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Maybe Baby in Armenia

The Effects of Cash Transfers on Fertility

Jens Ek Deneberg (25613) and Kim Vulic (26050)

Abstract

This paper estimates the effect of Armenia's 2014 reform to the Universal Childbirth Benefit Program on fertility. The program provides a one-time lump-sum cash transfer at childbirth, with amounts increasing by birth order. The 2014 reform raised benefits for third- and fourth-order births, while the second-order birth benefit remained unchanged. Using household survey data between 2012–2018, birth histories were reconstructed to build a woman-year panel for women aged 15–35. The analysis applied a difference-in-differences design, comparing women with two or three children to women with one child. We find that the reform increased the probability of an additional birth by 2.12 percentage points for the treatment group relative to the control group, corresponding to a 47.6% increase over the pre-reform baseline. A back-of-the-envelope calculation implies a cost of about USD 5,860 per additional policy-induced birth. Robustness checks across alternative specifications support the qualitative robustness of the results. While the policy appears effective at increasing births, our findings suggest that financial incentives alone are unlikely to reverse broader demographic decline. The study informs pronatalist policy design and contributes to existing literature on fertility responses to financial incentives.

Keywords: Armenia; fertility; cash transfers; pronatalist policy; minor field study
JEL: J13; I38; C23

Supervisor:	Abhijeet Singh
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Discussants:	Jonathan Rasmusson & Lena Kristoffersson
Examiner:	Johanna Wallenius

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Contents

1 Introduction

In recent decades, the world has experienced a rapid decline in global fertility rates. A UN report on world fertility shows that since the 1960s, the total fertility rate (hereinafter TFR) has declined by more than half, from an average of 5 births per woman to 2.2 births per woman in 2024 (United Nations Department of Economic and Social Affairs, 2025). In more than half of these countries, the fertility rate is below the population replacement level of 2.1 births per woman – a trend prominent across all regions and income groups. This decline threatens to shift the weight of the population structure toward older age groups and raises concerns about long-term productivity and economic growth (OECD, 2024). In response, governments are adopting a range of interventions designed to counteract this trend. However, due to the difficulty of establishing causality, the effectiveness of these policies remains uncertain.

Armenia is among the countries with exceptionally low fertility rates and has implemented a range of policies aimed at increasing population growth. Over the past three decades, the fertility rate in Armenia has dropped by more than 30% (Pinto et al., 2021). Simultaneously, the country has seen a net negative migration for every year since the fall of the Soviet Union in 1991, except for a positive surge of Russian immigrants in 2022–2023 following the war in Ukraine and the influx of ethnic Armenians following the most recent war in Nagorno-Karabakh (Macrotrends, 2026b; Statistical Committee of the Republic of Armenia [Armstat], 2023). The combination of high emigration and low fertility rates poses a severe challenge for Armenia, and the government has classified the stabilization and increase of its population as a long-term strategic goal.

This paper explores one of the policies adopted by the Armenian government to increase fertility rates. The policy studied is a reform of the Universal Childbirth Benefit Program (hereinafter, UCBP), a nationwide birth allowance program providing lump-sum cash transfers to parents upon the birth of a child. In 2014, the structure of the UCBP changed, increasing the one-time cash transfer to third- and fourth-order births, while the benefit for first- and second-order births remained unchanged. This arrangement presents a natural setting for studying the effects of the benefits on higher-order births. The analysis uses data from the Integrated Living Conditions Survey (ILCS) for the period 2012–2018. Specifically, following the methodology outlined by Pinto et al. (2021), a panel dataset with retrospective birth histories was constructed. A difference-in-differences strategy was employed, with age-, marz[1]-, and year-fixed effects, comparing mothers with two or three children to mothers with only one child. In our preferred specification, the results show that the reform increased the probability of having another child by 2.12 percentage points for the treatment group relative to the control group. A back-

[1] A marz is an administrative unit in Armenia, equivalent to a province or region. There are 11 marzes in the country, including Yerevan.

of-the-envelope calculation shows that the policy induced 593 extra births per year over the post period, and the effective cost of each additional birth was approximately AMD 2.2 million, or equivalently USD 5,860. The effect is heterogeneous across consumption expenditure, poverty status, educational attainment, labor force status, and settlement type. The strongest effects are observed among unemployed and very poor women, though these results need to be interpreted carefully. The model is tested through various robustness checks, which consistently show a positive, significant effect of the policy.

The rest of this paper is organized as follows. Section 2 provides a background to fertility trends in Europe and in Armenia, and presents the structure of the UCBP. Section 3 presents an overview of the existing literature. Next, section 4 outlines the methodology employed. Section 5 presents the main results, including heterogeneity analyses and robustness checks. Finally, Sections 6 and 7 discuss and conclude the findings.

2 Background

2.1 A Decreasing Population Growth: Examples from Europe

The TFR in Europe has been declining since the 1960s, reaching a historical low of approximately 1.34 births per woman in 2024 (Eurostat, 2026). The decline in fertility rates during this period is mirrored by a substantial reduction of total births in the EU, from its highest of 6.8 million total births per year in 1964 to about 3.55 million in 2024 (Eurostat, 2026). Whereas in earlier decades the decline in total fertility rates was largely confined to Southern Europe, it is now observed across the entire continent. Countries that historically have maintained more stable fertility levels, such as France and Sweden, are seeking policy responses to the challenge posed by an aging population (Sveriges Radio, 2026). Yet the underlying causes of declining fertility rates remain ambiguous, and how to construct effective policy interventions is disputed among policymakers and scholars.

One common type of pronatalist reform is providing financial incentives to parents, a popular measure in Europe. Two illustrative examples include Hungary and Poland. Hungary allocates about 6% of its GDP on pronatal policies, which include various monetary and tax benefits given to parents (Zeman et al., 2024). Starting from a TFR of 1.25 births per woman in 2010, one decade later, the same number was up at 1.51 births per woman (Zeman et al., 2024). This need not be solely attributable to policy: substantial emigration and shifts in the population age structure reduced the number of women of reproductive age between 2010 and 2020, thereby mechanically increasing the TFR. In absolute terms, Hungary recorded 92,000 live births in 2020, only 2,000 more than in 2010. Similarly, Poland launched an ambitious pronatalist program in 2015, when the country had the lowest TFR in the whole of the EU (Bokun, 2024). The 500+ benefit program, although being successful in decreasing extreme child poverty, failed to raise fertility rates (Ekert, 2022). The cases of Hungary and Poland suggest that even extensive financial support for families may be insufficient to address low fertility rates, raising questions about the effectiveness of this policy instrument.

2.2 The Case of Armenia

Armenia's demographic structure has undergone substantial transformation during recent decades. In particular, the country saw the most pronounced changes in fertility rates following the fall of the Soviet Union. Childbearing costs, previously largely subsidized by the state, increased for families (Pinto et al., 2021). In parallel, tensions with Azerbaijan increased to the point of an outbreak of a full-scale war. These developments reduced households' economic security and forced some families to relocate or leave the country, factors that have been found to decrease fertility among individuals in the affected groups (Ebenstein, 2013). As a result, from 1990 to 2001, the TFR in Armenia declined by approximately 56% (Macrotrends, 2026a). Although fertility levels have increased since then – from 1.2 births per woman in 2001 to 1.7 in 2024 – they remain below the population replacement threshold of 2.1 and are the lowest in the region (Armstat, 2026). Looking at total live births, the number decreased over the same period, from 37,520 in 2004 to 33,539 in 2024, despite an increase in the TFR (Armstat, 2005, 2025). The sustained negative net migration since the 1990s has further intensified the population decline in Armenia and reshaped the country's demographic structure. Today, Armenia has one of the highest emigration rates in the world, with over 30% of its citizens living in another country (International Labour Organization, 2021, p. 21).

In response to the country's demographic challenges, the Armenian government has framed population growth as a strategic national priority. Ambitious population targets have been set to reach 5 million citizens by 2050 (Armenpress, 2020). Multiple efforts have been made to reach this goal, including reforming the UCBP in 2009. Before that year, flat childbirth grants were given to mothers universally, but the amounts were relatively small (AMD 35 thousand) and did not vary by birth order. To encourage women to have more children, the program was reformed in 2009, increasing benefit amounts and introducing tiered benefits based on birth order. For the first and second child, the sum was set to AMD 50 thousand, whereas for the third and subsequent children it was set to AMD 430 thousand. In 2014, the UCBP was reformed again. For the third and fourth children, the benefit was increased to AMD one million, and for the fifth and subsequent children, it increased to AMD 1.5 million. Additionally, a family capital fund was introduced, which entailed that for third and subsequent children, only half or one third of the money would be transferred to the parents at the time of birth, while the remainder was kept in a state-owned fund until the child turned 18 (Government of Armenia, 2014, p. 96).

There is currently limited quantitative evidence on the effect of the 2014 reform on fertility. However, aggregate vital statistics on the distribution of live births by birth order suggest a structural change following its introduction. Before 2014, second-order births (for which benefits remained unchanged) and third- and fourth-order births (for which benefits increased) exhibited similar trends. After 2014, these trends diverged: third- and fourth-order births increased markedly, while second-order births declined (see figure 1). This apparent break is consistent with differential behavioral responses across groups and motivates a formal analysis of the causal impact of the 2014 UCBP reform on fertility.

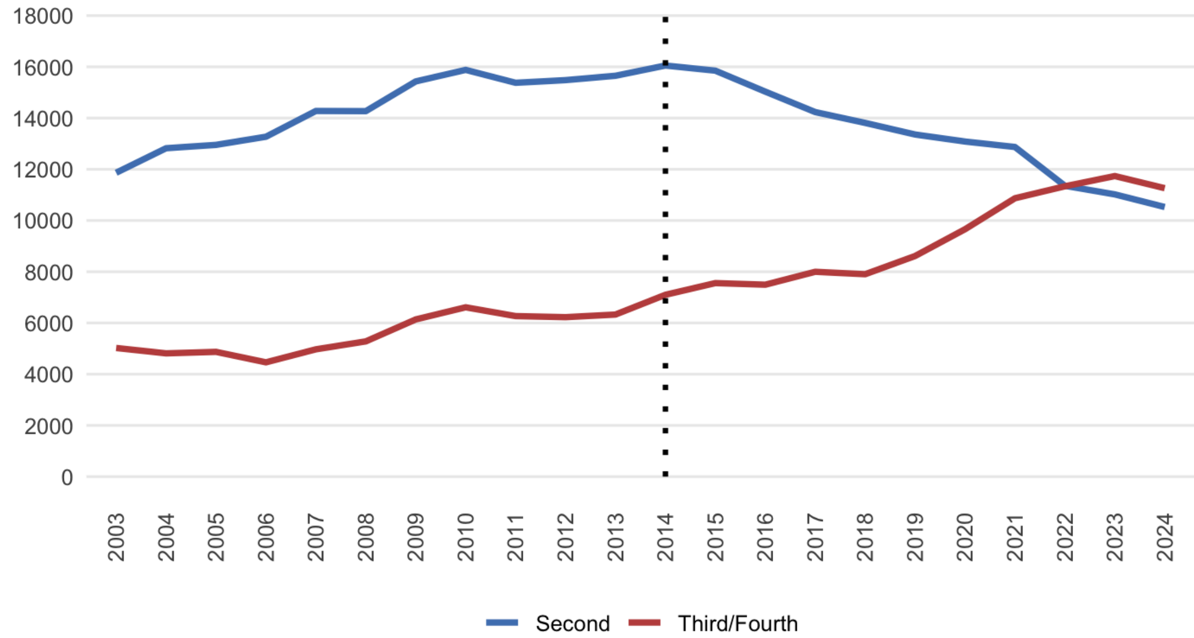

Figure 1: Distribution of live births for Second and Third/Fourth order births based on aggregate vital statistics from multiple rounds of the Demographic Handbook of Armenia, accessible via the Armstat website. The dotted line in 2014 marks the introduction of the policy studied. *Source*: The Demographic Handbook of Armenia 2009, 2014, 2025.

3 Previous Research

Traditional economic theories on fertility behavior, such as those of Becker (1960), propose that the decision to have a child depends on the opportunity cost of childbearing and upbringing in relation to the increase in utility provided by the child. According to this framework, an increase in birth allowances or a reduction in pregnancy costs would thus be expected to increase the willingness to have a child. This, in turn, implies that pronatalist policies in the form of financial incentives should stimulate the fertility rate. However, empirical evidence on this is mixed. In Australia, the effect of introducing a one-time cash transfer to mothers giving birth did not significantly increase fertility among

Australian women. However, for immigrant women, the effect was significant and stong: an increase in fertility of around 9–18%. Moreover, the effect was more pronounced in younger age groups with lower levels of human capital (Bonner & Sarkar, 2020). Evidence from Canada, on the other hand, shows that an introduced monetary transfer policy was especially effective for women with higher household income. An increase in family income of CAD 100 thousand increased policy responsiveness from 3.6 to 11.4 percentage points. The average effect of the policy was an increase of around 12% from pre-treatment birth probabilities (Milligan, 2005). The average effect of financial incentives on childbearing varies across countries and settings, too. Several studies point out that where cash transfers exhibit a positive effect on fertility, the magnitude is moderate or small, and does not persist in the long-term (see, for example, Chuard & Chuard-Keller, 2021; González & Trommlerová, 2023).

Beyond financial incentives and Becker's theory of the opportunity cost of having a child, other types of pronatalist policies have been tested. Using cross-country data, both Kalwij (2010) and OECD (2024) suggest that provisions for childcare, parental leave, and general work and family policies are more strongly associated with positive fertility outcomes compared to direct financial incentives. However, these studies fail to propose an alternative full model of explanation for fertility behavior.

Neyer et al. (2022) and Andersson (2026) give more in-depth insight into the decision to have a child. Based on the case of Sweden, they show that the decline in fertility rates is a consequence of changes in attitudes toward childbearing. Cohabitating couples are much less likely today to take on long-term commitments, such as marrying or having children. Andersson et al. suggest that this is because of global uncertainty, rising political populism, and increasing mental health problems among young adults. The decrease in TFR is thus primarily driven by an increase in couples deciding not to have children at all, rather than by an increase in couples wanting fewer children. In effect, the propensity to have another child among women who have already given birth to their first child seems to remain unchanged. The trend is homogeneous across municipalities, socio-economic, and socio-demographic groups. The results from Andersson et al. suggest that policies targeting higher-order fertility may be less effective for changing TFR.

Although the lion's share of research on financial incentives and fertility rates focuses on high-income countries, some papers examine the context of low- and middle-income countries. In the case of Armenia, the most relevant paper for our study is *Financial Incentives, Fertility, and Son Preference in Armenia* by Pinto et al. (2021). This was the first study to analyze the effects of lump-sum cash transfers to increase fertility in the country. Using data from the Armenian Demographic and Health Survey (DHS), the authors investigate the effects of the 2009 reform of the UCBP. Exploiting the timing of the policy and differences in benefit levels by birth order, they employ the same conceptual difference-in-differences methodology that inspired this study. Pinto et al. find that the

2009 reform increased the probability of having another child by 1.4 to 1.6 percentage points in the treatment group relative to the control group in the five years following the reform, or equivalently, a 58–64% increase relative to the pre-reform base probability. Because the increase in benefits under the 2014 reform was larger than under the 2009 reform, we would expect a larger response to the latter policy, disregarding potential non-linear marginal effects.

This paper contributes to the previous literature in the following dimensions. First, it adds to the already existing literature on financial incentives for increasing higher-order fertility. While a substantial body of research has examined cash-transfer policies, most evidence is based on high-income countries where fertility behavior and constraints may differ significantly from those in lower-income or transitional economies (see Gauthier, 2007; Olivetti & Petrongolo, 2017, for an overview of the effect of financial incentives to boost fertility in high-income countries). As such, there is limited research on how financial incentives operate in developing countries, particularly in post-Soviet countries. This study contributes to filling that research gap.

Furthermore, this is the first economic study to evaluate the effect of the 2014 UCBP reform on fertility rates. The results can be compared with those of the 2009 reform to provide a broader understanding of the marginal effects of the size of lump-sum cash transfers.

Moreover, the paper is the first of its kind to employ a methodology based on reconstructed birth histories derived from the ILCS dataset, a cross-sectional survey not primarily aimed at pure demographic research.

Finally, as of September 1, 2026, the Armenian government has decided to abolish the current structure of the UCBP and replace it with a flat provision of AMD 500 thousand regardless of birth order (Government of the Republic of Armenia, 2025). Understanding the dynamics of previous policy changes is therefore particularly relevant for ongoing and future adjustments to the reform.

4 Methodology

4.1 Data and Sample Construction

The data used for this study are drawn from seven survey waves of ILCS conducted between 2012 and 2018. The nationally representative annual survey is conducted by the Statistical Committee of the Republic of Armenia ("Armstat") to produce reliable data on income, expenditures, and living conditions of Armenian households ("Armenia - Integrated Living Conditions Survey", 2018). Survey data are categorized at the household and individual household member levels. For each year, there exist two separate anonymized survey datasets, accessible via the Armstat's website (Armstat, n.d.).

A repeated cross-sectional (RCS) dataset was created from household-level (38,880 observations) and individual-level (134,635 observations) data collected from 2012 to 2018. Descriptive statistics for the RCS datasets are found in the appendix, table 7. Out of 134,635 observations, the average age in the full RCS sample is 38.1 years. Among the individuals in the sample, 85.1% are employed, and 18.2% have a tertiary education. The sample consists of 54.9% women. The mean household size is 3.7 persons with a total average income of approximately AMD 144 thousand per month. Urban residents account for 65.6% of the smaple.

The household-level data were attributed to each household member by survey year and household number. Names and values of variables used in the analysis were standardized across years. The sample was then restricted to female respondents, and variables were constructed for the number of children and the sex and age composition of those children. Critically, the data do not contain a linkage between a mother and her children. Moreover, there exists no variable for the number of children a mother has. A reasonable baseline proxy for the number of children born is the number still living in the mother's household. As it is not uncommon in Armenia to live in multi-generational households, or households with multiple, unrelated families (so-called "complex households"), additional effort was made to match each woman with her children. Specifically, it was assumed that a person coded as "child" is the child of the household head and the household spouse. A person coded as "grandchild" is matched with a married daughter or daughter-in-law. Where a child could not be perfectly matched to its mother, the observation was dropped.

The proxy for a mother's total number of children – defined as the number of her children still living in the household – has two main limitations. First, it does not take into account deceased children or children who are placed in foster care. Neonatal and infant mortality affects fewer than 2% of births; while this cannot be accounted for in the data, the resulting measurement error is likely small[2]. Second, and more importantly, once a child moves out of the parents' household, the mother will be classified as having one less child than she actually has. This leads to some mothers being misspecified as part of the control group when they, in fact, are in the treatment group. The second issue with the proxy is illustrated in the appendix, figure 6, where age distributions are plotted for treatment and control groups in a naive final sample that does not include an age restriction. The probability of having more than one child still living at home significantly drops as parents and children become older. Hence, the proxy naturally weakens with age, and an age restriction must be included, following the approach of Milligan (2005).

As the age restriction attempts to correct the bias introduced by the proxy, it is important that the age band is chosen carefully. Therefore, a separate analysis was

[2]Derived from the Armenia DHS 2015–2016. The obtained mortality rate is assumed to be representative of the entire study period.

conducted to estimate the age of a mother at which her first child moves out of the household. This analysis used the Armenian Demographic Health Survey (DHS) from 2015–2016, a demographic survey with comprehensive information on ever-born children of each female respondent, as well as the number of children still living in the household. The analysis showed that up until the age of 35, the fraction of women with at least one child living outside the household is close to zero, as shown in figure 2. It is assumed that the 2015–2016 DHS survey results are representative of Armenia across years. The upper age limit was thus set to 35 and applied to every observation at the time of the survey wave. This is similar to Milligan's study, which set the age restriction to 34 (Milligan, 2005). The lower bound of the age restriction was set to 15, the most widely accepted first year of a female's reproductive age. The age distribution of the final sample using the age restriction is illustrated in the appendix, figure 7.

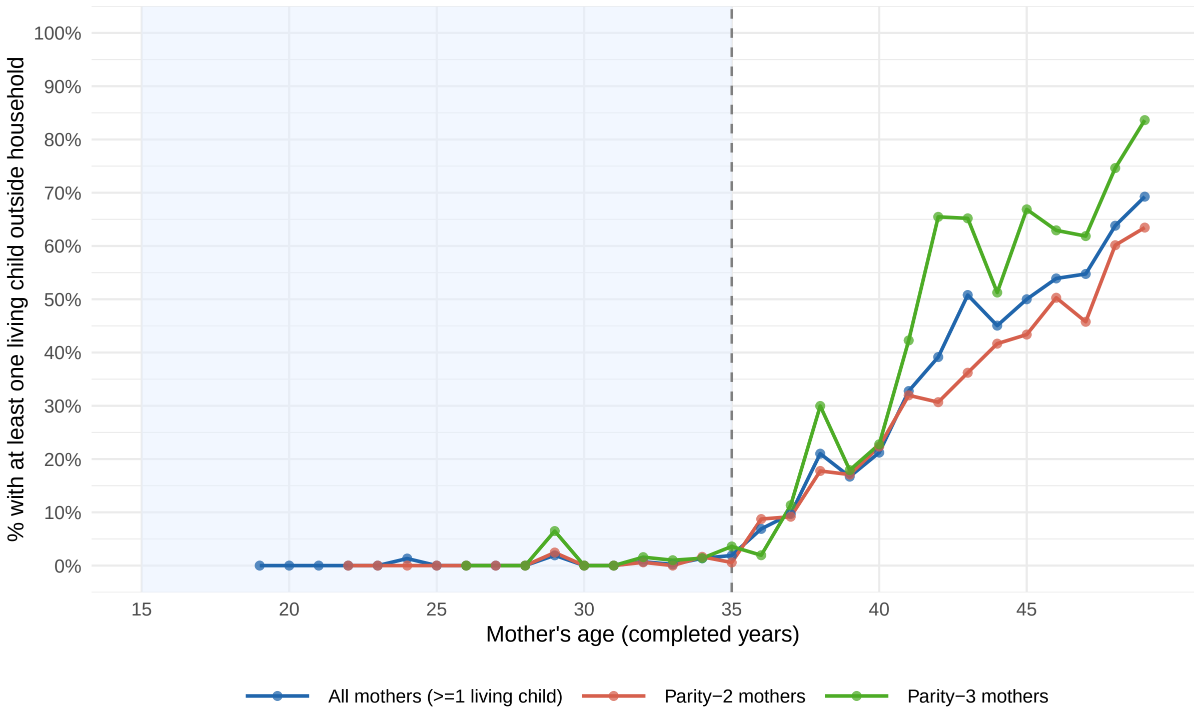

Figure 2: Share of mothers for whom the ILCS household-children proxy understates parity. Dashed line = age 35. The full sample as well as two subsamples including mothers with 2 or 3 children, respectively, are shown. At age 35, around 1% of mothers have at least one child living outside the household, suggesting that the proxy up until that age is essentially unbiased. *Source*: Armenia DHS 2015–16 (survey-weighted).

The RCS dataset was then expanded into a balanced panel of women-year observations. Specifically, each woman surveyed in year t was repeated retrospectively until calendar year 2009. Using each child's calendar age, retrospective birth years were imputed for mothers. For each survey wave, all women were repeated an equal number of times, thus creating a balanced panel consisting of 71,523 woman-year observations. The age restriction of 15–35 was set at the time of the survey wave rather than at each calendar year to keep the panel balanced. Statistical weights were split across the number of calendar years a woman was observed. Calendar years 2009–2011 are imputed from children's ages at interview and define parity and births at the start of each calendar year; they are not additional survey waves. Restricting the calendar window to 2012+ would shorten the panel and rescale panel weights without changing the definition of parity at 2012 for retained rows. We retain 2009–2011 for pre-reform calendar coverage and stable weighting.

The study's time frame was chosen to estimate the medium-run effects of the reform. In 2009, Armenia implemented the first reform to the UCBP, which moderately increased birth allowances. Thus, the study period starts in 2009 to avoid confounding the analysis with the effects of the previous reform. 2018 was chosen as the last year of the study because Armstat made major changes to the ILCS questionnaire and data labeling in 2019. In mid 2018, the Armenian government announced an increase in the cash transfers for the second child. However, births incentivized by this legislative change would not have occurred until 2019, and therefore do not confound the analysis.

Table 1 presents some key indicators for the full panel, including all observations aged 15–35 at survey year, within which only 44,578 woman-year observations had a treatment defined at year t.

Table 1: Descriptive statistics on a set of chosen indicators for the sample used in the analysis. Out of all woman-year observations in the panel, 44,578 had a treatment defined in year t. The values reported are based on the full panel, including woman-years without defined parity at t. Share with imputed birth in calendar year t represents the average share of women in the sample who give birth to a child in any given year. Tertiary education signifies graduate- or postgraduate education. Equivalized expenditure is calculated as the total household consumption expenditure divided by the square root of the household size, measured in log of AMD. Data based on the Integrated Living Conditions Survey 2012–2018.

Variable	Value
Woman-year observations (full panel, N)	71,523
Woman-years with treatment defined — parity 1–3 at t (N)	44,578
Share with imputed birth in calendar year t (%)	13.41
Woman's age (mean)	25.8
Woman's age (SD)	4.8
Woman tertiary (higher) education (%)	29.7
Woman employed (%)	38.5
Log equivalized expenditure (mean)	11.098
Urban (%)	65
Number of children at start of year t (mean)	1.12
Husband's age (mean)	27.9
Husband tertiary (higher) education (%)	23.2
Husband employed (%)	70

Finally, key indicators comparing the treatment and control groups are outlined in table 2. Out of the 44,578 treatment-defined mother-year observations, 39.8% had one child and were in the control group, whereas 60.2% had two or three children and were in the treatment group. The groups differ across most indicators, primarily driven by the difference in age and family composition. All indicators are therefore controlled for in the main specification.

Table 2: Comparison of key indicators between the control group and the treatment group for the sample used in the analysis. Share with imputed birth in calendar year t represents the average share of women in the subsample who give birth to a child in any given year. Tertiary education signifies graduate- or postgraduate education. Equivalized expenditure is calculated as the total household consumption expenditure divided by the square root of the household size, and is measured in log of AMD. Data based on the Integrated Living Conditions Survey 2012–2018.

Variable	Control (1 child)	Treatment (2–3 children)
N (woman-years)	17,741	26,837
Share with imputed birth in calendar year t (%)	21	4.5
Woman's age (mean)	26	29
Woman tertiary (higher) education (%)	33.2	19.7
Woman employed (%)	37.4	42.6
Log equivalized expenditure (mean)	11.096	11.02
Urban (%)	69.7	60.2
Husband's age (mean)	28.2	30.3
Husband tertiary (higher) education (%)	24.7	18.3
Husband employed (%)	71.6	68.8

4.2 Identification Strategy

Following the approach outlined by Pinto et al. (2021), the strategy exploits differences in cash transfers by birth order. The baseline difference-in-differences specification used is according to the following:

$$Y_{iamt} = \beta_0 + \beta_1 \text{treated}_i + \beta_2 \text{post}_t + \beta_3(\text{treated}_i \times \text{post}_t) + \gamma X_{iamt} + \delta_t + \mu_m + \alpha_a + \varepsilon_{iamt}$$

Where Y_{iamt} is a dummy equal to 1 if a woman i of age a in marz m gives birth to a child in year t. treated$_i$ indicates women with 2 or 3 children. Post$_t$ equals 1 in 2015 and after, and 0 otherwise. The policy came into effect in March 2014, but was applicable for all children born after January 1; the first births that could have been incentivized by the policy would occur nine months later (Government of Armenia, 2014, p. 96). As time periods are recorded in years, 2015 is used as the first post-treatment year. However, there is evidence that the reform was announced as early as September 2013 (ARKA News Agency, 2013). Therefore, as a robustness test, the event period is set to 2014 to account for potential anticipatory effects. In addition, a regression is estimated excluding 2014 entirely.

X_{iamt} is a vector of control variables, including maternal demographic and socioeconomic characteristics, husband characteristics, household per-capita equivalized expenditure, settlement type, and household composition by age and sex. Standard errors are clustered at the household level. δ_t, μ_m, and α_a denote sets of fixed effects for years, marzes, and age, respectively. By including fixed effects, the regression takes into account unobservable heterogeneity across marzes, years, and age. Age fixed effects are preferred over a quadratic specification in age because birth probabilities vary flexibly with integer age in this window; a quadratic imposes a restrictive global curvature that fits poorly at the extremes in residual diagnostics, whereas age fixed effects absorb a flexible age profile while keeping comparisons within age cells.

The control group consists of women with one child. Women with zero children are equally non-affected by the policy reform as the women with one child; however, the decision to have a first child is arguably different from the decision to have another child once a woman already has at least one child (see, for example, Billari et al., 2009). Therefore, women with zero children were not included in the control group.

The control variables were chosen in line with Pinto et al. (2021) and Baghdasaryan and Barseghyan (2024), and include, apart from standard individual characteristics, a set of variables for age categories and sex of previous children, as well as husband characteristics. Furthermore, a logarithmic equivalized expenditure variable was added, defined as total household consumption expenditure divided by the square root of the household size. The expenditure equivalent is recognized by Armstat as the best measure of poverty and welfare, as income is not well reported among poorer households (Armstat, 2024). Dividing by the square root of household size is a simple yet widely accepted method to normalize consumption for comparability (OECD, 2013, p. 174). For employment and labor force participation, missing values were included and treated as zero (unemployed, not in the labor force). Similar to the issue of matching mothers with their children, an effort was made to match women with their husbands. Missing values of husbands and husband characteristics were also included in the regression to avoid losing a large share of the observations. The share of woman-year observations with missing values for employment (among women in the labor force) and husband characteristics is 15.62%. Comparing the missing rows to the non-missing, a weighted regression indicates systematic differences along age, equivalized expenditure, and settlement (conditional on year and marz fixed effects), but not along treatment status, suggesting that imputation is not sharply concentrated in the treatment arm. In the panel construction, mothers' age and children's sex and age composition were treated as time-varying, whereas educational attainment, socioeconomic factors, and husband characteristics were set equal to the survey year for each calendar year.

For the chosen strategy, the identifying assumption is the parallel trends assumption. Although it cannot be formally tested, pre-treatment trends (pre-2015) were studied to strengthen the assumption. A joint Wald test shows that pre-treatment years were jointly insignificant (p = 0.35), meaning that non-parallel pre-trends cannot be proven (H_0: all the pre-treatment interaction coefficients [treatment * year] are equal to zero). Figure 3 plots a difference-in-difference between treatment and control for each year compared to that of the reference year 2013. The reference year was set to 2013 as it is the last completely clean year without any treatment effect from the policy change. Among the pre-treatment years, no year except 2009 shows a significant difference from 2013. As part of a robustness test, 2009 was excluded from the sample. The assumption of parallel trends can be further strengthened by visualizing the outcome variable for the treatment and control groups across years. Figure 1 shows that prior to 2014, the number of live births per year for the control and treatment group followed a similar trend. However, birth propensities could not be derived from the aggregated statistics, and, moreover, our analysis uses a different dataset. Therefore, a graph with birth probabilities for the treated and control groups, based on the sample, was created. It is found in figure 4 below. Again, 2014 marks a point after which the trends move in opposite directions, although the change is not as evident as for the aggregated vital statistics. In the presence of concurrent policies and events, legislation targeting families, poverty, and social services is commonly enacted. However, to the best of our knowledge, no major event during the study period differentially influenced birth probabilities across the treatment and control groups.

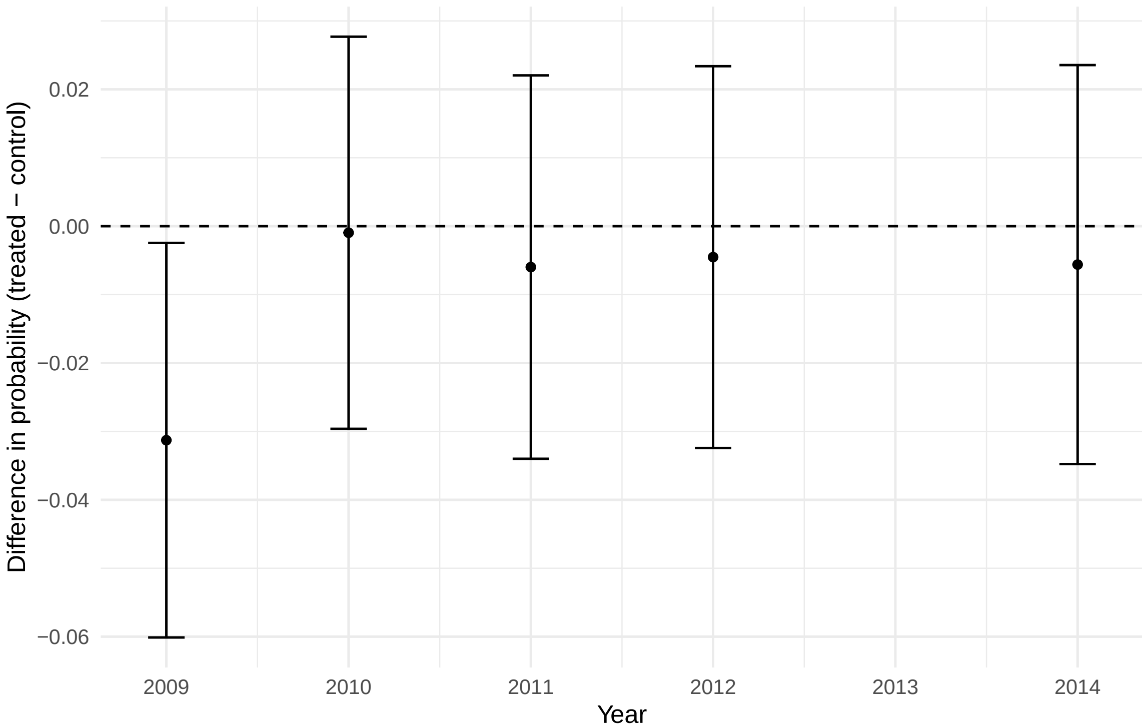

Figure 3: Pre-treatment trends for 2009-2014. Note: the minimum age was set to 15. For each year, the difference in the probability of having another child between the treatment and control group is calculated and compared against the baseline difference set in 2013. For a 95% confidence interval, no individual year apart from 2009 (β = -0.0313, significant at the 5% level) shows a significant difference compared with the baseline year of 2013. 2013 is chosen as the reference year as it is the last clean pre-treatment year before the reform. Data based on the Integrated Living Conditions Survey 2012–2018.

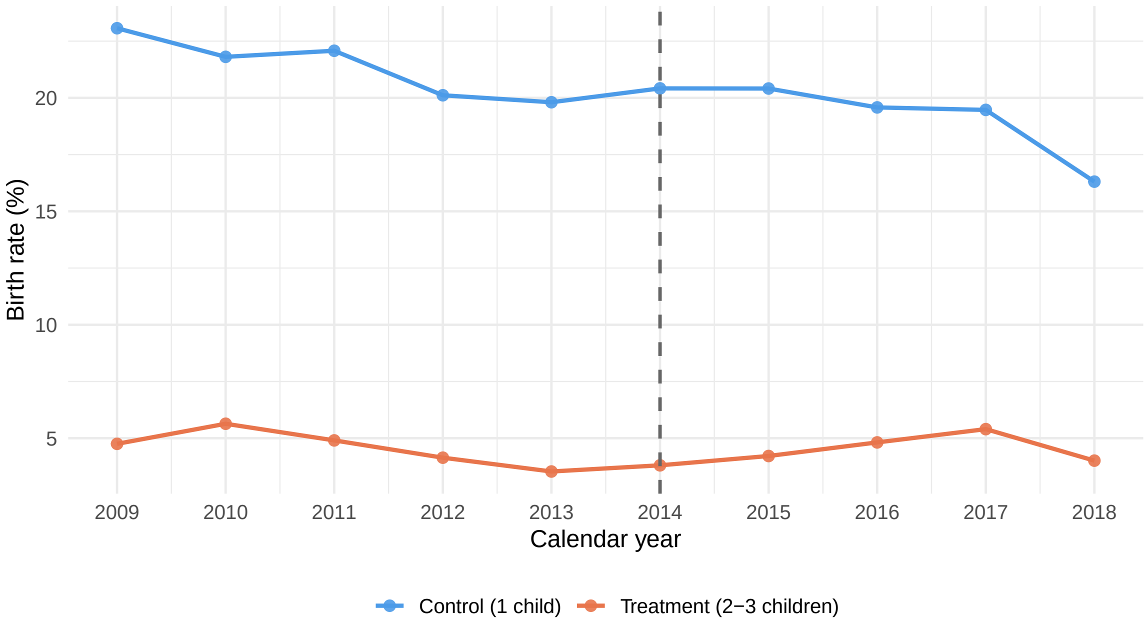

Figure 4: Birth rate trends for the control and treatment groups during the study period, based on the sample used in the study. Note that this graph is different from figure 1 for at least two reasons. First, it shows birth rates instead of total births. Second, the sample is restricted to women aged 15–35, whereas the aggregated data are unrestricted; this difference explains the high birth rate observed in the control group. Nevertheless, and although on a smaller scale, the graph suggests that for the treatment group, birth rate increased after 2014, whereas for the control group, it declined. Before 2014, the groups followed a somewhat similar trend. Data based on the Integrated Living Conditions Survey 2012–2018.

5 Results

5.1 Main Results

Results for the main specification are found in table 3. The first column represents a regression with no control variables; the second, with mother characteristics; and the third and fourth include all control variables. Only the last column includes age-, year-, and marz- fixed effects. Standard errors are clustered at the household level. Excluding control variables, the estimated effect of the reform is a 1.8 percentage-point higher probability of having another child in the treatment group relative to the control group (column 1). Adding mother characteristics and, subsequently, the age and sex composition of previous children increases the estimates to 1.9 and 2.1 percentage points, respectively (columns 2 and 3). In our preferred specification, which includes fixed effects, the probability of having another child for women in the treatment group increased by 2.1 percentage points relative to the control group following the policy change (column 4, significant at the 5% level). Although adding fixed effects changes the estimated DiD coefficient only marginally (column 3 vs. 4), we retain them as a conservative specification that reduces omitted-variable bias and aligns with standard DiD practice. The point estimate implies a 47.6% increase relative to the treatment group's 4.5% pre-treatment baseline probability of having another child. Women in the treatment group were on average 14.2 percentage points less likely to have another child compared with the control group. Higher education and employment have small negative effects on fertility, although employment is insignificant. Women outside the labor force, however, are almost 3 percentage points more likely to have another child than women in the labor force. Moreover, marriage has a strong effect on childbearing, an expected result in the Armenian context. The expenditure variable indicates that a doubling of equivalized expenditure is associated with a 0.8 percentage-point decrease in the probability of having another child, suggesting that consumption has little effect on fertility behavior. Finally, women living in urban areas were, on average, 2.2 percentage points more likely to have another child relative to women in rural areas.

Table 3: Results for the main specification. The variable of interest is Two or three x Post. In Column 1, no controls are included. Column 2 adds mother characteristics, and column 3 includes all controls. Column 4 includes all controls and fixed effects. Children's age & sex composition are a set of factors for age- and sex categories of the mothers' previous children, including the age categories: 0-4; 5-10; 11-18, and; 19+, for males and females, respectively. Husband characteristics include the age, education level, and employment status of the husband. Data based on the Integrated Living Conditions Survey 2012–2018.

	(1)	(2)	(3)	(4)
Two or three × Post	0.018**	0.019**	0.021**	0.021**
Two or three	−0.169***	−0.170***	−0.143***	−0.142***
Higher or post-graduate education		−0.021***	−0.011**	−0.009*
Employed		0.000	−0.009	−0.005
In labor force		−0.031***	−0.028***	−0.029***
Married		0.067***	0.059***	0.062***
Log. equivalized expenditure			−0.008**	−0.011***
Urban			0.020***	0.023***
Number of observations	44 578	44 578	44 578	44 576
Year fixed effects				X
Marz fixed effects				X
Age fixed effects				X
Children age & sex composition	No	No	Yes	Yes
Husband characteristics	No	No	Yes	Yes

* p <0.1, ** p <0.05, *** p <0.01

A back-of-the-envelope calculation was made to contextualize the results. Specifically, by summing the weights of the treated women in the post period and multiplying by the point estimate, we arrive at 2,371 additional births inferred by the policy. This represents around 593 additional births per year in the four post-year periods. Apart from all assumptions made in the analysis, this number further assumes that the statistical weighting is truly representative of the population; the calculation is sensitive to even small changes in the weights. Taking this one step further, we can infer the actual cost of these additional births. The 593 extra births per year cost around 1.3 billion AMD. This means that each additional birth costs approximately 2.2 million AMD, or equivalently 5,860 USD. For full transparency, a table of our calculations is presented in the appendix (table 8).

5.2 Heterogeneity Analysis

To examine whether the policy affected different groups of mothers differently, a heterogeneity analysis was conducted. In particular, heterogeneous effects were tested for consumption expenditure, poverty status, education level, labor force participation, and type of settlement. The regressions were run separately for each dimension and should be interpreted as independent, not nested. The results are presented in table 4. As shown in rows 2–5, the policy's effect differs across income groups. Women in the second quartile show the largest response of 3.16 percentage points, significant at the 10% level. For heterogeneous results on poverty status, women classified as very poor show a very large effect of 16 percentage points, which is significant despite the low number of observations. This estimate should be interpreted carefully, as individual observations can have a large impact. The estimate for poor women is negative and close to zero, although insignificant, whereas for non-poor women, the largest group, the effect size is only slightly larger than the overall average effect and significant. Rows 9–10 show that educational attainment does not affect the estimate much, although the estimate for women with higher educational attainment is insignificant. Rows 11–13 show that the group of women not in the labor force responded more weakly to the policy compared to employed and unemployed women. The effect size for unemployed women is especially strong at 10.2 percentage points. Finally, women living in urban areas were, on average, more affected by the policy than women living in rural settlements, although the difference is insignificant.

Table 4: Heterogeneous results with respect to income, poverty status, education level, labor force participation, and settlement type. The variable of interest is Two or three × Post, found in column 3. Data based on the Integrated Living Conditions Survey 2012–2018.

Dimension	Group	Two or three × Post	Std. error	p-value	Stars	N obs.
Expenditure quartile	Q1 (lowest)	0.0138	0.0196	0.481		11142
Expenditure quartile	Q2	0.0316	0.0177	0.074	*	11145
Expenditure quartile	Q3	0.0147	0.0163	0.368		11144
Expenditure quartile	Q4 (highest)	0.0177	0.0161	0.272		11143
Poverty status	Non poor	0.0247	0.0098	0.011	**	31239
Poverty status	Poor	-0.0025	0.0182	0.891		12517
Poverty status	Very poor	0.1613	0.0770	0.036	**	820
Education	Below Tertiary	0.0215	0.0108	0.046	**	33679
Education	Higher or post-graduate	0.0201	0.0151	0.182		10896
Labor force status	Not in labor force	-0.0022	0.0132	0.870		20224
Labor force status	Employed	0.0390	0.0121	0.001	***	19429
Labor force status	Unemployed / other LF	0.1017	0.0240	0.000	***	4920
Settlement	Yerevan	0.0344	0.0160	0.031	**	10855
Settlement	Other urban	0.0288	0.0141	0.041	**	16727
Settlement	Rural	0.0043	0.0150	0.775		16994

5.3 Robustness Checks

A set of tests was conducted to assess the robustness of the results. In table 5, four such tests are presented. Column 1 shows a placebo test with the treatment year set to 2012. The variable of interest is close to zero and non-significant. Column 2 redefines the treatment group to only include women with 2 children, excluding women with 3 children. The aim of this test was to assess the robustness of our treatment definition and whether the policy effects were heterogeneous across different parity levels for treated women. The result shows a significant 2.1 percentage point increase in the probability of having another child, consistent with the main specification estimate. In column 3, observations from 2009 were dropped. This test was conducted because 2009 showed individual pre-trends unparalleled relative to the reference year of 2013 (see figure 3). In this regression, the estimate is slightly smaller at 1.7 percentage points and is significant at the 10% level. This suggests that the estimate is somewhat sensitive to excluding the pre-trend outlier year 2009, but remains positive and economically meaningful. Finally, in column 4, excluding additional early calendar years (2010–2011) removes many woman-years but leaves the point estimate unchanged to the reported precision, suggesting that the sensitivity is driven primarily by 2009 rather than by the broader 2009–2011 window.

Table 5: Four tests for robustness. All regressions use the same control variables and fixed effects as in the main specification. Column 1 presents a placebo test when the treatment is set to 2012. The variable of interest, Two or three x Placebo post, is nonsignificant. As there could be systematic differences between the decision to have a third child and the decision to have a fourth child, column 2 presents the results of running the main regression with only women who have two children being part of the treatment group. The effect is significant and equal to that of the main specification. In column 3, observations from 2009 were dropped, as that year exhibited non-parallel trends when pre-trends were tested for the reference year 2013. Column 4 drops years 2009-2011, as these were not included as separate survey waves for the sample construction. Data based on the Integrated Living Conditions Survey 2012–2018.

	Placebo timing	Parity 2 vs 1 only	Drop 2009	Drop 2009-2011
Two or three × Placebo post	0.007			
Two vs one × Post		0.021**		
Two or three × Post			0.017*	0.017*
Two or three	−0.146***		−0.137***	−0.139***
Two vs one		−0.108***		
Number of observations	33 132	40 142	40 077	29 081

* p <0.1, ** p <0.05, *** p <0.01

Secondly, robustness checks for the timing of the policy were conducted. The results of the two tests are presented in table 6. In column 2, all observations in 2014 were dropped. The rationale behind this test is that it effectively removes both anticipatory effects and the fact that some births toward the end of 2014 could already have been incentivized by the policy. By excluding 2014, the remaining data is either purely pre- or post-policy implementation. The results show a significant estimate of 2.1 percentage points. Column 3 presents the main regression using 2014 as the year of the policy's coming into effect, rather than 2015. Again, the estimate is significant at the 10% level, although the magnitude is lower at 1.4 percentage points. This is consistent with 2014 behaving more like a transition year with a weaker (or more gradual) treatment–control divergence than 2015 and later.

Table 6: Robustness checks for timing of the policy. All regressions use the same control variables and fixed effects as in the main specification. Column 1 presents the baseline (main) regression. Column 2 drops all 2014 observations from the sample. Column 3 treats 2014 as the timing of the policy (instead of 2015). The estimates show a positive and significant result for both timing checks. Data based on the Integrated Living Conditions Survey 2012–2018.

	Baseline (post from 2015)	Drop 2014	Post from 2014 (post_2014)
Two or three × Post	0.021**	0.021**	
Two or three × Post (2014+)			0.014*
Two or three	−0.142***	−0.143***	−0.142***
Number of observations	44 576	38 895	44 576

* p <0.1, ** p <0.05, *** p <0.01

Finally,[3] a test was run to assess the validity of the sample construction. Specifically, a separate regression was run for different amounts of survey waves included in the dataset: starting from including only the 2018 wave (k=1) to including all waves 2012–2018 (k=7, main specification). A graph displaying the estimates and confidence intervals is shown in figure 5. The graph shows signs of qualitative robustness, as reconfiguring the dataset does not affect the positive direction or significance of the estimate. The confidence interval becomes smaller as earlier survey waves are added, which is expected as more observations are included in the regression. The estimate declines as earlier survey waves are included because the estimation sample includes different cohorts and calendar-year histories, changing the implicit weighting behind the interaction term; this is expected window sensitivity rather than mechanical bias in itself. However, it may also indicate that the policy had a delayed effect, as awareness of the reform increased over time.

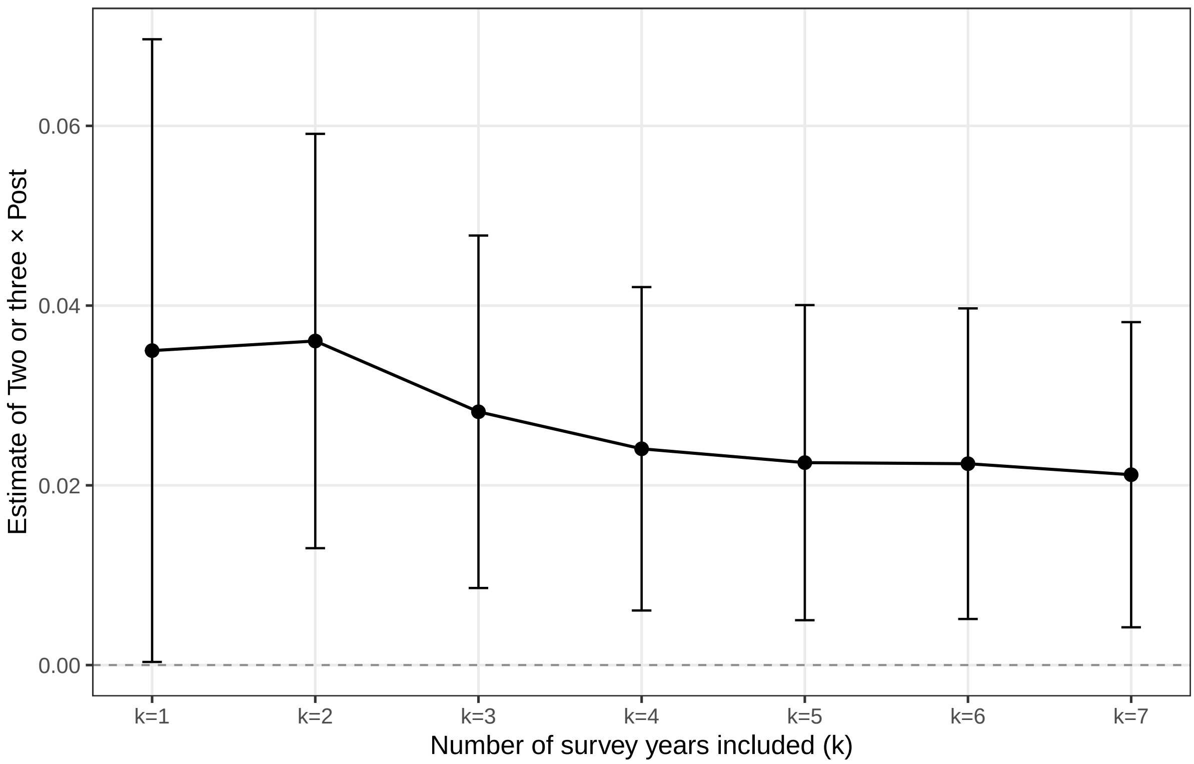

Figure 5: DiD estimate by number of included survey years. k = 1 uses 2018 only; k = 7 uses baseline window 2012–2018. Confidence intervals are at the 5% level. For all regressions, all control variables and fixed effects used in the main specification are applied. Data based on the Integrated Living Conditions Survey 2012–2018.

6 Discussion

This section discusses the results, the methodology, and the policy implications.

[3] We also re-estimated the main specification using alternative survey-weight constructions (splitting weights across calendar rows in different ways). The point estimates were very close to the baseline, so we do not report a separate table.

According to our main specification, the 2014 UCBP reform led to a 2.12 percentage point increase in the probability of having a new child for women in the treatment group relative to the control group. As we do not directly observe the benefit payments, the estimate represents an intention-to-treat (ITT) effect. However, by looking at the Armstat aggregated vital statistics, it can be inferred that all parents who register a new child with the authorities receive the payment[4], which reduces concerns that our results are driven by non-payment conditional on registration. Furthermore, because the sample restricts ages at survey to 15–35, it is important to stress that the results are local to the subpopulation studied, and cannot be claimed to represent an average treatment effect on all women of reproductive age. Most likely, the effect on older women, who are later in their reproductive window, is smaller. The effect is also local to the parity of the population studied: we can only infer that the policy affected birth probabilities among mothers with two or three children. We cannot claim that a similar benefit raise for first- or second-order births would result in an equal increase.

The biggest threat to the internal validity is the sample construction. The structure of the ILCS data necessitates a logical scheme to connect mothers with their children. This logic, however carefully constructed, can still falsely connect mothers with children who are not their own. This concern is mostly relevant for the complex households, and could potentially have a slight impact on the estimates. Moreover, in the sample construction, observations of women or children who could not be matched properly are dropped. It is assumed throughout the analysis that this group does not systematically differ from the observations kept. Finally, as we expand the RCS to a panel dataset with reconstructed birth histories, we risk double-counting women who have been surveyed in multiple years. Due to the work done by Armstat, with support from the World Bank, to improve the ILCS questionnaire and methodology in the early 2000s, we can only assume that the number of women surveyed across multiple years is small and should therefore have little impact on the sample construction.

The results of the heterogeneity analysis can seem contradictory: on the one hand, women in the second expenditure quartile are the most affected; on the other hand, non-poor women are more affected than poor women. This discrepancy does not speak to a concern about the internal validity as the poverty measurement by Armstat does not directly translate to the expenditure measurement. How can we explain the differences in responses to the policy by poverty status? Women classified as very poor exhibit the largest effect, but this should be interpreted with caution given the small sample size. Looking at the other groups, there are at least three reasons why non-poor women would respond more strongly to the policy compared with poor women. First, there could be

[4]The compliance rate is calculated as the ratio of the number of families receiving lump-sum birth benefits to the number of total families for each year between 2015 and 2018, using data from the Statistical Committee of the Republic of Armenia. The compliance rate has exceeded 100% in all years examined, possibly due to delays in birth registrations or differences in administrative reporting timelines.

a contemporaneous event or reform during the study period that affected the groups differently. Although poverty alleviation and social protection reforms are commonplace, to the best of our knowledge, there was no such major event during the study period. Second, it is possible that the diffusion rate of awareness of the policy differs across groups. The final year of the study period is 2018, four years after the policy was implemented. The timing of awareness of the policy change across groups is unobservable. However, non-poor women likely have better access to information and may therefore have become aware of the reform earlier than poor women. Finally, the different groups can be systematically different in the composition of compliers, never-takers, and inframarginal always-takers. For example, the cash benefit for childbirth may already have provided a strong incentive for poorer women before the increase, which would explain why no substantial increase in the probability of higher-order births is observed following the reform. Regardless of the underlying explanation, our results run counter to theories suggesting that poorer women should respond more strongly to financial incentives. Instead, we find a positive effect among non-poor women, while the effect for poorer women is negative or close to zero. From a policy perspective, this may imply that economic determinants of fertility are less important than previously assumed, or that pronatalist policies need to be more effectively targeted toward the intended groups.

As per labor force participation and employment status, the heterogeneity analysis showed the largest effect for women classified as unemployed/other LF. Other labor force includes women whose employment data were missing, and thus, we should again be careful in interpreting those results.

Compared to other cash-transfer policies studied previously, the effect found for the 2014 reform in Armenia is large (see, for example, Bonner & Sarkar, 2020; Chuard & Chuard-Keller, 2021; González & Trommlerová, 2023; Milligan, 2005). In absolute numbers, the policy is estimated to have generated approximately 593 additional births per year. However, this increase is insufficient to meaningfully offset Armenia's overall fertility decline and falls short of the stated demographic objective of reaching a population of five million in 2050. Aggregated vital statistics show that the total number of births has continued to decline for each year since the introduction of the policy, amounting to a cumulative reduction of 16,624 births during the years 2015-2018[5]; a number significantly larger than the total of 2,371 additional births induced by the policy. As such, our findings suggest that although financial incentives may mitigate the medium-term fertility decline, they are insufficient to reverse the prevailing demographic trend. Furthermore, the 2014 reform – despite increases in monetary support – demonastrates a smaller relative effect compared to the 2009 reform. Pinto et al. (2021) found that the reform to the UCBP in 2009 increased birth probabilities by 58–64%, compared to our estimated increase of

[5]This calculation compares the actual birth numbers during 2015–2018 with a baseline calculated as the average number of births between 2009–2014.

47.6% relative to pre-reform levels. This comparison suggests that the expanded financial incentives introduced in 2014 did not translate into proportionally stronger behavioral responses. From this, we can conclude that the marginal effect of increasing the amount of the cash transfer is not constant, but likely exhibits diminishing returns to fertility.

The cost-benefit assessment suggests that the program delivers strong returns. The 593 extra births per year cost around AMD 1.3 billion. This means that each additional birth costs approximately AMD 2.2 million, or equivalently USD 5,860 – a cheap policy considering the ballpark life-time societal value of an individual.

The finding that financial incentives are not a sufficient solution to the demographic crisis is consistent with previous research. Despite this, financial incentives and lump-sum cash transfers remain popular among policymakers. For example, in 2025, the right-wing populist party Alternative for Germany (AfD) proposed a policy very similar to the UCBP. The introduction of a "baby welcome grant" in Lower Saxony entails that mothers will receive a one-time payment of EUR 2,000 at the time of birth for each of their first two children. For third and subsequent births, an additional EUR 500 would be added to the benefit (AfD, 2025). Similar policies have also been adopted elsewhere in Germany, such as the introduction of a EUR 3,000 lump-sum child grant in Bavaria, which came into force on January 1, 2026. Although one should be careful in comparing Armenia to other countries, our results nevertheless highlight the limitations of one-time cash transfers in shaping overall fertility behavior. It points to the need to reassess existing pronatalist policies and to consider more comprehensive measures that address the broader structural determinants of fertility, such as labor market conditions, childcare availability, and work-family balance (see, for example, Kalwij, 2010; OECD, 2024).

Previous research has emphasized that the effectiveness of family policies cannot be understood solely in terms of their level of public spending, but rather in terms of how well they fit within the broader institutional, economic, and normative context of a society (Neyer & Andersson, 2008). To assess the effects of family policies on fertility rates – including instruments such as lump-sum cash transfers – it is therefore necessary to understand the full policy environment of a country, including the quantity, coherence, and timing of interventions. When policies show no or only modest effects, the literature suggests that the evaluation should also address the underlying reasons. It may be that the policy's construction is not coherent with other family policies, or that there is a mismatch between policy orientations and a country's development, both factually and normatively. For Armenia, a more comprehensive understanding of women's evolving preferences regarding support functions and shifting societal norms may therefore be of importance to make fertility policies more effective. As such, future research on this topic should look into the broader institutional climate for mothers.

Lastly, starting in September 2026, the UCBP program will be changed to a lump-sum cash transfer of AMD 500 thousand, regardless of a child's birth order (Government of the Republic of Armenia, 2025). This implies an increase in the benefit for first- and second-order births and a decrease in the benefit for subsequent births. What will be the effect of this policy change? Based on our results, it is theoretically possible to derive a crude estimate of the effect on third- and fourth-order births; however, it would not be particularly informative, as first- and second-order births will likely increase. Nevertheless, this study shows that it is worthwhile for Armenia to consider other pronatalist policies than purely financial ones in order to reverse the trend of decreasing births.

7 Conclusion

As the world is experiencing a rapid decline in fertility rates, pronatalist policies are imperative to reverse the trend and avoid skewed population distributions. In particular, interventions that either alleviate financial constraints for parents or provide financial incentives are growing in popularity. Yet, research on the effectiveness of such tools yields inconsistent results that vary across groups and countries.

Using household survey data from 2012–2018, we assess the effect of the 2014 reform of the UCBP on the probability of giving birth for mothers in Armenia with two or three children. Our preferred specification suggests that the probability of having another child increased by 2.12 percentage points in the treatment group relative to the control group, corresponding to a 47.6% relative increase. The effects are heterogeneous across income groups and poverty status, with the largest effects being observed among very poor women, whose propensity to have another child increased by 16 percentage points. However, this result should be interpreted with caution, as the small sample size of the group of very poor women may have affected these results. A back-of-the-envelope calculation shows that the policy was cheap: each additional birth induced by the policy cost approximately 5,860 USD. However, when compared with the findings of Pinto et al. (2021), the results suggest diminishing marginal effects of financial incentives on birth probabilities, indicating that increasing benefit levels alone may only mitigate the broader demographic decline rather than reverse it. Financial incentives as a policy tool to stimulate births may help in the short to medium run, but do not constitute a solution to the global demographic crisis.

Previous literature on financial incentives for higher-order fertility has focused primarily on high-income countries, despite the fact that fertility behavior and constraints may differ substantially in lower-income and transitional economies. This study addresses this gap by contributing to the relatively small body of research on low- and middle-income countries. It is also the first of its kind to use ILCS data for a pure demographic study.

Future research should examine the effects of the UCBP reform to be implemented from September 2026, with particular attention to first-order births. Previous research suggests that declining fertility is driven more by decisions not to have children at all than by preferences for smaller families (Neyer et al., 2022). In this context, increasing benefits for first- and second-order births may have positive effects and could potentially offset the reduction in benefits for higher-order births. More broadly, future work in the Armenian context should assess whether alternative policy instruments – such as expanded childcare provision, more generous parental leave, or measures to promote gender equality – may be more effective in supporting fertility. Ultimately, demographic change is complex, deeply rooted, and resistant to simple interventions. There is no silver bullet, and policymakers should be wary of looking for one.

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A Appendix

A.1 Descriptive Statistics

Table 7: Descriptive statistics of a set of chosen indicators for the baseline repeated cross-section samples: on the individual members-level (7A) and on the household level (7B). Tertiary education signifies graduate- or postgraduate education. Data based on the Integrated Living Conditions Survey 2012–2018.

Panel 7A

Variable	Value
Observations (person-years)	134,635
Female (%)	54.9
Age (mean)	38.1
Age (SD)	22.9
Tertiary (higher) education (%)	18.2
Employed (%)	85.1
In labor force (%)	49

Panel 7B

Variable	Value
Household-year observations	38,880
Household size (mean)	3.7
Household size (SD)	1.9
Consumption expenditure (mean, AMD)	143997
Urban (%)	65.6

A.2 Age Distributions

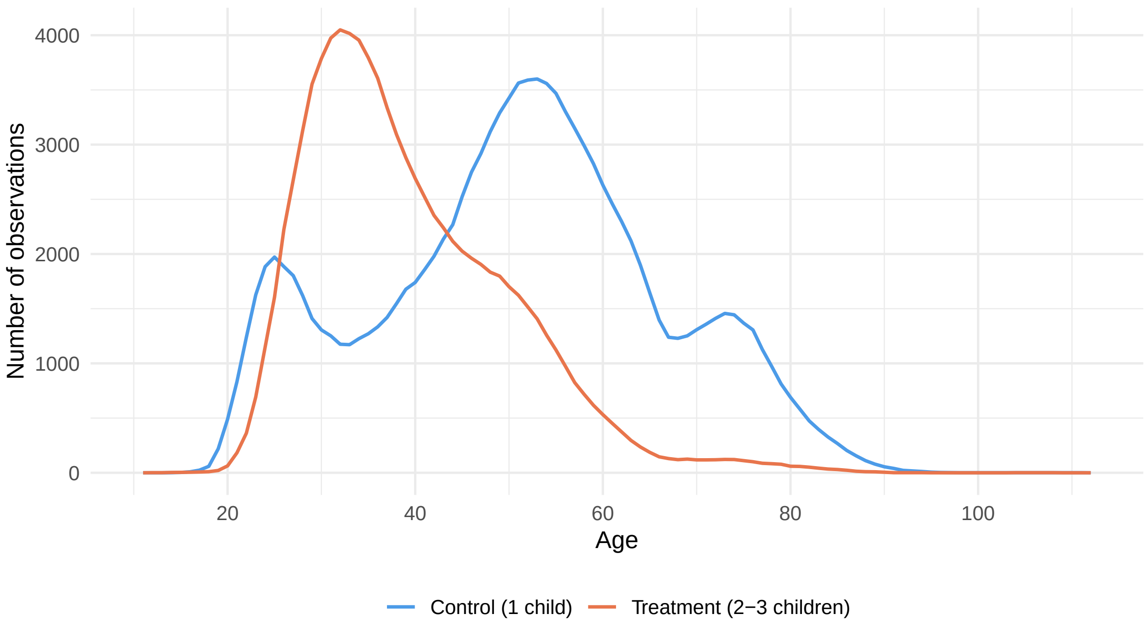

Figure 6: Age distribution by treatment status for a naive final sample without imposing an age restriction (no. woman-year observations = 210,085). The graph shows the weakness of the proxy and highlights the importance of imposing an age restriction to the sample. The control group consists of women with one child still living in the household. The treatment group consists of women with two or three children still living in the household. As the second chart suggests, the frequency of women in the treatment group drops significantly with age. The reason for this is that children move out of the household as they get older. The distribution of the chart can be explained by the following: around 18–25, women start having their first child. From 25–35, a lot of women choose to have a second child, meaning that the frequency of women in the control group decreases. Above 35, the oldest children start moving out, meaning that the frequency of women in the control group increases. Finally, around 50 and above, all children move out for the majority of households. Data based on the Integrated Living Conditions Survey 2012–2018.

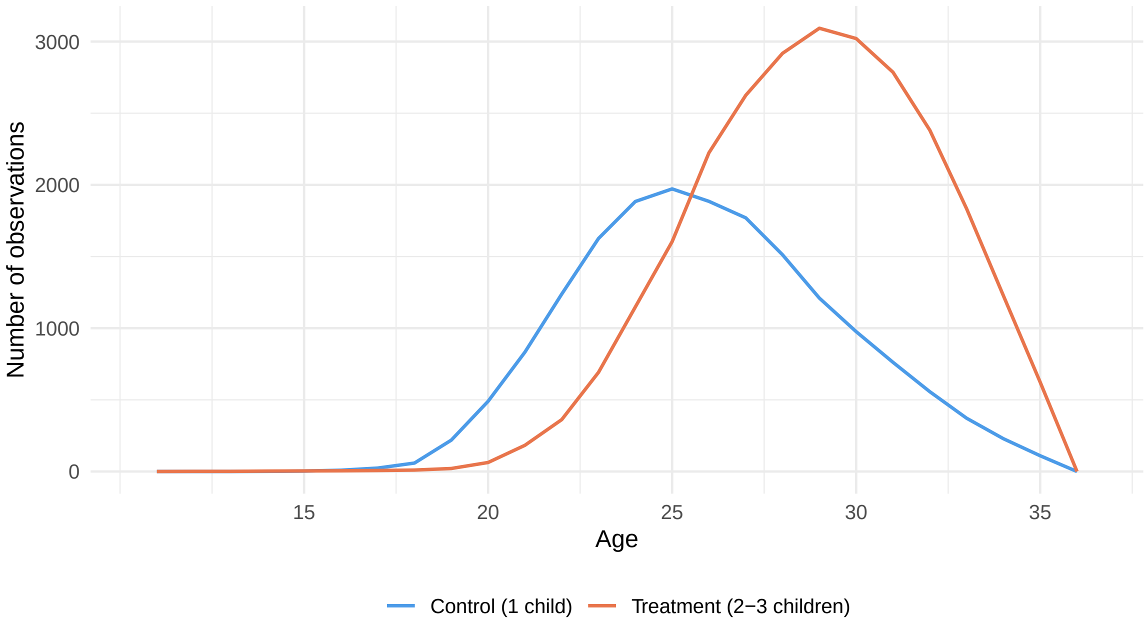

Figure 7: Age distribution for the final sample used in the analysis, restricting ages to include only women aged 15–35 (N = 44,578). For both the treatment and control groups, the distributions are bell-shaped. Control is centered earlier (age = 25) with a gradual decline toward 35; treatment is shifted right (age = 29) and declines more sharply at the top of the age window. The decline at higher ages is a mechanical feature: only women of maximum age 35 at the time of the survey are included in the sample. Data based on the Integrated Living Conditions Survey 2012–2018.

A.3 Cost-Effectiveness Calculations

Table 8: Cost-effectiveness: AMD (and USD) per additional birth induced by the UCBP reform. The benefit of having a second or third child before the reform was AMD 430,000. The reform increased this amount to AMD 1,000,000. The effective treated women per year represents the sum of the weights of the Post x Treated women, divided by the number of post-years (4). The inframarginal births per year represent the births that would have occurred regardless of the reform, based on the pre-period propensity of having another child for the treated group. The extra births per year are derived from our main specification estimate. The total cost of the reform represents the sum of the increase in benefit amount for children who would be born regardless, and the whole benefit amount for the policy-induced children. The calculation does not take into account the fact that half of the funds are placed in a state-owned savings account until the child turns 18. Data based on the Integrated Living Conditions Survey 2012–2018.

Quantity	Value
Benefit pre-reform (AMD)	430,000
Benefit post-reform (AMD)	1,000,000
Effective treated women per year (weighted)	27,982.8
Inframarginal births per year (counterfactual)	1,245.0
Extra births per year (policy-induced)	592.7
Windfall cost per year (AMD)	709,651,995.1
Marginal births cost per year (AMD)	592,701,087.5
Total marginal cost per year (AMD)	1,302,353,082.6
Cost per additional birth (AMD)	2,197,319
Cost per additional birth (USD, ∼375 AMD/USD)	5,859.52

A.4 AI Disclosure

The following AI tools have been used during the course of our thesis work:

- ChatGPT
- Claude Code via Cursor

Both AI tools have been used for support in coding. ChatGPT has been able to answer questions about the code and provide guidance, while Claude Code has written code directly in the RStudio file. All code has been read carefully to understand. Overall, data analysis does not entail complex coding. However, our project has required a lot of time spent cleaning data and generating the analyses in code. Therefore, the support from AI tools has been extremely valuable and drastically reduced the coding and debugging time. This has enabled us to devote more time to interpretations, economics, and ideation, which ultimately has increased the quality of the thesis.

The same AI tools have also helped with checking grammar and spelling. All text, however, is written by ourselves.

The main limitation of the AI tools used, we discovered, was that neither of them is strong in econometrics. The model design and all design choices are exclusively our own - whenever we would ask the AI tools for input, they would fall short of understanding a particular econometric problem.

The main risk with using AI is to blindly believe what it generates, without sense-checking. We mitigated this risk by making sure to read and understand all code generated by the AI. The main insight from using AI support for our thesis is that it is a great tool for rebalancing time management and putting more weight on things that require human attention, such as analysis, interpretation, contextualization, and ideation. The second insight is that none of these tools is yet capable of understanding complex econometric issues, and should be carefully used to give advice on this.