain why certain districts adopted the baby bonus earlier or later. Conditional on local government budgets and regional conditions, the residual variation in adoption timing appears exogenous, possibly driven by "arguably random factors, such as the distribution of policymakers' subjective beliefs about the efficacy of and political returns from the baby bonus and the

2.2 Data

We use the NHI claims data for a random sample of one million individuals selected in 2002 and tracked through 2018. From these records, we identify women who gave birth during the sample period, determine birth order, and document their mental healthcare usage around each childbirth. Because all healthcare utilization under the universal NHI system generates a claims record, which includes detailed diagnostic and treatment information, we are able to link individual records over time and assemble each person's complete medical history within the sample window.

A limitation of this dataset is that it does not directly report childbirth information. For privacy reasons, certain treatment codes—including those for deliveries—are masked in the claims-level data. We address this challenge in two ways. First, we focus on physician and hospital visits that are labeled as "childbirth"-related services among female patients. Second, we rely on standard medical practice indicating that uterotonic medication is prescribed to induce labor and prevent postpartum hemorrhage for both natural deliveries and cesarean sections, but not for other treatments (Buckley et al., 2023). Consequently, we use the prescription date of uterotonic to identify mothers and pinpoint their childbirth date.[5] Once a birth is identified, we establish birth order based on each woman's sequence of childbirth within our data.

Because we begin tracking births in 2002, this procedure can misclassify birth orders for women who delivered children before entering the sample. For example, if a woman gave birth to her third child in 2003, the data might mistakenly classify it as her first. To address this issue, we restrict our sample to births occurring from 2005 onward, enabling the initial three years of data to capture any earlier childbirths. This restriction also ensures that we observe at least three years of medical history before each childbirth.

We construct a balanced monthly panel of mothers spanning 36 months before and 36 months after each childbirth, distinguishing by birth order. We limit our analysis to first, second, and third childbirths, as these birth orders account for the majority of births in Korea. Because the claims data end in 2018, our final sample consists of mothers who gave birth between 2005 and 2008. Each month, we record a binary indicator reflecting whether a mother utilized any medical services that doctors classified as mental health. Thus, in this paper, we refer to mental healthcare utilization and diagnosis interchangeably. Although this measure may under- or over-estimate the true prevalence of mental health conditions, our interest lies in the change in observed outcomes around childbirth. Under the assumption that the probability of seeking care, given mental illness, remains stable before and after childbirth, we can consistently estimate childbirth effects on maternal mental health. All the sample restrictions together results in 51,839 mothers giving birth to their first child, 25,707 mothers giving birth to their second child, and 4,517 mothers giving birth to their third child between 2004 and 2015.

In addition to claims records and basic demographic information on enrollees (e.g., birth year and gender), the NHI dataset includes annually updated enrollment data, which provides infor-

distribution of idiosyncratic errors in forecasting local fiscal capacity to operationalize local baby-bonus programs."

[5] Any additional uterotonic prescription within 10 months of the initial date is considered part of the same birth.

mation on each enrollee's residential location, insurance premium, and enrollment type. Because insurance premiums are assessed based on earnings, they serve as a proxy for household income. Enrollment types indicate whether the mother is covered under her own employment or as a dependent under another enrollee, which allows us to track employment status. Lastly, residence information enables us to determine each mother's location each year and whether she received a baby bonus for her childbirth. We use the baby bonus policy dataset from Kim (2024), which contains information on whether each district offered a baby bonus for different birth orders during the sample period.[6]

In the bottom panel of Figure Figure 1, we plot the average annual mental healthcare utilization from 2002 to 2018 among all mothers identified in our sample (blue circles), mothers had two and three childbirths (red squares), and those with three childbirths (green diamonds). Overall, utilization rates increase over time across all birth orders, and the trends appear largely parallel. By construction, these women experienced childbirth at some point between 2004 and 2015, during which the baby bonus policy also became more widely adopted. The central aim of this paper is to assess how childbirth and the baby bonus contribute to the observed rise in mental healthcare utilization (or equivalently, diagnoses), net of aggregate time trends.

3 Empirical Strategy

We extend the event study framework from Kleven et al. (2021) to investigate the evolution of maternal mental health around childbirth and to evaluate the role of the baby bonus in shaping these dynamics.[7] Specifically, we track mental health changes for mothers who receive the baby bonus relative to those who do not, using a balanced monthly panel observed from three years before through three years after childbirth.

For each birth order, we estimate the following specification:

$$S_{i,t} = \sum_{\tau=-36}^{36} \left(\delta_\tau E_{i,t,\tau} + \omega_\tau E_{i,t,\tau} \times BB_i\right) + \phi_i + \psi_{c,y} + f\left(Age_{i,t}\right) + \epsilon_{i,t}, \tag{1}$$

where $S_{i,t}$ is the outcome, defined as a binary indicator for whether mother i received mental healthcare in time t (year-month pair). The event study indicator $E_{i,t,\tau}$ takes the value of 1 if time t is τ months before (for $\tau < 0$) or after (for $\tau > 0$) childbirth for mother i. BB_i is an indicator equal to 1 if mother i received the baby bonus. By normalizing δ_{-11} and ω_{-11} to zero, we set the month immediately preceding to pregnancy as the reference period.[8] Thus, the event

[6] In the appendix, Table A.1 reports the key statistics summarizing our sample of mothers in terms of mental health care utilization, baby bonus status, childbirth timing, age, employment status, and income. Figure A.1 plots the changes in the expenditures on OBGYN-related health services around childbirth to provide evidence supporting our strategy to identify models from claims-level records.

[7] A variant of the event study model has been widely applied to estimate dynamic effects of economic shocks (Jacobson et al., 1993; Braun and Raddatz, 2008; Gathmann et al., 2020), policy implementations and reforms (Lafortune et al., 2018; Kim, 2024), and parenthood (Kleven et al., 2019; Anderson and Nix, 2022; Kleven et al., 2024; Ahammer et al., 2024).

[8] A full-term pregnancy is 40 weeks, or 10 months.

study coefficients $\{\delta_\tau\}$ capture the change in the probability of receiving mental healthcare (or equivalently diagnosis of mental health conditions) at event time τ relative to the reference month (i.e., childbirth effects). Correspondingly, the coefficients on the interaction terms between the event study and baby bonus indicators $\{\omega_\tau\}$ capture the additional change in mental healthcare use for mothers who received the baby bonus at each event time, again relative to one month before pregnancy (baby bonus effects). Standard errors are clustered at the individual (mother) level to account for within-individual correlation and are robust to heteroskedasticity.

We further include a comprehensive set of fixed effects and a polynomial in age in Equation 1 to purge out three key sources of potential confounding. First, individual fixed effects ϕ_i account for all (un)observed, time-invariant characteristics that might influence each mother's overall mental healthcare use. Second, province-by-year fixed effects $\psi_{c,y}$ capture temporal variations or trends—such as changes in healthcare supply or social stigma associated with mental illness and care-seeking behavior—specific to each province. Finally, we include a polynomial function of the mother's age at time t, $f\left(Age_{i,t}\right)$, to control for increasing age profiles.

Identifying the causal impacts of childbirth and the baby bonus relies on several assumptions conditional on the individual fixed effects, province-by-year fixed effects, and age profiles.[9] The first is the standard parallel trends assumption: prior to pregnancy (thus childbirth and receiving baby bonus if their district of residency offered baby bonus), mental healthcare use would have followed, on average, the same trends across mothers who gave birth at different times and in different districts. The second is no anticipation: in the absence of pregnancy or baby bonus, the pre-childbirth trend in mental healthcare would remain unchanged. In other words, these assumptions require that mothers would have followed the same path had they not had a first child or an additional child (for higher-birth orders). Because we estimate childbirth and baby bonus effects separately by birth order, these assumptions need not hold across birth orders.[10] As evidence in support of these assumptions, we show that, across birth orders, the estimated coefficients on the event-time dummies and interaction terms for periods prior to pregnancy ($\tau < -11$) are not jointly statistically different from zero at the 5% level.

4 Results

4.1 Effects of birth and baby bonus on maternal mental health

Figure 2 presents our event-study estimates of the effects of childbirth and the baby bonus on the incidence of maternal mental health diagnoses, based on Equation 1. We report the estimated δ_τs and ω_τs, along with their 95 percent confidence intervals, in Panels A and B, respectively.

[9] A recent literature (de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2024) discusses the identifying assumptions behind difference-in-differences and event-study models.

[10] Childbearing decisions are rarely random. For example, a woman determined to remain childless for career reasons might already differ in many respects—such as earnings or mental health—from a woman actively planning pregnancy. We thus exploit variation in the timing of childbirth among those who eventually become mothers, rather than comparing them to individuals who remain childless throughout the sample period.

In Panel A, we present the effects of first, second, and third births in the left, middle, and right columns, respectively. The left and middle columns indicate that maternal mental health deteriorates after the first and second births. Specifically, the incidence of maternal mental health diagnoses begins to rise approximately 12 to 16 months after these births. In contrast, the estimates before childbirth are small in magnitude and statistically insignificant. One exception is the negative estimates observed during pregnancy, suggesting that mothers may reduce their use of mental healthcare services while pregnant. Unlike the patterns observed for the first two births, the right column provides little evidence that the third birth increases maternal mental health diagnoses. The estimates are noisy and statistically insignificant.

In Panel B, we present the effects of the baby bonus for first, second, and third births in the left, middle, and right columns, respectively. Across all columns, we find little evidence that the baby bonus mitigates the adverse effects of childbirth on maternal mental health. The estimates are small in magnitude and statistically insignificant. Although the right column suggests a potential increase in estimates approximately 24 months after the third birth, the estimates are generally noisy and statistically insignificant. Given this, we caution against overinterpreting these patterns.

Our findings on the adverse effects of childbirth on maternal mental health align with those of Chuard (2023) and Ahammer et al. (2024). These results suggest that the costs of childbirth and child-rearing may be underestimated if maternal mental health is not considered. Furthermore, our findings on the baby bonus indicate that government pro-natalist cash transfers may have a limited role in alleviating the maternal mental health costs associated with childbirth.

4.2 Potential Mechanisms

In this subsection, we explore potential mechanisms on the adverse mental health impacts of birth and limited roles of baby bonus.

Liquidity constraint For the first possible mechanism, we examine the role of liquidity constraints. Since childbirth and child-rearing impose significant financial burdens on mothers and households, these costs could contribute to deteriorating maternal mental health. If financial constraints are the primary driver of our baseline findings, we would expect the adverse effects of childbirth to be more pronounced among low-income mothers. Additionally, we would anticipate that the baby bonus mitigates these negative mental health effects for those mothers.

To test this hypothesis, we divide our sample into high- and low-income groups based on individuals' National Health Insurance (NHI) premium payments.[11] We then estimate the effects of childbirth and the baby bonus separately for each income group.

Figure 3 presents the effects of childbirth and the baby bonus on the incidence of maternal mental health diagnoses by income level. Panels A and B display the effects of childbirth and the baby bonus, respectively, by birth parity.

[11] Since higher-income individuals pay higher NHI premiums, we classify individuals as high-income if their premiums are above the median and low-income if they are below the median.

Panel A shows that the adverse effects of childbirth on maternal mental health are more pronounced among high-income mothers. The left column indicates that for first births, high-income mothers experience a greater deterioration in mental health. However, the middle and right columns suggest little heterogeneity in the mental health effects of second and third births by income level. Panel B reveals no significant heterogeneity in the effects of the baby bonus. Across all columns, the estimated effects are small in magnitude and statistically insignificant. These findings suggest that liquidity constraints might not be the primary cause of the adverse mental health effects of childbirth.

To reconcile results of our heterogeneity analyses by income, we conjecture that high-income households may spend relatively higher child-rearing costs as a proportion of their income. This suggests that, in the context of child-rearing expenses, high-income households may experience a more significant financial burden. To examine this conjecture, we use data from the Korean Labor and Income Panel Study (KLIPS) to estimate the relationship between household income and expenditures on children's education among households with children under the age of 19. We estimate the correlation between household income and (i) total expenditures on children's education and (ii) the share of children's education expenditures out of total household spending, while controlling for district-level fixed effects, province-by-year-month fixed effects, and the number of children under 19. The results indicate that household income has positive associations with both the absolute amount spent on children's education and the proportion of total household expenditures allocated to children's education.[12] Our results provide suggestive evidence that high-income households may face greater costs of child-rearing. Consequently, the adverse mental health impacts of birth can be greater for high-income group mothers and the effects of baby bonus in mitigating maternal mental health deterioration may be limited.

Job Separation and Return-to-work Pressure Childbirth can have a persistent negative impact on maternal labor market participation (Kleven et al., 2019, 2021; Anderson and Nix, 2022; Kleven et al., 2024). Since labor market participation is linked to life satisfaction (reference to be added once finalized), a decline in maternal labor supply may contribute to deteriorating mental health. To test this hypothesis, we estimate the effects of childbirth on maternal mental health by restricting the sample to mothers who left the labor force after childbirth and comparing them to those who never worked. Panel A of Figure 4 shows that the adverse mental health effects of childbirth are similar across maternal employment statuses.[13] These findings suggest that job separation is unlikely to be the primary driver of the negative mental health effects observed in our baseline analysis.

Additionally, mothers who remain employed may experience psychological distress from balancing work and childcare responsibilities. To examine the role of this return-to-work pressure, we

[12] In the appendix, Table A.2 reports the results regressing annual spending on children's education in levels in Column 1, in log unit in Column 2, and in fraction out of income in Column 3 on income. Figure A.2 shows that the average expenditure share on children's education increases in income quantiles.

[13] We exclude results on third births due to the relatively small sample size, which becomes even more limited when stratifying by maternal labor market status.

compare the mental health effects of childbirth among mothers who have continuously worked to those who have never worked. Panel B indicates that the adverse mental health effects of childbirth are similar between these groups, suggesting that return-to-work pressure is unlikely to be the main mechanism underlying our baseline findings. However, we acknowledge that our analysis may underestimate the role of return-to-work pressure, as our measure of maternal mental health is based on claims data. If working mothers have less time to seek medical care, the actual mental health impact of childbirth for these mothers may be greater than observed. Given this limitation, our findings regarding continuously employed mothers should be interpreted as suggestive rather than conclusive, warranting caution in their interpretation.

Relaxed Stigma Our findings may be driven by a reduction in stigma associated with mental health treatment. Mothers with preexisting mental health conditions may have avoided seeking care due to stigma. However, postpartum depression is widely recognized, which may provide a justification for seeking treatment. This could lead to an increase in diagnosed mental health conditions, meaning our results do not necessarily imply a negative mental health effect of childbirth. However, this explanation is unlikely for the following reasons: (i) If mothers with preexisting conditions were using postpartum depression as a justification for seeking treatment, diagnoses should increase immediately after birth and decline over time. Instead, we find that incidence rises several months after birth and continues to increase. (ii) If preexisting conditions explained the results, the increase in diagnoses should be larger after the first birth than after the second. However, we find stronger effects after the second birth. These patterns suggest that childbirth itself, rather than a reduction in stigma, explains the observed increase in diagnoses.

4.3 A relationship between maternal mental health and birth

In this subsection, we examine the relationship between the costs of childbirth and child-rearing and fertility decisions. Specifically, we investigate whether the adverse effects of childbirth on maternal mental health are negatively associated with the likelihood of having another child. To explore this, we estimate the heterogeneous effects of childbirth and the baby bonus based on whether mothers proceed to have another child. Among mothers who had a first (second) birth, we divide the sample into two groups: those who had a second (third) birth and those who did not.

Panel A of Figure 5 presents the estimated effects of childbirth based on subsequent birth status. The left column shows that the overall adverse mental health effects of the first birth are primarily driven by mothers who did not have a second child. These mothers experienced an increased likelihood of receiving a mental health diagnosis, whereas those who had a second child did not exhibit a significant increase in mental health diagnoses. The right column indicates little heterogeneity in the effects of the second birth based on whether mothers proceeded to have a third child. Panel B provides little evidence of heterogeneous effects of the baby bonus, regardless of birth parity.

Our findings suggest that the adverse mental health consequences of childbirth may discourage

subsequent fertility. However, the baby bonus appears unlikely to mitigate the negative relationship between maternal mental health and the decision to have another child. These results provide new evidence that the costs associated with childbirth may be a key mechanism contributing to declining fertility rates.

5 Concluding Remarks

This study provides evidence on the adverse mental health effects of childbirth and the limited effectiveness of government pro-natalist cash transfers in mitigating these effects. We find little evidence that the mental health impact of childbirth or the effectiveness of cash transfers is greater for low-income mothers. As a possible explanation, we suggest that child-rearing costs may be higher for high-income mothers. Our analysis of the relationship between maternal mental health and subsequent fertility decisions indicates that poor maternal mental health after childbirth can discourage having another child. This study offers novel evidence that the costs of childbirth and child-rearing may be underestimated if maternal mental health is not considered, and that government cash transfers may be less effective than policymakers initially anticipated.

We acknowledge the limitations of this study. Previous research has shown that government pro-natalist cash transfers can increase fertility, particularly among low-income mothers. However, our study finds little evidence that such transfers reduce the costs of childbirth and child-rearing, as measured by maternal mental health. We suggest that the canonical life-cycle model with liquidity constraints is unlikely to fully explain these findings. Instead, fertility decisions may also be influenced by behavioral factors, such as myopic decision-making. Although we are unable to examine the role of these behavioral factors or propose a policy alternative, we argue that further research is needed to better understand their impact on household fertility decisions and to inform more effective policy design.

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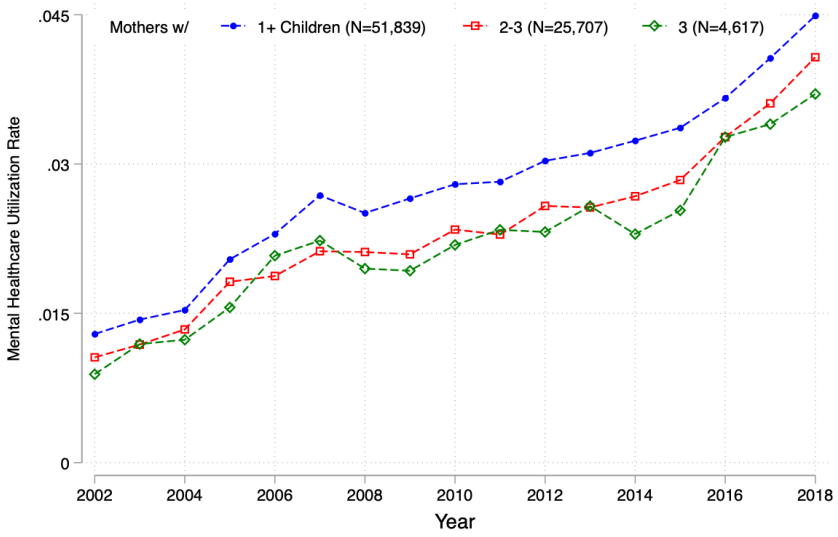
Figures and Tables

Figure 1: Baby Bonus and Maternal Mental Healthcare Use over Time

(a) Baby Bonus Prevalence



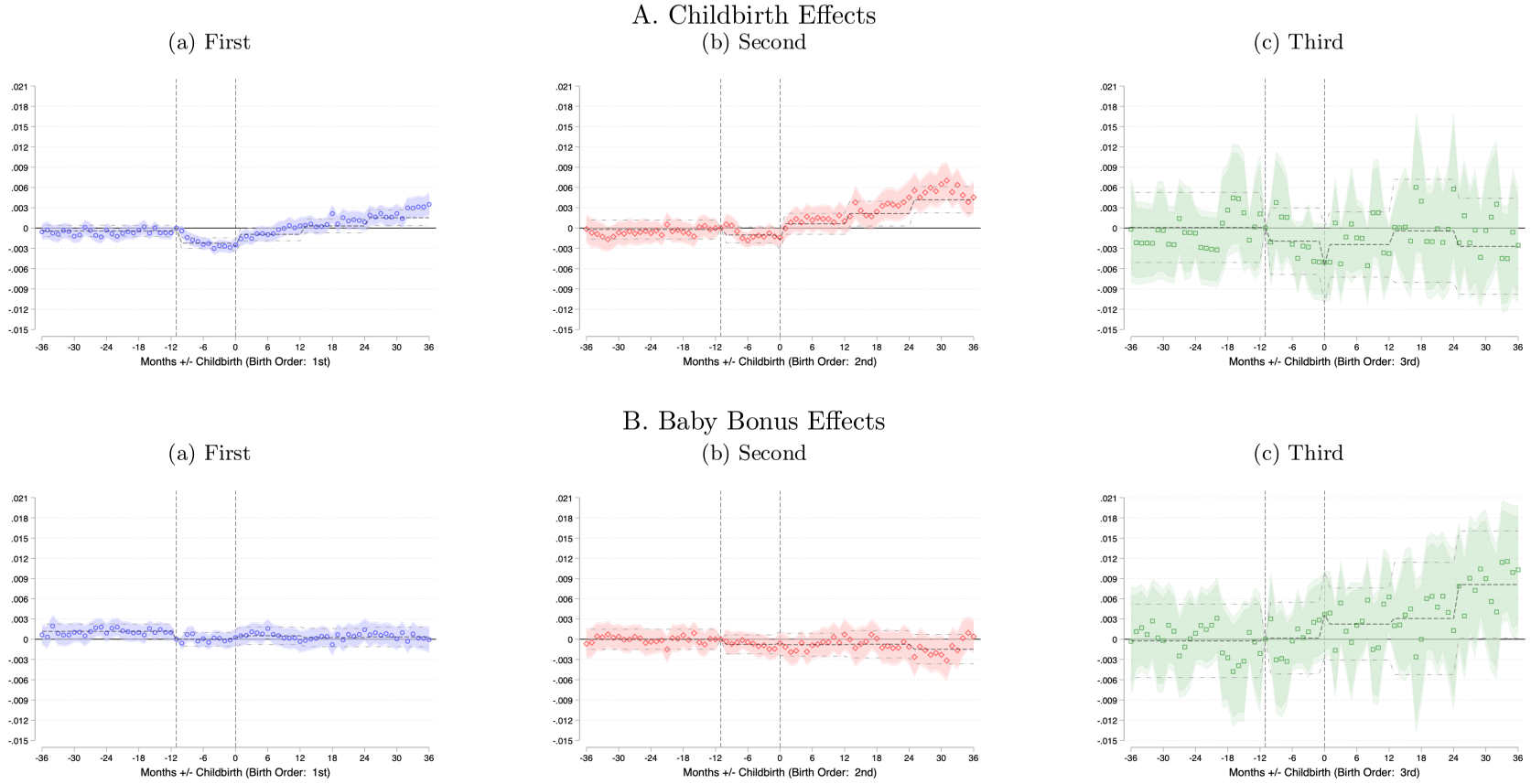
(b) Mental Healthcare Utilization



Data source: Kim (2024) and Health Insurance (South Korea), 2002 – 2018

Notes: The top panel of this figure plots the fraction of districts with baby bonus available for first (blue circle), second (red square), and third (green diamond) child from 2000 to 2015 in the top panel. The bottom panel shows the average mental healthcare utilization rate among from 2002 to 2018 among all mothers identified in our sample (blue circles), mothers had two and three childbirth (red squares), and those with three child birth (green diamonds).

Figure 2: Event-Study Design Estimates on the Effects of Birth and Baby Bonus on Maternal Mental Health



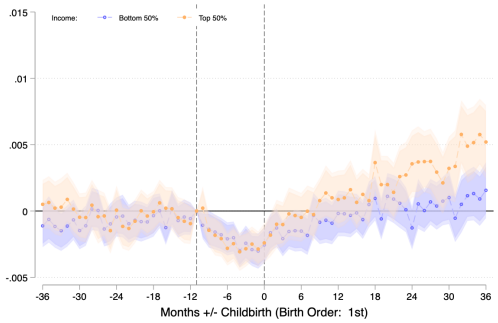
Data source: National Health Insurance (South Korea), 2002 – 2018

Notes: This figure plots the estimated childbirth (top) and baby bonus effects (bottom) on mental health for first (left), second (middle), and third (right) childbirth based on Equation 1. The dependent variable is a dummy variable indicating whether a mother had any health insurance claims with diagnosis of mental health issues during each month. For all panels, we include the individual fixed effects, province-by-year fixed effects, and age polynomials. We calculate standard errors clustered at individual mother level and corrected for heteroskedasticity. Lighter (darker) shades indicate 95% (90%) confidence intervals.

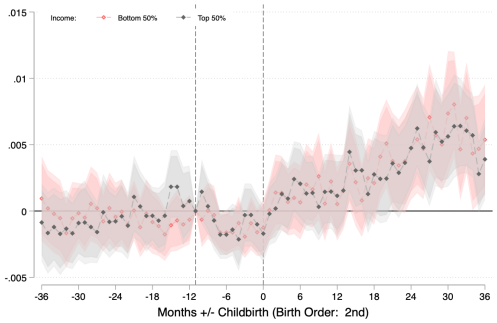
Figure 3: Heterogeneity by Household Income

A. Childbirth Effects

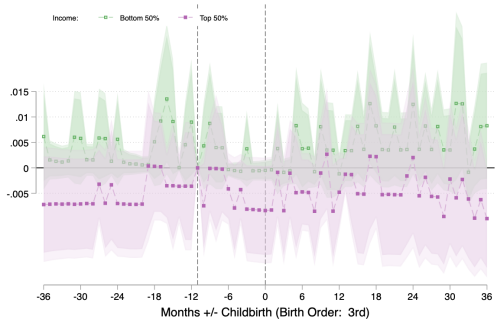
(a) First



(b) Second

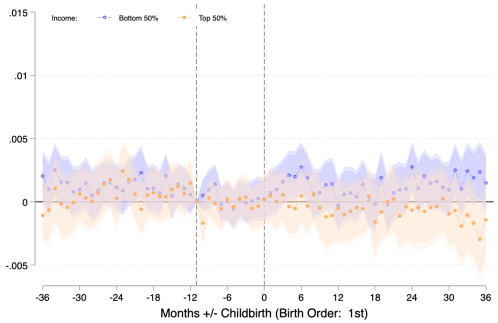


(c) Third

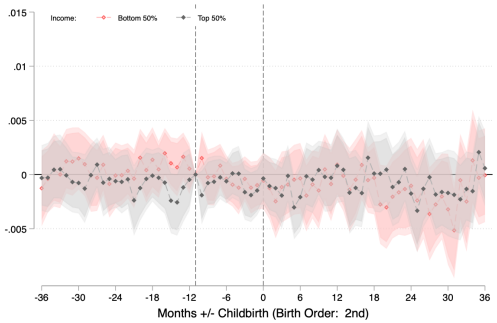


B. Baby Bonus Effects

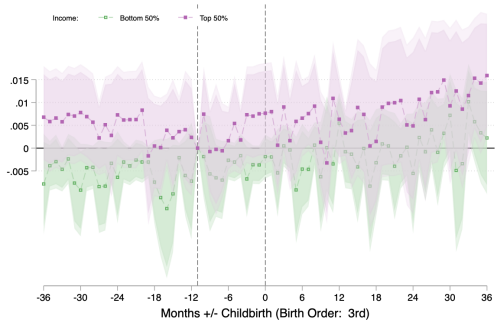
(a) First



(b) Second



(c) Third



Data source: National Health Insurance (South Korea), 2002 – 2018

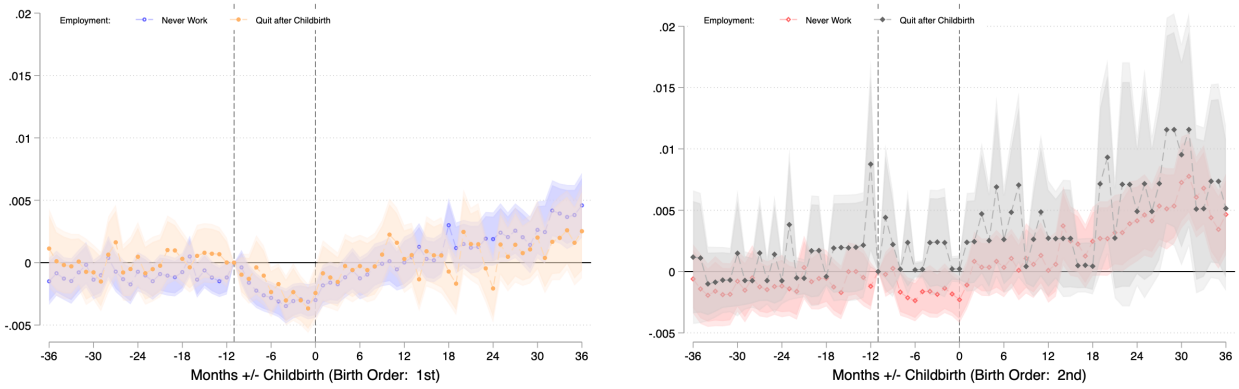
Notes: This figure plots the estimated childbirth (top) and baby bonus effects (bottom) on mental health for first (left), second (middle), and third (right) childbirth based on Equation 1. Each panel compares the estimated effects for mothers above to those below the median in terms of household income. The dependent variable is a dummy variable indicating whether a mother had any health insurance claims with diagnosis of mental health issues during each month. For all panels, we include the individual fixed effects, province-by-year fixed effects, and age polynomials. We calculate standard errors clustered at individual mother level and corrected for heteroskedasticity. Lighter (darker) shades indicate 95% (90%) confidence intervals.

Figure 4: Potential Labor Market Channels Explaining the effects of Childbirth

A. Job Separation

(a) First

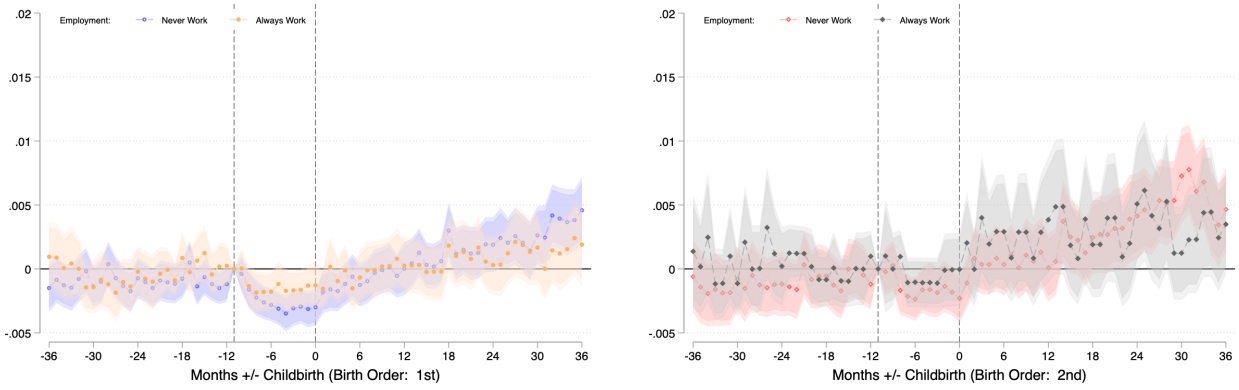
(b) Second



B. Return-to-work Pressure

(a) First

(b) Second



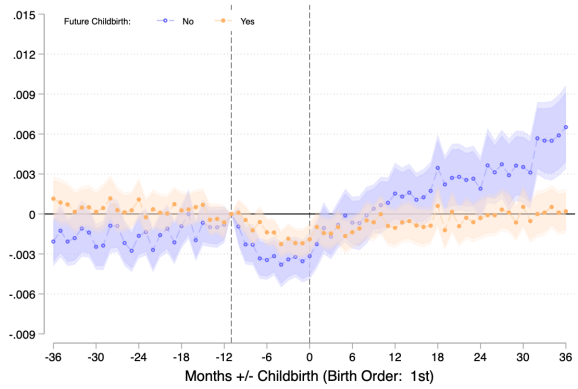
Data source: National Health Insurance (South Korea), 2002 – 2019

Notes: This figure plots the estimated childbirth on mental health for first (left) and second (right) childbirth based on Equation 1, separately by mother's employment status. The top (bottom) panels compare the estimated effects for mothers who did not work 24 months before and after childbirth to those who had worked 24 months before but were not employed 24 months after (to those who worked before and after childbirth). The dependent variable is a dummy variable indicating whether a mother had any health insurance claims with diagnosis of mental health issues during each month. For all panels, we include the individual fixed effects, province-by-year fixed effects, and age polynomials. We calculate standard errors clustered at individual mother level and corrected for heteroskedasticity. Lighter (darker) shades indicate 95% (90%) confidence intervals.

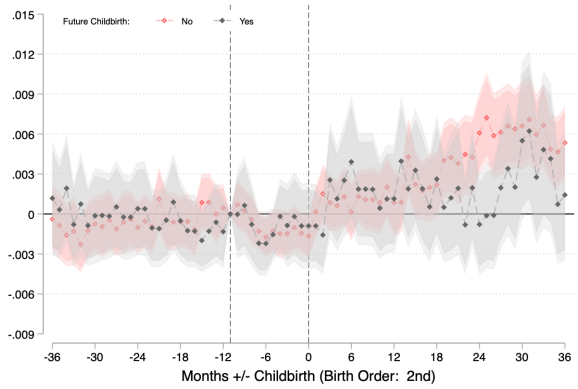
Figure 5: Heterogeneity by Future Child Birth

A. Childbirth Effects

(a) First

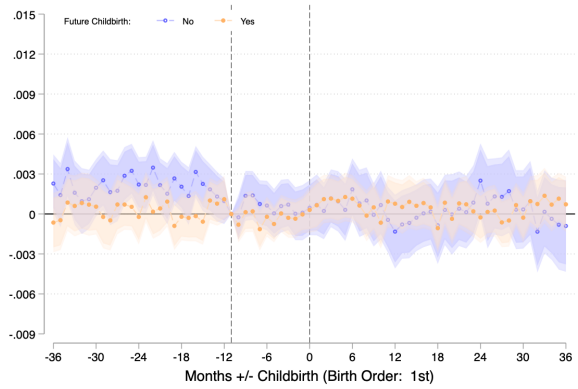


(b) Second

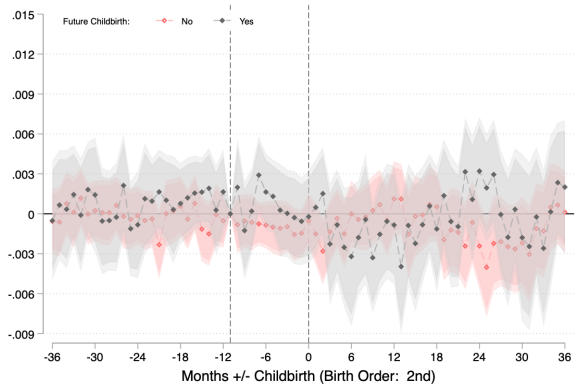


B. Baby Bonus Effects

(a) First



(b) Second



Data source: National Health Insurance (South Korea), 2002 – 2018

Notes: This figure plots the estimated childbirth (top) and baby bonus (bottom) effects on mental health for first (left) and second (right) childbirth based on Equation 1, separately by mother's future fertility. Each panel compares the estimated effects for mothers who had additional childbirth to those who did not. The dependent variable is a dummy variable indicating whether a mother had any health insurance claims with diagnosis of mental health issues during each month. For all panels, we include the individual fixed effects, province-by-year fixed effects, and age polynomials. We calculate standard errors clustered at individual mother level and corrected for heteroskedasticity. Lighter (darker) shades indicate 95% (90%) confidence intervals.

Appendix: Additional Figures and Tables

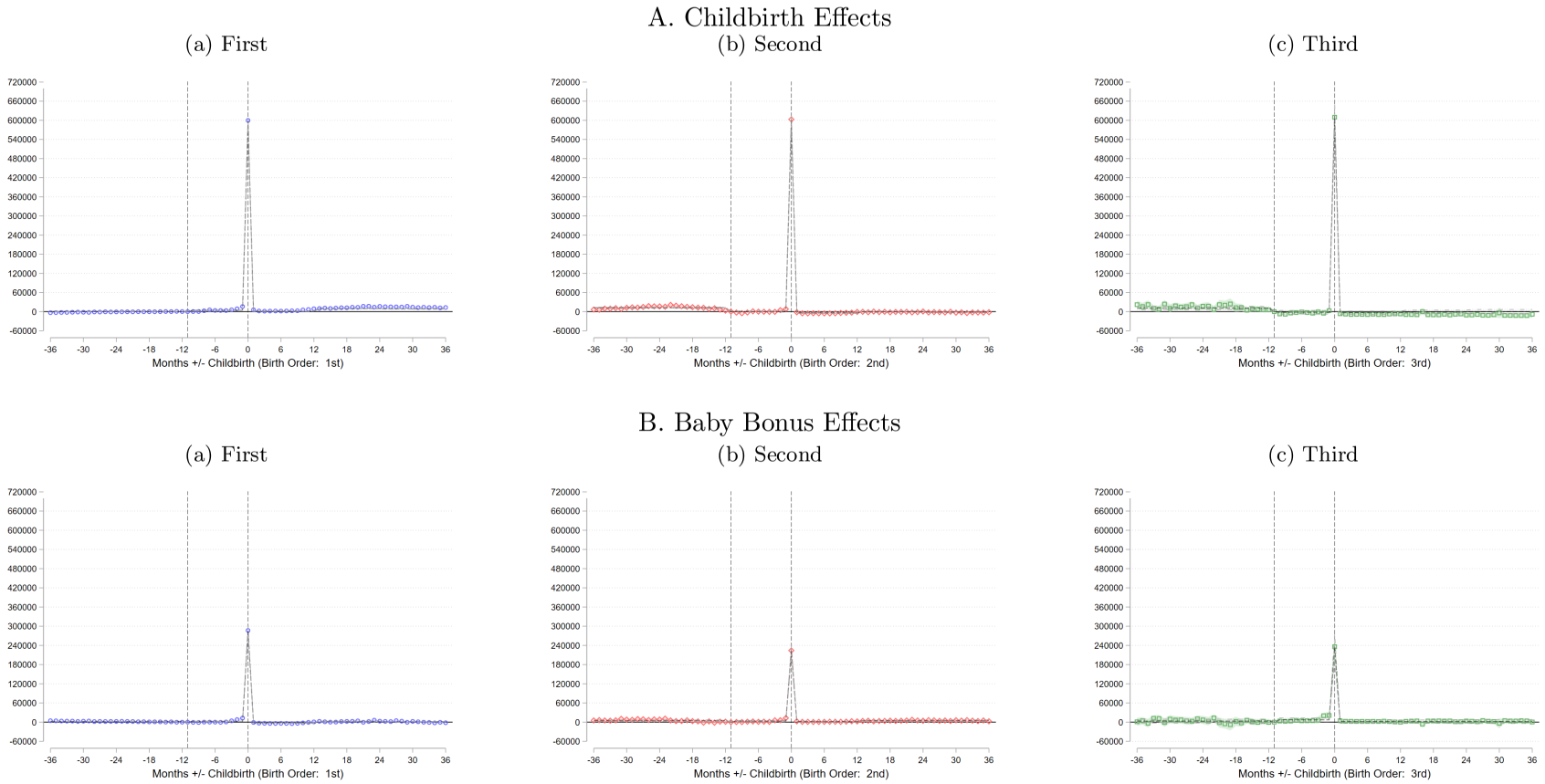
Table A.1: Summary Statistics

	First Child mean (sd)	**Second Child** mean (sd)	**Third Child** mean (sd)
PANEL A. OUTCOME VARIABLES			
Utilization Indicator	0.0053	0.0042	0.0045
	(0.0727)	(0.0650)	(0.0673)
Average Monthly Spending	547.96	414.28	453.27
	(22322.46)	(16411.84)	(19244.13)
Average Monthly Spending | Spending > 0	103175.6	97518.17	99761.78
	(288512.3)	(232248.65)	(267671.44)
PANEL B. INDIVIDUAL CHARACTERISTICS			
Baby Bonus (Fraction)	0.6306	0.7726	0.8908
	(0.4826)	(0.4192)	(0.3119)
year of child birth	2008.69	2009.90	2011.21
	(3.38)	(3.08)	(2.62)
age at child birth	30.62	31.93	33.45
	(4.40)	(3.67)	(3.52)
months since previous birth		33.21	32.88
		(18.8572)	(18.9981)
employment status (Fraction)	0.4452	0.3717	0.2777
	(0.4970)	(0.4833)	(0.4479)
gaibja_type (%)			
Type 0	0.0107	0.0058	0.0054
Type 1,2 (self-employed)	0.3071	0.2622	0.2827
Type 5,6 (employee)	0.6822	0.7319	0.7119
Observations	3,741,529	1,871,119	336,963
number of moms	51,839	25,707	4,617

Data source: Kim (2024) National Health Insurance (South Korea), 2002 – 2018

Notes: Panel A reports summary statistics of monthly medical spending on mental health in the period 2004-2019. Panel B reports summary statistics of the individual characteristics. Data on employment status, gaibja_type, income level for Type 1,2 and income level for Type 5,6 is based on 24 months before childbirth. All data is from the Korean National Health Insurance Panel.

Figure A.1: Data Validation: OBGYN Expenditures around Childbirth



Data source: National Health Insurance (South Korea), 2002 – 2019

Notes: This figure plots the estimated childbirth (top) and baby bonus effects (bottom) on mental health for first (left), second (middle), and third (right) childbirth based on Equation 1. The dependent variable is total spending on OBGYN health services each month. For all panels, we include the individual fixed effects, province-by-year fixed effects, and age polynomials. We calculate standard errors clustered at individual mother level and corrected for heteroskedasticity. Lighter (darker) shades indicate 95% (90%) confidence intervals.

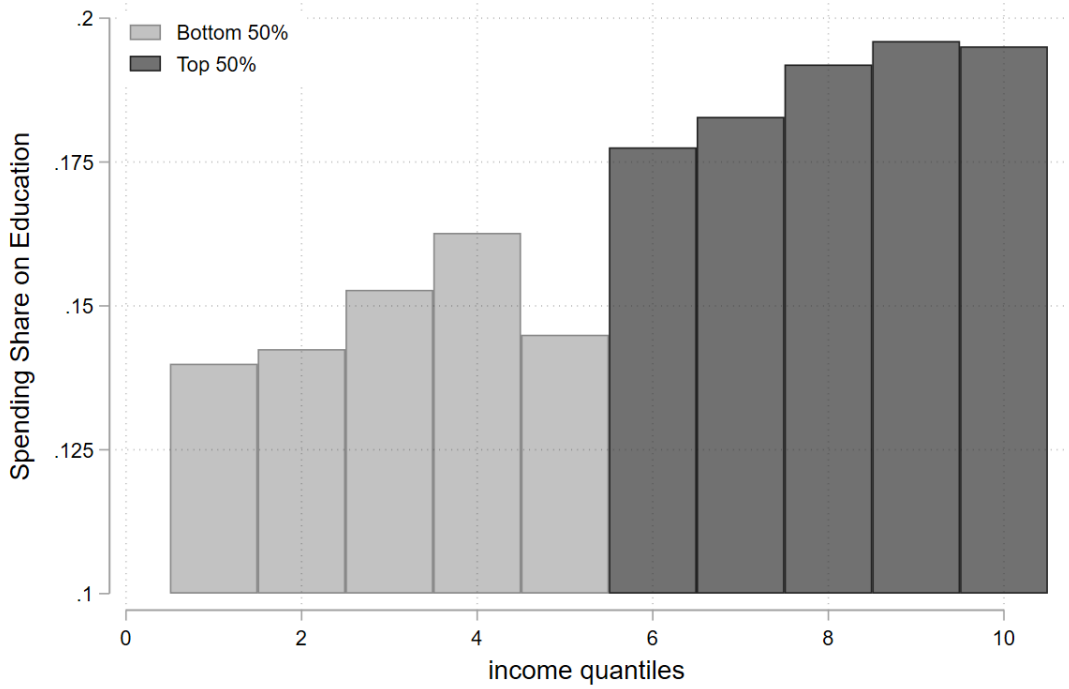
Table A.2: Correlation between Income and Child Education Expenses

Dependent Variable:	(1)	(2)	(3)
Spending on Education	Level	Log	Share
Income	0.0839***		
	(0.00463)		
Log(Income)		0.465***	0.0218***
		(0.0167)	(0.00121)
Constant	233.3***	2.356***	−0.0127
	(22.05)	(0.140)	(0.00991)
Fixed Effects:			
District	Yes	Yes	Yes
Survey Month-Year	Yes	Yes	Yes
Number of Children (under 19)	Yes	Yes	Yes
N	32,884	29,015	32,884
R^2	0.276	0.227	0.0252

Data source: Korea Labor & Income Panel Survey (Wave 8–22)

Notes: This table reports the results regressing spending on children's education in levels (Column 1), in log unit (Column 2), and in fraction out of income (Column 3) on household income for households with children under the age of 19. Across columns, we include district fixed effects, province-by-year-month fixed effects, and a set of dummy variables indicating the number of children under 19. Heteroskedasticity-robust standard errors are reported in parentheses: $*$ $p < 0.1$; $**$ $p < 0.05$; $***$ $p < 0.01$.

Figure A.2: Spending share of child education costs by income quantiles



Data source: Korea Labor & Income Panel Survey (Wave 8–22)

Notes: This figure plots the average expenditure share of children's education by income quantiles.