Responsibility-Shifting through Delegation: Evidence from China's One-Child Policy*

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Abstract

There is a growing body of experimental evidence indicating that delegation can foster the shifting of responsibility for unpopular actions from a principal to an agent. Using the well-known episode of the one-child policy in China (OCP), we provide field evidence for responsibility shifting through delegation. We compare the impact of the OCP on parents who experienced OCP during 1979-1990 (Phase I), when local governments were the primary enforcer, versus 1991-2015 (Phase II) when the enforcement of the policy was delegated to the civilians by incentivizing them to report their neighbors' violations of the policy, and appointing cluster leaders to monitor neighbors. Our identification strategy exploits the exogeneity of the gender of the first born child and argues that parents whose firstborn was a girl were more likely to violate the OCP because of the traditional Chinese "at least one son" preference. Consistent with the predictions of the responsibility-shifting theory, we find that parents who were more exposed to the OCP in Phase II currently trust their neighbors less and this effect is exacerbated for those parents whose firstborn was a girl. The OCP exposure does not undermine trust in local governments. However, parents exposed to the OCP in Phase I currently trust their local governments less. Again it is the parents

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whose first child was a girl that are impacted more strongly. The OCP exposure does not have a significant impact on trust in neighbors in that phase. We explore three alternative interpretations of the results, but none of them can account for these findings.

1 Introduction

In traditional principal-agent models, principals are assumed to hire agents because the agent either owns private information or has a lower opportunity cost. However, a growing body of experimental literature indicates that the principal-agent relationship might serve to shift responsibility for unpopular actions from the principal to the agent (Coffman, 2011; Hamman, Loewenstein and Weber, 2010; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). In the business sphere, companies use "corporate downsizing consultants" or "firing consultants" to lay off workers on their behalf so that they do not have to take responsibility. In the political sphere, beginning with Machiavelli, several scholars proposed that leaders should delegate the enactment of unpopular measures to agents, thus shifting the responsibilities to them (Fiorina, 1986; Vaubel, 1986).

Even though responsibility-shifting through delegation has been found to be quite effective in the lab, field evidence is still missing. In this work, we fill this gap using evidence from China's One Child Policy (OCP), which was a large scale and well-known birth control campaign carried out from 1979 to 2015.

The OCP provides us with an ideal setting to study the effect of delegation in shifting responsibility. The OCP is a highly undesirable policy. In 1979, the median family in China had 4.5 children. However, under the OCP, most urban couples were only allowed to have a single child. Violation of the policy led to enormous monetary penalties and non-monetary consequences. Starting in 1991, the enforcement of the OCP was delegated to civilians through mass mobilization. The 1990 census revealed that the government was far behind the target of limiting the population to 1.2 billion in 2000. In response, the government began to involve civilians in the enforcement of the policy. The local authorities created monetary and non-monetary incentives for people to report their neighbors' violations of the policy. Grass root enforcement organizations were established and civilians were appointed to enforce the policy in their neighborhoods.

The effects of delegation cannot be fully understood unless we simultaneously study what occurs when delegation is not used as a tool of shifting responsibility, which is enabled by the first phase of the OCP enforcement. From 1979 to 1990, the OCP was directly enforced by government officials, especially in urban areas. The birth-planning commission was established in 1981, and its officials were in charge of enforcing the policy.

Our central hypotheses are: first, when the OCP enforcement was delegated to the civilians in the second phase (1991-2015), parents who were more exposed to the OCP confronted more intense conflicts with their neighbors and trusted them less as a result of responsibility attribution. However, the impact on their trust in local governments is less clear. The local government could decide the strength of the enforcement of the OCP and its officials were heavily involved in the enforcement process. Thus, it can be assumed that citizens may have had reasons to hold the governments responsible. The enforcement of the OCP would not affect parents' trust in local governments if responsibility-shifting strategies are effective enough. Second, when local governments were the primary enforcers during the first phase (1979-1990), parents' trust in their neighbors should not have been affected as the neighbors were not involved in the enforcement. However, parents who were more exposed to the OCP are thought to have trusted local governments less as they were the sole party that was held responsible.

The strength of the OCP enforcement varied across provinces and time, depending on local economic conditions, the demographic setting, and other political concerns. We construct an individual level measure of the OCP exposure in urban areas from 1979 to 2015. In the second phase, we used the average fertility penalty rate a person faced within five years after the arrival of his or her first child as the measure. The fertility penalty was the amount of monetary punishment one needed to pay for an above-quota birth. The fertility penalty not only proxies the strictness of the OCP enforcement as suggested by the previous literature (Ebenstein, 2010; Huang, Lei and Sun, 2015), but it also positively correlated with the financial incentives the local government could provide for the informers. For example, an informer in Chongqing in 2009 could receive five percent of the fertility penalty paid by the victims. We count the penalty rates in the years after the birth of the first child because citizens are restricted by the OCP only after having their firstbirth. The five-year interval is selected because, according to the China Census, most people's second child arrives within five years after the first child.

The fertility penalty data is largely unavailable for Phase I. Therefore, while keeping the construction of the OCP exposure measure the same, we replace the fertility penalty with the rate of family planning, which was defined as the percentage of couples who were of fertility age and had taken birth control measures, like vasoligation, sterilization, and intrauterine devices (IUDs). In the 1980s, voluntary birth control was quite rare. Thus, the family planning rates measures how successful the local governments were in enforcing the OCP. In fact, family planning rate was one of the main indicators used by higher-order governments to evaluate the performance of the local birth-planning commissions.

Responsibility is the variable of interest in this study, and trust is our proxy variable for responsibility. A lower level of trust is a sign of being held responsible. In particular, we focus on citizens' trust in their neighbors and local governments, which are measured by the China Family Panel Study 2016 (CFPS-2016) survey.

The main empirical strategy of the study exploits the exogenous variation in the gender of the first child, which was previously used by Li and Wu (2011) to measure the bargaining power of women within the household, and by Wei and Zhang (2011a) to vary the strength of competitive saving motive. Given the deep-rooted belief that one family needs *at least one son* to maintain the family's lineage, parents whose firstborn was a girl were more likely to violate the OCP by trying to have a second child, instead of parents whose firstborn is a boy. Thus parents with a firstborn daughter were more exposed to the OCP.

Consistent with the predictions of the responsibility-shifting theory, we find that parents who were more exposed to the OCP in the second phase currently trust their neighbors less. This effect is significantly stronger for those parents whose firstborn child was a girl than for those whose firstborn child was a boy. However, the OCP exposure in Phase II does not undermine trust in local governments. Furthermore, the coefficients on trust in neighbors are significantly different from coefficients on trust in local governments at the 5% level. On the contrary, parents who were more exposed to the OCP in the first phase currently trust the local governments less and, this effect is significantly stronger for those parents whose firstborn was a girl. At the same time, the OCP exposure does not have a significant impact on their trust in their neighbors. Moreover, the coefficients on trust in neighbors are significantly different from the coefficients on trust in local governments at the 1% level.

A natural concern, given our identification strategy, is the possibility that the gender of the first child was not perfectly exogenous due to pervasive sex selection practices, such as selective abortion in China. However, multiple sources of evidence suggested that sex selection rarely happened with the first child (Ebenstein, 2010; Li, Yi and Zhang, 2011; Wei and Zhang, 2011b; Li and Wu, 2011). We also directly test the correlations between the gender of the first child and all the control variables in the empirical analysis. We do not find a significant correlation. To further validate our results, we use data from the year 2000 Census to test whether the sex ratios of the firstborn were higher than the natural ratio of 1.06 (boys to girls) or not. Only six provinces' sex ratios were significantly higher than 1.06 at the 5% level. Excluding the six provinces does not change our core results.

We consider three main alternative explanations for the empirical patterns and show that none are supported by the data. One alternative explanation is that the local governments were not held responsible in Phase II because people were unwilling/dare not to report their mistrust in the government. However, it should be noted that even though criticizing the central government is to some extent prohibited, people in China are allowed, and even encouraged, to openly express dissatisfaction against the local governments (King, Pan and Roberts, 2013; Berkowitz, Lu and Wu, 2018). In our sample, the mean value of trust in local governments is, in fact, smaller than the mean value of trust in neighbors. A second alternative interpretation is that people did not attribute responsibility to local governments in the second phase because people merely treat them as the policy enforcers, and they blamed the policy maker, the central government. We construct two proxies for trust in the central government due to the lack of direct measurement. Using the same identification strategy, we do not find any evidence that people held the central government responsible. Finally, another possible reason people did not hold the local governments responsible in the second phase is they performed better, regarding economic development and public goods provision, than the local governments in the first phase. Thus, the positive image canceled out the negative impact of the OCP. However, for this story to fully account for our results, it must be that the performance of the local governments in the second phase was positively associated with the fertility penalty, the measure of OCP exposure. We examine the relationship between the fertility penalty and government performance, but do not find a significant correlation.

We contribute to the lab responsibility-shifting literature by providing, to the best of our knowledge, the first field evidence (Coffman, 2011; Hamman, Loewenstein and Weber, 2010; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). In a typical lab setting, the principal in the control group chooses between a fair allocation and an unfair allocation that benefits herself at the cost of a recipient. In the treatment group, another option is added: she can delegate the allocation choice to an agent whose interest is aligned with hers. The authors find that when the unfair allocation is chosen by the principal, the recipient who is adversely affected is willing to costly punish the principal harshly. However, if the task is delegated to the agent and the unfair allocation is chosen, then the principal receives a much smaller punishment and the agent is punished. In this paper, we replicate the main results in a well-known and large-scale field setting.

By confirming the effectiveness of delegation in responsibility avoidance, this paper also provides a rationale for mass mobilization. By encouraging people to fight against each other, governments can avoid the responsibility for implementing an unpopular policy, a cost they must bear if they need to do all the work themselves. Even though mass mobilization is widely observed in authoritarian regimes, it has been overlooked in economics. The only exception is Lichter, Loeffler and Siegloch (2015). Using county-level data of the number of informers in the 1980s in East Germany, the authors show that higher levels of government surveillance led to lower levels of political trust in post-reunification Germany. The key difference is in their scenario, even though the number of informers was quite large, they were still contracted government employees. The governments were still responsible for their actions, while in our setting, the informers were mobilized civilians.

In a broader context, our findings contribute to the growing literature focusing on conflict and trust. Nunn and Wantchekon (2011) identify a persistent impact of the historical slave trades on current trust levels within Africa. Rohner, Thoenig and Zilibotti (2013) document causal effects of ethnic conflict on trust and ethnic identity using individual, county-level and district-level data from Uganda. Chen and Yang (2015) study the causal effect of the Great Chinese Famine (1958-1961) on the survivors' and the subsequent generation's distrust in the government. While the current literature rarely separate the possible mechanisms about how conflicts affected social trust, we provide a clear mechanism of how mass mobilization affects people's interpersonal and institutional trust.

Lastly, this study adds to the literature studying the impacts of the OCP in two ways. The consequences of the OCP range from economic growth (Li and Zhang, 2007) to sex ratio imbalance (Ebenstein, 2010; Li, Yi and Zhang, 2011), female education (Huang, Lei and Sun, 2015), promotions of mayors (Serrato, Wang and Zhang, 2016), and to competitive saving motive (Wei and Zhang, 2011a). While the current literature focuses on the effect of the policy *per se*, this project considers the hidden cost of the policy enforcement. Second, the construction of an individual-level OCP exposure enables us to exploit within-province variations of OCP enforcement in addition to the between-province variations used in previous studies.

Our paper proceeds as follows: Section 2 describes the historical background, institutional setup, and important features of the OCP. Section 3 describes the various data sources used in this study. Section 4 introduces our identification strategy and empirical model. We present the main results in Section 5. We provide a discussion of alternative explanations in Section 6. Section 7 concludes and discusses the policy implication.

2 Background of One Child Policy

China's One Child Policy is credited with dropping the total fertility rate from 2.81 in 1979 to 1.51 in 2000 (World Bank). The Chinese have long favored large families. Total fertility exceeded six births per mother throughout the 1960s (Banister, 1991). In the 1970s, after two decades of explicit encouragement of population growth, policy makers in China enacted a series of measures to curb population growth. The OCP was introduced in 1979 and began to be formally phased out in 2015. Under the OCP, most urban couples were only allowed to have a single child. However, the regulations varied among regions. Provincial governments localized the state fertility policy due to the diversity of demographic and socioeconomic conditions across China. As the main instrument for enforcing the OCP, financial penalties also varied across provinces.

The government employed various methods to enforce the OCP. Parents after the first or second birth were required to have insertion of an intrauterine device (IUD), or sterilization. The sterilization rate, which is defined as the percentage of women of reproductive age who underwent sterilization, increased from 21% to 35% between 1979 and 1999. (Scharping, 2013). At times, the government used a more draconian method - induced abortions of unauthorized pregnancies - as a "remedial measure making up for contraceptive failures" (Scharping, 2013). For above-quota births, the government mainly used fertility penalties to enforce the OCP. Depending on the province of residence and time period, the fertility penalty for an unauthorized child equaled 10% to 25% of a family's annual income for 7-14 years (Serrato, Wang and Zhang, 2016). In urban areas, other forms of punishments were also widely used. For example, people employed in urban units were threatened with the denial of health and welfare benefits, bonus payments, lack of job promotions, or even demotions.

From the 1990 census, the central government found a large number of "excess" births during 1986-1990. In order to achieve the 1.2 billion population limit for the year 2000, the central government began a stricter enforcement of the OCP. In May 1991, the central government started a 'one-vote-down' (yi piao fou jue) campaign. The chief Officials at each administrative levels were made personally responsible for achieving the birth-control targets. If targets were not achieved, the chief officials would not be promoted, and might even lose their jobs. Second, in December 1991, the central government began to take birth-control performance into the regular performance evaluations for government officials [1]. Local authorities increased the grants for the program and gave it stronger political support. Enforcement of the program was also strengthened. The campaigns resulted in a huge jump of penalties for one unauthorized child and were succeeded by a substantial decline in fertility.

2.1 Mobilizing the Masses

In response to the "excess" births found in the 1990 census, the central government launched a mass mobilization campaign. This movement was clearly stated by Tieying Li, who was a member of the Central Politburo of the Communist Party at that time. He said "No one is allowed to give birth beyond the birth-quota, and let the masses watch each other," in an internal speech on April 21, 1990. The rationale for this campaign was twofold. First, the local cadres lacked the necessary information about who was pregnant and whether it was above quota or not. There were not enough local officials to monitor every woman of childbearing age, and those parents who planned to give unauthorized births intentionally hided from them. The government thus relied on people who were close to the pregnant women to provide the information. Second, there were not enough local officials to enforce the policy even if information was provided. Enforcing the OCP was not only about sterilizations and forced abortions. The cadres also launched propaganda campaigns to promote the idea of one child and carried out the so-called "Three Examinations," which check women for the use of contraceptive rings, pregnancy, and illness four times (or more) a year. The crew was needed to be expanded in a short time with a limited budget, which made asking the citizens to be "volunteers" highly attractive.

Two measures were taken to mobilize the masses. First, to deal with the information asymmetry, the citizens were encouraged and incentivized to report unauthorized pregnancies and births of their neighbors, coworkers, and relatives to the authority. The incentives provided by the government were mainly monetary.[2] The payments varied across regions and were linked to fertility

[1]The Performance Evaluation System for government officials is an important component of the government personnel management in China. Cadres take the evaluations very seriously. The evaluation result is one of the most influential factors affecting their career appointments, promotions, transfers, and removals (Wang, 2013).

[2]In some cases, reporting could also lead to career rewards, and failing to report could sometimes cause collective punishments. The career rewards for denunciation were salient when there were competitions between colleagues. Public sector employees who were caught violating the OCP would not be promoted in most cases and might even lose their jobs. When there was a quota for promotions, the hidden career rewards could be huge. Collective punishments were collected When a violation was detected, but someone who was aware of it failed to report. During the years when the OCP was

punishments. For example, in Chongqing in 2009, the informer could be awarded five percent of the fertility penalty paid by the victims, which was the equivalent of one month's salary.[3] In addition, the informer's identity was kept confidential and there was no record of punishment for a false report.

Second, to deal with the ever-increasing workload, more at-will employees and "volunteers" were recruited by the government. They carried out most of the detailed work instead of government officials. They were generally seen by people as neighbors.

In order to discuss the recruitment and duties of those workers, facts about local management in urban China are needed. The family-planning commission is the institute in charge of the enforcement of the OCP. Its lowest level was located in the county government. The residents' committees were asked to work with it to achieve the birth-planning targets. The residents' committee (sometimes also translated as the neighborhood committee) is the lowest level of urban administration in China. According to the Chinese constitution, they enjoy a high degree of autonomy and are named the "self-government organizations of the masses." The residents' committee are allowed to recruit at-will employees and pay them independently. Although the residents' committees are formally the lowest organizational entities, there are entities one level below them – the residents' small groups (jumin xiaozu). They may comprise a neighborhood or just an apartment building. The small groups are a means of an internal organization and do not have a legal status on their own. Their leaders are called "cluster leaders". They are appointed by the residents' committee, but are not on the government payroll.

Enabled by the revenue collected from the fertility penalty in the 1990s, the residents' committees recruited many at-will full-time and part-time OCP enforcers. For example, Huangjiapu, a residents' committee with a population of five hundred in Shanxi Province, had fifteen full-time employees tasked with family-planning matters in its peak (Fong, 2016). As those people were paid by the residents' committees but not the higher authority, they were not entitled to the social welfare benefits enjoyed by government employees and thus were not cadres in people's eyes. They were often seen as neighbors instead. The recruitment of the OCP enforcers still failed to solve the labor shortage. Consequently, the cluster leaders were mobilized to enforce the pol-

most fiercely implemented, one worker's violation of the OCP could lead all of her or his coworkers to lose a significant part of their income in some state-owned enterprises. (http://wap.sciencenet.cn/blogview.aspx?id=749707)

[3] http://www.chinalawedu.com/lvshi/AAA635949214532/5$_8$912.*shtm*

icy. They were tasked with keeping track of households' reproductive habits and reporting those details to the local family-planning commission. These leaders were also seen as neighbors. In addition to the cluster leaders, state organizations such as the military, public schools, and hospitals had their internal family-planning units as did state-owned enterprises.

These neighborhood-level staffs and cluster leaders were the most basic building blocks of China's birth-planning machinery. According to a report issued by the national family-planning commission, while they only had half a million full-time employees, there were about 1.2 million neighborhood-level birth-planing staffs and more than six million cluster leaders (Fong, 2016).

3 Data

To estimate the effect of delegation on trust, we use measures of trust from the China Family Panel Study (CFPS) and individual-level OCP exposure data from Scharping (2013) and other sources. The CFPS is a nationally representative panel survey conducted by the Institute of Social Science Survey at Peking University. We introduce our measurement of different types of trust and enforcement level of the OCP in section 3.1 and section 3.2. Table 1 reports the summary statistics.

3.1 The Measurement of Trust

The primary outcomes of interest are citizens' trust in neighbors and government, which are measured by the CFPS-2016 survey. The questions asked in the survey are translated as follows: Please rate to what degree you trust neighbors. Similar questions were also asked on trust in local governments. One may be concerned about the validity of categorical trust measures. However, Johnson and Mislin (2012) provide experimental evidence that trust, as measured by the World Values Survey, is positively correlated with experimentally measured trust. The questions asked in the CFPS are very similar to those in the World Values Survey. From the summary statistics (Table 1), we can see that people generally trust their neighbors more than the local governments.

3.2 The Individual-level OCP Exposure

Our key explanatory variable is an individual-level measure of the exposure to the OCP: the average fertility penalty an urban resident faced during the five years after the arrival of his or her first child. More precisely, for an individual i living in urban areas of province p whose first child was born in year t, his or her exposure to the OCP is measured by the mean value of the penalty rate in province p from year $t+1$ to year $t+5$.

Following current literature (Ebenstein, 2010; Huang, Lei and Sun, 2015), we use provincial fertility penalties for one unauthorized child to measure the OCP enforcement. Ebenstein (2010) have found that the strictness of the OCP enforcement is well reflected by the monetary penalties that have been implemented since 1979. As displayed in Figure 1, fertility penalties vary across provinces and across time. At the provincial level, as documented by Scharping (2013), there are three forms of fertility fines. The first form is a percentage deduction in wage over several years. For example, in February 1980, Guangdong province ratified a fine of ten percent of income from each parent for fourteen years for an unsanctioned birth. The second type of fines is levied as a lump sum payment based on annual income. For example, Shanghai reported in 1992 that an unauthorized birth carried an immediate payment of three years of household income. The third type of fines is a certain amount of immediate payment regardless of household income. For example, from 1995 to 2000, Guangxi ratified the fine as an amount between 2,000 RMB and 50,000 RMB. Following Ebenstein (2010), we normalize all three types of fines and measure the fine rate as a percentage of household income. Regarding the time trend, fines increased over time but the timing of the changes were quite different among provinces. Our measure of the OCP exposure incorporates these two kinds of variations.

We improve the measure of the OCP enforcement in the literature in two ways. First, we modify the penalty data used in the literature for seven out of twenty-five provinces. As mentioned earlier, the fertility fine had three different formulas. For the first two, we follow Ebenstein (2010)'s calculations. We mainly improve the third one, which is a certain amount of money that does not depend on the household income. To normalize it into a percentage of income, Ebenstein (2010) assumes that the annual household income is fixed at 10,000 RMB from 1991 to 2000 in all provinces. However, China experienced rapid and unbalanced growth when the OCP was fiercely implemented. For instance, the annual average household incomes were 4,630 RMB in Liaoning in 1993, 7,095 RMB in Beijing in 1993 and 20,833 RMB in Beijing in 2000. To account for the variation in household income, we replace the fixed 10,000 RMB with the actual average household income in a certain province in a certain year, using data from China Statistical Yearbooks. More details of our calculation are provided in the Data Appendix.

Second, we construct an individual level measure of the OCP instead of the provincial level. In our measure, only penalty rates implemented after parents having their first child counts. The reason is that individuals were restricted by OCP only after having the first child. China Census data show that the interval between the birth of most couples' first and second children was no more than five years (Scharping, 2013). Therefore, the strongest impact of the birth control policy fell on young couples during the five years following the birth of their first child. That is why we only count the rates within five years. However, as shown in the next section, changing the interval to four or six years does not alter our results qualitatively. This measure exploits the individual variations in the timing of the first child's birth. People give birth to their first child at different times. One obvious factor is the birth cohort: older people have their first child earlier than younger people. However, even for two people of the same age, the chance that they will give their first birth at the same time is low. Many factors have impacts on the timing of having a child. One may worry about the possibility that parents rationally chose the timing of the first birth to enjoy a looser OCP enforcement. However, that is unlikely. First, the provincial OCP enforcement policy was mainly made by the provincial government and was highly unpredictable for people without special connections to the provincial government. Second, to choose a looser OCP enforcement, one would need to plan the timing of having the first child, the timing of the second, and foresee the enforcement strength for several years. Even if it could be done, it might be too costly or too complicated to implement.

Sample Selection For our empirical estimation, we limit our sample to individuals who completed all the CFPS-2010, CFPS-2012 and the CFPS-2016 survey. We further limit our sample based on two criteria: (i) individuals that resided in the urban sector and (ii) individuals who gave birth to their first child between 1979 and 1985 or between 1991 and 2010.

In practice, the fertility penalty was often the same for all households in the same village for the sake of lacking informatio1n. Additionally, some low income families could not afford the massive amount of penalties. In urban areas, this was not a problem as the penalties could be collected on a monthly basis and be directly deducted from the salary. However, the cash flow of the rural residents was not as stable as that of their urban counterparts. Rural cadres often chose to collect as much as the low-income family could afford on a lump-sum basis. Hence, there was a lot of randomness in the enforcement of the OCP in the rural areas.

We restrict our sample to urban households because we do not have a valid measure of OCP exposure in rural areas. Compared to urban areas, rural areas are farther away from administrative centers. It was much harder for the higher level government to make sure that the local cadres closely followed the provincial policy. Furthermore, it was also difficult for the local cadres to collect information on the households' annual income, on which the fertility penalty was based and calculated. In practice, the fertility penalty was often set to be the same for all households in the same village for the sake of lacking information. Additionally, some low income families could not afford the massive amount of penalties. In urban areas, this was not a problem as the penalties could be collected on a monthly basis and be directly deducted from the salary. However, the cash flow of the rural residents was not as stable as that of their urban counterparts. Rural cadres often chose to collect as much as the low-income family could afford on a lump-sum basis. Hence, there was a lot of randomness in the enforcement of the OCP in the rural areas.

For criteria (ii), The reason we choose to look at people who gave birth to the first child after 1979 is it is the year the OCP started. We restrict our sample to individuals who gave birth to their first child in (or after) before 2010 because the OCP was formally phased out in 2015. We exclude people who gave birth to the first child after 2010 from our sample as they only experienced OCP during part of the 5 years interval after the birth of their first child. For a similar reason, we exclude people whose first child arrived between 1986 and 1990 as we cannot tell whether they experienced Phase I OCP or Phase II OCP.

4 Empirical Strategy

4.1 Identification Strategy

Our main identification strategy exploits the exogenous variation in the gender of the first child. A deep-rooted belief in the Chinese culture is that each family needs *at least one* son to maintain the lineage. Consequently, urban couples whose first child is a girl are more likely to violate the OCP by trying to have a second child than parents whose first child is a boy. If so, they are more exposed to the OCP punishments. To validate our empirical strategy, we provide evidence that the gender of the first child is truly exogenous. Then we show that the propensity to give birth to a second child is higher among couples whose first child is a girl.

One may concern that the gender of the first child is not exogenous because sex selection is a widespread practice in China. However, there are few sex selections performed the first child. Sex selection practices can be performed either prenatally (sex selective abortion) or postnatally (for example, female infanticide). The bulk of sex-selection in China has been taking place prenatally as the accessibility of sex-selective technology improves (e.g. Edlund, 1999; Das Gupta, Jiang, Li, Xie, Woojin and Bae, 2003). Chen, Li and Meng (2013) provides evidence of few sex-selective abortions on the first-born child using data from the Chinese Children Survey. Figure A11 demonstrates that for first pregnancies, the sex ratio at birth (males/females) is close to being natural [4]. In some years, it was close to the average natural rate of 1.06 and was never higher than 1.14. Moreover, the abortion ratio (the number of abortions/number of children born) is smaller than 0.05% for the first born child. Both the abortion ratio and the sex ratio at first birth remained stable over the years of the survey (clustered in the lower left corner of the panel). The positive correlation between sex ratio and abortion rate is driven mostly by second pregnancies and higher order pregnancies.

Our statement that the gender of the first child is exogenous is confirmed and complemented by the nationwide census data of China in 1982, 1990, and 2000. As documented by Ebenstein (2010), the ratio of boys and girls for the first born child is also close to being natural. Row 2 of Table A2 indicates that the male fractions of the first birth were 0.511, 0.510, and 0.515 in 1982, 1990, and 2000 respectively. A simple transformation shows that the male/female sex ratios were 1.045, 1.041, and 1.062 in the three waves of the census.

To further check whether the gender of the first child is truly exogenous, we run a regression of the gender of the first child on all other control variables that will be used later in the empirical analysis. Table 3 reports the results using both a linear probability model and a probit model. People do not endogenously choose the gender of the first child based on the fertility penalties of previous years. Also, none of the other control variables is statistically significant in explaining the gender of the first child.

The exogenously determined gender of the first child affects Chinese parents' later childbearing behaviors. Theoretically, the number and gender composition of children a couple has been determined by the couple's child preferences, chances and constraints. A typical preference can be that a couple prefers two boys and two girls to four boys to one boy and one girl. The constraints can include monetary constraints, time constraints and policy constraints such as the OCP

[4] According to (Wilson and Hardy, 2002), the natural sex ratio at birth was estimated at 106 boys to 100 girls. We shall also point out that a range of natural and environmental factor may influence the natural sex ratio. For example, Mathews et al. (2005) suggested the following with extensive statistical evidence. The age of the mother impacted the sex ratio: mothers aged 25 to 35 had babies with a sex ration of 1.05 in average. But the sex ratio ranging between 0.94 and 1.11 for mothers who were below the age of 15 or above 40 had babies. The race was also an important factor: the ratio was 1.05 for the white non-Hispanic population, 1.04 for Mexican Americans, 1.03 for African Americans and Indians. It is highest (1.07) for mothers of Hawaiian, Filipino, Chinese, Cuban and Japanese ethnicity, with years as high as 1.14 over the 62-year study period. However, for the last result, whether those differences were purely driven by nature or social factors also played a role is still an open question.

in China. Here we define the "son preference" as preferring a children profile with at least one son to a profile with no son. Our definition of "son preference" does not mean that Chinese always prefer more sons. We allow a couple with "son preference" to prefer one boy and a girl to two boys. However, they must at the same time prefer two boys to one girl, two girls, or no child. This specific definition of "son preference" indicates that people whose first child is a girl are not eager to stop at one child. Even though trying to have more than one child leads to OCP penalties, the potential benefits of having a son outweighs the cost for some. However, for people whose first child is a boy, the potential benefits of an additional child vastly diminishes and they may find it too costly to violate the OCP. Therefore, we should expect parents whose first child is a girl to be more exposed to OCP penalties and denunciations.

This "at least one son" belief is deeply rooted in the Chinese culture. Confucianism,as the "state religion" in ancient China, is still strongly influencing the Chinese people in modern times. In Confucian philosophy, filial piety is one of the four virtues. It means to be good to one's parents, which requires ensuring male heirs. Mencius or Mengzi, who is the most famous Confucian after Confucius himself, once said "There are three forms of unfiliality, and bearing no heirs is the worst" (Chan, 2002; Shun, 1997).

Evidence from census data suggests that the "at least one son" belief is quite persistent in modern China. Column 5 of Table A2 illustrates the probabilities of stopping giving birth after having a certain combination of children using census data in 2000 (Ebenstein, 2010). It suggests that 49% of those with one daughter had a second child, while for those with a son the number was 35%. We consider this gap to be large as people who are not affected by the OCP were also included in the census. The preference of "at least one son" becomes even more clear when we look at people with two children. For people who already had two boys, the chance of having another child was 18%, which is slightly different from that of people who had one boy and one girl (16%). However, the chance increased to 46% for people who had two girls, suggesting that couples did make a particular effort to have at least one boy. Graham et al. (1998) provides similar evidence based on a household survey in Anhui Province in 1993.

4.2 Empirical specification

Let i index individuals, c index birth cohorts, and p index provinces. We model an outcome of interest y_{icp}, which could be trust in neighbors or local government. Our key independent variable of interest is $1stChildpenalty^{1-5}_{icp}$, which we define as the five year mean value of the penalty rates in province p after individual i has his/her first child.

$$y_{icp} = \sum_c \alpha_c + \sum_p \delta_p + \beta_0 1stChildpenalty^{1-5}_{icp} + \mathbf{X}'_{icp}\gamma + \beta_1 1stChildGirl_{icp}$$
$$\sum_c \alpha_c \times 1stChildGirl_{icp} + \sum_p \delta_p \times 1stChildGirl_{icp} + \mathbf{X}'_{icp}\gamma \times 1stChildGirl_{icp} \quad (1)$$
$$+\beta_2 1stChildpenalty^{1-5}_{icp} \times 1stChildGirl_{icp} + \varepsilon_{icp}$$

Where $1stChildGirl_{icp}$ is the dummy variable for the gender of the first child, where $1stChildGirl_{icp}$ equals to 1 if individual i's first child is a girl. β_0 captures the effect of OCP exposure on trust for individuals whose first child is a boy. The main coefficient of interest is β_2, capturing the different impact of OCP exposure on trust for individuals whose first child is a daughter from those with a male first child. $\mathbf{X}'_{icp}$ is a vector of observable characteristics for individual i birth cohort c province p. Here we include income, education attainment level in 2016. α_c and δ_p are full sets of birth cohort and province of current residence fixed effects. By conditioning on province fixed effects, our empirical specification absorbs all time-invariant province-specific trust characteristics. By conditioning on cohort fixed effects, we can difference out cross-cohort changes in trust that occur even in the absence of OCP. Lastly, ε_{icp} is the error term. We cluster our standard errors at the province level to allow for correlation over time within a province. Due to the smaller number of clusters in this case (25), we also implement a wild cluster bootstrap-t procedure (Cameron, Gelbach and Miller, 2008) for improved inference and present the corresponding p-values.

We choose this specification (equation 1) over the alternative specification (equation 2) for an improved chance to obtain unbiased estimates of the key parameters. The alternative specification is the following:

$$y_{icp} = \sum_c \alpha_c + \sum_p \delta_p + \beta_0 1stChildpenalty^{1-5}_{icp} + \beta_1 1stChildGirl_{icp}$$
$$+\beta_2 1stChildpenalty^{1-5}_{icp} \times 1stChildGirl_{icp} + \mathbf{X}'_{icp}\gamma + \varepsilon_{icp} \quad (2)$$

This alternative specification requires that the effects of all control variables such as the individual's age and gender on her trust are identical regardless of the gender of the first child. At times, it's difficult to satisfy this requirement. The baseline specification (equation 1) is one way to relax this unnecessary restriction: the coefficients on the control families are allowed to take different values for different households. Therefore, the baseline specification (equation 1) can help rule out the possibility that some unobserved individual characteristics simultaneously determine the gender of the first child and parents' trust. Running regressions this way would reduce possible bias in the estimates of β_1 at the cost of lower efficiency. With the relatively large sample size, we can afford to sacrifice some efficiency to obtain unbiased estimates.

5 Results

5.1 The Effect of OCP exposure between 1991 to 2015 on Trust

In Table 4, we present the regression estimates from equation 1 on the two trust outcomes: trust in neighbors and trust in local governments. In columns 1 and 3, we present the baseline estimates using a parsimonious specification that includes only province and birth cohort fixed effects. We add individual-level controls as our preferred specification from equation 1, including education attainment, and family income per capita. Our results are consistent across different specifications. The remaining discussion focuses on our preferred specification.

Regarding trust in neighbors (Column 1), the OCP enforcement significantly lowers trust in neighbors for couples whose first child is a girl, but not for couples whose first child is a boy. The estimates for the difference between the two groups (β_2) are statistically significant at less than 1 percent and economically large. As the gender of the first child is arguably exogenous, this result suggests that the effect we found is causal. The parameter estimates indicate that relative to people whose first child is a boy, a one standard deviation increase in the OCP exposure is associated with 0.244 standard deviation more reduction in citizens' trust in neighbors for people whose first child is a girl.

Next, we proceed to examine whether the OCP affects people's political trust. Due to the political sensitivity of eliciting trust in the central government in mainland China, we are only able to measure citizens' trust in the local government explicitly. Columns 3-4 of Table 4 present the corresponding estimation results. One can see that there is no significant impact of OCP exposure on trust in local government regardless of the gender of the first child. Also, there is no significant difference between the two groups of people. The estimate of coefficient β_2 is small and statistically insignificant. This finding is particularly striking because people blame their neighbors for turning them in, but they did not blame the government who initiated the harm.

However, this does not mean that OCP exposure has different effects on trust in neighbors and trust in local government. This is based on the impact on trust in neighbors, which is statistically significant and the impact on trust in government is not. We conduct a triple-difference analysis and report the p-value of the difference of the two coefficient estimates; one can see that the two coefficient estimates are significantly different at 5 percent level, suggesting OCP exposure has different effects on the two trust measures.

5.2 The Effect of OCP exposure in the 1980s on Trust

In the previous section, we showed that when the OCP implementation was delegated, more severe enforcement significantly undermined people's trust in neighbors but not in government. Nevertheless, another question arises: if the government implemented the policy by itself instead of delegating it, would the people blame the government but not the neighbor? The early ages of the OCP enable us to investigate this situation. The policy came into effect in 1979, but the masses were not mobilized until 1991. Thus parents whose first child was born between 1979 and 1990 were exposed to the OCP but were not hurt by their neighbors. Looking into their trust in government and neighbors can help us answer the above question.

Ideally, we want to keep our analysis consistent and use fertility penalty to measure OCP severity. However, 11 out of 31 provinces had not established their fertility penalty policies in 1984. Some provinces introduced their first fertility penalties in 1988. We use the family-planning rate instead as the measure of the OCP severity in this period. The rate of family-planning is the percentage of couples who are in fertility age and have taken birth-control measures such as sterilization, IUDs and birth-control pills. Voluntary birth control was quite rare in the 1980s; the family-planning rates measured how successful the local governments were at enforcing the OCP. It was one of the main indicators used to evaluate the performance of the local birth-planing commissions.

One limitation of this approach is that the local governments have strong incentives to over-report the family-planning rate in order to achieve its birth-control targets. However, as long as the degree of over-reporting is the same across province and time (which is likely as local cadres faced similar incentives when reporting the family-planning rate), our measure of the OCP exposure is still valid. Even if this assumption fails to hold, the degree of over-reporting is still orthogonal to the gender of the first child and thus our identification strategy can handle this problem.

Consistent with our individual-level measure of the OCP exposure, we use five-year average family-planning rate an urban resident experienced during the five years after the arrival of his/her first child. More precisely, for an individual i living in urban areas of province p whose first child was born in year t, his/her exposure to OCP is measured by the five-year average family-planning rate in province p from year $t+1$ to year $t+5$. To further validate the family-planning rate measure, we cross check the correlation between fertility penalties and the family-planning rate when both have data in the same year, the two measures are significantly correlated at 5% level (Table A10).

We can see from Table 7 that when the policy was solely implemented by the government during the 1980s, the results are quite the opposite: citizens blame their government, not neighbors. For people whose first child is a girl, a one standard deviation increase in family-planning rates leads to 0.38 standard deviation more decrease in trust in the local government relative to people whose first child is a boy. The estimates for the difference between the two groups of people (β_2) are statistically significant at 5% level. In contrast to the results of people's trust in local government, there's no significant impact on trust in neighbors regardless of the gender of the first child and the difference between the two groups of people is not significant.

5.3 Mechanism

Another important question about our analysis is whether our results are driven by systematic differences between couples whose first child is a girl or a boy beyond the OCP exposure. According to our story, people that have more than one child, regardless of the gender of the first child, all violate the OCP policy, so their exposure to the OCP is the same. Therefore, there should be no significantly different impact between couples whose first child is a girl or a boy. Table 8 and Table 9 show that our results are mainly driven by people who have only one child. There's no significant difference between people whose first child is a daughter and people whose first child is a son if they have more than one child. If the two groups are also different in ways other than the OCP exposure, then the other difference should be the same between people who have only one child and people who have more than one child. Moreover, the effects should also be the same between people who have only one child and people who have more than one child.

5.4 Robustness Checks

One potential problem of our work is that the gender of the first child is not perfectly exogenous. We argue that, if anything, such selections are likely to work against the results we find. Here, the couples whose first child is a girl are the treatment group and the couples whose first child is a boy are the control group. Due to the sex selections favoring boys, some couples who are in the treatment group are mistakenly identified as the control group, which makes it harder for us to observe a difference between the two groups. Furthermore, Table 5 lists the number of boys and girls firstbirths across provinces from June 1999 to June 2000 in urban areas according to the 2000 census. Since the sample sizes are too small to calculate a precise sex ratio, we construct a one-tailed t-statistics to test whether the calculated sex ratio (Column 4) is statistically different from the biological sex ratio (Appendix A0.2 further shows the detailed construction of the one-tailed t-statistics). We find that six provinces' sex ratios at first birth are significantly higher than the normal sex ratio at 5% level: Beijing, Jiangsu, Jiangxi, Hubei, Guangdong, and Guangxi. To further validate our results, we drop observations from the six provinces and replicate the specifications in Table 4. Estimation results, reported in Table 6, indicate that dropping them does not affect our findings.

Although we have argued that it is unlikely for parents to endogenously choose the timing of the first birth to enjoy looser OCP enforcement, we perform another robustness check using an alternative measure of the OCP exposure: the minimum penalty (family-planning rate) in the five years after people having their first child after 1991 (before 1991). We re-estimate our baseline equation with the alternative measures of OCP exposure. The results are unaffected (Table A4 and Table A5 in Appendix).

We next examine the robustness of the OCP exposure measure. Recall that we used the five-year average fertility penalties (or family-planning rates) after people having their first child. Our results are quite consistent when we use a four-year (table A6 and table A8) or six-year (table A7 and table A9) average fertility penalties (or family-planning rates).

6 Discussions

We showed when the enforcement of the OCP, an extremely unpopular policy, was delegated to civilians, people's trust in their neighbors declined significantly but not their trust in local government. However, when the government officials enforced the OCP, people's trust in it is undermined. The results are consistent with the responsibility-shifting effect of delegation. In this section, we address whether we can attribute the results to the delegation. In what follows, we explore three alternative interpretations of our results: the local governments are held responsible in the second phase but people are unwilling/dare not to report their mistrust; people don't blame the local government but they blame the central government in the second phase; or the local governments in the second phase performed better than the governments in the first phase and their good performance canceled out the responsibility.

6.1 Dare Not To Report Mistrust

An important concern regarding the absence of a significant impact on trust in local government in the second phase is that people try to express politically correct views. To address this possibility, we present the distributions of responses to the question of trust in local government (Table A3) in this period. From the broad range of answers to the question, an indication is that respondents are likely not attempting to provide "correct" responses. If there is such a "correct" response, then one would expect that 10 is the correct value of trust in government. However, in fact, people's trust in government is not abnormally high. The mean value of trust in local government is smaller than the mean value of trust in neighbors in our sample.

Also, we find a significant negative impact on trust in local government in the 1980s, which further suggests respondents are willing to report their mistrust in local government. The finding that no significant impact on trust in local government when the policy was delegated is not driven by the fact that respondents dare not to report their mistrust in local government.

6.2 Trust in Central Government

The second concern about our estimates is that people do not hold the local governments responsible in the second phase because people merely treat the local government as the policy enforcer, not the policy maker. So it is important to see whether there's any effect on trust in the central government in Phase II. Due to the political sensitivity of eliciting trust in the central government in mainland China, we are only able to measure trust in the local government. Even though we can potentially use trust in the local government to cautiously extrapolate trust in the central government (Cantoni, Chen, Yang, Yuchtman and Zhang, 2017), one might wonder how big the correlation is between the two trust measures. As an additional check, we construct two proxies for trust in the central government as the dependent variables.

The first proxy for trust in the central government is an indicator variable where 1 represents the individual is/was a member of the Communist Party. Choosing to join the Communist Party *per se* reflects one's political attitude and belief. This measure certainly has many problems. One problem is that people may choose to become a member of the Communist Party simply due to career concerns. Another problem is that people may lose their party membership due to violations of the OCP, which can also explain a negative correlation between OCP exposure and the probability of being a party member.

The second proxy is the difference between the number of days accessing political news through television and the number of days accessing political news through the Internet. It is a well-established fact that all television broadcasters in China are the "mouthpiece" of the Party (Zhao, 1998; Shirk, 2011). While Internet censorship is much looser than media censorship, Internet censors focus their attention on silencing speech that may generate collective action, rather than criticism of the government (King, Pan and Roberts, 2013). Moreover, Internet users can browse international news channels that are not provided on television. So if one does not trust the central government, she is more motivated to access the political news from a less censored media source - the Internet. We calculate the difference in media source to get rid of the variation in how much one cares about political news. The bigger the difference, the higher the level of trust in the central government.

From Table 10 across all columns, we do not find any significant difference in the impact of the OCP exposure on trust in the central government between people whose first child is a girl and people whose first child is a boy. This suggests that people not only do not blame the local government but they also do not blame the central government either.

6.3 Better Performance in the Second Phase

A third concern is that even though we find that there is more reduction in trust in local government when the OCP was enforced more strictly in the first phase and there is no such effect in the second phase, the difference can be attributed to other factors instead of delegation. One potential factor is the performance of the government. It could be that the governments in the second phase performed better regarding economic development or public goods provision and that is why people do not blame them. However, for this story to explain our results, it must be that the performance of the local governments is associated with the fertility penalty, which is our main measure of OCP exposure. As shown in Table 11, we did not find any significant correlation between the fertility penalty and GDP per capita, urbanization rate, or public expenditure.

7 Conclusion

This paper provides field evidence on responsibility avoidance through delegation using evidence from China's OCP. Consistent with the predictions of the responsibility-shifting theory, we find that when local governments were the primary enforcer of the policy in Phase I, parents strongly exposed to the OCP currently trust their local governments less and, it is the parents whose first child is a girl who are more strongly impacted. The OCP exposure does not undermine trust in neighbors. However, when the local governments delegated the enforcement of the policy to civilians in Phase II, parents who were more exposed to the OCP currently trust their neighbors less and, this effect is stronger for those parents whose first born child is a girl. We do not find any significant impact on trust in local governments among people who experienced Phase II OCP.

Authoritarian governments frequently mobilize the masses to enforce unpopular policies. Civilians are encouraged and incentivized to denounce their neighbors, friends, and co-workers. Grass-roots organizations recruit at-will workers and volunteers to help the government officials to enforce the policies. For example, during Stalin's Great Purge, civilians were often sent to the Gulag as a result of reports initiated by friends and neighbors (Fitzpatrick, 1999). In Nazi Germany, homemakers, dentists, and other average citizens turned in their Jewish neighbors after petty neighborhood quarrels (Johnson, 2000). In a recent episode, the President of the Philippines, Rodrigo Duterte, encouraged vigilantes among the general population to commit violence against suspected drug users in his brutal drug war. In addition to information collecting, our findings provide another rationale of why mass mobilization is so popular among the (authoritarian) governments-it helps the government avoid part of the responsibility.

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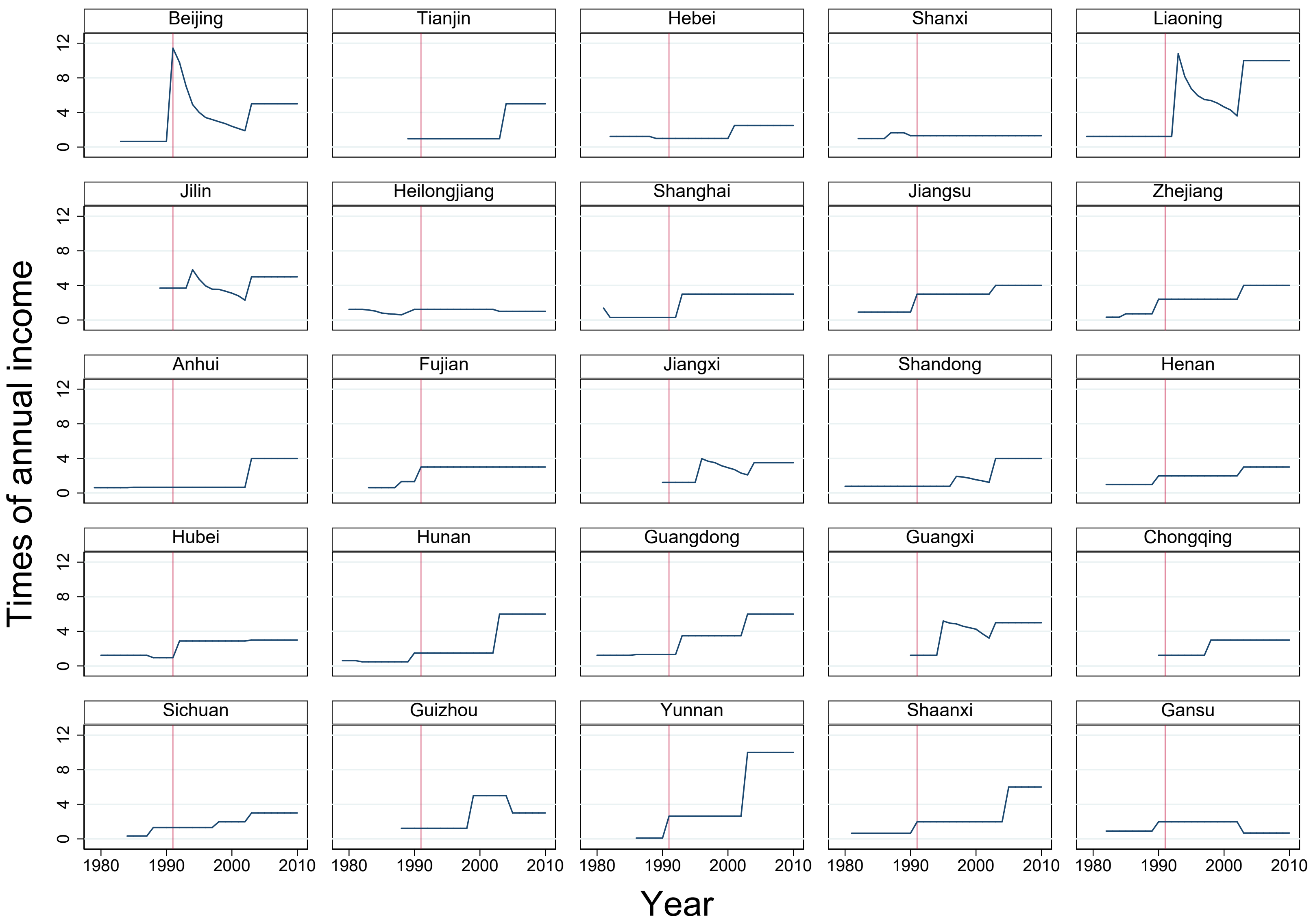
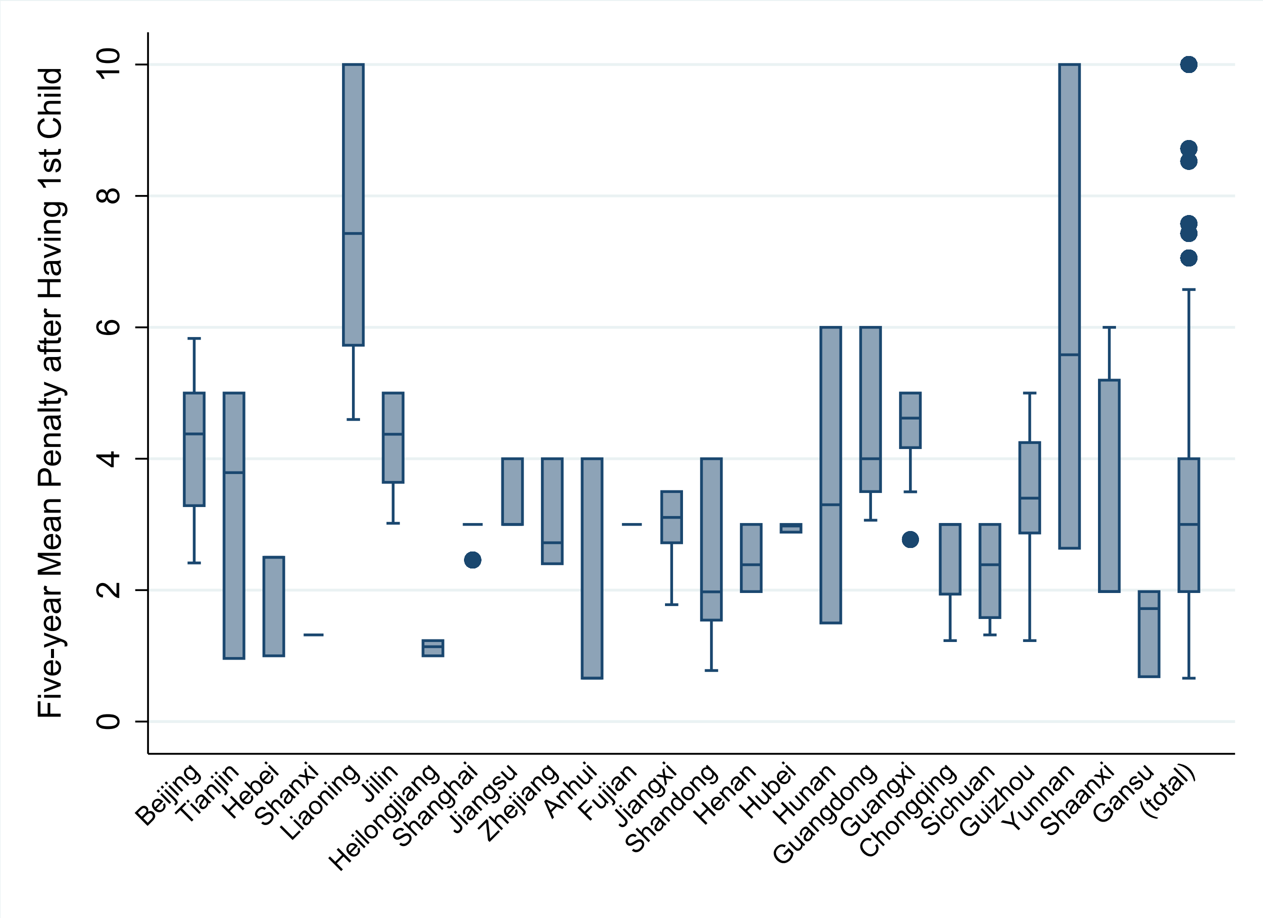
Figure 1: Provincial Fertility Penalties in Urban China



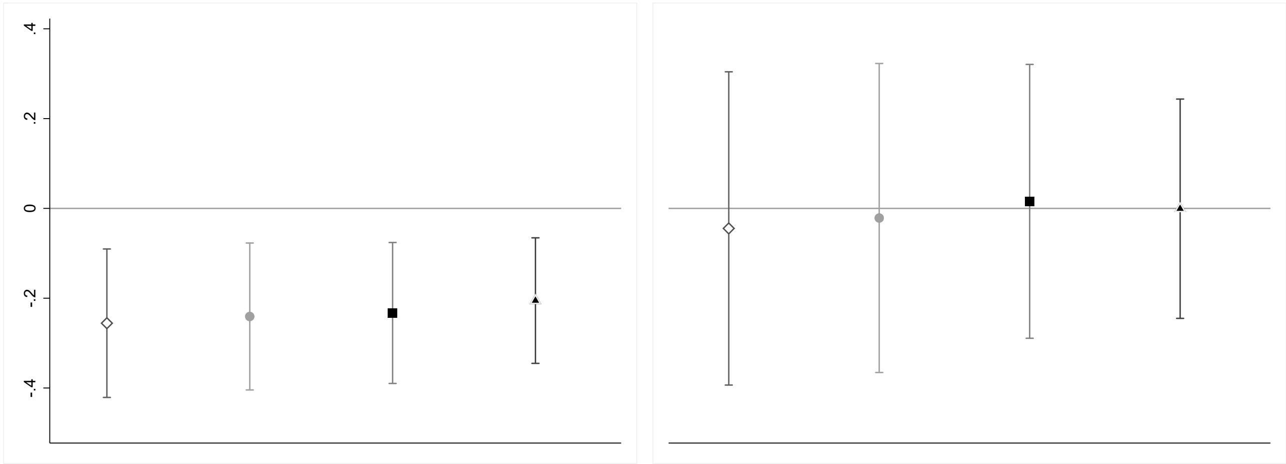
Note: The fertility penalties are measured in times of annual household income. For example, number 4 on the y-axis means a household needed to pay 4 times of its annual income after being caught violating the OCP. Data source: Scharping (2013) and authors' calculation.

Figure 2: Box-and-whisker Plots of Within- and Between-Province Variations in OCP Exposure



Note: We plot the within- and between-province variations in OCP exposure for each province and China as a whole (the first one to the right). The boxes cover the interquartile range, from the lower quartile to the upper quartile. The whiskers, denoted by horizontal lines, extend to cover most or all the range of the data. Here, we place the upper whisker at the upper quartile plus 1.5 times the interquartile range, or the maximum of the data if this is smaller. Similarly, the lower whisker is, the lower quartile minus 1.5 times the interquartile range, or the minimum should this be larger. Any data values outside the whiskers are represented with dots.

Figure 3: The Effect of OCP Exposure on Trust in Phase II

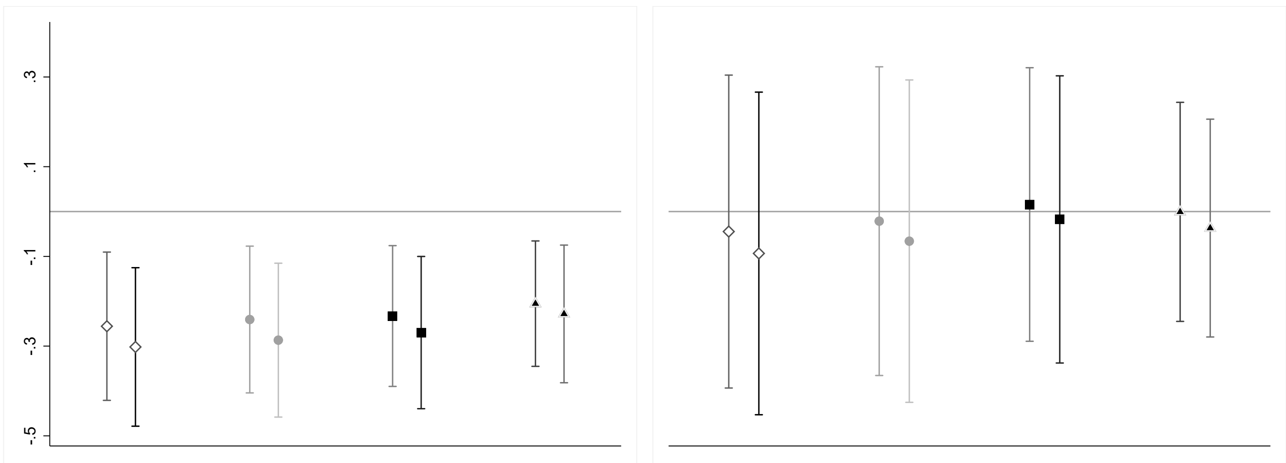


(a) Trust in Neighbors (b) Trust in Local Government

◇ 4 years ● 5 years ■ 6 years ▲ min of 5 years

Notes: Figure plots estimates of the effect of OCP exposure on trust in neighbors and trust in local governments in phase II, based on estimates of β_2 from equation 1. We report estimates using several OCP exposure: the five-year (four-year or six-year) average fertility penalty rate an individual face after having his/her first child and the minimum of the five year fertility penalty rate an individual face after having his/her first child.

Figure 4: The Effect of OCP Exposure on Trust in Phase II - Without Provinces of High Sex Ratios

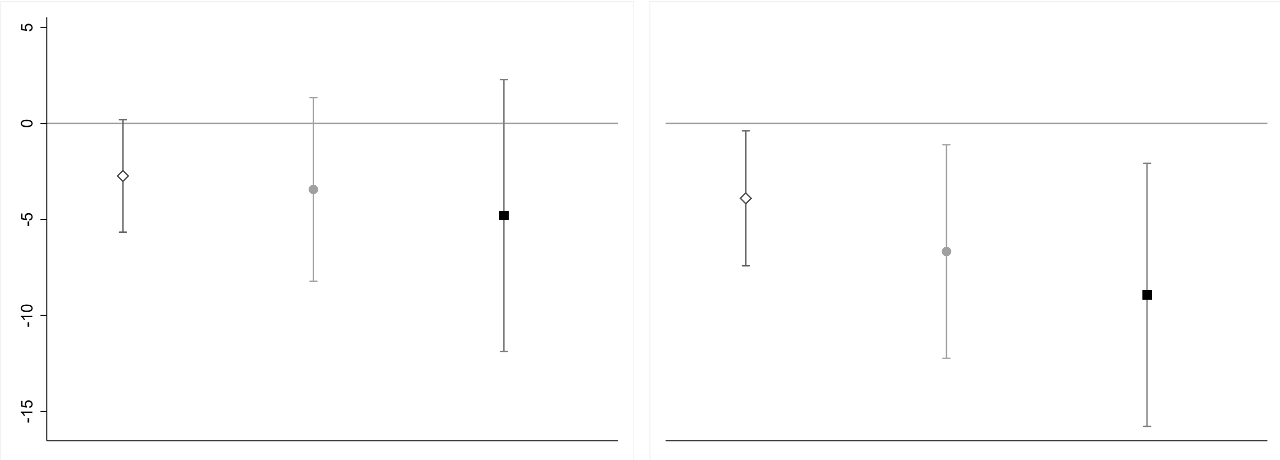


(a) Trust in Neighbors (b) Trust in Local Government

◇ 4 years ● 5 years ■ 6 years ▲ min of 5 years

Notes: Figure plots estimates of the effect of OCP exposure on trust in neighbors and trust in local governments in phase II, based on estimates of β_2 from equation 1. The sample excludes six provinces: Beijing, Jiangsu, Jiangxi, Hubei, Guangdong, and Guangxi. We report estimates using several OCP exposure: the five-year (four-year or six-year) average fertility penalty rate an individual face after having his/her first child and the minimum of the five year fertility penalty rate an individual face after having his/her first child.

Figure 5: The Effect of OCP Exposure on Trust in Phase I

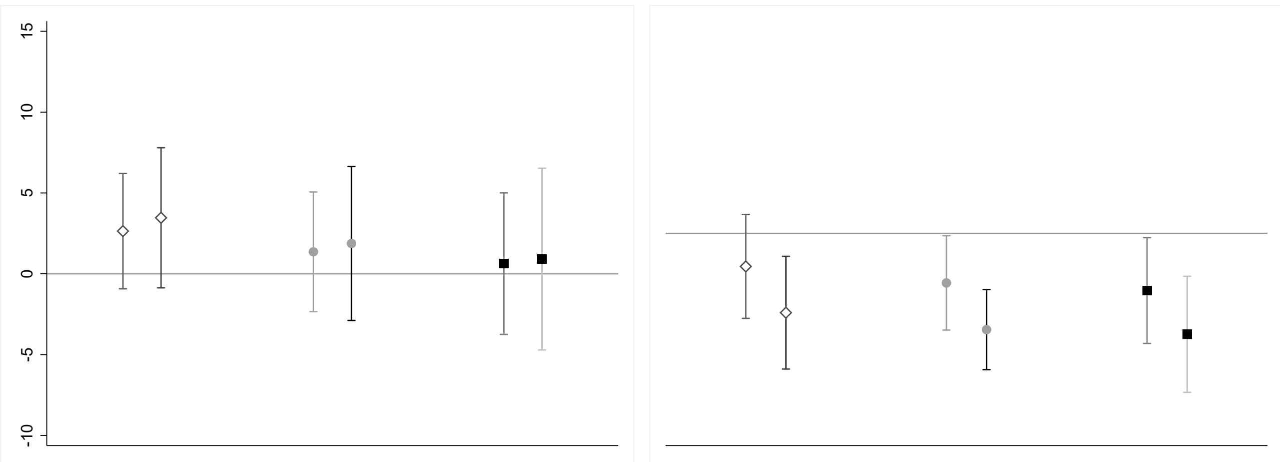


(a) Trust in Neighbors

(b) Trust in Local Government

Notes: Figure plots estimates of the effect of OCP exposure on trust in neighbors and trust in local governments in phase I, based on estimates of β_2 from equation 1. We report estimates using several OCP exposure: the five-year (four-year or six-year) average family-planning rate in the province an individual face after having his/her first child and the minimum of the five year fertility family-planning rate an individual face after having his/her first child.

Figure 6: The Effect of OCP Exposure on Trust in Phase I - Without Provinces of High Sex Ratios



(a) Trust in Neighbors

(b) Trust in Local Government

Notes: Figure plots estimates of the effect of OCP exposure on trust in neighbors and trust in local governments in phase I, based on estimates of β_2 from equation 1. The sample excludes six provinces: Beijing, Jiangsu, Jiangxi, Hubei, Guangdong, and Guangxi. We report estimates using several OCP exposure: the five-year (four-year or six-year) average family-planning rate in the province an individual face after having his/her first child and the minimum of the five year fertility family-planning rate an individual face after having his/her first child.

Tables

Table 1: Summary Statistics

Variable	Obs	Mean	Sd
1991-2010			
Trust in neighbors[a]	1897	6.407	2.065
Trust in local government[a]	1897	4.304	2.47
OCP exposure	1897	3.387	2.093
Age in 2016	1897	42.78	6.002
Family income per capita	1897	9.746	.867
Education attainment	1897	3.618	1.303
1979-1985			
Trust in neighbors[a]	822	6.582	2.072
Trust in local government[a]	822	4.957	2.502
Birth control rate	822	89.164	3.101
Number of female at fertility age (000s)	822	78.33	39.37
Age in 2016	822	59.78	3.683
Family income per capita (in thousands)	822	27.71	34.79
Education attainment	822	2.94	1.168

a: catagarical variables: 0 = extremely low trust;10 = extremely high trust

Table 2: Within-Province Variation in OCP Exposure

	Five-year Average Penalty after an Individual Having His/Her First Child			
Region	Mean (1)	Normalized Standard Deviation (Standard Deviation/Mean) (2)	Min (3)	Max (4)
China	3.22	0.66	0.66	10.00
Beijing	4.06	0.23	2.41	5.83
Tianjin	3.15	0.59	0.96	5.00
Hebei	1.93	0.35	1.00	2.50
Shanxi	1.32	0.00	1.32	1.32
Liaoning	7.52	0.27	4.60	10.00
Jilin	4.32	0.16	3.02	5.00
Heilongjiang	1.13	0.09	1.00	1.23
Shanghai	2.97	0.04	2.46	3.00
Jiangsu	3.41	0.14	3.00	4.00
Zhejiang	3.10	0.24	2.40	4.00
Anhui	2.03	0.76	0.66	4.00
Fujian	3.00	0.00	3.00	3.00
Jiangxi	2.98	0.17	1.78	3.50
Shandong	2.53	0.49	0.78	4.00
Henan	2.47	0.19	1.98	3.00
Hubei	2.95	0.02	2.88	3.00
Hunan	3.51	0.58	1.50	6.00
Guangdong	4.56	0.26	3.06	6.00
Guangxi	4.48	0.13	2.77	5.00
Chongqing	2.54	0.27	1.23	3.00
Sichuan	2.25	0.30	1.32	3.00
Guizhou	3.39	0.35	1.23	5.00
Yunnan	6.03	0.54	2.64	10.00
Shaanxi	3.28	0.52	1.98	6.00
Gansu	1.42	0.42	0.68	1.98

Note: The statistics refer to individuals who had their first child between 1991 and 2002 within each province.

Table 3: Factors that Predict Gender of the First Child

Dependent variable	Having a first-born daughter		
	Probit	Probit	Linear prob.
	(1)	(2)	(3)
OCP exposure	0.024	0.017	0.007
	(0.032)	(0.039)	(0.015)
Age		-0.002	-0.001
		(0.008)	(0.003)
Log (income)		-0.029	-0.011
		(0.027)	(0.010)
Years of education		0.010	0.004
		(0.007)	(0.003)
Province FE	Y	Y	Y
Observations	1,726	1,726	1,726

Note: *** p<0.01, ** p<0.05, * p<0.1. Table presents estimates of how observable characteristics predict the gender of the first child. The dependent variable is whether the first child is a girl. Robust standard errors are in parentheses.

Table 4: Estimates of OCP Enforcement Levels on Trust in Phase II (1991-2010)

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure	0.045	0.033	-0.041	-0.053
	(0.064)	(0.061)	(0.080)	(0.085)
First-born daughter	-0.282***	-0.275***	-0.074	-0.073
×OCP exposure	(0.086)	(0.087)	(0.156)	(0.163)
p-Value	[0.003]	[0.004]	[0.639]	[0.656]
Bootstrap p-value	[0.004]	[0.004]	[0.684]	[0.724]
	[0.023]	[0.029]		
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.407	6.407	4.304	4.304
Std.Dev.DV	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.089	0.098	0.078	0.088

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure in phase II is defined as the five year mean value of the fertility penalty rates in province p after individual i has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). We report the corresponding p-values in brackets. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 5: Sex ratio at First Birth (Males/Females) in Urban China in 2000

Province	Obs	Sex ratio	One-tailed t-statistics
Beijing	**4579**	**1.130**	**2.156**
Tianjin	2282	1.063	0.074
Hebei	8356	1.090	1.277
Shanxi	5582	1.081	0.742
Liaoning	10525	1.061	0.063
Jilin	4940	1.104	1.426
Heilongjiang	7108	1.079	0.747
Shanghai	5518	1.089	1.014
Jiangsu	**12635**	**1.099**	**2.039**
Zhejiang	8938	1.072	0.526
Anhui	6499	1.082	0.816
Fujian	5182	1.091	1.044
Jiangxi	**4367**	**1.178**	**3.489**
Shandong	17607	1.080	1.253
Henan	11090	1.076	0.808
Hubei	**9403**	**1.124**	**2.821**
Hunan	7072	1.052	-0.309
Guangdong	**20607**	**1.166**	**6.825**
Guangxi	**4418**	**1.131**	**2.160**
Chongqing	4010	1.022	-1.150
Sichuan	8763	1.098	1.644
Guizhou	3886	0.988	-2.201
Yunnan	4056	1.004	-1.729
Shaanxi	4365	1.060	-0.002
Gansu	3240	1.058	-0.042

Data source: China Census 10% sample (2000)

Table 6: Estimates of OCP Enforcement Levels on Trust in Phase II (1991-2010): Without High Sex Ratio Provinces

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure	0.038	0.021	-0.039	-0.059
	(0.063)	(0.062)	(0.097)	(0.101)
First-born daughter	-0.306***	-0.304***	-0.111	-0.108
$\times$OCP exposure	(0.087)	(0.085)	(0.172)	(0.180)
p-Value	[0.003]	[0.002]	[0.527]	[0.557]
Bootstrap p-Value	[0.004]	[0.004]	[0.692]	[0.784]
	[0.038]	[0.033]		
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.409	6.409	4.218	4.218
Std.Dev.DV	2.059	2.059	2.500	2.500
Observations	1,472	1,472	1,472	1,472
R-squared	0.105	0.119	0.091	0.103

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure in phase II is defined as the five year mean value of the fertility penalty rates in province p after individual i has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). We report the corresponding p-values in brackets. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 7: Estimates of OCP Enforcement Levels on Trust in Phase I (1979-1985)

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure	0.100	0.086	0.109	0.098
	(0.082)	(0.081)	(0.085)	(0.075)
First-born daughter	-0.067	-0.037	-0.338***	-0.337***
×OCP exposure	(0.087)	(0.084)	(0.074)	(0.082)
p-Value	[0.449]	[0.667]	[0.000]	[0.000]
Bootstrap p-Value	[0.552]	[0.688]	[0.004]	[0.004]
	[0.004]	[0.002]		
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.582	6.582	4.957	4.957
Std.Dev.DV	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.142	0.155	0.140	0.156

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in phase I is defined as the five-year average family-planning rate in province p after an urban resident has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). We report the corresponding p-values in brackets. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 8: Heterogeneous Effect of OCP Enforcement Levels on Trust in Phase II

Dependent variable:	Trust in			
	Neighbors		Local government	
	One child	≥ Two children	One child	≥ Two children
	(1)	(2)	(3)	(4)
OCP exposure	0.039	0.006	-0.114	-0.206
	(0.073)	(0.308)	(0.085)	(0.386)
First-born daughter	-0.258***	0.151	0.043	-0.306
×OCP exposure	(0.079)	(0.384)	(0.173)	(0.505)
Mean DV	6.370	6.558	4.227	4.621
Std.Dev. DV	2.067	2.058	2.454	2.509
Observations	1,516	380	1,516	380

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in phase II is defined as the five year mean value of the fertility penalty rates in province p after individual i has his/her first child. The sample in columns (1) and (3) is parents who have a single child. The sample in columns (2) and (4) is parents who have at least two children. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table 9: Heterogeneous Effect of OCP Enforcement Levels on Trust in Phase I

Dependent variable:	Trust in			
	Neighbors		Local government	
	One child	≥ Two children	One child	≥ Two children
	(1)	(2)	(3)	(4)
OCP exposure	0.016	0.228**	0.036	0.425
	(0.107)	(0.106)	(0.057)	(0.268)
First-born daughter	0.001	0.072	-0.411***	-0.084
×OCP exposure	(0.151)	(0.131)	(0.110)	(0.323)
Mean DV	6.439	6.783	4.802	5.176
Std.Dev. DV	2.111	2.003	2.475	2.527
Observations	481	341	481	341

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in phase I is defined as the five-year average family-planning rate in province p after individual i has his/her first child. All regressions include a full set of province and cohort fixed effects. The sample in columns (1) and (3) is parents who have a single child. The sample in columns (2) and (4) is parents who have at least two children. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table 10: Estimates of OCP Enforcement Levels on Trust in Central Government

Dependent variable:	Party membership	Media source for political news
	(1)	(2)
OCP exposure	0.004	-0.367***
	(0.009)	(0.112)
First-born daughter	0.007	0.147
×OCP exposure	(0.017)	(0.158)
Mean DV	0.112	-1.506
Std.Dev.DV	0.316	3.440
Observations	1,897	1,386
R-squared	0.195	0.122

*** p<0.01, ** p<0.05, * p<0.1. All regressions include a full set of province and cohort fixed effects. The dependent variable in column (1) is whether the individual is/was a member of the Communist Party. The dependent variable in column (2) is the difference between the number of days accessing political news through television and the number of days accessing political news through the Internet. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 19.

Table 11: Correlation between Fertility Penalty and Government Performance

Dependent variable:	Fertility penalty
ln (GDP per capita)	0.409
	(1.609)
Urbanization rate	-0.800
	(0.994)
Share of public expenditure	1.170
	(1.356)
Mean DV	2.600
Std.Dev. DV	2.071
Observations	534

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses.

Appendices

A0.1 Fertility Penalty Data

The fertility penalty data in Ebenstein (2010) are taken from Scharping (2013), which provides an overall view of China's fertility policies and outcomes. Scharping draws on a large number of primary and secondary sources (statistics, laws, directives, internal documents, conferences, etc.) at local, national and international levels, collected over ten years. Specifically, he documented the complete record of the published fine rates across provinces ranging from 1979 to 2000 (Table A1) [5]. We modified the penalty data for seven provinces: Beijing, Inner Mongolia, Liaoning, Heilongjiang, Jiangxi, Shandong and Guangxi. Furthermore, we extend the fine rates data from 2000 to 2010 using provincial governments' documents[6].

There are mainly three types of fines across provinces documented by Scharping (2013). We calculate all three types of fines as percentages of current year's annual income. The first type is collected from wage earners in the form of regular deductions. For fines levied as wage deductions, Ebenstein calculates the present value of the penalty at a 2 percent discount rate, yielding a single amount in years of income. For example, in February 1980 Guangdong province ratified a fine of 10 percent of income from each parent for 14 years for an unsanctioned birth, which in his data is calculated as having a present value of 1.21 years of income. The detailed calculation is

$$Penalty_{Guangdong}^{1980} = 0.1 + 0.1 \times (1-0.02) + 0.1 \times (1-0.02)^2 + ... + 0.1 \times (1-0.02)^{13} = 1.21 \tag{A0.3}$$

The second type of fines is levied as a share of annual income. For example, Shanghai reported in 1992 that an unauthorized birth carried an immediate payment of three years of household income. When provinces report a specific deduction as a share of annual income, the fine variable used in Ebenstein (2010) is taken directly from these provincial regulations.

The third type of fines is collected as an immediate payment in a certain amount of money. For example, from 1995 to 2000, Guangxi ratified the fine as an amount between 2,000 and 50,000 yuan. In this circumstance, Ebenstein (2010) calculate the fine amount with the following

[5] The data source of Scharping (2013) is based on two books: Zhongguo Jihua Shengyu Quanshu [Encyclopedia of Birth Planning in China] and Zhongguo Renkou Congshu 1987-1993.

[6] The data were downloaded from http://www.pkulaw.cn/

assumptions. First, the fine is collected at the maximum amount of the range[7]; second, the average household annual income is fixed at 10,000 RMB across province and time[8]. We can apply his rules to the Guangxi example. According to his first assumption, the maximum amount in the range, 50,000 yuan, is taken as the penalty amount. Then based on his second rule, the 50,000 yuan fine is equivalent to $\frac{50{,}000}{10{,}000} = 5$ years of income.

For the first two types of fines, we follow Ebenstein (2010)'s calculation rules.

For the third type of fines, instead of assuming that the annual household income is fixed at 10,000 RMB across province and time, we impute the fine into a share of income using the provincial average household annual income. The income is only averaged at the provincial level because fines data are only available at this level and we want to match the two. The income data are taken from the China Statistical Yearbooks. We add this variation of income across province and time for two reasons. First, there is an unneglectable variation of annual household income across the province in China at that time. For instance, the annual household incomes are 4,630 yuan in Liaoning and 7,095 yuan in Beijing in 1993. In the same year, both provinces levied an amount of 50,000 yuan fine for an unauthorized child. According to Ebenstein (2010)'s calculation, individuals in both provinces faced the same amount of penalties, i.e., five years of income. However, we calculate the fines as $\frac{50{,}000}{4{,}603} = 10.80$ years of income in Liaoning and $\frac{50{,}000}{7{,}095} = 7.05$ years of income in Beijing. Therefore, Ebenstein (2010)'s calculation underestimates both provinces' fines relative to other provinces and overestimates Beijing's penalties relative to Liaoning.

Second, there is also substantial time variation of annual household income within province due to the rapid economic growth in the 1990s. For example, Beijing ratified the fine as 50,000 yuan from 1991 to 2000, while the average household annual income increases from 4,371 yuan in 1991 to 20,833 yuan in 2000. Hence we calculate the fines as $\frac{50{,}000}{4{,}371} = 11.44$ years of income in 1991 and as $\frac{50{,}000}{20{,}833} = 2.40$ years of income in 2000. While in Ebenstein (2010)'s data, the fines variable is coded as five years of income from 1991 to 2000. Consequently, the penalties in Beijing is systematically underestimated in earlier years and overestimated in later years. This issue applies to all provinces who define fines as a range of specific amounts of currency.

We take the information on policy publishing month into consideration: the current year's

[7]We also assume that the fine is collected at the maximum amount, hereafter we only talk about the maximum amount.

[8]The only exception for the second assumption is Heilongjiang from 1983 to 1988, whose average annual household income is taken as 1,000 yuan.

penalty applies if the new policy is implemented before the end of June; otherwise, the previous year's penalty applies.

Table A1: Monetary Penalties for Excess Fertility, Urban China 1979-2010

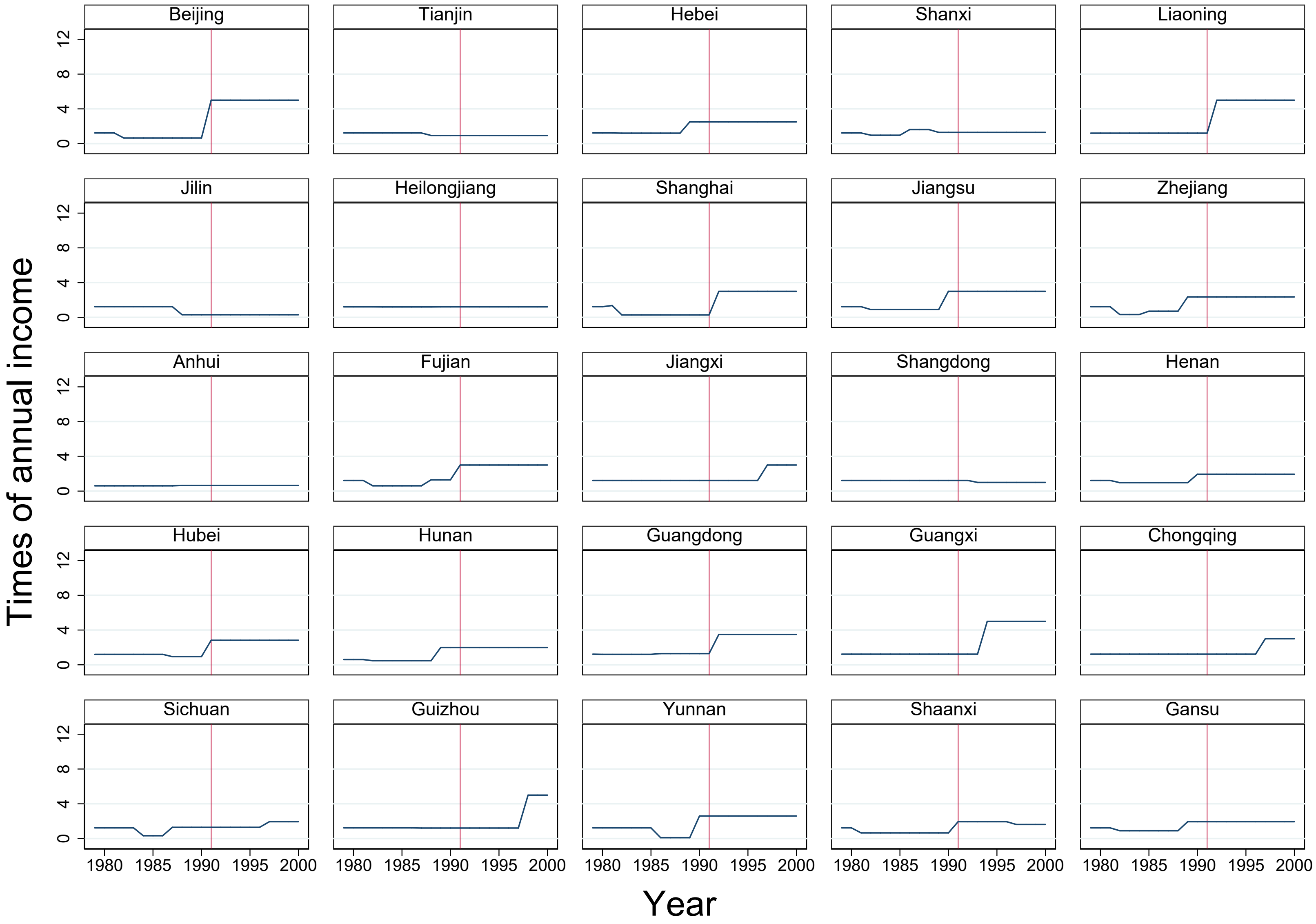
Province	First Report	Second Report	Third Report	Fourth Report	Fifth Report	Six Report
Beijing	1982.10: 7Y,10%	1991.5: 5-50000¥	2002.12: 1Y, 300%-500%			
Tianjin	1988.11: 5Y, 20%	1997.1: 5Y, 20%	2003.9: 200%-500%			
Hebei	1982.4: 14Y, 10%	1989.3: 1Y, 100%	1997.9: 1Y, 100%	2001.3: 1Y, 250%		
Shanxi	1982.6: 7Y, 15%	1986.12: 7Y, 25%	1989.9: 7Y, 20%	2002.9: 7Y, 20%(¿5000¥)		
Inner Mongolia	1982.6: 14Y, 10%	1995.11: 2-20000¥				
Liaoning	1979.6: 14Y, 10%		1988.5: 14Y, 10%	1992.9: 5-50000¥	1997.9: 5-50000¥	2003.4: 1Y, 500%-1,00
Jilin	1988.7: 14Y, 10%-30% [a]	1993.10: 1Y, 5-30,000¥	2002.9: 1Y, 200%-500%			
Heilongjiang	1979.9: 14Y, 10%	1983.1: ¿1200¥	1989.12: 14Y, 10%	2003.1: 1Y, 100%		
Shanghai	1981.7: 16Y, 10%	1982.6: 3Y, 10%	1992.10: 1Y, 300%		1997.12: 1Y, 300%	2002.9: 1Y, 300%
Jiangsu	1982.6: 7Y-10Y, 10%	1990.10: 1Y, 300%	1995.6: 1Y, 300%	1997.7: 1Y, 300%	2002.12: 1Y, 400%	
Zhejiang	1982.3: 7Y, 5%	1985.2: 5Y, 15%	1989.12: 5Y, 20%-50%	2002.9: 1Y,200%-400%		
Anhui	1979.4: 14Y, 5%	1984.12: 7Y, 10%	1992.8: 7Y, 10%	2002.9: 1Y, 300%-400%		
Fujian	1982.5: 10-14Y, 5%	1988.4: 7Y, 20%	1991.6: 1Y, 300%	2000.1: 1Y, 200%-300%		
Jiangxi	1996.: 7Y, 50% ($\leq$ 30000¥)	2004.2: 1Y 350%				
Shandong	1980.3: 4Y, 20% [b]	1996.10: 6-20,000¥	2002.9: 1Y, 300%-400%			
Henan	1982.6: 7Y, 15%	1985.12: 7Y, 15%	1990.4: 7Y, 20-30%	2003.1: 1Y, 300%		
Hubei	1979.9: 14Y, 10%	1987.12: 5Y, 20%	1991.12: 5Y, 20-60%	1997.3: 5Y,20-60%		
Hunan	1979.6: 14Y, 5%	1982.5: 5Y, 10%	1989.12: 1Y, 150%	2003.1: 1Y, 200%-600%		
Guangdong	1980.2: 14Y, 10%	1986.5: 7Y, 20%	1992.11: 1Y, 210%-350%	1998.9: 1Y, 210%-350%	2002.7: 1Y, 300%-600%	
Guangxi	1994.11: 2-50,000¥	2002.9: 1Y, 300%-500%				
Hainan	1989.3: 7Y, 20%	1995.10: 1Y, 200%-300%				
Chongqing	1997.9: 1Y, 200%-300%	2000: 1Y, 200%-300%				
Sichuan	1984.5: 7Y, 5%	1987.7: 7Y, 10%-20%	1997.10: 7Y,20%-30%	2002.10: 1Y, 300%		
Guizhou	1987.7: 7Y-14Y, 10%	1998.7: 1Y, 200%-500%	2004.7: 1Y, 200%-300%			
Yunnan	1986: 10%	1990.12: 7Y, 30%-40%	1997.12: 7Y, 30%-40%	2002.9: 1Y, 500%-1,000%		
Tibet	1986.5: 1000¥(Han)	1992.5: 3000¥(Han); 500¥(Tib)				
Shaanxi	1981.4: 7Y, 10%		1986.7: 7Y, 10%	1991.3: 7Y, 15%-30%	1997.8: 7Y, 20%-30%	2004.8: 1Y,300%-600
Gansu	1982.3: 10Y, 10%	1985.6: 10Y, 10%	1989.11: 7Y, 30%	1997.9: 7Y, 30%	2002.9: 3.5Y, 20%	
Qinghai	1982.6: 7Y, 10%	1986.4: 7Y, 10%	1992.2: 7Y, 25%	2004.2: 1Y, 300%-500%		
Ningxia	1982.9: 14Y,10%	1986.8: 14Y, 10%	1990.12: 14Y, 10%-30%			
Xinjiang	1988.4: 7Y, 10%	1991.8: 14Y, 10%-30%				

Data source: Scharping (2013) and authors' collection.

[a]Note: the authors corrected one mistake from Scharping (2003).

[b]Note: the authors added one penalty data.

Figure A7: Provincial Fertility Penalties in Urban China



Data source: Ebenstein (2010).

A0.2 The One-tailed T Test for Sex Ratios

The publicly available data of sex ratios at the first birth in urban China is very limited: we have one year across provinces sex ratios from the 2000 Census. The small sample size leads to a lot of random variations in the sex ratios. We can not simply say provinces with sex ratios that are out of the normal range have severe sex selections.

To address this concern, we construct a one-tailed t-statistics to test whether the calculated sex ratio (with limited sample size) is statistically different from the biological sex ratio (1.06 boys/girls). We treat the gender of the first child, D_i as a Bernoulli trial with a probability $p = 0.515(106/206)$ to be a boy. Let's denote child i's gender as D_i. Formally,

$$D_i = \begin{cases} 1, & \text{if i is a boy.} \\ 0, & \text{otherwise.} \end{cases} \tag{A0.4}$$

Under biological sex ratio, we should observe a boy with a probability 0.515. Let the biological share of boys be $\mathscr{P}(D_i = 1) = p$, the calculated share of boys be $\hat{p}$ and sample size be n. The null hypothesis is $H_0 : p = 0.515$. As D_i is a Bernoulli trial,

$$\hat{p} = \frac{\sum_{i=1}^{n} D_i}{n}. \tag{A0.5}$$

Then we can get $E(D_i) = p$ and $Var(D_i) = p(1-p)$. By the central limit theorem,

$$\sqrt{n}(\hat{p} - p) \equiv \frac{\sqrt{n}\sum_{i=1}^{n} D_i}{n} - p \xrightarrow{d} \mathbf{N}(0, p(1-p)) \tag{A0.6}$$

As a result,

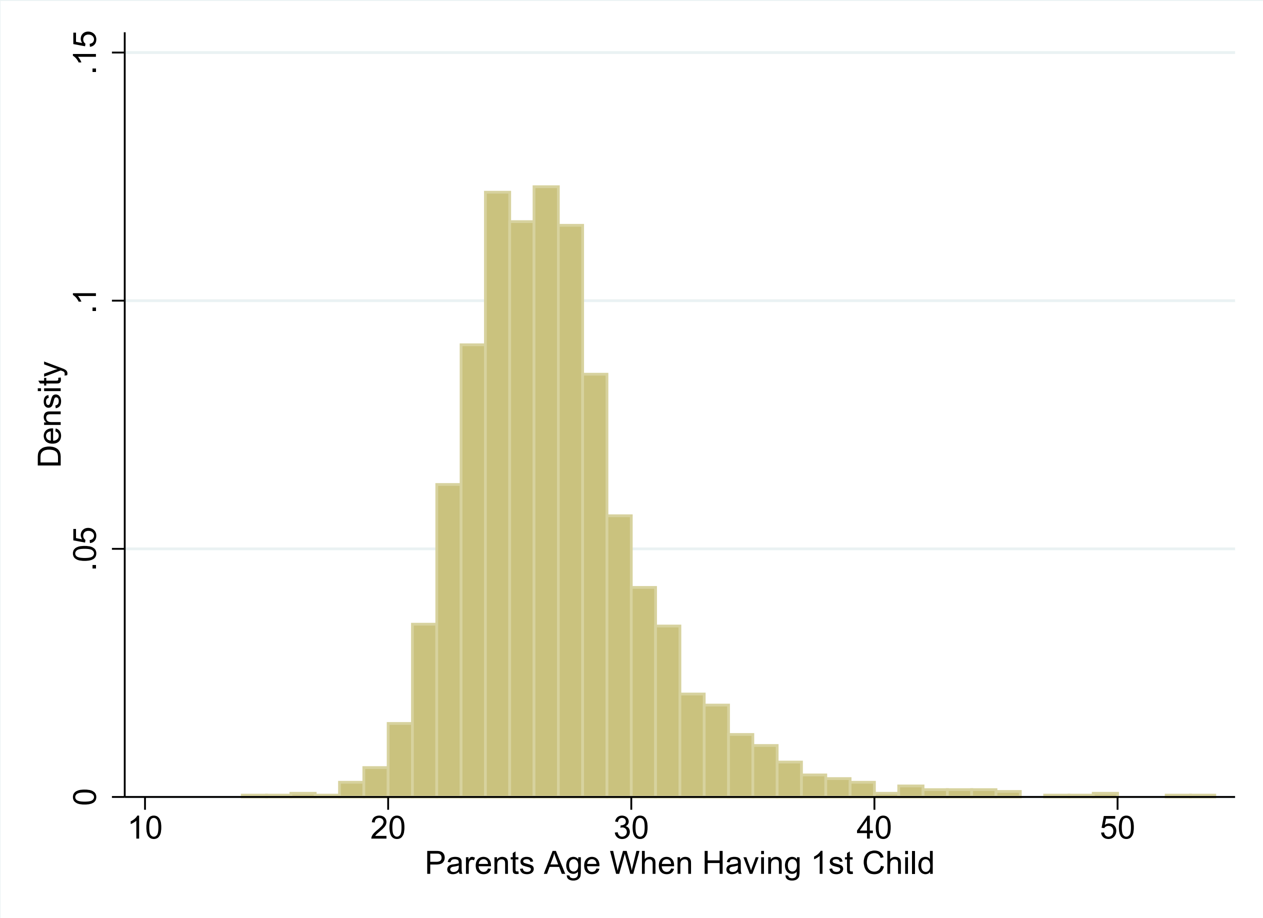
$$\frac{\sqrt{n}(\hat{p} - p)}{\sqrt{p(1-p)}} \xrightarrow{d} \mathbf{N}(0,1) \tag{A0.7}$$

So we can construct the following t-statistics:

$$t \equiv \frac{\sqrt{n}(\hat{p} - 0.515)}{\sqrt{\hat{p}(1-\hat{p})}}. \tag{A0.8}$$

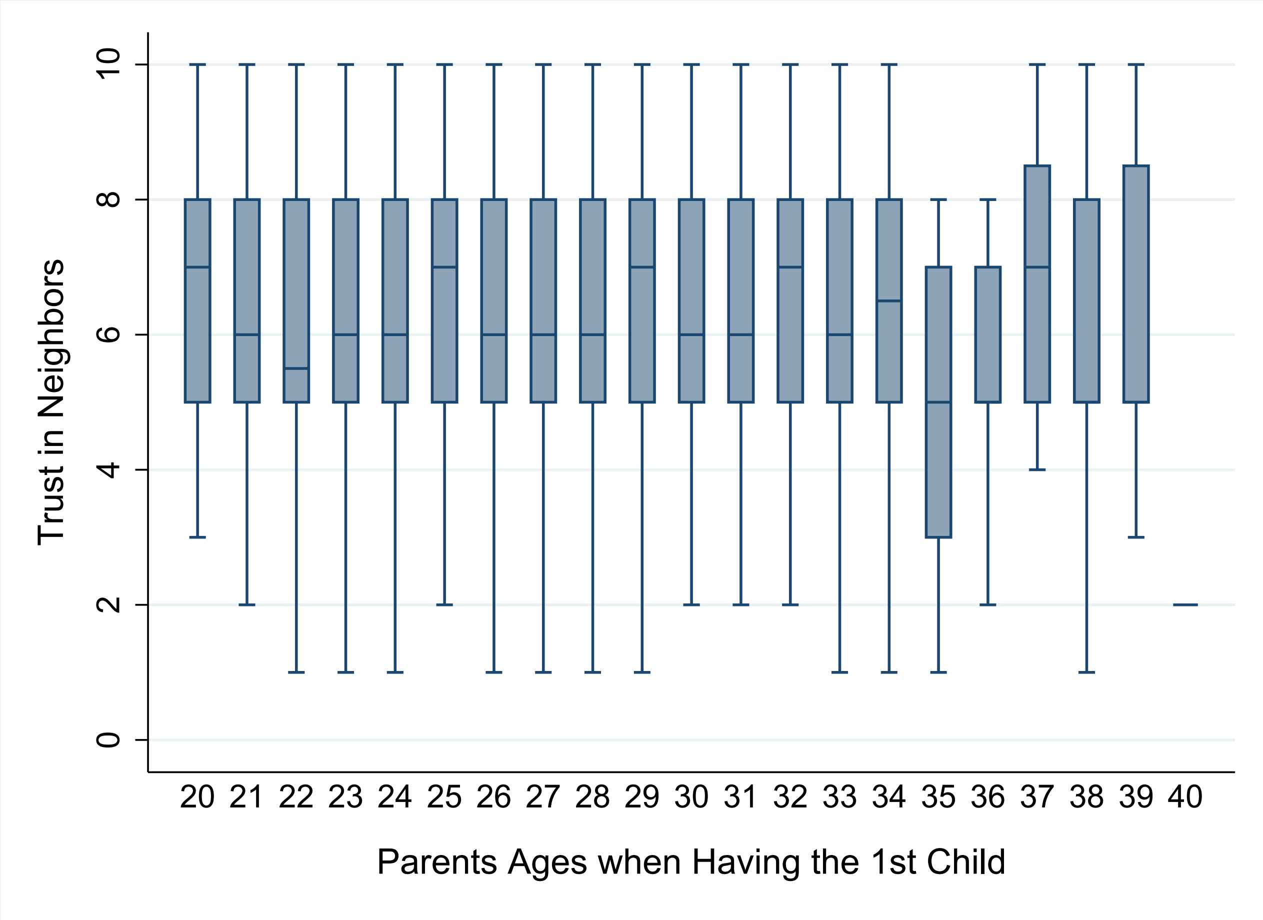
Since we are only concerned with the possibility that people endogenously choose boys over girls, we focus on a one-tailed t test and reject the null hypothesis if $t > 1.645$. We find that six provinces' sex ratios at first birth are significantly higher than the normal sex ratio at 5% level: Beijing, Jiangsu, Jiangxi, Hubei, Guangdong, and Guangxi (Table 5).

Figure A8: Distribution of Ages When Having the 1st Child



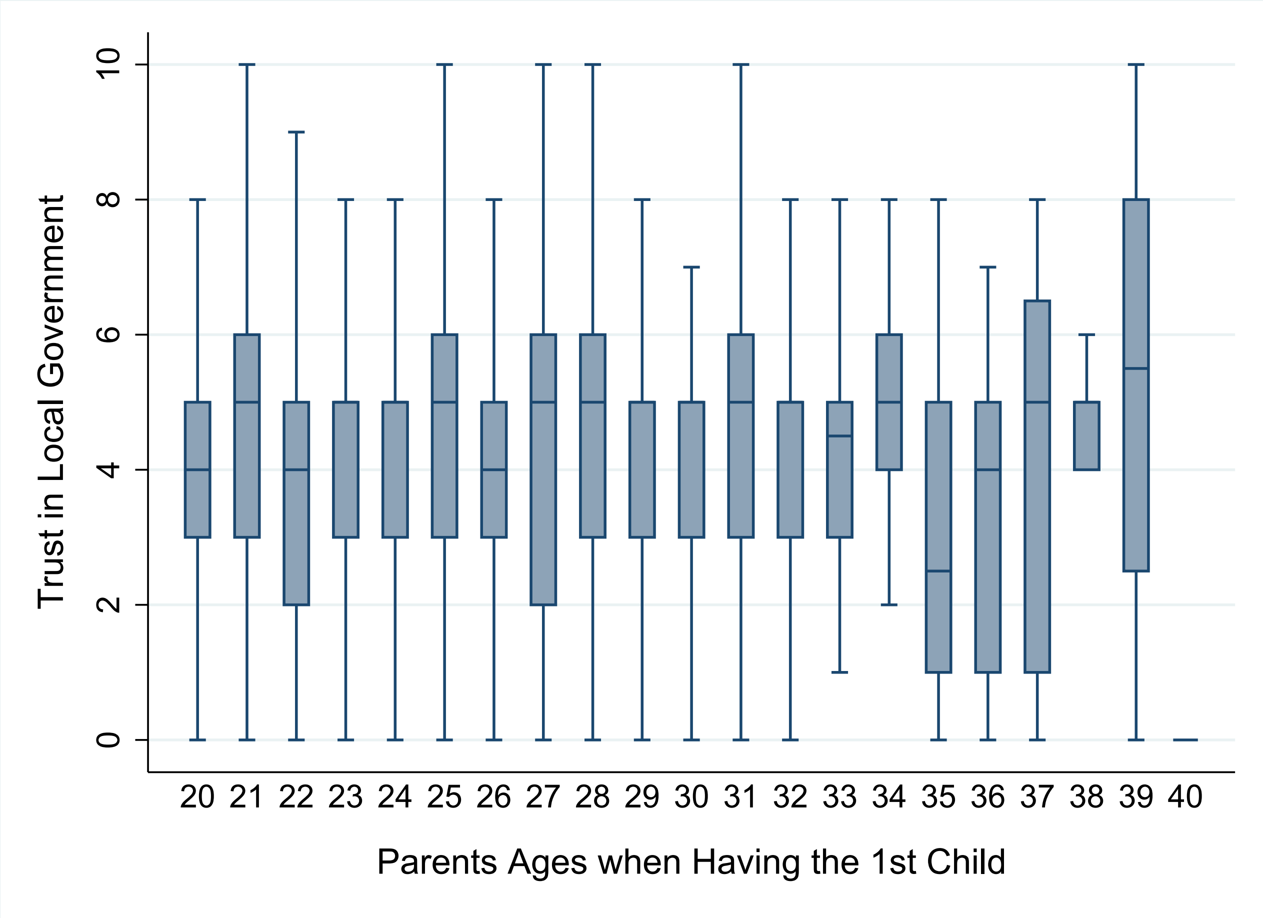
Data source: Authors' calculation based on China Family Panel Survey.

Figure A9: Trust in Neighbors and Parents by Ages When Having the 1st Child



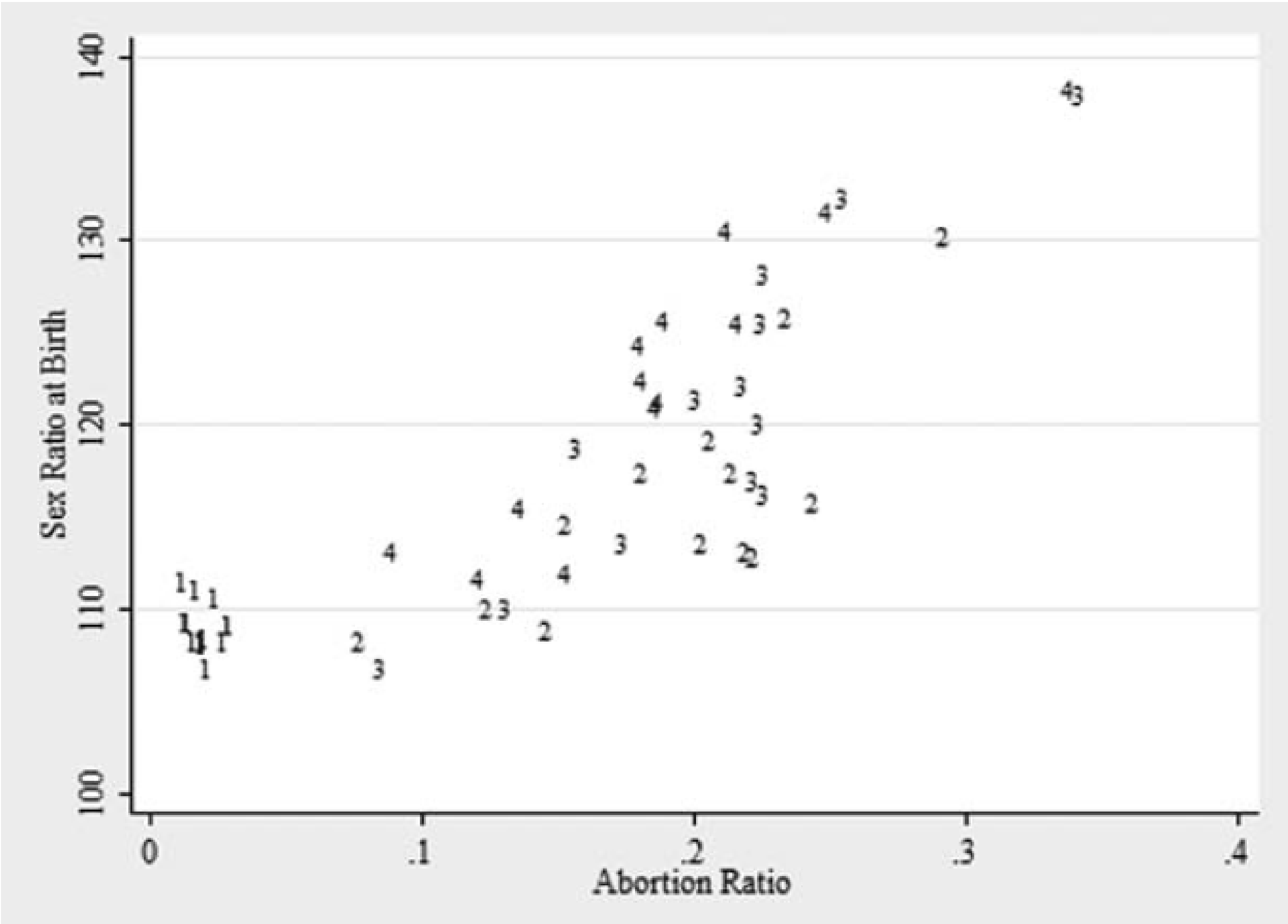
Data source: Authors' calculation based on China Family Panel Survey.

Figure A10: Trust in Local Government and Parents by Ages When Having the 1st Child



Data source: Authors' calculation based on China Family Panel Survey.

Figure A11: Sex Ratio at Birth and Abortion Ratio by Pregnancy Year and Pregnancy Order



Source: Chen, Li and Meng (2013), figure 2.
Note: Sex ratio at birth is defined as the number of male births per 100 female births. Abortion ratio is defined as the proportion of pregnancies ending in abortion. The data are aggregated to pregnancy year (1978-90) by pregnancy order cells. 1, 2, and 3 denote the first, second, and third pregnancies; 4 indicates fourth and above.

Table A2: Fertility Patterns in China by Sex of Existing Children

		Percent who have another child			Male Fraction of next birth		
Birth Order	Sex Combination	1982	1990	2000	1982	1990	2000
	Overall				0.516	0.520	0.533
First	None				0.511	0.510	0.515
Second	One boy	0.71	0.54	0.35	0.51	0.50	0.50
	One girl	0.75	0.60	0.49	0.52	0.55	0.62
Third	Two boys	0.53	0.30	0.18	0.50	0.43	0.39
	One girl, one boy	0.54	0.29	0.16	0.52	0.52	0.53
	Two girls	0.68	0.55	0.46	0.54	0.61	0.70
Fourth	Three boys	0.40	0.24	0.17	0.48	0.40	0.37
	One girl, two boys	0.36	0.17	0.11	0.51	0.49	0.52
	Two girls, one boy	0.44	0.23	0.14	0.52	0.55	0.58
	Three girls	0.62	0.54	0.50	0.56	0.64	0.72

Source: Ebenstein (2010), Table 1.
Note: Data is from China Census 1% sample (1982), 1% sample (1990), 10% sample (2000).

Table A3: Distribution of responses: trust in local gov.

	Trust in local government	
	1st child is a boy	1st child is a girl
0	9.87	9.73
1	6.65	5.42
2	8.86	9.85
3	10.57	12.94
4	5.94	5.53
5	29.81	31.97
6	9.77	8.41
7	7.25	5.75
8	7.05	6.42
9	2.01	2.32
10	2.22	1.66
Observations.	993	904
Mean DV	4.35	4.25
Std.Dev. DV	2.33	2.30

The table shows the distribution of responses to the question regarding trust in local government officials, split by the gender of the first child.

Table A4: Estimates of OCP Enforcement Levels on Trust in Phase II

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure (min)	0.077	0.068	-0.048	-0.058
	(0.078)	(0.074)	(0.063)	(0.066)
First-born daughter	-0.220***	-0.216***	-0.017	-0.010
$\times$OCP exposure (min)	(0.073)	(0.071)	(0.122)	(0.126)
Mean DV	6.407	6.407	4.304	4.304
Std.Dev.DV	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure is defined as minimum of the five year fertility penalty rates in province p after individual i has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table A5: Estimates of OCP Enforcement Levels on Trust in Phase I

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure (min)	0.078	0.072	0.165**	0.151**
	(0.068)	(0.069)	(0.059)	(0.064)
First-born daughter	-0.022	0.006	-0.227***	-0.212***
×OCP exposure (min)	(0.100)	(0.103)	(0.053)	(0.062)
Mean DV	6.582	6.582	4.957	4.957
Std.Dev.DV	2.072	2.072	2.502	2.502
Observations	822	822	822	822

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure is defined as minimum of the five year family-planning rate in province p after an individual has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table A6: Estimates of OCP Enforcement Levels on Trust in Phase II

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure (4-year mean)	0.072	0.060	-0.009	-0.022
	(0.062)	(0.059)	(0.080)	(0.087)
First-born daughter	-0.303***	-0.296***	-0.102	-0.100
×OCP exposure (4-year mean)	(0.087)	(0.087)	(0.162)	(0.170)
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.407	6.407	4.304	4.304
Std.Dev.DV	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.089	0.098	0.078	0.088

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure is defined as the four year mean value of the fertility penalty rates in province p after individual i has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table A7: Estimates of OCP Enforcement Levels on Trust in Phase II

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure (6-year mean)	0.036	0.023	-0.076	-0.088
	(0.069)	(0.066)	(0.074)	(0.076)
First-born daughter	-0.273***	-0.266***	-0.032	-0.033
×OCP exposure (6-year mean)	(0.082)	(0.083)	(0.136)	(0.141)
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.407	6.407	4.304	4.304
Std.Dev.DV	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.088	0.097	0.078	0.088

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure is defined as the six year mean value of the fertility penalty rates in province p after individual i has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table A8: Estimates of OCP Enforcement Levels on Trust in Phase I

Dependent variable:	Trust in			
	Neighbors		Local gov.	
	(1)	(2)	(3)	(4)
OCP exposure (4-year mean)	0.075	0.064	0.088	0.082
	(0.061)	(0.060)	(0.066)	(0.059)
First-born daughter	-0.065	-0.043	-0.250***	-0.248***
×OCP exposure (4-year mean)	(0.072)	(0.071)	(0.060)	(0.067)
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.582	6.582	4.957	4.957
Std.Dev.DV	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.141	0.155	0.139	0.155

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure is defined as the four-year average family-planning rate in province p after an urban resident has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). Number of clusters: 25.

Table A9: Estimates of OCP Enforcement Levels on Trust in Phase I

Dependent variable:	Trust in			
	Neighbors		Local government	
	(1)	(2)	(3)	(4)
OCP exposure (6-year mean)	0.144	0.127	0.152	0.138
	(0.111)	(0.108)	(0.117)	(0.104)
First-born daughter	-0.075	-0.037	-0.439***	-0.441***
$\times$OCP exposure (6-year mean)	(0.107)	(0.104)	(0.102)	(0.110)
Individual controls		Y		Y
Cohort FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean DV	6.582	6.582	4.957	4.957
Std.Dev.DV	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.143	0.157	0.140	0.157

*** p<0.01, ** p<0.05, * p<0.1. OCP exposure is defined as the six-year average family-planning rate in province p after an urban resident has his/her first child. All regressions include a full set of province and cohort fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with a small number of clusters (Cameron et al. 2008). We report the corresponding p-values in brackets. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table A10: Correlation between Fertility Penalty and Family-planning rate

Dependent variable:	Family-planning rate
Fertility penalty	1.588**
	(0.659)
Province FE	Y
Year FE	Y
Observations	142

*** p<0.01, ** p<0.05, * p<0.1 Standard errors in parentheses. The sample is a province-level panel (unbalanced) from 1979 to 1987, when both fertility penalty and family-planning rate are available.