



Going Places: Effects of Early U.S. Compulsory Schooling Laws on Internal Migration

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Abstract

Both the industrialization thesis and institutional theories of education hypothesize that early educational expansion increased internal migration. We take advantage of state variation in early U.S. compulsory schooling laws and use a regression discontinuity approach to test this hypothesis in 1860–1950 Census data. Results indicate that those required to attend school were more likely to leave their state of birth than others. Effects were stronger among men in states with low occupational status scores, suggesting education encouraged migration out of states with fewer occupational opportunities. Potential contemporary implications for the U.S. and developing countries are discussed.

Keywords Internal migration · Compulsory schooling · Educational expansion

Introduction

The second United Nations Millennium Development Goal was to achieve Universal Primary Education (UPE) by 2015. Developing countries made vast improvements in access to education (Child Fund International 2013; UN 2015) and efforts continue with the United Nations Sustainable Development Goals. Anticipated effects of UPE include economic and health benefits (Lutz et al.

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2008). Yet the implications of achieving UPE for migration are not fully understood. Urbanization, for example, is increasing rapidly in developing countries, raising concerns about inadequate resources, violence, and social instability (Muggah 2012; Vidal 2014). Residents of rural areas worry about “brain drain,” the idea that educated youth will leave the area and migrate to cities. This internal migration may reduce regional and local incentives to invest in education, hindering educational expansion and economic development at the national level.

The relationship between education and internal migration is also relevant for developed countries such as the United States. In 2012, President Obama called for states to extend compulsory education to age 18. As of 2015, 24 states required attendance through age 18, while others allow youth to leave school at a younger age (NCES 2015). The benefits states may enjoy from extended compulsory schooling depend on its effects on internal migration. For example, if the educated are more likely to migrate across state boundaries, particularly away from states with lower occupational status scores, states may benefit less from investing in education by extending the compulsory schooling age. Does education drive migration across internal boundaries?

Migration studies have long suggested that internal migration is driven by economic opportunity, with rural-to-urban migration reflecting the income advantage of jobs in industrialized areas (Lee 1966; Sjaastad 1962; Spring et al. 2016; Todaro 1969; see Cooke et al. 2018 and Lucas 1997 for reviews). Since the importance of economic factors for migration was first documented by Ravenstein (1885), Zelinsky (1971) noted that increasing migration (and migration from rural to urban areas) accompanies the modernization process. Specifically, Zelinsky (1971) suggested a “mobility transition”: that mobility increases with and is patterned by development (see Skeldon 2008, 2018 for helpful reviews of the relationship between migration and development). From this perspective, migration from rural to urban areas increased during the process of modernization, but decreased in advanced societies.

Despite the importance of the economy, economic interests alone cannot explain the high rate of internal migration among the educated (Speare and Harris 1986; Spring et al. 2016). Rather, education itself may encourage individuals to move away from home and family, whether due to skills and resources that make moving easier or exposure to new ideas, goals, and opportunities that make moving more attractive (Levy and Wadycki 1974; Spring et al. 2016). In that case, expansion of primary education—which develops these resources and new ideas—could help explain changing patterns of internal migration in the U.S. For example, the proportion of the U.S. population living outside one’s state of birth decreased and then increased from 1850 to 1990, with the lowest rate in 1890 (Spring et al. 2016). As compulsory schooling laws applied to a larger share of the population around the turn of the century, they may have gradually contributed to an increasing rate of inter-state migration in the U.S. population as a whole. In contrast, the proportion of the population experiencing residential mobility in the previous 1 or 5 years has declined since the mid-1900s (Spring et al. 2016). These apparently contradictory trends could reflect educational expansion in different economic contexts (Zelinsky 1971) and expansion at different levels of education. Consistent with this theory, early compulsory

schooling laws could be particularly related to inter-state migration away from rural areas in the context of industrialization.

During the twentieth century, education became increasingly important for many aspects of life in the U.S., including income, wealth, health, and marriage (Fischer and Hout 2006). As education became a stronger determinant of life outcomes, it may have become more consequential compared to other social institutions—such as family and the economy—in determining where we live. In *The Schooled Society*, David Baker (2014) argues that the institutionalization and massive expansion of formal education have influenced culture, the workplace, and religion, among other spheres of life, in this country and throughout the world. The institution of education, Baker suggests, does not merely react to but drives changes in society. Baker’s argument focuses on higher education, and geographic mobility among the college-educated is not new (e.g., Costa and Kahn 2000; Malamud and Wozniak 2012; Wozniak 2018). Do earlier levels of education have similar implications for geographic mobility?

As efforts to achieve UPE continue, this study asks whether the institution of education affects the likelihood of internal migration in a developing context. Specifically, we ask whether early compulsory schooling laws in the U.S. encouraged people to move to a different state. From 1852 to 1918, beginning with Massachusetts and finishing with Mississippi, each state passed a law requiring youth to attend school. (The year school attendance became required in each state is shown in Online Appendix Table A1). These laws increased school attendance and educational attainment, particularly among those from lower-socioeconomic status or minority backgrounds (Puerta 2009; Rauscher 2014). State variation in the timing of compulsory schooling laws allows us to approach a causal estimate of the relationship between education and the likelihood of internal migration. Results indicate that those required to attend school were more likely to leave their state of birth than others and those who moved were more likely to move to a non-farm destination.

Theoretical and Empirical Background

Early U.S. Compulsory Schooling Laws

State compulsory schooling laws aimed to achieve universal attendance among school-age children and, because many children already attended, were primarily aimed at lower-class and immigrant families who did not already send their children to school. As the Commissioner of Education (1891, p. 493) reported, “It must be borne in mind that the law applies to children of tender years, whose right it is to have schooling. If the misfortune or shiftlessness of parents has resulted in poverty, shall the burden of this fall upon young children?” The Commissioner reported that opposition to compulsory schooling came “from the lawless and criminal classes; from the idle and shiftless; from those who take no interest in the education of their children, or care nothing for them but to get work out of them; and, of course, from those who have felt the penalties of the law” (1891, p. 520). This suggests compulsory schooling laws were an attempt by state governments to override

“irresponsible” parents and increase attendance among lower-class and immigrant youth (Moore 1902; Perrin 1896). Although compulsory laws were not strongly enforced, by introducing potential punishments for non-compliance, they encouraged attendance. Furthermore, evidence suggests the laws increased school attendance (Puerta 2009) and educational attainment (Rauscher 2014), particularly among lower-class and minority youth, who were targeted by the laws. These large-scale changes in education may have demographic implications.

Industrialization and the Institutionalization of Education

The economy has long been viewed as a major driving force, contributing to changes in how we work (mechanization), where we live (urbanization), and what we believe (education), among other aspects of society (e.g., Marx 1978 [1846]). For example, a long-standing argument is that industrialization (i.e., the economy) drove educational expansion (Baker 1999; Carl 2009; Goldin 1998; Rahman 2017). Similarly, the industrialization thesis suggests that industrialization made education more important than other factors (e.g., race, family status) on the labor market, allowing skills to trump social background and facilitating social or intergenerational mobility (Treiman 1970).

These arguments are appealing, yet the suggested mechanisms for the relationship between the economy and education often rest on relatively vague ideas about employers or individual demands. For example, the industrialization thesis does not have strong evidence of a mechanism for how education could facilitate intergenerational mobility—that is, how it could weaken the socioeconomic similarity of parents and children. Furthermore, industrialization and manufacturing occurred mainly in urban areas, yet school expansion and enrollments were more substantial in rural areas (Greene and Jacobs 1992). This geographic mismatch between the growth in demand (urban) and supply (rural) of educated workers implies that we must assume large-scale internal migration among the educated from rural to urban areas in order for industrialization to have driven educational expansion.

Recent arguments suggest complex and alternative relationships between education and the economy. Goldin and Katz (2009), for example, highlight the reciprocal relationship between education and the economy, with growth in one area contributing to growth in the other. Others emphasize the independent effects of education on the economy (Lutz et al. 2008; Lutz and Samir 2011) and its implications for many other aspects of society, including marriage, family, work, lifestyle, and religion (Fischer and Hout 2006). Furthermore, although we conflate skill and education as in much existing work (Borjas 1995; Chiswick 1986), informal, tacit, and contextual knowledge is critical for labor market success and economic advancement (Iskander 2019; Iskander and Lowe 2010). Education can facilitate the development of both formal and informal skills, while also increasing the perceived value of education and helping to form a knowledge economy (Baker 2014; Powell and Snellman 2004).

From this perspective, education is an independent force in society, instilling cultural, as well as organizational and occupational transformations, which

contribute to economic changes (Baker 2014, 2009). Institutional theories of education (most notably Baker 2014) highlight the increasing importance of education as a core institution in society. As education expands, incorporating a growing share of the population for longer periods, it has implications for many other institutions in society, at least partly through ideas or beliefs (e.g., about efficiency and appropriateness) (Baker 2014, 2009). For example, by socializing youth, education is thought to create a national identity (Meyer et al. 1979, 1992; Meyer and Rubinson 1975; Ramirez and Boli 1987) and prepare citizens for the responsibilities and the civic duty of participating in a democratic government (Cremin 1951, 1961). Through its influence on culture (Baker 2014) and demography (Fischer and Hout 2006), as well as its ubiquity in nearly all aspects of life, these arguments suggest that education has become a key driver of many social changes.

Geographic mobility, including internal migration and urbanization, may be one social change to which education contributes. At the same time, this mobility may be a mechanism through which education influences other aspects of society. For example, by developing a national identity (Meyer et al. 1979, 1992; Meyer and Rubinson 1975; Ramirez and Boli 1987), education could enable educated people to move to other areas of the country and interact easily with others from different locations and backgrounds. Through this internal migration, education could facilitate the spread of ideas and the incorporation of those ideas into local institutions, such as workplaces. Furthermore, Rosenbloom and Sundstrom (2004) suggest that rising educational attainment may have been responsible for the upward trend in long-distance migration in the last century. They note that their study cannot rule out other potential explanations, and they establish an association between education and migration rather than causal evidence. However, they suggest that education may enable opportunities for migration, possibly to seek employment in other geographical areas (Spring et al. 2016). Thus, both the industrialization thesis and institutional theories of education hypothesize that early educational expansion through compulsory schooling laws increased the likelihood of internal migration.

Recent arguments about the importance of education focus on higher education as the key institution driving social change (Baker 2014) or as the level with the strongest implications for demographic inequality (Fischer and Hout 2006). However, earlier institutional arguments focus on the era of mass schooling (Meyer et al. 1979, 1992; Meyer and Rubinson 1975; Ramirez and Boli 1987). Building on research at an early point in the institutionalization of education, we extend Baker's argument about the institutional importance of education to earlier periods.

At the same time, by examining the relationship between education and geographic mobility, we offer a potential mechanism for the industrialization thesis (i.e., how the economy expands education and, in turn, facilitates intergenerational mobility). If education increases the likelihood of geographic mobility, individuals enjoy a wider array of potential opportunities—on both the labor and marriage markets. Whether education provides greater skills to make moving easier or instills the idea that moving away from one's family is a viable and acceptable option, education could weaken intergenerational similarity through geographic mobility. Similarly, by allowing more educated, rural populations to fill urban occupations, geographic

mobility could be one mechanism through which industrialization expanded education.

Educational expansion, industrialization, and urbanization are related in complex ways. In many ways, these three large-scale changes developed in tandem and identifying one factor that drives another is difficult. Geographic mobility may be a key link that can provide theoretical insight into the complex relationships between education, the economy, and urbanization.

Education and Internal Migration

Partly due to its potential theoretical importance, a long line of work has considered the relationship between education and internal migration, with a focus on early patterns of urbanization (Bowles 1970; Greenwood 1985; Todaro 1969). More recent studies examining periods from the mid-1900s and later have found a positive association between education and internal migration, but rarely establish a causal relationship (Borjas 2006; Feliciano 2005; Molloy, Smith, and Wozniak 2011; Smith and Jons 2015; White 2016). Those that do establish causality tend to focus on post-secondary education. For example, Malamud and Wozniak (2012) use the Vietnam draft lottery to address selection into college and find significant effects of college education on geographic mobility in the U.S. Similarly, research indicates that college education encourages a preference for large urban areas (Compton and Pollak 2007; Costa and Kahn 2000). College may be unique, however, because many young adults move away from home to attend. Furthermore, there may be unique features of the contemporary college-educated labor market, in what many consider a knowledge economy (Powell and Snellman 2004), that make it difficult to compare college with earlier levels of education or their effects in developing contexts.

Considering the complete distribution of education, human capital theory predicts higher levels of internal migration among those with more education (Bowles 1970). Empirical evidence typically supports this association among both the native and foreign-born (Bartel 1989; Bartel and Koch 1991; Gurak and Kritz 2000; Kritz and Nogle 1994; Molloy et al. 2011; Nogle 1994). However, this association does not always hold (McHenry 2013), is not always linear (Borsch-Supan 1990; Long 1973), and can depend on the local wage structure (Roy 1951; Borjas 1992). That is, those with more education may be more likely to migrate from some states but not others, depending on their particular skills and the wage premium in their current state. For example, the Roy (1951) model emphasizes selection and suggests that labor market choices (including migration) may depend on the distribution and correlation of skills and the technology available to use those skills. Alternatively, rather than influencing the likelihood of internal migration, education may matter more for the destination of a mover (Borjas 1992). Thus, while human capital theory leads us to hypothesize that education will increase the likelihood of moving, the heterogeneity identified by Borjas could yield null estimates of the aggregate effect of education on internal migration.

Research examining the theoretical relationship between earlier levels of education and migration often seeks to explain urbanization in developing contexts. For

example, the 1967 Todaro model suggested that mass migration to urban areas occurs because individuals move away from rural areas when their expected real earnings will increase (Todaro 1969, 1980). Because the anticipated benefits of moving off the farm would likely be higher for educated workers, this model implies that education should increase the likelihood of leaving rural areas. We may hypothesize, therefore, that early compulsory schooling laws increase the likelihood of moving and, among those who move, of settling somewhere other than a farm.

Cole and Sanders (1985) point out, however, that the Todaro model cannot explain the mass migration of uneducated workers from rural to urban areas. They note that the educated and uneducated employment sectors depend on each other; growth in the educated labor market increases demand for uneducated workers in developing contexts, possibly at higher rates. This relationship between urban employment sectors, as well as the large population in rural areas, encourages internal migration among the uneducated in developing contexts (Cole and Sanders 1985).

In addition, some evidence suggests that education could enable non-migration (Beals et al. 1967) or reduce the likelihood of migration (McHenry 2013). That is, while many individuals may be forced to leave a rural area for economic reasons, those with more education could achieve a comfortable standard of living in their area of origin and be more likely to stay. Alternatively, additional schooling near the bottom of the education distribution could increase social capital and increase job opportunities in the area, thus reducing the likelihood of migration (McHenry 2013). Thus, the relationship between education and migration is complex (Lucas 1997) and, even in terms of economic interests, the various models suggest contradictory relationships between early educational expansion and internal migration. In developing contexts such as the 19th Century U.S., these models suggest that education could increase, decrease, or have no effect on the chances of internal migration.

Related to this, beyond economic interests or skill-building, education could influence the likelihood of internal migration by providing additional information about areas and opportunities beyond one's area of origin (Levy and Wadycki 1974). The additional information gained through education would strengthen social ties among people from different areas, which can facilitate migration (Massey and Aysa-Lastra 2011; Massey and España 1987). At the same time, it could "reduce the importance of tradition and family ties" (Greenwood 1975, p. 406), making one more open to the idea of moving and increasing the likelihood of migrating.

This informational or ideational argument is consistent with evidence that movers with more education are more likely to move longer distances than movers with less education (Folger and Nam 1967; Levy and Wadycki 1974; Schwartz 1973). For example, compulsory schooling laws predated large-scale migration to the West (primarily among Whites) and in many Southern states occurred shortly before or in the early part of "the Great Migration," when millions of African Americans migrated North (Andrews and Wainer 2017). Because education may encourage longer-distance migration, we can hypothesize that early educational expansion increased the likelihood of moving to the "frontier"—the American west—and moving to a state further from one's state of origin.

Finally, little research on education and internal migration accounts for self-selection into education. Selection is a concern, in this case, because individuals may

partly gain education in order to help them migrate. For example, those who do not “fit in” in a given area may view education as a means to facilitate their ultimate goal of leaving the area. Similarly, individuals or families could migrate in order to gain better educational opportunities (Henderson 1986). Thus, research addressing potential selection into education is required to inform our understanding of its effects on internal migration. Malamud and Wozniak (2012) account for selection into education using the Vietnam draft lottery, but they examine postsecondary education, which could have different implications for migration than earlier levels. In fact, McHenry (2013), who conducts the most comparable study to this one, suggests low levels of education have a negative effect on internal migration. McHenry uses the extension of U.S. compulsory schooling laws in the twentieth century as an instrumental variable to estimate the effect of additional years of education at the bottom of the education distribution on the likelihood of internal migration. However, McHenry’s instrument is weak, which raises concerns about the estimates (Bound et al. 1995; Stock and Andrews 2005). Furthermore, the instrumental variable approach addresses a relatively narrow policy question, estimating the effect of extended schooling only among those whose schooling is affected by the laws (treatment-on-the-treated). While we applaud McHenry’s effort to account for selection effects, additional research is required to shed more light on the relationship between early levels of education and internal migration and to identify potentially broader effects of compulsory schooling laws. When estimating intent-to-treat effects, which are robust to potentially heterogeneous policy responses (Freedman 2006) and are not subject to bias due to weak instruments, we hypothesize that early U.S. compulsory schooling laws will increase the likelihood of internal migration.

Based on the above review, we identify the following four hypotheses. Using early U.S. compulsory schooling laws to account for potential selection into schooling in a developing context, we hypothesize that being required to attend school:

- (1) Increases the likelihood of moving to a different state;
- (2) Increases the likelihood of moving to a non-farm destination;
- (3) Increases the likelihood of moving to the “frontier” —the American west; and
- (4) Increases the distance moved from one’s state of origin.

Methods

To test these hypotheses, we take advantage of state variation in early U.S. compulsory schooling laws. Pooling across states and census years, we use a regression discontinuity (RD) approach to compare those within a narrow window on either side of the compulsory cutoff in each state. Because the timing of these cutoffs varied, we can control for time period and cohort, ensuring that estimates do not simply represent changes over time or cohort. Furthermore, because children could not opt into or out of compulsory assignment, which depended on one’s state of residence and age at the time of the law, this approach accounts for selection into education that could bias other estimates.

By examining the difference between treatment and control groups within a narrow window around an assigned cutoff threshold, RD provides a causal estimate of the treatment effect among otherwise similar individuals (Imbens and Lemieux 2008; Lee and Card 2008; Lee and Lemieux 2010). Figure 1 shows the 5-year window used in this study. Key assumptions are that meaningful unobserved differences between those within a narrow window on either side of the cutoff are eliminated; and other factors related to the outcome vary continuously over the assignment variable, which is controlled in the regression (Lee and Lemieux 2010, p. 287). By limiting our analyses to a narrow window (5 birth years) on both sides of the compulsory cutoff as shown in Fig. 1, we are able to compare individuals who should be similar, except for observed (and controlled) differences in the forcing variable that assigns individuals to the treatment group (age at the time of the law in one's birth state, centered at the age required to attend—called Compulsory School Cohort below). Thus, as Fig. 1 illustrates, identification is based on within-state variation in compulsory schooling across birth cohorts and within-cohort variation in compulsory schooling across states. [Note that Fig. 1 shows birth year cohorts that were or were not required to attend school when they were school-aged based on the year each state made attendance compulsory (shown in Online Appendix Table A1). The compulsory laws first affected cohorts born in years before the laws.]

To check the validity of the RD approach, we look for discontinuities in the forcing variable (Compulsory School Cohort) and in several control measures by the forcing variable. Discontinuities in the density of the forcing variable could suggest that individuals manipulated themselves around the cutoff (e.g., if parents lied about their child's age to avoid compulsory schooling) (McCrary 2008). In that case, compulsory assignment would no longer be exogenous. Beyond density, discontinuities in other variables that should be unrelated to the forcing variable could raise doubt about the RD assumptions. We do not find evidence of sharp discontinuities in the forcing or other variables, which supports the validity of the RD approach here. The appendix includes graphs of the forcing variable and key covariates (age, race, gender, proportion born in the south, state incarceration rate, and state proportions employed in manufacturing, illiterate, and non-white prisoners).

Several studies have used compulsory schooling laws as a natural experiment, including those estimating effects of education on longevity (Lleras-Muney 2005), earnings (Angrist and Krueger 1991; Oreopoulos 2006), educational outcomes (Oreopoulos et al. 2006), and fertility (Puerta 2009). Many of these studies use compulsory schooling as an instrumental variable. However, this approach is biased when the first stage (i.e., the effect of the compulsory law on schooling) is weak. In addition, the instrumental variable approach addresses a narrow question and estimates the local average treatment effect for only the children whose attendance was influenced by the laws (Angrist and Pischke 2009).

Given our interest in the potential implications of UPE, we prefer the intent-to-treat estimate of compulsory laws on migration. These estimates identify the effect of the policy on internal migration, regardless of compliance with the policy. A benefit is that intent-to-treat estimates remain unbiased to heterogeneous policy responses (Freedman 2006) and incorporate potential spillover effects. For example, individuals who comply with compulsory schooling may also be more or less likely to move. In that case,

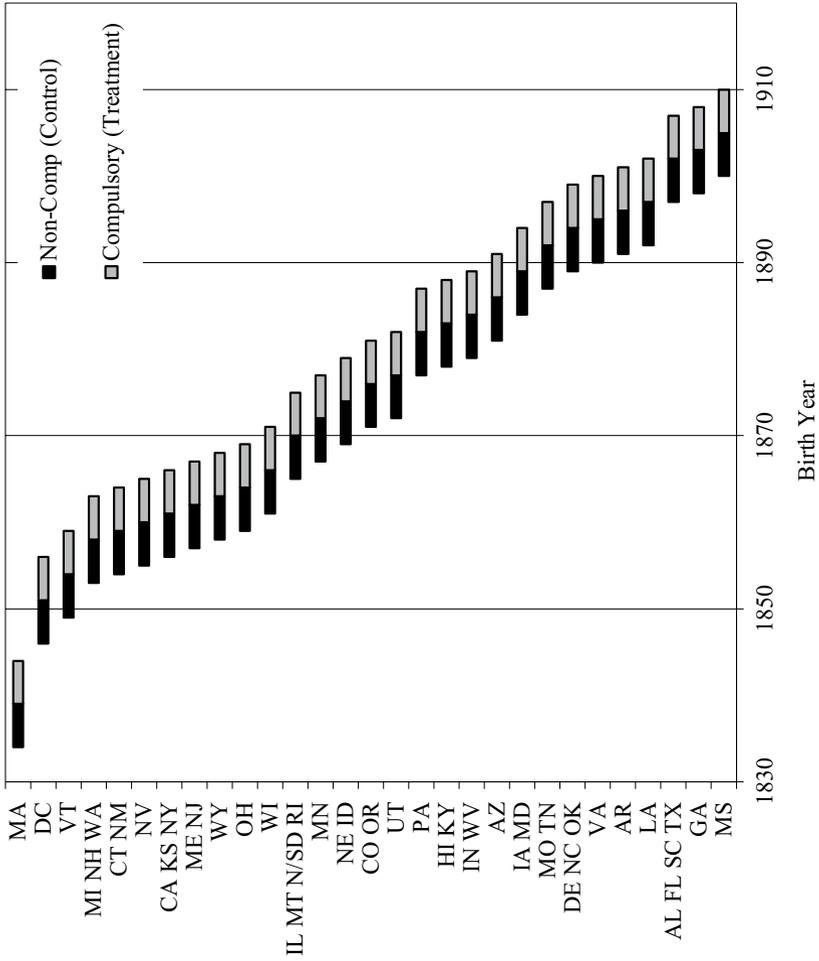


Fig. 1 Regression discontinuity design: 5-year “Treatment” and “Control” Window

alternative approaches such as instrumental variable models that estimate the effects of school attendance would be biased. By focusing on individual attendance, alternative estimates may also miss spillover effects such as exposure to more varied types of children in school due to large-scale increases in attendance. Thus, intent-to-treat estimates provide a more informative understanding of policy effects on a population, such as the implications of UPE.

Data

We use individual census data for years 1860–1950 from the Integrated Public Use Microdata Series (IPUMS), which provides consistent coding schemes over multiple years (Ruggles et al. 2019). This long range ensures statistical support in each state (i.e., some adults from each state who were required to attend school as children and some who were not required to attend). Specifically, the 1860 census includes adults who were school-age before the first compulsory schooling law (1852) and the 1950 census includes those who were school-age after the last law (1918). Census data include state of birth, state of current residence, age, and birth year for each individual. We limit the sample to native-born adults ages 18–50 to encompass prime employment ages in the early twentieth century. We limit the sample for the main analyses to those ages 50 and below to reduce potential selective inclusion in the sample due to higher survival rates among the educated. That is, those with more education survive to older ages than those with less education (Lleras-Muney 2005). If we included those at higher ages, the sample would over-represent those with more education at higher ages and yield biased results. For example, if a higher proportion of those required to attend school survive to older ages and move across a state boundary, the estimated effects of compulsory schooling on internal migration would be inaccurate. In addition, because people might leave their state of birth and move back in their old ages, the sample including the aged population could bias our estimates. Sensitivity analyses including those ages 18–65 yield estimates in the same direction as the main analyses (see Online Appendix Table A3). However, results do not reach statistical significance when predicting two outcomes, suggesting that unequal longevity may bias estimates at higher ages.

In preliminary analyses, we constructed state-level panel data based on ICPSR (1970) Historical Census Data and original census tables to investigate characteristics that best predict the timing of the first compulsory school attendance law in each state. The best-fitting model includes an indicator for states in the Southern census region, proportion illiterate, proportion employed in manufacturing, proportion of non-white prisoners, and incarceration rate (raw and interacted with time since 1850). We control for originating in the Southern census region in all models and for the additional state-level characteristics in sensitivity analyses (Online Appendix Table A6).

Measures

Combining state of birth, birth year, and year of state compulsory attendance law, we create an indicator for compulsory school assignment indicating whether an

individual was required to attend school as a child. Most states required attendance from ages 8 to 14 (Steinhilber and Sokolowski 1966), but a few states required attendance until age 15 (Maine, Rhode Island, Wisconsin) or age 16 (Connecticut, Minnesota, New Hampshire, Wyoming). In some cases, the law went into effect the year after it was passed. However, even when the law went into effect the same year it passed, many children would be a year older by the start of the next school year. Therefore, individuals who were the maximum age at which attendance was required when the law was passed are not coded as being required to attend school. Everyone one year below the maximum age at the time of the law is included in the compulsory group because they were legally required to attend school for at least one school term. This approach assumes that children remained in their state of birth until they were at least school-age. Because families may have moved when their children were young, compulsory assignment is measured with some error. However, mobility rates were lower in the early 1900s than they are today and the likelihood of inter-state migration is lower among those with children (Molloy et al. 2011).

Compulsory School Cohort is the forcing variable that assigns individuals to the treatment group in this study. It measures each individual's age at the time the compulsory schooling law was passed in one's state of birth, centered at the age compulsory attendance was required (i.e., positive values indicate individuals were older than the age required to attend school and were not covered by the compulsory law). This measure is what determines whether an individual was required to attend school or not. Controlling for it is central to the identification strategy in RD analyses, which assumes that unobserved factors related to the outcome vary continuously over the forcing variable (Lee and Lemieux 2010, p. 287). That is, internal mobility may have been changing across cohorts around the time each state made attendance compulsory, but holding Compulsory School Cohort constant allows us to estimate the effect of being required to attend school compared to similar individuals who were not required to attend.

To address the four hypotheses raised in the literature review, we measure internal mobility in four ways. First, we create (1) an indicator for whether each individual's state of residence as an adult differs from his/her state of birth. This measure identifies individuals who migrate across a state border, but not those who move within state boundaries. Although we would prefer to measure internal migration both within and between states, the data do not allow this. Thus, we examine a particular type of internal migration: across a state boundary.

Among those who moved across a state border, we create additional indicators for those who (2) moved states and do not live on a farm as an adult; and (3) moved from an Eastern state to a Western state (i.e., crossed the Mississippi River). Measurement of farm status varies slightly over time in the U.S. Census. Prior to 1910, it is an indicator for whether a household member operated a farm or held the occupation of farmer. In 1910, 1920, and 1960, farm status is determined by the size of the tract of land a household is on and the dollar value of produce it yielded in the previous year. In years 1930–1950, farm status indicates whether one's house is located on a farm. [See Ruggles et al. (2019) for more

measurement details.] Due to this variation, we refer to farm or non-farm destinations in general terms throughout the paper.

Finally, we create (4) a rough estimate of the number of miles an individual moved. Using the geographic midpoint of each state, we calculate the distance between an individual's state of birth and state of residence as an adult. This measure is coded zero for those who did not move states and, even among those who moved states, is measured with error because some individuals could move to an area just across the state border. Despite these limitations, this measure provides a rough measure of how far individuals moved. Together, these measures allow a more nuanced understanding of the relationship between education and internal migration. For example, education may have increased the likelihood of moving, but only to a non-agricultural area or longer distances.

Controls include age, age-squared, race, and gender, as well as an indicator for birth in a state in the Southern census region. We add controls for these characteristics because they are related to the likelihood of migration (Andrews and Wainer 2017; Spring et al. 2016). Sensitivity analyses add controls for time-varying state-level measures that best predict the timing of compulsory laws in the preliminary analyses (proportion illiterate, proportion employed in manufacturing, proportion of non-white prisoners, and incarceration rate—raw and interacted with time since 1850).

Statistical Analyses

We take advantage of the staggered timing of state compulsory attendance laws to estimate their effect on the likelihood of internal migration using an RD approach. Identification is based on the comparison of cohorts immediately before and after implementation of the compulsory laws.

The following linear probability model estimates the effect of compulsory assignment (being required to attend school as a child) on the predicted likelihood of moving from one's state of birth. In Eq. (1), *Comp School* indicates whether individual *i* was required to attend school as a child, *X* represents controls for the forcing variable (age at the time of compulsory law implementation, centered at the cutoff), age, age-squared, race, gender, as well as birth in a Southern state, and *u*, *λ*, and *π* represent fixed effects for state (*j*), census year (*k*), and cohort (*m*, in 5-year categories). Compulsory assignment is interacted with all controls to allow relationships to vary before and after the law. This step avoids assuming a constant linear trend in the likelihood of internal mobility before and after the compulsory law (Cappelleri and Trochim 2015; Lee and Lemieux 2010). However, we show results with and without these interactions.

$$\text{Moved States}_{ijkm} = a + \beta_1 \text{Comp School}_i + \beta_c X_{ijm} + u_j + \lambda_k + \pi_m + \varepsilon_{ijkm} \quad (1)$$

The parameter of interest is β_1 , which estimates the effect of compulsory assignment on geographic mobility. If β_1 is significant and positive, it would suggest the laws increased the likelihood of internal migration. We use the same model to investigate effects on the other mobility measures. Out of concern about logit and other

models that use non-linear link functions (Ai and Norton 2003; Karaca-Mandic et al. 2012; Mood 2010), we use linear probability models. Sensitivity models using logit models yield consistent results (see Online Appendix Table A4). Standard errors are adjusted for state of birth clustering in all models.

The width of the RD window involves a tradeoff between internal validity and sample size (i.e., power). We limit the sample to individuals five years on each side of the compulsory cutoff. Figure 1 illustrates the RD approach. Most states required attendance until age 14, so in most states this RD sample would include birth cohorts who were ages 9 to 18 in the year the compulsory law was passed. In addition to increasing internal validity, a benefit of the narrow window is that the entire sample was born before the laws were implemented, preventing the possibility that estimates reflect effects on parental fertility. Sensitivity analyses using a ten-year window yield similar results (see Online Appendix Table A5).

Results

Descriptive Statistics

Table 1 provides descriptive statistics for the sample, which includes 357,814 individuals who were within five years on either side of the compulsory assignment cutoff. About half of the sample was required to attend school as a child, and those required to attend were more likely to move to a different state and to a non-farm destination compared to their counterparts who were not required to attend ($p < 0.05$). Specifically, 32% of the compulsory school (treatment) group left their birth state, while 31% of the non-compulsory (control) group did.

Table 1 averages across cohorts before and after the compulsory school cutoff in each state. Figure 2 shows the average who moved states for each cohort relative to the compulsory cutoff (the first cohort required to attend in each state). This provides more information and can reveal trends on each side of the compulsory school cutoff. Specifically, Fig. 2 plots the proportion who do not live in their state of birth for each birth cohort (centered at the first birth cohort required to attend school in each state) and compares those who were and were not required to attend school. While the proportion of those leaving their state of birth was declining for both groups, the graph shows that a higher proportion of people who were required to attend school moved to a different state compared to those who were not affected by the compulsory schooling law. Both Table 1 and Fig. 2 indicate a slightly higher rate of inter-state migration among those required to attend school, but Fig. 2 illustrates the intuition behind the RD approach, which controls for variation over the forcing variable.

To provide more detail about where migrants settled, a higher proportion of migrants from the treatment group (26%) did not settle on a farm, compared to the control group (24%). In contrast, 8% of those not required to attend school moved across the Mississippi River, compared to 7% among the treatment group. Finally, the treatment group moved significantly further from their state of birth (185 miles), on average, than those not required to attend school (175 miles). That these

Table 1 Descriptive statistics

	Mean	(SD)	Compulsory schooling		Difference	
			Yes	No		
			Mean	Mean		
Compulsory schooling	0.503	(0.500)	1.000	0.000	(0.000)	
Dependent variables						
Move to a different state (DV1)	0.314	(0.464)	0.319	0.308	(0.462)	- 0.011 ***
Move to non-farm destination (DV2)	0.250	(0.433)	0.260	0.241	(0.427)	- 0.019 ***
Move from East to West (DV3)	0.074	(0.261)	0.071	0.076	(0.265)	0.004 ***
Miles moved (DV4)	179.828	(388.126)	184.882	174.703	(379.884)	- 10.180 ***
Independent variables						
Age	33.866	(9.694)	34.390	33.335	(10.150)	- 1.055 ***
Non-White	0.171	(0.376)	0.162	0.179	(0.384)	0.017 ***
Male	0.497	(0.500)	0.498	0.497	(0.500)	- 0.001
Born in south	0.524	(0.499)	0.512	0.536	(0.499)	0.024 ***
Compulsory schooling cohort	- 0.560	(2.819)	- 2.979	1.893	(1.402)	4.872 ***
Birth cohort (5-year)	1887.914	(15.432)	1890.055	1885.744	(15.273)	- 4.311 ***
Survey year	1919.780	(17.810)	1922.430	1917.093	(18.113)	- 5.337 ***
N	357,814		180,999	176,815		

Source 1860–1950 US Census data from IPUMS. Sample excludes those with missing data

* < 0.05, ** < 0.01, *** < 0.001

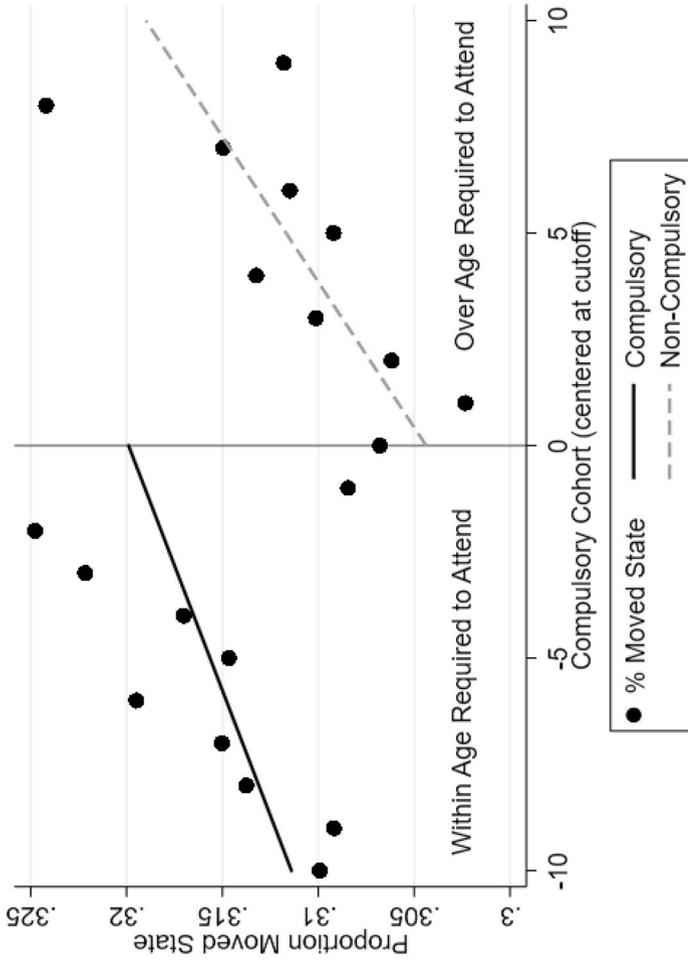


Fig. 2 Regression discontinuity plot by compulsory cohort: centered at compulsory cutoff. The figure plots the observed proportion who do not live in their state of birth for each birth cohort, centered at the age cutoff required to attend school in each state. The linear predictions compare those who were and were not required to attend school. Positive compulsory cohort values indicate those who were over the age required to attend school at the time of the compulsory law at not required to attend school (non-compulsory). Negative compulsory cohort values indicate those who were younger than the age required to attend school at the time of the law and were required to attend school (compulsory)

differences are significant is not surprising, given the large sample sizes. Of greater interest is whether these differences remain significant after accounting for potentially important heterogeneity.

When comparing descriptive statistics among men and women, men were more likely to leave their birth state, move to a non-farm destination and West, and move further from their birth state compared to women. Differences in the likelihood of mobility by compulsory school assignment are similar for men and women, except for moving West (see Online Appendix Table A2). In addition, men tended to move further, on average, but the difference in miles moved by compulsory assignment was larger for women (13 miles) compared to men (7 miles). These descriptive statistics suggest that compulsory school assignment may have affected men and women differently. Ravenstein (1885) also noted gender differences in migration patterns. In addition to our main analyses, we therefore estimate separate models by gender to assess potential heterogeneous effects.

In addition to measures of internal migration, Table 1 also includes descriptive statistics for other measures. The average age of the sample was 34, the majority was white (83%), and about half were born in South. The sample includes those born between 1831 and 1910.

Regression Discontinuity Analysis

Although descriptive statistics are informative, the observed differences in the dependent variables could be attributed to factors other than compulsory assignment. Using the RD models, we estimate the effects of compulsory schooling laws on geographic mobility, controlling for demographic characteristics including age, race, gender, cohort, and birth state.

Results of the RD analyses in Table 2 show coefficients for compulsory schooling from separate models. The first row displays the coefficients for compulsory schooling without controlling for other variables. Compulsory schooling has significant positive effects on the likelihood of leaving one's birth state, moving to a non-farm destination, and miles moved, consistent with descriptive statistics in Table 1. The second to fourth rows show that adding controls for compulsory cohort (the forcing variable) and fixed effects of birth cohort and Census year attenuates the effects of compulsory schooling on distance moved, but effects on the likelihood of inter-state migration and moving to a non-farm destination remain significant. Adding birth state fixed effects and interactions of compulsory schooling with all measures attenuates the effects of compulsory schooling, but effects of compulsory schooling reappear in the last set of models that include controls for age, race, gender, and Southern region. We prefer the final models with interactions and controls because they avoid assuming a constant linear trend in the likelihood of internal mobility before and after the compulsory law (Cappelleri and Trochim 2015; Lee and Lemieux 2010) and it holds constant factors that could influence both mobility and compliance or benefit from compulsory schooling (Rauscher 2014).

The full models in Table 2 show that those required to attend school were 6% ($p < 0.01$) more likely to leave their birth state than those not required to attend. This

Table 2 Estimated effects of compulsory school assignment on internal migration

	DV1 moved state	DV2 moved state	DV3 moved state	DV4 moved state
Effects of compulsory schooling with controls for				
None	0.011** (0.004)	0.019*** (0.003)	- 0.005 (0.006)	10.180*** (2.497)
+ Compulsory schooling cohort (forcing variable)	0.013* (0.006)	0.010* (0.005)	0.008 (0.009)	5.500 (5.067)
+ Birth cohort	0.013* (0.006)	0.011* (0.005)	0.003 (0.007)	3.479 (4.662)
+ Survey year	0.012* (0.005)	0.011** (0.004)	0.003 (0.006)	3.078 (3.693)
+ Birth state	0.006 (0.003)	0.007 (0.004)	0.000 (0.002)	1.128 (2.826)
+ Compulsory schooling interacted with all measures	0.031 (0.018)	0.014 (0.019)	- 0.023 (0.017)	- 64.407*** (17.787)
+ Age, Age2, Non-White, Male, and Born in South	0.062** (0.021)	0.045* (0.022)	- 0.019 (0.017)	- 48.682** (17.390)
N	357,814	357,742	358,019	357,814

Each value represents the coefficient for compulsory schooling from a separate model. Robust standard errors are adjusted state of birth clustering in all models. The first row shows coefficients from a model with compulsory schooling alone. Each successive row adds additional variables

* < 0.05, ** < 0.01, *** < 0.001

result is consistent with Hypothesis 1. Beyond compulsory assignment, results of the full models (available upon request) suggest that being older, non-white, and male increase the likelihood of moving to a different state compared to their counterparts. However, the differences by age, race, and gender are not larger than the difference by compulsory school assignment.

The full model in Column 2 of Table 2 shows that those covered by compulsory schooling laws during childhood were 5% more likely to move to a non-farm destination ($p < 0.05$). This finding supports Hypothesis 2 and suggests that education may have reduced reliance on agriculture. Column 3 in Table 2 finds that compulsory schooling did not significantly alter the likelihood of moving across the Mississippi River. This finding does not support Hypothesis 3, that being required to attend school increases the likelihood of moving to the “frontier”—the American West. Interestingly, although non-whites were significantly more likely to move to an urban and non-farm destination, they were significantly less likely (-3% , $p < 0.01$) to move West. This is consistent with the Great Migration of African Americans (the vast majority of non-whites during this time period) to northern, industrial cities (Andrews and Wainer 2017; Collins and Wanamaker 2014; Tolnay 2003).

Finally, those required to attend school moved about 49 miles closer to their state of origin than their counterparts (Column 4 in Table 2), contradicting Hypothesis 4. Since the geographic sizes of states vary substantially, we check the robustness of the results predicting distance moved by dividing the sample depending on whether one’s birth state was larger than the median size or not. The coefficient predicting distance moved is larger among those born in large states, but the coefficient is still negative and significant among those born in small states. Thus, compulsory school assignment increased the likelihood of leaving one’s state of origin, but reduced the distance of the destination state among those who moved.

Sensitivity Analysis

To assess potential heterogeneity, we estimate whether the compulsory assignment effects we find in Table 2 are stronger for the less-educated (McHenry 2013) or those born in low-SES states using the Duncan Socioeconomic Index (SEI). Sensitivity analyses excluding those in the top quartile of SEI score (see Online Appendix Table A7) yield results consistent with those in Table 2, although estimates are less precise. Specifically, compulsory schooling increased the likelihood of moving to another state by 7% ($p < 0.01$). Coefficients predicting other outcomes are not significant. Compulsory schooling laws may have had less impact on those with high educational attainment (who likely achieved high SEI scores), because they may have attended school regardless of the laws (McHenry 2013). However, Model 1 in Online Appendix Table A7 suggests that the effect of compulsory assignment on internal migration does not differ when excluding the highly educated. These results should be interpreted with caution because they could reflect measurement error (i.e., SEI may be a poor proxy for educational attainment) and heterogeneous responses to compulsory assignment (i.e., SEI is achieved after the treatment).

The effects of compulsory assignment may also differ by the relative economic context of the state. That is, the potential effects of education on internal migration may be stronger in states with lower levels of occupational opportunity. To assess this possibility, we create an indicator for states that were below the median on state-level mean SEI score, measured for an individual's state of birth at the same time as the other state-level measures included in the models. We add this indicator and its interaction with compulsory assignment to the models used in our main analyses. Results in Online Appendix Table A8 show that the interaction of compulsory schooling with low-SEI states is not significant when predicting distance moved or moving to a different state or a non-farm destination. However, when predicting moving West, results suggest that compulsory assignment effects were higher among those born in low-SEI states.

Descriptive statistics suggested potential heterogeneous effects of compulsory assignment by gender. To examine this possibility, we fit each model in Table 2 separately by gender. Results in Table 3 suggest that compulsory assignment significantly increased the likelihood of moving away from one's state of birth among both men and women (6%). Among men, compulsory assignment reduced the distance moved (78 miles), while among women, compulsory assignment increased the likelihood of moving to a non-farm destination (6%).

Overall, results in Table 3 suggest that compulsory schooling had largely consistent effects on internal migration by gender, but with some differences in the effects on destination. Compulsory schooling effects on internal migration may differ by gender for multiple reasons. For example, among those required to attend school, the potential benefit of moving across state borders may have been lower for women because their range of potential occupations was narrower (e.g., Goldin 1980; Hooks 1947). Ravenstein (1885) recognized that men tend to move longer distances than women, although females are more migratory than males. Furthermore, the compulsory laws may have had a weaker influence on girls' schooling (Rauscher 2014), resulting in weaker effects of the laws on migration for women; and parents may have expected to rely more on daughters than sons for care in their old age (Horowitz 1985), making daughters less likely to migrate regardless of schooling.

Table 3 Estimated effect of compulsory school assignment on internal migration by gender

	DV1 moved state	DV2 moved state	DV3 moved state	DV4 moved state
Men	0.064** (0.022)	0.033 (0.023)	- 0.034 (0.018)	- 78.046*** (20.322)
<i>N</i>	178,278	178,230	178,482	178,278
Women	0.059* (0.027)	0.059* (0.028)	- 0.002 (0.020)	- 16.347 (18.480)
<i>N</i>	179,536	179,512	179,537	179,536

Coefficients are from RD models with the same specifications as the final model (bottom row) in Table 2, but the sample is limited to men or women, respectively

* < 0.05, ** < 0.01, *** < 0.001

Although there is some evidence for different effects by gender, effects on the likelihood of inter-state migration are the same. Sensitivity analyses (not shown) testing interaction effects of compulsory assignment and gender are not significant. This implies that the effects of compulsory school assignment on internal migration did not differ significantly by gender. (Online Appendix Table A11 provides results from sensitivity analyses by gender).

Although the main effects may not differ by gender, Online Appendix Table A11 suggests that effects may differ by both gender and state economy. Among men, the interaction between compulsory assignment and states below the median SEI score is significant in all models. Compared to men from a high-SEI state, men from a low-SEI state who were required to attend school were 14% more likely to move states, 15% more likely to move to a non-farm destination, 4% more likely to move West, and moved 85 miles further from their birth state ($p < 0.05$). The interaction term is only significant when predicting moving West when including both genders and is never significant among women. This pattern suggests that compulsory assignment effects among men depended on the economy in one's birth state. That is, education increased the likelihood of migrating among men, but only if occupational opportunities in their place of origin were relatively low. In contrast, educational effects among women were not dependent on the economy. This could reflect a greater ability to migrate for occupational reasons among men in this period.

To summarize, Fig. 3 illustrates the estimated effects of compulsory schooling laws on each dependent variable for the full sample and by gender. In support of Hypotheses 1 and 2, we find significant effects of early compulsory laws on the likelihood of moving to a different state and to a non-farm destination. In contrast, early compulsory laws reduce the distance moved among those who migrate (in the full sample and when limited to men) and reduce the likelihood of moving from East to West (among men). In other words, results contradict Hypotheses 3 and 4. Sensitivity analyses indicate that these results hold when including a ten-year RD window (Online Appendix Table A5) and when controlling for a cubic and a quartic in age (Online Appendix Tables A9 and A10). When controlling for time-varying state characteristics, results are generally consistent with the main analyses, but only

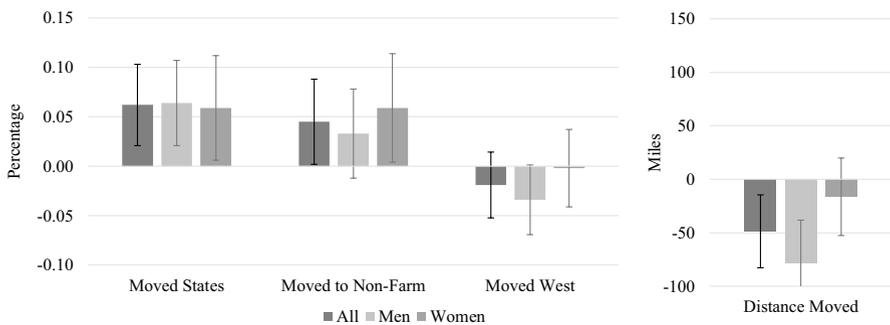


Fig. 3 Estimated effects of compulsory school assignment on internal migration by gender. The figure illustrates compulsory school coefficients from the final model (bottom row) in Table 2 and from models by gender in Table 3

reach significance when predicting movement to a non-farm destination (Online Appendix Table A6). Additional sensitivity analyses, including results of logit models, are provided in the Online Appendix Tables A3–A11.

Discussion

This study uses U.S. census data to evaluate whether early compulsory schooling laws increased the likelihood of internal migration across a state boundary. Taking advantage of state variation in the timing of compulsory laws, we use a regression discontinuity approach to address potential selection into schooling. Results indicate that those required to attend school were 6% more likely to leave their state of birth than others. Furthermore, they were significantly more likely to move to a non-farm destination, but moved shorter distances compared to those who were not required to attend school. On the other hand, compulsory assignment did not significantly alter the likelihood of moving West. Collectively, these results suggest those required to attend school may have been more likely to move to an urban area in a nearby state, rather than move West. This interpretation makes sense if education provides a higher return in urban areas than on the frontier.

Our findings indicate that educational policy changes can induce societal changes, in this case encouraging internal migration, supporting the institutionalist argument (Baker 2014; Fischer and Hout 2006; Lutz et al. 2008; Lutz and Samir 2011). Results suggest early educational expansion had important implications for society (Meyer et al. 1979, 1992; Meyer and Rubinson 1975; Ramirez and Boli 1987), extending Baker's (2014) argument about the contemporary institutional importance of education to an earlier period.

At the same time, our results provide a potential mechanism for the industrialization thesis (Treiman 1970). Whether through skill development or ideational changes, we find that education increased the likelihood of geographic mobility, which in turn could weaken the economic relationship between parents and children. Evidence suggests that a college degree reduces intergenerational inequality (Hout 1988; Torche 2011), perhaps in part by increasing geographic mobility (Costa and Kahn 2000). Future research could investigate the extent to which geographic mobility is required in order to enjoy the potential economic benefits of education (e.g., Wozniak 2018). If rural or disadvantaged families fear family separation, the greater likelihood of geographic mobility among the college-educated could deter enrollment among those who may enjoy the greatest economic benefits (Brand and Xie 2010).

Consistent with Borjas (1992), we find that education has implications for the destination of a mover, as well as the likelihood of mobility. Compulsory schooling reduced the likelihood of moving to a farm and reduced the distance moved. The difficulty of unpacking these results raises a limitation of our study. Specifically, we are only able to identify internal migration (and indicators for destinations, such as not on a farm) among those who moved across a state border. Education could have important effects on within-state migration. For example, we find some evidence that compulsory schooling increased migration to a non-farm

destination among those who moved to another state. However, effects may differ among those who move within the same state. Thus, our analyses of effects on migration off the farm or to urban areas warrant further investigation at a finer spatial scale.

A second limitation is that our data do not allow examination of the extent to which school attendance, rather than the legal requirement to attend school, influenced internal migration. That is, our data do not include information on school attendance as a child. The 1850–1930 census data provide information about school attendance among the cohorts in our sample when they were school-age. Using those data, we find that 89% of school-age youth (ages 6–13) attended school in the last year in states that had made attendance compulsory, compared to 60% in states that had not made attendance compulsory. Appendix Fig. 10A graphs attendance rates by cohort, comparing states with and without a compulsory law. Attendance is consistently higher in states that required attendance and the difference ranges from 14 to 35 percentage points depending on the cohort. This information cannot identify whether school attendance is the mechanism for the effects we find because it is only available for school-age children (rather than the adults examined here). However, one aim of this study is to identify the potential implications of efforts to achieve UPE in a developing context. We use intent-to-treat estimates to identify the effect of compulsory schooling laws on internal migration, regardless of compliance with the policy. This estimate is of value for policymakers, who need to understand the full implications of a policy for society rather than only for those who comply with it.

Previous research suggests that early U.S. compulsory schooling laws increased attendance by 7%, on average (Puerta 2009). By estimating intent-to-treat effects, we avoid potential concerns about a weak instrument (McHenry 2013), avoid bias due to potential heterogeneous policy responses (Freedman 2006), and incorporate potential spillover effects of the policy. However, because some readers may appreciate rough treatment-on-the-treated (TOT) estimates, we calculate those by dividing the intent-to-treat estimates by the average first-stage estimate of the effect of compulsory laws on school attendance. Using the coefficients from the final model in Table 2, the TOT estimate suggests that among those who complied, compulsory schooling laws increased the likelihood of migration to another state by 86% (0.06/0.07). For those who moved to another state, the TOT estimates suggest that compliance with the law increased the likelihood of moving to a non-farm location by 64% and reduced the distance moved by 695 miles. These estimates should be interpreted with caution due to potential heterogeneous responses to the policy and spillover effects.

The U.S. was an early leader in educational expansion (Garfinkel et al. 2010) and experienced rapid economic development around the same period as the compulsory schooling laws examined here (per capita GDP increased by 213% from 1850 to 1918; Bolt and van Zanden 2014). Thus, an additional limitation is that the evidence presented here reflects a particular historical context and expansion of higher levels of education could have different implications, given differences in migration patterns by level and field or quality of education (Faggian and Franklin 2014; Spring et al. 2016; Venhorst et al. 2010).

Despite these limitations, this study contributes to our understanding of migration and education. First, evidence that early compulsory schooling laws increased the likelihood of leaving one's birth state could help explain changing rates of inter-state migration in the U.S. For example, Spring et al. (2016) document a decreasing and then increasing proportion of the U.S. population living outside one's state of birth from 1850 to 1990, with the lowest rate in 1890. As compulsory schooling laws applied to a larger share of birth cohorts and states over each decade, they may have gradually shifted the trend, ultimately contributing to an increasing rate of inter-state migration in the whole U.S. population.

Second, this study may help make sense of two apparently contradictory trends in migration. While inter-state migration rates increased throughout the 1900s, the proportion of the population experiencing residential mobility in the previous 1 or 5 years has declined since the mid-1900s (Spring et al. 2016). These apparently contradictory mobility patterns could reflect different implications of education for migration by educational level and by economic context. Consistent with our results, expansion of primary education may particularly encourage migration by providing skills and resources that make moving easier and by developing new ideas, goals, and opportunities that make moving more attractive (Levy and Wadycki 1974). Similarly, expansion at the lowest non-universal level of education could yield the greatest impact (Walters and Rubinson 1983). The different trends could also reflect changing economic contexts. Declining residential mobility following a long period of increasing educational attainment is consistent with Zelinsky's (1971) seminal study, which suggested that internal migration increased during the process of modernization and decreased in advanced societies. Consistent with this theory, our findings suggest that early compulsory schooling laws may be particularly related to migration away from rural areas in the context of industrialization.

Third, our results offer potential implications for efforts to achieve Universal Primary Education (UPE), a United Nations Sustainable Development Goal. Anticipated effects of UPE typically include economic and health benefits, but our results suggest that primary school expansion could encourage internal migration—including movement off the farm. Rapid urbanization in developing countries has raised concerns about inadequate resources, violence, and social instability (Mughah 2012; Vidal 2014). Our results suggest primary educational expansion may encourage migration, including off the farm, which could have economic and social implications.

Finally, we extend Baker's (2014) argument about the contemporary institutional importance of education to an earlier period. Our findings contribute to evidence that education has important and independent effects on society (Baker 2014; Fischer and Hout 2006; Lutz et al. 2008; Lutz and Samir 2011; Rauscher 2015).

Conclusion

Over the last century, education became increasingly important for many aspects of life (Baker 2014; Fischer and Hout 2006). Our results extend this institutionalist argument, suggesting that expansion of a lower level of education—through

compulsory schooling—increased the likelihood of internal migration across a state boundary. If UPE has similar implications for internal migration in developing contexts, it could reduce regional and local incentives to invest in education. Local funding and support for schools may have spurred early educational expansion in the U.S. (Lindert 2004). However, our results suggest that national education funding may be a more efficient strategy for developing countries to facilitate expansion. That is, regional or local governments may have a disincentive to invest in education if it makes youth more likely to leave the area. Furthermore, sensitivity analyses indicate that the effects of compulsory schooling on internal migration are stronger for men in states with lower occupational status (Online Appendix Table A11). This suggests areas that could benefit more from educational expansion may have a weaker economic incentive to expand schooling. National funding for education could help prevent these regional differences from hindering educational expansion. In the context of Africa, simultaneous removal of school fees for many or all countries could help spur educational expansion without risking brain drain.

Similarly, our results suggest that the federal government, rather than individual states, should provide at least some of the funding necessary to extend the compulsory schooling age to 18 in the U.S. Because additional schooling may increase the likelihood of migrating across a state boundary, states have a disincentive to require (and fund) extended compulsory schooling. To reap the potential benefits of extended schooling (e.g., for crime, health, occupational structure, and economic growth; Lochner and Moretti 2004; Lleras-Muney 2005; Rauscher 2015; Lutz et al. 2008), the federal government may need to incentivize states to make up for the potential effects of extended compulsory schooling on internal migration.

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