Effects of Early U.S. Compulsory Schooling Laws on Educational Assortative Mating: The Importance of Context

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Abstract
Modernization theory predicts rising education should increase assortative mating by education and decrease sorting by race. Recent research suggests effects of educational expansion depend on contextual factors such as economic development. Using log-linear and log multiplicative models of male household heads ages 36 to 75 in the 1940 U.S. census data, the first census with educational attainment information, I investigate how educational assortative mating changed with one instance of educational expansion: early U.S. compulsory school attendance laws. To improve on existing research and distinguish effects of expansion from changes due to particular years or cohorts, I capitalize on state variation in the timing of these compulsory laws (which ranged from 1852 to 1918). Aggregate results suggest compulsory laws had minimal impact on assortative mating. However, separate analyses by region (and supplemental analyses by race) reveal that assortative mating by education decreased with the laws in the South, but increased in the North. Whether due to economic, legal, political, or other differences, results suggest the implications of educational expansion for marital sorting depend on context. Contemporary implications are discussed in light of President Obama's suggested extension of compulsory schooling.

Introduction

Inequality has grown in recent decades (Piketty 2014; Keister 2000; Piketty and Saez 2003; Harrison and Bluestone 1988; Morris and Western 1999) with implications for children and future generations (Duncan and Murnane 2011; Ermisch et al. 2012; Conley 2001; Yeung and Conley 2008). Partly due to diverging trends in marital patterns between parents with high and low levels of education, McLanahan (2004) notes the growing gap in resources and outcomes for children. As returns to education rise (Goldin and Katz 2008), children of parents with low levels of education are particularly marginalized. Perhaps increasing education among disadvantaged youth could help change marital patterns to reduce child inequality.

In January 2012, President Obama called for states to extend compulsory schooling to age 18. Evidence from 13 countries suggests educational expansion is associated with increased educational assortative mating[1] (Blossfeld and Timm 2003; Blossfeld and Drobnic 2001). Much of this expansion, however, occurred at the post-secondary level. Would expansion at the bottom of the educational distribution yield similar effects on assortative mating?

Because a compulsory school extension would affect those most likely to drop out of school, lengthened compulsory schooling would increase educational attainment most among disadvantaged and minority youth (Angrist and Krueger 1991; Oreopoulos et al. 2006; Oreopoulos 2006). How might this type of educational expansion – raising the minimum – affect educational assortative mating?

Research on the equalizing effects of education typically focuses on the labor market (Hout 1988; Torche 2011; Brand and Xie 2010). Yet educational expansion may have important marital implications as well. Marital patterns play a central – and arguably increasing – role in

[1] Assortative mating is the non-random sorting of marital partners with implications for intra- and inter-generational inequality.

inequality (Wu and Wolfe 2001; Ellwood and Jencks 2004; Looney and Greenstone 2012; Thompson 2013; Greenwood et al. 2014). Economic standing explains much marital inequality (Sweeney 2002; Goldstein and Kenney 2001; Oppenheimer et al. 1997; Lloyd and South 1996; Goldscheider and Waite 1986), but certain aspects of economic standing, such as education, may be more amenable to policy change than others. If educational expansion among disadvantaged or minority youth increases interaction among children from various class and racial backgrounds (Rauscher 2014), then extended compulsory schooling could increase marital sorting on education and reduce sorting on class, race, or ethnicity (Blossfeld and Timm 2003). Coupled with increased education for those at the bottom of the distribution, this pattern of marital effects could reduce childhood inequality (McLanahan 2004). Alternatively, extended education for disadvantaged youth may encourage upper class youth to stay in school longer, leaving educational assortative mating largely unchanged. Additional research is required in order to understand the potential marital implications of expanded education for disadvantaged youth.

One difficulty of establishing a causal relationship between education and assortative mating is ruling out alternative explanations. For example, rising demand for educated workers may have encouraged 20th century trends of increasing educational attainment and educational sorting in the marriage market due to the attractiveness of partners with high earning potential. Because most studies of assortative mating rely on cohort change (e.g., Blossfeld and Timm 2003; Kalmijn 1991; Rosenfeld 2008), existing demographic research has difficulty distinguishing between educational effects and cohort or other changes. In other words, existing research is largely descriptive. Establishing a causal estimate of the relationship requires a change in education that is independent of such temporal or cohort changes.

To partially address these methodological difficulties, I capitalize on state variation in the timing of early U.S. compulsory school attendance laws – the first law in each state requiring children in a given age range to attend school for a minimum number of weeks each year. By using these laws, I move beyond descriptive techniques and estimate effects on educational assortative mating. Because the timing of these laws varied across states, results approach a causal estimate and do not simply reflect changes particular to specific years or cohorts.

Early compulsory laws were passed by states in the late 1800s and early 1900s. Effects of extended compulsory schooling may certainly differ today, but there are several motivations to study effects of early laws. First, recent research finds evidence that educational sorting in the U.S. remained relatively stable despite educational expansion (Timm et al. 2003), but increased in other countries (Blossfeld and Timm 2003). Perhaps education held more power to influence sorting in the U.S. at the turn of the century, when educational attainment was lower, the timing of marriage and economic independence depended less on schooling, and traditional gender roles were stronger. Second, economic inequality is high today, mirroring levels experienced around the turn of the century (Piketty and Saez 2003; Alvaredo et al. 2013). If educational effects on marital sorting depend on inequality, the late 1800s and early 1900s offer a good comparison for today. Third, contemporary expansion is largely at the post-secondary level, but extended compulsory education (suggested by President Obama) would primarily influence low income and minority youth at the bottom of the distribution, with less than a high school degree. Similarly, compulsory laws at the turn of the century affected lower class and minority youth at the bottom of the distribution, who were not already attending school (Rauscher 2014). Finally, state variation in the timing of the laws offers a unique opportunity to distinguish effects of educational expansion from cohort change.

Capitalizing on this opportunity and the useful comparisons it offers for contemporary debates, I ask whether early U.S. compulsory school attendance laws – which increased educational equality by race and class background – influenced the strength of spousal similarity by education. Below I provide a brief overview of the laws, theoretical and empirical background, details about data and methods, results, and a discussion including potential theoretical and contemporary implications.

Early Compulsory Education Laws: A Brief Overview

Compulsory school attendance laws began with Massachusetts in 1852. Other states in New England and the North followed more quickly than the South, but by 1918 (when Mississippi passed the law) all states had made attendance compulsory.[2] Compulsory laws aimed to achieve universal school attendance and were primarily directed at lower class and immigrant families who did not already send their children to school. For example, the Commissioner of Education (1891:493) 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?" Opposition to compulsory schooling reportedly 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: 520). Compulsory schooling laws aimed to override "irresponsible" parents and increase attendance among lower class and immigrant youth (Perrin 1896; Moore 1902).

[2] The year school attendance became compulsory in each state is shown in supplementary Table S1.

Despite these efforts, compulsory laws were not perfectly enforced. By introducing potential punishments for non-attendance, however, they encouraged attendance. Consistent with the targeting of lower class youth, evidence suggests the laws increased educational equality by race and social class background, boosting attendance and attainment most among lower class and minority youth (Rauscher 2014). By expanding who attended school, theory suggests the compulsory laws should also have increased exposure among children from varying class and racial backgrounds.

Empirical and Theoretical Background

Modernization Theory

According to industrialization or modernization theory, as a society modernizes ascribed characteristics such as race become less important for life chances while achieved characteristics become more important (Treiman 1970). Educational expansion is a central feature of industrialization, meeting the growing demand for skilled and socialized workers. To remain competitive, industrialization theory suggests employers in a modernizing society increasingly rely on educational achievement rather than ascribed characteristics. Potential marriage partners may similarly rely increasingly on behavioral rather than ascribed characteristics. According to modernization theory, educational expansion should increase social exposure (and therefore intermarriage) among individuals from different socioeconomic and racial backgrounds (Rosenfeld 2008).

However, as more youth attend school and as that education trumps ascribed characteristics in determining earning potential, modernization theory also suggests individuals on the marriage market should rationally value the education (and therefore earning power) of a potential spouse more than his or her class or race. In other words, modernization theory

suggests early compulsory schooling laws should raise the earning potential of non-white and lower class youth (those most affected by the laws) while also increasing their exposure to potential partners from other social backgrounds (Rosenfeld 2008), thereby reducing assortative mating by race and class but increasing educational sorting. Consistent with modernization theory, therefore, **Hypothesis 1** is that early compulsory laws should increase marital sorting by education level.

Spousal similarity has implications for inequality and social boundaries (Schwartz 2013; Kalmijn 1998). By pooling resources, spousal similarity can increase income disparities between households (Schwartz 2013; Burtless 1999). In this way, assortative mating could strengthen the transmission of inequality across generations (Fernandez and Rogerson 2001; Kremer 1997). In addition, racial, ethnic, or religious similarity of spouses maintains social segregation along these boundaries, preventing integration and perhaps between-group understanding (Kalmijn 1998; Johnson 1980).

Given its importance for inequality, a great deal of research investigates how the strength of assortative mating varies across countries and over time, often implicitly or explicitly testing modernization theory (Kalmijn 1991; Rosenfeld 2008; Ultee and Luijkx 1990; Blossfeld 2009). For example, in support of industrialization theory, Kalmijn (1991) finds that education became increasingly important for spousal selection in the 20th century U.S., while religion became less important. Other research finds that educational expansion is associated with increased educational assortative mating in multiple countries (Blossfeld and Timm 2003).

Despite its theoretical appeal, however, research is not always consistent with modernization theory. For example, Rosenfeld (2008) finds relatively stable marital sorting by education and little relationship between education and marital sorting by race over time.

Similarly, reviews of the literature note that cross-national trends in educational assortative mating are not always consistent with modernization theory (Schwartz 2013; Blossfeld 2009). In particular, changes over time in the strength of educational assortative mating depend on education level (Schwartz and Mare 2005), measurement choices (Hou and Myles 2008; Wong 2003), or context (Blossfeld and Timm 2003).

The mixed evidence for modernization theory could reflect different effects depending on the level at which education expands. For example, expansion at the top of the educational distribution may carry different implications for assortative mating than expansion at the bottom of the distribution.

Level of Education

Schwartz and Mare (2005) find intriguing evidence that the strength of educational sorting by level of education varies over time. Specifically, they find that rising educational homogamy in the 1960s reflected an increased likelihood of college graduates to marry each other. From the 1970s, however, they find increased educational sorting at both the top and bottom of the distribution, with greater intermarriage at the middle of the distribution (i.e. between high school graduates and those with some college). In a similar vein, cross-national research finds that trends in educational homogamy depend on the number of educational levels and the educational distribution in each country (Blossfeld and Timms 2003).

Thus, it seems likely that effects of educational expansion may differ depending on the level of education that expands, the proportion of people with that level, the overall distribution of education, and other contextual factors. Therefore, if extended contemporary compulsory schooling laws increased educational attainment at the bottom of the distribution, effects on assortative mating may differ from effects of post-secondary expansion.

Existing research largely focuses on trends associated with expansion at the post-secondary level (Blossfeld and Timms 2003). Early compulsory schooling laws offer a unique opportunity to better understand the potential consequences of expansion at the bottom of the distribution. While the historical context certainly differs from the contemporary context, this analysis provides useful similarities (high inequality and rapid economic change) and allows the comparison of two contexts (North and South) within the same country.

Context

Compulsory effects on marriage markets could differ depending on context (Blossfeld and Timm 2003). For example, according to modernization theory, education should hold more importance in more industrialized contexts, such as the Northern U.S. around the turn of the century. If the value of education was higher in the industrialized North, educational expansion may have increased educational homogamy there while leaving it relatively unchanged in the more agricultural and traditional South. Alternatively, segregation or limited availability of school spots[3] despite the compulsory laws (Walters 2000) may have paradoxically maintained racial homogamy and limited educational homogamy in the South. **Hypothesis 2** therefore suggests that early compulsory laws should increase educational assortative mating in the North, but have no effect or reduce it in the less developed South.

Another potential explanation for contextual variation is that the function of education may have differed in the two contexts. As several authors argue, education may have served capitalist and elite interests in industrialized areas by reproducing inequality, preparing working class youth for obedience and elite youth for power (Bowles and Gintis 1976; Spring 1972; Field

[3] Segregation would prevent interaction among social groups. Limited availability of school spots would maintain educational inequality, limiting social exposure among groups and returns to education for disadvantaged youth.

1976; Cookson and Persell 1985; MacLeod 1995). If education emphasized social position in the more industrialized North, compulsory laws in those states may have strengthened assortative mating on ascribed characteristics. In contrast, educational expansion may encourage economic development and rationalization in less developed contexts (Lutz et al. 2008; Krueger and Lindahl 2001; Barro 2001, 1997), potentially lowering social boundaries. Consistent with these scenarios, **Hypothesis 3** suggests compulsory laws should reduce racial assortative mating in the less developed South, but increase it in the North. By comparing results in two contexts within the same country, this study cannot tease apart the relative importance of various factors, but does provide additional information about contextual factors that may moderate effects of educational expansion.

While a great deal of research focuses on the latter half of the 20th century (Timm et al. 2003; Schwartz and Mare 2005), studying early compulsory attendance laws shifts to an early stage of U.S. economic development. Smits et al. (1998) suggest economic development may be associated with weakening educational assortative mating at later stages of development, but rising educational homogamy at earlier stages. Thus, examination of earlier cohorts makes results more likely to support modernization theory. By focusing on earlier expansion, therefore, this study sets up a stronger test of modernization theory than is generally possible in more recent research.

Coupled with evidence that assortative mating is more complex than modernization theory suggests (Smits et al. 1998), the above discussion of the importance of education level and context suggest alternative methods and more detailed theories may be required to understand assortative mating (Schwartz 2013).

Methods

While a great deal of research investigates how the strength of assortative mating varies over time, less research has identified specific factors that strengthen or weaken homogamy (Smits et al. 1998). The empirical focus on trends limits the ability to identify specific factors that strengthen or weaken assortative mating (Smits et al. 1998). Thus, even if researchers achieved a consensus about trends in homogamy, we would not know what drove those trends. Changes could reflect modernization, economic growth, educational expansion, or other factors.

Because existing research documents trends in education and marital sorting, it cannot distinguish effects of education from cohort effects or other changes over time. For example, rising returns to schooling could drive both rising education levels and greater educational similarity of spouses. Cohorts raised during an economic recession could have both lower education levels and less assortative mating for reasons other than their educational attainment.

Moving beyond descriptive research requires distinguishing effects of educational expansion from changes that occurred in particular cohorts or years. To achieve this distinction, I capitalize on state variation in the timing of compulsory schooling laws. Pooling across states, I compare those within a narrow window on either side of the compulsory cutoff. Because these cutoffs occur in multiple years, differences do not represent changes in a particular time period or cohort. Thus, this method provides an estimate of the effect of educational expansion distinct from changes that occurred in particular years or cohorts. At the individual level, children could not opt into or out of compulsory assignment, which depended on state of residence and age at the time of the law.

Compulsory schooling laws have been used as a natural experiment in a variety of studies, including those estimating effects of education on longevity (Lleras-Muney 2005), earnings (Angrist and Krueger 1991; Oreopoulos 2006), child educational outcomes (Oreopoulos

et al. 2006), and fertility (Puerta 2009). Many of these analyses estimate the effect of compulsory assignment in the context of a multivariate instrumental variable regression. However, two criticisms of this approach to using compulsory laws as a natural experiment are that it: 1) is biased when the first stage (e.g., effect of the compulsory law on schooling) is weak; and 2) answers a narrow question and estimates the local average treatment effect for a limited group who is influenced by the laws (Angrist and Pischke 2009). This analysis, however, is explicitly interested in the effect of the laws on disadvantaged youth who are most affected by the laws. Furthermore, previous research shows a significant effect of early compulsory laws on schooling (Rauscher 2014; Puerta 2009). Most importantly, the approach used here represents an improvement over descriptive methods while maintaining the benefits of log linear models.

The field of assortative mating has long used log linear models (Johnson 1980). To maintain comparability with existing research, I use log multiplicative models to compare the strength of association by compulsory assignment category within a narrow window of the compulsory cutoff (discussed in more detail in the methods section). While the comparison groups are similar to those used in an instrumental variable analysis, this approach does not include controls and provides the equivalent of a reduced form (or intent-to-treat) estimate of the effect of compulsory assignment on assortative mating.

To summarize the above review, research offers mixed support for the ability of modernization theory to explain changes in assortative mating or its relationship with education. However, this could reflect a focus on expansion at higher levels of education, the particular time and context studied, or reliance on associational methods, among other things. By investigating effects of early compulsory laws, this study contributes to existing research in several ways. First, it investigates the consequences of expansion at the bottom of the educational distribution,

the section most relevant for contemporary discussions of extended compulsory schooling. Second, it investigates effects of expansion around the turn of the century, earlier than most existing research, when inequality was high, the timing of marriage and economic independence depended less on schooling, and traditional gender roles were stronger. Third, it compares results in two contexts within the same country – the U.S. North and South – to offer additional evidence about which contextual factors may moderate the relationship between educational expansion and assortative mating. Fourth, it improves on descriptive research by providing an estimate of the effect of an early instance of educational expansion (compulsory schooling laws) on assortative mating net of specific year or cohort effects. By focusing on the effects of early educational expansion on disadvantaged youth in two contexts within the U.S., this study sets up a strong test of modernization theory. Results of this analysis will provide more information about the contexts in which modernization theory might explain assortative mating patterns.

Data and Methods

Data

The first U.S. census with educational attainment information was 1940. Individual 1940 census data from IPUMS provide large sample sizes, with educational attainment for each household head as well as their spouse for those currently married. The sample is limited to household heads who were married in 1940 and who were within a narrow window of the compulsory assignment cutoff. That is, I include individuals who either just missed being required to attend school (i.e. were one to five years beyond the age legally required to attend school) or who were up to ten years beneath the compulsory cutoff (i.e. were required to attend school for up to ten years). To insure that each cohort includes some individuals required and not required to attend school, the sample is limited to those ages 36 to 75 in 1940. This excludes

individuals born after 1904, who were school-age after the last state made attendance required and were therefore all required to attend school. This study therefore includes individuals born between 1865 and 1904.

Unfortunately, spousal information is not available for widowed, divorced, or deceased individuals, which could bias results. For example, non-homogamous marriages may be more likely to dissolve (Tzeng 1992) or individuals required to attend school may live longer (Lleras-Muney 2005). While I cannot fully address these potential concerns, limiting the sample to individuals within five or ten years on each side of the compulsory cutoff reduces concern about bias due to differential mortality. In addition, divorce was rare in the early 1900s and therefore has limited ability to bias results. As shown in Table S2, only 1% of those who would otherwise appear in the sample were divorced, 4% were widowed or never married, and 91% were married.

Table 1 provides summary statistics. Comparing the proportion required to attend school in Tables 1 and S2 further reduces concern. Whether including or excluding individuals who were not married in 1940, the proportion required to attend school is 67%.

[Table 1]

Combining state of birth, year of compulsory education law, ages at which the state law required attendance, and birth year, compulsory assignment is a dummy variable that indicates whether an individual was ever school age after the law passed.[4] In most states, compulsory

[4] The compulsory assignment indicator is based on state of birth. Individuals may not have remained in their birth state throughout the time they were school-age, which would introduce error to the compulsory assignment measure and increase the likelihood of underestimating effects if this migration were random with respect to marital sorting and compulsory assignment. If, however, families were more likely to move if their child was required to attend school then the direction of the bias would depend on the characteristics of those families and whether those children are likely to partner with more or less similar spouses. I take two steps to reduce concern about this potential bias. First, I repeat analyses using a compulsory assignment indicator based on current state of residence and find similar results. This compulsory assignment measure is less desirable than that used in the main analysis because education could encourage migration to another state (e.g., for job or other opportunities), which could mediate effects of compulsory attendance on marital sorting. Second, I use IPUMS Linked Representative Samples to estimate the prevalence of inter-state migration among families with children in the late 1800s and compare migrant and non-migrant families with children on several demographic measures. Presented in Table S3, the data

laws required attendance for children 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. Even when the law went into effect the year it passed, many children would be a year older by the start of the next school year. Therefore, in creating the indicator for compulsory assignment, individuals who were the maximum age at which attendance was required when the law was passed are not considered part of the compulsory group. 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.[5]

Table 1 includes means for the whole sample as well as separately by compulsory category, including those within five years beyond the compulsory cutoff who were never required to attend school, those required to attend who were 1 to 5 years beneath the cutoff, and those required to attend who were 6 to 10 years beneath the cutoff. Comparisons reveal that age and birth year are similar across compulsory categories.

Education level ranges from one to five. Based on highest grade of schooling completed, level one includes those who did not complete first grade, level two includes those who completed at least first but less than eighth grade, three includes those who completed eighth grade, four includes more than eighth but less than twelfth grade, and five includes those who completed at least twelfth grade. Spouse education level is measured using the same categories. Table 2 shows the percentage of individuals and spouses falling in each category. Although the

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are consistent with limited potential bias due to selective migration. Most importantly, inter-state migration among families with children is independent of the timing of state compulsory schooling laws.

[5] Another reason for choosing this cutoff, historians have noted that early compulsory laws were poorly enforced and employment was common and even encouraged among older youth around age 13 or 14 (Tyack 1974). Compulsory laws are therefore more likely to influence younger children under the maximum age required at the time of the law, when social norms and any child labor laws may have restricted employment.

highest percentage falls in category two, the number of observations who have completed each grade in that level (first through seventh) is fairly evenly divided, making it difficult to disaggregate this category. In contrast, approximately 28% of individuals completed 8th grade alone, so I treat 8th grade as a separate category.

[Table 2]

Table 2 shows that spouse education level is slightly higher than individual education level. This suggests that, on average, men may have married spouses with slightly higher education levels than their own in the early 1900s. This difference highlights a potential complication of examining a dyadic outcome (i.e. similarity of married couples). Although compulsory laws influenced school attendance rates and educational attainment of both men and women, research suggests the effects in some cases differed by gender (Rauscher 2014). Log-linear and log multiplicative models control for marginal distributions, meaning they account for potential gender differences in the distribution of education. Moreover, while results do not generalize to individual outcomes, the aim of this paper is to understand implications for assortative mating.

Returning to Table 1, contrary to age and birth year, education level varies by compulsory category. As should be the case, education level is higher among those required to attend for longer. Spouse's education also increases across compulsory category. The proportion whose spouse has a higher level of education remains stable across compulsory category. However, the proportion with a less educated spouse slightly increases while the proportion with a similarly educated spouse (educational homogamy) slightly decreases among those required to attend longer.

Analysis

State differences in the timing of compulsory school attendance laws provide variation across cohorts in whether an individual was required to attend school or not. An individual born in 1864, for example, would have been 10 years old when Kansas required school attendance, but 23 years old when Nebraska required attendance. He would have been required to attend school if he lived in Kansas, but not if he lived in Nebraska.

While assortative mating was likely changing over time regardless of compulsory laws, I focus on a narrow window of birth cohorts on each side of the compulsory assignment cutoff. Within that narrow window, estimated effects of compulsory assignment rest on the assumption that individuals are similar except for whether or not they were required to attend school. Because state differences in timing ensure cohort variation in compulsory assignment, cohort-specific differences (such as war or economic recession) should not bias estimates.[6]

The analysis involves three steps. First, I compare the proportion married to a similarly educated spouse across compulsory categories. This paints a rough picture of how homogamy may have changed with compulsory laws. Second, I use log linear models to determine the model that best fits the aggregate distribution (combined data across compulsory categories and regions). Third, to provide a more detailed understanding, I use log multiplicative models to analyze the three-way relationship between individual education (5 categories), spouse's education (5 categories), and compulsory assignment category (3 categories). These models allow flexible analysis of spousal similarity, controlling for marginal distributions. In addition, I

[6] A potential concern is that estimates could simply reflect trends in assortative mating. As modernization theory suggests, educational assortative mating may have been increasing across cohorts with economic development (Smits et al. 1998). Because individuals required to attend school were necessarily in later cohorts than those never required to attend, estimates could simply reflect these trends. This scenario makes results more likely to support modernization theory, offering a stronger test of that theory. The narrow cohort window on either side of the compulsory cutoff and the representation of multiple cohorts in each compulsory category also help address this concern.

disaggregate the data by region (North and South) and compare results by region to test hypotheses two and three.

In step two, I compare fit statistics across multiple models to choose the specification that best captures the relationship between individual and spouse characteristics. Bayesian information criteria (BIC), Akaike information criteria (AIC), and deviance measures are used to select the model best balancing data fit and parsimony (Raftery 1986; 1995). BIC alone is insufficient because it may overvalue parsimony or simpler models (Weakliem 1999). While log linear models are appealing because of their flexibility, log multiplicative (also called uniform difference) models offer a more parsimonious estimate of whether the strength of assortative mating varies by compulsory category (Xie 1992; Erikson and Goldthorpe 1992). These models classify individual education by spouse's education by compulsory category, hypothesizing that spousal similarity is constant regardless of compulsory assignment. The log multiplicative model can be written:

$$F_{ijk} = \tau\tau_i^{R}\tau_j^{C}\tau_k^{L}\tau_{ik}^{RL}\tau_{jk}^{CL}\exp(\psi_{ij}\phi_k) \quad (1)$$

where the τ parameters control for marginal effects for rows (R, individual education category), columns (C, spouse's education category), and layers (L, compulsory category), and their two-way interactions to best predict the number of observations in each individual-spouse-layer category (Powers and Xie 2008:110-1). The ϕ parameters (of primary interest) measure the strength of individual-spouse association (assortative mating) by compulsory category. The local log odds of having a spouse in education category *j* given individual category *i* for compulsory group *k* is calculated as:

$$\log(\theta_{ij|k}) = (\psi_{ij} + \psi_{(i+1)(j+1)} - \psi_{i(j+1)} - \psi_{(i+1)j})\phi_k = \phi_k \log(\theta_{ij}) \quad (2)$$

Supplemental Analyses of Racial Assortative Mating

Beyond effects on educational homogamy, modernization theory suggests educational expansion should also reduce assortative mating on ascribed characteristics such as race. As a further test of modernization theory, I therefore present supplementary analyses of racial assortative mating. Because of the small proportion of interracial marriages at the time, however, this information is merely suggestive and results should be interpreted with caution. Analyses follow the same procedures described above, using the same 1940 census data but replacing educational category with racial category.[7]

As shown in Table 1, the proportion of white individuals and spouses increases across compulsory categories. The proportion with a same race spouse rounds to 100% in all categories. This limits variation for analyses of assortative mating by race, making models that fit the diagonal cells more likely to provide the best fit for the data. Nevertheless, there is a small amount of variation and log linear models are equipped to deal with small cell values, provided the overall sample size is large enough. Log linear models require a sample size at least five times the number of cells. Race is specified in three categories: white, black, and Native American or Asian. The table of individual race (3 categories) by spouse race (3 categories) by compulsory category (3 categories) holds 27 cells, yielding a minimum sample size of 135. The sample used here is 40,697, providing more than enough cases. Therefore, while higher rates of interracial marriage might provide more information, log linear models are equipped to deal with such situations. One limitation of the small number of racial categories, however, is that there are fewer degrees of freedom for the analysis of racial sorting. Therefore, fewer models are applicable or suitable for analysis of racial assortative mating.

[7] Supplemental analyses of 1910-1930 census data yield similar results for assortative mating by race. Educational assortative mating cannot be investigated in these census years because educational attainment is unavailable.

Results

Educational Assortative Mating

Table 3 compares the proportion of men married to a spouse with the same education level – among those in all states as well as in the North and South. Figure 1 illustrates these statistics for men in all states, showing that the proportion married to a spouse with a similar education level declines across compulsory categories (from 58% to 56%), while the proportion married to a spouse with a lower level of education increases (from 15% to 17%). The proportion married to a spouse with higher education is consistently higher than the proportion married to a less educated spouse, but this figure remains constant across compulsory categories (27%). At the same time, Table 3 suggests there may be regional differences. In the South, educational homogamy decreases across compulsory category, while it tends to increase in the North. Based on these mean comparisons, there appear to be changes in assortative mating with the compulsory laws and these changes may vary by region.

[Table 3, Figure 1]

To better understand the effects of the laws, Table 4 presents results of log-linear regression analyses of aggregate data (collapsed by compulsory category and region). I compare fit statistics across a variety of models (including independence, row effects, column effects, uniform association, quasi-symmetry, crossings, and row-column effects) to determine which best captures educational assortative mating patterns. Comparing deviance, BIC, and AIC statistics clearly shows that the quasi-symmetry model best fits the distribution. I therefore use this model to examine variation in the strength of assortative mating by compulsory category.[8]

[8] The quasi-symmetry model allows marginal distributions to vary, while the distribution is constrained to be symmetric across the diagonal and each diagonal cell (where individual and spouse education level is equal) is specified. The quasi-symmetry model is also the best-fitting log-multiplicative model in the disaggregated data, so results from other models are not presented.

[Tables 4 and 5]

Table 5 presents the results of log-multiplicative analyses, which measure the strength of assortative mating by education across compulsory categories. Null effect models restrict the strength of spousal similarity to be the same across compulsory categories, while the multiplicative effect models allow the strength of assortative mating to vary (using two additional degrees of freedom). Table 5 shows model fit statistics, including the likelihood-ratio χ^2 statistic (G^2), BIC, and the index of dissimilarity (DI), which measures the proportion of observations that must be moved for a model to fit the data perfectly.[9] In every case, the null effect model fits the data better than the multiplicative effect model according to the BIC statistic. However, BIC penalizes large sample sizes more for each additional degree of freedom and, particularly with the large sample size used here, may overvalue simpler models (Powers and Xie 2008; Weakliem 1999). In contrast to BIC, the likelihood-ratio and the index of dissimilarity consistently suggest that allowing the strength of assortative mating to vary by compulsory category improves model fit.[10]

More important for this analysis is how the strength of assortative mating changed with the laws. The ϕ parameters measure differences in the strength of individual-spouse association by compulsory category. I present ϕ parameters scaled to 1.0, which does not change the model (Xie 1992:382), but eases interpretation and comparison across models. Values higher than 1.0 indicate stronger association than the non-compulsory group; values lower than 1.0 indicate weaker association. Illustrated in Figure 2, the estimates for all states suggest the compulsory laws had minimal effect on assortative mating by education. If anything, compared to men never

[9] For each of these statistics, smaller values indicate better fit of the data.
[10] Based on an increase of two degrees of freedom, the difference in G^2 only reaches significance at $p<0.05$ when predicting the distribution by education in the North. Nevertheless, there is some evidence that allowing spousal similarity to vary by compulsory category improves model fit.

required to attend, those required to attend for up to five years may have experienced slightly higher assortative mating by education, while those required to attend longer experienced slightly lower homogamy. Thus, results for all states do not offer a clear answer to hypothesis one.

[Figure 2]

Comparing results by region, however, yields a clear pattern. The ϕ parameters are higher than 1.0 in the North, but lower than 1.0 in the South. Consistent with hypothesis 2, this suggests early compulsory laws reduced educational assortative mating in the less developed South, but increased it in the North.

When interpreting the ϕ parameters, only the ratio is of interest not the magnitude (Xie 1992). Thus, compared to those never required to attend, spousal educational association in the North was 9% stronger among men required to attend school (for either 1-5 years or 6-10 years). In contrast, men in the South who were required to attend for 1-5 years had 4% weaker educational association with their spouse than those never required to attend. Educational assortative mating was 6% weaker for Southern men required to attend 6-10 years.

Education results therefore suggest that effects of educational expansion depend on context. Early compulsory laws had no aggregate effect on marital sorting. Rather, consistent with hypothesis two, these early laws increased educational assortative mating in the North, but reduced it in the less developed South.

Racial Assortative Mating

Similar to Table 3 for the educational analyses, Table 6 shows the proportion of men married to a spouse of different race by compulsory category and region (illustrated in Figure 1).

In all regions and categories, this rate of interracial marriage is less than 1%. To ease interpretation, these figures are multiplied by 10. Though tiny, the proportion married outside their racial group more than doubles from the non-compulsory group to those in either of the other two compulsory categories. Because this pattern of increasing racial exogamy (marriage outside one's social group) is similar among those in the North and the South, it could reflect a long-term trend toward increasing interracial marriage. However, historical analysis reveals that the likelihood of interracial marriage actually declined until around 1930 and only increased with the decline of Jim Crow laws and the Civil Rights era (Gullickson 2006). Thus, rising interracial marriage with the compulsory laws contradicts the temporal trend.

[Tables 6 and 7]

Equivalent to Table 4 in the educational analyses, Table 7 presents a comparison of model fit statistics for the racial sorting data. The distribution by race has only three categories and therefore fewer degrees of freedom. As shown in Table 7, several models are identical and therefore fit the data equally well. The best fitting models (in both the log and log-multiplicative models) include quasi-symmetry, the same model used for the education analyses. Therefore, similar to the analysis of educational sorting, I only present log-multiplicative results from the quasi-symmetry model of racial sorting.

Shown in Table 8, results of the log-multiplicative analyses show that BIC does not suggest an improvement in model fit when allowing the strength of assortative mating to vary by compulsory category. However, similar to the findings for educational sorting, G^2 consistently suggests that allowing the strength of racial sorting to vary by compulsory category improves model fit. Based on an increase of two degrees of freedom, the difference in G^2 does not reach

significance at $p<0.05$. Nevertheless, there is some evidence that allowing spousal similarity to vary by compulsory category improves model fit.

Finally, the ϕ parameters suggest that racial assortative mating remained similar before and after the compulsory law when including all states. However, similar to the pattern for education and consistent with hypothesis three, the strength of racial sorting increased in the North by 8% for those required to attend 1-5 years and by 4% if required to attend 6-10 years. In the South, however, the strength of racial sorting decreased by 2% (1-5 years required) and 6% (6-10 years) compared to those never required to attend. These differences are illustrated in Figure 2. Overall, results for racial assortative mating are consistent with those for educational sorting.

Conclusion

Improving on past research, this study works to isolate the relationship between expansion at the bottom of the educational distribution and assortative mating, net of specific year or cohort changes. By capitalizing on state variation in early compulsory schooling laws, this paper separates effects of educational expansion from other cohort changes At the same time, it sets up a strong test of modernization theory by shifting the focus to an early stage of U.S. economic development, when Smits and colleagues (1998) suggest educational assortative mating was rising.

Contrary to modernization theory, however, compulsory laws did not consistently increase assortative mating by education or reduce it by race. Rather, results suggest compulsory laws had minimal effect on assortative mating at the national level. Examining results separately by region, however, suggests this early instance of educational expansion impacted assortative

mating differently depending on context (consistent with hypotheses two and three). Possibly due to regional differences in economic development, educational expansion increased assortative mating in the North, but reduced it in the less developed South. Additional research is required to tease apart what aspect of context moderated the effect of educational expansion. Furthermore, these results are not definitive, because allowing association to vary by compulsory assignment does not improve model fit based on BIC statistics. However, modeling variation by compulsory category improves model fit according to two other measures and reveals a consistent pattern.

Coupled with existing evidence that assortative mating is more complex than modernization theory suggests (Smits et al. 1998; Schwartz 2013), results based on the alternative method used here suggest more detailed theories are required to understand assortative mating. Complicating modernization theory, results suggest early educational expansion impacted both educational and racial homogamy in the same direction, but this direction differed by region. Rather than increasing the importance of education over ascribed characteristics such as race, I find that compulsory laws had either an equalizing or an unequalizing effect on marital distributions, depending on context. In the more industrialized North, compulsory laws reduced equality of opportunity in the marriage market. In the more agricultural South, the laws increased equality.

Although results could reflect some other important contextual difference, this pattern is consistent with a potentially different role of education depending on economic development. In the more industrialized North, it may have primarily served capitalist interests and reproduced inequality (Bowles and Gintis 1976; Spring 1972; Field 1976). In contrast, educational expansion in the South may have encouraged economic development and rationalization (Lutz et

al. 2008; Krueger and Lindahl 2001; Barro 2001, 1997), potentially lowering social boundaries and increasing equality on the marriage market. Similarly, results are consistent with the possibility that educational expansion countered formal legal and political inequality in the South by increasing equality on the marriage market, but reduced marital equality in the North, where less formal rules maintained segregation.

Drastic changes since the period studied make it difficult to generalize results to today. For example, declining marriage rates, rising births to unmarried parents, and rising female labor force participation are likely to shape the relationship between education and marital patterns. Nevertheless, results imply that President Obama's suggested extension of compulsory schooling, which would increase attainment at the bottom of the educational distribution, may have little overall effect on assortative mating. In that case, extended compulsory schooling would have little effect on childhood inequality through marriage patterns.

At the same time, however, expansion could yield heterogeneous effects, increasing assortative mating in developed contexts while reducing it in developing contexts. Similar to results of this study, effects of extended compulsory schooling could vary within the U.S. Whether due to economic, legal, political, or other differences, findings suggest the marital sorting implications of expansion at the bottom of the educational distribution may depend on context. Further research is required to assess whether results generalize to contemporary levels of development in the U.S. If they do, results suggest the laws may reduce assortative mating in less developed areas, encouraging partnerships between spouses of different education levels or races but also increasing inequalities between families in more and less developed regions. Thus, while there are other important potential benefits, this historical evidence suggests extended compulsory schooling could amplify marital inequality in the U.S.

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Tables and Figures

Table 1: Descriptive Statistics

	Mean	Std. Dev.	Non-Comp	Comp 1-5	Comp 6-10
Education Level (1-5)	3.05	1.15	2.95	3.04	3.15
Spouse Education Level	3.22	1.16	3.13	3.22	3.31
Spouse Same Educ Level	0.58	0.49	0.58	0.58	0.56
Spouse Higher Educ Level	0.27	0.44	0.27	0.27	0.27
Spouse Lower Educ Level	0.16	0.36	0.15	0.16	0.17
White	0.87	0.34	0.81	0.88	0.92
Non-White	0.13	0.34	0.19	0.12	0.08
Spouse White	0.87	0.34	0.81	0.88	0.92
Spouse Non-White	0.13	0.34	0.19	0.12	0.08
Spouse Same Race	1.00	0.06	1.00	1.00	1.00
Age	49.10	9.90	49.24	48.92	49.13
Birth Year	1890.90	9.90	1890.76	1891.08	1890.87
Compulsory	0.67	0.47	0.00	1.00	1.00
N	40697		13606	13461	13630

Source: 1940 US Census.

Limited to male household heads age 36-75, married with spouse information, born in the US, and either 5 years beyond (Non-Comp = never required to attend) or up to 10 years under the compulsory cutoff (Comp 1-5 = 1-5 years compulsory attendance; Comp 6-10 = 6-10 years compulsory attendance).

Table 2: Percentage of Observations in Each Education Level

Education Level	Individual	Spouse
1 - None	2.74	1.6
2 - 1st-7th Grade	38.23	33.27
3 - 8th Grade	28.51	27.7
4 - 9th-11th Grade	12.65	16.31
5 - 12th Grade +	17.87	21.13
N	40697	40697

Source: 1940 US Census. Sample is the same as in Table 1.

Table 3: Spouse Education by Compulsory Category and Region

	Non-Comp	Comp 1-5 years		Comp 6-10 years	
All					
% Same Educ Level	0.58	0.58		0.56	*
% Lower Educ Level	0.15	0.16		0.17	*
% Higher Educ Level	0.27	0.27		0.27	
N	13606	13461		13630	
North					
% Same Educ Level	0.53	0.56	*	0.55	*
% Lower Educ Level	0.18	0.17		0.18	
% Higher Educ Level	0.29	0.27	*	0.27	*
N	5203	6682		8616	
South					
% Same Educ Level	0.61	0.60	*	0.58	*
% Lower Educ Level	0.13	0.14	+	0.15	*
% Higher Educ Level	0.26	0.26		0.27	
N	8403	6779		5014	

Significant difference from the non-compulsory category indicated by * $p<0.05$, + $p<0.1$.
Source: 1940 US Census. Sample is the same as in Table 1.

Table 4: Log-Linear Models of Aggregate Education Data – Fit Statistics

	Deviance	df	BIC	AIC	% Deviance Explained
Constant	57172.8	24	57,096	2295.1	0%
Independence	23496.5	16	23,445	948.6	59%
Row effects	4517.4	12	4,479	189.8	92%
Row effects, diagonals blocked	94.5	7	72	13.3	100%
Column effects	3988.2	12	3,950	168.6	93%
Column effects, diagonals blocked	212.4	7	190	18	100%
Uniform association	4950.4	15	4,902	206.9	91%
Uniform assoc, diagonals blocked	375.4	10	343	24.3	99%
Quasi independence	2931.5	11	2,896	126.4	95%
Quasi-symmetry	**41.1**	**6**	**22**	**11.2**	100%
Crossings	656.4	12	618	35.4	99%
Crossings, diagonals blocked	195.3	9	166	17.2	100%
Row-Column effects I	3880.7	9	3,852	164.6	93%

Source: 1940 US Census. Sample is the same as in Table 1.

Table 5: Log-Multiplicative Model Fit Statistics and Phi Parameter Estimates – Education

	G^2	BIC	DI	df	Phi Parameters (scaled to 1)	
ALL					Comp 1-5 yrs	Comp 6-10 yrs
Null Effect	99.0	-304.3	1.4	38		
Multiplicative Effect	96.9	-285.2	1.3	36	1.01	0.98
NORTH						
Null Effect	58.5	-318.8	1.4	38		
Multiplicative Effect	46.4	-311.0	1.2	36	1.09	1.09
SOUTH						
Null Effect	89.0	-287.7	2.0	38		
Multiplicative Effect	83.3	-273.6	1.7	36	0.96	0.94

G^2 = likelihood-ratio χ^2 statistic; BIC = Bayesian Information Criteria; DI = index of dissimilarity (the proportion of observations that must be moved for a model to fit the data perfectly); df = degrees of freedom. Smaller values indicate better fit.

Phi parameters measure variation in the strength of spousal association compared to the non-compulsory category, scaled to 1. Values higher than 1 indicate stronger association than the non-compulsory group; values lower than 1 indicate weaker association.

All models use the quasi-symmetry pattern of association.

Table 6: Spouse Race by Compulsory Category and Region

	Non-Comp	Comp 1-5 years		Comp 6-10 years	
All					
% Different Race x 10	0.01	0.03	*	0.03	*
N	13606	13461		13630	
North					
% Different Race x 10	0.01	0.03	*	0.03	*
N	5203	6682		8616	
South					
% Different Race x 10	0.01	0.02		0.05	*
N	8403	6779		5014	

Significant difference from the non-compulsory category indicated by * p<0.05, + p<0.1.
Source: 1940 US Census. Sample is the same as in Table 1.
Due to small proportions married to a spouse of different race, proportions are multiplied by 10.

Table 7: Log-Linear Models of Aggregate Race Data – Fit Statistics

	Deviance	df	BIC	AIC	% Deviance Explained
Constant	144323	8	144,306	16042.1	0%
Independence	31022.8	4	31,014	3454.1	79%
Row effects	611.3	2	607	75.5	100%
Column effects	666.7	2	662	81.6	100%
Uniform association	739.4	3	733	89.5	99%
Uniform assoc, diagonals blocked	**0.54**	**1**	**-2**	**7.8**	100%
Quasi independence	**0.54**	**1**	**-2**	**7.8**	100%
Quasi-symmetry	**0.54**	**1**	**-2**	**7.8**	100%
Crossings	204	2	200	30.2	100%
Crossings, diagonals blocked	**0.54**	**1**	**-2**	**7.8**	100%

With only 3 categories, race tables provide fewer degrees of freedom. The Row-Column model fails to converge and row and column effect models with diagonals blocked become full interaction models and are therefore excluded. df = degrees of freedom
Source: 1940 US Census. Sample is the same as in Table 1.

Table 8: Log-Multiplicative Model Fit Statistics and Phi Parameter Estimates – Race

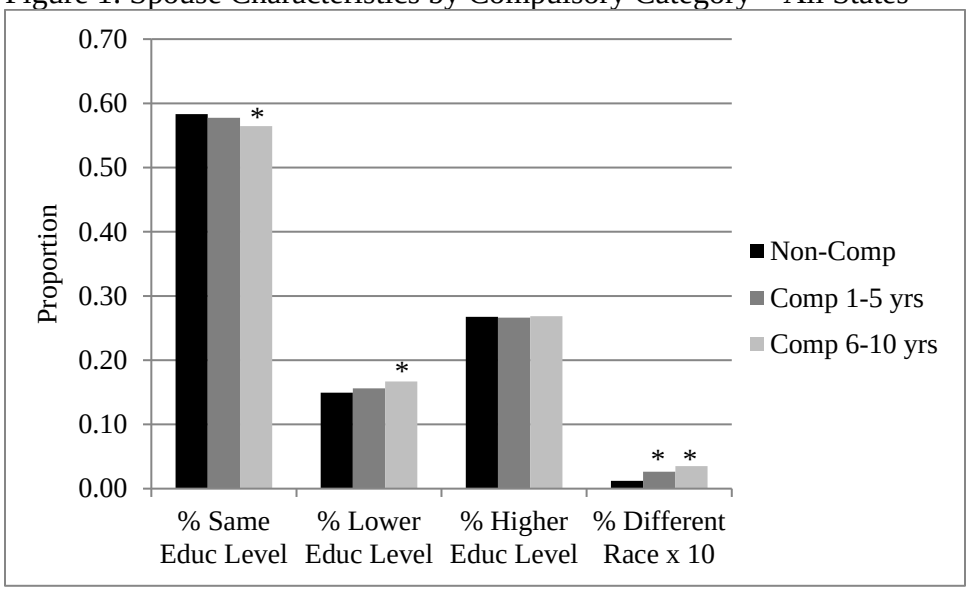
	G^2	BIC	DI	df	Phi Parameters (scaled to 1)	
ALL					Comp 1-5 yrs	Comp 6-10 yrs
Null Effect	10.1	-85.4	0.0	9		
Multiplicative Effect	9.5	-64.8	0.0	7	1.02	0.99
NORTH						
Null Effect	7.7	-81.6	0.0	9		
Multiplicative Effect	6.5	-63.0	0.0	7	1.08	1.04
SOUTH						
Null Effect	7.8	-81.4	0.1	9		
Multiplicative Effect	6.3	-63.1	0.0	7	0.98	0.94

G^2 = likelihood-ratio χ^2 statistic; BIC = Bayesian Information Criteria; DI = index of dissimilarity (the proportion of observations that must be moved for a model to fit the data perfectly); df = degrees of freedom. Smaller values indicate better fit.

Phi parameters measure variation in the strength of spousal association compared to the non-compulsory category, scaled to 1. Values higher than 1 indicate stronger association than the non-compulsory group; values lower than 1 indicate weaker association.

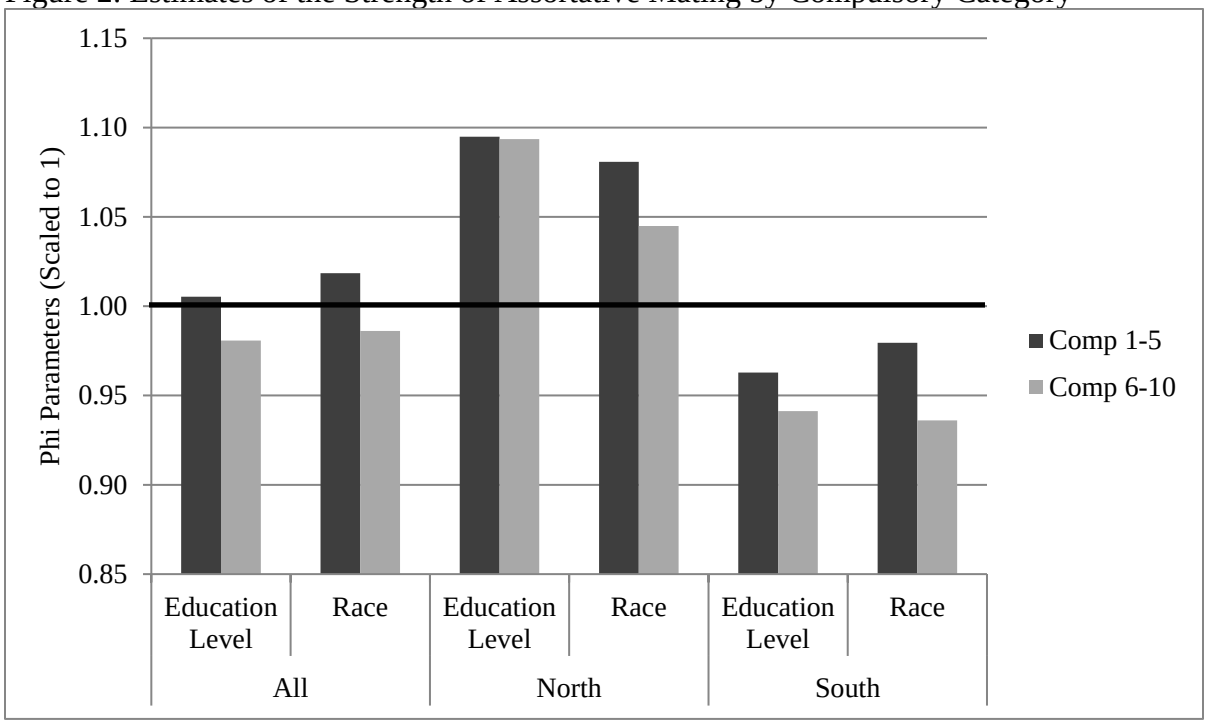
All models use the quasi-symmetry pattern of association.

Figure 1: Spouse Characteristics by Compulsory Category – All States



Based on values in Table 3 for all states.
Significant difference from the non-compulsory category indicated by * p<0.05.

Figure 2: Estimates of the Strength of Assortative Mating by Compulsory Category



Based on phi parameter estimates from log-multiplicative models in Table 5.
Values higher than 1 indicate stronger association than the non-compulsory group; values lower than 1 indicate weaker association.

Supplemental Material

Table S1: Year of First Compulsory School Attendance Law by State

State	Compulsory School Year
Massachusetts	1852
District of Columbia	1864
Vermont	1867
Michigan, New Hampshire, Washington	1871
Connecticut, New Mexico	1872
Nevada	1873
California, Kansas, New York	1874
Maine, New Jersey	1875
Wyoming	1876
Ohio	1877
Wisconsin	1879
Illinois, Montana, N./S. Dakota, Rhode Island	1883
Minnesota	1885
Nebraska, Idaho	1887
Colorado, Oregon	1889
Utah	1890
Pennsylvania	1895
Hawaii, Kentucky	1896
Indiana, West Virginia	1897
Arizona	1899
Iowa, Maryland	1902
Missouri, Tennessee	1905
Delaware, North Carolina, Oklahoma	1907
Virginia	1908
Arkansas	1909
Louisiana	1910
Alabama, Florida, South Carolina, Texas	1915
Georgia	1916
Mississippi	1918
Alaska	1929

Sources: U.S. Bureau of Education 1914:10; U.S. Bureau of the Census 1924:22; Steinhilber and Sokolowski 1966.

Table S2: Marital Status of Men Who Would Otherwise Appear in the Sample

	Mean	Std. Dev.
Married	0.91	0.28
Widowed	0.04	0.20
Divorced	0.01	0.09
Never Married	0.04	0.19
Compulsory	0.67	0.47
N	45516	

Source: 1940 US Census.

Limited to male household heads age 36-75, born in the US, and either 5 years beyond (never required to attend) or up to 10 years under (required to attend) the compulsory cutoff.

Table S3: Comparison of Inter-State Migrants and Non-Migrants in Linked Census Data
Panel A: 1870-1880 IPUMS Linked Representative Sample – Men

	Non-Migrant		Inter-State Migrant		
	Mean	Std Dev	Mean	Std Dev	Difference
Max SEI Score	22.77	20.81	24.39	21.90	1.62
Head SEI Score	4.85	12.64	5.90	13.95	1.05
Max Occup Income Score	20.20	11.76	20.89	12.51	0.69
Head Occup Income Score	4.76	9.74	5.65	10.53	0.89
Max Age	45.66	13.12	43.70	12.33	-1.96
Head Age	19.67	17.08	19.29	15.92	-0.38
White	0.91	0.29	0.94	0.23	0.04
Black	0.09	0.29	0.06	0.23	-0.04
Native American	0.00	0.01	0.00	0.00	0.00
Native Parents	0.78	0.41	0.80	0.40	0.02
Rural	0.84	0.36	0.84	0.36	0.00
Group Quarters	0.00	0.06	0.01	0.09	0.00
Family Size	6.76	2.41	6.38	2.37	-0.38
State Comp. School Year	1890.63	17.02	1890.31	15.24	-0.32
N	54837		5986		
%	0.90		0.10		

Panel B: 1880-1900 IPUMS Linked Representative Sample – Men

	Non-Migrant		Inter-State Migrant		
	Mean	Std Dev	Mean	Std Dev	Difference
Max SEI Score	24.02	20.95	25.56	22.40	1.54
Head SEI Score	5.92	13.47	6.33	14.48	0.41
Max Occup. Income Score	20.91	11.46	21.56	12.24	0.65
Head Occup. Income Score	5.61	10.20	5.82	10.67	0.21
Max Age	43.97	13.12	43.28	13.24	-0.70
Head Age	19.69	16.01	17.59	14.68	-2.10
White	0.95	0.23	0.96	0.20	0.01
Black	0.05	0.23	0.04	0.20	-0.01
Native American	0.00	0.00	0.00	0.00	0.00
Native Parents	0.71	0.45	0.72	0.45	0.01
Rural	0.78	0.41	0.80	0.40	0.02
Group Quarters	0.00	0.06	0.01	0.09	0.00
Family Size	6.29	2.31	6.12	2.36	-0.17
State Comp. School Year	1889.05	16.66	1889.14	15.91	0.09
N	32353		6194		
%	0.84		0.16		

Source: IPUMS Linked Representative Samples of Men 1870-1880 and 1880-1900.
Sample includes individuals living in a household with at least one child under age 14 (i.e. school-age or approaching school-age in most states requiring attendance) in the earlier census (1870 in Panel A, 1880 in

Panel B). All measures are from the earlier census, except inter-state migrant status, which indicates a change in state of residence between the first and second census.

Notes: These comparisons are limited to the linked samples of men due to concerns about data quality. Women who change their last name at marriage would not be able to be linked from one census to the next. However, this comparison includes all individuals, regardless of gender or whether they are the individual linked between censuses.
Non-Migrant = state of residence is the same in both censuses
Inter-State Migrant = state of residence changes between the first and second census
SEI Score = socio-economic index score, sometimes considered occupational status, is based on the median income and educational attainment associated with each occupation among men in 1950
Occup. Income Score = occupational income score measures the median income of men in an occupation in 1950
State Comp. School Year = the year in which the state of current residence required school attendance for school-age children
Max = household maximum value

State of residence during school-age is not available in the 1940 census. To determine the extent of potential bias introduced by determining compulsory assignment based on state of birth, Table S3 shows the prevalence of inter-state migration among families with children between 1870 and 1880 (Panel A) and between 1880 and 1900 (Panel B). The 1890 census was destroyed by fire and could not be linked. Table S3 also compares inter-state migrant and non-migrant families with children on several demographic measures. While they cannot rule it out, these comparisons are consistent with limited potential bias due to selective migration. While most of the differences between migrant and non-migrant measures are small, but statistically significant, the difference in state compulsory school year is not significant in either Panel A or B. This suggests inter-state migration among families with children is independent of the timing of state compulsory schooling laws.