Essays on Family Policy, Fertility and Children's Outcomes

by

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DEDICATION

I dedicate my dissertation to my parents, Natalia and Serghei Malkov, who have encouraged me to dig beneath the surface of any problem, and taught me that hard work and perseverance are necessary to uncover the solution.

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I am especially grateful to my dissertation committee chair, Martha Bailey, for her continued guidance from the very start of my time in the graduate program. I could not have grown into the researcher I am now if it were not for her willingness to invest a lot of time to mentor me. I have also greatly benefited from the willingness to provide comments and feedback of the members of my committee, Charles Brown, John DiNardo and David Lam. I am also grateful to David Agrawal, Hoyt Bleakley, Breno Braga, Jeffrey Smith, Ugo Troiano and Robert Willis for helpful discussions. In creating new knowledge, it is important to receive multiple perspectives, as otherwise research may remain one-dimensional. I hope that incorporating valuable insights from my committee members has enabled me to produce multi-dimensional work.

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ABSTRACT

Governments around the world have long provided financial incentives that encourage and discourage childbearing with a goal of improving children's outcomes. Empirical evidence concerning the effects of these interventions on childbearing and children's outcomes is crucial to resolve on-going policy debates about whether to continue providing, expand, or introduce funding for such programs. However, the long-term effectiveness of these incentives remains an open question because of the difficulty of assessing the causal effects of interventions. My dissertation provides credible estimates of the effects of the introduction of various family policies in Russia and in the United States.

The first and second chapters use newly-available data from Russian censuses to estimate the effect of introducing a maternity benefit program in Russia on short-term and long-term childbearing as well as on long-term children's outcomes. The first chapter exploits the program's two-stage implementation and finds evidence that women had more children as a result of the program. The program induced nearly 5 million births over its duration, where an extra birth cost the government about 1.4 times a year's average national earnings. The second chapter finds that the program resulted in slightly lower educated cohorts, but had no other influence on many economic and family structure outcomes in adulthood. Thus, the maternity benefit program was able to induce more births, but there is little evidence that it induced extra costs on the government in

the longer-term based on outcomes in adulthood of children born after the start of the program.

The third chapter uses restricted American census data to estimate the effect of introducing family planning program funding in the United States on children's economic well-being. This chapter finds that household incomes were 3 percent higher among children born after family planning programs began. These children were also 8 percent less likely to live in poverty and 11 percent less likely to live in households receiving public assistance.

Chapter I

Can Maternity Benefits Have Long-Term Effects on Childbearing? Evidence from Soviet Russia

I.1 Introduction

Eighty-four percent of developed countries offer subsidies or parental leave benefits at an average cost of 2.6 percent of GDP (United Nations 2013). Some of these programs are tremendously expensive.[1] Countries implement these programs in part to increase childbearing, because they are worried that below replacement fertility levels accompanied by an increase in life expectancy may negatively affect their economies in several ways. First, this demographic shift threatens the ability of many countries to finance old-age benefits.[2] Second, a shrinking working-age population compared to a rising elderly population may result in lower economic growth because of a decline in workers per capita (Bloom et al. 2009). Although maternity benefits are costly, they may be ineffective if they result in only a short-run increase in childbearing due to a shift in timing of childbearing, instead of a long-run increase due to women having more children.

[1] Germany, for instance, spends nearly 100 billion dollars per year on family benefits.

[2] The total fertility rate is below the replacement level of 2.1 in 113 countries (CIA Factbook 2013). Population aging is a concern for 92 percent of developed countries, where 22 percent of the population is over 60 (United Nations 2013).

Whether these programs are effective in raising childbearing is an open question. The provision of more generous parental benefits is associated with a country's demand for children, which makes estimating the effects of programs themselves difficult. To address this problem, the literature uses a variety of natural experiments in different countries to provide evidence that parental leave (Lalive and Zweimüller 2009, [Austria]) and cash transfer programs (Cohen et al. 2013, [Israel]; Gonzalez 2012, [Spain]; Milligan 2005, [Canada]) have short-run, positive effects on childbearing in developed countries. But, the limited time-horizon of available data and empirical methods limit inferences about long-run effects. Consequently, much of the estimated effects may reflect changes in the timing rather than a permanent increase in childbearing.

This paper leverages the two-stage introduction of Russia's 1981 expansion of maternity benefits to evaluate both its short-run and long-run effects on childbearing. Similar to the goals of programs in developed countries today, the program was intended to increase completed childbearing by providing a sizable expansion in partially paid parental leave until a child turned one, unpaid parental leave until a child turned a year and a half, and cash transfers at the birth of the first, second or third child. Eighty-five percent of women were eligible for benefits, because they met the provision stipulating that they be in the labor force.

My research design uses the Soviet government mandate that the benefits start in 32 oblasts in 1981 (similar to states; I call these oblasts "early beneficiaries"), and then in 50 oblasts ("late beneficiaries") one year later. The historical vantage point allows me to estimate long-run effects of the maternity benefits expansion. Another contribution of the project is the re-construction of Russian population and characteristics of regions data from recovered vital statistics, censuses, yearbooks, and surveys, which I cull and translate from published sources.

My results show that the 1981 Russian maternity benefit program is associated with an immediate and sustained increase in childbearing. Fertility rates rose *immediately* after the program started by approximately 8.2 percent in the first twelve months. The elasticity of fertility rates with respect to a change in cost of a child is -3.7, which is in the range of short-run effects found in other studies.[3] Three empirical findings underscore that this increase reflects higher completed childbearing. First, period fertility rates remained on average 14.6 percent higher for the ten-year duration of the program. Second, children born after the expansion were more likely to have been higher order births. Third, children born after the expansion were born to mothers who were older and had a longer interval since their previous birth.

This study is the first to find a positive effect of maternity benefits on long-run fertility rates.[4] This is at odds with some papers that have argued that maternity benefits have small or no effects on raising short-run fertility rates and long-run childbearing (Demeny 1986; Gauthier et al. 2007).[5] Theoretically, maternity benefits could decrease childbearing over the longer term. To demonstrate this, I develop two theoretical

[3] I calculate the price elasticity of fertility rates with respect to changes in the cost of having a child for the first 18 years after birth. Most of the prior literature does not convert their estimates into price elasticities. I calculate the price elasticity in Lalive and Zweimüller (2009) as -4.4, the elasticity in Gonzalez (2012) as -3.8, the elasticity in Milligan (2005) as -4.1, while the reported elasticity in Cohen et al. (2003) is -0.54.

[4] Lalive and Zweimüller (2009) find suggestive evidence of long-run effects when women who already had one child were more likely to progress to a second child if they randomly received extra paid parental leave for the first child. The women who were eligible for extra leave were also eligible for an automatic renewal of benefits if they gave birth to the second child up to 27.5 months after the first birth, while women who were not eligible could only receive automatic renewal if they gave birth to the second child 15.5 months after. Thus, this result may largely be due to other types of benefits, and is not applicable to typical interventions affecting benefits for future births.

[5] Papers on the United States focus on low income women to study effects of welfare policies on fertility rates, and find inconclusive evidence due to a large variation in results (Hoynes 1997; Moffitt 1998).

extensions of the Becker and Lewis (1973) model that incorporate two types of maternity benefits—paid leave, which lowers the opportunity cost of childbearing, and a cash transfer, which increases income. The first extension allows parents to choose both the number of children and their level of investments in each child ("quality"), which has the well-known effect of making the income effect theoretically ambiguous. The second extension achieves the same result through non-standard channels: by allowing women to make endogenous choices about their time off from work.

Theoretically, the effect of maternity benefits on childbearing depends on the shadow price of a child (Becker 1965) and on income (Becker and Tomes 1976; Galor and Weil 1999). My empirical results suggest the opportunity cost of having a child as the mechanism for the effect of maternity benefits on childbearing, because less educated and more rural areas experienced a greater increase in fertility rates for the duration of the program. Because the benefits were the same amounts for everyone, women with lower opportunity costs experienced larger relative decreases in the opportunity costs of having a child. Maternity benefits may have had long-run effects on childbearing in Russia, because Russian women had both low opportunity costs of taking time off and low costs of raising children. My best evidence on this comes from the fact that Russian women had lower earnings compared to men and a flatter age-earnings profile than women in other countries (Brainerd 2000, Gregory and Kohlhase 1988). They also had access to wide-spread and affordable preschool care for children of *all* ages.

In summary, this paper shows that maternity benefits can have an effect on both short-run *and* long-run childbearing behavior. In the Russian case, the program induced nearly 5 million births over its duration, where an extra birth cost the government about

1.4 times a year's average national earnings. This paper also provides a cautionary tale for countries interested in designing their maternity benefit programs: behavioral responses to these incentives may vary tremendously across contexts and will reflect the interaction of maternity benefits with other social and public programs.

I.2 Russian Family Benefits

Before 1981, the major beneficiaries of family subsidies were families with many children or low income families. From 1947, women received a one-time payment beginning with their third child and monthly supplements until a child's fifth birthday beginning with their fourth child (Presidium Verhovnogo Soveta 1947). In addition, after 1974, families with monthly per capita income below a threshold received monthly supplements for each child under the age of eight (Presidium Verhovnogo Soveta 1974). However, these benefits did not provide incentives to an "average" family to have a second or a third child.

The government also provided limited benefits to working mothers, but did not provide financial support for women who wanted to stay home with a child for a longer period of time. The most generous benefit was a fully-paid maternity leave of 56 days before and 56 days after a birth. In addition, women could take an unpaid job-protected parental leave until a child turned one (Goskomtrud 1970). Job protected leave was an important feature in Soviet Russia, where about half of the labor force consisted of women in 1980.

In the late 1970s the Soviet government formulated a pronatalist policy with the goal of encouraging all women to have second and third births. The government was interested in securing the replacement level of children “from each physically and morally healthy family, instead of a maximum of children from a minimum of families” (Desfosses 1981). One of the motivations was the desire to achieve more population growth in areas with labor shortages (DiMaio 1981).[6] In January 1978, Litvinova (1978), who was the Senior Research Associate with the Institute of State and Law of the USSR Academy of Sciences, wrote that the government needs to be concerned about the “quality” of the population. She added that “the state cannot be indifferent to what kind of population increase occurs, whether it is highly mobile or, owing to a variety of circumstances (including large families and language barriers), bound to one specific region”.

I.2.1 Description of the Maternity Benefits Expansion

The outcome of these discussions was the 1981 maternity benefit program that aimed to increase childbearing by providing “good conditions for population growth”, to “ease the status of working mothers”, and to “decrease the differences in standard of living depending on having children” (TSK KPSS 1981). The maternity benefit program provided three new benefits. First, women received partially paid parental leave until a child turned one, which represented a flat monthly payment equaling roughly 27 percent of the average national female monthly salary.[7] Second, women could keep their job

[6] Women in the Central Asia republics (e.g. Kazakhstan, Uzbekistan) were the majority of beneficiaries of income-tested and higher parity births benefits in the Soviet Union, because they had more children compared to women in Russia. Mobility from the Central Asia republics was low, thus it was difficult to move excess population to labor-shortage regions. One way to even out the spread of population was to provide incentives for childbearing in areas with lower fertility rates and labor shortages.

[7] Women received 50 rubles per month until the child turned one in Siberia, Far East and the Northern regions of Russia, and 35 rubles in the rest of Russia. Wages were higher in regions that received 50 rubles per month, so benefits represented the same share of average national monthly salary. The benefit at the birth of the first child was 50 rubles, while the benefit at the birth of the second and third child was 100 rubles.

while staying home until their child turned 18 months old. Third, women received a one-time per birth cash transfer which was about 38 percent of the average national monthly salary for first births, and 76 percent of the average national monthly salary for second and third births.[8]

Unlike previous programs for poor or large families, most families were eligible. Women who worked for at least a year as well as students regardless of work experience were eligible for the benefits.[9] Non-working women were also eligible for a small flat payment at the birth of the first, second, and third children which equaled to about 20 percent of the average national female monthly wage. Given that 85 percent of women were employed and 59 percent of college students were women, this program covered the vast majority of women. Notably, the program left previous means-tested benefits and benefits for large families unchanged.[10]

Even though the benefits lasted about ten years, women probably expected them to stay in place permanently. The government had never cancelled previous benefits and typically expanded them. In fact, in 1989 the government further expanded partially paid parental leave until the child turned 18 months, and unpaid leave until the child turned three (Sovmin 1989). However, by 1992, hyperinflation reduced the benefit to almost zero and the subsequent collapse of the Soviet Union ended the program.

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[8] The shares reported are for women in late beneficiary regions. The shares were lower for women in the early beneficiary regions, because wages were higher there.

[9] Students from a wide variety of institutions could receive the benefit – universities, secondary special, professional-technical schools, clinical, and schools that provided courses to improve qualifications.

[10] Before 1980, women received a one-time cash transfer of 20 rubles after the birth of the third child. Under the new program, this benefit became 100 rubles.

I.3 Expected Effects of Russian Maternity Benefits on Childbearing

Introducing paid parental leave and birth transfers directly reduced the cost of having a child for working women. But, evaluating the effects of maternity benefits on childbearing decisions is more complicated than what the standard model of consumer behavior suggests. This section begins with that standard model and then extends it in two ways to discuss the theoretical effects of maternity benefits on childbearing.

I.3.1 Maternity Benefits in the Neo-Classical Consumer Model

In the neoclassical consumer model parents maximize utility, $U(n,z)$, choosing the quantity of children, n, and another consumption good, z. Parents face a simple lifetime budget constraint, $\left[t\left(w_f - a\right) - b\right]n + \pi_z z = T(w_f + w_m)$, where both the husband and wife can work for units of time, T, while the wife receives, w_f, for her work and the husband receives, w_m, for his work. I assume that only women take time off, t, to take care of each child, for which they receive a benefit, a, thus making the benefit-adjusted opportunity cost of childrearing, $t(w_f - a)$. This model does not limit the length of time a mother can receive the benefit, but it is less than one year in the context I study. Finally, women receive a cash transfer, b, for each child.

This model predicts that both an increase in paid leave and cash transfers will increase the quantity of children, as long as children are normal goods. This reflects the fact that the effect of maternity benefits, $\frac{dn}{da}$ and $\frac{dn}{db}$, is the sum of income and substitution effects: the income effect is positive if children are normal goods, while the substitution effect is always positive. However, the quantity of children need not increase when the cost of children decreases, even if children are normal goods, once the model allows parents to choose both quantity *and quality* (Becker and Lewis 1973; Willis 1973) or endogenously choose mothers' *time off from work* (Becker 1965). In the next sections I provide two important extensions of these models that incorporate maternity benefits.

I.3.2 Maternity Benefits with Endogenous Choice of Child Quantity and Quality

Maternity benefits may lead women to have fewer children due to the interaction of quantity and quality. It may become optimal for women to have fewer children, if they invest more into each child because of the benefits (and thus produce children of a higher quality). To demonstrate this, I incorporate the choice of quality of each child, q, in the neoclassical consumer model. Parents maximize utility, $U(n, q, z)$, and face a lifetime budget constraint, $\pi q n + \left[t\left(w_f - a\right) - b\right]n + \pi_z z = T(w_f + w_m)$. Parents pay, π, for a quality unit of a child which they must spend on each child individually, such as college tuition. This budget constraint differs from the one in the standard quantity-quality model, because it incorporates maternity benefits into the cost of a child.

Even if children are normal goods, the observed impact of an increase in income on the number of children may be negative. Children are defined as normal goods if the true income elasticity of quantity (η_n) is positive: η_n uses a measure of income that is calculated using shadow prices (marginal costs) whose ratios in equilibrium are equal to the marginal rates of substitution in the utility function. To fix ideas, consider an increase in income where both child quantity and child quality are normal goods, but the true income elasticity is greater for quality than for quantity, $\eta_q > \eta_n$. At first, both quantity and quality will rise, where quality will rise more than quantity. As shown by Becker and Lewis (1973), the fact that q and n enter the budget constraint multiplicatively leads to an increase in n to increase the shadow price of q and vice versa. Once quality increases by more than quantity, the shadow price of child quantity will rise by more than the shadow price of quality. As a result, an increase in income may reduce childbearing if the true income elasticity for quality is large enough relative to the true income elasticity for quantity. This intuition can be formalized in the following proposition and corollary.

PROPOSITION 1: The effect of parental leave (a) on childbearing is positive if the difference of the true income elasticity for quantity and the true income elasticity for quality, $\eta_n - \eta_q$, is sufficiently large enough.

Proof: I differentiate the first order conditions of the utility maximization problem and the budget constraint (see the full details in appendix A). The resulting elasticity of childbearing with respect to paid leave may be decomposed into a combination of income and substitution elasticities,

$$\frac{dln(n)}{dln(a)} = \frac{atn}{I}\left(\underbrace{\frac{dln(n)}{dln(I)}|_{\pi' s\ const}}_{Income\ Elasticity}\right) + \underbrace{\frac{dln(n)}{dln(a)}|_{I\ const}}_{Substitution\ Elasticity}$$

The income elasticity equals to $\frac{dln(n)}{dln(I)}|_{\pi' s\ const} = f(\eta_n) - g(\eta_q)$, where $f_{\eta_n} > 0$ and $f_{\eta_q} > 0$. Thus, the income elasticity of childbearing is positive if $f(\eta_n) - g\big(\eta_q\big) > 0$, which is true when $\eta_n - \eta_q$ is large enough. The substitution elasticity is always positive.

□

COROLLARY 1: The effect of cash transfers (b) on childbearing is positive if the difference of the true income elasticity for quantity and the true income elasticity for quality, $\eta_n - \eta_q$, is sufficiently large enough.

Proof: See appendix A. □

While the theoretical income effect is ambiguous, a growing empirical literature suggests that husband's earnings (Lindo 2010, Black et al. 2013) and housing wealth (Lovenheim and Mumford 2013) have positive effects on completed childbearing.

I.3.3 Maternity Benefits with Endogenous Choice of Time Off

Another way maternity benefits may lead women to have fewer children is through the interaction of quantity and time off from work. To see this, I extend the neoclassical consumer model by incorporating the choice of time off from work, t, for the mother. Households maximize utility, $U(n, t, z)$, and face a lifetime budget constraint, $\left[t\left(w_f - a\right) - b\right]n + \pi_z z = T(w_f + w_m)$. Unlike in the quantity-quality extension, the household's problem no longer involves quality, but the mother may now choose time off from work, t, while paying wages less the parental leave benefit, $w_f - a$, for each time unit spent out of the labor force for each child.

A notable feature of parental leave benefits is that they subsidize not only the quantity, but also potentially the quality of children because they reduce the opportunity cost of the mother's time with her child.[11] This feature renders the interpretation of time off from work, t, as similar to that of quality, q, in the previous extension. If women

[11] Assuming public child care, the care of relatives or nannies are not perfect substitutes or better than a mothers' time with the child, a mother spending time with the child at a young age may improve child quality. A growing empirical literature finds a positive link between maternity benefits and children's outcomes (Rossin 2011, Carneiro et al. 2014).

spend more time out of the labor force when they have access to partially paid leave, it may result in a greater cost of a child in a world with paid leave compared to the world with unpaid leave. In this case, it may become optimal for women to have fewer children.

Again, consider an increase in income where both child quantity and time off from work are normal goods. Time off will rise by more than quantity, if the true income elasticity is greater for time off than for quantity, $\eta_t > \eta_n$. Then, the shadow price of quantity will rise by more than the price of time off from work because *t* and *n* enter the budget constraint multiplicatively. Finally, an increase in income may reduce childbearing if the true income elasticity for time off is large enough relative to the true income elasticity of quantity. This intuition can be formalized in the following proposition and corollary.

PROPOSITION 2: The effect of parental leave (a) on childbearing is positive if the difference of the true income elasticity of quantity and the true income elasticity of time off, $\eta_n - \eta_t$, is sufficiently large enough and the partial elasticity of substitution between *n* and *t*, σ_{nt}, is positive.

Proof: I differentiate the first order conditions of the utility maximization problem and the budget constraint (see the full details in appendix A). The resulting elasticity of childbearing with respect to paid leave may be decomposed into a combination of income and substitution elasticities:

$$\frac{dln(n)}{dln(a)} = \frac{atn}{I}\left(\underbrace{\frac{dln(n)}{dln(I)}|_{\pi' s\ const}}_{Income\ Elasticity}\right) + \underbrace{\frac{dln(n)}{dln(a)}|_{I\ const}}_{Substitution\ Elasticity}$$

The income elasticity equals to $\frac{dln(n)}{dln(I)}|_{\pi' s\, const} = f(\eta_n) - g(\eta_t)$, where $f_{\eta_n} > 0$ and $f_{\eta_t} > 0$. Thus, the income elasticity of childbearing is positive if $f(\eta_n) - g(\eta_t) > 0$, which is true if $\eta_n - \eta_t$ is large enough. The substitution elasticity equals to, $\frac{dln(n)}{dln(a)}|_{\pi' s\, const} = h(\sigma_{nt})$, where $h_{\sigma_{nt}} > 0$. Thus the substitution elasticity of childbearing is positive if $\sigma_{nt} > 0$. □

COROLLARY 2: The effect of cash transfers (b) on childbearing is positive if $\eta_n - \eta_t$ is sufficiently large enough.

Proof: See appendix A. □

I.3.4 Heterogeneous Responses to Benefits Based on Opportunity Cost and Income

The effect of prices on childbearing depends on the shadow price of a child (Becker 1965) and on income (Becker and Tomes 1976). Similarly, women's adjustment of childbearing in response to maternity benefits depends on their opportunity costs, costs of raising a child and their incomes. The neoclassical model provides different predictions on what types of women adjust their childbearing the most compared to models that allow for the choice of quality or time off from work.

The neo-classical consumer model predicts that women with lower wages and lower incomes will increase their childbearing the most. Due to the fact that the benefit is the same across all women, the benefit represents a greater relative decrease in the opportunity cost of a child and a greater relative increase in income for women with lower opportunity costs and incomes. Thus women with lower opportunity costs experience a larger relative shift of their budget constraint, which leads them to increase their childbearing by more.

However, in models that endogenize either quality or time off women with lower wages and lower incomes may increase their childbearing the least. This is because women for whom the benefits are relatively more important may use the extra income to have children of higher quality both in terms of monetary and time investments. As shown in previous extensions, the sign of the income effect may be negative when η_q and η_t are large relative to η_n. Women with lower opportunity costs may have a more negative income effect, because their η_q and η_t may be larger. This is because it may be less costly to increase child quality when an individual starts at a low initial level of quality.

I.4 Roll-Out of Russian Maternity Benefits and Expected Timing of Responses

On January 22, 1981, the Soviet government passed a ruling about its intention to expand maternity benefits (TSK KPSS 1981). The ruling described the components of the program, the exact benefit amounts, and eligibility requirements. The ruling stated that the program was to be implemented in waves around the country. The early beneficiaries of the program were the Far East, Siberia and the Northern regions of Russia, while the late beneficiaries of the program were the rest of the regions in Russia. The announcement stated that the early beneficiaries would start receiving benefits in 1981, but did not provide information on the exact date of when benefits would start in late beneficiaries.

The Russian government may have decided the order of the benefits based on the fact that it prioritized population growth in areas with labor shortages that were important in terms of industrial production (Weber and Goodman 1981). Perevedentsev (1974), one of the most highly respected population economists, wrote "The task for the next 10 years is to balance the distribution of the country's population and natural resources. It is the East that has the most abundant natural resources."[12] Figure I.1 shows a map of the roll-out of benefits across Russia, where the early beneficiary oblasts are shaded. The early beneficiary oblasts were less populous and only 25 percent of the Russian population resided there.[13]

Early and late beneficiary regions were similar in many respects. Table I.1 shows that the share of employed women, educated individuals, women living in rural areas and children in preschool were similar in 1980. However, the population in early beneficiary oblasts is, on average, younger relative to the late beneficiary oblasts, which may explain higher fertility rates in early beneficiary oblasts.

Women were aware of the details of the maternity benefit program even before the ruling, because the introduction of this program was widely publicized in major Russian newspapers. Next, I present information that I translated from original sources which were in Russian. The first mention of the program was on December 2nd, 1980 in both major Russian newspapers (*Pravda* 1980; *Izvestija* 1980). Several other articles discussed these plans in more detail before and after the official announcement in January

[12] When discussing the Soviet population distribution policy, Chinn (1977) wrote that "ideally, Siberia and the Far East should grow relative to the rest of the country."
[13] Note that based on this evidence, the order of benefits was based on some fixed characteristics of regions, and not based on factors that changed over time. Including region fixed effects in regressions takes these fixed characteristics into account.

and mentioned that they would be introduced in waves starting in 1981 (*Pravda* 1981a; *Izvestija* 1981a).[14] The newspapers also mentioned that newspaper editors received a lot of mail with positive reviews of the program, and one woman wrote that every house in her town was talking about the new program (*Pravda* 1981b; *Izvestija* 1981b).

Later on September 2nd in 1981, a government ruling announced the exact timing of the start of benefits across regions (Sovmin 1981), and it appeared in a major newspaper shortly afterwards (*Izvestija* 1981d). This time it stated that the early beneficiaries would receive benefits starting on November 1, 1981. The late beneficiaries would receive benefits starting on November 1, 1982.[15] The timing of a woman's benefit eligibility depended on her location of permanent work or study and not on the place of birth of the child or residence.[16]

These policy changes lead to the following hypotheses. I expect an increase in childbearing in early beneficiary regions in 1981, and a larger increase in childbearing in 1982 when benefits were in place for a full year. It is likely that women adjusted their childbearing decisions after they found out about the program, because they knew that the program would start in 1981. Fertility rates could go up in early beneficiaries due to an increase in conceptions and in foregone abortions. Fertility rates may have taken longer to adjust if it was costly for women to switch methods of contraception. Given that the

[14] The most detailed description of the law appears on March 31st in 1981 as front page news in both major Russian newspapers (*Izvestija* 1981c; *Pravda* 1981c).

[15] Only women who gave birth after the policy's implementation were eligible to receive the one time birth transfer, while women could receive the monthly paid parental leave for the remaining months after implementation until the child turned one. For example, a woman who gave birth to her first child in November, 1981 in an early beneficiary region received 50 rubles as the birth transfer and ten 50 ruble payments until her child turned one. But, a woman who gave birth to her first child in June, 1981 in an early beneficiary region only received eight 50 ruble payments until her child turned one (Goskomtrud 1982).

[16] Housing shortages and the internal passport system limited geographic mobility (Brainerd 1998).

main method of contraception was abortion, it is reasonable to expect a quick adjustment of fertility rates (Popov 1991).

Women living in late beneficiary regions could respond in two different ways. First, they may have increased childbearing when they became eligible for benefits similarly to women in early beneficiaries. This would manifest as an increase in fertility rates in 1982 and a larger increase in 1983 when benefits were in place for a full year. Second, they may have decided to postpone childbearing after the announcement to be able to take advantage of benefits once they started in their region. This could have resulted in a reduction of fertility rates in these regions before November 1982. However, the incentive to postpone was not as strong because they would still receive some benefits even if they gave birth before their region became eligible.[17] Thus, the sign of the change in childbearing in late beneficiaries is ambiguous in 1982, because of the combination of a potential increase and delay in childbearing.

I.5 The Effect of Maternity Benefits on Childbearing in the Short-Run

I analyze whether maternity benefits affected childbearing in the short-run by quantifying their effect within a year of the start of benefits. To do this, I construct the general fertility rate (GFR) which consists of the annual number of births per thousand women ages 15 to 44. I use non-public data on the number of births by region and year for my time period of interest which I obtained from the Russian Federal State Statistics Service (Rosstat). For the denominator, I use the 1989 Russian census to estimate the

[17] If women gave birth before the start of the program in their region, they could receive benefits for the remaining months until the child turned one.

number of women of childbearing age in each year. The details for fertility rate estimation are in appendix B.

I.5.1 Descriptive Evidence on Childbearing Responses to Maternity Benefits

The evolution of fertility rates in early and late beneficiary oblasts provides preliminary evidence of a positive effect of the maternity benefit program on childbearing. Figure I.2 plots the GFR separately for each area. Early and late beneficiaries had similar trends in fertility rates before the start of the program. Due to the fact that regions were eligible for maternity benefits for part of the year in 1981 and for the whole year in 1982, the GFR in the early beneficiaries jumped in 1981 and rose even further in 1982, which is consistent with the expected response of fertility rates. The GFR in the late beneficiaries did not decrease in 1981, which indicates that women did not delay childbearing after the announcement of the benefits. Also, GFR in late beneficiary regions increased in 1982, which likely resulted from a combination of an increase in childbearing among women induced to have children due to the benefits and a decrease in childbearing of women who decided to postpone childbearing until their regions became eligible for benefits. Similar to behavior in early beneficiary oblasts, GFR in late beneficiary oblasts jumped the most in 1983 when benefits were in place for the whole year. Finally, fertility rates in both early and late beneficiaries stayed higher after the start of the program, suggesting the program increased completed childbearing.

I.5.2 Generalized Differences-in-Differences Framework

I next use this two-stage implementation in a generalized differences-in-differences framework to adjust these raw comparisons for other covariates and construct confidence intervals (Jacobson et al. 1993),

$$GFR_{o,y} = \alpha + \gamma_y + \delta_o + \sum_{t=1975}^{1979} \theta_t * D_o * 1(y{=}t) + \sum_{t=1981}^{1986} \pi_t * D_o * 1(y{=}t) + X_{o,y} + \varepsilon_{o,y} \qquad \text{(I.1)}$$

where $GFR_{o,y}$ is the general fertility rate in oblast o and year y, γ is a set of year fixed effects that capture changes common to all oblasts, D_o equals one if an oblast was an early beneficiary and δ is a set of oblast fixed effects that capture time-invariant oblast level differences. The dummy for the year before the start of the program, $1(y = 1980)$, is omitted which normalizes the estimates for θ and π to zero in 1980.[18] The regression also includes the following limited set of covariates that vary at the year and oblast level and measure production, agricultural output and economic activity: amount of bricks, concrete, timber, meat, and canned goods produced, as well as the value of retail trade.[19] These covariates allow me to test for whether the change in fertility rates was due to some other coincidental economic shocks across regions. The point estimates of interest, θ and π, directly test whether fertility rates were on parallel trends before the start of the benefits and whether estimates diverged after implementation.

The coefficient π_{1981} captures the treatment effect of the program on fertility rates in early beneficiary regions in the first two months of the program; π_{1982} captures the effect in the first full year of implementation. These coefficients may be biased upward if women in late beneficiaries delayed childbearing in response to the benefit announcement. I test whether the fertility rate in late beneficiaries dropped discontinuously between November 1981 and October 1982 in response to the program

[18] For notational convenience, θ is a vector that contains all the coefficients, θ_t for years, *t* from 1975 to 1979, while π is a vector that contains all the coefficients, π_t for years, *t* from 1981 to 1986.
[19] These covariates represent a sample of the most relevant statistics available for that time period at the oblast and year level, which I manually entered using 1975-1992 "Narodnoe Hozyaystvo" yearbooks.

announcement.[20] I find no evidence of this and instead estimate a statistically insignificant 0.63 percent increase in fertility rates in these regions in the year when they were not eligible for benefits.

The coefficients π_{1983} to π_{1986} capture the reversion of the mean difference to its pre-program level after the late beneficiary oblasts gained eligibility. If the late beneficiaries adjusted their fertility rates once they became eligible for the full year of benefits in 1983, π_{1983} to π_{1986} should be smaller in magnitude compared to π_{1982} when the two areas differed in eligibility.

I.5.3 Results: Effect of Maternity Benefits Using Annual Data

Figure I.3 displays estimates from specification (I.1), which represent the covariate-adjusted differences in fertility rates between the early and late beneficiaries in a year compared to the difference in 1980. The results are weighted by the population of women aged 15 to 44 in 1980 in each oblast.[21] The standard errors are clustered at the oblast-level to allow for an arbitrary correlation structure within an oblast.

These results support the hypothesis that increasing maternity benefits increased fertility rates. First, there is no difference in fertility rate trends in the early and late

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[20] For the sample of late beneficiary oblasts, I estimate $GFR_{o,y,m}=\alpha+\gamma_1 y(m)+\beta_3 post+\delta_o+\delta_m+\epsilon_{o,y,m}$, where the unit of observation is at the oblast, *o*, year, *y*, and month, *m*, level. The specification includes a linear time trend, *y(m)*, which accounts for any smooth fertility trends, a set of month fixed effects, δ, which account for seasonality in births, and a dummy, post, for the period from November 1981 to October 1982. The coefficient of interest is β_3 which tests for a discontinuous change in GFR in late beneficiaries during the year when they were not eligible for benefits but the early beneficiaries were. My result is robust to the inclusion of flexible polynomials in time. The details for the construction of month-level fertility rates are described later in this section.

[21] The motivation for weighting is to correct for heteroskedasticity that is related to population size in the oblast by year error terms, when the estimates of the treatment effect with or without weighting may differ (Solon et al. 2013, DuMouchel and Duncan 1983). I fail to reject that the treatment effect coefficients in weighted and unweighted specifications are the same using a Hausman test (Deaton 1997). Thus, I presented the weighted results in the paper.

beneficiaries five years before program implementation. The point estimates for years 1975 to 1979 are individually indistinguishable from zero and follow a flat trend. Second, these results adjusted for covariates support the findings from the unadjusted series from figure I.2. Third, the difference between early and late beneficiaries rises from 1981 to 1982 and reverts toward the program mean from 1983 to 1986.

Fertility rates rose immediately once early beneficiaries became eligible for benefits in 1981. As expected, the increase in GFR was larger when early beneficiaries were eligible for the whole year (1982) than for part of the year (1981) of benefits. Estimates in table I.2 imply that GFR jumped by 2.4 and 6.2 births per 1,000 women of childbearing age in the first partial (1981) and full year (1982) of benefits. This represents a 3.2 and 8.2 percent increase over a pre-treatment mean of 76.0 in early beneficiaries. Fertility rates rose immediately once late beneficiaries became eligible for the full year of benefits in 1983. This is evidenced by the fact that estimates for years 1983 to 1986 are smaller in magnitude than estimates for 1982 and are not statistically different from zero. Thus, the difference between the GFR of early and late beneficiaries shrank due to an increase in fertility rates in late beneficiaries once they became eligible for benefits.

A potential threat to internal validity of these estimates could occur if coincidental policies or economic factors affected outcomes. It is unlikely that the estimated effect of maternity benefits is due to other policies or factors, because other factors would need to change in a specific order in early and late beneficiaries. In particular, my estimates will not capture the effects of maternity benefits if other factors affecting childbearing changed discontinuously in 1981 in early beneficiaries, and in 1982 in late beneficiaries.

In addition to controlling for covariates, I also gather more aggregated data on additional relevant measures of economic activity to look for broader patterns in the evolution of the growth of industrial product, production of oil, and natural gas in a subset of early beneficiaries and in all of Russia.[22] In contrast to hypotheses that the increase in fertility rates was due to other factors, figures in appendix D show little evidence in changes in economic activity at the start of the program, because all indicators visually stayed on the same trend during the relevant period.

I.5.4 Testing for the Role of Abortion Using Monthly Data

The immediate increase in fertility rates could be due to an increase in foregone abortions as well as an increase in conceptions. In the 1980s, abortion was the most widely used method of fertility regulation in Russia, in part because women had limited access to and education on other types of contraception (Popov 1991). Abortions were legal up to the 12th week of pregnancy, but illegal abortions were also prevalent. Thus, fertility rates may have gone up six months after women found out about the benefits if only legal abortions were present. Fertility rates may have gone up even earlier if women had illegal abortions. Further, fertility rates may have gone up nine months after women found out about the benefits due to an increase in conceptions. The adjustment of fertility rates would not be as immediate, if women were using contraceptive methods that required medical help to remove or a waiting period until full fecundity.

The analysis using annual data hides the month-to-month dynamics of fertility rate adjustment that would allow me to distinguish between births due to foregone

[22] I was only able to collect aggregated data on economic indicators in 16 of the early beneficiary oblasts. Data on these indicators is not available for all oblasts at the year and oblast level.

abortions or conceptions. To examine these patterns, I construct month-level fertility rates using retrospective information from the 2002 census to estimate the monthly number of births.[23]

The census does not include individuals who were born in Russia, and who emigrated or died before the census. Consequently, my estimates of the number of births using the census are lower than the true number of births due to mortality and mobility. To correct these estimates, I use information on the true number of births by oblast and year from the vital statistics data presented in the previous section. Specifically, I calculate the proportion of births that are still present in the census

$$p_{o,y} = (Births_{o,y}^{Census})/(Births_{o,y}^{Vital})$$

where $Births_{o,y}^{Census}$ is the number of births in oblast, *o*, in year, *y*, recorded in the census, and $Births_{o,y}^{Vital}$ are the number of births in oblast, *o*, in year, *y*, in the vital statistics data. To adjust census estimates of the number of births, I scale them by the proportion of births that are still present in the census:

$$Births_{o,y,m}^{adjusted} = \frac{Births_{o,y,m}^{Census}}{p_{o,y}} \qquad (\mathrm{I}.2)$$

This procedure scales birth rates in each year by a common factor and assumes no differential mortality/mobility by month. Thus, these estimates preserve some of the information on seasonal variation but are rescaled to be comparable to the annual estimates in the previous section.

[23] These data need to be constructed because month-level fertility rates at the region-level are not published for Russia in my time period of interest.

I use month-level fertility rates to estimate the following extension of specification (I.1):

$$FR_{o,y,m} = \alpha + \gamma_y + \gamma_{m,o} + \delta_o + \sum_{t=1978}^{1983} \sum_{k=1}^{12} \pi_{t(k)} * D_o * 1(y = t, m = k) + X_{o,y} + \varepsilon_{o,y,m} \qquad \text{(I.3)}$$

where $FR_{o,y,m}$ is measured in oblast *o*, year y and month *m*, γ is a set of month-by-oblast fixed effects that capture seasonality in fertility rates in each oblast, 1() is a dummy for month of observation, while the rest of the variables are the same as those defined in equation (I.1). Dummies for months in 1977 are omitted, which normalizes the estimates for π to zero in 1977.[24] The coefficient, $\pi_{t(k)}$, represents the difference between fertility rates in early and late beneficiaries in year, *t*, and month, *k*, compared to the difference in 1977.

Estimates of π in figure I.4 using month-level census data suggest that the increase in fertility rates was partly due to foregone abortions and partly due to an increase in conceptions. Fertility rates in early beneficiary oblasts rose about six months after women found out about the benefits compared to fertility rates in late beneficiaries, which may be attributable to an increase in foregone abortions. This is before benefits began and may reflect the fact that women planning abortions may not have known when they were due. Further, fertility rates rose even further in the period between November 1981 and October 1982 and stayed higher in every month during that period, which may be attributable to an increase in conceptions. Finally, the difference in fertility rates declined once women in late beneficiaries also received the benefits.

[24] For notational convenience, π is a vector that contains all the coefficients, $\pi_{t(k)}$ for years, *t*, from 1978 to 1983 and months, *k*, from 1 to 12.

I.5.5 The Long-Run Effect of Maternity Benefits on Childbearing

This short-run increase in fertility rates may have been the result of two channels –women gave birth sooner, or women had children they would not have otherwise had. Given that the goal of maternity benefits was to induce women to have more children, I test whether maternity benefits resulted in an increase in completed childbearing in three ways. First, I examine the response in fertility rates by parity. Second, I estimate the effect of the program on fertility rates over its duration. Third, I estimate the change in the demographic composition of mothers after the start of the program.

I.5.6 The Effect of Maternity Benefits on Fertility Rates by Parity

As a first test of whether completed childbearing increased due to the program, I examine the response of fertility rates by parity. An increase in first birth fertility rates likely reflected women having desired children sooner, because most women in Russia had at least one child. However, an increase in higher parity fertility rates suggests women had children they would not have otherwise had. Because, fertility rates by parity and oblast are not published in Russia for my time period of interest, I construct first birth and higher order (second and higher order births) fertility rates using the 2010 Census data. I then use equation (I.1) to test whether the increase in fertility rates was due to first or higher parity births.

The results suggest that the short-run increase in fertility rates was due to an increase in completed childbearing, because only higher parity fertility rates increased after the start of benefits. Panel A in table I.3 shows that the higher parity fertility rate in early beneficiaries increased by 17.9 percent (6.7 divided by the pre-program mean of

37.4) in the first year of benefit receipt.[25] Higher parity fertility rates in early and late beneficiaries followed a parallel trend five years before the start of the program in panel A of figure I.5, which supports the internal validity of these results. The late beneficiaries responded to the program after they became eligible for it; the difference between the fertility rates of early and late beneficiaries shrank starting in 1983. Panel B of figure I.5 shows that first parity fertility rates did not change after the program, because there is no trend-break in the difference between early and late beneficiaries after the program compared to before the program began.

I.5.7 The Effect of Maternity Benefits on Long-Run Childbearing

As a second test of whether completed childbearing increased due to the program, I examine the evolution of fertility rates for the ten-year duration of the program. Fertility rates would have risen temporarily, and then have fallen below previous levels, if women only shifted the timing of childbearing. If true, this would have resulted in a zero net increase in fertility rates over the duration of the program. However, fertility rates would have stayed consistently higher for the duration of the program, if women increased their completed childbearing.

To quantify the effects of maternity benefits on childbearing for the duration of the program, I employ an event-study framework. This allows me to study the dynamics of the response over a longer time period. My specification compares outcomes within an oblast before and after the start of the program compared to outcomes right before the start of the program (Bailey et al. 2014). The specification is the following:

[25] I can only obtain fertility rates by parity using census data, so the data used in these regressions is at the month level. The specification is the same as in (I.1), but also includes month by oblast fixed effects.

$$GFR_{o,y} = \alpha + \gamma_y + \delta_o + \sum_{t=-5}^{-1} \theta_t * 1(\text{y-y*=t}) + \sum_{t=1}^{11} \pi_t * 1(\text{y-y*=t}) + X_{o,y} + \varepsilon_{o,y} \qquad \text{(I.4)}$$

where y^* is the year before the start of the program which is different for early and late beneficiaries ($y^* = 1980$ for early beneficiaries, while $y^* = 1981$ for late beneficiaries), and $1()$ is a dummy that represents years relative to the start of the program. In this specification, π_t measures the effect of maternity benefits on fertility rates t years after the start of the program. Note that *t*=0 is omitted. The coefficient π_t when *t*=1 should be the smallest in equation (I.4), because in 1981 the early beneficiary regions while in 1982 the late beneficiary regions only had the benefit in place for two months. Estimates of θ document whether pre-existing trends bias estimates of π, and whether "effects" preceded the program.

The event-study analysis has a causal interpretation if the timing of benefit receipt was conditionally random, which is supported by the fact that fertility rates were on parallel trends in early and late beneficiaries. Also, the fact that the government mandated the order of benefits provides evidence against specific types of regions choosing to receive the benefits first. There are no official published reasons for the choice of the order of benefits by the government. But, because one of the goals was to increase childbearing in less populated and labor shortage regions, it is reasonable that the government prioritized the early beneficiary regions that met these criteria.

I find evidence that completed childbearing increased as a result of the program, because the increase in childbearing was sustained for the entire duration of the program. In figure I.6, I present event-study coefficients θ and π from specification (I.4) that show changes in fertility rates relative to the year before the benefits started. Estimates to the left of the vertical axis capture the evolution of fertility rates before the start date, and estimates to the right of the vertical axis capture the effect of maternity benefits on fertility rates 1 to 10 years after the start of the program.

The maternity benefit program was associated with a sustained increase in childbearing, which was due to an increase in higher order fertility rates. Panel A of figure I.6 shows that fertility rates were higher for ten years after the start of the program compared to before the program. Table I.4 shows that fertility rates were on average 14.6 percent higher 1 to 10 years after the start of the program. There is no evidence that differential pre-existing trends may bias this analysis or that effects preceded the program, because estimates of θ display a flat trend in the five years before the start of the program. Consistent with the previous section, panel B of figure I.6 exhibits a trend-break and shows that higher parity fertility rates were on average 27.8 percent higher for the ten-year duration of the program.

I.5.8 The Effect of Maternity Benefits on the Composition of Mothers

As a third test of whether completed childbearing increased due to the program, I quantify changes in the composition of mothers who gave birth after the program. The increase in fertility rates was due to women having desired children sooner, if women who had children after the program were younger and waited less to have another child compared to women who had children before the program. No publicly available vital statistics data on the characteristics of mothers in Russia are available. Instead, I use the Generations and Gender Survey conducted in 2004 to obtain characteristics of mothers based on when their children were born.

I find evidence that completed childbearing increased as a result of maternity benefits, because women who had children after the program were older and waited longer to have a child. The panels of figure I.7 present results from specification (I.4), where I use age of mother at birth (panel A), years since last birth (panel B) and number of previous children at the time of birth (panel C) as dependent variables.[26] Women who had children 1 to 10 years after the start of the program were consistently older, had more previous children, and waited more years to have a child compared to women who had children before the start of the program. These effects are summarized in table I.5, and are statistically different from zero. Again, there is no evidence that differential pre-existing trends may bias this analysis or that effects preceded the program, because estimates of θ display a flat trend in women's characteristics for five years before the start of the program.

I.6 Testing for Mechanisms in Responses to Maternity Benefits

The increase in childbearing after the expansion of maternity benefits may be due to three channels. First, if the opportunity cost channel predominates then women with lower opportunity costs of having a child may increase childbearing by more, because the benefit is a higher fraction of their salary, and they have a more flexible work schedule to accommodate another child. Second, if the child cost channel predominates then women for whom the cost of raising a child is lower, for instance lower cost of childcare, may increase childbearing by more. Third, if the housing size channel predominates then women who do not have housing size constraints may increase childbearing by more.

[26] Time relative to treatment is 0 if birth year is 1981 for early beneficiaries and if birth year is 1982 for late beneficiaries. Also, the unit of observation in these regressions is at the birth-mother level: I treat each birth of the mother as a separate observation. The standard errors are clustered at the oblast level.

Women in rural areas may have increased childbearing by more because of the opportunity cost and housing size channels, while less educated women may have increased childbearing by more because of the opportunity cost channel. Women in rural areas were mostly employed as manual laborers and were heavily underrepresented in the prestigious occupations of machine operators which required special training and skill (Bridger 1987). As a result, women's opportunity costs in rural areas were low due to low wages, and the seasonal nature of their work, which gave them greater flexibility in caring for a child compared to women in urban areas. Moreover, women in rural areas had larger housing which allowed them more room to have another child, whereas women in urban areas were more space constrained. Less educated women had lower opportunity costs due to lower wages and due to less of a penalty on wages from taking longer leave because of slower skill depreciation.

To establish the importance of these different channels, I examine whether effects of maternity benefits differed across oblasts. In particular, I analyze whether fertility rates increased by more in more rural compared to more urban areas or less educated compared to more educated areas. For this purpose, I obtain characteristics of regions using the 1979 Russian census and follow the approach of Finkelstein (2007):

$$GFR_{o,y}=\alpha+\gamma_{\mathrm{y}}+\sum_{\mathrm{t=-6}}^{-1}\theta_{\mathrm{t}}*Z_o*1(\mathrm{y\text{-}y^*=t})+\sum_{\mathrm{t=1}}^{10}\pi_{\mathrm{t}}*Z_o*1(\mathrm{y\text{-}y^*=t})+\delta_{\mathrm{o}}+\varepsilon_{o,y} \qquad (\mathrm{I.5})$$

where Z_{o} represents continuous variables at the oblast level measured in 1979 to be included in separate regressions: share of women age 15 to 44 in an oblast who were living in a rural area, and share of individuals age 10 and older who have not completed high school. All other covariates remain the same as in specification (I.4). This specification is the same as specification (I.4), except now the event-year dummies, $1(y - y^* = t)$, are interacted with Z_o.

As before, this empirical strategy tests for a break in any pre-existing differences in the level or trend of fertility rates across regions around the time of the start of the program that is correlated with Z_o. The identifying assumption is that without benefits the differences in fertility rates before the start of the program would have continued on the same trend. Thus, this strategy does not assume that areas that differ in their composition of residents had the same level or growth of fertility rates before the program.

The results show that increased maternity benefits are associated with a greater increase in fertility rates among more rural and less educated areas. Figure I.8 plots estimates of θ and π from specification (I.5) that correspond to interactions of the event-year dummies with independent variables measuring a region's rural (panel A) and education status (panel B) in separate regressions. The time pattern of π (estimates to the right of the vertical axis) presents changes in fertility rates after the start of the program in areas with larger expected increases in childbearing relative to areas with smaller expected increases. The dashed lines indicate a 95-percent confidence interval for each coefficient.

Both panels show a notable level-shift in coefficients after the start of the program in a region. The coefficients are individually statistically indistinguishable from zero in the years leading up to the start of the program. This indicates that before the start of the program fertility rates evolved similarly in areas with different shares of urban women and educated individuals. However, after the start of the program the coefficients, π, jump discontinuously in both panels, and are summarized in table I.6. This indicates a larger increase in fertility rates in more rural compared to more urban areas, and a larger increase in fertility rates in less educated compared to more educated areas. After the initial jump, the GFR in more rural or less educated areas continued increasing by more for the first eight years after the start of the program. However, the difference across regions disappeared ten years after the start of the program, which is consistent with benefits losing their value due to inflation and the end of the program.

The fact that fertility rates increased by more in more rural and less educated regions indicates that the increase in fertility rates was largely due to the opportunity cost channel. This provides suggestive evidence that the magnitudes of effects of maternity benefits depend on the opportunity costs of women's work. Areas where women earn less have more flexible schedules and have flatter age-earnings profiles may experience larger increases in fertility rates due to maternity benefits.

I.7 Conclusion and Discussion

Low fertility rates are an important concern for many OECD countries, who have implemented various family friendly policies. I find an immediate response in fertility rates after the introduction of paid parental leave and cash transfers in Russia. Moreover, I find that the effects on fertility rates persist in the long run. These results indicate that maternity benefits affected both the timing and the number of children women had. This study provides new evidence on the topic of whether family policy can raise childbearing.

These results imply that childbearing is elastic with respect to the cost of a child. A back-of-the envelope calculation quantifies this price elasticity. I use estimates from

Russian demographers to calculate the cost of a child as the sum of the costs over the first 18 years of a child's life (Valentei 1987). In Russia, maternity benefits decreased the cost of a child by 2.2 percent.[27] According to estimates in table I.2, this is associated with an 8.2 percent increase in fertility rates (6.2/76) during the year after the start of the program, which means that the short-run price elasticity of childbearing equals to -3.7 (8.2/2.2).

How does the effect of maternity benefits on childbearing in Russia compare to effects of family benefits in other countries? To address this question, I compare the estimates of short-run elasticities from other studies, which figure I.9 shows range from -4.4 to 0.54. The estimate of the elasticity in this study is in this range. I also construct corresponding confidence intervals for these elasticities using a parametric bootstrap method (Johnston and DiNardo 1997).[28] Figure I.9 shows that in Austria the elasticity was equal to -4.4, in Spain it equaled -3.8, in Canada it was -4.1, while in Israel it was -0.54. The confidence intervals of all but the study on Israel overlap, which indicates that the estimate in the Russian context is in the range of other countries.[29]

This study is the first to find a positive long-run effect of maternity benefits on childbearing. My estimates imply that the program induced about 5 million births during

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[27] The details of this calculation are in appendix E.

[28] The procedure for calculating elasticities and their corresponding confidence intervals in other studies is described in appendix E.

[29] This comparability may be less surprising because Russia in 1980 is comparable in many respects to OECD countries today in terms of key demographic and economic variables. Table I.7 shows the characteristics of Russia in 1980 compared to characteristics of Austria, Spain, Italy, the United States, and Sweden in 2010-2013. Russia had a below replacement fertility rate, a high women's labor force participation rate where women earned less than men, and high rates of preschool enrollment. Also, the inequality in Russia is similar to European countries today, but lower than in the US.

its ten-year duration at a cost of roughly 2,830 rubles per birth induced.[30] In terms of what that meant at the time, the cost represented 1.4 times the average national yearly salary in 1980. How applicable is this finding in the context of other countries?

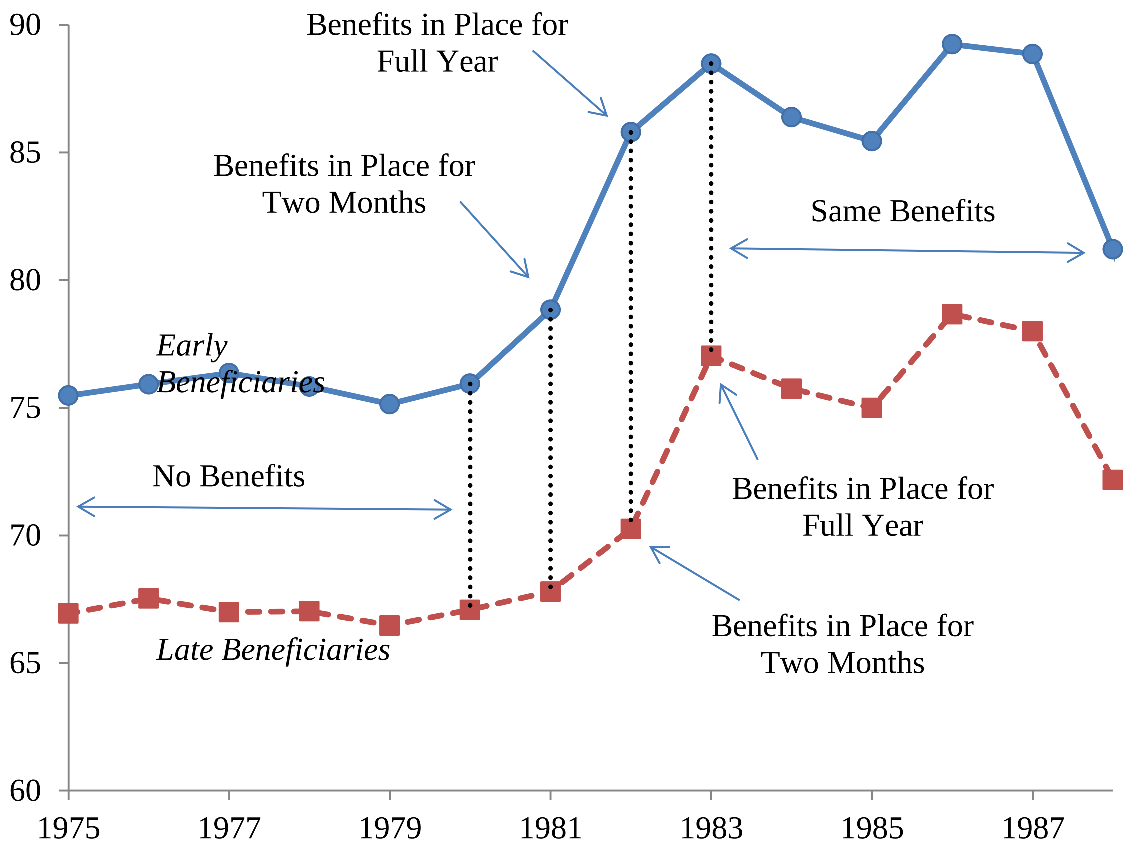
The long-run effect of maternity benefits may vary across countries because it may depend on the cost of a child or the opportunity cost of the woman's time. Maternity benefits may have had a long-run effect on childbearing in Russia, because women had lower opportunity costs and lower costs of raising children. In Russia, women earned substantially less than men, the age-wage profile was flatter than in other countries, and childcare was widespread and heavily subsidized. What are the policy implications? The results of this study suggest that maternity benefits may be effective in their goal of increasing long-run childbearing. Whether maternity benefits can increase long-run childbearing in other countries depends on opportunity costs of women, and the interaction of maternity benefits with other social or public programs.

[30] I obtain estimates of the number of induced births by multiplying the population of women age 15 to 44 in Russia in 1980 (32,696,400) by the event-study estimates from specification (I.4) and summing over event-years 1 to 11. I estimate that the program cost was about 14 billion rubles, where I assume that women took the entire leave (thus giving an upper bound of the cost). I calculate the cost from parental leave outlays by multiplying the fixed payments by 10 months (maximum time of leave) for each birth; I calculate the cost from the cash transfers by multiplying the fixed transfer by the number of births; I calculate the additional cost in terms of extra maternity benefits for births that were induced by multiplying the average national monthly salary by 4 months for the estimated induced 5 million births.

Figure I.1 Map of Benefit Roll-Out Across Russia

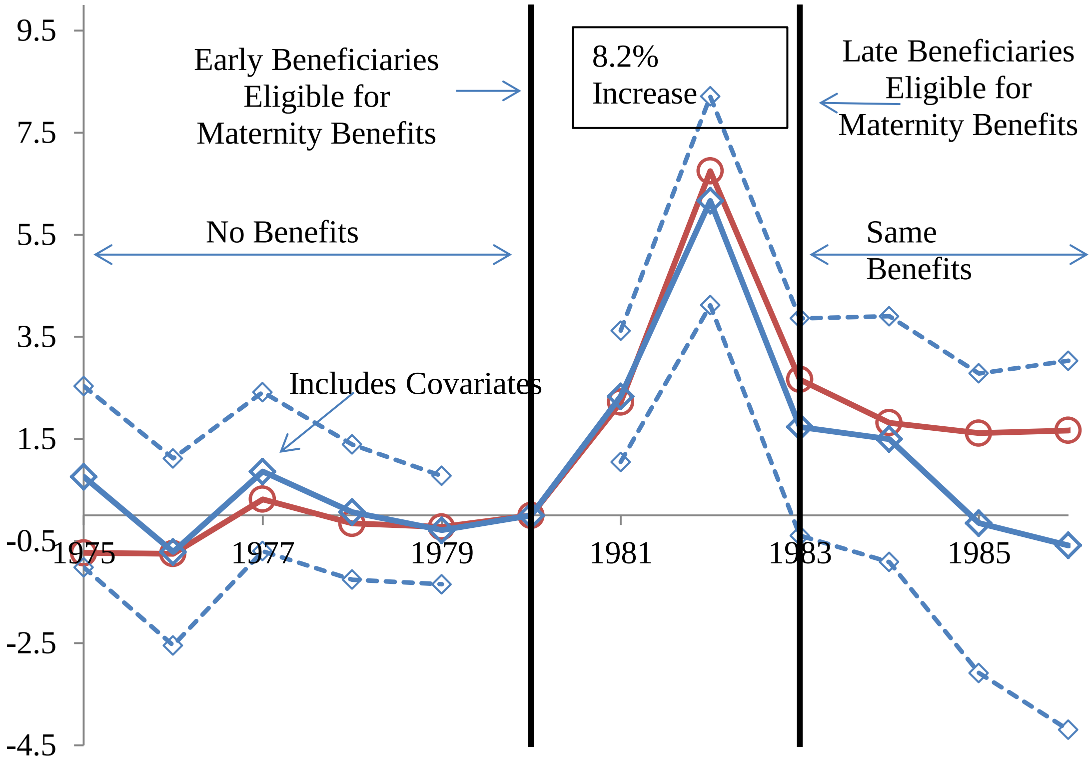
Notes: The early beneficiaries (Northern, Siberia and Far East regions) where maternity benefits started in November, 1981 are shaded. The late beneficiaries (rest of Russia) where maternity benefits started in November, 1982 are white.

Figure I.2 Descriptive Evidence of the Effect of Maternity Benefits on Fertility Rates



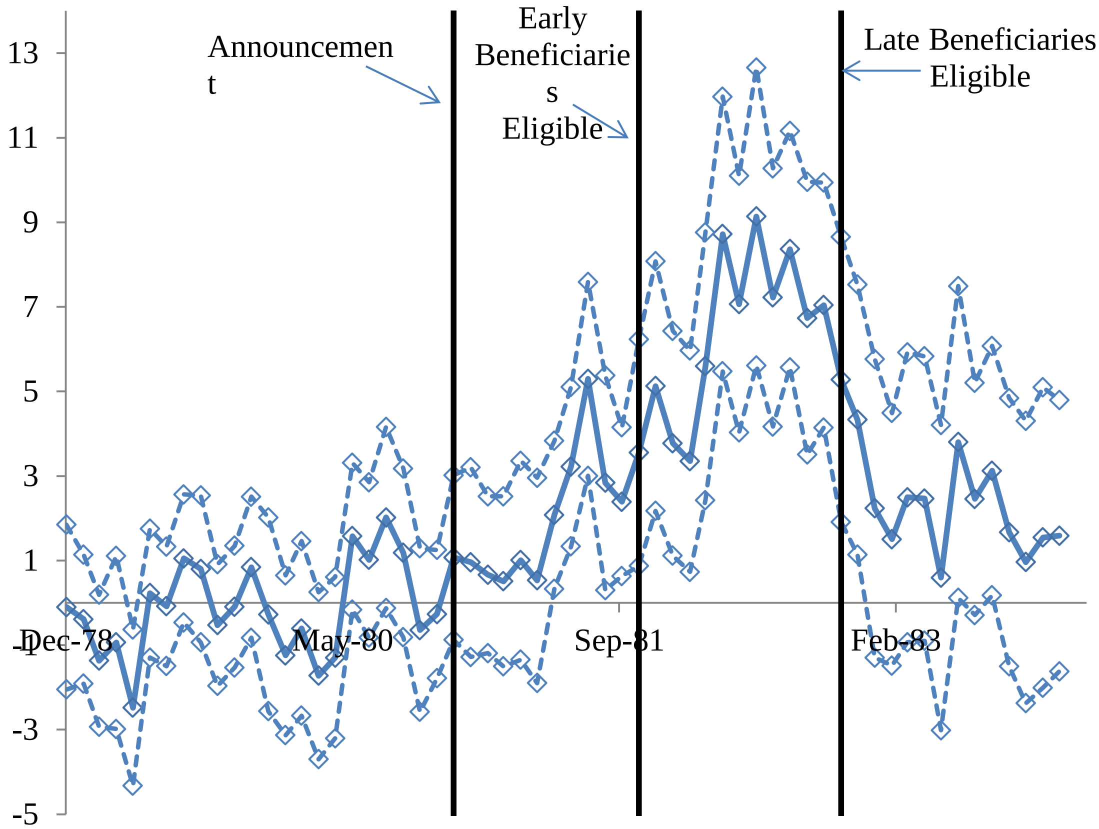
Notes: The figure plots the evolution of general fertility rates (GFR) in the early and late beneficiary regions. The GFR is the number of births per thousand women ages 15 to 44. Sources: Russian Federal State Statistics Service (Rosstat) and the 1989 Russian Census.

Figure I.3 The Short-Run Effect of Maternity Benefits on Fertility Rates



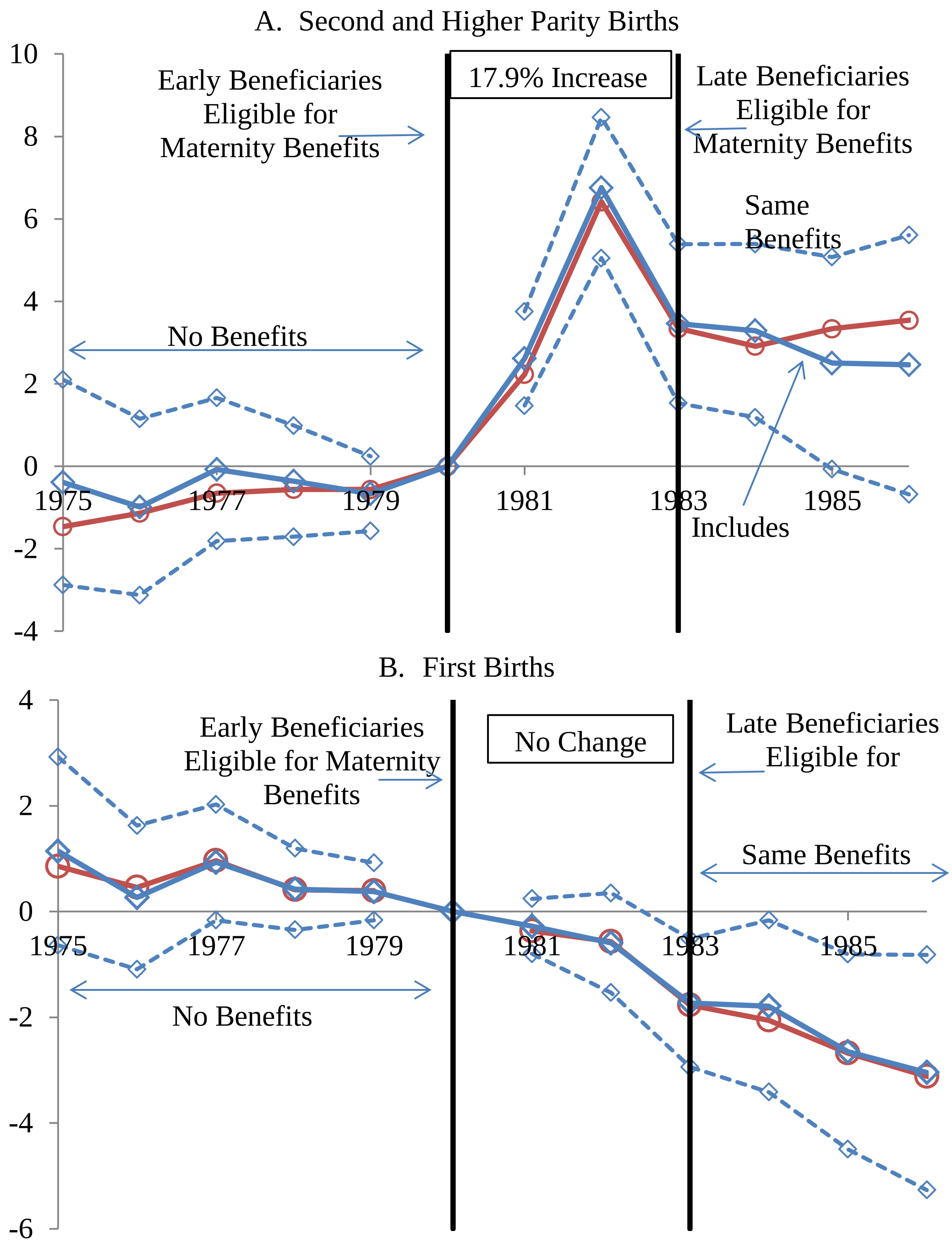
Notes: These coefficients represent the difference in GFR (general fertility rates) between the early and late beneficiary regions in each year relative to the difference in 1980. I present θ and π from equation (I.1) using GFR as a dependent variable. The coefficient on year 1981 presents the effect of the program when maternity benefits were in place in early beneficiary regions for two months. The coefficient on year 1982 presents the effect of the program when maternity benefits were in place in early beneficiary regions for the full year. For the coefficients shown as circles the model includes year and oblast fixed effects; for the coefficients shown as diamonds the model also adds oblast by year covariates. Heteroskedasticity-robust standard errors clustered by oblast construct 95-percent, point-wise confidence intervals (dashed lines) for the coefficients that include oblast by year covariates. Regressions are weighted by the number of women who are ages 15 to 44 living in an oblast in 1980. See appendix B for details on the fertility rate construction, and appendix C on data descriptions. Sources: Rosstat, 1989 Russian Census, 1975-1986 "Narodnoe Hozyaystvo" yearbooks.

Figure I.4 The Short-Run Effect of Maternity benefits on Monthly Fertility Rates



Notes: These coefficients represent the monthly difference in fertility rates between early and late beneficiaries from January 1978 to December 1983 relative to the difference in 1977, which is 0 by construction. I present θ and π from equation (I.3). I use monthly fertility rates constructed using 2002 Census data and adjusted for mortality and mobility using equation (I.2). Monthly fertility rates represent number of births in a month per 1,000 women ages 15 to 44 in a year, and they have been multiplied by 12 to match the scale of the general fertility rate estimates in figure I.3. Details on the fertility rate construction are in appendix B. The model includes year, oblast, month by oblast fixed effects, and annual covariates for each oblast. Weights are the number of women who are ages 15 to 44 living in an oblast in 1980. Heteroskedasticity-robust standard errors clustered by oblast construct 95-percent point-wise confidence intervals (dashed lines). Sources: Rosstat, 1989 and 2002 Russian Censuses, and 1975-1986 “Narodnoe Hozyaystvo” yearbooks.

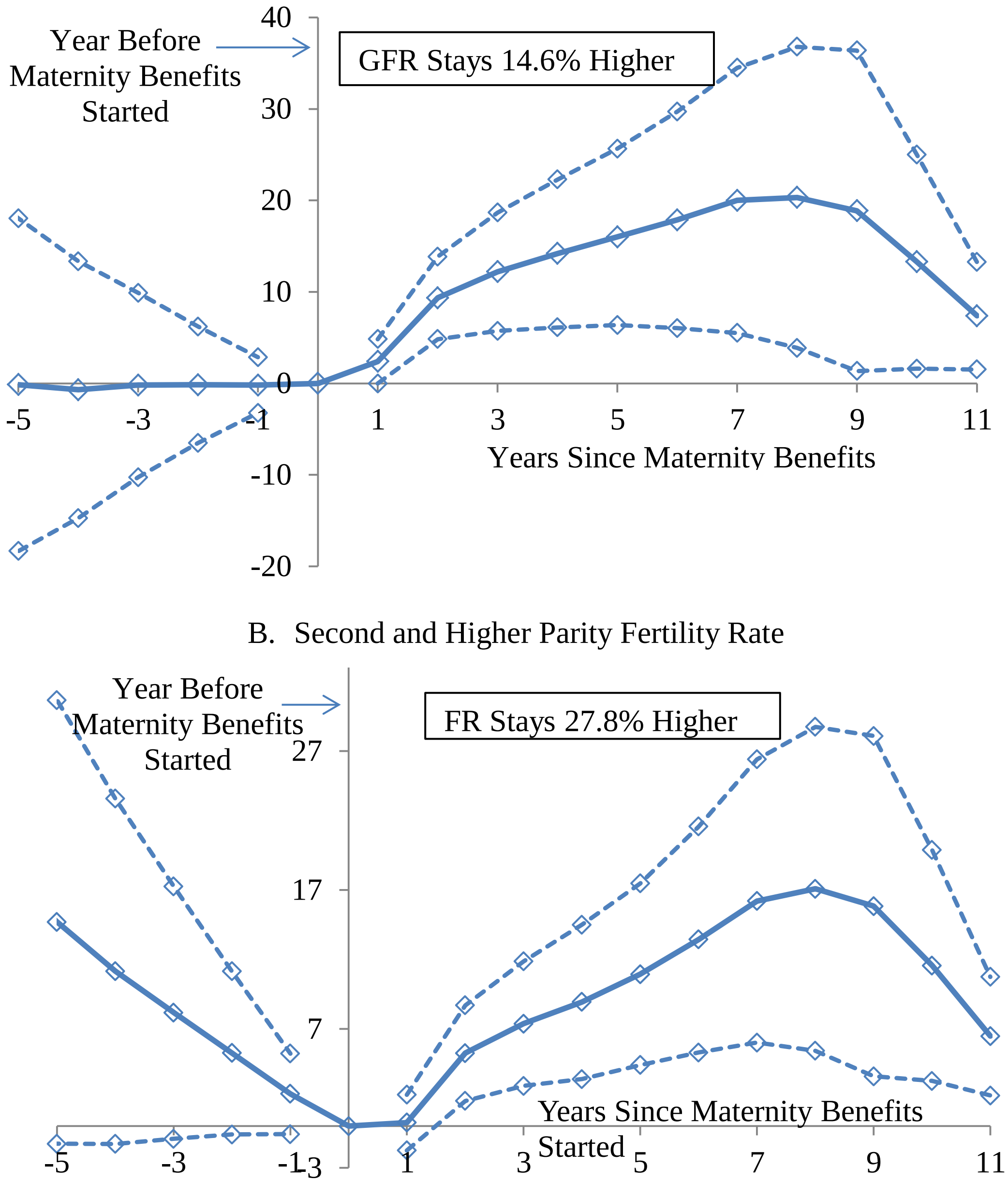
Figure I.5 The Short-Run Effect of Maternity Benefits on Fertility Rates by Parity



Notes: See notes for figure I.3. The unit of observation is at the year-month-oblast level, and regressions include month by oblast fixed effects. Sources: Rosstat, 1989 and 2010 Russian Censuses, and 1975-1986 "Narodnoe Hozyaystvo" yearbooks.

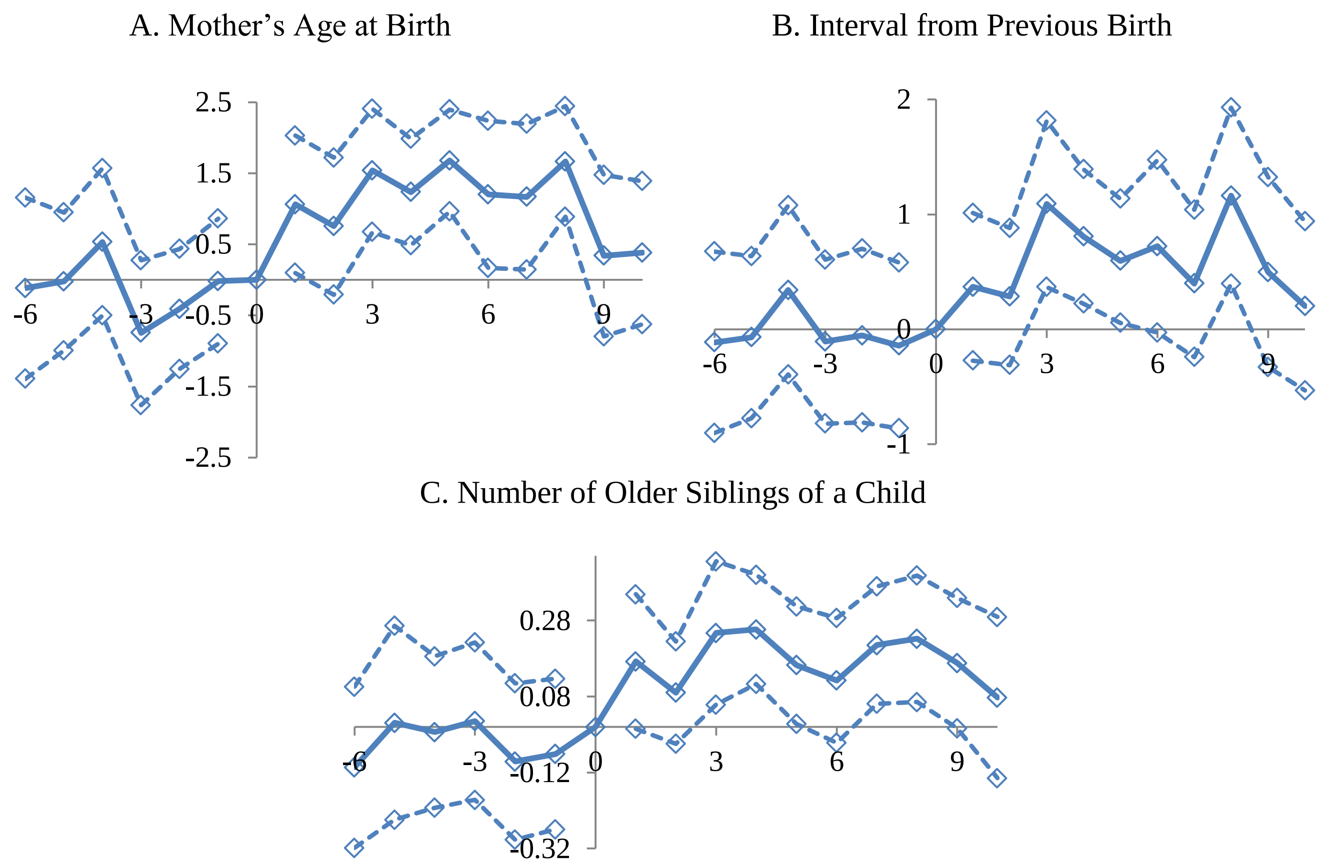
Figure I.6 The Long-Run Effect of Maternity Benefits on Fertility Rates

A. General Fertility Rate



Notes: These coefficients show the evolution of fertility rates conditional on covariates 5 years before and 11 years after maternity benefits started. I present θ and π from equation (I.4) using GFR as a dependent variable in panel A, and using second and higher parity fertility rate as a dependent variable in panel B. Years since maternity benefits started equal to zero if birth year equals 1980 in early beneficiary oblasts, and equal to zero if birth year equals 1981 for late beneficiary oblasts. Regressions are weighted by the number of women who are ages 15 to 44 living in an oblast in 1980. Heteroskedasticity-robust standard errors clustered by oblast construct 95-percent, point-wise confidence intervals (dashed lines). Sources: See sources for figure I.5.

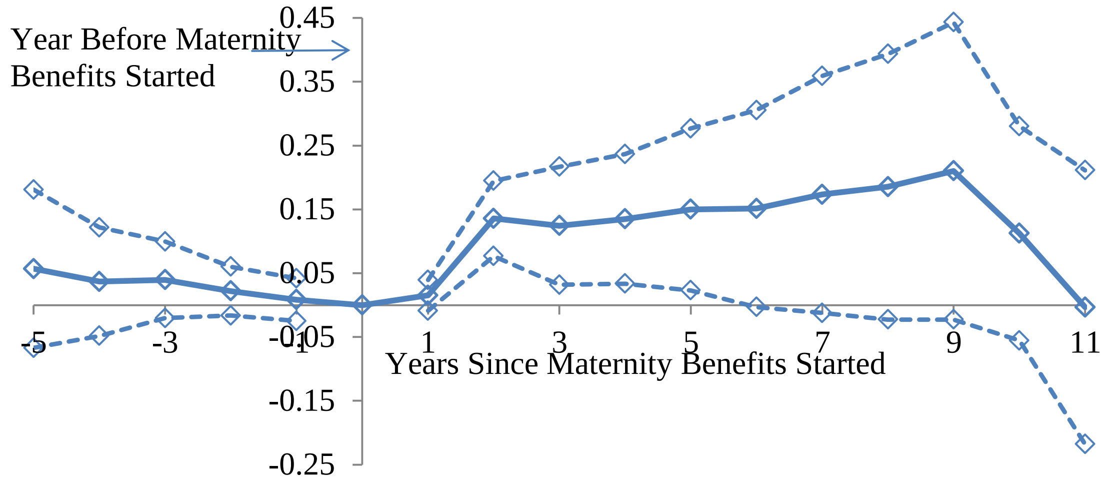
Figure I.7 The Effect of Maternity Benefits on the Demographic Composition of Mothers



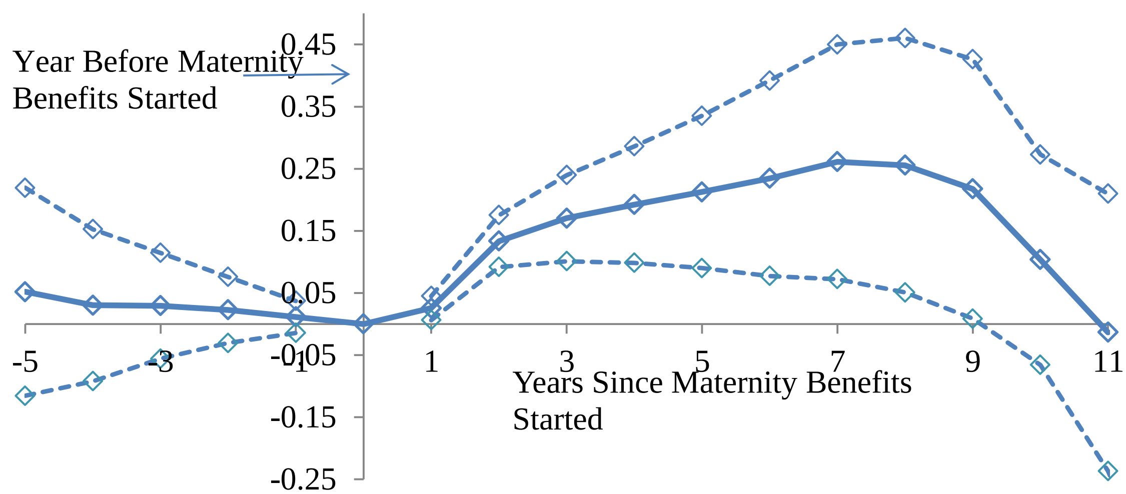
Notes: These coefficients show the evolution of the composition of mothers conditional on covariates before and after maternity benefits started. I present θ and π from equation (I.4) using mother’s age at birth, interval from previous birth and number of older siblings of a child as dependent variables. The unit of analysis is a mother-birth observation. The x-axis presents years since benefits started. Years since benefits started equal to zero if birth year equals 1981 for early beneficiaries, and equal to zero if birth year equals 1982 for late beneficiaries. The coefficient on years since treatment=0 is normalized to zero. Results are weighted using survey sampling weights. Heteroskedasticity-robust standard errors clustered by oblast construct 95-percent, point-wise confidence intervals (dashed lines). Source: Generations and Gender Survey, 2004.

Figure I.8 Heterogeneous Responses of Fertility Rates to Maternity Benefits Across Regions

A. Share of Women Age 15 to 44 Living in Rural Areas

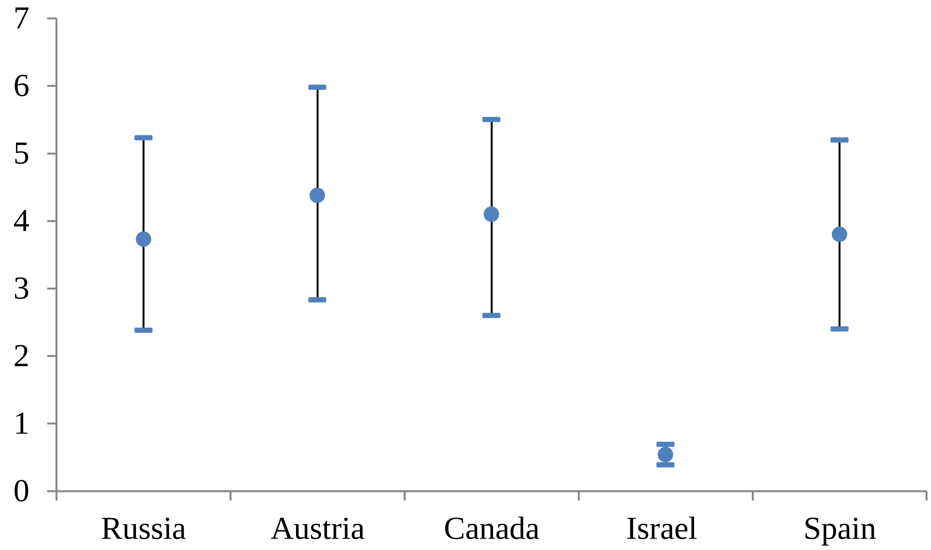


B. Share of Individuals Older than 10 with Less than High School Education



Notes: The coefficients show the pattern over time in GFR in regions with more rural compared to regions with more urban resident women (panel A), and in regions with less educated compared to regions with more educated individuals (panel B). I present θ and π from equation (I.5) using GFR as a dependent variable. These coefficients present interactions of event-year dummies with a continuous oblast level characteristic measured in 1979 (share of women age 15 to 44 in rural areas {panel A}; share of individuals with less than high school education {panel B}). Years since maternity benefits started equal 0 if birth year equals 1980 in early beneficiaries, and if birth year equals 1981 for late beneficiaries. Regressions are weighted by the number of women who are ages 15 to 44 living in an oblast in 1980. Heteroskedasticity-robust standard errors clustered by oblast construct 95-percent, point-wise confidence intervals (dashed lines). Sources: 1979 and 1989 censuses, Rosstat, 1975-1992 "Narodnoe Hozyaystvo".

Figure I.9 Comparison of Short-Run Childbearing Elasticities across Countries



Notes: This plot presents estimates of short-run elasticities of childbearing with respect to the cost of a child until the child is 18 years old in Russia (calculated in this paper) and in other countries based on estimates from other studies. The plot shows the estimate of the elasticity (circle) which is surrounded by its 95-percent confidence interval. The figure presents estimates for the following countries: Austria (Lalive and Zweimüller 2009), Canada (Milligan 2005), Israel (Cohen et al. 2013), and Spain (Gonzalez 2012). Details of elasticity and confidence interval constructions are in appendix E.

Table I.1 Characteristics of Early and Late Beneficiary Oblasts

	Early Beneficiaries	Late Beneficiaries
Proportion of Population in 1979		
with college degree among employed	9.0	9.1
with less than high school among employed	46.2	48.5
employed among women ages 15 to 54	84.1	85.3
in rural areas among women ages 15 to 44	25.7	24.9
in preschool among children younger than 7	49.0	50.2
65 years and older among women	9.6	14.7
Number of Oblasts	37	51
Share of Total Population Living in Oblasts	25.5	74.5

Notes: These statistics are based on the entire population of Russia. Sources: 1979 Census, 1979 "Narodnoe Hozyaystvo" Yearbook.

Table I.2 The Short-Run Effect of Maternity Benefits on Fertility Rates

	(1)	(2)	(3)
Dependent Variable	General Fertility Rate		
	Before Program Mean in 1980: 75.95		
Before Program			
(Years 1975 to 1979)*Early Beneficiary	-0.309	-0.309	0.142
	[0.747]	[0.779]	[0.632]
After Early Beneficiaries Eligible			
(Year 1981)*Early Beneficiary	2.218	2.218	2.354
	[0.618]	[0.645]	[0.634]
(Year 1982)*Early Beneficiary	6.748	6.748	6.192
	[1.291]	[1.347]	[1.031]
After Everyone Eligible			
(Years 1983 to 1986)*Early Beneficiary	1.940	1.940	0.502
	[2.139]	[2.233]	[1.292]
R-squared	0.152	0.945	0.962
Covariates	Year FE	Year FE, Oblast FE	Year FE, Oblast FE, $X_{o,y}$
Oblast-year cells	984	984	984
Oblasts	82	82	82

Notes: These coefficients represent the difference in GFR between the early beneficiaries compared to the late beneficiaries in grouped years relative to the difference in 1980. Standard errors clustered at the oblast-level are in brackets. These results are comparable to the coefficients in figure I.3. These coefficients represent interactions of the grouped year dummies and the dummy for the early beneficiary regions using the regression model described in equation (I.1). The grouped years represent: the period before the start of the program (years 1975 to 1979), the year when benefits were in place for only two months (1981) in early beneficiaries, the year when the benefits were in place for the whole year (1982) in early beneficiaries, and the period after all regions became eligible for benefits (years 1983 to 1986). I omit the year before the start of the program (1980), so estimates for that year are normalized to zero. The unit of observation is oblast by year. Coefficients in column 1 include year fixed effects, coefficients in column 2 add oblast fixed effects, while coefficients in column 3 add annual oblast-level covariates where the covariates are the same as included in equation (I.1). Regressions are weighted by the number of women who are ages 15 to 44 living in an oblast in 1980. Sources: See notes for figure I.3.

Table I.3 The Short-Run Effect of Maternity Benefits on Fertility Rates by Parity

	(1)	(2)	(3)
Dependent Variable	A. Second and Higher Order Birth		
	Before Program Mean in 1980: 37.44		
Before Program			
(Years 1975 to 1979)*Early Beneficiary	-0.932	-0.932	-0.385
	[0.654]	[0.683]	[0.766]
After Early Beneficiaries Eligible			
(Year 1981)*Early Beneficiary	2.589	2.589	2.616
	[0.579]	[0.604]	[0.570]
(Year 1982)*Early Beneficiary	7.325	7.325	6.717
	[1.137]	[1.187]	[0.865]
After Everyone Eligible			
(Years 1983 to 1986)*Early Beneficiary	4.371	4.371	2.856
	[1.885]	[1.968]	[1.164]
R-squared	0.212	0.914	0.932
Dependent Variable	B. First Birth		
	Before Program Mean in 1980: 38.50		
Before Program (Years 1975 to 1979)*Early Beneficiary	0.617	0.617	0.523
	[0.449]	[0.469]	[0.509]
After Early Beneficiaries Eligible			
(Year 1981)*Early Beneficiary	-0.367	-0.367	-0.259
	[0.256]	[0.267]	[0.260]
(Year 1982)*Early Beneficiary	-0.567	-0.567	-0.524
	[0.429]	[0.448]	[0.465]
After Everyone Eligible			
(Years 1983 to 1986)*Early Beneficiary	-2.403	-2.403	-2.350
	[0.864]	[0.902]	[0.830]
R-squared	0.862	0.951	0.954
Covariates	Year FE, Month FE	Year FE, Month-Oblast FE, Oblast FE	Year FE, Month-Oblast FE, Oblast FE, $X_{o,y}$
Oblast-year-month cells	11,808	11,808	11,808
Oblasts	82	82	82

Notes: See notes for table I.2. The unit of observation is at the year-month-oblast level.These results are comparable to the coefficients displayed in figure I.5. Sources: See sources for figure I.5.

Table I.4 The Long-Run Effect of Maternity Benefits on Fertility Rates

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	General Fertility Rate			Second and Higher Order Fertility Rate		
	Before Program Mean: 69.98			Before Program Mean: 34.60		
Before Program						
(Event Years -5 to -1)	-0.0137	-0.0137	-0.0610	1.434	1.434	1.359
	[1.020]	[1.050]	[1.018]	[1.000]	[1.029]	[0.985]
After Program						
(Event Years 1 to 3)	8.994	8.994	9.063	7.047	7.047	7.004
	[1.422]	[1.464]	[1.497]	[0.979]	[1.008]	[0.999]
(Event Years 4 to 6)	10.62	10.62	10.71	9.933	9.933	9.985
	[2.178]	[2.242]	[2.277]	[1.590]	[1.637]	[1.647]
(Event Years 7 to 10)	10.77	10.77	10.84	11.71	11.71	11.82
	[3.345]	[3.444]	[3.463]	[2.589]	[2.664]	[2.636]
R-squared	0.279	0.914	0.914	0.286	0.885	0.887
Covariates	Year FE	Year FE, Oblast FE	Year FE, Oblast FE, $X_{o,y}$	Year FE, Month FE	Year FE, Month-Oblast FE, Oblast FE	Year FE, Month-Oblast FE, Oblast FE, $X_{o,y}$
Observations	1,444	1,444	1,444	17,328	17,328	17,328
Oblasts	82	82	82	82	82	82

Notes: The coefficients represent dummies of grouped years to maternity benefits (event years) from the regression equation (I.4) where GFR (columns 1 to 3) and higher order fertility rates (columns 4 to 6) are dependent variables. Standard errors clustered at the oblast-level are in brackets. These estimates are directly comparable to coefficients presented in figure I.6. The grouped years represent: the years before maternity benefits started (from 5 to 1 years before the program), 1 to 3, 4 to 6, and 7 to 10 years after the program started. The year before maternity benefits started is omitted, so estimates are normalized to zero for that event year. The unit of observation is oblast by year for columns 1 to 3, while the unit of observation is oblast by year by month for columns 4 to 6. Regressions are weighted by the number of women who are ages 15 to 44 living in an oblast in 1980. Sources: see sources for figure I.6.

Table I.5 The Long-Run Effect of Maternity Benefits on the Composition of Mothers

	(1)	(2)	(3)
Dependent Variable	**Mother's Age at Birth**	**Number of Older Siblings of Child**	**Interval From Previous Birth**
Before Program Mean	24.85	0.782	2.723
After Program			
(Event Years 1 to 3)	1.585	0.231	0.899
	[0.322]	[0.0615]	[0.207]
(Event Years 4 to 6)	2.089	0.219	1.191
	[0.432]	[0.101]	[0.272]
(Event Years 7 to 10)	1.799	0.23	1.368
	[0.578]	[0.146]	[0.438]
R-squared	0.041	0.04	0.036
Oblasts	69	69	69
Observations (individuals)	4,457	4,457	3,327

Notes: This table shows the change in average age at birth, number of children before birth and interval from previous birth for women who had children 1 to 3, 4 to 6, and 7 to 10 years after the start of the program. Standard errors clustered at the oblast level are in brackets. These coefficients are directly comparable to the coefficients presented in figure I.7. The unit of observation is at the woman-birth level. The grouped event years represent: dummies for 1 to 3, 4 to 6, and 7 to 10 years after maternity benefits started. The omitted category is 7 to 0 years before the program started and is normalized to zero. Regressions are weighted by the survey sampling weights. Source: Generations and Gender Survey, 2004.

Table I.6 Heterogeneous Fertility Rate Increases to Maternity Benefits across Regions

	(1)	(2)	(3)
Dependent variable		A. General Fertility Rate	
Before Program			
(Event Years -5 to -1) ∗ % Rural	0.026	0.026	0.023
	[0.0203]	[0.0207]	[0.0250]
After Program			
(Event Years 1 to 3) ∗ % Rural	0.116	0.116	0.127
	[0.0406]	[0.0413]	[0.0424]
(Event Years 4 to 6) ∗ % Rural	0.129	0.129	0.148
	[0.0673]	[0.0687]	[0.0713]
(Event Years 7 to 10) ∗ % Rural	0.140	0.140	0.157
	[0.0899]	[0.0921]	[0.0885]
R-squared	0.498	0.913	0.914
Dependent variable		B. General Fertility Rate	
Before Program			
(Event Years -5 to -1) ∗ % Less than HS	0.006	0.007	0.007
	[0.0150]	[0.0159]	[0.0158]
After Program			
(Event Years 1 to 3) ∗ % Less than HS	0.150	0.149	0.149
	[0.0298]	[0.0287]	[0.0295]
(Event Years 4 to 6) ∗ % Less than HS	0.183	0.181	0.181
	[0.0515]	[0.0498]	[0.0507]
(Event Years 7 to 10) ∗ % Less than HS	0.183	0.182	0.179
	[0.0664]	[0.0640]	[0.0646]
R-squared	0.285	0.915	0.915
Covariates	Year FE	Year FE, Oblast FE	Year FE, Oblast FE, $X_{o,y}$
Oblast-year cells	1,444	1,444	1,444
Oblasts	82	82	82

Notes: These coefficients present interactions of grouped event-year dummies with a continuous oblast level characteristic measured in 1979 (share of women age 15 to 44 living in rural areas {panel A}; share of individuals with less than high school education {panel B}). See the description of event-year dummies in table I.4. Standard errors clustered at the oblast level are in brackets. These coefficients are directly comparable to the coefficients presented in figure I.7. Weights are the number of women who are ages 15 to 44 living in an oblast in 1980. Sources: see sources in figure I.7.

Table I.7 Similarity of Russia in 1980 to Other Countries Today

	1980	2010-2013				
	Russia	**USA**	**Austria**	**Italy**	**Spain**	**Sweden**
TFR	1.9	1.9	1.4	1.4	1.3	1.9
Childless Women	9.5	14	21	24	13	13
In Formal Care (age <3)	35	43.2	13.9	24.2	39.3	46.7
In Formal Care (age 3-6)	65	66.5	75	90	91	92
Female LFP	85	67	70	54.4	70	79
Female/Male Wage Ratio	67	82.2	80.8	72.6	93.9	85.7
Gini	0.29	0.39	0.28	0.32	0.34	0.27

Sources: 1979 Russian Census, 1979 “Narodnoe Hozyaystvo” Yearbook, OECD Statistics.

Chapter II

Maternity Benefits and Children's Adult Outcomes

II.1 Introduction

Countries often implement maternity benefit programs out of concern for future economic growth due to low rates of childbearing. Another goal is to ease a mother's transition into childbearing with extra time or money available for the newborn, which could have a positive effect on children's outcomes.[31] However, such programs may result in changes in the composition of mothers having children. If mothers who give birth after such programs are implemented are more economically disadvantaged (for instance, they are less educated and have lower incomes) compared to mothers who give birth before the program, then their children may also be more economically disadvantaged. Thus, even if such programs are successful at increasing childbearing, these extra births may be costly to the government in the longer-term. The government may have to spend more on public assistance and medical outlays, because maternity benefits may result in less skilled and less healthy cohorts, which could potentially lower economic growth. As a result, it is important to consider the net effect of maternity benefits on children, which includes both the direct effect through more parental time or

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[31] The first year of a child's life is critical for future cognitive and social development (Lewis and Brooks-Gunn, 1979).

money spent on children, and the indirect effect through changes in the composition of mothers and increases in cohort and household size.

The literature evaluating the effects of maternity benefit programs has mainly focused on their direct effect on children at young ages and later in adulthood. The economics literature provides mixed evidence that parental leave programs had no direct effect (Dustmann et al. 2012, Dahl et al. 2013) or had a positive effect (Rossin 2011, Carneiro 2011) on children's outcomes. The literature evaluating the effects of legalizing abortion on children's outcomes focuses on changes in the composition of mothers in explaining changes in children's outcomes before and after abortion legalization. Gruber et al. (1999) find that children born after abortion legalization were less likely to die as infants, less likely to live in a single parent family, less likely to live in families receiving welfare and less likely to live in poverty. Further in adulthood, these children are more likely to complete college, less likely to be on welfare, and less likely to be single parents (Ananat et. al 2006).

This paper analyzes the effect of maternity benefits expansion in Russia on children's outcomes in adulthood. It exploits the staggered introduction of the benefits, where benefits started in 32 oblasts in 1981 (similar to states; I call these oblasts "early beneficiaries"), and then in 50 oblasts ("late beneficiaries") one year later. I use Russian census data from 2010 which confers great precision to examine a rich set of outcomes for individuals in adulthood, and its inclusion of oblast of birth allows me to evaluate the net effect of the expansion of maternity benefits. I use a difference in difference framework, where I compare the outcomes of children born after the start of the program in early and late beneficiary oblasts to the outcomes of children born before the program.

This paper's results indicate that the Russian maternity benefit program resulted in lower educated cohorts, but did not have an effect on other economic and family structure outcomes. Children born in the first full-year of program implementation were 1.5 percent less likely to have completed college compared to children born before the program. Children born in the first full year of implementation did not have statistically different economic outcomes such as their employment status, or receipt of public assistance compared to children born before the program. Finally, children born in the first full year of implementation did not have statistically different family outcomes such as marital status, teen-motherhood, or the number of children ever born compared to children born before the program.

How do these results compare with what the abortion literature finds in the United States? While in the United States childbearing fell by 4 to 8 percent in states with legal abortion (Levine et al. 1996), in Russia childbearing increased by 9 percent in the early beneficiaries of maternity benefits. As a result of changes in the composition of mothers due to abortion legalization in the United States, children born after abortion legalization were 2.7 percent less likely to not have completed college, and 14.8 percent less likely to be on welfare compared to children born before abortion legalization. In Russia, children born after the maternity benefits expansion were 0.74 percent more likely to not have completed college, and were not statistically different in economic and family status outcomes compared to children born before the expansion. Thus, the effect in the abortion literature on education is 3.5 times the size of that found in this study, while its effect on the birth rate is smaller than the one after maternity benefits expansion in Russia. The reason for smaller effects in this study could be because maternity benefits also provide cash compensation for each birth (and consequently have the potential of direct effects on outcomes), while such compensation is absent in the case of abortion legalization. The indirect composition channel operates through extra births induced by the program, while the direct parental time and income channels operate on all children whose mothers were eligible for maternity benefits.

Whether maternity benefits have a direct effect on children's outcomes remains an open question. Studies that find no effects of parental leave programs focus on programs that are on a smaller scale than the one studied in this paper.[32] Carneiro et al. (2011) study the introduction of a four month fully-paid leave, which is roughly similar to the expansion in Russia in its replacement rate of income over a twelve month horizon, find that taking an extra four months off from work results in 2.7 percent more children completing high school and 5 percent higher earnings. Black et al. (2012) find that a 10 percent increase in yearly household income leads to positive effects on children's test scores at the bottom end of the income distribution.

This study is able to recover the net effect of maternity benefits on adult outcomes, but is agnostic about the importance of the direct and indirect effects. Given that the maternity benefits program in Russia induced births to less educated and more rural women, it is a concern that children born after the maternity benefits expansion would similarly have poorer economic outcomes. The vast similarity of cohorts born to mothers who were affected by the program compared to those not affected provides

[32] Dustman et al. (2012) study an expansion of paid leave from 2 to 6 months (one third income replacement) and 6 to 10 months which increased time out of the labor force as well as household income while the child was younger than one. Other studies focus on policies that only increased time away from work: Rasmussen (2010) studies an expansion of fully paid leave from 14 to 20 weeks, Dahl et al. (2013) study successive expansions of fully-paid leave that are 2 to 4 weeks in length, Liu and Skans (2010) study an expansion from 12 to 15 months of nearly fully-paid leave.

evidence against extra longer-term costs to the government as a result of this program. The Russian case appears to be a success story – permanent effects on childbearing were not coupled with a costlier labor force in the future.

II.2 Expected Effects of Maternity Benefits on Adult Outcomes

Maternity benefits may affect children's outcomes in adulthood directly and indirectly through several channels. The sign of the effect of the combination of these channels is ambiguous, which motivates the empirical study of the net effect of the expansion of maternity benefits on outcomes in adulthood. Next, I describe five channels that may affect outcomes.

First, the parental time channel may affect children's outcomes directly, if maternity benefits induce mothers to take longer leaves. If spending more time with the mother at early stages of life is instrumental for child development, then it may improve children's long-term educational and economic outcomes. The effect of the mother spending more time with her young child depends on the alternative mode and quality of child care for young children. In Russia, the alternative was federal subsidized day-care or leaving children in the care of grandmothers – children benefit more from spending time with their mothers if other forms of child care are vastly inferior. The literature quantifying the effects of this channel is still small, and includes both positive and null effects.

Second, the household income channel may affect children's outcomes directly, if maternity benefits affect household income. The effect of maternity benefits on household income is ambiguous. If mothers choose to spend so much time with their children that their household income is lowered, then it may negatively affect the outcomes of children. On the other hand, access to paid leave may increase household income during the child's first year of life, if mothers do not change the amount of leave they take. If mothers invest this extra income in their children, it may affect the development and consequently later life outcomes of their children.

Third, the composition channel may affect children's outcomes indirectly, if the composition of women who gave birth after the program was different from the composition of women who gave birth before the program. Chapter 1 shows an increase in childbearing after the start of the maternity benefits program in Russia. This increase was driven by higher-parity births to older mothers. Moreover, the increase in childbearing was greater in areas with larger shares of women living in rural areas and with lower shares of educated individuals. This indicates that the composition of women who had children after the expansion was less educated and more economically disadvantaged.

Fourth, the cohort size channel may affect the outcomes of children indirectly, if maternity benefits lead to an increase in cohort size. Chapter 1 shows a persistent increase in cohort-size after the start of maternity benefits. The increase in cohort-size may have a negative effect on children's educational outcomes which is independent of any changes in the composition of mothers, if larger class sizes are detrimental to the outcomes of children.

Fifth, the household size channel may affect the outcomes of children indirectly, if maternity benefits lead to children to be born in families with more children. Chapter 1 finds evidence that women had more children as a result of the maternity benefit program. Having more siblings may decrease the resources available for each child, which may be detrimental to the outcomes of children.

The net effect of the maternity benefits program consists of the combined effect of all these channels. While the sign of the direct effect of the program is ambiguous, the sign of the composition channel is likely negative. The comparison of the outcomes of children born before and after the program produces estimates which reflect all these channels. The average difference between cohorts born before and after the program allows the analysis of whether the program led to lower educational outcomes, poorer economic outcomes and differences in family outcomes.

II.3 Description of Maternity Benefits Expansion

The maternity benefit program provided three new benefits. First, women received partially paid parental leave until a child turned one, which represented a flat monthly payment equaling roughly 27 percent of the average national female monthly salary. Second, women could keep their job while staying home until their child turned 18 months old. Third, women received a one-time per birth cash transfer which was about 38 percent of the average national female monthly salary for first births, and 76 percent of the average female national monthly salary for second and third births.

The program was implemented in waves around the country. The early beneficiaries of the program were the oblasts (similar to states) in the Far East, Siberia and the Northern regions of Russia. Women in early beneficiary regions started receiving benefits in November 1981. The late beneficiaries of the program consisted of the rest of Russia, and women in these oblasts started receiving benefits in November 1981.

Even though women could start receiving parental leave only after a certain date, they could receive some part of the leave even if their child was born before that date. They could receive paid leave for the remaining months until their child turned one starting from the date their region became eligible for the benefits. Figure II.2 shows the number of months of paid leave a mother was eligible for based on the child's month of birth in the early and late beneficiary regions. Mothers in the early beneficiary regions who gave birth in November, 1980 or earlier did not receive any paid leave until a child turned one. However, mothers who gave birth in December 1980 could receive 1 month of paid leave starting from November 1981. Similarly, mothers in late beneficiary regions who gave birth in November, 1981 or earlier did not receive any paid leave until a child turned one. However, mothers who gave birth in December 1981 could receive 1 month of paid leave starting from November 1982. Differently from the rules for paid leave, mothers could only receive the one-time birth credit if they gave birth after their region became eligible for the benefits.

II.4 Data

To evaluate the effect of the program, I compare the outcomes of children born before and after the maternity benefits program. For this purpose, I use the Russian 2010 census data that includes counts of individuals by different categories.[33] The advantage of the census is that it includes information on the oblast of birth of each child. Oblast of

[33] The census is not at the individual level. Instead, it contains counts of individual with a particular birth year, birth place and with particular outcome categories.

birth, instead of current oblast of residence, is necessary to correctly determine how maternity benefits affected the composition of mothers at the time of birth. The unit of analysis is a cohort according to the child's oblast and year of birth.

These data are also beneficial, because the census contains information on everyone born, and thus is the largest available data set to measure the well-being of children in adulthood. Very large sample sizes are required to identify potential effects of the program, given the identification strategy employed in this paper.

The cost to using the census for this analysis is that for most outcomes, it is impossible to disentangle aging effects and time effects within a given sample. Children born later will also be younger in the 2010 census. The ideal outcomes would be outcomes such as education or marital status by a certain age, and I only have this for the teen motherhood outcomes and educational outcomes (provided that individuals in my sample stop changing their educational attainment after they turn age 25). This is less of a problem in this context, because I am comparing the relative change in outcomes in the early and late beneficiary oblasts. However, this may introduce problems if child age effects have different impacts in different oblasts. Based on graphs, there are trends in the difference in outcomes by birth year from the early and late beneficiaries. This is the reason for inclusion oblast-specific linear time trends in the regression framework, where these controls should capture time-varying differences across states.

My sample consists of all individuals in the 2010 census born between 1976 and 1984 (all individuals from age 25 to age 34). The census tabulations are of the entire country, and contain observations for the whole Russia. The census was taken from

October 14 to 25 in 2010. Data from smaller oblasts may be noisier because the program that produces these data counts adds or subtracts a small number from the counts to protect confidentiality of their subjects.[34] I focus on the following outcomes: college completion, average years of education measure, teen motherhood, marital status, number of children ever born, employment status and receiving public assistance. My sample of observation is at the oblast and year level, because the independent variables of interest only vary at this level. I do not have access to individual-level data, but having counts of individuals in these cells is enough because the independent variables of interest only vary at the oblast and year level. This aggregation results in 656 observations from 82 oblasts.

Due to some data limitations, I dropped the year 1980 from my analysis, because it may influence my results because it is in the year right before treatment. There is a data anomaly in the 2010 census. In round years such as 1960, 1970, 1980, and 1990 there is an extra number of births compared to the trend in previous years. This is also not matched by the trend in the true number of births. This is presented in the first figure in appendix F. Moreover, this is also reflected in the outcome variables I am interested in. For instance, the share of educated individuals is unusually high in these round years, which is shown in the second figure in appendix F. It seems that because there is a pattern to this, then this is a result of consistent mis-reporting by individuals or the census itself.[35] Because fertility rates in other years are well-matched with official data, then it seems it is just something specific to the round years. The exclusion of 1980 does not

[34] The number is independent of the size of the oblast, and thus may have a larger effect on estimates using less populous oblasts.

[35] I have contacted the census about this issue, and they said that it may be because the data is self-reported by individuals, and they may be rounding their birth years. They did not admit to a problem of table creation on their part.

affect the general interpretation of the results. All coefficients are statistically insignificant as they are when I omit 1980 (see tables II.1, II.2 and II.3), while the magnitude on the coefficient on college completion is smaller and no longer significant. I present results where I drop the year 1980 from the analysis.

II.5 Estimation of the Net Effect of Maternity Benefits on Adult Outcomes Using Differences in Differences Methodology

I use a difference in difference methodology where I compare the outcomes of children born in early beneficiaries to those born in late beneficiaries before and after the program. The effects measured in this specification reflect the net effect of the maternity benefits expansion which consists of the combination of the parental time and income, composition, cohort and household size channels. The coefficients in the specification below show the change in children's average living circumstances caused by the maternity benefit program,

$$Y_{oy} = \alpha + \pi_{81} D_o 1(y = 81) + \pi_{82} D_o 1(y = 82) + \pi_{83} D_o 1(83 \leq y \leq 84) + \gamma_y + \delta_o + \delta_o * y + \epsilon_{oy} \quad \text{(II.1)}$$

where Y_{oy} is one of the outcomes for the cohort of children born in oblast *o* and in year *y*; D_o is a dummy for a cohort born in an early beneficiary state; $1(y = 81), 1(y = 82), 1(83 \leq y \leq 84)$ are dummies for the years 1981, 1982 and 1983-1984 respectively, δ_o is a set of oblast fixed effects, γ_y is a set of year fixed effects; $\delta_o y$ are linear time trends for every oblast. The linear time trends control for any differential trends in outcomes by age and oblast, which is important if these trends are not parallel in the early and late beneficiary oblasts.

The coefficient π_{81} captures the effect of maternity benefits on adult outcomes, when all channels were only operating partially. First, based on figure II.2, most mothers who gave birth in 1981 in the early beneficiary regions were not eligible for full benefits yet. Second, as I show in the results, fertility rates did not yet adjust fully, because the program was just announced earlier that year. The coefficient π_{82} captures the effect of maternity benefits on adult outcomes when both the indirect (composition) and indirect (income and parental time) channels are operating fully. By 1982, mothers in early beneficiary regions were eligible for full benefits, and they had enough time to adjust their childbearing in response to the program announcement. It is important to remember that in 1982 both channels are operating partially in the late beneficiary regions as well. First, based on figure II.2, mothers who gave birth in 1982 in the late beneficiary regions were eligible for some of the benefits. Second, fertility rates adjusted in these regions, but not completely yet. Thus, the coefficient π_{82} is an under-estimate of the net effect of maternity benefits on adult-outcomes, because all channels are operating in both the early and late beneficiaries, but they are operating fully in the early beneficiaries and only in part in late beneficiaries.

The coefficients π_{83} will capture the reversion of the mean difference to its pre-program level, after the late beneficiary oblasts gained eligibility. If the effect of the program was the same in the early and late beneficiary regions, then π_{83} should be smaller in magnitude compared to π_{82} when both areas differed in eligibility.

These estimates ignore the fact that the children born before the program may also be affected by the increase in benefits if they have siblings born after the program. However, the composition channel does not affect the outcomes of these children. Thus, the estimate will downplay the influence of all other channels, while the effect of changes in composition of mothers will still remain.

II.6 Measuring the Extent of Composition and Parental Time/Income Channels

To measure the potential role of changes in composition of mothers on results, I analyze whether the program resulted in an increase in births using the 2010 census. Using equation (II.1), table II.1 shows that fertility rates rose in the early beneficiary oblasts. I construct the fertility rate as the number of individuals with birth year, *y*, and birth oblast, *o*, present in the 2010 census divided by the number of women of childbearing age.[36] I find that the program resulted in a 3 and 8 percent increase in childbearing in 1981 and 1982 in early beneficiary oblasts relative to the late beneficiary oblasts. This confirms the findings of chapter 1 using vital statistics data. Thus, I expect the composition channel to have a smaller effect in 1981 compared to 1982 due to the difference in magnitude of the effects on childbearing (but also smaller effects of other channels, because the benefits were not in full-force in the early beneficiary regions in 1981). This substantial increase in childbearing is similar in magnitude to the effect reported in the abortion literature on the United States.

It would also be important to know the relative importance of the parental time and income channels. To do this, I test whether women spent more time with their children, or the program resulted only in an increase in income. I do not have access to information on how much leave women took. If women took more leave as a result of the

[36] I obtain the number of women of childbearing age using the 1989 Russian census. This census contains tabulations of the number of individuals present of a certain age and in a certain oblast. I calculate the number of women who are ages 15 to 44 in 1989 and backwards induct this number of women for years 1966 to 1988.

program, it is reasonable to expect that the share of all children in preschool may drop. Kindergartens were heavily subsidized by the government and widely used by parents. Parents had to pay 15 percent of the total cost of keeping their child in preschool. There was no limitation on the age at which the mothers could send their children to nurseries. There was variation in the availability of preschools geographically, where some areas lagged behind in their provision. A smaller share of children in rural areas was enrolled in preschool than in urban areas, because of limited availability of preschools in rural areas.[37] In 1980, about fifty percent of children younger than seven were in preschool, and about ten percent of children younger than three were in nurseries.

I use the preschool enrollment data to examine the effect of maternity benefits on preschool enrollment, but do not find evidence of a change in preschool enrollment after the start of the program.[38] This may imply that the program operated purely through the income channel – women were already spending a year with their children at home, and the program simply resulted in an increase in household income. However, if women used informal care by grandparents, instead of formal care, then we would miss an effect on this metric.

II.7 Descriptive Results: Evolution of Adult Outcomes by Birth Year and Birth Place

Descriptive analysis provides evidence of a limited effect of maternity benefits on children's outcomes. Figure II.1 presents the evolution of outcomes by birth year

[37] In 1984, 36.9 and 69.7 percent of children younger than seven were enrolled in preschool in rural and urban areas respectively.
[38] I manually transcribe data on the number of children younger than age 7 and enrolled in preschool from yearbooks called "Narodnoe Hozjajstvo".

separately for children born in early and late beneficiary oblasts.[39] The education level of children born after the start of the maternity benefit program grows at a smaller rate than for the children born before the program. This change in the growth rate appears to start a year earlier for children born in early compared to late beneficiary oblasts.

There do not appear to be changes in growth rates or jumps for children born after the start of the program for outcomes such as percent employed, percent receiving public assistance, percent whose main income is through disability benefits, percent married, and the average number of children ever born. The percent who were teen mothers seems to drop for children born after the start of the benefits, but the drop appears simultaneous in both early and late beneficiary oblasts.

II.8 Results: Net Effect of Maternity Benefits on Adult Outcomes

Tables II.1, II.2 and II.3 display estimates from specification (II.1), which represent the covariate-adjusted differences in adult outcomes between the early and late beneficiaries in the years after the start of the maternity benefit program compared to the years before. The results are weighted by the number of children born in 1979 in each oblast. The standard errors are clustered at the oblast-level to allow for an arbitrary correlation structure within an oblast.

It appears that the maternity benefit program resulted in less educated children. However, the effect is quite small. The program resulted in a 0.5 (s.e. 0.24) percentage point decline in college completion (table II.1, column 2), representing a 1.5 percent decline (0.5 percentage points out of a mean of 33) in college completion in children born

[39] The graphs omit the year 1980, because as mentioned earlier, the average outcomes of children born in that year are not representative.

in 1982 in early beneficiary oblasts compared to children born in the years before. The program resulted in a statistically insignificant decline in the measure that parametrizes the categorical years of education, with only a 0.2 percent decline in this measure.

I do not find evidence that the maternity benefit program has affected the adult economic circumstances of children. Table II.2 presents results, where the difference in the employment and public assistance outcomes between the early beneficiaries and late beneficiaries in 1981 and 1982 (when only early beneficiaries were treated) is not statistically different from the difference in earlier years.

The maternity benefit program does not appear to have affected the adult family structure of children. Table II.3 presents results, where the difference in the marital status, teen motherhood and number of children outcomes between the early beneficiaries and late beneficiaries in 1981 and 1982 is not statistically different from the difference in earlier years.

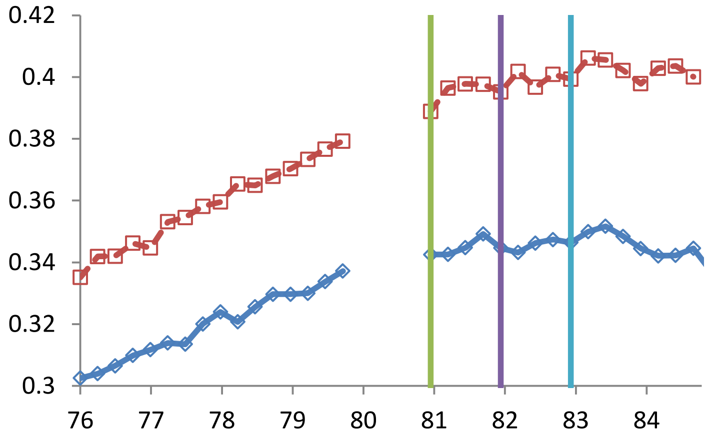
What is the overall effect of maternity benefits on outcomes, when you pool all outcomes into one measure? I create a summary index of several equally weighted average z-scores of outcomes, which move in the same direction, following the method of Kling et al. (2007). The index includes percent who have completed college, a linear education measure, whether work earnings is the main income, not a mother at age 17 or younger, and not having disability earnings as the main earnings. I find that maternity benefits had no significant effect on this index ($\pi_{82} = .053$, and $s.e. = .066$). This supports the findings after performing the analysis separately for every dependent variable, where a significant effect was only present for one educational attainment outcome.

II.9 Conclusion

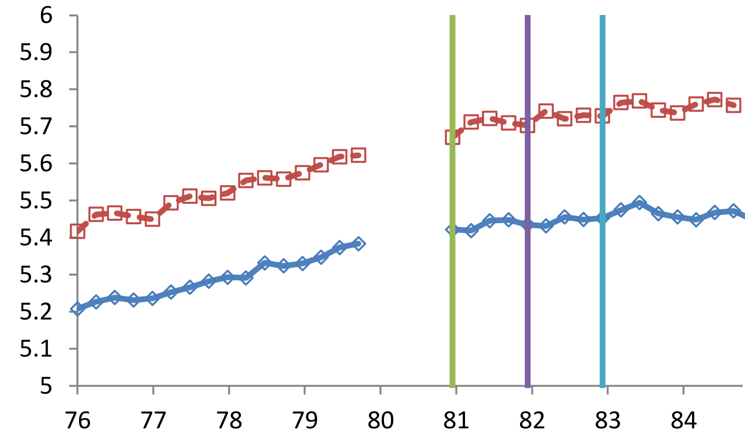
This paper focuses on how maternity benefits may affect children's adult outcomes. I study the expansion of these benefits in Russia in 1981 and find that children born one year after the program started were 1.5 percent less likely to complete college compared to children born right before the program. However, children born one year after the program are not statistically different in economic outcomes such as employment status, and receiving government assistance, as well as in family structure outcomes such as marital status, teenage childbearing and the number of children. These results indicate that while the composition of mothers changed after the program to include more economically disadvantaged women, there are very small if any differences between children born before compared to after the program started. Thus, while this program induced more births, its net effect on these births' adult outcomes is negligible.

Figure II.1 Descriptive Evidence of the Effect of Maternity Benefits on Adult Outcomes

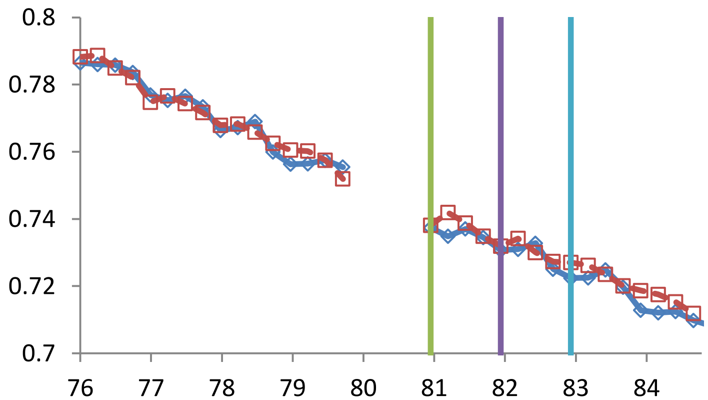
A. Percent who Have Completed College



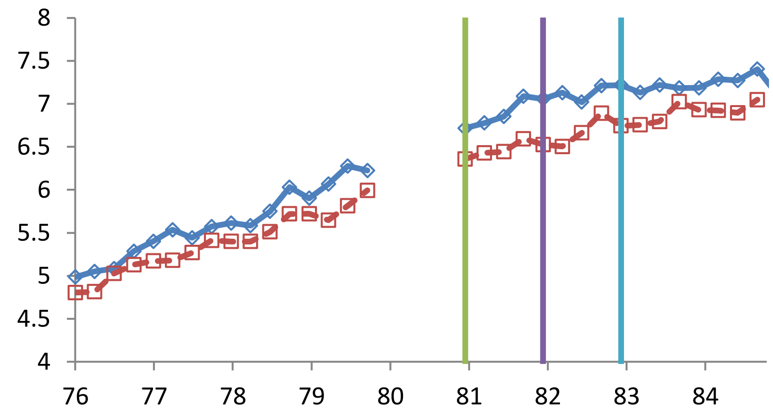
B. Years of Education Constructed Measure



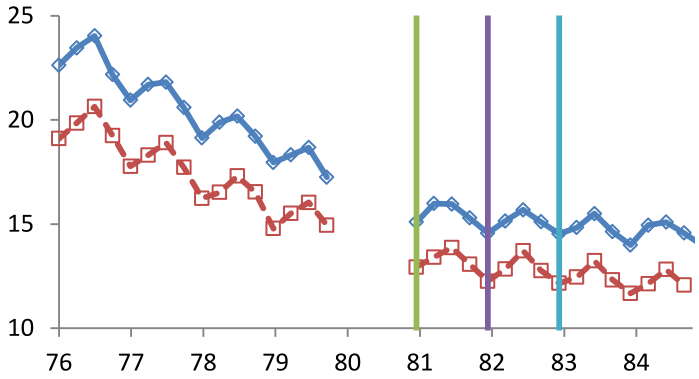
C. Percent of Women Employed



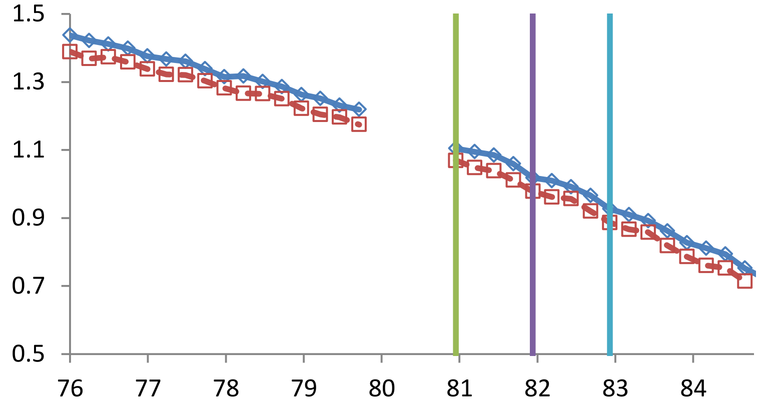
D. Percent on Government Assistance



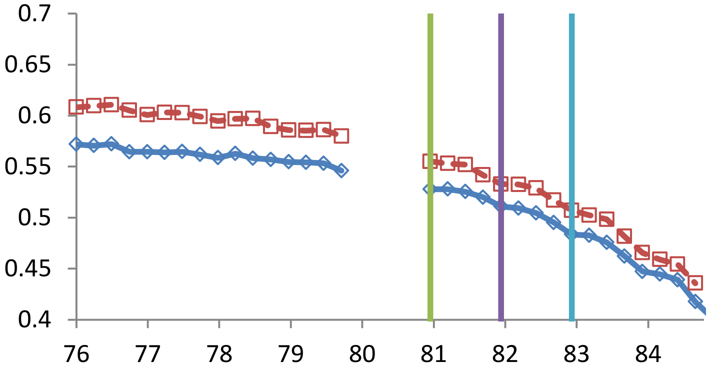
E. Percent Teenage Mothers



F. Average Number of Children

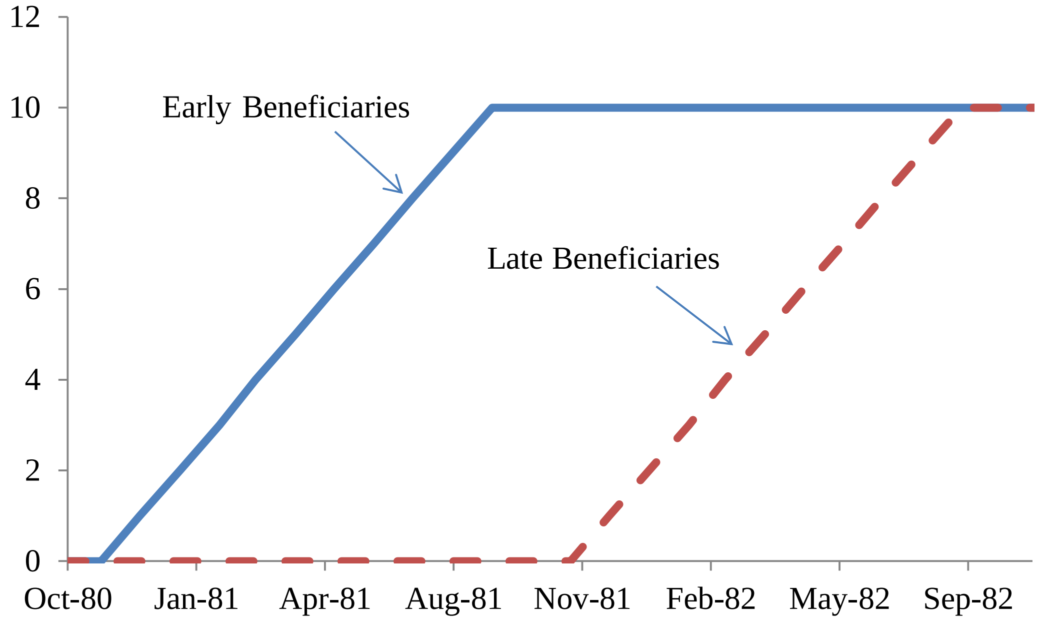


G. Percent Married



Notes: The blue solid line with diamonds represents children born in early beneficiary oblasts, and the red dashed line with squares represents children born in late beneficiary oblasts. The graphs represent the average outcomes of children by birth year and place of birth. In 1981 early beneficiaries started receiving partial benefits, while they received full benefits in 1982; in 1982 late beneficiaries started receiving partial benefits, while they received full benefits in 1983.The year 1980 is dropped, because of the over-presence of individuals with round birth years such as 1970, 1980, and 1990. Source. 2010 Russian Census.

Figure II.2 Months of Parental Leave by Month of Birth in Early and Late Beneficiaries



Notes: This figure shows the number of months of partially paid leave women were eligible to receive depending on the birth month of their child. The figure also shows the difference in the number of months of leave depending on whether the women lived in the early beneficiary or the late beneficiary oblasts.

Table II.1 The Effect of Maternity Benefits on the Fertility Rate and Adult Education Outcomes

	(1)	(2)	(3)
Dependent Variable	General Fertility Rate	Percent completed college	Linear education measure
Before Program mean in 1979	54.5	33.1	5.3
After Early Beneficiaries Eligible			
(Year 1981)*Early Beneficiary	2.086	-0.362	-0.005
	[4.767]	[0.213]	[0.011]
(Year 1982)*Early Beneficiary	4.767	-0.522	-0.014
	[0.762]	[0.243]	[0.012]
After Everyone Eligible	0.685	-0.633	-0.017
(Years 1983 to 1984)*Early Beneficiary	[0.977]	[0.304]	[0.016]
R-squared	0.996	0.998	0.999
Covariates	Year FE, Oblast FE, Xo,y, Oblast FE*Year	Year FE, Oblast FE, Xo,y, Oblast FE*Year	Year FE, Oblast FE, Xo,y, Oblast FE*Year
Oblast-birthyear cells	656	656	656
Oblasts	82	82	82

Notes: Columns (2) and (3) represent the difference in outcomes between the children born in early beneficiaries compared to late beneficiaries in grouped years relative to the difference in years 1976 to 1979. Column (1) represents the difference in the general fertility rate in the early beneficiaries compared to the late beneficiaries. Standard errors clustered at the oblast-level are in brackets. These coefficients represent interactions of the grouped year dummies and the dummy for the early beneficiary regions using the regression model described in equation (II.1). The grouped years represent: the year when benefits were in place for only two months (1981) in early beneficiaries, the year when the benefits were in place for the whole year (1982) in early beneficiaries, and the period after all regions became eligible for the benefits (years 1983 to 1984). The unit of observation is oblast by year. All columns include birth year fixed effects, oblast fixed effects, oblast by year covariates, and linear trends specific to each oblast. Regressions are weighted by the number of children born in each oblast in 1979 (columns 2 and 3), and by the number of women of childbearing age living in each oblast in 1979 (column 1, this is only for the general fertility result, and in all subsequent tables this weight is replaced by the weights in columns 2 and 3). Source: 2010 Census, 1989 Census.

Table II.2 The Effect of Maternity Benefits on Adult Economic Outcomes

	(1)	(2)	(3)
Dependent Variable	Percent women employed	Percent on public assistance	Percent on disability as main
Before Program mean in 1979	75.9	6.6	2.0
After Early Beneficiaries Eligible			
(Year 1981)*Early Beneficiary	0.098	-0.006	-0.018
	[0.310]	[0.078]	[0.0623]
(Year 1982)*Early Beneficiary	0.206	0.00231	-0.0789
	[0.333]	[0.101]	[0.073]
After Everyone Eligible	0.239	-0.162	-0.098
(Years 1983 to 1984)*Early Beneficiary	[0.447]	[0.119]	[0.089]
R-squared	0.998	0.996	0.976
Covariates	Year FE, Oblast FE, Xo,y, Oblast FE*Year	Year FE, Oblast FE, Xo,y, Oblast FE*Year	Year FE, Oblast FE, Xo,y, Oblast FE*Year
Oblast-birthyear cells	656	656	656
Oblasts	82	82	82

Notes: See notes for table I.1.

Table II.3 The Effect of Maternity Benefits on Adult Family Structure

	(1)	(2)	(3)
Dependent Variable	Percent married	Percent teen mothers	Average number of children
Before Program mean in 1979	54.3	19.0	1.3
After Early Beneficiaries Eligible			
(Year 1981)*Early Beneficiary	0.21	0.0646	0.004
	[0.263]	[0.287]	[0.005]
(Year 1982)*Early Beneficiary	0.191	0.240	0.006
	[0.308]	[0.396]	[0.007]
After Everyone Eligible	0.269	0.597	0.006
(Years 1983 to 1984)*Early Beneficiary	[0.424]	[0.468]	[0.008]
R-squared	0.998	0.991	0.999
Covariates	Year FE, Oblast FE, Xo,y, Oblast FE*Year	Year FE, Oblast FE, Xo,y, Oblast FE*Year	Year FE, Oblast FE, Xo,y, Oblast FE*Year
Oblast-birthyear cells	656	656	656
Oblasts	82	82	82

Chapter III

Does Family Planning Increase Children's Opportunities? Evidence from the War on Poverty and the Early Years of Title X

with Martha J. Bailey and Zoë McLaren

III.1 Introduction

Prenatal and early childhood experiences are widely accepted as important determinants of lifetime health, earnings, and well-being. Child and family characteristics measured around the age of five are as predictive of future earnings as more standard human capital measures such as educational attainment (Almond and Currie 2011). Children's household income, in particular, is one of the strongest correlates of completed education and adult health (Case 2002, Almond and Currie 2011). But, how best to design policies and programs to increase children's resources and improve their adult prospects remains an open question.

This paper evaluates federal family planning policies as a means to increase children's economic resources and, ultimately, lifetime opportunities. Both Presidents Lyndon Johnson and Richard Nixon argued that family planning would provide a healthful and stimulating environment for all children during their first five years of life.[40] Predictions from economic theory support this relationship as well. Family planning programs reduce the cost of using more effective contraception, which may reduce unwanted childbearing. Holding parents' income constant, a reduction in the number of children should increase per-child resources and promote parents' investments in children.[41] In addition, family planning programs may directly increase parents' income. Reducing the cost of delaying childbearing also reduces the cost for parents to invest in their own human capital and form more stable partnerships (Goldin and Katz 2002, Bailey et al. 2012), both of which can raise household income. Increases in income also promote parental investments in children (Becker and Lewis 1973, Willis 1973) and can mitigate the importance of credit constraints.

Empirical tests of the causal relationship between family planning and children's resources and opportunities has been limited by the dearth of data and available identification strategies.[42] Moreover, the presence of selection complicates the interpretation of well-identified estimates. If family planning programs cause more disadvantaged women to opt out of childbearing, this would mechanically cause the resources of the average child to rise while failing to increase the opportunities of any child that is born. The selection effect is potentially large, even for programs that are not

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[40] President Johnson said in a Special Message to the Congress on Domestic Health and Education, "We have a growing concern to foster the integrity of the family and the opportunity for each child. It is essential that all families have access to information and services that will allow freedom to choose the number and spacing of their children." (March 1, 1966).

[41] A reduction of the number of children born decreases the relative price of child quality in the standard quantity-quality frameworks (Becker and Lewis 1973). In addition, unwantedness may have an independent negative effect on child outcomes through mechanisms linked to health at birth (Corman and Grossman 1985; Grossman and Joyce 1990; Gruber, Levine and Staiger 1999) or parents' treatment of children (David et al. 2003, David 2006).

[42] A large literature studies the relationship of childbearing to child welfare. Schultz (2008) provides a thorough review of these studies in the context of developing countries. A much smaller literature studies the relationship of family planning programs and children's outcomes. See Miller and Babriaz (forthcoming) for a review of these studies for middle- and low-income countries. Section II of our paper also reviews related studies of abortion legalization in the U.S.

means-tested. For instance, the literature on abortion legalization claims a primary role for selection in explaining effects on child outcomes in the short run (Gruber, Levine and Staiger 1999) as well as in the longer run (Ananat, Gruber, Levine and Staiger 2009; Pop-Eleches 2006). Because family planning programs provided subsidies for related services, these programs' effects should also be concentrated among lower income women (Jaffe, Dryfoos and Corey 1973; Torres and Forrest 1985).

This paper provides new empirical evidence on the effect of family planning programs on children's economic resources and also employs a novel bounding exercise to illuminate the importance of selection in explaining these results. Our identification strategy uses the uneven expansion of federally funded family planning programs in the 1960s and early 1970s. This expansion first occurred through the Office of Economic Opportunity (OEO) during what was described as a "wild sort of [grant making] operation" (Gillette 1996: 193) and then, later, through the Department of Health Education and Welfare (DHEW) under Title X of the 1970 Public Health Service Act. The resulting county-level variation permits a research design that compares the outcomes of children in the same county born before and after federally funded family planning programs began.

Supporting the internal validity of this approach, the timing of the first federal family planning grant is uncorrelated both with differences and with changes in county-level fertility rates before family planning programs began as well as with 1965 measures of economic disadvantage, childbearing, sexual behavior, birth control use, and attitudes about sex and family before family planning programs began. Using newly compiled administrative data and the 1970 National Fertility Study (NFS), we show that use of family planning services and the birth control pill rose significantly among disadvantaged women after these programs began.

The analysis next links these county-level data on federal family planning programs to the full, restricted-use long-form 1970 and 1980 census samples and uses an event-study methodology to describe the evolution of children's economic resources for cohorts born before and after the programs began. The results show that children born after family planning programs began were significantly more advantaged in a number of dimensions. Children born after family planning programs began lived in households with 3 percent higher annual incomes relative to children in the same counties born before family planning programs began. Because roughly 11 percent of children's mothers increased their use of the Pill, these magnitudes imply that the incomes of affected children increased by 27 percent. In dollar terms, these estimates imply that the value of mother's access to family planning is as important for children's financial resources as adding another minimum wage worker to the household. These effects were largest among more disadvantaged families. Children born after family planning programs began were 8 percent less likely to live in poverty and 11 percent less likely to live in households receiving public assistance. Children born after family planning programs began were slightly more likely to live with two parent families.

A final contribution of this paper is a bounding exercise that tests the importance of selection in explaining the paper's main results—a first in this literature. Under the extreme assumption that all of the 2 percent of births averted due to family planning programs would have been the *poorest* children in the 1959 distribution, selection can only explain around 72 percent of the family planning program induced increase in children's household incomes by 1980. In the more likely and interesting case of probabilistic selection (which we calibrate to match empirical moments in family planning service users), selection may account for only 33 percent of the children's household income gains. These calculations, therefore, imply that other channels such as parents' own human capital investments (e.g., in their educations or careers) and investments in their partnerships (e.g., selecting and staying with their mates) explains at least 28 percent of our findings and, more likely, around 67 percent. These findings are consistent with the predictions of economic theory, which suggest that family planning increases children's resources and opportunities.

These results also allow a comparison between family planning programs and other policies that aim to improve the outcomes of disadvantaged children. Family planning programs begun in the 1960s reduced child poverty by 1 percentage point for every $556 million spent (2013 dollars). This is about one seventh of the cost of Temporary Assistance for Needy Families (TANF) and one hundredth of the cost of the Earned Income Tax Credit (EITC) for the same decrease in child poverty. In short, family planning programs may reduce childhood disadvantage at a much lower cost than many other public programs.

III.2 A Brief History of U.S. Family Planning Programs, 1960 to 1980

Enovid, the first birth control pill, was approved for use as a contraceptive by the U.S. Food and Drug Administration in 1960 and was immediately in high demand. But *Enovid* was under patent and prohibitively expensive. In the early 1960s, an annual supply of "the Pill" sold for the equivalent of $812 in 2013 dollars—roughly twice today's annual cost and equivalent to more than three weeks of full-time work at the 1960 minimum wage (without factoring in the cost of visiting a physician).

The implications of the Pill's costs raised concern among policy makers. Social scientists noted the strong *negative* relationship between household income and the number of children and the strong *positive* relationship of household income with birth control pill use. In 1960, 54 percent of women with less than a high school education had two or fewer children versus 77 percent among more educated women; 30 percent of women with less than a high school education had four or more children versus only 13 percent among more educated women.[43] The 1965 NFS also showed that poor women were significantly less likely to have ever used the Pill.

Widespread concern about disparities in access to the Pill, higher rates of childbearing among lower income women (National Research Council 1965), population growth (Wilmoth and Ball 1992, 1995), and the cycle of poverty galvanized support for federal intervention. The architects of President Johnson's War on Poverty viewed reducing income-based disparities in access to contraception as a means of promoting children's opportunities and increasing well-being in the long run.

The first U.S. family planning programs were quietly funded under the 1964 Economic Opportunity Act (EOA), a centerpiece of President Johnson's War on Poverty. The Office of Economic Opportunity (OEO), the office in charge of administering EOA funding, supported the opening of new clinics in disadvantaged areas and, to a lesser

[43] These figures are from the 1960 decennial census using a sample of ever-married women ages 41 to 50 years (Ruggles et al. 2010). In the 1965 NFS, women in households with incomes below the poverty line for a family of four had 0.60 more children on average (s.e. 0.16) than families earning at least four times as much. Poor women were over 44 percent (0.082 percentage points / 0.185, s.e. 0.023) less likely to use the Pill than the same group of more affluent women.

extent, the expansion of existing family planning programs. With the designation of family planning as a "national emphasis" program under the 1967 EOA amendments, federal funding for family planning rose to roughly 427 million (2013) dollars by 1970.

The aim of these programs was to bring education, counseling, and the provision of low-cost contraceptives and related medical services to disadvantaged women. (Programs did not provide abortion, which was still illegal except in special circumstances before 1970). But little else is known about these programs' day-to-day operations. During these early years, organizations ran programs with little oversight from the federal government. The federal government did not collect information on their services or patients, and officials talked very little about them.[44] The varied implementation of this program implies that its treatment effect represents a combination of many services and types of programs, all of which provided reduced cost contraceptives and related services.

The second large increase in federal funding for family planning occurred under President Nixon. In 1969, Nixon asked Congress to "establish as a national goal the provision of adequate family planning services within the next five years to all those who want them but cannot afford them." In November 1970, Congress passed Title X of the Public Health Service Act (also known as the Family Planning Services and Population Research Act, P.L. 91-572). By 1974, this legislation had increased federal support by 50 percent in real terms over EOA levels. As with family planning under the EOA, little is

[44] Sar Levitan (1969: 209) wrote that, "Contrary to the usual OEO tactic of trying to secure the maximum feasible visibility for all its activities, the OEO prohibited [family planning] grantees from using program funds to 'announce or promote through mass media the availability of the family planning program funded by this grant.'" Before 1965, U.S. federal involvement and investments in family planning had been modest.

known about how federal family planning dollars were spent in this early period. Most are believed to have paid for education, counseling, and the provision of low-cost contraceptives and related medical services. Abortion had been legalized in several states, but Title X explicitly prohibited the use of federal funds "in programs where abortion is a method of family planning" (§1008).

By 1973, federal funding had initiated or substantially expanded over 660 family planning programs. Over the next decade, federal appropriations for Title X fell to an average of roughly $400 million per year. But even as federal funding fell, seed dollars in the early 1970s helped nascent programs to gain support from other sources. By 1980, the Alan Guttmacher Institute (2000) estimated that only 50 percent of public support for family planning came from Title X, with the remaining funding coming from other state and local governments and non-profit organizations such as Planned Parenthood. By the early 1980s, family planning programs with any federal funding had grown to serve around 5 million patients annually. Roughly 83 percent of these family planning patients had incomes below 150 percent of the poverty line, and 13 percent were recipients of Aid to Families with Dependent Children (AFDC, the principal cash welfare program at the time). About 70 percent of patients were white. One quarter were black (Torres and Forrest 1985: 284).

III.3 Expected Effects of Family Planning on Childbearing and Child Outcomes

The potential effects of family planning programs on children's economic resources and living circumstances relate to their effects on parents decisions. Lower priced contraceptives and more convenient family planning services may decrease the number of *wanted* births and change the *desired* timing of childbirth. Family planning programs may also reduce *unwanted* and *ill-timed* childbearing by encouraging more consistent use of contraceptives or more reliable methods (Michael and Willis 1976).

This section reviews empirical work on the effects of family planning on fertility rates as well as the related literature on abortion legalization. We then develop a theoretical framework to simulate the potential impact of family planning programs through selection and other channels.

III.3.1 Empirical Studies of the Effects of Family Planning and Abortion Policy

Two recent empirical studies of family planning programs are consistent with the fertility effects of family planning being large. Kearney and Levine's (2009) state-level, differences-in-differences study uses variation in Medicaid eligibility for family planning among the near poor and finds that greater eligibility for services in 17 states significantly reduced birth rates among teens (by 4 percent) and among older women (by 2 percent) within a few years. In work closely related to this study, Bailey (2012) examines the impact of U.S. family planning programs on fertility rates by exploiting their county-level roll-out in the 1960s and early 1970s. She finds that general fertility rates fell by roughly 2 percent within 5 years of federally funded family planning programs beginning. Fertility rates remained approximately 1.7 percent lower up to 15 years after programs began. Bailey, Malkova and Norling (2014) use a differences-in-differences design based on Bailey's (2012) methodology to study the effects of family planning on child poverty in *public* census data. However, data availability at the county group level in public census data limits inferences in two ways. First, many county groups are so large that women in the county group would not live close enough to benefit from a family planning program. Second, as we show later in this paper, mobility across county groups between the time of birth and the time of the census is also substantial. Consequently, they find suggestive (though imprecise) evidence that U.S. family planning programs reduced child poverty rates in the short run as well as for affected cohorts in adulthood.

Additional evidence from the literature on abortion suggests that family planning programs could affect child outcomes as well. The staggered legalization of first-trimester abortion in the U.S., initially in five states around 1970 and then in the remainder of states with *Roe v. Wade* in 1973, provides the research design for a series of studies closely related to this paper. Levine et al. (1996) show that birth rates fell by 4 to 8 percent after 1970 in states with legal abortion. Gruber, Levine and Staiger (1999) use the same design and find that the children not born after abortion was legalized would have been 35 percent more likely to die as infants, 70 percent more likely to live in a single parent family, 50 percent more likely to live in families receiving welfare, and 40 percent more likely to live in poverty. Donohue and Levitt (2001) show that children not born after abortion legalization would have been more likely to commit crime,[45] and Charles and Stephens (2006) show they would have been more likely to use controlled substances in their late teens. For the case of Romania, Pop-Eleches (2006) provides evidence that the dictator's 1967 declaration that abortion and family planning were illegal increased birth rates by around 30 percent and worsened children's socio-economic outcomes. Both the sign and magnitudes of the effects of abortion legalization may differ from that of family planning programs, because abortion legalization and family planning programs may affect a different set of women (what we will call the

[45] Claims that abortion reduced crime is disputed (Foote and Goetz 2008, Donohue and Levitt 2004; Joyce 2004).

"selection channel") and because the treatment effect of abortion legalization likely differs from that of preventive family planning services.

III.3.2 The Expected Effects of U.S. Family Planning Programs on Children's Outcomes

The effects of family planning programs on children's living circumstances and economic resources could operate through several channels:

<u>The Selection Channel:</u> Family planning policy may *indirectly* affect the economic resources and living circumstances of children by altering selection into parenthood. This is particularly true for the federal family planning programs of the 1960s and 1970s, as they disproportionately benefited poorer households who could not have otherwise afforded these services. For instance, if family planning programs enable some lower-than-average-income households to opt out of childbearing, this would increase the average household income of parents and, therefore, their children.

<u>The "Human Capital" Channel:</u> Family planning programs may also *directly* affect the economic resources and living circumstances of children by altering a variety of parents' decisions. (We use the short hand term of "human capital" to refer to *all* changes in parents' decisions, though these are certainly broader than what is typically called "human capital.") For instance, soon-to-be parents may use family planning services to delay childbearing in order to get more education, select different career paths, or obtain different amounts of work experience and job training. Women might make different investments in their careers or stay attached to a job if they *expect* to be able to control future childbearing. These changes would tend to increase the economic resources of their future children. Alternatively, parents may decide to have children

earlier in their careers if subsequent births are more easily avoided, which could reduce the incomes of children in the short run but not necessarily in the long run.[46]

Another way in which family planning could affect children's household income is through partnership decisions. For instance, family planning programs could reduce the price of delaying marriage (Goldin and Katz 2002) and improve spouse matching, thereby reducing subsequent divorce rates (Christensen 2011, Rotz 2011). These forces would tend to raise the household incomes of children. On the other hand, family planning programs could increase non-marital childbearing (Akerlof, Yellen and Katz 1996), which should lower household incomes available to children. In short, the predictions about the direction of the effect of family planning on children's outcomes are theoretically ambiguous.

III.3.3 A Formalization of the Selection and Human Capital Channels

To illustrate the ways in which family planning affects children's household income, consider the function, $g(\quad)$, relating childbearing to household income, y, among parents before the introduction of family planning programs where $0 \leq g(y)$ for all y and $\sum g(y) = N$. The pre-family planning mean of children's household income can be written as $\mu \equiv \frac{1}{N} \sum y g(y)$. Let $g^{fp}(\,)$ represent a new function of childbearing after the introduction of a family planning program, where $0 \leq g^{fp}(\,)$ and $\sum g^{fp}(\,) = M$. For instance, if household income was unaffected by family planning, $g^{fp}(y)$ would differ

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[46] Recent empirical work informs these hypotheses. Studies evaluating the effects of early legal access to the Pill show that these law changes increased women's investments in their careers and, ultimately, their wages (Bailey, Hershbein, and Miller 2012). Early access to the Pill also appears to have increased men's educational attainment (Hock 2008), perhaps through intra-household bargaining. On the other hand, studies examining women who became mothers in their teens (relative to teens who miscarried) had *higher* subsequent levels of employment and earnings (Hotz, McElroy and Sanders 2005).

from $g(y)$ only to the extent that parents with lower incomes opt to have fewer children, $M < N$. To capture the human capital channel, let $h(y)$ represent the change in household income due to the introduction of a family planning program at each level of pre-family-planning income, y. The mean household income among children born after family planning programs begin can, therefore, be written as $\mu^{fp} = \frac{1}{M}\sum h(y) g^{fp}\big(h(y)\big)$. The effect of family planning programs on children's household income is:

$$\tau^{fp} \equiv \mu^{fp} - \mu = \sum \left(\frac{1}{M} h(y) g^{fp}\big(h(y)\big) - \frac{1}{N} y g(y) \right) \qquad \text{(III. 1)}$$

$$\frac{1}{M}\sum \underbrace{h(y)\left[(g^{fp}\big(h(y)\big) - \frac{M}{N} g\big(h(y)\big) \right]}_{selection\ effect}$$

$$+ \underbrace{\frac{1}{N}\sum g(y)[h(y) - y] + h(y)[g\big(h(y)\big) - g(y)]}_{human\ capital\ effect}$$

The first term in the second line captures what we call the selection channel: the change in the relationship between childbearing and household income. The second term represents what we will call the human capital channel: the shift in household income (holding constant the relationship between childbearing and income) plus the change in the income composition of parents resulting from this income increase.[47] In what follows, we simulate the magnitudes of these independent channels under different assumptions.

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[47] The second line of the equation is obtained by adding and subtracting $\frac{1}{N} g(y) h(y)$ and $\frac{1}{N} g(h(y)) h(y)$. As is standard in decompositions, the weighting of the selection and human capital effects will vary with the choice of cross-terms.

III.3.4 Probabilistic Selection

Consider the case of probabilistic selection and assume for simplicity that there is no human capital effect, $h(\ y) = y$. In this special case, the treatment effect arises only through changes in the distribution of childbearing by household income and equation (III.1) simplifies to:

$$\tau^s \equiv \mu^s - \mu = \frac{1}{M}\sum y\left[g^{fp}(y) - \frac{M}{N}g(y)\right] \qquad \text{(III. 2).}$$

Because family planning programs were disproportionately used by lower income women during this time, we expect the treatment effect under probabilistic selection to be positive.[48]

III.3.5 The Human Capital Channel

Women's human capital and partnership investments (we call this "human capital" for short) provide an additional and potentially complementary channel to selection. Assuming no selection effects, the distribution of childbearing by household income would be unchanged, $g^{fp}(\quad) = g(\,)$ and M=N. The treatment effect of family planning in equation (III.1) simplifies to:

$$\tau^h \equiv \mu^h - \mu = \frac{1}{N}\sum g(y)[h(\ y) - y] + h(\ y)\big[g\big(h(\ y)\big) - g(y)\big] \qquad \text{(III. 3).}$$

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[48] Selection by truncation, the most extreme case of probabilistic selection, would result in the following treatment effect of family planning on mean household income of children:
$\tau^T \equiv \mu^T - \mu = \frac{1}{M}\sum_{y>T} yg(y) - \frac{1}{N}\sum yg(y)$. The first term in the equation, μ^t, is the mean of children's household income after truncation. Under selection by truncation, we approximate the treatment effect directly by using Bailey's (2012) estimates of the effects of family planning programs on fertility rates ((N-M)/N) and the percentiles of the 1960 empirical distribution of children's household income.

If family planning programs raise the incomes of lower income women by allowing them to finish school, increase the incentives for investing in careers, or facilitate partner search, we expect the treatment effect to be positive. On the other hand, if family planning encourages non-marital childbearing (Akerlof et al. 1996), these programs could decrease children's resources.

III.3.6 Simulated Effects

Table III.1 provides several simple examples to illustrate the potential magnitudes of effects of the selection and the human capital channels if they operated in isolation or in combination. We examine two cases of selection and four cases of human capital effects.

<u>S1. Probabilistic selection:</u> We drop 2 percent of children from the 1960 census and assume that the averted births would have been born to mothers with household incomes distributed normally with a mean at the poverty threshold for a family of four with two children ($3000 annually in 1959 distribution) and a variance of ¼ of the standard deviation of the 1959 empirical distribution of children's household income. These assumptions result in 85 percent of the women who averted births having incomes below 150 percent of the poverty threshold, which is similar to the poverty rate of family planning program users in administrative data (83 percent). Under this case, the household incomes of children born after family planning programs began are about 1 percent higher (table III.1, col. 2, row 1).

<u>S2. Selection by truncation</u>: We assume that 2 percent of children from the 1960 census would have been averted and that the averted births occurred to children in the lowest

part of the 1959 income distribution. This results in truncation of children with household incomes below $286.25 annually in the 1959 distribution of children's household income. Under this case, household incomes of children born after family planning programs began are 2.16 percent higher (table III.1, col. 3, row 1).

H1-H3. Additional education: We assume that 4 percent of mothers increased their human capital due to the establishment of family planning programs.[49] Consistent with an increase in education of 1, 2 and 4 years of one or two earners combined, H1 assumes household incomes increased by 6 percent, H2 by 12 percent, and H3 by 24 percent. In these cases, the household incomes of children born after family planning began are 0.34, 0.68 and 1.36 percent higher (table III.1, col. 1, rows 2-4).

H4. Additional earner: We assume that family planning programs induce 4 percent more mothers to have a child within a stable partnership and with an equal earning partner, effectively increasing the household income of children by 100 percent. In this case, the household incomes of children born after family planning are 5.67 percent higher on average (table III.1, col. 1, row 5).

Table III.1 additionally presents combinations of these effects (rows 2 through 5, cols. 2 and 3). Noteworthy is that the effects through different channels can be very similar. For instance, selection by truncation yields an increase of roughly 2.16 percent, whereas increasing human capital (H3) combined with probabilistic selection (S1) increases children's household income by 2.22 percent. In addition, the interaction of probabilistic selection and human capital effects may yield sizable treatment effects.

[49] Section III estimates that 4 percent is the share of ever-married women using the Pill because of family planning programs.

III.3.7 Implications for Empirical Analysis and Interpretation of Results

Our analysis quantifies the treatment effect of family planning by taking the difference between the outcomes of children born just before and just after family planning programs began using cross-sectional data. The implicit assumption is that cohorts born just before family planning programs began allow us to estimate the pre-family planning mean, $\mu \equiv \frac{1}{N}\sum yg(y)$, whereas cohorts born just after family planning programs began allow us to estimate $\mu^{fp} = \frac{1}{M}\sum h(y)g^{fp}\big(h(y)\big)$. The difference between the two means will understate the treatment effect of family planning on children's outcomes if children born before family planning programs began also benefitted (e.g., through intra-household spillovers).

A limitation of this literature is that it is nearly impossible to disentangle empirically the selection and what we call "human capital" channels. As our analysis makes clear, this cannot be done without further assumptions on $g^{fp}(\ )$ and $h(\ )$.[50] Using changes in the composition of parents to proxy for selection is inappropriate, because these changes may reflect selection or endogenous changes in parents' human capital or some combination of both. Our simulation makes clear that the effects of either channel operating in isolation could be of similar magnitudes, and that the effects of both channels could be large. It also makes clear that the interaction of the human capital and selection channels (and the choice of weights implicit in the cross-term) does not permit a clean separation of the two effects, except in the simplest case of selection by truncation.

[50] An alternative strategy would be to use longitudinal data to examine selection and human capital channels directly, but available datasets are too small to estimate effects of the magnitude found in this study using our research design.

To inform the interpretation of our estimates, we use the 2.16 percent increase in household income implied by the extreme case of selection by truncation to generate an upper bound on the role of selection and, therefore, a lower bound on the role of human capital channels.

III.4 Data, Research Design, and Measurement Error Corrections

Our empirical analysis builds upon Bailey's (2012) empirical methodology to examine the relationship of family planning to the economic resources and living circumstances of children. Information on children's outcomes comes from the restricted, long-form samples of the 1970 and 1980 censuses (20-percent and 16-percent samples of the U.S. population, respectively). In addition to their large sample sizes, the restricted samples include identifiers for counties, which are restricted in public use data.[51] Our analysis aggregates the economic resources and living circumstances of children under age 18 into birth-year/county cohorts. These birth-year/county cohorts are then linked to their parents' access to federally funded family planning programs.

Information on family planning programs from 1964 to 1973 is derived from two sources: the National Archives Records about Community Action Program Grants and Grantees (NACAP), which contain information on family planning programs funded under the EOA, and the National Archives Federal Outlay (NAFO) files, which contain information on family planning programs funded under Title X. Figure III.1 shows the roll-out of federally funded family planning programs at the county level with different

[51] We gained access to both datasets after a formal application process in the University of Michigan Research Data Center.
Public data identify only county groups (which change between 1970 and 1980) and are much smaller samples of the population.

shades of grey. The earliest programs, established between 1964 and 1967, are shaded in the lightest gray; the programs established between 1968 and 1969, during the expansion of family planning as a national emphasis program, are shaded in the next darkest gray; and programs established from 1970 to 1973 under Title X are in black. By 1973, programs had begun in each of the coterminous 48 states and roughly 60 percent of the U.S. population of women ages 15 to 44 in 1960 lived in counties with federally funded family planning programs. Funded counties (what we call those receiving a federally funded family planning program) differed from unfunded counties. Funded counties were more urban, had more elderly residents, and were more educated and affluent. Funded counties also had lower poverty rates (appendix G table G1). Our analysis accounts for these cross-sectional differences using county-fixed effects or by examining a sample of funded counties only. After describing our empirical specification, we provide empirical evidence supporting the validity of using variation in the timing of first family planning grant as a research design.

III.4.1 Empirical Specification

Our analysis builds on Bailey's (2012) research design. Our primary specification describes the evolution of outcomes for cohorts born before versus after family planning programs began in

their county of residence within the following event-study framework:[52]

$$Y_{j,t} = \theta_j + \gamma_{s(j),t} + \sum_{c=a}^{b} \tau_c D_j 1(t - T_j^* = c) + \boldsymbol{X}'_{jt}\boldsymbol{\beta} + \varepsilon_{j,t} \qquad \text{III. 4} ,$$

[52] We run separate regressions by census years. This is because in the 1970 census data only the 461counties that received their first family planning grant before 1970 identify τ, whereas for 1980 the full set of funded counties identify τ.

where Y is a measure of the outcomes for children residing in county j within state s and born in calendar year t. D_j is an indicator equal to 1 if county j received a federal family planning grant from 1964 to 1973, and $1(\,)$ indexes birth cohorts relative to the year of the first federal family planning grant, T_j^* —our proxy for the date the program started.[53] Thus, event time, c, runs from a years before up to b years after the date of the first family planning grant, which varies by census year.[54] Other covariates are added sequentially and include θ, a set of county fixed effects which capture time-invariant county-level differences, and γ, a set of state-by-birth year fixed effects that should capture time-varying changes in state policies, including abortion legalization and the roll-out of Medicaid. Following Almond, Hoynes and Schanzenbach (2011), $\boldsymbol{X}$ includes annual information on per capita measures of government transfers from the Bureau of Economic Analysis Regional Information System (REIS) (cash public assistance benefits such as Aid to Families with Dependent Children, Supplemental Security Income, and General Assistance; medical spending such as Medicare and military health care; and cash retirement and disability payments). Following Bailey (2012), we also include the number of abortion providers, which accounts for within-state changes in the provision of

[53] Data limitations (federal family planning grant information is missing for some years) make it impossible to use federal grant dollars as the independent variable of interest. But even if this were feasible, we prefer a binary measure of family planning access for several reasons. Using variation in federal funding could also be related to program performance, which could induce reverse causality and threaten the interpretation of our estimates. Second, as described in section I, federal dollars paid for infrastructure when needed and also many programs were heavily supported by other public and non-profit funds by the end of the period of interest. Thus, federal dollars are poor proxies of program size or intensity. Our specification captures the fact that federal dollars *created* or significantly expanded family planning programs.

[54] The 1980 census allows us to examine the evolution of outcomes for cohorts born up to six years after the establishment of the family planning program for a balanced set of counties. The 1980 census, however, only observes a two-year cohort pre-trend for a balanced set of counties because many of the individuals in cohorts born before 1963 had begun leaving home (and the earliest family planning programs began in 1965). Therefore, we set $a=-3$ when $c\leq-3$ and $b=7$ when $c\geq7$, and event-years -2 through 6 are estimated using all funded counties for the 1980 census. We estimate separate regressions with the 1970 census and set $a=-7$ when $c\leq-7$ and $b=1$ when $c\geq1$. $c=0$ is omitted in both cases to facilitate easy comparisons across census years.

abortion between 1970 and 1979.[55] This analysis, thus, recovers the regression-adjusted evolution of children's outcomes for cohorts born from six years before (in the 1970 census) and up to six years after (1980 census) each county received its first federal family planning grant. Standard errors are robust to heteroskedasticity and corrected for serial correlation within state (Arellano 1987, Bertrand, Duflo, and Mullainathan 2004).

III.4.2 Evidence Supporting the Internal Validity of the Research Design

The timing of family planning program initiation provides identifying variation for the analysis. Few written records document family planning funding decisions, but oral histories and interviews provide a rich picture of the funding decisions in this era. Donald Baker, Chief Counsel of the OEO, recalls: "It was a wild sort of operation in those early days, making the first grants. We didn't have any guidelines and didn't have the time really to draft them to start out" (Gillette 1996: 193). Robert Levine (1970) sums up the situation saying, "It was an era of great administrative confusion."

Quantitative evidence from four complementary sources supports these accounts. First, county characteristics in the 1960 census found to predict the roll-out of other War on Poverty programs fail to predict when federal family planning programs began (Hoynes and Schanzenbach 2009; Almond, Hoynes, and Schanzenbach 2011). The main correlate of these programs' initiation is the share of the county population in urban areas, perhaps owing to the fact that urban areas are better able to apply for funding and have more infrastructure for programs to build upon (see appendix G table G2). Second,

[55] The interactions of county covariates are identical to those in Almond, Hoynes and Schanzenbach (2011). Because information on abortion providers is not available at the county level before 1973, we follow Joyce, Tan, and Zhang (2013) in assuming the number of providers in 1970 to 1972 in states that legalized before Roe v. Wade is identical to the number observed in 1973. Note that changes in the distance to states providing legal abortion before 1970 is accounted for in the state-by-year fixed effects.

1964 fertility rates or 1960 to 1964 changes in fertility rates (appendix figure G1) are uncorrelated with the initiation of federal family planning programs. It does not appear that administrators used fertility rates to prioritize funding or that communities with higher fertility rates were more likely to obtain funding. Third, reproductive and contraceptive attitudes and behaviors in the 1965 NFS are uncorrelated with the initiation of federal family planning programs. This is true of individual predictors (appendix G table G3) and pooled outcomes. To improve the statistical power to detect correlations that move in a common direction, we follow Kling, Liebman and Katz (2007) and create a summary index of equally weighted average z-scores of pro-natalist responses to questions regarding contraceptive attitudes, behaviors, and other correlates of the number of children. Including the variables in appendix G table G3 in a common index (normalized by the mean and standard deviation of the unfunded distribution), we find that the year of the first federal grant for family planning is not significantly related to the index of pro-natalist variables (coefficient: 0.054, robust standard error: 0.047, observations: 2,857).[56]

A fourth analysis shows that the initiation of federal family planning programs is uncorrelated with the initiation of other War on Poverty programs (appendix G figure G2). The likelihood of receiving a family planning grant does not appear to be correlated with the likelihood of receiving a community health center, a Head Start grant, a jobs program grant, a legal services grant, or a grant for maternal and infant care. Altogether, this empirical evidence supports oral accounts of the "wild" grant making.

[56] This z-score index excludes age at first pregnancy and age at first Pill use from the index. Including the age at fist pregnancy (which implicitly omits women who never became pregnant), we find that the year of the first federal grant to family planning is positively but insignificantly related to the index (coefficient: 0.043, robust standard error: 0.054, observations: 2,607). We exclude when a woman first used the Pill (appendix G table G3, column 6) because its inclusion severely limits our sample size.

III.4.3 Evidence of the Relevance of Federal Programs in Increasing the Use of Family Planning

Another key assumption in our research design is that the initiation of federal family planning programs meaningfully increased the use of family planning services. The four-fold increase in patients of federally funded family planning programs from 1969 to 1983 is suggestive, but it might largely reflect federal crowd out of other family planning programs.

Two new data sources allow us to quantify the importance of federal family planning program initiation on the use of these services. Hand-entered OEO reports, based on surveys of *all known* providers of family planning (hospitals, health departments, and clinics operated by other agencies) in fiscal year (FY) 1968, calendar year (CY) 1969, and FY 1971 (OEO 1969, 1971, 1974), provide information from the clinic perspective.[57] Using a restricted version of equation (III.4), panel A of table III.2 shows that federal family planning programs significantly increased the use of family planning services among "medically indigent" women.[58] The share of medically indigent women using family planning services increased by around 2.7 percentage points *after* the federal family planning programs began. These estimates are robust to the inclusion

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[57] Completion rates of the survey were high. In 1968, for example, 97 percent of hospitals and 100 percent of all other agencies responded (OEO 1969, table 3: 244). The purpose of the survey was to approximate the universe of potential family planning providers for our period of interest and document the number of "medically indigent" patients (but not other patients).

[58] We estimate $Y_{j,t} = \theta_j + \gamma_{s(j),t} + \tau 1(t > T_j^*) + \boldsymbol{X}'_{j,t}\boldsymbol{\beta} + \varepsilon_{j,t}$ where $Y_{j,t}$ is the share of medically indigent patients in county *j* using family planning services from any provider (federally funded or not) in time *t* (FY 1968, CY 1969, and FY 1971), θ_j is a set of county fixed effects, $\gamma_{s(j),t}$ is a set of state-time fixed effects, and $\boldsymbol{X}$ is a set of covariates including REIS controls and 1960 county covariates interacted with a linear trend. The binary indicator, $1(t > T_j^*)$, is equal to 1 for observations in years after the date county *j* received its first federal family planning grant, T_j^*. The point estimate of interest, τ, captures the differential change in share of medically indigent women using family planning services after federal family planning programs were established. With the inclusion of county fixed effects, only counties receiving federal programs between 1968 and 1971 identify τ.

of additional covariates (columns 1-3) and about half the magnitude of the national increase in family planning program use over the same period.

The 1970 NFS provides an alternative perspective from the point of view of ever-married women between the ages of 18 and 44. Our restricted specification of equation (III.4) tests whether respondents living in counties that had received a federal family planning grant before 1970 were more likely to have used the Pill (panel B). We also examine the interaction of living in a county that received a federal family planning grant with the respondent's poverty level to examine whether the programs' effects on use were indeed concentrated among poorer women.[59] The estimates show that poor women in areas funded before 1970 were much more likely to have used the Pill relative to poor women in areas funded after 1970, an increase of approximately 25 percent (15 to 17 percentage points) over the mean among poor women. In fact, the effect of a federal family planning grant is large enough to erase income-based differentials in Pill use.[60]

A final piece of evidence consistent with federally funded family planning programs affecting the use of these services is that they reduced fertility rates. Using an

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[59] We estimate the specification, $\Pr(\mathrm{Use}_{ij}) = \mathbf{F}(\mathbf{Z}_{ij}\boldsymbol{\delta} + \theta_1 1(\mathrm{T}_j^* < 1970) + \theta_2 1(\mathrm{Pov}_{ij}) + \theta_3 1(\mathrm{Pov}_{ij}) 1(\mathrm{T}_j^* < 1970))$, where Use_{ij} is equal to 1 if an individual *i* in county *j* had ever used family planning or the birth control pill, $1(\mathrm{T}_j^* < 1970)$ is a binary variable equal to one if county *j* received a federal family planning grant before the survey date, and $1(\mathrm{Pov}_{ij})$ is a binary variable equal to 1 if the annual household income in 1970 fell below the poverty line. County fixed effects cannot be included with this single cross-section, but we include a rich set of covariates, **Z**, including state fixed effects, dummy variables for age, educational achievement, population size of the county, and Catholic religion. In addition, one specification includes dummy variables for the "number of children most desirable" to capture residual, unaccounted-for differences in the demand for children. Panels B and C of table III.2 report average partial effects associated with θ_1 and θ_3 from probits and bootstrapped standard errors (1000 replications) and capture differences in the use of family planning services and the Pill in funded counties by 1970.

[60] Another interesting finding is that Pill use increased by about 4 percentage points for women *above* the poverty line in funded locations. This estimate is not statistically significant but suggests family planning grants may have reduced the price of the Pill among women using other sources of family planning.

unweighted, event-study specification and county-level Vital Statistics data on births from 1959 to 1988, figure III.2A shows that fertility rates fell dramatically in counties after federal family planning programs began. Although the pre-treatment differences in the general fertility rate are close to zero and statistically insignificant, the fertility rate fell more rapidly in funded locations after federal family planning programs began—even after accounting for county and year effects, state-by-year fixed effects, and time-varying county-level covariates. Fifteen years after family planning programs began, the general fertility rate remained 1.4 percent to 2 percent lower than in the year the program started. As shown in Bailey (2012), these findings are robust to (1) omitting unfunded counties, (2) weighting the regressions, (3) including county-level linear time trends, and (4) only using the sample of counties where programs were funded before Title X.

III.4.4 Strategies to Minimize Misclassification Error in Treatment Status

An important challenge to our analysis is that the censuses only contain information on a child's residence in the census year, not at the time of the child's birth. Consequently, mothers' access to federal family planning around the time of conception may be measured with error. If misclassification is random with respect to mother's access to family planning programs, this should lead to an understatement of the program's effects on children's outcomes. The effects of systematic misclassification are harder to sign.

A comparison between birth rates derived from the 1980 census and Vital Statistics suggests misclassification error is empirically important and is of the attenuation variety. In figure III.2A, estimates using Vital Statistics (which uses mothers' county of residence at the time of birth) show a large and precisely estimated reduction in fertility rates following the introduction of federally funded family planning programs. Using the same cohorts, model, and dependent variable, census estimates are substantially attenuated. In fact, misclassification error is large enough to completely obscure the treatment effect of family planning on fertility rates.[61]

We use several strategies to reduce misclassification error in access to family planning programs. First, we use county of residence in 1975 rather than in 1980 because it is more temporally proximate to births between 1963 and 1975 and determined prior to births occurring from 1976 to 1979. Second, we exclude unfunded areas from our estimation sample in order to minimize the role of attenuation due to differential mobility in funded areas in the post-period (see footnote 21). Third, our analysis does not include population weights because mobility (and hence mobility-induced misclassification error) was much greater in more populous places receiving family planning programs. Weighting by population, therefore, increases the relative importance of high-mobility areas, thus attenuating our estimates. Aside from reducing misclassification error, these choices should have little effect on our results for children's outcomes, because estimates

[61] Two hypotheses could explain these results. The first relates to the selection channel where cohorts born after family planning programs began were more likely to be wanted, and, therefore less likely to die in infancy or childhood. Gruber et al. (1999), for instance, finds that infant mortality rates dropped among cohorts born in states after abortion was legalized. A family-planning-induced reduction in infant or child mortality would reduce the discrepancies between Vital Statistics and census data in the post period and lead our analysis to find a smaller reduction in fertility rates in the census than in the Vital Statistics. However, Bailey (2013) finds little evidence that federal family planning grants were associated with decreases in infant mortality.

The second hypothesis relates to the human capital channel where the largest beneficiaries of family planning programs were more likely to move (e.g., to attend school or follow a partner). Information on whether respondents moved locations in the 5 years before the census supports this hypothesis. Appendix G figure G3 shows that children born after family planning programs began were significantly more likely to live with a parent who changed treatment status in the five years before the census. Limiting the analysis to only funded counties (which implies omitting D_j from equation III.4) reduces the mobility differential in misclassification of the post-period. In the funded sample, however, parents of children born after family planning programs began were <u>not differentially likely to change treatment status</u>. Moreover, the treatment effects of family planning on fertility rates in the funded sample are very similar to those in the entire sample (Bailey 2012).

of effects of federally funded family planning programs on fertility rates are robust to restricting the sample to funded counties or weighting by population.

A final correction for measurement error adapts the approach of Card and Krueger (1992) to characterize the sign and magnitude of any remaining bias due to mobility. Their insight is that mobility induced measurement error leads the estimated coefficients, τ_c, to be a reweighting of the true coefficients, τ_j^*. In our case,

$$\tau_c = \sum_{j=a}^{b} \tau_j^* \cdot p_{j,c} \qquad \text{(III. 5)}$$

Here $p_{j,c}$ is the probability of being born in a county treated with family planning j years before birth conditional on living in a county at the time of the census that was treated c years before birth. To characterize how mobility could bias our estimates, we use the matrix form of equation (III.5) above to recover the full set of estimates, $\boldsymbol{\tau}^* = \boldsymbol{\tau}\boldsymbol{P}^{-\mathbf{1}}$.[62] Figure III.2B shows that this ex post adjustment for misclassification increases the magnitude of fertility estimates, which is again consistent with mobility induced misclassification error attenuating the results. But after having made the three adjustments above, this final correction has a very modest effect on the estimated magnitudes. Also noteworthy is that the census estimates—adjusted and unadjusted for

[62] $\boldsymbol{\tau}$ is a (b-a+1) × 1 column vector containing each of the event-study coefficients, τ_c for c=a, a+1, a+2…, b, and $\boldsymbol{P}$ is a (b-a+1) × (b-a+1) matrix with elements of the transition probabilities, $p_{j,c,}$ such that $\boldsymbol{\tau} = \boldsymbol{\tau}^*\boldsymbol{P}$. Note that this inversion also assumes that migration is uncorrelated with treatment, which holds in this context in our analysis. We estimate $p_{j,c}$, as the probability of living in a county in 1975/1965 receiving a family planning program j years before a birth conditional on living in a county in 1980/1970 that received a family planning program c years before birth. Implicitly, this assumes that county-to-county misclassification of treatment status between 1975 and 1980 (or 1965 and 1970) is correlated with mobility induced misclassification that occurred before 1965/1975.

misclassification using equation (III.5)—are virtually identical to the Vital Statistics estimates in event years 4 through 5. We focus our discussion on these estimates.

III.5 Estimation Results: Economic Resources and Living Conditions among Children Born after Family Planning Programs Began

The first set of results shows that cohorts born after federal family planning programs began had much higher household incomes, both in total and per capita terms. To simplify the interpretation of the coefficients, figure III.3 plots estimates of τ divided by the 1980 pre-treatment mean dependent variable from our baseline model, which includes county and state-by-birth-year fixed effects. The series, therefore, denote changes in *percent* for each birth cohort indexed relative to the year the family planning program began. Estimates to the left of the vertical axis capture cohorts born in event years before family planning programs began (1970 census; plotted in dashed lines with markers), and estimates to the right of the vertical axis capture cohorts born after family planning programs began (1980 census, solid lines with markers). Dashed lines present 95-percent, point-wise confidence intervals for the baseline model.

Figure III.3A shows a notable trend-break after the county received its first federally funded family planning grant. Children born four to five years after the program began had household incomes that were on average 2.1 (event year 4; table III.3, column 2: 1086/68,417) to 3.0 percent higher (event year 5; table III.3, column 2: 1456/68,417). In contrast, the coefficients on household incomes among children born up to six years before family planning programs began exhibit little trend and are not statistically different from the year the county received its first family planning grant. Table III.3 demonstrates the robustness of the estimates across specifications for the 1980 census (appendix G table G4 present 1970 estimates). Column 1 includes county and year fixed effects; column 2 adds 850 state-by-year fixed effects (50 states *17 birth years); and column 3 adds county-level covariates. Column 4 adjusts for misclassification error in our baseline model. As expected, applying our adjustment for misclassification using 5-year migration patterns raises the post-period estimates slightly. Trimming the top and bottom 1 percent of children's household incomes reduces these estimates slightly.[63]

These event-study estimates capture intention-to-treat effects but imply much larger effects on children whose parents were directly affected. Table III.2 implies that roughly 4 percent more of ever-married women ever used the Pill in areas receiving a federal family planning grant. Assuming that the 4 percent increase applies to the population of married *and unmarried* women with children (who in treated areas had 2.8 children in the 1980 census), then around 0.112 percent (0.04*2.8) in the 1980 sample of children were affected by family planning programs. This implies that the typical federal family planning grant increased the family incomes of affected children by roughly 19 (0.021/0.112) to 27 (0.03/0.112) percent—an increase in the mean of $13,000 to $18,500 in 2013 dollars. This increase is equivalent to a 55 to 78 percent increase over the 2013 poverty threshold for a family of four. It is large enough to be almost the same as adding another minimum wage worker to the household (0.90 to 1.27 percent of an annual minimum wage worker's wage of $14,500).

[63] Because the restricted census data is not top-coded, we also investigate the importance of outliers by trimming. Trimming the top and bottom 1 percent of incomes somewhat reduces the magnitude of the estimate at year +6 but has a negligible effect on event years 4 and 5. One exception is event year −5 in 1970, where one child in a very small county came from an extremely affluent family. This single observation inflated the estimate at event year −5 by 30 percent in the full sample over the trimmed sample. Figure III.2 suppresses this outlier from the presentation. See appendix G tables G4 for the full set of estimates.

The most extreme form of selection by truncation implies an increase of only 2.16 percent in the mean household income of children (section II), an upper bound on the independent role of selection in our estimates. Thus, human capital channels explain at least 28 percent (0.84/3) of children's household income gains.[64] The implication is that family planning programs enabled disadvantaged parents to raise their own earnings capacity (e.g., by investing more in their educations and careers) and select more stable and higher earning partnerships (e.g., by selecting different mates). The more interesting case of probabilistic selection suggests that the human capital channel may explain 67 percent of the gains in children's household income.

These results are driven by strong effects among more disadvantaged families. Recall that administrative statistics show that 83 percent of these family planning patients had incomes below 150 percent of the poverty line and 30 percent were nonwhite (whereas only 17 percent of women ages 15 to 44 are nonwhite). The former statistic suggests that the effects of family planning programs on children should be larger at the lower end of the income distribution. The latter suggests that the effects of family planning should be larger among nonwhites.

[64] The reduction in fertility rates implies that family planning programs could translate into larger increases in income per capita than in average household income. Our analysis using household income per capita as a dependent variable is consistent with this claim (see appendix G table G4). The point estimates are larger than can be explained by the pure mechanical relationship implied by dividing by fewer people (i.e. assuming fertility rates dropped at random by 2 percent) and are more precisely estimated. Estimates for event years 2 to 6 are each (individually) statistically different from zero. For cohorts born in event year 5, household incomes per capita were 3.2 percent higher versus 2.2 percent implied by simple division (2.1/0.98). For cohorts born in event year 6, household incomes per capita were 3.9 percent higher, again greater than the 3.1 percent implied by simple division (3.0 / 0.98). These results underscore the conclusion that family planning-induced human capital and selection effects increase the resources of the average child.

Figure III.4A shows that the reductions in poverty were stronger at the bottom of the income distribution. Averaging event years 4 to 6, children born after federal family planning programs began were 6.8 percent less likely to live in poverty (figure III.4A; table III.4, column 1), 6.0 percent less likely to live below 1.5 times the poverty line (figure III.4B; table III.4, column 2), and 3.1 percent less likely to live below 2 times the poverty line (figure III.4C; table III.4, column 3). As with household income, we find no evidence that these reductions reflect a pre-trend. Small changes in the average household incomes of nonwhite children (figure III.3C) mask large changes at the lower end of the income distribution for this group.[65] Although the mean increases in household income were larger among white children (figure III.3B), the absolute reductions in poverty rates were three to five times larger in the post-period among nonwhite children (table III.4; columns 7 to 9) than for white children (columns 4 to 6). The share of nonwhite children living in poverty was 5.1 percent lower for cohorts born 4 to 6 years after family planning programs began (column 7), whereas poverty rates for white children born in the same years was 2.2 percent lower and statistically insignificant (column 4). Within race group, the effects are also stronger at the bottom of the income distribution. The relative reduction in the share of children in poverty and below 150 percent of the poverty line is also larger than the reduction in the share of children below twice the poverty line, both overall and within race group.

One natural question relates to how much of the reduction on poverty rates is attributable to changes in family size (which determines the income threshold for

[65] Note that the available data limit our ability to detect effects among nonwhites. Although nonwhites were overrepresented in the population of family planning patients, family planning patients were more likely to be white and the white population was significantly larger. Consequently, estimates for nonwhites are less precise for household income and other outcomes considered.

poverty). Family planning programs could reduce the likelihood that the household members live in poverty (holding income fixed) because they reduce future childbearing.[66] We, therefore, repeat our analysis using household structure at the time of the census to reconstruct poverty thresholds for each child at the time of birth, subtracting out younger siblings that arrived before the 1980 census. Using this *lower* alternative threshold, however, generates even stronger reductions in poverty rates, demonstrating that childbearing delay is an important mechanism through which family planning affects children's resources.

Another indication that family planning programs affected children at the bottom of the income distribution is the reduction in the share of children living in households receiving public assistance. Figure III.5A shows that cohorts born 4 to 6 years after family planning programs began were on average 11.4 percent less likely to live in households receiving public assistance (table III.5, column 1) relative to those born just before family planning programs began. Figure III.5B and III.5C also show that, although white children were much less likely to be in households receiving public assistance than were nonwhite children, the absolute and relative reductions in public assistance receipt were significantly larger within this group. White children born 4 to 6 years after family planning programs began were 10.9 percent less likely to live in households receiving public assistance (table III.5, column 2). Although the estimates in the post-family planning period are statistically zero among nonwhite children (column 3), the positive trend in public assistance receipt disappears in the post-period.

[66] Consider a family of two earning $18,000 in 2013: they would *not* fall below the federal poverty line of $15,500. But, having a child at the same annual income would put the same family below the federal poverty threshold of $19,530 for a family of three.

Family planning programs also appear to have had little adverse effect on family structure. Figure III.6A shows that children born after family programs began were no more likely to live in single-headed households, in contrast to conventional wisdom or theoretical arguments along the lines of Akerlof et al. (1996). Although the 95-percent confidence intervals in figure III.6A (table III.5, column 4) include zero, one-sided tests reject that the share of children living with single parents increased among cohorts born after family planning programs began. The estimates are generally small and negative for white children but much noisier for nonwhite children (figures III.6B and III.6C; table III.5, columns 5 and 6). These estimates are inconsistent with claims that greater access to family planning led to an increase in childbearing among couples who never married or entered marriages that were less durable.(See also appendix G figure G4 that shows little direct effect of federally funded family planning programs on marriage and divorce rates in Vital Statistics data).

Two final analyses examine the impact of family planning programs on characteristics of mothers. Figure III.7A shows that the average age of mothers (at the time of birth) fell for each cohort born after family planning programs began. Overall and for both white (figure III.7B) and nonwhite children (figure III.7C), mothers' average age at birth fell by roughly one quarter of a year for cohorts born 5 years after family planning programs began (table III.6, columns 1 to 3). This does not mean that family planning programs did not lead couples to delay childbearing. It implies, however, that averaging delays in childbearing (e.g., an increase in the age at first birth from age 20 to 22) with much larger reductions in age at birth through the prevention of accidental pregnancies (e.g., a reduction in the age at last birth from 38 to 30) resulted in a net fall in the average age at birth. Figure III.8A additionally shows that children born after family planning programs began had significantly fewer older siblings,[67] a result which is quantitatively much larger among nonwhite households. Together, these findings imply that older women with more children were more likely to use family planning programs to avoid further childbearing, which may have had effects on the resources of older children born before family planning programs.

III.6 Discussion and Conclusions

Using a new research design and large, restricted-use census samples, this paper quantifies the effects of the earliest family planning programs on children's economic resources and living circumstances. Our comparison of children in the same county born before and after the introduction of family planning programs suggests that affected cohorts were economically better off. Cohorts born five years after family planning programs began lived in households earning 3 percent more annually. Scaling these estimates by our best approximation of benefiting children implies a treatment effect on the treated of a 27 percent increase in household income ($18,500 in 2013 dollars). The largest gains in household income accrued to the most disadvantaged families. Children born after family planning programs began were 8 percent less likely to live in poverty and 11 percent less likely to live in households receiving public assistance. These results may understate the broader effects of family planning programs on children's economic resources to the extent that older siblings (born before family planning programs began) also benefitted.

[67] We calculate the number of older siblings in the household at the time of each child's birth by subtracting out younger siblings from those present in the household.

A second key finding is that much of this effect may have been achieved through the *direct* impact of family planning on disadvantaged parents, who delayed or constrained childbearing to make investments in their own human capital and partnerships. Our bounding exercise shows that the most extreme form of selection—truncating the poorest 2 percent of children from the 1959 distribution—implies an increase of only 2.16 percent of the estimated 3 percent increase in children's mean household income. This upper bound on the role of selection implies a lower bound on the role of other channels. Parents' human capital and partnership decisions explain at least 28 percent of the gains in children's household income. In the more likely and interesting case of probabilistic selection, parents' human capital and partnership decisions account for 67 percent of these gains.

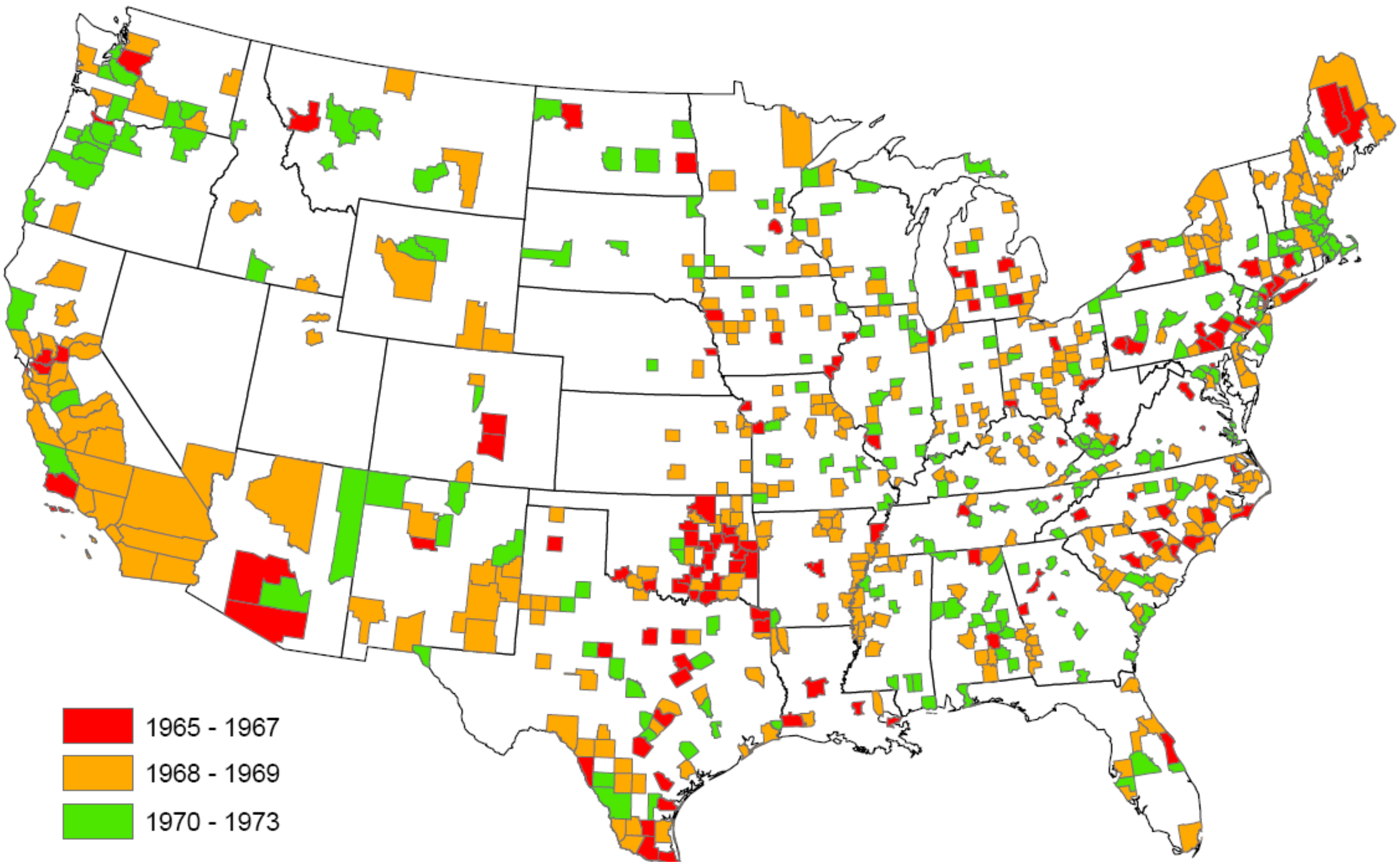
Simple cost-benefit calculations permit comparisons of family planning programs with other public policies aiming to increase the resources of disadvantaged children. In the 1960s the federal government spent an average of around $278 million per year (2013 dollars) on family planning, or $4.4 billion cumulating over 1965 to 1980 (the period considered in this analysis). Combined with our estimates, this implies that child poverty was 1 percent lower for every $556 million spent on family planning ($4.4 billion/8). If the family planning program reduced public assistance cases by at least 5.5 percent (recall our estimates imply an 11 percent reduction in *children* living in households receiving public assistance), family planning programs would have generated more cost savings *in one year* than they consumed in outlays over the entire 1965 to 1980 period. Because there were roughly two children per public assistance recipient in 1979, family planning programs may have paid for themselves *in full* through their short-run effects on public assistance receipt.

Comparisons to other programs targeting child poverty are also illuminating. According to the supplemental poverty measure (SPM) for 2012, TANF cost $10.24 billion in 2013 dollars and reduced child poverty rates by 2.7 percent (from 18.5 to 18.0 percent). A conservative estimate that ignores offsetting behavioral changes and deadweight loss implies that TANF reduced child poverty by 1 percent for every $3.8 billion spent. Another useful comparison is the EITC. In 2012, the EITC cost around $63 billion, and the 2012 SPM suggests that EITC and the refundable portion of the child tax credit reduced child poverty rates by 27 percent (from 24.7 to 18.0 percent). A similar calculation implies that EITC reduced child poverty by 1 percent for every $2.3 billion spent. Accounting for behavioral changes, the effects of TANF may have been smaller and the effects of EITC larger (Hoynes, Miller and Simon forthcoming), but the EITC (without the benefit of other child tax credits) would have to induce a 4-fold larger reduction in child poverty to be comparable to family planning in terms of the per-child cost in child poverty rate reduction.

Family planning may also have broader effects on outcomes not captured in the census. Studies of the effects of income transfers through the tax system on academic test scores provide related evidence on this point. Dahl and Lochner (2012) use variation in EITC eligibility over time and find a 4 to 6 percent of a standard deviation improvement in children's test scores for each $1,000 of additional income. Milligan and Stabile's (2011) study of Canada's child benefit programs and Chetty, Friedman and Rockoff's (2011) study of U.S. tax credits find comparable estimates. Ignoring the effect of family planning on the relative price of investing in children (implied by standard quality-quantity models), the more conservative estimate implies that family planning programs could increase test scores among affected children by three quarters ($18,500/1,000×0.04) of a standard deviation.

Family planning programs may have long-run implications as well (Cunha and Heckman 2007, Almond and Currie 2011). Aizer, Eli, Ferrie, and Lleras-Muney (2014) show that children receiving a 12 to 25 percent increase in household income through the mother's pension program in the early twentieth century went on to attain about 0.4 years more schooling, had healthier weights in adulthood, earned about 14 percent more as adults, and lived about one year longer. Consistent with this, Bailey (2013) provides suggestive evidence from public census data that cohorts born after family planning programs began were 2 percent more likely to attain 16 or more years of education and had 1 percent higher family incomes as adults (see also Schultz 2008 and 2009 for evidence from developing countries). Future work should investigate these longer-run linkages as well as the intergenerational implications of family planning programs on the economy.

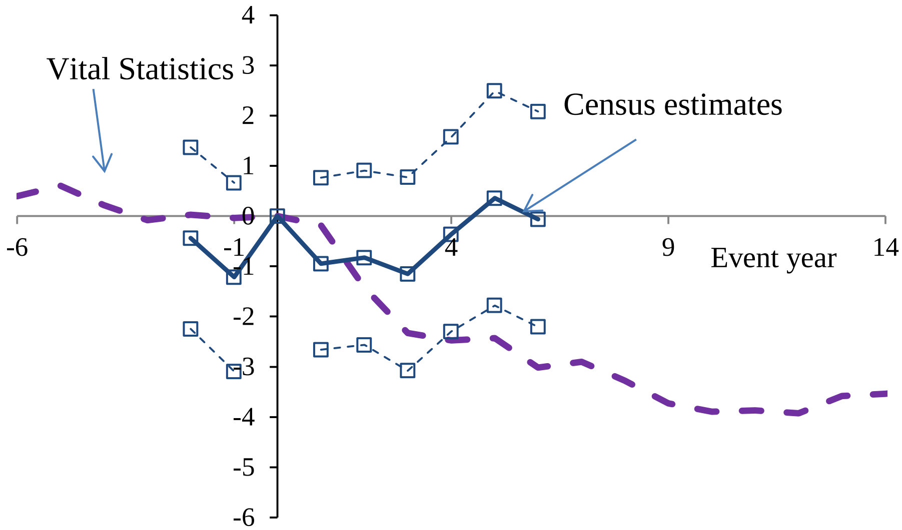
Figure III.1   The Roll-Out of Federally Funded Family Planning Programs, 1965-1973



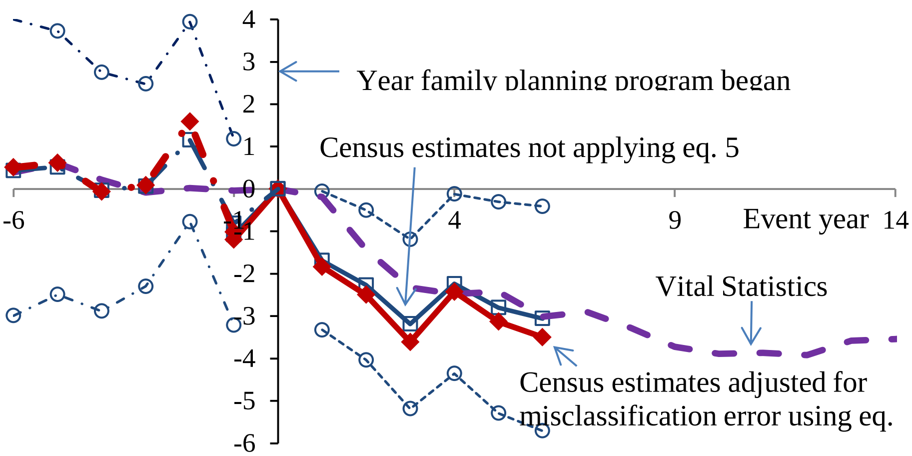
Dates are the year that the county first received a federal family planning grant. Counties not receiving a family planning grant between 1965 and 1973 are not shaded. Sources: NACAP, NAFO and OEO (1969, 1971 and 1974).

Figure III.2 General Fertility Rates Before and After Family Planning Programs Began

A. Comparison of Vital Statistics and Census Estimates before Misclassification Error Adjustments

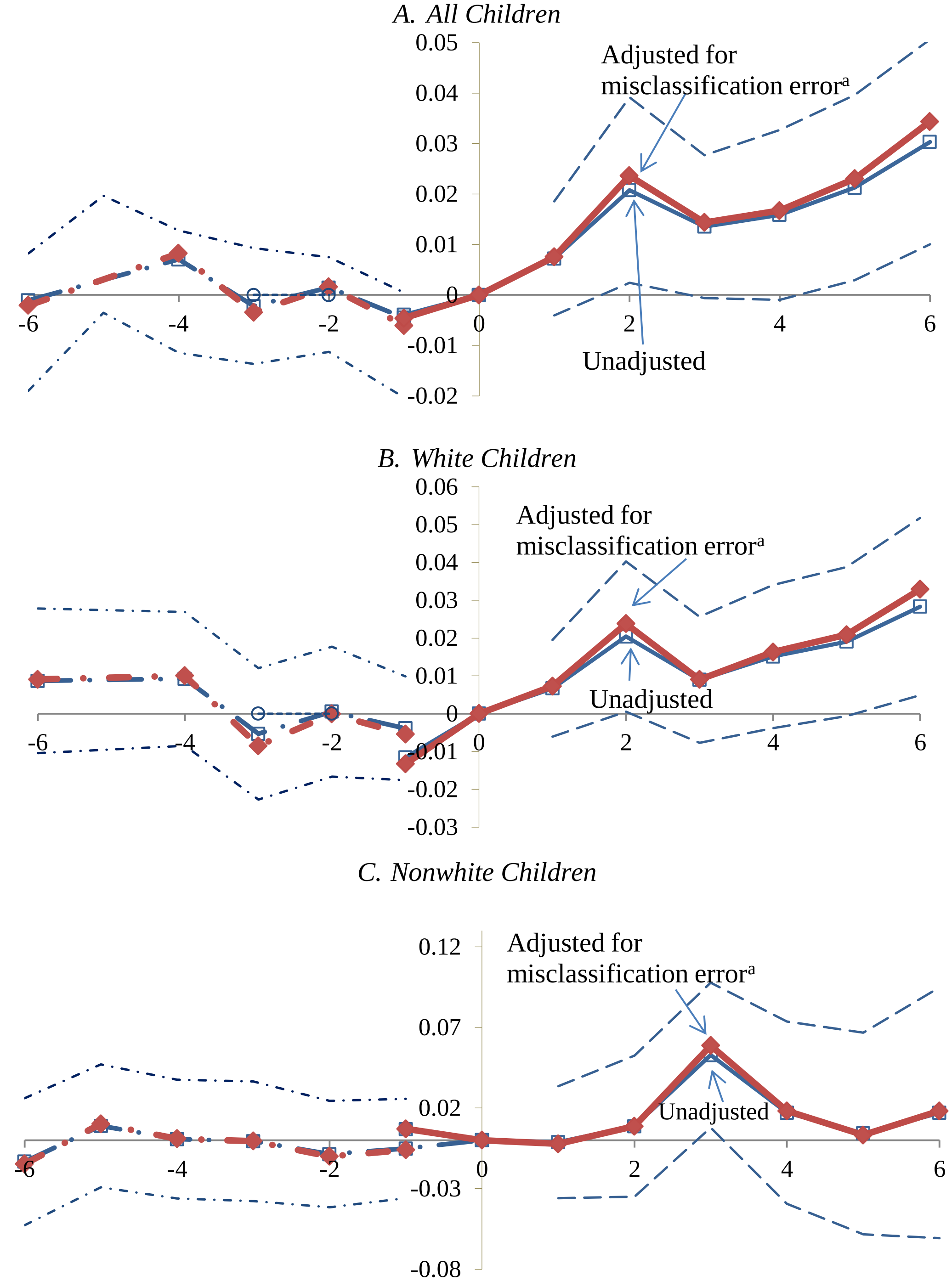


B. Comparison of Vital Statistics and Census Estimates after Misclassification Error Adjustments



Series plot estimates of τ from our baseline model of equation (III.4). The x-axis plots the event year, equal to year of observation minus year of first family planning grant, the year the federal family planning began. The dependent variable is the general fertility rate (GFR) calculated using either Vital Statistics or the 1980 census. The Vital Statistics have little measurement error in county of birth, as county represents county of mother's residence at the time of the birth as reported on the birth certificate. The census estimates use the GFR implied by the county of residence in 1965/1975 and age of the child in the census. Census estimates in panel A include both funded and unfunded counties and weight by population. Census estimates in panel B include only funded counties and are unweighted. [a] Adjustment for misclassification error indicates that eq. 5 has been used to alter the estimates after estimation as described in text. Sources: 1970 and 1980 restricted long-form census samples for both numerator and denominator estimates. Vital Statistics estimates use, for GFR numerators, hand-entered, county-level birth aggregates published in Vital Statistics from 1959 to 1967 and the Natality Detail Files from 1968 to 1988 (NCHS 2002). For GFR denominators, SEER county-level estimates of women ages 15 to 44 from 1969-1988 are augmented with interpolated, county-level estimates of the same population between the 1960 census and the 1969 SEER.

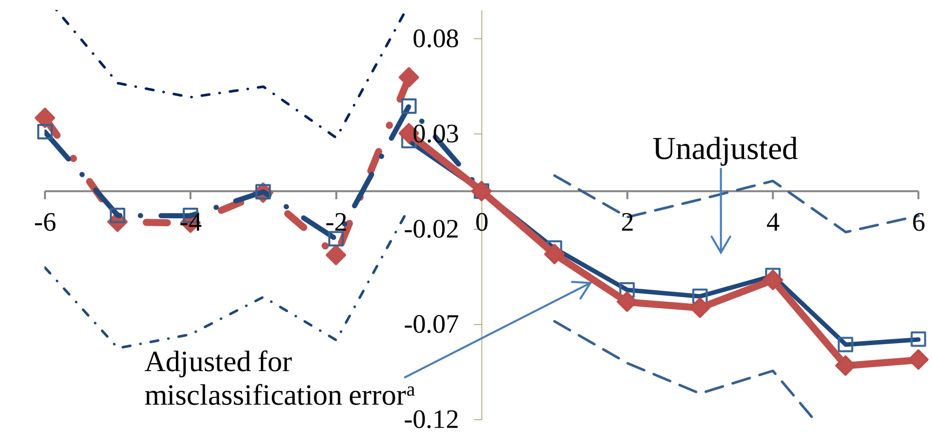
Figure III.3 Percent Change in Children's Household Income for Cohorts Born before and after Family Planning Programs Began



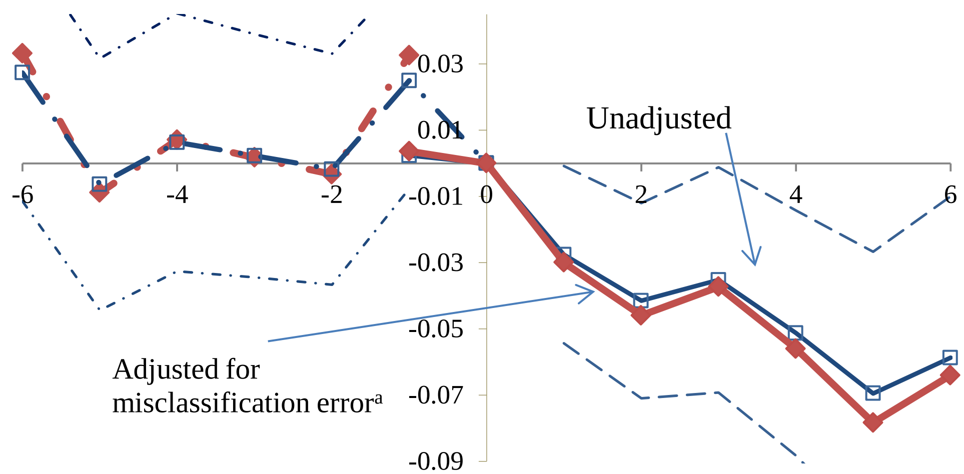
Panels plot of τ from equation (III.4) for model 2, both unadjusted and adjusted for misclassification error, divided by the average dependent variable for cohorts in the same county born before family planning programs began in the 1980 census. [a] Adjustment for misclassification error indicates that eq. 5 has been used to alter the estimates after estimation as described in text. Heteroskedasticity-robust standard errors clustered by county are used to construct 95-percent, point-wise confidence intervals for the baseline model (dashed lines). Sources: 1970 (dashed lines with markers) and 1980 (solid lines with markers) restricted-use censuses. See table III.3 for estimates.

Figure III.4 Percent Change in Children Living in Poverty for Cohorts Born before and after Family Planning Programs Began

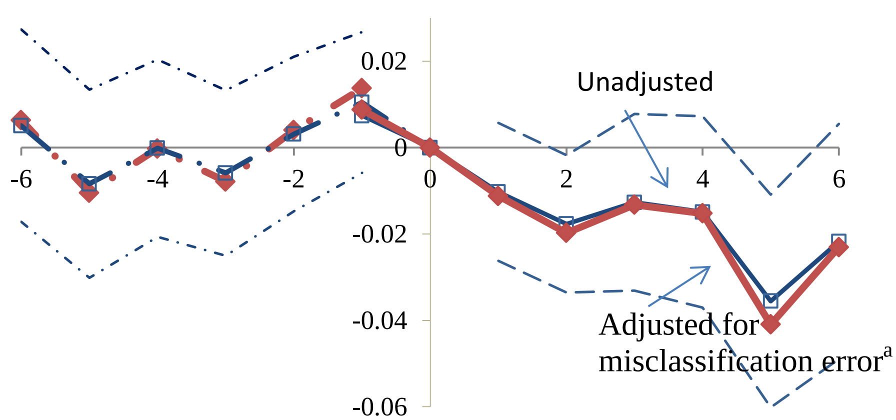
A. Below Poverty Line



B. Below 150% of the Poverty Line

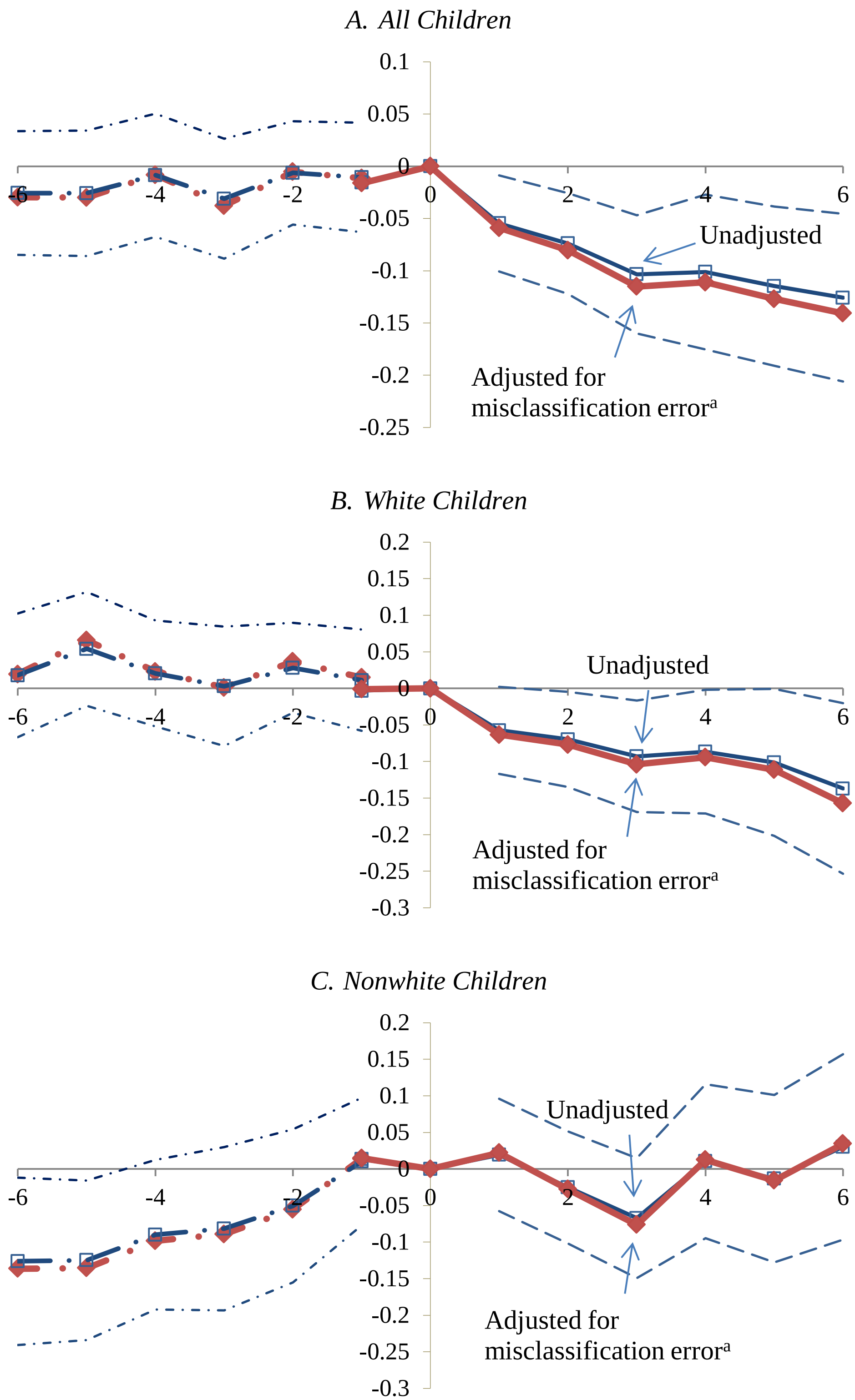


C. Below 200% of the Poverty Line



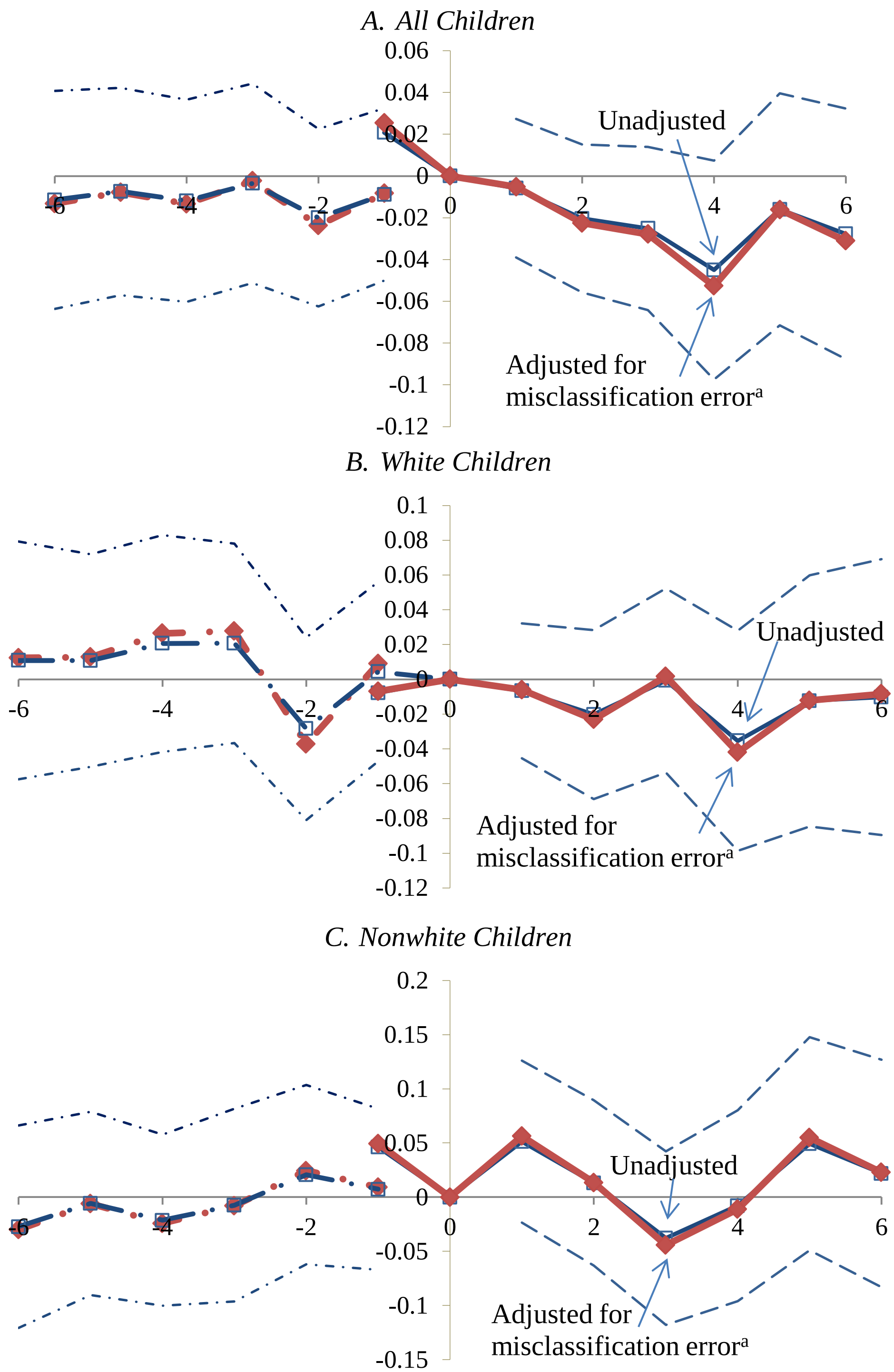
See notes for figure III.3 and estimates in table III.4.

Figure III.5 Percent Change in Children Living in Households Receiving Public Assistance for Cohorts Born before and after Family Planning Programs Began



See notes for figure III.3 and estimates in table III.5.

Figure III.6 Percent Change in Children Living in Single-Parent Households for Cohorts Born before and after Family Planning Programs Began



See notes for figure III.3 and estimates in table III.5.

Figure III.7 Average Age of Mother for Cohorts Born before and after Family Planning Programs Began

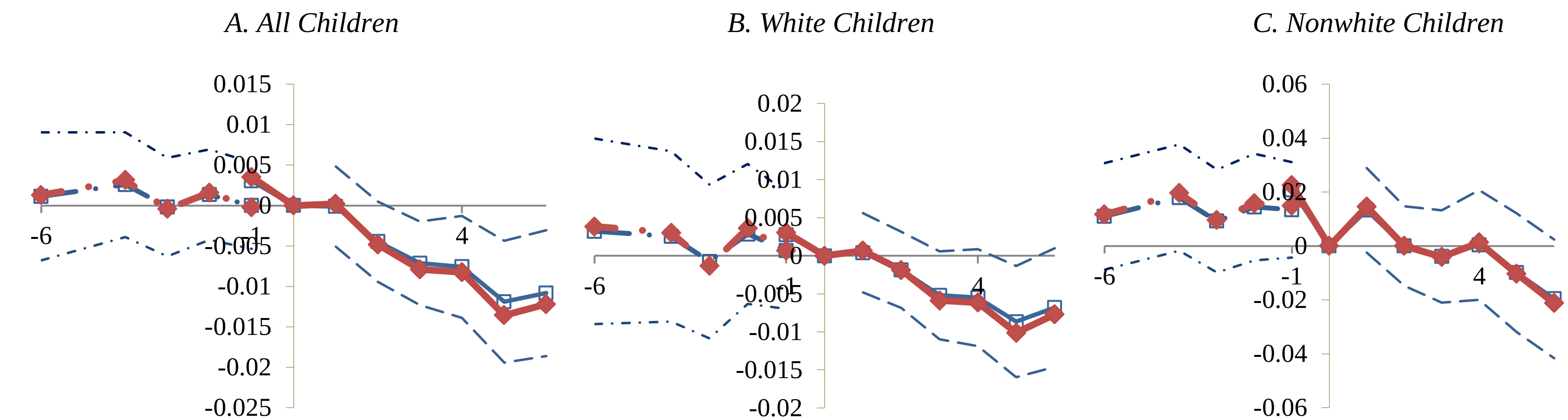
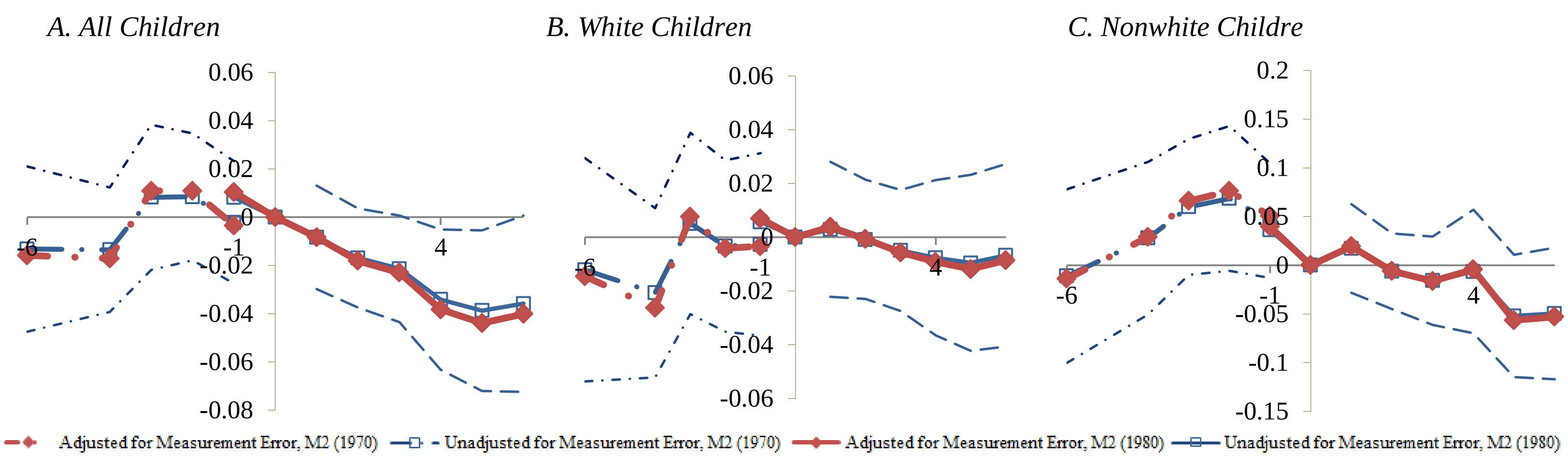


Figure III.8 Average Number of Older Siblings for Cohorts Born before and after Family Planning Programs Began



See notes for figure III.3 and estimates in table III.6.

Table III.1 Simulated Effects of Family Planning Under Different Hypotheses Regarding Selection and Human Capital Accumulation

	(1) S0. No selection	(2) S1. Probabilistic selection	(3) S2. Selection by truncation
H0. No human capital effects	--	0.97	2.16
H1. One year of schooling (6 % increase in household earnings)	0.34	1.19	2.50
H2. Two years of schooling (12 % increase in household earnings)	0.68	1.53	2.85
H3. Four years of schooling (24 % increase in household earnings)	1.36	2.22	3.55
H4. Additional worker in household (100 % increase in household earnings)	5.67	6.61	7.94

The table presents percent change in household income of children through various channels described in the text, both in isolation and in combination. For this analysis, we use the sample of children younger than 18 and observed in the 1960 census. We estimate the average household income of children before family planning as the average of the whole sample. For S1, the children we drop 2 percent of children such that the dropped children have incomes distributed normally with mean as the poverty threshold for a family of 4 and with one quarter of the standard deviation of the distribution of incomes of all children in 1960. For S2, we truncate children below the 2nd percentile in 1960 distribution of children's incomes. For H1-H4, we calculate the effects of family planning when 4 percent of mothers whose distribution of incomes before family planning is the same as in S1 increase by 6% (H1), 12% (H2), 24% (H3), and 100% (H4). Source: 1960 census in the IPUMS (Ruggles et al. 2010).

Table III.2 The Use of Family Planning Services before and after Federal Family Planning Programs Began

	(1)	(2)	(3)
	A.Dependent Variable: Share of Medically Indigent Patients Using Family Planning Services (1968 Mean=0.046)		
After family planning began	0.027	0.028	0.027
	[0.011]	[0.011]	[0.012]
R-squared	0.71	0.75	0.75
Counties	666	666	666
Observations	1998	1998	1998
Covariates	C,Y	C,S-Y	C,S-Y,R,X
	B.Dependent Variable: 1=Ever Used the Pill (Pre-treatment Mean=0.56)		
After family planning began	0.040	-0.005	-0.007
	[0.024]	[0.023]	[0.020]
In Poverty	-0.165	-0.160	-0.179
(Mean DV=0.58)	[0.084]	[0.075]	[0.073]
After family planning began × In Poverty *(Mean DV=0.65)*	0.166	0.132	0.146
	[0.076]	[0.070]	[0.076]
Pseudo R-squared	0.026	0.157	0.165
Observations	3699	3699	3681
State fixed effects	X	X	X
Other covariates		A,C,E,P	A,C,E,P,K

Panel A. The unit of observation is a county-year in FY1968, CY1969 and FY1971, and estimates are of τ from a restricted version of equation (III.4) using funded counties. Column 1 includes county, C, and year, Y, fixed effects. Column 2 adds state-by-year, S-Y, fixed effects. Column 3 adds 1960 county covariates interacted with a linear trend, X, and REIS controls, R. Heteroskedasticity-robust standard errors clustered by county are presented beneath each estimate in brackets. Sources: OEO 1969, 1971 and 1974. Panel B: The unit of observation is a married woman ages 18 to 44 in 1970. The estimates are average partial effects associated with θ_1, θ_2, and θ_3 from a probit specification of a restricted form of equation (III.4) using funded counties. Bootstrapped standard errors (1000 replications) are reported in brackets beneath. Columns 1-3 include state fixed effects, column 2 adds dummy variables for age categories (A), Catholic (C), educational achievement (E), and PSU size (P); and column 3 adds a set of dummy variables for the "ideal number of children" to proxy for other differences in the demand for children (K). Source: 1970 National Fertility Study.

Table III.3 Changes in Children's Household Income for Cohorts born after Family Planning Programs Began

	(1)	(2)	(3)	(4)
	Dependent Variable: Household Income *Pre-Treatment Mean in 1980[a]: $68,417*			
A. Difference-in-differences estimates [b]				
Pooled event years 1 - 6	570.4	767.2	716.4	851.6
	[343.7]	[408.3]	[410.9]	[451.7]
R-squared	0.34	0.41	0.41	0.41
B. Event-study estimates [c]				
-2	224.5	136.9	175.1	189.0
	[484.5]	[549.7]	[547.0]	[583.4]
-1	-121.6	-269.0	-266.8	-315.4
	[401.0]	[417.5]	[415.7]	[434.9]
0 (omitted)				
1	414.6	495.3	454.1	516.5
	[352.8]	[395.3]	[400.6]	[406.7]
2	1263.0	1422.0	1353.0	1615.8
	[588.0]	[641.9]	[648.7]	[685.2]
3	779.4	927.5	833.8	983.0
	[469.0]	[494.3]	[506.4]	[511.5]
4	1095.0	1086.0	979.8	1144.0
	[546.6]	[587.4]	[618.9]	[620.5]
5	1479.0	1456.0	1321.0	1576.4
	[588.1]	[640.1]	[671.8]	[685.0]
6	2193.0	2076.0	1935.0	2352.5
	[714.4]	[707.8]	[743.7]	[769.7]
R-squared	0.339	0.405	0.407	0.405
Model [d]	1	2	3	2M
Covariates [e]	C, Y	C, Y, S-Y	C, S-Y, R, A	C, Y, S-Y, adjusted using eqn. 5
County-year cells	11313	11313	11313	11313
Counties	666	666	666	666

The table presents point estimates of the change in household income of children for cohorts born before and after family planning programs began (event year 0). Heteroskedasticity-robust standard errors clustered by county are presented in brackets.

[a] Pre-treatment mean in 1980 is calculated as the mean of the dependent variable in event years t=0, t=–1 and t=–2 in 2013 dollars.

[b] Coefficients are least-squares estimates of τ in equation (III.4) using the 1980 restricted-use census data.

[c] Our baseline model is model 2. Event study estimates from models 2 and 2M are plotted in figure III.3.

[d] Covariate abbreviations are as follows: C and Y denote county and year fixed effects. S-Y denotes state-by-year fixed effects. X, R and A indicate county-level covariates interacted with linear time trends, REIS variables and abortion access measures (see text). Model 2M applies equation (III.5) to the estimates.

Source: 1980 restricted-use census data.

Table III.4 Percent of Children in or Near Poverty among Cohorts Born after Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variables	Percent of Children below			Percent White Children below			Percent Non-white Children below		
	100% Poverty	150% Poverty	200% Poverty	100% Poverty	150% Poverty	200% Poverty	100% Poverty	150% Poverty	200% Poverty
Pre-treatment Mean [a]	*17.4*	*28.5*	*40.4*	*15.8*	*30.8*	*48.1*	*44.0*	*63.4*	*76.7*
A. Difference-in-differences estimates [b]									
Event years 1 to 6	-0.846	-0.872	-0.701	-0.284	-0.439	-0.251	-1.994	-1.307	-0.986
	[0.258]	[0.323]	[0.346]	[0.285]	[0.369]	[0.407]	[0.809]	[0.860]	[0.905]
R-squared	0.09	0.13	0.19	0.08	0.11	0.16	0.11	0.11	0.13
B. Event-study estimates [c]									
-2	0.737	0.175	0.139	0.540	0.034	-0.025	0.946	0.020	0.020
	[0.434]	[0.389]	[0.410]	[0.485]	[0.435]	[0.521]	[1.093]	[1.083]	[1.083]
-1	0.459	0.067	0.381	0.545	0.219	0.679	0.779	-0.305	-0.305
	[0.366]	[0.391]	[0.406]	[0.406]	[0.476]	[0.513]	[1.208]	[1.264]	[1.264]
0 omitted									
1	-0.522	-0.786	-0.523	0.177	-0.236	-0.070	-0.993	-1.325	-1.325
	[0.340]	[0.389]	[0.437]	[0.360]	[0.506]	[0.567]	[1.194]	[1.058]	[1.058]
2	-0.903	-1.182	-0.918	-0.391	-0.826	-0.256	-2.148	-0.795	-0.795
	[0.340]	[0.428]	[0.432]	[0.334]	[0.488]	[0.522]	[1.288]	[1.164]	[1.164]
3	-0.961	-1.003	-0.651	-0.382	-0.399	0.088	-2.704	-2.133	-2.133
	[0.452]	[0.494]	[0.558]	[0.453]	[0.543]	[0.634]	[1.168]	[1.236]	[1.236]
4	-0.773	-1.460	-0.755	0.015	-0.721	0.071	-3.007	-2.585	-2.585
	[0.442]	[0.538]	[0.603]	[0.455]	[0.624]	[0.733]	[1.385]	[1.549]	[1.549]
5	-1.401	-1.979	-1.860	-0.819	-1.468	-0.950	-1.582	-1.272	-1.272
	[0.524]	[0.622]	[0.668]	[0.544]	[0.679]	[0.735]	[1.566]	[1.633]	[1.633]
6	-1.353	-1.672	-1.124	-0.227	-0.855	-0.288	-2.195	-0.694	-0.694
	[0.576]	[0.708]	[0.732]	[0.603]	[0.736]	[0.798]	[1.664]	[1.810]	[1.810]
R-squared	0.092	0.130	0.191	0.080	0.110	0.163	0.111	0.114	0.114

See table III.3 notes. In columns 1-6, the number of observations and counties are identical to table III.3. For columns 7-9, only 529 counties have sufficient numbers of nonwhites for inclusion. These estimates are based on 8855 county-year observations.

Table III.5 Percent of Children in Households Receiving Public Assistance or Headed by Single Parents for Cohorts Born after Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables	Percent Children in Households Receiving Public Assistance			Percent Children in Single Parent Households		
Sample	All	White	Non-white	All	White	Non-white
Pre-treatment mean [a]	*11.7*	*7.9*	*25.7*	*17.4*	*7.94*	*25.7*
A. Difference-in-differences estimates [b]						
Event years 1 to 6	-0.643	-0.446	-0.426	-0.290	-0.044	0.108
	[0.212]	[0.189]	[0.740]	[0.269]	[0.252]	[0.935]
R-squared	0.10	0.08	0.12	0.10	0.12	0.11
B. Event-study estimates [c]						
-2	-0.052	-0.046	-0.048	-0.438	-0.586	1.322
	[0.287]	[0.283]	[1.020]	[0.340]	[0.389]	[1.198]
-1	-0.180	-0.016	0.343	0.363	-0.109	1.588
	[0.269]	[0.241]	[1.218]	[0.358]	[0.385]	[1.261]
0 omitted						
1	-0.638	-0.455	0.496	-0.102	-0.092	1.763
	[0.273]	[0.241]	[1.008]	[0.295]	[0.275]	[1.310]
2	-0.862	-0.554	-0.648	-0.355	-0.282	0.457
	[0.286]	[0.264]	[1.005]	[0.315]	[0.345]	[1.338]
3	-1.206	-0.738	-1.731	-0.438	-0.009	-1.300
	[0.337]	[0.309]	[1.077]	[0.348]	[0.376]	[1.405]
4	-1.180	-0.687	0.274	-0.785	-0.492	-0.270
	[0.441]	[0.343]	[1.384]	[0.466]	[0.449]	[1.546]
5	-1.338	-0.805	-0.344	-0.279	-0.172	1.696
	[0.453]	[0.407]	[1.503]	[0.494]	[0.513]	[1.724]
6	-1.467	-1.087	0.776	-0.483	-0.143	0.753
	[0.477]	[0.473]	[1.666]	[0.534]	[0.563]	[1.841]
R-squared	0.102	0.084	0.122	0.095	0.123	0.115

See table III.3 and III.4 notes and the text for variable definitions.

Table III.6 Mother's Age at Birth and Number of Older Siblings among Cohorts Born after Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables	Mother's Age at Birth			Mean Number of Older Siblings		
Sample	All	White	Non-white	All	White	Non-white
Pre-treatment mean [a]	*25.5*	*25.4*	*25.3*	*1.8*	*1.6*	*2.4*
A. Difference-in-differences estimates [b]						
Event years 1 to 6	-0.066	-0.041	0.056	-0.021	0.001	-0.003
	[0.045]	[0.049]	[0.144]	[0.014]	[0.014]	[0.045]
R-squared	0.20	0.14	0.16	0.54	0.45	0.32
B. Event-study estimates [c]						
-2	0.045	0.083	0.206	-0.019	-0.015	0.049
	[0.0602]	[0.0684]	[0.181]	[0.0234]	[0.0233]	[0.0635]
-1	0.078	0.070	0.526	0.015	0.009	0.088
	[0.0615]	[0.0678]	[0.177]	[0.0215]	[0.0208]	[0.0580]
0 omitted						
1	-0.003	0.010	0.335	-0.015	0.005	0.042
	[0.0644]	[0.0676]	[0.202]	[0.0197]	[0.0205]	[0.0560]
2	-0.114	-0.047	0.000	-0.030	-0.001	-0.015
	[0.0645]	[0.0646]	[0.191]	[0.0189]	[0.0181]	[0.0480]
3	-0.182	-0.132	-0.099	-0.039	-0.008	-0.038
	[0.0671]	[0.0750]	[0.221]	[0.0203]	[0.0184]	[0.0559]
4	-0.193	-0.140	0.010	-0.062	-0.012	-0.016
	[0.0820]	[0.0825]	[0.263]	[0.0267]	[0.0236]	[0.0783]
5	-0.303	-0.220	-0.252	-0.070	-0.015	-0.126
	[0.0977]	[0.0949]	[0.285]	[0.0306]	[0.0267]	[0.0775]
6	-0.276	-0.173	-0.497	-0.065	-0.011	-0.120
	[0.101]	[0.101]	[0.285]	[0.0336]	[0.0277]	[0.0834]
R-squared	0.203	0.140	0.159	0.541	0.455	0.326

See table III.3 and III.4 notes and the text for variable definitions.

APPENDIX A

Expected Effects of Maternity Benefits on Childbearing

Proof of Proposition 1

I derive the effect of family benefits on the number of children by maximizing utility, $U(n,q,z)$, subject to the lifetime budget constraint, $\pi qn + [t(w_f - a) - b]n + \pi_z z = T(w_f + w_m)$. The budget constraint is nonlinear in *n* and *q*, which leads to the ambiguous effect of the benefits on the number of children discussed in the paper. This problem is associated with the following first order conditions: $\frac{U_n}{U_q} = \frac{\pi q + t(w_f - a) - b}{n\pi}$ (i), $\frac{U_q}{U_z} = \frac{\pi n}{1}$ (ii), $\frac{U_n}{U_z} = \frac{\pi q + t(w_f - a) - b}{1}$ (iii). Notably, the shadow price of quantity, p_n, depends on quality, q, while the shadow price of quality, p_q, depends on quantity, n.

I take the total derivative of the budget constraint and the first order conditions. I impose standard assumptions on the utility function $U_n > 0, U_q > 0, U_z > 0, U_{nn} < 0, U_{qq} < 0, U_{zz} < 0$, and I assume that the utility function is additively separable such that $U_{nz} = U_{zn} = U_{nq} = U_{qn} = U_{qz} = U_{zq} = 0$. The consumption good, z, serves as the numéraire and its price is set to one. The total derivatives of the first order condition (i) with respect to a is $\pi n\left(\frac{dq}{da}\right) + \pi q\left(\frac{dn}{da}\right) + \left(\frac{dn}{da}\right)\left(t(w_f - a) - b\right) - nt + \left(\frac{dz}{da}\right) = 0$, of condition (ii) is $U_{nn}\pi n\left(\frac{dn}{da}\right) + U_n\pi\left(\frac{dn}{da}\right) = U_{qq}\left(\frac{dq}{da}\right)\left(\pi q + t(w_f - a) - b\right) + U_q\left(\pi\left(\frac{dq}{da}\right) - t\right)$, and of condition (iii) is $U_{qq}\left(\frac{dq}{da}\right) = U_{zz}\left(\frac{dz}{da}\right)\pi n + U_z\pi\left(\frac{dn}{da}\right)$, $U_{nn}\left(\frac{dn}{da}\right) = U_{zz}\left(\frac{dz}{da}\right)\left(\pi q + t(w_f - a) - b\right) + U_z\left(\pi\left(\frac{dq}{da}\right) - t\right)$.

After some algebraic manipulation of the total derivatives, I derive the following effect of parental leave on childbearing,

$$\frac{dn}{da} = \frac{\pi n^2 t U_{ZZ}\left(U_{qq}\left(\pi q + t\left(w_f - a\right) - b\right) + U_q \pi\right) - U_q t\left(U_{qq} + \pi n U_{zz}\right)}{\left(U_{nn}\pi n + U_n \pi\right)\left(U_{qq} + \pi n U_{zz}\right) - \left(U_{zz}\left(-\pi^2 qn - \left(t\left(w_f - a\right) - b\right)n\pi + U_z \pi\right)\left(U_{zz}\left(\pi q + t\left(w_f - a\right) - b\right) + U_q \pi\right)}$$

whose sign is ambiguous. I decompose the above formula,

$$\frac{dln(n)}{dln(a)} = \frac{atn}{I}\left(\underbrace{\frac{dln(n)}{dln(I)}|_{\pi s\ constant}}_{Income\ Elasticity}\right) + \underbrace{\frac{dln(n)}{dln(a)}|_{I\ constant}}_{Substitution\ Elasticity}$$

where the elasticity of childbearing with respect to paid leave is the sum of income and substitution elasticities. This general formula does not make clear what generates the ambiguity of the sign of the overall effect.

To obtain more intuitive expressions for the income and substitution elasticities, I solve the maximization problem by replacing the non-linear budget constraint with a linear one by making it a function of shadow prices: $p_n n + p_q q + p_z z = I - \pi qn = R$. The shadow price of n is $p_n = \pi q + t\left(w_f - a\right) - b$, the shadow price of q is $p_q = \pi$ and of z is $p_z = \pi_z$, which means that: $dln(p_n) = \left(\frac{1}{k_n}\right)[\left(dln(\pi) + dln(q)\right)k + dln(t)\left(\frac{(w_f - a)tn}{R}\right) + dln\left(w_f\right)\frac{tnw_f}{R} - dln(a)\frac{ant}{R} - dln(b)\frac{bn}{R}]$ and $dln\left(p_q\right) = (\frac{k}{k_q})(dln(\pi) + dln(n))$. In these equations, $k = \frac{nq\pi}{R}; k_n = \frac{p_n n}{R}; k_q = \frac{p_q q}{R}$.

To derive the income and substitution elasticities I use the following propositions:

$$dln(n) = \eta_n dln\left(\frac{R}{p}\right) + k_z \sigma_{nz} dln(p_z) - (1 - k_n)\overline{\sigma_n} dln(p_n) + k_t \sigma_{nt} dln(p_t)$$

$$dln(t) = \eta_t dln\left(\frac{R}{p}\right) + k_z \sigma_{tz} dln(p_z) + k_n \sigma_{nt} dln(p_n) - (1 - k_t)\overline{\sigma}_t dln(p_t)$$

where the σ's are the Allen partial elasticities of substitution in the utility function, and $\overline{\sigma_n}$ is the average elasticity of substitution of n against z and q, and $\overline{\sigma}_t$ is the same elasticity for t against z and n. The average elasticities are always positive. Also define: $p = k_z dln(p_z) + k_n dln(p_n) + k_t dln(p_t)$.

To derive the income elasticity, let income, I, change while holding the π's constant, which results in: $dln(p_n) = \left(\frac{k}{k_n}\right) dln(q)$, and $dln(p_q) = (\frac{k}{k_q}) dln(n)$, and $dln\left(\frac{R}{p}\right) = (1-k)dln(I)$. Plugging these into the above propositions, I obtain the following expression:

$$\frac{D}{1-k}\underbrace{\left(\frac{dln(n)}{dln(I)}\right)|_{\pi's\ constant}}_{Income\ Elasticity} = (1 - k\sigma_{nq})\eta_n - \frac{k(1-k_n)\overline{\sigma_n}}{k_n}\eta_q$$

where $D = \left(1 - k\sigma_{nq}\right)^2 - \frac{k^2(1-k_n)(1-k_q)\overline{\sigma_n}\,\overline{\sigma_q}}{k_n k_q}$ and $(1 - k\sigma_{nq})$ must be positive by the second order conditions. In the equation, η_q is the true elasticity of quality with respect to income, where it uses a measure of income that is calculated using shadow prices (marginal costs) whose ratios in equilibrium are equal to the marginal rates of substitution in the utility function, while η_n is the true elasticity of quantity with respect to income. Thus, if $\eta_q > \eta_n$ by a sufficiently large enough magnitude, then the income elasticity could be negative. The sufficient condition for the income effect to be positive is if: $(1 - k\sigma_{nq})\eta_n > \frac{k(1-k_n)\overline{\sigma_n}}{k_n}\eta_q$.

In a similar manner I also derive the elasticity of substitution:

$$D\frac{dln(n)}{dln(a)}|_{I\ const} = \frac{(1-k_n)\overline{\sigma_n}ant}{k_n R}$$

and the sign is unambiguously positive.

<u>Proof of Corollary 1</u>

Following the same procedure as in the proof of proposition 1, I derive the following effect of the cash transfer, b, on childbearing:

$$\frac{dn}{db} = \frac{\pi n^2 U_{ZZ}\left(U_{qq}\left(\pi q + t\left(w_f - a\right) - b\right) + U_q \pi\right) - U_q\left(U_{qq} + \pi n U_{zz}\right)}{\left(U_{nn}\pi n + U_n \pi\right)\left(U_{qq} + \pi n U_{zz}\right) - \left(U_{zz}\left(-\pi^2 q n - \left(t\left(w_f - a\right) - b\right) n\pi + U_z \pi\right)\left(U_{zz}\left(\pi q + t\left(w_f - a\right) - b\right) + U_q \pi\right)\right.}$$

where the sign of the expression is ambiguous. The elasticity of childbearing with respect to cash transfers is:

$$\frac{dln(n)}{dln(b)} = \frac{bn}{I}\left(\underbrace{\frac{dln(n)}{dln(I)}|_{\pi s\ constant}}_{Income\ Effect}\right) + \underbrace{\frac{dln(n)}{dln(b)}|_{I\ constant}}_{Substitution\ Effect}$$

where it is the sum of income and substitution elasticities. This general formula does not make clear what generates the ambiguity of the sign of the overall effect.

To obtain more intuitive expressions for the income and substitution elasticities, I follow the procedure outlined in proposition 1. The expression for the income elasticity is the same as in proposition 1, whose sign is positive under the same conditions. The elasticity of substitution can be expressed as: $\frac{dln(n)}{dln(b)} = \frac{(1-k_n)\overline{\sigma_n} bn}{k_n R}$, and is always positive.

<u>Proof of Proposition 2</u>

I derive the effect of family benefits on the number of children by maximizing, $U(n,t,z)$, subject to the lifetime budget constraint, $\left[t\left(w_f - a\right) - b\right]n + \pi_z z = T(w_f + w_m)$. The budget constraint is nonlinear in *n* and *t*, which leads to the ambiguous effect of the benefits on the number of children discussed in the paper. This problem is associated with the following first order conditions: $\frac{U_N}{U_Z} = \frac{t(w_f - a) - b}{1} = \frac{p_n}{p_z}$, $\frac{U_n}{U_t} = \frac{t(w_f - a) - b}{(w_f - a)n} = \frac{p_n}{p_t}$, $\frac{U_t}{U_z} = \left(w_f - a\right)n = \frac{p_t}{p_z}$. Note that the shadow price of quantity, p_n, depends on time off from work, *t*, while the shadow price of time off from work, p_t, depends on quantity, *n*.

I take the total derivative of the budget constraint and the first order conditions. I impose standard assumptions on the utility function $U_n > 0, U_t > 0, U_z > 0, U_{nn} < 0, U_{tt} < 0, U_{zz} < 0$, and I assume that the utility function is additively separable such that $U_{nz} = U_{zn} = U_{nt} = U_{tn} = U_{tz} = U_{zt} = 0$. The consumption good z serves as the numeraire and its price is set to one. After substituting the total derivative formulas into each other I obtain the following expression,

$$\frac{dn}{da} = \frac{(-U_{zz}n(w_f - a)\pi_n + U_z(w_f - a))n(U_{zz}nt(w_f - a) - U_z) + (t(U_{zz}\pi_n n - U_z))(U_{tt} + U_{zz}n^2(w_f - a)^2)}{(U_{nn} + U_{zz}\pi_n^2)\left(U_{tt} + U_{zz}n^2(w_f - a)^2\right) - (-U_{zz}n(w_f - a)\pi_n + U_z(w_f - a))(-U_{zz}\pi_n(w_f - a)n + U_z(w_f - a)}$$

where $\pi_n = t(w_f - a) - b$. The sign of this effect is ambiguous. I derive the elasticity of childbearing with respect to paid leave as:

$$\frac{dln(n)}{dln(a)} = \frac{ant}{I}\left(\underbrace{\frac{dln(n)}{dln(I)}|_{\pi's\ const}}_{Income}\right) + \underbrace{\frac{dln(n)}{dln(a)}|_{I_{const}}}_{Substitution}$$

where it is a combination of income and substitution elasticities. This general formula does not make clear what generates the ambiguity of the sign.

To derive more intuitive expressions of income and substitution effects, I linearize the budget constraint, such that it equals to: $p_n n + p_t t + p_z z = R = I + tn(w_f - a)$, where $p_n = t(w_f - a) - b$ and $p_t = (w_f - a)n$. Thus, $dln(p_t) = dln(w_f)\left(\frac{w_f nt}{R}\right)\left(\frac{1}{k_t}\right) + dln(a)\left(\frac{ant}{R}\right)\left(\frac{1}{k_t}\right) + dln(n)$, and $dln(p_n) = \left(\frac{1}{k_n}\right)[-dln(b)\left(\frac{bn}{R}\right) + dln(t)k_t + dln(w_f)\left(\frac{tw_f n}{R}\right) - dln(a)\left(\frac{ant}{R}\right)]$, where $k = \frac{tn(w_f - a)}{R}$, $k_n = \frac{p_n n}{n}$ and $k_t = \frac{p_t t}{R} = k$.

To calculate the income elasticity, hold prices constant which leads to: $dln(p_t) = dln(n)$; $dln(p_n) = dln(t)\left(\frac{k_t}{k_n}\right)$; $dln\left(\frac{R}{p}\right) = (1 - k)dln(I)$. This leads to the following expression:

$$\frac{dln(n)}{dln(I)}|_{\pi s\ const} = \frac{1}{1-k_t\sigma_{nt}}(\eta_n(1-k_t\sigma_{nt}) - \eta_t\frac{k_t^2(1-k_n)(1-k_t)\overline{\sigma_n}}{k_nk_t})$$

where the income elasticity is positive if, $\eta_n(1-k_t\sigma_{nt}) > \eta_t\frac{k_t^2(1-k_n)(1-k_t)\overline{\sigma_n}}{k_nk_t}$.

In a similar manner obtain the elasticity of substitution:

$$D\frac{dln(n)}{dln(a)}|_{I\ const} = \frac{k_a\big((1-k_n)\overline{\sigma_n} + \sigma_{nt}k_n\big)(1-\sigma_{nt}k_t) + k_a(\overline{\sigma_n} + \frac{(1-k_t)\overline{\sigma_q}}{k_t})}{(1-k_n)\overline{\sigma_n}k_tk_n}$$

where $D = (1-k_t\sigma_{nt})^2 - \frac{(1-k_t)\overline{\sigma_q}(1-k_n)\overline{\sigma_n}k_t^2}{k_nk_t}$ and $(1-k\sigma_{nt})$ are positive by the second order conditions. The sign of the substitution elasticity is positive if $\sigma_{nt} > 0$.

<u>Proof of Corollary 2</u>

Following the same procedure as in the proof of proposition 1, I derive the following effect of paid leave on childbearing:

$$\frac{dn}{db} = \frac{\big(-U_{zz}n(w_f-a)\pi_n + U_z(w_f-a)\big)\big(nU_{zz}n(w_f-a)\big) + (U_{zz}\pi_nn - U_z)(U_{tt} + U_{zz}n^2(w_f-a)^2)}{(U_{nn} + U_{zz}\pi_n^2)\left(U_{tt} + U_{zz}n^2(w_f-a)^2\right) - (-U_{zz}n(w_f-a)\pi_n + U_z(w_f-a))(-U_{zz}\pi_n(w_f-a)n + U_z(w_f-a))}$$

where the sign is ambiguous. The elasticity of childbearing with respect to cash transfers is:

$$\frac{dln(n)}{dln(b)} = \frac{bn}{I}\left(\underbrace{\frac{dln(n)}{dln(I)}|_{\pi's\ const}}_{Income}\right) + (\underbrace{\frac{dln(n)}{dln(a)}|_{I_{const}}}_{Substitution}),$$

which is a combination of income and substitution effects. The formula for the income effect as well as the condition for it to be positive are the same as in proposition 2. The substitution elasticity is: $D\left(\frac{b}{n}\right)\frac{dln(n)}{dln(b)}|_{I\ const} = \frac{(1-k_n)\overline{\sigma_n}bn}{k_nR}$ and is always positive.

APPENDIX B

Estimation of Fertility Rates Using Census Data

To estimate fertility rates, it is important to have information on the number of women of childbearing age. Information on the age structure of the population by region is only published in the decennial censuses. The 1989 census data is the closest to the year of the policy's start, compared to 2002 and 2010 census data, and should be the least affected by misclassification error due to mortality and mobility. Only individuals who have not died or moved out of the country appear in the census, thus the number of women of childbearing age calculated using 2002 census data will underestimate the true number of such women. The 1989 census data provide counts of men and women in one year age groups by region of residence as of January, 1989. I estimate birth year of the woman using her *age* in 1989 as 1989-*age*-1, because the census took place between January 12 and January 19 in 1989. This calculation will only understate the birth year of women born between January 1st and January 11th. I use these data to backward-estimate the number of women each year (from 1975 to 1989) who are of childbearing age – ages 15 to 44. For instance, the number of women who are age 15 to 44 in 1979 is the same as the number of women who are age 25 to 54 in 1989. For years 1990 to 1992, I use published statistics in Rosstat on the number of women by age in each oblast.

My main outcome of interest is the General Fertility Rate (GFR) which is the number of births per thousand women of childbearing age. I estimate GFR in year *y* and region *o* as the number of children born in year *y* (except those who were still born), in oblast *o* as recorded in Rosstat data per thousand women aged from 15 to 44 in year *y*, and living in oblast *o* in 1989 as recorded in the 1989 Census.

$$(a)\ GFR_{y,o}^{1989} = \frac{Number\ of\ Births_{y,o} * 1000}{Number\ of\ Women\ Age\ 15\ to\ 44_{y,o}\ from\ 1989\ Census}$$

I estimate the GFR in month *m*, year *y* and region *o* as the number of children born in month *m*, year *y* and oblast *o* and present in Russia in the 2002 Census per thousand women aged from 15 to 44 in year *y*, and living in oblast *o* in 1989 as recorded in the 1989 Census.

$$(b)\ GFR_{m,y,o}^{2002} = \frac{Number\ of\ Births_{m,y,o}\ from\ 2002\ Census * 1000}{Number\ of\ Women\ Age\ 15\ to\ 44_{y,o}\ from\ 1989\ Census}$$

In my analysis comparing the effect of the program on first and higher parity births I will construct fertility rates (FR) by parity. I estimate fertility rates for first births in month, *m*, year, *y*, and oblast, *o*, as the number mothers born in oblast *o* and present in Russia during the 2010 Census who report that their first child was born in month, *m*, and year, *y*, per thousand women aged from 15 to 44 in year, *y*, and living in oblast, *o*, in 1989 as recorded in the 1989 Census.

$$(c)\ FR(1^{st}\ Birth)_{m,y,o}^{2010} = \frac{Number\ of\ First\ Births_{m,y,o}\ from\ 2010\ Census * 1000}{Number\ of\ Women\ Age\ 15\ to\ 44_{y,o}\ from\ 1989\ Census}$$

Second, I estimate fertility rates for all higher parity births in month, *m*, year, *y*, and oblast, *o*, as the total number of births in month, *m*, year, *y*, and oblast, *o*, minus the number of first births estimate used in (c) per thousand women aged from 15 to 44 in year *y*, and living in oblast *o* in 1989 as recorded in the 1989 Census.

$(d)\ FR(Higher\ Parity\ Birth)_{m,y,o}^{2010}$

$$= \frac{(Number\ of\ Births - Number\ of\ First\ Births)_{m,y,o}\ from\ 2010\ Census * 1000}{Number\ of\ Women\ Age\ 15\ to\ 44_{y,o}\ from\ 1989\ Census}$$

APPENDIX C

Data Description

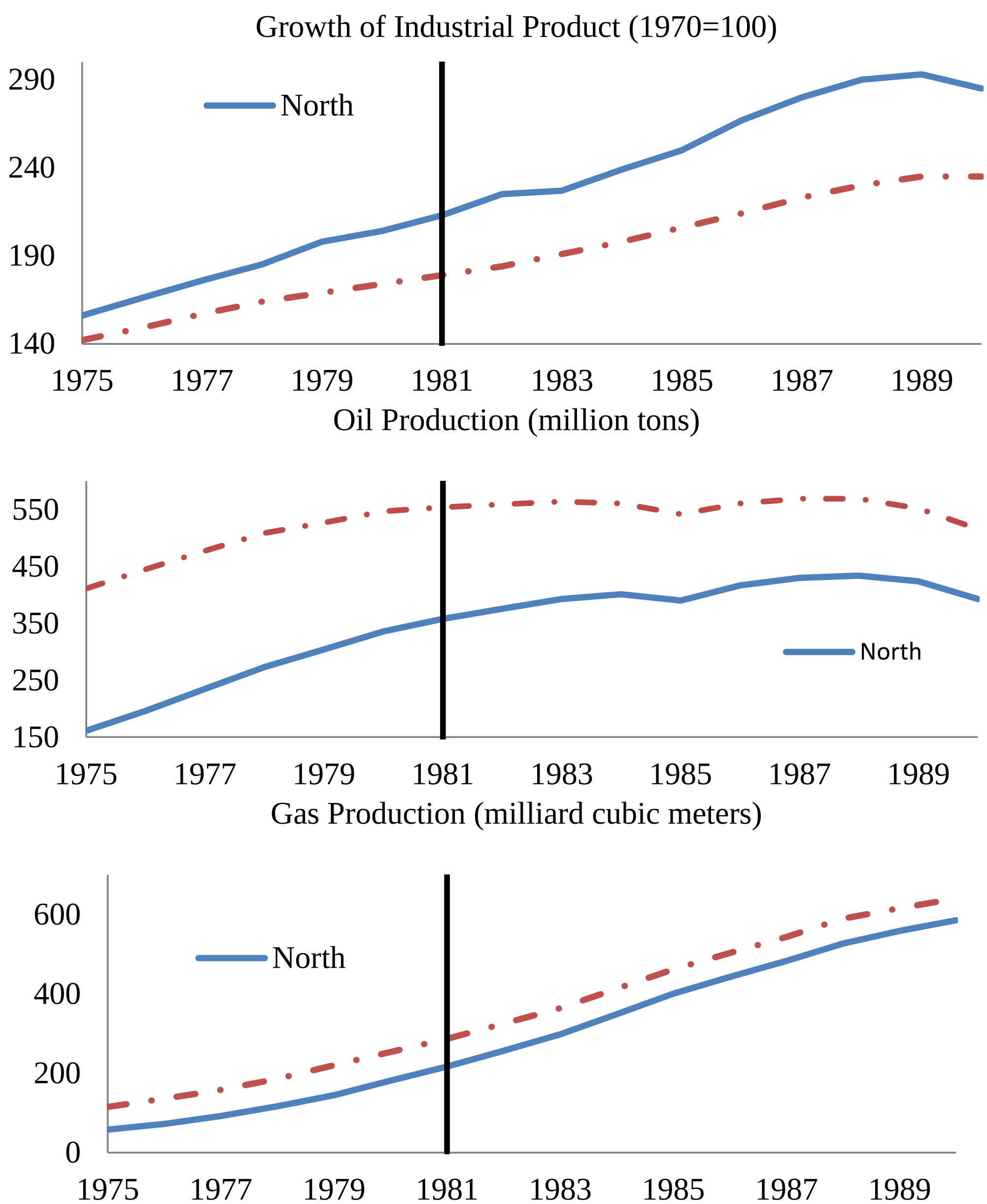
Table C.1 Description of Structure of Data Used in Analysis

Data Source	Type	Description
1989 Russian Census	count data	counts of persons living in Russia in 1989 by age, sex, and oblast of residence
2002 Russian Census	count data	counts of persons living in Russia in 2002 by birth year, birth month and oblast at birth
2010 Russian Census	count data	counts of persons living in Russia in 2010 by birth year, birth month and oblast at birth; counts of women present in Russia in 2010 by birth year and birth month of their first child, and by oblast at birth
1979 Russian Census	count data	counts of persons living in Russia in January, 1979 by age, sex, oblast of residence, and by urban/rural status of the oblast; counts of persons living in Russia in January, 1979 who are older than 10 by education categories (only elementary, incomplete high school, complete high school, incomplete college, complete college)
2004 Generations and Gender Survey	micro-level data	sample of roughly 11,000 individuals aged 18 to 79 which is representative of Russia, where this analysis uses data on the birth year of the mother, and the birth year of her every child
1975-1992 "Narodnoe Hozyaystvo" Yearbooks	counts and averages data	characteristics of each oblast and in each year, where this analysis uses the production of bricks, concrete, timber, canned goods, meat, and the value of trade

APPENDIX D

Evolution of Economic Indicators across Early and Late Beneficiaries

Figure D.1 Evolution of Economic Indicators across Early and Late Beneficiaries



Notes: The North is composed of 16 early beneficiary oblasts: Jakutsk ASSR, Murmansk, Kamchatsk, Magadan, Sahalin, Komi and Burjat ASSR, Krasnojarsk kraj, Primorsk, Habarovsk, Archangelsk, Tomsk, Tyumen, Irkutsk, Chitinsk, Amur oblasts. Sources: 1975-1990 “Narodnoe Hozyaystvo” yearbooks.

APPENDIX E

Creation of Childbearing Elasticities with Respect to Cost of a Child across Studies

I use a parametric bootstrap procedure to generate confidence intervals for the elasticity estimates in all studies (Johnston and DiNardo 1997). I generate 10,000 bootstrap draws of the reduced-form coefficients from normal distributions with means and standard errors equal to the point estimates reported in the paper. I calculate the percent change in childbearing, $dln(n)$, by dividing my bootstrap draws by the appropriate pre-treatment mean. I obtain an estimate of the cost of having a child until the child is 18 years old for a country and period relevant to the study, and generate 10,000 draws of average costs from normal distributions with means equal to the average cost estimate and standard errors equal to 10 percent of the cost for Russia, and 5 percent of the cost for all other studies. I calculate the percent change in costs, $dln(c)$, by dividing the change in monetary cost of a child induced by a policy by the bootstrap draws of average costs. Finally, I obtain 10,000 realizations of elasticities as: $\frac{dln(n)}{dln(c)}$. The values of the 2.5th and the 97.5th percentiles of the distribution of my generated elasticities constitute the 95-percent confidence interval for my estimated elasticity.

<u>Russia</u>

The percent change in the cost of a child is calculated as follows. The per-month cost estimate of a child is 90 rubles, which is provided by a Soviet demographer (Valentei 1987). I calculate the cost of a child over 18 years, which equals to 90*12*18. The cost after the program subtracts parental leave payments (assuming full take-up), 35*10, and a cash transfer at the birth of a child, 75 (this is the average of the payment of 50 for a first birth and a payment of 100 for a second and third birth). Thus, the cost changed from 90*12*18 to 90*12*18-35*10-75, which represents a 2.2 percent decrease.

Austria

Guger (2003) uses the Austrian Consumer Survey from 2001 to estimate the monthly cost of a child in Austria as 500 euros, which is 389 euros in 1990 euros. The cost of raising a child before was: 388*12*18, while after the expansion of paid leave the cost became: 388*12*18-340*12 (the monthly paid leave was a fixed amount of 340 euros; I am assuming full-take up in this calculation). The cost of raising a child decreased by 4.8 percent. Childbearing went up by 21 percent (the coefficient scaled by the pre-treatment mean is, 0.068/0.32, and the standard error of the coefficient equals to 0.12; these estimates come from table I.7 in Lalive and Zweimüller (2009)). Thus, the elasticity equals to: -4.4.

Spain

The average total cost of a child is estimated to be 150,000 euros. This estimate comes from surveys conducted by the consumer organization CEACCU in 42 Spanish provinces (as of 2008). The cost of a child after the benefit became: 150,000-2,500, where parents received a cash transfer of 2,500 euros at the birth of a child, which is a reduction in the cost of raising a child of 1.7 percent. The effect of the program on fertility rates is estimated at 0.063 percent (estimate in table A1 (Gonzalez 2012), with a standard error of 0.0115). This results in an elasticity of -3.8.

Canada

I calculate the change in cost of a child by taking a weighted average of changes in costs for first, second and third and higher parity births based on numbers reported in the paper. The benefit was 500 dollars for the first birth, 1000 dollars for second births, and 8000 dollars for third and higher parity births. The annual costs of a child as reported in the paper were: 7,935, 6,348, and 5,324 for first, second, and third births respectively. I weight these costs of a child by the share of births in each category. The resulting change in costs equals to: (500/(7935*18))*0.45+(1000/(6348*18))*0.35+(8000/(5324*18))*0.2. Thus, costs decreased by 2.1 percent. The fertility rate increased by 0.087 percent (table 6 (Milligan 2005); model c). Thus, the elasticity equals to: -4.1.

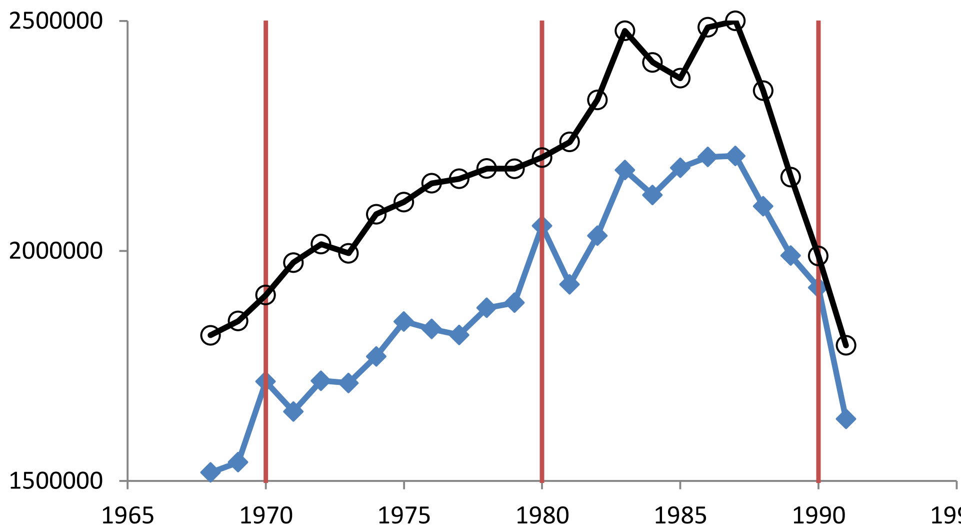
Israel

The elasticity, 0.54, and the standard error, 0.077, presented in this paper were derived in Cohen et al. (2013).

APPENDIX F

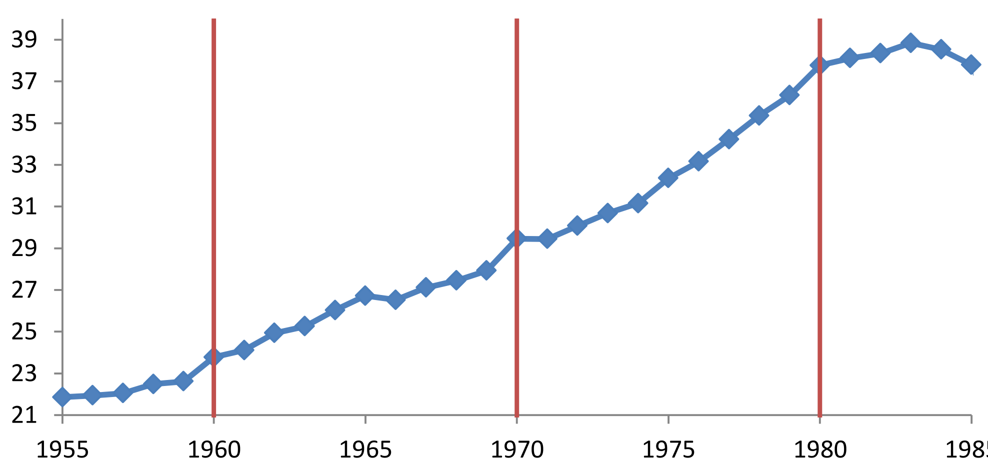
Comparison of Census and Vital Statistics Data

Figure F.1   Number of Births from 2010 Census and Official Data



Notes: The blue line with filled diamonds are the number of births in each birth year according to the number of people present in the 2010 Census. The black line with empty circles is the number of births in each birth year from official data.

Figure F.2   Percent who Have Completed College by Birth Year Using the 2010 Census



Notes: The graph represents the share of individuals in each birth year who have completed college. Source: 2010 Census.

APPENDIX G

Additional Results on the Effect of Family Planning Programs

Table G.1 Characteristics of Counties Receiving Federal Family Planning Programs from 1965 to 1973

	(1) All Counties	(2) Not Funded	(3) First Funded in years 1965-7	1968-9	1970-3	(4) All Funded	(5) Col 4-Col 2 (std. error)
Number of counties	3073	2407	124	337	205	666	
Mean population	55,385	30,735	239,319	138,895	96,271	144,473	113,738
Percent of 1960 population in counties							(12,721)
in Northeast	21.7	16.4	31.5	16.7	38.5	25.7	9.4
							(4.91)
in Midwest	30.3	38.2	20.8	27.1	22.9	24.3	-13.9
							(6.18)
in South	31.8	37.7	29.5	25.2	29.1	27.3	-10.4
							(4.84)
in West	16.0	7.4	18.2	30.8	9.5	22.6	15.2
							(5.62)
in urban areas	68.4	53.7	85.7	79.5	71.6	79.8	26.1
							(4.13)
in rural/farm areas	7.9	13.3	2.2	4.2	5.1	3.7	-9.5
							(1.19)
Percent of 1960 county residents							
Under 5 years of age	11.4	11.3	11.4	11.6	11.5	11.5	0.22
							(0.12)
65 or older	9.2	9.8	8.9	8.6	9.0	8.8	-0.97
							(0.20)
Nonwhite	11.0	11.4	13.0	10.6	7.9	10.8	-0.55
							(1.04)
with 12 years of education	42.7	41.0	43.2	44.9	42.8	43.9	2.9
							(3.83)
with fewer than 4 years of education	8.4	9.6	7.9	7.1	7.9	7.5	-2.1
							(0.40)
of households with income under $3,000	22.0	27.0	17.4	18.1	19.0	18.1	-8.9
							(1.43)
of households with income over $10,000	14.7	11.7	17.4	17.6	15.3	17.1	5.3
							(1.25)

Table G.2 Correlates of the Timing of Federal Family Planning Program Establishment

	(1)	(2)	(3)	(4)
	Dependent Variable: *Year of first federal family planning grant*			
1(25 to 49 percent of population in urban areas)	0.55	0.69	0.74	0.86
	[0.29]	[0.32]	[0.43]	[0.46]
1(50 to 74 percent of population in urban areas)	0.78	0.89	1.1	0.81
	[0.53]	[0.59]	[0.76]	[0.76]
1(75 to 100 percent of population in urban areas)	0.48	0.57	1.28	0.65
	[0.70]	[0.77]	[1.00]	[1.03]
Proportion of residents				
in urban areas	-0.02	-0.02	-0.04	-0.03
	[0.01]	[0.01]	[0.02]	[0.01]
in rural or farm areas	0.01	-0.00	0.00	0.00
	[0.01]	[0.01]	[0.02]	[0.02]
under 5 years of age	0.06	-0.03	-0.03	-0.07
	[0.09]	[0.07]	[0.13]	[0.16]
65 or older	-0.03	-0.07	-0.07	-0.16
	[0.06]	[0.04]	[0.12]	[0.08]
Nonwhite	-0.01	-0.01	-0.02	-0.02
	[0.01]	[0.01]	[0.01]	[0.02]
with 12 years of education	-0.00	0.02	0.02	0.03
	[0.02]	[0.02]	[0.03]	[0.03]
with less than 4 years of education	-0.01	-0.00	0.02	0.01
	[0.02]	[0.02]	[0.08]	[0.06]
of households with income <$3,000	-0.02	-0.00	-0.02	0.01
	[0.01]	[0.01]	[0.05]	[0.04]
of households with income >$10,000	-0.01	-0.03	-0.00	0.01
	[0.03]	[0.03]	[0.04]	[0.04]
Weighted by population of women 15 to 44			X	X
State fixed effects		X		X
Observations	666	666	666	666
R-squared	0.07	0.26	0.13	0.38

Each column reports estimates from a separate linear regression. Heteroskedasticity-robust standard errors are corrected for correlation within state and presented in brackets beneath each estimate.

Table G.3 The Relationship between the Roll-Out of Federal Family Planning Programs and 1965 Determinants of Childbearing

	(1) Population Growth a Problem	(2) Ideal Number of Children	(3) Approve of Abortion	(4) Coital Frequency	(5) Ever Used the Pill	(6) When 1st Used Pill | Ever Used	(7) Surgically Sterilized	(8) Children Ever Born to Mother
Mean Dependent Variable	*0.80*	*3.3*	*0.39*	*6.04*	*0.22*	*772*	*0.198*	*5.1*
Year Family Planning	-0.005	0.010	-0.001	0.036	-0.004	0.198	-0.004	-0.054
Program Established	[0.007]	[0.022]	[0.005]	[0.071]	[0.010]	[0.384]	[0.008]	[0.066]
Observations	3,106	3,069	3,106	2,967	3,106	742	3,106	3,101
R-squared	0.021	0.038	0.023	0.136	0.154	0.022	0.095	0.075

	(9) Married Once	(10) Age at 1st Marriage	(11) Age at 1st Pregnancy	(12) Children Ever Born	(13) Husband's Income	(14) Catholic	(15) Highest Grade	(16) 2 Parents at age 14
Mean Dependent Variable	*0.87*	*20.8*	*22.3*	*2.7*	*7620*	*0.29*	*11.3*	*0.78*
Year Family Planning	0.006	0.054	0.063	0.017	50.6	0.023	0.036	0.004
Program Established	[0.005]	[0.059]	[0.066]	[0.031]	[157]	[0.016]	[0.104]	[0.006]
Observations	3,106	3,103	2,815	3,106	3,006	3,106	3,105	3,106
R-squared	0.040	0.111	0.160	0.141	0.170	0.061	0.092	0.016

Dependent variables are coded as follows by column: (1) Do you consider the growth of world population a serious problem? Yes=1, (2) What is the ideal number of children for average American family? (3) Index from three questions about whether the respondent approves of abortion if a woman is not married, for health concerns, or in the case of financial hardship. 1=approve in all three cases; (4) Coital frequency in the last four weeks? (5) Have you ever used the Pill? Yes=1, (6) When did you first use the Pill? (month and year, 772 = March 1964), (7) Have you or your husband had an operation making it impossible to have (another) child? 1=Yes; (8) How many children did your mother have? (9) Is this your first marriage? 1=Yes; (10-11) Age in months constructed from month and year of birth and month and year of first marriage and month and year of first pregnancy end date; (12) How many live births have you had? (13) Husband's income in nominal dollars. (14) Respondent identifies as "Roman Catholic." (15) Highest grade attained by the respondent. (16) Did you live with both parents at age 14? 1=Yes. Estimates are obtained from weighted regressions of the indicated dependent variable on the year the family planning program was established. To account for sampling design, the regressions control for size of sampled PSU, decade of respondent's birth, and race (1=Nonwhite). Source: 1965 National Fertility Study.

Table G.4.1 General Fertility Rate before and after Family Planning Began

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: General Fertility Rate				
A. 1970 Census: Mean DV			*96.830*		
-6	-0.629	0.436	0.434	-0.144	0.508
	[1.618]	[1.654]	[1.653]	[1.649]	[1.784]
-5	-0.302	0.520	0.510	-0.039	0.617
	[1.529]	[1.481]	[1.478]	[1.485]	[1.585]
-4	-1.164	-0.033	-0.052	-0.556	-0.066
	[1.328]	[1.345]	[1.333]	[1.325]	[1.438]
-3	-1.307	0.065	0.011	-0.423	0.089
	[1.188]	[1.150]	[1.135]	[1.109]	[1.218]
-2	0.067	1.156	1.077	0.805	1.585
	[1.075]	[1.117]	[1.105]	[1.101]	[1.203]
-1	-1.085	-0.815	-0.908	-1.008	-1.016
	[1.046]	[1.034]	[1.027]	[1.031]	[1.121]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.336	0.394	0.397	0.418	0.394
B. 1980 Census: Mean DV			*85.990*		
-2	0.528	0.677	0.496	0.184	0.817
	[0.918]	[0.960]	[0.957]	[0.954]	[0.934]
-1	-0.747	-1.080	-1.179	-1.335	-1.199
	[0.927]	[0.937]	[0.940]	[0.935]	[0.925]
	Event year zero omitted (Year family planning program began)				
1	-1.615	-1.689	-1.577	-1.445	-1.837
	[0.805]	[0.834]	[0.836]	[0.833]	[0.878]
2	-1.709	-2.268	-2.187	-1.878	-2.499
	[0.817]	[0.901]	[0.900]	[0.897]	[0.912]
3	-2.818	-3.187	-3.122	-2.702	-3.612
	[0.968]	[1.020]	[1.004]	[1.002]	[0.963]
4	-1.890	-2.237	-2.198	-1.663	-2.426
	[1.054]	[1.081]	[1.063]	[1.063]	[0.986]
5	-1.917	-2.799	-2.864	-2.213	-3.132
	[1.218]	[1.272]	[1.250]	[1.253]	[1.053]
6	-2.568	-3.058	-3.215	-2.473	-3.497
	[1.312]	[1.351]	[1.312]	[1.302]	[1.075]
Observations	11313	11313	11313	11313	11313
R-squared	0.223	0.300	0.304	0.313	0.300
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See table III.2 notes and figure III.2 notes in main paper. Point estimates of τ from our various specifications of equation (III.4). The dependent variable is the general fertility rate (GFR) calculated using the 1970 or 1980 censuses. The census estimates use the GFR implied by the county of residence in 1965/1975 and age of the child in the census from funded counties and do not weight

Table G.4.2 Average Household Income for Children Born Before Family Planning Programs Began

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent Variable: Household Income					
A. 1970 Census: Mean DV			*53809*			*53420*
-6	-427.6	-73	-223.3	-234	-141.6	-363.3
	[518.6]	[515.1]	[518.7]	[510.0]	[518.5]	[468.4]
-5	1375	1978	1828	1759	2434.3	544.6
	[1,113]	[1,374]	[1,355]	[1,379]	[1518.5]	[399.9]
-4	102.1	482.6	336.5	218.9	564.1	44.57
	[485.1]	[467.5]	[464.8]	[464.5]	[452.4]	[416.8]
-3	-388.8	-150.6	-284	-425.5	-234	-148.5
	[406.0]	[448.4]	[452.5]	[445.3]	[448.7]	[396.1]
-2	-167.8	100.5	-7	-121.9	110.7	-127.9
	[381.3]	[438.8]	[443.2]	[433.0]	[450.9]	[322.8]
-1	-258.9	-309	-400.8	-449.4	-414.8	-668.7
	[362.8]	[393.9]	[390.1]	[395.2]	[420.9]	[358.2]
	Event year zero omitted (Year family planning program began)					
Observations	7324	7324	7324	7324	7324	7324
R-squared	0.122	0.185	0.188	0.198	0.185	0.429
Model	1	2	3	4	2M	2T
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification	C, Y, S-Y, Trimmed sample
Counties	666	666	666	666	666	666

See notes in table III.3 and figure III.2 for estimates using the 1980 census.

Table G.4.3 Average Household Income per Capita for Children

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Household Income per Capita				
A. 1970 Census: Mean DV			*10977*		
-6	5.2	55.5	33.8	24.8	53.7
	[108.9]	[116.5]	[116.7]	[117.3]	[114.9]
-5	388.3	495.0	473.6	455.0	607.2
	[276.4]	[346.1]	[340.6]	[347.4]	[382.8]
-4	71.8	121.5	100.9	74.7	140.7
	[104.8]	[107.8]	[108.0]	[108.7]	[101.7]
-3	-38.8	-24.7	-43.6	-72.9	-42.9
	[93.44]	[107.3]	[109.0]	[108.0]	[106.8]
-2	-45.4	-20.2	-35.9	-58.7	-34.1
	[88.91]	[104.9]	[105.3]	[103.7]	[107.1]
-1	-27.7	-59.6	-72.3	-81.8	-81.3
	[81.02]	[86.39]	[85.79]	[86.18]	[90.7]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.007	0.065	0.066	0.072	0.065
B. 1980 Census: Mean DV			*13792*		
-2	-32.7	-60.1	-54.3	-19.7	-58.5
	[97.62]	[112.5]	[112.3]	[112.4]	[119.2]
-1	-32.7	-77.6	-77.9	-58.7	-88.2
	[84.83]	[92.67]	[92.54]	[92.24]	[96.8]
	Event year zero omitted (Year family planning program began)				
1	110.7	122.1	114.5	101.2	125.8
	[97.02]	[102.8]	[103.4]	[103.8]	[107.5]
2	291.9	326.2	313.0	285.4	363.9
	[115.2]	[126.8]	[127.8]	[126.9]	[133.5]
3	240.2	286.1	267.9	227.5	304.9
	[113.2]	[124.1]	[125.7]	[124.5]	[129.4]
4	343.9	367.5	345.8	295.9	394.5
	[129.5]	[141.8]	[144.5]	[142.2]	[149.5]
5	472.2	483.7	455.1	398.0	530.8
	[145.0]	[158.5]	[163.6]	[160.2]	[169.7]
6	563.8	589.6	558.5	494.8	659.4
	[175.4]	[185.9]	[190.5]	[185.2]	[202.9]
Observations	11313	11313	11313	11313	11313
R-squared	0.218	0.285	0.287	0.303	0.285
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.4 Share of Children Living Below 100 Percent of Poverty Line

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Below 100% Poverty Line				
A. 1970 Census: Mean DV			*21.730*		
-6	1.006	0.540	0.550	0.467	0.668
	[0.602]	[0.632]	[0.639]	[0.640]	[0.679]
-5	0.230	-0.223	-0.208	-0.250	-0.281
	[0.569]	[0.617]	[0.614]	[0.617]	[0.662]
-4	0.212	-0.224	-0.204	-0.210	-0.291
	[0.499]	[0.552]	[0.551]	[0.550]	[0.590]
-3	0.268	-0.006	0.027	0.048	-0.015
	[0.480]	[0.490]	[0.489]	[0.491]	[0.521]
-2	-0.094	-0.437	-0.398	-0.382	-0.585
	[0.449]	[0.471]	[0.471]	[0.473]	[0.508]
-1	0.851	0.775	0.822	0.815	1.039
	[0.434]	[0.474]	[0.472]	[0.475]	[0.520]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.017	0.105	0.107	0.130	0.105
B. 1980 Census: Mean DV			*18.750*		
-2	0.547	0.737	0.720	0.664	0.819
	[0.383]	[0.434]	[0.426]	[0.417]	[0.412]
-1	0.358	0.459	0.455	0.423	0.526
	[0.338]	[0.366]	[0.364]	[0.362]	[0.351]
	Event year zero omitted (Year family planning program began)				
1	-0.368	-0.522	-0.543	-0.520	-0.576
	[0.303]	[0.340]	[0.344]	[0.345]	[0.331]
2	-0.707	-0.903	-0.937	-0.895	-1.012
	[0.297]	[0.340]	[0.341]	[0.343]	[0.333]
3	-0.681	-0.961	-0.994	-0.934	-1.065
	[0.384]	[0.452]	[0.451]	[0.448]	[0.429]
4	-0.696	-0.773	-0.845	-0.771	-0.812
	[0.461]	[0.442]	[0.446]	[0.438]	[0.421]
5	-1.280	-1.401	-1.507	-1.426	-1.594
	[0.466]	[0.524]	[0.522]	[0.512]	[0.488]
6	-1.253	-1.353	-1.485	-1.396	-1.540
	[0.579]	[0.576]	[0.584]	[0.562]	[0.529]
Observations	11313	11313	11313	11313	11313
R-squared	0.021	0.092	0.095	0.100	0.092
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.5 Share of Children Living Below 150 Percent of Poverty Line

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Below 150% Poverty Line				
A. 1970 Census: Mean DV			*37.170*		
-6	0.942	0.780	0.822	0.783	0.946
	[0.561]	[0.564]	[0.569]	[0.592]	[0.603]
-5	0.084	-0.180	-0.136	-0.123	-0.252
	[0.535]	[0.553]	[0.548]	[0.557]	[0.588]
-4	0.319	0.181	0.227	0.283	0.202
	[0.519]	[0.566]	[0.565]	[0.579]	[0.607]
-3	0.130	0.065	0.117	0.197	0.050
	[0.519]	[0.534]	[0.528]	[0.536]	[0.574]
-2	0.111	-0.050	0.000	0.065	-0.095
	[0.484]	[0.507]	[0.506]	[0.507]	[0.550]
-1	0.627	0.712	0.762	0.785	0.929
	[0.459]	[0.474]	[0.474]	[0.477]	[0.516]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.017	0.094	0.097	0.113	0.094
B. 1980 Census: Mean DV			*30.950*		
-2	0.018	0.175	0.168	0.128	0.207
	[0.347]	[0.389]	[0.385]	[0.382]	[0.372]
-1	-0.068	0.067	0.065	0.038	0.103
	[0.369]	[0.391]	[0.390]	[0.388]	[0.375]
	Event year zero omitted (Year family planning program began)				
1	-0.815	-0.786	-0.804	-0.792	-0.853
	[0.357]	[0.389]	[0.391]	[0.393]	[0.378]
2	-1.191	-1.182	-1.209	-1.194	-1.309
	[0.404]	[0.428]	[0.433]	[0.432]	[0.415]
3	-0.881	-1.003	-1.031	-0.998	-1.062
	[0.436]	[0.494]	[0.494]	[0.484]	[0.473]
4	-1.353	-1.460	-1.512	-1.473	-1.595
	[0.505]	[0.538]	[0.542]	[0.522]	[0.511]
5	-1.782	-1.979	-2.052	-2.018	-2.231
	[0.523]	[0.622]	[0.625]	[0.594]	[0.580]
6	-1.469	-1.672	-1.761	-1.723	-1.824
	[0.671]	[0.708]	[0.722]	[0.690]	[0.650]
Observations	11313	11313	11313	11313	11313
R-squared	0.053	0.130	0.131	0.140	0.130
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.6 Share of Children below 200 Percent of Poverty Line

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Below 200% Poverty Line				
A. 1970 Census: Mean DV			*53.580*		
-6	0.224	0.173	0.272	0.281	0.229
	[0.586]	[0.616]	[0.608]	[0.649]	[0.660]
-5	-0.169	-0.548	-0.448	-0.390	-0.674
	[0.543]	[0.604]	[0.596]	[0.618]	[0.646]
-4	0.141	-0.105	-0.007	0.092	-0.124
	[0.552]	[0.568]	[0.561]	[0.591]	[0.608]
-3	-0.222	-0.411	-0.314	-0.198	-0.534
	[0.509]	[0.532]	[0.524]	[0.538]	[0.572]
-2	0.179	0.083	0.169	0.263	0.113
	[0.455]	[0.490]	[0.489]	[0.491]	[0.531]
-1	0.488	0.483	0.561	0.601	0.639
	[0.440]	[0.446]	[0.446]	[0.450]	[0.485]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.010	0.078	0.084	0.100	0.078
B. 1980 Census: Mean DV			*44.080*		
-2	0.174	0.139	0.163	0.118	0.149
	[0.378]	[0.410]	[0.406]	[0.404]	[0.393]
-1	0.300	0.381	0.397	0.368	0.453
	[0.395]	[0.406]	[0.404]	[0.402]	[0.391]
	Event year zero omitted (Year family planning program began)				
1	-0.699	-0.523	-0.549	-0.525	-0.570
	[0.397]	[0.437]	[0.436]	[0.432]	[0.423]
2	-1.045	-0.918	-0.946	-0.924	-1.027
	[0.393]	[0.432]	[0.434]	[0.427]	[0.422]
3	-0.725	-0.651	-0.679	-0.640	-0.678
	[0.472]	[0.558]	[0.559]	[0.545]	[0.532]
4	-0.898	-0.755	-0.797	-0.753	-0.769
	[0.575]	[0.603]	[0.606]	[0.579]	[0.570]
5	-2.112	-1.860	-1.902	-1.867	-2.145
	[0.585]	[0.668]	[0.674]	[0.642]	[0.622]
6	-1.383	-1.124	-1.164	-1.134	-1.189
	[0.681]	[0.732]	[0.744]	[0.703]	[0.673]
Observations	11313	11313	11313	11313	11313
R-squared	0.111	0.191	0.192	0.200	0.191
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.7 Share of Children Living in Households Receiving Any Public Assistance

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share Receiving any Welfare				
A. 1970 Census: Mean DV			*7.555*		
-6	-0.148	-0.299	-0.367	-0.312	-0.347
	[0.330]	[0.352]	[0.374]	[0.374]	[0.376]
-5	-0.112	-0.301	-0.360	-0.288	-0.349
	[0.341]	[0.357]	[0.378]	[0.376]	[0.381]
-4	-0.031	-0.100	-0.150	-0.065	-0.096
	[0.320]	[0.350]	[0.367]	[0.367]	[0.375]
-3	-0.148	-0.362	-0.388	-0.297	-0.440
	[0.307]	[0.341]	[0.349]	[0.348]	[0.368]
-2	0.013	-0.074	-0.077	-0.003	-0.062
	[0.248]	[0.294]	[0.298]	[0.293]	[0.316]
-1	0.034	-0.124	-0.106	-0.068	-0.133
	[0.299]	[0.312]	[0.314]	[0.315]	[0.342]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.005	0.079	0.083	0.097	0.079
B. 1980 Census: Mean DV			*11.560*		
-2	-0.022	-0.052	-0.069	-0.159	-0.056
	[0.266]	[0.287]	[0.288]	[0.292]	[0.274]
-1	-0.136	-0.180	-0.187	-0.234	-0.189
	[0.265]	[0.269]	[0.268]	[0.271]	[0.259]
	Event year zero omitted (Year family planning program began)				
1	-0.636	-0.638	-0.644	-0.604	-0.687
	[0.255]	[0.273]	[0.274]	[0.273]	[0.266]
2	-0.957	-0.862	-0.878	-0.797	-0.938
	[0.252]	[0.286]	[0.289]	[0.287]	[0.280]
3	-1.177	-1.206	-1.224	-1.111	-1.342
	[0.331]	[0.337]	[0.341]	[0.339]	[0.325]
4	-1.269	-1.180	-1.221	-1.079	-1.294
	[0.440]	[0.441]	[0.443]	[0.445]	[0.414]
5	-1.516	-1.338	-1.403	-1.238	-1.481
	[0.441]	[0.453]	[0.457]	[0.451]	[0.422]
6	-1.623	-1.467	-1.553	-1.369	-1.642
	[0.500]	[0.477]	[0.488]	[0.481]	[0.440]
Observations	11313	11313	11313	11313	11313
R-squared	0.010	0.102	0.103	0.114	0.102
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.8 Share of Children Living with Single Parents

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Share in Single Headed Households				
A. 1970 Census: Mean DV			*12.840*		
-6	-0.013	-0.200	-0.225	-0.077	-0.230
	[0.438]	[0.464]	[0.492]	[0.488]	[0.498]
-5	0.064	-0.129	-0.154	-0.022	-0.137
	[0.422]	[0.442]	[0.468]	[0.464]	[0.471]
-4	-0.006	-0.207	-0.231	-0.119	-0.233
	[0.406]	[0.431]	[0.452]	[0.453]	[0.461]
-3	0.081	-0.061	-0.080	0.013	-0.041
	[0.407]	[0.425]	[0.437]	[0.436]	[0.459]
-2	-0.214	-0.348	-0.360	-0.296	-0.413
	[0.362]	[0.378]	[0.386]	[0.387]	[0.409]
-1	-0.043	-0.154	-0.163	-0.130	-0.145
	[0.339]	[0.367]	[0.369]	[0.369]	[0.402]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.007	0.062	0.062	0.068	0.062
B. 1980 Census: Mean DV			*16.940*		
-2	-0.399	-0.438	-0.420	-0.381	-0.497
	[0.322]	[0.340]	[0.339]	[0.341]	[0.324]
-1	0.336	0.363	0.365	0.387	0.442
	[0.334]	[0.358]	[0.355]	[0.358]	[0.341]
	Event year zero omitted (Year family planning program began)				
1	0.002	-0.102	-0.143	-0.164	-0.091
	[0.298]	[0.295]	[0.296]	[0.294]	[0.288]
2	-0.080	-0.355	-0.424	-0.449	-0.393
	[0.290]	[0.315]	[0.312]	[0.310]	[0.308]
3	-0.086	-0.438	-0.528	-0.563	-0.486
	[0.326]	[0.348]	[0.351]	[0.348]	[0.338]
4	-0.392	-0.785	-0.913	-0.954	-0.918
	[0.434]	[0.466]	[0.460]	[0.459]	[0.438]
5	0.190	-0.279	-0.451	-0.489	-0.281
	[0.462]	[0.494]	[0.480]	[0.482]	[0.460]
6	0.057	-0.483	-0.681	-0.719	-0.541
	[0.495]	[0.534]	[0.524]	[0.526]	[0.491]
Observations	11313	11313	11313	11313	11313
R-squared	0.026	0.095	0.098	0.105	0.095
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.9 Mother's Age at the Time of Child's Birth

	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Average Mother's Age at Birth				
A. 1970 Census: Mean DV			*25.790*		
-6	0.062	0.029	0.034	0.042	0.033
	[0.0966]	[0.103]	[0.0990]	[0.0957]	[0.111]
-5	0.083	0.044	0.050	0.062	0.052
	[0.0887]	[0.0915]	[0.0887]	[0.0869]	[0.097]
-4	0.094	0.066	0.072	0.086	0.080
	[0.0797]	[0.0840]	[0.0816]	[0.0810]	[0.090]
-3	0.003	-0.005	0.003	0.017	-0.011
	[0.0744]	[0.0791]	[0.0764]	[0.0760]	[0.085]
-2	0.020	0.034	0.043	0.051	0.040
	[0.0750]	[0.0728]	[0.0709]	[0.0702]	[0.077]
-1	0.001	0.000	0.009	0.008	-0.006
	[0.0628]	[0.0694]	[0.0686]	[0.0687]	[0.075]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.104	0.177	0.182	0.203	0.177
B. 1980 Census: Mean DV			*24.940*		
-2	0.028	0.045	0.047	0.029	0.047
	[0.0545]	[0.0602]	[0.0599]	[0.0584]	[0.063]
-1	0.065	0.078	0.082	0.072	0.089
	[0.0589]	[0.0615]	[0.0614]	[0.0609]	[0.064]
	Event year zero omitted (Year family planning program began)				
1	-0.013	-0.003	0.002	0.010	0.005
	[0.0613]	[0.0644]	[0.0642]	[0.0639]	[0.067]
2	-0.116	-0.114	-0.106	-0.092	-0.123
	[0.0572]	[0.0645]	[0.0654]	[0.0642]	[0.066]
3	-0.156	-0.182	-0.172	-0.154	-0.201
	[0.0623]	[0.0671]	[0.0680]	[0.0664]	[0.069]
4	-0.170	-0.193	-0.181	-0.158	-0.210
	[0.0753]	[0.0820]	[0.0827]	[0.0814]	[0.087]
5	-0.269	-0.303	-0.284	-0.260	-0.346
	[0.0861]	[0.0977]	[0.0988]	[0.0961]	[0.105]
6	-0.236	-0.276	-0.254	-0.228	-0.311
	[0.0921]	[0.101]	[0.0999]	[0.0975]	[0.110]
Observations	11313	11313	11313	11313	11313
R-squared	0.133	0.203	0.208	0.224	0.203
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Table G.4.10 Average Number of Older Siblings for Cohorts

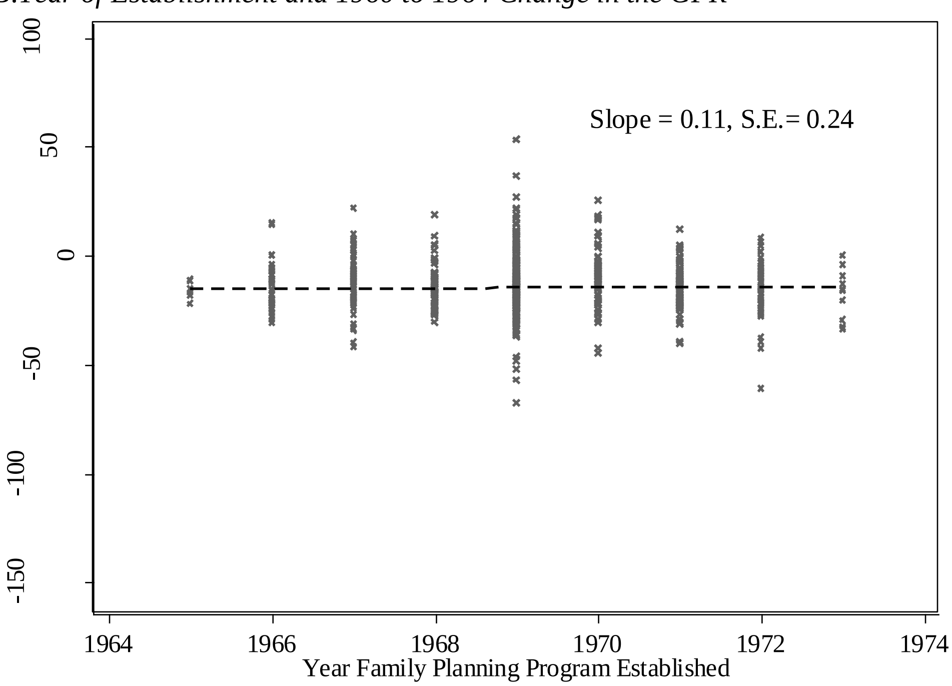
	(1)	(2)	(3)	(4)	(5)
	Dependent Variable: Average Number of Older Siblings				
A. 1970 Census: Mean DV			*1.894*		
-6	0.000	-0.024	-0.024	-0.015	-0.029
	[0.0300]	[0.0315]	[0.0313]	[0.0311]	[0.033]
-5	0.022	0.001	0.001	0.011	0.001
	[0.0270]	[0.0272]	[0.0269]	[0.0268]	[0.029]
-4	-0.009	-0.024	-0.023	-0.013	-0.031
	[0.0228]	[0.0237]	[0.0235]	[0.0237]	[0.024]
-3	0.016	0.015	0.017	0.027	0.020
	[0.0268]	[0.0277]	[0.0273]	[0.0277]	[0.029]
-2	0.016	0.015	0.018	0.025	0.020
	[0.0245]	[0.0242]	[0.0241]	[0.0240]	[0.026]
-1	0.001	-0.004	-0.001	0.002	-0.006
	[0.0212]	[0.0233]	[0.0234]	[0.0235]	[0.025]
	Event year zero omitted (Year family planning program began)				
Observations	7324	7324	7324	7324	7324
R-squared	0.234	0.295	0.297	0.313	0.295
B. 1980 Census: Mean DV			*1.378*		
-2	-0.016	-0.019	-0.018	-0.024	-0.021
	[0.0210]	[0.0234]	[0.0231]	[0.0226]	[0.024]
-1	0.008	0.015	0.016	0.013	0.019
	[0.0202]	[0.0215]	[0.0214]	[0.0215]	[0.022]
	Event year zero omitted (Year family planning program began)				
1	-0.023	-0.015	-0.014	-0.012	-0.015
	[0.0183]	[0.0197]	[0.0197]	[0.0196]	[0.020]
2	-0.043	-0.030	-0.028	-0.025	-0.033
	[0.0176]	[0.0189]	[0.0190]	[0.0187]	[0.018]
3	-0.044	-0.039	-0.035	-0.031	-0.041
	[0.0175]	[0.0203]	[0.0204]	[0.0197]	[0.021]
4	-0.072	-0.062	-0.057	-0.051	-0.069
	[0.0229]	[0.0267]	[0.0267]	[0.0259]	[0.028]
5	-0.086	-0.070	-0.062	-0.057	-0.079
	[0.0237]	[0.0306]	[0.0305]	[0.0293]	[0.032]
6	-0.081	-0.065	-0.055	-0.050	-0.072
	[0.0270]	[0.0336]	[0.0331]	[0.0316]	[0.036]
Observations	11313	11313	11313	11313	11313
R-squared	0.492	0.541	0.543	0.557	0.541
For Panels A and B:					
Model	1	2	3	4	2M
Covariates	C, Y	C, Y, S-Y	C, Y, S-Y, R, A	C, Y, S-Y, R, A, Ctrend	C, Y, S-Y, Adjusted for Misclassification
Counties	666	666	666	666	666

See notes in table III.3.

Figure G.1 Fertility Rates and the Roll-Out of Federal Family Planning Programs

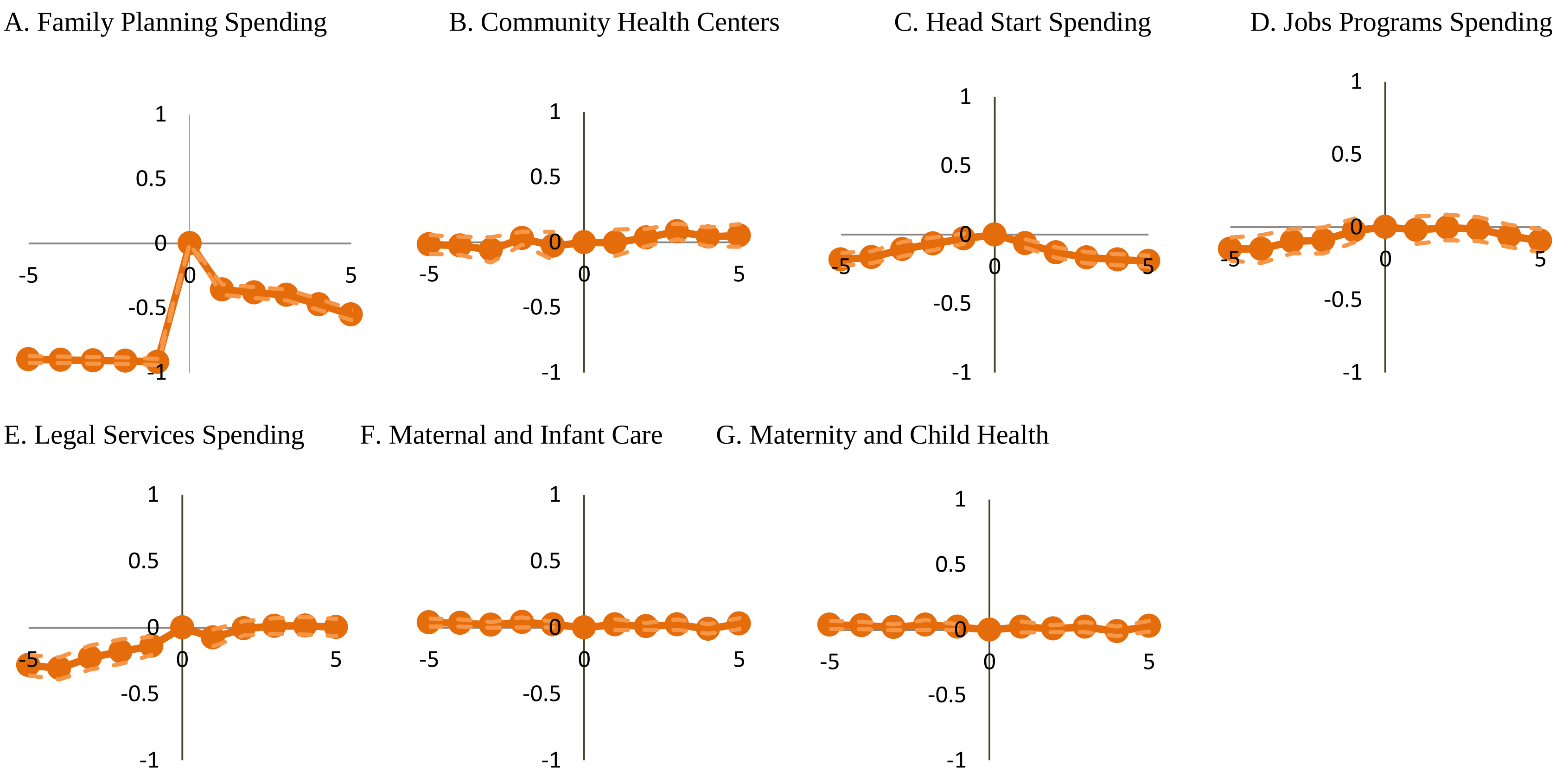
A.Year of Establishment and 1964 General Fertility Rate (GFR)

B.*Year of Establishment and 1960 to 1964 Change in the GFR*



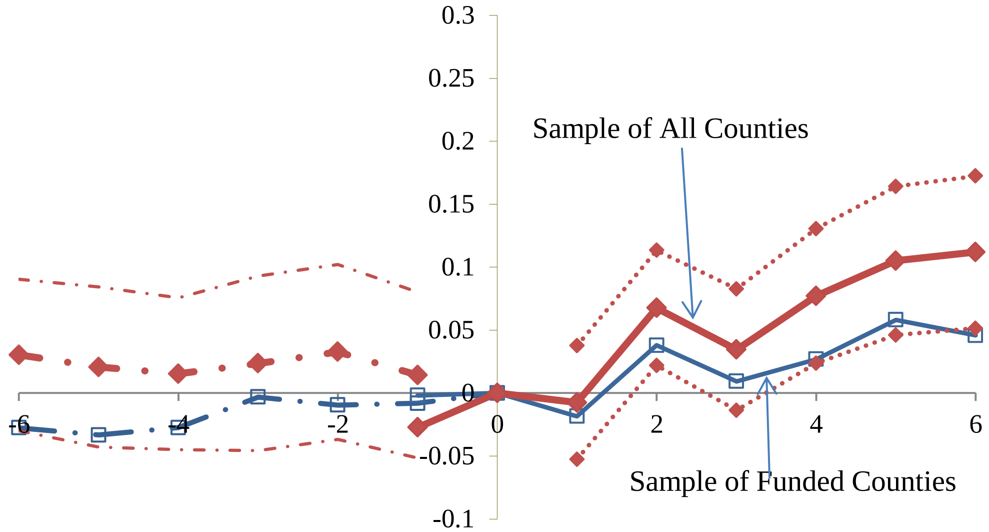
The x-axis plots the year when the first federal family planning program was established in the county. The y-axis in panel A plots the 1964 GFR, the y-axis in panel B plots the change in the GFR from 1960 to 1964. The dashed lines indicate the estimated relationship between the x and y variables using linear regression. The estimated slope and standard error for each relationship are indicated. Sources: Family planning data: NACAP, NAFO and OEO (1969, 1971 and 1974); GFR: Bailey (2012).

Figure G.2 Funding for Other War on Poverty Programs Relative to the First Federal Family Planning Grant



Each panel plots weighted least-squares estimates of τ from equation (III.1) including funded and unfunded counties and state-year fixed effects. The dependent variable is equal to 1 if the county received any federal funding for the indicated program. The weights are the 1970 population of women ages 15 to 44. The dependent variable is equal to 1 if the county received *any* federal grant for the indicated program. Sources: NACAP and NAFO.

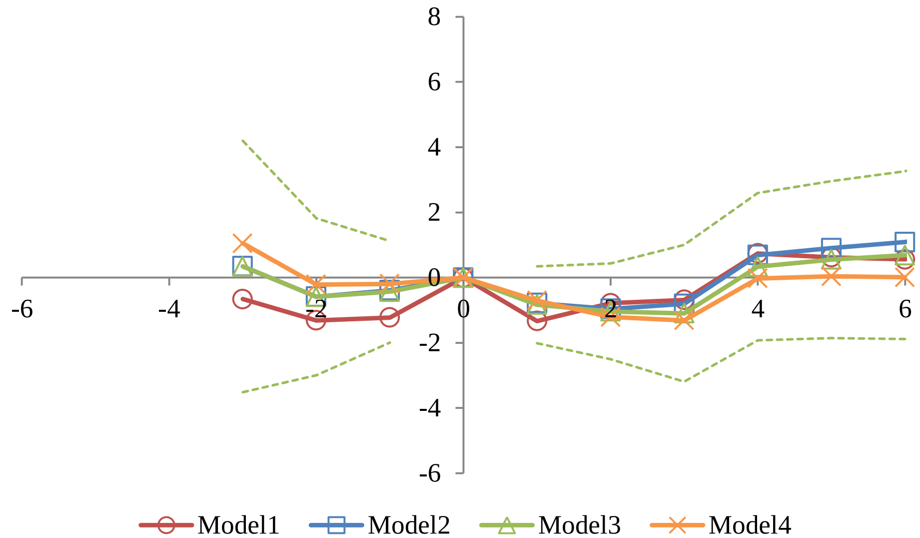
Figure G.3 Relationship of Misclassification of County of Birth and Parents' Treatment with Federally Funded Family Planning Program



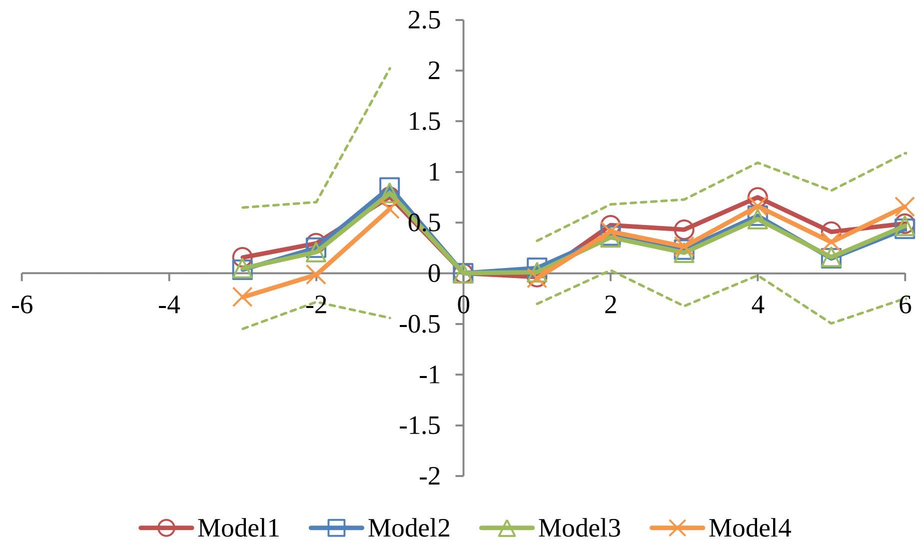
Series plot estimates of τ from our baseline model of equation (III.4) for a sample of all counties (red diamonds) and a sample of only funded counties (blue squares). The x-axis plots the event year, equal to year of observation minus year of first family planning grant, the year the federal family planning began. The dependent variable is the share of children who have changed their treatment status between 1975 and 1980 (between 1965 and 1970 when using the 1970 census). Heteroskedasticity-robust standard errors clustered by county are used to construct 95 percent confidence intervals for a sample of all counties and are presented as dashed lines. Sources: 1970 (dashed lines with markers) and 1980 (solid lines with markers) restricted-use censuses.

Figure G.4 Effects of Federally Funded Family Planning on Marriage and Divorce

A.Marriages per 1000 Women Ages 15 to 44



Divorces per 1000 Women Ages 15 to 44



Series plot point estimates of τ from specifications of equation (III.4) using both funded and unfunded counties. See table 2 notes in main paper for information on specifications and models. The dependent variable in panel A is the number of marriages per 1000 women ages 15 to 44; the dependent variable in panel B is the number of divorces per 1000 women ages 15 to 44. Sources: Numerators are hand entered from published county-level tabulations from Vital Statistics, 1962 to 1988. Denominators rely on SEER population data from 1969 forward and data interpolated between the 1960 census and 1969 SEER data for the rest of the 1960s.

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