# 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 micro-fertility data from China. The test is executed by regressing one household's fertility on the average fertility of neighboring households. This exercise is complicated by endogeneity problems from three sources: simultaneity, omitted community variables and sorting by fertility preference. China's household registration and birth control policies provide a natural experiment for solving all three sources of endogeneity. First, these policies prevent households from moving for fertility reasons, thus endogenous sorting is not a problem. Second, the unique affirmative birth control policy allows us to conduct two natural experiments: (1) testing the external effect from the dominant *Han* Chinese to minority households; and (2) identifying the external effect using Instrumental Variables that are based on the differences-in-differences. We find fertility has a large external effect in general, and in particular among households that appear to have more social interactions.



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# 1 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 friends consume them (Gaviria and Raphael, 2001). College students tend to demand high grades when their roommates have high GPAs (Sacerdote, 2001). Rural households are more likely to buy a TV if the neighbors have bought one. There are also many examples of the external effect in investment or other behavior. For example, Foster and Rosenzweig (1995) find that rural households are more likely to use fertilizers if neighbors have used them. Hong et al. (2000) find that security analysts tend to imitate each other's forecasts of corporate returns. In all examples, one's own behavior not only fulfills self-satisfaction, but also has some *external effect* on others.[1]

There have been a number of studies offering theoretical explanations for imitative behavior (Akerlof, 1997; Becker, 1991; Bernheim, 1994; Ellison and Fuderberg, 1995; Glaeser et al., 1997). Bikhchandani et al. (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 raised 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

[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 influences", "neighborhood effects", "conformity", "imitation", "contagion", "epidemics", "bandwagons", "herd behavior", "social interactions", or "interdependent preferences" (Manski, 1993). In this paper, we will 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.

fertility is caused by some or all 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. The imitation behavior will sustain the equilibrium — or the high fertility caused by historical 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, we need to regress the fertility of one family on the average fertility of other families in a community. If the variable average fertility has a positive coefficient, then we can claim that there is an external effect. This simple regression method, however, is biased due to the endogeneity of the average fertility of other families. The endogeneity comes from three potential sources. 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, 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. As is well known, these unobserved variables could bias the estimates. Third, if households are located in places where raising a large family is favorable, then similar people are likely to live in the same community because they endogenously sort themselves by fertility preferences.

This paper tests the external effect of fertility by using unique methods to break the endogeneity. Specifically, we employ micro-fertility data from China, where household reg-

[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 could determine a household's social status. As a result, households following the norm will be rewarded with a high social status (the second mechanism), while the deviants may be looked down upon (the first mechanism). A household may also imitate other households' fertility behavior through communication, or *social learning* as defined by Kohler et al. (2001). A household may have limited information about the optimal number of children they should have, because both the costs of raising children and benefits of old-age security from children will 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.

istration, land redistribution and birth control policies provide a natural experiment for testing the external effect of fertility. The household registration system requires that people be registered in the location of birth. Moving from one location to another has been until recent very difficult, if not impossible. This specific policy environment makes our analysis immune from endogenous sorting, and we are thus required to deal only with the first two sources of endogeneity in the study.

One way to deal with the first two sources of simultaneity is to employ the two stage least squares (2SLS) estimator, and use the household attributes of average neighbors as instruments.[4] Assuming that the average attributes of neighboring households, such as the age and education of wives, do not affect the fertility of the studied household except through their average fertility, the first two sources of simultaneity bias can be corrected by this 2SLS method.

The assumption that the average attributes of neighboring households do not affect the fertility of the studied household except through their average fertility, however, may not be valid. The average characteristics of neighboring households could affect the studied household's fertility, if other relevant community variables, such as *community level* birth control or other policies, are omitted. For example, if more educated people opt for a stricter birth control policy, then the average education of neighboring households not only affects their own fertility, but also affects the studied household's fertility through stricter community birth control policies (omitted variables).

One easy way to correct the omitted variable bias, as suggested by Levitt (1997) in a different context, is to add some measures related to community birth control polices in the second stage regression. Adding these policy variables, however, will not solve our problem. Community-level birth control policies may be endogenous themselves. Tougher community polices may be a result of high local fertility, which is on the left-hand side of our regression.

The main innovation in this paper is the way in which we solve the omitted variable bias. We do so by making use of a unique aspect of the *national* birth control policy, which

[4]Gaviria and Raphael (2001) use family background information to identify peer effects of teenage behavior such as drug use and alcohol consumption.

provides a natural experiment for testing the external effect of fertility. China started its one-child policy in 1979. Under this policy, each family is allowed only one child, and the second or higher-parity births are fined. The one-child-per-family policy, however, is only applied to the *Han* Chinese, and by way of affirmative policies, all ethnic minorities in China were allowed to have two or more children until the end of 1980s (Peng, 1996). In some provinces, like Tibet, there is no restriction on the number of children per family (Deng, 1995).

This unique affirmative policy allows us to test one side of the external effect, that of the *Han* on minorities, without being concerned about the omitted community policy variables. Since some community birth control policies, such as fining families for second births, are only applied to *Han* families, we can employ the 2SLS estimator without worrying about the bias caused by omitted community policy variables.

The affirmative birth control policy also provides us with another unique way to identify the external effect. Specifically, we use the differences-in-differences estimator (Angrist and Krueger, 1999), which exploits the fertility difference between *Han* Chinese and ethnic minorities both before and after the policy change. Thus, we use the interaction of the policy timing and minority indicator as an instrument. The instrument is excellent because it identifies the external effect only by using exogenous variability in fertility of the neighbors that results from the enacting of the policy. This identification strategy is very clean, since we do not have to rely on IVs based on the average household attributes, which may be correlated with the studied household's fertility through forces other than the external effect.

This paper, to the best of our knowledge, is the first to empirically test the external or neighborhood effect of fertility. Most existing studies have been based on either theoretical or historical evidence (Dasgupta, 1995), and the endogeneity issues described above have never been seriously dealt with.[5] Our finding that the probability of a household having

[5]There are two earlier empirical studies on related issues, but neither uses systematic econometric methods to estimate the external effect. Easerlin et al. (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 declines over time. Kohler et al. (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.

a second child decreases when the proportion of neighboring households having the second child decreases confirms Dasgupta's theoretical hypothesis.

The rest of the paper is structured as follows. In Section 2, we briefly introduce the policy environments in China, including household residence registration policies and birth control policies. We will discuss how birth control policies differ between the *Han* Chinese and minorities. In Section 3, we specify our empirical strategy. In Section 4, we introduce the survey data. In Section 5, we test the external effect of fertility. Section 6 further tests whether the external effect takes place through social interaction. Section 7 concludes the study.

# 2 The Household Policy Environment in China

In this section, we briefly describe some 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.

## 2.1 The one-child policy

China started its unique one-child-per-family policy in 1979. Under this policy, one household is allowed only one child. Households are given birth quotas, and births without a quota, or "above-quota births," are penalized. To facilitate our analysis later, we classify birth control policies into two categories: national level and community level policies. National policies, such as the one-child-per-family policy is exogenous to our analysis of micro data, but community policies such as penalties for above-quota births and preventative mechanisms may very likely be endogenous in our analysis.

One unique aspect of the national policy is that it is an affirmative policy.[6] The government has enacted tighter control over the birthrate 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

[6]There are many other affirmative policies. For example, minorities can go to colleges with lower grades than the *Han*, are subject to lower tuition fees, and enjoy a range of special subsidies from the government.

some provinces, like 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 the policies should be less restrictive (CCCPC, 1994; Hardee-Cleaveland and Banister, 1988). However, until the end of 1988, except in three of China's largest cities — Beijing, Shanghai and Tianjin,[7] — minorities were allowed to have a second child (Deng, 1995). For those ethnic groups that have a population of less than 10 million, second and third children are allowed. Ethnic groups with a population larger than 10 million are subject to the same policy as *Han*. The *Zhuang* were the only ethnic group with a population larger than 10 million at the end of the 1980s, most of whom live in Guangxi. On 17 September 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 came under the authority of the one-child policy. To summarize, for most of the 1980s, minorities were allowed to have more than one child, which provides a unique natural experiment to test the external effect of fertility.[8]

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 issued a policy document in 1982 that lists all conditions under which a *Han* household may have a second child (CCCPC, 1982; Qian, 1997). One condition allows *Han* households in remote minority areas to have a second child.[9] 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.

[7]Our sample does not include these three cities.

[8]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 time that these women could have a baby was July 1989, which should not affect our sample, which was collected in June 1989.

[9]Other conditions include, for example, when a first child who is disabled, adopted, etc.

To implement the birth control policies, local (including community) governments are given incentive contracts. This takes 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 all future income and other benefits associated with government positions.

The community policies, though on average tough, demonstrate great heterogeneity. At the community level, fines have been the primary penalty used by local government officials for above-quota births (Short and Zhai, 1998). Various studies have shown that the fines are heavy and vary enormously across communities. The fines range between 20-200 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 very substantial, especially in light of the fact that many households in rural areas are still below the poverty line.[10] Empirical studies also have shown that fertility decreases with the size of fine (McElroy and Yang, 2000; Li and Zhang, 2002).

Other than fines, community government officials also invest in contraceptive facilities. In many communities in our sample, there are clinics that oversee birth control. IUD insertions are the main mechanism of birth control in China, and the easy availability of these clinics is very important for birth control.

## 2.2 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.[11] The household registration system requires that a person be registered where he or she is born. Each household has a registration certificate (*hukouben*), which records all members of the household. All administrative activities such as land distribution, issuing ID cards, and registering a child in school are based on this registration certificate. It was also used for distributing food, oil and clothing coupons until the early 1990s, and has made

[10] Using the standard of per capita income of one US dollar by the World Bank (Stern, 2001), the poverty line of per capita income should be 347 yuan when using the most conservative exchange rate of 3.8 yuan per US dollar. Even with such a low poverty line, 24 percent of our sampled households in rural areas are below it.

[11] Although the Chinese government has been gradually reforming the system from the mid-1990s, the registration system is still very strict in most places.

moving across localities very restrictive in both urban and rural areas (Cheng and Selden, 1994).

People who have moved from the location of their permanent residence, such as migrant workers, have to follow the birth control policies of their own villages. Although there are large numbers of migrant workers who have moved from rural to urban areas, they are still registered as farmers in their home villages. When migrant women from rural areas fall pregnant, they need a permit from their home village 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 where she gives the birth bear responsibility (Goldstein et al., 1997; Hardee-Cleaveland and Banister, 1988; The State Council of China, 1991). Their children, even if born outside of the village, 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 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, 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 either, because local villagers neither want to cater for above-quota children nor want to assign the household a piece of land, which has to be taken from existing residents (Li and Rozelle, 1998).

In summary, a strict residential registration system prevents people from moving for the purpose of bearing children and, therefore, sorting across locations may not be a major problem for our analysis. Although anecdotal evidence shows that some farmers hide temporarily in other places to bear children, they are usually required to go back to their home villages and accept the penalties (Johnson, 1994).

# 3 The Empirical Framework

Following Akerlof (1997), Bernheim (1994) and Dasgupta (1993), we model the external effect through a reduced form utility function: the fertility of other households enters a household's utility function directly.[12] In other words, households may behave as if they care about fertility of other households. Let $i$ be any one of the $n$ households in our sample, and $-i$ be all households other than $i$ in a community. Define $y_i$ the number of children in household $i$, and $y_{-i}$ the average number of children in all other households in the same community. The utility function takes the following form $U_i(y_i, y_{-i}, x_i, z)$, where $x_i$ represents individual household characteristics and $z$ represents community variables. Household $i$ chooses $y_i$ to maximize $U_i$, given all other variables. We assume the utility function is well behaved such that there is a unique solution $y_i^*(y_{-i}, x_i, z)$ to $\partial U/\partial y_i = 0$, and $\partial^2 U/\partial y_i^2 < 0$. In other words, given any $y_{-i}$, there is a unique optimal $y_i$ for household $i$.

We further assume that $\partial^2 U/\partial y_i \partial y_{-i} > 0$, or there is strategic complementarity (Cooper and John, 1988). In other words, the marginal utility of having an additional child for household $i$ increases with the average number of children of other households in the community. Given this property, simple comparative statistics will show that $\partial y_i/\partial y_{-i} > 0$. To see this, totally differentiating the first-order condition, we get

$$\partial y_i/\partial y_{-i} = \frac{\partial^2 U/\partial y_i \partial y_{-i}}{-\partial^2 U/\partial y_i^2} > 0. \tag{1}$$

Our empirical work will focus on estimating the response function of household $i$, $y_i^*(y_{-i}, x_i, z)$, and testing (1).[13] The estimation, however, is complicated by the endogeneity of $y_{-i}$. As stated before, other households' fertility choices $y_{-i}$ may be endogenous 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.

[12] Note that the four mechanisms are all consistent with such a reduced form utility function.

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

Second, if relevant community variables in $z$, which affect 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 simply caused by the unobserved community variables, rather than the external effect. Some researchers call the external effect caused by omitted community variables the "contextual" effect, which is to contrast the "interactive external" effect that we want to identify (Gaviria and Raphael, 2001; Manski, 1995). For example, teenagers could use drugs because they observe that their peers are using drugs (interactive external effects); but they might also use drugs because of a large drug supply and thus low drug prices in the neighborhood (contextual effects). If the latter is true, then using parental attributes as instruments to identify peer effects to a teenager is problematic because these attributes, such as education and incomes, may be correlated with the neighborhood drug supply, which in turn affects teenager's drug-using behavior. The contextual effect could also exist in our study of fertility externality, though the mechanism is different. In our study, the contextual effect means that the neighbors' attributes may affect a household's fertility choice by affecting some unobserved birth control policies of the community. In short, our task is to identify the interactive effect, but to do that we have to control for the contextual effect.

Third, endogeneity could also arise if each household $i$ chooses to live in a community where other households have high fertility. In this case, households sort themselves according to their preferences for children.

Although all three sources of endogeneity could exist theoretically, only the first two matter in the case of our sample from China. As described above, a strict residential registration system in China prevents people from moving for the purpose of bearing more children, and thus, sorting across locations may not be a major problem.[14] The first source of endogeneity, or the one caused by simultaneity, may also be trivial as long as we have

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

a large number of households in $-i$. This is because the external effect of one individual household on all other households, even if existing, must be very small. We will concentrate on dealing with the second source of endogeneity, or the one caused by omitted community variables, though our methods in most cases can also solve endogeneity related to the first source.

If we solve each household's optimization problem, we can obtain a Nash equilibrium of this non-cooperative game, $(y_1(x_1, x_{-1}, z), y_2(x_2, x_{-2}, z), ..., y_n(x_n, x_{-n}, z))$. From this reduced form solution, we see that $y_i$ is a function of not only household $i$'s own characteristics $x_i$, but also other households' characteristics $x_{-i}$ and community variables $z$. The mechanisms of how these variables affect $y_i$, however, are different. As can be seen from $y_i^*(y_{-i}, x_i, z)$, $x_i$ and $z$ affect $y_i$ directly, but $x_{-i}$ affects $y_i$ only through $y_{-i}$. These different mechanisms essentially provide us with the instrumental variables (IVs) to correct for the first two sources of endogeneity. As long as we can find those $x_{-i}$ variables, we can use them as instruments to identify the effect of $y_{-i}$ on $y_i$.

The estimation strategy can be explained by the following. Assume that $y_i^*(y_{-i}, x_i, z)$ takes a linear form, and omit the star to simplify notation, then the equation to be estimated is

$$y_i = \beta_0 + \beta_1 y_{-i} + x_i\beta_3 + z\beta_4 + \epsilon, \tag{2}$$

where $x_i$ and $z$ are vectors of household and community variables. In the following empirical work, $-i$ represents the average of all other households in the community to which individual $i$ belongs. In other words, we want to measure the external effect of all other households in the community on household $i$. Thus, it might be more sensible to refer to this external effect as "the neighborhood or community effect." Under the assumption that $x_{-i}$ does not affect $y_i$ except through $y_{-i}$, Equation (2) can be identified by using $x_{-i}$ as instruments. Specifically, we employ the two stage least squares (2SLS) method to estimate (2).

The key to using the 2SLS is to find valid IVs. 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 will be devoted to finding valid IVs for our problem.

# 4 Data

In this paper, we use the survey 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 by an international team of researchers whose backgrounds include nutrition, public health, sociology, Chinese studies, demography and economics.

The survey contains information on the number of children and birth control policy in each community, including the size of the fine and the existence of a birth control clinic or hospital. It also has detailed information on household characteristics. The sampled households were randomly drawn in eight provinces including rich ones such as Jiangsu and poor ones such as Guizhou.[15] In total, we have 3,795 families in the sample. From this number, two-thirds are from rural areas, and one-third from urban areas; 81 percent are *Han* Chinese and 19 percent are minorities. The sampled households have a median per capita income of 666 yuan in 1989, about 10 percent higher than the national average of 602 yuan.

In this paper, a community refers to a village in rural areas or a neighborhood in urban areas.[16] Villages and neighborhoods are the lowest levels of China's administrative hierarchy in rural and urban areas respectively, the border of which are determined by higher level officials. Of the 191 communities in the sample, the average size is 622 households, with a standard deviation of 905. While the smallest community has 34 households, the community at the fifth percentile has 105 households. The relatively large size of communities in the sample suggests we should be less concerned about the simultaneity bias, which is most likely to occur when estimating the external effect in small communities. Between 20-35 households were selected from each of the 191 communities in the sample.

[15] The other six provinces are Liaoning, Shandong, Henan, Hubei, Hunan and Guangxi.

[16] The Chinese term for village is *cun* and the term for neighborhood is *jiedao*.

The survey was conducted in both minority and non-minority communities. The definition of minority communities is based on the administrative classification of the Chinese government.[17] The questionnaire contains an explicit question asking whether the community is classified as a minority one. In total, 19 percent of the communities are minority communities.[18]

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 implemented ten years previously. This is not surprising since the sample includes children born before the one-child policy took effect in 1979. On average, a family has 2.38 children per household, 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).[19] 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 (row 6). This is also comparable to the national level of 63 percent.[20]

In general, rural families have more children than urban families, and minorities have more children than the *Han* Chinese. In rows 2 and 3 of Table 1, we divide the sample into two sub-samples: urban and rural families. Grouping this way, we find that on average rural families have 0.15 more children than urban families. The fertility difference between rural and urban families in our sample is smaller than the national average difference of 0.64, and this is mainly caused by the higher fertility of our urban sample (2.28 for our urban sample, whereas for the national urban population it stands at 1.87). There could be two reasons for this. First, the survey survey may have sampled urban residence in small towns, where fertility tends to be higher than larger cities like Beijing and Shanghai. Second, the eight

[17]The Chinese government assigns minority status to each administrative level, including minority provinces, cities, counties, townships and villages. These classifications are based on the percentage of minority populations, and administrative and political criteria.

[18]We are unable to determine whether a community is minority or not due to missing data for 45 percent of the communities. As a result, we restrict our analysis to a smaller sample when we need to use this information.

[19]The national statistics on children cover all women in the age group of 20-64. Most women (99 percent) in our sample are in this age range.

[20]Note that both the average number of children per household and the proportion of women having a second child are higher in our sample. This may be because our sample only includes married women, whereas the national statistics include both married and unmarried women.

provinces (and the counties) covered have a smaller urban-rural difference than the rest of the country.[21]

Similarly, comparing row 4 to 5, we find that minorities on average have 0.08 more children than the *Han* Chinese. The fertility gap between minority and *Han* families is lower than the national average fertility gap of 0.24 (2.31 for the *Han* and 2.55 for minorities of the whole nation). The smaller gap of our sample is justifiable, since the sample does not include several minority provinces that have the highest minority fertility in China, such as Xinjiang, Ningxia and Tibet. Using provincial level statistics, we find that the average fertility gap between *Han* and minority women of the sampled provinces is only 0.11.[22] This is again comparable to the average fertility gap of our sample.

The 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? Using the newly available census conducted in the year 2000, we find that the fertility gap between minority and *Han* households is even smaller, which is only 0.02 for the two provinces from which we have data available.[23] Furthermore, in all sampled provinces, in both the 1990 and 2000 censuses, the average provincial fertility of minority women is almost always the same as that of *Han* women. The small within-province fertility gap could partially be caused by common economic and policy environment faced by all households in the same community (the contextual effect). More importantly for our study, the small difference could also be caused by the interactive external effect. In the next two sections, we attempt to isolate the interactive external effect from the contextual effect.

[21]We are not able to check the fertility level for counties since the survey sites, including the name of counties, are confidential.

[22]For the provincial level statistics, we only have information for the group of women aged between 15-64. The average fertility of this group is 2.31 for the *Han* and 2.42 for minorities.

[23]We have data from two sampled provinces: Liaoning and Hubei. Data from other provinces will be available in the next few months. In the 2000 census, the Chinese Statistical Bureau changed the age group for calculating fertility. They have information for the group of women aged between 15-50. All these women have been subject to birth control policies for most of their childbearing period. The average fertility of women in these two provinces is 1.19 for the *Han* and 1.21 for minorities. Although these numbers are not completely comparable to the old statistics, which cover a different age range, we can still see that the one-child policy has been very effective in reducing fertility to the ideal of one child.

# 5 Empirical Tests

In this section, we systematically test whether fertility has a positive external effect, and measure the magnitude of the external effect if it exists. To meet this goal, we will conduct the following four tests. We first test whether the fertility of a household increases along with the average number of its neighbor's children by employing both OLS and 2SLS estimations. We then examine the validity of the IVs used in the 2SLS estimations. To solve the omitted variable bias, we explore the natural experiment provided by China's affirmative birth control policies in two unique ways: (1) examining the external effect from *Han* households to minority households in the same community; and (2) identifying the external effect using IVs that are based on the differences-in-differences.

## 5.1 An initial test using $x_{-i}$ as IVs

In this section, we estimate Equation (2) by OLS and 2SLS. We use the number of surviving children per household as the dependent variable. The independent variables include the average number of children of neighboring households, the sex of the first child, the woman's age and education, household per capita income, and an urban indicator. The key hypothesis is that the coefficient of the average fertility is positive. Regression results are reported in Table 2.

The first column of Table 2 reports the results of the OLS regression. The OLS regression shows that a household's fertility increases with the average fertility of neighboring households. The coefficient on the variable average number of children in neighboring households is positive and significant with a t-statistic of 23.04. The magnitude of the effect is also large. An increase in the average fertility of neighboring households by one increases a household's fertility by about 0.7.

Other variables in the OLS regression are also significant and give rise to the expected signs. The number of children in a household increases by 0.262 if the first child is a girl. This means that if we compare 100 households whose first child is a girl with another 100 households whose first child is a boy, the first group of households will have about 26 more children than the second group. Fertility increases with the woman's age, because older women have been exposed to longer childbearing time. Fertility decreases with woman's education, per capita income, and is lower in urban areas. All these findings are consistent with previous empirical findings.[24]

As argued above, the OLS estimates may be biased, because the variable average fertility is most likely to be endogenous. To deal with the endogeneity problem, we employ the 2SLS estimation, using the average attributes of neighboring households as instruments, including the women's age and education, per capita income and the sex of the first child. Assuming that the average attributes of neighboring households, such as the age and education of the wife, do not affect the fertility of the studied household except through their average fertility, the endogeneity problem can be corrected by the 2SLS method.[25]

The 2SLS estimates are reported as columns 2-5 in Table 2. The reported t-statistics are calculated by using standard errors corrected by plugging in the original value of the endogenous variable. Generally speaking, except the coefficients on the average fertility, the 2SLS estimates are very similar to the OLS estimates in column 1. The coefficients on the average fertility of the 2SLS estimates are about 30 percent smaller than that of the OLS estimate (0.436-0.530 vs. 0.691), and an F-test shows that this difference is significant at the one percent level. This finding confirms the conjecture in the literature studying external effects that OLS estimates might have overstated an external effect due to the simultaneity and omitted variable biases. The larger coefficient of the OLS estimate could be a result of reverse causality or some omitted variable that causes a co-movement of the dependent variable and the independent variable.

## 5.2 The validity of IVs

The effectiveness of the 2SLS estimator hinges crucially on the validity of IVs. 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, namely through the average fertility of neighboring households. The instrumental variables used above, such as

[24] See Johnson (1994), Zhang (1994), McElroy and Yang (2000) and Li and Zhang (2002).
[25] We will examine the validity of this assumption in the next sub-section.

the averages of the women's age and education, the household's per capita income and the sex of the first child, are for some obvious reasons correlated with the average fertility of neighboring households.[26]

These IVs, however, might not satisfy the second condition. In particular, they may affect the fertility of household $i$, or $y_i$ through channels other than $y_{-i}$, for example, through omitted community birth control policy variables. Essentially, IV estimates, when IVs themselves are correlated with the omitted variables, cannot solve the endogeneity problem. This is because these omitted community policy variables are left in the error term, which is correlated with the fitted value $\hat{y}_{-i}$.

The average household characteristics, or the IVs, may be correlated with community birth control policies through two channels. First, households demonstrating certain attributes, such as high education, may favor strict birth control policies, or at least demonstrate no hostility towards these policies. Second, the pressures caused by a large number of above-quote births could affect community birth control policies. On one hand, local officials, who do not want to be demoted for allowing numerous above-quota births, may enforce stricter policies. On the other hand, they may have more lenient policies if local households with high preferences for children demand more lenient policies. The correlation between the pressure of having above-quota births and education, age structure, and income of the average household is clear. Such pressure is also correlated with the proportion of girls to the first births in the community, whereby households having a girl as a first child are more likely to want a second child. In summary, the average attributes of other households of the community not only affect their own fertility, but also affect household $i$'s fertility through their effect on community birth control policies. As a result, the IVs used may well be correlated with omitted community policy variables.

Before proceeding with our empirical strategies, let us look more carefully at community birth control policies. There are two kinds of community birth control policies: *hard* and *soft* policies. Hard policies refer to those policies that are enforceable on households, such

[26]They are jointly significant in the first stage regressions, which have high R-squares. This is also reflected in the high t-ratios of these household attributes in Table 2.

as fines on above-quota births. For a policy to be termed hard, its implementation, such as the level of fine, has to be verifiable.

Other policies, such as using contraceptives, are not so easily verifiable. We call these unverifiable soft policies. Communities in which birth control pressure is great not only increase the punishment for above-quota births, such as fines, but also try to provide more preventative mechanisms, such as contraceptives. Although households may not be required to use them, the mere presence of birth-control facilities may increase a household's likelihood of using them, which in turn reduces its fertility. The key factor differentiating soft policies from hard ones is that a community can only make contraceptives, such as condoms, easily accessible to households, but cannot enforce the use of them.[27]

An easy way to test and control for this omitted policy variable bias is to add some measures of the community birth control policies. We use the size of fine for above-quota births to measure hard policies, and the availability of contraceptive facilities as a measure of soft policies. We put these variables on the right hand of Equation (2). As argued by Levitt (1997), if these variables can control for such policies, then our instruments will not be correlated with the error term, which does not contain these policies. Even if these variables cannot measure every policy dimension, they can at least provide a test of whether policies matter. Statistically, if omitted community policy variables are positively correlated with $\hat{y}_{-i}$, including them in the regression as regressors will reduce the magnitude of the coefficient on $\hat{y}_{-i}$ (Greene, 1993). If, however, we find that including these policy measures only account for limited changes to the coefficient on $\hat{y}_{-i}$, then omitted variable bias may not be the driving force for the estimated external effect.

Table 3 reports OLS and 2SLS regressions with the two measures of birth control policies. In column 1, we report an OLS regression omitting the variable of average fertility. We see that the fine is negative and significant, which means that a larger fine is indeed associated with lower fertility. The variable contraceptive facilities, however, have a positive sign. The positive sign indicates that the policy measures themselves may be endogenous.

[27]Some communities do require every woman who gives birth to agree to the use of some form of birth control mechanism, such as IUD insertion. However, it a simple matter to visit a private clinic to reverse the procedure (Li, 1995).

It could be that when fertility ($y_i$) is high, there are larger fines and more contraceptives available.

In columns 2-6, we add the average fertility back into the regression. With this change, both the fine and contraceptive facilities completely lose their explanatory power. On one hand, this means that these policy measures are indeed correlated with the average fertility because the latter has picked up all their effect. On the other hand, if we compare columns 2-6 of Table 3 with columns 1-5 of Table 2, we find that adding these policy measures has a negligible effect on the coefficient of average fertility. This could mean two things: (a) omitted variable bias is trivial, even if it does exist; or (b) these policy variables are endogenous and the estimation is still biased. If it is the latter, then we have to resort to better ways to resolve this problem. In the next two subsections, we will introduce two innovative approaches consisting of a number of natural experiments that may solve the omitted policy variable bias.

Before proceeding, however, we also report regression results for an alternative test of the external effect, which serve as a benchmark for the next two subsections. We test whether the probability of a household having a second child increases with the proportion of neighboring households having a second child. The estimates of the external effects, the coefficients on the variable average fertility, are reported in Table 4. For regressions with and without the policy measures (row 1 vs. 2), the coefficients on the external effect are highly significant and are similar in magnitude, around 0.5 for the 2SLS estimates.

## 5.3 A natural experiment

Adding variables measuring community birth control policies are not without cost. As argued above, these policies themselves could be endogenous.[28] Since large fines or an increase in the number of contraceptive facilities could be the result of high fertility, we again have a simultaneity bias. Essentially, the approach proposed by Levitt (1997), of finding proxies for the omitted variables and putting them in the second stage regression, cannot solve our problem, because these omitted variables are themselves endogenous.

[28] Also see Li and Zhang (2002) for a similar argument.

China's affirmative birth control policies have provided a natural experiment to better solve the omitted variable bias. Since birth control policies, such as the fine for the second child, only applied to *Han* households, but were exempted for minority households in the sample period, this essentially provides a natural experiment to solve the omitted variable bias. Let's divide each sampled community into two groups: the *Han* and minority. 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 minority households. Specifically, we want to estimate a slightly different equation, or

$$y_{j,i}^m = \beta_0 + \beta_1 y_j^h + x_i^m \beta_3 + z_j \beta_4 + \epsilon, \tag{3}$$

where $y_{j,i}^m$ is the fertility of a minority household $i$ in community j, $y_j^h$ is the average fertility of the *Han* households in community $j$. We use the size of the fine or the average characteristics of the *Han* households in community $j$, $x_j^h$, to identify the external effect from *Han* to minority families. Assuming that these IVs do not affect $y_{j,i}^m$ except through $y_j^h$, our IV estimates will be free of the endogeneity problem.

In the first column of Table 5, we report regression results of the second stage regression with the fine as an IV. The reported t-statistics are calculated according to corrected standard errors. Although the coefficient on the variable the proportion of *Han* households with the second births is positive, it is not significant at the five percent level.

One reason for the insignificant external effect when using the fine as an IV is that it has relatively weak predictive power in the first stage regression, with the R-squares only about 30 percent of that when other IVs such as the average age of women are used (0.03 vs. 0.10). As argued in Nelson and Startz (1990) and Bound et al. (1995), when instruments are weakly correlated with the right-hand side endogenous variables in the first stage estimation, the properties of estimators derived using these "poor" instruments in small samples are of major concern. Thus, the fine may not be a good IV, even if it is the most intuitive one.

As this natural experiment shows, the average household attributes used in Sections 5.1 and 5.2 are actually better IVs than the size of the fine. First, each of the average *Han* household attributes has a very high correlation with the proportion of *Han* households with second births. Thus, we do not have to worry about the weak correlation problem exhibited by the fine as an IV. Second, and more importantly, these natural conditions also solve the omitted community policy variable bias without introducing a new source of endogeneity. Even if $x_j^h$ may be correlated with unobserved hard policies, since minorities are exempted from these policies, our IV estimation is immune from the omitted hard policy variable bias. In the case of soft policies, we can simply use the availability of contraceptives as a control variable in $z_j$. Here, the availability of contraceptives is not endogenous anymore, because the second birth in minority households will not add to above-quota births, and thus will not exert pressure on community birth control policies.

Columns 2-5 of Table 5 report results of the second stage regressions with different combinations of the household attributes as IVs. The reported t-statistics are calculated according to corrected standard errors. Regression results of the natural experiment further confirm our hypothesis that fertility has a positive external effect. The coefficients on the average fertility in all four regressions are positive and significant. The magnitudes of these coefficients are rather close to those in Table 4. To statistically examine the validity of our IVs in this natural experiment, we conduct a Hausman overidentification restriction test to regression 3-5. The test results show that our IVs can be excluded from the second stage regressions.[29]

## 5.4 IV method based on differences-in-differences

Except community policy variables, non-policy community variables, which are likely to be correlated with our IVs, may also be omitted or unobserved. As the natural experiment in the last subsection demonstrated, we assume that our IVs such as age and the sex of the first child are uncorrelated with these non-policy community variables. Even if all our IVs passed the overidentification restriction test, we still need examine whether the assumption is well grounded.

[29]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 $N \times R^2$, 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 all three sets of IVs are close to zero, which indicate that the null hypothesis that there is no correlation between the exogenous instruments and the error term from the second stage equation can not be rejected.

Non-policy variables include all community variables that could affect the demand for children with the exception of community birth control policies. For example, if a community prefers good education or good child quality, then they may choose to have fewer children.[30] Some of our IVs may not be obvious correlated with these non-policy variables. For example, it is hard to argue that preference for education is caused by the average age of women in the community. In a sense, the validity of age as an IV is a purely empirical question.

The other IV, the sex of the first child, is — under normal conditions — a random event, which should be uncorrelated with any community variables.[31] However, our situation is more complicated. In Chinese communities, the preference for boys together with the one child policy may mean that parent actively choose the sex of their child. It has been documented that rural households abort unborn or even kill baby girls (Chow, 2002). In this case, the proportion of girls among the first births in a community could be correlated with a number of community variables, such as the conditions of old age security, which affect fertility of both minority and *Han* households. For example, in communities where old-age security relies on many surviving boys, then both *Han* and minority households will choose to have a lot of children, and *Han* households will choose to abort girls to avoid the fine. Our finding that 49.4 percent of first births are girls may indicate that the households choosing the sex of the first birth may not be a serious problem (Table 1), but it is an empirical question whether this has biased our estimate.

To gain more confidence of our results, we need a better identification strategy; one that not only tests external effects directly, but can also be used to test whether other identification strategies provide robust estimates. The affirmative birth control policy provides us with a better method to identify the external effect, a method that does not rely on using these household attributes as IVs. Unique in our setup, we can use the interaction of the policy timing and minority indicator as an instrument. Based on the differences-in-differences (DD) method (Angrist and Krueger, 1999), this interaction term allows us to identify the effect of the neighbor's fertility on the studied household's fertility using only the variability

[30]See Becker and Lewis (1976) for a classical argument of child quality-quantity substitution.

[31]See Angrist and Evans (1998) and Rosenzweig and Wolpin (2000).

in the neighbor's fertility that results from the policy. Note that in this method, we can estimate the external effect for all households, rather than only estimating that from *Han* to minority households.

To understand this new identification strategy, we need first explain how the DD method can be applied to estimate the effect of the one-child policy on fertility. Consider the following equation

$$y_i = \beta_0 + \beta_1 M_i + \beta_2 T_i + \beta_3 M_i T_i + \epsilon_i, \tag{4}$$

where $M_i$ is the minority indicator that equals one for a minority household, $T_i$ is the policy timing variable, defined as the proportion of a woman's childbearing time subject to the one-child policy.[32] The two variables, $M_i$ and $T_i$, pick up the main effects, i.e., the effect of being a minority on fertility and the effect of time on fertility. The coefficient on the interaction term, or $\beta_3$, is essentially our DD estimator. Assuming that without the one-child policy, the change of fertility of minority and *Han* households is the same between 1979 and 1989,[33] then the interaction term picks up the effect of the one-child policy on fertility. In other words, the interaction term measures the fertility gap between minority (the control group) and *Han* (the treatment group) households that is caused by the affirmative one-child policy.

We can rewrite Equation (4) in terms of the neighbors:

$$y_{-i} = \beta_0 + \beta_1 M_{-i} + \beta_2 T_{-i} + \beta_3 M_{-i} T_{-i} + \epsilon_{-i}, \tag{5}$$

where $M_{-i}$ is the proportion of minority households of the neighbors and $T_{-i}$ is the average proportion of a woman's childbearing time of the neighbors. Similarly, $\beta_3$ picks up the effect of the one-child policy on neighbors' fertility.

The DD method can be used as a way to identify the external effect. Specifically, we can apply 2SLS to estimate

$$y_i = \beta_0 + \beta_1 y_{-i} + \beta_2 M_i + \beta_3 T_i + \beta_4 M_i T_i + \beta_5 M_{-i} + \beta_6 T_{-i} + \epsilon_i, \tag{6}$$

[32]Since the earliest child bearing age is 14 in our sample, the maximum menopause age is 55 (WHO, 1996) and the survey year 1989 is 10 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 $age_i \leq 24$, and $T_i = 0$ for $age_i > 65$. Note that although minority households were not subject to the one-child policy, $T_i$ is defined using the same formulas.

[33]This is the same as assuming that $\beta_3$ is zero without the one-child policy.

using the interaction variable $M_{-i}T_{-i}$ as the instrument for $y_{-i}$. Since this interaction term picks up the effect of the policy on the neighbors, it is exogenous when we also include $M_iT_i$ in the second stage equation (6). Essentially, this model will identify the effect of the neighbor's fertility on the study household's fertility only using the exogenous variability in the fertility of the neighbors that results from the introduction of the affirmative one-child policy.

The full model we estimate differs from equation (6) in three ways. First, as described in Section 2.1, the one-child policy may have been relaxed toward the *Han* Chinese in minority communities, which are usually located in remote mountainous areas. This means that the policy may be less important in increasing the fertility gap between minority and *Han* households in minority areas than in non-minority areas. To address the issue that birth control policies toward the $Han$ may be different in minority and non-minority communities, we interact the minority community indicator, $V_i$ with the five variables in equation (6), $M_i, T_i, M_iT_i, M_{-i}$, and $T_{-i}$. By doing this, we have two instrumental variables, $M_{-i}T_{-i}V_j$ and $M_{-i}T_{-i}(1-V_j)$, to identify the external effect.

Second, in two of the regression models, we also exclude $M_{-i}V_j$, $T_{-i}V_j$, $M_{-i}(1-V_j)$ and $T_{-i}(1-V_j)$ from the second stage regression. By doing this, we have more IVs to identify the external effect. In the last two regression models, we also add the average household characteristics as additional IVs, which allows us to test their validity. Third, in some regression models, we include the same set of household control variables used in previous subsections to control for observable household characteristics.

Regression results using the policy measures as instruments are consistent with the previous finding that fertility has an external effect (Table 6, columns 1 and 2). The coefficient on the external effect is positive and significant in both equations. The magnitude is 0.748-0.879, very similar to the estimates of previous models. Adding more IVs will change the magnitude of the estimated external effect (columns 3-6), but the change is not significantly different from zero at the five percent level. We again employ the overidentification restriction tests to these additional IVs, and all passed the tests. These tests further prove that the above design based on natural experiment, which tests the external effect from *Han* to minority households, is as valid as the IV method based on the differences-in-differences.

These regressions also show that the birth control policy has indeed had a positive effect on the fertility gap between minority and *Han* households in non-minority communities. In particular, note that the coefficient on the variable minority*proportion (or $M_iT_i$) is positive and significant. The magnitude of 0.277 means that the increase of the proportion of childbearing time by one standard deviation (0.25) will increase the fertility gap between a minority and a $Han$ woman by 0.07, which is very close to the raw difference of 0.06 in the sample. The policy has no effect on the fertility gap in minority communities since the coefficient on the variable minority*proportion for minority communities is not significant.

In summary, when valid IVs are used, our regressions consistently show that fertility has a positive external effect. The magnitude is large. For the number of children it is 0.435-0.525 (Table 3), and for the probability of a second child it is 0.391-0.995 (Tables 5 and 6).

# 6 Social Interactions

In this section, we test another important aspect of Dasgupta's theory: whether the external effect of fertility takes effect through social interactions. To test this, we need to employ the natural experiment strategy, i.e., testing the external effect from the *Han* to minority households.[34] As tested, results generated from this natural experiment are unbiased.

## 6.1 Minority vs. non-minority communities

The external effect may be different in different communities.[35] In non-minority communities, where most residents are *Han* Chinese, minorities may have to interact more with local *Han* families. As a result, the external effect of *Han* on minority could be large. In minority communities, on the other hand, many residents are minorities, and minorities are

[34]We cannot apply the IV method based on differences-in-differences, because that strategy relies on the variation of minority proportion to identify the external effect, while the first stage regression in this section uses only the *Han* sample.

[35]Borjas (1995) shows that ethnic groups that insulate themselves in American cities tend to foster strong external effects to each other within groups.

more likely to socialize with each other, but not with the local *Han* families. Thus, the external effect of the *Han* on minorities should be smaller in minority communities than in non-minority communities; the external effect of the *Han* on minorities should also be smaller than that of minorities on minorities in minority communities.[36]

In order to test whether the external effect in minority communities is different from that in non-minority communities, we divide the sample into minority and non-minority communities. We use the same regressors in Table 5 for the regressions of the divided sample. Regression results of the divided sample confirm our hypothesis that the external effects in non-minority communities are larger than in minority communities (Table 7, row 1 vs. 2). None of the coefficients of the external effect for the minority community is significant. Magnitudes are also very small (0.264-0.684). The coefficients of the external effect, however, are all significant for the non-minority communities, with magnitudes of 0.574-0.731, which are much larger than those for minority communities. These results confirm our conjecture that the external effect of the *Han* on minorities is larger in non-minority communities, where minorities tend to have more social interactions with the *Han*.

We could also test the external effect between minority households. Although the within-minority external effect is not one-sided, our IVs are still valid. This is because minorities were not subject to the one-child policy, and we need not be concerned about the omitted policy variables. Nor are we concerned about omitting non-policy variables since, as tested above, all our IVs, are valid.

Regression results show that the external effect of the *Han* on minorities is smaller than that of minorities on minorities in minority communities (Table 7, row 1 vs. 3). The coefficients of the external effect among minority households are all significant, with magnitudes of 0.523-0.586 (row 3), which are larger than those in row 1 except in one case. Magnitudes of the external effect are also comparable to those from the *Han* to minority households in non-minority communities (row 2).[37] These results indicate that the external effect for minorities in minority communities is still strong, but only among minority households, who

[36]Poston and Shu (1987) find that differences in fertility behavior and fertility rates between minorities and the *han* Chinese become smaller when minorities live closer to a large *han* group.

[37]The coefficients in row 2 are not significantly different from those in row 3 according to an F-test.

are more likely to socialize with each other.

### 6.2 Rural vs. urban communities

The external effect in rural communities may also be different from that in urban communities. In rural communities, where residents are generally in close proximity, we should observe numerous interactions among households and thus a large external effect. Rural households may also have a stronger sense of traditions and social norms, which helps to strengthen the external effect. In urban communities, where households have relative fewer interactions with each other and a weaker sense of tradition, the external effect should be weaker.

To test whether the external effect in rural communities is different from that in urban communities, we divide the sample into rural and urban communities. We use all the regressors in Table 5 except the urban indicator in the regressions of the divided samples. Regression results of the divided samples in general support our hypothesis that the external effects in rural communities are larger than in urban communities (Table 7, row 4 vs. 5). The coefficients of the external effect for both the rural and urban communities are positive and significant. The magnitudes of the rural sample are larger than those of the urban sample except in one case (column 5).

## 7 Conclusions

In this paper, we test the external or neighborhood effect of fertility, by using unique methods to break the endogeneity. China's special policy environment allows us to solve endogeneity caused by both sorting and unobserved community variables. On one hand, the unique household registration system in China makes our analysis immune from endogenous sorting bias. On the other hand, the unique affirmative birth control policy enables us to conduct two natural experiments. As a result, we can identify with high confidence the pure "interactive" external effect. Such natural experiments are rare in the analysis of peer or neighborhood effects, and in economics in general.

Employing micro-fertility data from China, we find fertility has a large external or neighborhood effect. An increase of the average number of children of neighboring households by 10 percentage points increases a household's fertility by about five percentage points, and an increase of the proportion of second children in neighboring households by 10 percentage points increases a household's probability of having a second child by 4-10 percentage points. These findings are robust for all our methods of controlling for potential biases.

We also find fertility has a strong external effect in non-minority communities where minorities are more likely to have great social interactions with the *Han*. On average, an increase in the proportion of second births for the *Han* by 10 percentage points will increase the probability of a second birth in a minority family by about seven percentage points. The external effect of the *Han* on minority in minority communities, however, is negligible, which could be a result of reduced social interactions between the two communities. In the latter case, social interaction is important only within minority households themselves.

The findings in this paper have important policy relevance. As suggested in the literature, economic development is negatively associated with population growth. If high fertility is one of the most important causes of underdevelopment, then birth control policies is one of the keys to development. China's mandatory birth control policies may not be transferable to other countries, but policies such as providing contraceptives, educating women, and increasing the pay of women can help or even induce some households to experiment with lower fertility. Through the external effect, any initial impact of such policies that break with tradition will be much stronger and sustainable, and will eventuate in low-level fertility equilibrium.

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Table 1: Descriptive Statistics of Fertility and Other Variables in China (N=3774)

| Variables | Mean | Standard Deviation | Min | Max |
|---|---|---|---|---|
| **Number of children per household** | | | | |
| Whole sample | 2.38 | 1.50 | 0 | 9 |
| Rural | 2.43 | 1.46 | 0 | 9 |
| Urban | 2.28 | 1.58 | 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 | 90 |
| Woman's education | 7.0 | 4.0 | 0 | 18 |
| Per capita income (*yuan*) | 1,027 | 939 | 0 | 17,333 |
| Urban indicator | 0.34 | 0.47 | 0 | 1 |
| Minority indicator | 0.19 | 0.39 | 0 | 1 |
| Fine (*yuan*) | 1,332 | 1,455 | 0 | 6,600 |
| Contraceptive facilities (1=available in the community, 0=otherwise) | 0.33 | 0.47 | 0 | 1 |

Table 2: OLS and 2SLS Regressions on the External Effect of the Number of Children

| Independent variables | Dependent variable: Number of Children in a Household | | | | |
|---|---|---|---|---|---|
| | OLS | 2SLS with the following IVs | | | |
| | | Women's age | Women's age and education | Women's age, education and household per capita income | Women's age, education, household per capita income and sex of first child |
| | (1) | (2) | (3) | (4) | (5) |
| Average number of children in neighboring households | 0.691***<br>(23.04) | 0.436***<br>(7.33) | 0.520***<br>(9.68) | 0.530***<br>(10.07) | 0.527***<br>(10.04) |
| First child is a girl | 0.262***<br>(6.03) | 0.256***<br>(5.85) | 0.258***<br>(5.92) | 0.258***<br>(5.93) | 0.258***<br>(5.93) |
| Woman's age | 0.034***<br>(18.55) | 0.036***<br>(18.94) | 0.035***<br>(18.83) | 0.035***<br>(18.82) | 0.035***<br>(18.83) |
| Woman's education | -0.016**<br>(-2.47) | -0.023***<br>(-3.48) | -0.020***<br>(-3.16) | -0.020***<br>(-3.12) | -0.020***<br>(-3.13) |
| Per capita income | -0.057**<br>(-2.46) | -0.071***<br>(-3.04) | -0.066***<br>(-2.85) | -0.066***<br>(-2.83) | -0.066***<br>(-2.84) |
| Urban indicator | -0.181***<br>(-3.84) | -0.200***<br>(-4.19) | -0.194***<br>(-4.08) | -0.193***<br>(-4.07) | -0.194***<br>(-4.07) |
| Observations | 3762 | 3762 | 3762 | 3762 | 3762 |
| Model F-statistics | 230*** | 148*** | 156*** | 157*** | 157*** |

Notes:
Numbers in parentheses are t-statistics. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***.

Table 3: OLS and 2SLS Regressions on the External Effect of the Number of Children

| Independent variables | Dependent variable: Number of Children in a Household | | | | | |
|---|---|---|---|---|---|---|
| | OLS | OLS | 2SLS with the following IVs | | | |
| | | | Women's age | Women's age and education | Women's age, education and household per capita income | Women's age, education, household' per capita income and sex of first child |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Average number of children in neighboring households | | 0.691***<br>(22.80) | 0.435***<br>(7.21) | 0.517***<br>(9.45) | 0.525***<br>(9.77) | 0.522***<br>(9.74) |
| First child is a girl | 0.247***<br>(5.33) | 0.261***<br>(6.02) | 0.256***<br>(5.84) | 0.258***<br>(5.91) | 0.258***<br>(5.92) | 0.258***<br>(5.91) |
| Woman's age | 0.039***<br>(19.62) | 0.034***<br>(18.56) | 0.036***<br>(18.95) | 0.035***<br>(18.85) | 0.035***<br>(18.84) | 0.035***<br>(18.85) |
| Woman's education | -0.034***<br>(-5.14) | -0.016**<br>(-2.46) | -0.022***<br>(-3.45) | -0.020***<br>(-3.14) | -0.020***<br>(-3.11) | -0.020***<br>(-3.12) |
| Per capita income | -0.091***<br>(-3.73) | -0.056**<br>(-2.42) | -0.069***<br>(-2.94) | -0.065***<br>(-2.78) | -0.064***<br>(-2.77) | -0.064***<br>(-2.77) |
| Urban indicator | -0.209***<br>(-4.09) | -0.177***<br>(-3.71) | -0.189***<br>(-3.91) | -0.185***<br>(-3.85) | -0.185***<br>(-3.85) | -0.185***<br>(-3.85) |
| Fine | -0.033**<br>(-2.15) | 0.006<br>(0.42) | -0.008<br>(-0.56) | -0.004<br>(-0.26) | -0.003<br>(-0.22) | -0.004<br>(-0.24) |
| Contraceptive facilities | 0.122**<br>(2.51) | 0.034<br>(0.75) | 0.067<br>(1.43) | 0.056<br>(1.22) | 0.055<br>(1.19) | 0.056<br>(1.20) |
| Observations | 3762 | 3762 | 3762 | 3762 | 3762 | 3762 |
| Model F-statistics | 108*** | 172*** | 112*** | 118*** | 118*** | 118*** |

Notes:
Numbers in parentheses are t-statistics. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***.

Table 4: OLS and 2SLS Regressions on the External Effect of Having the Second Child (coefficients on the proportion of households with second birth)

| | Dependent variable: Whether or not to have a second child (1=yes, 0=no) | | | | |
|---|---|---|---|---|---|
| | OLS | 2SLS with the following IVs | | | |
| | | Women's age | Women's age and education | Women's age, education and household per capita income | Women's age, education, household per capita income and sex of first child |
| | (1) | (2) | (3) | (4) | (5) |
| All households: without controlling for the fine and contraceptive facilities | 0.693*** (20.75) | 0.445*** (5.88) | 0.536*** (7.70) | 0.581*** (9.04) | 0.575*** (8.97) |
| All households: controlling for the fine and contraceptive facilities | 0.690*** (20.48) | 0.449*** (5.92) | 0.532*** (7.58) | 0.573*** (8.77) | 0.567*** (8.70) |

Notes:
Numbers in parentheses are t-statistics. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***.

Table 5: A Natural Experiment on the External Effect of Having the Second Child from *Han* Households to Minority Households

| Independent variables | Dependent variable: Whether or not to have a second child (1=yes, 0=no) | | | | |
|---|---|---|---|---|---|
| | 2SLS with the following IVs | | | | |
| | Fine | Women's age | Women's age and education | Women's age, education and household per capita income | Women's age, education, household per capita income and sex of first child |
| | (1) | (2) | (3) | (4) | (5) |
| The proportion of neighboring *han* households with second birth | 0.586<br>(1.03) | 0.697***<br>(3.32) | 0.720***<br>(3.50) | 0.754***<br>(3.73) | 0.795***<br>(3.94) |
| First child is a girl | 0.076*<br>(1.87) | 0.080**<br>(2.22) | 0.081**<br>(2.24) | 0.082**<br>(2.27) | 0.083**<br>(2.30) |
| Woman's age | 0.006***<br>(2.74) | 0.006***<br>(3.69) | 0.006***<br>(3.65) | 0.006***<br>(3.58) | 0.006***<br>(3.50) |
| Woman's education | 0.000<br>(0.02) | 0.001<br>(0.13) | 0.001<br>(0.16) | 0.001<br>(0.19) | 0.001<br>(0.23) |
| Per capita income | -0.019<br>(-0.73) | -0.016<br>(-0.77) | -0.015<br>(-0.73) | -0.014<br>(-0.68) | -0.013<br>(-0.62) |
| Urban indicator | -0.069<br>(-1.49) | -0.065<br>(-1.57) | -0.064<br>(-1.55) | -0.062<br>(-1.51) | -0.060<br>(-1.47) |
| Contraceptive facilities | -0.023<br>(-0.52) | -0.028<br>(-0.75) | -0.029<br>(-0.77) | -0.031<br>(-0.81) | -0.033<br>(-0.86) |
| Observations | 630 | 630 | 630 | 630 | 630 |
| Model F-statistics | 7.74*** | 9.04*** | 9.20*** | 9.40*** | 9.59*** |

Notes:
Numbers in parentheses are t-statistics. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***.

Table 6: Two Stage Least Squares Examining the External Effect of Having a Second Child Using the Affirmative Birth Control Policy Variables as Instruments

| | | | | | | |
|---|---|---|---|---|---|---|
| | Dependent variable: whether or not to have a second child (1=yes, 0=no) | | | | | |
| Main instrumental variables in (1)-(6) | TMV and TM(1-V) | | | | | |
| Other instrumental variables | None | None | T, M, TMV, T(1-V), M(1-V), and TM(1-V) | | TMV, TM(1-V), age, education, per capita income, sex of first child | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **External effect** | | | | | | |
| The proportion of neighboring households with second birth | 0.879*** | 0.748** | 0.391*** | 0.457*** | 0.995*** | 0.920** |
| | (2.80) | (2.41) | (3.62) | (4.53) | (7.40) | (6.34) |
| **Policy effect in non-minority communities** | | | | | | |
| Minority | -0.076 | -0.083 | -0.100* | -0.101* | -0.074 | -0.081 |
| | (-1.25) | (-1.43) | (-1.68) | (-1.76) | (-1.21) | (-1.39) |
| Proportion of a woman's childbearing years subject to the one-child policy | -0.578*** | -1.538*** | -0.585*** | -1.527*** | -0.576*** | -1.550*** |
| | (-13.91) | (-14.89) | (-14.50) | (-15.10) | (-13.86) | (-15.23) |
| Minority*proportion | 0.272** | 0.279** | 0.276** | 0.292** | 0.273** | 0.280** |
| | (2.26) | (2.42) | (2.28) | (2.52) | (2.26) | (2.42) |
| Proportion of minority in neighborhood | -0.036 | -0.019 | | | -0.026 | -0.006 |
| | (-0.47) | (-0.25) | | | (-0.35) | (-0.09) |
| Average proportion of a woman's childbearing years in neighborhood | 0.513 | 0.305 | | | 0.624*** | 0.473*** |
| | (1.65) | (0.95) | | | (3.77) | (2.68) |
| **Policy effect in minority communities** | | | | | | |
| Minority | 0.034 | 0.039 | 0.048 | 0.054 | 0.026 | 0.029 |
| | (0.38) | (0.44) | (0.76) | (0.89) | (0.28) | (0.34) |
| Proportion of a woman's childbearing years | -0.559*** | -1.596*** | -0.490*** | -1.460*** | -0.570*** | -1.613*** |
| | (-4.79) | (-10.64) | (-6.72) | (-12.28) | (-4.85) | (-10.89) |
| Minority*proportion | -0.099 | -0.135 | -0.165 | -0.208 | -0.079 | -0.117 |
| | (-0.59) | (-0.84) | (-1.11) | (-1.46) | (-0.48) | (-0.74) |
| Proportion of minority in neighborhood | -0.029 | -0.045 | | | -0.022 | -0.032 |
| | (-0.36) | (-0.57) | | | (-0.28) | (-0.41) |
| Average proportion of a woman's childbearing years in neighborhood | 0.550* | 0.455 | | | 0.650*** | 0.594*** |
| | (1.85) | (1.56) | | | (3.42) | (3.12) |
| **Control variables** | | | | | | |
| First child is a girl | | 0.112*** | | 0.111*** | | 0.112*** |
| | | (6.49) | | (6.41) | | (6.47) |
| Woman's age | | -0.021*** | | -0.020*** | | -0.021*** |
| | | (-9.73) | | (-9.61) | | (-9.93) |
| Woman's education | | -0.001 | | -0.002 | | -0.001 |
| | | (-0.37) | | (-0.58) | | (-0.19) |
| Per capita income | | -0.034*** | | -0.042*** | | -0.030*** |
| | | (-2.69) | | (-4.19) | | (-2.81) |
| Urban indicator | | 0.004 | | -0.018 | | 0.019 |
| | | (0.10) | | (-0.55) | | (0.54) |
| Observations | 2077 | 2072 | 2077 | 2072 | 2072 | 2072 |
| Model F-statistics | 29.14*** | 33.84*** | 44.32*** | 44.18*** | 33.11*** | 35.78*** |

Notes:
Numbers in parentheses are t-statistics. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***. The definitions of the instrumental variables are as follows. T is the proportion of a woman's childbearing years subject to the one-child policy, M is the minority indicator (1=minority, 0=*Han*), and V is the minority community indicator (1=minority community, 0=non-minority community).

Table 7: A Natural Experiment on the External Effect of Having a Second Child (coefficients on the proportion of households with a second birth)

| Sub-samples | Dependent variable: Whether or not to have a second child (1=yes, 0=no) | | | | |
|---|---|---|---|---|---|
| | OLS | 2SLS with the following IVs | | | |
| | | Women's age | Women's age and education | Women's age, education and household per capita income | Women's age, education, household per capita income and sex of first child |
| | (1) | (2) | (3) | (4) | (5) |
| **From *Han* to Minority Households** | | | | | |
| Minority communities | 0.264 | 0.684 | 0.498 | 0.376 | 0.335 |
| | (1.27) | (1.05) | (1.11) | (0.88) | (0.79) |
| Non-minority communities | 0.574*** | 0.717*** | 0.690*** | 0.731*** | 0.693*** |
| | (6.56) | (3.18) | (3.07) | (3.36) | (4.05) |
| **From Minority to Minority Households** | | | | | |
| Minority communities | 0.544*** | 0.523*** | 0.525*** | 0.545*** | 0.586*** |
| | (9.40) | (3.68) | (3.77) | (3.96) | (4.34) |
| **From *Han* to Minority Households** | | | | | |
| Rural communities | 0.476*** | 0.737** | 0.750*** | 0.800*** | 0.384* |
| | (5.26) | (2.51) | (2.81) | (3.11) | (1.81) |
| Urban communities | 0.466*** | 0.521* | 0.508* | 0.526* | 0.425** |
| | (3.41) | (1.91) | (1.88) | (1.95) | (2.29) |

Notes:
Numbers in parentheses are t-statistics. Significance level 0.1, 0.05 and 0.01 are noted by *, **, and ***.