More Schooling, More Generous? Estimating the Effect of Education on Intergenerational Transfers

Ting Yin[*], Junchao Zhang[†]

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Abstract

This paper studies the causal effect of education on intergenerational transfers from/to adult children. Using micro-data from the China Health and Retirement Longitudinal Study, we exploit exogenous variations in parents' schooling induced by China's Great Famine to take account for the endogeneity of education and then estimate the effect of schooling on the probability of receiving/giving transfers from/to adult children. The instrumental variable estimates show that an additional year of schooling has a negative effect on the probability of receiving transfers, but a positive effect on the probability of giving transfers at old age. Our results have some implications regarding social security and education policies in aging societies.

JEL classification: D64; H55; I21

KEY WORDS: China; education; intergenerational transfer; instrumental variable

[*] *Research Institute of Economy, Trade and Industry, 1-3-1, Kasumigaseki, Chiyoda-ku, Tokyo 100-8901, JAPAN*

[†] *The Institute of Statistical Mathematics, 10-3, Midori-cho, Tachikawa, Tokyo 190-8562, JAPAN E-mail address: ztempest0218@gmail.com*

1 Introduction

The rapid aging process is a severe problem that China is now facing due to the one child policy. When the baby boom generation born during the 1960s are getting older and older, children of them will have a massive pressure of supporting the parents in old age. The old-age dependency ratio would be 40% in 2050 though it was only 13% in 2015[1]. However, developed countries such as the U.S., Japan and EU countries whose social security systems cover the majority of the elderly before the aging of the population. China is still at a low level of economic development and with few social security systems, especially for elder cohorts and rural people.

Other than social security systems, private transfers among adult generations within the family are the most important source of old-age care in China. With its long history and culture, China has unique traditional family values. The "filial piety," measured by monetary and time transfers from adult children to their elderly parents, has adequately filled up the lack of social security systems. Also, intergenerational transfers behaviors have a crucial policy implication in human society, as many public programs, such as social security systems and the taxation of savings, tend to depend upon the intergenerational link within families.

Economists have established a lot of theoretical models to explain transfer behavior across generations (Becker, 1974; Kotlikoff, 1988; Altonji et al., 1997, etc.). On the one hand, the altruistic model suggests that parents care mainly about the happiness of their children, transfers flow from the least to the most financially needy generation independently of any present or future mutual help (Becker, 1991; Fujiu & Yano, 2008). On the other hand, in the exchange model, financial transfers from parents reflect the

[1]Source: European Commission

payment of services and visits provided by children (Cox, 1987; Cox & Rank, 1992). Cox & Stark (2005) develops a new theoretical model of transfers that is not based on the monetary mechanism. The "demonstration effect" model considers that a child's propensity to furnish parents with attention and care can be conditioned by parental example.

The literature on intergenerational transfers has been accumulated rapidly in China. İmrohoroğlu & Zhao (2017) argues that family support plays a prominent role in the well-being of the elderly and often substitutes for the lack of government-provided old-age support systems in China. Using national level data, Zhu et al. (2014) investigates transfer behavior in the context of China's one child policy, and they find that a decrease in the number of children results in parents investing more in their children's schooling. Cai et al. (2006), Lei et al. (2012), and Wu & Li (2013) and are among the very few studies that use individual-level data to analyze intergenerational transfer behavior. Lei et al. (2012) analyzes patterns and correlates of intergenerational monetary transfers, and they find that transfers are significantly affected by adult children's financial capabilities. Educated and married children are more likely to provide transfers to their elder parents. Cai et al. (2006) and Wu & Li (2013) show that intergenerational transfers to the elderly parents are conditioned on pre-transfer household resources. According to Wu & Li (2013), one *yuan* loss of income among the elderly parents would be compensated by an increase of 0.75 *yuan* of transfers from their children.

Education could be a potential consequence of intergenerational transfer behavior; however, it is difficult to estimates its effect because of endogenous educational decisions. Figure 1 shows the graphical correlation between intergenerational transfers and schooling. Panel A and Panel B are the probabilities of receiving and giving transfers graphed by schooling, respectively. In Panel A, the probability of receiving inter vivos transfers from adult children decreases in years of schooling. However, in Panel B, the probability of giving inter vivos transfers to adult children increases in years of schooling. Private transfer pattern between elderly parents and their children are changing rapidly according to the increasing average schooling among elderly parents. Due to endogeneity issues arising from reverse causality, as well as unobservable factors that may be correlated with intergenerational transfers and education, few studies have investigated the causal link.

This paper makes two contributions to the literature on intergenerational transfer. First, using a representative micro-data from China, we provide new evidence suggesting that intergenerational transfers behaviors are causally related to parents' education. Previous studies mainly focus on distinguishing between the altruism and the exchange motives. However, the causal evidence on intergenerational transfer is lacked. We exploit the sudden change in schooling induced by China's Great Famine during 1959 to 1961, which caused millions of students to drop out of primary schools, to estimate the effect of schooling on intergenerational transfers. Our instrumental variable estimates show that an additional year of schooling has a negative effect on the probability of receiving transfers from, but a positive effect on the probability of giving transfers to their adult children at old age. To our knowledge, this is the first paper that investigates the causal relationship between education and intergenerational transfer.

Second, we identify the pure education effect on transfer behavior instead of using schooling as a proxy for incomes. Education is predicted to raise a person's permanent wage. Thus, more educated parents could receive fewer transfers just because they are rich. Several previous studies(Cai et al., 2006; Wu & Li, 2013) have shown that pre-transfer household income is an important determinant of receiving transfers from adult children at old age. Using the rich information on household assets and individual incomes, we have eliminated the income effect of intergenerational transfers. Holding incomes and savings constant, we find that education consistently shows a significant effect on transfers behaviors. Other than monetary mechanism, education plays an important role in private transfer behavior, which implies that policies makers should pay more attention to enhance social security benefits in the context of education expansion. Given the substitution between social security and private transfer, increasing average schooling would causally decrease private intergenerational transfers, and consequently, increases the economic burden of social security expenditure.

Besides, this paper is also related to an accumulating literature on the long-term consequences of China's Great Famine(Peng, 1987; Chen & Zhou, 2007; Fan & Qian, 2015; Xu et al., 2016). As Chen & Zhou (2007) has pointed out that mechanisms through which exposure to famine translates into economic outcomes in later life remain unknown in current literature. Instead of estimating the direct effect of famine experience during early childhood on subsequent outcomes in adulthood, this paper estimates the indirect effect of famine on intergenerational transfer at old age through the channel of education. Compared to previous studies, this paper helps to understand the mechanism how famine affects people's behaviors in later periods.

The remainder of this paper organized as follows. Section 2 briefly introduces the background of China's Great Famine. Section 3 is the identification strategy. Section 4 describes the dataset and offers descriptive statistics. The following section presents the results of OLS and IV estimation. Section 6 checks the robustness of our results. Section 7 concludes.

2 China's Great Famine

In the early 1950s, China started to collectivize its agriculture to satisfy the increasing demand for agricultural products. Collectivization was regarded as a strategy to promote the development of agriculture and industry(Lin, 1990). In 1958, China officially carried out the national wide "Great Leap Forward" movement. All rural households were assigned into thousands of People's communes, which especially organized food production among other general government functions. However, in the following three years (1959-1961), China experienced a great famine regarding grain shortage, which resulted in an unprecedented number of deaths. The famine led to 20-30 million deaths and 30 million lost births(Ashton et al., 1984; Peng, 1987). Demographers, economist, and social scientists have long been interested in the causes of the China's Great Famine. Chen & Zhou (2007) concludes that the famine is a consequence of bad weather, excessive procurement by the state, delayed response to the food shortage, weakened production incentives due to the sweeping collectivization program in 1958, and resource diversion as a result of massive industrialization.

Many previous studies have investigated the facts and causes of China's Great Famine(Lin, 1990; Lin & Yang, 1998, 2000; Kung & Lin, 2003; Meng et al., 2010), and its long-term consequences on risks of mental illness(St Clair et al., 2005; Song et al., 2009), health(Chen & Zhou, 2007; Fan & Qian, 2015; Xu et al., 2016), and economic conditions(Chen & Zhou, 2007). Chen & Zhou (2007) shows that China's great famine has critical negative impacts on adulthood height, labor supply, and earnings for those in early childhood during 1959-1961. Gorgens et al. (2012) criticized that taller children are more likely to survive a famine, and this selection issue makes height a biased measure of health or economic conditions. Xu et al. (2016) finds little evidence that China's Great

Famine has long-term effects on health, as the regression results are highly sensitive to the choices of health indicators and model specifications. In a word, the evidence on health is mixed.

The famine not only resulted in an unprecedented number of deaths and malnutrition among survivors but also disturbed the social order. However, very few studies have investigated indirect effects of China's Great Famine. During the famine period, the supply side of education system experienced a sharp shrinkage. Schools were suspended, and teachers were sent to do manual labor(Hannum, 1999; Dikötter, 2010). The number of agriculture middle schools fell back from 20,000 in 1958 to only 4,000 by 1963, and new enrollments fell back to the 1957 level by 1961(Bramall, 2008). Moreover, primary students were organized to engage in school-based farms and factories, which made them have less schooling than other cohorts. Compared to the channel of health, the indirect effect of China's Great Famine on adulthood outcomes in later life could be more long lasting, as the loss of human capital accumulation would further affect people's labor supply and earnings. Huang & Zhou (2013) is among the very few that tests the indirect effect of the famine through education. They find that absence of schooling induced by China's Great Famine significantly reduces cognition abilities at old age. Compared to Huang & Zhou (2013), this paper controls an additional vector of assets, savings, and incomes. Holding assets, savings, and incomes constant, our results imply that education may affect transfer behavior at old age upon unobserved non-cognitive human capital.

3 Identification Strategy

To estimate the effect of schooling on intergenerational transfers from/to adult children, the benchmark model is specified as follows:

$$Transfer_i = \beta_0 + \beta_1 Educ_i + \beta_2 X_i + u_i \tag{1}$$

where the $Transfer_i$ is a binary outcome variable indicating receiving/giving transfers from/to adult children. β_1 is the coefficient of interest, capturing the effect of education. $Educ_i$ is measured by years of schooling. X_i is a vector of control variables including age, age squared, gender, marital status, *hukou* status, number of children, ownership of current residence, and other household assets. u_i is the error term.

β_1 estimated by Equation (1) might suffer from omitted variable bias, as educational decisions are related to family background, individual ability, and other unobserved characteristics. To take account for the endogeneity of education, we use China's Great Famine as a natural experiment and then perform an IV estimation. The first stage of IV estimation is specified as follows:

$$Educ_i = \gamma_0 + \gamma_1 Cohort_i^{1948-1953} + \gamma_2 X_i + v_i \tag{2}$$

where $Cohort_i^{1948-1953}$ is a binary instrumental variable that equals to 1 if individual i was born during 1948 to 1953 and 0 otherwise. X_i is the same vector of control variables as in Equation (1). v_i is the error term. Following Huang & Zhou (2013), we define the treatment group as those who were born during 1948 and 1953, suffering from China's Great Famine when they were primary school students.

The instrument is valid when the following two necessary conditions are satisfied

$$Cov(Educ_i, Cohort_i^{1948-1953}) \neq 0 \tag{3}$$

$$Cov(Cohort_i^{1948-1953}, v_i) = 0 \tag{4}$$

we can check the first assumption by estimated coefficients in first stage results of IV. In Table 2, famine cohort has fewer years of schooling, which is significant in statistics at 1% level. That is to say, the first assumption is satisfied. For the second assumption, we assume that being born during 1948 to 1953 is not related to the error term in Equation (2). Although we could not directly test the second assumption by design, it is a persuasive assumption because the binary variable of 1948-1953 born cohort is predetermined before China's Great Famine in 1959-1961. As people could not predict the famine at the timing of 1948-1953, the probabilities of selection into pregnancy and birth are very low.

4 Data

Our data source is the China Health and Retirement Longitudinal Study (CHARLS) that is a high quality nationally representative sample of Chinese residents ages 45 and older. The baseline national wave of CHARLS is conducted in 2011 and includes about 10,000 households and 17,500 individuals. The respondents of CHARLS will be followed up every two years. We only use the national wave of 2011 as cross-sectional data, as our identification strategy relies on birth information that is not time-variant. Panel structure is not required in this study. The CHARLS is very similar to the Health and Retirement Study (HRS), contains abundant information on demography, family, health, work/retirement, and income/expenditures/assets. The CHARLS includes main respondents and his/her spouses in the survey data. The main respondents are randomly selected. Because the intergenerational transfers are measured on the household level, we only keep the main respondents to analyze the transfers behaviors among generations.

For the analysis of education and intergenerational transfers, the sample is restricted as follows: (1) We only use individuals whose birth information is not missing. Those who were born before 1930 or after 1970 are dropped, as they are not too old or young to analyze intergenerational transfers. (2) We only use individuals that have adult children living apart from them. Like any other international data, the CHARLS does not measure the transfers with co-resident children because transfers within households are not specified. (3) We construct various covariate regarding gender, hukou status, marital status, number of children, ownership of residence and car, working status, labor and non-labor incomes, and savings, any observations having missing values in these variables are dropped. For the re-married family, the number of children is complicated because both of the couples may have children respectively from the previous relationship, and also they may have children in the newly constructed family. To simplify the analysis, we only take children who have a biological relationship with the respondents into account.

Schooling is the key variable in this study, which is calculated by educational attainment and dropout status. First, we recode categorical educational attainment into equivalent years. Educational attainment is defined by nine discrete educational categories as following: (1) illiterate, (2) less than primary education (includes who are semi-illiterate and those who drop out from the primary school), (3) primary school, (4) junior high school, (5) high school, (6) vocational school, (7) junior college, (8) college/university, (9) master degree. Second, for those who were dropped out of schools, schooling is corrected according to their completed grade.

Table 1 reports the descriptive statistics of the final sample, which is tabulated by famine and non-famine cohorts. In the full sample (Column 1), the average age is 60.46, and 47.4% of them are males. Most of the individuals are married and living with their spouses, and the average number of children is 2.38. Only 14.1% individuals are still working, and only 16.2% individuals have non-labor incomes regarding pensions and various subsidies. Column (2) and (3) show the statistics for famine and non-famine cohorts, respectively. Compared to non-famine cohorts, famine cohort has higher/lower likelihood of receiving/giving transfers from/to adult children, and they have fewer years of schooling. To test the difference between famine and non-famine cohorts, we show the results of t-statistic in Column (4). For the covariates, the famine cohort is only different in marital status and number of children, however, is not different in individual income and household assets.

5 Results

In this section, we report the estimated coefficients of schooling on intergenerational transfers using OLS and IV methods. In all regressions, linear probabilities are used along with robust standard errors. Before presenting the results of the OLS and IV, we discuss the validity of China's Great Famine as an instrument for education. Policy implications are discussed at the end of this section.

5.1 The First Stage

Table 2 reports the first stage results of IV estimation, which consistently indicates negative effects of China's Great Famine on schooling. In particular, average schooling among famine cohort is approximately 0.68 years shorter than other cohorts. The estimates keep highly stable across different specifications controlling for demographical characteristics, household assets, and individual incomes. Given the low average schooling among elder cohorts, the impact of the famine on years of schooling is quite large. We also report the F-statistic of the excluded instrument, which stably ranges between 87 and 94.

A good IV, in this case, should be highly correlated with schooling but should not affect the intergenerational transfers except through schooling. In other words, a valid IV should not be correlated with unobserved characteristics that are captured by the error term, u_i, in Equation (1). Some previous studies have shown that the famine directly affects people's health(Chen & Zhou, 2007; Fan & Qian, 2015);however, there is also evidence against them. Xu et al. (2016) finds that the effect of China's Great Famine on health is quite sensitive to the choices of health indicators and regression specifications. Even in some particular cases, positive effects on health are observed. Gorgens et al. (2012) points out the selection issues in the context of famine, and it helps understand the mixed empirical evidence on health. In the section on robustness checks, we control for objective health status to make sure that our instrument, exposure to China's Great Famine during school age, would not affect transfer behavior at old age except through schooling. Including health status does not alter our results.

5.2 OLS and IV

Table 3 presents the OLS and IV estimates of the effect of education on receiving transfers from adult children. Regardless of different specifications, education consistently indicates a small effect on receiving transfers from adult children in OLS estimation. According to Column (1), (2), and (3), one additional year of schooling is related to a 0.5% decrease in the likelihood of receiving financial transfers from children. After controlling for endogenous educational decisions, the corresponding IV estimates indicate a larger effect of schooling in magnitude. According to Column (4), (5), and (6), one additional year of schooling causally decreases the likelihood of receiving transfers by 2.6-3.5%. Specifically, the effect of schooling remains significant in statistics, even when household assets and individual incomes are controlled (Column 6).

Table 4 presents the OLS and IV estimated of the effect of education on giving transfers to adult children. In all OLS specifications (Column 1, 2, and 3), education stably has a very small effect on giving transfers to adult children. One additional year of schooling is related to a 0.5% increase in the likelihood of giving inter vivos transfers. On the contrary, the corresponding IV estimates are approximately 7 times larger than OLS, indicating a 3.7-3.9% increase in the likelihood of inter vivos transfers (Column 4, 5, 6). Similar with Table 3, additionally controlling for household assets and individual incomes does not alter our estimates. Other than education, gender, household assets, and number of children also have significant effects on giving transfers to adult children.

In both Table 3 and Table 4, the differences between OLS and IV estimates are quite large. When the endogeneity of schooling is not accounted for, the estimated coefficients of schooling on receiving/giving transfers from/to adult children are stably very small, although significant at 1% level in statistics. The IV estimates in each table do not alter the sign of OLS estimates, however, show larger coefficients in magnitude. The dramatic difference between OLS and IV suggests that schooling is related to intergenerational transfers through unobservable factors. As we have controlled for economic characteristics in both household and individual levels, the effect of education reflects a pure effect of education instead of acting as a proxy for incomes. Holding assets, incomes, and savings constant, the effect of schooling on transfers behaviors could be explained by unobserved non-cognitive human capital, such as generosity.

Our results have important policy implication regarding education and social security systems. According to the estimates in Table 3 and Table 4, the effect of schooling is robust across different specifications, indicating a positive impact on giving transfers and a negative impact on receiving transfers at old age, even when household assets and individual assets are holding constant. It implies that parents who have more years of schooling are not likely to depend on their children (this effect is not through higher incomes). Thus, in turn, implies that in addition to improving average educational attainments (e.g. extension of compulsory schooling, enrollment expansion of colleges), it is also essential to put effort into social security systems simultaneously. For example, introducing not only a public but also private long-term care insurance program to meet various elderly' demands.

6 Robustness Checks

6.1 Specification Tests

In this subsection, we check the robustness of our results by including more variables may be correlated with both education and intergenerational transfers. In particular, we have additionally controlled for household incomes and health status. Household incomes are defined as the sum of labor and non-labor incomes for both husband and wife. Health status is measured by a vector of 14 objective dummy variables indicating the status of disease, which includes (1)hypertension, (2)dyslipidemia, (3)diabetes or high blood sugar, (4)cancer or malignant tumor, (5)chronic lung diseases, (6)liver disease, (7)heart disease, (8)stroke, (9)kidney disease, (10)stomach or other digestive disease, (11)emotional, nervous, or psychiatric problems, (12)memory-related disease, (13)arthritis or rheumatism, and (14)asthma.

Table 5 reports the effect of education on receiving/giving transfers from/to adult children, after controlling for household incomes and health status. Specification 1 includes full controls as in Equation (1) and (2), which are also listed in the descriptive statistics in Table 1. Specification 2 controls for household incomes, and specification 3 additionally includes objective health status. The coefficients on education are highly stable across different specifications, although the sample size is different with Table 1 because of missing values on household incomes and health status. One additional year of schooling consistently reduces the likelihood of receiving inter vivos transfers and increases the likelihood of giving inter vivos transfers. Including household incomes and health status does not alter our results.

6.2 Placebo Tests

We also use four placebo treatments to test the robustness of the IV estimates. These placebo treatments are 1939-1941, 1942-1944, 1957-1959, and 1960-1962 birth cohorts, which are prior and posterior to the actual treatment. These placebo treatments should not have any effect on intergenerational transfers. If we find an impact of a placebo treatment, the actual treatment based results might be driven by other cohort-specific unobserved factors instead of China's Great Famine. The reduced form estimates of placebo treatments are displayed in Table 6, which are not significant even at 10% level for all placebo treatments. None of these placebo treatments have any effect on intergenerational transfers.

6.3 Heterogeneity

In this subsection, we investigate the heterogeneous effect of schooling by gender, as education could affect male's and female's transfer behavior in two different ways. We estimate effects of fathers' schooling and mothers' schooling on transfer behavior with adult children, respectively. To compare the results with baseline OLS and IV in Section 5, we include full control variable as listed in Table 1, which are basic demographic characteristics, assets, savings, and incomes.

Table 7 shows fathers' and mothers' schooling on inter vivos transfers, respectively. The first two columns show the estimates of schooling on receiving transfers, while the following two columns show those of giving transfers. Panel A shows the results of the sample of fathers, while Panel B shows those of mothers. On one hand, the effect of schooling on giving transfers to adult children at old age shows quite consistent signs and magnitude across genders. According to Column (4), an additional year of schooling is causally related to 3.8% increase in the likelihood of giving inter vivos transfers, regardless of gender. On the other hand, the effect of schooling on receiving transfers is heterogeneous between male and female. According to Column (2), an additional year of schooling of a father is causally related to 6.3% decline in the likelihood of receiving transfers, while the mother's schooling has no effect. All OLS estimates are significant but small in magnitude.

7 Concluding Remarks

In this paper, we estimate the causal effect of education on intergenerational transfer. Using representative micro data of Chinese elderly, we exploit exogenous variation in schooling induced by China's Great Famine to identify the causal effect of education on receiving/giving transfers from/to adult children at old age. During 1959 to 1961, millions of students had to drop out of primary schools. We use 1948-1953 birth cohort, which was eligible to attend primary schools during 1959 to 1961, as an instrument for schooling to take account for the endogeneity of educational choices.

Two major findings are as follows. First, schooling is causally related to lower probabilities of receiving transfers from adult children, and higher probabilities of giving transfers to adult children. The IV estimates show that one additional year of schooling reduces probabilities of receiving transfers by 3.6 percentage points, and probabilities of giving transfers by 3.8 percentage points. However, the corresponding OLS estimates are both at 0.4 percentage points level. The differences in decision-making regarding intergenerational transfers with adult children across schooling are assumed to be associated with unobserved non-cognitive human capital.

Second, to understand the mechanism how education affects transfer behavior, we estimate the effect of schooling conditioned on wealth levels. Holding assets,savings, and incomes constant, education consistently shows a positive effect on giving transfers, and a negative effect on receiving transfers. The net transfer flows from educated parents to their adult children regardless of pre-transfer household resources. This evidence implies that it is essential to enhance social security benefits when the private intergenerational transfer is shrinking in the context of education expansion.

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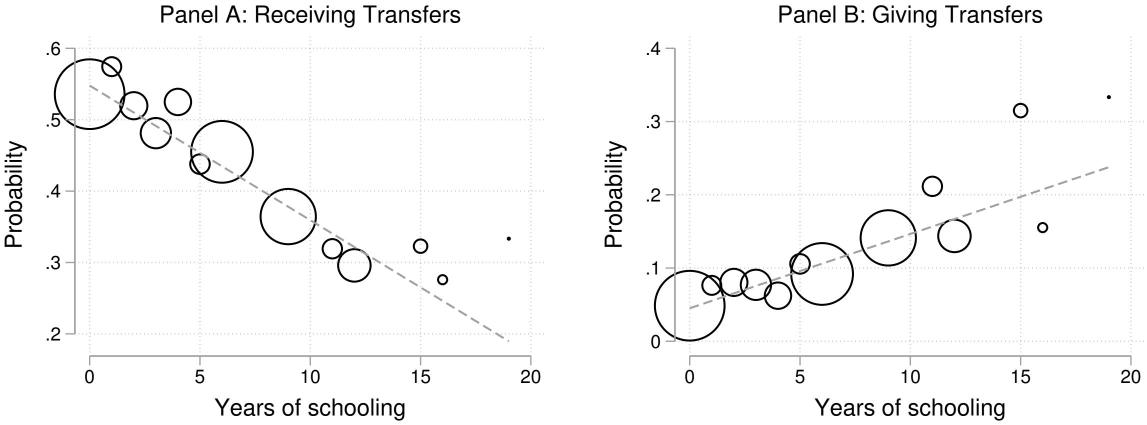
Figure 1: Probabilities of Receiving/Giving Transfers by Schooling



Notes: Author's calculation by China Health and Retirement Longitudinal Study 2011. Each circle is weighted by the number of people within each schooling group. Dash lines indicate weighted linear fit.

Table 1: Descriptive Statistics

		Cohort		
	(1)	(2)	(3)	(4)
	Full	Famine	Non-Famine	Difference
Receiving transfers	0.456	0.466	0.452	0.013
	(0.498)	(0.499)	(0.498)	(0.011)
Giving transfers	0.095	0.076	0.101	-0.025***
	(0.293)	(0.265)	(0.301)	(0.006)
Years of schooling	4.881	4.433	5.035	-0.602***
	(4.155)	(3.855)	(4.243)	(0.089)
Male	0.474	0.482	0.471	0.011
	(0.499)	(0.500)	(0.499)	(0.011)
Age	60.463	60.389	60.489	-0.100
	(8.821)	(1.680)	(10.178)	(0.189)
Urban *hukou*	0.191	0.191	0.191	-0.000
	(0.393)	(0.393)	(0.393)	(0.008)
Marital Status				
Married and living together	0.813	0.853	0.799	0.054***
	(0.390)	(0.354)	(0.401)	(0.008)
Married but not living together	0.069	0.064	0.071	-0.007
	(0.253)	(0.245)	(0.256)	(0.005)
Separated	0.004	0.005	0.004	0.001
	(0.067)	(0.074)	(0.064)	(0.001)
Divorced	0.007	0.008	0.007	0.001
	(0.084)	(0.086)	(0.083)	(0.002)
Widowed	0.107	0.070	0.119	-0.049***
	(0.309)	(0.255)	(0.324)	(0.007)
Household Assets				
#Children	2.380	2.271	2.417	-0.146***
	(1.409)	(1.116)	(1.495)	(0.030)
Ownership of residence	0.895	0.906	0.892	0.014*
	(0.306)	(0.292)	(0.311)	(0.007)
#Children*Ownship of residence	2.049	2.024	2.058	-0.034
	(1.462)	(1.243)	(1.530)	(0.031)
More residential property	0.096	0.103	0.093	0.009
	(0.294)	(0.304)	(0.291)	(0.006)
Having a car	0.046	0.041	0.048	-0.008
	(0.210)	(0.198)	(0.214)	(0.005)
Individual Income				
Working	0.141	0.139	0.142	-0.004
	(0.348)	(0.346)	(0.349)	(0.007)
Working*Labor income	0.717	0.611	0.754	-0.143
	(5.296)	(3.824)	(5.715)	(0.114)
Other income(Pension+Subsidy)	0.162	0.139	0.170	-0.031
	(1.897)	(1.827)	(1.921)	(0.041)
Saving	4.863	5.287	4.717	0.570
	(46.530)	(63.211)	(39.187)	(0.998)
Observations	11,420	2,923	8,497	11,420

Note: Standard errors in parentheses. Column (4) shows the raw difference between famine and non-famine cohorts. Descriptive statistics on provincial dummies are available upon request, which are not listed due to the space constraint. *** p<0.01, ** p<0.05, * p<0.1

Table 2: First Stage Results of China's Great Famine on Years of Schooling

VARIABLES	(1) Schooling	(2) Schooling	(3) Schooling
Famine cohort	-0.649***	-0.665***	-0.659***
	(0.069)	(0.069)	(0.069)
Male	2.782***	2.748***	2.592***
	(0.063)	(0.063)	(0.065)
Age	-0.146***	-0.132***	-0.123***
	(0.004)	(0.005)	(0.005)
Urban *hukou*	3.627***	3.540***	3.403***
	(0.095)	(0.095)	(0.095)
Marital Status			
Married but not living together	-0.129	-0.133	-0.241*
	(0.128)	(0.127)	(0.126)
Separated	-0.444	-0.400	-0.395
	(0.465)	(0.465)	(0.470)
Divorced	0.710*	0.745**	0.768**
	(0.368)	(0.363)	(0.352)
Widowed	-0.241**	-0.251**	-0.290***
	(0.101)	(0.101)	(0.101)
Household Assets			
# Children		-0.105*	-0.101*
		(0.059)	(0.059)
Ownership of residence		0.313	0.296
		(0.221)	(0.220)
# Children*Ownship of residence		-0.015	-0.007
		(0.060)	(0.060)
More residential property		0.358***	0.304***
		(0.106)	(0.106)
Having a car		0.714***	0.619***
		(0.159)	(0.157)
Individual Income			
Working			0.908***
			(0.102)
Working*Labor income			0.010**
			(0.005)
Other income(Pension+Subsidy)			0.061***
			(0.021)
Saving			0.004***
			(0.001)
Constant	12.435***	11.462***	10.800***
	(0.978)	(0.993)	(0.976)
Provincial FE	Yes	Yes	Yes
F-Statistic of instrument	87.61	94.16	93.38
Observations	11,420	11,420	11,420

Note: The base group for marital status is married with spouse. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3: The Effect of Education on Receiving Transfers from Adult Children

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Years of schooling	-0.005***	-0.004***	-0.004***	-0.027*	-0.035**	-0.036**
	(0.001)	(0.001)	(0.001)	(0.016)	(0.016)	(0.016)
Male	-0.012	-0.004	-0.005	0.051	0.082*	0.077*
	(0.010)	(0.010)	(0.010)	(0.045)	(0.044)	(0.042)
Age	0.012***	0.007***	0.008***	0.008***	0.003	0.004*
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Urban *hukou*	-0.143***	-0.121***	-0.122***	-0.060	-0.011	-0.014
	(0.013)	(0.013)	(0.013)	(0.059)	(0.057)	(0.055)
Marital Status						
Married but not living together	0.009	0.013	0.013	0.007	0.010	0.006
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Separated	-0.090	-0.084	-0.085	-0.101	-0.097	-0.098
	(0.068)	(0.065)	(0.065)	(0.068)	(0.066)	(0.066)
Divorced	-0.017	-0.027	-0.026	-0.001	-0.004	-0.002
	(0.051)	(0.051)	(0.051)	(0.054)	(0.055)	(0.055)
Widowed	0.043***	0.046***	0.046***	0.039**	0.041***	0.039**
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Household Assets						
# Children		0.033***	0.033***		0.031***	0.031***
		(0.009)	(0.009)		(0.009)	(0.009)
Ownership of residence		-0.095***	-0.095***		-0.083**	-0.083**
		(0.031)	(0.031)		(0.033)	(0.033)
# Children*Ownship of residence		0.009	0.009		0.008	0.008
		(0.009)	(0.009)		(0.009)	(0.009)
More residential property		0.035**	0.035**		0.046***	0.044***
		(0.015)	(0.015)		(0.017)	(0.016)
Having a car		-0.044**	-0.045**		-0.021	-0.024
		(0.020)	(0.020)		(0.024)	(0.023)
Individual Income						
Working			0.000			0.029
			(0.014)			(0.020)
Working*Labor income			0.001			0.001*
			(0.001)			(0.001)
Other income(Pension+Subsidy)			0.002			0.004*
			(0.002)			(0.002)
Saving			-0.000			0.000
			(0.000)			(0.000)
Constant	-0.532***	-0.267***	-0.272***	-0.251	0.087	0.067
	(0.067)	(0.073)	(0.070)	(0.209)	(0.195)	(0.185)
Provincial FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,420	11,420	11,420	11,420	11,420	11,420

Note: The base group for marital status is married with spouse. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: The Effect of Education on Giving Transfers to Adult Children

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Years of schooling	0.005***	0.005***	0.004***	0.040***	0.039***	0.038***
	(0.001)	(0.001)	(0.001)	(0.010)	(0.009)	(0.010)
Male	-0.005	-0.005	-0.012**	-0.100***	-0.097***	-0.100***
	(0.006)	(0.006)	(0.006)	(0.028)	(0.027)	(0.025)
Age	-0.004***	-0.004***	-0.004***	0.001	0.000	0.001
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Urban *hukou*	0.088***	0.088***	0.082***	-0.037	-0.031	-0.034
	(0.009)	(0.009)	(0.009)	(0.036)	(0.035)	(0.034)
Marital Status						
Married but not living together	0.009	0.009	0.002	0.013	0.013	0.010
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
Separated	0.016	0.021	0.021	0.033	0.035	0.035
	(0.039)	(0.039)	(0.039)	(0.044)	(0.044)	(0.043)
Divorced	0.040	0.046	0.049	0.016	0.021	0.023
	(0.043)	(0.042)	(0.042)	(0.044)	(0.044)	(0.043)
Widowed	0.000	0.002	-0.001	0.006	0.008	0.006
	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Household Assets						
# Children		0.012***	0.013***		0.015***	0.015***
		(0.004)	(0.004)		(0.005)	(0.005)
Ownership of residence		0.066***	0.066***		0.053***	0.054***
		(0.017)	(0.017)		(0.019)	(0.019)
# Children*Ownship of residence		-0.015***	-0.014***		-0.013***	-0.013***
		(0.004)	(0.004)		(0.005)	(0.005)
More residential property		0.033***	0.030***		0.021*	0.020*
		(0.010)	(0.010)		(0.011)	(0.011)
Having a car		0.010	0.006		-0.015	-0.016
		(0.015)	(0.015)		(0.017)	(0.016)
Individual Income						
Working			0.060***			0.029**
			(0.011)			(0.014)
Working*Labor income			0.000			-0.000
			(0.001)			(0.001)
Other income(Pension+Subsidy)			0.006***			0.004*
			(0.002)			(0.002)
Saving			0.000			0.000
			(0.000)			(0.000)
Constant	0.414***	0.346***	0.311***	-0.009	-0.034	-0.051
	(0.100)	(0.102)	(0.102)	(0.153)	(0.145)	(0.142)
Provincial FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,420	11,420	11,420	11,420	11,420	11,420

Note: The base group for marital status is married with spouse. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: IV Estimates with More Control Variables

	From Children			To Children		
	(1)	(2)	(3)	(4)	(5)	(6)
	Spec 1	Spec 2	Spec 3	Spec 1	Spec 2	Spec 3
Years of schooling	-0.032*	-0.032*	-0.031*	0.035***	0.035***	0.036***
	(0.016)	(0.016)	(0.016)	(0.010)	(0.010)	(0.010)
Constant	0.275	0.275	0.248	-0.115	-0.115	-0.126
	(0.174)	(0.174)	(0.178)	(0.098)	(0.098)	(0.100)
Household Income	No	Yes	Yes	No	Yes	Yes
Health Status	No	No	Yes	No	No	Yes
Observations	10,942	10,942	10,942	10,942	10,942	10,942

Note: Robust standard errors in parentheses. All specifications include full control variables in Table 2. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Placebo Tests

	From Children			To Children		
	(1) Spec 1	(2) Spec 2	(3) Spec 3	(4) Spec 1	(5) Spec 2	(6) Spec 3
Panel A: 1939-1941 Birth Cohort						
Placebo	0.005	0.005	0.007	0.014	0.014	0.015
	(0.020)	(0.020)	(0.020)	(0.010)	(0.010)	(0.010)
Panel B: 1942-1944 Birth Cohort						
Placebo	0.013	0.013	0.011	-0.002	-0.002	-0.002
	(0.018)	(0.018)	(0.018)	(0.009)	(0.009)	(0.009)
Panel C: 1957-1959 Birth Cohort						
Placebo	0.000	0.000	0.000	-0.012	-0.012	-0.013
	(0.016)	(0.016)	(0.016)	(0.011)	(0.011)	(0.011)
Panel D: 1960-1962 Birth Cohort						
Placebo	-0.004	-0.004	-0.003	0.022	0.022	0.022
	(0.018)	(0.018)	(0.018)	(0.014)	(0.014)	(0.014)
Observations	10,942	10,942	10,942	10,942	10,942	10,942
Household Income	No	Yes	Yes	No	Yes	Yes
Health Status	No	No	Yes	No	No	Yes

Note: Robust standard errors in parentheses. All specifications include full control variables in Table 2. Each coefficient denote a separate regression. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Heterogeneity by Gender

	From Children		To Children	
	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)
Panel A: Fathers				
Years of schooling	-0.004**	-0.063***	0.004***	0.038***
	(0.002)	(0.023)	(0.001)	(0.014)
Observations	5,412	5,412	6,008	6,008
Panel B: Mothers				
Years of schooling	-0.007***	-0.013	0.005***	0.038***
	(0.002)	(0.021)	(0.001)	(0.012)
Observations	5,412	5,412	6,008	6,008

Note: Robust standard errors in parentheses. All specifications include full control variables in Table 2. Each coefficient denote a separate regression. *** p<0.01, ** p<0.05, * p<0.1