A Grand Socioeconomic Reshuffle: The One-Child Policy and Intergenerational Mobility in China*

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Abstract

We examine whether and how the world's largest population planning program, the One-Child Policy, has shaped intergenerational mobility in China. Using a dataset with 2,096,798 child-parent(s) pairs combined from various rounds of ten separate national household surveys, we leverage exogenous variation in fine rates imposed for One-Child Policy violations across provinces to study causal impacts of the One-Child Policy on intergenerational persistence. Using a continuous difference-in-differences approach, we find that for cohorts born between 1980 and 1996, the One-Child Policy reduced persistence in intergenerational income, education, and social class, comparing to those born prior to 1979. We estimate that the overall effect of the One-Child Policy fines was to reduce persistence in intergenerational income, education, and social class by 28.1%, 48.7%, and 24.8%, respectively. Analyzing mechanisms, we find that the One-Child Policy boosted China's intergenerational mobility by diminishing elite family heirship, concentrating resources for lower-income families, and decreasing returns to education. The One-Child Policy has brought about a significant socioeconomic reshuffle that has reshaped the role of China's long-standing class solidification.

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Do those gentry certainly have blue blood?
— In 209 B.C., by Chen Sheng,
a well-known leader of a large peasants' revolt,
recorded in *Historical Records,*
the Hereditary House of Chen She[4]

1. Introduction

One key indicator of a society's equality and a nation's ability to sustain economic growth is intergenerational mobility, or the ability to alter one's socioeconomic standing. Higher intergenerational mobility enhances equality, stability, and production in a society (Bell et al., 2019).

China provides an informative case study for researching intergenerational mobility. In contrast to many other industrialized and developing nations, China has historically had lower levels of intergenerational income mobility, dating back to millennia of aristocracy (Qin, Wang, and Zhuang, 2016).[5] In periods between revolutions, Chinese generations have generally lived in a society with cemented classes with extremely low mobility (Bian, 2002). Following uprisings by lower classes or the ascension of a new emperor, a grand class reshuffle starts, and a new elite class is built, typically leading to a temporary increase in intergenerational mobility (Tanner and Feder, 1993; Schmidt-Glintzer and Jansen, 1994; Feng, 2010; Lipset and Bendix, 2018). However, despite a lack of uprisings or ascensions, there has been an upsurge in intergenerational mobility in China in recent years (Fan, Yi, and Zhang, 2021). Understanding the underlying causes of this observed pattern of increased mobility is crucial because it has the power to end a protracted cycle of recursive revolution and class solidification (Bütikofer, Dalla-Zuanna, and Salvanes, 2018).

Family size is a significant element that has been linked to mobility (Berent, 1952; Boggs, 1957; Gittleman and Joyce, 1999; Bavel et al., 2011). Chinese households were frequently exceedingly large until the late twentieth century due to an elite culture's pro-natalist ideals, which were contained in the traditions of Chinese familism (*chuan cheng xiang huo*) and honored early

[4] The text in Chinese is *wang hou jiang xiang ning you zhong hu* (王侯将相，宁有种乎*?). Historical Records* were written by Sima Qian during 104–118 B.C. They are the representative of the Chinese ancient books and have been widely passed down through the generations.

[5] Intergenerational income elasticity estimates for many nations are shown in Table 1 of Qin, Wang, and Zhuang (2016) based on the research of a number of academics.

marriage and abundant offspring (Ebery, 1978; Scharping, 2003).

However, this "big family" tradition was abruptly altered in 1979 with the introduction of the One-Child Policy. The One-Child Policy is the largest family planning program in the world (Huang, 2017) and may be the largest social experiment in history (Li, Yi, and Zhang, 2011). After 1979, couples who had more than a single child (with some exceptions) were subject to severe punishments such as heavy monetary fines, resulting in a dramatic shift in the typical family structure from multiple children to a single child. Millions of households had their family size set and controlled by the One-Child Policy over the duration of the policy's 36-year operation. Consequently, the One-Child Policy has had wide-ranging impacts on many aspects of Chinese society (Ebenstein, 2010). In particular, the new single-child family structure may have severely undermined China's class solidification and resulted in a restructuring of the intergenerational transmission of income, education, and social class.[6]

In this study, we identify the causal effect of the One-Child Policy on the intergenerational persistence of children's income, education, and social class relative to their parents. We use rank-rank regression models to characterize intergenerational persistence by comparing the rank of the parent to that of the child (Chetty et al., 2014), focusing on cohorts born three, five, and ten years before and after the implementation of the One-Child Policy and all-sample child cohorts born before and after.[7] To identify the causal effect of the One-Child Policy, we conduct a difference-in-differences analysis exploiting province-level variation in the rate of monetary fines imposed on parents for violating the policy.[8] We combine ten distinct data sources to construct a dataset with 2,096,978 observations, covering cohorts spanning over fifty years (1949–1996) and multiple waves of data collection. The size and breadth of our sample allows us to analyze parent-child pairs by cohort and year, while controlling for age to attenuate lifecycle bias (Chetty et al., 2014;

[6] Based on an individual's governmental position and annual income range, we divide one's social class into one of the five categories—high, middle-high, middle, middle-lower, or lower class. Section 3.2 contains more information.
[7] Our all-sample child cohorts contain those who were born between 1949, the year when the People's Republic of China was founded, and 1996, the year after which the child did not reach the minimum wage-earning age in our sample.
[8] The approach of using the monetary fines as the proxy of the strength of the One-Child Policy is well supported in the literature (Hardee-Cleaveland and Banister, 1988; McElroy and Yang, 2000; Li, Zhang, and Zhu, 2005; Ebenstein, 2010; Liu, 2014; Huang and Zhou, 2015; Huang, Lei and Zhao, 2015; Huang, Lei, and Sun, 2021), as "though the monetary penalty is only one aspect of the policy, and the government may take other administrative actions (e.g. loss of party membership or employment), it is still a good proxy for the policy because an increase in fines is usually associated with other stricter policies (Huang, 2017)". We discuss more detail about the randomness of the variations in the monetary fines in Section 2.2.

Emran and Shilpi, 2019). The empirical results show that, in comparison to cohorts born before 1979, the One-Child Policy lowered persistence in average income, education, and social class by 28.1%, 48.7%, and 24.8%, respectively, for cohorts from 1980 to 1996. Different sub-groups, including daughters, sons, rural residents, urban dwellers, residents in the three different regions across China, exhibit similar drops in intergenerational persistence.

We conduct numerous robustness tests on this result. We assess robustness to identification assumptions by assessing pre-trends, including possible effects of the birth control policy implemented prior to the One-Child Policy, placebo tests, tests for treatment heterogeneity, and spillover of the One-Child Policy on cohorts born before 1979 but affected by having fewer siblings or being the only child. We evaluate alternative explanations for the result, including other contemporaneous policies in China, and explore alternative measures of both parents' rank and the One-Child Policy fine rates. We explore external validity by investegating different subsamples of the data, including changing the policy interim period; gender-balanced samples; samples containing surveys that have addressed the co-residency issue, which has to do with whether the parent-child pair surveyed lives in the same residence; samples with parents who experienced China's Great Famine before giving birth and those who did not; samples with children who inter-provincially migrated off their birth province and those who did not migrate; and reweighing the models by cohort size.[9] We find that results of each of these tests support our main conclusions.

Finally, we theoretically and empirically investigate the potential for our finding that the One-Child Policy has reduced intergenerational socioeconomic persistence. We begin by developing a theoretical model to investigate how the One-Child Policy could affect intergenerational persistence. We then focus on four hypotheses related to child number, assortative mating, resource concentration, and returns to education. These hypotheses inform corresponding empirical investigations to detect the presence of these mechanisms. Results suggest that a variety of channels, including weakening of elite family heirship (including a decline in the fertility of political elite families and in assortative mating), resource concentration for lower-income families, and a reduction in returns to education may each account for how the One-Child Policy alters intergenerational mobility.

A key strength of our work is that we substantially expand the number of observations, which is

[9] China's Great Famine (1958–1961) has significant negative impact on people's fertility physically and spiritually (Shi, 2011). See details in Section 5.2.4.

vital and often challenging for studies focusing on the developing world, relative to previous work. Studies that examine mobility in developed nations typically rely on administrative tax records, but as developing nations often lack comparable datasets, statistical power for studying mobility in such cases is often limited (Emran, Greene, and Shilpi, 2018; Emran and Shilpi, 2019). In developing countries, administrative tax databases often only cover a small and unrepresentative portion of the population, and datasets linking parent and child wages or incomes are rare; the lack of abundant and reliable data results in less reliable inferences that are sensitive to selection criteria (Couch and Lillard, 1998; Qin, Wang, and Zhang, 2016). Among recent papers, Mohammed (2019) uses single-year income data totaling 1,779 observations for parent-child pairs to estimate intergenerational mobility in rural India; Fan, Yi, and Zhang (2021) calculate intergenerational income elasticity in China using 10,980 observations covering the 1970–1980 birth cohorts as the "early cohort" versus 11,333 observations for the 1980–1988 cohorts as the "late cohort". In the two studies most similar to the current study, Zou (2015) and Yu, Fan, and Yi (2021) examine the impact of the One-Child Policy on intergenerational mobility using 77,658 and 25,618 observations, respectively. We address the concern about data limitation by constructing a dataset comparable in size to those used by developed-country mobility research (*e.g.*, Alesina, Stantcheva, and Teso, 2018; Feigenbaum, 2018), composed of 2,096,978 observations spanning over five decades.

Our work contributes to the literature linking family size with the transmission of socioeconomic status between generations. Numerous studies in economics, sociology, and evolutionary anthropology have argued that larger families have detrimental effects on children's outcomes. For example, they argue that larger families in modern societies have fewer opportunities for intergenerational upward mobility because of resource dilution or because the average household member is less mature (Ajami, 1969; Sloan and Theodossiou, 1996; Black, Devereus, and Salvanes, 2005; Kulu, 2008; Van Bavel et al., 2011).[10] The quantity-quality model is a widely used economic model in this literature stream.[11] Its fundamental component is an interaction between quantity and

[10] Arsène Dumont, in his 1890 classic *Dépopulation et Civilisation*, argues that adults with ambition tend to limit their family size because having many children is equivalent to "making inconvenient luggage"; in other words, having many children hinders success and achievements and exacerbates or complicates the social situation for the next generation. Scholars also hold that the Western world's decline in fertility is the consequence of parents' ambitions for their children's success and their own (Banks, 1954; Aries, 1980; Zuanna, 2007).

[11] The quantity-quality model, developed in Becker (1960) and expanded in Becker and Lewis (1973) and Becker and Tomes (1976), is a classic model to explain the observed negative correlation between family income and family size.

quality in the budget constraint that results in increasing marginal costs of quality with regard to family size.[12] However, most earlier research focused on incomplete empirical data and the unresolved prerequisite—the choice to have children—which is difficult to rule out as endogeneity (Black, Devereux, and Salvanes, 2005; Van Bavel et al., 2011). It is therefore uncertain whether intergenerational mobility is directly impacted by family size, or the characteristics that lead to smaller families also have effects on mobility. China's One-Child Policy acts as an exogenous shock to the decision of family size in our work, making it an ideal natural experiment to address the causality conundrum.

Our work also relates to the literature on intergenerational mobility. Intergenerational mobility research in economics starts with Becker and Tomes (1979) and Becker and Tomes (1986), who construct an altruistic utility function and provide the field a theoretical foundation. Recent research by Lee and Solon (2009), Chetty et al. (2014), Clark (2014), Becker et al. (2018), Alesina et al. (2019) raises the possibility that the level of intergenerational mobility is both low and stubbornly stable over time and across nations. If this is the case, social and economic policies aimed at promoting mobility may have little impact, and the current inequality will inevitably be repeated in succeeding generations. We add to the body of literature by showing a counterexample of the positive effect of the One-Child Policy on China's mobility.

Specifically, this work contributes to the underrepresented area of intergenerational mobility research that uses a natural experiment to pinpoint the causes of intergenerational persistence. Shea (2000), Oreopoulos, Page, and Stevens (2008), and Rege, Telle, and Votruba (2011) concentrate on the role of income shocks caused by a parent's labor market status; Morris, Duncan, and Rodrigues (2004) and Dahl and Lochner (2012) examine the effect of income provided through welfare programs and tax credit benefits on children's outcomes; Bütikofer, Dalla-Zuanna, and Salvanes (2020) examine the effect of the Norwegian oil boom in the 1970s on intergenerational mobility by exploiting variations across local labor markets in their exposure to the resource shock; and Fan, Yi, and Zhang (2021) compare the impact of China's economic reform since 1979 on the intergenerational elasticity in income for the birth cohort of 1970–1980 versus 1981–1988. For the two related studies on One-Child Policy effects, Zou (2015) finds that fertility has a negative causal

[12] The interaction item generates a trade-off between quantity and quality, in which quality is measured by the current and future well-being of the children, including their income when they become adults (Black, Devereux, and Salvanes, 2005; Zuanna, 2007; Becker, Stanley, and Becker, 2009; Van Bavel et al., 2011).

effect on intergenerational income and educational mobility when the One-Child Policy resistance rate interacts with a dummy variable (whether one cohort was exposed to the One-Child Policy); similarly, Yu, Fan, and Yi (2021) find a negative causal effect of fertility on intergenerational income mobility using differences in One-Child Policy exposure between women in rural and urban areas; both results are consistent with ours. We extend the research of these two related studies by substantially increasing the number of observations and producing accurate intergenerational rankings for yearly cohort-based analysis. Our current study is also the first to develop and apply a theoretical framework that aligns with our empirical results on the causal relationship between the One-Child Policy and intergenerational income, education, and social class persistence; in addition, we are also the first to examine the mechanisms that drive the impacts of the One-Child Policy. Our approaches extend beyond the straightforward implications of population change and add to the body of research on One-Child Policy impacts both theoretically and empirically. We also generate the outcome of intergenerational social class persistence, which incorporates components such as position and wealth, reflecting social standing with Chinese characteristics.

Finally, our analysis advances knowledge of the mechanisms underlying the transmission of socioeconomic status through generations. The literature examines nature versus nurture effects that drive intergenerational mobility. The nature effect refers to the genetic connections between generations within a family (Black, Devereux and Salvanes, 2009; Anger and Heineck, 2010; Björklund, Eriksson and Jäntti, 2010; Gronqvist, Öckert and Vlachos, 2017), and the nurture effect is found to be influential in shaping children's outcomes, with human capital playing a significant role (Black, Devereux, and Salvanes, 2005; Mogstad, 2017; Li, Stephan, and Weber, 2018; Smith et al., 2019; Fagereng, Mogstad, and Rønning, 2021). Researchers also investigate other channels, such as ethnic capital (Borjas, 1992), redistribution preferences (Alesina, Stantcheva, and Teso, 2018), the neighborhood effect (Chetty and Hendren, 2018 a,b), financial behaviors (Black et al., 2020), and differential fertility (Yu, Fan, and Yi, 2021). Extending the fertility differentiation mechanism by including more precise and specific intermediates (such as diminishing elite family heirship, concentrating resources for poor families, and decreasing returns to education) allowed us to explain how fertility differentiation influences intergenerational transmission.

Despite the policy receiving harsh criticism for interfering with fundamental human reproductive rights, it is important to document how the policy has increased intergenerational

mobility. This was in part due to the fact that it severely restricted family size, breaking China's tradition of inherited elite class. China's One-Child Policy implementation can serve as an example for other large-population countries with established social standing to thwart potential violent regime changes.

The rest of the paper is organized as follows. Section 2 presents a theoretical model of the One-Child Policy's impact on intergenerational persistence and its mechanisms. Section 3 discusses the data construction and provides descriptive statistics. The empirical strategy is described in Section 4. Section 5 describes the findings and includes robustness tests. Section 6 provides the historical context for China's One-Child Policy and fine rates, as well as data on intergenerational mobility for one-child families. Section 7 presents empirical evidence on the underlying mechanisms. Section 8 concludes.

2. Background

In this section, we first provide an overview of the One-Child Policy's institutional context, and the formulation of the One-Child Policy fine rates. Then, we demonstrate the relationship between intergenerational mobility and fertility.

2.1. The institutional background of the One-Child Policy (OCP)

After the two decades of pro-birth by the Mao-era government, the successive Chinese Communist Party (CCP) leaders decided to contain the population growth by implementing fertility control measures in the 1970s (Scharping, 2003; Ebenstein, 2010). China's central government promulgated the One-Child Policy (OCP) in 1979, which is the first fertility control measure that has been written into the law (Banister, 1991; Scharping, 2003).[13] Differing from its predecessors and many family planning policies in other countries, the OCP 1) allowed a married couple to have one and only one child, 2) was mandatory rather than voluntary, and 3) imposed penalty on violators.[14]

[13] China's OCP was first announced in 1978 and was enshrined in the amended Constitution in 1982 (Huang and Zhou, 2015; Zhang, 2017).

[14] There were certain birth quota exemptions based on place of residence (urban/rural) and ethnicity (Han/non-Han). For example, the one-and-a-half-child policy allowed Han households in rural areas to have a second child after four to six years if the first child was a female; non-Han households were permitted to have two or three children in the majority of regions (Huang, 2017; Huang, Lei, and Sun, 2021). Gu et al. (2007) calculated that 63% of couples would have just one child, 36% would have two children, and only 1% would have three or more children if all couples under different policy regimes fully adhered to the OCP.

The CCP leaders regarded direct financial penalties as a potent tool for reducing childbearing, and these financial penalties often equaled a sizable fortune for an average household given the limited wealth available at that time (Feng and Hao,1992; McElroy and Yang, 2000; Li, Zhang, and Zhu, 2005). At the very beginning of the OCP, Vice Premier *Chen Muhua*, specifically assigned to head the OCP, proposed to pass new legislation to penalize unauthorized births (Huang, Lei, and Sun, 2021). The central government also set suggested fertility penalties to the provincial governments (Scharping, 2003; Huang and Zhou, 2015). In the actual implementation of the OCP, public resistance and complaint arose (Huang, Lei, and Sun, 2021). The central government thus issued Document 11 in February 1982 to authorize the provincial governments to revise the penalty rules and then issued Document 7 in 1984 to authorize provincial governments to set their own fine rates (Gu et al., 2007).[15] However, because social stability was a key assessment indicator for provincial officials, the provincial governments were little incentivized to design a high fine rate (Scharping, 2003; Huang, Lei, and Sun, 2021).[16] As a result, the OCP fine rates in most provinces virtually unchanged from 1979 to 1989, and these rates in some provinces even decreased after the authorization of fine rate setting (Figure A1).

The OCP fine rates experienced significant increase at the end of the 1980s, when the central government associated the promotion of local officials with the success of fertility control under the OCP in their territories (Scharping, 2003; Huang, Lei, and Sun, 2021). In the spring of 1989, premier *Li Peng* addressed to the governors that the population growth rate affected the survival of the Chinese race and should be controlled under evaluative targets (Greenhalgh and Winckler, 2005). In 1990, local officials were required to report the average number of children per woman in their region in order to rigorously check whether childbearing complied with the quantitative indicator— "policy fertility"—assigned by the state family planning commission (Gu et al., 2007). In March 1991, to show its strong resolution on the population control, the central government listed family planning among the three basic state policies in the Eighth Five-Year Plan passed by the National People's Council. The plan explicitly set an objective of reducing the Chinese population's natural growth rate to less than 1.25% on average over the following decade. To

[15] Document 11 allowed provincial governments to issue specific and locally tailored regulations. Document 7 stated that birth control regulations would be developed in accordance with local conditions and approved by the provincial Standing Committee of the People's Congress and provincial governments (Gu et al., 2007).

[16]The Guangdong provincial government received over 5,000 letters complaining about or protesting the OCP's implementation, raising concerns about social stability (Huang, Lei, and Sun, 2021).

achieve such a challenging objective, national leaders employed a "responsibility system" to induce subnational or provincial officials to set high penalty rates (Huang, Lei, and Sun, 2021). Furthermore, beginning in 1991, the central government had consistently listed the enforcement of the OCP as among one of the three highest priorities, along with economic growth and social stability, when evaluating the performance of local officials (Birney, 2014). As a result, during the short period between 1989 and 1992, fine rates significantly increased (greater than the annual income of the average household) in 16 of the 30 provinces, with the average fine rate jumping from 1.0 to 2.8 times the average household's annual income. Also, during this period, 16 of the 21 significant fine increases in the policy's history occurred (Figure A1; Huang, Lei, and Sun, 2021).

The OCP impacted millions of couples and lasted for 36 years, ending in December 2015. Scholars generally agreed that the OCP was a highly successful family planning policy and was largely responsible for China's documented fertility rate decline (e.g., Lavely and Freedman, 1990; Yang and Chen, 2004; Li, Zhang, and Zhu, 2005). In our sample, China's fertility rate dropped from ?? children per woman in the ??? to ??? children per woman from ??? to ??? (Table A1).

2.2. OCP fine rates across provinces

Since the central government authorized local governments to enact specific fertility policy regulations at the start of the OCP, there has been a large variation in policy implementation across provinces over time. We use the average monetary penalty rate for one unauthorized birth in the province-year from 1979 to 2000 to measure the strength of the OCP in the provincial level, following previous studies (e.g., Ebenstein, 2010; Wei and Zhang, 2011; Huang, Lei, and Zhao, 2015; Huang and Zhou, 2015; Huang, Lei, and Sun, 2021). Even though OCP penalty included both monetary penalty and administrative actions (e.g., expulsion from party membership or loss of job), monetary penalty, or OCP fines, is still a good proxy for the OCP because an increase in fines is usually associated with stricter administrative actions and higher-fine regimes are associated with low fertility and smaller family size (McElroy and Yang, 2000; Scharping, 2003; Gu et al., 2007; Ebenstein, 2010; Huang, 2017).

The OCP fine rates are calculated as multiples of the average annual household income of the province by Ebenstein (2010).[17] Figures A1 and A2 depict the chronological and geographical

[17] See Appendix III of Ebenstein (2010) for detailed calculation process.

change patterns of policy fines in each province from 1979 to 2000; the time trend of the OCP fine rates across provinces associated closely with the importance of the OCP implementation to the provincial officials' concern on their political prospects. From 1979 to 1988, the fine rates remained the same in most provinces or moderately decreased in a small number of provinces, because implementing the OCP raised complaints and resistance, which threaten the core appraisal indicator for the provincial officials—social stability. Between 1989 to 1992, when the central government expressed the resolution to tighten up the OCP and then established the birth control outcome of the OCP implementation as a superior core appraisal indicator to social stability for the provincial officials, 16 out of 30 provinces saw a significant increase in the OCP fine rates, from 0.8% to 3.0% of annual household incomes, on average. 16 of the 21 significant increases in OCP fine rates in the OCP history happened in this short period (Figure A1; Huang and Zhou, 2015; Huang, Lei, and Sun, 2021).[18]

As to the variations in OCP fines across provinces, the promotion incentive for provincial officials could have been a significant driving force.[19] Huang and Zhou (2015) and Huang, Lei, and Sun (2021) find a strong correlation between increases in fine rates and the incidences of government succession. Twelve of the 16 significant fine increases mentioned above occurred within the first two years of a new provincial governor's tenure.[20] The average age of the 16 provincial governors who increased the fertility fines was 56 years, which was much younger than the national average of 59 years (p-value = 0.02), indicating that they had a higher probability to be promoted in the CCP cadre appointment regulations; they were more incentivized to perform well in the core appraisal policy implementation of the OCP.[21] These findings are consistent with the premise that a governor's personal characteristics, such as inauguration time and age, influence their incentive to raise the fine rates (Huang, Lei, and Sun, 2021).

In addition, provincial governors were not restricted by the performance of the OCP

[18] A "significant" increase is greater than the annual income of the average household.

[19] The reasons why the fines change are not formally or precisely documented. (Huang, 2017).

[20] The average tenure of these 16 provincial governors was approximately six years, and they had a higher chance of promotion than their peers. For example, two of them were appointed standing members of the Political Bureau of the Central Committee of the Communist Party: one was promoted to executive vice premier and the other to chairman of the Chinese People's Political Consultative Conference (Huang, Lei, and Sun, 2021).

[21] Their efforts were compensated. For instance, among the 16 governors, one was elevated to executive vice premier, and the other to chairman of the Chinese People's Political Consultative Conference. Both of them were made permanent members of the Political Bureau of the Central Committee of the Communist Party, the central leadership of the CCP.

implementation before inauguration. Former governors (who retired, or were promoted or transferred to other provinces) whose successors increased the fine rates by more than one year's worth of household income had political careers that were at least as successful as those of their peers, thereby putting to rest the concern that these former governors had been replaced due to poor performance in OCP implementation (*e.g.*, higher fertility rates) (Huang and Zhou, 2015; Huang, Lei, and Sun, 2021).[22]

Counterintuitively, the OCP fines were not designed with consideration for local fertility preferences. Using data from the CHNS national survey, Liu (2014) finds that neither the relaxation of the OCP nor the amount of the fines for unauthorized births were related to the preference for child quantity at the community level. We regress the provincial OCP fine rates on the local birth rate one, two, and three years before the OCP and find no significant correlation (row 2, Table A2), consistent with the findings of Liu (2014).

Third, there is nonsignificant correlation between changes in fine rates and changes in macro indices at the province-year level. Huang, Lei, and Sun (2021) regress the fine rates on a specific index or set of indices for demography, macroeconomics, government behaviors, and sanitary and educational conditions and find that none of these macro indicators are significantly correlated with the OCP fine levels or changes at the 10% level after controlling for year and province fixed effects as well as province-specific linear time trends.[23]

It is possible that changes in the penalty rate are related to other policies or previous economic conditions in a particular locality, and the specific locality could contribute to human capital accumulation and economic development. For example, the ambitious officials who raised the fines may also have been aggressive in their implementation of other policies, such as compulsory schooling laws (CSLs) and pension programs. As emphasized by the Eighth Five-Year Plan, economic growth and poverty reduction are also factors to be evaluated. Because our measure of

[22] Several of these former governors, such as *Zhu Rongji*, *Li Changcun*, and *Wu Guanzheng*, rose to become central government political leaders. There is no evidence that any provincial governors were fired as a result of poor OCP implementation (Huang, Lei, and Sun, 2021).

[23] The provincial-level indices include: 1) Demographic variables of provincial population, urban population, rural population, birth rate, and death rate; 2) Macroeconomic indexes of provincial employment, urban employment, rural employment, wage of workers, wage of urban workers, wage of rural workers, GDP per capita, unemployment rate, and urban unemployment rate; 3) Government behaviors such as pension scheme participants, medical institution participants, low-income households (*dibao* participants), total government expenditure, and government expenditure on administration, on agriculture, on education, and on social security; and 4) Sanitary and educational conditions such as the numbers of beds in hospitals, teachers in primary schools, teachers in secondary schools, and teachers in higher-level schools (Table B.1, Huang, Lei, and Sun, 2021).

the OCP is province-year level, the potential endogeneity should originate from the province-year level confounding factors. To examine this, Huang, Lei, and Sun (2021) use 28 province-year-level macroeconomic indices from four large categories to examine their correlation with the OCP fines.[24] For each provincial-year level macroeconomic index, the authors examine whether its average levels in the previous one, two, or three years are correlated with fertility fines. They also test if the changes in fines are correlated with the changes in these indices. Results show that none of the macro indicators are significantly correlated with the OCP penalty levels or changes at the 10% level.

However, the results only suggest that the penalty changes are insignificantly correlated with changes in macro indices at province-year level. Therefore, we extend the research of Huang, Lei, and Sun (2021) and find that fine rates are not significantly related subpopulation groups within provinces (e.g., public servants versus nonpublic servants), boy preference (proxied by genealogy book density and gender survival ratio in the Great Famine), social inequality (e.g., Gini coefficient), and returns to education (rows 2–7, Table A2). Additionally, we find a nonsignificant correlation between fine rates and contemporary policies such as "reform and opening-up" (measured by GDP) and reforms to university enrollment (proxied by provincial admission rate) (rows 8–9, Table A2). These tests all suggest that the variation in OCP fines across provinces over time is exogenous to the local socioeconomic conditions (conditional on covariates).

2.3. Intergenerational mobility and one-child families

Higher intergenerational mobility generally increases equality and benefits productivity in a society (Bell et al., 2019). High intergenerational mobility also brings advantages to the stability in a society because a flexible upper class can avoid some of the problems and issues associated with social class solidification (Lipset and Bendix, 2018). Unfortunately, generations of Chinese people have typically had to endure decades or even centuries in less mobile societies with solidified classes.

To increase their children's capacity to survive and maximize their incomes, educational levels,

[24] The four sets of factors are: (a) demographic variables, including population size, and birth and death rates; (b) basic economic conditions, such as employment, wages, gross domestic product (GDP), and the unemployment rate; (c) government programs and expenditure, including numbers of pension program and health insurance participants; and (d) social services, such as the number of hospitals and educational resources (measured by the number of teachers). These factors are found to be possibly influence economic outcomes. See details in Section II.A of Huang, Lei, and Sun (2021).

and social standing, traditional Chinese parents work hard, save carefully, and develop and widen their networks. In addition, older children in the family are made accountable for nurturing younger ones, just like parents (Diamant, 2000; Lewis, 2008). As a result, some families have all or most of their members pursue the same occupations, get comparable levels of schooling, and advance up specific hierarchies (Lorge, 2000). These families are frequently connected by marriage to build communities of interest (Holmgren, 1982). Persistent close relationships both inside and between families were woven into a solid network in order to support and assist family members in their climb up the socioeconomic ladder. Such alliances between families are likely to contribute to a society with low intergenerational mobility; a child who inherited wealthy resources and strong connections tends to remain in a relatively high hierarchy, while his counterpart from a low-income family or weak background finds it difficult to compete with him.[25] Thus, the implementation of the OCP may alter the entangled and interconnected "big family" culture and result in a grand socioeconomic reshuffling of Chinese society.

We find that a child born after 1979 has lower intergenerational persistence in income, education, and social class, which indicates to higher intergenerational mobility, than a child born before 1979, when the OCP began to be implemented (Table A3).[26] On average, if their parents' income in their cohort ranks 1 percentile higher, a child born after 1979 is expected to rank 0.258 percentile higher (Column 2, Panel A) while a child born before 1979 is expected to rank 0.513 percentile higher (Column 1, Panel A). Lower intergenerational persistence for a child born after 1979 than one before 1979 are also found in education (Panel B) and social class (Panel C).

3. Data

In this section, we first describe the data sources that we used to assemble our dataset. We then explain the process of extracting the main variables from the dataset. The statistics of the data are

[25] Feng (2010) documents a modern city (*xian*) in central China where a small number of families with authority virtually occupy all administrative leadership positions. These families have intermarried relationships.

[26] In our study, intergenerational persistence—which is derived by a univariable regression of rank of parent on rank of child within their cohorts, respectively—is used to quantify intergenerational mobility. The intergenerational persistence measures how much a child's socioeconomic outcomes are influenced by those of his parents (Dearden, Machin, Reed, 1997; Corak, 2013; Chetty et al., 2014). Chetty et al. (2014) hold that rank-rank specification is more stable than other specifications for measuring intergenerational mobility, and that it can also be applied to all variables including non-cardinal variables (e.g., education, social class). Emran, Jiang, and Shilpi (2020) also argue that this specification results in the least bias in slope and intercept estimations when used to calculate intergenerational persistence in developing countries.

summarized last.

3.1. Data Sources

We construct our dataset from ten national household survey datasets (Table A4): China Family Panel Studies (CFPS), Chinese General Social Survey (CGSS), China Health and Retirement Longitudinal Study (CHARLS), Chinese Household Finance Survey (CHFS), Chinese Household Income Project (CHIP), China Health and Nutrition Study (CHNS), China Labor-force Dynamics Survey (CLDS), Chinese Social Survey (CSS), National Population Census of China (NPCC, Long Form database), and the Urban household survey (UHS). These surveys are nationally representative and were rigorously conducted by academic institutions, universities, and official organizations in a series of waves (every two years, except for census data) from 1982 to 2018; together, they provide an accurate representation of household demographics in China (Gan et al., 2014; Xie and Hu, 2014; Zhao et al., 2014).[27]

The dataset generated by combining these surveys is significantly larger than the datasets used in earlier studies on intergenerational mobility in China, all of which had incomplete survey waves and used only one (Fan, Yi, and Zhang, 2021; Yu, Fan, and Yi, 2021) or two datasets (Zou, 2015). Importantly, this larger dataset enables us to rank the outcome by cohorts of children and parents and attenuates the lifecycle bias.[28] Additionally, longer observation periods (over decades) have also enabled us to present a comprehensive and systematic pattern of intergenerational mobility change in China. Due to its broadened scope and informative value, the dataset itself thus constitutes a significant contribution to future research on intergenerational mobility in China.

3.2. The construction of main variables

The three major dependent variables that we focus on in our study of intergenerational mobility in China are the intergenerational persistence of income, education, and social class. Intergenerational income persistence is the most commonly studied variable in previous literature and is also the most intuitive way to measure intergenerational mobility (Solon, 1999; Black and Devereux, 2011). An advantage of studying intergenerational educational persistence as well is that it can overcome the attenuation bias in intergenerational income persistence; education is a

[27] The Chinese Censuses used in this study are the ones from 2000, and 2005, which are surveys from adjacent years.
[28] The intergenerational persistence measurement method of (children') rank—(parents') rank effectively reduces the lifecycle bias. Results are improved by larger amount of data and controlling for parents' age (Chetty et al., 2014; Emran and Shilpi, 2019).

less noisy, stable attribute that does not fluctuate once achieved, whereas income is subject to greater fluctuations (Feigenbaum, 2018). In addition to the income and education variables, we specifically created an intergenerational social class persistence variable by synthetically measuring social status while considering both administrative and wealth levels, which constructs a more comprehensive measure for intergenerational mobility in China. These three types of intergenerational persistence enable cross-validation of one another, given that they each measure different aspects of persistence.

We include all yearly wages and monetary gains to determine individual income. For those in the labor market, income mainly includes wages, bonuses, monetary subsidies, etc. from all full-time and vocational jobs as well as capital gains. For those who are retired, income is mainly composed of their pension, property gains, and capital gains.

We measure years of education as the number of years spent in school until the individual gains their highest graduation certificate, excluding the number of years spent in school without earning a diploma. The education variable is defined this way because the labor market pays employees in accordance with their highest degree, without considering their incomplete study experience.

When ranking social class for each observation, we consider position, income, and access to additional fringe benefits, which together signify social standing that is acknowledged in Chinese society (Lu, 2003). We sort observations into five classes: high, middle-high, middle, middle-lower, and lower. Based on the seminal sociology work of Lu (2003) on Chinese social stratification, we assigned those with governmental positions at the "Deputy Bureau-Director (*fu ting ju ji*)" level and above are assigned to high class, those at the "Division-Director (*xian chu ji*)" or "Deputy Division-Director (*fu xian chu ji*)" levels to middle-high class, and those at the "Section-Director (*xiang ke ji*)" or "Deputy Section-Director (*fu xiang ke ji*)" to middle class; all other government workers are assigned to middle-lower class. For observations without an associated governmental ranking, we assign social class based on income. Following Li and Zhang (2008), who state that middle-class families corresponded to incomes of 60,000 to 500,000 *yuan* a year in 2005, we classify observations associated with income higher than 500,000 *yuan* in 2005 to middle-high class, between 60,000 and 500,000 *yuan* to middle class, higher than the sample median income but lower than 60,000 into middle-low class, and lower than the sample median income into lower class. However, the assigned class for an agricultural worker cannot be higher than middle class for a synthetic equivalency (Li and Zhang, 2008). In order to establish

intergenerational relationships within this dataset, we first identify each possible parent(s)-child pair from the pool of respondents and their spouse, parents, children, and/or other close relatives who are required to provide information.

The raw dataset contains information on income amount, education years, social class rank, intergenerational relationships, and demographics such as birth year, birth province, gender, and marriage status. Observations missing data on all of the three dimensions (income, education, and social class information) are considered uninformative for the purposes of this study and are thus filtered out. With complete data in at least one of the three dimensions, we then extract a sample of child-parent pair observations.

To reduce lifecycle bias, we rank within each cohort to obtain quantiles on outcomes for both parents and children (Chetty et al., 2014; Emran and Shilpi, 2019).[29] If both parents are present, we take the average rank of the parents.

For sample age range, we remove observations for individuals under 22 years of age during the survey years, as they cannot be assumed to have completed schooling and entered the labor market to earn wages. In addition, we exclude observations for individuals born prior to 1949, when the People's Republic of China was founded, to frame observations in the same regime for the sake of comparability.

To quantify OCP intensity across the country in order to identify policy effects, we associate each observation with data detailing the rate of average monetary penalty for one authorized birth ("Fine Rate") in the province-year level; the fine rate is formulated in multiples of annual income (Gu et al., 2007; Ebenstein, 2010; Wei and Zhang, 2011). Since human gestation is approximately nine months, parents' decision to have a child should be made close to a year in advance.[30] Therefore, we construct a variable, $Fine$, which is the OCP fine rate in a year before the birth year in a given province, and match the data of this variable into the aforementioned combined dataset to assign a value for each observation, thus incorporating a metric for OCP intensity into the dataset.[31] The effective fine rate was 0 for children born before 1979, when the OCP was started. We exclude children born in 1979 because it is a transitional year and provinces launched the OCP

[29] We also control parents' age to attenuate lifecyle balance (Chetty et al., 2014; Emran and Shilpi, 2019). The rank-rank method is believed to be the most accurate measurement for intergenerational persistence (Chetty et al., 2014; Gallipoli, Low, and Mitra, 2020).

[30] See details on the fine rate construction in Appendix Section III of Ebenstein (2010).

[31] It is possible that parents need more than one year to decide whether to have a child. We thus perform a robustness check using the average fine rate of three years prior to birth. Details can be found in Section 5.2.3.

in different times during this year. We also exclude children born after 1996 to ensure that individuals living in the sample years completed their education and were able to work.

3.3. Statistical summary

After cleaning the data, the final dataset consisted of 2,096,978 child-parent(s) pairs, of which the summary statistics are shown in Table A5, with the average age of children 35.91 years old. The birth years of the observations span from 1949 to 1996 (Table A6), and the survey years from 1986 to 2019 (Table A7). The source data of CHNS, NHS, and NPCC only contain information on household heads, most of whom are male; because our data construction centers on identifying a child to pinpoint the parent(s)-child pairs, 73% of the child observations are of males. We therefore conducted a robustness test with a gender-balanced subsample (Section 5.2.4) and the findings remain consistent.

Among the included observations, 540,307 of the child observations, 347,541 of the father observations, and 376,309 of the mother observations contain information on income. These observations have a mean annual income of 123,793 *yuan*, 39,281 *yuan*, and 6,786 *yuan* (adjusted by CPI), respectively. Information on education is available for 2,086,168 child, 1,814,276 father, and 1,975,662 mother observations, with a mean of 7.93, 6.76, and 5.22 years of education, respectively. Similarly, 551,236 observations of children, 357,293 observations of fathers, and 385,979 observations of mothers contain data on social class (where 0 denotes the lowest class and 4 denotes the highest class), with means of 0.56, 0.55, and 0.41, respectively. 28.3% children come from one-child families and 71.7% of children from multi-child families (with an average of 3.0 children), which means that there are three times as many children from multi-child families as from one-child families; in terms of the number of families in these two categories, the number of one-child families and the number of multi-child families is roughly equal, 28.3% and 23.9% respectively. Since 12.6% of the child observations in our sample were made after 1979, even though our sample indicates that 10% of children are the only child in the household, the one-child rate of our sample is consistent with the OCP compliance rate of about 50%.[32]

[32] Parents could still be considered OCP-compliant even when they have two or three children if they meet specific requirements, such as having a first child who is disabled or being married to someone who belongs to a minority group.

4. Empirical Methodology

We employ the difference-in-differences (DID) method to estimate the effects of the OCP on intergenerational persistence. We use the average monetary penalty for one unauthorized birth (the OCP fine) in the province-year panel from 1979 to 2000 as the proxy of the OCP strength. The OCP fine is formulated in multiples of annual income (Gu et al., 2007; Ebenstein 2010).[33] The mathematical expression is:

$$Rank_{iht} = \beta_0 + \beta Fine_{hp} \times Rank_{ift} \times I(h > 1979) + \beta_1 Fine_{hp} \times I(h > 1979) + \beta_2 Rank_{ift} + X_i{}'\beta_3 + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (1)$$

The subscript i stands for child i; p for the province where child i was born; h for child i's cohort, or birth year; d for survey source; and t for survey year. $Rank_{iht}$ is child i's rank in income, education, and social class within their cohort h in year t; $Rank_{ift}$ is the parents' rank within the parents' cohort f in year t. $Fine_{hp}$ is the amount of the fine in child i's birth province p one year before the child was born in year h.[34] This variable captures key elements of the variation in the strictness of the local implementation of the OCP (Huang, Lei, and Zhao, 2015; Huang and Zhou, 2015; Huang, Lei, and Sun, 2021). It remains at zero prior to 1979, the year the OCP was initially implemented, varies across provinces after 1979, and is adjusted by the revision of local OCP rules. We exclude cohort 1979 data in the DID analysis because 1979 was a transition year when the OCP was being formed and fertility decisions were being adjusted.

Control variables, X_i, include child i's gender, marital status (whether child i is married/in de facto union), and whether only one parent is present. We include ω_h , τ_t , φ_d and λ_p, which stand for cohort, year, dataset, and province fixed effects, in order to control for all time-invariant differences between cohorts and provinces and changes over time that affect all cohorts and provinces similarly. Finally, we cluster standard errors at the provincial level in our main analysis and report alternative standard errors such as those using the two-way clustering of province and survey source standard errors in Table A8.[35]

We focus on the key coefficient, β, which shows the effect of the OCP on intergenerational persistence in income, education, and social class. We select four alternative time periods to observe the focal effect: 1976–1982, 1974–1984, 1969–1989, and 1949–1996 cohorts (the 1979

[33] See Ebenstein (2010) Section III for the details of the construction of the fine rate data. See details in Section 2.2.
[34] See details in Section 3.2 about the construction of $Fine_{hp}$.
[35] The results are robust to the different standard error calculation methods.

cohort being excluded), that is, three, five, and ten years before and after the initial implementation of OCP in 1979 and all sample cohorts (from when *the People's Republic of China* was founded to the latest cohort who finished schooling).

Our DID identification strategy relies on the parallel-trend assumption: the outcomes prior to OCP implementation should not be related to the intensity of the treatment—the fines across provinces. In addition to demonstrating the exogeneity of fine rates (Section 2.2), we employ the method of event study to examine the link between fines and intergenerational persistence year by year to determine whether different intergenerational persistence trends already existed due to different fines across the country prior to the OCP. The specification is as follows:

$$Rank_{iht} = \beta_0 + \sum_{\gamma=1949}^{1996} \beta_\gamma Fine_{hp} \times Rank_{ift} \times I(h = \gamma) + \sum_{\delta=1949}^{1996} \beta_\delta Fine_{hp} \times I(h = \delta) + \beta_1 Rank_{ift} + X_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (2)$$

where the 1979 cohort is excluded. The rest settings are the same as in Equation (1). We construct a pseudo-fine variable for periods before the OCP implementation. The pseudo-fine is the average fine following the application of the OCP since 1979 and thus represents the OCP intensity in each province.

[Figure 1 approximately here]

We report the associated coefficients in Table A9 and the corresponding graph as Figure 1. Results show that prior to the implementation of the OCP, there are no pre-trends for the cohorts' intergenerational persistence in terms of income, education, and social class.

5. Main results and robustness checks

In this section, we present the main results of the DID estimations, followed by our robustness tests, which concentrate on identification assumptions, contemporary policies, alternative measures for key variables, and external validity.

5.1. Main results

The DID estimations of the key coefficient on children's income ranks remain negative for the comparison of different cohort periods, suggesting that the OCP has a negative effect on intergenerational persistence (Table 1).

The results of the regression show that for observations made on the 1980–1982 versus the 1976–

1978 cohorts (three years before and after the OCP), the interaction term of income and fine has a coefficient of -0.095 (Row 1, Column 1). This coefficient indicates that the OCP decreases the (children's) rank–(parents') rank intergenerational income persistence by 9.5 percentage points for every fine equal to the annual income that a parent earns. Furthermore, the coefficient remains significant negative for other comparing time windows. Results show that the OCP decreases the intergenerational persistence rate by 9.0 percentage points for every fine equal to the annual income that a parent earns for the 1980–1984 versus the 1974–1978 cohorts (five years before and after the OCP), by 10.5 percentage points for the 1980–1989 versus the 1969–1978 cohorts (ten years before and after the OCP), and by 13.3 percentage points for the 1980–1996 versus the 1949–1978 cohorts (all sample years) (Column 1).

The OCP also shows similar strongly negative decreasing effect on intergenerational persistence in education and social class. In terms of the intergenerational education persistence, the OCP decreases it by 15.5 percentage points for the 1980–1982 versus the 1976–1978 cohorts, by 15.5 percentage points for the 1980–1984 versus the 1974–1978 cohorts, by 16.3 percentage points for the 1980–1989 versus the 1969–1978 cohorts, and by 13.5 percentage points for the 1980–1996 versus the 1949–1978 cohorts (Column 2).

In terms of the intergenerational social class persistence, the OCP decreases it by 8.2 percentage points for the 1980–1982 versus the 1976–1978 cohorts, by 8.6 percentage points for the 1980–1984 versus the 1974–1978 cohorts, by 10.9 percentage points for the 1980–1989 versus the 1969–1978 cohorts, and by 14.4 percentage points for the 1980–1996 versus the 1949–1978 cohorts (Column 3).

[Table 1 approximately here]

We further calculate an average total OCP effect on intergenerational persistence for cohorts born between 1980 and 2000 (Table A10). The calculation is based on the population-weighted fine for cohorts born between 1980 and 1996 multiplied by the baseline estimate of the elasticity of the one-year income equivalent fine (We use the estimations of 3 years before and after OCP implementation as the baseline here; Row 1, Column 1; Row 1, Column 2; and Row 1, Column 3, Table 1). If we make a strong assumption here that the marginal effect of the OCP consistently exists for each year, then for the 1980–1996 cohorts nationwide, the average fine is equal to 1.519

annual incomes, and the OCP decreased the intergenerational persistence in income, education, and social class by 14.4 (Column 2), 23.5 (Column 4), and 12.5 (Column 6) percentage points, respectively. In terms of relative impact, the OCP reduces intergenerational income, education, and social class persistence by 28.1% (Column 3), 48.7% (Column 5), and 24.8% (Column 7), respectively, based on estimates of intergenerational persistence prior to 1979 (Column 1, Table A3).

We also calculate the OCP effect on the intergenerational persistence for each province, with the strong assumption that OCP marginal effect consistently exists (Table A10). We find that the OCP decreased income, education, and social class persistence respectively by a range of 11.7% to 44.2% (Column 3), 20.2% to 76.5% (Column 5), and 10.3% to 38.9% (Column 7) for different provinces.

Since our sample has large size and covers numerous decades across the country, we study the heterogeneity of the OCP effect in gender, location, geographical distribution, and parents' rankings. Results show that the OCP consistently negatively affect the intergenerational persistence in income, education, and social class in each subsample (male children v. female children, urban residents v. rural dwellers, individuals dwelling in the eastern, central, or western region) (Tables A23–A25).[36]

5.2. Robustness checks

We conduct robustness tests on identification assumptions (test for the LLF effect, placebo test, treatment heterogeneity test, and spillover effect test), an alternative explanation involving a contemporary human capital policy, alternative measurements (for key independent variables of parents' rank and fine rates), and external validity (for different datasets and model weighing methods).

5.2.1. Identification assumptions

We conduct four robustness tests regarding the parallel-trend assumption for DID estimations. First, while pre-trend tests are passed, we test whether the prior birth control policy—the "Later, Longer, and Fewer (LLF; beginning in 1971 and ending roughly in 1975, with the last batch of

[36] Gu et al. (2007) categorize the provinces in China into three regions according to the level of strength—the eastern region (Shanghai, Jiangsu, Zhejiang, etc.), the central region (Hebei, Hubei, Hunan, etc.), and the western region (Ningxia, Shanxi, Shaanxi etc.). The eastern region had highest OCP fine rates and the western region's fine rates were the lowest.

Family Planning Leading Group built in Xinjiang Autonomous Region and Guizhou Province; Section 2.1)"—had already induced an unequal trend for intergenerational persistence prior to the OCP's implementation. We use the establishment years of the Family Planning Leading Group in each province as the proxy for the variable of LLF (Chen and Fang, 2021). We employ a triple-difference method with the 1971–1975 cohorts as the focus and the child's rank in income, education, or social class as the dependent variable to determine whether the LLF influenced intergenerational persistence prior to the OCP. The coefficients of the interactive term (LLF × average OCP fine rates × parent rank in income/education/social class) are nonsignificant (Table A11).[37] This result indicates that the LLF had no effect on intergenerational persistence before the implementation of OCP.

Second, to evaluate the validity of the design, we use a permutation test to randomize the treatment in the sample by randomly shuffling the fine variable to run the baseline regressions with 1000 draws. Results from the 95^{th} and 5^{th} quantiles of the placebo coefficients indicate that the permutation placebo test has been passed, supporting our main findings (Table A12).

Third, for Two-way Fixed-effects (TWFE) estimators with multiple time periods and variation in treatment timing, Callaway, Goodman-Bacon, and Sant'Anna (2021) suggest that one must further restrict different types of treatment effect heterogeneity to recover reasonable treatment effect parameters. In order to address treatment heterogeneity issues (including negative weighting), we test our samples for "No Treatment Effect Dynamics" (Assumption 6(a)) and "Homogeneous Causal Responses across Groups" (Assumption 6(b)) (Callaway, Goodman-Bacon, and Sant'Anna, 2021). Within any random timing group, there is no detected difference in the causal response of the child rank to the treatment of the interaction of the OCP fine and income, education, or social class (Table A13).

Finally, the OCP may have a spillover effect on cohorts born before 1979. Although not restricted by the OCP when born, these children may have no more siblings or became the only child in the family even if their parents were willing to have more children, similar to the situation of children born after 1979; the magnitude of the OCP effect could therefore be underestimated. To avoid the possible impact of spillover, we exclude child-parent pairs with the mother younger than the 90th percentile for gestational age. Results show consistent negative coefficient as the

[37] The fine rates used here are the same as those used in the event study for the pre-trend test. Section 3.2 and Section 4 contain more information.

baseline but with smaller or the same magnitude, indicating that spillover effect does not exist (Table A14).

5.2.2. Alternative explanation

We explore the influence of a potential alternative interpretation: human capital accumulation, a recognized factor that might modify intergenerational persistence. We use China's university re-enrollment and enrollment expansions to proxy the variation in human capital across time and provinces. These are educational reforms that have been reinstated since 1977 in the university enrollment system (specifically entrance exams and an increase in enrollments), substantially altering demographic patterns in university settings. This contemporaneous historical event may have also generated a negative link between OCP and intergenerational persistence. We therefore use provincial university enrollment rates to represent the current educational policy and use the interaction between enrollment rate and parent rank as the control variable to determine whether the main effect of the OCP on intergenerational persistence still exists. This modern educational policy has a significant impact on outcomes; however, the key coefficient of the OCP and parent rank are still significant and of the same magnitude as the baseline, indicating that this contemporary policy does not affect the OCP's impact (Table A15).

5.2.3. Alternative measures

As another alternative, we change the measure of the two key independent variables—parent(s)' rank and fine rates—to test whether the main effects still exist.

In one test, we use the rank of the father as a reflection of the rank status of both parents because of the male-breadwinning family norms in Chinese society. In practice, the father also frequently takes responsibility for work and earns a higher share of income than the mother in the family. The results are consistent with the key findings of baseline regressions (Table A16).

In another test, we replace the yearly fine rate variable with a variable representing the 3-year average fine rate before a child's birth. In many cases, it can take young couples years to decide when to have their first child; thus, fines imposed a few years before the birth of a child could also influence the parents' birth decision, similar to the fine in the current year. Our findings are similar to the baseline, supporting the hypothesis that the OCP decreases intergenerational persistence and has similar effects across different groups (Table A17).

5.2.4. External validity for different datasets

We explore the external validity of our findings by utilizing different datasets to test whether the main effects consistently exist.

First, we remove data from three gender-imbalanced surveys (Census, CHIP, and CHNS) and rerun the regressions using data from the remaining, generally gender-balanced surveys to address the issue of observations having more males than females in the main sample (see Section 3.3). The demographic characteristics between the control groups (before OCP) and the treatment groups (after OCP) are largely equivalent excepting gender, according to the summary statistics of the diff-in-diff groups (Table A18). The early cohorts have a greater male percentage, since three surveys (Census, CHIP and CHNS) only have the key information for predominantly male household heads. Therefore, this robustness test excludes these three surveys in order to keep the male percentage comparable to the female percentage. Results remain the same, supporting our key findings that the OCP reduces intergenerational persistence (Table A18).

Second, we rerun the regressions with datasets that provide details for co-residents. Male offspring are more likely than female offspring to live with their parents in China, and the primary respondents of a household typically provide more detailed information about their co-residents (Xie and Hu 2014; Xie and Lu 2015). Co-residency is present in the census, CGSS, CHIP, and UHS data; however, other national surveys that we use in this analysis, including CFPS, CHARLS, CHFS, CHNS, and CLDS, address the issue of missing co-resident data by including questions on all family members, whether they were co-residents or not (Xie and Hu 2014; Xie and Lu 2015; Emran and Shilpi 2019). To evaluate the impact of co-residency, we therefore establish a subsample that does not include information from the first four data sources. Co-residency has little bearing on our main themes, which are consistent with our baseline findings (Table A19).

Third, we divide data into subsamples whether or not parents experienced China's Great Famine (1958–1961) before giving birth. Starvation experience can affect an individual's fertility willingness and behavior (Shi, 2021), making the control group and the treatment group lack of comparability.[38] In both subsamples, the impact of the OCP consistently remains negative on the

[38] China's Great Famine was brought on by an overestimation of food production (Becker 1996; Thaxton 2008). For fear of not meeting the production goals set by the central government, local cadres faked their grain output figures and submitted inflated totals to the central government, resulting in a severe grain shortage. The death toll from the famine was massive, with estimates ranging from 16.5 to 30 million, and with many of those deaths concentrated in

intergenerational persistence in income, education, and social class, suggesting that parents' famine experience does not impact the main effect of the OCP (Table A20).

Fourth, we divide data into subsamples whether or not children migrated inter-provincially from their birth province to a different residential province. People usually migrate for better opportunities in work and education and thus may differ from those who choose not to migrate in will and capacity. We therefore divide child-parent pair observations into two subsamples with migrant children or with non-migrant children. Results show similar negative decreasing effect of the OCP on intergenerational persistence as the baseline for both subsamples (Table A21).

Finally, we reweigh our models by the sample size of each cohort by assuming that every cohort contributes equally to our estimate of the average treatment effect of OCP. Results remain consistently similar to the baseline, the average treatment effect net of cohort size (Table A22).

5.2.5 Heterogeneity in the OCP effect

Since our sample has large size and covers numerous decades across the country, we study the heterogeneity of the OCP effect in gender, location, geographical distribution, and parents' rankings.

Results show that female children have generally lower intergenerational persistence in income, education, and social class than male children, and this disparity enlarges when the comparing time window of the treatment and control groups increases (Table A23). For rural versus urban children, the former shows lower intergenerational persistence than the latter (Table A24). For geographical locations, individuals in the eastern region generally show the highest intergenerational persistence, those in the central region show the second highest, and those in the western region show the lowest (Table A25).

6. Theoretical Model

This section investigates the channels through which OCP fine rates might affect intergenerational mobility in a theoretical framework. We assume two periods of life: childhood and adulthood. Each parent has n ($n \in Z, n \geq 1$) children at the beginning of adulthood, which means that parents and children overlap when the latter are young; that is, the same individual can

infants and young children; The famine also caused China's fertility rate to plummet (Ashton et al. 1992; Chang and Wen 1997; Li and Yang 2005).

be designated as a child of aging parents and a parent of their own young children. Adults use the human capital accumulated as children to generate labor income; these earnings can be spent on consumption and investing in the human capital of their children.

Parental preferences are assumed to depend on the number of children (n), the education of their children indicating their investment (e), and the parents' own consumption (normalized in m). A natural formulation is:

$$U(n, e, m) = \ln n + \alpha \ln e + \beta m \tag{3}$$

where $0 < \alpha < 1$, and $\beta > 0$. We assume α has a moderate value between 0 and 1, because China has the tradition of "more sons, more blessings (*duo zi duo fu*)," which tends to value quantity over quality, and we do not consider $\alpha \gg 1 - \alpha$.

People in the PRC regime cautiously built wealth and possessions due to the long-term impact of proletarian ideology and the absence of property law.[39] As a result, we assume that an individual's wealth is accumulated through earnings. Individuals in each generation are divided into two types: high type and low type. Individuals with wealth greater than the 50th quantile of their generation are high-type, while those with wealth lower than the 50th quantile are low-type. The proportion of high-type individuals in the population at time t is denoted by $\lambda_t (0 < \lambda_t < 1)$. In the parents' generations, total resources owned by parents of high type, w_h, are θ ($\theta > 1$) times those of low type, w_l. Therefore, by definition, $w_l = w$ and $w_h = \theta w$, where $w > 0$.

Parents violate the OCP if they have an above-quota child or children. To simplify, we exclude exceptional circumstances in which parents may have two children, and instead assume that all parents are only allowed to have one child. We normalize the fine for each above-quota child against wealth to assess the relative impact of fines on the different types (high and low). In addition, employees in government institutions who violate the OCP face demotion or even dismissal. As a result, they are presumed to be charged with an additional fine, b_e. Thus, the fine for each above-quota child for a low-type parent, $b(w)$, is:

$$b(w) = \frac{b_o + I(e) b_e}{w} \tag{4}$$

where b_o is the provincial OCP fine, and $I(e)$ is a dummy variable. If the parent works in a

[39] Before March 2007, there were no property laws in China. The Chinese 2007 Property Law made progress in strengthening ownership rights and restraining the grabbing hand of local government by providing equal protection to public and private properties and allowing people to own the premises of their houses or businesses (Zhang, 2008).

government institution, $I(e) = 1$, otherwise $I(e) = 0$.

A high-type parent's OCP fine is:

$$b(w) = \frac{b_o + I(e)b_e}{\theta w} \tag{5}$$

We consider the OCP fine, $b_o + I(e)b_e$, to be much smaller than an individual's life-time wealth, that is, $b(w) \ll 1$.

The budget constraint to which an individual is subject is:

$$ne + (n-1)b(w) + \mathrm{m} = w \tag{6}$$

The equation of consumption for low-type individuals is calculated from equations (4) and (6) as:

$$m = w - (\mathrm{n}-1)\left(\frac{b_o + I(e)b_e}{w}\right) - ne \tag{7}$$

Plugging Equation (7) into (3), we have:

$$\max \; U(n,e) = \ln n + \alpha \ln e + \beta\left(w - (n-1)\left(\frac{b_o + I(e)b_e}{w}\right) - \mathrm{ne}\right)$$

We derive the first order condition with respect to n and e, respectively, as follows:

$$\frac{\partial U}{\partial n} = \frac{1}{n} - \beta\left(\left(\frac{b_o + I(e)b_e}{w}\right) + e\right) = 0 \tag{8}$$

$$\frac{\partial U}{\partial e} = \frac{\alpha}{e} - \beta n = 0 \tag{9}$$

Combining Equations (8) and (9), we have:

$$e_l = \frac{\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right) \tag{10}$$

$$n_l = \frac{(1-\alpha)}{\beta}\frac{1}{\left(\frac{b_o + I(e)b_e}{w}\right)} \tag{11}$$

Similarly, for high-type individuals, we have:

$$e_h = \frac{\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) \tag{12}$$

$$n_h = \frac{(1-\alpha)}{\beta} \frac{1}{\left(\frac{b_o + I(e) b_e}{\theta w}\right)} \tag{13}$$

where n_l represents the fertility of a low-type individual, and n_h represents the fertility of a high-type individual.

The probability of a child of low-type parents becoming high-type is denoted as p, and the probability of a child of high-type parents becoming high-type is denoted as q. As in Fan and Zhang (2013), we assume the probabilities p and q are:

$$p = 1 - exp(-e_l)$$

$$q = 1 - exp(-e_h)$$

where e_l is the educational level of children whose parents are of the low type and e_h is the educational level of children whose parents are of the high type.

We also consider the potential contributions of assortative mating and returns to education:

1) Assortative mating

Both types of parents have an impact on the outcome of their children. For example, a child born to a high-type/low-type pair of parents is more likely to be low type than a child born to a high-type/high-type pair of parents, and the latter child is more likely to marry a high-type spouse than the former. We introduce a coefficient of assortative mating, A, to characterize the similarity level of the husband and wife. A greater A value results in a greater proportion of identical-type parents and a lower proportion of mixed-type parents, raising q while decreasing p. A is normalized to follow a uniform distribution from 0 to 1.

2) Returns to education.

A higher return to education widens the wage disparity between skilled and unskilled workers, amplifying the impact of parents' investment in education. We introduce a coefficient of the return to education, R, to characterize the amplifying effect of education investment. R is normalized to a uniform distribution, $U\ (0, 1)$.

To maintain the center of the probability as the transformation of AR, we set the form of p and q as:

$$p = 1 - exp(-ARe_l + (AR-1)log2)$$

$$q = 1 - exp(-ARe_h + (AR-1)log2)$$

By definition, upward mobility share can be denoted as:

$$M_u = 1 - exp(-ARe_l + (AR-1)\log 2)$$

$$= 1 - exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right) + (AR-1)log2\right)$$

and downward mobility share is:

$$M_d = 1 - \left(1 - exp(-ARe_h)\right) = exp(-ARe_h)$$

$$= exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) + (AR-1)log2\right)$$

Therefore, persistence share is:

$$P = 1 - \frac{1}{2}M_u - \frac{1}{2}M_d = 1 - \frac{1}{2}\left(1 - exp(-ARe_l)\right) - \frac{1}{2}exp(-ARe_h)$$

$$= 1 - \left(1 - exp(-ARe_l) - \frac{1}{2} + \frac{1}{2}exp(-ARe_l)\right) - \frac{1}{2}exp(-ARe_h)$$

$$= 1 - 1 + exp(-ARe_l) + \frac{1}{2} - \frac{1}{2}exp(-ARe_l) - \frac{1}{2}exp(-ARe_h)$$

$$= \tfrac{1}{2} + \tfrac{1}{2}exp(-ARe_l) - \tfrac{1}{2}exp(-ARe_h) \qquad (14)$$

Combining Equation (14) with Equations (10) and (12), we have:

$$P = \frac{1}{2} + \frac{1}{2}exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right) + (AR-1)log2\right)$$

$$- \frac{1}{2}exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) + (AR-1)log2\right) \qquad (15)$$

We derive the following propositions by taking derivatives of P with respect to various variables:

Proposition 1. *The higher the extra OCP fine, b_e, for employees in government institutions, the lower the persistence share, P (or the higher the intergenerational mobility, that is, $\frac{\partial P}{\partial b_e} \leq 0$; see Appendix B1.1 for proof).*

Proposition 2. *The higher the assortative mating level, A, the greater the persistence share, P (or the lower the intergenerational mobility, that is, $\frac{\partial P}{\partial A} > 0$; see Appendix B1.2 for proof).*

Proposition 3. *The higher the OCP penalty, b_o, the lower the persistence share, P (or the higher*

the intergenerational mobility; that is, $\frac{\partial P}{\partial b_o} < 0$*; see Appendix B1.3 for proof).*

Proposition 4. *The lower returns to education,* R*, due to the smaller population of the child generation generated by the OCP, the lower the persistence share, P (or the higher the intergenerational mobility; that is,* $\frac{\partial P}{\partial R} > 0$*; see Appendix B1.4 for proof).*

7. Mechanisms

In this section, we discuss three potential channels through OCP may affect intergenerational persistence, in line with the four propositions in Section 6. These channels are (1) the weakening of elite family heirship through reduced child numbers and assortative mating, (2) resource concentration, and (3) the decline in returns to education.

7.1. The weakening of elite family heirship

We will first explore whether the OCP has changed the conventional hierarchy of elite families, rendering "blooded aristocratic" households unsustainable. Chinese aristocratic culture places a strong emphasis on family, which results in an enduring family legacy of transferring wealth and endowments between ancestors and descendants (Tanner and Feder, 1993; Schmidt-Glintzer and Jansen, 1994). The change in conventional family heirship system could break traditional aristocratic rule, triggering a major socioeconomic reshuffle in Chinese society.

The traditional political elite family used to have many children so that a child, like their parents, could pass on capital and endowments to siblings, supporting family growth generation after generation (Feng, Poston *Jr.*, Wang, 2014; Chen and Jordan, 2018). Elite officials (those in leadership positions) were more severely constrained by the OCP because it was created and implemented by local governments, and they were therefore more likely to adhere to local policies. Their families frequently held positions of great authority and wealth and were generally regarded as political elite families (Shambaugh, 2008; Chen and Kung, 2019; Han and Gao, 2019). Therefore, as shown in Proposition 1, the drop in the number of children in these households would likely result in a reduction in the resources and jobs that they could hold.[40] We use a DDD regression to test the hypothesis that the OCP reduces the number of children in elite families as

[40] Appendix B2 shows another mathematical proof for Proposition 1. In this new proof, we explain how a decrease in the number of children from elite families raises the rank of children from non-elite families, which lowers intergenerational persistence for the next generation, thereby supporting Proposition 3 from a different angle.

follows:

$$Child_number_{iht} = \beta \times Fine_{hp} \times Parent_level_{iht} \times I(h > 1979) + \beta_l Parent_high_level_{iht} + \beta_f Fine_{hp} \times I(h > 1979) + G_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (17)$$

where $Child_number_{iht}$ is the total number of children, $Parent_level_{iht}$ is a dummy variable indicating whether at least one parent is an elite official, which is here defined as equal or higher than the lowest levels of leadership in China's administrative system ("Section-Director (*xiang ke ji*)" or "Deputy Section-Director (*fu xiang ke ji*)"), and G_i is the gender. The remaining variables are identical to those in the baseline (Equation 1). The essential coefficient for determining if elite officials have different child populations is β, the coefficient of the interacting term between OCP intensity and whether a parent is an elite official. Results show a negative value of β, -0.680 (Column 1, Table 2), which means that the number of children for elite officials drops by 0.680 for every fine equal to annual income, compared to those who are not elite officials. This further suggests that elite officials are more compliant with the OCP and have fewer children.

[Table 2 approximately here]

Elite families in China have typically had multiple offspring and married into other elite households (Watson et al., 1991; Holmgren, 1991; Yan, 2012). However, the OCP limited the number of offspring, making it less likely that two individuals from elite families would come together through assortative mating. Because of this, the high hierarchy of the elite households might be challenging to uphold or reinforce, leading to less intergenerational persistence, as shown in Proposition 2.

Empirically, we examine whether the OCP reduces assortative mating using the following series of DDD regressions:

$$Spouse_rank_{iht} = \beta \times Fine_{hp} \times rank_{iht} \times I(h > 1979) + \beta_o rank_{iht} + \beta_f Fine_{hp} \times I(h > 1979) + G_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (18)$$

where $rank_{iht}$ is the individual's rank in income, education, or social class, and $Spouse_rank_{iht}$ is the spouse's rank. The remaining variables are identical to those in Equation (17). β is the crucial coefficient showing how OCP influences assortative mating.

Results show that the coefficients for income (-0.095; Column 1, Table 2), education (-0.061;

Column 2, Table 2) and social class (-0.030; Column 3, Table 2) are significantly negative, indicating that an OCP fine equal to one year's income will decrease spouse income, education, or social class rank by 9.5, 6.1 and 3.0 percentage points, respectively. The coefficients suggest that the OCP indeed decreased assortative mating.

Evidence therefore suggests that the OCP reduced intergenerational persistence by weakening elite family heirship, especially by reducing the number of children born to political elites and weakening assortative mating.

7.2. Resource concentration for poor families

As is demonstrated in Proposition 3, the threat of OCP fines can reduce the number of OCP violations, and the policy effect is likely to be greater for the poorer 50% of families. OCP fines affected poor families more severely than wealthy families in terms of the proportional cost of the OCP. As a result, it is likely that OCP had a greater impact on poor families' capacity to have children than on wealthy ones.

Children from poor families are thus likely to receive greater shares of their family's resources than they otherwise would because they had fewer or no siblings. This beneficial effect also included non-financial advantages like companionship and parentally taught non-cognitive skills (Carneiro and Heckman, 2003; Cuhna and Heckman, 2007). Therefore, the OCP may indirectly support future success and upward mobility in low-income households, especially by increasing the focus of resources on a single child.

We use a series of DDD regressions to investigate the impact of OCP on wealthy and poor families in order to empirically test for the presence of this channel of resource concentration. We analyze outcomes including the number of siblings, marriage gifts (measured by logarithms of the sum of cash and cash equivalents), education investment from parents (measured by the total amount spent on schooling), and the quantiles of non-cognitive skills (self-efficacy, ambition, and trust in people; all measured by standardizing the corresponding sum of scale scores). The specification is as follows:

$$Outcomes_{iht} = \beta_p Poor_{ift} + \beta_f Fine_{hp} \times I(h > 1979) + \beta \times Fine_{hp} \times Poor_{ift} \times I(h > 1979) + G_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (19)$$

where $Poor_{ift}$ is a dummy variable indicating whether the parents' income is in the lower half of the population. The rest of the variables are the same as those in Equations (17) and (18).

With a coefficient of -0.166 (Column 1), the findings in Table 3 confirm our hypothesis that the OCP has had a greater impact on reducing number of children in lower-income families more than in higher-income families. According to the coefficient, each fine equal to one year's income results in 0.166 fewer siblings for poor people than for wealthy ones.

In line with reducing child numbers, the OCP increased parents' marriage gifts to and educational investments in their children for lower-income families more than those for higher-income families.[41] Parents in lower-income families gave their children 45.1% more marriage gifts (Column 2) and invested 74.9% more in their children's education (Column 3) for every year of income equivalent OCP fine. The OCP also improves the cognitive skills of children from lower-income families. Children from lower-income households show greater levels of self-efficacy by 4.282 standard deviations (Column 4), ambition by 5.107 standard deviations (Column 5), and trust in others by 2.304 standard deviations for every one-year income equivalent OCP fine (Column 6).

[Table 3 approximately here]

In summary, our findings support the hypothesis that the OCP concentrated resources for lower-income families by reducing their child numbers. Specifically, the OCP increased the marriage gifts, educational investments, and cognitive skills that children from lower-income families received, which facilitated their competitiveness with children from wealthy families, echoing Proposition 3.

7.3. The decrease in returns to education

As detailed in Proposition 4, intergenerational persistence is predicted to drop due to lower returns to education. Children from more wealthy families acquire higher levels of education and are thus more likely to become skilled workers (Figure A3). As the wage gap between children from wealthy and poor families narrows, intergenerational mobility has been demonstrated to grow (Bütikofer, Dalla-Zuanna, and Salvanes, 2018).

Therefore, we investigate whether the OCP lowers returns to education. The OCP reduces labor

[41] In Chinese tradition, marriage gifts from parents typically include big cash gifts, properties, and the down payment on a house.

supply among the next generation of workers, forcing low-end labor markets to raise wages to maintain adequate labor supply. This is predicted to reduce the wage gap between skilled and unskilled workers. To test this hypothesis, we employ similar DDD regressions to Equations (17)–(19) as follows.:

$$ln\ (income)_{iht} = \beta_e education_dummy_{iht} + \beta_f Fine_{hp} \times I(h > 1979) + \beta \times Fine_{hp} \times education_{dummy_{iht}} \times I(h > 1979) + G_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (20)$$

where $ln\ (income)_{iht}$ is the logarithm of income, and we use the education dummy variable, $education_dummy_{iht}$, to represent individual i's education level, including whether or not individual i has completed junior high school, high school, or college. The remaining variables are the same as those in Equations (17)– (19). β is the key coefficient of the interaction term of OCP intensity and education level dummy, which is used to test the hypothesis.

Results show that OCP consistently lowers returns to education for every OCP fine equal to annual income, reducing returns by 96.1 percentage points for middle school, 104.7 points for high school, and 114.8 points for college degrees (Table 4). This supports the existence of a channel of returns to education.

[Table 4 approximately here]

We also investigate whether the OCP changed the long-held son preference in traditional Chinese culture, since the OCP limits parents' options for having additional children in order to produce a son if their first child is female. Parental investment in sons versus daughters is influenced by son preference, which may have an effect on intergenerational persistence. Our empirical data suggest no discernible change in son preference (Table C1).

In addition, we investigate whether the OCP changed the migrating behavior of the children, since being an only child might have affected willingness to migrate—either negatively, as parents would be left without support at home or as easier cohabitation with parents reduces costs, or positively, as migration requires resources, and people migrate for better opportunities in work, education, etc.., leading to the change in intergenerational persistence. The regression results show that the OCP decreases migrating behaviors, suggesting that migration is not a channel to affect intergenerational persistence in China's context (Table C2).

8. Conclusion

This study examines the effects of the One-Child Policy (OCP) on income, education, and social class persistence. We first develop a theoretical model to explain how the OCP can affect intergenerational persistence, and then use the diff-in-diff method to estimate the OCP's causal impacts on intergenerational persistence in China. We find that the OCP has reduced intergenerational income, education, and social class persistence in China.

In addition, we present three channels—which correspond to the theoretical model's deductions—through which the OCP might affect intergenerational persistence. The first channel is the breakdown of the conventional "blooded aristocracy" by the OCP's weakening of elite family heirship through reducing child number and assortative mating. The second channel is the concentration of resources on a single child, which enables the only child of a poor family to amass resources equivalent to those of a child from a wealthy household. The third channel is the OCP's reduction in returns to education, which has an impact on intergenerational persistence for all children, whether or not their parents complied with the policy. We find empirical evidence for the existence of these channels.

Although results suggest that the OCP unintendedly increased the intergenerational mobility in China and may thus help to provide nonviolent alternatives to bloody revolutions and regime changes that had formed a cycle of recursive incidences for hundreds of times in Chinese history, this does not imply that the OCP is good in general. The side effects of this mandatory birth control policy include the deprivation of life, loss of reproductive freedom, and distortion of gender ratio. The long-run consequences of the OCP still need prudent study.

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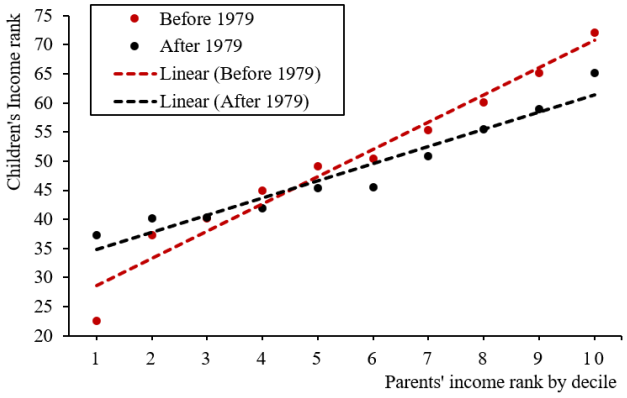
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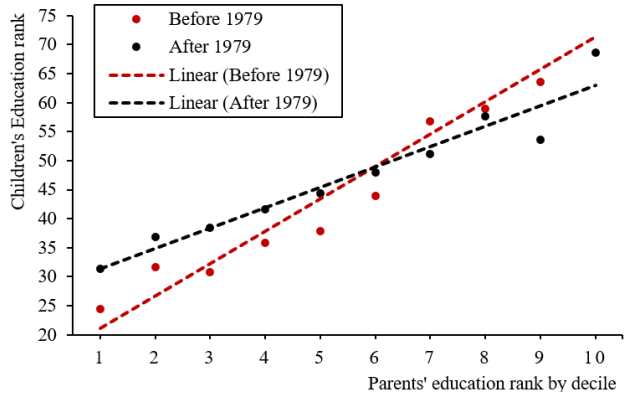
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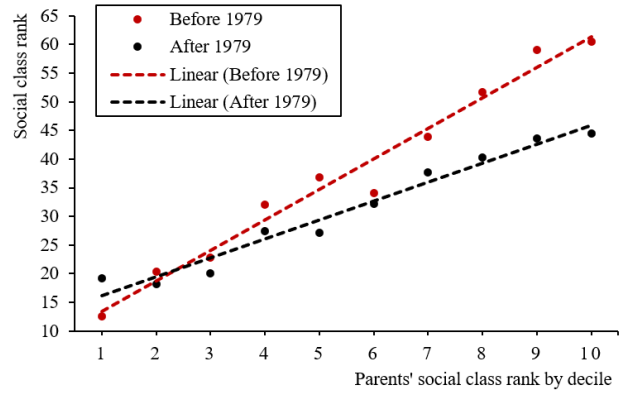
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(a) Income Rank



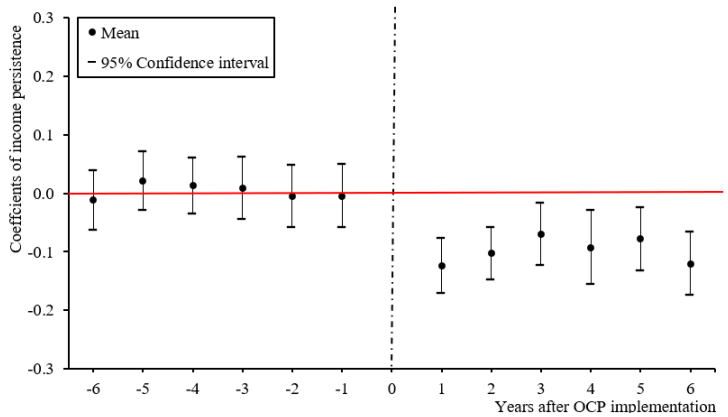
(b) Education Rank



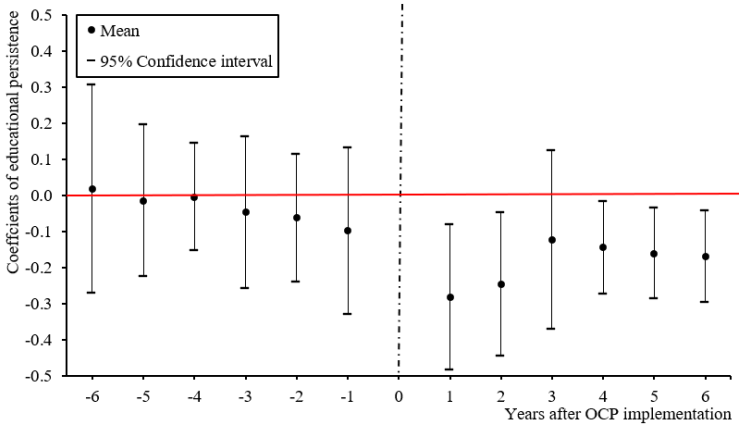
(c) Social Class Rank

Figure 1. The Intergenerational mobility before and after OCP implementation in 1979

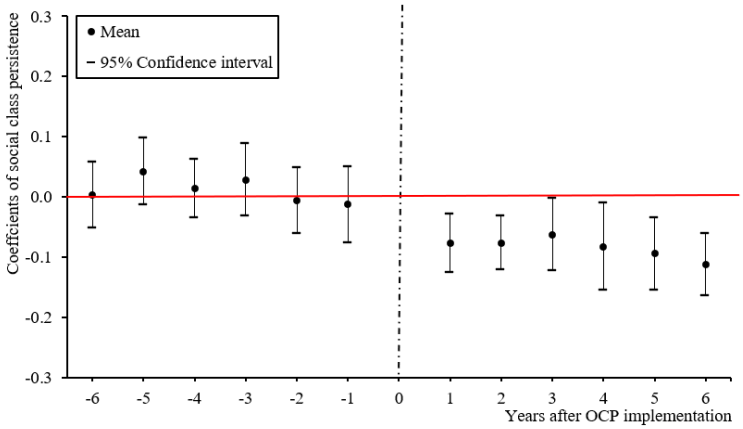
Note: (a), (b) and (c) visualizes the dynamic effects of OCP fines on income, education, and social class. Y-axis are children's income, education and social class ranking by percentile, X-axis are Parents' income, education and social class ranking by decile. The dash lines are linear predictions of the relation, steeper slopes stand for higher intergenerational persistence (lower intergenerational mobility). Red dots and lines are before OCP implementation (in 1979), black dots and lines are after OCP implementation.



(a) Relative Income Persistence



(b) Relative Educational Persistence



(c) Relative Social class Persistence

Figure 2. The Dynamic Impacts of OCP Fines on the Intergenerational Persistence

Note: (a), (b) and (c) visualizes the dynamic effects of OCP fines on income, education, and social class persistence respectively for 1973–1985 cohorts, excluding the 1979 cohort, where the solid line connects the estimates and the dashed lines indicate the 95% confidence intervals with standard errors clustered at the province level.

Table 1. One-child Policy Effect on Relative Persistence

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine: (1976–1978) v. (1980–1982)	-0.095***	-0.155**	-0.082***
	(0.014)	(0.072)	(0.015)
Observations	72,365	345,462	73,609
R-squared			
Parent's Rank X Fine: (1974–1978) v. (1980–1984)	-0.090***	-0.155***	-0.086***
	(0.010)	(0.055)	(0.010)
Observations	113,677	558,289	115,812
R-squared			
Parent's Rank X Fine: (1969–1978) v. (1980–1989)	-0.105***	-0.163***	-0.109***
	(0.009)	(0.037)	(0.010)
Observations	213,940	1,102,717	218,277
R-squared			
Parent's Rank X Fine: (1949–1978) v. (1980–1996)	-0.133***	-0.135***	-0.144***
	(0.010)	(0.021)	(0.011)
Observations	348,207	2,031,504	358,158
R-squared			
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class within their cohort, and the key independent variable is the OCP fine rate at the province-year level interacting with the rank of their parents' corresponding rank within the parents' cohort. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table 2. OCP Effect on Amplifying Family Power: Assortative Mating and Effect on Elite Officials

	Child number	Spouse income rank	Spouse education rank	Spouse social class rank
	(1)	(2)	(3)	(4)
Elite officials	-0.076			
	(0.058)			
Elite officials X fine	-0.763***			
	(0.074)			
Income rank		0.358***		
		(0.011)		
Income rank X fine		-0.101***		
		(0.012)		
Education rank			0.307***	
			(0.025)	
Education X fine			-0.062***	
			(0.020)	
Social class rank				0.316***
				(0.010)
Social class rank X fine				-0.033**
				(0.012)
Gender	-0.045***	-16.936***	-9.307***	-12.947***
	(0.010)	(0.593)	(0.917)	(0.396)
Fine	0.090	4.886***	0.942	3.451***
	(0.067)	(0.972)	(0.874)	(0.692)
Province FE	X	X	X	X
Cohort FE	X	X	X	X
Dataset FE	X	X	X	X
Year FE	X	X	X	X
Observations	1,554,781	177,957	1,316,042	187,693
R-squared	0.204	0.307	0.191	0.265

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on different outcome variables. The dependent variables are rank of spouse income, education, social class, and child number. All regressions are controlled for survey year Fixed Effects (FE), province FE, and cohort FE. Robust standard errors are in parentheses and clustered by province. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table 3. OCP Effect on Poor vs. Rich Families: Number of Siblings, Birth Age, Wealth Transmission and Non-Cognitive Skills

	Number of siblings	Marriage gift from parents	Education investment from parents	Self-efficacy	Ambition	Trust
	(1)	(2)	(3)	(4)	(5)	(6)
Poor X fine	-0.136***	0.905***	0.656**	8.575***	5.659**	2.586***
	(0.035)	(0.314)	(0.245)	(3.061)	(2.716)	(0.723)
Poor	0.108***	-0.409	0.231	-0.001	5.097	-0.580
	(0.025)	(0.243)	(0.194)	(2.033)	(3.908)	(0.427)
Fine	0.144***	-1.076***	0.109	-1.648	-5.905**	-2.677**
	(0.043)	(0.327)	(0.634)	(2.477)	(2.268)	(1.013)
Gender	-0.196***	0.563	0.448***	1.270	1.734	-1.493***
	(0.016)	(0.519)	(0.120)	(1.063)	(1.310)	(0.247)
Province FE	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X
Dataset FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Observations	351,671	1,372	15,712	4,851	3,472	49,422
R-squared	0.416	0.219	0.219	0.061	0.147	0.091

Note Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on different outcome variables between poor (the lower-half) and rich families. The dependent variables are sibling number, logarithms of marriage gift from parents and education investment from parents, and the quantiles of non-cognitive skills: self-efficacy, ambition, and trust to people around. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are in parentheses and clustered by province. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table 4. OCP Effect on Returns to Education

	Income, middle school (1)	Income, high school (2)	Income, college (3)
Gender	0.877***	0.956***	1.014***
	(0.046)	(0.048)	(0.049)
Fine	0.710***	0.542***	0.222*
	(0.120)	(0.124)	(0.113)
Middle school	1.747***		
	(0.062)		
Middle school X Fine	-1.005***		
	(0.081)		
High school		1.826***	
		(0.085)	
High school X Fine		-1.137***	
		(0.090)	
College			1.861***
			(0.095)
College X Fine			-1.277***
			(0.101)
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X
Observations	523,733	523,733	523,733
R-squared	0.286	0.284	0.269

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) effect on the return of education on income. we use the returns to education dummies: middle school (column 1), high school (column 2) and college (column 3). The dependent variables are logarithms of income. All regressions are controlled for survey year Fixed Effects (FE), province FE, and cohort FE. Robust standard errors are clustered by province in parentheses. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Appendix A. Supplemental Figures and Tables to the Main Text

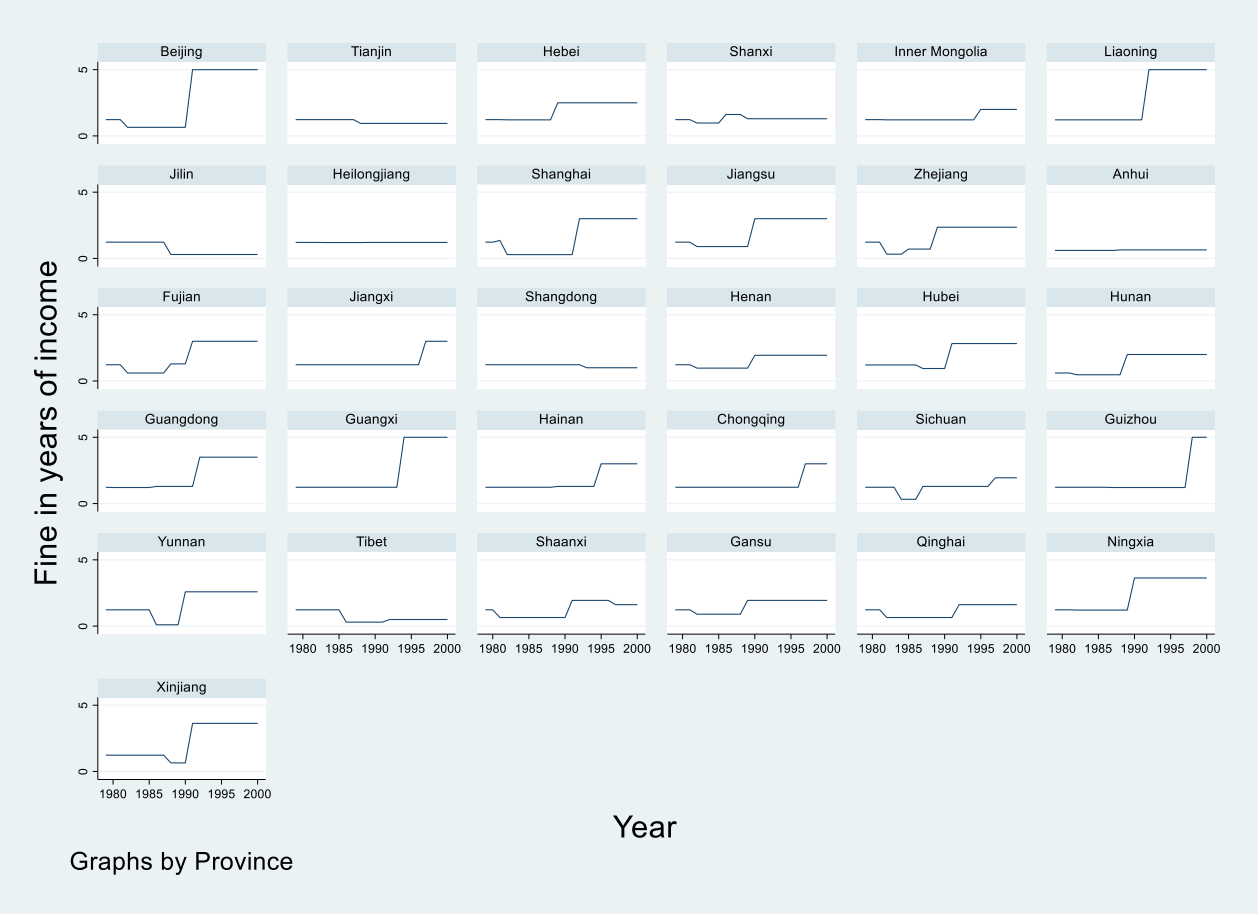
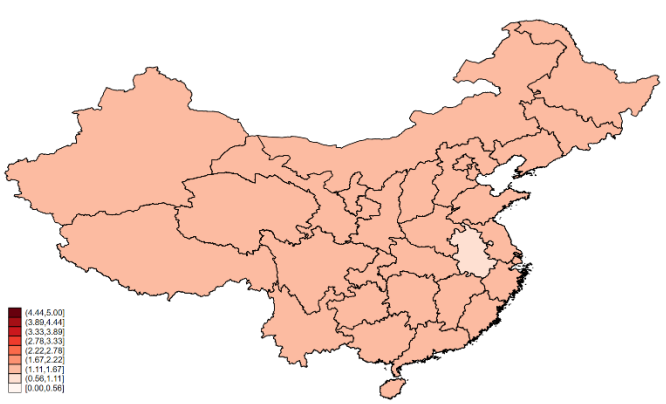
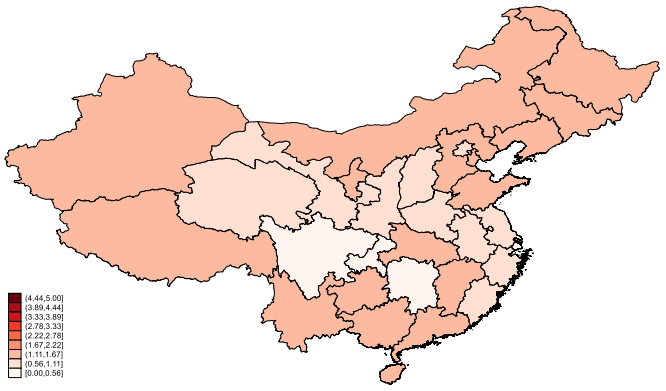


Figure A1. The OCP Fine Rates from 1979–2000, by Province

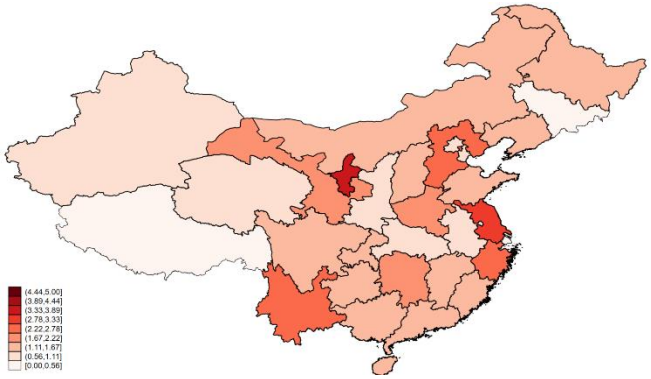
Note: Data come from Ebenstein (2010). The fine rates are measured as multiples of local household annual income in 31 provinces and municipalities from 1979 to 2000. The severity of the fine rates reflects the strength of the OCP's implementation.



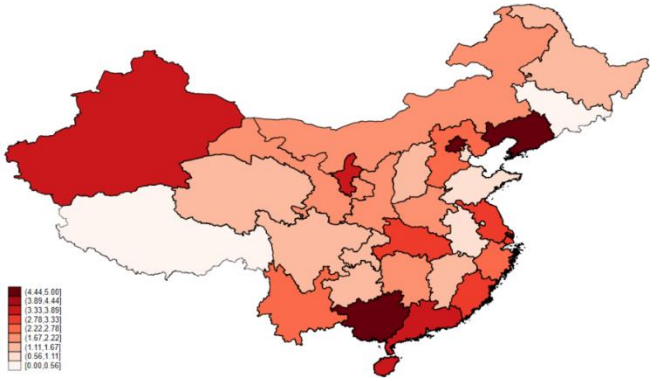
(a) OCP Fine Rates in 1980



(b) OCP Fine Rates in 1985



(c) OCP Fine Rates in 1990



(d) OCP Fine Rates in 1995

Figure A2. The OCP Fine Rates across Mainland China over Times

Note: These are heat maps visualizes the OCP fine rates (Ebenstein 2010) in each province in different times: 1980, 1985, 1990 and 1995. The maps depict how fine rates changed geographically over time in different provinces. Lighter colors correspond to lower fine rate.

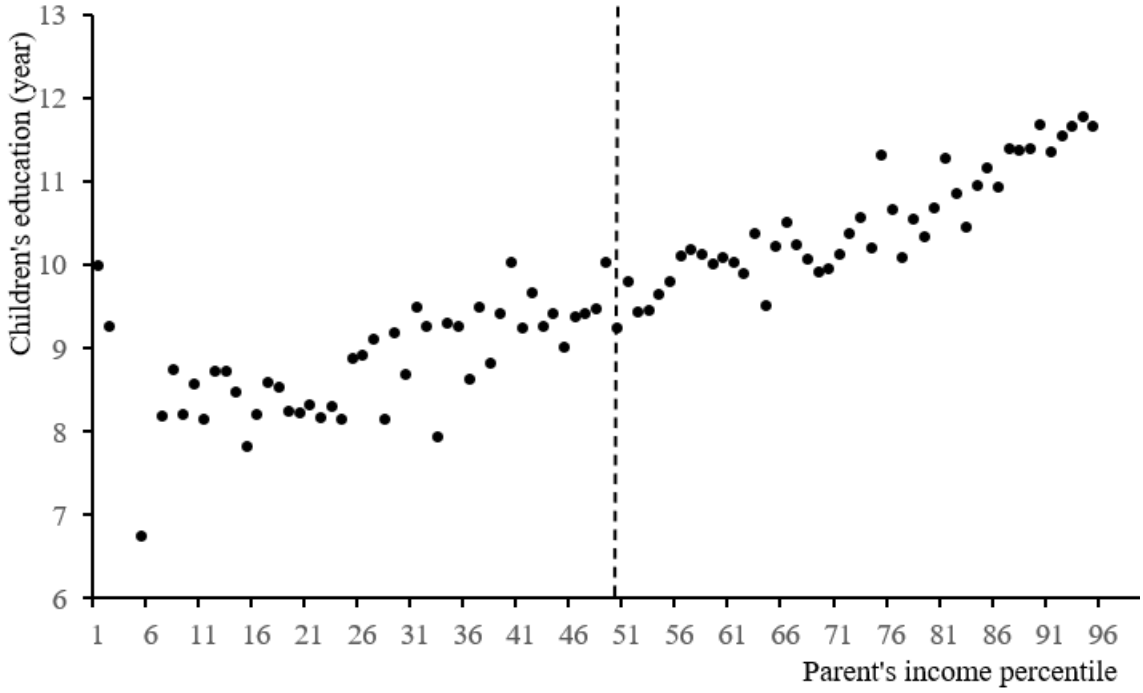


Figure A4. Children from rich families are more educated (cohorts after 1979)

Note: In this figure, the independent variable is the income percentile of parents, and dependent variable is the education years of children. The vertical dashed line is at the 50th percentile. The sample use is the cohorts after 1979. The figure shows that children from richer people receive higher education.

Table A1. The Change of China's One-Child Policy and Fertility Rate

Year	Fertility Rate
1971	2.9379
1972	2.8956
1973	2.7809
1974	2.7986
1975	2.6162
1976	2.7920
1977	2.6768
1978	2.6448
1979	2.5152
1980	2.3851
1981	2.2789
1982	2.3296
1983	2.1815
1984	1.7909
1985	1.8448
1986	1.8347
1987	1.9498
1988	1.9895
1989	1.8217
1990	1.7209

Note: The Fertility refer to Total Fertility Rate (Woman average birth number dividing by life expectancy). The Calculation is based on the Sample: Chinese Household Finance Survey (CHFS) 2013, 2015, and 2017 waves, China Family Panel Studies (CFPS) 2010, 2012, 2014, 2016, and 2018 waves, China Health and Retirement Longitudinal Study (CHARLS) 2011, 2013, 2015, and 2018 waves, China Health and Nutrition Study (CHNS) 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011, and 2015 waves, Chinese General Social Survey (CGSS), the fifth National Population Census of China Long Form database (2000, 2005), Chinese Social Survey (CSS) 2006, 2008, 2011, 2013, 2015 and 2017 waves, Urban household survey (UHS) 1986-2015 waves, Chinese Household Income Project (CHIP) 1988, 1995, 1999, 2002, 2007, 2008 and 2013 waves and China Labor-force Dynamics Survey 2012, 2014, and 2016 waves.

Table A2. P-Values for Correlations of Fertility Fines with Macro Variables

Provincial-level variables	Current year (1)	Previous 1 year (2)	Previous 2 years (3)	Previous 3 years (4)
Birth rate	0.246	0.204	0.182	0.232
Gini coefficient	0.736	0.694	0.782	0.861
Population	0.242	0.240	0.242	0.244
Ratio of elite officials in provincial population	0.739	0.715	0.205	0.336
Sexist social belief (proxied by the density of genealogy books)	0.653	–	–	–
Sexist social belief (proxied by the gender survival ratio of the Great Famine)	0.165	–	–	–
Returns to education	0.865	–	–	–
GDP				
Provincial college admission rate				

Note: We perform univariate regression of fine rates on each provincial-level variable and clustered by province. The Gini figures come from the China Statistical Yearbook. To achieve stational sequences for the two variables, we use first order difference. The government manager ratio is derived from the same source as in Section 7.2, in which government manager is defined as positions equal or higher than "Section-Director (*xiang ke ji*)" or "Deputy Section-Director (*fu xiang ke ji*)". Genealogy book density is derived from the Shanghai Library's *Comprehensive Catalog of Chinese Genealogy (Zhongguo Jiapu Zongmu)*. Data on return to educate comes from Xing, Chen, and Cao (2021). The first four datasets are panel, while the remaining are cross-sectional.

Table A3. Relative Persistence Estimates at the National Level: Different Demographic Groups

Dependent variable: Children's rank	Before 1979 (1)	After 1979 (2)
Parents' income rank	0.513***	0.258***
	(0.022)	(0.018)
Observations	245,977	111,359
R-squared	0.228	0.075
Parents' education rank	0.483***	0.318***
	(0.039)	(0.033)
Observations	1,828,502	220,117
R-squared	0.228	0.098
Parents' social class rank	0.503***	0.269***
	(0.025)	(0.024)
Observations	253,081	114,211
R-squared	0.166	0.068

Note: We report in each cell the rank-rank slope of the OLS regressions of an outcome of a child rank on his parents associated rank. Robust standard errors are in parentheses, and number of observations in the third rows. ***, **, and * indicate statistical significance at the 1%, 5%, and 10%, respectively.

Sample: Chinese Household Finance Survey (CHFS) 2013, 2015, and 2017 waves, China Family Panel Studies (CFPS) 2010, 2012, 2014, 2016, and 2018 waves, China Health and Retirement Longitudinal Study (CHARLS) 2011, 2013, 2015, and 2018 waves, China Health and Nutrition Study (CHNS) 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011, and 2015 waves, Chinese General Social Survey (CGSS), the fifth National Population Census of China Long Form database (2000, 2005), Chinese Social Survey (CSS) 2006, 2008, 2011, 2013, 2015 and 2017 waves, Urban household survey (UHS) 1986-2015 waves, Chinese Household Income Project (CHIP) 1988, 1995, 1999, 2002, 2007, 2008 and 2013 waves and China Labor-force Dynamics Survey 2012, 2014, and 2016 waves. Sample is the same in tables below.

Table A4. Information on Datasets

Dataset	Wave	Data Type	Investigators	Obs. number in our sample
Chinese Household Finance Survey (CHFS)	2013, 2015, 2017	Longitudinal data	Southwestern University of Finance and Economics	98,931
China Family Panel Studies (CFPS)	2010, 2012, 2014, 2016, 2018	Longitudinal data	National Natural Science Foundation of China, Peking University	147,271
China Health and Retirement Longitudinal Study (CHARLS)	2011, 2013, 2015, 2018	Longitudinal data	National Natural Science Foundation of China, Peking University	170,062
China Health and Nutrition Study (CHNS)	1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011, 2015	Multi-part longitudinal data	National Institute of Nutrition and Food Safety, China Center for Disease Control and Prevention, Carolina Population Center, the University of North Carolina at Chapel Hill	21,834
Chinese General Social Survey (CGSS)	2003, 2006, 2008, 2010, 2011, 2012, 2013, 2015	Multi-part longitudinal data	National Survey Research Center at Renmin University of China	51,669
National Population Census of China (NPCC) Long Form database	2000, 2005	Cross sectional data	National Bureau of Statistics of the People's Republic of China	1,393,374
Chinese Social Survey (CSS)	2006, 2008, 2011, 2013, 2015 2017	Multi-part longitudinal data	Institute of sociology, Chinese Academy of Social Sciences	23,547
Urban household survey (UHS)	Every year during 1986-2015	Multi-part longitudinal data	National Bureau of Statistics of the People's Republic of China	99,157
Chinese Household Income Project (CHIP)	1988, 1995, 1999, 2002, 2007, 2008 2013	Multi-part longitudinal data	Institute of economics, Chinese Academy of Social Sciences	53,948
China Labor-force Dynamics Survey (CLDS)	2012, 2014, 2016	Longitudinal data	Sun Yat-Sen University	37,185

Note: This table lists all of the data sources that we used to create our main dataset.

Table A5. Summary Statistics

Variables	Observations	Mean	Std. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)
Children:					
Gender: Male = 1	2,096,978	0.73	0.44	0	1
Age	2,096,978	35.91	10.27	23	69
Marriage status: Married and live together = 1	2,079,345	0.75	0.43	0	1
Income	540,307	123793.70	1111594.00	0	20000000
Education years	2,086,168	7.93	3.75	0	22
Social class (0 = lowest, 4 = highest)	551,236	0.56	0.70	0	4
Only child = 1	1,972,726	0.11	0.31	0	1
Majority ethnicity = 1	1,941,590	0.91	0.29	0	1
Rural = 1	1,798,014	0.63	0.48	0	1
Born at Rural = 1	2,071,505	0.65	0.48	0	1
Parents:					
Father income	347,541	39281.18	509478.80	0	9999999
Mother income	376,309	6786.19	20348.97	0	6300000
Father education years	1,814,276	6.76	4.43	0	22
Mother education years	1,975,662	5.22	4.65	0	22
Father social class (0 = lowest, 4 = highest)	357,293	0.55	0.68	0	4
Mother social class (0 = lowest, 4 = highest)	385,979	0.41	0.56	0	4

Note: Data for our sample are compiled from Chinese Household Finance Survey (CHFS) 2013, 2015, and 2017 waves, China Family Panel Studies (CFPS) 2010, 2012, 2014, 2016, and 2018 waves, China Health and Retirement Longitudinal Study (CHARLS) 2011, 2013, 2015, and 2018 waves, China Health and Nutrition Study (CHNS) 1989, 1991, 1993, 1997, 2000, 2004, 2006, 2009, 2011, and 2015 waves, Chinese General Social Survey (CGSS), the fifth National Population Census of China Long Form database (2000, 2005), Chinese Social Survey (CSS) 2006, 2008, 2011, 2013, 2015 and 2017 waves, Urban household survey (UHS) 1986-2015 waves, Chinese Household Income Project (CHIP) 1988, 1995, 1999, 2002, 2007, 2008 and 2013 waves and China Labor-force Dynamics Survey 2012, 2014, and 2016 waves. Tables below are the same.

Table A6. Sample Counts by Birth Cohort

Cohorts	Number of observations	Percent (%)	Cum. Percent (%)
1949	22,134	1.06	1.06
1950	24,401	1.16	2.22
1951	25,271	1.21	3.42
1952	32,361	1.54	4.97
1953	30,898	1.47	6.44
1954	36,550	1.74	8.18
1955	37,035	1.77	9.95
1956	36,479	1.74	11.69
1957	42,509	2.03	13.72
1958	37,209	1.77	15.49
1959	31,362	1.50	16.99
1960	35,377	1.69	18.67
1961	29,581	1.41	20.08
1962	56,654	2.70	22.79
1963	74,487	3.55	26.34
1964	66,525	3.17	29.51
1965	68,161	3.25	32.76
1966	70,285	3.35	36.11
1967	67,848	3.24	39.35
1968	90,732	4.33	43.68
1969	86,115	4.11	47.78
1970	102,194	4.87	52.66
1971	103,286	4.93	57.58
1972	96,201	4.59	62.17
1973	91,122	4.35	66.51
1974	95,214	4.54	71.05
1975	88,858	4.24	75.29
1976	107,508	5.13	80.42
1977	108,186	5.16	85.58
1978	38,884	1.85	87.43
1979	37,256	1.78	89.21
1980	31,262	1.49	90.7
1981	31,742	1.51	92.21
1982	34,303	1.64	93.85
1983	16,525	0.79	94.64
1984	15,315	0.73	95.37
1985	14,992	0.71	96.08
1986	15,669	0.75	96.83
1987	15,362	0.73	97.56
1988	13,494	0.64	98.21
1989	11,888	0.57	98.77
1990	9,823	0.47	99.24
1991	5,755	0.27	99.52

1992	4,159	0.20	99.71
1993	2,770	0.13	99.85
1994	1,679	0.08	99.93
1995	1,434	0.07	99.99
1996	123	0.01	100
Total	2,096,978		

Note: The table shows the number of the observations by each cohort from the data sets we have constructed.

Table A7. Sample Counts by Survey year

Survey year	Number of observations	Percent (%)	Cum. Percent (%)
1986	1,406	0.07	0.07
1987	1,414	0.07	0.13
1988	7,951	0.38	0.51
1989	2,397	0.11	0.63
1990	1,262	0.06	0.69
1991	2,492	0.12	0.81
1992	1,619	0.08	0.88
1993	3,660	0.17	1.06
1994	1,934	0.09	1.15
1995	10,545	0.50	1.65
1996	3,334	0.16	1.81
1997	5,667	0.27	2.08
1998	4,903	0.23	2.32
1999	5,296	0.25	2.57
2000	902,144	43.02	45.59
2001	1,602	0.08	45.67
2002	12,236	0.58	46.25
2003	8,600	0.41	46.66
2004	10,783	0.51	47.17
2005	505,603	24.11	71.29
2006	18,989	0.91	72.19
2007	10,237	0.49	72.68
2008	22,222	1.06	73.74
2009	19,289	0.92	74.66
2010	28,320	1.35	76.01
2011	52,120	2.49	78.5
2012	49,342	2.35	80.85
2013	89,834	4.28	85.13
2014	41,564	1.98	87.11
2015	99,139	4.73	91.84
2016	40,944	1.95	93.79
2017	42,018	2.00	95.8
2018	78,528	3.74	99.54
2019	9,584	0.46	100
Total	2,096,978		

Note: The table shows the number of the observations by each survey year from the data set we have constructed.

Table A8. Robustness checks, for different clustering methods

	(1976–1978) v. (1980–1982)	(1974–1978) v. (1980–1984)	(1969–1978) v. (1980–1989)	(1949–1978) v. (1980–1996)
	(1)	(2)	(3)	(4)
Panel A				
Income rank X Fine, Baseline	-0.095***	-0.090***	-0.105***	-0.133***
	(0.014)	(0.010)	(0.009)	(0.010)
Income rank X Fine, Clustered by province and data	-0.095***	-0.090***	-0.105**	-0.133**
	(0.024)	(0.025)	(0.033)	(0.048)
Income rank X Fine, Baseline with wild bootstrap standard errors				
Panel B				
Education rank X Fine, Baseline	-0.155**	-0.155***	-0.163***	-0.135***
	(0.072)	(0.055)	(0.037)	(0.021)
Education rank X Fine, Clustered by province and data	-0.155***	-0.155***	-0.163***	-0.135***
	(0.044)	(0.031)	(0.023)	(0.028)
Education rank X Fine, Baseline with wild bootstrap standard errors				
Panel C				
Social class X Fine, Baseline	-0.082***	-0.086***	-0.109***	-0.144***
	(0.015)	(0.010)	(0.010)	(0.011)
Social class X Fine, Clustered by province and data	-0.082***	-0.086***	-0.109***	-0.144**
	(0.017)	(0.015)	(0.025)	(0.048)
Social class X Fine, Baseline with wild bootstrap standard errors				
Education X Fine, Baseline				

Note: This table reports the robustness checks results using different clustering method. Robust standard errors in the baseline analysis are clustered by province. As alternatives, standard errors are two-way clustered by province and survey resources; we also provide wild bootstrap standard errors. Results from all three methods are consistent.
Each cell in this table reports the coefficient from a staggered DID regression of OCP intensity (violation punishment fine rate). The dependent variable is the rank of children's income, education, or social class in their cohort. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE and cohort FE. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the 1%, 5%, and 10%, respectively.

Table A9. The Dynamic Impact of OCP Fines on Relative Persistence

Years after OCP implementation	Income persistence			Educational persistence			Social class persistence		
	Mean	95% Confidential interval		Mean	95% Confidential interval		Mean	95% Confidential interval	
-6	-0.0002	-0.0491	0.0488	0.0185	-0.2709	0.3079	0.0033	-0.0519	0.0585
-5	0.0244	-0.0280	0.0768	-0.0142	-0.2235	0.1951	0.0424	-0.0137	0.0984
-4	0.0294	-0.0179	0.0767	-0.0037	-0.1525	0.1451	0.0141	-0.0348	0.0629
-3	0.0159	-0.0351	0.0669	-0.0472	-0.2571	0.1626	0.0280	-0.0322	0.0881
-2	0.0039	-0.0463	0.0541	-0.0629	-0.2409	0.1151	-0.0060	-0.0607	0.0487
-1	0.0007	-0.0497	0.0511	-0.0988	-0.3298	0.1323	-0.0128	-0.0762	0.0505
0									
1	-0.1207	-0.1647	-0.0766	-0.2820	-0.4843	-0.0797	-0.0773	-0.1261	-0.0285
2	-0.0938	-0.1377	-0.0500	-0.2458	-0.4449	-0.0467	-0.0762	-0.1204	-0.0321
3	-0.0629	-0.1149	-0.0108	-0.1234	-0.3717	0.1248	-0.0629	-0.1230	-0.0028
4	-0.0833	-0.1427	-0.0238	-0.1447	-0.2737	-0.0157	-0.0824	-0.1545	-0.0104
5	-0.0671	-0.1171	-0.0171	-0.1607	-0.2874	-0.0340	-0.0946	-0.1550	-0.0341
6	-0.1170	-0.1674	-0.0667	-0.1693	-0.2961	-0.0426	-0.1120	-0.1634	-0.0607

Note: This table corresponds to Figure 1. It shows the mean and the 95% confidence intervals of the dynamic effects of OCP fines on income persistence and educational persistence respectively for 1970 to 1988 cohorts, excluding the 1979 cohort, also excluding early cohort 1949-1953 as the baseline.

Table A10. Average OCP's Effect on Intergenerational Persistence, for 1980–1996 cohorts

Province	Average fine (Yearly income)	Total effect on income persistence	Magnitude on income persistence	Total effect on educational persistence	Magnitude on educational persistence	Total effect on social class persistence	Magnitude on social class persistence
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Beijing	1.945	0.185	36.0%	0.301	62.4%	0.159	31.7%
Tianjin	1.094	0.104	20.3%	0.169	35.1%	0.090	17.8%
Hebei	1.802	0.171	33.4%	0.279	57.8%	0.148	29.4%
Shanxi	1.281	0.122	23.7%	0.199	41.1%	0.105	20.9%
Inner Mongolia	1.366	0.130	25.3%	0.212	43.8%	0.112	22.3%
Liaoning	2.385	0.227	44.2%	0.370	76.5%	0.196	38.9%
Jilin	0.756	0.072	14.0%	0.117	24.2%	0.062	12.3%
Heilongjiang	1.207	0.115	22.4%	0.187	38.7%	0.099	19.7%
Shanghai	1.169	0.111	21.6%	0.181	37.5%	0.096	19.1%
Jiangsu	1.820	0.173	33.7%	0.282	58.4%	0.149	29.7%
Zhejiang	1.519	0.144	28.1%	0.236	48.8%	0.125	24.8%
Anhui	0.630	0.060	11.7%	0.098	20.2%	0.052	10.3%
Fujian	1.627	0.155	30.1%	0.252	52.2%	0.133	26.5%
Jiangxi	1.488	0.141	27.6%	0.231	47.7%	0.122	24.3%
Shandong	1.170	0.111	21.7%	0.181	37.6%	0.096	19.1%
Henan	1.421	0.135	26.3%	0.220	45.6%	0.117	23.2%
Hubei	1.695	0.161	31.4%	0.263	54.4%	0.139	27.6%
Hunan	1.204	0.114	22.3%	0.187	38.6%	0.099	19.6%
Guangdong	2.031	0.193	37.6%	0.315	65.2%	0.167	33.1%
Guangxi	2.148	0.204	39.8%	0.333	68.9%	0.176	35.0%
Hainan	1.632	0.155	30.2%	0.253	52.4%	0.134	26.6%
Chongqing	1.502	0.143	27.8%	0.233	48.2%	0.123	24.5%
Sichuan	1.244	0.118	23.0%	0.193	39.9%	0.102	20.3%
Guizhou	1.723	0.164	31.9%	0.267	55.3%	0.141	28.1%
Yunnan	1.652	0.157	30.6%	0.256	53.0%	0.135	26.9%
Tibet	0.670	0.064	12.4%	0.104	21.5%	0.055	10.9%
Shaanxi	1.138	0.108	21.1%	0.176	36.5%	0.093	18.5%
Gansu	1.503	0.143	27.8%	0.233	48.2%	0.123	24.5%
Qinghai	1.106	0.105	20.5%	0.171	35.5%	0.091	18.0%
Ningxia	2.384	0.226	44.1%	0.370	76.5%	0.195	38.9%
Xinjiang	2.104	0.200	39.0%	0.326	67.5%	0.172	34.3%
National Average	1.519	0.144	28.1%	0.235	48.7%	0.125	24.8%

Note: The average fine is weighted by the population of 1980-1996 cohorts. The nationwide fine is weighted by the population of each province. The population data is collected from China Statistical Yearbook, 2000. The total effects of OCP on income, education, and social class persistence are calculated from the corresponding average fine multiplied by the baseline estimation of the elasticity of 1-year-income equivalent fine for OCP (3-year before and after OCP implementation; Row 1, Table 1). The magnitude is the percentage of decreased persistence compared to the nationwide estimation of average persistence before 1979 (Column 1, Table A3).

Table A11. Results of Triple-Difference Tests to Check the Effect of the "Later, Longer, and Fewer" Campaign (LLF) on Children's Rank prior to the OCP

	Children's Income rank (1)	Children's Education rank (2)	Children's Social class rank (3)
LLF X Fine X Parent(s)' Income Rank	-0.105		
	(0.219)		
LLF X Fine X Parent(s)' Educational Rank		-0.712	
		(0.633)	
LLF X Fine X Parent(s)' Social class Rank			0.158
			(0.251)
Controls:			
LLF X Fine, Fine	X	X	X
Gender, Single parent, Marital status	X	X	X
Cohort, Year, Data FE	X	X	X
Observations	45,201	390,093	46,249
R-Squared	0.154	0.290	0.150

Note: Each cell in this table reports the coefficient from a DDD regression of OCP intensity (violation punishment fine rate), LLF, interacting with parent income rank (column 1), educational rank (column 2), and social class rank (column 3), respectively. We use the establishment years of the Family Planning Leading Group as the proxy for the variable of LLF (Chen and Fang, 2021). The dependent variable is the rank of children's income, education, and social class in their cohort. All regressions are controlled for survey year Fixed Effects (FE), Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. We focus on 1971–1975 cohorts. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A12. Effects from Permutation Placebo Test vs. Baseline Effects

	(1976–1978) versus (1980–1982)	(1974–1978) versus (1980–1984)	(1969–1978) versus (1980–1989)
	(1)	(2)	(3)
Income X Fine, Baseline	-0.095***	-0.095***	-0.103***
	(0.013)	(0.011)	(0.009)
Income X Fine, 95th percentile placebo	0.006	0.002	0.008
	(0.005)	(0.001)	(0.007)
Income X Fine, 5th percentile placebo	-0.007	-0.001	-0.008
	(0.006)	(0.001)	(0.006)
Education X Fine, Baseline	-0.162**	-0.164***	-0.167***
	(0.074)	(0.056)	(0.038)
Education X Fine, 95th percentile placebo	0.005	0.001	0.005
	(0.004)	(0.001)	(0.005)
Education X Fine, 5th percentile placebo	-0.006	-0.001	-0.007
	(0.005)	(0.001)	(0.005)
Social class X Fine, Baseline	-0.081***	-0.089***	-0.110***
	(0.013)	(0.011)	(0.009)
Social class X Fine, 95th percentile placebo	0.005	0.001	0.004
	(0.003)	(0.001)	(0.003)
Social class X Fine, 5th percentile placebo	-0.005	-0.001	-0.005
	(0.003)	(0.001)	(0.003)

Note: This table reports the 95th percentile and 5th percentile permutation coefficients permutation test to randomize the treatment (the fine rate variable) in the sample and randomly shuffle the fine variable to run the baseline regressions with 1000 draws. The baseline effect also being presented here (same as Table 1). The setting of permutation regression is the same baseline regression. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE and cohort FE. Robust standard errors are clustered by province in parentheses. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A13. Permutation Heterogeneity Treatment Test, the OCP Effect on Relative Persistence

	(1976–1978) versus (1980–1982)	(1974–1978) versus (1980–1984)	(1969–1978) versus (1980–1989)
	(1)	(2)	(3)
Income X Fine, Baseline	-0.095***	-0.095***	-0.103***
	(0.013)	(0.011)	(0.009)
Random Group X Income X Fine, 95th percentile placebo	0.002	0.001	0.003
	(0.005)	(0.001)	(0.005)
Random Group X Income X Fine, 5th percentile placebo	-0.003	-0.001	-0.003
	(0.006)	(0.001)	(0.005)
Education X Fine, Baseline	-0.162**	-0.164***	-0.167***
	(0.074)	(0.056)	(0.038)
Random Group X Education X Fine, 95th percentile placebo	0.002	0.001	0.004
	(0.003)	(0.001)	(0.004)
Random Group X Education X Fine, 5th percentile placebo	-0.003	-0.001	-0.005
	(0.005)	(0.001)	(0.006)
Social class X Fine, Baseline	-0.081***	-0.089***	-0.110***
	(0.013)	(0.011)	(0.009)
Random Group X Social class X Fine, 95th percentile placebo	0.002	0.002	0.002
	(0.003)	(0.001)	(0.002)
Random Group X Social class X Fine, 5th percentile placebo	-0.001	-0.001	-0.003
	(0.003)	(0.001)	(0.002)

Note: This table reports the 95th percentile and 5th percentile permutation coefficients permutation test to randomize the post sample into two groups and test the difference, regressions with 100 draws. The test is following Callaway, Goodman-Bacon, and Sant'Anna (2021). Thus, we test our samples for treatment effect heterogeneity". Each cell in this table reports the coefficient from a generalized DID regression of OCP intensity (violation punishment fine rate) interact with random groups on income or education or social class. The dependent variable is the rank of children's income or education or social class in their cohort. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE and cohort FE Robust standard errors are clustered by province in parentheses. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A14. Spillover of the One-child Policy Effect on Relative Persistence: Dropping child-parent pairs in which mothers would have been able to have another child if the One-Child Policy had not been implemented

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine: (1976–1978) v. (1980–1982)	-0.097***	-0.168**	-0.082***
	(0.013)	(0.072)	(0.014)
Observations	70,781	333,537	71,993
R-squared	0.141	0.284	0.176
Parent's Rank X Fine: (1974–1978) v. (1980–1984)	-0.095***	-0.173***	-0.089***
	(0.010)	(0.055)	(0.010)
Observations	110,153	527,863	112,207
R-squared	0.147	0.269	0.176
Parent's Rank X Fine: (1969–1978) v. (1980–1989)	-0.118***	-0.186***	-0.120***
	(0.010)	(0.042)	(0.011)
Observations	197,673	964,261	201,660
R-squared	0.168	0.303	0.180
Parent's Rank X Fine: (1949–1978) v. (1980–1996)	-0.159***	-0.173***	-0.170***
	(0.011)	(0.027)	(0.012)
Observations	293,356	1,436,492	300,464
R-squared	0.268	0.329	0.224
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class within their cohort, and the key independent variable is the OCP fine rate at the province-year level interacting with the rank of their parents' corresponding rank within the parents' cohort. We exclude child-parent pairs in which mothers would have been able to have another child if the One-Child Policy had not been implemented. These mothers were younger than the 90th percentile for gestational age. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A15. Alternative Interpretations: Reinstitution of The College Entrance Examination and University Enrollment Expansions

	(1976–1978) v. (1980–1982) (1)	(1974–1978) v. (1980–1984) (2)	(1969–1978) v. (1980–1989) (3)	(1949–1978) v. (1980–1996) (4)
Panel A: Income persistence				
Fine X Income	-0.109***	-0.090***	-0.074***	-0.039***
	(0.013)	(0.012)	(0.009)	(0.006)
Reinstitution of The College Entrance Examination and University Enrollment Expansions X Income	0.156*	-0.000	-0.171***	-0.450***
	(0.087)	(0.062)	(0.038)	(0.021)
Observations	72,365	113,677	213,940	348,207
Panel B: Educational persistence				
Fine X Education	-0.183***	-0.161***	-0.085**	-0.150***
	(0.046)	(0.043)	(0.042)	(0.029)
Reinstitution of The College Entrance Examination and University Enrollment Expansions X Education	0.313	0.044	-0.426**	0.068
	(0.376)	(0.245)	(0.206)	(0.077)
Observations	345,462	558,289	1,102,717	2,031,504
Panel C: Social class persistence				
Fine X Social class	-0.081***	-0.072***	-0.072***	-0.044***
	(0.014)	(0.013)	(0.011)	(0.007)
Reinstitution of The College Entrance Examination and University Enrollment Expansions X Social class	-0.006	-0.107*	-0.207***	-0.469***
	(0.087)	(0.062)	(0.040)	(0.023)
Observations	73,609	115,812	218,277	358,158
Controls:				
Gender, Single parent, Marital status	X	X	X	X
Province FE	X	X	X	X
Cohort FE	X	X	X	X
Year FE	X	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) interacted with the intensity of a contemporary historical event: reinstitution of the college entrance examination and university enrollment expansions on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE and cohort FE. Robust standard errors are clustered by province in parentheses. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A16. The OCP Effect on Relative Persistence, with the father representing parents

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine:	-0.082***	-0.128*	-0.063***
(1976–1978) v. (1980–1982)	(0.012)	(0.070)	(0.014)
Observations	68,108	345,453	69,265
Parent's Rank X Fine:	-0.075***	-0.133**	-0.063***
(1974–1978) v. (1980–1984)	(0.009)	(0.052)	(0.009)
Observations	107,340	558,277	109,301
Parent's Rank X Fine:	-0.104***	-0.162***	-0.094***
(1969–1978) v. (1980–1989)	(0.009)	(0.035)	(0.009)
Observations	194,522	1,041,875	198,626
Parent's Rank X Fine:	-0.143***	-0.161***	-0.139***
(1949–1978) v. (1980–1996)	(0.009)	(0.021)	(0.009)
Observations	303,270	1,755,655	312,963
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We use the fathers' rank as the proxy for parents' rank that is employed in the baseline, as fathers are the main breadwinners in many traditional families. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A17. The OCP Effect on Relative Persistence, 3-year average fine

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine:	-0.065***	-0.051	-0.066***
(1976–1978) v. (1980–1982)	(0.012)	(0.074)	(0.011)
Observations	60,792	206,536	61,713
Parent's Rank X Fine:	-0.079***	-0.056	-0.087***
(1974–1978) v. (1980–1984)	(0.010)	(0.058)	(0.009)
Observations	99,829	430,966	101,583
Parent's Rank X Fine:	-0.085***	-0.064	-0.103***
(1969–1978) v. (1980–1989)	(0.009)	(0.041)	(0.011)
Observations	189,356	969,650	193,256
Parent's Rank X Fine:	-0.124***	-0.082***	-0.143***
(1949–1978) v. (1980–1996)	(0.011)	(0.022)	(0.012)
Observations	326,275	1,969,451	335,769
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We take the average fine rates of three years before the individual's birth as OCP fine rates as a robustness check because birth decision may take years. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A18. The OCP Effect on Relative Persistence, dropping gender-biased datasets

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine:	-0.090***	-0.147***	-0.078***
(1976–1978) v. (1980–1982)	(0.015)	(0.011)	(0.015)
Observations	63,452	94,179	64,687
Parent's Rank X Fine:	-0.090***	-0.138***	-0.087***
(1974–1978) v. (1980–1984)	(0.011)	(0.014)	(0.012)
Observations	97,916	150,415	100,012
Parent's Rank X Fine:	-0.109***	-0.131***	-0.111***
(1969–1978) v. (1980–1989)	(0.011)	(0.015)	(0.012)
Observations	181,286	300,109	185,562
Parent's Rank X Fine:	-0.136***	-0.131***	-0.148***
(1949–1978) v. (1980–1996)	(0.010)	(0.013)	(0.011)
Observations	297,689	596,556	307,568
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE and cohort FE. Robust standard errors are clustered by province in parentheses. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A19. Excluding Census, CGSS, CHIP and UHS to Examine the Issue of Co-residency

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine:	-0.045***	-0.147***	-0.050***
(1976–1978) v. (1980–1982)	(0.011)	(0.015)	(0.012)
Observations	34,760	61,776	35,997
Parent's Rank X Fine:	-0.070***	-0.134***	-0.087***
(1974–1978) v. (1980–1984)	(0.012)	(0.015)	(0.012)
Observations	57,159	102,829	59,257
Parent's Rank X Fine:	-0.115***	-0.110***	-0.134***
(1969–1978) v. (1980–1989)	(0.010)	(0.013)	(0.011)
Observations	122,323	225,880	126,613
Parent's Rank X Fine:	-0.157***	-0.108***	-0.185***
(1949–1978) v. (1980–1996)	(0.010)	(0.012)	(0.012)
Observations	212,336	471,172	222,240
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We exclude data from Census, CGSS, CHIP and UHS because the remaining data sources include survey questions about every family member, whether they were co-residents or not, to address the concern of co-residency. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE and cohort FE. Robust standard errors are clustered by province in parentheses. The 1979 cohort has been excluded. ***, **, and * indicate statistical significance at the level 1%, 5%, and 10%, respectively.

Table A20. One-child Policy Effect on Relative Persistence: with Parents who Experienced the Great Famine v. with Parents who did not Experienced the Great Famine

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Panel A: with Parents who did not experience the Great Famine			
Parent's Rank X Fine:	-0.100***	-0.164***	-0.093***
(1976–1978) v. (1980–1982)	(0.018)	(0.057)	(0.020)
Observations	12,045	24,373	12,178
Parent's Rank X Fine:	-0.185***	-0.196***	-0.175***
(1974–1978) v. (1980–1984)	(0.018)	(0.041)	(0.018)
Observations	23,110	38,773	23,394
Parent's Rank X Fine:	-0.293***	-0.173***	-0.266***
(1969–1978) v. (1980–1989)	(0.021)	(0.026)	(0.019)
Observations	64,942	101,223	66,252
Parent's Rank X Fine:	-0.257***	-0.122***	-0.242***
(1949–1978) v. (1980–1996)	(0.019)	(0.014)	(0.018)
Observations	101,144	152,363	106,037
Panel B: with parents who experienced the Great Famine			
Parent's Rank X Fine:	-0.092***	-0.162**	-0.050
(1976–1978) v. (1980–1982)	(0.014)	(0.075)	(0.032)
Observations	60,319	321,088	9,264
Parent's Rank X Fine:	-0.070***	-0.167***	-0.051
(1974–1978) v. (1980–1984)	(0.010)	(0.056)	(0.033)
Observations	90,565	519,515	14,424
Parent's Rank X Fine:	-0.070***	-0.202***	-0.073**
(1969–1978) v. (1980–1989)	(0.009)	(0.040)	(0.032)
Observations	148,998	1,001,493	27,341
Parent's Rank X Fine:	-0.132***	-0.198***	-0.112***
(1949–1978) v. (1980–1996)	(0.011)	(0.028)	(0.027)
Observations	247,061	1,879,138	45,316
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We divide the sample according to whether parents experience the Great Famine and compare the OCP effects on intergenerational persistence for different subsamples. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A21. One-child Policy Effect on Relative Persistence: Non-Migrant v. Migrant Children

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Panel A: Non-Migrant Child subsample			
Parent's Rank X Fine:	-0.096***	-0.188***	-0.082***
(1976–1978) v. (1980–1982)	(0.016)	(0.017)	(0.018)
Observations	65,442	216,618	65,609
Parent's Rank X Fine:	-0.097***	-0.152***	-0.089***
(1974–1978) v. (1980–1984)	(0.013)	(0.016)	(0.012)
Observations	102,414	369,095	102,704
Parent's Rank X Fine:	-0.114***	-0.115***	-0.112***
(1969–1978) v. (1980–1989)	(0.011)	(0.017)	(0.011)
Observations	190,502	741,893	190,936
Parent's Rank X Fine:	-0.142***	-0.099***	-0.150***
(1949–1978) v. (1980–1996)	(0.011)	(0.012)	(0.012)
Observations	312,342	1,378,837	312,842
Panel B: Migrant Child subsample			
Parent's Rank X Fine:	-0.084***	-0.205*	-0.050
(1976–1978) v. (1980–1982)	(0.022)	(0.104)	(0.032)
Observations	6,923	128,844	9,264
Parent's Rank X Fine:	-0.053**	-0.211**	-0.051
(1974–1978) v. (1980–1984)	(0.026)	(0.097)	(0.033)
Observations	11,263	189,194	14,424
Parent's Rank X Fine:	-0.074***	-0.287***	-0.073**
(1969–1978) v. (1980–1989)	(0.023)	(0.070)	(0.032)
Observations	23,437	360,824	27,341
Parent's Rank X Fine:	-0.103***	-0.284***	-0.112***
(1949–1978) v. (1980–1996)	(0.023)	(0.049)	(0.027)
Observations	35,865	652,667	45,316
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We divide the sample into one subsample of children who inter-provincially migrated from the birth province to the residential province (Panel A) and another subsample of children who did not migrate inter-provincially (Panel B) in order to compare the OCP effects on intergenerational persistence for different subsamples. All regressions are controlled for survey year Fixed Effects

(FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A22. One-child Policy Effect on Relative Persistence, weighted by cohort size

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Parent's Rank X Fine: (1976–1978) v. (1980–1982)	-0.099***	-0.148**	-0.092***
	(0.014)	(0.072)	(0.016)
Observations	72,365	345,462	73,609
R-squared	0.157	0.260	0.186
Parent's Rank X Fine: (1974–1978) v. (1980–1984)	-0.098***	-0.161**	-0.095***
	(0.011)	(0.061)	(0.011)
Observations	113,677	558,289	115,812
R-squared	0.162	0.252	0.186
Parent's Rank X Fine: (1969–1978) v. (1980–1989)	-0.112***	-0.187***	-0.114***
	(0.010)	(0.045)	(0.008)
Observations	213,940	1,102,717	218,277
R-squared	0.183	0.292	0.188
Parent's Rank X Fine: (1949–1978) v. (1980–1996)	-0.141***	-0.188***	-0.148***
	(0.010)	(0.031)	(0.010)
Observations	348,207	2,031,504	358,158
R-squared	0.244	0.282	0.209
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class within their cohort, and the key independent variable is the OCP fine rate at the province-year level interacting with the rank of their parents' corresponding rank within the parents' cohort. All regressions are weighted by cohort size and are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A23. One-child Policy Effect on Relative Persistence, Heterogeneity: Male v. Female

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Panel A: Female child subsample			
Parent's Rank X Fine:	-0.099***	-0.159***	-0.072***
(1976–1978) v. (1980–1982)	(0.019)	(0.047)	(0.019)
Observations	31,826	102,347	32,526
Parent's Rank X Fine:	-0.091***	-0.151***	-0.071***
(1974–1978) v. (1980–1984)	(0.013)	(0.039)	(0.012)
Observations	49,877	161,556	51,073
Parent's Rank X Fine:	-0.104***	-0.136***	-0.094***
(1969–1978) v. (1980–1989)	(0.009)	(0.032)	(0.009)
Observations	92,430	294,797	94,938
Parent's Rank X Fine:	-0.115***	-0.125***	-0.111***
(1949–1978) v. (1980–1996)	(0.010)	(0.019)	(0.011)
Observations	151,445	533,007	157,024
Panel B: Male child subsample			
Parent's Rank X Fine:	-0.093***	-0.183*	-0.088***
(1976–1978) v. (1980–1982)	(0.012)	(0.090)	(0.014)
Observations	40,539	243,115	41,083
Parent's Rank X Fine:	-0.093***	-0.185**	-0.098***
(1974–1978) v. (1980–1984)	(0.011)	(0.070)	(0.012)
Observations	63,800	396,733	64,739
Parent's Rank X Fine:	-0.108***	-0.202***	-0.122***
(1969–1978) v. (1980–1989)	(0.011)	(0.044)	(0.011)
Observations	121,510	807,920	123,339
Parent's Rank X Fine:	-0.146***	-0.162***	-0.166***
(1949–1978) v. (1980–1996)	(0.011)	(0.025)	(0.012)
Observations	196,762	1,498,497	201,134
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We divide the sample into female child (Panel A) and male child (Panel B) subsamples to compare the OCP effects on intergenerational persistence for different subsamples. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A24. One-child Policy Effect on Relative Persistence, Heterogeneity: Rural v. Urban

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Panel A: Rural subsample			
Parent's Rank X Fine:	-0.109***	-0.111***	-0.097***
(1976–1978) v. (1980–1982)	(0.013)	(0.035)	(0.016)
Observations	57,969	206,215	58,743
Parent's Rank X Fine:	-0.106***	-0.123***	-0.096***
(1974–1978) v. (1980–1984)	(0.010)	(0.031)	(0.009)
Observations	89,224	347,299	90,530
Parent's Rank X Fine:	-0.112***	-0.144***	-0.112***
(1969–1978) v. (1980–1989)	(0.012)	(0.029)	(0.011)
Observations	159,167	695,927	161,933
Parent's Rank X Fine:	-0.123***	-0.135***	-0.134***
(1949–1978) v. (1980–1996)	(0.012)	(0.021)	(0.014)
Observations	245,808	1,304,308	252,479
Panel B: Urban subsample			
Parent's Rank X Fine:	-0.040**	-0.216***	-0.036**
(1976–1978) v. (1980–1982)	(0.018)	(0.075)	(0.017)
Observations	13,825	138,695	14,293
Parent's Rank X Fine:	-0.038**	-0.200***	-0.055***
(1974–1978) v. (1980–1984)	(0.015)	(0.058)	(0.018)
Observations	22,386	208,953	23,213
Parent's Rank X Fine:	-0.070***	-0.199***	-0.088***
(1969–1978) v. (1980–1989)	(0.010)	(0.042)	(0.012)
Observations	45,203	397,250	46,770
Parent's Rank X Fine:	-0.138***	-0.152***	-0.154***
(1949–1978) v. (1980–1996)	(0.012)	(0.024)	(0.015)
Observations	77,085	702,006	80,352
Controls:			
Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the rank of their parents corresponding rank in the parents' cohort interacting with the OCP fine rate at the province-year level. We divide the sample into rural (Panel A) and urban (Panel B) subsamples to compare the OCP effects on intergenerational persistence for different subsamples. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A25. One-child Policy Effect on Relative Persistence, Heterogeneity: Geographical Regions

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Panel A: (1976–1978) v. (1980–1982)			
Eastern Region	-0.109***	-0.160*	-0.105***
	(0.021)	(0.085)	(0.022)
Observations	32,469	137,313	32,562
Central Region	-0.075***	-0.152*	-0.050***
	(0.018)	(0.078)	(0.013)
Observations	22,644	111,449	22,692
Western Region	-0.095***	-0.123**	-0.069***
	(0.023)	(0.060)	(0.021)
Observations	17,247	96,698	18,351
Panel B: (1974–1978) v. (1980–1984)			
Eastern Region	-0.102***	-0.165**	-0.106***
	(0.014)	(0.063)	(0.013)
Observations	49,903	216,758	50,064
Central Region	-0.073***	-0.151**	-0.053***
	(0.022)	(0.061)	(0.016)
Observations	35,274	180,478	35,362
Western Region	-0.097***	-0.124***	-0.084***
	(0.018)	(0.044)	(0.019)
Observations	28,499	161,052	30,385
Panel C: (1969–1978) v. (1980–1989)			
Eastern Region	-0.113***	-0.184***	-0.118***
	(0.014)	(0.046)	(0.014)
Observations	89,356	421,986	89,572
Central Region	-0.111***	-0.151**	-0.088***
	(0.019)	(0.061)	(0.013)
Observations	66,710	180,478	66,856
Western Region	-0.097***	-0.132***	-0.109***
	(0.015)	(0.033)	(0.018)
Observations	57,874	326,440	61,849
Panel D: (1949–1978) v. (1980–1996)			
Eastern Region	-0.136***	-0.154***	-0.143***
	(0.013)	(0.028)	(0.016)
Observations	148,031	792,980	148,268
Central Region	-0.163***	-0.165***	-0.159***
	(0.018)	(0.032)	(0.015)
Observations	107,944	645,954	108,117
Western Region	-0.118***	-0.106***	-0.141***
	(0.012)	(0.025)	(0.014)
Observations	92,232	592,569	101,773
Controls:			

Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the OCP fine rate at the province-year level interacting with the rank of their parents' corresponding rank in the parents' cohort across three regions in China (per OCP strictness, with eastern region the highest and western region the lowest; Gu et al., 2007), respectively. We divide the sample into four panels according to the different comparing time windows. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table A26. One-child Policy Effect on Relative Persistence, Heterogeneity: Parents' Rankings

	Child's Income rank (1)	Child's Education rank (2)	Child's Social class rank (3)
Panel A: (1976–1978) v. (1980–1982)			
(Parents Ranked top 25%) X Fine	-1.489	4.643	-1.244
	(2.189)	(3.550)	(1.270)
(Parents Ranked 26%–50%) X Fine	-4.759**	-1.573	-4.160***
	(2.117)	(2.166)	(1.188)
(Parents Ranked 51%–75%) X Fine	-7.313***	-7.617**	-4.580**
	(2.141)	(2.787)	(1.962)
(Parents Ranked lower 25%) X Fine	-8.685***	-8.502**	-5.817***
	(2.620)	(3.348)	(1.766)
Observations	72,365	345,462	73,609
Panel B: (1974–1978) v. (1980–1984)			
(Parents Ranked top 25%) X Fine	0.622	4.479	-0.939
	(1.310)	(2.965)	(1.153)
(Parents Ranked 26%–50%) X Fine	-3.097**	-1.984	-4.178***
	(1.246)	(1.689)	(1.007)
(Parents Ranked 51%–75%) X Fine	-5.275***	-6.637***	-4.142***
	(1.236)	(2.110)	(0.885)
(Parents Ranked lower 25%) X Fine	-6.784***	-9.223***	-6.397***
	(1.004)	(2.653)	(1.158)
Observations	113,677	558,289	115,812
Panel C: (1969–1978) v. (1980–1989)			
(Parents Ranked top 25%) X Fine	1.613*	3.615*	0.276
	(0.932)	(1.886)	(1.119)
(Parents Ranked 26%–50%) X Fine	-3.236***	-3.202**	-4.274***
	(0.907)	(1.344)	(1.114)
(Parents Ranked 51%–75%) X Fine	-5.464***	-6.765***	-4.977***
	(1.018)	(1.699)	(1.125)
(Parents Ranked lower 25%) X Fine	-7.383***	-11.111***	-7.526***
	(1.078)	(2.606)	(1.488)
Observations	213,940	1,102,717	218,277
Panel D: (1949–1978) v. (1980–1996)			
(Parents Ranked top 25%) X Fine	3.609***	3.821**	1.995***
	(0.593)	(1.416)	(0.503)
(Parents Ranked 26%–50%) X Fine	-2.821***	-2.409***	-4.203***
	(0.601)	(0.748)	(0.598)
(Parents Ranked 51%–75%) X Fine	-5.590***	-5.169***	-6.099***
	(0.825)	(0.866)	(0.668)
(Parents Ranked lower 25%) X Fine	-8.732***	-8.521***	-10.297***
	(0.821)	(1.620)	(1.070)
Observations	348,207	2,031,504	358,158
Controls:			

Gender, Single parent, Marital status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income, education, and social class. The dependent variable is the rank of children's income, education, and social class in their cohort, and the key independent variable is the OCP fine rate at the province-year level interacting with the rank of their parents corresponding rank in the parents' cohort at the top 25%, 26%–50%, 51%–75%, and lower 25% level, respectively. We divide the sample into four panels according to the different comparing time windows. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Appendix B. Theory Appendix

B1. Proofs of propositions

B1.1. Proof to Proposition 1.

Proof From Equation (15), by taking partial derivative of P with respect to b_e, we have

$$\frac{\partial P}{\partial b_e} = -\frac{AR\alpha I(e)}{2\theta w(1-\alpha)} exp\big((AR-1)log2\big)\, exp\left(\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right)\right)\right.$$

$$\left. - exp\left(-\frac{AR\alpha}{\theta(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right)\right)\right) = I(e)\frac{\partial P}{\partial b_o}$$

From Proposition 1, we know

$$\frac{\partial P}{\partial b_o} < 0$$

and

$$I(e) = 0 \; or \; 1$$

Therefore, we have

$$\frac{\partial P}{\partial b_e} \leq 0$$

B1.2. Proof to Proposition 2.

Proof From Equation (15), by taking partial derivative of P with respect to A, we have

$$\frac{\partial P}{\partial A} = \frac{R}{2}\left(-\frac{\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) + log2\right) exp\,((AR-1)log2))(\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\,(\frac{b_o + I(e)b_e}{w}\right)$$

$$- exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right)\right))$$

$$= R\left(-\frac{\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) + log2\right) exp\,(AR-1)(\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\,(\frac{b_o + I(e)b_e}{w}\right)$$

$$- exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right)\right))$$

Same as in the proof to Proposition 1, denoting $x \equiv \frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right)$, we have

$$\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\,(\frac{b_o + I(e)b_e}{w})\right) - exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right)\right) = \theta\, exp(-x) - \exp\left(-\frac{x}{\theta}\right)$$

As in the previous section, we have

$$\theta\, exp(-x) - \exp\left(-\frac{x}{\theta}\right) > 0$$

Since $\frac{b_o+I(e)b_e}{\theta w} \ll 1$, and we don't consider $\alpha \gg 1-\alpha$, we have

$$-\frac{\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) + log2 > 0$$

In addition, $Rexp\ (AR-1)$>0.

Therefore, we have

$$\frac{\partial P}{\partial A} > 0$$

B1.3. Proof to Proposition 3.

Proof From Equation (15), by taking partial derivative of P with respect to b_o, we have

$$\begin{aligned}\frac{\partial P}{\partial b_o} = &-\frac{AR\alpha}{2w(1-\alpha)} exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right) + (AR-1)log2\right)\\ &+\frac{AR\alpha}{2\theta w(1-\alpha)} exp\left(-\frac{AR\alpha}{(1-\alpha)}(\frac{b_o + I(e)b_e}{\theta w}) + (AR-1)log2\right)\\ = &-\frac{AR\alpha}{2\theta w(1-\alpha)}(\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right) + (AR-1)log2\right)\\ &+ exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{\theta w}\right) + (AR-1)log2\right))\\ = &-\frac{AR\alpha}{2\theta w(1-\alpha)} exp\big((AR-1)log2\big)\,(\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right)\right)\\ &- exp\left(-\frac{AR\alpha}{\theta(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right)\right))\end{aligned}$$

Because $-\frac{AR\alpha}{2\theta w(1-\alpha)} exp\big((AR-1)log2\big) < 0$, the sign of $\frac{\partial P}{\partial b_o}$ lies in the sign of $\theta exp\left(-\frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o+I(e)b_e}{w}\right)\right) - exp\left(-\frac{AR\alpha}{\theta(1-\alpha)}\left(\frac{b_o+I(e)b_e}{w}\right)\right)$.

We denote

$$x \equiv \frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o + I(e)b_e}{w}\right)$$

so that (14) can be written as

$$\theta exp(-x) - exp\left(-\frac{x}{\theta}\right)$$

Recall the conditions of $\frac{b_o+I(e)b_e}{w} \ll 1$, close values of α and $1-\alpha$, and A, R uniformed between 0 and 1, we have

$$0 < \frac{AR\alpha}{(1-\alpha)}\left(\frac{b_o+I(e)b_e}{w}\right) < 1, \text{ or } 0 < x < 1.$$

Because $0 < x < 1 < \frac{\theta \log(\theta)}{\theta-1}$, $\theta\, exp(-x) - ex\, p\left(-\frac{x}{\theta}\right) > 0$. Therefore, we have

$$\frac{\partial P}{\partial b_o} < 0.$$

B1.4. Proof to Proposition 4.

Proof This is a dual problem of Proposition 2. From Equation (15), by taking partial derivative of P with respect to R and similar procedures, we have

$$\frac{\partial P}{\partial R} > 0$$

B2. Another explanation for Proposition 3

We solve Equations (8) and (9) to find the optimal fertility, n^*, and education level, e^*. Elite officials in government institutions, despite having tenure and power within government structures, receive relatively low pay and are subject to demotion or even dismissal if they violate the OCP. As a result, in addition to being classified into low type, they are presumed to be charged with an additional fine, b_e. For an elite official in a government institution, his or her optimal fertility and education levels are

$$n_o{}^* = \frac{(1-\alpha)}{\beta}\frac{w}{(b_o+b_e)}$$

$$e_o{}^* = \frac{\alpha}{(1-\alpha)}\ \frac{(b_o+b_e)}{w}$$

where $b_e > 0$. The higher the position of the individual, the greater b_e imposed on him or her.

For other individuals, their optimal fertility and education levels are

$$n^* = \frac{(1-\alpha)}{\beta}\frac{w}{b_o}$$

$$e^* = \frac{\alpha}{(1-\alpha)}\frac{b_o}{w}$$

Suppose λ ($0 < \lambda < 1$) is the proportion of elites in the population, and children of elites remains elites and outperform any child with non-elite parents due to their parents' political resources. Non-elite parents have identical children of the same rank. As a result, the rank of a non-elite parent is

$$R_{p,l} = \lambda$$

The rank of his or her child is

$$R_{c,l} = \frac{\lambda {n_o}^*}{\lambda {n_o}^* + (1-\lambda)n^*}$$

The intergenerational ranking persistence is

$$P_R = \frac{R_{c,l}}{R_{p,l}} = \frac{{n_o}^*}{\lambda {n_o}^* + (1-\lambda)n^*}$$

Since ${n_o}^* < n^*$, $P_R < 1$.

Taking derivative of P_R with respect to ${n_o}^*$, we have

$$\frac{\partial P_R}{\partial {n_o}^*} = \frac{(1-\lambda)n^*}{(\lambda {n_o}^* + (1-\lambda)n^*)^2} > 0$$

For the inequation, we know that the higher the fertility of elites, the greater the intergenerational ranking persistence (the lower the intergenerational mobility).

Appendix C1. The Potential Decline in Son Preference

China has a tradition of son preference, in which daughters usually receive fewer resources from parents than their brothers (Chen et al., 2015). However, the OCP could potentially decrease such a preference; OCP-compliant families that have a daughter have no option but to pass all resources to their daughter, regardless of any gender preference. We therefore use a DDD regression to examine whether the OCP increases marriage gifts from parents and education investment in female children compared to male children. The specification is as follows:

$$Outcomes_{iht} = \beta_g Gender_{ift} + \beta_f Fine_{hp} \times I(h > 1979) + \beta \times Fine_{hp} \times Gender_{ift} \times I(h > 1979) + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad \text{(c1)}$$

where the setting is similar to the second channel, Equation (19), except that the interaction term is gender instead of poor. We also run regressions of Equation (19) with outcome as marriage gifts and education investment from parents compared among rich and poor in different gender sub-samples.

The empirical results do not strongly support such a hypothesis. The DDD regression showed that gender differences in OCP effects were not significant (Columns 1–2, Table C1), suggesting that the OCP did not have an obvious heterogeneity effect for different genders on marriage gift and education investment from parents. When comparing children of the same gender across parental incomes, the OCP do result in significantly greater amounts of marriage gifts for poorer female children only (56.8% for one year's income that a parent is fined for OCP violation, Column 4, Table C1); however, this evidence alone is not sufficient to confirm the hypothesis that son preference declines.

[Table C1 approximately here]

Appendix C2. The Potential Choice of Migration

Individuals typical choose to migrant from their birth province to reside in another province for better opportunities in work, education, etc., possibly leading to a decrease in the intergenerational persistence for those who migrated. Being an only child might have affected willingness to migrate—either negatively, as parents would be left without support at home or as easier cohabitation with parents reduces costs, or positively, as migration requires resources. We thus

examine whether the OCP affects the migrating behavior of children. The specification is as follows:

$$Migrate_or_not_{iht} = \beta Fine_{hp} \times I(h > 1979) + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad \text{(c2)}$$

where the setting is similar to Equation (c1).

The empirical results show that the OCP decreases the children's migrating behavior (Table C2). Migration is not a mechanism for the OCP to affect intergenerational persistence in China's context.

[Table C2 approximately here]

Table C1. OCP Effect on Poor vs. Rich Families: Wealth Transmission for Male and Female Children

	Marriage gift from parents, both gender	Education investment from parents, both gender	Marriage gift from parents, male	Marriage gift from parents, female	Education investment from parents, male	Education investment from parents, female
	(1)	(2)	(3)	(4)	(5)	(6)
Gender	-0.155	1.044***				
	(0.362)	(0.160)				
Fine	-0.100	0.880	-1.239**	-0.897**	-0.463	0.467
	(0.207)	(0.540)	(0.583)	(0.328)	(0.696)	(0.615)
Gender X fine	0.269	-0.944***				
	(0.166)	(0.163)				
Poor			-0.354	0.030	0.562**	-0.309
			(0.300)	(0.551)	(0.209)	(0.200)
Poor X fine			0.784*	0.846*	0.505	1.012***
			(0.389)	(0.453)	(0.321)	(0.218)
Province FE	X	X	X	X	X	X
Cohort FE	X	X	X	X	X	X
Dataset FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Observations	17,716	16,223	805	560	8,543	7,169
R-squared	0.217	0.230	0.276	0.197	0.212	0.258

Note: Each column in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on parent investments between genders (column 1 and 2) and between poor and rich families but in different gender sub-samples (columns 3–6). The dependent variables are logarithms of marriage gift from parents and education investment from parents. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.

Table C2. One-child Policy Effect on the Migration Decision

Whether migrating inter-provincially away from one's birth province	(1976–1978) v. (1980–1982)	(1974–1978) v. (1980–1984)	(1969–1978) v. (1980–1989)	(1949–1978) v. (1980–1996)
Fine	-0.232***	-0.182***	-0.232***	-0.085***
	(0.049)	(0.044)	(0.049)	(0.021)
Observations	82,295	129,597	82,295	393,995
Controls:				
Gender, Single parent, Marital status	X	X	X	X
Province FE	X	X	X	X
Cohort FE	X	X	X	X
Dataset FE	X	X	X	X
Year FE	X	X	X	X
R-squared	0.287	0.289	0.287	0.353

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on the probability of inter-provincial migrating from one's birth province to another province to reside. Each column reports the results of different cohort groups. All regressions are controlled for survey year Fixed Effects (FE), province FE, Data FE, and cohort FE. Robust standard errors are clustered by province in parentheses. ***, **, and * indicate statistical significance at the level of 1%, 5%, and 10%, respectively.