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Women's Liberation, Household Revolution

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Abstract

How does women's empowerment affect fertility and children's education? In a dramatic revolution, U.S. states gave economic rights to married women between 1850-1920. Prior to this "women's liberation," married women were subject to the laws of coverture, which granted the husband virtually unlimited power of the purse within the household. Women's legal identities were subsumed (or covered) by their husbands. We show that granting women economic rights led to less fertility and more education. We employ an event study using the full count U.S. census and contiguous county-border pairs in bordering states that gave rights at different times. Additionally, rights were not retroactive, implying differences between those married before/after reforms. This alternative identification strategy confirms our findings and illuminates mechanisms. Quantitatively, women's empowerment can account for 15% (20%) of the decline (increase) in fertility (education) during the U.S. demographic transition. We find that shifting bargaining power accounts for these results with the underlying spousal disagreement relating to maternal mortality risk. Wealthier families decreased their fertility by more than other families, consistent with the notion that differences in control over wealth are responsible for our results. Articles from the New York Times confirm that people were aware of the implications of the legal changes. We provide evidence to negate mechanisms besides bargaining power shifts to explain our findings. Considering that the U.S. was a developing country at the time, our findings may be relevant for policy in developing countries today, where maternal mortality is still high and women are economically disadvantaged.

JEL Classification: N/A

Keywords: Women's liberation, Women's Empowerment, Household bargaining, Fertility, Education, Property rights

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If the principle of *séparation den biens*[1] were to be made a rule of law instead of an exception, our whole social relations would be changed. Old-fashioned people like himself were not ashamed to declare that it was written in nature and in Scripture that the husband was and ought to be lord of his household, the regulator of its concerns, and the protector of its inmates, which, if this Bill passed, he would no longer be.

Member of Parliament, Sir Alexander Beresford Hope, during the debate on the Married Women's Property Act of 1870, as described in Hansard (1870a).[2]

1 Introduction

How does women's empowerment affect fertility and education of children? And how important are these changes in accounting for the demographic transition? In one of the most dramatic shifts of economic power in human history, common law countries began giving economic rights to married women in the second half of the 19th century. Before this "women's liberation," married women were subject to the laws of coverture.[3] Coverture had detailed regulations as to which spouse had ownership and control over property and income, granting the husband virtually unlimited power within the household. Indeed, so great was the husband's power that a common saying was that "man and wife are one, but the man is the one" (Williams, 1947).[4] This paper explores the ramifications of coverture's demise on the decision making of households. In particular, we use the complete count U.S. Census from 1850 to 1920 and use two separate identification strategies to show that women's legal empowerment reduced fertility and increased the education of children. We find that women's economic rights can account for about 15% of the decline in fertility and 20% of the increase in children's education during the demographic transition of this time period.

Under coverture, personal property, including money, stocks, bonds, furniture, and livestock, became the husband's property entirely upon marriage. He could sell or give the property away, or even bequeath it to others. Real assets, such as land and structures, were placed under the husband's partial control while remaining in the wife's name. He could manage the assets as he saw fit, including any income generated by the assets, but he could not sell or bequeath the property without his wife's consent.[5] A married woman could not contract, and any income she earned in the labor force became her husband's property. Thus, coverture granted the husband virtually unlimited power of the purse within a household. This intrahousehold dynamic changed with the

[1] Separation of property between husband and wife.

[2] British House of Commons, April 14th, 1870.

[3] Coverture was an inherent aspect of British common law, and as such applied both in England and her colonies, including those that formed the U.S., Canada, and Australia.

[4] Indeed, Blackstone's commentaries on English common law declared "[b]y marriage, the husband and wife are one person in law; that is the very being or legal existence of the woman is suspended during the marriage . . . " (Blackstone, 1896).

[5] See Blackstone (1896) for the laws of coverture. For a summary of the general responsibilities that husbands and wives had to one another under coverture, see Basch (1982) Tables 1 and 2. We discuss further details of the laws of coverture in Appendix A.1.

introduction of married women's property laws, which was done state by state in the U.S., largely between 1850 and 1920.

The first of our two identification strategies exploits contiguous pairs of counties on either side of the border between two states that granted rights at different times, using an event-study approach. We find that fertility decreased following rights, with the probability of giving birth by about 1 percentage point, with the decline increasing for the first decade after rights are granted. This is consistent with the idea, discussed below, that the people driving the change in behavior are those married after rights are granted, and that the fraction of such people increases over time. Similarly, the number of children under 5 fell after rights. Both measures suggest a decrease of fertility by about 3% when rights were granted, and up to 7% three decades after rights were granted, accounting for about 15% of the overall decline in fertility between 1850 and 1920 in the US. The probability of a child being in school also dynamically increased by about 6-7% after rights were granted, representing about 20% of the overall increase in education in this time period. This increase in education was concentrated among primary school age children, and there was no quantitative or statistical difference between the effect of women's rights on sons and daughters.[6] A recent literature has documented econometric issues with event studies using two-way fixed effects of the sort used in this paper and has offered a few potential avenues to address these issues (de Chaisemartin and D'Haultféuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021; Gardner, 2021). As a robustness analysis, when performing our event studies, we also employ a two-step estimator of the sort analyzed in Thakral and Tô (2020), who generalize an approach introduced by Gardner (2021). The results of this robustness test are qualitatively and quantitatively very similar to our benchmark exercise, and thus we conclude that our benchmark event study analysis is appropriate.

The second identification strategy exploits the fact that these economic rights were not granted retroactively.[7] The 1900 and 1910 censuses asked people about the duration of their current marriage, allowing us to identify and compare couples who were married before and after rights were granted, within a county. We find that women age 20-39 who were married after rights were granted had about a 1 percentage point lower probability of giving birth in a year than those married before rights were granted. Thus, this evidence supports the hypothesis that the declines in fertility documented by the event-study approach are potentially accounted for by people married after rights are granted. The 1900 and 1910 censuses also asked about measures of completed fertility. Using a sample of women 45-59 years old, who presumably had completed their fertility, we find that those married after rights were granted had approximately 0.2 fewer children than

[6] One may wonder why invest in a daughter's education given the low married women's labor force participation rates. Behrman et al. (1999) argue that a mother's education is an important input into the education of children. Educating a daughter not only directly affects the education of grandchildren, but also increases the daughter's marriage market prospects.

[7] That is, property that was transferred from the wife to her husband, as a result of coverture, was not returned to the wife upon granting women economic rights. However, newly acquired property, such as newly received bequests, could be held by women married prior to rights being granted as long as the property was received after rights had been granted.

those married before rights were granted. This is quantitatively consistent with the probability of giving birth declining by 1 percentage point over 20 years.[8] Thus, the results documented are very similar between the two identification strategies, and suggests strongly that people married after rights are driving the declines in fertility we document.[9] We also find that children born to parents married after rights were granted are more likely to be in school than those born to parents married before rights were granted. We find that this effect is stronger for older children, which is presumably due to the fact that this exercise is performed in 1900 and 1910, when the relevant margin for increasing education was to allow older children to go to school.[10]

Section 6 argues that a shift in household bargaining power from husband to wife is the most reasonable mechanism to account for the results documented in this paper. First of all, legislators at the time were concerned that granting women rights would affect household tranquility by taking away men's power to make decisions.[11] Second people married after rights were granted can quantitatively account for our results. This suggests that perhaps only people who were actually affected by the law, which was not retroactive, changed their behavior. Third, we provide evidence consistent with maternal mortality risk being the underlying source of marital disagreement over the number of children. Indeed, we find that states with the highest maternal mortality risk saw declines in fertility following women's rights of more than twice what other states experienced. The importance of maternal mortality risk is not surprising: Albanesi and Olivetti (2016) discuss how approximately 1 in 125 live births resulted in maternal death in 1900, while disability among mothers was even greater. They calculate that, on average, disability-adjusted life years, which takes into account both death and disability risk, was about 1.1 years per pregnancy in 1930, and was presumably larger in our time period.[12] It is reasonable to assume that husband and wife disagreed over their willingness to tolerate such risks in having additional children.[13] As such, a transfer in bargaining power from husband to wife would yield a decline in fertility. Fourth, we provide evidence that wealthier families decreased their fertility by more than other families, consistent with the notion that differences in control over wealth are responsible for our results.[14] Finally, as discussed below in Sections 2 and 6, our findings are consistent with other

[8]We further document that this decline in fertility was along the intensive margin, rather than extensive margin.

[9]We note that this exercise is not subject to the critique of event-studies with two-way fixed effects discussed above. The fact that we find similar results here as in our event studies suggest that the concerns of the two-way fixed effect event-study literature is not of first-order significance for our analysis.

[10]By this time period, almost all primary school age children were in school anyway.

[11]This can be seen in the quote at the start of this paper, and in the economics and historical literatures referenced in Sections 3.3 and 6.

[12]This is still true in the developing world today. WHO (2021) finds that the probability that a 15 year old woman will eventually die from maternal causes to be 1 in 45 in low income countries. Bhalortra et al. (2021) note that "[t]here is no single cause of death and disability for men aged 15-44 that is close in magnitude."

[13] Alternatively, different levels of information could also generate a similar phenomenon. Ashraf et al. (2020) study developing countries in modern times and find that husbands have a much lower level of knowledge about maternal mortality and morbidity risks than their wives do. Once these men are educated on the topic, they display a reduced desire for fertility.

[14]Relatedly, we provide evidence in Section 3.1 that the topic of women's property rights was widely covered by newspapers at the time. This suggests that people, especially the wealthy who were more likely to read newspapers, were indeed aware of the changes occurring in the legal system.

papers showing the effects of empowering women in households.

Section 6 continues to discuss why other mechanisms seem to fail to account for the facts. First, we evaluate the hypothesis that women's rights increased the opportunity cost of women's time, and thus affected fertility. In the appendix to this paper we document that there is no change in labor force participation (LFP) rates among married women as a result of granting women economic rights.[15] The lack of impact on LFP is perhaps not surprising given the low rate of married women's LFP at the time, which was below 5-6% during our entire sample time period.[16] Similarly, such a mechanism might predict an incentive to increase investment in the education of daughters relative to sons, which we do not find in the data. Second, the fact that being married after rights were granted can account for much of the changes we document strongly suggest that the underlying mechanism was within households affected by the change in rights. This is as opposed to mechanisms, such as general equilibrium effects of women's rights, that would change behavior for all households, regardless of whether they married before or after rights were granted.[17] Finally, Doepke and Tertilt (2009) argue theoretically that men wanted to grant economic rights to give *other* men's wives power, which would increase investment in the human capital of other children (since they assume women to have stronger preferences for quality of children, rather than quantity).[18] On the face of it, this theory makes similar predictions to our own: women's rights give women more power, decreasing fertility and increasing education. However, their theory cannot account for the fact that maternal mortality risk is strongly associated with the decline in fertility following women's rights. Thus, while their mechanism might have a role to play, it cannot explain all of our findings.

The paper proceeds as follows. Section 2 relates this study to the current economics literature. Section 3 discusses the history of coverture and its demise in the U.S., our choice of sample timing, and why men chose to give married women property rights. Section 4 discusses the data and empirical strategies used in this paper, including the conditions under which we can take a causal interpretation of our results. Section 5 presents our regression results, including a variety of robustness exercises. Section 6 discusses various economic mechanisms that connect married women's property rights and fertility, investment in education, and married women's labor force

[15]There is a large and growing literature on the effects of gendered laws and women in the workforce. Hyland et al. (2020) studies gendered laws across 190 countries and 50 years. They find that countries that pass laws beneficial to women see a shrinking gender pay gap and an increase in female labor force participation. While this paper does not find any effects on labor force participation, it is possible that property rights affected the gender pay gap. We are not aware of any available data that would allow us to evaluate this hypothesis.

[16]It is perhaps not surprising that women's rights did not affect LFP. As discussed below, pregnancy was incredibly dangerous for women, reducing both their ability to work and their incentive to invest in careers given anticipated negative health outcomes with future pregnancies. Improvements in maternal health over the 20th century have been credited with increasing women's labor supply and human capital investment (Albanesi and Olivetti, 2014, 2016).

[17]One such general equilibrium effect could be that women's rights cause deeper financial markets and growth (Hazan et al., 2019). This change might increase the returns to education, and thus reduce fertility through a quantity-quality tradeoff. However, this mechanism would affect all households equally, and not just those married after rights were granted. Thus, we do not believe that this hypothesis can account for our findings.

[18]They argue that increased returns to human capital led men to give women rights in order to further all children's human capital investment (which, in their model, has a positive externality through the marriage market).

participation. We explain in detail our conclusion that shifting bargaining power from husband to wife can account for the results we document. We conclude in Section 7.

2 Literature Review

In this section, we discuss how this paper relates to three sets of literatures. The first is the role of women's empowerment in fertility and human capital investment in children, or, more broadly, the relationship between women's empowerment and development. The second is the theoretical and empirical literature examining the demographic transition in the U.S. towards lower fertility and more education. The third is the importance of laws for household bargaining and decision making. Naturally, this paper also relates to the general literature on women's economic rights during this time period, reviewed below in Section 3.3. Similarly, this work relates to the literature on the importance of health. This shows up in the literatures on disagreements between husband and wife, as well as the demographic transition, discussed below, along with the importance of maternal mortality risk for understanding women's labor supply and human capital investments discussed above.

We begin by relating to the literature documenting the impact of women's empowerment on fertility and education of children. Central to the claim is the idea that men and women have different preferences over the quantity and quality of children. There is empirical evidence that husbands tend to prefer more children than wives (Rasul, 2008; Doepke and Tertilt, 2018; Doepke and Kindermann, 2019) and that more household income in the wife's hands affects investment in children (Thomas, 1993; Lundberg et al., 1997; Attanasio and Lechene, 2002; Basu, 2006; Qian, 2008; Bobonis, 2009; Doepke and Tertilt, 2019).[19] The idea we focus on in this paper is that women bear significant mortality and morbidity risk in childbearing, especially in developing countries (such as the US in the 19th century), and thus may prefer smaller families (Albanesi and Olivetti, 2014; Ashraf et al., 2014; Albanesi and Olivetti, 2016; Ashraf et al., 2020).[20] A quantity-quality tradeoff would immediately translate reduced fertility into more investment in children's education. Indeed, Bhalortra et al. (2021) find that gender quotas increasing the representation of women in the parliaments of developing nations yield lower maternal mortality risk, as health care increases, alongside a decrease of 6-7% in fertility and an increase in schooling of young women. We complement these works by documenting how a major reworking of the laws governing property rights within marriage can affect fertility and investment in children's education.[21]

[19] It is also worth noting that the literature on women's suffrage has documented strong effects of giving women the right to vote. Lott and Kenny (1999) document increases in public expenditures following women's suffrage, while Miller (2008) shows that women's suffrage led to better health outcomes, especially for children. These facts further support the notion that women and men have different preferences, especially over children's welfare.

[20] Another theory as to why women prefer lower fertility rates and to educate their children more may be is evolutionary theory, with men uncertain over their paternity status or women having more limited reproductive capacity. Indeed, Doepke and Tertilt (2009) use this theory to justify their assumption that women put a higher weight on child investment than men. See their paper for references to the relevant theoretical and empirical literature on the subject.

[21] Another direction the literature has taken is to study the impact of empowering adolescent women with both

More broadly, this paper contributes to the literature on the two way connection between women's empowerment and economic development (Duflo, 2012; Doepke and Tertilt, 2018, 2019). We contribute to this literature by empirically documenting how legal changes granting women more economic rights affect fertility and human capital.[22] Thus, our work has direct implications for the developing world today, which in many ways resembles the US in the 19th century. In particular, our analysis can be used to infer the implications of legal changes affecting female empowerment in the developing world.

Next, there is a large theoretical literature on the demographic transition (e.g., Galor and Weil, 2000; Galor and Moav, 2002), but relatively few empirical papers studying the demographic transition in the U.S. Bleakley and Lange (2009) study how the elimination of the hookworm affected the cost of investing in child quality, and thus fertility. Doepke (2005) rejects the hypothesis that a decline in infant mortality was a factor in the demographic transition in the U.S. Specifically, his model predicts that a decline in infant mortality reduces total fertility, but increases net fertility. However, he argues that since total fertility in the U.S. has been declining from the early 1800s, while infant mortality remained high till the end of the 1800s, this (popular) explanation seems unappealing for the U.S. experience.[23] Greenwood and Seshadri (2002) attribute much of the demographic transition to rising income, as well as structural transformation away from the farm. Jones and Tertilt (2008) and Jones et al. (2010) find an income elasticity of fertility of about -0.30 to -0.38, and argue that rising income can account for much of the change in fertility in the US between roughly 1830 and 1960. We complement this work by empirically showing the role that legal changes empowering women had for the demographic transition in the U.S.

Finally, there is a large literature on how legal changes can affect household bargaining.[24] Wolfers (2006) studies the introduction of unilateral divorce laws in the U.S., which occurred by state, and finds that they increased the probability of divorce. He argues that this finding is evidence that Coase's theorem does not apply in marriage. This yields the prospect that reassigning rights within a marriage, be they rights over property or rights to divorce, may yield changes in household choices. Stevenson and Wolfers (2006) similarly study the change of these laws, and find that they reduced the probability of suicide and spousal homicide. This is due to the fact that unilateral divorce laws transfer the power to end the relationship to the abused spouse. Voena (2015) examines how the introduction of unilateral divorce laws affected labor supply and savings choices. She studies a sample of households married prior to the changes in laws, and compares the im-

vocational knowledge and information on sex, reproduction and marriage. Bandiera et al. (2020) find that this form of empowerment leads women to be self employed, less likely to be teen mothers, enter into an early marriage, or report forced sex. The women's empowerment we study here could presumably have had similar effects, though data are lacking.

[22]Our previous work, Hazan et al. (2019) also shows that women's economic rights affect financial markets and growth.

[23]Our work also relates to Bleakley and Lange (2009) and Doepke (2005) as they study the importance of health and mortality for the demographic transition. More generally, Hazan and Zoabi (2006) study theoretically the relationship between health and longevity and growth through the impact on the quantity and quality of children.

[24]Iyigun and Walsh (2007) discuss how general changes in institutions that shift power towards women can lead to lower fertility and more education, similar to that documented in this paper.

pact of unilateral divorce on couples in states that divide assets between spouses equally upon divorce to those with a title-based regime.[25] We differ from this literature by emphasizing the role of property rights during marriage, rather than the right to divorce or division of assets upon divorce, affect household bargaining.[26]

3 Women's Economic Rights

In this section, we discuss which laws are relevant for our analysis, and issues related to the timing of rights granted by states, followed by our discussion of why we follow the literature in analyzing economic rights as the union of rights over property and labor income. We also include a discussion of public awareness of these legal changes. We then discuss our choice of sample time period. We conclude by discussing hypotheses as to why men granted women economic rights.

In Appendix A we give an overview of the history of coverture. We discuss differences between states that followed civil law, which treated marital property according to community property rules, rather than common law with its use of coverture, as well as why these states should be included in our main analyses. As discussed below, we perform robustness tests dropping these states. We also discuss issues of equitable estates in the U.S.

3.1 Timing of Rights

Married women were not given economic rights in the U.S. overnight; rather, different sets of rights were granted in successive waves.[27] Property laws were passed by state legislatures, generally narrowly interpreted by courts (Chused, 1983; Zeigler, 1996), and updated again.[28]

We use the data on the timing of women's liberation by state from Geddes and Lueck (2002).[29] They code the year in which states first granted women rights over both their own property

[25]We are unaware of any papers specifically linking the change in household bargaining power that resulted from these legal changes on investment in children. However, Gruber (2004) argues that children who grew up under unilateral divorce laws were more likely to experience bad outcomes, such as lower education, income, and more suicide, as adults. However, that paper does not specifically address the effect of unilateral divorce laws on bargaining within married households, and thus investment of resources in children, as opposed to the effect of divorce, which rose as a result of these laws, on children. Relatedly, Clark (1999) argues that both divorce and property law affect the outside option of women inside marriage.

[26]We discuss different regimes for dividing assets between husband and wife in the U.S. in Section A.2. We perform robustness analyses on states that divide property differently than the common law would suggest, as discussed below.

[27]Similarly, rights were granted in waves in England. Married women received partial rights over property in 1870, specifically with regard to certain types of savings/investment accounts and inheritances up to 200 pounds, though the reform was not always upheld in court. The 1870 law was updated in 1874 to prevent fraud. A more significant update to property rights came in 1882, which more or less granted women the same economic rights as men. Further minor updates occurred over the 20th century (Holcombe, 1983, pp.178-205)

[28]States almost never retracted rights once they were granted, presumably since the rights increased economic growth. Many experts from states that granted rights were invited to testify in the British House of Commons during the debate on granting women property rights in England, which passed in 1870. Dudley Field of New York, which had granted rights prior to England, argued that "[s]carcely any one of the great reforms which have been effected in this State has given more entire satisfaction than this." Mr. Fisher from Vermont testified that "I do not believe that I have ever seen an individual in the State who wanted to go back to the old law" (Hansard, 1870b).

[29]We thank the authors for making their data available to us.

and their labor earnings, which we refer to as Geddes and Lueck "both" dates, or *rights*. Their methodology in dating rights is as follows: "[f]or control of property, we used the earliest year a state passed an act allowing married women management and control of their separate estate (similarly for earnings). If a state passed a married woman's property act, but the act did not grant the woman management and control of her separate estate, then this date was not used. This approach provides a specific characterization of married women's property that emphasizes control by the wife" (Geddes and Lueck, 2000, p. 65). These statutes were certainly enough to grant substantial power to women.

Four questions arise regarding our choice to use Geddes and Lueck (2002)'s dates. The first issue is: why use the dates in Geddes and Lueck (2002) as opposed to other, earlier, waves of laws? The second is: why use the timing of *both* property and earnings rights, rather than examine the potentially different effects of each type of rights separately? The third question is: how should we evaluate states that had community property laws, as in civil law, rather than formal coverture, as in common law? Finally, the fourth question is: did women have ways of circumventing the laws of coverture, such as through separate estates through the equity courts?

Property laws prior to those studied by Geddes and Lueck (2002), known as "debt statutes," did not significantly affect women's rights. Indeed, Chused (1983, p.1361) argues that "[t]hese acts . . . created a set of assets available for family use when husbands found themselves in trouble with creditors" and concluded that they "made only modest adjustments in coverture law, and generally confirmed rather than confronted prevailing domestic roles of married women."[30] Thus, while these statutes did protect a wife's real and moveable property from her husband's creditors, they did not protect women from their husbands.[31] Accordingly, these statutes did not protect the wife's property or earnings from her husband, and thus change the balance of power in the household.

Turning to the second question, it is not necessarily clear that "both" dates represent the correct set of dates for this study. We should use the date a state passed (or implemented) a law that both withstood legal tests and granted women extra economic rights. Presumably, we could even analyze earnings rights and property rights separately. However, there are two reasons that "both" is more appropriate as a benchmark, and thus used by the literature (Geddes and Lueck, 2002; Fernández, 2014; Hazan et al., 2019).

The first reason for using "both" is that there is a high degree of complementarity in these rights

[30] In a fascinating paper, Koudijs and Salisbury (2020) study how these debt statutes, by preserving some family assets in the case of default, affected risk-taking behavior in the U.S. South.

[31] How is it possible for a woman to have separate moveable assets if common law allows the husband to take them upon marriage? For a husband to own his wife's moveable assets, he had to "reduce them to possession," or actively take control of his wife's property. If he did not do so, they remained her assets and, after the debt statutes were passed, were immune from his creditors. The exact definition of what constituted reduction to possession varied state by state and over time, and had implications for the ability of a husband's creditors to seize the assets. For one example of this in Ohio, see the discussion on pp. 114-115 of Chused and Williams (2016). Before these debt statutes, a wife's separate moveable property was liable for a husband's debt even if he had not reduced these assets to possession. See Justice Wright's discussion in *Dickerman v. Abrahams* 21 Barb. 551 (1854), Supreme Court of New York.

such that it is inappropriate to consider property rights without earnings rights, or vice versa. We first discuss the inability to consider property rights without earnings rights. An example of the complementarity in rights is seen in *Apple & Co. v. Ganong* 47 Miss. 189 (1872). In this case, a Mr. Ganong, husband of Louisa Ganong, declared bankruptcy in Mississippi. His creditors sued to gain possession of Louisa's land. At the time, her separate estate was protected from her husband's creditors, but her *earnings* were not. Was her land part of her separate estate? She purchased the land with a combination of money from the sale of a gift of cotton from her mother and earnings from sewing for soldiers during the Civil War. The court ruled that her husband implicitly owned the share of her land that was purchased with her labor income, and thus it was liable for his debts. This case and others like it show how difficult it was to establish property rights when only partial rights existed, strengthening the argument for "both" dates to be used. Indeed, Chatfield (2014) argues that these types of cases help explain why Mississippi granted women rights over their earnings, making investigations into how women purchased property unnecessary.[32]

Similarly, consider *Glover v. Alcott* 11 Mich. 470 (1863). In this case, Deborah Alcott, a married woman, owned and operated a mill in Michigan after married women were granted the right to own and dispose of all types of property, but *before* they were given the right to their labor market earnings. Husbands still had the right to their wives' time, services, and labor income. The case came down to the question of whether Mrs. Alcott had the right to manage her business for her own benefit, or if this was considered labor income and thus attributed to her husband.[33] The Supreme Court of Michigan ultimately decided that this income indeed belonged to her husband, despite the fact that business was performed on her property, by her, and with her property used as collateral for the associated capital. Indeed, Justice Christiancy in his deciding opinion argued that, if women were allowed to take income from a business they owned, nothing could stop them from setting up a pass-through business and circumventing the earnings law, such that she "... would have it in her power to deprive her husband entirely of all right to the time and services in the care and management of his household." Justice Campbell, dissenting, argued that this ruling would not allow a wife to place a mill on her land, as she could if unmarried, leaving it unproductive. The lack of earnings rights was therefore a serious disability in property rights.

It is relatively easy to understand why earnings rights without property rights were ineffective: "... where her wages mingled indistinguishably with her husband's in savings accounts or in common household possessions, she lost her title to her earnings as well as to the furniture, clothing, and utensils purchased by the joint fund ... For when the earnings of husband and wife mixed, neither juries nor creditors had a way to ascertain what belonged to her and what belonged to him" (Stanley, 1988, p. 497). That is, if a wife earned money in the labor force, and could not put

[32] See Chatfield (2014) for a longer discussion on how partial rights created confusion in the U.S. credit markets.

[33] The distinction between capital income and labor income is still hotly debated today, as seen in the debate over taxation of "carried interest." See, for example, http://www.nytimes.com/2012/03/04/business/capital-gains-vs-ordinary-income-economic-view.html

the money into an account completely separate from her husband, the money effectively belonged to him, even if she technically had rights over these earnings.

We note a corollary to the inability to have one type of rights without the other: we cannot use the time difference of the of granting each type of rights to deduce which mechanisms drive our results. That is, suppose we believe that property rights are more important for household bargaining, and that earnings rights increase the opportunity cost of women's time at home. In theory, we could exploit the difference in the timing of these types of rights within states to ascertain the relative importance of each mechanism. Unfortunately, we cannot do so, as it does not make sense to discuss one type of rights without the other.

The second reason that "both" dates may be appropriate is that, given the legal issues that arose around granting rights, state governments often needed more than one round of legislation to effectively grant economic rights (Chused, 1983; Zeigler, 1996). Consider that property rights were generally granted before earnings rights, but that issues with property rights were often only solved when updating earnings rights. For instance, New York gave married women property rights in 1848. It is therefore curious that the 1860 earnings bill includes explicit protection of women's personal property in Section 2. Why did the legislature include this seemingly redundant protection? Turning to *Dickerman v. Abrahams* 21 Barb. 551 (1854) in the Supreme Court of New York, in which Justice J. Wright gives a legal overview of the 1848 law. Justice Wright explains that the New York legislature made a series of mistakes when passing the law, for instance, the law was interpreted as only providing married women with rights over real estate.[34] Rights over personal assets were granted only later together with labor earnings rights in 1860. New York is not a random example- New Jersey copied the New York statute almost verbatim, and Wisconsin, Virginia, and West Virginia all also used similar language as New York.

We next make use of the *New York Times* archive from the 19th century to show that the press covered legal changes in marital property laws, court cases of importance for understanding these legal changes, and scholarly discussions of the legal history. This shows public interest in, and knowledge of, the changes in married women's property rights as they were happening.

We begin with the *New York Times* covering legal changes to married women's economic rights. For example, it printed the 1860 law, discussed above, in its entirety upon passage (New York Times, 1860c). However, interest in the topic was so great that they did not only print updates of laws in New York, but rather around the U.S., and indeed the U.K. as well. For instance, in 1852 they reprinted an article entitled "Women's Rights and Wrongs" from the *Detroit Tribune* explaining exactly the difference in legal rights between men and women in the state at the time

[34] He begins by noting that, under the reform, "[t]he disposition of her personal property and of the rents, issues and profits of her real estate had been taken from her husband, and lodged nowhere." That is, while the 1848 law indeed protected a wife's assets from her husband, they gave her no control over them, a by-product of her inability to contract, a capability which came later with earnings rights. Simply put, *no one* had control over a married woman's property after the 1848 law. Justice Wright continues by noting that when women's rights were updated in 1849, semantic issues around the words "convey" and "devise" led him to believe that women still did not have rights over their personal property.

(New York Times, 1852). They also reprinted an article from the *St. Paul Press* when Minnesota granted women economic rights (New York Times, 1869).[35] In 1870, they printed an article explaining to their readers that the "married women of Connecticut suffer injustice," as women had not yet been granted rights in that state, before delving into the efforts in that state to change the law (New York Times, 1870). After women's rights were eventually passed in 1870, they reported on a New-Haven *Journal* article providing a "summary, more complete than we have yet seen, of the provisions of the new law of Connecticut in relation to the property rights of married women ..." (New York Times, 1877a). The *New York Times* also printed updates on the debates and legal changes in the United Kingdom (New York Times, 1871, 1877b, 1882).

Considering that the courts often interpreted legal changes conservatively, it is also important to ask whether the public was made aware of the ongoing changes in the interpretation of the law, rather than just the passages of laws. The answer is an emphatic "yes," again both within New York as well as around the country. For instance, consider the case of *Vandevoort v. Gould*, 36 N.Y 639 (1867), which asked legal questions pertaining to whether married women's property acts applied to property applied before the passage of the act. The *New York Times* covered this case closely, giving readers updates while the case was still on, as well as the final decision and implications (New York Times, 1860a, 1862a,c,b).[36] Again, they did not confine coverage to New York. They reported on court cases in Maine (New York Times, 1868), Illinois (New York Times, 1875), and Missouri (New York Times, 1888). They also went into detail covering public lectures on women's rights, such as given by James T. Brady, Esq. entilted "The Legal Disabilities of Women" (New York Times, 1858).[37] The reviewed scholarly work, such as Ostrogorski (1893), when this work was imported into the United States (New York Times, 1894), and gave summaries of cases where married women did not seem to have their rights enforced (New York Times, 1879).

As such, it seems reasonable to conclude that the class of people who read newspapers such as the *New York Times* were both interest in, and informed about, the evolving state of married women's property rights.[38]

3.2 Sample Period

Returning to the dates used in this paper, Figure 1 shows the date when each state granted women "both" rights. Massachusetts was the first state to grant these rights, in 1846. Ideally, we would start our analysis in 1840. However, Ruggles et al. (2020) provide U.S. census data that are com-

[35]It is worth noting that this article refers to the old laws as "barbarous," and explains exactly what rights the new laws do and not convey to each spouse.

[36]This court case is not unique. We found several other articles in the *New York Times* covering other cases of importance for women's economic rights (New York Times, 1854, 1860b, 1865a,b, 1866).

[37]The article covers the first of a series of lectures to be given on women's rights by Mr. Brady, and gives details as to his view on the legal history of women's economic rights. As further evidence as to how interested people were in the topic, the article describes the audience as "large and markedly fashionable."

[38]As shown in Section 6, wealthier households changed their behavior the most in response to married women's rights. It is reasonable to assume that the readers of the *New York Times* were more likely to be among these wealthier households.

parable over time beginning only in 1850. Accordingly, our analysis begins in 1850. We follow Geddes and Lueck (2002) in stopping our analysis in 1920. This is for two reasons. First, by 1920 rights were granted in all states except Florida (1943), Arizona (1973), New Mexico (1973), and Louisiana (1980). Second, as noted in Geddes and Lueck (2000), it may not be fair to call the post-1920 era true coverture, as the 19th Amendment (passed in 1920) granted women the right to vote, which may well have affected *de facto* implementation of laws.

3.3 Considerations of Giving Women Rights

We turn next to the existing literature on why legislatures – all comprised and controlled by men – gave women economic rights.

We begin by noting that the economics and history literatures are united in making explicit that men viewed a loss of bargaining power at home as the main downside of granting women rights. Indeed, Griffin (2003), in reviewing the debate over women's property rights in England, makes clear that men were hesitant to give up their own rights at home. The reasoning, from the historical archive, for granting women property rights seems to be to protect women from abusive husbands who might leave their families impoverished.[39] Holcombe (1983) similarly discusses the history of women's property rights in England in the context of defending families against male-inflicted poverty. Stanley (1988) discusses similar motives in state legislatures in the U.S.

Similarly, while the feminist movements of the time clearly supported women's property rights, our reading of the history and economics literatures does not support the notion that feminism was a driving cause of married women's economic rights, though it seems to have been a major driver behind women's suffrage. Indeed, the first law passed in New York State to grant married women property rights, discussed below, was passed three months *before* the Seneca Falls convention, widely considered to be the beginning of the feminist movement in the U.S. One simple way to empirically examine whether feminism is the driving force is embedded in Figure A.11, which plots the year that each U.S. state granted women economic rights on the Y axis against the date of women's suffrage on the X-axis. There is no correlation between the timing of these rights. Case in point is Massachusetts, the first state to grant women economic rights, and among the last states to grant women political rights in 1920.[40] Indeed, Stanley (1988, p. 484) argues that "[m]arried women gained legal title to their wages, noted a lawyer who wrote often for the *Women's Journal*, 'not from a sound philosophical view of the case,' but simply from 'expediency or necessity.' "

The economics literature diverges on the economic incentives to give women these rights. As discussed in the introduction, Doepke and Tertilt (2009) argue that men wanted to grant rights to give

[39] Similarly, we read the debate in the British Parliament on granting women property rights. The debate included fascinating discussions about defending indigent women against drunk husbands, for example, or the potentially ill effects of women's rights on the "harmony" of previously male-dominated households.

[40] Notice that only 4 states, Utah, Idaho, Wyoming, and Colorado, gave women both political rights relatively early. It has been hypothesized that these rights were granted in order to draw women to the frontier as the U.S. expanded westward.

other men's wives power, which would increase investment in the human capital of other children. They argue that increased returns to human capital led men to give women rights in order to further all children's human capital investment. As such, our results on the increase in education (of children of both genders) can be viewed as supporting the idea in Doepke and Tertilt (2009) that women's rights would lead to more education, though we introduce a different mechanism here. Our results do not support (or negate) their premise that increasing returns to education may have been a driving force behind granting women's rights. Fernández (2014) argues that fertility rates determine women's rights. The author posits that if fertility is low, then the size of the inheritance that daughters *do not* receive, as it is taken by the son in law, under coverture is large, representing a loss to fathers. Fathers may have wanted to ensure that their daughters could actually receive their inheritance, and thus granted women rights. The author measures fertility as the number of children in a state between ages 10-19 divided by the number of women age 20-39. Using this cross-state measure, she finds a negative correlation between fertility rates and women's rights. Our results, discussed below, reject this hypothesis. Our data makes use of the 100% census count, and analyzes fertility in households, rather than the average number of children divided by the average number of women, as in Fernández (2014). This allows for our event-study comparisons of people on either side of county-border pairs, in which we do not see any trend in fertility rates in the county on the side of the border that gets rights first prior to rights being granted, and a decline afterwards. Furthermore, as we document below, this decline in fertility occurred predominantly among those married after rights were granted, strongly suggesting that economic rights caused the fertility decline, rather than vice versa. Thus, we do not find evidence that supports the idea that declines in fertility led to women's economic rights, but we find substantial evidence suggesting the opposite is true.

Geddes and Lueck (2002) argue that coverture decreased women's incentive to work, as their earnings went to their husbands. While we do not find support of this mechanism in the U.S., as we find no evidence that women's labor force participation rates increased when economic rights were granted, this may have been a significant mechanism in England, where married women's labor force participation was much higher than in the U.S. at this time. Finally, Hazan et al. (2019) argue that coverture led to a distortion in portfolios by incentivizing single women, and parents of all women, to invest in real assets, rather than personal assets such as bank accounts. They find that granting women rights led households to reallocate their portfolios towards personal property, which in turn led to an aggregate increase in bank deposits, a reduction in interest rates, and an increase in bank loans. This financial market deepening in turn led to a reallocation of workers from agriculture towards non agriculture (manufacturing), with this reallocation biased in favor of capital-intensive industries. While they do not explicitly evaluate the hypothesis that this financial market deepening and economic growth may have been the reason to give women economic rights, it is a potential hypothesis nonetheless.[41]

[41] Hazan et al. (2019) discuss and empirically evaluate potential hypotheses for why men chose to grant women's economic rights in Section 2 of their Online Appendix. However, they are unable to draw any robust empirical conclu-

4 Data and Empirical Strategy

In this section, we outline our data, including summary statistics, and empirical strategy.

4.1 Data

We first turn to the data used in our analysis. We first describe the data source, sample selections, and our outcome variables of interest.

Our data for the event-study analyses, unless otherwise specified, come from the complete census count from 1850-1920, less the 1890 census which was destroyed in a fire (Ruggles et al., 2020). When looking at the education of children, we use the 1900 5% sample instead of the full sample, as the full sample does not currently include information on education. When examining female LFP (Appendix F), our analysis begins in 1860, as no information is available in 1850 for married women. Our data comparing outcomes for households where the husband and wife were married before or after rights comes from the 1900 and 1910 censuses, as these were the only two censuses to ask couples about the duration of their current marriage, which in turn allows us to infer the year of their marriage.

Our sample consists of households with white, non-Hispanic, married women living in the same state in which they were born. We restrict attention to married households to abstract from any issues related to out of wedlock birth, which was exceedingly rare at the time, or investment in human capital in single parent households. We document in Appendix C that granting women property rights had only a negligible impact on marriage markets, as measured by the propensity to get married, the age of married people, and age gap between husband and wife.[42] This reduces concerns that our sample selection of married households could bias our results.[43] We restrict attention to whites in order to abstract from issues related to race in this time period.[44] We restrict attention to women who live in the same state in which they were born in order to avoid issues related to misunderstanding marriage property laws that may arise from migration.[45] We discuss below our age restrictions on our sample for each of our exercises.

We begin by discussing our outcome variables for measuring fertility. Our first outcome variable of interest is "birth," which we define as whether a wife gave birth in the previous calendar year,

sions.

[42]We do not have a measure of marital sorting available in our data.

[43]It may be surprising that such a profound change in marital laws had no measurable effect on the propensity to marry. However, we note that, at the time, the decision to get married was likely a "corner solution" for most people. Without marriage, people were unable to have children, which were implicitly their old-age security system (Neher, 1971), and could not achieve the considerable gains to specialization according to comparative advantage, with the husband in the labor force and wife taking care of the household (Greenwood et al., 2005b,a; Greenwood and Guner, 2008; Greenwood et al., 2016). Socially, the undesirability of remaining unmarried can be seen by the negative view of older, unmarried women, or "spinsters." While eventually these women would be seen as having a feminist role, during the 19th century, they "were scorned as having failed in the main business of a woman's life, the marriage market," and "spinsterhood was still represented as a social and individual problem" (Oram, 1992, p. 414).

[44]Hispanics were a very small part of the population at the time.

[45]The misunderstanding could come from either being unaware of a difference in laws between states, or which state's laws apply to a given marriage.

which we infer by whether there was a child in the household whose birth year was in the previous calendar year.[46] Notice that this variable is binary, and does not take different values in case there was more than one child born in the previous year. Our second measure of fertility is the number of children under age five. This is the sum of the members of a household, who are children of the head of the household, and are aged four or less. It might seem natural to look at other outcome variables, such as the total number of children that a couple has; however, we note that our method of calculating the number of children requires them to be at home when the census was taken.[47] Considering that older children may have left home, we limit our analysis to the number of children under five. We study these variables both in our event-studies, as well as when comparing households married before and after rights were granted. When analyzing these variables, we restrict attention to households where the wife is age 20-39, and the husband is age 20-50, in order to focus attention on households that are likely to have newborns and young children.

In the 1900 and 1910 censuses, women were also asked about the number of children they ever gave birth to ("children ever born"), as well as the number of surviving children they birthed ("surviving children"). These variables measure completed fertility. As such, we analyze these variables in households where the wife is age 45-59 in order to capture women who have finished giving birth. Since we have completed fertility data from two years, and no states gave women rights between 1900 and 1910, an event-study design is not appropriate for analyzing these variables. However, these two censuses include information on the duration of marriage, and thus we can do our analysis comparing households married before and after rights were granted.

Our measure of female labor force participation is to use IPUMS' occupation (1950 basis) code.[48] This variable is available for women from our sample in all of the full-count US censuses from 1860-1920. In contrast, the census variable "labforce," which directly measures whether a person is in the labor force, is available only in the 5% sample for the 1900 census.[49] However, these two variables agree on labor force participation rates, with a correlation of 0.93, when both variables are available. We examine this variable both in our event study design as well as in our analysis comparing households married before and after rights were granted. We do so both for households with women age 20-39, in order to be consistent with the sample of women in our event studies on fertility, as well as 45-59, in order to cover the remaining set of married women who potentially work.[50]

[46] The census was taken during different months in different years. Looking at the previous calendar year provides a consistent measure of birth probabilities between the census years.

[47] One concern might be how infant and child mortality rates affect our estimates. In general, we will not observe a child who has already died, and thus our fertility statistics will be biased downward. However, in untabulated results, we find that the probability of children surviving was not affected by women's property rights. As such, the bias in the fertility statistics is constant around the timing of rights and should not affect our estimates.

[48] We denote any woman with a code ≤ 970 as being in the labor force.

[49] Notice that the 1900 census is crucial for our exercises comparing households married before and after rights were granted.

[50] There is one another issue with this variable worth addressing. In the 1880 100% sample, some 11% of women are

Our measure of schooling for children is the IPUMS' variable "school," which measures where a child is currently in school. Prior to 1900, the question was asked of persons "of school age." In 1900 and 1910, the question was applied to all persons age 5-21; however, implicit in the question was whether the student was in college. We restrict attention to children ages 8-17, as they are under their parent's legal control. We examine households in which the wife is 20-59 and the husband 20-69 years old, in order to capture older children born to older parents.

We next turn to summary statistics for our variables of interest.

4.2 Summary Statistics

The top left panel of Figure 2 shows the probability of giving birth, by age, for women in our event-study sample, depending on whether or not they have economic rights. This variable is net of year fixed effects, in order to capture general demographic trends in the US between 1850 and 1920. The probability of giving birth rises after age 20, peaks at about age 23, and then declines. Clearly visible is the difference in the probability of giving birth between women with and without rights. Women who have economic rights are less likely to give birth, especially after age 25. Similarly, the top right panel of this figure does the same exercise examining the number of kids under age 5. The number of kids under age 5 rises until the mother is approximately 26-28, and then falls. Consistent with birth probabilities, after age 25 there are fewer kids under age 5 in households where the wife has economic rights. The bottom panel of this figure does the same exercise on the labor force participation rates of these women. While there are differences in the labor force participation rates of women younger than 32, they are quantitatively quite small, with the maximum difference between these curves being less than a half a percentage point. If anything, the labor force participation rates of women with economic rights are lower than those of women without rights.[51] The left panel of Figure 3 shows the density of the number of children ever born, for women age 45-59 in 1900-1910, as a function of whether or not they were married with economic rights. The right panel repeats this exercise for surviving children. The distribution of both measures of completed fertility are skewed to the left for those married with economic rights, suggesting that these women indeed had fewer kids. Taken together, these exercises suggest that fertility is lower for women with economic rights, as will be analyzed in detail in Section 5.1, while labor force participation rates are virtually the same, as will be shown in detail in Appendix F.

Table 1 shows summary statistics of variables of interest and main control variables for the analysis of fertility and labor force participation rates in our event-study analyses described below. Panel A reports summary statistics for our fertility analyses, where the sample is restricted to the wife

classified as "Managers, officials, and proprietors" (code 290 in Occ1950). In the 1% and 10% samples, this occupation accounts for about 0.11% of married women. Following advice on the IPUMs website, we recode these women as being out of the labor force. See https://forum.ipums.org/t/problem-with-occ1950-coding-in-1880-100-sample/3466 for more.

[51] This is the opposite of the point estimates suggested by the analyses in Appendix F, however those analyses find no statistically significant difference in the labor force participation rates between women with and without rights.

being age 20-39 between 1850 and 1920. It reports the mean, standard deviation, and number of observations, separately for observations in which women do and do not have economic rights, for the probability of giving birth, the number of children in the household under age 5, age of the wife, age of the husband, and year the observation is made. The probability of a birth last year and the number of children under age 5 are substantially lower when women have rights. Both husband and wife are slightly older in the sample where women have rights. Consistent with the notion, described above, that women's rights were never revoked once granted, the sample where women have rights is from a later period, on average, than when women do not have rights. This motivates our heavy use of interactions between control variables and year fixed effects, described below.

Panel B of Table 1 repeats Panel A, and includes women's labor force participation rates, when women are 20-39. Panel C does so for women age 45-59. For both groups, the labor force participation rate is 3- 4%. As in Panel A, people are generally slightly older (except women in Panel C), and the time period slightly later, in the sample with economic rights. Recall that these exercises use data from 1860-1920, and as such the average observation is slightly later than that of Panel A.

Table 2 shows summary statistics of variables of interest and main control variables for the analysis of fertility and labor force participation rates in our analyses comparing couples married before and after economic rights were granted. Panel A shows the probability of giving birth, the number of children under age 5, and labor force participation rates of women, on the sample where the wife is age 20-39. As can be seen, the probability of giving birth is higher (0.21) when women do not have rights than when they do have rights (0.18). Similarly, there are fewer children under age 5 at home with rights (1.08) than without rights (1.23). Women's labor force participation rates are slightly lower with rights than without rights. In this sample, the average age of the wife is 29-30, while the husband is about 34, with no difference between these numbers comparing couples married before and after rights were granted. The data are from the full US census in 1900 and 1910, and thus the average year of observation is in between.

Panel B of Table 2 shows the number of children ever born to the wife of the household, number of surviving children, and labor force participation rates, for the sample of women age 45-59. For couples married prior to rights being granted, the number of children ever born (surviving children) is 6.01 (4.69), while for those married after rights it is 4.29 (3.39) children. Women's labor force participation rates are slightly higher for those married after rights are granted. Those married after rights are about 1.5-2.5 years younger than those married before rights. As before, the data are from the full US census in 1900 and 1910, and thus the average year of observation is in between.

Turning towards education, the top panel of Figure 4 shows school attendance rates, over time, by gender. In 1850, about two-thirds of children were in school. This rate declines after the Civil War to slightly less than 60%, and then begin to rise again, sharply so after 1900, and reaches over 80% by the end of the sample. We note a cross-over in the data: prior to 1900, male children were more likely to be in school than female children. After 1900, this difference switches. The combination of trends in education over time and a cross-over between sons and daughters being more likely to be in school motivates the usage of gender-year fixed effects when studying schooling rates. The bottom left panel of Figure 4 shows the probability of a child being in school, by age, when women do or do not have rights. The bottom right panel repeats the bottom left panel, net of gender-year fixed effects. We note that economic rights are associated with more children in school, but that the effect is larger among younger children. Indeed, these summary statistics cannot reject the hypothesis that older children are unaffected by women's economic rights. These results motivate the analysis in Section 5.2.

Table 3 shows summary statistics of variables of interest and main control variables for the analysis of education of children in our event-study analyses described below. The first three columns report summary statistics when the sample is all children age 8-17. The next three do so for the sample of children age 8-13 while the final three columns do so for the sample of children age 14-17. We report the average propensity to be in school for all children, sons, and daughters. We additionally report the average age of their mother and father, as well as the average sample year.

About 78% (65%) [82%] of all children (before rights) [after rights] age 8-17 are in school, with these numbers very similar for boys, 78% (66%)[82%], and girls, 78% (64%)[83%]. The age of mothers (fathers) is about 39 (44), and unchanged between the sample with and without women's rights. The sample with women's rights is, on average, from after 1900 while the sample without rights is from before 1870. Again, this motivates our interaction of control variables with year fixed effects. Turning towards younger kids (8-13), 84%(68%)[89%] are in school, with no difference between boys and girls. Their mothers (fathers) are slightly younger at 37(43) years old. For older children (14-17), 66%(58%)[68%] are in school. For boys, these numbers are 66%(61%)[68%], while they are slightly different for girls 66%(56%)[69%]. The differences between boys and girls can be attributed to the "crossing over" of education rates by gender over time discussed above.

Table 4 shows summary statistics for the analysis of education of children when comparing those with parents married after rights were granted rather than before. 81%(75%)[82%] of all children (before rights)[after rights] age 8-17 were in school in our sample. These numbers are very similar for boys and girls. Mothers (fathers) were about 39 (44) years old, with those married before rights about 3 years older than those married after rights. For children ages 8-13, 87%(80%)[89%] were in school, with the numbers virtually identical between boys and girls. Their mothers (fathers) were on average 38(43) years old, with those married before rights about two years older than those married after rights. For children ages 14-17, 68%(66%)[69%] were in school, with somewhat more girls in school than boys. Their parents were about 4 years older than parents of younger children.

We next turn to the details of our empirical approaches.

4.3 Empirical Approach 1: Event-Study

In this subsection, we first describe the structure of the regressions we estimate in our event studies. We then discuss the data constructed on county-border pairs that we exploit in these event studies. We then turn to a discussion of the conditions under which our results can be interpreted as causal, as well as an empirical analysis of these conditions. Finally, we outline our robustness analyses, including the alternative estimation procedure outlined in Thakral and Tô (2020).

When performing our event studies, we estimate regressions of the following form:

$$Y_{hsct} = \sum_k \alpha_k \cdot rights^k_{st} + \beta_{c,b(c)} + \gamma_{c,b(c)} + \lambda_s + \lambda_t + X'_{hsct}\delta + \epsilon_{hsct}, \tag{1}$$

where Y_{hsct} is our outcome variable of interest listed above, such as whether or not a woman in household h gave birth in the previous year or a child was in school, in state s, county c, and year t, $t \in \{1850, 1860, \ldots, 1920\}$, $rights^k_{st}$ is a series of dummy variables set equal to one if a state had granted rights k years ago, where $k \in \{\leq -30, -20, -10, 0, 10, 20, \geq 30\}$, $\beta_{c,b(c)}$ are fixed effects for each county c and its border pair $b(c)$, $\gamma_{c,b(c)}$ are linear time-trends for each county-border pair, λ_s and λ_t are state and year fixed effects, respectively, and X'_{hsct} contain controls variables, such as age, that depend on the specific exercise being performed.[52] Standard errors are double-clustered at the state and county-border pair level, as elaborated upon below. In appendix H, we provide illustrations of our benchmark results in each exercise.

Notice that we use increments of 10 in k for the variables $rights^k_{st}$, as our data are dependent on the decennial census. We therefore have to take a stand on how to round a state's granting of women's rights to the decennial census year. For example, New Jersey gave rights in 1874. When is the first decennial census year in which we assume New Jersey granted women rights? We "round up" to the next decade, as in Geddes and Lueck (2002), Fernández (2014), and Hazan et al. (2019). Accordingly, New Jersey is coded as having granted rights in 1880. The advantage of rounding up is that it guarantees that we never treat a state as having rights when it did not. Thus, the dummy variable $rights^0_{st}$ takes the value of one for New Jersey in 1880, while the dummy variable $rights^{20}_{st}$ takes the value of one for New Jersey in 1900.

We now turn to the construction of county-border pairs, which is detailed more fully in Appendix B. We compare households in two adjacent counties on either side of a state border. The data on the evolution of US historical county boundaries comes from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System (NHGIS).[53] The construction of these border-pairs raises some issues along the way.

[52] Sun and Abraham (2021) argue that event-study specifications with linear time trends tend to be underidentified. We note that this critique does not apply to our approach, as the linear time trend is on a county-border pair, while the event study examines only the part of the pair in which women receive economic rights.

[53] These data are available at http://www.nhgis.com Manson et al. (2019). Although there are other projects featuring US historical boundaries and spatial data within a Geographic Information Systems (GIS) framework, we use the NHGIS border definitions, as they provide a better fit for mapping US federal census data from IPUMS.

The first issue is that county borders were themselves ever changing. Imagine a county A in state 1 bordering another county B in state 2. If the county A splits into two counties, then in order for our exercise to remain consistent, we must treat the two new counties formed from county A as being one county, and keep track of such changes over time. This is a painstaking process that allows for a consistent dataset, as described in Appendix B, where we also include an example of the evolution of the border between Indiana and Illinois (Figure A.10). Similarly, as the U.S. spread westward over the 19th century, more states (and thus, state borders) developed.[54] Maps showing our data on borders over time can be seen in Appendix Figures A.2-A.9.

The second issue is, what if county A has more than one bordering county, potentially even in more than one bordering state? To address this issue, we replicate each observation in county A according to the number of counties it borders. Each observation is set to a different pairing with a neighboring county.[55] Econometrically, this approach raises two issues. The first is that duplicated observations could bias estimates.[56] Accordingly, when we duplicate an observation n times, we reweight each observation to have a weight of $1/n$.[57] The second issue is that, by replicating observations between county-border pairs, we are artificially introducing a correlation in the error terms between two clusters of counties. Thus, we double cluster at the state and county-border pair level.[58]

We next turn to the question of whether our results from these event studies can be interpreted as causal. There are a number of issues at hand. The first is whether the parallel trends assumption of the event study is satisfied. The second is whether a state granting women rights is plausibly exogenous for these exercises. The third issue is omitted variable bias, or whether there are some other, contemporaneous and unmeasured changes driving our results, such as other law changes. The final issue is whether women's rights affected marriage itself, and thus our sample.

The best metric we have for the parallel trends assumption is to show the evolution of the estimates on $rights_{st}^{k}$ prior to the granting of rights. In all of our event studies, we omit the dummy variable for 10 years prior to rights being granted, and then show that there is no economically meaningful and statistically significant trend in our outcome variables (fertility, LFP, and education) prior to rights being granted, as measured by $rights_{st}^{k}$, when k measures at least 30 years prior to rights, and 20 years prior to rights. That is, for the three decades prior to rights being granted, there is no difference between our treatment and control groups, as we document in each event-study described below.

[54] Vandenbroucke (2008) analyzes the westward expansion, and finds that it was largely induced by decreasing transportation costs. Population growth induced investment in local productive land (prairie clearing).

[55] This methodology of replicating observations for each county-border pair is as in Dube et al. (2010).

[56] For instance, if Emily, age 30, in county A is duplicated in order to be compared with two neighboring counties, then her effect on any fixed effect for the age "30" will be doubled.

[57] Otherwise, all weights are 1 when examining fertility or LFP in these analyses, as we use the full count of the U.S. census. When examining education, the census weights from the 1900 5% sample are not all 1. In this case, when we reweight, we set the weight equal to the person weight/n.

[58] Dube et al. (2010) also double-cluster the standard errors, following the methodology in Cameron et al. (2011), as we do in this paper.

We next turn to the question of whether a state granting women rights is plausibly exogenous. This question actually contains two questions, with the first being did states grant women economic rights because of changing fertility rates or education rates, rather than vice versa? The historical record, discussed above, seems to suggest not.[59] However, we also note that, if states granted women rights in order to drive the results we find, then our exercises could be interpreted as measuring their success. Furthermore, the historical record, discussed above, shows clearly that states granted rights, which were then overturned by the courts, sometimes due to technicalities that the legislature did not foresee, until they were passed again. It is highly implausible to believe that the final timing of women's rights in a state was endogenous. Even if one believes that granting women rights was endogenous, as long as the change in the law was plausibly exogenous to a county on that state's border, our analysis still captures the causal effects of women's rights. To see this point, consider a county on the border between Ohio and Pennsylvania. This county does not contain Columbus, the capital of Ohio, or Cleveland, Akron, Toledo, or Cincinnati, though it may contain Youngstown. It is plausible to believe that the Ohio state legislature passes laws without taking this county into account, making state law changes plausibly exogenous to counties on the periphery of a state. This argument is potentially invalid if there is little heterogeneity within states. That is, if all the counties of a state are very similar to one another, then state policy is not exogenous to individual counties, as there is no disagreement between counties within the state. In Appendix D we show that this is not the case. Specifically, in every year, we compute the average fertility, education, and labor force participation rates for each county in the US. We then regress these averages on state fixed effects, and report the R^2 and adjusted R^2. In all exercises, these numbers turn out to be low, suggesting that the heterogeneity between counties is not explained by state.[60]

The third question is omitted variable bias, in particular in the form of other legal changes happening contemporaneously. We are unaware of any legal changes occurring contemporaneously that might have affected fertility and education decisions, with the exception of child labor laws and mandatory schooling laws. We control for these laws, as described below. However, we also perform randomization exercises that show the dates women were granted rights were unlikely to randomly produce the results we document here. In particular, for each state we pick a random year between 1850 and 1920, and assume that women were granted rights in that year. We rerun our estimates using these fake dates of women's rights, and redo this exercise 1,000 times. We show that the estimates are centered at 0, implying that it is unlikely that our estimators are biased. Additionally, we will use this approach to show below that very few of these estimates using random dates find effects larger than those we document with the actual dates. As such, we conclude that it is highly likely that the years in which women granted rights contain actual infor-

[59] As discussed above, much of the reasoning behind granting women rights seemed to have been to protect women against delinquent husbands.

[60] Indeed, this logic prevails in the minimum wage literature: while a change in the federal minimum wage is presumably endogenous to national economic conditions, it is plausibly exogenous to any particular state (Baskaya and Rubinstein, 2012).

mation. As discussed below, we report the results of these randomization exercises in Appendix E.

The final question is whether these legal changes affected the marriage market. In Appendix C, we show that these rights did not affect the marriage market. In particular, they did not affect the propensity of people to marry, the age of marriage, or the age gap between husband and wife. Below, in Section 4.4, we show evidence that people did not change the timing of their marriage in order to marry before or after rights were granted. As discussed above, we do not have any measures of marital sorting.

When performing our event study exercises, we always include three robustness tests. The first is to drop any county that is on the border between a southern state and a non-southern state.[61] We do so to allow for the fact that the south of the US may have evolved differently over time, especially in light of the destruction faced in the civil war. The second robustness test is to drop counties on the border between community property states and neighboring states. This is to allow for the fact that community property states may have had a different response to women's rights.

The third robustness test addresses issues with difference in difference estimators with two-way fixed effects, of the sort analyzed in this paper. As described above, a recent literature (de Chaisemartin and D'Haultféuille, 2020; Sun and Abraham, 2021; Goodman-Bacon, 2021; Gardner, 2021) has documented econometric issues with these sorts of analyses. As a robustness analysis, when performing our event studies, we also employ a two-step estimator of the sort analyzed in Thakral and Tô (2020), who generalize an approach introduced by Gardner (2021). Our results are qualitatively and quantitatively very similar, and thus we conclude that our benchmark event study analysis is appropriate.

The main issue is that, if the treatment effect is heterogeneous across groups and time, then the classic event study estimator, described above, will not necessarily capture an average treatment effect. Furthermore, it is also possible that, if the effects are heterogeneous but assumed to be homogenous, then estimates of some fixed effects will be biased.[62] Additionally, the use of a treated state as a control group can be problematic if the effects are dynamic.[63] The approach outlined in Thakral and Tô (2020) and Gardner (2021) addresses these concerns by taking a two-step approach to the analysis that is valid under the parallel trends assumptions. The first stage estimates all coefficients, except for the event-study coefficients, on all non-treated data. Specifically, the first stage estimates regressions of the following form:

[61] To be clear, we leave borders between southern states.

[62] For example, if the effects of women's rights are stronger in later time periods, but the estimated impact of women's rights is assumed to be constant, then year fixed effects in later time periods will be biased, as we assume a smaller treatment effect than actually occurs.

[63] For example, consider the event of Pennsylvania granting women rights, and using Ohio, which had granted rights approximately 10 years prior, as a control. If Ohio was still realizing the effects of women's rights, then the estimated impact of women's rights in Pennsylvania would be biased downward, a point made forcefully by Goodman-Bacon (2021).

$$Y_{hsct} = \beta_{c,b(c)} + \lambda_s + \lambda_t + X'_{hsct}\delta + \nu_{hsct}, \tag{2}$$

where all variables are as described above, but the sample is restricted to only include observations for people living in states that have not yet given women rights.[64] λ_t are year fixed effects.[65] Thus, the estimates of these parameters are not contaminated by the effects of women's rights, as discussed above. Many of the regressions we estimate in our benchmark models include interactions between controls, such as age of the wife in the household, and year fixed effects. When doing these two-step exercises, we do not interact any of our controls with year fixed effects, since we are estimating our data on observations without women's rights, and almost every state had granted rights by the later years of our sample. Finally, we note that since Massachusetts gave women rights before our time period began (1846), we cannot include observations in Massachusetts, or states on her border, when doing these exercises.

In the second step, we estimate regressions of the following form on all data:

$$Y_{hsct} = \sum_k \alpha_k \cdot rights^k_{st} + \hat{\beta_{c,b(c)}} + \hat{\gamma_{c,b(c)}} + \hat{\lambda}_s + X'_{hsct}\hat{\delta} + \epsilon_{hsct}, \tag{3}$$

where all variables are as described above, and parameters $\hat{\beta_{c,b(c)}}, \hat{\gamma_{c,b(c)}}, \hat{\lambda}_s, \hat{\delta}$ are as estimated in Equation (2). Under the parallel trends assumption, this estimator is unbiased (for more, see Thakral and Tô, 2020; Gardner, 2021). We block-bootstrap standard errors as described in Appendix G. As will be shown below, the estimates using this alternative strategy are very similar both qualitatively and quantitatively to those using a standard event-study approach. We thus conclude that our basic difference-in-difference event-study approach is a reasonable benchmark exercise.

As a final note, when performing these event studies on the education of children, in some specifications, we add interactions of $rights^k_{st}$ with dummy variables indicating whether the child is female. This allows for us to capture the potentially different dynamic effect of women's economic rights on the education of daughters rather than sons.

4.4 Empirical Approach 2: Couples Married Before vs After Rights

In this subsection, we first describe the structure of regressions we estimate in our analyses comparing households married before and after rights were granted. We then discuss the conditions under which our results can be interpreted as causal.

[64] Notice that, for these exercises, we do not include county border pair linear time trends. This is due to the fact that each side of the border got rights at different times, potentially yielding one side affecting the linear trend component. For example, consider a county pair on the border between Ohio and Pennsylvania. Ohio gave rights prior to Pennsylvania. Since we only look at observations without rights, only the side of the border in Pennsylvania will be the one used to identify the linear time trends for the county pair, specifically when that side of the border does not have rights, which would bias the estimates of the linear trends. Thus, we do not include the linear trends.

[65] As noted above, our benchmark exercises interact many of our control variables with year fixed effects. Since we do not do so here, as explained below, we include year fixed effects as a control.

$$Y_{hsct} = \alpha \cdot MarriedRights_{hsct} + \beta_{c,t} + X'_{hsct}\delta + \epsilon_{hsct}, \tag{4}$$

where Y_{hsct} is our outcome variable of interest listed above, such as whether or not a woman gave birth in the previous year, how many children the woman has had, or whether she works, in state s, county c, and year t, $t \in \{1900, 1910\}$, $MarriedRights_{hsct}$ is a dummy variables set equal to one if household h married after rights were granted in state s, $\beta_{c,t}$ are fixed effects for each county-year, and X'_{hsct} contain controls variables, such as age, that depend on the specific exercise being performed. Notice that we use all counties in a state, rather than just those at the state border. Standard errors are clustered at the state level.

The assumption necessary for a causal interpretation of the results documented with this approach is that selection into marriage did not change due to economic rights, and that people did not strategically time their decision to get married around the date that women's rights were granted. In Appendix C we argue that it is indeed the case that selection into marriage was not affected by women's rights. In Figure A.1, we provide evidence that couples did not time their marriage around the granting of women's rights. In particular, the top-left panel shows the fraction of people getting married relative to the year their state gave rights in the 1900 US census, when limited to white non-Hispanic couples where the wife is 20-39 years old. The top-right panel does the same for couples where the wife is 45-59. The bottom-left and bottom-right panels repeat this pattern using the 1910 census. In all cases, except for couples where the wife is 20-39 years old in 1910, there is clearly no break in the data around the year a state gave rights, nor is there any bunching behavior. The 1910 data for couples where the wife is 20-39 in 1910 is noisy, and thus harder to interpret. This is due to the small sample of states that gave rights in the relevant time frame.[66]

We perform the same robustness tests as in the event-study design. Specifically, we perform an event study where we drop the south, and another where we drop community property states. Additionally, we perform the randomization exercises described above.

Given that we have two separate identification strategies, that both are likely capturing the causal effects of women's rights, and the estimated impact of women's rights are similar between the two sets of results (as discussed below), we conclude that it is highly likely that our empirical approach is capturing the causal impact of women's economic rights on fertility and education.

5 Results

In this section we describe the results of our empirical exercises. We first examine the impact of women's rights on fertility, using both the event-study approach and comparing couples married before and after rights. We find that fertility declines dynamically after women are granted economic rights, and that this decline can be accounted for by couples who got married after rights

[66] Assuming that couples got married between ages 20 and 40, there were only 2 (small) states that gave rights in the relative time period prior to 1910: West Virginia (1893) and Utah (1897).

were granted. We use our event-study methodology to show that more children, of both genders, started going to school once rights were granted, and that this effect can potentially be accounted for by children born to parents married after rights were granted.

5.1 Fertility

In this section, we first examine the impact of women's rights on fertility, using both the event-study approach and comparing couples married before and after rights.

5.1.1 Fertility: Event Study Approach

We estimate regressions of the form described in Equation (1), where the dependent variable is either whether the wife gave birth in the previous year or the number of kids under age five in the household. The controls in variable X_{hsct} include fixed effects for the wife's age and the husband's age, both interacted with year fixed effects. Some specifications include "extra controls," which include fixed effects for the husband's industry and husband's occupation, both interacted with the year fixed effect.[67] These extra controls allow us to control for how a husband's career might affect family size, and how the relationship may change over time. In addition, they implicitly allow us to control for effects of income growth and the general economic transformations of this time period.

Table 5 shows the results when the dependent variable is whether the wife of a household gave birth in the previous year. Column 1 does not include our extra controls. Column 2 repeats Column 1, but adds the extra controls, and is thus our preferred specification. Column 3 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 4 also repeats Column 2, but drops counties in community property states, as well as their border-pairs.[68] Column 5 also repeats Column 2, but uses the two-step estimator.[69]

In all specifications, the point estimates prior to granting rights are quantitatively virtually zero, and have no pattern to them, suggesting no trend in fertility around the time of giving rights, a point we return to below.[70] In all specifications, the impact of rights on the probability of giving birth is between -0.006 and -0.008 when rights are given, with the effect statistically significant at the 1-5% levels. One decade after rights are granted, the magnitude of the effect grows in

[67] We use IPUMs variables "ind1950" and "occ1950", respectively.

[68] Additionally, in untabulated results we perform robustness exercises on our main event-study analyses where we use cross-state variation, rather than county-border pair analyses. The results are quantitatively similar to our main findings discussed below.

[69] Recall that, for the two-step estimator, we drop Massachusetts and counties on her border. This accounts for the decrease in the number of observations. Additionally, we do not interact the controls with year fixed effects in order to economize on the computational intensity of the exercise.

[70] Additionally, the estimates are not statistically significant due to large standard errors. The only exception is Column 5, where the quantitatively meaningless estimate on 3 decades before rights is statistically significant due to a very small standard error. The standard errors in this specification are calculated via block-bootstrapping, as described in Appendix G. The standard errors calculated in this method are similar to the standard errors in other specifications after rights are granted, but smaller before rights are granted.

all specifications, with the range of estimates being between -0.010 and -0.012, with all estimates statistically significant at the 1% level. Two decades after rights are granted, the magnitude of the effect again grows in all specifications, with the range of estimates being between -0.010 and -0.013, with all estimates statistically significant at the 1% level. Three decades and more after rights are granted, the magnitude of the effect is again larger, with the range of estimates being between -0.010 and -0.016, with all estimates statistically significant at the 1% level. As Column 2 is our preferred specification, we visualize these results in Figure A.12. This figure shows the lack of a trend in fertility (in point estimates), relative to our controls, prior to rights being granted, and a sharp, dynamic decrease in fertility thereafter. Considering that the average probability of giving birth was about 0.20, corresponding to roughly 4 births over a twenty-year horizon, the magnitude of the estimates ranges from a decline of about 3-3.5% when rights are granted to a decline of 6-8% three decades after rights are granted.

We now return to the issue of trends in fertility around the time rights are granted. While the point estimates prior to rights being granted suggest no pretrend, being quantitatively small and having no pattern, the standard errors about these estimates are large in our benchmark specification. In principle, one could draw a line in Figure A.12 connecting the top of the confidence intervals prior to rights being granted through the post-rights confidence intervals, potentially suggesting that time trends can explain our results. We reject this hypothesis for a three reasons. One is that this is not true in Column 5, using the two-step estimator. This specification yields very similar point estimates to the other specifications, but small confidence intervals prior to rights being granted. Thus, a line cannot be drawn suggesting that time-trends can explain our findings, as can be seen in Figure A.13. Second, we include county-border pair linear trends in our specifications, that presumably capture such trends. Finally, and most importantly, the married-after exercise discussed below finds quantitatively very similar results and, by design, is not subject to any concerns about regional time trends, as we compare people in the same county and the same state who were married before or after rights were granted. Thus, we do not believe that time trends are a concern for this analysis.

Table 6 shows the results when the dependent variable is the number of kids under age 5. Table 6 follows the same pattern as Table 5. In all specifications, the estimates prior to granting rights are quantitatively small, follow no pattern, and statistically insignificant. This, along with the married-after exercise, supports the idea that there were no differences in trends in fertility between counties on either side of the state border prior to rights being granted, as discussed above. In all specifications, the impact of rights on the number of kids under 5 is between -0.025 and -0.029 when rights are given, with the effect statistically significant at the 10% level in Columns 1 and 2. One decade after rights are granted, the magnitude of the effect grows in all specifications, with the range of estimates being between -0.036 and -0.042, with all estimates statistically significant at the 5% level. Two decades after rights are granted, the magnitude of the effect again grows in all specifications, with the range of estimates being between -0.053 and -0.059, with all estimates statistically significant at the 1-5% level. Three decades and more after rights are granted, the magnitude of the effect is again larger, with the range of estimates being between -0.074 and -0.084, with all estimates statistically significant at the 1% level. As Column 2 is our preferred specification, we visualize the results documented here in Figure A.14. Considering that the average number of kids under five was about 1.19, the magnitude of the estimates ranges from a decline of about 2-2.5% when rights are granted to a decline of about 6.3-7.1% three decades after rights are granted. Notice that, in percentage terms, these fertility declines are remarkably consistent with those described above. We note that the estimates on the impact of rights on the number of kids under 5 is roughly five times that of the impact on the probability of giving birth, which makes these estimates consistent in magnitude. Finally, in Column 5, with the two-step estimator, finds remarkably similar point estimates to its counterpart in Column 2, with the exception of the immediate impact of rights on the number of children under 5. That estimate is -0.053, which is larger than the counterpart (-0.027) in Column 2. We note that the standard errors in this specification are remarkably similar to the standard errors in other specifications, except for estimates before rights were granted, in which case the standard errors are significantly lower.[71] Since the results of the two-step estimator are remarkably similar to their counterpart using our benchmark event-study difference-in-difference estimator (Column 2) in both tables, we conclude that the concerns raised by the literature on the traditional difference-in-difference estimator are not a major concern in this exercise.

Appendix E reports the results of our randomization exercise for this event-study analysis of the decline in fertility following women's rights. The results of that exercise suggest that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would have yielded results similar to those documented here.

We conclude that granting women economic rights led to a dynamic decrease in fertility of about 3-8% over the subsequent decades. We note that while the point estimates show an increasing magnitude of the effect of rights over time, we cannot reject the hypothesis that the effect of rights is the same two and three decades after rights are granted. One reason for this time-delayed effect of rights, discussed in more detail below, is the idea that the decline in fertility is driven mostly by people married after rights were granted. Under this hypothesis, as more time passes since rights were granted a higher fraction of the population was married after rights were granted, and the effect of rights on the aggregate grows.

5.1.2 Fertility: Couples Married Before/After Rights

We next turn to the impact of women's economic rights on fertility, as measured by comparing couples married before and after rights were granted. As such, we estimate equations along the lines of those described in Equation (4).

[71] As before, the standard errors before rights are granted in this specification are small relative to the other specifications. This helps preclude the notion of trends prior to rights being granted, as it is harder to fit a line through smaller confidence intervals. This can be seen in Figure A.15.

Table 7 shows the results when the dependent variable is whether the wife of the household gave birth last year (Panel A), or the number of children under 5 (Panel B). As in Section 5.1.1, we restrict attention to households where the wife is age 20-39, and her husband 20-50, in order to maintain comparability between the sets of results. Column 1 includes as controls fixed effects for the wife's age, the husband's age, and how long the couple has been married, all interacted with year fixed effects.[72] Column 2 adds the "extra controls," described above, which are fixed effects for the husband's occupation and industry, interacted with year fixed effects. Column 3 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 4 also repeats Column 2, but drops counties in community property states as well as their bordering counties. Columns 5 and 6 also repeat Column 2, but do so only on the sample from 1900 or 1910, respectively.[73]

Panel A shows that couples married after rights were granted, as compared to those married before rights, had a lower probability of giving birth in a given year of 0.009-0.011 in Columns 1-5. These estimates are statistically significant at the 1% level in Columns 1, 2, 4 and 5, and not statistically significant in Column 3. The reduced significance in Column 3 is presumably due to smaller sample sizes. We note that the point estimates are virtually identical in these specifications. In Column 6, using only the 1910 sample, the point estimate is only -0.004, and it is not statistically significant. This is presumably due to the small number of states that gave rights in the 20 years prior to the 1910 sample, which could be used to identify the effect of being married with rights.[74]

We note that the results so far are consistent with those found using the event study approach above. The probability of giving birth is estimated to decline by 0.009-0.011 using this approach, which is basically the same as the impact of women's rights on the probability of giving birth in the event study a decade after rights were granted. Thus, it is possible that these two exercises are picking up the same effect, and that most of the decline in fertility rates after women's rights comes from those couples who got married after rights were granted. Under this view, the reason that the event-study approach has an increasing dynamic effect of women's rights on the probability of giving birth is that the stock of married couples is dynamically changing to include more people married after rights were granted over time.

Panel B shows that couples married after rights were granted, as compared to those married before rights, had 0.138-0.169 fewer kids at home under age 5 in a given year in Columns 1-5. These

[72] While being married after rights is perfectly determined by the duration of a marriage within a given state, this is not true across states. For example, two couples married in 1890 in Utah (which gave rights in 1897) and in South Carolina (which gave rights in 1887), will have the same duration of marriage at any given year, despite being married before and after rights, respectively. The inclusion of many such couples from states which granted rights at different times allows for separate identification of marriage duration and married-after-rights status.

[73] In untabulated results, we also perform exercises with county-border pairs. Specifically, we restrict our sample to people living in counties bordering counties in other states, and compare people married before and after rights in the joint set of counties. The results are qualitatively similar to our main findings discussed below.

[74] To see this point, assume that people marry in their 20s. As such, when looking at couples age 20-40 in 1910, only states that gave rights between 1890 and 1909 could be used to identify the effect of being married after rights were granted. This means only West Virginia (1893) and Utah (1897). In contrast, a similar thought experiment for the 1900 sample would add Alabama (1887), South Carolina (1887), Montana (1887), Vermont (1888), and Washington (1889).

results are statistically significant in Columns 1,2, 4, and 5 at the 1% level. While they are not statistically significant in Column 3, the point estimate is very similar to the other specifications. As before, the effect is a bit weaker in Column 6, with the point estimate -0.117, and this estimate is also statistically significant at the 1% level.

We note that the results in Panel B are quantitatively larger than those implied by Panel A. That is, if the probability of giving birth declines by 1 percentage point, then we'd expect the number of kids under age 5 to decline by about 0.05, whereas the estimates here are 2-3 times larger.[75] Similarly, we also note that the results here are quantitatively larger than those documented in the event-study approach. This fact further reinforces the idea that declines in fertility are being driven by couples married after rights were granted.

Table 8 shows the results when the dependent variable is completed fertility, which thus necessitates changing the sample to households where the wife is age 45-59.[76] Panel A of Table 8 has the dependent variable be the number of children ever born (CEB) to the wife of the household, while Panel B has the dependent variable be the number of surviving children the wife has. As before, Column 1 includes as controls fixed effects for the wife's age, the husbands age, and how long the couple has been married, all interacted with year fixed effects. Column 2 adds the "extra controls," described above, which are fixed effects for the husband's occupation and industry, interacted with year fixed effects. Here, Column 3 repeats Column 2, but on a sample of women who have ever had a child ($CEB > 0$). Column 4 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 5 also repeats Column 2, but drops counties in community property states as well as their bordering counties. Columns 6 and 7 also repeat Column 2, but do so only on the sample from 1900 or 1910, respectively.

Beginning with Panel A, in all specifications, the number of children ever born decreases by 0.204-0.239 children, and is statistically significant at the 5% level in all specifications, except for Columns 4 and 7 where it is significant at the 10% level. This is roughly twenty times the estimate of the impact of being twenty years after rights on the probability of giving birth, suggesting that these estimates are compatible (a reduction in the probability of giving birth by 0.010 for 20 years reduces fertility by 0.20 children). We also note that the estimated impact in Column 3, which is similar to other specifications, where we restrict attention to mothers, suggests that most of the impact of rights comes from a decline in the intensive margin of fertility, rather than extensive margin.

Infant and child mortality were relatively high during most of our sample time period and only start to decline in the late 19th century or early 20th century (Haines, 1998). As such CEB doesn't necessarily reflect the demand for children (Doepke, 2005). Parents could replace an infant or a child who passed away with another birth. Thus, in Panel B we replace CEB with the number of

[75] One potential explanation for the difference is that couples married after rights might also time their fertility differently. In untabulated results, we find that the decline in the number of children under 5 is much larger for younger couples (where the wife is under 30) than older couples.

[76] As noted in Section 4.1, data on completed fertility are only available in 1900 and 1910.

surviving children as a better measure of the demand for children. In all specifications in Panel B of Table 8, the number of surviving children decreases by 0.129-0.191 children, and is statistically significant at the 5% level in Columns 1, 2, 3, 5, and 6, the 10% level in Column 7, and not significant in Column 4. This furthers the notion that demand for children decreased following women's economic rights.

Appendix E reports the results of our randomization exercise for these analyses of the decline in fertility following women's rights. The results of the exercises suggest that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would have yielded results similar to those documented here.

To further explore the decline in fertility, we repeat the above exercise 16 times where the dependent variable is whether or not the household has a given parity, as measured by children ever born, from 0 to 15 children. Table 9 reports the results. As can be seen, the probability that a household has 1-6 children increased, while the probability that a household had 7-15 children decreased. The increase (decrease) is particularly large and statistically significant for parities of 2, 3, and 4 (8, 9, 10, 11, 12, and 13). Interestingly, the probability of a household being childless (parity of 0) decreased by 1.2 percentage points.[77] Figure A.18 visualizes these results. Thus we find that households decreased their fertility along the intensive margin.

We conclude that couples married after rights were granted, as compared to those married before rights were granted, had lower fertility rates, especially along the intensive margin. Additionally, this decline in fertility rates can potentially account for the decline in fertility rates documented in the event-study approach, suggesting that couples married after rights were granted drove the results documented in Section 5.1.1. Finally, we note that the probability that a married white woman 20-39 years old giving birth fell from about 24.8% to 17.4% over the course of our sample. Both approaches documented here suggest that women's rights can account for about 15% of the overall change between 1850 and 1920.

5.2 Education

In this section, we examine the impact of women's rights on education using the event-study approach.

[77] There are at least two reasons why childlessness may have decreased with women's rights. The first reason is that, since women's rights led to more education for children, as documented below, women may have viewed having a family at all as being more attractive. That is, the extensive and intensive margins may have moved in the opposite direction, due to the change in the propensity to educate children. This mechanism is very similar to that described in Aaronson et al. (2014).

The second is that it is possible that women lost a lot of bargaining power with childbirth. The laws of coverture gave men virtually unlimited power over the children in the household, such that a woman might be afraid that her husband would take her children away from her. Granting women bargaining power through property rights presumably increased her ability to negotiate with her husband over the fate of her children.

5.2.1 Education: Event Study Approach

We estimate regressions of the form described in Equation (1), where the dependent variable is whether a child is currently in school. As discussed above, in some specifications, we add interactions of $rights^{k}_{st}$ with dummy variables indicating whether the child is female. The controls in variable X_{hsct} include fixed effects for the child's age, whether the child is female, the mother's age and the fathers's age, all interacted with year fixed effects. Some specifications include "extra controls," which include fixed effects for the father's industry and occupation, the number of children in the household, whether this child was allowed to not work, whether the child was allowed to not be in school, all interacted with the year fixed effect.[78]

Table 10 reports the results of our analyses. Column 1 sets the sample to be all children age 8-17, and does not include the extra controls. Column 2 repeats Column 1, but includes the extra controls. Column 3 repeats Column 2, but adds the interactions of $rights^{k}_{st}$ with dummy variables indicating whether the child is female.

In all three specifications, there is no trend in the probability of a child being in school prior to rights being granted.[79] The point estimate on the effect of rights on education is between 4.2 and 4.3 percentage points (p.p.), and statistically significant at the 5% level in all three specifications. A decade after rights, the estimates rise to 4.8-5.2 p.p., and are statistically significant at the 5% level for all specifications. The estimates rise further to 5.3-6.0 p.p. two decades after rights, and are statistically significant at the 5% level in all three specifications. The point estimates drop somewhat to 3.4-4.6 p.p. and lose their statistical significance three decades after rights are granted. Column 3 shows no meaningful economic or statistical difference in the educational attainment of daughters following women's rights, as compared to sons. We conclude that the effect of women's rights was the same between daughters and sons.[80] Relative to an average propensity to be in school of about 74%, these estimates reflect an increase of about 5-8% in schooling following women's economic rights.

Columns 4-6 of Table 10 repeat Columns 1-3 but restrict the sample to be children ages 8-13. As before, there is no trend in the probability of a child being in school prior to rights being granted. After rights are granted, the estimates are universally larger than their counterparts in Columns 1-3. The increase in the effect is by about 0.5-0.7 p.p. when rights are granted, to as much as 4.3 p.p. three decades after rights are granted. Additionally, all estimates in Columns 4-6 are at least somewhat more statistically significant than their counterparts in Columns 1-3, with the most dramatic effect seen in the estimates three decades after rights are given. While in Columns

[78]The data on whether a child of a given age was allowed to work or allowed to not be in school comes from Clay et al. (2016). These variables are only available starting from 1880. Before that, we assume no law was in effect restricting children. For more on the impact these laws had on children, see Shanan (2021).

[79]If anything, the point estimates are positive, indicating that the probability of being in school slightly dropped prior to rights being granted.

[80]In untabulated results, we also estimate Column 2 separately for sons and daughters. The estimated impact of rights on education is virtually the same by gender of the child.

1-3, these estimates are not statistically significant, in Columns 4-6 they are significant at the 1-5% levels. As before, there is no differential impact of women's rights on the education of daughters as opposed to sons (Column 6). Figure A.16 visualizes the results of Column 5's event study on younger children.[81]

Columns 7-9 of Table 10 again repeat Columns 1-3, but restrict the sample to children ages 14-17. As before, there is no trend in the probability of a child being in school prior to rights being granted. Here, the estimates are universally smaller and less statistically significant than their counterparts in Columns 1-3. The immediate impact of women's rights on the education of older children is 3.0-3.4 p.p. when rights are granted, but this estimate is only statistically significant at the 10% level in Columns 7 and 9, and the 5% level in Column 8. Ten years after rights are granted, the point estimates are quantitatively the same and statistically significant at the 10% level in Column 8, but not significant in Columns 7 and 9. The remaining estimates are not statistically significant. As before, we find no differential impact of women's rights on the education of daughters as opposed to sons (Column 9). One may wonder why invest in a daughter's education given the low married women's labor force participation rates. Behrman et al. (1999) argue that a mother's education is an important input into the education of children. Educating a daughter not only directly affects the education of grandchildren, but also increases the daughter's marriage market prospects.

Appendix E reports the results of our randomization exercise for this event-study analysis of the rise in education following women's rights. The results of that exercise suggest that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would have yielded results similar to those documented here.

Table 11 performs our robustness analysis for this exercise.[82] Column 1 of Table 11 repeats Column 3 of Table 10 but removes from the sample observations in counties on the border between Southern and non-Southern states. Column 2 repeats this but instead drops observations on the border between community property states and other states. Columns 3 and 4 repeat Columns 1 and 2 on the sample of children age 8-13, while Columns 5 and 6 do so on the sample of children age 14-17. Quantitatively and qualitatively, the results are very similar to those reported in Table 10, with a large, dynamic, and statistically significant increase in education for all children after rights are granted. This increase is stronger for children 8-13 than for those 14-17, and there is no differential impact of rights on girls rather than boys. The robustness exercises using the two-step estimator are more nuanced when studying education rather than fertility, and as such are relegated to Appendix G, however the results are robust to using this alternative framework.

[81] As can be seen in this figure, it is not clear how much education there was after rights were granted relative to 20 or 30 years prior to women's rights due to noisy estimates. As such, we also repeat this event study where we compare the effect of women's rights relative to all years prior to rights being granted, rather than by decade prior to rights being granted. Figure A.17 visualizes these results. As can be seen, the basic qualitative and quantitative results are strikingly similar when doing the event study in this manner.

[82] As before, we also perform (in untabulated results) robustness checks using cross-state variation in the timing of rights, rather than county-border pairs.

We conclude that women's rights led to a dynamic increase in the educational attainment of children, with the effect concentrated on younger (8-13 year old) rather than older (14-17 year old) children. Importantly, we find no evidence that women's economic rights led to any differential impact on the education of daughters rather than sons.[83]

5.2.2 Education: Parents Married Before/After Rights

We next turn to the impact of women's economic rights on education, as measured by comparing children of couples married before and after rights were granted. As such, we estimate equations along the lines of those described in Equation (4), using the controls described above in Section 5.2.1.

Table 12 presents the results of these estimations. We begin with Panel A, which tabulates the regression estimates when using data from both 1900 and 1910. Column 1 includes all of the control variables, interacted with year, on the sample of children ages 8-17. Column 2 repeats Column 1, but includes an interaction between the parents of the child being married after rights were granted and the child being female. This allows us to capture potentially differential effects on sons versus daughters. We find that children born to parents who married after rights were granted were 0.9-1.0 percentage points more likely to be in school than those whose parents married before rights, with the estimates significant at the 10% level in both specifications. Columns 3 and 4 repeat this pattern for children ages 8-13. The point estimates suggest that these children were 0.3-0.5 percentage points more likely to be in school if their parents were married after rights were granted; however, the estimates are not statistically significant. Columns 5 and 6 again repeat this pattern, but for children 14-17. We find that children of parents married after rights were granted were 1.9-2.2 percentage points more likely to be in school, with the estimates significant at the 1-5% levels. As in Section 5.2.1, we find no evidence that the impact of women's rights on daughters was different that that on sons.[84]

Panel B repeats Panel A, but only uses data from 1910. The estimates in Columns 1-4 are remarkably similar to their counterparts in Panel A, except here they are all statistically significant at the 1-5% levels. The estimates in Columns 5 and 6 are somewhat larger than than their counterparts in Panel A, standing at 2.4 percentage points, and are statistically significant at the 1% level. As before, there is no evidence that rights had a differential impact on daughters.

Panel C repeats Panel A, but only uses data from 1900. The point estimates are quantitatively very similar to those in Panel A, but the statistical significance is greatly reduced. Indeed, no estimate in Columns 1-4 or Column 6 is significant. The estimate is significant at the 5% level in Column 5.

[83] Geddes et al. (2012) find a differential effect of women's rights on older daughter's education. Their approach relies on a triple-difference estimator comparing not only daughters to sons, but older children to younger children. As such, are not directly comparable with our own.

[84] As before, in untabulated results, we also perform exercises with county-border pairs. The results are qualitatively similar to our main findings discussed below.

It is no surprise, however, that the estimates from 1910 are more statistically significant than those from 1900, as the 1900 data is from the 5% sample while the 1910 data is from the full sample.

This analysis is mostly focused on 1910, by which time most younger children were already in school. As such, it is not surprising that the main effect would be on older children. We also note that the magnitude of the effect documented here on older children is similar to that in Section 5.2.1. That is, the estimate of the effect of parents being married after women's rights on the probability of a child age 14-17 being in school is within a standard error of the estimated impact of women's rights on older children being in school within 2 decades.

Finally, Appendix E reports the results of our randomization exercise for this analysis of the rise in education following women's rights. The results of that exercise suggest that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would have yielded results similar to those documented here.

As such, we conclude that it is plausible that the increase in the education of children following women's rights was driven by children whose parents were married after rights were granted.

6 Discussion: Mechanisms

In this section, we argue that granting married women economic rights caused a shift in household bargaining power from husband to wife that can account for the decline in fertility and increase in education of children that we document above. Roughly speaking, we make two general arguments to this effect.

The first general argument is that shifting household bargaining power is a plausible economic mechanism, as discussed in Section 6.1. This is due to five observations that we make. First of all, the historical archives suggest that lawmakers were concerned about shifting household power. Secondly, our results can be accounted for by couples married after rights were granted, rather than all couples. This is important, as rights were not granted retroactively, and as such we would expect to find a bargaining impact on those married after rights were granted rather than households married before rights were granted. Third, we provide evidence consistent with husbands and wives preferring different levels of fertility in this time period due to maternal mortality risk, providing the underlying source of marital disagreement. Fourth, we find that households with greater wealth changed their fertility by more than households with lower wealth, consistent with changes in property rights affecting bargaining in households with property. Finally, our results are consistent with a wide literature that suggests that shifting household power causes changes in fertility and education such as those we document.

The second general argument that bargaining power can account for our results is to negate other potential mechanisms, as discussed in Section 6.2. In particular, we negate the idea that women's economic rights led to women working more, and thus through an increased opportunity cost of their time, a decline in their fertility. We also negate the notion that general equilibrium effects can cause the documented results, and relate our mechanism to that proposed by Doepke and Tertilt (2009).

6.1 Bargaining Power is a Plausible Mechanism

We now elaborate on the five observations mentioned above to contend that bargaining power is a plausible mechanism to explain the results we find and then discuss how exactly women's rights could affect bargaining. As noted above in Section 3.3 the economics and history literatures are united in making explicit that men viewed a loss of bargaining power at home as the main downside of granting women rights. Griffin (2003) in particular makes clear that British members of Parliament (MPs), all of whom were men, were hesitant to give up their own rights at home. Similarly, we read the debate in the British Parliament on granting women property rights. The debate included fascinating discussions about defending indigent women against drunk husbands, for example, or the potentially ill effects of women's rights on the "harmony" of previously male-dominated households. Holcombe (1983) also discusses the history of women's property rights in England in the context of defending families against male-inflicted poverty. Stanley (1988) discusses similar motives in state legislatures in the U.S. Thus, our first observation is that the historical literature supports the notion that people believed women's rights would impact household decision making.

The second observation is to point out that our results can be accounted for by couples married after rights were granted, rather than all couples. We discuss these points empirically in Sections 5.1.2 and 5.2.2. The conclusion is that rights impacted those who were married after rights were granted more than those who were married before rights were granted. Since marital property rights were not granted retroactively, this strongly suggests that the mechanism by which rights affected fertility must come from a change at the household level. Bargaining power between husband and wife is an appealing story. Indeed, we have difficulty thinking of any other mechanism that would have this implication. We use this fact again below to negate other potential mechanisms.

The third observation is that maternal mortality risk could be the underlying reason for husband and wife to differ in desired fertility. Albanesi and Olivetti (2016) discuss how approximately 1 in 125 live births resulted in maternal death in 1900, while disability among mothers was even greater. They calculate that, on average, disability-adjusted life years, which takes into account both death and disability risk, was about 1.1 years per pregnancy in 1930 (and was presumably larger in our time period). It is reasonable to assume that husband and wife disagreed over their willingness to tolerate such risks in having additional children.[85] As such, a transfer in bargaining power from husband to wife would yield a decline in fertility. Presumably, this effect is

[85] As noted in Footnote 13 Ashraf et al. (2020) study developing countries in modern times and find that husbands have a much lower level of knowledge about maternal mortality and morbidity risks than their wives do. Once these men are educated on the topic, they display a reduced desire for fertility.

largest in states with the highest maternal mortality rates. Accordingly, we re-evaluate the impact of rights on fertility separately by states with relatively high and low maternal mortality risk. Albanesi and Olivetti (2014), Figure 3, shows cross-state variation in maternal mortality risk, and divides states into higher and lower than average risk. The time period used in their analysis is 1925-1934, which overlaps with the end of our sample. There is no maternal mortality risk data prior to this time period. We take their ranking to explore how women's rights affected fertility differentially by maternal mortality risk, as explained below. Before delving into these results, it is important to note that there is no correlation between the timing of a state granting rights and its maternal mortality rate, as seen in Figure 5. We note that Figure 5 does not include the 4 states that gave rights after 1920, since it is unclear how coverture was enforced after the 19th amendment was passed.

Table 13, Panel A, repeats Table 7, while Panel B repeats Table 8. However, here we include an interaction between a couple being married after rights were granted and living in a state in the top 25% of maternal mortality risk ("High MMR"). Beginning with Panel A, Column 1 recreates the benchmark specification (Column 2 from Table 7 Panel A studying the probability of giving birth) for ease of comparison. Column 2 replicates Column 1 with the interaction term. Column 3 repeats Column 2, but uses only states that granted rights prior to 1920 in order to be consistent with Figure 5. Columns 4-6 repeat this pattern for the number of kids under age 5. Panel B again repeats this pattern, but uses Table 8 as a starting point. Columns 1-3 of analyze children ever born, while columns 4-6 analyze surviving children.

In all four cases, the point estimates on the effect of being married after rights on fertility are negative, and about 70-75% the magnitude of the baseline case. This indicates that being married after rights reduced fertility in all states, according to all four measures of fertility. Additionally, in all four cases, the interaction term indicates that high maternal mortality risk states saw a decline in fertility more than twice the magnitude of other states. In particular, the highest risk states saw children ever born decline by an extra 0.5 children, above the reduction of 0.17 children other states experienced, with the estimate significant at the 1% level. Thus it seems that much of the documented impact of women's rights on fertility is coming from states where women were at the highest risk of dying in childbirth. These are exactly the states where women would use their bargaining power to reduce fertility the most.[86]

The rationale behind the bargaining power hypothesis implies that the effects of women's rights should be larger among households with greater assets. The idea is that, since property rights affect people with property, wealthier people should presumably respond more to the change in women's economic status. This is indeed what we find, and constitutes our fourth observation. In 1860 and 1870, and only these two years, the US census asked about measures of both real and personal property at the household level.[87] We deflate these nominal values to make them

[86]We also perform a similar exercise comparing the education of children in states with high versus low maternal mortality rates. The estimates are not statistically different from one another. These results are omitted for brevity.

[87]Technically, the census takers were instructed to ask the head of each household about the holdings of each in-

real, in 1870 dollars, using the deflator from Burgess (1920).[88] We then estimate regressions of the structure described in Equation (1) with two changes. The first is that we restrict the sample to be 1860 and 1870 in order to to be able to use the wealth data described here. Since we only have two years, our second change is to replace the event study design with a simple difference-in-difference estimator. Technically, this means replacing the term $\sum_k \alpha_k \cdot rights^k_{st}$ in Equation (1) with a simply binary variable $rights_{st}$ indicating whether state s had rights in year t. We then add "High Wealth", indicating whether a household was in the top 25th percentile for wealth, as well as an interaction between High Wealth and rights. As before, we use our sample of married, white, non-Hispanic women age 20-39, married to men age 20-50, who live in the same state in which they were born.

Table 14 reports the results for this exercise. Column 1 has the dependent variable of whether the woman gave birth last year. We find that women's rights are associated with a 0.8 p.p. decrease in the probability that a woman gave birth, with the estimate significant at the 10% level. This finding is remarkably similar to those reported in Section 5.1.1, suggesting that women's rights didn't have a differential impact in the 1860s and 1870s as opposed to the rest of our sample period. Column 2 repeats Column 1, but includes the "High Wealth" indicator variable as well as its interaction with women having rights. Women's rights still has a negative impact of half a percentage point, but this estimate is no longer statistically significant. High wealth households have lower fertility, but the estimate is also not statistically significant. However, wealthy households reduce their fertility by 1 percentage point when rights are granted, with the estimate significant at the 1% level. Columns 3 and 4 repeat Columns 1 and 2, with the dependent variable being the number of children under age 5. The findings are remarkably similar, and quantitatively compatible.[89] These results are consistent with the notion that women's rights affected household bargaining, as wealthier families should be most affected by changes in property rights. We also note that these wealthier families are more likely to read newspapers and be informed on the state of women's rights, as discussed above in Section 3.1.

Our fifth and final point is that it is worth noting that our results are consistent with a wide literature that suggests that shifting household power causes changes in fertility and education such as

dividual in the household. However, to the best of our knowledge, all of the literature using this data looks at household-level data, as it seems that most heads of households simply reported all assets as belonging to them. Indeed, Rosenbloom and Stutes (2008) argues that "[m]any of these individuals were part of larger households, whose assets were likely to be reported as belonging to the head of the household" (p. 148). Koudijs and Salisbury (2020), which studies the effects of protecting married women's assets from credits, also uses this data at the household level rather than breaking down the assets between husbands and wives.

[88]Furthermore, the value of real assets was to be assessed "without any deduction on account of mortgage or other incumbrance, whether within or without the census subdivision or the country. The value meant is the full market value, known or estimated" (Ruggles et al., 2020). Moveable or "personal" property included the "contemporary dollar value of all stocks, bonds, mortgages, notes, livestock, plate, jewels, and furniture" in 1870, and included the value of slaves in 1860. Moveable property of value less than $100 was not recorded in 1870. Accordingly, for consistency, we recode any observations of less than $100 in 1860, in real terms, to be $0. This approach is consistent with the one we employ in Hazan et al. (2019).

[89]That is, the decline in the number of kids under age 5 is about 5 times the estimated decrease in the probability of giving birth.

those we document. It has been widely documented in the literature that women prefer smaller families with greater investment in the education of children (Thomas, 1993; Lundberg et al., 1997; Attanasio and Lechene, 2002; Qian, 2008; Rasul, 2008; Bobonis, 2009; Doepke and Tertilt, 2019, 2018; Doepke and Kindermann, 2019). This could be due to either an evolutionary rationale, or simply that women fear childbirth due to the health risks and thus prefer fewer children as we discuss next. When women gain more power in the household, we expect to see fewer children and more investment in education. Additionally, we would not expect that this increased investment in education would be differential by the gender of child.[90] Thus, the results documented in this paper sit comfortably within those found in the wider development literature.

We next turn to the issue of how exactly women's rights affected bargaining. The classic approaches to modeling household bargaining tend to include divorce as the disagreement point in Nash bargaining (Manser and Brown, 1980; McElroy and Horney, 1981).[91] If divorce is not permitted, due to the constraints of the time, how does granting women property rights affect the disagreement point, and thus allocations? We note that the disagreement point in Nash bargaining need not be divorce, but rather what happens during disagreement between spouses. This idea dates back at least to Lundberg and Pollak (1993).[92] Prior to rights being granted, women had no power in such a situation. They could not access bank accounts, write contracts, or run businesses. With rights, a woman could withdraw money from her account, purchase merchandise downtown, and continue the marital spat on her own terms. It seems reasonable to conclude that their disagreement point improved dramatically.

Thus, we conclude that shifting bargaining power from husband to wife can indeed account for the results we document in this paper, and that maternal mortality risk is a plausible underlying mechanism for disagreement between spouses.

6.2 Other Mechanisms Don't Work

We next discuss four other potential mechanisms by which women's property rights may have given rise to changes in these household decisions, and why they do not appear to be consistent with the results documented here.

[90] Doepke and Tertilt (2009) explicitly predict, though do not empirically document, that women's rights would lead to more investment in education, exactly due to the impact of women's rights on household bargaining power. Their model also generates a decline in fertility through a quantity-quality tradeoff. Thus, while our findings identify a causal relationship from women's rights to reduction in fertility that may have led to an increase in education, our evidence does not refute a direct positive effect of rights on education as suggested by Doepke and Tertilt (2009). However, as discussed above, we document evidence that maternal mortality risk is associated with the effect of women's rights on fertility choices.

[91] This is not a universal feature of models of household bargaining. For instance, Voena (2015) studies the impact of introducing unilateral divorce laws. She models household bargaining as a Pareto problem with a participation constraint that both spouses prefer marriage to divorce when unilateral divorces are permitted. When they are not permitted, household bargaining is a Pareto problem with bargaining exogenous weights.

[92] More recent work includes Gobbi (2018), who studies a semi-cooperative model of marital decision making to understand child quality outcomes. Moreover, González and Zoabi (2021) models cooperation within households as an agreement between spouses with a within marriage outside option given by a noncooperative game while allowing for a divorce threat.

The first mechanism is that women's rights may lead women to work more (Geddes and Lueck, 2002). This would increase the opportunity cost of a mother's time, and in turn reduce fertility (Galor and Weil, 1996). A quantity-quality tradeoff would yield a rise in investment in education. This hypothesis is less consistent with the data. First of all, labor force participation rates were incredibly low during our entire period, at roughly 3-5%. Additionally, in Appendix F, we document that women's labor force participation rates were unaffected by economic rights, using both the event-study approach as well as by comparing couples married before and after rights were granted. Thus, while this mechanism is appealing at first glance, it is rejected by the data.

The second mechanism is that women's rights might increase the desire to invest in a daughter's education differentially to the impact on a son's education. This could be the case since daughters might grow up to either work or manage assets now that they have economic rights. More education would therefore be helpful. However, we find no evidence of a differential impact by gender of the child. As discussed above, it is possible that people wanted to educate their daughters, since this education would be an input into their grandchildren's education, and thus perhaps have marriage market returns. Thus, the lack of labor market returns to a daughter's education does not imply that there are no returns to a daughter's education (Behrman et al., 1999).

The third is the hypothesis is that general equilibrium effects could potentially account for our results. For example, Hazan et al. (2019) document that granting women property rights yields financial market deepening and economic growth, especially biased towards capital intensive manufacturing. A reasonable hypothesis might well be that the growth they document might have caused a decline in fertility and increase in education. However, this mechanism would affect *all* households, rather than just those married after property rights are granted.[93] As such, this hypothesis is inconsistent with the fact that the decline in fertility that we document seems to be driven by households married after economic rights were granted, rather than all households. On a larger scale, any mechanism by which women's rights may affect households through a general equilibrium effect, rather than the direct effect of rights on a household's decision, will run into this issue.

Finally, the fourth mechanism is that purposed by Doepke and Tertilt (2009). They argue that men granted women rights in order to induce an increase in education, since women's rights increases women's bargaining power and women are assumed to put greater weight on children's education. Thus women's rights increases education, and through a quantity-quality tradeoff, reduces fertility. This theory is related to ours, in the sense that both theories assume that women's rights increase women's bargaining power, and predict a decrease in fertility and an increase in education of children. However, Doepke and Tertilt (2009) would not predict that the declines in fertility would be strongest in states with the highest maternal mortality risk. Indeed, our findings, discussed above, suggest that maternal mortality risk was a key factor behind the decline in fertility

[93]Similarly, these returns to education should imply more investment in the education of *sons*, rather than daughters (Galor and Weil, 2000; Galor and Moav, 2002). This is because sons were much more likely to work in the labor force (as adults) than daughters. This is counterfactual.

we document. Thus, while there may be a role for the mechanism suggested by Doepke and Tertilt (2009), it cannot be the whole story as it would not predict this pattern in the data.

A shift in bargaining power from husband to wife, with maternal mortality risk yielding a disagreement between spouses over fertility, has a lot of appeal. It is backed by the literature, historical evidence, direct empirical evidence, and is consistent with every finding we document in this paper. On the other hand, potential competing mechanisms all seem to be at odds with at least one significant finding in our work. Thus, we conclude that intrahousehold bargaining is the main mechanism accounting for the findings documented in this paper.

7 Conclusions

In this paper, we exploit the staggered timing of coverture's demise in the U.S. in order to study the impact of women's empowerment on fertility and education of children. We find that legal changes can account for 15-20% of the changed in fertility and education during the demographic transition in the U.S.

We analyze several mechanisms and conclude that a shift in household bargaining power can account for the changes we document. In particular, it seems that maternal mortality risk was a likely underlying cause of spousal disagreement over the number of children.

Our previous work showed that women's economic rights deepened financial markets and aided industrialization (Hazan et al., 2019). Combined with this paper showing the effect of women's rights on fertility and education, it is reasonable to hypothesize that states that granted rights earlier had long run differences in outcomes compared to other states. This idea is along the lines of papers such as Nunn (2009), which discuss the literature around the importance of history and institutions in understanding economic outcomes in the present. We leave this agenda for future research.

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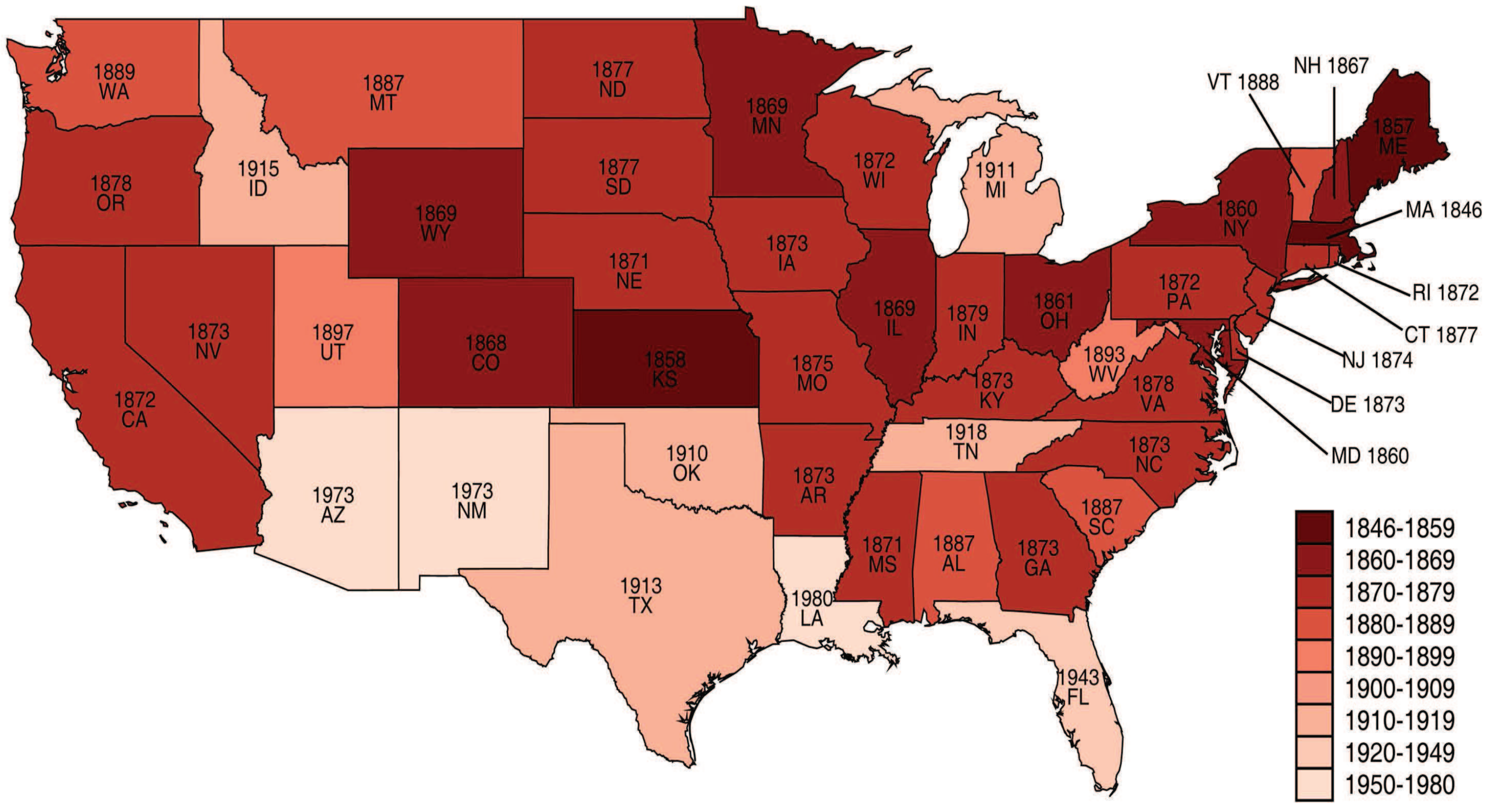

Figure 1: **Timing of women's rights by state.**

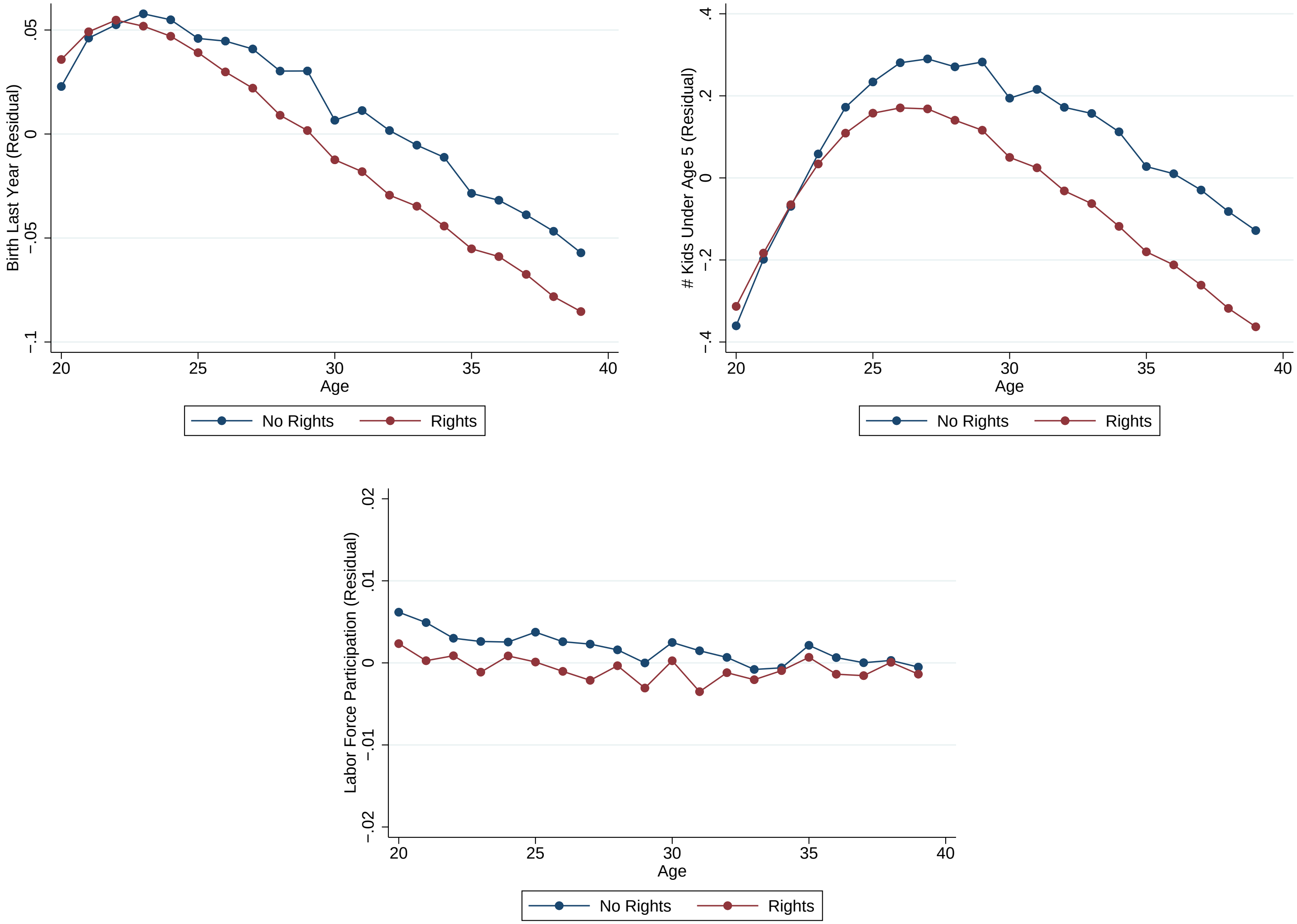

Figure 2: The top left panel shows the probability of giving birth by age of mother, with and without economic rights. The top right does the same analysis for the number of kids under age 5 in a household. The bottom panel also repeats this analysis for the labor force participation rate of the wife of the household. In all cases, the sample is white, non-Hispanic married women age 20-39 who live in the same state in which they were born. All variables are net of year fixed effects.

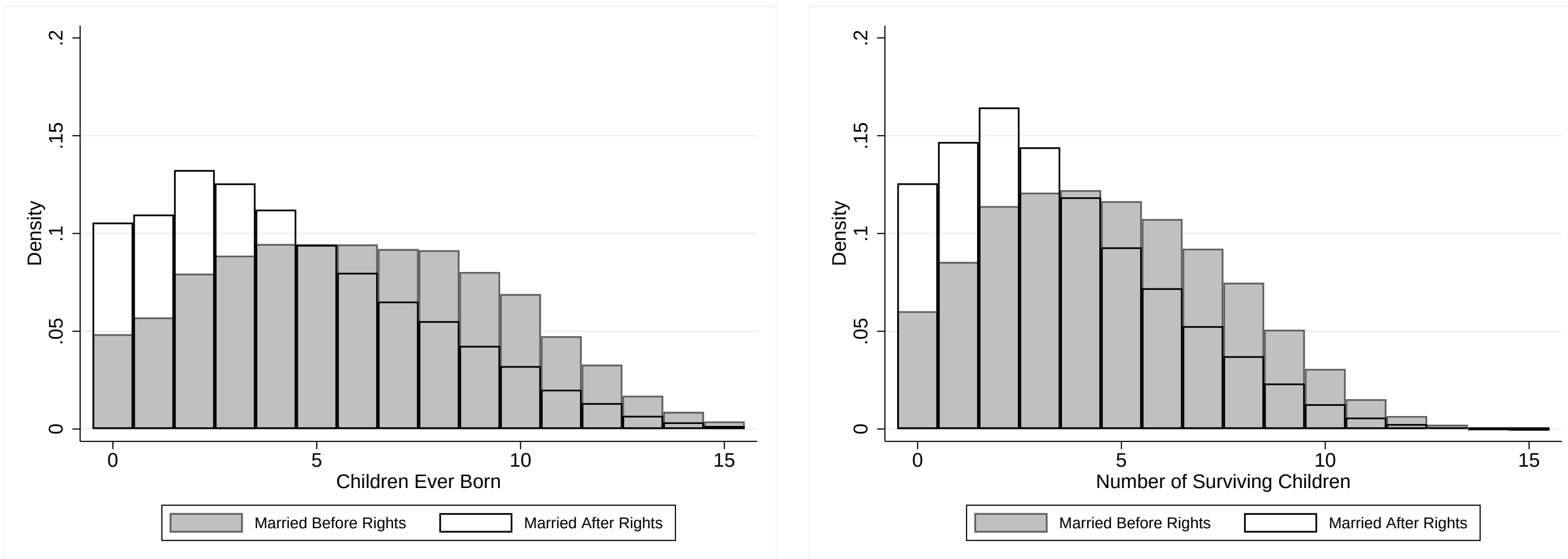

Figure 3: The left panel plots the density of children ever born to white, non-Hispanic married women age 45-59 who live in the same state in which they were born in 1900 and 1910. The plot is done separately by whether these women were married with economic rights or not. The right panel repeats this exercise for surviving children.

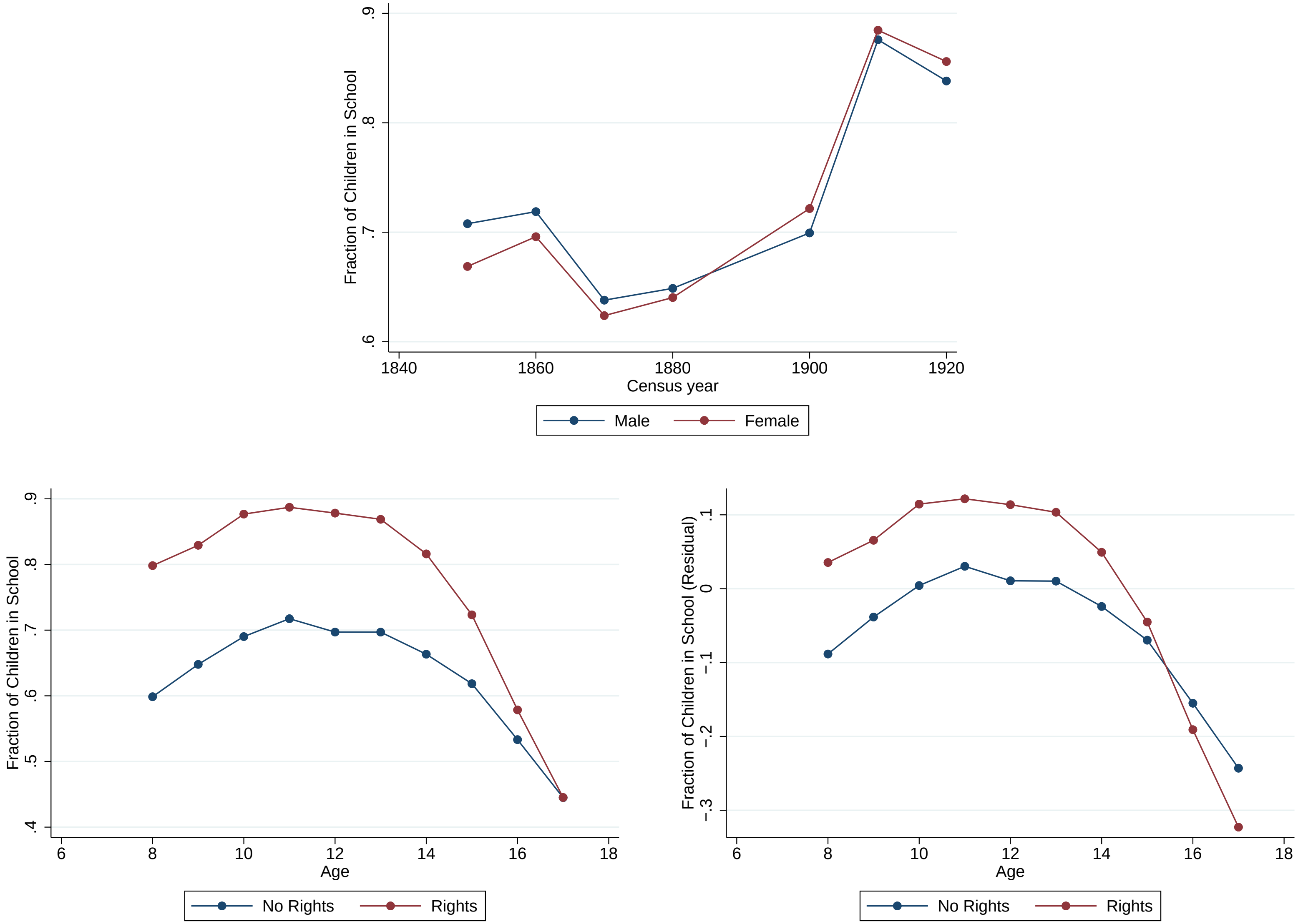

Figure 4: The top panel shows the fraction of kids in school, by gender, over time in the US. The bottom left panel shows the fraction of kids in school, by age and whether or not there are women's economic rights. The bottom right panel repeats the bottom left panel, but nets out year interacted with gender fixed effects.

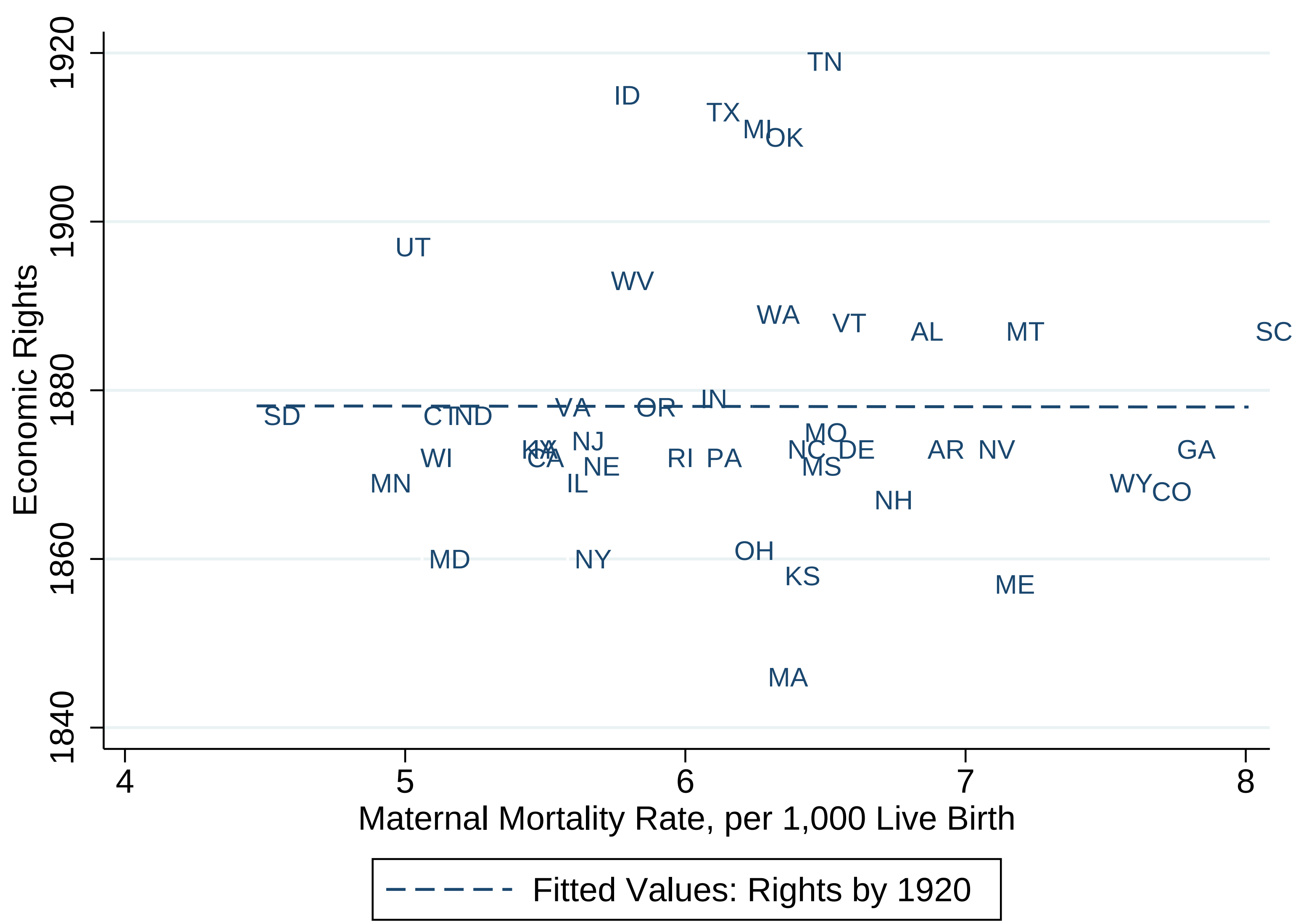

Figure 5: **Maternal Mortality Rate and Rights** The X-axis is the average maternal mortality rate (MMR) in a state between 1925 and 1934. The Y-axis is the date a state granted women economic rights. The line of best fit is not statistically significant or economically meaningful, yielding the conclusion that MMR was not associated with the timing of rights.

Table 1: Summary Statistics by Rights, Event Study

	Whole Sample	Before Rights	After Rights
	Panel A: Fertility		
Birth Last Year	0.20	0.24	0.19
	(0.40)	(0.43)	(0.39)
# of Kids Under Age 5	1.16	1.39	1.11
	(1.02)	(1.03)	(1.02)
Age	29.27	28.63	29.42
	(5.44)	(5.45)	(5.42)
Spouse's Age	33.58	33.21	33.67
	(6.71)	(6.87)	(6.67)
Year	1898.30	1870.46	1904.82
	(21.79)	(19.93)	(16.39)
N	14,460,963	2,743,165	11,717,798
	Panel B: Labor Force Participation 20-39		
Labor Force Participation Rate	0.04	0.04	0.04
	(0.19)	(0.19)	(0.18)
Age	29.29	28.58	29.42
	(5.44)	(5.46)	(5.42)
Spouse's Age	33.60	33.21	33.67
	(6.72)	(6.93)	(6.67)
Year	1900.97	1877.25	1905.16
	(19.11)	(18.54)	(15.87)
N	13,705,569	2,059,634	11,645,935
	Panel C: Labor Force Participation 40-59		
Labor Force Participation Rate	0.03	0.03	0.03
	(0.18)	(0.18)	(0.18)
Age	47.11	47.08	47.11
	(5.29)	(5.34)	(5.29)
Spouse's Age	50.85	51.14	50.81
	(7.46)	(7.65)	(7.43)
Year	1902.04	1877.08	1905.71
	(18.88)	(18.46)	(15.92)
N	6,787,373	870,424	5,916,949

Table 2: Summary Statistics by Rights, Married Before-After Rights

	Whole Sample	Before Rights	After Rights
	Panel A: Ages 20-39		
Birth Last Year	0.19	0.21	0.18
	(0.39)	(0.40)	(0.39)
# of Kids Under Age 5	1.10	1.23	1.08
	(1.02)	(1.04)	(1.01)
Labor Force Participation	0.03	0.04	0.03
	(0.18)	(0.19)	(0.18)
Age	29.51	29.25	29.55
	(5.41)	(5.51)	(5.40)
Spouse's Age	33.62	33.76	33.59
	(6.62)	(6.86)	(6.59)
Year	1905.57	1905.54	1905.57
	(4.97)	(4.97)	(4.97)
N	7,258,587	992,236	6,266,351
	Panel B: Ages 45-59		
Children Ever Born	4.78	6.01	4.29
	(3.37)	(3.48)	(3.21)
Surviving Children	3.76	4.69	3.39
	(2.75)	(2.87)	(2.61)
Labor Force Participation	0.04	0.04	0.05
	(0.20)	(0.19)	(0.21)
Age	50.11	51.60	49.94
	(4.03)	(4.18)	(3.87)
Spouse's Age	53.74	55.43	53.08
	(7.11)	(6.91)	(7.07)
Year	1905.72	1903.70	1906.52
	(4.95)	(4.83)	(4.76)
N	2,266,313	640,058	1,626,255

Table 3: Summary Statistics by Rights, Education Event Study

	Ages 8-17			Ages 8-13			Ages 14-17		
	Whole Sample	Before Rights	After Rights	Whole Sample	Before Rights	After Rights	Whole Sample	Before Rights	After Rights
In School	0.78	0.65	0.82	0.84	0.68	0.89	0.66	0.58	0.68
	(0.41)	(0.48)	(0.38)	(0.37)	(0.47)	(0.31)	(0.47)	(0.49)	(0.46)
Boys in School	0.78	0.66	0.82	0.84	0.68	0.89	0.66	0.61	0.68
	(0.41)	(0.47)	(0.39)	(0.37)	(0.47)	(0.31)	(0.47)	(0.49)	(0.47)
Girls in School	0.78	0.64	0.83	0.84	0.68	0.89	0.66	0.56	0.69
	(0.41)	(0.48)	(0.38)	(0.37)	(0.47)	(0.31)	(0.47)	(0.50)	(0.46)
Mother's Age	39.27	39.00	39.36	37.74	37.55	37.80	42.28	42.00	42.37
	(7.33)	(7.60)	(7.24)	(7.04)	(7.31)	(6.95)	(6.95)	(7.30)	(6.83)
Father's Age	44.19	44.18	44.19	42.63	42.69	42.61	47.25	47.26	47.24
	(8.14)	(8.35)	(8.06)	(7.99)	(8.22)	(7.91)	(7.53)	(7.76)	(7.45)
Year	1896.45	1867.87	1905.74	1896.19	1867.88	1905.61	1896.97	1867.85	1905.99
	(24.74)	(18.71)	(18.59)	(24.77)	(18.64)	(18.63)	(24.67)	(18.86)	(18.51)
N	18,522,654	4,541,931	13,980,723	12,261,162	3,060,763	9,200,399	6,261,492	1,481,168	4,780,324

Table 4: Summary Statistics by Rights, Education Married Before-After Rights

	Ages 8-17			Ages 8-13			Ages 14-17		
	Whole Sample	Before Rights	After Rights	Whole Sample	Before Rights	After Rights	Whole Sample	Before Rights	After Rights
In School	0.81	0.75	0.82	0.87	0.80	0.89	0.68	0.66	0.69
	(0.40)	(0.43)	(0.38)	(0.33)	(0.40)	(0.31)	(0.47)	(0.47)	(0.46)
Boys in School	0.80	0.74	0.81	0.87	0.80	0.89	0.66	0.65	0.67
	(0.40)	(0.44)	(0.39)	(0.34)	(0.40)	(0.32)	(0.47)	(0.48)	(0.47)
Girls in School	0.81	0.76	0.83	0.87	0.81	0.89	0.70	0.68	0.70
	(0.39)	(0.43)	(0.38)	(0.33)	(0.39)	(0.31)	(0.46)	(0.47)	(0.46)
Mother's Age	39.35	41.20	38.85	37.82	39.36	37.43	42.26	44.17	41.66
	(7.10)	(7.93)	(6.77)	(6.83)	(7.28)	(6.53)	(6.67)	(7.33)	(6.33)
Father's Age	44.19	46.48	43.56	42.66	44.72	42.14	47.09	49.31	46.39
	(7.80)	(8.40)	(7.50)	(7.68)	(8.37)	(7.40)	(7.18)	(7.67)	(6.88)
Year	1905.34	1904.35	1905.61	1905.28	1904.41	1905.50	1905.44	1904.25	1905.82
	(4.99)	(4.96)	(4.96)	(4.99)	(4.97)	(4.97)	(4.98)	(4.94)	(4.93)
N	6,368,189	1,138,809	5,229,380	4,130,291	711,547	3,418,744	2,237,735	427,262	1,810,473

Table 5: Birth, 1850-1920

Dependent Variable	Birth Last Year				
	(1)	(2)	(3)	(4)	(5)
$\geq$ 3 Decades Before	0.000	0.001	0.000	-0.001	0.001***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.000)
2 Decades Before	-0.001	-0.001	-0.002	-0.002	0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)
1 Decade Before	0	0	0	0	0
Rights Given	-0.007***	-0.007***	-0.007**	-0.006**	-0.008***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)
1 Decade After	-0.010***	-0.011***	-0.011***	-0.010***	-0.012***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
2 Decades After	-0.012***	-0.012***	-0.013***	-0.010***	-0.010***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)
$\geq$ 3 Decades After	-0.015***	-0.015***	-0.016***	-0.013***	-0.010**
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Controls	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	All-Two Step
N	14,460,963	14,460,963	11,652,654	13,945,960	13,403,911
Adj. R^2	0.025	0.027	0.028	0.027	–
Mean Dep. Var.	0.20	0.20	0.20	0.20	0.20

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include fixed effects for both the wife's and husband's ages, interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 3 excludes all borders of Southern States with non-Southern States. Column 4 excludes all Community Property States and their bordering states. Column 5 performs the two-step estimator described in the paper. The sample includes white, non-Hispanic women, age 20-39, married to men up to 50 years old, who live in the same state in which they were born.

Table 6: # of Kids Under 5, 1850-1920

Dependent Variable	# of Kids Under Age 5				
	(1)	(2)	(3)	(4)	(5)
$\geq$ 3 Decades Before	0.005	0.008	0.011	0.002	-0.005***
	(0.021)	(0.020)	(0.020)	(0.020)	(0.002)
2 Decades Before	-0.014	-0.014	-0.014	-0.017	-0.013***
	(0.016)	(0.016)	(0.017)	(0.016)	(0.004)
1 Decade Before	0	0	0	0	0
Rights Given	-0.028*	-0.029*	-0.025	-0.026	-0.053***
	(0.015)	(0.016)	(0.016)	(0.016)	(0.012)
1 Decade After	-0.037**	-0.040**	-0.042**	-0.036**	-0.047***
	(0.017)	(0.017)	(0.018)	(0.017)	(0.015)
2 Decades After	-0.056**	-0.059***	-0.059***	-0.053**	-0.068***
	(0.022)	(0.021)	(0.020)	(0.022)	(0.017)
$\geq$ 3 Decades After	-0.080***	-0.081***	-0.084***	-0.074***	-0.076***
	(0.024)	(0.023)	(0.023)	(0.024)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	All-Two Step
N	14,460,963	14,460,963	11,652,654	13,945,960	13,403,911
Adj. R^2	0.108	0.120	0.123	0.120	–
Mean Dep. Var.	1.19	1.19	1.17	1.18	1.19

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include fixed effects for both the wife's and husband's ages, interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 3 excludes all borders of Southern States with non-Southern States. Column 4 excludes all Community Property States and their bordering states. Column 5 performs the two-step estimator described in the paper. The sample includes white, non-Hispanic women, age 20-39, married to men up to 50 years old, who live in the same state in which they were born.

Table 7: Birth Last Year & # of Kids Under Age 5, Married After Rights 1900-1910

Panel A:	Dependent Variable: Birth Last Year					
	(1)	(2)	(3)	(4)	(5)	(6)
Married After Rights	-0.010***	-0.009***	-0.011	-0.009***	-0.010***	-0.004
	(0.003)	(0.003)	(0.009)	(0.003)	(0.003)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,258,587	7,258,567	5,096,244	6,746,354	3,219,519	4,039,048
Adj. R^2	0.0501	0.0525	0.0514	0.0523	0.0485	0.0539
Mean Dep. Var.	0.19	0.19	0.17	0.18	0.20	0.17
Panel B:	Dependent Variable: # of Kids Under Age 5					
Married After Rights	-0.143***	-0.138***	-0.169	-0.146***	-0.142***	-0.124***
	(0.038)	(0.039)	(0.108)	(0.039)	(0.042)	(0.033)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,258,587	7,258,567	5,096,244	6,746,354	3,219,519	4,039,048
Adj. R^2	0.1898	0.2019	0.1769	0.1986	0.2030	0.2000
Mean Dep. Var.	1.10	1.10	1.00	1.09	1.13	1.08

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. "Control" include wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property states and their bordering states. The sample includes white, non-Hispanic women, age 20-39, married to men up to age 50, who live in the same state in which they were born.

Table 8: Children Ever Born & Surviving Children, Married After Rights 1900-1910

Panel A:	Dependent Variable: Children Ever Born (CEB)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Married After Rights	-0.234**	-0.239**	-0.220**	-0.204*	-0.253**	-0.251**	-0.218*
	(0.102)	(0.103)	(0.095)	(0.101)	(0.104)	(0.112)	(0.113)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	CEB> 0	No South	No CP	1900	1910
N	2,266,313	2,266,292	2,063,535	1,602,073	2,185,335	969,420	1,296,872
Adj. R^2	0.2642	0.2818	0.2491	0.2316	0.2773	0.2847	0.2778
Mean Dep. Var.	4.78	4.78	5.25	4.27	4.73	4.93	4.67
Panel B:	**Dependent Variable: Surviving Children**						
Married After Rights	-0.180**	-0.183**	-0.169**	-0.129	-0.191**	-0.188**	-0.175*
	(0.084)	(0.085)	(0.077)	(0.085)	(0.087)	(0.091)	(0.097)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	CEB> 0	No South	No CP	1900	1910
N	2,266,313	2,266,292	2,063,535	1,602,073	2,185,335	969,420	1,296,872
Adj.R^2	0.2434	0.2602	0.2258	0.2095	0.2565	0.2641	0.2562
Mean Dep. Var.	3.7584	3.7584	4.1276	3.3546	3.7249	3.8478	3.6916

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the state level are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. “Controls” include wife’s age and husband’s age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects, interacted with year fixed effects. “Extra Controls” include husband’s occupation and husband’s industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic women, age 45-59, married to men up to age 70, who live in the same state in which they were born.

Table 9: Children Ever Born by Parity, Married After Rights 1900-1910

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Parity=0	Parity=1	Parity=2	Parity=3	Parity=4	Parity=5	Parity=6	Parity=7
Married After Rights	-0.012**	0.001	0.012**	0.015**	0.014**	0.009	0.005	-0.002
	(0.006)	(0.004)	(0.005)	(0.006)	(0.007)	(0.006)	(0.005)	(0.003)
N	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292
Adj.R^2	0.1350	0.0437	0.0301	0.0149	0.0069	0.0051	0.0086	0.0150
Mean Dep. Var.	0.0895	0.0948	0.1175	0.1152	0.1072	0.0943	0.0840	0.0728
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Parity=8	Parity=9	Parity=10	Parity=11	Parity=12	Parity=13	parity=14	Parity=15
Married After Rights	-0.006**	-0.009**	-0.009**	-0.007*	-0.005*	-0.003*	-0.001	-0.000
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)	(0.001)	(0.000)
N	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292	2,266,292
Adj.R^2	0.0232	0.0284	0.0317	0.0269	0.0216	0.0130	0.0081	0.0046
Mean Dep. Var.	0.0654	0.0532	0.0426	0.0278	0.0188	0.0097	0.0049	0.0022

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level, are in parentheses. The dependent variable is an dummy variable taking the value of 1 if a woman had a given parity level (as measured by children ever born). All specifications include county-year fixed effects and state-year fixed effects. All specifications include as controls the wife's age and husband's age fixed effects, duration of marriage fixed effects, as well as the husband's occupation and husband's industry fixed effects. All controls are interacted with year fixed effects. The sample includes white, non-Hispanic women, age 45-59, married to men up to 70, who live in the same state in which they were born.

Table 10: School, 1850-1920

Dep. Var.	Probability of Being in School								
Children's Age	8-17			8-13			14-17		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\geq$ 3 Decades Before	0.025	0.028	0.028	0.021	0.020	0.019	0.034	0.024	0.028
	(0.028)	(0.028)	(0.028)	(0.029)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)
2 Decades Before	0.021	0.020	0.021	0.017	0.016	0.015	0.029	0.019	0.023
	(0.029)	(0.026)	(0.027)	(0.031)	(0.027)	(0.027)	(0.027)	(0.026)	(0.027)
1 Decade Before	0	0	0	0	0	0	0	0	0
Rights Given	0.042**	0.043**	0.043**	0.048**	0.049**	0.051***	0.030*	0.034**	0.030*
	(0.020)	(0.019)	(0.019)	(0.022)	(0.019)	(0.019)	(0.018)	(0.017)	(0.017)
1 Decade After	0.052**	0.048**	0.049**	0.063***	0.064***	0.067***	0.025	0.034*	0.031
	(0.020)	(0.019)	(0.020)	(0.021)	(0.018)	(0.019)	(0.023)	(0.020)	(0.023)
2 Decades After	0.060**	0.053**	0.054**	0.079***	0.080***	0.082***	0.018	0.039	0.036
	(0.025)	(0.024)	(0.024)	(0.025)	(0.021)	(0.021)	(0.027)	(0.024)	(0.026)
$\geq$ 3 Decades After	0.046	0.034	0.036	0.069**	0.074***	0.077***	-0.002	0.030	0.027
	(0.028)	(0.026)	(0.026)	(0.029)	(0.027)	(0.027)	(0.030)	(0.027)	(0.028)
$\geq$ 3 Decades Before$\times$Female			-0.000			0.002			-0.008
			(0.006)			(0.004)			(0.012)
2 Decades Before$\times$Female			-0.001			0.002			-0.008
			(0.005)			(0.004)			(0.010)
1 Decade Before$\times$Female			0			0			0
Rights Given$\times$Female			-0.000			-0.004			0.007
			(0.003)			(0.002)			(0.006)
1 Decade After$\times$Female			-0.001			-0.005			0.008
			(0.006)			(0.004)			(0.013)
2 Decades After$\times$Female			-0.002			-0.005			0.006
			(0.007)			(0.004)			(0.016)
$\geq$ 3 Decades After$\times$Female			-0.003			-0.007**			0.005
			(0.006)			(0.003)			(0.015)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	18,522,654	18,522,654	18,522,654	12,261,162	12,261,162	12,261,162	6,261,492	6,261,492	6,261,492
Adj.R^2	0.197	0.209	0.209	0.205	0.215	0.215	0.155	0.182	0.182
Mean Dep. Var.	0.74	0.74	0.74	0.80	0.80	0.80	0.63	0.63	0.63

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state levels, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include the child's age, gender, mother's age and father's age fixed effects, all interacted with year fixed effects. "Extra controls" include father's occupation and industry fixed effects, the number of children at home, whether this child was allowed to work, and whether this child was allowed to not be in school, all interacted with year fixed effects. The sample includes children age 6-18 who are sons of white, non-Hispanic mothers, age 20-60, married to men up to age 70, who live in the same state in which they were born.

Table 11: School, 1850-1920. Robustness

Dep. Var.	Probability of Being in School					
Children's Age	8-17		8-13		14-17	
	(1)	(2)	(3)	(4)	(5)	(6)
$\geq$ 3 Decades Before	0.034	0.032	0.024	0.020	0.030	0.029
	(0.026)	(0.028)	(0.026)	(0.029)	(0.026)	(0.026)
2 Decades Before	0.026	0.021	0.021	0.015	0.026	0.021
	(0.029)	(0.026)	(0.030)	(0.028)	(0.028)	(0.026)
1 Decade Before	0	0	0	0	0	0
Rights Given	0.041**	0.040**	0.054***	0.051***	0.029*	0.026
	(0.018)	(0.019)	(0.016)	(0.019)	(0.017)	(0.017)
1 Decade After	0.053**	0.043**	0.072***	0.067***	0.037	0.025
	(0.023)	(0.020)	(0.020)	(0.019)	(0.024)	(0.023)
2 Decades After	0.053**	0.046*	0.086***	0.084***	0.040	0.026
	(0.023)	(0.025)	(0.019)	(0.022)	(0.025)	(0.027)
$\geq$ 3 Decades After	0.033	0.026	0.088***	0.080***	0.029	0.016
	(0.028)	(0.028)	(0.026)	(0.029)	(0.029)	(0.029)
$\geq$ 3 Decades Before$\times$Female	-0.002	-0.003	0.002	0.002	-0.010	-0.015
	(0.006)	(0.006)	(0.004)	(0.004)	(0.012)	(0.010)
2 Decades Before$\times$Female	-0.000	-0.000	0.002	0.002	-0.006	-0.007
	(0.006)	(0.005)	(0.005)	(0.004)	(0.010)	(0.009)
1 Decade Before$\times$Female	0	0	0	0	0	0
Rights Given$\times$Female	0.002	0.001	-0.003	-0.004*	0.009*	0.009
	(0.003)	(0.003)	(0.002)	(0.002)	(0.006)	(0.006)
1 Decade After$\times$Female	0.002	0.001	-0.003	-0.004	0.012	0.011
	(0.006)	(0.007)	(0.004)	(0.004)	(0.011)	(0.013)
2 Decades After$\times$Female	0.000	0.000	-0.005	-0.006	0.011	0.013
	(0.007)	(0.008)	(0.004)	(0.004)	(0.014)	(0.017)
$\geq$ 3 Decades After$\times$Female	0.000	0.001	-0.008**	-0.007*	0.013	0.014
	(0.007)	(0.008)	(0.004)	(0.004)	(0.015)	(0.018)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	No South	No CP	No South	No CP	No South	No CP
N	15,708,291	19,153,775	9,734,519	11,837,038	5,973,772	7,316,737
Adj.R^2	0.245	0.235	0.235	0.213	0.216	0.209
Mean Dep. Var.	0.71	0.71	0.79	0.80	0.58	0.58

Notes: $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include the child's age, gender, mother's age and father's age fixed effects, all interacted with year fixed effects. Extra controls include father's occupation and industry fixed effects, the number of children at home, whether this child was allowed to work, and whether this child was allowed to not be in school, all interacted with year fixed effects. The sample includes children age 6-18 who are sons of white, non-Hispanic mothers, age 20-60, with husbands up to age 70, who live in the same state in which they were born.

Table 12: Married After Rights, Attending School, 1900-1910

Dependent Variable:	Probability of Being in School					
Children's Age	8-17		8-13		14-17	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: 1900-1910			
Married After Rights	0.009*	0.010*	0.003	0.005	0.022***	0.019**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.007)	(0.008)
Married After Rights×Female		-0.002		-0.004		0.006
		(0.003)		(0.003)		(0.006)
N	6,368,189	6,368,189	4,130,291	4,130,291	2,237,735	2,237,735
Adj.R^2	0.2130	0.2130	0.1842	0.1842	0.2141	0.2141
Mean Dep. Var.	0.81	0.81	0.87	0.87	0.68	0.68
			Panel B: 1910			
Married After Rights	0.009***	0.010**	0.002**	0.003**	0.024***	0.024***
	(0.003)	(0.004)	(0.001)	(0.001)	(0.006)	(0.006)
Married After Rights×Female		-0.002		-0.002*		-0.001
		(0.002)		(0.001)		(0.005)
N	6,097,295	6,097,295	3,950,852	3,950,852	2,146,413	2,146,413
Adj.R^2	0.1857	0.1857	0.0818	0.0818	0.1704	0.1704
Mean Dep. Var.	0.88	0.88	0.95	0.95	0.76	0.76
			Panel C: 1900			
Married After Rights	0.009	0.010	0.003	0.006	0.021**	0.016
	(0.006)	(0.007)	(0.005)	(0.005)	(0.009)	(0.011)
Married After Rights×Female		-0.002		-0.006		0.011
		(0.005)		(0.005)		(0.009)
N	270,894	270,894	179,439	179,439	91,322	91,322
Adj.R^2	0.1629	0.1629	0.1308	0.1308	0.1793	0.1793
Mean Dep. Var.	0.72	0.72	0.78	0.78	0.59	0.59

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level, are in parentheses. All specifications include fixed effects for the ages of the child, mother, and father, fixed effects for the industry and occupation of the father, and county fixed effects. All specifications also include indicator variables for whether the child is female, allowed to work, and allowed to not be in school. All controls are interacted with year fixed effects. Standard errors are clustered by state.

Table 13: Fertility, Married After Rights 1900-1910 by MMR

	Panel A: Birth Last Year & # of Kids Under Age 5					
Dependent Variable:	Birth Last Year			# of Kids Under Age 5		
	Baseline	(2)	(3)	Baseline	(5)	(6)
Married After Rights	-0.009***	-0.006	-0.006*	-0.138***	-0.098**	-0.102**
	(0.003)	(0.004)	(0.004)	(0.039)	(0.042)	(0.042)
Married After Rights×High MMR		-0.009**	-0.009**		-0.119***	-0.119***
		(0.004)	(0.004)		(0.037)	(0.037)
Sample	All	All	Rights≤ 1920	All	All	Rights≤ 1920
N	7,258,567	7,258,567	7,103,333	7,258,567	7,258,567	7,103,333
Adj. R^2	0.0525	0.0525	0.0525	0.2019	0.2019	0.2007
Mean Dep. Var.	0.19	0.19	0.19	1.10	1.10	1.09

	Panel B: Children Ever Born & Surviving Children					
Dependent Variable:	Children Ever Born			Surviving Children		
	Baseline	(2)	(3)	Baseline	(5)	(6)
Married After Rights	-0.239**	-0.172*	-0.181*	-0.183**	-0.125	-0.129
	(0.103)	(0.102)	(0.102)	(0.085)	(0.083)	(0.084)
Married After Rights×High MMR		-0.502***	-0.505***		-0.442***	-0.444***
		(0.168)	(0.169)		(0.139)	(0.140)
Sample	All	All	Rights≤ 1920	All	All	Rights≤ 1920
N	2,266,292	2,266,292	2,229,846	2,266,292	2,266,292	2,229,846
Adj. R^2	0.2818	0.2820	0.2792	0.2602	0.2604	0.2583
Mean Dep. Var.	4.78	4.78	4.75	3.76	3.76	3.74

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state-year level, are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. Additional controls include wife's age, husband's age fixed effects, duration of marriage fixed effects, husband's occupation and husband's industry fixed effects, all interacted with year fixed effects. Column labeled "Baseline" is Column (2) of Table 7 for Panel A and Table 8 for Panel B. "High MMR" is an indicator that a household is in a state in the top 25% of maternal mortality risk.

Table 14: Fertility, by Wealth 1860-1870

Dependent Variable	Birth		# of Kids Under Age 5	
	(1)	(2)	(3)	(4)
Rights	-0.008*	-0.005	-0.018	-0.003
	(0.004)	(0.004)	(0.014)	(0.017)
High Wealth		-0.003		-0.018
		(0.002)		(0.013)
High Wealth × Rights		-0.010***		-0.054**
		(0.003)		(0.025)
Controls	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes
N	1,991,122	1,991,122	1,991,122	1,991,122
Adjusted R^2	0.022	0.022	0.110	0.111
Mean Dep. Var.	0.22	0.22	1.30	1.30

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the state and county-pair level, are in parentheses. All specifications include county-pair fixed effects and state fixed effects. "Controls" include wife's age and husband's age fixed effects, all interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. The sample includes white, non-Hispanic women, age 20-39, with husbands up to age 50, who live in the same state in which they were born. "High Wealth" includes those households at least at the 75th percentile of wealth.

Appendices

Appendix A More on Women's Rights

In this appendix, we first discuss the history of coverture at greater length and then discuss states that didn't follow coverture explicitly (civil law states) as well as potential ways in which women could avoid legal disabilities while under coverture (equitable estates).

A.1 Coverture and Slavery

Where did coverture come from?[94] A good starting place is the debate on married women's property rights in the U.K. House of Commons that took place on May 14, 1870. Mr. Jessel gives a brief overview of the history of coverture and women's legal status in England during an explanation of his support for removing women's economic disabilities that is useful to quote at length:

"The existing law was a relic of slavery, and the House was now asked to abolish the last remains of slavery in England. In considering what ought to be the nature of the law, they could not deny that no one should be deprived of the power of disposition unless on proof of unfitness to exercise that power; and it was not intelligible on what principle a woman should be considered incapable of contracting, immediately after she had, with the sanction of the law, entered into the most important contract conceivable. The slavery laws of antiquity were the origin of the Common Law on this subject. The Roman law originally regarded the position of a wife as similar to that of a daughter, who had no property, and might be sold into slavery at the will of her father. When the Roman law became that of a civilized people, the position of the wife was altogether changed. She was allowed, as was proposed by this Bill, to have the absolute disposal of her property, and full power of contracting, with the sole exception that her immoveable property was not to be alienated without the consent of her husband. The ancient Germans - from whom our law was derived - put the woman into the power of her husband in the same sense as the ancient Roman law did. She became his slave. The Law of Slavery, whether Roman or English, for we once had slaves and slave laws in England, gave to the master of a slave the two important rights of flogging and imprisoning him. A slave could not possess property of his own, and could not make contracts except for his master's benefit, and the master alone could sue for an injury to the slave; while the only liability of the master was that he must not let his slave starve. This was exactly the position of the wife under the English law; the husband had the right of flogging and imprisoning her, as might be seen by those who read Blackstone's chapter on the relations of husband and wife. She could not possess property - she could not contract, except as his agent; and he alone could sue if she were libelled or suffered a personal injury; while all the husband was compellable to do for her was to pay for necessaries. It was astonishing that a law founded on such principles should have survived to the nineteenth century" (Hansard (1869)).

The relationship between wives and slaves was not lost on Americans either, with senators discussing definition of freedom including the right to contract, explicitly denied to married women,

[94] The laws of coverture had been in place in England since at least the fourteenth century. Butler (2013) details examples of coverture in legal history, using the "Year Books" as a primary source. The Year Books are a collection of debates between the king's "justices and pleaders," detailing the deliberations and conclusions of a variety of lawsuits. Butler analyzes many of the court cases from the fourteenth and fifteenth centuries, with a special emphasis on studying married women's legal disabilities and the so-called "civil death" that women experienced upon marriage, as their identities became legally inseparable from those of their husbands. Husband and wife were considered to be one under the law, even to the extent that a 1365 case of conspiracy between a husband and his wife to falsely accuse someone of murder was dismissed, as conspiracy requires at least two people.

while discussing the emancipation of slaves. It was not lost on the senators that the rights being granted to freed slaves were not necessarily granted to women (Stanley, 1988). It is sufficient to say that the laws of coverture made a wife essentially a slave to her husband and that granting property rights to women was a major step towards equality.[95]

A.2 Community Property and Equitable Estates

We next turn to the issue of community property, followed by a discussion of equitable estates. Eight states, namely Arizona, California, Idaho, Louisiana, Nevada, New Mexico, Texas, and Washington, had community property laws governing marital asset ownership and control as per the traditions of civil law, rather than common law's doctrine of coverture.[96] Community property divided household assets into three classes: the husband's separate property, the wife's separate property, and community property. According to Spanish law, property of all types acquired after marriage, except by gifts and inheritance, became community property with each spouse having a 50% interest.

At first glance, these laws seem to preclude gender-based property rights discrimination after marriage. However, it is not clear that many people were even aware of the difference in laws between community property and common law states. Schuele (1994, p. 260) describes the history of the Californian constitutional debate, indicating that the Americans involved in the debate had little understanding of the community property system they were adopting on paper, and indeed argues that the constitution "... did not clearly mandate a community property system." The constitution did call for further laws to be passed on the subject. Shortly after ratification, there was a large increase in migration from the rest of the U.S., which pushed California to adopt common law as the jurisprudence of the state. As a result, Schuele (1994, p. 262) argues that "[l]egislatures appear to have been unable to ignore their common-law heritage and may even have been hostile towards property rights of married women. Contrary to the spirit of [the constitution], women were given no management rights over their separate property, much less over the common property."[97]

However, it was not even the case that community property states gave women equality *de jure*, even if people were confused about the laws and didn't follow them *de facto*. We begin by discussing men's rights over their own separate estate, then over the community property, and then over their wives' separate estates.

Men had absolute control and interest over their own separate estate. In practice they basically had the same rights over the community property. How did the community property work in

[95]The legal disabilities that married women suffered under coverture, beyond the lack of property rights, were severe and often personally traumatic. For instance, consider custody over children, of which one author notes "The common law on this matter is easily summed up: the father had the absolute right to custody of the children; the mother had no rights at all." (Holcombe (1983), p. 33). Furthermore, a husband had a great deal of control over his wife's body and actions, as "... women could not refuse sexual relations unless performance of the duty threatened their lives, nor could they withhold domestic services. A man controlled access to the home and could 'imprison' his wife to prevent her from 'going off with an adulterer' or 'squandering his property.' In [a legal commentator]'s view, because a man was 'criminally responsible for her acts of crime committed in his presence, and civilly for her torts whether he is present or absent,' he needed 'physical control over her' sufficient to 'free himself' from liability" (VanBurkleo (2001), p. 77).

[96]"Civil law" refers to law coming from either French (Louisiana) or Spanish legal traditions.

[97]There were a number of other distinctions between Californian law and Spanish law. For instance, Spanish law considered the revenue generated by separate property, such as stock dividends, to be community property, while in California these dividends would remain the separate property of whichever spouse owned the stock (Schuele (1994), p. 279). Arizona also classified revenue from separate property to be part of the separate estate, under the influence of common-law lawyers (Lyons (1955)).

practice? Of the great number of migrants into California during this time, "many of them, having reached adulthood in the East, might not have realized that California's marital property laws supposedly differed from those in the rest of the nation" Schuele (1994, p. 262). This is especially true since Basch (1982) notes that, while many details of common law might have been lost on the general public, common law classifications of married women's rights were well represented in popular books and magazines of the time. Indeed, the confusion was so deep that two years after women were given property rights in California, state senator Laine reportedly considered introducing a bill to reestablish common law in California, apparently unaware that common law had never been imposed in the first place (Schuele (1994)). As such, de Funiak (1943), as cited in Schuele (1994, p. 25), found that "[m]any lawyers trained in the common law ... seem to fail to comprehend ... that the management of the common property placed in the husband was an administrative duty only ... and not in any sense the equivalent of the common law 'control' by the husband to the wife's property which made him virtual owner and gave him the right to appropriate its use to his own enjoyment and benefit." While technically community property was bequeathed according to each spouse's 50% interest in it, the husband had full management rights and "... with almost his last breath, he [could] convey away the community property so deftly that no known law [could] reach it" (Stow (1877), p. 65). That is, the husband could convey or gift community property as he wished up until his death, which perhaps undid any measure by which the woman's interests in community property were protected.

Even more importantly, a husband also had absolute management over his wife's separate property, with the lone stipulation that he must manage it for her "benefit", a poorly defined term. This alone would presume to give him most, if not all, of the power over household financial decisions. Given that people's understanding of property definitions was erroneously based on common law (Schuele (1994)), it is reasonable to assume that most husbands would act as if common law were in force, and not alienate a women's separate real property while doing as they wished with her personal property. This is not just a conjecture, as the difference in treatment between real and personal assets is particularly noticeable in New Mexico. A 1901 state amendment required that wives agree to all sales and mortgages involving the common real estate before such transactions could occur. Courts decided that this did not apply to moveable assets, which remained in the absolute control of husbands (Lyons (1955)). It seems reasonable to assume that other community property states, which are mostly in the west, went through similar experiences.[98] This supports the idea that, in practice, there was not much difference *de facto* between community property states and common law states. Since the basic hypothesis of this study seems to stand in community property states, we include these states in our benchmark exercises. However, we perform robustness exercises in which we drop these community property states, verifying that these states are not biasing the results of our empirical work.

Finally, there is the issue of separate estates for married women. These separate estates date back to sixteenth-century England, and were run under courts of equity, or "chancery," rather than courts of common law. In principle, they allowed married women to have property put into a separate trust, run by a trustee, who was immune from their husbands' influence. These trusts were either created before marriage or upon receipt of an inheritance. They varied widely in the level of protection from the woman's husband, the amount of control the woman had, and the degree of control the trustee had. In England, these separate estates were prohibitively expensive for all but the wealthiest women (Holcombe (1983)). In America, these trusts, also called "marriage settlements," were even rarer: only 12 states had equity courts (Geddes and Lueck (2002)). American

[98] For a fascinating study of the history of the relationship between civil law and common law in Texas, see Lazarou (1980).

women needed their fiancé's or husband's permission for these trusts to be created (Salmon (1986), p.15) and these estates were deemed fraudulent if created to deceive a fiancé (Salmon (1986), p. 89). In much of New England, such as Connecticut and Massachusetts, the legislatures refused to empower equity courts to enforce the rules. This was a result of Puritan influence, which was harshly opposed to courts of equity (Salmon (1986), pp. 120-140). Other states, such as Pennsylvania, "held an equitable jurisdiction allowing them to enforce trusts for married women, but evidence indicates that the judiciary felt uncomfortable in exercising the full panoply of equitable rules and precedents" (Salmon (1986), p. 186). Indeed, Chatfield (2014, pp. 16-17) argues that "equity courts tended to interpret contracts between husbands and wives narrowly, and with greater deference to creditors than to married women or widows, meaning that there was no sure guarantee that a woman's property would be protected upon becoming married." As such, it should not be surprising that (Salmon (1986), p. 79) argues that "[b]ecause the rules fell under the supervision of courts of equity and were never defined by statute ... they remained inaccessible to the majority of women. This explains why most historians have depicted the nineteenth-century married women's property acts as more important to women than the equitable developments of the early modern period ...."[99]

Appendix B Construction of County-Border Pairs

This appendix documents the data and empirical approach used in creating our county-border pairs for the event-study analysis. We include an example showing the evolution of the border between Indiana and Illinois.

The data on the evolution of US historical county boundaries comes from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System (NHGIS), available at http://www.nhgis.com. Although there are other projects featuring US historical boundaries and spatial data within a Geographic Information Systems (GIS) framework, we use the NHGIS border definitions, as they provide a better fit for mapping US federal census data from IPUMS. We start by obtaining eight geometry file maps corresponding to the 1850-1920 census year boundaries. These shapefiles consist of polygons, each of which is defined by a list of vertices with two-dimensional coordinates. We use QGIS as our primary tool for handling the shapefiles. In order to identify the best topologically continuous set of bordering counties (i.e., counties adjacent to the counties borders from another state) over the entire 1850-1920 period, we develop the following four-step procedure:

Step 1 We identify for every polygon in the shapefile all of its immediate neighbors. A polygon is considered a neighbor of another polygon if they touch or intersect. The script records the unique county (GISJOIN2 variable) and state identifiers of all neighbors. We eliminate counties that are only adjacent to counties from the same state/territory in order to arrive at a sample of county-border pairs. We manually examine the resulting samples and eliminate polygons that correspond to the administrative units that have not been partitioned into counties, such as large territories without political subdivisions.

Step 2 The borders in the year 1920 are the final borders for our study. The borders in earlier decades were unstable due to the evolution of states, as well as counties within the states. We created a stable system of IDs for each region based on the map of 1920. For the earlier decades (1850-1910), we adopt the names given in 1920. Each county's ID in 1850-1910 is

[99] Salmon (1986) does, however, argue that these equity-based rules were important for their impact on legal thinking.

defined by the highest intersected area with the IDs in 1920. In other words, each county x in 1850-1910 takes the ID of the county y in 1920 if and only if county y has the highest intersected area with county x across all different intersected counties in 1920.

Step 3 If a county breaks into multiple counties over the course of time, we look at new counties after separation as one cluster based on their borders before separation. This allows us to maintain constant geographic areas as our points of comparison. To be more precise, for each county in decade d, we look at the corresponding counties in previous decades: $t \in [1850, .., d - 10]$. If a county from decade t, x_{it} intersects with several counties in decade d, with an intersected area that exceeds 25 percent of the area x_{id}, then all these counties in d are considered as a unique county: x_{it}. We unify overlapping clusters into one.

Step 4 We then develop a stable set of pair-dummies that corresponds to neighboring fixed counties in neighboring states. We proceed as follows:

- For each county from each decade we find all neighboring counties from other states in the same decade.
- If the joint border between a pair of neighboring counties from 2 different states is longer than 10 percent of the length of each county's border with the other state, then we constitute a pair-dummy for this pair.
- If a county-pair was not considered in previous decades– perhaps since the area wasn't well defined or stable– we create a new name for the dummy variable based on the combination of IDs. This step allows us to produce a stable structure of dummies through time.

Maps showing our data on borders over time can be seen in Figures A.2-A.9. Figure A.10 shows the evolution of the border between Indiana and Illinois. Each map shows this border in different years, from 1850 until 1920. The solid black line in the middle, roughly going from north to south, denotes the border between the states. Each polygon represents a county in each year on either side of the state border. Counties in the same state in the same color that touch one another are grouped in a cluster and treated as one county for the purposes of this exercise. The dotted lines show which clusters of counties were compared to which clusters on the other side of the state border. The border between Illinois and Indiana is particularly useful as an example since it allows us to illustrate a number of issues that arose while creating our county-border pair exercises. Many of them have no clear-cut answer as to what to do and require a judgement call. We now go over some issues that arose.

1. Whether to clump counties together when they merged. Consider the 3 orange colored counties in Illinois, almost all the way in the north, in 1860. These counties are roughly geographically constant from 1860 until 1920. However, the middle of these 3 counties did not exist in 1850, and was indeed part of the other two counties. We decided to treat this entire area as one county for the entire time period. An alternative strategy would have been to throw out the data from 1850 and have 3 distinct counties from 1860 onwards.

   Another example of this is the blue county directly south of these orange counties. In 1850, this region was one large county. Starting in 1860, this region was divided into two counties: one directly on the border with Illinois, and one not directly on the border. We decided that we would consider these two counties as one during the entire time period, and thus include a county not directly on the border, rather than begin the exercise in 1860.

2. Small changes in county areas. Consider the most north-western sliver of the blue county adjacent to the orange counties discussed above, in 1850. In 1860, some of this territory is transferred into the orange counties. The area in question is outlined in white in the 1860 map. Considering that this area is quite small, we decided to ignore this change in county areas.

While reasonable people can disagree as to whether the approaches described above are correct or not, both in general and in the specific context of Indiana and Illinois, we hope that it illustrates the need to make a general set of rules and apply our best judgement throughout. Any rule that might seem more/less appropriate on this border, might seem the opposite on a different border.

Appendix C Marriage Market Balancing

In this Appendix, we test whether the marriage market was affected by women's rights. To do so, we begin by estimating regressions of the form described in Equation (1), where the dependent variable is whether a man is married, the average age of married men, or the average age gap between husband and wife. We do so on two subsamples of men. The first subsample is all white non-Hispanic men age 15-60, and the second changes the age range to 15-30. We then change the dependent variable to be either whether a man is newly married (in the previous 12 months), the average age of newly married men, and the average age gap between newly married husband and wife. We again use the same two subsamples. We then estimate regressions of the form described in Equation (4), comparing couples married before and after rights, where the dependent variable is the age gap between husband and wife, on the sample of married white non-Hispanic women living in the state where they were born in 1900 and 1910 (the two years for which we have duration of marriage data).

Table A.9 shows the results when the dependent variable takes the value of 1 if a man is married and 0 otherwise. Column 1 uses the sample of men age 15-60 and includes as controls age fixed effects interacted with year fixed effects. Column 2 repeats Column 1 but also includes occupation and industry fixed effects interacted with year fixed effects. Columns 3 and 4 repeat Columns 1 and 2 on the sample of men age 15-30. Columns 1 and 2 find some evidence that there were fewer married men two and three decades before rights were granted, relative to a decade before rights were granted. However, the estimates are small quantitatively, and only significant in Column 2 for the estimate two decades before rights were granted. Columns 3 and 4 do not find such evidence. All specifications find no impact of women's rights on the propensity for a man to be married.

Table A.10 repeats Table A.9, but switches the dependent variable to be the age of a man, conditions the sample on men being married, and removes the controls for the age of men. Thus, we measure the average age of married men before and after rights are granted. All specifications find no trend in the average age of married men prior to rights being granted. After rights are granted, Columns 1 and 2 find an increase in the average age of a married man of about a 0.25 to 0.6 years, with the effects statistically significant. However, this effect almost entirely goes away when looking at younger men. Indeed, Column 3 finds no consistent evidence of any change in the average age of married men, with at most an increase of 0.1 years 2 decades after rights. If anything, Column 4 suggests a decline in the average age of married men, but the only estimate that is significant is a decrease of 0.068 years a decade after rights are granted. We conclude that there is no consistent evidence that the average age of married men changed with women's rights.

Table A.11 also repeats Table A.9, switches the dependent variable to be the age gap between husband and wife, and conditions the sample on men being married. There is no pretrend in the age gap between husband and wife prior to rights being granted and no pattern of a change in this gap after rights are granted.

Table A.12 repeats Table A.9 but switches the dependent variable to be whether a man has been married in the last year (is "newly wed"). There is no evidence of any trend in the propensity to be newly married either before or after women's rights in any specification.

Table A.13 repeats Table A.10 but conditions the sample on a man being newly married, rather than just married. Largely speaking, we find no evidence that the average age of newly married men changed around the time women were granted rights. Column 3 finds some evidence that the average age decreased by a half a year 1 and 3 decades after rights were granted, but these results are both quantitatively small and not robust, as seen in other specifications.

Table A.14 repeats Table A.11 but conditions the sample on the man being newly married. In all specifications, there is no evidence that the age gap between husband and wife exhibits a trend prior to rights being granted. There is some evidence that the age gap decreases by a little more than a half a year after rights are granted, in Columns 2 and 4. This effect is statistically significant at the 10% level only one decade after rights in Column 2, and at the 5% level one decade after rights and at the 10% level two decades after rights in Column 4. We conclude that the effect is small, and not statistically robust.

Turning to the comparison of the age gap between couples married before and after rights are granted, Table A.15 shows our results. Column 1 controls for the age of husband fixed effects and duration of marriage fixed effects, both interacted with year fixed effects. Column 2 repeats Column 1 but adds the husband's occupation and industry fixed effects, both interacted with year fixed effects. Column 3 repeats Column 2, but does not include the South. Column 4 also repeats Column 2, but does not include community property states. Column 5 (6) also repeats Column 2, but restricts the sample to be data from 1900 (1910).

Panel A restricts attention to women age 45-59 in 1900 and 1910, while Panel B looks at women age 20-39. In Panel A, we find no economically meaningful or statistically significant difference in the age gap between spouses married before rather than after rights were granted. In Panel B, we find some evidence for a small difference in age, of about 0.20-0.25 years (2-3 months) and more of an age gap when the couple was married after women's rights. This estimate is statistically significant at the 1% level in Column 6, the 5% level in Columns 1, 2, 4, and 5, and not significant in Column 3. We note that this estimate is both small and the opposite sign of those found above. We thus conclude that women's rights did not have a clear impact on the age gap between husband and wife.

As such, we conclude that married women's property rights had a very limited impact, or even no impact, on the propensity to get married, the age of married (and newly married) men, and the age gap between husband and wife.

Appendix D County Heterogeneity Within State

As discussed in Section 4.3, state-level law changes are plausibly exogenous to individual counties in the state, allowing our event-study exercise to capture the causal effects of women's rights on households. However, this argument is invalid if all the counties within a state are similar. If this is the case, then state legislatures pass laws that all counties "agree" on, and reverse causality becomes a concern. In this appendix, we address this concern by studying heterogeneity within states during our sample period.

Specifically, for each year, we calculate the average fertility, education, and labor force participation rates in our sample for each county in each state. We then regress these averages on state fixed effects and report the R^2 and adjusted R^2. These measures reflect how much of the county-level heterogeneity can be accounted for by states.

Table A.8 reports the results. Panel A reports the results when the dependent variable is the probability a woman gave birth in the previous year. The number of counties in the sample increases from 1,492 to 3,063 over the course of the sample. The R^2 (adjusted R^2) ranges from about 0.1 (0.07) to 0.36 (0.35), suggesting that about 65-90% of variation between counties cannot be explained by state fixed effects. Panel B repeats this exercise for the number of children under age 5 and finds that the R^2 (adjusted R^2) ranges from about 0.25 (0.22) to 0.54 (0.53), suggesting that about 46-75% of variation between counties cannot be explained by state fixed effects. Panel C, D, and E repeat these exercises for children age 8-17, 8-13, and 14-17, respectively. In all cases, the R^2 (adjusted R^2) ranges from about 0.2 (02) to about 0.5 (0.5), suggesting that 50-80% of the variation between counties in education cannot be explained by state fixed effects. Finally, Panels F and G repeat these exercises for the labor force participation rates of women age 20-39 and 40-59, respectively. Approximately 70-99% of the variation in labor force participation rates cannot be explained by state fixed effects.

Since the R^2 and adjusted R^2 for these exercises are low, we conclude that there is substantial heterogeneity between counties within states in our sample. Thus, it is reasonable to conclude that state policies are exogenous to individual counties within our sample.

Appendix E Randomization Exercises

This appendix documents the randomization exercises discussed in the main paper. In these exercises, we do the following:

1. Randomly assign a date that each state granted women rights. The date is uniformly drawn between 1850 and 1920.
2. Rerun our estimation procedure on these fake dates of women's rights.
3. Repeat steps one and two 1,000 times.

The idea behind the exercise is twofold. First, if the estimates from these randomized regressions are centered at zero, it suggests that there is no bias in our regression specifications. Second, the percent of these estimates that give a larger effect of women's rights gives a second type of "p-value". This p-value can be interpreted as a measure of how unlikely a random set of years would be to generate our results, and thus indicates the significance of the actual years in which women were granted rights in the US.

Table A.2 documents the results of these exercises when looking at the event-study approach to fertility, as discussed in Section 5.1.1. In particular, we randomize the first specifications of Tables (5) (with the dependent variable being the probability the wife gave birth last year) and (6) (with the dependent variable being the number of children under age 5).[100] The results of these exercises are reported in Panels A and B, respectively. Columns 1-6 report the mean, standard deviation, minimum value, maximum value, the value from the regression on the actual dates ("Our Estimate"), and the fraction of random regressions that gave a larger estimate than our estimate ("p-value").

Beginning with Panel A, the randomized sample of all of our outcome variables (estimates on 3 decades before rights through 3 decades after rights) are centered at zero, and have minimum and maximum values approximately 3 standard deviations from zero. We conclude that the specification of the relevant regression is not biased. The "p-value" on estimates before rights is high: 50.1% for three decades before rights and 38.1% for 2 decades before rights. This reinforces the notion that there was no detectable pre-trend in fertility prior to rights being granted. The "p-value" for the impact of rights is 0.6%, one decade after rights is 1.6%, two decades after rights is 2% and three decades after rights is 3.5%. Thus we conclude that it is highly unlikely that a random set of dates would generate results similar to those we document in this paper.

Turning to Panel B, again the randomized sample of estimates is centered on zero, with the minimum and maximum approximately 3 standard deviations from zero. Again our estimates have large p-values prior to rights being granted (57.9% 3 decades before rights and 38.1% 2 decades before rights), and low p-values after rights have been granted (2.2% when rights are granted, 5.8% a decade later, 2.8% two decades later, and 2.6% three decades later). We thus again conclude that our regression specification is not biased, and that it is highly unlikely that a random set of dates would generate results similar to those we document in this paper.

Table A.3 does a similar set of exercises on the regressions estimating the effect of being married after rights. Panel A analyzes the fertility variables, as in Section 5.1.2, while Panel B analyzes the education variables, as in Section 5.2.2.

Beginning with Panel A of Table A.3, all of the distributions of the measures of fertility are centered at zero and have minimum and maximum values about 2-3 standard deviations around zero. The p-value of our estimates for the probability a woman gave birth last year, the number of kids under 5, children ever born, and the number of surviving children are 4.4%, 0.2%, 3%, and 3.3%, respectively. Accordingly, we conclude that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would generate results similar to those we document in this paper.

Turning to Panel B of Table A.3, again all of the distributions of the measures of fertility are centered at zero and have minimum and maximum values about 2-3 standard deviations around zero. The p-value of our estimates for the effect of parents being married after rights on the education of all children is 13.2%. For younger children, the value is much larger at 34.7%. However, for older children, there were no simulated dates that resulted in an estimate with a larger value than the actual dates. As such, the p-value is 0.

Table A.4 reports the results of the randomization for the event-study analysis of education, as described in Section 5.2.1. We perform the randomization exercise described above on Column 4 of Table 10, which is the least demanding specification for the younger children. As before, we pick this specification since it is substantially faster to compute. The distribution of all estimates

[100] We choose the least demanding specification, as these exercises are computationally demanding. Having fewer control variables substantially speeds up the computational process. Given that the results are very similar between the various econometric specifications, it is likely that these randomization exercises would yield similar results if performed on other specifications.

is centered at 0 and has minimum and maximum values dispersed approximately 3 standard deviations from zero. The p-values of our estimates prior to rights being granted are large. In particular, they are 25.5% three decades before rights are granted and 25.1% two decades before rights are granted. The p-values of our estimates after rights are granted are small. In particular, the p-value for the effect upon granting rights is 0.3%, a decade later it is 0.5%, two decades later it is 0.3%, and three decades later it is 4.3%. Accordingly, we conclude that our specifications are not biased, and it is very unlikely that random chance would generate results similar to those documented in this paper.

Appendix F Labor Force Participation

In this appendix, we argue that women's rights did not affect the labor supply of married women, using both the event-study approach as well as comparing couples married before and after rights were granted.

F.1 LFP: Event Study Approach

We estimate regressions of the form described in Equation (1), where the dependent variable is either whether the wife of the household is in the labor force, using the same controls as in Section 5.1.1. That is, the controls in variable X_{hsct} include fixed effects for the wife's age and the husband's age, both interacted with year fixed effects. Some specifications include "extra controls," which include fixed effects for the husband's industry and husband's occupation, both interacted with the year fixed effect. These extra controls allow us to control for how a husband's career might affect the labor force participation of his wife, and how the relationship may change over time. In some specifications, we also include fixed effects for the number of kids under age 5 in the previous year to control for how the stock of young children may affect the probability of a wife working.

Table A.5 repeats Table 5, where the dependent variable takes a value of 0 when the wife is not in the labor force and 1 when she is, on the sample of married women age 20-39. Column 1 does not include our extra controls or controls for the number of kids under 5 in the previous year. Column 2 repeats Column 1 but adds the extra controls and is thus our preferred specification. Column 3 repeats Column 2 but drops counties on the border between Southern and non-Southern states. Column 4 also repeats Column 2, but drops counties in community property states, as well as their border-pairs. Table A.6 repeats Table A.5 on the sample of married women age 40-59. All specifications in both tables show no relationship between married women's property rights and women's labor supply.

In all specifications for both tables there is no economically meaningful or statistically significant trend in LFP prior to rights being granted, or any quantitatively meaningful or statistically significant impact of rights on LFP for at least the first two decades after rights were granted. Table A.5 finds some evidence in Columns 1, 3, and 4 that LFP was higher two decades after rights, but this effect fades by three decades after rights, such that this estimate seems like noise. Table A.6 does not document such an effect.

We thus find no evidence, using this event-study design, that economic rights affected women's LFP.

F.2 LFP: Couples Married Before/After Rights

We next turn to the impact of women's economic rights on labor force participation, as measured by comparing couples married before and after rights were granted. As such, we estimate equations along the lines of those described in Equation (4), using the controls described in Section 5.1.2.

Table A.7 shows the results when the sample is women age 45-59 (Panel A), or 20-39 (Panel B), and follows the same pattern as in Table 7. Column 1 includes as controls fixed effects for the wife's age, the husbands age, and how long the couple has been married, all interacted with year fixed effects. Column 2 adds the "extra controls," which are fixed effects for the husband's occupation and industry, interacted with year fixed effects. Column 3 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 4 also repeats Column 2, but drops counties in community property states as well as their bordering counties. Columns 5 and 6 also repeat Column 2, but do so only on the sample from 1900 or 1910, respectively.

All specifications in both panels show no quantitative or statistical difference in labor force participation between couples married before and after women's rights.[101]

We make two notes on these results. The first is that the sample of women 20-39 is exactly the sample that reduces their fertility contemporaneously. That is, we document in Section 5.1.2 that these women reduce the probability that they gave birth as well as the number of children under age 5. Since the estimates of the change in their labor supply is virtually zero, we note that it cannot be that the decline in their fertility is caused by their increased labor supply (as such an increase did not occur).

Appendix G Education Two Estimators and Block Bootstrap Standard Errors

In this appendix, we first delve into the robustness exercise discussed briefly in Section 5.2.1 using the two-step estimator on education. We then discuss why this estimator runs into some mild issues. Finally, we describe our block-bootstrapping procedure for calculating standard errors in our two step estimators.

Table A.1 reports the results of the robustness exercise for education using the two-step estimator. However, there is an issue with using this estimator on the education data. As can be seen in Figure 4, there is a sharp increase in school attendance around 1910, suggesting a potential sharp change in the relationship between education and various controls. The two-step estimator is thus problematic: all fixed effects are estimated on a sample of states that had not yet given women rights. There were very few such states remaining by 1910. As such, we might expect that the two-step estimator is not 100% accurate for addressing data in 1910-1920. This would mostly affect our estimates for the impact of rights on education 20 and 30 years after rights are granted. Furthermore, this would be especially problematic for older children, who started attending school in higher numbers in later decades.

We proceed as follows. Columns 1-3 show the results of our two-step estimator using data from 1850-1920, where Column 1 of Table A.1 repeats Column 1 from Table 10 (children of all ages), Column 2 repeats Column 4 from Table 10 (younger children), and Column 3 repeats Column 7 from Table 10 (older children).[102] As can be seen, the impact of rights on education is much lower than the benchmark exercise, and even negative two and three decades after rights for all children

[101]Column 3 of Panel A actually finds a (very small) decrease in LFP that is statistically significant at the 5% level. However, this result is not robust.

[102]We repeat the least-demanding specifications since our block-bootstrapping procedure is incredibly computationally expensive, and there is very little difference in the estimates between the specifications. Additionally, we do not interact our controls with year fixed effects. This both further lightens the computational load and helps avoid the issue discussed above with relatively little data in later years.

and older children. For younger children, the impact of rights on education is weaker, though still large and significant, than in the benchmark exercise when rights are given and two decades after but very similar one decade after rights are granted. The estimated impact of rights is negative three decades after rights are granted.

To address this issue, we restrict attention to data from 1850-1900. However, this restriction hurts our ability to use an event-study approach, since it limits the range of time we can examine states before and after rights were granted. As such, we change our main explanatory variable $Rights_{st}^{k}$ to $Rights_{st}$, simply indicating whether state s had granted rights by year t. Columns 4-6 of Table A.1 repeat Columns 1-3, but use our two-step estimator only on data from 1850-1900 and $Rights_{st}$ rather than $Rights_{st}^{k}$. The estimate in Column 4 is 0.019, suggesting that children 8-17 increased their school attendance by about 2 percentage points due to women's rights, but this estimate is not significant. Column 5 finds that younger children increased their attendance by 4.4 percentage points, and this estimate is significant. Column 6 finds a negative, but not significant, point estimate for older children. These results are consistent with the general message in Section 5.2 that women's rights increased education, and mostly for young children prior to 1900, and older children only afterwards.[103] This supports the notion that that the results in our two-step estimator are indeed driven by data from 1850-1900, and that perhaps data from 1910 and 1920, if properly treated, would help explain the difference with the benchmark results regarding the effects two and three decades after rights are granted.

We now discuss the bootstrapping procedure used to calculate the standard errors reported using our two step estimators in the event study. Our process is as follows:

1. Randomly draw, with replacement, a set of 47 states.
2. Keep all counties on the borders between these states and their neighbors. If a state is drawn more than once, it receives a new fixed effect every time it is drawn.
3. Rerun our two-step estimator.
4. Repeat steps 1-3 two hundred times. The standard error reported is the standard deviation of the estimates across these 200 estimates.

Appendix H Extra Figures

In this appendix, we present extra visualizations of the data.

We begin with Figure A.11, which compares the timing of women's rights by state with the timing of women's suffrage. We find no relationship, suggesting that feminism was not a driving force behind women's rights, as discussed in Section 3.3.

We next show figures illustrating the results of our event study analyses. Figures A.12 and A.14 illustrate the main event study findings for fertility, when the dependent variable is whether the wife gave birth last year (Table 5, Column 2) and the number of kids under 5 (Table 6, Column 2), respectively. Figure A.16 visualizes our benchmark findings for the event-study analyzing education of children 8-13 (Table 10, Column 5). Finally, Figure A.17 repeats Figure A.16, but compares the effect of women's rights to all observations prior to women's rights together, rather than 10 years before rights were granted as a benchmark.

Finally, Figure A.18 visualizes the results of Table 9.

[103] As a reminder, this later point is emphasized in Section 5.2.2.

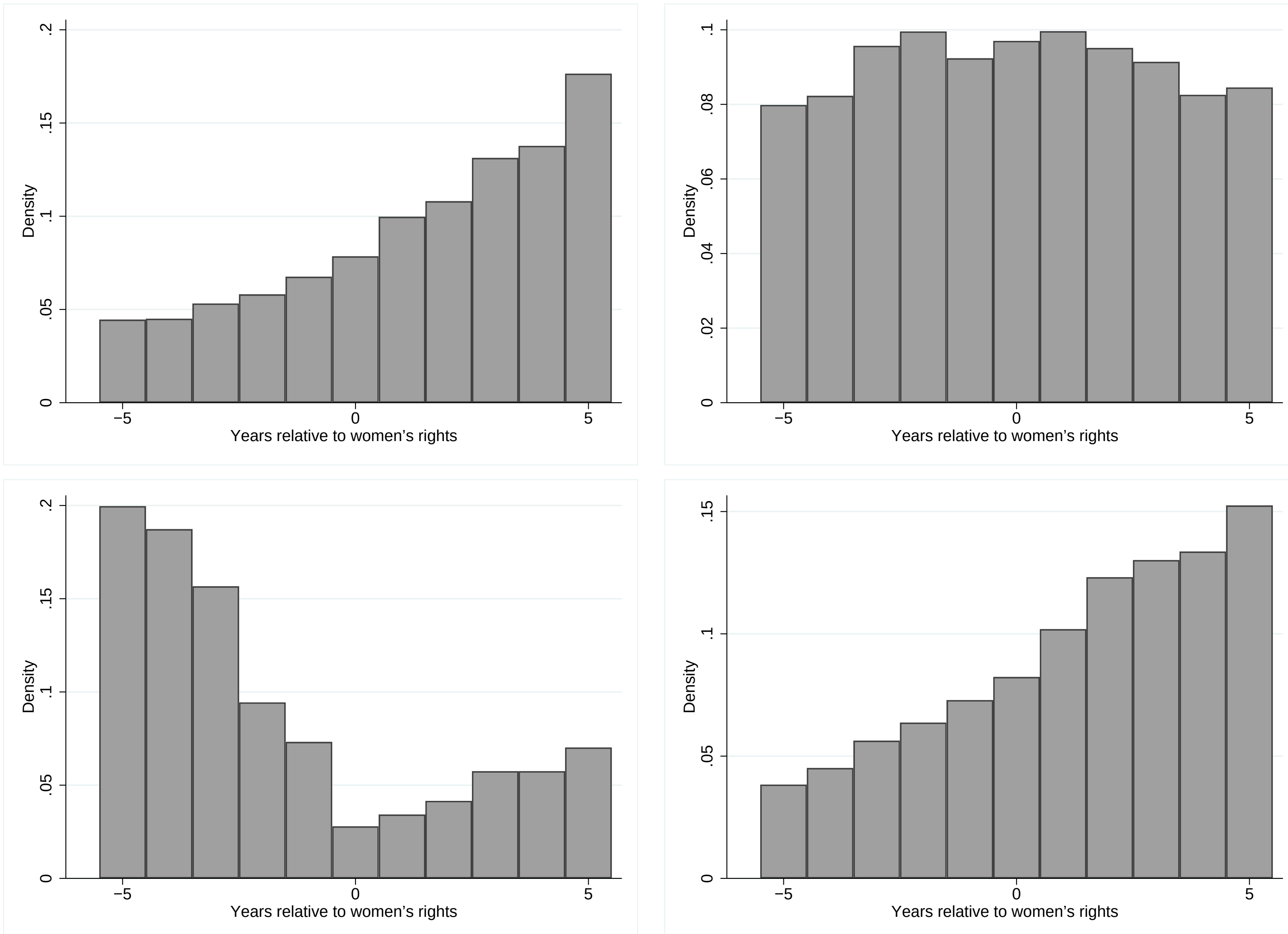

Figure A.1: The top panel left panel shows the number of people getting married, relative to the year their state gave rights, in the 1900 US census, when limiting to white non-Hispanic couples, where the wife is 20-39 years old. The top right panel does the same for couples where the wife is 45-59. The bottom left and bottom right repeat this patter using the 1910 census.

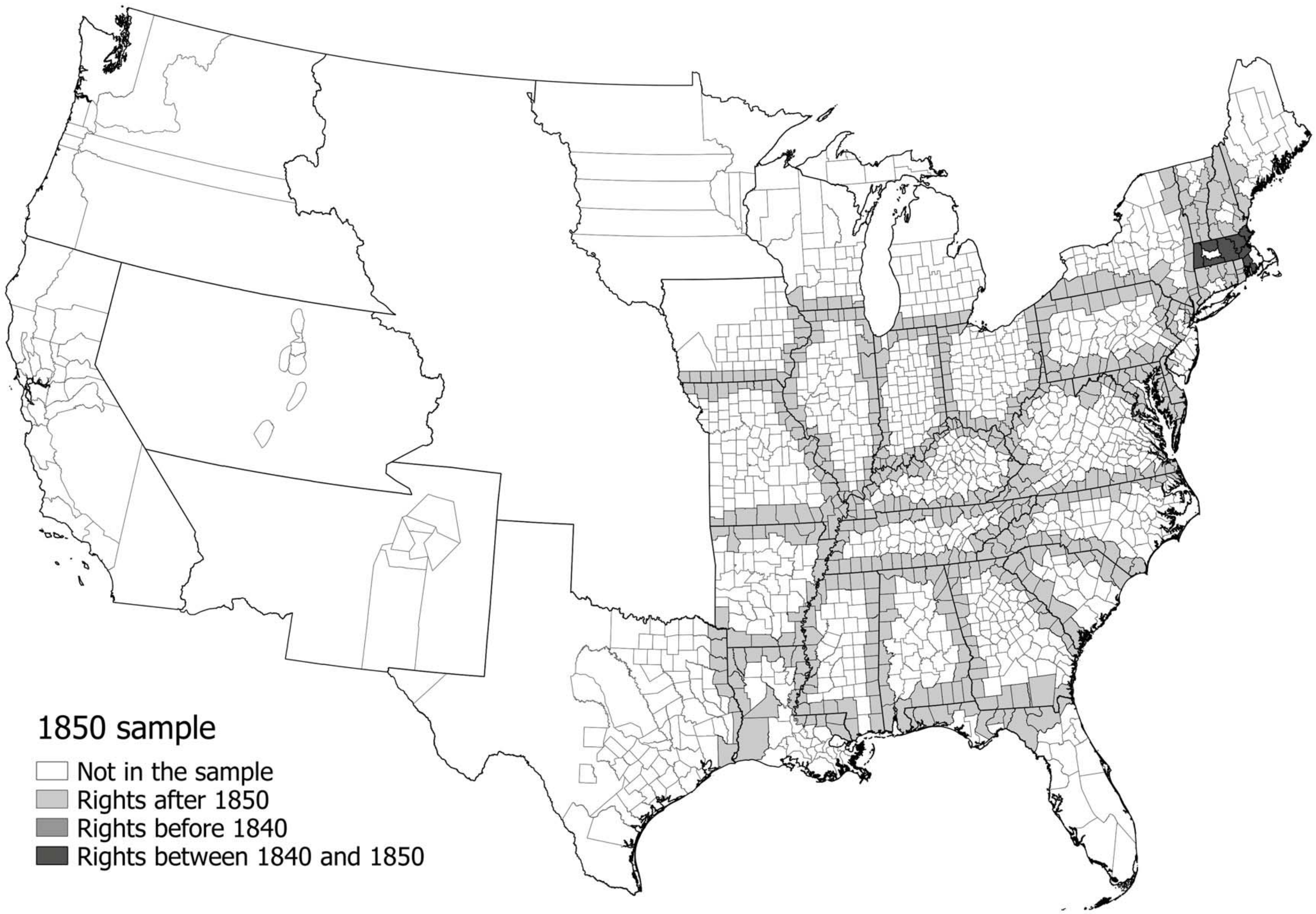

Figure A.2: **State borders, 1850.**

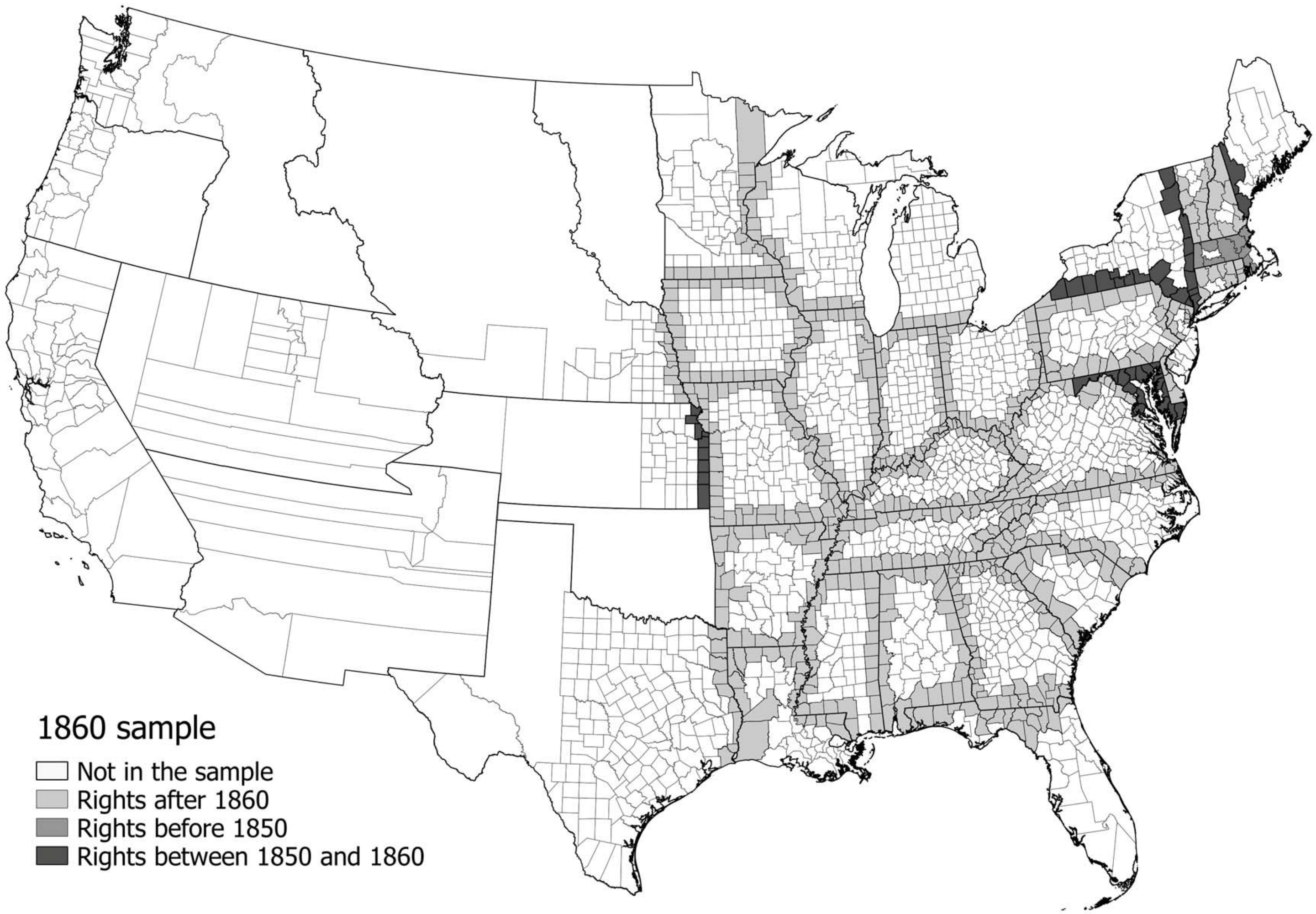

Figure A.3: **State borders, 1860.**

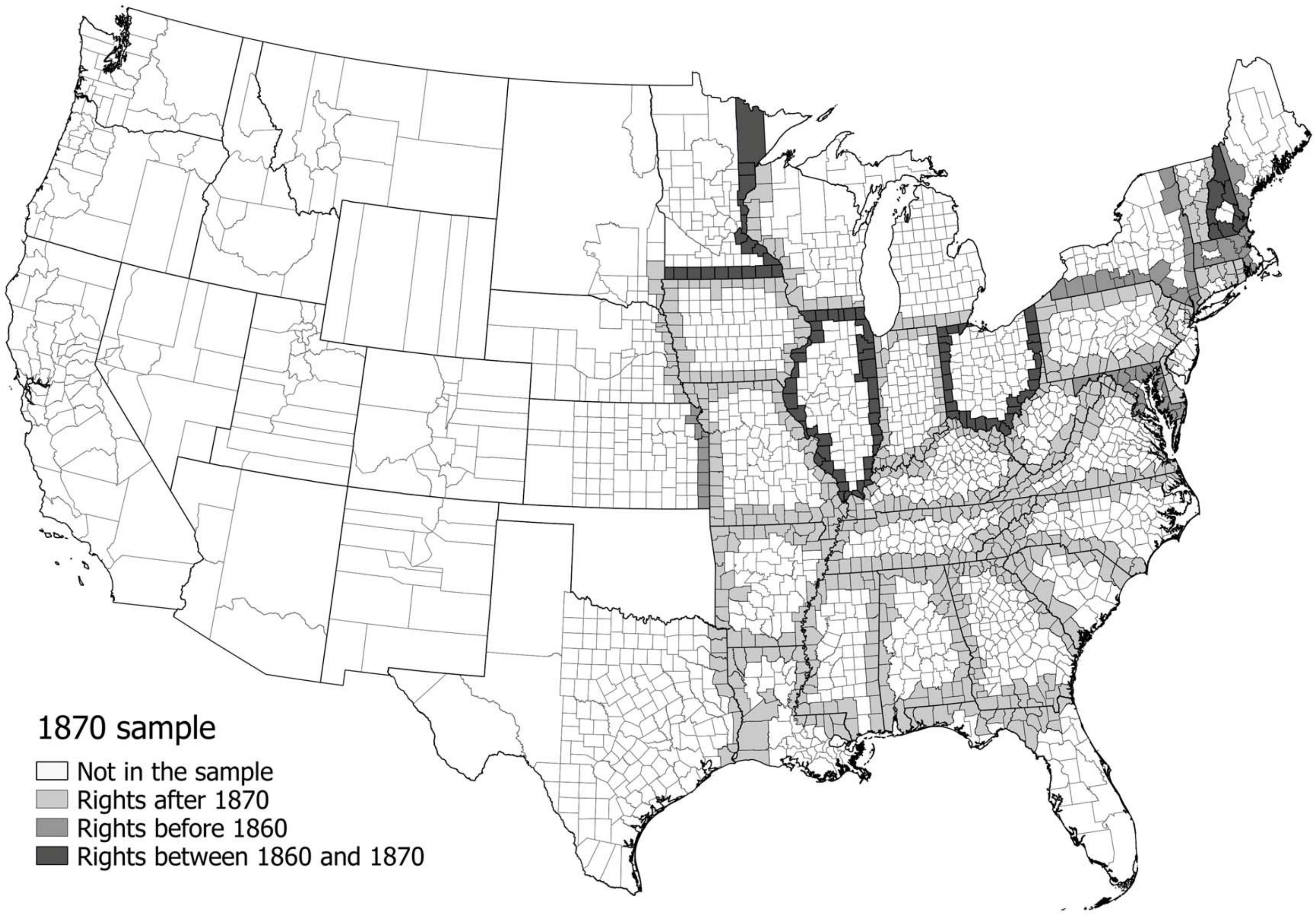

Figure A.4: **State borders, 1870.**

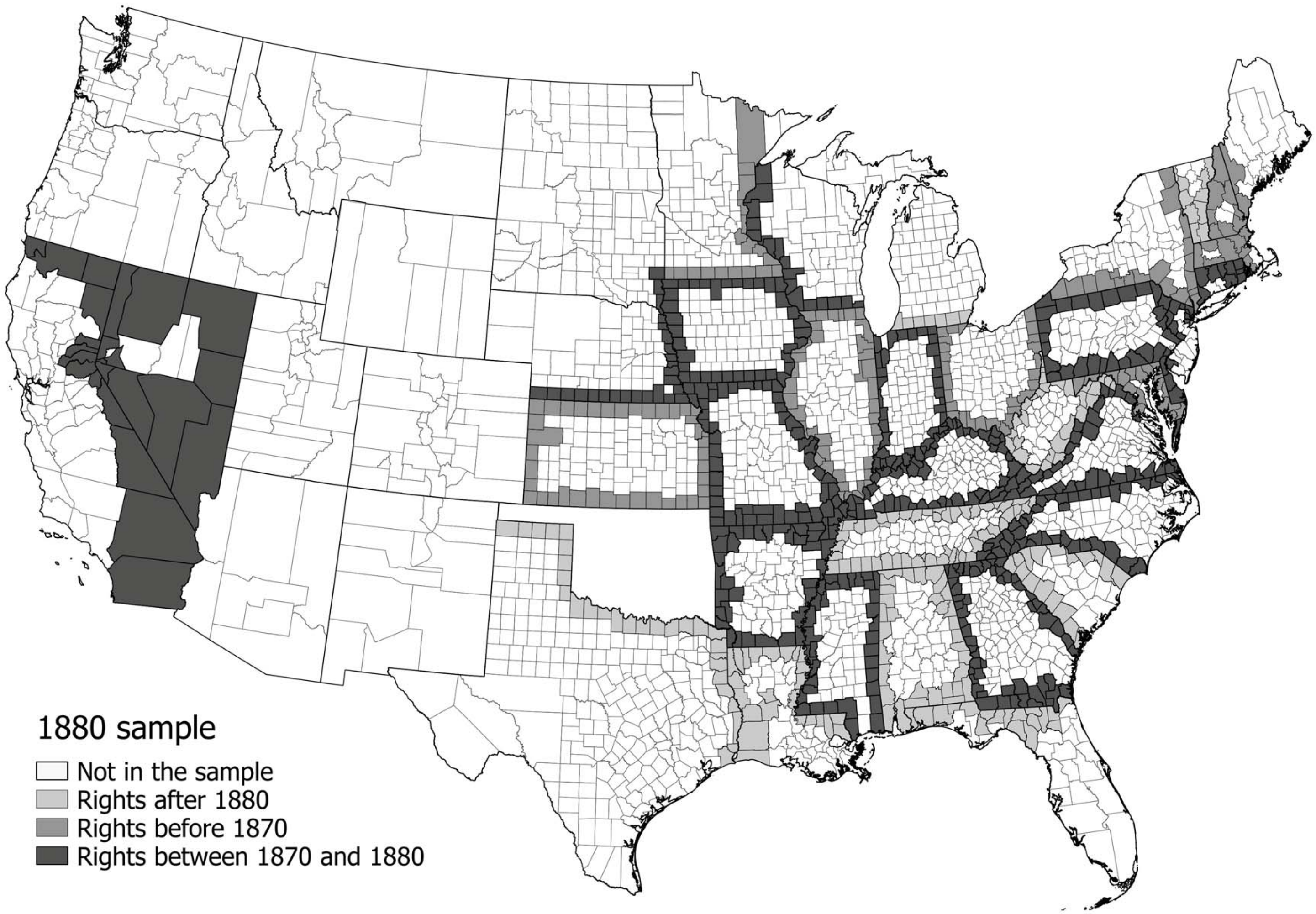

Figure A.5: **State borders, 1880.**

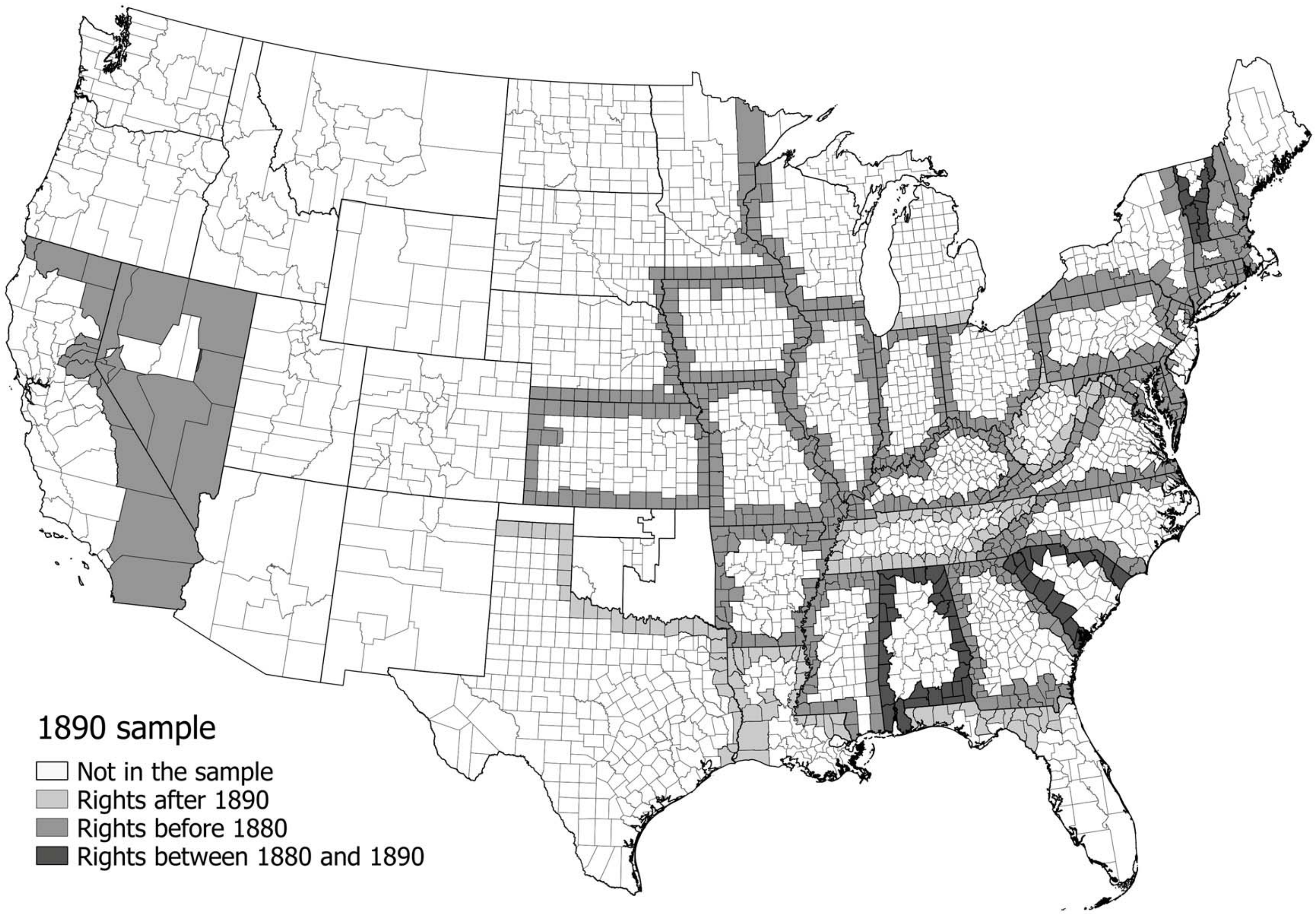

Figure A.6: **State borders, 1890.**

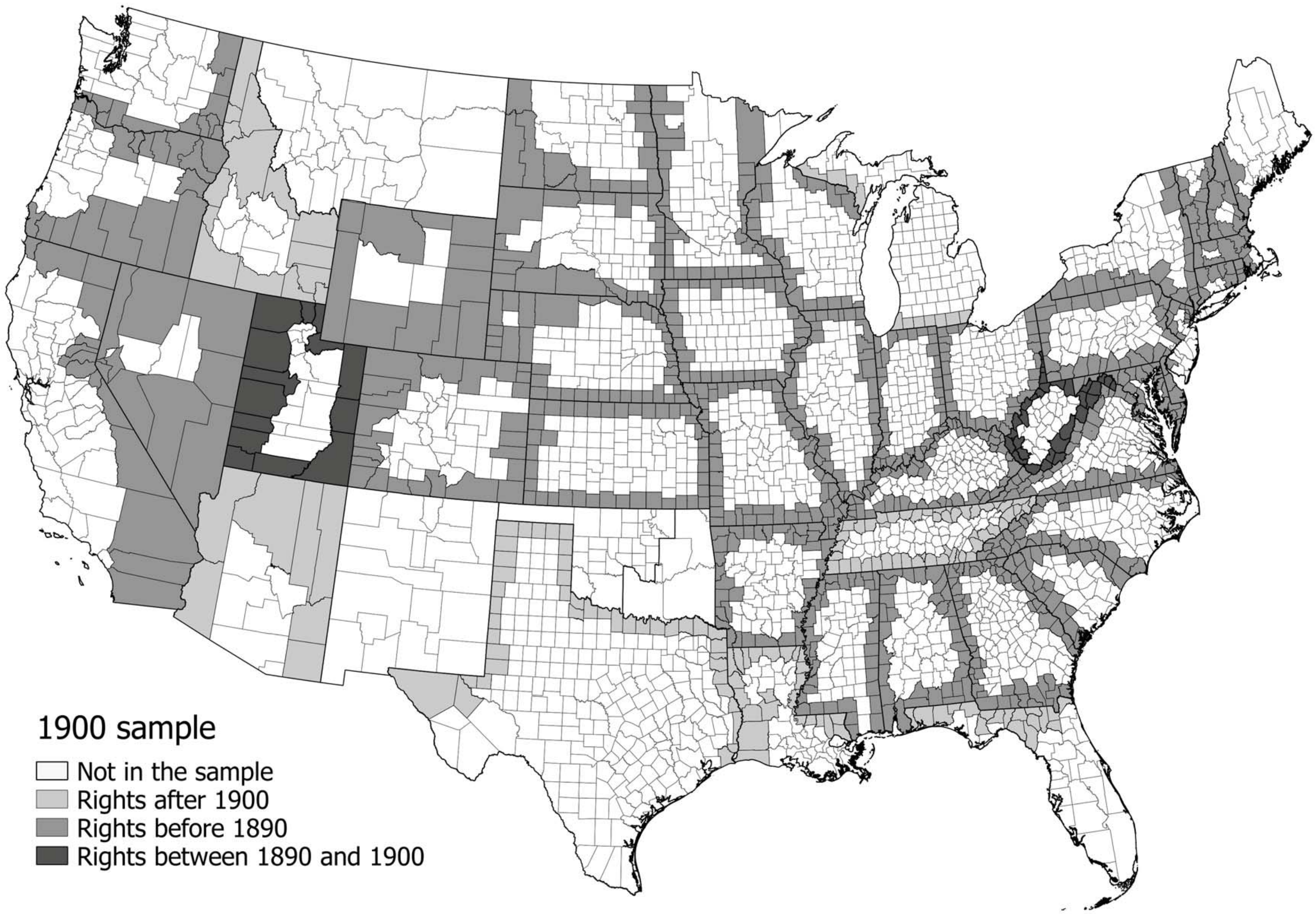

Figure A.7: **State borders, 1900.**

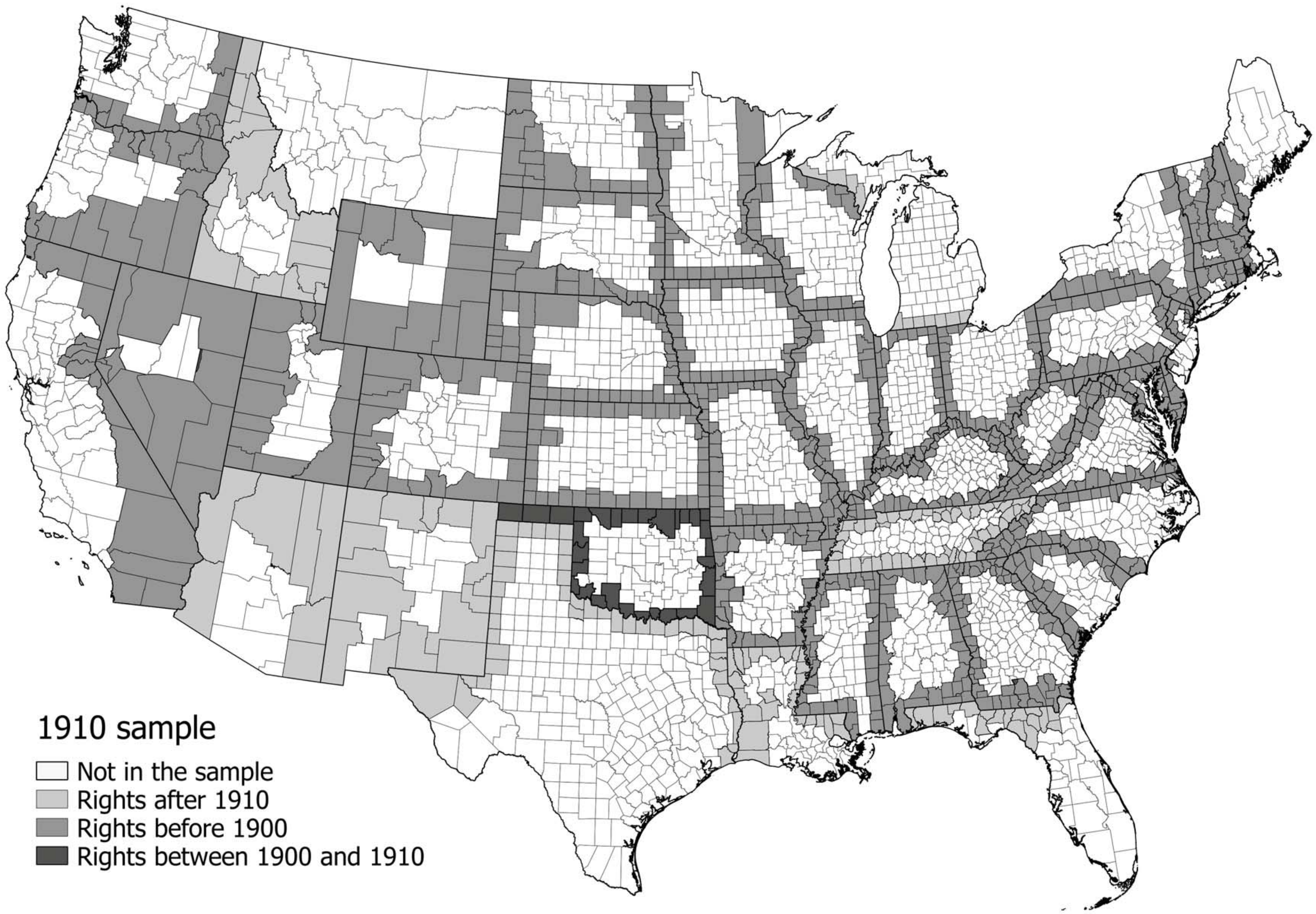

Figure A.8: **State borders, 1910.**

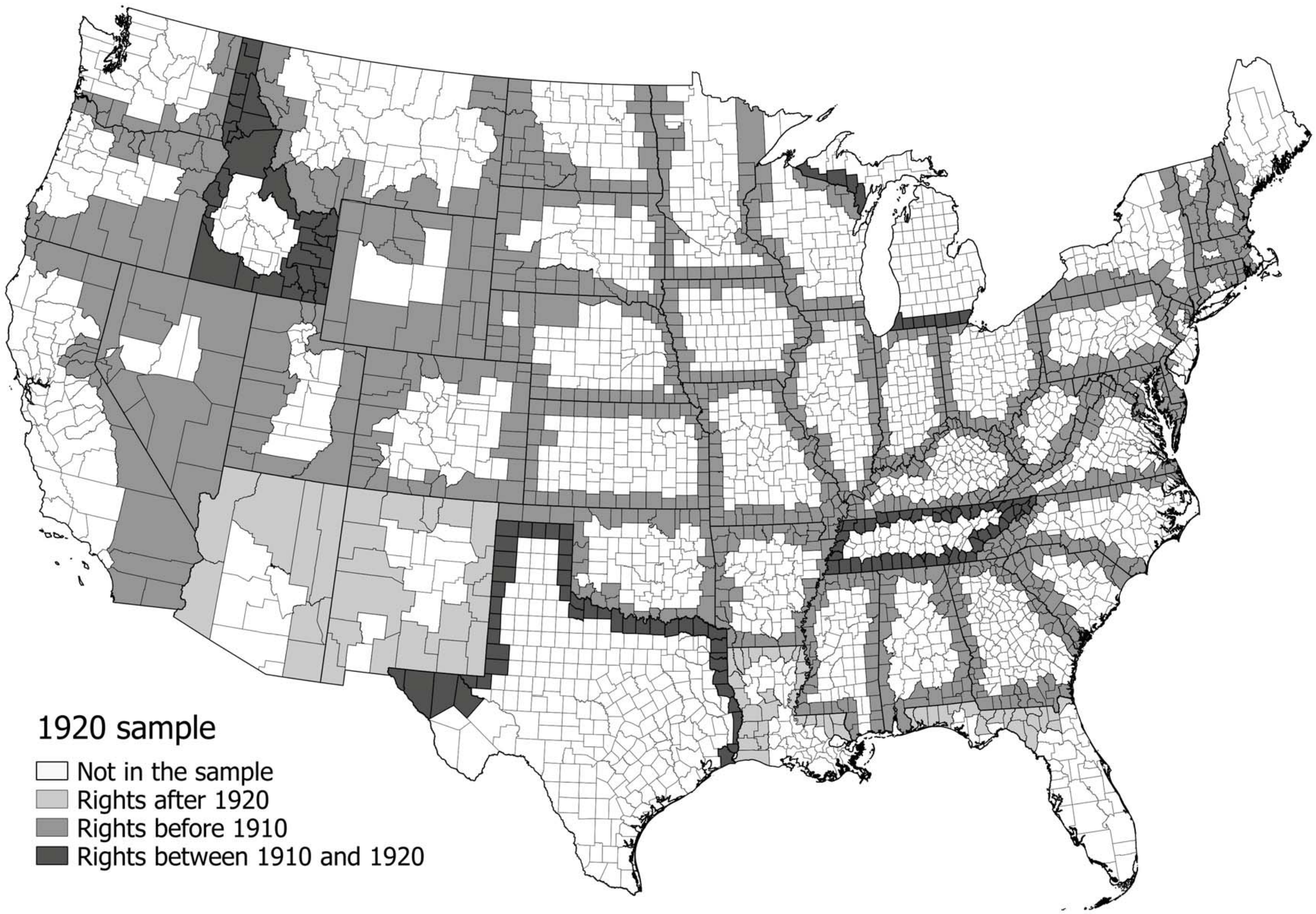

Figure A.9: **State borders, 1920.**

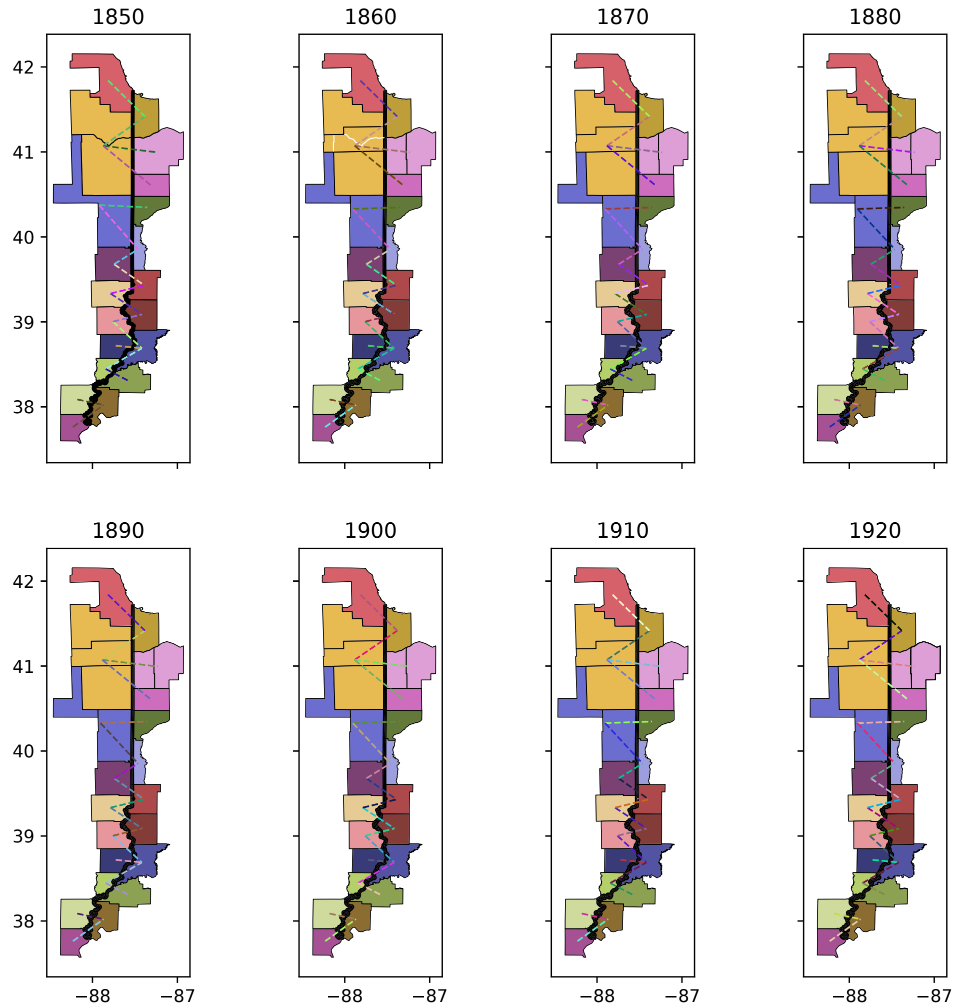

Figure A.10: **State borders, example.** This figure shows the evolution of the county-border pairs between Indiana (right) and Illinois (left) over time.

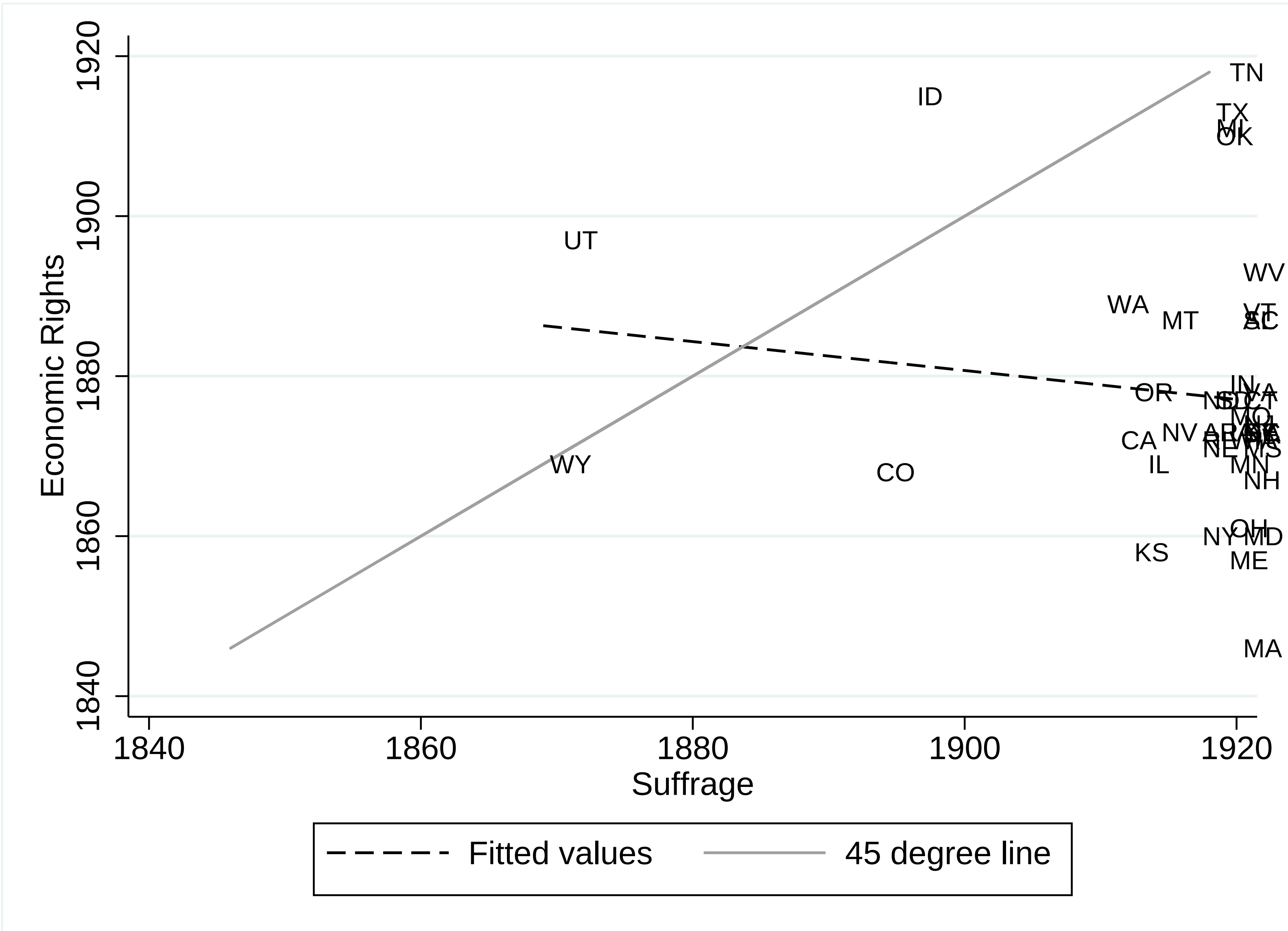

Figure A.11: **Women's economic rights and suffrage, by state.**

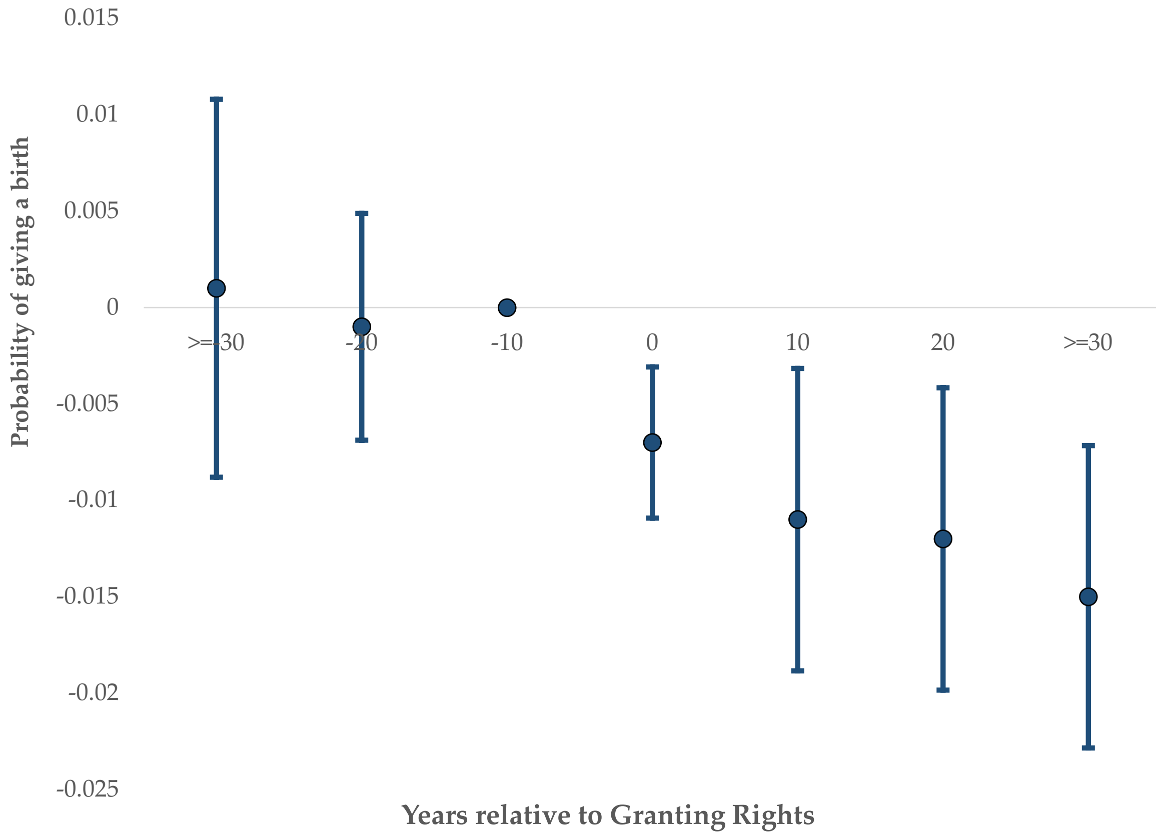

Figure A.12: **Event-Study: Probability of Birth** This figure shows the estimates and 95% confidence interval for the event-study showing the relationship between women's rights and the probability of giving birth. In particular, these estimates are from Column 2 of Table 5. For more details, see Section 5.1.1.

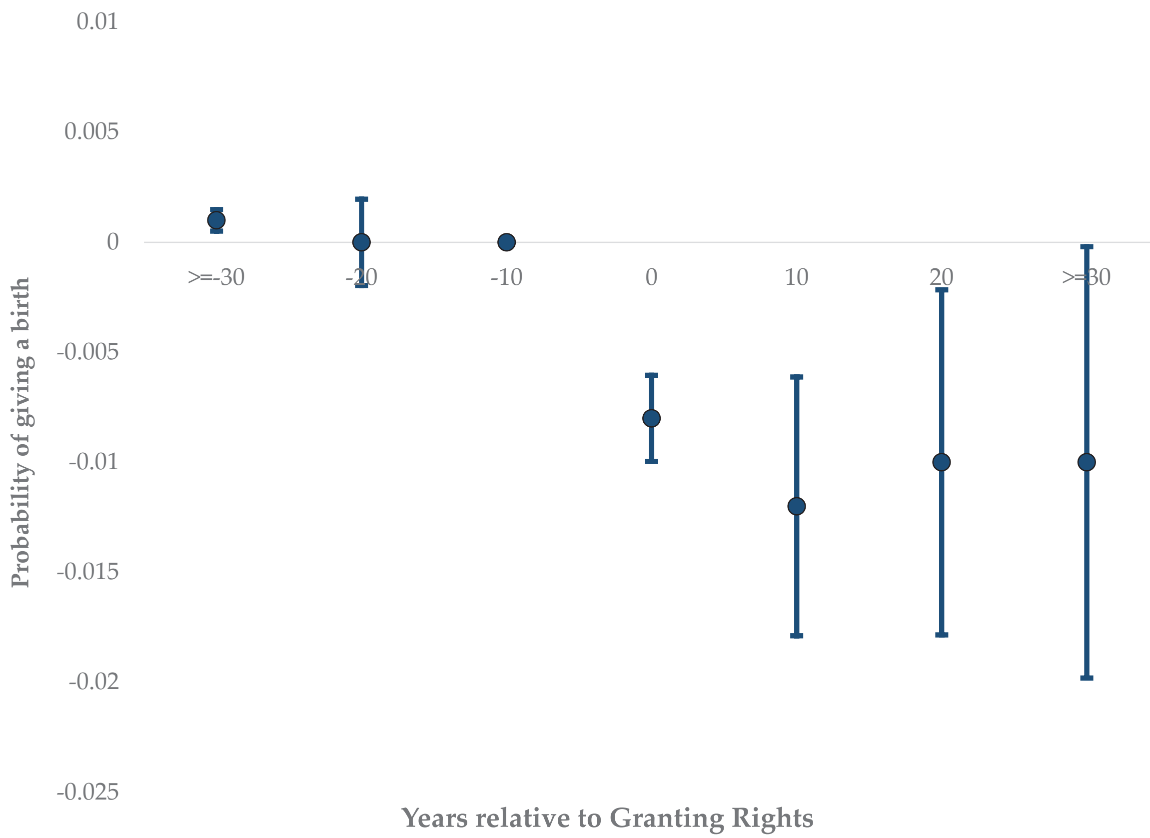

Figure A.13: **Event-Study: Probability of Birth, Two-Step Estimator** This figure shows the estimates and 95% confidence interval for the event-study showing the relationship between women's rights and the probability of giving birth. In particular, these estimates are from Column 5 of Table 5. For more details, see Section 5.1.1.

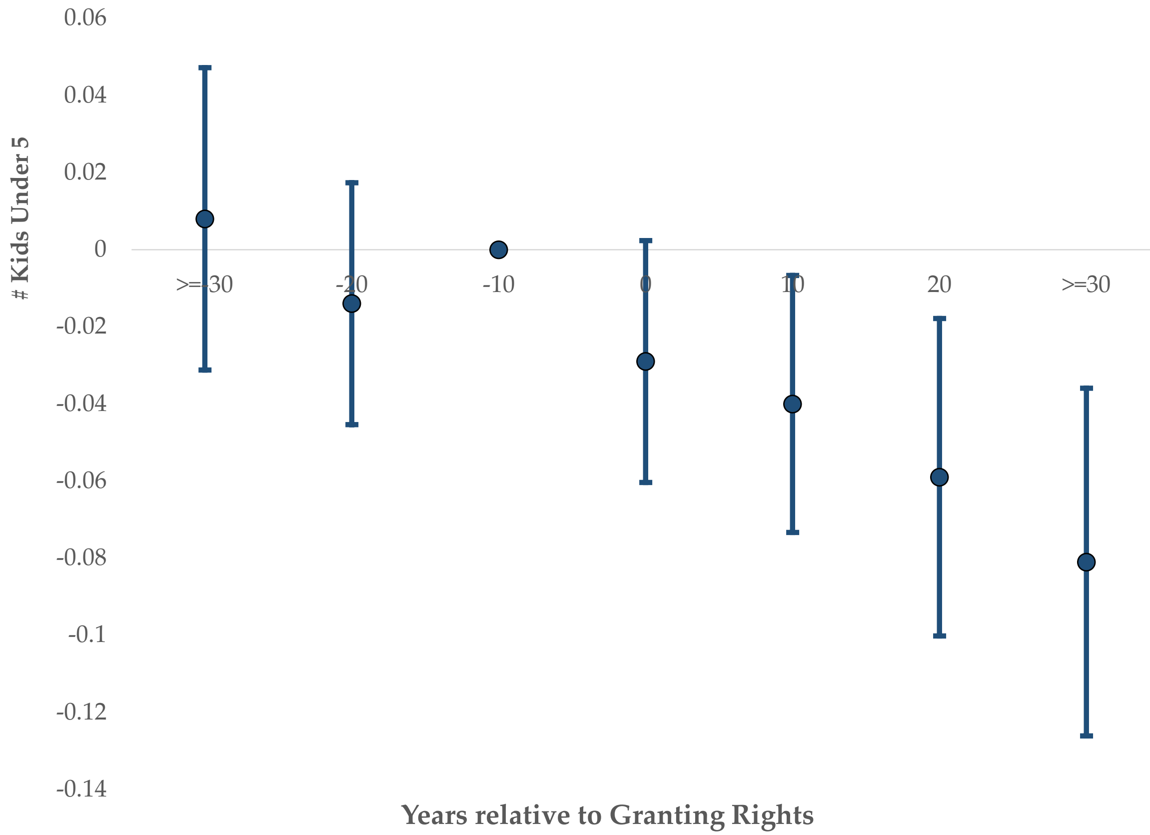

Figure A.14: **Event-Study: Number of Children Under 5** This figure shows the estimates and 95% confidence interval for the event-study showing the relationship between women's rights and the number of children under age 5. In particular, these estimates are from Column 2 of Table 6. For more details, see Section 5.1.1.

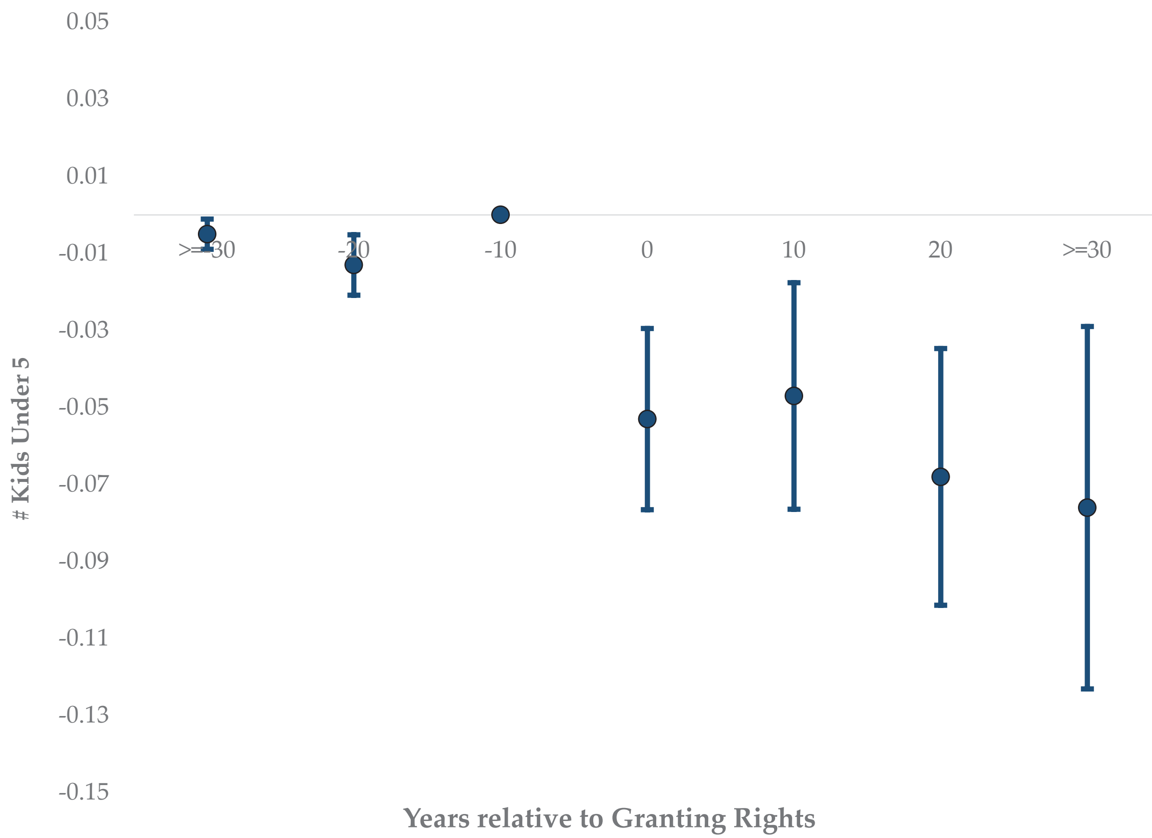

Figure A.15: **Event-Study: Number of Children Under 5, Two-Step Estimator** This figure shows the estimates and 95% confidence interval for the event-study showing the relationship between women's rights and the number of children under age 5. In particular, these estimates are from Column 5 of Table 6. For more details, see Section 5.1.1.

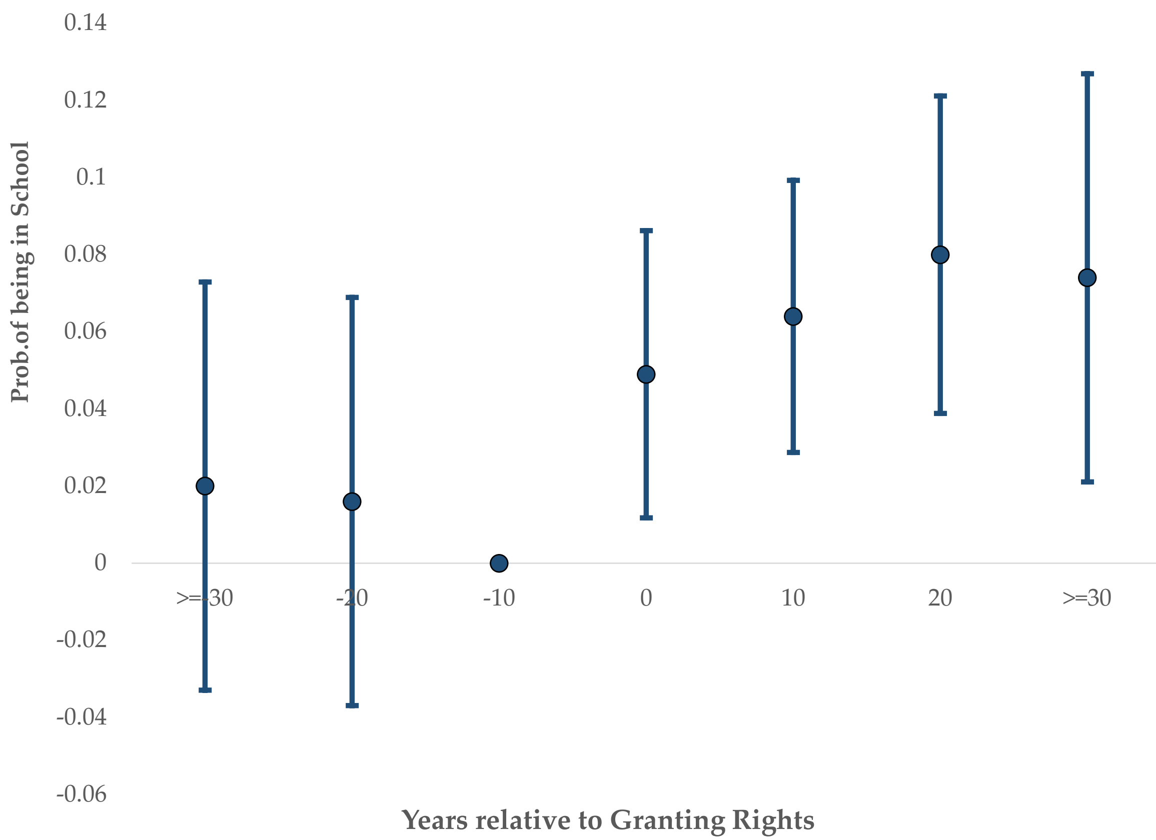

Figure A.16: **Event Study: Education, 8-13 years old.** This figure shows the estimates and 95% confidence interval for the event-study showing the relationship between women's rights and the probability a child age 8-13 is in school. In particular, these estimates are from Column 5 of Table 10. For more details, see Section 5.2.1.

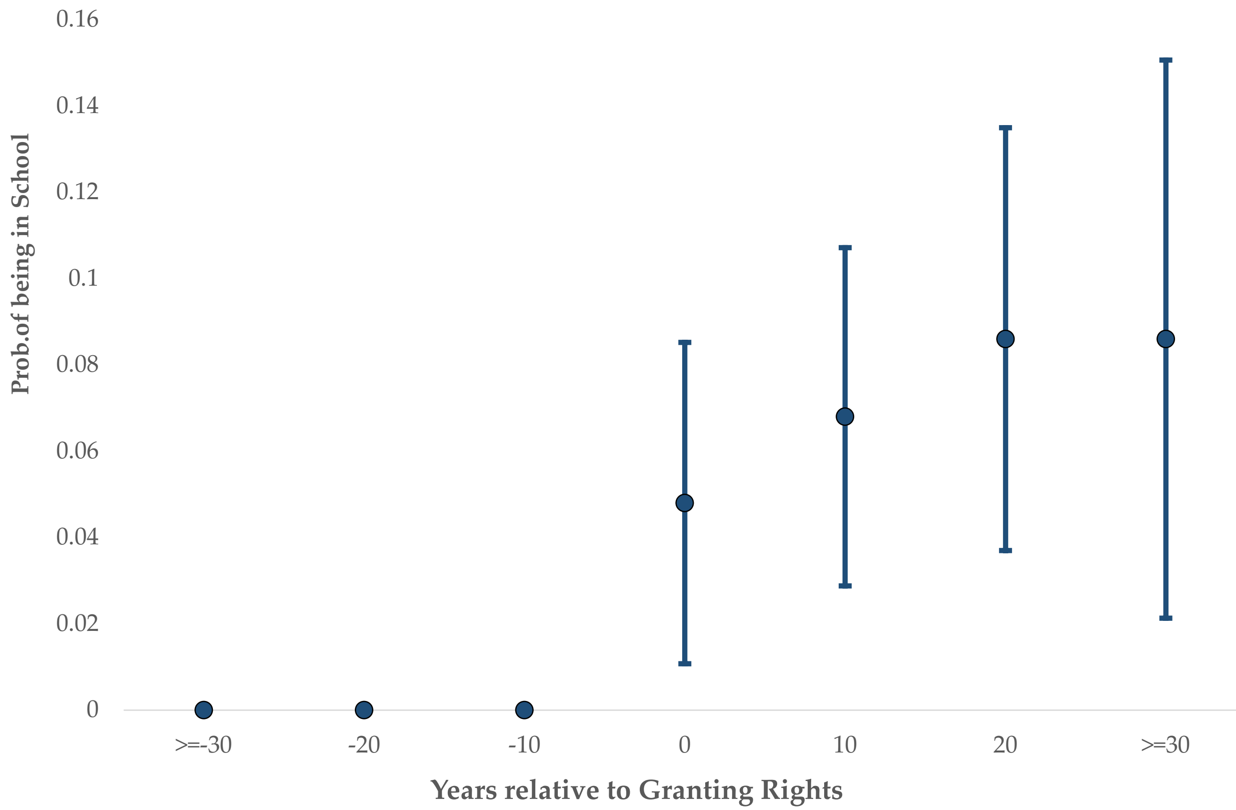

Figure A.17: **Event Study: Education, 8-13 years old, partial.** This figure shows the estimates and 95% confidence interval for the event-study showing the relationship between women's rights and the probability a child age 8-13 is in school. Unlike Figure A.16, here we show the dynamic effects on education as compared to all the data prior to rights, rather than just 10 years prior to rights being granted. For more details, see Section 5.2.1.

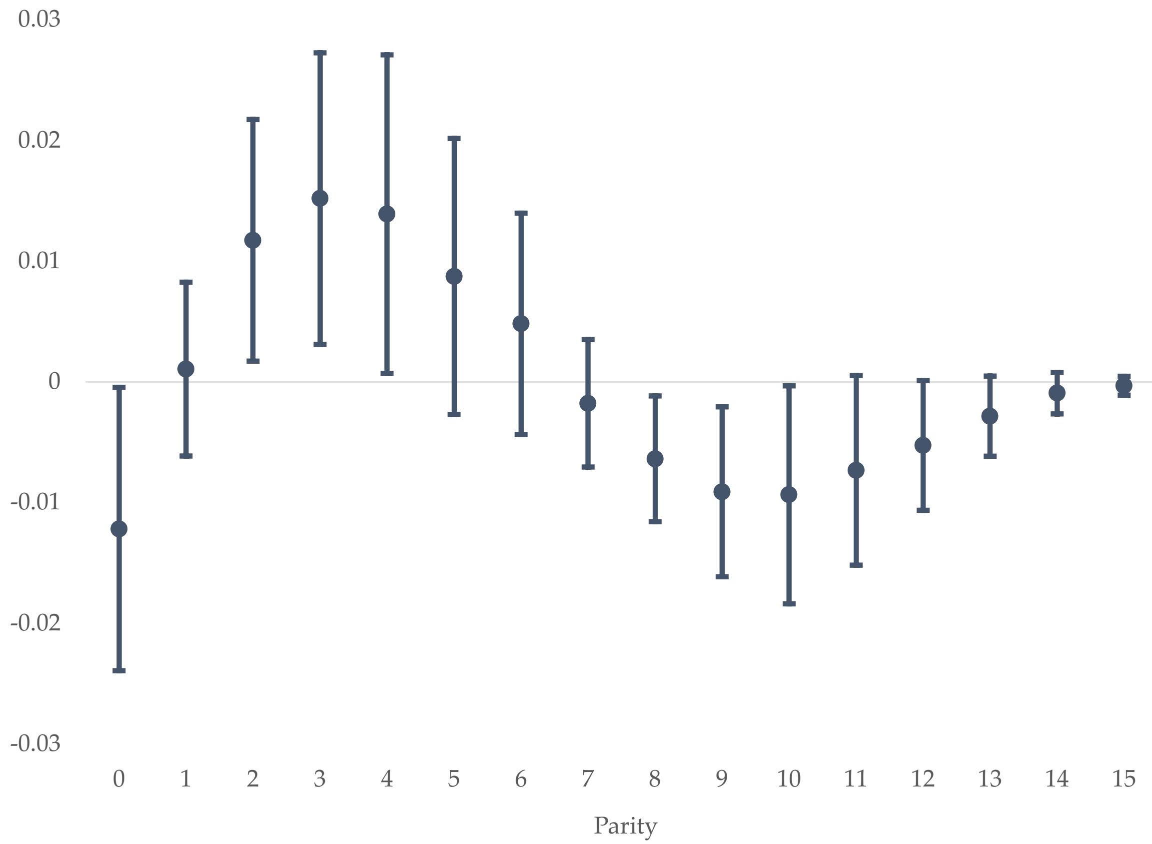

Figure A.18: **Changes in Parity** The X-axis is the parity of completed fertility, measured by children ever born, for women age 45-59 in 1900 and 1910. The Y-axis measures the difference in the fraction of households of a given parity for those married after rights as compared to those married before rights, after controls, as described in Section 5.1.2. For those married after rights, there is a rise in smaller households and a decline in larger households.

Table A.1: School, 1850-1920

Dep. Var.	Probability of Being in School					
Years Included:	1850-1920			1850-1900		
Children's Age	8-17	8-13	14-17	8-17	8-13	14-17
	(1)	(2)	(3)	(4)	(5)	(6)
Rights	–	–	–	0.019	0.044***	-0.014
				(0.015)	(0.016)	(0.015)
$\geq$ 3 Decades Before	0.004**	0.002	0.005**	–	–	–
	(0.002)	(0.002)	(0.002)	–	–	–
2 Decades Before	-0.006	-0.007	-0.004	–	–	–
	(0.005)	(0.005)	(0.005)	–	–	–
1 Decade Before	0	0	0	–	–	–
Rights Given	0.006	0.018*	-0.002	–	–	–
	(0.010)	(0.011)	(0.010)	–	–	–
1 Decade After	0.040***	0.074 ***	0.018	–	–	–
	(0.011)	(0.010)	(0.016)	–	–	–
2 Decades After	-0.022	0.036*	-0.037*	–	–	–
	(0.020)	(0.021)	(0.021)	–	–	–
$\geq$ 3 Decades After	-0.144***	-0.042	-0.135	–	–	–
	(0.029)	(0.030)	(0.028)	–	–	–
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	17,290,216	11,469,376	5,820,840	6,754,537	4,579,407	2,175,130
Mean Dep. Var.	0.80	0.80	0.80	0.74	0.74	0.74

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state levels, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include the child's age, gender, mother's age and father's age fixed effects. The sample includes children age 6-18 who are sons of white, non-Hispanic mothers, age 20-60, with husbands up to age 70, who live in the same state in which they were born.

Table A.2: Randomization Exercise, Birth & # of Kids Under 5, 1850-1920

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Dependent Variable is Birth					
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
$\geq$ 3 Decades Before	0.000	0.005	-0.013	0.016	0.000	0.501
2 Decades Before	0.000	0.003	-0.009	0.010	-0.001	0.381
Right Given	0.000	0.003	-0.010	0.008	-0.007	0.006
1 Decade After	0.000	0.005	-0.014	0.016	-0.010	0.016
2 Decades After	0.000	0.006	-0.020	0.019	-0.012	0.020
$\geq$ 3 Decades After	0.000	0.009	-0.031	0.026	-0.015	0.035
	Panel B: Dependent Variable is # of Kids Under 5					
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
$\geq$ 3 Decades Before	0.001	0.021	-0.057	0.075	0.005	0.579
2 Decades Before	0.000	0.015	-0.040	0.054	-0.014	0.166
Right Given	0.001	0.015	-0.044	0.052	-0.028	0.022
1 Decade After	0.000	0.024	-0.067	0.090	-0.037	0.058
2 Decades After	-0.000	0.030	-0.087	0.102	-0.056	0.028
$\geq$ 3 Decades After	-0.001	0.042	-0.146	0.143	-0.080	0.026

Notes: Distribution of 1,000 estimates on randomly assigned dates that each state gave rights, and rerun the estimates from Column (1) in Tables (5) and (6). "Our estimate" is the estimated parameter value using the dates that women were actually granted rights. *p-value* is the fraction of estimates in the randomization exercise that are equal or smaller than "our estimate".

Table A.3: Randomization Exercise, Married After Rights: Fertility (Panel A) and Schooling (Panel B), 1900-1910

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Fertility					
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
Birth	-0.001	0.005	-0.017	0.011	-0.010	0.044
# of Kids Under 5	-0.005	0.043	-0.165	0.094	-0.143	0.002
Children Ever Born	-0.003	0.127	-0.368	0.464	-0.234	0.030
Surviving Children	-0.003	0.104	-0.289	0.398	-0.180	0.033
	Panel B: Schooling					
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
Ages 8-17	0.003	0.005	-0.017	0.017	0.009	0.132
Ages 8-13	0.001	0.004	-0.011	0.013	0.003	0.347
Ages 14-17	0.007	0.006	-0.013	0.021	0.022	0.000

Notes: Distribution of 1,000 estimates on randomly assigned dates that each state gave rights. Panel A reruns the estimates from Column (2) in Tables 7 (Panels A and B) and 8 (Panels A and B). Panel B reruns the estimates for Columns 1, 3, and 5 of Panel A of Table 12. "Our estimate" is the estimated parameter value using the dates that women were actually granted rights. *p-value* is the fraction of estimates in the randomization exercise that are equal or smaller than "our estimate".

Table A.4: Randomization Exercise, Probability of Being in School, 1850-1920

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean	Std. Dev.	Min	Max	"Our Estimate"	*p-value*
$\geq$ 3 Decades Before	0.000	0.034	-0.099	0.109	0.021	0.255
2 Decades Before	0.000	0.025	-0.069	0.074	0.017	0.251
Right Given	-0.001	0.019	-0.061	0.058	0.048	0.003
1 Decade After	-0.000	0.028	-0.088	0.090	0.063	0.005
2 Decades After	-0.001	0.031	-0.097	0.112	0.079	0.003
$\geq$ 3 Decades After	-0.000	0.040	-0.108	0.135	0.069	0.043

Notes: Distribution of 1,000 estimates on randomly assigned dates that each state gave rights, and rerun the estimates from Column (4) in Tables (10). "Our estimate" is the estimated parameter values using the dates that women were actually granted rights. *p-value* is the fraction of estimates in the randomization exercise that are equal or smaller than "our estimate".

Table A.5: Labor Force Participation Ages 20-39, 1860-1920

Dependent Variable	Labor Force Participation			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.013	-0.007	-0.005	-0.008
	(0.017)	(0.014)	(0.015)	(0.015)
2 Decades Before	-0.000	0.001	0.005	0.001
	(0.009)	(0.008)	(0.008)	(0.008)
1 Decade Before	0	0	0	0
Rights Given	0.004	0.004	0.006	0.004
	(0.004)	(0.003)	(0.004)	(0.004)
1 Decade After	0.006	0.004	0.005	0.005
	(0.008)	(0.007)	(0.008)	(0.008)
2 Decades After	0.019**	0.013	0.015*	0.014*
	(0.009)	(0.008)	(0.008)	(0.008)
$\geq$ 3 Decades After	0.012	0.006	0.006	0.007
	(0.013)	(0.011)	(0.012)	(0.012)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes
Sample	All	All	No South	No CP
N	13,705,569	13,705,569	11,070,259	13,196,290
Adj.R^2	0.037	0.050	0.047	0.051
Mean Dep. Var.	0.03	0.03	0.04	0.03

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include fixed effects for both the wife's and husband's ages, interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 3 excludes all borders of Southern States with non-Southern States. Column 4 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic women, age 20-39, married to men up to 50 years old, who live in the same state in which they were born.

Table A.6: Labor Force Participation Ages 40-59, 1860-1920

Dependent Variable	Labor Force Participation			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.017	-0.014	-0.013	-0.015
	(0.017)	(0.015)	(0.016)	(0.016)
2 Decades Before	-0.004	-0.003	0.001	-0.004
	(0.009)	(0.009)	(0.010)	(0.009)
1 Decade Before	0	0	0	0
Rights Given	0.001	0.002	0.004	0.002
	(0.004)	(0.003)	(0.004)	(0.004)
1 Decade After	0.001	0.001	0.002	0.001
	(0.008)	(0.007)	(0.008)	(0.007)
2 Decades After	0.011	0.009	0.012	0.010
	(0.008)	(0.007)	(0.008)	(0.008)
$\geq$ 3 Decades After	0.009	0.006	0.008	0.008
	(0.011)	(0.010)	(0.011)	(0.010)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes
Sample	All	All	No South	No CP
N	6,787,373	6,787,373	5,399,072	6,634,776
Adj.R^2	0.047	0.058	0.057	0.059
Mean Dep. Var.	0.03	0.03	0.03	0.03

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are double clustered at the county-border pair and state levels, in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include fixed effects for both the wife's and husband's ages, interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 3 excludes all borders of Southern States with non-Southern States. Column 4 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic women, age 20-39, married to men up to 50 years old, who live in the same state in which they were born.

Table A.7: Labor Force Participation, Married After Rights 1900-1910

Dependent Variable:	Labor Force Participation					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Women Aged 45-59					
Married After Rights	0.001	0.000	-0.001	0.000	-0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	2,266,313	2,266,292	1,602,073	2,185,335	969,420	1,296,872
Adj.R^2	0.0236	0.0502	0.0456	0.0495	0.0474	0.0479
Mean Dep. Var.	0.0427	0.0427	0.0393	0.0424	0.0307	0.0517
Panel B	Women Aged 20-39					
Married After Rights	0.001	0.001	-0.001	0.001	0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,258,587	7,258,567	5,096,244	6,746,354	3,219,519	4,039,048
Adj.R^2	0.0546	0.0625	0.0276	0.0589	0.0354	0.0662
Mean Dep. Var.	0.03	0.03	0.02	0.03	0.02	0.04

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level in parentheses. All specifications include county-year fixed effects and state-year fixed effects. "Controls" include wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects, interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, who live in the same state in which they were born. Panel A restricts attention to women age 45-59, while Panel B restricts attention to women age 20-39.

Table A.8: R^2 and Adjusted R^2: Regressing County level outcome on State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1850	1860	1870	1880	1900	1910	1920
				Panel A: Birth Last Year			
N	1,492	1,774	2,094	2,412	2,756	2,947	3,063
R^2	0.1584	0.0877	0.0978	0.1108	0.2184	0.3560	0.3013
Adjusted R^2	0.1394	0.0693	0.0780	0.0939	0.2052	0.3456	0.2904
				Panel B: # of Kids Under Age 5			
N	1,492	1,774	2,094	2,412	2,756	2,947	3,063
R^2	0.3386	0.2549	0.2363	0.4067	0.4551	0.5348	0.4501
Adjusted R^2	0.3236	0.2399	0.2196	0.3954	0.4458	0.5273	0.4415
				Panel C: Probability of Being in School, Ages 8-17			
N	1,458	1,704	1,998	2,317	2,509	2,935	3,061
R^2	0.3788	0.3669	0.5718	0.4656	0.2512	0.4137	0.1838
Adjusted R^2	0.3648	0.3536	0.5628	0.4550	0.2369	0.4042	0.1711
				Panel D: Probability of Being in School, Ages 8-13			
N	1,454	1,699	1,996	2,307	2,502	2,932	3,060
R^2	0.3975	0.3840	0.5800	0.4938	0.2929	0.4580	0.1957
Adjusted R^2	0.3840	0.3710	0.5712	0.4838	0.2794	0.4492	0.1831
				Panel E: Probability of Being in School, Ages 14-17			
N	1,396	1,633	1,903	2,221	2,368	2,899	3,051
R^2	0.2974	0.2609	0.4939	0.3636	0.1270	0.2878	0.2323
Adjusted R^2	0.2814	0.2452	0.4833	0.3504	0.1097	0.2760	0.2202
				Panel F: Labor Force Participation, Ages 20-39			
N	NA	1,774	2,094	2,412	2,756	2,947	3,063
R^2	NA	0.0307	0.0555	0.0498	0.0677	0.3089	0.2349
Adjusted R^2	NA	0.0112	0.0348	0.0318	0.0519	0.2977	0.2230
				Panel G: Labor Force Participation, Ages 40-59			
N	NA	1,559	1,838	2,114	2,556	2,871	3,040
R^2	NA	0.0284	0.0316	0.0490	0.1434	0.1034	0.0807
Adjusted R^2	NA	0.0060	0.0095	0.0297	0.1277	0.0884	0.0662

Table A.9: Probability of being Married, 1850-1920

Dependent Variable	Married			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.009	-0.012	-0.005	-0.007
	(0.007)	(0.008)	(0.010)	(0.010)
2 Decades Before	-0.004	-0.005*	-0.001	-0.003
	(0.003)	(0.003)	(0.005)	(0.005)
1 Decade Before	0	0	0	0
Rights Given	0.004	0.003	0.005	0.005
	(0.003)	(0.003)	(0.005)	(0.005)
1 Decade After	0.006	0.006	0.007	0.009
	(0.005)	(0.005)	(0.008)	(0.008)
2 Decades After	0.003	0.004	0.002	0.007
	(0.007)	(0.008)	(0.011)	(0.012)
$\geq$ 3 Decades After	0.001	0.002	-0.001	0.002
	(0.011)	(0.011)	(0.017)	(0.017)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	48,469,511	48,469,511	11,685,680	11,685,680
Adjusted R^2	0.022	0.031	0.057	0.077
Mean Dep. Var.	0.93	0.93	0.92	0.92

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. Sample "All" includes all men ages 15-60. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Extra Controls" include occupation and industry fixed effects, interacted with year fixed effects.

Table A.10: Rights and the Age of Married Men, 1850-1920

Dependent Variable	Age of Married Men			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.235	0.032	0.068	0.012
	(0.254)	(0.285)	(0.054)	(0.041)
2 Decades Before	-0.322	-0.097	0.059	-0.054
	(0.203)	(0.149)	(0.062)	(0.033)
1 Decade Before	0	0	0	0
Rights Given	0.241	0.237^{**}	0.060	-0.005
	(0.147)	(0.107)	(0.043)	(0.029)
1 Decade After	0.664^{***}	0.504^{***}	-0.004	-0.068^{*}
	(0.219)	(0.162)	(0.037)	(0.039)
2 Decades After	0.552^{**}	0.635^{**}	0.076	-0.039
	(0.260)	(0.277)	(0.047)	(0.043)
$\geq$ 3 Decades After	0.211	0.566^{*}	0.024	-0.058
	(0.256)	(0.308)	(0.059)	(0.055)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	44,584,983	44,584,983	10,661,314	10,661,314
Adjusted R^2	0.009	0.050	0.020	0.037
Mean Dep. Var.	39.13	39.13	26.46	26.46

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state, are in parentheses. Sample "All" includes all married men ages 15-60. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include age fixed effects, interacted with year fixed effects. "Extra Controls" include occupation and industry fixed effects, interacted with year fixed effects.

Table A.11: Rights and the Age Gap, 1850-1920

Dependent Variable	Age Gap Between Spouses			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.091	-0.012	-0.103	-0.070
	(0.114)	(0.118)	(0.110)	(0.087)
2 Decades Before	0.022	0.054	0.026	0.023
	(0.053)	(0.054)	(0.064)	(0.053)
1 Decade Before	0	0	0	0
Rights Given	0.026	0.018	0.099	0.095
	(0.056)	(0.052)	(0.074)	(0.063)
1 Decade After	0.069	0.037	0.210*	0.162
	(0.103)	(0.099)	(0.113)	(0.105)
2 Decades After	0.024	-0.026	0.182	0.116
	(0.133)	(0.137)	(0.123)	(0.116)
$\geq$ 3 Decades After	-0.027	-0.071	0.015	-0.012
	(0.167)	(0.171)	(0.170)	(0.158)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	44,584,983	44,584,983	10,661,314	10,661,314
Adjusted R^2	0.095	0.098	0.061	0.067
Mean Dep. Var.	4.08	4.08	2.04	2.04

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state levels, are in parentheses. Sample "All" includes all married men ages 15-60. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include age fixed effects, interacted with year fixed effects. "Extra Controls" include occupation and industry fixed effects, interacted with year fixed effects.

Table A.12: Probability of being Newly Wed, 1850-1910

Dependent Variable	Newly Wed			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	0.002	0.001	0.008	0.005
	(0.002)	(0.002)	(0.006)	(0.006)
2 Decades Before	0.002	0.002	0.004	0.003
	(0.002)	(0.002)	(0.006)	(0.007)
1 Decade Before	0	0	0	0
Rights Given	0.002*	0.002*	0.008	0.009
	(0.001)	(0.001)	(0.005)	(0.006)
1 Decade After	-0.002	-0.001	-0.005	0.000
	(0.002)	(0.002)	(0.005)	(0.006)
2 Decades After	0.001	0.002	0.006	0.008
	(0.002)	(0.002)	(0.007)	(0.007)
$\geq$ 3 Decades After	-0.002	-0.001	-0.004	-0.000
	(0.002)	(0.002)	(0.008)	(0.008)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	22,800,160	22,800,160	5,522,429	5,522,429
Adjusted R^2	0.063	0.068	0.048	0.063
Mean Dep. Var.	0.03	0.03	0.08	0.08

Notes: $^+$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state level, are in parentheses. Sample "All" includes all men ages 15-60. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include age fixed effects, interacted with year fixed effects. "Extra Controls" include occupation and industry fixed effects, interacted with year fixed effects.

Table A.13: Rights and the Age of Newly Wed Men, 1850-1910

Dependent Variable	Age of Newly Wed Men			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.154	-0.239	0.373	0.617***
	(0.526)	(0.512)	(0.253)	(0.224)
2 Decades Before	0.343	0.266	0.058	0.117
	(0.651)	(0.592)	(0.321)	(0.342)
1 Decade Before	0	0	0	0
Rights Given	-0.260	-0.214	-0.252	0.184
	(0.703)	(0.688)	(0.298)	(0.300)
1 Decade After	-0.499	-0.114	-0.528**	-0.154
	(0.531)	(0.533)	(0.266)	(0.366)
2 Decades After	-0.251	0.121	-0.375	-0.018
	(0.872)	(0.593)	(0.285)	(0.325)
$\geq$ 3 Decades After	-0.100	0.047	-0.499*	-0.228
	(0.820)	(0.555)	(0.302)	(0.328)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	595,923	595,923	442,801	442,801
Adjusted R^2	0.019	0.120	0.044	0.157
Mean Dep. Var.	28.06	28.06	24.43	24.43

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state levels, are in parentheses. Sample "All" includes all married men ages 15-60. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include age fixed effects, interacted with year fixed effects. "Extra Controls" include occupation and industry fixed effects, interacted with year fixed effects.

Table A.14: Rights and the Age Gap between Newly Wed Couples, 1850-1910

Dependent Variable	Age Gap Between Newly Wed Couples			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.090	-0.222	-0.041	-0.188
	(0.281)	(0.298)	(0.313)	(0.284)
2 Decades Before	0.102	-0.143	0.015	-0.185
	(0.228)	(0.229)	(0.229)	(0.208)
1 Decade Before	0	0	0	0
Rights Given	-0.210	-0.674*	-0.451	-0.685**
	(0.310)	(0.345)	(0.380)	(0.331)
1 Decade After	-0.069	-0.415	-0.305	-0.542*
	(0.311)	(0.321)	(0.325)	(0.311)
2 Decades After	-0.143	-0.521	-0.338	-0.546
	(0.424)	(0.431)	(0.409)	(0.385)
$\geq$ 3 Decades After	-0.493	-0.683	-0.623	-0.751
	(0.522)	(0.512)	(0.490)	(0.462)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	595,923	595,923	442,801	442,801
Adjusted R^2	0.360	0.404	0.242	0.314
Mean Dep. Var.	4.27	4.27	2.87	2.87

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, double clustered at the county-border pair and state levels, are in parentheses. Sample "All" includes all married men ages 15-60. All specifications include county-border pair fixed effects and county-border pair linear time trend. "Controls" include age fixed effects, interacted with year fixed effects. "Extra Controls" include occupation and industry fixed effects, interacted with year fixed effects.

Table A.15: Rights and the Age Gap, Married After Rights 1900-1910

Dependent Variable:	Age Gap Between Spouses					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Women Aged 45-59					
Married After Rights	-0.020	-0.020	-0.032	-0.031	-0.013	-0.032
	(0.034)	(0.034)	(0.034)	(0.033)	(0.054)	(0.047)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	2,266,313	2,266,292	1,602,073	2,185,335	969,420	1,296,872
Adjusted R^2	0.7858	0.7860	0.7785	0.7846	0.7982	0.7743
Mean Dep. Var.	3.34	3.34	3.32	3.30	3.13	3.49
Panel B	Women Aged 20-39					
Married After Rights	0.231**	0.221**	0.152	0.242**	0.227**	0.195***
	(0.091)	(0.092)	(0.134)	(0.092)	(0.109)	(0.065)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,458,884	7,458,865	5,212,311	6,931,508	3,312,279	4,146,586
Adjusted R^2	0.6867	0.6884	0.6587	0.6865	0.7006	0.6771
Mean Dep. Var.	4.58	4.58	4.38	4.52	4.71	4.47

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. “Controls” include husband’s age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects, interacted with year fixed effects. “Extra Controls” include husband’s occupation and husband’s industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic women, who live in the same state in which they were born. Panel A restricts attention to women age 45-59, married to men up to age 70, while Panel B restricts attention to women age 20-39 married to men up to age 50.