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

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Abstract

We provide evidence on how responsibility-shifting through delegation occurred in China's implementation of the one-child policy. We show that trust in local governments was reduced when they were the primary enforcer of the policy (1979–1990), while trust in neighbors was reduced when civilians were incentivized to report neighbors' violations of the policy to the authorities (1991–2015). This effect was more pronounced among parents of a firstborn daughter, who were more likely to violate the policy due to the deep-rooted son preference. This study provides the first set of field evidence on the responsibility-shifting effect of delegation.

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1 Introduction

Can the government implement an unpopular policy without bearing responsibility for it? Governments constantly need to take unpopular policy measures to achieve political goals that benefit the general public or special interests. Political leaders employ many strategies to avoid the electoral cost or political distrust that comes with these unpopular policies (Hood, 2010). One such strategy, as proposed by many philosophers and scholars starting with Machiavelli, is to delegate the enactment of unpopular measures to agents, thus shifting the responsibilities to them (Fiorina, 1986; Vaubel, 1986). However, the effectiveness of delegation on responsibility-shifting is assumed rather than empirically tested (Hinterleitner, 2017).

A growing body of experimental literature indicates that delegation is not merely a way to take advantage of agents' private information or lower opportunity cost, as in traditional principal-agent models. It can also serve as a tool to shift responsibility for unpopular actions from the principal to the agent (Hamman et al., 2010; Coffman, 2011; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). Even though the effectiveness of delegation in responsibility-shifting has been examined in the lab, field evidence is still missing. In this work, we fill this gap using evidence from China's one-child policy (OCP), which provides us with an ideal setting to study the effect of delegation in shifting responsibility.

The OCP was a large-scale birth control campaign carried out from 1979 to 2015, and it was a highly undesirable policy. In 1979, the median family in China had 4.5 children. However, under the OCP, most urban couples were only allowed to have a single child. Starting in 1991, as a response to the unexpected number of "excess" births revealed by the 1990 census, OCP enforcement was delegated to civilians through mass mobilization. The local authorities created monetary and non-monetary incentives for people to report their neighbors' violations of the policy. Grassroots enforcement organizations were established, and civilians were appointed to enforce the policy in their neighborhoods.

Taking into account the fact that provinces differed substantially as to their economic con-

ditions, demographic settings, and other political situations, the central government granted the provincial governments a high degree of discretion in determining the strength of OCP enforcement, which generated variations in enforcement across provinces and time. Taking advantage of this feature, we construct an individual-level measure of OCP exposure in urban areas. We use trust as the outcome variable of interest in this study as political trust is one of the most important indicators of regime support. We gather citizens' trust in local governments from a nationally representative survey.

Our results are consistent with the responsibility-shifting effect of delegation. People who experienced a stricter OCP enforcement in the post-1991 period currently have less trust in neighbors to whom OCP enforcement was delegated. However, surprisingly, OCP exposure does not negatively correlate with people's current trust in local governments even though the local governments directly decided the strength of OCP enforcement and provided incentives for people to report their neighbors.[1]

To address the concern that OCP enforcement variations reflect systematic differences in provinces and our findings could be driven by those differences, our empirical strategy exploits the variation in the gender of the first child. Given the deep-rooted belief that one family needs *at least one son* to maintain the family's lineage, parents whose firstborn was a girl were more likely to violate the OCP by trying to have a second child, unlike parents whose firstborn was a boy. Thus, parents with a firstborn daughter were more affected by the OCP. A natural concern is the possibility that the first child's gender was not perfectly exogenous due to pervasive sex selection practices in China, such as selective abortion. However, multiple sources of evidence suggest that sex selection rarely happened with the *first birth* due to the fear of infertility after abortion (Li et

[1]To test whether people blame the central government instead for the OCP, we construct two proxies for trust in the central government in light of the lack of direct measurement. Using the same empirical strategy, we do not find any evidence that people hold the central government responsible either.

al., 2011; Li and Wu, 2011). We also test the relationship between parents' characteristics that are related to trust and the first child's gender but do not find a significant correlation.

Consistent with previous findings, we find that exposure to the OCP in the post-1991 period leads to a significantly larger reduction in trust in neighbors for those whose firstborn child was a girl than for those whose firstborn was a boy. However, OCP exposure does not undermine the trust that parents of a firstborn daughter have in local governments more than parents of a firstborn son. Furthermore, the coefficients on trust in neighbors are significantly different from the coefficients on trust in local governments at the 5% level.

One alternative explanation for the finding that OCP exposure lowers trust in neighbors but not in local governments is that people are unwilling or do not dare to report distrust in local governments. OCP enforcement practices in the 1979–1990 period provide evidence against this alternative explanation. During this period, the National Family Planning Commission was established and its (government) officials were in charge of enforcing the policy. In contrast to parents who experienced the post-1991 period OCP, parents who had a firstborn girl in this period lose more trust in local governments with stronger exposure to the OCP compared to parents with a firstborn boy. Meanwhile, exposure to this period's OCP does not have a significant impact on trust in neighbors. These results suggest that a government can lose its political trust when the unpopular policy is directly enforced by its own officials.

Finally, we test whether the effect of delegation on trust in neighbors extends to generalized trust, which is a core form of social capital that has important implications for economic development and civic engagement and well-being (Nannestad, 2008). Using the same empirical strategy as in the previous analysis, we find that when the OCP enforcement was delegated to civilians in the post-1991 period, people's generalized trust is lower when exposure to the OCP is higher. However, this detrimental effect of OCP exposure on generalized trust disappears when the government officials directly enforced the OCP in the 1979–1990 period without delegation. Those results suggest that responsibility shifting through delegation comes at a societal cost.

This study's research is closely related to the lab experiment literature on responsibility-shifting

through delegation (Hamman et al., 2010; Coffman, 2011; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). The literature find that when an unfair allocation is directly chosen by the principal, the recipient who is adversely affected is willing to punish the principal harshly. However, if the allocation task is delegated to an agent whose interest is aligned with the principal and she chooses an unfair allocation, then the principal receives a much smaller punishment and the agent is punished. By investigating the impact of OCP exposure on political trust with and without delegation, we contribute to this literature by providing the first field evidence of the effect of delegation on responsibility-shifting. Our findings suggest that delegation in the form of mass mobilization can help the government to maintain political trust even when people's lives are deeply affected by its unpopular policy.

By confirming the effectiveness of delegation in responsibility avoidance, this paper also provides a rationale for mass mobilization. By encouraging people to fight against each other, governments can avoid responsibility for implementing an unpopular policy, a cost they must bear if they need to do all the work themselves. Even though mass mobilization is widely observed in authoritarian regimes, it has been overlooked in economics. The only exception is Lichter et al. (2015). Using county-level data on the number of informers in the 1980s in East Germany, the authors show that higher levels of government surveillance led to lower levels of political trust in post-reunification Germany. The key difference between our work and theirs is who the task is delegated to. In Lichter et al.'s (2015) scenario, even though there was a large number of informers, they were still contracted government employees. The government was still responsible for their actions, while in our setting the informers were mobilized civilians with no government affiliations. Our results highlight the importance of the agent to whom the government is delegating the enforcement of the unpopular policy in responsibility avoidance.

In a broader context, our findings contribute to the growing literature focusing on conflict and trust. Nunn and Wantchekon (2011) show how the historical slave trades affect current trust levels within Africa. Rohner et al. (2013) examine the impacts of ethnic conflicts on trust and ethnic identity using multi-level data from Uganda. Chen and Yang (2019) identify a causal impact of the

Great Chinese Famine on the survivors' and subsequent generations' trust in the government. We add to this literature by providing a clear mechanism of the effect of conflict on distrust. We show how mass mobilization, a measure popular among governments during a social conflict, affects people's interpersonal and institutional trust.

Last, this study adds to the literature studying the impacts of the OCP. The consequences of the OCP range from economic growth (Li and Zhang, 2007) to sex ratio imbalances (Ebenstein, 2010; Li et al., 2011) to female education (Huang et al., 2021) to competitive saving motives (Wei and Zhang, 2011). We add to this literature by showing that the enforcement of the policy incurred a hidden cost to the civil society by lowering people's interpersonal trust.

Our paper proceeds as follows. Section 2 provides an overview of the institutional context of the policy. Section 3 describes the data, and Section 4 outlines the empirical strategy. We present the main results in Section 5. Section 6 discusses and rules out alternative explanations, and Section 7 concludes.

2 Background of the One-Child Policy

China's one-child policy is credited with dropping the total fertility rate (births per woman) from 3.01 in 1978 to 1.66 in 2015.[2] Chinese culture has long favored large families. For example, total fertility exceeded six births per mother throughout the 1960s (Banister, 1991). To slow down population growth, the OCP was introduced in 1979 and began to be formally phased out in 2015. Under the OCP, most urban couples were only allowed to have a single child. However, the regulations varied among regions, and provincial governments localized the state fertility policy according to different demographic and socioeconomic conditions (Huang et al., 2021).

[2]The data come from the World Population Prospects 2019, conducted by the United Nations: https://www.macrotrends.net/countries/CHN/china/fertility-rate.

The government employed various methods to enforce the OCP. First, parents would face severe penalties for "above-quota" births. The most widely used penalties were monetary sanctions. Depending on the province of residence and year of violation, the fine for an unauthorized child could be up to ten times the annual household income (Scharping, 2013). In urban areas, non-monetary punishments were also sometimes used. For example, people employed in urban units were threatened with the denial of health and welfare benefits, lack of job promotions, or even demotions if they violated the OCP. Second, after meeting the birth quota, women of reproductive age were required to have an intrauterine device (IUD) inserted or undergo female sterilization (Scharping, 2013). To ensure a high compliance rate, the so-called Three Examinations was also carried out, which checked women of reproductive age for the use of an IUD, pregnancy, and illness four or more times a year. If unauthorized pregnancies were detected, forced abortion was a possible consequence (Fong, 2016).

2.1 Mass Mobilization in the Post-1990 Era

From the 1990 census, the central government found many "excess" births during 1986–1990. To achieve the 1.2 billion population limit for the year 2000, the central government began stricter OCP enforcement. A centerpiece of the policy bundle was the mass mobilization campaign. This movement was clearly stated by Tieying Li, who was a member of the Central Politburo of the Communist Party at that time. "No one is allowed to give birth beyond the birth quota, and let the masses watch each other," he said in an internal speech on April 21, 1990.

The rationale for this campaign was twofold. First, the local cadres lacked the necessary information about who was pregnant and whether or not it was above quota. There were not enough local officials to monitor every woman of childbearing age, and those parents who planned to give unauthorized births intentionally hid from them. The government thus relied on people who were close to the pregnant women to provide the information. Second, there were not enough local officials to enforce the policy even if the information was provided. Enforcing the OCP was not only about sterilizations and forced abortions. The cadres also launched propaganda campaigns

to promote the idea of one child and carried out the Three Examinations. An expanded crew, with a limited budget, was needed within a short time frame, which made asking citizens to be "volunteers" highly attractive.

Two measures were taken to mobilize the masses. First, to deal with the information asymmetry, citizens were encouraged and incentivized to report unauthorized pregnancies and births of their neighbors, coworkers, and relatives to the authorities. The incentives provided by the government were mainly monetary, and the payments varied across regions and were linked to fertility punishments.[3] For example, in Chongqing in 2009, the informer could be awarded 5% of the fertility penalty paid by the victims, which was the equivalent of one month's salary.[4] In addition, the informer's identity was kept confidential, and there was no record of punishments for a false report. Second, to deal with the ever-increasing workload, more at-will employees and "volunteers" were recruited by the government. Generally seen by people as neighbors, they carried out most of the detailed work instead of government officials.

To discuss the recruitment and duties of those workers, we first must mention several facts about local management in urban China. The National Family Planning Commission was in charge of OCP enforcement, and its lowest level was located in the county government, which worked with resident committees to achieve the birth-planning targets. The resident committee (sometimes also translated as the neighborhood committee) is the lowest level of urban administration in China.

[3]Reporting could also lead to career rewards, and failing to report could sometimes cause collective punishments. The career rewards for reporting were salient when there were competitions between colleagues. Public sector employees who were caught violating the OCP would not be promoted in most cases and might even lose their jobs. Collective punishments were collected when someone was aware of an OCP violation but failed to report it. During the years when the OCP was most fiercely implemented, one worker's violation of the OCP could lead to all their coworkers losing a significant part of their income in some state-owned enterprises. See a news report at http://epaper.oeeee.com/epaper/A/html/2020-10/04/content_29205.htm.

[4]See a news report at http://www.chinalawedu.com/lvshi/AAA635949214532/5_8912.shtm.

According to the Chinese constitution, it enjoys a high degree of autonomy and is named the "self-government organizations of the masses." It is allowed to recruit at-will employees and pays them independently. Although the residents' committees are formally the lowest organizational entities, there are entities one level below them—the residents' small groups (jumin xiaozu). They may comprise a neighborhood or just an apartment building. The small groups are used as an internal organization tool and do not have a legal status of their own. Their leaders are called "cluster leaders" and are appointed by the residents' committee but are not on the government payroll.

Enabled by the revenue collected from the fertility penalty in the 1990s, the resident committees recruited many at-will, full- and part-time OCP enforcers. For example, Huangjiapu, a resident committee with a population of 500 in Shanxi Province, had 15 at-will, full-time employees tasked with family-planning matters at its peak (Fong, 2016). Because those employees were paid by the resident 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.

Nevertheless, the OCP enforcers still failed to solve the labor shortage, and as a result, cluster leaders were also mobilized to enforce the policy. They were tasked with keeping track of households' reproductive habits and reporting those details to the local family-planning commission. These leaders were also seen as neighbors. In addition to the cluster leaders, state organizations such as the military, public schools, and hospitals had their own internal family-planning units, as did state-owned enterprises.

These neighborhood-level staff and cluster leaders were the basic building blocks of China's OCP machinery. According to a report issued by the National Family Planning Commission, while there were only half a million full-time employees combined in the central and local commission, there were about 1.2 million neighborhood-level birth-planning staff and more than six million cluster leaders who were mobilized to enforce the OCP (Fong, 2016).

2.2 Provincial Variations in OCP Enforcement

One significant feature of the OCP is that the local governments are heavily involved in both making and enforcing policies. Even though birth controlling is a national matter in this period (1978–2015), no national-level birth-controlling regulations were ever passed (Scharping, 2013). The provincial governments were granted almost full discretion in specific OCP rules, including the amount of the fertility penalty, the incentive scheme for reporting unauthorized births, and the budget to recruit at-will employees tasked with OCP enforcement.

The most salient aspect of the OCP in terms of regional variations is the fertility penalty, which is the amount of monetary punishment a household needed to pay for an above-quota birth. We later use fertility penalties for one unauthorized child to measure the strictness of the OCP enforcement in our empirical analysis. As displayed in Figure 1, fertility penalties, measured as a multiple of income, varied across provinces and across time.[5] The amount of fertility penalty could be as high as 12 times the household income in some provinces such as Beijing and Liaoning in some years but could also be as low as half to 4 times the household income in some provinces such as Hubei and Chongqing during the whole period. Regarding the time trend, the average amount of fines increased over time in most provinces. However, the timings of the changes were quite different among provinces, and there were ups and downs in many provinces. Those provincial and

[5]At the provincial level, as documented by Scharping (2013), there were three forms of fertility fines. The first form was a percentage deduction from wages over several years. For example, in 1996 Jiangxi province reported that an unauthorized birth carried a fine of 50% wage deduction from both parents for seven years. The second type of fines was levied as a lump-sum payment based on annual income. For example, in 1991, Fujian province levied a fine equivalent to three times the annual household income for an unauthorized birth. The third form was a certain amount of immediate payment regardless of household income. For example, from 1995 to 2000, Guangxi ratified the fine as an amount between 2,000 RMB and 50,000 RMB. Following Ebenstein (2010), we transform all three types of fines into percentages of household income.

time variations caused people who registered with different provinces and gave birth at different times to experience the OCP in different forms.

3 Data

To estimate the effect of delegation on trust, we use measures of trust from the China Family Panel Studies (CFPS, a nationally representative survey conducted by Peking University) and individual-level OCP exposure data from Scharping (2013) and statistical yearbooks. We introduce our measurement of different types of trust in Section 3.1 and individual-level exposure to the OCP in Section 3.2.

3.1 Measurement of Trust

The primary outcomes of interest are citizens' trust in neighbors and local governments, which are measured by the CFPS-2016 survey.[6] The wording of the question of trust in neighbors is "Please rate, on a scale from 0 to 10, to what degree you trust your neighbors? 0 represents very untrustworthy, while 10 represents very trustworthy." A similar question was also asked on trust in local government officials: "Please rate, on a scale from 0 to 10, to what degree you trust your local government officials? 0 represents very untrustworthy, while 10 represents very trustworthy." Therefore, our measure of trust in local governments is also a construct of trust in a specific group of people, not trust in an institute, enabling us to compare trust in local governments to trust in neighbors, another specific trust in a group of people.

One may also be concerned with the validity of categorical trust measures. However, some widely used surveys (e.g., the World Value Survey and the General Social Survey) use the same

[6]Trust measures in the CFPS have been extensively used in previous studies (see, e.g., Bai and Wu, 2020 and Chen and Yang, 2019).

questions to measure trust. Also, Johnson and Mislin (2012) provide experimental evidence that trust, as measured by surveys, is positively correlated with experimentally measured trust.

3.2 Individual-Level OCP Exposure

Our key explanatory variable is an individual-level measure of exposure to the OCP: the average fertility penalty rate that a person faced within five years after the arrival of their first child. As shown in Figure 1, fertility penalties—the amount of monetary punishment a household needed to pay for an above-quota birth—varied across provinces and time. Note that fertility penalties not only represented the strictness of OCP enforcement as suggested by the previous literature (Ebenstein, 2010; Huang et al., 2021) but are also positively correlated with the financial incentives that the local governments could provide for informers. For example, an informer in Chongqing in 2009 could receive 5% of the fertility penalty paid by the victims.

We improve the measure of OCP enforcement in the literature in two ways. First, we modify the formula for transforming the fines into multiples of household income to reflect the rapid and unbalanced growth in the Chinese economy in the last 30 years.[7] We also extend the penalty data from 2000 to 2015. Second, we construct an individual-level measure of OCP exposure by exploiting individual variations in the timing of the first birth. In our measure, only the penalty rates implemented after parents had their first child count because individuals were restricted by the OCP only after having their first child. China census data show that the interval between the birth of most couples' first and second child was no more than five years (Scharping, 2013). Therefore, the strongest impact of the birth control policy fell on couples during the five years following the birth of their first child, which is why we only count the penalty rates in those five years. Our main results are not sensitive to the five-year window in OCP exposure. As shown in Appendix Tables A6 and A7, changing the interval to four or six years does not qualitatively alter our results.

[7] More details of our calculation are provided in Appendix A.

3.3 Sample Selection and Descriptive Statistics

This study only focuses on urban households who gave birth to their first child between 1979 and 1985 or between 1991 and 2010. We restrict our sample to urban households because we do not have a valid measure of OCP exposure in rural areas. Compared to urban areas, rural areas are farther away from administrative centers, making it much harder for the higher-level government to ensure that the local cadres closely followed the provincial policy. Furthermore, it was also difficult for the local cadres to collect information on the annual household income, on which the fertility penalty was based and calculated. In practice, the fertility penalty was often set to be the same for all households in the same village due to lacking information.

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

We exclude people who gave birth to their first child after 2010 from our sample as they only experienced OCP during part of the five-year interval after that child was born. For a similar reason, we exclude people who gave birth to their first child between 1986 and 1990 as we cannot tell whether they experienced the OCP in the earlier phase or the later phase. The CFPS includes slightly different variables in different years. Since in our empirical analysis we use the first child's date of birth from CFPS-2010, urban/rural residence status from CFPS-2012, and post-policy trust from CFPS-2016, we only keep individuals who completed all CFPS-2010, CFPS-2012, and CFPS-2016 surveys in our sample.

Table 1 provides summary statistics of the key variables of interest. For people who gave birth to their first child between 1991 and 2010 (Panel A), the mean value of trust in neighbors is 6.41, which is higher than the mean value of trust in local governments, 4.30. The average fertility penalty they face is 3.39 times the household annual income. In our sample, 45.8% are male

respondents and 13% are Communist Party members. For people who gave birth to their first child between 1979 and 1985 (Panel B), their average trust in neighbors and trust in local governments are slightly higher compared to those who gave birth to their first child between 1991 and 2010. The share of male respondents and the share of Communist Party members are slightly higher, and the share of people employed in 2016 are lower because they are in general older. In our main specification, we control for age (birth dummies), family income per capita, education, gender, Communist Party member dummies, and employment status.

4 Empirical Strategy

4.1 Baseline Estimates

We start our analysis by estimating the overall effect of OCP exposure on political and interpersonal trust. Specifically, we estimate the following model:

$$y_{icp} = \sum_{c} \alpha_c + \beta Exposure_{ip} + \mathbf{X}'_{icp}\gamma + \mathbf{X}'_{p}\rho + \varepsilon_{icp}, \tag{1}$$

where i indexes the individual, c indexes the individual's birth year, and p indexes the province. y_{icp} is current trust (measured in 2016) in neighbors and local governments. $Exposure_{ip}$ is defined as the five-year average of the penalty rates in province p after individual i of cohort c had their first child. α_c are individual birth-year fixed effects, accounting for age-specific shocks that might influence individuals' trust across all provinces. β is our coefficient of interest, which captures the impact of the OCP on people's current trust. The vector $\mathbf{X}'_p$ consists of provincial-level variables: GDP per capita, population, and unemployment rate in the baseline year. We include them in the regression analysis to capture provincial variations that can potentially contribute to both the OCP's strictness and people's interpersonal and political trust. $\mathbf{X}'_{icp}$ is a vector of observable characteristics at the individual level. Here, we include the log of (current) family income per person,

education attainment, gender, Communist Party membership, and employment status, which are all shown to be correlated with trust (Alesina and La Ferrara, 2002; Bellows and Miguel, 2009).

4.2 Preferred Estimates

In our baseline estimation, we intend to test whether OCP exposure and trust are negatively correlated. However, the correlation could also be explained by omitted variables that are correlated with OCP exposure and with subsequent trust. Our identification strategy exploits the exogenous variation in the gender of the first child. A deep-rooted belief in the Chinese culture is that each family needs *at least one* son to maintain the lineage.[8] Consequently, urban couples whose first child was a girl were more likely to violate the OCP by trying to have a second child than parents whose first child was a boy. If so, they were more exposed to the OCP penalties. Evidence from census data suggests that the "at least one son" preference was quite persistent in modern China. Table 2 illustrates the probabilities of having more than one child after having either a firstborn boy or a firstborn girl using 1990, 2000, and 2005 census data. It suggests that, for instance, 49% of those with a firstborn daughter had more than one child, while for those with a firstborn son the number was 36% in the 2000 census. We consider this gap to be large since people who were not affected by the OCP were also included in the census.

One potential concern is that the first child's gender is not exogenous because sex selection was a widespread practice in China. However, there were few sex selections performed for the first child due to the fear of infertility. For example, Chen et al. (2013) provide evidence of few sex-selective abortions on the firstborn child, using data from the Chinese Children Survey, a large-scale survey

[8]This "at least one son" preference is a core component of Confucianism, the "state religion" in ancient China. 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 that "there are three ways to be unfilial, and bearing no heirs is the worst" (Chan, 2002; Shun, 1997).

conducted by the National Bureau Statistics of China. They show that for first pregnancies, the (self-reported) abortion ratio is lower than 5%, while the ratio is above 20% for third and fourth pregnancies. Importantly, the authors also find that whether the ultrasound technology is available or not in a region is not correlated with the sex ratio of the first child (but the availability of the technology is associated with higher level of sex ratios for higher order births).

The nationwide census data confirm and complete our argument that the first child's gender is exogenous. As shown in Table 2, the ratio of boys and girls for the firstborn child was close to the biological norm. According to Lazarus (2002), the average male-female sex ratio at the first birth is estimated to be 106:100.[9] The sex ratios were 1.052, 1.071, and 1.024, respectively, in the 1990, 2000, and 2005 census waves, all close to the biological norm.

To further validate that the gender of the first child is orthogonal to OCP exposure, we run a regression of the first child's gender on OCP exposure and factors related to trust according to previous studies. The results, reported in Table 3, indicate that people did not endogenously choose the first child's gender based on the fertility penalties in previous years. Also, none of the other control variables, including age, education, Communist Party membership, and parents' education, significantly affect the first child's gender.

[9]Most estimations of the biological sex ratio were calculated based on the White population. However, studies have shown that there are substantial racial differences in human sex ratios, with the highest among Asians, lowest among Black Africans, and in between among Whites (Shaw et al., 2021). Therefore, 106 is likely an underestimation of the biological sex ratio in China.

To estimate the effect of OCP exposure on trust, we estimate the following equation:

$$\begin{aligned} y_{icp} = &\sum_c \alpha_c + \sum_p \delta_p + \mathbf{X}'_{icp}\gamma + \beta_0 Exposure_{ip} + \beta_1 1stChildGirl_{icp} \\ &+ \beta_2 Exposure_{ip} \times 1stChildGirl_{icp} + \sum_c \alpha_c \times 1stChildGirl_{icp} \\ &+ \sum_p \delta_p \times 1stChildGirl_{icp} + \mathbf{X}'_{icp}\gamma \times 1stChildGirl_{icp} + \varepsilon_{icp}, \end{aligned} \tag{2}$$

where $1stChildGirl_{icp}$ is an indicator that equals one if individual i's first child was a girl. β_0 captures the effect of OCP exposure on trust for individuals whose first child was a boy. The coefficient of interest is β_2, which captures the differential impact of OCP exposure on trust of people with a firstborn girl compared to trust of those with a firstborn boy. α_c are birth-year fixed effects, and δ_p are province of current residence fixed effects. This setting accounts for cohort-specific shocks to individuals' trust across all provinces as well as for time-invariant, province-specific trust characteristics. The individual-level controls $\mathbf{X}'_{icp}$ are the same as in equation 1, including the log of (current) family income per person, education attainment, gender, Communist Party membership, and employment status. Last, ε_{icp} is the error term. We cluster our standard errors at the province level to allow for correlation over time within a province. We employ the wild cluster bootstrap-t procedure (Cameron et al., 2008) to account for the small number of clusters.

5 Results

5.1 Main Results

Before we turn to our preferred specification identifying the causal effect of OCP exposure on trust, we start by showing the overall effect of OCP exposure on trust, based on estimates from equation 1. Panel A of Table 4 presents the estimates in the post-1991 period when the policy enforcement was delegated to neighbors. Columns 1 and 3 show estimates that only include birth-year fixed effects and provincial-level control variables in the baseline year. We add individual-

level controls in columns 2 and 4, which indicate that our results are consistent across different specifications.

When the policy was delegated from local governments to civilians, parents' current trust in neighbors are significantly lower if they were exposed to a more severe OCP enforcement. A one standard deviation increase in fertility penalty rates leads to a 0.09 standard deviation decrease in trust in neighbors. However, there is no significant impact of OCP enforcement on parents' trust in local governments. The magnitude of the impact on trust in local governments is also very small, close to zero. This result is consistent with the responsibility-shifting effect of delegation: people do not lower their trust in the delegator of the unpopular policy, the local governments, but they lower their trust in the delegate, the neighbors.

The results in Panel A indicate an overall negative correlation between OCP exposure and trust in neighbors but not trust in local governments. To complement those overall correlational results, Panel B presents causal estimates of the impact of OCP exposure on trust from equation 2, using our identification strategy that exploits the gender of the first child. In columns 1 and 3, we present the parsimonious estimates that only include province and birth-year fixed effects. We add individual-level controls as our preferred specification and show the results in columns 2 and 4. The remaining discussion focuses on our preferred specification.

Regarding trust in neighbors, OCP exposure in the post-1991 era with delegation significantly lowers current trust in neighbors for couples who had a female firstborn but not for those whose firstborn was a boy. The estimated differences between the two groups (β_2) are significantly different from zero at the 1% level and are economically large (Panel (b) of Figure 2). As the gender of the firstborn is arguably exogenous, this result suggests that the effect we find is causal. The parameter estimates indicate that a one standard deviation increase in OCP exposure is associated with a 0.29 standard deviation decrease in trust in neighbors for people whose firstborn was a girl than for those who had a male firstborn.

Next, we examine whether OCP exposure affects people's political trust (columns 3 and 4, Panel B of Table 4). Consistent with the overall effect, Table 4 shows no significant impact of

OCP exposure on trust in local governments regardless of the first child's gender. Also, there is no significant difference between the two groups of people (Panel (b) of Figure 2). The estimate of the coefficient β_2 is small and not significantly different from zero. Our results are also consistent if we use wild bootstrap p-values.

If responsibility shifting drives our results, we should only expect two group-specific trusts, namely, trust in neighbors and government, to be affected by OCP exposure. Other group-specific trusts, including trust in parents and trust in Americans, as in the CFPS survey, should remain constant regardless of OCP exposure. Those two trusts are constructed the same way as trust in neighbors, with their values varying between 0 and 10. In Table 5, we report results for each of these group-specific trusts in Column (1) to Column (4) using our identification strategy that exploits the gender of the first child. Reassuringly, we do not find any significant differences in these group-specific trusts between people who were exposed to the OCP in different strengths.

One potential concern with the results on trust in neighbors is the time gap between the exposure to the OCP and the elicitation of trust. Whereas people experienced the OCP between 1991 and 2015, CFPS measured their trust in 2016, one year after the policy ended. It could be that their neighbors in 2016, when CFPS elicited trust, were different from their neighbors in the earlier years when OCP still existed. However, this is not a threat to our results for two reasons. First, the survey question on trust in neighbors does not explicitly refer to a particular neighbor and instead asks about people's trust in neighbors as a group, not their current neighbors. Second, even though the identity of neighbors is changing, people can still hold a belief about the trustworthiness of this group. A neighbor's trustworthiness can be seen as a random draw of the group-level trustworthiness, and untrustworthy behaviors of one neighbor can lower people's trust in this group through belief updating.

A natural follow-up question is whether the enforcement of OCP through delegation also changes people's general belief of the trustworthiness of their fellow members of society. To

this end, we study the impact of OCP exposure on people's generalized trust.[10] We again exploit variation in the gender of the first child to estimate the effect of OCP exposure on generalized trust and present the results in Column (5) and (6) of Table 5. Consistent with our hypothesis, the detrimental effect of responsibility shifting through delegation on trust extends from trust in neighbors to generalized trust. Compared to parents with a firstborn boy, exposure to the OCP significantly lowers the generalized trust of parents with a firstborn girl. A one standard deviation increase in OCP exposure is associated with a 0.188 standard deviation decrease in generalized trust for people whose firstborn was a girl.

5.2 OCP Enforcement without Delegation

The post-1991 period results suggest that when OCP enforcement was delegated from local governments to civilians, more severe enforcement significantly undermined people's trust in neighbors but not in local governments. However, failing to find a detrimental effect of OCP enforcement on trust in local governments could also be driven by people's unwillingness or fear to report distrust in local governments. To show that their distrust shifted from local governments to neighbors, we must also look at what happens when there is no delegation, which occurred in the earlier phase of the OCP.

Ideally, we would like to continue to use the fertility penalty to measure OCP exposure. However, the fertility penalty data are largely unavailable in this phase.[11] Therefore, while keeping the construction of the OCP exposure measure the same, we replace the fertility penalty with the rate of family planning, which was defined as the percentage of couples who were of fertility age and

[10] In the 2016 CFPS, the following question is asked to elicit generalized trust: "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?"

[11] Eleven out of 31 provinces had not established their fertility penalty policies in 1984. Some provinces introduced their first fertility penalties in 1988.

had taken birth control measures, like vasoligation, sterilization, and IUDs. In the 1980s, voluntary birth control was quite rare, and thus the family-planning rates measure how successful the local governments were in enforcing the OCP. To validate this measure, we cross-check the correlation between fertility penalties and family-planning rates when both data are available. Specifically, we regress family-planning rates on fertility penalties conditional on province fixed effects. The two measures are positively correlated and statistically significant at the 1% level (Appendix Table A2).

Panel A of Table 6 reports the estimates in the 1979–1990 phase when the policy was solely enforced by government officials, that is, when there was no delegation. Parents' current trust in local governments are significantly lower if they were exposed to a more severe OCP enforcement. A one standard deviation increase in birth control rates leads to a 0.08 standard deviation decrease in trust in local government, and its magnitude is about twice as large as that in the post-1991 period. Meanwhile, we do not find any detrimental effect of OCP exposure on parents' trust in neighbors. Interestingly, people's trust in neighbors is actually higher if they were more exposed to the OCP, perhaps due to neighbors in this period helping each other to hide violations from government officials, which enhanced their mutual trust. However, this result must be interpreted with caution since OCP exposure and trust can both be correlated with omitted variables.

Panel B of Table 6 shows the impact of OCP exposure on trust for parents of a firstborn girl versus parents of a firstborn boy. The results are highly consistent with the responsibility-shifting effect of delegation. For parents whose first child was a girl, a one standard deviation increase in OCP exposure leads to a 0.42 standard deviation decrease in trust in local governments relative to parents whose first child was a boy. The estimates for the difference between the two groups (β_2) are statistically significant at the 1% level (Panel (a) of Figure 2). In contrast to the results on people's trust in local governments, OCP exposure has no significant impact on trust in neighbors regardless of the first child's gender, and the difference between the two groups is not significant. Reassuringly, we also do not find any significant differences in trust in parents and Americans between people who were exposed to the OCP in different strengths (Column (1) to (4) of Appendix

Table A10).

Since exposure to the OCP without delegation does not lower trust in neighbors or other groups, we do not expect OCP enforcement in this phase to be detrimental to generalized trust. Results in Columns (5) to (6) of Appendix Table A10 lend support to our hypothesis. When there is no delegation, we do not find a significant difference between parents whose firstborn was a girl and their counterparts in how their generalized trust respond to OCP exposure. This finding suggests that OCP enforcement does not necessarily undermine social capital; it only does so when coupled with delegation as in the post-1990 phase.

5.3 Robustness Checks

Alternative measures of OCP exposure. We first examine the robustness of our results to alternative measures of OCP exposure. Recall that we used the five-year average fertility penalties (or family-planning rates) after parents had their first child as our measure of OCP exposure. Our results are consistent when we instead use four- or six-year average fertility penalties (or family-planning rates). See Appendix Tables A6 and A7 for more details.

In our main specification, we assume that parents' decisions to try for a second child is orthogonal to the strength of the OCP enforcement they might face, which is why we use the average fertility penalties after having the first child to measure their OCP exposure. If they knew the future levels of fertility penalties and chose to try for a second child when the penalties were low, then the more reasonable measure of OCP exposure would be the minimum level of penalties in the five years after they had their first child. We re-estimate our baseline equations with this alternative measure of OCP exposure. Our results are unaffected qualitatively (Appendix Table A5).

Sex selection at the first birth. In Section 4.2 we provide multiple pieces of evidence to show that sex selection rarely happened at the first birth. However, even if such selections were more prevalent than we estimated, they are more likely to work against our results. If we define couples whose first child is a girl as the treatment group and couples whose first child is a boy as the control group, then sex selections favoring boys lead some couples in the treatment group to be mistakenly

identified as being in the control group, making it harder for us to observe a difference between the treatment and control group.

Even though we show in Section 4.2 that the national average sex ratio is close to the biological norm at the first birth, it does not mean sex selection at the first birth was not a problem in all parts of China. To this end, Appendix Table A3 lists the number of male and female first births across provinces from June 1999 to June 2000 in urban areas in the 2000 census. Since the sample sizes are too small to calculate a precise sex ratio, we construct a one-tailed t-statistic to test whether the calculated sex ratio (column 4) is statistically different from the biological sex ratio (Appendix Section B further shows the detailed construction of the one-tailed t-statistic). We find that six provinces' sex ratios at first birth are significantly higher than the normal sex ratio at the 5% level: Beijing, Jiangsu, Jiangxi, Hubei, Guangdong, and Guangxi. To further validate our results, we drop observations from the six provinces and report estimation results in Appendix Table A4. Dropping those observations does not affect our findings qualitatively.

Different clustering. We next check the extent to which our main results are influenced by alternative choices of clustering. In our main results we cluster standard errors at the province level, allowing error terms to be correlated across individuals living in the same province. In Appendix Table A8, we cluster standard errors at the birth cohort level, allowing error terms to be correlated across individuals of the same age across different provinces. And in Appendix Table A9, we cluster standard errors at both the province and the cohort level, allowing error terms to be correlated across individuals living in same province and of the same age. The statistical inferences of our main results are not affected by these alternative choices of clustering.

Placebo tests using OCP strength before having the first child. If people are indeed exposed to OCP after having their first child, then fertility penalties before having the first child should not affect people's trust. We use the average fertility penalty rate that a person faced within five years *before* the arrival of her first child as the placebo measure of OCP exposure and then run our main specifications. Appendix Table A11 reports the results of this placebo test. Reassuringly, we do not find any significant effect of the placebo OCP exposure on trust in either neighbors or local

governments in the post-1990 phase.[12]

Pseudo-treatment placebo test. We demonstrate the statistical power of the inferences using our main specification by conducting a placebo test based on another pseudo-OCP exposure. We first randomly assign the OCP exposure intensity and gender of the first child to individuals by drawing randomly without replacement from the set of true values, and we then estimate the regression model using the pseudo-treatment instead of the actual data. We randomly assign 2,000 sets of pseudo-treatment draws in this manner; theoretically, these pseudo-treatments should have no effect on trust.

In Panel (a) of Appendix Figure A1, we plot the distribution of coefficient estimates from the 2,000 estimated pseudo-treatment effects on trust in neighbors in the 1991–2015 phase. In Panel (b), we plot the distribution of pseudo-treatment effects on trust in local governments in the 1979–1990 phase. We also mark the location of the treatment effect using the actual OCP exposure and first child's gender within the pseudo-treatment effect distribution. As evident from the figure, the effect estimates from the actual data fall in the extreme left tail of the distribution of pseudo-treatment, suggesting that it is unlikely that effects we identify are due to chance.

Omitted variables bias. Lastly, we consider the possibility that both the gender of the first child and the individual-level OCP exposure are correlated with time-varying unobservables that also determine trust levels. To estimate the degree to which omitted variables may drive our results, we implemented an exercise proposed by Oster (2019), which is based on earlier work by Altonji et al. (2005). The test utilizes the information from the selection of observables to estimate the extent to which the selection of unobservables is needed to produce a treatment effect of zero. The results of this exercise show that selection on unobservables needs to be, on average, 3-4 times

[12]Since the OCP started in 1979, there was no documentation of provincial-level data on birth control rates before 1979. Thus we cannot calculate the five-year average of birth control rates before people started having their first child during 1979–1985.

greater than that of the variables included in equation 2 to zero out the trust results we document. In other words, our results are unlikely to be significantly affected by unobserved selection.

6 Discussions

Our main findings are that, when OCP enforcement, an extremely unpopular policy, was delegated to civilians, there was a significant decline in people's trust in their neighbors but not in local governments. However, when government officials enforced the OCP, people's trust in the government was undermined (Figure 2). These results are consistent with the responsibility-shifting effect of delegation. In this section, we address whether we can attribute the results to the delegation and explore four alternative interpretations of our results.

Endogenous selection of OCP exposure. One may worry about the possibility that parents rationally choose the location and timing of the first birth to enjoy a more lenient OCP enforcement, in which case the correlation between OCP exposure and trust may reflect factors associated with both trust and the choice of location and time for having the first child. However, this is not likely for two reasons.

First, there are multiple reasons why OCP-induced migration is rare. A citizen does not face less stringent enforcement if they simply move to another province. OCP fines are authorized by the citizen's registered *hukou* province, not the province in which she lives. Changing one's *hukou* province was especially difficult during the period of our study (1979–2010) (Liu et al., 2014). The percentage of people who changed their *hukou* provinces within five years during this period was never higher than 5% according to the census.

Second, it is also extremely unlikely that parents chose their first child's birth timing to lower the fertility penalties they would need to pay for the second child, because to do that they would have had to be able to predict the amounts of penalties in the next several years. As discussed in the background section, the making of the specific OCP rules were highly decentralized and opaque (Scharping, 2013). The provincial governments could easily change the amount of penalties in the

form of regulations. Therefore, unless someone was well connected to the provincial governments, it was difficult to predict future penalties.

Fear of reporting mistrust. An important concern regarding the absence of a significant impact of OCP exposure on trust in local governments in the 1991–2015 phase is people trying to express politically correct views. To address this possibility, we present the distributions of responses to the question of trust in local governments (Figure 3) in this period. Instead of coordinating on one "correct" answer to the question, the respondents' answers vary widely among the range. If there is such a "correct" response, then one would expect that 10 is the correct value. However, in fact, people's trust in local governments is not abnormally high. In our sample the mean value is smaller than the mean value of trust.

Also, we find a significant negative impact of OCP exposure on trust in local governments in the earlier phase when government officials were in charge of enforcing the policy, which further suggests respondents are willing to report their mistrust in local governments. Thus it is unlikely that people's fear to report mistrust in local governments drives our post-1991 phase results.

Trust in central government. The second concern about our estimates is that people do not trust the local governments less in the 1991–2015 phase because they treat them as merely the policy enforcer, not the policymaker. It is therefore important to see whether there is any effect on trust in the central government in this phase. However, the CFPS only surveyed people on their trust in local governments, not the central government. Even though we can potentially use trust in local governments to cautiously extrapolate trust in the central government (Cantoni et al., 2017), one might wonder how big the correlation is between the two trust measures. As an additional check, we construct two proxies for trust in the central government.

The first proxy is an indicator variable, where one represents the individual being a member of the Communist Party. Choosing to join the party *per se* reflects one's political attitudes and

beliefs.[13] The second proxy is the difference between the number of days an individual accesses political news through television and the number of days they access it through the internet. It is a well-established fact that all television broadcasters in China are the "mouthpiece" of the party (Zhao, 1998; Shirk, 2011), while internet censorship is much looser than media censorship. The focus of internet censorship is not to silence criticism of the government either (King et al., 2013), and internet users can browse international news channels that are not provided on television. So if one does not trust the central government, she is more motivated to access political news from a less censored media source—the internet. We calculate the difference in the media source to remove the variation in how much one cares about political news. The bigger the difference, the higher the level of trust in the central government.

From Table 7 across all columns, we do not find any significant difference in the impact of OCP exposure on trust in the central government between people whose first child was a girl and those whose first child was a boy. This suggests that people do not blame the local governments nor the central government.

Better performance in the second phase. A third concern is that even though we find more reductions in trust in local governments when the OCP was enforced more strictly in the first phase and there is no such effect in the second phase, the difference can be attributed to other factors rather than delegation. One potential factor is the government's performance. It could be that the governments in the second phase performed better regarding economic development or public goods provision, and that is why people do not attribute responsibility to them. However, for this story to explain our results, the performance of the local governments must be associated with the

[13]This measure certainly has its shortcomings. One issue is that people may choose to become a member of the Communist Party simply due to career concerns. Another issue is that people may lose their party membership due to OCP violations, which can also explain a negative correlation between OCP exposure and the probability of being a party member.

fertility penalty, which is our main measure of OCP exposure. As shown in Table 8, we do not find any significant correlation between the fertility penalty and measures of government performance, including GDP per capita, the urbanization rate, and public expenditure.

7 Conclusion

This paper provides field evidence on responsibility avoidance through delegation using evidence from China's one-child policy. Consistent with the predictions of the responsibility-shifting literature, we find that when local governments delegated the policy enforcement through incentivizing civilians to report their neighbors' violations of the policy, people who experienced more severe enforcement trust their neighbors—the delegate—less but do not trust the delegator—local governments—less. In contrast, when local government officials directly enforced the policy without delegation, parents strongly exposed to the OCP trust local governments less. OCP exposure does not undermine trust in neighbors in this situation. These results suggest that responsibility avoidance through delegation is effective not only in laboratory experiments but also in large-scale and highly influential field settings.

Governments often rely on the public to enforce unpopular policies by encouraging and incentivizing civilians to report their neighbors, friends, and coworkers to authorities. This tactic has been used throughout history, such as during Stalin's Great Purge when reports from acquaintances led many civilians to be sent to the Gulag (Fitzpatrick, 1999). During the COVID-19 pandemic, police forces and politicians in several countries (including the UK, Australia, Singapore, and Canada) have encouraged citizens to call law enforcement if they witness violations of social distancing rules.[14] While mass mobilization may help governments avoid responsibility for im-

[14] See news reports at https://www.bbc.com/news/world-52525463 and https://www.ctvnews.ca/health/coronavirus/people-are-reporting-on-their-neighbours-over-covid-19-concerns-1.4872683.

plementing unpopular policies due to grassroots information collection or police force shortages, our findings suggest that it comes at a significant cost: undermining generalized trust which is an essential component of social capital. Therefore, even self-interested governments should consider this social cost before deciding whether or not to mobilize the masses.

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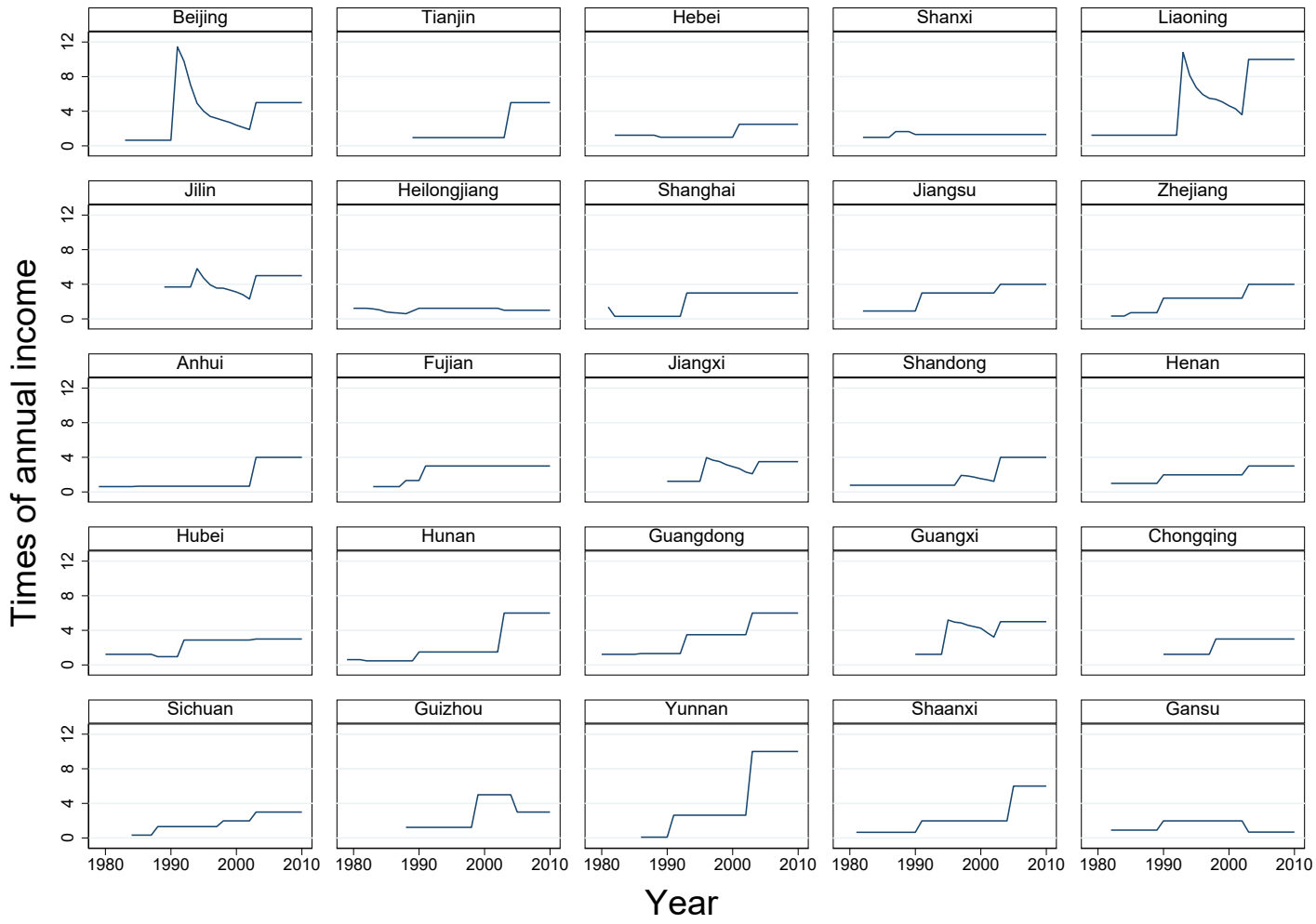
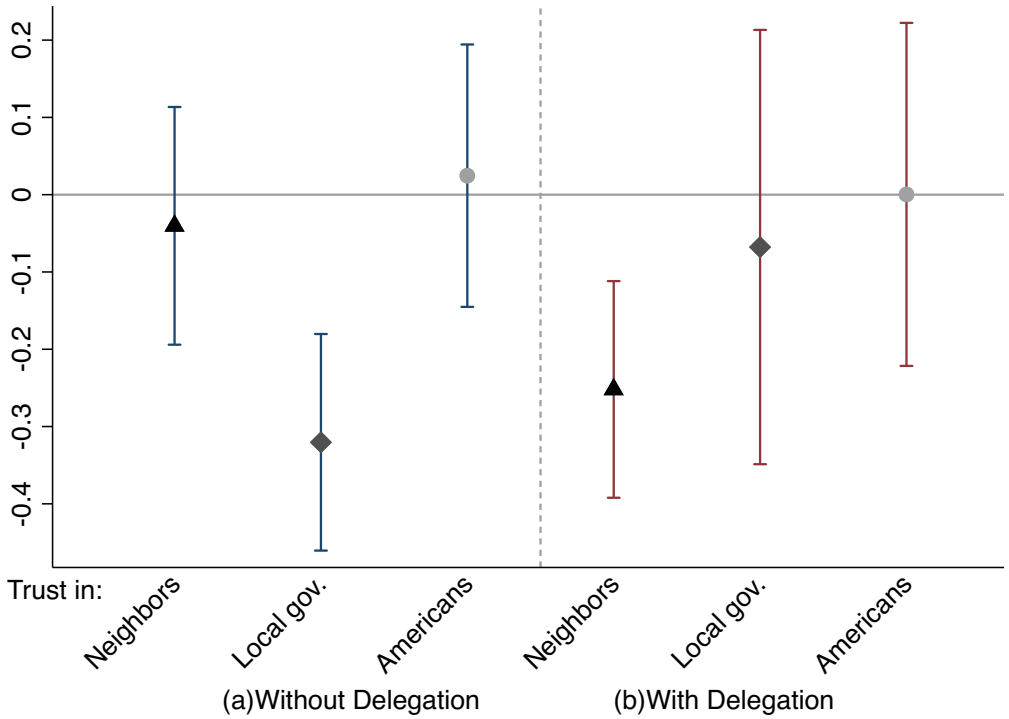
Figure 1: Provincial Fertility Penalties in Urban China



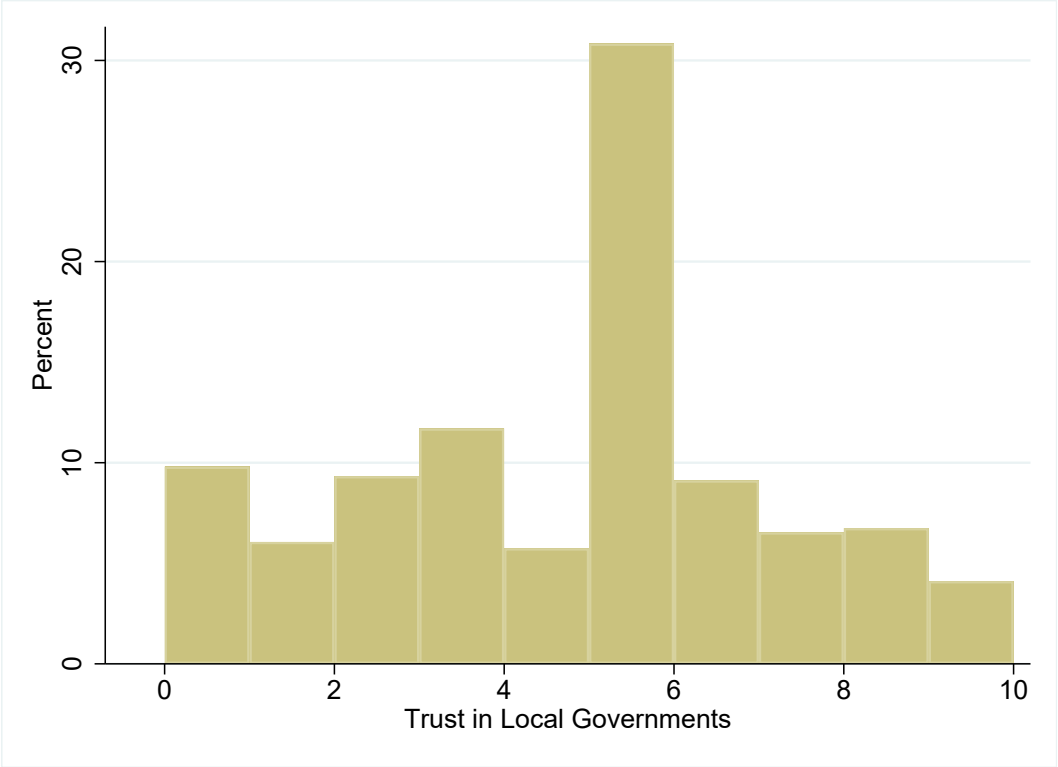
The figure plots fertility penalties across provinces and across time in urban China. The fertility penalties are measured as a multiple of annual household income. For example, number 4 on the y-axis means a household needed to pay 4 times the annual household income after being caught violating the OCP. Data source: Scharping (2013) and authors' calculation.

Figure 2: OCP Exposure and Trust



The figure plots coefficients and 95% confidence intervals for estimates of β_2 in equation 2. The coefficients are the difference in the effect of OCP exposure on trust between people whose firstborn is a daughter and those whose firstborn is a boy. Outcome variables are trust in neighbors, trust in local governments, and trust in Americans. Panel (a) plots coefficients when the policy was enforced without delegation, and Panel (b) plots coefficients when the policy was enforced with delegation. Standard errors are clustered at the province level.

Figure 3: Distribution of Responses: Trust in Local Governments



The figure plots the distribution of responses to the question of trust in local governments. Data source: CFPS (2016).

Table 1: Summary Statistics

Variable	Obs	Mean	Sd
Panel A: With delegation (1991–2015)			
Trust in neighbors	1,897	6.407	2.065
Trust in local governments	1,897	4.304	2.47
Trust in parents	1897	9.623	1.155
Trust in Americans	1889	2.163	2.328
Generalized trust	1897	0.605	0.489
OCP exposure	1,897	3.387	2.093
Age in 2016	1,897	42.78	6.002
Family income per capita (in thousands)	1,897	24.78	34.16
Education attainment	1,897	3.618	1.303
Male	1,897	0.458	0.498
Communist Party member	1,897	0.131	0.338
Employment status	1,897	0.842	0.364
Panel B: Without delegation (1979–1990)			
Trust in neighbors	822	6.582	2.072
Trust in local governments	822	4.957	2.502
Trust in parents	822	9.495	1.251
Trust in Americans	810	1.925	2.343
Generalized trust	822	0.533	0.499
Birth control rate	822	89.164	3.101
Age in 2016	822	59.78	3.683
Family income per capita (in thousands)	822	27.71	34.79
Education attainment	822	2.94	1.168
Male	822	0.474	0.5
Communist Party member	822	0.162	0.368
Employment status	822	0.337	0.473

Trust in neighbors and trust in local governments are categorical variables, where 0 represents extremely low trust and 10 represents extremely high trust. OCP exposure is in terms of annual household income. Employment status is a dummy variable where 1 represents employed and 0 represents unemployed or retired.

Table 2: Fertility Patterns in China

	Share of families with more than one child		Sex ratio of
	Firstborn: boy	Firstborn: girl	first birth
1990	0.479	0.538	1.052
2000	0.358	0.493	1.071
2005	0.285	0.418	1.024

Data are from the China Census 1% sample (1990), 0.95% sample (2000), and 1% sample (2005). We restrict our sample to married women who had their first child after 1979 and whose ages were between 21 and 40 in the census. We also restrict their matched children to those who were between ages 0 and 18 in the census.

Table 3: Factors That Predict First Child's Gender

Dependent variable:	Having a firstborn daughter	
	Probit	Linear prob.
	(1)	(2)
OCP exposure	0.0002	0.0002
	(0.010)	(0.010)
Age having 1st child	-0.001	-0.001
	(0.003)	(0.003)
Years of education	0.003	0.003
	(0.003)	(0.004)
Communist Party member	-0.002	-0.001
	(0.036)	(0.036)
Father's education	0.004	0.004
	(0.011)	(0.011)
Mother's education	-0.018	-0.018
	(0.014)	(0.014)
Province FE	Y	Y
Year FE	Y	Y
Observations	1,897	1,897

The table presents estimates of how observable characteristics predict the first child's gender. The dependent variable is whether the first child is a girl. OCP exposure is defined as the previous year's fertility penalty rate in province p before individual i had their first child. We control province fixed effects and birth-year fixed effects. Column 1 reports the marginal effects in the probit models.

Table 4: Estimates of OCP Exposure on Trust: With Delegation

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: Overall effect				
OCP exposure	-0.086**	-0.091**	-0.034	-0.038
	(0.019)	(0.017)	(0.039)	(0.032)
Provincial controls	Y	Y	Y	Y
R-squared	0.041	0.055	0.032	0.058
Panel B: Parents of firstborn daughter vs parents of firstborn son				
OCP exposure	0.045	0.008	-0.041	-0.062
	(0.064)	(0.061)	(0.080)	(0.090)
Firstborn daughter	-0.282***	-0.252***	-0.074	-0.068
×OCP exposure	(0.086)	(0.082)	(0.156)	(0.164)
Bootstrap p-value	[0.004]	[0.008]	[0.684]	[0.720]
Province FE	Y	Y	Y	Y
R-squared	0.089	0.108	0.078	0.105
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897

OCP exposure in the 1991–2015 phase is defined as the five-year mean value of the fertility penalty rates in province p after individual i had their first child. In Panel A, all regressions include birth-year fixed effects and provincial-level control variables in the baseline year (1991). Provincial variables include GDP per capita, population, and the unemployment rate. In Panel B, all regressions include province and birth-year fixed effects. We control individual-level variables in columns 2 and 4. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure to account for the small number of clusters (Cameron et al., 2008). We report the corresponding p-values in brackets. Number of clusters: 25.

Table 5: Estimates of OCP Exposure on Other Group-Specific Trust and Generalized Trust: With Delegation

Dependent variable:	Trust in Americans		Trust in Parents		Generalized Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
OCP exposure	-0.006	-0.048	-0.061	-0.069	0.017	0.014
	(0.095)	(0.091)	(0.071)	(0.072)	(0.012)	(0.013)
Firstborn daughter	-0.005	0.000	-0.005	0.002	-0.044***	-0.044***
×OCP exposure	(0.128)	(0.130)	(0.090)	(0.093)	(0.012)	(0.013)
Individual controls		Y		Y		Y
Birth-year FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Mean dependent variable	2.163	2.163	9.623	9.623	0.605	0.605
Standard deviation dependent variable	2.328	2.328	1.155	1.155	0.489	0.489
Observations	1,889	1,889	1,897	1,897	1,897	1,897
R-squared	0.115	0.168	0.084	0.101	0.053	0.083

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase is defined as the five-year mean value of the fertility penalty rates in province p after individual i had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. Number of clusters: 25.

Table 6: Estimates of OCP Exposure on Trust: Without Delegation

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: Overall effect				
OCP exposure	0.051*	0.054*	-0.072**	-0.064*
	(0.028)	(0.030)	(0.031)	(0.034)
Provincial controls	Y	Y	Y	Y
R-squared	0.050	0.077	0.044	0.063
Panel B: Parents of firstborn daughter vs parents of firstborn son				
OCP exposure	0.100	0.083	0.109	0.090
	(0.082)	(0.086)	(0.085)	(0.076)
Firstborn daughter	-0.067	-0.040	-0.338***	-0.320***
×OCP exposure	(0.087)	(0.090)	(0.074)	(0.082)
Bootstrap p-value	[0.552]	[0.78]	[0.004]	[0.004]
Province FE	Y	Y	Y	Y
R-squared	0.142	0.174	0.140	0.162
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Mean dependent variable	6.582	6.582	4.957	4.957
Standard deviation dependent variable	2.072	2.072	2.502	2.502
Observations	822	822	822	822

OCP exposure in the 1979–1990 phase is defined as the five-year average family-planning rates in province p after an urban resident had their first child. In Panel A, all regressions include birth-year fixed effects and provincial-level control variables in the baseline year (1979). Provincial variables include GDP per capita, population, and unemployment rate. In Panel B, all regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure to account for the small number of clusters (Cameron et al., 2008). We report the corresponding p-values in brackets. Number of clusters: 25.

Table 7: Estimates of OCP Exposure on Trust in Central Government: With Delegation

Dependent variable:	Party membership	Media source for political news
	(1)	(2)
OCP exposure	0.007	-0.324**
	(0.009)	(0.137)
Firstborn daughter	-0.002	0.063
×OCP exposure	(0.016)	(0.192)
Mean dependent variable	0.112	-1.506
Standard deviation dependent variable	0.316	3.440
Observations	1,897	1,386
R-squared	0.252	0.159

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable in column 1 is whether the individual is/was a Communist Party member. The dependent variable in column 2 is the difference between the number of days the individual accesses political news through television and the number of days they access it through the internet. All regressions include province fixed effects, birth-year fixed effects, and individual-level control variables. In parentheses are standard errors clustered by province. Number of clusters: 19.

Table 8: Correlation between Fertility Penalty and Government Performance

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

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. The regression includes year fixed effects. The urbanization rate is defined as the urban population share. The share of public expenditure is defined as the share of government expenditure on culture, education, health, etc. Robust standard errors are in parentheses. Number of clusters: 25.

Appendix

A Fertility Penalty Data

The fertility penalty data are taken from Scharping (2013), who provides an overall view of China's fertility policies and outcomes. Scharping draws on a large number of primary and secondary sources (statistics, laws, directives, internal documents, conferences, etc.) at local, national, and international levels, collected over ten years. Specifically, he documents the complete record of the published fine rates across provinces ranging from 1979 to 2000 (Table A1).[15] We modified the penalty data for seven provinces: Beijing, Inner Mongolia, Liaoning, Heilongjiang, Jiangxi, Shandong, and Guangxi. Furthermore, we extend the fine rates data from 2000 to 2010 using provincial government documents.[16]

There were mainly three forms of fines as documented by Scharping (2013). We transform all three forms into percentages of a household's annual income. The first type was collected as a percentage deduction from the wage over several years. We follow Ebenstein (2010)'s method by calculating the present value of the penalty at a 2% discount rate. For example, Jiangxi province reported that in 1996 an unauthorized birth carried a fine of 50% of a year's salary of both parents for 7 years. The fine's present value is then 3.30 years of income. The detailed calculation is as follows:

$$Penalty^{1980}_{Guangdong} = 0.5 + 0.5 \times (1-0.02) + 0.5 \times (1-0.02)^2 + ... + 0.5 \times (1-0.02)^6 = 3.30. \tag{A1}$$

[15]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*.

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

The second type of fines was collected as a share of annual income that must be paid in a single payment. For example, Fujian province employed a rule in 1991 that an unauthorized birth carried an immediate payment of three times the annual household income. In this case, no transformation is needed. The third type of fines was collected as an immediate payment of a certain amount of money independent of one's household income. 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) calculates the fine amount with the following assumptions: the fine was collected at the maximum amount of the range,[17] and the average annual household income was fixed at 10,000 RMB across province and time.[18] We can apply his rules to the Guangxi example. According to the first assumption, the maximum amount in the range, 50,000 yuan, is taken as the penalty amount. Then, based on the second assumption, the 50,000 yuan fine is equivalent to $\frac{50{,}000}{10{,}000} = 5$ times the annual household income.

For the third type of fines, instead of assuming that the annual household income was fixed at 10,000 RMB across province and year, we impute the fine into a share of household income using the provincial average annual household income in a certain year.[19] The income is only averaged at the provincial level because fines data are only available at this level and because we want to keep the units of data consistent. We add this variation of income across province and time for two reasons. First, there is a substantial variation of annual household income across provinces in China at that time. For instance, the annual household incomes were 4,630 yuan in Liaoning and 7,095 yuan in Beijing in 1993. In the same year, both provinces levied a 50,000 yuan fine for an unauthorized child. Clearly, the same amount of fines would not be the same for households in

[17] We also assume that the fine was collected at the maximum amount; hereafter we only discuss the maximum amount.

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

[19] The income data are taken from the China Statistical Yearbooks.

Liaoning and households in Beijing.

Second, there is also substantial time variation in annual household income within a province due to the rapid economic growth in the 1990s. For example, Beijing ratified the fine to 50,000 yuan from 1991 to 2000, while the average annual household income increased 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.

B One-Tailed T-Test for Sex Ratios

The publicly available data of sex ratios for the first birth in urban China is very limited: we have one year across provinces' sex ratios from the 2000 census. The small sample size leads to a lot of random variations in the sex ratios. Thus we cannot simply say provinces with sex ratios outside the normal range have severe sex selections.

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

$$D_i = \begin{cases} 1, & \text{if } i \text{ is a boy.} \\ 0, & \text{otherwise.} \end{cases} \tag{A2}$$

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

$$\hat{p} = \frac{\sum_{i=1}^{n} D_i}{n}. \tag{A3}$$

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

$$\sqrt{n}(\hat{p} - p) \equiv \frac{\sqrt{n}\sum_{i=1}^{n} D_i}{n} - p \xrightarrow{d} \mathbf{N}(0, p(1-p)). \tag{A4}$$

As a result,

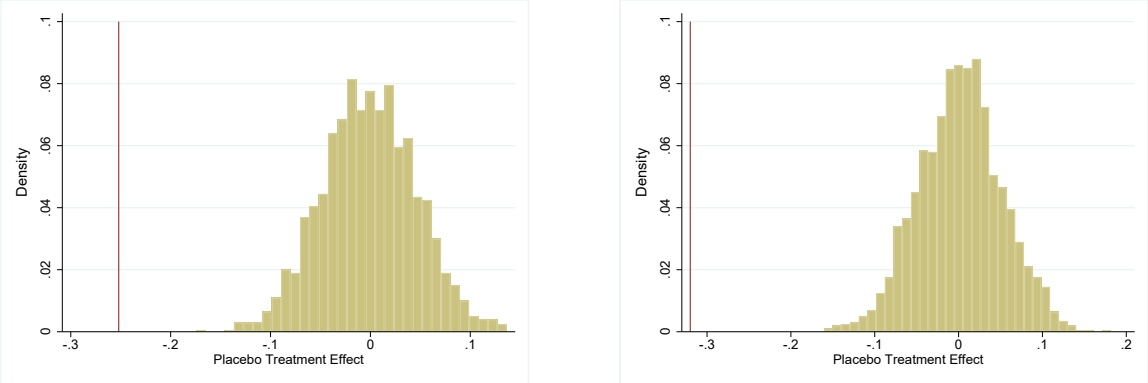
$$\frac{\sqrt{n}(\hat{p} - p)}{\sqrt{p(1-p)}} \xrightarrow{d} \mathbf{N}(0,1). \tag{A5}$$

We can thus construct the following t-statistic:

$$t \equiv \frac{\sqrt{n}(\hat{p} - 0.515)}{\sqrt{\hat{p}(1-\hat{p})}}. \tag{A6}$$

Since we are only concerned with the possibility that people endogenously choose boys over girls, we focus on a one-tailed t-test and reject the null hypothesis if $t > 1.645$. We find that six provinces' sex ratios at first birth were significantly higher than the normal sex ratio at the 5% level: Beijing, Jiangsu, Jiangxi, Hubei, Guangdong, and Guangxi (Table A3).

Figure A1: Pseudo-Treatment Effects



(a) Trust in neighbors in the 1991–2015 phase

(b) Trust in local gov. in the 1979–1990 phase

The figure plots the distribution of pseudo-treatment estimates of the effects of OCP exposure on trust using the main specification, with 2,000 random assignments of OCP exposure and the first child's gender. The red vertical lines mark the treatment effect using the actual OCP exposure and the first child's gender. Panel (a) reports the effects on trust in neighbors in the second phase (1991–2015), while Panel (b) reports the effects on trust in local governments in the first phase (1979–1990).

Table A1: Monetary Penalties for Unauthorized Births in Urban China, 1979–2015

Province	First report (1)	Second report (2)	Third report (3)	Fourth report (4)
Beijing	1982.10: 7Y, 10%	1991.5: 5,000-50,000¥	2002.12: 1Y, 300%-500%	
Tianjin	1988.11: 5Y, 20%	2003.9: 200%-500%		
Hebei	1982.4: 14Y, 10%	1989.3: 1Y, 100%	2001.3: 1Y, 250%	
Shanxi	1982.6: 7Y, 15%	1986.12: 7Y, 25%	1989.9: 7Y, 20%	2002.9: 7Y, 20%
Inner Mongolia	1982.6: 14Y, 10%	1995.11: 2,000-20,000¥		
Liaoning	1979.6: 14Y, 10%	1992.9: 5,000-50,000¥	2003.4: 1Y, 500%-1,000%	
Jilin	1988.7: 14Y, 10%-30%	1993.10: 1Y, 5,000-30,000¥	2002.9: 1Y, 200%-500%	
Heilongjiang	1979.9: 14Y, 10%	1983.1: >1,200¥	1989.12: 14Y, 10%	2003.1: 1Y, 100%
Shanghai	1981.7: 16Y, 10%	1982.6: 3Y, 10%	1992.10: 1Y, 300%	
Jiangsu	1982.6: 7Y-10Y, 10%	1990.10: 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%	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%	2004.2: 1Y, 350%		
Shandong	1980.3: 4Y, 20%	1996.10: 6,000-20,000¥	2002.9: 1Y, 300%-400%	
Henan	1982.6: 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%	
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%	2002.7: 1Y, 300%-600%
Guangxi	1994.11: 2,000-50,000¥	2002.9: 1Y, 300%-500%		
Hainan	1989.3: 7Y, 20%	1995.10: 1Y, 200%-300%		
Chongqing	1997.9: 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%	2002.9: 1Y, 500%-1,000%	
Tibet	1986.5: 1,000¥	1992.5: 3,000¥		
Shaanxi	1981.4: 7Y, 10%	1991.3: 7Y, 15%-30%	1997.8: 7Y, 20%-30%	2004.8: 1Y, 300%-600%
Gansu	1982.3: 10Y, 10%	1989.11: 7Y, 30%	2002.9: 3.5Y, 20%	
Qinghai	1982.6: 7Y, 10%	1992.2: 7Y, 25%	2004.2: 1Y, 300%-500%	
Ningxia	1982.9: 14Y,10%	1990.12: 14Y, 10%-30%		
Xinjiang	1988.4: 7Y, 10%	1991.8: 14Y, 10%-30%		

Data source: Scharping (2013) and authors' collection. Column n represents the nth report of the fertility penalty issued by provincial governments. Each report contains two pieces of information: at what time the fine came into effect and the amount of the fine. When the fine is a lump-sum payment independent of the household income, we represent the amount by a range of Chinese Yuan. When the fine is a percentage deduction from the family income, we present the amount by both the number of years the fine was charged and the percentage of household annual income a family needed to pay in each of those years.

Table A2: Correlation between Fertility Penalty and the Family-Planning Rate

Dependent variable:	Family-planning rate
Fertility penalty	0.696***
	(0.211)
Province FE	Y
Observations	279
R-squared	0.723

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are in parentheses. The sample is a province-level panel (unbalanced) from 1979 to 1992, when both fertility penalties and family-planning rates are available.

Table A3: Sex Ratio at First Birth (Males/Females) in Urban China in 2000

Province	Obs	Sex ratio	One-tailed t-statistics
Beijing	**4,579**	**1.130**	**2.156**
Tianjin	2,282	1.063	0.074
Hebei	8,356	1.090	1.277
Shanxi	5,582	1.081	0.742
Liaoning	10,525	1.061	0.063
Jilin	4,940	1.104	1.426
Heilongjiang	7,108	1.079	0.747
Shanghai	5,518	1.089	1.014
Jiangsu	**12,635**	**1.099**	**2.039**
Zhejiang	8,938	1.072	0.526
Anhui	6,499	1.082	0.816
Fujian	5,182	1.091	1.044
Jiangxi	**4,367**	**1.178**	**3.489**
Shandong	17,607	1.080	1.253
Henan	11,090	1.076	0.808
Hubei	**9,403**	**1.124**	**2.821**
Hunan	7,072	1.052	-0.309
Guangdong	**20,607**	**1.166**	**6.825**
Guangxi	**4,418**	**1.131**	**2.160**
Chongqing	4,010	1.022	-1.150
Sichuan	8,763	1.098	1.644
Guizhou	3,886	0.988	-2.201
Yunnan	4,056	1.004	-1.729
Shaanxi	4,365	1.060	-0.002
Gansu	3,240	1.058	-0.042

Data source: China Census 10% sample (2000).

Table A4: Estimates of OCP Exposure on Trust with Delegation: Excluding High Sex Ratio Provinces

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
OCP exposure	0.051	-0.002	-0.052	-0.092
	(0.066)	(0.062)	(0.094)	(0.106)
Firstborn daughter	-0.303***	-0.258***	-0.084	-0.058
×OCP exposure	(0.089)	(0.085)	(0.176)	(0.192)
Bootstrap p-value	[0.004]	[0.004]	[0.676]	[0.796]
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean dependent variable	6.388	6.388	4.194	4.194
Standard deviation dependent variable	2.065	2.065	2.499	2.499
Observations	1,509	1,509	1,509	1,509
R-squared	0.102	0.122	0.089	0.122

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase is defined as the five-year mean value of the fertility penalty rates in province p after individual i had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. We use a wild cluster bootstrap-t procedure to account for the small number of clusters (Cameron et al., 2008). We report the corresponding p-values in brackets. We also report the p-values using OLS. Number of clusters: 19.

Table A5: Estimates of Alternative Measure of OCP Exposure on Trust: Five-Year Minimum

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: With delegation				
OCP exposure (min)	0.077	0.058	-0.048	-0.057
	(0.078)	(0.073)	(0.063)	(0.066)
Firstborn daughter	-0.220***	-0.209***	-0.017	-0.016
×OCP exposure (min)	(0.073)	(0.068)	(0.122)	(0.122)
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.087	0.106	0.077	0.105
Panel B: Without delegation				
OCP exposure (min)	0.078	0.070	0.165**	0.145**
	(0.068)	(0.072)	(0.059)	(0.062)
Firstborn daughter	-0.022	0.013	-0.227***	-0.197**
×OCP exposure (min)	(0.100)	(0.112)	(0.053)	(0.071)
Mean dependent variable	6.582	6.582	4.957	4.957
Standard deviation dependent variable	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.141	0.174	0.135	0.157
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase (with delegation) is defined as the minimum of the five-year fertility penalty rates in province p after individual i had their first child. OCP exposure in the 1979–1990 phase (without delegation) is defined as the minimum of the five-year family-planning rate in province p after an individual had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. Number of clusters: 25.

Table A6: Estimates of Alternative Measure of OCP Exposure on Trust: Four-Year Mean

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: With delegation				
OCP exposure (4-year mean)	0.072	0.039	-0.009	-0.029
	(0.062)	(0.058)	(0.080)	(0.090)
Firstborn daughter	-0.303***	-0.276***	-0.102	-0.097
×OCP exposure (4-year mean)	(0.087)	(0.083)	(0.162)	(0.170)
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.089	0.108	0.078	0.105
Panel B: Without delegation				
OCP exposure (4-year mean)	0.075	0.061	0.088	0.076
	(0.061)	(0.064)	(0.066)	(0.060)
Firstborn daughter	-0.065	-0.045	-0.250***	-0.236***
×OCP exposure (4-year mean)	(0.072)	(0.075)	(0.060)	(0.066)
Mean dependent variable	6.582	6.582	4.957	4.957
Standard deviation dependent variable	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.141	0.173	0.139	0.161
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase (with delegation) is defined as the four-year mean value of the fertility penalty rates in province p after individual i had their first child. OCP exposure in the 1979–1990 phase (without delegation) is defined as the four-year average family-planning rate in province p after an urban resident had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. Number of clusters: 25.

Table A7: Estimates of Alternative Measure of OCP Exposure on Trust: Six-Year Mean

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: With delegation				
OCP exposure (6-year mean)	0.036	-0.004	-0.076	-0.099
	(0.069)	(0.067)	(0.074)	(0.084)
Firstborn daughter	-0.273***	-0.240***	-0.032	-0.025
×OCP exposure (6-year mean)	(0.082)	(0.079)	(0.136)	(0.144)
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.088	0.108	0.078	0.105
Panel B: Without delegation				
OCP exposure (6-year mean)	0.144	0.126	0.152	0.128
	(0.111)	(0.117)	(0.117)	(0.106)
Firstborn daughter	-0.075	-0.042	-0.439***	-0.418***
×OCP exposure (6-year mean)	(0.107)	(0.110)	(0.102)	(0.111)
Mean dependent variable	6.582	6.582	4.957	4.957
Standard deviation dependent variable	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.143	0.175	0.140	0.162
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase (with delegation) is defined as the six-year mean value of the fertility penalty rates in province p after individual i had their first child. OCP exposure in the 1979–1990 phase (without delegation) is defined as the six-year average family-planning rate in province p after an urban resident had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. Number of clusters: 25.

Table A8: Estimates of OCP Exposure on Trust: Clustering at the Birth Cohort Level

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: With delegation				
OCP exposure	0.045	0.008	-0.041	-0.062
	(0.089)	(0.091)	(0.094)	(0.091)
Firstborn daughter	-0.282**	-0.252**	-0.074	-0.068
×OCP exposure	(0.124)	(0.124)	(0.146)	(0.135)
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.089	0.108	0.078	0.105
Panel B: Without delegation				
OCP exposure	0.100*	0.083*	0.109	0.090
	(0.052)	(0.043)	(0.099)	(0.106)
Firstborn daughter	-0.067	-0.040	-0.338***	-0.320***
×OCP exposure	(0.092)	(0.093)	(0.113)	(0.104)
Mean dependent variable	6.582	6.582	4.957	4.957
Standard deviation dependent variable	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.142	0.174	0.140	0.162
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase is defined as the five-year mean value of the fertility penalty rates in province p after individual i had their first child. OCP exposure in the 1979–1990 phase is defined as the five-year average family-planning rate in province p after an urban resident had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered at the cohort level.

Table A9: Estimates of OCP Exposure on Trust: Two-Way Clustering

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Panel A: With delegation				
OCP exposure	0.045	0.008	-0.041	-0.062
	(0.093)	(0.091)	(0.091)	(0.098)
Firstborn daughter	-0.282**	-0.252**	-0.074	-0.068
×OCP exposure	(0.113)	(0.111)	(0.176)	(0.180)
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.089	0.108	0.078	0.105
Panel B: Without delegation				
OCP exposure	0.100	0.083	0.109	0.090
	(0.078)	(0.077)	(0.108)	(0.110)
Firstborn daughter	-0.067	-0.040	-0.338***	-0.320***
×OCP exposure	(0.105)	(0.106)	(0.094)	(0.089)
Mean dependent variable	6.582	6.582	4.957	4.957
Standard deviation dependent variable	2.072	2.072	2.502	2.502
Observations	822	822	822	822
R-squared	0.142	0.174	0.140	0.162
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase (with delegation) is defined as the five-year mean value of the fertility penalty rates in province p after individual i had their first child. OCP exposure in the 1979–1990 phase (without delegation) is defined as the five-year average family-planning rate in province p after an urban resident had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered at both the province and cohort level.

Table A10: Estimates of OCP Exposure on Other Group-Specific Trust and Generalized Trust: Without Delegation

Dependent variable:	Trust in				Generalized	
	Americans		Parents		Trust	
	(1)	(2)	(3)	(4)	(5)	(6)
OCP exposure	-0.052	-0.056	-0.025	-0.035	0.015	0.019*
	(0.058)	(0.048)	(0.045)	(0.039)	(0.010)	(0.011)
Firstborn daughter	-0.014	0.025	0.024	0.055	-0.024	-0.026
×OCP exposure	(0.113)	(0.099)	(0.064)	(0.042)	(0.016)	(0.018)
Individual controls		Y		Y		Y
Birth-year FE	Y	Y	Y	Y	Y	Y
Province FE	Y	Y	Y	Y	Y	Y
Mean dependent variable	1.925	1.925	9.495	9.495	0.533	0.533
Standard deviation dependent variable	2.343	2.343	1.251	1.251	0.499	0.499
Observations	810	810	822	822	822	822
R-squared	0.150	0.168	0.137	0.173	0.092	0.115

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. OCP exposure in the 1991–2015 phase is defined as the five-year mean value of the fertility penalty rates in province p after individual i had their first child. OCP exposure in the 1979–1990 phase is defined as the five-year average family-planning rate in province p after an urban resident had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. Number of clusters: 25.

Table A11: Estimates of Pre-OCP Exposure on Trust

Dependent variable:	Trust in			
	Neighbors		Local governments	
	(1)	(2)	(3)	(4)
Pre-OCP exposure	-0.017	-0.047	-0.100*	-0.113*
	(0.083)	(0.078)	(0.058)	(0.058)
Firstborn daughter	-0.084	-0.064	0.041	0.033
×OCP exposure	(0.111)	(0.111)	(0.078)	(0.084)
Individual controls		Y		Y
Birth-year FE	Y	Y	Y	Y
Province FE	Y	Y	Y	Y
Mean dependent variable	6.407	6.407	4.304	4.304
Standard deviation dependent variable	2.065	2.065	2.470	2.470
Observations	1,897	1,897	1,897	1,897
R-squared	0.083	0.103	0.078	0.105

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. This table reports the impact of the pre-OCP exposure on trust in the 1991–2015 phase. Pre-OCP exposure is defined as the five-year mean value of the fertility penalty rates in province p before individual i had their first child. All regressions include province and birth-year fixed effects. In parentheses are standard errors clustered by province. Number of clusters: 25.