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

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Dec 3, 2022

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 2000, 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%, 50.9%, and 24.5%, 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 aristocracy.

* We are grateful to Wesley Blundell, Benjamin Cowan, Giovanni Gallipoli, Jesse Rothstein, Marlon Seror, and Jonathan Yoder for their helpful comments.

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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). Due to the elite class's advocacy and practice of having more children and its demonstrative effect on social norms of familial inheritance (*chuan cheng xiang huo*), which encourage families to produce as many children as possible in order to

[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.

sustain family prosperity across generations, Chinese households were frequently exceedingly large until the late twentieth century (Ebery 1978).

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 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 avoid lifecycle bias (Chetty et al., 2014; Emran and Shilpi,

[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; Scharping, 2003; Li, Zhang, and Zhu, 2005; Ebenstein, 2010; Liu, 2014; 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.

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%, 50.9%, and 24.5%, respectively, for cohorts from 1980 to 1996. Different sub-groups, including daughters, sons, rural residents, and urban dwellers exhibit similar drops in intergenerational persistence.[9]

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; and tests for treatment heterogeneity. 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 looking at different subsamples of the data, including changing the policy interim period, gender-balanced samples, and 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. 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 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

[9] China's largest ethnic group, the *Han*, comprises around 92% of the country's population. While restricting the *Han* ethnic group to having only one child, the OCP was lenient toward minority groups, who were permitted to have more than two children (Ebenstein, 2010). Our results are robust to control for ethnicity.

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 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

[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.

[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).

(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 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)

Chinese parents have traditionally preferred big families. In addition to the traditional multi-child family structure, governments had encouraged parents to have as many children as possible after the founding of the People's Republic of China in 1949 in an effort to revive and reassemble the post-war national capacities. As a result, the 1970s saw an unprecedented population increase that was built on the previous three decades (the total fertility rate exceeded six births per mother throughout the 1960s according to the World Bank, following a devastating nationwide famine) even though grain production stagnated during the same period, remaining at the level of the 1950s.

Communist Party leaders were concerned about this population boom because it increased the possibility of a "Malthusian collapse"—unrestrained population expansion that eventually outpaces growth in the food supply and causes a catastrophic famine (Ebenstein, 2010). They considered population control as an effective measure to alleviate the severe social, economic, and environmental challenges of the time, such as the high unemployment rate and the scarcity of land resources (Greenhalgh, 1986; Greenhalgh and Bongaarts, 1992; Wang, 2012). In addition, population control was seen as an emergent and efficient method to raise GDP *per capita*, a widely used indicator of population living standards (Croll, 1984; Croll, Kane, and Davin, 1985; Bongaarts and Greenhalgh, 1985; Wang, 1996; Wang, Cai, and Gu, 2012; Zhang, 2017).

Family planning was therefore believed to be an urgently needed method for effectively reducing the rate of population expansion since it directly planned and regulated births (Wang, Cai, and Gu, 2012). The Communist Party subsequently advocated a series of voluntary fertility control

measures, culminating in the radical birth-planning policy: the One-Child Policy (OCP) (Qian, 2009).

Despite the fact that the concept of family planning was first introduced in December 1962 with the publication of Document No. [62]698 "Instructions on Seriously Advocating Family Planning (*guanyu renzhen tichang jihua shengyu de zhishi*)", family planning was not implemented in the 1960s due to the start of the Cultural Revolution. In early 1970, Premier Enlai Zhou declared that the implementation of family planning policies should begin with trials. In 1971, the State Council released Document No. [71]51, "Report on Better Implementing Family Planning Policy (*guanyu zuohao jihua shengyu gongzuo de baogao*)", which required provinces to form a family planning leading group to develop and implement family planning policies. As a result, steering committees were set up in every province and given substantial power as significant, high-level provincial institutions; in most cases, the head of a provincial family planning committee was also the general secretary of the province Communist Party Committee.

One of the leading group's main duties was to enforce the "Later, Longer, Fewer (*wan, xi, shao*)" campaign (Chen and Fang, 2021). "Later" referred to marrying at a later age—23 for women and 25 for men. "Longer" referred to a birth planning rule that required births to be spaced apart by more than three years. "Fewer" meant that a couple was restricted to having fewer than three children. This family planning campaign was technically voluntary, and policy enforcement was much more lenient than during the One-Child Policy period (Zhang, 2017).

The One-Child Policy (OCP), officially implemented in 1979, differed from its predecessors and many family planning policies in other countries by being mandatory rather than voluntary. As stated by its name, OCP prohibited a married couple from having more than one child. There were birth quota exemptions based on place of residence (urban/rural) and ethnicity (*Han*/non-*Han*). For example, the "one-and-a-half-child policy" was an exception that allowed *Han* households in rural areas to have a second child after four to six years if the first child was a female. *Han* households in urban areas were often rigidly limited to having only one child, whereas non-*Han* households were permitted to have two or three children, regardless of gender, in the majority of regions (Huang, 2017). 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.

Local governments implemented coercive measures to enforce the OCP, rewarding single-child

couples with large cash incentives and punishing over-quota births with severe fines. Policy restrictions and media propaganda heavily promoted the severe penalties connected with defying the OCP; individuals who did not watch the propaganda could observe how non-compliance with the OCP affected friends and neighbors (Li, 1995). The central government recommended that lower-level jurisdictions offer benefits such as moderate monthly financial assistance, subsidized health care and childcare, welfare allowances, paid maternity and additional leaves, and priority access to housing, employment, college admission, and education to couples who pledged to have just one child under the OCP. Single-child households in rural areas received larger allotments of "responsibility land" (land farmed both to produce the grain quota and for commercial purposes), paid lower in-kind taxes, and owed the government fewer unpaid grain quotas (McElroy and Yang, 2000).

In contrast, those who violated the OCP compliance pledge and had more children than the allotted number received unprecedented harsh penalties.[13] Penalties were enforced by local authorities, such as the heads of work units (for urban residents) or of villages (for rural residents), who were themselves subject to various economic and disciplinary pressures to make sure that the number of infants born in their jurisdiction conformed to the birth quota assigned by the upper levels of administration (Greenhalgh 1990). Such authorities regarded direct financial penalties as a potent tool for reducing childbearing. The amount of the penalties varied according to the severity of the violation. In urban regions, the financial fines for having a second child without a permit normally varied from 10% to 50% of the couple's combined yearly salary, and the fines were applied every year for a duration of 2 to 14 years; in rural areas, the most typical punishment for an above-quota birth was a sizable, one-time, lump-sum fine, which frequently represented a sizable portion of a worker’s annual income (Feng and Hao,1992; McElroy and Yang, 2000). These financial penalties often equaled a sizable fortune for an average household given the limited wealth available at that time (Li, Zhang, and Zhu, 2005). More severe punishments were meted out to individuals who had a third or higher parity child without permission. Additionally, those who had their first child outside of the quota, such as unwed mothers, risked punishment (Feng and Hao,1992).

[13] Couples who had a permit for a higher-order birth but delivered the baby too soon after the birth of their first child (typically, a minimum of four years of separation was required) could also face sanctions. A person could be penalized for getting married before the legal marriage age, having children out of wedlock, or adopting a child without permission (Li, 1995).

Along with the financial fines, there were administrative sanctions as well. Urban dwellers were subject to direct government control and at risk of more severe punishment than only financial penalties, as many of them received wages or other allocations from the state (Li, 1995). People who violated the OCP risked losing benefits that were provided and directly controlled by the government through their work unit, including housing, medical care, maternity leave, childcare, education, commuting assistance, and access to recreational facilities. They could also lose other benefits including labor insurance, food rations, and guaranteed employment (Guo and Liu, 1990; Walder, 1986). More severely, administrative disciplinary penalties could be imposed on individuals, including exclusion from opportunities for job promotion and political advancement, demotion from their current positions, salary deductions, discharge from public employment (*kaichu gongzhi*), and even renunciation of worker registration (Li, 1995; Zhang, 2017). Due to the fact that these government organizations employed the majority of urban dwellers at the time, losing party membership also precluded parents from being promoted or moving employment (Li, Zhang, and Zhu, 2005). Similarly, rural people, the majority of whom were peasants, faced the possibility of being expelled from the Party, refused access to a private land parcel or house tract, or barred from opportunities like work in township businesses (Li, 1995). Additionally, a child who exceeded the birth quota would not be qualified for government welfare, such as free public education or health insurance (Short and Zhai, 1998; Vortherms, 2019). There were no further educational possibilities for such a child due to the lack of private institutions (Li, Zhang, and Zhu, 2005). Likewise, these children were not eligible for medical treatments like vaccinations (Vortherms, 2019). The child would have to live as "*heihu*", a person without a legal resident identity, if parents did not pay the fine. A "*heihu*" lacks access to government protections like those against poverty, cost-of-living subsidies, and rural land allocation, which are frequently determined by family size; the number of "*heihu*" people as a result of OCP violations was predicted to exceed 7.8 million by the year 2016 (Vortherms, 2019). OCP punishments were at their harshest in the early 1980s and included forced abortions for unmarried women, forced IUD insertions, denial of water and electricity, unroofing of dwellings belonging to peasant families, among others (Scharping, 2003).

The OCP impacted millions of couples and lasted for 36 years, ending in December 2015. According to the World Bank, China's overall fertility rate dropped from more than 6 children per woman in the 1960s to 1.6 children per woman from 1995 to 2015. Population growth in China

was reduced, falling drastically from more than 2.5% in the late 1960s to 0.5% in 2010 mostly because of the remarkable decline in the fertility rate (Wang, Zhao, and Zhao, 2017). Thus, scholars have suggested that the OCP was a highly successful family planning policy and was largely responsible for China's documented fertility rate decline (Lavely and Freedman, 1990; Yang and Chen, 2004; Li, Zhang, and Zhu, 2005).

2.2. Fine rates of the OCP

The OCP regulatory fine rates are recognized as a reliable proxy for the strength of the policy (McElroy and Yang, 2000; Scharping, 2003; Gu et al., 2007; Ebenstein, 2010; Huang, 2017; Huang, Lei, and Sun, 2021). OCP fines stayed in effect for the duration of the policy, even though many of the other penalties were ultimately softened. They were known as "fines for violating the OCP (*chaosheng fakuan*)" from 1979 to 1995, as "unplanned fertility fee (*jihuawai shengyufei*)" from 1996 to 1999, and as "social child-raising fee (*shehui fuyangfei*)" from 2000 to 2021.[14] The central government mandated that OCP enforcement be varied through time and across provinces under the general principle of slowing down population growth, resulting in varying fine rates. An increase in fines is usually associated with other stricter birth control policies (Huang, 2017), and higher-fine regimes are associated with low fertility and smaller family size (McElroy and Yang, 2000; Ebenstein, 2010). Zhang (2017) summarizes the effect of higher fines using different estimation methods and notes that an additional penalty of roughly $177 in 1991, when the average yearly income of a full-time urban worker was $443.70, would have reduced the family size by 0.33 to 0.47.

For this study, we therefore use the average monetary penalties for an unauthorized birth in the province-year panel from 1979 to 2000, as in previous studies (Ebenstein, 2010; Wei and Zhang, 2011; Huang, Lei, and Sun, 2021), as a proxy for policy strength. The fine rates are calculated as multiples of the average annual household income of the province. Figure B.1 and Figure 1 depict the chronological and geographical change patterns of policy fines in each province from 1979 to 2000.[15]

[14] **Hu, Danping**. 2021. "Shehui fuyangfei tuichu lishi wutai [Social child-raising fee has been phased out of history]." Pengpai News Agency, July 20. https://www.thepaper.cn/newsDetail_forward_13658601.

[15] The amount of OCP fine collected was not made public until recently: the total for the year 2012 was around 20 billion RMB (US$ 3.3 billion) for the 24 provinces that reported figures. For example, Guangdong, one of China's richest provinces, received 1.5 billion RMB. In comparison, total local government expenditure on compulsory education in the same year was 10.5 billion RMB (Huang, Lei, and Sun, 2021).

The OCP's initial decade (1979–1989) saw moderate fine rates, as fertility control was not listed as one of the local governments' assessment objectives. This was despite the fact that one of the vice premiers, Muhua Chen, proposed enforcing penalties on unauthorized births, and the central government issued Document 11 in February 1982 and Document 7 in 1984, authorizing provincial governments to create local OCP fine regulations (Gu et al., 2007).[16] The initial fine rates were maintained (or even decreased; Figures A1 and A2), as provincial governments had little motivation to impose high fine rates since high rates increased social instability by provoking protests and criticism of government policies.[17]

However, fine rates grew dramatically by the end of the 1980s when the central government began to associate local official promotions with the success of fertility control under the OCP (Huang, 2017; Huang, Lei, and Sun, 2021). In the spring of 1989, premier Peng Li demanded that the OCP be supplemented with more detailed management by adding objectives and requiring that the results of birth control be "evaluative" (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). Since 1991, the central government has 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). Therefore, provincial officials were induced to impose higher fine rates and stricter rules in order to reduce fertility rates in their governed regions. Figure B1 demonstrates how fine rates significantly increased in 16 of the 30 provinces during the short period between 1989 and 1992, with the average fine rate jumping from 1.0 to 2.8 times the average household's annual income.[18] During this period, 16 of the 21 significant fine increases in the policy's history occurred (Huang, Lei, and Sun, 2021).

The formulation of fine rates exhibits exogeneity. First, the promotion incentive for provincial officials could have been a significant driving force in the changes to the fine rates. As stated in the previous paragraph, fine rates varied as provincial officials' assessment priorities shifted from

[16] 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).
[17] 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).
[18] A "significant" increase is greater than the annual income of the average household.

stability to birth control. A strong correlation between increases in fine rates and the occurrences of provincial government succession is also found by Huang, Lei, and Sun (2021). Twelve of the 16 significant fine increases mentioned above occurred within the first two years of a new provincial governor's tenure.[19] 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). 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. In addition, 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).[20]

Second, 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.

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.[21]

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[19] 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).

[20] Several of these former governors, such as Rongji Zhu, Changcun Li, and Guanzheng Wu, 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).

[21] 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).

Finally, as shown in Table A1, we extend the research of Huang, Lei, and Sun (2021) and find that fine rates are not significantly correlated with social inequality (*e.g.*, Gini coefficient), returns to education, regional culture (*e.g.*, son preference), or subpopulation groups within provinces (*e.g.*, percentage of elite officials in the provincial population). 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). These tests all suggest that the variation in OCP fines across provinces over time is exogenous (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 the "blooded aristocracy" (Lipset and Bendix, 2018). Unfortunately, generations of Chinese people/citizens 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, 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.[22] Thus, the

[22] Feng (2010) documents a city (*xian*) in central China where a small group of familes with authority control all of the administrative leadership positions. These families have intermarried relationships.

implementation of 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 better intergenerational mobility than a child born before 1979, when the OCP began to be implemented (Columns 1–2, Table A2). 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.

In our data set, on average, a child born after 1979 is expected to rank 0.258 percentile higher if their parents' income in their cohort ranks 1 percentile higher (Column 2, Row 1, Table A2), while a child born before 1979 is expected to rank 0.513 percentile higher (Column 1, Row 1, Table A2). Similar situations are found in relation to educational (Row 2, Table A2) and social class persistence (Row 3, Table A2). Therefore, the evidence supports the conclusion that the OCP may lead to lower intergenerational persistence, or conversely greater intergenerational mobility. The intergenerational persistence also demonstrates heterogeneity in geography and in gender. In terms of income, education, and social class rankings, a child born in an urban region has higher intergenerational persistence than a child born in a rural location (Columns 3–4, Table A2). A daughter also has better intergenerational persistence than a son in these ranks (Columns 5–6, Table A2).

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 summarized last.

3.1. Data Sources

We construct our dataset from ten national household survey datasets (Table A5): 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).[23]

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, which minimizes the lifecycle bias (Chetty et al., 2014; Emran and Shilpi, 2019). 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 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

[23] The Chinese Censuses used in this study are the ones from 2000, and 2005, which are surveys from adjacent years.

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, ethnicity (*Han* or non-*Han*), 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). 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 monetary fines levied for violating OCP ("Fine Rate") created by Ebenstein (2010). We match the "Fine Rate" data into the aforementioned combined dataset to assign a value for each observation, thus incorporating a metric for OCP intensity into the dataset.

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 A4. The birth years of the observations span from 1949 to 1996 (Table A5), and the survey years from 1986 to 2019 (Table A6). The average age of children is 35.91 years old (Table A4). 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. Twenty-eight point three percent 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%.[24]

4. Empirical Methodology

We employ the difference-in-difference (DID) method to estimate the effects of the OCP on intergenerational persistence. The penalty rules formulated by each provincial-level government indicate its strictness when implementing the OCP, which can include punitive measures varying from fines to discharge from work (Ebenstain, 2010). In order to determine the causal relationship between the OCP and relative mobility in China, we use the amount that policy violators were penalized with pay adjustments across provinces generated by Ebenstain (2010) as the policy intensity variable, which is a widely used proxy in literature (Section 2.2). The mathematical expression is:

$$Rank_{iht} = RP^{rr} \times Rank_{ift} + \beta \times Fine_{hp} \times Rank_{ift} \times I(h > 1979) + \beta_f Fine_{hp} \times I(h > 1979) + X_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (1)$$

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 his cohort h in year t; $Rank_{ift}$ is his parents' rank within the

[24] 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

parents' cohort f in year t, and RP_i^{rr} (termed as the rank-rank relative intergenerational persistence to measure mobility) is the coefficient of a child's rank regressed on his parent's rank.

$Fine_{hpt}$ is the amount of the fine in child i's birth province p in year t. This variable captures key elements of the variation in the strictness of the local implementation of the OCP (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.[25] The coefficient β identifies the effect of the OCP on intergenerational persistence.

Control variables, X_i, include child i's gender, marital status (whether child i is married/in de facto union), whether child i is the only child, 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 sources and the wild-bootstrap standard errors in Table A7.[26]

We focus on the key coefficient, RP_i^{rr}, which shows the effect of the OCP on intergenerational persistence. We select three alternative time periods to observe the focal effect: 1976–1982, 1974–1984, and 1969–1989 cohorts (the 1979 cohort being excluded), that is, three, five, and ten years before and after the initial implementation of OCP in 1979.

To further detect the heterogeneities, we perform the same regressions in two sets of sub-samples—male and female child samples, urban and rural family samples—and the total sample after controlling for ethnicity (*Han* or non-*Han*). We control for ethnicity due to research demonstrating that OCP regulations were more lenient for non-*Han* minorities (Huang, Lei, and Zhao, 2016).

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

[25] We conduct robustness tests that include the 1979 cohort data in Section 5.2.3 and that adjust the fine to a three-year-average in Section 5.2.4 to account for the temporality of reproductive decisions affected by the OCP.
[26] Our results are robust to the different standard error calculation methods.

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:

$$Rank_{iht} = RP^{rr} \times Rank_{ift} + \sum_{\gamma=1949}^{2000} \beta_\gamma Fine_{hp} \times Rank_{ift} \times I(h = \gamma) + \beta_f Fine_{hp} \times I(h > 1979) + X_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht} \quad (2)$$

where the 1979 cohort is excluded. We used a pseudo-fine for periods before OCP implementation. The pseudo-fine is the average fine following the application of the OCP since 1979; as a result, the pseudo-fine can represent the OCP intensity in each province.

[Figure 1 approximately here]

We report the associated coefficients in Table A8 and the graph shown in Figure 1, showing 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.

The results of the regression show that for observations made on the 1976–1978 cohorts versus the 1980–1982 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, Table 1). This coefficient indicates that rank-rank intergenerational income persistence decreases by 9.5 percentage points for every fine equal to the annual income that a parent earns. Furthermore, this coefficient remains significantly negative for all child groups, including female (-0.104; Column 2, Row 1), male (-0.089; Column 3, Row 1), urban (-0.043; Column 4, Row 1), and rural (-0.110; Column 5, Row 1) subsamples, as well as after controlling for ethnicity (-0.044; Column 6, Row 1).

In addition, results of the 1974–1978 versus 1980–1984 cohorts (five years before and after OCP

introduction, Row 2) and of the 1969–1978 versus 1980–1989 cohorts (ten years before and after OCP implementation, Row 3) both show that the OCP impacts on intergenerational income persistence are similarly strongly negative.

[Table 1 approximately here]

For the regression on intergenerational educational persistence in the 1976–1978 cohorts versus 1980–1982 cohorts (three years before and after the OCP), the interaction term of income and fine has a coefficient of -0.162 (Row 4, Column 1, Table 1). This shows that rank-rank intergenerational educational persistence decreases by 16.2 percentage points for each fine equal to one year's income. Furthermore, the OCP has a significant negative effect on intergenerational persistence in education in most subsamples (Rows 4–6, Table 1).

Similarly, the rank-rank intergenerational social class persistence falls by 8.1 percentage points (Row 7, Column 1, Table 1) for each fine equal to one year's income when comparing cohorts of 1976–1978 to those of 1980–1982. Furthermore, the OCP effect on intergenerational social class persistence was equally strong, negative, and significant for intergenerational social class persistence across all groups (Rows 7–9).

These findings imply that the OCP may increase mobility while also having a spillover effect on non-compliant individuals. This is corroborated by the result that, after controlling for ethnicity, the major impacts of the OCP on intergenerational persistence remained consistent, with the non-*Han* minority ethnic group being subjected to less restriction. One plausible explanation is that the OCP limited the number of children overall, affecting their entire generation, which includes both people who are in compliance and those who are not.[27]

We further calculate an average total OCP effect on intergenerational persistence for cohorts born between 1980 and 2000 (Table A9). The calculation is based on the population-weighted fine for cohorts born between 1980 and 2000 multiplied by the baseline estimate of the elasticity of the one-year income equivalent fine (3 years before and after policy implementation; Row 1, Column 1; Row 4, Column 1; and Row 7, Column 1). For the 1980–2000 cohorts nationwide, the average fine is equal to 1.519 annual incomes. The OCP decreased the intergenerational persistence in

[27] Our theoretical framework in Section 6 and empirical research on mechanisms of the OCP influence on intergenerational persistent in Section 7.3 provide further details to explain the OCP's spillover effect.

income, education, and social class by 14.4 (Column 2), 24.6 (Column 4), and 12.3 (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), 50.9% (Column 5), and 24.5% (Column 7), respectively, based on estimates of intergenerational persistence prior to 1979 (Table A2, Column 3).

We also calculate the OCP effect on the intergenerational persistence for each province. We find that the OCP decreased income, education, and social class persistence respectively by a range of 11.7% to 44.2% (Column 3), 21.1% to 80.0% (Column 5), and 10.1% to 38.4% (Column 7) for different provinces.

5.2. Robustness checks

We conduct robustness tests on identification assumptions (test for the LLF effect, placebo test, and treatment heterogeneity 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).

5.2.1. Identification assumptions

We conduct three 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 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 A10).[28] This result indicates that the LLF had no effect on intergenerational persistence before the implementation of OCP.

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

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 A11).

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 A12).

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 A13).

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 A14).

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 A15).

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.

We first perform the regression including the cohort of 1979 to determine whether baseline effects remained. This transitory cohort is not included in baseline regressions. The result demonstrates that the conclusion's relevance and size does not change when the 1979 cohort's data are added (Table A16).

Second, 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 A17). 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. The primary outcomes remain the same, supporting our key findings that the OCP reduces intergenerational persistence (Table A18).

Third, 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 1; Table A19).

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 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.[29] As a result, we assume that an individual's wealth is accumulated through earnings. Individuals in each generation are divided

[29] 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).

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 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:

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

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

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

$$= \frac{1}{2} + \frac{1}{2} exp(-ARe_l) - \frac{1}{2} exp(-ARe_h) \quad (14)$$

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

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

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.[30] We use a DDD regression to test the hypothesis that the OCP reduces the number of children in elite families as 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]

[30] 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.

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.[31] 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]

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

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 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).

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.

The OCP is often criticized as a cruel intervention that curtailed basic human reproductive rights, particularly in some extreme cases of brutal implementation. However, the unintended improving effects of the OCP on intergenerational mobility and thus social stability can be beneficial. The OCP averted "Malthusian collapse," putting an end to a cycle of recursive and often bloody revolutions and regime changes that had occurred hundreds of times in Chinese history. Such regime changes, from the standpoint of intergenerational mobility, are essentially violent redistributions of social class when intergenerational mobility is insufficient to provide nonviolent alternatives. Increased intergenerational mobility as a result of the OCP may thus provide adequate alternatives to violent regime changes in the future.

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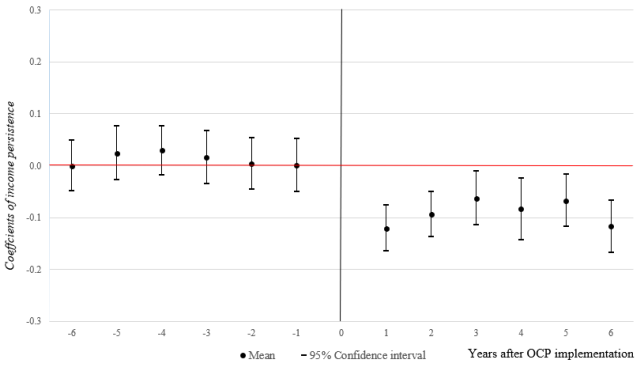
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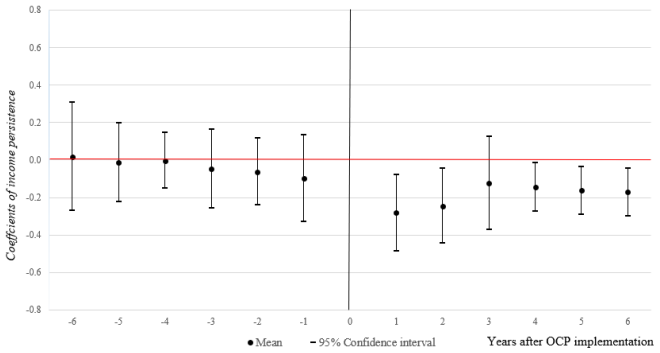
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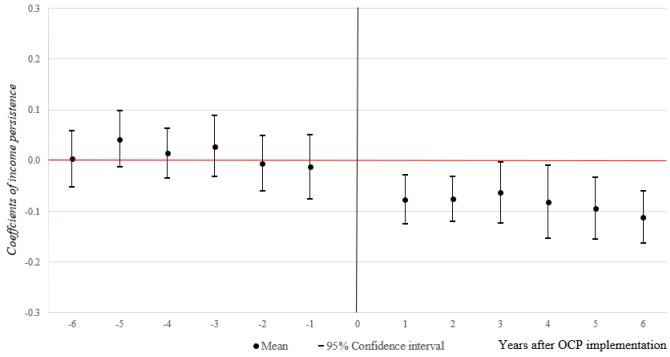
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(a) Relative Income Persistence



(b) Relative Educational Persistence



(c) Relative Social class Persistence

Figure 1. 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

	All	Female	Male	Urban	Rural	All, ethnicity controlled
	(1)	(2)	(3)	(4)	(5)	(6)
1.Income X Fine: (1976–1978) versus (1980–1982)	-0.095***	-0.104***	-0.089***	-0.043**	-0.110***	-0.044***
	(0.013)	(0.017)	(0.013)	(0.019)	(0.012)	(0.010)
Observations	69,817	30,915	38,902	13,228	56,019	36,207
2. Income X Fine: (1974–1978) versus (1980–1984)	-0.095***	-0.100***	-0.093***	-0.036*	-0.111***	-0.059***
	(0.011)	(0.016)	(0.012)	(0.018)	(0.011)	(0.010)
Observations	108,974	48,111	60,863	21,238	85,670	59,495
3. Income X Fine: (1969–1978) versus (1980–1989)	-0.103***	-0.106***	-0.103***	-0.069***	-0.110***	-0.097***
	(0.009)	(0.010)	(0.011)	(0.011)	(0.011)	(0.009)
Observations	201,587	88,278	113,309	42,753	149,269	122,200
4.Education X Fine (1976–1978) versus (1980–1982)	-0.162**	-0.170***	-0.190**	-0.226***	-0.115***	-0.132*
	(0.074)	(0.051)	(0.092)	(0.075)	(0.036)	(0.076)
Observations	332,590	95,675	236,915	134,151	197,892	299,427
5. Education X Fine (1974–1978) versus (1980–1984)	-0.164***	-0.171***	-0.192**	-0.210***	-0.124***	-0.146**
	(0.056)	(0.043)	(0.072)	(0.057)	(0.031)	(0.060)
Observations	535,977	149,974	386,003	201,183	332,763	486,826
6. Education X Fine (1969–1978) versus (1980–1989)	-0.167***	-0.147***	-0.205***	-0.200***	-0.136***	-0.173***
	(0.038)	(0.034)	(0.045)	(0.043)	(0.029)	(0.041)
Observations	1,052,278	269,903	782,375	381,282	661,475	970,702
7. Social class X Fine: (1976–1978) versus (1980–1982)	-0.081***	-0.075***	-0.086***	-0.039**	-0.096***	-0.046***
	(0.013)	(0.018)	(0.014)	(0.018)	(0.016)	(0.011)
Observations	69,987	30,967	39,020	13,317	56,099	36,374
8. Social class X Fine: (1974–1978) versus (1980–1984)	-0.089***	-0.076***	-0.099***	-0.053**	-0.099***	-0.072***
	(0.011)	(0.014)	(0.013)	(0.021)	(0.011)	(0.013)
Observations	109,278	48,204	61,074	21,416	85,795	59,794
9. Social class X Fine: (1969–1978) versus (1980–1989)	-0.110***	-0.097***	-0.120***	-0.090***	-0.113***	-0.111***
	(0.009)	(0.009)	(0.011)	(0.015)	(0.010)	(0.010)
Observations	202,019	88,414	113,605	43,010	149,443	122,626

Controls:						
Gender	X			X	X	X
Single parent, One child, Marriage status	X	X	X	X	X	X
Ethnicity						X
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

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income. The dependent variable is the rank of children's income 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. ***, **, and * indicate statistical significance at the 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	-0.133**			
	(0.055)			
Elite officials X fine	-0.680***			
	(0.079)			
Income rank		0.358***		
		(0.011)		
Income rank X fine		-0.095***		
		(0.012)		
Education rank			0.307***	
			(0.025)	
Education X fine			-0.061***	
			(0.019)	
Social class rank				0.315***
				(0.010)
Social class rank X fine				-0.030**
				(0.012)
Gender	-0.068***	-16.936***	-9.307***	-12.946***
	(0.013)	(0.593)	(0.917)	(0.395)
Fine	-0.010	4.912***	0.887	3.480***
	(0.059)	(0.907)	(0.877)	(0.669)
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	1,065,457	177,958	1,316,020	187,694
R-squared	0.228	0.307	0.191	0.265

Note: Each column in this table reports the coefficient from a series of generalized DID regression to estimate OCP intensity (fine rate) effect on the self-rank on spouse rank (column 1 to 3) and compare the difference of OCP intensity (fine rate) effect on child number between elite officials and other people (column 4). 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 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	-0.166***	0.451*	0.749**	4.282*	5.107**	2.304***
	(0.034)	(0.260)	(0.274)	(2.255)	(2.019)	(0.629)
Poor	0.131***	-0.021	0.188	0.314	5.464	-0.513
	(0.026)	(0.253)	(0.193)	(1.841)	(3.453)	(0.400)
Fine	0.134***	-0.545**	0.146	-0.624	-5.732**	-3.009***
	(0.039)	(0.233)	(0.562)	(2.253)	(2.244)	(0.947)
Gender	-0.211***	0.452	0.444***	2.281**	1.667	-1.485***
	(0.017)	(0.502)	(0.119)	(1.109)	(1.312)	(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	315,714	1,371	15,717	5,250	3,480	49,433
R-squared	0.436	0.209	0.220	0.076	0.144	0.092

Note: Each column in this table reports the coefficient from a series of generalized DDD regression to compare the OCP intensity (fine rate) effect difference between poor 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 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	0.878***	0.957***	1.013***
	(0.046)	(0.048)	(0.049)
Fine	0.719***	0.525***	0.236**
	(0.117)	(0.116)	(0.108)
Middle school	1.736***		
	(0.061)		
Middle school X Fine	-0.961***		
	(0.075)		
High school		1.808***	
		(0.084)	
High school X Fine		-1.047***	
		(0.087)	
College			1.825***
			(0.094)
College X Fine			-1.148***
			(0.100)
Province FE	X	X	X
Cohort FE	X	X	X
Dataset FE	X	X	X
Year FE	X	X	X
Observations	523,732	523,732	523,732
R-squared	0.286	0.284	0.269

Note: Each column in this table reports the coefficient from a series of generalized DID regression to estimate OCP intensity (fine rate) effect on the return of education on income. we used 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 1%, 5%, and 10%, respectively.

Appendix A. Supplemental Figures and Tables to the Main Text

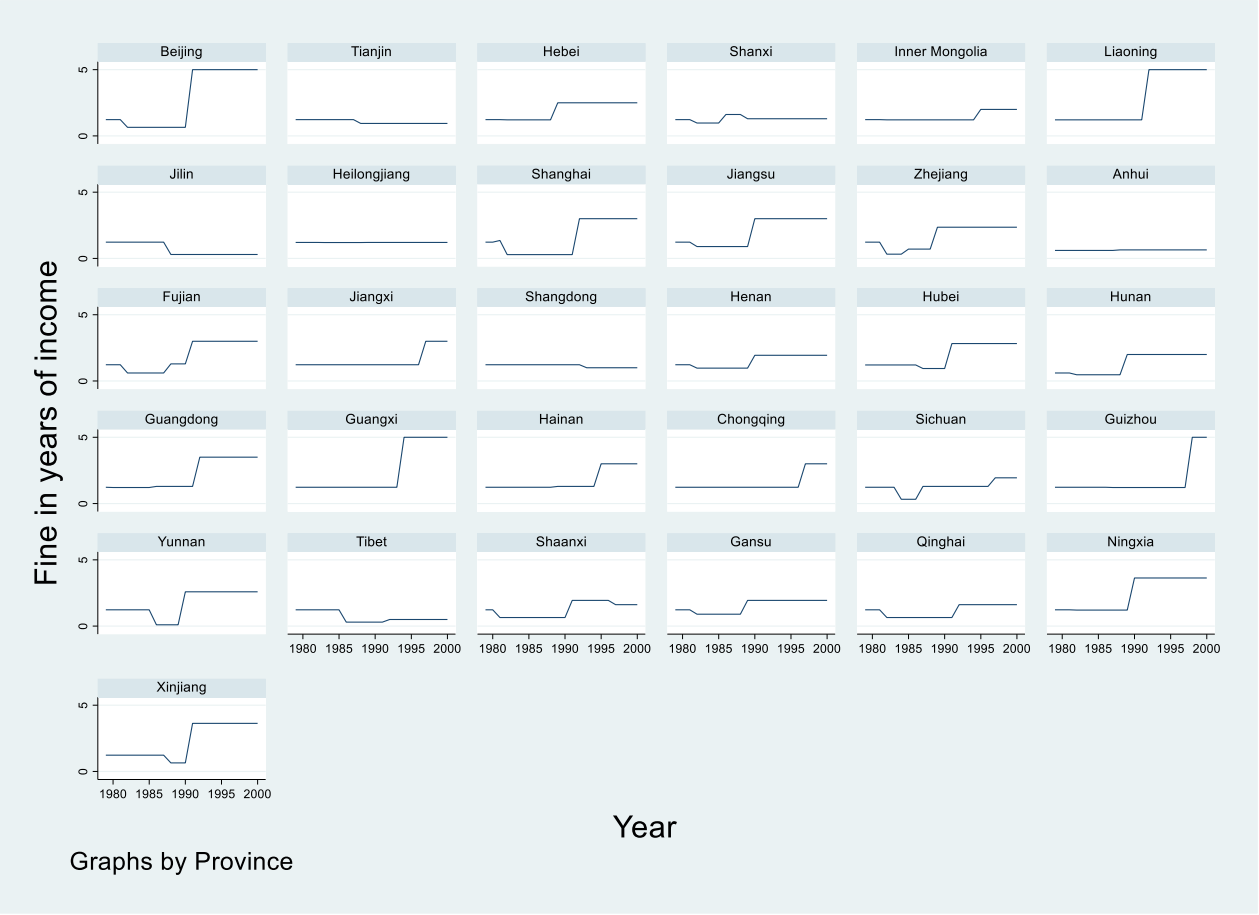
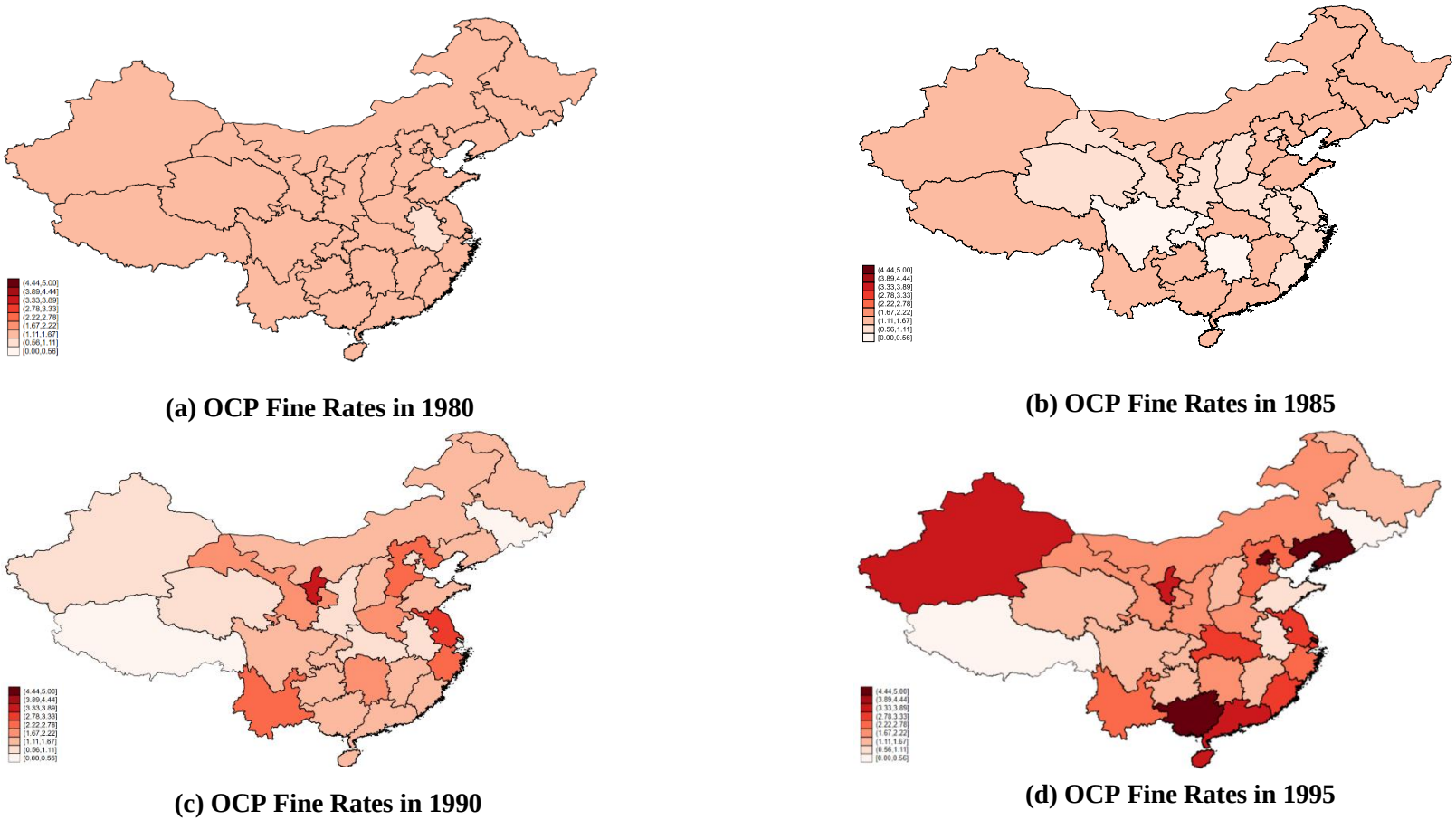


Figure A1. The OCP Fine Rates from 1979–2000, by Province

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 Province 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.

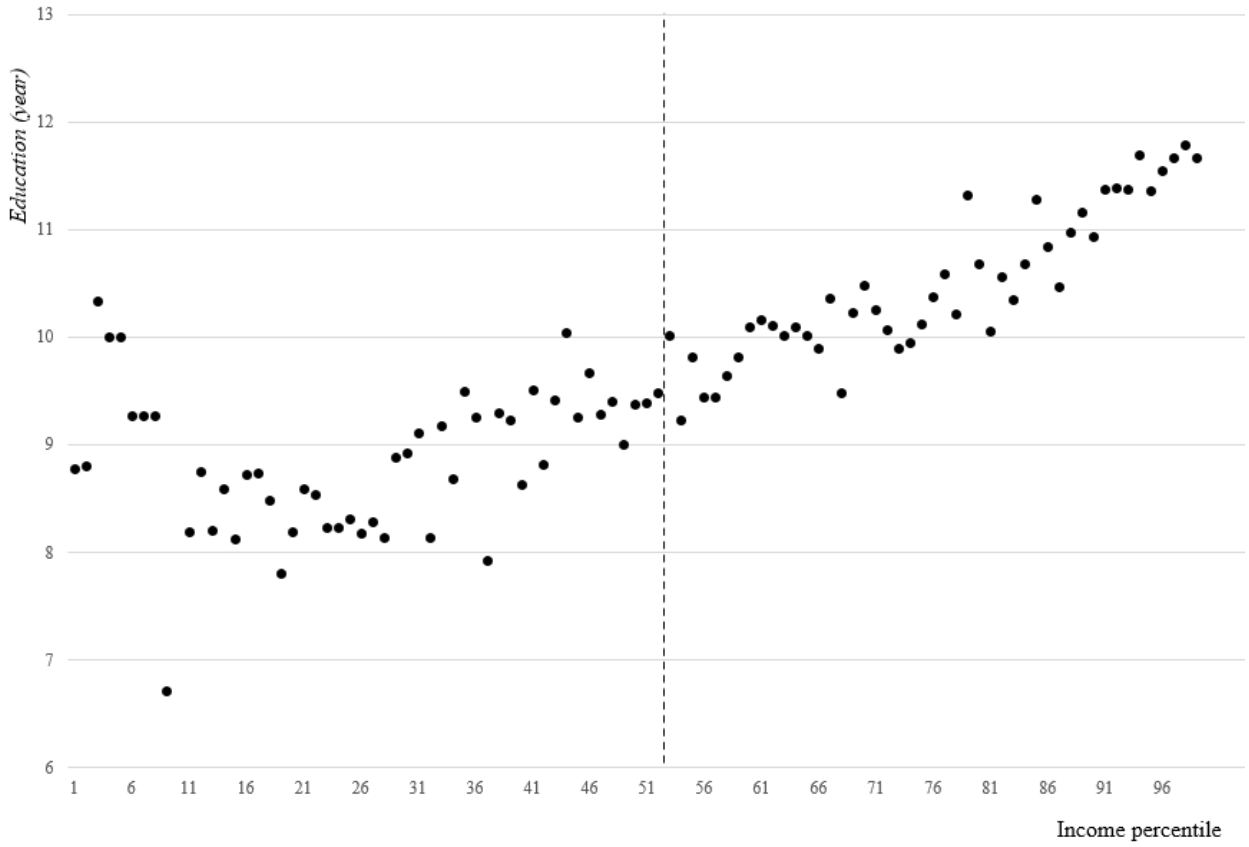


Figure A3. Children from rich families are more educated (cohorts after 1979)

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. 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)
Gini coefficient	0.736	0.694	0.782	0.861
Observations	162	135	108	81
Sexist social belief (proxied by the density of genealogy books)	0.653	–	–	–
Observations	638			
Ratio of elite officials in provincial population	0.739	0.715	0.205	0.336
Observations	494	494	494	494
Returns to education	0.865	–	–	–
Observations	286			

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 A2. Relative Persistence Estimates at the National Level: Different Demographic Groups

Dependent variables	Before 1979	After 1979	Urban	Rural	Female	Male
	(1)	(2)	(3)	(4)	(5)	(6)
1. Income rank	0.513***	0.258***	0.635***	0.519***	0.454***	0.400***
	(0.022)	(0.018)	(0.021)	(0.027)	(0.025)	(0.017)
Observations	245,978	111,359	53,684	179,284	161,616	209,554
2. Education rank	0.483***	0.317***	0.638***	0.408***	0.481***	0.456***
	(0.039)	(0.033)	(0.046)	(0.033)	(0.020)	(0.042)
Observations	1,828,502	220,117	655,335	1,138,255	551,175	1,310,428
3. Social class rank	0.503***	0.269***	0.574***	0.475***	0.421***	0.413***
	(0.025)	(0.024)	(0.022)	(0.035)	(0.027)	(0.021)
Observations	253,082	114,211	56,820	185,978	167,314	214,018

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 A3. 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 A4. 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 A5. 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 A6. 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 A7. Robustness checks, for different clustering methods

	(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, Clustered by province and data	-0.095***	-0.095**	-0.103**
	(0.022)	(0.028)	(0.039)
Income X Fine, Baseline with wild bootstrap standard errors	-0.095***	-0.095***	-0.103***
	(0.013)	(0.011)	(0.009)
Education X Fine, Baseline	-0.162**	-0.164***	-0.167***
	(0.074)	(0.056)	(0.038)
Education X Fine, Clustered by province and data	-0.162**	-0.164***	-0.167***
	(0.050)	(0.039)	(0.024)
Education X Fine, Baseline with wild bootstrap standard errors	-0.162**	-0.164***	-0.167***
	(0.074)	(0.056)	(0.038)
Social class X Fine, Baseline	-0.081***	-0.089***	-0.110***
	(0.013)	(0.011)	(0.009)
Social class X Fine, Clustered by province and data	-0.081***	-0.089***	-0.110**
	(0.016)	(0.022)	(0.032)
Social class X Fine, Baseline with wild bootstrap standard errors	-0.081***	-0.089***	-0.110***
	(0.013)	(0.011)	(0.009)

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 A8. 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 A9. Average OCP's Effect on Intergenerational Persistence, for 1980–2000 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%	0.315	65.2%	0.158	31.3%
Tianjin	1.094	0.104	20.3%	0.177	36.7%	0.089	17.6%
Hebei	1.802	0.171	33.4%	0.292	60.4%	0.146	29.0%
Shanxi	1.281	0.122	23.7%	0.208	43.0%	0.104	20.6%
Inner Mongolia	1.366	0.130	25.3%	0.221	45.8%	0.111	22.0%
Liaoning	2.385	0.227	44.2%	0.386	80.0%	0.193	38.4%
Jilin	0.756	0.072	14.0%	0.122	25.3%	0.061	12.2%
Heilongjiang	1.207	0.115	22.4%	0.196	40.5%	0.098	19.4%
Shanghai	1.169	0.111	21.6%	0.189	39.2%	0.095	18.8%
Jiangsu	1.820	0.173	33.7%	0.295	61.0%	0.147	29.3%
Zhejiang	1.519	0.144	28.1%	0.246	51.0%	0.123	24.5%
Anhui	0.630	0.060	11.7%	0.102	21.1%	0.051	10.1%
Fujian	1.627	0.155	30.1%	0.264	54.6%	0.132	26.2%
Jiangxi	1.488	0.141	27.6%	0.241	49.9%	0.121	24.0%
Shandong	1.170	0.111	21.7%	0.190	39.3%	0.095	18.8%
Henan	1.421	0.135	26.3%	0.230	47.7%	0.115	22.9%
Hubei	1.695	0.161	31.4%	0.275	56.8%	0.137	27.3%
Hunan	1.204	0.114	22.3%	0.195	40.4%	0.097	19.4%
Guangdong	2.031	0.193	37.6%	0.329	68.1%	0.165	32.7%
Guangxi	2.148	0.204	39.8%	0.348	72.0%	0.174	34.6%
Hainan	1.632	0.155	30.2%	0.264	54.7%	0.132	26.3%
Chongqing	1.502	0.143	27.8%	0.243	50.4%	0.122	24.2%
Sichuan	1.244	0.118	23.0%	0.201	41.7%	0.101	20.0%
Guizhou	1.723	0.164	31.9%	0.279	57.8%	0.140	27.8%
Yunnan	1.652	0.157	30.6%	0.268	55.4%	0.134	26.6%
Tibet	0.670	0.064	12.4%	0.109	22.5%	0.054	10.8%
Shaanxi	1.138	0.108	21.1%	0.184	38.2%	0.092	18.3%
Gansu	1.503	0.143	27.8%	0.244	50.4%	0.122	24.2%
Qinghai	1.106	0.105	20.5%	0.179	37.1%	0.090	17.8%
Ningxia	2.384	0.226	44.1%	0.386	80.0%	0.193	38.4%
Xinjiang	2.104	0.200	39.0%	0.341	70.6%	0.170	33.9%
National Average	1.519	0.144	28.1%	0.246	50.9%	0.123	24.5%

Note: The average fine is weighted by the population of 1980-2000 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 policy implementation, Table 1, Row 1, Column 1; Row 4, Column 1; and Row 7, Column 1). The magnitude is the percentage of decreased persistence compared to the nationwide estimation of average persistence before 1979 (Table A2, Column 1).

Table A10. 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	-0.029		
	(0.225)		
LLF X Fine X Parent(s)' Educational Rank		-0.675	
		(0.556)	
LLF X Fine X Parent(s)' Social class Rank			0.134
			(0.276)
Controls:			
LLF X Fine, Fine	X	X	X
Gender	X	X	X
One child, Marriage status	X	X	X
Cohort, Year, Data FE	X	X	X
Observations	41,162	249,675	41,192

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 1%, 5%, and 10%, respectively.

Table A11. 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 1%, 5%, and 10%, respectively.

Table A12. 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 1%, 5%, and 10%, respectively.

Table A13. Alternative Interpretations: Reinstitution of The College Entrance Examination and University Enrollment Expansions

	(1976–1978) versus (1980–1982) (1)	(1974–1978) versus (1980–1984) (2)	(1969–1978) versus (1980–1989) (3)
Panel A: Income persistence			
Fine X Income	-0.098***	-0.088***	-0.066***
	(0.014)	(0.012)	(0.010)
Reinstitution of The College Entrance Examination and University Enrollment Expansions X Income	0.036	-0.052	-0.210***
	(0.086)	(0.061)	(0.040)
Observations	69,817	108,974	201,587
Panel B: Educational persistence			
Fine X Education	-0.190***	-0.170***	-0.089**
	(0.047)	(0.045)	(0.044)
Reinstitution of The College Entrance Examination and University Enrollment Expansions X Education	0.326	0.045	-0.433*
	(0.391)	(0.263)	(0.223)
Observations	332,590	535,977	1,052,278
Panel C: Social class persistence			
Fine X Social class	-0.073***	-0.071***	-0.063***
	(0.015)	(0.013)	(0.010)
Reinstitution of The College Entrance Examination and University Enrollment Expansions X Social class	-0.093	-0.137**	-0.263***
	(0.089)	(0.063)	(0.041)
Observations	69,987	109,278	202,019
Controls:			
Gender	X	X	X
Single parent, One child, Marriage status	X	X	X
Province FE	X	X	X
Cohort FE	X	X	X
Year FE	X	X	X

Note: Each column in the three panels reports the coefficient from a generalized DID regression of OCP violation punishment fine and/or a contemporary historical event: reinstitution of the college entrance examination and university enrollment expansions. The dependent variables are children's income, educational, or social class persistence 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 1%, 5%, and 10%, respectively.

Table A14. The OCP Effect on Relative Persistence, with the father representing parents

	All	Female	Male	Urban	Rural	All, ethnicity controlled
	(1)	(2)	(3)	(4)	(5)	(6)
1.Income X Fine: (1976–1978) versus (1980–1982)	-0.080***	-0.071***	-0.084***	-0.041**	-0.089***	-0.052***
	(0.011)	(0.015)	(0.012)	(0.019)	(0.010)	(0.008)
Observations	65,862	29,499	36,363	12,367	52,937	33,558
2. Income X Fine: (1974–1978) versus (1980–1984)	-0.078***	-0.064***	-0.088***	-0.031*	-0.089***	-0.058***
	(0.009)	(0.013)	(0.011)	(0.017)	(0.009)	(0.010)
Observations	103,061	46,039	57,022	19,953	81,062	55,373
3. Income X Fine: (1969–1978) versus (1980–1989)	-0.099***	-0.072***	-0.116***	-0.072***	-0.103***	-0.102***
	(0.009)	(0.009)	(0.012)	(0.012)	(0.010)	(0.011)
Observations	183,929	81,950	101,979	38,971	135,870	109,198
4.Education X Fine (1976–1978) versus (1980–1982)	-0.135*	-0.157***	-0.158*	-0.199**	-0.099***	-0.104
	(0.072)	(0.046)	(0.090)	(0.079)	(0.029)	(0.077)
Observations	332,583	95,673	236,910	134,149	197,887	299,421
5. Education X Fine (1974–1978) versus (1980–1984)	-0.141**	-0.160***	-0.162**	-0.185***	-0.110***	-0.123**
	(0.054)	(0.038)	(0.069)	(0.060)	(0.024)	(0.059)
Observations	535,967	149,970	385,997	201,180	332,756	486,817
6. Education X Fine (1969–1978) versus (1980–1989)	-0.168***	-0.152***	-0.200***	-0.201***	-0.133***	-0.171***
	(0.036)	(0.032)	(0.043)	(0.041)	(0.025)	(0.038)
Observations	992,915	259,017	733,898	368,657	615,193	914,304
7. Social class X Fine: (1976–1978) versus (1980–1982)	-0.062***	-0.045***	-0.072***	-0.028	-0.070***	-0.045***
	(0.012)	(0.016)	(0.015)	(0.018)	(0.015)	(0.009)
Observations	65,948	29,532	36,416	12,413	52,977	33,643
8. Social class X Fine: (1974–1978) versus (1980–1984)	-0.064***	-0.039***	-0.081***	-0.031*	-0.071***	-0.058***
	(0.010)	(0.013)	(0.013)	(0.018)	(0.011)	(0.011)
Observations	103,201	46,087	57,114	20,036	81,119	55,512
9. Social class X Fine: (1969–1978) versus (1980–1989)	-0.093***	-0.058***	-0.115***	-0.083***	-0.091***	-0.102***
	(0.008)	(0.008)	(0.010)	(0.013)	(0.010)	(0.011)
Observations	184,145	82,024	102,121	39,106	135,951	109,413

Controls:						
Gender	X			X	X	X
Single parent, One child, Marriage status	X	X	X	X	X	X
Ethnicity						X
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

Note: Each cell in this table reports the coefficient from a generalized DID regression of OCP intensity (violation punishment fine rate) on income. The dependent variable is the rank of children's income 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 1%, 5%, and 10%, respectively.

Table A15. The OCP Effect on Relative Persistence, 3-year average fine

	All	Female	Male	Urban	Rural	All, ethnicity controlled
	(1)	(2)	(3)	(4)	(5)	(6)
1.Income X Fine: (1976–1978) versus (1980–1982)	-0.082***	-0.090***	-0.079***	-0.023	-0.101***	-0.041***
	(0.013)	(0.018)	(0.013)	(0.017)	(0.014)	(0.010)
Observations	69,992	30,821	39,171	13,870	55,537	37,846
2. Income X Fine: (1974–1978) versus (1980–1984)	-0.089***	-0.099***	-0.084***	-0.032*	-0.106***	-0.060***
	(0.012)	(0.016)	(0.012)	(0.016)	(0.014)	(0.011)
Observations	107,472	47,436	60,036	21,709	83,691	61,278
3. Income X Fine: (1969–1978) versus (1980–1989)	-0.089***	-0.094***	-0.089***	-0.055***	-0.097***	-0.094***
	(0.010)	(0.011)	(0.012)	(0.011)	(0.012)	(0.010)
Observations	196,498	86,062	110,436	42,563	144,382	121,921
4.Education X Fine (1976–1978) versus (1980–1982)	-0.112	-0.129**	-0.125	-0.169	-0.060	-0.079
	(0.094)	(0.059)	(0.123)	(0.112)	(0.040)	(0.106)
Observations	317,505	93,783	223,722	123,359	193,590	286,171
5. Education X Fine (1974–1978) versus (1980–1984)	-0.129*	-0.135***	-0.148	-0.175**	-0.081**	-0.118
	(0.070)	(0.042)	(0.091)	(0.085)	(0.038)	(0.080)
Observations	519,552	147,454	372,098	190,405	327,114	473,894
6. Education X Fine (1969–1978) versus (1980–1989)	-0.122**	-0.101***	-0.153**	-0.150**	-0.085**	-0.135**
	(0.047)	(0.030)	(0.059)	(0.055)	(0.034)	(0.053)
Observations	1,031,463	265,540	765,923	369,629	652,325	954,485
7. Social class X Fine: (1976–1978) versus (1980–1982)	-0.076***	-0.072***	-0.081***	-0.033**	-0.091***	-0.051***
	(0.012)	(0.019)	(0.014)	(0.015)	(0.015)	(0.011)
Observations	70,128	30,863	39,265	13,942	55,600	37,980
8. Social class X Fine: (1974–1978) versus (1980–1984)	-0.091***	-0.082***	-0.099***	-0.054***	-0.101***	-0.078***
	(0.011)	(0.016)	(0.013)	(0.017)	(0.013)	(0.011)
Observations	107,741	47,521	60,220	21,871	83,797	61,543
9. Social class X Fine: (1969–1978) versus (1980–1989)	-0.103***	-0.091***	-0.114***	-0.077***	-0.108***	-0.109***
	(0.010)	(0.010)	(0.011)	(0.015)	(0.010)	(0.010)
Observations	196,902	86,189	110,713	42,803	144,545	122,320

Controls:						
Gender	X			X	X	X
Single parent, One child, Marriage status	X	X	X	X	X	X
Ethnicity						X
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

Note: Each cell in this table reports the coefficient from a generalized DID regression of OCP intensity (violation punishment fine rate) on income. The dependent variable is the rank of children's income 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 1%, 5%, and 10%, respectively.

Table A16. The OCP Effect on Relative Persistence, including the 1979 cohort

	All	Female	Male	Urban	Rural	All, ethnicity controlled
	(1)	(2)	(3)	(4)	(5)	(6)
1.Income X Fine: (1976–1978) versus (1979–1981)	-0.073***	-0.080***	-0.068***	-0.036**	-0.084***	-0.034***
	(0.011)	(0.015)	(0.012)	(0.017)	(0.011)	(0.010)
Observations	83,102	36,850	46,252	15,533	66,948	43,058
2. Income X Fine: (1974–1978) versus (1979–1983)	-0.079***	-0.082***	-0.078***	-0.035**	-0.091***	-0.051***
	(0.011)	(0.015)	(0.011)	(0.016)	(0.011)	(0.009)
Observations	122,259	54,046	68,213	23,543	96,599	66,346
3. Income X Fine: (1969–1978) versus (1979–1988)	-0.094***	-0.096***	-0.095***	-0.067***	-0.098***	-0.091***
	(0.009)	(0.010)	(0.010)	(0.011)	(0.011)	(0.009)
Observations	214,872	94,213	120,659	45,058	160,198	129,051
4.Education X Fine (1976–1978) versus (1979–1981)	-0.081	-0.074	-0.111	-0.174**	-0.007	-0.054
	(0.074)	(0.049)	(0.086)	(0.083)	(0.038)	(0.080)
Observations	367,129	105,462	261,667	151,035	215,496	327,305
5. Education X Fine (1974–1978) versus (1979–1983)	-0.103	-0.105**	-0.127	-0.173**	-0.036	-0.080
	(0.064)	(0.043)	(0.076)	(0.072)	(0.038)	(0.071)
Observations	570,516	159,761	410,755	218,067	350,367	514,704
6. Education X Fine (1969–1978) versus (1979–1988)	-0.124**	-0.116***	-0.147**	-0.167***	-0.079**	-0.119**
	(0.048)	(0.033)	(0.057)	(0.057)	(0.032)	(0.054)
Observations	1,086,817	279,690	807,127	398,166	679,079	998,580
7. Social class X Fine: (1976–1978) versus (1979–1981)	-0.061***	-0.050***	-0.069***	-0.035**	-0.072***	-0.035***
	(0.012)	(0.016)	(0.013)	(0.016)	(0.015)	(0.010)
Observations	83,279	36,907	46,372	15,626	67,031	43,232
8. Social class X Fine: (1974–1978) versus (1979–1983)	-0.073***	-0.058***	-0.086***	-0.051**	-0.079***	-0.062***
	(0.010)	(0.013)	(0.012)	(0.019)	(0.010)	(0.011)
Observations	122,570	54,144	68,426	23,725	96,727	66,652
9. Social class X Fine: (1969–1978) versus (1979–1988)	-0.099***	-0.084***	-0.110***	-0.086***	-0.099***	-0.103***
	(0.009)	(0.010)	(0.010)	(0.015)	(0.010)	(0.009)
Observations	215,311	94,354	120,957	45,319	160,375	129,484

Controls:						
Gender	X			X	X	X
Single parent, One child, Marriage status	X	X	X	X	X	X
Ethnicity						X
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

Note: Each cell in this table reports the coefficient from a generalized DID regression of OCP intensity (violation punishment fine rate) on income. The dependent variable is the rank of children's income 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 1%, 5%, and 10%, respectively.

Table A17. Summary Statistics of the Difference-in-differences Groups

Variables	Control group (1976-1978)	Treatment group (1980-1982)	Control group (1974-1978)	Treatment group (1980-1984)	Control group (1969-1978)	Treatment group (1980-1989)
Children:						
Gender: Male = 1	0.706	0.690	0.725	0.652	0.758	0.607
Standard deviation	0.456	0.463	0.447	0.476	0.429	0.488
Observations	254,578	97,307	438,650	129,147	917,568	200,552
Age	26.807	27.678	27.566	28.004	30.103	27.694
Standard deviation	4.795	4.637	4.940	4.446	5.729	3.957
Observations	254,578	97,307	438,650	129,147	917,568	200,552
Income	116118.700	89374.510	83446.770	129888.000	119406.200	126335.800
Standard deviation	1,169,914	1,011,572	959,942	1,281,464	1,202,686	1,218,699
Observations	42,034	38,358	62,599	63,650	142,530	114,480
Education years	6.875	4.625	7.259	5.801	7.861	7.382
Standard deviation	3.287	4.020	3.170	4.623	3.269	4.984
Observations	251,665	95,698	435,288	125,946	913,563	195,337
Social class (0 = lowest, 4 = highest)	0.619	0.626	0.590	0.673	0.582	0.756
Standard deviation	0.711	0.708	0.693	0.726	0.696	0.724
Observations	42,693	39,179	63,907	64,834	145,241	116,550
Only child = 1	0.140	0.315	0.118	0.325	0.097	0.297
Standard deviation	0.347	0.465	0.323	0.468	0.297	0.457
Observations	246,936	91,303	424,696	119,557	883,718	181,624
Majority ethnicity = 1	0.899	0.873	0.900	0.862	0.906	0.870
Standard deviation	0.301	0.333	0.300	0.345	0.292	0.337
Observations	236,912	78,266	411,747	101,648	864,376	162,907
Rural = 1	0.620	0.465	0.631	0.518	0.631	0.558
Standard deviation	0.485	0.499	0.483	0.500	0.483	0.497
Observations	234,961	77,370	404,519	96,206	840,667	130,941
Born at Rural = 1	0.630	0.519	0.641	0.573	0.641	0.617
Standard deviation	0.483	0.500	0.480	0.495	0.480	0.486
Observations	254,139	97,167	436,801	128,915	908,379	200,135
Parents:						
Father income	38929.380	47905.080	36160.440	41632.210	39904.340	31458.830
Standard deviation	491505.300	555966.300	467994.900	495010.400	512570.700	382162.100
Observations	36,760	37,754	59,705	58,291	115,385	98,775
Mother income	8077.330	9689.372	7694.025	9632.859	7349.081	8744.506
Standard deviation	18063.650	21054.010	17121.120	21403.060	24428.540	20366.930
Observations	38,752	38,882	62,409	60,572	123,310	103,891
Father education years	7.479	7.948	7.406	7.746	7.254	7.278
Standard deviation	3.871	4.393	3.918	4.675	4.039	4.844
Observations	252,865	96,626	436,863	127,589	856,474	196,798

Mother education years	6.109	7.187	5.998	6.893	5.783	6.270
Standard deviation	4.144	4.540	4.187	4.811	4.330	4.944
Observations	254,010	97,048	438,029	128,602	890,142	198,836
Father social class (0 = lowest, 4 = highest)	0.617	0.647	0.610	0.642	0.602	0.612
Standard deviation	0.682	0.721	0.674	0.726	0.663	0.723
Observations	37,289	38,312	60,648	59,200	117,585	100,545
Mother social class (0 = lowest, 4 = highest)	0.483	0.500	0.477	0.487	0.477	0.441
Standard deviation	0.567	0.597	0.563	0.600	0.560	0.593
Observations	39,269	39,443	63,341	61,481	125,490	105,634

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

	All	Female	Male	Urban	Rural	All, ethnicity controlled
	(1)	(2)	(3)	(4)	(5)	(6)
1.Income X Fine: (1976–1978) versus (1980–1982)	-0.092***	-0.101***	-0.085***	-0.064***	-0.103***	-0.044***
	(0.013)	(0.017)	(0.015)	(0.021)	(0.013)	(0.014)
Observations	62,365	28,889	33,476	10,594	51,202	29,047
2. Income X Fine: (1974–1978) versus (1980–1984)	-0.093***	-0.098***	-0.091***	-0.062***	-0.102***	-0.064***
	(0.012)	(0.015)	(0.013)	(0.021)	(0.012)	(0.013)
Observations	96,249	44,500	51,749	16,795	77,389	47,415
3. Income X Fine: (1969–1978) versus (1980–1989)	-0.108***	-0.109***	-0.110***	-0.094***	-0.107***	-0.107***
	(0.010)	(0.010)	(0.013)	(0.012)	(0.012)	(0.010)
Observations	177,564	81,585	95,979	33,608	134,391	100,751
4.Education X Fine (1976–1978) versus (1980–1982)	-0.170***	-0.050***	-0.277***	-0.131***	-0.164***	-0.183***
	(0.012)	(0.012)	(0.017)	(0.025)	(0.011)	(0.016)
Observations	83,138	39,320	43,818	16,687	65,905	51,150
5. Education X Fine (1974–1978) versus (1980–1984)	-0.160***	-0.078***	-0.235***	-0.120***	-0.158***	-0.168***
	(0.014)	(0.011)	(0.021)	(0.025)	(0.014)	(0.016)
Observations	131,849	62,586	69,263	26,980	102,839	84,715
6. Education X Fine (1969–1978) versus (1980–1989)	-0.130***	-0.079***	-0.179***	-0.098***	-0.127***	-0.117***
	(0.013)	(0.010)	(0.020)	(0.017)	(0.014)	(0.013)
Observations	259,854	123,855	135,999	56,091	194,242	184,241
7. Social class X Fine: (1976–1978) versus (1980–1982)	-0.077***	-0.071***	-0.082***	-0.055**	-0.093***	-0.044***
	(0.014)	(0.018)	(0.015)	(0.021)	(0.017)	(0.015)
Observations	62,531	28,937	33,594	10,679	51,282	29,210
8. Social class X Fine: (1974–1978) versus (1980–1984)	-0.087***	-0.077***	-0.097***	-0.079***	-0.093***	-0.076***
	(0.013)	(0.015)	(0.015)	(0.023)	(0.012)	(0.016)
Observations	96,524	44,584	51,940	16,944	77,514	47,685
9. Social class X Fine: (1969–1978) versus (1980–1989)	-0.114***	-0.098***	-0.128***	-0.110***	-0.113***	-0.121***
	(0.011)	(0.010)	(0.013)	(0.014)	(0.011)	(0.011)
Observations	177,956	81,708	96,248	33,826	134,564	101,137

Controls:						
Gender	X			X	X	X
Single parent, One child, Marriage status	X	X	X	X	X	X
Ethnicity						X
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

Note: Each cell in this table reports the coefficient from a generalized DID regression of OCP intensity (violation punishment fine rate) on income. The dependent variable is the rank of children's income 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 1%, 5%, and 10%, respectively.

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

	All	Female	Male	Urban	Rural	All, ethnicity controlled
	(1)	(2)	(3)	(4)	(5)	(6)
1.Income X Fine: (1976–1978) versus (1980–1982)	-0.043***	-0.047***	-0.039**	-0.054**	-0.031**	-0.044***
	(0.013)	(0.016)	(0.018)	(0.022)	(0.013)	(0.013)
Observations	33,890	15,748	18,142	10,170	23,420	29,703
2. Income X Fine: (1974–1978) versus (1980–1984)	-0.072***	-0.077***	-0.068***	-0.059***	-0.075***	-0.066***
	(0.012)	(0.017)	(0.016)	(0.021)	(0.013)	(0.012)
Observations	55,413	25,730	29,683	16,205	38,723	48,563
3. Income X Fine: (1969–1978) versus (1980–1989)	-0.115***	-0.108***	-0.121***	-0.094***	-0.117***	-0.108***
	(0.010)	(0.011)	(0.012)	(0.012)	(0.013)	(0.010)
Observations	117,777	53,993	63,784	32,819	83,977	103,257
4.Education X Fine (1976–1978) versus (1980–1982)	-0.183***	-0.064***	-0.297***	-0.121***	-0.196***	-0.174***
	(0.014)	(0.018)	(0.023)	(0.027)	(0.013)	(0.016)
Observations	55,206	26,325	28,881	15,779	39,150	51,026
5. Education X Fine (1974–1978) versus (1980–1984)	-0.166***	-0.087***	-0.243***	-0.112***	-0.179***	-0.160***
	(0.015)	(0.015)	(0.024)	(0.025)	(0.016)	(0.016)
Observations	91,468	43,740	47,728	25,579	65,439	84,597
6. Education X Fine (1969–1978) versus (1980–1989)	-0.111***	-0.071***	-0.151***	-0.092***	-0.094***	-0.111***
	(0.012)	(0.013)	(0.018)	(0.017)	(0.012)	(0.013)
Observations	200,210	95,638	104,572	54,103	145,170	185,069
7. Social class X Fine: (1976–1978) versus (1980–1982)	-0.051***	-0.051***	-0.051**	-0.053**	-0.043***	-0.047***
	(0.013)	(0.015)	(0.019)	(0.021)	(0.015)	(0.013)
Observations	34,056	15,796	18,260	10,255	23,500	29,866
8. Social class X Fine: (1974–1978) versus (1980–1984)	-0.092***	-0.086***	-0.099***	-0.083***	-0.089***	-0.082***
	(0.014)	(0.016)	(0.018)	(0.024)	(0.013)	(0.014)
Observations	55,688	25,814	29,874	16,354	38,848	48,833
9. Social class X Fine: (1969–1978) versus (1980–1989)	-0.136***	-0.120***	-0.152***	-0.116***	-0.138***	-0.125***
	(0.011)	(0.011)	(0.013)	(0.015)	(0.013)	(0.011)
Observations	118,175	54,120	64,055	33,042	84,151	103,649

Controls:						
Gender	X			X	X	X
Single parent, One child, Marriage status	X	X	X	X	X	X
Ethnicity						X
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

Note: Each cell in this table reports the coefficient from a DID regression of OCP intensity (violation punishment fine rate) on income. The dependent variable is the rank of children's income in their cohort. 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 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

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

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

$$\theta exp(-x) - exp\left(-\frac{x}{\theta}\right) \qquad (2)$$

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 C. 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:

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

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]

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	-0.148	0.977***				
	(0.363)	(0.153)				
Fine	-0.086	0.909*	-0.535	-0.422	-0.138	0.289
	(0.158)	(0.519)	(0.341)	(0.315)	(0.607)	(0.573)
Gender X fine	0.201	-0.830***				
	(0.141)	(0.151)				
Poor			-0.036	0.503	0.552**	-0.388*
			(0.345)	(0.560)	(0.201)	(0.203)
Poor X fine			0.405	0.351	0.530	1.167***
			(0.364)	(0.352)	(0.341)	(0.246)
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	17,715	16,228	805	559	8,547	7,170
R-squared	0.217	0.230	0.261	0.193	0.212	0.260

Note: Each column in this table reports the coefficient from a series of generalized DDD regression to compare the OCP intensity (fine rate) effect difference on parent investments between gender (column 1 and 2) poor and rich families but in different gender sub-samples (column 3 to 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, and cohort FE. Robust standard errors are in parentheses and clustered by province. ***, **, and * indicate statistical significance at the 1%, 5%, and 10%, respectively.