Testing the External Effect of Household Behavior

The Case of the Demand for Children

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ABSTRACT

This paper tests the external effect of household childbearing behavior by drawing on microfertility data from China. The test is executed by regressing one woman's fertility on the average fertility of neighboring women. China's unique affirmative birth control policy provides us with quasi-experimental fertility variation that facilities identification. We present two identification methods: (1) Testing the external effect from the dominant Han Chinese on minority women by using the fertility fine as an instrumental variable; and (2) identifying the external effect using an instrumental variable that is based on the difference-in-differences. We find that fertility has a large external effect.

I. Introduction

Social scientists have found that people tend to imitate the consumption behavior of their friends or neighbors. For instance, teenagers may use drugs or drink alcohol when their friends do so (Gaviria and Raphael 2001). College students tend to aim for high grades when their roommates have high grade point averages (Sacerdote 2001). There are also many examples of the external effect in investment or other types of behavior. For example, Foster and Rosenzweig (1995) find that rural

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households are more likely to use fertilizers if their neighbors have used them. Hong, Kubik, and Solomon (2000) find that security analysts tend to imitate each other's forecasts on corporate returns. In all of these examples, one person's behavior has some external effect on others.[1]

A number of studies offer theoretical explanations for imitative behavior (Akerlof 1997; Becker 1991; Bernheim 1994; Ellison and Fuderberg 1995; Glaeser, Sacerdote, and Scheinkman 1996). Bikhchandani, Hirshleifer, and Welch (1992) summarize these into four primary mechanisms: (1) sanctions on deviants, (2) positive payoff externalities, (3) conformity preference, and (4) communication.[2] In different economic or social contexts, these mechanisms may work individually or jointly to generate external effects from human behavior.

In this paper, we study the external or neighborhood effect of a unique consumption and/or investment good, that is, children. The idea that the demand for children, or fertility choice, has an external effect was first suggested by Dasgupta (1993, 1995, and 2000). Dasgupta uses the theory of externality to explain the puzzle of why fertility rates remain high when mortality has fallen dramatically in contemporaneous developing countries. He argues that families within a community tend to imitate each other in fertility decisions and in actions that determine fertility, such as the use of contraceptives, the timing of breast feeding, and the frequency of intercourse. Moreover, he suggests that imitative behavior with regard to fertility is caused by some or all of the aforementioned mechanisms.[3] When there are strategic complementarities (Cooper and John 1988; Bernheim 1994; Bongaarts and Watkins 1996), or the marginal utility to a family of having an additional child is increasing in the number of children in other families, families in a community "collectively" choose an equilibrium fertility level, either high or low. This imitation behavior will sustain the equilibrium—or the high fertility caused by historically high mortality—unless some external shocks force a transition to a new equilibrium.

Testing the external effect of fertility, however, is complicated. To test the external effect, one needs to regress the fertility of one family on the average fertility of other families in a community. If the coefficient on the average fertility variable is positive, then we can claim that there is an external effect. This simple regression method, however, is biased for three reasons (Evans, Oates, and Schwab 1992; Manski

1. Social scientists, including economists, have given a number of names to external effects. Depending on the contexts, these effects can be termed "social norms," "peer influence," "neighborhood effects," "conformity," "imitation," "contagion," "epidemics," "bandwagons," "herd behavior," "social interactions," or "interdependent preferences" (Manski 1993). In this paper, we use the terms "external effect," "community effect," and "neighborhood effect" interchangeably.

2. Also see Glaeser and Scheinkman (1999) for a recent survey of the theoretical literature.

3. As argued by Dasgupta (1993), people enjoy being the same as others. In this case, households enjoy having the same number of children as their neighbors (the third mechanism). He further argues that the number of children can determine a household's social status. As a result, households following the norm will be rewarded with a high social status (the second mechanism), whereas those who deviate may be looked down upon (the first mechanism). A household may also imitate the fertility behavior of other households through communication or social learning, as defined by Kohler, Behrman, and Watkins (2001). A household may have limited information about the optimal number of children they should have, because both the costs of raising children and the benefits of old-age security from children occur in the future. When making a choice under uncertainty, it is rational for a risk-averse household to learn from others, because if everybody is doing it, then it is very likely to be the optimal choice.

1993). First, there is a simultaneity bias due to the two-sided nature of the external effect: the average fertility of other people, as a regressor, is also affected by the fertility of the studied family, especially if the community concerned is not large. Second, the fertility of all households in a community may be affected by the same community variables, for example, the fixed costs of raising a child, which may not be observed by an econometrician. Third, when selecting residential locations, households may endogenously sort themselves by fertility preferences.

The main innovation in this paper is that we identify the causal external effect of fertility by making use of a unique aspect of China's national birth control policy. The country began its one-child policy in 1979. Under this policy, each family is allowed only one child, and there are fines on second or higher-parity births. The one-child-per-family policy, however, is only applied to the Han Chinese, and, through affirmative policies, all ethnic minorities in China were allowed to have two or more children until the end of the 1980s. In some provinces, such as Tibet, there is no restriction on the number of children per family.

This unique affirmative policy provides exogenous fertility variation to identify the causal external effect of fertility. Intuitively, if a fertility policy affected one's neighbor's fertility, but left one's own incentives unchanged, then we could observe the causal effect of outside fertility on fertility choice. More specifically, we exploit quasi-experimental fertility variation to present two methods of identification. First, we test one side of the external effect, that of the Han on minorities. Because community birth control policies, such as fining families for second births, are only applied to Han women, they become valid instrumental variables to identify the external effect from Han on minority women. Second, we use a difference-in-differences estimator, which exploits the fertility difference between Han Chinese and ethnic minorities, both before and after the policy change. Specifically, we use the interaction of the proportion of minority women in a community with the age structure of women in a community as an instrument that identifies the external effect only by using the exogenous variability in the fertility of neighbors that results from the enactment of the policy.

Employing China Health and Nutrition Survey Data, we find that fertility has a large external effect. This finding is robust for both estimation methods and to a series of sensitivity tests. An increase in the proportion of second children in neighboring households by one percentage point increases a household's probability of having a second child by 0.5 -0.9 percentage points.

This paper, to the best of our knowledge, is the first to deal with omitted variable bias in empirically testing the external or neighborhood effect of fertility. Most existing studies have been based on either theoretical or historical evidence (Dasgupta 1995).[4] Our finding that the probability of a household having a second child decreases when the

4. There are two earlier empirical studies on related issues, but neither uses systematic econometric methods to estimate the external effect. Easterlin, Pollak, and Wachter (1980) find that people growing up in larger families tend to have more children, and Watkins (1990) shows, by using historical data, that fertility differences among households in a community decline over time. Kohler, Behrman, and Watkins (2001) appear to be the first researchers to attempt to identify the mechanisms through which the external effect of contraceptive usage takes place, but they do not address endogeneity or omitted variable bias issues in their paper.

proportion of neighboring households having a second child decreases confirms Dasgupta's theoretical hypothesis.[5]

The rest of the paper is structured as follows. In Section II, we briefly describe household residence registration and birth control policies. We discuss how birth control policies differ between the Han Chinese and minorities. In Section III, we specify our empirical strategy. In Section IV, we introduce the survey data. In Section V, we test the external effect of fertility. Section VI concludes the study.

II. Population and Household Registration Policies in China

In this section, we briefly describe the special policy environment in China, upon which our empirical strategies are built. Specifically, we concentrate on two important household policies: the one-child policy and the household registration system.

A. The One-Child Policy

China started its unique one-child-per-family policy in 1979. Under this policy, each household is allowed only one child. Households are given birth quotas, and births that are "above-quota" are penalized. To facilitate our later analysis, we classify birth control policies into two categories: national-level and community-level policies.

One unique aspect of the national policy is that it is an affirmative policy. The government has enacted tighter control over the birth rate of Han Chinese compared to minorities, who are normally allowed to have more children (Anderson and Silver 1995; Hardee-Cleaveland and Banister 1988; Park and Han 1990; Peng 1996; Qian 1997). In some provinces, such as Xinjiang, minorities can have as many as four children. In rural areas of Tibet, there are no restrictions on the number of children minority families can have. In April 1984, five years after the one-child policy had been implemented for the Han, the Chinese government for the first time stated that there should also be birth control policies for minorities, but that these policies should be less restrictive (CCCPC 1994; Hardee-Cleaveland and Banister 1988). However, until the end of 1988, minorities were allowed to have a second child (Deng 1995). Starting from the late 1980s, ethnic groups with a population larger than 10 million are subject to the same policy as the Han. The Zhuang were the only ethnic group with a population larger than 10 million at the end of the 1980s, and most of them live in Guangxi. On September 17, 1988, the Guangxi provincial government introduced the one-child policy for ethnic Zhuang families (Guangxi Autonomous Government 1988), and other provinces started to apply the same policy to Zhuang families in the 1990s. By 1990, the population of Manchu, the second largest ethnic group in China, also topped 10 million, and they thus also came under the restriction of the one-child policy. To summarize, for

5. Our study, however, does not try to differentiate several mechanisms of external effects. For example, a household may choose to have fewer children because all of its neighbors are having fewer children, but it may also have fewer children because it reduces the frequency of intercourse in the knowledge that its neighbors are having less intercourse. Such differentiations are not likely given data limitations.

most of the 1980s, minorities were allowed to have more than one child, which provides a unique quasi-experiment to test the external effect of fertility.[6]

It should also be noted that a number of Han households may not be subject to the one-child policy. For example, the Central Committee of the Chinese Communist Party (CCCPC) issued a policy document in 1982 that lists all of the conditions under which a Han household may have a second child (Qian 1997). One condition allows Han households in remote areas with majority non-Han populations to have a second child.[7] This policy means that in most minority communities, which are usually located in remote mountainous areas, both minority and Han households can have a second child. In some rural areas, Han households may be allowed to have a second child if their first child is a girl (Hardee-Cleaveland and Banister 1988).

To implement the birth control policies, local (including community) governments are given incentive contracts by the governments above them. These take the form of fiscal rewards for fulfilling birth targets and heavy penalties for falling short (Short and Zhai 1998). Moreover, government officials may be demoted for allowing too many above-quota births in their community, which means that they will lose future income and other benefits associated with government positions.

These community policies demonstrate great heterogeneity across localities in terms of strictness. At the community level, one-time fines have been the primary penalty used by local government officials for above-quota births (Short and Zhai 1998). Various studies have shown that these fines are heavy and vary enormously across communities. They range from 20 to 200 percent of a household's annual income (Li 1995; Short and Zhai 1998). Even at the lower end of the range, the fines are still substantial, especially in light of the fact that many households in rural areas are still below the poverty line.[8] Empirical studies have also shown that fertility decreases with the size of fines (Li and Zhang 2005).[9]

B. Household Registration System

In the early 1950s, the Chinese government established the household registration system to consolidate socialist governance, control population flow, and administer the planned economy.[10] This system requires that a person be registered where he or she is born. Each household has a registration certificate (hukouben) that records

6. Even though the one-child-policy applied to the Zhuang in Guangxi in September 1988, it was only applied to women who fell pregnant after the issuance of the policy. Generally speaking, the earliest date that these women could have a baby was July 1989, which should not affect our sample, which was collected in June 1989.

7. Other conditions include, for example, when the first child is disabled or adopted.

8. Twenty-four percent of the families in our sample fall below the World Bank's poverty line of one U.S. dollar per day.

9. In a separate paper, we examine the determinants of fines, both theoretically and empirically. There is a tradeoff for local governments between revenue collection and birth control targets in determining the size of the fines. Although a very large fine can minimize above-quota births, local governments then sacrifice revenues from fertility fines, which have become an important part of local budgets. A local government also cannot have too low of a fine, because there will then be too many above-quota births, and local officials could thus be demoted. Our theoretical and empirical work shows that the size of the fine, which is determined by the local government, increases with the income level and the level of political control in a locality, but decreases with the local government's incentives to raise fiscal revenues.

10. Although the Chinese government has been gradually reforming the system since the mid-1990s, the registration system is still very strict in most places.

all members of the household. All administrative activities, such as land distribution, the issuance of ID cards, and the registration of a child in school, are based on this registration certificate. Until the early 1990s, it was also used to distribute food, cooking oil, and clothing coupons, and it has made moving across localities very restrictive in both urban and rural areas (Cheng and Selden 1994).

Individuals, such as migrant workers, who have moved from the location of their permanent residence have to follow the birth control policies of their own villages. Although many migrant workers have moved from rural to urban areas, they are still registered as farmers in their home villages. A migrant woman needs a permit from her home village in order to give birth in an urban hospital. When a migrant woman has an above-quota birth, officials in both her home village and the community in which she gives birth bear responsibility (Goldstein, While, and Goldstein 1997; Hardee-Cleaveland and Banister 1988; The State Council of China 1991). Children, no matter where they are born, can only acquire registration rights or ID cards from their parents' place of permanent residence. If these children are above-quota births, then the government still considers them to be above-quota births for the place of permanent residence, and their parents are required to pay the fine to the administrative unit under which they are registered. If children are not registered, then they become "black" children, who have no ID cards, no right to receive public education and land, and no right to formal jobs. Thus, rural households cannot avoid penalties for above-quota births simply by moving to an urban location (Chan and Zhang 1999).

It is even more difficult to move to another rural community for the purpose of having above-quota children. There are two reasons for this. First, the parents still need to return to their own villages to register their children. Second, above-quota children are not welcomed in the target villages because local villagers neither want to care for above-quota children nor to assign the household a piece of land, which would have to be taken from existing residents (Li and Rozelle 1998).

In summary, China has a very strict residential registration system, which prevents people from moving in general, and from moving for the purpose of bearing children in particular. Although anecdotal evidence indicates that some migrant workers hid temporarily in other places to bear children (Scharping 2003), they eventually have to go back to the village of their permanent residence to get their children registered, and they must accept the penalties then (Johnson 1994; Cai and Lavely 2003; Banister 2004). Moreover, changing one's permanent residency for childbearing purposes is almost impossible.

III. The Empirical Framework

Our empirical work focuses on estimating the following equation,

$$(1)\quad y_i = \beta_0 + \beta_1 y_{-i} + x_i\beta_2 + z\beta_3 + \varepsilon_i$$

where i is any one of the n household in our sample, and $-i$ includes all households other than i in a community. y_i is the number of children in household i, and y_{-i} the average number of children in all other households in the same community. x_i and z are vectors of individual/household and community variables. βs are coefficients to be estimated, and ε is the residual.

This estimation, however, is complicated as the ordinary least squares (OLS) estimate of β_1 is likely to be biased for three reasons. First, y_{-i} is the fertility response of all households other than i, which may also be a function of y_i. Thus, there may be a simultaneity bias. Second, if the relevant community variables in z, which affect the fertility choices of all households in the community, are omitted, then we have an omitted variable bias. In this case, the positive correlation between y_i and y_{-i} is caused by the unobserved community variables, rather than the external effect.[11] Third, bias could also arise if each household i chooses to live in a community in which other households have similar fertility preferences. In this case, households sort themselves according to their preferences for children.

Although all three sources of bias could exist theoretically, the second one matters more in the case of our sample from China. The simultaneity bias can be ignored for reasonably large communities. Moreover, the strict residential registration system in China prevents people from changing their permanent residencies for the purpose of bearing more children. As our sample of households was drawn according to permanent residency (or hu kou), sorting across locations may not be a major problem.[12] Even if a woman is working in another location as a migrant worker, she is still considered to be a resident of the neighborhood of her permanent residence, and thus our definition of neighborhood is not formed by fertility preference sorting. However, it should be noted that to the extent that some women may be migrant workers who are less affected by their neighborhood of permanent residence, the external effect we identified is biased downward.

We thus concentrate on dealing with the bias caused by omitted community variables. The key is to find valid instrumental variables (IVs) for identification. A good IV should be highly correlated with the average fertility of neighboring households, but should not affect the fertility of household i except through the external effect, or through the average fertility of neighboring households. In particular, the IVs should not be correlated with any omitted community variables in z, which affect the fertility of all households. Much of the rest of the paper is devoted to the IVs estimations.

IV. Data

In this paper, we use the China Health and Nutrition Survey (CHNS) data collected by the Carolina Population Center (CPC), the Institute of Nutrition and Food Hygiene, and the Chinese Academy of Preventive Medicine. The survey was conducted in 1989 through face-to-face interviews by an international team of researchers whose backgrounds include nutrition, public health, sociology, Chinese

11. Some researchers call the external effect that is caused by omitted community variables the "contextual" effect, which is in contrast to the "interactive external" effect that we want to identify (Gaviria and Raphael 2001; Manski 1993).

12. Different methods have been used to deal with the bias caused by sorting. Gaviria and Raphael (2001) use family backgrounds as instrumental variables, but they find that sorting according to preferences remains a potential problem for their analysis. By explicitly modeling the sorting process, Evans, Oates, and Schwab (1992) find that most of the claimed peer group effects of teenage behavior can be explained by sorting according to parental characteristics. To solve the sorting problem, Sacerdote (2001) uses randomly assigned college students to study peer effects.

studies, demography, and economics. To ensure data quality, the CPC also has a team of researchers who design and monitor the data collection and entry process and check and correct data errors. The CHNS data are probably the most widely used Chinese data in several fields of research, including economics.

The survey was conducted at both the community and household level. A community refers to a village in a rural area or a neighborhood in an urban area,[13] and is the lowest level of China's administrative hierarchy. The sampled communities were randomly drawn in eight provinces, including wealthy ones, such as Jiangsu, and poor ones, such as Guizhou.[14] The size of a community in our study is generally large. Of the 191 communities in the sample, the average size is 622 households, with a standard deviation of 905. The community survey collected information on local socioeconomic variables, health facilities, price levels, and birth control policies. For our purposes, it collected information on the size of the fine for above-quota births and subsidies for one-child families.

Between 20 and 35 households were randomly drawn from each community, and the survey covers all members formally registered in a household (or those with permanent residence − *hu kou).* In total, we have 3,774 families in the sample. Two-thirds are from rural areas, and one-third are from urban areas; 81 percent are Han Chinese and 19 percent are minorities. The household survey has detailed information on each individual registered in the household, including basic demographic information, labor market activities, time allocation, income, health, and nutrition. The sampled households had a median annual per capita income of 666 yuan in 1989, which is about 10 percent higher than the national average of 602 yuan. Table 1 reports the summary statistics of the sample.

Preliminary examination of the data shows that the average number of children is far more than one per family, despite the fact that the one-child policy had been in place for ten years by 1989. This is not surprising, as the sample includes children born before the one-child policy was implemented in 1979, and some of the rural women may have been allowed to have two children after 1979 if their first had been a girl. On average, each woman has 2.38 children, with a standard deviation of 1.5 (Table 1, Row 1). Some families have as many as nine children. The average of 2.38 is only a little higher than the national average of 2.33 (the 1990 Census of China).[15] When we examine the proportion of families having a second child, we find that 68 percent of married women in our sample have a second child. This is also comparable to the national level of 63 percent.

Comparing Row 2 to 3, we find that minorities on average have 0.08 more children than the Han Chinese. This small fertility gap between the minority and Han families is puzzling. Why, for instance, did the affirmative fertility policy toward minorities not lead to a larger fertility gap between minority and Han households? The small fertility gap could have been caused by the external effect, which we attempt to identify in the following econometric exercises.

13. The Chinese term for village is *cun*, and the term for neighborhood is *jiedao*.
14. The other six provinces are Liaoning, Shandong, Henan, Hubei, Hunan, and Guangxi.
15. The national statistics on children cover all women in the 20-64 age group. Most women (99 percent) in our sample are in this age range.

Table 1
Descriptive Statistics of Fertility and Other Variables in China (N = 3,774)

Variables	Mean	Standard Deviation	Minimum	Maximum
Number of children per household				
Whole sample	2.38	1.50	0	9
Han	2.36	1.52	0	9
Minority	2.44	1.41	0	9
Proportion of households with a second child				
Whole sample	0.68	0.46	0	1
Rural	0.73	0.45	0	1
Urban	0.60	0.49	0	1
Han	0.67	0.47	0	1
Minority	0.73	0.44	0	1
Other variables				
Sex of first child (male = 0, female = 1)	0.494	0.500	0	1
Woman's age	42.7	13.6	19.0	90.0
Woman's education (years of schooling)	7.0	4.0	0	18.0
Per capita income (thousand yuan)	1.027	0.939	0	17.333
Urban dummy	0.34	0.47	0	1
Minority dummy	0.19	0.39	0	1
Fine (thousand yuan)	1.332	1.455	0	6.600
Subsidy (thousand yuan)	0.051	0.112	0	1.000

The data also show that the fines are substantial and vary greatly for the households in the sample. Moreover, although each village had a uniform fine for all of the households in the village, the fines varied greatly across villages (and households). Of the 154 villages for which we have information on the size of the fine, only two did not impose any fine. The average (one-time) fine was 1,332 yuan, which exactly doubles the median per capita income of 666 yuan. The fine as a percentage of annual household income was not only substantial, but also varied greatly. The fine was only 4 percent of household income for the first decile of the percentage, but was ten times the household income for the tenth decile. The large amount and variation of the fine are necessary conditions for it to be a good instrumental variable for fertility.

V. Empirical Results

In this section, we systematically test whether there is an external effect of fertility, and measure the magnitude of the external effect if it exists. To meet this goal, we first test whether the fertility of a household increases with the average fertility of its neighbors by employing OLS estimations. To deal with the omitted variable bias, we exploit quasi-experimental fertility variation provided by China's affirmative birth control policies in two unique ways: (1) by examining the external effect from Han women on minority women in the same community; and (2) by identifying the external effect using IVs that are based on the difference-in-differences.

A. OLS Estimates

In this section, we estimate Equation 1 by OLS. We use the second-child dummy (one if a woman has at least two children, and zero otherwise) as the dependent variable. The independent variables include the sex of the first child, the woman's age and level of education, household per capita income, the urban dummy, and the community (village or neighborhood) mean of the second-child dummy. The key hypothesis is that the coefficient of the community mean of the second-child dummy is positive.

The first column of Table 2 reports the results of a simple OLS regression with the community mean of the second child as the only independent variable. This simple regression shows that a household's fertility increases with the average fertility of the neighboring households. The coefficient on the community average fertility is positive and significant with a t-statistic of 26.73. The magnitude of the effect is also large. When the probability of neighbors having a second child increases by one percentage point, the probability of a woman having a second child increases by about 0.81 percentage points.

In the second column, we include the sex of the first child as an additional covariate. Note that the sex of the first child has a large effect on the likelihood of having a second child. The probability increases by 10.4 percentage points if the first child is a girl. This means that if we compare 100 women whose first child was a girl with another 100 women whose first child was a boy, then there will be ten more women from the first group than the second group who have a second child. After we control for the sex of the first child, the estimate of the external effect only changes slightly.

In the third column, we include age, education, income, and the urban dummy as independent variables. With the inclusion of these household characteristics, the estimate of the external effect becomes a little smaller, but it is still large and very significant. Most of these newly added variables are also significant and give rise to the signs that are expected from previous studies.[16] Fertility increases with a woman's age, because older women were less likely to have been subject to the one-child policy. Fertility decreases with per capita income, and it is lower in urban areas.

Finally, we examine whether the external effect is the same for minorities and Han women. We do this because we will only examine the external effect on minorities in the next subsection. In Columns 4 and 5, we report the OLS results for the minority and Han subsamples, respectively. Interestingly, the external effect is identical for the two groups.

B. The External Effect from Han on Minority Households

1. Identification Method

As argued above, the OLS estimates may be biased because the coefficient on the average fertility variable is likely subject to the omitted variable bias. In this section,

16. See Johnson (1994), Zhang (1994), and Li and Zhang (2005).

Table 2
OLS Regressions Estimating the External Effect of Fertility

	Dependent variable: Second-Child dummy				
Sample	Whole (1)	Whole (2)	Whole (3)	Minority (4)	Han (5)
Second child	0.812***	0.790***	0.698***	0.697***	0.697***
(Community mean)	(26.73)	(26.09)	(21.85)	(8.36)	(20.04)
First child is a girl		0.104***	0.102***	0.058	0.112***
		(5.58)	(5.79)	(1.49)	(5.69)
Age			0.010***	0.009***	0.011***
			(12.11)	(4.05)	(11.40)
Education			-0.000	0.001	-0.000
			(0.07)	(0.24)	(0.14)
Income			-0.030***	-0.018	-0.032***
(thousand yuan)					
			(3.60)	(0.91)	(3.61)
Urban dummy			-0.071***	-0.074*	-0.070***
			(4.32)	(1.88)	(3.86)
Observations	3,774	3,774	3,762	710	3,052
R-squared	0.14	0.16	0.21	0.17	0.21

Notes: Numbers in parentheses are t-statistics robust to heteroscedesticity and clustering at the community level. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***. All regressions include a dummy variable to control for missing values of the sex of the first child (one if missing).

we identify the external effect from Han on minority households by exploring the unique one-child policy. Because the birth control policies, such as a fine for a second child, only applied to Han households, and were exempted for minority households in the sample period, this essentially provides a quasi-experimental variation for identification. Rather than estimating the external effect among all households, we can concentrate on one side of the external effect, that from the Han households to the minority households. Specifically, we estimate a slightly different equation,

$$(2) \quad y^m_{j,i} = \beta_0 + \beta_1 y^h_j + x^m_{j,i}\beta_2 + z_j\beta_3 + \varepsilon_{j,i}$$

where $y^m_{j,i}$ is the fertility of minority household i in community j, and y^h_j is the average fertility of the Han households in community j. We use the size of the fine as an IV to identify the external effect from Han on minority families.

For this identification method to work, the fine should not be correlated with the fertility of minorities except through the external effect. Because minorities are not subject to the one-child policy, their probability of having a second child should not be directly affected by the fine. However, the fine may still be correlated with other community variables that affect the fertility behavior of minority households, thus making it an invalid IV. We discuss the conditions necessary for this identification method to work and some robustness tests below.

In addition to the fine, the set of IVs also includes the interactions of the fine with three age variables, including fine*age, fine*age45, and fine*age45*age, where age45 is a dummy variable that equals one for women who were aged 45 or younger in 1989 (aged 35 in 1979, the year the one-child policy started). We use fine*age to capture the differential effect of the fine on women of different ages, and fine*age45 and fine*age45*age to reflect the fact that the fine may only have a large impact on younger women, as older women (older than 35 in 1979) may have already borne two children by the time the one-child policy started. The first-stage regression is thus specified as follows.

$$y_j^h = \alpha_0 + \alpha_1 F + \alpha_2 F{*}age^h + \alpha_3 F{*}age45^h + \alpha_4 F{*}age45^h{*}age^h + x_{j,i}^m \alpha_5 + z_j \alpha_6 + \mu_{j,i} \tag{3}$$

where F represents the fertility fine, age^h is the mean of age for Han women, and $age45^h$ is the mean of age45 for Han women. The instrumental variables include F, $F^* age^h$, $F^* age45^h$, and $F^* age45^h * age^h$.

2. Results

We first test whether these IVs have explanatory power for the fertility of Han households in the first-stage regressions. In the first three columns of Table 3, we report the estimates of Equation 3 with the average fertility of Han households in a community as the dependent variable. The independent variables include the instrumental variables and other exogenous variables in the second-stage equations. Among the four instruments, only fine*age45 and fine*age45*age are significant, with fine*age45 having a negative coefficient and fine*age45*age having a positive coefficient, which suggests that the fine has a negative effect only for younger Han women and that the magnitude of the effect decreases with age. Importantly, the F-statistics for the joint test of the IVs are very large (at least 25.99), which suggests that these IVs have a high explanatory power for the endogenous variables in the second-stage equations.

The two-stage least squares (2SLS) estimates reported in Column 4 of Table 3 continue to show a large external effect of fertility. The estimated external effect from Han to minority women is positive and significant, although the magnitude of the effect is smaller compared to that in Table 2. To statistically examine the validity of our IVs in this natural experiment, we conduct a Hausman overidentification restriction test. The test results reported in Table 3 show that our IVs can be excluded from the second-stage regression.[17]

3. Robustness Check

One concern is that minorities may also be affected by the one-child policy. Although minorities are allowed to have two children, they may also receive a subsidy

17. The Hausman test is a Lagrange multiplier test (Hausman 1983). The chi-square distributed test statistic with k-1 degrees of freedom, where k is the number of IVs, is $N{*}R^2$, where N is the number of observations, and R^2 is the measure of goodness of fit of the regression of the residuals from the second-stage equations on the variables, which are exogenous to the system. The test statistics for the IVs in Table 3 are smaller than the critical value, which indicates that the null hypothesis that there is no correlation between the exogenous instruments and the error term from the second-stage equation cannot be rejected.

Table 3
2SLS Estimates of the External Effect of Fertility from Han Women to Minority Women

Dependent variable	Second child (Han community mean)			Second-child dummy		
Model	OLS (first stage)			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Second child (Han community mean)				0.668***	0.655***	0.636**
				(3.53)	(3.52)	(2.47)
First child is a girl	-0.037	-0.029	-0.035	0.068	0.055	0.068
	(1.62)	(1.27)	(1.60)	(1.51)	(1.23)	(1.50)
Age	0.002	0.002	0.002	0.009***	0.009***	0.010***
	(1.41)	(1.51)	(1.49)	(4.28)	(4.21)	(4.27)
Education	-0.005*	-0.004	0.001	0.001	-0.000	0.001
	(1.70)	(1.42)	(0.30)	(0.22)	(0.09)	(0.26)
Income	-0.026***	-0.025***	-0.004	-0.016	-0.019	-0.010
(Thousand yuan)	(2.77)	(2.77)	(0.52)	(0.81)	(0.97)	(0.47)
Urban dummy	-0.043**	-0.042*	0.013	-0.068*	-0.070*	-0.054
	(2.00)	(1.94)	(0.65)	(1.68)	(1.75)	(1.26)
One-child subsidy		-0.592***			0.959**	
		(3.45)			(2.58)	
Education			-0.028***			-0.0003
(Community mean)			(4.35)			(0.02)
Income			-0.106***			-0.041
(Community mean)			(5.47)			(0.74)

IVs (Han community mean)						
Fine	-0.050	-0.015	-0.106			
	(0.48)	(0.14)	(1.04)			
Fine*age	-0.000	-0.001	0.001			
	(0.18)	(0.49)	(0.44)			
Fine*age45	-0.492***	-0.545***	-0.355***			
	(3.94)	(4.50)	(3.00)			
Fine*age45*age	0.014***	0.015***	0.011***			
	(4.90)	(5.42)	(4.01)			
F-statistics for IVs	29.88***	32.03***	25.99***			
Hausman tests						
Test statistics				2.84	2.39	3.34
Critical value (5%)				7.81	7.81	7.81
Observations	630	630	630	630	630	630
R-squared	0.19	0.21	0.27			

Notes: Numbers in parentheses are *t*-statistics robust to heteroscedesticity and clustering at the community level. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***. Columns 1 to 3 report the first stage regressions for Columns 4 to 6. Age45 is a dummy variable (one if age is less than or equal to 45, zero otherwise). The number of observations (minority women) is down from 710 to 630 due to missing values for some independent variables. All regressions include a dummy variable to control for missing values of the sex of the first child (one if missing).

for complying with the one-child policy, just as Han Chinese do. If such a subsidy has an impact on the fertility of both minority and Han women, and if this is correlated with the fine, then the fine becomes an invalid IV. The CHNS survey, however, collected information on the one-child subsidy so that we can directly test how important the one-child subsidy is in affecting fertility. The data suggest that the one-child subsidy is not very important. The average amount of the subsidy is 51 yuan per year,[18] which is only 3.8 percent of the average fine of 1,332 yuan. The amount of the subsidy seems to be too small to have any significant impact on the fertility behavior of households. Indeed, 2SLS regressions that include the subsidy as a control variable (Column 5 of Table 3) show that the amount of the subsidy has no significant impact on fertility, and its sign is even positive. Moreover, by controlling for the amount of subsidy, the estimated external effect does not change much.

Another concern is that the fertility fine may be correlated with other community-level variables that affect fertility and that this correlation would invalidate the instruments. Generally speaking, it is very difficult to completely solve this problem, as many of the variables may not be observable or known to researchers. However, we can still address this concern partially by conducting several sensitivity tests.

First, we test whether the estimate of the external effect is sensitive to the inclusion of community-level variables such as average education and income. Column 6 of Table 3 shows that the estimated external effect of this augmented model is also positive and significant and that the magnitude of the effect does not change much. Moreover, the included community-level education and income are not significant themselves. These results suggest that the omission of these community-level variables may not have biased our IV estimation.

Second, we directly examine whether the IVs are correlated with the educational level and income of minority households. We execute the test by regressing the educational level and income of minorities on all of the exogenous variables, including the IVs. The results are reported in Table 4 (Columns 1 and 2). Interestingly, our IVs are not highly correlated with the educational level and income of minority households, as the IVs are neither individually nor jointly significant at the 5 percent level. These results also support the use of fine variables as IVs.

Finally, we run a falsification test. Specifically, we apply our identification method to a sample of women who were aged 45 or above at the time of the survey (35 or older in 1979, when the one-child policy started). Presumably, these women should not be affected by the one-child policy, and thus our identification should not work for this sample of older women. If, using this sample, we find a similar external effect to that before, then our identification method could be misspecified. The falsification test results reported in Columns 3 and 4 of Table 4 do not suggest such misspecification. Neither does the fine have a negative effect on fertility in the first-stage regression, and nor is there a positive external effect identified in the second-stage regression.

Another concern is that we have used linear regressions, ignoring the fact that the dependent variable is binary. To examine whether the results are sensitive to

18. Parents can get the one-child subsidy until the child reaches 15.

Table 4

Sensitivity Tests for the External Effect of Fertility from Han Women to Minority Women (Minority Sample)

	Correlation of fine with observables		2SLS using a sub-sample of older women		Probit models	
	OLS	OLS	First stage	Second stage	Probit	IV-probit
Dependent variable	Education (minority) (1)	Income (minority) (2)	Second child (mean) (3)	Second-child dummy (4)	Second-child dummy (5)	Second-child dummy (6)
Second child				-0.446	0.460***	0.632**
(Community mean)				(0.83)	(5.72)	(2.33)
First child is a girl	1.393***	-0.120*	-0.088	-0.142	0.056	0.068
	(4.96)	(1.89)	(0.71)	(0.62)	(1.31)	(1.47)
Age			-0.001	-0.008**	0.010***	0.010***
			(0.46)	(1.98)	(3.88)	(3.84)
Education			0.000	0.011*	0.001	0.002
			(0.01)	(1.68)	(0.09)	(0.29)
Income			-0.019	-0.145***	-0.025	-0.011
(Thousand yuan)			(1.09)	(3.15)	(1.19)	(0.52)
Urban dummy	0.527	-0.028	0.023	0.024	-0.083*	-0.057
	(1.39)	(0.31)	(0.65)	(0.37)	(1.90)	(1.24)
Education	0.764***	0.017				-0.001
(Community mean)	(10.47)	(1.23)				(0.05)
Income	-0.014	0.863***				-0.049
(Community mean)	(0.04)	(7.83)				(0.82)

(*continued*)

Table 4 (*continued*)

	Correlation of fine with observables		2SLS using a sub-sample of older women		Probit models	
	OLS	OLS	First stage	Second stage	Probit	IV-probit
Dependent variable	Education (minority) (1)	Income (minority) (2)	Second child (mean) (3)	Second-child dummy (4)	Second-child dummy (5)	Second-child dummy (6)
IVs						
Fine	0.277	0.808*	0.394***			
(Thousand yuan)	(0.15)	(1.71)	(4.08)			
Fine*age	0.004	-0.014	-0.007***			
	(0.11)	(1.52)	(4.10)			
Fine*age45	-0.679	-0.766				
	(0.34)	(1.41)				
Fine*age45*age	0.003	0.013				
	(0.08)	(1.15)				
F-statistics for IVs	2.01*	1.59				
Observations	685	685	200	200	630	630
R-squared	0.21	0.21	0.10		0.11	

Notes: Numbers in parentheses are t-statistics robust to heteroscedesticity and clustering at the community level. For IV-probit model in Column 6, t-statistics calculated using bootstrapped standard errors. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***. Columns 5 and 6 use a subsample of women aged at least 35 in 1979, the year one-child policy started. Column 5 reports the first stage regression for Column 6. All regressions include a dummy variable to control for missing values of the sex of the first child (one if missing).

regression models, we employ probit and IV-probit models.[19] In the last two columns of Table 4, we report the marginal effect, dF/dx, to make them comparable to the coefficients from the linear regressions. For the IV-probit models, the standard errors are obtained by bootstrapping. Generally speaking, the probit and IV-probit estimates of the external effect (Columns 5 and 6 of Table 4) are very similar to the OLS and 2SLS estimates, thus supporting the use of linear models for our main analysis.

C. Identification Based on Difference-in-differences

1. Identification Method

The affirmative birth control policy allows us to identify the impact of the one-child policy on fertility of an individual by a difference-in-differences (DD) method. Similar to the first identification method in Part B of Section V, this identification method also explores the fact that the one-child policy only reduces fertility of those Han women who were still young when the policy started. Thus, the average fertility level in a community should depend on the interaction of the proportion of minority (Han) women and the age structure of these women.

Define M_i as the minority indicator that equals one for a minority household and zero otherwise. Define T_i as the proportion of a woman's childbearing period subject to the one-child policy. Because the earliest childbearing age is 14 in our sample, the maximum menopause age is 55 (World Health Organization 1996), and the survey year of 1989 is ten years after the one-child policy was implemented, we define T_i in three age ranges: $T_i = 10/(age_i - 14)$ for women with $24 < age_i \leq 65$ in 1989, $T_i = 1$ for women with $age_i \leq 24$, and $T_i = 0$ for women with $age_i > 65$.[20]

To identify the external effect specified in Equation 1, the first-stage regression is specified as

$$(4) \quad y_{-i} = \alpha_0 + \alpha_1 M_{-i} + \alpha_2 T_{-i} + \alpha_3 M_{-i} T_{-i} + x_i \alpha_4 + w\alpha_5 + \varepsilon_{-i}$$

where M_{-i}, the community average of M_i, is the percentage of minority women in the neighborhood, T_{-i}, the community average of T_i, is the average proportion of women's childbearing period subject to the one-child policy. w is a vector of community variables and is similar to z in Equation 1. The variable M_{-i} picks up the effect of the percentage of minority on the community fertility level, and T_{-i} picks up the effect of the proportion of young women on the community fertility level.[21]

The coefficient on the interaction term, or α_3, is the DD estimator for the effect of the one-child policy on community-level fertility. Assuming that without the one-child policy, the change of fertility for minority and Han women would be the same between 1979 and 1989,[22] then the interaction term picks up the effect of

19. The IV-probit model is implemented by inserting the predicted value of the average fertility into the probit model. The standard errors are obtained by bootstrapping.

20. Note that although minority households were not subject to the one-child policy, T_i is defined using the same formulas.

21. With the first stage specification in Equation 4, z in Equation 1 should now be understood as the community variables including M_{-i} and T_{-i} as well. Likewise, x_i in Equation 1 should now be individual/household variables including M_i, T_i, and $M_i T_i$.

22. This is the same as assuming that α_3 is zero without the one-child policy.

the one-child policy on fertility at the community level. This interaction term ($M_{-i}T_{-i}$) will serve as an IV to identify the external effect using only the exogenous increase of fertility in a community (relative to other communities) that is caused by its larger proportion of minority women falling into the childbearing ages. Essentially, this model will identify the effect of neighbors' fertility on the studied woman's fertility using only the exogenous variability in the fertility of the neighbors that results from the introduction of the affirmative one-child policy. Note that this method allows us to estimate the external effect for all women, rather than that from Han on minority women (the first identification method).

2. *Results*

Before reporting the estimates of the external effect, we first examine the performance of our new instrumental variable. In the first two columns of Table 5, we report the first-stage regressions—the estimations of Equation 4. These regressions indeed show that the birth control policy has had a positive effect on the probability of having a second child. In particular, note that the coefficient on our IV, the community mean of minority multiplied by the community mean of the proportion of childbearing period overlapped with the one-child policy period (or $M_{-i}T_{-i}$), is positive and significant, which suggests that the fertility gap between minority and Han households is increasing in younger generations.

The 2SLS and IV-probit regressions reported in Columns 3 to 6 using these new IVs confirm our early findings. The coefficient on the external effect is positive and significant in all specifications. The magnitude of the effect (between 0.727 and 0.926) is rather close to previous estimates.

3. *Robustness Check*

The key identification assumption of the DD method is that without the one-child policy, the change of fertility in minority and Han women would be the same between 1979 and 1989. This assumption will be violated if there are other socioeconomic changes that affect the fertility of minority and Han women differently in the same period. In these cases, the DD method may have simply picked up the effects of other variables on fertility. Generally speaking, it is very difficult to control for all of these variables, because we would not know what is in force a priori.

We use two methods to partially address this concern. First, we control for observable community variables that could affect the fertility of minority and Han differently, such as community-level income and education. If they are important determinants of fertility and are correlated with the DD estimator, then including these variables will reduce the estimated external effect. The results reported in Columns 4 and 6 in Table 5 show that the inclusion of these community variables does not reduce the estimated external effect, thus suggesting that there is no evidence that the DD method is picking up other effects.

Second, we directly test whether the DD estimator is picking up other effects by using alternative outcome variables as dependent variables (in the first-stage regressions). In particular, we employ education and income as alternative dependent variables for the first-stage regressions to check whether the Han-minority gap for these

Table 5
2SLS Estimates of the External Effect of Fertility Using the DD Estimator as an Instrumental Variable

Dependent variable	Second child (community mean)		Second-child dummy			
Model	OLS First stage (1)	OLS First stage (2)	2SLS (3)	2SLS (4)	IV-probit (5)	IV-probit (6)
Second child (community mean)			0.727*	0.743*	0.880*	0.926*
			(1.78)	(2.02)	(1.67)	(1.64)
First child is a girl	0.003	0.004	0.137***	0.137***	0.152***	0.152***
	(0.32)	(0.38)	(6.12)	(6.13)	(5.96)	(5.88)
Minority	-0.001	-0.002	-0.090	-0.091	-0.082	-0.086
	(0.05)	(0.06)	(1.05)	(1.06)	(1.09)	(1.15)
Proportion of childbearing years	0.408***	0.349***	-0.810***	-0.833***	-0.796***	-0.826***
	(4.88)	(4.46)	(2.66)	(2.80)	(2.73)	(2.98)
Minority*proportion	-0.023	-0.029	0.279	0.280	0.269**	0.274**
	(0.45)	(0.59)	(1.60)	(1.59)	(2.07)	(2.11)
Age	0.021***	0.019***	0.027	0.026	0.027*	0.026
	(4.46)	(4.34)	(1.64)	(1.60)	(1.65)	(1.54)
Age squared	-0.000***	-0.000***	-0.000***	-0.000***	-0.0004***	-0.0004***
	(4.06)	(3.96)	(3.05)	(3.02)	(2.89)	(2.73)
Education	-0.003**	-0.001	-0.000	0.001	-0.0005	0.0005
	(2.38)	(0.52)	(0.02)	(0.30)	(0.12)	(0.13)
Income (thousand yuan)	-0.028***	-0.003	-0.035*	-0.030**	-0.036*	-0.032***
	(6.03)	(0.75)	(1.85)	(2.00)	(1.85)	(2.60)

(*continued*)

Table 5 (*continued*)

Dependent variable	Second child (community mean)		Second-child dummy			
Model	OLS First stage (1)	OLS First stage (2)	2SLS (3)	2SLS (4)	IV-probit (5)	IV-probit (6)
Urban dummy	-0.100***	-0.076***	-0.002	0.006	0.002	0.010
	(6.30)	(5.10)	(0.04)	(0.16)	(0.03)	(0.17)
Community mean						
Education		-0.018***		-0.011		-0.010
		(4.50)		(1.29)		(0.65)
Income		-0.144***		-0.021		-0.008
		(12.87)		(0.37)		(0.08)
Minority	-0.795***	-0.649***	-0.012	0.006	-0.0003	0.009
	(5.09)	(4.41)	(0.21)	(0.13)	(0.00)	(0.11)
Proportion of Childbearing years	-1.122***	-1.025***	0.329	0.415	0.447	0.547
	(15.91)	(14.45)	(0.83)	(1.14)	(0.92)	(1.10)
Minority*proportion	1.772***	1.636***				
(IV)	(4.88)	(4.80)				
Observations	1,674	1,674	1,674	1,674	1,674	1,674
R-squared	0.20	0.30				

Notes: Numbers in parentheses are *t*-statistics robust to heteroscedesticity and clustering at the community level. For IV-probit model in Columns 5 and 6, *t*-statistics calculated using bootstrapped standard errors. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***. Column 1 reports the first stage regression for Columns 3 and 5, and Column 2 reports the first stage regression for Columns 4 and 6. The instrumental variables for Columns 3 to 6 is community mean of minority dummy interacted with the community mean of the proportion of a woman's childbearing years overlapped with the one-child policy period. All regressions include a dummy variable to control for missing values of the sex of the first child (one if missing).

variables is widening over time. If it is, then the basic assumption for the DD estimator is violated. The regression results reported in the first two columns of Table 6 show that the DD estimate (the coefficient on the interaction term) does not have a significant effect on either education or income. Thus, our DD estimator passes this simple test.

Another concern is that some Han women may have changed their ethnicity from Han to minority in response to the one-child policy. In this case, minority status becomes an endogenous variable. Although there is some anecdotal evidence of people changing their ethnicity (Scharping 2003), the proportion of Chinese doing so would be small. According to the Chinese statistics, the proportion of minorities in the entire country went up by only one percentage point, from 6 percent in 1979 to 7 percent in 1989. Even if 1 percent of the Chinese had changed their ethnicity, the proportion of minorities would have risen by about 2 percent, which is much larger than the 1 percent rise recorded in reality. Thus, the proportion of Chinese who have successfully changed their ethnicity must be very small, and thus should have a negligible effect on our estimate.

Nonetheless, we test how serious the changing of ethnicity is by excluding provinces that are more likely to have such a issue. Changing ethnicity would be easy only if the local minorities and the Han had no differentiable attributes, such as appearance, religion, and language. Anecdotal evidence suggests that the easiest ethnicity to change to would be Manchu, who live in Northeastern China, since they have been completely assimilated by the Han (Scharping 2003).[23] To examine the impact of changing ethnicity, we exclude Liaoning province from the sample, as it has a large Manchu group. The 2SLS estimates reported in Columns 3 to 6 in Table 6 show a slightly stronger external effect compared to the 2SLS estimates using the whole sample, which suggests that changing ethnicity should not be a big concern for us.

VI. Conclusions

In this paper, we test the external or neighborhood effect of fertility, by using unique methods to deal with the omitted variable bias. The unique affirmative birth control policy enables us to exploit quasi-experimental variation in the fertility of the majority-ethnic group to identify the external effect of neighborhood fertility.

Employing microfertility data from China, we find that fertility has a large external or neighborhood effect. An increase in the proportion of second children in neighboring households by one percentage point increases a household's probability of having a second child by 0.4-0.9 percentage points. These findings are robust for all of our methods of controlling for potential biases. The existence of the external effect suggests that evaluations of birth control policies may be more complicated than one would have thought.

23. Another large minority group, the Mongols, mainly live in Inner-Mongolia, which is not included in our sample.

Table 6
Sensitivity Tests for Estimates That Use DD Estimator as an Instrumental Variable

Dependent variable	DD estimate for other outcome variables		2SLS estimates excluding Liaoning Province			
Model Dependent variable	OLS Education (1)	OLS Income (2)	First stage Second child (mean) (3)	First stage Second child (mean) (4)	Second stage Second-child dummy (5)	Second stage Second-child dummy (6)
Second child (community mean)					0.787*** (2.92)	0.878*** (4.19)
First child is a girl	0.054 (0.31)	-0.032 (0.67)	0.006 (0.66)	0.008 (0.89)	0.118*** (5.25)	0.118*** (5.21)
Minority	0.227 (0.50)	0.036 (0.29)	-0.003 (0.11)	-0.003 (0.11)	-0.074 (0.80)	-0.076 (0.81)
Proportion of childbearing years	-5.444*** (3.81)	0.715* (1.83)	0.350*** (4.31)	0.303*** (4.01)	-0.841*** (3.02)	-0.881*** (3.30)
Minority*proportion	0.173 (0.19)	0.166 (0.67)	-0.004 (0.08)	-0.005 (0.10)	0.247 (1.25)	0.249 (1.25)
Age	-0.391*** (4.95)	0.041* (1.92)	0.017*** (3.80)	0.016*** (3.85)	0.023 (1.58)	0.022 (1.50)
Age squared	0.001** (2.35)	-0.000** (2.11)	-0.000*** (3.38)	-0.000*** (3.46)	-0.000*** (2.98)	-0.000*** (2.92)
Education			-0.002 (1.18)	0.000 (0.26)	0.001 (0.42)	0.002 (0.62)

Income			-0.026***	-0.004	-0.032*	-0.028*
(thousand yuan)			(5.59)	(0.92)	(1.90)	(1.74)
Urban dummy	0.752***	-0.017	-0.113***	-0.085***	0.001	0.016
	(2.81)	(0.23)	(7.60)	(6.09)	(0.02)	(0.70)
Community mean						
Education		0.025		-0.006		-0.007
		(1.31)		(1.62)		(1.35)
Income		0.806***		-0.172***		-0.013
(Thousand yuan)		(15.13)		(14.56)		(0.34)
Minority	-2.215	-0.178	-0.579***	-0.703***	0.033	0.031
	(0.85)	(0.25)	(3.16)	(4.09)	(0.59)	(0.50)
Proportion of	-2.108*	-0.545	-0.814***	-0.910***	0.457**	0.561***
childbearing years	(1.76)	(1.57)	(11.58)	(13.08)	(2.37)	(3.02)
Minority*proportion	6.946	0.461	1.626***	2.153***		
(IV)	(1.15)	(0.28)	(3.81)	(5.37)		
Observations	1,802	1,802	1,556	1,556	1,556	1,556
R-squared	0.32	0.15	0.15	0.26		

Notes: Numbers in parentheses are *t*-statistics robust to heteroscedesticity and clustering at the family-level. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***. Column 3 reports the first stage regression for Column 5, and Column 4 reports the first stage regression for Column 6. The instrumental variables for Columns 5 and 6 is community mean of minority dummy interacted with the community mean of the proportion of a woman's childbearing years overlapped with the one-child policy period. All regressions include a dummy variable to control for missing values of the sex of the first child (one if missing).

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