

# Women's Liberation and the Demographic Transition\*

Moshe Hazan  
Monash University and  
CEPR

David Weiss  
Tel Aviv University

Hosny Zoabi  
The New Economic  
School

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## Abstract

U.S. states gave economic rights to married women between 1850 and 1920. Prior to this “women’s liberation,” the laws of coverture granted virtually unlimited power to husbands. Using the full count U.S. census and county-border pairs, we use an event-study analysis to show that rights reduced fertility. Additionally, women’s rights were not granted retroactively, allowing us to compare people married before and after the reforms, confirming our findings. Shifting bargaining power accounts for our results, with spousal disagreement over fertility plausibly due to maternal mortality risk. Women’s empowerment explains about 20% of the decline in fertility during the demographic transition.

Keywords: Women’s liberation, demographic transition, household bargaining, fertility, property rights, maternal mortality risk.

JEL: D1, E02, I15, J13, K11, K38, N31

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

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

## 1 Introduction

One of the most dramatic advances in women's rights in history was when common law countries began to give legal and 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).<sup>3</sup> We explore the ramifications of coverture's demise on the "Demographic Transition," or the transition countries experience from high birth rates to low birth rates as they become wealthier.<sup>4</sup> The ramifications of this transition are hard to overstate. Lower fertility rates are widely understood to increase income per capita (Galor, 2011) and investment in children's health and education, while adversely impacting a country's ability to support old-age pensions (Lee, 2003).<sup>5</sup> We use the complete count U.S. Census from 1850 to 1920 and two separate identification strategies to show that women's legal empowerment reduced fertility. Women's economic rights can account for about 20% of the decline in fertility during the demographic transition of this time period.

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<sup>1</sup>Separation of property between husband and wife.

<sup>2</sup>British House of Commons, April 14<sup>th</sup>, 1870.

<sup>3</sup>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).

<sup>4</sup>While the demographic transition was characterized by both reductions in fertility and mortality, we focus on fertility in this paper.

<sup>5</sup>Perhaps less well known is the impact of the demographic transition on international migration (Lee, 2003), the rise of secularism and modernity (Johnson-Hanks, 2008), and a country's willingness to wage war (Brooks et al., 2018).

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.<sup>6</sup> 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. Coverture's demise was thus one of the most significant transformations married women experienced in US history: they could manage their own bank accounts, real estate, labor market, and write contracts. We argue that this legal change shifted intrahousehold bargaining to advantage of women.

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. For example, consider county A in Ohio bordering county B in Pennsylvania. The people in these counties are very similar, and thus good controls for one another. Ohio granted women rights prior to Pennsylvania, allowing us to examine what happened to fertility in county A using women in county B as a control. We find that fertility decreased following rights, with the probability of giving birth falling by about 1 percentage point. The full effect of women's rights on fertility did not appear at once, but rather over a decade or two after rights were 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 20% of the overall decline in fertility between 1850 and 1920 in the U.S.

The second identification strategy exploits the fact that these economic rights were not granted retroactively. That is, property transferred from the wife to her husband, as a result of coverture, was not returned to the wife upon granting women

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<sup>6</sup>We discuss further details of the laws of coverture in the appendices of Hazan et al. (2019) and Hazan et al. (2023).

economic rights.<sup>7</sup> Our second identification strategy is therefore to look at the fertility rate of women married after rights were granted using similar women married before rights were granted as a control. 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 strongly suggests that people married after rights are driving the declines in fertility we document. Section 4 formalizes both empirical approaches and discusses the assumptions under which our findings can be interpreted as causal. We argue that these conditions are met.

Section 6 posits that a shift in household bargaining power from husband to wife is the most plausible mechanism to explain the results documented in this paper. First, legislators of the era were concerned that granting women rights would disturb household tranquility by taking away men's power to make decisions (Griffin, 2003). Second, our results can be largely accounted for by people married after rights were granted. This implies that perhaps only those directly affected by the non-retroactive law changed their behavior. Third, we present evidence consistent with the notion that maternal mortality risk could have been a source of marital disagreement over the number of children. In fact, we find that states with the highest maternal mortality risk saw declines in fertility following women's rights of more than twice the rate observed in other states. The importance of maternal mortality risk is hardly surprising: approximately 1 in 125 live births led to mater-

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<sup>7</sup>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.

nal death in 1900, while disability-adjusted life years, which consider both death and disability risk, was about 1.1 years per pregnancy in 1930, and presumably higher in our study period (Albanesi and Olivetti, 2016).<sup>8</sup> We provide evidence that women expressed their fears of pregnancy, which was a major factor driving the mid-19th century abortion boom. It's reasonable to assume that husbands and wives disagreed over their willingness to endure such risks in having additional children.<sup>9</sup> Therefore, a transfer in bargaining power from husband to wife would logically result in a decline in fertility.<sup>10</sup> Notably, we find no evidence that the ratio of surviving children to children ever born changed with women's rights, suggesting that the primary disagreement between husband and wife revolved around maternal health, not child health. Fourth, we present evidence that wealthier families decreased their fertility more than other families, consistent with the idea that differences in control over wealth are responsible for our results.<sup>11</sup> Fifth, our findings align with other papers demonstrating the effects of women's empowerment. Finally, we argue that alternative mechanisms cannot fully explain the patterns in the data.

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 explanation for our findings. We conclude in Section 7.

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<sup>8</sup>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. Bhalotra 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."

<sup>9</sup>Differential levels of information could also create this pattern. Ashraf et al. (2020) study developing countries in contemporary times and find that husbands have less knowledge about maternal mortality and morbidity risks than their wives. When these men are educated on the topic, they exhibit a reduced desire for fertility.

<sup>10</sup>This should also increase women's life expectancy. We calculate this effect to be as much as a 2.1% increase in life expectancy for women in high-risk states.

<sup>11</sup>In connection, we provide evidence in Section A of the Online Appendix that the topic of women's property rights received widespread coverage in newspapers at the time. This suggests that people, especially the wealthy who were more likely to read newspapers, were indeed aware of the changes occurring in the legal system.

## 2 Literature Review

We begin by discussing the literature on the impact of women’s empowerment on fertility. Central to the claim is the idea that men and women have different preferences over the number 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). 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).<sup>12</sup> Bhalotra 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.

This paper also contributes to the literature on the connection between women’s empowerment and economic development (Duflo, 2012; Doepke and Tertilt, 2018, 2019; Hazan et al., 2019). We contribute to this literature by documenting how legal changes granting women more economic rights affect fertility. Thus, 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 (2023) 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.<sup>13</sup>

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<sup>12</sup>Bazzi et al. (2022) find 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.

<sup>13</sup>Blanc (2022) finds that secularization can account for much of the demographic transition in France.

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.<sup>14</sup> 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.

Finally, this paper relates to the literature on other rights women were being either considered or granted, namely, suffrage. Lott and Kenny (1999) find that women's suffrage expanded the size of state governments. Miller (2008) finds that this expanded spending reduced child mortality through health outcomes. As we document below, women's economic rights did not seem to affect child mortality.

### **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.

In the appendices of Hazan et al. (2019) and Hazan et al. (2023), 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. In Section B of the Online Appendix of this paper, we have an extended treatise on the historical context not discussed either in the literature review above or in the discussions of women's

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<sup>14</sup>Iyigun and Walsh (2007) discuss how changes in institutions that shift power towards women can lead to lower fertility and more education. González and Zoabi (2021) finds that a paternity leave expansion reduces fertility and promote equality within households.

rights below, including a further discussion of women’s labor force participation (by socioeconomic status), education, marital stability, and birth control methods. For further discussions on some of these topics, see Goldin (1980), Goldin (1990), and Goldin (2006).

### 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.<sup>15</sup>

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.”<sup>16</sup> 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.<sup>17</sup>

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<sup>15</sup>Interestingly, the British House of Commons invited experts from American states that already granted rights to testify during their debate on women’s rights. Dudley Field of New York argued that “[s]carcely any one of the great reforms which have been effected in this State has given more entire satisfaction than this.” Mr. Fisher from Vermont testified that “I do not believe that I have ever seen an individual in the State who wanted to go back to the old law” (Hansard, 1870b).

<sup>16</sup>Koudijs and Salisbury (2020) argue these laws protect family assets in the case of default, and thus risk-taking.

<sup>17</sup>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



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.

Additionally 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 place a mill on her land, as she could if unmarried, leaving it unproductive. The lack of earnings rights was therefore a serious

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property was liable for a husband’s debt even if he had not reduced these assets to possession. See Justice Wright’s discussion in *Dickerman v. Abrahams* 21 Barb. 551 (1854), Supreme Court of New York.

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 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.<sup>18</sup>

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<sup>18</sup>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.

### 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).<sup>19</sup>

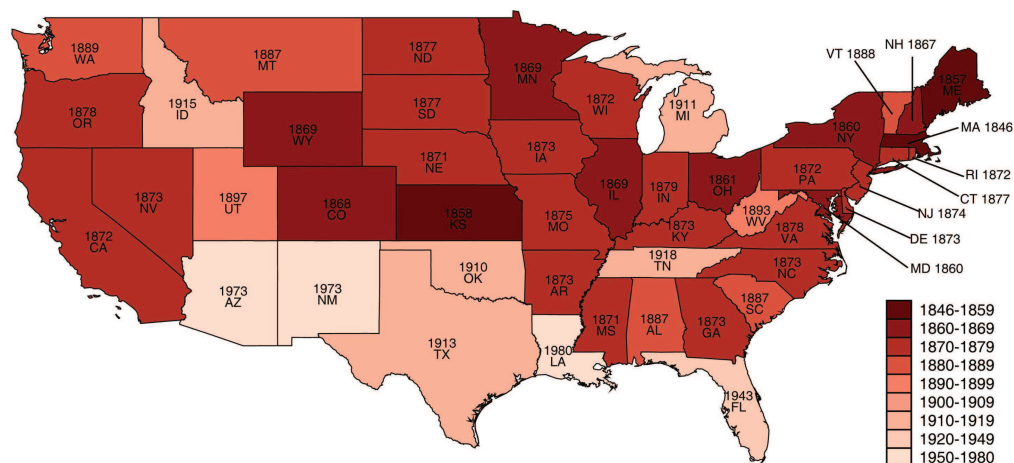


Figure 1: **Timing of women’s rights by state.** Dates are from (Geddes and Lueck, 2002).

### 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 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.

<sup>19</sup>By this time, rights were granted in all states except Florida (1943), Arizona (1973), New Mexico (1973), and Louisiana (1980).

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 Online Appendix Figure A.11, which plots the year that each U.S. state granted women economic rights on the Y axis against the date of women's suffrage on the X-axis. There is no correlation between the timing of these rights, 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., 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.<sup>20</sup>

## 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). 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. We restrict attention to married households to abstract from any issues related to out of wedlock birth, which was exceedingly rare at the time. In Section 5.2 we show that granting women property rights had only a negligible impact on marriage markets, as measured by the propensity to get married, the age of newly-weds, and age gap between newly-married husband and wife.<sup>21</sup> Furthermore, we document in Online Appendix H that there did not seem to be much clumping of marriage around legal changes, suggesting that people did not time their marriages around women’s rights.<sup>22</sup> This reduces concerns that our sample selection of married households could bias our results.<sup>23</sup> We restrict attention to whites to

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<sup>20</sup>Hazan et al. (2019) discuss and empirically evaluate potential hypotheses for why men chose to grant women’s economic rights in Section 2 of their Online Appendix. However, they are unable to draw any robust empirical conclusions.

<sup>21</sup>We do not have a measure of marital sorting available in our data.

<sup>22</sup>This is consistent with the idea that the laws were passed and then challenged in court, making it difficult to know when the laws would be upheld.

<sup>23</sup>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,

abstract from issues related to race. There is a question of determining the status of women's rights: if a woman's birthplace is New Jersey but is living in Pennsylvania, which rights regime is the relevant one for our analysis? In theory, the law in the state a couple married is the law that is relevant.<sup>24</sup> In practice, this is impossible to determine from our dataset. Furthermore, there is an issue of potentially endogenous migration. We assign women's rights by state of birth, and as such take an "intent to treat" approach towards legal changes. In all exercises, we include robustness checks where we restrict attention only to women who stayed in their state of birth. This simultaneously adds bias to our estimate by restricting to a selected sample, while reducing measurement error by increasing the certainty over which state a couple was married (and thus the relevant legal regime). Generally, we find that this sample selection does not yield dramatically different estimates. For brevity, we do not report summary statistics for the sub sample of women who remained in the state they are born.

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. We also do secondary analyses on the marriage market and labor force participation, which is only available for women starting in 1860.

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.

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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).

<sup>24</sup>Consider a couple that marries in a state with no rights. The husband could immediately reduce his wife's moveable property to possession.

## 4.2 Summary Statistics

Table 1 shows summary statistics of outcome and control variables for our event-study analyses. We report these statistics by year and by status of women's rights.

Panel A of Table 1 reports statistics for women age 20-39, with husbands up to age 50. The probability of a birth last year and the number of children under age 5 are substantially lower, when women have rights. Women's labor force participation is low throughout our entire sample. Women and their spouses with rights tend to be slightly older than their counterparts without rights, but the difference is relatively small. Figure 2 visualizes fertility rates over time. The probability that a married white woman in our sample gave birth dropped from about 0.25 to 0.17, and the number of kids under age five dropped from about 1.4 to a little over 1 over our sample time period.

Panel B of Table 1 reports statistics for women age 40-59, with husbands up to age 70. We only use this sample to examine labor force participation, which is available starting in 1860. Labor force participation is low throughout our sample time period. Women (their husbands) in the sample are roughly 47 (51) years old, which is constant across time and between those with and without rights. Panel C examines men age 20-50 in our sample, which we use to examine the potential impact of women's rights on the marriage market. Roughly 2-4 percent of them are newly-weds, as defined by being married in the previous year. The average age of newly married men is approximately 26-30 and roughly constant over time and between states with and without rights. Similarly, the age gap with their spouse is between 4 and 5 years and roughly constant over our sample. The age gap is somewhat higher in states without rights.

Table 2 reports similar statistics for our 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 in 1900 (1910) is 0.215 (0.186) for those married without rights. This is higher than its equivalent of 0.191 (0.166) for those married with rights. The number of children under age 5 at home for those without rights in 1900 (1910) is 1.246 (1.167). This is higher than its equivalent of 1.167 (1.029) for those married with rights. The average age of the wife is between 29 and 30 while the husband is about 34, with no major difference between types of couples.

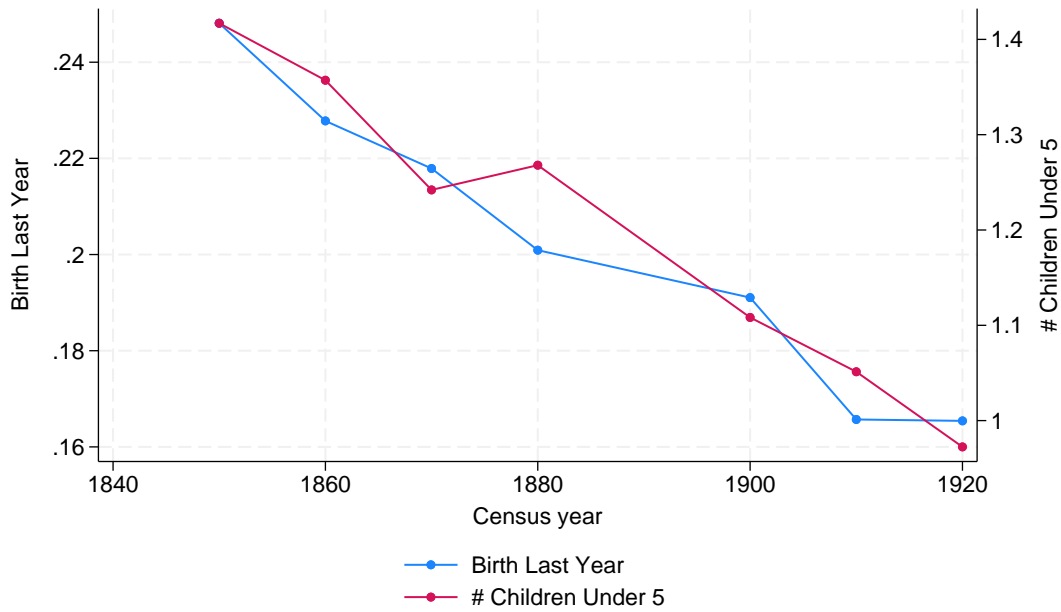


Figure 2: **Fertility Rates Over Time.** Summary statistics on fertility for white, non-Hispanic married women 20-39 in the US from 1850-1920. Variables as defined in the text.

Panel B of Table 2 shows summary statistics for the sample of women age 45-59. For couples married prior to rights being granted, the number of children ever born in 1900 (1910) is 6.094 (5.955), which is higher than the number of children ever born for couples married after rights, which stood at 4.266 (4.449). Similarly, the number of surviving children in 1900 (1910) for couples married before rights was 4.711 (4.648), which was higher than that of those married after rights, which stood at 3.334 (3.521). The number of children ever born, conditional on having any children, was also higher in 1900 (1910) for those married before rights, 6.364 (6.284), than those married after rights, 4.785 (4.909). The ratio of children surviving to children ever born (conditional on having any children) is constant over this time period and between those who were married with and without rights at approximately 80%. In both samples the age of women married after rights was about a year younger than for those married before rights, while their husbands were between 2-3 years younger.



### 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:

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

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, in state  $s$ , county  $c$ , and year  $t$ ,  $t \in \{1850, 1860, \dots, 1920\}$ ,  $rights_{st}^k$  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,  $\lambda_s$ ,  $\lambda_{s'}$ , and  $\lambda_t$  are state of residence, state of birth, and year fixed effects, respectively, and  $X'_{hsct}$  contain controls variables, such as age, that depend on the specific exercise being performed.<sup>25</sup> 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  $rights_{st}^k$ , 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  $rights_{st}^0$  takes the value of one for New Jersey in 1880, while the dummy variable  $rights_{st}^{20}$  takes the value of one for New Jersey in 1900.

We next discuss the construction of county-border pairs, detailed fully in Online Appendix C. The data on the evolution of U.S. historical county bound-

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<sup>25</sup>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.

aries 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 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 process that allows for a consistent dataset, as described in Online Appendix C, 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.<sup>26</sup> Maps showing our data on borders over time can be seen in Online 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.<sup>27</sup> 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 rights

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<sup>26</sup>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).

<sup>27</sup>This methodology of replicating observations for each county-border pair is as in Dube et al. (2010).

because of changing fertility rates? The historical record seems to suggest not.<sup>28</sup> Furthermore, states granted rights, which were then overturned by the courts, often due to unforeseen 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.<sup>29</sup> 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. We perform randomization exercises in Online Appendix F 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 regressions 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.

We include a battery of robustness tests. One worth discussing here 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 Gardner et al. (2024). The results are qualitatively and quantitatively 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:

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<sup>28</sup>The reasoning behind granting women rights seems to have been to protect women against delinquent husbands.

<sup>29</sup>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 Online Appendix E we show that this is not the case. Specifically, in every year, we compute the average fertility, 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.

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

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.<sup>30</sup>  $\lambda_t$  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:

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

where all variables are as described above, and parameters  $\hat{\beta}_{c,b(c)}, \hat{\gamma}_{c,b(c)}, \hat{\lambda}_s, \hat{\lambda}_{s'}, \hat{\delta}$  are as estimated in Equation (2). Under the parallel trends assumption, this estimator is unbiased (for more, see Gardner et al., 2024). We block-bootstrap standard errors, as described in Online Appendix G.

#### 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.

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

where  $Y_{hsct}$  is our outcome variable of interest as 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

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<sup>30</sup>We do not include county border pair linear time trends, as cases would involve not including one side of the border.

county-year, and  $X'_{hscf}$  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. We find below that women's rights did not affect the propensity to marry, the age of marriage, or the age gap between spouses, though are data do not permit us to analyze marital sorting. In Online Appendix H, we provide evidence that couples in our sample did not time their marriage around the granting of women's rights. Given the issues with the timing of women's rights discussed above – especially that the courts often struck down legislation – the lack of timing marriage around rights is perhaps not surprising.

We perform similar robustness tests as in the event-study design. We also perform an analysis dropping couples married immediately before/after the reform. The randomization exercises are reported in Online Appendix F.

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.

## 5 Results

### 5.1 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 children under age five in the household. The controls in vector  $X_{hscf}$  include fixed effects for the wife's age and the husband's age, both interacted with year fixed effects. Our main 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. We include robustness analyses that do not use these controls, as they are plausibly endogenous to fertility choices.

Table 3 reports the results of these regressions. Columns 1 and 2 analyze whether there was a birth last year. Column 1 uses our standard estimator from Equation (1), and is thus our benchmark specification, while Column 2 uses the two-step estimator described above. Columns 3 and 4 repeat this pattern, but use the number of children under age 5 as the dependent variable. In both columns 1 and 2, the estimates prior to rights are quantitatively close to zero with no pattern or statistical significance, consistent with no differential pretrends in fertility. Upon granting women's rights, there is a 0.6 percentage point (p.p.) decrease in the probability of giving birth in both specifications, with the estimates significant at the 1% level.<sup>31</sup> A decade later, the probability of giving birth drops by 1.2 p.p. in both specifications, with again the estimates significant at the 1% level. In column 1, the estimates remain roughly constant at 1.2-1.4 p.p. for two and three or more decades after rights are granted, with the estimates significant at the 1% level. In column 2, the estimates lose significance starting two decades after rights were granted, though three decades after rights the estimates are within a standard error of the estimates in column 1.

Figure 3 visualizes these estimates. This figure shows the lack of a trend in fertility (in point estimates), relative to our controls, prior to rights being granted, and a sharp, dynamic decrease in fertility thereafter. Considering that the average probability of giving birth was about 0.20, corresponding to roughly 4 births over a twenty-year horizon, the magnitude of the estimates ranges from a decline of about 3-3.5% when rights are granted to a decline of 6-8% three decades after rights are granted.

We next turn to Columns 3 and 4, analyzing the impact of rights on the number of children under age 5. In both specifications, there is no indication of a pretrend in declining fertility prior to women's rights.<sup>32</sup> We again see an immediate and dynamic impact of rights on fertility, with the number of children under age 5 in

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<sup>31</sup>As discussed above in Section 4.3, we "round up" the date that women's rights were granted. This implies that "Upon granting women's rights..." is on average 4.5 years after women were actually granted rights.

<sup>32</sup>In Column 4 the two estimates prior to women's rights are statistically significant, but negative, indicating that, if anything, fertility was slightly rising before women's rights were granted. Quantitatively, these estimates are quite small, however.

Column 3 (Column 4) decreasing by 2.8 p.p. (4.2 p.p.) immediately after rights are granted to 7.8 p.p. (4.2 p.p.) three decades after rights are granted. All estimates are statistically significant at the 1% level, with the exception of the estimate on 3 decades after rights are granted in Column 4, which is significant at the 10% level. We note that the estimates in these specifications are approximately 5 times the estimates of their counterparts in Columns 1 and 2, which is to be expected. Figure 4 visualizes these findings.

In Online Appendix D we perform a number of exercises to test the robustness of our findings and further break them down. In particular, we perform robustness exercises, including an analysis that examines the differential impact of rights on border counties versus interior counties, an analysis by the age of the woman, an exclusion of plausibly endogenous controls such as a husband's industry and occupation, an exclusion of the border between South and non-South states and between community property and non-community property states, an exclusion of people who moved states (and thus we cannot tell for sure where they were married), and an attempt to conduct an event study on women married after rights were granted. Our exercises collectively indicate that our results are robust, there are no significant differences between interior and border counties. We also perform an analysis comparing migrants from states with rights to their neighbors in states without rights. These migrants have lower fertility rates as well, with similar estimates to the exercises presented above, suggesting that women's rights did not have a differential impact by selection into migration. Finally, we find no heterogeneous effect of women's rights on states that granted rights later rather than earlier.

Online Appendix F 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. The point estimates in our benchmark exercises show an increasing magnitude of the effect of rights over time (though not with the two step estimator). We hypothesize that the decline in fertility is driven mostly by

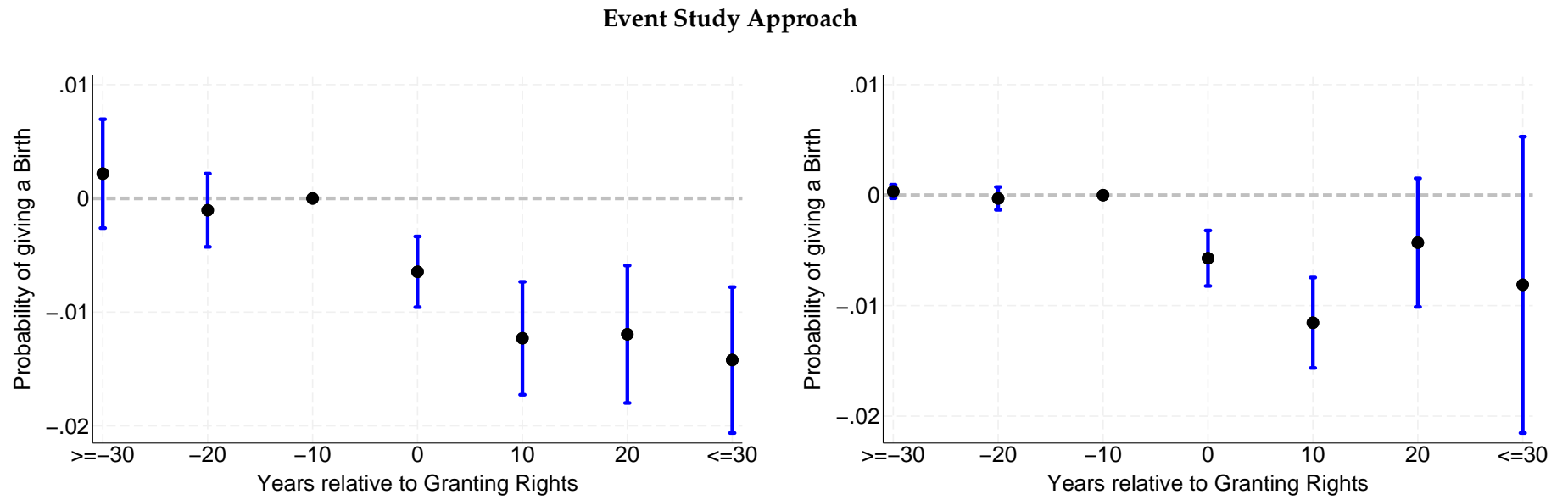


Figure 3: Left: Probability of Birth (Benchmark, Column 1); Right: Probability of Birth (Two-Step Estimator, Column 2); Table 3.



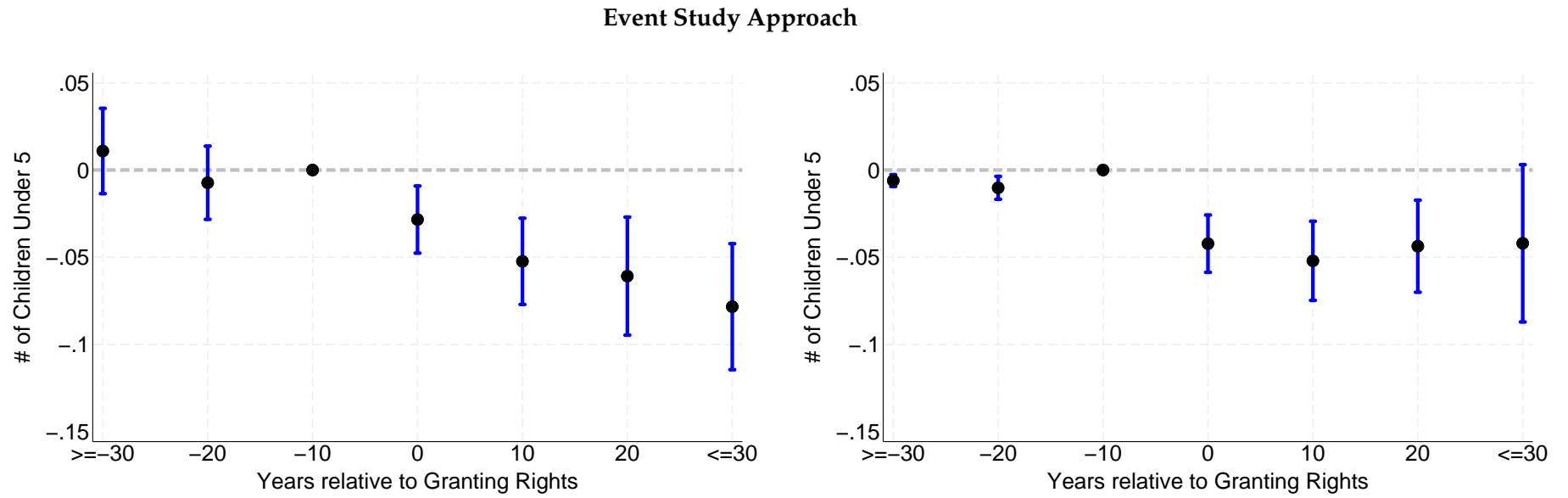


Figure 4: Left: Number of Children Under 5 (Benchmark, Column 3); Right: Number of Children Under 5 (Two-Step Estimator, Column 4); Table 3.

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.

## 5.2 Other Outcomes: Event Study Approach

We next use our event study approach to analyze other outcome variables, specifically related to married women's labor force participation rates (LFP) and the marriage market.

Table 4 reports our findings. We begin with Column 1 (2), which explores the LFP of white non-Hispanic US born married women in our sample ages 20-39 (40-59) with spouses up to age 50 (70).<sup>33</sup> There is no economically or statistically significant trend in women's LFP prior to women's rights. Afterwards, there is no statistically significant rise in LFP. Two decades after rights are granted there is an increase of 1.1-1.4 p.p. in LFP that is statistically significant at the 5% level. However, this effect disappears three decades after rights. We conclude that from this exercise that there is no robust evidence indicating married women's LFP increased after rights were granted.

We next turn to our marriage market exercises. We take the sample of white, non-Hispanic men ages 20-50 who were born in the US. In Column 3, we ask whether these men were married in the previous year, that is, are "Newly Weds" (NW), and perform our event study including controls for their occupation, industry, and age all interacted with year fixed effects. We find no discernable relationship between women's rights and the propensity to get married. In Column 4, we restrict attention to the newly wedded men and ask how old they are. We again include controls for occupation and industry, interacted with year fixed effects. We find no systematic relationship between the timing of women's rights and the age of men when they get married. Indeed, all estimates are positive relative to right before rights are granted –including those prior – and none are statistically significant. In Column 5 we again use the sample of newly-wedded men and uses the age gap with their spouse as the dependent variable. Again, there is no pattern of the age gap with respect to the timing of women's rights. We conclude with caution that the

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<sup>33</sup>As a reminder to the reader, this exercise begins in 1860, when labor force participation rates of married women become available in the census.

marriage market was not affected by women's rights. The caution expressed here is that we are unable to measure marital sorting, which may have indeed changed.

### 5.3 Fertility: Couples Married Before/After Rights

We estimate models along the lines of those described in Equation (4), and report our findings in Table 5.

In Columns 1-3 we use the sample and dependent variables described above in the event study, namely married white non-Hispanic women age 20-39 who were born in the US, and studies the probability of having given birth in the last year, the number of children under age 5, and whether or not the woman is participating in the labor force. Column 1 regresses whether there was a birth last year on whether the couple was married after rights, and includes controls for the age of each spouse, county of residence, duration of current marriage, the industry and occupation of the husband, all interacted with year fixed effects, as well as birth-place fixed effects. We find that being married after rights were granted reduced the probability of giving birth by about 0.9 p.p., with the estimate significant at the 1% level. This remarkably similar to the findings in the event study documented above. Column 2 repeats Column 1 but switches the dependent variable to be the number of children under age 5. The estimates imply a decrease of 0.116 children under age 5, with the effect significant at the 1% level. This is quantitatively larger than those implied by Column 1. That is, if the probability of giving birth declines by 0.9 p.p., then we'd expect the number of kids under age 5 to decline by about 0.045. The estimates here are 2-3 times larger. Similarly, the estimates here are larger than those documented in the event-study approach.<sup>34</sup> Together, these findings reinforce the idea that declines in fertility are being driven by couples married after rights were granted. Column 3 again repeats Column 1, but replaces the dependent variable to be LFP. We find no impact of women's rights on LFP.

In Columns 4-8 we switch the sample to US born married white non-Hispanic women age 45-59. These women have most likely completed their fertility, and are thus good candidates to study the overall impact of women's rights on fertility.

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<sup>34</sup>One potential explanation is that couples married after rights might time their fertility differently. In Online Appendix D.2 we find that the decline in the number of children under 5 is much larger for younger couples (where the wife is under 25) than older couples. The estimates for other age group line up well with the idea that the number of kids under 5 should decline by about five times as much as the probability of giving birth, suggesting there is indeed a slight effect of delaying fertility.

Column 4 regresses children ever born (CEB) on whether the couple was married after rights, using the same controls as Columns 1-3. We find that being married after rights reduces CEB by 0.189 children, with the estimate significant at the 1% level. Notice that this estimate is consistent with women's rights reducing the probability of giving birth by about 1 p.p. for 20 years. Column 5 repeats Column 4, but conditions on having ever given birth. The estimate is quite similar, suggesting that fertility decrease was largely along the intensive margin of fertility.<sup>35</sup> Column 6 repeats Column 5 but replaces the dependent variable with the number of surviving children.<sup>36</sup> The number of surviving children decreases by 0.147 children, with the estimate significant at the 1% level. We then calculate the "Surviving Ratio," which is the ratio of surviving children to children ever born. Column 7 examines whether or not the surviving ratio changes for couples married after rights were granted, and finds no effect whatsoever. This suggests that couples always agreed that they wanted a child to survive conditional on being born. It also suggests that there was no change in gender preference of children.<sup>37</sup> Column 8 examines how being married after rights were granted affects LFP and finds no effect.

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

As with the event study, in Online Appendix D we perform a number of exercises to test the robustness of our findings and further break them down. In particular, we redo our exercises separately for border counties rather than interior counties, by age of the woman, and similar robustness exercises such as not including the plausibly endogenous controls for a husband's industry and occupation, dropping the border between South and non-South states, between community property and non-community property states, and a "donut" exercise where we drop couples right before and after rights are granted. Our exercises collectively indicate that our results are robust. Furthermore, as discussed above, they indicate that there may be a slight effect of delay in fertility in addition to the overall decline in fertility documented here. It is possible that the LFP of these particular women increases

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<sup>35</sup>The sample sizes are roughly the same as well. Most women in our sample were mothers.

<sup>36</sup>The census asked how many children are surviving at the time of the census, rather than how many survived until a given age.

<sup>37</sup>That is, suppose there is son preference prior to women's rights. We might expect to see women's rights increase the probability of a daughter surviving. We do not see such an effect.

while they are young, but the effect disappears by age 25.

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 a bit more than 1 percentage point. Thus we find that households decreased their fertility along the intensive margin. Online Appendix Figure A.13 visualizes these 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 24.9% to 17.1% over the course of our time period, with women's rights accounting for up to 1.4 p.p. of this 7.8 p.p. decline (Column 1 of Table 3), or 18%.<sup>38</sup>

#### 5.4 Taking Stock

Before turning to the potential mechanisms behind our findings in Section 6, we pause to address several key considerations regarding our identification strategy and to consolidate the evidence supporting our interpretation. How should we consider the event study together with this married-after approach?

First, one might worry about anticipation effects: if individuals foresaw the reforms well in advance, the year of passage could become an imprecise treatment date for the event study. However, as discussed in Section 3.1, legislatures passed laws that courts initially interpreted conservatively, making the precise timing of effective rights uncertain. In addition, Online Appendix Figure A.12 shows that couples did not appear to time their marriages strategically around the passage year.

Second, the event-study estimates in Section 4.3 reveal a gradually increasing effect over the decade following enactment. We interpret this as cohort replacement: each

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<sup>38</sup>Similarly, the number of kids under age 5 declines from 1.42 in 1850 to 0.99 in 1920, for a change of 0.43 kids. Our estimates can account for up to 0.078 of this decline, or 18% (Column 3 of Table 3).

census wave contains a larger share of marriages formed after women's rights were granted, so the difference grows over time, but converges roughly 10-15 years after rights were granted.<sup>39</sup> The married-after design in Section 4.4, which precisely matches marriage year and law year, yields consistent findings, reinforcing the role of cohort replacement rather than a pure time trend.

Third, it is reassuring that, prior to adoption, fertility in bordering counties appears similar on both sides of each state border, suggesting parallel trends. Although this cannot fully prove the absence of anticipatory behavior, it supports our identifying assumption.

Finally, in Online Appendix F, we present a placebo exercise that assigns law-passage years randomly. Unlike the true adoption pattern, these random dates are not associated with any systematic fertility shifts, suggesting that the actual legal reforms are indeed responsible for our main results.

As a separate note, how should we consider the findings on the decline in children ever born and surviving children as a function of women's rights? Doepke (2005) argues that declining child mortality rates decrease children ever born, as women need to get pregnant fewer times for the same net fertility rate. However, net fertility increases as the cost of a given number of surviving children decreases. Here, we find that women's rights decreases both children ever born and surviving children, implying that child mortality was not the main driving force behind our findings. However, maternal mortality risk may have been a key concern, as discussed below.

## **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. We formalize the intuition that shifting bargaining power towards women can decrease fertility in a household bargaining model in Section I of the Online Appendix. We further show that this effect is stronger when there is greater maternal mortality risk or when households are wealthier. Thus, the model provides a conceptual framework to understand the empirical findings of the paper, including those discussed below.

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<sup>39</sup>Footnote 31 clarifies that the first post-reform wave in the event-study occurs around four to five years after a state's law was passed, because we use decennial census data.

## 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 (Albanesi and Olivetti, 2016). Some of the risks are hard to comprehend from the point of view of modern society. One particularly horrible result of giving birth was fistulas, which could leave a woman incontinent. This condition "made them unpleasant companions, and even their loved ones found it hard to keep them constant company" (Leavitt, 1986, p. 137). The risk inherent in pregnancy was not lost on the women of the time period, who lived in "the shadow of maternity," as documented in many diaries (Leavitt, 1986).

So great was women's fear of childbirth that it was associated with the thriving abortion industry of the 19th century (Mohr, 1978). The abortion rate in the US was as high as 1 per every 5-6 live births by 1860, while many other estimates through

the 1870s were significantly higher, even as much as a third of all pregnancies Mohr (1978, pp. 50-82). Mohr (1978, p. 170) writes that “[o]ccasionally, a physician would even recognize and acknowledge the deep fear of pregnancy and childbirth instilled in many nineteenth-century women, for whom those processes held a very real prospect of death. As some doctors pointed out, many women considered abortion a cure, an escape from a situation many women themselves considered pathological and frightening.”

Given these risks, 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 larger in states with the higher maternal mortality rates, as argued formally in Online Appendix I. 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 5.<sup>40</sup>

Table 6, Panel A, Column 1 repeats Table 5 Column 1, while Column 1 of Panel B does so for Column 4. The next two 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 Figure 5. Columns 4-6 repeat this pattern for the number of kids under age 5 (Panel A) and surviving children (Panel B).

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 substantially larger than in other states. The probability of giving birth dropped by approximately an

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<sup>40</sup>Figure 5 does not include the 4 states that gave rights after 1920, since it is unclear how coverage was enforced after the 19th amendment was passed.



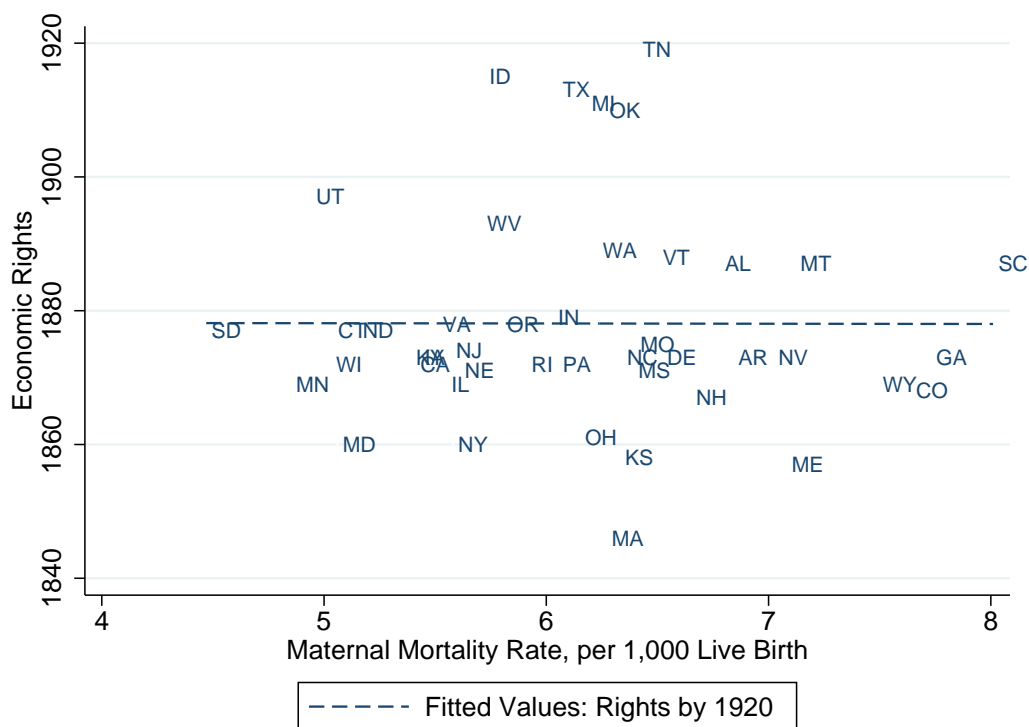


Figure 5: **Women’s economic rights and Maternal Mortality Rate.** Data on maternal mortality rate is from Albanesi and Olivetti (2014).

additional 50-150% in high risk states, while the number of kids under 5 dropped by an additional 45-70%. For measures of completed fertility, high risk states saw more than twice the decline in fertility as other states.

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 result 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 0.189 pregnancies, gained 2.5 extra months. However, women in high risk states, who reduced their fertility by 0.505 pregnancies, gained about 6.7 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 about 1.3%.

Since property rights affect people with property, wealthier people should respond more to women’s rights (again as in Online Appendix I). 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 7 shows our findings. Column 1 has the dependent variable of whether the woman gave birth last year. Women’s rights are associated with a 0.7 p.p. decrease, with the estimate significant at the 1% level. This is remarkably similar to our results in Section 5.1, 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 0.6 p.p., with the estimate significant at the 5% level. High wealth households in general have somewhat lower fertility, but the estimate is not significant. However, wealthy households reduce their fertility after women’s rights by an additional 0.6 p.p., with the estimate significant at the 10% 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.

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 (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).<sup>41</sup> 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. Indeed we include such a bargaining protocol in our model in Online Appendix I.

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). 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 as discussed above.

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

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<sup>41</sup> 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 non-cooperative game allowing for a divorce threat. Doepke and Kindermann (2019) also use a model with non-cooperation as an outside option for couples deciding on having extra children.

Another mechanism present in the literature is from Gay et al. (2023). They argue that women's ability to inherit property as a result of the French Revolution decreased and delayed marriage, and thus fertility, and explain that inheritances increased the outside option of women to marriage. Our setting is quite similar, and thus one might reasonably assume that women's rights would yield a similar outcome. However, as noted above, we find no impact of women's rights on the marriage market, and thus this mechanism does not seem to be the main one at play.

Finally, as discussed above, Doepke and Tertilt (2009) theoretically argue that women's rights increases education and thus 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 account 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. We find that legal changes can account for up to 20% of the changed in fertility during the demographic transition in the U.S. We analyze several mechanisms and conclude that a shift in household bargaining power can account for the changes we document. In particular, it seems that maternal mortality risk was a likely underlying cause of spousal disagreement over the number of children.

Our analysis leaves open questions, such as what happened to the children of those married after rights were granted as well as why married women's rights seemingly had little impact on the marriage market (though we cannot currently measure marital sorting). We leave these questions to future research.

**Author contributions:** Moshe Hazan, David Weiss, and Hosny Zoabi conceived the research, assembled the data, conducted the analyses, and wrote the manuscript.

**Competing interests:** The authors declare no competing interests.

**Data availability:** The raw data is publicly available from IPUMS. We provide code that together with the downloadable data from IPUMS enables one to replicate all of the results reported in the paper and Online appendix.

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

Rights	1850		1860		1870		1880		1900		1910		1920	
	No Rights	Rights	No Rights	Rights	No Rights	Rights	No Rights	Rights	No Rights	Rights	No Rights	Rights	No Rights	Rights
Panel A: Women age 20-39														
Birth Last Year	0.254 (0.436)	0.182 (0.386)	0.242 (0.428)	0.190 (0.393)	0.237 (0.425)	0.194 (0.396)	0.228 (0.419)	0.197 (0.398)	0.215 (0.411)	0.190 (0.392)	0.192 (0.394)	0.166 (0.372)	0.191 (0.393)	0.170 (0.376)
# Children < 5	1.453 (1.035)	1.034 (0.932)	1.438 (1.027)	1.143 (0.973)	1.332 (1.007)	1.126 (0.976)	1.430 (1.050)	1.227 (1.021)	1.252 (1.047)	1.094 (1.026)	1.225 (1.030)	1.041 (1.008)	1.167 (1.013)	0.988 (0.985)
LFP			0.061 (0.239)	0.034 (0.182)	0.013 (0.112)	0.012 (0.107)	0.028 (0.166)	0.026 (0.158)	0.025 (0.157)	0.026 (0.160)	0.052 (0.222)	0.046 (0.210)	0.032 (0.175)	0.042 (0.200)
Age	28.98 (5.448)	30.12 (5.392)	28.81 (5.463)	29.61 (5.358)	28.77 (5.457)	29.32 (5.398)	28.82 (5.447)	29.06 (5.411)	28.96 (5.472)	29.69 (5.405)	29.04 (5.457)	29.74 (5.419)	28.97 (5.456)	29.84 (5.369)
Spouse's Age	33.53 (6.684)	34.12 (6.380)	33.61 (6.778)	34.36 (6.500)	33.55 (7.086)	34.20 (6.737)	33.11 (6.944)	33.63 (6.709)	33.94 (6.968)	34.18 (6.642)	33.89 (6.854)	34.01 (6.663)	34.39 (6.966)	33.99 (6.657)
N	1,129,524	90,624	1,112,675	401,238	1,081,505	708,359	385,586	2,114,790	266,361	3,558,626	433,054	4,574,554	99,538	6,017,159
Panel B: Women age 40-59														
LFP			0.058 (0.234)	0.035 (0.184)	0.012 (0.108)	0.011 (0.102)	0.023 (0.151)	0.024 (0.152)	0.028 (0.165)	0.030 (0.215)	0.050 (0.217)	0.046 (0.209)	0.031 (0.174)	0.038 (0.191)
Age			47.16 (5.364)	47.38 (5.410)	47.37 (5.412)	47.35 (5.381)	47.10 (5.362)	47.32 (5.420)	47.16 (5.292)	47.11 (5.326)	47.11 (5.312)	47.22 (5.281)	46.78 (5.214)	47.11 (5.219)
Spouse's Age			50.91 (7.510)	51.04 (7.256)	51.69 (7.571)	51.57 (7.259)	51.73 (8.137)	51.79 (7.601)	50.88 (7.772)	50.67 (7.646)	51.57 (7.475)	51.06 (7.300)	51.74 (7.891)	50.86 (7.388)
N			522,299	238,341	546,921	383,342	173,824	1,023,295	134,728	1,970,243	192,620	2,560,319	41,214	3,448,138
Panel C: Men age 20-50														
Prob. Newly-Wed	0.028 (0.165)	0.021 (0.145)	0.033 (0.179)	0.026 (0.158)	0.018 (0.133)	0.016 (0.126)	0.012 (0.111)	0.013 (0.111)	0.036 (0.186)	0.033 (0.179)	0.039 (0.193)	0.036 (0.185)		
N	1,441,905	132,672	1,400,480	530,876	1,344,560	875,015	483,060	2,524,726	345,614	4,586,991	541,865	5,899,827		
Age Newly-Wed	26.61 (6.068)	28.12 (6.620)	28.19 (7.516)	29.91 (7.762)	26.23 (6.066)	26.83 (6.024)	26.81 (6.552)	26.81 (6.131)	27.49 (6.719)	27.70 (6.352)	27.50 (6.800)	27.88 (6.733)		
Age Gap Newly-Wed	4.677 (4.942)	4.296 (5.114)	4.862 (5.222)	4.897 (5.161)	4.322 (5.166)	4.471 (4.914)	4.521 (5.521)	4.293 (5.154)	4.960 (5.970)	4.237 (5.669)	4.917 (5.620)	4.027 (5.299)		
N	40,467	2,846	46,157	13,544	24,203	14,124	6,005	31,588	12,244	154,576	21,104	210,508		

Notes: The sample is married, white non-Hispanic households that lived in counties bordering counties in other states between 1850 and 1920. These are summary statistics for the event study approach by year and whether observations were from states with women's rights.

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

	1900		1910	
	Before	After	Before	After
Panel A: Ages 20-39				
Birth Last Year	0.215 (0.411)	0.191 (0.393)	0.186 (0.389)	0.166 (0.372)
# Children under 5	1.246 (1.051)	1.082 (1.018)	1.167 (1.023)	1.029 (1.002)
Age	29.71 (5.555)	29.70 (5.378)	29.24 (5.462)	29.81 (5.390)
Spouse's Age	34.45 (6.906)	33.99 (6.605)	33.73 (6.797)	33.91 (6.614)
N	565,784	3,823,132	726,177	4,925,066
Panel B: Ages 45-59				
Children Ever Born	6.094 (3.410)	4.266 (3.153)	5.955 (3.503)	4.449 (3.216)
Surviving Children	4.711 (2.803)	3.334 (2.546)	4.648 (2.882)	3.521 (2.627)
Children Ever Born (CEB>0)	6.364 (3.229)	4.785 (2.943)	6.284 (3.299)	4.909 (3.025)
Surviving Ratio	0.791 (0.216)	0.808 (0.240)	0.799 (0.218)	0.815 (0.235)
Age	51.55 (4.082)	49.65 (3.906)	51.81 (4.308)	50.14 (3.836)
Spouse's Age	55.09 (6.475)	52.28 (7.135)	55.64 (6.503)	53.41 (6.401)
N	620,548	894,862	344,681	1,625,271

*Notes:* Naturally, there are fewer observations for CEB>0, and Surviving Ratio. In particular, there are 594,179 and 797,710 observations in 1900 for women married before and after rights, respectively. Likewise, there are 326,637 and 1,472,911 observations in 1910 for women married before and after rights, respectively.

Table 3: Fertility: Event Study

Dep. Var.	Flow Measures of Fertility			
	(1)	(2)	(3)	(4)
	Birth Last Year		# Children under 5	
$\geq 3$ Decades Before	0.002 (0.002)	0.000 (0.000)	0.011 (0.012)	-0.006*** (0.000)
2 Decades Before	-0.001 (0.002)	-0.000 (0.001)	-0.007 (0.011)	-0.010*** (0.003)
1 Decade Before	0	0	0	0
Rights Given	-0.006*** (0.002)	-0.006*** (0.001)	-0.028*** (0.010)	-0.042*** (0.008)
1 Decade After	-0.012*** (0.003)	-0.012*** (0.002)	-0.052*** (0.013)	-0.052*** (0.012)
2 Decades After	-0.012*** (0.003)	-0.004 (0.003)	-0.061*** (0.017)	-0.044*** (0.013)
$\geq 3$ Decades After	-0.014*** (0.003)	-0.008 (0.007)	-0.078*** (0.018)	-0.042* (0.023)
Controls	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes
Sample	All	All-Two Step	All	All-Two Step
N	21,973,593	20,434,899	21,973,593	20,434,899
Adj. $R^2$	0.028	–	0.124	–
Mean Dep. Var.	0.198	0.194	1.173	1.157

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, fixed effects for both the wife's and husband's ages, interacted with year fixed effects, as well as husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Additionally they include state fixed effects, birthplace fixed effects, and year fixed effects. The sample includes all married, white non-Hispanic women born in the US. Columns 1 and 2 analyze whether the woman gave birth in the previous year while 3 and 4 look at the number of children under age 5.

Table 4: LFP and the Marriage Market

Dep. Var.	(1)	(2)	(3)	(4)	(5)
	Labor Force Participation		Marriage Market-Men 20-50		
	LFP		Prob.	Age	Age-Gap
	20-39	40-59	NW	NW	NW
$\geq 3$ Decades Before	-0.001 (0.008)	-0.004 (0.008)	0.001 (0.001)	0.123 (0.261)	-0.185 (0.131)
2 Decades Before	-0.001 (0.005)	-0.002 (0.005)	0.001 (0.002)	0.321 (0.244)	-0.024 (0.090)
1 Decade Before	0	0	0	0	0
Rights Given	0.004 (0.003)	0.002 (0.002)	0.000 (0.001)	0.266 (0.194)	-0.039 (0.130)
1 Decade After	0.007 (0.004)	0.004 (0.004)	-0.001 (0.001)	0.168 (0.199)	0.158 (0.201)
2 Decades After	0.014** (0.006)	0.011** (0.005)	-0.001 (0.001)	0.107 (0.293)	-0.014 (0.248)
$\geq 3$ Decades After	0.007 (0.006)	0.008* (0.005)	-0.002 (0.001)	0.205 (0.323)	-0.239 (0.292)
N	20,753,445	11,235,284	20,107,591	577,366	577,366
Adj. $R^2$	0.046	0.036	0.069	0.101	0.038
Mean Dep. Var.	0.035	0.034	0.029	27.54	4.39

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. Additionally they include state fixed effects, birth-place fixed effects, and year fixed effects. Columns (1) and (2) also include fixed effects for both the wife's and husband's ages, interacted with year fixed effects, as well as husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column (3) also includes age fixed effects interacted with year fixed effects. The dependent variable in Column (3) is the probability of getting married within the past year, the dependent variable in Column (4) is the age of men who got married within the past year, and the dependent variable in Column (5) is the age gap between spouses who got married within the past year.

Table 5: Fertility, Married After Rights 1900-1910

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Women Age 20-39			Women Age 45-59				
Dep Var:	Birth Last Year	# Children Under 5	LFP	CEB	CEB>0	Surviving Children	Surviving Ratio	LFP
Married After Rights	-0.009*** (0.002)	-0.116*** (0.025)	-0.000 (0.001)	-0.189*** (0.059)	-0.183*** (0.054)	-0.147*** (0.049)	-0.001 (0.001)	0.000 (0.001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	10,040,159	10,040,159	10,040,159	3,485,362	3,191,435	3,191,435	3,191,435	3,485,362
Adj. $R^2$	0.052	0.201	0.053	0.261	0.234	0.213	0.027	0.035
Mean Dep. Var.	0.180	1.072	0.034	4.844	5.290	4.146	0.807	0.036

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, 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. They also include husband's occupation and husband's industry fixed effects, interacted with year fixed effects, and birthplace fixed effects.

Table 6: Fertility, Married After Rights , 1900-1910 by MMR

	Baseline	(2)	(3)	Baseline	(5)	(6)
Panel A: Women Age 20-39						
Dep. Var.	Birth Last Year			# Children Under 5		
Married After Rights	-0.009*** (0.002)	-0.006*** (0.002)	-0.008*** (0.002)	-0.116*** (0.025)	-0.098*** (0.021)	-0.106*** (0.022)
Married After Rights × High MMR		-0.009*** (0.003)	-0.005* (0.004)		-0.067*** (0.018)	-0.047*** (0.016)
Sample	All	All	Rights ≤ 1920	All	All	Rights ≤ 1920
N	10,040,159	10,040,159	9,856,715	10,040,159	10,040,159	9,856,715
Adj. R <sup>2</sup>	0.052	0.052	0.050	0.192	0.201	0.200
Mean Dep. Var.	0.180	0.180	0.178	1.070	1.072	1.067
Panel B: Women Age 45-59						
Dep. Var.	Children Ever Born			Surviving Children		
Married After Rights	-0.189*** (0.059)	-0.144** (0.055)	-0.154*** (0.055)	-0.147*** (0.049)	-0.110** (0.045)	-0.116** (0.045)
Married After Rights × High MMR		-0.361*** (0.117)	-0.339** (0.121)		-0.305*** (0.110)	-0.284** (0.114)
Sample	All	All	Rights ≤ 1920	All	All	Rights ≤ 1920
N	3,485,362	3,485,362	3,438,518	3,485,362	3,485,362	3,438,518
Adj. R <sup>2</sup>	0.261	0.261	0.259	0.242	0.242	0.241
Mean Dep. Var.	4.844	4.844	4.821	3.796	3.796	3.782

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, and birthplace fixed effects. Columns labeled "Baseline" are Columns (1), (2), (3), and (5) of Table 5. "High MMR" is an indicator that a household is in a state in the top 25% of maternal mortality risk.

Table 7: Fertility, by Wealth 1860-1870

	(1)	(2)	(3)	(4)
Dep. Var.	Birth Last Year		# Children Under 5	
Rights	-0.007*** (0.002)	-0.006** (0.003)	-0.016** (0.007)	-0.005 (0.009)
High Wealth		-0.003 (0.002)		-0.008 (0.011)
High Wealth $\times$ Rights		-0.006* (0.004)		-0.047** (0.018)
Controls	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes
N	3,302,806	3,302,806	3,302,806	3,302,806
Adjusted $R^2$	0.020	0.020	0.106	0.107
Mean Dep. Var.	0.224	0.224	1.313	1.313

Notes: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors, are double clustered at the county-border pair and state levels, 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, all interacted with year fixed effects. "Extra Controls" include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Additionally, they include birthplace and state fixed effects. The sample includes all US born 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.