Women's Liberation, Household Revolution*

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Abstract

How does women's empowerment affect fertility and education? How important are these changes for the demographic transition? In a dramatic revolution, U.S. states gave economic rights to married women between 1850 and 1920. Prior to this "women's liberation," married women were subject to the laws of coverture, which granted the husband virtually unlimited power. Using the full count U.S. census and contiguous county-border pairs bordering states that gave rights at different times, we show that rights led to less fertility and more education. Additionally, rights were not retroactive, implying differences between those married before/after reforms. This alternative identification strategy confirms our findings and illuminates mechanisms. Shifting bargaining power accounts for these results, with the underlying spousal disagreement relating to maternal mortality risk. Women's empowerment can account for 15% (20%) of the decline (increase) in fertility (education) during the demographic transition, and may be relevant for policy in developing countries today.

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 (1870).[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. 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. 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).[3] We explore the ramifications of coverture's demise on the decision making of households. 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. 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.

[1]Separation of property between husband and wife.

[2]British House of Commons, April 14$^{\text{th}}$, 1870.

[3]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).

Under coverture, personal property, including money, stocks, furniture, and livestock, became the husband's property 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 control while remaining in the wife's name. He could manage the assets as he saw fit, including any income they generated, but he could not sell or bequeath the property without his wife's consent.[4] A married woman could not contract, and any income she earned from labor 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 introduction of married women's property laws, which was done by state in the U.S., 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 U.S. 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. 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 robust-

[4]We discuss further details of the laws of coverture in the appendices of Hazan et al. (2019) and Hazan et al. (2022).

ness 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 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.[5] 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 those married before rights were granted. This is quantitatively consistent with the probability of giving birth declining by 1 percentage point over 20 years. 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.[6] 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.

[5]Property 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.

[6]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.

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. 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: approximately 1 in 125 live births resulted in maternal death in 1900, while 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 (Albanesi and Olivetti, 2016).[7] It is reasonable to assume that husband and wife disagreed over their willingness to tolerate such risks in having additional children.[8] As such, a transfer in bargaining power from husband to wife would yield a decline in fertility.[9] Relatedly, we find no evidence that the ratio of surviving children to children ever born changed with women's rights, suggesting that the first order disagreement between husband and wife was over maternal health, rather than child health. 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.[10] Finally, our findings are consistent with other

[7]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."

[8] Different levels of information could also generate this pattern. Ashraf et al. (2020) study developing countries in modern times and find that husbands have less 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.

[9]It should also increase women's life expectancy. We calculate this effect to be as much as 2.1% extra life expectancy for women in high risk states.

[10]Relatedly, we provide evidence in Section A.1 of the Online Appendix that the topic of women's property rights was widely covered by newspapers at the time. This suggests that

papers showing the effects of empowering women.

Section 6 continues to discuss why other mechanisms cannot 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 of Hazan et al. (2022) we document that there is no change in labor force participation (LFP) rates among married women as a result of granting women economic rights.[11] This is not surprising given the low rate of married women's LFP at the time, which was below 5-6%. Similarly, one might predict an incentive to increase investment in the education of daughters relative to sons, which we empirically reject. Second, the fact that those married after rights can account for much of our findings strongly suggest that the underlying mechanism was within households affected by women's rights. This is as opposed to mechanisms, such as general equilibrium effects of women's rights, that would change behavior for households regardless of when they married. 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 (they assume women to have stronger preferences for quality of children, rather than quantity). On the face of it, this theory makes similar predictions to our own: women's rights decreases fertility and increases 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 may play a role, it can only explain a small part of our findings.

The paper proceeds as follows. Section 2 relates this study to the literature. Section 3 discusses the history of coverture and its demise in the U.S. Section 4 discusses the data and empirical strategies used in this paper. Section 5 presents our empirical results. Section 6 argues that bargaining power shifts, with maternal mortality risk as the source of marital disagreement, are the most promising

people, especially the wealthy who were more likely to read newspapers, were indeed aware of the changes occurring in the legal system.

[11] 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.

explanation for our findings. We conclude in Section 7.

2 Literature Review

We begin by discussing the literature on 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). 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 U.S. 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).[12] A quantity-quality tradeoff would immediately translate reduced fertility into more investment in children's education. 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 affected fertility and investment in children's education.[13]

This paper also contributes to the literature on the connection between women's empowerment and economic development (Duflo, 2012; Doepke and Tertilt, 2018, 2019). We contribute to this literature by documenting how legal changes granting women more economic rights affect fertility and human capital. Thus,

[12]Bazzi et al. (2022) that women on the 19th century U.S. frontier had higher fertility and lower female LFP rates, but higher status occupations for those women who worked. Interestingly, women's economic rights are associated with higher female LFP and lower fertility on the frontier.

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

our work can inform on the implications of female empowerment in the developing world today, which in many ways resembles the U.S. in the 19th century.

Next, there is a large theoretical literature on the demographic transition (e.g., Galor and Weil, 2000; Galor and Moav, 2002), but few empirical studies of the demographic transition in the U.S. Bleakley and Lange (2009) find that the elimination of the hookworm reduced 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. Beach and Hanlon (Forthcoming) find a role for cultural transmission of fertility preferences during the demographic transition. Greenwood and Seshadri (2002) attribute much of the demographic transition to rising income and the structural transformation away from agriculture. We contribute by showing the role that legal changes empowering women had for the demographic transition.

There is a literature on how legal changes can affect household bargaining.[14] 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. Stevenson and Wolfers (2006) study the change of these laws, and find that they reduced the probability of suicide and spousal homicide. Voena (2015) examines how unilateral divorce laws affected labor supply and savings choices. 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.

This paper also relates to the literature on women's economic rights during this time period, reviewed below in Section 3.3.

3 Women's Economic Rights

Here, we discuss which laws we analyze, issues related to the timing of women's rights, the importance of analyzing rights over both property and labor income, public awareness of these legal changes, and our sample time period. We conclude by discussing the potential endogeneity of rights.

[14] Iyigun and Walsh (2007) discuss how changes in institutions that shift power towards women can lead to lower fertility and more education.

In the appendices of Hazan et al. (2019) and Hazan et al. (2022), for brevity omitted here, we give a detailed overview of the history of coverture, as well as a comparison between community property states and common law states. As discussed below, we perform robustness tests dropping these states.

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. Property laws were passed by state legislatures, generally narrowly interpreted by courts (Chused, 1983; Zeigler, 1996), and updated again. States almost never retracted rights once they were granted.

We use the timing of women's liberation by state from Geddes and Lueck (2002). They code the year in which states granted women rights over both their own property and labor earnings, which we refer to as "both" dates, or *rights*. The choice to use their coding raises four questions.

The first is: why use these laws rather than earlier waves of laws? 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."[15] As such, while these statutes did protect a wife's property from her husband's creditors, they did not protect women from their husbands, and thus didn't change the balance of power in the household.[16]

[15] Koudijs and Salisbury (2020) argue these laws protect family assets in the case of default, and thus risk-taking.

[16] 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 discus-

The second question is: are "both" dates the correct set of laws for this study? Presumably, we could analyze earnings rights and property rights separately. However, there are two reasons that "both" is more appropriate (Geddes and Lueck, 2002; Fernández, 2014; Hazan et al., 2019).

The first reason is that there is strong interaction between these rights. Can a woman have property rights without earnings rights? Consider *Apple & Co. v. Ganong* 47 Miss. 189 (1872). Louisa Ganong's husband declared bankruptcy in Mississippi. His creditors sued to gain possession of Louisa's land. Her separate estate was protected from her husband's creditors, but her *earnings* were not. She purchased her land with money from a gift of cotton from her mother and earnings from sewing for soldiers during the Civil War. The court ruled that a percentage of her land commiserate with the percentage funded by her labor earnings belonged to her husband, and was thus liable for his debts, be given to his creditors. This case shows the difficulty of establishing property rights without earnings rights. 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.

Consider *Glover v. Alcott* 11 Mich. 470 (1863). Deborah Alcott, a married woman, owned and operated a mill in Michigan. Her husband declared bankruptcy. Were her profits from the mill liable for his debts? 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 belonged to her husband. The Supreme Court of Michigan 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 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

sion in *Dickerman v. Abrahams* 21 Barb. 551 (1854), Supreme Court of New York.

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.

Earnings rights without property rights were similarly 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). Thus, a wife who worked, and didn't immediately spend her income, effectively transferred income to her husband. We conclude that it is inappropriate to study one type of rights without the other.

The second reason is that state governments often needed more than one round of legislation to effectively grant economic rights (Chused, 1983; Zeigler, 1996). Property rights were generally granted before earnings rights, but issues with property rights were often solved when granting earnings rights. For instance, New York gave married women property rights in 1848. Why did the 1860 earnings bill include explicit protection of women's personal property? Justice J. Wright, of the Supreme Court of New York, gave a legal history of the 1848 law in *Dickerman v. Abrahams* 21 Barb. 551 (1854). He explained 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. 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.

Online Appendix Section A.1 documents that the *New York Times* (NYT) carefully covered the topic of married women's property laws. The NYT reported on changes in the laws around the country and England. The NYT updated readers on court cases, expert lectures, and the intricacies of the law. It seems reasonable to conclude that the class of people who read newspapers such as the NYT were both interested in, and informed about, the evolving state of married women's property rights.[17]

3.2 Sample Period

Figure 1 shows the date when each state granted women "both" rights. Massachusetts was the first in 1846. Data limitations force us to begin our analysis in 1850, rather than 1840 (Ruggles et al., 2020). We stop our analysis in 1920 since the 19th Amendment (passed in 1920) granted women the right to vote, which may well have affected *de facto* implementation of coverture (Geddes and Lueck, 2002).[18]

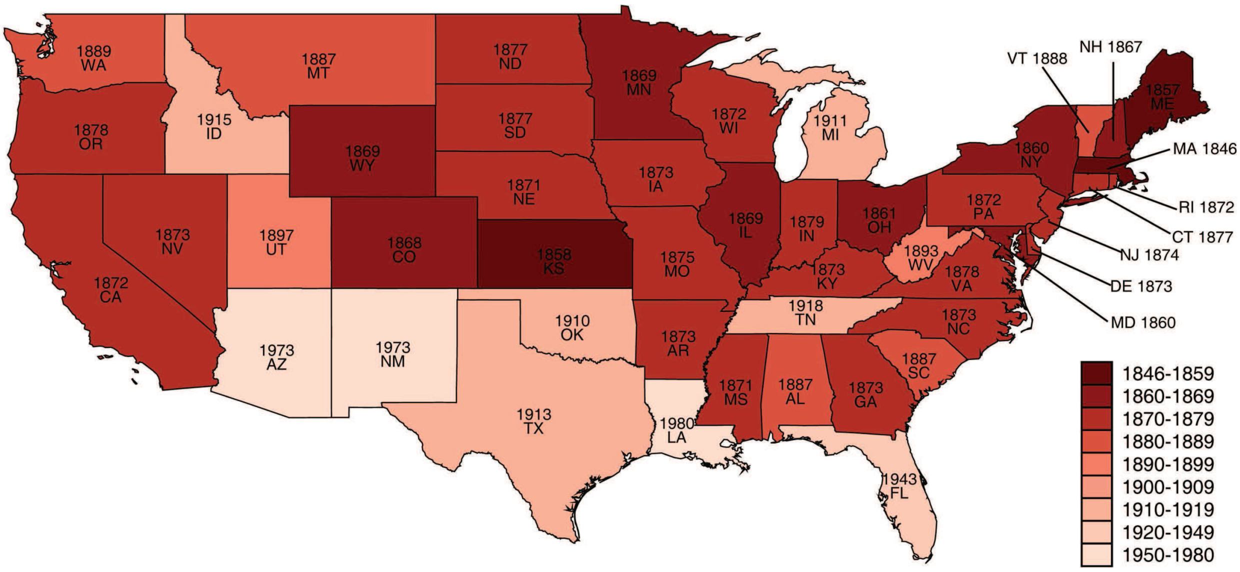

Figure 1: **Timing of women's rights by state.**

3.3 Considerations of Giving Women Rights

Why did legislatures – controlled by men – give women economic rights?

The economics and history literatures are united in arguing that men viewed a loss of bargaining power at home as the main downside of women's rights. For example, Griffin (2003) makes clear that men were hesitant to give up their

[17] Section 6 documents that wealthier households changed their behavior the most in response to women's rights. These households likely were among the readers of the NYT.

[18] By this time, rights were granted in all states except Florida (1943), Arizona (1973), New Mexico (1973), and Louisiana (1980).

own rights at home when debating reform in England. The reason in the historical literature for granting women property rights seems to be to protect women from abusive husbands who might leave their families impoverished. 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.

Our reading of the historical literature negates the notion that the feminist movement drove women's economic rights, though it seems to have led to women's suffrage. The first law passed in New York to grant married women property rights was three months *before* the Seneca Falls convention, widely considered to be the beginning of the feminist movement in the U.S. Furthermore, consider Appendix Figure A.10, 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, negating the relationship between feminism and economic rights. 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 above, Doepke and Tertilt (2009) argue that men wanted to grant rights to give *other* men's wives power. Fernández (2014) argues that if fertility is low, then each child receives a relatively large inheritance. Without women's rights, a son in law will take a lot of wealth by marrying a daughter, representing a large loss to a father. Granting women rights thus makes sense when fertility is low. 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 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, the correlation found in Fernández (2014) reflects the opposite causation than she assumes. Rights led to a decrease in fertility, rather than a decrease in fertility leading to women's rights.

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. (Hazan et al., 2022), this may have been a significant mechanism in England, where married women's labor force participation was high. Finally, Hazan et al. (2019) argue that ending coverture expanded investor protection to women, yielding financial market deepening and economic growth. While they do not evaluate the hypothesis that this may have been the reason to give women economic rights, it is a potential hypothesis nonetheless.

4 Data and Empirical Strategy

Here, we outline our data, including summary statistics, and empirical strategy.

4.1 Data

Our data for the event-study analyses come from the complete census count from 1850-1920, less the 1890 census (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. Our data comparing outcomes for households 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.

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 Hazan et al. (2022) 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.[19] This reduces concerns that our sample selection of married households could bias our results.[20] We restrict attention to whites to abstract from issues related to race. We focus on women who live in the state they were born to avoid property rights issues that arise from migration between states with different laws.

Our first outcome variable of interest is "birth," which is whether a wife gave birth in the previous calendar year. Our second measure is the number of children under age five. Considering that older children may have left home, we limit our analysis to the number of children under five.[21]

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"). We analyze these variables in households where the wife is age 45-59 in order to capture women who have finished giving birth. Since the data is from only two years, an event-study design is not appropriate. 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 schooling is the Integrated Public Use Microdata Series (IPUMS) variable "school," which measures whether a child is currently in school. We restrict attention to children ages 8-17. 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.

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

[20]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." 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).

[21]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.

Table 1: Mean and (Standard Deviation) by Rights, Event Study

	Whole Sample		Before Rights		After Rights	
Birth Last Year	0.20	(0.40)	0.24	(0.43)	0.19	(0.39)
# Kids < 5	1.16	(1.02)	1.39	(1.03)	1.11	(1.02)
Age	29.27	(5.44)	28.63	(5.45)	29.42	(5.42)
Spouse's Age	33.58	(6.71)	33.21	(6.87)	33.67	(6.67)
Year	1898.30	(21.79)	1870.46	(19.93)	1904.82	(16.39)
N	14,460,963		2,743,165		11,717,798	

4.2 Summary Statistics

Table 1 shows summary statistics of outcome and control variables for the analysis of fertility in our event-study analyses. The probability of a birth last year and the number of children under age 5 are substantially lower, and husband and wife are slightly older, when 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 use of interactions between control variables and year fixed effects.

Table 2 does the same for the exercise comparing couples married before and after economic rights. Panel A shows the probability of giving birth and the number of children under age 5 on the sample where the wife is age 20-39. The probability of giving birth is higher (0.21) without rights than with rights (0.18). There are fewer children under age 5 at home with rights (1.08) than without rights (1.23). The average age of the wife is 29-30, while the husband is about 34, with no difference between types of couples.

Panel B of Table 2 shows the number of children ever born to the wife of the household, and number of surviving children 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. Those married after rights are about 1.5-2.5 years younger than those married before rights.

Table 3 shows summary statistics for the analysis of education in our event-

Table 2: Mean and (Standard Deviation) by Rights, Married Before-After Rights

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

studies. 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 also report the average age of their mother and father.

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

Table 3: Summary Statistics by Rights, Education Event Study

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

education rates by gender over time discussed above, which motivates including interactions between a child's gender and year.

4.3 Empirical Approach 1: Event-Study

We first describe the structure of the regressions we estimate in our event studies, the data on county-border pairs, the conditions under which our results can be interpreted as causal, and robustness analyses.

We estimate regressions of the following form:

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

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.[22] Standard errors are double-clustered at the state and county-border pair level, as elaborated upon below.

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 next discuss the construction of county-border pairs, detailed fully in Appendix A.2. The data on the evolution of U.S. historical county boundaries comes from the IPUMS National Historical Geographic Information System (Manson et al., 2019). The construction of these border-pairs raises some issues.

The first issue is that county borders were themselves ever changing. Imagine

[22] Sun and Abraham (2021) argue that event-studies with linear time trends tend to be underidentified. 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.

a county A in state 1 bordering another county B in state 2. If 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 A.2, where we also include an example of the evolution of the border between Indiana and Illinois (Figure A.9). Similarly, as the U.S. spread westward over the 19th century, more states (and thus, state borders) developed.[23] Maps showing our data on borders over time can be seen in Appendix Figures A.1 - A.8.

The second issue is, what if county A has more than one bordering county? 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.[24] Econometrically, this approach raises two issues. The first is that duplicated observations could bias estimates. Accordingly, when we duplicate an observation n times, we reweight each observation to have a weight of $1/n$. 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 (Dube et al., 2010).

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.

Are women rights plausibly exogenous? Did states grant women economic

[23]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).

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

rights because of changing fertility rates or education rates? The historical record seems to suggest not.[25] Furthermore, states granted rights, which were then overturned by the courts, often due to unforseen technicalities. It is hard to believe that the final timing of women's rights in a state was endogenous. For our purposes, as long as the change in the law was plausibly exogenous to a county on that state's border, our analysis captures the causal effects of rights. 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. Most likely Ohio passed laws without taking this county into account, making state law changes exogenous to this county.[26] Finally, we 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.

Did other legal changes happen simultaneously? We, and the historical literature, are unaware of any relevant changes, with the exception of child labor laws and mandatory schooling laws, which we control for. We perform randomization exercises in Appendix A.4 to delve further into this issue. For each state we pick a random year for women's rights between 1850 and 1920. We rerun our estimates using these fake dates 1,000 times. The estimates are centered at 0, implying that it is unlikely that our estimators are biased. Additionally, very few of these estimates using random dates find effects larger than those we document with the actual dates. We conclude that it is highly likely that the years in which women granted rights contain actual information.

Did these changes affect the marriage market? In Hazan et al. (2022) we show that these rights did not affect the propensity of people to marry, the age of marriage, or the age gap between husband and wife. Additionally, people did not change the timing of their marriage in order to marry before or after rights

[25] The reasoning behind granting women rights seems to have been to protect women against delinquent husbands.

[26] 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 A.3 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 U.S. 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.

were granted. We do not have any measures of marital sorting.

We 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, to account for the differential experience of the South during the Civil War and Reconstruction. Second, we drop counties on the border between community property states and neighboring states, as their property rights regimes slightly differed.

The third robustness test addresses issues with difference in difference estimators with two-way fixed effects, of the sort analyzed in this paper. We employ a two-step estimator of the sort analyzed in Thakral and Tô (2020), who generalize an approach introduced by Gardner (2021). The results are very similar, and we thus conclude that our benchmark event study analysis is appropriate.

The first stage estimates all coefficients, except for the event-study coefficients, on non-treated data. Specifically, we estimate regressions of the following form:

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

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.[27] λ_t are year fixed effects. Thus, the estimates of these parameters are not contaminated by the effects of women's rights. 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. Since Massachusetts gave women rights before our time period began (1846), we cannot include her, or her neighbor's, observations.

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

[27] We do not include county border pair linear time trends, as cases would involve not including one side of the border.

$$Y_{hsct} = \sum_k \alpha_k \cdot rights_{st}^k + \hat{\beta}_{c,b(c)} + \hat{\gamma}_{c,b(c)} + \hat{\lambda}_s + X'_{hsct}\hat{\delta} + \epsilon_{hsct}, \tag{3}$$

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 Online Appendix A.5.

As a final note, when performing these event studies on the education of children, in some specifications, we add interactions of $rights_{st}^k$ with dummy variables indicating whether the child is female, allowing us to control for the crossing-over of education by gender over time.

4.4 Empirical Approach 2: Couples Married Before vs After Rights

We now describe the structure of regressions we estimate in our analyses comparing households married before and after rights were granted, as well as whether our results can be interpreted as causal.

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

where Y_{hsct} is our outcome variable of interest listed above, in state s, county c, and year t, $t \in \{1900, 1910\}$, $MarriedRights_{hsct}$ is an indicator variable for if household h was married after rights were granted in state s, $\beta_{c,t}$ are fixed effects for each county-year, and X'_{hsct} contain controls variables that depend on the specific exercise. 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 Hazan et al. (2022) we argue that it is indeed the case that selection into marriage was not affected by women's rights. In Online Appendix A.6, we provide evidence that couples did not time their marriage around the granting of women's rights.

We perform the same robustness tests as in the event-study design, dropping the south or community property states. The randomization exercises are reported in Online Appendices A.4.

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

5.1 Fertility

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. Most specifications include "extra controls," which include fixed effects for the husband's industry and husband's occupation, both interacted with the year fixed effect. This allows us to control for how a husband's career might affect family size, differentially over time.

Table 4 analyzes whether there was a birth last year. Column 1 does not include our extra controls. Column 2 includes these 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 on the border between community property and other states.[28] Column 5 also repeats Column 2, but uses the two-step estimator.

In all specifications, the point estimates prior to granting rights are quantitatively virtually zero, and have no pattern to them, suggesting no trend in fertil-

[28] In untabulated results we perform robustness exercises on our event-study analyses where we use cross-state variation. The results are similar to our main findings.

Table 4: 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.

ity around the time of giving rights, a point we return to below.[29] 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 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. We visualize Column 2 in Figure 2 (top left panel). 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.

Returning to the issue of trends, 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 2 (top left panel) 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 2 (top right

[29]The estimates are not statistically significant due to large standard errors, except in 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 calculated in this specification are similar to the standard errors in other specifications after rights are granted, but smaller before rights are granted.

panel). 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.

Table 5 follows the pattern of Table 4 when the dependent variable is the number of kids under age 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, as before. 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, and 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. We visualize Column 2 in Figure 2 (bottom left panel). 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. In percentage terms, these fertility declines are remarkably consistent with those described above. 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. 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,

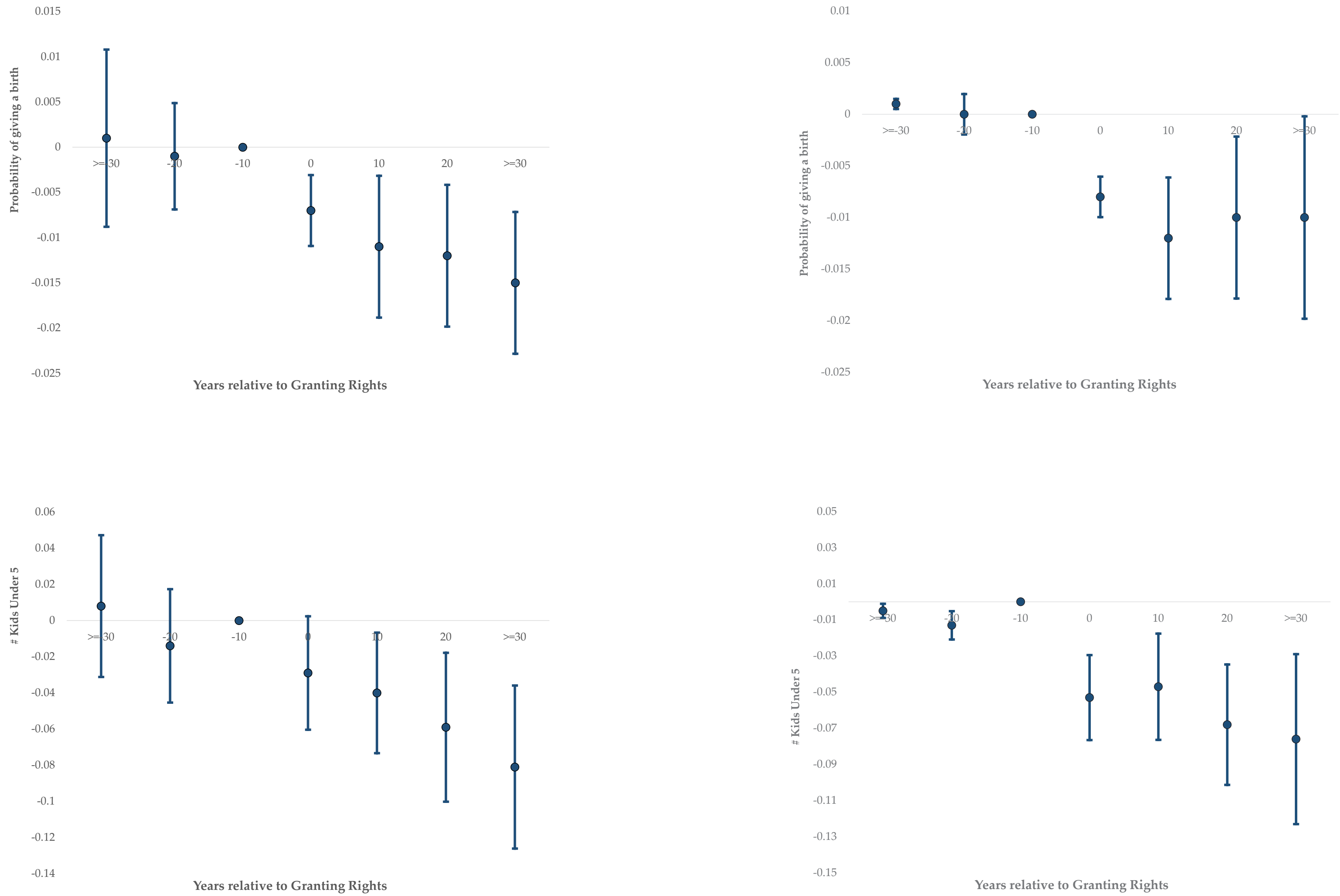

Figure 2: Top Left: Probability of Birth (Benchmark, Column 2); Top Right: Probability of Birth (Two-Step Estimator, Column 5); Table 4. Bottom Left: Number of Children Under 5 (Benchmark, Column 2); Bottom Right: Number of Children Under 5 (Two-Step Estimator, Column 5); Table 5

which is larger than the counterpart (-0.027) in Column 2. 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.[30] Since the results of the two-step estimator are remarkably similar to our benchmark exercise, we conclude that the concerns raised by the literature on the traditional difference-in-difference estimator are not a major concern in this exercise.

Online Appendix A.4 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 economic rights led to a decrease in fertility of about 3-8% over the subsequent decades. While the point estimates show an increasing magnitude of the effect of rights over time, it is possible that the effect of rights is the same two and three decades later. We hypothesize that the decline in fertility is driven mostly by people married after rights were granted. As such, as 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. We return to this hypothesis below.

5.1.2 Fertility: Couples Married Before/After Rights

We estimate equations along the lines of those described in Equation (4).

Table 6 shows our findings 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). Column 1 includes as controls for the wife's age, the husband's age, and how long the couple has been married, all interacted with year fixed effects.[31] Column 2 adds the "extra controls," which include the husband's oc-

[30] As before, it is harder to fit a line through smaller confidence intervals, rejecting a pretrend.

[31] 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,

Table 5: # 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 6: 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	-0.010***	-0.009***	-0.011	-0.009***	-0.010***	-0.004
Rights	(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	-0.143***	-0.138***	-0.169	-0.146***	-0.142***	-0.124***
Rights	(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.

cupation 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.[32]

Panel A shows that couples married after rights were granted had a lower probability of giving birth 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 significant in Column 3. 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 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.[33]

These results are consistent with those found using the event study approach. The probability of giving birth is estimated to decline by 0.009-0.011, 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. It is plausible 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 increasing effect of women's rights on fertility is that the stock of married couples changes to include more people married after rights were granted.

Panel B shows that couples married after rights had 0.117-0.169 fewer kids at home under age 5 in Columns 1-5. These results are statistically significant in Columns 1,2, 4, 5, and 6 at the 1% level. While they are not significant in Column 3, the point estimate is very similar to the other specifications.

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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.

[32]In untabulated results, we perform exercises where 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 similar to our main findings.

[33]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).

The estimates 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. The estimates here are 2-3 times larger. One potential explanation is that couples married after rights might 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. Similarly, the estimates here are larger than those documented in the event-study approach. These findings reinforce the idea that declines in fertility are being driven by couples married after rights were granted.

Table 7, Panel A, shows the results when the dependent variable is either children ever born (CEB), while Panel B shows surviving children. We use the sample of households where the wife is age 45-59.[34] 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 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, 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). The fact that the estimate in Column 3 is similar to other specifications suggests that most of the impact of rights comes

[34]In Online Appendix A.6, we show figures of raw data for our measures of completed fertility, showing that couples married after rights were granted had fewer children.

from a decline in the intensive margin of fertility, rather than extensive margin.

Given the high child mortality rate of the time, in Panel B we replace CEB with the number of surviving children as a better measure of the demand for children. Panel B of Table 7 shows that 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. Demand for children decreased following women's economic rights.

Table 7: 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	-0.234**	-0.239**	-0.220**	-0.204*	-0.253**	-0.251**	-0.218*
Rights	(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	-0.180**	-0.183**	-0.169**	-0.129	-0.191**	-0.188**	-0.175*
Rights	(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.

Appendix A.4 reports the results of our randomization exercise for these analyses. It is highly unlikely that the estimators are biased or that a random set of dates would have yielded similar results.

We repeat the above exercise 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. We find that 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. Thus we find that households decreased their fertility along the intensive margin. Online Appendix Figure A.13 visualizes our findings.

We conclude that couples married after rights were granted had lower fertility rates, especially along the intensive margin. This decline in fertility rates can potentially account for the decline in fertility rates documented in the event-study approach. Finally, the probability that a woman gave birth fell from about 24.8% to 17.4% over the course of our sample. Thus, women's rights can account for about 15% of the overall change between 1850 and 1920.

5.2 Education

5.2.1 Education: Event Study Approach

We estimate regressions described in Equation (1), where the dependent variable is whether a child is currently in school. 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 are 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 year fixed effects.[35]

Table 8 reports the results. 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

[35]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.

includes the extra controls. Column 3 repeats Column 2, but adds the interactions of $rights_{st}^{k}$ with whether the child is female.

There is no trend in schooling prior to rights being granted. 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. A decade after rights, the estimates rise to 4.8-5.2 p.p., and are significant at the 5% level. The estimates rise further to 5.3-6.0 p.p. two decades after rights, and are significant at the 5% level. The point estimates drop somewhat to 3.4-4.6 p.p. and lose their significance three decades after rights are granted. Column 3 shows no meaningful 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.[36] These estimates reflect an increase of about 5-8% in schooling, relative to the 74% average schooling rate.

Columns 4-6 repeat Columns 1-3 but restrict the sample to be children ages 8-13. There is no trend in schooling prior to rights being granted. After rights are granted, the estimates are universally larger than their counterparts in Columns 1-3. The estimates here are about 0.5-0.7 p.p. larger than their counterparts in Columns 1-3 when rights are granted, and as much as 4.3 p.p. three decades after rights are granted. 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 1-3, these estimates are not statistically significant, in Columns 4-6 they are significant at the 1-5% levels. There is no differential impact of women's rights on education by gender. Online Appendix Figure A.15 visualizes the results of Column 5's event study.[37]

Columns 7-9 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 imme-

[36]We confirm this finding by separately estimating Column 2 by gender (untabulated).

[37]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.16 visualizes these results. The findings are strikingly similar.

Table 8: 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.

diate 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 thus increases the education of grandchildren, which also increases the daughter's marriage market prospects.

Appendix A.4 reports the results of our randomization exercise for this event-study analysis of the rise in education following women's rights. The findings suggest that our regression specifications are not biased, and that it is highly unlikely that a random set of dates would have yielded similar results. This appendix also includes our robustness analysis for this exercise (Table A.7) as well as the results with the two-step estimator (Table A.5).

We conclude that women's rights led to a dynamic increase in the educational attainment of children, with the effect concentrated on younger rather than older children. Women's economic rights did not lead to any differential impact on the education of daughters rather than sons.[38]

5.2.2 Education: Parents Married Before/After Rights

We next compare children of couples married before and after women's rights.

Table 9 presents the results of these estimations. 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 being married after rights and the child being female. We find that children born to

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

parents married after rights were granted were 0.9-1.0 percentage points more likely to be in school, with the estimates significant at the 10% level. Columns 3 and 4 repeat this pattern for children ages 8-13. The estimates suggest that these children were 0.3-0.5 p.p. more likely to be in school; 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 p.p. more likely to be in school, with the estimates significant at the 1-5% levels. We again find no evidence that the impact of women's rights on daughters was different that that on sons.[39] In Online Appendix A.4, we repeat this exercise separately for data from 1900 and 1910.

Table 9: Attending School, Married After Rights, 1900-1910

Dependent Variable:	Probability of Being in School					
Children's Age	8-17		8-13		14-17	
	(1)	(2)	(3)	(4)	(5)	(6)
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

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.

At first glance, these results are not entirely consistent with those found in the event study, as the larger effect for older children than younger children. However, the event study was over the time period 1850-1920, while this analysis is mostly focused on 1910, by which time about 95% of 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

[39]We also perform exercises with county-border pairs (untabulated). The results are qualitatively similar to our main findings.

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, Online Appendix A.4 reports the results of our randomization exercise for this analysis. The regression specifications are not biased, and that it is highly unlikely that a random set of dates would have yielded similar findings.

We conclude that education increased following women's rights, and that it is plausible that this was driven by parents married after rights were granted.

6 Discussion: Mechanisms

Next, we argue that shifting household bargaining power, with maternal mortality risk as a source of marital disagreement, can account for our findings. We then negate other potential mechanisms.

6.1 Bargaining Power is a Plausible Mechanism

We make five observations to contend that bargaining power is a plausible mechanism to explain the results we find. First, 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, 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 the U.S.

The second observation is to point out that our results can be accounted for by couples married after rights were granted. Since marital property rights were not granted retroactively, this strongly suggests that the mechanism by which rights affected households must come from a change at the household level. Bargaining power between husband and wife is an appealing story.

The third observation is that maternal mortality risk could be the underlying reason for husband and wife to differ in desired fertility. Approximately 1 in 125 live births resulted in maternal death in 1900. Disability-adjusted life years (DALY), 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) (Albanesi and Olivetti, 2016). It is reasonable to assume that husband and wife disagreed over their willingness to tolerate such risks in having additional children. Thus, a transfer in bargaining power to the wife decreases fertility. Presumably, this effect is 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. We take Albanesi and Olivetti (2014) maternal mortality rates by state to explore how women's rights affected fertility differentially by risk. Their data is from 1925-1934, around the end of our sample, with no data available prior. There is no correlation between the timing of a state granting rights and its maternal mortality rate, as seen in Figure A.11 in the Online Appendix.[40]

Table 10, Panel A, Column 1 repeats Table 6 Column 2, while Panel B does so for Table 7. The remaining specifications 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"). 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 Online Appendix Figure A.11. Columns 4-6 repeat this pattern for the number of kids under age 5. Panel B again repeats this pattern, but uses Table 7 as a starting point. Columns 1-3 of analyze children ever born, while columns 4-6 analyze surviving children.

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. However, the interaction term indicates that high MMR states saw a decline in fertility more than twice the magnitude of other states. These states saw children ever born decline

[40]Figure A.11 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 10: 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		-0.009**	-0.009**		-0.119***	-0.119***
×High MMR		(0.004)	(0.004)		(0.037)	(0.037)
Sample	All	All	Rights $\leq$ 1920	All	All	Rights $\leq$ 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		-0.502***	-0.505***		-0.442***	-0.444***
×High MMR		(0.168)	(0.169)		(0.139)	(0.140)
Sample	All	All	Rights $\leq$ 1920	All	All	Rights $\leq$ 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 6 for Panel A and Table 7 for Panel B. "High MMR" is an indicator that a household is in a state in the top 25% of maternal mortality risk.

by an extra 0.5 children above the reduction of 0.17 children other states experienced, with the estimate significant at the 1% level. The highest risk states can thus account for much of our findings. These are exactly the states where women would use their bargaining power to reduce fertility the most.[41]

Since property rights affect people with property, wealthier people should respond more to women's rights. Our fourth observation is to empirically confirm this hypothesis. In 1860 and 1870, and only these two years, the U.S. census asked about measures of both real and personal property at the household level. We then estimate regressions of the structure described in Equation (1) on these data. However, since we only have two years, we replace the event study design with a simple difference-in-difference estimator. We 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.

Table 11 our findings. Column 1 has the dependent variable of whether the woman gave birth last year. Women's rights are associated with a 0.8 p.p. decrease, with the estimate significant at the 10% level. This is remarkably similar our results 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 rights. Women's rights still has a negative impact of half a percentage point, but this estimate is not significant. High wealth households have lower fertility, but the estimate is not significant. However, wealthy households reduce their fertility by an additional 1 p.p. 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. These findings are consistent with women's rights affecting household bargaining, as wealthier families should be most affected by property rights.

One back-of-the-envelope way to calculate the importance of this decrease in fertility for women is to calculate how much DALY they gained as a re-

[41]We also perform an exercise (untabulated) comparing education in states with high versus low maternal mortality rates. The estimates are not statistically different from one another.

sult of women's rights. DALY was about 1.1 years per pregnancy in 1930 (Albanesi and Olivetti, 2016), presumably larger in our time period, and much higher for the high risk states. To be conservative, we use 1.1 years as our estimate. Our results thus indicate that women in general reduced by 0.172 pregnancies, gained 2.27 extra months. However, women in high risk states, who reduced their fertility by 0.674 pregnancies, gained about 8.9 months. In 1900, women at age 20 had a life expectancy of another 42.9 years (Bell et al., 1992). Thus, women's rights increased their effective life expectancy by 2.1%.

Table 11: Fertility, by Wealth 1860-1870

Dependent Variable	Birth Last Year		# 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)
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. All specifications also include fixed effects for wife's age, husband's age , husband's occupation, husband's industry, all 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.

Our fifth and final point is that our results are consistent with a wide literature that suggests that shifting household power towards women causes a decline in fertility and increase in education (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).

How exactly did women's rights affected bargaining? The classic approaches to modeling household bargaining use divorce as the disagreement point in

Nash bargaining (Manser and Brown, 1980; McElroy and Horney, 1981). If divorce is not permitted, due to the constraints of the time, what is the disagreement point? We observe that it need not be divorce, but rather what happens during disagreement between spouses. This idea dates back at least to Lundberg and Pollak (1993).[42] Prior to rights being granted, women had virtually no power in such a situation. With rights, a woman could withdraw money from her account, purchase merchandise downtown, and continue the marital disagreement on her terms. It seems reasonable to conclude that their disagreement point improved dramatically.

We conclude that shifting bargaining power from husband to wife can account for our findings, and that maternal mortality risk is a plausible underlying mechanism for disagreement between spouses.

6.2 Other Mechanisms Don't Work

The first potential other mechanism that we consider 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 not consistent with the data. Labor force participation rates were incredibly low during our entire period, at roughly 3-5%, and were unaffected by economic rights (Hazan et al., 2022).

Second, did women's rights increase the desire to invest in a daughter's education relative to a son's education? Perhaps daughters might grow up to either work or manage assets now that they have economic rights. More education may be helpful. However, we find no evidence of a differential impact by gender of the child.

The third is the hypothesis is that general equilibrium effects could account for our results. Hazan et al. (2019) document that granting women property rights

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

yields financial market deepening and economic growth, especially biased towards capital intensive manufacturing. One might hypothesize 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. 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, as discussed above, Doepke and Tertilt (2009) theoretically argue that women's rights increases education and reduces fertility. 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 suggest that maternal mortality risk was a key factor behind the decline in fertility. While there may be a role for the mechanism suggested by Doepke and Tertilt (2009), it cannot for a major part of the story.

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.

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