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DISCUSSION PAPER SERIES

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ABSTRACT

Paid Childcare Leave, Fertility, and Female Labor Supply in South Korea*

We consider the effects of a paid childcare leave subsidy on maternal behavior in South Korea using a difference-in-difference design and a fertility survey with information on conception, contraception, and labor supply arrangements. Childcare subsidies increased conception and decreased contraception. The arc elasticities of the responses of conception and contraception to the childcare subsidy are 0.65 and -0.10, respectively. However, we do not find effects on employment arrangements. In a country with the lowest total fertility rate in the world and that often performs middling in rankings of gender inequality, we conclude that paid childcare leave for working women confers some positive benefits.

JEL Classification: J18, J13

Keywords: childcare leave, fertility, labor supply, Korea

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1 Introduction

Large increases in life expectancy usually result in rapid population aging. This is an important economic risk as it can increase fiscal burdens through a rapid increase in welfare expenses. This is especially true in richer countries where governments will struggle to provide healthcare and pensions for their retirees. In fact, the International Monetary Fund estimated that the costs of the Global Financial Crisis in 2008-09 were just 10% of age-related costs in the G20 in the same period (The Economist, 2009).

In particular, aging has had large impacts on South Korea (henceforce Korea) due to a large decline in fertility. The total fertility rate (TFR) in Korea first dropped below the population replacement level of 2.0 in 1983. Subsequently, it was roughly 1.6 from the mid-1980's to the mid-1990's and then it further decreased to approximately 1.2 during the period 2000 to 2017. By 2018, it dropped below unity to 0.98 - the lowest TFR in the world. As of 2020, TFR in Korea was 0.84.

The rapid decline in fertility rates in Korea combined with rising life expectancy quickened the pace of population aging. This has dramatically increased the dependency ratio of the population aged 65 and over.[1] These demographic changes have raised concerns of numerous negative socioeconomic impacts including a decrease in productivity, a reduction in the tax base, and an increase in social security expenditures.

To mitigate the adverse effect of aging, many countries have employed pro-natal policies. Examples of pro-natal policies include increases in maternity leave, child tax credits, and childcare subsidies. Such policies have become increasingly common throughout the world with Poland, Hungary, Canada, the United States, Japan, and Korea all having pro-natal policies. In fact, data from the United Nations indicate that 28% of nations had pro-natal policies in 2015, whereas only 15% did in 2001 (Stone, 2020).

In response to low TFR, the Korean government implemented an aggressive pro-natal

[1] The age 65 dependency ratio is defined as the ratio of people 65 and older to the population aged 15 to 64.

program in 2011 in which it provided paid childcare leave for working mothers for up to one year. The program provided a monthly subsidy of 500 USD (US Dollars) to all women with children earning less than 1250 USD per month. The subsidies then increased by 0.40 USD per dollar of earnings up to 1000 USD. The subsidies were then held constant at 1000 USD for all working mothers earning 2500 USD or more per month. Prior to 2011, all women were eligible for a flat 500 USD childcare monthly subsidy irrespective of their income. Accordingly, the policy change increased childcare subsidies for higher earning women but held them constant for lower earning women. This policy function is illustrated in Figure 1. In this paper, we employ this policy to identify the effects of childcare subsidies on fertility, contraception, and labor supply.

At its core, our identification strategy is a difference-in-difference (DD) design similar to other studies in the pro-natal policy literature (Milligan, 2005; Cohen et al., 2013; Raute, 2019). We use two waves of survey data prior to the implementation of the policy and two survey waves after its implementation while exploiting differences across women whose predicted earnings are above and below 1250 USD where we predict earnings using exogenous characteristics. We then build on this DD design by exploiting marginal changes in subsidies at the kinks generated by the policy depicted in Figure 1 in a regression kink (RK) design (Card et al., 2015; Simonsen et al., 2016). A major advantage of RK estimates is that they allow researchers to back out marginal behavioral responses which cannot be obtained from straight DD designs.[2] However, they are local to the thresholds and can be less precise than the DD estimates - a problem that is exacerbated by smaller sample sizes in survey data.[3]

[2]Importantly this RK strategy allows us to estimate the impact of giving additional subsidies to women on outcomes as well as taking them away. The reason is that at the 1250 USD threshold, women were eligible for 0.40 *additional* USD of subsidies, whereas at the 2500, women were eligible for 0.40 *fewer* USD of subsidies. So, the marginal fertility incentives are positive at the lower threshold and negative at the higher threshold. A similar exercise is conducted by González and Trommlerová (2021) who study the impacts of providing and then taking away fertility subsidies in Spain.

[3]As we still exploit the fact that we have survey information both before and after the policy was in place, the RK estimation is also done within a difference-in-difference framework. Specifically, the 2011 policy should have changed the impact of the marginal dollar of earnings at the 1250 and 2500

We find that this pro-natal policy increased fertility. Conception increased by between 2.3 and 2.5 percentage points (PP). The arc elasticity of conception with respect to one dollar of potential subsidy is 0.65. Consistent with this, we also find the the childcare subsidies decreased contraception by between 3.4 and 3.6 PP with an implied arc elasticity of -0.10. There is also some weak evidence that the policy had the largest effects on childless women.

We do not find consistent evidence that the childcare subsidies impacted working arrangements. This result is similar to Asai (2015) who showed that a paid leave policy change in Japan did not affect the labor supply of new mothers.

This paper contributes to a growing body of literature that examines the effects of pro-natal policies policies on fertility based mainly on the experiences of Western countries. For example, Milligan (2005) and Cohen et al. (2013) used DD strategies to investigate the effects of a large fertility subsidy in Quebec and Israel, respectively. Both studies found large impacts. Raute (2019) also used a DD strategy exploiting a policy function very similar to ours to estimate the impact of an earnings-based maternity leave reform in Germany on fertility. She found that the policy increased the fertility of college educated women by 23% thereby narrowing the "baby gap" between more and less educated mothers. Like Milligan (2005) and Cohen et al. (2013), Laroque and Salanié (2014) also consider the effects of a child credit estimating a static structural nested logit model using French fertility data and find large effects.[4] Finally, González and Trommlerová (2021) study the effects of a child subsidy in Spain using rich administrative data and find that its implementation increased fertility by 3% whereas its cancellation decreased fertility by 6%.

Our study advances the literature in a number of ways. First, most of the studies of

USD thresholds. Our identification strategy relies upon changes in the effects of marginal subsidies at these two thresholds before and after the policy was in effect. Accordingly, this strategy combines a DD strategy with the RK design.

[4]Related work includes Wolpin (1984) and Keane and Wolpin (2010) who also estimate structural, dynamic models of fertility behavior.

which we are aware are based in Western nations, whereas few are set in East Asia despite the fact that many Asian nations have some of the lowest TFR in the world.[5] Second, our study combines DD estimates with RK estimates, whereas most of this literature exclusively uses DD designs or structural models. RK estimates are important because they allow researchers to identify marginal response of fertility with respect to childcare subsidies. These are policy relevant parameters that are not commonly estimated in the literature. Third, we are also able to estimate the effects of the Korean pro-natal policy on contraception and, importantly, some aspects of female labor supply. Hence, while the absence of administrative data in this study is a limitation in some ways, the use of survey data does enable us to investigate other important outcomes.

The remainder of this paper is organized as follows. In the next section, we describe the institutional details of the childcare leave policy. We then discuss the data that we employ followed by some details of our research designs. Next, we discuss our empirical results and then we offer some concluding remarks.

2 Institutional Details

The Korean government introduced a scheme in 2001 which provided financial support for employees experiencing a decrease in their labor income while on childcare leave. Specifically, the reform states that any employee with a child under the age of eight, or in school in second grade or below, is entitled to paid childcare leave.[6] To be eligible, workers must have been employed for more than one year consecutively, up to the day before childcare leave begins. Benefits do not depend on household income and birth

[5] Probably the most related study is Yoon and Hong (2014) who examine the link between Korea's childcare leave program and wages, but it does not investigate the effect of childcare leave on fertility.

[6] In principle, workers can apply for alternative working hours which entitles them to retain their job during and after their childcare leave. However, not all employers are required to comply with this. Jeon et al. (2022) show that 60% of employers do not allow these alternative arrangements. While the policy does include a right to return clause that allows the worker to return to their previous job, anecdotal evidence suggests that this is not always enforced.

order, but they do depend on *individual* income. Self-employed women were not eligible for benefits. The maximum period of paid leave is one year. Both fathers and mothers are eligible as long as the periods of the leave do not overlap. Nevertheless, the majority of parents who utilize childcare leave are mothers. Benefits are funded by the Employment Insurance Fund operated by the Korean government. Initially, benefits were 300 USD per month in 2001-2003, 400 USD per month in 2004-2006, and then 500 USD per month in 2007-2010.[7]

However, a 2011 reform dramatically increased these benefits for higher income people. This reform was announced as part of the "Second Basic Plan on Low Fertility and Aging Society" in November 2010 and went into effect on January 1, 2011. The level of financial benefits changed from a monthly lump sum payment of 500 USD for everyone to a more complicated schedule depicted in Figure 1. Specifically, women earning less than 1250 USD still received 500 USD per month. However, eligible parents earning between 1250 and 2500 USD per month were provided 500 USD plus 0.40 USD on each dollar of earnings above 1250 USD. Eligible parents earning above 2500 USD received a flat subsidy of 1000 USD. Note that the minimum monthly payment was set at 500 USD guaranteeing that benefits would not decrease following the reform. These benefits and means tests were kept constant in nominal terms during the years of our study and depended on the woman's earnings in the previous year. Finally, one important feature of this policy change is that it did not affect the duration of time that new mothers could receive childcare subsidies. This allows us to isolate the effects of changes in the subsidy generosity on fertility and labor supply outcomes.

[7]This policy is very similar to the German policy studied by Raute (2019). See Table 2 and Figure 1 of that paper, for example.

3 Data

We employ the National Fertility and Family Health Survey from the years 2006, 2009, 2012, and 2015 - a nationally representative cross-sectional survey conducted triennially by the Ministry of Health and Welfare and the Korea Institute for Health and Social Affairs. The survey interviews married women aged 19-49 from 6,000-10,000 households. Since childbirth by unmarried women is uncommon in Korea, we do not include women who gave birth outside of marriage. Further, we exclude separated and divorced women since they were not asked questions concerning conception, birth, and contraception. Because sample sizes vary across survey years, we apply survey weights. The survey contains information on pregnancies, births, use of contraception, health status, employment, as well as other household attributes. We restrict the sample to female wage and salary workers. We exclude women who were self-employed at the time that wages were measured (see Table 1).[8] However, in a robustness check, we also estimate the models with self-employed women included. The total number of respondents in the collected sample (excluding self-employed women) is 11,423 although there are some missing observations for some of the variables. As the policy was implemented in 2011, the 2006 and 2009 surveys represent the pre-policy periods, while the 2012 and 2015 surveys represent the post-policy period.

The data contain little retrospective information on wages, though fertility history is reported extensively. As shown in Table 1 (Panel A), the 2009 survey was conducted in June 2009 and contains information on average wages during the three months prior to the survey from April 2009 to June 2009. However, the survey contains fertility information on conceptions and births from January 2007 to June 2009. The other surveys have a similar chronological gap.

We employ conception as our primary fertility variable. The period of analysis is

[8] The Korean labor market is segmented between workers in the formal and informal sectors (Kim and Cho, 2009; Lee, 2018). Not only are women in the informal sector ineligible for benefits, but there is also little movement between the two. Moreover, the formal sector is strongly preferred to the informal sector given that it provides more stability, higher wages, and access to social services.

one calendar year before the survey date in which wages were measured (see Panel B). Throughout the duration of the paper, we use the years of conception in Table 1 of 2005, 2008, 2011, and 2014 to refer to the survey years. We use the one year window for conceptions to ensure that the birth roughly corresponds to that time at which wages were measured. Wages at the time of birth are the best predictors of benefits. However, this does raise a potential endogeneity issue that women might alter their labor supply behavior after the conception but before the birth so as to maximize their childcare benefits. To address this issue, we will employ a measure of predicted exogeneous earnings as our running variable. Ostensibly, this variable cannot be manipulated as it depends on predetermined characteristics.

We report descriptive statistics in Table 2 separated by treatment status and pre- and post-policy periods. We have one comparison group and one treatment group. The comparison group is all women with predicted monthly earnings less than 1250 USD. The treatment group is women whose predicted earnings are more than 1250 USD. We do not employ the 2500 USD threshold in our design since the predicted earnings variable never exceeds it (see the next section). We report information on conception, contraception, and regular employment which are the main dependent variables in this study. We also report statistics on age, the number of children, education, monthly wages, husband's wages, household income, and *potential* subsidies (defined as the subsidy that she would earn if she claimed the benefit).

Some clarification is needed on the regular employment variable. Our sample is restricted to employed women but excluding those who are self-employed. Accordingly, we restrict the sample to women whose employment status was in one of three categories: regular employment, temporary employment, and casual workers. The last two categories are temporary, whereas the first is permanent. Given this, the share of regular workers reported in Table 2 is the percentage of women who are regularly employed and its complement is the percentage who are temporarily employed.

Information on potential subsidies is reported at the bottom of the table. This is the monthly subsidy for which the woman is eligible should she have a baby. Prior to 2011, these subsidies are 500 USD for everyone. Importantly, we defined potential subsidies based on *actual* income but the treatment groups were based on *predicted* income to help mitigate numerous endogeneity concerns. Because of this, the average potential subsidy for the comparison group in the post period is 602.48 USD - not 500 USD. The average potential subsidy for the treatment group is 768.95 USD.

The sample sizes for each treatment arm are given at the bottom of the table. These sum to 11,423. However, because there are missing observations for many of the variables, our estimations will typically have fewer observations.

4 Methods

We obtain core estimates from a difference-in-difference (DD) design that exploits how potential childcare leave benefits vary with the respondent's wage both before and after the policy reform (see Figure 1). DD designs are now quite standard in this literature having been employed by Milligan (2005), Cohen et al. (2013), and Raute (2019). Because women are only eligible for subsidies if they have children, these estimates are intentions to treat.

An important feature of our research design is that we use predicted earnings rather than actual earnings as a running variable as the latter could potentially be manipulated by women to increase their benefits. To address this, we regress monthly earnings onto a fully saturated model with a college education indicator, an urban indicator, and a quadratic in age. The independent variables in this estimation are all plausibly exogenous. The R^2 of this estimation is 12.58%.

We present kernel density estimates of predicted earnings in Figure 2 for women with and without college degrees. First, we see that predicted earnings never exceed the 2500

USD threshold and so our design can only identify the margin from below to above the 1250 USD threshold. Second, the density for women without college degrees never exceeds 1250 USD. This implies that our estimated effects are only applicable to college educated women and that we cannot estimate heterogeneous treatment effects by education.

Our DD estimates are based on the the canonical 2 × 2 difference-in-difference specification:

$$Y_{it} = \alpha + \pi POST_t + \phi TREAT_i + \beta \times POST_t \times TREAT_i + X_{it}\theta + \upsilon_{it} \tag{1}$$

where the variable $POST_t$ is a dummy for the post-policy years of 2011 and 2014, $TREAT_i \equiv 1(\text{Pred Income} \geq 1250)$, and X_{it} is a vector of pre-determined variables including a quadratic function of age, an indicator for having a college degree, indicators for the number of children, the husband's monthly wage, and household income. The income variable inside the indicator function is the predicted wife's monthly earnings. We also estimate an event analysis version of equation (1) to test for parallel trends.[9] The DD parameter is β and it delivers the average treatment effect on the treated.

Importantly, the policy was announced in November 2010 and the pre-period in our data come from 2005 and 2008 recall periods - well before the announcement date. This is strongly suggestive that the behavior in our data during the pre-period was not influenced by anticipation of the policy. This lends credence to the "no anticipation assumption" that is required in DD research designs (Wooldridge, 2021).

Next, we supplement our DD estimations with regression kink (RK) estimates. These estimates are also difference-in-difference estimates as they exploit variation in the policy over time. Importantly, these estimates identify a different policy-relevant parameter. Specifically, these estimations are useful for gaining insights into the behavioral responses

[9] Raute (2019) exploits a similarly kinked policy function to implement a DD design. See Panel A of Figure 1 of that paper. Also, note that she used earnings prior to giving birth as a running variable determining treatment status.

at the thresholds. It is important to note that because we have survey data with a small sample size relative to many administrative data sets used in this literature, *e.g* Raute (2019) and González and Trommlerová (2021), these estimates can be imprecise. For this reason, we use these estimations to supplement the analysis, but they do not constitute the core estimations in this paper.

The RK estimates are obtained by estimating the regression model:

$$\begin{aligned} Y_{it} = \kappa + \xi \text{Income}_{it} + \tau_{1250}\text{Income}_{it} \times 1(\text{Income} \geq 1250) + \tau_{2500}\text{Income}_{it} \times 1(\text{Income} \geq 2500) \\ + \rho POST_t + \psi \text{Income}_{it} \times POST_t + \Delta_{1250}\text{Income}_{it} \times 1(\text{Income} \geq 1250) \times POST_t \\ + \Delta_{2500}\text{Income}_{it} \times 1(\text{Income} \geq 2500) \times POST_t + X_{it}\lambda + \varepsilon_{it} \end{aligned} \quad (2)$$

where all variables are defined as they were in equation (1). However, for this exercise, we use actual earnings of the wife rather than predicted earnings. The design includes separate linear functions for monthly income less than 1250 US, between 1250 and 2500 USD, and greater than 2500 USD. We allow this piece-wise linear function to vary in the pre- and post-policy periods. Identification of causal effects is achieved by variation in this relationship between the pre- and the post-periods.[10] So, this design combines both an RK and a DD strategy. Accordingly, the RK estimates are given by Δ_{1250} and Δ_{2500}. As before, we compute robust standard errors.

The RK parameters identify the local average response parameter from Altonji and Matzkin (2005) or (equivalently) the treatment-on-the-treated parameter from Florens et al. (2008). For example, Δ_{1250} delivers the marginal impact of a dollar of income on outcomes at the USD 1250 threshold. Because the marginal subsidy at this threshold is

[10] The validity of the RK design relies upon variation in female earnings essentially being "random" in a small neighborhood of the kinks and, related, lack of manipulation of the running variable. Manipulation is highly unlikely in our scenario since we employ a fertility survey in which the responses to the earnings questions have no relevance to amount of childcare subsidies the women receives. Actual subsidies depend on administrative tax information which is not employed in this study. Finally, any manipulation of the running variable would have to occur in a very small neighborhood of the thresholds which we view as highly unlikely.

0.40 USD, we can divide Δ_{1250} by 0.40 to obtain the marginal impact of *eligibility* for one dollar of childcare subsidies on key outcomes (Card et al., 2015). The interpretation of Δ_{2500} is similar except that the (potential) marginal change in childcare subsidies is -0.40 USD. Finally, it is important to bear in mind that women are eligible for the monthly subsidies for up to 12 months. Accordingly, we also divide the parameters Δ_{1250} and Δ_{2500} by 12×0.40 to deliver a more accurate annualized treatment parameter.

5 Results

5.1 Selection Tests

We begin by testing for selection into the treatment groups in the post-policy period. To do this, we estimate a variant of equation (1) in which we replace the dependent variable with either education, household income, urban status, or an indicator for the husband working. The results are reported in Table 3.

These results do indicate that observable variables are associated with changes in treatment status. For example, we see a positive association with education and a negative association with household income, urban status, and the husband's work status. Note, however, only education and household income are significantly associated with conception with the both having a positive association.[11] We also highlight that the DD estimate in column one for education is positive and is negative in column two for earnings. This suggests that any potential bias is not systematic. Nevertheless, because of the possibility of any bias, we report estimations both with and without these additional controls.

[11] These results are not reported.

5.2 Core Estimates

We report the core DD estimates in Table 4. We use three outcomes: conception, contraception, and regular employment. For each outcome, we estimate the model twice with two sets of controls. The first is parsimonious - a quadratic function of age and indicators for the number of children. The second set further includes controls for education and both the husband's employment status and the household's income to address the potential selection concerns raised in the previous subsection.[12]

First and most importantly, there is evidence that the childcare subsidies increased conception in the first two columns. Both DD estimates are in the range of 2.3 to 2.5 PP and are significant at the 95% level. Noting that the mean of the potential subsidies in Table 2 for the treatment group in the post period is USD 768.95, we obtain an arc elasticity of 0.65 indicating that a one USD increase in potential subsidies raises the probability of conception by 0.65%.[13]

Second, we see evidence that the subsidies decreased contraception in columns three and four. Both estimates indicate that contraception declined by between 3.4 to 3.6 PP and are significant at the 99% level. The implied arc elasticity of contraception with respect to potential childcare subsidies is -0.10%.[14]

Third, we estimate the effects of the subsidies on regular employment in columns five and six. While the estimate in the fifth column is negative and significant, the estimate in the final column with the additional controls is not. We view this as evidence that the childcare subsidies had no systematic impacts on labor supply decisions.

In Table A1, we estimate the same models as in Table 4 but we also include self-employed women in these estimations. We prefer the estimates excluding self-employed

[12] While we agree that some of these controls can be viewed as endogenous, the estimates in the odd numbered columns do not suffer from this issue. On the whole, the results are not meaningfully impacted. We report both sets as some readers may prefer some of the additional controls.

[13] The calculation for this is $\frac{0.025 \times 634.48}{0.0905 \times 268.95}$.

[14] The calculation for this is $\frac{-0.034 \times 634.48}{0.844 \times 268.95}$.

women as the self-employed are not eligible for the subsidies. However, this is an important robustness check as there could be measurement error in the employment status variable resulting in some self-employed women (as reported to the survey enumerator) being eligible for the subsidies. These results echo those from Table 4: conceptions increase, contraception use declines, and null effects on regular employment.

Finally, in Figure 3, we report the results of an event study specification. We normalized the interactions with the 2008 dummy to be zero which is the survey year that immediately precedes the implementation of the policy. All of the interactions of the thresholds with the 2005 dummy are not statistically different from zero which suggests the absence of pre-trends.

5.3 Estimates by Parity

Did the policy have larger impacts at the extensive (first births) or the intensive margins (later births)? In Tables A2 (conception), A3 (contraception), and A4 (regular employment), we report the same estimations while disaggregating by birth parity. Looking at Table A2, we see that the largest point-estimates of the DD estimate are for childless women. These range between 7.2 and 8.2 PP. However, they are not precisely estimated due to smaller sample sizes. There are no significant impacts on women with one child. There are significant impacts on women with two or more children that range between 1.8 and 1.9 PP but these have smaller magnitudes than the estimates for childless women. This indicates substantially larger impacts at the extensive margin. However, the fact that there were effects for women with at least two children indicates that the policy most likely increased completed fertility rather than just timing of births. Finally, there are no significant effects on contraception in Table A3 and, while there are significant effects on regular employment in Table A4, the signs of the estimates are not consistent across parities.

5.4 Regression Kink Estimates

We report the regression kink estimates of equation (2) in Table 5. First, the marginal dollar increased conception at the 1250 USD threshold but there were no statistically significant impacts at the 2500 USD threshold. The point estimates of Δ_{1250} in the first two columns are 0.0031 and 0.0040 indicating that the marginal dollar increased conception at this threshold. These estimates are significant at the 90 and 95% levels, respectively. On an annual basis, the implied marginal effects are 0.00065 and 0.00083. The numbers indicate that the *potential* marginal dollar of childcare subsidies per month (on an annual basis) increases conception by between 0.065 and 0.083 PP.[15] Second, the marginal dollar of earnings increased contraception at the 2500 USD threshold but had no impacts at the 1250 USD threshold. The estimates of Δ_{2500} are 0.0042 and 0.0044 in columns three and four. Both estimates are significant at the 90% level. This implies that taking away a marginal dollar of childcare subsidies *increases* contraception use. On an annual basis, the marginal effects are -0.00088 and -0.00092. The annualized numbers indicate about a 0.1 PP decrease in contraception use at this threshold in response to one addition USD of subsidy per month. Third, we do not see consistent evidence that childcare subsidies increased regular employment in the final two columns. In column six, we see that the estimate of Δ_{1250} is 0.0095 and significant at the 95% level, but none of the other three estimates are significant.

6 Conclusions

In this paper, we estimated the effects of a pro-natal policy in Korea that provided paid childcare leave to working mothers. We employed a difference-in-difference design and used multiple waves of a fertility survey containing information on conception, contraception, and employment status. We supplemented the analysis with a regression kink

[15]Recall that the caclulation is $\frac{\Delta}{12\times0.4}$.

design.

We find that the policy had large effects on maternal behavior. Difference-in-difference estimates imply that the arc elasticity of the response of conception to the childcare subsidy is 0.65. While the effects of the policy were highest at the extensive margin, there is also suggestive evidence that it increased completed fertility. We also show that childcare subsidies reduced contraception use. The implied arc elasticity of contraception use with respect to one dollar of subsidy is -0.10. Finally, we do not find evidence that the childcare subsidies induced a movement towards permanent working arrangements.

One limitation of this paper is that we cannot look at the effects of the policy on abortions as was done by González and Trommlerová (2021). They showed that child subsidies in Spain resulted in fewer abortions. Abortion was, in fact, illegal (albeit somewhat common) in Korea until 2021. That said, there is some evidence from a survey conducted by the Korea Institute for Health and Korean Affairs suggesting that abortion did become less common after the policy was implemented.[16] The survey indicates that, among women ages 15-44, 2.98% of women had an abortion in 2005, (342,433 estimated cases), 1.58% had an abortion in 2010 (168,738 estimates cases), 0.48% did so in 2017 (49,764 estimated cases). So, while we do provide solid evidence that reduced contraception was a mechanism generating higher fertility, we cannot rule out that abortion was also another important channel.

All told, we conclude that the policy generally worked as intended. It had large impacts on fertility behavior, ostensibly because it provided an environment that was more amenable to working mothers. This is an important outcome in a country that often performs middling in rankings of gender inequality (UNData, 2021) and, thus, constitutes a step in the right direction.

[16] The report (in Korean) is available here: https://www.kihasa.re.kr/news/press/view?seq=2063.

Declarations

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Table 1: Time of Survey and Period of Analysis

Panel A: Time of Survey

	Date surveyed	Fertility history (Conceptions, Births)	Wage (Avg. 3 months prior to the survey)
Survey 2006	Jun.2006	Lifetime	Apr.-Jun.2006
Survey 2009	Jun.2009	Jan.2007-Jun.2009	Apr.-Jun.2009
Survey 2012	Apr.2012	Jan.2010-Apr.2012	Feb.-Apr.2012
Survey 2015	Aug.2015	Lifetime	Jun.-Aug.2015

Panel B: Period of Analysis

	Conceptions	Wage
Survey 2006	2005	Apr.-Jun.2006
Survey 2009	2008	Apr.-Jun.2009
Survey 2012	2011	Feb.-Apr.2012
Survey 2015	2014	Jun.-Aug.2015

Source: the National Fertility and Family Health Survey

Table 2: Summary Statistics

	Comparison Predicted Income < 1250		Treatment Predicted Income ≥ 1250	
	2005 & 2008	2011 & 2014	2005 & 2008	2011 & 2014
Conception	0.022	0.017	0.089	0.092
	(0.15)	(0.13)	(0.29)	(0.29)
Contraception	0.911	0.907	0.847	0.840
	(0.29)	(0.29)	(0.36)	(0.37)
Regular Employment	0.443	0.497	0.741	0.742
	(0.50)	(0.50)	(0.44)	(0.44)
Age	40.247	41.825	35.808	38.369
	(5.98)	(5.64)	(6.35)	(6.03)
Number of Children	1.847	1.868	1.371	1.520
	(0.75)	(0.78)	(0.91)	(0.86)
College Education	0.000	0.000	0.994	0.999
	(0.00)	(0.00)	(0.08)	(0.03)
Monthly Wages	1088.960	1384.543	1920.056	2208.096
	(659.87)	(740.66)	(1162.96)	(1276.14)
Husband's Monthly Wages	2107.102	2736.850	2938.133	3611.251
	(1162.61)	(1368.82)	(1638.80)	(1786.45)
Household Income	3284.025	4393.259	4799.682	6068.554
	(1449.70)	(1709.45)	(2292.80)	(3040.90)
Potential Subsidy	500.000	602.484	500.000	768.954
	(0.00)	(148.03)	(0.00)	(206.14)
N	2653	2940	1680	4149

Notes: Calculated by the authors using the National Fertility and Family Health Survey. We display means and standard deviations (in parentheses).

Table 3: Selection Tests

	(1)	(2)	(3)	(4)
	Education	HH Income (log)	Urban Status	Husband Work
Treatment × POST	0.0030*	–0.073***	–0.068***	–0.037***
	(0.0016)	(0.019)	(0.017)	(0.013)
One Child	–0.0042*	0.047***	–0.0078	0.040**
	(0.0025)	(0.017)	(0.015)	(0.016)
Two or More Children	–0.0064***	0.10***	–0.029**	0.056***
	(0.0024)	(0.016)	(0.015)	(0.015)
N	11,419	11,418	11,421	11,421
R^2	1.0	0.19	0.052	0.020
Pre-period Mean	0.99	6.1	0.80	0.92

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. All estimations include a quadratic in age. Treatment is equal to one if predicted (log) earnings exceeded 1250 US/month.

Table 4: Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Conception		Contraception		Regular Employment	
Treatment × POST	0.023**	0.025**	–0.036***	–0.034***	–0.045**	–0.024
	(0.011)	(0.011)	(0.013)	(0.013)	(0.021)	(0.021)
One Child	–0.12***	–0.12***	0.36***	0.35***	–0.076***	–0.085***
	(0.018)	(0.018)	(0.021)	(0.021)	(0.019)	(0.018)
Two or More Children	–0.15***	–0.15***	0.52***	0.51***	–0.13***	–0.15***
	(0.016)	(0.016)	(0.019)	(0.020)	(0.018)	(0.017)
N	11,421	11,416	10,630	10,627	11,421	11,416
R^2	0.11	0.11	0.24	0.24	0.088	0.14
Pre-period Mean	0.089	0.089	0.85	0.85	0.74	0.74
Controls		X		X		X

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Control variables include a set of dummy variables for employment status, region, husband's employment status, log monthly wage, log household income, and education. All estimations include a quadratic in age. Treatment is equal to one if predicted (log) earnings exceeded 1250 US/month.

Table 5: Regression Kink Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
	Conception		Contraception		Regular Employment	
Income	–0.0014	–0.0028	–3.0e-06	–0.0014	0.069***	0.065***
	(0.0055)	(0.0055)	(0.0053)	(0.0052)	(0.017)	(0.017)
Income × POST	–0.013	–0.017	–0.024**	–0.027**	0.074*	0.064
	(0.015)	(0.015)	(0.012)	(0.012)	(0.043)	(0.041)
Income $\geq$ USD 1250 × POST	0.0031*	0.0040**	–0.0020	–0.0016	0.0071	0.0095**
	(0.0017)	(0.0017)	(0.0021)	(0.0021)	(0.0048)	(0.0047)
Income $\geq$ USD 2500 × POST	0.0038	0.0040	0.0042*	0.0044*	–0.0022	–0.0011
	(0.0025)	(0.0025)	(0.0024)	(0.0024)	(0.0040)	(0.0039)
One Child	–0.12***	–0.12***	0.35***	0.35***	–0.056***	–0.054***
	(0.018)	(0.017)	(0.021)	(0.021)	(0.017)	(0.017)
Two or More Children	–0.15***	–0.15***	0.52***	0.51***	–0.084***	–0.082***
	(0.016)	(0.016)	(0.019)	(0.020)	(0.016)	(0.016)
N	11,421	11,416	10,630	10,627	11,421	11,416
R^2	0.11	0.12	0.24	0.24	0.26	0.26
Controls		X		X		X

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Control variables include a set of dummy variables for employment status, region, husband's employment status, log monthly wage, log household income, and education. All estimations include a quadratic in age.

Figure 1: Childcare Leave Benefits as a Function of Earnings

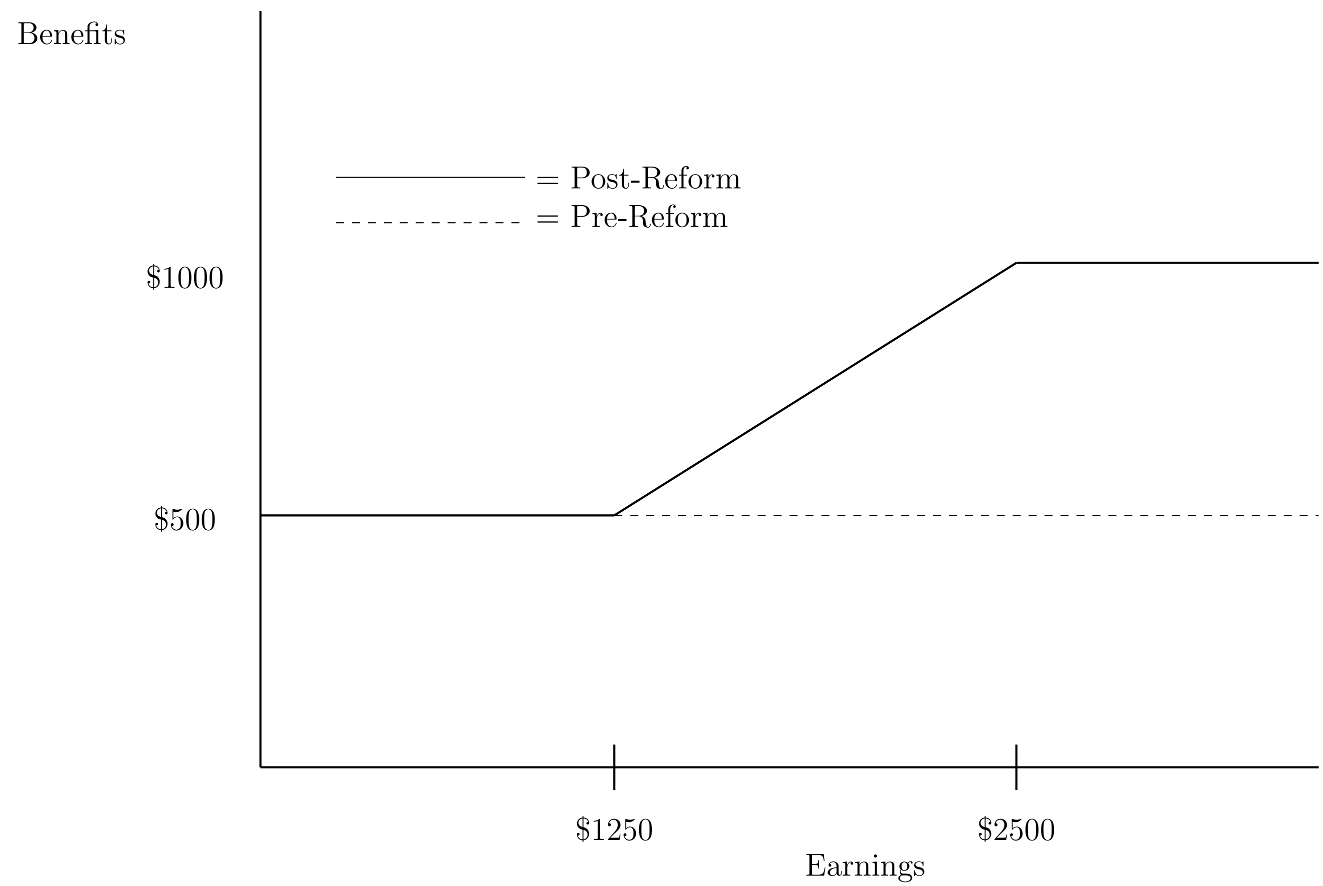
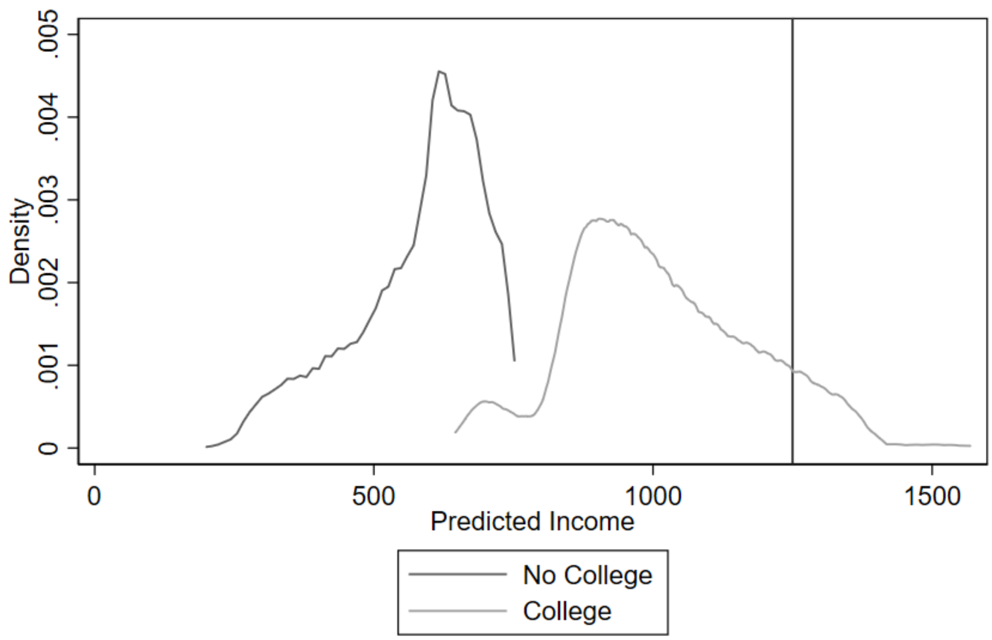
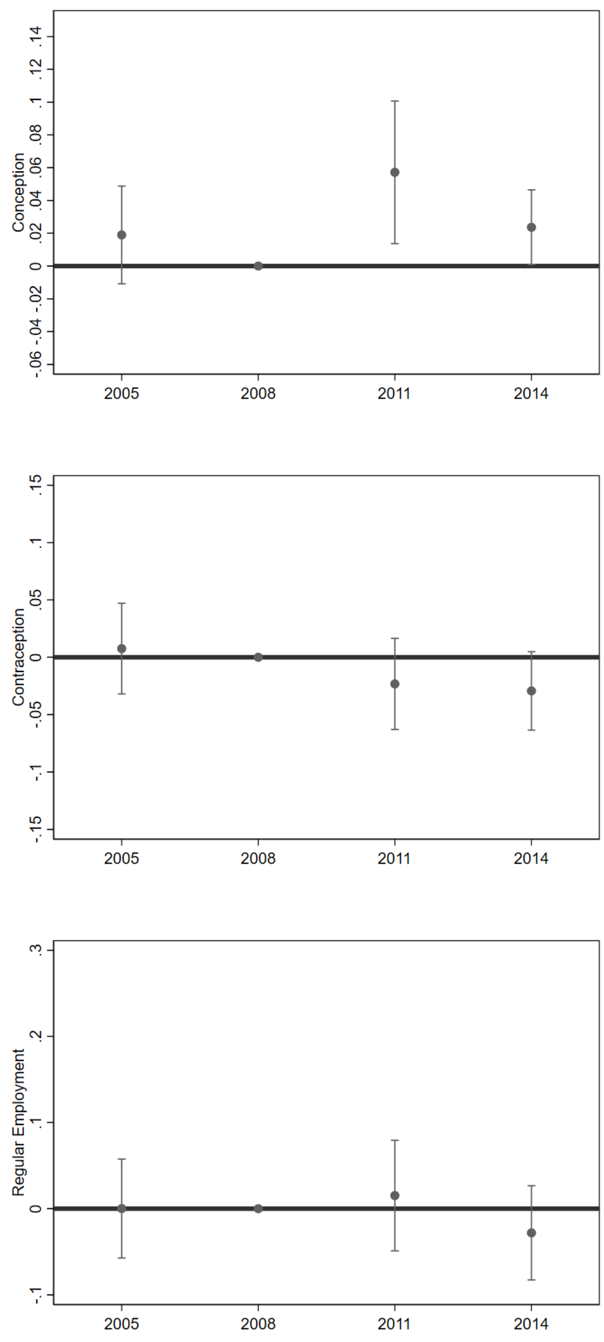


Figure 2: Predicted Income Densities



Notes: Displays the densities of predicted earnings by education status. The vertical line is 1250 USD.

Figure 3: Event Analyses



Notes: Each panel displays interactions between period dummies and treatment dummy. All estimations include the set of controls from the even columns of Table 4.

Table A1: Difference-in-Difference Estimates: Includes Self-employed Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Conception		Contraception		Regular Employment	
Treatment × POST	0.021**	0.023**	–0.038***	–0.036***	–0.032	–0.012
	(0.0096)	(0.0097)	(0.012)	(0.012)	(0.020)	(0.020)
One Child	–0.11***	–0.11***	0.35***	0.35***	–0.078***	–0.088***
	(0.016)	(0.016)	(0.020)	(0.020)	(0.019)	(0.018)
Two or More Children	–0.14***	–0.14***	0.52***	0.52***	–0.12***	–0.14***
	(0.015)	(0.014)	(0.018)	(0.018)	(0.018)	(0.017)
N	13,165	13,158	12,271	12,266	13,165	13,158
R^2	0.11	0.11	0.24	0.24	0.071	0.11
Pre-period Mean	0.080	0.080	0.86	0.86	0.63	0.63
Controls		X		X		X

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Control variables include a set of dummy variables for employment status, region, husband's employment status, log monthly wage, log household income, and education. All estimations include a quadratic in age. Treatment is equal to one if predicted (log) earnings exceeded 1250 US/month.

Table A2: Effects of Childcare Subsidies on Conception: Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		No Children		One Child		Two or More Children	
Treatment × POST	0.023**	0.025**	0.072	0.082*	–0.015	–0.015	0.018***	0.019***
	(0.011)	(0.011)	(0.049)	(0.050)	(0.035)	(0.035)	(0.0069)	(0.0071)
One Child	–0.12***	–0.12***						
	(0.018)	(0.018)						
Two or More Children	–0.15***	–0.15***						
	(0.016)	(0.016)						
Controls		X		X		X		X
N	11,421	11,416	1,419	1,417	2,203	2,202	7,800	7,798
R^2	0.11	0.11	0.046	0.063	0.043	0.043	0.048	0.049

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Control variables include a set of dummy variables for employment status, region, husband's employment status, log monthly wage, log household income, and education. All estimations include a quadratic in age. Treatment is equal to one if predicted (log) earnings exceeded 1250 US/month.

Table A3: Effects of Childcare Subsidies on Contraception: Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		No Children		One Child		Two or More Children	
Treatment × POST	–0.036***	–0.034***	–0.010	–0.018	–0.063	–0.065	–0.014	–0.0099
	(0.013)	(0.013)	(0.076)	(0.076)	(0.040)	(0.040)	(0.010)	(0.010)
One Child	0.36***	0.35***						
	(0.021)	(0.021)						
Two or More Children	0.52***	0.51***						
	(0.019)	(0.020)						
Controls		X		X		X		X
N	10,630	10,627	1,127	1,127	1,991	1,990	7,513	7,511
R^2	0.24	0.24	0.027	0.032	0.012	0.012	0.0023	0.0083

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Control variables include a set of dummy variables for employment status, region, husband's employment status, log monthly wage, log household income, and education. All estimations include a quadratic in age. Treatment is equal to one if predicted (log) earnings exceeded 1250 US/month.

Table A4: Effects of Childcare Subsidies on Regular Employment: Difference-in-Difference Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		No Children		One Child		Two or More Children	
Treatment × POST	–0.045**	–0.024	–0.10*	–0.074	0.13***	0.15***	–0.079***	–0.053**
	(0.021)	(0.021)	(0.061)	(0.059)	(0.050)	(0.046)	(0.027)	(0.026)
One Child	–0.076***	–0.085***						
	(0.019)	(0.018)						
Two or More Children	–0.13***	–0.15***						
	(0.018)	(0.017)						
Controls		X		X		X		X
N	11,421	11,416	1,419	1,417	2,203	2,202	7,800	7,798
R^2	0.088	0.14	0.058	0.12	0.089	0.18	0.069	0.12

Notes: Robust standard errors in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent level. Control variables include a set of dummy variables for employment status, region, husband's employment status, log monthly wage, log household income, and education. All estimations include a quadratic in age. Treatment is equal to one if predicted (log) earnings exceeded 1250 US/month.