Could a child subsidy increase long-run fertility and stability of families? Could it have equilibrium effects? Evidence from the "Maternity Capital" program in Russia.*

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Abstract

Does subsidizing childbirth affect the overall fertility or just the timing of births? Do such policies have equilibrium effects? We evaluate the effects of a series of subsidies, called Maternity Capital, aimed at increasing fertility in Russia introduced in 2007-2012. Maternity Capital resulted in a significant increase in fertility both in the short run (by 10%) and in the long run (by more than 20%), and has already resulted in an increase in completed fertility for a large cohort of Russian women. The estimates imply that an additional birth costs approximately 50,000 US dollars in child subsidies. There are substantial general equilibrium effects: It affects family stability and the housing market (as the subsidy is conditional and can be used mainly to buy housing). The effects are heterogeneous. Fertility grew faster in regions with a shortage of housing and with a higher ratio of subsidy to housing prices.

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1 Introduction

In the United States and all European countries, fertility rates are below the replacement level (United Nations, 2015, 2017). This concern has prompted most developed countries to implement large-scale and expensive pro-natalist policies.[1] The effectiveness of these measures as well as the design of an optimal pro-natalist policy remains a challenge. This paper addresses three important questions about the evaluation of these programs. The first is whether such policies can increase fertility in the short-run and/or over a longer horizon. The second question is how such policies affect the stability of families. Finally, a third question is whether such policies, which are usually large-scale and expensive, have important equilibrium effects on the economy.

To examine these questions, we utilize a natural experiment aimed at increasing fertility in Russia. Motivated by a decade-long decrease in fertility and depopulation, the Russian government introduced a sizable conditional child subsidy (called Maternity Capital). The program was implemented in two waves. The first, the federal Maternity Capital program, was enacted in 2007. Starting in 2007, a family that already has at least one child and gives birth to another, becomes eligible for a one-time subsidy. Its size is approximately 10,000 dollars, which exceeds the country's average 18-month wage and the country's minimum wage over a 10-year period. Five years later, at the end of 2011, Russian regional governments introduced regional programs that gave additional money - on the top of the federal subsidy - to families with newborn children.

We document that the Maternity Capital program results in a significant increase in fertility rates both in the short run (by 10%) and in the long run (by more than 20%). The effects of the policy are not limited to fertility. By reducing the share of single and non-married mothers, the program also affects family stability. Additionally, the policy affects the housing market.[2] In particular, we find that the supply of new housing and housing prices increased significantly as a result of the program.[3] Confirming a close connection between the housing market and fertility, we find that in regions where the subsidy has a higher value relative to housing costs, the program has a larger effect: the effect of Maternity Capital was stronger, both in the short and long run, in regions with a shortage of housing and regions with a higher ratio of subsidy to the price of apartments (i.e., those regions where the real price of the subsidy as measured in square meters of housing is higher). Finally, we demonstrate that Maternity Capital is costly: our calculations show that the government pays approximately 50,000 dollars per additional birth that is induced by the program.

Figure 1 highlights the effect of Maternity Capital. Panel A shows the monthly number of births between 2004 and 2012. It indicates a jump in the number of births in July 2007, nine months after the announcement of the federal program. Panel B shows annual data on the total fertility rate

[1] Eighty-four percent of developed countries have implemented various pro-natalist policies that cost on average 2.6 percent of GDP (Milligan, 2005, Malkova, 2019, United Nations, 2015).

[2] The recipients of the subsidy can use it only on three options: housing, the child's education, and the mother's pension. Eighty-eight percent of families use it to buy housing. For more details, see section 3.

[3] This result also identifies those who are penalized by the program: home-buyers who did not plan to have a new baby, suffer from the rising costs of housing.

(TFR) for 2002-2016. It shows increases in the number of births in 2007 and 2012 when the first and second waves of the program were introduced.[4] According to Panel B, after 2012, when both waves of Maternity Capital came in force, TFR exceeds the pre-reform level by 35%. To the best of our knowledge, this rise in fertility is significantly higher in magnitude than any effect of a pro-natalist policy documented in the literature.[5] Panel C plots short-run changes in the share of children that live with a single parent for families that gave birth before and after the Maternity Capital program using retrospective 2010 Census data. It shows a significant drop in the share of children who live with a single parent right after the introduction of the federal Maternity Capital program.

The structure of Maternity Capital provides us several ways to identify the effect of the program. The large-scale and universal federal program was unanticipated by the public, allowing us to employ an RD design to infer initial short run effects. To do so we utilize high-frequency (monthly) data and use regression discontinuity (RD) analysis within a relatively short time interval near the adoption of the child subsidies. We show discontinuous changes both in fertility rates and in important characteristics of parents of newborn children. The subsequent regional programs differ by size, and thus we utilize Difference-in-Differences estimators where we use variation in the levels of regional subsidies. Finally, we employ differences in the intensity of treatment for families with different family structure, and combine two dimensions of variation (by regions and by family structure) in a triple difference estimator.

To find the cumulative long-run effect on fertility, we show that the shocks that were identified in the analysis above are persistent over time. We confirm the credibility of this result using various checks. We show that general predictions of re-scheduling hypotheses do not hold: contrary to these predictions, time spacing between children, age of mother, desired number of children, as well as number of births of higher-parity children increased after the reform. In a robustness check, we also compare the post-reform fertility growth in Russia with that of Eastern European countries that showed similar pre-reform trends in fertility. Taken together, all the evidence points toward a long term effect of Maternity Capital on fertility.

Our paper makes several contributions to the existing literature.

First, it contributes to a large body of empirical literature that has tested whether fertility responds to financial incentives or not (see the next section for a detailed review of related literature). Our study shows the responsiveness of fertility to policy. Moreover, the estimated size of the effect we estimate is larger than that typically found in existing studies and widespread across all population groups. The other distinctive feature of our paper is that it, contrary to the most existing studies, also documents the long-run effect, or change in overall fertility.

We also contribute to the literature that discusses the relationship between pro-natalist policies

[4] Note that annual data underestimates the jump in fertility rate after the introduction of the federal program because it occurred in the middle of 2007.

[5] Recent studies that find a positive effect on fertility document an increase in fertility rates in the range from 8% to 15% (see Gonzales, 2013, Cohen, Dehejia, and Romanov, 2007, Milligan, 2005, Malkova, 2019, and Slonimczyk and Yurko, 2014).

and family outcomes. The common agreement in the literature is that welfare programs usually provide incentives for single motherhood (see Moffit (1998) for discussion). In particular, Rosenzweig (1999) finds an impact of AFDC on the decision of young women to be single mothers. In our paper we find the opposite effect: we find that reform significantly reduced the share of single mothers and increased the share of married parents.

Finally, while most of the existing studies concentrate on the effect of pro-natalist policies on fertility and mothers' labor market outcomes, ours shows that the effects of these large-scale policies may go far beyond this scope. We provide an example of the importance of the general equilibrium effects for policy evaluation (on the housing market), which contributes to the existing discussion (Acemoglu, 2010). By demonstrating the sizable effect of the program on the housing market, our paper shows a strong connection between childbearing decisions and housing (Lovenheim and Mumford, 2013, Dettling and Kearney, 2014). Ignoring general equilibrium issues may result in substantial bias in the evaluation of both the short and long-run costs and benefits of the program (Acemoglu, 2010).

The paper proceeds as follows. In the next section, we discuss the literature. Section 3 describes the institutional environment of the Russian Maternity Capital program. Sections 4, 5, and 6 present the data, short-run analysis, and long-run analysis for Russia. Sections 7, 8, and 9 study general equilibrium effects, changes in mother characteristics, and willingness to pay for an additional child. Section 10 provides robustness checks. Section 11 concludes the paper.

2 Previous Research on Financial Incentives and Fertility

Our paper contributes to the existing empirical literature on the relationship between financial incentives and fertility in several ways.

First, it contributes to a large body of empirical literature that has tested whether fertility responds to financial incentives or not.[6] Hotz et al. (1997), Hoynes (1997), and Moffit (1998) survey the earlier papers that studied the relationship between welfare programs and fertility in the US. The shared view of these surveys is that earlier literature provides inconclusive evidence due to a large variation in results as well as due to issues with finding plausibly exogenous variation, small sample sizes and lack of substantial variation in variables of interest. The recent literature seeks to answer this question using new evidence from policy experiments in different countries. The results are mixed. Kearny (2004) finds little effect of the Aid to Families with Dependent Children (AFDC) program on fertility. Gauthier and Hatzius (1997) compare pro-natalist policies in developed countries and reach a conclusion similar to Kearney (2004). The other strand of literature finds a response of fertility to pro-natalist policies (see Milligan, 2011 for Canada, Malkova, 2019 for USSR, Cohen, Dehejia, and Romanov, 2013 for Israel, Gonzales, 2013 for Spain, Lalive and Zweimüller, 2009 for Austria, Slonimczyk and Yurko, 2014 for the initial years of the Russian Maternity Capital

[6] For theoretical treatment see Becker (1960), Becker (1965), Willis (1973).

program, and Laroque and Salanie, 2004 for France). Our paper also shows the responsiveness of fertility to policy, moreover, the effect documented in our paper is larger in magnitude and widespread across all population groups.

The other distinctive feature of our paper is that, contrary to most of the studies above, it documents a long-run effect on overall fertility. A short-run increase in fertility may be driven by re-scheduling the timing of births rather than changes in overall fertility, i.e. the total number of children a woman would like to have. While both short- and long-run effects are of interest (Bloom et al. 2009), only the latter changes the future size of the workforce and a country's ability to finance old-age benefits. Making conclusions about overall fertility is difficult because the completed fertility cycle of mothers usually is not observed yet. Because scholars do not observe the completed fertility, they have to make strong assumptions about future fertility behavior. In our case, because the policy has already lasted for a long time (12 years) and because the effect of the policy on fertility is substantial, we already can claim that regardless of future changes in fertility, the policy has changed the completed fertility rate for a large cohort of women. This result contributes to a long lasting and yet inconclusive discussion of this question.

The large body of literature, including Adda, Dustmann, and Stevens (2017), Sobotka and Lutz (2011), Parent and Wang (2007), and Schoen (2004) argues that many policies, even when they have positive short run effects, result in little or no long-run fertility growth. In particular, Adda, Dustmann, and Stevens (2017) estimate a dynamic life-cycle model to show that the long-run effect of the pro-natalist policy is considerably smaller than the short-run response in recent fertility rates in Germany. There are also two recent papers that show long-lasting effects. Lalive and Zweimüller (2009) find the long-lasting effect of the increase in terms of parental leave on births of second parity children in Austria. Malkova (2019) uses an event study to document the rise in second and higher parity births in response to a maternity program in the Soviet Union.

Our paper adds to these studies in several ways. Similar to these papers, we document long-run changes in fertility, but, importantly, we get more "external validity" for our results. Lalive and Zweimüller (2009) estimate local average treatment effects for a specific policy experiment that may not be applicable to typical pro-natalist interventions.[7] Malkova (2019) provides evidence from the expansion of maternity benefits in the USSR. The USSR's socialistic economy has several important distinguishing features that make it difficult to extrapolate results to other settings. In particular, housing was free in the USSR, and the costs of raising children were low: every family had access to free childcare, healthcare, a high school, and college education. The opportunity cost (of raising children) was also low: the earning profile was flat, and women were guaranteed their jobs back following maternity leave. In our study, we provide evidence of the effect of policy on a very broad part of the population using evidence from the market environment.[8] Besides, these two studies

[7]See also Malkova (2019) for discussion.

[8]Also our data allows us analyze a broader set of important outcomes that would be impossible in a closed non-market socialistic economy.

(as well as many other papers that document increases in fertility rates) document increases in second-parity births. Our paper has a more general result: we are able to document not only an increase in higher-parity births, but also in overall fertility. Therefore, we do not need to rely on any assumptions about patterns of substitution between parities to claim an increase in fertility. This argument is important because policymakers care about the total increase in population. The other advantage of our study is a methodological improvement in the analysis of aggregate data. Following critics that come from the demographic literature (see, for example, Schoen, 2004), we explore not only the regional dimension of the data, but also, simultaneously, mother's variation by mother's cohort. This improvement allows us to account for differences in groups that potentially bias earlier studies, as well as changes in cohort-specific or age-specific patterns in fertility that could be important confounding factors.[9] For example, in Malkova (2019), the control group has a lower proportion of women of age 25-35 than the treatment group due to the second wave of the demographic hole caused by WW2. Because women age 25-35 have the highest fertility rates, it may cause a bias in her estimates.

Finally, our paper is close to an earlier study by Slonimczyk and Yurko (2014) that documents an increase in fertility rates within four years after the adoption of the first wave of Maternity Capital. Our study differs from (and extends) Slonimczyk and Yurko (2014) in several dimensions. First, we use richer data and a different identification strategy for the estimation of a short-run effect. The survey data that was used in Slonimczyk and Yurko (2014) contains annual-level information on relatively few birth cases. In our study, we utilize high-frequency (monthly) data on the whole population of births events, which allows us to utilize an RD design to show the immediate effect of the reform on fertility. It also allows us to document important changes in parental characteristics induced by the reform (see Panel C of Figure 1). Second, when analyzing the long-run effect of the policy, we use a significantly longer time span of the data that covers both waves of the reform (federal and regional). This benefits the analysis in several ways. It allows us to explore both temporal and regional variation in levels of subsidies in the difference-in-difference framework. Also, because we are using a significantly longer time span, we are able to show that the effect of the reform is higher than was documented in Slonimczyk and Yurko (2014). This happens because fertility increases during the later years of the reform too. Importantly, this also allows us to show the increase in the completed fertility rate.

3 Institutional Environment: The Russian Maternity Capital Program

The Russian federal Maternity Capital program became effective on January 1, 2007. Families that adopted or gave birth to a second or higher birth order child became eligible for a one-time subsidy of

[9] In our analysis we control for age, cohort and regional trends in fertility, as well as looking at changes in fertility for mothers of different age and cohort.

250,000 rubles (10,000 dollars), an amount that exceeds the country's average 18-month wage. This amount is updated annually to account for inflation (see Table 1 for the ruble and dollar amount of Maternity Capital). Families do not receive the money in cash. Instead, they receive a certificate that can be used only to pay for three options: "improvement to current living conditions", (i.e., for housing, including existing mortgages), their child's college education, and the mother's pension.[10] The money from this certificate is transferred directly from the pension fund (the administrator of the program) to the education facility or the home seller or mortgage holder. The subsidy is granted only once per family. According to the initial (2007) version of the Maternity Capital law, a family could utilize the Maternity Capital certificate money only after their child reaches two years of age. As of December 2008, the family can use the Maternity Capital subsidy to pay for a mortgage immediately after the birth of a child.

Of the three options (housing, education, pension), 88% of the families spend their subsidy on housing. One of the reasons for this is that the option of buying a house (or apartment), in contrast to other options, can be realized shortly after the birth of a child. An important restriction that we will explore further is that using the certificate to buy an apartment requires that the child automatically become its co-owner. This makes the apartments less liquid. In particular, if a family decides to sell the apartment, it will need to comply with the regulations of guardianship and trusteeship bodies. As a result, some families, mainly buyers of expensive apartments, prefer not to use Maternity Capital.[11]

The other important feature of the Maternity Capital program is that it was unanticipated by the public until October 2006 (see Slonimczyk and Yurko, 2014), when the bill creating it was introduced in the State Duma (Parliament). In May 2006, Mr. Putin asked the Duma to prepare a federal program aimed to increase birthrate. The program was prepared for three summer months without any public discussion. Then, in October 2006, it was introduced in the Duma, immediately adopted and announced to the public, and then widely publicized in the mass media.

In the first 12 years after the adoption of Maternity Capital, 8.9 million families received Maternity Capital certificates, and 5.1 million families used the subsidy in its entirety; 3.3 million families used Maternity Capital to pay for a mortgage, while more than 1.9 million families used it to pay for housing without using a mortgage.

Since the start of the Maternity Capital program, many Russian regions (states) have also adopted laws that offer families a subsidy in addition to the federal program. Two regions adopted Maternity Capital programs in 2008. At the end of 2010, Russian President Dmitry Medvedev requested that regional governments adopt local child support programs. In most of the other regions, laws were passed in the second half of 2011 and came into force in 2012. By 2012, 87%

[10] In 2014, the option of using Maternity Capital to pay for pre-school also became available (see the comment to Federal Law 14.07.2014 N 648).

[11] Also, the government applies additional restrictions to ensure that families use their Maternity Capital to improve current living conditions, but not to make investments. Thus, although they can use Maternity Capital to buy housing, recipients cannot use it to buy relatively cheap alternatives like land or a summer house (dacha).

of the regions had adopted an additional subsidy, averaging about 25% of the federal subsidy. The amounts of the regional subsidies vary, from 0 to 108% of the federal subsidy. Most of the programs (85%) give a subsidy for the third and higher birth order child, yet one region gives it for the first child, three for the fourth and two for the fifth child. The programs also vary across regions in other dimensions: 1) by the types of restrictions on the use of the subsidy: many regional programs give unconditional subsidies in cash, some restrict it's usage (legitimate expenditures include housing, education, taxes, pension, medical spending, insurance, rental expenses, and cars); and 2) by which families are eligible: in some regions only families with an income below a certain threshold are eligible for a regional subsidy.

Initially, both the federal and regional Maternity Capital programs were set to last for 10 years, expiring January 1, 2017. This timing was unchanged until the very end of the program. However, in 2016, the federal government extended the federal program until 2018. In 2018, the federal program was extended again until 2021. Most of the regional programs were extended initially until 2018, and then until 2021. Also, starting from 2016, the nominal (ruble) value of the subsidy was not updated to account for inflation. These perturbations created uncertainty and caused a drop in the fertility rate in 2017, mainly affecting birth rates of lower-parity children. Yet the total fertility rate in 2017 remained 25% higher, and the fertility of higher-parity children remained more than 40% higher than the pre-reform level. Figure 2 shows the evolution of parity-specific birth rates in Russia.

4 Data

In our study, we utilize several datasets.

First, we use regional (state) level data on various regional characteristics from the Russian Statistical Agency, Rosstat and the Russian Fertility and Mortality Database (RFMD).[12] These data includes monthly counts of births at the national and regional level. The Russian Fertility and Mortality Database contains annual data on age-specific birth rates for all Russian regions, and on the birth rates by birth order for a half of the regions. The Rosstat data provides different regional data with an annual and/or quarterly and/or monthly frequency. In particular, the data on regional birth counts is available monthly, whereas the data on regional housing prices is available quarterly, and that on the amount of new housing only on an annual basis.

Second, we use the 2010 Russian census and 2015 Russian micro-census provided by Rosstat.[13] Such data can be obtained in the form of counts of individuals within narrow groups defined by a set of demographic and regional characteristics. For our purposes, we extract several samples. The first sample contains counts of children born in a particular month and year, to a mother of

[12]For details see Rosstat web-site (www.gks.ru) and the Russian Fertility and Mortality Database web-site (http://demogr.nes.ru/en/demogr_indicat/).

[13]Data extracts from the Census were executed several times within a period from September, 2017 until April, 2019.

a particular age, and living in a family with k children (k=1,2,..). The second sample contains counts of children within a particular county (rayon), born in a particular month and year, living in a family with k children (k=1,2,..), and living in a family with one or two parents. The third and fourth samples provide the same counts but aggregated at the state (region) and national levels, respectively.[14] Thus, the rayon-(or region-) level datasets contain monthly data on the number of children that were born in a particular month and year in families with one, two, three or more children (including newborns) for families with either a single parent or with two parents for 2,351 of Russian rayons (or 85 regions) for the period from 2000 to 2010 (2010 is a census year).

The resulting datasets contain 2,857,200 and 160,200 cells (observations) representing rayon- and region-level data respectively. In addition to the 2010 Census, we utilize data on the 2015 Russian micro-census that surveys 1.7 percent of the population. Due to size limitations, we extract counts not on monthly, but quarterly birth date frequency. Census (micro-census) data on monthly birth rates are richer compared to Rosstat: in particular, using census data we can calculate monthly birth counts by parity, by maternal age, as well as by other demographic characteristics. However, the census provides retrospective information on counts of births based on information obtained in 2010 (2015), thus some births are missing due to child mortality. Consequently, for our regressions, we use both Rosstat and census data.[15] In addition to aggregate counts discussed above, the 2010 Russian census is available at the individual level for a sub-sample of 7 million people. Unfortunately, this individual-level dataset does not contain many variables important for analysis, so we are restricted to using it only for a supportive analysis.

Third, we utilize individual-level data from the Russian Longitudinal Monitoring Survey (RLMS).[16] The RLMS is a nationally-representative annual survey that covers more than 10,000 individual respondents from 1994 to 2015. The RLMS survey contains rich information on demographic and socioeconomic characteristics, including data on the date of birth and birth order, as well as various demographic and socio-economic characteristics of children and their families. In our analysis, we restrict the time span of the data to the years 2000-2015. The year of the adoption of Maternity Capital lies in the middle of this period.

Finally, to to conduct a national-level analysis and cross-country comparisons, we use the Human Fertility Database (HFD) provided by the Max Plank Institute for Demographic Research (MPIDR) and the Vienna Institute of Demography.[17] The HFD contains annual country-specific data on age-specific birth rates, on the birth rates by birth order, as well as monthly counts of births.

The summary statistics of variables used in the analysis are shown in Table A1 in the appendix.

Birth Rates Variables and Data Used. For our short-run analysis, we use monthly data in the main specification. Monthly counts of births are available at the national and regional level,

[14] There are 2,351 rayons and 85 regions in Russia.
[15] Results of regressions are similar for all datasets.
[16] See https://www.hse.ru/en/rlms/
[17] See http://www.fertilitydata.org/ and http://www.humanfertility.org/cgi-bin/main.php

thus we utilize national and regional-level data and use log counts of births in the main specification. In the robustness section, we construct estimates of the population of females of childbearing age by smoothing out the available data and use the constructed log fertility rate (log number of births divided by the number of females of childbearing age) instead of the log number of births. For our within-country long-run analysis, we use available regional- and national-level annual data on the log of age-specific fertility rates.[18] For our cross-country case-study, we use data on age-specific fertility, total fertility rate, cumulative fertility rate, and tempo-adjusted fertility rates that are available at the country level (for definitions, see note 1 in Appendix).

5 Short-Run Effect on Fertility

5.1 Short-Run Effect of the Federal Maternity Capital Program

The main challenge in the analysis of the effect of a universal natural experiment like the introduction of the federal Maternity Capital program is to choose a credible counterfactual.[19] One credible solution is to employ an RD design that resembles perfect randomization in the neighborhood of the threshold and does not rely on a control group. The RD approach estimates the local treatment effect that we interpret as the short-run effect.

In our RD strategy, we compare fertility rates within a short time interval before and after the introduction of the Maternity Capital program. For the federal Maternity Capital program, we treat October 2006, the official date of the announcement of the program, as the threshold date for conception decisions (see Slonimczyk and Yurko, 2014). This means that we treat July 2007 as a threshold month for realized birth outcomes.[20] For the regional Maternity Capital programs, we treat January 2012, the start of the majority of those programs, as the threshold date for realized birth outcomes.[21]

To estimate the effect of Maternity Capital in the short run, we employ several specifications.

Our baseline regression uses the following flexible RD specification

$$Y_{rt} = \theta I(t \geq 0)_{rt} + f(t) + g(t) * I(t \geq 0)_{rt} + D'_{rt}\Gamma + u_{rt} \quad (1)$$

where t is date ($year + (month - 1/12)$) normalized to be 0 at the threshold dates discussed above, $f(t)$ and $g(t)$ are the smooth functions of time, $g(0) = 0$, and Y_{rt} stands for the dependent

[18] Data on age-specific births are available monthly only for retrospective 2010 Census data; thus we do not use them in the short-run main specification, but we do use them in robustness analysis.

[19] For example, the option to use Difference-in-Difference approach and families that give birth to their first child as a control group would be an imperfect solution because the program may facilitate birth rates of the first child too.

[20] The threshold time point in decisions in the housing market is similar to conception decisions, i.e., the threshold date is October 2016. In the housing market, one can buy housing using a mortgage before obtaining the Maternity Capital certificate and then, after getting Maternity Capital, use it to pay a mortgage.

[21] Recall that information about regional Maternity Capital programs became publicly available a year before January of 2012.

variable (log births); because birth rates are seasonal we include the set of controls D_{rt} that contains the month fixed effects to control for seasonality. In all regressions, we use the triangular kernel; $f(t)$ and $g(t)$ are parametrized to be first-order polynomial, and the error terms u_{rt} are clustered at the date level. The parameter of interest θ stands for the effect of Maternity Capital. We estimate the model using monthly data on national-, regional-, and rayon-level cells. The baseline specification uses data at the national-month level. In addition, we show results for regional-, and rayon-level cells to be consistent with further elaboration of our results in which we utilize regional and rayon heterogeneity in the effect of the program. The subscript r in model (1) refers to the cross-sectional dimension (national, regional, or rayon), and the subscript t refers to time (date). The bandwidth was set to be 3 in the baseline specification.[22]

Table 2 shows the results of the RD estimates of the effect of Maternity Capital on birth rates.[23] Panels A, B, and C display the results of the RD regressions at national×month bins, regional×month bins, and rayon×month bins, respectively. All panels indicate that Maternity Capital results in a 9% increase in birth rates. The subsidy affects the birth rates of second and higher birth order children more. While the fertility rate for the first child increased by 7%, fertility rates for second, third and higher birth order children increased by 12%, and 15% correspondingly.[24] Interestingly, the results suggest that the reform increases birth rates not only for second and higher parity children that are eligible for a subsidy, but also for first children. We see two different explanations for this. First, for a family that preferred to be childless before the reform, it became beneficial to give birth to two children and thus become eligible for a Maternity Capital subsidy. As a result, some couples opted to have a first child. A second explanation the Maternity Capital program's massive promotional campaign encouraged some childless couples to start families. Indeed, the recent literature provides many examples in which fertility decisions are sensitive to persuasion (see Bassi and Rasul, 2017, Chong, Duryea, and Laferrara, 2012).

Indeed, the observation that fertility reacts quickly with the introduction of a child subsidy is not limited to Russia. Gonzales (2013) documents both a jump in conceptions and a drop in abortions after the introduction of a child subsidy in Spain.[25] Malkova (2019) shows a rise in fertility within

[22]Figure A1 in the Appendix shows RD estimates for different bandwidth sizes. The estimates are the same for bandwidths greater than 1.5. We treat specification (1) as primary because it is more flexible. In particular, in this specification, we can control for seasonality or can estimate the heterogeneity of the Maternity Capital effects with respect to initial housing prices. In the robustness section, we use the data-driven bandwidth selector and RD estimator by Calonico, Cattaneo, and Titiunik (2014) to confirm our main specification results.

[23]Figure 2, Panel B shows the short-run effect of the federal Maternity Capital program for the births of different parity. Panel A of Figure 2 shows the effect of Maternity Capital on total fertility rate (TFR) and on the decomposition of births using annual data for the period until 2017. Both figures show that Maternity Capital affects births of second and higher parity children more.

[24]Columns 1 and 2 of Panels A and B show results for two data sets, Rosstat (RFMD) and the 2010 Census. Rosstat and HFD provide monthly counts of births at the date of birth. Census data provide retrospective information on monthly counts of births based on information obtained in 2010, and thus some births are missing due to child mortality. The results shown in columns 1 and 2 are similar.

[25]Unfortunately, we do not have access to monthly or quarterly data on abortions, and thus could not provide similar RD estimates. Annual data (that is available) shows that the abortion rate, which is relatively high in Russia,

one year after the introduction of maternal benefits in USSR. In our paper, in a robustness check we provide an example of an increase in fertility after the introduction of a child subsidy in neighboring Ukraine that (importantly) happened at a different time (see section 10.1). Furthermore, we show that the Russian Maternity Capital program results in a discontinuous change not only in the fertility rate but also in the characteristics of couples who give birth. This result that may also serve as an additional validity check of our results. To further confirm that our results are not driven by a choice of regression specification or choice of variables, in Section 10 we provide various robustness checks where we estimate a model using different measures of fertility, utilizing data on age-specific fertility rates, as well as applying an alternative to our main specification robust RD estimator by Calonico, Cattaneo, and Titiunik (2014). All these alternatives lead to estimates that are similar to our baseline regressions. Finally, in Section 10.1 we conduct placebo experiment to show that jumps in fertility in Russia coincides with the introduction of Maternity Capitals.

Yet, there are several possible concerns regarding the use of an RD strategy in this set-up. First, couples that gave birth before 2007 may try to falsify the declared birth date to change it to later time. However, this concern is not relevant in our case because the jump in fertility occurred in July 2007, half a year after the Maternity Capital program was initiated.[26] Second, while we have information on dates of birth, exact conception dates are unknown. Therefore, using the rule that conception occurred nine months before the birth date provides noisy information on the exact conception dates, resulting in attenuation bias. Third, while one can expect to see an immediate effect of the program because it encourages conceptions and discourages contraception use and abortions, many couples are not immediately successful when they try to conceive. In particular, the literature suggests that it usually takes three to six months for a couple to conceive when actively trying (see Gonzales, 2013). There is also a chance that general knowledge of the reform is not immediate, resulting in a transitional period in the implementation of the reform, and then RD regression may underestimate the short run effect. To deal with this issue, we propose a robustness check where we allow for a narrow six-month transitional period between the initial announcement date and full realization of the program. This approach is similar to Clark and Del Bono's (2016) and assumes that there is a sharp increase rather than jump in the probability of treatment across the borderline dates.[27] The exact specification is as follows:

$$Y_{rt} = \theta TR(t)_{rt} + f(t) + g(t) * I(t \geq 0)_{rt} + D'_{rt}\Gamma + u_{rt} \tag{2}$$

The treatment variable $TR(t)_{rt}$ equals one for birth dates after September, 1, 2007 and zero for dates before March, 1, 2007, and increases linearly from 0 to 1 in a half-year period between March,

has been falling for the whole time span of our analysis. The ratio of abortions to births is 1.8 in 2000, decreases to 1.1 in 2006, and further decreases to 0.45 in 2015.

[26] Also, today it is almost impossible to falsify birth dates for more than a couple of days. Registration of birth date takes place immediately after birth and directly in hospital where the mother gives birth.

[27] The other option is to apply the RD design to the situation in which the discontinuity point is unknown (see, for example Card, Mas, and Rothstein, 2008, van der Klaauw, 2002, Porter and Yu, 2015)

1, 2007 and September, 1, 2007. The set of controls and size of bandwidth are the same as in (1). Error terms are clustered at the date level.

Panel D shows the results of this regression using national-level data. Compared to RD estimates the estimated effect in (2) is on average 1.5 percentage points higher: the fertility rate increased by 10.6%, 7.7%, 13.6%, and 16.5% for all births and for births of first, second, third and higher parity children, respectively.

5.1.1 Important Heterogeneity by Housing Availability and Marital Status

Next, to confirm a close relationship between the housing market and fertility decisions, we explore the regional (and rayon-level) heterogeneity in the effect of the Maternity Capital program. The vast majority of families use federal Maternity Capital to buy housing.[28] Thus, one can expect that in regions with a housing shortage, the take-up for Maternity Capital would be higher. We then compare the effect of the program in regions with high- and low-priced housing. The average price of apartments varies greatly across Russian regions: in 2007, with Maternity Capital funds one could buy a 20-square-meter apartment in the North Ossetia region, whereas in Moscow one could buy only 2.4 square meters. Given that buying apartments using Maternity Capital is accompanied by future legal costs (see Section 3), it is reasonable to expect that the effect of maternity capital will be greater in places with lower housing prices (or, equivalently, a higher real value of Maternity Capital). To check the differential effect, we add pre-reform regional characteristics, the shortage of housing and housing affordability, and their interactions with the program dummy $I(t \geq 0)_{rt}$ in regression (3).

$$Y_{rt} = \theta I(t \geq 0)_{rt} + \gamma I(t \geq 0)_{rt}(Z_{rt0} - \overline{Z_{rt0}}) + \mu Z_{rt0} + f(t) + g(t) * I(t \geq 0)_{rt} + D'_{rt}\Gamma + u_{rt} \tag{3}$$

In this regression, Z_{rt0} stands for pre-reform regional characteristics (in 2006), the availability of housing is defined as the average square meters of owned housing per person in the region, and the affordability of housing is the size of an apartment that can be purchased using Maternity Capital. The set of controls and size of the bandwidth are the same as in (1). Error terms are clustered at the date level.

Panel A of Table 3 shows the results of the estimation. In regions with a shortage of housing or more affordable housing, the effect of Maternity Capital is greater. The effect is economically high: in regions where the price of an apartments and the size of the living area are one standard deviation lower than the mean, fertility increases by an additional 2.8 and 2 percentage points, respectively (compared to an average increase of 8 pp). We find a similar differential effect caused by the program when we explore heterogeneity at the rayon level. Panel B shows that in rayons where the average

[28]Figure A2 plots birth rates over time for various Russian regions. Indeed, it shows that in rich regions such as Moscow there is no visible effect of Maternity Capital, whereas in less wealthy Russian regions, like Bryansk, Nizhniy Novgorod, and Tatarstan the effect is sizable.

number of rooms in apartments per household is one standard deviation lower than the average, the growth in fertility is 3 pp higher.

Next, Table 4 shows individual-level heterogeneity in the reform effect.

Columns 1 to 6 show changes in fertility for married versus unmarried couples, and for dual versus single-parent families. Columns 1 to 6 show that, while the effect of reform is sizable for all groups, unmarried couples as well as single-parent families respond to reform less. Consequently, the share of children born into single-parent families and families with unmarried parents decreased, by 0.4 and 0.5 pp respectively.[29] These results indicate that, contrary to some notable policies in other countries, like the AFDC program in the US (see Rosenzweig, 1999), this policy has a positive effect on family stability. Also, these results are important because family stability is the pressing public policy concern in Russia: The share of children who live with a single parent constitutes 30% in Russia. This number is higher than that in the United States, where 25% of children live with a single parent, and in all European countries.[30]

The effect discussed above reflects changes in the characteristics of the whole pool of parents that include both always-takers (those who would give birth to a child independent of whether program exists or not) and compliers (those couples who decide to give birth because of the program). Because the program results in a 9% change in fertility, it implies that only 8.25% (i.e. 9%/109%) of couples that give birth after reform are compliers, and this 8.25% drives changes in the characteristics of the whole pool of couples. To more systematically evaluate differences in the characteristics of compliers and always-takers, we provide an analysis similar to Card and Giuliano (2016). First, we calculate the mean characteristics of couples that give birth to a child before the introduction of the program. This set of couples includes only always-takers. Then, using the results of Table 4, we estimate the changes in the characteristics of couples in the result of the program. Given that the share of compliers post-reform is 8.25% and the remaining are always-takers, we evaluate the mean characteristics of compliers. The last three rows of Table 4 show averages of characteristics of compliers and always-takers. They show sizable differences between them. The share of unmarried mothers is 12.8% among always-takers and only 7.9% among compliers. The share of single mothers is 29% among always-takers and 22.5% among compliers.

Columns 7 to 12 provide further evaluation of the heterogeneity results. They show RD estimates for changes in fertility by different socioeconomic groups. Columns 7 to 11 show that while all educational groups are affected by the reform, the increase in fertility is higher among low-educated mothers. As a result, the share of mothers with college degrees dropped after reform. Column 12

[29]The other evidence of the effect of Maternity Capital on family stability is shown in Figure A4 in the Appendix. That figure demonstrates that the number of children who have been abandoned by parents decreased since 2007 by more than 50%. We have only country-level statistics for this data and therefore do not include it in the main analysis.

[30]For a review of family statistics in Rosstat demographic data, see http://www.gks.ru/free_doc/new_site/perepis2010/croc/Docume russia.pdf for Russia, https://www.pewresearch.org/fact-tank/2018/04/27/about-one-third-of-u-s-children-are-living-with-an-unmarried-parent/ for the United States, and Iacovou and Skew (2011) for the European Union.

shows that mothers become older after the reform.[31] This is an important observation because it opposes the re-scheduling explanation for fertility change. These estimates imply that the share of mothers with college degrees among always-takers constitute 35%, while among compliers is only 17%. The share of mothers with only primary education among always-takers is 6.2%, while among compliers is only 9.9%. The mothers' average age is 26.2 and 29 for always-takers and compliers respectively.

These documented heterogeneous effects are unconditional: they are not disentangled from each other or any other possible omitted heterogeneity.[32] To deal with this issue and to further elaborate our analysis, we explore individual-level sub-sample of the 2010 Census that allows us to simultaneously control for several factors. This data set, however, has own limitation: in particular it contains only year of birth (no month of birth), and thus does not allow to estimate in the same way as (1) RD specification.[33] This restricts us to the use of a difference estimator. Using this regression framework, we check several things: first we simultaneously include several factors for which we have important heterogeneity outcomes. Thus, we check whether fertility grew faster among married couples and in regions with a shortage of housing, controlling for change in other characteristics of parents that could also change in the result of the reform. Second, we check the validity of our result by looking at a triple difference: we check whether the interaction term between marital status and housing availability has the correct sign.

To do so, we look at the birth history of women age 15-50 and check how the probability of giving a birth in particular year changes after year 2007 for different demographic groups as well for different regions. To do so, we utilize the following regression:

$$\begin{gathered} I(birth)_{rfy} = \gamma_1 I(year \geq 2007)_t \times (Z_{rt0} - \overline{Z_{rt0}}) \times F_{ft} \\ +\gamma_2 I(year \geq 2007)_t \times (Z_{rt0} - \overline{Z_{rt0}}) + \gamma_3 I(year \geq 2007)_{rt} \times F_{ft} + D'_{rft}\Gamma + u_{rft} \end{gathered} \tag{4}$$

where subscripts r, f, and t stand for region, family, and year, Z_{rt0} stands for pre-reform regional characteristics (same as in (3)), F_{ft} stands for maternal characteristics (single parent or unmarried couple), set of socioeconomic and demographic characteristics D_{rt} includes F_{ft}, Z_{rt0}, their interactions with time trends, regional, age and year fixed effects, additional characteristics, such as maternal education, indicator for urban area, log average regional real income and their interactions with post reform dummies and with time trends. Errors are clustered at the regional level. The

[31] Figure A3 in appendix displays the distribution of the RD effect by the age of the mother and by birth order. It shows that this short-run effect is driven by the increase in the proportions of mothers between age 33 and 40 who gave birth to a second or higher birth order child.

[32] Unfortunately, the data extraction procedure for birthrate by marriage status and by other family (maternal) characteristics does not allow us to simultaneously extract many characteristics due to the curse of the dimensionality problem (see Section 3 for discussion). The second issue with the estimates is that, because we use retrospective data (from 2010 Census), our RD estimates show the cumulative effect of the program through two factors: selection of compliers (married couples are likely to participate in the program) and program-induced changes in families (parents are less likely to divorce if they get a Maternity Capital subsidy). We respond to this issue in section 8 and leave further elaboration of these results as a task for future research.

[33] The other limitation of this dataset is that it contains significantly fewer variables that are available for extraction at the aggregate level.

sample of birth years used in analysis are from 2003 to 2010.

Table 5 reports the results of the model (4). It shows that all the previously documented heterogeneity remains in this model, though it is now conditional on several factors. It shows again that in regions with a higher real benefit level of Maternity Capital (relative to local housing prices) and in regions with a shortage of housing, the effect of the program is higher. It shows that the effect of the reform is smaller for non married couples, and for single parents. Importantly, it shows that that triple-differences (γ_1) are statistically significant: for example, in a region with a shortage of housing the effect of the program is higher for married couples and smaller for single parents.

5.2 Short-Run Effect of Regional Maternity Capital Programs

We provide a similar RD analysis of the short-run effects of the 2012 wave of regional Maternity Capital programs. We treat January 2012, the starting date of the majority of the programs, as the threshold date for realized birth outcomes. The specification of the RD regression is similar to (1), where the running variable t is normalized to be 0 in January 2012.

Table 6 shows the results of the RD estimates of the effect of regional Maternity Capital on birth rates. Panels A and B display the results of the RD regressions at the national and regional levels. All panels indicate that regional Maternity Capital results in a further increase in birth rates by 4.7%. The regional programs primarily affect births of first and third children (by 5.4%, and 5.7%, respectively) because the majority of these programs were designed to induce births of children of this parity. Similar to the analysis of the federal program, we provide a robustness check by allowing for a six-month transitional period for the implementation of the reform (see equation (2)). Panel C shows the results of this estimation: that the magnitude of the effects is 1 pp higher.[34]

6 Long-Run Effect on Fertility

We establish evidence of the long-run effect of the program in several steps.

First, using a series of difference-in-difference regressions we show that 1) the Maternity Capital program resulted in higher long-run growth in birth rates of second and third children by parity relative to births of first-parity children; 2) total fertility grew faster in regions with a higher regional subsidy. Second, in within-country analyses we show that an initial short run change in fertility does not vanish, but rather increases over time. Third, we provide indirect evidence to show that the re-scheduling motive is not a driving force in the observed change in fertility. We demonstrate that the time between children as well as the age of the mother did not decrease as a result of the reform, and that the desired number of children significantly increased. Fourth, as a robustness check, we compare the long-term growth in fertility in Russia with Eastern and Central European countries

[34]The regression specification with a transitional period may be more relevant (than RD) in this case because of some fuzziness around the announcement dates for regional programs. While most of the programs started in January 2012, the dates when these programs were announced differ by regions.

that have similar initial trends in fertility and face similar economic conditions. Finally, using our regression estimates, we simulate the effect of Maternity Capital on completed (long-run) cohort fertility rates and show that the reform already increased completed cohort fertility for a sizable number of Russian women.

6.1 Difference-in-Difference Analysis

6.1.1 Cross-regional evidence

To elaborate further on the effect of Maternity Capital programs, we utilize the differences in regional subsidies in a Difference-in-Difference analysis. As was discussed in Section 3, regional programs vary by size. Besides, while most of the Russian regions introduced their own Maternity Capital programs in 2012, some were initiated in 2008, and in some regions, there were no programs at all. Thus, we explore both differences in the size of subsidies and the timing of the regional programs.

Now, we analyze the effect of the programs over the long-run period rather than immediate effects documented in Sections 5.1 and 5.2: we estimate the effect of the programs until the last year of available data, 2017. The demographic literature that analyzes fertility over long time horizons suggests accounting for changes in age distribution among the female population (see, for example, Schoen, 2004).[35] To deal with this issue, we utilize data on age-specific fertility rates and use the mother's age-specific time trends to control for possible demographic changes in the female adult population. We utilize data on birth rates from 2000 to 2017 and use the following Dif-in-Dif regression:

$$Y_{art} = \gamma S_{rt} + \delta_t + \delta_a + t * \delta_a + \delta_r + t * \delta_r + D'_{rt}\Gamma + u_{art} \tag{5}$$

where Y_{art} stands for the log of the fertility rate of mothers of age a, in a region r, at year t. To make the results comparable with Section 5.1, we normalize the regional subsidy by the size of the federal one: S_{rt} stands for the ratio of the regional child subsidy to the subsidy that is given by the federal Maternity Capital program. In our data, S_{rt} varies from 0 (region does not give a subsidy) to 1.09 (region gives a subsidy that exceeds the federal one by 9%). The parameter of interest, γ, shows an additional effect of a regional program in a region that introduces a subsidy that exceeds the average regional subsidy by an amount equal to the federal Maternity Capital. Further, δ_r, δ_t, δ_a, $t * \delta_a$, and $t * \delta_r$ stand for regional, year, mother age fixed effects, mother-age-specific, and regional time trends respectively. The set of control variables D_{rt} includes the log average income and housing availability in a region. Errors are clustered at the regional×year level.

[35] For example, Figure A5 in the Appendix shows that the size of a young cohort of the female population starts decreasing in the late 2010s whereas the size of the older cohort increases. If younger women have different fertility rates compared to older women, then the change in age distribution may bias aggregate estimates of the effect of reforms. Recall that this concern would not contaminate the RD analysis because the size of the female population did not change discontinuously at the time of the introduction of subsidies (see Figure 3). To confirm this, in the robustness section we show that RD estimates for age-specific birth rates are similar to the main RD specification.

Next, similar to the short-run estimates, we check that Maternity Capital has a stronger effect on the fertility rates in regions with a shortage of housing options and the higher relative price of federal Maternity Capital (relative to the local price of housing). To test this prediction, we use a similar Dif-in-Dif specification and include the interaction of these variables with $I(year \geq 2007)_{rt}$.[36] Note that one can interpret variation in the relative price of Maternity Capital as variation in the real price of federal Maternity Capital (in terms of housing), and thus treat these estimates as additional Dif-in-Dif estimates of the effect of the real price of federal Maternity Capital.

Panel A of Table 7 reports the results of the regressions. Column 1 shows the results of the baseline specification. It shows that in a region that gives Maternity Capital equivalent to the federal subsidy, the average fertility rates grew by 7.3%. Columns 3 to 5 show the results of the regression after excluding time trends. All columns show an effect that is similar in magnitude.[37] Column 2 shows that, in regions with lower availability of housing and regions with higher relative prices of Maternity Capital, the effect of the programs on birth rates is greater: in regions where the housing price and the size of the living area are one standard deviation lower than the mean, fertility increases by an additional 4.2 and 5 percentage points, respectively.

Columns 6 and 7 show the results of regressions in which we check the parallel trend assumption. To do so, we take pre-reform years and regress pre-reform birth rates on the time trend multiplied by the level of a (future) regional subsidy (controlling for time trends and the same covariates as in (4)). This interaction term, $\overline{S_r} * t$, shows the differential time trends in birth rates in regions that give different subsidies. Column 6 shows the result of a regression where we use a sample of all regions in the 2001 to 2007, before all Maternity Capital programs started. Column 7 shows the result of a regression where we look at all years, but look only on those regions and those years where there was no subsidy. Columns 8 and 9 check the robustness of results where additional time trends were included in the regressions. Columns 6 to 9 show no difference in pre-reform trends in fertility.

6.1.2 Triple Difference Estimator

The structure of the benefits also allows us to perform a triple difference estimator that serves as an validity check for our results. In the DDD regression we compare the differential growth of third-parity versus lower-parity births in regions with higher versus lower subsidies. Columns 8 and 9 of Table 7 show the results of regressions in which we check the growth of third parity births relative to first and second (column 8) and relatively to only second births (column 9). Both columns show (relative) increase in third parity births.

[36] The set of additional control variables D_{rt} includes the same variables as in (4) plus housing affordability and the interaction of log average income with the federal program dummy.

[37] In the robustness section, we will also look at births by birth order. Unfortunately, the regional-level data on parity-specific birth rates has an important limitation: while the data on all birth rates (without parity) is available for all regions and for the whole time horizon 2000 to 2017, the regional-level data on parity-specific birth rates is available only for half of the regions, and the selection process for this pool of regions is unknown (see Section 4 for discussion). Thus, we leave the discussion of results to the robustness section and treat them as only suggestive.

6.1.3 Robustness check: Births of second and third children by parity relative to first-parity children

Finally, for robustness we check how the reform affects birth rates for second and third children by parity *relative* to births of first-parity children. Second and higher parity children are eligible for a federal child subsidy, whereas first-parity children are not. In addition, third parity children are eligible for the most regional child subsidies. Therefore, one would expect these program to affect the births of higher parity children more.

Yet, comparing the relative growth of birth rates by parity would not allow us to quantify the net effect of Maternity Capital because, as we already argued, Maternity Capital could have an indirect effect on the births of first children too. Our previous analysis confirms this argument by showing an increase in births of first parity children as well.[38]

However, we still can infer the effect of the intensity of the treatment effect under the assumption that births of second and third parity children benefited more from the program than those of first children. To estimate the effect of the intensity of treatment we use national-level data on age-specific birth rates for births of first, second or third children and use the following Dif-in-Dif regression:

$$\begin{aligned} Y_{apt} = \gamma_{21} I(year \geq 2007) I(parity \geq 2) + \gamma_{22} I(year \geq 2012) I(parity \geq 2) \\ \delta_t + \delta_{ap} + t * \delta_{ap} + u_{apt} \end{aligned} \tag{6}$$

where Y_{apt} stands for the log of the fertility rate of mothers of age a, for children of parity p, at year t. Parameters of interest γ_{21} and γ_{22} remain for a relative (in comparison to births of first children) increase in births of second and third children after the 2007 and 2012 reforms, respectively. δ_t, δ_{ap}, and $t * \delta_{ap}$ remain for time fixed effects, age×parity fixed effects, and age×parity-specific time trends. Errors are clustered at the age∗parity level.

Panel B of Table 7 reports the results of regression (5). Column 1 shows a sizable relative increase in second and higher parity children after 2007 and after 2012. Births of second and third children by parity increase by 12% after 2007 and then further increase by 6% after 2012, resulting in a total increase in fertility rate by 18%. Column 2 shows no statistically significant difference in per-reform trends in fertility. In addition, column 3 reports the results of the regression where we estimate relative growth in fertility separately for second and third-parity children.[39] It shows a relative (to births of first children) increase in third parity children by parity after 2007 and after 2012, and an increase in birth rates of second parity children after 2007. These estimates are consistent with the observation that Federal Maternity Capital program gives a subsidy for the second and higher parity child whereas most of the regional programs give a subsidy for the third child by parity.

[38] Also, comparison of first-parity births with higher-parity births suffers from comparability issue: because the fertility rate for first births was high before the reform, there was less room for further increase compare to higher-parity births.

[39] The regression specification in this case is as follows: $Y_{apt} = I(year \geq 2007) * (\gamma_{21} I(parity = 2) + \gamma_{31} * I(parity = 3)) + I(year \geq 2012) * (\gamma_{22} I(parity = 2) + \gamma_{32} I(parity = 3)) + \delta_t + \delta_{ap} + t * \delta_{ap} + u_{apt}$. Control variables and error structure are the same as in regression (4).

6.2 Cumulative Effect

While the previous sections documents several effects of the Maternity Capital programs, the cumulative long-run effect of these policies may differ from the simple summation of these effects for several reasons.

On the one hand, the cumulative effect may be smaller than the sum of short-run effects because of a re-scheduling effect and because of the selection (to compliers) at the initial stage of the program. Parents respond to the introduction of Maternity Capital by re-scheduling a birth to coincide with the time when the policy is effective rather than by increasing the total number of children they want to have. Also, the program in its initial stage may affect the large pool of parents from the older cohort, which later stages of the program would not affect. For example, a couple that gives birth to a second child right before the program became effective may decide to have a third child after its introduction in order take advantage of the subsidy, whereas a couple that gives birth to a second child right after the program's initiation may choose not to have more children because they already got the subsidy for the second child. On the other hand, the cumulative effect may also be greater for alternative reasons. First, as discussed earlier, some families do not immediately react to the campaign by giving a birth to a child. It may take time to conceive and for knowledge of the reform, and trust in the program, to become widespread. Finally, the policy may have a cumulative (multiplicative over time) effect, the result of changes in social habits and preferences (see, for example, Maurin and Moschion, 2009, Yakovlev, 2018).

6.2.1 Estimates of Long-Run Effect and Effect on Completed Fertility Rate

In this section, we provide a cross-regional analysis of the long-run effect. To do so, we utilize regional data on age-specific fertility rates from 2000 to 2017 and use the following regression:

$$\begin{aligned} Y_{art} = \theta_1 I(year \geq 2007)_{rt} + \theta_2 I(year \geq 2012) + \gamma S_{rt} \\ + \delta_a + \delta_r + \delta_c + t * \delta_a + t * \delta_c + t * \delta_r + D'_{rt}\Gamma + u_{art} \end{aligned} \tag{7}$$

where Y_{art} stands for the log of the birth rate of mothers of age a, in region r, at year t. θ_1 and θ_2 show the change in fertility rates across the 2007 to 2017, and 2012 to 2017 periods and γ shows the additional effect of the relative size of the regional subsidy. δ_a, δ_c, δ_r, $t * \delta_a$, $t * \delta_c$, , $t * \delta_r$ stand for age, regional, and year of birth cohort fixed effects, and age-specific, cohort-specific, and region-specific time trends, respectively. The set of control variables D_{rt} includes the log average income and housing availability in a region.[40]

In the main specification, we include both variables that stay for the effect of regional maternity programs, $I(year \geq 2012)$ and S_{rt}. While these two variables are collinear, we decided to include both of them for several reasons. First, while S_{rt} captures the effect of the variation in the size of the

[40] Age corresponds to a 1-year age group, birth cohort corresponds to 5-year birth cohort group. For methodological discussion of decomposition into age and cohort groups, see Deaton (1997).

subsidy, $I(year \geq 2012)$ may capture the additional effects of the county-wide expansion of regional programs, like making regional programs salient, as well as the effect of some other benefits and features of regional programs rather than the size of the subsidy. Yet, for robustness, we estimate the regression (6), where we include only one of the variables for regional programs, $I(year \geq 2012)$ or S_{rt}.

Table 8 documents the results of the regressions. Column 1 shows, that after accounting for various time trends, the federal program results in an increase in birth rates of 8.5 percentage points, and the regional programs result in a further increase of 7 percentage points. On top of a countrywide increase in fertility rate, in a region that introduced a subsidy that exceeded the country average by a level equal to federal Maternity Capital, the subsidy resulted in an additional increase in the birth rates of 9.7% ($\gamma = 0.097$). Note that, θ_1 and θ_2 show an average increase in birth rates (over the existing trend) for the 2007 to 2017 and 2012 to 2017 periods, while the RD estimates obtained in the previous section show an immediate (short-run) change. In the absence of post-reform trends, one should not see any differences between RD and long-run estimates; however, in case of rescheduling (see Adda, Dustmann, and Stevens, 2017), the RD estimates should be higher than the average long-run changes. Indeed, the results show that an average long-run increase is slightly higher than the sum of the short-run changes.

Columns 2 to 5 provide robustness checks. Column 2 shows the results of a regression where only S_{rt} remains for regional programs. It shows that coefficient with S_{rt} becomes twice as high in magnitude, implying that it captures the effect of the other variable that was omitted. Columns 3 to 5 show that the results of the main specification are robust by including different sets of time trends. Indeed, excluding a subset of trends increases the magnitude of the results, possibly due to an increase in the information to noise ratio and diminishing attenuation bias. Columns 6 to 9 show the effect of the programs on birth rates by parity. Columns 8 to 9 show that the federal program affects more births of second and third-parity children, while the regional programs affect more births of third children.[41]

Finally, we estimate the effect of the program separately for every 5-year age group and separately for a 5-year birth cohort of women. Panel B of Table A2 in the appendix shows the results of the regressions. It shows that reform affects birth rates for all age groups and all birth cohorts of women, with a higher effect for women of age 25-39, and for women who born in 1970-1984. This result confirms that the estimated effects are not driven by changes in the distribution of mothers'

[41] Recall that the regional-level data is available only for a subset of Russian regions (see Section 3). For robustness checks, we re-run our regression using age specific national-level data on birth rates. At the national level, we do not observe regional heterogeneity in size of the subsidies, and the regression specification is $Y_{atb} = \theta_1 I(year \geq 2007)_t + \theta_2 I(year \geq 2012)_t + \delta_a + t * \delta_a + D_t'\Gamma + u_{at}$, where Y_{atb} stands for the log of the birth rate of mothers at age a, in year t and for parity b; θ_1 and θ_2 show the change in fertility rates across the 2007 to 2017, and 2012 to 2017 periods, δ_a, $t * \delta_a$ stand for age fixed effects, and age-specific time trends. Table A2, Panel A in the appendix shows the results of national-age-level regressions, and similar (or slightly higher) estimates of θ_1 and θ_2. It also show that the federal program affects more births of second children, while the regional programs affect more births of third children.

characteristics.

This analysis also allows us to simulate the lower bound of the effect on completed cohort fertility rates. Ideally, to infer a long-run effect on fertility, one would have to check the effect of the program on the completed fertility rate, i.e., the average number of children that have been born to women who have completed their childbearing years. In our case, this comparison is infeasible because women who have been affected by the program have not yet reached the end of their childbearing years. Thus, to see whether the program already affected completed fertility rates, we simulate its effect in the unrealistically pessimistic scenario in which women from the treatment group stop giving birth completely after 2018, and at the same time, women from a hypothetical control group experience the highest - over the whole period for which we have data (from 1978 to 2017) - growth in fertility.

We perform this simulation in several steps. First, we take age-specific per-period fertility cohort rates and calculate the fertility in the comparison group by subtracting the estimated effects of the federal and regional Maternity Capital Programs, calculated in Table 8. Then we calculate cumulative fertility rates by summing up per-period fertility for every birth-year cohort. Finally, for the control group, we project a complete cumulative fertility rate under the assumption that women from the control group would experience the highest historical (over 1978 to 2017) growth in fertility.[42]

To explain more clearly, let us take an example. Consider women who are at age 45 in 2017, i.e. born in 1972. For the treatment group, we look at the average number of children that women have in 2017. In the control group, we look at the projected upper bound of the number of children women would have at age 55, i.e. in 2027. According to Table 8, women from the control group have 10 to 25 percent lower fertility during the eleven years of reform (depending on the wave of reform and region). The question we investigate is could the fertility in the control group catch up with the treatment group during the remaining 10 reproductive years (age 45-55). According to vital statistics, fertility decreases after age 30, moreover, after age 35 it decreases rapidly: on average it halves every two years. In 2017 (as well as in other years) the cumulative fertility (or the total number of births) of women of older than 45 is smaller than that of women of age 44 and less than half of that of women of age 43 and more than ten times less than that for females of age 40, etc. Because only for women at age 40 does the reform give an additional 20% increase in fertility, a back of the envelope calculation suggests that women of older than 45 could not catch up. Moreover, even if we use data on the highest historical fertility rates for females older than 45, the result will hold.

Our simulation results show that the size of the cohort women for which we already can claim

[42] To do so, we use data on age-specific per period cumulative fertility rates for years 1978 to 2017. For every age, we pick the maximum (over years) observed percentage increase in cumulative fertility from this age until age 55. Then, to get a projection for completed fertility rates, we multiply the cumulative fertility rate of this age by the maximum historical growth.

an increase in completed fertility rate is large.[43] In a region with an average regional subsidy, we document an increase in completed fertility for a cohort of women aged 38 to 55 in 2017. In a region with a maximum subsidy level, the increased completed fertility is documented for ages 36 to 55.[44] These estimates put a lower bound on cohort size. In particular, these estimates assume that fertility in the control group can catch up at the rates observed during reform. If we evaluate the maximum level of catch-up fertility only for the period before the reform, then we can claim the increase in completed fertility rates for the wider cohort of women (women of age 34-55 in the year 2017). Similarly, we can do it if we assume a reasonable non-zero lower bound in fertility rates in out-of-sample ages for the women in the treatment group.

6.2.2 Additional Evidence: Birth Spacing, Mother's Age, and Desired Number of Children

This section tests the predictions of the re-scheduling argument, which posits that families might react to the subsidy not by increasing their total number of children, but rather by re-scheduling the timing of a planned birth to occur at an earlier date, thereby becoming eligible for Maternal Capital (see Adda et al., 2017, Schoen, 2004).

This re-scheduling behavior should result in a decrease in time spacing between children and in a decrease in the age at which mothers give birth. At the same time, it should not affect the total number of children a couple desires. We test these predictions using household-level data that comes from the RLMS survey as well as from 2010 Census data.[45] Figure 4 demonstrates how the average interval between births, maternal age, and the desired number of children changes between 2000-2015. Panel A shows data on the time between children using 2010 Census and RLMS data; it shows no change (or a slightly positive change for the 2010 Census) in the average spacing between births. Panel B, which plots changes in the average age of mothers, shows an increase in age and positive changes in the slopes of the trends after 2007; in addition, census data shows a small bump after the introduction of Maternity Capital.[46] Panel C shows that the average number of children that a family would like to have jumped after 2007 from 1.4 to 2. To sum up, all of these figures show patterns that are not consistent with predictions of re-scheduling behavior. Table 9 quantifies the results plotted in Figure 4. It shows that after controlling for time trends, the average mother's age at birth increases by 0.23 after the introduction of the Maternity Capital program, and that the average desired number of children increases by 0.18. It also shows no effect of Maternity Capital on the time between children.

[43] See Figure 3 for simulation results.

[44] The regional subsidy levels vary from 0 to 108% of the federal level. In our simulation experiment for a region with a maximum subsidy, we take a hypothetical region where regional subsidy is equal to the to federal level.

[45] The data on birth spacing available from RLMS and the 2010 Census microdata. Both these datasets contain information only on the year of birth, thus we are restricted to using only annual birth data. The data on the desired number of children is available from RLMS up to 2009. Data on later years comes from Rosstat.

[46] Recall that Figure 3 shows changes in average age among all women of reproductive age (not only mothers). It shows no change in average age among the population of females of reproductive age.

7 General Equilibrium Effects

In this section, we discuss the effect of the Maternity Capital program on other markets. For the purpose of explanation, we mainly explore the short-run effect of the 2007 federal Maternity Capital program and leave other analyses for future research.

7.1 Maternity Capital and Housing Market

In Sections 5.1 and 6.1, we already showed the connection between the housing market and the Maternity Capital program by documenting a larger effect of the program in regions where the subsidy has a higher value for the housing market. Figure 11 provides further evidence of the effect of the program on that market. Panel A shows the quarterly and annual indicators of the Russian housing market for the period from 2005 to 2015. It shows an increase in housing prices and the supply of new housing after the announcement of the program at the end of 2006.[47] Panel B uses cross-sectional 2010 Census data to demonstrate the change in housing conditions with the date of childbirth. It shows an increase in the average number of rooms per household member after January 2007. The causal interpretation of the magnitude of the effects that are shown in Figure 11 is suggestive. The effect on the housing market shown in Panel A may be at least partly explained by the development of the mortgage market in Russia. Panel B may, in turn, underestimate the overall change in housing options because of the delay between the birth date and acquisition of the Maternity Capital certificate, then buying and moving into a new home. In addition, many mothers in Russia prefer to stay with grandparents, who can offer help with the care of a newborn child, and delay moving into a new home after childbearing (recall that in the 2010 Census data, those born in 2007 are three years old).

To quantify the effect of Maternity Capital on the housing market in the short-run, we use specification (1) discussed in Section 5.3. In addition, we look at the long-run impact on regional housing prices and the supply of housing. To do so, we use a region-level analog of regression (5) with an extended set of controls. The set of control variables D_{rt} includes log average real income, log population, housing availability, the total amount of mortgage credits given by regional banks, average mortgage interest rate, the average term of mortgages, and number of banks that are certified

[47] The mortgage market has existed in Russia since the middle of the 1990s and grew from 0.2% of GDP in 2004 to 2.5% in 2011. Still, the Russian mortgage market was and is underdeveloped compared to that in Eastern European countries, the European Union and the United States. In 2007 the share of mortgage loans to GDP was 1.5% in Russia compared to 11% in Poland, 40% in the European Union, and more than 60% in the United States. In 2011, the share of mortgage loans to GDP was 2.5%, 19%, 75%, and 40% for Russia, Poland, the United States, and the European Union correspondingly (see http://www.cesifo-group.de/de/ifoHome/facts/DICE/Banking-and-Financial-Markets/Banking/Comparative-Statistics.html). One of the reasons for the small size of the mortgage market is the high price of mortgages in Russia: in 2007 the annual interest rate was 11.4% and 13.7% for mortgages in U.S. dollars and Russian rubles, correspondingly (see Central Bank of Russia, www.cbr.ru).

In the first 12 years after the adoption of Maternity Capital, 5.2 million families used Maternity Capital for housing. The share of transactions that involved the Maternity Capital subsidy constitutes about one sixth of the total transactions in the housing market.

to give mortgages.

Table 10 shows the estimation results. Panel A1 shows the results of short-run regressions, that children born after 2007 live in bigger houses (apartments) and their families share housing with other households less often. Panel A2 shows that the federal Maternity Capital program significantly affects local housing markets: it results in an increase in housing prices and construction of new housing by 16% and 14%, respectively. Panel B shows the results of long-run regressions: a sizable statistically significant effect of the federal Maternity Capital program and smaller and statistically insignificant effect of regional Maternity Capital. Additionally, it shows that Federal Maternity Capital increases housing prices and construction of new housing by 18% and 15%, respectively.[48]

8 Long-run Change in Mothers' Characteristics

In this section, we analyze changes in the characteristics of mothers who gave birth before and after the introduction of the program.

For this purpose, we utilize an individual level panel survey, RLMS, that provides a rich set of maternal characteristics at the moment of the birth of a child.[49] We look at women aged 18 to 50 over the 2000 to 2015 period, and see how the characteristics of those who give birth changed after 2007 using the following Difference-in-Difference regression:

$$\begin{aligned} Y_{it} = \gamma I(year \geq 2007)_{it} \times I(give\, birth)_{it} + \theta I(year \geq 2007)_{it} + \beta I(give\, birth)_{it} \\ + \delta_t + \delta_r + \delta_a + t * \delta_a + u_{it} \end{aligned} \tag{8}$$

The dependent variable Y_{it} stands for the mother's and her family characteristics; $I(give\, birth)$ is an indicator of whether a woman gave birth to a child within the last year; δ_t, δ_r δ_a, $\delta_a t$, represent year, regional, age fixed effects and age-specific time trends, respectively. Errors are clustered at the individual level.

The Dif-in-Dif parameter of interest in this model is γ. It shows how the characteristics of women who gave birth in a particular year changed after 2007 compared to those of other women of the same age and region. Table 11 shows the results of regression (7). While most of the effects are statistically insignificant, it shows that the program affects more older women, married couples, and families that belong to the top 25% by income of the family head.

[48] This result also identifies those who are at a disadvantage because of the program: buyers of homes who did not plan to have a new baby suffer from the rising cost of housing.

[49] For this particular analysis, we chose the RLMS survey over census data for two reasons. First, as discussed in Section 7, census data shows the cumulative effect of selection and program effects. In this section, we are primarily interested in quantifying the selection effect. In addition, census data does not contain information on several important personal characteristics that are of primary interest for this analysis, such as personal or family income. The disadvantage of the RLMS survey relative to Census data is that birth events are rare in the RLMS. The RLMS surveys on average 10,000 respondents in every round and contains data on average on 150 births per every round of the survey. Thus, we do not have enough power for the hypothesis tests in our regression analysis.

9 Willingness To Pay for Additional Child

In this section, we roughly calculate how much the Russian government is paying for each additional child born as a result of the program.[50]

While a family receives 10,000 dollars for a child, that does not imply that the government's willingness to pay for the birth of any additional child is equal to the subsidy level.

Willingness To Pay (WTP) is different because of two reasons. On one hand, the government supports not only to those families who decided to give birth to a child because of Maternity Capital (compliers), but also those who would have given birth independent of the subsidy (always-takers). On the other hand, the subsidy increases birth rates not only of second children, but also of first children, for which the government does not offer Maternity Capital.

The rough calculation of WTP is as follows. The Maternity Capital subsidy results in an increase in fertility rates by 7% and 13% for first and higher birth order children respectively (see Table 2). For this increase in fertility, the government pays *all* (100%) families that give birth to second and higher birth order children (10,000 dollars per child). There are approximately equal numbers of births of first and second or higher birth order children. Thus, the government's willingness to pay for the birth of an additional child that is implied by the Maternity Capital program equals 10,000*(100%/(7%+13%)), or approximately 50,000 dollars.

10 Robustness Checks

To verify the validity of RD estimates, we check whether economic and social factors (average wage, industrial output, mortality, migration, and crime) as well as age distribution in the female population do not change discontinuously at the time of the introduction or announcement of Maternity Capital. This test serves as a validity check for the RD strategy. If the timing of shocks in income or other factors coincides with the introduction of Maternity Capital, then factors other than Maternity Capital may drive the results. Figure A6 in the Appendix shows the results of the RD estimates for different placebo threshold dates: there are no statistically significant discontinuous changes in economic factors in October 2006 (the announcement date of the Maternity Capital program) or in July 2007 (the date of the increase in birth rates).

Table 12 shows the results of various robustness checks of the estimation of the effects on fertility. Columns 1 to 6 of Panel A show the results of an RD estimation using log fertility rates instead of log number of births as a dependent variable. Columns 7 and 8 of Panel A show the results of an RD estimation for only the resident (non-immigrant) population. Panel B shows the results of an RD estimation using the Calonico, Cattaneo, and Titiunik (2014) robust RD estimator. Panel C shows results of regressions where we allow for a transition period for treatment variable from 0 to 1 within a half year before the programs start instead of a discontinuous jump in the treatment variable

[50] For other examples of empirical studies of WTP see Chay and Greenstone, 2005, Greenstone and Jack, 2015.

from 0 to 1 at the threshold date (see Clark and Del Bono, 2016, for a similar approach). In all panels, results correspond to our main specification results. Panel D shows the RD estimates using maternal age cells, and controlling for age-specific time trends (using 2010 Census data). Estimates are similar to our main specification results. Panel E shows the long-run effect of the program on birth rates for births by parity for a subset of regions using available data on birth rates by parity. It shows estimates of the effect of the program that are similar to the main specification. Table A4 in the Appendix documents the results of robustness checks for cross-country case study analysis (see Section 6.4). It shows changes in various alternative measures of fertility in Russia compared to Eastern European countries. Table A4 shows results similar to the main specification.

We further provide several cross-country case-studies where we compare growth in fertility in Russia with neighboring countries that face economic and demographic conditions similar to Russia.

10.1 Validity Check of Short Run effect (RD analysis): Ukraine Case Study

In this section, we discuss the case study of Ukraine, which provides an additional validity check for the RD results. The RD estimates would show a spurious effect if the introduction of Maternity Capital coincides with some unobservable economic or social shock that also affects fertility. Although we already checked this possibility by showing that no other factors changed discontinuously around the threshold date, the Ukrainian case study provides an additional validity check. Facing similar demographic challenges, Ukraine also introduced a sizable child subsidy, but at a different time (one year later than Russia). This allows us to explore the effect of timing in the introduction of the subsidy to see if fertility responded differently in the two countries after the subsidy was introduced.

Ukraine significantly changed its child support policy twice. The first policy change was in April 2005, when the government introduced a one-time child benefit of 8,500 UAH (1,700 dollars). The second increase in child benefits was introduced in the Ukrainian Rada (Parliament) on October 2007 and became effective in January 2008. According to the new policy, a family that gives birth to a first, second, third or higher birth order child receives a child benefit of 12,240 UAH, 25,000 UAH, and 50,000 UAH (2,500, 5,000, and 10,000 dollars), correspondingly. In contrast to Russia, the subsidy in Ukraine can be used for any purpose.

Figure A7, Panel A displays monthly data on the number of births in Ukraine. It shows a jump in fertility rates in July 2008, nine months after the announcement of the child subsidy. It resulted in an immediate increase in the birth rate of 8%. To demonstrate that Ukraine and Russia experienced shocks in fertility at different points in time, we run placebo experiments. We estimate placebo RD coefficients for a jump in fertility within different placebo threshold dates that vary from January 2006 till 2010. Panel B of Figure A7 shows the results of placebo experiments for both Ukraine and Russia. The placebo RD coefficients plot for Russia shows an inverse U-shape with peaks in July 2007. The placebo RD coefficients plot for Ukraine shows two peaks that happen in January 2006 and July 2008.

Thus, we show that the jumps in birth rates in Ukraine and Russia coincided with the changing child policy in these countries. Since the dates of the initiation of their Maternity Capital programs are different, we provide additional evidence that these increases are driven by the change in child support policies and not by random economic or social shocks (that would have been likely to hit both neighbor countries at the same time).

10.2 Robustness Check of Long-Run Effect: Russia vs. Eastern Europe Case Study

As a robustness check, we compare the long-term growth of fertility rates in Russia with Eastern and Central European countries that face similar economic conditions and had similar pre-reform fertility trends.[51] Like Russia, Eastern European countries experienced a drop in fertility rates right after the collapse of the Soviet Union and had similar trends in fertility until 2007.[52] Figure 5 plots the fertility rates for these countries, Russia, and the United States over the 1995 to 2015 period. It shows that, while experiencing similar trends in fertility before 2007, Russia significantly surpassed all the countries from this comparison group after that time. One advantage of using cross-country analysis is that national-level vital statistics are richer compared to within-country statics, which allows us to check the robustness of our conclusions to various measures of fertility, including those that accounts for tempo (re-scheduling) effects. To start, we use a standard measure of fertility, the total fertility rate. Then, following the demographic literature, we also use Bongaarts-Feeney tempo-adjusted TR measures (Bongaarts and Feeney, 1998) to account for the possible rescheduling of birth rates (the so-called tempo effect; see Sobotka, 2004, Yi and Land, 2001, Schoen, 2004, Sobotka and Lutz, 2001).

To estimate the effect of fertility, we employ two Dif-in-Dif regressions in which we compare the growth of fertility rates in Russia with the control group.

Columns 1 to 5 of Table A3 show the results of the regressions with the first control group of countries. For both measures, Russia demonstrates significantly higher growth in fertility rates relative to the control group. The effect is economically large: the lowest estimates show that Maternity Capital results in an average increase in fertility of 11%, and that the effect becomes stronger over time: in 2014, the last year of observation, the tempo-adjusted total fertility rates exceed the pre-reform level by 20%. The effect of the reform is higher for the higher birth order birth rate. The total fertility rate increases by 6.2%, 11.2%, and by 25,9% for first, second, third and higher birth order respectively. Again, the effect becomes stronger over time: in 2014, the total

[51] We exclude former Yugoslavian countries because recent war conflicts might have created different demographic patterns. We also exclude Caucasian and Central Asian countries due to their significantly higher fertility rates. In our first Dif-in-Dif estimates we use the remaining 14 Eastern and Central European countries as a control group.

[52] Some of these countries, including Ukraine and Belarus, adopted pro-natalist policies recently (see Frejka and Gietel-Basten, 2016). Thus, we are likely to underestimate the effect of Maternity Capital in this Dif-in-Dif approach

fertility rates exceed pre-reform levels by 17%, 21%, 34% for the first, second, third, and higher birth order, respectively. Columns 6 to 10 show the results of regressions with the second control group. As expected, in this case, the magnitude of the effect is significantly higher (by approximately one half). According to this specification, in 2014, the total fertility rate exceeds the pre-reform level by 33% for all children, and 24%, 35%, and 57% for the first, second, third and higher birth order, respectively.

We also provide a robustness check for our estimation of the effect on completed fertility rate (see Section 6.2.1). To calculate the cumulative effect of the program, we further compare the cohort cumulative fertility rates in 2006 and 2014.[53] Also, we construct a projected 2016 cohort fertility rate using available data up to 2016 on TFR, and data on age-specific fertility rates until 2014.[54] Figure A8, Panels A and B, show the results of a regression that compares changes in age-specific cumulative fertility rates in Russia and Eastern European countries from 2006 to 2014, and from 2006 to 2016, respectively. To do so, we repeat the Dif-in-Dif regressions described in equation (4) for the years 2006 and 2014 (2016). Figure A9 then shows the Dif-in-Dif coefficients and confidence intervals for regressions for CFR at every particular age. Figure A9 shows that, for any particular age from 20 to 40, the cumulative fertility rate increases by 20% relative to the control group. The growth in fertility is facilitated by births of higher birth order children: while the cumulative fertility for the first child increases by 10%, for higher birth order children, it increases by more than 50%. Thus, one can conclude that the reform results in a significant increase in final cohort fertility for older ages. According to the fertility database, in any year of observation the 99th and 90th percentiles of age at which a mother gives birth to a child does not exceed 40 and 35 years, respectively (see Figure A9). This means that, even in the unrealistically pessimistic scenario where Russian women who are 35 and older in 2016 stop giving birth completely, the average number of children they will have at the end of childbearing years will exceed that of the control group by at least 15%. Again, the total effect on the births of higher birth order children is higher: in the pessimistic scenario, the share of families that have two or more children will exceed that for the control group by 40%.

11 Conclusion

This paper documents the strong effect of sizable child subsidies on fertility.

We find that the introduction of the subsidies in 2007 and 2012 resulted in a significant increase in fertility both in the short run and long run. To identify the causal effect of the subsidy in the short run, we apply the regression discontinuity strategy soon after the subsidy's adoption. The

[53] We restrict this analysis to 2014 because there is no data for fertility rates after that year for most of the countries in the control group.

[54] The human fertility database contains data on TFR, age-specific fertility until the year 2014. The data on later years (2015 to 2017) is collected by the authors using different sources (World Bank, CIA World Factbook, and Rosstat).

short-run effects do not vanish over time. We find that the program created a decade-long increase in fertility of 20% and has already resulted in an increase in completed fertility for a certain cohort of Russian women.

We also find that the subsidy had a substantial general equilibrium effect. It affected the housing market: the price of housing and the supply of new homes increased as a result of the program. It also affected family stability, resulting in a decrease in the share of single mothers and a higher marriage rates.

Finally, we show that this government intervention comes at a substantial cost: each additional birth induced by the program equals approximately 50,000 dollars.

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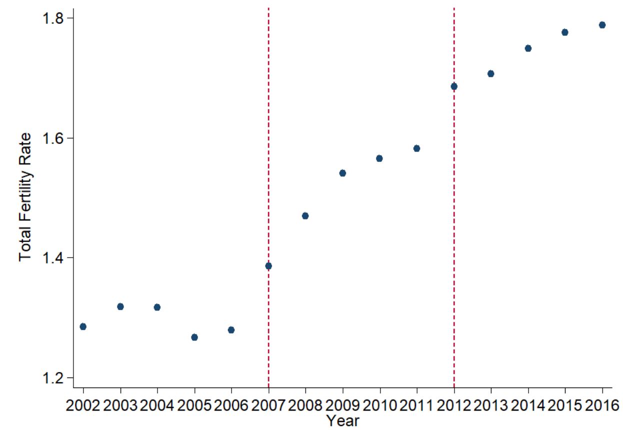
Tables and Figures

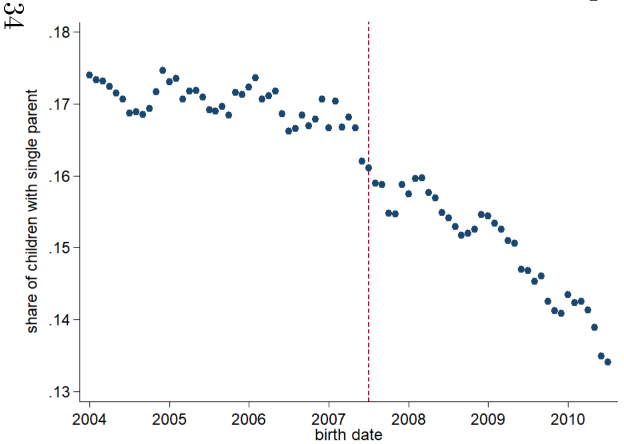
Figure 1: Effect of Maternity Capital Program: highlights

Panel A. Monthly number of births, 2003-2012

Panel B. Total Fertility Rates (TFR) annual data, 2002-2017



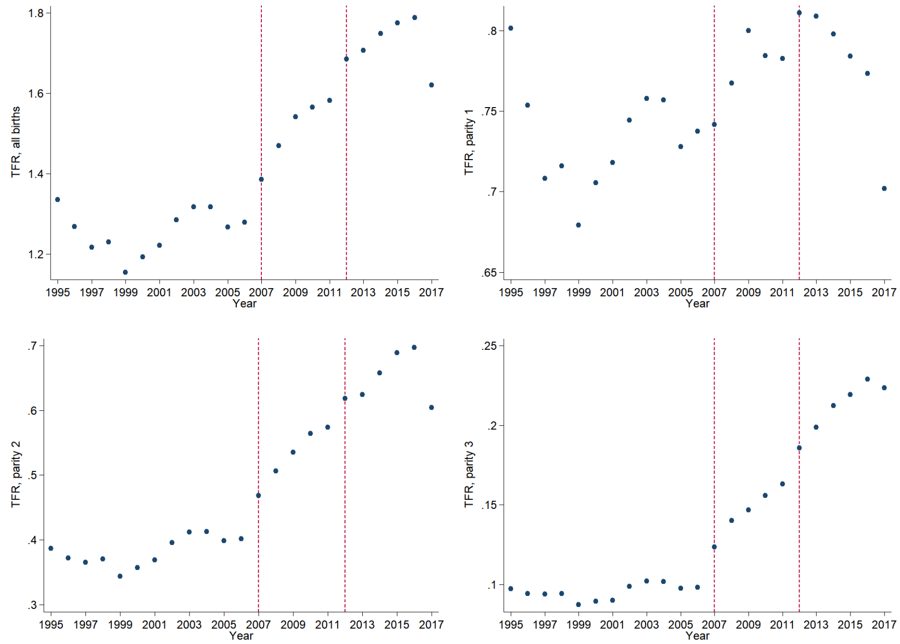
Panel C. Share of children who live with a single parent



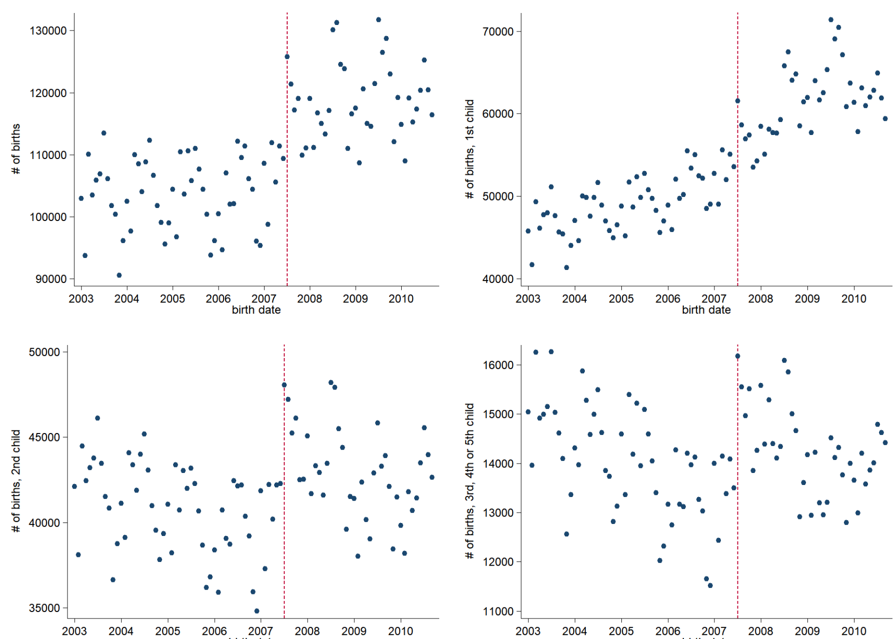
Note: Panel A shows monthly number of births between 2004 and 2012. It indicates jump in the number of births in July 2007, nine months after the announcement of the federal program. Panel B shows annual data on the total fertility rate (TFR) for 2002-2016. It shows increases in the number of births in 2007 and 2012 when the first and second waves of the program were introduced. Because Panel B uses annual data, part of increase in fertility that happen in the middle of 2007 is hidden (smoothed). Panel C plots short-run changes in the share of children that live with a single parent for families that give birth before and after the Maternity Capital program using retrospective 2010 Census data. It shows a significant drop in the share of children who live with a single parent right after the introduction of the federal Maternity Capital program.

Figure 2: Birth rates by parity

Panel A. Total Fertility Rates by parity



Panel B. Monthly births (2010 Census Retrospective Data)



Note: Panel A shows annual data for Total Fertility Rates for all births; and for births by parity. The drop in TFR in 2017 shown in Panel A may happen because families scheduled giving birth within the initially proposed 10-year interval of Maternity Capital. Panel B shows the monthly counts of births by birth order using retrospective data from 2010 Russian Census.

Figure 3: Effect on Completed Fertility: simulations

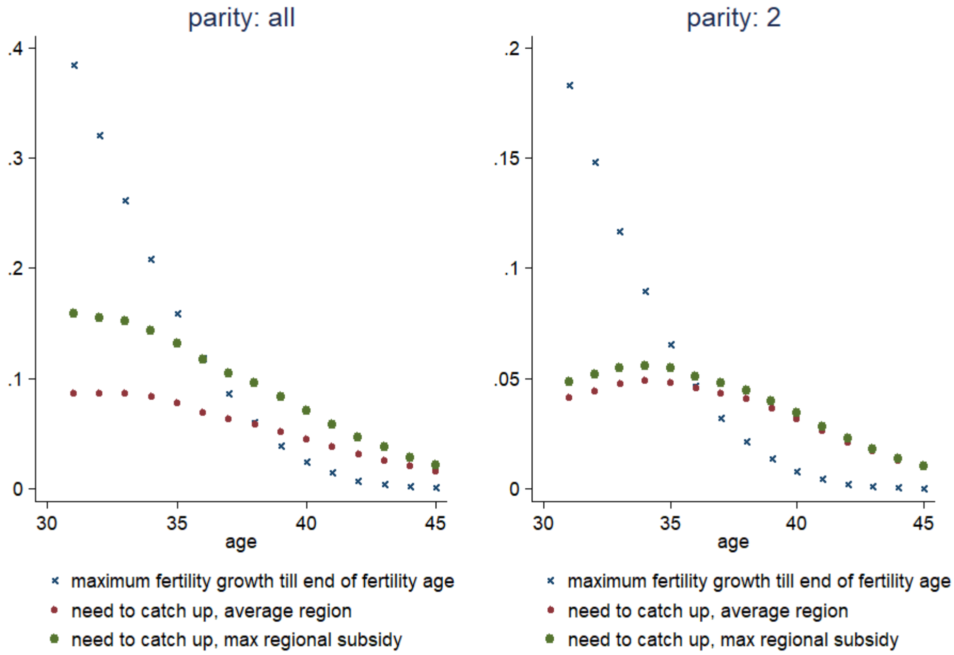
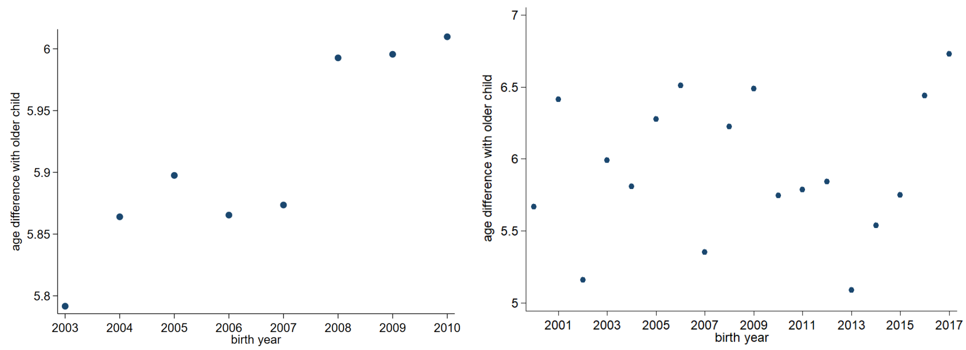
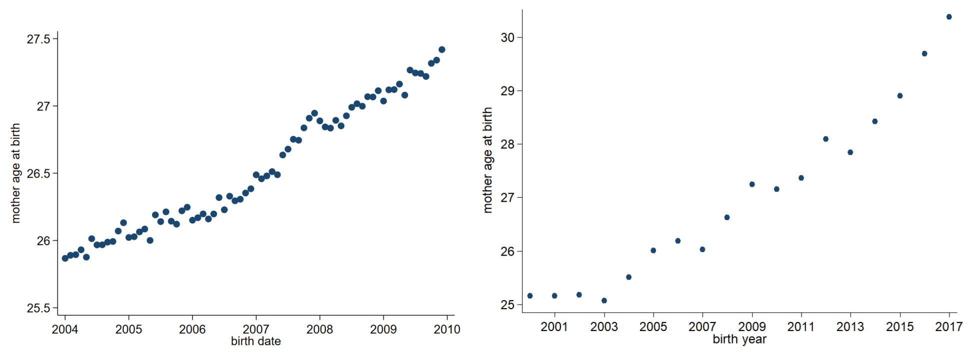


Figure 4: Time spacing between children, mother age, and desired number of children

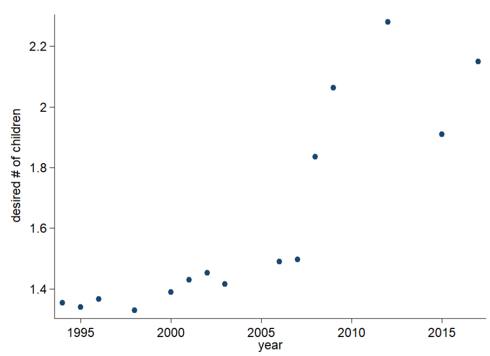
Panel A: Time spacing between births



Panel B: Age of mother



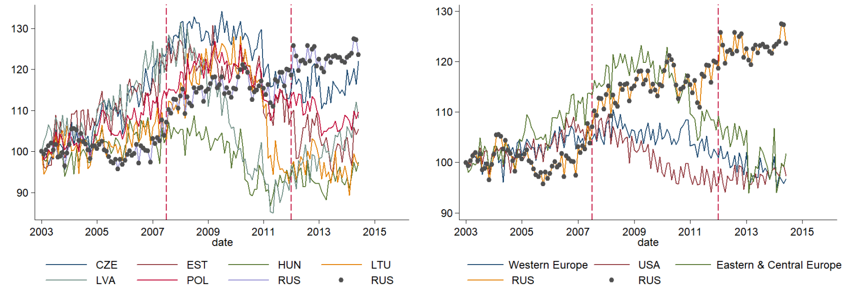
Panel C: Desired size of family



Note: Panel A shows data on time spacing using 2010 Census data (left) and RLMS data (right). Panel B shows changes in the average age of mothers using 2010 Census data (left) and RLMS data (right). Panel C shows that the average number of children that family would like to have according to RLMS (data available for years 1994 to 2009) and Rosstat (data available for years 2010 to 2016).

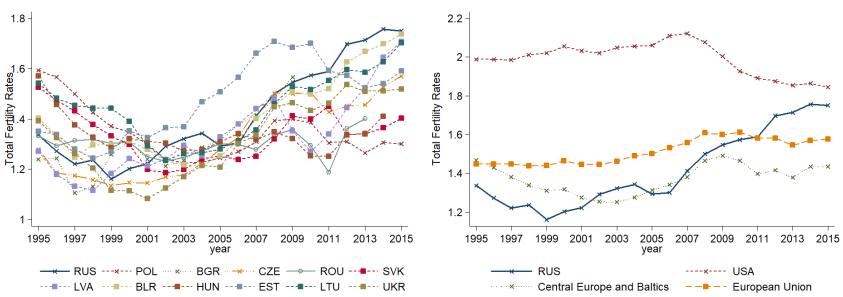
Figure 5: Birth rates in Russia, Eastern European countries, US, and Western Europe

Panel A: Monthly births



Note: Graphs represent normalized monthly births in Russia, Eastern European countries, the United States, and Western Europe. Births are normalized for every country: 2003=100%. A list of Western European countries includes Spain, Austria, Denmark, Finland, Ireland, Italy, France, Portugal, Sweden, Luxembourg, and the Netherlands. List of countries restricted to those for which monthly data is available. Source: http://www.fertilitydata.org/.

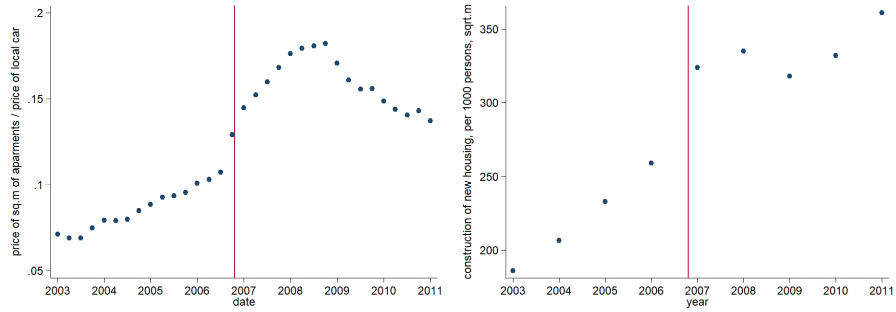
Panel B: Total Fertility Rate by country



Note: Graphs show annual TFR (total fertility rate) in Russia, Eastern European countries, the United States, and Western Europe Source: http://www.fertilitydata.org/.

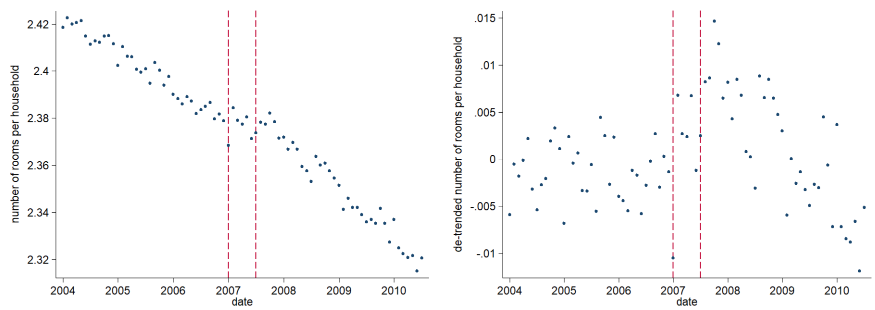
Figure 6: Housing Market, Short Run

Panel A: Housing prices. Panel B: Construction of new houses



Notes: Left panel shows the quarterly data on average housing prices; right panel shows annual levels of construction of new housing. Source: Rosstat 2015

Panel B: Number of rooms per household member by date of birth, Census 2010



Notes: Left panel shows average # of rooms per household member by date of childbirth. The right panel shows the same variable after subtracting the date-of-birth trend. Source: 2010 Census

Tables

Table 1: Value and Usage of federal Maternity Capital, by years

	Value, Maternity Capital		Issued	Total
year	rubles	dollars	certificates	Fertility rate
2006	0	0	0	1.278
2007	250000	9784	313803	1.386
2008	276250	11106	568644	1.469
2009	312162	9827	940290	1.541
2010	343378	11304	789387	1.566
2011	365698	12441	700505	1.582
2012	387640	12475	724279	1.686
2013	408960	12818	786000	1.706
2014	429408	11124	823400	1.749
2015	453026	7388	1073042	1.775
2016	453026	6778	924787	1.788
2017	453026	7808	725000	1.620

Table 2: RD estimates: Effect of federal Maternity Capital program on birth rates

Panel A. National Level Regressions

	(1)	(2)	(3)	(4)	(5)
			log births		
birth order:	all	all	1st	2nd	3rd
I(after 2007)	0.082***	0.089***	0.066***	0.114***	0.144***
	[0.008]	[0.012]	[0.018]	[0.017]	[0.018]
Obs	72	72	72	72	72
Data	HFD		2010 Census		

Panel B. Regional level regressions

	(1)	(2)	(3)	(4)	(5)
			log birth rate		
birth order:	all	all	1st	2nd	3rd
I(after 2007)	0.080***	0.094***	0.081***	0.131***	0.172***
	[0.019]	[0.012]	[0.017]	[0.016]	[0.019]
Observations	6,560	6,400	8,850	8,850	8,845
Data	Rosstat		2010 Census		

Panel C. Rayon level regressions

	(1)
	# of births
I(after 2007)	8.009***
	[2.244]
pp change	.15
Observations	283,339
R-squared	0.001

Panel D. National level regressions with transitional period

	(1)	(2)	(3)	(4)	(5)
			log births		
birth order:	all	all	1st	2nd	3rd
$TR(t)$	0.106***	0.110***	0.077***	0.136***	0.165***
	[0.012]	[0.015]	[0.017]	[0.019]	[0.020]
Obs	73	73	73	73	73
Data	HFD		2010 Census		

Notes: Table 2 shows the results of the RD estimates of the effect of maternity capital on birth rates by parity. Panels A, B, C show coefficients and standard errors for RD regressions based on nation×month, region×month, and rayon ×month levels respectively. In Panel D, the treatment variable $TR(t)$ equals one for dates of birth later than September 1, 2007, and zero for dates before March 1, 2007, and increases linearly from 0 to 1 in a half-year period between March 1, 2007, and September, 1, 2007. Counts of births instead of the log of counts of births are used in Panel C (rayon-level) because counts of births contain zero values. Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Short-Run effect: Heterogeneity in local conditions

Panel A: Regional Level Data				
	(1)	(2)	(3)	(4)
	log birth rate			
birth order	all births	all births	all births	births of 2nd child
I(after 2007)×	-0.006***		-0.007***	-0.025**
living area	[0.001]		[0.001]	[0.012]
I(after 2007)×		0.007***	0.002	0.019***
meters per MC		[0.002]	[0.002]	[0.002]
I(after 2007)×			-0.034**	-0.014***
log income			[0.013]	[0.002]
I(after 2007)	0.080***	0.081***	0.081***	0.131***
	[0.019]	[0.019]	[0.019]	[0.016]
Observations	6,396	6,240	6,240	8,468
R-squared	0.461	0.246	0.497	0.341

Panel B: rayon-level data		
	(1)	(2)
VARIABLES	# of births	# of births
I(after 2007)×	-21.174***	
Rooms per capita	[3.809]	
I(after 2007)×		-2.308***
Rooms per household		[0.675]
I(after 2007)	7.548***	7.548***
	[1.515]	[1.515]
Observations	223,814	223,814
R-squared	0.034	0.016

Notes: Table 3 shows the differential short-run effect of Maternity capital on birth rates in different localities. In regions with a shortage of housing or more affordable housing, the effect of maternity capital is bigger. Counts of births instead of the log of counts of births are used in Panel B (rayon-level) because counts of births contain zero values. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Individual Heterogeneity in Short Run Effect

	(1) log births: married	(2) log births: not married	(3) share non married	(4) log births: both parents	(5) log births: single parent	(6) share with single parent
I(after 2007)	0.103***	0.067***	-0.004***	0.107***	0.083***	-0.005**
	[0.017]	[0.019]	[0.001]	[0.014]	[0.017]	[0.003]
Observations	73	73	73	73	73	73
R-squared	0.921	0.872	0.593	0.936	0.669	0.926
Mean before (always-takers)			.128			.29
Predicted Mean After			.124			.284
Mean for Compliers			.079			.225

	(7) log births: college	(8) log births: no college	(9) log births: primary	(10) share with college	(11) share with primary	(12) mother age
I(after 2007)	0.056***	0.122***	0.149***	-0.015***	0.003***	0.235***
	[0.016]	[0.019]	[0.021]	[0.002]	[0.001]	[0.031]
Observations	73	73	73	73	73	66
R-squared	0.948	0.893	0.858	0.904	0.599	0.988
Mean before (always-takers)				.35	.062	26.21
Predicted Mean After (always-takers + compliers)				.335	.065	26.45
Mean for Compliers				.166	.099	29.02

Note: Table 4 shows the individual-level heterogeneity in RD estimates for the effect of Federal Maternity capital. In columns 7 to 12 we use both the share of married parents and share of single parents as dependent variables because a couple may be married, but not live together. Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1. Data source: 2010 Census.

Table 6: RD estimates: Effect of Regional Maternity Capitals on Birth Rates

Panel A. National Level Regressions

	(1)	(2)	(3)	(4)	(5)
			log births		
birth order:	all	all	1st	2nd	3rd
I(after 2012)	0.047***	0.037**	0.055**	0.021	0.058*
	[0.012]	[0.017]	[0.020]	[0.022]	[0.029]
Observations	71	71	71	71	71
Data source	HFD		2015 Micro Census		
Level			Nation×month		

Panel A. Regional Level Regressions

	(1)	(2)	(3)	(4)	(5)
			log births		
birth order:	all	all	1st	2nd	3rd
I(after 2012)	0.048**	0.043***	0.084***	0.011	0.101***
	[0.024]	[0.015]	[0.026]	[0.019]	[0.033]
Observations	5,460	2,214	2,214	2,213	2,195
Data source	Rosstat		2015 Micro Census		
Level	Region ×month		Region × quarter		

Panel C. National level regressions with transitional period

	(1)	(2)	(3)	(4)	(5)
			log births		
birth order:	all	all	1st	2nd	3rd
$TR(t)$	0.060***	0.038**	0.055**	0.030	0.062
	[0.013]	[0.017]	[0.024]	[0.022]	[0.040]
Observations	73	73	73	73	73
Data source	HFD		2015 Micro Census		
Level			Nation×month		

Notes: Table 5 shows the results of the RD estimates of the effect of Regional Maternity Capital on birth rates by parity. Panels A and B show coefficients and standard errors for RD regressions based on nation×month, region×month (quarter) data. In Panel C, the treatment variable $TR(t)$ equals one for dates of birth later than March 1, 2012, and zero for dates before October 1, 2011, and increases linearly from 0 to 1 in a half-year period between October 1, 2011, and March 1, 2012. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Heterogeneity: micro-analysis

	(1)	(2)	(3)	(4)
		I(give birth)		
I(after 2007)× non married × meters per MC	-0.002***	-0.002**		
	[0.001]	[0.001]		
I(after 2007)× non married × living area	0.002***	0.002**		
	[0.001]	[0.001]		
I(after 2007)× single parent × meters per MC			-0.002***	-0.003***
			[0.001]	[0.001]
I(after 2007)× single parent × living area			0.002***	0.003**
			[0.001]	[0.001]
I(after 2007)× non married	-0.062***	-0.071***		
	[0.001]	[0.002]		
I(after 2007)× single parent			-0.077***	-0.074***
			[0.002]	[0.002]
I(after 2007)× meters per MC	0.001**	0.001	0.001**	0.001
	[0.000]	[0.000]	[0.000]	[0.000]
I(after 2007)× living area	-0.001***	-0.001**	-0.001***	-0.001**
	[0.000]	[0.000]	[0.000]	[0.000]
I(after 2007)×I(college)	-0.007***	-0.008***	-0.003***	-0.002*
	[0.000]	[0.001]	[0.001]	[0.001]
I(after 2007)×I(urban)	0.004***	0.003***	0.000	-0.002*
	[0.001]	[0.001]	[0.001]	[0.001]
I(after 2007)× log average regional income	-0.002	-0.002	-0.001	-0.002
	[0.002]	[0.002]	[0.001]	[0.002]
I(after 2007)× non married ×	0.004	0.003		
log average regional income	[0.003]	[0.004]		
I(after 2007)× single parent×			0.002	-0.002
log average regional income			[0.003]	[0.004]
Observations	12,998,320	4,738,571	12,998,320	4,738,571
R-squared	0.031	0.015	0.031	0.012
Sample (by mother age)	all	25 to 35	all	25 to 35

Note: The regressions uses birth event reconstruction from 5-persent 2010 Census micro data sample. Robust standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. Set of controls includes year, age, regional FE, mother education, I(urban area) and their interaction with I(after 2007).

Table 7: Difference-in-Difference Effect on Fertility

Panel A: Cross-regional Difference-in-Difference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log Fertility Rate								
S_{rt}	0.073***	0.055***	0.117***	0.119***	0.071***				
	[0.023]	[0.020]	[0.027]	[0.027]	[0.022]				
$I(year \geq 2007)\times$		0.012***							
meters per MC		[0.004]							
$I(year \geq 2007)\times$		-0.014***							
living area		[0.003]							
$\overline{S_{rt}} * t$						0.003	-0.005		
						[0.010]	[0.010]		
DDD: $S_{rt}\times I(parity = 3)$								0.328*	0.258**
								[0.176]	[0.116]
Observations	47,926	47,220	47,926	47,926	47,926	20,810	33,036	30,066	19,834
R-squared	0.974	0.975	0.964	0.973	0.964	0.972	0.970	0.779	0.913
Time span			2000-2017						
Regional, Year, Age FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
age-specific trends	YES	YES		YES		YES	YES	YES	YES
time trend			YES						
region-specific trends	YES	YES			YES			YES	YES

Panel B: Relative increase in birth rates of higher-parity children.

	(1)	(2)
	Log Fertility Rate	
γ_{21}: $I(year \geq 2007)\times$	0.116***	
$I(parity \geq 2)$	[0.035]	
γ_{22} : $I(year \geq 2012)\times$	0.061	
$I(parity \geq 2)$	[0.037]	
$\gamma_{21} + \gamma_{22}$	0.177***	
	[0.053]	
$t \times I(parity \geq 2)$		-0.003
		[0.010]
Observations	1,116	434
R-squared	0.966	0.967
Time span	2000-2017	2000-2006

	(3)		
	Log Fertility Rate		
γ_{21}: $I(year \geq 2007)\times$	0.137***		
$I(parity = 2)$	[0.039]		
γ_{22} :$I(year \geq 2012)\times$	0.010	$\gamma_{21} + \gamma_{22}$	0.147**
$I(parity = 2)$	[0.043]		[0.064]
γ_{31}: $I(year \geq 2007)$	0.109***		
$I(parity = 3)$	[0.038]		
γ_{32}: $I(year \geq 2013)\times$	0.154***	$\gamma_{31} + \gamma_{32}$	0.262***
$I(parity = 3)$	[0.038]		[0.055]
Observations	1,672		
R-squared	0.942		
Time span	2000-2017		

Notes: Panel A estimates the differential effect of regional Maternity Capital programs, with control for local economic conditions, regional specific and mother age-specific trends. Panel B shows differential effect of the Maternity Program on higher parity children using national×age specific data. Age and order-specific time trends, as well as year, age and parity fixed effects are included in all regressions of Panel A. In all regressions, robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Long-Run Effect on Fertility Rates: Within country analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Log Fertility Rate				
birth order:	all	all	all	all	all	1st	2nd	3rd	3rd
$\theta_1 : I(year \geq 2007)$	0.085***	0.071***	0.095***	0.120***	0.120***	0.020**	0.154***	0.222***	0.221***
	[0.009]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.011]	[0.010]
$\theta_2 : I(year \geq 2012)$	0.072***		0.077***	0.051***	0.079***	0.061***	0.103***	0.173***	0.170***
	[0.009]		[0.008]	[0.010]	[0.010]	[0.009]	[0.010]	[0.011]	[0.012]
S_{rt}	0.097***	0.175***	0.105***	0.141***	0.142***	0.066***	0.032	0.077***	0.108***
	[0.023]	[0.026]	[0.023]	[0.026]	[0.026]	[0.021]	[0.021]	[0.024]	[0.027]
Observations	47,926	47,926	47,926	47,926	47,926	20,961	20,840	19,536	19,536
R-squared	0.974	0.974	0.974	0.973	0.972	0.974	0.968	0.945	0.945
Age time trends	YES	YES	YES	YES		YES	YES	YES	YES
Regional time trends	YES	YES	YES			YES	YES	YES	YES
Cohort time trends	YES	YES				YES	YES	YES	YES
Time trend					YES				
Regional FE	YES	YES	YES	YES		YES	YES	YES	YES
Age FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
cohort FE	YES	YES	YES	YES		YES	YES	YES	YES

Notes: Table 7 estimates the long-run effect of Federal and Regional Maternity Capital programs on birth rates by parity. The data on age (of mother) specific birth rates is used in regressions, both and national and at regional levels. The long run effect is estimated as a sum of two (DIF) effects of Federal Maternity Capital Program, I(after 2007) and Regional Maternity Capital Programs, I(after 2012)), and differential effect of Regional Maternity Capital Programs (DIF-in-DIF, $(S_{rt} - \overline{S})$), with control for regional specific and mother age specific trends and for regional-specific and mother age specific fixed effects; average income and housing availability. Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1

Table 9: Changes in Mother's Age, Time Spacing between Births and Desired Number of Children

	(1)	(2)	(3)	(4)	(5)	(6)
	mother age at birth		age difference with older child		desired number of children	
I(after 2007)	0.230***	0.256	0.010	-0.258	0.180***	0.058**
	[0.030]	[0.252]	[0.046]	[0.342]	[0.025]	[0.025]
I(after 2007)*t	0.068***	0.311***	0.026	-0.013		0.258***
	[0.017]	[0.055]	[0.022]	[0.072]		[0.012]
t	0.182***	0.068	0.016	0.020	0.039***	0.005
	[0.009]	[0.047]	[0.019]	[0.064]	[0.004]	[0.004]
Observations	72	7,264	198,665	3,130	12,298	12,298
R-squared	0.986	0.062	0.000	0.000	0.075	0.108
Data source	2010 Census	RLMS	2010 Census microdata	RLMS	RLMS	RLMS

Note: Robust standard errors in brackets; *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10: Maternity Capital and Housing Markets

Panel A: Short-run Effect, 2007 Federal Mat. Capital

Panel A1: Regional housing markets

	(1)	(2)	(3)
	log real price, 1 sq.m		log construction of housing
	new	secondary	
I(after 2007)	0.160***	0.196***	0.116***
	[0.037]	[0.034]	[0.029]
Observations	5,629	7,418	580
R-squared	0.322	0.332	0.086

Panel A2: Housing characteristics, Census 2010

	(3)	(4)
	number of rooms per household member	live with other households
I(after 2007)	0.010***	-0.002***
	[0.002]	[0.000]
Observations	73	73
R-squared	0.979	0.651

Panel B: Long-run Effect

	(1)	(2)	(3)
	log real price, 1 sq.m		log construction of housing
	new	secondary	
$I(year \geq 2007)$	0.187***	0.162***	0.147***
	[0.022]	[0.027]	[0.046]
$I(year \geq 2012)$	0.043***	0.026	0.021
	[0.016]	[0.016]	[0.040]
Log real	0.280***	0.411***	0.589***
income	[0.083]	[0.089]	[0.191]
log population	-0.035	-0.377	-2.165**
	[0.545]	[0.535]	[1.059]
Housing	0.013	-0.030	-0.040
availability	[0.016]	[0.020]	[0.027]
log # banks	0.001	-0.047	-0.039
	[0.042]	[0.043]	[0.059]
log credits	0.081***	0.114***	0.101***
	[0.017]	[0.020]	[0.028]
Term credit	0.002***	0.002***	0.000
	[0.000]	[0.000]	[0.001]
Interest rate	0.000	0.003	0.026*
	[0.008]	[0.012]	[0.014]
Time trend	Yes	Yes	Yes
Regional FE	Yes	Yes	Yes
Observations	651	694	697
R-squared	0.540	0.600	0.559
Number of id	76	79	79

Notes: Panel A shows the short-run effect of Federal Maternity Capital. Panel A1 shows the results of regressions at× date level. Housing price data is available at the quarterly level; data on the construction of new housing is available at the annual level. The childbirth date is a running variable in Panel 2. The childbirth date is at the monthly level. Panel B of Table 10 shows the effect of Federal and Regional Maternity Capitals on the regional housing markets. Robust standard errors are in brackets. *** p<0.01, ** p<0.05, * p<0.1

Table 11: Family characteristics before and after program

Panel A: Sample: females age 18-50; Regression controls for age, region, and year fixed effects, and age-specific time trends

	(1)	(2)	(3)	(4)	(5)	(6)
	mother:	mother:	quantiles by head of household income			grandfather
	I(married)	I(college)	bottom 25%	middle 50%	top 25%	college
$I(year \geq 2007)$	0.029	0.008	-0.031	-0.014	0.044**	-0.018
$\times\ I(give\ birth)$	(0.020)	(0.018)	(0.019)	(0.022)	(0.018)	(0.018)
Observations	66,771	66,771	65,920	65,920	65,920	47,678
R-squared	0.116	0.103	0.113	0.070	0.199	0.09

Panel B: Sample: females age 25-50; regression controls for age, region, and year fixed effects, and age-specific time trends

	(1)	(2)	(3)	(4)	(5)	(6)
	mother:	mother:	quantiles by head of household income			grandfather
	I(married)	I(college)	bottom 25%	middle 50%	top 25%	college
$I(year \geq 2007)$	0.074***	-0.006	-0.049*	-0.000	0.050*	-0.044*
$\times\ I(give\ birth)$	(0.028)	(0.029)	(0.029)	(0.032)	(0.027)	(0.027)
Observations	50,304	50,304	49,597	49,597	49,597	37,060
R-squared	0.042	0.099	0.139	0.085	0.208	0.098

Panel B: Sample: females age 18-50; unconditional effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	mother:	mother:	quantiles by head of household income			grandfather:	mother:
	I(married)	I(college)	bottom 25%	middle 50%	top 25%	I(college)	age
$I(year \geq 2007)$	0.078***	0.047**	-0.045**	0.004	0.041**	-0.018	1.861***
$\times\ I(give\ birth)$	(0.020)	(0.020)	(0.020)	(0.023)	(0.020)	(0.019)	(0.260)
Observations	66,771	66,771	65,920	65,920	65,920	47,678	66,771
R-squared	0.010	0.011	0.001	0.016	0.019	0.002	0.021

Notes: Table 10 shows the changes in mothers' characteristics after the introduction of Maternity Capital programs. It shows how characteristics of women who gave birth in a particular year changed after 2007 compared to changes in characteristics of other women of the same age and the same region. Robust standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 12: Robustness check

Panel A. Short Run Effect on Log Birth rates. Federal MC program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log fertility rate, all births						log births	
I(after)	0.082***	0.090***	0.069***	0.085***	0.050**	0.054***	0.094***	0.060***
	[0.008]	[0.013]	[0.017]	[0.013]	[0.023]	[0.012]	(0.018)	(0.016)
Data	HFD	Census	Rosstat	Census	HFD	Rosstat	Census	Census
sample	National×month		Regional×month		National×month		Residents, national×month	
	Federal (2007) MC				Regional (2012) MC		Federal MC	Regional MC

Panel B. CCT Regression Discontinuity estimates. Federal MC program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	log births							
Birth order	all	1st	2nd	3rd	all	1st	2nd	3rd
	National×month level data				Regional×month level data			
Robust RD	0.079***	0.086**	0.094***	0.120***	0.095***	0.091***	0.100***	0.085
	[0.026]	[0.035]	[0.032]	[0.038]	[0.029]	[0.028]	[0.025]	[0.062]
bandwidth	1.951	1.766	1.721	2.096	.66	1.056	1.005	1.302

Panel C. Estimates with a half-year transition period of treatment variable.

	(1)	(2)	(3)	(4)
	log fertility rate, all births			
I(after)	0.092***	0.076***	0.067***	0.063***
	[0.011]	[0.010]	[0.012]	[0.005]
Data	National	Regional	National	Regional
	Federal (2007) MC		Regional (2012) MC	

Panel D. Age of Mother cells. Federal and Regional MC programs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log Fertility Rate				Log Fertility Rate			
birth order	all	1st	2nd	3rd	all	1st	2nd	3rd
RD	0.107***	0.058***	0.154***	0.122***	0.059**	0.044	0.102***	0.086*
	[0.025]	[0.020]	[0.034]	[0.028]	[0.023]	[0.035]	[0.037]	[0.045]
	Federal (2007) MC				Regional (2012) MC			

Panel E. Long-Run effect for births by parity, regional-level regressions

	(1)	(2)	(3)
	Log Fertility Rate		
birth order:	1st	2nd	3rd
$(S_{rtb} - \overline{S}_b)$	0.042		0.156***
	[0.052]		[0.041]
$I(year \geq 2007)$	0.098***	0.189***	0.165***
	[0.008]	[0.009]	[0.011]
$I(year \geq 2012)$	0.061***	0.079***	0.183***
	[0.009]	[0.009]	[0.009]

Notes: Table 11 shows the results of various robustness checks of the estimation of the effects on fertility. Columns 1 to 6 of Panel A show the results of an RD estimation using log fertility rates instead of the log number of births as a dependent variable. Columns 7 and 8 of Panel A show the results of an RD estimation for only resident (without immigrants) population. Panel B shows the results of an RD estimation using CCT regression discontinuity estimator. Panel C shows results of regressions where we allow for a transition period of treatment variable from 0 to 1 within a half of year before the programs start instead of a discontinuous jump of treatment variable from 0 to 1 at the threshold date. Panel D shows the RD estimates using mother age cells, and controlling for age-specific time trends. Panel E shows the long-run effect of the program on birth rates for births by parity using available for a subset of regions data on birth rates by parity. In all panels robust standard errors are in brackets, *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX

Figure A1. RD estimates for different bandwidth sizes

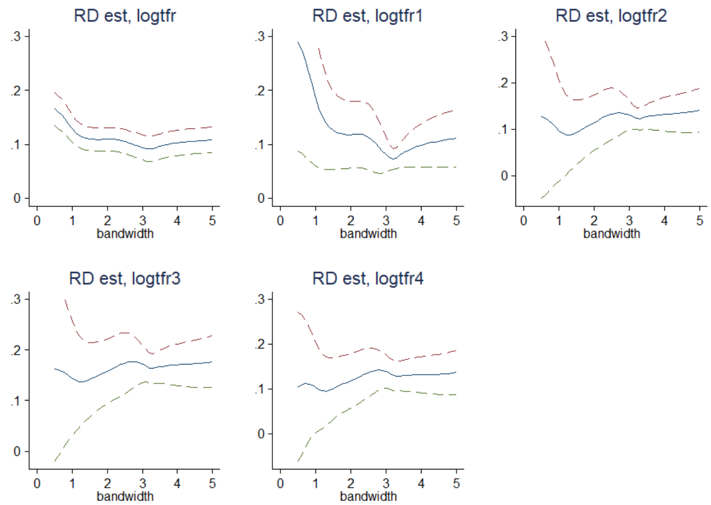
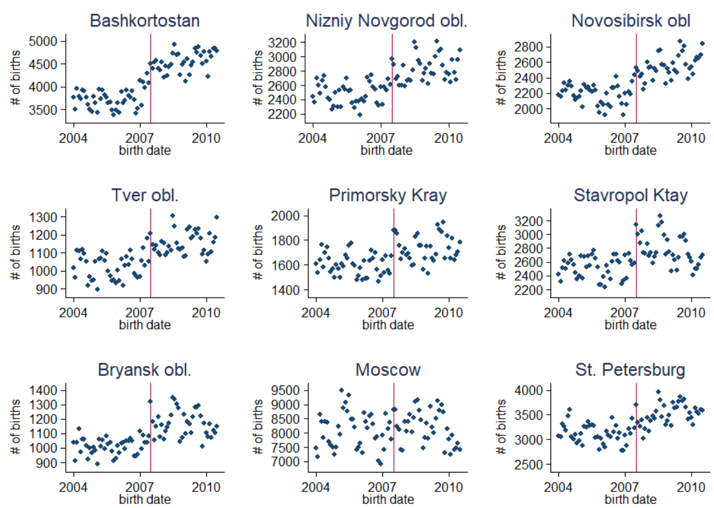


Figure A2. Effect of Maternity Capital, by regions



Note: The graph shows the monthly counts of births in different Russian regions. The dashed line stands for the threshold date for Federal Maternity Capital Program. Source: Russian Census 2010. Monthly bins.

Figure A3. Short-Run effect on births by age of mother and order of child

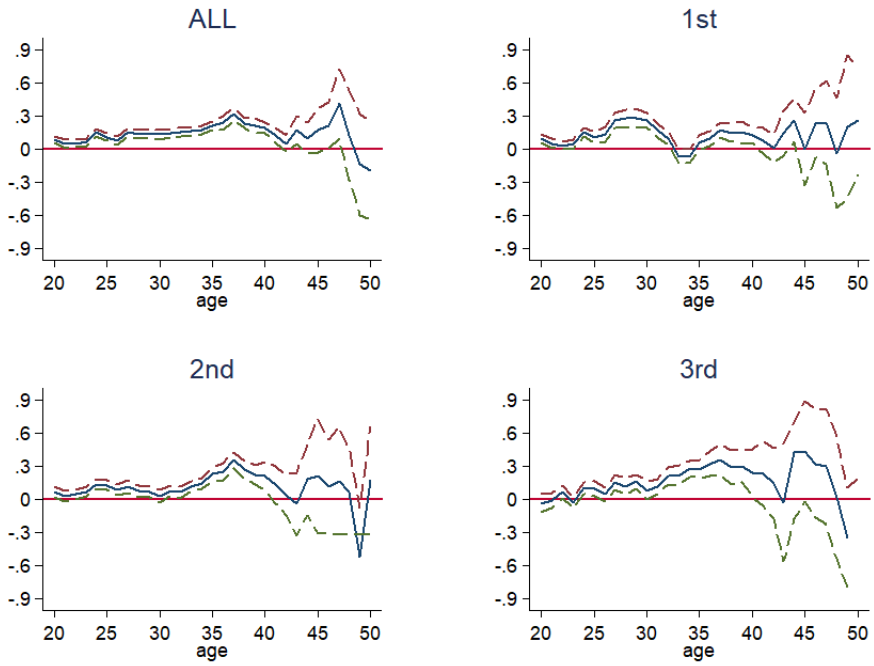
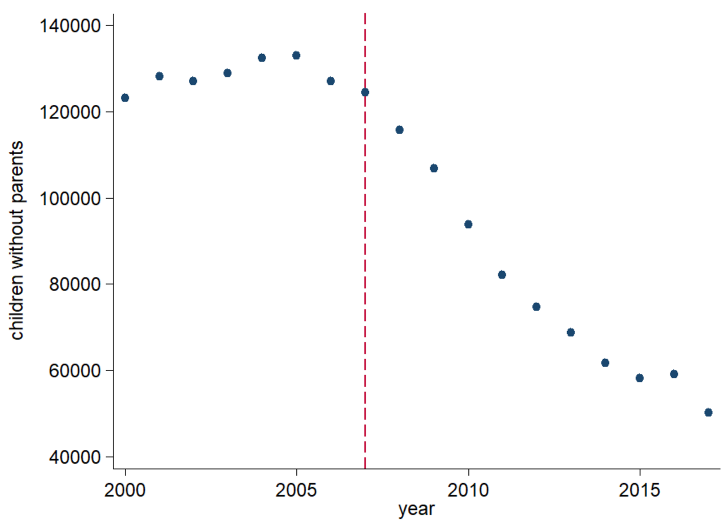


Figure A4. Maternity Capital and the number of children that have been abandoned by parents



Note: Source: The Ministry of Education, http://www.usynovite.ru/structure/

Figure A5. Size of age cohorts of female population over 2000-2017.

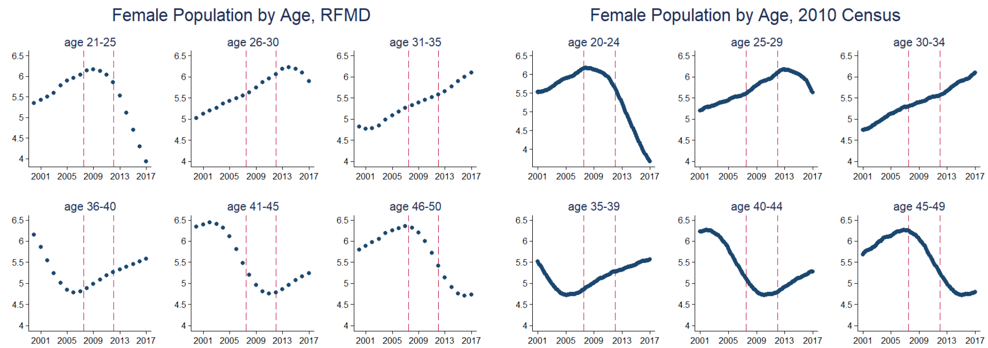
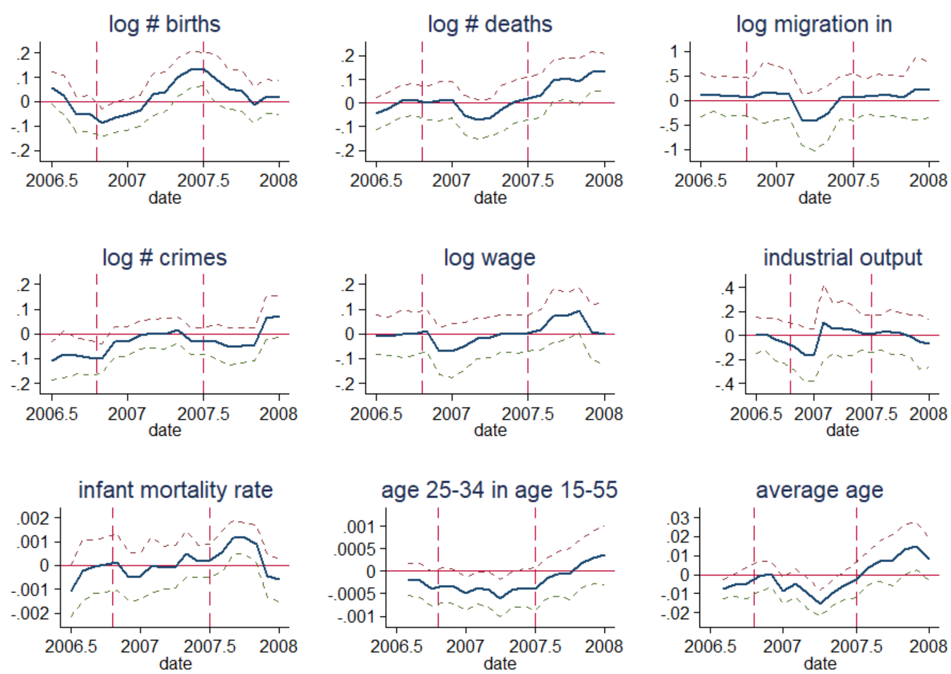


Figure A6. Placebo RD estimates for different placebo dates in different covariates.

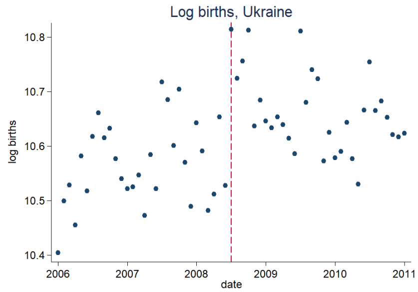


A4

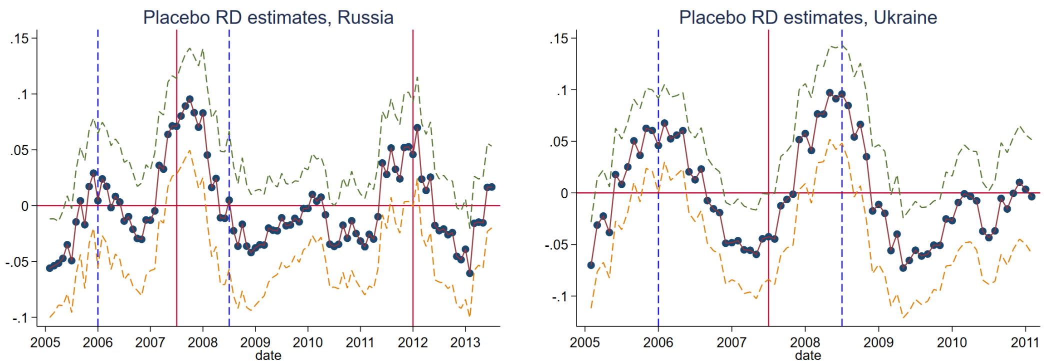
Note: Graphs test for jumps (using placebo RD estimates for different placebo dates) in different covariates. Solid lines represent the announcing date of federal Maternity Capital Program and 9 months after the announcing date. Variables average age and age 25-34 in age 20-55 show characteristics of the distribution of the female population. Variable average age stands for the average age of the female population of reproductive age (age 15-55), variable age 25-34 in age 15-55 stands for share of females aged 25-34 in total female population of age 15-55. Source: Rosstat, www.gks.ru.

Figure A7. Birth rates in Russia and Ukraine

Panel A. Number of births, by birth date. Ukraine



Panel B. Placebo Experiments for RD estimates in Russia and Ukraine



Note: Panel A Graph shows the log of monthly counts of births in Ukraine. The dashed vertical line stands for the starting date of the child support program in Ukraine. Source: Ukrstat, http://ukrstat.gov.ua/. Panel B. Left and right graphs show RD estimates for different placebo dates of the reform in Russia and in Ukraine correspondingly. Solid vertical lines stand for starting dates of Maternity Capital programs in Russia, dashed vertical lines stand for starting dates of child support programs in Ukraine. The RD estimates of the effect of the subsidy in Ukraine shows an jump in the birth rate of 8%.

Figure A8: Change in CFR, all births: 2006 vs 2014 (Left Panel) and 2006 vs 2016 (Right Panel)

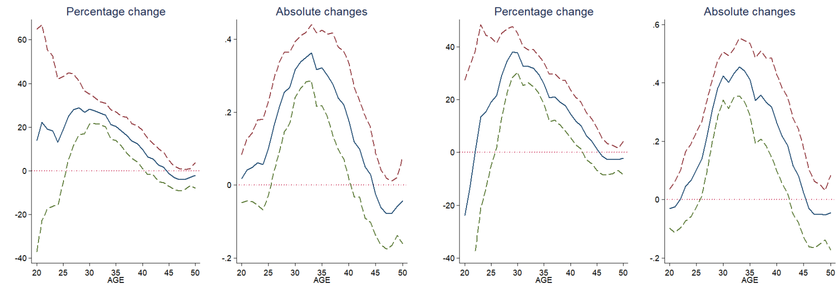
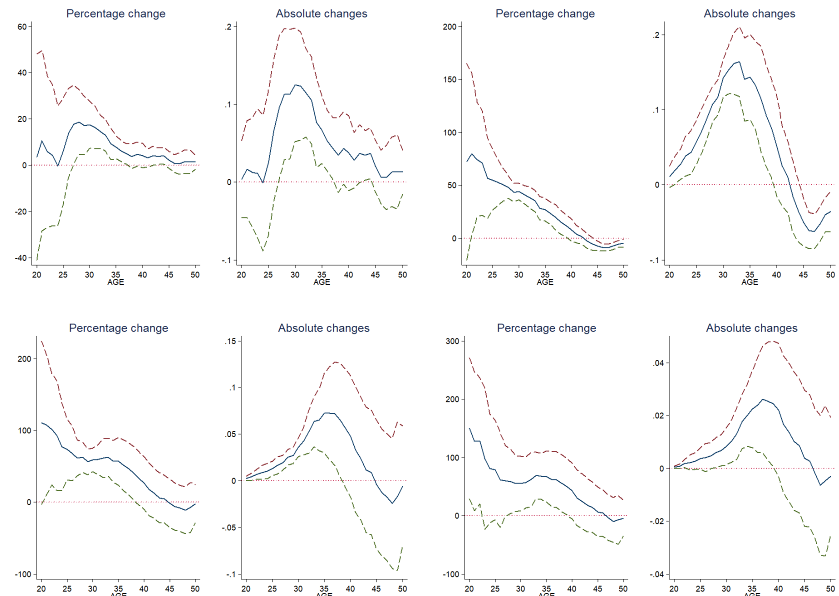


Figure B: Changes in CFR, by birth order: 2006 vs 2014

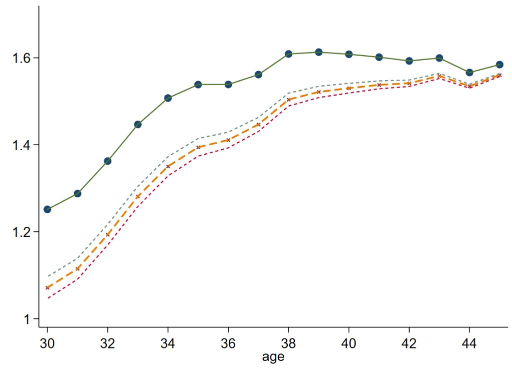


Notes: The figure shows long-run effect (using Dif-in-Dif estimates) of the effect of the Maternity Capitals on age-specific Cumulative Fertility Rates for all births (top panel) and by birth order (bottom panels).

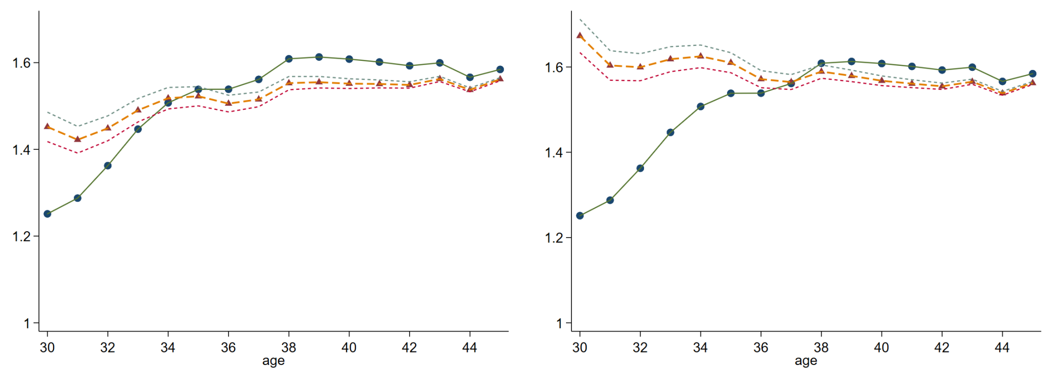
Notes: Solid lines represent age-specific RD estimates of the effect of the federal Maternity Capital program. Dashed lines represent 95% CI.

Figure A9. Changes in Age-Specific Cumulative Fertility Rates

Panel A: Cumulative Fertility Rates of Treatment and Control Group



Panel B and C: Cumulative Fertility Rates of Treatment Group and Projected Maximum of Completed Fertility Rate of Control Group



Notes: In all panels: Solid line: treatment group; dashed lines: control group. Panel A shows cumulative fertility rates for females age 30 to 45 in 2017. Panel B and Panel C compare projected completed fertility rates. Panel B uses pre-program years (years 1992-2006) to project maximal change in fertility for control group, and Panel C uses all years 1992-2017 to make a projection.

Table A1. Summary Statistics

Variable	Obs	Mean	SD	Min	Max
Rayon×month Data, Census 2010					
# of births	228576	48.95	108.4	0	1990
Rooms per HH	228576	2.535	.4127	1.013	4.503
Rooms per cap	228576	.7650	.0941	.386	1.152
Individual Level Surveys, RLMS, females, age 18-50					
I(gave birth)	66771	.0372	.1892	0	1
I(gave birth,					
order≥2)	66771	.0174	.1309	0	1
Relative wage	53710	1	.235	.590	1.979
I(college)	66771	.3041	.460	0	1
Region×month Data, Rosstat					
net migration	11227	256.9	1796	-5335	53629
log # crimes	12764	7.414	1.080	2.83	10.55
log wage	12674	9.806	.5843	8.02	11.65
log unemployment	13367	2.527	.9252	-1.20	5.930
# of births	13302	1759	1664	9	13627
log TR	6560	8.509	.2018	6.39	9.583
marriages/divorces	6708	2.209	3.201	.295	76.38
log house price	6452	10.19	.5002	8.43	12.04
Annual Regional Data, Long Run					
ratio of reg. to					
federal subsidy	664	.1028	.1730	0	1.085
living area	1239	21.68	3.399	4.2	30.4
log real income	1235	6.004	.567	4.126	7.588
metrs of housing per					
Mat. Cap.	1065	10.13	3.061	2.821	19.04

Variable	Obs	Mean	SD	Min	Max
Region×month, Census 2010, Fertility Database					
# of births, by birth order					
all	6400	1622	1398	37	9510
1st	9000	705.7	696.5	0	5832
2nd	9000	561.0	511.7	0	3423
3rd	9000	138.6	172.9	0	1565
4th	9000	38.98	74.3	0	723
Share of Single Parents, by birth order					
all	6400	.1928	.0511	.035	.4375
1st	9440	.381	.0640	0	.666
2nd	9440	.188	.0469	0	.6875
3rd	9426	.178	.0792	0	1
4th	9165	.180	.1667	0	1
National Level×month, Census 2010, Fert.Database					
Births, by birth order (thousands)					
all	81	129.8	10.50	109.9	152.8
1st	120	52.93	11.08	0	74.28
2nd	120	42.08	6.642	0	50.30
3rd	120	10.40	1.634	0	12.45
4th	120	2.923	0.488	0	3.640

Note: Source: Rosstat (www.gks.ru), 2010 Census, 2015 Microcensus, Russian Fertility Database (http://demogr.nes.ru/).

Table A2. Long-Run Effect on Fertility : Supplemental regressions

Panel A: National - level regressions

	log fertility rate	
	(1)	(2)
birth order:	all	1st
$I(year \geq 2007)$	0.16***	0.055*
	[0.020]	[0.031]
$I(year \geq 2012)$	0.11***	0.048*
	[0.025]	[0.026]
Observations	736	704
R-squared	0.997	0.996
	log fertility rate	
	(3)	(4)
birth order:	2nd	3rd
$I(year \geq 2007)$	0.14***	0.14***
	[0.034]	[0.043]
$I(year \geq 2012)$	0.05	0.15***
	[0.047]	[0.050]
Observations	702	690
R-squared	0.996	0.993

Panel B: Cohort and Age-specific effect

log fertility rate, all births							
	(5)		(6)		(7)		(8)
born 1955-59 ×	0.143	born 1975-79 ×	0.129***	age 15-19 ×	0.025	age 35-39 ×	0.143***
$I(year \geq 2007)$	[0.113]	$I(year \geq 2007)$	[0.016]	$I(year \geq 2007)$	[0.045]	$I(year \geq 2007)$	[0.015]
born 1955-59 ×	0.144	born 1975-79 ×	0.104***	age 15-19 ×	0.068*	age 35-39 ×	0.071***
$I(year \geq 2012)$	[0.178]	$I(year \geq 2012)$	[0.014]	$I(year \geq 2012)$	[0.039]	$I(year \geq 2012)$	[0.015]
born 1960-64 ×	0.052	born 1980-84 ×	0.064***	age 20-24 ×	0.009	age 40-44 ×	0.157***
$I(year \geq 2007)$	[0.048]	$I(year \geq 2007)$	[0.021]	$I(year \geq 2007)$	[0.024]	$I(year \geq 2007)$	[0.034]
born 1960-64 ×	0.004	born 1980-84 ×	0.088***	age 20-24 ×	0.107***	age 40-44 ×	0.100***
$I(year \geq 2012)$	[0.090]	$I(year \geq 2012)$	[0.018]	$I(year \geq 2012)$	[0.018]	$I(year \geq 2012)$	[0.030]
born 1965-69 ×	0.115***	born 1985-89 ×	-0.003	age 25-29 ×	0.085***	age 45-49 ×	0.054
$I(year \geq 2007)$	[0.026]	$I(year \geq 2007)$	[0.032]	$I(year \geq 2007)$	[0.018]	$I(year \geq 2007)$	[0.067]
born 1965-69 ×	0.063	born 1985-89 ×	0.077***	age 25-29 ×	0.091***	age 45-49 ×	0.096
$I(year \geq 2012)$	[0.043]	$I(year \geq 2012)$	[0.019]	$I(year \geq 2012)$	[0.016]	$I(year \geq 2012)$	[0.069]
born 1970-74 ×	0.150***	born 1990-94 ×	0.075	age 30-34 ×	0.126***		
$I(year \geq 2007)$	[0.015]	$I(year \geq 2007)$	[0.057]	$I(year \geq 2007)$	[0.014]		
born 1970-74 ×	0.098***	born 1990-94 ×	0.096***	age 30-34 ×	0.102***		
$I(year \geq 2012)$	[0.020]	$I(year \geq 2012)$	[0.029]	$I(year \geq 2012)$	[0.015]		
Observations	47,926			Observations	47,926		
R-squared	0.974			R-squared	0.974		

Note: Robust standard errors in brackets.*** $p<0.01$, ** $p<0.05$, * $p<0.1$. In panel B age, cohort, region fixed effects, and and age, cohort, regional-specific time trends are included in the regressions

Table A3. Long-Run Effect on Total Fertility Rates: Russia vs Eastern Europe Case Study

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
VARIABLES	TR	adjTFR	adjTFR1	adjTFR2	adjTFR3	TR	adjTFR	adjTFR1	adjTFR2	adjTFR3
A)										
Russia×$I(year \geq 2007)$	0.147***	0.212***	0.056	0.070***	0.057***	0.191***	0.340***	0.115**	0.105**	0.078***
	[0.030]	[0.064]	[0.034]	[0.021]	[0.010]	[0.037]	[0.059]	[0.035]	[0.026]	[0.008]
Percentage change:										
2000/6 vs 2007/14	.084	.122	.065	.118	.281	.109	.195	.135	.177	.383
B)										
Russia×I(2007)	0.027	0.060	0.011	0.008	0.024**	0.044	0.138*	0.076*	0.007	0.033*
Russia×I(2008)	0.048	0.159**	0.018	0.071***	0.045***	0.073*	0.286***	0.107**	0.090***	0.055***
Russia×I(2009)	0.076**	0.229**	0.054	0.086***	0.063***	0.112**	0.359***	0.153**	0.091***	0.076***
Russia×I(2010)	0.130***	0.151**	-0.004	0.060**	0.065***	0.197**	0.252**	0.039	0.083**	0.083***
Russia×I(2011)	0.169***	0.163*	0.030	0.047	0.059***	0.237***	0.318**	0.062	0.122**	0.086***
Russia×I(2012)	0.237***	0.360***	0.138**	0.108**	0.079***	0.298***	0.538***	0.178**	0.193***	0.114***
Russia×I(2013)	0.249***	0.389***	0.150**	0.125**	0.072***	0.300***	0.580***	0.202***	0.206***	0.115***
	[0.041]	[0.112]	[0.049]	[0.051]	[0.019]	[0.045]	[0.060]	[0.050]	[0.027]	[0.009]
Russia×I(2014)	0.244***					0.284***				
	[0.046]					[0.065]				
Percentage change:										
2014 vs 2006	.139	.223	.176	.21	.355	.162	.333	.238	.347	.565

Note: Table 8 compares long-term growth in fertility rates in Russia with Eastern and Central European countries. Panel A shows results of the following specification:$Y_{ct} = \theta I(Russia)_c I(year \geq 2007)_{ct} + \alpha I(Russia)_c + \beta I(year \geq 2007)_{ct} + D'_{ct}\Gamma + u_{ct}$. Panel B shows the year-specific effect on fertility in the post-reform years by estimating the following specification: $Y_{ct} = \sum_{y=2007}^{2015} \theta_y I(year = y)_{ct} I(Russia)_{ct} + \alpha I(Russia)_c + \sum_{y=2007}^{2015} \beta_y I(year = y)_{ct} + D'_{ct}\Gamma + u_{ct}$ .In both regressions, the set of controls includes time trend and country-level fixed effects. All regressions use country-level data on tempo-adjusted fertility rates by parity. In regressions shown in columns 1 to 5 all Eastern European Countries are in the control group. In regressions shown in columns 6 to10 Eastern European Countries that have not introduced own pro-natalist policies are in the control group. In all regressions, country FE, time trends are included. Robust standard errors in brackets. *** p<0.01, ** p<0.05,* p<0.1.

Table A4. Long-Run Effect on Fertility Rates.

Panel A All Eastern European Countries are in Control Group.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	ASFR	ASFR1	ASFR2	ASFR3	CPFR	CPFR1	CPFR2	CPFR3
Russia×$I(year \geq 2007)$	0.002***	-0.001**	0.002***	0.001***	0.072***	-0.016*	0.051***	0.026***
	[0.001]	[0.000]	[0.000]	[0.000]	[0.021]	[0.009]	[0.010]	[0.005]
Russia×I(2007)	0.001	-0.001***	0.001***	0.000***	0.011	-0.025***	0.019**	0.010**
Russia×I(2008)	0.001	-0.001***	0.001***	0.001***	0.018	-0.034***	0.027***	0.017***
Russia×I(2009)	0.001	-0.001***	0.001***	0.001***	0.027	-0.025***	0.028***	0.016***
Russia×I(2010)	0.002*	-0.001**	0.002***	0.001***	0.051*	-0.016	0.040***	0.019**
Russia×I(2011)	0.003**	-0.001	0.002***	0.001***	0.077**	-0.005	0.048***	0.024***
Russia×I(2012)	0.005***	-0.000	0.003***	0.002***	0.120***	0.000	0.070***	0.034***
Russia×I(2013)	0.005***	-0.001	0.003***	0.002***	0.136***	0.001	0.079***	0.039***
	[0.001]	[0.001]	[0.001]	[0.000]	[0.031]	[0.015]	[0.015]	[0.007]
Russia×I(2014)					0.144***	-0.015	0.093***	0.046***
					[0.032]	[0.016]	[0.015]	[0.007]

Panel B. Eastern European Countries without pro-natalist policy are in control group.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ASFR	ASFR1	ASFR2	ASFR3	CPFR	CPFR1	CPFR2	CPFR3
Russia×$I(year \geq 2007)$	0.004**	-0.001*	0.003***	0.002***	0.112**	-0.020	0.076***	0.039***
	[0.001]	[0.001]	[0.001]	[0.000]	[0.032]	[0.014]	[0.012]	[0.005]
Russia×I(2007)	0.001	-0.001**	0.001**	0.001***	0.033	-0.021*	0.029**	0.016**
Russia×I(2008)	0.002	-0.001**	0.002**	0.001***	0.047	-0.030**	0.041**	0.024***
Russia×I(2009)	0.002	-0.001**	0.002*	0.001***	0.048	-0.029*	0.041**	0.025***
Russia×I(2010)	0.004	-0.002	0.003**	0.002***	0.098	-0.023	0.070**	0.035***
Russia×I(2011)	0.004*	-0.002	0.004***	0.002***	0.143**	-0.006	0.087***	0.043***
Russia×I(2012)	0.007***	-0.001	0.005***	0.002***	0.185***	0.000	0.108***	0.053***
Russia×I(2013)	0.008***	-0.001	0.005***	0.003***	0.202***	0.001	0.119***	0.059***
	[0.002]	[0.001]	[0.001]	[0.000]	[0.034]	[0.016]	[0.010]	[0.005]
Russia×I(2014)					0.191***	-0.022	0.125***	0.062***
					[0.039]	[0.020]	[0.010]	[0.006]

Note: Robust standard errors in brackets. ***p<0.01,**p<0.05,*p<0.1.
In all regressions, country FE, age FE, time trends are included.

ONLINE APPENDIX

NOTE OA1: FERTILITY RATES MEASURES: CALCULATION

This description is copied from the methodology section in the human fertility database (www.humanfertility.org, Jasilioniene et al 2016).

The period total fertility rate for all birth orders combined and by birth order is computed as follows:

$$\text{TFR}\,(t) = \sum_{x=x_{\min}}^{x_{\max}} f\,(x,t)$$

$$\text{TFR}_i\,(t) = \sum_{x=x_{\min}}^{x_{\max}} f_i\,(x,t)$$

In formula above, $x_{\min}$ corresponds to 12 years or younger. The values of the TFR and TFR_i are computed for age $x_{\max} = 55 + years$; i.e., for the age span covering all reproductive ages. The HFD also lists a parallel estimate based on the sum of the observed fertility rates by age 40; i.e, with $x_{\max} = 39$ years. This information is more useful for cohort fertility analysis, where the cumulated fertility rates of cohorts nearing the end of their reproductive period provide a valuable approximation of their future completed fertility.

Tempo-adjusted total fertility rate Changes in period fertility measures are often driven by the temporary postponement or advancement of births. It is therefore difficult to identify to what extent fluctuations seen in the period TR result from such —timing changes, and to what extent these are —real (quantum) changes that would influence the completed fertility of real birth cohorts. A comparison of period and cohort fertility measures reveals that tempo distortions can cause a substantial gap between the two indicators for an extended period of time (Sobotka, 2004a, 2004b).

Tempo distortions in period fertility measures have inspired efforts to develop an adjustment method that would help to eliminate them. A simple and widely used TR adjustment, based on order-specific TFRs and changes in order-specific mean ages at birth, was proposed by Bongaarts and Feeney (1998). The Bongaarts-Feeney tempo-adjusted TR is computed as a sum of order-specific TFRs adjusted for changes in the mean age of order-specific fertility schedule, $r_i(t)$ as shown in formula below:

$$\text{adj TFR}\,(t) = \sum_i \text{adj TFR}_i\,(t)$$

where

$$\text{adj TFR}_i\,(t) \coloneqq \frac{\text{TFR}_i\,(t)}{1 - r_i\,(t)}$$

Following Bongaarts and Feeney (2000: 563), the adjustment factor $r_i(t)$ is estimated as follows:

$$r_i(t) := \frac{1}{2}\left(\mathrm{MAB}_i(t+1) - \mathrm{MAB}_i(t-1)\right)$$

where $\mathrm{MAB}_i(t)$ is the mean age at birth order i calculated from unconditional age- and order-specific fertility rates

$$\mathrm{MAB}_i(t) := \frac{\sum_{x=x_{\min}}^{x_{\max}} \bar{x} \cdot f_i(x,t)}{\sum_{x=x_{\min}}^{x_{\max}} f_i(x,t)}$$

Value $\bar{x}$ is the mean age at birth within the elementary age interval $[x, x+1)$:

$$\bar{x} = x + a(x)$$

where $a(x)$ is the average share of the age interval $[x, x+1)$ lived before giving birth to a child. We assume that all $a(x)$ values are equal to 0.5 for any completed age x and birth order i (for data organized by Lexis squares and horizontal parallelograms) and zero for any age x reached during the year and birth order i (for data organized by vertical parallelograms).

The tempo distortion in the observed TR then equals adj $\mathrm{TFR}(t) - \mathrm{TFR}(t)$.

Cumulative fertility rates computed for birth cohorts refer to the average number of children born to a woman by a certain age. They are usually shown for all birth orders combined, but they can also be disaggregated by birth order. When computed from period fertility rates, cumulative fertility is a hypothetical construct that can be interpreted as the average number of children that would be born to a woman by age x if she experienced at all ages below x the set of age-specific fertility rates observed in a given year.

In the HFD, cumulative fertility rates are calculated from unconditional age-specific fertility rates sorted by Lexis squares and vertical parallelograms (period dimension) and horizontal parallelograms (cohort dimension):

Cumulative period fertility rates by age x for year t for all birth orders combined (Lexis squares and vertical parallelograms):

$$\mathrm{CPFR}(x,t) = \sum_{z=x_{\min}}^{x-1} f(z,t)$$

Cumulative period fertility rates by age x for year t for birth order i (Lexis squares and vertical parallelograms):

$$\mathrm{CPFR}_i(x,t) = \sum_{z=x_{\min}}^{x-1} f_i(z,t)$$

In formulae above, x and z refer to the age in completed years (ACY) in case of the Lexis squares and the age reached during the year (ARDY) for Lexis vertical parallelograms; $x_{\min}$ corresponds to age 12 or younger. If the upper age limit of the summation is equal or very close to the maximum reproductive age (i.e., if it is 50 or higher), the cumulative fertility rate equals the total fertility rate (TR).

The cumulative cohort fertility rate (CCFR) refers to the average number of children born to a woman from birth cohort c by age x, and is computed by summing up the set of age-specific fertility rates of the cohort c observed over their reproductive lives up to age x. CCFRs are calculated for all cohorts c who are observed from age $x_{\min}$ that is equal to 15 or younger.

Cumulative cohort fertility rates by age x for cohort c for all birth orders combined (horizontal parallelogram) is

$$\mathrm{CCFR}\left(x, c\right) = \sum_{z=x_{\min}}^{x-1} f\left(z, c\right)$$

Cumulative cohort fertility rate by age x for cohort c and birth order i (horizontal parallelogram) is

$$\mathrm{CCFR}_i\left(x, c\right) = \sum_{z=x_{\min}}^{x-1} f_i\left(z, c\right)$$

For birth cohorts, the corresponding quantities represent the completed cohort fertility (CCF). The completed cohort fertility for all birth orders combined and by birth order is computed as follows:

$$\mathrm{CCF}\left(c\right) = \sum_{z=x_{\min}}^{x_{\max}} f\left(x, c\right)$$

$$\mathrm{CCF}_i\left(c\right) = \sum_{z=x_{\min}}^{x_{\max}} f_i\left(x, c\right)$$

The CCF is calculated for all cohorts c that are observed from age $x_{\min}$ that is equal to age 15 or younger until age 50 or older. Again, two types of the CCF are shown. The first one represents the CCF at age 50 or older ($x_{\max} = 49+$ years), whereas the second one shows the CCF (or, more correctly, cumulated cohort fertility) by age 40 (with $x_{\max} = 39$ years) and thus represents an incomplete approximation of the future CCF.