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Financial Incentives, Fertility, and Son Preference in Armenia

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Abstract

Armenia experienced dramatic demographic changes in the past three decades: the share of adults age 65 and over nearly doubled, the total fertility rate reduced by more than 30 percent, and the male-to-female sex ratio at birth increased to one of the world's highest. Like other middle-income countries concerned with the implications of an aging population for long-term growth and fiscal sustainability, Armenia introduced financial incentives to promote fertility. This paper estimates the effect of the 2009 reform of the universal Childbirth Benefit Program, which increased the amounts of lump sum transfers conditional on birth. The analysis relies on a quasi-experimental strategy exploiting the timing of the policy change and eligibility rule—women get a larger transfer for third and higher-order births. The findings show that the annual probability of an additional birth among women with at least two other children increased between 1.4 and 1.6 percentage points in the five years following the policy change. These effects are equivalent to 58 and 64 percent of the pre-reform birth probability for women who had at least two children. Given the previously demonstrated relationship between fertility level and sex ratio in societies with strong son preference, the reform may potentially alleviate the sex imbalance without directly targeting it. Parents who already have at least one son and are less likely to engage in sex selection and more likely to have additional births; however, the findings do not indicate a significant increase in the likelihood of having daughters.

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1. Introduction

The share of adults aged 65 and above in Armenia has almost doubled from 5.6% in 1990 to 11.5% in 2019.[1] As in other middle-income countries, the aging of the population raises concerns about productive capacity and fiscal sustainability, especially with respect to pension and health care systems. To a large extent, the aging of the Armenian population can be attributed to decreasing fertility rates. The total fertility rate decreased from 2.5 births per woman in 1990 to 1.7 births in 2000 and subsequently remained flat.[2] In the face of these fast-paced demographic changes, Armenia reformed its universal Childbirth Benefit Program in 2009 with the aim of incentivizing higher fertility rates.[3] The reform substantially increased cash transfer amounts and instituted differentiation based on birth order. Women became eligible to receive a one-time cash transfer of about 125 USD at birth for their first and second births and approximately 1,050 USD for their third and higher order births. For benchmarking, the lower amount represents 4 percent and the larger amount represents 38 percent of GNI per capita.

Most of the literature evaluating the impact of financial incentives on fertility presents results from high-income countries. Such examples include childbirth grants in Spain and Canada, monthly child subsidies in Israel, and pronatalist tax policies in France, the United States, and Germany (Gonzalez 2013, Milligan 2005, Cohen, Dehejia and Romanov 2013, Laroque and Salanie 2014, Whittington et al. 1990, Crump et al. 2011, Zhang, Quan and van Meerbergen 1994, and Riphahn and Wiynck 2017).[4] Consistent with Becker's (1960) framework in which the demand for children depends on the costs associated with having an additional child, these studies show that fertility can increase with the introduction of financial incentives. However, results differ with respect to the impact of financial incentives on beneficiaries of different socio-economic backgrounds and by policy instrument. Milligan (2005) finds that Canadian women with higher family income are more likely to respond to a pronatalist transfer and concludes that increased access to non-labor income

[1] Statistics from World Development Indicators.
[2] Life expectancy at birth also grew in Armenia, from 70.94 years in 1980 74.94 years in 2018. It should be noted that there was a drop in life expectancy around 1990 due to the independence war from the Soviet Union. All statistics from World Development Indicators.
[3] Prior to 2009, flat childbirth grants were offered to all the population, but they did not differentiate by birth order. Beneficiaries of the main cash transfer program, the Family Benefit Program, also received a cash top-up with each birth that starting in 2007 was larger for the third and higher-order births.
[4] Another set of papers study more generally the impact of family and work-life balance policies on fertility. Gauthier (2007) and Olivetti and Petrongolo (2017) provide reviews of the literature.

increases responsiveness to the transfer. In Israel, however, Cohen et al. (2007) find that the magnitude of the child subsidy effect decreases with income and suggest that this pattern arises because the subsidy constitutes a smaller percentage of the household income. These within-country heterogeneities by income level give rise to the question: do pronatalist financial incentives have different impacts across countries with different income levels?

The Armenian pronatalist policy reform occurred in a context not only characterized by low fertility, but also by strong son preference. Decreases to the Armenian fertility rate were accompanied by an increased sex ratio at birth, rising from 107 to 117 male births per 100 female births between 1990 and 2002.[5] The simultaneous reduction in fertility and rise in the sex ratio at birth is consistent with experiences in other countries such as China, India and the Republic of Korea (Park and Cho 1995, Das Gupta and Bhat 1997, Ebenstein 2010, Jayachandran 2017, Anukriti 2018). Given that the likelihood of having at least one son by chance is positively correlated with the number of pregnancies, women or couples with a strong preference for boys might be more likely to make use of sex-selective abortion at low fertility levels. In a study of the relationship between desired fertility and sex ratio, Jayachandran (2017) finds that between a third and half of India's recent sex ratio increase is due to fertility decline. This relationship between fertility level and sex ratio suggests that a policy that successfully increases fertility might also alleviate the sex imbalance. Data from Hebei province in China, for example, suggest that the sex ratio at birth declined as the one-child-policy was gradually relaxed (Fan et al. 2020).

In this paper, we estimate the impact of the 2009 reform of the Armenia Childbirth Benefit Program on births by women with at least two children in the five years following the policy change.[6] We employ a quasi-experimental empirical strategy that exploits the timing of the policy change and the difference in grant amounts women were eligible to receive. We construct an annual birth panel data set using fertility histories reported in the 2010 and 2015 rounds of the Armenia Demographic and Health Survey (ADHS). The rules of the policy change enable us to define treatment and control groups according to year and number of previous births. We estimate the overall effect on births and test whether women of different socio-economic backgrounds respond differently to the changing prices of having additional children.

[5] Statistics from World Development Indicators.

[6] We restrict the analysis to the period before 2014 because the program was reformed again that year.

Our study contributes to the literature not only by presenting results from a middle-income country, but also by presenting results of a pronatalist financial incentives policy introduced in a context characterized by strong son-preference. Relatedly, Anukriti (2018) studies a program in the Indian state of Haryana that used financial incentives with the objective of *reducing* both fertility and the male-female sex ratio at birth. To our knowledge, our study is the first to consider son preference in the context of a policy that encourages *higher* fertility. In Armenia, son preference is manifested by sex selective abortions as well as by decreased likelihood of additional pregnancies by parents who already have sons —usually referred to as stopping behavior (Duthé et al. 2011, Guilmoto 2013, Ebenstein 2013). We therefore separately assess whether the policy change impacted the likelihood of having male and female newborns and whether the response to the reform differed according to whether parents already had sons or not.

We find that the reform in the childbirth grant policy increased fertility among women who already had at least two prior births. In our preferred specification, the annual birth probability for this group increased by 1.4 to 1.6 percentage points in the period between 2009 and 2013, which represents 58 and 64 percent of the birth probability of women with at least two children before the policy change. The increase in birth probability is not immediate but materializes gradually. We show that the effect is not a result of anticipation of the policy change and that it is robust to alternative econometric specifications. We find no evidence of heterogeneous impacts by household wealth, education level or urban versus rural residence.

The Childbirth Grant reform reduced stopping behavior by women who already had at least one son, resulting in higher fertility. Before the reform, these women were less likely to have more than two births and were less likely to engage in sex selection when having a third pregnancy. The increased fertility by this group is likely to reduce the sex imbalance. There is no such impact for women who already had two or more girls (that is, no sons), who are anyway likely to have additional births even without incentivization due to son preference. Between these two groups of women (those with any son and those with none), there are similar impacts of the policy reform on the likelihoods of having additional sons and daughters. These results imply that the sex balance among the additional births caused by the policy change would diminish the male-female sex ratio at birth. However, the sample size of the ADHS does not enable us to precisely estimate the sex ratio among the additional births and we interpret these findings as suggestive.

The rest of the paper is organized as follows. In the next section we provide background to the study by describing fertility trends in Armenia and the Childbirth Benefit Program. Section 3 describes the data used in the analysis and our empirical strategy. Section 4 presents the results together with robustness checks. Finally, section 5 provides concluding remarks.

2. Background

The collapse of the Soviet Union occurred as Armenia was still recovering from a devastating earthquake in 1988 and was followed by an armed conflict with neighboring Azerbaijan. The demographic changes following these events were equally dramatic. First, it is estimated that roughly a third of the population has migrated out of the country since 1990 (World Bank, 2011). Second, the total fertility rate dropped from 2.5 births per woman at the beginning of the 1990s to 1.6 by 2000 according to the World Development Indicators, representing about a 30 percent fall (Figure 1). The rate remained between 1.6 and 1.7 in the following 15 years. The persistence of a low total fertility rate of 1.6, with no net migration, would imply that population size will halve in approximately 90 years (Goldstein et al. 2009).

This pattern of falling fertility was not particular to Armenia: in almost every former Soviet country, fertility rates declined below their replacement level during the transition years (Billingsley 2010). The collapse of the former Soviet Union, in which the costs associated with raising children were mostly born by the state, implied a substantial increase in the cost of having children. The slow recovery in real wages throughout the transition and the recent history of armed conflict with Azerbaijan are also theorized to affect fertility behavior. Ebenstein (2013), for example, documents lower fertility among families forced to migrate, as well as couples with an unemployed husband, suggesting a link between economic instability and low fertility rates.[7]

At the same time that fertility rates dropped, an imbalance in the sex ratio at birth emerged in Armenia: it rose from a biologically normal range of 104-107 boys per 100 girls (Chahnazarian 1988) to 117 in the early 2000s (Figure 1), reaching levels comparable to those in China, Azerbaijan and some Indian states. The sex ratio has since been slowly decreasing, though it remains above normal levels. The sex imbalance is a consequence of an increase in stopping behavior of parents who already have a son (Filmer et al. 2008) as well as sex selective abortion (Duthé et al. 2012,

[7] Several studies have analyzed the relationship between economic recessions and fertility. See Sobotka et al. (2011) for a recent review.

Ebenstein 2013). These trends suggest a link between the observed fertility rate and sex imbalance. In other words, although son preference might exist in a society, it may not be revealed until families reduce the number of children they have.

While Armenia has had a universal childbirth lump-sum transfer —the Childbirth Benefit Program (CBBP)— in place since the 1990s, it was of a small magnitude and did not differentiate by birth order. In 2009 the benefits were increased, and the program introduced a differentiation in the benefit size by birth parity. The size of the transfer was increased from 87 USD (35,000 AMD) to about 125 USD (50,000 AMD) for parents having their first or second child, and from 87 USD (35,000 AMD) to 1,050 USD (430,000 AMDs) for the third and higher order births. The amount of the benefit is large, both relative to the average income in Armenia and when compared to similar programs in other countries. In 2009, the benefit for first and second-order births was higher than the monthly minimum wage, while the benefit for third or higher-order births were over one-third of Armenia's GNI per capita[8] The universal Childbirth Benefit gained support among policy makers and the amount of the benefit was further increased in January 2014. In this paper, however, we only study the period between these two reforms.

In addition to the universal Childbirth Benefit Program, poor families that were beneficiaries of the Family Benefit Program (FBP), the main social assistance program in Armenia, received a top-up called the Childbirth One-Off Assistance.[9] In 2009, 12 percent of households were beneficiaries of the program.[10] Table 1 summarizes the sizes of the transfers by year and birth order provided by the two programs. The One-Off Assistance consisted of similar transfer amounts for all birth parities until 2007 when differentiation by birth order was introduced. In 2009, this differentiation was undone when the universal high childbirth grants were introduced as part of the CBBP.

The 2009 policy change is first described in a government decree from April 2008. The document did not detail the actual changes to the program but mentions that the benefit will increase. The

[8] The minimum wage was 30,000 AMDs in 2009, while Armenia's Gross National Income per capita was about 1,133,000 AMDs in the same year.

[9] The Family Benefit Program is a means tested social assistance program that gives a monthly allowance to support poor families. Program participation is based on an eligibility formula that takes into account the vulnerability of the household (based mainly on family structure and labor force status), income of the household, place and dwelling of residence, and a set of automatic disqualifiers (such has owning land, etc.). There are no associated conditionalities and the participation on the program grants access to other benefits (such as the *Childbirth One-Off Assistance*, gas subsidies and more).

[10] According to the Social Protection and Expenditure Database for East Europe and Central Asia (SPEED) of the World Bank.

first mentioning of the proposed change in the transfer amount found in the (English) media is dated October 27, 2008. To our knowledge, no other policies affecting fertility in different ways for different birth parities were enacted in the same period.

A couple of features make Armenia different from other economies with similar policies to address declining birthrates and which have been rigorously evaluated (including France, Germany, Sweden, and the Canadian province of Quebec). First, Armenia is a middle-income country, with a large share of the population under the poverty line (about 30 percent), who also tend to have more children on average (Billingsley 2008). Second, there is a high preference for sons in the country. Armenia has the fourth largest male-to-female ratio at birth in the world, smaller only than those in China, Azerbaijan and Vietnam (World Bank 2019). It is important to note that, unlike China, low fertility levels in Armenia are not imposed by law. If the financial incentive changes the overall demand for children of households, it could affect both the total number of births and the sex composition of children.

One last point to note is that the Armenian economy was in distress around the time of the introduction of the Childbirth Benefit Program due to the 2008 global financial crisis. Real GDP dropped by 14 percent in 2009.[11] As suggested by previous studies, individuals tend to postpone childbearing decisions when faced with economic uncertainty and job instability, and the Great Recession was no exception. For instance, Comolli (2017) analyzes the fertility response to the Great Recession in 32 Western economies and finds that a 1 percent increase in the unemployment rate is associated with a decline in fertility rates the following year of 0.08 percent.

3. Data and Empirical Framework

3.1 Data

This analysis uses the two latest rounds of the Armenian Demographic and Health Survey (ADHS), collected in 2010 and 2015-16. The ADHS is a nationally representative sample of women 15-49 years old and includes detailed fertility histories that enable us to construct a panel of annual births from 2003 to 2013 linked with women's number of previous births by sex and a set of socio-demographic characteristics. From a sample of 8,449 ever-married women, we created an

[11] World Development Indicators.

unbalanced panel of 72,469 woman-year observations.[12] [13]

Table A1 in the Appendix presents the summary statistics of some key indicators related to women's socio-demographic characteristics, their fertility histories, and husbands' characteristics for married women.[14] Panel A presents characteristics observed for the 8,449 ever married women in the sample at the time of the survey, while Panel B reports information obtained retrospectively for women observed between 2003 and 2013 (the period under analysis), where the unit of observation is a woman-year combination.[15] Women in the sample had an average of two births. Less than 10 percent had no children while over 70 percent had two or more. The average age at first birth was 22 years old and the probability of giving birth in any given year in the 2003-2013 period was 10 percent (Panel B).

The ADHS respondents were on average 35 years old and 90 percent were married at the time of the survey. In comparison to women with fewer than two children, women with two or more children were on average older by 7 years and more likely to be married by 10 percentage points. Women with two or more children were more likely to achieve education beyond the primary level but less likely to have higher education. Overall, 53 percent of the sample had higher education, while only 36 percent reported that they were employed at the time of the survey. Husbands of married women in the sample were 39 years old on average, and 44 percent of them had higher education. Contrary to what is observed for women, 24 percent of husbands had migrated for work-related reasons over the three years before the survey.

With respect to son preference, women had on average 1.06 sons and 0.94 daughters, a difference that is statistically significant. This implies that at the time of the survey 53 percent of children ever born were male. Moreover, the probability of an additional birth is markedly different depending on whether women already had a boy or not: conditional on having at least one child, the probability of an additional one is 13 percent for women whose previous children are all daughters, while it is only 4 percent for those who already had a son. This difference is statistically

[12] Given that the ADHS sample consists of women who are 15-49 years by 2015-16, older women who were 49 years old or less at some point before but who are over 49 by the time of the survey are not included in the sample.
[13] Out-of-the wedlock births are uncommon in Armenia accounting for less than 1 percent of our sample.
[14] Husband's characteristics are only reported by women who are married at the time of the survey.
[15] Although we are able to reconstruct each woman's fertility history thanks to the reported date of birth of each child, most of the covariates used in the analysis refer to the time of the survey, except for women's age, composition of their children and (imperfectly) marital status, which can be obtained backwards for each point in time.

significant at the 1% level.

3.2 Identification strategy

To identify the impact of the childbirth grants on fertility outcomes, we rely on the variation in transfer amounts based on the timing of the unexpected policy change and on the number of children women had in each year since the amount of the benefit differed by birth rank. This strategy is similar to those employed by Milligan (2005) and Cohen, Dehejia and Romanov (2013).[16] The benchmark difference-in-differences econometric specification is the following:

$$b_{it} = \alpha + \beta_1 Two\_more_{it} + \beta_2 Two\_more_{it} \times Post_t + \gamma X_{it} + \lambda_t + \epsilon_{it} \qquad (1)$$

where b_{it} is an indicator for whether woman *i* gave birth in year *t*. Two_more_{it} is a binary variable equal to one if woman *i* had at least two children at the beginning of year *t*, $Post_t$ is a dummy equal to one for the years after the policy change (2009-2013) and λ_t are a set of year fixed effects. X_{it} is a vector of women's (and their husbands') characteristics, added to control for compositional differences between women with two or more and women with less than two children that could affect the probability of giving birth. Our baseline specification is a probit model with standard errors clustered at the mother level. The coefficient that corresponds to the interaction between both dummies, β_2, is interpreted as the impact of the policy change on fertility, assuming that changes over time in fertility rates would be similar for women regardless of the number of children in the absence of the policy change (parallel trends assumption). Given that we do not observe whether women received the childbirth transfers, our estimates represent intention-to-treat effects.

There are different ways in which we can define the control group for the analysis. One of them is to consider women with less than two children in year *t*, since they are not eligible to the transfer. This control group definition includes women with both one and zero children. However, the decision to have an additional child is arguably different from the decision to have a first child. Billari et al. (2009), for example, find that in Bulgaria the decision to become parents is more influenced by social norms while decisions regarding higher parity births are more influenced by perceived costs and benefits. Moreover, the group of childless women includes infertile women (or

[16] Milligan (2005) employs a triple-difference approach by also exploiting variation policies across jurisdictions in Canada.

women married to infertile spouses), on which the childbirth benefit would not have any impact. Therefore, changes in fertility rates by women with two children could theoretically be more similar to those by women with one child than to those by childless women. Hence, a second definition of the control group used in our analysis includes only women with one child.

While we cannot rule out the existence of other factors contemporaneous to the 2009 policy change that might have affected birth rates differently for women with different number of children, we can test whether the parallel trends assumption could be rejected. We run a probit model of the fertility outcome in the pre-reform period on the indicator for having two or more children, year dummies and the interaction between them. We run specifications both on the whole sample of ever-married women as well as restricting the sample only to women with at least one child, with and without controls. The results of this test (Table 2) validate the assumption: none of the coefficients associated with each of the interactions is individually significant, neither when we include childless women in the control group nor when we exclude them. Moreover, we perform a Chi-squared test of joint significance of all the interaction terms together, and in neither of the four specifications we find evidence that allows us to reject the hypothesis that all of the coefficients together are equal to zero.

One concern about the identification strategy is that women with fewer than two children could theoretically also react to the policy change by increasing their fertility in expectation of getting the large transfer in the future. This could be particularly the case if the policy was not perceived to continue over a long period of time. A priori, this type of response is more likely for younger women than for women closer to the end of their fertile period, for whom a third child may be perceived to be out of reach. If such fertility behavior by the control group exists, then it will diminish the size of our estimate and our results would provide a lower bound for the true fertility increase.

A second concern is that women could switch from the control to the treatment group over the period of analysis due to the policy reform. Women with one child may choose to have a second child to become eligible to receive the larger childbirth grant, and therefore endogenously move from the control to the treatment group. The results in Table 2 suggest that the parallel trends assumption cannot be rejected for the pre-reform period. However, it could be that the fertility response to the reform changed composition of the treatment and control groups with respect to unobserved determinants of fertility, potentially biasing our estimates. As part of our robustness checks, we employ an alternative specification in which we define treatment and control groups which do not vary over time and are determined by the number of births women had in 2008, just before the policy reform.

Another challenge in identifying the effects of the childbirth grant reform arises from the financial crisis that hit Armenia at the same time of the policy change. The policy change was enacted in 2009, a year in which the country's GDP shrank by 14.1 percent.[17] If the financial crisis impacted differently women with different number of children, we could not separate the impacts of the crisis on fertility from those of the policy change. In order to assess whether such differential impacts occurred, we use auxiliary data from the Armenia Integrated Living Conditions Survey.[18] The annual cross-sectional data allow us to assess whether trends related to key economic and demographic outcomes changed differentially for women with different number of children following the financial crisis. We employ a difference-in-difference approach similar to that described in equation (1), controlling for women's age, education, and rural residence. As reported in Table A3 in the appendix, we do not find differential impacts on women's marital or employment status, or employment status of the spouse (for married women). However, we find differential trends with respect to residence in a household in which a member migrated internationally in the preceding year. Relative to the pre-crisis (and pre-reform) period and to women with two or more children, childless women or those with one child were more likely to reside in a household where at least one member migrated abroad in the previous year by two percentage points. Overall, we do not find strong evidence that women with two or more children were impacted differently by the financial crisis relative to those with fewer children.

[17] Statistics from World Development Indicators.

[18] The Integrated Living Conditions Survey (ILCS) is a survey conducted annually, whose main goal is to assess poverty and provide information on living conditions in Armenia. We use data for the years 2004 to 2012 and construct individual yearly birth histories for all women of reproductive ages, based on the date of birth of children listed in the households (the ILCS does not include questions directly related to fertility). We restrict the sample in order to have the same age distribution as in the DHS panel.

4. Results

4.1. Main Results

Table 3 reports the marginal effects from the estimation of our benchmark probit model.[19] The results in Columns 1 and 3 are from an analysis of the full sample of ever-married women while the results in Column 2 are from a subsample excluding women with no children. In all specifications, we control for women's socioeconomic background characteristics, the sex and age composition of her previous children, and regional dummies.[20] The specification in Column 3 includes controls for marital status, husband's age and education (for those married at the time of the survey). [21]

We find a positive and statistically significant impact of the policy reform on fertility in the period between 2009 and 2013. When analyzing the full sample, we find an overall increase of 1.6 percentage points in the annual birth probability by women with at least two children (Column 1, p-value<0.01). The coefficient and the significance level do not change when excluding childless women from the regression sample (Column 2). Relative to the probability of an additional birth by women with at least two children before 2009, these estimated impacts represent an increase of 58 and 64 percent in the fertility rate. When controlling for marital status and husbands' characteristics, the impact decreases to 1.4 percentage points (Column 3).

To complement the regression analysis presented in Table 3, Figure 2 presents plots of the (unconditional) birth rates per thousand women with two or more children and women with fewer than two children. As can be seen, birth rates by women who already have at least two children are consistently lower than those by women with zero or one child. After 2009, there is a reduction in the birth rates by women with fewer than two children. This implies that the estimated effects are driven by a decrease in birth rates by women with fewer than two children, rather than by an

[19] The reported marginal effects are the average of the individual marginal effects.

[20] Specifically, we control for dummies for age categories (15-19, 20-24, 25-29, 30-34, 35-39, 40+), whether the woman had a birth during the last year, dummies for educational level (primary or less (omitted), secondary or higher), dummies for the number of children in specific gender-age groups (number of boys and number of girls aged 0-4; 5-10; 11-18; 19 or more), a dummy for urban/rural and dummies for region of residence. The results are also robust to the inclusion of a second- order polynomial of age instead of age category dummies.

[21] In the survey, women reported current marital status as well as timing of first cohabitation and the number of unions ever had. We use this retrospective information to construct yearly marital status. The marital status indicator might be less accurate for women with more than one marriage or those divorced, widowed or separated. Our results are robust to the restricting the sample only to women for whom we can measure precisely their marital status at the time of giving birth (results not reported).

increase in births for women with two or more children. While we cannot prove a causal relationship, it is likely that the decline in fertility by women with one or no child is associated with the devastating economic crisis. The Child Benefit Program might have helped eligible women to counteract similar impacts on their fertility.

Figure 3 presents results of an alternative dynamic difference-in-difference specification, both for the full sample as well as for the sample that excludes childless women. Each dot represents the marginal effect of the interaction between the corresponding year dummy and the indicator for having two or more children, and the bars represent the 95% confidence intervals around the point estimate. The regression includes the same set of controls as in Column 1 of Table 3, and the last year before the policy change, 2008, is the omitted category. The full regression results are reported in Table A2 in the appendix. As can be seen, the impact of the policy is estimated to have increased over time, although there are large overlaps in the confidence intervals. The coefficient is close to zero for the first year after the policy implementation (i.e. 2009) but gradually increases subsequently. The coefficients remain statistically insignificant until 2012 and become significant in 2013 at least at the 5% level. One of the reasons for a delay in birth response to the reform could be the time needed for conception and pregnancy. In addition, it might have taken time for the population to learn about the childbirth grants and how they are administered. We can also not rule out that the introduction of the reform at a time of economic crisis might have also affected the timing of the response, with women postponing their fertility decisions.

After establishing that the policy change impacted overall fertility, we turn to examine heterogeneity in response to the policy change by socio-economic background. The childbirth grant associated with a third or higher-order birth is substantial relative to the average income in the country. Yet, it represents a substantially higher proportional increase in the income of poor households relative to wealthier ones. Alternatively, women's employment or broader economic opportunities, related for instance to their educational level or whether they live in urban versus rural areas of the country, could also determine responsiveness to the benefit. To assess whether there were heterogeneous responses among Armenian women, we run a set of regressions where all the independent variables included in equation (1) are interacted with an indicator for a given woman's characteristic.

We first consider heterogeneity with respect to household wealth. Our analysis is limited by the fact that the ADHS provides us only with household wealth in 2010 and 2015.[22] We must assume that the ranking implied by the wealth index was constant, although we cannot rule out the existence of past economic shocks that simultaneously affected both wealth and fertility. There is a positive correlation between education attainment and wealth, suggesting that the variation in household wealth is meaningful. In addition, we use broad categories, such as terciles and quintiles, that are less likely to change. Nevertheless, these results should only be interpreted as suggestive.

We start by testing whether women in the bottom wealth quintile responded differently to the reform than women from wealthier households. On the one hand, we might expect the poorest women to be most responsive to the reform because the transfer amounts constitute a larger percentage of their household income. On the other hand, the policy change we study represents a smaller change in transfer amount for beneficiaries of the Family Benefit Program, that had introduced differentiation by birth order in the Childbirth One-Off Assistance already in 2007 (see Table 1). The smaller change in magnitude of the transfer amount might reduce their responsiveness to the 2009 reform. As we cannot identify FBP beneficiary status in the DHS data, we proxy it using the lowest wealth quintile in wealth score provided by DHS. As seen in Column 1 of Table 4, we find women in the bottom wealth quintile are less likely to have an additional birth in response to the policy change by one percentage point relative to wealthier women, although the results are not statistically significant (p-value of 0.13).

The results on the response of women below the 20th wealth percentile could also be driven by differential impacts of the financial crisis. When analyzing trends of key economic and demographic outcomes before and after 2009, we find differential changes by wealth quintile. Table A4 in the appendix presents results of a difference-in-difference analysis conducted with the ILCS data, in which we test for heterogeneity in trends by wealth quintile. Relative to the 2004-2008 period, there was an overall increase in reporting of employment by women in the 2009-2012 period. However, employment among women in the bottom wealth quintile increased by additional two percentage points relative to the increase in the rate among wealthier women. The poorest group also experienced significantly larger increases in the share that is married to an employed

[22] We use a wealth index provided by the ADHS, which is constructed taking into account urban-rural differences in household and dwelling characteristics, based on assets owned. For more information on the index construction, see National Statistical Service Armenia et al. (2017).

husband or resides in a household with a member who migrated internationally. Given these results, we are unable to separate heterogeneous impacts of the childbirth grant reform from differential impacts of the financial crisis by the poorest households.

In Column 2 of Table 4, we present results of an alternative specification in which we consider heterogeneity by wealth terciles. While the impact on women in the middle wealth tercile is stronger by 1.6 percentage points, the difference is not statistically significant. In Column 3 of Table 4, we present results of a regression specification in which we interact the coefficient of interest with a binary variable for having completed higher education (53 percent of the sample). In Column 4, we consider heterogeneity by terciles in years of education. The advantage of using education as a proxy for socioeconomic status is that the vast majority of women completed their education prior to the policy change and before having their first pregnancy. Using both specifications, we do not find statistically significant differences in responses. Likewise, the use of other education classifications (such as dummies for different levels or years of education) yields the same results.

Lastly, we test for heterogeneity in impacts by urban and rural residence. Such difference in residence could imply differences in access to employment opportunities, availability of public services and costs associated with raising children. All these factors could theoretically affect fertility decisions and response to the pro-natalist financial incentives. However, as seen in Column 5 of Table 4, we do not find different responses to the program for women residing in urban or rural areas.

4.2 Robustness checks

In Table 5, we present several robustness checks to examine how sensitive our results are to different empirical choices. We first remove the time window before and after the policy change. The first announcement in the press of the reform, which is known to us, occurred in October 2008. Given the time it takes to conceive and the normal duration of pregnancies, the program should not impact births in the first half of 2009 and including the year in our analysis might reduce the estimated impact. On the other hand, the population might have known of the government decree from April 2008 or have been aware that such policy change was being considered. This might lead couples to reduce births prior to the policy to wait to be eligible for the large transfers. Such anticipation effect might increase the estimation of the policy effect. The first two columns in Table

5 report the results of the difference-in-difference specification excluding years 2008 and 2009 from the analysis sample, respectively. The marginal effect associated with the interaction term indicates an increase in the annual birth probability to give birth of 1.7 percentage points when excluding data from 2008 in the analysis, an estimate that is negligibly different from the one of our baseline model. This result suggests that our findings are not driven by anticipation effects. When the 2009 data is excluded, the estimated marginal effect increases to 2.3. The higher coefficient is consistent with gradual learning about the program by the population and with the expectation that response to the program is unlikely to materialize immediately due to the time needed for conception. The coefficient, however, is not statistically different from the baseline coefficient.

Next, we use a linear probability model (LPM) instead of a probit model, and report the results for the whole sample, with and without controlling for husband's characteristics, and the subsample excluding childless women in columns 3 to 5. These specifications result in an effect of two percentage points increase in the annual birth probabilities of women with at least two children using the full sample, 2.7 percentage points in the sample excluding women with no children, and 1.6 percentage points when controlling for spouse characteristics. All these coefficients are not statistically different from the corresponding probit specifications (Table 3). While it is reassuring that both models produce positive and statistically significant coefficient estimates, between 19 and 22 percent of the predicted probabilities generated with the LPM specification are outside the unit interval. Following the intuition that the LPM estimate is more likely to be biased the larger the relative share of the predicted probabilities that fall outside the unit interval (Horrace and Oaxaca 2006), we chose the probit model as our preferred specification.

Finally, we consider the fact that ineligibility for the larger transfer amount is not fixed and, more importantly, it is directly impacted by the reform, because women with one child may choose to have an additional child to become eligible for the larger transfer. If the comparison group changes based on unobserved determinants of fertility, then our treatment and comparison groups not only differ in terms of their treatment status but also in terms of other unobservable characteristics, potentially biasing our estimates. In order to address this concern, in columns 6-8 of Table 5 we estimate a similar difference-in-differences model, but defining treatment and control groups based on the number of children that women had immediately before the policy change, that is, in 2008.[23] The results based on this alternative identification strategy yield an estimated impact of three percentage points when using the full sample, an estimate that is not cannot be distinguished statistically different from the baseline model's estimate. Controlling for husband's characteristics does not significantly change the results, with an estimated effect of 3.2 percentage points (Column 8), while it increases to 3.4 percentage points when excluding childless women from the regression sample (p-value<0.05, Column 7). Overall, this alternative definition of treatment and control groups yields results that are consistent with our main findings, suggesting that our results are not driven by unobserved determinants of fertility that drive women to switch treatment status over time.

4.3 Fertility incentives in the context of son preference

Son preference in fertility behavior in Armenia is manifested by higher stopping probabilities by parents who already have sons as well as by sex selection of newborns. These patterns are portrayed in Table 6, based on statistics from the ADHS 2015/16 data set.[24] Panel A of the table shows the probabilities of having an additional child, conditional on the number and sex composition of existing children. For women with one child, the likelihood of having an additional child is only negligibly higher conditional on having a daughter relative to having a son. However, women with two daughters are almost twice as likely to have an additional birth relative to women with two sons or with one son and one daughter. The same pattern appears with respect to women's decision regarding their fourth birth.

Panel B of Table 6 shows the sex ratio at birth by parity and sex composition of existing children. The overall ratio is 120 male births per 100 female births, a value that is substantially higher than the natural rate. For women (or couples) having their second child, the sex ratio at birth of their newborns is noticeably different according to the sex of the previous child. The sex ratio at birth is 116 for births by women who previously had a daughter relative to 96 for births by women who previously had a son. The sex imbalance, however, is particularly pronounced for women with two daughters. They are twice as likely to give birth to a boy relative to a girl.

[23] For this analysis, only the time period 2008-2013 is used. Otherwise, including previous years would yield artificially negative coefficients.
[24] Ebenstein (2013) presents the same analysis with previous rounds of the ADHS.

To assess whether the policy reform impacted stopping behavior, we test for heterogeneity in impact on women who only have daughters relative to those who have at least one son. The first column of Table 7 presents the results of a regression in which the impact coefficient is interacted with an indicator for not having any son. The sample is restricted only to women who already have at least one child (and thus comparable to the second column of Table 3). Among women who already had at least one son, the annual birth probability increased by 1.7 percentage points, statistically significant at the 5% level. These results imply that the reform alleviated stopping behavior by women who already had sons. Among women who previously only had daughters, the annual birth probability increases by 1.1 percentage points. The difference between the two coefficients is not statistically significant. It would not be surprising, however, if the program was less impactful on women without sons, who are anyway more likely to give additional births even without incentives.

Next, we assess whether the policy reform affected the sex ratio at birth. In Columns 2 and 3 we present results of our regression analysis on the outcome of having a son or a daughter. The full sample of ever-married women is used for this analysis. The childbirth grant policy reform similarly increased the annual likelihood of having a son and a daughter by 0.9 percentage points. We therefore cannot reject that the sex ratio among the additional births induced by the program follows the biologically normal rate. Given that the sex ratio at birth for third and higher parities is above 137 male births to 100 female births, a natural sex ratio would imply an improvement in the overall sex ratio. However, given the size of the standard errors, we also cannot reject that the sex ratio among the additional births follow the pre-reform sex ratio.

As shown in Table 6, the sex ratio at birth is particularly imbalanced among newborns to women with two or more children who did not previously have any son. Therefore, we test whether this group responds differently to the reform in terms of sex selection relative to women who already had sons. As shown in columns 4 and 5 of Table 7, we do not find significant heterogeneities in the propensities to have additional boys or girls. In response to the policy change, women with two or more daughters but no sons are more likely to give births to additional sons and daughters by 1.1 (p-value<0.1) and 0.03 (p-value>0.1) percentage points.

In sum, we do not find that the policy reform significantly increased the likelihood of giving birth to daughters. The reform might have had an impact on the sex ratio by increasing fertility among women who previously gave birth to boys and are less likely to engage in sex selection. However, the DHS sample does not provide us with sufficient statistical power to determine such result with confidence and we interpret the change in the sex ratio at birth as suggestive. It is also important to note that we are only measuring impacts on women who previously had two births and therefore make decisions based on the sex composition of the children they already have. Consider a couple with two daughters and a strong preference for a son. If the couple thinks that they will have four instead of three children thanks to the program, it might reduce their likelihood of using sex selective abortion on the third pregnancy because the chance of naturally having a son increases. However, if the reform changes their desired fertility from two to three children, their decision of whether to use sex selective abortion depends only on the strength of their preference for having a son. The Childbirth Grant program might eventually affect sex selection in lower parities if it has a long-run impact on the total fertility rate.

5. Discussion

In countries with rapidly aging populations, policy makers worry about long-term growth opportunities and the sustainability of elder care and pension systems as the share of the working-age population decreases. As a result, many middle-income countries, and almost all transition economies, are implementing policies—and dedicating substantial shares of social spending—to promoting fertility and slowing down the aging of their populations. Notably, research on the impacts of pronatalist policies overwhelmingly focuses on high-income countries, although many of the key factors that drive fertility-related decision-making are likely to differ in other settings.

We find that the 2009 reform of the Armenia Childbirth Grant program, which substantially increased the transfer amount provided for third and higher order births, increased fertility in women eligible for the largest transfers. The estimated impact of the policy is not driven by an increase in birth rates by women with two or more children, but rather by a decrease in birth rates by women with a single child. Under the parallel trends assumption, the policy change was successful at preventing decreased fertility among women eligible for the large transfer amounts. More broadly, our results are in line with previous studies from high-income countries that showed that fertility rates can increase in response to financial incentives.

An important factor when studying fertility behavior in Armenia is its highly pronounced sex imbalance, one of the highest in the world. Previous research has recorded that in societies with strong son preference, sex imbalances materialize as fertility rate decline (e.g. Jayachandran 2017). Therefore, it could have been hoped that the reform's positive impact on birth rates would also reduce the imbalance. The reform increased fertility by women who already had sons - a group with a much higher propensity to stop fertility after two births - whereas it did not impact fertility of women with two or more daughters and no sons. Importantly, women who already have a son are also less likely to engage in sex selection during subsequent pregnancies. Overall, we do not find that the reform significantly increased the likelihood of having daughters. These results suggest that the male-female sex ratio at birth among the additional births is lower than the pre-reform ratio and should attenuate the overall sex imbalance. However, the estimation of the sex-specific birth impacts are not precise enough for us to reject that there was no change in the sex ratio at birth.

Our study has several limitations. First, the policy reform might have theoretically also changed the behavior of women with fewer than two children, our control group. If women with zero or one child believe that the program is not long-lasting, they might change their desired number of children or have their next birth earlier. If such behavior exists, it would bias downwards our estimated impacts. Another limitation is that we only estimate the impact on annual birth rates in the five years immediately following the policy change. We therefore cannot determine whether the total fertility rate is increasing, or whether our results simply represent shifts in the timing of births. Finally, the studied reform was introduced concurrent to the Great Recession. In the analysis of auxiliary data, we do not find evidence that the global financial crisis affected differentially women eligible for the larger transfer from those who are not. Nevertheless, some of the observed patterns might be particular to that point in time. For example, we find that it took a few years for the impact on birth rates to materialize. This might not be the case in a more stable economic environment.

The positive impact of the policy reform on fertility is reassuring given the large amount of resources devoted by middle-income countries to pronatalist policies aimed at slowing population aging. While this study demonstrates that financial incentives can affect fertility decisions in middle-income countries, policy-relevant questions remain to be answered regarding the design of pronatalist incentive schemes. For example, what is the optimal size of the transfer? Are lump-sum transfers at time of birth (like in Armenia or Spain) more effective than monthly subsidies (like in

Israel)? What are the long-term impacts of such schemes? Such questions are likely to gain importance as countries' rates of aging continue to outpace GDP growth.

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Tables and Figures

Table 1: Childbirth grant in Armenia over time

Period	Program	Beneficiary	Size of transfer (AMDs)				
			First Child	Second Child	Third Child	Fourth Child	Fifth+ child
Before 2007	Childbirth One-Off Assistance	FBP [a]	35,000	35,000	35,000	35,000	35,000
	Childbirth Benefit	Universal	35,000	35,000	35,000	35,000	35,000
2007	Childbirth One-Off Assistance	FBP [a]	35,000	35,000	200,000	200,000	200,000
	Childbirth Benefit	Universal	35,000	35,000	35,000	35,000	35,000
2008	Childbirth One-Off Assistance	FBP [a]	35,000	35,000	300,000	300,000	300,000
	Childbirth Benefit	Universal	35,000	35,000	35,000	35,000	35,000
2009-2013	Childbirth One-Off Assistance	FBP [a]	50,000	50,000	50,000	50,000	50,000
	Childbirth Benefit	Universal	50,000	50,000	430,000	430,000	430,000
2014-	Childbirth One-Off Assistance	FBP [a]	50,000	50,000	50,000	50,000	50,000
	Childbirth Benefit	Universal	50,000	50,000	1 million*	1 million*	1.5 million*

Source: Karapetyan (2014).

[a] Family Benefit Program.

* 500,000 AMD at birth, and remainder deposited in a "Family capital" account that can be used for certain family expenditures such as on education and medical expenses.

Table 2: Pre-reform parallel trends test

	Dep. variable: =1 if gave birth in year t			
	Full sample		Excluding childless women	
	(1)	(2)	(3)	(4)
2003 * Two or more	0.006	0.004	0.006	0.002
	(0.014)	(0.014)	(0.012)	(0.012)
2004 * Two or more	-0.003	-0.004	-0.002	-0.003
	(0.013)	(0.014)	(0.011)	(0.011)
2005 * Two or more	-0.002	-0.003	0.007	0.005
	(0.013)	(0.013)	(0.012)	(0.012)
2006 * Two or more	0.001	-0.003	0.003	0.002
	(0.014)	(0.014)	(0.012)	(0.012)
2007 * Two or more	-0.004	-0.006	-0.005	-0.005
	(0.014)	(0.014)	(0.011)	(0.011)
Two or more [a]	-0.239***	-0.100***	-0.178***	-0.092***
	(0.014)	(0.014)	(0.016)	(0.017)
N	33,990	33,766	29,107	28,883
Chi-squared	0.73	0.64	1.49	1.01
Prob>Chi-squared	0.98	0.99	0.91	0.96
Year fixed effects	Yes	Yes	Yes	Yes
Controls for women's characteristics [b]	No	Yes	No	Yes

Note: The table reports average marginal effects from probit regressions run on an annual birth panel for the period 2003-2008 created with the ADHS 2010 and ADHS 2015-16 data, for the sample of ever married women. Each row reports the coefficients for year dummies interacted with the indicator for having at least two children. The omitted category is 2008. Standard errors clustered at the mother level in parentheses below each estimate.

[a] A dummy for having at least two children.

[b] Controls variables include dummies for age categories (15-19, 20-24, 25-29, 30-34, 35-39, 40+), dummies for number of children in specific gender-age groups (number of boys and number of girls aged 0-4, 5-10, 11-18, 19+) and dummies for region of residence.

The Chi-squared statistic tests the joint significance of all the interaction terms together.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Table 3: Impact of the 2009 Childbirth Benefit Program reform on fertility

	Dep. variable: =1 if gave birth in year t		
	(1)	(2)	(3)
Two or more [a] x Post 2009 [b]	0.016***	0.016***	0.014**
	(0.006)	(0.005)	(0.006)
Two or more	-0.097***	-0.091***	-0.111***
	(0.009)	(0.012)	(0.010)
Gave birth in past year	-0.062***	-0.045***	-0.064***
	(0.004)	(0.003)	(0.004)
Urban	-0.020***	-0.018***	-0.018***
	(0.003)	(0.003)	(0.003)
High school education	-0.002	-0.004	-0.005
	(0.006)	(0.005)	(0.006)
Higher education	-0.005	-0.008	-0.005
	(0.007)	(0.006)	(0.007)
N (mother-year observations)	55,539	47,645	52,278
Year fixed effects	Yes	Yes	Yes
Dummies for region of residence	Yes	Yes	Yes
Dummies for previous children composition and women's age categories	Yes	Yes	Yes
Husband's characteristics [c]	No	No	Yes

Note: The table reports average marginal effects from probit on an annual birth panel for the period 2003-2013 created with the ADHS 2010 and ADHS 2015-16 data. Standard errors clustered at the mother level in parentheses below each estimate. Columns (1) and (3) present results from the entire sample of ever married women, while the analysis presented in column (2) restricts the sample to those who have at least one child. All columns control for dummies for age categories (15-19, 20-24, 25-29, 30-34, 35-39, 40+), dummies for number of children in specific gender-age groups (number of boys and number of girls aged 0-4, 5-10, 11-18, 19+) and dummies for region of residence.

[a] A dummy for having at least two children.

[b] A dummy for years after the policy change (2009-2013).

[c] Husband's controls include dummies for age categories (15-19, 20-24, 25-29, 30-34, 35-39, 40+) and dummies for educational level (primary or less -omitted-, secondary or higher). A dummy for marital status at the time of survey is also included.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Table 4: Heterogeneous effects of the 2009 Childbirth Benefit Program reform on fertility

	Dep. variable: =1 if gave birth in year t				
	(1)	(2)	(3)	(4)	(5)
Post 2009 x Two or more	0.020***	0.013	0.018**	0.029	0.018**
	(0.007)	(0.010)	(0.008)	(0.024)	(0.009)
Bottom 20 percent of wealth *x Post 2009 x Two or more*	-0.010				
	(0.012)				
Second wealth tercile *x Post 2009 x Two or more*		0.016			
		(0.015)			
Third wealth tercile *x Post 2009 x Two or more*		0.000			
		(0.016)			
Higher education *x Post 2009 x Two or more*			-0.002		
			(0.012)		
Second tercile of years of education *x Post 2009 x Two or more*				-0.011	
				(0.022)	
Third tercile of years of education *x Post 2009 x Two or more*				-0.012	
				(0.022)	
Urban *x Post 2009 x Two or more*					-0.002
					(0.012)
N	54,820	53,374	54,459	54,267	55,085
Controls for women's characteristics [a]	Yes	Yes	Yes	Yes	Yes

Note: The table reports average marginal effects from probit regressions an annual birth panel for the period 2003-2013 created with the ADHS 2010 and ADHS 2015-16 data, for the sample of ever married women. Standard errors clustered at the mother level in parentheses below each estimate. The first row reports the treatment effect (Post 2009 x Two or more) for the omitted group, which corresponds to women in the first wealth tercile in column (1), women in the top 4 quintiles of wealth in column (2), women with secondary education or less in column (3), women in the first tercile of years of education in column (4) and women in rural areas in column (5). Also included (but not reported) are the interactions of each characteristic with the Post 2009 dummy, the Two or more dummy, year dummies, and all other control variables.

[a] Control variables as in Table 3, column (1) are included but not reported.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Table 5: Robustness checks

	Dep. variable: =1 if gave birth in year t							
			Linear probability model [b]			Fixed comparison group		
	Excluding year 2008 [a]	Excluding year 2009 [a]	Full sample	Exclude childless women	Full sample	Full sample	Exclude childless women	Full sample
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post 2009 x Two or more	0.017***	0.023***	0.020***	0.027***	0.016**	0.030**	0.034***	0.032**
	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)	(0.015)	(0.013)	(0.015)
Two or more [d]	-0.096***	-0.096***	-0.121***	-0.136***	-0.136***	-0.055***	-0.050***	-0.067***
	(0.010)	(0.009)	(0.008)	(0.007)	(0.008)	(0.013)	(0.014)	(0.014)
N	49,603	49,529	56,340	48,446	53,049	19,363	16,909	17,581
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for women's characteristics [e]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls for husband's characteristics [f]	No	No	No	No	Yes	No	No	Yes

Note: The table reports results from regressions run on an annual birth panel for the period 2003-2013 created with the DHS 2010 and DHS 2015-16 data, for the sample of ever married women. Standard errors clustered at the mother level in parentheses below each estimate.

[a] Average marginal effects from probit regression excluding data from the mentioned year.

[b] Regression coefficients from a linear probability model.

[c] A dummy for years after the policy change.

[d] A dummy for having at least two children

[e] Control variables as in Table 3, column (1) are included but not reported.

[f] Control variables as in Table 3, column (3) are included but not reported.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Table 6: Stopping probability and sex ratio at birth

Parity	Sex combination	DHS 2015			
		Panel A		Panel B	
		Percent who have another child		Sex ratio (of next birth)	
		Mean	Obs.	Mean	Obs.
1st	None			107	4,082
2nd	One boy	0.77	2,118	96	1,658
	One girl	0.79	1,964	116	1,567
3rd	Two boys	0.30	833	104	266
	One girl, one boy	0.27	1,664	109	466
	Two girls	0.56	728	200	419
4th	Three boys	0.23	132	164	27
	One girl, two boys	0.15	380	145	61
	Two girls, one boy	0.15	498	156	74
	Three girls	0.50	141	181	71

Note: The sample includes all ever-married women aged 15-49 at the time of the survey. Source: ADHS 2015-16.

Table 7. Impact of the childbirth benefit and sex imbalance

Dependent variable	Gave birth	Gave birth to a boy	Gave birth to a girl	Gave birth to a boy	Gave birth to a girl
	(1)	(2)	(3)	(4)	(5)
Post 2009 [a] x Two or more [b]	0.017**	0.009*	0.009	0.015**	0.003
	(0.007)	(0.005)	(0.005)	(0.006)	(0.005)
Two or more	-0.113***	-0.049***	-0.054***	-0.064***	-0.050***
	(0.014)	(0.006)	(0.007)	(0.009)	(0.011)
All daughters [c]	0.003			0.004	0.002
	(0.009)			(0.006)	(0.007)
Post 2009 x Two or more x All Daughters	-0.006			-0.004	-0.0002
	(0.010)			(0.007)	(0.008)
Post 2009 x All Daughters	-0.004			-0.002	-0.001
	(0.006)			(0.005)	(0.005)
Two or more x All Daughters	0.052***			0.046***	0.006
	(0.012)			(0.011)	(0.007)
N	47,645	56,000	54,749	48,106	46,855
Linear combination: (Post 2009 x Two or more) + (Post 2009 x Two or more x All daughters)	0.011			0.011*	0.003
	(0.008)			(0.006)	(0.007)
P-val. of the diff. (Post 2009 x Two or more)		0.980		0.175	
P-val. of the diff. (Post 2009 x Two or more x All dgts.)				0.750	
P-val. of the diff. (linear combination)				0.533	
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls for women's characteristics [d]	Yes	Yes	Yes	Yes	Yes

Note: The table reports results from regressions run on an annual birth panel for the period 2003-2013 created with the DHS 2010 and 2015-16 data, for the sample of ever married women. Standard errors clustered at the mother level in parentheses below each estimate.

[a] A dummy for years after the policy change.

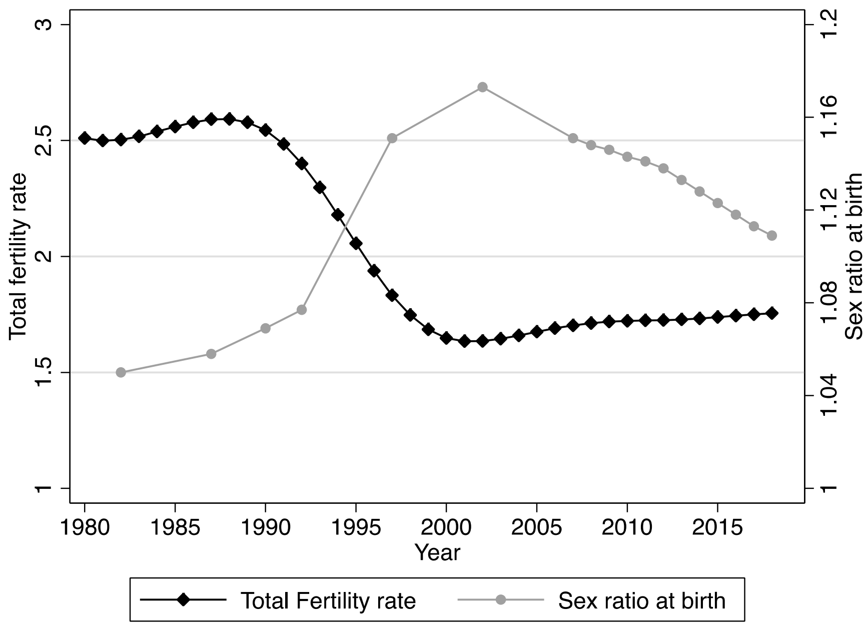
[b] A dummy for having at least two children.

[c] A dummy for having only daughters (no son).

[d] Control variables as in Table 3, column (1) are included but not reported.

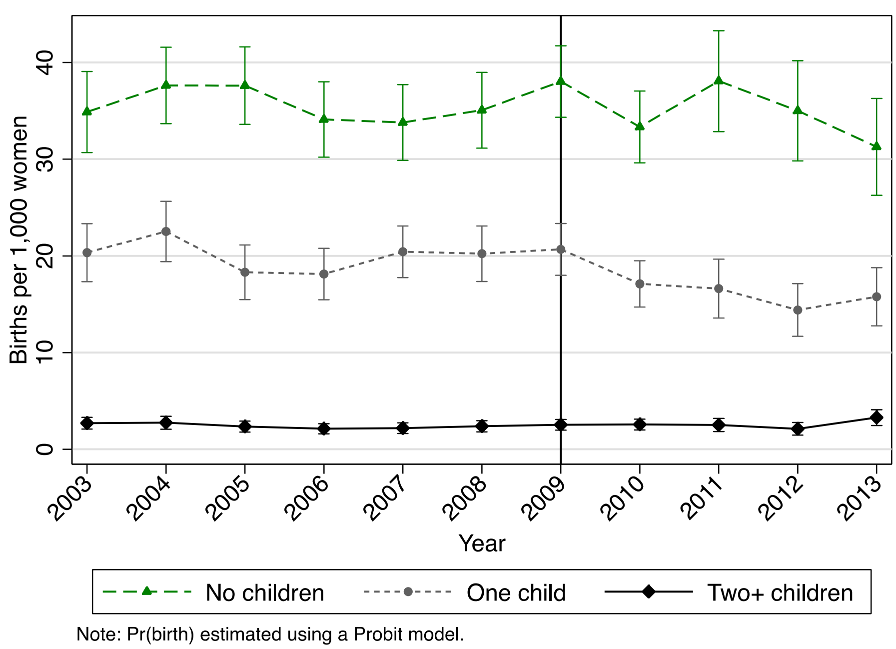
*** Significant at 1%; ** significant at 5%; * significant at 10%.

Figure 1. Fertility rate and sex ratio at birth in Armenia



Note: Source: World Development Indicators, accessed on July 2020.

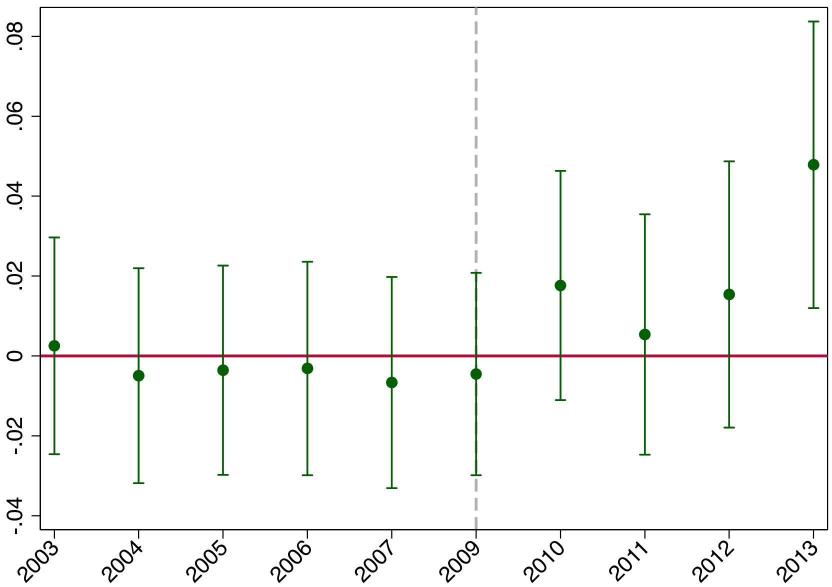
Figure 2. Birth rates, 2003-2013



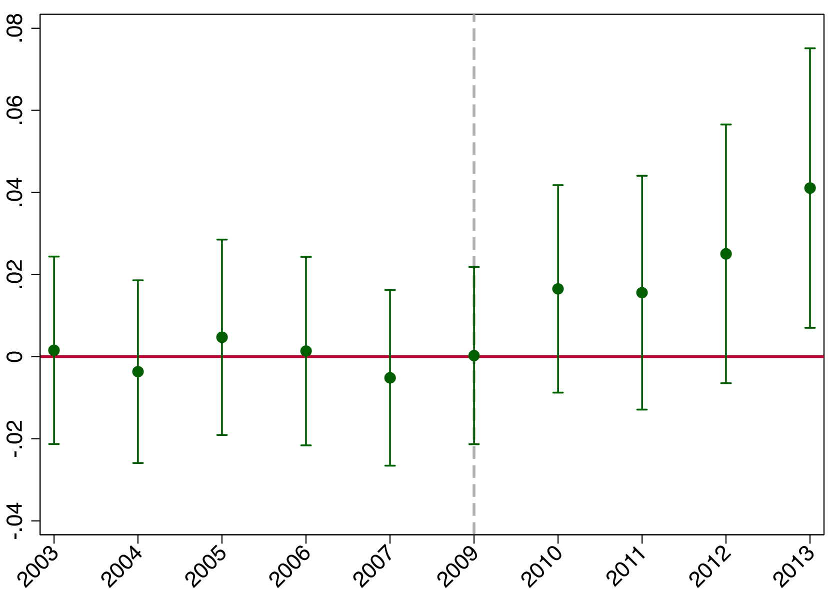
Note: The figure shows the number of ever married women giving birth in a given year per 1,000 women. Source: ADHS 2015-16.

Figure 3. Fertility results, dynamic difference in difference

Panel A. Full sample



Panel B. Excluding childless women



Note: The figures report average marginal effects from probit regressions run on an annual birth panel for the period 2003-2013 created with the DHS 2010 and DHS 2015-16 data, for the sample of ever married women. The dots and bars represent the coefficients and 95% confidence intervals for year dummies interacted with the indicator for having at least two children. All regressions include year fixed effects, a dummy for having at least two children, and controls as in Table 3 column (1). The omitted category is 2008. Standard errors clustered at the mother level.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Appendix Tables

Table A1: Summary statistics

	Full sample		By number of children			
			Less than two		Two or more	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Characteristics observed at time of survey						
Children born	2.0	1.0	0.7	0.4	2.5	0.7
No children	0.1	0.3	0.3	0.4	0.0	0.0
One child	0.2	0.4	0.7	0.4	0.0	0.0
Two or more children	0.7	0.4	0.0	0.0	1.0	0.0
Age at first birth	22.4	3.6	24.2	4.2	21.9	3.2
Sons born	1.1	0.8	0.4	0.5	1.3	0.8
Daughters born	0.9	0.9	0.3	0.5	1.2	0.9
Age	35.1	7.9	30.1	7.7	37.0	7.1
Married	0.9	0.3	0.8	0.4	0.9	0.3
Education - Primary	0.1	0.2	0.03	0.2	0.06	0.2
Education - Secondary	0.4	0.5	0.35	0.5	0.44	0.5
Education - Higher	0.5	0.5	0.61	0.5	0.50	0.5
Years of education	11.8	2.3	12.3	2.3	11.7	2.3
Bottom 40 percent of wealth score	0.4	0.5	0.3	0.5	0.4	0.5
Bottom 20 percent of wealth score	0.2	0.4	0.2	0.4	0.2	0.4
Employed	0.4	0.5	0.3	0.5	0.4	0.5
Worked abroad	0.0	0.2	0.0	0.3	0.0	0.2
Urban	0.6	0.5	0.7	0.5	0.6	0.5
Husband's age	39.3	8.8	32.8	8.0	41.5	7.9
Husband education - Primary	0.1	0.3	0.1	0.3	0.1	0.3
Husband's education - Secondary	0.5	0.5	0.4	0.5	0.5	0.5
Husband's education - Higher	0.4	0.5	0.5	0.5	0.4	0.5
Husband worked abroad	0.2	0.5	0.2	0.5	0.2	0.5
Panel B: Birth probability generated from retrospective birth histories						
Probability of giving birth	0.1	0.3	0.2	0.4	0.0	0.1
Probability of giving birth to a 1st child	0.3	0.5				
Probability of giving birth to a 2nd child	0.2	0.4				
Probability of giving birth to a 3rd child	0.0	0.1				
Probability of giving birth if has a son	0.04	0.2	0.2	0.4	0.0	0.1
Probability of giving birth if has no son	0.13	0.3	0.2	0.4	0.1	0.2

Note: The sample includes all ever-married women aged 15-49 at the time of each survey. The total number of observations is 8,449 (2,179 with less than two children and 6,270 with two or more). Statistics referring to husbands' characteristics are based on a subsample of women married at the time of survey (7,641 observations). The unit of observation in the last panel is woman-year, with a total of 72,468 observations (23,292 with less than two and 49,176 with two or more children). Source: DHS 2010 and DHS 2015.

Table A2: Impact of the 2009 Childbirth Benefit Program reform on fertility, dynamic difference in difference specification

	Dep. variable: =1 if gave birth in year t		
	(1)	(2)	(3)
2003 * Two or more	0.003	0.002	0.001
	(0.014)	(0.012)	(0.014)
2004 * Two or more	-0.005	-0.004	-0.003
	(0.014)	(0.011)	(0.014)
2005 * Two or more	-0.004	0.005	-0.001
	(0.013)	(0.012)	(0.014)
2006 * Two or more	-0.003	0.001	-0.003
	(0.014)	(0.012)	(0.014)
2007 * Two or more	-0.007	-0.005	-0.004
	(0.013)	(0.011)	(0.014)
2009 * Two or more	-0.005	0.000	-0.005
	(0.013)	(0.011)	(0.013)
2010 * Two or more	0.018	0.017	0.019
	(0.015)	(0.013)	(0.015)
2011 * Two or more	0.005	0.016	0.002
	(0.015)	(0.015)	(0.016)
2012 * Two or more	0.015	0.025	0.011
	(0.017)	(0.016)	(0.017)
2013 * Two or more	0.048***	0.041**	0.048***
	(0.018)	(0.017)	(0.019)
N	55,539	47,645	52,278
Controls for women's characteristics [a]	Yes	Yes	Yes
Controls for husband's characteristics [b]	No	No	Yes

Note: The table reports average marginal effects from probit regressions run on an annual birth panel for the period 2003-2013 created with the DHS 2010 and DHS 2015 data. Each row reports the coefficients for year dummies interacted with the indicator for having at least two children. All columns include year fixed effects as well as a dummy for having at least two children. The omitted category is 2008. Standard errors clustered at the mother level in parentheses below each estimate. Columns (1) and (3) are run on the entire sample of ever married women, while column (2) restricts the sample to those who have at least one child.

[a] Control variables as in Table 3, column (1) are included but not reported.

[b] Control variables as in Table 3, column (3) are included but not reported.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Table A3: Testing for differential economic-demographic changes by number of children, ILCS 2004-2012

	Dependent variable:			
	Married	Employed	Married to an employed husband	Internationa l migrant in household
	(1)	(2)	(3)	(4)
Post 2009 [a] x Two or more [b]	0.003	0.003	-0.011	0.019**
	(0.011)	(0.010)	(0.009)	(0.009)
Two or more	-0.060***	-0.003	-0.049***	-0.024***
	(0.011)	(0.011)	(0.009)	(0.008)
Number of observations	52,128	51,636	47,234	54,289
Year fixed effects	Yes	Yes	Yes	Yes
Controls for women's characteristics [c]	Yes	Yes	Yes	Yes

Note: The table reports average marginal effects from probit regressions. Robust standard errors in parentheses below each estimate. Each column refers to a different outcome variable: a dummy for being married (column 1), a dummy for being employed (column 2), a dummy equal to one for women married to an employed husband and zero for non-married women or women married to a non-employed husband (column 3) and a dummy for living in a household with at least one member who migrated internationally in the preceding year (column 4). The sample is restricted to have the same age composition as in the DHS panel. Source: ILCS 2004-2012.

[a] A dummy for years after the policy change (2009-2013).

[b] A dummy for having at least two children.

[c] Women's characteristics include dummies for age categories (15-19, 20-24, 25-29, 30-34, 35-39, 40+), whether the woman had birth during the last year, dummies for educational level (primary or less -omitted-, secondary or higher), dummies for number of children in specific gender-age groups (number of boys and number of girls aged 0-4, 5-10, 11-18, 19+), a dummy for urban/rural and dummies for region of residence.

*** Significant at 1%; ** significant at 5%; * significant at 10%.

Table A4: Testing for differential economic-demographic changes by household wealth, ILCS 2004-2012

	Dependent variable:			
	Married	Employed	Married to an employed husband	Int'l migrant in hh.
	(1)	(2)	(3)	(4)
Post 2009 [a] x Bottom 20 [b]	0.010	0.023*	0.038***	0.034***
	(0.011)	(0.012)	(0.011)	(0.011)
Bottom 20	-0.010	0.036***	-0.024***	-0.021***
	(0.009)	(0.009)	(0.008)	(0.008)
Number of observations	52,128	51,636	47,234	54,289
Year fixed effects	Yes	Yes	Yes	Yes
Controls for women's characteristics [c]	Yes	Yes	Yes	Yes

Note: The table reports average marginal effects from probit regressions. Robust standard errors in parentheses below each estimate. Each column refers to a different outcome variable: a dummy for being married (column 1), a dummy for being employed (column 2), a dummy equal to one for women married to an employed husband and zero for non-married women or women married to a non-employed husband (column 3) and a dummy for living in a household with at least one member who migrated internationally in the preceding year (column 4). The sample is restricted to have the same age composition as in the DHS panel. Source: ILCS 2004-2012.

[a] A dummy for years after the policy change (2009-2013).

[b] A dummy for women living in a household in the bottom 20 percent of the household wealth distribution for each year.

[c] Women's characteristics include dummies for age categories (15-19, 20-24, 25-29, 30-34, 35-39, 40+), whether the woman had birth during the last year, dummies for educational level (primary or less -omitted-, secondary or higher), dummies for number of children in specific gender-age groups (number of boys and number of girls aged 0-4, 5-10, 11-18, 19+), a dummy for urban/rural and dummies for region of residence.

*** Significant at 1%; ** significant at 5%; * significant at 10%.