Estimating Treatment Effects of the One-Child Policy: A Self-Report Approach*

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Abstract

We employ a self-reported survey measure of the ideal number of children to assess the One-Child Policy's impact on couples' childbearing. We take advantage of the novel feature of the self-reported measure that asks about couples' preferences in a counterfactual setting to help identify the treatment effect of the One-Child Policy. The study estimates the policy's treatment effect by using couples' pre-policy ideal child numbers in 2014 and using the answer again in the post-policy period, under the assumption that the conditional distribution of the ideal number of children given the actual number of children is stationary without policy constraint. Findings indicate a significant average reduction of 0.1583 children per couple in 2014. Variations in policy effects are explored across educational, urban/rural, and occupational groups, with highly educated urban women in government jobs experiencing the most pronounced impact. Regional variations are also noted, mirroring policy stringency differences among provinces.

Keywords: Self-reported measures, One-Child Policy, treatment effect, heterogeneous treatment effect
JEL Code: J13, J18, C21

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1 Introduction

In 1979, China introduced an unprecedented one-child policy, imposing penalties on households surpassing the established birth quota. Evaluating the policy's impact on family outcomes presents complexities. Its nationwide enforcement across China complicates the identification of a clear control group for assessing the treatment effect. Additionally, the policy followed an aggressive family planning policy in the early 1970s and coincided with China's market-oriented economic reforms, both of which lowered fertility rates over subsequent decades. Therefore, it is both important and challenging to establish robust methods to identify the effect of the One-Child Policy on various family outcomes in China.

This paper proposes an innovative approach to quantify the impact of the One-Child Policy on couples' childbearing decisions. By utilizing a self-reported survey measure that captures couples' aspirations regarding the ideal number of children, we introduce a novel methodology to identify and estimate the treatment effect of the One-Child Policy on the actual number of children born. Such a self-report approach is generally applicable to heterogeneous treatment effect models. This is because, in most economic applications, the individual herself has the best knowledge of her heterogeneous potential outcomes, which makes self-reported information useful for identifying the treatment effect. To be specific, in the 2014 wave of the survey, couples were asked about the ideal number of children they would want to have without considering the One-Child Policy restriction. The answer to this question provides information on the fertility decisions the couple would make in a counterfactual scenario where there were no policy restrictions. The One-Child Policy was removed in 2016. In the survey wave 2018, couples were asked the same question again. This time, they were directly asked how many children they would ideally want to have since the policy restriction was already removed.

We utilize the answers to the question in two waves, one before the policy relaxation and one after, and we impose the assumption that, in the absence of any policy constraint, the conditional distribution of the ideal number of children given the actual number of children should remain constant over time. In other words, the gap between the ideal and actual numbers of children would be caused by the same factors in 2014 and 2018 if there were no policy constraints. Under this assumption, we can estimate the counterfactual number of children couples would have had in 2014 if there had been no One-Child Policy. This estimation leverages the ideal number of children in 2014, along with the ideal and actual number of children in 2018. By comparing the number of children couples would have had in 2014 with and without the One-Child Policy, we can identify the policy's effect on fertility decisions.

Our empirical findings show that, on average, couples had 0.1583 fewer children than they would have if the One-Child Policy had not been instituted. This result suggests a significant negative impact of the One-Child Policy on couples' fertility rates. In addition to the overall average treatment effect, we also estimate the heterogeneous treatment effect of the policy on different subgroups of people. Results show that this policy has mostly affected women with higher education levels (-0.4354), residing in urban areas (-0.2506), Han Chinese (-0.1690), and engaged in government employment (-0.3810), revealing extensive differentials that could have far-reaching societal implications.

We also study variations in the treatment effect treatment impact across provinces. This analysis sheds light on the regional differences in policy effects, showing how the interplay of geographical and policy-specific contexts amplified or muted the One-Child Policy's impact. The results indicate that provinces located on the eastern coast of China and the four municipalities were mostly affected by the One-Child Policy. Provinces located in the west and middle regions exhibit a weak policy effect due to high agriculture intensity and ethnic minority existence.

We defend our main results by providing two sets of robustness checks, where we relax the strict assumption that the gap between the ideal number of children and the actual number of children is solely due to policy constraints. Instead, we assume that the conditional distribution of the ideal number of children, based on the actual number of children, individual characteristics, and other time-varying factors, remains constant over time. This approach accounts for the potentially changing macroeconomic environment and individual factors that might influence couples' fertility intentions. In the first robustness check, we control for women's age to consider the effect of changing age profiles on their fertility realizations in 2014 and 2018. In the second robustness check, we additionally control for provincial-level housing prices to account for the impact of surging housing prices from 2014 to 2018 on childrearing decisions. The estimated average treatment effects and heterogeneous treatment effects for couples with various demographics in these analyses are consistent with the main results, thereby validating our primary conclusions. The findings of this paper provide quantitative evidence of the significant impact of the One-Child policy on people's fertility decisions and, at the same time, highlight the heterogeneous policy effects among people with different socioeconomic characteristics and across geographical regions.

This study contributes to two streams of literature. The first one is estimating the policy effect of the One-Child Policy on various outcomes. One popular approach in the literature to identify the policy effect is to use cross-sectional and temporal variations on fines across provinces (McElroy and Yang , 2000; Liu, 2014; Huang et al., 2016). For example, McElroy and Yang (2000) studied regional variations in fines and rural fertility. Huang et al. (2016) used the fines as a measure for the one-child policy implementation to estimate its reduced-form effect on girls' educational attainment. The problem with this method is that fines might not be exogenously determined and it might be correlated with the local fertility demand.

Another common approach is to compare the fertility rates of Han and minority women in a difference-in-differences framework where minority women are used as a control group since the One-Child Policy did not regulate them (Li et al., 2005; Li and Zhang, 2007). However, the common trend assumption for Han and minority groups might not hold as the economic reforms that happened in the 1980s might affect them differently. What's more, although the One-Child policy did not regulate minority women, there might be a spillover effect from Han to minority groups if minority women adopt Han women's lifestyle and fertility decisions. Another approach developed recently is to use the excess fertility rate to measure the largely exogenous regional policy stringency in the one-child policy implementation, which is taken as the pure policy variable (Li and Zhang, 2017; Zhang, 2017). Our paper contributes to the literature by proposing a novel method for estimating the effect of the One-Child Policy on couples' childbearing decisions. Instead of relying on exogenous variations in policy implementations, we utilize self-reported measures of the ideal number of children couples would have had in the absence of policy constraints. The advantage of our approach is that individuals have the best understanding of their own preferences and counterfactual decisions. By leveraging this self-reported information, we can accurately estimate the effect of the One-Child Policy on fertility behaviors, including the heterogeneous treatment effects across different demographic groups.

The second stream of literature this study relates to using self-reported subjective measures as proxy variables to gain identification for unobserved attributes. To name a few examples, several papers on child development use proxy measures about cognitive and non-cognitive abilities to understand human capital formation (Cunha et al., 2010; Del Boca et al., 2013). Papers on how different gender attitudes affect household behaviors utilize subjective belief reports about gender roles (Goussé et al., 2017; Oh, 2021). This paper contributes to this literature by applying self-reported subjective measures to directly identify the treatment effect of an important policy, which broadens the application of self-reported subjective measures in the literature.

The rest of the paper is organized as follows. We first provide the institutional background on the One-Child Policy in Section 2, and we discuss the data we use in Section 3. We then present the model we use to identify and estimate the treatment effect of the policy in Section 4, followed by the estimation results in Section 5. We provide robustness checks in Section 6. Finally, Section 7 concludes.

2 Institutional Background

In response to rapid population growth, the Chinese government implemented a series of family planning policies in the 1970s to control births. The policies began with a mild version that discouraged couples from having many children before 1979. This eventually led to the strict One-Child Policy in 1979, which allowed couples to have only one child. The policy was gradually enforced firmly across the country in 1980. A series of actions were implemented to ensure the effectiveness of the policy by the local governments. For instance, if the parents refuse to pay the fine, the unauthorized children will be blacklisted from hukou birth registration, which will deny them access to various public services, including schooling.

Although this policy was implemented nationwide in China, there were variations among provinces in the strictness of the policy implementation. For instance, in 1981, the fine was 1.23 times the annual household income in Beijing, but only 0.647 times the annual household income in the province of Shaanxi (Ebenstein, 2010). There were also variations in policy strictness across people with different characteristics. For example, in addition to paying fines, parents with an excess birth may also be expelled from work if they work for the government or state-owned enterprises. There were also differences between urban and rural areas, where the policy was generally more restrictive and firmly enforced in urban areas than in rural areas. In fact, in most of the provinces, people from rural areas were allowed to have a second child if their first child was a girl (the so-called 1.5-child policy). Finally, the policy only restricted Han ethnic couples (which comprise around 92% of the total population in China), whereas the ethnic minorities were not subject to any policy restrictions.

The One-Child Policy went through several rounds of relaxation since the early 2000s in fear of the falling fertility rate. In 2011, all provinces started to permit couples who were both only children to have two children. In November 2013, this new policy came out, allowing couples in which at least one of the marital partners was an only child to have two children. However, even eligible couples under the policy would need to apply for permission to have a second child. In December 2015, the universal Two-Child Policy was announced, which allowed all couples to have two children, effective on January 1, 2016. Unlike the earlier rounds of relaxation, this universal Two-Child Policy no longer requires couples to apply for a birth permit in order to give birth to a second child legally. This drastic shift in the policy was intended to address the combined problems of the falling fertility rate, skewed sex ratio, and vast aging population that resulted from the strict One-Child Policy before.

3 Data

The primary dataset we use is the China Family Panel Study (CFPS), a nationally representative, bi-annual longitudinal survey. This dataset employs a household-based survey design, featuring a survey for the household asking about its socioeconomic characteristics and one survey for each gene or core member living in this household. Taking advantage of the household linkage data, we are able to obtain the complete fertility histories of each couple, and we can track each couple over the years using their personal surveys. The dataset provides information on the couple's basic characteristics such as age, education, ethnicity, marital status, urban/rural status, province of residence, etc. Additionally, it has a detailed module on the job history of respondents, including their employment status, the wage and income they receive from their jobs if employed, their working hours, and the employer type, industry, and occupation for each job. The baseline families were determined in the first wave in 2010 and have been tracked every two years, resulting in six waves up to now, which covers the policy change period in 2016.

What makes this dataset ideal for our research is that it includes a self-reported measure of fertility intentions by asking the wife and husband about their ideal number of children. The questions were asked in two waves: the first in 2014 before the policy change and the second in 2018 after the policy change. The question in 2014 is as follows:

> *"Regardless of policy constraints, how many children do you think would be ideal?"*

As of 2014, the One-Child Policy was still in effect. This question aims to determine how many children the couple would ideally want to have in a counterfactual world without the policy restriction. The question is asked separately to both the wife and the husband. In 2018, the question was asked again using the following statement:

"How many children do you think you would ideally have?"

As the One-Child Policy was removed before 2018, this question directly inquires about the ideal number of children the couple desires. The question is again asked separately to both the wife and the husband. The answers to these questions are crucial for our identification strategy, as they provide insights into the counterfactual choices couples would have made in the absence of the One-Child Policy before 2016. This information helps us, under certain assumptions, recover their actual number of children in 2014 had the One-Child Policy not been in place.

As previously mentioned, we can observe the fertility histories of each couple in the data, in addition to their ideal number of children. In other words, we can observe both the ideal and the realized number of children in 2014 and 2018 for each couple. To derive the final dataset for estimation, we follow the steps below. Firstly, we focus on married women between the ages of 30 and 42 in 2014 and 2018. These groups are particularly relevant for family planning policies because they were likely affected by the One-Child Policy during their fertility history. Additionally, they are still within their fertile years and can respond to the Two-Child Policy implemented in 2016. Secondly, we include only those women whose ideal and actual number of children is one, two, or three. This decision is based on the observation that 98.59% of married couples in our data desire to have one, two, or three children, and 97.18% of them have one, two, or three children. Therefore, we focus on this majority group for our analysis. Finally, we combine the ideal and actual number of children of two and three into a single category of two. This simplification is driven by our focus on whether the couple has a second child, which is the primary target of the One-

Child Policy. Additionally, only 6.76% of couples desire three children and 8.96% have three children. By coding these numbers as two, we streamline our analysis. The entire filtering procedure results in an unbalanced panel dataset consisting of 4,205 women and a total of 5,448 observations, which will be used in our analysis.

Table 1 presents the summary statistics for key variables. The average age of the 2,870 women in 2014 is 36.34 years, while the average age of the 2,578 women in 2018 is 35.47 years, indicating a balanced age distribution across the two subgroups. Regarding urban/rural status and ethnicity, slightly more than half of the surveyed women are from urban areas. Additionally, 90.89% are Han Chinese, while the remaining 9.11% belong to one of the 55 officially recognized ethnic minority groups in China.

Regarding educational levels, most women have less than a high school degree, with only 32% having completed high school or higher. In 2014, the average ideal number of children desired by women was 1.8411, which decreased to 1.8053 in 2018. The average number of children women had in 2014 was 1.5404, increasing to 1.6067 in 2018. For work status, we examine a combined total of 5,448 observations across both waves. Of these, 19.33% were unemployed, and 11.82% were self-employed. Additionally, 28.96% were employed in the agricultural sector, indicating a significant proportion. Lastly, 39.89% of the observations were of women employed by an employer.

Out of the employed observations, the majority (62.13%) were employed by private companies, while 28.72% worked for the Chinese government or state-owned enterprises. Additionally, 5.89% were employed by foreign-owned companies, and 3.27% worked for other types of companies. When estimating the treatment effect of the One-Child Policy on the number of children people had, we will calculate both the average treatment effect for the overall population and the heterogeneous treatment effect for different subgroups. These subgroups are defined based on education, urban/rural status, ethnicity, job status, and province of residence.

Figure 1 presents detailed statistics for the ideal and realized number of children for

Table 1: Summary Statistics for Key Variables

Variables	Mean	Std. Dev.	Min	Max	Obs
Age in year 2014	36.34	3.85	30	42	2,870
Age in year 2018	35.47	3.87	30	42	2,578
Urban residence	0.5308	0.4991	0	1	4,205
Han Chinese	0.9089	0.2878	0	1	4,205
Educational level					
- Less than high school	0.6804	0.4664	0	1	4,205
- High school graduate	0.1396	0.3466	0	1	4,205
- College and higher	0.1800	0.3843	0	1	4,205
Fertility					
- Ideal number of children in 2014	1.8411	0.3656	1	2	2,870
- Ideal number of children in 2018	1.8053	0.3961	1	2	2,578
- Actual number of children in 2014	1.5404	0.4985	1	2	2,870
- Actual number of children in 2018	1.6067	0.4886	1	2	2,578
Job status					
- Unemployed	0.1933	0.3949	0	1	5,448
- Self-employed	0.1182	0.3229	0	1	5,448
- Agriculture	0.2896	0.4536	0	1	5,448
- Employed	0.3989	0.4897	0	1	5,448
- Private	0.6213	0.4852	0	1	2,173
- Government	0.2872	0.4525	0	1	2,173
- Foreign	0.0589	0.2355	0	1	2,173
- Other	0.0327	0.1778	0	1	2,173

Note: The dataset contains an unbalanced two-period panel of 4,205 women, with data from the years 2014 and 2018, resulting in 5,448 observations in total. For ethnicity, urban residence, and educational level, we calculate statistics using data from the 4,205 women. For age, and the ideal and realized number of children, we calculate summary statistics separately for women in 2014 and 2018. For job status, we combine data from both years and calculate the summary statistics for the entire sample. Conditional on being employed, we calculate the statistics for different employer types. "Private" refers to companies privately owned by individuals, "Government" includes government positions and state-owned enterprises, "Foreign" refers to companies owned by foreigners, and "Other" aggregates all other types of employers.

couples in 2014 and 2018. The first subplot shows the average numbers for all couples, while the remaining subplots display the average numbers for different subgroups. The first observation is that the average actual number of children is consistently smaller than the average ideal number of children. This indicates that couples do not achieve their desired number of children due to various potential reasons, including policy constraints, high costs of raising children, and infertility.

The second observation is that the gap between the ideal and actual number of children is smaller in 2018 than in 2014 across all subplots. This suggests that at least part of the gap in 2014 was due to the policy constraints imposed by the One-Child Policy. Assuming that the conditional distribution of the ideal number of children given the actual number of children remains constant over time in the absence of policy constraints, we can estimate the counterfactual actual number of children in 2014 had there been no One-Child Policy. By using the ideal and actual number of children data from 2018 and the ideal number of children data from 2014, we can back out this counterfactual number. This approach allows us to estimate the policy effect on the number of children couples had in 2014. We formally lay out the model environment, our identification assumption, and the treatment effect estimator in Section 4.

Finally, we observe significant heterogeneity across different subgroups regarding both the ideal and realized number of children in 2014 and 2018. For instance, the rural population had a higher ideal number of children than the urban population in both years. This aligns with the understanding that rural couples tend to have stronger fertility preferences than urban couples. Additionally, the urban population had far fewer realized children than the rural population in 2014. The fact that the gap between the ideal and realized number of children shrinks more from 2014 to 2018 for urban couples than for rural couples suggests that the One-Child Policy had a larger effect on urban couples. We can observe similar patterns of heterogeneity across ethnic groups, educational levels, and employment status. This underscores the benefits of our method in estimating the heterogeneous treatment effects for different groups. Since individuals are most aware of their own preferences and counterfactual choices, using their self-reported data can provide a convenient and convincing estimate of the treatment effect.

4 Model

This section presents the model we use for identifying and estimating the treatment effect of the One-Child Policy on the number of children couples had by utilizing the self-reported ideal number of children. Consider a population with members $i = 1, ..., N$. For each member of the population, we use $\{Y_i, S_i, D_i\}, i = 1, ..., N$ to denote the key variables for couple i, where

$$\begin{cases} Y_i &= \text{Outcome variable: number of children} \\ D_i &= \text{Treatment indicator: whether under One-Child Policy} \\ S_i &= \text{Self-reported number of children a couple wants} \end{cases}.$$

$D_i = 1$ if the couple was under the One-Child Policy and $D_i = 0$ otherwise.

We can then define the potential or latent outcomes as the following:

$$\begin{cases} Y_i(0) &= \text{Number of children a couple has if not under One-Child Policy} \\ Y_i(1) &= \text{Number of children a couple has if under One-Child Policy} \end{cases}.$$

Similarly, we define the potential ideal number of children as the following:

$$\begin{cases} S_i(0) &= \text{Self-reported number of children a couple wants if not under One-Child Policy} \\ S_i(1) &= \text{Self-reported number of children a couple wants if under One-Child Policy} \end{cases}.$$

Furthermore, our identification is based on panel data from before and after the One-Child Policy was relaxed. Let $\{Y^b(0), Y^a(0)\}$ denote the number of children couples have if not under the One-Child Policy before and after the policy change, respectively. Let $\{S^b(0), S^a(0)\}$ denote the self-reported number of children couples want if not under the One-Child Policy before and after the policy change, respectively. Let $\{Z^b, Z^a\}$ denote the time-varying macroeconomic factors and individual characteristics that affect people's childrearing behav-

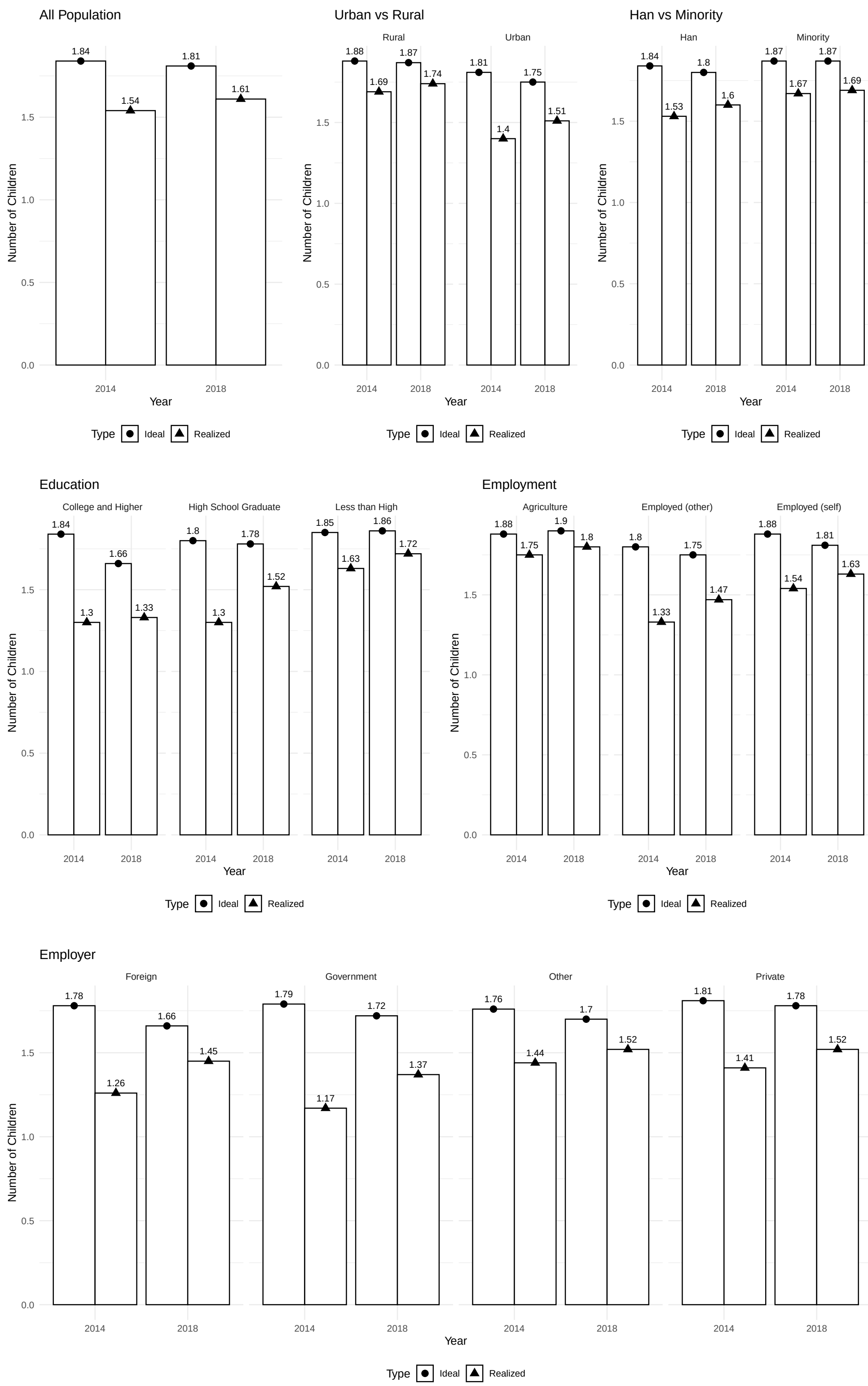

Figure 1: Ideal and Actual Number of Children: 2014 vs 2018

ior before and after the policy change.

After defining the basic notations, we now introduce the key assumption for our identification argument.

Assumption 1. *The distribution of self-reported potential outcomes conditional on the true potential outcome and other individual characteristics does not change before and after the policy change, i.e.,*

$$f_{S^b(0)|Y^b(0),Z^b} = f_{S^a(0)|Y^a(0),Z^a}. \tag{4.1}$$

The distribution $f_{S^b(0)|Y^b(0),Z^b}$ denotes the conditional distribution of $S(0)$ given $Y(0)$ and Z before the policy change, and $f_{S^a(0)|Y^a(0),Z^a}$ denotes the conditional distribution of $S(0)$ given $Y(0)$ and Z after the policy change. In other words, the gap between the self-reported ideal number of children and the actual number of children should be caused by the same factors before and after the policy change if there were no policy constraints and conditional on individual and macroeconomic factors, leading to a stationary conditional distribution over time. The additional factor Z may include pre-determined individual characteristics such as ethnicity, education, and rural/urban status. It can also include time-varying factors such as age, housing price, and childrearing costs. For demonstration purposes, we first look at the simplest case where Z^b and Z^a degenerate. In other words, we assume that $f_{S^b(0)|Y^b(0)} = f_{S^a(0)|Y^a(0)}$ and prove Lemma 1 based on this strong assumption.

Consider the situation where S^b, S^a, Y^b, Y^a are all discrete variables [1]. Let the support of S^b be $\mathcal{S}^b = \{s_1^b, s_2^b, ..., s_L^b\}$ and the support of S^a be $\mathcal{S}^a = \{s_1^a, s_2^a, ..., s_L^a\}$, the support of $Y^b(0)$ be $\mathcal{Y}^b(0) = \{y_1^b, y_2^b, ..., y_L^b\}$, and finally the support of Y^a be $\mathcal{Y}^a = \{y_1^a, y_2^a, ..., y_L^a\}$ [2]. We define

$$\overrightarrow{p(S^b)} = [f_{S^b}(s_1^b), f_{S^b}(s_2^b), ..., f_{S^b}(s_L^b)]^T$$

$$M_{S^a|Y^a} = \left[f_{S^a|Y^a}(s_l^a|y_p^a)\right]_{l=1,2,...,L;p=1,2,...,L}.$$

We have the following lemma.

[1] We use the discrete case in our proof both because of its simplicity and because in our empirical study, both the number of children and self-reported ideal number of children are discrete. Nevertheless, for continuous variables, the identification result still holds with similar arguments.

[2] For simplicity, we assume all the discrete variables have the same dimensionality for their support.

Lemma 1. *Suppose that Assumption 1 holds with degenerated Z^b and Z^a and that $M_{S^a|Y^a}$ is invertible. The average treatment effect of the policy $E[Y^b(1) - Y^b(0)]$ is identified and can be estimated as follows:*

$$E[Y^b(1) - Y^b(0)] = E[Y^b] - (y_1^b, y_2^b, ..., y_L^b) \times M_{S^a|Y^a}^{-1} \times \overrightarrow{p(S^b)}. \tag{4.2}$$

Proof. We can write the marginal distribution of $S^b(0)$ using the following equation:

$$f_{S^b(0)}(s) = \sum_{y \in \mathcal{Y}^b(0)} f_{S^b(0)|Y^b(0)}(s|y) f_{Y^b(0)}(y), \tag{4.3}$$

where $f_{Y^b(0)}$ is the marginal distribution of $Y^b(0)$. Before the relaxation of the One-Child policy, the individuals are asked to self-report the number of children that the couples would want without considering the policy constraint. Therefore, we observe $S^b(0)$ as S^b

$$S^b(0) = S^b. \tag{4.4}$$

In other words, the self-reported ideal number of children S_j observed in the data equals the potential self-reported ideal number of children without any policy constraints. In addition, after the policy constraint has already been removed, we have

$$S^a(0) = S^a, \quad Y^a(0) = Y^a. \tag{4.5}$$

Given Assumption 1, we can plug equation A.3 into equation A.1, which gives

$$\begin{aligned} f_{S^b(0)|Y^b(0)} &= f_{S^a(0)|Y^a(0)} \\ &= f_{S^a|Y^a}. \end{aligned} \tag{4.6}$$

Plugging equation A.2 and A.4 into equation A.1, we have

$$f_{S^b}(s) = \sum_{y \in \mathcal{Y}^b(0)} f_{S^a|Y^a}(s|y) f_{Y^b(0)}(y). \tag{4.7}$$

We define

$$\overrightarrow{p(Y^b(0))} = [f_{Y^b(0)}(y_1^b), f_{Y^b(0)}(y_2^b), ..., f_{Y^b(0)}(y_L^b)]^T.$$

Equation A.5 is then equivalent to

$$\overrightarrow{p(S^b)} = M_{S^a|Y^a} \times \overrightarrow{p(Y^b(0))}.$$

Since both $\overrightarrow{p(S^b)}$ and $M_{S^a|Y^a}$ can be estimated from the data, by conducting matrix inversion, we obtain the marginal distribution of $Y^b(0)$:

$$\overrightarrow{p(Y^b(0))} = M_{S^a|Y^a}^{-1} \cdot \overrightarrow{p(S^b)}. \tag{4.8}$$

Notice that $\overrightarrow{p(Y^b(0))}$ contains the same information as the distribution $f_{Y^b(0)}$. Therefore, we can identify and estimate $f_{Y^b(0)}$ from equation A.6. The average treatment effect of the One-Child Policy on the number of children couples had before the policy was relaxed can be then estimated using the following equation:

$$\begin{aligned} E[Y^b(1) - Y^b(0)] &= E[Y^b(1)] - E[Y^b(0)] \\ &= E[Y^b] - (y_1^b, y_2^b, ..., y_L^b) \times \overrightarrow{p(Y^b(0))}, \end{aligned} \tag{4.9}$$

where Y^b is the observed number of children a couple has before the policy was relaxed in year j and $\overrightarrow{p(Y^b(0))}$ can be estimated using equation A.6. QED.

□

Next, we consider a more realistic case where we allow for heterogeneous treatment effects across subgroups of people with various fixed characteristics. Let X denote the time-constant individual heterogeneity. We define

$$\overrightarrow{p(S^b|X=x)} = [f_{S^b|X}(s_1^b|x), f_{S^b|X}(s_2^b|x), ..., f_{S^b|X}(s_L^b|x)]^T$$

$$M_{S^a|Y^a,X=x} = \left[f_{S^a|Y^a,X}(s_l^a|y_p^a, x)\right]_{l=1,2,...,L;p=1,2,...,L}.$$

We have the following lemma.

Lemma 2. *Suppose that Assumption 1 holds with $Z^b = Z^a = X$ and that $M_{S^a|Y^a,X=x}$ is invertible. The conditional average treatment effect of the policy $E[Y^b(1) - Y^b(0)|X = x]$ given characteristic $X = x$ is identified and can be estimated as follows:*

$$E[Y^b(1)-Y^b(0)|X=x] = E[Y^b|X=x]-(y_1^b, y_2^b, ..., y_L^b)\times M^{-1}_{S^a|Y^a,X=x}\times \overrightarrow{p(S^b|X=x)}. \tag{4.10}$$

The proof is similar to the proof for Lemma 1, where we adopt the same matrix-inversion trick as in the main proof to obtain $f_{Y^b(0)|X}(y|x)$, which is the conditional distribution of the potential number of children people would have before the policy relaxation if there was no One-Child Policy, given individual characteristics X. Having estimated $f_{Y^b(0)|X}(y|x)$, we can then identify and estimate the conditional average treatment effect using Equation 4.10.

Lemma 2 shows that we can take advantage of the self-reported measure to estimate the heterogeneous treatment effects for subgroups with different characteristics. These subgroups include women with varying educational levels, women from urban and rural areas, women of different ethnicities, and women with different job statuses and employment types.

Furthermore, we estimate the average treatment effect for women in different provinces to study the heterogeneous treatment effects across geographic regions. We present the results for the overall treatment effect and the heterogeneous treatment effects for people with different characteristics in Section 5.

Finally, we present the main result for the most general case, where we assume that the distribution of self-reported potential outcomes conditional on the true potential outcomes and other time-varying factors does not change before and after the policy change. We define

$$\overrightarrow{p(S^b|Z^b=z)} = [f_{S^b|Z^b}(s_1^b|z), f_{S^b|Z^b}(s_2^b|z), ..., f_{S^b|Z^b}(s_L^b|z)]^T$$

$$M_{S^a|Y^a,Z^b=z} = \left[f_{S^a|Y^a,Z^b}(s_l^a|y_p^a, z)\right]_{l=1,2,...,L;p=1,2,...,L}.$$

We have the following theorem.

Theorem 1. *Suppose that Assumption 1 holds and that $M_{S^a|Y^a,Z^a=z}$ is invertible. The conditional average treatment effect of the policy $E[Y^b(1)-Y^b(0)|Z^b=z]$ given characteristic $Z^b=z$ is identified and can be estimated as follows:*

$$E[Y^b(1)-Y^b(0)|Z^b=z] = E[Y^b|Z^b=z] - (y_1^b, y_2^b, ..., y_L^b) \times M_{S^a|Y^a,Z^a=z}^{-1} \times \overrightarrow{p(S^b|Z^b=z)}. \tag{4.11}$$

Finally, we can integrate over the distribution of Z^b to obtain the marginal average treatment effect:

$$E[Y^b(1)-Y^b(0)] = \int_z E[Y^b(1)-Y^b(0)|Z^b=z] f_{Z^b}(z)\mathrm{d}z, \tag{4.12}$$

where $f_{Z^b}(z)$ is the marginal distribution of Z^b. The proof for Theorem 1 follows a similar approach to that of Lemma 1 and is detailed in Appendix A.

Having described the model and the identification arguments, Section 5 presents the results for the overall average treatment effect using Lemma 1 and the heterogeneous average treatment effects for people with different fixed characteristics following Lemma 2. Finally, Section 6 shows the robustness check that shows the heterogeneous average treatment effects controlling for time-varying variables, based on the main result in Theorem 1.

5 Results

We use Equation 4.2 and Equation 4.10 to calculate the average treatment effect of the One-Child Policy on the number of children in 2014 and the conditional treatment effect of the policy on the number of children given various individuals' characteristics. We interpret $E[Y^{14}(1) - Y^{14}(0)]$ and $E[Y^{14}(1) - Y^{14}(0)|X]$ as the treatment effect of the One-Child Policy on people's number of children, because in 2014, the One-Child Policy was still effective in China. Although there were several rounds of relaxation of the One-Child Policy before 2014 already, we argue that the effect of these relaxations was minimal for the following reasons. In 2011, all provinces started to permit couples who were both only children to have two children. However, couples that satisfy this requirement are rare in numbers. In November 2013, the new policy allowed couples in which at least one of the marital partners was an only child to have two children. However, even eligible couples under the policy would need to apply for permission to have a second child. Therefore, the policy effect of this relaxation should not be reflected in the early survey data in 2014, since applying for permissions takes several months to complete. In conclusion, we argue that couples in 2014 were still facing a relatively strict One-Child Policy and therefore $E[Y^{14}(1) - Y^{14}(0)]$ captures the effect of the One-Child Policy on couples' number of children.

Average Effects and Heterogeneous Effects Across Different Demographic Groups

Table 2 presents the first set of results for the average treatment effect of the One-Child Policy on the number of children people had in 2014. The first column shows the estimation result for the average treatment effect for the total population, and the second column shows the average treatment effects for women from urban and rural areas separately. The third column presents the average treatment effects for women of Han Chinese and minority ethnic groups and the last column shows the average treatment effects for women having different educational levels. When conducting the matrix inversion to back out $\overrightarrow{p(Y^b(0))}$ in equation A.6, we use the empirical distribution in the data to represent $f_{S^{14}}$ and $f_{S^{18}|Y^{18}}$. Similarly, for urban residence, ethnic groups, and educational levels, since the sample size is sufficient for each subgroup, we again use the empirical distribution in the data to represent $f_{S^{14}|X}$ and $f_{S^{18}|Y^{18},X}$. We also use the empirical distribution in the data to estimate $f_{Y^{14}}$ and $f_{Y^{14}|X}$ in the two analyses, respectively. Finally, we obtain the standard errors by bootstrapping the data 100 times.

The estimated average treatment effect for the total population is -0.1583^{***}, indicating that, on average, couples in 2014 had 0.1583 fewer children than they would have had if the One-Child Policy had not been in place. When comparing the effects on urban and rural populations, we find that the effect on urban couples (-0.2506^{***}) is significantly larger than that on rural couples (-0.0479). The estimated policy effect on rural populations is insignificant in this analysis. This result is consistent with the stricter enforcement of the One-Child Policy for urban couples, while rural couples were generally less strictly regulated and were allowed to have a second child if their first child was a girl. Comparing different ethnic groups, we find that the effect on Han Chinese is -0.1690^{***}, while the effect on ethnic minorities is small and insignificant (-0.0303). This aligns with the fact that the One-Child Policy primarily targeted Han Chinese, while ethnic minorities were largely exempt from the restrictions.

When examining the impact of the One-Child Policy on various educational levels, there is a significant gap between the effects on women with less than a high school education (-0.0615^{***}) and those with a high school degree (-0.2828^{***}) or a college degree and higher (-0.4354^{***}). Several reasons might explain these significant differences. Women with lower educational levels tend to come from rural areas, where the policy was less strictly enforced compared to urban areas, which are home to more women with higher educational levels. Additionally, women with higher educational levels are more likely to be able to afford to have more children, whereas women with lower educational levels might not have the financial means to do so. Therefore, the One-Child Policy is more likely to be binding for women with higher educational levels than for those with lower educational levels. Consequently, the policy effect on the number of children is much larger for women with higher educational levels than for those with less than a high school degree. Regardless of the potential reasons, the results indicate that women with higher educational levels were most affected by the One-Child Policy.

Table 2: Treatment Effect of One-Child Policy on the Number of Children: Part I

Treatment Effect	Total Population	Urban/Rural Status	Ethnic Groups	Education
Total Treatment effect	-0.1583^{***}			
	(0.0204)			
Treatment effect: urban		-0.2506^{***}		
		(0.0306)		
Treatment effect: rural		-0.0479		
		(0.0297)		
Treatment effect: Han Chinese			-0.1690^{***}	
			(0.0222)	
Treatment effect: ethnic minorities			-0.0303	
			(0.0991)	
Treatment effect: less than high school				-0.0615^{**}
				(0.0258)
Treatment effect: high school graduate				-0.2828^{***}
				(0.0756)
Treatment effect: college and higher				-0.4354^{***}
				(0.0503)

Note: This table shows the first set of estimation results for the average treatment effect of the One-Child Policy on the number of children people had in 2014. Standard errors are shown in parentheses. *, **, and *** represent significance levels of 10, 5, and 1 percent. The first column shows the estimation results for the total average treatment effect, and the second column shows the average treatment effect for women from urban and rural areas separately. The third column shows the average treatment effect for women of Han Chinese and minority ethnic groups. The last column shows the average treatment effect for women having different educational levels. Standard errors were obtained via bootstrapping by sampling the dataset 100 times.

Heterogeneous Effects Across Different Employment and Job Statuses

Next, we look at the policy effect on people of various employment and job statuses. Table 3 presents the second set of results for the average treatment effect of the One-Child Policy on the number of children people had in 2014. The first column shows the estimation result for the average treatment effect for the total population, and the second column shows the average treatment effect for women with different employment statuses. The last column shows the average treatment effect for employed women who have different types of employers. Similar to the first set of estimations, we use the empirical distribution in the data to represent $f_{S^{14}|X}$, $f_{S^{18}|Y^{18},X}$, and $f_{Y^{14}|X}$ when conducting subgroup analysis for the treatment effect of the policy. Again, we obtain the standard errors by bootstrapping the data 100 times.

Firstly, we examine how the policy effects differ across employment statuses. Compared to the overall effect of -0.1583^{***}, working in the agricultural sector has suffered from almost zero policy impact (-0.0007), while those self-employed or employed by others in the non-agricultural sector experience a larger policy effect (-0.2796^{***}, -0.2697^{***}). Meanwhile, the policy effect on unemployed women is significantly negative (-0.1276^{**}) but smaller than the effects on the employed population, yet larger than on agricultural workers. These results make intuitive sense because people working in the agricultural sector are likely residing in rural areas, where the One-Child Policy was loosely regulated. Therefore, these individuals experience a much smaller policy effect compared to those self-employed or employed in non-agricultural sectors. The policy effect is smaller for unemployed people than for employed people because unemployed individuals do not face the risk of job loss due to having excess births. Consequently, the penalties imposed on them are smaller than those on the employed population, resulting in a smaller policy effect for the unemployed. It is somewhat surprising that self-employed individuals suffer from a slightly larger policy effect than those employed by others. One possible explanation is that self-employed people are more sensitive to fines due to financial constraints. Therefore, the One-Child Policy has a larger impact on this group compared to those employed by others.

To further investigate whether the policy effect differs across employer types, we focus on women working in non-agricultural sectors and employed by others and conduct subgroup analysis for them working for different types of employers. The results of our analysis are both interesting and consistent with our prior knowledge. We find that government-related positions (including those in state-owned enterprises) have a more negative policy effect on the number of children (-0.3810^{***}) compared to positions in privately owned firms (-0.2046^{***}). This finding is not surprising, given that government workers face the highest penalties for having excess births, which can result in both substantial fines and job loss. In contrast, employees of privately owned firms typically only face fines for excess births without risking their jobs. To summarize our findings on job and employment-related types: Women employed and self-employed in non-agricultural sectors were the most negatively affected by the One-Child Policy in terms of having more children. Among employed women, those working for the government were particularly negatively affected.

Table 3: Treatment Effect of One-Child Policy On Number of Children: Part II

Parameters	Total Population	Employment Type	Employer
Total Treatment effect	-0.1583^{***}		
	(0.0204)		
Treatment effect: unemployed		-0.1276^{**}	
		(0.0545)	
Treatment effect: employed		-0.2697^{***}	
		(0.0327)	
Treatment effect: self-employed		-0.2796^{***}	
		(0.0592)	
Treatment effect: agriculture		-0.0007	
		(0.0378)	
Treatment effect: employed–government			-0.3810^{***}
			(0.0695)
Treatment effect: employed–private			-0.2046^{***}
			(0.0446)

Note: This table shows the second set of estimation results for the average treatment effect of the One-Child Policy on the number of children people had in 2014. *, **, and *** represent significance levels of 10, 5, and 1 percent. The first column shows the estimation results for the total average treatment effect, and the second column shows the average treatment effect for women with different employment statuses. The last column shows the average treatment effect for employed women who have different types of employers. Standard errors, indicated in brackets, were obtained via bootstrapping by sampling the dataset 100 times.

Regional Effects Across Different Provinces

For the last set of subgroup analyses, we investigate heterogeneity in the aggregate policy effect across geographical regions. To do so, we estimate the treatment effect for women in each province. Due to the limited sample size for each province, it is infeasible to calculate the empirical distribution for $f_{S^{14}|X}$ and $f_{S^{18}|Y^{18},X}$ for each province X using the data. Therefore, we model the conditional distribution using a logistic regression and use predicted probabilities for the outcome variables as the estimated conditional distribution. We obtained results for a total of 22 provinces, which are shown in Figure 2 below.

Colored segments on the map represent provinces with corresponding estimated policy effects. The closer the color is to red, the stronger the policy effect, while the closer it is to blue, the weaker the policy effect. It is evident that numerous provinces on the east coast of the country experience a strong policy effect, whereas several provinces located in the west and middle regions exhibit a weak policy effect. This is because the provinces located in the western and central regions tend to be agriculturally intensive and have a large proportion of ethnic minority groups. As a result, the degree of policy regulation is low. Provinces situated on the eastern coast tend to be more economically developed and urban-intensive. Therefore, they experienced a larger policy effect of the One-Child Policy on the number of children compared to other provinces.

Another observation is that the four municipalities (Beijing, Tianjin, Shanghai, Chongqing) had large policy effects on the number of children, which makes sense as the One-Child Policy strictly regulated these four regions. Finally, two provinces in the north-east region (Jilin and Heilongjiang) also had large policy effects, as they had many state-owned enterprises before, therefore regulating couples at a substantial penalty. The majority of the treatment effect findings for provinces align with our intuition and previous knowledge. The regional subgroup analysis confirms that there are substantial variations in the policy effect of the One-Child Policy across provinces in China.

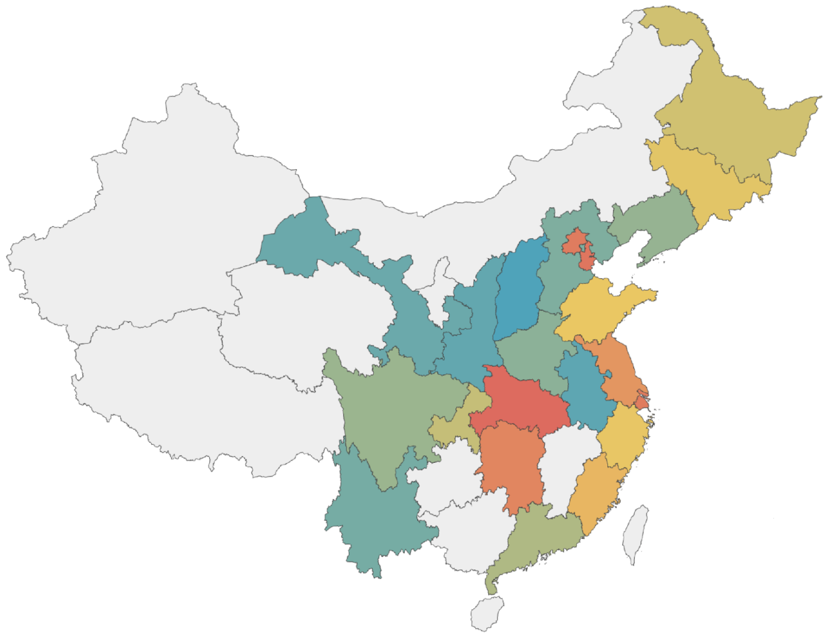

Figure 2: Provincial Variations in Treatment Effect

6 Robustness Check

In this section, we provide further evidence to defend our main findings for the heterogeneous treatment effects of the One-Child Policy on couples' childbearing, by controlling for time-varying characteristics that might affect the conditional distribution of the ideal number of children on the actual number of children over time. This is because, over four years, there have been substantial changes in couples' age structures and the macroeconomic conditions in China, influencing a couple's intention to have children. We adopt the most general version of Assumption 1, where we assume that conditional on these time-varying factors, the conditional distribution of the ideal number of children given the actual number of children should be stationary over time, assuming no policy constraints. Building on Assumption 1, we can apply our main result in Theorem 1 to estimate the treatment effect of the policy.

Controlling for Individual's Age

Firstly, we choose $\{Z^{14}, Z^{18}\}$ to represent women's age in 2014 and 2018, to control for potential life cycle patterns in the conditional distribution of the ideal number of children given the actual number of children. We model the three conditional distributions $f_{S^{14}(0)|Z^{14}}(s|z)$, $f_{S^{18}|Y^{18},Z^{18}}(s|y,z)$ and $f_{Y^{14}(1)|Z^{14}}(y|z)$ using logistic regression. In these models, we regress the dependent variables on age and use the predicted probabilities as the estimated conditional distributions. Following Equation 4.11, we calculate the conditional treatment effect and integrate over the age distribution of women in 2014 to obtain the final average treatment effect using Equation 4.12. Additionally, we estimate heterogeneous treatment effects as in the main section. Here, we calculate the conditional treatment effect for different subgroups of women and then integrate over the age distribution of these subgroups in 2014 to obtain the average treatment effect. This is done for women from urban/rural areas, Han Chinese and ethnic minorities, different educational levels, and various employer/job types.

Table 4 and Table 5 present the results. We find that the average and conditional treatment effects, given different individual characteristics, are consistent with those obtained in the main section, where we did not control for age over the years. The estimated total average treatment effect is -0.1624^{***}. After controlling for age, the policy effect for rural women becomes significantly negative (-0.0584^{***}), although it remains far smaller than the effect observed for urban women. Urban women, Han Chinese women, and women with a college education experience the largest negative impact from the One-Child Policy, consistent with our findings in the main section. The results for different job statuses and employers are also consistent with the main findings. These results confirm that our main conclusions are robust in estimating both the average treatment effect of the One-Child Policy on the number of children and the conditional treatment effects for various subgroups of the population.

Table 4: Treatment Effect of One-Child Policy On Number of Children: Age-Adjusted (I)

Treatment Effect	Total Population	Urban/Rural Status	Ethnic Groups	Education
Total Treatment effect	-0.1624^{***}			
	(0.0202)			
Treatment effect: urban		-0.2505^{***}		
		(0.0310)		
Treatment effect: rural		-0.0584^{**}		
		(0.0282)		
Treatment effect: Han Chinese			-0.1727^{***}	
			(0.0218)	
Treatment effect: ethnic minorities			-0.0457	
			(0.1048)	
Treatment effect: less than high school				-0.0685^{***}
				(0.0249)
Treatment effect: high school graduate				-0.2806^{***}
				(0.0770)
Treatment effect: college and higher				-0.4365^{***}
				(0.0498)

Note: This table shows the robustness check results, where the estimation for the average treatment effect of the One-Child Policy on the number of children people had in 2014 is conditional on age. *, **, and *** represent significance levels of 10, 5, and 1 percent. The first row presents the average treatment effect of the policy, integrating over the empirical distribution of the age in 2014 from the dataset. The rest of the rows present the average treatment effect of the policy conditional on women's different characteristics, including urban/rural status, ethnicity, and educational levels. Standard errors, indicated in brackets, were obtained via bootstrapping by sampling the dataset 100 times.

Additionally Controlling for Housing Prices

Secondly, in addition to controlling for age, we add the provincial-level average housing prices in 2014 and 2018 into $\{Z^{14}, Z^{18}\}$. Therefore, now Z^{14} contains $\{\text{age}^{14}, \text{hprice}^{14}\}$, where age^{14} is the age for the woman in 2014 and hprice^{14} is the average housing price of the province where the woman resides in 2014. Similarly, Z^{18} contains $\{\text{age}^{18}, \text{hprice}^{18}\}$. This is to control for characteristics other than the policy constraint and age profile that might affect the conditional distribution of the ideal number of children on the actual number of children. From 2014 to 2018, a surging housing price is considered to be the major reason that prevents

Table 5: Treatment Effect of One-Child Policy On Number of Children: Age-Adjusted (II)

Parameters	Total Population	Employment Type	Employer
Total Treatment effect	-0.1624^{***}		
	(0.0202)		
Treatment effect: unemployed		-0.1445^{***}	
		(0.0559)	
Treatment effect: employed		-0.2691^{***}	
		(0.0331)	
Treatment effect: self-employed		-0.2725^{**}	
		(0.0631)	
Treatment effect: agriculture		-0.0038	
		(0.0371)	
Treatment effect: employed–government			-0.3832^{***}
			(0.0698)
Treatment effect: employed–private			-0.2047^{***}
			(0.0452)

Note: This table shows the robustness check results, where the estimation for the average treatment effect of the One-Child Policy on the number of children people had in 2014 is conditional on age. *, **, and *** represent significance levels of 10, 5, and 1 percent. The first row presents the average treatment effect of the policy, integrating over the empirical distribution of the age in 2014 from the dataset. The rest of the rows present the average treatment effect of the policy conditional on women's different characteristics, including job status and employer types. Standard errors, indicated in brackets, were obtained via bootstrapping by sampling the dataset 100 times.

couples from reaching their ideal number of children (Pan and Xu, 2012; Liu et al., 2014). Therefore, we choose to control for both age and housing price as the additional sources of causes for the gap between the ideal number of children and the actual number of children. As before, we model the three conditional distributions $f_{S^{14}(0)|Z^{14}}(s|z)$, $f_{S^{18}|Y^{18},Z^{18}}(s|y,z)$ and $f_{Y^{14}(1)|Z^{14}}(y|z)$ parametrically using logistic regression, where the regressors now include age and provincial-level housing prices. We then use predicted probabilities for the outcome variables as the estimated conditional distribution. We again calculate the conditional treatment effect following Equation 4.11, and integrate over the age and house price distributions of women in 2014 to obtain the final average treatment effect using Equation 4.12. Similar to the first set of robustness checks, we estimate heterogeneous treatment effects by calculating the conditional treatment effect for different subgroups of women and then integrating over the age and house price distributions of these subgroups in 2014 to obtain the average treatment effect. We run the analysis for women from urban/rural areas, Han Chinese and ethnic minorities, different educational levels, and various employer/job types.

Table 6 presents the estimation results conditional on housing prices. The first row presents the average treatment effect of the policy on the number of children people had in 2014, integrating over the empirical distribution of the age and housing price in 2014 from the dataset. The estimated effect (-0.1261^{***}) is slightly smaller than what we have obtained (-0.1583^{***}) in Section 5. The estimated effect for rural women is again significantly negative (-0.0601^{**}), while much smaller than the urban effect (-0.1963^{***}). The effects for other subgroups are also similar to those obtained in the main section, although some effects are smaller in this analysis. However, the main result remains consistent: urban women, Han Chinese women, women with college degrees, and women employed by the government were the most negatively affected groups by the One-Child Policy in terms of childbearing.

In summary, we run two sets of robustness checks by allowing the weakest assumption among the three versions that the gap between the ideal number of children and the actual number of children over the years depends on not only policy restrictions but also other factors including the housing price and the age structure. By adopting Theorem 1, we estimate and show that the results are close to what we have obtained in Section 5, thus validating our main findings on estimating the treatment effect of the One-Child Policy on the number of children couples had in 2014.

7 Conclusion

This paper uses a self-reported survey measure of the ideal number of children to identify the treatment effect of the One-Child Policy on the number of children couples had. We utilize the self-reported information to identify the treatment effect because, often the time, the individual herself has the best knowledge of her heterogeneous potential outcomes. Taking advantage of the fact that couples were asked about their ideal number of children without

Table 6: Treatment Effect of One-Child Policy On Number of Children: Age and Housing Price Adjusted (I)

Treatment Effect	Total Population	Urban/Rural Status	Ethnic Groups	Education
Total Treatment effect	-0.1261^{***} (0.0239)			
Treatment effect: urban		-0.1963^{***} (0.0373)		
Treatment effect: rural		-0.0601^{**} (0.0302)		
Treatment effect: Han Chinese			-0.1367^{***} (0.0256)	
Treatment effect: ethnic minorities			-0.0360 (0.1354)	
Treatment effect: less than high school				-0.0636^{**} (0.0264)
Treatment effect: high school graduate				-0.1721 (0.1112)
Treatment effect: college and higher				-0.3952^{***} (0.0531)

Note: This table shows the robustness check results, where the estimation for the average treatment effect of the One-Child Policy on the number of children people had in 2014 is conditional on housing prices. *, **, and *** represent significance levels of 10, 5, and 1 percent. The first row presents the average treatment effect of the policy, integrating over the empirical distribution of the age in 2014 from the dataset. The rest of the rows present the average treatment effect of the policy conditional on women's different characteristics, including urban/rural status, ethnicity, and educational levels. Standard errors, indicated in brackets, were obtained via bootstrapping by sampling the dataset 100 times.

considering any policy restrictions in 2014, the answer to this survey question reveals information about how many children couples would have in a counterfactual world with no policy restrictions. Since the question was asked again in 2018 when the One-Child Policy was removed and by making the assumption that the conditional distribution of the ideal number of children on the actual number of children should stay the same across years if there were no policy restrictions, we are able to identify and estimate the treatment effect of the policy on the number of children couples had in 2014.

The results suggest that, on average, couples in 2014 had 0.1583 fewer children than

Table 7: Treatment Effect of One-Child Policy On Number of Children: Age and Housing Price-Adjusted (II)

Parameters	Total Population	Employment Type	Employer
Total Treatment effect	-0.1261^{***}		
	(0.0201)		
Treatment effect: unemployed		-0.1301^{*}	
		(0.0716)	
Treatment effect: employed		-0.2134^{***}	
		(0.0388)	
Treatment effect: self-employed		-0.2567^{***}	
		(0.0787)	
Treatment effect: agriculture		-0.0248	
		(0.0421)	
Treatment effect: employed–government			-0.3541^{***}
			(0.0764)
Treatment effect: employed–private			-0.1389^{**}
			(0.0554)

Note: This table shows the robustness check results, where the estimation for the average treatment effect of the One-Child Policy on the number of children people had in 2014 is conditional on housing prices. *, **, and *** represent significance levels of 10, 5, and 1 percent. The first row presents the average treatment effect of the policy, integrating over the empirical distribution of the age in 2014 from the dataset. The rest of the rows present the average treatment effect of the policy conditional on women's different characteristics, including urban/rural status, ethnicity, and educational levels. Standard errors, indicated in brackets, were obtained via bootstrapping by sampling the dataset 100 times.

they would have had if the One-Child Policy had not been in place at that time, which demonstrates that the One-Child policy had a significant negative impact on the fertility rates of people. Meanwhile, we estimate the heterogeneous policy effect for various subgroups of people and show that there exists substantial variations in the policy effect among people with different socioeconomic statuses. In particular, women of high education, who live in urban areas and work for the government are the most affected population. Women living in rural areas, working in the agricultural sector, and Han Chinese were much less affected by the policy in terms of fertility behavior. Finally, we identify and estimate the variations in treatment effects among provinces. The results partially reflect differences in the strictness of policy regulations at the aggregate provincial level.

We admit that the identification of our study relies on the crucial assumption that the conditional distribution of the ideal number of children on the actual number of children does not vary across time if there were no policy restrictions. To further support our main results, we conduct additional robustness checks, relaxing the assumption to allow the disparity between the ideal number of children and the actual number of children to depend on housing prices and couples' age. The average treatment effects, controlling for age and housing prices, are similar to those obtained in the main results, thereby validating our estimated policy effects.

In summary, this paper proposes a novel method utilizing the self-reported survey measure of the ideal number of children to identify the treatment effect of the One-Child Policy on couples' fertility outcomes. We provide empirical evidence of the significantly negative impact of the One-Child Policy on fertility outcomes in China across different demographic groups. By leveraging the unique feature of the self-reported measure that asks about couples' fertility preferences in a counterfactual setting, and relying on reasonable assumptions, we estimate the counterfactual number of children couples would have had without the One-Child Policy. Our method enhances the literature on identifying and estimating the effect of the One-Child Policy by using self-reported information instead of relying on exogenous policy variations. This approach offers a clean identification strategy that can be widely applied to other policy evaluation questions where self-reported information is available.

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A Proof for Theroem 1

We can write the marginal distribution of $S^b(0)$ using the following equation:

$$f_{S^b(0)|Z^b}(s|z) = \sum_{y \in \mathcal{Y}^b(0)} f_{S^b(0)|Y^b(0),Z^b}(s|y,z) f_{Y^b(0)|Z^b}(y|z), \tag{A.1}$$

where $f_{Y^b(0)|Z^b}$ is the conditional distribution of $Y^b(0)$ given Z^b. Before the relaxation of the One-Child policy, the individuals are asked to self-report the number of children that the couples would want without considering the policy constraint. Therefore, we observe $S^b(0)$ as S^b

$$S^b(0) = S^b. \tag{A.2}$$

In other words, the self-reported ideal number of children S_j observed in the data equals the potential self-reported ideal number of children without any policy constraints. In addition, after the policy constraint has already been removed, we have

$$S^a(0) = S^a, \quad Y^a(0) = Y^a. \tag{A.3}$$

Given Assumption 1, we can plug equation A.3 into equation A.1, which gives

$$\begin{aligned} f_{S^b(0)|Y^b(0),Z^b} &= f_{S^a(0)|Y^a(0),Z^a} \\ &= f_{S^a|Y^a,Z^a}. \end{aligned} \tag{A.4}$$

Plugging equation A.2 and A.4 into equation A.1, we have

$$f_{S^b|Z^b}(s|z) = \sum_{y \in \mathcal{Y}^b(0)} f_{S^a|Y^a,Z^a}(s|y,z) f_{Y^b(0)|Z^b}(y|z). \tag{A.5}$$

We define

$$\begin{aligned} \overrightarrow{p(S^b|Z^b=z)} &= [f_{S^b|Z^b}(s_1^b|z), f_{S^b|Z^b}(s_2^b|z), ..., f_{S^b|Z^b}(s_L^b|z)]^T \\ M_{S^a|Y^a,Z^a=z} &= \left[f_{S^a|Y^a,Z^a}(s_l^a|y_p^a,z)\right]_{l=1,2,...,L;p=1,2,...,L} \\ \overrightarrow{p(Y^b(0)|Z^b=z)} &= [f_{Y^b(0)|Z^b}(y_1^b|z), f_{Y^b(0)|Z^b}(y_2^b|z), ..., f_{Y^b(0)|Z^b}(y_L^b|z)]^T. \end{aligned}$$

Equation A.5 is then equivalent to

$$\overrightarrow{p(S^b|Z^b = z)} = M_{S^a|Y^a,Z^a=z} \times \overrightarrow{p(Y^b(0)|Z^b = z)}.$$

Since both $\overrightarrow{p(S^b|Z^b = z)}$ and $M_{S^a|Y^a,Z^a=z}$ can be estimated from the data, by conducting matrix inversion, we obtain the conditional distribution of $Y^b(0)$ given Z^b:

$$\overrightarrow{p(Y^b(0)|Z^b = z)} = M^{-1}_{S^a|Y^a,Z^b=z} \cdot \overrightarrow{p(S^b|Z^b = z)}. \tag{A.6}$$

Notice that $\overrightarrow{p(Y^b(0)|Z^b = z)}$ contains the same information as the distribution $f_{Y^b(0)|Z^b}(\cdot|z)$. Therefore, we can identify and estimate $f_{Y^b(0)|Z^b}$ from equation A.6. The conditional average treatment effect of the One-Child Policy on the number of children couples had given Z^b can be then estimated using the following equation:

$$\begin{aligned} E[Y^b(1) - Y^b(0)|Z^b = z] &= E[Y^b(1)|Z^b = z] - E[Y^b(0)|Z^b = z] \\ &= E[Y^b|Z^b = z] - (y^b_1, y^b_2, ..., y^b_L) \times \overrightarrow{p(Y^b(0)|Z^b = z)}, \end{aligned} \tag{A.7}$$

where $E[Y^b|Z^b = z]$ represents the expected number of children a couple has before the policy was relaxed in year j, given $Z^b = z$. This expectation can be estimated using observed data. Additionally, $\overrightarrow{p(Y^b(0)|Z^b = z)}$ can be estimated using equation A.6. QED.