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Women's Liberation, Household Revolution

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PRELIMINARY AND INCOMPLETE

Abstract

Households, traditionally run by a husband and wife, are responsible for many of the most crucial decisions made in society. Anything that changes the power structure within the household will thus have dramatic implications for the economy at large. In one of the greatest shifts of household bargaining power in human history, common law countries began giving economic rights to married women in the second half of the 19th century. Before this "women's liberation," married women were subject to the laws of coverture, which granted the husband virtually unlimited power within the household. This paper explores the ramifications of coverture's demise on the decision making of households. In particular, we use the full US census from 1850 to 1920 and exploit contiguous county-pairs bordering states that granted rights at different times. Using an event-study design, we show that granting women property rights led to a dynamic decrease in fertility and an increase in the education of children of both sons and daughters. However, we do not find evidence that women's rights affected female labor force participation. Additionally, we exploit the fact that these rights were not granted retroactively, and compare couples married before and after rights were granted. This alternative strategy both confirms our findings and provides evidence for potential mechanisms. We argue that shifting bargaining power from husband to wife is the economic mechanism most likely to account for the documented effects of women's rights.

Keywords: Women's liberation, household bargaining, fertility, education, property rights.

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

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

1 Introduction

Households, traditionally run by a husband and wife, are responsible for many of the most crucial decisions made in society. Anything that changes the power structure within the household will thus have dramatic implications for the economy at large. In one of the greatest shifts of household bargaining power in human history, common law countries began giving economic rights to married women in the second half of the 19th century. Before this "women's liberation," married women were subject to the laws of coverture.[3] Coverture had detailed regulations as to which spouse had ownership and control over property and income, granting the husband virtually unlimited power within the household. Indeed, so great was the husband's power that a common saying was that "man and wife are one, but the man is the one" Williams (1947).[4] This paper explores the ramifications of coverture's demise on the decision making of households. In particular, we use the full US census from 1850 to 1920 and exploit contiguous county-pairs bordering states that granted rights at different times. Using an event-study design, we show that granting women property rights led to a dynamic decrease in fertility and an increase in the education of children of both sons and daughters. However, we do not find evidence that women's rights affected female labor force participation. Additionally, we exploit the fact that these rights were not granted retroactively, and compare couples married before and after rights were granted. This alternative strategy both confirms our findings and provides evidence for potential mechanisms. We argue that shifting bargaining power from husband to wife is the economic mechanism most likely to account for the documented effects of women's rights.

Under coverture, property was divided into two types. Personal property, including money, stocks, bonds, furniture, and livestock, became the husband's property entirely upon marriage. He could sell or give the property away, or even bequeath it to others. Real assets, such as land and structures, were placed under the husband's partial control while remaining in the wife's name. He could manage the assets as he saw fit, including any income generated by the assets,

[1] Separation of property between husband and wife.

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

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

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

but he could not sell or bequeath the property without his wife's consent.[5] A married woman could not contract, and any income she earned in the labor force became her husband's property. Thus, coverture granted the husband virtually unlimited power of the purse within a household. This dynamic changed with the introduction of married women's property laws, which was done by state in the U.S., largely between 1850 and 1920.

Using the staggered nature of coverture's demise across U.S. states, we study the impact of these rights on household decision making, specifically fertility, education, and women's labor force participation (LFP). Massachusetts was the first state to grant married women property rights in 1846. By 1920 all but four states had followed suit. Geddes and Lueck (2002) argue that it may not be fair to call the post-1920 era true coverture, as the 19th Amendment (passed in 1920) granted women the right to vote. This may well have affected the *de facto* implementation of coverture. Accordingly, we use 1850 to 1920 as our sample period whenever the data permit.

We make use of two separate identification strategies. The first 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. These exercises are done using the full census count from 1850 - 1920, with the exceptions that data on education is only available in the 5% sample for 1900, there is no data on female LFP in the 1850 census, and the 1890 census was not used as it was destroyed in a fire. We discuss in Section 3.3 the conditions under which these results can be interpreted as causal and whether or not these conditions apply.

Using this event-study design, we show that there are no differences in trends in household decision making on either side of state borders prior to rights being granted. After rights are granted, we document three facts. First, fertility dynamically decreases by about 3% when rights were granted, and up to 7% three decades after rights were granted. Second, the probability of a child being in school also dynamically increased by about 6-7%. This increase in education was concentrated among children age 10 and under, rather than those over age 10, and there was no quantitative or statistical difference between the effect of women's rights on sons rather than daughters. Third, there is no change in labor force participation (LFP) rates among married women as a result of granting women economic rights.

The second identification strategy exploits the fact these economic rights were not granted retroactively.[6] The 1900 and 1910 censuses asked people about the duration of their current marriage, allowing us to identify couples who were married before and after rights were granted, within a county. The evidence supports the hypothesis that the declines in fertility documented by the

[5] See Blackstone (1896) for the laws of coverture. For a summary of the general responsibilities that husbands and wives had to one another under coverture, see Basch (1982) Tables 1 and 2. We discuss further details of the laws of coverture in Section 2.1.

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

event-study approach are entirely accounted for by people married after rights are granted. Similarly, women married after rights were granted did not change their labor force participation rates relative to those married before rights were granted. We discuss in Section 3.4 the conditions under which these results can be interpreted as causal, and whether or not these conditions apply.

There is more than one potential mechanism that connects women's rights with the changes in the family documented here. Section 5 outlines the various connections made by the literature, and argues that a shift in household bargaining power from husband to wife is the most reasonable mechanism to account for the results documented in this paper. This is for a number of reasons. First, the historical evidence, discussed in Section 2.5, shows that legislatures at the time were concerned about the impact of women's economic rights on the man's ability to dominate his household.[7] Indeed, other papers in both the economics and history literatures emphasize the role of shifting household bargaining power.[8] Second, our findings that women's rights decrease fertility and increase education of children are consistent with the literature on the difference between the preferences of husband and wife, discussed below. Finally, as we discuss in Section 5, there are inconsistencies between the empirical findings and other potentially mechanisms. This is for two reasons. First, the fact that being married after rights were granted can account for much of the changes we see in behavior strongly suggest that the cause of changed behavior was within households affected by the change in rights, rather than an external mechanism (such as a general equilibrium effect of women's rights) that would change behavior for all households, regardless of whether they married before or after rights were granted. Second, mechanisms besides shifting household bargaining power would tend to predict more of an increase of investment in the education of either sons or daughters following women's rights, not an equal increase in education of both types of children, as we discuss in more detail in Section 5.

This is not the first paper to examine how legal changes can affect household bargaining. Wolfers (2006) studies the introduction of unilateral divorce laws in the U.S., which occurred by state, and finds that they increased the probability of divorce. He argues that this finding is evidence that Coase's theorem does not apply in marriage. This yields the prospect that reassigning rights within a marriage, be they rights over property or rights to divorce, may yield changes in household choices. Stevenson and Wolfers (2006) similarly study the change of these laws, and find that they reduced the probability of suicide and spousal homicide. This is due to the fact that unilateral divorce laws transfer the power to end the relationship to the abused spouse. Voena (2015) exam-

[7] The full quote that begins this paper continues "...[t]he present system was one in which confidence and caution were fairly blended; it might be defined as general confidence, regulated according to individual circumstances by sufficient caution, whereas the proposed change would bring into existence a system in which normal mistrust would alternate with spasmodic improvidence." Member of Parliament, Sir Alexander Beresford Hope, during the debate on the Married Women's Property Act of 1870, as described in Hansard (1870a).

[8] As discussed below, Griffin (2003), in reviewing the debate over women's property rights in England, makes clear that men were hesitant to give up their own rights at home. The reasoning, from the historical archive, for granting women property rights seems to be to protect women from abusive husbands who might leave their families impoverished. 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.

ines how the introduction of unilateral divorce laws affected labor supply and savings choices. She studies a sample of households married prior to the changes in laws, and compares the impact of unilateral divorce on couples in states that divide assets between spouses equally upon divorce to those with a title-based regime. Crucial to her analysis is the fact that unilateral divorce, combined with equitable distribution of assets upon divorce, implicitly transfers power to the economically weaker spouse (historically, the wife). When assets are divided equally upon divorce, unilateral divorce leads to more savings, and less female labor supply (and more female leisure).[9] We differ from this literature by looking at how property rights during marriage, rather than than the right to divorce or division of assets upon divorce, affect household bargaining.[10] Doepke and Tertilt (2009) assume that mothers have higher preferences for investment in children than fathers, and employ a theoretical model to argue that men grant women economic rights in order to increase educational investment for the economy at large. While the underlying mechanism, and sets of laws, they consider are similar to those analyzed here, we differ by performing in depth empirical assessments of the impact of women's rights, expand the analysis to include fertility and labor supply, and are thus able to differentiate the proposed mechanism from others in the literature.[11]

There is also a large literature documenting the impact of female bargaining power on fertility and education of children. Central to the claim is the idea than men and women have different preferences over the quantity and quality of children. There is empirical evidence that husbands tend to prefer more children than wives (Rasul, 2008; Doepke and Tertilt, 2018; Doepke and Kindermann, 2019) and that more household income in the wife's hands increases investment in children (Thomas, 1993; Lundberg et al., 1997; Attanasio and Lechene, 2002; Qian, 2008; Bobonis, 2009; Doepke and Tertilt, 2019).[12] One theory as to why this may be is evolutionary theory, with men uncertain over their paternity status or women having more limited reproductive capacity.[13] However, there is another possible mechanism at work. Women bear significant mortality risk in childbearing, especially in developing countries (such as the US in the 19th century), and thus may pre-

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

[10]While we discuss different regimes for dividing assets between husband and wife in the U.S. in Section 2.3, divorce at the time was exceedingly rare. We nevertheless perform robustness analyses on states that divide property differently than the common law would suggest, as discussed below.

[11]Similarly, Fernández (2014) empirically analyzes the connection between women's economic rights and fertility rates. As discussed in Section 2.5, the results documented here contradict those found in her paper. Fernández (2014) includes a role for bargaining between spouses, but does not allow for differences in parental preferences over the number or education of children.

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

[13]Indeed, Doepke and Tertilt (2009) use this theory to justify their assumption that women put a higher weight on child investment than men, in their paper on why men gave women economic rights. See their paper for references to the relevant theoretical and empirical literature on the subject.

fer smaller families (Albanesi and Olivetti, 2014; Ashraf et al., 2014; Albanesi and Olivetti, 2016; Ashraf et al., 2020).[14] Indeed, Albanesi and Olivetti (2016) show that maternal mortality rates remind high well past our sample time period. Similarly, Doepke and Kindermann (2019) document different preferences over the number of children between husband and wife, and attribute them to women bearing the costs of child rearing. We complement these works by documenting how a major reworking of the laws governing property rights within marriage can affect fertility and investment in children's education.

The paper proceeds as follows. Section 2 discusses the history of coverture, the timing of its demise in the U.S., issues related to community property states and equitable estates, and why men chose to give married women property rights. Section 3 discuses the data and empirical strategies used in this paper, including the conditions under which we can take a causal interpretation of our results. Section 4 presents our regression results, including a variety of robustness exercises. Section 5 discusses various economic mechanisms that connect between married women's property rights and fertility, investment in education, and married women's labor force participation. We explain in detail our conclusion that shifting bargaining power from husband to wife can account for the results we document. We conclude in Section 6.

2 Women's Economic Rights

In this section, we give a brief overview of the history of coverture, and then discuss the history of women's economic rights in the U.S. We discuss which laws are relevant for our analysis, and issues related to the timing of rights granted by states. We conclude by discussing hypotheses as to why men granted women economic rights.

2.1 Coverture and Slavery

Where did coverture come from?[15] A good starting place is the debate on married women's property rights in the U.K. House of Commons that took place on May 14, 1870. Mr. Jessel gives a brief overview of the history of coverture and women's legal status in England during an explanation of his support for removing women's economic disabilities that is useful to quote at length:

"The existing law was a relic of slavery, and the House was now asked to abolish the last remains of slavery in England. In considering what ought to be the nature of the law, they could not deny that no one should be deprived of the power of disposition unless on proof of unfitness to exercise

[14] A quantity-quality tradeoff would immediately yield more that reduced fertility implies more investment in children's education.

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

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

The relationship between wives and slaves was not lost on Americans either, with senators discussing definition of freedom including the right to contract, explicitly denied to married women, while discussing the emancipation of slaves. It was not lost on the senators that the rights being granted to freed slaves were not necessarily granted to women (Stanley, 1988). Sufficient to say that the laws of coverture made a wife essentially a slave to her husband, and that granting property rights to women was a major step towards equality.[16]

2.2 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.[17] Property laws were passed by state legislatures, gen-

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

[17]Similarly, rights were granted in waves in England. Married women received partial rights over property in 1870, specifically with regard to certain types of savings/investment accounts and inheritances up to 200 pounds, though the

erally narrowly interpreted by courts (Chused (1983); Zeigler (1996)), and updated again.[18]

We use the data on the timing of women's liberation by state from Geddes and Lueck (2002).[19] They code the year in which states first granted women rights over both their own property and their labor earnings, which we refer to as Geddes and Lueck "both" dates, or *rights*. Their methodology in dating rights is as follows: "[f]or control of property, we used the earliest year a state passed an act allowing married women management and control of their separate estate (similarly for earnings). If a state passed a married woman's property act, but the act did not grant the woman management and control of her separate estate, then this date was not used. This approach provides a specific characterization of married women's property that emphasizes control by the wife" (Geddes and Lueck (2000), p. 65). These statutes were certainly enough to grant substantial power to women.

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

Property laws prior to those studied by Geddes and Lueck (2002), known as "debt statutes," did not significantly affect women's rights. Indeed, Chused (1983, p.1361) argues that "[t]hese acts . . . created a set of assets available for family use when husbands found themselves in trouble with creditors" and concluded that they "made only modest adjustments in coverture law, and generally confirmed rather than confronted prevailing domestic roles of married women."[20] Thus, while these statutes did protect a wife's real and moveable property from her husband's creditors, they did not protect women from their husbands.[21] Accordingly, these statutes did not protect

reform was not always upheld in court. The 1870 law was updated in 1874 to prevent fraud. A more significant update to property rights came in 1882, which more or less granted women the same economic rights as men. Further minor updates occurred over the 20th century (Holcombe (1983), pp.178-205).

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

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

[20] In a fascinating paper, Koudijs and Salisbury (2016) study how these debt statutes, by preserving some family assets in the case of default, affected risk-taking behavior in the U.S. South.

[21] 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 pages 114-115 of Chused and Williams (2016). Before these debt statutes, a wife's

the wife's property or earnings from her husband, and thus change the balance of power in the household.

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

The first reason for using "both" is that there is a high degree of complementarity in these rights. Indeed there is so much complementarity between the rights that it is inappropriate to consider property rights without earnings rights, or vice versa. We first discuss the inability to consider property rights without earnings rights. An example of the complementarity in rights is seen in *Apple v. Ganong* 47 Miss. 189 (1872). In this case, a Mr. Ganong, husband of Louisa Ganong, declared bankruptcy in Mississippi. His creditors sued to gain possession of Louisa's land. At the time, her separate estate was protected from her husband's creditors, but her *earnings* were not. Was her land part of her separate estate? She purchased the land with a combination of money from the sale of a gift of cotton from her mother and earnings from sewing for soldiers during the Civil War. The court ruled that her husband implicitly owned the share of her land that was purchased with her labor income, and thus it was liable for his debts. This case and others like it show how difficult it was to establish property rights when only partial rights existed, strengthening the argument for "both" dates to be used. Indeed, Chatfield (2014) argues that these types of cases help explain why Mississippi granted women rights over their earnings, making investigations into how women purchased property unnecessary.[22]

Similarly, consider *Glover vs. Alcott* 11 Mich. 470 (1863). In this case, Deborah Alcott, a married woman, owned and operated a mill in Michigan after married women were granted the right to own and dispose of all types of property, but *before* they were given the right to their labor market earnings. Husbands still had the right to their wives' time, services, and labor income. The case came down to the question of whether Mrs. Alcott had the right to manage her business for her own benefit, or if this was considered labor income and thus attributed to her husband.[23] The Supreme Court of Michigan ultimately decided that this income indeed belonged her husband, despite the fact that business was performed on her property, by her, and with her property used as collateral for the associated capital. Indeed, Justice Christiancy in his deciding opinion argued that, if women were allowed to take income from a business they owned, nothing could stop them from setting up a pass-through business and circumventing the earnings law, such that she "...would have it in her power to deprive her husband entirely of all right to the time and services

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

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

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

in the care and management of his household." Justice Campbell, dissenting, argued that this ruling would not allow a wife to place a mill on her land, as she could if unmarried, leaving it unproductive. The lack of earnings rights was therefore a serious disability in property rights.

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

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

The second reason that "both" dates may be appropriate is that, given the legal issues that arose around granting rights, governments often needed more than one round of legislation to effectively grant economic rights (Chused (1983); Zeigler (1996)). Consider that property rights were generally granted before earnings rights, but that issues with property rights were often only solved when updating earnings rights. For instance, New York gave married women property rights in 1848. It is therefore curious that the 1860 earnings bill includes explicit protection of women's personal property in Section 2. Why did the legislature include this seemingly redundant protection? Turning to *Dickerman v. Abrahams* 21 Barb. 551 (1854) in the Supreme Court of New York, in which Justice J. Wright gives a legal overview of the 1848 law. Justice Wright explains that the New York legislature made a series of mistakes when passing the law, for instance, the law was interpreted only as providing married women with rights over real estate.[24] 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.

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

2.3 Community Property and Equitable Estates

We next turn to the issue of community property, followed by a discussion of equitable estates. Eight states namely Arizona, California, Idaho, Louisiana, Nevada, New Mexico, Texas, and Washington, had community property laws governing marital asset ownership and control as per the traditions of civil law, rather than common law's doctrine of coverture.[25] Community property divided household assets into three classes: the husband's separate property, the wife's separate property, and community property. According to Spanish law, property of all types acquired after marriage, except by gifts and inheritance, became community property with each spouse having a 50% interest.

At first glance, these laws seem to preclude gender-based property rights discrimination after marriage. However, it is not clear that many people were even aware of the difference in laws between community property and common law states. Schuele (1994, p. 260) describes the history of the Californian constitutional debate, indicating that the Americans involved in the debate had little understanding of the community property system they were adopting on paper, and indeed argues that the constitution "…did not clearly mandate a community property system." The constitution did call for further laws to be passed on the subject. Shortly after ratification, there was a large increase in migration from the rest of the U.S., which pushed California to adopt common law as the jurisprudence of the state. As a result, Schuele (1994, p. 262) argues that "[l]egislatures appear to have been unable to ignore their common-law heritage and may even have been hostile towards property rights of married women. Contrary to the spirit of [the constitution], women were given no management rights over their separate property, much less over the common property."[26]

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

Men had absolute control and interest over their own separate estate. In practice they basically had the same rights over the community property. How did the community property work in practice? Of the great number of migrants into California during this time, "many of them, having reached adulthood in the East, might not have realized that California's marital property laws supposedly differed from those in the rest of the nation" Schuele (1994, p. 262). This is especially true since Basch (1982) notes that, while many details of common law might have been lost on the general public, common law classifications of married women's rights were well represented in

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

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

popular books and magazines of the time. Indeed, the confusion was so deep that two years after women were given property rights in California, state senator Laine was reported considering introducing a bill to reestablish common law in California, apparently unaware that common law had never been imposed in the first place (Schuele (1994)). As such, de Funiak (1943), as cited in Schuele (1994, p. 25), found that "[m]any lawyers trained in the common law ... seem to fail to comprehend ... that the management of the common property placed in the husband was an administrative duty only ... and not in any sense the equivalent of the common law 'control' by the husband to the wife's property which made him virtual owner and gave him the right to appropriate its use to his own enjoyment and benefit." While technically community property was bequeathed according to each spouse's 50% interest in it, the husband had full management rights and "... with almost his last breath, he [could] convey away the community property so deftly that no known law [could] reach it" (Stow (1877), p. 65). That is, the husband could convey or gift community property as he wished up until his death, which perhaps undid any measure by which the woman's interests in community property were protected.

Even more importantly, a husband also had absolute management over his wife's separate property, with the lone stipulation that he must manage it for her "benefit", a poorly defined term. This alone would presume to give him most, if not all, of the power over household financial decisions. Given that people's understanding of property definitions was erroneously based on common law (Schuele (1994)), it is reasonable to assume that most husbands would act as if common law were in force, and not alienate a women's separate real property while doing as they wished with her personal property. This is not just a conjecture, as the difference in treatment between real and personal assets is particularly noticeable in New Mexico. A 1901 state amendment required that wives agree to all sales and mortgages involving the common real estate before such transactions could occur. Courts decided that this did not apply to moveable assets, which remained in the absolute control of husbands (Lyons (1955)). It seems reasonable to assume that other community property states, which are mostly in the west, went through similar experiences.[27] This supports the idea that, in practice, there was not much difference *de facto* between community property states and common law states. Since the basic hypothesis of this study seems to stand in community property states, we include these states in our benchmark exercises. However, we perform robustness exercises in which we drop these community property states, and which verify that these states are not biasing the results of our empirical work.

Finally, there is the issue of separate estates for married women. These separate estates date back to sixteenth-century England, and were run under courts of equity, or "chancery," rather than courts of common law. In principle, they allowed married women to have property put into a separate trust, run by a trustee, that was immune from their husbands' influence. These trusts were either created before marriage or upon receipt of an inheritance. They varied widely in the level of protection from the woman's husband, the amount of control the woman had, and the degree

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

of control the trustee had. In England, these separate estates were prohibitively expensive for all but the wealthiest women (Holcombe (1983)). In America, these trusts, also called "marriage settlements," were even rarer: only 12 states had equity courts (Geddes and Lueck (2002)). American women needed their fiancé's or husband's permission for these trusts to be created (Salmon (1986), p.15) and these estates were deemed fraudulent if created to deceive a fiancé (Salmon (1986), p. 89). In much of New England, such as Connecticut and Massachusetts, the legislatures refused to empower equity courts to enforce the rules. This was a result of Puritan influence, which was harshly opposed to courts of equity (Salmon (1986), pp. 120-140). Other states, such as Pennsylvania, "held an equitable jurisdiction allowing them to enforce trusts for married women, but evidence indicates that the judiciary felt uncomfortable in exercising the full panoply of equitable rules and precedents" (Salmon (1986), p. 186). Indeed, Chatfield (2014, pp. 16-17) argues that "equity courts tended to interpret contracts between husbands and wives narrowly, and with greater deference to creditors than to married women or widows, meaning that there was no sure guarantee that a woman's property would be protected upon becoming married." As such, it should not be surprising that (Salmon (1986), p. 79) argues that "[b]ecause the rules fell under the supervision of courts of equity and were never defined by statute ... they remained inaccessible to the majority of women. This explains why most historians have depicted the nineteenth-century married women's property acts as more important to women than the equitable developments of the early modern period ...."[28]

2.4 Sample Period

Returning to the dates used in this paper, Figure 1 shows the date when each state granted women "both" rights. Massachusetts was the first state to grant these rights, in 1846. Ideally, we would start our analysis in 1840. However, Ruggles et al. (2010) provide U.S. census data beginning only in 1850 that are comparable over time. Accordingly, our analysis begins in 1850. We follow Geddes and Lueck (2002) in stopping our analysis in 1920. This is for two reasons. First, by 1920 rights were granted in all states except Florida (1943), Arizona (1973), New Mexico (1973), and Louisiana (1980). Second, as noted in Geddes and Lueck (2000), it may not be fair to call the post-1920 era true coverture, as the 19th Amendment (passed in 1920) granted women the right to vote, which may well have affected *de facto* implementation of laws.

2.5 Considerations of Giving Women Rights

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

We begin by noting that the economics and history literatures are united in making explicit that men viewed a loss of bargaining power at home as the main downside of granting women rights. Indeed, Griffin (2003), in reviewing the debate over women's property rights in England, makes

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

clear that men were hesitant to give up their own rights at home. The reasoning, from the historical archive, for granting women property rights seems to be to protect women from abusive husbands who might leave their families impoverished.[29] Holcombe (1983) similarly discusses the history of women's property rights in England in the context of defending families against male-inflicted poverty. Stanley (1988) discusses similar motives in state legislatures in the U.S.

Similarly, while the feminist movements of the time clearly supported women's property rights, our reading of the history and economics literatures does not support the notion that feminism was a driving cause of married women's economic rights, though it seems to have been a major driver behind women's suffrage. Indeed, the first law passed in New York State to grant married women property rights, discussed below, was passed three months *before* the Seneca Falls convention, widely considered to be the beginning of the feminist movement in the U.S. One simple way to empirically test whether feminism is the driving force is embedded in Figure 2, which plots the year that each U.S. state granted women economic rights on the Y axis against the date of women's suffrage on the X-axis. There is no correlation between the timing of these rights. Case in point is Massachusetts, the first state to grant women economic rights, and among the last states to grant women political rights in 1920.[30] Indeed, Stanley (1988, pg. 484) in 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. Doepke and Tertilt (2009) argue that men wanted to grant rights to give *other* men's wives power, which would increase investment in the human capital of other children. They argue that increased returns to human capital led men to give women rights in order to further all children's human capital investment. However, their paper is largely theoretical, and does not include an empirical section. As such, our results on the increase in education (of children of both genders) can be viewed as supporting the mechanism in Doepke and Tertilt (2009).[31] Fernández (2014) argues that fertility rates determine women's rights. The author posits that if fertility is low, then the size of the inheritance that daughters *do not* receive under coverture is large, representing a loss to fathers. Fathers may have wanted to ensure that their daughters could actually receive their inheritance, and thus granted women rights. Our results, discussed below, reject this hypothesis by showing that fertility rates declined after rights were granted, not before rights were granted.[32]

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

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

[31]Doepke and Tertilt (2009) are silent on the implications of women's rights on labor supply and fertility.

[32]To be more specific, in our event-study comparisons of people on either side of county-border pairs, 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. Thus, we do not find evidence that supports the idea that declines in fertility led to women's economic rights, but we find substantial evidence suggesting the opposite is true.

Geddes and Lueck (2002) argue that coverture decreased women's incentive to work, as their earnings went to their husbands. While we do not find support of this mechanism in the U.S., as we find no evidence that women's labor force participation rates increased when economic rights were granted, this may have been a significant mechanism in England, where married women's labor force participation was much higher than in the U.S. at this time. Finally, Hazan et al. (2019) argue that coverture led to a distortion in portfolios by incentivizing single women, and parents of all women, to invest in real assets, rather than personal assets such as bank accounts. They find that granting women rights led to households to reallocate their portfolios towards personal property, which in turn led to an aggregate increase in bank deposits, a reduction in interest rates, and an increase in bank loans. This financial market deepening in turn led to a reallocation of workers from agriculture towards non agriculture (manufacturing), with this reallocation biased in favor of capital-intensive industries. While they do not explicitly evaluate the hypothesis that this financial market deepening and economic growth may have been the reason to give women economic rights, it is a potential hypothesis nonetheless.[33]

3 Data and Empirical Strategy

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

3.1 Data

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

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

Our sample consists of households in which a 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, or investment in human capital of single parent households. We document in Appendix B that granting women property rights had only a negligible impact on marriage markets, as measured by the propensity to get married, the age of married, and age gap between husband and wife.[34] This reduces concerns that our sample selection of married

[33] Hazan et al. (2019) discuss and empirically evaluate potential hypotheses for women's rights in Section 2 of their Online Appendix. However, they are unable to draw any robust empirical conclusions.

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

households could bias our results. We restrict attention to whites in order to abstract from issues related to race in this time period.[35] We restrict attention to women who live in the same state in which they were born in order to avoid issues related to misunderstanding marriage property laws that may arise from migration. We discuss below our age restrictions on our sample for each of our exercises.

We begin by discussing our outcome variables for event-study analyses measuring fertility. Our first outcome variable of interest is "birth," which we define as whether a wife gave birth in the previous calendar year, which we infer by whether there was a child in the household whose birth year was in the previous calendar year.[36] Notice that this variable is binary, and does not taken different values in case there was more than one child born in the previous year. Our second measure of fertility is the number of children under age five. This is the sum of the members of a household, who are children of the head of the household, and are aged five or less. It might seem natural to look at other outcome variables, such as the total number of children that a couple has, however we note that our method of calculating the number of children requires them to be at home when the census was taken. Considering that older children may have left home, we limit our analysis to the number of children under five. We study these variables both in our event-studies, as well as when comparing households married before and after rights were granted. When analyzing these variables, we restrict attention to households where the wife is age 20-39, and the husband is age 20-59, in order to focus attention on households that are likely to have newborns and young children.

In the 1900 and 1910 censuses, women were also asked about the number of children they ever gave birth to ("children ever born"), as well as the number of surviving children they birthed ("surviving children"). These variables measure completed fertility. As such, we analyze these variables in households where the wife is age 40-60 in order to capture women who have finished giving birth. Since we have completed fertility data from two years, and no states gave women rights between 1900 and 1910, an event-study design is not appropriate to analyze these variables. However, these two censuses include information on the duration of marriage, and thus we can do our analysis comparing households married before and after rights were granted.

Our measure of female labor force participation is to use IPUMS' occupation (1950 basis) code.[37] This variable is available for women of our sample in all of the full-count US censuses from 1860-1920. In contrast, the census variable "labforce," which directly measures whether a person is in the labor force, is available only in the 5% sample for the 1900 census.[38] However, these two variables agree on labor force participation rates, with a correlation of 0.93, when both variables are

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

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

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

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

available. We examine this variable both in our event study design as well as in our analysis comparing households married before and after rights were granted. We do so both for households with women age 20-39, in order to be consistent with the sample of women in our event studies on fertility, as well as 40-60, in order to cover the remaining set of married women who potentially work.

Our measure of schooling for children is the IPUMS' variable "school," which measures where a child is currently in school. Prior to 1900, the question was asked of persons "of school age." In 1900 and 1910, the question was applied to all persons age 5-21, however implicit in the question was whether the student was in college. We restrict attention to children ages 6-18, as they are under their parent's legal control. We examine households in which the wife is 20-60 and the husband 20-70 years old, in order to capture older children born to older parents.

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

3.2 Summary Statistics

Table 1 shows summary statistics of variables of interest and main control variables for the analysis of fertility and labor force participation rates in our event-study analyses described below. Panel A reports summary statistics for our fertility analyses, where the sample is restricted to the wife being age 20-40 between 1850 and 1920. It reports the mean, standard deviation, and number of observations, separately for observations in which women do and do not have economic rights, for the probability of giving birth, the number of children in the household under age 5, age of the wife, age of the husband, and year the observation is made. The probability of a birth last year and the number of children under age 5 are substantially lower when women have rights. Both husband and wife are slightly older in the sample where women have rights. Consistent with the notion, described above, that women's rights were never revoked once granted, the sample where women have rights is from a later period, on average, than when women do not have rights. This motivates our heavy use of interactions between control variables and year fixed effects, described below.

Panel B of Table 1 repeats Panel A for exercises examining women's labor force participation rates when women are 20-39. Panel C does so for women age 40-69. As can be seen, the average labor force participation rates are very similar between the two samples, being different by less that 0.4 percentage points in both samples. As in Panel A, people are generally slightly older (except women in Panel C), and the time period slightly later, in the sample with economic rights. Recall that these exercises use data from 1860-1920, and as such the average observation is slightly later than that of Panel A.

Table 2 shows summary statistics of variables of interest and main control variables for the analysis of education of children in our event-study analyses described below. Panel A reports summary statistics when the sample is all children age 6-18. Panel B does so for the sample of children age 6-10, while Panel C does so for the sample of children age 11-18. We report the average propensity to be in school for all children, sons, and daughters. We additionally report the average age of their mother and father, as well as the average sample year.

In Panel A, about 60% (61%) [59%] of children (boys) [girls] age 6-18 are in school prior to rights being granted, while this number rises to 77% (77%)[78%] after rights are granted. The age of mothers (fathers) is about 38 (43), and unchanged between the sample with and without women's rights. The sample with women's rights is, on average, from after 1900 while the sample without rights is from before 1870. Again, this motivates our interaction of control variables with year fixed effects. Panels B and C report surprisingly similar numbers to Panel A, with the main exception being that parents of older children are somewhat older than parents of younger children.

Table 3 shows summary statistics of variables of interest and main control variables for the analysis of fertility and labor force participation rates in our analyses comparing couples married before and after economic rights were granted. Panel A shows the probability of giving birth, the number of children under age 5, and labor force participation rates of women, on the sample where the wife is age 20-39. As can be seen, the probability of giving birth is higher (0.2) when women do not have rights than when they do have rights (0.18). Similarly, there are fewer children under age 5 at home with rights (1.08) than without rights (1.21). Women's labor force participation rates are slightly lower with rights (3.3 percentage points) than without rights. In this sample, the average age of the wife is 29-30, while the husband is about 34, with no difference between these numbers comparing couples married before and after rights were granted. The data are from the full US census in 1900 and 1910, and thus the average year of observation is in between.

Panel B of Table 3 shows the number of children ever born to the wife of the household, number of surviving children, and labor force participation rates. For couples married prior to rights being granted, the number of children ever born (surviving children) is 5.8 (4.6), while for those married after rights it is 4.2 (3.3) children. Women's labor force participation rates are a half a percentage point higher for those married after rights are granted. Those married after rights are about 3 years younger than those married before rights. As before, the data are from the full US census in 1900 and 1910, and thus the average year of observation is in between.

We next turn to some basic figures to get a general sense of the relationship between our variables of interest and women's economic rights.

The top left panel of Figure 10 shows the probability of giving birth, by age, for women in our event-study sample, depending on whether or not they have economic rights. This variable is net of year fixed effects, in order to capture general demographic trends in the US between 1850 and 1920. The probability of giving birth rises after age 20, peaks at about age 23, and then declines. Clearly visible is the difference in the probability of giving birth between women with and without rights. Women who have economic rights are less likely to give birth, especially after age 25. Similarly, the top right panel of this figure does the same exercise examining the number of kids under age 5. The number of kids under age 5 rises until the mother is approximately 26-28, and then falls. Consistent with birth probabilities, after age 25 there are fewer kids under age 5 in households where the wife has economic rights. The bottom panel of this figure does the same exercise on the labor force participation rates of these women. While there are differences in the labor force participation rates of women younger than 32, they are quantitatively quite small, with the maximum difference between these curves being less than a half a percentage point. If anything, the labor force participation rates of women with economic rights are lower than those of women without rights.[39] Similarly, the left panel of Figure 11 shows the density of the number of children ever born, for women age 40-60 in 1900-1910, as a function of whether or not they were married with economic rights. The right panel repeats this exercise for surviving children. The distribution of completed fertility is skewed to the left for those married with economic rights, suggesting that these women indeed had fewer kids. Taken together, these exercises suggest that fertility is lower for women with economic rights, as will be analyzed in detail in Section 4.1, while labor force participation rates are virtually the same, as will be shown in detail in Section 4.3.

Turning towards education, the top panel of Figure 12 shows school attendance rates, over time, by gender. In 1850, about two-thirds of children were in school. This rate declines after the Civil War to slightly less than 60%, and then begin to rise again, sharply so after 1900, and reaches over 80% by the end of the sample. We note a cross-over in the data: prior to 1900, male children were more likely to be in school than female children. After 1900, this difference switches. The combination of trends in education over time and a cross-over between sons and daughters being more likely to be in school motivates the usage of gender-year fixed effects when studying schooling rates. The bottom left panel of Figure 12 shows the probability of a child being in school, by age, when women do or do not have rights. The bottom right panel repeats the bottom left panel, net of gender-year fixed effects. We note that economic rights are associated with more children in school, but that the effect is larger among younger children. Indeed, these summary statistics cannot reject the hypothesis that older children are unaffected by women's economic rights. These results motivate the analysis in Section 4.2.

We next turn to the details of our empirical approaches.

3.3 Empirical Approach 1: Event-Study

In this subsection, we first describe the style of regressions we estimate in our event studies. We then discuss the data constructed on county-border pairs that we exploit in these event studies. Finally, we discuss the conditions under which our results can be interpreted as causal, as well as an empirical analysis of these conditions.

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

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

[39] This is the opposite of the point estimates suggested by the analyses in Section 4.3, however those analyses find no statistically significant difference in the labor force participation rates between women with and without rights.

where Y_{hsct} is our outcome variable of interest listed above, such as whether or not a woman in household h gave birth in the previous year or a child was in school, in state s, county c, and year t, $t \in \{1850, 1860, \ldots, 1920\}$, $rights_{st}^{k}$ 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, λ_s are state fixed effects, and X'_{hsct} contain controls variables, such as age, that depend on the specific exercise being performed.[40] 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. All of our data for these exercises comes from the U.S. census, which is conducted once per decade. 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 now turn to the construction of county-border pairs, which is detailed more fully in Appendix A. We compare households in two adjacent counties on either side of a state border. The construction of these border-pairs raises some issues along the way.

The first issue is that county borders were themselves ever changing. Imagine a county A in state 1 bordering another county B in state 2. If the county A splits into two counties, then in order for our exercise to remain consistent, we must treat the two new counties formed from county A as being one county, and keep track of such changes over time. This is a painstaking process that allows for a consistent dataset, as described in Appendix A. Similarly, as the U.S. spread westward over the 19th century, more states (and thus, state borders) developed. Maps showing our data on borders over time can be seen in Figures 3-9.

The second issue is, what if county A has more than one bordering county, potentially even in more than one bordering state? To address this issue, we replicate each observation in county A according to the number of bordering counties it faces. Each observation is set to a different pairing with a neighboring county.[41] Econometrically, this approach raises two issues. The first is that duplicated observations could bias estimates.[42] Accordingly, we when duplicating an obser-

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

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

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

vation n times, we reweight each observation to have a weight of $1/n$.[43] 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.[44]

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

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

We next turn to the question of whether a state granting women rights is plausibly exogenous. This question actually contains two questions, with the first being did states grant women economic rights because of changing fertility rates or education rates, rather than vice versa? The historical record, discussed above, seems to suggest not.[45] However, we also note that, if states granted women rights in order to drive the results we find, then our exercises could be interpreted as measuring their success. Furthermore, the historical record, discussed above, shows clearly that states granted rights, which were then overturned by the courts, sometimes due to technicalities that the legislature did not foresee, until they were passed again. It is highly implausible to believe that the final timing of women's rights in a state was endogenous. Even if one believes that granting women rights was endogenous, as long as the change in the law was plausibly exogenous to a county on that state's border, our analysis still captures the causal effects of women's rights. To see this point, consider a county on the border between Ohio and Pennsylvania. This county does not contain Columbus, the capital of Ohio, or Cleveland, Akron, Toledo, or Cincinnati, though it may contain Youngstown. It is plausible to believe that the Ohio state legislature passes laws without taking this county into account, making state law changes plausibly exogenous to counties on the

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

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

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

periphery of a state. Indeed, this logic prevails in minimum wage literature: while a change in the federal minimum wage is presumably endogenous to national economic conditions, it is plausibly exogenous to any particular state (Baskaya and Rubinstein, 2012).[46]

The third question is omitted variable bias, in particular in the form of other legal changes happening contemporaneously. We are unaware of any legal changes occurring contemporaneously that might have affected fertility and education decisions, with the exception of child labor laws and mandatory schooling laws. We control for these laws, as described below. WE DONT YET DO THIS YET However, in Appendix **??**, we also perform randomization exercises that show the dates women were granted rights were unlikely to randomly produce the results we document here. As such, we conclude that it is highly likely that the years in which women granted rights contain actual information. The only negation of this exercise would be that if all states changed another law, hitherto unknown and unnoticed by the history profession, exactly when they granted women's economic rights, in which case we could not separately identify the effects of these laws.

The final question is whether these legal changes affected the marriage market. In Appendix B, we use the event-study design described here, as well as the comparison of couples married before and after economic rights were granted described below, to show that these rights did not affect the marriage market. In particular, they did not affect the propensity of people to marry, the age of marriage, or the age gap between husband and wife. Below, in Section 3.4, we show evidence that people did not change the timing of their marriage in order to marry before or after rights were granted.

When performing our event study exercises, we always include two robustness tests. The first is to drop any county that is on the border between a southern state and a non-southern state.[47] We do so to allow for the fact that the south of the US may have evolved differently over time, especially in light of the destruction faced in the civil war. The second robustness test is to drop counties on the border between community property states and neighboring states. This is to allow for the fact that community property states may have had a different response to women's rights.

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

[46]This argument is potentially invalid if there is little heterogeneity within states. That is, if all the counties of a state are very similar to one another, then state policy is not exogenous to individual counties, as there is no disagreement between counties within the state. In Appendix C we show that this is not the case. Specifically, in every year, we compute the average fertility, education, and labor force participation rates for each county in the US. We then regress these averages on state fixed effects, and report the R^2 and adjusted R^2. In all exercises, these numbers turn out to be low, suggesting that the heterogeneity between counties is not explained by state.

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

3.4 Empirical Approach 2: Couples Married Before vs After Rights

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

$$Y_{hsct} = \alpha \cdot MarriedRights_{hsct} + \beta_{c,t} + X'_{hsct}\delta + \epsilon_{hsct}, \tag{2}$$

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

The assumption necessary for a causal interpretation of the results documented with this approach is that selection into marriage did not change due to economic rights, and that people did not strategically time their decision to get married around the date that women's rights were granted. In Appendix B we argue that it is indeed the case that selection into marriage was not affected by women's rights. In Figure 13, we provide evidence that couples did not time their marriage around the granting of women's rights. In particular, the top panel left panel shows the fraction of people getting married, relative to the year their state gave rights, in the 1900 US census, when limiting to white non-Hispanic couples, where the wife is 20-39 years old. The top right panel does the same for couples where the wife is 40-60. The bottom left and bottom right panels repeat this pattern using the 1910 census. In all cases, except for couples where the wife is 20-39 years old in 1910, there is clearly no break in the data around the year a state gave rights, nor is there any bunching behavior. The 1910 data for couples where the wife is 20-39 in 1910 is noisy, and thus harder to interpret. This is due to the small sample of states that gave rights in the relevant time frame.[48]

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

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

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

4 Results

In this section we describe the results of our empirical exercises. We first examine the impact of women's rights on fertility, using both the event-study approach and comparing couples married before and after rights. We find that fertility declines dynamically after women are granted economic rights, and that this decline can be accounted for by couples who get married after rights are granted. We use our event-study methodology to show that more children, of both genders, started going to school once rights were granted. Finally, we study the impact of women's property rights on women's labor supply using both the event-study approach and comparing couples married before and after rights, and find that women's rights has no measurable impact on labor force participation rates.

4.1 Fertility

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

4.1.1 Fertility: Event Study Approach

We estimate regressions of the form described in Equation 1, where the dependent variable is either whether the wife of the household gave birth in the previous year or the number of kids under age five in the household. The controls in variable X_{hsct} include fixed effects for the wife's age and the husband's age, both interacted with year fixed effects. Some specifications include "extra controls," which include fixed effects for the husband's industry and husband's occupation, both interacted with the year fixed effect.[49] These extra controls allow us to control for how a husband's career might affect family size, and how the relationship may change over time. In some specifications, we also include fixed effects for the number of kids under age 5 in the previous year to control for how the stock of young children may affect the probability of having an extra child.

Table 4 shows the results when the dependent variable is whether the wife of a household gave birth in the previous year. Column 1 does not include our extra controls or controls for the number of kids under 5 in the previous year. Column 2 repeats Column 1, but adds the extra controls. Column 3 repeats Column 2 but adds the controls for the number of kids under 5 in the previous year. Column 4 repeats Column 3, but drops counties on the border between Southern and non-Southern states. Column 5 also drops counties in community property states, as well as their border-pairs.

In all specifications, the estimates prior to granting rights are quantitatively virtually zero, and statistically insignificant, supporting the idea that there were no differences in trends in fertility between counties on either side of the state border prior to rights being granted. In all specifications, the impact of rights on the probability of giving birth is between -0.006 and -0.007 when

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

rights are given, with the effect statistically significant at the 1% level in Columns 1-3, and the 5% level in Columns 4 and 5. One decade after rights are granted, the magnitude of the effect grows in all specifications, with the range of estimates being between -0.009 and -0.011, with all estimates statistically significant at the 1% level, except in Column 5 where the estimate is significant at the 5% level. Two decades after rights are granted, the magnitude of the effect again grows in all specifications, with the range of estimates being between -0.010 and -0.012, with all estimates statistically significant at the 1% level. Three decades and more after rights are granted, the magnitude of the effect is again larger, with the range of estimates being between -0.012 and -0.015, with all estimates statistically significant at the 1% level. Considering that the average probability of giving birth was about 0.19, corresponding to roughly 4 births over a twenty-year horizon, the magnitude of the estimates range from a decline of about 3-3.5% when rights are granted to a decline of 6-8% three decades after rights are granted.

Table 5 shows the results when the dependent variable is the number of kids under age 5. Table 5 follows the same pattern as Table 4, except does not include the number of kids under 5 in the previous year as a control starting in Column 3. That is, Columns 1 and 2 exactly replicate the pattern of Columns 1 and 2 in Table 4, and Columns 3 and 4 replicate Columns 5 and 6 of Table 4, respectively, but do no include controls number of kids under 5 in the previous year.

In all specifications, the estimates prior to granting rights are statistically insignificant, supporting the idea that there were no differences in trends in fertility between counties on either side of the state border prior to rights being granted. In all specifications, the impact of rights on the number of kids under 5 is between -0.025 and -0.028 when rights are given, with the effect statistically significant at the 10% level in Columns 1 and 2, and the 15% level in Columns 3 and 4.[50] One decade after rights are granted, the magnitude of the effect grows in all specifications, with the range of estimates being between -0.036 and -0.042, with all estimates statistically significant at the 5% level. Two decades after rights are granted, the magnitude of the effect again grows in all specifications, with the range of estimates being between -0.053 and -0.059, with all estimates statistically significant at the 5% level in Columns 1 and 4, and the 1% level in Columns 2 and 3. Three decades and more after rights are granted, the magnitude of the effect is again larger, with the range of estimates being between -0.074 and -0.084, with all estimates statistically significant at the 1% level. Considering that the average number of kids under five was about 1.15, the magnitude of the estimates range from a decline of about 3-3.5% when rights are granted to a decline of about 6.5-7.3% three decades after rights are granted. Notice that, in percentage terms, these fertility declines are remarkably consistent with those described above. Additionally, we note that the estimates on the impact of rights on the number of kids under 5 is roughly five times that of the impact on the probability of giving birth, which makes these estimates consistent in magnitudes.

We conclude that granting women economic rights led to a dynamic decrease of fertility of about

[50] The reduced significance in Columns 3 and 4 is presumably due to smaller sample sizes in the robustness exercises.

3-8% over the subsequent decades.

4.1.2 Fertility: Couples Married Before/After Rights

We next turn to the impact of women's economic rights on fertility, as measured by comparing couples married before and after rights were granted. As such, we estimate equations along the lines of those described in Equation 2.

Table 6 shows the results when the dependent variable is whether the wife of the household gave birth last year (Panel A), or the number of children under 5 (Panel B). As in Section 4.1.1, we restrict attention to households where the wife is age 20-39, and her husband 20-60, in order to maintain comparability between the sets of results. Column 1 includes as controls fixed effects for the wife's age, the husband's age, and how long the couple has been married, all interacted with year fixed effects. Column 2 adds the "extra controls," described above, which are fixed effects for the husband's occupation and industry, interacted with year fixed effects. Column 3 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 4 also repeats Column 2, but drops counties in community property states as well as their bordering counties. Columns 5 and 6 also repeat Column 2, but do so only on the sample from 1900 or 1910, respectively.

Panel A shows that couples married after rights were granted, as compared to those married before rights, had a lower probability of giving birth in a given year of 0.009-0.010 in Columns 1-5. These estimates are statistically significant at the 1% level in Columns 1, 2, 4 and 5, and not statistically significant in Column 3. The reduced significance in Columns 3 is presumably due to smaller sample sizes. We note that the point estimates are virtually identical in these specifications. In Column 6, using only the 1910 sample, the point estimate is only -0.004 and it is not statistically significant. This is presumably due to the small number of states that gave rights in the 20 years prior to the 1910 sample, which could be used to identify the effect of being married with rights.[51]

We note that the results so far are consistent with those found using the event study approach above. The probability of giving birth is estimated to decline by 0.009-0.010 using this approach, which is basically the same as the impact of women's rights on the probability of giving birth in the event study a decade after rights were granted. However, we also note that this estimate is within a standard deviation of both the immediate impact of rights on the probability of giving birth, and the impact on the probability of giving birth two decades after rights were given. Thus, it is possible that these two exercises are picking up the same effect, and that the entire reason that fertility rates decline after women's rights comes from those couples who got married after rights were granted. Under this view, the reason that the event-study approach has an increasing

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

dynamic effect of women's rights on the probability of giving birth is that the stock of married couples is dynamically changing to include more people married after rights were granted over time.

Panel B shows that couples married after rights were granted, as compared to those married before rights, had 0.135-0.165 fewer kids at home under age 5 in a given year in Columns 1-5. These results are statistically significant in Columns 1,2, 4, and 5 at the 1% level. While they are not statistically significant in Column 3, the point estimate is very similar to the other specifications. As before, the effect is a bit weaker in Column 6, with the point estimate -0.117, and this estimate is also statistically significant at the 1% level.

We note that the results in Panel B are quantitatively larger than those implied by Panel A. That is, if the probability of giving birth declines by 0.9 percentage points, then we'd expect the number of kids under age 5 to decline by about 0.05, whereas the estimates here are 2-3x larger. Similarly, we also note that the results here are larger quantitatively than those documented in the event-study approach. This fact further reinforces the idea that declines in fertility are being driven by couples married after rights were granted.

Table 7 shows the results when the dependent variable is completed fertility, which thus necessitates changing the sample to households where the wife is age 40-60.[52] Panel A of Table 7 has the dependent variable be the number of children ever born (CEB) to the wife of the household, while Panel B has the dependent variable be the number of surviving children the wife has. As before, Column 1 includes as controls fixed effects for the wife's age, the husbands age, and how long the couple has been married, all interacted with year fixed effects. Column 2 adds the "extra controls," described above, which are fixed effects for the husband's occupation and industry, interacted with year fixed effects. Here, Column 3 repeats Column 2, but on a sample of women who have ever had a child ($CEB > 0$). Column 4 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 5 also repeats Column 2, but drops counties in community property states as well as their bordering counties. Columns 6 and 7 also repeat Column 2, but do so only on the sample from 1900 or 1910, respectively.

Beginning with Panel A, in all specifications, the number of children ever born decreases by 0.210-0.238 children, and is statistically significant at the 5% level in all specifications, except for Column 4 where it is significant at the 10% level. This is roughly twenty times the estimate of the impact of being twenty years after rights on the probability of giving birth, suggesting that these estimates are compatible (a reduction in the probability of giving birth by 0.010 for 20 years reduces fertility by 0.20 children).

Infant and child mortality were relatively high, and only start to decline in the late 19th century or early 20th century (Haines, 1998)). As such CEB doesn't necessarily reflects the demand for children (Doepke, 2005). Parents could replace an infant or a child who passed away with another

[52] As noted in Section 3.1, data on completed fertility are only available in 1900 and 1910.

births. Thus, in Panel B we replace CEB with the number of surviving children as a better measure of the demand for children. In all specifications in Panel B of Table 7, the number of surviving children decreases by 0.134-0.176 children, and is statistically significant at the 5% level in Columns 1, 2, 3, 5, and 7, the 10% level in Column 6, and not statistically significant in Column 4. This furthers the notion that demand for children decreased following women's economic rights.

We conclude that couples married after rights were granted, as compared to those married before rights were granted, had lower fertility rates. Additionally, this decline in fertility rates can account for the decline in fertility rates documented in the event-study approach, suggesting that couples married after rights were granted drove the results documented in Section 4.1.1.

4.2 Education

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

4.2.1 Education: Event Study Approach

We estimate regressions of the form described in Equation 1, where the dependent variable is whether a child is currently in school. As discussed above, in some specifications, we add interactions of $rights_{st}^k$ with dummy variables indicating whether the child is female. The controls in variable X_{hsct} include fixed effects for the child's age, whether the child is female, the mother's age and the fathers's age, all interacted with year fixed effects. Some specifications include "extra controls," which include fixed effects for the father's industry and occupation, the number of children in the house, whether a this child was allowed to not work, whether the child was allowed to not be in school, all interacted with the year fixed effect.[53]

Table 8 reports the results of our analyses. Column 1 sets the sample to be all children age 6-18, and does not include the extra controls. Column 2 repeats Column 1, but includes the extra controls. Column 3 repeats Column 2, but includes the interactions of $rights_{st}^k$ with dummy variables indicating whether the child is female.

In all three specifications, there is no trend in the probability of a child being in school prior to rights being granted.[54] The point estimate on the effect of rights on education is between 3.9-4.0 percentage points (p.p.), and statistically significant at the 5% level in all three specifications. A decade after rights, the estimates rise to 4.4-4.6 p.p., and are statistically significant at the 5% level in Column 1 and 1% level in Columns 2 and 3. The estimates rise further to 5.1-5.2 p.p. two decades after rights, and are statistically significant at the 5% level in all three specifications.

[53]The data on whether a child of a given age was allowed to work or allowed to not be in school comes from Clay et al. (2016). These variables are only available starting from 1880. Before that, we assume no law was in effect restricting children.

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

The point estimates drop somewhat to 3.4-3.7 p.p., and lose their statistical significance, three decades after rights are granted. Column 3 shows no meaningful economic or statistical difference in the educational attainment of daughters following women's rights, as compared to sons. We conclude that the effect of women's rights was the same between daughters and sons. Relative to an average propensity to be in school of about 70%, these estimates reflect an increase of about 6-7% in schooling following women's economic rights.

Columns 4-6 of Table 8 repeat Columns 1-3, but restrict the sample to be children ages 6-10. As before, there is no trend in the probability of a child being in school prior to rights being granted. After rights are granted, the estimates are universally larger than their counterparts in Columns 1-3. The increase in the effect is by about 0.5-0.7 p.p. when rights are granted, to as much as 3.2 p.p. three decades after rights are granted. Indeed, as opposed to in Columns 1-3, in Columns 3-4 the estimates are all statistically significant at the 5% level (Column 4) or 1% level (Columns 5 and 6).[55] As before, there is no differential impact of women's rights on the education of daughters as opposed to sons (Column 6).

Columns 7-9 of Table 8 again repeat Columns 1-3, but restrict the sample to be children ages 11-18. As before, there is no trend in the probability of a child being in school prior to rights being granted. Here, the estimates are universally smaller and less statistically significant than their counterparts in Columns 1-3. The immediate impact of women's rights on the education of older children is 3.3-3.4 p.p. when rights are granted, but this estimate is only statistically significant at the 10% level. Ten years after rights are granted, the point estimates range from 3.3-3.6 p.p., with the estimates statistically significant at the 10% level in Columns 7 and 8, but not in Column 9. The remaining estimates are not statistically significant. As before, we find no differential impact of women's rights on the education of daughters as opposed to sons (Column 9).

Table 9 performs our robustness analysis for this exercise. Column 1 of Table 9 repeats Column 3 of Table 8, but removes from the sample observations in counties on the border between Southern and non-Southern states. Column 2 repeats this, but instead drops observations on the border between community property states and other states. Columns 3 and 4 repeat Columns 1 and 2 on the sample of children age 6-10, while Columns 5 and 6 do so on the sample of children age 11-18. Quantitatively and qualitatively, the results are very similar to those reported in Table 8, with a large, dynamic, and statistically significant increase in education for all children after rights are granted. This increase is stronger for children 6-10 than for those 11-18, and there is no differential impact of rights on girls rather than boys.

We conclude that women's rights led to a dynamic increase in the educational attainment of children, with the effect concentrated on younger (6-10 year old) rather than older (11-18 year old) children. Importantly, we find no evidence that women's economic rights led to any differential impact on the education of daughters rather than sons.

[55] We also note that the statistical significance of all other estimates on the impact of women's rights on education is at the 1% level, with the exception of the immediate effect of women's rights on education in Column 4, which is significant at the 5% level.

4.2.2 Education: Parents Married Before/After Rights

We next turn to the impact of women's economic rights on education, as measured by comparing children of couples married before and after rights were granted. As such, we estimate equations along the lines of those described in Equation 2, using the controls described above in Section 4.2.1.

Table 10 presents the results of these estimations. We begin with Panel A, which tabulates the regression estimates when using data from both 1900 and 1910. Column 1 includes all of the control variables, interacted with year, on the sample of children ages 8-17. Column 2 repeats Column 1, but includes an interaction between the parents of the child being married after rights were granted and the child being female. This allows us to capture potentially differential effects on sons versus daughters. We find that children married to parents who married after rights were granted were 0.9-1.0 percentage points more likely to be in school than those whose parents married before rights, with the estimates significant at the 10% level in both specifications. Columns 3 and 4 repeat this pattern for children ages 8-13. The point estimates suggests that these children were 0.3-0.5 percentage points more likely to be in school if their parents were married after rights were granted, however the estimates are not statistically significant. Columns 5 and 6 again repeat this pattern, but for children 14-17. We find that children of parents married after rights were granted were 1.9-2.4 percentage points more likely to be in school, with the estimates significant at the 5% level in Column 6 and 1% level in Column 5. As in Section 4.2.1, we find no evidence that the impact of women's rights on daughters was different that that on sons.

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

Panel C repeats Panel A, but only uses data from 1900. The point estimates are quantitatively very similar to those in Panel A, but the statistical significance greatly reduced. Indeed, of Columns 1-4, only the estimate in Column 2 is significant, and even this estimate is only significant at the 15% level. The estimate is significant at the 5% level in Column 5 and 15% level in Column 6. It is no surprise that the estimates from 1910 are more statistically significant than those from 1900, as the 1900 data is from the 5% sample while the 1910 data is from the full sample.

DO WE LIKE THIS PARAGRAPHS? At first glance, these results are not entirely consistent with those found in the event study, described above in Section 4.2.1, as the larger effect is on older children, rather than younger children. However, we note that the event study was over the time period 1850-1920, while this analysis is mostly focused on 1910, by which time most younger children were already in school. As such, it is not surprising that the main effect would be on older children. Additionally, we note that the magnitude of the effect documented here on older children is similar to that in Section 4.2.1. That is, the estimate of the effect of parents being married after women's rights on the probability of a child age 14-17 being in school is within a standard deviation of the estimated impact of women's rights on older children being in school within 2 decades. WHAT TO DO ABOUT THE FACT THAT THE EVENT STUDY GIVES IMMEDIATE IMPACT ON OLDER KIDS, WHO'S PARENTS CANNOT HAVE BEEN MARRIED BEFORE RIGHTS?

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

4.3 Labor Force Participation

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

4.3.1 LFP: Event Study Approach

We estimate regressions of the form described in Equation 1, where the dependent variable is either whether the wife of the household is in the labor force, using the same controls as in Section 4.1.1. That is, the controls in variable X_{hsct} include fixed effects for the wife's age and the husband's age, both interacted with year fixed effects. Some specifications include "extra controls," which include fixed effects for the husband's industry and husband's occupation, both interacted with the year fixed effect. These extra controls allow us to control for how a husband's career might affect the labor force participation of his wife, and how the relationship may change over time. In some specifications, we also include fixed effects for the number of kids under age 5 in the previous year to control for how the stock of young children may affect the probability of a wife working.

Table 11 repeats Table 4 when the dependent variable takes a value of 0 when the wife is not in the labor force and 1 when she is, on the sample of married women age 20-39. Column 1 does not include our extra controls or controls for the number of kids under 5 in the previous year. Column 2 repeats Column 1, but adds the extra controls. Column 3 repeats Column 2 but adds the controls for the number of kids under 5 in the previous year. Column 4 repeats Column 3, but drops counties on the border between Southern and non-Southern states. Column 5 also drops counties in community property states, as well as their border-pairs. Table 12 repeats Table 11 on the sample of married women age 40-60. All specifications in both tables show no relationship between married women's property rights and women's labor supply.

In all specifications for both tables, the point estimates for two decades before rights, when rights are given, one decade after rights are given, and three or more decades after rights are given are quantitatively small and statistically insignificant. There is some evidence that labor supply was low three or more decades prior to rights being granted, or high exactly two decades after rights were granted.[56] However, there does not seem to be a clear pattern of changes in labor supply around the time women are granted rights that is discernable in this data.

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

4.3.2 LFP: Couples Married Before/After Rights

We next turn to the impact of women's economic rights on labor force participation, as measured by comparing couples married before and after rights were granted. As such, we estimate equations along the lines of those described in Equation 2, using the controls described in Section 4.1.2.

Table 13 shows the results when the sample is women age 40-60 (Panel A), or 20-39 (Panel B), and follows the same pattern as in Table 6. Column 1 includes as controls fixed effects for the wife's age, the husbands age, and how long the couple has been married, all interacted with year fixed effects. Column 2 adds the "extra controls," which are fixed effects for the husband's occupation and industry, interacted with year fixed effects. Column 3 repeats Column 2, but drops counties on the border between Southern and non-Southern states. Column 4 also repeats Column 2, but drops counties in community property states as well as their bordering counties. Columns 5 and 6 also repeat Column 2, but do so only on the sample from 1900 or 1910, respectively.

All specifications in both panels show no quantitative or statistical difference in labor force participation between couples married before and after women's rights.[57] These results reinforce the findings above that women's labor force participation rates did not change when rights were granted.

5 Discussion: Bargaining Power Can Account for the Results

In this section, we discuss several potential mechanisms that can relate between granting married women property rights and the changes in households documented above. We begin by discussing how shifting bargaining power from husband to wife is consistent with all of the empirical results documented above, as well as the historical evidence discussed in Section 2.5. We then discuss issues with other proposed mechanisms for explaining our findings.

It has been widely documented in the literature that women prefer smaller families with greater investment in the education of children (Thomas, 1993; Lundberg et al., 1997; Attanasio and Lechene, 2002; Qian, 2008; Rasul, 2008; Bobonis, 2009; Doepke and Tertilt, 2019, 2018; Doepke and Kindermann, 2019). This could be due to either an evolutionary rationale, or

[56]There is also slight evidence that women age 40-60 work more three decades after rights, but this estimate is only statistically significant, at the 10% level, in Column 1 of Table 12. The other specifications find a much smaller and not statistically significant effect.

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

simply that women fear childbirth due to the health risks, and thus prefer a quantity-quality tradeoff in favor of fewer, more highly educated children.[58] Thus, when women gain more power in the household, we would expect to see fewer children and more investment in education. Furthermore, we would not expect that this increased investment in education would be differential by the gender of child.[59] Additionally, we would expect to see these effects in households married after rights are granted, rather than all households in a state that grants rights, as rights were not granted retroactively, and thus only couples married after rights were granted should be affected by the legal changes.[60]

Indeed, we find such results. Fertility declines after rights are granted (Section 4.1.1). This decline in fertility can be accounted for by couples married after rights are granted (Section 4.1.2). Education increases, and this increase is not differential by gender of child (Section 4.2.1). Thus, we conclude that shifting bargaining power from husband to wife can indeed account for the results we document in this paper. This is consistent with the historical evidence suggesting that the legislatures at the time were aware that these rights would have a profound impact on household bargaining (Section 2.5).

We next discuss two other potential mechanisms by which women's property rights may have given rise to changes in these household decisions, and why they do not appear to be consistent with the results documented here. The first mechanism relates to the opportunity cost of women's time. The second set of mechanisms we discuss fall under the broad umbrella of development, but we note that the takeaway points can be widely applied.

The first mechanism is that women's rights may lead women to work more (Geddes and Lueck, 2002). This could have two effects. The first is that the increase the opportunity cost of mother's time may reduce fertility. A quantity-quality tradeoff would yield a rise in investment in education. The second is that this may yield an increase in the desire to invest in daughter's education, as they are expected to work in the future, and through a quantity-quality tradeoff reduce fertility. We can reject both of these hypotheses as women's labor force participation rates did not change with economic rights (Section 4.3), and the educational investment in daughters did not rise relative to sons (Section 4.2).

Turning to the second mechanism, Hazan et al. (2019) document that granting women property rights yields financial market deepening and economic growth, especially biased towards capital

[58] See Section 1 for a discussion of these points.

[59] Doepke and Tertilt (2009) explicitly predict, though do not empirically document, that women's rights would lead to more investment in education, exactly due to the impact of women's rights on household bargaining power. While they do not explicitly discuss fertility, it is reasonable to assume that their model would generate a decline in fertility through a quantity-quality tradeoff. Thus, our results are consistent with the mechanisms analyzed in Doepke and Tertilt (2009).

[60] We also note that there is no clear theoretical connection between granting married women property rights and their labor force participation through this channel. On one hand, granting women rights increases their incentives to work, as they no longer fear their husbands taking their income (Geddes and Lueck, 2002). On the other hand, a wealth effect may induce them to work less (Roberts, 2007). The results of our exercises suggest no impact of rights on labor supply, even comparing households married after rights were granted to those married before rights were granted.

intensive manufacturing.[61] A reasonable hypothesis might well be that the growth they document might well cause a decline in fertility and increase in education. We note two problems with this hypothesis. The first issue is that growth 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 that is through a general equilibrium effect, rather than the direct effect of rights on a household's decision, will run into this issue. As such, any proposed mechanism must affect individual households who were married with rights, rather than all households.

Similarly, one might think that future growth in manufacturing would imply higher returns to education, yielding the rise in education and, through a quantity-quality tradeoff, decline in fertility.[62] We note that these returns to education should imply more investment in the education of *sons*, rather than daughters (Galor and Weil, 2000; Galor and Moav, 2002). This is as sons were much more likely to work in the labor force (as adults) than daughters. Indeed, this fact remained unchanged as economic rights did not seem to increase women's labor force participation rates. Thus, this hypothesis would suggest the returns to investing in a son's education increased, while that of a daughter's education increase less.[63] However, we document an increase in education for all children regardless of gender. As such, any proposed mechanism must not work through changing returns to education or labor force participation, as we document that the increase in education was for all children – not just sons who were likely to work – and that women did not change their labor force participation rates.

We conclude that it is reasonable that a change in bargaining power from husband to wife is the central mechanism at work. We come to this conclusion as a shift in bargaining power can be reconciled with all of our empirical findings, it was clearly discussed in the historical record, and we do not have any other proposed mechanism that are consistent with the all of the implications of women's economic rights we document.

6 Conclusion

In this paper, we exploit the staggered timing of coverture's demise in order to study the impact of women's economic rights on household decision making. Using an event-study approach comparing counties on opposite sides of state borders, we find that fertility rates decline after rights are granted. We also find that education increases for children, and not differentially by the gender of the child. We find no impact of women's rights on labor force participation. Similarly,

[61] Similarly, Doepke and Tertilt (2019) discuss the relationship between female empowerment and economic growth, which may yield similar analyses.

[62] Again, we note that this effect would be on all households, and not just those married after rights were granted, and thus inconsistent with the data.

[63] Parents might invest in their daughters education, even if the girls do not work, because of returns to the education of the subsequent generation (Behrman and Rosenzweig, 2002).

comparing couples married before and after rights were granted yields comparable results, and indeed suggest that the decline in fertility can be entirely accounted for by households married after women were granted these rights.

We analyze several mechanisms, and conclude that a shift in household bargaining power can account for the changes we document.

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Appendix

A Construction of County-Border Pairs

The data on the evolution of US historical county boundaries comes from the Integrated Public Use Microdata Series (IPUMS) National Historical Geographic Information System (NHGIS), available at http://www.nhgis.com. Although there are other projects featuring US historical boundaries and spatial data within a Geographic Information Systems (GIS) framework, we use the NHGIS border definitions as they provide a better fit for mapping US federal census data from IPUMS. We start by obtaining eight geometry file maps corresponding to the 1850âĂŞ1920 census year boundaries. These shapefiles consist of polygons, each of which is defined by a list of vertices with two-dimensional coordinates. We use QGIS as our primary tool for handling the shapefiles. In order to identify the best topologically continuous set of bordering counties (i.e., counties adjacent to the counties borders from another state) over the entire 1850âĂŞ1920 period, we develop the following four-step procedure:

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

Step 2 The borders in the year 1920 are the final borders for our study. The borders in earlier decades were unstable due to evolution of states, as well as counties within the states. We create a stable system of IDs for each region based on the map of 1920. For the earlier decades (1850-1910), we adopt the names given in 1920. Each county's IDs in 1850-1910 are defined by the highest intersected area with the ones in 1920. In other words, each county x in 1850-1910 takes ID of the county y in 1920 if and only if county y has the highest intersected area with county x across all different intersected counties in 1920.

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

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

– For each county from decade we find all neighboring counties from other states in the same decade.

– If the joint border between pair of neighboring counties from 2 different states is longer than 10 percent of the length of each county's border with the other state, then we constitute pair-dummy for this pair.

– If a county-pair was not considered in previous decades– perhaps since the area wasn't well defined or stable– we create a new name for dummy variable based on the combination of IDs. This step allows us to produce a stable structure of dummies through time.

ANTON IS STILL MAKING EXAMPLE MAPS FOR INDIANA-ILLINOIS

B Marriage Market Balancing

In this Appendix, we test whether the marriage market was affected by women's rights. To do so, we begin by estimating regressions of the form described in Equation 1, where the dependent variable is either whether a man is married, the average age of married men, and the average age gap between husband and wife. We do so on two subsamples of men. The first subsample is all white non-Hispanic men age 15-60, and the second changes the age range to 15-30. We then change the dependent variable to be either whether a man is newly married (in the previous 12 months), the average age of newly married men, and the average age gap between newly married husband and wife. We again use the same two subsamples. We then estimate regressions of the form described in Equation 2, comparing couples married before and after rights, where the dependent variable is the age gap between husband and wife, on the sample of married white non-Hispanic women living in the state they were born in 1900 and 1910 (the two years we have duration of marriage data).

Table 14 shows the results when the dependent variable takes the value of 1 if a man is married and 0 otherwise. Column 1 uses the sample of men age 15-60, and includes as controls age fixed effects interacted with year fixed effects. Column 2 repeats Column 1, but also includes occupation and industry fixed effects interacted with year fixed effects. Columns 3 and 4 repeat Columns 1 and 2 on the sample of men age 15-30. Columns 1 and 2 find some evidence that there were fewer married men two and three decades before rights were granted, relative to a decade before rights were granted. However, the estimates are small quantitatively. Columns 3 and 4 do not find such evidence. All specifications find no impact of women's rights on the propensity for a man to be married.

Table 15 repeats Table 14, but switches the dependent variable to be the age of a man, conditions the sample on men being married, and removes the controls for the age of men. Thus, we measure the average age of married men before and after rights are granted. All specifications find no trend in the average age of married men prior to rights being granted.[64] After rights are granted, Columns 1 and 2 find an increase in the average age of a married man of about a 0.25 to 0.6 years, with the effects statistically significant. However, this effect almost entirely goes away when looking at younger men. Indeed, Column 3 finds no consistent evidence of any change in the average age of married men, with at most an increase of 0.1 years 2 decades after rights.[65] If anything, Column 4 suggests a decline in the average age of married men, but the only estimate that is significant is a decrease of 0.072 years a decade after rights are granted. We conclude that there is no consistent evidence that the average age of married men changed with women's rights.

Table 16 also repeats Table 14, switches the dependent variable to be the age gap between husband and wife, and conditions the sample on men being married. There is no pretrend in the age gap between husband and wife prior to rights being granted, and no pattern of a change in this gap after rights are granted.

Table 17 repeats Table 14, but switches the dependent variable to be whether a man has been married in the last year (is "newly wed"). There is no evidence of any trend in the propensity to be newly married either before or after women's rights in any specification.

Table 18 repeats Table 15, but conditions the sample on a man being newly married, rather than just married. Largely speaking, we find no evidence that the average age of newly married men changed around the time women were granted rights. Column 3 finds some evidence that the average age decreased by a half a year 1 and 3 decades after rights were granted, but these results are both quantitatively small and not robust, as seen in other specifications.

Table 19 repeats Table 16, but conditions the sample on the man being newly married. In all specifications, there is no evidence that the age gap between husband and wife exhibits a trend prior to rights being granted. There is some evidence that the age gap decreases by a little more than a half a year after rights are granted, in Columns 2 and 4. This effect is statistically significant at the 10% level only one decade after rights in Column 2, and at the 5% level one decade after rights and 10% level two decades after rights. We conclude that the effect is small, and not statistically robust.

Turning to the comparison of the age gap between couples married before and after rights are granted, Table 20 shows our results. Column 1 controls for the age of husband fixed effects and duration of marriage fixed effects, both interacted with year fixed effects. Column 2 repeats Column 1, but adds the husband's occupation and industry fixed effects, both interacted with year fixed effects. Column 3 repeats Column 2, but does not include the South. Column 4 also repeats Column 2, but does not include community property states. Column 5 (6) also repeats Column 2, but restricts the sample to be data from 1900 (1910).

[64]Column 4 has an estimate that is negative and statistically significant at the 10% level. However, considering that the estimates after rights are granted are also negative, this does not suggest a pretrend.

[65]This effect is significant at the 1% level, but not consistent with the other estimates in the same specification.

Panel A restricts attention to women age 40-60 in 1900 and 1910, while Panel B looks at women age 20-39. In Panel A, we find no economically meaningful or statistically significant difference in the age gap between spouses married before rather than after rights were granted. In Panel B, we find some evidence for a small difference in age age, of about 0.20-0.25 years (2-3 months) more of an age gap when the couple was married after women's rights. This estimate is statistically significant at the 1% level in Column 6, the 5% level in Columns 1, 2, 4, and 5, and not significant in Column 3. We note that this estimate is both small and the opposite sign of those found above. We thus conclude that women's rights did not have a clear impact on the age gap between husband and wife.

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

C County Heterogeneity Within State

As discussed in Section 3.3, state-level law changes are plausibly exogenous to individual counties in the state, allowing our event-study exercise to capture the causal effects of women's rights on households. However, this argument is invalid if all the counties within a state are similar. If this were the case, then state legislatures pass laws that all counties "agree" on, and reverse causality becomes a concern. In this appendix, we address this concern by studying heterogeneity within states during our sample period.

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

Table 21 reports the results. Panel A reports the results when the dependent variable is the probability a woman gave birth in the previous year. The number of counties in the sample increases from 1,492 to 3,063 over the course of the sample. The R^2 (adjusted R^2) ranges from about 0.1 (0.07) to 0.36 (0.35), suggesting that about 65-90% of variation between counties cannot be explained by state fixed effects. Panel B repeats this exercise for the number of children under age 5, and finds that the R^2 (adjusted R^2) ranges from about 0.25 (0.22) to 0.54 (0.53), suggesting that about 46-75% of variation between counties cannot be explained by state fixed effects. Panel C, D, and E repeat these exercises for children age 8-17, 8-13, and 14-17, respectively. In all cases, the R^2 (adjusted R^2) ranges from about 0.2 (02) to about 0.5 (0.5), suggesting that 50-80% of the variation between counties in education cannot be explained by state fixed effects. Finally, Panels F and G repeat these exercises for the labor force participation rates of women age 20-39 and 40-60, respectively. Approximately 70-99% of the variation in labor force participation rates cannot be explained by state fixed effects.

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

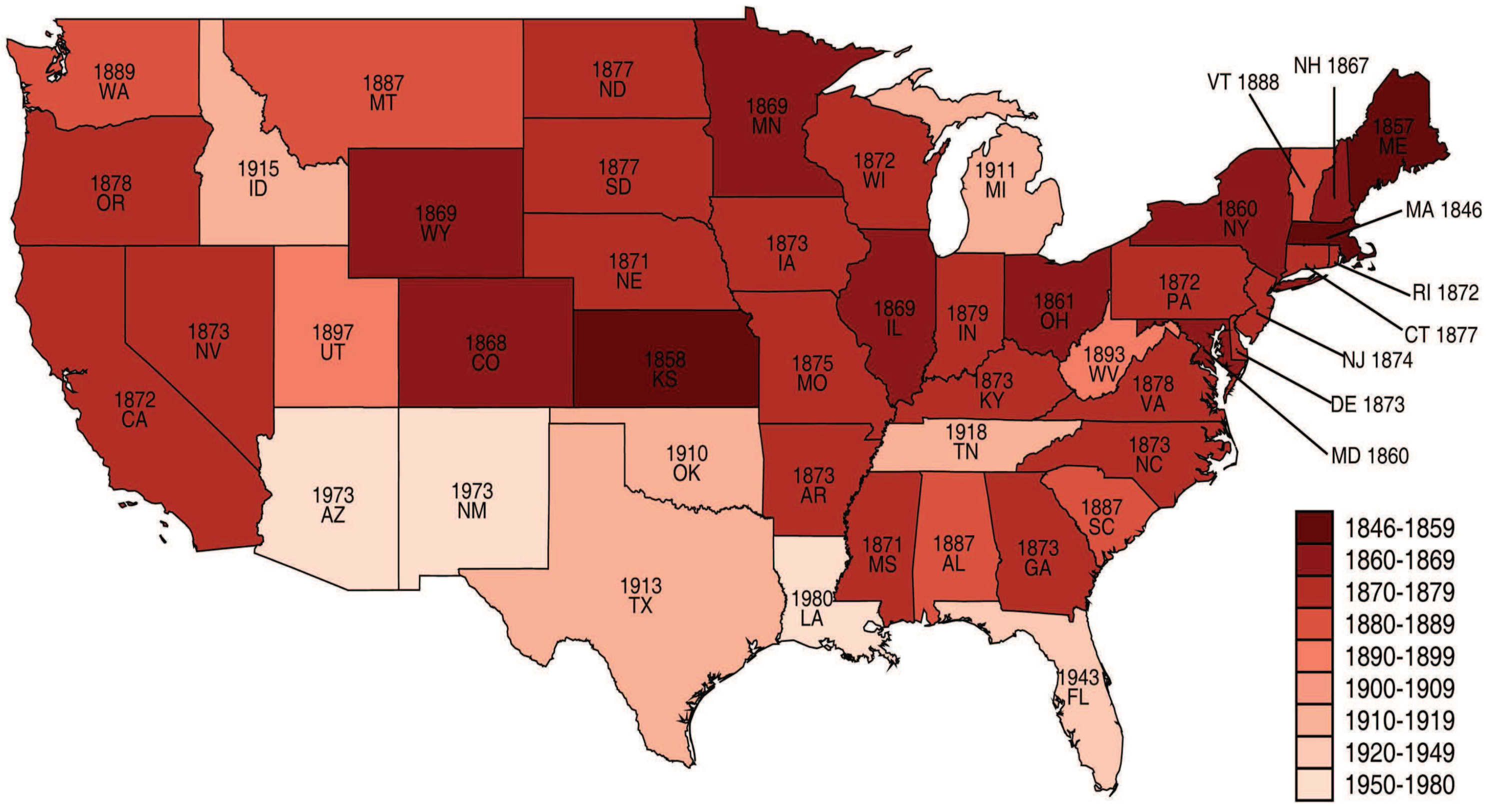

Figure 1: **Timing of women's rights by state.**

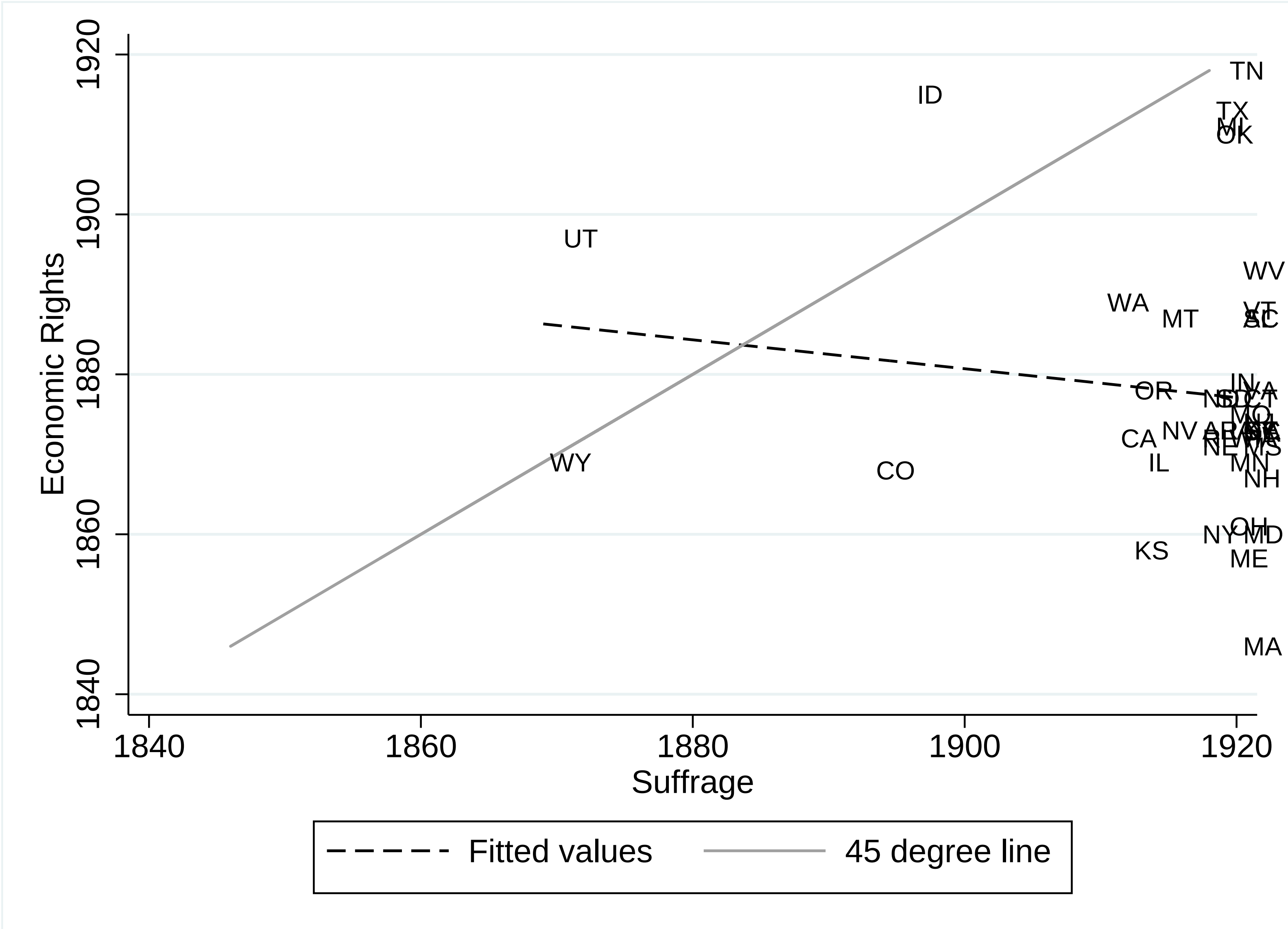

Figure 2: **Women's economic rights and suffrage, by state.**

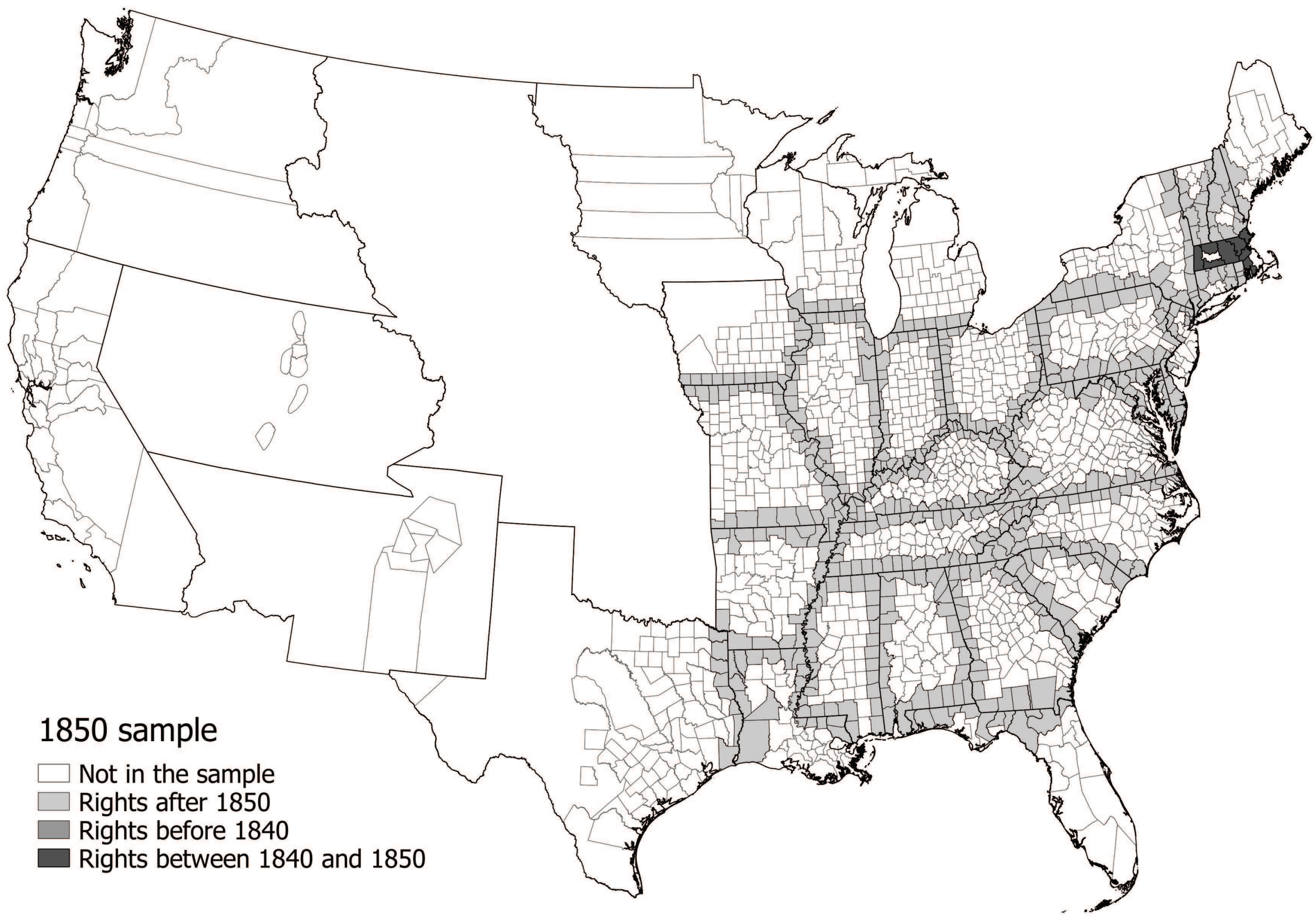

Figure 3: **State borders, 1850.**

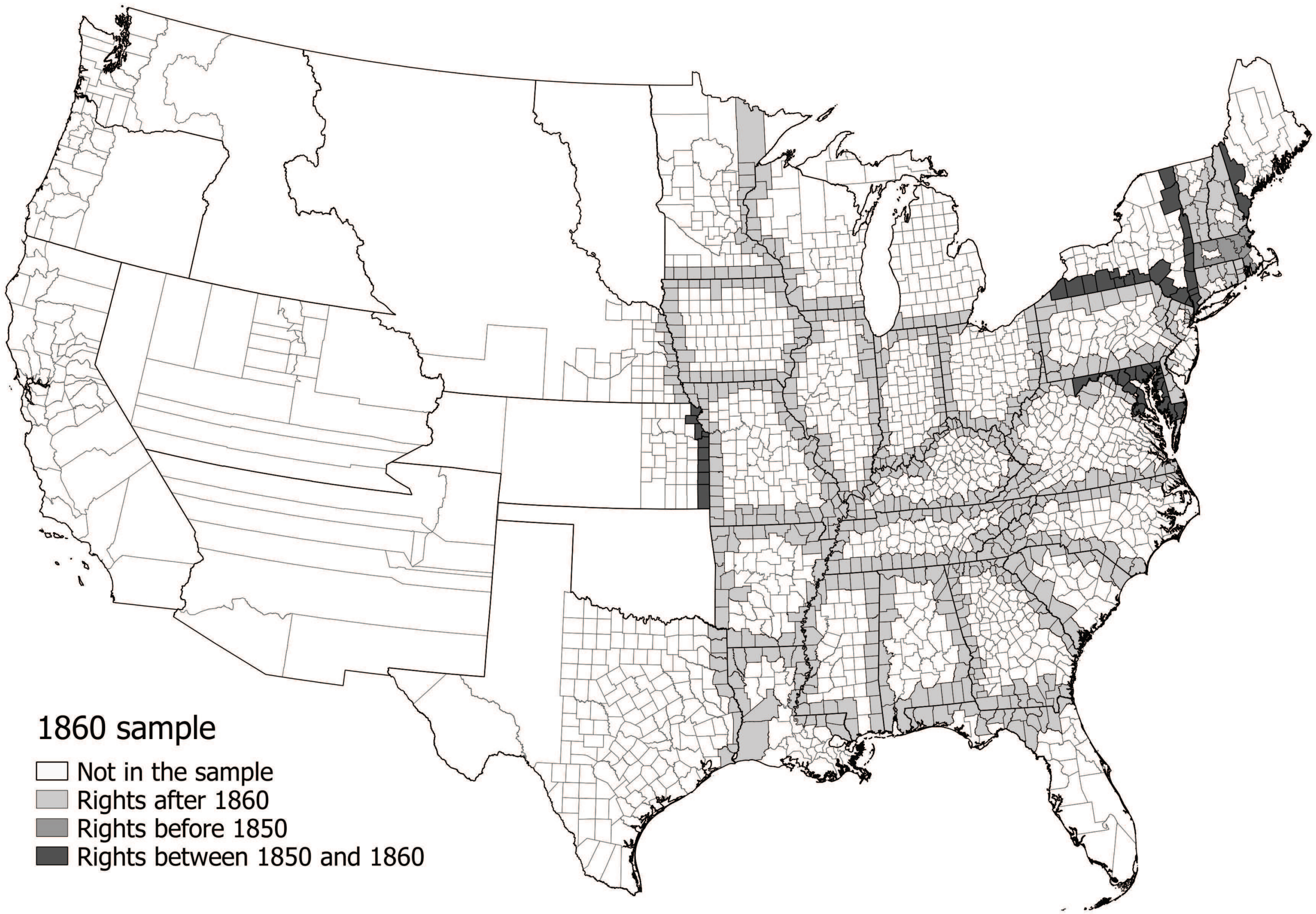

Figure 4: **State borders, 1860.**

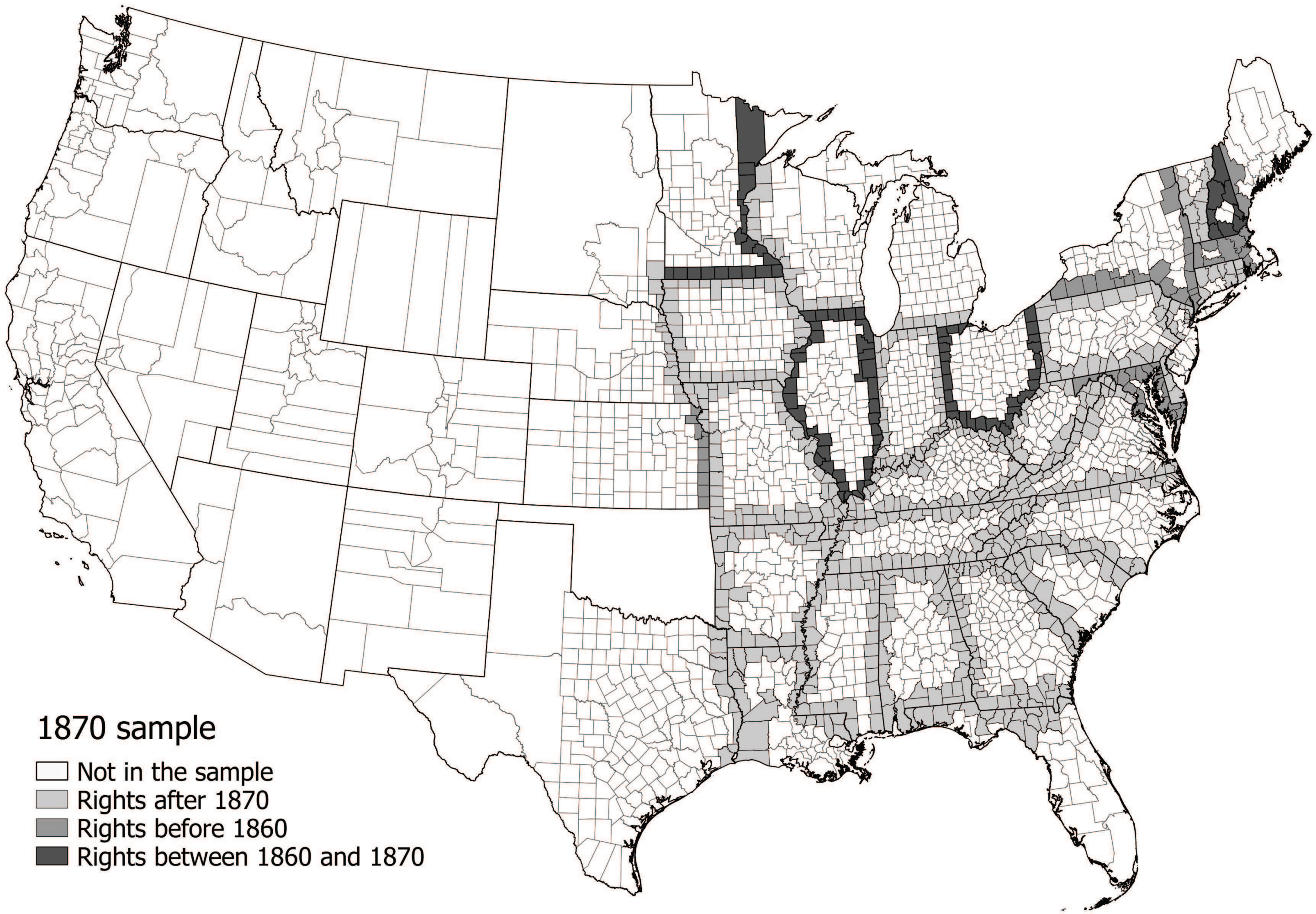

Figure 5: **State borders, 1870.**

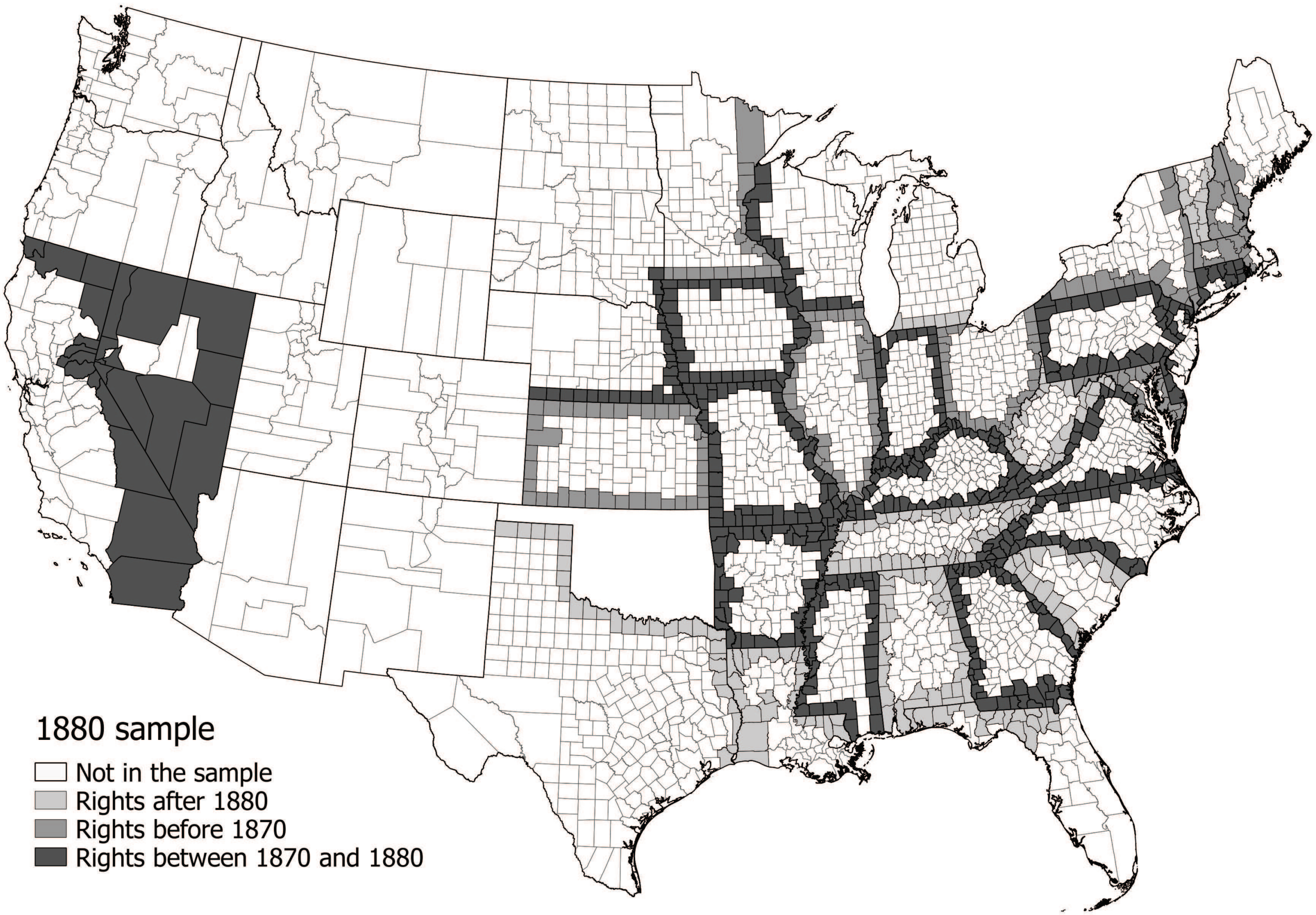

Figure 6: **State borders, 1880.**

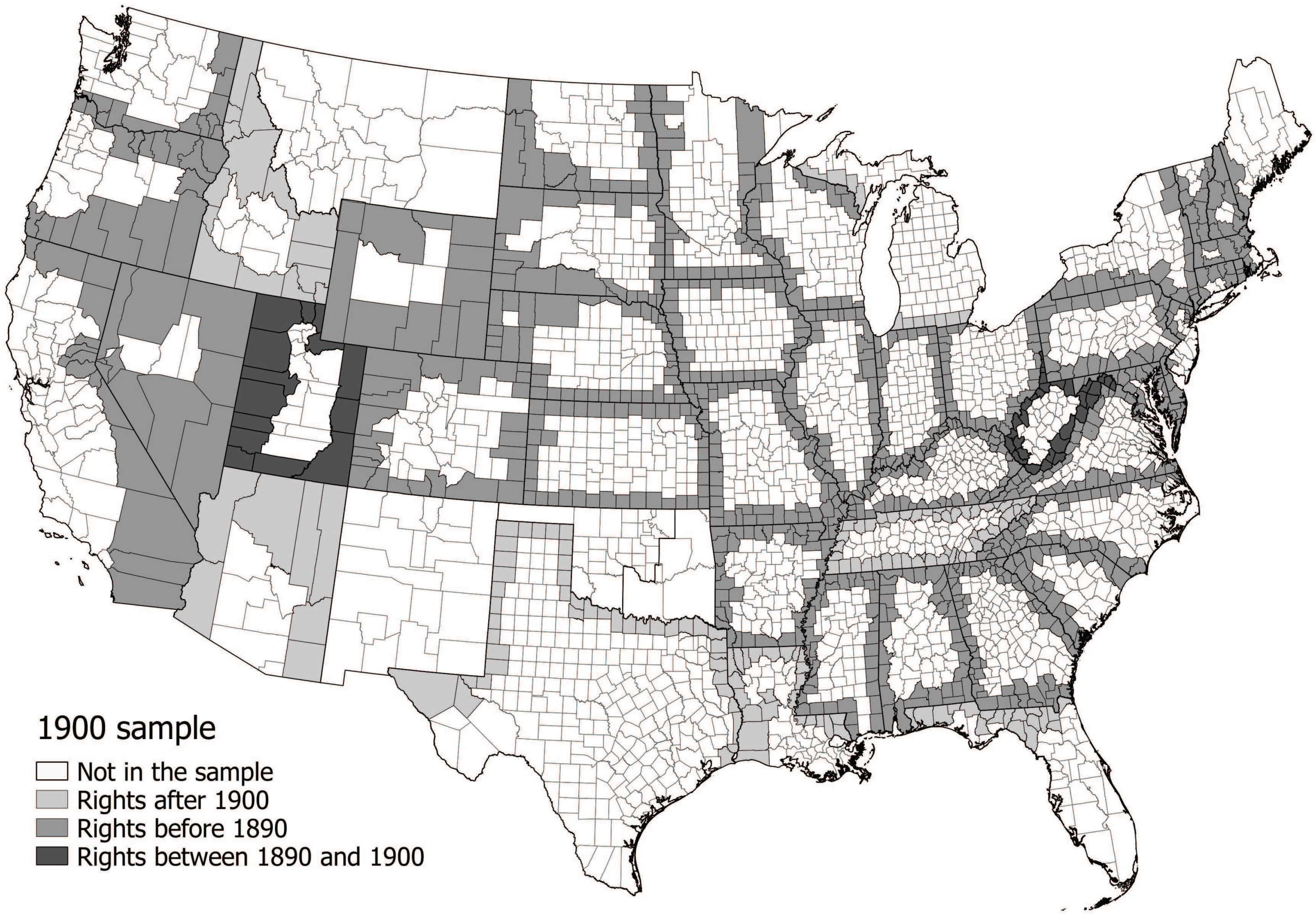

Figure 7: **State borders, 1900.**

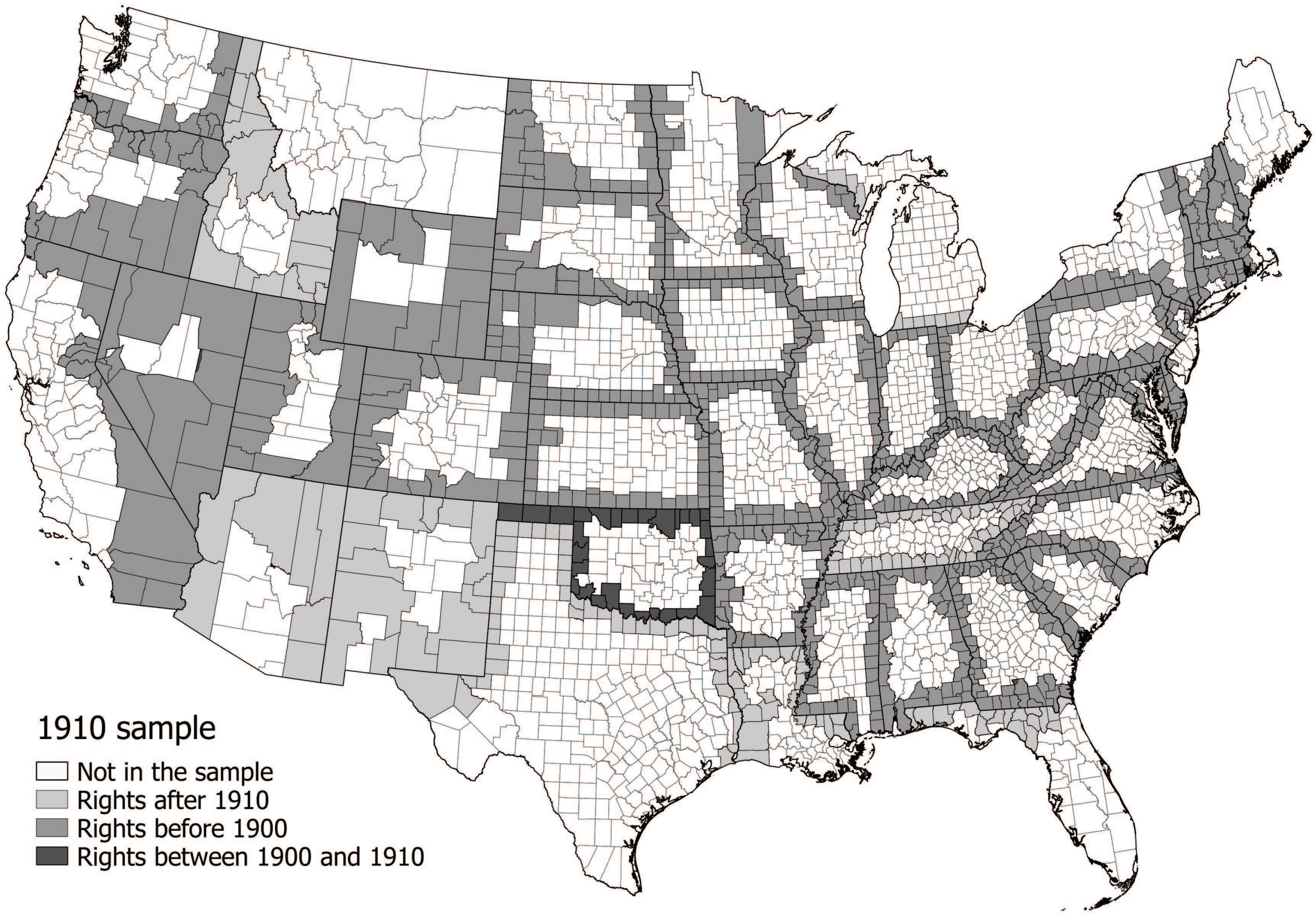

Figure 8: **State borders, 1910.**

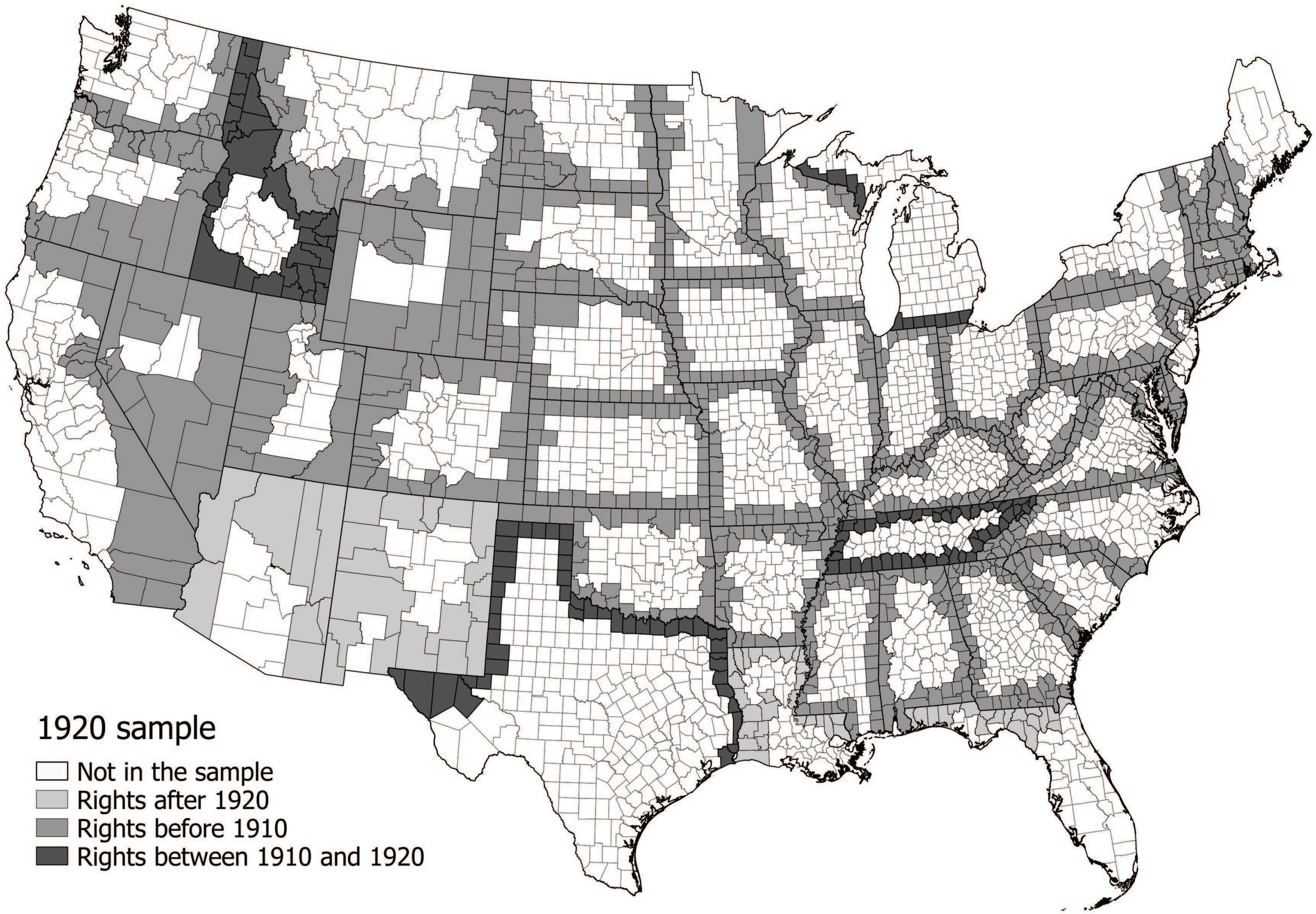

Figure 9: **State borders, 1920.**

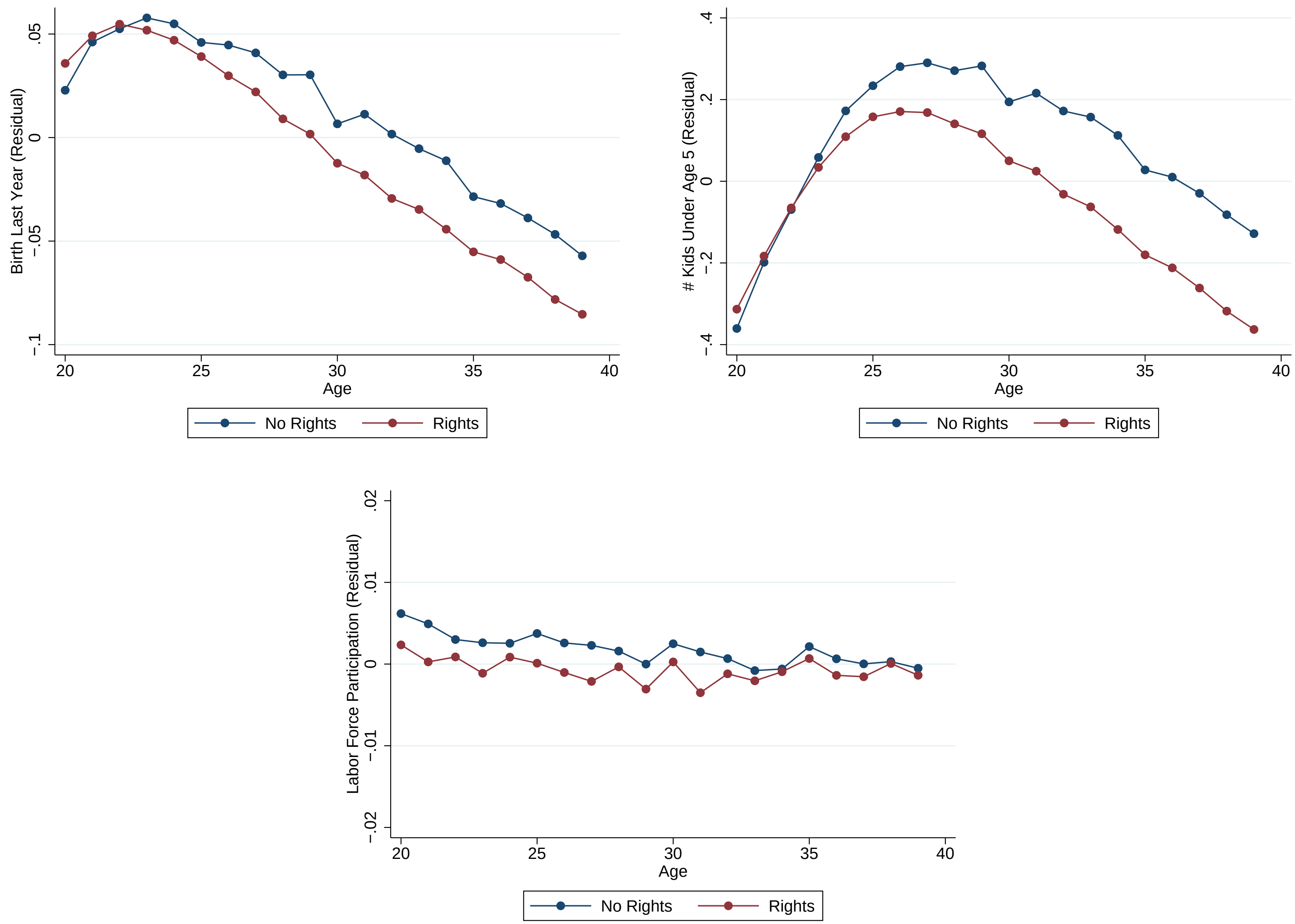

Figure 10: The top left panel shows the probability of giving birth by age of mother, with and without economic rights. The top right does the same analysis for the number of kids under age 5 in a household. The bottom panel also repeats this analysis for the labor force participation rate of the wife of the household. In all cases, the sample is white, non-Hispanic married women age 20-39 who live in the same state in which they were born. All variables are net of year fixed effects.

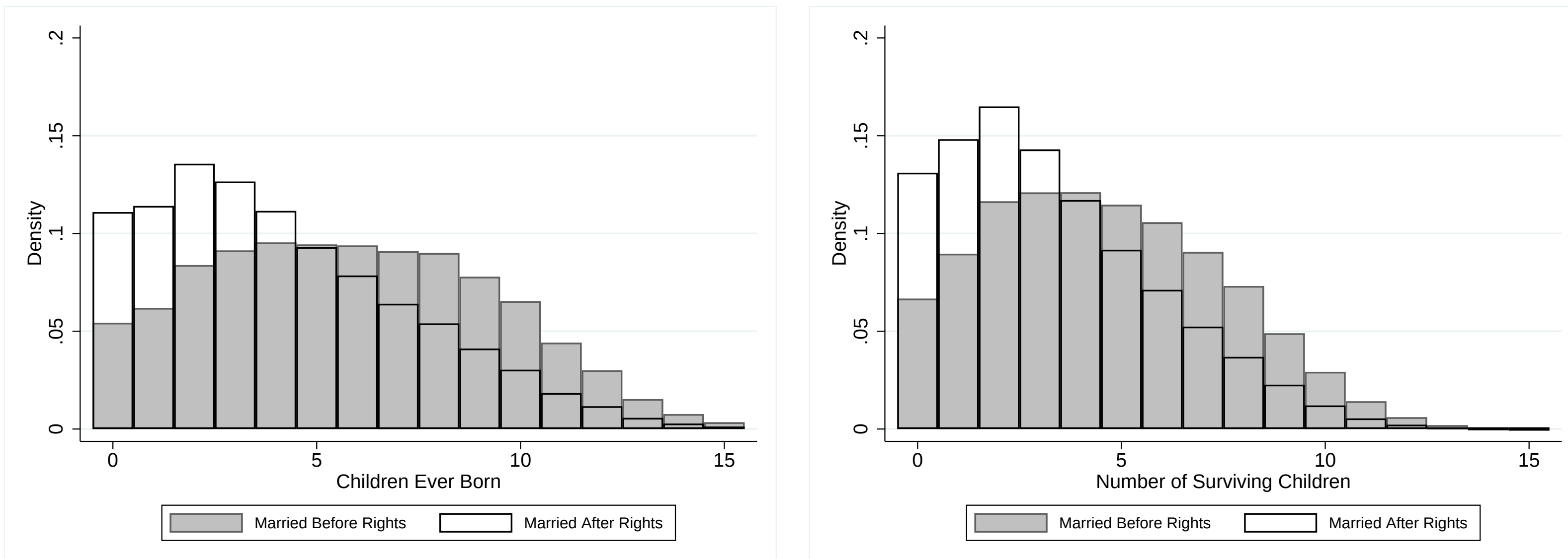

Figure 11: The left panel plots the density of children ever born to white, non-Hispanic married women age 40-60 who live in the same state in which they were born in 1900 and 1910. The plot is done separately by whether these women were married with economic rights or not. The right panel repeats this exercise for surviving children.

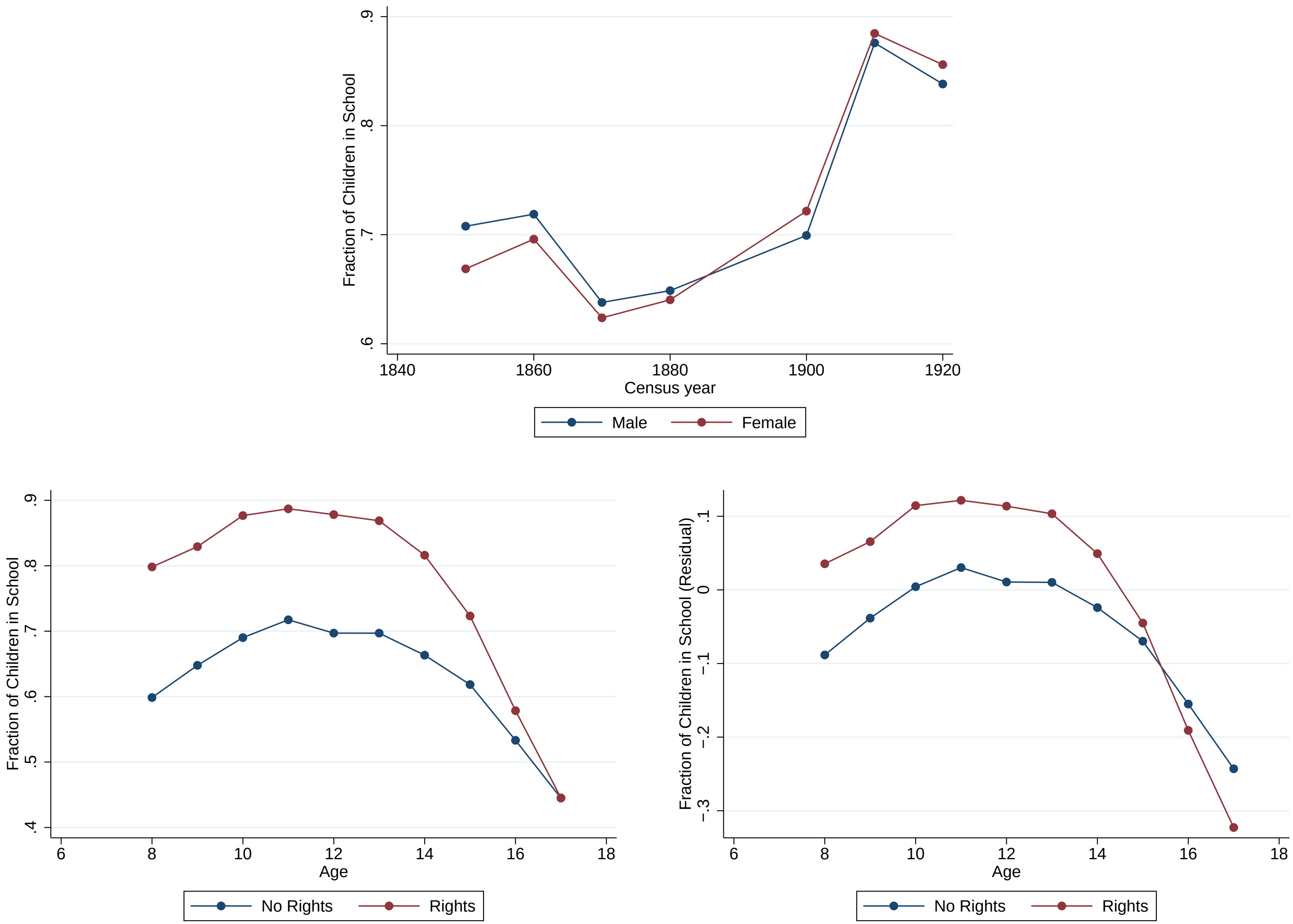

Figure 12: The top panel shows the fraction of kids in school, by gender, over time in the US. The bottom left panel shows the fraction of kids in school, by age and whether or not there are women's economic rights. The bottom right panel repeats the bottom left panel, but nets out year interacted with gender fixed effects.

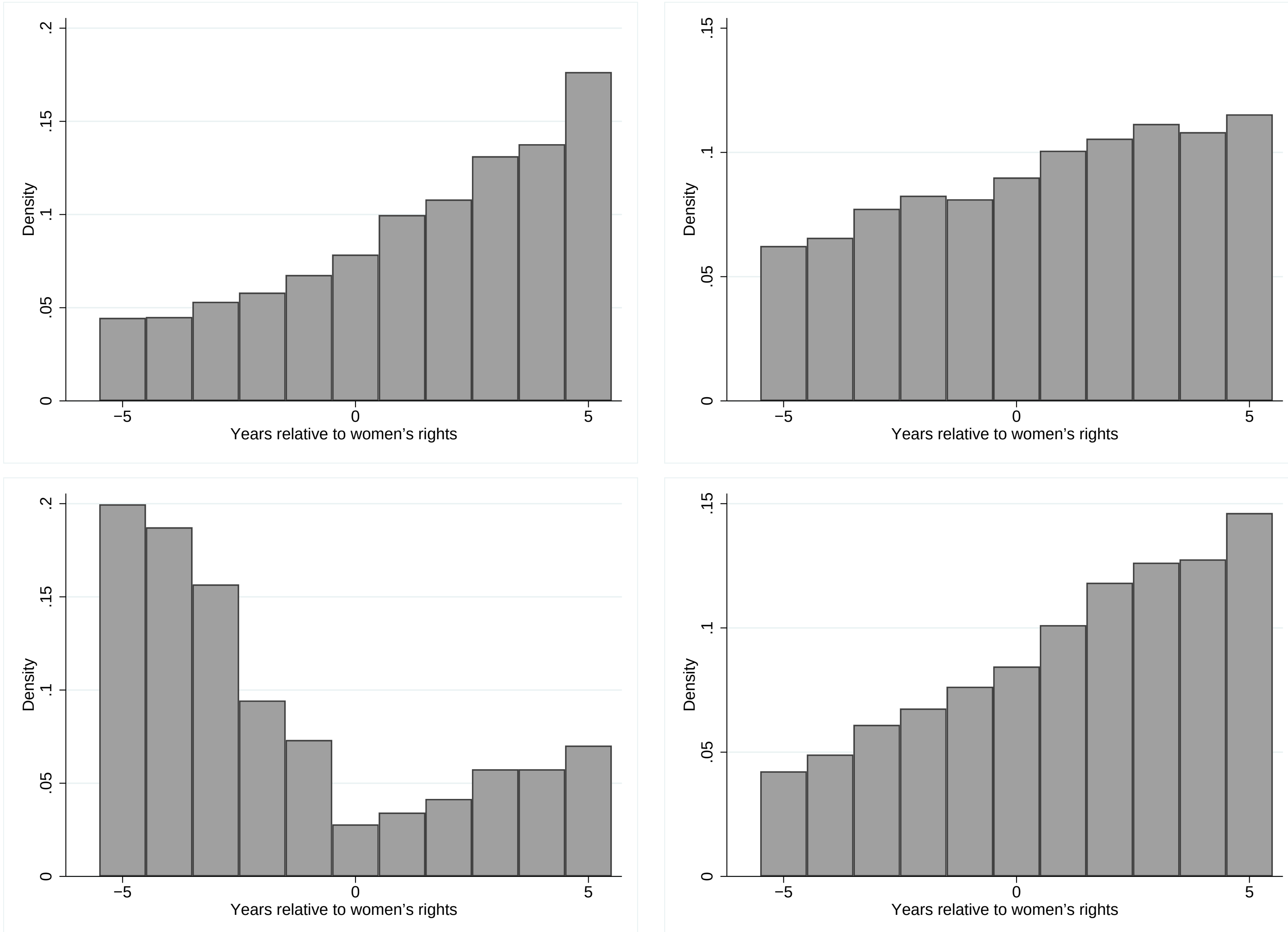

Figure 13: The top panel left panel shows the number of people getting married, relative to the year their state gave rights, in the 1900 US census, when limiting to white non-Hispanic couples, where the wife is 20-39 years old. The top right panel does the same for couples where the wife is 40-60. The bottom left and bottom right repeat this patter using the 1910 census.

Table 1: Summary Statistics by Rights, Event Study

	Before Rights		After Rights	
	Mean	Stand. Dev.	Mean	Stand. Dev.
	Panel A: Fertility			
Birth Last Year	0.239	0.426	0.187	0.390
# of Kids Under Age 5	1.386	1.030	1.111	1.016
Age	28.633	5.451	29.422	5.424
Spouse's Age	33.211	6.866	33.668	6.672
Year	1870.457	19.926	1904.824	16.395
N	2,743,165		11,717,907	
	Panel B: Labor Force Participation 20-39			
Labor Force Participation Rate	0.0488	0.215	0.0502	0.218
Age	28.582	5.460	29.419	5.424
Spouse's Age	33.215	6.933	33.666	6.673
Year	1877.246	18.543	1905.162	15.868
N	2,059,634		11,646,044	
	Panel C: Labor Force Participation 40-60			
Labor Force Participation Rate	0.0419	0.200	0.045	0.207
Age	47.079	5.336	47.113	5.285
Spouse's Age	51.138	7.653	50.810	7.434
Year	1877.08	18.459	1905.711	15.920
N	870,424		5,916,952	

Table 2: Summary Statistics by Rights, Education Event Study

	Before Rights		After Rights	
	Mean	Stand. Dev.	Mean	Stand. Dev.
	Panel A: In School, All			
In School, 6-18	0.602	0.490	0.774	0.418
Boys in School, 6-18	0.613	0.487	0.770	0.421
Girls in School, 6-18	0.590	0.492	0.778	0.415
Mother's Age	38.289	7.809	38.626	7.529
Father's Age	43.445	8.612	43.436	8.376
Year	1867.863	18.756	1905.660	18.579
N	6,051,442		18,536,547	
	Panel B: In School, Ages 6-10			
In School, 6-10	0.584	0.493	0.805	0.396
Boys in School, 6-10	0.588	0.492	0.804	0.397
Girls in School, 6-10	0.580	0.494	0.806	0.395
Mother's Age	35.417	7.133	35.485	6.847
Father's Age	40.494	8.244	40.248	7.973
Year	1867.782	18.736	1905.479	18.591
N	2,880,430		8,428,919	
	Panel C: In School, Ages 11-18			
In School, 11-18	0.618	0.486	0.748	0.434
Boys in School, 11-18	0.635	0.481	0.741	0.438
Girls in School, 11-18	0.600	0.490	0.755	0.430
Mother's Age	40.898	7.474	41.238	7.058
Father's Age	46.125	8.044	46.094	7.753
Year	1867.937	18.774	1905.812	18.567
N	3,171,012		10,107,628	

Table 3: Summary Statistics by Rights, Married Before-After Rights

	Before Rights		After Rights	
	Mean	Stand. Dev.	Mean	Stand. Dev.
	Panel A: Ages 20-39			
Birth Last Year	0.203	0.402	0.182	0.386
# of Kids Under Age 5	1.211	1.035	1.079	1.015
Labor Force Participation 20-39	0.038	0.192	0.033	0.178
Age	29.024	5.467	29.704	5.428
Spouse's Age	33.974	7.544	34.104	7.202
Year	1905.862	4.925	1905.518	4.973
N	938,738		6,489,403	
	Panel B: Ages 40-60			
Children Ever Born	5.837	3.476	4.178	3.156
Surviving Children	4.593	2.866	3.348	2.597
Labor Force Participation	0.039	0.192	0.046	0.192
Age	49.580	5.883	46.640	5.233
Spouse's Age	53.633	7.904	50.144	7.663
Year	1903.953	4.889	1906.158	4.864
N	896,181		2,868,915	

Table 4: Birth, 1850-1920

Dependent Variable	Birth Last Year				
	(1)	(2)	(3)	(4)	(5)
$\geq$ 3 Decades Before	0.000	0.001	0.001	0.001	-0.001
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
2 Decades Before	-0.001	-0.001	-0.001	-0.002	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
1 Decade Before	0	0	0	0	0
Rights Given	-0.007***	-0.007***	-0.006***	-0.006**	-0.006**
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
1 Decade After	-0.010***	-0.011***	-0.010***	-0.010***	-0.009**
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
2 Decades After	-0.012***	-0.012***	-0.011***	-0.012***	-0.010***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)
$\geq$ 3 Decades After	-0.015***	-0.015***	-0.014***	-0.015***	-0.012***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes
# of Kids Under 5	No	No	Yes	Yes	Yes
Sample	All	All	All	No South	No CP
N	14,461,072	14,461,072	14,461,072	11,652,763	13,946,069
R^2	0.025	0.027	0.032	0.033	0.032

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include wife's age and husband's age fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. # of Kids Under 5 is a set of fixed effects for the number of kids under age 5 in the household as of last year. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, age 20-39, who live in the same state in which they were born.

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

Dependent Variable	# of Kids Under Age 5			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	0.005	0.008	0.011	0.002
	(0.021)	(0.020)	(0.020)	(0.020)
2 Decades Before	-0.014	-0.014	-0.014	-0.017
	(0.016)	(0.016)	(0.017)	(0.016)
1 Decade Before	0	0	0	0
Rights Given	-0.028*	-0.029*	-0.025^{+}	-0.026^{+}
	(0.015)	(0.016)	(0.016)	(0.016)
1 Decade After	-0.037**	-0.040**	-0.042**	-0.036**
	(0.017)	(0.017)	(0.018)	(0.017)
2 Decades After	-0.056**	-0.059***	-0.059***	-0.053**
	(0.022)	(0.021)	(0.020)	(0.022)
$\geq$ 3 Decades After	-0.080***	-0.081***	-0.084***	-0.074***
	(0.024)	(0.023)	(0.023)	(0.024)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes
Sample	All	All	No South	No CP
N	14,461,072	14,461,072	11,652,763	13,946,069
R^2	0.108	0.120	0.123	0.120

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include wife's age and husband's age fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 3 excludes all borders of Southern States with non-Southern States. Column 4 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, age 20-39, who live in the same state in which they were born.

Table 6: Birth Last Year & # of Kids Under Age 5, Married After Rights 1900-1910

Panel A:	Dependent Variable: Birth Last Year					
	(1)	(2)	(3)	(4)	(5)	(6)
Married After Rights	-0.009***	-0.009***	-0.010	-0.009***	-0.009***	-0.004
	(0.003)	(0.003)	(0.009)	(0.003)	(0.003)	(0.003)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,428,141	7,428,122	5,195,858	6,902,782	3,297,166	4,130,956
R^2	0.0509	0.0534	0.0523	0.0531	0.0497	0.0547
Panel B:	Dependent Variable: # of Kids Under Age 5					
Married After Rights	-0.140***	-0.135***	-0.165	-0.143***	-0.139***	-0.117***
	(0.037)	(0.038)	(0.106)	(0.038)	(0.041)	(0.033)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,428,141	7,428,122	5,195,858	6,902,782	3,297,166	4,130,956
R^2	0.1898	0.2020	0.1774	0.1988	0.2032	0.2002

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

Table 7: Children Ever Born & Surviving Children, Married After Rights 1900-1910

Panel A:	Dependent Variable: Children Ever Born						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Married After Rights	-0.220**	-0.223**	-0.211**	-0.212*	-0.238**	-0.230**	-0.210**
	(0.099)	(0.100)	(0.096)	(0.110)	(0.101)	(0.112)	(0.098)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	CEB> 0	No South	No CP	1900	1910
N	3,765,096	3,765,075	3,397,968	2,689,512	3,614,044	1,644,200	2,120,875
R^2	0.2708	0.2898	0.2568	0.2358	0.2845	0.2908	0.2856

Panel B:	Dependent Variable: Surviving Children						
Married After Rights	-0.167**	-0.169**	-0.160**	-0.128	-0.177**	-0.169*	-0.169**
	(0.080)	(0.081)	(0.076)	(0.088)	(0.082)	(0.087)	(0.084)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	CEB> 0	No South	No CP	1900	1910
N	3,765,096	3,765,075	3,397,968	2,689,512	3,614,044	1,644,200	2,120,875
R^2	0.2487	0.2670	0.2318	0.2127	0.2625	0.2703	0.2620

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. Control include wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states.The sample includes white, non-Hispanic married women, age 40-60, who live in the same state in which they were born.

Table 8: School, 1850-1920

Dep. Var.	Probability of Being in School								
Children's Age	6-18			6-10			11-18		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
≥ 3 Decades Before	0.023	0.024	0.025	0.020	0.018	0.017	0.028	0.030	0.032
	(0.025)	(0.026)	(0.026)	(0.028)	(0.027)	(0.027)	(0.025)	(0.026)	(0.026)
2 Decades Before	0.022	0.020	0.020	0.024	0.022	0.023	0.020	0.018	0.019
	(0.025)	(0.023)	(0.023)	(0.025)	(0.022)	(0.022)	(0.027)	(0.025)	(0.026)
1 Decade Before	0	0	0	0	0	0	0	0	0
Rights Given	0.039**	0.040**	0.040**	0.045**	0.047***	0.048***	0.033*	0.035*	0.034*
	(0.018)	(0.017)	(0.017)	(0.019)	(0.017)	(0.017)	(0.019)	(0.019)	(0.019)
1 Decade After	0.044**	0.046***	0.046***	0.050***	0.053***	0.055***	0.036*	0.033*	0.033
	(0.018)	(0.017)	(0.018)	(0.019)	(0.016)	(0.017)	(0.021)	(0.020)	(0.021)
2 Decades After	0.051**	0.052**	0.052**	0.069***	0.075***	0.077***	0.034	0.032	0.030
	(0.023)	(0.022)	(0.023)	(0.024)	(0.020)	(0.020)	(0.024)	(0.024)	(0.025)
≥ 3 Decades After	0.037	0.034	0.034	0.055**	0.064***	0.066***	0.020	0.013	0.013
	(0.025)	(0.024)	(0.024)	(0.026)	(0.024)	(0.024)	(0.027)	(0.027)	(0.028)
≥ 3 Decades Before×Female			-0.000			0.002			-0.004
			(0.005)			(0.004)			(0.008)
2 Decades Before×Female			-0.001			-0.000			-0.002
			(0.004)			(0.003)			(0.006)
1 Decade Before×Female			0			0			0
Rights Given×Female			0.000			-0.002			0.002
			(0.003)			(0.002)			(0.004)
1 Decade After×Female			-0.001			-0.002			0.001
			(0.005)			(0.003)			(0.008)
2 Decades After×Female			-0.000			-0.004			0.004
			(0.006)			(0.004)			(0.010)
≥ 3 Decades After×Female			-0.000			-0.004			0.002
			(0.006)			(0.003)			(0.010)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
N	24,587,989	24,587,989	24,587,989	11,309,349	11,309,349	11,309,349	13,278,640	13,278,640	13,278,640
R^2	0.230	0.240	0.240	0.258	0.267	0.267	0.226	0.243	0.243

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include the child's age, gender, mother's age and father's age fixed effects, all interacted with year fixed effects. Extra controls include father's occupation and industry fixed effects, the number of children at home, whether this child was allowed to work, and whether this child was allowed to not be in school, all interacted with year fixed effects. The sample includes children age 6-18 who are sons of white, non-Hispanic married mothers, age 20-60, who live in the same state in which they were born.

Table 9: School, 1850-1920. Robustness

Dep. Var.	Probability of Being in School					
Children's Age	6-18		6-10		11-18	
	(1)	(2)	(3)	(4)	(5)	(6)
$\geq$ 3 Decades Before	0.030	0.028	0.022	0.021	0.038	0.034
	(0.025)	(0.027)	(0.026)	(0.029)	(0.026)	(0.027)
2 Decades Before	0.025	0.022	0.026	0.024	0.024	0.019
	(0.026)	(0.024)	(0.025)	(0.023)	(0.029)	(0.026)
1 Decade Before	0	0	0	0	0	0
Rights Given	0.041**	0.039**	0.051***	0.047***	0.035*	0.034*
	(0.016)	(0.017)	(0.015)	(0.017)	(0.019)	(0.019)
1 Decade After	0.053***	0.044**	0.057***	0.052***	0.044*	0.032
	(0.020)	(0.018)	(0.018)	(0.017)	(0.024)	(0.021)
2 Decades After	0.058***	0.050**	0.081***	0.076***	0.037	0.029
	(0.020)	(0.023)	(0.018)	(0.021)	(0.025)	(0.026)
$\geq$ 3 Decades After	0.042	0.032	0.075***	0.064**	0.020	0.013
	(0.026)	(0.025)	(0.023)	(0.026)	(0.031)	(0.029)
$\geq$ 3 Decades Before×Female	-0.001	-0.002	0.001	0.002	-0.004	-0.006
	(0.005)	(0.005)	(0.004)	(0.004)	(0.008)	(0.007)
2 Decades Before×Female	-0.001	-0.000	-0.002	0.001	0.000	-0.001
	(0.005)	(0.004)	(0.004)	(0.003)	(0.007)	(0.006)
1 Decade Before×Female	0	0	0	0	0	0
Rights Given×Female	0.001	0.000	-0.002	-0.002	0.004	0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
1 Decade After×Female	0.000	-0.000	-0.003	-0.001	0.003	0.001
	(0.005)	(0.006)	(0.004)	(0.003)	(0.008)	(0.008)
2 Decades After×Female	0.000	0.000	-0.005	-0.005	0.006	0.006
	(0.005)	(0.007)	(0.004)	(0.004)	(0.009)	(0.011)
$\geq$ 3 Decades After×Female	-0.000	0.001	-0.006*	-0.004	0.003	0.004
	(0.006)	(0.006)	(0.003)	(0.004)	(0.009)	(0.011)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	Yes	Yes	Yes	Yes	Yes	Yes
Sample	No South	No CP	No South	No CP	No South	No CP
N	19,505,593	23,753,238	8,982,663	10,899,939	10,522,930	12,853,299
R^2	0.250	0.239	0.284	0.265	0.249	0.242

Notes: $^+$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include the child's age, gender, mother's age and father's age fixed effects, all interacted with year fixed effects. Extra controls include father's occupation and industry fixed effects, the number of children at home, whether this child was allowed to work, and whether this child was allowed to not be in school, all interacted with year fixed effects. The sample includes children age 6-18 who are sons of white, non-Hispanic married mothers, age 20-60, who live in the same state in which they were born.

Table 10: Married After Rights, Attending School, 1900-1910

Dependent Variable:	In School					
Children's Age	8-17		8-13		14-17	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: 1900-1910					
Married After Rights	0.009*	0.010*	0.003	0.005	0.022***	0.019**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.007)	(0.008)
Married After Rights×Female		-0.002		-0.004+		0.006
		(0.003)		(0.003)		(0.006)
N	6,368,189	6,368,189	4,130,291	4,130,291	2,237,735	2,237,735
R^2	0.2138	0.2138	0.1854	0.1854	0.2162	0.2162
	Panel B: 1910					
Married After Rights	0.009***	0.010**	0.002**	0.003**	0.024***	0.024***
	(0.003)	(0.004)	(0.001)	(0.001)	(0.006)	(0.006)
Married After Rights×Female		-0.002		-0.002*		-0.001
		(0.002)		(0.001)		(0.005)
N	6,097,295	6,097,295	3,950,852	3,950,852	2,146,413	2,146,413
R^2	0.1862	0.1862	0.0826	0.0826	0.1717	0.1717
	Panel C: 1900					
Married After Rights	0.009	0.010+	0.003	0.006	0.021**	0.016+
	(0.006)	(0.007)	(0.005)	(0.005)	(0.009)	(0.011)
Married After Rights×Female		-0.002		-0.006		0.011
		(0.005)		(0.005)		(0.009)
N	270,894	270,894	179,439	179,439	91,322	91,322
R^2	0.1718	0.1718	0.1448	0.1448	0.2036	0.2036

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level, are in parentheses. All specifications include fixed effects for the ages of the child, mother, and father, fixed effects for the industry and occupation of the father, and county fixed effects. All specifications also include indicator variables for whether the child is female, allowed to work, and allowed to not be in school. All controls are interacted with year fixed effects. Standard errors are clustered by state.

Table 11: Labor Force Participation, 1860-1920

Dependent Variable	Labor Force Participation				
	(1)	(2)	(3)	(4)	(5)
$\geq$ 3 Decades Before	-0.037^{+}	-0.028^{+}	-0.028^{+}	-0.029*	-0.033*
	(0.023)	(0.018)	(0.018)	(0.015)	(0.018)
2 Decades Before	-0.009	-0.009	-0.009	-0.008	-0.010
	(0.011)	(0.009)	(0.009)	(0.011)	(0.010)
1 Decade Before	0	0	0	0	-0
Rights Given	-0.007	-0.006	-0.006	-0.002	-0.006
	(0.011)	(0.010)	(0.010)	(0.009)	(0.011)
1 Decade After	0.015	0.010	0.010	0.005	0.012
	(0.015)	(0.014)	(0.014)	(0.013)	(0.014)
2 Decades After	0.033**	0.021*	0.021*	0.023**	0.023*
	(0.014)	(0.012)	(0.012)	(0.011)	(0.013)
$\geq$ 3 Decades After	0.022	0.010	0.010	0.010	0.012
	(0.018)	(0.016)	(0.016)	(0.016)	(0.017)
Controls	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes
# of Kids Under 5	No	No	Yes	Yes	Yes
Sample	All	All	All	No South	No CP
N	13,705,678	13,705,678	13,705,678	11,070,368	13,196,399
R^2	0.066	0.077	0.078	0.074	0.079

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include wife's age and husband's age fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. # of Kids Under 5 is a set of fixed effects for the number of kids under age 5 in the household as of last year. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, age 20-39, who live in the same state in which they were born.

Table 12: Labor Force Participation 40-60, 1860-1920

Dependent Variable	Labor Force Participation				
	(1)	(2)	(3)	(4)	(5)
$\geq$ 3 Decades Before	-0.037*	-0.031*	-0.031*	-0.030**	-0.033*
	(0.021)	(0.017)	(0.017)	(0.014)	(0.017)
2 Decades Before	-0.013	-0.012	-0.012	-0.008	-0.012
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
1 Decade Before	0	0	0	0	0
Rights Given	-0.004	-0.003	-0.003	0.003	-0.003
	(0.011)	(0.010)	(0.010)	(0.008)	(0.011)
1 Decade After	0.009	0.007	0.007	0.005	0.008
	(0.011)	(0.010)	(0.010)	(0.010)	(0.011)
2 Decades After	0.026**	0.021**	0.021**	0.022**	0.022**
	(0.011)	(0.010)	(0.010)	(0.009)	(0.011)
$\geq$ 3 Decades After	0.025*	0.017	0.017	0.017	0.019
	(0.014)	(0.013)	(0.013)	(0.012)	(0.014)
Controls	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes
# of Kids Under 5	No	No	Yes	Yes	Yes
Sample	All	All	All	No South	No CP
N	6,787,376	6,787,376	6,787,376	5,399,075	6,634,779
R^2	0.047	0.058	0.059	0.057	0.059

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the county-border pair and state, are in parentheses. All specifications include county-border pair fixed effects and county-border pair linear time trend. Control include wife's age and husband's age fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. # of Kids Under 5 is a set of fixed effects for the number of kids under age 5 in the household as of last year. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, age 40-60, who live in the same state in which they were born.

Table 13: Labor Force Participation, Married After Rights 1900-1910

Dependent Variable:	Labor Force Participation					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Women Aged 40-60					
Married After Rights	0.000	0.000	-0.002**	0.000	-0.001	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	3,765,096	3,765,075	2,689,512	3,614,044	1,644,200	2,120,875
R^2	0.0256	0.0571	0.0538	0.0561	0.0524	0.0553
Panel B	Women Aged 20-39					
Married After Rights	0.001	0.001	-0.001	0.001	0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,428,141	7,428,122	5,195,858	6,902,782	3,297,166	4,130,956
R^2	0.0550	0.0631	0.0285	0.0595	0.0363	0.0665

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. Control include wife's age and husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, who live in the same state in which they were born. Panel A restricts attention to women age 40-60, while Panel B restricts attention to women age 20-39.

Table 14: Probability of being Married, 1850-1920

Dependent Variable	Married			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.011*	-0.015*	-0.008	-0.010
	(0.007)	(0.008)	(0.010)	(0.010)
2 Decades Before	-0.006**	-0.007**	-0.004	-0.005
	(0.003)	(0.003)	(0.005)	(0.005)
1 Decade Before	0	0	0	0
Rights Given	0.004	0.003	0.004	0.004
	(0.003)	(0.003)	(0.006)	(0.005)
1 Decade After	0.006	0.006	0.008	0.009
	(0.005)	(0.005)	(0.008)	(0.008)
2 Decades After	0.004	0.005	0.004	0.006
	(0.006)	(0.007)	(0.010)	(0.011)
$\geq$ 3 Decades After	0.004	0.005	0.003	0.006
	(0.009)	(0.010)	(0.015)	(0.015)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	57,108,103	57,108,103	13,676,699	13,676,699
R^2	0.020	0.030	0.056	0.076

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

Table 15: Rights and the Age of Married Men, 1850-1920

Dependent Variable	Age of Married Men			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.244	-0.056	0.021	-0.004
	(0.243)	(0.316)	(0.062)	(0.042)
2 Decades Before	-0.224	-0.057	0.040	-0.054*
	(0.206)	(0.134)	(0.055)	(0.029)
1 Decade Before	0	0	0	0
Rights Given	0.252*	0.236**	0.057	-0.007
	(0.144)	(0.108)	(0.036)	(0.025)
1 Decade After	0.600***	0.517***	0.009	-0.072*
	(0.213)	(0.184)	(0.035)	(0.037)
2 Decades After	0.442**	0.579**	0.100***	-0.041
	(0.214)	(0.295)	(0.038)	(0.037)
$\geq$ 3 Decades After	0.257	0.570*	0.046	-0.063
	(0.246)	(0.326)	(0.054)	(0.052)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	52,437,793	52,437,793	12,444,558	12,444,558
R^2	0.007	0.048	0.020	0.036

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

Table 16: Rights and the Age Gap, 1850-1920

Dependent Variable	Age Gap Between Spouses			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.132	-0.035	-0.126	-0.084
	(0.137)	(0.140)	(0.123)	(0.102)
2 Decades Before	0.011	0.045	0.049	0.038
	(0.054)	(0.055)	(0.064)	(0.051)
1 Decade Before	0	0	0	0
Rights Given	0.018	0.011	0.095	0.092
	(0.052)	(0.049)	(0.072)	(0.061)
1 Decade After	0.036	-0.000	0.205^{*}	0.152
	(0.100)	(0.096)	(0.113)	(0.103)
2 Decades After	0.037	-0.014	0.140	0.089
	(0.135)	(0.136)	(0.122)	(0.117)
$\geq$ 3 Decades After	0.021	-0.036	0.014	-0.003
	(0.166)	(0.169)	(0.165)	(0.156)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	52,437,793	52,437,793	12,444,558	12,444,558
R^2	0.093	0.096	0.061	0.067

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

Table 17: Probability of being Newly Wed, 1850-1920

Dependent Variable	Newly Wed			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	0.002	0.001	0.007	0.005
	(0.002)	(0.002)	(0.006)	(0.006)
2 Decades Before	0.002	0.002	0.004	0.003
	(0.002)	(0.002)	(0.006)	(0.006)
1 Decade Before	0	0	0	0
Rights Given	0.002**	0.002*	0.008	0.009
	(0.001)	(0.001)	(0.005)	(0.006)
1 Decade After	-0.002	-0.001	-0.004	0.001
	(0.002)	(0.002)	(0.005)	(0.006)
2 Decades After	0.001	0.002	0.007	0.008
	(0.002)	(0.002)	(0.007)	(0.007)
$\geq$ 3 Decades After	-0.002	-0.001	-0.004	0.000
	(0.002)	(0.002)	(0.009)	(0.009)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	31,437,126	31,437,126	7,513,119	7,513,119
R^2	0.064	0.068	0.048	0.060

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

Table 18: Rights and the Age of Newly Wed Men, 1850-1920

Dependent Variable	Age of Newly Wed Men			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.153	-0.244	0.361	0.615***
	(0.537)	(0.509)	(0.256)	(0.226)
2 Decades Before	0.284	0.226	0.036	0.103
	(0.626)	(0.559)	(0.304)	(0.318)
1 Decade Before	0	0	0	0
Rights Given	-0.305	-0.266	-0.263	0.162
	(0.699)	(0.673)	(0.292)	(0.293)
1 Decade After	-0.521	-0.151	-0.536**	-0.181
	(0.550)	(0.525)	(0.260)	(0.353)
2 Decades After	-0.233	0.105	-0.399	-0.022
	(0.861)	(0.597)	(0.289)	(0.333)
$\geq$ 3 Decades After	-0.103	0.043	-0.529*	-0.264
	(0.845)	(0.558)	(0.307)	(0.340)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	813,951	813,951	604,323	604,323
R^2	0.018	0.108	0.045	0.144

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

Table 19: Rights and the Age Gap between Newly Wed Couples, 1850-1920

Dependent Variable	Age Gap Between Newly Wed Couples			
	(1)	(2)	(3)	(4)
$\geq$ 3 Decades Before	-0.080	-0.220	-0.049	-0.191
	(0.279)	(0.290)	(0.305)	(0.278)
2 Decades Before	0.133	-0.129	0.034	-0.171
	(0.221)	(0.219)	(0.215)	(0.194)
1 Decade Before	0	0	0	0
Rights Given	-0.187	-0.644*	-0.425	-0.657**
	(0.312)	(0.344)	(0.381)	(0.331)
1 Decade After	-0.094	-0.428	-0.284	-0.516*
	(0.307)	(0.315)	(0.327)	(0.311)
2 Decades After	-0.179	-0.556	-0.364	-0.590
	(0.432)	(0.437)	(0.419)	(0.389)
$\geq$ 3 Decades After	-0.483	-0.673	-0.585	-0.734
	(0.523)	(0.519)	(0.497)	(0.469)
Controls	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	No	Yes
Sample	All	All	$\leq$ 30	$\leq$ 30
N	813,951	813,951	604,323	604,323
R^2	0.352	0.388	0.229	0.291

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

Table 20: Rights and the Age Gap, Married After Rights 1900-1910

Dependent Variable:	Age Gap Between Spouses					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A	Women Aged 40-60					
Married After Rights	0.017	0.018	-0.010	0.012	0.019	0.016
	(0.024)	(0.024)	(0.024)	(0.023)	(0.034)	(0.033)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	3,765,096	3,765,075	2,689,512	3,614,044	1,644,200	2,120,875
R^2	0.6954	0.6957	0.6866	0.6937	0.7118	0.6806
Panel B	Women Aged 20-39					
Married After Rights	0.231**	0.221**	0.152	0.242**	0.227**	0.195***
	(0.091)	(0.092)	(0.134)	(0.092)	(0.109)	(0.065)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Extra Controls	No	Yes	Yes	Yes	Yes	Yes
Sample	All	All	No South	No CP	1900	1910
N	7,458,884	7,458,865	5,212,311	6,931,508	3,312,279	4,146,586
R^2	0.6869	0.6886	0.6589	0.6868	0.7008	0.6774

Notes: $^{+}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered at the state level are in parentheses. All specifications include county-year fixed effects and state-year fixed effects. Control include husband's age fixed effects, interacted with year fixed effects, as well as duration of marriage fixed effects, interacted with year fixed effects. Extra controls include husband's occupation and husband's industry fixed effects, interacted with year fixed effects. Column 4 excludes all borders of Southern States with non-Southern States. Column 5 excludes all Community Property States and their bordering states. The sample includes white, non-Hispanic married women, who live in the same state in which they were born. Panel A restricts attention to women age 40-60, while Panel B restricts attention to women age 20-39.

Table 21: R^2 and Adjusted R^2: Regressing County level outcome on State Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	1850	1860	1870	1880	1900	1910	1920
Panel A: Birth Last Year							
N	1,492	1,774	2,094	2,412	2,756	2,947	3,063
R^2	0.1584	0.0877	0.0978	0.1108	0.2184	0.3560	0.3013
Adjusted R^2	0.1394	0.0693	0.0780	0.0939	0.2052	0.3456	0.2904
Panel B: # of Kids Under Age 5							
N	1,492	1,774	2,094	2,412	2,756	2,947	3,063
R^2	0.3386	0.2549	0.2363	0.4067	0.4551	0.5348	0.4501
Adjusted R^2	0.3236	0.2399	0.2196	0.3954	0.4458	0.5273	0.4415
Panel C: Probability of Being in School, Ages 8-17							
N	1,458	1,704	1,998	2,317	2,509	2,935	3,061
R^2	0.3788	0.3669	0.5718	0.4656	0.2512	0.4137	0.1838
Adjusted R^2	0.3648	0.3536	0.5628	0.4550	0.2369	0.4042	0.1711
Panel D: Probability of Being in School, Ages 8-13							
N	1,454	1,699	1,996	2,307	2,502	2,932	3,060
R^2	0.3975	0.3840	0.5800	0.4938	0.2929	0.4580	0.1957
Adjusted R^2	0.3840	0.3710	0.5712	0.4838	0.2794	0.4492	0.1831
Panel E: Probability of Being in School, Ages 14-17							
N	1,396	1,633	1,903	2,221	2,368	2,899	3,051
R^2	0.2974	0.2609	0.4939	0.3636	0.1270	0.2878	0.2323
Adjusted R^2	0.2814	0.2452	0.4833	0.3504	0.1097	0.2760	0.2202
Panel F: Labor Force Participation, Ages 20-39							
N	NA	1,774	2,094	2,412	2,756	2,947	3,063
R^2	NA	0.0307	0.0555	0.0498	0.0677	0.3089	0.2349
Adjusted R^2	NA	0.0112	0.0348	0.0318	0.0519	0.2977	0.2230
Panel G: Labor Force Participation, Ages 40-60							
N	NA	1,559	1,838	2,114	2,556	2,871	3,040
R^2	NA	0.0284	0.0316	0.0490	0.1434	0.1034	0.0807
Adjusted R^2	NA	0.0060	0.0095	0.0297	0.1277	0.0884	0.0662