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Empowered Mothers, Empowered Generations: The Impact of Women's Economic Rights

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OCTOBER 2025

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ABSTRACT

Empowered Mothers, Empowered Generations: The Impact of Women's Economic Rights*

This paper examines the long-run effects of women's economic rights on generations exposed to property and earnings acts during childhood. We find that childhood exposure to these reforms reduced the probability of marriage—particularly among women—and increased female labor force participation in adulthood. To explore potential mechanisms, we document several short-run effects among the adult generation contemporaneous to the reforms, including improved occupational standing, reduced fertility, lower child mortality, and increased schooling among children. Taken together, our findings suggest that expanding economic rights for women can shape outcomes across multiple generations, underscoring the enduring importance of legal and institutional reforms that promote women's economic empowerment.

JEL Classification: D13, J12, J16, N31

Keywords: women's economic rights, intergenerational mobility, long-run effects

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1 Introduction

Over the last 200 years, gender roles have evolved significantly, with women increasingly moving beyond traditional domestic roles (Goldin, 2021). Historically, women’s sphere was largely confined to the household, and married women were deprived of economic rights. Legally, they were unable to own property, manage their earnings, or engage in commerce independently (Tertilt et al., 2022). Upon marriage, they became legally subordinate to their husbands. Only in the nineteenth century did women in the United States begin to gain basic economic rights, at a time when suffrage was still limited to men (Chused, 1982; Basch, 1979). Despite substantial progress over the past two centuries, gender disparities persist worldwide. Although women’s legal status is now formally equal to men’s in most countries, gaps remain in labor market outcomes, political participation, and exposure to violence (Pal et al., 2024). In some regions, women still lack fundamental economic rights, such as the ability to own and control property independently (World Bank, 2024).

This paper studies the long-run effects of granting economic rights to women. Throughout the second half of the nineteenth century, U.S. states enacted property and earnings acts that allowed married women to retain their property and wages. We exploit the introduction of these acts to analyze how women’s economic rights affected the next generation’s fertility, marriage, and labor market outcomes. Economic rights for women are expected to redistribute resources within households—shifting them from men to women—and to strengthen women’s bargaining power by increasing their economic independence.¹ Property and earnings acts predated other forms of women’s rights, such as suffrage, by several decades. This allows us to isolate the effects of *economic* empowerment from those of other forms of empowerment, including political participation (Slotwinski and Stutzer, 2023), control over fertility (Goldin and Katz, 2002), and divorce law reforms (Genadek et al., 2007).

Prior research shows that these acts influenced outcomes for married women living at the time of their enactment (e.g., Hazan et al., 2021; Khan, 1996). In this paper, we examine whether changes in women’s economic rights affected not only the contemporaneous generation but also those that followed. Assessing the long-run consequences of women’s economic rights is crucial for understanding persistent gender gaps in labor market outcomes across and within countries (e.g., Black et al., 2014) and for tracing the historical roots of women’s economic progress (e.g., Bertrand et al., 2015). Our findings also shed light on the potential effects of women’s economic empowerment in regions where basic rights remain limited today.

To evaluate the long-run effects of women’s economic rights, we exploit variation in exposure during childhood and examine how this exposure affected fertility, marriage, and labor force participation later in life. We use decennial U.S. Census data from 1860 to 1910. Our results show that individuals exposed to economic rights as children were less likely to marry, with stronger effects for women. We find a pronounced gender difference in labor force participation: effects are significant for women but absent for men. We explore mechanisms behind these patterns by analyzing short-term impacts on the adult generation contemporaneous to

¹In line with the relief of a potential principal-agent problem, economic rights for married women may also reduce inefficiencies in household decision-making (Tertilt et al., 2022).

the reforms. Economic rights increased women’s occupational standing, raised human capital, reduced fertility, and increased children’s schooling. Using newly digitized Vital Statistics data, we further show that granting economic rights to women decreased child mortality. In the short run, we do not find evidence that these rights affected marital decisions or labor force participation, suggesting that some effects unfolded only gradually, influencing future generations.

This study contributes to several strands of literature. First, it adds to research on the effects of women’s economic rights as granted by married women’s property and earnings acts. Prior studies show that these reforms influenced household portfolios (Hazan et al., 2019), investment and bankruptcy decisions (Koudijs and Salisbury, 2020), and female patenting (Khan, 1996). Most closely related, Hazan et al. (2021) document that the acts reduced fertility among married women, while Geddes et al. (2012) find increases in girls’ school attendance relative to boys’. Both studies focus on the immediate effects of the reforms. We extend this literature by showing that women’s economic rights also had long-term, intergenerational consequences.

Second, we contribute to research on the persistent effects of legal and social change. Exposure to women’s suffrage during childhood increased educational attainment for disadvantaged children (Kose et al., 2021) and longevity in adulthood (Noghanibehambari and Noghani, 2023). Compulsory schooling laws have been shown to reduce delinquent behavior (Chalfin and Deza, 2019) and improve educational outcomes for the next generation (Oreopoulos et al., 2006). Similarly, the rise in female labor force participation during World War II reshaped gender norms for subsequent generations (Fernández et al., 2004). To our knowledge, ours is the first paper to examine the long-term effects of childhood exposure to women’s economic rights.

Finally, we contribute to the broader literature on female economic empowerment, which has focused primarily on developing countries. Prior work shows that empowering women can enhance market efficiency (Udry, 1996; Goldstein and Udry, 2008), increase investment in children (Lundberg et al., 1997; Duflo, 2003), affect fertility (Tandel et al., 2023), and improve child health (Thomas, 1993; Hossain and Nikolov, 2021). We add to this literature by examining the isolated effects of women’s economic rights at a time when women lacked other forms of empowerment.

The rest of the paper proceeds as follows. Section 2 provides historical context on the property and earnings acts. Section 3 describes the data, and Section 4 outlines our identification strategy for estimating long-run effects. Section 5 presents the main results, Section 6 discusses robustness and identification challenges, and Section 7 explores potential mechanisms. Section 8 concludes.

2 Historical Context

Historically, the legal system of coverture restricted married women’s economic rights in the US. Once married, women’s property, earnings and general economic activities came under the control of their husbands. Married women were not able to sell property or dispose of revenues from any property. This concerned any property acquired before or during the marriage

(Shammas, 1994; Chused, 1982; Geddes and Tennyson, 2013). Married women could not sue, be sued, have wills or make contracts. In case of divorce, men gained custody over children (Geddes and Lueck, 2002).

Throughout 1850 until 1920, nearly all states altered married women's legal position by giving them economic rights². Property acts allowed married women to control their own property. Earnings acts granted married women access to control their own earnings. Geddes and Tennyson (2013) provide a comprehensive overview on the timing and contents of married women's property and earning acts introduced in the US. On average, earning acts were passed almost eight years after property acts (Geddes and Tennyson, 2013). For the timing of the acts see figure A2 and A1 in the appendix, as well as table A1. We discuss reasons for and consequences of property and earnings acts in more detail in section A in the appendix.

The salience of earnings and property acts is reflected by newspaper articles covering legal changes, court cases, and legal scholar's opinions on earnings and property acts (Hazan et al., 2025). Literacy among the white population at the time was already high, with literacy rates at around 90%. Literacy rates for the black population are lower, ranging between 20-65% depending on the census year³. Existing literature showing short run results of economic rights for married women in the US further substantiates their salience - see section A in the appendix.

Women's access to earnings and property at the time was limited, but not impossible. Single women's labor force participation became increasingly common. Married women rarely worked: In 1890, 4.6 percent of married women were in the labor force. With 2.5 percent, the participation rate was even lower for white married women (Goldin, 1977). Generally, women worked mostly in low-paying industries and occupations which further limited their ability to accumulate assets through labor income (Chused, 1982). Beyond participation on the labor market, women had access to assets via dowry and inheritance. Nevertheless, women only owned a minor share of property prior to the legal changes (Shammas, 1994).

3 Data

We use decennial US census microdata for the years 1860, 1870, 1880, 1900, and 1910⁴. The data is cross-sectional and representative of the population. It includes the information crucial for our identification: each individual's birth place and age, which we use to construct their year of birth⁵.

We restrict our sample to the white and native population for the main results. Nativity is essential to our identification strategy, as we assign the timing of earnings and property acts based on state of birth. We restrict the sample to the white population, as we believe salience was higher among them due to higher literacy rates (see figure C1), and access to earnings and

²Some states passed debt statutes already in the first half of the 19th century. According to Chused (1982), these laws were only designed to shield husbands from creditors and did not give married women significant rights.

³There are little gender differences within racial groups. Literacy rates over time can be found in figure C1.

⁴1890 Census data is not available.

⁵Year of birth is constructed by subtracting an individual's age from the survey year. For the years 1900 and 1910, the data would also include self-reported year of birth. For comparability, we use the age variable to construct year of birth for all census years.

property was extremely limited for the black population at the time. Slavery was abolished in the US only in 1865. We report findings on the black population in the appendix in section C.4.

We include individuals aged 20-40 for our main results. While our identification relies on exposure to economic rights during childhood, we are interested in how that translates into changes in marriage, fertility, and labor market participation *upon adulthood*. We use the following outcomes: a dummy variable for being married ⁶, the age gap to one’s spouse, a dummy variable for having children, the number of children, a dummy variable for labor force participation ⁷. For more details on the construction of these outcome variables, see table B1 in the appendix. Other potentially interesting outcomes would be educational attainment or the probability to get divorced. Unfortunately, information on educational attainment is not available for any of the census years in our sample (collected since 1940), and information on divorce is only available since 1880.

Table 1: Summary Statistics

	N	Min	Max	Mean	SD	N	Min	Max	Mean	SD
	<i>Panel A: Men</i>					<i>Panel B: Women</i>				
Age	236458	20	40	29.203	6.125	232298	20	40	29.063	6.129
Literate	236458	0	1	0.945	0.228	232298	0	1	0.937	0.243
Farm	236458	0	1	0.43	0.495	232298	0	1	0.413	0.492
Urban	236458	0	1	0.294	0.456	232298	0	1	0.31	0.462
Labor Force Participation	236458	0	1	0.946	0.225	232298	0	1	0.15	0.357
Married	236458	0	1	0.552	0.497	232298	0	1	0.672	0.47
Age gap to Spouse	130415	0	50	4.367	3.59	156024	0	75	5.753	5.416
Has child u.5	236458	0	1	0.352	0.478	232298	0	1	0.439	0.496
Nr children u.5 (cond. on having children)	83197	1	5	1.531	0.654	102033	1	5	1.532	0.651

Table 1 shows summary statistics for the sample used in the analyses of long-run effects of economic rights. Sample: white and native, aged 20-40. Data from the US Census 1860, 1870, 1880, 1900, 1910.

Table 1 shows descriptive statistics for the sample we use in our main analyses. The sample is balanced by gender, comprising 236,458 men and 232,298 women. The average age is 29, as we restrict our sample to individuals aged 20-40. Over 90% of the sample are literate, more than half do not live on farms, and around 30% live in urban areas. While men and women have similar characteristics on most dimensions, a pronounced discrepancy emerges in terms of their labor force participation: 95% of men are labor force participants compared to only 15% of women. Women in the sample are also more likely to be married than men. The majority of individuals have children under the age of 5 - among individuals with children, the average number of children is 1.5.

4 Empirical Strategy

In our identification strategy, we exploit variation in individuals’ exposure to women’s economic rights during childhood. Two separate acts contribute to women’s economic rights: earnings acts and property acts. 19 states introduce both acts at the same time, 21 states grant property rights before earnings rights, and 8 states enact earnings acts before property acts.

⁶As marital status is only available since 1880, we use the variable SPLOC as a proxy for being married. The variable links spouses within household and allows us to create a dummy indicating whether somebody is married, i.e. has a spouse. We use this definition for all census years for comparability.

⁷From 1850 to 1930, labor force participation is defined as reporting any gainful employment - i.e. an occupation that generates income.

This variation raises a key conceptual question: which timing is most relevant for identifying exposure effects? Is it the introduction of the first act, either act individually, or the point at which women had both rights? Much of the existing literature uses the timing when both property and earnings rights were granted (Geddes and Lueck, 2002; Hazan et al., 2021), some studies look at effects of reforms separately (Khan, 1996). The relevant timing for long-run exposure effects - the focus of this study - may differ from that used in short-run analyses. Long-run effects may stem not only from full legal equality, but from changes set in motion by early reforms, even if they were partial or incomplete at the time.

In our main analysis, we therefore use the timing of property acts to identify potential exposure effects. We do so for two reasons: On average, property acts were introduced earlier than earnings acts (Geddes and Tennyson, 2013). Also, at the time, female labor force participation - especially among married women - was very low (Goldin, 1977), suggesting that rights to own and control property may have been the more binding constraint on women's economic agency. We report results for exposure to earnings acts, to the first act (of either type), and to comprehensive economic rights (earnings + property acts) in section 6.3.3.

Our sample includes two types of states based on the timing of property acts. For 34 states, the timing of property acts allows us to observe treated and untreated cohorts across census years 1860-1910, providing within-state variation in exposure. Additionally, there are 5 states where no cohorts were treated throughout our observation period; they serve as part of a control group. The following states are included in our main sample:

- **Untreated States:** Alabama, Florida, Louisiana, Tennessee, Texas
- **Treated States:** New York (1848), Pennsylvania (1848), Wisconsin (1850), New Jersey (1852), Maine (1855), Massachusetts (1855), Michigan (1855), Kansas (1858), Maryland (1860), New Hampshire (1860), Colorado (1861), Illinois (1861), Ohio (1861), North Carolina (1868), South Carolina (1868), West Virginia (1868), Arizona (1871), Rhode Island (1872), Arkansas (1873), Delaware (1873), Georgia (1873), Iowa (1873), Missouri (1875), Connecticut (1877), Virginia (1877), Oregon (1878), Indiana (1879), Mississippi (1880), Vermont (1881), Washington (1881), New Mexico (1884), Montana (1887), Kentucky (1894), Idaho (1903)

We exclude Alaska, Hawaii, DC following Geddes and Tennyson (2013). We additionally exclude 9 states as there is only negligible variation in the treatment status of the cohorts, with the majority (i.e. 95% or more of all individuals) being already treated⁸. Figure C2 displays how cohorts from each state enter in our sample.

The variation in the timing of property acts across states creates variation in treatment status *within* birth cohorts; additionally within states, there is variation in treatment status based on individuals' year of birth. Exploiting the variation in exposure to women's economic rights on state \times year of birth level, we estimate the following equation:

$$Y_{ics} = \beta_0 + \sum_{k \neq 16, 17} \beta_k \mathbf{1}[\text{Age_at_Treat}_{cs} = k] \cdot \text{Treated}_{cs} + \gamma_1 X_{ics} + \gamma_2 X_{cs} + \delta_c + \sigma_s + \theta_t + \alpha_a + \epsilon_{ist} \quad (1)$$

⁸We vary these restrictions in robustness checks; it does not affect our findings, see section 6.3.2.

, where i refers to an individual born in state s in year c .

The coefficients of interest are β_k , where subscript k refers to the age at the introduction of property acts. The identifying variation is on the state \times year of birth level. For each coefficient, we bin 2 ages together. As a reference category, we use binned individuals who were exposed at ages 16 and 17. In our choice of the omitted category we follow [Kose et al. \(2021\)](#). 16 is the usual school-leaving age. We label cohorts who were older than 17 at the introduction of property acts in their state as “not exposed”, i.e. not exposed during childhood. The intuition is that individuals only exposed after leaving schools are already on a path in terms of their planned fertility, marital decisions, and labor force participation. We can directly test whether this assumption is valid by examining the statistical significance of the coefficients of the “not exposed” cohorts. We label cohorts who were already born at the introduction of property acts in their state, but younger than 16 as “partially exposed”; and cohorts born after the introduction of property acts as “fully exposed”.

We include as individual level controls X_{ics} a dummy for being literate, living in urban area, living on farm, and being female. We include as time-varying state controls X_{cs} the urbanization rate and literacy rate to capture state-specific developments.

We include several fixed effects: cohort fixed effects δ_c to capture common shocks specific to individuals born in a specific cohort, such as overall macroeconomic conditions; state fixed effects σ_s capturing time-invariant characteristics of states, such as long-standing institutional, cultural, or geographic differences; survey year fixed effects θ_t absorbing all factors specific to each census year, such as the timing of the survey or other aspects of data collection; age fixed effects α_a to control for general life-cycle effects that vary systematically with age ⁹.

Standard errors are clustered at the state level.

5 Results

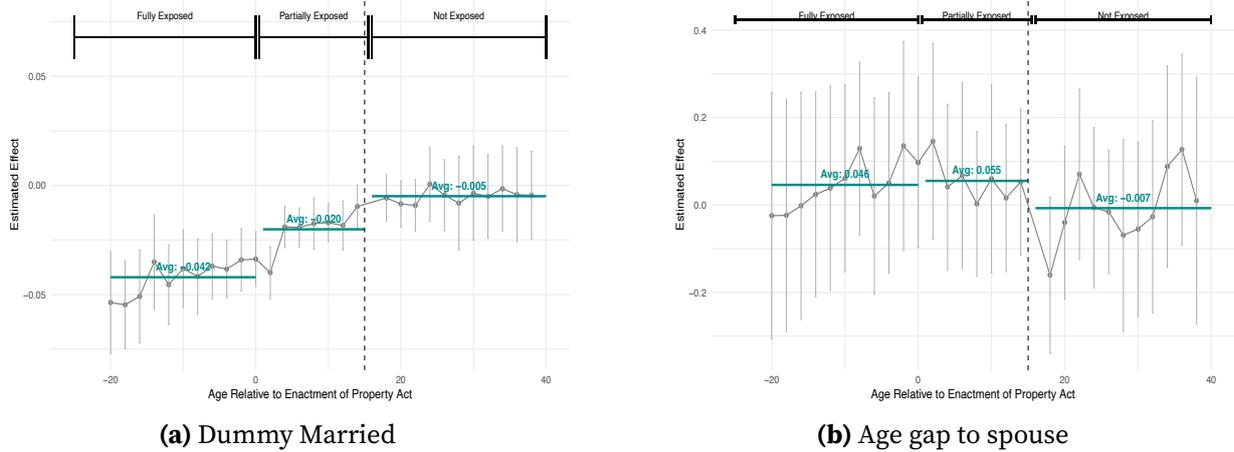
Women’s economic rights may shift gender norms and outcomes of following generations, which may manifest in changes in fertility, marriage decisions, and labor force participation. To test that, we present result from estimating equation 1 in the following section.

5.1 Marriage

Figure 1 shows the effect of exposure to property acts during childhood on marriage decisions. As outcomes, we look at a dummy indicating being married in subfigure 1a, and for the subsample of married individuals the age gap to one’s spouse in subfigure 1b as an indicator for match quality. These marriage outcomes are observed upon adulthood, i.e. among individuals aged 20-40. The horizontal axis in the plots displays the age at which a property act was introduced in one’s state of birth. We group cohorts into three categories: “not exposed”, “partially exposed”, and “fully exposed”. Individuals who were already 18 or older as at the time of the legal change are classified as *not exposed*. While these individuals will be exposed to the reforms as adults, they were *not exposed* during their childhood. We do not see any statistically significant effects of property acts on the marriage decisions of these cohorts who were not

⁹We observe individuals of each birth cohort in multiple census years, which allows us to control for age- and birth year fixed effects.

Figure 1: The effect of exposure to property acts during childhood on marriage decisions



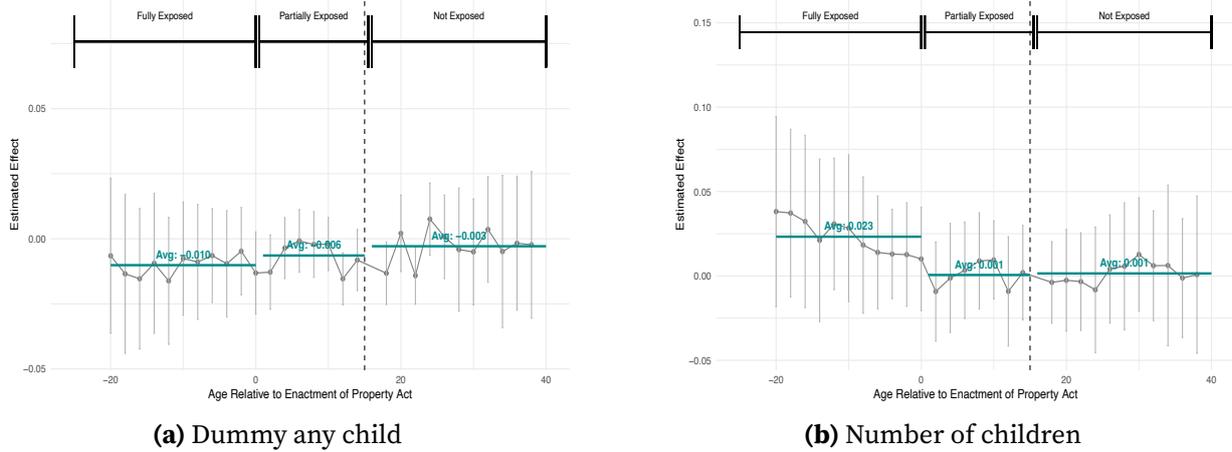
Notes: Figure 1 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married in figure 1a, and the age gap to one's spouse in subfigure 1b. Results displayed in subfigure 1b are based on the subsample of married individuals. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

exposed during childhood, but only afterwards. This is in line with papers showing that marriage decision of contemporaneous cohorts - i.e. individuals who were adults around the time of the reform - were not affected by women's economic rights (Hazan et al., 2021).

Next, we turn to partially and fully exposed cohorts. We find that cohorts partially exposed to property acts during their childhood display a 2pp lower probability to be married; the probability of being married is 4.2pp lower among fully exposed cohorts. Relative to the sample mean, this effect translates into a drop in the probability to be married by 7 percent. We split our sample by gender in figure C4 and we find that the decrease in the probability to be married is slightly larger for women: Among fully exposed cohorts, men are 3pp (5.4% relative to the sample mean) less likely to be married and women 5pp (7.4% relative to the sample mean).

Also match quality of couples might change to women's improved property rights. The historical data only allows us to explore match quality on the basis of spousal age differences. We test for changes in the age gap to one's spouse in subfigure 1b. We do not observe any statistically significant changes in the age gap to one's spouse among cohorts partially or fully exposed to property rights. While we are not able to provide a comprehensive measure of match quality, the evidence suggests that, at least in terms of age composition, the spousal matching remains unaffected. Figure C5 does not unveil any differences by gender.

Figure 2: The effect of exposure to property acts during childhood on fertility



Notes: Figure 2 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5 in figure 2a, and the number of children below the age of five in subfigure 2b. Results displayed in subfigure 2b are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

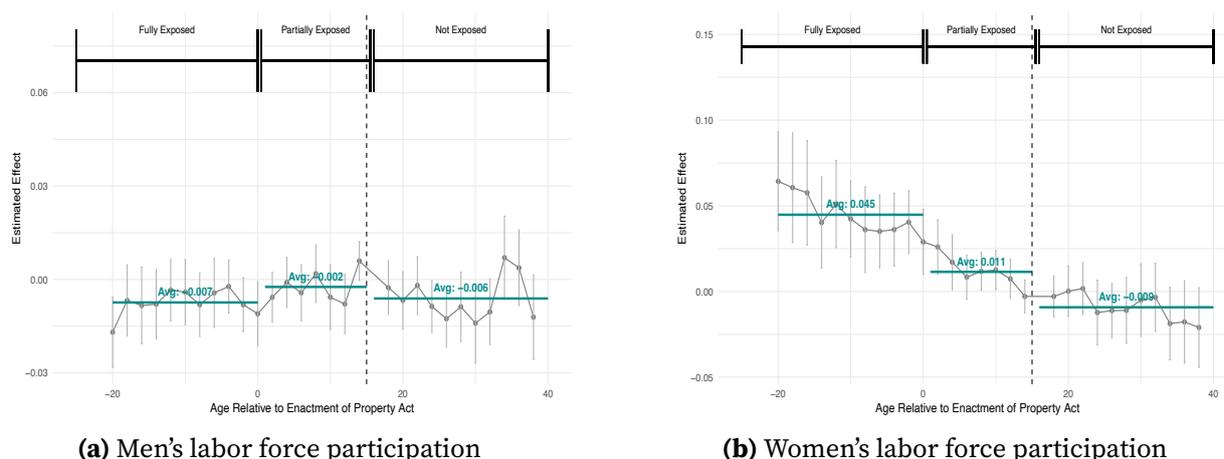
5.2 Fertility

Next, we turn to the effect of exposure to property acts during childhood on fertility outcomes. As outcomes, we use a dummy for having any child below the age of 5 in subfigure 2a, and in the subsample of individuals with children, the number of children below the age of 5 in subfigure 2b. Once again, these outcomes are observed upon adulthood, among individuals aged 20-40.

We do not find any statistically significant changes in the probability to have children, or the number of children. Coefficients estimated for the effect of exposure to property acts on number of children among fully exposed cohorts are positive, however not statistically significant. We report findings for both these outcomes without restriction on children's age in figure C8 in the appendix - they are similar to the findings reported in figure 2. The only notable difference is that we find a reduction in the probability of having children that is marginally significant for partially exposed, and statistically significant for fully exposed cohorts. Splitting the sample by gender, further shows no effects on the number of children for either gender (figure C6), but a statistically significant effect on the probability to have children for women only (figure C7).

How can these findings be consolidated with the substantial negative fertility effects documented by Hazan et al. (2021)? The key distinction lies in the sample and timing of exposure used for identification. Hazan et al. (2021) focus on married women who were adults at the time the laws were introduced, capturing immediate responses to expanded economic rights.

Figure 3: The effect of exposure to property acts during childhood on labor force participation



Notes: Figure 3 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy for labor force participation in both subfigures. Subfigure 3a shows results based on the subsample of men, subfigure 3b the subsample of women. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Our study examines exposure to these rights during childhood, aiming to capture longer-run effects. Our results do not contradict the observed fertility declines among adult women; they indicate that there are no or only negligible (additional) fertility effects driven by childhood exposure to property acts.

5.3 Labor Force Participation

As our last outcome, we turn to labor force participation. As labor force participation was very much gendered at the time, we report findings by gender - men's labor force participation is reported in subfigure 3a, and women's labor force participation in subfigure 3b. We do not find any statistically significant changes in men's labor force participation tied to their exposure to women's property rights. For women, we find a statistically significant increase in the labor force participation among partially and fully exposed cohorts. We do not see any statistically significant effects of property acts on the labor force participation on cohorts who were only exposed after the 17th birthday, i.e. the "not exposed" group. This is in line with prior research documenting that earnings and property acts did not affect women's labor force participation in the short run (Roberts, 2014). The coefficients suggest a increase in women's labor force participation among partially exposed cohorts by 1.1pp - the coefficients are marginally significant. For fully exposed cohorts, we find a sizable and statistically significant increase in the probability to participate in the labor force by 4.5pp. Relative to the sample mean, this corresponds to an increase of 30 percent.

The findings discussed in this section underscore the importance of the long-run perspective. Short-run analyses indicate that women’s economic rights did not affect marriage decisions (Hazan et al., 2021) or female labor force participation (Roberts, 2014). The long-run analysis presented in this paper suggests that, while these effects may not be immediate, they unfold for the generations to follow. Taking into account both short- and long-run perspective allows us to better gauge the full impact of women’s economic rights.

In all main figures 1 - 3, the sample consists of the white population. This allows us to abstract from any changes in fertility, marriage, or labor force participation tied to race. We report findings for black individuals in section C.4 in the appendix. We do not detect any effects of exposure to property acts in the black population. This lack of an effect is not unexpected, as black individuals likely faced greater barriers to asset ownership. Moreover, lower literacy rates may have reduced awareness of the reforms.

6 Identification Challenges

In this section, we address possible identification challenges that come with our identification strategy and provide several robustness checks.

6.1 Timing of Laws

Our identification relies on variation in the adoption of property acts across states. The underlying identification assumption is that the timing of the laws is not correlated with unobserved trends in outcomes across states. We address this identification challenge in several ways.

First, we expand the pre-period - i.e. not exposed cohorts - to check whether the cut-off we choose for the main figures for our event studies mask pre-trends in outcomes. The corresponding results can be found in figures C12, C13, and C14. They do not unveil pre-trends.

Second, we test whether the timing of property acts is predicted by state characteristics as of 1860:

$$Y_s = \alpha + X_s^{1860} \beta + \epsilon_s \quad (2)$$

, where Y_s is the year in which a property act was enacted in state s . X_s^{1860} contains collapsed state-level characteristics; most importantly our outcome variables in 1860.

The aim is to evaluate whether outcomes prior to the roll-out of economic rights for married women predict the adoption of these laws. In the absence of selection effects, we should find that the coefficients β are not statistically different from zero. Table C1 presents the results from that exercise. We do not see any statistically significant relationship between state characteristics and the timing of women’s economic rights.

6.2 Confounders

Another identifying assumption is that there are no events confounding with women’s economic rights. We collect information on the timing of compulsory schooling laws, statehood, child labor laws, suffrage, and women’s labor laws (night work and seating laws). In Figure C3, we plot the distribution of the time intervals between the enactment of each of these laws and property acts. The majority of them were passed decades before property acts (e.g. statehood),

or decades after property acts (e.g. suffrage or women’s labor laws). Out of these potential reforms, compulsory schooling laws are closest to the timing of property acts. To address this potential confounder, we (1) introduce a dummy variable indicating whether an individual was exposed to compulsory schooling laws as an additional control; (2) exclude cohorts exposed to compulsory schooling laws.

For all robustness checks discussed throughout this section, we report the coefficients for $k=-10$, $k=8$, and $k=28$ in table C2, where k refers to the period relative to the enactment of the property act in $k=0$. The first row of the table presents the coefficients from the main specification to allow for a direct comparison. We provide estimates from robustness checks concerning the confounding effect of compulsory schooling laws in panel A of table C2. Both these robustness checks support that compulsory schooling laws are not the drivers of the long-run effects we document. The coefficients are very similar to those obtained from our main specification reported in the top row of the table.

6.3 Robustness

6.3.1 Varying Sample

Migration. An additional concern is selective migration, if cohorts migrate to states with higher levels of women’s economic rights. Therefore we run a robustness check excluding all individuals who do not live in their state of birth anymore. The coefficients we estimate from this subsample can be seen in panel B of table C2. They support our findings.

Additionally, we test whether exposure to property acts affects migration decisions directly. The results can be seen in figure C15. We do not find any evidence that exposure to property acts affects migration decisions.

Stricter inclusion threshold. For our main sample, we exclude 9 states as there is only negligible variation in the treatment status of the cohorts, with the majority (i.e. 95% or more of all individuals) being already treated. In a robustness check, we apply a stricter exclusion threshold and exclude 5 additional states where 90% or more of all individuals are treated. The results from this exercise can be seen in panel B of table C2. We find similar results under this stricter inclusion threshold for treated states.

6.3.2 Varying Specification

Next, we test the robustness of our findings to variations in the specification: (1) we estimate the main equation without including any controls other than the fixed effects; (2) we include state-specific age trends $\chi_s \times a$ to account for systematic differences across states in the evolution of our outcomes over individuals’ adult life. We report coefficients from estimating the specification under these two alterations in panel C of table C2. The coefficients support our main findings.

6.3.3 Varying Exposure Measure

In our main specification we exploit childhood exposure to property acts. Earnings acts additionally expand married women’s economic rights. In the majority of states, they were enacted either at the same time or later than property acts. We explore the effects of exposure to earn-

ings acts in section C.6.1 in the appendix. We adapt our sample, to follow the same rules we apply for the main results, but now relative to earnings acts. There are 11 states where we do not observe cohorts affected by the earnings act - they now enter the sample as control states. We exclude 6 states for lack of variation in exposure to earnings acts, with the the majority (at least 95%) of all individuals being already treated during our observation period. For 31 states, we observe within-state variation in the exposure to earnings acts - they enter as part of the treatment group. We do not detect any statistically significant coefficients for cohorts exposed to earnings acts during their childhood for marriage, fertility, or labor force participation. Even though the coefficients are not statistically significant, their direction is in line with the effects we document for exposure to property acts. This is not surprising, as the enactment of both laws coincides in several states. We will show in section 7, that the weaker effects for exposure to earnings acts is in line with the short-run effects also being more pronounced for property acts. Intuitively, we may also expect the property acts having a stronger impact, as women's - especially married women's - labor force participation was very low at the time.

We report results on exposure to any act (earnings *or* property act) in section C.6.2, and exposure to both acts in section C.6.3. Again, effects are similar to those obtained from our main specification, but captured more imprecisely.

7 Mechanisms

In a next step, we explore potential mechanisms behind the long-run effects of women's economic rights. We, therefore, turn to an analysis of the short run effects of women's economic rights in the generation of married women contemporaneous with the enactment of these laws, and their children. Several factors might contribute to the long-run effects: (1) Economic rights might have an effect on human capital and labor market outcomes of the married women in the short run. This might lead to differing investments into the children and be one reason for the long-run effects; (2) Economic rights might create an incentive to participate in the economic sphere and might reduce women's fertility. According to the quantity-quality trade-off theory, a decrease in fertility would lead to more investments per child - also fertility could be a driver behind long-run effects; (3) If empowered mothers are more likely to put their children into school - because of preference or a lower incentive of child labor - this could also be a reason behind long-run effects. (4) If economic rights affect marriage decisions or the likelihood of getting divorced due to an improved female bargaining position that may shape outcomes of following generations; (5) Lastly, economic rights shift resources to women, who usually display different spending patterns: They are more likely to spend resources on their children. This could be displayed in better child health, which could also be a mechanism behind long-run effects of women's economic rights. These channels are not mutually exclusive, long-run effects may be the result of a mix of the above. In the following, we show evidence on all of the aforementioned potential mechanisms.

In order to explore the mechanisms behind our results, we exploit the geographic and temporal variation in the enactment of the economic rights for women - i.e. property acts and earning acts -, and estimate the following equation:

$$y_{ist} = \alpha + \beta_1 \text{property-act}_{st} + \beta_2 \text{earning-act}_{st} + X'_{ist} \gamma + \rho_s + \theta_t + \epsilon_{ist} \quad (3)$$

where i refers to an individual living in state s in census year t . As our outcome variables y_{ist} we employ several dummy variables for women’s labor market outcomes, their relative standing within households, fertility, and literacy. We use decennial US Census data for the years 1860, 1870, 1880, 1900 and 1910 (Ruggles et al., 2020) to investigate responses due to the introduction of economic rights for women ¹⁰.

We now restrict our sample the adult generation contemporaneous to the reforms: married women, aged 20-50. Up until 1880, marital status is not featured in the Census Data. Therefore, we use the variable “SPOLOC” as a proxy: This variable indicates if a woman’s husband is present in the household. Also after 1880, we keep the same classification to define our sample. Descriptive statistics for the sample can be seen in table C3.

For the short run effects, we focus on adult married women and estimate the average effect of residing in a state with a property/earnings law in effect on their outcomes. This captures the immediate impact of the laws on outcomes of adult married woman at the time of the law’s implementation. The coefficients β_1 and β_2 represent the average treatment effect of the acts on the dependent variable separately. This approach differs from the identification strategy used to estimate long-run effects, described in section 4: In the identification strategy for the long run effects, we examine individuals who were exposed to property acts during childhood and assess their outcomes in adulthood. This approach isolates the long-run impact of early-life exposure to legal reforms.

Equation 3 includes the vector X_{ist} , which accounts for a range of individual characteristics such as a categorical variable for age (10-year brackets), a dummy indicating whether there are children younger than 5 years old in the household, a dummy for literacy, a dummy for being born in a foreign country, a dummy for living in an urban region, a dummy for living on a farm, and a dummy for being hispanic. To conclude, equation 3 also includes state and year fixed effects. The state fixed effects ρ_s address unobserved and time-invariant area-specific characteristics. The fixed temporal effects, captured by θ_t , account for aggregate level shocks potentially impacting women’s outcomes.

7.1 Married women’s human capital and labor market outcomes

In a first step, we employ several measures for married women’s labor market outcomes and their human capital: Literacy is coded as a dummy that indicates an individual’s ability to read or write. A dummy for labor force participation indicates whether a person is in the labor force, i.e. “gainfully” employed. Furthermore, we investigate whether the legal changes enhanced married women’s occupational standing. For the Census years 1860-1910, two different composite measures of occupational standing are provided: the Nam-Powers-Boyd Occupational Status Score and the Duncan Socioeconomic Index. The Nam-Powers-Boyd Occupational Status Score is based on earnings and education, while the Duncan Socioeconomic Index combines income, education and prestige of occupations into a ranking of occupational standing.

¹⁰Most of the 1890 Census were lost in a fire and thus, data on 1890 is not available.

Additionally, we use three one-dimensional measures of occupational standing: the occupational income score, Siegel's prestige score and the occupational education score ([Minnesota Population Center, 2020b,a](#); [Hauser and Warren, 1997](#))¹¹. A summary on the different social standing measures we employ is provided in table B4.

The results from estimating equation 3 are displayed in table C4. The estimated coefficients show that property acts have no effect on women's employment. This finding is supported by [Roberts \(2014\)](#) and [Hazan et al. \(2021\)](#). However, women's literacy and occupational standing improves with the introduction of property acts. Regardless of the indicator we use, property rights are linked to a higher occupational standing amongst employed married women. Although not statistically significant across all different measures for women's occupational standing, a consistently positive coefficient prevails. The coefficients corresponding to the earnings act are not statistically significant in any of the specifications.

Additionally, our findings suggest that employed married women may have a higher occupation standing relative to their husbands - the corresponding findings can be seen in table C5. Again, we use several indicators to measure men's and women's occupational standing within couples. As dependent variables, we employ a dummy variable indicating whether the woman has an equally high or even higher occupational standing compared to her husband. The probability for women to occupational standing as least as high as their husbands seems to increase. We do not find anything similar for earnings acts. The effects are not statistically significant across all outcomes - however, given the small sample of households where we observe occupational standing of husband and wife, that is not surprising.

7.2 Marriage and divorce

We evaluate whether women's economic rights affect marriage decisions and number of divorces in the short run. This may result from an improved female bargaining position due to economic empowerment. Corresponding results can be seen in table C6. Due to data limitations, we take two approaches to examine marriage decisions. In column (1) we use the sample of married women and test whether the age composition changes following the introduction of earnings or property acts, using age as the dependent variable. In column (2) we expand our sample to all white women aged 20-50 white and do not restrict to married women. In this sample we test whether there is an effect of property or earnings act on the probability to be married.

For divorce, we use county level data on the number of divorces we obtain from IPUMS NHIS. The divorce data is derived from reports from the US Bureau of Labor and available annually from 1867-1886. We use the number of divorces in column (3) and the log of the same variable (column (4)) as outcomes and test whether there are any effects tied to the introduction of earnings or property rights.

¹¹We use composite and one-dimensional measures of occupational standing because there is a debate which of them are more reliable. While there is literature arguing that composite measures of social standing are obsolete, others point out that they might paint a more comprehensive picture ([Boyd, 2008](#); [Hauser and Warren, 1997](#)).

The available data on marriage and divorce are limited, making it difficult to fully assess the impact of property and earnings acts. However, the evidence we do have suggests that the reforms did not affect divorce rates or women’s marriage decisions. This is supported also by [Hazan et al. \(2021\)](#).

7.3 Fertility

In a next step, we evaluate the effects of earnings and property acts on fertility in the short run - results can be seen in table [C7](#). The results indicate a reduction in both the number of children and the number of children aged below 5 years. The number of children only includes own children living in the same household as the mother at the time of the census. The age at the birth of the first child ¹² increases as a consequence of property rights, which is partially reversed by earnings acts. Negative fertility effects of earnings and property acts have already been documented by [Hazan et al. \(2021\)](#).

7.4 Children’s schooling

Next, we explore the effect of property and earnings acts on children’s schooling in the short run. Children’s schooling is captured by a binary variable - it takes on the value one if a child is currently enrolled in school. Thus, we analyse whether married women’s improved legal standing spurs investment into children’s human capital. We use a sample of white children aged 6-11.

Our findings in table [C8](#) suggest that granting married women economic rights positively impacts children’s schooling. This positive effect does not differ substantially by gender ¹³. Once again, property acts trigger responses in children’s schooling, while the effect of the earning act is not statistically significant or of substantial size.

7.5 Child mortality

Using new digitized data, we explore the impact of women’s economic rights on children’s health in the short run. We digitize information on child mortality from the decennial US Vital Statistics for the time period 1860-1900 ¹⁴. Just as the individual-level Census, the US Vital Statistics are available for 1860, 1870, 1880 and 1900, but the information provided is aggregated to the state-level.

As child mortality data is only available on the state level, we adapt equation [3](#):

$$y_{st} = \alpha + \beta_1 \text{property-act}_{st} + \beta_2 \text{earning-act}_{st} + X'_{st} \gamma + \rho_s + \theta_t + \epsilon_{st} \quad (4)$$

We still include separate dummies indicating whether women in a state s at time t had property or earning rights. The vector X_{st} includes the following time-varying state-level controls: the share of people living in an urban area, the share of black people, and the population

¹²Again, only children living in the same household can be taken into account.

¹³[Geddes et al. \(2012\)](#) document that among adolescents (age 15-19) girls’ school attendance rates relative to boys’ increases following earnings and property acts.

¹⁴We accessed US Vital Statistics for 1900 via the Centers for Disease Control Prevention and for the years before via the US Census Bureau.

share living on a farm. These control variables are constructed from the decennial (individual-level) US Census. We include state ρ and year θ fixed effects.

As outcome variables y_{st} we use two measures of child mortality: the number of child deaths (1) relative to total deaths and (2) relative to surviving children of the same age group. We analyse child mortality for children aged < 1 year and children aged < 5 years. We create these measures for child mortality for different genders and age groups. Further details on the variables we use in the state level analysis can be found in table B3.

Table C9 shows the results from estimating equation 4. The findings suggest that female economic empowerment decreases child mortality across different measures and age groups. The effects are linked to property acts rather than to earnings acts.

To sum up, in the short run, women's economic empowerment leads to higher occupational standing, higher literacy, and we find some suggestive evidence for a higher relative position within the household for women. Children's outcomes show an increase in schooling and a decrease in child mortality. All these effects are driven by property acts rather than by earning acts. The low employment rates of white married women at these times could be one explanation for this pattern. We do not find evidence that women's economic rights affect married women's labor force participation, marriage decisions, or divorces in the short run. Effects of women's economic empowerment on marriage decisions and female labor force participation only unfold in the long run.

8 Conclusion

This paper studies the long-run effects of granting economic rights to women through the introduction of married women's property and earnings acts in the nineteenth-century United States. These reforms, which allowed married women to own property and control their earnings, marked a turning point in women's economic independence. We exploit variation in exposure to these rights during childhood to assess their long-term impact on fertility, marriage, and labor market outcomes.

Our results show that exposure to women's economic rights had persistent intergenerational effects. Individuals—particularly women—who were children at the time of the reforms were less likely to marry and more likely to participate in the labor market as adults. The effects are gender-specific: we find strong and lasting impacts for women but none for men. These findings suggest that legal changes that expanded women's economic autonomy shaped not only the opportunities of married women directly affected by the reforms but also the life choices and economic behavior of the next generation.

Exploring potential mechanisms, we find that the reforms improved women's occupational standing and human capital, reduced fertility, and increased children's schooling. Using newly digitized Vital Statistics data, we further document a decline in child mortality following the enactment of property and earnings acts. These results indicate that the economic empowerment of women within households generated broader welfare improvements that extended beyond the immediate generation. In the short run, we find limited effects on labor force participation or marriage among adults living during the reforms, suggesting that many

of the consequences of women's economic rights emerged only gradually, through changes in investments, norms, and opportunities that affected children growing up under the new legal regime.

Our findings contribute to a growing literature demonstrating that legal and institutional reforms can have long-lasting effects on economic and social outcomes. In particular, they show that policies expanding women's control over economic resources can reshape family structures, human capital accumulation, and gender roles across generations. The evidence from the nineteenth-century United States offers valuable lessons for contemporary contexts where women continue to lack fundamental economic rights. Ensuring women's legal and economic autonomy not only promotes gender equality today but can also create lasting improvements in welfare and mobility for generations to come.

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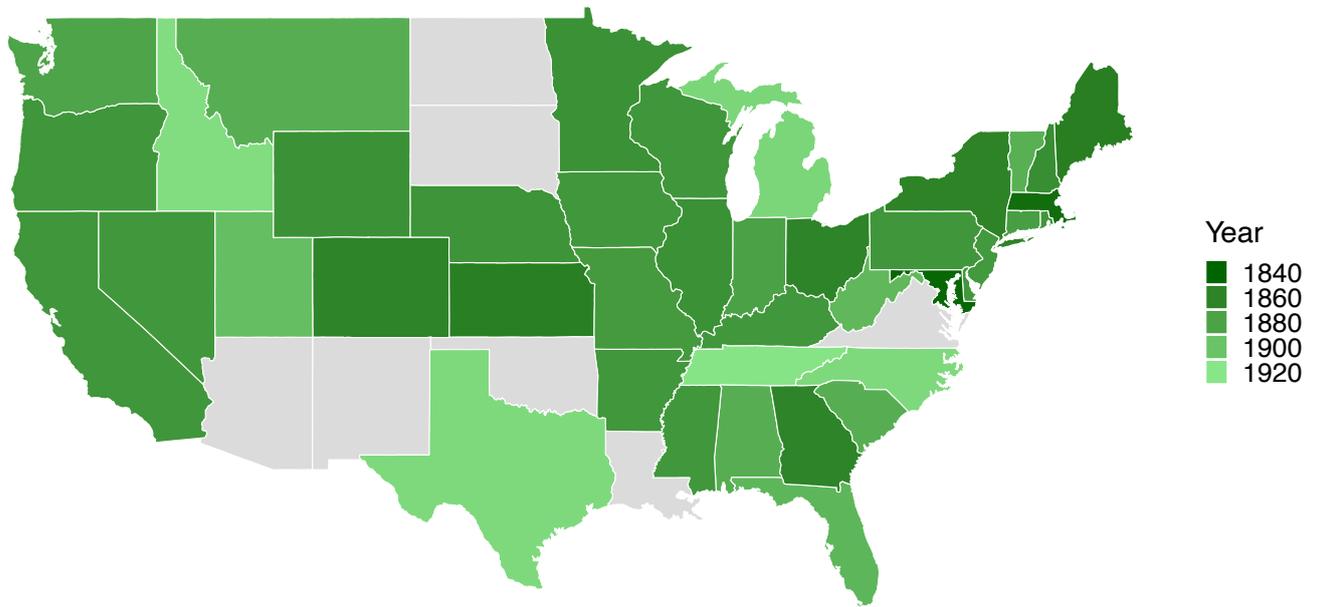
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APPENDIX

Figure A2: Timing of Earning Acts



Notes: Source: [Geddes and Tennyson \(2013\)](#). Grey color indicates that a state did not pass an earning act until 1920.

Table A1: Timing of Earnings and Property Acts

State	Property Act	Earnings Act
Alabama		1887
Arizona	1871	1973
Arkansas	1873	1873
California	1872	1872
Colorado	1861	1861
Connecticut	1877	1877
District of Columbia		
Delaware	1873	1873
Florida	1943	1892
Georgia	1873	1861
Idaho	1903	1915
Illinois	1861	1869
Indiana	1879	1879
Iowa	1873	1873
Kansas	1858	1858
Kentucky	1894	1873
Louisiana	1916	1928
Maine	1855	1857
Maryland	1860	1842
Massachusetts	1855	1846
Michigan	1855	1911
Minnesota	1869	1869
Mississippi	1880	1873
Missouri	1875	1875
Montana	1887	1887
Nebraska	1871	1871
Nevada	1873	1873
New Hampshire	1860	1867
New Jersey	1852	1874
New Mexico	1884	
New York	1848	1860
North Carolina	1868	1913
North Dakota	1877	
Ohio	1861	1861
Oklahoma	1883	
Oregon	1878	1872
Pennsylvania	1848	1872
Rhode Island	1872	1872
South Carolina	1868	1887
South Dakota	1877	
Tennessee	1919	1919
Texas	1913	1913
Utah	1872	1897
Vermont	1881	1888
Virginia	1877	1888
Washington	1881	1881
West Virginia	1868	1893
Wisconsin	1850	1872
Wyoming	1869	1869

Notes Source: [Geddes and Tennyson \(2013\)](#). Acts passed after 1920 are not complete.

Figure A2, A1 and table A1 show the timing of earnings and property acts. There is a discussion in the literature as to the reasons for passing these earning and property acts - there are several potential explanations. The first wave of property acts - the so-called debt statutes - seems to be a consequence of the economic crisis in 1839 and the following economic depression. It became of public interest to shield married women's property from their husbands' creditors to prevent the bankruptcy of households and provide a buffer to husbands' financial activities. [Chused \(1985\)](#) argues that the further ex-

pansion of married women's economic rights in the second and third waves was a consequence of vague laws created in the first wave. In many cases it was not clear which pieces of property were shielded from husbands' creditors and what married women were allowed to do with their property. More and more couples petitioned in court and a universal solution had to be found. This led to the second and third wave of married women's economic rights. [Lemke \(2016\)](#) argues that inter-jurisdictional competition could be why states grant married women property rights. It became less costly for women to move to other states as the railroads expanded and they increasingly gained access to the formal labor force. A government could benefit from an increasing population, especially territorial governments had an incentive to attract people in order to be allowed as a state in the US. These phenomena provided incentives to engage in inter-jurisdictional competition and grant economic rights for women to attract them to their own legislation.

[Doepke and Tertilt \(2009\)](#) and [Fernández \(2014\)](#) provide theoretical models on why men could have had the incentive to start granting their wives economic rights. They assume that men face a trade-off between what they would want for their wives (no economic rights) and what they would want for their daughters (comprehensive economic rights). They propose different channels why men have an incentive to push women's economic rights: [Doepke and Tertilt \(2009\)](#) conclude that the more important human capital becomes in an economy, the more men want to give married women economic rights. Their assumption is that women's economic power is important for children's education and men benefit from an expansion of women's rights because it increases educational investments in children. [Fernández \(2014\)](#) argue that with increasing wealth accumulation and decreasing fertility, men are more and more willing to grant married women economic rights to benefit their daughters in the urge to strengthen their position relative to their husbands'.

[Geddes and Lueck \(2002\)](#) suggest that women's economic rights are positively linked to household wealth, urbanization, and increases in female human capital. At some point it became too costly to exclude women from the economy and the potential returns from women's market work also benefited men. [Geddes and Tennyson \(2013\)](#) furthermore conclude that, on average, community property states passed married women's property acts later than other states, and that states with equity courts passed married women's property acts earlier than other states. In contrast to [Lemke \(2016\)](#), they do not identify any differences in timing between territories and other states. They conclude that women's economic rights might have spurred compulsory schooling laws and the formation of suffrage groups, and not vice versa.

Historical literature furthermore suggests, that there was some lobbying by women's rights groups, but it was of negligible size. According to C. Degler "(t)he women's movement throughout the 19th century left untouched the great mass of women, married and unmarried. At no time were more than a few thousand women actively involved in the feminist or suffrage causes" ([Chused, 1982](#)). Expanding married women's property rights was not aimed to foster women's emancipation, or their participation in the economic and political sphere, but rather a reflection of women's responsibility in the domestic sphere ([Chused, 1982](#)).

A.1 Consequences of Property and Earning Acts

Several papers have looked into effects of the expansion of economic rights for married women at the end of the 19th century in the US. [Khan \(1996\)](#) examines whether several legal changes strengthening married women's economic rights spurred female commercial activity in terms of patents. She finds an increase in female patenting activity, which is mainly driven by metropolitan areas. [Koudijs and Salisbury \(2020\)](#) examine whether this same first wave of property acts had an impact on household investment and bankruptcy. They find heterogeneous effects – if mostly the husband contributed to the household wealth, the acts led to an increase in investment and vice versa. [Hazan et al. \(2019\)](#) show that married women's economic rights changed portfolio allocations, increased bank loans and deposits, and led to a shift to the non-agricultural sector.

[Geddes et al. \(2012\)](#) investigate potential impacts of married women's property rights on children's school attendance. Their hypothesis is that girls now have an increased incentive to invest in their human capital. They analyze changes in girls' school attendance relative to boys' attendance and find an increase in girls' school attendance. [Jung \(2024\)](#) shows that increases in schooling are more pronounced in households with literate mothers. [Hazan et al. \(2021\)](#) exploit contiguous county-pairs in bordering states with different treatment status in their identification strategy as well as the timing of marriage in a second identification strategy. They show that women's empowerment led to a decrease in fertility. By negating alternative mechanisms, they conclude that women's increased bargaining power explains

their findings. [Roberts \(2014\)](#) examines labor supply responses of married women in response to acts granting married women economic rights. He concludes that in the short run the effects on women's labor force participation are negligible, which is also supported by [Hazan et al. \(2021\)](#).

[Alshaikhmubarak et al. \(2019\)](#) investigate whether single women also responded to the introduction of economic rights for married women. Before the legal changes, women were not entitled to custody over their children. With the passing of the property acts for married women, it might become more convenient to have children as a married woman. The authors indeed find that single women are less likely to have children in response to married women's property acts.

B Description of Variables

Table B1: Description of Main Variables - Long-Run Analyses

Outcome Variables	
Married	Dummy variable that takes on the value 1 if the spouse is present in the household
Age Gap Spouse	The age gap between an individual and their husband or wife
Any Child	Dummy variable that takes on the value 1 if child under the age of 5
Number of Children	Number of children under the age of 5
Labor Force Participation	Dummy variable that takes on the value 1 if person is gainfully employed
Individual Level Controls	
Urban	Dummy that takes on the value 1 if a person lives in an urban region, 0 otherwise
Farm	Dummy that takes on the value 1 if a person lives on a farm, 0 otherwise
Hispanic	Dummy indicating whether a person is Hispanic, 0 otherwise
Literate	Dummy indicating whether a person is literate, 0 otherwise
State Level Controls	
Literacy rate	Share literate
Urbanization rate	Share living in urban region
Agricultural population	Share living on a farm

Table B2: Description of Main Variables - Individual Level Analyses

Outcome Variables	
Literacy	dummy that takes on the value one, if person is able to read/write; 0 otherwise
School attendance	dummy that takes on the value one if a person is in school, 0 otherwise
Employment	dummy that takes on the value one if a person has a gainful employment, 0 otherwise
Nr. Children	Number of own children living in the same household
Nr. Children u. 5	Number of own children younger than 5 years living in the same household
Age 1st child	Constructed by subtracting the age of the eldest own child living in the same household from the mother's age
Occupational Standing	Scores assigned to occupations based on one of 6 indices: Duncan Socioeconomic Index, Siegel's prestige score, Occupational Income Scores, Occupational Education Score, Occupational Earning Score or Nam-Powers-Boyd score
Relative standing	dummy for at least as good as husband, 0 otherwise (1) dummy = 1 if the woman's position is at least as good as her husband's (measured in index 1 = OCCSCORE = Occupational Income Score), 0 otherwise (2) dummy = 1 if the woman's position is at least as good as her husband's (measured in index 2 = SEI = Duncan Socioeconomic Index), 0 otherwise (3) dummy = 1 if the woman's position is at least as good as her husband's (measured in index 3 = PRESGL = Occupational Prestige Score, Siegel), 0 otherwise (4) dummy = 1 if the woman's position is at least as good as her husband's (measured in index 4 = ERSCORE = occupational earnings score), 0 otherwise (5) dummy = 1 if the woman's position is at least as good as her husband's (measured in index 5 = NPBOSS50 = Nam-Powers-Boyd occupational status score), 0 otherwise (6) dummy = 1 if the woman's position is at least as good as her husband's (measured in index 6 = EDSCORE = occupational education score), 0 otherwise
Explanatory Variables	
Earning Act	Dummy variable that takes on the value 1 if married women have the right to own/control earnings, 0 otherwise
Property Act	Dummy variable that takes on the value 1 if married women have the right to own/control property, 0 otherwise
Individual Level Controls	
Age	age categories (15-24, 25-34, 35-44, 45+)
Children	Dummy that takes on the value 1 if at least one child aged below 5 years, 0 otherwise
Foreign	Dummy indicating whether a person was born outside the US, 0 otherwise
Urban	Dummy that takes on the value 1 if a person lives in an urban region, 0 otherwise
Farm	Dummy that takes on the value 1 if a person lives on a farm, 0 otherwise
Hispanic	Dummy indicating whether a person is Hispanic, 0 otherwise

Table B3: Description of Main Variables - State Level Analyses

Outcome Variables	
Child Deaths/Total Deaths:	<ul style="list-style-type: none"> • nr. of children died below age of 1/nr. total deaths • nr. of children died below age of 5/nr. total deaths • nr. of female children died below age of 1/nr. total female deaths • nr. of female children died below age of 5/nr. total female deaths • nr. of male children died below age of 1/nr. total male deaths • nr. of male children died below age of 5/nr. total male deaths
Child Deaths/Child Survival:	<ul style="list-style-type: none"> • nr. of children died below age of 1/nr. alive children below age of 1 • nr. of children died below age of 5/nr. alive children below age of 5 • nr. of female children died below age of 1/nr. alive female children below age of 1 • nr. of female children died below age of 5/nr. alive female children below age of 5 • nr. of male children died below age of 1/nr. alive male children below age of 1 • nr. of male children died below age of 5/nr. alive male children below age of 5
Explanatory Variables	
Earning Act	Dummy variable that takes on the value 1 if married women have the right to own/control earnings, 0 otherwise
Property Act	Dummy variable that takes on the value 1 if married women have the right to own/control property, 0 otherwise
State Level Controls	
Urbanization	share of people living in urban area
Racial composition	share of black individuals
Agriculture	Population share living on farm

Table B4: Social Standing - Indicators

Index	Description
Duncan Socioeconomic Index	Components of the index: Income, Education, Prestige; The index uses income and education data from the 1950 census data and the occupational prestige ratings of the 1947 National Opinion Research Center survey/the North-Hatt study
Occupational income score	Occupational income scores based on economic rewards of occupations inferred from people working at them in 1950
Occupational prestige score, Siegel	It is a constructed variable that assigns a Siegel prestige score to each occupation
Nam-Powers-Boyd occupational status score	Components of the index: median earnings and median educational attainment
Occupational education score	Scores indicate the percentage of persons in each occupation with one or more years of college education
Occupational earnings score	Scores indicate the median earned income of persons in each occupation.

C Additional Results

C.1 Descriptive figures

Figure C1: Literacy rate by gender and race

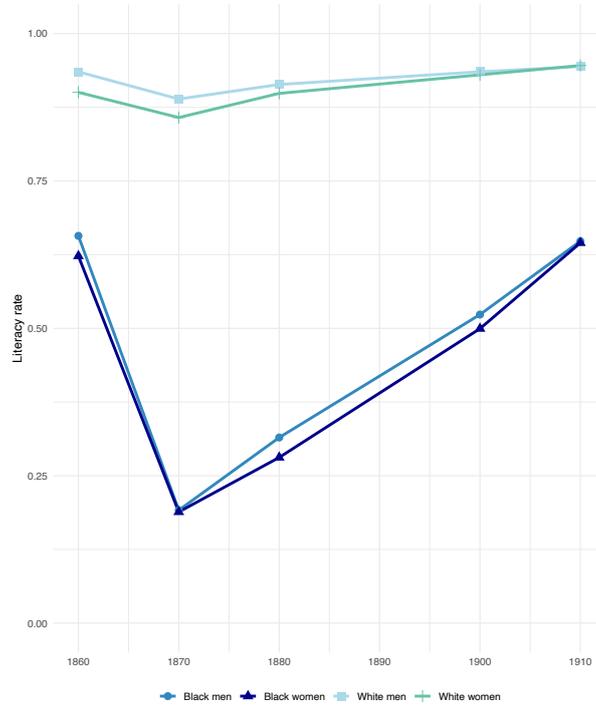


Figure C2: Coverage of cohorts by state

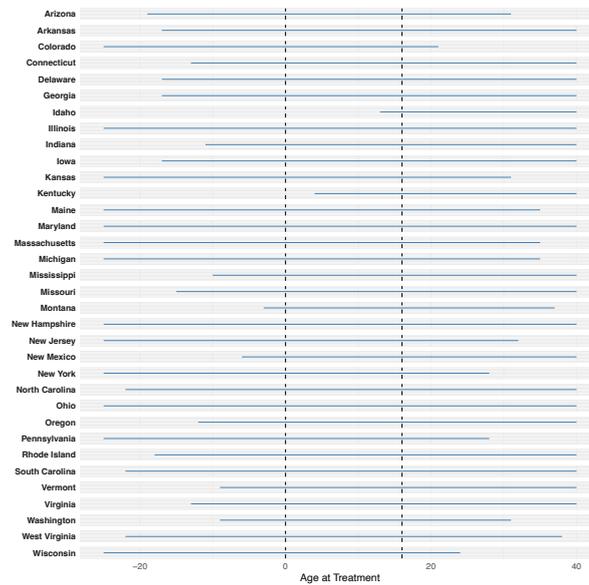
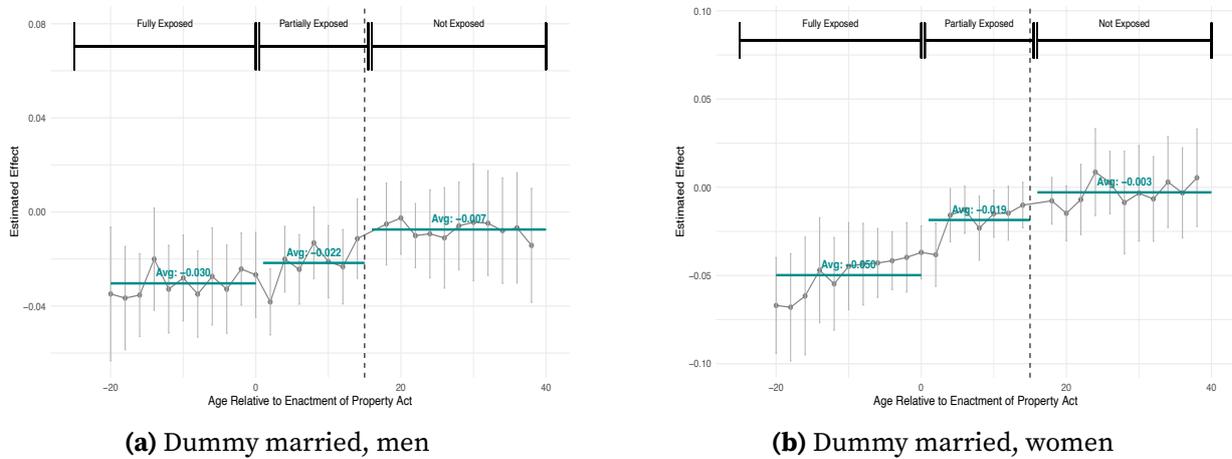


Figure C3: Timing of other laws relative to women's property acts



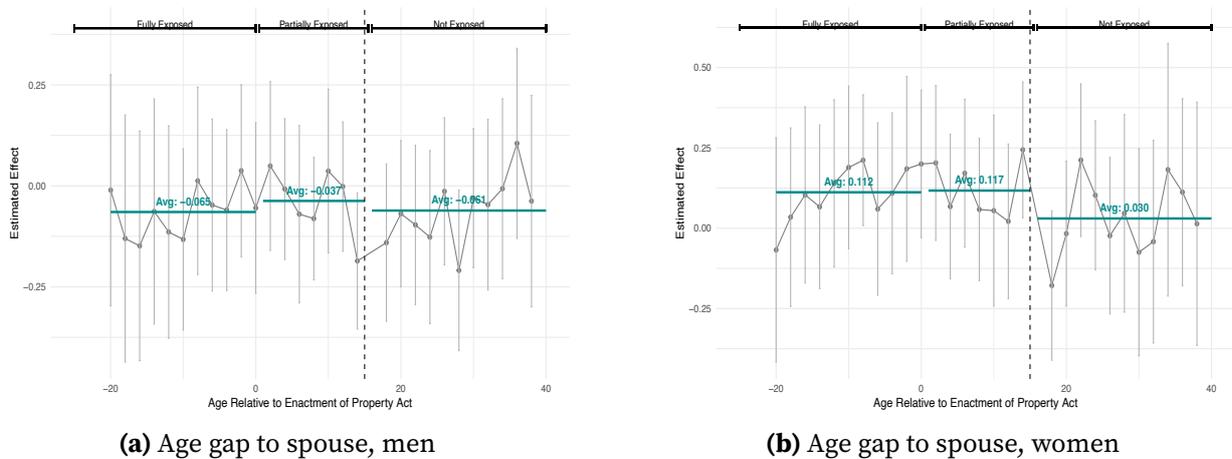
C.2 Effects on fertility and marriage by gender

Figure C4: The effect of exposure to property acts during childhood on probability to be married



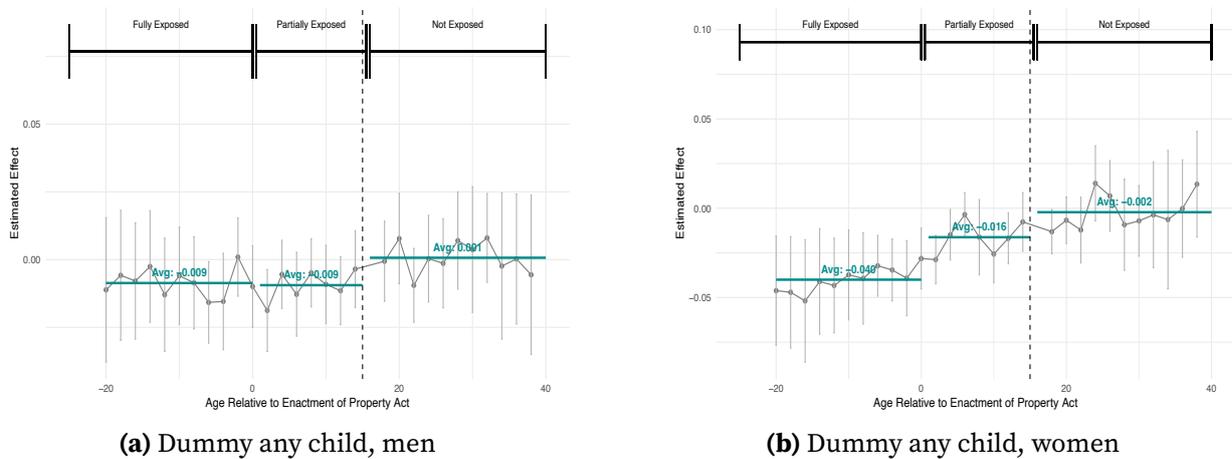
Notes: Figure C4 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married. We show results for men in subfigure C4a, and women in subfigure C4b. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C5: The effect of exposure to property acts during childhood on age gap to spouse



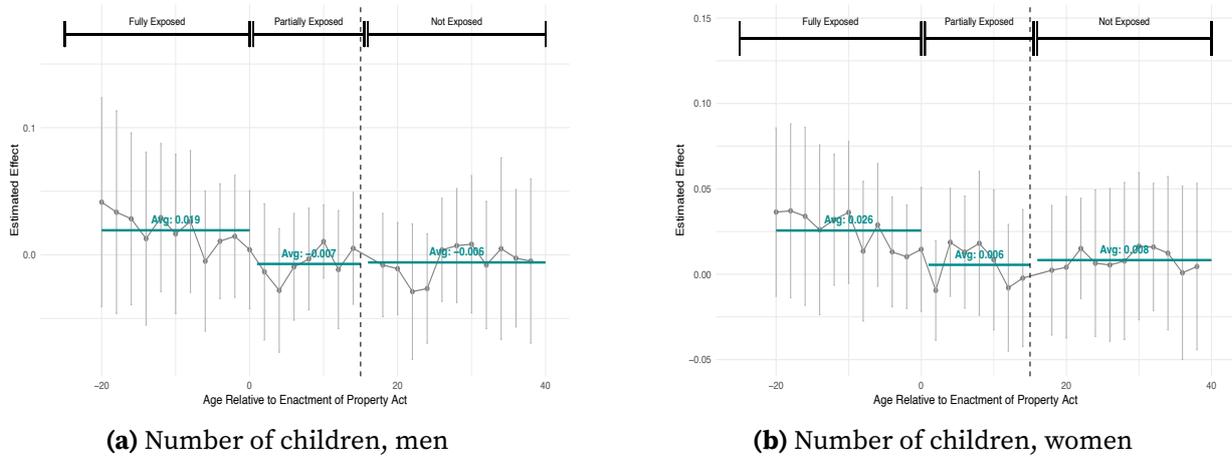
Notes: Figure C5 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is the age gap to one's spouse. Results are based on the subsample of married individuals. The dependent variable is a dummy indicating whether a person is married. We show results for men in subfigure C5a, and women in subfigure C5b. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C6: The effect of exposure to property acts during childhood on probability to have children



Notes: Figure C6 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5. We show results for men in subfigure C6a, and women in subfigure C6b. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

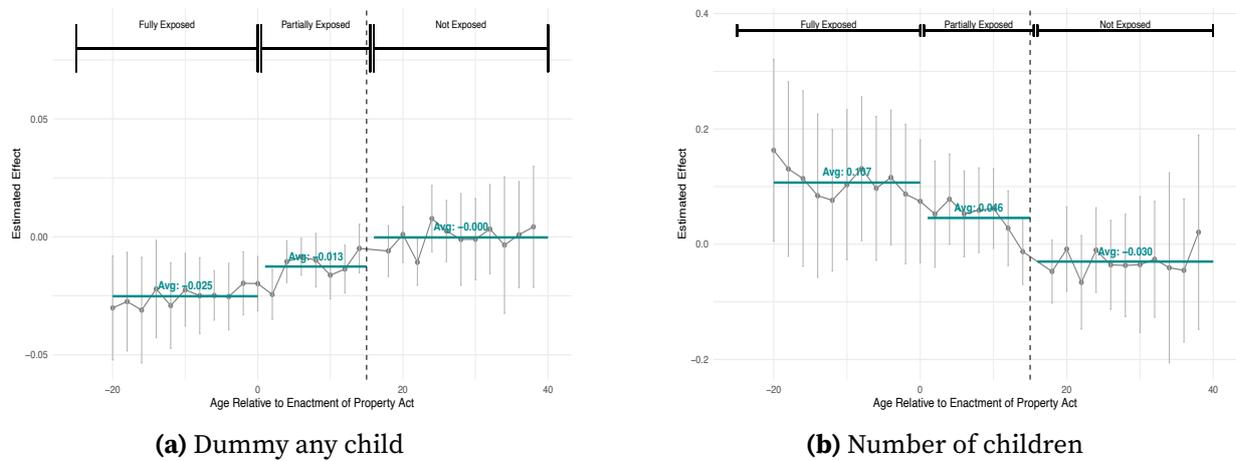
Figure C7: The effect of exposure to property acts during childhood on number of children



Notes: Figure C7 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is number of children below the age of five in subfigure 2b. Results are based on the subsample of individuals with children. We show results for men in subfigure ??, and women in subfigure C7b. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

C.3 Alternative outcomes

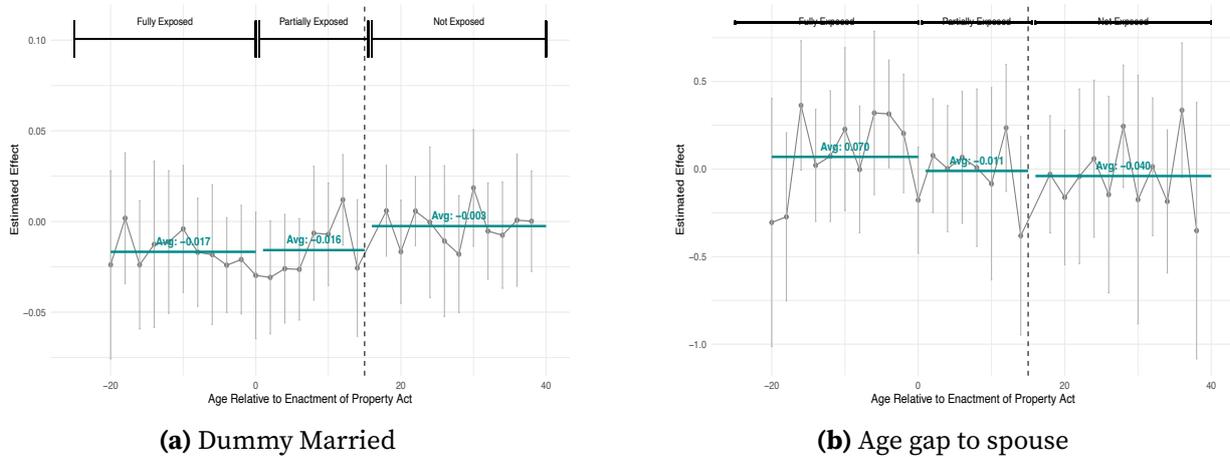
Figure C8: The effect of exposure to property acts during childhood on fertility - Any age



Notes: Figure C8 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children in figure C8a, and the number of children in subfigure C8b. In contrast to figure 2, we also count children above the age of 5. Results displayed in subfigure C8b are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

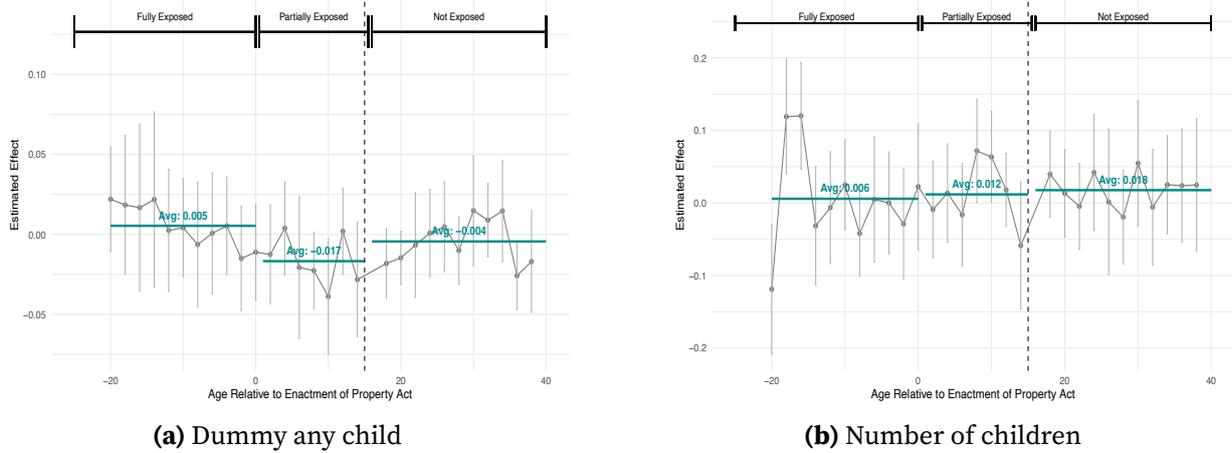
C.4 Black population

Figure C9: Black: The effect of exposure to property acts during childhood on marriage decisions



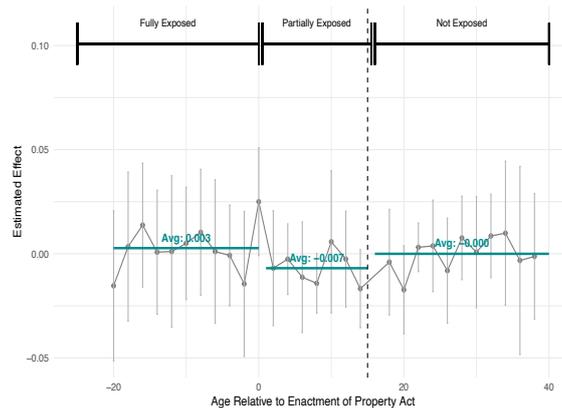
Notes: Figure C9 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes black native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married in figure C9a, and the age gap to one's spouse in subfigure C9b. Results displayed in subfigure C9b are based on the subsample of married individuals. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C10: Black: The effect of exposure to property acts during childhood on fertility



Notes: Figure C10 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes black native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5 in figure C10a, and the number of children below the age of five in subfigure C10b. Results displayed in subfigure C10b are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C11: Black: The effect of exposure to property acts during childhood on labor force participation

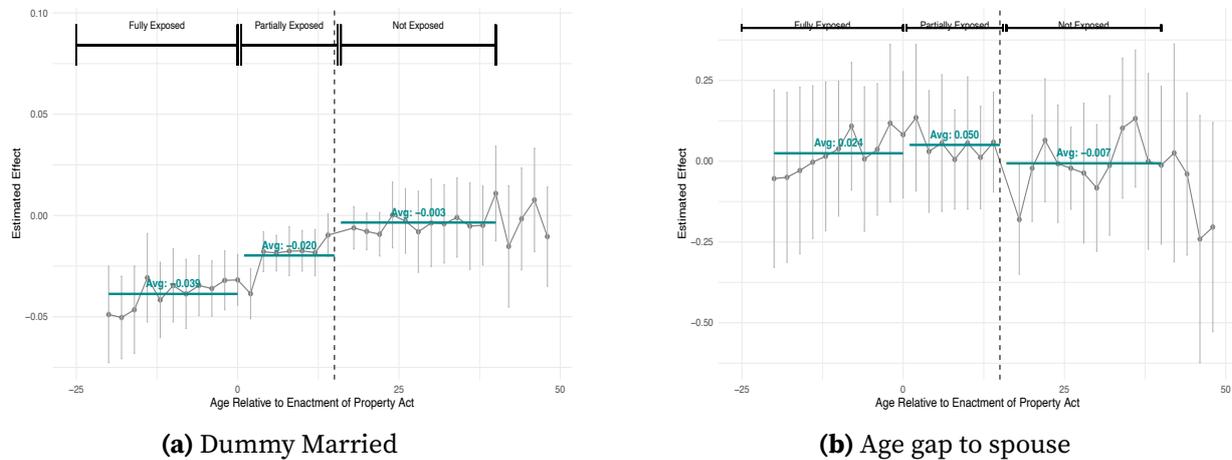


Notes: Figure C11 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes black native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy for labor force participation. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

C.5 Robustness

C.5.1 Longer pre-period

Figure C12: The effect of exposure to property acts during childhood on marriage decisions—longer pre-period

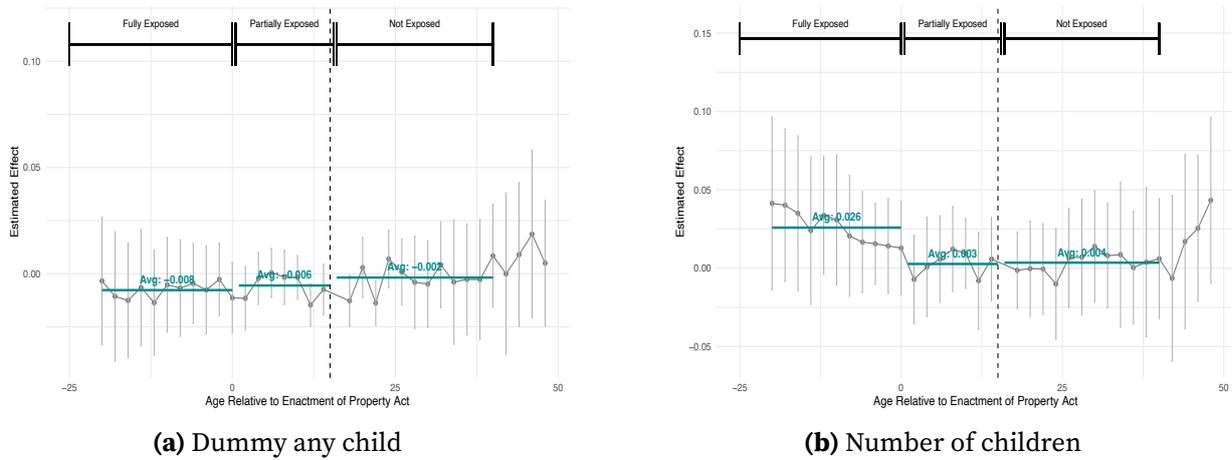


Notes: Figure C12 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married in subfigure (a), and the age gap to one's spouse in subfigure (b). Results displayed in subfigure (b) are based on the subsample of married individuals. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

C.5.2 Endogeneity of timing

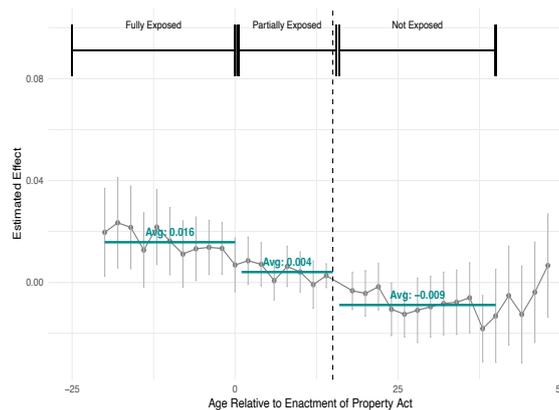
C.5.3 Effect on migration decisions

Figure C13: The effect of exposure to property acts during childhood on fertility- longer pre-period



Notes: Figure C13 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5 in figure (a), and the number of children below the age of five in subfigure (b). Results displayed in subfigure (b) are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C14: The effect of exposure to property acts during childhood on labor force participation - longer pre-period



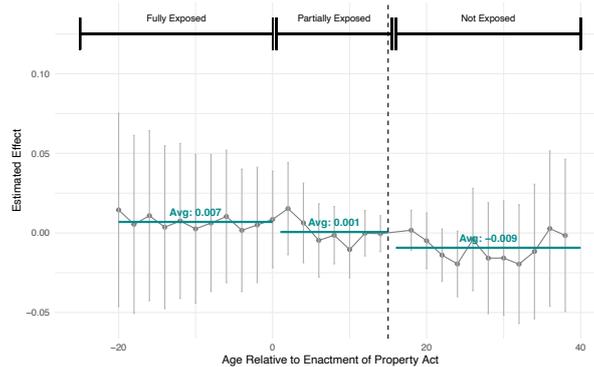
Notes: Figure C14 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy for labor force participation. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Table C1: Endogeneity of timing of property acts to state characteristics

	Timing PA
Literacy	-25.457 (51.185)
Employment	-1.599 (322.733)
Dummy Child <5	45.294 (260.909)
Unmarried	-529.595 (717.239)
Farm	14.676 (55.689)
Urban	30.131 (52.532)
Age	1.818 (2.562)
Dummy Any Child	-427.210 (513.453)
Nr Children	60.176 (104.683)
Hispanic	20.851 (45.711)
Mean DV	1881.483
Num. obs.	29

Table C1 shows coefficients from regressing the timing of property acts on pre-treatment characteristics. Standard errors are presented in parentheses. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910, collapsed to the state level.

Figure C15: The effect of exposure to property acts during childhood on migration decisions



Notes: Figure C15 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of property acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of property acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of property acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person moved to a state different from where they were born. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

C.5.4 Varying sample and specification

Table C2: Robustness

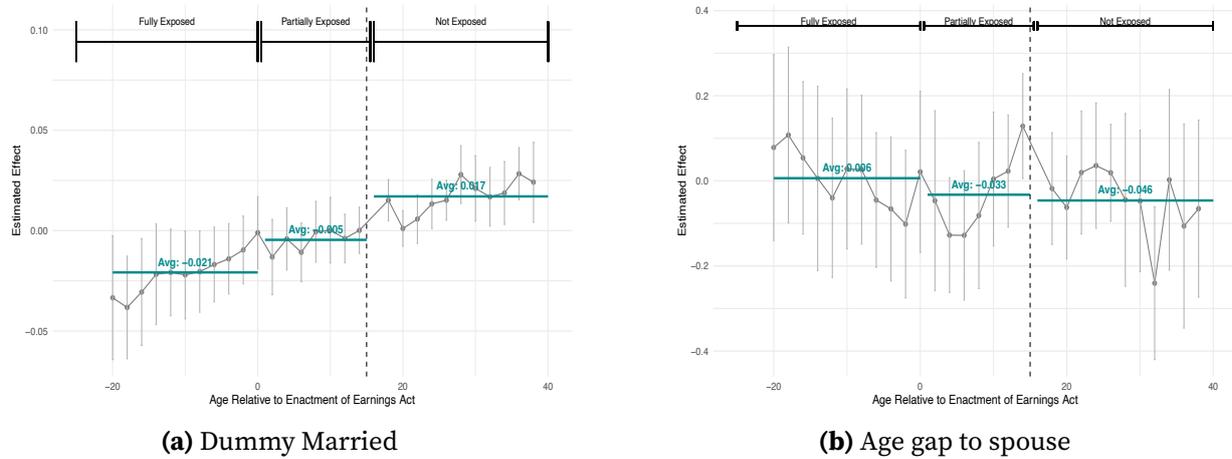
	Married			Age Gap Spouse			Has child			Nr. children			Labor Force Participation		
	fully exp.	part. exp.	not exp.	fully exp.	part. exp.	not exp.	fully exp.	part. exp.	not exp.	fully exp.	part. exp.	not exp.	fully exp.	part. exp.	not exp.
	k=-10	k=8	k=28	k=-10	k=8	k=28	k=-10	k=8	k=28	k=-10	k=8	k=28	k=-10	k=8	k=28
Main Results	-0.038 0.009	-0.017 0.006	-0.008 0.011	0.061 0.109	0.003 0.084	-0.069 0.112	-0.008 0.011	-0.002 0.006	-0.004 0.012	0.028 0.022	0.009 0.015	0.006 0.019	0.019 0.007	0.007 0.004	-0.010 0.007
<i>Panel A: Confounder Compulsory Schooling Law (CSL)</i>															
Excl. CSL exposed	-0.029 0.010	-0.015 0.006	-0.008 0.010	0.158 0.120	0.000 0.094	-0.084 0.114	0.006 0.011	-0.003 0.006	-0.002 0.012	0.034 0.026	0.012 0.014	0.007 0.019	0.020 0.008	0.008 0.004	-0.007 0.006
Control for CSL	-0.037 0.009	-0.017 0.006	-0.008 0.011	0.067 0.111	0.003 0.084	-0.069 0.112	-0.009 0.011	-0.002 0.006	-0.004 0.012	0.024 0.022	0.009 0.014	0.006 0.019	0.018 0.007	0.007 0.004	-0.010 0.007
<i>Panel B: Varying Sample</i>															
Excl. migrants	-0.043 0.012	-0.013 0.007	-0.010 0.013	0.061 0.151	0.016 0.108	-0.030 0.154	-0.009 0.013	-0.007 0.009	-0.014 0.015	0.020 0.028	-0.005 0.020	-0.006 0.020	0.015 0.007	0.007 0.005	-0.010 0.008
Excl. treated >90%	-0.035 0.009	-0.017 0.006	-0.010 0.011	0.050 0.112	0.010 0.085	-0.063 0.113	-0.004 0.011	-0.001 0.006	-0.006 0.012	0.037 0.023	0.015 0.014	0.009 0.019	0.020 0.007	0.006 0.004	-0.011 0.007
<i>Panel C: Varying Specification</i>															
No controls	-0.058 0.007	-0.022 0.006	-0.004 0.012	-0.061 0.123	-0.027 0.088	-0.047 0.112	-0.041 0.011	-0.009 0.006	0.002 0.015	0.001 0.020	0.002 0.014	0.010 0.020	0.019 0.008	0.007 0.006	-0.013 0.010
Age trend	-0.027 0.008	-0.011 0.006	-0.011 0.010	-0.012 0.121	-0.059 0.089	-0.029 0.116	-0.012 0.010	-0.007 0.006	-0.002 0.011	0.024 0.023	0.004 0.014	0.004 0.020	0.012 0.006	0.003 0.003	-0.007 0.005

Table C2 shows results from several robustness checks. We report coefficients for all our main outcomes: a dummy for being married, the age gap to the spouse, a dummy for having a child, the number of children, and labor force participation. For each outcome, we report point estimates for $k = -10$ (fully exposed), $k = 8$ (partially exposed), and $k = 28$ (not exposed). The top row reports our main results, for comparison. The following rows report coefficients estimated from several robustness checks. In panel A, we test the robustness of our findings to the exclusion of cohorts exposed to compulsory schooling laws; and to the inclusion of a dummy variable controlling for whether a compulsory schooling law is in place. In panel B, we vary the sample: We exclude people who moved away from their state of birth; and apply a stricter exclusion threshold and exclude 5 additional states where 90% or more of all individuals are treated. In panel C, we vary our specification: We report results from estimating equation 1 without the inclusion of controls; and we test for robustness to the inclusion of state-specific linear age trends. Standard errors are reported in below coefficients.

C.6 Alternative Exposure Measure

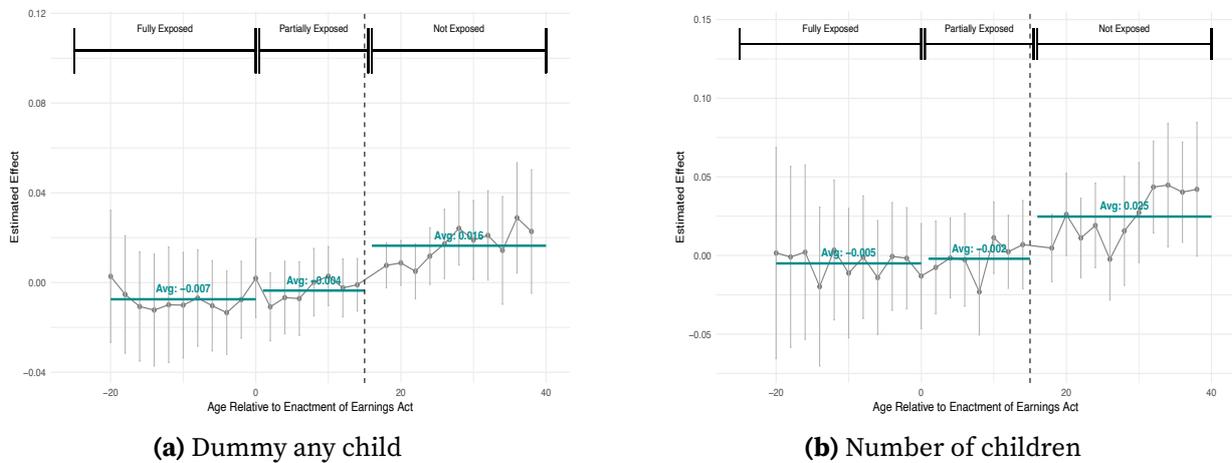
C.6.1 Exposure to Earnings Act

Figure C16: The effect of exposure to earnings acts during childhood on marriage decisions



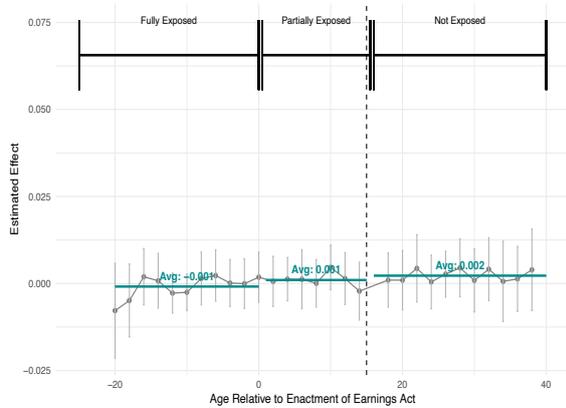
Notes: Figure C16 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of earnings acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of earnings acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of earnings acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married in figure C16a, and the age gap to one's spouse in subfigure C16b. Results displayed in subfigure C16b are based on the subsample of married individuals. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C17: The effect of earnings to earnings acts during childhood on fertility

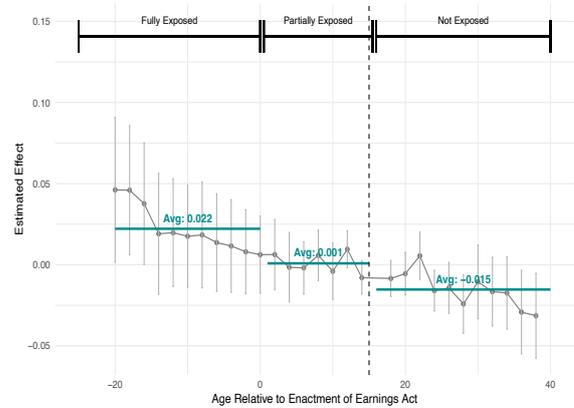


Notes: Figure 2 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of earnings acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of earnings acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of earnings acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5 in figure 2a, and the number of children below the age of five in subfigure 2b. Results displayed in subfigure 2b are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C18: The effect of exposure to earnings acts during childhood on labor force participation



(a) Men's labor force participation

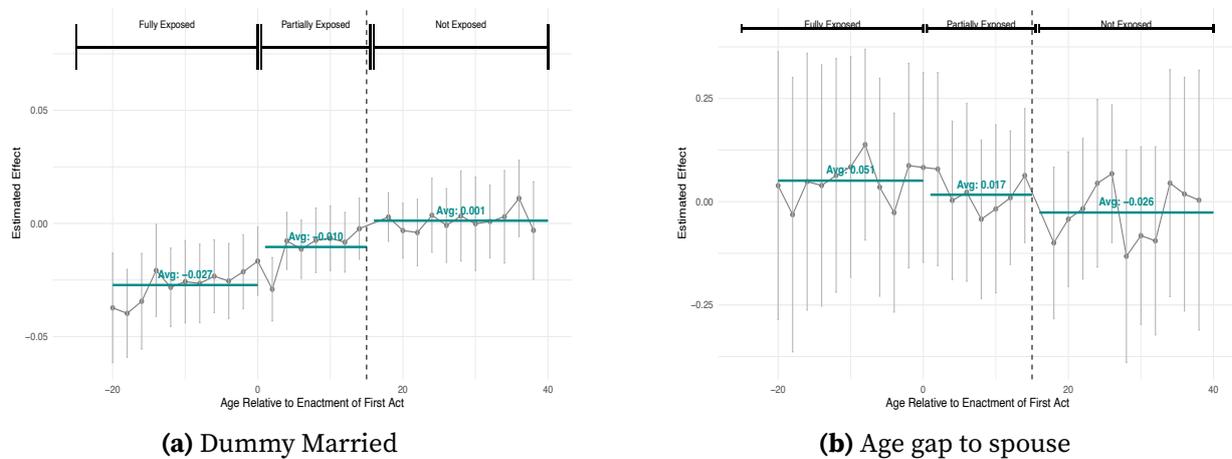


(b) Women's labor force participation

Notes: Figure C18 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of earnings acts. We label cohorts as *not exposed*, if they were older than 17 at the introduction of earnings acts in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of earnings acts. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy for labor force participation in both subfigures. Subfigure C18a shows results based on the subsample of men, subfigure C18b the subsample of women. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

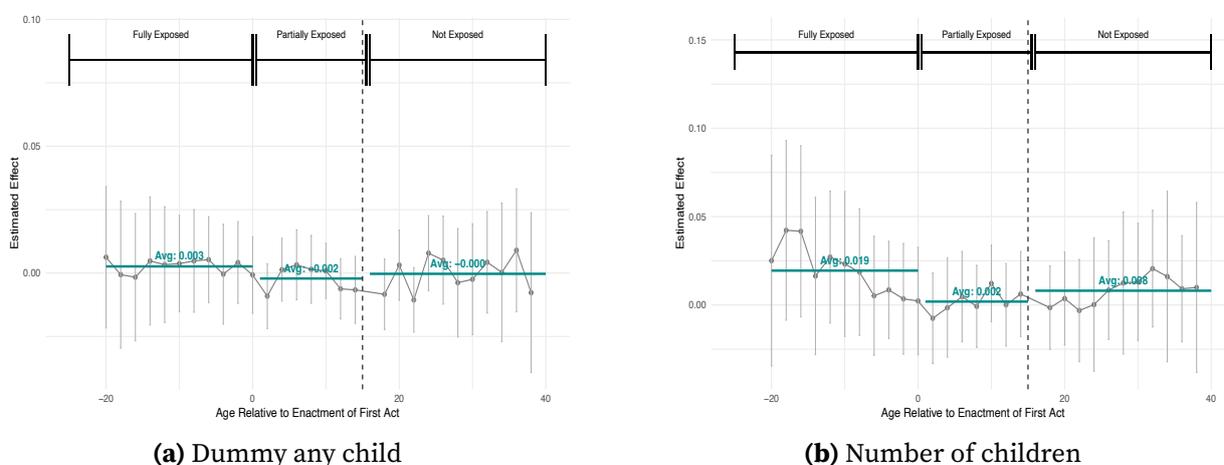
C.6.2 Exposure to Any Act

Figure C19: The effect of exposure to first act (either earnings or property act) during childhood on marriage decisions



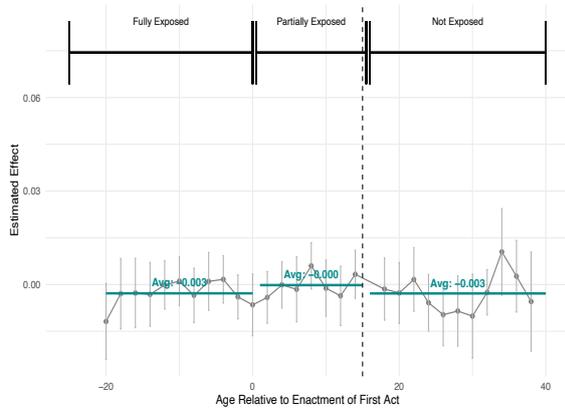
Notes: Figure C19 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of the first of either earnings or property act. We label cohorts as *not exposed*, if they were older than 17 at the introduction of the first act in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of the first act. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married in figure C19a, and the age gap to one's spouse in subfigure C19b. Results displayed in subfigure C19b are based on the subsample of married individuals. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C20: The effect of exposure to first act (either earnings or property act) during childhood on fertility

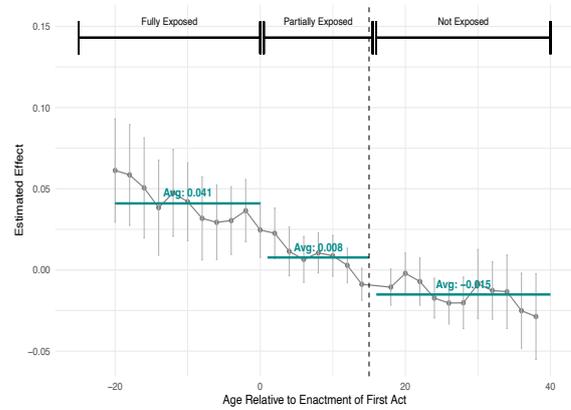


Notes: Figure C20 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of the first of either earnings or property act. We label cohorts as *not exposed*, if they were older than 17 at the introduction of the first act in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of the first act. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5 in figure C20a, and the number of children below the age of five in subfigure C20b. Results displayed in subfigure C20b are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C21: The effect of exposure to first act (either earnings or property act) during childhood on labor force participation



(a) Men's labor force participation

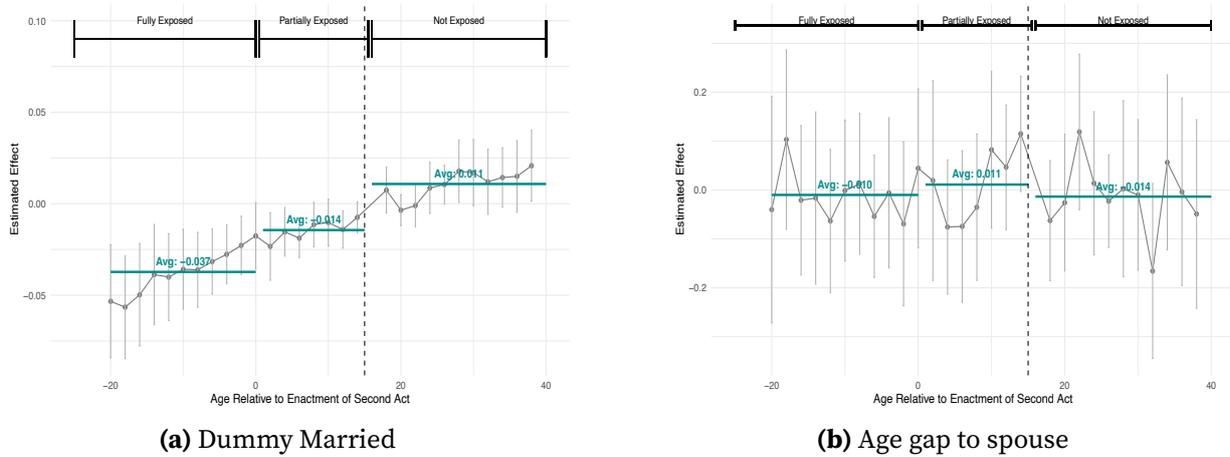


(b) Women's labor force participation

Notes: Figure C21 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of the first of either earnings or property act. We label cohorts as *not exposed*, if they were older than 17 at the introduction of the first act in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of the first act. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy for labor force participation in both subfigures. Subfigure C21a shows results based on the subsample of men, subfigure C21b the subsample of women. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

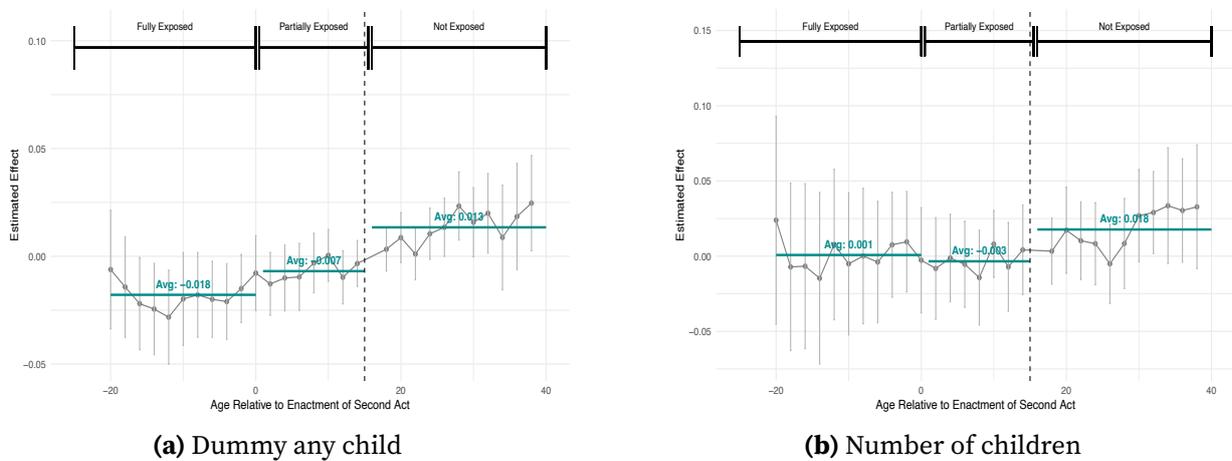
C.6.3 Exposure to Both Acts

Figure C22: The effect of exposure to second act (earnings and property act in effect) during childhood on marriage decisions



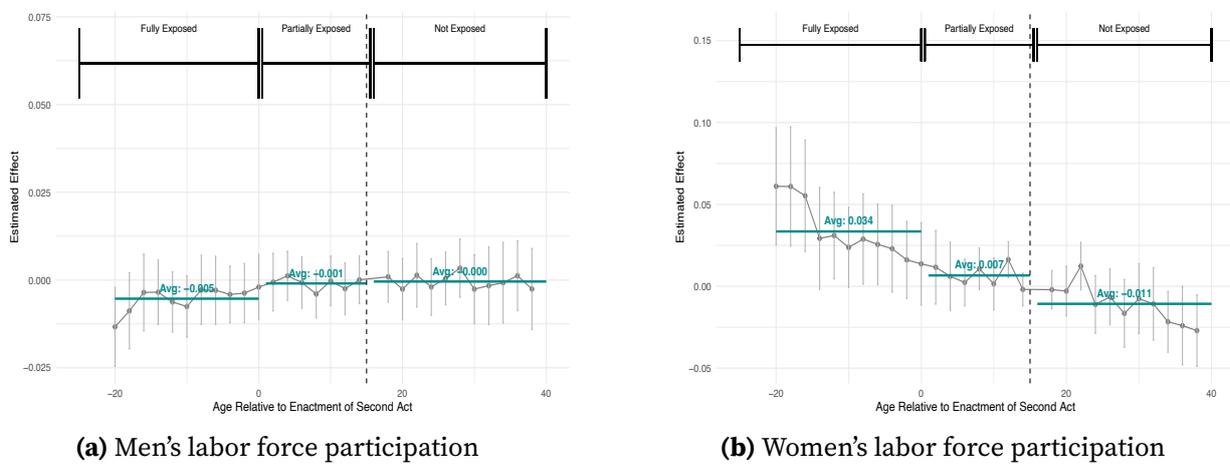
Notes: Figure C22 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment the second act. We label cohorts as *not exposed*, if they were older than 17 at the introduction of the second act in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of the second act. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person is married in figure C22a, and the age gap to one's spouse in subfigure C22b. Results displayed in subfigure C22b are based on the subsample of married individuals. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C23: The effect of exposure to second act (earnings and property act in effect) during childhood on fertility



Notes: Figure C23 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of the second act. We label cohorts as *not exposed*, if they were older than 17 at the introduction of the second act in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of the second act. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy indicating whether a person has children below the age of 5 in figure C23a, and the number of children below the age of five in subfigure C23b. Results displayed in subfigure C23b are based on the subsample of individuals with children. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

Figure C24: The effect of exposure to second act (earnings and property act in effect) during childhood on labor force participation



Notes: Figure C24 shows estimation results from the empirical approach discussed in section 4. It is based on decennial US census data for the years 1860, 1870, 1880, 1900, and 1910 and includes white native individuals aged 20-40. The vertical axis refers to the age at the enactment of the second act. We label cohorts as *not exposed*, if they were older than 17 at the introduction of the second act in their state, as *partially exposed* if they were between the age of 0 and 15, and as *fully exposed* if they were born after the enactment of the second act. Each coefficient bins two ages, the omitted group are individuals exposed at ages 16-17. The dependent variable is a dummy for labor force participation in both subfigures. Subfigure C24a shows results based on the subsample of men, subfigure C24b the subsample of women. The control variables included are specified in the discussion of equation 1. State, age, survey year, and cohort fixed effects are included in all specifications. Standard errors are clustered at the state level.

C.7 Short run

Table C3: Summary Statistics - Short-Run Sample

	Num. Obs.	Min.	Max.	Mean	SD
Age	353845	20	50	33.757	8.359
Farm	353845	0	1	0.36	0.48
Hispanic	353845	0	1	0.007	0.084
Literate	353845	0	1	0.914	0.28
Urban	353845	0	1	0.386	0.487
Working	353845	0	1	0.034	0.181
Child aged < 5	353845	0	1	0.528	0.499
Index 1	11878	4	80	15.834	10.806
Index 2	11932	4	96	24.834	19.347
Index 3	11931	12.2	81.5	29.283	11.981
Index 4	11931	0.6	100	24.807	26.072
Index 5	11931	0.7	100	31.557	25.03
Index 6	11931	0	93.9	11.446	18.616

Table C3 shows summary statistics for the sample used in the analyses of short-run implications of economic rights. Sample: white women aged 20-50 who live with their husband. Data from the US Census 1860, 1870, 1880, 1900, 1910.

Table C4: The short-run effect of economic rights on women's labor market outcomes and human capital

	Occupational Standing							
	Employment	Literacy	Index 1	Index 2	Index 3	Index 4	Index 5	Index 6
Earnings Act	0.002 (0.006)	0.010 (0.013)	-0.724 (0.767)	-0.600 (1.219)	-0.819 (0.897)	-0.951 (1.685)	-0.984 (1.613)	-1.650 (1.166)
Property Act	-0.010 (0.005)	0.033 ** (0.011)	1.500 (0.915)	3.852 ** (1.413)	2.816 ** (1.041)	2.641 (2.044)	3.380 (1.882)	3.294 ** (1.245)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean DV	0.033	0.915	15.846	24.945	29.320	24.829	31.640	11.520
Num. obs.	352915	352915	11765	11819	11818	11818	11818	11818
Dimension			Income	Inc. + Educ. + Prest.	Prestige	Earnings	Educ. + Earn.	Education

Table C4 shows estimation results for equation 3 for white married women. It is based on data from the US Census for the years 1860, 1870, 1880, 1900, 1910. The sample includes white women aged 20 to 50 where the husband is present in the household. In column 1, the dependent variable is a dummy that takes on the value one if a person is gainfully employed, zero otherwise. In column 2, the dependent variable is a dummy that takes on the value one if a person is literate (i.e. can read and write), zero otherwise. In Columns 3-8, the dependent variables are different indices for occupational standing: We use the occupational income score in column 3, the Duncan Socioeconomic Index in column 4, Siegel's occupational prestige score in column 5, the occupational earnings score in column 6, the Nam-Powers-Boyd occupational status score in column 7, and the occupational education score in column 8. The table reports specifications with individual level controls. State and year fixed effects are included in all specifications. We estimate linear (probability) models. Standard errors are clustered at the state level and reported in parenthesis. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table C5: The short-run effect of economic rights on women's relative position in marriage

	Relative Position based on ...					
	Index 1	Index 2	Index 3	Index 4	Index 5	Index 6
Earnings Act	-0.053 (0.046)	0.030 (0.073)	0.021 (0.041)	-0.029 (0.045)	-0.027 (0.050)	-0.002 (0.040)
Property Act	0.088 (0.051)	0.156 (0.082)	0.093 * (0.043)	0.061 (0.047)	0.055 (0.050)	0.112 ** (0.043)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean DV	0.340	0.643	0.472	0.330	0.399	0.513
Num. obs.	11335	11391	11381	11381	11381	11381
Dimension	Income	Inc. + Educ. + Prest.	Prestige	Earnings	Educ. + Earn.	Education

Table C5 shows estimation results for equation 3 for white married women. It is based on data from the US Census for the years 1860, 1870, 1880, 1900, 1910. The sample includes white women aged 20 to 50 where the husband is present in the household. The sample is further restricted to couples where both individuals are employed. As dependent variables, we use dummy variables that take on the value one if a woman's occupational standing is at least as high as her husband's. We measure occupational standing based on 6 different indices. The measure of the woman's relative position within the couple is based on the occupational income score in column 1, the Duncan Socioeconomic Index in column 2, Siegel's occupational prestige score in column 3, the occupational earnings score in column 4, the Nam-Powers-Boyd occupational status score in column 5, and the occupational education score in column 6. The table reports specifications with individual level controls. In addition to the controls discussed in Section 7, we control for husbands' age and literacy. State and year fixed effects are included in all specifications. We estimate linear probability models. Standard errors are clustered at the state level and reported in parenthesis. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table C6: The short-run effect of economic rights on women’s marriage decisions and on divorce

	Age of Married	Married		Nr Divorces	Log(Divorces)
Earnings Act	0.000 (0.116)	0.010 (0.009)	Earnings Act	0.817 (0.541)	0.016 (0.024)
Property Act	0.147 (0.158)	0.004 (0.009)	Property Act	-0.994 (0.531)	0.026 (0.023)
State FE	Yes	Yes	State FE	Yes	Yes
Year FE	Yes	Yes	Year FE	Yes	Yes
Mean DV	33.759	0.681	Mean DV	9.584	1.596
Num. obs.	352916	518382	Num. obs.	33153	33153

Table C6 shows estimation results for equation 3. It is based on data from the US Census for the years 1860, 1870, 1880, 1900, and 1910. In column 1, we use the individual age as the dependent variable, while using a sample of all white women aged 20-50 living with their husband. For column 2, we use a sample of all white women aged 20-50. We use a dummy that takes on the value a person has a spouse, zero otherwise. The table reports specifications with individual level controls. In columns 2-3 we use county level data on the number of divorces for the years 1867-1886. The dependent variable is the number of divorces, and the log divorces. State and year fixed effects are included in all specifications. We estimate linear (probability) models. Standard errors are clustered at the state level and reported in parenthesis. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table C7: The short-run effect of economic rights on women’s fertility

	Nr. Children	Nr. Children u. 5	Age 1st child
Earnings Act	-0.030 (0.059)	-0.034 (0.018)	-0.196 ** (0.070)
Property Act	-0.137 * (0.061)	-0.039 * (0.019)	0.335 *** (0.062)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean DV	2.596	0.809	23.074
Num. obs.	352915	352915	293002

Table C7 shows estimation results for equation 3 for white married women. It is based on data from the US Census for the years 1860, 1870, 1880, 1900, 1910. The sample includes white women aged 20 to 50 where the husband is present in the household. In column 1, the dependent variable is the number of children. In column 2, the dependent variable is the number of children below 5 years old. In Column 3, the dependent variable is the age at the first child - only children living in the household can be taken into account. The table reports specifications with individual level controls. The dummy variable controlling for the presence of a child younger than 5 in the household is not used in the specifications. State and year fixed effects are included in all specifications. Standard errors are clustered at the state level and reported in parenthesis. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table C8: The short-run effect of economic rights on children's school attendance

	All	Girls	Boys
Earnings Act	0.010 (0.024)	0.011 (0.026)	0.009 (0.024)
Property Act	0.072 ** (0.023)	0.068 ** (0.024)	0.077 *** (0.022)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Mean DV	0.735	0.735	0.736
Num. Obs.	281187	139003	142184

Table C8 shows estimation results for equation 3 for married women's children aged 6 to 11 years. It is based on data from the US Census for the years 1860, 1870, 1880, 1900, 1910. The dependent variable in all columns is a dummy variable indicating whether a child is attending school, zero otherwise. In Column 1, we use the full sample of all children of married women, in column 2 a sub-sample of girls, and in column 3 we use a sub-sample of boys. The table reports specifications with individual level controls. In addition to the controls discussed in Section 7, we control for mothers' age, and literacy. State and year fixed effects are included in all specifications. We estimate linear probability models. Standard errors are clustered at the state level and reported in parenthesis. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Table C9: The short-run effect of economic rights on child mortality

Panel A: Child Deaths/Total Deaths						
	All Children		Girls		Boys	
	Age < 1	Age < 5	Age < 1	Age < 5	Age < 1	Age < 5
Earnings Act	-0.001 (0.012)	-0.012 (0.018)	0.003 (0.010)	-0.003 (0.018)	-0.007 (0.011)	-0.020 (0.016)
Property Act	-0.014 (0.012)	-0.018 (0.018)	-0.016 (0.010)	-0.016 (0.019)	-0.021 * (0.011)	-0.031 (0.016)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean DV	0.224	0.41	0.202	0.378	0.209	0.374
Num. Obs	177	177	177	177	177	177
Panel B: Child Deaths/Children Living						
	All Children		Girls		Boys	
	Age < 1	Age < 5	Age < 1	Age < 5	Age < 1	Age < 5
Earnings Act	0.002 (0.010)	0.003 (0.004)	0.007 (0.010)	0.004 (0.004)	-0.002 (0.010)	0.001 (0.004)
Property Act	-0.031 ** (0.010)	-0.013 ** (0.004)	-0.033 *** (0.010)	-0.013 ** (0.004)	-0.029 ** (0.010)	-0.012 ** (0.004)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Num. obs.	0.105	0.038	0.096	0.036	0.114	0.041
Mean DV	177	177	177	177	177	177

Table C9 shows estimation results for equation 4. It is based on state level data from the US Vital Statistics for the years 1860, 1870, 1880, 1900 and data from the US Census for 1860, 1870, 1880, 1900. The dependent variables in panel A are based on the number of child deaths relative to the total number of deaths for different subgroups: Column 1 refers to the number of deaths of children aged < 1 year relative to the total number of deaths, column 2 refers to the number of deaths of children aged < 5 years relative to the total number of deaths, column 3 refers to the number of deaths of girls aged < 1 year relative to the total number of females who died, column 4 refers to the number of deaths of girls aged < 5 years relative to the total number of females who died, column 5 refers to the number of deaths of boys aged < 1 year relative to the total number of males who died, column 6 refers to the number of deaths of boys aged < 5 years relative to the total number of males who died. The dependent variables in panel B are based on the number of child deaths relative to the number of children surviving (in the same age group): Column 1 refers to the number of deaths of children aged < 1 year relative to the number of 1 year old children surviving, column 2 refers to the number of deaths of children aged < 5 years relative to the number of 5 year old children surviving, column 3 refers to the number of deaths of girls aged < 1 year relative to the number of 1 year old girls surviving, column 4 refers to the number of deaths of girls aged < 5 years relative to the number of 5 year old girls surviving, column 5 refers to the number of deaths of boys aged < 1 year relative to the number of 1 year old boys surviving, column 6 refers to the number of deaths of boys aged < 5 years relative to the number of 5 year old boys surviving. The table reports specifications with state level controls. State and year fixed effects are included in all specifications. Standard errors are reported in parenthesis. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.