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THREE ESSAYS ON INTERGENERATIONAL MOBILITY

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ABSTRACT

The study of intergenerational mobility has a long history in the social sciences. Previous studies have proposed various mobility concepts, strived to overcome empirical barriers to achieve accurate national measures, and mapped out cross-country patterns and time trends of mobility. The three essays in dissertation contribute to a recent strand of this literature which seeks to understand the mechanisms through which social status is transmitted across generations.

After an overall introduction in chapter one, chapter two uses recently published county-level data to study the determinants of intergenerational mobility, measured by income levels and teen birth rates. Following Solon's mobility model, we study the impacts of public investment in human capital, returns to human capital, and taxation. The results show that better school quality and higher returns to education increase adult incomes and reduce teen birth rates for children from low income families. By comparing counties within or adjacent to metropolitan areas to other counties, this study finds that urban upward mobility is sensitive to parents' education while non-urban upward mobility is sensitive to migration opportunities.

Chapter three employs court-ordered School Finance Reforms (SFRs) as quasi-experiments to quantify the effects of education equity on intergenerational mobility within commuting zones. First, I use reduced form difference-in-difference analysis to show that 10 years of exposure to SFRs increases the average college attendance rate by about 5.2% for children with the lowest parent income. The effect of exposure to SFRs decreases with parent income and increases with the duration of exposure. Second, to directly model the causal pathways, I construct a measure for education inequity based on the association between school district education expenditure and median family income. Using exposure to SFRs as the instrumental variable, 2SLS analysis suggests that one standard deviation reduction in education inequality will cause the average college attendance rate to increase by 2.2% for children at the lower end of the parent income spectrum. Placing the magnitudes of these effects in context, I conclude that policies aimed at increasing education equity, such as SFRs, can substantially benefit poor children but they alone are not enough to overcome the high degree of existing inequalities.

Chapter four studies the Intergenerational Persistence of Self-employment in China across the Planned Economy Era. It finds that children whose parents were self-employed before China's socialist transformation were more likely to become self-employed themselves after the economic reform even though they had no direct exposure to their parents' businesses. The effect is found in both urban and rural areas, but only for sons. Furthermore, asset holding data indicate that households with self-employed parents before the socialist transformation were more risk tolerant. These findings suggest that the taste for self-employment is an important conduit of parents' effects on self-employment, and that the taste being transferred can be mapped to known entrepreneurial attitudes.

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I would like to thank my family for their support, without them this dissertation would not be possible.

Chapter 1 Introduction

The study of intergenerational mobility has a long history in the social sciences. In this multi-disciplinary endeavor, sociologists focus on occupational status mobility and class mobility, while economists study income mobility. Previous studies have proposed various mobility concepts, strived to overcome empirical barriers to achieve accurate national measures, and mapped out cross-country patterns and time trends of mobility. A recent strand of this literature seeks to understand the mechanisms through which social status is transmitted across generations. The three essays from chapter 2 to chapter 4 contribute to this literature. This first section of the present chapter provides a common background for these three essays by familiarizing readers with the notions and concepts, and briefly summarizes spatial and temporal mobility patterns found in the previous literature. The second section of this chapter gives an overview for each of the essays.

1.1 Background

The concepts and measurements of intergenerational mobility

Intergenerational mobility, most broadly defined, is the extent to which children's social status depends on their family backgrounds. In order to operationalize mobility, a researcher needs to decide how to measure social status and how to measure dependence. The commonly used status measures include occupational prestige, class, and income. The measurements of dependence can generally be categorized into absolute and relative mobility measures. Relative mobility measures, in one way or another, compare the social status of children with different family social status. All measures without peer comparison, such as the absolute outcome of children at a certain level of family social status, children's living standard compared to their parents (not peers), and the absolute flow from one status group to another, are referred to as absolute mobility. The specific definitions of relative mobility and absolute mobility varies from study to study.

The study of intergenerational mobility has a long history in sociology, stretching back at least to the 1960s (Blau & Duncan, 1967) . The sociological study of intergenerational mobility is based on occupations as the building blocks for measuring social status. Starting from detailed occupation information of parents and children, there are two alternative traditional approaches in sociology. One approach is to assign a status or prestige value to each occupation based on the average income and education levels of occupations (Hauser & Warren, 1997). The correlation between father's occupational prestige and sons' occupational prestige, called occupational status mobility, is then used as a measure for relative mobility. Another approach is to collapse occupations into discrete categories called classes based on sector (manual, non-manual, and agricultural), employment (self-employed, salaried), skill level and supervisory status (supervisor, supervisee). The most detailed forms distinguish 12 different classes (Erikson, Goldthorpe, & Portocarero, 1979), but in practice they are often collapsed into 7 categories or less (Goldthorpe & Erikson, 1992). The frequency table with each cell containing the absolute flow from a class origin to a class destination contains the full mobility information under the given class definition. One concern is that some of observed flow from class origin to class destination is mechanically caused by structural changes of the society, such as the shrinking of the farm sector. To separate social fluidity from structural change, sociologists rely on odd ratios which are independent of the distribution of parents and children in the classes. This most common practice is then to model log ratios parsimoniously using log linear models to facilitate mobility comparison.

In the recent two decades, mobility research had captured the interest of economists as well (Black & Devereux, 2011; Solon, 1992). The reason for economists' later entry into this field is partly due to their preference for income as the status measure: adult children cannot recall their parents' income nearly as well as they remember their occupations. With the maturity of large scale panel datasets, study of income mobility became possible. However, the empirical measure of income mobility is still challenged by measurement issues regarding income. Even when income is accurately recorded, there are still discrepancies between observed income and the underlying permanent income. The first discrepancy comes from transitory fluctuations in income which creates a measurement error that biases the mobility measure down (Zimmerman, 1992). It can be shown that the bias is stronger when transitory income shocks are positively correlated across years, which is a common situation (Mazumder, 2005). The solution is to average income over multiple years. The second discrepancy comes from the ages at which parent and child incomes are measured (Haider & Solon, 2006). While transitory income shocks create random errors, one-period income systematically differ from permanent depending on the ages at which the incomes are measured. This life cycle bias could be upward or downward. Empirical studies that look at earnings over lifetimes have found that life-cycle bias stabilizes during middle age (roughly 30 ~ 50); therefore it is preferable to measure incomes at this age range.

While the discussion above covers the measurement of status in the economic literature, there remains the question of how to measure dependence. Similar to class mobility, the complete mobility pattern for income can be summarized by a transition matrix which summarizes mobility patterns across quintiles. However, traditionally, economists preferred a summary measure called intergenerational elasticity (IGE), which is the coefficient from regressing the log of children's income on the log parents income. (A closely related measure is intergenerational correlation, which is equivalent to the aforementioned IGE but with both the dependent variable and independent variable standardized to have standard deviation of one). These are a measures of immobility since they are higher when the intergenerational dependence is stronger.

Collapsing a two dimensional association into one parameter, the IGE, makes it easier to compare mobility across space and time. However, the reduction of dimensionality comes at the cost of information. First, IGE does not distinguish between upward and downward mobility in the sense that it does not tell us the proportion of children who have moved up or down. Second, using an OLS coefficient may impose linearity on a relationship that is potentially non-linear. Third, compared to other measures introduced below, IGE is more sensitive to measurement errors related to transitory income shocks and life-cycle biases. Fourth, using the log specification requires researchers to make arbitrary decisions regarding how to treat non-positive income. Finally, IGE depends on the distribution of parent and child income (Corak, Lindquist, & Mazumder, 2014).

With the recent availability of administrative datasets that have larger numbers of linked parent-child pairs compared to surveys, economists have developed new mobility measures. These new measures are based on income ranks, which automatically addresses the problem of dependence on income distribution. Some of these measures essentially look at cells, or a combination of cells, in transition matrices. For example, the "American Dream Index" introduced by Chetty, Hendren, Kline, & Saez (2014) looks at the possibility of children in the bottom 20 percentile to move into the top 20 percentiles. The directional rank mobility (DRM) measures the percentile of children who moved above or below their parents by certain percentiles (0 being the most basic set-up). Analogous to IGE, overall mobility can be measured by the coefficient from regressing parent income rank on child income rank (Bhattacharya & Mazumder, 2011). At least in the case of US, administrative data better conform to rank-rank linearity than to log-log linearity (Chetty, Hendren, Kline, & Saez, 2014).

Time trends and cross-sectional comparisons of intergenerational mobility

Besides developing and refining the measurement of mobility, many mobility studies are dedicated to mapping out the cross-sectional patterns and time trends of mobility. Until very recently, mobility has been measured at the country-level. However, mobility estimates are not directly comparable across studies because datasets from different countries are not compatible. Taking IGE for example, Grawe (2006) looks at more than 20 studies from different countries and finds that 20% of the variance in the estimated IGE can be attributed to the different ages at which parent income is measured. Other empirical details, such as the number of years that income is averaged over, the coding of non-negative income, and the definition of income, can all create artificial variances in IGE estimations. Similar compatibility issues also exist in class mobility studies. Therefore, in the examination of cross-country evidence, studies that use harmonized data and techniques should be given more weight.

In terms of income mobility, Jantti et al., (2006) estimated the IGE and intergenerational correlation between fathers and sons in a consistent way for 7 countries (US, UK, Denmark, Norway, Finland, Norway, and Sweden). In order to achieve consistency, they had to make compromises by using one year's earnings for father's income. Therefore, their estimates may not be the most accurate for individual countries, but are designed to reflect the relative standing between countries. They find that, for both IGE and intergenerational correlation, the measures for the US and UK are higher than the Nordic countries. These findings are consistent with patterns found in studies for individual countries and suggest that the US society is relatively immobile. Studies looking at father-daughter pairs and family income (Raaum et al., 2008) instead of father-son pairs have found a similar ranking. However, this conclusion is somewhat weakened by studies using alternative income mobility measures. Jantti et al., (2006) examine the transitional matrices and find that low US mobility is mostly caused by lower mobility at the two tails. Recently, Corak et al. (2014) conducted another study using comparable datasets and techniques and find that the differences between the US and Sweden in terms of intergenerational correlation and rank-rank correlation are from subtle to non-existent. It suggests that the previously observed difference might be caused by different degrees of non-linearity and inequality. These recent findings move the income mobility results closer to the class and occupational status mobility literatures in sociology, which consistently finds the US to be more mobile in terms of occupational prestige and class than most other industrialized countries (Björklund & Jäntti, 2000).

In terms of time trends in income mobility, earlier studies produce mixed results for the United States: some studies (Aaronson & Mazumder, 2008; Foroohar, 2011) find decreasing mobility while others find mobility to be trendless (Lee & Solon, 2009). These studies are based on IGE and intergenerational correlations. Recently, Chetty, Hendren, Kline, Saez, & Turner (2014) use higher quality data and rank-based mobility measures and find evidence supporting the later view. Therefore, in terms of time trends, the developments in the income mobility literature is also moving closer to the sociological literature which finds class mobility to be trendless in industrialized countries, at least in the 20th century (Goldthorpe & Erikson, 1992).

Understanding the mechanisms of intergenerational mobility

To understand the spatial and time trends of mobility has great theoretical and practical importance. First, the study of the aggregated mobility measures can shed light on how social status is obtained and maintained in the constituent families. It can provide empirical evidence to formulate and test micro-level theories of the intergenerational process. Second, studying the causal effects of socio-economic conditions on mobility outcomes can inform policymaking by evaluating the extent to which children’s outcome can be affected by intervention, and which types of policies are most likely to be effective.

Unsurprisingly, most of the studies seek to explain spatial and temporal variations in mobility by differences in the education system. One theoretical prediction is that mobility should increase with the quality of public education (Davies, Zhang, & Zeng, 2005). In support of this view, Ichino, Karabarbounis, & Moretti (2011) find that, across 10 developed countries, there is a negative correlation between public education expenditure and IGE, hence a positive relationship between mobility and the quality of public education. A state-level study in the US draws similar conclusions with state fixed effects being controlled (Mayer & Lopoo, 2008). Pekkarinen, Uusitalo, & Kerr (2009) study the effects of exogenous changes in the education system on intergenerational mobility using a Finnish education reform that delayed tracking from age 11 to age 16. Given that the students going into the lower tracking, which had no access to college education, are mostly from families with lower social status, the reform can potentially increase access to higher education for these children. Capitalizing on the fact that the reform gradually rolled out in different regions in the country, the authors use the difference-in-difference method to find that this reform had decreased IGE and increased intergenerational mobility.

Compared to cross-country studies, studying the subnational patterns of mobility benefits from relatively homogeneous samples, especially when multiple cohorts are observed and regional fixed effects can be controlled. Furthermore, exploiting exogenous changes in the education system can parse out the causal relationship from correlation. Using the combination of subnational data and exogenous changes in the education system is a promising approach to understand the determinants of intergenerational mobility.

1.2 Overview of dissertation

The remainder of this dissertation consists of three essays that seek to contribute to the understanding of social mobility. By using novel data sources, and by creatively using conventional data sources, these essays provide new evidence on the roles played by education and family in the evolution of social structures.

The first two essays utilize the recently available subnational mobility measures compiled by Chetty, Hendren, Kline, & Saez (2014) from deidentified federal tax records. Compared to conventional survey studies, this dataset is superior in terms of number of observations and accuracy of income and other information. With a sample covering more than 90% of the population, the CHKS data provides the first accurate data for mobility in small geographical areas such as counties and commuting zones. It offers an opportunity to examine cross-sectional patterns and differential regional trends in mobility in order to draw casual conclusions. Using the CHKS datasets, the first two essays focus on how mobility is shaped by social environments such as education and labor market conditions.

The first essay is a cross-sectional study of upward mobility in US counties for the 1980 ~ 1982 birth cohort. This study focuses on the income rank and teen birth rate of children whose parent income ranks 25% nationally. Because of the linear relationships between these two mobility outcomes and parents' national income rank, the outcomes at the 25% percentile rank also characterize the average outcomes of children in the bottom 50% of the parent income distribution.

The analysis is motivated by Solon's (2004) model of intergenerational income mobility. In this model, altruistic parents derive utility from their own consumption as well as their children's income. With a given level of government investment in human capital, parents choose an optimal private level of investment in education to maximize their utility. This model predicts that children's income should increase with the level of public investment in education and market returns to human capital, and decrease with taxation. While existing theories for teen birth rate are too abstract to apply to empirical research, the literature suggests that the decision to give early birth is the direct result of a lack of economic opportunities (Kearney & Levine, 2011).

Since teen birth rate reflects the same underlying economic opportunities as adult income, a similar set of variables is adopted for teen birth rates.

The empirical challenge of testing Solon's model comes from the myriad of endogenous local conditions. Because this study uses cross-sectional data, it is impossible to conduct a natural experiment type study. It also not possible to control for all sources of omitted variables. Instead, this study hypothesizes major sources of endogeneity for each variable of interest and addresses them using appropriate instrumental variables. For the quality of public education and tax rate variables, it is assumed that the endogeneity arises from unobserved parent characteristics affecting both private investment in human capital and public policies through political process. Therefore, time-lagged and spatial lagged variables are chosen as instruments because they are not affected by parents' actions at the time and location under study. For the returns to human capital variable, it is assumed that the major source of endogeneity comes from the simultaneity of the supply and demand of human capital. Therefore national level labor wage shocks are used as the instrument for it.

Using instrumental variable regressions, this study finds that higher school quality leads to higher adult income and lower teen birth rates, consistent with Solon's model. Evaluating the effects of returns to education on mobility, I find that the average returns to education in a region have positive impacts on low income children's adult income, also consistent with Solon's model. The result for tax rates is mixed. While Solon's model predicts that a higher tax rate should lead to lower mobility, this prediction is only supported by the teen birth rates regression in the non-urban sample. The expenditure effect of taxation may confound the income-decreasing effect, which points to the need of a model that takes into account the complex and potentially conflicting effects of tax collection and spending.

By comparing urban counties with non-urban counties, it is shown that the existing human capital stock is more important for mobility in urban areas while migration opportunities are more important in non-urban areas. The finding that rural mobility relies more on out-migration to move up the income ranking presents a challenge for policymakers who care not only about the prosperity of people, but also the prosperity of places. To be able to retain upwardly mobile children, rural areas need to create high skilled jobs and favorable environments for start-up entrepreneurs.

The second essay looks at the differential trends of intergenerational mobility across multiple birth cohorts in U.S. commuting zones. Specifically, I quantify to what extent, if any, the exogenous shift in school equality caused by court mandated school finance reforms (SFRs) improves regional intergenerational mobility. School finance lawsuits started with the Serrano v. Priest (1973) case in California and fundamentally changed the landscape of primary and secondary education finance in the United States. Plaintiffs in these lawsuits argued that the states' education finance system had failed to provide equitable education financing guaranteed by law. If the plaintiffs prevailed in the lawsuits, the courts would order the state legislatures to revise their education funding formulas and channel more funding to poor districts. By 2010, school finance lawsuits had been brought in 42 states among which 29 had at least one verdict in favor of reforms. These lawsuits are still going on today and will continue to happen in the future. Given their prevalence and importance, it is crucial to thoroughly evaluate the effects of past SFRs. In terms of policy evaluation, this study provides much needed additional information on the long-term impacts of SFRs, especially the impacts of later reforms. It fills gaps in the literature by quantifying the impact over the entire parent income spectrum, and by studying the impacts of SFRs on college access. More generally, it is one of the first causal studies on the determinants of intergenerational mobility.

The first part of this essay uses reduced form difference-in-difference analysis to quantify the impacts of exposure to SFRs on college access and adult income. Using panel fixed effect models, controlling for time varying commuting-zone-level covariates, it finds that 10 years of exposure to SFRs increases the average college attendance rate of lowest-parent-income children by 5.72%, and reduces the attendance gap between lowest and highest-parent-income children by 3.92%. The impact of SFRs gradually decrease as parent income increases. SFRs have no statistically significant impact above the 70% parent income rank, however the point estimate of its impact remains positive throughout the parent income ranking. This result suggests that SFRs achieve equalization through leveling-up, not leveling-down. The analysis on children's adult income did not find SFRs to have any statistically significant impacts across the parent income distribution. The most likely reason is that child income is measured only for one year at age 26. Single year income this early in the life is likely to be a poor measure of permanent income.

The second part of this essay attempts to answer the more general question of how education equity determines intergenerational mobility. For each state and each birth cohort, school district level per pupil expenditures are regressed on the school district's average income, and the regression coefficients are taken as measures of education equity. Since both educational expenditures and college access depend on unobserved student characteristics, exposure to school finance reforms is used as the instrumental variable to address the endogeneity problem. The result shows that a one standard deviation increase in education equity increases the college attendance rate of the poorest children by 2.87%. This impact also decreases with parent income and becomes small and statistically insignificant for children from high income families.

Overall, the results of this study suggest that SFRs, and increasing the equity of education in general, are effective in increasing the college attendance rate for the poorest children without negative effects on wealthier children. While qualitatively the college attendance gap between low and high income children is reduced, the magnitude of this reduction is small compared to the large existing inequality.

While the first two essays explore the important roles played by institutions in shaping intergenerational mobility, the third essay emphasizes the strength and endurance of family influence. Turning from the United States to China, I study the persistence of self-employment in Chinese families across the planned economy era. Entrepreneurship is considered as an engine for economic growth and a pathway for social mobility. Empirically, self-employment is often used as an indicator for entrepreneurship. Studies across the world consistently find the intergenerational correlation of self-employment to be strong.

This intergenerational persistence was put to the severest test in China during the planned economy era. In 1950s, China went through socialist transformation under the rule of the Chinese Communist Party. As the transformation was completed around 1960, all private economic activities became illegal and remained so for more than two decades until the economic reform in late 1970's. As a result, a generation of children had no chance to observe or participate in their parents' business. Under the political environment where entrepreneurs are marginalized and vilified, parents would have no incentive to teach their children business skills. While the intergenerational persistence of entrepreneurship is widely observed across the world, one doubts whether it could survive this historical period in China. Answering this question helps us understand the historical legacy of that era and sheds light on the mechanism through which entrepreneurship is passed on from one generation to the next.

Using a nationally representative survey, this study finds children whose parents were self-employed before the socialist transformation were still more likely to become self-employed after the economic reform. Relatively speaking, having at least one self-employed parent (before the socialist transformation) increases the children's chance to be self-employed by 28% in urban areas and by 30% in rural areas. However, this effect is only found for sons, and not for daughters. For daughters in urban areas, the result indicates that the self-employment decision is strongly influenced by parents-in-law, suggesting either a direct influence from in-laws or indirect influence through husbands.

To further explore the mechanisms of this intergenerational persistence, I explored a possible explanation which is the transfer of the preference for self-employment. It is well documented in the literature that entrepreneurs have a distinctive set of personal characteristics, such as higher risk tolerance. Self-employed parents could pass on these characteristics to their children and make them more inclined to become entrepreneurs. In the economic literature, the share of risky assets is a widely used indicator for the owner's risk attitude. By analyzing the structure of household financial assets in the urban area, I find that children with self-employed parents own higher shares of risky assets, indicating that they are more risk tolerant. Although this analysis cannot rule out alternative explanations such as the inheritance of ability, it provides suggestive evidence for the inheritance of characteristics.

Overall, the three essays in this dissertation paint a picture of balanced influences of environmental factors and the family on children's social status. The first two essays find that the quality and progressivity of public education as well as labor market conditions can significantly influence children's economic outcomes, while the third essay finds the influence of families to be fundamental and long lasting. It suggests that the effectiveness of policy interventions in changing mobility likely differ by the mobility concept in question and there is no unique answer to the nature vs. nurture question.

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Chapter 2 Human Capital and Intergenerational Mobility in U.S. Counties

Abstract

With inequality returning to historically high levels, intergenerational mobility has attracted growing attention from policymakers and academics. We use recently published county-level data to study the determinants of intergenerational mobility, measured by income levels and teen birth rates. Following Solon's mobility model, we study the impacts of public investment in human capital, returns to human capital, and taxation. The results show that better school quality and higher returns to education increase adult incomes and reduce teen birth rates for children from low income families. By comparing counties within or adjacent to metropolitan areas to other counties, we find that urban upward mobility is sensitive to parents' education while non-urban upward mobility is sensitive to migration opportunities.

Keywords: Income mobility, teen birth, counties, human capital

2.1 Introduction

As inequality in the United States approaches an all-time high (Piketty & Saez, 2003, series updated to 2014), the need to understand determinants of intergenerational mobility has become critical because higher intergenerational mobility may even the odds that children today improve their social status. However, despite being acclaimed as “the land of opportunity”, empirical studies often find that a child’s success in the U.S. depends more on her parents’ status than is true in most other developed counties, especially Scandinavian countries (Björklund & Jäntti, 1997; Solon, 2002; Corak, 2013).

Improving intergenerational mobility requires knowing how it is determined. Factors suggested in the theoretical literature (for example, Becker & Tomes, 1979, 1986; also see Behrman, 1997 and Mulligan, 1997 for surveys of mobility theories), such as the quality of public education and tax policies, are often determined by local governments and communities. Therefore, exploring the causal relationship between variation of policy and economic conditions, and of mobility across space, promises to enhance our understanding of the determinants of mobility. For regional scientists, studying intergenerational mobility at the regional-level is a natural extension of the longstanding interests in regional poverty (Levernier, Partridge, & Rickman, 2000; Partridge & Rickman, 2007; Partridge, Rickman, Olfert, & Ali, 2015) and regional inequality (Glaeser, Resseger, & Tobio, 2009; Levernier, Partridge, & Rickman, 1998; Partridge, Rickman, & Levernier,1996; Partridge, 2005; Rodríguez-Pose, & Tselios, 2009). By studying mobility, we may better understand how poverty and inequality persist across generations with regions.

Chetty, Hendren, Kline, & Saez, (2014, CHKS from here on) recently achieved a major breakthrough in the analysis of small-area intergenerational mobility. Using deidentified Federal income tax files, they were able to develop county level estimates of relative and absolute intergenerational upward mobility in the U.S. They find a high degree of cross-county variation in mobility comparable to cross-country variations found in earlier studies. Within regions, nearby cities can occupy opposite ends of the mobility spectrum (for example, Pittsburgh ranks 2nd among the 50 largest US cities while Cleveland, merely 130 miles away, ranks 40th). Furthermore, in contrast to the perceived economic disadvantage in rural areas in rural areas, mobility is not only higher in relatively rural regions, such as the Great Plains, but also higher in rural areas than in neighboring urban areas. These findings provide additional support for the notion that intergenerational mobility is to a large degree locally determined. A subsequent paper (Chetty Hendren, & Katz, 2016) presents quasi-experimental evidence of a causal link between time spent growing up in a better neighborhood (as measured by the upward mobility of children who spend their entire lives in the neighborhood) and children's outcome.

Building on CHKS and subsequent works, this study uses a different approach in seeking to establish the causal relationship between mobility and locality. It contributes to the literature by studying specific locational characteristics as mobility determinants and by exploring the sources of the urban-rural mobility gap. This paper is one of the first to utilize the local level mobility measures published with the CHKS study. We study two outcomes for children in the 1980 ~ 1982 cohort: the national percentile income rank (absolute upward mobility) and the teen birth rate. Following CHKS, we focus on children whose parent income is at 25% in the national ranking. As CHKS showed, both child income rank and teen birth rate have linear relationships with parent income rank, and therefore children's outcomes at 25% parent income rank also represent the average outcomes of children in the bottom 50% income distribution. Using both pre-labor market outcomes (teen birth rates) and labor market outcomes (absolute upward mobility) allows us to shed light on how and when the mobility determinants become effective.

The theoretical foundation of this paper is Solon's (2004) intergenerational mobility model. This model hypothesizes that public investment in human capital and returns to human capital (the increase in income associated with additional educational attainment[1]) have positive impacts on absolute income mobility while the tax rate has a negative impact. Among these three factors, investment in human capital has been well studied, but returns to human capital and taxation have received far less attention in the empirical mobility literature.

The major empirical challenge faced by this kind of study is the endogeneity of mobility determinants. We assume that the endogeneity of the two policy variables (public investment in education and the tax rate) mainly arises from unobserved parental characteristics that influence both the policy formation process and children's outcomes. Deeply time-lagged variables and spatially lagged variables are used as instruments since they are correlated with the endogenous variables through historical legacy and institutional peer effects, yet are not affected by parents' political influence. For the returns to human capital variable, we assume that the endogeneity is related to the simultaneity of the demand for and supply of human capital. National wage shocks are used to instrument for the returns to education since they may change the wage difference between skilled and unskilled labor. Various statistical tests are conducted to evaluate the performance of the instruments.

In order to study urban-rural differences, our analysis is conducted at the county-level, where urban and rural areas can be more accurately defined than at the commuting-zone level

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[1] Returns to human capital (education) are multi-dimensional, and include increases in adult income, cognitive skill levels and learning, appreciation for art and literature, effectiveness in civil participation, as well as positive social externalities. This study is only concerned with the economic dimension, i.e. returns to education that contribute to the increase in income.

(commuting zones contain both rural and urban counties). Separate analyses are conducted for counties within or adjacent to metropolitan areas (which we call urban counties) and other counties (which we call non-urban counties[2]). When possible, the regression results are compared using statistical tests to see how urban mobility and non-urban mobility respond differently to the independent variables.

We find that the income-adjusted high school dropout rate, an output-based measure of public education quality (or the lack thereof), decreases absolute upward mobility and increases teen birth rates. The difference between the median earnings of college and high school graduates, a proxy for returns to education, also positively impacts absolute upward mobility. By comparing urban counties with non-urban counties, we show that the urban-rural mobility gap is caused by both the differences in the levels of mobility determinants, such as the lower school quality in urban counties, and differences in how these determinants affect mobility, for instance non-urban mobility responds more sensitively to migration opportunities.

This study contributes to the nascent literature of regional intergenerational mobility in several aspects. First, we address the endogeneity issues of major mobility determinants to separate causality from correlation. Second, we follow a unified theoretical framework that leads us to include in our analysis returns to human education and tax rates, two previously under-

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[2] Our geographic units are different than the conventional metropolitan and nonmetropolitan geographies. We use both an "urban/non-urban" dichotomy and a more conventional "metro/adjacent/nonmetro". Our "urban" geography includes both metropolitan counties and the nonmetropolitan counties that are adjacent to, and thus most influenced by, the metropolitan areas. Our "non-urban" counties are the nonmetropolitan counties that are not adjacent to metro areas.

studied mobility determinants. Third, we shed light on the sources of the urban-rural mobility gap, which may help local policymakers better understand the challenges they face.

The rest of this chapter is organized as follows. Section 2.2 briefly introduces Solon's (2004) intergenerational mobility model. Section 2.3 first discusses the endogeneity issues and our empirical strategy, and then describes key variables with more detailed information for the control variables discussed in the Data Appendix. Section 2.4 reviews descriptive statistics for all variables, evaluates the performance of instrumental variables, and presents regression results. Section 2.5 discusses the results and conclusions.

2.2 Theory

Solon (2004) offers a model of intergenerational income mobility that considers the roles played by public investment in human capital (education), market returns to human capital (education), and taxation. In this model altruistic parents maximize a utility function (Equation (2.1)) that consists of their own consumption C_p and their children's future income y_c, weighted by the intergenerational altruism factor α. The choice variables are the amount of own consumption C_p and investment in children's human capital I_p, which, via the budget constraint (Equation (2.2)), add up to parent's after tax income $(1-\tau)y_p$, with τ being the tax rate.

(2.1) $$U_p = (1-\alpha)logC_p + \alpha logy_c$$

(2.2) $$(1-\tau)y_p = C_p + I_p$$

Children's human capital h_c (Equation (2.3)) contains the observable components of private (I_p) and public (G_p) investment in human capital, and a random component (e). A child's human capital then determines her income according to Equation (2.4), where p is the market return to human capital.

(2.3) $$h_c = \theta \log\left(I_p + G_p\right) + e$$

(2.4) $$logy_c = \mu + ph_c$$

Solving the first order conditions for optimal private investment in human capital I_p^*, and entering them into (3) and (4), we arrive at an expression of children's income conditional on their parents' income (Equation (2.5)). Equation (2.5) shows that a child's conditional expected income increases with G_p and p, and decreases with τ. Intuitively, a child will earn higher income if the government invests more in her human capital and if her human capital earns a higher return. Note that the effect of p on absolute mobility is opposite to its effect on relative mobility: while higher human capital price is relatively more beneficial for wealthier children who are more likely to be better educated, it benefits all children in absolute terms. Higher tax rates levied on parents reduce children's future income by decreasing disposable income which lowers private investment in human capital.

(2.5) $$(logy_c)_{\bar{y}_p} = \mu + \theta plog\left[\frac{\alpha\theta p}{1-\alpha(1-\theta p)}\right] + \theta plog\left[(1-\tau)\bar{y}_p + G_p\right] + p(e)_{\bar{y}_p}$$

Solon's model is a strictly micro-level model and does not consider how governments spend money other than on public education, and how those expenditures affect intergenerational mobility. Below we suggest that this limitation explains why the predictions about the negative effects of tax rates are not consistently supported by the results.

Existing models of fertility (Becker & Lewis, 1973) and teen birth (Kearney & Levine, 2011) are highly abstract and do not offer direct guidance for variable selection as in the case with Solon's model. However, a common theme in these models is that the decision to have a child is rational. Early child bearing occurs when teenagers perceive limited chances in life, and thus a low opportunity cost for early child bearing. This theoretical view is supported by empirical evidence that much of the observed ill effects of teen parenthood on later outcomes are results of pre-existing disadvantages (see Hoffman [1998] for a review of earlier studies, and Levine & Painter [2003] for a more recent example).

In other words, teen birth rate also measures (the lack) of economic opportunities, so CHKS included it as a supplementary mobility measure. In the teen birth analysis, we choose mostly the same independent variables as in the income analysis since teen birth rate also measures economic opportunities. We drop the returns to human capital variable in the teen birth analysis since it affects children through the labor market which happens after teen birth.

2.3 Method

2.3.1 Empirical strategy

Solon's model produces a log-linear functional form for the relationship between parent and child income, the slope of which is the well-known intergenerational elasticity. CHKS (2014), however, found a strong linear relationship between parent and child income rankings (and teen birth rate), instead of between parent and child log income levels (see figure 2A and figure 4B in CHKS). Therefore we use income rank and teen birth rate instead of log child income as outcome variables. We estimate the linear approximation of Equation (2.5) at the county-level:

$$y_j = \beta_0 + \beta_1 G_j + \beta_2 p_j + \beta_3 \tau_j + \boldsymbol{\gamma} \cdot \mathbf{Z_j} + e_j \tag{2.6}$$

In the above equation, j is the index for each county. Variable y_j represents the income of the child conditioning on parent income. G_j, p_j, and τ_j stand for public investment in education, returns to education, and the tax rate. $\mathbf{Z_j}$ is a vector of control variables, β's and $\boldsymbol{\gamma}$ are regression coefficients, and e_j is the error term. In a supplementary analysis, y_j is replaced by teen birth rates.

Public education, returns to education and the tax rate are all endogenous to the determination of intergenerational mobility, and it is difficult to identify all the mechanisms that can cause them to be correlated with the error term. However, it is helpful to consider the most likely pathways in order to inform the selection of instrumental variables and robustness checks.

The quality of public education and tax rate can be correlated with omitted parent characteristics through the political processes that determine these public policies. Parents who support higher investment in schools and tolerate higher tax rates are also likely to commit more private resources to their children's education. We choose deeply time lagged variables and spatially lagged variables to solve this potential endogeneity problem since they are correlated with the endogenous variables through historical legacy and institutional peer effects (meaning that they are likely to adopt similar policies with the neighbors as well as maintaining their own policies in the past), yet they are not influenced by parent characteristics or lobbying efforts in that county at that time.

Parents' location choices can also cause public policies and parent characteristics to be correlated. Our instruments cannot address this kind of endogeneity because the current state of public policies, whether caused by historical legacies or institutional peer effects, remains the basis of parents' location choice. To evaluate the bias caused by migrating parents, we conduct a robustness check by reproducing the analysis at a higher geographical level (commuting zones) where the migration rate is lower. If location choice is driving the results, the effects should diminish when there is less migration.

The endogeneity issue with the returns to education variable is most likely caused by the familiar demand-supply simultaneity. The dependent variable, children's income, is partly derived from supplying of human capital, while the independent variable, return to human capital, is the price. We use national wage rate shocks as instruments for the return to education variable. These wage shocks are relevant instruments because they can create differential changes in the earnings of college and high school graduates. Later on, it is going to be shown in the first stage regression results that the current wage gaps are indeed determined by past shocks. This is not surprising since we use shocks over a long period of time. Since these shocks are determined by national trends and slow changing local industry compositions (see the data section for how they are constructed), they are arguably exogenous. Assuming the control variables in $\mathbf{Z_j}$ to be exogenous, we have 3 endogenous variables and 9 instruments (see next section for details), which allows us to conduct over-identification tests. Other local policy variables such as subsidies are also relevant, yet they are likely to be endogenous and require additional instruments which would make the model intractable.

2.3.2 Data

The two dependent variables, absolute upward mobility and teen birth rates, are both based on deidentified federal income tax records of children born between 1980 and 1982, matched to their parents' tax records (CHKS) [3]. A child is assigned to the location where her parents first claimed her as a dependent (data start in 1996). In a given geographical unit, absolute upward mobility (Figure 2.1) is defined as the average income percentile ranking in 2012 (0~100 from low to high income) of adult children whose parents ranked 25% in the national ranking in 1996-2000[4]. Pre-tax income is averaged over 1996~2000 for parents and 2011~2012 for children. Since the rankings are national, actual income is a monotonically increasing function of ranking,

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[3] http://www.equality-of-opportunity.org/index.php/data

[4] Since CHKS found that child and parent incomes have a strong linear relationship, conditioning child income ranking at the 25% parent ranking, the midpoint of the bottom half, is equivalent to conditioning on parent ranking being in the bottom 50%.

and empirically this function is almost linear within the range of observed absolute upward mobility. Therefore, after standardization, using the ranking will produce almost identical results as using actual income. The teen birth rates variable (Figure 2.2) is defined as the percentage of female children that claimed a dependent child when their age was between 13 and 19, also conditioning on the parent income ranking at 25%. In this mobility dataset 325 counties are missing because of insufficient sample size (less than 250 matched children). The mobility measures for these counties are first filled with commuting zone values, and if the entire commuting zone is missing, filled with the average value of adjacent counties.

Within the limits of data availability, we measure the endogenous independent variables at the time when they have direct effects. Following CHKS, public education is measured by the income-adjusted high school dropout rate (residual from regressing unadjusted dropout rate on per capita income), compiled from 1991 to 2000 Local Education Agency Finance Survey (F-33) Data. The returns to education is proxied by the difference between median earnings of college and high school graduates using 2006~2010 American Community Survey 5-year estimates. It is calculated at the commuting-zone-level instead of county-level because commuting zones better represent labor markets. The tax rate is defined as total per capita state and local tax revenue, averaged over 1982, 1987, 1992, and 1997, divided by per capita income. Tax revenue data are compiled from the Historical Database on Individual Government Finances (INDFIN). Since the tax rate is the average for all tax payers, the amounts low income households actually pay also depend on the distribution of tax burdens. Therefore we control for the tax progressivity using state-level Suits index, which is a GINI-like index that takes into account the progressivity of most state and local taxes (see Data Appendix for more details),

The instruments for the dropout rate include past (1972 and 1977) government capital outlays in elementary and secondary education, as well as the average dropout rate of adjacent counties. The instruments for returns to education are commuting-zone-level wage shocks over

1998 to 2002, 2002 to 2007, and 2008 to 2010, calculated using equation (2.7). These wage shocks are similar to the industrial mix employment growth rates commonly used in the regional science literature as exogenous employment shocks (Bartik, 1991; Blanchard et al., 1992; Partridge, et al., 2012). For location j, period t, the wage shock is the sum of national wage growth rate g_{it} in each industry i, weighted by the employment share s_{ijt} of industry i in location j at the start of period t. The employment share and wage rate data are from County Business Patterns (CPB). Missing values in CPB are imputed using the method introduced by Autor et al., (2013). Finally, the tax rate is instrumented by past tax rates in 1972 and 1977, and by the average per capita tax revenue of adjacent counties[5].

$$wageshock_{jt} = \sum_i s_{ijt} g_{it} \tag{2.7}$$

Control variables include Suits tax progressivity index, the percentage of adults with bachelor's degree or more, the percentage of single parent families, the percentage of non-white population, the percentage of workers who commute less than 15 minutes to work, per capita income, out-migration network closeness, and dummy variables for metro counties, rural counties, and counties that are adjacent to metro counties. While some of these variables (such as the percentage of single families) influence children before they enter the labor market, others (such as the percentage of short commutes) are likely to directly affect children's adult income. However, since these variables are treated as exogenous, we use the 1990 data (20 years before children's income is measured) to mitigate endogeneity concerns. The definitions and sources for these control variables can be found in the Data Appendix. Summary statistics for all variables

[5] Using the spatial lag of the tax rate itself would be a more natural choice. However, it does not pass the over-identification test.

are in Table 2.1. In the regressions, all variables are standardized to have means of zero and standard deviations of 1. The error terms are clustered at the state-level.

2.4 Results

2.4.1 Descriptive analysis

At the regional level, absolute upward mobility is high in the Rocky Mountains, the Plains, and the eastern part of the Southwest region (Oklahoma and Texas). The Far West and Northeast have lower mobility, and the Southeast has the lowest (Figure 2.1). There are substantial county-level variations, since state-level variation only explains 59% of the total variation of absolute upward mobility across counties. As shown in Table 2.1, on average, urban counties have lower absolute upward mobility (42.79) than non-urban counties (45.34). The higher income-adjusted dropout rate in urban counties may contribute to the urban-non-urban gap, while the higher returns to education in urban counties may reduce the gap (Table 2.1). The map for teen birth rates is similar to the map of absolute upward mobility, suggesting that children's pre-labor-market and labor-market outcomes may share common determinants. There are subtle differences between the two maps, for example the eastern part of the Southwest has high absolute upward mobility but also high teen birth rates.

Table 2-1. Summary statistics

Variable	All counties		Urban		Non-urban	
	Mean	S.d.	Mean	S.d.	Mean	S.d.
Mobility	44.00	5.88	42.79	4.81	45.34	6.62
Dropout	-0.17	2.53	0.02	2.28	-0.37	2.76
Return to edu. ($1000)	15.51	3.89	17.00	3.56	13.88	3.57
Tax rev. ($1000)	0.19	0.06	0.18	0.05	0.21	0.06
Suits index	-0.06	0.02	-0.07	0.02	-0.06	0.02
Bachelor's degree	12.95	5.95	13.96	6.75	11.83	4.68
Minority	0.12	0.15	0.13	0.14	0.12	0.16
Single parent	0.19	0.06	0.19	0.06	0.18	0.07
Migration	0.47	0.04	0.48	0.04	0.45	0.03
Commute	0.45	0.14	0.38	0.11	0.53	0.14
Income ($1000)	10.92	2.45	11.76	2.70	9.98	1.71
Rural	0.25	0.43				
Metro	0.22	0.41				
Adjacent	0.31	0.46				
Instruments						
Cap. Layout72 ($1000)	0.11	0.12	0.11	0.13	0.10	0.12
Cap. Layout77 ($1000)	0.13	0.19	0.13	0.17	0.13	0.21
SL Dropout	0.04	0.02	0.04	0.02	0.04	0.02
Wageshock98_02	15.45	1.19	15.48	0.68	15.42	1.57
Wageshock03_07	15.08	1.38	15.18	1.19	14.96	1.55
Wageshock08_10	18.28	23.61	20.93	29.22	15.37	14.65
Taxrate72	0.19	0.09	0.18	0.06	0.21	0.11
Taxrate77	0.22	0.09	0.20	0.07	0.24	0.10
SL tax rev. ($1000)	2.07	0.54	2.07	0.50	2.08	0.58
Observations	2776		1457		1319	

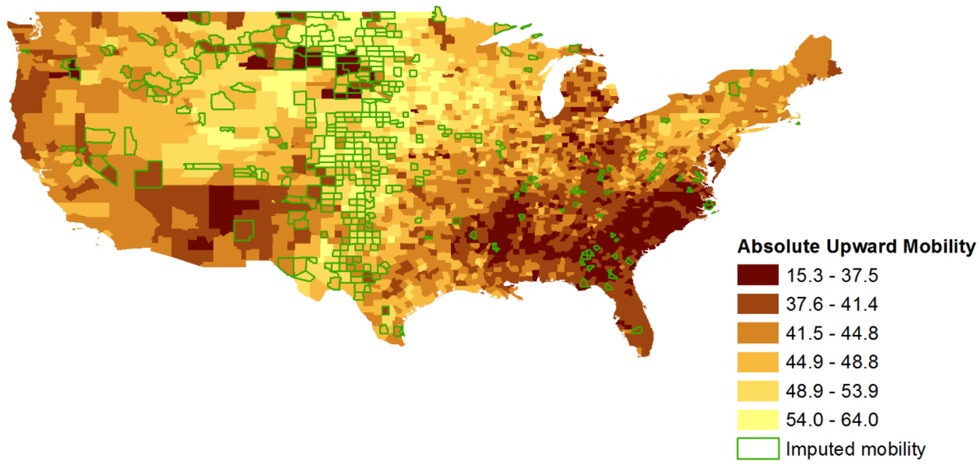

Figure 2-1. Absolute upward mobility in U.S. counties

Note: Absolute upward mobility is defined as the average income percentile ranking (0~100 from low to high income) of children whose parents rank 25% nationally. Missing data are first imputed by filling in commuting zone level mobility published by CHKS, then, if the whole commuting zone is missing, they are filled with the average value of adjacent counties from other commuting zones (source: CHKS).

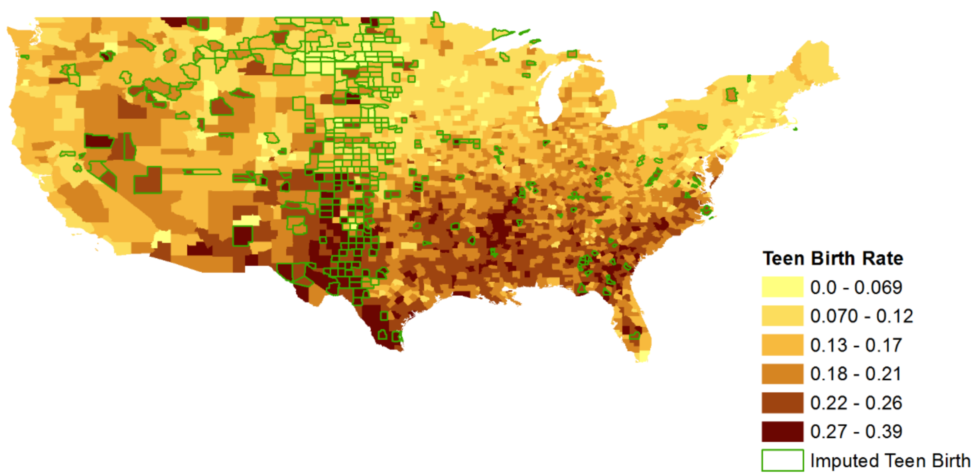

Figure 2-2. Teen birth rate in U.S. counties.

Note: Teenage birth is defined as the percentage of female children that claimed a dependent child when their age was between 13 and 19, conditioning on the parent (of the teen mother) income being in the bottom 50%. Missing data are first imputed by filling in commuting zone level teen birth rate published by CHKS, then, if the whole commuting zone is missing, they are filled with the average value of adjacent counties from other commuting zones (source: CHKS).

2.4.2 Absolute upward mobility regressions

The OLS and IV estimations of Equation (2.6) are presented in Table 2.2 for absolute upward mobility, and in Table 2.3 for teen birth rates. In addition to the analysis for all counties, separate analyses for urban counties and non-urban counties are presented to explore urban-rural differences. Compared to OLS results, the dropout rate and the returns to education have higher impacts while the tax rate has a lower impact on absolute upward mobility in the IV regression for all counties. The coefficient for the returns to education changes from the unexpected negative to the expected positive, suggesting that the endogeneity bias has been corrected. A one standard deviation (SD) increase in the dropout rate decreases absolute upward mobility by 0.386 SD while a one SD increase in the return to education variable increases mobility by 0.567 SD. Both of the above coefficients are statistically significant at the 1% level and are consistent with Solon's model. The positive impacts of returns to education on absolute upward mobility could also be explained by the local spillover effect from high income skilled workers to unskilled workers, as documented by Moretti (2010). The coefficient for the tax rate is not significant.

Table 2-2. OLS and IV regression results for all counties, metro and adjacent counties, and non-metro, non-adjacent counties.

Dep. Var.	All counties		Urban		Non-urban	
Mobility	OLS	IV	OLS	IV	OLS	IV
Endogenous variables						
Dropout	-0.116*	-0.386***	-0.063	-0.279***	-0.219***	-0.491***
	(0.059)	(0.115)	(0.050)	(0.101)	(0.048)	(0.147)
Return to edu. ($1000)	-0.031	0.567***	0.009	0.424***	-0.094**	0.370
	(0.028)	(0.176)	(0.024)	(0.135)	(0.042)	(0.242)
Tax rate	0.108***	0.038	0.201***	0.140	0.061	0.052
	(0.040)	(0.077)	(0.067)	(0.120)	(0.045)	(0.085)
Exogenous Controls						
Suits tax progr. index	0.099*	0.172**	0.063	0.110	0.129**	0.181**
	(0.054)	(0.073)	(0.055)	(0.070)	(0.057)	(0.076)
Bachelor's Degree	-0.001	0.075	0.053	0.098**	-0.090**	-0.007
	(0.031)	(0.058)	(0.036)	(0.049)	(0.039)	(0.088)

Minority	-0.094*	-0.245***	-0.089	-0.219***	-0.126*	-0.188*
	(0.053)	(0.078)	(0.053)	(0.070)	(0.064)	(0.096)
Single parent	-0.437***	-0.313***	-0.484***	-0.342***	-0.358***	-0.309***
	(0.047)	(0.058)	(0.051)	(0.079)	(0.048)	(0.046)
Migration	0.077**	0.065**	0.025	-0.011	0.126**	0.160***
	(0.036)	(0.031)	(0.033)	(0.033)	(0.052)	(0.058)
Close Commute	0.311***	0.401***	0.283***	0.366***	0.352***	0.342***
	(0.041)	(0.085)	(0.042)	(0.073)	(0.050)	(0.067)
Income ($1000)	0.011	-0.223**	0.002	-0.171**	-0.033	-0.148
	(0.047)	(0.101)	(0.043)	(0.077)	(0.062)	(0.110)
Rural	0.029	0.063				
	(0.044)	(0.071)				
Metro	-0.004	-0.269**				
	(0.059)	(0.113)				
Adjacent	0.029	-0.158**				
	(0.039)	(0.078)				
Constant	-0.015	0.091	0.005	-0.049	-0.063	0.099
	(0.063)	(0.076)	(0.055)	(0.073)	(0.061)	(0.107)
Observations	2,776	2,776	1,457	1,457	1,319	1,319
R-squared	0.647	0.628	0.354	0.635	0.439	0.621
Under-ID test		p=0.029		p=0.032		p=0.027
Over-ID test		p=0.710		p=0.175		p=0.263

Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. All variables are standardized to have mean of zero and standard deviation of one. Urban is defined as counties that are within or adjacent to metropolitan areas while non-urban is defined as all other counties.

The coefficient estimates for the exogenous control variables are mostly consistent with prior expectations: absolute upward mobility increases with tax progressivity, the percentage of close commutes (representing accessibility of the local labor market), and migration closeness (representing accessibility of distant labor markets and the flow of information), while it decreases with shares of racial minorities and single parents. The only surprise is the negative and significant coefficient for per capita income. However, this is consistent with the well documented negative correlation between inequality and mobility (Corak, 2013; Krueger, 2012): since the dependent variable is comparing all children whose parents had the same income, the children who live in high per capita income counties experience higher inequality than those who live in low per capita income counties. If the inequality-mobility correlation is causal, the former would thus be disadvantaged by inequality and less upwardly mobile. The indicators for metro counties and adjacent-to-metro counties are significantly negative, showing that beyond the variables included in the model, other unobserved factors also decrease absolute upward mobility in more urban areas.

The results for urban counties are qualitatively similar to those for non-urban counties (Table 2.2, column 3~6). In the IV regressions for these two subsamples, the coefficients are statistically different in their magnitudes for two independent variables[6]. First, in urban areas, the coefficient for the percent of bachelor degrees in the home county in 1990 is larger. This variable represents the overall human capital stock in the county, which could also be correlated with parents' education attainment. To the extent that a previous generation (parents or not) can pass on human capital to the next generation, existing human capital stock will have larger impacts on children's income in urban areas where the returns to human capital is higher. Second, the coefficient for out-migration closeness is smaller in urban areas. The abundant job opportunities offered by urban areas allow urban children to move up the income rank without leaving their home towns, whereas children in rural areas rely on out-migration as an essential channel of upward mobility for local job opportunities are scarce and less diverse. Therefore, the centrality of a county on the migration network (and the resulting better access to information about jobs in other places) is more important for non-urban counties than for urban counties in fostering upward mobility. The coefficient for the dropout rate variable differs substantially between the

[6] The difference is tested for by including all counties in a separate regression and including interaction terms between the independent variables and an indicator variable for the urban subsample. The significance of the difference is the significance of the interaction term.

two samples, yet the difference is not statistically significant. A robustness check shows that conducting the analysis at the commuting-zone-level does not make much of a difference for the dropout rate variable, indicating that the bias caused by migrating parents is not driving the results (robustness checks are available upon request).

2.4.3 Teen birth rates regressions

The OLS and IV regression results for teen birth rates are presented in Table 2.3, with separate analyses for all counties (Table 2.3, column 1~2) and the two subsamples (Table 2.3, column 3~6). The returns to education variable is excluded because it is a labor market variable and teen birth is a pre-labor-market outcome. Also, including the returns to education variable would cause several regressions to fail the over-identification test. Precluding the possibility of reverse causations, the IV regression for all counties shows that lower school quality, measured by the dropout rate, causes teen birth rates to increase. This could be caused by the peer pressure that drives teenagers to have sex before they fully understand it, by the lack of sexual education, or by lowering children's expectation of their future success (Kearney & Levine, 2012). The coefficient for the tax rate is again insignificant. Among the control variables, the minority variable increases teen birth rates while tax progressivity, bachelor's degree, and per capita income reduces teen birth rates. Notably, per capita income has a positive effect on the pre-labor-market outcome (reducing teen birth rates) but has a negative impact on the final mobility outcome, suggesting that living near rich neighbors may have different impacts at different stages of life. Since the instruments are weak for the non-urban results, we did not statistically test the difference between urban and non-urban samples.

Table 2-3. OLS and IV regression results for teen birth rates in all counties, metro and adjacent counties, and non-metro, non-adjacent counties.

Dep. Var.	All counties		Urban		Non-urban	
Teen birth	OLS	IV	OLS	IV	OLS	IV
Endogenous variables						
Dropout	0.089*	0.202**	0.063	0.090	0.132**	0.300**
	(0.050)	(0.096)	(0.050)	(0.090)	(0.053)	(0.117)
Tax rate	0.094	0.053	0.011	-0.042	0.156**	0.129*
	(0.066)	(0.082)	(0.076)	(0.106)	(0.068)	(0.076)
Exogenous Controls						
Suits tax progr. index	-0.342***	-0.335***	-0.302***	-0.292***	-0.384***	-0.381***
	(0.092)	(0.099)	(0.075)	(0.081)	(0.108)	(0.114)
Bachelor’s Degree	-0.270***	-0.261***	-0.237***	-0.236***	-0.287***	-0.270***
	(0.036)	(0.037)	(0.037)	(0.037)	(0.050)	(0.049)
Minority	0.273***	0.272***	0.356***	0.365***	0.228***	0.217***
	(0.066)	(0.070)	(0.081)	(0.084)	(0.068)	(0.073)
Single parent	0.077	0.042	0.025	0.014	0.097	0.047
	(0.067)	(0.061)	(0.089)	(0.088)	(0.065)	(0.057)
Migration	0.002	-0.001	0.040	0.039	-0.087	-0.119**
	(0.034)	(0.033)	(0.036)	(0.036)	(0.063)	(0.058)
Close Commute	-0.008	0.031	0.065	0.093	-0.081	-0.037
	(0.088)	(0.073)	(0.087)	(0.079)	(0.091)	(0.077)
Income ($1000)	-0.138***	-0.146***	-0.247***	-0.251***	0.055	0.036
	(0.049)	(0.048)	(0.051)	(0.050)	(0.073)	(0.074)
Rural	0.045	0.082				
	(0.067)	(0.060)				
Metro	-0.080	-0.061				
	(0.105)	(0.096)				
Adjacent	-0.022	-0.003				
	(0.066)	(0.061)				
Constant	0.012	-0.007	0.012	0.013	0.096	0.074
	(0.056)	(0.058)	(0.100)	(0.102)	(0.073)	(0.079)
Observations	2,774	2,774	1,457	1,457	1,317	1,317
R-squared	0.561	0.549	0.673	0.671	0.477	0.461
Under-ID test		p=0.088		p=0.032		p=0.198
Over-ID test		p=0.571		p=0.124		p=0.330

Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. All variables are standardized to have mean of zero and standard deviation of one. Urban is defined as counties that are within or adjacent to metropolitan areas while non-urban is defined as all other counties.

2.5. Discussion and conclusion

Using recently available county-level absolute upward mobility and teen birth rates data based on federal tax records, this paper has studied the determinants of intergenerational mobility using the framework of Solon's model. To ameliorate endogeneity concerns, deeply time lagged variables, spatially lagged variables, and national wage rate shocks are used as instruments. Various statistical tests and robustness checks were conducted to verify our empirical approach. In addition to the analysis with all counties, the subsamples of urban counties and non-urban counties were analyzed separately to explore urban-rural differences.

We find that higher school quality will lead to higher absolute upward mobility, and low teen birth rates, consistent with Solon's model. For policymakers, these results show that high quality public education is the key to enhance the equality of opportunity. The fact that the dropout rate has impacts on both teen birth rates and absolute upward mobility shows that school quality not only has short term impacts on educational outcomes, but also has long term impacts on children's success as adults.

Using the dropout rate as a measure of school quality has the potential drawback that it may also be affected by family and individual factors such as economic hardship and marriage stability. Although we tried to parse out school quality by adjusting dropout rate with income, the other factors may still conflate with the measurement. Also, using output-based school quality measures leaves answering the question of how to achieve better school quality to future studies.

Evaluating the effects of returns to education on mobility, we find robust positive impacts of returns to education on absolute upward mobility, also consistent with Solon's model. Although our measure of return to education is crude, the results nevertheless shed light on an important mobility determinant that previously received little attention. Greater returns to education in a locality provide incentives for low-income youth to invest more in their education.

Our results suggest that the prevalence of high skilled jobs that reward education may benefit low income children in the long term by providing both incentives and eventual rewards in higher wages. Even if they work in low skill jobs, they may still benefit from living close to high income skilled workers whose spending supports higher wage rates in the service sector.

The result for tax rates is mixed. While Solon's model predicts that a higher tax rate leads to lower mobility, this prediction is only supported by the teen birth rates regression in the non-urban sample. The tax rate variable even has a statistically significant positive impact on absolute upward mobility in some robustness checks. The expenditure effect of taxation may confound the income-decreasing effect, which points to the need of a model that takes into account the complex and potentially conflicting effects of tax collection and spending.

The comparison between urban and non-urban counties shows that the existing human capital stock is more important for children in urban areas while migration opportunities are more important in non-urban areas. The finding that rural children rely more on out-migration to move up the income ranking presents a challenge for policymakers who not only care about the prosperity of people, but also the prosperity of places. To be able to retain talented children, rural areas need to create high skilled jobs and favorable environments for start-up entrepreneurs.

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Appendices

A1. Data sources and descriptions

Suits (1977) tax progressivity index is a GINI-like measure that ranges from -1 for most regressive to +1 most progressive. Phares (1980) systematically calculated the Suits index for all states for the year 1977, taking into account most types of state and local taxes. The benchmark results for all state and local taxes (Phares 1988, pp 152-153) are used here.

Bachelor's degree is defined as the percentage of adults (25+) with a bachelor's degree or more. Data are from the 1990 census accessed through the National Historical Geographic Information System (NHGIS) website.

Minority is non-white population divided by total population. Data are from the 1990 census accessed through NHGIS website.

Single parent is the number of families with single head and children divided by total number of families with children. Data are taken from 1990 census, accessed through the NHGIS website.

Migration is the out-migration closeness (Wasserman and Galaskiewicz, 1994) defined by:

$$C_i = \frac{J_i^2}{(N-1)\sum_{j=1}^{N} d_{ij}}$$

Where J_i is the number of counties that receive at least one migrant from county i, N is total number of counties, and d_{ij} is the number of nodes on the maximum flow route from county i to county j minus 1. To illustrate how d_{ij} is calculated, let the migration route between city A and C via city B have the flow $w_{ab} * w_{bc}$, where the w's are out-migration rates (the number of people migrating from the origin to the destination divided by the population at the origin). This route has a d value of 2. If this route happens to be the maximum flow route, then $d_{ac} = 2$. Otherwise, if the direct flow from A to C is larger, i.e., $w_{ac} > w_{ab} * w_{bc}$, then $d_{ac} = 1$. Therefore, the closeness measure favors more migration connections J_i, as well as direct access to other counties. Empirically, the map of closeness in the U.S. reveals that, in general, larger cities have higher closeness because they have higher non-zero migration connections (J). For counties of the same size, those located in the central area have higher closeness, because they are located on migration pathways. Out-migration flow data are from Census 1990.

Close commute is the percentage of workers who commuted less than 15 minutes to work. Data are taken from 1990 census accessed through NHGIS.

Income is per capita income in thousands of dollars. Data are taken from 1990 census accessed through NHGIS website.

Metro, Rural and Adjacent are defined by the USDA's rural-urban-continuum-code (RUCC) of 1983. *Metro* is defined as those counties with RUCC of 1, 2, and 3. *Rural* is defined as those counties with RUCC of 8 or 9. *Adjacent* represents non-metro counties that are adjacent to metro counties, i.e. RUCC code 4, 6, and 8. The excluded category is non-metro counties with RUCC of 5 and 7.

A2. Evaluation of Instrumental Variables.

Under-identification tests (Kleibergen & Paap, 2006) show that the instruments are relevant (null rejected) in all cases except for the teen birth rates regression with the non-urban sample. Furthermore, in all regressions, over-identification tests (Sargan, 1958; Hanson, 1982) fail to reject the null that the instruments are uncorrelated with the error term, indicating that the instruments are valid. The first stage regressions (Appendix table 2.1~2.6) show that the instruments are identifying the endogenous variables as expected: for each endogenous variable in each of the six IV regressions in Table 2.2 and Table 2.3, at least one of the corresponding instrument variables (i.e. two time lagged capital outlays and one spatially lagged dropout rate for the dropout rate variable, three wage rate shocks for the returns to education, and two time lagged and one spatially lagged variables for the tax rate) are statistically significant. Moreover, in the first stage regressions, the coefficients for the instrumental variables have the expected signs: in the dropout rate and the tax rate regressions, coefficients for the corresponding spatially lagged variables are always positive, which can be interpreted as positive institutional peer effects. Time lagged tax rates (when significant) have positive impacts on the tax rate, showing the persistence of tax policies. National level wage rate shocks (when significant) have positive impacts on the returns to education variable, telling us that when there is a positive wage rate shock in the local industries, the gap between skilled and unskilled workers tend to increase. Time lagged capital outlays in elementary and secondary education are never significant in any of the dropout rate regressions, meaning that capital investments are either severely depreciated or ineffective in improving school quality. Overall, statistical tests and first stage regression results demonstrate that the instruments are performing well, and we will proceed with the IV regression as our preferred specification.

A3. Robustness checks

Robustness checks for the absolute upward mobility regression with all counties are presented in Appendix Table 2.7. The first three regressions (Appendix Table 2.7, column 1-3) examine the robustness of the results to alternative ways of variable construction. These checks show that most of our results are robust to including counties with missing dropout rates by using a dummy variable for those counties, excluding imputed mobility measures, and using dropout rates constructed from pre-1998 data (because 1998 is the year when children in the 1980~1982 birth cohort started to graduate from high school). The coefficient for tax rates in the second check is positive, contrary to the predictions of Solon's model, which we will discuss in the paragraph below. We also notice that the coefficients for the dropout rate and returns to human capital are larger in absolute magnitudes in the third robustness check. This is most likely caused by the omission of counties from the seven states (Florida, Indiana, Kansas, North Carolina, South Carolina, Texas, and Washington) that have no dropout data before 1998. It suggests that the effects of the independent variables might be heterogeneous in different states and we are capturing average effects.

The fourth test (Appendix Table 2.7, column 4) shows that conducting the analysis at the commuting-zone-level does not make much of a difference for the dropout rate variable, indicating that the bias caused by migrating parents is not driving the results. The return to education variable loses its significance but is still positive. As in robustness check two, the tax rate variable is positive, contradicting Solon's model. At the local level, taxation is not just a burden on households, as assumed in Solon's model, but also the source of funding for infrastructure investments, public libraries, and so on. While government expenditures on education are reflected in the dropout rate in the model, the tax rate variable may capture the effects of other expenditures. The final robustness check (Appendix Table 2.7, column 5) addresses the concern for spatial dependency using the spatial error model with additional endogenous variables (Arraiz, Drukker, Kelejian, & Prucha, 2010; Drukker, Prucha, & Raciborski, 2013). Compared to the main specification, the signs and significance of most coefficients remain the same.

The same robustness checks for absolute upward mobility are carried out for teen birth rates (Appendix Table 2.8, column 5). Among the five robustness checks, test (3) and (5) do not pass the over-identification tests. In other cases, the results for dropout rates are mostly consistent across different specifications, s except that the coefficient is insignificant in the spatial error model. The tax rate significantly increases teen birth rates in the spatial error model (Appendix Table 2.8, column 5), which is similar to the non-urban result in the main specification.

Appendix table 2-1. First stage regressions for absolute upward mobility in all counties.

	Endogenous variables		
Excluded variables	Dropout	Return to edu.	Tax rev.
Suits index	0.042	-0.132*	0.060
Bachelor's degree	-0.098***	-0.159***	0.005
Minority	0.014	0.258***	0.097***
Single parent	0.195**	-0.068	-0.018
Migration	0.084	0.021	0.032*
Commute	-0.044	-0.251***	0.035
Income ($1000)	0.027	0.297***	-0.360***
Rural	-0.073	-0.096	0.110***
Metro	0.044	0.359	-0.022
Adjacent	-0.026	0.232	0.011
Cap. Layout72 ($1000)	0.010	-0.007	0.024**
Cap. Layout77 ($1000)	0.007	0.012	0.063***
SL Dropout	0.415***	0.049	-0.016
Wageshock98_02	-0.011	0.059***	0.022
Wageshock03_07	-0.007	0.050**	-0.002
Wageshock08_10	0.000	-0.009	-0.004
Taxrate72	-0.035	-0.102	-0.064
Taxrate77	0.001	0.032	0.616***
SL tax rev. ($1000)	0.029	0.241***	0.289***
Constant	0.017	-0.125	-0.026
R-squared	0.31	0.41	0.75
F-value	14.0***	12.2***	236.4***
Observations	2776	2776	2776

Note: Standard errors are clustered at the state level.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix table 2-2. First stage regressions for absolute upward mobility in urban counties.

	Endogenous variables		
Excluded variables	Dropout	Return to edu.	Tax rev.
Suits index	0.025	-0.140**	0.054
Bachelor's degree	-0.089**	-0.138**	0.007
Minority	-0.004	0.285***	0.122***
Single parent	0.161***	-0.154*	-0.039
Migration	0.085***	0.085**	0.009
Commute	-0.013	-0.278***	0.082**
Income ($1000)	0.035	0.292***	-0.30***
Cap. Layout72 ($1000)	0.013	-0.005	-0.014
Cap. Layout77 ($1000)	-0.018	-0.019	0.032*
SL Dropout	0.452***	0.043	-0.005
Wageshock98_02	-0.025	0.028	0.039
Wageshock03_07	0.008	0.148***	0.015
Wageshock08_10	-0.005	-0.001	-0.005
Taxrate72	-0.058	-0.264***	0.036*
Taxrate77	-0.004	0.217**	0.617***
SL tax rev. ($1000)	0.041	0.269***	0.180***
Constant	0.014	0.113*	0.018
R-squared	0.23	0.45	0.72
F-value	37.8	11.4	162.3
Observations	1457	1457	1457

Note: Standard errors are clustered at the state level.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix table 2-3. First stage regressions for absolute upward mobility in non-urban counties.

	Endogenous variables		
Excluded variables	Dropout	Return to edu.	Tax rev.
Suits index	0.059*	-0.106	0.064
Bachelor’s degree	-0.105***	-0.224***	0.002
Minority	0.028	0.176**	0.087**
Single parent	0.226***	0.050	-0.023
Migration	0.165***	-0.042	-0.025
Commute	-0.075***	-0.113	-0.016
Income ($1000)	0.043	0.231***	-0.419***
Cap. Layout72 ($1000)	0.005	-0.007	0.068**
Cap. Layout77 ($1000)	0.018	0.049	0.097***
SL Dropout	0.376***	0.067	-0.021
Wageshock98_02	-0.010	0.056***	0.024
Wageshock03_07	-0.007	-0.022	-0.010
Wageshock08_10	0.031	-0.009	0.013
Taxrate72	-0.022	-0.080	-0.074
Taxrate77	0.012	-0.044	0.588*
SL tax rev. ($1000)	0.025	0.200***	0.375***
Constant	0.038	-0.285***	0.011***
R-squared	0.43	0.19	0.74
F-value	6.57	12.2	82.9
Observations	1319	1319	1319

Note: Standard errors are clustered at the state level.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix table 2-4. First stage regressions for teen birth rate in all counties.

	Endogenous variables	
Excluded variables	Dropout	Tax rev.
Suits index	0.043	0.059
Bachelor's degree	-0.100***	0.008
Minority	0.013	0.098***
Single parent	0.196***	-0.018
Migration	0.083**	0.034*
Commute	-0.042	0.037
Income ($1000)	0.030	-0.366***
Rural	-0.073	0.115***
Metro	0.041	-0.017
Adjacent	-0.028***	0.014
Cap. Layout72 ($1000)	0.010	0.024**
Cap. Layout77 ($1000)	0.009	0.066***
SL Dropout	0.415	-0.017
Taxrate72	-0.034	-0.064
Taxrate77	-0.001	0.614***
SL tax rev. ($1000)	0.027	0.293***
Constant	0.019	-0.029
R-squared	0.31	0.75
F-value	16.9	155.0
Observations	2274	2274

Note: Standard errors are clustered at the state level.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix table 2-5. First stage regressions for teen birth rate in urban counties.

	Endogenous variables	
Excluded variables	Dropout	Tax rev.
Suits index	0.027	0.049
Bachelor’s degree	-0.091**	0.013
Minority	-0.004	0.125***
Single parent	0.160***	-0.042
Migration	0.084***	0.012
Commute	-0.012	0.077*
Income ($1000)	0.040	-0.316***
Cap. Layout72 ($1000)	0.013	-0.014
Cap. Layout77 ($1000)	-0.019	0.033**
SL Dropout	0.453***	-0.006
Taxrate72	-0.059	0.031
Taxrate77	-0.004	0.622***
SL tax rev. ($1000)	0.037	0.189
Constant	0.014	0.018
R-squared	0.31	0.72
F-value	43.2	173.1
Observations	1457	1457

Note: Standard errors are clustered at the state level.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix table 2-6. First stage regressions for teen birth rate in non-urban counties.

	Endogenous variables	
Excluded variables	Dropout	Tax rev.
Suits index	0.060*	0.065
Bachelor’s degree	-0.108***	0.008
Minority	0.028	0.086**
Single parent	0.227***	-0.022
Migration	0.166***	-0.023
Commute	-0.079**	-0.015
Income ($1000)	0.047	-0.430***
Cap. Layout72 ($1000)	0.004	0.067**
Cap. Layout77 ($1000)	0.021	0.103***
SL Dropout	0.379***	-0.022
Taxrate72	-0.020	-0.072*
Taxrate77	0.008	0.583***
SL tax rev. ($1000)	0.023	0.377***
Constant	0.040	0.008
R-squared	0.43	0.74
F-value	8.85***	82.40***
Observations	1317	1317

Note: Standard errors are clustered at the state level.
* p<0.1, ** p<0.05, *** p<0.01.

Appendix table 2-7. Robustness checks for absolute upward mobility in all counties.

Dep. Var. Mobility	(1)	(2)	(3)	(4)	(5)
Endogenous variables					
Dropout	-0.276***	-0.321***	-0.533***	-0.281**	-0.179***
	(0.081)	(0.108)	(0.123)	(0.127)	(0.038)
Return to edu. ($1000)	0.675***	0.469***	0.884***	0.207	0.200***
	(0.206)	(0.167)	(0.277)	(0.234)	(0.051)
Tax rate	0.019	0.216***	-0.020	0.206***	0.025
	(0.107)	(0.084)	(0.127)	(0.075)	(0.020)
Exogenous Controls					
Suits tax progr. index	0.154***	0.076	0.172*	0.037	0.076***
	(0.057)	(0.068)	(0.091)	(0.030)	(0.017)
Bachelor’s Degree	0.161**	0.097*	0.111	0.082*	0.006
	(0.067)	(0.053)	(0.089)	(0.048)	(0.020)
Minority	-0.294***	-0.232***	-0.215*	-0.166*	-0.200***
	(0.080)	(0.076)	(0.118)	(0.095)	(0.024)
Single parent	-0.344***	-0.363***	-0.346***	-0.414***	-0.294***
	(0.058)	(0.066)	(0.067)	(0.047)	(0.021)
Migration	0.015	-0.022	0.088*	0.105**	-0.007
	(0.060)	(0.032)	(0.047)	(0.051)	(0.018)
Close Commute	0.492***	0.452***	0.568***	0.472***	0.159***
	(0.073)	(0.064)	(0.110)	(0.081)	(0.023)
Income ($1000)	-0.329**	-0.147	-0.369**	-0.271***	-0.074***
	(0.127)	(0.094)	(0.152)	(0.078)	(0.028)
Rural	0.034	0.218***	0.123		-0.039
	(0.043)	(0.058)	(0.094)		(0.032)
Metro	-0.102*	-0.080	-0.285**		-0.165***
	(0.055)	(0.093)	(0.132)		(0.046)
Adjacent	-0.020	-0.112*	-0.134		-0.091***
	(0.040)	(0.067)	(0.090)		(0.031)
Constant	-0.176	0.086	0.148	0.079***	0.008
	(0.111)	(0.065)	(0.101)	(0.021)	(0.021)
Observations	3,001	2,464	2,072	647	2776
R-squared	0.629	0.521	0.131	0.762	
Under-ID test	Δ	p=0.023	p=0.071	p=0.098	Δ
Over-ID test	Δ	p=0.751	p=0.869	p=0.262	Δ

Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. Column (1) includes counties that miss dropout rate data by including a dummy variable for missing dropout rate. Column (2) excludes counties with imputed mobility. Column (3) only uses dropout rate before 1998. Column (4) reproduces the result at commuting zone level. Column (5) is the result of the spatial error model. Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. All variables are standardized to have mean of zero and standard deviation of one. Δ：The statistical test cannot be straight forwardly calculated.

Appendix table 2-8. Robustness checks for teen birth rate in all counties.

Dep. Var. Teen birth rate	(1)	(2)	(3)	(4)	(5)
Endogenous variables					
Dropout	0.180**	0.155*	0.387***	0.270	0.041
	(0.084)	(0.093)	(0.081)	(0.164)	(0.040)
Tax rate	0.123	-0.120	-0.098	-0.163**	0.163***
	(0.132)	(0.090)	(0.079)	(0.079)	(0.019)
Exogenous Controls					
Suits tax progr. index	-0.334***	-0.252***	-0.216***	-0.248***	-0.170***
	(0.094)	(0.081)	(0.070)	(0.040)	(0.017)
Bachelor's Degree	-0.259***	-0.282***	-0.181***	-0.308***	-0.248***
	(0.045)	(0.033)	(0.036)	(0.040)	(0.018)
Minority	0.296***	0.314***	0.122*	0.436***	0.193***
	(0.072)	(0.069)	(0.067)	(0.056)	(0.022)
Single parent	0.024	0.059	0.126**	-0.057	0.122***
	(0.062)	(0.070)	(0.056)	(0.060)	(0.020)
Migration	-0.036	0.089**	-0.067	0.151***	-0.006
	(0.051)	(0.036)	(0.045)	(0.040)	(0.018)
Close Commute	0.030	0.002	-0.034	-0.066	0.023
	(0.069)	(0.058)	(0.054)	(0.054)	(0.020)
Income ($1000)	-0.157**	-0.234***	-0.207***	0.184***	-0.056***
	(0.059)	(0.036)	(0.046)	(0.067)	(0.021)
Rural	0.081	-0.126**	0.122**		0.106***
	(0.059)	(0.058)	(0.053)		(0.031)
Metro	-0.024	-0.106	-0.144**		-0.032
	(0.094)	(0.078)	(0.069)		(0.041)
Adjacent	0.022	-0.006	-0.107**		-0.001
	(0.066)	(0.048)	(0.044)		(0.028)
Constant	-0.113	-0.046	-0.101**	-0.035	0.021
	(0.249)	(0.062)	(0.048)	(0.032)	(0.018)
Observations	2,999	2,464	2,070	614	2,774
R-squared	0.550	0.684	0.584	0.520	
Under-ID test	Δ	p=0.067	p=0.071	p=0.000	Δ
Over-ID test	Δ	p=0.150	p=0.036	p=0.000	Δ

Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. Column (1) includes counties that miss dropout rate data by including a dummy variable for missing dropout rate. Column (2) excludes counties with imputed teen birth rates. Column (3) only uses dropout rate before 1998. Column (4) reproduces the result at commuting zone level. Column (5) is the result of the spatial error model. Note: Standard errors are clustered at the state level. * p<0.1, ** p<0.05, *** p<0.01. All variables are standardized to have mean of zero and standard deviation of one. Δ： The statistical test cannot be straight forwardly calculated.

Chapter 3 Education Equity and Intergenerational Mobility: Quasi-Experimental Evidence from Court-Ordered School Finance Reforms

Abstract

Starting in the early seventies, court-ordered school finance reforms (SFRs) have fundamentally changed the landscape of primary and elementary education finance in the US. This paper employs SFRs as quasi-experiments to quantify the effects of education equity on intergenerational mobility within commuting zones. First, I use reduced form difference-in-difference analysis to show that 10 years of exposure to SFRs increases the average college attendance rate by about 5.2% for children with the lowest parent income. The effect of exposure to SFRs decreases with parent income and increases with the duration of exposure. Second, to directly model the causal pathways, I construct a measure for education inequity based on the association between school district education expenditure and median family income. Using exposure to SFRs as the instrumental variable, 2SLS analysis suggests that one standard deviation reduction in education inequality will cause the average college attendance rate to increase by 2.2% for children at the lower end of the parent income spectrum. Placing the magnitudes of these effects in context, I conclude that policies aimed at increasing education equity, such as SFRs, can substantially benefit poor children but they alone are not enough to overcome the high degree of existing inequalities.

Key words: public education finance, intergenerational mobility, school finance reforms, quasi-experiments

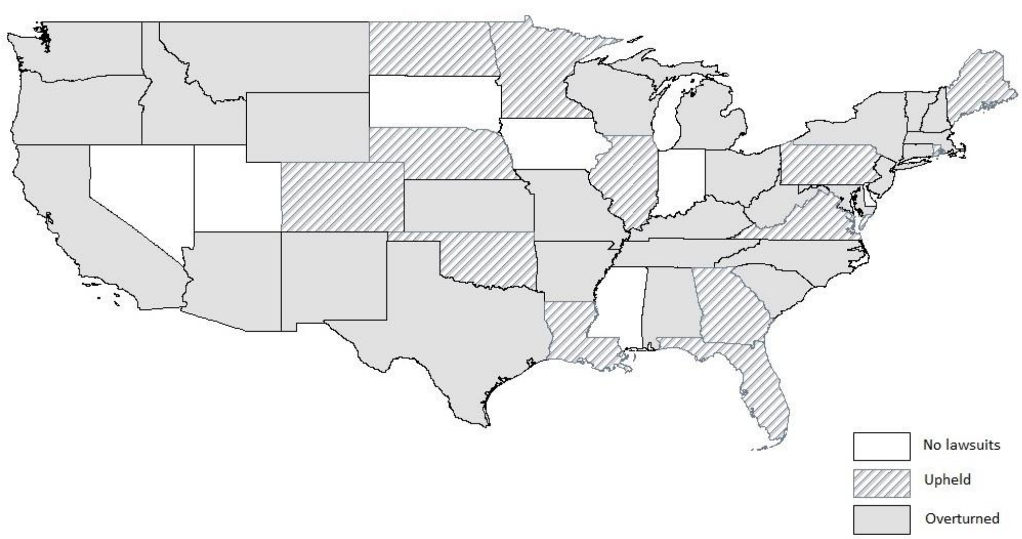
3.1 Introduction

Starting from the Serrano v. Priest (1971) case in California, school finance lawsuits have fundamentally changed the landscape of primary and secondary education finance in the United States. In these lawsuits, the plaintiffs argued that the states had failed to provide just education financing and demanded reforms. The courts that ruled in favor of reforms would order the state legislatures to revise their education funding formulas. By channeling more state funds to poor districts, these court-ordered school finance reforms (SFRs) have increased the equity of education expenditures. A tally by Jackson, Johnson, and Persico (2015) counted that by 2010, school finance lawsuits had been brought in 42 states, some of which had experienced multiple lawsuits (Figure 3.1). These lawsuits are still going on today and will continue to happen in the future. Given their prevalence and importance, it is crucial to thoroughly evaluate the effects of past SFRs.

This paper provides new evidence on the long-term impacts of SFRs on intergenerational mobility, measured by the college attendance rates and child income rank. Exploiting SFRs as natural experiments, this study also answers a broader question: To what degree, if any, intergenerational mobility is causally determined by education equity?

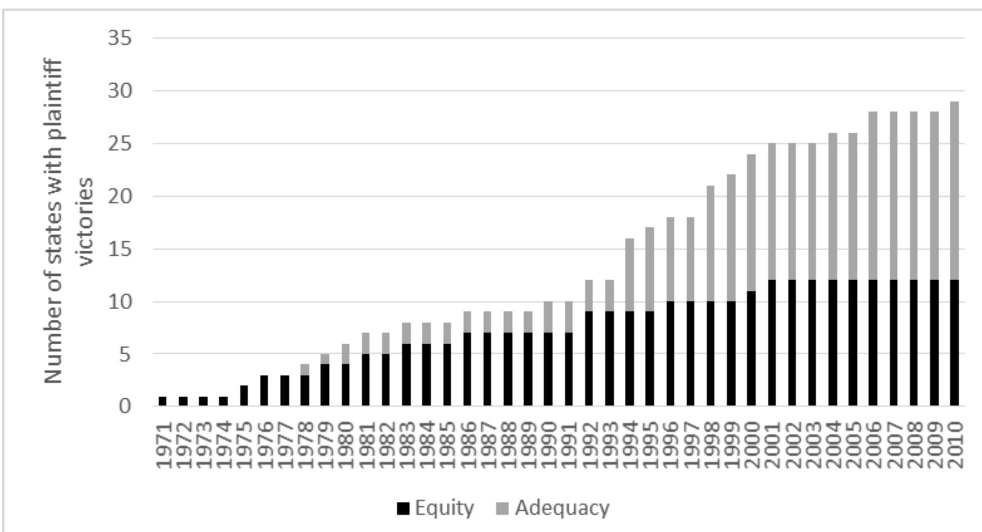
The past literature on court-ordered SFRs agreed that they had achieved the fiscal goals of curbing the within-state expenditure disparities (Murray, Evans, and Schwab 1998; Corcoran and Evans 2008) and reducing the correlation between expenditure and family income (Card and Payne 2002). Also, higher expenditure leads to more educational inputs such as the number of teachers per pupil and teacher salary (Jackson, Johnson, and Persico 2016). However, the effects of SFRs on students' outcomes during school are contested (Papke 2005; Guryan 2001; Downes and Figlio 1998; Hoxby 2001; Fischel 2006; Hanushek 2003), and the evidence on the long-term impacts of SFRs are very sparse. Card and Payne (2002) found tentative evidence that the equalization caused by SFRs narrows the SAT score outcomes across parent education categories. Jackson et al. (2016) found that the exogenous increase in school expenditures caused by court-ordered SFRs raises low-income children's income and education attainment. Besides the lack evidence on the long-term impacts of SFRs in general, the effects of SFR on intergenerational mobility have not been studied. Intergenerational mobility is defined over the entire range of parent income, yet existing studies (such as Card and Payne 2002 and Jackson et al. 2016) only grouped children by crude categories and therefore cannot provide quantitative predictions regarding intergenerational mobility.

Figure 3-1. Map of school finance lawsuits in 2010.



Other than providing additional evidence for policy evaluation, quantifying the impacts of SFRs on intergenerational mobility is also an important contribution to the mobility literature. Previous theoretical studies reached no consensus on the effects of education equity on intergenerational mobility: on one hand, more equitable education finance can transfer more funds to poor students and increase mobility. On the other hand, over-centralized education financing may weaken individual incentives to invest in education and decrease mobility (Solon 2004; Checchi, Ichino, and Rustichini 1999). The task of determining the relationship between education equity and integrational mobility is left to empirical studies. While cross-country studies provide suggestive correlational evidence, CHKS data offers a unique opportunity to establish causal effects using panel data models. Although superior to cross-sectional analysis, OLS analysis using panel data still suffers from serious endogeneity concerns. Public funding is usually tied to the proportion of disadvantaged students, such as students who are learning English as a second language. An increase in the proportion of these students would cause public funding to increase. However, the positive impacts of additional funding might be offset by the negative effects of unobservable student characters. SFRs generated exogenous changes in education equity, which can be exploited as quasi-experiments to facilitate causal inference.

Figure 3-2. Number of states with court verdicts overturning the education system, by type of lawsuits.



In this study, I use the differential trends of commuting-zone-level intergenerational mobility across birth cohorts (Chetty, Hendren, Kline, and Saez 2014; Chetty et al. 2014, CHKS from here on) to provide additional evidence on the long-term effects of SFRs. Using population based federal tax records, CHKS provides college attendance rates (child income rank) for 10 (7) birth cohorts from 1984 to 1993 (1980 to 1986), across the parent income distribution. Due to the timing of SFRs, the duration of exposure to SFRs during school years varies from cohort to cohort, which serves as the treatment variable that creates exogenous shifts in education equity.

The first part of the analysis uses a panel fixed effect model to evaluate the effects of exposure to SFRs on children's college attendance rates. The empirical setup is equivalent to the difference-in-difference (DD) analysis in the panel data setting. I find that 10 years of exposure to SFRs increases the average college attendance rate of lowest-parent-income children by 5.72%, and reduces the attendance gap between lowest and highest-parent-income children by 3.92%. Event analysis shows that the impact increases with the duration of exposure following an approximately linear trend. Furthermore, as parent income rank increases, the impact of SFRs diminishes, also in an approximately linear relationship. The impact of SFRs loses statistical significance at about 70% parent income rank (100% being the highest). These results indicate that, in the long-term, SFRs have substantial equalization effects on college attendance, and the equalization is achieved by leveling-up, not leveling-down. However, similar analysis on child income ranks finds no significant results. Possible reasons for not finding significant results for income ranks are discussed.

The second part of the analysis quantifies the impacts of education expenditure equity on intergenerational mobility, using exposure to SFRs as the instrumental variable. I first construct education equity measures following Card and Payne (2002). For each cohort in each state, the equity of education is measured by the expenditure-income gradient, defined as the association (regression coefficient) between school district education expenditure per pupil and median income. I find that exposure to SFRs reduces the expenditure-income gradient, and that one standard deviation decrease in expenditure-income gradient increases the college attendance rates for the lowest-parent-income students by 2.06% and reduces the college attendance inequality by 2.87%. In contrast to the 2SLS results, OLS regressions produce unexpected results, demonstrating the importance of addressing the endogeneity bias. Overall, these results are consistent with the theoretical prediction (Solon 2004) that weakening the linkage between education expenditure and parent income should lead to higher intergenerational mobility. Placing the magnitudes of these effects in context, I concluded that policies aimed at increasing education equity, such as SFRs, can substantially increase poor children's college attendance but they alone are not enough to overcome the high degree of existing inequalities.

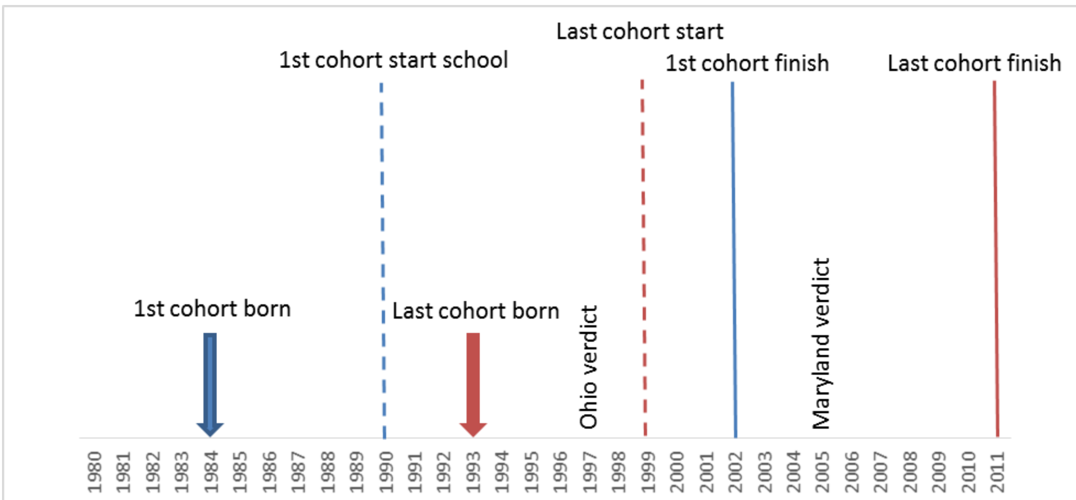
The remainder of the paper is organized as follows. Section 3.2 outlines empirical strategies and Section 3.3 introduces data. Section 3.4 presents results from the DD analysis that evaluates the impacts of SFRs on intergenerational mobility. Section 3.5 presents the 2SLS estimations that quantifies the impacts of education equity on intergenerational mobility, using exposure to SFRs as instruments, and Section 3.6 concludes.

3.2 Empirical strategy

3.2.1 Difference-in-difference analysis of the impact of SFRs.

The objective of this part of the analysis is to test whether exposure to court-ordered school finance reforms has positive impacts on various mobility measures, and if yes, quantify the sizes of these impacts. For college attendance analysis (demonstrated in Figure 3.3), the first cohort in this study started school in 1990 and finished in 2002, while the last cohort started school in 1999 and finished in 2011. For child income rank, the first cohort was born in 1980 and the last cohort was born in 1986. The duration of exposure to SFRs during school years, ranging from 0 to 12, is our key independent variable.

Figure 3-3. An illustration of the variation in the treatment variable (exposure to SFRs) by state and cohort.



Note: The first (last) cohort in this study, represented by the color blue (red), was born in 1984 (1993). They enter primary school in 1990 (1999) when they are 6 years old, and finish high school in year 2002 (2011) when they are 18 years old. In Ohio, the first court verdict in favor of SFRs came in 1997, so the first cohort experienced 7 years of SFRs (Exposure = 7) while the last cohort experienced 12 (Exposure = 12). For the last cohort in Ohio, the verdict came 2 years before they entered primary school, so for them the vintage of the reform is 2 years (Vintage = 2). If a court verdict came after the cohort entered school or if there is no court verdict Vintage is set to 0.

The mobility measures include the college attendance rates (income rank) of children from various family backgrounds, measured by their parents' income ranking. Another useful measure of mobility is the slopes that characterize the linear relationships between the college attendance rate (child income rank) and parent income ranking, which represent the outcome gaps between children from highest and lowest income families. Let $Mobility_{cst}$ be the omnibus symbol for the mobility measures for birth cohort t in commuting zone c of state s, the regression model is of the following form:

(3.1) $$Mobility_{cst} = \beta_{exp}Exposure_{st} + \beta_{vin}Vintage_{st} + \beta_{c}Covariates_{cst} + \theta_{c} + \gamma_{t} + \varepsilon_{cst}$$

$$Vintage_{st} = birth\_yr + 6 - reform\_yr \;\; if \;\; birth\_yr + 6 - reform\_yr > 0, \; 0 \; otherwise$$

The coefficient β_{exp} is the main result of interest. For SFRs that happened before a certain cohort entered school, the $Vintage_{st}$ variable allows older reforms to have different impacts from more recent reforms (Lafortune, Rothstein, and Schanzenbach 2016). I control for a host of commuting zone-cohort level covariates to increase the precision of the model. Commuting zone fixed effects are represented by θ_c which captures time-invariant omitted variables, cohort fixed effects γ_t captures time variant omitted variables that are common to all commuting zones. With the inclusion of both θ_c and γ_t, this is the standard setup for a DD analysis in the panel data setting with the treatments applied at different time points for different groups (in our case states). In absence of the treatment (when $Exposure_{st}$ is 0), this model assumes that commuting zones will follow the same time trend γ_t, which is the DD assumption. The error term ε_{cst} is clustered at the state level, allowing for within-state spatial correlations as well as autocorrelations (Bertrand, Duflo, and Mullainathan 2004).

The above parametric model assumes the impact of SFRs to be proportional to the duration of the exposure. However, non-linearity can arise for various reasons. For instance, if later investment in education depends on earlier investment, then SFRs will only have effects if the duration of exposure is sufficiently long. On the other hand, if there are decreasing returns to investment, then the effects of SFRs will plateau after certain amount of exposure. The following semi-parametric specification is estimated to relax the linearity assumption:

(3.2) $$Mobility_{cst} = \sum_{i=-5}^{12} Exposure\_i_{st} + \beta_{c}Covariates_{cst} + \theta_{c} + \gamma_{t} + \varepsilon_{cst}$$

In the above equation, the linear exposure variable in equation (1) is replaced by a set of dummy variables $Exposure\_i_{st}$, representing different exposure duration. For instance,

$Exposure\_8_{st} = 1$ if the cohort experienced exactly 8 years of SFRs, $Exposure\_8_{st} = 0$ otherwise. Besides the ability to capture non-linearity, this setup (sometime known as event analysis) can test for preexisting trends. If court verdicts are truly exogenous, they should not have effects on cohorts that missed the reform. On the other hand, if the observed treatment effect is the artifact of certain unobserved trends, these trends will also have effects on cohorts that missed the treatment. In this placebo test, we expect that dummy variables representing missing the SFRs (i<0) to have little effects.

3.2.2 2SLS estimation of the impacts of education equity on intergenerational mobility

Intuitively, if public education financing becomes more equitable, children from poor families are going to receive more resources resulting in higher levels of upward mobility. This intuition is formalized in Solon's (2004) model where the author shows that intergenerational mobility, reversely measured by intergenerational elasticity, increases with the progressivity of the public education system.

However, cross-country patterns of intergenerational mobility and education equity do not always conform to this intuition and the predictions of Solon's model. For example, Checchi et al. (1999) present evidence that Italy, despite having a more egalitarian education system than the U.S., exhibit less intergenerational mobility across occupations and education level. To reconcile this paradox, the authors devised a theoretical model in which a centralized education system could produce less intergenerational mobility compared to the private education system by stifling individual efforts. Needless to say, causality cannot be established with a two-country comparison. However, Checchi et al. (1999) demonstrate that the relationship between education equity and intergenerational mobility is not theoretically given, and that more empirical evidence is needed to establish the causality between the two.

One straightforward approach to estimate the impacts of education equity on intergenerational mobility is to directly regress mobility measures on education equity measures using the panel fixed effects model. Although superior to cross-sectional analysis, OLS analysis using panel data still suffers from strong endogeneity concerns. Public funding is usually tied to the proportion of disadvantaged students, such as students who are learning English as a second language. An increase in the proportion of these students would cause public funding to increase. However, the positive impacts of additional funding might be offset by the negative effects of unobservable student characters.

In this part, I use court-ordered finance reform as the exogenous shifter to quantify the causal impact of education equity on intergenerational mobility. Inequity measures are constructed for each state-cohort pair using a method that is similar to that used in Card and Payne (2002). The following school-district-level regression is estimated for each state s and cohort t:

$$PPE_d = \beta_{0,} + \beta_{inc} * median_{inc_d} + \beta_{con}Control_d + e_d \qquad (3.3)$$

School districts are indexed by d, the indexes for states and cohorts are suppressed. Total per pupil expenditure PPE_d is averaged over the period when the children are in school (from 6 to 18 years old.) School district median family income $median\_inc_d$ is measured at the time when the child first enter school (6 years old). The coefficient of education expenditure regressed on median family income β_{inc} is the measure of education inequality for that state/cohort (called expenditure-income gradient, or EIG). Following Card and Payne (2002), the control variables include dummy variables for school districts with less than 100 pupils, with 100 to 200 students, and with 200 to 300 students; the log of the average school size within the district, the ratio of population living in urban areas, and the ratio of black and native American population, all measured when the cohort is 6-years-old. The regression is weighted by the number of pupils in the school district.

State-cohort level education equity (reversely measured by EIG) is used as an explanatory variable for the different measures of intergenerational mobility. It is assumed to be endogenous to the determination of mobility, and is instrumented by the cohort's exposure to court-ordered SFRs using the 2SLS method:

(3.4) $$EIG_{cst} = \beta_r Exposure_{st} + \beta_{c1} Covariates_{cst} + \theta_c + \gamma_t + \varepsilon_{cst}$$

(3.5) $$Mobility_{cst} = \beta_p \widehat{EIG}_{cst} + \beta_{c2} Covariates_{cst} + \theta'_c + \gamma'_t + \varepsilon'_{cst}$$

Because $\widehat{EIG}_{cst}$ is an estimated coefficient, the first stage regression is weighted by the inverse of the standard deviation of the equity estimation.

3.3 Data

3.3.1 School Finance Lawsuits

Starting with the Serrano v. Priest 1971 case in California, school finance lawsuits had been brought up in 42 states. Among them, 29 had at least one verdict in favor of reforms. Political scientists and economist (Lynn 2011; Baicker and Gordon 2006) have found the results of school finance lawsuits, often determined by intense and protracted legal battles, hard to predict. Therefore, they are often considered as exogenous by economists.

Table 3-1. Court verdicts of school finance litigations and the subsequent reforms types.

State	Year of first verdict against SFRs	Year of first verdict for SFRs	Type of lawsuits
Alabama		1993	Adequacy
Alaska		1999	Adequacy
Arizona	1973	1994	Adequacy
Arkansas		1983	Equity
California	1986	1971	Equity
Colorado	1982		
Connecticut	1985	1978	Equity
Delaware			
District of Columbia			
Florida	1996		
Georgia	1981		
Hawaii			
Idaho	1975	1998	Adequacy
Illinois	1973		
Indiana			
Iowa			
Kansas	1981	1972	Equity
Kentucky	2007	1989	Adequacy
Louisiana	1976		
Maine	1995		
Maryland	1972	2005	Adequacy
Massachusetts	2005	1993	Adequacy
Michigan	1973	1997	Adequacy
Minnesota	1971		
Mississippi			
Missouri		1993	Adequacy
Montana		1989	Equity
Nebraska	1993		
Nevada			
New Hampshire		1993	Adequacy
New Jersey		1973	Equity
New Mexico		1998	Equity
New York	1972	2003	Adequacy
North Carolina	1987	1997	Adequacy
North Dakota	1993		
Ohio	1979	1997	Adequacy
Oklahoma	1987		
Oregon	1976	2009	Adequacy
Pennsylvania	1975		

Rhode Island	1995		
South Carolina	1988	2005	Adequacy
South Dakota			
Tennessee		1993	Equity
Texas	1989	1973	Equity
Utah			
Vermont	1994	1997	Equity
Virginia	1994		
Washington	1974	1977	Adequacy
West Virginia		1979	Adequacy
Wisconsin	1989	1976	Equity
Wyoming		1980	Equity

Source: Jackson et al. (2015) table D1.

Due to the broad interests in SFRs, inventories of school finance lawsuits have been assembled by various sources. In this study, I adopt a recent catalog by Jackson et al. (2015), which contains information for both legislative reforms and school finance lawsuits, the type of the lawsuits, the results of the lawsuits, and the type of school finance formulas adopted before and after the SFRs (table 3.1). In this study, I only consider the court-ordered reforms, because the legislative reforms might be endogenous. Also, since a successful lawsuit may open the gate for successive lawsuits, I only consider the first lawsuit overturning the existing system. Finally, the treatment is defined by the result of the lawsuits, ignoring differences in the strength of state response and the type of new funding formulas adopted. These responses are from the legislation and are not exogenous to local conditions as court decisions. Therefore, the estimations in this paper should be interpreted as the average total effect of initial SFRs and the subsequent SFRs that they brought about.

3.3.2 Mobility data

Intergenerational mobility data are published with the CHKS studies, and detail information can be found in the original papers. Here I only provide a brief introduction to facilitate readers' understanding of the present study. Intergenerational mobility is measured at the commuting-zone-level by the associations between children's college attendance rate (income rank) and their parents' family income. These aggregates come from population based, individual level federal tax records, which remain confidential to most researchers. The college attendance (child income rank) data is available for 10 (7) birth cohorts from 1984 to 1993 (1980 to 1986).

College attendance is measured at age 19, child income is measured at age 26, and parent income is measured when the children is 15 ~ 19 years old. Both child and parent income ranks are national: for example, a parent income rank of 0.25 means that the parents' family income is higher than 25% of other parents of the child's birth cohort in the US. Children are assigned to locations based on where they were first claimed as dependents in tax records, regardless of whether they migrated later on. Because the first year of the tax data is 1996, 98.9% of children are assigned to their location in 1996 or 1997. CHKS showed that in a given geographical unit, both the college attendance rate and child income rank have approximately linear relationships with parent income rank and can be summarized by intercepts and slopes. The intercept represents the average college attendance rate (income rank) of children with the lowest parent income rank, while the slope represents the difference between children with the highest and the lowest parent income rank. The average outcome of children with any parent income rank can be calculated using the intercept and the slope. Because parent income is measured as national ranks, the results are comparable across commuting zones.

3.3.3 School districts finance and demographic data

State-cohort level education expenditure equity measures are constructed from school district-level data. These data include elementary and secondary school expenditure data from Census of Government (compiled in Data Files on Historical Finances of Individual Governments) and Local Education Agency Finance Survey (F-33) data from National Center for Education Statistics (NCES). The former contains information for independent school districts from 1967 to 2012, while the later contains all school districts from 1990 to 2010 (1991, 1993, and 1994 are not available). The NCES data are used when possible, and missing observations are imputed using the average of the two nearby years. I only keep school districts that have no missing data after imputation for all years under study (from 1990 to 2011). School district characteristics are from Local Education Agency Universe Survey Data by NCES, which contains enrollment and the number of schools. Demographic data for school districts include median family income, the ratio of population living in urban areas, and the ratios of black and Native American population, which can be found in the Census School District Tabulation Data accessed through the School Districts Demographic System of NCES. For each cohort, the expenditure variable is averaged over the years that the cohort is in primary and secondary schools, and the school characteristics and demographic variables are measured at year when the cohort is 6 years old. Demographic data are only available every 10 years, and the years in-between are calculated using linear interpolations.

3.3.4 Commuting-zone-level covariates

The following time-variant commuting zone level variables are included as controls to increase the precision of the model. Medicaid benefits, Supplemental Nutrition Assistance Programs (SNAP) benefits, and Earned Income Tax Credits (EITC) benefits (all from Bureau of Economic Analysis), normalized by the population below 125% poverty line (which is roughly how the eligibility of these programs is determined) control for other government policies. Poverty rates (Decennial Census and American Community Survey), unemployment rates (Bureau of Labor Statistics), the share of manufacturing employment (BEA), per capita personal income (BEA), and violent crimes (Federal Bureau of Investigation, Uniform Crime Reporting Program Data) per year per thousand population control for local socio-economic conditions. The number of degree granting institutions with Title IV programs per million population, the enrollment (first-time undergraduate in the fall semester) of these colleges per thousand population, and the enrollment weighted 1-year tuitions and fees for in-state undergraduate students living on campus control for local access to colleges (National Center for Education Statistics, Integrated Postsecondary Education Data System). Monetary variables have been deflated to 1992 dollars using the nation Consumer Price Index published by the Bureau of Labor Statistics. The college access variables are measured at the year when the child turns 18 (e.g. 2011 college data for the 1993 cohort), all other variables are averaged over the period between birth and 18 years old (e.g. 1993~2011 data for the 1993 birth cohort.) Poverty between census years are linear interpolations from nearest census years.

Table 3-2. Summary statistics for college attendance analysis.

Variable	Mean	Std. Dev.	Pct. Change
College gradient	0.72	0.08	-8.32
College intercept	0.15	0.08	14.43
Poverty rate	18.05	6.76	13.75
Child poverty rate	18.20	6.78	14.29
Per capita income	18.88	3.54	12.28
Manu. Employment	11.22	5.21	-31.16
Medicaid	2.91	1.03	51.72
SNAP	0.35	0.12	23.13
EITC	0.40	0.08	65.47
Unemployment	5.94	2.12	1.96
Crime	3.07	2.07	-7.69
#. Of colleges	19.30	16.68	-3.77
Enrollment	9.88	36.96	7.00
Tuition	4.26	3.20	2.86

Notes: The first column and the second column are the means and standard deviations of the variables. The third column is the percentage change from the average of the 1984 and 1985 cohorts to the average of the 1993 and 1994 cohorts. All changes are statistically significant at the 1% level by pairwise t-tests.

Table 3-3. Summary statistics for income rank analysis.

Variable	Mean	Std. Dev.	Pct. Change
Rank-rank slope	0.27	0.06	1.43
Rank intercept	0.39	0.06	1.66
Poverty rate	17.84	7.09	-0.56
Child poverty rate	17.94	7.12	-0.47
Per capita income	17.50	3.14	7.87
Manu. Employment	13.70	7.52	-13.81
Medicaid	2.13	0.83	40.37
SNAP	0.34	0.12	-2.83
EITC	0.27	0.06	55.51
Unemployment	6.07	2.47	-5.49
Crime	3.20	2.38	-1.43
#. Of colleges	19.84	16.73	-1.41
Enrollment	8.00	7.78	8.54

Tuition	3.62	2.88	14.22

Notes: The first column and the second column are the mean and standard deviations of the variables. The third column is the percentage change from the average of the 1980 and 1981 cohorts to the average of the 1985 and 1986 cohorts. All changes are statistically significant at the 1% level by pairwise t-tests.

3.4 Evaluating the effects of SFRs on intergenerational mobility using DD

3.4.1 DD results

This section reports the effects of exposure to SFRs on intergenerational mobility measured by college attendance rates and child income rank. Table 3.4 reports the effects of years of exposure on the college attendance rates of children with bottom parent income, top parent income, and on the college attendance slope. Results with and without commuting-zone-level covariates are reported separately, and the latter is the preferred specification. For children with bottom parent income, 10 years of exposure to the initial SFR increase average college attendance rate by 5.72%, which is about a third of the pooled average at the bottom parent income level (table 3.2). For children with top parent income, 10 years of exposure to the initial SFR has a statistically insignificant effect of 1.28%. In the specification with no covariates, the effect is higher (2.37%) and weakly significant, but it is insubstantial compared to the pooled average of 87% at the top parent income ranking. Turning to the college attendance slope, I found that 10 years of exposure to SFR reduces the attendance gap between children with top and bottom parent income by 3.92%. This reduction reflects that fact that SFRs have strong positive effects on children with bottom parent income and weaker positive effects on children with top parent income. The estimation is stable with or without the inclusion of commuting-zone-level covariate, which is evidence supporting the exogeneity of court-ordered SFRs.

Table 3-4. DD estimations of the impacts of 10-years-exposure to SFRs on the college attendance rate of bottom and top income children and on the college attendance gradient.

	College attendance bottom income		College attendance top income		College attendance gradient	
Exposure	0.0572***	0.0520***	0.0237*	0.0128	-0.0335*	-0.0392**
	(0.0127)	(0.0129)	(0.0127)	(0.0138)	(0.0182)	(0.0155)
Reform vintage	0.0065	0.0152*	-0.0291*	-0.0196	-0.0355*	-0.0348**
	(0.0100)	(0.0081)	(0.0165)	(0.0131)	(0.0177)	(0.0140)
Poverty rate		-0.0382		-0.1030***		-0.0648**
		(0.0308)		(0.0276)		(0.0314)
Child poverty rate		0.0449		0.1034***		0.0585*
		(0.0306)		(0.0267)		(0.0309)
Per capita income		-0.0023		0.0028		0.0051
		(0.0028)		(0.0031)		(0.0047)
Manu. Employment		0.0005		0.0009		0.0004
		(0.0007)		(0.0014)		(0.0015)
Medicaid		-0.0088		0.0002		0.0090
		(0.0083)		(0.0144)		(0.0142)
SNAP		0.0250		0.0570		0.0320
		(0.0823)		(0.1082)		(0.1214)
EITC		0.3073***		-0.1160*		-0.4233***
		(0.0573)		(0.0675)		(0.0947)
Unemployment		0.0149***		0.0290***		0.0141
		(0.0045)		(0.0081)		(0.0096)
Crime		-0.0003		-0.0030		-0.0027
		(0.0024)		(0.0019)		(0.0028)
#. Of colleges		0.0001		0.0004		0.0002
		(0.0003)		(0.0006)		(0.0007)
Enrollment		0.0006		-0.0005		-0.0011**
		(0.0004)		(0.0003)		(0.0004)
Tuition		-0.0006		-0.0010		-0.0003
		(0.0008)		(0.0006)		(0.0007)
Missing tuition		0.0159*		-0.0348**		-0.0507***
		(0.0085)		(0.0141)		(0.0187)
_cons	0.1210***	-0.2191**	0.8471***	0.6261***	0.7261***	0.8452***
	(0.0164)	(0.0850)	(0.0145)	(0.1205)	(0.0204)	(0.1464)
N	7,665	7,655	7,665	7,655	7,665	7,655

However, when similar analysis is performed on child income rank, exposure to SFRs has small and statistically insignificant effects on all three mobility measures. The lack of effects could be due to deficiencies in the data. The income data only contains 7 cohorts (1980 ~ 1986), meaning that the first and last cohorts have substantial overlap in primary and secondary schools. Also, to have a panel as long as possible, children's income is measured in one year at age 26. This age is at the lower end of the acceptable range to measure child income (Solon 1992), and can cause income measures to be noisy. Despite these data concerns, there remains the possibility that SFRs have little impacts on children's adult income, which would contradict recent finding by Jackson et al. (2015) and confirm early conclusions by (Hanushek 2003). The remaining analysis is only conducted on college attendance rate.

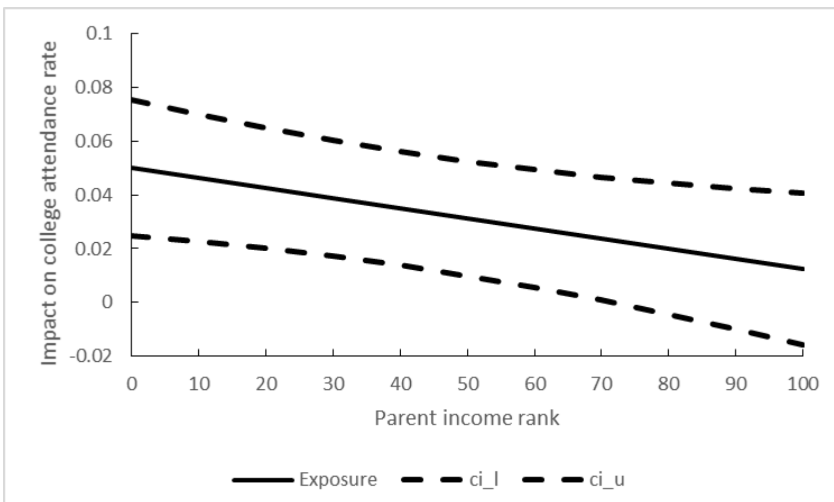
Table 3-5. The effects of ten years exposure to SFR on children's income rank.

	Child income rank at bottom parent income		Child income rank at top parent income		Rank-rank slope	
Exposure	0.0008	0.0021	-0.0041	-0.0049	-0.0049	-0.0070
	(0.0117)	(0.0060)	(0.0116)	(0.0066)	(0.0183)	(0.0085)
Reform vintage	-0.0042	0.0002	0.0071	-0.0000	0.0113	-0.0002
	(0.0127)	(0.0070)	(0.0112)	(0.0071)	(0.0147)	(0.0083)
Covariates	N	Y	N	Y	N	Y
Constant	0.3921***	0.3958***	0.6637***	0.6393***	0.2716***	0.2435**
	(0.0090)	(0.0551)	(0.0096)	(0.0737)	(0.0137)	(0.0957)
N	5,416	5,409	5,416	5,409	5,416	5,409

Note: * p<10%, ** p<5%, *** p<1%. Standard errors are clustered at the state level.

Next, the regression with covariates is conducted for children on different points of the parent income spectrum. Because the trend is smooth, the analysis is conducted on 10% intervals on the parent income ranking. As shown in Figure 3.4, the impact of SFR is highest for children with bottom parent income, and decreases to statistical insignificance at about 70% parent income ranking. With every 10% increase in parent income ranking, the impact of SFRs decreases about 0.39%, but is positive throughout the parent income spectrum. There are potentially two mechanisms driving this downward trend: first, the funding changes caused by SFRs are progressive; second, the outcomes of children with lower parent income are more sensitive to the same amount of funding increase, because of diminishing returns to education investment. These results show that SFRs have progressive impacts on children's college attendance rate, achieved by raising the performance of children with lower parent income. This is consistent with previous literature that found SFRs achieve equalization by leveling-up (Murray et al. 1998).

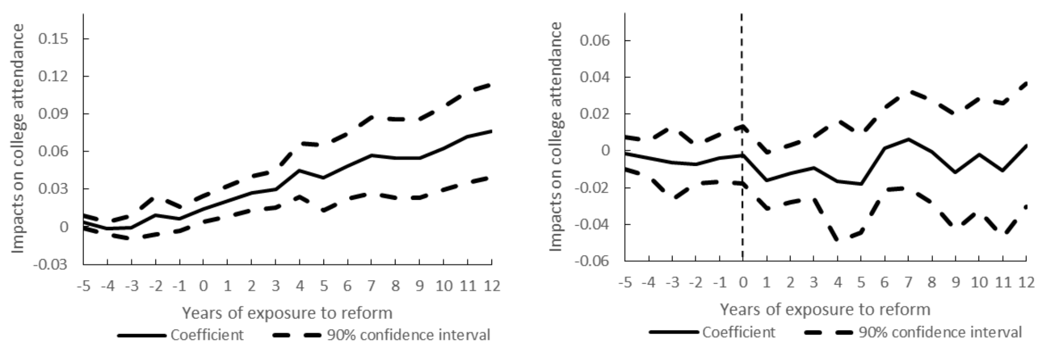
Figure 3-4. The impacts of SFRs across the parent income spectrum.

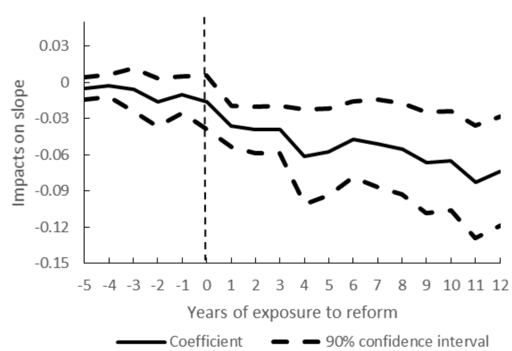


Note: College attendance rates are calculated for 10% intervals of parent income ranks using the intercepts and slopes published by CHKS. Regressions in table 3.3 (with covariates) are estimated for each parent income rank percentile. The coefficient estimates are represented by the solid line and 90% confidence intervals are represented by the dashed lines. The y-axis represents the change in college attendance rate caused by 10 years of exposure to SFRs.

By measuring the impacts of SFRs as the number years of exposure, it is assumed that the effect increases linearly with the duration of exposure. I examine this assumption by replacing years of exposure with dummies variables indicating each year of exposure. As Figure 3.5 shows, starting from no exposure (=0), the effects of the initial SFR on children with lowest parent income increase in an approximately linear relationship with the duration of the exposure, suggesting that the more parsimonious specification has captured the underlying data generation process. Also, the college attendance slope decreases with the years of exposure, also in an approximately linear fashion. The effects of SFRs on children with highest parent income hover around zero throughout the years and is not statistically significant.

Figure 3-5. The effect of the duration of exposure on the average college attendance rate of children with lowest parent income (upper-left), highest parent income (upper-right), and college attendance slope (bottom-left).





Note: Replacing the years of exposure in the regressions (with covariates) in table 3 by dummy variables representing each year of exposure. Negative exposure means SFRs happen after the cohort's graduation. Coefficient estimates are represented by the solid line and 90% confidence intervals are represented by the dashed lines.

3.4.2 Robustness checks

Testing the exogeneity of court verdicts using failed lawsuits

The identifying assumption of this study is that court verdicts are unpredictable, and therefore can be treated as exogenous events. However, while the verdicts themselves are arguably exogenous, the decisions by legal activists to file these lawsuits may depend on local conditions. For example, legal activists may prioritize states that have large existing inequality in education. Since to have a verdict there has to be a lawsuit first, the observed treatment effects could be driven by unobserved local conditions that are correlated with the occurrence of lawsuits.

To test for this potential endogeneity, exposure to the first court verdict upholding the current system (i.e. failed lawsuits) is included together with exposure to SFRs. The results in table 3.6 show that in all regressions under the preferred specification, exposure to failed lawsuits have no significant impact on children’s outcome. Even if the occurrence of lawsuits is driven by unobserved local trends, this test finds no evidence that these trends are correlated with mobility outcomes.

Table 3-6. The effects of ten years exposure to SFR on children's income rank.

	College attendance bottom income		College attendance top income		College attendance gradient	
Exposure	0.0555***	0.0500***	0.0234*	0.0124	-0.0321	-0.0377**
	(0.0130)	(0.0127)	(0.0134)	(0.0140)	(0.0193)	(0.0163)
Upheld	-0.0188	-0.0199	-0.0025	-0.0047	0.0164	0.0153
	(0.0318)	(0.0346)	(0.0216)	(0.0212)	(0.0308)	(0.0253)
Reform vintage	0.0050	0.0136*	-0.0293*	-0.0200	-0.0342*	-0.0336**
	(0.0105)	(0.0080)	(0.0170)	(0.0135)	(0.0186)	(0.0146)
Covariates	N	Y	N	Y	N	Y
Constant	0.1356***	-0.2090**	0.8490***	0.6285***	0.7134***	0.8375***
	(0.0313)	(0.0889)	(0.0261)	(0.1227)	(0.0379)	(0.1480)
N	7,665	7,655	7,665	7,655	7,665	7,655

Note: * p<10%, ** p<5%, *** p<1%. Standard errors are clustered at the state level.

The problem caused by migration

In the CHKS data, children are assigned to the location where they first showed up as dependents of their parents in the tax records. For 98.9% of children, their location is determined by where they lived in 1996 or 1997, which is the first year of available tax records. Because children's location is only observed once, they could have lived in a different state from where they are assigned to. Although inter-state migration rate is low in the U.S. and have been decreasing in recent decades (Molloy, Smith, and Wozniak 2011), its potential impacts on our results should not be neglected.

First, migration can cause measurement errors in the treatment variable. The number of years of exposure is calculated based on the assumption that the children live in the same state throughout their school years, which is not true for children who migrated cross states. The standard econometric results tell us that measurement errors in independent variables will bias the coefficients toward zero, making the treatment effects that we measured a conservative lower bound.

Second, parents' can migrate in response to the SFRs, causing endogeneity. Low-income parents may choose to migrate into states with SFRs seeking better education for their children. These children are likely to be more upward mobile either because their parents are more willing to invest in their education and/or because they are more talented. Compared to the measurement errors caused by exogenous migration, endogenous migration causes a bigger problem because it may change how the treatment effect should be interpreted: instead of the result of investment in education, the treatment effect could merely be the result of changing demographic composition in cohorts.

Since most children's location is assigned in 1997, we expect that only SFRs that happened before the location assignment have impacts on the cohort composition. If endogenous migration is driving the observed treatment effects, SFRs after 1997 would have much lower effects because they cannot influence cohort composition. Table 3.7 presents results where reforms before (including) 1997 and reforms after 1997 are entered separately. The results show that post-1997 SFRs have slightly stronger effects on the intercept, in contrary to the predictions of endogenous migration. Therefore, although I cannot completely rule out the existence of endogenous migration, there is no evidence that it is driving the observed treatment effect.

Table 3-7. Robustness check for sorting.

	College attendance bottom income		College attendance top income		College attendance gradient	
Exp_before1997	0.0554***	0.0509***	0.0198	0.0089	-0.0356*	-0.0420**
	(0.0131)	(0.0135)	(0.0132)	(0.0143)	(0.0188)	(0.0161)
Exp_after1997	0.0732***	0.0626***	0.0591**	0.0517*	-0.0141	-0.0110
	(0.0210)	(0.0215)	(0.0257)	(0.0306)	(0.0361)	(0.0413)
Vintage	0.0058	0.0147*	-0.0305*	-0.0212	-0.0364*	-0.0359**
	(0.0100)	(0.0080)	(0.0169)	(0.0135)	(0.0182)	(0.0142)
Covariates	N	Y	N	Y	N	Y
Constant	0.1140***	-0.2222**	0.8316***	0.6147***	0.7176***	0.8369***
	(0.0171)	(0.0866)	(0.0140)	(0.1205)	(0.0227)	(0.1490)
N	7,665	7,655	7,665	7,655	7,665	7,655

Note: * p<10%, ** p<5%, *** p<1%. Standard errors are clustered at the state level.

3.5 2SLS estimations

This section presents 2SLS estimations where exposure to court-ordered SFRs is used as the instrumental variable to quantify the impact of education equity on college attendance outcomes. The baseline specifications use one variable to measure exposure, while dummy variables representing years of exposure (from 0 to 12) are used as instruments in robustness checks. Education equity is measured by the expenditure-income gradient, representing how much per pupil expenditure would increase with one dollar increase in school-district median family income. To make the results easier to interpret, the expenditure-income gradient variable has been normalized to mean of zero and standard deviation of 1. The dependent variables are the average college attendance rates for children with the lowest parent income, for children with highest parent income, and the attendance slope representing the difference between the two.

The first stage regression results (Appendix Table 3.1) show that 10 years of exposure to reform reduces the expenditure-income gradient by about 1.04 standard deviations (s.d.=2.65 cents/dollar). This is consistent with Card and Payne (2002) who use similar first-stage regressions. It shows that exposure to SFR increases education equity by reducing the expenditure-income gradient.

Table 3-8. OLS and 2SLS estimations of the impacts of the expenditure-income gradient on the college attendance rate of bottom and top income children and on the college attendance gradient.

	College attendance, bottom income			College attendance, top income			College attendance gradient		
	OLS	2SLS (1)	2SLS (2)	OLS	2SLS (1)	2SLS (2)	OLS	2SLS (1)	2SLS (2)
Expenditure-gradient	0.0087**	-0.0287**	-0.0223**	-0.0034	-0.0045	-0.0017	-0.0113**	0.0242*	0.0206*
	(0.036)	(0.0107)	(0.0094)	(0.0043)	(0.0122)	(0.0112)	(0.0049)	(0.0126)	(0.0119)
Commuting-zone covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
First stage F-value	NA	13.20	12.13	NA	13.2	12.13	NA	13.2	12.13
N	7516	7655	7655	7516	7655	7655	7516	7655	7655

Note: * p<10%, ** p<5%, *** p<1%. Standard errors are clustered at the state level. The instrument for 2SLS (1) is the number of years exposed to SRFs (0~12). The instruments for 2SLS (2) are dummy variables for the years of exposure to SFRs. The first stage of the 2SLS regressions are weighted by the inverse of the estimation standard error of the expenditure-gradient.

I use the predicted expenditure-income gradient in second stage regressions (table 3.8). The baseline results show that a one standard deviation increase in expenditure-income gradient will decrease the average college attendance rate of children with the lowest parent income by 2.87%, and reduces the attendance slope by 2.42%. These effects are slightly smaller in the robustness checks using dummy variables as instruments. The expenditure-income gradient shows no significant effect on the attendance rates of children with the highest parent income. OLS estimations, when statistically significant, produce the opposite results with 2SLS, demonstrating the importance of correcting the endogeneity bias of education expenditures.

3.6 Conclusions

SFRs are built upon the premise that by increasing the equity of education expenditures, the students' outcome will depend less on their family backgrounds. Using commuting-zone-level intergenerational mobility data for 10 birth cohorts, this study evaluates the long-term effects of SFRs on college attendance rates, and tests the above premise. The results show that SFRs have made a meaningful impact on low-income-students in terms of college attendance: for children with lowest parent income, 10 years exposure to SFRs will increase their college attendance rate by 5.72%, or a 35.2% relative increase from children with corresponding parent income but did not experience SFRs. This impact decreases linearly with parent income rank, and becomes statistically insignificant at about 70% parent income rank, but remains positive. As a result, 10 years of exposure to SFRs decreases attendance rate gap between children with lowest and highest parent income by 3.94%. These findings suggest that SFRs have improved the college attendance equality by lifting up low income children. Event analysis shows that effect increases in proportion to the duration of exposure to SFRs. However, when income rank is used as the mobility measure, SFRs show no significant effects. This seems to contradict recent results by Jackson et al. (2015), however it could also be the result of lower data quality in income ranks compared to college attendance rates.

After quantifying the impacts of SFRs, this paper uses SFRs as natural experiments to answer a broader question: to what degree, if any, intergenerational mobility is determined by educational equity? This paper finds that education equity has statistically significant effects, but the size of effect depends on the mobility concept in question. If mobility is measured by the chance of children from poorest families making it to college, then one standard deviation increase in education equity can increase this chance by 2.87%, or a 17.7% relative increase; if mobility is measured by the relative difference between the poorest and richest children, then one standard deviation increase reduces this gap by 2.42%, which is only a 3.38% relative decrease. For policymakers, it suggests that policies aimed at increasing education equity, such as SFRs, can substantially benefit poor children but they alone are not enough to overcome the high degree of existing inequalities.

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Appendix. First-state regression results

Appendix Table 3-1. First Stage regression of expenditure gradient regressed on exposure and other covariates.

Dependent variable	Expenditure Gradient
Exposure	-1.0370***
	(0.2854)
Reform vintage	0.4692***
	(0.1243)
Poverty rate	0.0720
	(0.1981)
Child poverty rate	-0.0624
	(0.1968)
Per capita income	-0.0247
	(0.0388)
Manu. Employment	0.0235**
	(0.0099)
Medicaid	0.0546
	(0.0477)
SNAP	0.7670
	(0.9871)
EITC	0.7803
	(0.6679)
Unemployment	-0.0325
	(0.0283)
Crime	-0.0481*
	(0.0242)
#. Of colleges	0.0009
	(0.0053)
Enrollment	-0.0012
	(0.0020)
Tuition	-0.0043
	(0.0044)
Missing tuition	0.0101
	(0.0851)
cohort_dummy1	1.0013***
	(0.1294)
cohort_dummy2	0.7867***
	(0.1201)
cohort_dummy3	0.6533***
	(0.1093)
cohort_dummy4	0.5223***

	(0.0984)
cohort_dummy5	0.4193***
	(0.0879)
cohort_dummy6	0.3160***
	(0.0773)
cohort_dummy7	0.2306***
	(0.0667)
cohort_dummy8	0.1709***
	(0.0548)
cohort_dummy9	0.1040***
	(0.0372)
_cons	-0.5034
	(0.6415)
N	7,516
R2_W	0.74

Note: * p<10%, ** p<5%, *** p<1%. Standard errors are clustered at the state level. The regression is weighted by the inverse of the estimation standard error of the expenditure-gradient.

Chapter 4 The Intergenerational Persistence of Self-employment and the Inheritance of Risk Tolerance in China

Abstract

Children whose parents were self-employed before China's socialist transformation were more likely to become self-employed themselves after the economic reform even though they had no direct exposure to their parents' businesses. The effect is found in both urban and rural areas, but only for sons. Furthermore, asset holding data indicate that households with self-employed parents before the socialist transformation were more risk tolerant. These findings suggest that the taste for self-employment is an important conduit of parents' effects on self-employment, and that the taste being transferred can be mapped to known entrepreneurial attitudes.

4.1 Introduction

Family background is one of the most important determinants of self-employment. Studies reviewed by Arum & Müller (2009) estimated that having an entrepreneurial father increases a son's probability of becoming an entrepreneur by 30-120%. Traditional explanations for this intergenerational correlation include the inheritance of family business, financial capital, and human capital (Dunn and Holtz-Eakin 2000; Fairlie and Robb 2007a; Parker 2009). Recently, a number of studies have suggested that the inheritance of taste for self-employment may be an important mechanism (Lindquist, Sol, and Van Praag 2015; Sørensen 2007; Halaby 2003). These studies also suggest that more research is needed on two aspects.

First, most previous studies cannot conclusively separate the effects of taste from the effects of business human capital. While some studies control for working experience in family businesses (Sørensen 2007; Hoffmann, Junge, and Malchow-Møller 2015), they cannot rule out the transmission of business human capital because children can be exposed to family business before they enter the labor market (Laband and Lentz 1983; Lentz and Laband 1990). Given that exposure to family businesses can have crucial effects on self-employment (Fairlie and Robb 2007a; Carr and Sequeira 2007; Fairlie and Robb 2007b), it is not clear how much of the intergenerational correlation in self-employment is caused by the inheritance of taste.

Second, previous studies inferred the importance of taste from indirect evidence such as gender and occupational patterns of the intergenerational transmission of self-employment (Lindquist et al. 2015; Hoffmann et al. 2015). As a result, the nature of the taste being transferred remains poorly understood. Considering that self-employment is associated with a established set of attitudes (Douglas and Shepherd 2002; Simoes, Crespo, and Moreira 2015) and that some of these attitudes, such as risk tolerance, can be transmitted across generations (Dohmen, Falk, Huffman, and Sunde 2012), it is a natural extension that these attitudes could be the conduits for parent effects. However there have been few studies in this direction (Wyrwich 2015; Zhang et al. 2009).

China's recent history of a planned economy provides a unique opportunity to shed light on the first issue. Because the long absence of private economic activities ruled out any exposure to family business for a generation, it is plausible to assume that this generation did not inherit business human capital. Several papers have studied the determinants of entrepreneurial activity in China, and confirmed the existence of parental effects (Djankov, Qian, Roland, & Ekaterina, 2006; Lu & Tao, 2010; Mohapatra, Rozelle, & Goodhue, 2007; Wu, 2006). However, these studies only consider the effects of parents' current or recent self-employment status, and none of them address whether the intergenerational persistence of self-employment survived more than 20 years of planned economy.

Using a nationally representative cross-sectional household survey and controlling for city (urban areas) or county (rural areas) fixed effects, individual characteristics, and environmental factors, we find that having at least one self-employed parent before the planned economy era[7] increases one's own probability to be self-employed by 26% in urban areas and 30% in rural areas. In both urban and rural areas parent effects are only found for men but not for women. Moreover, the self-employment of women in urban area is affected by the self-employment of their parents-in-law, suggesting that their decisions are strongly influenced by their husbands.

To provide direct evidence that the taste for self-employment can be transferred from parents to children, and to demonstrate that such taste can be mapped to known attitudes of entrepreneurs, we compare the risk attitudes of children with and without self-employed parents.

[7] From here on, when parents' self-employment is mentioned, it means self-employment before the planned economy era unless otherwise indicated.

Risk attitude is a crucial personal trait for entrepreneurs (Caliendo, Fossen, and Kritikos 2009; Fairlie and Holleran 2012; Hvide and Panos 2013; Parker 2009) that can be rigorously defined and measured. Previous studies (Hu 2014; Djankov et al. 2006) find that it also plays important roles in entrepreneurial decisions in China. The detailed household asset information in our data allows us to evaluate risk attitude using the share of risky asset holdings (Arano, Parker, and Terry 2010; Cardak and Wilkins 2009; Friend and Blume 1975) in the urban area[8]. Controlling for other determinants of asset holding and city fixed effects, we find that households with at least one self-employed parent hold 17% more risky assets compared to those without self-employed parents.

This study makes two main contributions to the entrepreneurship literature. First, it establishes that self-employment can be transmitted across generations without any exposure to family businesses, indicating the importance of taste in the intergenerational persistence of self-employment. Second, by showing that self-employed parents pass their risk tolerance on to their children we gain a more concrete understanding about the nature of the taste being transmitted. Furthermore, by documenting the remarkable resilience of entrepreneurship, this study may be of interest to scholars who wish to understand the historical legacies of the planned economic era and the vigorous revival of the private economy after the economic reform.

The remaining chapters are arranged as follows: Section 4.2 briefly reviews the historical background before, during, and after the planned economy era in China. Section 4.3 introduces the CHIP data for 2002 and the empirical setup. Section 4.4 presents the intergenerational persistence results for urban and rural areas, explores gender differences, and rule out direct exposures before and after the planned economy era. Section 4.5 introduces the method for estimating risk aversion using risky asset holdings and presents results. Section 4.6 concludes.

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[8] This analysis cannot be done for the rural areas where the stock market was under-developed because virtually no household in our rural sample owns stocks.

4.2 Historical background

In 1952, a couple of years after the founding of the People's Republic of China, socialist transformation had just started, and private economy still accounted for 78.7% of national income (the sum of "capitalist" and "individual" economy in the official statistics). In sectors where self-employment are most concentrated, the private economy still dominated. For example 96.9% of handicraftsmen worked in private individual businesses. In capitalist industry, privately owned enterprises were still producing 39% of total output, although down from 62.3% in 1949 (State Statistical Bureau 1960).

China's socialist transformation accelerated in 1953. Through a combination of policies including expropriation and buy-out of industrial enterprises and cooperatives for small proprietors, the ownership of enterprises transferred from private individuals to the state. By 1956, this transformation was basically competed, with 99% of industrial enterprises and 92% of small proprietors in cooperatives (State Statistical Bureau, 1960, pp36-38). As the Great Leap Forward started in 1958, the private economy had mostly been eliminated (Wu, 2010, pp-155). Although it is possible that a "shadow economy" continued to exist after the transformation (Kornai, 1992, pp86), private entrepreneurial activities were extremely marginalized.

Private business was gradually legalized again after the First Session of the Fifth National People's Congress in 1978. A series of subsequent laws and regulations established the legal rights of private businesses (for a more detailed account of entrepreneur policy reform, see Gold [1990].) When the legalization of private business activities first started, it was partly intended to reduce urban unemployment, and only individuals living on the margins of society were expected to take up the opportunities. However, as early entrepreneurial adventures turned out to be profitable, more individuals from different social strata joined the ranks of the self-employed. In both urban and rural areas, studies around the year 2000 found that most self-employed individuals were entrepreneurs by opportunity, not necessity (Liu and Huang 2016; Mohapatra et al. 2007).

4.3 Method

4.3.1 Data

This study uses the CHIP 2002 data. CHIP is a nationally representative repeated cross-sectional survey (Gustafsson, Li, and Sato 2014). The 2002 wave is chosen because it is the only time that parent information was asked. An urban sample was drawn from 70 cities in 12 provinces and a rural sample was drawn from 122 counties in 22 provinces. We analyze the urban and rural samples separately.

Since parents' self-employment experience before the transformation is only asked for household heads and their spouses, other household members are excluded from the sample. We further exclude individuals whose parents had not reached the age of 20 in 1958 (i.e. birth year > 1938 for both parents). The child's age range is set between 25 and 75 in 2002, so that they were old enough to make an occupational choice by 2002, but not too old to work in 1978 when the reform started. In rural areas, self-employment status is only available for those currently working, so only working individuals are included. The percent of working household heads and spouses is high in rural areas (85.3%), allowing us to preserve most of the sample. To remove the inheritance of family business, we eliminate the 5 individuals in the urban sample who inherited their business (this information is not available in the rural sample). This procedure produces an urban sample with 10,751 individuals and rural sample with 10,295 individuals. The exact number of observations further varies depending on the specifications.

Table 4-1. Summary statistics.

Variables	Urban			Rural		
	Full sample	With self-employed parent(s)	Self-employed	Full sample	With self-employed parent(s)	Self-employed
Self-employed	6.5%	8.2%		7.1%	9.9%	
At least one parent self-employed	10.0%		12.6%	6.5%		9.1%
Father self-employed	9.0%	90.4%	11.9%	6.3%	96.9%	8.8%
Mother self-employed	4.0%	39.7%	4.6%	1.3%	19.3%	2.7%
At least one parent-in-law self-employed	9.2%	32.0%	11.9%	6.4%	26.2%	8.9%
Female	48.3%	50.3%	45.1%	45.1%	43.6%	29.2%
Married	97.0%	97.1%	97.6%	97.9%	98.4%	98.5%
Household size	3.0	3.0	3.2	4.1	4.1	4.0
Age	48.1	50.6	43.8	46.3	46.6	44.4
Age squared/100	24.1	26.6	19.8	22.2	22.5	20.5
Years of education	10.5	10.2	9.3	6.6	7.2	7.4
Years of education squared/100	1.2	1.1	0.9	0.5	0.6	0.6
Belong to the largest extended family				43.5%	44.2%	44.3%
Distance to nearest county seat (km, village level)				22.9	20.3	18.7
Non-ag business per household (village level)				1.5%	1.7%	2.1%
A member of the CCP	33.2%	31.5%	12.1%	12.6%	16.5%	13.2%
Total asset (million)	0.141	0.149	0.131	0.043	0.056	0.071
The average years of education of parents	4.7	4.3	4.5	2.0	2.8	2.5
At least one parent is a member of the CCP	30.7%	14.2%	27.2%	11.2%	21.0%	15.4%
The average birth years of parents	1924.1	1919.9	1927.2	1925.6	1925.4	1927.0
N	10751	1070	696	10295	668	729

4.3.2 Definitions of self-employment for the parent and child generations

In both urban and rural areas information regarding parents' self-employment is captured by the survey question: "*Did they (the father/mother of the household head/spouse) engage in any kind of industry or commerce?*" An individual is considered to have self-employed parent(s) if the answer is "yes" for his/her mother and/or father. When exploring gender differences, we also consider the influence of parents-in-law. The self-employment status of parents-in-laws is also drawn from this question. Parents' information is included in the regression regardless of whether they are alive or deceased.

In the urban sample, we start with a narrow definition that only includes the currently working sample and define self-employed children as those whose occupation is "owner or manager of private firm" or "self-employed", and those whose "net income of businessmen or self-employed" is positive (we use the latter information to capture secondary occupations, which are not recorded in urban areas). We then broaden this definition to include previous self-employment experience as much as possible. Doing so allows us to include the unemployed and retired in the study, and we refer to this as the full sample. In the full urban sample, individuals who satisfy at least one of the following conditions are classified as self-employed:

1. For those currently working, in addition to the original definition, we include those who "got the current job by starting their own business" and those whose occupation within the past three years is "owner or manager of private firm" or "self-employed".

2. For those currently unemployed, the occupation type of the first and/or last job from which they became unemployed has to be "owner or manager of private firm" or "self-employed", or they have to have secured at least one of those jobs by starting own business.

3. For those currently retired, the occupation of the job they retired from has to be "owner or manager of private firm" or "self-employed".

4. The individual's net income from private business and self-employment is greater than zero in 2002.

In the rural sample, only current occupation is available, and an individual in the child generation is defined as self-employed if his/her primary or secondary occupation is "owner or manager of enterprises" or "non-farm individual enterprise owner".

4.3.3 Regression Models

We regress the self-employment status of children on the self-employment status of parents using logistic regressions. The demographic control variables are common to urban and rural regressions. They include gender (male=0, female=1), age, age squared (divided by 100), years of education, years of education squared (divided by 100), marriage (married=1, otherwise 0), and household size. Location fixed effects are included as dummy variables representing each city in urban areas and each county in rural areas. In rural areas, the distance of the village to the county seat and the number of non-agricultural businesses per household in the village are included to capture within county variations. Whether the individual belongs to the largest surname family (yes=1, no=0) in the village is included as a proxy for social capital (Peng 2004).

In both urban and rural areas, political credentials (e.g., China Communist Party [CCP] membership) and total assets are conceptually important determinants of self-employment. However, these two variables raise concerns about endogeneity because they could be the result of entering self-employment: it is harder for the self-employed to join the CCP, and total assets may be higher as a result of running a business. We first exclude these two variables and then include them as robustness checks. Finally, we include three observable parent characteristics to check whether they can explain parent effects. They include the average birth year of parents, average years of education of parents, and whether at least one of the parents is a member of CCP.

4.4 The Intergenerational persistence of self-employment

4.4.1 Summary statistics

The summary statistics for the urban (full) sample and the rural sample are presented in table 4.1. In the urban sample, 6.5% of individuals are self-employed, while in the rural sample 7.5% are self-employed. In both samples, the self-employment ratio is higher among individuals with self-employed parents, and odds ratio tests show that these univariate differences are statistically significant (with odds ratio=1.34, p=0.015 for the urban sample and odds ratio=1.48, p=0.004 for the rural sample). In the following section we use multi-variate analysis to test whether this conclusion still holds. In both urban and rural areas, most individuals who have a self-employed mother also have a self-employed father, making it difficult to separately identify the effects of father and mother, therefore this approach is not pursued here.

A comparison of the socio-economic variables shows that self-employed individuals in the child generation have different social status in urban and rural areas. In urban areas, even though self-employment attracted individuals with higher status as the reform progressed, the self-employed at this time still lagged behind the average person in terms of years of education (9.3 years vs. 10.5 years) and assets (0.131 million Yuan vs. 0.141 million yuan). The self-employed especially lack political credentials, measured by CCP membership (12.1% vs. 33.2%). On the other hand, the social status of the self-employed in rural areas is higher than average, with more years of education (7.4 years vs. 6.6 years), higher party membership rate (13.2% vs. 12.6%), and more assets (0.071 million Yuan vs. 0.043 million yuan).

4.4.2 Marginal effects of parents' self-employment on children's self-employment

In this section, we first present results of logistic regressions of children's self-employment status on parents' self-employment experience. Then results by gender are presented and the effects of parents-in-law are included. Finally, we discuss and rule out pre-transformation and post-transformation exposures to family business.

In the urban sample, we start with the currently working sample and define self-employment as currently self-employed (table 4.2, column 1). The average marginal effect of having at least one self-employed parent on the probability of own self-employment is 1.83%. When the full base sample and the broad definition of self-employment are used (table 4.2, column 2), the marginal effect of self-employed parents is found to be 1.67%, or a 26% increase from the self-employment probability for persons without self-employed parents. We use the second specification (table 4.2, column 2) to draw estimations, test alternative specifications, and explore gender differences for the urban sample.

Among the control variables, the probability of becoming self-employed is lower for women. The effect of being married is not significant, possibly because almost all individuals in our sample are married (97%). Having a larger household size leads to higher probability of being self-employed, possibly because family members provide labor and other forms of support to the family business. At the sample means, each additional year of age decreases the probability of self-employment by about 0.42%, and each additional year of education decreases probability of self-employment by about 0.39%.

We present two alternative specifications as robustness checks. First (table 4.2, column 3), we test whether controlling for current CCP membership and assets changes the main result regarding parent effects. This robustness check shows that whether these two variables are included or not has no noticeable impact on the parent effect estimation. Being a CCP member is associated with a 5.3% decrease in the probability of self-employment. Current total asset has a statistically significant but economically small correlation with the probability of being self-employed: a 10% increase of total assets from the mean value of 0.141 million Yuan is associated with a probability increase of 0.07%.

Last but not least, we test whether the estimated parent effects can be explained by observable parent characteristics, which could help shed light on the pathways of the intergenerational persistence of self-employment (table 4.2, column 4). Parents' party membership has a sizable negative impact (-1.57%) on children's self-employment status, suggesting the importance of family connections in securing jobs in formal sectors. Parents' average birth year and years of education have no significant impact on children's self-employment status, meaning that their effects mainly work through children's characteristics such as their own age and years of education. These observable parent characteristics only explain a small proportion (from 1.67% in table 4.3, column 2 to 1.49% in column 4) of the overall parent effect.

Table 4-2. Average marginal effects of self-employed parents on urban children's self-employment from logistic regressions.

	Urban				Rural		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
At least one parent self-employed	0.0183	0.0167	0.0161	0.0149	0.0204	0.0173	0.0191
	(0.0100)*	(0.0071)**	(0.0071)**	(0.0072)**	(0.0087)**	(0.0087)**	(0.0095)**
Female	-0.0029	-0.0187	-0.0242	-0.0186	-0.0485	-0.0531	-0.0450
	(0.0065)	(0.0046)***	(0.0046)***	(0.0046)***	(0.0056)***	(0.0056)***	(0.0062)***
Married	-0.0069	-0.0137	-0.0137	-0.0134	0.0146	0.0129	0.0034
	(0.0214)	(0.0148)	(0.0147)	(0.0148)	(0.0196)	(0.0191)	(0.0205)
Household size	0.0124	0.0102	0.0092	0.0099	-0.0036	-0.0061	-0.0036
	(0.0048)***	(0.0032)***	(0.0032)***	(0.0032)***	(0.0023)	(0.0023)***	(0.0026)
Age	-0.0142	0.0005	0.0007	0.0007	-0.0039	-0.0056	-0.0035
	(0.0042)***	(0.0025)	(0.0024)	(0.0025)	(0.0025)	(0.0024)**	(0.0027)
Age squared/100	0.0128	-0.0049	-0.0047	-0.0053	0.0019	0.0040	0.0020
	(0.0048)***	(0.0026)*	(0.0026)*	(0.0027)**	(0.0026)	(0.0026)	(0.0030)
Years of education	0.0060	0.0057	0.0044	0.0057	0.0078	0.0082	0.0080
	(0.0044)	(0.0031)*	(0.0030)	(0.0031)*	(0.0039)**	(0.0039)**	(0.0043)*

Years of education squared /100	-0.1231 (0.0241)***	-0.0911 (0.0169)***	-0.0747 (0.0167)***	-0.0900 (0.0169)***	-0.0397 (0.0265)	-0.0471 (0.0268)*	-0.0399 (0.0288)
Belong to the largest extended family					-0.0095 (0.0055)*	-0.0110 (0.0054)**	-0.0122 (0.0061)**
Distance to nearest county seat					-0.0005 (0.0002)***	-0.0004 (0.0002)**	-0.0005 (0.0002)***
Family business per household					0.2384 (0.0880)***	0.2116 (0.0947)**	0.2338 (0.0937)**
Member of the CCP			-0.0530 (0.0071)***			-0.0201 (0.0076)***	
Total asset (million)			0.0514 (0.0143)***			0.5725 (0.0539)***	
The average years of education of parents				0.0005 (0.0007)			0.0019 (0.0015)
At least one parent is a member of the CCP				-0.0157 (0.0055)***			0.0112 (0.0076)
The average birth years of parents/100				-0.0026 (0.0004)			0.0438 (0.0421)
City/county fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	7,131	10,751	10,751	10,737	10,295	10,274	8,534

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors are calculated using the Delta method and reported in the parenthesis. Column (1) is the baseline estimation for the urban working sample. Column (2) ~ (4) are the results for the urban full sample with various specifications. Columns (5) ~ (7) are results for the rural sample with various specification.

In the rural sample, having self-employed parents increases the probability of a child being self-employed by 2.04%, a 30% increase from the self-employment rate of 6.9% among individuals without self-employed parents (table 4.2, column 5). This is the baseline estimation for rural areas. Being a woman in the rural area has a larger negative impact than in urban areas, suggesting higher barriers for women to enter self-employment. Years of education has a positive impact on self-employment, in contrast to the negative impact in urban areas. One more year of education increases the probability of self-employment by 0.78%. This result implies the “quality” of self-employment is high in rural areas (Mohapatra et al. 2007), with higher returns attract better educated people and relatively sophisticated operations that demand more education of the owner. Belonging to the family with the largest surname in the village decreases one's probability to become self-employed slightly, by 0.9%. The surname variable has been used in earlier studies as the proxy for the social capital. Our finding suggests that higher social capital exerts a net push from self-employment, possibly by increasing access to other job opportunities. The distance from the village to the nearest county seat has a negative impact on self-employment, indicating the importance of market access. The positive effect of the average number of businesses per household in the village captures other village level factors that are conducive to self-employment, potentially including local economic conditions, institutions, and peer effects.

Next, we test two alternative specifications similar to the robustness checks in the urban sample. First (table 4.2, column 6), we include two potential self-employment determinants that are likely to be endogenous. Including these two variables decreases the parent effect from 2.04% to 1.73%, but it is still statistically significant. Similar to the urban results, CCP party membership is negatively related to self-employment, while total assets are positively related to self-employment. Second (table 4.2, column 7), including observable parent characteristics has little impact on the parent effect estimation, and none of the parent characteristics are significant.

We further explore gender differences in the inheritance of self-employment status. To this end, we run the baseline regressions (table 4.2, column 2 for urban areas and column 5 for rural areas) for men and women separately (Table 4.3). In both urban and rural areas, own parents only have significant effects on sons but not on daughters. The fact that women did not retain their parents' self-employment status could explain the gender gaps in self-employment rates in urban and rural areas. However, we find that the women's self-employment status in urban areas is strongly influenced by parents-in-law, implying that their self-employment decisions may be influenced either by their husbands or by their parents-in-law directly.

Table 4-3. Average marginal effects of self-employed parents and parent-in-laws on children's self-employment by gender.

	Sons		Daughters	
	Urban			
At least one parent self-employed	0.0214 (0.0103)**	0.0208 (0.0107)*	0.0159 (0.0105)	0.0096 (0.0108)
At least one parent-in-law self-employed		0.0024 (0.0114)		0.0257 (0.0107)**
N	5,515		4,925	
	Rural			
At least one parent self-employed	0.0279 (0.0136)**	0.0235 (0.0139)*	0.0113 (0.0146)	0.0063 (0.0151)
At least one parent-in-law self-employed		0.0234 (0.0143)		0.0203 (0.0148)
N	5525		3324	

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors are calculated using the Delta method and reported in the parenthesis. The urban result is based on column (2), table 4.2, and the rural results are based on column (4), table 4.2. Coefficients estimates for control variables are omitted. Additional cities have to be dropped because having no self-employed sons or daughters, which causes the numbers of observations to decrease.

4.4.3 Ruling out direct exposures

Although it is unlikely that the observed intergenerational persistence is due to children having direct exposure to family businesses, this possibility still exists in two ways. The first is for children who are old enough to have direct exposure before the socialist transformation, and the second possibility is that some pre-transformation self-employed parents may have re-entered self-employment after the economic reform, and their children may have received post-reform exposure. We further investigate and rule out these two possibilities here.

For pre-transformation exposures, we first remove people born before 1958. Using the baseline models (Table 4.2, column 2 and column 5 for rural), we find that parent effects are actually larger for the younger samples (2.94% for urban and 4.2% for rural; full results available from authors upon request), possibly because younger people had more opportunity after the economic reform to act on their entrepreneurial intentions. Alternatively, we use the full samples and add interaction terms between having self-employed parents and the number of years the person lived before 1958 to control for pre-transformation exposure. The added interaction terms are not significant in both urban and rural samples and the estimated parent effects (1.77% for urban and 2.56%) are closer to the original ones. Therefore, pre-transformation exposure can be ruled out as the driver of the results.

For post-transformation exposures, we first remove children whose parents are self-employed after the economic reform from the sample, then control for the post-reform self-employment status. In both cases, parent effects are only slightly smaller (1.55% and 1.49%) and still statistically significant. Parents' post-reform self-employment is positive but not statistically significant when pre-transformation self-employment is also included, but significant otherwise. Parents' post reform occupation is not available in the rural survey; therefore we use an alternative strategy of keeping only those whose parents were older than 60 years (birth year < 1920). The estimated parent effects barely changed (1.95%).

4.5 Self-employed parents and children's risky asset holding

In this section we present evidence based on urban asset holdings to show that the children of self-employed parents are less risk averse. The ratio of risky asset holding is a commonly used empirical measure of risk aversion (Riley and Chow 1992; Bucciol and Stuefer 2012; Schooley and Worden 1996; Arano et al. 2010). Its theoretical foundation can be traced back to Friend and Blume (1975), who show that under a first order approximation, a household's ratio of risky assets has a linear relationship with its relative risk aversion coefficient (Pratt 1964). With additional assumptions (Arano et al. 2010), it can be shown that the relative risk aversion coefficient is proportional to the ratio of risky assets up to a constant that is the same for all households in the market.

Table 4-4. Summary statistics for asset analysis in urban areas.

	All	With self-employed parent(s)	Self-employed
Asset shares			
Fixed deposits	45.7%	45.8%	36.4%
Current deposits	20.9%	21.3%	20.8%
Stocks	5.6%	6.3%	3.3%
Bonds	1.9%	2.0%	1.1%
Money lent out	3.1%	3.7%	4.1%
Family business fund	2.2%	2.9%	16.2%
Investment in other businesses	1.1%	1.6%	1.2%
Accumulated housing fund	13.2%	9.9%	6.2%
Commercial insurance deposit	3.6%	3.9%	3.6%
Fixed production assets	2.7%	2.5%	7.2%
Risky asset ratio	6.7%	7.9%	4.5%
Other variables			
At least one parent self-employed	15.8%	-	21.3%
Total investment asset (million Yuan)	0.042	0.047	0.051
Asset squared	0.0049	0.0057	0.0097
5-yr average income (Million Yuan)	0.018	0.018	0.017
Household size	3.0	3.0	3.2
Years of education	10.5	10.1	9.6
Years of education squared/100	0.5	0.4	0.4
Age	48.1	50.9	43.7
Age squared/100	24.2	27.0	19.6
Married	95.1%	96.4%	97.7%
Household head and/or spouse is currently working	77.0%	68.9%	94.0%

The analysis is conducted at the household level. A household is defined as self-employed if the head or his/her spouse is self-employed by the full definition. A household is considered to have a self-employed parent if at least one of the four household parents is self-employed. The analysis is conducted for urban area only since risky assets mainly consist of stocks, and virtually no rural households own any stocks in the data. We define risky assets as the monetary value of stocks and investments in enterprises or other businesses (not including one's own family business). We have tested alternative definitions of risky assets with only stocks, and the results (available upon request) are similar. Self-owned funds for a family business and fixed production assets are considered safe assets in order to avoid the concern that higher risky asset holding is only a result of self-employment. In doing so we under-estimate the risk tolerance of currently self-employed individuals who take risks in investing in their own business. This will in turn bias the effect of self-employed parents toward lower risk tolerance, since as shown in the previous section, their children are more likely to become self-employed. Total assets include all investment assets (as listed in the upper panel of table 4.4), and exclude durable goods and other assets. Housing assets can be both for consumption and for investment, and the property rights for housing are complicated in China. We exclude housing from total assets in the main specification, and include them for a robustness check (results available upon request), and the results are similar. To reduce the impact of outliers, households with total assets less than 1000 RMB or shares of risky assets above 90% are removed from the sample.

We regress the ratio of risky assets on the self-employment status of parents, control variables, and city fixed effects using Tobit regressions with the dependent variable bounded between zero and one. The control variables include total assets and their square, controlling for possible increasing or decreasing relative risk aversion; average household come for the past 5 years, average age of the household head and spouse (if present), average age squared, dummy variables for having at least one working member between the household head and spouse, average years of education, average years of education squared, and household size.

Table 4-5. Total marginal effects from Tobit model for risky asset holding in urban areas.

	All	Non-self-employers	Self-employers
At least one parent self-employed	0.0108	0.0124	0.0202
	(0.0051)**	(0.0055)**	(0.0132)
Total household investment asset (million Yuan)	1.1500	1.2052	0.8602
	(0.0741)***	(0.0805)***	(0.1981)***
Asset squared	-2.1131	-2.1228	-1.8922
	(0.2004)***	(0.2198)***	(0.5035)***
5-yr average income (Million Yuan)	-0.0415	-0.0803	0.2975
	(0.1962)	(0.2218)	(0.3679)
Household size	0.0002	0.0015	-0.0156
	(0.0027)	(0.0028)	(0.0092)*
Years of education squared/100	-0.0002	0.0000	-0.0232
	(0.0032)	(0.0034)	(0.0100)**
Years of education squared/100	0.0135	0.0086	0.1408
	(0.0147)	(0.0157)	(0.0489)***
Age	0.0010	-0.0004	0.0204
	(0.0017)	(0.0018)	(0.0080)**
Age squared/100	-0.0019	-0.0007	-0.0223
	(0.0017)	(0.0018)	(0.0088)**
Married	0.0003	0.0003	0.0095
	(0.0099)	(0.0105)	(0.0329)
Household head and/or spouse is currently working	0.0084	0.0125	0.0027
	(0.0067)	(0.0072)*	(0.0282)
N	5,747	5,174	517

Notes: * p<0.1; ** p<0.05; *** p<0.01. Standard errors are calculated using the Delta method and reported in the parenthesis.

The regression described above is estimated using the entire sample, then for non-self-employed households and self-employed households separately (table 4.5). Even with the downward bias on risk tolerance, having a self-employed household parent is still estimated to increase the ratio of household's share of risky assets by 0.0108 (out of 1). This is economically significant considering the mean risky asset ratio in the sample without self-employed parents is only 0.065 (a 17% increase). The estimated parent effect is higher in the non-self-employed sample possibly because they do not have the downward bias from excluding family business assets. Not surprisingly, self-employed parents have no significant effect in the self-employed sample. Self-employed households are self-selected by their risk-loving attitudes, and there is no obvious reason why self-employed parents should have an effect in this highly selective group.

4.6 Conclusion

Although studies in many countries have found that children with self-employed parents are more likely to become self-employed themselves, the fact that this intergenerational correlation had survived the two decades of planned economy era in China is still surprising. Using data from the CHIP 2002 survey, we find that having parent(s) who were self-employed before the socialist transformation increases a child's probability of being self-employed by 26% in urban areas and by 30% in rural areas. This intergenerational persistence of self-employment is only found for sons, and not for daughters. In urban areas, where female self-employment rates are higher, we find that women's self-employment is influenced by their parents-in-law, instead of their own parents. It would be interesting to carry out comparative studies in the future with other former planned economies to assess whether the above findings hold under different historical circumstances.

Compared to previous studies that control for work experience in family businesses, this study goes a step further by ruling out all experience of family business, and thus more convincingly establishes that the taste for self-employment alone can sustain substantial intergenerational self-employment correlation. The magnitude of the parent effects found in this study is comparable to those found in other studies with family business experiences, suggesting that taste can explain a significant proportion of the total parent effect. This general finding is consistent with both the genetic and role model explanations of parent effects on self-employment.

The gender differences found in this study with parents affecting only sons is consistent with the role model explanation which predicts same-sex role modeling (Lindquist et al. 2015; Hoffmann et al. 2015; Ruef, Aldrich, and Carter 2003). Since our data contains few mothers that were self-employed independently of their husbands, mothers' influence on daughters may not be accurately measured. On the contrary, the gender pattern found in this paper is the opposite of what a previous genetic study of twins found (Zhang et al. 2009).

This study enriches our understanding of role model explanation by showing that the taste for self-employment being passed on through role models can be mapped to the extensively studied personal characteristics of entrepreneurs. Using risk tolerance as an example, we find that children of self-employed parents hold 17% more risky assets compared to those without self-employed parents, which indicates they have higher risk tolerance. This result points to a new direction in the area of family and entrepreneurship studies, which is to systematically examine the inheritability of the personal characteristics associated with entrepreneurs.

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