Pricing Children, Curbing Daughters: Fertility and the Sex Ratio During China's One-Child Policy

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Abstract

I characterize China's One-Child Policy as an individually tailored pricing system allowing women to have second and third children. The average policy price for having a second or third child is 13,900 dollars (2020, purchasing-power parity). It varies by age and other individual characteristics. I estimate policy elasticities exploiting within-individual price variation in a sample of women that annually replicates the national total fertility rate between 1979 and 2000. A 1% increase in the policy price decreases the probability of having a child by 0.7% (s.e. 0.1%). Without the policy, the total fertility rate would have been 2.7 children. The policy lowered this rate by 0.3 (s.e. 0.04) by curbing the number of girls born. This gendered policy impact further distorted the sex ratio at birth. Without the policy, the excess of boys per 100 girls born would have been 4.3. The policy increased the excess by 1.1 (s.e. 0.4).

JEL Codes: J13, J10, N35.

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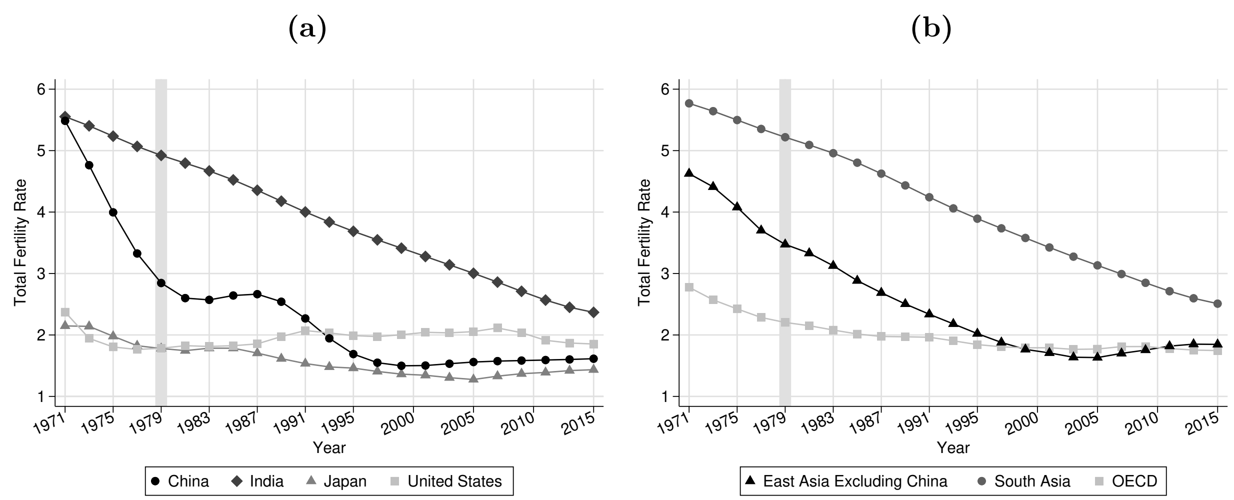
1. Introduction

The One-Child Policy regulated the fertility outcomes of approximately 500 million Chinese women between 1979 and 2010.[1] Academics disagree on the impact of the policy on fertility. Duflo (2008) describes it as "a great success in terms of controlling fertility," and several studies argue that the impact on fertility in turn affected other outcomes like crime, education, and marriage (Zhang, 2017). Chen and Fang (2018) summarize the current literature as finding a "fairly limited impact on lifetime fertility." Figure 1 illustrates the challenge in identifying the impact of the policy on fertility. Fertility was already decreasing before 1979 in China and neighboring countries; it continued to smoothly decrease after the policy's implementation. It is challenging to isolate the impact of the policy when other factors could have potentially caused fertility to further decrease after 1979 (e.g., wage growth, increasing educational attainment, agricultural reforms).[2]

Figure 1. Total Fertility Rate, 1970-2015



Note: Two-year binned total fertility rate. East Asia: China, Japan, Mongolia, South Korea, and North Korea. South Asia: Afghanistan, Bangladesh, Bhutan, India, Maldives, Nepal, Pakistan, and Sri Lanka. **Source:** The World Bank (2017a).

[1]The precise statistic is 504.3 million. It is the total number of women who were between 15 and 45 years old for at least one year between 1979 and 2010 according to the 1982, 1990, and 2000 census (Minnesota Population Center, 2017).

[2]Sen (2015), Whyte et al. (2015), and Huang et al. (2019) elaborate on this argument.

The One-Child Policy did not mandate that only one-child families were to be created. Instead, it introduced a system that priced permits for having children.[3] Permits for having a first child were free. Permits for having a second or third child had a price. Permits for having more than three children were generally unavailable. I characterize the pricing system building on two sources. I use the province-level, year-specific prices documented by Ebenstein (2010a) as a baseline. Then, I include province-level, year-specific policy "exemptions" documented from archival records by Scharping (2003). These exemptions set the prices of permits for having second or third children to zero based on seventeen individual characteristics (e.g., age and sex of the first child, ethnicity, scarcity of males in the family, type of job).

I map the policy characterization to a sample of women surveyed by the China Health and Retirement Longitudinal Study (CHARLS, National School of Development, 2017). I construct the fertility histories of the women in this sample and show that they annually replicate the national total fertility rate during the period analyzed, 1979-2000. Mapping the policy characterization to longitudinal fertility and other demographic information allows me to observe within-woman variation in the policy prices. I perform two exercises to validate my policy characterization. First, I use digitized records from city gazetteers and yearbooks collected by Suárez-Serrato et al. (2019) to verify that the pricing system was used to enforce aggregate fertility objectives of the Communist Party.[4] Second, I verify that my policy characterization aligns with information from a nationally representative survey which posed questions about policy implementation to more than 400 village and town government officials.

I estimate policy-price elasticities using dynamic panel models, accounting for individual and age × year × province invariant characteristics. The models regress the flow of

[3]Throughout the paper, I refer to "women," "women and their partners," and "households" as the units of observation who are making fertility decisions and paying the prices.

[4]Anecdotal evidence indicates that reductions in social security, forced abortions, and sterilization were other policy-enforcement mechanisms. These mechanisms were not official and are impossible to formally quantify. I discuss how ignoring them could bias my results in the body of the paper.

fertility on the policy price and the stock of children in the previous year. Individual-level event studies based on price spikes justify the identification assumption of price exogeneity. A generalization of the Arellano and Bond (1991) moment estimator accounts for the endogeneity of the stock of children. The survey posing questions to government officials about policy implementation allows me to construct an instrument to account for measurement error in the documentation of the pricing system. I find that a 1% increase in the policy price decreases the probability of having a child by an average of 0.052 (s.e. 0.007) percentage points. The fraction of women having a child at any point in the sample is 0.072, implying a policy-price elasticity of $0.052/0.072 \approx 0.7$ (s.e. 0.1). The policy impact is driven exclusively by a reduction in the number of girls born. I estimate the average reduction in the probability of having a girl after a 1% increase in the policy price by age. This semi-elasticity increases monotonically from -0.1 (s.e. 0.02) percentage points at age 20 to -0.0004 (0.0006) at age 40, passing through -0.02 (s.e. 0.004) at age 30. The analogous semi-elasticities for the probability of having a boy are all virtually 0.

The sex ratio at birth of first children is not distorted in China (Almond et al., 2019; Ebenstein, 2010a). This fact holds in the sample of women that I analyze. The gendered policy-price semi-elasticities are driven by mothers whose first children are girls. Women whose first children are boys are virtually unaffected by the policy. Mothers thus respond to the policy by combining a stopping rule with sex-selective abortions. Suggestive evidence using longitudinal data on abortions is consistent with this explanation. Mothers of first-child boys are unaffected by the policy because they do not pursue fertility after their first child. Upon a policy-price increase, their likelihood of aborting a pregnancy is unaffected. Mothers of first-child girls pursue fertility further. Upon a policy-price increase, their likelihood of aborting a pregnancy increases. Aborting allows them to pursue their preferred sex combination (girl-boy). This evidence is suggestive because the results for mothers of first-child girls have large standard errors. However, both the results for mothers with first-child girls and mothers with first-child boys yield the same conclusion across several specifications.

The age-specific policy-price semi-elasticities enable predicting the decrease in the total fertility rate due to the policy. The total fertility rate in the sample of women that I analyze is 2.37 children.[5] In the absence of the policy, this rate would have been 0.30 (s.e. 0.04) children higher—i.e., 11.4% (s.e. 1.2%) higher. I qualify this calculation as stylized in the body of the paper, as it requires additional, strong assumptions. A similar calculation indicates that the policy increased the excess of boys born per 100 girls born. The observed ratio is 110.4 boys per 100 girls born. In the absence of the policy, it would have been 1.1 (s.e. 0.4) lower. That is, 109.3 boys per 100 girls born. This phenomenon of fertility decreasing while the sex ratio at birth increases has also been documented in India (Anukriti, 2018; Jayachandran, 2017).

Zhang (2017) surveys economic studies of the One-Child Policy. Most use the temporal variation in province-level prices documented by Ebenstein (2010a). Generally, they map this variation into various census waves to study policy impacts on the marriage market (Huang and Zhou, 2015), the sex ratio at birth (Ebenstein, 2010b), urban-rural migration (Huang et al., 2019), and other household outcomes (Huang et al., 2021). They exploit province × cohort price variation in repeated cross sections.[6] Qian (2009) uses gender of the first child, one of the seventeen exemptions that set the policy price to zero, to study the intergenerational impact of the policy on education. I combine the temporal variation in province-level prices with most of the exemptions. Mapping this characterization to longitudinal data enables exploiting within-individual price variation to estimate policy-price elasticities.

[5]This rate may appear to be high. However, note that the total fertility rate considers all women who were 15 to 45 years old between 1979 and 2000. Some women could have had a relatively large number of children before the policy and still be included in the calculation of the total fertility rate. More importantly, women were allowed to have two or three children if they paid the policy prices. Especially in rural areas, average completed fertility in the later part of the 20^{th} century is well-known to be greater than one.

[6]Li et al. (2005) study the impact of the policy on fertility using this same variation and one of the exemptions that I use (based on ethnicity). Other studies use similar designs to study the policy impact on fertility as a first stage when analyzing outcomes such as migration and intergenerational quantity-quality trade-offs (e.g. Li and Zhang, 2017; Liu, 2014; Wang et al., 2017). McElroy and Yang (2000) use county-level variation in policy intensity to study fertility in a cross section of households from a handful of provinces surveyed in 1992.

Recent research argues that population-control policies were an important driver of the global decrease in fertility observed during the second half of the 20th century (e.g., De Silva and Tenreyro, 2017). Others argue that the opportunity cost of raising children and the increasing demand for high-skilled human capital are the main drivers of demographic transitions (e.g., Galor, 2012). In the context that I analyze, where the government had thorough control over household economic decisions, the response to the policy was inelastic. This does not imply that the One-Child Policy did not reduce fertility. Instead, it implies that large prices were required to reduce fertility. The restrictions implied by the policy interacted with the strong preference of Chinese families for having male children (Sen, 1991), which contributed to the sex-ratio imbalance in the population.[7] China's One-Child Policy aimed to curb the number of children born per household; it only curbed the number of daughters.

2. Context and Data

2.1 Economic and Institutional Setting

The economic and institutional setting was inflexible during the era of the One-Child Policy. Although the government implemented a series of reforms based on market principles beginning in 1979, it still exerted considerable control over household economic activities.

During the period that I analyze, the *hukou* (residency) dictated the location at which an individual could live, which was usually the location of birth, and assigned them either an agricultural or a non-agricultural status, which was usually the status of their parents. Government rules, taxes, and subsidies were tied to the individual *hukou*. The *hukou* system is so fundamental to the organization of the Chinese economy that the book detailing its

[7] This male-biased preference is widespread in China and East and South Asia (e.g., Das Gupta et al., 2003). Anukriti (2018) reports a male-biased sex ratio beyond the natural rate in Albania, Armenia, Azerbaijan, China, Georgia, India, South Korea, Macedonia, Montenegro, Nepal, and Vietnam. Between 1980 and 2015, the percentage of the world's population inhabiting these countries remained stable at 40% (The World Bank, 2017b), which illustrates how widespread sex-selection practices are. Anukriti (2018) reports the same bias in Asian populations in Canada, Greece, Spain, the United Kingdom, and the United States.

rules is referred to as *zhongguo diyi zhengjian*, or "China's Number 1 Document" (Tian, 2003). Until 2000, individuals with non-agricultural *hukou* belonged to a *danwei* (working unit). Membership to a *danwei* was regulated by the central government. The *danwei* provided permanent employment and dictated total labor income—including access to food, health care, pensions, children's education, and housing (Tang and Parish, 2000; Whyte and Parish, 1985). Individuals remained in their *danwei* for life (Lu and Perry, 1997). After the liberalization of the economy in 1979, the *danwei* system continued to regulate economic decisions (Naughton, 2007).

Individuals with an agricultural *hukou* mostly worked as farmers in collective systems until 1984. The collective to which they belonged dictated labor income through a point system. Individuals earned points per work day and received basic in-kind payments. At the end of the harvest year, the government procured the grain. The surplus was sold at fixed prices and the money was divided according to work days. An agrarian reform began in 1978. After the reform, the land was divided as part of a contract between the collective and households, allowing for centralized control of grain prices (Cai, 2003). In locations where agricultural activities lost importance, individuals became part of town and village enterprises, or other centralized activities which regulated decision-making, in a similar way to the *danwei* system (Naughton, 2007).

Appendix Figure A.2 illustrates how the institutional setting limited decision making. For the sample of women that I describe and analyze below, there is virtually no variation in the probability of supplying full-time labor. It is 0.90 in the full sample, and remains at that level when only considering women who gave birth during the year observed or one or two years before. In 80% of the villages and towns across China, 10% or fewer household heads report to have ever migrated or relocated. Migration was mostly temporary, regulated by government permits, and within villages and towns (Chan, 2001).[8] The inflexibility implied

[8]Massive waves of rural-to-urban migration began in the mid-1990s (Chan, 2001). These waves happened too late to affect the core of the sample that I analyze.

by the institutional setting is useful in my empirical analysis. The simultaneity of decisions like labor force participation, migration, and education is a concern when analyzing fertility (Hotz et al., 1997). In this setting, government mandates fixed labor supply and migration. They also fixed education to specific levels (Fan et al., 2015; Postiglione, 2015). Simultaneity of these decisions with fertility is thus a second-order concern in my analysis.

Bureaucratic Implementation. After establishing the People's Republic of China in 1949, Mao Zedong encouraged population growth. Birth control, which would reduce the size of the workforce, was condemned. Imports of contraceptives were banned. Abortions were illegal. After unfettered population growth and events like the Great Famine, the government launched family-planning campaigns in the 1970s, which encouraged the use of contraception, the delay of marriage, and the formation of smaller families (Powell, 2012). Towards the end of the decade, abortions became legal (Scharping, 2003). Before 1979, specific fertility limits were suggested and enforced in some regions of the country. For people with agricultural *hukou*, which comprised the majority of the population before 1979, there were no limits (Wang et al., 2017).[9] Women had to register any new pregnancy beginning in the early 1960s. Family-planning policies that preceded the One-Child Policy further established a bureaucracy for population control.[10]

In 1979, the government announced the One-Child Policy at the federal level. The mandate placed local authorities in charge of the timing and implementation of the policy. Local authorities had the incentive to implement the policy because their effectiveness in curbing fertility factored into their job evaluations. Failing to effectively implement the policy resulted in administrative penalties, such as disaffiliation from the Chinese Communist Party

[9]The empirical analysis in this paper relies on within-woman policy variation. Including policies that preceded the One-Child Policy requires documenting and justifying other types of variation, which falls outside the scope of this study. Babiarz et al. (2018) and Banerjee et al. (2014) are examples of studies that analyze or include policies that preceded the One-Child Policy.

[10]Given the size of China's population, registering every single birth and later enforcing the pricing system associated with the One-Child Policy suggests the need for an immense bureaucracy. Employment in Birth-Planning Commissions grew from fewer than 5,000 employees in 1970 to approximately 400,000 in 1995 (Scharping, 2003).

(Scharping, 2003). Appendix Table A.1 shows an example of the forms used to evaluate local authorities. Aggregate fertility and citizens' knowledge of the policy were evaluation criteria. Local authorities also faced opposition to the policy from citizens (Hardee-Cleaveland and Banister, 1988). Arguably, the trade-off between conforming to national policy and mitigating social discontent at the local level led officials to design the policy as a pricing system allowing women to have second and third children. Greenhalgh and Winckler (2005) document support for this interpretation. For instance, in 1989, Premier Li Peng addressed provincial governors, stating that: "To achieve substantial compliance, policy must be supplemented with more detailed management by objectives .... Targets should be evaluative."

2.2 Empirical Characterization of the Pricing System

China's One-Child Policy implemented the following pricing system. Permits for having a first child were free, permits for having a second or third child had a price, and permits for having more than three children were unavailable. Children born without a permit had no *hukou* registration and, thus, no rights (e.g., education and health services). Their parents were subject to punishments such as being dismissed from their *danwei*, therefore permanently losing their jobs.

The price per permit for second or third children was either a proportion of household labor income or a lump sum. In either case, the price had to be paid for a fixed number of years for each child additional to the first. Let $\mathcal{I}$ index women. I analyze discrete annual periods and do not observe the onset of menarche or menopause. I let $\mathcal{A}_i$ index the ages when woman $i \in \mathcal{A}_i$ is observed between ages 15 and 45. A woman i of age $a \in \mathcal{A}_i$ had to register her pregnancy to the local birth-planning authority before her fourth month of pregnancy for it to be legal. If she did not want to keep her child, she was able to get an abortion for free. If she wanted to keep her child and was in her second or third pregnancy, she needed to sign a "One-Child Policy contract" with the government. The contract stipulated the price and form of payment. A signed contract was not revised if the policy were to change after its terms were agreed upon.

I refer to the present value of the price per permit for second or third children as policy price and denote it by Ξ_{ia}, which summarizes the One-Child Policy contract:

$$\Xi_{ia} := \mathbf{1}[\text{proportional-price province}]_{ia} \cdot \underbrace{\left[\sum_{\ell=1}^{L_{ia}^{\text{prop}}} \beta^{\ell-1} \left(\kappa_{ia} y_{ia}\right)\right]}_{\text{present-value proportional price}} + \mathbf{1}[\text{lump-sum price province}]_{ia} \cdot \underbrace{\left[\sum_{\ell=1}^{L_{ia}^{\text{lump}}} \beta^{\ell-1} \left(\tau_{ia}\right)\right]}_{\text{present-value lump-sum price}}, \quad (1)$$

where κ_{ia} is the fraction of household labor income to be paid in proportional-policy provinces and τ_{ia} is the amount to be paid in lump-sum-policy provinces. L_{ia}^{prop} and L_{ia}^{lump} are the number of years in which the prices were paid in the respective regimes. y_{ia} is household labor income and β is a discount factor. When signing the contract, households had perfect certainty about either κ_{ia} and L_{ia}^{prop} or τ_{ia} and L_{ia}^{lump}. I define Ξ_{ia} using a constant value of household labor income y_{ia}, which assumes that women calculated the present value of the price using age-a (current) household labor income. This assumption is consistent with stationary expectations. The price Ξ_{ia} condenses the information of a contract establishing multiple payments into a present-value. It is a function of an unknown discount factor and involves assumptions about the evolution of household labor income. I discuss these two features as potential disadvantages in Section 3. For now, I set β to 0.98 (i.e., discount rate = 0.02).

Local officials announced the values of κ_{ia} and L_{ia}^{prop} or τ_{ia} and L_{ia}^{lump} each year. They also specified whether the price was proportional or lump sum. I follow two steps when constructing Ξ_{ia}. First, I set the value of Ξ_{ia} to the province- and year-specific value documented by Ebenstein (2010a).[11] For example, if a woman was 34 years old in 1987 and lived in the

[11] Ebenstein (2010a) documents province-level, year-specific values of Ξ_{ia} for the period 1979-2000. He uses a 2% discount rate.

Table 1. Criteria Setting the Policy Price for Having Second or Third Children to Zero

Panel a. **First Child Dead, Disabled, Older**		
1. First child is disabled or dead	2. Pregnancy after long years of childless marriage and a subsequent adoption	3. In remarriage one spouse has been childless, the other spouse already had one or two children
4. Three, four, or five years after birth of first child		
Panel b. **Job Difficulty**		
5. One spouse is disabled and cannot work	6. A peasant couple lives in sparsely settled mountain, reclamation, or border seas	7. One spouse has been constantly working in underground mining for more than 5 years
8. Couple has real economic difficulties or claims other peculiar reasons		
Panel c. **Male Scarcity in the Family**		
9. One spouse or both spouses are single children	10. Only one child or one son has been born to a family for two generations	11. Husband settles in the family of his wife which has daughters and no sons
Panel d. **Other, Classified as Categories of their Own**		
12. One or both spouses belong to a national minority with less than 10 million members	13. One spouse is the (single) child of a revolutionary martyr	14. First child is a girl
Panel e. **Other, Not Used in Analysis**		
15. One spouse is a deep-sea fisherman	16. One or both spouses returned to China from Hong Kong or Taiwan	17. Among brothers, only one is able to have children

Note: This table lists the criteria setting to the policy price for having a second or third child to zero. The applicability of these criteria varies by province and time as indicated in Tables A.3, A.4, A.5, and A.6. In the sample analyzed below, I can observe whether women comply to each criterion except for the criteria in ***Panel e.*** (I do not observe the individual characteristics required to observe compliance based on these three criteria). Appendix Table A.7 describes the empirical construction of compliance to the criteria. **Source:** Adapted from Scharping (2003).

province of Sichuan, I set Ξ_{ia} to the value reported by Ebenstein (2010a) for the year 1987 in that province. Second, I set Ξ_{ia} to 0 if woman i qualified for an exemption when she was a years old using the documentation in Scharping (2003). Table 1 lists seventeen exemption criteria that applied for at least one year in at least one province. There is province and time variation in the criteria that applied. I list this variation and the empirical construction of the criteria in the Appendix. I construct Ξ_{ia} for a longitudinal sample of women with rich, annual demographics available. This enables observing within-woman variation in Ξ_{ia}, even when accounting for individual and age $\times$ year $\times$ province invariant characteristics.[12]

Figure 2 displays the time series of the average Ξ_{ia} for the sample of women that I analyze, in units of annual average household labor income during the year labeled (e.g., if the average of Ξ_{ia} is 2 in 2000, then the price equals two years of average household labor income in 2000). The rules were uniform across demographics before exemptions became widespread. After an initial period of strict implementation, the government relaxed the policy and reinforced local, incentive-based implementation. It encouraged exemptions by issuing "Document 7" in 1984 (Qian, 2009). In the late 1980s, Premier Li Peng addressed governors and stated that population growth was still economically unsustainable, and advocated for a more rigid implementation (Greenhalgh and Winckler, 2005). Figure 2 is consistent with these facts. It shows that the prices were uniform across demographic groups in 1980, decreased and diverged in the 1980s, and started to quickly increase after Li Peng made public his concerns about population growth.

2.3 Analysis Sample

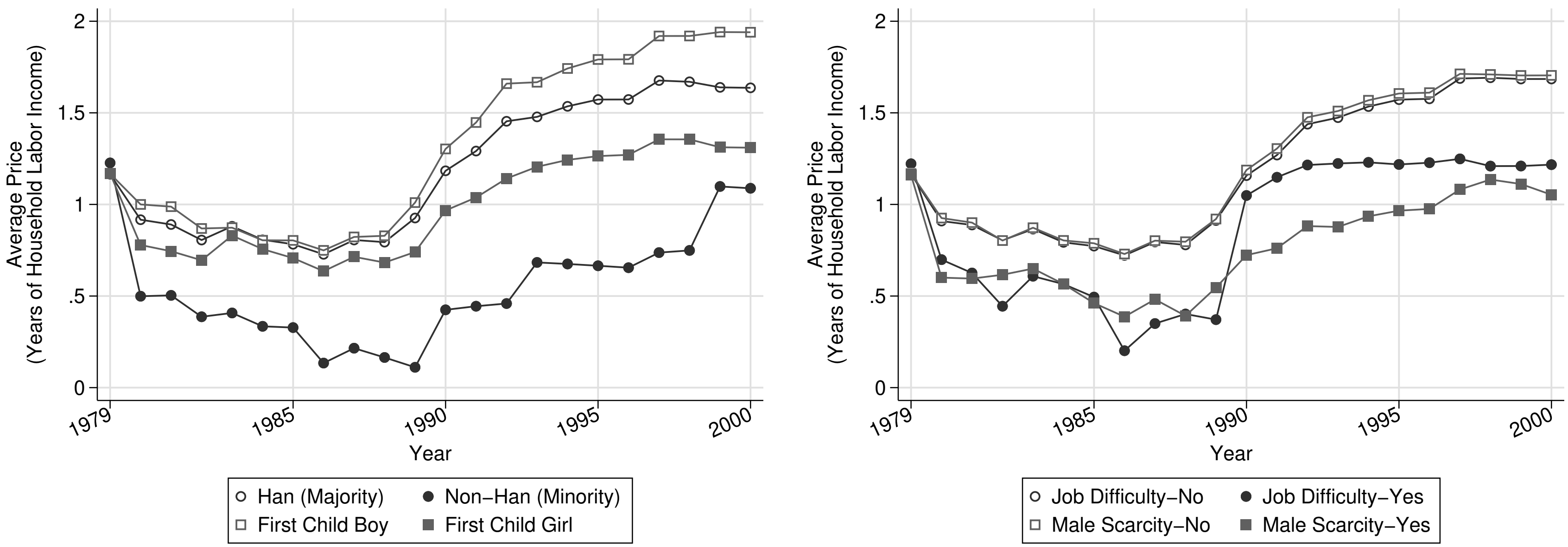
I use data collected by the China Health and Retirement Study (CHARLS) (National School of Development, 2017) on a sample of women (household heads or spouses of household heads) who were 45 years old or older in 2010. The sample was collected in 443 villages and

[12] It is possible to construct Ξ_{ia} for any woman, independently of her stock of children, given her demographic characteristics and the calendar year when she is a years old. If she has no children, Ξ_{ia} is the policy price that she would have needed to pay to have a second or third child, if she would have had one or two children instead of zero.

Figure 2. Policy Prices for Having Second or Third Children

(a) By Ethnicity and Sex of the First Child

(b) By Job Difficulty and Male Scarcity



Note: Panel (a) displays a time series of the average (present-value) policy price of having a second or third child by ethnicity and sex of the first child. I calculate the price using the formula in Equation (1) and express it in years of average household labor income during the year labeled. Panel (b) is analogous to Panel (a) by job difficulty and male scarcity in the family. Job difficulty includes the following categories: one spouse is disabled and cannot work, a peasant couple lives in sparsely settled mountains, reclamation, or border areas, one spouse has constantly been working in underground mining for more than five years, and a couple has real economic difficulties or claims other (related) peculiar reasons. Male scarcity in the family includes the following categories: one spouse or both spouses are single children, only one child or one son has been born to a family for two generations, and the husband settles in the family of his wife which has daughters and no sons. **Sample:** Nationally representative panel of women who were 15 to 45 years old during the year in the label.

towns of 27 provinces. It is nationally representative. Retrospective fertility and employment data were collected, including the number and sex of children (dead or alive), the number of (induced) abortions, and extensive margin labor-force participation.[13] I analyze these variables in longitudinal format. The sample includes the 6,950 women who were between 15 and 45 years old between 1979 and 2000. I observe them annually for an average of 16.97 times. The sample is an unbalanced panel because women were born in different years. A number of women turn 15 and enter the sample during the window of observation. Similarly, a number of women turn older than 45 and exit the sample. Appendix Figure A.1 shows that the sample analyzed annually replicates the time series of the national total fertility rate reported by The World Bank (2017a). This suggests that the sample analyzed is accurate at depicting the aging and fertility profiles of the women "treated" by the One-Child Policy.[14]

Table 2 describes the sample for the entire period analyzed and by five-year sub-periods. Panel a. describes the sample size and Panel b. describes the demographics of the sample. Panel c. describes the fertility variables. Panel d. summarizes policy variables. My preferred specifications contain one-year lags, so observations from the year 1979 are only included through these lags. The stocks are the number of children and abortions up to the current period. The flows are the first-differences of the stocks. Only 0.18% of first-differences are greater than 1 due to twinning or higher-order births. I thus reclassify the flows of children and code them as indicators of changes in the stocks. Reclassifying the flow of abortions is unnecessary because no woman in the sample has more than 1 abortion in a year.

Panel d. of Table 2 summarizes the policy price, Ξ_{ia}, labeled as "price, documentation"

[13]Retrospective fertility information could be advantageous in this context. Chinese parents underreported their number of children when the children were between ages 0 and 4 in the 1990 and 2000 census (e.g., Goodkind, 2011, 2004). Underreporting is less prevalent as children become older (Yi et al., 1993).

[14]An important caveat is worth noting. Life expectancy for the older cohorts in the sample was relatively high (The World Bank, 2021). Thus, the samples analyzed by The World Bank (2017a) to calculate the annual fertility rates and the sample analyzed in this paper could be selected. This selection would compromise the validity of the estimates in this paper as representative of the women affected by China's One-Child Policy. The average age in the sample that I analyze is 32 (right around the middle of the assumed ages in which women are fertile). This fact together with the replication of the annual fertility rate in The World Bank (2017a) ameliorate this concern, but do not discard it definitively.

Table 2. Descriptive Statistics, Analysis Sample

	All	1980-1984	1985-1989	1990-1994	1995-2000
***Panel a.* Sample Size.**					
N (Individuals)	6,950	6,851	6,585	5,886	4,785
Average A_i (Periods Observed)	16.550	4.725	4.771	4.617	5.028
***Panel b.* Demographics.**					
Age	32.473	27.998	31.238	34.656	37.642
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Rural	0.634	0.633	0.636	0.636	0.629
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Agricultural Hukou	0.830	0.827	0.832	0.830	0.830
	[0.007]	[0.007]	[0.007]	[0.007]	[0.006]
Han Ethnicity	0.940	0.940	0.940	0.939	0.938
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
State-Enterprise Worker	0.049	0.050	0.048	0.048	0.050
	[0.160]	[0.161]	[0.158]	[0.160]	[0.163]
Job Difficulty	0.041	0.041	0.042	0.040	0.041
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Male Scarcity	0.083	0.086	0.085	0.082	0.078
	[0.039]	[0.044]	[0.040]	[0.036]	[0.032]
***Panel c.* Fertility.**					
Stock of Children	1.993	1.657	1.970	2.244	2.191
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Stock of Boys	1.048	0.880	1.039	1.177	1.140
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Stock of Abortions	0.122	0.065	0.105	0.154	0.185
	[0.136]	[0.136]	[0.133]	[0.135]	[0.140]
Flow of Children	0.072	0.105	0.105	0.048	0.014
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Flow of Boys	0.038	0.056	0.056	0.025	0.007
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Flow of Abortions	0.007	0.009	0.009	0.007	0.002
	[0.136]	[0.136]	[0.133]	[0.135]	[0.140]
Sex Ratio, 1st Birth	103.115	104.755	103.935	102.500	100.450
(Boy per 100 Girls)	[0.034]	[0.029]	[0.032]	[0.036]	[0.043]
Sex Ratio, After 1st Birth	111.110	111.520	111.110	111.110	110.905
(Boy per 100 Girls)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
In Labor Force	0.891	0.872	0.896	0.902	0.899
	[0.124]	[0.122]	[0.121]	[0.124]	[0.131]
***Panel d.* Policy.**					
Price (2020 USD),	3,319	3,089	2,854	3,510	4,022
Documentation	[0.007]	[0.022]	[0.001]	[0.000]	[0.000]
Price (2020 USD),	3,010	2,312	2,784	3,327	3,885
Surveys	[0.066]	[0.079]	[0.059]	[0.061]	[0.060]
Lump-Sum Price	0.246	0.165	0.161	0.288	0.416
Province	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
***Panel e.* Sample of Mayors.**					
Fraction in Sample of Mayors	0.578	N/A	N/A	0.459	0.823
	[0.826]			[0.652]	[0.646]
Mayor Performance	1.636	N/A	N/A	-1.975	3.800
	[0.000]			[0.000]	[0.000]
Experienced Mayor	0.523	N/A	N/A	0.441	0.572
	[0.000]			[0.000]	[0.000]

Note: Panel a. describes the sample size and the average number of years that each woman in the sample is observed. Panels b. to e. display the average of the variables in the label, as well as the fraction of observations for which the variable is missing in squared parentheses. The fraction missing is calculated using the sample described in Panel a. as a base. N/A: Years not available in the sample of mayors.

in the table. The unit of the policy price is years of average household labor income.[15] To aid interpretation, the description in the table standardizes the price by its average in 1979, and then multiplies it by my estimate of average household labor income for each year. In the period 1980-2000, the policy price averages $3,319$ dollars (2020), after the appropriate exchange-rate and inflation adjustments.[16] Before multiplying the price by my estimate of average household labor income, it averages two years of 1979 household labor income.

I match a panel of prefecture mayors for the years between 1990 and 2000 collected by Suárez-Serrato et al. (2019) to the sample. These authors digitized city gazetteers and yearbooks to obtain the variables described in Panel (e) of Table 2. Their data contain a measure of mayoral performance at implementing aggregate fertility objectives (net target birth rate less net realized birth rate),[17] and an indicator of being an experienced mayor ($>$ 2.5 years in office). I also construct and match a dataset characterizing the economic environment using two sources: province-level, year-specific data from Chinese Statistical Yearbooks available in China Data Center (2017) and village or town, year-specific data from CHARLS. Appendix Table A.8 provides a full description. I construct variables describing aggregate employment, education, agriculture, trade, and others. I elaborate on these variables in the empirical analysis.

[15]After accounting for the policy exemptions, the price is zero for 24% of the (longitudinal) sample observations. My empirical analysis uses the price in logarithmic form. I follow the common practice of adding 1 to the price before taking the natural log of the price throughout the paper. This transformation enables straightforward interpretation of my estimates as elasticities, but is not necessarily innocuous. I justify this decision by comparing results using prices in logarithmic form and prices in levels in the Appendix. The checks show that my main estimates remain stable when calculating elasticities from the estimates obtained using regressions in levels. The reason for this robustness is that the distribution of the prices is tight, containing most of the values greater than zero in the interval $(0, 3]$. An alternative is to estimate specifications using geometric transformations. I do not explore this alternative given that the regressions in levels yield results that are very similar to the results obtained from regressions using the logarithmic transformation that I employ.

[16]This price amounts to 13,898 if adjusted for purchasing-power parity. I round this adjusted price to 13,900 in the abstract of the paper.

[17]The birth rate targets that the mayors intended to impose were part of the Five-Year Plans of the local Communist Party. The net birth rate is the crude birth rate minus the crude death rate. Suárez-Serrato et al. (2019) digitized the Provincial Five-Year Plans in 1985, 1990, and 1995. A province's 1985 plan set the target for 1986–1990, and similarly for the 1990 and 1995 plans. The years documented between 1985 and 1990 by Suárez-Serrato et al. (2019) are sparse and that impedes estimating Equation (2) below. I thus only consider the panel of mayors for the years between 1990 and 2000.

2.4 Validation of the Pricing System's Empirical Characterization

Before turning to the main analysis, I provide two validation exercises of the pricing system described so far.

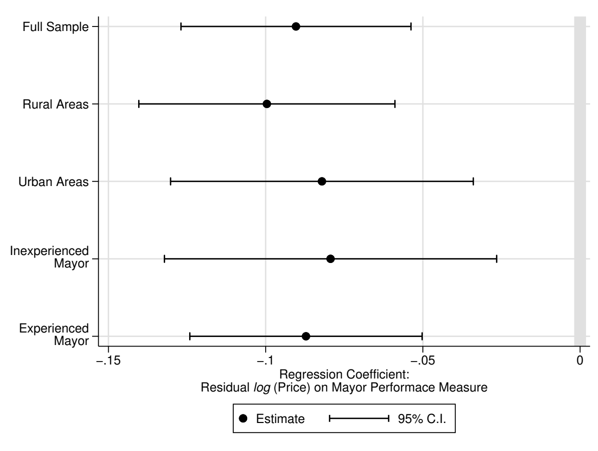
Policy Prices and the Official Fertility Targets. I provide suggestive evidence indicating that the pricing system documented above was used by local officials to regulate fertility. I consider the model

$$\ln\left[\Xi_{ia}\right] = \text{constant} + \zeta_{\text{performance}} \cdot \text{mayor performance}_{ia-1} + \eta_{ia}, \qquad (2)$$

where mayor performance$_{ia-1}$ is the measure of mayoral performance in Panel (e) of Table 2 and η_{ia} is an error term. A positive correlation (i.e., $\zeta_{\text{performance}} > 0$) between the price Ξ_{ia} and the performance of the mayor during the previous year is consistent with the pricing system reflecting fluctuations in aggregate phenomena related to fertility. For example, prefectures where fertility is low could be those in which prices are high. This would simply be a reflection of an aggregate taste for smaller families and not necessarily a stringent policy. A negative correlation (i.e., $\zeta_{\text{performance}} < 0$) aligns with the pricing system being used to enforce the trade-off between conforming to national policy and mitigating social discontent discussed in Section 2.1. For instance, if a mayor performs well at imposing low fertility in a given year, he would then dictate a lower price for the following year.

Figure 3 provides various estimates indicating that the correlation is negative. I standardize the mayor-performance measure to a mean of 0 and a standard deviation of 1. The full-sample estimate indicates that a one-standard deviation performance increase in the previous period is associated with an average decrease in the individual-level policy price of almost 10%. When obtaining these estimates, I include age × year × province fixed effects and drop the constant in Equation (2). I do this because my preferred specification for obtaining the main results below includes these fixed effects, which enable accounting for

Figure 3. Policy Prices and the Performance of Mayors at Imposing Fertility Targets



Note: This figure displays estimates of $\zeta_{\text{performance}}$ in Equation (2) (i.e., the regression coefficient associated with a measure of the performance of mayors at imposing fertility targets when regressing the natural log of the policy price for having a second or a third child ($\ln[\Xi_{ia}]$) on the performance measure). The performance measure is the target net birth rate less the realized net birth rate during the year before the price is dictated by the prefecture mayor. The details of the full-sample regression are: N (individuals) = 2,400, Average A_i (periods observed) = 5.34, $R^2 = 0.95$. Robust 95% confidence intervals (province × birth-year clustered) surround point estimates. **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1990 and 2000.

temporal variation in province-specific fertility patterns.

Comparison to an Alternative Measure of the Pricing System. The empirical characterization of the pricing system described so far is prone to measurement error (recall that it involves assumptions about the discount rate and the evolution of household income). Further, the policy implementation could have differed from what historical documentation indicates—e.g., local authorities could have been corrupt or more ruthless at implementing the policy than what official documents acknowledge.

I use a nationally representative survey with 443 village and town government officials in 27 provinces of China that was conducted by CHARLS in 2011 as a complement to its household surveys. The survey asked village and town officials to provide a retrospective annual estimate of the total amount that a woman had to pay in order to get a permit to have a second or third child. Put differently, the survey asked the officials to provide a village- or town-specific time series of estimates of Ξ_{ia}. I match these estimates by village or town and year to each woman in the sample and denote them by $\widetilde{\Xi}_{ia}$.[18]

I convert $\widetilde{\Xi}_{ia}$ to 2020 dollars using the same conversion procedure applied to Ξ_{ia}. Panel a. of Table 2 indicates that the two measures of the prices closely align. A strong empirical relationship between Ξ_{ia} and $\widetilde{\Xi}_{ia}$ is a useful method for corroboration given the independence of the sources. I test the strength of this relationship by estimating the model

$$\ln\left[\Xi_{ia}\right] \;=\; \text{constant} + \zeta_{\text{price}} \cdot \ln\left[\widetilde{\Xi}_{ia}\right] + \eta_{ia}, \tag{3}$$

where I reuse the notation from above. When estimating this model, I also include age × year × province fixed effects and drop the constant. Figure 4 displays the estimation details and plots the associated binned scatterplot. The difference in the R^2 of a model allowing $\zeta_{\text{price}} \neq 0$ and the R^2 of a model imposing $\zeta_{\text{price}} = 0$ is eight percentage points. The residual relationship between the two measures of the pricing system is remarkably strong.

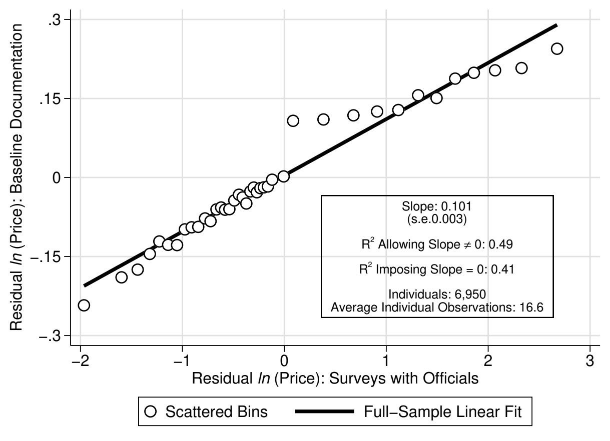
3. Policy-Price Elasticities

Event Study. I use an event study for an initial exploration of the relationship between the pricing system and fertility. Let n_{ia} denote the number of children of woman $i \in \mathcal{I}$ at age $a \in \mathcal{A}_i$. Her flow of children is $\Delta n_{ia} := n_{ia} - n_{ia-1}$: the first difference of her stock. Recall from Section 2 that a woman was required to register her pregnancy by the end of the first quadrimester. If she already had one or two children and decided to continue with the pregnancy, she needed to obtain a permit. The price of this permit was set by the policy when she was $a-1$ years old. This is the policy price that most immediately impacts her flow of fertility at age a. I denote its first difference by $\Delta\Xi_{ia-1}$ when using the price based on documentation and $\Delta\widetilde{\Xi}_{ia-1}$ when using the price based on surveys.[19]

[18] The survey asked the officials for estimates of the value of Ξ_{ia} for the general population. It also asked them for estimates for ethnic minorities and for women with first-child girls. The survey did not ask for estimates for the other cases in Table 1. For these other cases, I apply the exemptions to the policy and set $\widetilde{\Xi}_{ia}$ to zero in the cases that I set Ξ_{ia} to zero. In the empirical analysis, I apply to $\widetilde{\Xi}_{ia}$ the same logarithmic transformation applied to Ξ_{ia}.

[19] For a small number of women getting pregnant late in the calendar year, the price at age a could be the most immediately relevant. This imprecision is another motivation to account for measurement error.

Figure 4. Relationship Between Two Measures of the Policy Prices



Note: This figure displays the binned (residual) relationship between the two policy-price measures in natural logs. It also presents details from the full-sample estimation of the model in Equation (3). The slope is an estimate of ζ_{price}. The standard error (s.e.) is robust and province × birth-year clustered. The number of bins in the scatterplot is calculated using the procedure in Cattaneo et al. (2019). **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000.

I classify a woman i of age a as facing a price spike if the first difference of the policy price when she is $a-1$ years old is above the 80$^{\text{th}}$ percentile of the in-sample distribution of the price first difference. I relabel age to event time, denoted by s, so that years are counted relative to when the price spike occurs. Then, I consider event-time periods $s = -5, \ldots, 5$ to estimate the model

$$\Delta n_{is} \;=\; \text{constant} + \sum_{\substack{s\in\{-5,\ldots,5\}\\ s\neq -1}} \gamma_s \cdot \mathbf{1}\left[\text{ event time } = s\right]_s + \varepsilon_{is}, \qquad (4)$$

where γ_s is the average of Δn_{is} s years from a price spike. ε_{is} is an error term. The event study is based on variables in first differences, so individual or woman fixed effects are implicit in this model. Additionally, I residualize the flow of children and the price first differences from age × year × province fixed effects using ordinary least squares (OLS) in the full sample before defining the price spike and estimating the model. Price spikes are woman-specific. The age at which they occur varies across women, but not within. The coefficients γ_s are thus relative to $s = -1$ and identified off within-woman price variation.[20,21]

I estimate the model in Equation (4) two times; one time defining price spikes using $\Delta\Xi_{ia-1}$ (documentation) and one time using $\Delta\widetilde{\Xi}_{ia-1}$ (surveys). Figure 5 displays the estimates of γ_s. In both cases, a price spike generates a sudden and sustained average decrease of approximately 0.12 standard deviations in the average (residual) flow of children. The figure suggests that innovations in the pricing system, occurring through policy changes that generate within-woman price variation, are potentially exogenous to the fertility process. It thus accumulates evidence against women manipulating their eligibility to qualify for exemptions or timing their fertility process to face lower prices (e.g., opting for an abortion in a given period to get a permit for a lower price in the future). If this manipulation had happened, their response to price spikes would not appear as sudden and sustained in the figure.[22] I now explore a parsimonious specification allowing me to interpret the relationship between the pricing system and the flow of children as a price elasticity.

Empirical Framework and Main Estimates. I postulate the model

$$\Delta n_{ia} = \text{constant} + \gamma_{\Xi} \cdot \ln\left[\Xi_{ia-1}\right] + \gamma_{n-1} \cdot n_{ia-1} + \varepsilon_{ia}, \tag{5}$$

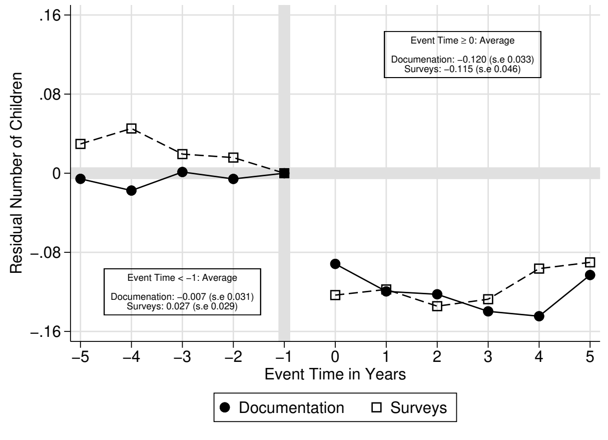
where Δn_{ia}, Ξ_{ia-1}, and n_{ia-1} are defined as before, ε_{ia} is an error term, and γ_{Ξ} and γ_{n-1} are coefficients. In this model, n_{ia-1} summarizes the dependency of the fertility process

[20] I consider at most eleven periods ($s = -5, \ldots, 5$) per woman (recall that the panel is unbalanced). I drop observations before -5 or after 5. Only a handful of women in the sample face more than one price spike. When this occurs, I consider the first spike for estimating the model in Equation (4). Appendix Figure A.4 shows that defining the events using other percentiles yields the same conclusions as those summarized by Figure 5. Only a handful of women do not face a price spike based on either Ξ_{ia-1} or $\widetilde{\Xi}_{ia-1}$ included in the exercise discussed here or in the exercises of Appendix Figure A.4.

[21] There is individual-level variation in the timing of treatment in this design. Recent research points out that variation in the timing of treatment compromises the interpretation of difference-in-difference designs constructed in a way similar to Equation (4) (e.g., Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021). My identification strategy below is not based on difference-in-differences. Figure 5 only aims to illustrate a trend break in the (residual) flow of children after a price spike. The stability at 0 before the spike could be driven by across-individual averaging out of increasing and decreasing trends, as pointed out by Sun and Abraham (2021). However, the objective of Figure 5 is only to illustrate the break after the price spike, and not to show that trends are parallel before such a spike.

[22] Examples of studies using price spikes to justify exogeneity in prices determined by policies include Simon (2016) and Fuest et al. (2018).

Figure 5. Number of Children After Policy-Price Spikes, Event Studies



Note: This figure displays estimates of γ_s in Equation (4) (i.e., the average residual flow of children s periods from a policy-price spike). Before constructing the price-spike indicators and estimating Equation (4), I residualize the flow of children and the natural log of the price first differences from age × year × province fixed effects using ordinary least squares in the full sample. The residualized flow of children is standardized to a mean of 0 and a standard deviation of 1. The figure displays results from two estimation exercises, one defining the price spike using the prices based on documentation and one defining the price spike using the prices based on surveys. The standard errors displayed are bootstrapped and province × birth-year clustered. **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000.

on policy prices realized before $a-1$. Conditional on n_{ia-1}, Ξ_{ia-1} is the policy price that most immediately affects the flow of children at age a. To interpret the model, assume for a moment that ε_{ia} is mean independent and that $\gamma_{n-1}=0$. If this holds, γ_Ξ is a semi-elasticity (i.e., the percentage-point change in the probability of having a child upon a 1% increase in the policy price Ξ_{ia-1}).[23] $\gamma_\Xi/\mathbb{E}[\Delta n_{ia}]$ is the corresponding policy-price elasticity in expectation. If $\gamma_{n-1}\neq 0$ and mean independence still holds, the semi-elasticity and elasticity are conditional (they hold fixed the stock of children n_{ia-1}). If mean independence does not hold, solutions to endogeneity are required to maintain a causal interpretation.

I estimate the model in Equation (5) including all women in the sample independently

[23]To see this, note that mean independence and $\gamma_{n-1}=0$ imply that $\mathbb{E}[\Delta n_{ia}|\Xi_{ia-1}]=\gamma_\Xi\cdot\ln[\Xi_{ia-1}]$. Consider two prices Ξ_1 and Ξ_2 with $\Xi_1/\Xi_2=1.01$ (i.e., Ξ_1 is 1% larger than Ξ_2). Then, $\underbrace{(\mathbb{E}[\Delta n_{ia}|\Xi_{ia-1}=\Xi_1]-\mathbb{E}[\Delta n_{ia}|\Xi_{ia-1}=\Xi_2])\times 100}_{\text{percentage-point change in the flow of children}}=\gamma_\Xi\cdot\underbrace{\ln(1.01)\times 100}_{\approx 1}\approx\gamma_\Xi$. If Δn_{ia} is an indicator of having a child, it follows that γ_Ξ is the percentage-point change in the probability of having a child upon a 1% price increase (recall that twins and higher-order births are minimal in the sample, so I reclassify Δn_{ia} as an indicator of whether woman i has a child at age a).

of their stock of children n_{ia-1}. If $n_{ia-1} = 0$, woman i does not need to pay a price associated with the policy when having a child at age a. Still, Ξ_{ia-1} is observed both by her and the econometrician. That is, both she and the econometrician observe the price that she would have needed to pay if she had at least one child, given her demographic characteristics and the calendar year when she is a years old. Including women with $n_{ia} = 0$ in the estimation allows the current policy price to affect all margins determining completed fertility and the total fertility rate.

I construct a preferred specification for estimating Equation (5) progressively in Panel a. of Table 3. I first use a Logit model because the dependent variable is binary. Instead of displaying $\hat{\gamma}_\Xi$ in the table, I show the corresponding marginal effect at the mean of Ξ_{ia-1} and n_{ia-1}. The estimates obtained using the analogous linear-probability specification are in the next column (OLS). The Logit and OLS specifications yield essentially the same estimates (a policy-price semi-elasticity of 0.03 and a corresponding elasticity of 0.4). I thus use linear-probability specifications henceforth, as they enable convenient, well-known solutions to the challenges discussed next. Transforming the model to

$$n_{ia} = \text{constant} + \gamma_\Xi \cdot \ln\left[\Xi_{ia-1}\right] + \left(1 + \gamma_{n-1}\right) \cdot n_{ia-1} + \varepsilon_{ia}, \tag{6}$$

provides additional flexibility and yields identical estimates.[24] The third column of Panel a. of Table 3 verifies the equivalence of the two models by showing OLS estimates based on the transformed model.

The estimates discussed so far are based on specifications without controls. Age, temporal, or unobserved regional components such as market conditions determining the costs of having children could correlate with prices and introduce omitted-variable bias. Other aspects like local aggregate preferences could also shift fertility, correlate with prices, and, if

[24]The transformation uses the definition of $\Delta n_{ia} := n_{ia} - n_{ia-1}$ and moves the term n_{ia-1} to the right-hand side of Equation (5). I subtract 1 from estimates of $(1 + \gamma_{n-1})$ to obtain estimates of γ_{n-1} from Equation (6).

Table 3. Policy-Price Semi-Elasticities: Main Specification and Robustness Checks

Panel a. **Main-Specification Construction.**							
	Logit	OLS	Transformed	Fixed Effects	System GMM	Additional Stock Lags	Measurement Error
Semi-Elastcity: $\hat{\gamma}_\Xi$	-0.027	-0.031	-0.031	-0.016	-0.031	-0.030	-0.052
(s.e.)	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.006)	(0.007)
Elasticity	-0.372	-0.426	-0.431	-0.218	-0.440	-0.480	-0.724
(s.e.)	(0.027)	(0.032)	(0.033)	(0.032)	(0.086)	(0.091)	(0.094)
$\hat{\gamma}_{n-1}$	-0.030	-0.030	-0.031	-0.020	-0.156	-0.117	-0.150
(s.e.)	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.007)	(0.007)
N	6,950	6,950	6,950	6,950	6,950	6,666	6,950
Average A_i	16.550	16.550	16.550	16.550	15.549	13.121	15.549
Panel b. **Robustness to Controls and Simultaneity of (Proportional) Prices and Income.**							
	Flow of Children					Change in Labor Force Particiation	
	Current	*+ 1-Year Lags*	*+ 2-Year Lags*	*Proportional Price*	*Lump-Sum Price*	*No Lagged Fertility*	*Lagged Fertility*
Semi-Elastcity: $\hat{\gamma}_\Xi$	-0.050	-0.049	-0.049	-0.052	-0.047	-0.003	-0.003
(s.e.)	(0.007)	(0.007)	(0.007)	(0.009)	(0.009)	(0.002)	(0.002)
Elasticity	-0.701	-0.684	-0.689	-0.678	-0.885	N/A	N/A
(s.e.)	(0.097)	(0.095)	(0.095)	(0.120)	(0.164)		
$\hat{\gamma}_{n-1}$	-0.149	-0.149	-0.148	-0.152	-0.121		-1.000
(s.e.)	(0.007)	(0.007)	(0.007)	(0.009)	(0.016)		(0.001)
N	6,939	6,939	6,939	5,793	2,572	6,094	6,094
Average A_i	15.559	15.559	15.559	13.979	10.529	15.582	15.582

Panel a.: The column Logit shows Logit estimates of the marginal effects associated with the policy price and the stock of children in Equation (5). The column OLS displays ordinary-least squares (OLS) estimates of γ_Ξ and γ_{n-1} in Equation (5). The column Transformed displays (OLS) estimates of these same coefficients based on the model in Equation (6). The column Fixed Effects is obtained as the previous column, but I include include age $\times$ year $\times$ province fixed effects in the OLS estimation. The column System GMM displays estimates of γ_Ξ and γ_{n-1} based on Equation (6) and the system of moment conditions explained in the text, which aim to tackle the endogeneity of n_{ia-1}. The column Adding Stock Lags is obtained as the previous column, but I include up to five lags of the stock of children in the model of Equation (6). The column Measurement Error is obtained as the column System GMM, but I add the additional moment conditions to address measurement error discussed in the text into the system of moment conditions. The last three columns residualize the variables from age $\times$ year $\times$ province fixed effects before estimation. Each column displays the semi-elasticity as well as the elasticity, which divides the semi-elasticity by the empirical counterpart of the expectation of the flow of children $\mathbb{E}[\Delta n_{ia}]$. Each column displays standard errors for each estimate (s.e.), the number of women in the sample (N), and the average of A_i (the number of periods that each woman is observed). For estimates based on Logit and OLS, standard errors are robust and clustered at the province $\times$ cohort level. For estimates based on moment-condition estimators, standard errors are robust to heteroskedasticity and finite-sample corrected as in Windmeijer (2005). **Panel b.:** All specifications under the subpanel Flow of Children are obtained as the column Measurement Error of Panel a. The first three columns include (1) current, (2) current and 1-year lagged, and (3) current, 1-year lagged, and 2-year lagged controls describing the economy (see Section 2). The following two columns provide separate estimates for women facing lump-sum or proportional policy prices. The last two columns are analogous in format to the last column of Panel a., but replace the dependent variable with an indicator of labor force participation. **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000.
N/A: Not available or defined for corresponding model.

omitted, also introduce bias. Multiple conjectures about how these unobserved components enter the fertility process are plausible. It is thus necessary to account for them comprehensively. The relatively large number of observations and periods observed for each woman in the sample allow me to consider a datum-intensive specification with age × year × province fixed effects. The column Fixed Effects displays OLS estimates that include these fixed effects when estimating the model in Equation (6). The parameter estimates change, making it relevant to keep these fixed effects in the remainder of the paper.

I then tackle a major source of potential endogeneity. The stock of children n_{ia-1} in Equation (6) violates mean independence by construction. This biases the estimates discussed so far. I consider a generalization of an estimator that accounts for stock endogeneity in dynamic panel models proposed by Arellano and Bond (1991). These authors propose a general method of moments (GMM) estimator by first differencing Equation (6) and exploiting the condition $\mathbb{E}\left[n_{ia-b} \cdot \Delta\varepsilon_{ia}\right] = 0$ for $a \geq 3$ and $b \geq 2$.[25] The restrictions on a and b guarantee that the moment condition holds as long as $\mathbb{E}\left[\Delta \ln \Xi_{ia-1} \cdot \Delta\varepsilon_{ia}\right] = 0$, for which Figure 5 provides evidence. The generalization of the Arellano and Bond (1991) estimator that I employ considers the additional moment condition $\mathbb{E}\left[\Delta n_{ia-1} \cdot \varepsilon_{ia}\right] = 0$ for $a \geq 3$, as in Blundell and Bond (1998). Including the additional moment condition is recommended when working with persistent processes because some of the lagged outcomes n_{ia-b} may be weak instruments.[26] The column System GMM shows the estimates exploiting all of the moment conditions. The next column presents estimates based on the same estimator and model, except up to five lags of the stock of children are included. The similarity of the estimates, with the inclusion of the additional lags, discards the omission of higher-order

[25]Two practical aspects of this estimator are worth clarifying. First, its consistency properties hold when the number of individuals is large and the number of periods in which these individuals are observed is fixed. The sample analyzed satisfies this requirement. Second, exploiting the moment condition $\mathbb{E}\left[n_{ia-b} \cdot \Delta\varepsilon_{ia}\right] = 0$ for $a \geq 3$ and $b \geq 2$ requires observing at least three periods for each women in the sample. All of the 6,950 women in the sample satisfy this requirement. If more lags are considered in Equation (6), more periods of individual observation are required. The average number of periods in which women are observed in the sample is 16.5, which allows including multiple lags with little loss of individual observations.

[26]Table 3 provides evidence of the persistence of the fertility process. Across specifications, $(1 + \hat{\gamma}_{n-1})$, the coefficient estimate characterizing persistence, is at least 0.84.

persistence in the fertility process.[27]

Logit and OLS estimates of the policy price semi-elasticity based on Equation (6) are similar. They halve in magnitude when accounting for age × year × province fixed effects. They increase to a similar magnitude to that of the Logit and OLS estimates once the potential endogeneity of n_{ia-1} is taken into account. The System-GMM estimator is based on the first-difference of Equation (6). It thus directly communicates with the evidence in Figure 5, which indicates that the policy price variation identifying the semi-elasticities is plausibly exogenous.

The estimates discussed so far only use the policy price based on documentation, Ξ_{ia}. I exploit the price based on surveys, $\widetilde{\Xi}_{ia}$, to account for potential measurement error in Ξ_{ia} by including the moment conditions $\mathbb{E}\left[\Delta \ln \widetilde{\Xi}_{ia} \cdot \Delta\varepsilon_{ib}\right] = 0$ and $\mathbb{E}\left[\ln \widetilde{\Xi}_{ia} \cdot \varepsilon_{ib}\right] = 0$ for $b \geq a$ in the System GMM.[28] The validity of these additional moment conditions rests on prices being plausibly exogenous (Figure 5) and on the close relationship between the two measures of the prices (Figure 4). The validity of the moment conditions used to account for the endogeneity of the stock of children rests on the structure of Equation (6) and on the persistence of the fertility process. The last column of Panel a. of Table 3 shows the estimates. The policy price semi-elasticity increases in magnitude and indicates that the measurement-error correction is relevant.

The last column of Panel a. of Table 3 presents my preferred estimates. They are based on a System GMM estimation of Equation (6) that includes age × year × province fixed effects, accounts for the endogeneity of the stock n_{ia-1}, and addresses measurement error in Ξ_{ia-1}.[29] The System-GMM estimator is based on the first difference of Equation (6).

[27]When using GMM, I employ the standard two-step efficient weighting of the system of moment conditions.

[28]Note that these moment conditions are weaker than strict exogeneity. Identification is possible under these weaker conditions because of the rest of the additional moment conditions in the system discussed before.

[29]The fact that the System-GMM estimator yields similar estimates to the OLS and Logit estimator when not accounting for measurement error does not imply an instrumental-variable estimator correcting for measurement error suffices to tackle the challenges at hand. My preferred System-GMM estimator accounts for all of the issues *simultaneously*. Using instrumental variables without solving the endogeneity of the stock

It thus implicitly differences out woman fixed effects. Identification of the semi-elasticities thus relies on within-woman price variation. Panel b. of Table 3 provides checks for this specification and addresses the robustness of the corresponding System GMM estimator. I provide estimates that include time-varying (1) current, (2) current and 1-year lagged, and (3) current, 1-year lagged, and 2-year lagged controls describing the state of the economy when women make fertility decisions. These controls are based on a comprehensive set of economic indicators that are classified by category, and then dimension-reduced into principal factors. They vary within age × year × province cells because they contain village and town information (see Section 2). The estimates of the semi-elasticities are insensitive to including these controls.

Section 2 documents that, for some women, the policy prices were charged as a fraction of household labor income. For them, the price variation could reflect fluctuations in labor income and not only policy intensity. In standard models of fertility (e.g., Hotz et al., 1997), an increase in labor income would disincentivize fertility by raising the opportunity cost of having children. An increase in labor income would generate an increase in the policy price and make a negative relationship between this price and fertility spurious. However, household labor income is unlikely to drive the price variation that I exploit. This variation differences out individual fixed effects and age-year regional economic characteristics. Further, governmental policy largely fixed the individual-level labor supply (see Section 2). Women who faced lump-sum permit prices help to verify this. For them, there is no potential spurious relationship between the prices and fertility induced by labor income. Panel b. of Table 3 shows that the policy semi-elasticity is virtually the same across the subsamples of women who faced proportional and lump-sum permit prices. Furthermore, when estimating a model analogous to Equation (6) with full-time labor force participation as the dependent variable, there is a precise, null relationship between this variable and the policy price.

My preferred estimate (the last column of Panel a. of Table 3) indicates that a 1% in-

is an incomplete solution that leads to different point estimates than those that I present as preferred.

crease in the policy price decreases the probability of having a child by $\hat{\gamma}_{\Xi} = 0.052$ percentage points. I reject the null hypothesis of this semi-elasticity differing from 0 when employing standard significance levels.[30] The corresponding elasticity is $\gamma_{\Xi}/\mathbb{E}\left[\Delta n_{ia}\right]$. I estimate this to be 0.724 (s.e. 0.094).

Bergsvik et al. (2021) review the literature analyzing policies targeting fertility since 1970 in several countries. They classify the policies into childcare, health services, parental leave, and universal transfers. My study relates to universal transfers (i.e., policies directly shifting the price-tag of having a child, as opposed to indirectly subsidizing it). Milligan (2005) reports that a 1% increase in lump-sum fertility benefits increases the rate at which women have children by 0.107%. He exploits a reform in Quebec, Canada during the 1990s when estimating this policy-benefit elasticity. His estimate is consistent with the cross-country policy-benefit elasticity of 0.16 for the period 1970-1990, as documented by Gauthier and Hatzius (1997). It is also consistent with the policy-benefit elasticity ranging between 0.05 and 0.11 reported by Zhang et al. (1994), who analyze Canadian policies in the period 1921-1988. Whittington et al. (1990) report similar elasticities for the United States for the period 1913-1984. Their estimates range between 0.12 and 0.24. My estimate of the elasticity is greater in magnitude. Still, it indicates that fertility responds inelastically to benefits and (negative) transfers, even when the government has thorough control over economic decisions.[31]

Demographic Heterogeneity. Table 4 shows estimates for different subgroups of the population. I discuss the three main patterns. Women in rural areas or with agricultural *hukou* face lower policy prices by design (they were exempted from paying prices more often,

[30] The standard errors in the last column of Panel a. of Table 3 are robust to heteroskedasticity as in White (1980). They include the finite-sample correction of Windmeijer (2005), which is relevant when using two-step GMM. The standard error of the semi-elasticity 0.052 is 0.007. Alternatively, I compute bootstrap standard errors clustered at the province $\times$ cohort level and at the province level and find that they are 0.005 and 0.006, respectively.

[31] Inelastic policy-benefit responses have been documented in Australia, Canada, Europe, the United States, and the United Kingdom (Bergsvik et al., 2021). Studies documenting related elasticities in France, Germany, Israel, and Spain are Cohen et al. (2013), Ebenstein et al. (2016), Gathmann and Sass (2018), González (2013), Laroque and Salanié (2014), and Manski and Mayshar (2003).

and that lowers their average price). Their decrease in the probability of having a child upon a 1% increase in the policy price is not too far off from the analogous decrease for women in urban areas. However, their elasticities are lower than the elasticities of urban women because urban women have a lower average flow of fertility at any given time. For urban women, the probability of having a child is more price-sensitive in percentage terms. As a consequence of policy design, the price increases as a function of education. More educated women are more price elastic. This greater elasticity results from their lower propensity to have children and lower average flow of children at any given time. Finally, the prices increase over time as discussed in Section 2. The price elasticity displays the same pattern, which is not necessarily due to a greater price sensitivity given a fixed average flow of children. Instead, the decreasing secular trend in fertility generates an increasing price sensitivity in percentage terms.

Age Profiles by Gender. I now use my preferred specification of Equation (6) and the corresponding System-GMM estimator replacing the number of children n_{ia} with the number of girls or the number of boys. This allows me to display the policy-price semi-elasticities of the flow of girls or boys in Panel a. of Figure 6. For each age between 20 and 40 years old, I estimate the semi-elasticity using the observation at that age, as well as observations from five periods before and five periods after. I include the periods before and after to exploit within-woman price variation. A 1% increase in the policy price has virtually no effect on the probability of having children. The semi-elasticity estimates are precisely estimated and very close to 0. A 1% increase in the policy price decreases the probability of having a girl by 0.1 percentage points at age 20. This semi-elasticity decreases in magnitude as women age. There are two plausible explanations. First, households are more resource-constrained and thus more price-sensitive when women are relatively young. Second, as women age their natural propensity to have children decreases and restrictions imposed by the policy prices become less relevant.

Panel b. of Figure 6 investigates further the gendered impact of the policy. I only

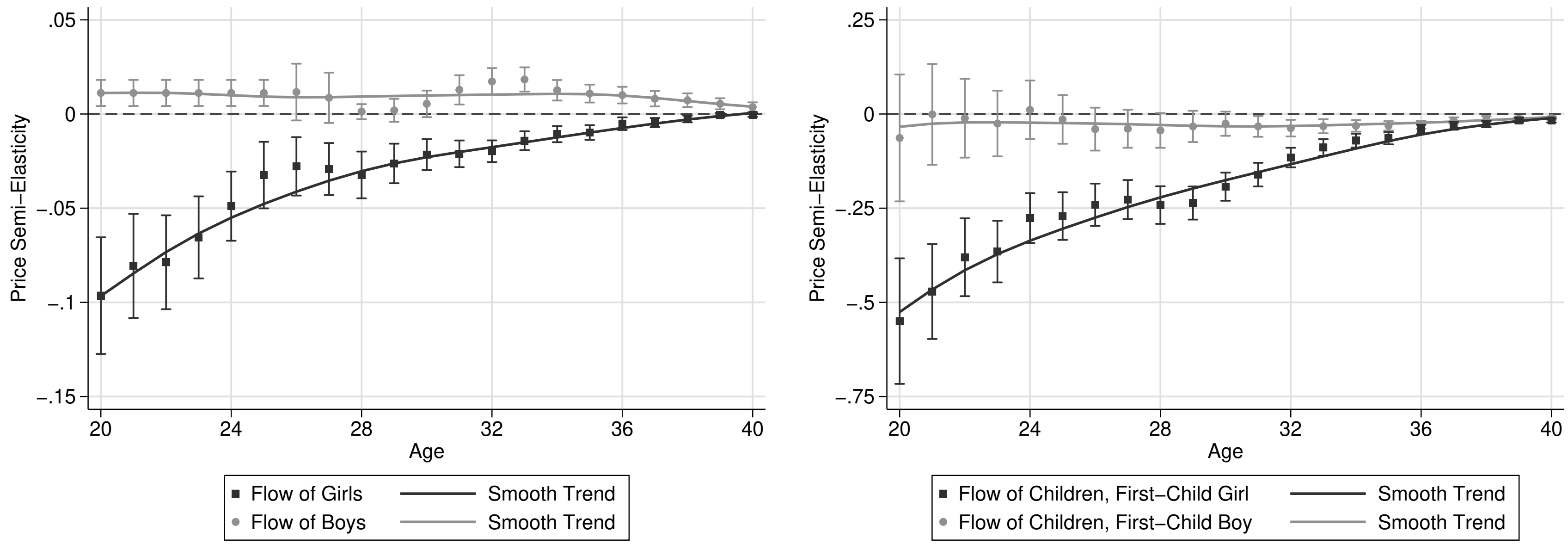
Table 4. Policy-Price Elasticities: Heterogeneity

Panel a. **Sector.**				
	Rural	Urban	Agricultural	Non-Agricultural
Semi-Elastcity: $\hat{\gamma}_{\Xi}$	-0.060	-0.060	-0.050	-0.095
(s.e.)	(0.008)	(0.015)	(0.007)	(0.022)
Elasticity	-0.761	-0.942	-0.640	-1.886
(s.e.)	(0.103)	(0.229)	(0.093)	(0.443)
Price (2020 USD)	3,136	3,620	3,276	3,496
N	4,369	2,581	5,704	1,201
Average A_i	15.680	15.327	15.628	15.204
Panel b. **Ethnicity and Employment Source.**				
	Non-Han	Han	Not SE Worker	SE Worker
Semi-Elastcity: $\hat{\gamma}_{\Xi}$	-0.090	-0.054	-0.050	-0.113
(s.e.)	(0.033)	(0.007)	(0.007)	(0.109)
Elasticity	-1.017	-0.753	-0.656	-1.900
(s.e.)	(0.372)	(0.099)	(0.096)	(1.830)
Price (2020 USD)	2,363	3,374	3,271	3,580
N	419	6,531	5,599	297
Average A_i	15.594	15.546	15.596	15.165
Panel c. **Economic Hardship and Male Presence in the Family.**				
	No Job Difficulty	Job Difficulty	No Male Scarcity	Male Scarcity
Semi-Elastcity: $\hat{\gamma}_{\Xi}$	-0.055	-0.030	-0.052	-0.065
(s.e.)	(0.007)	(0.048)	(0.008)	(0.022)
Elasticity	-0.756	-0.339	-0.698	-0.943
(s.e.)	(0.093)	(0.534)	(0.104)	(0.318)
Price (2020 USD)	3,332	2,863	3,367	2,663
N	6,651	296	6,077	576
Average A_i	15.571	15.003	15.685	14.983
Panel d. **Education.**				
	None	Elementary	Middle School	High School
Semi-Elastcity: $\hat{\gamma}_{\Xi}$	-0.051	-0.125	-0.136	-0.160
(s.e.)	(0.009)	(0.018)	(0.019)	(0.031)
Elasticity	-0.750	-1.565	-1.685	-2.174
(s.e.)	(0.128)	(0.231)	(0.240)	(0.428)
Price (2020 USD)	3,222	3,463	3,459	3,457
N	3,885	1,744	1,645	502
Average A_i	14.792	17.283	17.380	17.857
Panel d. **Calendar Years.**				
	1980-1984	1985-1989	1990-1994	1995-2000
Semi-Elastcity: $\hat{\gamma}_{\Xi}$	-0.012	-0.069	-0.053	-0.023
(s.e.)	(0.013)	(0.027)	(0.009)	(0.006)
Elasticity	-0.104	-0.642	-1.064	-1.610
(s.e.)	(0.117)	(0.255)	(0.179)	(0.440)
Price (2020 USD)	2,998	2,855	3,510	4,022
N	6,783	6,584	5,886	4,785
Average A_i	3.762	4.758	4.614	5.028

Note: Each subpanel displays estimates of γ_{Ξ} and γ_{n-1} based on Equation (6) and the system of moment conditions explained in the text, which aim to tackle the endogeneity of n_{ia-1} and correct for measurement error (i.e., each subpanel is analogous to the last column of Panel a. of Table 3). I residualize the variables in the estimation from age × year × province fixed effects in the pooled sample before estimation of the subpanels. Each subpanel displays the semi-elasticity as well as the elasticity, which divides the semi-elasticity by the empirical counterpart of the expectation of the flow of children $\mathbb{E}[\Delta n_{ia}]$. Each subpanel displays standard errors for each estimate (s.e.), the number of women in the sample (N), and the average of A_i (the number of periods that each woman is observed). The standard errors are robust to heteroskedasticity and finite-sample corrected as in Windmeijer (2005). **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000. *SE*: state-owned enterprise. *Job Difficulty:* one spouse is disabled and cannot work, a peasant couple lives in sparsely settled mountains, reclamation, or border areas, one spouse has constantly been working in underground mining for more than five years, and the couple has real economic difficulties or claims other (related) peculiar reasons. *Male Scarcity:* one spouse or both spouses are single children, only one child or one son has been born to a family for two generations, and the husband settles in the family of his wife which has daughters and no sons.

Figure 6. Policy-Price Semi-Elasticities: Age Profiles

(a) Full Sample **(b)** At Least One Child



Panel (a): Each point displays an estimate of γ_{Ξ} based on Equation (6) with either the number of boys or the number of girls replacing the number of children in the model. The estimates are based on the system of moment conditions explained in the text, which aim to tackle the endogeneity of the stock of children and correct for measurement error. To obtain each estimate, I include in the sample the period in the label, the five periods before, and the five periods after. I residualize the variables in the estimation from age × year × province fixed effects in the pooled sample before estimation. The estimates are surrounded by their 95% confidence interval, constructed using standard errors that are robust to heteroskedasticity and finite-sample corrected as in Windmeijer (2005). **Panel (b)** is analogous in format to Panel (a). It uses the number of children as the relevant dependent variable and considers women whose first children are girls and women whose first children are boys separately. Women without children are included for the estimation of Panel a. and excluded for the estimation of Panel b. **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000.

consider women with at least one child. Women with at least one child have larger semi-elasticities because policy prices more immediately restrict them. The figure shows that the policy only impacts women whose first children are girls. This follows because women whose first children are boys are fine having only one male child (i.e., they use a stopping rule). Women whose first children are girls are restricted by the policy. They would like to have a boy after having a girl because of cultural norms (Sen, 1991), and also because of economic incentives like having additional land workers at home (Almond et al., 2019). The sex ratio at birth of first children in China is balanced and that holds in the sample analyzed. Panel b. of Figure 6 thus indicates that the policy's contribution to the sex-ratio imbalance is through a distortion of the flow of children for women with at least one child.

Abortions. Longitudinal data on abortions provide evidence consistent with my interpretation of the policy's gendered impact. Table 5 displays estimates using my preferred specification of Equation (6) and the corresponding System-GMM estimator, replacing the number of children with the number of (induced) abortions on both sides of the equation. I also add into the model the stock of female and male children. For estimation, I use moment conditions analogous to those discussed before for the stocks of abortions and female and male children.[32]

Panel a. of Table 5 shows estimates for women whose first children are girls. The estimates of the policy semi-elasticity are noisy, though they imply large elasticities. Their semi-elasticities consistently indicate that a 1% increase in the policy price increases the probability of having an abortion by between 0.04 and 0.06 percentage points. Panel a. of Table 5 shows the analogous estimates for mothers whose first children are boys. These estimates are inconsistent across women with different stocks of children. They generate semi-elasticities and elasticities that are all close to 0. Table 5 indicates that women whose first children are girls sex-select using abortions when restricted by the policy. Sex-selective

[32]The moment conditions are $\mathbb{E}\left[x_{ia} \cdot \Delta\varepsilon_{ia}\right] = 0$ for $x = \{\text{abortions}_{ia-b}, \text{boys}_{ia-b}, \text{girls}_{ia-b}\}$ with $a \geq 3$ and b ≥ 2, the additional moment condition of the System-GMM estimator $\mathbb{E}\left[\Delta\text{abortions}_{ia-1} \cdot \varepsilon_{ia}\right] = 0$ for $a \geq 3$, and the measurement-error correction moment condition on the prices discussed before.

Table 5. Policy-Price Elasticities: Abortions

	Panel a. **First-Child Girl**				***Panel b.*** **First-Child Boy**			
	Stock: 1	*Stock:* $1-2$	*Stock:* ≥ 1	*Stock:* ≥ 2	*Stock:* 1	*Stock:* $1-2$	*Stock:* ≥ 1	*Stock:* ≥ 2
Semi-Elastcity: $\hat{\gamma}_{\Xi}$	0.006	0.006	0.004	0.006	-0.002	-0.004	-0.002	-0.001
(s.e.)	(0.011)	(0.005)	(0.004)	(0.004)	(0.016)	(0.006)	(0.004)	(0.004)
Elasticity	0.371	0.565	0.539	1.199	-0.122	-0.355	-0.258	-0.168
(s.e.)	(0.625)	(0.510)	(0.561)	(0.730)	(0.813)	(0.547)	(0.568)	(1.030)
Price (2020 USD)	3,174	3,134	3,086	3,065	3,562	3,578	3,559	3,558
N	1,554	2,033	2,894	2,605	1,632	2,147	3,012	2,459
Average A_i	4.987	10.795	13.754	12.305	6.420	11.769	13.412	12.167

Note: Each column displays estimates of γ_{Ξ} and γ_{n-1} based on Equation (6), replacing the number of children with the number of abortions in the model. The estimates are based on the system of moment conditions explained in the text, which aim to tackle the endogeneity of the stock of abortions and measurement error. For the estimates in this table, I also add into the model the stock of boys and girls. I residualize the variables in the estimation from age $\times$ year $\times$ province fixed effects in the pooled sample before estimation of the columns. Women without children are not included for the estimation in this table. The classification based on stock labeled in each column is based on the stock of children, independent of the gender composition of the stock. Each column displays the semi-elasticity as well as the elasticity, which divides the semi-elasticity by the empirical counterpart of the expectation of the flow of abortions. Each column displays standard errors for each estimate (s.e.), the number of women in the sample (N), and the average of A_i (the number of periods that each woman is observed). The standard errors are robust to heteroskedasticity and finite-sample corrected as in Windmeijer (2005). **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000.

abortions are known to skew the sex ratio at birth in China (Almond et al., 2019; Chen et al., 2013). Table 5 also indicates that mothers whose first children are boys do not pursue fertility further. Thus, their probability of having an abortion is essentially unaffected by policy price changes.

4. Interpreting the Aggregate Relevance of the Policy Elasticities

I now estimate the aggregate fertility implications of the policy elasticities. Recall that only a handful of women in the sample have twins or higher-order births. I thus reclassify Δn_{ia}, the flow of children, as an indicator of whether woman $i \in \mathcal{I}$ has a child when she is age $a \in \mathcal{A}_i$ years old. $\mathbb{E}\left[\Delta n_{ia}\right]$ is the probability that she gives birth at age a. $\mathbb{E}\left[\Delta n_{ia}\right]$ is also known as the age-a (specific) fertility rate. In principle, only women who are fertile when they are a years old should be considered when estimating $\mathbb{E}\left[\Delta n_{ia}\right]$. However, I do not observe the onset of menarche or menopause. I thus assume that all women are fertile when they are between 15 and 45 years old. In that case, the total fertility rate is

$$\text{total fertility rate} = \sum_{a=15}^{45} \underbrace{\mathbb{E}\left[\Delta n_{ia}\right]}_{\text{age-}a\text{ fertility rate}} . \tag{7}$$

By definition, the cumulative age-a fertility rate (i.e., the sum of all age-specific fertility rates up to age a) at age 45 is the total fertility rate.

Appendix Figure A.1 displays annual estimates of the total fertility rate using the sample analyzed. For estimation, I use the formula in Equation (7) and the women who were between 15 and 45 years old at least during one year between 1979 and 2000. The time series of the estimates closely aligns with the time series of the total fertility rate reported by The World Bank (2017a). I pool calendar years and analyze the sample as representing the main, large cohort of fertile women impacted by the policy. Pooling instead of analyzing annual total fertility rates increases the precision of the counterfactual estimates discussed below. Panel a. of Figure 7 displays estimates of the realized cumulative age-specific fertility rates. The point estimate at age 45 is the total fertility rate in the sample, 2.37 (s.e. 0.01) children.[33]

The model in Equation (5) suggests a straightforward no-policy counterfactual age-specific fertility rate: $\mathbb{E}\left[\Delta n_{ia}\right] - \gamma_{\Xi} \cdot \mathbb{E}\left[\ln\left[\Xi_{ia}\right]\right]$, where the conditioning of the expectations is omitted to avoid notation cluttering.[34] This expression is the probability that woman i has a child at age a less the corresponding probability predicted by the policy price. That is, the age-a specific fertility rate in the absence of the policy. Cumulative at age 45, this statistic provides the no-policy counterfactual total fertility rate. Throughout the paper, I assume that the price was the only device used for implementing the policy. I then correct the price for measurement error. If there were an additional fixed cost for having children associated with the policy, that would go into the model's constant. This additional cost would not compromise the identification of γ_{Ξ}; its derivative with respect to the price would be 0. However, it would compromise the identification of the no-policy counterfactual, which is a prediction in levels. The prediction would be off by the fixed cost. If the additional cost were age-specific or time-varying, the identification compromise would be similar and appear annually. Evidence regarding costs associated with the policy in addition to the price is anecdotal. However, I am cautious about their potential existence and qualify the no-policy counterfactual estimates as stylized.

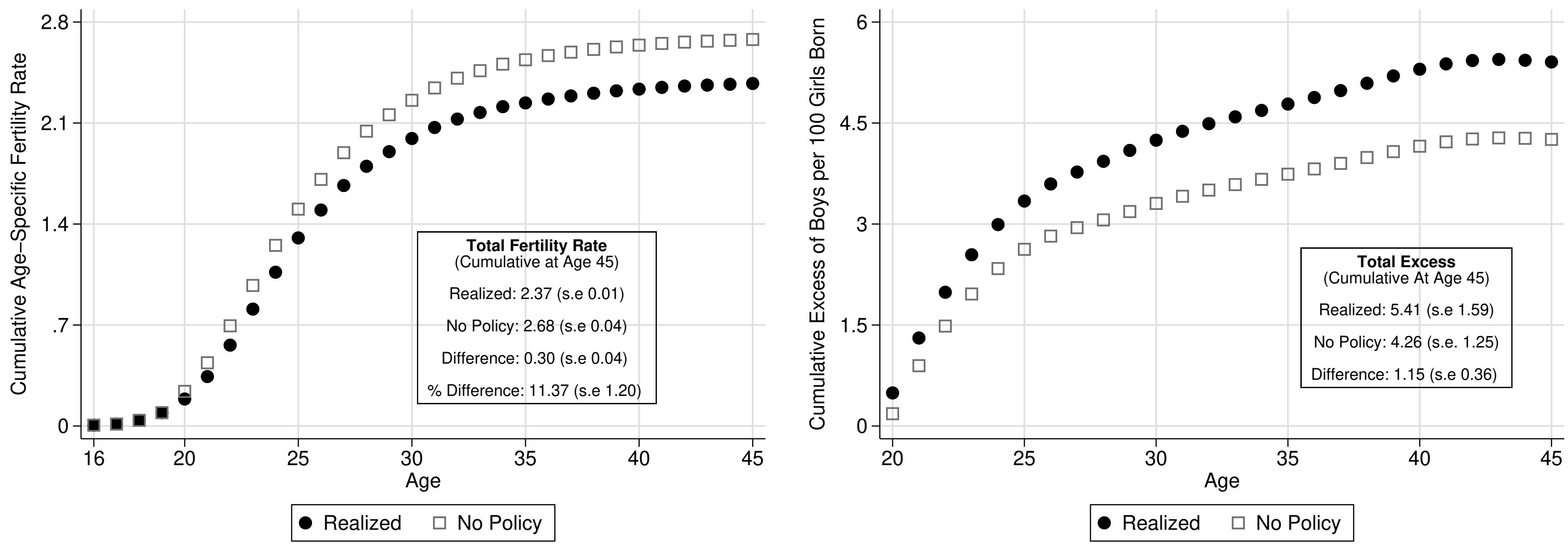
The age-specific and total fertility rates can be calculated using Equation (7). They can also be calculated by gender using the age-specific probability of having a son (daughter) to obtain the male (female) total fertility rate. The sum of the male and female age-specific fertility rates yields the age-specific total fertility rate $\mathbb{E}\left[\Delta n_{ia}\right]$. The sum of the cumulative gender-specific rates at age 45 yields the total fertility rate. Recall from Section 3 that the policy has essentially no impact on the probability of having a son. I thus set the no-policy

[33]Throughout this section, I display province × cohort clustered bootstrapped standard errors. The bootstrap allows me to account for the sampling variation in the multiple steps required to calculate the realized and no-policy age-specific and total fertility rates, as well as the excess of boys above the sex ratio.

[34]The interpretation and construction of no-policy counterfactuals is based on Equation (5), while I obtain my preferred estimate of γ_{Ξ} using Equation (6). That is not an issue because, in Section 3, I show that Equations (5) and (6) yield identical estimates of γ_{Ξ}.

Figure 7. Realized and No-Policy Counterfactual Aggregate Fertility

(a) Total Fertility Rate **(b)** Excess of Boys for Every 100 Girls Born



Panel (a): Each point displays an estimate of the age-specific cumulative fertility rate at the point in the label. The point at age 45 is an estimate of the total fertility rate. The realized fertility rates are based on the empirical counterparts of Equation (7). The no-policy counterfactual fertility rates are the realized fertility rates less the policy price prediction. The predictions use the age-specific policy semi-elasticities displayed in Figure 6, as described in the text. **Panel (b):** Each point displays the quotient between the cumulative male and female age-specific fertility rate at the point in the label. I subtract 1.055 from the age-specific quotients and multiply the resulting quantity by 100 so that the point estimates represent the excess of boys over the natural sex ratio for every 100 girls born. The realized and no-policy counterfactual sex ratios are constructed analogously to the fertility rates in Panel (a). The standard errors (s.e.) are bootstrapped and province × cohort clustered. They account for sampling variation in all of the steps required in the estimation of each point estimate. **Sample:** Nationally representative panel of women who were 15 to 45 years old between 1979 and 2000.

male age-specific fertility rate to be identical to its realized counterpart. For the female age-specific fertility rate, I adapt the no-policy counterfactual formula discussed in the previous paragraph to the flow of girls. When doing so, I use the age-specific semi-elasticities of the flow of girls in Panel a. of Figure 6, and the average price in the subsample employed in the estimation of each of the age-specific semi-elasticities. Panel a. of Figure 7 displays the no-policy counterfactual cumulative age-specific total fertility rate. At age 45, this provides the no-policy counterfactual total fertility rate: 2.68 (s.e. 0.04). The results indicate that the policy decreased the total fertility rate by 0.30 (s.e. 0.04) or 11.37% (s.e. 1.20%).

The quotient between the male and female age-specific fertility rates provides the age-specific sex ratio. Panel b. of Figure 7 displays the realized and no-policy counterfactual cumulative age-specific sex ratios. The age-pattern shows how the policy distorts the sex ratio over the ages in which women are fertile.[35] At age 45, the figure displays the "final" sex ratio at birth for the sample analyzed. If the sex ratio were not distorted, the age profile would be flat at the natural sex ratio (1.055).[36] To aid interpretation, I subtract 1.055 from the age-specific quotients and multiply the resulting quantity by 100. That yields the excess of boys over the natural sex ratio for every 100 girls born. As discussed in Section 3, the sex ratio at birth was distorted in China before the One-Child Policy for cultural and economic reasons. The policy distorted the ratio further by 1.15 (s.e. 0.36) boys for every 100 girls born. The semi-elasticities in Section 3 indicate that the distortion results from the policy curbing the probability of having daughters for mothers whose first children are girls.

5. Summary and Implications for Fertility after 2000

I characterize China's One-Child Policy as an individually tailored pricing system allowing women to have second and third children. I map this characterization to a longitudinal sample of women that annually replicates the national total fertility rate during the period

[35]The low age-specific fertility rates between ages 15 and 20 make the estimates of the sex ratio before age 20 unreliable. I thus set estimates before age 20 to 0 and omit them from Panel b. of Figure 7.

[36]The natural (male-to-female) sex ratio at birth is between 1.05 and 1.06. I set it to 1.055 for my calculations.

1979-2000. This allows me to exploit within-woman price variation to identify policy elasticities using a dynamic panel data model. The model has the flow of children as the dependent variable. That is convenient because the age-specific expectation of the dependent variable is the age-specific total fertility rate in the sample. The cumulative age-specific fertility rate at age 45 is the total fertility rate. Subtracting the prediction associated with the policy price from these age-specific fertility rates enables computing the no-policy counterfactual total fertility rate and sex ratio.

I find that the response to the policy was price-inelastic. Nonetheless, my estimates indicate that the One-Child Policy lowered the total fertility rate by 11%, for a large cohort of women who were in their fertile years between 1979 and 2000. This decrease resulted entirely from a decrease in the probability of having a girl. The policy priced all children, but it only curbed the number of daughters that women had. It did not affect their number of sons. The evidence in this paper indicates that the gendered impact of the policy was generated by a combination of a stopping rule and sex-selective abortions. Women whose first children were boys did not pursue fertility further. Women whose first children were girls pursued fertility further. Restricted by policy prices, they used sex-selective abortions to increase their chances of getting at least one son.

Below-replacement fertility rates are now a policy concern in China (Morgan et al., 2009). The Chinese government recently announced relaxations of the One-Child Policy (BBC, 2021). In 2015, it announced that essentially all couples would be able to have two children. In 2021, it increased the limit to three children. Figure 1 shows that the total fertility rate remained remarkably stable in China after 2000. I speculate that in 2000, fertility reached a level in which restrictions were no longer binding. The relaxations to the policy are likely non-binding, and will have little effect on fertility. The inelastic response to China's One-Child Policy indicates that only large monetary incentives would be effective at increasing fertility. Such incentives would need to consider the potential of fertility policies to imbalance the sex ratio.

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