

Returns to Education for Women in the Mid-Twentieth Century: Evidence from Compulsory Schooling Laws *

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Abstract

Women in the mid-twentieth century United States attained schooling levels comparable to men, yet evidence on the returns to women's education remains limited. Using compulsory schooling laws as instruments and the 1940 complete-count census data, I find that an additional year of education increased women's probability of gainful employment by 7.1 pp. and being in a skilled occupation by 1.2-3.3 pp. Education also positively shaped women's marriage market outcomes: while it did not affect the probability of marriage, it increased the probability of divorce by 1.4 pp., and improved match quality among those who married, as reflected in lower unemployment and higher earnings of their husbands. Across both labor and marriage markets, education appeared to have a more substantial effect among women from lower socioeconomic backgrounds.

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1 Introduction

Women and men had similar educational attainment during the early 20th-century United States. Although the returns to education for men during this period have been well-studied (Goldin and Katz, 2000; Feigenbaum and Tan, 2020; Clay et al., 2021), much less is known about the returns for women. On the one hand, a woman with higher educational attainment “could secure a position as a clerk, stenographer, or, better yet, a secretary,” and on the other hand, women would also have “a better chance of securing a higher income husband” (Goldin and Katz, 2008). This paper examines how education shaped women’s economic and social opportunities by analyzing its returns in both the labor market and the marriage market.

Because education is an endogenous choice that is often correlated with family socioeconomic background, OLS estimates are likely biased downward. To establish the causal effect of education, I instrument years of education completed with state-level legislation on compulsory attendance, child labor, and school continuation following Clay et al., 2021. The first-stage results show that laws requiring children to attend school for seven, eight, and nine years or above positively affected women’s educational attainment, and the magnitudes of the effects are comparable to those of men (Stephens and Yang, 2014; Clay et al., 2021). These laws provide plausibly exogenous variation in compulsory schooling across states and birth cohorts, since their enactment was independent of other progressive reforms of the time, such as early suffrage laws for women or the movement for women’s collective activism.

To study women’s labor and marriage market outcomes, I use the 1940 complete-count decennial census data, the first federal census that documents information about educational attainment (Ruggles et al., 2021). I restrict the sample to native-born White women born between 1885 and 1912 who were most likely to have been directly affected by compulsory schooling laws. I exclude foreign-born women and Black women from the sample, since the former may not have completed schooling in the United States and the latter predominantly lived in the agricultural South that was late to introduce compulsory schooling laws. Additionally, I also draw on Census Tree linked data that uses genealogical records to track women longitudinally (Price et al., 2021; Buckles et al., 2023a) to examine how the returns to education varied for women with different socioeconomic backgrounds. Women’s socioeconomic backgrounds are measured by their fathers’ occupations, farm and urban status.

First, I show that women experienced large returns to education in the labor market. The two-stage-least-square (2SLS) estimate indicates that an additional year of schooling increased women's probability of having a gainful occupation by 7.1 percentage points (pp.), which is a large effect since only 25% of women in the sample were gainfully employed in 1940. Furthermore, conditional on earning positive wages, an additional year of schooling increased working women's wage earnings by 16.9%. This sizable effect likely reflects that education enabled women to move into higher-paying skilled occupations, such as by increasing the probability of working as a stenographer, clerical worker, or bookkeeper.

Examining the heterogeneous returns in the labor market shows that women from low socioeconomic background (SES) and the rural areas might have benefited more. On gainful employment, the effect is 9.1 pp. for women from low SES households and 4.3 pp. for women from high SES households. Because children from low SES households started from a lower baseline of employment in 1940, the relative gains in employment were even more pronounced for them, although the difference in estimates by SES is only marginally significant. Additionally, education also had significantly stronger effects in rural areas, raising gainful employment, labor supply, and occupational status simultaneously.

Despite the positive labor market returns to education, education did not increase women's likelihood of never marrying, though it appears to have delayed the age at first marriage by about 0.7 years—consistent with the predictions of a simple dynamic model of women's work and marriage decisions with rising levels of education. However, these effects were largely concentrated among women from high SES and non-farm households. Among women from low SES and farm households, additional schooling increased the probability of being never married in 1940 but had little effect on their age at first marriage. These differences suggest that education may have changed marriage decisions through different channels by social status: it primarily raised the opportunity cost of marriage for low SES women, while it extended the period before marriage among high SES women.

In addition, an additional year of education increased women's probability of divorcing or separating by 1.4 pp., a large increase relative to the average share of 2.5% of women who had been divorced or separated. This finding suggests that more education helped women leave marriages, possibly due to the fact that they could achieve financial independence through improved labor market opportunities.

Finally, I find that higher levels of education improved match quality for married women, as reflected by their husbands' labor market status. Conditional on marriage formation and the husband's education, an additional year of the wife's schooling was positively associated with a decrease in the probability of the husband being unemployed by 3.88 pp., with the effect being larger for women from low SES households. This is a sizable effect, given that the unemployment rate was 13% in 1940, just a few years after the Great Depression. I also show that an additional year of the wife's education was positively associated with increasing the husband's wage earnings by 7.4%, but it did not affect the husband's labor supply or occupational status. Interestingly, although educated women were more likely to be matched with husbands of higher labor market status, there is no strong evidence that educated women were marrying up relative to their own socioeconomic status. Rather, education seems to have reduced the extent of marrying downward.

The findings on women's education and marriage outcomes contribute to a broader literature on marriage patterns and their implications for intergenerational mobility in the historical United States (Eriksson et al., 2023; Bailey and Lin, 2025). While prior work has documented the long-run effect of assortative mating on intergenerational mobility, this paper shifts attention to a key determinant during the process: education. By showing how compulsory schooling laws raised women's educational attainment and, in turn, improved their marriage market outcomes, this paper provides a foundation for understanding the mechanisms that shaped the assortative mating patterns highlighted in the literature.

I advance the literature in two additional dimensions. First, I focus on how legislative actions helped people achieve higher educational attainment and thereby affected future labor market and marriage market outcomes. My paper is closely related to prior work on the effects of compulsory schooling laws (Angrist and Krueger, 1991; Schmidt, 1996; Margo and Finegan, 1996; Lleras-Muney, 2002; Goldin and Katz, 2008; Stephens and Yang, 2014; Clay et al., 2012a; Gihleb and Lang, 2016; Rauscher, 2016; Clay et al., 2021) and studies on the educational and earnings impacts of the G.I. Bill (Bound and Turner, 2002; Angrist and Chen, 2011; Lennon, 2021). It also complements research studying how female-specific labor regulations increased women's employment (Haddad and Kattan, 2024).

In addition, my findings add new insights into the large literature on returns to schooling in the United States (Angrist and Krueger, 1991; Card, 1993; Goldin and Katz, 2008; Oreopoulos

and Salvanes, 2011; Stephens and Yang, 2014). The estimates on the returns to schooling in the labor market are larger for women than those for men in Feigenbaum and Tan, 2020, which is consistent with evidence in the broader literature showing that the education coefficient is often higher for women (Mincer and Polachek, 1974; Corcoran and Duncan, 1979; Kane and Rouse, 1995; Card, 1999; Dougherty, 2005, to name a few). The fact that only a quarter of women were employed may explain this, as education likely served as a stronger signal of ability and productivity for women, resulting in higher relative returns compared to men.

2 Background and Data

2.1 State Laws on Compulsory Attendance, Child Labor, and Continuation

State-level legislation on compulsory attendance, child labor, and school continuation was enacted between the 1850s and the 1910s to specify the period during which children should be in school. There are significant variations in the components of these laws across states.

Among the three categories of laws to keep children in school, compulsory attendance laws were established the earliest. The laws set a minimum school entry age, a maximum school leaving age, and a minimum period that children should be in school. Massachusetts was the first state to enact compulsory attendance laws in 1852, and other states in the Northeast (such as Vermont and New Hampshire) followed suit quickly, establishing their own compulsory attendance laws in the 1860s and 1870s. States in the south, on the other hand, generally did not establish compulsory attendance laws until the early 1900s. By 1920, all states in the U.S. had established compulsory attendance laws.

Over the years, compulsory attendance laws were updated to increase the length of mandatory attendance, such as by lowering the minimum entry age and increasing the maximum leaving age. In some cases, truant officers and fines were established to enforce compliance with the laws. According to W.E.B. Du Bois, who grew up in a small town in western Massachusetts, "truant laws were enforced" and he was brought up with "the idea of regular attendance at school" (Du Bois, 1983, page 12).

Complementary to the compulsory attendance laws, a large wave of state-level child labor

laws was introduced during the early 1900s.¹ Child labor laws often allowed children to leave school for work at age 14 by having exemptions for children who completed certain levels of schooling, which varied by state. While many states specified that children must complete 8th grade to be eligible for a labor permit, some states (such as Arizona, Missouri, and Vermont) required a lower level of schooling (Deffenbaugh and Keesecker, 1935).

State governments also enacted school continuation laws, which required working children who were still under the maximum school leaving age to attend school in their spare time. The required school time was short, usually only a few hours per week, but it increased the opportunity cost of hiring children under the maximum school leaving age since they might not always be excused from school (Goldin and Katz, 2011).

Laws on compulsory attendance, child labor, and school continuation combined generate large variations in length of school attendance across birth cohorts and states. This paper follows the procedures in Stephens and Yang, 2014 and Clay et al., 2021, which calculate the number of cumulative years each child needed to be in school while taking age limits and exemptions into consideration.² Using the data obtained from the replication package of Clay et al., 2021, I show the variations in length of schooling required for birth cohorts born between 1880 and 1910 in Figure 1. Later cohorts were required to be in school for longer, and children outside of the South were affected by these laws earlier than children in the South.

2.1.1 Are State Laws Endogenous?

To argue that state-level compulsory schooling laws provide exogenous variation, one must rule out several confounding factors. For example, Lleras-Muney, 2002 shows that compulsory attendance and child labor laws enacted between 1915 and 1939 did not predict past educational attainment, thereby alleviating concerns of reverse causality.

Nevertheless, one might worry that these laws are correlated with other state-level factors that also contributed to rising levels of women's education during this period. For instance,

¹A few states had enacted child labor laws before 1900, but they lacked enforcement, and there was an increase in child labor between 1880 and 1900 (Bliss, 1905). Between 1900 and 1910, however, more states established child labor laws and committees to enforce these laws, and the share of young children (13 years old and under) working declined below 5 percent by 1930 (Feigenbaum and Russo, 2020), although Moehling, 1999 finds that minimum age limits had little effect in the decline of child labor in the long run.

²Many previous works have also discussed the variations in length of school attendance generated by state laws, see Moehling, 1999; Lleras-Muney, 2002; Goldin and Katz, 2011; Clay et al., 2012b; Rauscher, 2015.

early suffrage laws may have improved children's schooling by increasing public spending on education and health (Kose et al., 2021), obscuring the independent effect of compulsory schooling laws. To directly test this, I estimate three sets of regressions where the dependent variables are the adoption of compulsory attendance laws, child labor laws, and school continuation laws, respectively. The independent variable for each set of regressions is a potential source of endogeneity, such as early suffrage legislation or the share of foreign-born individuals. All specifications include state and region by year fixed effects, and standard errors are clustered at the state by year level.

Table A1 explains how different factors affected the passage of compulsory schooling laws. Importantly, two plausible confounders appear unrelated to the timing of legislation. First, early suffrage laws had little impact on the adoption of compulsory attendance, child labor or school continuation laws. The establishment of a state federation of the General Federation of Women's Clubs (GFWC), a measure of women's collective activism and a proxy for women's status, also was not associated with the timing of these laws. This suggests that women's political influence was not the primary driver of compulsory schooling laws.

Some correlations do emerge with state-level characteristics. Industry composition mattered: farming was negatively correlated with the passage of compulsory schooling laws, while manufacturing was positively correlated, though both effects were modest in magnitude. Demographic factors also played a role: states with larger shares of foreign-born and non-white individuals were more likely to pass compulsory schooling laws, whereas states with higher literacy rates were less likely to do so. Since the sample is restricted to native-born White individuals, the endogeneity issue related to demographic variables could be partially mitigated.

2.2 Women's Education during the Late 19th and Early 20th Centuries

Beginning in the early 19th century, the American education system embraced the virtue of gender neutrality, offering female and male students similar levels of schooling up to age 15 (Goldin and Katz, 2008). Evidence from both federal and state censuses, summarized in Figure 3, confirms that women and men achieved similar levels of educational attainment. The analysis with the 1940 complete-count federal census (restricted to native-born white individuals aged 28 to 55) shows that women were slightly more likely than men to complete both 8th and 12th

grade across all birth cohorts. To mitigate concerns that this finding is driven by women being more likely to appear in the 1940 census since they often lived longer, the 1915 Iowa state census shows the same pattern among slightly younger cohorts.

How did women benefit from relatively high levels of educational attainment at a time when they faced significant trade-offs between work and marriage? The following discussion draws on qualitative evidence to illustrate the benefits of education for women in both the labor and marriage markets, and then introduces a simple dynamic model to show how returns in these two markets would vary with different levels of schooling.

2.2.1 Education and Women's Labor and Marriage Market Returns

Most women did not attend school with the expectation of pursuing lifelong careers. Rather, education was framed as a means of acquiring the knowledge and morals needed to become good wives and mothers. Yet the very skills designed to improve women's performance as homemakers often expanded their potential as workers. Training in bookkeeping and accounting, for instance, was intended to help women manage household finances after marriage, but it also created opportunities for paid work as bookkeepers. Likewise, instruction in typewriting and stenography enabled many women to work as secretaries or stenographers, often for a short period before marriage (Rury, [1984](#)).

This acquisition of transferable skills through education directly influenced women's employment prospects and returns to schooling. Educated women gained access to more skilled occupations that offered higher wages (Goldin and Katz, [2008](#)). The value of their education was further amplified by demand-side factors (Becker, [1985](#)); technological advancements, for example, created entirely new job categories like telephone operation, which became a major employer for American women early in the 20th century (Feigenbaum and Gross, [2024](#)). Occupational segregation may have also amplified the value of education, as men were often funneled into management and administrative roles, leaving lower-ranked positions that educated women could fill.

On the other hand, education was also seen as enhancing women's value in the marriage market. Reformers and educators argued that schooling made women more efficient homemakers and better mothers by combining moral instruction with practical training in domestic

skills such as sewing and cooking—qualities considered essential for managing households and raising children (Rury, 1991; Nash, 2005). Additionally, education also enabled women to contribute directly to household income through skilled employment, providing valuable insurance in times of a husband's job loss. Empirical evidence further supports the positive relationship between women's education and marital gains, though most studies focus on women from more recent generations (Benham, 1974; Lefgren and McIntyre, 2006; Kim and Sakamoto, 2017).

2.2.2 A Simple Dynamic Model on Women's Work and Marriage Choices

While the historical evidence highlights that women's education improved outcomes in both labor and marriage markets, it remains unclear how these benefits interacted within women's decision-making processes. The model below formalizes these trade-offs and helps interpret the differential effects across groups. Suppose time is discrete, denoted by $t = 0, 1, 2, \dots$, and schooling is represented by s . Each woman begins unmarried, and her labor market payoffs are defined as follows:

- If the woman works, she gets $u_W(s) = w(s) - c$, where $w(s)$ is the wage earnings that depend on schooling, and c is the cost of leisure.
- If the woman does not work, she gets u_N which could be normalized to 0.

For every period, the woman also has probability π to receive a marriage offer with match quality θ drawn from a distribution $F(\theta)$:

- If the woman accepts the marriage offer, she gets a payoff $u_M(s, \theta) = m(s) + \theta$ for every period after, with a discount factor β . Once married, she does not face any more work decisions due to marriage bars.
- If the woman does not accept the marriage offer, she remains single and continues to face work decisions.

As a result, the Bellman values for an unmarried and a married woman are:

$$V_U(s) = \max\{u_W(s), u_N\} + \beta \left[(1 - \pi) V_U(s) + \pi \mathbb{E}_\theta [\max\{V_U(s), V_M(s, \theta)\}] \right],$$

$$V_M(s, \theta) = \sum \beta^t u_M(s, \theta) = \frac{m(s) + \theta}{1 - \beta},$$

This means that the woman will accept a marriage offer if and only if $\theta \geq \theta^*(s)$ where $V_M(s, \theta^*(s)) = V_U(s)$. Since $\frac{d\theta^*}{ds} = (1 - \beta)V'_U(s) - m'(s)$, this condition is equivalent to accepting a marriage offer when $(1 - \beta)V'_U(s) > m'(s)$ and remaining single when $(1 - \beta)V'_U(s) < m'(s)$. One immediate implication of this condition is that education allows women to search for longer, since a higher level of education increases the marginal surplus of work, and raises $\theta^*(s)$ —the reservation quality of a marriage offer. However, the effect of education on the probability of ever marrying is ambiguous. Although higher education increases $\theta^*(s)$ and thus delays marriage, it may also improve the distribution of offers $F(\theta)$ by expanding women's social networks and exposure to higher-quality potential partners, leading to better matches and not necessarily to lower marriage rates. Hence, education may delay marriage without deterring it.

Overall, the model suggests that women's selection into marriage depends on whether the marginal surplus from marriage exceeds the marginal surplus from work. The differences between the marginal surplus from work and marriage can be correlated with many factors, such as family socioeconomic background. To capture heterogeneity by SES, let x denote a woman's socioeconomic status. In the labor market, higher education increases potential earnings $w(s, x)$, but the marginal return to education could be larger for those from lower SES backgrounds (i.e., $\frac{\partial^2 w(s, x)}{\partial s \partial x} < 0$), reflecting their initially lower access to high-paying jobs. In the marriage market, higher education increases $u_M(s, \theta, x) = m(s, x) + \theta$, where $m(s, x)$ captures the contribution of schooling and socioeconomic background to marriage. Similarly, it could also be the case that $\frac{\partial^2 m(s, x)}{\partial s \partial x} < 0$, which indicates that as SES increases, the marginal effect of education on the marriage market gets smaller, and education raises marriage market returns more strongly for women from lower SES backgrounds.

3 Data

3.1 Compulsory Schooling Laws

Data on compulsory attendance, child labor, and school continuation laws are obtained from the replication package of Clay et al., 2021. The data provide cohort-by-state level variation in

years of compulsory schooling required for those born between 1885 and 1912.

3.2 1940 Complete-Count Census

Data on women's labor and marriage market outcomes are based on the 1940 complete-count census (Ruggles et al., 2021), the first federal census that has information about education. I restrict the sample to native-born White women between 1885 and 1912. This is because foreign-born individuals might not have completed schooling in the US and, as a result, might not be subjected to state-level compulsory schooling laws. In addition, since the US South was late in introducing state schooling laws, and enforcement was weak after the laws were established, I do not include Black women in the sample (Margo, 1990; Lleras-Muney, 2002; Stephens and Yang, 2014).

3.3 Census Tree Data

To study the heterogeneous returns to education based on family socioeconomic background, I use the linked data for women from the Census Tree Project (Price et al., 2021; Buckles et al., 2023a). The Census Tree data relied on user-contributed links from FamilySearch.org, a genealogy platform where users find their ancestors using historical records. This approach overcomes a major challenge in historical data linkage—tracking women who often changed their names after marriage—and represents a substantial improvement in the ability to follow women longitudinally.

In particular, I use the Census Tree linked data to follow women in the 1940 census back to their childhood censuses, either in 1900, 1910, or 1920 (Buckles et al., 2023b; Buckles et al., 2023c; Buckles et al., 2023d). The linked data provide information on the father's occupation and occupational score, which are used as measures of socioeconomic background. Occupational score is a numerical score assigned to an occupation, and a higher occupational score indicates a higher socioeconomic background. I classify women as being from a high socioeconomic background if their father's occupational score is above the median or if he held a skilled occupation. Similarly, I also use the Census Tree linked data to follow the married women's husbands in 1940 back to their childhood censuses to measure their husbands' socioeconomic background.

4 Identification: Instrumental Variables

Education is an endogenous choice that often correlates with family socioeconomic backgrounds, making OLS estimates unlikely to capture causal effects. In particular, during the historical period in the United States, women from higher socioeconomic backgrounds were more likely to complete additional schooling but less likely to work after marriage.³ This selection bias leads OLS estimates to understate the true effect of education on employment. To address this, I instrument years of schooling using laws related to compulsory attendance, child labor, and school continuation. The IV estimates help mitigate omitted variable bias and better identify the causal effect of education.

4.1 Compulsory Schooling Laws As Instruments

Almost 90% of women aged 28 to 55 in 1940 had been affected by compulsory schooling laws while they were young. As shown in [Figure 2](#), among those who were required to attend school, the vast majority were required to attend school for at least six years and at most ten years, while very few were required to attend school for only one to five years.

Due to this non-linearity in the number of required years of attendance, I use a group of dummy variables (instead of a continuous variable) that indicate different lengths of compulsory attendance as instruments for years of schooling completed. In particular, the instruments consist of four dummy variables, which are equal to 1 if the required number of years of attendance is six, seven, eight, and nine years or above.

The positive effect of compulsory attendance on years of schooling completed is illustrated in [Figure 4](#) and [Table A2](#). The results show that requiring children to attend school for six, seven, eight, and nine years or above increased their probability of completing sixth, seventh, eighth, and ninth grade respectively, which suggests that compulsory attendance laws are effective in increasing years of education. Among these estimates, requiring children to attend school for nine years or above has the largest effect, which increased the probability of finishing the ninth grade by 4.1%, although it did not affect the probability of finishing high school.

³As shown in [Table A5](#), women from higher socioeconomic backgrounds had more years of schooling and were more likely to complete 8th and 12th grade.

4.2 Instrumental Variables Specification

The instrumental variables specification used to estimate the returns to education for women is the following:

$$Y_{icsr} = \beta_0 + \beta_1 Educ_{icsr} + \gamma_s + \gamma_{cr} + \epsilon_{icsr}$$

$$Educ_{icsr} = \alpha_0 + CA'_{icsr}\Omega + \theta_s + \theta_{cr} + u_{icsr}$$

Y_{icsr} is the outcome variable in the 1940 census (that measures labor market returns and marriage market returns) for individual i who was born in birth cohort c , state s , and region r , and $Educ_{icsr}$ is the years of schooling completed. CA_{icsr} is a vector of instruments consisting of four dummy variables, which are equal to 1 if the required number of years of attendance is six, seven, eight, and nine years or above. The specification also includes birth state fixed effects γ_s and birth year by birth region fixed effects γ_{cr} , with the standard errors clustered at the birth year by birth state level. With both fixed effects, β_1 captures changes within the state over time and variations across states but within the same region and birth cohort. This is similar to the specification used by Stephens and Yang, 2014 and Clay et al., 2021.

4.3 First Stage Results

Table 1 presents the first stage estimates of the instruments on years of schooling completed for several groups of women. Column 1 includes all native-born White women born between 1885 and 1912. The results first show that women required to attend school for six years did not experience a large positive effect on years of education relative to women who were not required to attend school and women who were required to attend school for one to five years.⁴ On the other hand, requiring women to attend school for seven, eight, and nine years or above increases women's educational attainment by 0.069, 0.078, and 0.153 years, respectively. A higher number of required years of attendance seems to be more effective at increasing educational attainment among women. The Kleibergen-Paap Wald F-statistic in Column 1 is around 16.

The magnitudes of all these estimates in Column 1 are slightly smaller than those of men

⁴The results are similar if we include compulsory attendance for one to five years as an instrument in the first stage. See more details in Table A3.

(shown in Stephens and Yang, 2014 and Clay et al., 2021), indicating that the laws might have a slightly weaker effect on increasing women's education. This could be because women's labor during their teenage years was valued less than men's labor, and women were more likely to attend school than men in the first place.

In addition, I further restrict the sample to currently married women in 1940, working women who earned positive wages in 1940, sample-line women in the 1940 census (which is less than 5 percent of the female population), and women born outside the south in Columns 2, 3, 4, and 5. Using various samples allows me to examine the returns to education among different groups. The first-stage results from Columns 2, 3, 4, and 5 are similar to those of Column 1, suggesting that compulsory attendance laws have a positive effect on years of schooling for married women, working women, sample-line women, and women born outside the South as well. Note that the results for working women in Column 3 are slightly different because the coefficients do not increase monotonically with the increasing number of required years of attendance, which could be due to the selection of women who were earning positive wages (which consists of less than 25% of the female population in the analysis). In addition, the F-statistic among sample-line women is smaller than 10, suggesting that the instruments are weak in this case, which could be due to the much smaller sample size of sample-line women.

4.4 First Stage Results By Socioeconomic Background

The IV estimates capture the local average treatment effect (LATE), which is the effect of education among compliers who followed compulsory schooling laws. The likelihood of compliance with these laws is potentially correlated with several factors, such as family socioeconomic background (SES) and urban status. As a result, I additionally examine the heterogeneous effect of compulsory attendance laws on education by household socioeconomic background using the linked Census Tree sample of women. The household socioeconomic background is measured by (1) the father's occupational score, where a higher score reflects higher occupational status, and (2) the father's occupation category, which can be skilled (such as managers, carpenters, and motormen), and others (such as guards and farm laborers).

Since the linked sample of women likely differs from the complete-count sample of women,⁵

⁵For example, Table A4 suggests women in the linked sample were more likely to have been married/be in farming households in 1940.

we might be concerned that the selection into the linked sample of women could bias the results. To address this issue, I show that the first stage results from the linked sample of women are similar to those of the complete-count sample of women aged 28 to 55 in 1940 (see the comparison between Columns 1 of [Table 2](#) and [Table 1](#)). This indicates that compulsory attendance laws positively affected education attainment for women in the linked sample, which is reassuring.

Columns 2 to 5 of [Table 2](#) show that compulsory attendance laws affected children from high and low socioeconomic backgrounds differently. For children from higher socioeconomic backgrounds (fathers with above-median-level occupations), compulsory attendance laws that required students to attend school for 6, 7, 8, and 9 years or more increased their educational attainment. This suggests that children from higher socioeconomic backgrounds were more likely to comply with compulsory schooling laws, possibly because the opportunity cost of an extra year of schooling was relatively low. Conversely, for children from lower socioeconomic backgrounds (fathers with below-median-level occupations), only compulsory attendance laws that required students to attend school for nine years or above increased their education, and other types of laws mostly had a null effect. This suggests that many children from lower socioeconomic backgrounds might not have complied with compulsory schooling laws below the nine-year threshold.

The lack of benefit from certain compulsory attendance laws for children from lower socioeconomic backgrounds is initially surprising, since conventional wisdom often suggests that such laws should reduce educational inequality between different class backgrounds.⁶ One explanation for this lies in the non-linear returns to education. For children from low SES households facing significant economic constraints, only higher educational requirements that substantially improved labor market prospects could justify the opportunity cost of staying in school. While mandates of 6 to 8 years of schooling did little to expand access to better jobs, requiring 9 or more years effectively initiated high school education, creating opportunities to access more skilled occupations. Other factors, such as the degree of enforcement of compulsory attendance laws, may also have played a role.⁷

⁶For example, Rauscher, 2016 finds compulsory attendance laws improved inequality in school attendance among white male children using data from 1850 to 1920.

⁷It is plausible that stronger enforcement was positively correlated with longer length of compulsory attendance requirements. According to Deffenbaugh and Keesecker, 1935, there are ten different types of provisions a state can impose to enforce the compulsory attendance laws (see more details in [Figure A1](#)). The data suggest the Midwest region had the highest average number of provisions regarding enforcement ([Figure A2](#)), and the Midwest also

Additional analysis by household type indicates that compulsory attendance laws were less effective in raising educational attainment among children from farm households compared to those from non-farm households, but they were similarly effective for children in rural and urban settings (see Columns 6–9 of Table 2). Taken together, these results suggest that requiring fewer than nine years of schooling had little effect on the educational attainment of children from lower socioeconomic backgrounds.

5 Returns to Education on Women’s Labor Market Outcomes

Because women’s labor supply was more elastic, education is expected to affect the extensive margin of labor market participation more than the intensive margin. Accordingly, the first outcome of interest is whether a woman was employed in 1940, defined as reporting a gainful occupation in the 1940 Census.⁸ Column 1 of Table 3 shows that an additional year of education increased the probability of employment by 2.2 pp. in the OLS specification, while the IV estimate suggests a much larger effect of 7.1 pp., a substantial increase given that only 25% of women held gainful occupations in 1940. Consistent with these findings, education also raised women’s labor supply, as indicated by positive OLS and IV estimates in Columns 2 and 3. In particular, the IV results suggest that an additional year of schooling increased weeks worked per year by 3.4 and hours worked per week by 2.3.

Conditional on the sample of working women who earned positive wages in 1940, Column 4 shows that an additional year of education substantially increased women’s earnings. The OLS and IV estimates suggest that an additional year of schooling increases wage earnings by 11.2 to 16.9 percent. One reason behind the large returns on wage earnings might be that education allowed women to enter higher-paying and skilled occupations, such as clerical workers or bookkeepers. To test this hypothesis, I examine some additional outcome variables that are dummy variables equal to one if the woman was a stenographer, a clerical worker, a bookkeeper,

had more states enacting laws requiring children to attend school for nine years or above: Illinois, Indiana, Ohio, and Nebraska required some birth cohorts to do so. The other states with similar laws were New Mexico, Utah, Wyoming, and New Jersey. However, definitively examining the impact of enforcement on educational attainment is challenging. The available enforcement data are limited to 1935, and many states significantly strengthened their compulsory attendance laws between 1915 and 1935 (Schmidt, 1996), making it difficult to disentangle the effects of the laws themselves from their varying enforcement over time.

⁸Based on instructions to census enumerators, occupations were defined as words “which most accurately indicate the particular kind of work done by which the person enumerated earns money or a money equivalent.” More details can be found here: <https://usa.ipums.org/usa/voliii/inst1920.shtml>.

or a housekeeper. The first three occupations were higher-paying occupations, while the last was not. Columns 5 to 8 of [Table 3](#) show that an additional year of schooling increases the probability of being a stenographer/clerical worker/bookkeeper by 1.2 to 3.3 pp. while it decreases the probability of being a housekeeper by 1.9 pp. Although the improvement in women's occupational status might not fully account for the 16.9% increase in women's earnings because women earning positive wage earnings in 1940 were highly selected, it provides supportive evidence that women with more education were more likely to enter higher-paying occupations and receive larger returns to education in the labor market.⁹

Examining the heterogeneous returns by children's socioeconomic backgrounds shows that children from low SES households might have benefited more from education. As shown in Panels A and B of [Table 4](#), an additional year of education increased the probability of gainful employment by 4.3 pp. for children whose father's occupational score was above the median level while it increased the probability of gainful employment by 9.1 pp. for children whose father's occupational score was below the median level. Because children from low SES households started from a lower baseline of employment (only 16.2% were gainfully employed in 1940, compared to 24.1% among children from high SES households), the relative gains in employment were even more pronounced for them, although the difference in estimates by SES is only marginally significant. In contrast, the differences in labor supply and wage earnings are less pronounced by these two SES groups. This pattern suggests that while education enabled more women from low SES households to enter the labor force, the resulting increases in labor supply and wages were similar across socioeconomic backgrounds.

On the other hand, we observe large differences in the effect of education for those from rural versus urban areas. Panels C and D of [Table 4](#) show that an additional year of education significantly increased gainful employment, labor supply and occupational status for those from rural areas, but had little to no effect for those from urban areas. This pattern is consistent with the hypothesis that the marginal returns to education were higher in rural settings, where access to formal employment opportunities was more limited and educational attainment was

⁹Note that the results from [Table 3](#) are the average effect among native-born White women aged 28 to 55 in 1940, and the identification strategy cannot separate the effect of education on earnings over the life cycle. We might expect education not only benefit women immediately after graduation but also benefit women when they have the option to rejoin the labor force when they are older and after the children leave the households (Bailey, 2006, Goldin, Kerr, et al., 2022). The effects over the life cycle are more difficult to examine due to the lack of panel data during this period.

generally lower.

Because the laws were adopted later during the South and the birth cohorts affected in the South were the youngest in 1940, one might be worried that Southern-born White women were driving the results. To mitigate this concern, I show that the estimates remain similar when restricting the sample to women born outside of the south in [Table A6](#) and [Table A7](#).

6 Returns to Education on Marriage Market Outcomes

6.1 Marriage Formation and Dissolution

Given the positive returns to education in the labor market, one might expect that additional schooling would reduce women's likelihood of marriage, as some women could forgo marriage to pursue careers.¹⁰ Consistent with this intuition, the OLS estimate in Column 1 of [Table 5](#) shows a positive association between education and being never married: an additional year of schooling is associated with a 1.36 pp. increase in the probability of remaining never married. In contrast, the IV estimate is negative (−0.41) but small in magnitude relative to the 12.5% share of women aged 28–55 who were never married in 1940, and it is not statistically significant, suggesting no clear evidence that education affected the probability of never marrying. While the IV results provide little evidence that education reduced the probability of marriage overall, results on the timing of marriage suggest that education may have delayed marriage. As shown in Column 3, an additional year of schooling increased women's age at first marriage by 0.38 to 0.75 years, consistent with findings from the post–World War II United States (Lennon, [2022](#)) and mid-twentieth-century United Kingdom (Powdthavee and Adireksombat, [2010](#)).

However, comparing women by different socioeconomic background in [Table 6](#) reveals an interesting pattern: among women from low-SES and farm households, additional schooling increased the probability of being never married in 1940 but had little effect on their age at first marriage, whereas among women from high-SES and non-farm households, education mainly delayed marriage without affecting the likelihood of ever marrying. These differences suggest that education may have changed marriage decisions through different channels by social status: it primarily raised the opportunity cost of marriage for low SES women, while it extended the

¹⁰For example, Lafortune et al., [2022](#); Olivetti et al., [2024](#) show that women from the highest socioeconomic backgrounds were also least likely ever to marry.

period before marriage among high SES women.

Education also appears to have strengthened women's bargaining power within marriage. Column 2 of [Table 5](#) shows that an additional year of education increased the probability of divorce or separation by 0.06 pp. in the OLS estimates and by 1.43 pp. in the IV estimates, despite only 2.5% of women in the 1940 sample being divorced or separated. These findings suggest that more educated women may have been better able to leave marriages, possibly due to the fact that they could achieve financial independence through improved labor market opportunities. That said, the effects do not differ substantially by socioeconomic background, suggesting that education may have strengthened women's bargaining power within marriage across both high- and low-SES groups.

Conditional on marriage formation, there is strong evidence of assortative matching. Column 4 of [Table 5](#) shows that each additional year of a wife's education is associated with 0.7 to 0.9 additional years of her husband's education, consistent with findings from prior research on assortative mating in the historical United States (Goldin, [2004](#); Bailey, Guldi, et al., [2014](#)).

Finally, the results remain similar when restricting the sample to non-Southern-born women (see [Table A8](#) and [Table A9](#)). This helps alleviate concerns that the main findings might be driven by Southern-born White women, who were affected differently by later adoption of compulsory schooling laws and were relatively younger in 1940.

6.2 Match Quality Among Married Couples

More education may have enabled women to form higher-quality matches in the marriage market. To assess this, I examine the effect of women's education on their husbands' labor market outcomes. Because men and women were subject to compulsory attendance laws during the same period, it is difficult to disentangle the effect of a wife's education on match quality from that of her husband's education. To mitigate this concern, I control for the husband's years of education in the specification. While this does not fully establish causality, it allows me to assess whether a wife's education, which is independent of her husband's education to some extent, contributes to match quality.

I measure match quality using the husband's labor market outcomes. The first outcome of interest is whether the husband was unemployed in 1940, a category that also includes men em-

ployed on public emergency projects under the New Deal.¹¹ Because unemployment remained widespread in the aftermath of the Great Depression, a lower probability of unemployment can be interpreted as reflecting higher match quality. Column 1 of Table 7 shows that, in the OLS specification, both the wife's and husband's education are associated with a 0.84–0.92 pp. decrease in the husband's probability of unemployment. By contrast, the IV estimate implies a larger effect: an additional year of the wife's education is associated with a 3.88 pp. lower probability that her husband was unemployed, a sizable magnitude given that only 13% of husbands in the sample were unemployed. Examining this by socioeconomic background in Table 8 further shows that the negative correlation between the wife's education and the husband's unemployment is larger among those from low SES and farm households, suggesting that women from lower socioeconomic backgrounds may have gained more from education in terms of match quality in the marriage market.

Columns 2–4 of Table 7 show that the wife's education had no significant effect on her husband's occupational status or labor supply, although Table 8 shows weak evidence of a small positive association for those from low SES households, farm households, and rural areas. Despite that, an additional year of the wife's education was positively correlated with the husband's wage earnings by 7.4%, with the coefficient being significant at the 10% level. This effect may reflect that a more educated wife enhanced her husband's productivity on the job, or alternatively, that more educated women were more likely to marry more productive men. While the effect on husbands' wage earnings does not vary substantially by socioeconomic background, the effect on non-wage earnings does. Column 6 of Table 8 shows that an additional year of the wife's education was associated with a 9.4–11.6 pp. lower probability that her husband had non-wage income, particularly among low SES, farm, and rural households. This suggests that more educated women in these groups were less likely to marry self-employed men and were instead matched with men who had more stable sources of income.

6.2.1 Did Women Marry Up?

Were more educated women more likely to marry upward? I further examine the effect of the wife's education on outcomes related to the husband's socioeconomic background, and the

¹¹Public emergency work projects during this period were designed to move workers from unemployment to relief work. Thus, participation in such projects indicates prior unemployment.

relative difference between the husband's and the wife's SES. I measure the husband's SES using his father's occupational score and occupational rank. Columns 1 and 2 of [Table 9](#) indicate that an additional year of the wife's education is associated with a higher value of both measures, suggesting that more educated women tended to marry men from families with higher occupational standing. In contrast, Columns 3 and 4 show that an additional year of the wife's education is negatively associated with the SES gap between spouses. This implies that while better-educated women married men of higher socioeconomic background, they actually married lower relatively to their own status.

This finding highlights an important nuance in patterns of assortative matching. On the one hand, women's education allowed them to find partners of better quality, consistent with the idea that education improved women's prospects in the marriage market and increased their marriage surplus. On the other hand, the negative association with the SES gap suggests that more educated women did not necessarily "marry up" in relative terms, but rather reduced the extent of marrying downward.

7 Conclusion

Although women and men had similar levels of educational attainment during the historical United States, the returns to education for women are not well documented. This paper fills the gap in the literature by estimating the effect of women's education on their labor market and marriage market outcomes using compulsory schooling laws as instrumental variables.

I find that additional years of education significantly increased women's likelihood of employment, generating substantial returns in the labor market. By contrast, there is little evidence that more education reduced women's probability of marrying, though it did slightly increase the likelihood of divorce. Conditional on marriage, women's education was positively correlated with their husbands' labor market status, reflecting that educated women were able to find a better match in the marriage market. Across both labor and marriage markets, women from lower socioeconomic backgrounds appear to have benefited more from education.

Future research should explore the mechanisms behind these effects, though this is challenging given data limitations. For instance, do more educated women enhance their husbands' productivity, thereby improving the husbands' labor market outcomes? Understanding these

mechanisms is crucial for interpreting the broader role of women's education in shaping both labor market success and marital outcomes.

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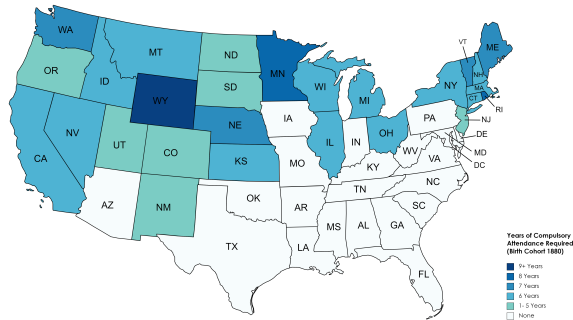
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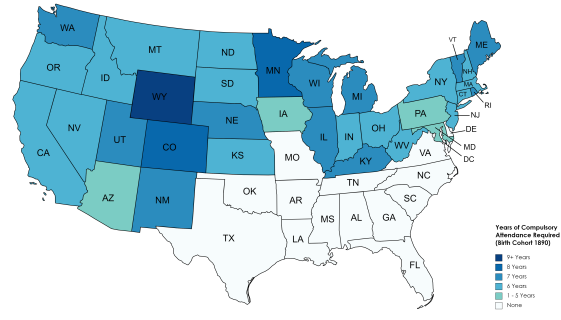
8 Figures

Figure 1: States with Different Lengths of Compulsory Attendance

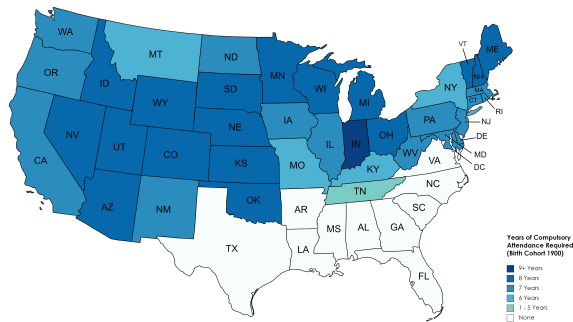
(a) Birth Cohort 1880



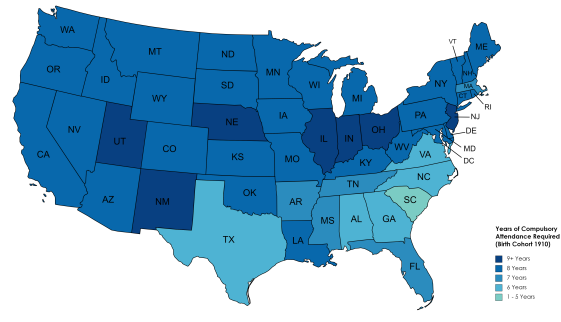
(b) Birth Cohort 1890



(c) Birth Cohort 1900

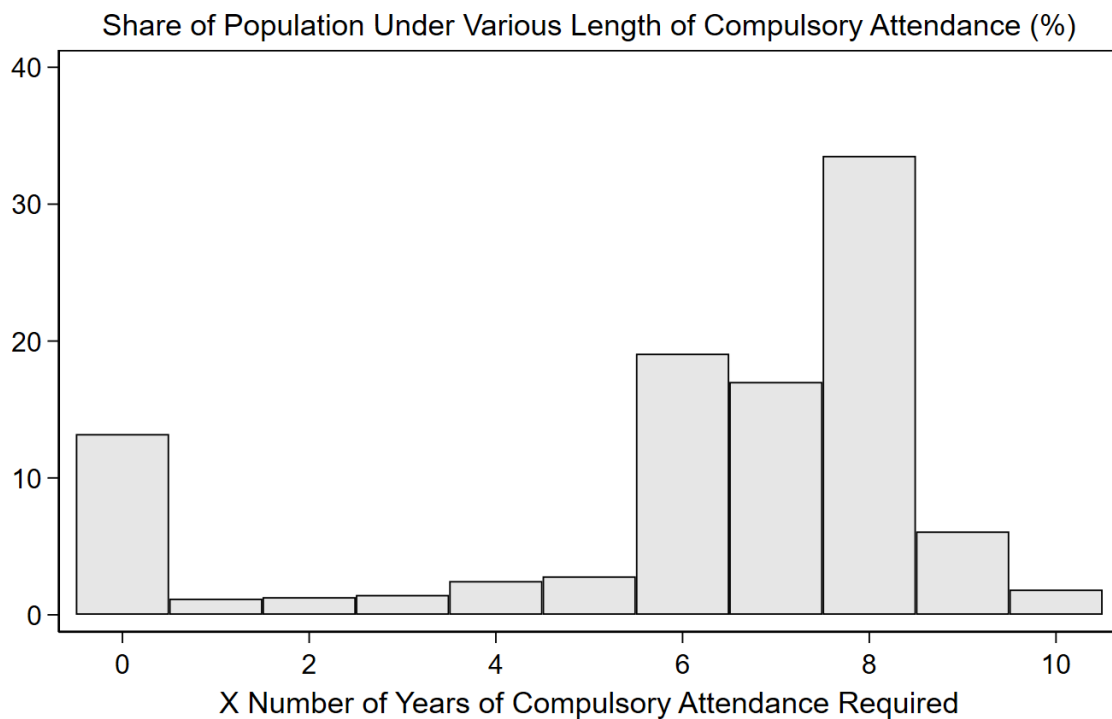


(d) Birth Cohort 1910



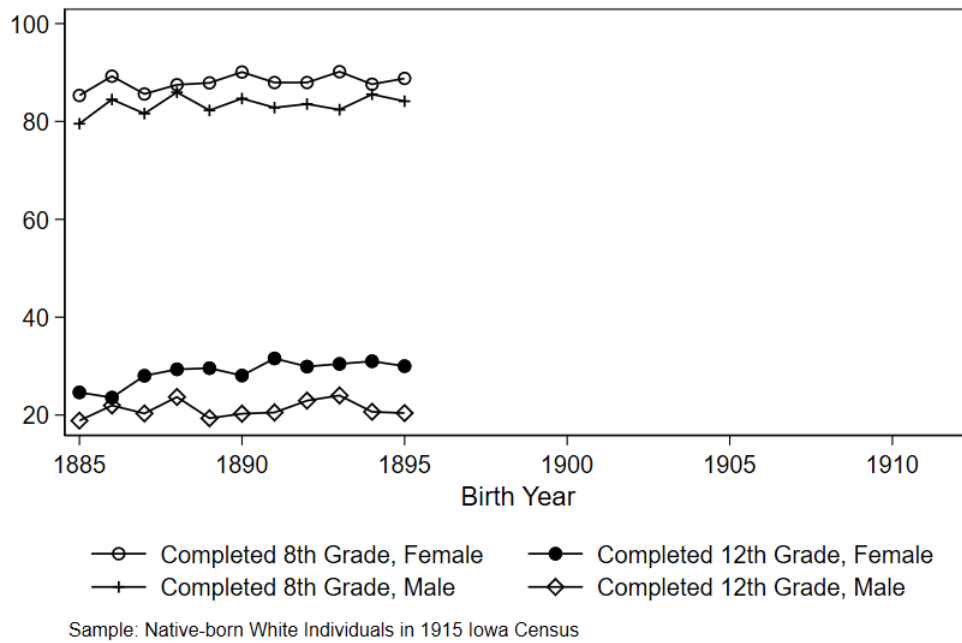
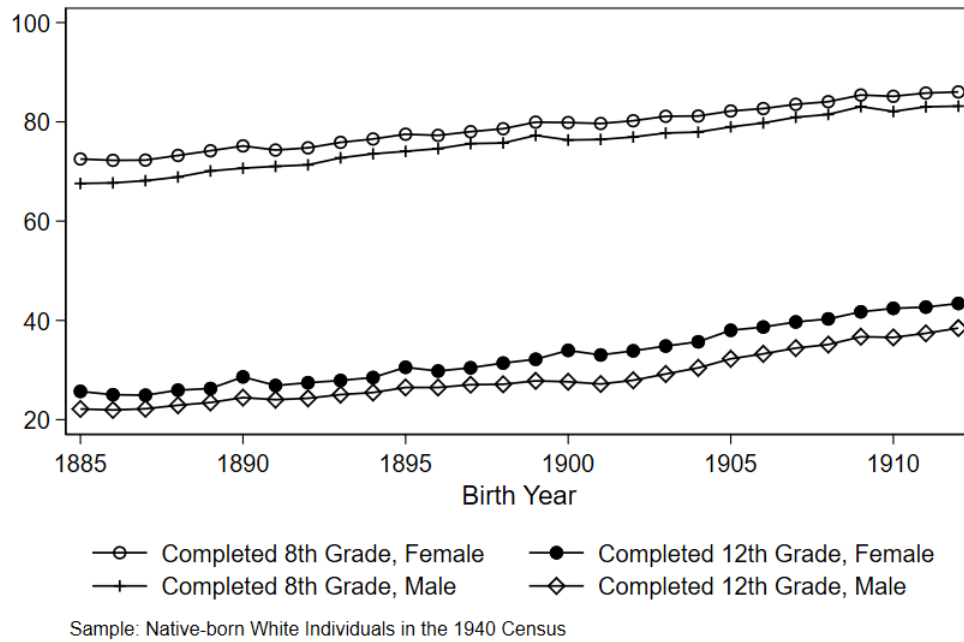
The figure illustrates the lengths of compulsory attendance for birth cohorts 1880, 1890, 1900, and 1910 in different states. Lengths of compulsory attendance are categorized into (1) None; (2) 1-5 years; (3) 6 years; (4) 7 years; (5) 8 years; (6) 9 years and above.

Figure 2: Share of Population Under Various Lengths of Compulsory Attendance (%)



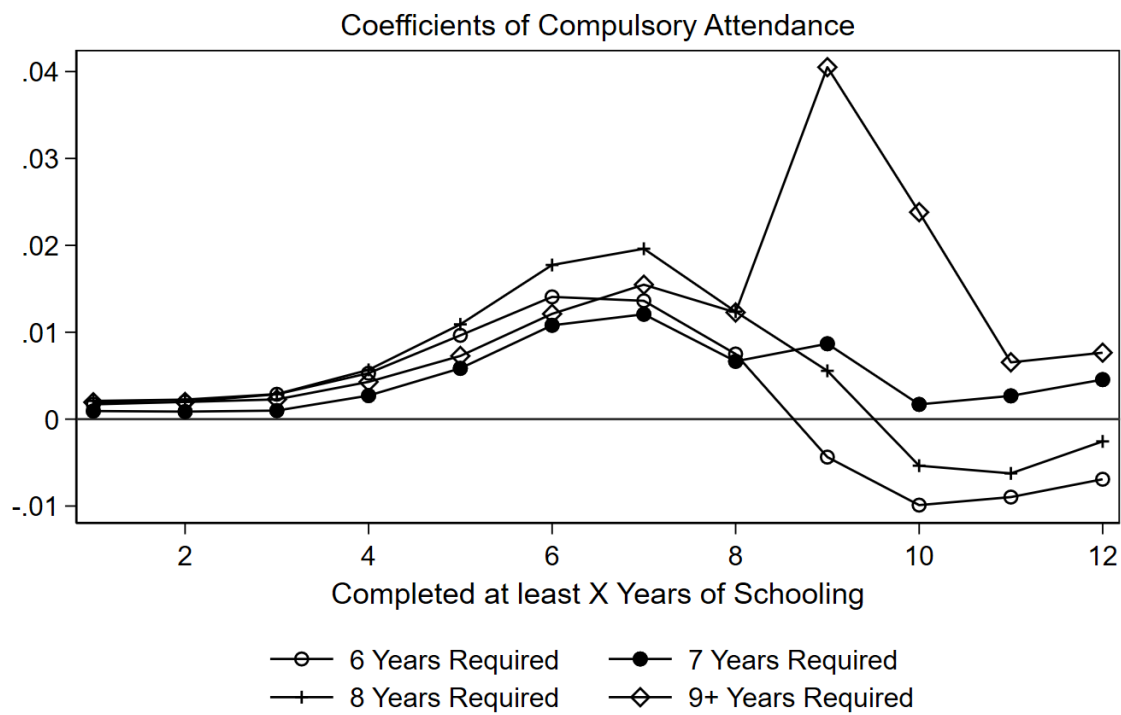
The figure shows the share of the population that was under various lengths of compulsory attendance laws in 1940. The sample is restricted to white and native-born women who were born between 1885 and 1912.

Figure 3: Share of Population that Completed 8th and 12th Grade by Gender (%)



The figure at the top shows the share of the population completed 8th and 12th grade as reported in the 1940 Decennial Census. The sample is restricted to white and native-born people who were born between 1885 and 1912. The figure at the bottom shows the share of the population completed 8th and 12th grade as reported in the 1915 Iowa State Census. The sample is restricted to white and native-born people who were at least 20 years old when the 1915 Iowa State Census was taken.

Figure 4: The Effect of Compulsory Attendance Laws on Years of Schooling Completed



The figure plots the coefficients of dummy variables that indicate different lengths of compulsory attendance laws from separate regressions. Each regression has a different outcome variable that indicates whether one completed at least X years of schooling, where X ranges from 1 to 12. Detailed results from the regressions are shown in [Table A2](#). The sample is restricted to white and native-born women who were born between 1885 and 1912 ($N=18836237$).

9 Tables

Table 1: First Stage - The Effect of Compulsory Attendance on Years of Schooling Completed

	(1) All Women	(2) Married Women	(3) Working Women	(4) Sample-line Women	(5) No South
<i>Required Years of Schooling</i>					
6 Years	0.018 (0.013)	0.010 (0.013)	0.111*** (0.018)	0.030 (0.020)	0.009 (0.015)
7 Years	0.069*** (0.015)	0.060*** (0.016)	0.193*** (0.022)	0.081*** (0.023)	0.060*** (0.017)
8 Years	0.078*** (0.017)	0.067*** (0.018)	0.206*** (0.023)	0.096*** (0.025)	0.063*** (0.018)
9 Years +	0.153*** (0.023)	0.154*** (0.024)	0.184*** (0.028)	0.168*** (0.032)	0.138*** (0.023)
N	18836237	14902197	4151245	936920	14784416
F Stats	15.983	15.541	24.121	8.392	15.283

The outcome variable is years of education completed reported in the 1940 census. The independent variables are dummy variables indicating years of required compulsory attendance. The regression includes birth state fixed effects and birth year by region fixed effects. The sample from Column 1 is White, native-born women who were born between 1885 and 1912. Columns 2, 3, 4, and 5 further restrict the sample to currently married women in 1940, working women who earned positive wages in 1940, sample-line women in the 1940 census, and women born outside of the South. Kleibergen-Paap Wald F-statistic is reported.

Table 2: First Stage By Socioeconomic Background - The Effect of Compulsory Attendance on Years of Schooling Completed

	(1) All	(2) > Median OccScore	(3) <= Median OccScore	(4) Skilled Occ.	(5) Other Occ.
<i>Required Years of Schooling</i>					
6 Years	0.030* (0.014)	0.013 (0.018)	0.007 (0.016)	-0.013 (0.017)	0.010 (0.016)
7 Years	0.074*** (0.015)	0.086*** (0.021)	0.016 (0.018)	0.079*** (0.020)	0.007 (0.018)
8 Years	0.095*** (0.017)	0.103*** (0.022)	0.032 (0.021)	0.081*** (0.021)	0.024 (0.021)
9+ Years	0.205*** (0.024)	0.211*** (0.030)	0.225*** (0.033)	0.213*** (0.029)	0.148*** (0.031)
N	4761628	2148175	2613453	1711690	2785253
F Stats	19.565	16.328	16.616	21.704	7.577
		(6) Non- Farm	(7) Farm	(8) Rural	(9) Urban
6 Years		0.033 (0.017)	-0.020 (0.017)	0.026 (0.015)	-0.032 (0.020)
7 Years		0.100*** (0.020)	-0.013 (0.020)	0.036* (0.017)	0.066** (0.023)
8 Years		0.125*** (0.021)	-0.008 (0.024)	0.059** (0.021)	0.068** (0.025)
9+ Years		0.228*** (0.028)	0.191*** (0.035)	0.231*** (0.032)	0.130*** (0.032)
N		2495413	2266215	3400574	1361054
F Stats		19.558	18.074	15.526	11.571

The outcome variable is years of education completed reported in the 1940 census. The independent variables are dummy variables indicating years of required compulsory attendance. The regression includes birth state fixed effects and birth year by region fixed effects. The sample from Column 1 is the linked sample of White native-born women who were born between 1885 and 1912. Columns 2 and 3 select the sample of women based on their father's occupational scores (above and below median), Columns 4 and 5 select the sample of women based on the category of their father's occupations (skilled and others), and Columns 6 to 9 select the sample of women based on farm and urban status of their fathers.

Table 3: The Returns to Education on Labor Market Outcomes

	(1) Gainful Occupation		(2) Weeks Worked		(3) Hours Worked		(4) ln Wage	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	2.190*** (0.019)	7.129*** (1.862)	0.907*** (0.009)	3.399*** (0.829)	0.752*** (0.006)	2.289*** (0.614)	0.112*** (0.001)	0.169*** (0.023)
\bar{Y}	25.871	25.871	11.114	11.114	9.219	9.219	6.391	6.391
	(5) Steno- graphers		(6) Clerical Workers		(7) Book- keepers		(8) House- keepers	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	0.515*** (0.011)	3.342*** (0.724)	0.238*** (0.003)	1.777*** (0.438)	0.213*** (0.002)	1.214*** (0.225)	-0.282*** (0.003)	-1.899*** (0.391)
\bar{Y}	2.482	2.482	1.748	1.748	1.284	1.284	2.012	2.012

The outcome variables are whether one had a gainful occupation in 1940 (*100), weeks worked in 1939, weekly hours worked in 1940, the natural log of wages reported in 1940, and whether the woman was a stenographer, a clerical worker, a bookkeeper, or a housekeeper (*100). The independent variable is the number of years of education completed. Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample is White, native-born women who were born between 1885 and 1912 (N=18836237). Column 4 further restricts the sample to working women who earned positive wages (N=4151245).

Table 4: The Returns to Education on Labor Market Outcomes By Socioeconomic Background

	(1) Gainful Occ.	(2) Weeks Worked	(3) Hours Worked	(4) OccScore	(5) ln(Wage)	(6) Steno- grapher	(7) House- keeper
<i>Panel A: Father's OCCSCORE > Median</i>							
Educ	4.317* (1.936)	2.211* (0.907)	1.368 (0.731)	1.259** (0.465)	0.164** (0.052)	2.926*** (0.837)	-0.431 (0.251)
\bar{Y}	24.079	10.155	8.210	5.600	6.518	3.159	0.897
<i>Panel B: Father's OCCSCORE ≤ Median</i>							
Educ	9.057*** (1.747)	3.865*** (0.751)	2.918*** (0.551)	2.332*** (0.437)	0.185*** (0.047)	1.798*** (0.493)	-0.676* (0.290)
\bar{Y}	16.167	7.718	6.172	3.462	6.145	0.965	1.132
<i>Panel C: Children from Farm Households</i>							
Educ	8.835*** (1.497)	3.857*** (0.641)	2.858*** (0.518)	2.207*** (0.375)	0.168* (0.065)	1.163*** (0.293)	-0.511 (0.290)
\bar{Y}	15.095	7.449	5.954	3.161	6.043	0.638	1.132
<i>Panel D: Children from Non-farm Households</i>							
Educ	3.290 (1.829)	1.620 (0.840)	1.236 (0.682)	1.043* (0.438)	0.201*** (0.046)	2.418** (0.753)	-0.359 (0.242)
\bar{Y}	23.391	9.912	8.004	5.420	6.501	2.983	0.934
<i>Panel E: Children from Rural Areas</i>							
Educ	8.412*** (1.511)	3.692*** (0.667)	2.777*** (0.504)	2.217*** (0.382)	0.206*** (0.048)	1.493** (0.471)	-0.906** (0.303)
\bar{Y}	16.807	7.828	6.292	3.623	6.134	0.929	1.173
<i>Panel F: Children from Urban Areas</i>							
Educ	0.038 (1.832)	0.304 (0.844)	-0.326 (0.699)	0.235 (0.441)	0.158** (0.056)	3.394*** (0.940)	-0.200 (0.289)
\bar{Y}	25.494	10.704	8.633	5.999	6.605	3.829	0.777

The outcome variables are whether one had a gainful occupation in 1940 (*100), weeks worked in 1939, weekly hours worked in 1940, the natural log of wages reported in 1940, and whether the woman was a stenographer, a clerical worker, a bookkeeper, or a housekeeper (*100). The independent variable is the number of years of education completed. Only IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample in Panel A and B consists of women aged 28 to 55 in 1940 whose fathers had OCCSCORE above and below the median level (N=2148175 and N=2613453). The sample in Panel C and D consists of women aged 28 to 55 in 1940 who were from farm and non-farm households when they were children (N=2266215 and N=2495413). The sample in Panel E and F consists of women aged 28 to 55 in 1940 who were from rural and urban areas when they were children (N=3400574 and N=1361054). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table 5: The Returns to Education on Marriage Market Outcomes

	(1) Never Married		(2) Divorced/ Separated		(3) Age at First Marriage		(4) H's Educ	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	1.361*** (0.017)	-0.410 (0.851)	0.063*** (0.003)	1.430*** (0.317)	0.382*** (0.003)	0.746** (0.272)	0.717*** (0.001)	0.891*** (0.071)
\bar{Y}	12.455	12.455	2.492	2.492	21.616	21.616	9.004	9.004

The outcome variables are whether one had never been married (*100), whether one had been divorced or separated (*100), age at first marriage, and the husband's years of education. The independent variable is the number of years of education completed. Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample is White, native-born women who were born between 1885 and 1912 (N=18836237). Column 3 further restricts the sample to sample-line women (N=710094) since information for age at first marriage is only available for this group. Column 4 further restricts the sample to White, native-born women who were married and with their husband present in the household in 1940 (N=13559925).

Table 6: The Returns to Education on Marriage Outcomes By Socioeconomic Background

	(1) Never Married	(2) Divorced/ Separated	(3) Age at First Marriage	(4) Husband's Education
<i>Panel A: Father's OCCSCORE > Median</i>				
Educ	0.276 (1.687)	0.473 (0.318)	1.370** (0.489)	0.763*** (0.089)
\bar{Y}	12.828	1.726	21.665	9.742
<i>Panel B: Father's OCCSCORE ≤ Median</i>				
Educ	2.496* (1.073)	0.876*** (0.243)	0.329 (0.359)	0.820*** (0.070)
\bar{Y}	7.220	1.154	21.114	8.391
<i>Panel C: Children from Farm Households</i>				
Educ	2.501** (0.893)	0.519* (0.212)	0.136 (0.413)	0.818*** (0.066)
\bar{Y}	6.544	1.039	21.080	8.188
<i>Panel D: Children from Non-farm Households</i>				
Educ	-0.937 (1.579)	0.802** (0.294)	1.546* (0.615)	0.677*** (0.078)
\bar{Y}	12.298	1.695	21.620	9.636
<i>Panel E: Children from Rural Areas</i>				
Educ	1.698* (0.826)	0.664** (0.226)	0.195 (0.428)	0.816*** (0.065)
\bar{Y}	7.110	1.202	21.103	8.467
<i>Panel F: Children from Urban Areas</i>				
Educ	-1.436 (1.787)	0.488 (0.399)	2.045* (0.865)	0.535*** (0.116)
\bar{Y}	14.565	1.826	22.037	10.062

The outcome variables are whether one had never been married (*100), whether one had been divorced or separated (*100), age at first marriage, and the husband's years of education. The independent variable is the number of years of education completed. Only IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample in Panel A and B consists of women aged 28 to 55 in 1940 whose fathers had OCCSCORE above and below the median level (N=2148175 and N=2613453). The sample in Panel C and D consists of women aged 28 to 55 in 1940 who were from farm and non-farm households when they were children (N=2266215 and N=2495413). The sample in Panel E and F consists of women aged 28 to 55 in 1940 who were from rural and urban areas when they were children (N=3400574 and N=1361054). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table 7: The Returns to Education on Match Quality - Married Women Only

	(1) H's Unemployed		(2) H's OccScore		(3) H's Weeks Worked	
	OLS	IV	OLS	IV	OLS	IV
Educ	-0.919*** (0.009)	-3.884** (1.427)	0.411*** (0.004)	0.348 (0.531)	0.380*** (0.005)	0.825 (0.631)
H's Educ	-0.838*** (0.008)	0.788 (0.783)	1.080*** (0.005)	1.115*** (0.291)	0.480*** (0.005)	0.237 (0.347)
\bar{Y}	13.0	13.0	25.8	25.8	43.0	43.0
	(4) H's Hours Worked		(5) H's ln(Wage)		(6) H's Nonwage Earnings > \$50	
	OLS	IV	OLS	IV	OLS	IV
Educ	0.568*** (0.006)	0.723 (0.849)	0.053*** (0.001)	0.074 (0.042)	1.261*** (0.020)	-2.526 (3.100)
H's Educ	0.365*** (0.007)	0.280 (0.467)	0.073*** (0.000)	0.062** (0.023)	0.077*** (0.017)	2.154 (1.700)
\bar{Y}	38.0	38.0	7.0	7.0	46.8	46.8

The outcome variables are whether the husband was unemployed or employed on public emergency work projects in 1940 (*100), the husband's occupational score, the husband's weeks worked in 1939, the husband's hours worked per week, the husband's natural log of wage earnings, and whether the husband owned the home (*100). Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The independent variables are the number of years of education completed by the wife (instrumented in the IV regressions) and the number of years completed by the husband. The sample is White, native-born women who were married and with her husband present in the household in 1940 (N=13559925). Column 5 further restricts the sample to households where the husband was earning positive wages in 1940 (N=9347049). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table 8: The Returns to Education on Match Quality By Socioeconomic Background

	(1) H's Unem- ployed	(2) H's OccScore	(3) H's Weeks Worked	(4) H's Hours Worked	(5) H's ln(Wage)	(6) H's Nonwage > \$50
<i>Panel A: Father's OCCSCORE > Median</i>						
Educ	-1.937 (1.620)	0.492 (0.562)	0.955 (0.817)	0.732 (0.948)	0.080 (0.045)	4.040 (2.421)
\bar{Y}	11.716	28.683	43.895	38.008	7.232	32.513
<i>Panel B: Father's OCCSCORE ≤ Median</i>						
Educ	-5.777*** (1.390)	0.531 (0.463)	2.238*** (0.631)	2.043* (0.996)	0.075 (0.040)	-9.635*** (2.792)
\bar{Y}	12.797	22.842	43.047	39.049	6.810	48.074
<i>Panel C: Children from Farm Households</i>						
Educ	-5.565*** (1.371)	1.292* (0.512)	1.675** (0.597)	1.418 (1.107)	0.072 (0.041)	-11.598*** (3.015)
\bar{Y}	12.601	21.894	42.995	39.333	6.717	50.375
<i>Panel D: Children from Non-farm Households</i>						
Educ	-1.747 (1.476)	0.220 (0.511)	1.129 (0.711)	0.907 (0.833)	0.076 (0.041)	2.438 (2.208)
\bar{Y}	12.004	28.267	43.786	38.016	7.208	33.749
<i>Panel E: Children from Rural Areas</i>						
Educ	-3.792** (1.322)	1.092* (0.551)	1.955** (0.640)	1.875 (1.031)	0.100* (0.042)	-9.352** (3.034)
\bar{Y}	13.212	23.340	42.817	38.595	6.827	45.615
<i>Panel F: Children from Urban Areas</i>						
Educ	-1.553 (1.655)	0.285 (0.549)	0.470 (0.819)	1.145 (0.974)	0.045 (0.040)	0.931 (2.566)
\bar{Y}	10.758	29.821	44.506	38.342	7.336	31.149

The outcome variables are whether the husband was unemployed or employed on public emergency work projects in 1940 (*100), the husband's occupational score/weeks worked in 1939/hours worked per week/natural log of wage earnings, and whether the husband earned more than \$50 in nonwage earnings in 1940. Only IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The independent variables are the number of years of education completed by the wife (instrumented in the IV regressions) and the number of years completed by the husband. The sample in Panel A and B consists of married women aged 28 to 55 in 1940 whose fathers had OCCSCORE above and below the median level (N=1675655 and N=2222345). The sample in Panel C and D consists of married women aged 28 to 55 in 1940 who were from farm and non-farm households when they were children (N=1937002 and N=1960998). The sample in Panel E and F consists of married women aged 28 to 55 in 1940 who were from rural and urban areas when they were children (N=2874004 and N=1023996). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table 9: Assortative Mating Based on Father's Status

	(1) H Father's OccScore		(2) H Father's OccScore Rank		(3) Diff in OccScore		(4) Diff in OccScore Rank	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	0.179*** (0.006)	2.438** (0.855)	0.037*** (0.002)	1.159*** (0.324)	-0.003*** (0.000)	-0.002 (0.028)	-0.006*** (0.000)	-0.119** (0.044)
H's Educ	0.736*** (0.007)	-0.531 (0.479)	0.200*** (0.002)	-0.429* (0.182)	0.015*** (0.000)	0.014 (0.016)	0.009*** (0.000)	0.072** (0.025)
\bar{Y}	19.090	19.090	3.984	3.984	0.295	0.295	0.349	0.349

The outcome variables are the husband's father's OccScore, the husband's father's OccScore rank (measured in deciles), the difference between the wife's and the husband's father's OccScore, and the difference between the wife's and the husband's father's OccScore rank (measured in deciles). The independent variables are the number of years of education completed by the wife (instrumented in the IV regressions) and the number of years completed by the husband. Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample is White, native-born women who were married and with her husband present in the household in 1940, both linked backwards to their childhood censuses using the Census Tree data (N=1948426).

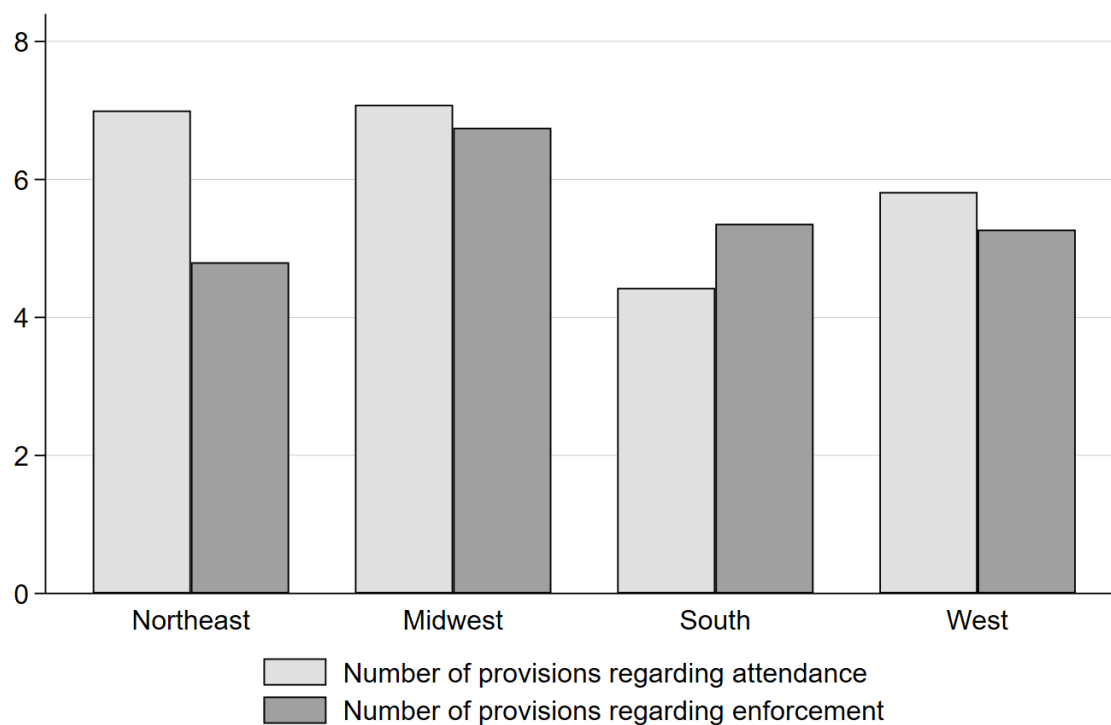
10 Appendix

Figure A1: Provisions Regarding Compulsory Attendance Laws

<i>Provisions Relating to Attendance, Age, Exemptions, Term of Attendance</i>	
1. Minimum compulsory attendance age less than 8.....	(32)
2. Maximum compulsory attendance age 16 or more.....	(42)
3. No exemptions allowed for poverty.....	(29)
4. No exemptions allowed for distance.....	(26)
5. No exemptions allowed for indefinite reasons.....	(31)
6. Eighth grade or more required for work permit.....	(20)
7. Attendance required for full term.....	(39)
8. Nine years or more attendance required.....	(33)
9. School term of 8 or more months.....	(25)
10. Attendance officers certificated.....	(8)
<i>Provisions for Enforcement</i>	
11. Annual or continuous school census.....	(36)
12. Truancy defined.....	(21)
13. Provides when truancy shall cease.....	(29)
14. Penalty on principals or teachers for failure to report truancy.....	(16)
15. Penalty on attendance officers for neglect of duty.....	(19)
16. Requires truancy to be reported immediately.....	(11)
17. Requires attendance officers to act immediately.....	(16)
18. Provides who shall grant exemptions.....	(38)
19. Provides who shall report truancy.....	(37)
20. Regular attendance officer required.....	(45)

The figure shows different provisions regarding compulsory attendance laws in Deffenbaugh and Keesecker, 1935.

Figure A2: Number of Provisions Regarding Compulsory Attendance Laws in Different Regions



The figure shows the number of provisions regarding compulsory attendance laws in different regions with data collected by Deffenbaugh and Keesecker, [1935](#).

Table A1: Factors Related to the Passage of Compulsory Attendance, Child Labor and School Continuation Laws

	(1) Compulsory Attendance	(2) Child Labor	(3) School Continuation
Early Suffrage	0.024 (0.028)	0.042 (0.041)	0.033 (0.036)
GFWC Chapter	-0.021 (0.026)	0.034 (0.021)	0.020 (0.016)
# Farms PC	-2.389*** (0.714)	-0.615 (0.503)	0.016 (0.377)
Farm Value PC	0.002 (0.005)	-0.034*** (0.008)	-0.022*** (0.006)
% Manufacturing	0.117 (0.782)	1.177 (0.904)	2.039** (0.747)
Manufacturing Wages	-0.145 (0.103)	0.238 (0.124)	0.233* (0.106)
% Foreign Born	0.740* (0.320)	1.330*** (0.227)	0.866*** (0.182)
% Women	-1.503 (0.855)	-0.462 (0.615)	0.622 (0.544)
% White	-2.711*** (0.507)	0.051 (0.289)	-0.765** (0.282)
% Literate	-0.175 (0.281)	-0.675*** (0.199)	-0.300* (0.118)
State FE	Yes	Yes	Yes
Region*Year FE	Yes	Yes	Yes
N	1968	1968	1968

The table shows separate regression results where the dependent variables are the adoption of compulsory attendance laws, child labor laws, and school continuation laws. The independent variable for each set of regressions is the following: early suffrage laws, GFWC state chapter, # farms per capita and farm value per capita (in \$1,000), % manufacturing and manufacturing wages, and the share of foreign-born/women/whites/literate in the population. All specifications include state and region by year fixed effects, and standard errors are clustered at the state by year level.

Table A2: The Effect of Compulsory Attendance on Years of Schooling Completed

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Years of Schooling Completed==</i>	<i>One</i>	<i>Two</i>	<i>Three</i>	<i>Four</i>	<i>Five</i>	<i>Six</i>
6 Years	0.0017*** (0.0004)	0.0020*** (0.0005)	0.0029*** (0.0007)	0.0053*** (0.0010)	0.0096*** (0.0015)	0.0141*** (0.0019)
7 Years	0.0009 (0.0006)	0.0009 (0.0007)	0.0010 (0.0009)	0.0027* (0.0012)	0.0058*** (0.0017)	0.0108*** (0.0022)
8 Years	0.0021** (0.0007)	0.0023** (0.0008)	0.0029** (0.0010)	0.0057*** (0.0013)	0.0109*** (0.0018)	0.0177*** (0.0023)
9 Years +	0.0019* (0.0009)	0.0020 (0.0011)	0.0023 (0.0014)	0.0043* (0.0017)	0.0073** (0.0022)	0.0121*** (0.0027)
	(7)	(8)	(9)	(10)	(11)	(12)
<i>Years of Schooling Completed==</i>	<i>Seven</i>	<i>Eight</i>	<i>Nine</i>	<i>Ten</i>	<i>Eleven</i>	<i>Twelve</i>
6 Years	0.0136*** (0.0021)	0.0075*** (0.0021)	-0.0044 (0.0026)	-0.0099*** (0.0023)	-0.0090*** (0.0023)	-0.0069** (0.0023)
7 Years	0.0121*** (0.0025)	0.0067** (0.0025)	0.0087** (0.0030)	0.0017 (0.0028)	0.0027 (0.0030)	0.0046 (0.0029)
8 Years	0.0196*** (0.0027)	0.0124*** (0.0029)	0.0056 (0.0035)	-0.0054 (0.0029)	-0.0062* (0.0029)	-0.0026 (0.0029)
9 Years +	0.0155*** (0.0032)	0.0123*** (0.0034)	0.0405*** (0.0050)	0.0238*** (0.0043)	0.0065 (0.0042)	0.0076 (0.0040)

The table shows results from 12 separate regressions, and each regression has a different outcome variable that indicates whether one completed at least X years of schooling, where X ranges from 1 to 12. The independent variables are dummy variables that indicate different lengths of compulsory attendance laws. The regression includes birth state fixed effects and birth year by region fixed effects. The sample includes white and native-born women who were born between 1885 and 1912 (N=18836237).

Table A3: First Stage Results with An Alternative Set of Instruments

	(1) All Women	(2) Married Women	(3) Working Women	(4) Sample-line Women	(5) No South
<i>Required Years of Schooling</i>					
1 - 5 Years	0.028 (0.018)	0.020 (0.018)	0.038 (0.024)	0.038 (0.027)	0.019 (0.031)
6 Years	0.043* (0.021)	0.028 (0.022)	0.144*** (0.029)	0.063* (0.031)	0.024 (0.030)
7 Years	0.094*** (0.023)	0.079** (0.024)	0.227*** (0.032)	0.115*** (0.033)	0.076* (0.032)
8 Years	0.104*** (0.026)	0.086** (0.026)	0.241*** (0.034)	0.130*** (0.036)	0.079* (0.034)
9 Years +	0.180*** (0.030)	0.173*** (0.031)	0.220*** (0.038)	0.204*** (0.042)	0.155*** (0.038)
N	18836237	14902197	4151245	936920	14784416
F Stats	12.858	12.435	19.443	6.993	12.160

The outcome variable is years of education completed reported in the 1940 census. The independent variables are dummy variables indicating years of required compulsory attendance. The regression includes birth state fixed effects and birth year by region fixed effects. The sample from Column 1 is White, non-South, native-born women who were born between 1885 and 1912. Column 2, 3, 4 and 5 further restricts the sample to currently married women in 1940, working women who earned positive wages in 1940, sample-line women in the 1940 census, and women born outside of the South. The Kleibergen-Paap Wald F-statistic is reported.

Table A4: 1940 Characteristics of Women in the Linked Census Tree Sample and Complete-Count Sample

	(1) Tree	(2) All Women
Age	40.7 (7.6)	39.8 (7.9)
Ever Married	89.7 (30.5)	87.5 (33.0)
Farm	22.9 (42.0)	19.7 (39.8)
Urban	54.7 (49.8)	58.8 (49.2)
Northeast	32.1 (46.7)	29.6 (45.7)
Midwest	40.5 (49.1)	41.9 (49.3)
South	21.5 (41.1)	21.6 (41.2)
Educ	9.6 (3.1)	9.5 (3.2)
Gainfully Employed	20.6 (40.4)	25.9 (43.8)
Self Employed	2.6 (15.9)	3.0 (17.0)
OCCSCORE	4.7 (9.9)	5.7 (10.5)
ln(Wage)	6.4 (1.0)	6.4 (1.0)
Nonwage Earnings > \$50	11.3 (31.7)	12.8 (33.4)
N	4847541	19244286

The outcome variables are age in 1940, whether one had been married in 1940 (*100), farm/urban status in 1940 (*100), whether one lived in the Northeast/Midwest/South region in 1940 (*100), years of education, whether one was gainfully employed/self-employed in 1940 (*100), one's OCCScore in 1940, natural log of wage earnings in 1940, and whether one had non-wage earnings higher than \$50 in 1940 (*100). The sample from Column 1 is the linked Census Tree sample of White and native-born women born between 1885 and 1912. The sample from Column 2 is the complete-count sample of White and native-born women born between 1885 and 1912.

Table A5: Educational Attainment By Socioeconomic Background

	(1) All Women	(2) > Median OccScore	(3) ≤ Median OccScore	(4) Rural	(5) Urban
Years of Schooling	9.6 (3.1)	10.1 (3.0)	9.0 (3.1)	9.1 (3.1)	10.2 (2.9)
Completed 8th Grade (%)	79.2 (40.6)	84.1 (36.6)	73.0 (44.4)	73.9 (43.9)	86.1 (34.6)
Some HS Education (%)	39.5 (48.9)	44.5 (49.7)	33.1 (47.0)	34.3 (47.5)	46.2 (49.9)
Completed 12th Grade (%)	32.6 (46.9)	38.3 (48.6)	25.4 (43.5)	26.7 (44.3)	40.3 (49.0)
<i>N</i>	4847541	2182685	2664856	3465308	1382233

The table shows descriptive statistics on educational attainment by socioeconomic background. The average level of schooling, the share who completed 8th grade, the share who had some high school education, and the share who completed 12th grade are reported. The sample from Column 1 is the linked sample of White native-born women who were born between 1885 and 1912. Columns 2 and 3 select the sample of women based on their fathers' occupational scores (above and below the median), and Columns 4 and 5 select the sample of women based on the rural and urban status of their fathers.

Table A6: The Effect of Years of Schooling on Labor Market Outcomes (No South)

	(1) Gainful Occupation		(2) Weeks Worked		(3) Hours Worked		(4) ln Wage	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	2.333*** (0.020)	11.644*** (2.282)	0.984*** (0.009)	5.135*** (1.011)	0.772*** (0.007)	3.612*** (0.743)	0.113*** (0.001)	0.171*** (0.026)
\bar{Y}	26.484	26.484	11.307	11.307	9.360	9.360	6.439	6.439
	(5) Steno- graphers		(6) Clerical Workers		(7) Book- keepers		(8) House- keepers	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	0.557*** (0.014)	4.462*** (0.880)	0.245*** (0.004)	2.317*** (0.525)	0.219*** (0.003)	1.633*** (0.270)	-0.290*** (0.002)	-2.048*** (0.437)
\bar{Y}	2.714	2.714	1.927	1.927	1.383	1.383	2.113	2.113

The outcome variables are whether one had a gainful occupation in 1940 (*100), weeks worked in 1939, weekly hours worked in 1940, the natural log of wages reported in 1940, and whether the woman was a stenographer, a clerical worker, a bookkeeper, or a housekeeper (*100). The independent variable is the number of years of education completed. Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample is White, non-South, native-born women who were born between 1885 and 1912 (N=14784416). Column 4 further restricts the sample to working women who earned positive wages (N=3371943).

Table A7: The Returns to Education on Labor Market Outcomes By SES (No South)

	(1) Gainful Occ.	(2) Weeks Worked	(3) Hours Worked	(4) OccScore	(5) ln(Wage)	(6) Steno- grapher	(7) House- keeper
<i>Panel A: Father's OCCSCORE > Median</i>							
Educ	6.338** (2.217)	3.062** (1.044)	1.971* (0.833)	1.763*** (0.532)	0.161** (0.052)	3.383*** (0.985)	-0.508 (0.280)
\bar{Y}	24.051	10.125	8.135	5.583	6.537	3.209	0.956
<i>Panel B: Father's OCCSCORE ≤ Median</i>							
Educ	9.508*** (1.723)	3.979*** (0.734)	3.055*** (0.542)	2.406*** (0.429)	0.252*** (0.061)	1.994*** (0.489)	-0.655* (0.286)
\bar{Y}	16.473	7.848	6.289	3.558	6.203	1.129	1.346
<i>Panel C: Children from Farm Households</i>							
Educ	8.416*** (1.381)	3.593*** (0.586)	2.730*** (0.482)	2.066*** (0.344)	0.314** (0.108)	1.266*** (0.277)	-0.433 (0.277)
\bar{Y}	15.043	7.478	6.009	3.161	6.077	0.719	1.394
<i>Panel D: Children from Non-farm Households</i>							
Educ	4.637* (2.062)	2.162* (0.953)	1.624* (0.766)	1.390** (0.495)	0.191*** (0.045)	2.788** (0.873)	-0.444 (0.269)
\bar{Y}	23.495	9.935	7.973	5.440	6.524	3.063	0.994
<i>Panel E: Children from Rural Areas</i>							
Educ	9.362*** (1.566)	4.022*** (0.688)	3.003*** (0.517)	2.416*** (0.395)	0.282*** (0.062)	1.724*** (0.491)	-0.955** (0.312)
\bar{Y}	16.898	7.850	6.312	3.651	6.167	1.021	1.394
<i>Panel F: Children from Urban Areas</i>							
Educ	0.574 (2.013)	0.500 (0.930)	-0.266 (0.760)	0.381 (0.482)	0.150** (0.058)	3.527*** (1.068)	-0.171 (0.321)
\bar{Y}	25.416	10.661	8.547	5.968	6.615	3.809	0.816

The outcome variables are whether one had a gainful occupation in 1940 (*100), weeks worked in 1939, weekly hours worked in 1940, the natural log of wages reported in 1940, and whether the woman was a stenographer, a clerical worker, a bookkeeper, or a housekeeper (*100). The independent variable is the number of years of education completed. Only IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample in Panel A and B consists of non-southern women aged 28 to 55 in 1940 whose fathers had OCCSCORE above and below the median level (N=1832706 and N=1817255). The sample in Panel C and D consists of non-southern women aged 28 to 55 in 1940 who were from farm and non-farm households when they were children (N=1537634 and N=2112327). The sample in Panel E and F consists of non-southern women aged 28 to 55 in 1940 who were from rural and urban areas when they were children (N=2430970 and N=1218991). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table A8: The Returns to Education on Marriage Market Outcomes (No South)

	(1) Never Married		(2) Divorced/ Separated		(3) Age at First Marriage		(4) H's Educ	
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Educ	1.544*** (0.014)	0.333 (0.927)	0.045*** (0.003)	1.590*** (0.361)	0.400*** (0.003)	0.817** (0.293)	0.701*** (0.002)	0.928*** (0.076)
\bar{Y}	13.356	13.356	2.556	2.556	21.912	21.912	9.281	9.281

The outcome variables are whether one had never been married (*100), whether one had been divorced or separated (*100), age at first marriage, and the husband's years of education. The independent variable is the number of years of education completed. Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample is White, native-born women who were born between 1885 and 1912 (N=14784416). Column 3 further restricts the sample to sample-line women (N=552530) since information for age at first marriage is only available for this group.

Table A9: The Returns to Education on Marriage Outcomes By SES (No South)

	(1)	(2)	(3)	(4)
	Never Married	Divorced/ Separated	Age at First Marriage	Husband's Education
<i>Panel A: Father's OCCSCORE > Median</i>				
Educ	0.680 (1.901)	0.654 (0.353)	1.510** (0.548)	0.786*** (0.097)
\bar{Y}	13.255	1.696	21.799	9.785
<i>Panel B: Father's OCCSCORE ≤ Median</i>				
Educ	2.673* (1.051)	0.841*** (0.239)	0.098 (0.380)	0.789*** (0.068)
\bar{Y}	7.700	1.247	21.431	8.790
<i>Panel C: Children from Farm Households</i>				
Educ	2.384** (0.833)	0.389 (0.201)	0.008 (0.430)	0.770*** (0.061)
\bar{Y}	6.761	1.120	21.405	8.562
<i>Panel D: Children from Non-farm Households</i>				
Educ	-0.910 (1.772)	1.016** (0.326)	1.695* (0.689)	0.720*** (0.084)
\bar{Y}	12.806	1.680	21.769	9.731
<i>Panel E: Children from Rural Areas</i>				
Educ	1.818* (0.839)	0.655** (0.229)	0.087 (0.473)	0.813*** (0.065)
\bar{Y}	7.398	1.275	21.369	8.797
<i>Panel F: Children from Urban Areas</i>				
Educ	-1.665 (2.002)	0.810 (0.444)	2.113* (0.916)	0.519*** (0.124)
\bar{Y}	14.871	1.774	22.130	10.044

The outcome variables are whether one had never been married (*100), whether one had been divorced or separated (*100), age at first marriage, and the husband's years of education. The independent variable is the number of years of education completed. Only IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The sample in Panel A and B consists of non-southern women aged 28 to 55 in 1940 whose fathers had OCCSCORE above and below the median level (N=1832706 and N=1817255). The sample in Panel C and D consists of non-southern women aged 28 to 55 in 1940 who were from farm and non-farm households when they were children (N=1537634 and N=2112327). The sample in Panel E and F consists of non-southern women aged 28 to 55 in 1940 who were from rural and urban areas when they were children (N=2430970 and N=1218991). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table A10: The Returns to Education on Match Quality - Married Women Only (No South)

	(1) H's Unemployed		(2) H's OccScore		(3) H's Weeks Worked	
	OLS	IV	OLS	IV	OLS	IV
Educ	-0.973*** (0.011)	-4.677** (1.523)	0.381*** (0.003)	0.731 (0.582)	0.382*** (0.006)	1.828** (0.697)
H's Educ	-0.919*** (0.008)	0.990 (0.785)	1.106*** (0.006)	0.926** (0.299)	0.512*** (0.006)	-0.233 (0.360)
\bar{Y}	13.0	13.0	26.5	26.5	43.1	43.1
	(4) H's Hours Worked		(5) H's ln(Wage)		(6) H's Nonwage Earnings > \$50	
	OLS	IV	OLS	IV	OLS	IV
Educ	0.565*** (0.008)	1.239 (0.903)	0.046*** (0.000)	0.064 (0.042)	1.045*** (0.023)	-7.864* (3.078)
H's Educ	0.370*** (0.009)	0.022 (0.468)	0.070*** (0.000)	0.061** (0.022)	-0.086*** (0.016)	4.503** (1.584)
\bar{Y}	37.8	37.8	7.1	7.1	47.1	47.1

The outcome variables are whether the husband was unemployed or employed on public emergency work projects in 1940 (*100), the husband's occupational score, the husband's weeks worked in 1939, the husband's hours worked per week, the husband's natural log of wage earnings, and whether the husband owned the home (*100). Both OLS and IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The independent variables are the number of years of education completed by the wife (instrumented in the IV regressions) and the number of years completed by the husband. The sample is White, native-born women who were married and with her husband present in the household in 1940 (N=10560740). Column 5 further restricts the sample to households where the husband was earning positive wages in 1940 (N=7528960). The table also shows the mean of the outcome variables \bar{Y} in each category.

Table A11: The Returns to Education on Match Quality By SES (No South)

	(1) H's Unem -ployed	(2) H's OccScore	(3) H's Weeks Worked	(4) H's Hours Worked	(5) H's ln(Wage)	(6) H's Nonwage > \$50
<i>Panel A: Father's OCCSCORE > Median</i>						
Educ	-2.534 (1.772)	0.702 (0.620)	1.525 (0.914)	1.155 (1.025)	0.095 (0.049)	1.606 (2.504)
\bar{Y}	11.851	28.751	43.818	37.720	7.248	31.767
<i>Panel B: Father's OCCSCORE ≤ Median</i>						
Educ	-6.135*** (1.304)	0.421 (0.434)	2.388*** (0.594)	2.634** (0.938)	0.074* (0.038)	-10.053*** (2.600)
\bar{Y}	12.927	23.589	43.191	39.418	6.911	46.541
<i>Panel C: Children from Farm Households</i>						
Educ	-5.267*** (1.191)	0.930* (0.434)	1.709** (0.531)	1.727 (0.973)	0.064 (0.038)	-10.788*** (2.542)
\bar{Y}	12.759	22.421	43.121	39.904	6.809	49.324
<i>Panel D: Children from Non-farm Households</i>						
Educ	-2.674 (1.666)	0.329 (0.570)	1.602 (0.822)	1.226 (0.926)	0.087 (0.046)	0.845 (2.390)
\bar{Y}	12.085	28.466	43.753	37.771	7.233	32.820
<i>Panel E: Children from Rural Areas</i>						
Educ	-4.171** (1.281)	0.874 (0.521)	2.156*** (0.634)	2.306* (1.023)	0.101* (0.042)	-10.509*** (3.002)
\bar{Y}	13.500	23.935	42.839	38.733	6.904	44.025
<i>Panel F: Children from Urban Areas</i>						
Educ	-1.292 (1.753)	0.382 (0.599)	0.778 (0.908)	1.504 (1.062)	0.058 (0.042)	-1.082 (2.824)
\bar{Y}	10.912	29.746	44.385	38.040	7.338	30.646

The outcome variables are whether the husband was unemployed or employed on public emergency work projects in 1940 (*100), the husband's occupational score/weeks worked in 1939/hours worked per week/natural log of wage earnings, and whether the husband earned more than \$50 in nonwage earnings in 1940. Only IV estimates are reported. The regression includes birth state fixed effects and birth year by region fixed effects. The independent variables are the number of years of education completed by the wife (instrumented in the IV regressions) and the number of years completed by the husband (not reported). The sample in Panel A and B consists of non-southern married women aged 28 to 55 in 1940 whose fathers had OCCSCORE above and below the median level (N=1428703 and N=1546579). The sample in Panel C and D consists of non-southern married women aged 28 to 55 in 1940 who were from farm and non-farm households when they were children (N=1317909 and N=1657373). The sample in Panel E and F consists of non-southern married women aged 28 to 55 in 1940 who were from rural and urban areas when they were children (N=2057561 and N=917721). The table also shows the mean of the outcome variables \bar{Y} in each category.