The Effects of Subsidies for Childbearing on Migration and Fertility: Evidence from Korea

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Abstract – This study examines the effects of a fertility subsidy that was instituted in specific regions in Korea in 2004. The value of the subsidy ranged from $4,000 to $9,000 (2004 US$) depending on area of residence. Using a difference-in-differences estimation strategy, we measure the effects of the policy on migration in and out of subsidized areas, childbearing, and the sex ratio of newborns. Our estimates for migration suggest that the policy significantly increased the net inflow of females into subsidized areas. This effect is driven roughly equally by a decrease in the outflow from and an increase into the subsidized areas. Our estimated effects on fertility are in line with previous results from the literature; we find that a $1,000 increase in fertility subsidies leads to a 0.108% increase in the chance of becoming pregnant for all age groups (21 to 45), and that the policy increased total births in 2005 by 11,000. We find no effect of the subsidies on the sex ratio of children.

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I. Introduction

This study examines the effects of a policy implemented in Korea in 2004 to subsidize fertility in specific areas. We measure the effects of the subsidy on migration in and out of affected areas, on overall fertility, and on the sex ratio of children. Subsidies and tax incentives for fertility are common worldwide both in developed and in developing countries. The structure of the policy examined here provides a unique opportunity to estimate how such financial incentives for fertility affect couples' behavior.

Many studies have examined the effects of financial incentives on fertility (e.g. Whittington et al. (1990), Kearney (2004), Milligan (2005), Baughman and Dickert-Conlin (2006); Moffitt (1998) provides a useful review). The literature finds mixed results, with elasticities of fertility ranging from -0.022 to 0.248.[1] This study improves upon previous research in four primary ways. First, we are the only study to our knowledge that examines financial incentive effects on both fertility and migration. Second, our estimates are applicable for an Asian country.[2] Third, the recent policy change is relatively large in value and focused on child care which has not been adequately examined in the literature.[3] Lastly, we use a unique identification strategy controlling for migration which has not been previously addressed in the literature.

[1] Whittington et al. (1990) estimates the effect of the dependent tax exemption on fertility and finds elasticities between 0.127 and 0.248. Kearney (2004) finds a point estimate of 0.002, rejecting the hypothesis that a family cap leads to a decline in births of one percent or more. Milligan (2005) estimates the effect of a child subsidy in the province of Quebec and finds an elasticity of 0.107. Baughman and Dickert-Conlin (2006) analyze the effect that the EITC has on fertility rates and finds an elasticity of -.022 for higher level births for white women and an elasticity of -.009 for childless women.
[2] There have been very few studies related to fertility responses to government policies in Asia. Those that have been written have mostly focused on China's one-child policy. See Ahn (1994), Hesketh et al. (2005), Li et al. (2005), and McElroy et al. (2000) for a review of this literature.
[3] Notable studies which examine the effect of child care costs and availability on fertility include Andersson et al. (2004), Blau and Robbins (1989), Del Boca (2002), Hank and Kreyenfeld (2003), and Mason and Kuhlthau (1992).

The fertility subsidies examined in this study were implemented throughout three cities (Seoul, Incheon, and Daejeon) and one county (Sancheong) in the first six months of 2004. In three of the areas, the subsidies were given in the form of a reimbursement payment for childcare costs. The fourth area's (Incheon) subsidy was simply a cash payment with no requirements other than bearing an additional child. To be eligible for the benefit, a household had to already have at least two children, plus a child born after the implementation of the policy. The present value of the subsidy ranges from $4,000 to $9,000 (2004 US$) depending on area of residence.[4] Figure 1 outlines the timing of the policies and Table 1 displays the cash benefits by area.[5]

We use the recent policy change to identify the effect of financial incentives on fertility, migration, and the sex ratio of children. A difference-in-differences research design using a probit model is implemented to isolate these effects. Our primary analysis focuses on the change in fertility rates induced by the policy. Korean census data are used to compare the difference in fertility rates between treatment areas (Seoul, Incheon, Daejeon, and Sancheong) and control areas (all other Korean cities and counties) before the policy change with the difference in fertility rates between treatment and control areas after the policy change. In addition, we use an instrumental variable approach to address possible bias caused by migration between treatment and control areas.

We find an elasticity of fertility equal to 0.031. This is in line with previous estimates in the literature. Our results suggest that the policy led to an increase of 11,000 births in the subsidized areas in 2005. Our migration results suggest that the policy

[4] We use a discount rate of 7% as suggested in Gruber (2007).

[5] During this same time period, several other local governments began to subsidize fertility as well. Table 1 describes all of the known subsidies by their area and amount. The report is rather vague in regards to the benefits paid out in the city of Seoul. For our estimates, we assume Seoul has a benefit equal to $5,410 (2004 US$). This value is equal to the average benefit paid out to the areas of Incheon, Daejeon, and Sancheong.

generated a significant increase in the net inflow of females into subsidized areas. This effect was driven roughly equally by a decrease in the outflow from the subsidized areas as well as an increase in the inflow from the non-subsidized areas. We find no effect of fertility subsidies on the sex ratio of children.

The remainder of the paper proceeds as follows. Section II outlines our identification strategy. Section III describes the data used in our estimations. Section IV presents our results. Section V concludes.

II. Identification Strategy

We use the recent policy change in Seoul, Incheon, Daejeon, and Sancheong as our primary means of identification in a difference-in-differences framework. The following probit model is used to estimate the effect that the subsidies had on fertility rates:

$$\text{Prob}(Y_{ikt}) = \Phi(\beta_0 + Z'_{ikt}\beta_1 + \beta_2\text{Post}_t + \beta_3\text{Benefit}_k + \beta_4(\text{Post*Benefit})_{kt}) \qquad (1)$$

Where, Y is a dummy variable equal to one if female *i* bore a child in time period *t* in area *k*.[6] Z denotes a vector of socio-demographic and economic variables. Post is a dummy variable equal to one if female *i* is from the period after the policy change. Benefit is the

[6] This study uses survey data from a survey conducted in November of 2000 and November of 2005. Benefits were given to only those individuals in treatment areas having a third or higher child. Therefore, Y_{ikt} takes a value of one if a female has a third or further child who is less than two years old as of November 1st of the respective survey year. Y_{ikt} takes a value of 0 if the female has two children, with the youngest child at least one year old, or if the female has at least three children, with the youngest child at least two years old as of November 1st of the respective survey year.

U.S. dollar value (in $1,000's) of the fertility subsidy given to eligible females in the four treatment areas. β_4 is the coefficient of interest. This can be interpreted as the increase in the probability of becoming pregnant due to a $1,000 increase in benefits.

A standard difference-in-differences estimator typically has several assumptions that need to be met in order to estimate unbiased coefficients. First, the trends of the treatment and control groups need to follow the same trend line. If the treatment and control groups have differential trends, then the estimates will be biased. The amount of the bias will increase as the time lapse increases between pre and post periods. The fertility trends of the primarily affected age cohort (females aged 21 to 33) in both the treatment and control areas are shown in Figure 2. Notice that the trends for the treatment and control groups follow the same path leading up to the implementation of the policy. Therefore, this assumption appears not to be violated.

The second assumption is that, besides the subsidy being analyzed, there are no other mitigating factors causing the change in fertility rates between the treatment and control areas. To our knowledge, there are no other factors or policy changes taking place during this time period which would bias our results.

The last assumption is that the control areas are not affected by the policy change. There are four ways in which this last assumption might be flawed. (1) The chance exists that individuals might migrate from control areas to treated areas to take advantage of the fertility subsidy. If these individuals bear children quickly in order to take advantage of the subsidy, then the OLS estimates will be biased upwards. (2) If the individuals who otherwise would have moved are staying and immediately taking advantage of the subsidy, then the difference-in-differences estimator will tend to overestimate the true effect of the policy. (3) If individuals migrate from control areas to treatment areas and plan to take advantage of the subsidy in the distant future (but do not bear children soon), then the OLS estimates will be biased downwards. (4) If the individuals who otherwise would have moved stay, and plan to take advantage of the subsidy in the distant future, then the OLS estimates will tend to underestimate the true effect of the policy. Since it is not clear which of the four possibilities might be taking place, the effect of the bias is ambiguous.

In order to determine if the subsidy caused an overall change in the migration patterns of females, we estimate the following equation:

$$\text{Net Inflow}_{kt} = \beta_0 + \beta_1\text{Post}_t + \beta_2\text{Treat}_k + \beta_3(\text{Post*Treat})_{kt} + \varepsilon_{kt} \qquad (2)$$

Where, Net Inflow_{kt} equals the total number of individuals immigrating to area *k* minus the number of individuals migrating out of that area in time period *t* divided by the total number of residents in that area. Post is a dummy variable equal to one if the time period is after the policy implementation. Treat is a dummy variable equal to one for any of the four areas where the policy has been implemented. The coefficient of interest is β_3, which can be interpreted as the percentage increase in the net inflow of individuals into treatment areas due to the policy change.

We find endogenous migration in and out of treatment areas. Therefore, the coefficients for Benefit and Post*Benefit in Equation (1) are expected to be biased due to the migration of females. To correct for this, we re-estimate Equation (1) using two-stage least squares. The lagged values of Treatment Status and the lagged values of Treatment

Status interacted with Post are used as instrumental variables for both Benefit and the interaction of Benefit and Post in the first stage. The two equations estimated in the first stage are shown below:

$$\text{Benefit}_{ikt} = \beta_0 + Z'_{ikt}\beta_1 + \beta_2\text{Post}_t + \beta_3\text{LTreat}_{ikt} + \beta_4(\text{Post*LTreat})_{ikt} + \varepsilon_{ikt} \quad (3)$$

$$\text{Post*Benefit}_{ikt} = \beta_0 + Z'_{ikt}\beta_1 + \beta_2\text{Post}_t + \beta_3\text{LTreat}_{ikt} + \beta_4(\text{Post*LTreat})_{ikt} + \varepsilon_{ikt} \quad (4)$$

Where LTreat is a dummy variable equal to one if female *i* lived in the treatment area five years prior to time period *t*. We assume that, if a female lived in the treated area before the policy implementation, then she is likely to have lived there in the post period (hence, LTreat is correlated with Benefit).[7] In addition, her living in the treated area in the pre period should not affect her fertility. Therefore, LTreat and Post*LTreat should be uncorrelated with Y in Equation (1). These two assumptions satisfy the minimum requirements to obtain consistent estimates using two-stage least squares. The second stage uses the estimates for Benefit and Post*Benefit in Equations (3) and (4) to re-estimate Equation (1).

Lastly, we estimate the effect of the fertility subsidy on the sex ratio of children. Due to the high costs of bearing and raising children, as well as societal norms, Korean couples have one of the lowest birth rates in the world.[8] In Korean society, it is typical for males to take care of their parents in old age. In addition, males are usually the main income earners in the family. If the family is constrained to remain small by the high cost

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[7] Along similar lines, Post*LTreat is correlated with Benefit in Equation (3), and the same is true with Post*Benefit in Equation (4).

[8] South Korea's current fertility rate is 1.20 children born per woman over their lifetime (Central Intelligence Agency, 2008). South Korea is ranked 218 out of 223 countries, giving it one of the lowest fertility rates in the world.

of raising children, it can be assumed (for some couples) that a male birth is more favorable than a female birth. The fertility subsidy examined in this study reduces these costs. Therefore, there should be a reduced emphasis on the importance of bearing a male child (since the couple can afford to bear a male child in the future).

Due to the importance placed on bearing male children in Korean society, a couple might abort a female fetus in favor of a male. However, since the policy reduces childrearing costs, it might reduce the number of aborted female fetuses and increase the number of female births. This would change the sex ratio of children in treatment areas. In order to determine if the fertility subsidy has an effect on the sex ratio of children, we estimate the following equation using two-stage least squares:

$$\text{Sex Ratio}_{ikt} = \beta_0 + Z'_{ikt}\beta_1 + \beta_2\text{Post}_t + \beta_3\text{Benefit}_k + \beta_4(\text{Post*Benefit})_{kt} + \varepsilon_{ikt} \qquad (5)$$

Where, Sex ratio equals the number of male children divided by the total number of children born by female *i* in time period *t* in area *k*. Z denotes a vector of socio-demographic and economic variables. Post is a dummy variable equal to one if the time period is after the policy implementation. Benefit is the U.S. dollar value of the fertility subsidy given to eligible females in the four treatment areas estimated from Equation (3). Post*Benefit is the variable of interest resulting from Equation (4). The coefficient (β_4) for this variable can be interpreted as the change in the sex ratio in treatment areas due to a $1,000 increase in benefits.

III. Data and Variables

This study uses repeated cross-sectional data taken from the Population and Housing Census, whose data are gathered through the use of a survey implemented by the Korean National Statistical Office. The survey is based on information as of November 1st every 5th year (1995, 2000, 2005, etc.). It is based on a two-percent sample of the entire South Korean population. The data files include information on each individual's socio-demographics, family structure, location status, and housing unit characteristics. Summary statistics are presented in Table 2.

A response to the policy is presumably recognized by a new birth in a household with at least two children in the policy areas.[9] We restrict the sample to households where a married female is aged from 21 to 45 and has at least two children, so that households included in this study become eligible for the benefits immediately with a new birth. Households that have only two children, with the younger of the two less than one year old, are excluded. Due to biological constraints, these households cannot respond to the policy immediately, and the inclusion of this type of household in the estimation could bias the effect of the policy.

One unique aspect of Population and Housing data is the ability to track the migration patterns of females. We are able to identify which females moved in and out of the policy areas, both before and after the fertility subsidies went into effect. As

[9] Even though a household with less than two children could have a new birth as a response to the policy, it is assumed that a new birth as a first or a second child is less affected by the policy than a new birth as a third or further child by a household with at least two children. For this reason we focus on the births of children for females having at least two children. Further research could be conducted on the effects of the policy on other birth orders.

discussed previously, our empirical strategy hinges on the ability to track female migration patterns. Other studies have largely ignored the endogenous migration of females in and out of policy areas. The ability to correct for migration bias is one of the major contributions of this paper to the literature.

Control variables are added to the regressions as determinants of fertility. These variables include age, educational attainments, a dummy variable indicating whether a married female is a household head or not, and family income. Age is expected to have a negative effect, since females with at least two children are less likely to give birth as they age. Educational attainments are measured by years of schooling. This variable is expected to have a negative effect on the likelihood of the female to bear a new baby. Females with higher education levels tend to marry at older ages and also receive higher salaries if they work. Relatively larger foregone salaries make their opportunity cost for childbearing higher. When a married female is head of the household, she plays the main role of taking care of family members and is less likely to have an additional birth.

Unfortunately, the Population and Housing Census does not have a variable which directly measures family income. Therefore, as a proxy, we measure family income by the interaction term between a dummy variable indicating whether the household owns its home or not, and the size of the housing unit in square meters. The Korean financial market for housing is underdeveloped, and buying a house with a loan is not as frequent as in advanced countries. Thus, the interaction term is an appropriate measure of family income. Theoretically, the effect of family income is ambiguous. Households with high incomes are likely to be fertile, on the condition that a child is a normal good. However, it has been argued that high-income families may invest more in the quality of each child, so that income may have a negative effect on the number of children.[10]

IV. Results

Table 3 presents Non-IV results with and without controlling for socio-demographic and economic variables for Equation (1) across all age cohorts. The coefficients shown are the marginal probabilities. The coefficient of interest in each of the six columns is the interaction of Post and Benefit, which can be interpreted as the increase in the probability of becoming pregnant due to a $1,000 increase in benefits.

The Non-IV results from Table 3 are largely the same, with or without the inclusion of control variables. This suggests that the fertility subsidy is driving the results and not other variables. The coefficient of interest is positive across all specifications, indicating that the policy had a positive effect on fertility rates. Column 2 shows the overall effect of the policy for all females aged 21 to 45. The point estimate for the coefficient of interest is 0.00044 and significant at the 10% level. Since the average subsidy in treatment areas is $6,200, the Non-IV estimates indicate the policy leads to a 0.273% increase in fertility rates for women aged 21 to 45. As expected, the strongest effect of the policy occurred for women of prime fertility age (21 to 33). The point estimate for the coefficient of interest is 0.0053, with significance at the 1% level for the 21 to 33 age cohort. The least effect of the subsidy occurred for females aged 34 to 45. We find little evidence to suggest the policy affected females in this age cohort. The coefficient of interest is relatively small (0.00001 and insignificant) in comparison to the younger age cohort. It appears the vast proportion of the overall effect on fertility is

[10] See Becker and Lewis (1973).

being driven by the 21 to 33 age cohort.

The control variables from Table 3 provide a number of useful insights. Overall, females are less likely to bear children as they become older. This is expected, especially considering that women's prime fertility years are in their 20's and early 30's. Family income, measured by the interaction term between home ownership and the size of the owned housing unit, has a statistically significant and negative effect on fertility decisions, but not a large one. Becker (1991) suggests that the opportunity cost of bearing children increases as income levels rise. However, couples might substitute quality for quantity as their income level increases, as stated in Becker and Lewis (1973). Therefore, economic theory is ambiguous as to what sign the coefficient for family income should be. Empirically, we find family income has a negative effect on childbearing. The coefficient for household head is statistically significant and negative as expected. The years of schooling variable is statistically significant and positive, indicating that females with more education are more likely to bear children. This is opposite to most findings in the literature.

Table 4 presents the regression results for Equation (2). As Column (1) shows, the availability of the fertility subsidy leads to a significant and positive effect on the net inflow of females into policy areas. The coefficient for the interaction of Post and Treat is 0.08491, which indicates the policy led to an 8.49% increase in the net inflow of females into subsidized areas.

To determine if this effect is being driven primarily by the influx of females moving into treatment areas or a decreased number of migrants moving out of treatment areas, we re-estimate Equation (2) with the dependent variables being Inflow and

Outflow. Inflow is defined as the total number of females immigrating to area *k*, divided by the total population in that area. Outflow is defined as the total number females migrating out of area *k*, divided by the total population in that area. The results for these regressions are shown in Columns (2) and (3) in Table 4. We find that the increase in the net inflow of females is driven roughly equally by a decrease in the outflow from the subsidized areas and an increase in the inflow from non-subsidized areas.

Columns (4), (5), and (6) in Table 4 are shown as a falsification test. If the policy was the primary cause for the change in migration patterns of females from 2000 to 2005, using the same estimation strategy, we should see no similar change in previous years. We use the years 1995 and 2000 as a way to test this hypothesis. The coefficient for the interaction of Post and Treat is insignificant when using the years 1995 and 2000. This is a strong indication that the change in migration patterns of females is being driven by the change in policy.

Two-stage least square results for the effect of the policy on fertility are presented in Table 5. The findings closely resemble those from the non-instrumented estimates. The coefficient of interest (the interaction of Post and Benefit) in all of the specifications is the same sign and significance as those from the non-instrumented results. This provides further evidence that the subsidy has a positive effect on fertility rates.

The two-stage least square estimates show a slightly higher effect from the policy on fertility than the non-instrumented results. There are two explanations for the higher estimates in the two-stage least square results. The first is that the individuals who otherwise would have moved outside the policy areas are now staying, with the intent of using the subsidy in the future. Since they are not utilizing the subsidy in the short-term, this is keeping the true effect of the policy artificially low in our non-instrumented results. The other explanation is that the policy change is causing more people to immigrate to the subsidized areas, people who otherwise would not have in the absence of the policy change. These individuals intend to use the subsidy in the long-term, but since they are not currently utilizing the subsidy, the short-term estimates are biased downwards. In either case, the effect of migration does not change the sign for the coefficient of interest, indicating that the subsidy has a positive effect on fertility rates.

As is the case in the non-instrumented regressions, the two-stage least square results change very little, with or without the addition of control variables. Once again, the largest effect is found with the 21 to 33 age cohort. The estimates for this cohort, provided in Column (4) show a positive effect of the subsidy on fertility rates, at a 1% level of significance. The estimates presented for the 34 to 45 age cohort in Column (6) indicate the fertility subsidy had little or no effect on childbearing for this cohort. The overall effect of the policy on all age groups (21 to 45) is positive and statistically significant at the 10% level. The point estimate for the coefficient of interest is 0.00108 with a 95% confidence interval ranging from -0.00013 to 0.00229. This point estimate indicates that a $1,000 increase in fertility subsidies leads to a 0.108% increase in the chance of becoming pregnant for those aged 21 to 45. This translates into an elasticity of 0.031.[11] The estimated overall effect of the fertility subsidy led an increase of roughly 11,000 births in treatment areas.[12]

We use data from 2000 and 2005 to analyze a policy change in 2004. Thus, the

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[11] Average annual household income for South Koreans was $28,955 US$ in 2004 (Korea National Statistical Office).

[12] This number comes from the regression results with the inclusion of control variables. There were 1,662,800 married females between the ages of 21 and 45 in the policy areas in 2005. Our estimates indicate that the policy caused an increase in the probability of becoming pregnant by 6.2*0.00108 = 0.006696. Therefore, the policy led to an increase in the number of births by 1,662,800*0.006696 = 11,134.

main results presented in Table 5 are short-term in nature. The long-term effects of the policy change are ambiguous in light of the current available data. For instance, it might be the case that females in subsidized areas previously intended to have babies in the future, but the policy change simply moved up their birthing schedule to take advantage of the subsidy. Therefore, the policy did not really affect overall fertility rates in the long-term, but rather just increased the fertility rates in the short-term. Given that the subsidy went into effect in 2004, and that the most recent data we have are from 2005, there is currently no way to test the long-term implications of the subsidy on fertility. Further analysis of the policy should be taken in the coming years when new data sources become available to assess the long-term impact of fertility subsidies.

As a falsification test, we apply two-stage least squares to estimate Equation (1) using different years. We use 1995 as our Pre-policy period and 2000 as our Post-policy period. These results are presented in Table 6. The coefficient of interest (the interaction of Post and Benefit) for this regression is expected to be insignificant, since there was no policy change for this time period. If it is positive, this would be evidence that another factor or factors besides the fertility subsidy are driving the estimates in our main results. When we include 1995 as our Pre-policy period and 2000 as our Post-policy period, the coefficient for the interaction of Post and Benefit is statistically insignificant across all specifications. This is strong evidence that our results are being driven largely by the fertility subsidy implemented in 2004.

The four areas where the policy took effect are Seoul, Incheon, Daejeon, and Sancheong. These are categorized as treatment areas in our main results (Table 5). All other Korean areas are classified as control areas. As shown in Table 1, some of the areas identified as control areas have a small fertility subsidy. However, their subsidies are minute, and we assume fertility behavior was not affected by them.[13] We believe the effects from these subsidies are too small to be effective, since the vast majority is of the order of only a couple hundred dollars. If these subsidies are, in fact, having an effect on fertility in these areas, then the effect can be assumed to be positive. This would make our estimates a lower bound to the actual effect of the fertility subsidies on childbearing. Once again, we strongly believe this not to be the case, given the small amount of money involved with these subsidies in other areas.

The regression results from Equation (5), which estimates the effect of the policy on the sex ratio of children, are shown in Table 7.[14] We find little evidence that the policy led to a change in the sex ratio of children. The sign for the coefficient of interest (the interaction of Post and Benefit) is mixed across different specifications. The sign is positive for the 21 to 33 age cohort, negative for the 34 to 45 age cohort, and negative for all ages grouped together (21 to 45). The negative sign for all ages (21 to 45) suggests that, after the policy implementation, there were fewer males being born in the treatment areas in comparison to the control areas. This is in keeping with our theoretical argument that the lower cost in childrearing should decrease the overall sex ratio of children. However, the results are insignificant across all specifications. Therefore, we cannot reject the null hypothesis that the sex ratio was unaffected by the policy.

[13] Therefore, in our main estimates (Tables 3 and 5), we use a benefit equal to $0 for these areas.
[14] Table 7 shows two-stage least square results for the effect of the policy on the sex ratio of children. The results from OLS estimation are similar to those shown in Table 7.

VI. Conclusion

This study examines the effect of a recently implemented fertility subsidy in 2004 on childbearing and migration patterns of Korean females. The present value of the subsidy ranges from $4,000 to $9,000 (2004 US$), depending on the area of residence. We use Korean Census data to isolate the effect of this policy through a difference-in-differences research design. Our results provide evidence that the subsidy had a significant positive effect on both fertility rates and the net inflow of females into areas where the policy was implemented.

The strongest effect on fertility from the subsidy occurred for women of prime fertility age (21 to 33). We find little evidence that the policy led to any change in fertility behavior for the 34 to 45 age cohort. Our results indicate that a $1,000 increase in fertility subsidies leads to 0.108% increase in the chance of becoming pregnant for all age groups (21 to 45). Considering the average benefit was $6,200, this translates into 11,000 additional births in the treatment areas as a result of the policy. The elasticity associated with the 21 to 45 age cohort is 0.031, which is in line with previous estimates.

The migration results indicate that the fertility subsidies led to a significant and positive effect on the net inflow of females into policy areas. This effect is driven roughly equally by a decrease in the outflow of females from the subsidized areas and an increase in the inflow from the non-subsidized areas.

We find little evidence that the policy led to a change in the sex ratio of children. The coefficient of interest showing the effect of the policy on the sex ratio of children is negative. This is in line with economic theory. However, the results are insignificant across all specifications. Therefore, we cannot reject the null hypothesis that the sex ratio was unaffected by the policy.

Many countries offer financial incentives to their populations in order to increase fertility rates. The literature is mixed on the effects of these programs. This study adds to the literature by including migration while using a unique identification strategy. Overall, we find that fertility subsidies have a significant positive effect on both childbearing and the net inflow of females into areas where the policy was implemented. We find no effect of the subsidies on the sex ratio of children. Subsidies such as the one examined here give policy makers the option to increase fertility rates in the short-term. Further research on the long-term effects of the fertility subsidy should be conducted when new data sources become available.

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Figure 1: Timeline of the Policy

Figure 2: Probability to Have a Third or Higher Child among Females Aged between 21 and 33

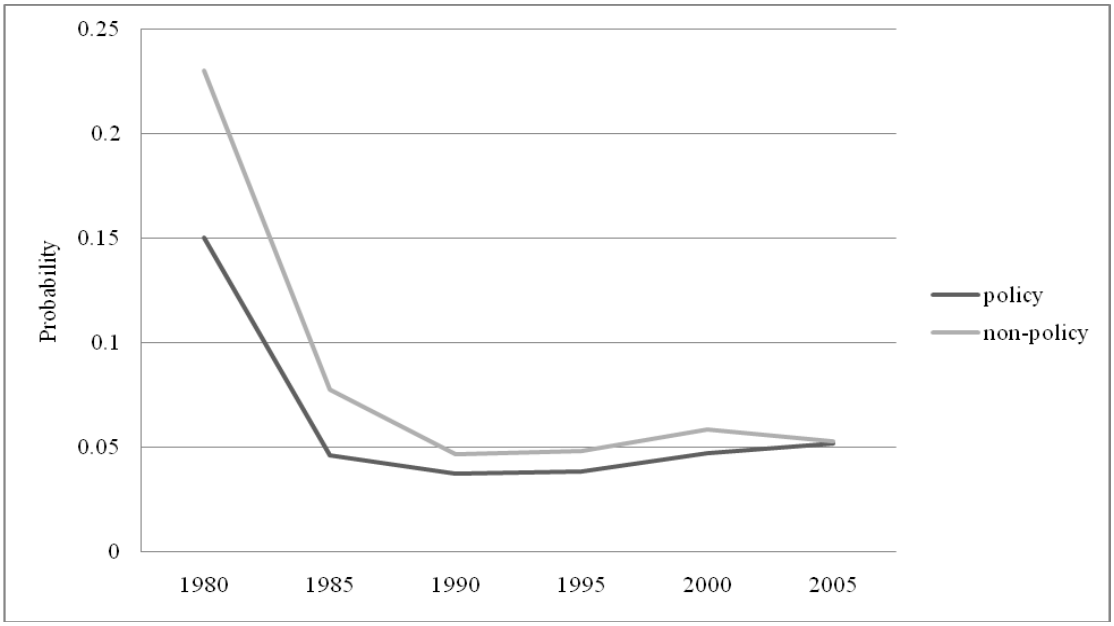

Table 1: Benefits by Region

(as of August 2004)

Policy	Area	Effective from	Eligibility	Benefits
Subsidy for a given time period	Seoul	March 2004	A third or later child	Reimburse childcare costs of the newborn baby for three years.
	Incheon	January 2004	A third or later child	$172.35 per month for five years
	Daejeon	July 2004	A third or later child taken care of in the childcare center	$172.35 - $221.47 per month for three years
	Sancheong, Gyeongnam	January 2004	A third or later child taken care of in the childcare center	$103.41 - $155.16 per month for three years
One-time cash or in-kind subsidy	Suwon, Gyeonggi	January 2004	A third or later child	Gift certificate equivalent to $172.35
	Gapyeong, Gyeonggi	January 2004	A third or later child	Gift certificate equivalent to $86.18
	Yeoncheon, Gyeonggi	August 2003	A newborn baby	0.012401lb of pure gold
	Yangpyeong, Gyeonggi	August 2003	A newborn baby in a rural area	$30.16
	Namyangju, Gyeonngi	January 2005	A third or later child	$86.18 - $258.53
	Taebaek, Gangwon	August 2003	A newborn baby	Gift certificate equivalent to $86.18
	Samcheok, Gangwon	January 2004	A newborn baby	Gift certificate equivalent to $86.18
	Cheongwon, Chungbuk	January 2003	A newborn baby	In-kind subsidy equivalent to $301.61
	Boeun, Chungbuk	January 2004	A newborn baby	Diaper and bath stuff
	Okcheon, Chungbuk	January 2004	A newborn baby	Diaper and bath stuff
	Yeongdong, Chungbuk	February 2004	A newborn baby	Diaper and bath stuff
	Danyang, Chungbuk	February 2004	A newborn baby	Diaper and bath stuff
	Yeongi, Chungnam	January 2003	A newborn baby	Gift certificates equivalent to $301.61 at the birth and $86.18 at the first birthday of the baby
	Gunsan, Jeonbuk	January 2004	A third or later child	$172.35 up to the 150th birth in the area in each year
	Jeongeup, Jeonbuk	January 2004	A third or later child	$258.53 up to the 330th birth in the area in each year
	All 22 areas in Jeonnam	January 2001	A newborn baby from the family lived in the rural area for at least one year	$258.53
	Gunwi, Gyeongbuk	January 2001	A newborn baby	$172.35 at the birth and $258.53 at the first birthday of the baby up to the 360th birth in the area in each year

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Policy	Area	Effective from	Eligibility	Benefits
One-time cash or in-kind subsidy	Uisung, Gyeongbuk	January 2004	A newborn baby	$43.09
	Yeongyang, Gyeongbuk	January 2004	A newborn baby	Gift certificate equivalent to $258.53
	Cheongdo, Gyeongbuk	January 2003	A newborn baby	$43.09
	Goryeong, Gyeongbuk	September 2004	A newborn baby	$43.09
	Bonghwa, Gyeongbuk	May 2003	A newborn baby	Gift certificate equivalent to $301.61 up to the 250th birth in the area in each year
	Hamyang, Gyeongnam	January 2001	A newborn baby	$86.18
	Hapcheon, Gyeongnam	March 2003	A newborn baby	$258.53
	Sancheong, Gyeongnam	July 2000	A newborn baby	$86.18 up to the 250th birth in the area in each year
	Bukjeju, Jeju	?	A third or later child	Gift certificate
	Namjeju, Jeju	January 2004	A newborn baby	$258.53 up to the 700th birth in the area in each year

Source: Joongang il bo, August 2004. Exchange rate is 1,160.43 Korean Won per US$.

Table 2: Mean and Standard Deviation of Variables

	Year 1995		Year 2000		Year 2005	
	No Policy	Policy	No Policy	Policy	No Policy	Policy
Age	34.1540	34.4296	35.7281	36.3823	36.4937	36.5501
	(5.8595)	(5.9127)	(5.7438)	(5.6559)	(5.5508)	(5.5061)
Years of schooling	11.2313	11.8748	12.0366	12.6706	12.7425	13.1767
	(2.8358)	(2.7886)	(2.651)	(2.6288)	(2.2864)	(2.2893)
Household head	0.0337	0.0383	0.0359	0.0351	0.0552	0.0584
	(0.1804)	(0.1918)	(0.1861)	(0.1841)	(0.2285)	(0.2345)
Family Income	37.5152	33.4280	49.6448	51.2042	44.4210	39.6632
	(50.0791)	(54.8523)	(52.0588)	(58.9443)	(49.7133)	(51.3258)
Observations	84,581	40,549	69,586	25,744	75,072	29,381

Note: Standard deviation in the parentheses.

Table 3: Non-IV Fertility Regression Results

	Aged from 21 to 45 years		Aged from 21 to 33 years		Aged from 34 to 45 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.01152	0.00029	-0.01643	-0.01031	0.00324	0.00148
	(6.37)	(0.32)	(2.77)	(1.74)	(2.69)	(2.00)
Benefit (in US$1,000)	-0.00169	-0.00007	-0.00072	0.00027	-0.00037	-0.00012
	(3.36)	(0.43)	(0.64)	(0.23)	(1.41)	(0.83)
Post*Benefit	0.00130	0.00044	0.00514	0.00530	0.00012	0.00001
	(2.57)	(1.76)	(3.31)	(3.11)	(0.34)	(0.04)
Age		-0.00957		-0.04807		-0.00517
		(78.01)		(37.50)		(41.43)
Schooling		0.00383		0.03084		0.00138
		(17.37)		(16.96)		(9.33)
Household Head		-0.00872		-0.05897		-0.00412
		(4.32)		(3.87)		(2.57)
Family Income		-0.00005		-0.00017		-0.00003
		(5.48)		(2.83)		(4.86)
Observations	131,717	131,717	26,155	26,155	105,562	105,562
Pseudo R^2	0.0011	0.2440	0.0005	0.0608	0.0006	0.1258
Log-L	-32,412	-24,531	-14,218	-13,360	-12,686	-11,097

Note: Probit on the probability of having a newborn child for females able to have a third or further newborn child, where the control group includes females at the same eligibility in all areas without the policy and the benefit level is measured in $1,000's. Reported coefficients are marginal probabilities from the probit regression of having a third or higher child. All equations include a constant term, but its coefficient is not reported. t-statistics using robust standard errors with clustering on wards, counties, or urban areas are reported in parentheses.

Table 4: OLS Migration Results

	Between 2000 and 2005			Between 1995 and 2000		
	Net inflow	Inflow	Outflow	Net inflow	Inflow	Outflow
Post	-0.05255	-0.06290	-0.01035	0.03679	0.00001	-0.03678
	(3.04)	(3.69)	(1.46)	(1.42)	(0.00)	(4.16)
Treat	-0.08402	0.00241	0.08643	0.10184	0.20328	0.10144
	(3.89)	(0.13)	(8.28)	(0.63)	(1.30)	(5.65)
Post*Treat	0.08491	0.04309	-0.04182	-0.18485	-0.19938	-0.01453
	(3.19)	(1.87)	(2.94)	(1.13)	(1.27)	(0.70)
Constant	0.02911	0.22707	0.19796	-0.00869	0.22557	0.23426
	(1.94)	(15.67)	(37.66)	(0.41)	(11.89)	(33.11)
Observations	468	468	468	464	464	464
R^2	0.0272	0.0309	0.1395	0.0021	0.0182	0.1811

Note: OLS migration results on the share of net inflows among females aged between 21 and 45 and having at least two children. Robust t statistics in parentheses. In 1996, some areas were either divided or merged. Since the level of observations is each area, geographic adjustments were made, to make the dependent variable comparable over time.

Table 5: IV Fertility Results

	Aged from 21 to 45 years		Aged from 21 to 33 years		Aged from 34 to 45 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.01156	-0.00230	-0.01838	-0.01379	0.00306	0.00240
	(5.82)	(1.26)	(2.77)	(2.17)	(2.43)	(1.85)
Benefit (in US$1,000)	-0.00026	0.00082	0.00262	0.00317	0.00016	0.00024
	(0.35)	(1.26)	(1.07)	(1.44)	(0.49)	(0.77)
Post*Benefit	0.00127	0.00108	0.00657	0.00774	0.00023	0.00022
	(2.00)	(1.76)	(2.39)	(2.92)	(0.62)	(0.63)
Age		-0.01767		-0.05105		-0.00716
		(78.78)		(37.31)		(38.13)
Schooling		0.00230		0.02917		0.00161
		(8.14)		(16.74)		(8.92)
Household Head		-0.01165		-0.05611		-0.00641
		(4.28)		(4.22)		(3.15)
Family Income		-0.00005		-0.00015		-0.00004
		(4.35)		(2.70)		(5.02)
Constant	0.07246	0.70970	0.23391	1.44296	0.02382	0.29019
	(38.42)	(72.50)	(48.39)	(30.83)	(26.22)	(34.44)
Observations	131,717	131,717	26,155	26,155	105,562	105,562
R^2	0.0003	0.1129		0.0665	0.0000	0.0266

Note: IV fertility results on the probability of having a newborn child for females able to have a third or further newborn child, where the control group includes females at the same eligibility in all areas without the policy and the benefit level is measured in $1,000's. Information on residence 5-year ago is used to control for the possibility of bias resulting from the change in migration patterns of females across treatment and control groups. Reported coefficients are from two-stage least square regressions. t-statistics using robust standard errors with clustering on wards, counties, or urban areas are reported in parentheses. Benefit and Post*Benefit are instrumented by their lagged values (as shown in Equations (3) and (4)).

Table 6: Falsification Test for Fertility Results using the years 1995 and 2000

	Aged from 21 to 45 years		Aged from 21 to 33 years		Aged from 34 to 45 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.02038	0.00043	0.00141	0.00265	-0.00266	-0.00002
	(8.55)	(0.44)	(0.26)	(0.53)	(2.14)	(0.03)
Benefit (in US$1,000)	-0.00101	-0.00001	0.00092	0.00048	-0.00021	-0.00012
	(1.62)	(0.08)	(0.84)	(0.48)	(0.96)	(0.93)
Post*Benefit	-0.00098	-0.00004	-0.00165	0.00028	-0.00016	-0.00001
	(1.59)	(0.15)	(1.25)	(0.22)	(0.53)	(0.07)
Age		-0.01194		-0.05382		-0.00525
		(87.67)		(49.91)		(36.48)
Schooling		0.00450		0.02554		0.00154
		(22.08)		(21.84)		(11.77)
Household Head		-0.00459		-0.02176		-0.00205
		(1.84)		(1.41)		(1.18)
Family Income		-0.00006		-0.00035		-0.00002
		(7.04)		(5.99)		(3.54)
Observations	140,845	140,845	38,496	38,496	102,349	102,349
Pseudo R^2	0.0031	0.2546	0.0000	0.0845	0.0005	0.1132
Log-L	-40,427	-30,228	-21,150	-19,364	-12,172	-10,800

Note: Probit on the probability of giving birth for females able to have a third or further child, where the control group includes females with the same eligibility in all non-policy areas for the time period between 1995 and 2000 (when the policy was not present). Reported coefficients are marginal probabilities from probit regression of having a third or higher child. All equations include a constant term, but its coefficient is not reported. t-statistics using robust standard errors with clustering on wards, counties, or urban areas are reported in parentheses.

Table 7: IV Sex Ratio Results

	Aged 21 to 45 years		Aged 21 to 33 years		Aged 34 to 45 years	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.00487	-0.00445	-0.02095	-0.01990	-0.00161	-0.00069
	(2.18)	(1.92)	(4.07)	(3.86)	(0.70)	(0.29)
Benefit (in US$1,000)	-0.00125	-0.00126	-0.00126	-0.00116	-0.00126	-0.00122
	(2.09)	(2.10)	(0.85)	(0.78)	(2.05)	(1.99)
Post*Benefit	-0.00044	-0.00042	0.00248	0.00243	-0.00100	-0.00100
	(0.54)	(0.52)	(1.30)	(1.28)	(1.15)	(1.14)
Age		0.00050		-0.00048		-0.00007
		(2.24)		(0.50)		(0.18)
Schooling		-0.00029		-0.00105		-0.00032
		(0.73)		(0.89)		(0.73)
Household Head		-0.01064		-0.02606		-0.00807
		(2.54)		(2.09)		(1.69)
Family Income		0.00005		0.00000		0.00006
		(2.66)		(0.07)		(2.68)
Constant	0.53191	0.51420	0.52944	0.55782	0.53266	0.53591
	(276.76)	(49.81)	(144.88)	(18.31)	(263.23)	(29.93)
Observations	131,717	131,717	26,155	26,155	105,562	105,562
R^2	0.0001	0.0003	0.0005	0.0007	0.0001	0.0002

Note: IV sex ratio results, where sex ratio is measured by the number of male children divided by the total number of children. The control group includes females at the same eligibility in all areas without the policy. t-statistics using robust standard errors with clustering on wards, counties, or urban areas are reported in parentheses. Benefit and Post*Benefit are instrumented by their lagged values (as shown in Equations (3) and (4)).