ESSAYS ON ECONOMIC AND EDUCATION REFORMS IN DEVELOPING COUNTRIES

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ABSTRACT

Economic restructuring changes the distribution of labor earnings and family incomes, with implications for schooling and future incomes. The potential magnitude of the schooling effect is examined in the first chapter for a major economic restructuring, China's state-owned enterprises (SOEs) reform. This study exploits variations in children's exposure to the policy change and in the shock intensity across sectors where these children's parents initially worked. The empirical results show that the shock enlarged the earning gap between SOEs and non-SOE public organizations (hereinafter referred to as non-SOEs) and reduced SOE children's high school and college attendance, compared with their non-SOE peers.

The second chapter examines to what extent and how upper secondary education affects sex selection. To study this link, I exploit an event that disrupted the normal operation of high school education during China's Cultural Revolution. Urban high schools in China were suspended for two years from 1966 to 1968. When the high school reopened, the schooling years were shortened from 3 to 2 years and the education quality plunged. I examine the impact of school closure and degradation in education quality on sex selection with the Regression Kink Design. The empirical results show that lacking good-quality education significantly increases future sex selection.

In the third chapter, I investigate whether parents' education affects their children's early childhood skill acquisition. To identify a causal effect, I exploit the introduction of the CSL as an instrument for parents' education. I find that the CSL considerably increased years of schooling and cognitive skills, and reduced incidences of teenage birth. I also find more years of mother's education lower children's age of developing their skills of walking, speaking, counting, and getting independent, while more years of father's education are associated with children's earlier age to develop skills of speaking and independence. Further investigation suggests that the effect of parents' education is more likely to work through its impact on parents' nurturing style than on other channels such as the changes of family income or fertility pattern.

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Chapter I Economic Restructuring and Children's Education

1 Introduction

Economic restructuring is frequently seen in the economic history, sometimes as a result of general development trends – such as globalization and technological progress, or caused by more purposeful actions – such as government-initiated reforms or updates of certain industry-specific policies. While the relocation of resources induced by economic restructuring often increases economic productivities, many studies have explored the side effects of economic restructuring, such as job separations and rising income inequality; these studies share a focus on its impact on the immediate generation being affected.[1] Yet, research is yet to be done to investigate whether economic restructuring has an impact on the next generation. This issue is crucial because policy makers need to be aware of the possible intergenerational cost of economic restructuring in order to respond through social protection policies, or, to fully evaluate existing policies that cause economic restructuring for future references. This chapter seeks to address this issue by examining the impact of economic restructuring on the next generation's educational attainment. While it seems natural to presume a relationship between economic restructuring and children's education, as restructuring can result in significant income redistribution and earning changes, the specific effect is to some extent a priori ambiguous and needs to be investigated empirically.[2]

In this chapter, I study the impact of economic restructuring on children's educational attain-

[1] For example, Autor et al. (2014) and Walker (2013) show that the structural reallocation of labor induced by trade competition and certain industry-specific regulation can reduce the lifetime earnings of workers initially employed in the restructured industries; Keane and Prasad (2002) discuss the rising inequality associated with economic restructuring in transition economies; Autor, Katz and Krueger (1998) and Acemoglu (2002) demonstrate that the skill-biased technical change can lead to changes in the wage structure and increase income inequality.

[2] Children's education in developing countries are susceptible to negative household income shocks (Jensen, 2000), but economic restructuring is different from a simple income shock and its impact on children's education can be a priori ambiguous. Parents who lose jobs during economic restructuring may change their view towards education. For example, if they realize that better education can protect them from being laid off, they may increase investment in their children's education.

ment by exploiting China's state-owned enterprises (SOE) reform in the mid-1990s as a quasi-natural experiment. The progressive economic restructuring caused by the SOE reform had resulted in mass layoffs of around 43 million workers in urban areas from 1995 to 2001; most of them were SOE workers, although a few were laid off from other public-owned entities. While laid-off workers usually failed to find new jobs quickly and suffered drastic income drops, survivors of mass layoffs were also subjected to impacts of the reform: as SOE-associated welfare and services diminished, their disposable incomes shrank quickly as well. In contrast, employees from other public-owned entities, including the government and other public institutions such as schools and hospitals, were much less influenced. Hence, the shock greatly enlarged the income gap between SOE workers and employees of non-SOE public organizations (hereinafter referred to as non-SOEs).[3] Using data from China Urban Labor Survey 2001 (CULS2001), I compare the high school and college attendance of SOE children with those of non-SOE children, both before and after the shock, and evaluate the impact of economic restructuring on children's educational attainment using a Difference-in-Difference (DID) strategy.

China actually provides an ideal empirical setting for the use of DID, as the compositions of the control (non-SOEs) and the treatment (SOEs) groups are relatively stable over time. It's very difficult, if possible, for workers in urban areas to switch from one work unit to another before 1992. This unique feature is due to China's socialist legacy: the private sector only started to take shape in the 1980s; and when the private sector was still much underdeveloped, the labor market barely existed and urban residents were assigned to employment tenures in public-owned work units. In other words, labor mobility across firms or sectors was almost zero.

I find rigorous evidence showing that in terms of high school and college attendance, children of SOE workers – regardless of whether they were laid off or not – were significantly more adversely affected by the SOE reform than were children of non-SOE employees. I conduct a series of

[3]Throughout this chapter, I use the term "SOE" to refer to three types of enterprises with public ownership: wholly state-owned ones, majority state-owned ones, and collectively owned ones located in the urban area. The size of the private sector in urban China before the SOE reform is almost negligible compared to the public sector – see section 2.3 for more details. For this reason, this chapter mainly focuses on the comparisons between entities within the public sector, namely, SOEs and non-SOEs; the latter consist of government agencies (GOVs) and public institutions (PUBs).

robustness checks to make sure that the results are not driven by other cohort-varying confounders, such as the divergence of returns to education. I also demonstrate that other reforms implemented by Chinese government around the same time do not confound the above results.

Mass layoffs caused by economic restructuring can generate geographical externalities to children's educational attainment through two channels: father's labor market interactions and children's competitions over the limited educational resources.[4] Specifically, in areas with more laid-offs, individual unemployed workers face higher competition for new positions, which suggests lower chances of re-employment and lower equilibrium wage. And this may worsen the performance of children with fathers being laid off. On the other hand, in areas with mass layoffs, there are also more students experiencing father's job loss and family income drops, which would reduce competition for high schools and colleges and in turn would benefit anyone else's possibility of enrollment. I develop a model that incorporates labor market interaction and features of educational enrollment procedure in China to interpret the two opposing externalities.

To empirically examine the geographical externalities of economic restructuring, I use a Difference-in-Difference-in-Difference (DDD) strategy across cities, children's cohorts, and the organizations where their fathers worked. The shock intensity across cities is measured in three different ways: the pre-reform SOE share (hereinafter referred to as SOE share), the post-reform layoff share (hereinafter referred to as layoff share), and a city dummy indicating higher or lower intensity of the shock. The estimated results support a negative externality, i.e., the adverse impact of economic restructuring on education is amplified in cities with higher percentage of layoff or higher share of SOE workers. More importantly, the DDD result differences out cohort-varying unobservables, which could possibly affect parental income differentially and confound the main results.

This study is one of the first that examines the intergenerational impact of economic restructuring, and it addresses this issue in the context of a developing country where children are more susceptible to economic shocks. Several studies focusing on other developing countries have pointed out that nation-wide economic crises may reduce family budgets on children's education and in-

[4] Ananat et al. (2011), for example, discuss how the widespread changes in the income distribution could generate intergenerational spillovers due to general equilibrium considerations.

crease the dropout rate as well as the infant mortality rate(Frankenberg et al., 1999, Thomas et al., 2004, Paxson and Schady, 2005, Cameron, 2009). Unlike economic crises, which are usually unintended and can cause recessions that diminish the welfare of the whole society, economic restructuring may result from well-intended policies and only certain industries or sectors would be negatively affected. This feature of economic restructuring - as well as the unique situation in China - allows me to exploit novel sources of variations of the shock, thereby identifying the impact of economic restructuring on the next generation's educational attainment. This chapter contributes to the existing literature on economic shocks by demonstrating a causal relationship between exterior economic environment and children's development in a country where social protection program is underdeveloped. It also calls attention to the often neglected intergenerational effects of economic restructuring, which people need to recognize in order to fully evaluate current policies and respond via new policies.

This chapter builds on a growing literature examining the impact of family income change associated with parental working status and cash transfer on children's education and future life outcomes. Using Canadian data, Oreopoulos, Page and Stevens (2008) find that people tend to have lower earnings whose father lost jobs in their youth. Rege, Telle and Votruba (2011) and Bratberg, Nilsen and Vaage (2008) suggest that in Norway, parental job loss has a negative effect on children's school performance, though its impact on children's future earnings is insignificant. Ananat et al. (2011) find a short-term negative impact of state-wide job loss on children's eighth-grade math scores. One limitation of these papers using firm closure as the instrument variable for parental layoff is that firm closure may not be exogenous due to the positive assortative matching between firms and workers. By exploiting the time-of-event variation of parental layoff, Hilger (2013) shows that the impacts of father's job loss on children's college attendance and future earning are small. Though their specific settings differ and findings vary, the above studies in general show that in developed countries, parental employment status has limited impact on children's education and future earnings. This might be attributed to the existence of sound social safety net programs, which more of less alleviate the income shock caused by job loss. Two recent papers

have sought to explore the causal relationship between family income and children's education by studying various cash transfer programs. Dahl and Lochner (2012) finds a positive impact of cash transfer on children's test scores, but it is unclear whether the improvement on test scores can be translated to higher educational achievement. Aizer et al. (2014), on the other hand, shows that cash transfer received via social welfare programs does lead to children's more years of schooling.

This article contributes to this literature in that it demonstrates that income change induced by economic restructuring can have large impacts on children's education. One key feature of this research is that economic restructuring often leads to greater changes in permanent income than idiosyncratic layoff or cash transfer does, which could more profoundly affect family's decision on children's education investment. In addition, this chapter address the negative externality of economic restructuring that may exaggerate the income effect, a specific channel that has never been discussed before.

The chapter proceeds as follows. Section 2 discusses the historical background of the SOE reform and education policies in China. Section 3 gives a detailed description of the data and variables used in this chapter. Section 4 articulates the identification strategies. The estimation results and robustness checks are presented in Section 5. Section 6 explores heterogeneous effects of the economic shock and the channels through which families are affected by the shock, and it is followed by a conclusion that generalizes main findings and contributions of this study.

2 Historical Overview

2.1 Job Allocation in the Pre-Reform Era

Most of the urban jobs were distributed through government plans before 1980.[5] The local government assigned job quotas to designated schools or residential districts and specify the number of new workers needed. Given these quotas, school or residential authorities then decided how

[5]Other less common forms of employment are either through replacement, namely that children take over their parents' job after they retire, or through informal connections and recommendations.

Table I.1: Summary statistics (unweighted): SOE vs Non-SOE workers

	Mean		Diff
	SOE Workers	Non-SOE Workers	
Demographics			
Layoff	0.193	0.0910	0.102***
Age	47.87	50.02	-2.15***
Male	0.488	0.519	-0.032**
Years of Education	10.01	11.66	-1.654***
Marriage	0.855	0.870	-0.015
Height	165.3	165.6	-0.289
Lives in city before 16	0.860	0.805	0.054***
Party Member (before work)	0.138	0.234	-0.096***
Children	1.432	1.568	-0.136***
Siblings	2.038	1.787	0.251***
Brother	1.030	0.934	0.097***
Sister	1.007	0.852	0.155***
Occupations			
Farmer	0.004	0.002	0.002
Worker	0.774	0.376	0.398***
Self-employed	0.0630	0.132	-0.069***
Run a business	0.0770	0.243	-0.166***
Administrator	0.0470	0.0890	-0.043***
Technician	0.0300	0.0970	-0.067***
Observations	4557	1988	

Source: CULS2001

they were allocated to individuals (Bian, 1994*b*). Although the mechanism of job allocation at this level is not well documented, anecdotes suggest that local authorities made decisions based on their subjective assessment of the candidate's characteristics, such as gender, education, family background, preference, etc. In general, non-SOE workers are more educated and less likely to be party members than SOE workers, as shown in Table I.1.

Since workers were entitled lifetime employment and tied up to their working units once jobs assigned, the labor mobility in urban areas was extremely low throughout the 1980s. Switching jobs from one work unit to another was rare before 1992, as it often incurred considerable administrative costs, even possibly involving bribing (Bian, 1994*a*).

The centralized job assignment system in the planned era was gradually replaced by the contract system as China's market-oriented reform moved forwards. In 1986, the government introduced the contract labor system, requiring that all newly hired workers be assigned to a five-year labor contract (Naughton, 1996). Since then, the proportion of contract workers in the labor force started increasing from about 4% of total employment in 1985 to 13% in 1990 and further to 39% in 1995 (Meng, 2000). Meanwhile, the government also gradually reduced state allocation of jobs and encouraged self-employment or open recruitment by enterprises. State-assigned jobs eventually phased out of the economy after 1996.

2.2 Economic Restructuring

Prior to the opening up policy in 1978, SOEs could generate before-tax cash flows around 14 percent of GNP. This number had been shrinking since then. By 1993, SOEs conversely received support from the government more than 4 percent of GNP to maintain its expenditures (Brandt and Zhu, 2000).

The declining profit in SOEs was a joint consequence of their nature in the planned economy and China's gradualist economic reform. The permanent employment system prohibits SOEs from dismissing workers, which resulted in overstaffing and lowered workers' incentives. Additionally, SOEs are not conventional firms but multifunctional social units dedicated for a variety of social goals and obliged to provide workers with the necessities of life, such as schools, housing, health care, etc. (Lee, 2000). These extra social expenses burdened SOEs and their inherent economic inefficiency was aggravated when the market became increasingly competitive following the rise of the private sector. One of the challenges faced by SOEs came from the Township and Village Enterprises (TVEs), which experienced a period of high-speed growth in the late 1980s, benefiting from the free-up-price policy and the lower labor cost in rural areas.[6] In urban areas, the private sector also started burgeoning. These competition pressured SOEs and continued driving down their profitability (Yusuf, Nabeshima and Perkins, 2006, Naughton, 2007).

[6]TVEs were once misclassified as a form of Collectively Owned Enterprises. For a thorough discussion of the nature of TVEs, see Huang (2008).

Although many were making a loss in the early 1990s, SOEs had been strongly supported by the government through favorable loans and various forms of subsidies. The momentum of economic reform in urban areas halted shortly after the Tiananmen Square protests in 1989. The political forces at the top were divided and there was a lack of consensus on whether to continue economic reforms after this incident (Naughton, 2008). However, this situation was changed after Deng Xiaoping's southern tour in 1992, where he delivered an influential speech and successfully cleared the ideological obstacles for further reforms in the urban area. The SOE reform was officially initiated in 1994, marked by the implementation of *zhua da fang xiao* (Grasping the large, letting go of the small) policy, which permits small loss-making SOEs shut down or privatized while keeping and reforming the large SOEs. The Labor Law passed in 1994 also laid legal foundations for SOEs to dismiss no-fault workers to reduce surplus labors (Cai, Park and Zhao, 2008). These policies altogether led to mass layoffs around 43 million workers with 34 million from the state sector in the following years (Giles, Park and Cai, 2006).[7] Figure I.1 shows that the urban unemployment rate rose gradually after 1994 and quickly accelerated after 1997.[8]

Since the reform was mainly targeted on SOEs, non-SOE public organizations were mildly affected. Figure I.2 plots the fraction of unemployment incidence for the two groups of workers respectively at their given birth year. The figure shows how the unemployment incidence differs between SOE and non-SOE workers and how the difference evolves over time. Given that China's mandatory age for retirement in the 1990s was 60 for men and 50 for women, for those born before 1947, many of them had been retired or close to retirement by 1997 when the mass layoffs came up, so there is no notable difference between SOE and non-SOE workers in the unemployment incidence. For younger cohorts, however, they were at a much higher risk of being affected by the SOE reform. The bifurcation of the unemployment incidence indicates that SOE workers were

[7] Shanghai, Qinghai, and Heilongjiang launched the policy slightly earlier than others. In the robustness check, I drop Shanghai to double check whether the main results are driven by the special policy of Shanghai. The results give similar statistical inference.

[8] The registered unemployment rate reported by the official labor statistics is flawed, as it only counts the unemployed as those who registered for unemployment benefit and thereby significantly understates the true unemployment rate (Giles, Park and Zhang, 2005). For more accurate survey-based unemployment rate, see Feng, Hu and Moffitt (2015).

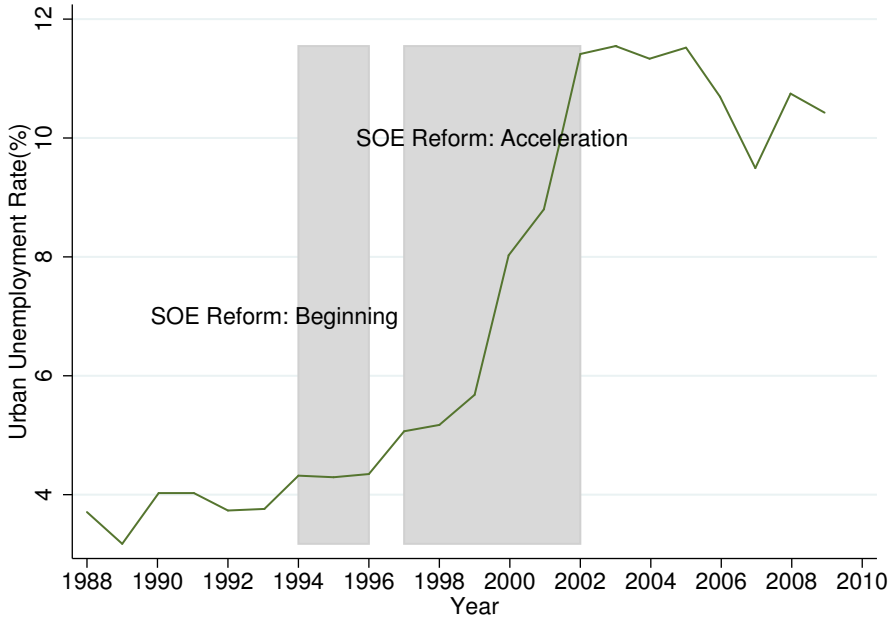


Figure I.1: Urban unemployment rate, 1988-2010

Source: This graph is adapted from , where the urban unemployment rate is calculated based on China's Urban Household Survey.

much more likely to be laid off than non-SOE ones after the reform.

A large proportion of laid-off workers failed to find a new job quickly. The data from CULS2001 show that among those who experienced unemployment during the SOE reform, only 34.8% were able to get re-employed within 12 months, and 44.7% were re-employed by 2002 (Giles, Park and Cai, 2006). There are three main unemployment compensations, i.e., the public subsidies (including *xiagang* subsidies, unemployment subsidies, and Minimum Living Standard Programme (MLSP, also known as *Dibao*) payments), the pension for the early forced retiree, and the lump-sum severance payments. In fact, these compensations played a limited role in mitigating the shock. Giles, Park and Cai (2006) find that men aged 40-55 and women aged 40-50 were covered by subsidies with more than half, whereas for other age groups, only less than half were covered. In addition, the annualized unemployment subsidy and the pension income are 607 RMB per capita and 2172 RMB per capita respectively for families with one unemployed man. As a comparison, the national average disposable income for urban residents in 2001 is 6860 RMB and the income

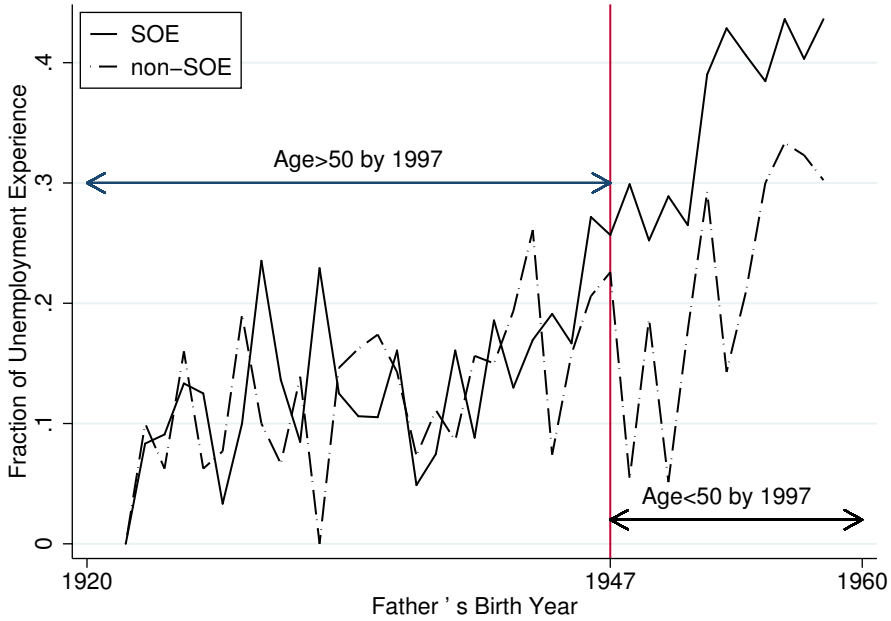


Figure I.2: Reported incidence of unemployment, by birth year, 1920-1960

Source: CULS2001. This figure shows the differential impacts of SOE reform on the unemployment probability of SOE and non-SOE workers. Unemployment experience is a dummy taking a value of one if the person has ever been unemployed at least once. The unemployment experience refers to the one's historical unemployment situations, including being laid off, involuntary retirement, registered unemployment, and being jobless and actively searching for jobs.

per capita for the household without any laid-off member in the data is 9840 RMB, which suggests that a large proportion of family income was not insured. Family members who were laid off mostly relied on their own savings or other family member's income to survive the period of hardship.

Even if workers were not unemployed, the welfare of SOE workers diminished quickly compared with the non-SOEs workers. Figure I.3 plots the evolution of the average wages in SOEs and non-SOEs throughout 1990s. The average wage gap between SOE and non-SOE workers did not emerge until the wage reform in 1994 and was enlarged after 1997 when the unemployment rate began to surge in urban areas as a result of the SOE reform. Shocks to SOE workers were also reflected from the wage arrears, reduced benefit including lost health insurance and health expenditure reimbursement, reduced pension benefits, and changes in housing benefits.

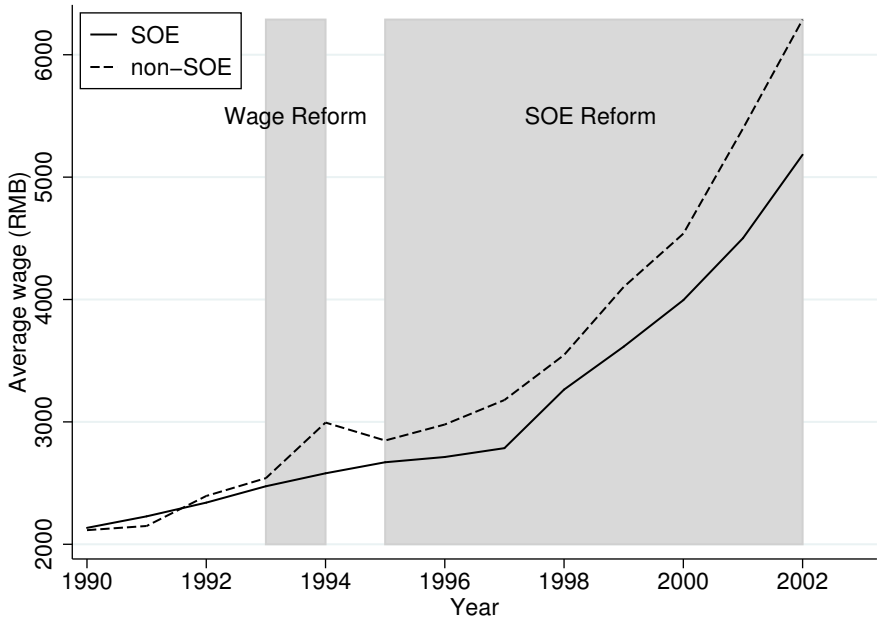


Figure I.3: Average wages, SOEs versus non-SOEs

Sources: China Statistical Yearbook 1991-2003. All wages are deflated by the officially-reported price indices in the urban area. The non-SOE wage is an average of government and public institution wage weighted by the number of workers employed by government and public institutions respectively.

Table I.2: Summary statistics of the college sample and the high school sample

	All		SOE Children		Non-SOE Children	
	Mean	SD	Mean	SD	Mean	SD
College Sample						
college attendance	0.435	0.496	0.381	0.486	0.539	0.499
Age	27.15	5.766	26.68	5.797	28.07	5.594
Gender	1.479	0.500	1.483	0.500	1.472	0.500
Siblings	1.032	1.061	0.949	1.025	1.196	1.110
Sisters	0.369	0.606	0.347	0.594	0.414	0.625
Brothers	0.361	0.582	0.330	0.536	0.422	0.659
Observations	1855		1232		623	
High School Sample						
high school attendance	0.679	0.467	0.651	0.477	0.728	0.445
Age	30.30	8.832	29.41	8.892	31.91	8.491
Gender	1.481	0.500	1.484	0.500	1.475	0.500
Siblings	1.374	1.318	1.299	1.332	1.511	1.282
Sisters	0.574	0.793	0.543	0.797	0.629	0.785
Brothers	0.575	0.739	0.533	0.715	0.652	0.775
Observations	2822		1832		990	

Source: CULS2001

2.3 School Enrollment and Schooling Cost

The secondary school enrollment in urban areas was interrupted for two years in the late 1960s because of the Cultural Revolution (1966-1976) but resumed and remained stable thereafter. Table I.2 shows that average high school attendance in five cities surveyed is around 68% in 2001.[9] The average expenditure of high school in the five cities surveyed is around 2,300 RMB according to CULS2001, as shown in Figure I.4. During the Cultural Revolution, colleges were closed for four years and the total enrollment dropped to the historical minimum level. Colleges were back to normal after 1977 and the enrollment had gradually increased since then. College tuition was waived for all students before 1992, but rose up quickly in later years. Figure I.4 shows that the average expenditure of college is almost as twice as that of high school in 2001.

[9]This number is close to 12% in rural areas based on the rural survey of Chinese Household Income Project 2002(CHIP2002)

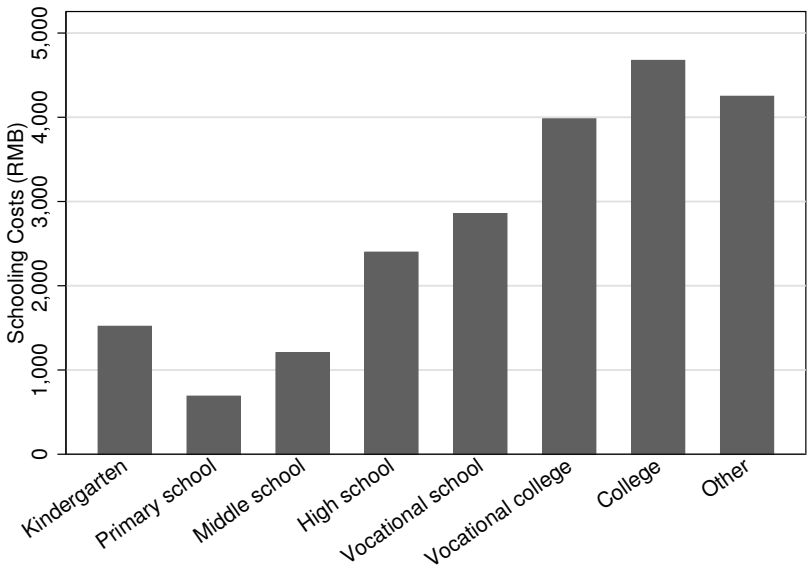


Figure I.4: Schooling expenditure per capita at various educational stages

Source: CULS2001. Schooling costs include tuition and other related expenses.

Compared to other schooling stages, the education expenses for colleges impose an especially large burden on ordinary families in China. The estimated college expenditure as a percentage of average disposable income per urban resident is around 50% by 2001, while in the US for comparison, the expenditure of private universities accounts around 38%. College students from developed countries generally have access to various forms of financial aid or can do a part-time job to partly finance their schooling cost. In China, the nationwide student loan was not initiated until 2000. Other financial instruments were largely underdeveloped. College students mainly rely on their parents to finance their college expenses.

3 Data and Descriptive Statistics

3.1 Data

The data used in this chapter come from China Urban Labor Survey 2001 (CULS2001), which is administered by the Institute of Population Studies at the Chinese Academy of Social Sciences

and is designed primarily to study the impacts of the SOE reform. The sample frame of the survey is constructed based on the 2000 census. The survey covers 3500 urban permanent resident households and around 8100 individuals aged above 16 in Fuzhou, Shanghai, Shenyang, Wuhan, and Xi'an, five provincial capitals with remarkable regional diversity and variation in economic structure.

One distinctive feature of the survey is that it traces out the detailed employment history of workers. This information is crucial for my identification strategy, as the type of organizations where workers worked before the shock is used to determine the treatment status. The survey also contains rich information on the family relationships so that each child can be uniquely linked to their parents, even if the child is not living with them. Other relevant information of the survey includes respondent's education, marriage, occupation, early life experiences, family structure, etc. Details on the sample and the construction of key variables are described below.

3.2 Measurement of the Dependent Variable

The CULS2001 asks each individual's children's educational attainment and current schooling status. The measures of educational attainment used in this chapter are dummies of children's college and high school attendance.[10] I choose these two measures instead of the years of schooling for the following reasons. First, many children were still in school and their ultimate years of schooling could not be observed from the survey when it was conducted in 2001. Yet, I include them in the sample, because whether they were currently attending college or high school is known. Second, the compulsory schooling law promulgated in China in 1986 stipulates that children must at least be enrolled in middle school, so there are fewer variations of educational attainment at middle school level or lower.

[10]High schools only refer to the academic high schools and do not include vocational schools or other post-junior educational institutions.

3.3 Sample and Descriptive Statistics

The original children's sample is reshaped into two subsamples, the college sample, and the high school sample. The two subsamples differ in children's cohorts included and are used to study the impact of economic restructuring on college and high school attendance respectively. The youngest cohort is 18 years old in the college sample and 16 years old in the high school sample, the ages at which whether they attended or were attending college and high school can be observed. The oldest cohort was born in 1954 in the high school sample and in 1964 in the college sample. The reason why the oldest cohort is younger in the college sample is that those born between 1954 and 1964 experienced the closure of college during the Cultural Revolution (1966-1976), which may distort the pre-trends of their college attainment. To be cautious, children in those age groups are excluded from the college sample.

Table I.1 reports the summary statistics for the workers in the individual survey. Unsurprisingly, workers employed in SOE before 1992 are different from those with those employed in non-SOES among a number of characteristics. SOE workers are younger, less educated, and more likely to be party members before work. Table I.2 describes the high school sample and the college sample in the main analysis respectively. Children with fathers initially employed in SOEs are also younger and less educated. These observations suggest that the distribution of labor across sectors is not random and a simple comparison between these two groups may lead to biased results.

4 Empirical Strategy

In the first part of this section, I discuss the DID method in detail. Following that, I introduce a simple model to illustrate the geographical externality of the economic restructuring shock, and then empirically test the hypotheses of externality predicted by the model using a DDD method.

4.1 Economic Restructuring and Children's Educational Attainment

Using the DID strategy, I exploit two sources of variations of the economic restructuring shock. The first one is children's childhood exposure to the shock over cohorts. In the analysis of college attendance, the post-shock group consists of children born between 1980 and 1983, who were about to entering college in 1 to 4 years when the urban unemployment rate started to accelerate in 1997. In the high school analysis, the corresponding post-shock group consists of children who are between 12 and 16 years old in 1997. These are children whose college or high school education is most susceptible to the economic restructuring shock in the sample. Since the number of observations within each year group is small, I cluster children into cohort groups to lessen the sample variance. That results in five groups of cohorts in the college sample and six groups of cohorts in the high school sample.

The shock intensity also varies across different types of economic organizations where children's father worked – SOEs and non-SOEs. As demonstrated in Figure I.2 and Figure I.3, economic restructuring was mainly targeted on SOEs. SOE workers experienced slower wage growth and higher risks of being laid off than those employed in government or public institutions. Children whose fathers initially worked in SOEs are more likely to be affected by economic restructuring. The treatment group thus includes children with fathers working in SOEs before the shock, and the control group includes those with fathers originally working in either government or public institutions.

The specification of the empirical model is the following:

$$E_{ias} = \alpha_0 + \alpha_1 SOE_{is} \times Postshock_{ia} + \rho_{ia} + \eta_{is} + \theta^J X_{ias} + \varepsilon_{ias} \tag{1}$$

where E^C_{ias} is the college attendance or high school attendance of children i, in age group a, with father employed in sector s. SOE_{is} is a dummy taking a value of one if children i's father or mother was employed in SOE before 1992. The post-shock group are children younger than the schooling age when economic restructuring started. ρ_{ia} is children's cohort fixed effect and η_{is} is sector fixed

effect. X_{ias} is a rich set of children controls, including the number of children's siblings, sisters, and brothers, and father controls, including father's education, party membership, height, occupation dummies, industry dummies, early life experiences, school ranking, school quality, etc.

The standard DID imposes a strong assumption that the treated group do not self-select into other groups after the shock. This assumption requires that the composition of the treatment and the control are relatively constant over time, but this is to some extent untenable in the case of China's SOE reform. Although it maybe difficult for SOE workers to find jobs in government agencies and public institutions, which usually require higher level of education, there could still exist considerable amount of opportunities for pre-SOE workers to switch to the rising private sector after 1992, when the urban labor market became less rigid. One way to get around this issue is to determine the treatment status by looking at father's working organizations before the shock rather than after the shock. Using data from CULS2001, I can keep track of workers' employment history and conduct an Intent-to-Treat analysis by defining the treated group as children whose father worked in SOE before 1992. The advantage of this definition is that it has accommodated the possibility of labor mobility and the loss of non-compliers induced by the post-shock sorting.[11]

The validity of DID in this research also hinges on that people do not anticipate the shock and thereby self-select into "safer sectors" before the shock. There are two reasons why this assumption is plausible in the context of China. First, it is hardly possible for the majority to anticipate before 1992 the SOE reform and its consequence. As discussed in section 2.2, there lacks consensus among top leaders as for whether to deepen the marketization reform, and major reforms were mainly undertaken in rural areas until Deng made his famous southern speech in 1992, which substantially expedited the marketization in urban China. Given the above facts, it is thus unlikely for workers to foresee a sweeping social transition and thereby to make corresponding adjustment ahead of time. Second, the labor market was rigid and the labor mobility across firms was extremely low before 1992. This is because the labor assignment was controlled by the personnel department and most positions were permanent once jobs were assigned. It is difficult and costly

[11]This definition is similar to Autor et al. (2014), who define the exposure to the trade shock as the sectors where workers were initially employed prior to the shock.

for workers to switch jobs before 1992 from SOEs to non-SOEs or the other way around.

Another concern is related to the allocation of jobs. SOE workers and non-SOE workers are different in many aspects. The former generally tend to be younger, less educated, and less likely to be party members when they started working. Although my identification strategy does not rely on the assumption of random job assignment, it does require no omitted cohort-varying and sector-specific effect correlated with the allocation of jobs. This assumption could potentially be violated, for instance, if there is mean divergence in returns to education (observable) or returns to political connection (unobservable). In both cases, we may wrongly conclude there is a negative effect even in the absence of the economic restructuring shock. I address this concern by proposing two different strategies. First, I add a variety of father pre-job-allocation demographic variables, which might predict the allocation of father's jobs, and interact them with the post-shock cohort dummy. Second, a triple-difference strategy is adopted, as described in the next subsection, to difference out any other cohort-varying and sector-specific unobservable changes.

The exclusive focus on father's job change misses an important fact that China's female labor force participation rate is particularly high compared to other countries. Although father is typically considered the main family supporter, mother's wage is also an important part of family income in urban China. For this reason, I also report the estimated impact of economic restructuring from mother's side on children's education in section 5.1.

4.2 Geographical Externality

In this section, I investigate whether the impacts of economic restructuring on children vary across cities with different economic structure. Geographically concentrated firms within the same industry can benefit from each other through within-industry externalities. However, such agglomeration economies may backfire if the local economy is struck by an industry-specific shock. GM layoffs in Metro Detroit and mine closure in resource-exhausted cities are examples, where laid-off workers found it more difficult to get re-employed locally during economic restructuring, because when the whole local economy was restructured, the unemployed from mass layoffs had to compete

against one another with similar skills at the same time, leading to either prolonged unemployment or lower equilibrium wage. In the extreme case, the local labor market could eventually become saturated to absorb any newly unemployed workers. Such negative externality can be aggravated in developing countries, where the labor supply is inelastic due to poverty, constraints of migration, or underdeveloped credit market. In these scenarios, unemployed workers often have no choice but to take the job whatever the wage is in order to survive, which could further drive down the equilibrium wage or prolong the unemployment spell.[12]

In the case of China's SOE reforms, the old industrial zones with historically high percentage of SOEs employment had a significantly larger proportion of layoffs. Children living in those cities are expected to be more severely affected by the shock because of the negative externality. In contrast, children living in Fuzhou and Xi'an, where there was less proportion of workers employed in SOEs beforehand, are expected to be less affected.

On the other hand, economic restructuring may have a positive geographical externality. The enrollment into high school and college is competitive. To be qualified, students must pass a city-wide or a nation-wide exam, i.e., the High School Entrance Exam and College Entrance Exam in China. Only those with scores above a certain threshold can be admitted and the threshold is an increasing function of the overall competitiveness of the exam. In a city with widespread layoffs, the threshold could be lower for everyone, because the exam becomes less competitive when test takers' scores are negatively affected by their parents' layoff. The lower threshold could potentially benefit everyone – even including those with fathers laid off – for a better chance of attending high school or college, and partially offset the aforementioned negative externality of economic restructuring.

In the appendix, I present a simple model formalizing the mechanisms described above. The model predicts that the impact of economic restructuring on children's college attendance is always larger in cities with larger scale of shock intensity, but such effect on high school attendance is ambiguous and depends on specific parameters including the size of the layoffs and the amount

[12]Jayachandran (2006), for instance, illustrates how those factors could exacerbate the productivity shock for the poor in rural India as a result of their inelasticity of labor supply.

of new jobs available. The intuition behind the two different results is that, in the case of college entrance exam, the competition is national not local, so the threshold of the exam is exogenously given and not a function of shock intensity in any particular city. Therefore, the aforementioned positive geographical externality does not exist. In the case of high school attendance, since both the negative and positive externality exist, the sign of the externality will depend on the magnitude of the two opposing effects, and the absolute effect of the externality ought to be smaller.

To empirically test the geographical externality of the economic shock generated by SOE reform, I estimate the following two regressions both with a triple-difference setup:

$$E_{iasc} = \alpha_0 + \alpha_1 SOE_s \times Postshock_a \times Intensity_c + \tau_{cs} + \lambda_{as} + \mu_{ac} + \theta^J X_i + \varepsilon_{iasc} \tag{2}$$

$$E_{iasc} = \beta_0 + \sum_{city=c}^{5} \beta_c SOE_s \times Postshock_a \times City_c + \tau_{cs} + \lambda_{as} + \mu_{ac} + \theta^J X_i + \varepsilon_{iasc} \tag{3}$$

where c denotes city, s sector, and a age group. E_{iasc} is child i's educational outcome. $Intensity_c$ is the shock intensity in city c and is measured in three ways. The first one is the share of workers who report ever being laid off over the last six years in each city surveyed in CULS2001. The second one is the city-wide employment share of SOE workers prior to the reform. This information is obtained from various city-level statistical yearbooks.[13] The third measure is a binary variable indicating whether the city is more severely affected by the policy shock than others.[14] $City_c$ are city dummies. The specifications include a full set of double interactions, namely city-sector(τ_{cs}), age-sector(λ_{as}), age-city(μ_{ac}), and ε_{iasc} is a random disturbance term. Other parameters and variables

[13] This information is subject to availability. All the cities in the sample report these statistics except Shanghai.

[14] I do not use the unemployment rate as the measure of the shock intensity for two reasons. First, there is a lack of a canonical unemployment rate in China that is publicly available. The urban unemployment rate published by the official labor statistics has long been blamed for not reflecting the reality, as it is not calculated based on a representative sample survey. Second, the de facto unemployment rate often fails to capture the true layoff intensity, as the unemployed may take low-paying jobs to sustain their life during the crisis. For example, Fallon and Lucas (2002) documents that during the 1997 Asian Financial Crisis, the unemployment rate in many Asian countries that experienced the crisis only mildly increased and the labor force participation rate even expanded, because the informal sector was able to absorb considerable amount of layoffs, despite large turnover in employment across formal wage jobs.

are defined in the same way as in Equation 1. α_1 is expected to be negative if the negative geographical externality dominates. β_c captures how the double differences in educational attainment vary across different cities.

Migration can be a concern if people can freely migrate to cities less affected by the SOE reform. Nevertheless, the scale of urban-urban migration in China has been rather limited by 2000, though the rural-urban migration is more common. The urban-urban migrants between 1995 and 2000 only account for less than 4% of the total urban population according to the 2000 Census.[15] Moreover, the advantage of the 2001 survey is that it was conducted shortly after the SOE reform ended. Even if workers decided to migrate out in response to the shock, they would not be able to adjust and find jobs in other cities that quickly by 2001.

5 Main Results

5.1 Impacts of Economic Restructuring on Children's Education

Table I.3 presents the results estimated with the difference-in-difference strategy. Columns 1 and 3 of Panel A show that economic restructuring reduces SOE children's likelihood of attending college and high school relative to non-SOE children. However, these results may not hold if the parallel trend assumption – which assumes that the gap between the treatment and the control group would remain the same in absence of the shock – is violated, due to the mean divergence for example. Columns 2 and 4 of Panel A present the results with the preferred specification, where I relax the parallel-trend assumption and allow for linear group-specific trends. The estimated impacts of economic restructuring on college and high school attendance are around 10% and 8% respectively. If there is the mean divergence, we would expect the effect of economic restructuring dampened after controlling for the pre-trends. To the contrary, the absolute effect on college attendance strengthens, and that on high school attendance remains. These results in-

[15]The urban-urban migrants are defined as those whose origins is the city and the out-migrating destination is another city. The percentage of urban-urban migrants is the number of urban-urban migrants last five years divided by the total amount of urban residents from the original city revealed from the 2000 Census.

Table I.3: Difference-in-Difference

DEP VARIABLES	(1) College	(2) College	(3) High School	(4) High School
Panel A: Father's Channel				
Post-shock Cohort × Father in SOE	-0.0564*	-0.111***	-0.0803***	-0.0784**
	(0.0318)	(0.0391)	(0.0180)	(0.0378)
Mean of Outcome Variable	0.4345	0.4345	0.6871	0.6871
Observations	1,855	1,855	2,822	2,822
Panel B: Mother's Channel				
Post-shock Cohort × Mother in SOE	-0.0514*	-0.111**	0.0109	0.00788
	(0.0294)	(0.0487)	(0.0538)	(0.0522)
Mean of Outcome Variable	0.4119	0.4119	0.6622	0.6622
Observations	1,964	1,964	3,200	3,200
Children and Parental Controls	Yes	Yes	Yes	Yes
Children's Cohort × Parental Job FE	No	Yes	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters). College sample includes children whose birth year ranges from 1964 to 1983, and high school sample ranges from 1956 to 1985. The post-shock cohorts in Columns 1-2 were born between 1980 and 1983, and those in Columns 3-4 were born from 1981 to 1985. Cohort fixed effect and job sector fixed effect are included in all specifications. Parental controls include the parent's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls include the number of siblings, sisters, and brothers.

dicate that the pre-trend of college attendance is more likely converging than diverging, and the parallel trend assumption roughly holds for the case of high school attendance. Figure I.5 shows a consistent pattern. To provide further evidence on the pre-trends of the two groups, I estimate a restricted DID model with discrete interaction terms in the appendix, which shows similar results. The convergence of college attendance between the treatment and the control group is probably due to the gradual college enrollment expansion after the Cultural Revolution, which might have disproportionately benefited the SOE children. The high school attendance, on the other hand, has been stable over time.

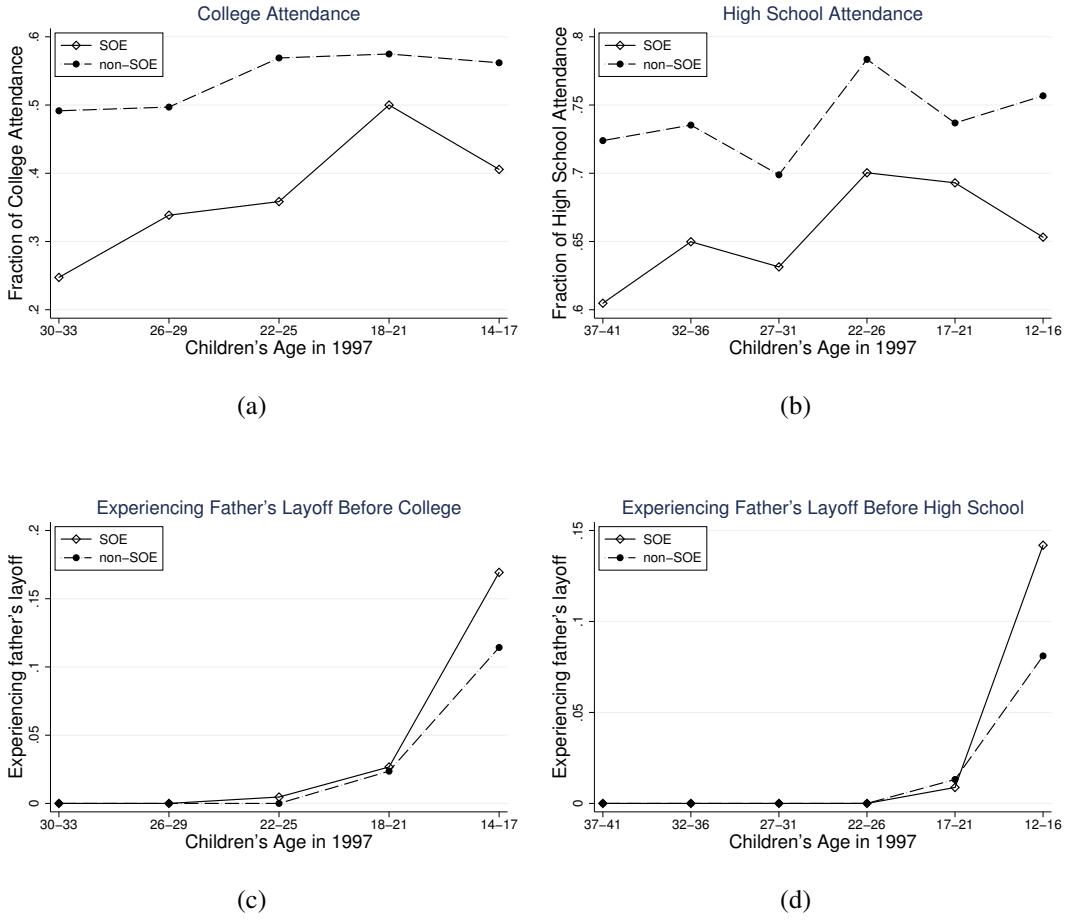
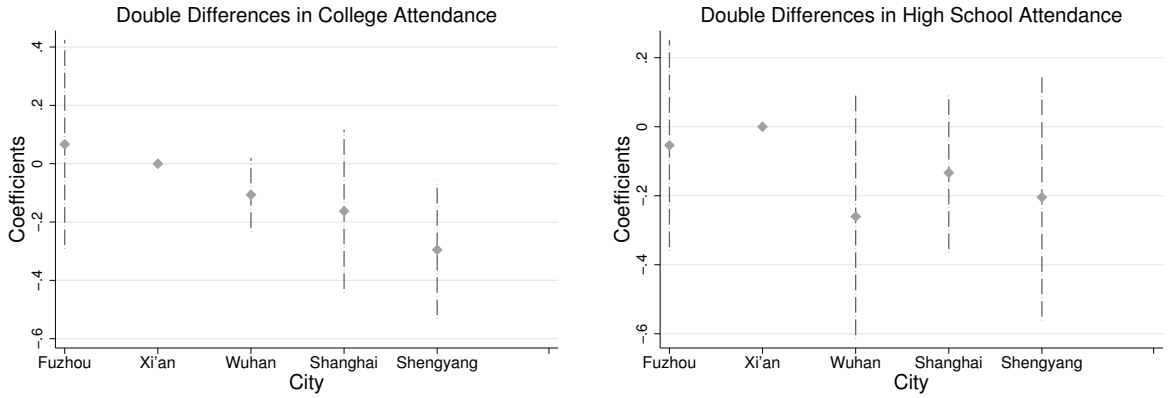


Figure I.5: Children's school attendance and experience of father's layoff (Unweighted Average)

Sources: CULS2001. Y axis in Panel (a) and (b) is the unweighted average of school attendance. Y axis in Panel (c) and (d) represents the fraction of children experiencing father's layoff in their youth.

This paper mainly focuses on the channels of father's employment, as the father is typically the main supporter of a family. Yet, since China's female labor participation rate was especially high as compared to other countries (Maurer-Fazio et al., 2011, Meng, 2012), mother's earnings may also play a role in children's education.[16] The impact of economic restructuring via mother's channel is explored in the same manner and the results are presented in Table I.3. Panel B shows that economic restructuring from mother's side has negative impacts on children's college attendance, but not so on high school attendance, indicating that mother's earnings are only relevant to children's college attendance. One probable reason is that colleges are much more costly than high schools, so children's college enrollment is sensitive to both parents' earnings change. In comparison, both the opportunity cost and the real cost of attending high school is much smaller. As a result, the economic influence from mother side plays a less significant role. For this reason, I only focus on the father's channel in the following analysis.



(a) College attendance

(b) High school attendance

Figure I.6: Triple-difference with discrete terms, by city

Source: CULS2001. Each panel plots regression coefficients and 95% confidence intervals from estimating Equation.

Figure I.6 plots the coefficients estimated from Equation 3, and shows visual evidence in line with the hypothesis of a negative geographical externality. In Panel a, each dot represents the

[16]Women's labor participation rate was particularly high in China. According to World Bank, this number is around 70% as compared to 59% in the US in 2001. In urban areas, Meng (2012) estimates that the employment rate of women was 75% in 1988, while the same number in OECD countries is 52.4%.

estimated double differences in college attendance for a given city. As can be seen from this graph, the gap in educational attainment increased less after the reform in Fuzhou and Xi'an than in Shanghai, Wuhan, and Shenyang, where both the pre-reform SOE share and post-reform layoff share are larger. This pattern indicates that children's college attendance in cities with a smaller pre-reform SOE share are less affected by the SOE reform. In Panel b, the same results rougly hold for high school attendance, but both the economic and statistical magnitudes are much smaller.

Table I.4 confirms with these results with more detailed information. As displayed in Columns 1 and 4, the impact of economic restructuring on children is larger in cities with more prevalent layoffs and a higher percentage of SOE workers prior to the reform. The rest of columns in Table I.4 present more evidence on that regard with continuous measures of shock intensity instead. Column 3 and 6 show that the impact of economic restructuring increases by 7% if the share of pre-reform SOE workers at the city level increases by 10%. Comparing Columns 1-3 to 4-6 and Panel a to Panel b, the absolute value of negative externality appears larger on college attendance than on high school attendance. This observation is consistent with the prediction of the model presented in the appendix, which states that the negative externality of mass layoffs to high school enrollment can be mitigated by its positive externality at the city level.

More importantly, the triple-difference analysis confirms with the DID results by ruling out potential confounders caused by cohort-varying unobservables.[17] For example, non-SOE workers are generally more politically connected and the return to political connection might have increased accompanying the reform. Non-SOE children could increasingly benefit from fathers' political resources allowing them to access to better education opportunities. Since the return to political connection is unobserved, the DID results are confounded in this case. However, other competing hypotheses similar to the change of political return fail to explain such regional variation shown in Table I.4.

[17] In the robustness check, I also control several cohort-varying variables that may be correlated with father's initial job allocation.

Table I.4: Triple differences (DDD)

INTENSITY MEASURE	College			High School		
	SWS	Layoff Share	SOE Share	SWS	Layoff Share	SOE Share
	(1)	(2)	(3)	(4)	(5)	(6)
Post-shock Cohort × Father in SOE	-0.208**	-0.409*	-0.737**	-0.181*	-0.290*	-0.774
× Intensity	(0.103)	(0.228)	(0.314)	(0.106)	(0.163)	(0.528)
Post-shock Cohort × Father's Job FE	Yes	Yes	Yes	Yes	Yes	Yes
City Dummy × Father's Job FE	Yes	Yes	Yes	Yes	Yes	Yes
City Dummy × Post-shock Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Mean Outcome of Variable	0.4345	0.4345	0.4345	0.6871	0.6871	0.6871
Observations	1,855	1,855	1,498	2,822	2,822	2,272

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters). Dependent variables are college enrollment and high school enrollment. The post-shock group in the college sample consists of children born from 1980 to 1983, and in high school sample born from 1981 to 1985. The college sample includes children with birth year ranging from 1964 to 1983. Children in the high school sample were born from 1956 to 1985. Layoff Share is the percentage of workers who report ever being laid off during the SOE reform. SWS is a dummy that takes a value of 1 if the city is Shenyang, Wuhan, or Shanghai, three cities with the layoff share significantly larger than others. SOE Share is the city-wide employment share of SOE workers before the shock. This information is available in city-level statistical yearbooks except for Shanghai. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls include the number of siblings, sisters, and brothers. Father and children controls are included in all specifications.

5.2 Other Reforms and Policies

The Chinese government introduced numerous policies to reform the socialist system during the mid-1990s. In this subsection, I conduct a survey on some of the notable reforms that might confound the main results.

5.2.1 Wage Reform

In the era of planned economy, urban wages in China were centrally regulated and scaled based on a number of indicators, such as regions, occupations, industries, sectors, the level of management of enterprises, characteristics of the workplace, etc. (Yueh, 2004). This old wage system became increasingly rigid after China launched its economic reform in 1978. In response to the rising competition from the private sector, the Ministry of Labor (MOL) adjusted the mechanism of SOE wages in 1985 by linking wage budgets to SOEs' profitability, in an attempt to enhance workers' incentives. Starting from 1993, the MOL issued new rules to grant more economic freedom to SOEs and public institutions – SOEs were permitted to design their own internal wage structure within the budget established by the government, and public institutions were reformed to be more market-oriented with a certain degree of freedom to set their wages as well.[18]

Figure I.3 shows that there was indeed an abnormal increase in the basic wages of non-SOEs following the reform. If the wage reform in 1993 continued increasing non-SOE wages in later years and the relative change in wages between SOEs and non-SOEs differs systematically across cities, it may pose a threat to the triple-difference strategy and confound the impact of economic restructuring with that of the wage reform. For example, if wages in non-SOEs grow much faster than SOEs in cities where the impact of economic restructuring was larger – e.g. Wuhan and Shenyang (WS), then the resulting children's educational gap between the two sectors might also increase in these cities, so the triple-difference analysis would fail to disentangle the impact of the two reforms.

To examine whether the alternative hypothesis holds, I compare non-SOE workers' income

[18]Government wages were still following a nation-wide standard system by 1993.

growth in WS with that in three other provincial capitals similar to WS in terms of city scale and economic size.[19] Using data from China Household Income Project Survey 2002 (CHIPS2002), I plot the average personal income for non-SOE workers over years from 1998 to 2002 in the five cities. If the wage reform did drastically increase non-SOE wages in WS, the average non-SOE income in both of cities is expected to grow faster than other cities during the same period. However, Figure I.A.1 shows that the non-SOE income growth rate in WS is quite similar to other cities with Wuhan even slightly lower. These results are unsurprising because cities struck by larger impacts of economic restructuring generally had slower growth of government revenue. The lower income growth rate in non-SOEs in WS is probably the result of the lack of financial support from the government.

5.2.2 College Enrollment Expansion

A policy to expand the higher education enrollment was initiated in 1999. The Gross Enrollment Ratio (GER) in higher education increased drastically after 2001 in China according to the World Bank. Accompanying with this policy was a sharp increase in tuition fee (Shen and Li, 2003). Both of these policy changes may affect SOE and non-SOE children differently, and if they disproportionately favor the non-SOE children, the effect of economic restructuring on children's college attendance can be biased.

I conduct the following two robustness checks to make sure that the education policy in the late 1990s does not confound the results. First, I drop children born after 1981 from the sample who were likely exposed to the college expansion policy and estimate Equation 1 only with older cohorts. The result from Columns 1 of Table I.A.1 shows that the impact of economic restructuring on college attendance is robust to the alternative specification.

Second, if the education policy can benefit the rich more, one implication of the "favoring-the-rich" hypothesis is that children growing up in urban areas would on average have a better chance of attending college than those from rural areas after the education reform, as urban areas in China

[19]The selection of cities is dictated by the availability of data.

are typically richer. I examine the implication of this hypothesis using data from China Family Panel Survey 2010 (CFPS2010). The survey asks respondents' educational attainment and their residence at 12 years old. Given this information, I plot over cohorts the fraction of people that were admitted to college by their rural/urban residence in the youth. If the hypothesis is true, we are expected to see the gap of college enrollment between urban and rural children jumped after the education reform. Nevertheless, although the gap of attending college between the two groups did increase over time as displayed in Figure I.A.2, it seems that it did not drastically increase comparing the pre-shock cohorts with post-shock ones born after 1981 – the gap actually diverges at a fixed rate between the two groups. This result is thus inconsistent with the claim that the higher education expansion policy particularly benefits the rich.

5.2.3 Housing Reform

New housing stock was historically allocated to urban residents through state work units after 1949. Starting from 1994, individuals with public housing were allowed to purchase full or partial property rights of their current homes. The price was highly subsidized and most buyers paid less than 15% of the market value for their own homes (Wang, 2010, Gao, 2010). The housing reform might disproportionately benefit the non-SOE workers with the housing subsidies if they were more likely to reside in public housing than SOE workers prior to the reform.

Using data from two urban household surveys, Chinese Household Income Project Survey 1988 and 1995 (CHIPS1988 and CHIPS1995), I check whether the distribution of public housing differs between SOEs and non-SOEs before the reform. The Figure I.A.3 shows that the percentages of SOE and non-SOE householders living in public housing are quite close to each other in both years with 0.845 and 0.88 in 1988, and 0.452 and 0.465 in 1995 respectively. On the other hand, for households working in private sectors, the number is much lower than SOE and non-SOE households in both years. This evidence demonstrates that the allocation of public housing is balanced between SOEs and non-SOEs before the reform.

5.3 Other Robustness Checks

This section provides several additional robustness checks for the main results obtained from section 5.1. As a direct-controlled municipality by the central government, Shanghai is different from other cities in many aspects. The special economic zone set up in 1990 is also a sign that Shanghai may have implemented distinct economic policies (Wang, 2013). These policies may affect SOE workers and non-SOE workers differently as well as economic restructuring does. In addition, as pointed out by Huang (2008), Shanghai, together with Heilongjiang and Qinghai provinces, implemented the SOE reform slightly earlier than other ones. Given the unique political and economic position of Shanghai, I exclude Shanghai from the regression to see whether my results are mainly driven by Shanghai's special policies. As displayed in Columns 2 of Table I.A.1, dropping Shanghai doesn't greatly affect the results.

The control group in this study includes children whose fathers working in government agencies and public institutions. Although economic restructuring did not target the private sector, children with fathers working in the private sector are excluded from the control because the difference between the private sector and the treatment is large and the size of the private sector was negligible in the period considered. To show that excluding the private sector does not affect the results, I use a broader definition of the control group by including children with fathers working in the private sector before 1992. The results presented in Column 3 of Table I.A.1 confirm that the previous results are not driven by the inappropriate definition of the control group.

Meng (2012) and Ge and Yang (2014) documented the divergence of return to education in urban China in the 1990s using Urban Household Survey. The return to college-and-above education rose from around 16% in 1988 to over 50% in 2003, while that to junior high school remained below 20%. Since SOE workers were on average less educated than non-SOE workers, their income might diverge not because of economic restructuring but because of the differential returns to education. To deal with this concern, I control for the cohort-varying variables related to father's education by adding father's educational attainment, school quality, and school performance

interacted with children's cohorts. As shown in Table I.A.2, adding these controls do not change the main results much and most coefficients of these controls are insignificant. [20]

I also perform a falsification test to examine whether there are time-varying unobservables driving the results. In the main analysis, the post-shock cohort is defined as the youngest cohort of children not eligible for college or high school by 1997. In the falsification test, I use an earlier cohort – those not eligible for college or high school by 1990 – as the placebo post-shock group. If the results are driven by time-varying unobservables, we would see a negative effect even in absence of the economic restructuring. The results presented in Table I.A.3, however, do not support this hypothesis. Most of the estimates are either insignificant or wrong-signed.

Including more cohorts increases the number of observations and lends more statistical power to the analysis, but the downside is that older cohorts may have a higher attrition rate, as their fathers may have deceased and their characteristics are thus unobservable. If the attrition is systematically different between the treatment and the control group, the DID estimates will be biased. To make sure that the results are not driven by mortality attrition, I estimate Equation 1 with groups of younger cohorts as a sensitivity analysis. The results presented in Table I.A.4 suggests that mortality attrition does not pose a major threat to the identification strategy – the main results still hold as I sequentially drop the older cohorts out of the sample.

6 Discussions

6.1 Heterogeneity

Understanding the heterogeneity of the economic shock can give insights into policy design on more targeted social protection policies. In this section, I investigate whether there are heteroge-

[20] As shown in Table I.1, some other father's pre-treatment characteristics are unbalanced between the treatment and the control group as well. The dynamics of children's outcome variables may be associated with these initial differences of father's characteristics. To control for these potential cohort-varying observables, I add children's cohort dummy interacted with a rich set of father's demographic variables including height, party membership, and personal early life experiences – e.g., being sent down to the rural area and living in the urban area in the youth. The results presented in Column 4 of Table I.A.1 indicates that adding these demographic interactions does not significantly change the results.

neous impacts of economic restructuring between children's gender and across family characteristics.

Gender Girls are generally more susceptible to aggregate economic shocks than boys in developing countries. Previous research has found that female infants are more vulnerable to negative income shock than male infants (Ferreira and Schady, 2009, Bhalotra, 2010, Baird, Friedman and Schady, 2010). To examine whether the impact of economic restructuring varies between children's gender, I estimate the following equation:

$$E_{iash} = \alpha_0 + \alpha_1 SOE_s \times Postshock_a \times Boy_h + \alpha_3 SOE_s \times Postshock_a + Boy_h + \rho_a + \eta_s + \theta^J X_i + \varepsilon_{iasc} \quad (4)$$

where Boy_h is a dummy taking a value of one if child i is a boy. Other parameters and variables are defined in the same way as in Equation 1. If boys are less affected by the shock of economic restructuring, we expect a positive α_1 and an even smaller α_3 than that in Equation 1. The results from Columns 1 and 4 of Table I.5, however, show small and statistically insignificant differences between boys and girls, indicating that the gender difference of the economic restructuring shock is small in urban areas.

Extended Family Member Extended family members can possibly provide certain forms of protection to alleviate the economic restructuring shock on children. Existing evidence from developing countries shows that siblings can provide informal insurance in the form of gift money, informal loans, or other non-monetary support (Fafchamps and Lund, 2003, Fafchamps, 2011). Moreover, siblings can help children indirectly by mobilizing their social resources to help children's parents to get re-employed.

Table I.5: Heterogeneous effects

SETTINGS	College			High School		
	Gender	Siblings	Brothers	Gender	Siblings	Brothers
	(1)	(2)	(3)	(4)	(5)	(6)
Post-shock Cohort × Father in SOE	-0.0741	-0.194***	-0.210***	-0.0748*	-0.177***	-0.137***
	(0.0489)	(0.0467)	(0.0406)	(0.0417)	(0.0646)	(0.0473)
Post-shock Cohort × Father in SOE × Boy	-0.0724			-0.00680		
	(0.0844)			(0.0700)		
Post-shock Cohort × Father in SOE × Parental Siblings		0.0161**	0.0391***		0.0184**	0.0206***
		(0.00743)	(0.0101)		(0.00767)	(0.00661)
Gender FE	Yes	No	No	Yes	No	No
Sibling FE	No	Yes	Yes	No	Yes	Yes
Brother FE	No	No	Yes	No	No	Yes
Mean Outcome of Variable	0.4345	0.4345	0.4345	0.6871	0.6871	0.6871
Observations	1,855	1,855	1,855	2,822	2,822	2,822

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters). Dependent variables are college enrollment and high school enrollment respectively. The post-shock cohort in the college sample consists of children born from 1980 to 1983, and in the high school sample born from 1981 to 1985. Sibling and Brother are the total amounts of siblings and brothers from both mother and father sides. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls include the number of children's siblings, sisters, and brothers. Cohort fixed effect, job sector fixed effect, father controls, and children controls are included in all specifications.

The empirical specification is the following:

$$E_{iash} = \alpha_0 + \alpha_1 SOE_s \times Postshock_a \times Sib_h + \alpha_3 SOE_s \times Postshock_a + S^f + S^m + \rho_a + \eta_s + \theta^J X_i + \varepsilon_{iasc} \quad (5)$$

where Sib_h is the total number of siblings that the children's parents have. I control for S^f and S^m, the number of paternal and maternal siblings respectively. Other parameters and variables are defined in the same way as in Equation 1. Table I.5 report the results in equation (5). As shown in Columns 1 and 3, having one more parental sibling significantly reduces the impact of economic restructuring on children's educational attainment. In addition, children in families without any parental siblings suffered even more from the shock.

To interpret the results as the protection from parental siblings, an important complication needs to be addressed. The number of siblings can be correlated with a certain family trait, and both of these two may shift along the same dimension. For example, parents with rich family background may have more siblings and these parents can be generally less vulnerable to negative income shock. If so, the effect identified in equation 1 cannot be necessarily interpreted as the effect of sibling's support. To get around this issue, I use a different empirical strategy. Since having one more brother or sister is plausibly random and the male generally has larger financial capacity than the female, I instead examine whether children with more uncles, as compared to aunts, were less affected by the shock.

Following Zhou (2014), I replace the total number of siblings with the total number of brothers and meanwhile control for the total number of siblings. The identifying assumption is that conditional on the total number of siblings in each family, the number of male siblings is exogenous.[21] The regression equation to be estimated is as follows:

[21] Note that this assumption may fail if households with son preference use the "stopping rule" to conduct sex selection, which is common in Asian countries such as China and India(Yamaguchi, 1989, Jensen, 2005, Barcellos, Carvalho and Lleras-Muney, 2014). If the household's fertility decision is biased towards the son, the number of male siblings may be endogenously correlated with the outcome variable even after the total number of siblings is controlled. However, Zhou (2014) finds that the sex selection does not play a significant role in urban areas during the baby boom (1946–1978) and thus does not pose a major threat in this setting, as most siblings were born before 1978.

$$E_{iash} = \alpha_0 + \alpha_1 SOE_s \times Postshock_a \times Bro_h + \alpha_3 SOE_s \times Postshock_a \tag{6}$$
$$+ Bro_h + S^f + S^m + \rho_a + \eta_s + \theta^J X_i + \varepsilon_{iasc}$$

where Bro_h is the total number of brothers that the children's parents have. In this specification, I also control for the total number of brothers in addition to the number of paternal and maternal siblings. Given the total number of siblings fixed, Bro_h captures the effect of the ratio of male parental brothers on children's education.

Column 2 and 4 show that the results remain robust and close to Columns 1 and 3 even with this alternative specification. Taken together, these pieces of evidence suggest that parental siblings can partly alleviate the impact of economic restructuring shock on children's education.

6.2 Mechanisms of Income Change

Children's education has been demonstrated to be sensitive to family income changes(Aizer et al., 2014, Dahl and Lochner, 2012). Economic restructuring may lead to two types of income changes: the reduction in relative earnings associated with job loss (extensive margin) and the reduction in relative earnings and welfare at the initial employer (intensive margin).[22] This section discusses the two channels in details.

Figure I.5 presents visual evidence showing that economic restructuring exposes children to a higher risk of experiencing father's job loss. Panel (c) and (d) plot the fraction of children who experienced father's job loss before they went to college or high school. The likelihood of experiencing father's job loss drastically increases from almost zero to around 10% and 15% following the shock, highlighting the impact of mass layoffs induced by economic restructuring. The rising risk of experiencing father's job loss also coincides with the decrease of educational

[22] Autor et al. (2014) summarizes two other types of income change induced by economic restructuring: increased government benefits (transfer margin) and changes in earnings associated with the new employer (reallocation margin), but these two channels barely exist in the case of China's SOE reform, as discussed in Section 2.2.

Table I.6: Likelihood of experiencing father's job loss

	College Sample (1)	High School Sample (2)
Post-shock Cohort × Father in SOE	0.0526*	0.0539**
	(0.0286)	(0.0267)
Mean of Outcome Variable	0.0410	0.0269
Observations	1,855	2,822

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters). The dependent variable is a dummy taking a value of one if the child experienced father's job loss between 6 and 18 years old in the college sample, and between 6 and 15 years old in the high school sample. The post-shock group in the college sample consists of children born from 1980 to 1983, and in the high school sample born from 1981 to 1985. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls include the number of children's siblings, sisters, and brothers. Cohort fixed effect, job sector fixed effect, father controls, and children controls are included in all specifications.

attainment for the children in the treatment group, as illustrated in Panel (a) and (b).

To more accurately quantify these effects, I estimate a reduced-form model with the similar specification as Equation 1. The dependent variable is a dummy taking a value of one if the child experienced father's job loss before entering college or high school. Other parameters and variables are defined in the same way as in Equation 1. The results presented in Table I.6 are consistent with the visual evidence. Children in the treatment group were significantly more likely to experience father's job loss following the SOE reform.

The magnitude of the effect is quantitatively small, implying that the earning change at the intensive margin may also play a role. While the nature of the data does not permit a direct test of this hypothesis, Figure I.3 provides supportive evidence showing that the basic wage of SOE and non-SOE sector diverged after 1997.[23] This piece of evidence is consistent with the finding from

[23]To quantify the effect of earning change at the intensive margin, we need data to keep track of father's yearly earnings prior to children's schooling. This information, however, is not available in CULS2001.

Fallon and Lucas (2002) and McKenzie (2004), in which they show that the reduction of real wage at the initial employer rather than the unemployment was the main contributor to the fall of average income during the financial crisis in Argentina in 2002 and in Southeast Asia in 1997. Overall, these results suggest that both of these channels play a role in influencing children's educational attainment.

That said, economic restructuring may also affect children's outcomes through some non-monetary channels, such as parents' time spent with children or job-related stress and pressure. While these effects may have been fully or partially caused by the earning change, this study does not completely rule out these possible non-monetary factors. To what extent these channels can affect children's education is an interesting topic to explore in the future.

7 Conclusions

China's SOE reform in the mid-1990s provides an ideal case for researchers to delve into the intergenerational consequences of economic restructuring. While workers employed in the non-SOE public organizations were less affected, SOE workers - both the laid-offs and the survivors - suffered a drastic drop in their cumulative earnings. This immediate impact also has striking intergenerational implications. SOE children were more susceptible to the economic shock and were less likely to attend high school and college compared to their non-SOE peers. Moreover, I present evidence on the existence of informal insurance within extended families in urban China. SOE children whose parents have fewer siblings were even more adversely affected by the reform.

Furthermore, this study develops a conceptual framework to understand the mechanisms of two opposing externalities of economic restructuring on children's education. The negative externality is related to the job displacement induced by economic restructuring. In cities with a higher percentage of SOE workers in the pre-reform labor force, there were more laid-off workers after the reform. SOE workers were more severely affected by the reform in those cities, as they faced more intense job competition with their peers after the shock, which led to lower wage or longer

unemployment spell. As a result, their children's educational attainment were more affected. On the other hand, the positive externality of economic restructuring to education is associated with the competition over scarce education opportunities. In cities with higher pre-reform SOE labor share, the number of children that had father laid off was larger. Although it means that more students' education was affected in these cities, it actually lowered the overall competitiveness of enrollment into college and high school and in turn benefitted anyone's chance of getting enrolled. I construct a simple model to illustrate these mechanisms and examine the model predictions by comparing cities with different pre-reform SOE share. The empirical results support the evidence of a negative externality: the shock of economic restructuring was amplified in cities with higher percentages of pre-reform SOE workers.

The evidence points to an adverse impact of economic restructuring on next generation's educational attainment in a society lacking effective social safety net programs and sound credit markets. Although the existing informal social network could partially alleviate the shock, the SOE reform, in general, had produced profound impacts on not only the immediate generation but also the next generation. The magnitude of the impacts suggests that intergenerational costs of economic restructuring should be taken into consideration in policy design and evaluation.[24]

While the government-initiated SOE reform facilitated the relocation of resources, increased economic efficiency, and spurred economic growth (Song, Storesletten and Zilibotti, 2011), the distributional consequences of the reform, especially its intergenerational implications, has not yet been fully explored. Existing empirical evidence suggests that the economic efficiency was achieved to a large extent at the cost of welfare of SOE workers, which has contributed to an enlarged income gap in urban areas.[25] Taking this result a step further, this chapter shows that the increased income inequality can lead to greater inequality in the educational attainment of the next generation. A potential topic for future research to explore is to what extent the income distribution

[24]There are some successful experiences in targeting laid-off families with children in school in developing countries during the economic crisis. See, for example, Cameron (2009), Galasso and Ravallion (2004).

[25]Based on the calculation from Ravallion and Chen (2007), both the relative and absolute Gini index in the urban area increased dramatically after 1990 as compared to that in the rural area. See Xia et al. (2014), Benjamin et al. (2008), Knight and Song (2003) for more discussion on China's rising inequality associated with the SOE reform in the 1990s.

of the next generation in the urban area has been altered by the economic restructuring.

Chapter II Does Education Affect Sex Selection?

1 Introduction

In traditional rural societies, males are the main source of agricultural labor and important guarantee of parents' old-age support. Such an inelastic demand for males makes son preference prevail in these societies. One may wonder whether social progress brought by urbanization and industrialization can reduce the demand for sons and thus overcome the son-biased prejudice. The chance, however, seems slim based on the development experience from East Asian countries. It is puzzling that rapid urbanization and industrialization in those societies with a son preference tradition are oftentimes accompanied with a rising rather than falling male-biased sex ratio.[26] To understand why sex selection persists in modern societies despite a lack of economic incentives, this chapter explores whether and how education—one major non-economic factor—impacts one's sex preference and sex-selective behavior.

Theoretically, the effect of education on sex selection is unknown a priori. On the one hand, more years of education may increase one's awareness of and financial capacity for sex-selective technologies or services, thereby facilitating the sex-selective behavior. For example, evidence from India suggests that women who have at least completed middle-school education are more likely to conduct sex-selective abortion because they have better knowledge of and access to the abortion service (Retherford and Roy, 2003, Bhalotra and Cochrane, 2010). On the other hand, the morals and values imparted by teachers, such as belief in equality and respect for life, may challenge the entrenched sex prejudice and discourage the sex-selective behavior.

This chapter focuses on China out of both practical and methodological considerations. Firstly, the sex ratio in China had been rising despite China's fast urbanization and social transformation

[26] For example, the sex ratio at birth in South Korea and Taiwan surged in the 1980s and 1900s when the urbanization and industrialization accelerated (Gupta et al., 2003, Chung and Gupta, 2007).

from the Reform and Opening Up in 1978 to 2010. As shown in Figure II.1, the sex ratio at birth

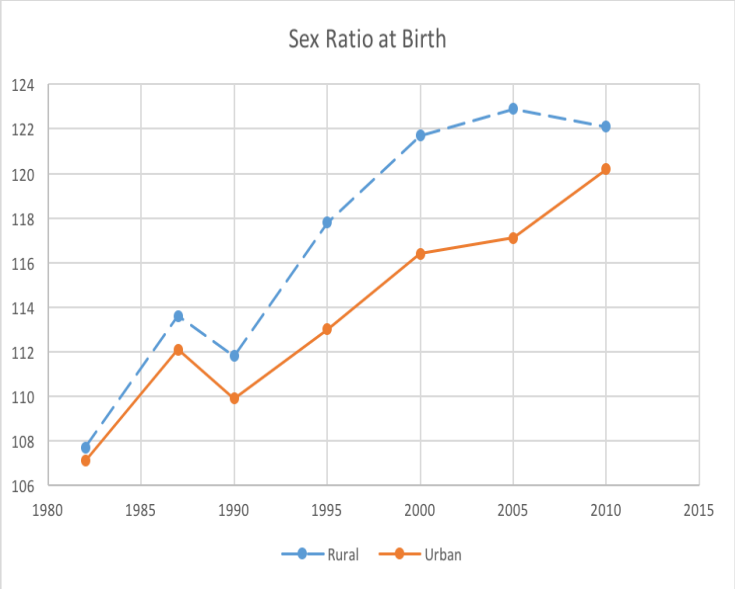


Figure II.1: Sex ratio at birth, 1980-2010

Note: This graph is adapted from Figure 1.12 of UNICEF (2014).

in urban areas, though consistently lower than that in rural areas, increased from 1.07 in 1982 to 1.20 in 2010, far exceeding the global average (1-1.06). The unbalanced sex ratio has raised concerns over a wide range of social issues, including the marriage-market imbalance and urban crimes (Ebenstein and Sharygin, 2009, Edlund et al., 2013).

Secondly, the empirical evidence on the effect of education on sex selection is largely absent in the literature. One challenge to establish a causal relationship between the two is that both can be affected by unobservables such as family background or genes. Therefore, the effect estimated by Ordinary Least Square (OLS) strategy will be biased because it fails to control for these unobserved variables. To tackle this issue, I exploit a unique incident that exogenously altered the length and quality of education: the disruption of normal operation of high school education in the late 1960s of China.

In the first two years of the Cultural Revolution (1966-1976), high schools in urban areas were shut down. When the school enrollment resumed in 1968, both the quantity and the quality of

education dropped substantially. The length of schooling was shortened from three to two years, standard textbooks were inundated with ideological propaganda, and many skilled teachers were displaced from urban to rural areas. The policy generates plausibly exogenous shocks on both the quantity and the quality of education that urban high-school students received. Older cohorts that had completed schools prior to school closure received standard three-year education, whereas the younger cohorts that did not graduate before school closure in 1966 or enrolled after school resumed in 1968 had fewer years and lower quality of education, though they still obtained the same high school credentials. The education shock produces two kinks in high school student's education function against the birth cohort. The non-linear exposure to the education shock across cohorts allows this study to use the Regression Kink Design to examine the education effect.

One key finding of the chapter is that fewer years of high school education combined with lowered education quality lead to more sex-selective behaviors, as illustrated by the more biased sex ratio at birth towards sons as a result of the education shock. There are two possible channels through which education can affect sex selection. The first one is the income channel. Shorter schooling decreases future family income. Therefore, if a son is an inferior good, then impacted families would tend to have more sons. The second is the sex-preference channel, which states that, if sex preference is malleable in the youth, then son-biased preference may be moderated by good quality secondary education. This study provides empirical evidence to demonstrate that the income channel is not likely to hold while the sex preference channel is more plausible.

One concern over the identification strategy is that China was in a turbulent period during the Cultural Revolution when many catastrophic events occurred, which might affect sex selection thereby confounding the education effect. I discuss in detail some of these key concurrent events, such as the Red Guard Movement and the Send-Down Policy, and conduct robustness checks to ensure that they do not drive the main results. Additionally, since the education shock of interest is high-school specific, the sex ratios of children of primary and middle school graduates are supposed to evolve smoothly, provided that these confounding events do not affect sex selection. I perform a falsification test with primary and middle school graduates as the placebo treatment

group to assess whether this pattern holds. Results produced by these additional checks are in line with the main findings.

The rising male-biased sex ratio in China has intrigued many researchers to probe its causes. Ebenstein (2010) finds that family planning policies incentivize people to conduct sex-selective abortions to avoid fines on excess fertility. Chen, Li and Meng (2013) focus on the popularization of ultrasound technologies, which make sex detection service more accessible to the public. Almond, Li and Zhang (2014) attribute the rising sex selection to improved economic conditions brought about by economic reform. Unlike these studies that stress economic motives, which are extrinsic factors contributing to sex selection, this chapter focuses on an intrinsic element that explains the persistence of sex-selective behavior.

This study also contributes to the literature on the social benefits of education in three ways.[27] First, since most of the existing research is based on developed countries, few works have been done to examine the role education plays in sex selection. Second, it adds to the discussion of education policies in developing countries, where there lacks enough rigorous empirical research quantifying the benefit of universalizing upper secondary education. Third, this study disentangles the pecuniary effect of education from its non-pecuniary effect, which is rarely discussed in many existing studies. Education decides one's future income, which in turn affects a wide range of things; therefore, a clear distinction between the income effect and the direct education effect is hard to establish. By focusing on the group of people who, though impacted by the education shock, still obtained the same educational credentials as their unaffected peers, the signaling effect of education in this study is muted; hence the income variation of education is minimized.

The rest of the chapter proceeds as follows. Section 2 discusses the historical background of the education shock. Section 3 gives a detailed description of the data and variables used in this chapter. Section 4 articulates the identification strategies. The estimation results and robustness

[27]For example, Currie and Moretti (2003) find that women's schooling can significantly improve children's health. de Walque (2007) finds that individuals with more years of education are less likely to smoke, and are more likely to quit if they have been smokers. Milligan, Moretti and Oreopoulos (2004) and Dee (2004) provide evidence that more years of education can promote civic participation. Lochner and Moretti (2004) find additional years of schooling could reduce criminal activities. Also, see Oreopoulos and Salvanes (2011) for a comprehensive survey of this literature.

checks are presented in Section 5. Section 6 explores the channels through which education affects sex selection, and it is followed by a conclusion that generalizes main findings and policy implications of this chapter.

2 Historical Background

The three-year high school education was instituted at the establishment of the People's Republic of China in 1949. Although fewer years of schooling was experimented in some areas, the three-year schooling length predominated until 1966. During the Cultural Revolution, urban high schools were temporarily shut down and their enrollment was halted between 1966 and 1968.[28] Though still granted high school diploma, students already enrolled before 1966 lost one to three years of education because of the school closure. Those that should have gone to high schools had to postpone their schooling. When urban high schools resumed enrollment in 1968, their length of schooling was shortened from three to two years.[29]

Not only the length of schooling decreased, but the quality of education also plunged after reopening. First, in the anti-intellectual political movement, large numbers of respectable and skillful teachers in urban schools were persecuted and purged from work. The paucity of high-quality teachers in secondary education was rather common in major cities of China after 1968 (MacFarquhar and Schoenhals, 2009). In addition, it was almost impossible to maintain school disciplines and regulations even after school reopened. Teachers became extremely cautious for fear of incurring political charges and persecutions; students were in the revolutionary mode and many could not be bothered to study. Last but not the least, the standard academic curriculum was reshaped and infused with ideological indoctrination and knowledge on industrial or farming skills. For example, in some schools, the Humanities, such as Chinese, History, Music, and Fine

[28] In urban areas, most primary schools, middle schools, and vocational high schools were closed too in this period, and the enrollment of universities was suspended for almost four years from 1966 to 1970.

[29] By the time when urban high schools were shut down, the rural high schools had been very underdeveloped. Only 9 percent of all high school students were in rural areas in 1965 (Ministry of Education of the People's Republic of China, 1979). Given their small proportion, whether and to what extent rural high schools were affected by the education shock are not well documented in the literature.

Art, were substituted by an ideological class called "Revolutionary Art and Literature", the content of which were mainly based on Mao Zedong's writings and speeches. Math and Physics were replaced by work classes teaching how to use agricultural implements, such as the tractor, the diesel engine, the electromotor, and the water pump. Chemistry and Biology gave way to training on agricultural production and farming practices. Foreign Language was entirely removed from the standard curriculum (Mao and Shen, 1989).

In summary, those who completed high schools before the shutting-down incident received an education of relatively higher quality, whereas the younger cohorts enrolled during the Cultural Revolution experienced either the disruption or disorder of schooling. Both the quantity and the quality of education they received plunged after 1966, following a transition from a level with three years and high quality of education, to a low level, two years and relatively low quality of education.[30]

3 Data and Sample

3.1 Data

The data employed in the main analysis comes from Census 1990, which covers 1% of the total population in China and contains detailed information on individual characteristics including marital status, educational attainment, rural/urban residence, the number and the sex composition of children, etc. The merit of using the census is that its large sample size is critical in identifying the slope change in the Regression Kink Design. But the census does not contain all the essential information for this research, for example, the years of schooling in high school. Therefore, I use data from micro-level surveys as supplements whenever necessary. Appendix B gives a detailed description of these micro-level surveys used in this chapter.

[30]The three-year length of schooling was restored gradually in the 1980s after the end of the Cultural Revolution.

3.2 Measurement of Sex Selection

A proper micro-level measure for sex selection requires the information on children's sex given the birth order. However, Census 1990 only reports one's total number of sons and daughters but not specifically each child's age or birth order, which makes it hard to construct a direct measure for sex selection. Instead, I infer one's sex-selective behavior from her children's sex composition. The primary indicator used in this article is whether the respondent "has a girl as her only child", which captures the idea that the more likely a mother is willing to have a girl as her only child, the less likely she conducts son-biased sex selection. Although this measure does not require the knowledge of children's birth order, one concern of using it is that it may pick up one's quantity preference rather than her sex preference and thus complicate the interpretation of the results. Section 5.5 addresses this issue and discusses the robustness of this measure. As a further robustness check, I use a method to elicit the information of children's age and sex from the census and construct two alternative measures of sex selection in Section 5.2.

3.3 Sample

The procedure of obtaining the main sample is as follows. Firstly, I keep the female sample, because Census 1990 only asks women's fertility outcomes. To be consistent, I also drop the male sample when using data from other micro-level surveys. Note that the focus on women's fertility outcome is not a choice but is dictated by the data availability. Because the education shock is not gender-specific, this chapter cannot address how the relative economic status of women in the household affects her fertility outcomes.[31]

Secondly, the sample is restricted to urban population throughout the chapter, because the high school closure mainly affected the urban area and it is the urban sex selection that is of interest in this chapter.[32] Dividing the sample into urban and rural residence needs some caution. The urban

[31] For the women-specific income change that leads to the change of intra-household bargaining power and thereby women's sex choice, see the discussion of a nonunitary household model in Qian (2008).

[32] Another reason for dropping the rural sample is that the high school enrollment in the rural area expanded rapidly after 1966 (Pepper, 1980). Such expansion made it easier for rural students to access to high schools and changed the

residence in 1990 does not necessarily imply the enrollment in urban areas in the 1960s – it is entirely possible that people who received high school education in rural areas migrated to cities subsequently. However, it is reasonable to assume that the current residence in Census 1990 is a good proxy for the original one because China has been enforcing its "Hukou" Policy (Household Registration System) since 1958, which effectively restricted the scale of rural-to-urban migration before 1990 (Zhang, 2012). According to Census 1990, only around 2% of the cohorts in the sample are migrants. For more recent surveys, the current residence is less accurate to represent the original one, as China's urbanization accelerated and the rural-urban migration scaled up in the 1990s. The rural/urban status is thus decided by the respondent's retrospective residence rather than her current one.

Thirdly, I focus on the cohorts born between 1945 and 1955 to keep the bandwidth short and to avoid the potential effects of other high school policies in the early 1960s on cohorts born before 1945. In the robustness check, I extend the bandwidths to longer period to examine the sensitiveness to such variation.

Lastly, the sample is restricted to academic high school graduates only.[33] Since the shock of interest is high school-specific, those with education up to primary school or middle school are dropped from the sample. College graduates are also excluded, because during the Cultural Revolution, colleges started admitting the so-called worker-peasant-soldier students (the gong-nong-bing xueyuan), many of whom did not have high school degrees, but instead had only middle school education background or even lower. For example, among seven colleges in Beijing in 1971, only 21 percent of students had a high school degree; 78 percent had a middle school degree; and 0.6 percent had only primary education. 30 percent of science-majored students in Wuhan

total composition of high school students. If the rural high school students have a stronger son preference, then sex selection may be affected by the composition change of high school students.

[33]Note that high school graduates specifically refer to those graduated from academic high schools throughout this chapter. Vocational high school (VHS) graduates and those graduated from other post-junior educational institutions are excluded from the sample, because adding them into the pool of high school students could potentially dilute the effect of the education shock. Although VHSs also experienced the school closure and the shortened length of schooling, I find that the education shock had little impacts on VHS students' sex selection, possibly because VHS' curriculum is more focused on practical skills than on liberal arts education, such as Chinese, History, and Arts, which are more relevant to students' moral formation and accordingly their sex preference.

Table II.1: Summary statistics: Census 1990

Subsample	Main Sample	Sample with Birth Order Information			
		All	# Children$\geq$ 1	First child is a girl	First child is a boy
Demographics					
Having a girl as the only child	0.270				
Age	38.33	38.10	38.10	38.10	38.10
Having a job	0.959	0.962	0.962	0.962	0.962
Married	0.967	0.968	0.976	0.976	0.976
# Children	1.480	1.440	1.420	1.460	1.380
Ethnicity = Han	0.956	0.959	0.958	0.956	0.960
First child is a girl			0.481		
Observations	21485	20211	19891	9565	10326

Note: The main sample is obtained from Census 1990 (see Section 3.3 for more details). The procedure of obtaining the sample with birth order information is described in Section 5.2.
Source: Census 1990

University, another prestigious university in China, had only primary education. Some college students were even illiterate (Cheng, 2001). Column 1 of Table II.1 presents the summary statistics of the main sample.

4 Identification Strategy

The key idea behind this empirical strategy is that, due to the education shock, the likelihood of experiencing shortened length of schooling and lower quality of education for high school students surged shortly after 1966, and remained high from 1968 and on. This can be seen from Figure II.2, which plots the average years of schooling and the likelihood of receiving standard three-year education in high school as a function of one's birth year. The shock should have little effect on cohorts born before 1949 years of schooling or quality of education because they were older than 17 in 1966, the age by which most of them had finished high school. By contrast, those born between 1949 and 1951 were mostly ready to enter high school or already enrolled but later encountered the school closure. Many of them missed one to three years of schooling in high school. The quality of

education they received fell as well after the urban high school was reopened two years later. For those younger than 15 in 1966, they were deprived of the opportunity of enrolling in high school until high schools were reopened in 1968. Their years of schooling in high schools was shortened and the quality of education they received dropped to a lower level.

The disruption of schooling and the shortened years of education produce two kinks in the education function as shown in Panel (a) and (b) of Figure II.2. The change in the slope at the two critical birth years, 1949 and 1951, visually illustrates how one's years of education in high school was hindered by the shock. Likewise, Panel (a) of Figure II.3 shows that there are two kinks in the sex selection function at the same positions. While there is an overall rising trend of sex selection, the slope of the sex selection function drastically drops after 1949 and rises after 1951.

The kinky pattern of the education and the sex selection function is essential to the identification strategy. The graphical evidence that both kinks appearing at the same position indicates the shock effect on high school students' years of schooling and the sex-selective behavior in their later life. To identify these two kinks formally, I use the Regression Kink Design (RKD), which is originally proposed by Card et al. (2015) and recently employed by Landais (2015) to study the labor supply response of unemployment benefit. I extend the approach to the case where there are two kinks to be estimated in the functions. At the first step, I establish the shock impact on high school students' education by estimating the slope change of the years of schooling function at the critical birth years, 1949 and 1951. Then, I use RKD to examine whether there are also changes in the slope of the sex selection function at these critical birth years. Specifically, the following equation estimates the two kinks in the education function at the first stage: [34]

$$\begin{aligned} Z &= \alpha_0 + \sum_{i=1,2} \alpha_i D_i (T - C_i) + \sum_{i=1,2} \beta_i D_i + \mu T + \gamma X' + \varepsilon, \\ &\quad where\ \ T \in (C_1 - h, C_2 + h),\ \ C_1 < C_2,\ \ and\ \ h > 0 \end{aligned} \tag{7}$$

[34]The information on education quality is not readily available in the data. I herein merely focus on one's length of schooling at the first step.

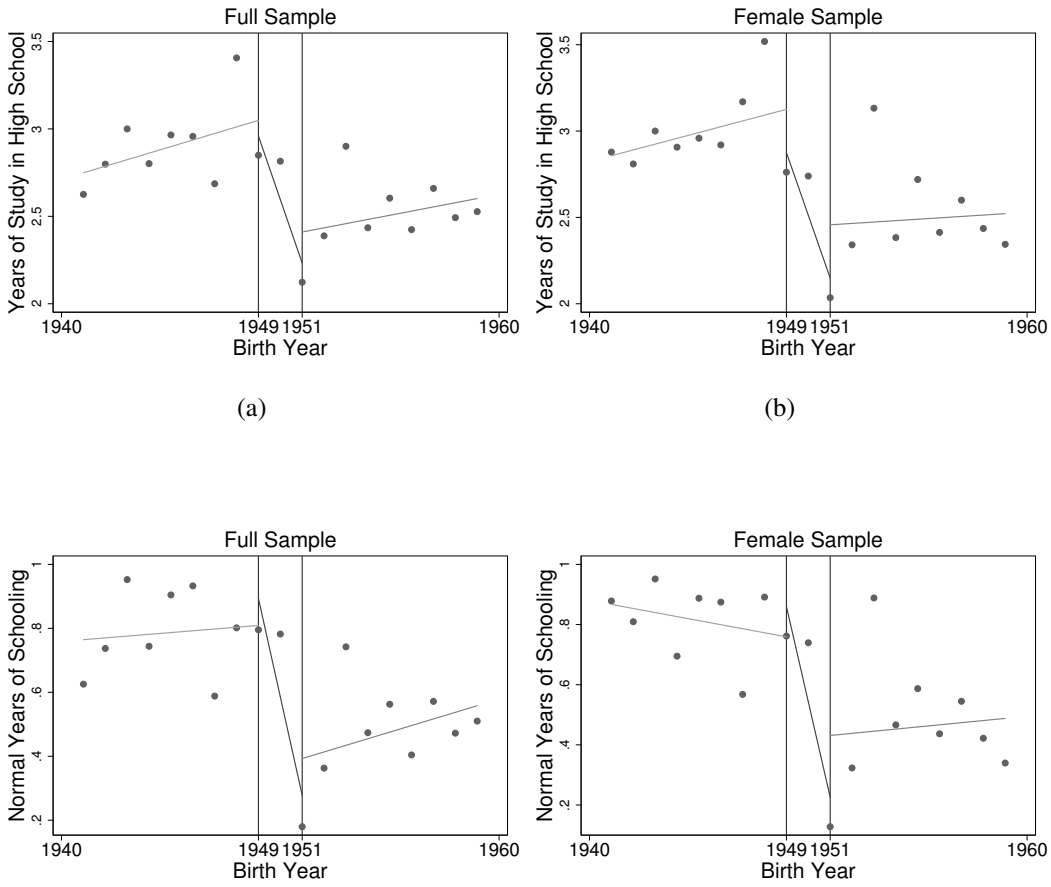
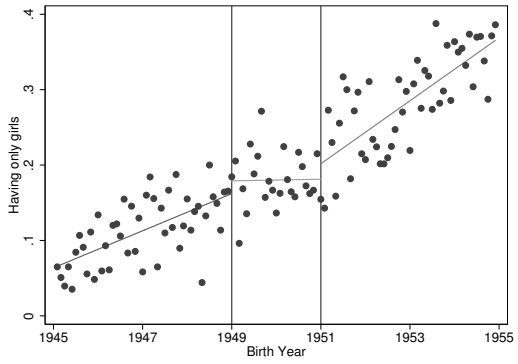
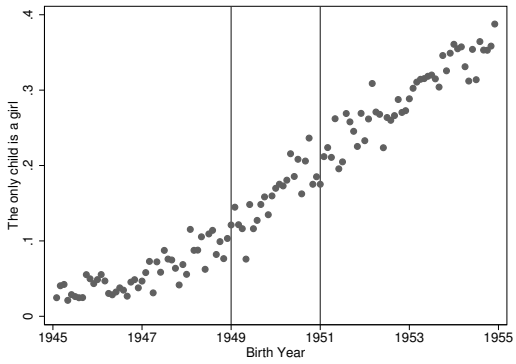


Figure II.2: Shortened length of schooling in urban high schools

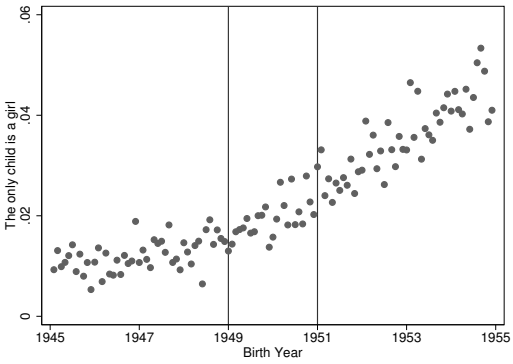
Note: The graphs show how the years of schooling in high school evolved and were affected by the education shock. In Panel (a) and (b), each dot represents the mean years of schooling in high school at given birth year. In Panel (c) and (d), each dot represents the likelihood of receiving standard length of schooling, i.e., three years in high school.
Source: CULS2001 and CFPS2010



(a) High School Graduates



(b) Middle School Graduates



(c) Primary school or lower

Figure II.3: The evolution of sex-selective behavior against birth cohorts

Note: The graphs show how sex selection, which is measured by having a girl as the only child, evolves against the birth month. The main sample includes urban females with high school education. The comparison samples include those with middle school and primary school education respectively.
Source: Census 1990

Z is years of schooling or a dummy taking the value of one if the respondent received the normal length of schooling in high school. $D_1 = \mathbb{1}(T < C_1)$ and $D_2 = \mathbb{1}(T > C_2)$ are indicators that the birth year T is below the critical value C_1 and above C_2 respectively. h is a neighborhood around critical birth years C_i. The term $D_i(T - C_i)$ captures the change in the slope of the education function at C_i. Dummies D_i are added to control for the possible slope break at C_i. As shown in Appendix C.1, the coefficients of interests, α_1 and α_2, are expected to be positive if the change in the slope of the education function decreases at C_1 and increases at C_2. The decrease of the slope at the first kink suggests that the education shock reduced the normal years of schooling. The increase of the slope at the second kink indicates that the years of schooling stopped further decreasing and remained at a lower level after the shock. The control variables at the first stage include parents' education, the number of siblings, and the number of children. Although these covariates are not necessarily needed for consistently estimating α_i, they are added to check the robustness of the model.

Next, the change in the slope of the sex selection function can be estimated by the following equation:

$$\begin{aligned} Y &= \alpha_0' + \sum_{i=1,2} \alpha_i' D_i(T - C_i) + \sum_{i=1,2} \beta_i' D_i + \mu' T + \gamma' X' + e, \\ &\quad \textit{where } T \in (C_1 - h, C_2 + h),\ C_1 < C_2,\ \textit{and } h > 0 \end{aligned} \tag{8}$$

where Y is the measure of sex selection. Other variables in Equation (8) are the same as in Equation (7). Similar to the first-stage analysis, α_1' and α_2' are expected to be positive, if the change in the slope of the sex selection function decreases at C_1 and increases at C_2.

An important identifying assumption of RKD is that the function of the dependent variable is continuous and differentiable at critical birth years in the absence of the education shock. Colloquially, it requires that had the shock not occurred, both the education function and the sex selection function evolve smoothly at C_i. That is, there should be no other factors that produce the two kinks

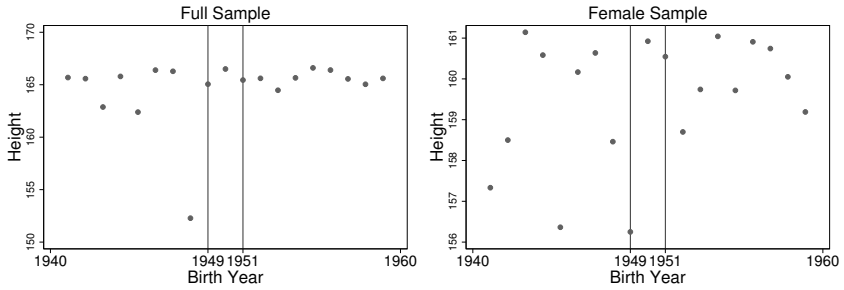
Table II.2: Test on the discontinuity of pre-determined characteristics

DEP VAR	Height	Siblings	Meduc	Feduc	Sent-down
	(1)	(2)	(3)	(4)	(5)
$\triangle Slope\mid_{birthyear=1949}$	2.516*	0.337	0.948	1.327	0.155
	(1.461)	(0.566)	(1.516)	(1.736)	(0.156)
$\triangle Slope\mid_{birthyear=1951}$	1.822	0.0824	1.291	1.082	0.0740
	(1.262)	(0.515)	(1.377)	(1.423)	(0.128)
P-value from F-test	0.560	0.590	0.738	0.862	0.548
Obs	142	142	127	126	142

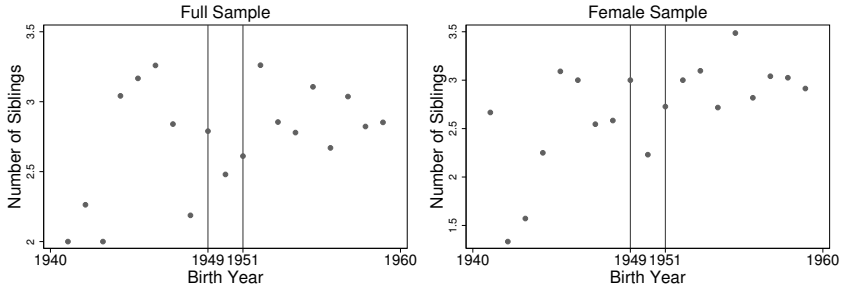
Note: This table reports coefficients from a test of the pre-determined characteristics based on Equation (8). P values of a test of joint significance of the changes in the slope are also reported. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS2010 and CULS2001

on the education and the sex selection function other than the high school shock does. Although the smoothness assumption can not be tested directly, data from CFPS 2010 and CULS 2001 permit two indirect tests. First, I examine the evolution path of a set of pre-determined individual characteristics against their birth year, including the anthropometrics, family background, and early-life experiences. If the identifying assumption holds, these variables are supposed to evolve smoothly at C_i. Figure II.4 plots the trends of these individual characteristics for both the female and the full sample. The results show that there are no noticeable changes in the slope at critical birth years, which is consistent with the smoothness assumption. A formal test presented in Table II.2 also confirms these results.

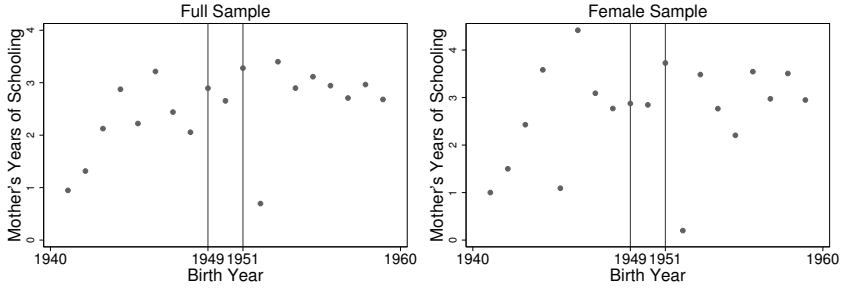
Second, the smoothness assumption of RKD also requires that the running variable, high school students' birth year, not be manipulated, which is similar to that in the Regression Discontinuity Design. There are two reasons why it holds in this context. First, it is practically impossible for parents to foresee the education shock that occurred in more than fifteen years and circumvent it by adjusting the timing of childbearing. Moreover, I use the standard McCrary Test to examine the continuity of the density function of birth year (McCrary, 2008).



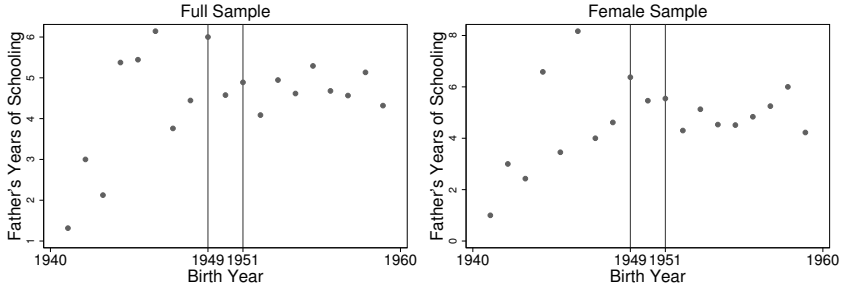
(a) Height



(b) Number of Siblings



(c) Mother's Education



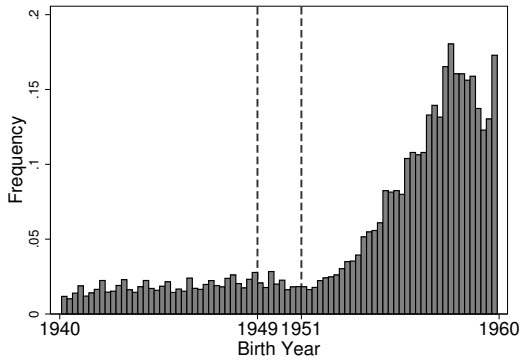
(d) Father's Education

Figure II.4: Covariates from CULS2001and CFPS2010

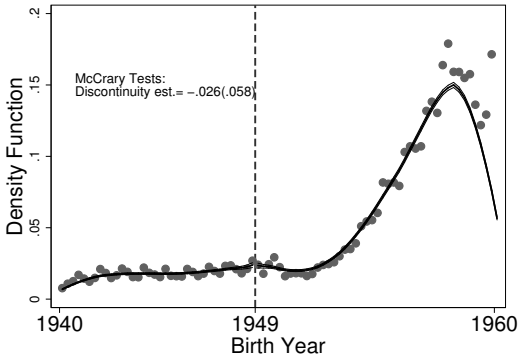
Note: The graph plots the mean value of pre-determined individual characteristics over cohorts for the full and the female sample respectively. It shows that the evolution of covariates is smooth at critical birth years, 1949 and 1951.

Source: CULS2001 and CFPS2010

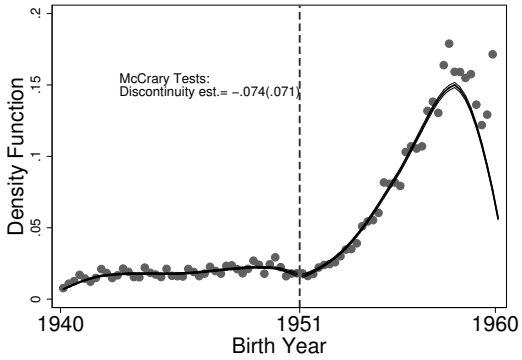
If children's birth date can be manipulated by their parents, the distribution of birth year would exhibit discontinuities at the critical points. In the absence of manipulation, the distribution of birth year is expected to be



(a) Histogram



(b) McCrary Density Test at Year=1949



(c) McCrary Density Test at Year=1951

Figure II.5: Mccrarry Test

Note: The graphs show the p.d.f of the running variable in the sample. The standard McCrary tests for the discontinuity of the density function of the running variables at two kinks. I report the log difference in height of the p.d.f. of the kinks.
Source: Census 1990

continuous. The histogram in Panel (a) of Figure II.5 shows that the density function of birth years is continuous at both critical values. The McCrary Tests presented in Panel (b) and (c) confirm that there are no signs of discontinuity in the density function.

5 Empirical Results

5.1 Main Results

The first-stage analysis evaluates the impact of the education shock on high school student's years of schooling utilizing data from China Urban Labor Survey (CULS) 2001 and China Family Panel Study (CFPS) 2010.[35] Because the sample size for each survey is small, I combine the two data sets to gain more statistical power. Table II.3 presents the estimation results based on Equation (7), where the outcome variable is years of schooling or the likelihood of receiving the standard length of education in high school. Column 1 and 3 show that the changes in the slope at both critical birth years are positive, with an estimated drop of around 40 percent at the first kink and an estimated increase of 30 percent at the second. These results suggest that the school closure negatively affected high school students' years of schooling and their chance of receiving the standard education.

[35] I do not to use the census data because the information on years of schooling at particular education level is not available in the census. The detailed description of the two micro-level data sets can be found in the appendix.

Table II.3: Regression kink design: first-stage results

DEP VAR	Year of Study at High School		Experience Shortened Length of Schooling	
	(1)	(2)	(3)	(4)
$\triangle Slope\mid_{birthyear=1949}$	0.390**	0.444***	0.310***	0.351***
	(0.151)	(0.165)	(0.112)	(0.121)
$\triangle Slope\mid_{birthyear=1951}$	0.254*	0.318**	0.248**	0.298***
	(0.153)	(0.158)	(0.107)	(0.114)
Controls		x		x
P-value from F-test	0.0254	0.0272	0.0235	0.0142
Obs	192	192	206	206

Note: This table reports coefficients from four separate regressions based on Equation 8, where the dependent variable is years of schooling in high school. Control variables include father and mother's education, number of siblings, number of children. P-values are from a test of joint significance of the coefficients α_1 and α_2. The bandwidth spans 11 years from 1945 and 1955. Similar results with alternative bandwidth sizes are reported in Table II.A.3. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS2010 and CULS2001

Column 2 and 4 display similar results with individual controls including father and mother's education, the number of siblings, the number of children, etc. For each specification, the p-values of the F-test of the joint significance of the changes in the slope are reported. The F-test rejects the hypothesis of zero slope changes at both critical birth years at 5% confidence level. These results corroborate that the high school shock severely disrupted the normal instructional activities.

Next, I proceed to examine whether the education shock affects one's sex-selective behavior. The second-step results presented in Column 1 of Table II.4 show that the changes in the slope of the sex selection function are statistically significant at critical birth years. Column 2 and 3 add control variables including the working status and the total number of children. Column 4 keeps only Han people in the sample. Varying the model specifications or restricting the sample to the major ethnic group gives similar results.

The changes in the slope of the sex selection function are not likely driven by a general time trend, because the function evolves in a distinct way that, for the time trend to be the confounder, there ought to be certain specific events affecting the time trend. In Section 5.3, I discuss two

potential events that might distort the time trend in a particular non-linear way. Another way to check the time trend is to look at the same cohorts with lower education levels. Since the education shock is high-school-specific, the primary school and the middle school graduates' sex selection functions are expected to evolve smoothly if the time trend does not lead to the results.[36] I thus use a falsification test to examine whether their sex selection functions have kinks at critical birth years. Panel (b) and (c) of Figure II.3 plot the sex selection function of primary and middle school graduates. As expected, there are no kinks at the critical birth years for these two groups. Column 5 and 6 of Table II.4 present the results of the formal test. The changes in the slope of primary or middle school graduates' sex selection function at the critical birth years are either statistically insignificant or wrong signed. These results further confirm that the general time trend is not likely to generate the non-linear pattern of high school students' sex selection function.

[36]The primary school and the middle school graduates hereafter refer to those whose education level is up to primary school and middle school respectively.

Table II.4: Regression kink design: main results

	High School				Middle School	Primary School
	(1)	(2)	(3)	(4)	(5)	(6)
$\triangle Slope\mid_{birthyear=1949}$	0.0240*	0.0237*	0.0212*	0.0222*	-0.0118**	-0.00871**
	(0.0132)	(0.0125)	(0.0126)	(0.0130)	(0.00499)	(0.00420)
$\triangle Slope\mid_{birthyear=1951}$	0.0369***	0.0275**	0.0262**	0.0268**	-0.00150	0.00587
	(0.0129)	(0.0122)	(0.0123)	(0.0127)	(0.00491)	(0.00442)
Number of Children		-0.153***	-0.144***	-0.148***	-0.124***	-0.0665***
		(0.00349)	(0.00363)	(0.00378)	(0.00154)	(0.00130)
Occupation Controls			x	x	x	x
Han Only				x		
P-value from F-test	0.00146	0.0698	0.0785	0.0872	0.000	0.000
Obs	21,485	21,485	21,485	20,539	80,872	42,366

Note: This table reports regression coefficients from seven separate regressions based on Equation (8), where the dependent variable is a dummy taking the value of one if "having a girl as the only child" and zero otherwise. The P-values are from a F test of joint significance of the coefficients α_1' and α_2'. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: Census1990

5.2 Results Using Alternative Measures

The measure of sex selection used in the above analysis is not an ideal one, as it may not only capture one's sex preference but also her quantity preference. In this section, I construct two alternative measures based on the birth order information elicited from a subsample. Specifically, to allow for the oldest child's sex to be observed, the census is trimmed if it satisfies one of the following conditions: 1) both the respondent and all her children were surveyed in the Census 1990 so that each child's age and sex can be observed; 2) the respondent has only one child; 3) the respondent's children are either all boys or all girls. Using this information, I can create two new measures for sex selection – they are two dummies, one taking the value of one if the oldest survived child is a girl, and another taking the value of one if the respondent has more than one child conditional on that the first-born child is a girl. One concern over the sample selection procedure is that it could make the whole sample not representative. For example, older children who do not live with their parents are less likely to appear in the survey. While this is true, the subsample still maintains about 95 percent of the observations for the cohorts of interest in the analysis (born between 1945 and 1955). Moreover, Table II.1 shows that the difference of individual characteristics between the subsample and the original one are relatively small.

These measures defined above are associated with two types of sex selection. The first one is related to the phenomenon of missing child, mostly a girl, through abandonment, maltreatment death, or infanticide. These practices have a long history in China as a way to regulate population growth and sex ratio (Lee, Feng and Campbell, 1994, Lavely and Wong, 1998, Lee and Feng, 2001). More recently, the sex-detection technology was introduced in China's major cities around the mid-1970s, which made it possible to use abortion to conduct sex selection (Chen, Li and Meng, 2013).[37] I use the new measure to examine whether lacking good-quality education boosts selection on the sex of the first child. Consistent with the main findings, the results presented in Column 1 of Table II.5 confirm with this type of behavior. The changes in the slope of the sex

[37]The bearing age of the urban high school females in the sample is mostly between 23 and 30. The birth year of their first child is largely distributed between 1972 and 1981. It is thus possible for them to use the sex-detection technology to conduct sex selection when the technology was popularized in China in the mid-1970s.

Table II.5: Regression kink design: alternative measures

DEP VAR	First Child is Female	Having Two or More Children	
	# Children$\geq$ 1	# Children$\geq$ 1	
Subsample		First Child is a Girl	First Child is a Boy
	(1)	(2)	(3)
$\triangle Slope\mid_{birthyear=1949}$	0.0452**	-0.0357*	-0.00456
	(0.0188)	(0.0195)	(0.0181)
$\triangle Slope\mid_{birthyear=1951}$	0.0383**	-0.0506***	0.00542
	(0.0176)	(0.0182)	(0.0166)
P-value from F-test	0.0556	0.00503	0.450
Obs	19,891	9,565	10,326

Note: The model specification is identical to that in Tabel 4, except that the dependent variable in Column (1) is a dummy taking the value of one if the oldest living child is female, and in Column (2) and (3) is a dummy taking the value of one if the respondent has two or more children. The sample used here contains the birth order information. In Column (1), the sample is further restricted to those who have at least one child, whereas in Column (2) the sample is furthermore restricted to those whose first child is a girl and in Column (3) those whose first child is a boy. P-values are from a F test of joint significance of changes in the slope of the sex selection function. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: Census 1990

selection function at the critical birth year are positive and statistically significant, suggesting that the education shock decreases the likelihood of the oldest survived child being a girl.

The second measure captures the type of sex selection that involves using the male-biased stopping rules. The family with son preference may keep having children until they eventually achieve a desired amount of sons. Prvovided the gender probability of natural birth constant, adopting the stopping rules can bias the aggregate sex ratio towards sons (Jensen, 2005, Barcellos, Carvalho and Lleras-Muney, 2014). To utilize the new measure, I restrict the sample to those whose first-born child is a girl and examine whether their propensity of having more children increased in response to the education shock. The model specification is identical to Equation (8), except that the dependent variable is a dummy taking the value of one if the respondent has more than two children. If the education shock induces parents to use the stopping rule, the slope of the

sex selection function is supposed to rise at the first kink and drop at the second one. Table II.5 presents the estimation results. Given that the first child is a girl, the changes in the slope are negative and statistically significant. In contrast, given that the first child is a boy, the shock impact on the probability of having more children is statistically insignificant, suggesting that the result is not driven by the fertility effect of education. In summary, the above evidence is consistent with the findings in the main analysis and suggests that both types of sex selection were adopted.

5.3 The Impact of the Education Shock on Sex Selection

The changes in the slope of the education and the sex selection function provide statistical evidence on the impact of the education shock on one's sex-selective behavior. To gauge the magnitude of the impact, I follow Landais (2015) to construct the estimates utilizing the identified slope changes. As demonstrated in Appendix C.2, the impact of the education shock on sex selection can be estimated by the ratio, $\delta = \frac{\alpha_i}{\alpha_i'}$, where α_i is the slope change identified from Equation (7) and α_i' is that identified from Equation (8). The dependent variable in Equation (8) is a dummy taking the value of one if the person experienced the shortened years of schooling. The standard error is computed with the bootstrap method.[38]

I estimate δ with three measures of sex selection, as each estimate bears its own economic meaning. Column 1 of Table II.6 shows that, at the first kink, experiencing the high school education shock decreases the probability of having a girl as the only child by roughly 6 percent. Column 2 and 3 suggest that the selection on the first child's sex increased by 13 percent and that the type of sex selection that involves the stopping rule increased by at least 8 percent in response to the education shock. The magnitude of the impacts is similar at the second kink, though the estimation for the third measure is less accurate because of the smaller sample size after sample restriction in the second-step analysis. Overall, these estimates show that the education shock had economically meaningful impacts on urban high-school graduates' sex selective behavior.

[38] Specifically, I first draw a bootstrap sample from the second-stage analysis and estimate α_i. Then, I draw a bootstrap sample of the first stage and estimate α_i'. After repeating the above steps for 1000 times, I estimate the standard errors and covariance of α_i and α_i'. Lastly, I compute the standard errors of δ using the delta method.

Table II.6: Estimates of the impact of the education shock on sex selection

Measure of Sex Selection	Having a Girl as the Only Child	First Child is a Girl	Two or More Children Given the 1st Child is a Girl
	(1)	(2)	(3)
$\delta_{birthyear=1949}$	-0.0617	-0.1312	0.0863
	(0.0444)	(0.0732)	(0.0648)
$\delta_{birthyear=1951}$	-0.0882	-0.1269	0.1521
	(0.0552)	(0.0779)	(0.0863)
Mean of Outcome Variable	0.270	0.481	0.387

Note: The scaled effect of the education shock on sex selection is calculated based on Equation (15). The standard errors in the parenthesis are computed with the bootstrap method.
Source: Census 1990, CULS 2001, and CFPS 2010

5.4 Threats to Identification

China was in a turbulent period when many catastrophic events occurred during the Cultural Revolution. This section discusses two important ones, the effect of which may confound the main findings.

5.4.1 The Red Guard Movement

As China's Cultural Revolution began, the Red Guard movement broke out in colleges and then spread out to secondary schools across all major cities. The participants of the movement include middle school, high school and college students, as well as young workers. Many of these young people were zealots of Mao Zedong and called upon by him to attack whoever they think as the "counter-revolutionaries" and the so-called "four olds", i.e. old habits, old customs, old culture, and old ideas (Israel, 1967, Walder, 2009, MacFarquhar and Schoenhals, 2009, Bai, 2014). The movement quickly evolved into a disastrous social turmoil. Red Guards seized the control of various local administrative and law enforcement agencies. Numerous intellectuals and government officials were charged and persecuted in the name of "counter-revolutionaries". Many high-ranking

figures in the factories and schools were harassed, arrested or even killed by Red Guards. Their properties were confiscated or disposed of arbitrarily if deemed as "four olds" or being ideologically incorrect.

Although the movement is considered to have far-reaching influences on the society, there are several reasons to believe that it does not affect high school graduates' sex-selective behavior. First, high school students played a relatively minor role as compared to college students and young workers. As documented by Xu (1999), high school students were mostly followers in groups led by older cohorts because they were inexperienced in political campaigns and less ideologically mature.

Second, since middle school students were involved as well, if the movement does affect sex selection, their sex selection function should also be affected and have similar kinks at their critical brith years. However, panel (b) of Figure II.3, which plots middle school graduates' sex selection function in the same range of birth year, shows that the function is smooth at both critical birth years. Similar results from a falsification test on these kinks are presented in Column 5 of Table II.4.[39]

Moreover, if being Red Guards matters, those high school students who graduated earlier (born before 1949) should also be affected by the movement, and their critical birth year should accordingly shift to the left of the axis. But this is inconsistent with what Panel (a) of Figure II.3 shows.

Furthermore, if anything, the Red Guard movement is more likely to weaken than to strengthen one's son preference. The radicalism propagated by the movement essentially opposes traditional values and culture. As a cultural prejudice that long exists in Chinese society, son preference is therefore more likely to be rejected than accepted by Red Guards. Anecdotal evidence from Chan (1985) in fact supports this speculation. A sample of Red Guards she interviewed unanimously considers their parents behind the times or politically backward, and that their older family members have little impacts on them.[40] If the Red Guard movement weakens son preference, the

[39]The implication of the test should be taken with caution, as the validity of the test hinges on the assumption that the Red Guard movement does not have heterogeneous effects. This assumption may fail, for example, if the movement only has effects on high school graduates.

[40]The relationship between the Red Guard movement and the objection of traditional values and culture may not be

eduction effect of sex selection would be underestimated.

5.4.2 The Sent-down Policy

Another key event related to high school students during the Cultural Revolution is the "Up to the mountains and down to the countryside" movement or the "sent-down" policy. Starting from the early 1960s, this policy was initiated to guide and organize graduates of urban secondary schools to voluntarily relocate to rural areas. The stated goals of the policy are to assist the development of disadvantaged areas and to develop "sent-down" youth's talents by working and living with local villagers. The scale of relocation grew at a steady pace in the first five to six years but drastically leveled up two years after the Cultural Revolution took place. After 1968, the "sent-down" policy evolved to be mandatory for all fresh graduates of urban secondary schools. Millions of youths were thereafter displaced to rural areas between 1968 and the mid of the 1970s (Zhou and Hou, 1999, Xie, Jiang and Greenman, 2008, Li, Rosenzweig and Zhang, 2010, Bernstein, 1977). Because son preference is more prevalent in rural areas, the "sent-down" youth's son preference might be influenced by villagers through their daily interaction. If so, being "sent-down" may boost sex selection of urban high school graduates and confound the education effect.

The evidence displayed in Figure II.A.1, however, contradicts this conjecture. Panel (a) plots the total number of urban youth sent down to the rural area each year using data from China's Labor and Wage Statistics, 1949-1985. The plot shows two waves of the sent-down movement – the first one culminates at 1969 while the second one around 1975. Panel (b) plots the likelihood of ever being sent down at any given birth year with data from CFPS 2010 and CULS 2010. Correspondingly, the cohorts born in 1950 and 1956 were most likely to be sent down, as most of them would have graduated from high schools by 1969 and 1975 when they turned 19. If the sent-down experience affects sex selection, the evolution of sex selection should resemble the pattern in Panel (b) and have kinks at the birth years 1950 and 1956. However, the "timing" of

causal. The positive correlation between the two documented by Chan (1985) might be caused by self-selection. The decision to take part in the movement can be self-determined – one with opposing views against sex selection may self-select into Red Guards.

being sent down does not match the slope change of the sex selection function at the critical birth years observed in Panel (a) of Figure II.3. In a more formal setting, I test the slope changes of the function of the sent-down experience at critical birth years with the same setting as in Equation (8). Consistent with the graphical evidence, the estimated results in Column (5) of Table II.2 show that there are no noticeable slope changes at neither of the critical birth years.

5.5 Other Robustness Checks

The main indicator of sex selection used in the analysis is a dummy taking the value of one if the respondent "has a girl as her only child". One concern over using the measure is that it captures not only mother's sex preference but also her quantity preference. A woman may have a girl as her only child either because she prefers a girl or because she prefers a boy and has an even stronger preference of one child. If the latter incentive dominates, the measure would fail to distinguish the education effect on her sex-selective behavior from that on her fertility choice – more years of schooling may have a negative effect on fertility (Black, Devereux and Salvanes, 2008, Cygan-Rehm and Maeder, 2013). I use an empirical model with the same specification as in Equation (8) to evaluate the education effect on fertility. Results from Column 1 of Table II.A.1 show that the fertility outcome is mildly affected by the education shock and the impact is only statistically significant at the second critical birth year, which suggests a weak relationship between the education shock and the fertility. Nevertheless, I control the number of children in all the regressions of the reduced-form analysis to ensure that the quantity preference does not confound the result. Another way to check the quantity preference is to replace the measure, "having a girl as the only child", with "having a boy as the only child", and examine how the measure responses to the education shock. If the shock makes having one child more likely, then kinks should also appear at the critical birth years of the function of the new measure. However, the results presented in Column 2 of Table II.A.1 show that this is not the case – both coefficients capturing changes in the slope are statistically insignificant. Overall, these pieces of evidence indicate that the quantity preference is not likely to play a strong role here.

The first-stage analysis uses data from the survey of CFPS 2010 and CULS 2001. I combine these two samples to gain statistical power because the sample size for each survey is small after sample restriction. One concern over combining these two data sets is that they have different sample frames – CFPS is a national survey and CULS 2001 a survey of five cities. I re-weight the sample and re-estimate the coefficients of Equation (7) using the inverse probability weighting method. The weights are calculated based on the city population and the size of the original sample. Table II.A.2 shows that the estimation results using this method are quite similar to the main results presented from Table II.3.

Another sensitivity check is to use alternative bandwidths to estimate the slope changes in Equation (7). The bandwidth used in the baseline specification spans 11 years between 1945 and 1955. In this exercise, I extend the bandwidths to 13, 15, and 17 years. Table II.A.3 show that the results are consistent across different bandwidth sizes. I do not use even larger bandwidths, as doing so would involve using older cohorts born during China's Great Famine (1958–60), which may have an impact on sex preference.

An important assumption made when using the restricted sample of high school students is that the quality of enrolled students remains stable over time. If this assumption fails, it would be difficult to differentiate the impact of the education shock from the change in the quality of high school students. I herein discuss one scenario that may alter the quality of high school graduates. As documented by Zhou (1997), the criteria for enrollment after the education shock was adjusted with more weights given to students' family's background than to their academic achievement. Students with lower social economic background were more likely to be admitted, even though they may not be academically qualified. If it is common that these students had stronger son preference, then sex selection would rise in the absence of the education shock. One way to check whether the enrollment criteria has an effect on high school graduates' overall quality is to examine how their pre-determined characteristics evolve over cohorts. For the quality of high school students to be the confounder, their pre-determined characteristics ought to change with a kinky pattern for cohorts born at critical birth years. Figure II.4, however, shows that the height,

the number of siblings, and parents' education all have a smooth transition between 1949 and 1951. Results from more formal tests presented in Table II.2 also confirm with this conclusion – almost all the estimated kinks on these covariates are insignificant.

6 How Education Affects Sex Selection

6.1 Income Channel

One channel that education can affect sex selection is through its impact on economic conditions. Fewer years of education reduce future income, and lower income may boost sex selection for males if sons are inferior goods. For example, poorer family may want more sons because they are less costly to bring up, and sons can give their parents better old-age support than daughters when the family is lack of savings. While there is a lack of systematic evidence on whether sons are normal or inferior goods, a recent study by Almond, Li and Zhang (2014) shows that the rise of household income induced by the land reform increased rather than reduced sex selection, suggesting that sons are more likely normal goods in China. Additionally, I examine whether the education shock affects the future income using a model with the identifical specification as Equation (8). This analysis uses CHIPS 1988 instead of Census 1990 because the former has more detailed information on earnings and various types of income. The outcome variables of interests include one's housing condition, meat consumption, and various forms of wages. If the high school shock reduces one's future income, the slope changes of income function at critical years are expected to be positive. To the contrary, Table II.7 shows that the impact of education shock on housing, food, and wage are either wrong-signed or insignificant at the critical birth years.

There are two possible reasons why the education shock has little effects on one's future economic conditions. First, since the study only focuses on the group of people with high school degree, the variation of education is limited. In the abscence of the signaling effect, the impact of education on income is small compared to studies using variation across

Table II.7: The impact of high school education shock on economic conditions

	$\triangle Slope\,\vert_{birth\,year=1949}$		$\triangle Slope\,\vert_{birth\,year=1951}$		P-value from F-test	Observations
Outcome variables	Effect	SE	Effect	SE		
Housing						
# Rooms	-0.695*	(0.410)	0.659	(0.759)	0.153	422
Room Area	-5.536*	(3.168)	-0.665	(1.976)	0.205	419
Public Housing	-0.0251	(0.0553)	-0.0191	(0.0414)	0.860	422
Food Consumption						
Pork	-1.224	(2.761)	-1.440	(2.410)	0.827	415
Beef	1.041	(0.742)	0.348	(0.628)	0.374	379
Poultry	-1.693	(1.719)	-4.398***	(1.630)	0.0252	375
Fish	-1.084	(1.403)	-2.059	(1.791)	0.504	392
Wages						
Basic Wage	-0.647	(5.185)	-3.727	(5.409)	0.700	417
Basic & Float Wage	-3.301	(6.118)	-3.831	(6.007)	0.808	417
Basic, Float Wage, & Subsidy	0.167	(3.616)	-1.775	(3.978)	0.869	417

Note: This table reports regression coefficients from ten separate regressions based on Equation (8). The P-values are from a F test of joint significance of the change in the slopes. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CHIPS 1988

education levels.[41] The small income effect of education accords with the empirical findings of Ebel and Hu (2016) that credentials seem more relevant than the length of schooling in determining one's future earnings in China. Another interpretation of the negligible education effect of income is that the education shock may have a general equilibrium effect, which partially offset the direct shock effect on individual's income (Khanna, 2015).

6.2 Preference Channel

To examine whether high school education affects sex-selective behavior through its effect on sex preference, I use data from the Family Module of China General Social Survey (CGSS) 2006. The survey asks each respondent's son preference in the following question: "If only allowed to have one child, do you prefer a boy, a girl, or are you indifferent?". Based on this question, son preference is defined as a dummy taking a value of one if the respondent answers "Boy" and zero otherwise.

I estimate the effect of education on son preference using a simple Ordinary Least Square (OLS) strategy.[42] One limitation of OLS is that the unobserved individual characteristics may affect both educational attainment and son preference and bias the esimated effects, so the results estimated by OLS should be taken with caution. That said, this type of bias is minimized in this setting because the data contain a comprehensive set of individual and family controls, including age, marital status, household income, rural/urban residence, the number and the sex of children, religion, ethnicity, parents' education and occupation, personal sent-down and military service experience, etc.

In Table II.8, the OLS estimates from Column 1 suggest that one more year of schooling on average is associated with one percent lower the stated son preference. Column 2-5 present

[41] As pointed out in Section 2, high school students still get their credentials even though they lost one to three years of schooling.

[42] To be consistent with the previous analysis, the better empirical framework to evaluate the channel would be Regression Kink Design, but the small sample size of CGSS 2006 makes it too demanding to implement the strategy. The data from the individual survey of CGSS 2006 have 10000 observations. Since only household head's was asked about their son preference in CGSS 2006, the observations of the data that can be used is reduced to 2562. After further restriction to urban high-school females, the sample left is less than 100 observations.

Table II.8: The effect of education on son preference

DEP VAR = Son Preference	(1)	(2)	(3)	(4)	(5)
Years of Education	-0.0180***	-0.0113***	-0.0112***	-0.0104***	-0.0101***
	(0.00242)	(0.00294)	(0.00296)	(0.00312)	(0.00313)
Age		-0.00300***	-0.00320***	-0.00324***	-0.00313***
		(0.000896)	(0.000913)	(0.00102)	(0.00105)
Male		0.0614***	0.0625***	0.0635***	0.0673***
		(0.0169)	(0.0170)	(0.0175)	(0.0178)
Married		0.0393*	0.0398*	0.0345	0.0359
		(0.0222)	(0.0223)	(0.0243)	(0.0244)
Log(Household Income)		0.00388	0.00393	0.00471	0.00498
		(0.00398)	(0.00400)	(0.00414)	(0.00413)
Rural		0.0737***	0.0731***	0.0771***	-0.119
		(0.0229)	(0.0229)	(0.0250)	(0.188)
Basic Controls		x	x	x	x
# of Sisters and Brothers			x	x	x
Detailed Family Background				x	x
Early Life Experience					x
Mean of Outcome Variable	0.2589	0.2589	0.2589	0.2589	0.2589
Obs	2,562	2,562	2,562	2,562	2,562

Note: This table reports regression coefficients from OLS estimation where the dependent variable is son preference. Basic controls include number of children, number of daughters, religion, and ethnicity dummy. Detailed family background includes parents' education, occupation and rural/urban residence. Early life experiences consist of sent-down and military service experience. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CGSS 2006

similar results with individual and family controls. Rural residence and the male identity are strong predictors of son preference. These findings are consistent with the previous literature (Dahl and Moretti, 2008). The magnitude of the education effect does not differ much when more control variables added.

There are two channels through which high school education can affect sex preference. First, the morals and values imparted by teachers, especially in liberal arts courses, may moderate the gender prejudice. If this channel holds, the education shock is expected to adversely affect the high school students' cognition in the arts and humanities. To empirically evaluate this channel, I

(a) Histogram

Figure II.6: Cognitive test scores

Note: The graph displays how urban high school graudates' cognitive skills change over birth cohorts. Each dot represents the mean test scores of word test and math test at the given birth year.
Source: CFPS 2010

use data from CFPS 2010, which measures one's cognitive abilities by a word and a math test. In the word test, the interviewer presents a list of nouns to the respondent, who is then asked to recall and read these nouns as many as possible. The math test asks the respondent to solve a set of math calculation questions. The difficulty level of the two tests is adjusted corresponding to the respondent's education. I use standard z-score of the two test results as the proxy for one's knowledge in the arts and humanities and her knowledge in math and science respectively. Figure II.6 plots the mean of scores of the two tests over cohorts. As can be seen from the graph, although the value has large variance due to the small sample size, the word test score drops steeply to a lower level for cohorts born after the critical birth year. In comparison, the math test score barely changes at the critical birth years. These findings are consistent with the conjecture that the education shock has larger impacts on high school students' knowledge in the arts and humanities than that in the math and science.

Second, education may affect one's sex preference indirectly through peer effects in high

schools. One may be influenced by her classmates, friends, teachers, or whoever holds the value or the ethics against sex selection. If such effect exists, more years of schooling in high schools may moderate one's son-biased sex preference, while missing high school education will make her lose this opportunity.

The effect of education on sex preference can be magnified because of the positive externality of education. Some societies have the cultural tradition of son preference because the patriarchal lineage places sons in the center of the kinship system. In these societies, women are often under pressure to have a son, because only sons are entitled to inherit the family name, and the family history is recorded through male's lineage. Women's pressure of having sons can be even larger when her husband is the only son of his family. It is not uncommon that women who are unable to deliver a son sometimes are ridiculed as infertile (Gupta et al., 2003). This type of peer pressure may be alleviated when the whole society becomes more civilized and more open-minded towards women with only female offsprings with more years of good-quality education.

7 Concluding Remarks

The development experience from several East Asian Countries suggests that the economic progress does not necessarily result in the fall of sex selection (Banister, 2004). To make sense of the phenomenon, it is essential to comprehend how non-economic factors contribute to the persistence of sex-selective behavior in modern society. This chapter takes one of the first step towards this direction by studying the effect of education on one's sex-selective behavior. The high school closure in the late 1960s of urban China provides a unique opportunity to look into this issue. To identify a causal effect, I use the Regression Kink Design to disentangle the effect of education on sex selection from that of unobserved variables such as family background or genes. The results show that the interrupted high school education nurtured more sex-selective behaviors. I also find that education affects sex selection more likely through its impact on one's sex preference rather than her income.

These findings convey an important message to developing countries concerned with the biased sex ratio due to sex selection, such as China and India. Both of these two countries had made considerable progress towards universalizing basic education. Yet, the findings of this chapter indicate that the benefit of universalizing higher level of education is underestimated with regard to restraining sex selection, and should be taken into account in the future policy design and evaluation.

One limitation of the study is that it only focuses on a subgroup of people with urban high school degree. Hence, the estimated effect of education may not generalize to other education levels. As mentioned earlier, more years of schooling at a lower level may even increase rather than decrease sex selection. To what extent and how primary and middle school education affect one's sex-selective behavior is a relevant topic to explore in the future. More evidence in this regard is needed to inform population and education policies in .

Chapter III Compulsory Schooling Law, Parental Education, and Early Childhood Development

1 Introduction

Skills and abilities formed in the early life, such as reasoning, language, and problem solving, are critical in the process of human development. Studies have shown that early childhood development (ECD) can facilitate future skill acquisitions in a complementary and dynamic way, and that the abscence of the early childhood development and education can lead to underinvestment in human capital in later life and impede one's future achievement (Cunha et al., 2006, Cunha, Heckman and Schennach, 2010). This observation has led to early education policies in developed countries, such as pre-K and Head Start programs in US, in an effort to increase children's access to pres-chool education. However, these policies have not received adequate attentions in developing countries because of their high expenses and overlooked social benefits, which leaves children's early education predominantly determined by their parents. How well can home environment facilitate children's early-life skill formation? Understanding this question and quantifying its effect can provide useful information for policies aiming to reduce the possible opportunity inequality at the begining of life. To shed light on this question, I specifically study the effect of parents' education on children's ECD including their age of walking, speaking, counting, and independence in China.

The focus on China is out of both practical and methodological consierations. Firstly, the low level of children's cognitive skills in rural China has raised concerns over their future education achievement (Luo et al., 2012, Yue et al., 2016, Li et al., 2017). Approximately 39 percent of infants and toddlers (ages 0 to 3) that are born and raised in rural Chinese villages exhibit cognitive or psychomotor delay (Li et al., 2017). The problem is mainly due to the absence of modern

parenting; only about one-third of caregivers read, talk, sing, or play with children in ways that have been shown to be important for cognitive development.

Secondly, one common perception about children's development is that those with better educated parents naturally learn and acquire their skills earlier. However, such relationship may be spurious, because unobservables such as family background or genes, can simultaneously affect parents' education and the stage at which children develop their skills. The estimation with OLS strategy is biased because unobservalbes cannot be easily controlled. The challege of the research is to find a credible identification strategy to validate the causal relationship between parent's education and children's ECD. To identify a causal effect, I exploit a quasi-natural experiment which exogenously affect parents' education: China's promulgation of Compulsory Schooling Law (CSL) in 1986.

China is one of the first developing countries to implement the CSL. This policy introduced nine-year compulsory schooling for primary and lower secondary education, waived the tuition fee for schooling, and imposed penalties to hire minors. The first question to consider in this chapter is whether this policy effectively elevates the education level by incentivizing parents to send their children to school. The effecitiveness of the policy would be questionable, for example, if there are widespead dropouts and truancy. To evaluate its success, I exploit the timing variation of the introduction of the CSL across provinces—although the law was promulgated nationally, its implementation was carried out sequentially—and use the difference-in-difference strategy to study its effect on educational attainment, cognitive skills, fertility outcomes, etc. The evidence show that the CSL considerably increased the immidiate generation's years of schooling and cognitive skills, and reduced the fertility and the incidences of teenage birth.

China's CSL provides a unique opportunity to study its international impact. With over thirty years of timespan, the policy lasts long enough for researchers to observe its intergenerational outcomes. I use the policy shock as the instrument for parents' education and proceed to investigate with the Two Stage Least Square (2SLS) strategy whether more years of parents' education improve next generatios' early-life skill development. The empirical results show that more years of

mother's education lower children's age of developing their skills of walking, speaking, counting, and getting independent, while more years of father's education are associated with children's earlier age to develop skills of speaking and independence. I then examine theree possible channels through which the increase in parents' years of schooling can affect children's early-life skills. Results show that the effect of parents' education is more likely to work through its impact on parents' nurturing style than on family income or on changes of fertility pattern.

The benefits of compulsory schooling have been examined by a large body of research. Evidence shows that more years of education induced by the CSL not only affect future earnings but also a wide range of non-pecuniary outcomes including health, crime, fertility, and the sense of citizenship (Acemoglu and Angrist (2001); Lochner and Moretti (2004); Milligan, Moretti and Oreopoulos (2004); Lleras-Muney (2005); Oreopoulos (2006); Black, Devereux and Salvanes (2008); Huang (2015)). This study is different from this literature in two ways. First, it is among the first to address the intergernational impacts of compulsory schooling on the next generation's early-stage skill development. Second, for those studies also focusing on the intergenerational impacts of the CSL, e.g. on children's education or health (Black, Devereux and Salvanes (2005); Oreopoulos, Page and Stevens (2006); Lindeboom, Llena-Nozal and van Der Klaauw (2009)), one limitation of these studies is that they are mostly based on developed countries, where there is a relatively high pre-CSL education level, which leaves less room for the education reform to leverage. Consequently, these studies generally find zero to modest intergeneraional effects of the CSL, because the education shock as the instrument is lacking of statistical power.[43] Comapred with these earlier studies, the increase of years of education induced by China's CSL is significantly larger, due to its unbalanced education development in the 1980s.

This chapter also adds to the literature on the impacts of home environment on children's development (Behrman and Rosenzweig (2002); (Carneiro, LÃžken and Salvanes, 2011); Zhang et al. (2014)). One feature that distinguishes this study from the earlier research is that it points out

[43] Another possible reason why they find a modest intergenerational effect is that the wildely available pre-school service and other social programs in developed countries can partially alleviate the disadvantage of children with under-educated parents.

the stage when parents' education starts to exert influence on their children. Making this distinction is important, as it helps improving the understanding of the intergenerational transmission of education inequality.

2 Policy Background: The Compulsory Schooling Law

Chinese government began a series of reforms in the 1980s after recovering from the impact of the Cultural Revolution. The *Decision on Several Issues Concerned with Primary Education* formulated in 1980 stressed the importance of universalizing the primary education as a predominant task. Although the state is committed to assist the universalization of primary education in the most disadvantaged areas, the policy states that the development of education could be allowed to vary across regions depending on their economic strength and cultrural status.

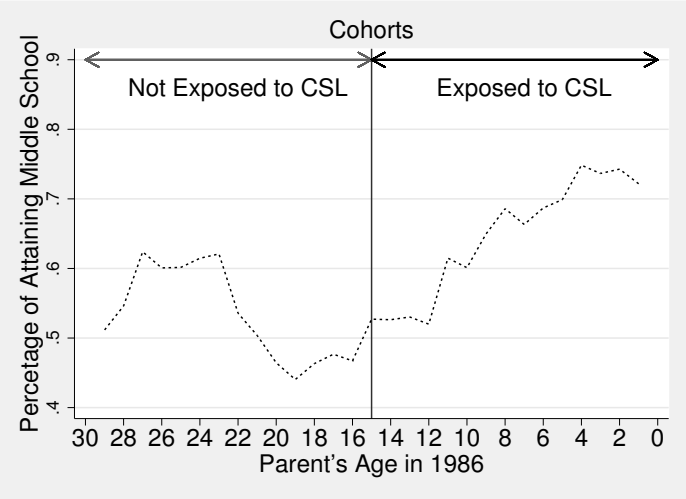


Figure III.1: Trend of Education

Note: The graph displays the percentage of population attaining middle school by parent's age. 15 years old is the cutoff age determining the exposure of the CSL shock.
Source: CFPS 2010

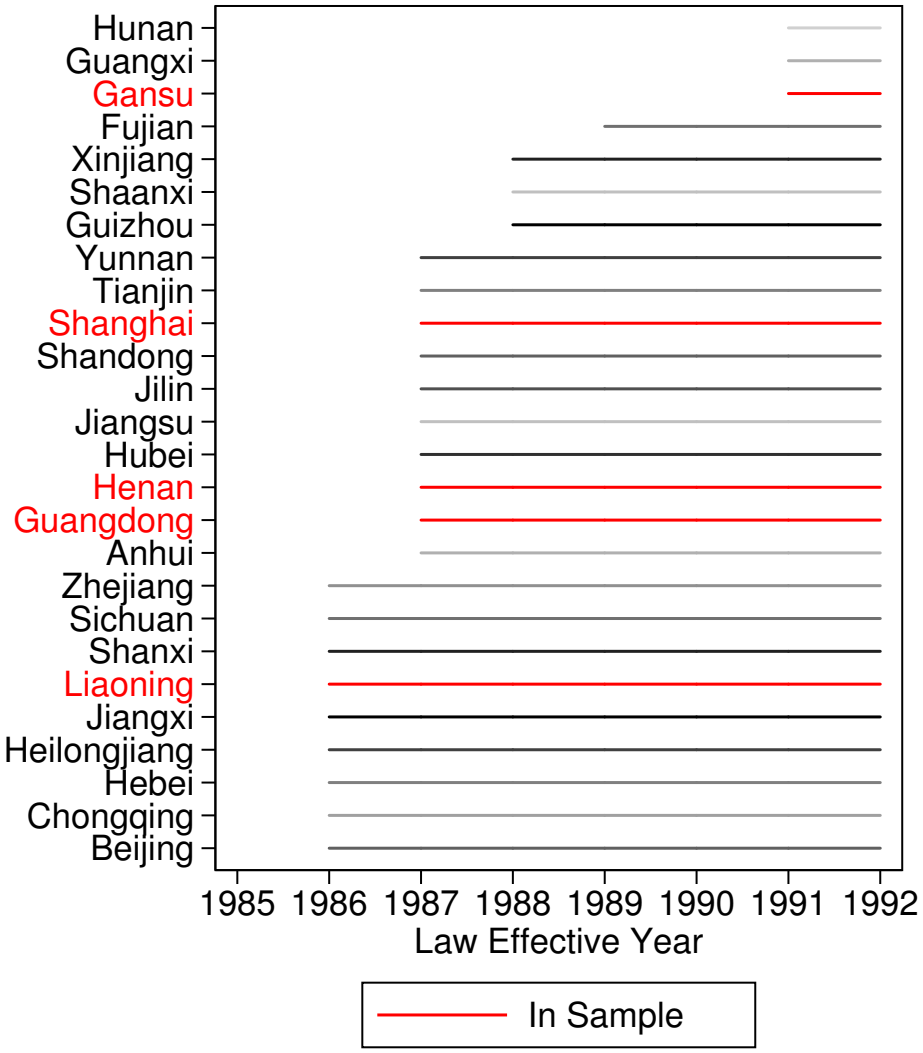


Figure III.2: CSL Effective Year, by Province

Note: The graph shows the timing variation of the CSL implementation across provinces. The five provinces highlighted in red are included in the sample.
Source: Huang (2015)

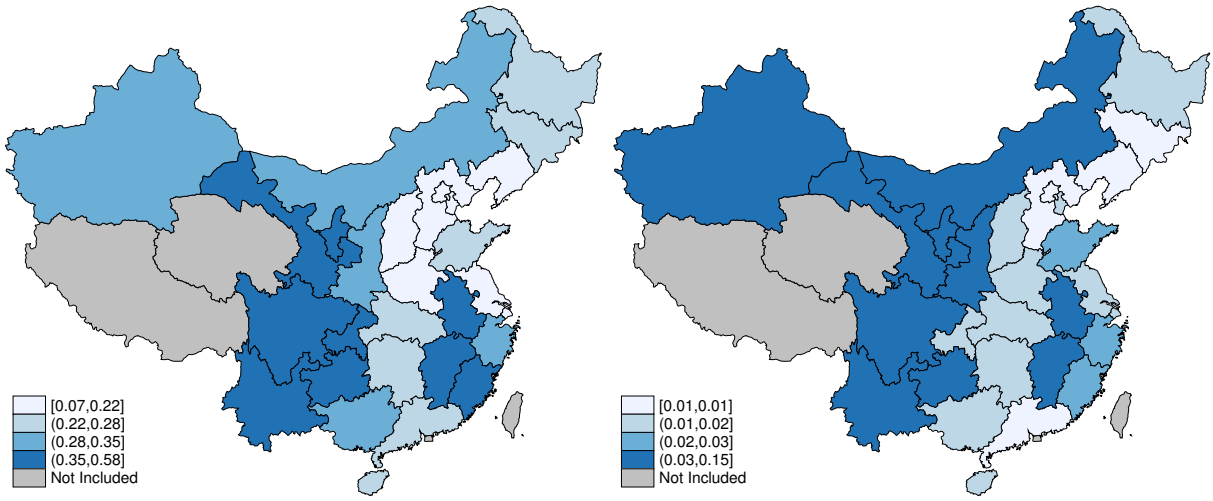
The government promulgated the Compulsory Schooling Law in 1986, which established rights and obligations of individuals and governments with respect to compulsory education. The law officially made nine years of schooling compulsory throughout China. The tuition fee was waived for primary schools and middle schools (Fang et al., 2012, Rawlings, 2014). It became unlawful to

employ children who are in their compulsory schooling years. Local governments were allowed to collect education taxes to finance compulsory education. Although the policy was called out by the central government in 1986, the exact date of implementation is allowed to vary across provinces. Figure **??** show the law effective year for provinces surveyed in the CFPS 2010 data, which shows great variation of the implementation date. This timing variation is essential for the Difference-In-Difference strategy to identify its policy impact on parent's education (Huang, 2015).

This policy was supported and advised by World bank. The government was able to mobilize resources in different ways to support the policy. By relying on community resources and the support of international aid agencies, the government has been able to implement many projects targeting poor rural areas such as the Basic Education Development Project in Poor Areas, National Compulsory Education Program for Poor Areas (NCEP), Rural-Urban Aid Program, Renovation of Dilapidated School Buildings in Poverty Areas, Project of Hope, and Spring Bud Scheme (Tiedao et al., 2004).

Before the CSL was promulgated, the education gap among provinces in China is huge. Figure III.3 shows the distribution of the undereducated people as a percentage of the total population across provinces. Those with the highest fraction of undereducated population (with the deeper color) are mostly concentrated in inland provinces, while those with lowest proportion of undereducated people (represented by the lighter color) in the coastal area. The unbalanced education development across provinces provides great room for the policy to lift up the education level in the backward areas. After the CSL was promulgated, there is a big improvement in the lower level of education – as shown in Figure III.4, the proportion of undereducated population shrank in most of the provinces after the CSL.[44]

[44]The pre-CSL population are those born between 1961 and 1971 who had been out of middle school when the CSL was promulgated in 1986, while the post-CSL population is defined as those born between 1974 and 1984 who were exposed to and would benfit from the CSL. The proportion change is the difference between the pre-CSL undereducated proportion of population and the post-CSL undereducated proportion.



(a) Pre-CSL Proportion of Population up to Primary Education

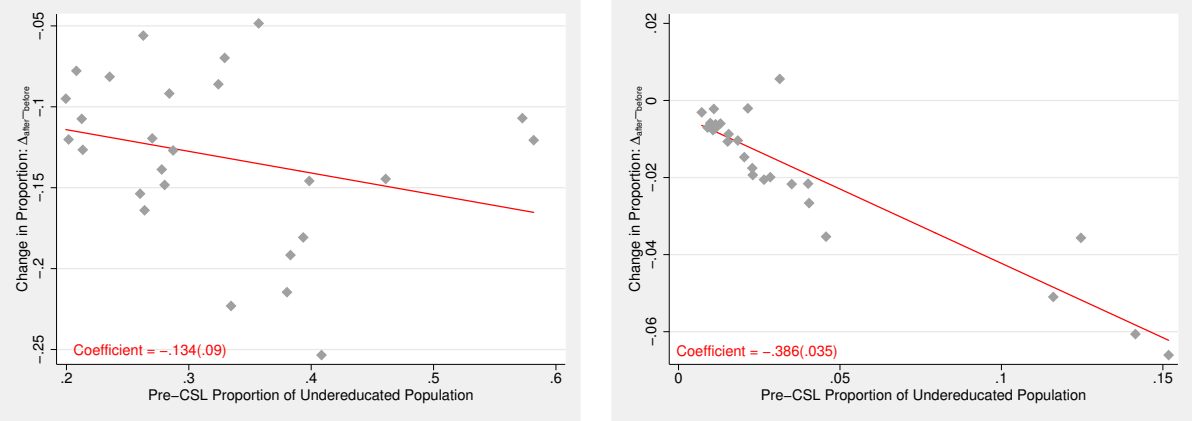
(b) Pre-CSL Proportion of Illiterate Population

Figure III.3: The Pre-CSL Proportion of Population up to Primary Education

Note: Panel (a) plots the proportion of population with less than 6 years of education before the CSL was enacted. Panel (b) shows the pre-CSL distribution of the illiterate as a percentage of the total population. The illiterate population is defined as those with one year of education or lower.
Source: Census 2000

In Panel (a), the undereducated population is defined as those with primary-school education or lower. It shows that the higher the initial undereducated proportion is, the larger magnitude this proportion would drop after the CSL. Panel (b) shows a similar pattern with the undereducated people being defined as those with one year of education or lower. Both of tshese graphs show that there is a negative relationship between the initial proportion of undereducated population and its absolute change in response to the policy shock, indicating that the CSL decrease the education inequality at the lower level across provinces. [45]

[45]Note that these graphs only provide suggestive evidence of the effect of the CSL on the covergence of education. To interpret this relationship as causal, one needs to use more sophisticated methods to control for latent factors that could affect the initial condition and the improvment of education.



(a) (b)

Figure III.4: Convergence of Education at the Lower Level

Note: The graphs show the relationship between the initial proportion of undereducated population and the change in the proportion in response to the CSL. The undereducated popluation is defined as those with primary-school education or lower in Panel (a) and with one year of education or lower in Panel (b). Beijing, Shanghai, Tianjin, Tibet, Qinghai, and Taiwan are not included in the sample. The provinces highlighted with red are those included in the sample.
Source: Census 2000

3 Data and Sample

3.1 Data

The data used in the chapter come from an ongoing longitudinal survey, China Family Panel Study (CFPS). It is a a large-scale integrated socio-economic and health survey launched jointly by Renmin University of China and the Hong Kong University of Science and Technology in 2003. The CFPS uses a multistage probability proportional to size (PPS) sampling method. The sample covers 25 provinces which represents 95 percent of the total population. [46] The first wave in 2010 surveys 14960 families and 42590 individuals. One feature of this survey is that it records detailed

[46]The CFPS uses two independent sampling frames for five selected provinces and other twenty provinces with more weights given to the five provinces. The oversampling of five provinces may make the statistical inference biased when analyzing the national data. I discuss the potential sampling frame issues that may confound the main findings in the section of the robustness check.

information on individual's immediate family member. The CFPS also comes with a children survey whose age below 16 years old collecting information on children's health, education, skills, etc. Table II.1 presents the summary statistics of both the adult sample and the children sample.

Table III.1: Summary Statistics

Variable	N	Mean	S.D.	Min	Max
	Panel A: Adult Sample				
Age	16528	45.8	16.7	16	109
Years of Education	16521	6.56	5.03	0	22
Math Test Score	16528	10.4	6.9	-8	24
Word Test Score	16528	17.6	10.9	-8	34
Number of Children	16528	1.75	1.36	0	10
Teenage Birth	16528	.0218	.146	0	1
Urban/Rural	16528	.484	.5	0	1
Monthly Income	15505	1094	3006	0	150000
Yearly Income	15536	9906	19663	0	600000
	Panel B: Children Sample				
Children's Age	3661	9.15	3.87	3	15
Gender	3661	.526	.499	0	1
Father's Education	3590	7.25	4.36	0	22
Mother's Education	3570	5.85	4.72	0	19
Father's Age	3626	37.5	6.12	22	75
Mother's Age	3608	35.6	5.85	20	72
Urban/Rural	3661	.37	.483	0	1
Outcomes of Interest					
Age of Walking (Month)	3630	14.8	5.66	6	72
Age of Speaking (Month)	3629	21.9	9.08	1	72
Age of Counting (Month)	3420	36.8	15.5	6	72
Age of Urinating Ind. (Month)	3504	35.5	13.5	6	120

Source: CFPS 2010

CFPS has two independent sampling frames. The survey has two sets of provinces, five big ones, Shanghai, Henan, Liaoning, Gansu, Guangdong, and twenty small ones, with two independent sampling frames. Each set of provinces targets 8000 households, so the five big provinces are oversampled as compared to the other small province (Xie and Hu, 2014). This type of sampling design calls for caution when making statistical inference using the national sample, as it overly represents population of the big provinces. Despite the inconsistent sampling frame, there are two

ways to use the national sample. The first way is to use the sampling weight offered by CFPS to reweight the regression. Secondly, the CFPS team also resampled from the five big provinces and the rest twenty small provinces to make a single consistent sampling frame (by reducing the observation from the five big provinces).

In the main analysis, I only keep the sample with the consistent sampling frame by restricting the sample to five provinces, Liaoning, Henan, Guangdong, Gansu, and Shanghai.[47] I discuss more details about the sample in Section 4.1.

3.2 Measurement of ECD

To measure children's early-life skills, I use four types of proxies. They are children's monthly age of walking, speaking, counting, and being able to independently urinate, that is, when the child start to wear and take off his/her trousers indpendently in order to urinate (this measure concerns more about parents' attitude and awareness of training their children). These are retrospective data by psarents, so there might be concerns that parents might misrember the exact data. Using yearly age as the measure can increase the precision of children's age starting to grasp these skills, but the downside of doing so is that the monthly age has much more variation at the margin. To alleviate this concern, I use yearly measure, which is less likely to misremember, although the downside of using this measure is that it has less variation within each yearly age. In the robustness check, I show that basic results still hold with the yearly measure.

3.3 Children's Sample

For children whose age below 10 years old, the custodian answered the question (grandparents answer these questions for children's parents if they are absent). For children age between 10 and 15, custodian answered most of the question, while the child should also answer some questions on his/her own. CFPS2010 records detailed immediate family information. However, in some cases,

[47] Note that Shanghai is that it is a city with special policy in the 1980s, which may confound the effect of the CSL, but including it does not change the main results. in the robustness check, dropping Shanghai does not affect the main results.

parents are not surveyed.

For children's outcomes, I use data from CFPS 2010 and 2012. The sample consists of children aged between 3 and 16 in 2010, whose early life skills can be observed from the data, because the survey only ask children with age older than 3 so that they are old enough for me to observe how their children develop their early life skills. For children that are already 3 or 4 or 5 years old but still unable to count or urinate indpendently, we resort to CFPS 2012 to keep track of their ability development, because CFPS is a panel survey and we can keep track the information of each individual. Total observations in the children's sample are around 8900, after restricting the age, there are 7100 observations left.

Despite this effort, there are still considerable missing values for children with age between 3 and 5, because these children have not been able to develop their skills in counting and being indpendent. Therefore, I raise the cutoff age of sample used for evaluating the effect of parent's education on children's abiliuty of counting and being independent. As a result, the sample for evaluating children's ability of walking and speaking is slightly larger than.

For walking and speaking, the probability density distribution plotted in Figure III.5 show that most of children achieved the skills before 3 years old. Given that walking and speaking are skills that can be grasped before 3 years old, the sample kept is those children between 3 and 16 so that children's skill-achieving age can be observed. For counting and being indpendent, these skills generally developed at a later age. The average age of grasping these skills are around 3 years old on average (See Table II.1). Therefore, to evaluate children's ability of counting and being independent, I use sample of children between 5 and 15 years old. [48].

Sample weight is used in the main analysis, because only parents with offsprings (3 -16 years old) are included in the sample, parents might be subject to differential selection into the treatment and control group. Although parents are initially selected from the random sample, if only keeping these children, the sample is truncated in the sense that these children's parents' education is not random any more. Because some parents are more likely to be dropped out as they do not have

[48]The cutoff age 5 is not sensitive to the choice. The basic results still hold for older age. But there is a cost of statistical power, so I use age 5 instead of children with older age.

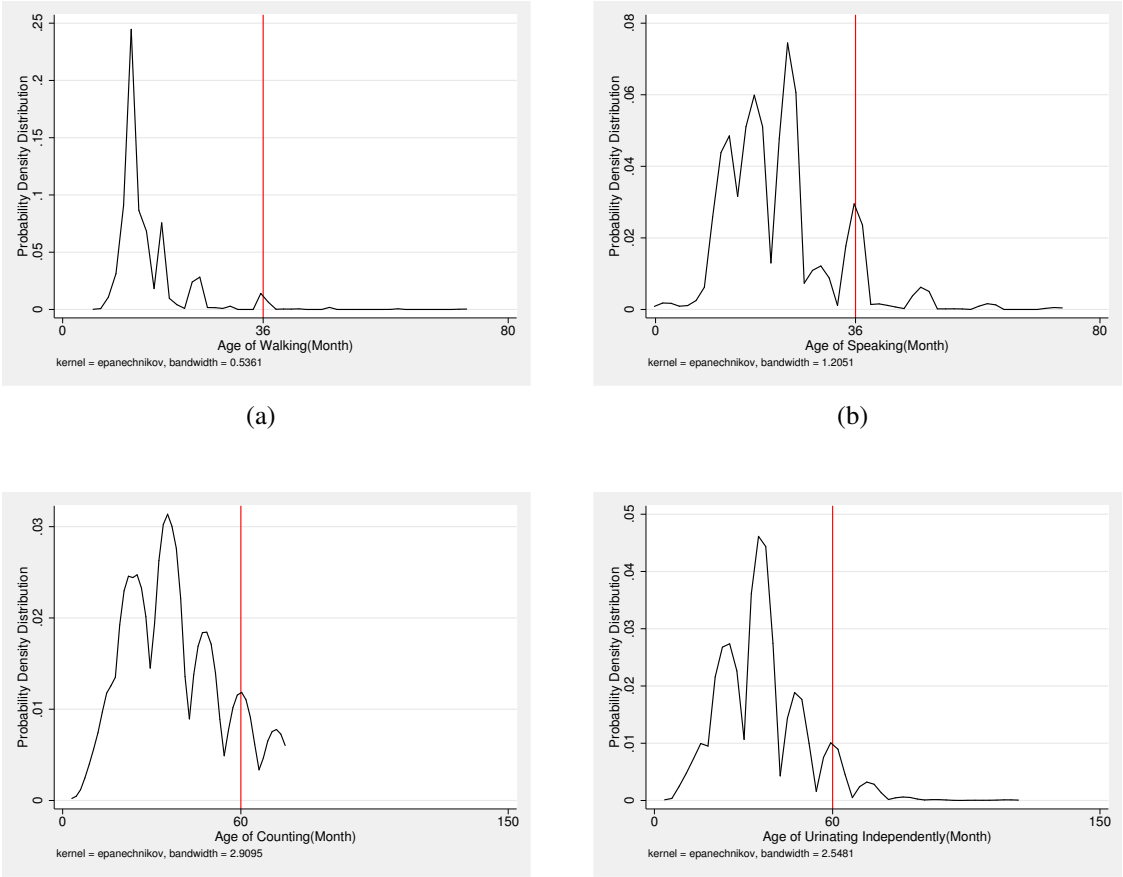


Figure III.5: The Probability Density Function of Children's Age of Achieving Early Life Skills

children or having children too early or too young. Therefore, we need to weight the sample to rebalance the treatment and control group. The procedure: 1. generate a dummy taking value of one if one has the child between 3 and 16 years old. 2. predict the probability of having such a child as a functin of the parent's individual characteristics. 3. use the inverse probability weighting method to weight children's data. That is, children with parents' characteristics that predict they are more liekly to show in the sample are given smaller weight, while children with the opposite characteristics are given higher weight.

The summary statistics in Table II.1 report the children's and related characteristics inlcuding the age when these children grasp the earliy-life skills. The sample is trimmed so that these

children's father lived in the five provinces when they were young (less than 12 years old).

4 Empirical Framework and Results

4.1 Parents' Outcomes, First-stage Analysis

4.1.1 Empirical Strategy

A naive OLS regression of parent's education on children's early-life skills is subject to omitted bias. Unobservables such as family background and other inheritable family characteristics could potentially affect both parent's educational attainment and children's early development. For example, parents' from good family background may have a better education and generally have a family tradition of how to take good care of children[49]. The OLS strategy fails to differentiate the impact of parent's education from other unobservables. Instead, I use Two Stage Least Squares and use the policy shock of CSL in 1986 as an instrument for parents' education.

At the first stage, I use data from CFPS 2010 to examine the impacts of CSL on educational attainment and cognitive skills. To identify a causal effect, I exploit the timing variation of the effectiveness of the law across provinces. The control group is defined as parents who have already turned 15 or older by 1986 and were unlikely to be affected by the policy, while those younger than 7 by 1986 were about to enter primary schools and would be exposed to the policy. The latter are referred to as the full-treatment cohorts. Those between 7 and 14 in 1986 are required to stay in schools until 15, but the impact of the policy on them is a decreasing function of their age in 1986. I refer this group as the early-treatment cohort.

At the first stage, I estimate the following model with the difference-in-difference setting:

$$Educ^{P}_{ija} = \alpha_0 + \alpha_1 Earlytreat_{ja} + \alpha_2 Fulltreat_{ja} + \lambda_j + \delta_a + \lambda X'_{ija} + \varepsilon_{ija} \tag{9}$$

where $Educ^{P}_{ija}$ is years of schooling or the educational attainment of person i in province j with

[49] OLS results show that all coefficients are significant.

age a. $Earlytreat_{ja}$ is a dummy taking value of one if the person's age is between 12 and 15, and $Fulltreat_{ja}$ is a dummy equal to one if the person's age is below 12 in 1986, respectively when CSL introduced in province j. These dummies represent the post-exposure cohorts who experienced the education reform. The sample also includes the pre-exposure cohorts, aged between 15 and 24 in 1986 and not eligible for nine-year compulsory schooling. The model controls province and cohort fixed effect. X_{ija} represents a group of control variables including the urban/rural residence and other demographic characteristics. Both α_1 and α_2 are expected to be positive if the CSL increases parents' education.

Table III.2: The Effect of CSL on Education, by Sample Cagegory

Sample	DEP VAR: Years of Education				
	Nationl Sample	National Sample, Weighted	National Sample, Resampled	Subsample, Five Big Provinces	Subsample, Rest of Provinces
	(1)	(2)	(3)	(4)	(5)
Early Treatment	0.680***	0.320	0.387	1.099***	0.230
	(0.235)	(0.309)	(0.356)	(0.179)	(0.376)
Full Treatment	1.307***	0.988*	0.935	2.199***	0.531
	(0.410)	(0.530)	(0.577)	(0.266)	(0.640)
Obs	33,023	33,023	21,371	16,521	16,502

Note: This table reports regression coefficients estimatd from the test based on Equation (9). Basic controls include the birth year and province fixed effect, linear province-specific trend, and urban/rural residence. The Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

The sample used in the main analysis comes from five big provinces. I use this sample instead of the small-province sample or the resampled sample because there is larger first-stage impact of the policy shock on education. Table III.2 shows that either the smapll-province sample or national sample with weighted method or resampled CFPS sample produce insignificant results, which gives weak first-stage outcome. The larger variation in the timing of implementation among the big provinces, on the other hand, grants larger statistical power in the first stage of 2SLS analysis.

There are two reasons why the first-stage impact is stronger with the big-province sample. First of all, there is larger variation in the timing of implementation. Second, the impact of the CSL is not homogenous across regions. As can be seen from Figure III.3, there is large unbalanced pre-CSL educational development among the big provinces. For inland provinces, many of which have a lower level educational attainment, students would benefit more from the policy as the education has larger room to improve. The analysis using the big-province sample, which has large number of observations in the education-lag-behind provinces, thus produce more significant first-stage results.

4.1.2 CSL Impact on Parents' Education and Cognitive Skills

The results from Table III.3 show that CSL has significant effects on years of education. Column 1 reports the basic specification while Column 2 and 3 adds controls of rural/urban residence and the linear province-specific time trend. The results are robust to these additional controls and alternative specifiations. Because there are only 4 provinces, we address the possibility of small sample bias in the clustered standard errors by also presenting p-values derived from wild bootstraps as recommended by Cameron and Miller (2015). I report the wild bootstrapped p-value immediately below the main results. Column 3 is the one with the prefereed specification.

Although schooling is compulsory for children with age below 15, if truancy and dropout are widespread in schools, they may undermine the effectiveness of the policy. I examine the issue by testing whether the CSL affects one's cognitive skills using the 2SLS strategy. CFPS 2010 measures one's cognitive skills by two sets of tests: a word test and a math test. In the word test, the interviewer presents a list of nouns to the respondent who is asked to recall and read as many as possible. The math test, on the other hand, asks the respondent to solve a set of math calculation questions. The two tests have different degrees of difficulty that can be adjusted corresponding to the respondent's education level. I use the test z-scores as the measure for the respondent's cognitive skills.

Table III.3: The Effect of CSL on Education, the First-stage Regression Results

DEP VAR	Years of Education			Level of Education			
				Primary School	Middle School	High School	College
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Early Treatment	1.116***	1.038***	1.099***	0.100***	0.114**	0.0295	0.0208
	(0.165)	(0.162)	(0.179)	(0.00842)	(0.0253)	(0.0261)	(0.0178)
Full Treatment	2.209***	2.083***	2.199***	0.215***	0.213***	0.0687**	0.0169
	(0.478)	(0.355)	(0.266)	(0.0195)	(0.0256)	(0.0217)	(0.0205)
Wild Bootstrap P-value	0.156	0.0938	0.0312	0.0312	0.0312	0.0938	0.406
Urban/Rural		x	x	x	x	x	x
Province $\times$ Birth Year Liner Trend			x	x	x	x	x
Mean of Outcome Variables							
Obs	16,521	16,521	16,521	16,528	16,528	16,528	16,528

Note: This table reports regression coefficients estimated from Equation 9. In addition to the covariates specified in the table, each regression controls the province and cohort fixed effect. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

The 2SLS results from Table III.4 show that one more year of education improve one's word skills by 0.17 standard deviation and math skills by 0.19 standard deviation. These results provide support that the policy was effectively implemented – both word and math test scores improved in response to the implementation of the education reform.

Table III.4: The Impact of the Education Reform on Cognitive Skills

DEP VAR	Math Test Z-Score	Word Test Z-Score
	(1)	(2)
	Panel A: Reduced Form	
Early Treatment	0.218***	0.245***
	(0.0364)	(0.0347)
Full Treatment	0.372***	0.426***
	(0.0349)	(0.0408)
Obs	16,528	16,528
	Panel B: Two-stage Least Square (IV=Full Treatment)	
Years of Education	0.169***	0.193***
	(0.0153)	(0.0115)
Kleibergen-Paap F Stat	68.20	68.20
Cragg-Donald Wald F Stat	36.02	36.02
Obs	16,521	16,521

Note: This table reports coefficients estimated from the reduced-form and 2SLS regressions, where the dependent variables are cognitive skills. Basic controls include the birth year and province fixed effect, linear province-specific trend, and urban/rural residence. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

This identification strategy relies on the assumption of common trends of education across provinces prior to the education reform. The assumption may fail and threaten the above result if the CSL effect on education is driven by the mean reversion of education across provinces. If so, we should observe that the high school and college attainment also show the sign of convergence. However, Column 7 and 8 show that this is not the case. The fact that only primary school and middle school attainment improved after the reform corroborates that the effect is most likely due to the compulsory schooling law, because the education reform is up to the middle school level, so

it is supposed not to affect people's high school or college attainment. I also control the parents' education interacted with the province dummy to control the linear province-specific time trend.

Another important assumption made when using this identification strategy is that the timing of the education reform is not correlated with the social and economic status. This assumption could fail if there are systematic changes related to the characteristics of provinces accompanying the reform (Stephens and Yang, 2014). To examine whether the timing is endogenous, I run OLS regression of timing on a list of provincial characteristics including income per capita, number of secondary schools and teachers per capita, student-teacher ratio, geographical location, etc. The results show that the main characteristics are barely correlated with the implementation year.[50] Moreover, in the robustness check, I control provincial characteristics interacted with the post-treatment cohort to account for the possible differential changes. All results are robust to these additional controls.

4.1.3 Other Parents' Outcomes: Fertility and Income

Since the CSL may have impacts on one's fertility pattern, which can further influence children's ECD, I study the possible impacts of the CSL on teenage birth and the fertility rate using the 2SLS strategy. Teenage birth is defined as the mother's birth age between 12 and 20 years old. The 2SLS results from Table III.5 show that additional years of education has statisitically significant negative effects on the incidence of teenage birth and the total number of children, though the economic magnitude is small – roughly one more year of education is associated with 0.006 lower teenage childbearing and 0.064 lower number of children. The fertility rate doesn't change much, possibly due to the restriction of the One-child policy.

[50]These results are available upon requests.

Table III.5: The Effect of the Education Reform on Fertility and Personal Income

DEP VAR	Fertility		Log(Income)			
	Teenage Childbearing	# Children			Heckman Correction	
			Monthly	Yearly	Monthly	Yearly
	Panel A: Reduced Form					
Early Treatment	-0.0187**	-0.105	0.0513	0.0587	0.0729**	0.273
	(0.00566)	(0.0522)	(0.0270)	(0.0934)	(0.0194)	(0.147)
Full Treatment	-0.0137**	-0.131	0.170**	0.0344	0.212**	0.305
	(0.00401)	(0.0636)	(0.0587)	(0.0828)	(0.0574)	(0.181)
Obs	16,528	16,528	8,632	10,402	8,627	10,400
	Panel B: Two-stage Least Square (IV=Full Treatment)					
Education	-0.00558***	-0.0643**	0.141**	0.0240	0.108***	0.177*
	(0.00171)	(0.0287)	(0.0590)	(0.0333)	(0.0336)	(0.104)
Kleibergen-Paap F Stat	165.4	165.4	23.01	163.3	16.90	22.18
Cragg-Donald Wald F Stat	46.65	46.65	6.698	23.21	13.10	9.977
Obs	16,521	16,521	8,625	10,397	8,620	10,395

Note: This table reports estimated effects of education on fertiliy pattern and personal income using both the reduced form and the 2SLS approach. Basic controls include the birth year and province fixed effect, linear province-specific trend, and urban/rural residence. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

Using the same strategy, I investigate effects of the CSL on personal income. The naive 2SLS regression show that more years of education increase personal monthly and yearly income but the latter is not statistically significant. After correcting the possible sample selection of job participation with the Two-step method proposed by Heckman (1979), both the results show that both monthly and yearly income significantly increase in repsonse to the education reform. The results that the impact of CSL on personal income is consistent with previous findings such as Fang et al. (2012) and Huang (2015).

4.2 Children's Early-Life Skills

4.2.1 Empirical Strategy

Next, I examine whether CSL affects children's early life skills through parents' education using the Two Stage Least Square. I use $Fulltreat_{ja}$ as the instrument, while controling $Earlytreat_{ja}$, because the former gives larger first-stage variation and has stronger statistical power in terms of the effect of the shock on parents' education.

The second-stage equation of 2SLS to be estimated is

$$Y_{ija}^{C} = \beta_0 + \beta_1 Educ_{ipa}^{P} + \beta_2 Earlytreat_{ja} + \lambda_p + \delta_a + \lambda X' + \varepsilon_{ipa} \tag{10}$$

wherer Y_{ija}^{C} represents early life skills of child i in household h with parents age a, including his/her age of speaking, walking, counting, etc. The dependent variable is measured by monthly age at which the child starts walking, speaking, countying, and getting indpendent of taking care of themselves. β_1 can be interpreted as the effect of CSL on children's age of obtaining their early life skills and is expected to be negative. $Educ_{ipa}^{P}$ is parents' years of schooling and $Fulltreat_{ja}$ is used as the instrument for $Educ_{ipa}^{P}$. I add basic controls the same as in the first-stage analysis: $Earlytreat_{ja}$, province fixed effect, parents linear province-specific time trend, and the rural/urban residence. As mentioned earlier, since the CSL can affect parents' fertility and the timing of birth through its impact on schooling, which in turn may affect ECD, I also add children's number of

siblings and their age as additional controls to rule out the fertility effect of education on ECD.[51] I follow Waldinger's suggestion to use only $Fulltreat_{ja}$ instead of both $Fulltreat_{ja}$ and $Earlytreat_{ja}$ to avoid potential weak instrument problem. For the concern over the weak instrument issue, I report Cragg-Donald Wald statistic as well as Kleibergen and Paap Wald statistic in each specification, which is robust to heterogeneity and sererial correlation of the error term(Cragg and Donald, 1993, Kleibergen and Paap, 2006). Consistent with the adult sample, the analysis only uses the big-province sample.

4.2.2 CSL Impacts on Children's ECD

[51] Studies have shown that more years of education are associated with lower ferility rate and incidence of teenage childbearing (Black, Devereux and Salvanes, 2008). It is possible that children with siblings are able to develop their skills earlier through the interaction with their siblings. For teenage childbearing, the mother who experienced teen child bearing might have physical or psychological issues that could influence children's ECD. To understand the direct influence of education on ECD, I control both the total number of children and their age to ensure that the effect of education on ECD is not driven by the change of fertility patern.

Table III.6: The Effect of Parents' Education on Children's Early-Life Skills, Main Results

DEP VAR	Monthly Measure				Yearly Measure			
	Walking	Speaking	Counting	Indpendence	Walking	Speaking	Counting	Indpendence
	Panel A: Father's Effect							
Father's Education	0.553	-2.409***	-0.922	-1.735***	0.0553	-0.301***	-0.0356	-0.144***
	(0.619)	(0.657)	(0.732)	(0.230)	(0.0602)	(0.0691)	(0.0541)	(0.0203)
Kleibergen-Paap F Stat	17.10	21.28	18.95	37.67	17.10	21.28	18.95	37.67
Cragg-Donald Wald F Stat	4.101	4.371	4.542	6.388	4.101	4.371	4.542	6.388
Obs	3,001	3,001	2,520	2,537	3,001	3,001	2,520	2,537
	Panel B: Mother's Effect							
Mother's Educations	-1.782***	-3.258**	-9.794***	-8.873**	-0.129***	-0.318*	-0.856***	-0.843*
	(0.592)	(1.635)	(2.994)	(4.417)	(0.0303)	(0.174)	(0.276)	(0.451)
Kleibergen-Paap F Stat	3.194	2.762	3.055	1.656	3.194	2.762	3.055	1.656
Cragg-Donald Wald F Stat	1.290	1.287	0.904	0.719	1.290	1.287	0.904	0.719
Obs	2,991	2,991	2,507	2,525	2,991	2,991	2,507	2,525

Note: This table reports coefficients estimated using the 2SLS strategy, where the dependent variables are children's age of walking, speaking, counting, and urinating independently. The sample for evaluating skills of walking and speaking consists of children aged above 3, while that for skilss of counting and being independent include children aged above 5. Basic controls include the parents' birth year and province fixed effect, linear province-specific trend, and urban/rural residence, children's age and total number of siblings. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

The results from Table III.6 show that father has a statistically significant effect on children's speaking and indpendence skills – 2SLS estimates that one more year of education could shorten the age when to speak and county by around 2.5 months, while mother has more influence on children's counting and independence skills, reducing the monthly age by 5 and 2 months respectively. The results also show that mother's education has overall positive impacts on children's ECD. Children tend to walk, speak, count, and be able to take care of themselves at an earlier age.

These results suggest that more years of parents' education can help children develop their intellectual skills earlier. [52] That said, mother's effect on children should be taken with caution, as the F-statistics for mother are small, which indicate that the CSL for mother's education may be subject to weak instrument variable issue. The reason could be that the CSL generally has a weaker effect on female's education than male's, as can be seen from Table III.2. However, in mother's education effect on children's outcomes, I use Limited Information Maximum Likelihood to reduce the possible bias caused by the weak IV (Bound, Jaeger and Baker, 1995, Stock, Wright and Yogo, 2002). I also use two instruments when there is overidentification of the instrument, in which I use Limited Information Maximum Likelihood, which produces similar results. These results are available upon request.

The measures of children's ECD are constructed based on retrospective data, which may call into question their accuracy. Parents are probably not able to perfectly recall children's early-life skill-achieving age, especially for those older children. It could cause problem if the misreporting is correlated with the parents' education. For example, if more years of education makes one report her chilren's age at an earlier age, then it is impossible to disentangle the education effect from the misreporting effect. Although there is no easy way to test the memory bias directly, I address this concern in the following ways. First, the misreporting is less of a concern as long as it is

[52]Note that these effects may contain the assortative mating effect. It's hard to single out the effect of either mother's or father's education on children's outcomes, as it requres the spouse's education to be controlled. However, that indicates a lot of missing values, as many children either has only father or mother 's information. more years of schooling may increase the probability of having a spouse with more years of schooling, the assortative mating effect. Therefore, the effect of father's education or the effect of mother's education on children's early-life skills is not the pure effect from each parent – it may contain part of the assotative mating effect, although this effect may not be that big.

a white noise that does not systematically bias parents' recollection on children's skill-achieving age in one way or another. In that case, the misreporting only causes heterscedacticity of the error term. Second, I add the control of children's age in the regression to alleviate misreporting because the custodians fail to recall as children get older. Third, I use yearly age instead of monthly age as a robustness check. Although we may lose some of variation of the dependent variable using the yearly measure, it is relatively easier for custodians to remember than the monthly measure and therefore less likely to be mistaken. The results shown in Table III.6 indicate that parents' education effects on children's early-life skills are robust to using the yearly measure.

The continous measures are useful in estimating the effect of parent's education on children's ECD, but they provide little information on children's critical skill-achieving age that parents' education exert effects on. Table III.7 reports the effect of parents' education on children's ECD using three discrete measures. It shows that the margin, at which the parents' education work on children's ECD. The effect of father's education on children's ability to speak work is stronger at an earlier age – at the cutoff age 2 years old, while the father's effect on children's ability to get independent is strongest at the age around 3 years old. Similarly, mother's effect on children's ability to speak is also strongest around 2 years old, for the ability to walk, mother's effect is strongest advance children's skill-achieving-age below 3 years old. and for the ability to count and be independent, mothers' effect is strongest at 4 years old.

4.2.3 Children's Other Outcomes: Health

There is a vast literature on the effect of parents' education on children's health. In CFPS 2010, there is lack of objective measure for children's health. Many are subjective questions such as whether the child went to hospital, whether the child was hospitalized, etc. These decisions can be made by children's parents' assessment of children's health and can not be used as a direct measure for children's health. Instead, I only use children's height and birth outcomes to examine whether the education shock affects children's health. The three meassures I used are the child's height z-score, gestational age, low birth weight. Following [Almond et al. 2005], the low birth weight

Table III.7: The Effect of Parents' Education on Children's Early-Life Skills Before a Given Age

Early-Life Skill	Walking	Speaking	Counting	Indpendence
	Panel A: Father's Effect			
DEP VAR	Skill Achived Below 2 Years Old			
Father's Education	-0.0257	0.222***	-0.0202	0.0174
	(0.0294)	(0.0378)	(0.0324)	(0.0146)
Kleibergen-Paap F Stat	20.93	20.93	30.67	30.67
Cragg-Donald Wald F Stat	4.626	4.626	6.477	6.477
DEP VAR	Skill Achived Below 3 Years Old			
Father's Education	0.00415	0.0932***	-0.0239	0.107***
	(0.0139)	(0.0286)	(0.0348)	(0.0186)
Kleibergen-Paap F Stat	20.93	20.93	30.67	30.67
Cragg-Donald Wald F Stat	4.626	4.626	6.477	6.477
DEP VAR	Skill Achived Below 4 Years Old			
Father's Education	0.266	-1.550***	-3.075**	-0.827
	(0.471)	(0.345)	(1.344)	(0.700)
Kleibergen-Paap F Stat	32.79	46.98	17.45	34.93
Cragg-Donald Wald F Stat	7.676	7.260	6.610	8.863
Obs	3,022	3,022	2,591	2,591
	Panel B: Mother's Effect			
DEP VAR	Skill Achived Below 2 Years Old			
Mother's Education	0.0594	0.236**	0.0669	0.0937
	(0.0453)	(0.114)	(0.0764)	(0.147)
Kleibergen-Paap F Stat	2.990	2.990	1.334	1.334
Cragg-Donald Wald F Stat	1.255	1.255	0.608	0.608
DEP VAR	Skill Achived Below 3 Years Old			
Mother's Education	0.0969**	0.168*	0.316*	0.432
	(0.0456)	(0.0900)	(0.175)	(0.275)
Kleibergen-Paap F Stat	2.990	2.990	1.334	1.334
Cragg-Donald Wald F Stat	1.255	1.255	0.608	0.608
DEP VAR	Skill Achived Below 4 Years Old			
Mother's Education	0.0171	-0.000782	0.380*	0.336*
	(0.0238)	(0.0375)	(0.219)	(0.198)
Kleibergen-Paap F Stat	2.990	2.990	1.334	1.334
Cragg-Donald Wald F Stat	1.255	1.255	0.608	0.608
Obs	3,012	3,012	2,578	2,578

Source: CFPS 2010

is defined as the children's birth weight is lower than 2500 grams. Table III.8 show that more years of father's education increase children's height and decrease the incidence of early gestation, presumably because fathers are able to provide better care of children and his spouse. More years of mother's education decrease the incidence of low birth weight of her child but has no significant impacts on children's height and the incidence of earliy gestation. These results are consistent with the literature.

Table III.8: The Effect of Parents' Education on Children's Health Outcomes

DEP VAR	Height Z-Score	Early Gestation	Low Birth Weight
	Panel A: Father's Effect		
Father's Education	0.0117**	-0.0503***	-0.0225
	(0.00485)	(0.0153)	(0.0157)
Kleibergen-Paap F Stat	23.44	26.63	10.12
Cragg-Donald Wald F Stat	5.307	7.919	4.097
Obs	3,419	3,668	2,977
	Panel B: Mother's Effect		
Mother's Educations	0.0287	-0.0167	-0.0453*
	(0.0334)	(0.0137)	(0.0242)
Kleibergen-Paap F Stat	4.623	7.770	8.982
Cragg-Donald Wald F Stat	2.662	4.192	4.788
Obs	3,410	3,656	2,978

Note: The table reports the estimated effect of parents' education on children's health outcomes. Basic controls include the parents' birth year and province fixed effect, linear province-specific trend, and urban/rural residence, children's age, and total number of siblings. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

5 Channels

More years of education can increase family income or influence parent's caring style, both of which may affect children's ECD. Higher family income allow parents to access to resources es-

sential for children's development of early-life skills, such as better nutrition or better early education service. Education may affect parents' caring style or attitude towards children's education by spending more time with children, being aware of right way to take care of children, etc. Lastly, as aforementioned, more years of education could affect one's fertility pattern, which influence children's ECD. This channel, however, is possible but unlikely, as in all the regressions, I control the number of children and children's date of birth to account for potential fertiliy or the mother-children age difference issue.

Since more years of education induced by the CSL increase individual income, I control the family income per capita in Equation (10) to examine whether income plays a role in children's ECD.[53] If increased family income improve children's ECD, the effect of parental education on ECD is supposed to lessen or dissappear after controling for this factor. However, Table III.9 shows that controling income does not change statistical significance of most of the results.

Another channel to examine is whether the parents affect children's ECD through the change of parenting style, that is, whether parents, after receiving more years of education, value higher children's early-life skill investment or change the way how they treat their children. Because of the limit of data, I specifically examine the two measures related to parents' pareting style: parent's communication with children (actively communicating with children), parents' attitudes towards children's education (children's pictorials, books, and learning materials). These are subjective assessment from the questionare interviewer, with a scale from 1 to 5. I also investigate two children's outcomes: whether the child was born in the hospital and whether the children are more likely to go to kindergarten.

[53] I cannot control parents' personal income, as children's parents are not always present to report their income. Intead, I use family income per capita as the control. The average family income is the total family income divided by the total family member. Whenever the average family income is not available, I use average of father and mother's income as the substitute.

Table III.9: The Effect of Parents' Education on Children's Early-life Skills – Controlling Family Income

DEP VAR	Walking (1)	Speaking (2)	Counting (3)	Indpendence (4)
	Panel A: Father's Effect			
Father's Education	0.908	-2.710***	-0.593	-1.593***
	(0.908)	(0.823)	(0.819)	(0.291)
Kleibergen-Paap F Stat	7.485	9.217	16.79	33.87
Cragg-Donald Wald F Stat	2.596	2.840	3.827	5.506
Obs	2,997	2,997	2,517	2,534
	Panel B: Mother's Effect			
Mother's Educations	-2.021*	-4.138	-10.57***	-10.21*
	(1.078)	(2.936)	(3.984)	(5.979)
Kleibergen-Paap F Stat	3.163	3.047	3.283	3.598
Cragg-Donald Wald F Stat	7.186	7.469	7.227	7.705
Obs	2,987	2,987	2,504	2,522

Note: The table reports the regression coefficients estimated using the same model speicification as in Table III.6. Basic controls include the parents' birth year and province fixed effect, linear province-specific trend, and urban/rural residence, children's age, and total number of siblings. Additional control is family income per capita. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

Table III.10 show that more years of mother's education increase the probability of mother activingly communicating with her children and that of caring children's education. More years of parent's education have significant impacts on children's probability of being born in the hospital. More years of father's education also have sizable impacts on children's likelihood of going to kindergarden. Although these factors might be related to the increased income, these evidence is consistent with the attitude change of how parents treat their children. [54]

[54]Note that since I cannot control all the parents' parenting style, I cannot do the same as controling income to rule out this channel.

Table III.10: The Effect of Education on Parenting Style

DEP VAR	Parents' Parenting Style		Children's Outomces	
	Care Children's Education	Communicate w/ Children	Born in Hospital	Kindergarten Education
	Panel A: Father's Effect			
Father's Education	-0.0435	0.0708	0.0812***	0.118***
	(0.0548)	(0.0561)	(0.0223)	(0.0351)
Kleibergen-Paap F Stat	26.60	26.60	26.63	23.26
Cragg-Donald Wald F Stat	7.919	7.911	7.919	4.734
Obs	3,667	3,666	3,668	3,012
	Panel B: Mother's Effect			
Mother's Education	0.201**	0.210*	0.156***	0.0521
	(0.0920)	(0.108)	(0.0491)	(0.0752)
Kleibergen-Paap F Stat	7.792	7.683	7.770	3.342
Cragg-Donald Wald F Stat	4.200	4.184	4.192	1.336
Obs	3,655	3,654	3,656	3,003

Note: The dependent variables are parents caring style and related children's outcomes. Basic controls include the parents' birth year and province fixed effect, linear province-specific trend, and urban/rural residence, children's age and total number of siblings. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.

Source: CFPS 2010

6 Robustness Checks and Additional Evidence

One issue with the self-reported ECD measure is that it could reflect parents' perception or expectation of children's skills instead of real ones. More years of education may elevate parents' expetcation of their children. If parents intentionally overstate children' skill-achieving age to match their higher expectation, or sincerely believie that their children are able to grasp these abilities at an earlier age because of the expectation, the identified parent's effect on CSL may be driven by parents' expectation. I propose two ways to deal with this issue. For the latter, I have controled children's age to deal with parents' . For the former, I examine whether the education elevate parents' expectation of their children. Specifically, I run the same regression on parents' expectation of their children's educational achievement, that is whether their children can make it to college, Ph.D., or foreign education. I find that the education has little effects on parents' expectation of children's these educational acheivement, as can be seen from Table III.11.

For the assumption of common trends, I run a falsification test using cohorts born five and seven years earlier than the population in the main analysis. I use three meassures of education, the continous measures of years of education and two dummies representing the primary school and middle school education attainment. If there is mean regression, the placebo effect of parents' education on ECD should be significant as well. However, Table III.12 shows that this is not the case. Only the effect of the placebo shock on priamry school attainment is significant at the fulltreatment. But this maybe becuase some early benefector of the CSL that are more likely to go to primary school. As we increase the gap to 7 or larger, the significance dissappears.

As a direct-controlled municipality by the central government, Shanghai is a provnicial administrative unit different from others in many aspects. The special economic zone set up in 1990 is also a sign that Shanghai may have implemented distinct policies (Wang, 2013). Given the unique political and economic position of Shanghai, I exclude Shanghai from the regression to see whether my results are driven by Shanghai's special policies. Table III.13 show that the results are not much affected by the inclusion of Shanghai into the sample.

Table III.11: The Effect of Education on Parents' Expectation of Children

DEP VAR	Parents' Expectation of Children		
	Foreign Education	College	Ph.D.
	Panel A: Father's Effect		
Father's Education	-0.0409	-0.110	-0.0278
	(0.0750)	(0.0822)	(0.0373)
Kleibergen-Paap F Stat	1.809	1.701	1.701
Cragg-Donald Wald F Stat	1.433	1.415	1.415
Obs	1,781	1,766	1,766
	Panel B: Mother's Effect		
Mother's Education	-0.0395	-0.0367	-0.00474
	(0.0595)	(0.0476)	(0.0377)
Kleibergen-Paap F Stat	3.245	3.304	3.304
Cragg-Donald Wald F Stat	2.387	2.439	2.439
Obs	1,774	1,759	1,759

Note: The table reports the estimated effect of parents' education on their expectation of children. Basic controls include the parents' birth year and province fixed effect, linear province-specific trend, and urban/rural residence, children's age, and total number of siblings. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

Table III.12: The Impact of CSL on Years of Education, Falsification Test

	Year Lag = 5			Year Lag = 7		
DEP VAR	Primary School	Middle School	Years of Education	Primary School	Middle School	Years of Education
	(1)	(2)	(3)	(4)	(5)	(6)
Early Treatment	0.0152	0.00723	0.105	-0.00396	-0.0163	-0.171
	(0.0270)	(0.0307)	(0.287)	(0.0179)	(0.0142)	(0.157)
Full Treatment	0.0807*	0.0609	0.650	0.00501	-0.00972	-0.122
	(0.0349)	(0.0313)	(0.424)	(0.0312)	(0.0304)	(0.336)
Obs	13,409	13,409	13,402	12,968	12,968	12,961

Note: This table reports regression coefficients from the placebo test. The model specification is identical to Table III.6, while the sample of cohorts consist of those born five years earlier than the original cohorts. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CGSS 2006

Table III.13: The Effect of Parents' Education on Children's Early-life Skills – Excluding Shanghai

DEP VAR	Walking (1)	Speaking (2)	Counting (3)	Indpendence (4)
	Panel A: Father's Effect			
Father's Education	0.480	-2.094***	-1.003	-1.778***
	(0.568)	(0.524)	(1.040)	(0.265)
Kleibergen-Paap F Stat	10.47	11.75	16.07	29.61
Cragg-Donald Wald F Stat	4.601	4.792	3.628	5.381
Obs	2,767	2,764	2,336	2,353
	Panel B: Mother's Effect			
Mother's Educations	-1.556***	-2.784***	-8.631***	-7.426***
	(0.344)	(1.031)	(1.290)	(2.379)
Kleibergen-Paap F Stat	5.982	5.660	4.597	3.525
Cragg-Donald Wald F Stat	1.572	1.659	1.452	1.393
Obs	2,757	2,753	2,325	2,342

Note: The table reports the regression coefficients estimated using the speicification identical to Table III.6. Shanghai is dropped from the main sample. Basic controls include the parents' birth year and province fixed effect, linear province-specific trend, and urban/rural residence, children's age, and total number of siblings. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CFPS 2010

Although the timing variation is hardly related to the provicial characteristics, as a further robustness check, I control provincial characteristics interacted with the post-treatment cohort to account for the possible differential changes. For the measure of the provincial characteristics, I use income per capita to measure the economic development, and the number of secondary schools per capita and the number of secondary school teachers per capita as the measure of education development level. Table III.14 shows that the main results are robust to controling these additional interactions.

Table III.14: The Effect of Parents' Education on Children's Early-Life Skills, Robustness Check to Additional Controls

DEP VAR	Walking	Speaking	Counting	Independence
	Panel A: Father's Effect			
	A.1: Controling Income Per Capita × Post-treatment			
Father's Education	0.195	-3.861**	-0.366	-2.247***
	(0.597)	(1.954)	(1.639)	(0.237)
Kleibergen-Paap Test	2.068	2.425	3.518	8.098
Cragg-Donald Wald F Stat	1.610	1.740	1.641	3.040
	A.2: Controling Secondary Schools Per Capita× Post-treatment			
Father's Education	0.460	-2.447***	-1.338	-1.922***
	(0.645)	(0.711)	(1.040)	(0.452)
Kleibergen-Paap Test	10.76	13.64	16.36	31.52
Cragg-Donald Wald F Stat	4.219	4.450	4.198	5.920
	A.3: Controling # Secondary-school Teachers Per Capita × Post-treatment			
Father's Education	-0.300	-4.721*	-2.526*	-2.897***
	(1.277)	(2.678)	(1.499)	(0.620)
Kleibergen-Paap Test	2.247	2.522	4.935	12.39
Cragg-Donald Wald F Stat	1.340	1.392	1.717	2.877
Obs	3,001	3,001	2,520	2,537
	Panel B: Mother's Effect			
	B.1: Controling Average Income × Post-treatment			
Mother's Education	-1.876***	-2.607**	-6.765***	-6.448***
	(0.559)	(1.139)	(0.940)	(1.951)
Kleibergen-Paap Test	3.820	3.266	3.928	2.711
Cragg-Donald Wald F Stat	2.460	2.365	1.537	1.430
	B.2: Controling Secondary Schools Per Capita × Post-treatment			
Mother's Education	-1.359***	-2.845**	-10.46**	-9.640
	(0.345)	(1.241)	(4.466)	(5.929)
Kleibergen-Paap Test	2.872	2.816	2.339	1.467
Cragg-Donald Wald F Stat	1.343	1.408	0.976	0.812
	B.3: Controling # Secondary-school Teachers Per Capita× Post-treatment			
Mother's Education	-1.586***	-1.957***	-5.873***	-5.365***
	(0.106)	(0.500)	(0.784)	(1.171)
Kleibergen-Paap Test	4.103	3.936	7.432	5.441
Cragg-Donald Wald F Stat	4.417	4.331	4.072	3.758
Obs	2,991	2,991	2,507	2,525

Source: CFPS 2010

7 Conclusion

This chapter empirically examines whether more years of parents' education benefits children's ECD. Using the compuslory schooling law as the quasi-natural experiment, I find that more years of mother's education lower children's age of acquiring skills of walking, speaking, counting, and getting independent, while more years of father's education are associated with children's earlier age to develop skills of speaking and independence. Further investigation shows that the effect of parents' education on children is more likely to work through its impact on parenting style than on family income.

These findings have two important takeways. First, educational inequality may be inheritable through the mechanism of intergenerational transmission. This study has shown that better educated parents are able to endow their children with important cognitive and noncognitive skills at an earlier age by providing them with a good home environment essential for their development. Such early-stage advantage, together with the imporved home environment, may enlarge children's education gap between the less educated families and the better educated ones. This observation provides rationales for policies – such as offering publicly accesible pre-school service to children from the poorly educated familiy – to remedy the opportunity ineqaulity at the begining of life.

Another implication of the finding is the potentially overlooked intergenerational benefit of the CSL. Although the CSL is one of the most effective way to univeralize basic education and defeat long-term poverty, there are still 20 percent of the countries in the world that do not offer universalized access to lower secondary education. Given that the ECD is relevant to one's future educational attainment and cognitive skills are critical to economic development (Hanushek and Woessmann, 2008), the potential intergenerational gain of the CSL should be taken into consideration in the future policy design and assessment in these developing countries.

Appendix I

A Figures and Tables

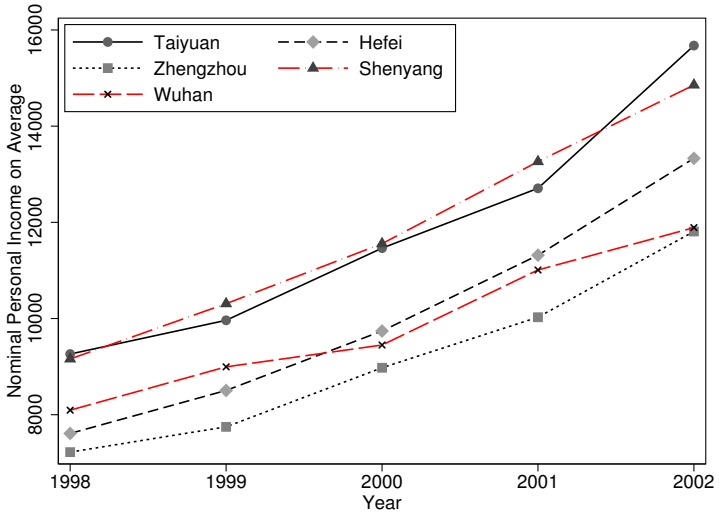


Figure I.A.1: Income growth in non-SOEs across selected cities

Source: CHIP2002. Personal income is defined as income earned from all enumerated sources throughout the year including all sources of subsidy, rewards, wage, and allowances, etc. The comparison cities are capital cities similar to Wuhan and Shenyang in terms of city scale and economic size.

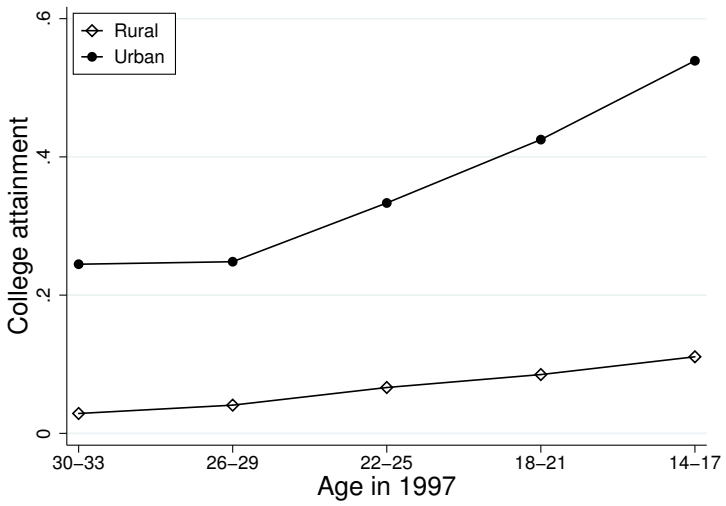


Figure I.A.2: College attainment, rural versus urban

Source: CFPS2010. Rural and urban area refers to the residence where the individuals lived at 12 years old.

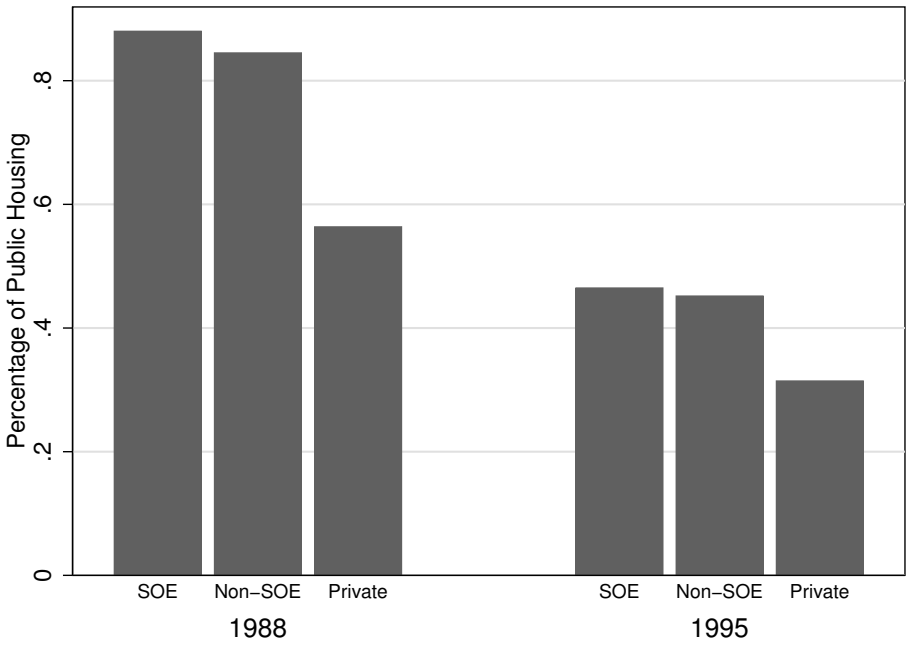
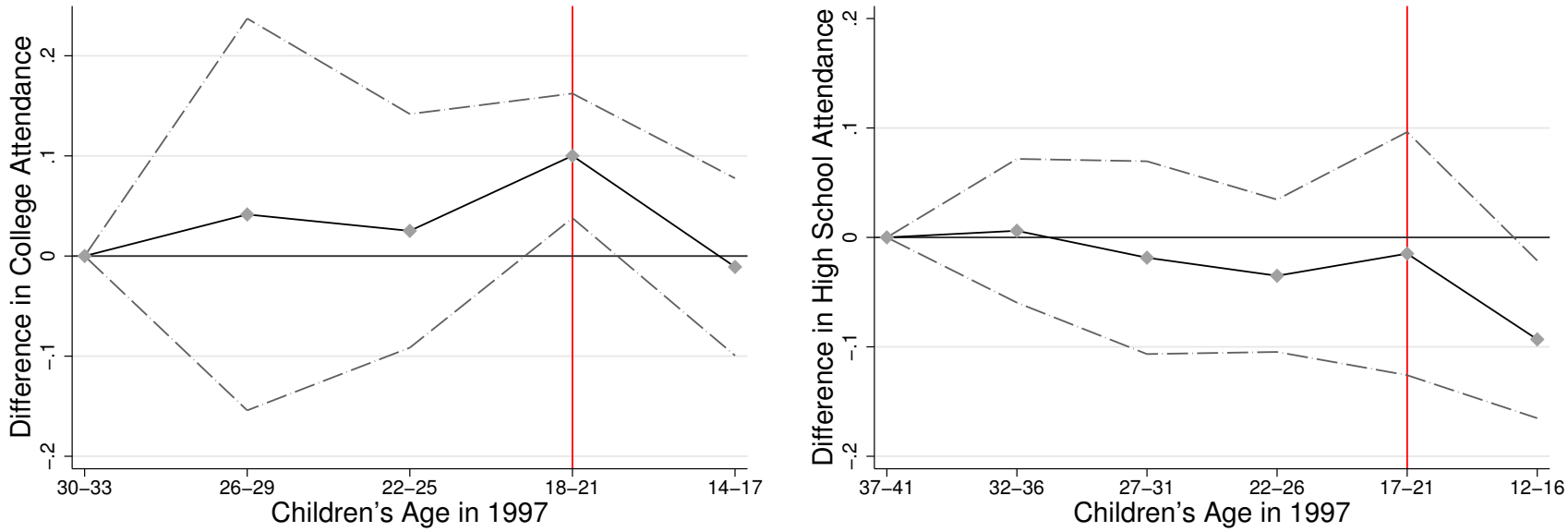


Figure I.A.3: Percentage of people living in public housing, by types of working units, 1988 and 1995

Source: CHIP1988 and CHIP1995.



(a) College

(b) High School

Figure I.A.4: Difference-in-Difference coefficients, by children's cohorts

Source: CULS2001. Each panel plots regression coefficients and 95% confidence interval from estimating Equation.

Table I.A.1: Robustness checks for DID

	(1)	(2)	(3)	(4)
Panel A: Dep Var = college attendance				
Post-shock Cohort × Father in SOE	-0.121**	-0.112*	-0.120***	-0.100**
	(0.0469)	(0.0591)	(0.0423)	(0.0405)
Observations	1,498	1,646	1,962	1,855
Panel B: Dep Var = high school attendance				
Post-shock Cohort × Father in SOE		-0.0790*	-0.0807*	-0.0743**
		(0.0457)	(0.0431)	(0.0348)
Observations		2,271	3,003	2,821

Note: Dependent variables are college enrollment in Panel A, and high school enrollment in Panel B. Except in Column 1, the college sample includes children born from 1964 to 1983, while the high school sample includes children born from 1956 to 1985. The post-shock cohorts in the college sample were born from 1980 to 1983 and in the high school sample were born from 1981 to 1985. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls include the total number of siblings, sisters, and brothers. The cohort fixed effect, the job sector specific trend, father controls, and children controls are included in all specifications.
*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters).

Specifications:
1. A robustness check for the college expansion and tuition increase. The post-shock cohort for college sample consists of children born in 1980 and 1981, and the total sample includes children born from 1964 to 1981.
2. Drop Shanghai from the sample.
3. Use children with fathers in all non-SOEs as the control group.
4. Include father's demographic interactions, which are children's cohort dummy interacted with father's education, height, party membership, and personal early life experience including whether was sent down to the rural area after 16 and whether lived in the urban area before 16.

Table I.A.2: Divergence of return to education and ability

DEP VARIABLES	(1) College	(2) College	(3) College	(4) High School	(5) High School	(6) High School
Post-shock Cohort × Father in SOE	-0.100**	-0.0886**	-0.0879**	-0.0743**	-0.0649*	-0.0729**
	(0.0405)	(0.0387)	(0.0387)	(0.0348)	(0.0352)	(0.0353)
Post-shock Cohort × Father's Education	0.0109	0.00599	0.00419	0.000996	0.00194	0.00137
	(0.00749)	(0.00729)	(0.00741)	(0.00645)	(0.00804)	(0.00582)
Post-shock Cohort × Father's Pschool Quality		0.160***	0.150**		0.108*	0.0941
		(0.0593)	(0.0735)		(0.0623)	(0.0681)
Post-shock Cohort × Father's Mschool Quality		0.226*	0.209*		0.0977	0.124**
		(0.127)	(0.125)		(0.0720)	(0.0593)
Post-shock Cohort × Father's Hschool Quality		-0.0211	-0.0376		0.0325	0.0312
		(0.0739)	(0.0722)		(0.0922)	(0.0951)
Post-shock Cohort × Father's Pschool Ranking			-0.147**			0.0176
			(0.0627)			(0.122)
Post-shock Cohort × Father's Mschool Ranking			0.0817			0.0704
			(0.186)			(0.177)
Post-shock Cohort × Father's Hschool Ranking			0.0899			-0.192
			(0.0857)			(0.132)
Mean of Outcome Variable	0.4345	0.4345	0.4345	0.6871	0.6871	0.6871
Observations	1,855	1,855	1,855	2,821	2,822	2,821

Note: The college sample includes children whose birth year ranges from 1964 to 1983, and high school sample ranges from 1956 to 1985. The post-shock cohorts in the college sample were born from 1980 to 1983, and in the high school sample from 1981 to 1985. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls include the number of children's siblings, sisters, and brothers. Demographic interactions are children's cohort dummy interacted with father's education, height, party membership, and personal early life experience including whether was sent down to the rural area after 16 and whether lived in the urban area before 16. Cohort fixed effect, job sector specific trend, father controls, children controls, demographic interactions are included in all specifications.

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters).

Table I.A.3: Falsification exercises using placebo treatments

Panel A: College

	(1)	(2)	(3)	(4)	(5)	(6)
Cohorts Sample	1968-1979	1968-1979	1968-1979	1972-1979	1972-1979	1972-1979
Post-shock Cohort × Father in SOE	0.0796	0.0854	0.0934	0.101*	0.101*	0.121*
	(0.0507)	(0.0631)	(0.0658)	(0.0568)	(0.0568)	(0.0628)
Observations	1,131	1,131	1,131	712	712	712
Father and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes
Children's Cohort × Father's Job FE	No	Yes	Yes	No	Yes	Yes
Father's Demographic Interactions	No	No	Yes	No	No	Yes

Panel B: High School

	(7)	(8)	(9)	(10)	(11)	(12)
Cohorts Sample	1966-1980	1966-1980	1966-1980	1971-1980	1971-1980	1971-1980
Post-shock Cohort × Father in SOE	0.0144	0.0148	0.0258	0.0368	0.0368	0.0512
	(0.0391)	(0.0569)	(0.0581)	(0.0537)	(0.0537)	(0.0528)
Observations	1,369	1,369	1,369	922	922	922
Father and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes
Children's Cohort × Father's Job FE	No	Yes	Yes	No	Yes	Yes
Father's Demographic Interactions	No	No	Yes	No	No	Yes

Note: Dependent variables are college enrollment in Panel A, and high school enrollment in Panel B. Children in the placebo post-shock cohort in the college sample were born from 1976 to 1979, and in the high school sample born from 1976 to 1980. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls are the number of children's siblings, sisters, and brothers. Father's demographic interactions are children's cohort dummies interacted with father's education, height, party membership, and personal early life experience including whether was sent down to the rural area after 16 and whether lived in urban areas before 16. Cohort fixed effect, job sector specific trend, father controls, children controls, demographic interactions are included in all specifications.

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters).

Table I.A.4: Mortality attrition

Panel A: College

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cohorts Sample	1968-1983	1968-1983	1968-1983	1972-1983	1972-1983	1972-1983	1976-1983	1976-1983	1976-1983
Post-shock Cohort × Father in SOE	-0.0670**	-0.113***	-0.0973**	-0.0576	-0.101**	-0.0839*	-0.108**	-0.108**	-0.0859**
	(0.0327)	(0.0377)	(0.0384)	(0.0387)	(0.0425)	(0.0440)	(0.0407)	(0.0407)	(0.0405)
Observations	1,549	1,549	1,549	1,130	1,130	1,130	803	803	803
Father and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Children's Cohort × Father's Job FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Father's Demographic Interactions	No	No	Yes	No	No	Yes	No	No	Yes

Panel B: High School

	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Cohorts Sample	1966-1985	1966-1985	1966-1985	1971-1985	1971-1985	1971-1985	1976-1985	1976-1985	1976-1985
Post-shock Cohort × Father in SOE	-0.0637***	-0.0710*	-0.0689*	-0.0724***	-0.0845**	-0.0823**	-0.0832**	-0.0832**	-0.0806**
	(0.0227)	(0.0362)	(0.0358)	(0.0224)	(0.0400)	(0.0398)	(0.0347)	(0.0347)	(0.0338)
Observations	1,951	1,951	1,950	1,504	1,504	1,503	1,071	1,071	1,070
Father and Children Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Children's Cohort × Father's Job FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Father's Demographic Interactions	No	No	Yes	No	No	Yes	No	No	Yes

Note: Dependent variables are college enrollment in Panel A, and high school enrollment in Panel B. Children in the post-shock cohort in the college sample are born from 1980 to 1983, and in the high school sample born from 1981 to 1985. Father controls include father's education, party membership, height, occupation dummies, industry dummies, early life experience, school ranking, school quality, etc. Children controls are the number of children's siblings, sisters, and brothers. Father's demographic interactions are children's cohort dummies interacted with father's education, height, party membership, and personal early life experience including whether was sent down to the rural area after 16 and whether lived in the urban area before 16. Father and children controls are included in all specifications.

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors are clustered at the community level (70 clusters).

B A Model of Geographical Externality

In this appendix, I construct a simple static model formalizing the geographical externality of economic restructuring. The model has N cities. Each city has a mass with population 1, and each individual has one child. All the jobs offer the same wage w and everyone is fully employed prior to the economic restructuring shock. After the shock, there is a proportion of τ_n ($0 < \tau_n < 1$) displaced workers competing for α_n vacancies of new jobs in city n. Both τ_n and α_n are exogenously given and the shock intensity is measured by τ_n. The expected wage for laid-off workers after the shock is $\frac{\alpha_n}{\tau_n} w$, where $\tau_n > \alpha_n$ and α_n/τ_n represents the probability of acquiring a new job and captures the fact that the more displaced workers are in the city, the lower the equilibrium wage is. Meanwhile, I assume that the wage of survivor workers is not affected by the shock to keep the model simple.

High school enrollment

Each child is required to take the high school entrance exam, and only those with scores above the city mean are able to enroll into high school.[55] Children's score of the high school entrance exam is a linear function of the parents' private investment and city n's public investment in education plus a disturbance term

$$\begin{cases} G_s = f(w) + g(\tau_n) + \varepsilon & \textit{Survivor worker} \\ G_l = f(\frac{\alpha_n}{\tau_n} w) + g(\tau_n) + \varepsilon & \textit{Displaced worker} \end{cases} \tag{11}$$

where $\varepsilon \sim N(0,1)$ and $f'(\cdot) > 0$. $g(\cdot)$ denotes the public education investment in city n and $g'(\cdot) < 0$. Both $g()$ and $f()$ are bounded between 0 and 1. The mean of the scores for the high school entrance exam in city n is

$$M^H = (1-\tau_n) G_s + \tau_n G_l \tag{12}$$

[55] Here I assume that all children prefer to achieve higher level of education

the probability of going to high school for children with displaced fathers is

$$\pi^H = Prob(G_l > M^H) \tag{13}$$

College enrollment

One difference between the probability of enrolling college and enrolling high school is the threshold. Note that the college exam is nation-wide. The threshold of college entrance is the national mean of the test scores rather than the city mean. And the former is exogenously given from the perspective of the exam-takers in a particular city.

The probability of going to college for children with displaced fathers thus is

$$\pi^C = Prob(G_l > \overline{M}) \tag{14}$$

where $\overline{M}$ is the nation-wide average scores for the college entrance exam.

Proposition 1. *If* $f(w) - f(\frac{\alpha_n}{\tau_n}w) < \frac{(1-\tau_n)\cdot f'\alpha_n w}{\tau_n^2}$ *, job displacement induced by economic restructuring has a negative externality on children's high school enrollment, i.e.,* $\partial\pi^H/\partial\tau_n < 0$*. If* $f(w) - f(\frac{\alpha_n}{\tau_n}w) > \frac{(1-\tau_n)\cdot f'\alpha_n w}{\tau_n^2}$ *, job displacement has a positive externality on children's high school enrollment, i.e.,* $\partial\pi^H/\partial\tau_n > 0$*.*

Proof of proposition 1:

Proof. Combing equation 11 and 12 and plugging 12 into 13, we have

$$\begin{aligned} \pi^H &= Prob[f(\frac{\alpha_n}{\tau_n}w) + g(\tau_n) + \varepsilon > (1-\tau_n)f(w) + \tau_n \cdot f(\frac{\alpha_n}{\tau_n}w) + g(\tau_n)] \\ &= Prob[\varepsilon > (1-\tau_n)\cdot(f(w) - f(\frac{\alpha_n}{\tau_n}w))] \\ &= \Phi[(1-\tau_n)\cdot(f(\frac{\alpha_n}{\tau_n}w) - f(w))] \end{aligned} \tag{15}$$

This gives

$$\frac{\partial \pi^H}{\partial \tau_n} = \Phi' \cdot [f(w) - f(\frac{\alpha_n}{\tau_n} w) - \frac{(1-\tau_n) \cdot f' \alpha_n w}{\tau_n^2}] \tag{16}$$

Since $\Phi' > 0$, we have $\partial \pi^H / \partial \tau_n > 0$ if $f(w) - f(\frac{\alpha_n}{\tau_n} w) < \frac{(1-\tau_n) \cdot f' \alpha_n w}{\tau_n^2}$ and $\partial \pi^H / \partial \tau_n < 0$ if $f(w) - f(\frac{\alpha_n}{\tau_n} w) > \frac{(1-\tau_n) \cdot f' \alpha_n w}{\tau_n^2}$. □

Proposition 1 shows two opposing externalities of economic restructuring at work simultaneously on children's high school enrollment. On the one hand, the increase of city-wide displaced workers increases the labor market competitiveness over the limited vacancies and accordingly lowers the expected re-employment wage. As a consequence, families with laid-off workers decrease their investment in education, which lowers children's test scores, and is reflected by the decrease of $f(\frac{\alpha_n}{\tau_n} w)$. The negative externality is scaled by $(1-\tau_n)$, because the marginal negative externality on each individual is smaller when there are already lots of workers unemployed.[56] The overall negative externality is thus captured by the marginal decrease of $f(\frac{\alpha_n}{\tau_n} w)$ multiplied by $(1-\tau_n)$, which is equal to $\frac{(1-\tau_n) \cdot f' \alpha_n w}{\tau_n^2}$.

On the other hand, more workers being laid off implies more children suffer from the family income loss. On the whole, the city-wide average score would fall, making the threshold lower and entering high school easier for everyone, even including those children with father laid off. Since the threshold is an increasing function of $\tau_n f(\frac{\alpha_n}{\tau_n} w)$ and a decreasing function of $\tau_n f(w)$, the lower $f(\frac{\alpha_n}{\tau_n} w)$ is and the higher $f(w)$ is, the more the threshold would decrease caused by the increase of τ_n. The marginal decrease of the threshold is thus captured by the term $f(w) - f(\frac{\alpha_n}{\tau_n} w)$, which represents the positive externality. Theoretically, if the gap between $f(w)$ and $f(\frac{\alpha_n}{\tau_n} w)$ is large enough, the positive externality can dominate the negative one. However, it turns out that the opposite is more likely to be true, as the $f(w) - f(\frac{\alpha_n}{\tau_n} w)$<1 by definition and is thus generally less than $\frac{(1-\tau_n) \cdot f' \alpha_n w}{\tau_n^2}$ when τ_n is small.

Proposition 2. *Job displacement induced by economic restructuring has a negative externality on*

[56] To understand this mechanism, consider an extreme case where everyone is laid off. The family' private investment in education would not make any difference in children's school performance. In that case, children's test scores are normally distributed and the negative externality is almost zero.

children's college enrollment, $\partial\pi^C/\partial\tau_n < 0$, and its overall externality on college enrollment is smaller than high school enrollment, $\partial\pi^C/\partial\tau < \partial\pi^H/\partial\tau_n$.

Proof of proposition 2:

Proof. Consider the probability of going to college for children whose father lost jobs

$$\begin{aligned} \pi^C &= Prob[f(\frac{\alpha_n}{\tau_n}w) + g(\tau_n) + \varepsilon > \overline{M}] \\ &= \Phi[f(\frac{\alpha_n}{\tau_n}w) + g(\tau_n) - \overline{M}] \end{aligned} \tag{17}$$

This gives

$$\frac{\partial\pi^C}{\partial\tau_n} = \Phi' \cdot [-\frac{f'\alpha_n w}{\tau_n^2} + g'(\tau_n)] \tag{18}$$

Since $f' > 0$ and $g' < 0$, we have $\frac{\partial\pi^C}{\partial\tau_n} < 0$.

Rearranging equation 15 yields

$$\frac{\partial\pi^H}{\partial\tau_n} = \Phi' \cdot [f(w) - f(\frac{\alpha_n}{\tau_n}w) + \frac{f'\alpha_n w}{\tau_n} - \frac{f'\alpha_n w}{\tau_n^2}] \tag{19}$$

Since $f(w) - f(\frac{\alpha_n}{\tau_n}w) > 0$, $\frac{f'\alpha_n w}{\tau_n} > 0$, and $g' < 0$, comparing (13) with (12), we have

$$\frac{\partial\pi^H}{\partial\tau_n} > \frac{\partial\pi^C}{\partial\tau_n} \tag{20}$$

□

Proposition 2 depicts a slightly different pattern than Proposition 1. The former states that the externality of economic restructuring on college enrollment is always negative regardless its scale. And the absolute value of negative externality on college enrollment is stronger than that on high school enrollment. The intuition is as follows. First, economic restructuring may damage the local economy, and drive down the government revenue and thus the government investment into

local public education. Since the supply of education matters for children's educational attainment (Duflo, 2001), children living in cities with widespread economic restructuring can suffer more. Nonetheless, in the case of high school enrollment, this channel does not hold any longer, as the competitors for high school all come from the same city, so such effect at the local level is canceled out for everyone.

Second, the college enrollment is nation-wide rather than city-wide. The probability of enrolling college does not depend on the city threshold but the national threshold, and the latter is actually exogenously given. Accordingly, the threshold of enrolling college is no longer a function of the scale of economic restructuring as in the case of high school enrollment, and the positive externality, which could partly offset the negative one on high school enrollment, does not exist in this case. The absolute value of negative externality is therefore bigger on college enrollment.

C Difference-in-Difference with Discrete Terms

The results presented in panel (a) and (b) of Figure I.5 only provide illustrative evidence of pre-trends for the treatment and control groups. In the appendix, I estimate a restricted model with discrete interaction terms to show analytically how the pre-trends of children's educational attainment evolve and how economic restructuring affects it,

$$E_{ias} = \alpha_0 + \sum_{a=1}^{A} \beta^a (SOE_s \times Cohort_i^a) + \rho_a + \eta_s + \theta^J X_i + \varepsilon_{ias} \tag{21}$$

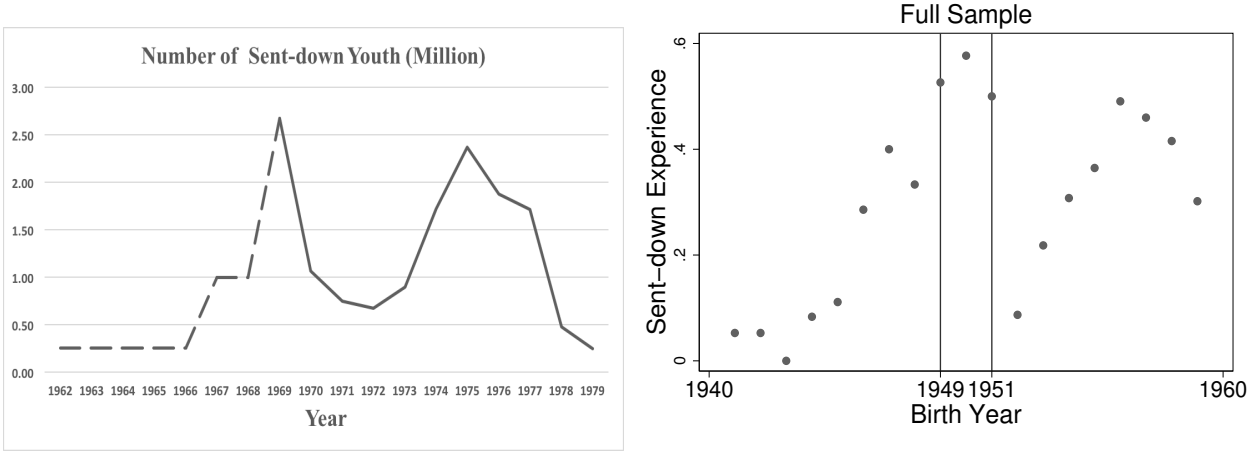
where $Cohort_i^a$ is a dummy that indicates whether children i is in age group a. Other parameters and variables are defined in the same way as in Equation 1. Each coefficient β can be interpreted as an estimate of the impact of the exogenous shock on the educational attainment of a given cohort.

Figure I.A.4 plots these coefficients and their 95% confidence intervals for college and high school attendance respectively. In the analysis of college attendance (Panel A), the coefficient of each age group increases over time but drops shortly after the economic restructuring shock, as shown in the post-shock group. In the case of high school attendance (Panel B), the pre-shock

coefficients roughly resolve around zero, but the post-shock coefficient drops and is significantly below zero. These results are consistent with the suggestive evidence in Figure I.5.

Appendix II

A Figures and Tables



(a) Number of Sent-down Youth

(b) Fraction of People Sent Down By Bith Year

Figure II.A.1: Send-down Movement

Sources: *(a).* Zhongguo laodong gongzi tongji ziliao: 1949-1985 (China's Labor and Wage Statistics, 1949-1985); *(b).* CFPS2010 and CULS2001. For Panel (a), since only aggregate data are available for the year range between 1962 and 1966, and between 1967 and 1968, I use the simple yearly average of the aggregrate data as a proxy for the number of sent-down youth at these years. These data are connected by the dashed line.

Table II.A.1: Robustness check for the measure of sex selection

DEP VAR	Number of Children	Having a boy as the only child
$\triangle Slope\mid_{birthyear=1949}$	-0.00180	-0.00209
	(0.0308)	(0.0136)
$\triangle Slope\mid_{birthyear=1951}$	-0.0612**	-0.0132
	(0.0287)	(0.0132)
P-value from F-test	21,485	21,485
Obs	0.000	0.0880

Note: The model specification is the same as in Tabel 3 and 4. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: Census 1990

Table II.A.2: First Stage Results with Weighted Sample

DEP VAR	Year of Study at High School		Experience Shortened Length of Schooling	
	(1)	(2)	(3)	(4)
$\triangle Slope\mid_{birthyear=1949}$	0.636***	0.667***	0.372***	0.403***
	(0.159)	(0.211)	(0.130)	(0.138)
$\triangle Slope\mid_{birthyear=1951}$	0.482***	0.477**	0.417***	0.435***
	(0.177)	(0.212)	(0.150)	(0.144)
Controls		x		x
P-value from F-test	0.000447	0.00806	0.0122	0.00745
Obs	192	192	206	206

Note: The model specification is the same as in Tabel 3 and 4. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: CULS 2001 and CFPS 2010

Table II.A.3: Regression kink design with different bandwidths

Panel (a)	First Stage		
	Bandwidth=17	Bandwidth=15	Bandwidth=13
	(1)	(2)	(3)
$\triangle Slope\mid_{birthyear=1949}$	0.337**	0.353**	0.399**
	(0.148)	(0.155)	(0.157)
$\triangle Slope\mid_{birthyear=1951}$	0.236	0.252*	0.271*
	(0.146)	(0.152)	(0.154)
P-value from F-test	0.00422	0.0166	0.0126
Obs	419	336	256
Panel (b)	Reduced Form		
	Bandwidth=17	Bandwidth=15	Bandwidth=13
	(4)	(5)	(6)
$\triangle Slope\mid_{birthyear=1949}$	0.0161***	0.0164***	0.0171***
	(0.00155)	(0.00203)	(0.00277)
$\triangle Slope\mid_{birthyear=1951}$	0.0140***	0.0170***	0.0186***
	(0.00123)	(0.00154)	(0.00207)
P-value from F-test	0	0	0
Obs	50,020	39,604	29,417

Note: The model specification is the same as in Tabel 3 and 4. Robust standard errors are in the paranthesis. *** p<0.01, ** p<0.05, * p<0.1.
Source: Panel (a): CULS 2001 and CFPS 2010; Panel (b): Census 1990

B Sources of Data Sets

CFPS 2010

China Family Panel Studies (CFPS) is a nationally representative, annual longitudinal survey of Chinese communities, families, and individuals launched in 2010 by the Institute of Social Science Survey (ISSS) of Peking University, China. The survey follows up respondents biennially and aims to form a micro-level dataset to study the social, economic, and demographic transition in China. Its sample frame covers 25 provinces and represents 95% of the total population. The first wave in 2010 surveys 14960 families and 42590 individuals. Some distinctive features of the first round survey includes its rich information on family structure and personal life experience.

CGSS 2006

China General Social Survey (CGSS) is a large-scale integrated socio-economic and health survey launched jointly by Renmin University of China and the Hong Kong University of Science and Technology in 2003. CGSS is also a member of the International Social Survey Programme (ISSP) on behalf of Mainland China. Starting from 2006, CGSS collaborated with Korean General Social Survey, Japanese General Social Survey, and Taiwan General Social Survey initiating the East Asian Social Survey (EASS) project to collect information on a certain specific topic in each survey. By 2015, CGSS has committed the EASS modules of family, globalization, health, social network and social capital, and work and economy. The sample frame of 2006 survey used in this chapter covers around 10000 individuals and 3000 families.

CULS 2001

China Urban Labor Survey (CULS) 2001 is administered by the Institute of Population Studies at the Chinese Academy of Social Science. The sample frame of the survey is constructed based on the 2000 census. The survey covers 3500 urban permanent resident households and around

8100 individuals aged above 16 in Fuzhou, Shanghai, Shenyang, Wuhan, and Xi'an, five province capitals.

CHIPS 1988

China Household Income Project Survey (CHIPS) is a national household survey data administered by China Institute for Income Distribution of Beijing Normal University. The purpose of this survey is to measure and estimate the distribution of income in both rural and urban areas in China. By far, CHIPS has successfully collected data in 1988, 1995, 2002, 2007, and 2013. The data collection in 1988 consists of two distinct samples of the urban and rural population of China which were selected from significantly larger samples drawn by the State Statistical Bureau. The total number of observation in the 1988 survey is around 32,000 in the urban sample.

C Proof of the Identification Strategy

C.1 Regression Kink Design

Under the assumption that $E[\varepsilon|T=t]$ and $\frac{\partial E[\varepsilon|T=t]}{\partial t}$ are continuous at C_i, the change in the slope, α_i, can be identified from Equation 7, where α_i is positive if the slope is decreasing at C_1 and increasing at C_2.

Proof. By Equation 7 and the definition of D_1, we have

$$\begin{aligned}
\lim_{t\uparrow C_1} E[Z|T=t] &= \alpha_0 + \alpha_1(t-C_1) + \beta_1 + \mu t + \lim_{t\uparrow C_1} E[\varepsilon|T=t] \\
\lim_{t\downarrow C_1} E[Z|T=t] &= \alpha_0 + \mu t + \lim_{t\downarrow C_1} E[\varepsilon|T=t]
\end{aligned}$$

Taking the partial derivates w.r.t. t on both sides yields

$$\lim_{t\uparrow C_1}\frac{\partial E[Z|T=t]}{\partial t} = \alpha_i+\mu+\lim_{t\uparrow C_1}\frac{\partial E[\varepsilon|T=t]}{\partial t} \quad (22)$$

$$\lim_{t\downarrow C_1}\frac{\partial E[Z|T=t]}{\partial t} = \mu+\lim_{t\downarrow C_1}\frac{\partial E[\varepsilon|T=t]}{\partial t} \quad (23)$$

By the smoothness assumption, we have

$$\lim_{t\uparrow C_1}\frac{\partial E[\varepsilon|T=t]}{\partial t} = \lim_{t\downarrow C_i}\frac{\partial E[\varepsilon|T=t]}{\partial t}$$

Therefore, Equation (3) minus (4) yields

$$\alpha_1 = \lim_{t\uparrow C_1}\frac{\partial E[Z|T=t]}{\partial t} - \lim_{t\downarrow C_1}\frac{\partial E[Z|T=t]}{\partial t} \quad (24)$$

If the change in the slope at C_1 is decreasing, i.e,

$$\lim_{t\uparrow C_1}\frac{\partial E[Z|T=t]}{\partial t} - \lim_{t\downarrow C_1}\frac{\partial E[Z|T=t]}{\partial t} > 0$$

Then, we have

$$\alpha_1 > 0$$

Likewise, at critical birth year C_2, we have

$$\lim_{t\uparrow C_2}E[Z|T=t] = \alpha_0+\mu t+\lim_{t\uparrow C_2}E[\varepsilon|T=t]$$

$$\lim_{t\downarrow C_2}E[Z|T=t] = \alpha_0+\alpha_2(t-C_2)+\beta_2+\mu t+\lim_{t\downarrow C_2}E[\varepsilon|T=t]$$

By the smoothness assumption,

$$\alpha_2 = -(\lim_{t\uparrow C_2}\frac{\partial E[Z|T=t]}{\partial t} - \lim_{t\downarrow C_2}\frac{\partial E[Z|T=t]}{\partial t}) \quad (25)$$

If the change in the slope at C_2 is increasing, we have

$$\alpha_2 > 0$$

For Equation 8, we can prove in the same way that

$$\alpha_i' > 0,\ i = 1,2$$

□

C.2 The Impact of the Education Shock

Next, I show that the impact of the education shock on sex selection, δ, can be estimated by $\frac{\alpha_i}{\alpha_i'}$ under the smoothness assumption.

Proof. Assuming that the following relationship holds for the sex selection function, $Y(t)$, and education function, $Z(t)$

$$Y = \delta Z + u \tag{26}$$

where $Y(t)$ and $Z(t)$ are both functions of birth year.

Equation (7) implies

$$\lim_{t\uparrow C_i} \frac{\partial E[Y|T=t]}{\partial t} = \delta \lim_{t\uparrow C_i} \frac{\partial E[Z|T=t]}{\partial t} + \lim_{t\uparrow C_i} \frac{\partial E[u|T=t]}{\partial t} \tag{27}$$

$$\lim_{t\downarrow C_i} \frac{\partial E[Y|T=t]}{\partial t} = \delta \lim_{t\downarrow C_i} \frac{\partial E[Z|T=t]}{\partial t} + \lim_{t\downarrow C_i} \frac{\partial E[u|T=t]}{\partial t} \tag{28}$$

Subtracting Equation (8) from (9) and rearranging the equation yields

$$\delta = \frac{\lim_{t\uparrow C_i} \frac{\partial E[Y|T=t]}{\partial t} - \lim_{t\downarrow C_i} \frac{\partial E[Y|T=t]}{\partial t}}{\lim_{t\uparrow C_i} \frac{\partial E[Z|T=t]}{\partial t} - \lim_{t\downarrow C_i} \frac{\partial E[Z|T=t]}{\partial t}}$$

Recalling Equation (5) and (6), we have

$$\delta = \frac{\alpha_i}{\alpha_i'} \tag{29}$$

□

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