Blame-shifting through Delegation: Evidence from China's One-Child Policy

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Abstract

It has long been suggested that when trying to implement an unpopular policy, the politicians should delegate the task to an external agency to shift the blame to them. Such blame-shifting practices are widely observed in the political sphere and have recently been shown to be effective in the lab. However, field evidence of the effect is still missing. In this study, using data from China's one-child policy (OCP), we investigate people's mistrust in the government when the unpopular task was delegated to the citizens and when it was conducted by the government itself. Our main empirical strategy exploits the exogeneity of the gender of a couple's first child. Due to the widely-shared "at least one son" preference, Chinese couples whose first child is a girl were more likely to violate the One Child Policy than their counterparts. We find that when the enforcement of the policy was delegated to the citizens, by encouraging and incentivizing them to report their neighbors' violations of the policy and join the local enforcement institutions, the blame is shifted from the government to its citizens. In particular, the results show that people who were exposed more severely to OCP exhibit significantly lower trust towards their neighbors today. However, the exposure to the policy does not significantly undermine their trust in the local governments. The results are reversed when the policy was solely implemented by the government officials in an earlier phase. We find that people who experienced this period's OCP blamed the local government but not their neighbors.

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1 Introduction

Authoritarian governments frequently mobilize the masses to implement unpopular policies. Civilians are encouraged and incentivized to denounce their neighbors, friends and co-workers. Grass-root organizations are established to 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, housewives, dentists, and other average citizens turned in their Jewish neighbors after petty neighborhood quarrels (Johnson, 2000). In a recent episode, the President in Philippine, Rodrigo Duterte, encouraged vigilantes among the general population to commit violence against suspected drug users in his brutal drug war.

Why is mass mobilization so popular among the (authoritarian) governments despite their threats to civil societies? One rationale this paper argues is that mass mobilization can help the government shift the blame from itself to its citizens. Starting from Machiavelli, several political science scholars proposed that leaders should delegate the enactment of unpopular measures to others, thus shifting the blame to others. This idea was not empirically tested until recently a small but growing experimental literature showed that delegation can shift the blame from the delegator to the delegatee in the lab (Coffman, 2011; Hamman, Loewenstein and Weber, 2010; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). They find that a principal was punished if he made an unpopular choice, but the punishment was much weaker if the same choice was made by a delegatee whose interest is aligned with the principal. Nevertheless, a test of the theory in the field is still missing. In this work, we fill this gap with evidence from China's One Child Policy (OCP).

Under China's OCP, most urban couples were limited to a single child. Starting from 1991, the government decided to mobilize the masses to help implement the policy. The authorities created monetary and non-monetary incentives for people to report their neighbors' violation of the policy to government officials. Grass-root enforcement organizations were established and civilians were appointed to enforce the policy in their neighborhoods. The strength of the 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 OCP exposure in urban areas from 1991 to 2010: the average fertility penalty rate one faced within five years after the arrival of his/her first child. Here we use the fertility penalty rate to measure the exposure to OCP because

it not only proxies the strictness of OCP implementation as suggested by the previous literature (Ebenstein, 2010; Huang, Lei and Sun, 2015), but also affects the incentives the government is able to provide for the citizens. We count the penalty rates after the birth of the first child because citizens are exposed to OCP restrictions only after the first birth. The 5-year interval is selected because most people's second child arrives within 5 years after the first child, according to the China Census. Our individual measure of OCP exposure generates substantial within-province variations in addition to the between-province variations in the previous literature. For example, as we can see in Table 2, Anhui province has a larger normalized standard deviation (standard deviation/mean) than the overall sample.

One central hypothesis is that people who were exposed more severely to OCP confronted more intense conflicts with their neighbors and became less trusting in them. However, their trust in local governments is less clear. On one hand, as the local government can decide the strength of the enforcement and its officials are heavily involved in the enforcement process, citizens may have reasons to be wary about their governments. On the other hand, however, the trust may not be affected by the implementation of the policy if blame-shifting strategies are effective enough.

The primary outcomes of interest are the citizens' trust in their neighbors and local governments, which are measured by China Family Panel Study 2016 (CFPS-2016) survey. Our main empirical strategy 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 (2011b) to vary the parental propensity to become entrepreneurs. Given the deep-rooted belief that one family needs *at least one son* to maintain the lineage, 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. Thus they are more exposed to OCP punishments and denunciations than their counterparts. One concern is that the gender of the first child is not exogenous due to the pervasive sex selection practices (e.g. selective abortion) in China. However, multiple sources of evidence suggested that sex selection rarely happens with the first child (Ebenstein, 2010; Li, Yi and Zhang, 2011; Wei and Zhang, 2011b; Li and Wu, 2011). We also conduct a direct test and find that the gender of the first child is not significantly impacted by any control variables in the empirical analysis. Another part of our empirical strategy exploits parents' lack of incentives to report their children's violation of the policy. Hence, while we expect the OCP to undermine trust among neighbors, it likely has no effect on trust toward their own parents.

We find strong support for our hypotheses. Regarding trust towards neighbors, people who

experienced more severe enforcement of OCP exhibit significantly lower trust in their neighbors. To validate this result, we compare parents with a female first child and parents with a male first child. Our results show that for couples whose first child is a girl, OCP exposure significantly lowers trust in neighbors. In comparison, for couples whose first child is a boy, no significant effect of OCP exposure is found. The difference between the two groups of people is also significant. Quite surprisingly, we do not find a significant impact of OCP exposure on people's trust towards the local governments. In addition, there is no significant difference between the two groups of people. This finding is particularly striking because people blame their neighbors who were incentivized or forced to enforce the policy, but they do not blame the government who initiated the harm. This suggests that blame-shifting through delegation is strongly effective.

A natural concern, given our identification strategy, is the possibility that the gender of the first child is not perfectly exogenous, even though rare sex selections still occurred with the first child. We address this concern by re-running our regressions excluding six provinces with high sex ratios at the first birth. Our core results remain unchanged. Another concern is that our results are affected by an unobservable that is correlated with both OCP enforcement and people's general trust level. To address such concerns, we re-run our main regressions with people's trust in their own parents as the dependent variable. In contrast to the results of people's trust in neighbors, no significant impact on trust in parents is detected.

Our results suggest that when the task is delegated, people blame their neighbors but not the local government. However, it is unclear whether the blame avoidance enjoyed by the local government is caused by the blame received by the neighbors. In other words, 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 OCP enable us to investigate this question. The policy came into effect in 1979, but mass mobilization was not started until 1991. Thus parents whose first child was born between the born of the policy and 1991 were exposed to OCP but were not hurt by their neighbors. However, we do not have a direct measure of OCP enforcement in this period. The fertility penalty, which is our main measure in the 1991-2010 period, was largely unrecorded in the 1980s. To deal with this, we construct a higher-order (2nd order or beyond) birth cohort size measure based on 1990 census data to proxy for the enforcement severity. The results indicate that in this period with no delegation, people's trust in neighbors are not affected by their exposure to OCP. More importantly, we find suggestive evidence that stronger implementation of the policy leads to lower trust in the local government: the negative effect of OCP exposure on

trust in the local government is significantly stronger for people whose first child is a girl. This period serves as an analogous control treatment as in an experimental study. It also suggests the fact that we do not find a significant decrease in political trust in the 1991-2010 period is not driven by people's fear to report distrust in the government.

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 of government. While the current literature rarely separates the possible mechanisms about how conflicts were affecting social trust, we provide a clear mechanism of how mass mobilization affects people's interpersonal and institutional trust.

We contribute to the blame-shifting literature by providing, to the best of our knowledge, the first field evidence of the theory. By confirming the effectiveness of blame-shifting, this paper also provides a rationale for mass mobilization. By encouraging people to fight against each other, the governments can avoid the blame for implementing an unpopular policy, a cost they must bear if they need to do all the jobs by themselves. This may explain why mass mobilization is so popular in (authoritarian) regimes despite that it may hurt the society in the long run.

Lastly, this study adds to the literature studying the impacts of OCP in two ways. The consequences of OCP range from economic growth (Li and Zhang, 2007) to sex ratio imbalance (Ebenstein, 2010; Li, Yi and Zhang, 2011) to female education (Huang, Lei and Sun, 2015) to promotions of mayors (Serrato, Wang and Zhang, 2016) to competitive saving motive (Wei and Zhang, 2011a). While current literature mainly focuses on the effect of the policy *per se*, this project pays more attention to the effect of the enforcement of the policy, a hidden cost of the policy. More importantly, the construction of an individual-level OCP exposure enables us to exploit within-province variations instead of between-province variations. This feature alleviates the endogeneity problem of the OCP exposure measure. The problem with a provincial level measure of OCP enforcement is that it suffers from reverse causality problem. We may observe that tighter OCP enforcement leads to lower trust in neighbors. However, a plausible explanation is that governments tend to implement OCP more strictly in regions where trust in neighbors is low. By adding individual-level variation to the OCP enforcement measure, this problem is potentially

solved as people in the same province can have quite different exposures to OCP.

Our paper proceeds as follows. Section 2 describes the historical background, institutional setup, and important features of OCP. Section 3 describes 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 robustness and 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). Historically, Chinese parents have 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, especially within the Han ethnicity. OCP was introduced in 1979 and began to be formally phased out in 2015.

OCP directly and compulsorily assigned limited birth quotas to couples. These quotas were strictly implemented by the Population and Family Planning Commissions at every level of government. OCP in principle restricts a couple to have only one child. However, the regulations vary among different regions and ethnicities. First, provincial governments localized the state fertility policy due to the diversity of demographic and socioeconomic conditions across China. As the main instrument for enforcing OCP, financial penalties also varied across provinces. Second, there is a Han-minority differentiation. Due to political reasons, OCP was strictly enforced on the Han ethnicity, while most Minorities were less restricted by it (Gu, Wang, Guo and Zhang, 2007).

A variety of birth control methods have been used to enforce OCP. Sterilization and insertion of an intrauterine device after the first or second birth were implemented on a large scale. Between 1979 and 1999, the percentage of women of reproductive age who underwent sterilization rose from 21% to 35% (Scharping, 2013). Meanwhile, induced abortions of unauthorized pregnancies had been used as a "remedial measure making up for contraceptive failures". For above-quota births, financial sanctions were the main instrument for enforcing OCP. As documented by Scharping (2013), assaults on local-planning cadres were frequently provoked by coercive abortions, sterilizations, and the administration of penalties in the 1980s and 1990s. Tight state control of the urban economy made enforcement of these measures relatively easy. In an urban area, there are other punishments widely used, such as denial of bonus payments or health and welfare ben-

efits for specified time periods, no job promotions, or demotions by one, two or even three wage grades for people employed in urban units. Self-employed businessmen were threatened with the revocation of business licenses, and private entrepreneurs leasing state or collective enterprises might face cancellation of their terms of the lease.

Economic reforms which accelerated the process of decentralization during the late 1980s and early 1990s also led to a weakening of administrative control of family planning offices at different levels. From 1990 census, the central government surprisingly 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 requested stricter enforcement of OCP. In May 1991, the chief officers at diverse administrative levels were made personally responsible for meeting family planning targets. If targets were not achieved, the Chief Official would neither be promoted nor honored, and might even lose his or her job. This was the so-called 'one-vote-down' (yi piao fou jue) campaign. Second, in December 1991, the Ministry of Personnel and the Organization Department of the Central Committee issued a joint circular that urged turning investigations of birth-control performance into an integral part of the regular cadre evaluations[1]. As a rule, the First Party Secretary of an administrative unit is forced to assume personal responsibility for coordination and all work related to birth control. Local authorities increased the grants for the program and gave it stronger political support. Implementation of the program was also strengthened. The campaigns resulted in a huge jump of penalties for one extra child and a substantial decline in fertility.

2.1 Mobilizing the Masses

In response to the "excess" births found in the 1990 census, another campaign which is more relevant to our theme was started. In 1990, the central government decided to mobilize the masses to implement OCP. 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

[1]The Cadre Evaluation System is one of the most important components of government personnel management in China. The system evaluates local party and government cadres based on performance criteria determined by their immediate superior level of government. Cadres take the CES very seriously, given that the evaluation results constitute one of the most influential factors affecting decisions about their career appointment, promotion, transfer, and removal (Wang, 2013).

beyond the birth-quota, and let the masses watch each other." in an internal speech on April 21st, 1990. The rationales for this campaign were twofold. First, the local cadres lack the necessary information about who is pregnant and whether it is 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 hide from them. The government thus relied on people who were close to the pregnant women to provide the information. Second, there are not enough local officials to implement the policy. Implementing OCP is not only about sterilizations and forced abortions. The cadres also needed to launch propaganda campaigns to promote the idea of one child, carried out the so-called "Three Examinations," which check women for use of contraceptive rings, pregnancy, and illness four times (or more) each year and so on. The crew was required to be expanded in a short time with a limited budget, which made asking the citizens to be "volunteers" to be highly attractive.

Two measures were taken to mobilize the masses. First, to deal with the information asymmetry, the citizens are encouraged and incentivized to report unauthorized pregnancies/births of their neighbors, coworkers and relatives to the authority. The incentives provided by the government included monetary rewards, career rewards and collective punishments. The amount of cash rewards varied across regions and was linked to the fertility punishment. For example, in Chongqing Province in 2009 the informer could get 5% of the fertility penalty paid by the victims, which was about one month's salary of them. [2] In addition, the informer's identity was kept confidential and there was no record of punishment for a false report. The career rewards for denunciation was salient when there were competitions between colleagues. Public sector employees who were caught violating 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. Lastly, collective punishments for violations of OCP were not a rare practice. When a violation was detected, someone who was aware of it but did not report to the authority could be punished, both in monetary term and career term. During the years when OCP was most fiercely implemented, one worker's violation of OCP could lead all her/his coworkers to lose a significant part of their income in some state-owned enterprises. [3]

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

[3] http://wap.sciencenet.cn/blogview.aspx?id=749707

Second, to deal with the ever-increasing workload, more at-will employees and "volunteers" were recruited by the government. Most of the detailed work had been carried out by them, not the cadres. They were generally seen by the people as neighbors.

Before discussing the recruitment and duties of those workers, some facts about the local management in urban China are needed in order to place them in context. The family-planning commission is the institute in charge of the enforcement of OCP. Its lowest level located at 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 the administrative hierarchy in urban China. According to the Chinese constitution, it enjoys a high degree of autonomy and is named the "self-government organizations of the masses". They are allowed to recruit at-will employees and pay them independently. Although the residents' committees are formally the lowest organizational entities, there are in fact entities even 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 only and do not have a legal status on their own. The head of them is called "cluster leader" who was usually appointed by residents' committee, but is not on the government payroll.

In the 1990s, enabled by the rocketed revenue collected from fertility penalty, The residents' committees recruited many at-will full-time and part-time OCP enforcers. For example, Huangjiapu, a five hundred population residents' committee in Shanxi Province, had fifteen full-timers 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 those OCP enforcers still failed to solve the labor shortage. Consequently, the "cluster leaders" were mobilized to enforce the policy. They were tasked with keeping track of households' reproductive habits and reporting these details to the local family-planning commission. They were surely seen as neighbors. In addition to the cluster leaders, state organizations such as the military, public school and hospital have their own internal family-planning units, as do state-owned enterprises.

These neighborhood-level staffs and cluster leaders are 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 have half a million full-time employees, there were about 1.2 million neighborhood-level birth-planing staff and more than 6 million cluster leaders (Fong, 2016).

3 Data

While we employ data from a variety of sources for this paper, many of the key variables are measured by the China Family Panel Study (CFPS). CFPS, with 36,000 completed adult observations, is a large-scale, almost nationally representative panel survey project conducted by the Institute of Social Science Survey at Peking University. The 25 provinces of China covered by CFPS represent about 95% of the Chinese population in mainland China, with only Inner Mongolia, Xinjiang, Tibet, Hainan, Ningxia, and Qinghai excluded from the overall sample. We introduce our measurement of different types of trust and enforcement level of 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, governments and parents, which are measured by CFPS-2016 survey. These questions that were asked in the survey are translated as follows: Please rate to what degree do you trust neighbors. Similar questions were also asked in terms of trust in parents and local government. One may concern that the categorical trust measures are not valid. However, Johnson and Mislin (2012) provides experimental evidence that trust, as measured by the World Values Survey, is positively correlated with experimentally measured trust. The questions asked in CFPS are very similar to those in the World Values Survey. From the summary statistics (table 1), we can see that people generally have the highest trust in parents and the lowest trust in local government.

While there is always a concern that respondents will be afraid to reveal politically incorrect beliefs, there are several reasons to take the responses in the survey at face value. An important concern is that respondents may be afraid to illustrate their true opinion regarding trust in government and even try to provide "correct" answers. One can see suggestive evidence in the distribution of the responses that the survey questions successfully elicited respondents' opinions. A first indication that respondents are likely not attempting to provide "correct" answers is the broad range of answers to this question. Regardless of the gender of the first child, we found responses in the full range, from 0 to 10, and in every case the modal response was provided by less than 32 percent of respondents. Another indication that there was not a clearly correct answer is that modal responses were not located at an end of the distribution.

3.2 The Individual-level OCP Exposure

Our key explanatory variable is an individual-level measure of OCP exposure: the average fertility penalty an urban resident faces 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 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 OCP enforcement. Ebenstein (2010) have found that the strictness of OCP enforcement is well reflected by the monetary penalties that have been implemented since 1979. As displayed in Figure 1, Fertility penalty varies across provinces and across time. At the provincial level, as documented by Scharping (2013), there are three forms of fertility fines. The first form of the fine is a percentage deduction in wage over several years. 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. The second type of fines is levied as a lump sum payment based on the 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 the household income. For example, from 1995 to 2000, Guangxi ratified the fine as an amount between 2,000 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, even though in general fines tend to increase over time, the timing of changes are quite different among provinces. Our measure of OCP exposure incorporates these two kinds of variations.

We improve the measure of OCP enforcement in the literature in two ways. First, we modify the penalty data used in the literature for 7 out of 25 provinces. As mentioned before, the fertility fine has three forms. For the first two, we follow Ebenstein (2010)'s calculation rules. We mainly improve the third form, which is a certain amount of money without taking the household income into consideration. 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 experiences a rapid and unbalanced growth during the time when OCP was fiercely implemented. For instance, the annual household incomes are 4,630 yuan in Liaoning in 1993, 7,095 yuan in Beijing in 1993 and 20,833 in Beijing in 2000. To account for the variation in household income, we replace the fixed 10,000 RMB by the actual average household income in a certain

province in a certain year, using data taken from China Statistical Yearbooks. More details of our calculation are provided in the Data Appendix.

Second, we construct an individual level measure of OCP instead of provincial level. In our measure, only penalty rates implemented after parents having their 1st child counts. The reason is people are exposed to OCP restrictions only after they have the first birth, but not before that. Census data show that the interval between the birth of a couple's first and second child is no more than 5 years (Scharping, 2013). Therefore, the strongest impact of the birth control policy falls on young couples during the 5 years following the birth of their first child. That is why we only count the rates within 5 years. However, as will be shown later in the next section, changing the 5 years to 4 or 6 years will not alter our results qualitatively. This measure exploits the individual variations in the timing of the first child's birth. People give their first birth at different times. One obvious factor is the birth cohort: older people have their first child earlier than younger people. But even for two people of the same age, the chance that they gave their first birth at the same time is small. A lot of factors have impacts on the timing of having a child. One may worry about the possibility that parents rationally choose the timing of the first birth to enjoy a looser OCP enforcement. But this is really unlikely. First, the provincial level OCP enforcement is mainly decided by the provincial government and is highly unpredictable for people without special connections to the government. Second, to choose a looser OCP enforcement, one needs to plan both the timing of having the first child and the timing of the second and foresee the enforcement for several years. Even if this can be done, it might be too costly or too complicated.

Within-Province Variation in OCP Exposure Using individual-level data, we can also show that there is significant variation in OCP exposure within provinces. Table 2 presents the estimated mean, normalized variance (standard deviation divided by the mean), and the minimum and maximum values of this OCP exposure proxy for China as a whole and for each province. These descriptive statistics show that for China as a whole, OCP exposure is approximately 3 years of household income. However, there is enormous spatial variation. For individuals with the most severe exposure of OCP, the penalties can be up to 10 years of household income, while the minimum level of OCP exposure is only 0.66 years of household income. The province-specific estimates show that there is both substantial cross-province and within-province variation in OCP exposure. The variation across provinces can be observed by comparing the OCP exposure in Liaoning Province, which is nearly 6.5 years of household income, to that of Heilongjiang, where OCP exposure is approximately one year of household income. The variation within provinces

can be observed from the large standard deviations of within-province OCP exposure shown in column (2). We also show the within-province variance of OCP exposure from 1991 to 2002 for each province and for China as a whole using box-and-whisker plots (Figure 4).

3.3 Sample Selection

For our empirical estimation, we limit our sample to individuals who completed both CFPS-2010 and CFPS-2016 survey. We further limit our sample based on two criteria: (i) individuals resided in the urban sector at the time of CFPS-2012; and (ii) individuals who gave birth to the first child in (or after) 1991 and before 2010.

We restrict our sample to urban households because fertility penalty is not a good measure of OCP exposure in rural areas. Since rural areas are further away from administrative centers, the monitoring cost of whether local cadres strictly enforce the policy is much higher in rural areas than that in urban areas. Furthermore, it's also difficult for local cadres to collect information about every rural household's annual income, on which the fertility penalty is calculated based. In practice, the penalty was often the same among households in one area for the sake of lacking information. Besides, some poor families cannot afford the massive amount of penalty, in which case the local cadres could only collect as much as the low-income family can afford. Hence, there's a lot of random variation in the amounts of penalty collected by local cadres[4].

For criteria (ii), we restrict our sample to individuals who gave birth to the first child in (or after) 1991 as mass mobilization was not started until that year. And we look at people who gave birth to the first child before 2010 because OCP was formally phased out in 2015. People who gave birth to the first child after 2010 are affected little by OCP.

[4]We cannot use birth rate to measure OCP enforcement degree for rural sample either. Because of the so-called "one vote down" campaign, birth rate played a crucial role in local governor's promotion. Regional governors had strong incentive to misreport birth rate. In addition, there's little cost of the falsification as the upper-level government also benefited from the misreported birth rates.

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 one 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. Thus they are more exposed to OCP punishments and denunciations than their counterparts. To validate our empirical strategy, we firstly 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, in fact, there are few sex selections on the first-born children. 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 children using Chinese Children Survey data. Figure 5 demonstrates that for first pregnancies, the sex ratio at birth (males/females) is close to being natural [5]. In some years, it was close to the average natural rate 1.06 and was never higher than 1.14. Moreover, the abortion ratio (number of abortions/number of children born) is smaller than 0.05%. Both the abortion ratio and the sex ratio at first birth remained stable over

[5] According to a study around 2002 (cite), the natural sex ratio at birth was estimated to be close to 1.06 males/female. We shall also point out that a range of natural and environmental factor may influence the natural sex ratio. For example, in an extensive study conducted by U.S. Department of Health and Human Services around 2005, statistical evidence suggested the following. The age of the mother affected the ratio: the overall ratio was 1.05 for mothers aged 25 to 35 at the time of birth; while mothers who were below the age of 15 or above 40 had babies with a sex ratio ranging between 0.94 and 1.11. 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.

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- and higher-order pregnancies.

Our statement that the gender of the first child is exogenous is confirmed and complemented by the national wide 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 3 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 males/females 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 4 reports the results using both a linear probability model and a probit model. People don't endogenously choose the gender of the first child based on fertility penalties of previous years. In addition, 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 children preference, chances and the constraints. A typical preference can be a couple prefer 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 as OCP in China. Here we define the so-called "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. In fact, we allow a couple with "son preference" to prefer one boy and a girl to two boys. However, she 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 willing to stop at one child. Even though trying to have more children than one leads to OCP penalties, the potential benefits of having a son outweighs the cost. 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 was the "state religion" in ancient China and is still strongly influencing the Chinese people in the modern time. 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 3 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. This "at least one son" preference 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 jumped 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.

Our main identification strategy aims at making sure that the correlation we find is causal. However, even if we find that more exposure to OCP leads to lower trust in neighbors, it is still unclear whether this effect works through the mechanism we proposed: delegation through mass mobilization. To deal with this concern, we propose a second identification strategy exploiting parents' lack of incentives to report an unauthorized birth. Even though the incentives provided by the government may be enough to induce the neighbors to report, it is highly likely that they are not enough for the parents. One obvious reason is parents care a lot more about their children than the neighbors. Another important reason is their grandchildren are also their own heirs in addition to their children's heirs. Reporting to the authority puts themselves in danger of losing their heirs. Therefore, if the mechanism of the negative impact of OCP exposure on trust in neighbors is mass mobilization, then we should expect that OCP exposure has no impact on trust in parents.

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, trust in parents, or trust in local government. Our key independent variable of interest is $1stChildpenalty_{icp}^{1-5}$, which for now we define as the

five-year mean value of the penalty rates in province p after individual i has his/her first child.

$$
\begin{aligned}
y_{icp} = \sum_c \alpha_c + \sum_p \delta_p + \beta_0 1stChildpenalty_{icp}^{1-5} + \mathbf{X}'_{icp}\gamma + \beta_1 1stChildGirl_{icp} \\
\sum_c \alpha_{1c} \times 1stChildGirl_{icp} + \sum_p \delta_{1p} \times 1stChildGirl_{icp} + \mathbf{X}'_{icp}\gamma \times 1stChildGirl_{icp} \\
+\beta_2 1stChildpenalty_{icp}^{1-5} \times 1stChildGirl_{icp} + \varepsilon_{icp}
\end{aligned} \tag{1}
$$

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 birth cohort fixed effects, our specification also absorbs all variations across age groups that might induce different trust towards neighbors in spite of same policy outcomes. 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:

$$
\begin{aligned}
y_{icp} = \sum_c \alpha_c + \sum_p \delta_p + \beta_0 1stChildpenalty_{icp}^{1-5} + \beta_1 1stChildGirl_{icp} \\
+\beta_2 1stChildpenalty_{icp}^{1-5} \times 1stChildGirl_{icp} + \mathbf{X}'_{icp}\gamma + \varepsilon_{icp}
\end{aligned} \tag{2}
$$

where the variables are similar to those explained earlier. This alternative specification requires that the effects of all control variables such as individual's age and gender on her trust are identical regardless of the gender of the first child. This requirement may not be realistic. 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 household types. 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 a lower efficiency. Given the relatively large sample size, we can afford to sacrifice some efficiency in exchange for an improved chance to obtain unbiased estimates of the key parameters.

5 Results

5.1 The Effect of OCP exposure between 1991 to 2010 on Trust

In table 5, we present the regression estimates from equation 1 on the three trust outcomes: trust in neighbors, trust in parents, and trust in local government. In columns 1, 4 and 7, we present the baseline estimates using a parsimonious specification that include only province fixed effects. We add individual-level controls as our preferred specification from equation 1, including birth-cohort fixed effects, education attainment and family income per capita. The third specification adds the year fixed effects when people having their first child to equation 1, controlling for some possible exogenous shocks in that year that may affect people's belief. Our results are consistent across different specifications. In the remaining discussion we focus on our preferred specification.

In terms of trust in neighbors (Colume 1), OCP enforcement significantly lowers trust in neighbors for couples whose first child is a girl. In comparison, for couples whose first child is a boy, OCP enforcement is not statistically significant. The estimates for the difference between the two groups of people (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, one standard deviation increase in OCP exposure is associated with 0.028 standard deviation more decrease of citizens' trust in neighbors for people whose first child is a girl.

A concern is that the negative association between OCP exposure and trust in neighbors are affected by an unobservable that is correlated with both One Child Policy enforcement degree and people's trust level. To address such concerns, we re-run our main regression with people's trust in parents as dependent variables. The estimation results are presented in Columns 4-6. Here, in contrast to the results of people's trust in neighbors, regardless of the first child gender, the coefficient on OCP enforcement is small in magnitude and statistically insignificant. Reassuringly, there's no significant difference between the two group of people. The coefficient of interest (β_2)

is small and statistically insignificant, consistent with our theory, indicating both that One Child Policy exposure is not a significant driver of people's trust in other people rather than neighbors and that One Child Policy exposure is not serving as a proxy for an unobserved variable that drives people's general trust. This indicates that for an unobservable factor to be driving our results it must not only be geographically and temporally personalized, but also very limited in scope affecting only people's trust in neighbors.

We next proceed to examine whether 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 explicitly measure citizens' trust in the local government. In fact, evidence from a separate survey conducted among elite college students in China demonstrates that there is a high correlation between reported trust in central and local government (Cantoni, Chen, Yang, Yuchtman and Zhang, 2017). Hence, one can cautiously extrapolate citizens' trust in the central government from their reported trust in the local government. Columns 7-9 of Table 5 present the corresponding estimation results. One can clearly see that there's no significant difference in the impact of OCP exposure on trust in local government between people whose first child is a boy and people whose first child is a daughter. The estimate of coefficient β_2 is small and statistically insignificant[6]. 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.

One potential flaw 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 boy, some couples who are actually 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 6 lists the sex ratios at first birth across provinces in 2000 in urban areas. There are six provinces where sex ratios at first birth are above 110 (boys per 100 girls): Beijing, Jilin, Jiangxi, Hubei, Guangdong, Guangxi. To further

[6]One may noticed that there's a negative correlation between OCP exposure and trust in local government for people whose first child is a boy, which is significant at the 5 percent level. This negative correlation can be driven by some omitted variable that is correlated with OCP exposure, but has no differential impact between people whose first child is a girl and people whose first child is a boy.

validate our results, we drop observations from the six provinces and replicates the specifications in Table 5. Estimation results are reported in Table 7 . One can see that dropping the sample with sex ratios at first-birth above 110 does not affect our findings.

5.2 Robustness Checks

Although we have argued that it is unlikely for parents endogenously choosing the timing of the first birth to enjoy a looser OCP enforcement in section 3.2, we still perform another robustness check to ensure that our results hold when we assume parents can rationally choose the birth timing. Specifically, we consider an alternative measure of OCP exposure: the minimum penalty in the five (or four/six) years after people having their first child. We reestimate our baseline estimating equation with the alternative measure of OCP exposure. The results are unaffected (Table 12 in appendix).

We next examine the robustness of the OCP exposure measure. Recall that we used the five-year average fertility penalties after people having their first child. Our results are quite consistent when we use four-year (table 13) or six-year (table 14) average fertility penalties.

5.3 The Effect of OCP exposure in 1980s on Trust

In the previous section, we showed that when OCP implementation was delegated, more severe enforcement significantly undermined people's trust in neighbors but not trust 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 OCP enable us to investigate this situation. The policy came into effect in 1979 but the mass was not mobilized until 1991, thus parents whose first child was born between the born of the policy and 1991 were exposed to 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. But 11 out of 31 provinces hadn't established their fertility penalty policies in 1984. Some provinces even introduced their first fertility penalties in 1988. An alternative measure for OCP exposure is birth-rate data. The idea is when OCP was enforced more strictly, fewer children should be born. The main drawback of using birth-rate to proxy for OCP severity is the under-reporting of children (Scharping, 2013; Goodkind, 2004). The rationales for this under-reporting were twofold. First, parents have strong incentives to hide unauthorized children to

avoid punishment. Second, the local governments also have strong incentives to under-report in order to achieve the birth-targets assigned by the upper-level governments.

To minimize the possibility that our results are interrupted by misreporting, we use the higher-order birth (birth order strictly higher than 1) cohort sizes observed in 1990 1% sample census data to proxy for OCP severity at the provincial level (Cai, 2013; Shi and Kennedy, 2017; Meng, Qian and Yared, 2015). For example, children who were 10 years old in 1990 were supposed to be born in 1980 and the number of them can give us a clue about the enforcement of OCP in that year. We only use higher-order birth to capture the enforcement because OCP only restricted the birth beyond the first one.

There are two limitations with this approach. First, birth cohort sizes can be affected by many variables outside of OCP. Some of them can be included in our analysis, like the number of women at fertility age, but there are also other exogenous shocks such as the end of Cultural Revolution that we are not able to measure. Second, in order to link observed birth cohort size to birth-rate, we still need to assume that the time trend of the infant and early childhood mortality rates are the same across provinces after controlling for time and province fixed effect.

Sample selection: We limit our sample to individuals who gave birth to the first child in (or after) 1979 and before 1985.

The reason we choose to look at people who gave birth to the first child after 1979 is it is the year OCP started. We only look at people who gave birth to the first child before 1985 because 1985 is five years away from 1991, which is the year the government initiated the mass mobilization campaign. People whose first child came later than 1986 might also confront their neighbors when they tried to have a second child.

Consistent with our individual-level measure of OCP exposure, we use the log of average higher order birth cohort sizes 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 log of average higher-order birth cohort size in province p from year $t+1$ to year $t+5$. We also control for the log of average sizes of women at fertility age in province p from year $t+1$ to year $t+5$. In order to be consistent with the fertility penalties in a sense that the higher the value of the variable, the more OCP exposure, we flip the sign of the measure. To further validate the cohort size measure, we cross-check the correlation between fertility penalties and higher-order birth cohort size, which is significant with or without province fixed effects and year fixed effects.

We can see from Table 8 that when the policy was solely implemented by the government during the 1980s, the results are quite opposite: citizens blame their government, not neighbors. For people whose first child is a girl, 10% increase in higher order birth cohort size leads to 0.24 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 difference in the effect of OCP exposure on trust in neighbors between the two groups of people. Besides, we observe a positive association between cohort sizes and trust in local government for those whose first child is a boy (i.e., a negative association between OCP severity and trust in local government). One possible explanation is reverse causality - OCP can be enforced more severely in areas with higher trust in their government.

To further validate the cohort sizes measure, we re-run our main regression with higher-order birth cohort sizes as the measure of OCP severity after 1991. Here, we use 2005 mini census data to construct higher-order birth cohort sizes. [7] The estimation results are presented in Table 15. We obtain similar results for both trust in neighbor and trust in government.

6 Discussions

We showed when the enforcement of OCP, an extremely unpopular policy, was delegated to the civilians, people's trust in their neighbors are significantly reduced but not their trust in local government; while when the government enforced OCP by its officials, people's trust in it is undermined. The results are consistent with the blame-shifting effect of mass mobilization. In this section, we address whether we can attribute the results to mass mobilization. In what follows, we discuss three alternative interpretations of our results. People blame the local government but are unwilling/dare not to report their mistrust, people don't blame the local government but they blame the central government, or the effects we identify are induced by the cultural revolution or the great famine. math.

[7] 1990 census is not employed here because one may worry that misreporting is still a big issue as most children born after 1991 were still under school age at 2000. 2010 census is another alternative, but it is not publicly available yet.

Dare Not To Report Mistrust

An important concern regarding we don't find any significant impact on trust in local government in the period 1991-2010 is that people try to express politically correct views. To address this possibility, we present the distributions of responses to the question on trust in local government (table 11). From the broad range of answers to the question, an indication is that respondents are likely not attempting to provide "correct" responses. Regardless of the gender of the first child, we found responses in the full range, from 0 to 10, and in every case the modal response was provided by less than 32 percent of respondents. Another indication is that modal responses were not located at an end of the distribution. Importantly, the mean value of trust in local government is smaller than the mean value of trust in neighbors in our sample.

In addition, we find a significant negative impact on trust in local government in the 1980s, which further suggest 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 respondents dare not to report their mistrust in local government.

Trust in Central Government

A second concern about our estimates is that people don't blame the local government 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. Due to the political sensitivity of eliciting trust in the central government in mainland China, we are only able to explicitly measure citizens' trust in the local government. Above we noted that one can cautiously extrapolate citizens' trust in the central government from their reported trust in the local government. Still one might wonder how big the correlation is between trust in local government and trust in central government. As an additional check, we construct three 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 people may lose their party membership due to violations of OCP, which can also explain a negative correlation between OCP enforcement and party membership rate.

The second proxy is the difference between the number of days accessing political news through TV and the number of days accessing political news through 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. In terms of 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 even gain access to websites that are otherwise blocked by the firewall via proxy servers. So if one doesn't trust the central government, she is more motivated to access the political news from the less censored media source - 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.

We can see from table 9 across all columns, we don't find any significant difference in the impact of 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, they do not blame the central government either.

Cultural Revolution and Great Famine

In the 20th century in China, except for OCP,there are two other influential events that may potentially lowered interpersonal trust and political trust: the cultural revolution and the great famine. Especially in the cultural revolution, the government also mobilized the masses by encouraging citizens to report "class enemies" and supporting grass-root organizations like the "Red Guards". If the enforcement of OCP is correlated with that of the cultural revolution or the severeness of the great famine, then potentially our results can also be explained by the the two events. However, this is unlikely for two reasons. First, our main identification strategy exploits the gender of the first child as a proxy for the attempt to have a second child and find significant differences between the two groups of people. At the same time, we would not expect that the cultural revolution or the great famine have differential effects on people with a first born son and people with a first born girl. Second, if the three events are somehow related, it is more likely that the correlation is on the provincial level. However, in our analysis, we controlled the provincial fixed effect. Our individual measure of OCP exposure mainly uses the variation of OCP enforcement over time and the timing of the arrival of the first child. As the two events all ended before the implementation of OCP, the variation of OCP across time should be orthogonal to them.

7 Conclusion

We are witnessing an epidemic of mistrust in governments and elites - Brexit, Trump's victory, Marine LePen's popularity, etc. However, mistrust is not a big issue in China. According to Edelman Trust Barometer,[8] Chinese average trust in institutions ranks 1st across the world in 2016. It thus raises the question that what determines the heterogeneous trust levels across countries. We are addressing this issue in one particular context - OCP - hoping to gain traction in understanding the wider issue.

This paper studies the causal effect of mass mobilization on interpersonal trust and political trust using evidence from China's One Child Policy. Using survey data from China Family Panel Study (2010, 2012, 2016), we construct an individual level measure of exposure to One Child Policy. In line with our hypothesis, the results show that people exposed more severely to One Child Policy exhibit significantly lower trust towards neighbors, but their trust in parents and strangers are not affected. Our main empirical strategy exploits the exogenous gender of a couple's first child. As people whose first child is a girl are more likely to violate the OCP than their counterparts, we postulate that their trust in neighbors are more affected by the OCP. Our hypothesis is confirmed by our empirical analysis.

However, surprisingly, the exposure to OCP did not significantly lower people's trust in local governments. We propose a blame-shifting theory based explanation of this finding and show that when the enforcement was not delegated in an earlier phase, people's trust in local government is significantly reduced by OCP. Our results suggest that one rationale for mass mobilization is that it can help the government avoid blame when it tries to implement an unpopular policy.

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[8] http://www.edelman.com/global-results/

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

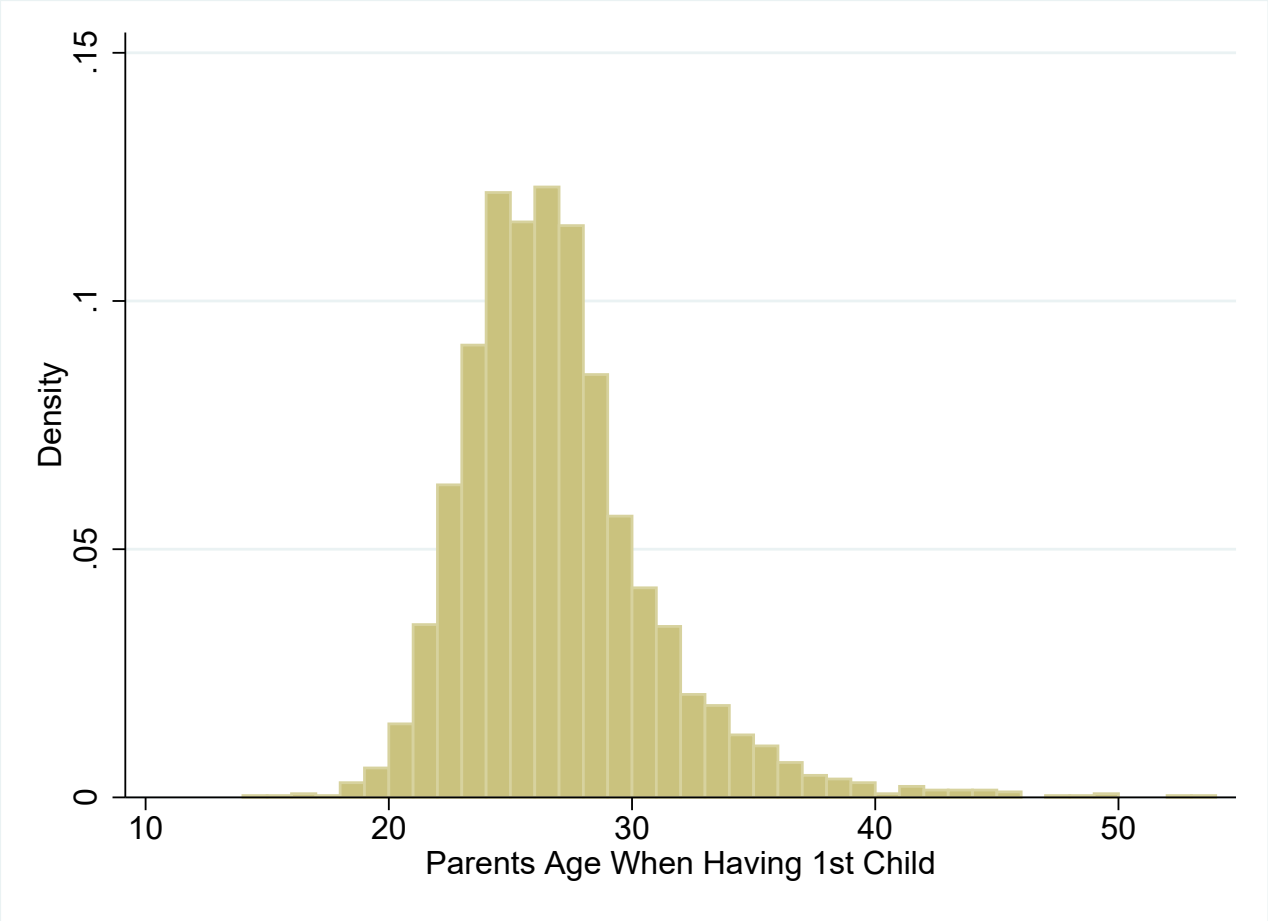


Figure 2: Trust in Neighbors and Parents by Ages When Having the 1st Child

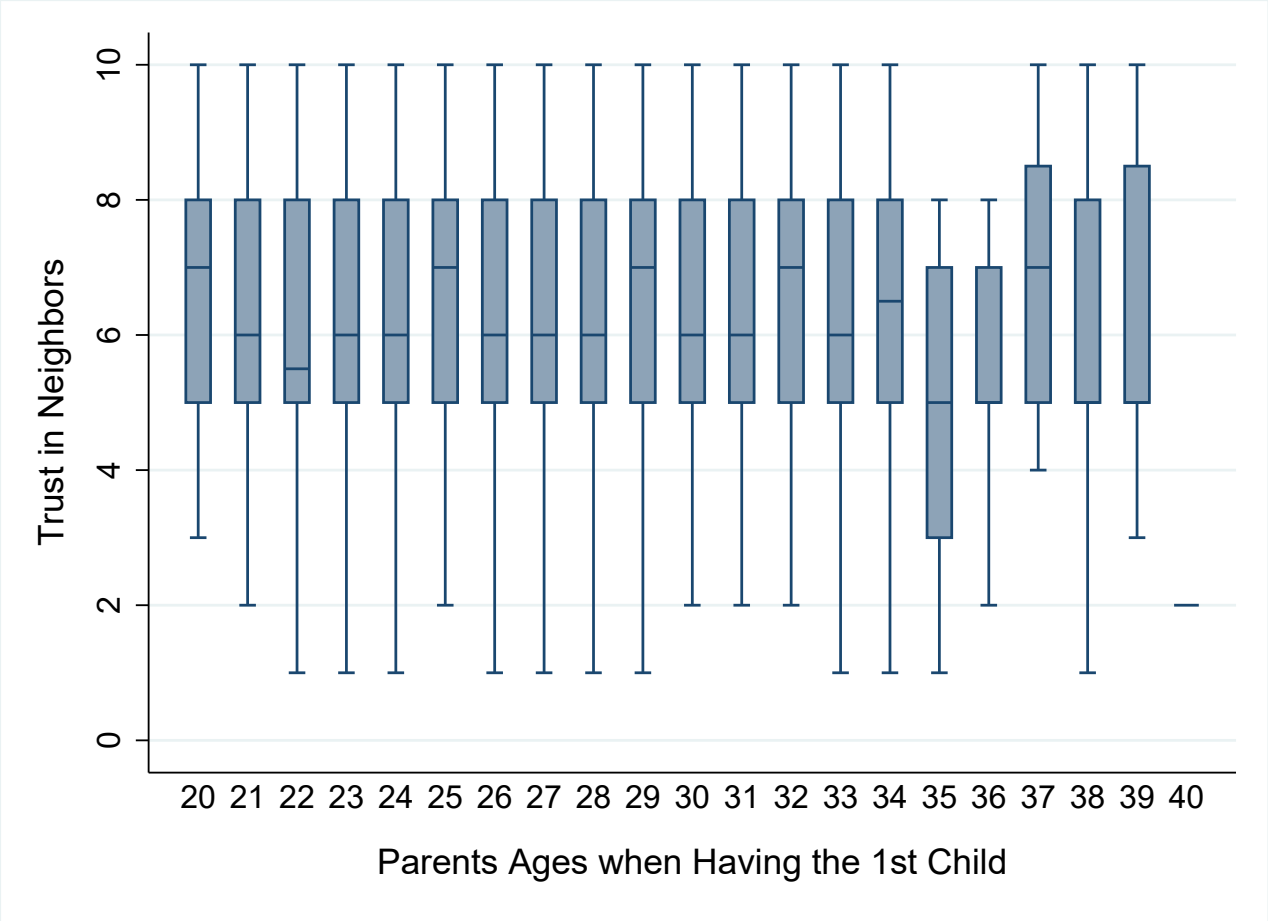


Figure 3: Trust in Local Government and Parents by Ages When Having the 1st Child

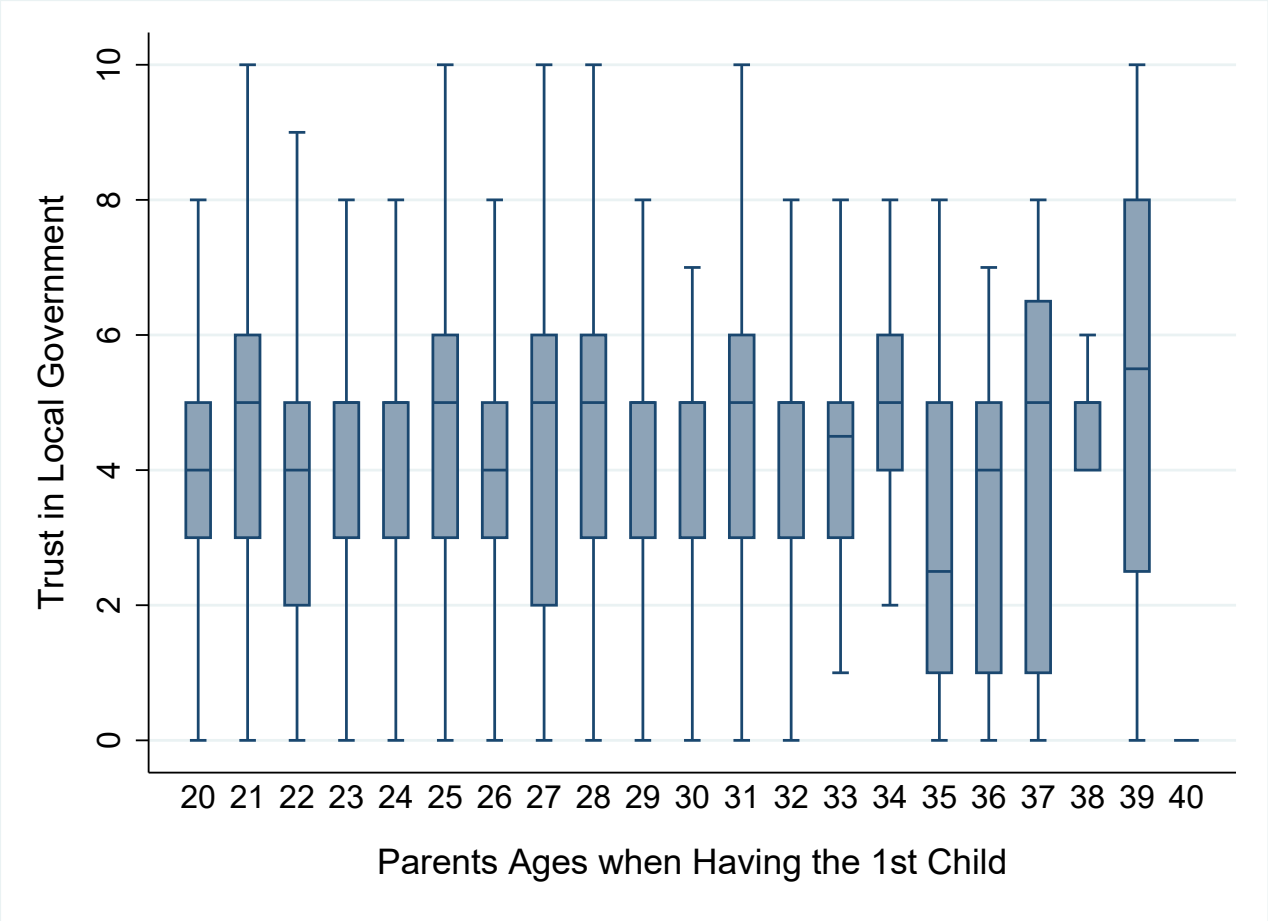


Table 1: Summary Statistics

Variable	Obs	Mean	Sd
1991-2010			
Trust in parents[a]	1897	9.623	1.155
Trust in neighbors[a]	1897	6.407	2.065
Trust in local government[a]	1897	4.304	2.47
$1stChildpenalty_{ip}^{1-5}$	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 parents[a]	1722	9.35	1.476
Trust in neighbors[a]	822	6.582	2.072
Trust in local government[a]	1722	4.237	2.336
Higher order birth cohort size	822	4183.956	3044.752
Female at fertility age	822	78331.96	39372.25
Age in 2016	822	59.78	3.683
Family income per capita	822	27708.83	34790.87
Education attainment	822	2.94	1.168

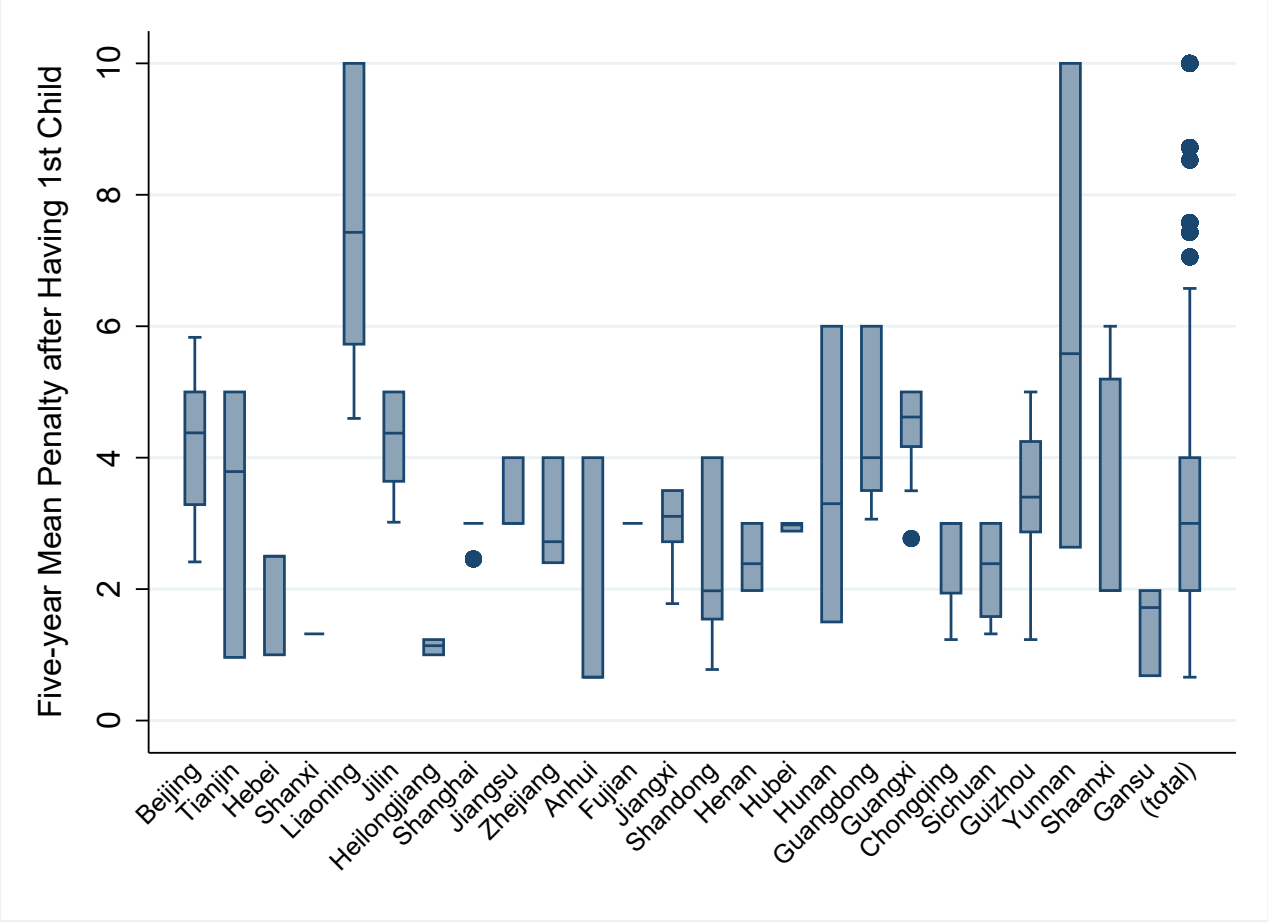
a: catagarical variables: 0 = extremely low trust;10 = extremely high trust

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

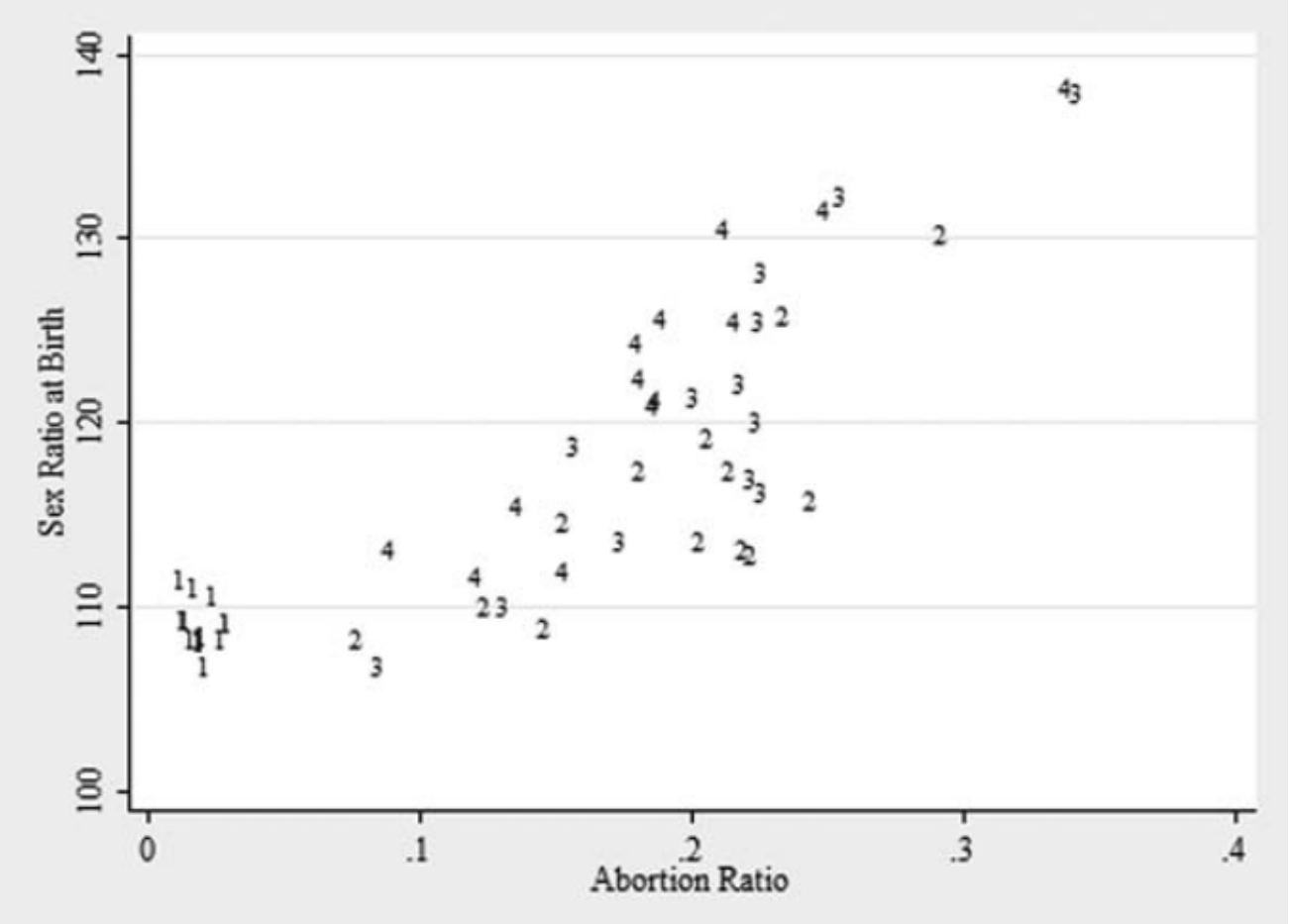
Note: The statistics refer to individuals who had their first child between 1991 and 2002 within each province.

Figure 4: Box-and-whisker Plots of Within-Province Variation of OCP Exposure



Note: We plot the within-province variation for each province and for China as a whole. 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 at 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 5: Sex Ratio at Birth and Abortion Ratio by Pregnancy Year and Pregnancy Order



Source: Chen et al. (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 1st, 2nd and 3rd pregnancies; 4 indicates 4th and above.

Table 3: 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
1st	None				0.511	0.510	0.515
2nd	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
3rd	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
4th	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: using data from China Census 1% sample (1982), 1% sample (1990), 10% sample (2000).

Table 4: Regression of Gender of the First Child on Control Variables

Dependent variable	Having a first-born daughter		
	Probit	Probit	Linear prob.
	(1)	(2)	(3)
$1stChildpenalty_{ip}^{-5-1}$	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

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Estimates of OCP Enforcement Levels on Trust, 1991-2010

Dependent variable:	Trust in neighbors			Trust in parents			Trust in local gov.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OCP exposure	-0.017	0.022	0.028	-0.028	-0.060	-0.020	-0.117**	-0.084	-0.194**
	(0.049)	(0.057)	(0.064)	(0.056)	(0.063)	(0.060)	(0.053)	(0.084)	(0.077)
OCP exposure	-0.198***	-0.241***	-0.206**	-0.010	-0.002	-0.049	0.010	-0.021	0.140
$\times 1stChild : girl$	(0.062)	(0.079)	(0.080)	(0.078)	(0.087)	(0.095)	(0.123)	(0.167)	(0.195)
P-Value	[0.004]	[0.006]	[0.016]	[0.899]	[0.982]	[0.609]	[0.934]	[0.899]	[0.479]
Bootstrap P-Value	[0.004]	[0.004]	[0.012]	[0.872]	[0.944]	[0.644]	[0.960]	[0.916]	[0.716]
1st Child Birth Year FE			Y			Y			Y
Individual controls		Y	Y		Y	Y		Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	6.407	6.407	6.407	9.623	9.623	9.623	4.304	4.304	4.304
Std.Dev.DV	2.065	2.065	2.065	1.155	1.155	1.155	2.470	2.470	2.470
Observations	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897
R-squared	0.051	0.072	0.089	0.041	0.071	0.094	0.042	0.062	0.081

All regressions include a full set of province and cohort fixed effects (not reported). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with small number of clusters (Cameron et al. 2008). We report the corresponding p-values. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 6: Sex ratio at birth (males/females) for first pregnancies in Urban China in 2000

Region	Sex Ratio at birth (males/females) of the first Child
Beijing	**1.130**
Tianjin	1.063
Hebei	1.090
Shanxi	1.081
Liaoning	1.061
Jilin	**1.104**
Heilongjiang	1.079
Shanghai	1.089
Jiangsu	1.099
Zhejiang	1.072
Anhui	1.082
Fujian	1.091
Jiangxi	**1.178**
Shandong	1.080
Henan	1.076
Hubei	**1.124**
Hunan	1.052
Guangdong	**1.166**
Guangxi	**1.131**
Chongqing	1.022
Sichuan	1.098
Guizhou	0.988
Yunnan	1.004
Shaanxi	1.060
Gansu	1.058

Data source: China Census 10% sample(2000)

Table 7: Estimates of OCP Enforcement Levels on Trust, 1991-2010-Without High Sex Ratio Provinces

Dependent variable:	Trust in neighbors			Trust in parents			Trust in local gov.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OCP exposure	-0.028	0.037	0.014	-0.023	-0.038	-0.020	-0.090	-0.067	-0.156**
	(0.058)	(0.065)	(0.074)	(0.071)	(0.076)	(0.071)	(0.061)	(0.093)	(0.068)
OCP exposure	-0.220***	-0.287***	-0.224**	0.001	-0.005	-0.029	-0.034	-0.066	0.073
$\times 1stChild : girl$	(0.071)	(0.082)	(0.081)	(0.093)	(0.103)	(0.102)	(0.131)	(0.171)	(0.179)
P-Value	[0.006]	[0.002]	[0.013]	[0.988]	[0.963]	[0.782]	[0.799]	[0.704]	[0.690]
Bootstrap P-Value	[0.004]	[0.004]	[0.004]	[0.944]	[0.944]	[0.848]	[0.968]	[0.92]	[0.528]
1st Child Birth Year FE			Y			Y			Y
Individual controls		Y	Y		Y	Y		Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	6.409	6.409	6.409	9.631	9.631	9.631	4.218	4.218	4.218
Std.Dev. DV	2.059	2.059	2.059	1.180	1.180	1.180	2.500	2.500	2.500
Observations	1,472	1,472	1,472	1,472	1,472	1,472	1,472	1,472	1,472
R-squared	0.051	0.081	0.104	0.030	0.067	0.095	0.038	0.066	0.093

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with small number of clusters (Cameron et al. 2008). We report the corresponding p-values. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 8: Estimates of OCP Enforcement Levels on Trust, 1979-1985

Dependent variable:	Trust in neighbors			Trust in parents			Trust in local gov.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$ln(higherorderbirthcohortsize_{ip}^{1-5})$	0.934	1.235	2.352*	0.139	0.227	0.812	4.017*	4.288**	3.402
	(1.756)	(1.398)	(1.319)	(0.490)	(0.552)	(0.750)	(1.973)	(1.883)	(2.642)
$ln(higherorderbirthcohortsize_{ip}^{1-5})$	-2.795	-3.440	-4.014	-1.027	-0.812	-1.297	-5.850*	-6.673**	-6.443*
$\times 1stChild : girl$	(2.579)	(2.311)	(2.437)	(0.632)	(0.738)	(0.971)	(2.851)	(2.687)	(3.732)
P-Value	[0.290]	[0.150]	[0.113]	[0.118]	[0.282]	[0.195]	[0.052]	[0.021]	[0.098]
Bootstrap P-Value	0.400	0.240	0.208	0.068	0.224	0.148	0.180	0.124	0.208
1st Child Birth Year FE			Y			Y			Y
Individual controls		Y	Y		Y	Y		Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	6.582	6.582	6.582	9.495	9.495	9.495	4.957	4.957	4.957
Std.Dev.DV	2.072	2.072	2.072	1.251	1.251	1.251	2.502	2.502	2.502
Observations	822	822	822	822	822	822	822	822	822
R-squared	0.084	0.123	0.135	0.094	0.119	0.145	0.080	0.107	0.128

All regressions include a full set of province and cohort fixed effects (not reported). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with small number of clusters (Cameron et al. 2008). We report the corresponding p-values. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 9: Estimates of OCP Enforcement Levels on Trust in Central Government

Dependent variable:	Partymembership	Media source for political news
	(1)	(2)
ocp exposure	0.004	-0.367***
	(0.009)	(0.112)
ocp exposure	0.007	0.147
$\times 1stChild : girl$	(0.017)	(0.158)
P-Value	[0.683]	[0.363]
Mean DV	0.112	-1.506
Std.Dev.DV	0.316	3.440
Observations	1,897	1,386
R-squared	0.195	0.122

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. We also report the p-values for OLS with clustered data. Number of clusters: 19.

9 Appendix A Data Appendix

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 (statististics, 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 10) [9]. We modified the penalty data for 7 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[10].

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

[9]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.

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

data is calculated as having a present value of 1.21 years of income. The detailed calculation is

$$Penalty^{1980}_{Guangdong} = 0.1 + 0.1 \times (1-0.02) + 0.1 \times (1-0.02)^2 + ... + 0.1 \times (1-0.02)^{13} = 1.21 \quad (3)$$

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 assumptions. First, the fine is collected at the maximum amount of the range[11]; second, the average household annual income is fixed at 10,000 RMB across province and time[12]. 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 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 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., 5 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

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

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

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 5 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 of policy publishing month into consideration: the current year's penalty applies if the new policy is implemented before the end of June; otherwise, previous year's penalty applies.

Table 10: Monetary Penalties for Excess Fertility, Urban China 1979-2010

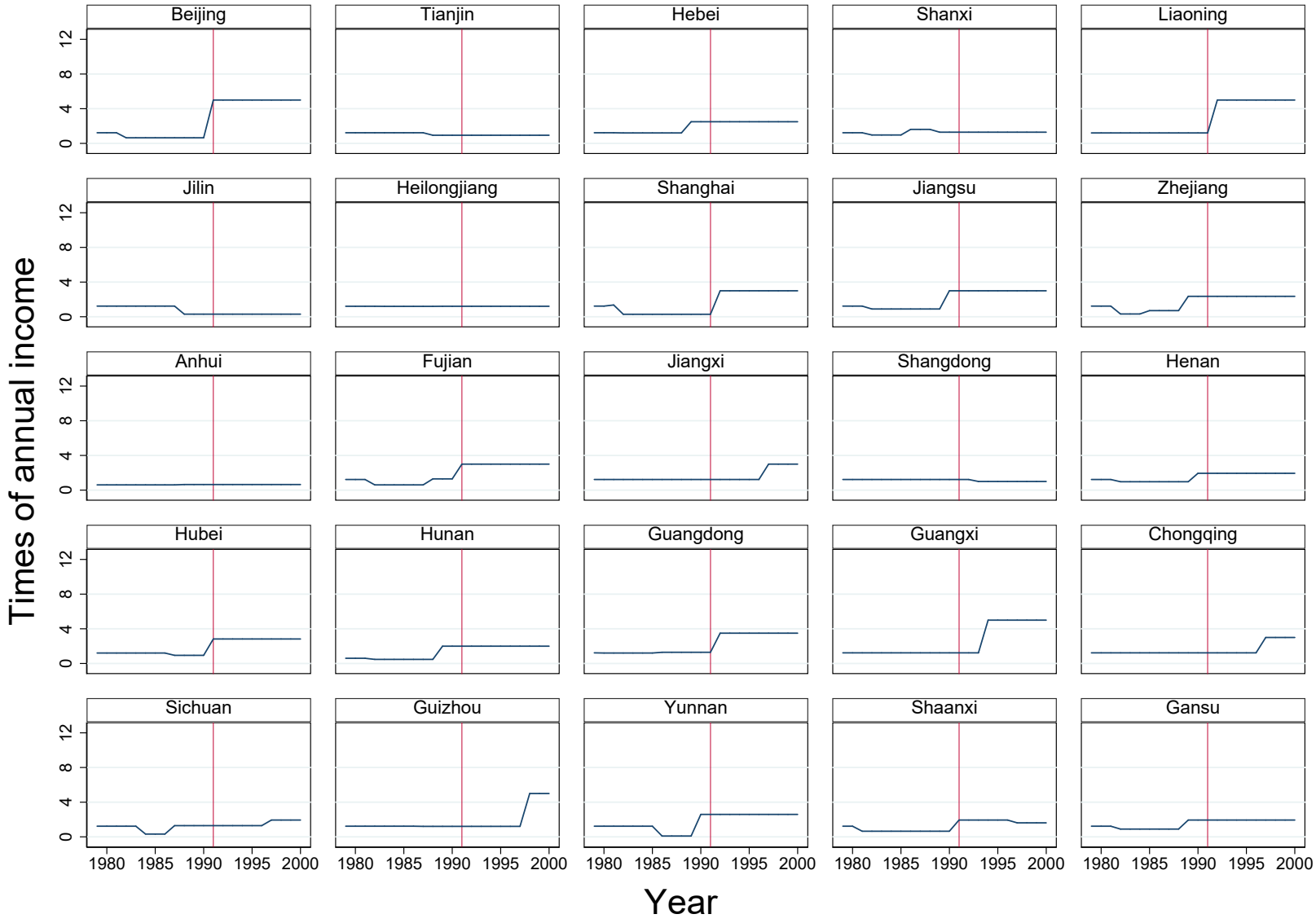
Province	First Report	Second Report	Third Report	Fourth Report	Fifth Report	Six Repo
Beijing	1982.10: 7Y,10%	1991.5: 5-50000¥	2002.12: 1Y, 300%-500%			
Tianjin	1988.11: 5Y, 20%	1997.1: 5Y, 20%	2003.9: 200%-500%			
Hebei	1982.4: 14Y, 10%	1989.3: 1Y, 100%	1997.9: 1Y, 100%	2001.3: 1Y, 250%		
Shanxi	1982.6: 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: 1
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: 1
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% (¡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: 1
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' own collection.

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

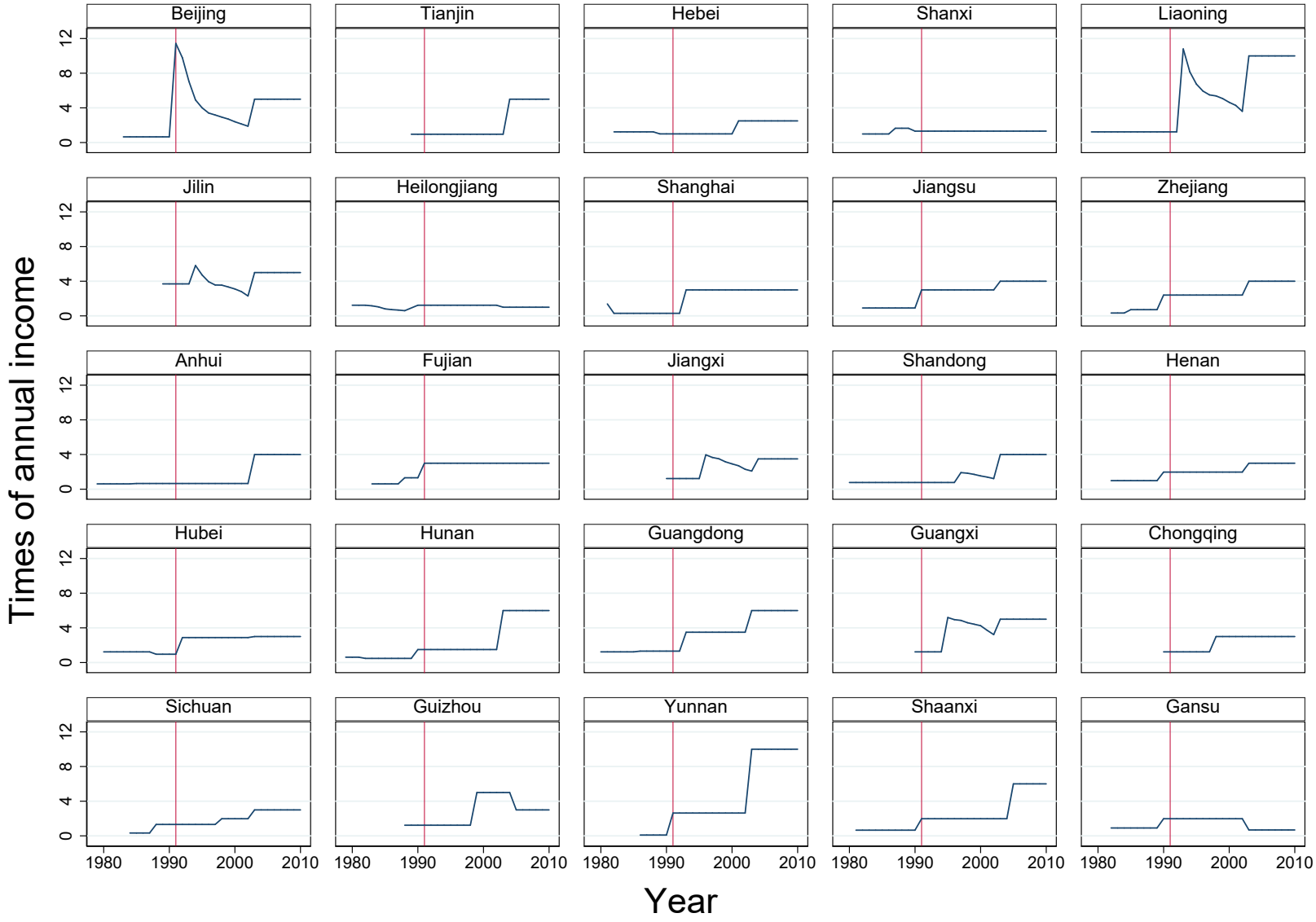
[b] Note: the authors added one penalty data.

Figure 6: Provincial Fertility Penalties in Urban China



Data source: Ebenstein (2010).

Figure 7: Provincial Fertility Penalties in Urban China-Modified Data



Data source: Scharping (2013) and authors' own calculation.

Table 11: Distribution of responses: trust in local gov.

	Trust in local gov.	
	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

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

Table 12: Estimates of OCP Enforcement Levels on Trust

Dependent variable:	Trust in neighbors			Trust in parents			Trust in local gov.		
	4 years penalty	5 years penalty	6 years penalty	4 years penalty	5 years penalty	6 years penalty	4 years penalty	5 years penalty	6 years penalty
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Min(OCP exposure)	0.031	0.066	0.108	-0.024	-0.046	-0.000	-0.088	-0.064	-0.131
	(0.061)	(0.070)	(0.075)	(0.046)	(0.050)	(0.054)	(0.052)	(0.066)	(0.079)
Min(OCP exposure)	-0.177***	-0.205***	-0.232***	0.006	0.019	-0.020	0.005	-0.001	0.055
$\times 1stChild : girl$	(0.060)	(0.068)	(0.061)	(0.060)	(0.068)	(0.081)	(0.089)	(0.118)	(0.161)
OLS P-Value	[0.007]	[0.006]	[0.001]	[0.924]	[0.781]	[0.804]	[0.952]	[0.995]	[0.736]
Bootstrap P-Value	[0.004]	[0.004]	[0.004]	[0.998]	[0.804]	[0.796]	[0.956]	[0.996]	[0.804]
Mean DV	6.407	6.407	6.407	9.623	9.623	9.623	4.304	4.304	4.304
Std.Dev.DV	2.065	2.065	2.065	1.155	1.155	1.155	2.470	2.470	2.470
Observations	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with small number of clusters (Cameron et al. 2008). We report the corresponding p-values. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 13: Estimates of OCP Exposure on Trust, 1991-2010

Dependent variable:	Trust in neighbors			Trust in parents			Trust in local gov.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OCP exposure	0.009	0.046	0.054	-0.024	-0.052	-0.013	-0.091	-0.055	-0.143
	(0.046)	(0.056)	(0.067)	(0.053)	(0.060)	(0.057)	(0.057)	(0.086)	(0.084)
OCP exposure	-0.217***	-0.256***	-0.225***	-0.012	-0.003	-0.050	-0.013	-0.045	0.077
$\times 1stChild : girl$	(0.064)	(0.080)	(0.080)	(0.076)	(0.085)	(0.092)	(0.128)	(0.169)	(0.197)
P-Value	[0.002]	[0.004]	[0.010]	[0.877]	[0.974]	[0.590]	[0.918]	[0.794]	[0.700]
Bootstrap P-Value	[0.004]	[0.004]	[0.008]	[0.864]	[0.944]	[0.624]	[0.920]	[0.836]	[0.832]
1st Child Birth Year FE			Y			Y			Y
Individual controls		Y	Y		Y	Y		Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	6.407	6.407	6.407	9.623	9.623	9.623	4.304	4.304	4.304
Std.Dev.DV	2.065	2.065	2.065	1.155	1.155	1.155	2.470	2.470	2.470
Observations	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897
R-squared	0.051	0.072	0.089	0.041	0.071	0.094	0.042	0.062	0.081

The OCP exposure measure is the 4-year average fertility penalties after someone having his/her first child. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with small number of clusters (Cameron et al. 2008). We report the corresponding p-values. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 14: Estimates of OCP Exposure on Trust, 1991-2010

Dependent variable:	Trust in neighbors			Trust in parents			Trust in local gov.		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OCP exposure	-0.028	0.014	0.017	-0.036	-0.072	-0.029	-0.142***	-0.117	-0.243***
	(0.055)	(0.061)	(0.062)	(0.055)	(0.062)	(0.060)	(0.045)	(0.072)	(0.069)
OCP exposure	-0.186***	-0.233***	-0.199**	-0.002	0.006	-0.047	0.042	0.016	0.200
$\times 1stChild : girl$	(0.060)	(0.076)	(0.074)	(0.079)	(0.089)	(0.098)	(0.107)	(0.148)	(0.179)
P-Value	[0.005]	[0.005]	[0.013]	[0.975]	[0.944]	[0.637]	[0.699]	[0.916]	[0.274]
Bootstrap P-Value	[0.004]	[0.004]	[0.004]	[0.928]	[0.992]	[0.656]	[0.764]	[0.956]	[0.444]
1st Child Birth Year FE			Y			Y			Y
Individual controls		Y	Y		Y	Y		Y	Y
Province FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Mean DV	6.407	6.407	6.407	9.623	9.623	9.623	4.304	4.304	4.304
Std.Dev.DV	2.065	2.065	2.065	1.155	1.155	1.155	2.470	2.470	2.470
Observations	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897	1,897
R-squared	0.050	0.071	0.089	0.041	0.072	0.095	0.042	0.063	0.082

The OCP exposure measure is the 6-year average fertility penalties after someone having his/her first child. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.We use a wild cluster bootstrap-t procedure that are clustered at the province level for improved inference with small number of clusters (Cameron et al. 2008). We report the corresponding p-values. We also report the p-values for OLS with clustered data. Number of clusters: 25.

Table 15: Correlation between Fertility Penalty and Higher-order birth cohort sizes

Dependent variable:	Higher-order birth cohort sizes	
	(1)	(4)
Fertility penalty	-16.134***	-5.061***
	(3.242)	(1.797)
Province FE	N	Y
Year FE	N	Y
Observations	676	676

Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1