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# Essays in Economic History

by

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Abstract

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William Faulkner once said, "To understand the world, you must first understand a place like Mississippi". This dissertation embraces Faulkner's insight by examining Southern states during the early 20th century to extract broader economic lessons about human capital development, household labor dynamics, and migration patterns. Through three interconnected essays, I examine how compulsory schooling laws, household labor allocation, and religious networks shaped individual outcomes and regional development during a transformative period in American history.

The first chapter introduces a novel dataset of county-level compulsory schooling laws in the US South from 1900-1940 to investigate how education affected wages through both direct and spillover effects. Using spatial variation in adoption timing, I find that white workers leveraged increased schooling to access higher-wage occupations, with a Mincer return of 0.15, while black workers gained similar years of education but saw no significant wage returns due to occupational segregation. A spatial model of skill supply and demand reveals strong complementarities between skill groups for white workers but much weaker effects for black workers, highlighting how labor market discrimination limited the equalizing potential of education policy.

The second chapter examines how households responded when compulsory schooling laws eliminated child labor as an economic resource. By constructing a measure of household-level exposure to these laws and linking individuals across censuses, I show that economically vulnerable families compensated by redirecting older siblings toward immediate labor market participation, sacrificing their human capital development. Older brothers in affected households experienced lower educational attainment and approximately 0.7 percent lower wages for each year of schooling required of their

younger siblings. These findings support the interpretation that child labor primarily served subsistence needs rather than reflecting parental preferences.

The third chapter investigates how religious networks influenced migration patterns during the Great Migration. Co-authored with Daria Bakhareva, this paper combines census data with records from the Census of Religious Bodies to demonstrate that religious similarity between counties significantly predicted higher migration flows for black Americans. These networks primarily facilitated migration through non-wage channels rather than by providing information about better-paying jobs. Using religious networks as an instrument for migration flows, we find that white workers in the South experienced substantial wage benefits from black out-migration through occupational upgrading, while black workers saw more modest gains from reduced labor market competition.

Together, these essays provide new insights into how educational expansion, family labor allocation, and migration networks shaped economic development and inequality in the American South during the early twentieth century.

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# Acknowledgments

Seven years. Three chapters. More than a few breakdowns. There are many ways to measure my time as a PhD student, but none would be so meaningful as counting the people who helped me along the way.

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# Chapter 1

# On Schooling and the Growth of Wages: Evidence from the Development of the US South

## 1.1 Introduction

There is a general consensus that education is a necessary condition for growth. This is reflected in the budgets of governments, who devote trillions of dollars to education every year; the research agenda of development economists, who have conducted over one hundred field experiments on education in the last 25 years (Glewwe and Muralidharan, 2016); and the priorities of NGOs, as the United Nations has included the universal proliferation of secondary education by 2030 in its Sustainable Development Goals.

However, this belief is motivated by the well-studied private return to schooling, often called the Mincer return, which only partially governs the effect of education policy. The total return depends not only on those who receive more education, but also on the spillover effect for all others in the economy. Do the benefits of education for one worker come at the cost of another? Or, is it possible that all workers benefit from a better-educated labor force? In this paper, I argue that the answer depends critically on one structural parameter—the substitutability of labor across skill groups. If they are substitutes, then wages of low skilled workers will fall as the supply of high skill workers increases. By contrast, if they are complements, then low skill workers will benefit as the demand for their labor increases with the supply of skilled labor.

A novel dataset of county-level US compulsory schooling laws provides me with a unique opportunity to study how education policy affects the broader economy through spillover effects. I collected this data directly from the original legislation in which the laws were passed, which I identified by searching through the archives of each state's

legislature. It includes over 100 laws passed by Southern states from 1900-1940, almost half of which were adopted at the county level. This spatial variation in adoption timing, combined with detailed census data on education and wages, allows me to separately identify both direct effects of compulsory schooling exposure and indirect spillovers through neighboring counties.

I leverage this spatial and temporal variation to develop a strategy for identifying effects on adult labor-market outcomes in the 1940 census. My approach uses a modified difference-in-difference framework that accounts for both direct exposure effects and spatial spillovers. By comparing cohorts just above and below compulsory schooling age requirements within the same county, I identify the direct effect of exposure on educational attainment and wages. Simultaneously, I capture spillover effects by examining outcomes for similar cohorts in nearby counties that were indirectly affected through interconnected labor markets. This spatial identification strategy allows me to distinguish between the private returns to schooling and broader general equilibrium effects, while controlling for unobserved confounding factors through comparison with more distant control counties that share similar regional characteristics.

The reduced-form results reveal black and white workers increased their educational attainment similarity following exposure to these laws. For white workers, direct exposure to compulsory schooling increased schooling by approximately 0.13 years. Black workers showed similar increases in schooling of about 0.10 years.

Despite these similar gains in years of schooling, the economic returns diverged sharply along racial lines. White workers translated their additional education into wage gains of about 1.5%, implying a Mincer return of around 0.15. They primarily leveraged education to access higher-wage occupations in the service sector. In contrast, black workers experienced no significant wage gains from their increased schooling. Instead, they primarily used education to transition from agricultural labor to farm operation, reflecting the severe occupational barriers of the Jim Crow era that limited their economic opportunities. These patterns highlight how labor market discrimination prevented black workers from accessing the higher-paying occupations that education made available to white workers.

Beyond these direct effects, I find evidence of significant spillover effects in cohorts born in nearby markets. Workers in counties neighboring those that adopted compulsory schooling laws show intriguing patterns - they attain modestly lower levels of schooling but experience higher wages. Specifically, while direct exposure increased schooling, indirect exposure through neighboring counties decreased schooling by approximately 0.06 years. Despite this decrease in educational attainment, these workers saw positive wage gains similar to those directly affected. These seemingly contradictory results - higher wages despite lower schooling in spillover counties - suggest important general equilibrium effects operating through labor markets.

I develop a spatial model of labor supply and demand to rationalize these spillover

results. The model reveals how compulsory schooling laws affect neighboring markets through migration channels. When compulsory schooling exposure increases in market A, it reduces the supply of low-skill labor, thereby raising wages for a fixed level of demand. Workers who anticipate employment in market A may observe these higher wages and choose to end their schooling early in response. This suggests that schooling decisions for any cohort depend not only on their direct exposure to compulsory schooling laws but also on the exposure of all other cohorts in the economy—a prediction supported by the empirical findings. The model thus provides a coherent framework for understanding the seemingly contradictory result of higher wages despite lower schooling in spillover counties, highlighting important general equilibrium effects operating through spatial labor markets.

The spatial model further illustrates that the total effect of education policy is governed by two key parameters—the private Mincer return and the elasticity of substitution between skill groups. It also motivates the identification strategy employed in the empirical analysis, as well as a migration-weighted measure of market-level exposure to compulsory schooling laws from all cohorts in the economy. This approach captures how the supply of skill in each market depends on the schooling decisions of all potential migrants, which are influenced by compulsory schooling exposure in their birth counties. Together, these instruments allow me to estimate both the private Mincer return to schooling and the elasticity of substitution between skill groups—parameters that govern the general equilibrium effects of education policy through both direct and spatial spillover channels.

The estimated model parameters reveal striking racial differences in how education policy affected wages through both direct and spillover channels. For white workers, I estimate a Mincer return to schooling of 0.147 and an elasticity of substitution of 6.3, both precisely estimated. This combination of positive returns to additional schooling and strong complementarities between skill groups explains why both directly and indirectly affected white workers experienced wage gains. In contrast, black workers show a much different pattern: a statistically insignificant negative return to schooling (-0.046) and a considerably lower elasticity of substitution (2.4). These stark differences highlight how occupational segregation under Jim Crow severely limited the economic opportunities available to black workers. While white workers could leverage education to access higher-wage occupations and benefit from skill complementarities, black workers remained largely confined to agricultural employment regardless of educational attainment, effectively preventing them from realizing the wage benefits that education policy created for white workers in the same labor markets.

This paper combines new data on a well-studied natural experiment with a novel spatial model of skill supply and demand to provide causally-identified insights into how education policy affects wages. It contributes to a rich literature on human capital and growth (Lucas Jr, 1988, Romer, 1989, Duflo, 2001, Acemoglu and Angrist,

2000, Moretti, 2004, Kimenyi, Mwabu, and Manda, 2006), with particular relevance to causally-identified studies of human capital externalities using microdata (Duflo, 2004, Wantchekon, Klanja, and Novta, 2015). Beyond contributing new evidence on direct and spillover effects, this paper develops a model to structure our thinking around how these effects arise in general equilibrium.

This paper combines new data on a well-studied natural experiment with a novel spatial model of skill supply and demand to provide causally-identified insights into the growth effects of education policy in general equilibrium. It builds on a rich literature that considers the relationship between human capital and growth (Lucas Jr, 1988, Romer, 1989, Duflo, 2001, Acemoglu and Angrist, 2000, Moretti, 2004, Kimenyi, Mwabu, and Manda, 2006). Within this literature it is most closely related to other causally-identified studies of human capital externalities from micro data, which is a fairly sparse strand of literature due to the high data requirements (Duflo, 2004, Wantchekon, Klanja, and Novta, 2015). Beyond providing new evidence on direct and spillover effects, this paper develops a structural framework that reconciles seemingly contradictory results in the literature. The approach connects to growing research employing spatial models to study education policy The model is a structural description of skill (schooling) supply and demand, connecting the paper to a growing body of work that studies education policy (Eckert and Kleineberg, 2021, Agostinelli, Luflade, Martellini, et al., 2021, Porzio, Rossi, and Santangelo, 2021, Hsiao, 2022, Khanna, 2023), offering insights into how both direct and indirect mechanisms shape economic outcomes through general equilibrium effects.

It also directly contributes to the empirical literature studying compulsory schooling in the United States (Acemoglu and Angrist, 2000, Lleras-Muney, 2002, Stephens and Yang, 2014). A key challenge for this literature has been the availability of primary sources, though recent studies have made great progress using archival state session laws (Stephens and Yang, 2014, Clay, Lingwall, and Stephens Jr, 2021). I view my novel data of county-level compulsory schooling laws as the capstone contribution to this process, providing the first comprehensive accounting of compulsory schooling laws in the United States that includes local variation in timing. I demonstrate the value of these data by showing that, contrary to previous findings in the literature, they provide a strong first-stage effect of compulsory schooling in the South. The additional spatial variation allows me to implement a modified version of the standard identification strategy that accounts for both direct effects and spillovers.

Lastly, my focus on the historic South provides new insights into the literature on racial wage gaps and education. Previous empirical work has focused primarily on differences in educational resources, consistently finding that unequal spending on education between races was a major contributor to the black-white wage gap (Welch, 1975, Margo, 1986, Card and Krueger, 1992, C. K. Carruthers and M. H. Wanamaker, 2017). I complement this literature by estimating how the returns to a year of schooling differed

between races, conditional on school quality. The stark differences I find in both direct returns and spillover effects—with white workers benefiting through higher-wage occupations while black workers primarily used education to transition within agriculture—suggests that labor market discrimination severely limited the equalizing potential of education policy. Though my results largely accord with historical evidence on occupational segregation, to my knowledge I present the first causally-identified estimates of race-specific returns to schooling in the Jim Crow Era that account for both direct effects and spatial spillovers.

The remainder of this paper is organized as follows. Section 1.2 describes the historical context, with a particular focus on the history of public schooling and the politics of education in the South. Section 1.3 describes the data analyzed using the strategy described in Section 1.4. Section 1.5 develops a model to rationalize the results in Section 1.4, and Section 1.6 concludes.

## 1.2 Context

This section provides essential historical background for understanding the identification strategy employed throughout this paper. By examining how compulsory schooling laws were adopted across the American South between 1900-1940, I establish the foundation for the spatial and temporal variation that drives my empirical analysis. The staggered implementation of these laws—with approximately one-third at the county level and two-thirds at the state level—creates the natural experiment setting that allows me to identify causal effects of education policy on wages, labor market outcomes, and human capital development. Understanding the political economy of these reforms, including the competing interests of parents, employers, and politicians, is crucial for interpreting why adoption patterns varied across geography and time, and for evaluating the validity of the identification assumptions underlying my research design. This context also explains the complex relationship between race and education during this period, which informs my separate analysis of effects for white and black workers in subsequent sections.

### Compulsory Schooling in the South

Despite their universal adoption throughout the rest of the country, only one Southern state (Kentucky) had adopted compulsory schooling laws by the end of the 19th century. This lag reflects the historical reluctance of elites in the south to support public education. The vast majority of public schools at that time were locally-funded community schools, and both their existence and quality varied wildly across communities. With-

out a readily-available public option to meet the obligation, governments were unable to legally require children to attend school.

As the 20th century progressed, rising demand for education led to increased costs, and many rural communities became unable to locally fund their own schools. County and state governments began appropriating more funds to support schooling, paving the way for expansions to the public schooling system. Education boards were established to oversee the disbursement of funds, and these boards which would ultimately provide the infrastructure used to implement required schooling. Once money was flowing into communities and a bureaucratic body was in place, the government had both justification and means to enforce attendance.

The rush to modernize once the foundation was in place led several of the first states to adopt required schooling laws to implement them at the county level. As mentioned above, compulsory schooling laws were preceded by a switch to a state-and-county funding system, and large variation in the capacity of county governments meant many were ready to adopt before others. Rather than wait for laggers to catch-up, some states allowed counties to begin adoption early. For example, the state legislatures of Tennessee and North Carolina passed a combined 51 unique laws related to compulsory attendance in specific counties before adopting state-wide mandates, resulting in one-in-five counties having early compulsory schooling laws. Similarly, the state legislature of Arkansas exempted over half of the state's counties in their first compulsory schooling law, creating both early and late-adopting counties.[1] South Carolina, Louisiana, and Mississippi also passed laws which allowed counties to opt-in to compulsory schooling, though there is no readily-available source on which counties chose to do so. By 1920 every Southern state had passed a state-wide compulsory schooling law, with Mississippi being the last in 1918.

Compulsory schooling laws were often adopted as part of large reforms pushed by politicians platforming on public education. Other aspects of reform focused on the quality of public education, with a surprising amount of regularity across states. Reformers emphasized improving quality primarily in rural areas through the consolidation of schools and the standardization of instruction. The standardization was accomplished by implementing county-wide curriculum, setting minimum teacher qualifications, and, in some cases, creating academies for training teachers. Consolidation was accomplished by providing greater support for central, often urban schools and facilitating access to those schools.

[1] Weeks, 1912 and Rennolds, 1925 for a history of education in Arkansas and Tennessee respectively.

## The Politics of Education Reform

Education policy is a community issue with many stakeholders. This is reflected in the political discourse around education reform. Concerns of politicians at the time imply that they were navigating the needs of two key stakeholders in the economy—parents and employers.

Parents forwarded many arguments both for and against education reform. Of course, the majority of parents who supported education reform did so because they believed their child would benefit from a reformed system, though some also made arguments on pro-social grounds as well. Some parents who opposed compulsory schooling were concerned that public-school curricula would not result in labor-market returns, leading some states to develop specialized vocational curricula (both agricultural and industrial). Others claimed they could not afford for the state to adopt compulsory schooling, as many poor families relied on supplemental income from child labor.

The deep connection between education and the labor market meant employers also voiced strong opinions on education reform. The economy of the South had long been based on an abundance of unskilled, low-wage labor, and education reform threatened to upend that system. A more-skilled labor force would require higher wages, eroding what they viewed as the South's comparative advantage to the North (Wright et al., 1986). However, more than simply raising their labor cost, employers were afraid that education would induce an already mobile population to empty out their labor markets entirely. This applied from local farmers fearing the loss of rural labor to local cities up to state legislatures fearing a mass exodus to the higher-wage North (Margo, 1990).

Despite the entrenched political powers of the employer elite, politicians in each state eventually passed education reforms. Two pressures ultimately forced them to do so. First, as the demand for education grew among parents, their willingness to migrate if that demand was not met forced politicians to provide at least some public education (Margo, 1990). Second, though the established industries had an incentive to keep the labor pool both unskilled and child-abundant, attracting modern industry ultimately required investing in a more skilled labor force.

It should go without saying that race also played an important role in the politics of education reform. The "separate-but-equal" doctrine adopted in Plessy v. Ferguson in 1896 meant that governments had a right to treat black children separately in education reform but also an obligation to ensure equal access to education. In practice, however, politicians faced few incentives to fund black schooling, resulting in an education system that was both segregated and unequal. This inequality was primarily expressed through differences in the quality of education, including the qualification of teachers and the length of term. By contrast, compulsory schooling itself was rarely a racial issue, and no compulsory schooling legislation includes race-specific requirements. In other words, compelling black children to attend school was not a contentious political issue, but the

quality of learning they would receive at that school was.

## 1.3 Data

I construct the novel dataset which makes this study possible directly from the sessions laws of 11 southern US states from 1900-1940.[2] Session laws are documents published by the state that includes all acts passed by the state's legislature, organized by date. I searched through these documents to identify over 150 unique acts related to compulsory schooling attendance, over half of which were implemented at the county level. The state laws are the same as those studied in Clay, Lingwall, and Stephens Jr, 2021.[3]

### Compulsory Schooling Laws

Compulsory schooling laws share many common features that are easily encoded into data. With very few exceptions, required schooling laws required all children between two specified ages to attend school, conditional on not having already completed a given amount of schooling.[4] Virtually all states initially required 8 to 14 year olds to attend school, with most increasing the age to 16 over the sample period. Years needed to be exempt range from none to 8, though these differences ultimately adds little variation to required-schooling exposure. The majority of the variation comes from differences in the final piece of information—the implementation date.

When considering which state laws to include in my data, I follow Clay, Lingwall, and Stephens Jr, 2021 in excluding any law which did not apply to all children in the state. In most cases, these are early state laws which allowed counties to opt-into required schooling using a framework provided by the laws, though in the cases of Kentucky and Louisiana it includes laws which only applied to children in urban areas. For all of these laws I am unable to infer who was actually affected, so I instead make the conservative assumption that no-one was treated under these laws.

For one state, Georgia, I exclude all laws prior to 1939. This is primarily motivated by historical evidence. First, the historical narrative of education in Georgia suggests that it lagged behind other southern states, with major reform not coming until 1937 (O'Brien, 1999). If the government was not making a concerted effort to improve education in the

[2]States included are Arkansas, Alabama, Mississippi, Louisiana, Texas, South Carolina, North Carolina, Tennessee, Kentucky, and Georgia.

[3]The inclusion of county-level compulsory schooling laws differentiates my data, as the broader focus of Clay, Lingwall, and Stephens Jr, 2021 motivates them to limit their attention to compulsory schooling laws that applied at the state level. While the additional variation provided by these laws is advantageous within the Southern context, it is of limited value when studying the nation as a whole.

[4]Two laws in North Carolina and Arkansas stipulated that on adoption only the youngest children would be required to attend, explicitly exempting older cohorts.

state, then it is unlikely they made a concerted effort to enforce compulsory schooling specifically. This is reflected in the compulsory schooling laws of Georgia, which didn't allow for non-compliant parents to be jailed until 1939. I consider this to be the first effectively implemented compulsory schooling law in Georgia, meaning it effectively never adopted compulsory schooling for the purposes of analyzing outcomes in 1940.

**Compulsory Schooling Exposure**

I use these laws to infer the degree to which each birth-county by age cohort was exposed to compulsory schooling. I do so following the method of Stephens and Yang, 2014. For each cohort, I iterate through the ages 7 to 16 to determine if the compulsory schooling laws in their county required their attendance in each year. When exemptions applied, I consider the cohort to not be compelled if they have been required to attend for a number of years equal to the number of completed school years needed to qualify for exemption. Summing the total number of years that I infer they were required then gives the cohort's exposure to the compulsory schooling laws.

The population-weighted distribution of this compulsory schooling exposure variable is shown in Figure A.1. There are two important things to note. First, roughly 40% of the population were not required to attend school at all. Second, virtually all compulsory schooling laws were written with the intention that a fully exposed cohort would attend school for at least 6 years, so cohorts with fewer than 6 required years are those to whom the laws only partially applied at the time of adoption. For example, a cohort which was 12 when laws requiring all children aged 8-14 to attend would have two years of required schooling exposure.

Panel A of Figure A.2 shows the year compulsory schooling came into effect for each county. State-level adoptions mean this age is uniform across state for the majority of counties. However, this is not the case for Arkansas, Tennessee, and North Carolina, which have two, four, and nine different adopting ages respectively. In Arkansas this variation is generated through state laws which exempted specific counties. I will leverage this additional variation in the timing of cohort exposure to identify the effects of compulsory schooling exposure.

Though additional variation in the timing of adoption is beneficial, the spatial variation these county-level laws provide is their true value-add. This is illustrated in Panel B of Figure A.2. Here I show the distance of each county to the nearest source of compulsory schooling exposure other than its own, which we can think of as the closest source of indirect exposure conditional on their direct exposure. Note that, without the county-level adoptions, I would be limited to studying the effect of indirect exposure using comparisons that are across state borders. The variation provided by the county-level adoptions allows for within-state exposure, allowing me to identify more comparable

cohorts on which to test for indirect effects. This strategy is discussed in further detail in Section 1.4 below.

### 1940 Population Census Data

The analysis is conducted using the publicly available 1940 fullcount census data, which includes observations from all census respondents in 1940.[5] I limit the sample to men aged 20 to 55 who were born in one of the southern states. I then use crosswalks provided by the Census Linking Project (Abramitzky et al., 2019) to link individuals to the fullcount data for the census in which they would have been 7-16, the standard ages for compulsory schooling.[6] From these linked data I can observe a wealth of information regarding the household in which the respondent resided, including the county in which it was located.

The 1940 census is notable as the first population census to include questions on respondent's human capital stock beyond simple literacy status. This question, which asks the respondent for the highest grade they completed, is the variable I use to measure years of schooling. Given its important distinction as the first detailed measurement of schooling, it is not surprising that the accuracy of this variable has been an important topic for researchers of education over the years. For example, because life expectancy and educational attainment are positively correlated—average education is most likely over-estimated for older cohorts in 1940. Of particular concern in the southern context is the high frequency of grade-less schools. Enumerators were instructed to enter the number of years the respondent had attended school if their school lacked a traditional grade structure. However, this introduces additional complexity, as the number of days which constituted a school year varied greatly over space, time, and race.[7] In short we should consider this variable a particularly noisy measure of the aggregate stock of human capital, with particularly little informational content on the quality of that stock.

## 1.4 Identifying Reduced-Form Effects

In this section I discuss how I use the compulsory schooling exposure measure described in Section 1.3 to identify effects on outcomes in the 1940 census. Given the stark differences in labor market opportunities between races during this period, I conduct all analyses separately for white and black workers. The results reveal striking differ-

[5] Full count census data was access through the IPUMS USA database (Ruggles et al., 2024).

[6] For example, a respondent with age 31 in 1940 would be matched to the 1920 census, in which they would be 11.

[7] The survivor bias is confounded with the secular bias and is no greater a threat to identification. Conducting the analysis by race and conditioning on space and time alleviate the other concerns.

ences in how workers of different races translated increased education into economic opportunity, reflecting the severe occupational barriers of the Jim Crow era.

The identification strategy is motivated by a thought experiment illustrated in Figure A.3. Figure A.3 depicts a county, shown in red, adopting compulsory schooling laws. The first-stage effect is an increase in the amount of schooling received by schooling-aged cohorts and all cohorts going forward. If compulsory schooling were adopted in response to growing demand for schooling, then there should be an increase in the schooling of older cohorts leading up to adoption as well. In the absence of such pre-trends, any sudden increase in schooling can be attributed to either compulsory schooling adoption or some unobserved confounding change.

Figure A.3 also motivates a control group for unobserved confounders. Consider cohorts born in the green or blue counties, which are near the adopting county but not adopting themselves. Note that the schooling-aged cohorts from these counties may still be indirectly affected by it if their schooling decisions are based on expected returns with the possibility of migration for work. This indirect effect will attenuate as the probability of migration decreases, which can be proxied by distance. Hence, there will be some radius around the adopting county in which cohorts may be indirect affected, and cohorts born just outside that radius may provide a valid control group for both directly and indirectly affected cohorts.

## Identification Strategy

Denote the birth cohort of worker $i$ as $x = (o, a)$, where $o$ and $a$ index county of birth and age respectively, and consider the following description of years of schooling:

$$\begin{aligned} sch_i &= f(cse(x)) + \underbrace{\phi_o + \phi_a}_{FEs} + \Xi_i(x) + \varepsilon_i(x) \\ \Xi_i(x) &= \Xi(x) + \xi_i \\ \varepsilon_i(x) &= \varepsilon(x) + e_i \end{aligned}$$

which says schooling decisions can be written as a function of compulsory schooling exposure, a set of fixed effects, and observable and residual terms each of which has a cohort-common component and an individual component. While each parameter in this specification carries an implicit race subscript since the analysis is conducted separately by race, I omit these subscripts here and throughout for notational clarity.

The $\Xi(x)$ and $\xi_i$ terms are factors which can be controlled for using observed information about $x$ and $i$. They are used to conditionally identify the parameters of $f$. I account for parental selection into labor markets based on compulsory schooling laws with flexible HH socio-economic status controls in $x_i$ and, motivated by the discussion in Section 1.2, I include measures related to the quality and accessibility of schooling in $o$

during the years in which $x$ would have been schooling aged in $\Xi(x)$.[8] I account for any remaining correlation between compulsory schooling exposure and the residual using a difference-in-difference strategy described below.

### Difference Across Cohort Age

Recall from Section 1.3 that the empirical distribution of $cse(x)$ has a large mass at 0, and note that, if there is a correlation between $\epsilon(x)$ and $cse(x)$ that is driven by trends in time, then those trends should continue in the $cse(x) = 0$ sample despite a constant level of exposure. Now note that once compulsory schooling exposure starts in $o$ it does not stop, and let $a_o$ be the age of the youngest cohort with $cse(x) = 0$ born in $o$. Any confounding time trends should then be reflected in the outcomes of cohorts born relative to this cohort in the same way pre-trends appear before adoption in a standard event-study design.

This comparison to the event-study design motivates the parameterization,

$$f(cse(x)) = \beta_\tau$$

where $\beta_\tau$ is the effect of a cohort being born $\tau$ years after the reference cohort. This provides a useful parameterization of compulsory schooling exposure's effect. It also defines the first difference, which is with the reference cohort aged $a_o$.

### Difference Across Birth-County Borders

The second difference is motivated by the variation shown in Panel B of Figure A.2. Recall that this variation can be interpreted as the closest source of indirect exposure conditional on direct exposure, and consider applying the event-study parameterization described above to this indirect exposure. To fix ideas, Figure A.3 uses an example to illustrate the effect of compulsory schooling law adoption in one county. Cohorts born in the adopting county, shown in red, will have their direct exposure affected by this adoption. As direct exposure in the red county increases, indirect exposure in nearby counties increases equivalently. Hence, the relative timing of effects should follow a similar pattern to those of the direct. Specifically, there should be no systemic change in cohorts leading up to the first indirectly exposed cohort, with the effect of indirect exposure increasing in cohorts younger than the reference cohort.

Let $\tau'$ be the age gap between cohort $x$ and the reference cohort of the nearest source of indirect exposure, and note that I can write indirect effects using the parameter $\beta_{indir,\tau'}$

[8] This includes the average number of teachers and schools per students, as well as the average teacher salary in $o$ during the schooling years of $x$. These controls are similar to the school quality controls used in Card and Krueger, 1992 and are constructed from 1910-1940 county-level data provided by C. Carruthers and M. Wanamaker, 2019.

in the same way $\beta_{dir,\tau}$ can be used to write direct effects. This motivates a structural parameterization,

$$f(cse(x)) = \beta_{dir,\tau} + \beta_{indir,\tau'}$$

In other words, I can think of compulsory schooling exposure having two effects on the schooling decisions of $x$. The first is a direct effect, which depends on their age relative to the timing of adoption in their birth county. The second is an indirect effect, which depends on their age relative to the timing of adoption in counties *near* their birth county. However, note that only cohorts born close enough to an indirect exposure will actually experience the effect of $\beta_{indir,\tau'}$. This means we can think of counties as belonging to either spillover or control based on their distance to the nearest source of indirect exposure. Figure A.3 shows this grouping in blue and green using 60 and 120 mile cutoffs to define spillover and control groups. Counties with no source of indirect exposure within 120 miles (Southwest Texas) are included in a separate "excluded" group.

With this definition, the structural relationship can be written as,

$$f(cse(x)) = \beta_{dir,\tau} + Tr(x) \cdot \beta_{indir,\tau'}$$

where $Tr(x)$ is an indicator variable for cohort $x$ being born in a spillover county. Workers with $Tr(x) = 1$ experience indirect exposure, reflected by $\beta_{indir,\tau'}$ in their schooling decisions. The schooling decisions of workers in control or excluded counties do not have $\beta_{indir,\tau'}$, reflected in $Tr(x) = 0$ for those groups. Now compare this expression to the following empirical specification of schooling decisions,

$$s_i = \sum_{\tau \neq 0} t^{\tau} \cdot Tr_g(x) \cdot b_{dir,\tau} + \sum_{g} \sum_{\tau' \neq 0} t^{\tau'} \cdot Tr_g(x) \cdot b_{g,\tau'} + \gamma_o + \gamma_a + C_i(x) + E_i(x) \qquad (1.1)$$

where $Tr_g(x)$ is an indicator for the cohort belonging to group $g \in \{spill, cont\}$ and $t^{\tau}(x) \equiv \mathbb{1}\{a = a_o + \tau\}$ is an indicator for cohort $x$ being born $\tau$ years removed from the appropriate reference cohort. Under the assumption that there is no sorting on indirect exposure—a weaker assumption than no sorting on direct exposure already made—then $\hat{b}_{dir,\tau}$ and $\hat{b}_{spill,\tau'}$ will be consistent estimators of $\beta_{dir,\tau}$ and $\beta_{indir,\tau}$. Last, note that by assumption there is no structural $\beta_{cont,\tau}$ to which $\hat{b}_{cont,\tau'}$ should converge. Instead, estimates of $\hat{b}_{cont,\tau'}$ will converge to a trend in $\varepsilon_i(x)$, the unobserved structural error. To the extent that such trends exist, they can then be removed using the difference,

$$\hat{b}_{dir,\tau} - \hat{b}_{cont,\tau}$$
$$\hat{b}_{spill,\tau} - \hat{b}_{cont,\tau}$$

This defines the second difference, which is with the spatial control cohorts.

## Identifying Assumption

To causally interpret the estimates from the strategy described above, we must address potential correlations between compulsory schooling exposure and unobserved factors affecting educational outcomes. These potential threats can be categorized into two main sources: (1) correlation with cohort-level unobservables $\varepsilon(x)$ and (2) correlation with individual-level unobservables $e_i$.

As discussed in Section 1.2, the most plausible cohort-level confounders relate to contemporaneous changes in school quality and accessibility that often accompanied compulsory schooling reforms. Historical evidence suggests that compulsory attendance laws were frequently implemented alongside broader educational improvements, as governments needed adequate infrastructure before they could reasonably mandate attendance.

Similarly, at the individual level, compulsory schooling exposure could be correlated with $e_i$ if families sorted non-randomly in response to these laws. For instance, if families with stronger educational preferences systematically migrated to counties adopting compulsory schooling earlier, or conversely, if families reliant on child labor relocated to avoid these requirements, our estimates would be biased.

To empirically assess these concerns, I examine treatment effects on the observed controls $\Xi(x)_i$ using a post-exposure indicator parameterization $post(x) = \mathbb{1}\{\tau > 0\}$. This approach resembles a standard balance test in experimental economics—if compulsory schooling exposure significantly affects baseline characteristics, this would suggest selective sorting along those dimensions.

Figure A.4 presents the results of this analysis, showing the effect of any compulsory schooling exposure on various baseline controls. The findings are largely reassuring: most controls show no significant relationship with exposure status. The limited exceptions include a slight positive correlation with the number of black teachers, consistent with the historical record of educational reform packages, and modest effects on household heads' sector of employment. Specifically, compulsory schooling exposure is associated with somewhat lower agricultural employment rates, potentially indicating that agricultural households were more likely to relocate to avoid compulsory schooling requirements.

Despite these minor exceptions, the general lack of correlation between compulsory schooling exposure $cse(x)$ and observable characteristics $\Xi(x)_i$ strengthens my confidence that it is similarly uncorrelated with unobservable factors $\varepsilon(x)_i$. While this does not definitively rule out all potential confounding, it substantially mitigates concerns about selection bias driving our results.

## Reduced-Form Results

In this section, I present the causal effects of compulsory schooling exposure on educational attainment and wages based on the identification strategy outlined above. The analysis proceeds in three parts. First, I examine the event study estimates to visualize both direct effects on exposed cohorts and spatial spillovers to neighboring counties, confirming the absence of pre-trends that would threaten identification. Second, I quantify these effects using pooled estimates that provide precise measures of impact magnitudes. Third, I explore heterogeneity by race and educational infrastructure to understand how compulsory schooling laws interacted with the broader socioeconomic context of the segregated South. Throughout, I find that while compulsory schooling increased educational attainment for both white and black workers, only white workers successfully translated this education into higher wages, revealing important racial disparities in the returns to human capital investment.

Panel A of Figure A.5 reports the results of estimating the parameters of Equation 1.1. Estimates are plotted by $\tau$ on the y-axis, and the x-axis shows the difference in mean schooling with the reference cohort. Estimates of $\beta_{dir,\tau}$ are shown in black, while estimates of $\beta_{spill,\tau}$ and $\beta_{cont,\tau}$ are shown in green and blue respectively. Recall that each of these estimates can be interpreted as the average difference between the youngest, fully unexposed cohort and the cohort born $\tau$ years later. Background bars show the average amount of compulsory schooling exposure $\overline{cse}(x)$ for the given cohorts.

The estimates of primary interest are those of $\beta_{dir,\tau}$ and $\beta_{spill,\tau}$. Each line can be interpreted as the change in schooling from youngest to oldest cohort relative to the last cohort to have to compulsory schooling exposure, shown by the marker at $\tau = 0$. For example, the black line shows that there is no systematic change in schooling as we consider cohorts progressively older than the reference cohort ($\leq 0$ on x-axis), with progressively younger cohorts increasing their schooling as direct exposure increases. Interestingly, the green line shows that cohorts born in neighboring counties *decreased* their schooling in response to indirect exposure, an effect which is totally absent in the control counties. To the contrary, the control estimates show a consistent, positive trend across cohorts, motivating the estimates shown in Panel B. This counterintuitive pattern—where neighboring counties show decreased schooling—suggests labor market complementarities that will be formally explored in the structural model.

Panel B of Figure A.5 shows the difference-in-difference estimates $\hat{\beta}_{dir,\tau} - \hat{\beta}_{cont,\tau}$ and $\hat{\beta}_{spill,\tau} - \hat{\beta}_{cont,\tau}$. Even after accounting for the control trend, there are still sizable effects with no signs of pre-trends in older cohorts. The model developed in Section C.2 interprets this negative schooling effect as a response to a change in the expected return to schooling. In short, compulsory schooling adoption increases expected wages for low-skill labor, enticing some infra-marginal workers to drop out early.

Panel C of Figure A.5 provides evidence that these expectations were correct. This

panel report difference-in-difference estimates from Equation 1.1 using log wage as the outcome. Like the schooling estimates, there are no trends in the cohorts progressively older than the reference cohort. In contrast to the schooling effect, both the direct and spillover effects following the onset of exposure are positive. The model in Section C.2 shows that this pattern of results can be rationalized with a positive Mincer return to schooling and strong complementarities between workers of different schooling levels. With these, wages can rise for spillover cohorts despite their lower levels of schooling.

To complement the event study evidence, Table A.1 reports the results of estimating Equation 1.1 using a post-exposure indicator parameterization $post(x) = \mathbb{1}\{\tau > 0\}$ to estimate effects pooling the indicators in Figure A.5. These estimates are reported in the row labeled $\hat{\beta}_{dir,\tau>0}$ while those in the row labeled $\hat{\beta}_{dir,\tau<0}$ show estimates for an analogous indicator for cohorts older than the reference cohort. The former will show effects in the presence of exposure effects, while the later will show effects only if there are confounding trends in schooling leading up to adoption.

For the full sample, direct exposure increased schooling by roughly .14 years, while indirect exposure through neighboring counties decreased schooling by .062 years. These magnitudes align with the cumulative effects visible in the event study plots. The control group shows no systematic trend, with estimates close to zero and only marginally significant. The pre-trend tests provide support for the identification strategy. All pre-trend estimates are small and statistically insignificant, suggesting no systematic changes in schooling before compulsory schooling adoption.

Results are similar when restricting to the primary analysis sample of wage earners. Though point estimates are slightly larger for both direct and spillover effects, they cannot be statistically distinguished from the full-sample estimates. Furthermore, indirect exposure in the control cohorts has virtually no effect on schooling. This, along with the insignificant pre-trend tests, justifies the single-difference estimation strategy used to estimate model parameters in Section C.2.

## Heterogeneity by Race and Education Infrastructure

Having established the core direct and spillover effects, I now examine how direct impacts varied by race and educational infrastructure—dimensions crucial for understanding the distributional consequences of education policy in the segregated South. I first analyze heterogeneity based on migration status for key labor market outcomes, with particular attention to racial differences in how education translated into economic opportunity. I then examine heterogeneity by cohort-level education policy measures to understand how compulsory schooling interacted with broader educational infrastructure.

Table A.2 presents estimates of how compulsory schooling exposure affected various labor market outcomes, with results estimated separately by race. The patterns reveal

striking racial differences in how education translated into economic opportunity. For white workers, direct exposure increased schooling by approximately 0.13 years and wages by 1.5%. The implied return to a year of schooling is 0.11, driven primarily by transitions into white-collar employment. These workers successfully leveraged their additional education to access higher-paying occupations outside of agriculture.

Black workers show a markedly different pattern. While they experienced similar increases in schooling (about 0.10 years), they saw no significant wage gains. Instead, the primary effect was a shift in occupational structure within agriculture - moving out of agricultural labor and into farm operation. This suggests that black workers used education to achieve greater economic independence within the constraints of the segregated labor market, rather than as a path to higher-wage occupations that remained largely closed to them.

Migration patterns further highlight these racial differences. White workers showed substantial geographic mobility, with some evidence of even higher returns for those who left the South. Black workers, however, were actually less likely to migrate after exposure to compulsory schooling. This apparently counterintuitive result aligns with their increased transition to farm operation, which required access to land and capital that was easier to obtain through local connections.

Table A.3 examines how the effects of compulsory schooling varied with other education policy measures, including teacher quality, education spending, and school availability. The results suggest important complementarities between compulsory attendance requirements and broader educational infrastructure. For white workers, the return to schooling was significantly higher in counties with more teachers, higher teacher spending, and more schools, with each additional school associated with a 2.8 percentage point higher wage effect.

For black workers, while the complementarity with teacher-related measures shows weaker wage effects, the interaction with school availability reveals a significant positive relationship. Each additional school is associated with a 0.9 percentage point higher wage effect for black workers. This suggests that the limited wage returns observed for black workers may be partially attributable to inadequate educational infrastructure. When compulsory schooling requirements were accompanied by investments in physical school infrastructure, black workers appeared more able to translate their increased education into modest wage gains, despite the severe occupational constraints of the Jim Crow era.

Together, these reduced-form results reveal a nuanced picture of how compulsory schooling laws shaped economic opportunities in the early twentieth-century South. While both white and black workers increased their educational attainment in response to these laws, only white workers were able to leverage this education into higher wages through occupational mobility. Meanwhile, the seemingly contradictory spillover patterns—lower education but higher wages in neighboring counties—point to important

general equilibrium effects operating through interconnected labor markets. These findings raise fundamental questions about the mechanisms through which education policies affect wages both directly and indirectly. To formalize these mechanisms and quantify their importance, I next turn to a structural model of skill supply and demand that can rationalize these empirical patterns while providing estimates of key parameters governing the returns to education.

## 1.5 A Spatial Model of Skill Supply and Demand

In this section I develop a structural model of the spatial supply and demand of skill that illuminates how education policy influences labor markets through both direct and spillover effects. The spatial supply of skill in the economy is modeled using a discrete choice framework in which workers choose both their schooling level and labor market, following the well-known method first introduced by McFadden et al., 1973. Production then occurs in each market by aggregating the output from each skill group using a constant elasticity of substitution technology. Following Bryan and Morten, 2019, I model the unobserved driver of individual heterogeneity in worker decisions using an extreme-value distribution.

The purpose of the model is twofold. First, it illustrates how the reduced-form effects documented in Section 1.4 can be generated by standard economic forces. Specifically, the direct effect on wages implies the existence of a Mincer return to schooling, while the indirect effects can be rationalized with strong complementarties between skill groups. Second, the model motivates two instruments which can be used to jointly estimate its key parameters. The first is the estimator developed in Section 1.4, and the second is a market-level measure of compulsory schooling exposure. It is similar in spirit to a Bartik shift-share instrument using a migration-weighted average of the compulsory schooling exposure from all cohorts in the economy.

### Production

Consider an economy in which output in each location $d$ is produced by aggregating across the output of each skill-group $s$ in $d$ according to the production technology:

$$Y_d = \left( \sum_s y_{sd}^{\frac{\sigma-1}{\sigma}} \right)^{\frac{\sigma}{\sigma-1}}$$
$$y_{sd} = \tilde{A}_{sd} N_{sd}$$

where $\tilde{A}_{sd}$ is an exogenous labor-productivity parameter and $N_{sd}$ is the effective amount of labor supplied to $sd$, and $\sigma$ is the constant elasticity of substitution between skill-group

outputs. The effective labor supply can be written as:

$$N_{sd} = \sum_{x} N_{sd}$$
$$N_{sd} = \sum_{i \in \mathbb{I}_{xsd}} h_{isd}$$

where $h_{isd}$ is defined as the human capital of worker $i$, which depends on both their $s$ and $d$ decision, and $\mathbb{I}_{xsd}$ is the set of workers from $x$ that made choice $sd$. I can write human capital as coming from a fixed amount of productivity from the skill decision ($\omega_s$) and some unobserved, separable component which is allowed to depend on labor market decision $d$. The effective amount of labor supplied to $sd$ is then the sum of the human capital of workers who choose $sd$, denoted as $\mathbb{I}_{xsd}$.

I assume that firms compete over workers in a perfectly competitive labor market with full information, so that the wage of a worker ($w_i$) is equal to the amount they produce ($y_i$). It then follows that:

$$w_i = y_i = \frac{\partial Y_d}{\partial h_{isd}} \cdot h_{isd} = \underbrace{\tilde{A}_{sd} \left( \frac{Y_d}{N_{sd}} \right)^{\frac{1}{\sigma}}}_{\equiv A_{sd}} \cdot h_{isd} \tag{1.2}$$

where $\tilde{A}_{sd}$ is some exogenous labor demand parameter. The composite parameter $A_{sd}$, which depends on both $\tilde{A}_{sd}$ and endogenous sorting decisions, is the effective productivity a worker who chooses $sd$ will see by virtue of observing wages $w_i$ and human capital $h_{isd}$.

## Worker Utility and Sorting

Workers choose their level of skill $s$ and optimal labor market $d$ to maximize their utility. Utility from decision $sd$ is written as:

$$U_i = \underbrace{w(s,d,x)z_{id}}_{\substack{\text{Wage/} \\ \text{Consumption}}} \times \underbrace{u(s,d,x)z_{is}}_{\substack{\text{Not Wage/} \\ \text{Taste}}}$$

where $w(s,d,x)$ and $u(s,d,x)$ are the parts of wage and taste that vary with exogenous observable characteristic $x$. Though I'll continue to treat $x$ as a generic characteristic below, in practice I will assign $x$ using cohorts defined by age and county of birth. The $z_{id}$ and $z_{is}$ terms are sources of residual individual-level heterogeneity. I discuss the interpretation of these in detail below.

**Wages and Taste Equations**

I assume that wages can be written as,

$$w_i = A_{sd} h_{isd} = A_{sd} \omega_s(x) z_{id}$$

where the term $A_{sd}$ is defined as above. The productive return to $s$ is represented by the term $\omega_s(x)$, which is allowed to vary with $x$. This reflects the idea that the productive return to a worker choosing to acquire more skill may depend on the quality of schooling in their birth county.

Similarly, taste can be written as,

$$u_i = \mu_s(x) \mu_{sd}(x) z_{is}$$

where $\mu_s(x)$ and $\mu_{sd}(x)$ are the cohort-specific utility costs of choosing $s$ and subsequently $sd$. The term $\mu_{sd}(x)$ can be thought of as a skill-specific migration cost to $d$. Similarly, $\mu_s(x)$ can be thought of as the utility cost of choosing $s$ for cohort $x$, which will depend on compulsory schooling exposure. The more exposure, the higher the cost of choosing lower values of $s$.

**Individual Heterogeneity**

To derive closed-form expressions for the share of workers making each choice, I assume that both individual heterogeneity terms are drawn *iid* from a multivariate Frechet distribution,

$$F\left(\mathbf{z}_j\right) = \exp\left\{ - \left[ \sum_{\mathbb{J}} [z_j]^{-\frac{\theta}{1-\rho}} \right]^{1-\rho} \right\}$$

which does not depend on cohort $x_i$. Here, $\tilde{\theta}$ measures the extent of skill dispersion or the importance of comparative advantage. As $\tilde{\theta}$ decreases, there is a greater difference between skills across locations. $\rho$ measures the correlation in skills across locations. As $\rho$ increases, individuals with a high draw in destination $d$ are also likely to have a high draw for destination $d'$. The interpretation is that each different location has a different set of required skills. To the extent that $\tilde{\theta}$ is estimated to be high, locations do not differ greatly in their skill requirements. I allow for correlation between skill draws to allow for general talent, and the case in which talent is unidimensional is a limiting case as $\rho \to 1$. Throughout it is useful to work with $\theta = \tilde{\theta}/(1-\rho)$ rather than $\tilde{\theta}$.

### Sorting into Skill Groups

Workers first make their schooling decision without knowing $z_{id}$ and, hence, the labor market $d^*$ in which they will ultimately decide to work. Instead, they set $s^*$ based on expected utility, which means the worker's choice problem for $s$ can be written as:

$$\begin{aligned}
\max_s \mathbb{E}[U_i(s, d^*, x)|sx] &= \mathbb{E}[w_i(s, d^*, x) \times u_i(s, d^*, x)|sdx] \\
&= \omega_s(x)\mu_s(x)z_{is}\mathbb{E}[A_{sd}\mu_{sd}(x)z_{id}|sx] \\
&= \omega_s(x)\mu_s(x)z_{is}\underbrace{\mathbb{E}[A_{sd}\mu_{sd}(x)|sx]}_{\equiv\alpha_s(x)}\underbrace{\mathbb{E}[z_{id}|sx]}_{=\mathbb{E}[z]} \qquad \text{iid} \\
&= \omega_s(x)\mu_s(x)\alpha_s(x)z_{is}
\end{aligned}$$

where the term $\alpha_s(x)$ can be thought of as the expected productivity cohort $x$ will be able to combine with $h_{isd}$. It is an expectation formed over labor markets, and its uncertainty is due to the unresolved $z_{id}$ draws. Individual heterogeneity averages out due to the independence assumption, but group-level uncertainty remains in $\alpha_s(x)$.

The assumption that $z_{is}$ is Frechet distributed gives that the share of workers from $x$ who choose skill level $s$ will be given by:

$$\pi_s(x) = \frac{\left(\omega_s(x)\mu_s(x)\alpha_s(x)\right)^\theta}{\Phi(x)} \tag{1.3}$$

A full derivation of this expression is included in Appendix Section A.2.

### Sorting into Labor Markets

Once workers learn their $z_{id}$ draws, they choose the labor market that provides them the highest utility conditional on their skill choice $s^*$. This means the worker's labor market choice problem can be written as:

$$\begin{aligned}
\max_d U_i(s^*, d, x) &= w_i(s^*, d, x) \times u_i(s^*, d, x) \\
&= A_{sd}\omega_s(x_i)z_{id} \times \mu_s(x)\mu_{sd}(x_i)z_{is} \\
&= A_{sd}\mu_{sd}(x_i)z_{id}
\end{aligned}$$

Again, the Frechet assumption on $z_{id}$ gives that the share of $x$ with $s^*$ making choice $d$ is given by,

$$\pi_{sd}(x) = \frac{\left(A_{sd}\mu_{sd}(x)\right)^\theta}{\Phi_s(x)}$$

and the expected ability of workers making this choices is given by:

$$\mathbb{E}[z_{id}|xsd] = (\pi_{sd}(x))^{\frac{-1}{\theta}}$$

A full derivation is included in Appendix Section A.2. This means that effective labor supply $N_{sd}$ can be written as,

$$\begin{aligned} N_{sd} = \sum_x N_{sd}(x) &= \sum_x L_{sd}(x)\mathbb{E}[h_{isd}|xsd] \\ &= \sum_x L_{sd}(x)\mathbb{E}[z_{id}|xsd] \\ &= \sum_x L(x)\pi_s(x)\pi_{sd}(x)^{\frac{\theta-1}{\theta}} \end{aligned} \tag{1.4}$$

which allows me to compute $N_{sd}$ from observables for a given value of $\theta$.

## Predicting Reduced-Form Effects

In this section, I show how the model generates the four key reduced-form effects documented in Section 1.4. Recall that exposure to compulsory schooling laws enters the worker sorting problem through cohort utility cost $\mu_s(x)$. The four effects of interest are:

1. Direct effect on own-cohort schooling: $\frac{\partial \pi_s(x)}{\partial \mu(x)}$
2. Direct effect on own-cohort wages: $\frac{\partial wage(x)}{\partial \mu(x)}$
3. Spillover effect on other cohorts' schooling: $\frac{\partial \pi_s(x')}{\partial \mu(x)}$
4. Spillover effect on other cohorts' wages: $\frac{\partial wage(x')}{\partial \mu(x)}$

First consider the direct effect on schooling decisions. Inspection of Equation 1.3 shows:

$$\frac{\partial \pi_s(x)}{\partial \mu_s(x)} = \theta \frac{\pi_s(x)}{\mu_s(x)\alpha_s(x)}[\alpha_s(x) + \mu_s(x)\frac{\partial \alpha_s(x)}{\partial \mu(x)}]$$

This highlights that two channels drive effects on own-cohort schooling decisions. The first is a direct effect as $\mu_s(x)$ appears in the worker's schooling decision. The second effect comes through $\alpha_s(x)$, reflecting that workers make schooling decisions based partially on expectations about productivity.

The direct wage effect operates through changes in the skill composition of labor supply. As cohort $x$ increases its schooling in response to higher $\mu_s(x)$, this affects wages through:

$$\frac{\partial wage(x)}{\partial \mu(x)} = \eta \frac{\partial s_i}{\partial \mu(x)} - \frac{1}{\sigma} \frac{\partial \ln(N_{sd})}{\partial \mu(x)}$$

where $\eta$ captures the Mincer return to schooling and $\frac{1}{\sigma}$ governs how changes in skill-group size affect wages.

The spillover effects on other cohorts arise because workers form expectations with the possibility of migration. If $p_{sd}(x')$ is the probability workers from cohort $x'$ place on working in market $d$, then changes in productivity in market $d$ due to cohort $x$'s compulsory schooling exposure affect cohort $x'$ through:

$$\frac{\partial \alpha_s(x')}{\partial \mu_s(x)} = \sum_d p_{sd}(x') \frac{\partial A_{sd}}{\partial \mu_s(x)}$$

This generates spillover effects on both schooling and wages for cohort $x'$. The schooling spillover depends on how the expected return to education $\alpha_s(x')$ changes. The wage spillover operates through the cross-price elasticities derived in the previous section - as cohort $x$ increases its schooling, this affects wages of cohort $x'$ through:

$$\frac{\partial wage(x')}{\partial \mu(x)} = \frac{\sigma - 1}{\sigma} \frac{\partial \ln(N_H)}{\partial \mu(x)}$$

The model thus predicts that compulsory schooling laws can generate both direct effects through own-cohort responses and indirect spillover effects through the spatial connection of labor markets. The strength of these spillovers depends on migration probabilities $p_{sd}(x')$, which are likely to decline with geographic distance. This provides the theoretical foundation for the spatial identification strategy employed in Section 1.4.

## Estimating Model Parameters

Note that based on Equation 1.2 log wages can be written as,

$$\ln(w_i) = \ln(\tilde{A}_{kd}) - \frac{1}{\sigma} \ln(N_{sd}) + \frac{1}{\sigma} \ln(Y_d) + \ln(\omega_s(x)) + \ln(z_{id})$$

To align with estimating Equation 1.1, I note that the cohort-specific return to schooling $\omega_s(x)$ can be written as,

$$\omega_s(x) = \omega_s \times \omega_o \omega_a \tilde{\omega}_s(x)$$
$$\omega_s = \exp(\eta s_i)$$

where $\omega_o$ and $\omega_a$ are cohort origin and age fixed effects respectively, and $\tilde{\omega}_s(x)$ is a residualized cohort-specific component to a year of schooling. The second equality is equivalent to assuming a standard Mincer return. This gives a wage equation,

$$\ln(w_i) = \eta s_i - \frac{1}{\sigma}\ln(N_{sd}) + \underbrace{\frac{1}{\sigma}\ln(Y_d)}_{d-fe} + \underbrace{\omega_o}_{o-fe} + \underbrace{\omega_a}_{a-fe} + \underbrace{\ln(\tilde{A}_{sd}) + \tilde{\omega}_s(x) + \ln(z_{id})}_{Error} \tag{1.5}$$

which provides a way to estimate the structural parameters $\eta$ and $\sigma$. However, note that both $s_i$ and $N_{sd}$ will depend on the error term through $\tilde{A}_{sd}$ and $\tilde{\omega}_s(x)$, so they cannot be estimated by OLS. Instead, I develop instruments for $s_i$ and $N_{sd}$ using exposure to compulsory schooling.

The first instrument is the direct exposure to compulsory schooling. The identification arguments in Section 1.4 are applicable to the exclusion restriction for this instrument, and the reduced-form analysis shows its relevance. To simplify the estimation I use only the single-difference while controlling for indirect exposure. This is partially justified by the results in Table A.1, which shows pooled estimates for the control cohorts are indistinguishable from 0. Using the difference-in-difference estimator as the instrument provide virtually identical point estimate but with wider standard errors. Future drafts will include corrected standard errors.

The second instrument is a measure of market-level exposure to compulsory schooling using worker decision weights. Recall that effective labor supply $N_{sd}$ can be inferred directly from observables through the relationship,

$$N_{sd} = \sum_x \underbrace{L(x)\pi_{sd}(x)^{\frac{\theta-1}{\theta}}}_{\equiv W(x)} \pi_s(x)$$

which suggests the effect of compulsory schooling exposure of $x$ for $sd$ depends on ex-ante cohort size $L(x)$ and the endogenous decision share $\pi_{sd}(x)$, weighted by the selection in the economy. The parameter $\theta$ has been shown to fall between 2 and 4 in a variety of contexts, with developing contexts tending to give higher estimates.[9] To proceed I assume that $\theta = 3$, though empirical results are robust to assuming a range of values between 2 and 4.

These instruments imply estimating the following first-stage equations,

$$s_i = \delta^s \ln(Z_{sd}) + \sum_g \beta_g^s \cdot Tr_g(x) \cdot post(x) + C_i^s + \gamma_d^s + \gamma_o^s + \gamma_a^s + \epsilon_i^s \tag{1.6}$$

$$\ln(N_{sd}) = \delta^N \ln(Z_{sd}) + \sum_g \beta_g^N \cdot Tr_g(x) \cdot post(x) + C_i^N + \gamma_d^N + \gamma_o^N + \gamma_a^N + \epsilon_{sd}^N \tag{1.7}$$

[9] See Bryan and Morten, 2019, Hsieh et al., 2019, and Hsiao, 2022.

where $Z_{sd} = \sum_x W(x)cse(x)$ is the migration-weighted exposure instrument, $C_i^s$ and $C_i^N$ are effects from observable controls included in Equation 1.1.

Table A.4 reports the results of estimating the parameters of Equation 1.5 via two-stage OLS using Equation 1.6 and Equation 1.7 in the first stage. I estimate parameters both for the full sample and separately by race. For the full sample, I estimate an elasticity of substitution of around 8.5, implying strong complementarities between skill groups. The IV estimate of the Mincer return is .104, though this is imprecisely estimated with a standard error of .076.

Breaking the results down by race reveals important heterogeneity. For white workers, I estimate a Mincer return of 0.147 and an elasticity of substitution of 6.3, both precisely estimated. By contrast, black workers show a negative (though insignificant) return to schooling of -0.046 and a much lower elasticity of substitution of 2.4. These patterns align with the reduced-form results showing that white workers leveraged education into higher-wage occupations while black workers primarily used education to transition from agricultural labor to farm operation.

The first stage results provide strong support for the instruments. For white workers, direct exposure to compulsory schooling increased years of education by 0.124 years, while the migration-weighted exposure measure is strongly predictive of local skill supply with a coefficient of 0.616. The effects are similar in magnitude though opposite in sign for black workers, with direct exposure increasing schooling by 0.136 years but the migration instrument having a negative effect on local skill supply, consistent with increased geographic mobility among more educated black workers. These first stage relationships are all precisely estimated and support the validity of the IV strategy.

These estimates suggest that education policy had complex effects that varied substantially by race in the historical South. While white workers experienced significant returns through both direct effects on their own wages and indirect complementarities with less-educated workers, black workers saw little direct wage benefit despite achieving similar gains in years of schooling. This pattern reflects the severe labor market discrimination of the Jim Crow era, which prevented black workers from accessing many of the higher-wage occupations that education made available to white workers.

## 1.6 Conclusion

This paper provides novel evidence on how education policy affects wages in a developing economy context. By studying the adoption of compulsory schooling laws in the US South from 1900-1940, I am able to separately identify the direct effects on those who increase their schooling as well as the indirect spillover effects on all other workers through labor market complementarities.

The results paint a nuanced picture. While white workers were able to leverage their additional education into higher-wage occupations in the service sector, black workers primarily used their increased schooling to transition from agricultural labor to farm operation. This led to no significant wage gains for black workers despite similar increases in years of schooling.

However, the estimated model parameters suggest education policy can still generate broad-based wage growth even in the presence of labor market frictions. I find a Mincer return to schooling of 0.15 and an elasticity of substitution between skill groups of 6.2, implying strong complementarities. This means that even workers who did not increase their own schooling benefited through higher demand for their labor as the supply of skilled workers grew.

These findings highlight the importance of considering both direct and indirect effects when evaluating the impacts of education policy. While the private return to schooling is a crucial input, the overall effect on wages depends critically on how readily different skill groups can substitute for one another in production. In the historical context of the segregated South, pre-existing labor market discrimination prevented black workers from fully capitalizing on the opportunities created by increased educational attainment.

The spatial model developed in this paper provides a framework for analyzing education policy that accounts for these nuanced general equilibrium forces. It could prove useful for studying other educational contexts and reforms, both historical and contemporary, where both direct and indirect effects shape the ultimate impacts on wages and inequality. More broadly, the results suggest that while education policy has the potential to drive broad-based economic development, policymakers must carefully consider complementary reforms to ensure equitable access to the opportunities created.

# Chapter 2

# When Children Go to School: Family Labor Substitution Effects of Compulsory Education in the American South

## 2.1 Introduction

Are households harmed when policy makers ban children from working? Economic theory does not provide clear answers. Households may respond by reducing consumption; however, if they were using their child's labor to meet subsistence needs, then they may redirecting labor from other members. Which dominates in practice is an empirical question with significant implications for understanding family labor dynamics and the distributional consequences of child labor regulation.

In this paper, I shed novel light on this question by investigating how individual wages change when households are exogenously deprived of child labor due to compulsory schooling laws. Southern legislatures adopted their first compulsory schooling laws between 1900 and 1920, with approximately one-third implemented at the county level and two-thirds at the state level. This staggered adoption providing unique spatial and temporal variation in households' ability to leverage their children as productive assets. By linking households between the 1900 and 1940 censuses, I can observe how labor market outcomes of household members who were too old to be directly affected vary with their household's exposure to these reforms.

My research makes novel contributions to the literature through extensive primary data collection. I hand-collected over 150 original child labor and compulsory schooling laws from Southern state archives, creating a comprehensive dataset that captures

the complex regulatory landscape of the early twentieth century. While the compulsory schooling data is analyzed in detail in Chapter 1 of this dissertation, the child labor legislation provides valuable context for understanding policy effectiveness during this period. Examination of these primary documents reveals that child labor laws were often rendered ineffective due to limited coverage of agricultural work, weak enforcement mechanisms, and numerous exemptions—insights that support my focus on compulsory schooling laws as the more binding constraint on household labor allocation. This archival evidence strengthens the historical context section by documenting how legislators attempted to regulate child labor directly before ultimately achieving greater success through education mandates, which indirectly constrained children's availability for market work.

My identification strategy exploits the spatial and temporal variation in compulsory schooling laws, with approximately one-third implemented at the county level and two-thirds at the state level, while controlling for endogenous family structure choices. I construct a novel measure of household-level exposure to compulsory schooling laws using the observed age of all children within the household in 1910. Conditional on this measure, which reflects the household's fertility decisions, I estimate the effect of household-level compulsory schooling exposure on individual outcomes. This approach addresses potential selection bias by comparing households with similar family structures (and thus similar ex-ante child labor potential) that faced different compulsory schooling requirements due to their geographic location and the timing of law implementation. The key identifying assumption is that, conditional on family structure and county fixed effects, the variation in compulsory schooling requirements is exogenous to unobserved household characteristics that might affect labor market outcomes.

I find that older brothers in households which lost child labor due to schooling laws have lower levels of schooling, are more likely to be engaged in agriculture, and —most importantly—experience significant wage penalties of approximately 0.7 percent for each year of required schooling imposed on their younger siblings. These effects are concentrated in households with low occupation scores, providing compelling evidence that economically vulnerable households redirected older siblings toward immediate labor market participation at the expense of their human capital development and future earnings potential.

These wage effects strongly support the interpretation that households in the historical American South relied on child labor primarily to meet subsistence needs rather than due to preferences or cultural factors. When compulsory schooling laws eliminated this labor source, families compensated by sacrificing older siblings' human capital investment, highlighting important distributional consequences within households that extend beyond the directly affected children.

This paper contributes to a growing literature on the economic motivations behind child labor and intra-household labor allocation. While seminal theoretical work by

Basu and Van, 1998 proposed that child labor primarily stems from subsistence needs rather than parental preferences, empirical evidence on this question remains mixed. My approach builds on Manacorda, 2006 finding that siblings substitute for one another in household labor supply, but extends beyond his analysis of schooling outcomes to examine long-term wage effects—a critical dimension previously unexplored in the historical literature. Unlike Moehling, 1999, who found limited effectiveness of child labor laws in reducing overall child labor rates, I focus on distributional consequences within households by tracking individuals across censuses to measure how compulsory schooling requirements affected siblings' future earnings. This research complements studies on income shocks and child labor (Beegle, Dehejia, and Gatti, 2006; Kruger, 2007), but leverages policy variation instead of economic fluctuations to identify causal effects. By demonstrating that economically vulnerable households redirected older siblings toward immediate labor market participation at the expense of their human capital development and future earnings potential, this work provides novel evidence supporting the subsistence interpretation of child labor advanced in theoretical models by Baland and Robinson, 2000.

The remainder of this paper proceeds as follows. Section 2 provides historical context on child labor in the early twentieth century American South, detailing the prevalence of agricultural child labor and the regulatory landscape that emerged during this period. Section 3 outlines my data sources and identification strategy, explaining how I construct household-level "potential child-labor years" to identify causal effects. Section 4 presents the main results, first examining labor market outcomes across household members and then exploring heterogeneity by household economic status. The paper concludes with a discussion of the implications for understanding household labor allocation decisions and the distributional consequences of child labor regulation.

## 2.2 Context

The early twentieth century American South was characterized by a predominantly rural economy undergoing rapid transformation. Between 1900 and 1930, the region experienced significant economic and social changes as industrialization gradually reshaped its traditionally agrarian structure. This transition provides crucial context for understanding the role of child labor and the impact of regulatory reforms during this period.

Child labor was deeply embedded in the Southern agricultural economy. Between 1900 and 1920, children represented a substantial portion of the agricultural workforce, with an estimated 16% of children classified as working in 1920. This high prevalence of child labor reflected both economic necessity and the nature of Southern agriculture. Cotton cultivation, the backbone of the Southern economy, was particularly conducive to child labor due to its labor-intensive harvesting process that could utilize workers

of various ages and physical capabilities. Children's smaller hands were often considered advantageous for certain agricultural tasks, especially cotton picking, which further incentivized their employment.

Importantly, child labor in the South was overwhelmingly concentrated in agriculture and primarily employed on family farms. Unlike the industrial child labor that dominated reform discussions nationally, Southern child labor often occurred within family units rather than factory settings. This family-based agricultural child labor operated largely outside the scope of early regulatory efforts, as most early child labor laws explicitly focused on manufacturing and other non-agricultural employment sectors.

The regulatory landscape began changing dramatically between 1900 and 1930. The first child labor laws adopted in the early 20th century primarily targeted industrial settings, leaving agricultural child labor largely untouched. However, these laws often proved ineffective for two primary reasons. First, their limited scope excluded the agricultural sector where most child workers were employed. Second, enforcement mechanisms remained weak, particularly in rural areas where oversight was minimal.

More consequential were the compulsory schooling laws that emerged during the same period. While technically separate from child labor legislation, these education reforms were frequently passed alongside or as part of broader reform packages addressing children's welfare. Compulsory schooling laws provided a more effective mechanism for limiting child labor by mandating school attendance during potential working hours. Between 1900 and 1920, Southern legislatures implemented an unprecedented county-by-county rollout of these laws, creating substantial variation in children's labor market availability across otherwise similar communities. The impact of these dual regulatory approaches became evident in labor participation statistics. Between 1920 and 1940, the share of children classified as working fell dramatically from 16% to just 5%. This decline occurred alongside significant increases in school enrollment and attendance rates across the South, suggesting the interrelated nature of education reform and child labor reduction.

This transitional period in the South provides an ideal context for examining how households responded to constraints on child labor availability. By leveraging the spatial and temporal variation in compulsory schooling laws, we can observe how families reallocated resources and labor when their access to children's economic contributions was restricted through policy interventions.

## 2.3 Data and Identification

### Census Data

I conduct my analysis using the publicity available fullcount census data from 1910 and 1940. These years were chosen due to the timing of compulsory schooling adoption, which largely occurred between 1900 and 1930. To simplify my analysis, I limit the sample to males. I use crosswalks provided by the Census Linking Project to link individuals in the 1940 census to their household in 1900 (Abramitzky et al., 2019).[1]

I use the 1900 census to infer each households ex-ante level of exposure to compulsory schooling laws. To do this, I use information on age and household membership to infer the potential amount of labor the household can expect from their children. Importantly, I do not use any information about the child's *actual* labor market status to make this inference. Instead, I note the number of years each member has remaining between the ages of 8 and 16. I then sum this number across members within households to construct a measure which can be interpreted as the number of "labor-years" to which the household has access. The household can then allocate these "labor-years" between years of schooling and years of work. The measures will be higher for households with more and/or younger children and 0 for households with no children; it is discussed in further detail below.

I use the 1940 fullcount census to observe labor-market outcomes. This census is notable as the earliest census to include information on two outcomes of interest—years of schooling and wages. The schooling question, which asks respondents for the highest grade they completed, provides the first detailed measurement of educational attainment beyond simple literacy status in the census. However, this variable should be considered a somewhat noisy measure of human capital, particularly due to the prevalence of gradeless schools in the South during this period. Enumerators were instructed to record the number of years attended for schools without traditional grade structures, but this introduces additional complexity as school year length varied substantially across space, time, and race. The 1940 census also collected comprehensive wage information, allowing for analysis of the relationship between education and economic outcomes. Together, these variables provide a unique opportunity to examine how compulsory schooling laws affected both human capital accumulation and labor market performance.

### Compulsory Schooling Data

The analysis utilizes a dataset of county-level compulsory schooling laws described in Chapter 1. This comprehensive dataset includes over 100 laws passed from 1900-1940,

[1] This results in a sample of XYZ men for whom I can observe both 1940 labor-market outcomes and the expected amount of child labor in their 1900 household.

with approximately half implemented at the county level rather than statewide. This unprecedented spatial variation in adoption timing provides unique identification opportunities across counties within the same state. The data covers 11 Southern states including Arkansas, Alabama, Mississippi, Louisiana, Texas, South Carolina, North Carolina, Tennessee, Kentucky, and Georgia. For each law, information was recorded on implementation dates, age requirements (typically 8-14 years initially, with most states later raising the upper bound to 16), and exemption conditions. Following standard methods in the literature, exposure to compulsory schooling was calculated for each birth-county by age cohort by determining the number of years a child would have been required to attend school between ages 7-16 based on these laws.

## Household Level Child Labor-Supply

I use both the fullcount census and compulsory schooling data to infer the amount of child-labor years available to the household in 1900. Based on county and age, I assign each individual an amount of compulsory schooling following the methodology discussed in Chapter 1. I then calculate the total amount of child-labor years

$$CLY_h = \underbrace{expected_h}_{\equiv exp_h} - \underbrace{required_{ch}}_{\equiv req_{ch}} \tag{2.1}$$

which makes it clear that my independent outcome of interest, $CLY_h$, has two components. A fully endogenous component $expected_h$, the number of labor-years the household chose through their fertility decisions, and $required_h$, the number of those years the state will ultimately require they invest in schooling. This dependence on county is reflected in the subscript $c$.

Table B.1 includes 3 example households to illustrate this construction. The table illustrates how child-labor years ($CLY_h$) is calculated across three example households. Household 1 has a total of 10 expected labor-years from its two children (ages 14 and 8), with 2 years required for schooling due to local laws, resulting in 8 potential child-labor years. Household 2 has 4 expected labor-years from its 12-year-old child but faces no compulsory schooling requirements, giving them all 4 years as potential child labor. Household 3, with only adult members (39 and 18), has zero potential child-labor years. These examples demonstrate how $CLY_h$ varies based on both family structure decisions and geographic exposure to compulsory schooling laws.

Figure B.1 displays the empirical distributions of my key analytical variables: expected child-labor years ($exp_h$) and required schooling years ($req_{hc}$). The distributions show notable mass at zero (approximately 20% of the sample), representing households without members under age 16 and households in counties with late compulsory schooling adoption. Beyond these zero values, the distribution follows a right-skewed pattern

extending to approximately 16 years, with a pronounced discontinuity thereafter. This discontinuity occurs at the threshold where the requisite number of children to generate additional labor-years increases from two to three.

The dashed, vertical lines show the values for a discrete stratification employed in my regression analysis. Given that each child potentially contributes 8 labor-years (from ages 8-16), these strata correspond to 0, 1-8, 9-16, and 17+ years, which can be interpreted as households with zero, one, two, and three or more child-equivalents of potential labor. This stratification enables us to examine non-linear effects in our estimation framework while maintaining sufficient observation density within each stratum to ensure statistical power.

The differential between the expected and required distributions represents our primary identification variation ($CLY_h$). This variation stems from the interaction between household fertility decisions (which determine $exp_h$) and geographic exposure to compulsory schooling laws (which determine $req_{hc}$), allowing us to isolate the causal effect of child-labor availability to the household on individual economic outcomes.

## Identification Strategy

I identify the causal effect of child labor availability on household outcomes by leveraging variation in compulsory schooling requirements across counties. The key insight is that households with similar family structures (and thus similar expected child labor potential) faced different constraints depending on their county's compulsory schooling laws.

Assume that for each year a household chooses to send their children to work rather than school, the labor-market outcome of individual $i$ is affected according to:

$$\begin{aligned} \text{outcome}_{ihc} &= \beta CLY_h + \Gamma X_i + \epsilon_i \\ &= \beta \text{exp}_h - \beta \text{req}_{hc} + \Gamma X_i + \epsilon_i \end{aligned}$$

where $X_i$ is a vector of controls and $\epsilon_i$ is an unobserved term. The parameter of interest $\beta$ can be identified from the (negative of) coefficients on $\text{req}_{hc}$ assuming that the bias induced by selection into exposure can be controlled for using $\text{exp}_{f(h)}$.

To implement this empirically, I estimate the following regression:

$$\text{outcome}_{ihc} = \beta_1 \text{req}_{hc} + \beta_2 \text{exp}_h + \gamma_c + \lambda X_i + \epsilon_i \tag{2.2}$$

I estimate Equation 2.2 labor market outcomes (wages, education, occupation) for individual $i$ from household $h$ in county $c$; $\text{req}_{hc}$ is the number of child-labor years

required for schooling; $\exp_h$ is the expected child-labor years based on family structure; $\gamma_c$ represents county fixed effects; and $X_i$ is a vector of individual-level controls including age, race, and household characteristics in 1910. The coefficient of interest is $\beta_1$, where $-\beta_1$ provides an estimate of the effect of an additional potential child-labor year on household outcomes.

I also explore potential non-linearities in the relationship between child labor potential and outcomes. To do this, I group households into discrete categories based on their expected child labor-years as shown by the vertical dashed lines in Figure 1: households with zero child labor potential, those with approximately one child's worth (1-8 years), two children's worth (9-16 years), and three or more children's worth (17+ years). This allows me to examine whether the effect of compulsory schooling requirements varies with the household's baseline child labor potential, which may be particularly important if there are threshold effects where households require a minimum level of child labor to meet subsistence needs.

As denoted, the identifying variation in $\text{req}_{hc}$ is generated by the household's 1910 county location $c$. I account for county-level differences by including $c$ fixed effects in $X_i$; however, this variation could be correlated with $\epsilon_i$ if households with different fertility decisions $h$ sorted differentially in response to child-labor laws. The balance analysis presented Figure A.4 of Chapter 1 indicates that the scope for such sorting is minimal in this context, supporting the validity of the identification strategy.

## 2.4 Results

This section examines how households responded to the loss of child labor opportunities due to compulsory schooling laws. I analyze labor market outcomes for household members who were too old to be directly affected by these laws, focusing on brothers and fathers separately. The results suggest that households reallocated labor resources in response to schooling requirements, with differential effects by household member type and socioeconomic status. Table 2 presents the main estimates of how required schooling affected educational attainment, agricultural employment, wage reporting, and earnings across different household members. The findings provide evidence on whether child labor primarily served to meet subsistence needs or reflected other household preferences.

### Effect on Labor Market Outcomes

Table B.2 presents the effects of compulsory schooling requirements on labor market outcomes measured in the 1940 Census. Panel A shows the impact on brothers who

were old enough to avoid direct effects of compulsory schooling laws, while Panel B shows effects on fathers.

Our analysis employs two complementary specifications. The odd-numbered columns present a non-linear specification that categorizes households by the approximate number of children subject to compulsory schooling (one, two, or three+). The even-numbered columns present our preferred specification, which treats child-labor years as a continuous measure, providing greater statistical power and a more straightforward interpretation of the effects.

The results provide evidence that households reallocated labor in response to compulsory schooling laws. For brothers, Column 2 shows that each additional year of required schooling for younger household members reduces older brothers' educational attainment by 0.034 years ($p<0.01$). This represents a meaningful effect given that the average years of schooling for this cohort was approximately 8 years. Consistent with reduced educational investment, Column 6 reveals that brothers from households with more compulsory schooling exposure were more likely to report having no wage (coefficient of 0.001, $p<0.05$), suggesting they were more likely to engage in non-wage work such as family farming or sharecropping. Column 8 shows that each additional year of required schooling reduces log wages for brothers by approximately 0.7 percent ($p<0.01$).

In contrast, Panel B shows that fathers experienced generally null labor market effects when their households faced compulsory schooling requirements. While some coefficients in the non-linear specification are statistically significant, our preferred continuous specification in the even-numbered columns shows no significant effects on years of schooling (Column 2) or log wages (Column 8). The only marginally significant effect is on wage reporting in Column 6 (coefficient of 0.002, $p<0.01$), but this effect is small in magnitude and does not translate to meaningful wage differences. These null effects for fathers suggest that households did not adjust paternal labor supply in response to compulsory schooling laws.

The expected child labor coefficients provide additional insights into household dynamics. Households with more expected child labor years (i.e., larger families) show consistently lower educational attainment for both brothers and fathers, and higher rates of agricultural employment. In the continuous specification (Column 2), each additional expected year of child labor is associated with 0.020 fewer years of schooling for brothers ($p<0.1$) and 0.040 fewer years for fathers ($p<0.1$). These patterns are consistent with selection into larger families based on preferences for child labor over education, particularly in agricultural settings.

Taken together, these results suggest that households responded to compulsory schooling laws by reallocating labor from older siblings rather than fathers. The negative effects on brothers' human capital and subsequent labor market outcomes imply that child labor served an important economic function in these households. The null effects on fathers, coupled with significant negative effects on brothers, suggest that

households protected fathers' labor market outcomes while sacrificing older siblings' human capital development. This pattern is consistent with households operating near subsistence levels, where the lost income from child labor necessitated compensatory adjustments in labor allocation among older siblings who were not directly affected by the laws.

## Heterogeneity by Household Economic Status

Figure B.3 presents heterogeneous effects of compulsory schooling requirements on brothers' outcomes by household head occupational status. This analysis provides critical insights into which households were most affected by the loss of child labor and supports the interpretation that child labor primarily served to meet subsistence needs rather than reflecting parental preferences.

The results in Table B.3 reveal important patterns of inequality in how families responded to compulsory schooling laws. For brothers (Panel A), the negative effect of required schooling on educational attainment was concentrated among economically vulnerable families. The coefficient of -0.087 years per year of required schooling for younger siblings represents the effect at the lowest occupational scores. However, the positive interaction term (0.320) indicates that this negative effect diminishes substantially as the household head's occupational score increases. This suggests that wealthier families could afford to maintain older siblings' education even when younger siblings were required to attend school, while poorer families could not.

Similarly, the agricultural employment results show that brothers from lower-status households were 0.4 percentage points more likely to work in agriculture for each year of compulsory schooling imposed on younger siblings. This effect reverses for higher-status families, as indicated by the significant negative interaction term (-0.012). The wage effects show a similar though less precisely estimated pattern, with negative wage impacts for brothers in lower-status households that diminish as occupational status increases.

The results for fathers (Panel B) tell a different story. Compulsory schooling laws had no significant effect on fathers' educational attainment regardless of occupational status. However, the agricultural employment effects mirror those for brothers, with lower-status fathers more likely to remain in agriculture when their children faced compulsory schooling requirements. Most strikingly, higher-status fathers experienced significant negative wage effects (-0.063 percentage points per occupational score point) relative to lower-status fathers when their children were subject to compulsory schooling.

These findings provide compelling evidence that the burden of adjustment to compulsory schooling laws fell most heavily on economically vulnerable households. While wealthier families could protect both fathers' wages and brothers' education when younger siblings were required to attend school, poorer families faced difficult tradeoffs that re-

sulted in sacrificed human capital development for older siblings and increased agricultural employment for both brothers and fathers.

## 2.5 Conclusion

This study has provided compelling evidence that compulsory schooling laws in the early twentieth-century American South generated significant labor substitution effects within households. By examining how labor market outcomes varied with household exposure to these laws, I have demonstrated that families responded to the loss of child labor by redirecting older siblings toward immediate productive work at the expense of their human capital development and future earnings potential.

The results reveal that older brothers in households affected by compulsory schooling requirements experienced decreased educational attainment, increased likelihood of agricultural employment, and wage penalties of approximately 0.7 percent for each year of required schooling imposed on their younger siblings. These effects were particularly pronounced in economically vulnerable households with low occupational scores, suggesting that child labor primarily served subsistence needs rather than reflecting cultural preferences or attitudes toward education.

These findings contribute to our understanding of how households allocate labor resources under constraint and highlight important distributional consequences of education policy that extend beyond directly affected children. While compulsory schooling laws undoubtedly benefited the younger children who gained additional education, they imposed significant costs on older siblings whose human capital investment was sacrificed to maintain household income. This tradeoff illustrates the difficult choices faced by economically vulnerable families in developing economies when child labor regulations limit their economic options.

The patterns documented in this paper help explain the persistent intergenerational transmission of poverty observed in the historical South and offer insights relevant to contemporary policy design in developing regions. Education policies that do not account for household-level economic incentives may inadvertently reinforce inequality by benefiting some household members at the expense of others. Future policy interventions might consider complementary measures such as income support or targeted subsidies to mitigate these unintended distributional effects and ensure that educational expansion delivers benefits across all household members.

# Chapter 3

# From Collection Plate to Migration Path: Black Religious Networks and the Economic Consequences of the Great Migration

## 3.1 Introduction

It is clear from the historical record that churches were the first social institutions to emerge for black Americans after slavery. While plantation-based worship had been permitted and even encouraged by some slaveholders as a means of social control, independent black religious institutions represented a revolutionary development in post-emancipation society. These autonomous organizations provided spaces for community governance, leadership development, and cultural expression beyond white oversight. The rapid proliferation of black churches across the South during Reconstruction testifies to their foundational importance in newly freed communities, setting the template for subsequent organizational development in African American society.

Religious institutions have historically played a crucial role in providing social services and financial support through charitable giving in the United States. This economic significance is evidenced by the scale of religious charitable activities - in 1926, U.S. churches spent $150 million on supporting their members and local communities, far exceeding the combined social expenditures of $60 million by state and local governments (Gruber and Hungerman, 2007) [1]. The economic importance of religious organizations persists today, with approximately 40% ($152B) of all charitable giving directed to religious organizations (Glaeser and Sacerdote, 2008). Religious institutions serve as

[1] Federal relief spending before New Deal was negligible.

particularly important social insurance mechanisms, with around 75% of congregations maintaining some form of financial support system for the poor (Cnaan, 2002). The historical role of religious institutions as providers of social insurance was especially pronounced in African American communities. As documented by Baradaran, 2018, by the end of the 19th century, virtually every black church had established mechanisms to insure their congregation against various hardships including sickness, death, and bankruptcy.

Despite this prevalence, we know very little about how religious institutions shaped the largest spatial reallocation of labor in US history—the Great Migration. In this paper, we match data from the Census of Religious Bodies, conducted every ten years from 1906 to 1936, with 1900-1940 fullcount census records to study how religious networks influenced migration during this watershed event. This novel data linkage enables us to examine the relationship between denominational affiliations and migration flows at an unprecedented level of detail. We analyze the distribution of three major black denominations—the Baptist National Convention, the African Methodist Episcopal Church, and the African Methodist Episcopal Zion Church—across counties and assess how similarity in religious composition between origin and destination locations affected migration decisions. This approach builds on work by Stuart and Taylor, 2021, who document persistent migration corridors between specific Southern origins and Northern destinations, by identifying a specific institutional mechanism—religious networks—that helped establish and maintain these corridors.

We find evidence that counties with more religiously similar populations experienced higher bilateral migration flows throughout the period. Our empirical analysis demonstrates that a 10% increase in religious similarity between origin and destination counties is associated with a 0.55% increase in migration flows, controlling for geographic distance and economic factors. Importantly, this pattern holds specifically for black religious networks and black migration, with no effect observed for white religious similarity and black migration flows. This racial specificity provides compelling evidence that our findings reflect the causal influence of religious networks on migration decisions.

Furthermore, we provide evidence that churches primarily facilitated migration by reducing non-economic costs such as information barriers and psychological adjustment challenges. Our analysis reveals that religious networks influenced migration decisions not by providing information about better economic opportunities, but rather by mitigating the social and psychological costs of relocation. When accounting for selection patterns in migration flows, we find that religious similarity had little direct effect on occupational outcomes or wages for Black migrants, suggesting these networks operated primarily through non-wage channels. This finding contrasts with patterns observed for white migrants, whose religious networks appear to have influenced both migration decisions and subsequent economic outcomes, highlighting the distinct role that Black

churches played in facilitating mobility during this transformative period.

We then motivate a shift-share instrument constructed from the religious network data to estimate the effect of the re-allocation of labor during the Great Migration on Southern wages in 1940 using a spatial model of labor supply and demand. Building on our finding that religious networks influenced migration patterns primarily through non-wage channels, we develop an instrumental variable approach that leverages this exogenous variation to identify the causal impact of black out-migration on local labor markets. Our methodology addresses the fundamental challenge of endogeneity in migration research. By focusing on religious network structures that developed independently of 1940 labor markets, we isolate variation in migration flows that is not driven by the economic conditions we aim to study.

Using our religious networks as an instrument for migration flows, we find that black out-migration significantly impacted Southern labor markets through general equilibrium effects. Our instrumental variable estimates reveal that white workers experienced substantial wage benefits from black out-migration, with a 1% decrease in black labor supply associated with a 0.88% increase in white wages. These gains primarily operated through occupational upgrading, as white workers transitioned from agricultural to manufacturing and service sector employment when black laborers departed. Conversely, black workers who remained in the South experienced modestly negative wage effects from black migrant inflows, with our estimates showing a -0.185% decrease in wages for each 1% increase in black migrant labor, though this effect diminishes when controlling for selection. This asymmetric distribution of economic gains and losses from the Great Migration highlights how this demographic transformation, while enabling mobility for migrants themselves, simultaneously reinforced existing racial inequalities within Southern labor markets despite reducing overall labor competition.

Our research contributes to several strands of literature. First, we add to studies on migration networks and social capital by quantifying religious similarity's role in location decisions during the Great Migration. While previous work has identified the importance of kinship ties (Carrington, Detragiache, and Vishwanath, 1996), information networks (Wiiliam J Collins, 1997), and labor market connections (Boustan, 2009) in shaping migration patterns, we are the first to systematically measure how religious networks influenced these decisions. This finding resonates with the work in Chen, 2010 on religious institutions as mechanisms for risk-sharing and social support, though our focus on migration represents a novel application of these insights. Our results also align with Chay and Munshi, 2015 emphasis on community networks in facilitating collective migration, providing specific evidence on how religious institutions contributed to these broader network effects.

Second, we contribute to the literature on religion and economic development. Scholars such as Barro and McCleary, 2003,Chaney, 2013, and Rubin, 2017 have explored how religious institutions shape economic outcomes in various contexts. More recently,

Petach, 2024 has documented the economic effects of religious adherence patterns on local economic development, while Ferrara and Testa, 2023 have analyzed how religious organizations influence social capital formation. Our work extends this literature by embedding religious networks into the analysis of a major demographic and economic transition, providing evidence on specific mechanisms through which religious institutions influence economic behavior.

Finally, we contribute to the literature on the economic consequences of the Great Migration. In addition to the aforementioned work by Derenoncourt, 2022, Mandle, 1978, and Wright et al., 1986, recent contributions by Bazzi et al., 2023 have examined how migration affected the development of cultural identities and social norms. These findings extend work by Mandle, 1978 and Wright et al., 1986, who analyzed how Southern agricultural labor markets adjusted to black out-migration, by providing causal estimates of the distributional consequences of these adjustments. Our results align with Hornbeck and Naidu, 2014, who found that black out-migration led to increased agricultural wages and mechanization in the South, though our approach uniquely identifies the role of religious networks in driving these migration flows. Our results also complement Collins and Wanamaker's (2014) analysis of selection patterns in migration and Tolnay and Beck, 1992 work on racial violence and out-migration patterns by identifying a specific institutional mechanism—religious networks—that influenced migration decisions beyond individual characteristics.

The remainder of the paper is organized as follows. Section 3.1 provides historical context on the Great Migration and the role of black churches in African American communities. Section 3.2 describes our data sources and the construction of key variables. Section 3.4 presents our empirical analysis of religious networks and migration flows. Section 3.5 estimates the impact of the Great Migration on Southern wages. Section 3.6 concludes with a discussion of implications for understanding the economic role of religious institutions and directions for future research.

## 3.2 Context

The first wave of the Great Migration (1915-1930) marked the beginning of one of the largest internal migrations in U.S. history. While popular accounts often focus exclusively on South-to-North movement, it is crucial to recognize that the majority of black migration during this period occurred within the South itself, primarily from rural to urban areas. This internal Southern migration reflected the urbanization trends reshaping the entire country while maintaining proximity to family networks and familiar cultural institutions. The approximately 1.5 million African Americans who did move to northern states during this initial wave represented only a portion of the overall mobility occurring at this time (Derenoncourt, 2022). This early migration took place in a dis-

tinct institutional context that shaped both the pattern of movement and its economic implications.

## Economic and Social Landscape

The post-Civil War South was characterized by profound economic transformation and social stratification that directly influenced migration patterns. Following the collapse of the plantation economy, a new agricultural system emerged based primarily on tenancy and sharecropping. By 1900, approximately 70% of black farmers in the South were tenant farmers or sharecroppers rather than landowners (Ransom and Sutch, 2001). This system created persistent economic insecurity, as tenants typically received only one-third to one-half of the crops they produced, leaving many in perpetual debt to landlords and merchants through exploitative credit arrangements known as "crop liens."

The financial infrastructure of the South remained severely underdeveloped compared to the North. As Jaremski and Fishback, 2018 document, there were virtually no National banks and very few State banks operating in predominantly black areas due to restrictive regulations and deliberate exclusion. Local white merchants controlled the private banks that did exist, establishing a monopolistic credit system that charged exorbitant interest rates, sometimes exceeding 40% annually (Baradaran, 2018). This financial exclusion severely limited economic mobility and capital accumulation for black Southerners, making geographic mobility an increasingly attractive option despite its costs.

Agricultural crises further destabilized rural livelihoods during this period. The devastation caused by the boll weevil infestation, which reached the Mississippi Delta by 1915 and spread throughout the Cotton Belt, destroyed cotton crops and eliminated agricultural jobs. Lange, Olmstead, and Rhode, 2009 estimate that the boll weevil reduced cotton production by approximately 50% in affected counties, creating economic shocks that drove rural-to-urban migration. Simultaneously, agricultural prices collapsed following World War I, plummeting by nearly 40% between 1919 and 1921, further undermining the economic viability of small-scale farming.

White landowners exhibited significant resistance to black out-migration, particularly concerned that migrant black workers would be unsuitable for agricultural labor compared to the local black workforce they had long relied upon. This resistance was grounded in assumptions that migrants lacked the local social-capital necessary to integrate into agricultural markets at this time (Wright et al., 1986). Many landowners actively worked to impede migration through various means, including leveraging debt obligations, restricting information about outside opportunities, and in some cases, direct intimidation. This tension between landowner interests in retaining local black labor and the economic incentives pushing workers to migrate created a complex dynamic in

Southern agricultural communities that influenced both migration patterns and subsequent labor market outcomes.

Industrial development in Southern cities created new economic opportunities that attracted rural migrants. Cities like Birmingham, Atlanta, Memphis, and New Orleans developed manufacturing sectors that offered alternatives to agricultural labor, though often in unskilled positions with lower wages than comparable Northern jobs. Between 1900 and 1930, the urban black population in the South increased by 127%, compared to a 49% increase in the rural black population, highlighting the significant rural-to-urban movement within the region (U.S. Census Bureau, 1940).

The timing of this migration coincided with significant labor market disruptions. World War I reduced European immigration while simultaneously increasing demand for industrial production, creating labor shortages in Northern factories. Many industries actively recruited Southern black workers, with some companies sending labor agents to the South and offering transportation allowances for migrants. The Pennsylvania Railroad alone recruited over 12,000 black workers from the South between 1916 and 1921 (Grossman, 1989). This external demand for labor intersected with internal migration patterns already established within the South.

The social landscape was equally influential in shaping migration decisions. The Jim Crow system of racial segregation and discrimination reached its zenith during this period, with the systematic disenfranchisement of black voters virtually complete by 1910. Between 1900 and 1930, more than 2,000 black Southerners were lynched, creating an atmosphere of terror that motivated some to leave (Tolnay and Beck, 1992). However, racial violence alone cannot explain migration patterns, as the relationship between lynching rates and out-migration was complex and varied by region.

The demographic profile of migrants reveals additional dimensions of the migration process. Those who moved tended to be younger, more educated, and more likely to have non-agricultural occupational skills than those who remained in rural areas (William J Collins and M. H. Wanamaker, 2014). This pattern of positive selection, particularly for longer-distance migration to the North, suggests that those with greater human capital were better positioned to overcome the substantial costs and risks associated with migration. However, this selectivity was less pronounced for short-distance migration within the South, which was more accessible to those with fewer resources and skills.

This complex economic and social landscape created varied incentives for migration that were mediated by institutional structures, including religious networks, that could facilitate or constrain movement. Understanding these contextual factors is essential for interpreting the migration patterns observed during this transformative period in American history.

## Black Churches as Economic and Social Insitutions

Black churches emerged as foundational institutions in African American communities following emancipation, serving as the first autonomous social organizations controlled entirely by and for the formerly enslaved population. While some black congregations predated the Civil War, particularly in Northern cities and among free blacks in the South, it was during Reconstruction that these institutions experienced explosive growth and institutional development.

The historical origins of independent black denominations reflect both resistance to racial discrimination and theological adaptation. The African Methodist Episcopal Church (AME), founded in Philadelphia in 1816 by Richard Allen, originated after black worshippers at St. George's Methodist Episcopal Church were forced to move from the main floor to a segregated gallery. This act of discrimination prompted Allen and others to establish a separate congregation that would later grow into the first fully independent black denomination in America. The African Methodist Episcopal Zion Church (AMEZ), similarly founded in New York in 1821, emerged from comparable experiences of discrimination within white Methodist congregations.

Baptist churches followed a different organizational pattern. Rather than forming a centralized denomination initially, independent black Baptist congregations proliferated across the South due to the denomination's less hierarchical structure. These diverse congregations eventually consolidated into the National Baptist Convention in 1895, which rapidly became the largest black religious organization in America. Unlike the Methodist denominations with their Northern urban origins, the Baptist convention emerged primarily from Southern congregations and reflected more rural theological and organizational traditions.

The post-Civil War expansion of these denominations was remarkable in scale and speed. The AME Church grew from approximately 20,000 members before the Civil War to 400,000 by 1884. The AMEZ Church experienced even more dramatic proportional growth, expanding from 4,600 to approximately 300,000 members during the same period (Lincoln and Mamiya, 1990). This growth was facilitated by missionaries, many of whom had been educated in Northern institutions or served as chaplains in the Union Army, who traveled throughout the South establishing new congregations.

By the early 20th century, these institutions had developed sophisticated organizational structures that extended well beyond spiritual functions. Churches became comprehensive community centers that addressed educational, social, and economic needs. In the absence of public education systems serving black students, many churches established schools that provided both basic literacy and vocational training. Church basements hosted political meetings, cultural performances, and community celebrations, creating spaces for civil society development in environments otherwise hostile to black public assembly.

The economic functions of black churches were particularly significant during the Great Migration era. As documented by Baradaran, 2018, by the end of the 19th century, virtually every black church had established mechanisms to insure their congregation against various hardships including sickness, death, and bankruptcy. These mutual aid societies, burial associations, and informal lending networks represented the primary financial infrastructure available to most black communities, especially in rural areas. Church-based economic institutions filled the void created by systematic exclusion from mainstream banking and insurance systems, providing both emergency assistance and pathways for modest capital accumulation.

The scale of religious charitable activities was substantial. In 1926, U.S. churches collectively spent $150 million supporting their members and local communities, far exceeding the combined social expenditures of $60 million by state and local governments (Gruber and Hungerman, 2007). Approximately 75% of congregations maintained some form of financial support system for the poor (Cnaan, 2002). Within black communities, these religious welfare systems were often the only institutional safety net available, making them crucial resources for families considering migration, whether to nearby cities or distant Northern destinations.

Churches also served as information hubs that facilitated migration decision-making. Ministers and church leaders often maintained correspondence with former congregation members who had migrated, collecting information about employment opportunities, housing conditions, and the social environment in destination areas. These letters were frequently read aloud during church services or discussed in church meetings, providing reliable firsthand accounts that helped potential migrants evaluate their options. Some congregations formalized this process by establishing "migration clubs" that pooled resources and knowledge to assist members in relocating.

The density of church networks varied significantly by region and denomination, creating differential access to migration pathways. Urban congregations typically maintained more extensive connections across geographic regions than their rural counterparts. Denominations with stronger Northern institutional presences, particularly the AME and AMEZ churches with their historical roots in Northern cities, provided natural bridges for South-to-North migration. Meanwhile, Baptist congregations, with their more localized autonomy and stronger Southern concentration, often facilitated rural-to-urban movement within the South.

These religious institutions maintained continuity during migration by establishing or strengthening congregations in destination cities. Migrants frequently sought out churches affiliated with their home denomination upon arrival, creating community anchors that eased the transition to urban environments. These transplanted religious communities provided not only spiritual continuity but also practical assistance with housing, employment, and social integration.

The economic significance of these religious networks during the Great Migration

period cannot be overstated. In an era when formal financial institutions systematically excluded black customers and public welfare systems were minimal, churches provided essential economic infrastructure that made mobility possible for many who would otherwise have lacked the resources to relocate. By examining the relationship between religious networks and migration patterns, we gain insight into how these foundational institutions shaped one of the most significant demographic transformations in American history.

### Migration Patterns and Networks

The geographic pattern of church networks during this period provides a unique opportunity for empirical research. The predominant rural-to-urban migration within the South was often facilitated by religious connections that provided information about housing, employment, and community support in destination cities. For those who did move North, settlement patterns frequently aligned with previously established migration channels connected to their home communities (Boustan, 2010, Black et al., 2015, Stuart and Taylor, 2021).

Religious networks served multiple functions throughout the migration process: information sharing about employment and housing opportunities in destination cities; direct financial assistance for migration costs; and social support systems for new arrivals. These functions were particularly vital for intra-South migrants, who while traveling shorter distances, still faced significant challenges in establishing themselves in new urban environments with limited resources.

Distinct migration corridors emerged during this period, both within the South and extending to Northern destinations. Within the South, state capitals and industrial centers attracted migrants from their surrounding rural areas. For those moving North, the eastern seaboard corridor connected the Southeast to northeastern cities, Midwestern destinations drew heavily from Mississippi Valley origins, and western destinations attracted migrants primarily from Texas and other southwestern states.

This early period of migration provides an ideal setting for examining how religious institutions influenced movement patterns. The institutional structure of black churches during this period, with clear denominational affiliations and geographic networks spanning both rural and urban areas, allows us to analyze how these networks facilitated both short-distance and long-distance migration and shaped subsequent economic outcomes.

## 3.3 Data

In this study, we employ two complementary data sources to examine the relationship between religious networks and migration patterns during the Great Migration, as well

as the subsequent impacts on labor markets. Our analysis spans the period from 1900 to 1940, capturing the first wave of this significant demographic shift that saw approximately 1.5 million black southerners relocate to urban areas throughout the US.

## Census of Religious Bodies

A key component of our analysis is data from the Census of Religious Bodies, which the U.S. Census Bureau conducted every ten years from 1906 to 1936. These surveys provide detailed county-level information on religious adherence across denominations throughout the United States. The Census Bureau collected this data through questionnaires sent to church officials of all known religious organizations, requesting information on membership, facilities, and finances. While there are some concerns about potential reporting errors, this dataset remains the most comprehensive source of information on religious adherence in the United States during the early 20th century.

To measure black religious networks, we focus on the three largest black denominations which capture over 95% of black worshipers:

- **Baptist National Convention (BNC)**: The largest black religious body in America with substantial presence in both the South and West. Its decentralized governance structure allowed for significant local autonomy, which facilitated adaptation to different regional contexts. The BNC had particularly strong representation in rural areas and developed growing influence in Southern urban centers.

- **African Methodist Episcopal Church (AME)**: Founded in Philadelphia in 1816 as the first independent black denomination in the United States, it grew from 20,000 members before the Civil War to approximately 400,000 by 1884. The AME Church established urban congregations across the South while expanding along northeastern and midwestern corridors, creating natural pathways for South-to-North migration.

- **African Methodist Episcopal Zion Church (AMEZ)**: Founded in New York in 1821, it expanded from 4,600 to about 300,000 members after the Civil War. The AMEZ Church remained more concentrated in southeastern states and certain northern urban centers, with relatively limited western presence.

For white religious networks, we use data from the five most prevalent white denominations: Catholic, Baptist, Methodist, Episcopal, and Lutheran. This allows us to conduct placebo tests examining whether our observed effects are specific to black religious networks or reflect broader patterns of denominational similarity that would affect all racial groups equally.

## Full-Count Census Data

Our second major data source is the U.S. Census full-count data for the years 1900, 1910, 1920, 1930, and 1940. These complete population records, recently digitized and made available for research, provide comprehensive demographic information for all individuals enumerated in each census year. Unlike the public-use samples that have been available for decades, these full-count data allow us to observe the entire population, which is particularly valuable for studying relatively rare events like long-distance migration and for conducting analysis at granular geographic levels.

We link individuals across consecutive censuses (e.g., 1910 to 1920, 1920 to 1930, and 1930 to 1940) to create a panel that captures migration decisions over time. This linkage relies on matching individuals based on name, age, birthplace, and other invariant characteristics, following established procedures in the historical economics literature (Abramitzky et al., 2019). The resulting linked dataset provides an unprecedented view of individual mobility during this critical period. The linkage is crucial for our analysis, as it allows us to observe both origin and destination locations for each migrant, as well as to calculate accurate migration rates for different county pairs.

We identify migrants as individuals who changed their county of residence between consecutive census periods. This definition captures both short-distance moves within states and long-distance migration across regions. By distinguishing between different types of moves (within-South, South-to-North, South-to-Midwest, South-to-West), we can examine how religious networks operated differentially across various migration corridors.

The 1940 census is particularly valuable for our analysis because it was the first to collect comprehensive information on both educational attainment and wages. This allows us to examine labor market outcomes for migrants and non-migrants, including detailed occupational categories that we classify into agricultural labor, farm ownership, manufacturing employment (blue-collar and white-collar), and service sector employment (blue-collar and white-collar).

From the census data, we also extract information on occupational choices and wages in 1940, which serve as key outcome variables in our analysis. In addition to that, we extract control variables that may influence migration patterns and economic outcomes, including agricultural and manufacturing employment shares by county, European immigration share, cotton acreage, and New Deal spending from 1933-1940. These variables help us account for local economic conditions that might affect both religious composition and migration decisions.

## Religious Similarity Index

Using the Census of Religious Bodies data, we construct a religious similarity index between any two counties $o$ and $d$, defined as:

$$relsim_{od} = \sum_{r=1}^{R} s_{ro} \cdot s_{rd} \tag{3.1}$$

where $s_{ro}$ represents the share of the population in origin county $o$ belonging to denomination $r$, and $s_{rd}$ represents the corresponding share in destination county $d$. $R$ is the total number of denominations included in the calculation.

This index can be interpreted as the probability that a randomly selected person from county $o$ and a randomly selected person from county $d$ would belong to the same denomination. For black religious networks, we calculate this measure using the three major black denominations discussed above. For white religious networks, we use the five most prevalent white denominations.

This approach to measuring religious similarity has several advantages. First, it captures the entire distribution of denominations rather than focusing only on the dominant religion in each county. Second, it allows for continuous variation in similarity rather than imposing binary classifications. Third, by computing the index separately for black and white denominations, we can conduct racial placebo tests that strengthen our causal identification.

By measuring religious similarity between counties prior to major migration waves (using religious census data from year $y - 14$ for migration flows in year $y$), we avoid potential endogeneity concerns that could arise if network formation was itself a response to migration patterns. For example, when analyzing migration flows in 1930, we use the religious composition from 1916, ensuring that our measure of religious networks predates the migration decisions being studied.

## Geographic Classification

We classify counties into migration regions based on their initial 1900 black population and geography. Figure C.1 presents the distribution of the black population in the United States at two critical points in time. Panel A displays the black population distribution in 1900, showing a heavy concentration in the Southeast, particularly in counties along the Mississippi River and throughout the Black Belt region. This map clearly reveals that black settlement was predominantly in the eastern half of Texas, with minimal presence in the western portion of the state. Similarly, only the eastern sections of Oklahoma had significant black populations. This initial distribution serves as the foundation for understanding the origins of the Great Migration.

Panel B of Figure C.1 illustrates the black population distribution in 1940, after several decades of migration. While the majority of the black population remained concentrated in the South, significant growth is visible in Northern and Western urban centers. This comparison highlights the substantial demographic shifts that occurred during the first wave of the Great Migration, though not as dramatic as the changes that would follow in the post-WWII period.

Figure C.2 presents our regional classification scheme that guides the analysis of migration patterns. The map divides the United States into four distinct regions: South (blue), North (green), Midwest (red), and West (yellow). Importantly, this classification recognizes the historical population distribution by including only the eastern portions of Texas and Oklahoma within the South region. Treating the western halves of these states as origins for the Great Migration would be historically inaccurate, as these areas had minimal black populations from which migration could occur.

These regional classifications provide an essential framework for our empirical analysis of migration patterns during the Great Migration. By accurately delineating the boundaries of the South, we establish historically precise parameters for studying both outward migration flows and their economic impacts. Furthermore, these regional definitions serve as bounds when we analyze the effects of black out-migration on 1940 Southern wages, ensuring that our examination of labor market dynamics focuses on the areas genuinely affected by these demographic shifts. This methodologically sound geographic classification enables us to trace both the determinants of migration decisions and their subsequent economic consequences with greater precision.

## Migration Flows and Labor Supply Measures

Using the linked census data, we construct bilateral migration flows between all county pairs for each decade between 1900 and 1940. For each origin-destination pair, we calculate $\pi_{ody}$, which represents the share of individuals observed in origin county $o$ in census year $y - 10$ who had migrated to destination county $d$ by year $y$.

These migration shares form the dependent variable in our core empirical specifications examining the relationship between religious networks and migration patterns. By focusing on shares rather than absolute numbers, we account for differences in population size across counties and isolate the relative attractiveness of different destinations for migrants from each origin.

For our analysis of labor market impacts, we construct two key measures of labor supply for each county $d$ in 1940:

- Local labor supply ($L_d$): The number of workers born in location $d$ who remained there

- Migrant labor supply ($M_d$): The number of workers born elsewhere who migrated to $d$

## 3.4 Religious Similarity and Migration Flows

In this section, we analyze how religious similarity between counties affected migration flows during the Great Migration. We then examine whether religious networks primarily operated through wage or non-wage channels to facilitate migration. The results motivate their use as an instrument in Section 3.5, in which we estimate the effect of the Great Migration on 1940 Southern wages.

### Religious Similarity and Migration Flows

To estimate the relationship between religious networks and migration decisions, we employ the following specification:

$$\ln \pi_{ody} = \beta \ln \text{relsim}_{ody} + \delta \ln \text{dist}_{od} + \mu_{oy} + \mu_{dy} + \ln \varepsilon_{ody} \tag{3.2}$$

where $\pi_{ody}$ is the share of workers observed in origin $o$ in census year $y-10$ who had migrated to destination $d$ by year $y$. This migration flow is constructed by linking consecutive census data for individuals observed in both periods. The error term $\epsilon_{ody}$ is assumed to be Poisson distributed, which is standard for modeling outcomes with many 0s such as migration flows. The key explanatory variable is $\text{relsim}_{ody}$, a religious-similarity index between counties $o$ and $d$ measured using Census of Religious Bodies data from year $y-14$ (religious censuses were conducted in 1906, 1916, 1926, and 1936). By using pre-migration religious network data, we avoid mechanical correlations that could arise if networks were measured during the migration period itself. We control for migration costs using distance between counties ($\text{dist}_{od}$) and include year-origin fixed effects ($\mu_{oy}$) and year-destination fixed effects ($\mu_{dy}$).

We analyze census periods $y \in \{1920, 1930, 1940\}$, constructing two separate religious similarity indices: one based on the three major black denominations (Baptist National Convention, African Methodist Episcopal, and African Methodist Episcopal Zion) and another based on the five most prevalent white denominations (Catholic, Baptist, Methodist, Episcopal, and Lutheran).

Table C.1 presents results from estimating this specification for different migration flow samples. Column (1) shows migration flows within the South, column (2) shows flows from South to North, column (3) shows flows from South to Midwest, and column (4) pools all South-to-destination flows.

Two key patterns emerge from these results. First, religious similarity based on Black denominations significantly predicts Black migration flows. In the pooled specification

(column 4), a 10% increase in religious similarity is associated with a 0.19% increase in the migration flow. This effect is approximately 2.6% (0.019/0.726) as large as the effect of a 10% increase in distance, indicating that religious networks played a meaningful though modest role in shaping migration decisions.

Second, we observe clear racial specificity in these effects. Black religious similarity significantly predicts within-South migration (column 1) and migration to the Midwest (column 3), with somewhat weaker effects for migration to the North (column 2). Importantly, white religious similarity has no significant predictive power for Black migration flows across any of the specifications. This racial specificity provides compelling evidence that these patterns reflect causal relationships rather than spurious correlations with omitted variables, which would likely affect both racial groups similarly.

The stronger effect for within-South migration suggests that religious networks may have been particularly important for shorter-distance moves, possibly because these networks were denser within regions. The variation in effects across destinations also highlights the differential role of religious institutions in facilitating migration to different regions of the country.

## Religious Similarity and Labor-Market Outcomes

A crucial question for interpreting these results—and for the identification strategy in Section 3.5—is whether religious networks primarily facilitated migration through wage channels (e.g., by providing information about better-paying jobs) or through non-wage channels (e.g., by offering social support and reducing the psychological costs of migration). To address this question, we estimate impacts on individual-level outcomes using the following specification:

$$\overline{\text{outcome}}_{ody} = \beta_1 \text{relsim}_{ody} + \beta_2 \pi_{ody} + \delta_{dy} + \delta_{oy} + \varepsilon_{ody} \tag{3.3}$$

where outcome$yod$ is the average outcome (e.g., agricultural employment share) for workers in census year $y$ who are from $o$ and working in $d$. The coefficient $\beta_1$ captures how outcomes differ when migrants move to destinations with differing levels of religious similarity. Controls include year by origin and destination fixed effects ($\delta yo$ and $\delta_{yd}$), as well as the migration-share $\pi_{yod}$.

The migration share is included to account for the possibility that migrants may be selected on some unobserved measure correlated with the outcome. If this is true, then any variable that manipulates the cost of migration will appear to be correlated with outcomes in the reduced-form regardless of the channel through which it operates. A simple discrete choice model of migration motivates migration shares as a sufficient statistic for unobserved selection of this sort. We outline such a model in Section 5 below.

Table C.2 presents the results from this analysis, examining how religious similarity affects agricultural employment, manufacturing employment, service sector employment, and wages. For Black workers, religious similarity is positively associated with agricultural employment and negatively associated with manufacturing employment and wages when we do not control for migration shares (columns 1, 3, and 7). However, these associations largely disappear when migration shares are included as controls (columns 2, 4, and 8). This pattern suggests that religious networks primarily facilitated Black migration through non-wage channels such as social support and community integration, rather than by providing information about better economic opportunities.

In contrast, for white workers, religious similarity remains significantly associated with occupational outcomes and wages even after controlling for migration shares. White religious similarity is positively associated with agricultural employment (columns 1-2), negatively associated with manufacturing and service employment (columns 3-6), and negatively associated with wages (columns 7-8). These persistent effects suggest that white religious networks operated at least partially through wage channels, possibly by facilitating entry into specific occupational niches.

The stark difference in how religious networks operated for Black and white migrants has important implications for our identification strategy in Section 3.5. The finding that Black religious networks primarily facilitated migration through non-wage channels supports their use as an instrument for labor supply that is plausibly exogenous to wage determination. The lack of direct wage effects (after controlling for selection) suggests that religious similarity affected migration decisions through channels other than wage differentials, making it a valid source of identifying variation for estimating the impact of migration on wages.

## 3.5 Estimating Labor-Supply Effects

Building on our empirical findings in Section 3.4, we now develop a framework for estimating how the Great Migration affected wages in both Southern and Northern labor markets. We first discuss how the empirical specification used above can be adapted to this specific question. We then outline a procedure for constructing instrument variables from the religious similarity data to estimate key wage-parameters related to labor-supply changes during the Great Migration. Finally, we present the estimation results using our instrumental variables strategy.

Our empirical strategy employs a three-step estimation procedure that connects the analyses in previous sections. First, we estimate migration shares using Equation 3.2, which identifies the relationship between religious similarity and migration flows during the Great Migration. These estimates capture how religious networks influenced bilateral migration patterns between origin and destination counties. Second, we use these

estimated migration shares to construct instruments for labor supply in 1940, leveraging the exogenous variation in migration flows driven by pre-existing religious networks. As demonstrated in Section 4.2, these religious networks primarily operated through non-wage channels rather than through direct effects on wages, making them valid instruments for identifying the causal impact of migration on wage outcomes. Third, we implement a standard two-stage least squares procedure using these instruments, where the first stage estimates labor supply determinants and the second stage estimates the impact on wages as described in our outcome specification below. This approach allows us to isolate the causal effect of Black migration patterns on Southern labor markets while addressing the endogeneity concerns inherent in migration research.

## Outcome Specification

To structure our thinking around effects on wages in 1940 census, we return to Equation 3.3. First, the restriction to 1940 means we can drop the $y$ index, as we are only concerned with one census year. Second, note that the effects in which we are interested are county-level effects, not cohort. Together these give a modified outcome equation:

$$\ln \overline{wage}_d = \alpha + \sigma_1 L_d + \sigma_2 M_d + \Gamma \ln X_d + \epsilon_d$$

where $L_d$ and $M_d$ are the number of black local workers and the number of black migrant workers observed in $d$ in 1940, and $X_d$ is a vector of controls related to labor supply and demand in $d$, and $\epsilon_d$ represents unobserved factors of labor supply and demand.

The results in Section 3.4 suggest we should carefully consider the set of controls included in $X_d$. Specifically, religious similarity may be correlated with with higher wages through the selection channel. If this were the case, then we would overestimate the effect of labor supply through an inflated reduced-form relationship. To control for this, we include the retention share of workers in $d$ in $X_d$. Furthermore, we show outcomes using both the overall average wage in $d$ and the average wage for only those workers who chose to remain in $d$. This later specification is fully consistent with selection for correction using the discrete choice model described in Appendix C.2.

In addition to the selection control, we also include measures of agronomic suitability in $X_d$. These measures, which account for the natural agricultural productive of county $d$, control for the overall demand for labor in $d$. Though in principal they only directly account for agricultural labor-demand, the historical narrative makes it clear that in practice agricultural and non-agricultural firms closely competed on labor in this setting (Wright et al., 1986). Hence, controls for demand in agriculture should capture non-agricultural demand as well.

## Instrument Construction

It is obvious that a naive regression of log wages onto our labor-supply measures will be biased, as both local retention and in-migration are likely to be higher in locations with high $\epsilon_d$. To account for this, we first note that each of the labor-supply variables can be written as a function of the migration shares analyzed in Equation 3.2:

$$\begin{aligned} L_d &= N_d - \sum_{j \neq d} \pi_{dj} N_d \\ &= N_d \left(1 - \sum_{j \neq d} \pi_{dj}\right) \\ M_d &= \sum_{j \neq d} \pi_{jd} N_j \end{aligned}$$

where $N_d$ is the number of black workers who start in county $d$. In other words, the number of locals ($L_d$) in $d$ is equal to the number of workers who start in $d$ minus the number who choose to leave, and the number of migrants is the sum of workers who choose to migrate to $d$ across all other location.

Using this observation, we construct the following instruments:

$$\begin{aligned} \widehat{L}_d &= N_d \left(1 - \sum_{j \neq d} \widehat{\pi}_{dj}\right) \\ \widehat{M}_d &= \sum_{j \neq d} \widehat{\pi}_{jd} N_j \end{aligned}$$

where

$$\widehat{\pi}_{od} = \exp\left(\widehat{\beta_1} relsim_{od}\right)$$

estimated from Equation 3.2. These instruments provide variation in migrant out and inflows that is driven by social network connections rather than by destination-specific productivity. The validity of this instrument is supported by our finding in Table C.2 that religious similarity did not directly affect wages or operate through productivity channels. Instead, religious networks primarily facilitated migration by reducing non-wage migration costs such as providing social support, housing assistance, and community integration.

## Estimation Results

Table C.3 presents estimates of the effect of Black labor supply on wages and agricultural employment during the Great Migration. Comparing the OLS and IV results reveals

striking differences, particularly for Black workers. In the OLS specification for agricultural employment (column 1), the presence of local Black workers is strongly associated with higher agricultural employment rates, while migrant Black workers correspond with lower rates. These results align with the historical narrative that rural areas retaining their Black population maintained agricultural production, while areas experiencing outmigration saw a decline in the agricultural workforce.

However, when we instrument for labor supply using religious network similarity (columns 2-3), these effects largely disappear for Black workers, suggesting that the OLS results may reflect endogenous sorting rather than causal relationships. For White workers, the IV results show a significant negative effect of local Black labor on agricultural employment and a positive effect of migrant Black labor, indicating a pattern opposite to what we observe for Black workers. This striking contrast suggests that Black workers had limited agency over their sector of employment, typically remaining in agriculture regardless of conditions, while White workers were more able to adjust their occupational choices in response to changes in Black labor supply. Specifically, the negative coefficient on local Black labor implies that when more Black workers were retained in an area, White workers were freed from agricultural labor—likely moving into manufacturing and service sectors with higher wages. Conversely, the positive coefficient on migrant Black labor suggests that White workers were pushed back into agriculture when new Black migrants arrived, possibly due to increased competition for non-agricultural jobs.

Most importantly, the wage results in columns 5-6 reveal that Black migrant inflows had a negative effect on local Black wages, with the IV estimate in column 5 showing a statistically significant coefficient of -0.185. However, this effect weakens and becomes statistically insignificant when controlling for selection in column 6. This finding parallels Derenoncourt (2024), who found that Black migrants negatively affected local Black workers in Northern cities during the second half of the Great Migration. Our results suggest a similar pattern occurred within the South during the first wave of the Great Migration, though the effect disappears when accounting for selection. For White workers, the IV results indicate that local Black labor supply positively affected White wages, suggesting that White workers may have benefited economically from the retention of Black labor in the South.

## 3.6 Conclusion

This paper has examined the role of religious networks in shaping migration patterns during the Great Migration and assessed the subsequent impacts on Southern labor markets. By linking novel for this literature data from the Census of Religious Bodies with full-count census records from 1900 to 1940, we have documented how religious simi-

larity between counties influenced bilateral migration flows for black Americans. Our findings demonstrate that religious institutions served as crucial conduits for information and social support that facilitated geographic mobility during a period of massive demographic transformation.

Several key results emerge from our analysis. First, we find that religious similarity between origin and destination counties significantly predicted higher migration flows for black Americans throughout the study period. Importantly, this effect was specific to black religious networks, with no comparable impact observed when using white denominational similarity to predict black migration patterns. This specificity strengthens our causal interpretation, suggesting that our results capture the unique role of black churches in facilitating migration rather than general religious patterns that would affect all demographic groups similarly.

Second, our individual-level analysis reveals that religious networks primarily operated through non-wage channels rather than by providing information about higher-paying jobs. This finding highlights the multifaceted role of churches in reducing migration costs, including psychological and social barriers to relocation, even when economic incentives remained relatively constant. Religious institutions thus facilitated migration not merely as information providers but as comprehensive support networks that mitigated various risks associated with geographic mobility.

Third, using religious networks to construct a shift-share instrument for migration flows, we estimate the causal impact of black out-migration on Southern labor markets. Our results indicate that white workers in the South experienced substantial wage benefits from black out-migration, primarily through opportunities for occupational upgrading as they transitioned from agricultural employment to manufacturing and service sector positions. Black workers who remained in the South also saw modest wage increases from reduced labor market competition, though these gains were smaller than those accruing to white workers.

These findings contribute to our understanding of both historical migration patterns and the contemporary role of charitable institutions in economic development. The substantial influence of religious networks on migration decisions underscores how social infrastructure can shape economic geography, particularly for marginalized communities with limited access to formal financial and information systems. Our results suggest that the charitable activities of religious organizations extended well beyond direct material assistance to include facilitation of geographic mobility and labor market access —functions that substantially affected both individual economic outcomes and broader regional development.

Several limitations of our analysis suggest fruitful directions for future research. First, while we document the importance of religious networks in facilitating migration, further work could explore the specific mechanisms through which churches transmitted information, provided resources, and maintained social connections across geo-

graphic space. Qualitative historical sources, including church records and correspondence, could complement our quantitative approach by illuminating these processes in greater detail.

Second, our focus on the first wave of the Great Migration (1900-1940) necessarily excludes the larger wave of migration that followed World War II. Extending this analysis to the later period could reveal whether religious networks continued to play a similar role as migration pathways became more established and alternative information sources became more accessible.

Finally, our finding that white workers disproportionately benefited from black outmigration raises important questions about the long-term consequences of the Great Migration for racial economic inequality in the South. Future work could examine how these immediate wage effects translated into persistent differences in wealth accumulation, human capital investment, and intergenerational mobility across racial groups.

In conclusion, our analysis demonstrates that religious networks played a crucial role in facilitating one of the largest internal migrations in American history, with subsequent impacts on labor markets that reshaped economic opportunities along racial lines. These findings highlight how charitable institutions can influence economic outcomes far beyond their immediate services, functioning as vital infrastructure for information sharing, risk mitigation, and geographic mobility. As policymakers and researchers consider the role of social institutions in economic development, particularly for historically marginalized communities, our results suggest that strengthening these networks may yield benefits that extend well beyond those typically measured in assessments of charitable impact.

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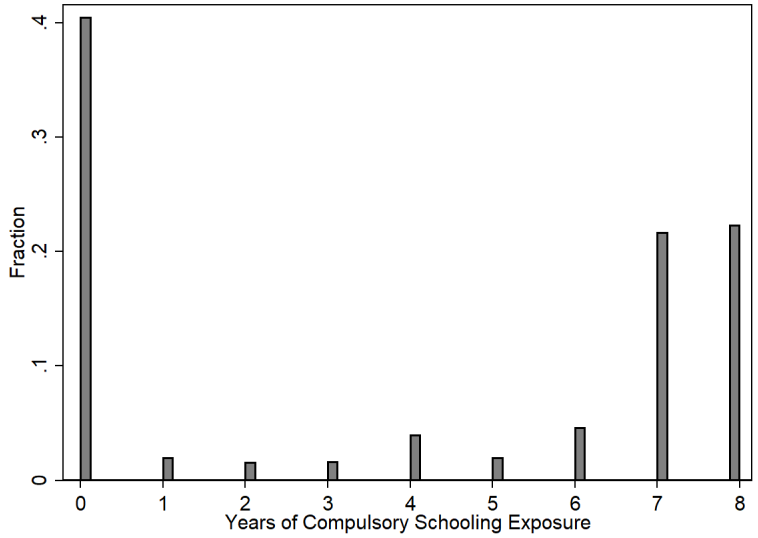
# Appendix A

# On Schooling and the Growth of Wages: Evidence from the Development of the US South

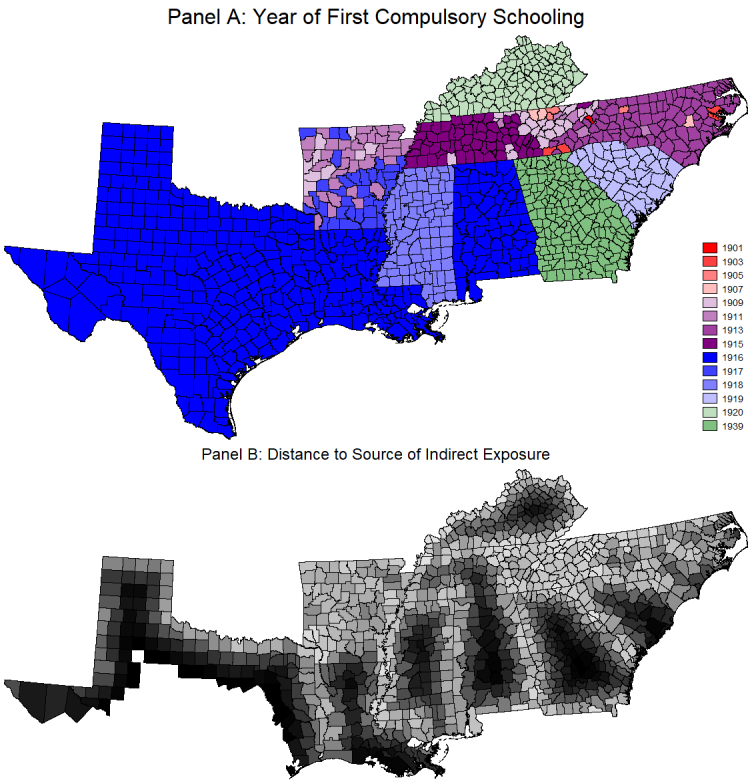
## A.1 Figures and Tables

Figure A.1: Population-Weighted Distribution of Compulsory Schooling Exposure



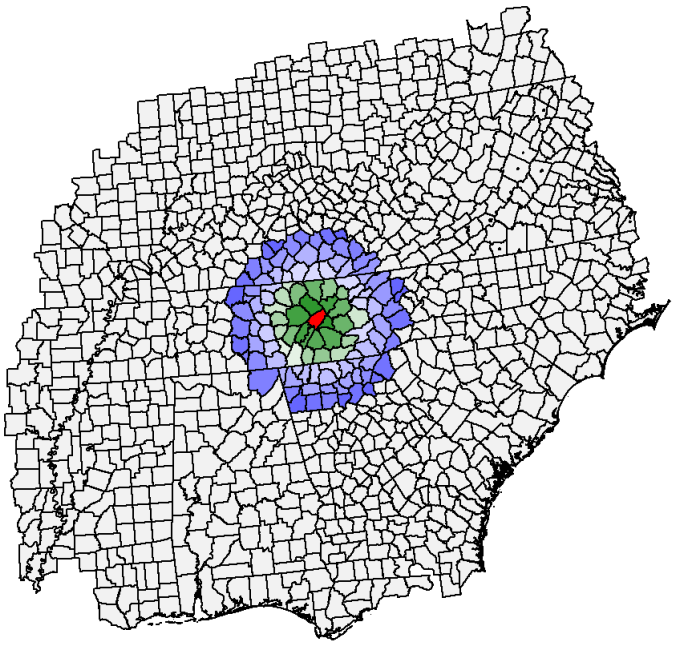

This figure shows the distribution of compulsory schooling exposure across the population. Approximately 40% of the population had zero required years, while most laws mandated at least 7 years of schooling for fully-compelled cohorts. The variation between 0-6 years represents cohorts who were partially exposed when laws were adopted.

Figure A.2: Spatial Variation in Compulsory Schooling Exposure



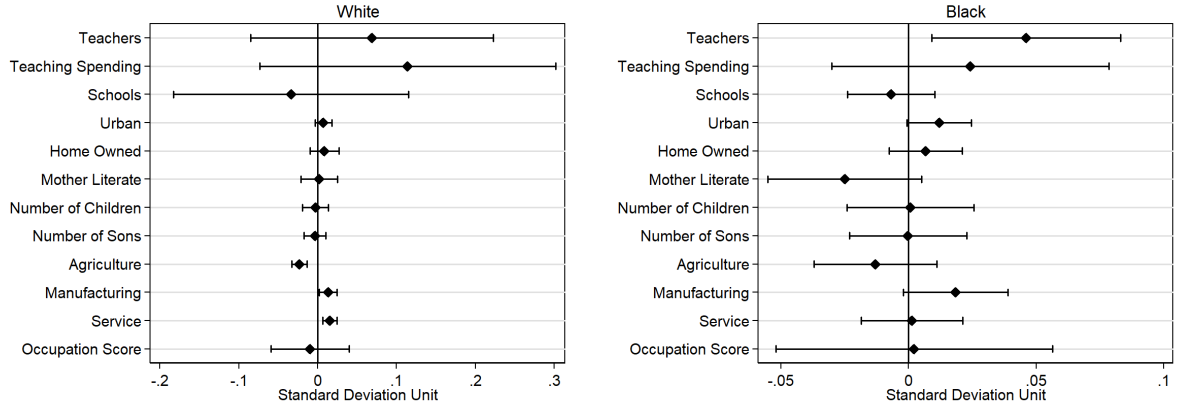

Panel A shows the year compulsory schooling laws came into effect for each county, with most variation occurring within Arkansas, Tennessee, and North Carolina due to county-level adoption. Panel B displays the distance of each county to the nearest source of compulsory schooling exposure other than its own, illustrating the spatial variation used to identify indirect effects.

Figure A.3: Example Spillover and Control Groups



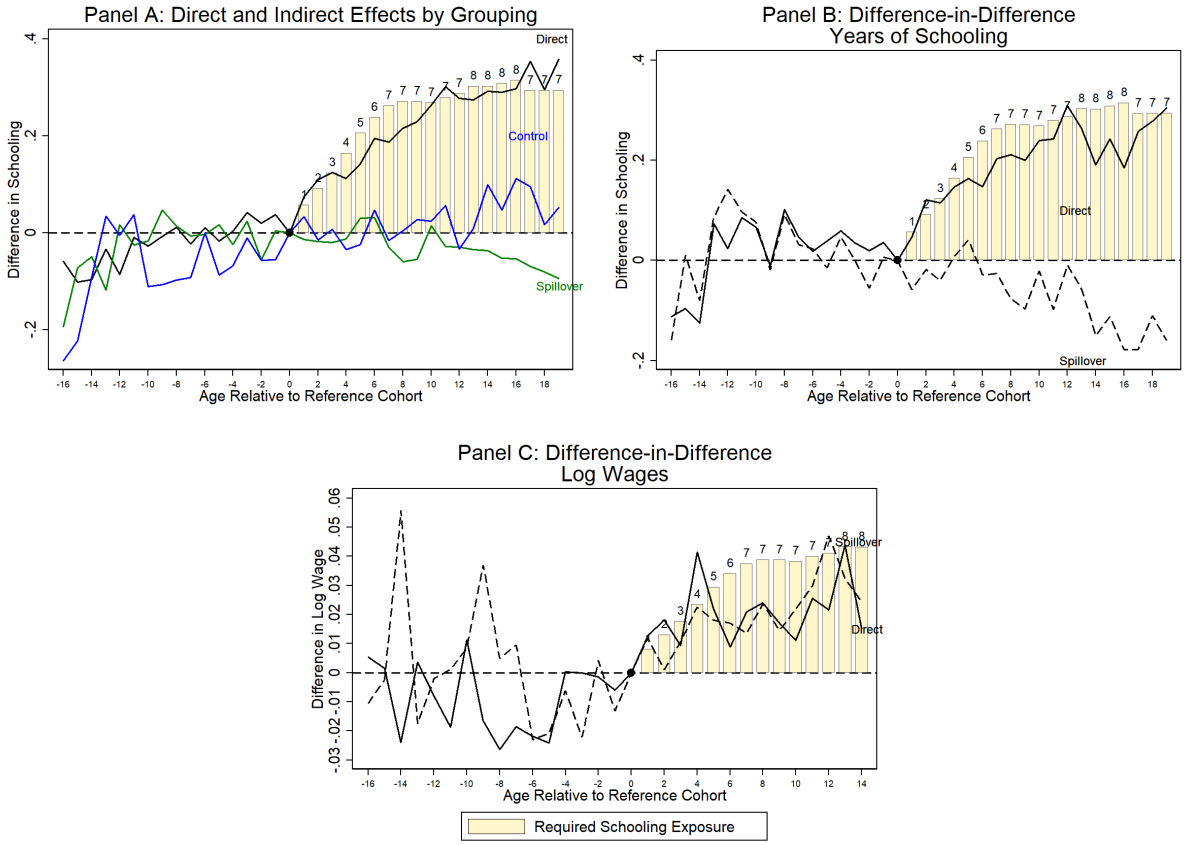
This figure illustrates the spatial identification strategy. The red county represents an adopting county, green counties represent the "spillover" zone within 60 miles, and blue counties represent the "control" zone between 60-120 miles. This spatial structure allows for identification of both direct and indirect effects of compulsory schooling laws.

Figure A.4: Balance Tests: Effects of Compulsory Schooling Exposure on Observable Characteristics




This figure presents balance tests examining whether compulsory schooling exposure is correlated with baseline characteristics that might independently affect educational outcomes. Each point represents the estimated effect of any compulsory schooling exposure on the indicated control variable, with horizontal lines showing 95% confidence intervals. The results are largely reassuring, with most characteristics showing no significant relationship with exposure status. The limited exceptions include a slight positive correlation with the number of black teachers, consistent with historical education reform packages, and modest effects on household heads' occupational distribution, with slightly lower agricultural employment rates in exposed counties. This general lack of correlation between compulsory schooling exposure and observable characteristics strengthens the causal interpretation of the main results by suggesting that selection on unobservables is unlikely to drive the findings.

Figure A.5: Effect of Compulsory Schooling Exposure on Years of Schooling




Panel A shows the raw estimates of exposure effects on schooling for direct (black), spillover (green), and control (blue) groups. Panel B presents the difference-in-difference estimates after controlling for trends in the control group. Panel C shows the corresponding effects on log wages. Background bars indicate average compulsory schooling exposure for each cohort.

Table A.1: Reduced-Form Effect of Compulsory Schooling on Years of Schooling

| | Full Sample | | | | | | Wage Earners | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Direct | | Spillover | | Control | | Direct | | Spillover | | Control | |
| **Years of Schooling** | | | | | | | | | | | | |
| $\hat{b}_{dir,\tau>0}$ | 0.146*** | 0.136*** | -0.052 | -0.045** | -0.069 | 0.059** | 0.163*** | 0.140*** | -0.078* | -0.061*** | -0.042 | 0.008 |
| | (0.046) | (0.028) | (0.034) | (0.018) | (0.075) | (0.028) | (0.045) | (0.027) | (0.040) | (0.020) | (0.064) | (0.026) |
| $\hat{b}_{dir,\tau<0}$ | 0.011 | | -0.007 | | -0.024 | | 0.026 | | -0.019 | | 0.017 | |
| | (0.045) | | (0.033) | | (0.072) | | (0.044) | | (0.040) | | (0.062) | |
| **Log Wage** | | | | | | | | | | | | |
| $\hat{b}_{dir,\tau>0}$ | 0.018* | 0.023*** | 0.029*** | 0.029*** | 0.014 | -0.003 | 0.018* | 0.023*** | 0.029*** | 0.029*** | 0.014 | -0.003 |
| | (0.010) | (0.007) | (0.008) | (0.006) | (0.011) | (0.005) | (0.010) | (0.007) | (0.008) | (0.006) | (0.011) | (0.005) |
| $\hat{b}_{dir,\tau<0}$ | -0.007 | | -0.001 | | -0.016 | | -0.007 | | -0.001 | | -0.016 | |
| | (0.010) | | (0.008) | | (0.010) | | (0.010) | | (0.008) | | (0.010) | |

*Notes:* This table reports the reduced-form effects of compulsory schooling exposure on years of schooling. The "Direct" column shows the effect of a worker's own exposure to compulsory schooling laws in their birth county. The "Spillover" column shows the effect of exposure in neighboring counties within 60 miles. The "Control" column shows effects for counties more than 120 miles away, which are used as a control group. The "$\hat{b}_{dir,\tau<0}$" row tests for any pre-existing trends in schooling prior to the adoption of compulsory schooling laws. Results are shown for the full sample as well as restricting to wage earners only. The difference-in-difference estimates control for common trends across all cohorts using the control group. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Reduced-Form Effect on 1940 Labor Market Outcomes by Migration Status

| | Ext | Schooling | Log Wage | | Agriculture | | | Manufacturing | | | Services | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Earning | Wage | Labor | Farmer | Owner | Labor | Blue | White | Labor | Blue | White | N/A |
| **Panel A: White** | | | | | | | | | | | | | | |
| Full Sample | | 0.132*** | -0.005 | 0.015** | 0.001 | 0.002 | -0.003** | -0.001 | -0.008*** | 0.002** | -0.001 | -0.001 | 0.008*** | 0.001 |
| | | (0.036) | (0.004) | (0.007) | (0.001) | (0.002) | (0.002) | (0.001) | (0.003) | (0.001) | (0.001) | (0.002) | (0.002) | (0.001) |
| Non-Migrant | 0.003 | 0.098* | -0.005 | 0.020 | 0.000 | 0.002 | -0.002 | 0.002 | -0.015*** | 0.003* | -0.000 | 0.004 | 0.005 | 0.000 |
| | (0.004) | (0.053) | (0.005) | (0.014) | (0.002) | (0.005) | (0.004) | (0.002) | (0.004) | (0.002) | (0.001) | (0.002) | (0.004) | (0.002) |
| Within-South | 0.001 | 0.124*** | -0.006 | 0.013* | 0.001 | 0.004* | -0.002 | -0.003** | -0.008** | 0.000 | -0.000 | -0.001 | 0.007*** | 0.002 |
| | (0.004) | (0.035) | (0.004) | (0.007) | (0.001) | (0.002) | (0.002) | (0.002) | (0.004) | (0.001) | (0.001) | (0.004) | (0.003) | (0.002) |
| Out-of-South | -0.004 | 0.206*** | -0.002 | 0.007 | 0.003 | -0.005** | -0.005*** | -0.002 | 0.004 | 0.004** | -0.002 | -0.010* | 0.013*** | -0.001 |
| | (0.005) | (0.036) | (0.006) | (0.009) | (0.003) | (0.003) | (0.002) | (0.004) | (0.007) | (0.002) | (0.001) | (0.005) | (0.005) | (0.002) |
| **Panel B: Black** | | | | | | | | | | | | | | |
| Full Sample | | 0.099*** | -0.015*** | 0.004 | -0.007** | 0.016*** | -0.004* | -0.008** | 0.001 | -0.001* | -0.001 | -0.000 | 0.005** | -0.000 |
| | | (0.031) | (0.004) | (0.009) | (0.003) | (0.003) | (0.002) | (0.004) | (0.003) | (0.001) | (0.003) | (0.003) | (0.002) | (0.002) |
| Non-Migrant | 0.013*** | 0.142* | -0.023** | 0.025 | -0.022*** | 0.041*** | -0.009 | -0.015** | 0.000 | -0.003*** | 0.001 | 0.004 | 0.006 | -0.004 |
| | (0.004) | (0.076) | (0.010) | (0.029) | (0.008) | (0.010) | (0.006) | (0.007) | (0.006) | (0.001) | (0.004) | (0.007) | (0.004) | (0.004) |
| Within-South | 0.001 | 0.074* | -0.026*** | 0.014 | -0.006 | 0.017*** | -0.000 | -0.010 | 0.000 | 0.000 | -0.003 | -0.002 | 0.003 | 0.001 |
| | (0.008) | (0.041) | (0.005) | (0.009) | (0.004) | (0.004) | (0.002) | (0.007) | (0.004) | (0.001) | (0.003) | (0.005) | (0.002) | (0.003) |
| Out-of-South | -0.014** | 0.176*** | 0.001 | -0.028** | -0.005 | -0.001 | 0.001* | -0.007 | 0.001 | -0.003* | -0.002 | 0.010 | 0.007* | -0.002 |
| | (0.007) | (0.062) | (0.007) | (0.014) | (0.003) | (0.002) | (0.001) | (0.007) | (0.006) | (0.001) | (0.005) | (0.007) | (0.003) | (0.005) |

*Notes:* This table presents the effects of compulsory schooling exposure on various labor market outcomes, separated by race and migration status. Outcomes include years of schooling, log wages, and occupational choices across agricultural, manufacturing, and service sectors. The results highlight racial differences in how increased education translated into economic opportunities.

Table A.3: Reduced-Form Heterogeneity by Educational Infrastructure Measures

| | Teachers | | Teacher Spending | | Schools | |
|---|---|---|---|---|---|---|
| | Schooling | Wages | Schooling | Wages | Schooling | Wages |
| **White** | | | | | | |
| $post(x) \times hetero(x)$ | 0.208*** | 0.051*** | 0.082*** | 0.020*** | 0.240*** | 0.028*** |
| | (0.043) | (0.013) | (0.013) | (0.004) | (0.018) | (0.005) |
| **Black** | | | | | | |
| $post(x) \times hetero(x)$ | 0.296*** | 0.005 | 0.198*** | 0.004 | 0.069*** | 0.009* |
| | (0.033) | (0.008) | (0.012) | (0.004) | (0.022) | (0.005) |

*Notes:* This table examines how the effects of compulsory schooling exposure varied with local educational infrastructure, as measured by three key indicators: teacher availability, teacher spending, and school availability. For each measure, the table reports the interaction effect ($post(x) \times hetero(x)$) on years of schooling and wages, estimated separately for white and black workers. The coefficient represents the additional effect of compulsory schooling exposure for each unit increase in the respective infrastructure measure. The results suggest important complementarities between compulsory attendance requirements and broader educational infrastructure, with generally stronger effects in counties with better educational resources. For white workers, all three infrastructure measures show significant positive interactions for both schooling and wages, while black workers exhibit stronger educational interactions but more limited wage interactions. Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Parameter Estimation Results

| | Full | | White | | Black | |
|---|---|---|---|---|---|---|
| | OLS | IV | OLS | IV | OLS | IV |
| **Panel A: Parameter Estimates** | | | | | | |
| $\hat{\eta}$ | 0.118*** | 0.119** | 0.110*** | 0.172** | 0.045*** | -0.023 |
| | (0.001) | (0.050) | (0.001) | (0.067) | (0.001) | (0.043) |
| $\sigma$ | 12.269*** | 16.101 | 12.625*** | 34.093 | 13.697*** | 19.222*** |
| | (0.804) | (10.595) | (0.714) | (61.838) | (1.138) | (6.587) |
| **Panel B: First Stage** | | | | | | |
| Schooling $s_i$ | | | | | | |
| $\hat{\beta}^{s}_{dir}$ | | 0.131*** | | 0.096*** | | 0.163*** |
| | | (0.030) | | (0.022) | | (0.040) |
| $\hat{\delta}$ | | -0.987*** | | -0.992*** | | 0.281 |
| | | (0.180) | | (0.184) | | (0.192) |
| Skill Supply $\ln(N\_kd)$ | | | | | | |
| $\hat{\beta}^{N}_{dir}$ | | 0.003 | | 0.004 | | 0.007** |
| | | (0.003) | | (0.003) | | (0.003) |
| $\hat{\delta}$ | | 1.198*** | | 1.251*** | | 1.130*** |
| | | (0.058) | | (0.062) | | (0.051) |

*Notes:* This table presents the structural parameter estimates from the spatial model of skill supply and demand. Panel A reports the estimated Mincer return to education ($\eta$) and the elasticity of substitution between skill groups ($\sigma$), using both OLS and IV specifications. The IV strategy leverages two instruments: 1) direct exposure to compulsory schooling laws in a worker's birth county, and 2) a migration-weighted measure of compulsory schooling exposure across all cohorts in the economy. Panel B shows the first-stage relationships, including the coefficients on the instrumental variables. Results are reported for the full sample as well as separately by race (white and black workers). The structural model allows for estimating heterogeneous returns and substitution patterns across racial groups.

Table A.5: List of All Acts Regulating Compulsory Schooling from 1900-1940

| State | Year | Act Number | Entry Age | Exit Age |
|---|---|---|---|---|
| Alabama | 1915 | 470 | 8 | 15 |
| Alabama | 1919 | 442 | 8 | 16 |
| Arkansas | 1909 | 234 | 8 | 16 |
| Arkansas | 1911 | 231 | 8 | 16 |
| Arkansas | 1917 | 294 | 7 | 16 |
| Arkansas | 1921 | 294 | 7 | 16 |
| Florida | 1915 | 6831 | 8 | 14 |
| Florida | 1919 | 7807 | 7 | 16 |
| Georgia | 1906 | 399 | 7 | 14 |
| Georgia | 1916 | 576 | 8 | 14 |
| Georgia | 1919 | 217 | 8 | 14 |
| Georgia | 1939 | 215 | 8 | 14 |

Continued on next page

**Table A.5 continued from previous page**

| State | Year | Act Number | Entry Age | Exit Age |
|---|---|---|---|---|
| Kentucky | 1896 | 36 | 7 | 14 |
| Kentucky | 1904 | 96 | 7 | 12 |
| Kentucky | 1908 | 43 | 7 | 16 |
| Kentucky | 1910 | 8 | 7 | 16 |
| Kentucky | 1912 | 65 | 7 | 16 |
| Kentucky | 1916 | 94 | 7 | 14 |
| Kentucky | 1918 | 68 | 7 | 14 |
| Kentucky | 1920 | 80 | 7 | 16 |
| Kentucky | 1920 | 121 | 7 | 16 |
| Kentucky | 1922 | 82 | 7 | 16 |
| Kentucky | 1934 | 121 | 7 | 16 |
| Louisiana | 1910 | 131 | 8 | 15 |
| Louisiana | 1912 | 27 | 7 | 15 |
| Louisiana | 1914 | 117 | 7 | 15 |
| Louisiana | 1916 | 222 | 8 | 14 |
| Louisiana | 1922 | 232 | 8 | 14 |
| Mississippi | 1918 | 258 | 7 | 14 |
| Mississippi | 1920 | 156 | 7 | 15 |
| Mississippi | 1924 | 19 | 7 | 16 |
| Mississippi | 1930 | 278 | 7 | 16 |
| North Carolina | 1901 | 525 | 7 | 16 |
| North Carolina | 1903 | 173 | 8 | 12 |
| North Carolina | 1903 | 208 | 8 | 14 |
| North Carolina | 1903 | 100 | 8 | 14 |
| North Carolina | 1905 | 136 | 7 | 14 |
| North Carolina | 1905 | 226 | 7 | 14 |
| North Carolina | 1905 | 243 | 8 | 14 |
| North Carolina | 1905 | 644 | 7 | 16 |
| North Carolina | 1905 | 649 | 8 | 14 |
| North Carolina | 1907 | 336 | 8 | 14 |
| North Carolina | 1909 | 671 | 8 | 14 |
| North Carolina | 1909 | 246 | 8 | 14 |
| North Carolina | 1911 | 657 | 8 | 14 |
| North Carolina | 1911 | 40 | 8 | 14 |
| North Carolina | 1911 | 799 | 8 | 16 |
| North Carolina | 1911 | | 8 | 14 |
| North Carolina | 1911 | 330 | 7 | 16 |

Continued on next page

**Table A.5 continued from previous page**

| State | Year | Act Number | Entry Age | Exit Age |
|---|---|---|---|---|
| North Carolina | 1913 | 489 | 7 | 16 |
| North Carolina | 1913 | 343 | 8 | 15 |
| North Carolina | 1913 | 200 | 8 | 16 |
| North Carolina | 1913 | 750 | 8 | 12 |
| North Carolina | 1913 | 241 | 8 | 16 |
| North Carolina | 1913 | 142 | 8 | 16 |
| North Carolina | 1913 | 647 | 8 | 16 |
| North Carolina | 1913 | 184 | 10 | 16 |
| North Carolina | 1913 | 243 | 8 | 14 |
| North Carolina | 1913 | 763 | 8 | 16 |
| North Carolina | 1913 | 243 | 8 | 14 |
| North Carolina | 1915 | 270 | 7 | 16 |
| North Carolina | 1915 | 131 | 7 | 17 |
| North Carolina | 1915 | 406 | 7 | 13 |
| North Carolina | 1917 | 173.1 | 7 | 15 |
| North Carolina | 1917 | 347 | 0 | 0 |
| North Carolina | 1917 | 245 | 8 | 14 |
| North Carolina | 1919 | 570 | 8 | 12 |
| North Carolina | 1923 | 718 | 8 | 14 |
| North Carolina | 1923 | 96 | 8 | 14 |
| North Carolina | 1925 | 375 | 7 | 16 |
| North Carolina | 1925 | 710 | 9 | 15 |
| Oklahoma | 1907 | 393 | 9 | 15 |
| Oklahoma | 1913 | 13 | 9 | 15 |
| Oklahoma | 1919 | 59 | 9 | 17 |
| Oklahoma | 1931 | 2039 | 9 | 17 |
| Oklahoma | 1936 | 175 | 8 | 17 |
| South Carolina | 1915 | 98 | 8 | 14 |
| South Carolina | 1919 | 135 | 8 | 14 |
| South Carolina | 1921 | 430 | 8 | 14 |
| South Carolina | 1937 | 344 | 7 | 16 |
| Tennessee | 1905 | 9 | 8 | 14 |
| Tennessee | 1905 | 143 | 7 | 16 |
| Tennessee | 1907 | 115 | 7 | 16 |
| Tennessee | 1907 | 108 | 8 | 14 |
| Tennessee | 1909 | 206 | 8 | 14 |
| Tennessee | 1909 | 236 | 8 | 16 |

Continued on next page

**Table A.5 continued from previous page**

| State | Year | Act Number | Entry Age | Exit Age |
|---|---|---|---|---|
| Tennessee | 1909 | 449 | 8 | 16 |
| Tennessee | 1909 | 582 | 8 | 16 |
| Tennessee | 1909 | 604 | 8 | 16 |
| Tennessee | 1909 | 234 | 8 | 16 |
| Tennessee | 1909 | 342 | 8 | 16 |
| Tennessee | 1909 | 331 | 8 | 16 |
| Tennessee | 1909 | 483 | 8 | 14 |
| Tennessee | 1909 | 494 | 8 | 14 |
| Tennessee | 1909 | 163 | 8 | 16 |
| Tennessee | 1909 | 340 | 8 | 16 |
| Tennessee | 1909 | 705 | 8 | 16 |
| Tennessee | 1909 | 711 | 8 | 16 |
| Tennessee | 1909 | 544 | 8 | 16 |
| Tennessee | 1909 | 494 | 8 | 14 |
| Tennessee | 1909 | 331 | 8 | 16 |
| Tennessee | 1909 | 494 | 8 | 14 |
| Tennessee | 1909 | 377 | 8 | 16 |
| Tennessee | 1909 | 494 | 8 | 14 |
| Tennessee | 1909 | 378 | 8 | 16 |
| Tennessee | 1909 | 494 | 8 | 14 |
| Tennessee | 1909 | 256 | 8 | 16 |
| Tennessee | 1909 | 331 | 8 | 16 |
| Tennessee | 1909 | 236 | 8 | 16 |
| Tennessee | 1909 | 671 | 8 | 16 |
| Tennessee | 1909 | 494 | 8 | 14 |
| Tennessee | 1911 | 347 | 8 | 15 |
| Tennessee | 1911 | 543 | 8 | 16 |
| Tennessee | 1911 | 234 | 8 | 16 |
| Tennessee | 1911 | 342 | 8 | 16 |
| Tennessee | 1911 | 494 | 8 | 14 |
| Tennessee | 1911 | 494 | 8 | 14 |
| Tennessee | 1911 | 494 | 8 | 14 |
| Tennessee | 1911 | 241 | 8 | 16 |
| Tennessee | 1911 | 543 | 8 | 16 |
| Tennessee | 1911 | 494 | 8 | 14 |
| Tennessee | 1911 | | | |
| Tennessee | 1911 | 494 | 8 | 14 |

Continued on next page

**Table A.5 continued from previous page**

| State | Year | Act Number | Entry Age | Exit Age |
|---|---|---|---|---|
| Tennessee | 1911 | 183 | 8 | 16 |
| Tennessee | 1911 | 319 | 8 | 16 |
| Tennessee | 1911 | 604 | 8 | 16 |
| Tennessee | 1913 | 235 | 8 | 16 |
| Tennessee | 1917 | 651 | 8 | 16 |
| Tennessee | 1919 | 494 | 8 | 14 |
| Tennessee | 1919 | 92 | 7 | 14 |
| Tennessee | 1919 | 494 | 8 | 14 |
| Tennessee | 1925 | 331 | 8 | 16 |
| Tennessee | 1925 | 483 | 8 | 14 |
| Tennessee | 1925 | 99 | 7 | 16 |
| Tennessee | 1939 | 494 | 8 | 14 |
| Texas | 1911 | 49 | 8 | 14 |
| Texas | 1915 | 121 | 8 | 14 |
| Texas | 1923 | 160 | 7 | 16 |
| Texas | 1931 | 4 | 7 | 16 |
| Texas | 1935 | 51 | 7 | 14 |
| Texas | 1939 | 221 | 7 | 15 |
| Virginia | 1908 | 64 | 8 | 12 |
| Virginia | 1918 | 412 | 8 | 12 |
| Virginia | 1922 | 381 | 8 | 13 |

## A.2 Derivation of Model Expressions

### Skill and Labor Market Choice Shares

In this section I derive the expression for $\pi_s(x)$ shown in Equation 1.3. First consider the probability of choosing $s = s'$ conditional on belonged to cohort $x$:

$$
\begin{aligned}
p_{s'}(x) = & Pr\left[\omega_s \mu_s(x) \alpha_s(x) z_{is} \leq \omega_{s'} \mu_{s'}(x) \alpha_{s'}(x) z_{is'} | x\right] \qquad \forall\ s \neq s' \\
& Pr\left[C_s(x) z_{is} \leq C_{s'}(x) z_{is'} | x\right] \\
& Pr\left[z_{is} \leq \frac{C_{s'}(x)}{C_s(x)} z_{is'} | x\right] \\
& \int F_{s'}\left(z, c_2 z, \ldots, c_S z\right) dz
\end{aligned}
$$

Recall,

$$F(z_1,\ldots,z_S) = \exp\left\{-\left[\sum_k [z_k]^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\}$$

$$F_1 = \theta[z_1]^{-\frac{\theta}{(1-\rho)}-1}\exp\left\{-\left[\sum_{k=1}[z_k]^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\}$$

Hence,

$$\begin{aligned}
p_{s'}(x) &= \int F_{s'}(z, c_2 z, \ldots, c_S z)\, dz \\
&= \int \theta[z_{s'}]^{-\frac{\theta}{(1-\rho)}-1}\exp\left\{-\left[\sum_{k=1}[c_k z_k]^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\} \\
&= \int \theta[z]^{-\frac{\theta}{(1-\rho)}-1}\exp\left\{-\left[\sum_{k=1}[c_k z]^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\} \qquad i.i.d. \\
&= \int \theta[z]^{-\frac{\theta}{(1-\rho)}-1}\exp\left\{-\left[[z]^{-\frac{\theta}{1-\rho}}\sum_{k=1} c_k^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\} \\
&= \int \theta[z]^{-\frac{\theta}{(1-\rho)}-1}\exp\left\{-[z]^{-\theta}\bar{\kappa}^{1-\rho}\right\} dz \\
&= \frac{1}{\bar{\kappa}^{1-\rho}}\int \theta\bar{\kappa}^{1-\rho} z^{-\frac{\theta}{(1-\rho)}-1}\exp\left\{-z^{-\theta}\bar{\kappa}^{1-\rho}\right\} dz \\
&= \frac{1}{\bar{\kappa}^{1-\rho}}\int dF(z) \\
&= \frac{1}{\bar{\kappa}^{1-\rho}} \\
&= \frac{1}{\left(\sum_{k=1}^{S} c_k^{-\frac{\theta}{1-\rho}}\right)^{1-\rho}} \\
&= \frac{(C_{s'}(x))^{\theta}}{\left(\sum_{k=1}(C_k(x))^{\frac{\theta}{1-\rho}}\right)^{1-\rho}} \\
&= \frac{(\omega_{s'}(x)\mu_{s'}(x)\alpha_{s'}(x))^{\theta}}{\left(\sum_{k=1}(\omega_k(x)\mu_k(x)\alpha_k(x))^{\frac{\theta}{1-\rho}}\right)^{1-\rho}} \equiv \frac{(\omega_{s'}(x)\mu_{s'}(x)\alpha_{s'}(x))^{\theta}}{\Phi(x)}
\end{aligned}$$

Now consider the probability of choosing $d'$, conditional on having chosen $s$,

$$
\begin{aligned}
p_{sd'}(x) &= Pr[A_{sd}\mu_{sd}(x)z_{id} \leq A_{sd'}\mu_{sd'}(x)z_{id'}|xs] && \forall d \neq d' \\
&= Pr[C_{sd}(x)z_{id} \leq C_{sd'}(x)z_{id'}|xs] && \forall d \neq d' \\
&= Pr[z_{id} \leq \frac{C_{sd'}(x)}{C_{sd}(x)} z_{id'}|xs] && \forall d \neq d' \\
&= \int F_{d'}\left(z, c_2 z, \ldots, c_D z\right) dz \\
&= \int \theta[z_{d'}]^{-\frac{\theta}{(1-\rho)}-1} \exp\left\{-\left[\sum_{k=1}[c_k z_k]^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\} \\
&= \int \theta[z]^{-\frac{\theta}{(1-\rho)}-1} \exp\left\{-\left[\sum_{k=1}[c_k z]^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\} && i.i.d. \\
&= \int \theta[z]^{-\frac{\theta}{(1-\rho)}-1} \exp\left\{-\left[[z]^{-\frac{\theta}{1-\rho}} \sum_{k=1} c_k^{-\frac{\theta}{1-\rho}}\right]^{1-\rho}\right\} \\
&= \int \theta[z]^{-\frac{\theta}{(1-\rho)}-1} \exp\left\{-[z]^{-\theta}\bar{\kappa}^{1-\rho}\right\} dz \\
&= \frac{1}{\bar{\kappa}^{1-\rho}} \int \theta\bar{\kappa}^{1-\rho} z^{-\frac{\theta}{(1-\rho)}-1} \exp\left\{-z^{-\theta}\bar{\kappa}^{1-\rho}\right\} dz \\
&= \frac{1}{\bar{\kappa}^{1-\rho}} \int dF(z) \\
&= \frac{1}{\bar{\kappa}^{1-\rho}} \\
&= \frac{1}{\left(\sum_{k=1}^{D} c_k^{-\frac{\theta}{1-\rho}}\right)^{1-\rho}} \\
&= \frac{\left(C_{sd'}(x)\right)^{\theta}}{\left(\sum_{k=1}\left(C_{sk}(x)\right)^{\frac{\theta}{1-\rho}}\right)^{1-\rho}} \\
&= \frac{\left(A_{sd'}\mu_{sd'}(x)\right)^{\theta}}{\left(\sum_{k=1}\left(A_{sk}\mu_{sk}(x)\right)^{\frac{\theta}{1-\rho}}\right)^{1-\rho}} \equiv \frac{\left(A_{sd'}\mu_{sd'}(x)\right)^{\theta}}{\Phi_s(x)}
\end{aligned}
$$

## Conditional Expected Ability

Let $z_i^*$ and $U_i^*$ denote the ability and utility respectively in the chosen labor market of worker $i$. Note that $U_i^*$ will inherit the Frechet distribution from $z_{id}$,

$$\begin{aligned}
Pr(U_i^* < \chi | xs) &= Pr(U_{id} < \chi | xs) && \forall d \\
&= Pr(A_{sd}\mu_{sd}(x) z_{id} < \chi | xs) && \forall d \\
&= Pr\left(z_{id} < \frac{\chi}{A_{sd}\mu_{sd}(x)} | xs\right) && \forall d \\
&= F\left(\frac{\chi}{A_{s1}\mu_{s1}(x)}, \cdots, \frac{\chi}{A_{sD}\mu_{sD}(x)}\right) \\
&= \exp\left\{ - \left[ \sum_{k=1}^{D} \left( \frac{\chi}{A_{sk}\mu_{sk}(x)} \right)^{-\frac{\theta}{1-\rho}} \right]^{1-\rho} \right\} \\
&= \exp\left\{ - \left[ (\chi)^{-\frac{\theta}{1-\rho}} \sum_{k=1}^{D} \left( \frac{1}{A_{sk}\mu_{sk}(x)} \right)^{-\frac{\theta}{1-\rho}} \right]^{1-\rho} \right\} \\
&= \exp\left\{ -(\chi)^{-\theta} \left[ \sum_{k=1}^{D} \left( \frac{1}{A_{sk}\mu_{sk}(x)} \right)^{-\frac{\theta}{1-\rho}} \right]^{1-\rho} \right\} \\
&= \exp\left\{ -\Phi_s(x) \chi^{-\theta} \right\}
\end{aligned}$$

which shows the utility of workers from cohort $x$ choosing $sd$ are also Frechet distributed with scale parameter $\Phi_s(x) = \left[ \sum_{k=1}^{D} \left( A_{sk}\mu_{sk}(x) \right)^{\frac{\theta}{1-\rho}} \right]^{1-\rho}$ and shape parameter $\theta$. But note that $U_{i|xs}^* = A_{sd}\mu_{sd}(x) z_{id}^*$, so the distribution of chosen talent must follow,

$$\begin{aligned}
z_{id}^* &\sim \exp\left\{ -\Phi_s(x) \left( A_{sd}\mu_{sd}(x) z \right)^{-\theta} \right\} \\
&= \exp\left\{ -\Phi_s(x) \left( A_{sd}\mu_{sd}(x) \right)^{-\theta} z^{-\theta} \right\} \\
&= \exp\left\{ -\frac{1}{p_{sd}(x)} z^{-\theta} \right\}
\end{aligned}$$

and it follows from the known expectation of Frechet distributed variables that,

$$\mathbb{E}[z_{id}^* | xsd] = \left( p_{sd}(x) \right)^{\frac{-1}{\theta}} \bar{\Gamma}$$

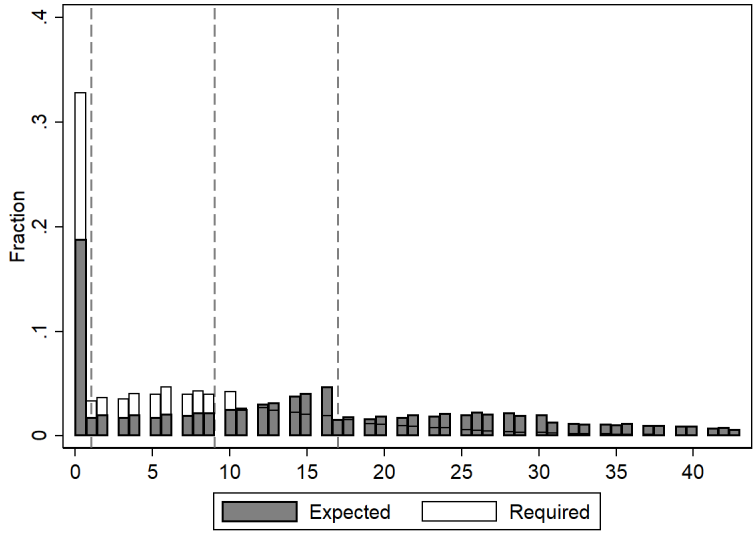
where $\bar{\Gamma} = \Gamma\left(1 - \frac{1}{\theta(1-\rho)}\right)$ and $\Gamma(\cdot)$ is the Gamma function.

# Appendix B

# When Children Go to School: Family Labor Substitution Effects of Compulsory Education in the American South

# B.1 Figures and Tables

Figure B.1: Distribution of Independent Variables




*Notes:* This figure displays the empirical distributions of key analytical variables: expected child-labor years ($exp_h$) and required schooling years ($req_{hc}$). The dashed vertical lines indicate the values used for discrete stratification in regression analysis (0, 1-8, 9-16, and 17+ years), which correspond to households with zero, one, two, and three or more child-equivalents of potential labor.

Table B.1: Example Potential Child-Labor Year Construction

| HH ID | Age | $expected_i$ | $required_i$ | $expected_h$ | $required_h$ | $PCL_h$ |
|---|---|---|---|---|---|---|
| 1 | 36 | 0 | 0 | 10 | 2 | 8 |
| 1 | 14 | 2 | 0 | 10 | 2 | 8 |
| 1 | 8 | 8 | 2 | 10 | 2 | 8 |
| 2 | 24 | 0 | 0 | 4 | 0 | 4 |
| 2 | 12 | 4 | 0 | 4 | 0 | 4 |
| 3 | 39 | 0 | 0 | 0 | 0 | 0 |
| 3 | 18 | 0 | 0 | 0 | 0 | 0 |

*Notes:* This table illustrates how child-labor years ($CLY_h$) is calculated across three example households. Household 1 has a total of 10 expected labor-years from its two children (ages 14 and 8), with 2 years required for schooling due to local laws, resulting in 8 potential child-labor years. Household 2 has 4 expected labor-years from its 12-year-old child but faces no compulsory schooling requirements, giving them all 4 years as potential child labor. Household 3, with only adult members (39 and 18), has zero potential child-labor years. These examples demonstrate how $CLY_h$ varies based on both family structure decisions and geographic exposure to compulsory schooling laws.

Table B.2: Effect on 1940 Labor-Market Outcomes

| | Years of Schooling | | Agricultural Employment | | Log Wage | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A:** Effect on Brothers | | | | | | |
| req_*hc* | | | | | | |
| One | -0.059 | -0.024*** | -0.005*** | 0.001*** | -0.007 | -0.003** |
| | (0.041) | (0.007) | (0.001) | (0.000) | (0.009) | (0.002) |
| Two | -0.181*** | | -0.007*** | | -0.039*** | |
| | (0.062) | | (0.002) | | (0.013) | |
| Three | -0.347*** | | -0.004* | | -0.047*** | |
| | (0.083) | | (0.003) | | (0.018) | |
| exp_*h* | | | | | | |
| One | -0.131*** | -0.039*** | 0.009*** | 0.002*** | -0.032*** | -0.008*** |
| | (0.042) | (0.008) | (0.001) | (0.000) | (0.009) | (0.002) |
| Two | -0.266*** | | 0.020*** | | -0.050*** | |
| | (0.051) | | (0.002) | | (0.011) | |
| Three | -0.320*** | | 0.031*** | | -0.058*** | |
| | (0.060) | | (0.002) | | (0.013) | |
| N | 1,292,324 | 1,292,324 | 1,292,324 | 1,292,324 | 855,706 | 855,706 |
| **Panel B:** Effect on Fathers | | | | | | |
| req_*hc* | | | | | | |
| One | 0.211** | 0.013 | -0.003 | 0.001* | 0.036 | -0.002 |
| | (0.105) | (0.022) | (0.003) | (0.001) | (0.032) | (0.007) |
| Two | 0.088 | | 0.011** | | 0.098* | |
| | (0.165) | | (0.005) | | (0.050) | |
| Three | 0.032 | | 0.027*** | | 0.153** | |
| | (0.237) | | (0.007) | | (0.072) | |
| exp_*h* | | | | | | |
| One | -0.370*** | -0.043** | 0.009*** | 0.002*** | -0.029 | 0.001 |
| | (0.093) | (0.020) | (0.003) | (0.001) | (0.028) | (0.006) |
| Two | -0.383*** | | 0.026*** | | -0.054 | |
| | (0.121) | | (0.004) | | (0.036) | |
| Three | -0.383** | | 0.031*** | | -0.084* | |
| | (0.152) | | (0.005) | | (0.046) | |
| N | 290,099 | 290,099 | 290,099 | 290,099 | 139,619 | 139,619 |

*Notes:* This table presents the effects of compulsory schooling requirements on labor market outcomes measured in the 1940 Census. Panel A shows results for brothers who were old enough to avoid direct effects of compulsory schooling laws, while Panel B shows effects on fathers. For each outcome (years of schooling, agricultural employment, and log wage), two specifications are presented: odd-numbered columns use a non-linear specification with categorical indicators for households affected by approximately one, two, or three children's worth of compulsory schooling, while even-numbered columns use a continuous measure. The "$req_{hc}$" rows show the effects of required schooling years, while "$exp_h$" rows show the effects of expected child labor years based on family structure. Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1. The results suggest that households redirected older siblings toward immediate labor market participation at the expense of their human capital development when younger siblings were required to attend school.

Table B.3: Heterogeneous Effect by Household Head Occupation Score

| | (1) Years of Schooling | (2) Agricultural Employment | (3) Log Wage |
|---|---|---|---|
| **Panel A:** Effect on Brothers | | | |
| $\text{req}_{hc}$ | -0.087*** | 0.004*** | 0.005 |
| | (0.027) | (0.001) | (0.005) |
| $\text{req}_{hc} \times \text{OccScore}_h$ | 0.320*** | -0.012*** | 0.018 |
| | (0.102) | (0.003) | (0.020) |
| N | 426,397 | 426,397 | 297,590 |
| **Panel B:** Effect on Fathers | | | |
| $\text{req}_{hc}$ | -0.021 | 0.004*** | 0.010 |
| | (0.030) | (0.001) | (0.008) |
| $\text{req}_{hc} \times \text{OccScore}_h$ | 0.132 | -0.016*** | -0.063** |
| | (0.092) | (0.003) | (0.025) |
| N | 290,187 | 290,187 | 139,745 |

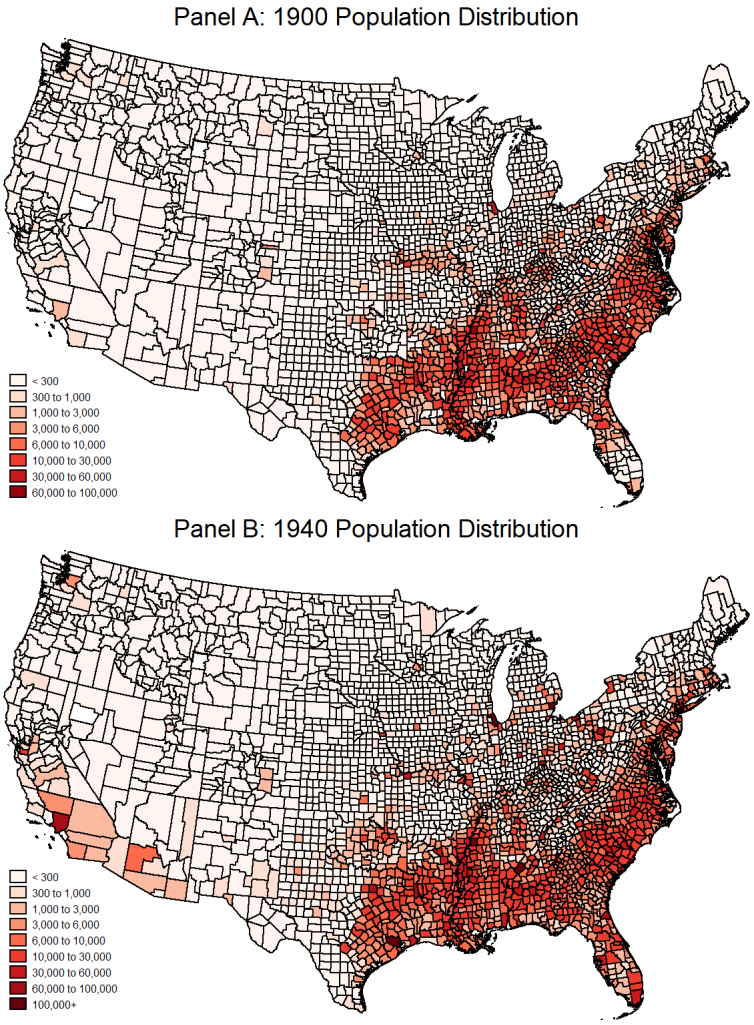
*Notes:* This table presents regression estimates showing how the effects of compulsory schooling requirements vary with household economic status, as measured by the household head's occupational score. For each outcome (years of schooling, agricultural employment, and log wages), the coefficient on $req_{hc}$ represents the effect at the lowest occupational scores, while the interaction term ($req_{hc} \times OccScore_h$) indicates how this effect changes as occupational status increases. Results are shown separately for brothers (Panel A) and fathers (Panel B). Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

# Appendix C

# From Collection Plate to Migration Path: Black Religious Networks and the Economic Consequences of the Great Migration

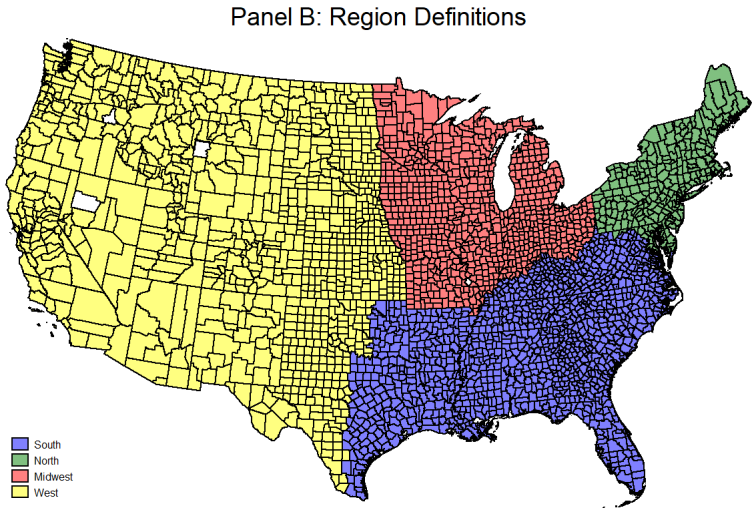
## C.1 Figures and Tables

Figure C.1: Population and Region Maps




*Notes:* This figure illustrates the geographic distribution of the Black population in the United States at key points of the Great Migration. Panel A shows the Black population distribution in 1900, revealing heavy concentration in the Southeast, particularly along the Mississippi River and throughout the Black Belt region. Panel B displays the Black population distribution in 1940, after several decades of migration, showing maintained concentration in the South but with significant growth in Northern and Western urban centers. The comparison highlights the demographic shifts that occurred during the first wave of the Great Migration.

Figure C.2: Definition of Migration Regions




*Notes:* This map presents the regional classification scheme used in analyzing migration patterns during the Great Migration. The United States is divided into four distinct regions: South (blue), North (green), Midwest (red), and West (yellow). This classification accounts for historical population patterns by including only the eastern portions of Texas and Oklahoma within the South region, reflecting the areas that had substantial Black populations from which migration could occur. These regional definitions serve as bounds for analyzing both outward migration flows and their subsequent economic impacts on Southern labor markets.

## Table C.1: Migration Flows and Religious Similarity

| | (1) Within South | (2) South to North | (3) South to Mid-West | (4) South to All |
|---|---|---|---|---|
| **Black Migration:** | | | | |
| Black Similarity | 0.742*** | -0.089 | 0.114 | 0.393*** |
| | (0.262) | (0.151) | (0.211) | (0.141) |
| White Similarity | 0.382 | -0.074 | 0.518 | 0.256 |
| | (0.242) | (0.939) | (1.018) | (0.186) |
| Distance | -1.433*** | -1.467*** | -1.484*** | -1.432*** |
| | (0.075) | (0.231) | (0.095) | (0.074) |
| | (1) | (2) | (3) | (4) |
| **White Migration:** | | | | |
| Black Similarity | 0.562* | 0.477*** | -0.022 | 0.508** |
| | (0.311) | (0.101) | (0.133) | (0.248) |
| White Similarity | 0.676*** | 2.866*** | 1.824*** | 0.877*** |
| | (0.209) | (0.617) | (0.707) | (0.230) |
| Distance | -1.893*** | -1.917*** | -2.084*** | -1.883*** |
| | (0.077) | (0.148) | (0.077) | (0.075) |
| N | 4,284,483 | 864,108 | 2,611,770 | 10617560 |

*Notes:* This table presents estimates of the relationship between religious similarity and migration flows during the Great Migration (1900-1940). The dependent variable is the log of the share of workers who migrated from origin o to destination d. "Black Similarity" measures religious similarity using the three major black denominations (Baptist National Convention, AME, and AMEZ), while "White Similarity" uses the five most prevalent white denominations. Columns (1)-(4) show results for different migration flows: within-South, South-to-North, South-to-Midwest, and pooled South-to-All destinations. All specifications include distance controls and origin and destination fixed effects. Standard errors in parentheses are clustered at the origin-destination pair level. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$

## Table C.2: Religious Similarity and Individual Labor Market Outcomes

| | Ariculture | | Manufacturing | | Service | | Log Wage | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Black:** | | | | | | | | |
| Log Similarity | 0.004*** | 0.001 | -0.004** | -0.002 | 0.000 | 0.000 | -0.062*** | -0.018 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.010) | (0.011) |
| Log Migration Share | | 0.012*** | | 0.002 | | -0.012*** | | -0.185*** |
| | | (0.001) | | (0.001) | | (0.001) | | (0.006) |
| **White:** | | | | | | | | |
| Log Similarity | 0.009*** | 0.007*** | -0.001* | -0.001** | -0.007*** | -0.006*** | -0.062*** | -0.028*** |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.003) | (0.003) |
| Log Migration Share | | 0.016*** | | -0.004*** | | -0.012*** | | -0.154*** |
| | | (0.001) | | (0.001) | | (0.001) | | (0.004) |
| N | 751,496 | 743,222 | 703,510 | 695,645 | 751,496 | 743,222 | 167,576 | 165,365 |
| Census Years | ALL | ALL | ALL | ALL | ALL | ALL | 1940 | 1940 |

*Notes:* This table reports the effects of religious similarity on individual labor market outcomes in 1940. The dependent variables are log wages (column 1) and probability of employment in different sectors: agriculture (columns 2-4), manufacturing (columns 5-7), and services (columns 8-10). Results are shown separately for within-South migration (top panel) and South-to-North migration (bottom panel). All specifications include origin and destination fixed effects and individual-level age-by-schooling fixed effects. Migration shares and distance between origin and destination are included as controls for selection. Standard errors in parentheses are clustered at the origin-destination pair level. $^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$

Table C.3: Labor Supply Effects on Wages and Occupational Choice

| | Agriculture | | | Wage | | |
|---|---|---|---|---|---|---|
| | (1) OLS | (2) IV | (3) IV | (4) OLS | (5) IV | (6) IV |
| **Black Workers:** | | | | | | |
| Local | 0.374*** | 0.083 | 0.003 | -0.075** | 0.152 | 0.066 |
| | (0.047) | (0.131) | (0.128) | (0.037) | (0.102) | (0.117) |
| Migrant | -0.366*** | -0.067 | 0.003 | 0.026 | -0.185* | -0.116 |
| | (0.050) | (0.132) | (0.130) | (0.040) | (0.105) | (0.120) |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **White Workers:** | | | | | | |
| Local | 0.014 | -0.243*** | -0.253*** | 0.012 | 0.129* | 0.187** |
| | (0.030) | (0.088) | (0.083) | (0.027) | (0.076) | (0.088) |
| Migrant | -0.067** | 0.188** | 0.182** | 0.123*** | 0.016 | -0.029 |
| | (0.032) | (0.089) | (0.085) | (0.029) | (0.077) | (0.090) |
| N | 918 | 896 | 896 | 918 | 896 | 896 |
| F-Statistic | | 108.467 | 108.467 | | 108.467 | 108.467 |
| Selection Correction | | | X | | | X |

*Notes:* This table presents instrumental variable estimates of the effects of black labor supply on 1940 wages and occupational outcomes. The dependent variables are log wages (column 1) and probability of employment in agriculture (column 2), manufacturing (column 3), and services (column 4). Results are shown separately for black workers (top panel) and white workers (bottom panel). $\gamma^L$ represents the elasticity of outcomes with respect to local black labor supply, while $\gamma^M$ represents the elasticity with respect to migrant black labor supply. All specifications include origin and destination fixed effects and controls for individual characteristics. Standard errors in parentheses are clustered at the destination level. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

# C.2 Structural Model of Spatial Migration and Wages

Building on our empirical findings in Section 3.4, we now develop a framework to understand how the Great Migration affected wages in both Southern and Northern labor markets. The goal of this section is sto derive a theoretically-grounded wage equation that accounts for both direct labor supply effects and selection.

## Spatial Model of Labor Supply and Demand

### Worker Choice Problem

We model the migration decision as a discrete choice problem in which workers born in origin location $o$ choose a destination $d$ to maximize their utility. The utility that worker $i$ receives from choosing destination $d$ can be written as:

$$U_{id}(o) = w_{id}(o) \times u_{id}(o)$$

where $w_{id}(o)$ represents the wage component of utility (consumption) and $u_{id}(o)$ represents the non-wage component (amenities, migration costs, etc.). This multiplicative formulation allows us to capture both the economic returns to migration and non-economic factors such as religious networks.

The wage component can be further decomposed as:

$$w_{id}(o) = A_d \cdot z_{id}^w$$

where $A_d$ represents the aggregate labor productivity in destination $d$, and $z_{id}^w$ captures individual-specific productivity or ability in the labor market. This formulation allows for both location-specific productivity differences and individual heterogeneity in earning potential.

Similarly, the non-wage component can be expressed as:

$$u_{id}(o) = \alpha_d \tau_{od} \cdot z_{id}^u$$

where $\tau_{od}$ captures systematic migration costs and amenity values associated with moving from origin $o$ to destination $d$. This term incorporates factors such as geographic distance and religious similarity between locations. Our empirical analysis in Section 4 demonstrates that religious similarity significantly influenced migration patterns, particularly operating through these non-wage channels. The term $z_{id}^u$ represents individual-specific idiosyncratic preferences for locations.

We assume that the individual heterogeneity terms $z_{id}^w$ and $z_{id}^u$ are drawn independently from Fréchet distributions with shape parameters $\theta_w$ and $\theta_u$, respectively:

$$F_w(z) = \exp(-z^{-\theta_w})$$
$$F_u(z) = \exp(-z^{-\theta_u})$$

The shape parameters govern the dispersion of abilities and preferences across individuals. A higher value of $\theta$ implies less heterogeneity, with individual draws more concentrated around the mean.

Under these assumptions, the probability that a worker from origin $o$ chooses destination $d$ is:

$$\pi_{od} = \frac{(\alpha_d A_d \cdot \tau_{od})^\theta}{\sum_{d'} (\alpha_{d'} A_{d'} \cdot \tau_{od'})^\theta}$$

where $\theta = \frac{\theta_w \cdot \theta_u}{\theta_w + \theta_u}$ is the composite parameter governing the dispersion of the product of wage and non-wage components.

This expression has an intuitive interpretation: the probability of migrating from $o$ to $d$ increases with the productivity of destination $d$ ($A_d$) and decreases with migration costs ($\tau_{od}^{-1}$). The parameter $\theta$ governs the responsiveness of migration decisions to these factors.

Taking logs of this equation yields:

$$\ln \pi_{od} = \theta \left( \ln \alpha_d + \ln A_d + \ln \tau_{od} \right) - \ln \Phi_o$$

where $\Phi_o = \left( \sum_{d'} (A_{d'} \cdot \tau_{od'})^{\theta} \right)^{1/\theta}$ represents the labor market access for workers from origin $o$.

**Wage Equation**

Building on the worker choice model, we now derive an expression for wages that accounts for both the aggregate labor supply in each location and selection effects. Let us begin by specifying how wages are determined in each location.

The wage of worker $i$ born in origin $o$ who chooses destination $d$ can be written as:

$$wage_{id} = A_d \cdot z_{id}^{w}$$

where $A_d$ is the aggregate labor productivity in destination $d$ and $z_{id}^{w}$ is the worker's individual productivity. To account for the impact of labor supply on productivity, we endogenize $A_d$ as follows:

$$A_d = L_d^{\gamma^L} M_d^{\gamma^M} X_d^{\Gamma} \tilde{A}_d \tilde{\epsilon}_{od}$$

where $L_d$ represents the supply of local workers (stayers), $M_d$ represents the supply of migrant workers, and $\tilde{A}_d$ and $\tilde{\epsilon}_{od}$ are exogenous productivity term conditional on other factors of labor demand ($X_d$). The parameters $\gamma^L$ and $\gamma^M$ capture the elasticity of wages with respect to local and migrant labor supply, respectively.

The average wage in destination $d$ for workers from origin $o$ can be expressed as:

$$\overline{wage}_{od} = A_d \cdot \mathbb{E}[z_{id}^{w} | od]$$

Given our Fréchet assumption on the distribution of worker productivity, the expected value of productivity conditional on the worker choosing $od$ is:

$$\mathbb{E}[z_{id}^{w}|od] = \pi_{od}^{-\frac{1}{\theta}}\overline{\gamma}$$

where $\overline{\gamma}$ is a constant derived from the properties of the Fréchet distribution and $\pi_{od}$ is the share of workers from origin $o$ who choose destination $d$. This selection term captures the fact that workers sort across destinations based partly on their location-specific productivity draws.

Substituting these expressions, we can write the average wage as:

$$\overline{wage}_{od} = L_d^{\gamma^L} M_d^{\gamma^M} X_d^{\Gamma} \tilde{A}_d \tilde{\epsilon}_{od} \cdot \pi_{od}^{-\frac{1}{\theta}}\overline{\gamma}$$

Taking logs, we obtain:

$$\ln \overline{wage}_{od} = \gamma^L \ln L_d + \gamma^M \ln M_d - \frac{1}{\theta}\ln \pi_{od} + \Gamma \ln X_d + \ln \overline{\gamma} + \ln \tilde{A}_d + \ln \tilde{\epsilon}_{od}$$

This equation forms the basis for our empirical strategy to estimate the impact of the Great Migration on wages. The parameters $\gamma^L$ and $\gamma^M$ capture how changes in local and migrant labor supply affected wages, while the term $-\frac{1}{\theta}\ln \pi_{od}$ controls for selection effects. The remaining terms $\ln \tilde{A}_d + \ln \tilde{\epsilon}_{od}$ constitute the error component, which includes both destination-specific productivity and individual-specific wage shocks.

For estimation purposes, we can reformulate this as:

$$\ln wage_{id} = \underbrace{\overline{\gamma}}_{Constant} + \underbrace{\gamma^L \ln L_d + \gamma^M \ln M_d}_{\text{Labor Supply Effects}} - \underbrace{\frac{1}{\theta}\ln \pi_{od}}_{\text{Selection Control}} + \underbrace{\ln \tilde{A}_d + \ln \tilde{\epsilon}_{id}}_{\text{Error Term}}$$