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

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## Abstract

We examine whether and how the world's largest population-planning program, China's One-Child Policy, shaped intergenerational mobility. Using a continuous difference-in-differences approach and a data set with 2,096,798 child-parent(s) pairs from ten national household surveys, we leverage exogenous variation in rates of fines imposed for One-Child Policy violations across provinces to study those questions. We find that for cohorts born between 1980 and 1996, the policy reduced intergenerational persistence in income, education, and social class, compared to those born prior to 1979, thus undermining long-standing class solidification. Specifically, the fines reduced persistence in intergenerational income, education, and social class by 28.1%, 48.7%, and 24.8%, respectively. Analyzing mechanisms, we find that the policy caused these effects by diminishing elite-family heirship, concentrating lower-income families' resources, and decreasing returns to education.

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## 1. Introduction

Intergenerational mobility, an individual's ability to change their socioeconomic status during their lifetime independent of their family background, holds significant importance in promoting key aspects of economic development, such as equality of opportunity, human capital development, and social cohesion (Chetty et al. 2014; Bell et al. 2019). Because family size influences early-life environment and parental investment in children, which are key determinants of intergenerational mobility (Black and Devereux 2011; Chetty and Hendren 2018a), the impact of family size and population planning policies on intergenerational mobility outcomes is a central question in development. In this study, we leverage the shock to family size created by the China's One-Child Policy to provide rigorous causal evidence on the impact of family size on intergenerational mobility. We construct a novel large-scale dataset that allows us to evaluate the mechanisms for this effect. In doing so, we provide evidence on the potent role of public policies in shaping mobility outcomes.

The One-Child Policy (OCP) was implemented in 1979 and enforced through strict penalties and monetary fines for noncompliance (e.g., Ebenstein 2010; Huang 2017). The policy led to a substantial shift in family structure from a norm promoting multiple children to a single-child norm. 1 Concurrently, China witnessed a significant rise in intergenerational mobility, as shown in Figure 1 (also see Fan, Yi, and Zhang 2021).

[Insert Figure 1 here]

To identify the OCP's causal effect on intergenerational persistence, we first use rank-rank regression models to compare the rank of parents to that of children (Chetty et al., 2014) before and after the implementation of the OCP.[1] We then conduct a difference-in-differences analysis exploiting province-level variation in the rate of monetary fines imposed on parents for violating the policy.[2] We combine ten data sources to construct a large data set covering cohorts spanning fifty years (1949–96). The size and breadth of our sample allows us to analyze parent-child pairs

[1] 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 children did not reach the minimum wage-earning age in our sample.

[2] The approach of using monetary fines as the proxy of the strength of the One-Child Policy is well supported in the literature (Hardee-Cleaveland and Banister, 1988; McElroy and Yang, 2000; Li, Zhang, and Zhu, 2005; Ebenstein, 2010; Liu, 2014; Huang and Zhou, 2015; Huang, Lei and Zhao, 2015; Huang, Lei, and Sun, 2021): "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 the randomness of the variations in the monetary fines in Section 2.2.

by cohort and year while controlling for age (Chetty et al., 2014; Emran and Shilpi, 2019). The empirical results show that, in comparison to cohorts born before 1979, the OCP lowered persistence in average income, education, and social class by 28.1%, 48.7%, and 24.8%, respectively, for cohorts from 1980 to 1996. Different subgroups exhibited similar drops in intergenerational persistence.

We conduct numerous robustness tests. First, we assess robustness to identification assumptions by assessing pre-trends, including possible effects of the birth control policy implemented prior to the OCP, placebo tests, tests for treatment heterogeneity (Callaway, Goodman-Bacon, and Sant'Anna, 2021), and spillover of the policy on cohorts born before 1979 but affected by having fewer siblings or being the only child. Second, We evaluate alternative explanations for the result, including other contemporaneous policies in China, and explore alternative measures of both parents' rank and the OCP fine rates. Fourth, We explore external validity by investigating different subsamples of the data, including samples with a different policy interim period; gender-balanced samples; samples containing surveys that have addressed the coresidency issue, which has to do with whether the parent and child in a surveyed pair live in the same residence; samples with parents who experienced the Great Famine before giving birth and those who did not; samples with children who migrated from their birth province and those who did not; and reweighing the models by cohort size.[3] We find that results of each of these tests support our main conclusions.

A key strength of our work is that we substantially expand the number of observations relative to previous work. Studies that examine mobility in developed nations typically rely on administrative tax records, but as developing nations often lack comparable data sets, statistical power for studying mobility in such cases is often limited (Emran, Greene, and Shilpi, 2018; Emran and Shilpi, 2019). In developing countries, administrative tax databases often only cover a small and unrepresentative portion of the population, and data sets 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

[3] China's Great Famine (1958–61) had a significant negative impact on people's fertility, both physically and spiritually (Shi, 2011). See details in Section 5.2.4.

1970–80 birth cohorts as the early cohort and 11,333 observations for the 1980–88 cohorts as the late cohort. In the two studies most similar to ours, Zou (2015) and Yu, Fan, and Yi (2021) examine the impact of the OCP on intergenerational mobility using 77,658 and 25,618 observations, respectively. We construct a data set comparable in size to those used in research on developed-country mobility (e.g., Alesina, Stantcheva, and Teso, 2018; Feigenbaum, 2018), composed of 2,096,978 observations spanning 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 family size harms children. For example, they argue that larger families in modern societies give children fewer opportunities for upward mobility because they dilute resources 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).[4] The quantity-quality model is widely used in this strand of the literature.[5] In it, the interaction between quantity and quality in the budget constraint results in increasing marginal costs of quality with regard to family size.[6] However, most earlier research focuses on incomplete data and the unresolved prerequisite—the choice to have children—which is difficult to rule out as endogeneity (Black, Devereux, and Salvanes, 2005; Van Bavel et al., 2011). It is therefore uncertain whether intergenerational mobility is directly impacted by family size or whether the characteristics that lead to smaller families also have effects on mobility. China's OCP acts as an exogenous shock to the family-size decision in our work, making it an ideal natural experiment to address the causality conundrum.

This work also contributes to the underdeveloped area of intergenerational-mobility research that uses natural experiments 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 parents' labor-market status; Morris, Duncan, and Rodrigues

[4] 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).

[5] 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.

[6] 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).

(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 variation across local labor markets in exposure to the resource shock; and Fan, Yi, and Zhang (2021) compare the impact of China's economic reform since 1979 on intergenerational elasticity in income for the 1970–80 versus 1981–88 birth cohorts.

In the same literature but regarding the OCP's effects specifically, Zou (2015) finds that fertility reduces intergenerational income mobility and educational mobility when she interacts the OCP resistance rate with a dummy for whether one cohort was exposed to the policy; similarly, using differences in OCP exposure between women in rural and urban areas, Yu, Fan, and Yi (2021) find fertility reduces intergenerational income mobility. Both results are consistent with ours. We build on these two related studies by substantially increasing the number of observations and producing accurate intergenerational rankings for yearly cohort-based analysis. Our study is also the first to examine the mechanisms that drive the impacts of the policy. Our approaches extend beyond the straightforward implications of population change and add to the research on the policy's impacts 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 intergenerational transmission of socioeconomic status. 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, concentration of poor families' resources, and decreasing returns

to education) allows us to explain how fertility differentiation influences intergenerational transmission.

Though the OCP was rightly criticized harshly for interfering with fundamental reproductive rights, it is important to document how it increased intergenerational mobility because it can serve as an example for other highly populated countries with established social standing to thwart violent regime changes.

The rest of the paper is organized as follows. Section 2 provides the historical context for the OCP as well as data on intergenerational mobility for one-child families and details the formulation of fine rates. Section 3 discusses the data construction and provides descriptive statistics. The empirical strategy is described in Section 4. Section 5 describes the findings and conducts robustness tests. Section 6 presents empirical evidence on the underlying mechanisms. Section 8 concludes.

## 2. Background

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

### 2.1. The institutional background of the One-Child Policy

After two decades of pro-birth by the Mao-era government, Chinese Communist Party leaders in the 1970s decided to contain population growth by implementing fertility-control measures (Scharping, 2003; Ebenstein, 2010). In 1979 the central government promulgated the OCP, the first fertility-control measure to be written into law (Banister, 1991; Scharping, 2003).[7] Unlike its predecessors and many family planning policies in other countries, the OCP allowed married couples to have one and only one child, was mandatory rather than voluntary, and imposed penalties on violators.[8]

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

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

The Communist Party leaders regarded direct financial penalties as a potent tool for reducing childbearing, and these penalties often constituted a sizable fortune for the average household (Feng and Hao,1992; McElroy and Yang, 2000; Li, Zhang, and Zhu, 2005). At the very beginning of the OCP, Vice Premier Chen Muhua proposed new legislation to penalize unauthorized births (Huang, Lei, and Sun, 2021). The penalties would be suggested to the provincial governments (Scharping, 2003; Huang and Zhou, 2015). Public resistance arose (Huang, Lei, and Sun, 2021). The central government thus issued Document 11 in February 1982 to authorize the provincial governments to revise the penalty rules and then issued Document 7 in 1984 to authorize provincial governments to set their own fine rates (Gu et al., 2007).[9] However, because social stability was a key assessment indicator for provincial officials, the provincial governments were hardly incentivized to set a high fine rate (Scharping, 2003; Huang, Lei, and Sun, 2021).[10] As a result, the fine rates in most provinces went virtually unchanged from 1979 to 1989, and in some provinces they even decreased after Document 7 was issued (Figure A1).

But the fine rates significantly increased at the end of the 1980s, when the central government tied the promotion of local officials to fertility control in their territories (Scharping, 2003; Huang, Lei, and Sun, 2021). In the spring of 1989, Premier Li Peng told governors that limiting the population growth rate was critical for the survival of the Chinese race and should be controlled under evaluative targets (Greenhalgh and Winckler, 2005). In 1990, local officials were required to report the average number of children per woman in their region so the central government could determine whether their region hit the quantitative target assigned by the state family planning commission (Gu et al., 2007). In March 1991, to show its resolve, the central government listed family planning among the three basic state policies in the Eighth Five-Year Plan. The plan set an objective of reducing the natural growth rate to less than 1.25% per year over the following decade. To achieve the objective, national leaders employed the so-called responsibility system to induce subnational or provincial officials to set high penalty rates (Huang, Lei, and Sun, 2021). Furthermore, beginning in 1991, the central government consistently listed enforcement of the OCP as one of its three highest priorities, along with economic growth and social stability, when

[9] 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).

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

evaluating local officials (Birney, 2014). As a result, between 1989 and 1992, fine rates significantly increased in sixteen of the thirty provinces, with the average fine rate jumping from 1.0 times the average household's annual income to 2.8 times. Also, during this period, sixteen of the twenty-one significant fine increases in the policy's history occurred (Figure A1; Huang, Lei, and Sun, 2021).

The OCP impacted millions of couples and lasted for thirty-six years, ending in December 2015. Scholars generally agree that it was largely responsible for China's fertility-rate decline (for example, Lavely and Freedman, 1990; Yang and Chen, 2004; Li, Zhang, and Zhu, 2005).

### 2.2. OCP fine rates across provinces

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

Ebenstein (2010) calculates the fine rates as multiples of the average annual household income of a province.[11] Figures A1 and A2 depict the chronological and geographical patterns of fine rates from 1979 to 2000. The time trend across provinces is associated closely with the degree to which the OCP affected provincial officials' political prospects. From 1979 to 1988, the rates remained the same in most provinces and moderately decreased in a few provinces because the OCP generated resistance, which threatened the core appraisal indicator for the provincial officials: social stability. Between 1989—when the central government expressed its resolution to make the OCP more stringent and then established the birth control outcome of the OCP implementation as the core appraisal indicator for the provincial officials—and 1992, sixteen of the thirty provinces

[11] See Appendix 3 of Ebenstein (2010) for detailed calculation.

saw a significant increase in the fine rates from 0.8% to 3.0% of annual household income, on average. Sixteen of the twenty-one significant increases in rates in the history of the OCP happened in this short period (Figure A1; Huang and Zhou, 2015; Huang, Lei, and Sun, 2021).[12]

Regarding variation in fines across provinces, the promotion incentive for provincial officials could again have been a significant driving force.[13] Huang and Zhou (2015) and Huang, Lei, and Sun (2021) find a strong correlation between increases in fine rates and the incidences of government succession. Twelve of the sixteen significant fine increases mentioned above occurred within the first two years of new provincial governors' tenure.[14] The average age of the sixteen governors who increased the fines was fifty-six years, much younger than the national average of fifty-nine years (p-value = 0.02), indicating that they had a higher probability of being promoted per the Communist Party's cadre-appointment regulations; thus they were more incentivized to perform well in implementing the OCP.[15] These findings are consistent with the conjecture that a governor's personal characteristics, such as inauguration year and age, influenced their incentive to raise the fine rates (Huang, Lei, and Sun, 2021).

In addition, provincial governors were not restricted by the performance of the OCP implementation before inauguration. Governors 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 easing any concern that these governors were replaced because of poor OCP performance (Huang and Zhou, 2015; Huang, Lei, and Sun, 2021).[16]

Counterintuitively, the fines were not tailored to local fertility preferences. Using data from the the China Health and Nutrition Study's national survey, Liu (2014) finds that neither the relaxation of the OCP nor the amount of the fines was related to the preference for child quantity at the community level. We regress the provincial fine rates on the local birth rate one, two, and three

[12] A significant increase is one greater than the annual income of the average household.
[13] The reasons why the fines change are not formally or precisely documented (Huang, 2017).
[14] The average tenure of these sixteen 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).
[15] Their efforts were compensated. For instance, among the sixteen governors, one was elevated to executive vice premier, and the other to chairman of the Chinese People's Political Consultative Conference. Both of them were made permanent members of the Political Bureau of the Central Committee of the Communist Party, the central leadership of the Chinese Communist Party.
[16] Several of these former governors, such as Zhu Rongji, Li Changcun, and Wu Guanzheng, rose to become central-government political leaders. There is no evidence that any provincial governors were fired as a result of poor OCP implementation (Huang, Lei, and Sun, 2021).

years before the OCP and find no significant correlation (Table A2, Row 2), consistent with the findings of Liu (2014).

There is no significant 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 specific indices or sets of indices for demography, macroeconomics, government behaviors, and sanitary and educational conditions and find that none of these indicators are significantly correlated with the fine levels or changes at the 10% level after controlling for year and province fixed effects as well as province-specific linear time trends.[17]

It is possible that changes in the penalty rate were related to other policies or previous economic conditions in a particular locality, and the specific locality could contribute to human-capital accumulation and economic development. For example, the ambitious officials who raised the fines may also have been aggressive in implementing other policies, such as compulsory-schooling laws and pension programs. As emphasized by the Eighth Five-Year Plan, economic growth and poverty reduction were among the promotion factors. Because our measure of the OCP is at the province-year level, the potential endogeneity should originate from province-year-level confounding factors. To examine the potential endogeneity, Huang, Lei, and Sun (2021) calculate the correlation between fines and twenty-eight province-year-level macroeconomic indices in four broad categories.[18] For each province-year-level macroeconomic index, the authors examine whether its average levels in the previous one, two, and three years are correlated with fines. They also test whether the changes in fines are correlated with the changes in these indices. Their results show that none of the indicators are significantly correlated with the penalty levels or changes at the 10% level.

However, their results only suggest that the penalty changes are insignificantly correlated with

[17] The provincial-level indices include (1) demographic variables (provincial population, urban population, rural population, birth rate, and death rate); (2) macroeconomic indexes (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).

[18] The four sets of factors are (1) demographic variables, including population size and birth and death rates; (2) basic economic conditions, such as employment, wages, GDP, and the unemployment rate; (3) government programs and expenditure, including numbers of pension-program and health-insurance participants; and (4) social services, such as the number of hospitals and educational resources (measured by the number of teachers). These factors are found to possibly influence economic outcomes. See details in Section 2.A of Huang, Lei, and Sun (2021).

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

### 2.3. Intergenerational mobility and one-child families

Higher intergenerational mobility generally increases equality and productivity (Bell et al., 2019). It also stabilizes society (Lipset and Bendix, 2018).

To increase their children's capacity to survive and maximize their incomes, educational levels, and social standing, traditional Chinese parents work hard, save money, and develop and widen their networks. In addition, older children are made accountable for nurturing younger ones (Diamant, 2000; Lewis, 2008). As a result, in some families most members pursue the same occupations, get comparable levels of schooling, and advance in specific hierarchies (Lorge, 2000). These families frequently connect with each other by marriage to build communities of interest (Holmgren, 1982). Persistent close relationships both inside and between families were woven into strong networks in order to help family members climb the socioeconomic ladder. The alliances between families are likely to contribute to a society with low intergenerational mobility: a child who inherited wealth and strong connections tends to attain a relatively high place in a hierarchy, while their counterpart from a low-income family or with weak social ties finds it difficult to compete with them.[19]

The OCP may have altered the big-family culture and resulted in a grand-scale socioeconomic reshuffling. In support of this conjecture, we find that a child born after 1979, when the OCP was instituted, has lower intergenerational persistence in income, education, and social class than a

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

child born before 1979 (Table A3).[20] On average, if their parents' income ranks 1 percentile higher in their cohort, a child born after 1979 is expected to rank 0.258 percentiles higher (Panel A, Column 2) while a child born before 1979 is expected to rank 0.513 percentiles higher (Panel A, Column 1). Similar patterns are also found in education (Panel B) and social class (Panel C).

## 3. Data

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

### 3.1. Data sources

We constructed our data set from ten national household-survey data sets (Table A4): China Family Panel Studies, Chinese General Social Survey, China Health and Retirement Longitudinal Study, Chinese Household Finance Survey, Chinese Household Income Project, China Health and Nutrition Study, China Labor-Force Dynamics Survey, Chinese Social Survey, National Population Census of China (Long Form database), and the Urban household survey. 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).[21]

The data set generated by combining these surveys is significantly larger than the data sets 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 data sets (Zou, 2015). Importantly, this larger data set enables us to rank the outcome by cohorts of children and parents

[20] 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. Intergenerational persistence measures how much a child's socioeconomic outcomes are influenced by those of their 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 noncardinal variables (for example, 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.

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

and attenuates life cycle bias.[22] Additionally, longer observation periods (decades) enable us to present a comprehensive and systematic pattern of intergenerational-mobility change in China. Given its broadened scope and greater informational value, the data set itself thus constitutes a significant contribution to future research on intergenerational mobility in China.

### 3.2. Construction of the main variables

The three major dependent variables that we focus on are the intergenerational persistence of income, education, and social class. Intergenerational income persistence is the most commonly studied variable in the 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, more stable attribute that does not fluctuate once achieved, whereas income is subject to greater fluctuations (Feigenbaum, 2018). We also created an intergenerational-class-persistence variable by synthetically measuring social status based on both administrative and wealth levels. These three types of intergenerational persistence can be cross validated, 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, and monetary subsidies 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-value gains, and capital gains.

We measure years of education as the number of years spent in school until an individual gains their highest graduation certificate, excluding the number of years spent in school without earning a certificate. 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 (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

[22] The intergenerational-persistence measurement method of child-parent rank-rank effectively reduces the life cycle bias. Results are improved by larger amount of data and controlling for parents' age (Chetty et al., 2014; Emran and Shilpi, 2019).

positions at the deputy bureau-director (*fu ting ju ji*) level and above to high class, those at the division-director (*xian chu ji*) or deputy division-director (*fu xian chu ji*) level to middle-high class, and those at the section-director (*xiang ke ji*) or deputy section-director (*fu xiang ke ji*) level 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 to middle-lower class, and lower than the sample median income to 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 data set, we first identify each possible parent(s)-child pair from the pool of respondents and their spouse, parents, children, or other close relatives who are required to provide information.

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

To reduce life cycle bias, we rank within each cohort to obtain quantiles on outcomes for both parents and children (Chetty et al., 2014; Emran and Shilpi, 2019).[23] If both parents are present, we take the average rank of the parents.

For sample age range, we remove observations for individuals under twenty-two 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.

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

To quantify OCP intensity across the country in order to identify policy effects, we associate each observation with data detailing the average monetary-penalty rate for one authorized birth at the province-year level; the fine rate is formulated in multiples of annual income (Gu et al., 2007; Ebenstein, 2010; Wei and Zhang, 2011). Since human gestation is approximately nine months, parents' decision to have a child is made close to a year in advance.[24] Therefore, we construct the variable $Fine$, which is the OCP fine rate a year before the birth year in a given province, and match the data of this variable with the combined data set to assign a value for each observation, thus incorporating a metric for OCP intensity into the data set.[25] The effective fine rate was 0 for children born before 1979, when the OCP was started. We exclude children born in 1979 because it is a transitional year and provinces launched the OCP at different times during this year. We also exclude children born after 1996 to ensure that individuals living in the sample years completed their education and were able to work.

### 3.3. Statistical summary

After cleaning the data, the final data set consists of 2,096,978 child-parent(s) pairs, for which the summary statistics are shown in Table A5. The average age of children is 35.91 years old. The birth years span from 1949 to 1996 (Table A6), and the survey years from 1986 to 2019 (Table A7). The data sets from the China Health and Nutrition Study, NHS, and National Population Census of China 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 conduct a robustness test with a gender-balanced subsample (Section 5.2.4); 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 means of 7.93, 6.76, and 5.22 years of education, respectively. Similarly, 551,236 observations of children, 357,293 observations of fathers, and

[24] See details on the fine-rate construction in Appendix Section 3 of Ebenstein (2010).
[25] It is possible that parents need more than one year to decide whether to have a child. We thus perform a robustness check using the average fine rate of three years prior to birth. Details can be found in Section 5.2.3.

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. Given that 28.3% children come from one-child families and 71.7% from multichild families (with an average of 3.0 children), there are three times as many children from multichild families as from one-child families; the number of one-child families and the number of multichild families is roughly equal, at 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%.[26]

## 4. Empirical methodology

We employ the difference-in-differences (DID) method to estimate the effects of the OCP on intergenerational persistence. We use the average monetary penalty for one unauthorized birth (the OCP fine) in the province-year panel from 1979 to 2000 as the proxy of OCP strength. The OCP fine is formulated in multiples of annual income (Gu et al., 2007; Ebenstein 2010).[27] The mathematical expression is as follows:

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

The subscript $i$ stands for child $i$; $p$ for the province where child *i* was born; $h$ for child *i*'s cohort, or birth year; $d$ for survey source; and $t$ for survey year. $Rank_{iht}$ is child $i$'s rank in income, education, and social class within their cohort $h$ in year $t$; $Rank_{ift}$ is the parents' rank within the parents' cohort $f$ in year $t$. $Fine_{hp}$ is the amount of the fine in child $i$'s birth province $p$ one year before the child was born in year $h$.[28] This variable captures key elements of the variation in the strictness of the OCP (Huang, Lei, and Zhao, 2015; Huang and Zhou, 2015; Huang, Lei, and Sun, 2021). It remains at 0 prior to 1979, varies across provinces after 1979, and adjusts with revisions of local OCP rules. We exclude 1979-cohort data in the DID analysis because 1979 was a transition year when the OCP was being formed and fertility decisions were being adjusted.

[26] 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.
[27] See Ebenstein (2010, sec. 3) for the details of the construction of the fine-rate data. See also Section 2.2.
[28] See details in Section 3.2 about the construction of $Fine_{hp}$.

Control variables, $X_i$, include child $i$'s gender, marital status (whether child $i$ is married or in de facto union or not), and whether only one parent is present. We include $\omega_h$ , $\tau_t$ , $\varphi_d$, and $\lambda_p$, which stand for cohort, year, data set, and province fixed effects, in order to control for all time-invariant differences between cohorts and provinces and changes over time that affect all cohorts and provinces similarly. Finally, we cluster standard errors at the provincial level in our main analysis and report alternative standard errors such as those using the two-way clustering of province and survey-source standard errors in Table A8.[29]

We focus on the key coefficient, $\beta$, which shows the effect of the OCP on intergenerational persistence in income, education, and social class. We select four alternative time periods to observe the focal effect: 1976–82, 1974–84, 1969–89, and 1949–96 cohorts (the 1979 cohort being excluded)—that is, three, five, and ten years before and after the initial implementation of OCP in 1979 and all sample cohorts.

Our DID identification strategy relies on the parallel-trends 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 event-study method to examine the link between fines and intergenerational persistence year by year to determine whether different intergenerational-persistence trends already existed because fines across the country prior to the OCP differed. The specification is as follows:

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

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

[Figure 1 approximately here]

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

[29] The results are robust to the different standard-error calculation methods.

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 cohorts, suggesting that the OCP reduces intergenerational persistence (Table 1).

The results of the regression show that for observations on the 1980–82 versus 1976–78 cohorts (three years before and after the OCP was instituted), the interaction term of income and fine has a coefficient of −0.095. This coefficient indicates that the OCP decreases the child-parent rank-rank intergenerational income persistence by 9.5 percentage points for every fine equal to the annual income that a parent earns. Furthermore, the coefficient remains significant and negative for other time windows. Results show that the OCP decreases the intergenerational persistence rate by 9.0 percentage points for every fine equal to the annual income that a parent earns for the 1980–84 versus 1974–78 cohorts (five years before and after the OCP), by 10.5 percentage points for the 1980–89 versus 1969–78 cohorts (ten years before and after the OCP), and by 13.3 percentage points for the 1980–96 versus 1949–78 cohorts (all sample years) (Column 1).

The OCP also shows a similar strongly negative effect on intergenerational persistence in education and social class. Regarding intergenerational education persistence, the OCP decreases it by 15.5 percentage points for the 1980–82 versus 1976–78 cohorts, by 15.5 percentage points for the 1980–84 versus 1974–78 cohorts, by 16.3 percentage points for the 1980–89 versus 1969–78 cohorts, and by 13.5 percentage points for the 1980–96 versus 1949–78 cohorts (Column 2).

Regarding intergenerational social-class persistence, the OCP decreases it by 8.2 percentage points for the 1980–82 versus 1976–78 cohorts, by 8.6 percentage points for the 1980–84 versus 1974–78 cohorts, by 10.9 percentage points for the 1980–89 versus 1969–78 cohorts, and by 14.4 percentage points for the 1980–96 versus 1949–78 cohorts (Column 3).

[Table 1 approximately here]

We further calculate an average total OCP effect on intergenerational persistence for cohorts born between 1980 and 2000 (Table A10). The calculation is based on the population-weighted fine for cohorts born between 1980 and 1996 multiplied by the baseline estimate of the elasticity of the one-year-income-equivalent fine (we use the estimations of three years before and after OCP implementation as the baseline here; Table 1's Row 1, Column 1; Row 1, Column 2; and Row 1, Column 3). If we make the strong assumption that the marginal effect of the OCP consistently exists for each year, then for the 1980–96 cohorts nationwide, the average fine is equal to 1.519 annual incomes, and the OCP decreases the intergenerational persistence in income, education, and social class by 14.4 (Column 2), 23.5 (Column 4), and 12.5 (Column 6) percentage points, respectively. In terms of relative impact, the OCP reduces intergenerational income, education, and social-class persistence by 28.1% (Column 3), 48.7% (Column 5), and 24.8% (Column 7), respectively, based on estimates of intergenerational persistence prior to 1979 (Table A3, Column 1).

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

Since our sample is large and covers numerous decades across the country, we study the heterogeneity of the OCP's effect by gender, location, geographical distribution, and parents' rankings. Results show that the OCP consistently reduces intergenerational persistence in income, education, and social class in each subsample (male children versus female children; urban residents versus rural dwellers; individuals dwelling in the eastern, central, or western region) (Tables A23–A25).[30]

### 5.2. Robustness checks

We conduct robustness tests on identification assumptions, an alternative explanation involving a contemporary human-capital policy, alternative measurements, and external validity.

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

### 5.2.1. Identification assumptions

We conduct four robustness tests regarding the parallel-trends assumption for DID estimations. First, while pre-trend tests are passed, we test whether the prior birth control policy—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 years in which the Family Planning Leading Group was established in each province as the proxy for LLF (Chen and Fang, 2021). We employ a triple-difference method with the 1971–75 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 interaction term LLF × average OCP fine rates × parent rank in income/education/social class are nonsignificant (Table A11).[31] This result indicates that the LLF had no effect on intergenerational persistence before the implementation of OCP.

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

Third, for two-way fixed-effects estimators with multiple 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 with income, education, or social class (Table A13).

Finally, the OCP may have a spillover effect on cohorts born before 1979. Although not

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

restricted by the OCP when born, these children may have no more siblings or may become the only child in the family even if their parents are willing to have more children, similarly to the situation of children born after 1979; the magnitude of the OCP effect could therefore be underestimated. To avoid the possible impact of spillover, we exclude child-parent pairs with the mother younger than the ninetieth percentile for gestational age. Results show a consistent negative coefficient as the baseline but with the same or smaller magnitude, indicating that a spillover effect does not exist (Table A14).

### 5.2.2. Alternative explanation

We explore the influence of a potential alternative interpretation: human-capital accumulation, a factor that might modify intergenerational persistence. We use China's university reenrollment 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 also generate 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 coefficients of the OCP and parent rank are still significant and of the same magnitude as the baseline, indicating that this contemporary policy does not affect the OCP's impact (Table A15).

### 5.2.3. Alternative measures

As another alternative, we change the measures of the two key independent variables—parent 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. The results are consistent with the key findings of baseline regressions (Table A16).

In another test, we replace the yearly fine rate with a variable representing the three-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, similarly to the fine in the current year. Our findings are similar to the baseline, supporting the hypothesis that the OCP decreases intergenerational persistence and has similar effects across different groups (Table A17).

#### 5.2.4. External validity for different data sets

We explore the external validity of our findings by using different data sets to test whether the main effects consistently exist.

First, we remove data from three gender-imbalanced surveys (the census, Chinese Household Income Project, and China Health and Nutrition Study) and rerun the regressions using data from the remaining, generally gender-balanced surveys to address the issue that observations include more men and boys than women and girls in the main sample (see Section 3.3). The demographic characteristics of the control groups (before OCP) and the treatment groups (after OCP) are largely equivalent excepting gender, according to the summary statistics of the DID groups (Table A18). The early cohorts have a greater male percentage, since three surveys (census, Chinese Household Income Project, and China Health and Nutrition Study) only have the key information for predominantly male household heads. Therefore, this robustness test excludes these three surveys in order to keep the male percentage comparable to the female percentage. Results remain the same, supporting our key finding that the OCP reduces intergenerational persistence (Table A18).

Second, we rerun the regressions with data sets that provide details for coresidents. 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 coresidents (Xie and Hu 2014; Xie and Lu 2015). Coresidency is present in the census, Chinese General Social Survey, Chinese Household Income Project, and Urban household survey data; however, other national surveys that we use in this analysis, including China Family Panel Studies, China Health and Retirement Longitudinal Study, Chinese Household Finance Survey, China Health and Nutrition Study, and China Labor-Force Dynamics Survey, address the issue of missing coresident data by including questions on all family members, whether they were coresidents or not (Xie and Hu 2014; Xie and Lu 2015; Emran and Shilpi 2019). To evaluate the impact of coresidency, we therefore establish a subsample that does not include information from the first four data sources.

Coresidency has little bearing on our main themes, which are consistent with our baseline findings (Table A19).

Third, we divide data into subsamples based on whether parents experienced the Great Famine (1958–61) before giving birth. Starvation experience can affect an individual's fertility willingness and behavior (Shi, 2021), making the control group and the treatment group lack comparability.[32] In both subsamples, the impact of the OCP on intergenerational persistence in income, education, and social class consistently remains negative, suggesting that parents' famine experience does not impact the main effect of the OCP (Table A20).

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

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

### 5.2.5. Heterogeneity in the One-Child Policy's effect

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

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

[32] China's Great Famine was brought on by an overestimation of food production (Becker 1996; Thaxton 2008). For fear of not meeting the production goals set by the central government, local cadres faked their grain output figures and submitted inflated totals to the central government, resulting in a severe grain shortage. The death toll from the famine was massive, with estimates ranging from 16.5 to 30 million, and with many of those deaths concentrated in infants and young children. The famine also caused China's fertility rate to plummet (Ashton et al. 1992; Chang and Wen 1997; Li and Yang 2005).

intergenerational persistence, those in the central region the second highest, and those in the western region the lowest (Table A25).

## 6. Mechanisms

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

### 6.1. The weakening of elite-family heirship

We first explore whether the OCP changed the conventional hierarchy of elite families, rendering aristocratic-blooded 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 the conventional family heirship system could have broken traditional aristocratic rule, triggering a major socioeconomic reshuffling in Chinese society.

The traditional political-elite family used to have many children so that a child 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 likely resulted in a reduction in their resources and the number of jobs that they could hold.[33] We use a difference-in-difference-in-differences (DDD) regression to test the hypothesis that the OCP reduced the number of children in elite families:

$$Child\_number_{iht} = \beta \times Fine_{hp} \times Parent\_level_{iht} \times I(h > 1979) +$$
$$\beta_l Parent\_high\_level_{iht} + \beta_f Fine_{hp} \times I(h > 1979) + G_i + \tau_t + \lambda_p + \omega_h + \varphi_d + \sigma_{iht}$$

[33] 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.

(17)

Here, $Child\_number_{iht}$ 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 to 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 whether elite officials have different child populations is $\beta$, the coefficient of the interaction term between OCP intensity and whether a parent is an elite official. Results show a negative value of $\beta$, −0.680 (Table 2, Column 1), which means that the number of children of elite officials dropped by 0.680 for every fine equal to annual income, compared to those who were not elite officials. This further suggests that elite officials were more compliant with the OCP and had fewer children.

[Table 2 approximately here]

Elite families in China 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 reduced assortative mating using the following series of DDD regressions:

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

Here, $rank_{iht}$ 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). $\beta$ is the crucial coefficient showing how the OCP influenced assortative mating.

Results show that the coefficients for income (−0.095; Table 2, Column 1), education (−0.061; Table 2, Column 2), and social class (−0.030; Table 2, Column 3) are significant and negative, indicating that an OCP fine equal to one year's income decreased spouse income, education, or social-class rank by 9.5, 6.1, or 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.

### 6.2. Resource concentration in poor families

As 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 the OCP had a greater impact on poor families' capacity to have children than on wealthy ones.

Children from poor families are thus likely to have received greater shares of their family's resources than they otherwise would have because they had fewer or no siblings. This beneficial effect also included nonfinancial advantages such as companionship and parentally taught noncognitive skills (Carneiro and Heckman, 2003; Cuhna and Heckman, 2007). Therefore, the OCP may have indirectly supported 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 the OCP on wealthy and poor families in order to empirically test for the presence of this channel. 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 noncognitive skills (self-efficacy, ambition, and trust in people, all measured by standardizing the corresponding sum of scale scores). The specification is as follows:

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

Here, $Poor_{ift}$ 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 had a greater impact on the number of children in lower-income families than in higher-income families. According to the coefficient, each fine equal to one year's income resulted 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 for higher-income families.[34] 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 increment in the OCP fine equivalent to a year of income. The OCP also improved the cognitive skills of children from lower-income families. Children from lower-income households showed 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 OCP fine equivalent to a year of income (Column 6).

[Table 3 approximately here]

In summary, our findings support the hypothesis that the OCP concentrated lower-income families' resources by reducing their number of children. 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, confirming Proposition 3.

### 6.3. The decrease in returns to education

As detailed in Proposition 4, intergenerational persistence is predicted to have dropped because returns to education fell. 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 grows (Bütikofer, Dalla-Zuanna, and Salvanes, 2018).

Therefore, we investigate whether the OCP lowered returns to education. The OCP reduced 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 have reduced the wage gap between skilled and unskilled workers. To test this hypothesis, we employ similar DDD regressions to Equations

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

(17)–(19):

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

Here, $ln\,(income)_{iht}$ is the logarithm of income, and we use the education dummy variable, $education\_dummy_{iht}$, to represent individual $i$'s education level, including whether individual $i$ completed junior high school, high school, or college. The remaining variables are the same as those in Equations (17)–(19). $\beta$ 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 lowered 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 returns-to-education channel.

[Table 4 approximately here]

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

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

## 7. Conclusion

This study developed a theoretical model to explain how the OCP affected intergenerational persistence of income, education, and social class and then used the DID method to estimate its

causal impacts empirically. We found that the OCP reduced intergenerational persistence. In addition, we presented three channels—which correspond to the theoretical model—through which the OCP might have had that effect. The first channel is the breakdown of the conventional blooded aristocracy by weakening elite-family heirship through reducing the number of children and assortative mating. The second channel is the concentration of family resources on a single child, which enabled 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 affected intergenerational persistence for all children, whether or not their parents complied with the policy. We found empirical evidence for all these channels.

Although the results suggest that the OCP increased intergenerational mobility and may thus have represented a nonviolent alternative to bloody revolutions and regime changes—which had formed a millennia-long cycle in Chinese history—this does not imply that the OCP was good, all things considered. The long-run consequences of the OCP still need to be carefully studied.

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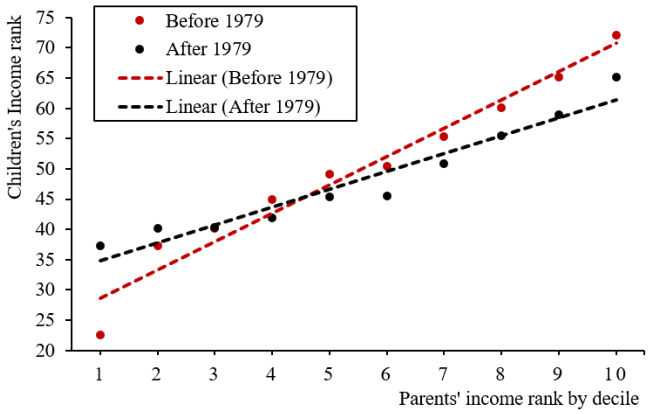
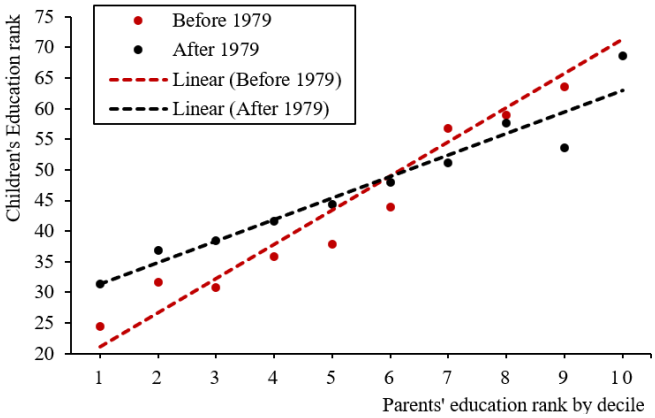
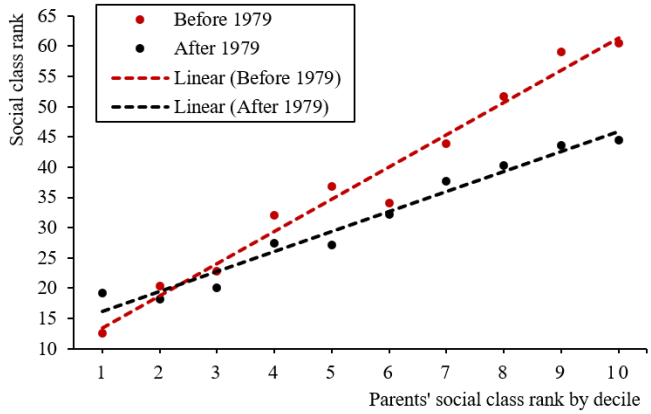



**(a) Income Rank**




**(b) Education Rank**

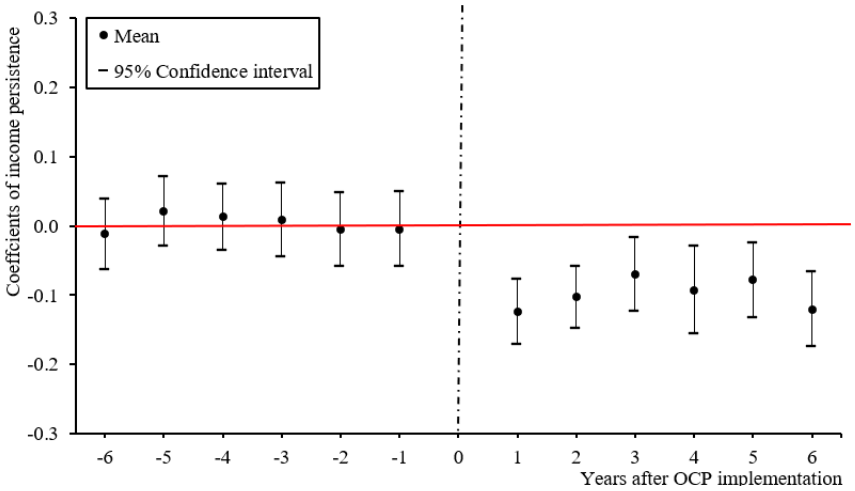



**(c) Social-Class Rank**

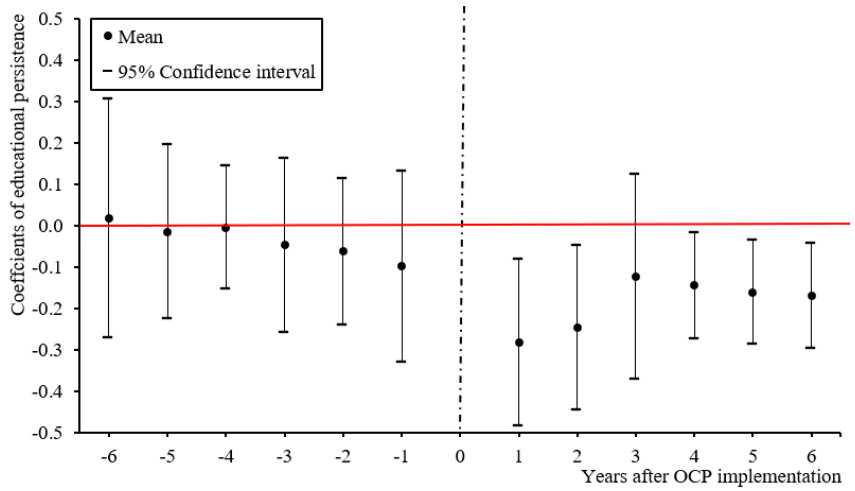
**Figure 1. Intergenerational Mobility before and after One-Child Policy's Implementation in 1979**

*Note:* (a), (b), and (c) visualize the dynamic effects of OCP fines on income, education, and social class. Y-axes are children's income, education, and social-class ranking by percentile; X-axes are parents' income, education, and social-class ranking by decile. The dashed lines are linear predictions of the relation, where steeper slopes stand for

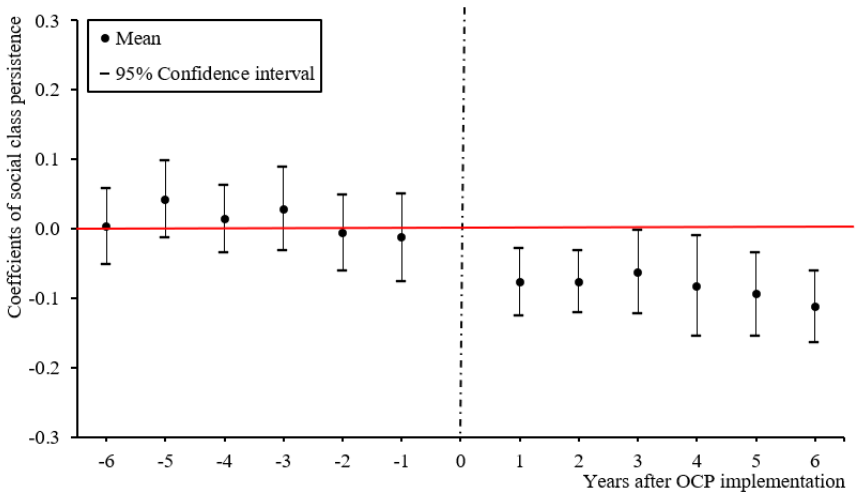
higher intergenerational persistence (lower intergenerational mobility). Red dots and lines are before OCP implementation (in 1979), while black dots and lines are after OCP implementation.




**(a) Relative Income Persistence**




**(b) Relative Educational Persistence**



**(c) Relative Social-Class Persistence**

**Figure 2. The Dynamic Impacts of One-Child Policy Fines on Intergenerational Persistence**

*Note:* (a), (b), and (c) visualize the dynamic effects of OCP fines on income, education, and social-class persistence respectively for 1973–85 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's Effect on Relative Persistence**

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

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

**Table 2. One-Child Policy's Effect on Amplifying Family Power: Assortative Mating and Effect on Elite Officials**

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

*Note:* Each cell in this table reports the coefficient from a DID regression of OCP intensity (fine rate) on different outcome variables. The dependent variables are rank of spouse income, education, social class, and number of children. All regressions control 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 levels of 1%, 5%, and 10%, respectively.

**Table 3. One-Child Policy's Effect on Poor versus Rich Families: Number of Siblings, Birth Age, Wealth Transmission, and Noncognitive Skills**

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

*Note:* Each cell in this table reports the coefficient from a DID regression of OCP intensity (fine rate) on different outcome variables between poor (the lower half) and rich families. The dependent variables are sibling number, logarithms of marriage gift from parents and educational investment from parents, and the quantiles of noncognitive skills: self-efficacy, ambition, and trust. All regressions are controlled for survey-year fixed effects (FE), province FE, data set 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 levels of 1%, 5%, and 10%, respectively.

**Table 4. One-Child Policy's Effect on Returns to Education**

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

*Note:* Each cell in this table reports the coefficient from a DID regression of OCP intensity (fine rate) effect on the return of education on income. We use 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 control 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 levels of 1%, 5%, and 10%, respectively.