

## Women's Liberation as a Financial Innovation

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### ABSTRACT

In one of the greatest extensions of property rights in human history, common law countries began giving rights to married women in the 1850s. Before this “women’s liberation,” the doctrine of coverture strongly incentivized parents of daughters to hold real estate, rather than financial assets such as money, stocks, or bonds. We exploit the staggered nature of coverture’s demise across U.S. states to show that women’s rights led to shifts in household portfolios, a positive shock to the supply of credit, and a reallocation of labor toward nonagriculture and capital-intensive industries. Investor protection thus deepened financial markets, aiding industrialization.

“It was now proposed that, for the first time in our history, the property of one-half of the married people of this country should receive the protection of the law. Up to this time the property of a wife had had no protection from the law . . .”

MP Russell Gurney, during the debate on the Married Women’s Property Act of 1870. (Hansard, British House of Commons, April 14th, 1870).

PROPERTY RIGHTS ARE AT THE heart of capitalism’s ability to efficiently allocate resources. In one of the greatest extensions of property rights in human history, common law countries began giving rights to married women in the second

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half of the 19<sup>th</sup> century. Before this “women’s liberation,” married women were subject to the laws of coverture.<sup>1</sup> Coverture had detailed regulations as to which spouse had ownership and control over various aspects of property, both before and after marriage, and strongly incentivized women to hold real estate, rather than financial assets such as money, stocks, or bonds. This paper explores the economic ramifications of coverture’s demise and the resulting expansion of investor protection to women. In particular, we exploit the staggered nature of coverture’s demise across the United States to show that these rights had a large impact on household portfolios, credit markets, and labor allocations.

Under coverture, property was divided into two types. Moveable property (also referred to as “personal property”), including money, stocks, bonds, furniture, and livestock, became the husband’s property entirely upon marriage. He could sell or give the property away, or even bequeath it to others. Real assets, such as land and structures, were placed under the husband’s partial control while remaining in the wife’s name. He could manage the assets as he saw fit, including any income generated by the assets, but he could not sell or bequeath the property without his wife’s consent.<sup>2</sup> After analyzing the laws of coverture, Holcombe (1983) concludes that “[w]hatever the reasons for the distinction between real and personal [moveable] property, the legal rules applying to these categories of property were substantially different. The common law afforded married women considerable protection with respect to real property. It afforded no protection for their personal property” (p. 20).

By differentially allocating property rights, coverture affected portfolio incentives not only for women, but also for parents wishing to bequeath or gift assets to their daughters. Consider a father who wanted to bequeath his estate to his daughter upon his death. He would face an incentive to hold his wealth in real assets. Indeed, parents did bequeath to daughters in the United States as primogeniture was abandoned after the War of Independence. The default became to divide inheritances of both types of assets equally among children, including girls (Shammas, Salmon, and Dahlin (1987), p. 67). Therefore, our first prediction is that undoing coverture should cause portfolios to shift toward moveable assets, such as financial assets, because removing legal constraints allows households to purchase assets with higher returns or diversify their portfolios.<sup>3</sup> Such a shift in portfolios toward moveable assets would yield an increase in the supply of financial assets. Accordingly, our second

<sup>1</sup> Coverture was an inherent aspect of British common law, and as such applied both in England and her colonies, including those that formed the United States, Canada, and Australia.

<sup>2</sup> 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 tables 1 and 2 of Basch (1982). We discuss further details of the laws of coverture, as well as the origin of the differentiation of the two types of property in Section I of the Internet Appendix. The Internet Appendix is available with the online version of this article on *The Journal of Finance* website.

<sup>3</sup> Baskerville (2008) studies the effects of women’s property rights in Canada and argues that, after rights were granted, “[i]f one were to take away the very rich and obviously powerful, then women’s activities and profiles in those areas [wealth holdings/portfolio choices] were often undistinguishable from those of most of their male counterparts” (Baskerville (2008), p. 237).

prediction is that after rights are granted, bank deposits—and loans—should increase, whereas interest rates should decrease. An increase in the supply of loanable funds would facilitate industrialization, as capital would be cheaper and thus entrepreneurs could invest more readily. Greater industrialization yields a sectoral reallocation of workers. Accordingly, our third prediction is that coverture's demise should lead to a shift in the labor force away from agriculture. Moreover, within the nonagricultural sector, cheaper capital is expected to boost investment in industries that are more capital-intensive. Thus, our fourth and final prediction is that rights should lead to a relative increase in employment in capital-intensive industries.

Using the staggered nature of coverture's demise across U.S. states, we conduct four sets of empirical exercises to test these four predictions. 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 19<sup>th</sup> 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.

Endogeneity of women's rights, in particular, omitted variable bias and reverse causality, is a natural concern. In Section III, we discuss these issues using the historical record of British parliamentary debate, the academic literature on women's rights, and empirical evidence from the United States. We argue that our exercises can be viewed as capturing the causal impact of granting women rights on economic outcomes, especially in light of the border level analyses described below.

Our first exercise uses population census data from 1860 and 1870, the only two census years for which data on portfolios by asset class are available. We show that households in states that granted rights during this decade increased their holdings of moveable property as a fraction of their portfolios relative to households in states that did not alter women's economic status. Combs (2005) uses a sample of British shopkeepers' wives to provide evidence on how property rights affected portfolio holdings in England. In contrast, we estimate our regressions that both the whole United States as well as a sample including state border pairs between states granting rights between 1860 and 1870 and their neighbors. This latter methodology is in the spirit of Holmes (1998) but uses a "difference-in-discontinuities" approach, as in Grembi, Nannicini, and Troiano (2016), which allows us to control for geographic variation in economic conditions. Our estimates suggest that the fraction of portfolios dedicated to moveables increased by 1.0 to 6.3 percentage points. In addition, the number of households holding any moveables increased by 1.3 to 7.4 percentage points after rights were given, whereas the number of households holding any real assets decreased by 1.7 to 6.9 percentage points.<sup>4</sup> We control for a variety of

<sup>4</sup> The shift from real to moveable assets may have had significant economic consequences beyond sectoral reallocation. Ferrie (2003) shows that higher holdings of moveable assets during this time period were associated with lower mortality rates, while there was no relationship between

state-level economic, demographic, and political controls to account for general trends in portfolios and political change.

In our second set of exercises, we use state-year-level data on interest rates (1878 to 1920) and national bank loans and deposits (1865 to 1920) to show that granting women economic rights is associated with lower interest rates and an increase in financial intermediation. These results are consistent with a positive shock to the supply of loanable funds. Specifically, our results suggest that as a result of women's property rights, interest rates dropped by 50 to 80 basis points, in real terms. As the average real interest rate in our sample is 8%, this represents a 6% to 10% reduction in the real interest rate. At the same time, bank deposits and loans increased. These results are robust to controlling for contemporaneous changes in financial regulations, such as usury laws (Holmes (1892)), bank reserve requirements, double liability for bank shareholders, and the existence of a state banking authority (Mitchener and Jaremski (2015)).<sup>5</sup>

In our third set of exercises, we use U.S. population census data (1850 to 1920) to examine the fraction of male employment in the nonagricultural sector, a measure of industrialization. Before rights are granted, there is no trend in industrialization. After rights are given, we find a statistically significant increase in the fraction of the labor force working in the nonagricultural sector. Among our controls are state-level regulations permitting limited liability corporations (LLCs) (Hamill (1999)). Our estimates suggest women rights led to a sectoral reallocation of 3.8 to 8.9 percentage points of the male labor force a decade after their implementation. We compare labor allocations between state border pairs and find both qualitatively and quantitatively similar results.

In our fourth and last set of exercises, we use the 1850 U.S. Census of Manufactures (Atack and Bateman (1999)) to rank industries according to capital intensity. We take the top (bottom) quartile of industries, as ranked by their capital-to-labor ratio, to be the most (least) capital-intensive. We then calculate the ratio of employment in the most versus least capital-intensive industries by state-year (1850 to 1920). We find that this ratio exhibits no trend prior to the implementation of women's rights, while it dynamically increases after rights are granted. The result comes from a roughly 40% increase in employment in the most capital-intensive industries a decade after rights are granted.

Our theory depends on financial markets being important for industrialization during the 19<sup>th</sup> century. This was indeed the case. For instance, prior literature documents the importance of finance for New England textile firms (Davis (1960)). Another example comes from the railroad industry. Investment in railroads between 1850 and 1880 in the United States was approximately \$8 billion (in 1909 dollars), or 70% of 1880 GDP. Moreover, railroad firms were substantially leveraged, with an average leverage rate of about 40% (Benmelech (2009)). Further emphasizing the industry's reliance on banking, Frydman and

mortality and real asset holdings. He suggests that the mechanism behind this correlation is the greater ability of moveable assets to smooth consumption.

<sup>5</sup> Usury laws on a state-year basis are available from Holmes (1892) until 1891. We extend this time series to 1920, as detailed in Section V of the Internet Appendix.

Hilt (2017) exploit a legislative change that prohibited bankers from serving on the boards of railroads for which they underwrote securities, and find that this change reduced bankers' ability to monitor railroads, increasing the railroads' cost of external funds and decreasing railroad investment. Using more standard macromeasures of finance depth (King and Levine (1993)), we find that deposits in national banks (see Section II) alone were approximately 10% of GDP by 1890, with this figure rising to 15% by 1910. In comparison, deposits across all depository institutions were approximately 17% of GDP in 2000.<sup>6</sup> Finally, Bodenhorn (2003) provides a summary of banking at the state level in the United States during this time period, and the central role that banks played in the boom-bust cycle of the 19<sup>th</sup> century.

Our paper is related to the literature that documents the effects of different legal systems on investor protection, the effects of these legal systems on the size of the financial sector, and the effect of the size of the financial sector on development. La Porta et al. (1998) examine legal rules and their enforcement across countries, and find large variation across types of legal systems, such as between civil and common laws. La Porta et al. (1997) use this variation in investor protection to show that weaker investor protection is associated with smaller and narrower capital markets. Demirgüç-Kunt and Levine (2005) further study the relation between legal origin and the obstacles that firms face in obtaining external finance. More recently, Giannetti (2003), Brown, Martinsson, and Petersen (2013), and Ponticelli and Alencar (2016) show that investor protection leads to financial market deepening. We view *coverture* as a class of laws that provided little protection to female investors, and we argue that *coverture* led to underinvestment in capital on the part of women. King and Levine (1993), Levine (1997), Acemoglu and Zilibotti (1997), and Rajan and Zingales (1998) show that more financial intermediation positively affects development. We similarly find that the financial market deepening that resulted from *coverture's* demise facilitated industrialization. The mechanism is similar to that in Acemoglu, Johnson, and Robinson (2001), who argue that expropriation risk from governments reduces per capita income as property rights are poorly enforced: *Coverture* led to a large risk of expropriation of a wife's assets by her husband, yielding worse economic outcomes.

The paper proceeds as follows. Section I develops our theoretical predictions for the economic effects of *coverture's* demise. Section II describes our data. Section III discusses the empirical strategy in this paper, including the implicit assumptions behind our exercises and the econometric concerns of omitted variable bias and reverse causality. Section IV presents our regression results, including a variety of robustness exercises and randomization checks. We conclude in Section V. Further discussion of property rights, details on the analysis, and robustness tests are provided in the Internet Appendix.

<sup>6</sup> Author's calculation, based on the variable Total Savings Deposits at all Depository Institutions [WSAVNS], from the Board of Governors of the Federal Reserve System (United States).

## I. Theoretical Framework and Testable Hypotheses

In this section, we describe our hypotheses relating women's liberation to financial markets and development. Our overall premise is that coverture distorted portfolio allocations of single women, or the parents of such women, away from moveable assets. Accordingly, the demise of coverture should lead to a reallocation of portfolios toward moveable assets, as people pursued higher returns and greater diversification.

**PREDICTION 1:** *Women's rights lead to portfolio reallocations toward moveable assets.*

Moveable assets include stocks, bonds, and money in banks, rather than real estate. Accordingly, we expect the predicted portfolio reallocation toward moveable assets to increase deposits in banks, increase loans from banks, and reduce interest rates, which represents a positive supply shock.

**PREDICTION 2:** *Women's rights lead to an increase in bank deposits, an increase in bank loans, and a reduction in bank interest rates.*

We expect greater financial intermediation and lower capital costs to boost growth in capital-intensive parts of the economy during industrialization. Assuming that agriculture is less capital-intensive than the rest of the economy, we thus expect labor to shift away from agriculture.

**PREDICTION 3:** *Women's rights lead to an increase in the fraction of the labor force in nonagricultural employment.*

Finally, we expect to see lower capital costs drive a reallocation of labor within the nonagricultural sector. Specifically, we expect to see relatively greater employment in capital-intensive industries as compared to industries with low capital intensity.

**PREDICTION 4:** *Women's rights lead to an increase in the fraction of the labor force in industries with high capital intensity relative to low capital intensity.*

The rest of the paper is dedicated to testing these four predictions.

## II. Data Sources and Sample Selection

In this section, we first discuss issues related to the coding of the timing of women's rights, as well as the geographic and temporal dispersion of these rights in the United States. These rights comprise our main explanatory variable of interest in our empirical exercises. We then discuss the data sources for the dependent variables in our empirical exercises.

### A. Women's Property Rights in the United States

Married women were not given economic rights in the United States overnight; rather, different sets of rights were granted in successive waves.<sup>7</sup> Property laws were passed by state legislatures, generally narrowly interpreted by courts (Chused (1983), Zeigler (1996)), and updated again.<sup>8</sup>

We use the data on the timing of women's liberation by state from Geddes and Lueck (2002).<sup>9</sup> 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).<sup>10</sup> These statutes were certainly enough to undo the distortion of interest.

Three 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 just property rights? And finally, the third question is: How should we evaluate states that had community property laws, as in civil law, rather than formal coverture? We address all three of these questions in detail in the Internet Appendix Section III. Below is a summary of our responses to these questions.

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,

<sup>7</sup> Similarly, rights were granted in waves in England. Married women received partial rights over property in 1870, specifically with regard to certain types of savings/investment accounts and inheritances up to 200 pounds, though the reform was not always upheld in court. The 1870 law was updated in 1874 to prevent fraud. A more significant update to property rights came in 1882, which more or less granted women the same economic rights as men. Further minor updates occurred over the 20<sup>th</sup> century (Holcombe (1983), pp. 178–205).

<sup>8</sup> States almost never retracted rights once they were granted, presumably because 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 (1870)).

<sup>9</sup> We thank the authors for making their data available to us.

<sup>10</sup> Other papers in the literature, discussed in the Internet Appendix, have other purposes in their timing of rights. We note that Geddes and Tennyson (2013) report dates that are quite similar to Geddes and Lueck (2002), and is useful as a secondary source in the Internet Appendix.

and generally confirmed rather than confronted prevailing domestic roles of married women.<sup>11</sup> 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.<sup>12</sup> Accordingly, these statutes did not undo the distortion in incentives to hold different types of assets, which is the subject of interest in this study.

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 undid the distortion to invest in real estate rather than moveable assets, which is presumably the property rights date rather than the earnings rights date. There are two reasons that "both" is more appropriate as a benchmark, however. The first is that there are strong complementarities between the two types of rights. *Apple v. Ganong* 47 Miss. 189 (1872) illustrates the confusion created in credit markets when some of a woman's assets (her labor income) belong to her husband while some of her assets (gifts from parents) belong to her. If she uses money to buy land, who does that land belong to, her or her husband? Could her husband's creditors claim her land? The court ruled that the husband's creditors could claim the amount of land up to the proportion that the wife's income was used to purchase the land.<sup>13</sup> We discuss this case in more detail with some related literature in the Internet Appendix.

The second reason that "both" dates may be more appropriate is that, given the legal issues that arose around granting rights, governments often needed more than one round of legislation to effectively grant property rights that undid investment distortions. These issues were often solved when updating other rights, especially 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. As

<sup>11</sup> 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.

<sup>12</sup> How is it possible for a woman to have separate moveable assets if common law allows the husband to take them upon marriage? For a husband to own his wife's moveable assets, he had to "reduce them to possession," or actively take control of his wife's property. If he did not do so, they remained her assets and, after the debt statutes were passed, were immune from his creditors. The exact definition of what constituted reduction to possession varied state by state and over time, and had implications for the ability of a husband's creditors to seize the assets. For one example of this in Ohio, see the discussion on pages 114–115 of Chused and Williams (2016). Before these debt statutes, a wife's separate moveable property was liable for a husband's debt even if he had not reduced these assets to possession. See Justice Wright's discussion in *Dickerman v. Abrahams* 21 Barb. 551 (1854), Supreme Court of New York.

<sup>13</sup> As a robustness test in the Internet Appendix, we go through each of the laws of state and select dates when women were granted equal rights over both types of assets, *ignoring* the interaction between earnings rights and property rights, and rerun our main exercises. The results generally hold.

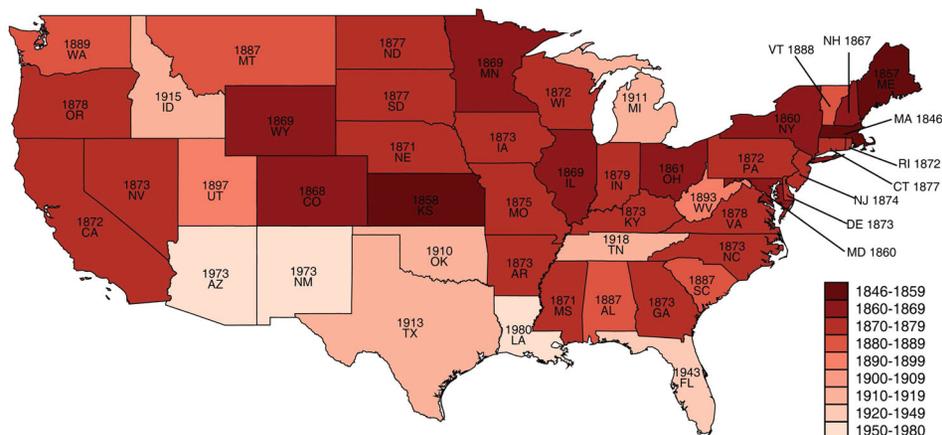
we detail in the Internet Appendix, Justice Wright explains that the New York legislature made a series of mistakes when passing the law, for instance, the law was interpreted only as providing married women with rights over real estate, which preserved the distortion. Rights over moveable assets were 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.

Finally, we turn to the issue of community property. 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.<sup>14</sup> 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 any gender-based property rights discrimination after marriage, much less a distortion in incentives to invest in real versus moveable assets. However, as we detail in the Internet Appendix, it is not clear that these laws were understood or enforced. First, we provide evidence of mass confusion among the numerous immigrants in these (mostly) Western states, many of whom believed that common law was in effect. Not only did the immigrants not know about the community property rules, but many lawyers and lawmakers were also confused. Second, even per civil law, the husband had full control over *all* household assets, even his wife's separate property. Thus, while he was supposed to manage her separate property for her benefit, it was a difficult intention to enforce. Under the assumption that her separate land was harder for him to abuse than her separate bank account (or other moveable property), the distortion we study was in effect. Because the basic hypothesis of this study seems to stand in community property states, we include these states in our benchmark exercises. However, in robustness tests we drop community property states and confirm that these states are not driving the results of our empirical work.

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 19<sup>th</sup> Amendment (passed in 1920) granted women

<sup>14</sup> "Civil law" refers to law coming from either French (Louisiana) or Spanish legal traditions.



**Figure 1. Timing of women's rights by state.** (Color figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com))

the right to vote, which may well have affected de facto implementation of laws. Finally, approximately one-third of states gave rights between 1870 and 1880. We perform robustness exercises in which we drop these states from our sample.

### B. Data on Outcome Variables

We perform four sets of empirical exercises. The first provides evidence on the impact of women's rights on portfolio allocations at the household level. The second exercise documents the impact of women's rights on state-level credit markets, consistent with changes in household portfolio reallocations. Our third exercise studies the dynamic effect of these financial market changes on labor allocations, by showing how women's rights affected the fraction of male employment in agriculture versus nonagriculture. Our final exercise expands on labor allocations by focusing on nonagricultural employment showing how rights led to a relative increase in employment in high capital intensity industries versus low capital intensity industries. In what follows, we describe how we build the data for each of these exercises. Details on the various other control variables used in our empirical analysis are described in the Appendix.

#### B.1. Household Portfolios

Our analysis of how rights affected portfolio choices employs data from the *U.S. Population Census*. The census data that we use refer to "moveable" property as "personal" property. In 1860 and 1870, and only these two years, the census asked about holdings of real assets and moveable assets at the

household level.<sup>15</sup> We deflate these nominal values to make them real, in 1870 dollars, using the deflator from Burgess (1920).

The value of real assets was to be assessed “without any deduction on account of mortgage or other incumbrance, whether within or without the census subdivision or the country. The value meant is the full market value, known or estimated” (Ruggles et al. (2010)). Moveable or “personal” property included the “contemporary dollar value of all stocks, bonds, mortgages, notes, livestock, plate, jewels, and furniture” in 1870, and included the value of slaves in 1860.<sup>16</sup> We restrict our sample to married households that have nonzero total wealth (real property plus moveable property). Slaves were considered moveable property, except in Louisiana. We accordingly drop the South from this sample due to the effect of the emancipation of slaves on portfolio allocations, unless otherwise specified.

### B.2. Credit Markets

We show how women's rights affect interest rates, bank deposits, and loans at the state-year level.

Interest rate data from 1878 to 1920 are from Bodenhorn (1995). These interest rates are widely used and have been developed over the years through a series of important works. As explained in the Appendix of Landon-Lane and Rockoff (2007, p. 11), “Bodenhorn (1995) followed Smiley (1975) and James (1976a,b), and purged the data originally compiled by Davis (1965) of various revenues and losses in order to arrive at something closer to contractual loan rates. Davis had attributed all bank earnings to loans, and divided that figure by total loans to get a proxy for the rate of interest. Smiley and James removed earnings on bonds and other nonloan earnings from the numerator and various nonloan assets from the denominator. Bodenhorn (1995) extended these estimates to 1960.”

For our analysis of how rights affected bank deposits and loans, we use the *Annual Report of the Comptroller of the Currency 1920*, which contains state-year-level data on loans and deposits in national banks from 1865 to 1920.<sup>17</sup>

<sup>15</sup> Technically, the census takers were instructed to ask the head of each household about the holdings of each individual in the household. However, to the best of our knowledge, all of the literature using these data look at household-level data as most heads of households simply reported all assets as belonging to them. Indeed, Rosenbloom and Stutes (2008) argues that “[m]any of these individuals were part of larger households, whose assets were likely to be reported as belonging to the head of the household” (p. 148). Koudijs and Salisbury (2016), who study the effects of protecting married women's assets from creditors, also use these data at the household level rather than breaking down the assets between husbands and wives.

<sup>16</sup> Moveable property of value less than \$100 was not recorded in 1870. Accordingly, for consistency, we recode any observations of less than \$100 in 1860, in real terms, to be \$0.

<sup>17</sup> As discussed in Benmelech and Moskowitz (2010), while we would ideally prefer to use data on both national and state banks, it does not seem that data on state banks exist. The Comptroller of the Currency only supervised national banks. It seems reasonable to assume, however, that loan and deposit data at national and state banks would be highly correlated. See footnote 11 in Benmelech and Moskowitz (2010) for more on this point.

We calculate the change in real bank deposits per capita and real bank loans per capita, by state-year, following Benmelech and Moskowitz (2010). We refer to these variables as “deposits” and “loans,” respectively. We calculate real interest rates, loans, and deposits using a price deflator from Burgess (1920).

### *B.3. Labor Allocations*

We use data from the decennial *U.S. Population Census* (Ruggles et al. (2010)) from 1850 to 1920 to calculate the labor force allocation between the agricultural and nonagricultural sectors. We calculate the fraction of men in the labor force, aged 20 to 60, who are in the nonagricultural sector by state-year as defined by the IND1950 variable. For our baseline exercise, we consider the agricultural sector to be IND1950 taking the value of 105. We classify all other IND1950 values as corresponding to the nonagricultural sector.

In a set of robustness tests, we exclude from nonagricultural employment a broader set of people. First, one could argue that a broader definition of agriculture, such as forestry and fishing, could perhaps be unaffected by distorted portfolio incentives, because women could own land. Additionally, because women could own structures, perhaps “mom and pop” shops would not be affected. Accordingly, we excluded from nonagricultural employment those employed in Forestry (code 116), Fisheries (code 126), and a list of industries classified as “Retail Trade” such as food stores and shoe stores (codes 636–699). Finally, we also exclude personal services (e.g., dressmaking shops) and professional services (e.g., hospitals) (codes 826) through the end of the index.

### *B.4. Capital Intensity*

To calculate the relative employment of workers in capital-intensive industries, we need to measure capital intensity. To do so, we turn to the *1850 Census of Manufactures* (Atack and Bateman (1999)). This census was performed at the beginning of our sample period and thus is not influenced by women’s rights. This census surveyed 5,550 firms and included data such as labor and capital use, as well as three-digit Standard Industrial Classification (SIC) industry codes.

We collapse the data into 23 unique two-digit SIC code industries.<sup>18</sup> For each industry, we calculate capital per worker as the total amount of capital across the firms divided by the total labor input. We then keep those industries in which there are at least 30 firms, which leaves 16 industries. We take the top and bottom quarter of these industries to be high and low capital intensity industries, respectively. See the Appendix for the list of the most and least capital-intensive industries, as well as our crosswalk between the two-digit SIC codes and IND1950 codes.

<sup>18</sup> These are the 23 industries left after excluding firms in which industry affiliation is not known. We also drop code 17, miscellaneous repair shops, which we cannot map into population census categories.

### III. Empirical Strategy

In this section, we discuss the validity of using cross-state variation in the timing of women's rights in the United States to provide empirical evidence on the theoretical predictions described in Section I. We first discuss the implicit assumption behind this exercise, particularly with respect to the openness of capital markets, and support this assumption with data and literature on U.S. regional development and capital markets during our sample period. We then discuss why women were granted property rights along with the implications for potential omitted variable bias and reverse causality.

Our theory depends on women's rights at the state level impacting the capital market of that state. This is only true under the assumption of imperfect capital market integration across states, which is consistent with the realities of the time. It is well known that banking was highly regulated at the state level during the 19<sup>th</sup> century.<sup>19</sup> Empirically, there was significant geographic dispersion in interest rates. For instance, Breckenridge (1898) documents regional dispersion in interest rates of "first class double-name commercial paper" (high-quality corporate bonds) in the 1890s. Breckenridge argues that the comparison of these commercial papers across cities takes into account default risk, and thus compares discount rates across the United States. We present a snapshot of his findings in Figure IA.18 in the Internet Appendix. The figure shows that interest rates varied from about 4% in Boston to more than 9% in Denver. Similarly, using the interest rates described in Section II.B.2, Figure IA.19 in the Internet Appendix displays the evolution of interest rates over time by region. The figure highlights two salient features of the data. First, there is large cross-regional variation in interest rates, supporting our treatment of states as having imperfectly integrated capital markets. Second, there are clear differences in the time trend across regions.<sup>20</sup>

Accordingly, in all of our exercises using state-level observations, we include specifications with region-year fixed effects, which additionally help control for differential regional effects of the U.S. Civil War. Although we do not take a particular stand on the issue, a large literature addresses the source of this regional variation in interest rates and the question of why capital did not flow to correct imbalances.<sup>21</sup> Considering these realities, we conduct our analysis under the assumption of imperfect capital market integration.<sup>22</sup>

We now turn to the endogeneity of women's rights. Our concerns are related to omitted variable bias and reverse causality. If rights were granted due to some other economic force, and that force also influenced portfolio allocations,

<sup>19</sup> For a thorough history of U.S. banking, see Calomiris (2000).

<sup>20</sup> In addition to financial dispersion, it has been noted that there was regional price variation in the United States during the 19<sup>th</sup> century (Coelho and Shepherd (1974), Haines (1989)). The available regional price indices do not cover our entire sample period.

<sup>21</sup> For a summary of and contribution to this literature, see Landon-Lane and Rockoff (2007).

<sup>22</sup> Benmelech, Bergman, and Seru (2011) show that regions in the United States still depend on local finance. For instance, they show that a financial shock in Japan in the 1990s affected U.S. labor market conditions in regions where Japanese banks operated.

financial market deepening, and sectoral reallocation of workers, then the empirical exercises we perform may capture a spurious correlation rather than the causal effects of rights. Alternatively, if households began allocating more assets toward moveable property, which in turn caused the other economic changes we document, then they may have lobbied for women to be given property rights in order to keep the assets in their families. We address each of these concerns in turn.

What if men granted women rights to undo the financial market distortions that we document? Under this scenario, we are estimating the effects of giving rights that men were hoping to induce, and thus our empirical strategy is still valid. However, there are still potentially omitted variables that are linked both to women's rights and financial markets and industrialization. For instance, total factor productivity (TFP) in the nonagricultural sector of the economy may increase growth in the demand for capital that outstrips the supply of capital under coverture, when women (or their parents) had a disincentive to purchase anything but real assets. Eventually, this distortion would be so large that men might give women rights to induce portfolio reallocations and financial market deepening, and thus a sectoral reallocation of labor toward the more productive areas of the economy. To alleviate this concern, we control for TFP in the nonagricultural sector relative to the agricultural sector when studying portfolio allocations and labor allocations.<sup>23</sup> We do not include this variable when looking at credit markets, as we only have TFP data for decennial census years, while our data on credit markets are annual. TFP growth would presumably result in more credit and, if anything, *higher* interest rates, consistent with a demand shock.<sup>24</sup> However, our results suggest that women's rights lead to lower interest rates and more credit, consistent with an increase in the supply of capital.

We turn next to the existing literature on why legislatures—all comprised and controlled by men—gave women economic 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. 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. Separately, Geddes and Lueck (2002) argue that coverture decreased women's incentive to work, as their earnings went to their husbands.<sup>25</sup> Note that none of these papers predict observed

<sup>23</sup> Turner et al. (2007, 2011), and Turner, Tamura, and Mulholland (2013) develop state-level time-series data that allow us to compute TFP for the agricultural and nonagricultural sectors. We thank the authors for making their data available to us.

<sup>24</sup> Not including TFP in these regressions suggests that our results are biased toward zero and thus that we are underestimating the effects of rights on interest rates.

<sup>25</sup> This may have been a significant mechanism in England, where married women's labor force participation was high at this time. However, in the United States, the participation rate was under 5%, making this mechanism less relevant to our analysis.

portfolio shifts, reductions in interest rates, increases in financial intermediation, or a sectoral reallocation of labor as a result of rights.<sup>26</sup> Therefore, if any of these mechanisms were the driving force behind women being granted rights, then the empirical exercises we explore can treat women's rights as exogenous. Additionally, as we describe above, we control for TFP in our exercises, which is the underlying force driving women's rights in Geddes and Lueck (2002), Doepke and Tertilt (2009), and Fernández (2014), alleviating omitted variable bias concerns.

To further control for omitted variable bias, we take three additional sets of precautions. First, in the Internet Appendix, we check a variety of economic, demographic, and political variables for correlations with women's rights. We take the relevant state-level variables as controls in our exercises to account for omitted variable bias. Second, we include controls for five other sets of legal changes related to financial markets and industrialization. If these laws were passed around the time of women's rights, then failure to include them among our controls may yield a spurious correlation between rights and our economic and financial results. Third, when possible, we include exercises comparing regions on either side of state borders, such as the areas of Ohio and Pennsylvania that border one another. To the extent that regional economic conditions transcend state borders, our exercises capture changes induced by the quasi-natural experiment of women's property rights.

Finally, we turn to the issue of reverse causality. If households began allocating more assets toward moveable property, which in turn caused the other economic changes we document, then they may have lobbied for women to be given property rights in order to keep the assets in their families. We address this concern in two ways. First, we rely on qualitative evidence. Specifically, 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. None of the Parliamentary discussion mentions this mechanism as a factor. Second, we compare the asset allocations of households in the United States in states that granted rights to those that did not. Table I does so for households in states that granted rights between 1860 and 1870 to those that did not and shows that real asset holdings were very similar between these groups. However, the

<sup>26</sup> Doepke and Tertilt (2009) predict an increase in human capital as a result of women's rights. This is presumably consistent with sectoral reallocation, as nonagricultural employment demands more skills. This would be a different mechanism than the one studied in this paper connecting rights with industrialization, albeit with a lag, though not to portfolios or credit markets. Indeed, Geddes, Lueck, and Tennyson (2012) find that the legal changes had an effect on women's schooling (relative to men's). To control for this alternative mechanism, we include the fractions of females in school and males in school, by state-year, in our industrialization regressions.

**Table I**  
**Mean and Number of Observations (in Parentheses): Portfolio and Rights**

This table includes data on household portfolios from the U.S. census in 1860 and 1870. “Switching States” are the states that granted rights between 1860 and 1870. “Border Sample” restricts attention to “Switching States” and their immediate geographic neighbors. We drop the South, as slaves were generally classified as moveable property, and thus household portfolios were influenced by the Emancipation Proclamation.

Variable	Switching States		Other States	
	1860	1870	1860	1870
Panel A: Full Sample				
<i>Fraction Moveable</i>	0.421 (6,022)	0.419 (8,095)	0.459 (19,272)	0.447 (24,841)
<i>Moveable Property (1870 Dollars)</i>	1,086.51 (6,022)	1,255.23 (8,095)	1,435.40 (19,272)	1,502.41 (24,841)
<i>Real Property (1870 Dollars)</i>	3,476.93 (6,022)	3,519.38 (8,095)	3,204.44 (19,272)	3,307.79 (24,841)
<i>Fraction Households</i>	0.930	0.932	0.932	0.924
<i>Moveable Property &gt;0</i>	(6,022)	(8,095)	(19,272)	(24,841)
<i>Fraction Households</i>	0.751	0.743	0.715	0.726
<i>Real Property &gt;0</i>	(6,022)	(8,095)	(19,272)	(24,841)
Panel B: Border Sample				
<i>Fraction Moveable</i>	0.421 (6,022)	0.419 (8,067)	0.455 (13,858)	0.448 (18,291)
<i>Moveable Property (1870 Dollars)</i>	1,086.51 (6,022)	1,255.66 (8,067)	1,406.31 (13,858)	1,291.76 (18,291)
<i>Real Property (1870 Dollars)</i>	3,476.93 (6,022)	3,524.98 (8,067)	3,043.22 (13,858)	2,823.16 (18,291)
<i>Fraction Households</i>	0.930	0.932	0.940	0.934
<i>Moveable Property &gt; 0</i>	(6,022)	(8,067)	(13,858)	(18,291)
<i>Fraction Households</i>	0.751	0.743	0.733	0.733
<i>Real Property &gt; 0</i>	(6,022)	(8,067)	(13,858)	(18,291)

states that granted rights had significantly lower moveable assets, negating the reverse causality hypothesis.<sup>27</sup>

We conclude that our exercises capture the causal effect of women’s rights on portfolios, credit markets, and labor market allocations.

#### IV. Results

In this section, we empirically test the theoretical predictions described in Section I, taking into account the issues raised in Section III. We first show that women’s rights had a large effect on household portfolios, as in Prediction 1.

<sup>27</sup> The fact that the households in the switching states saw a large relative rise in moveable property between 1860 and 1870 is consistent both with the hypothesis of this paper and the omitted variable bias story told above, reinforcing our need to control for TFP.

Next, we show that rights led to financial market deepening, as in Prediction 2. Third, we show that rights led to a dynamic reallocation of labor toward the nonagricultural sector, as in Prediction 3. Finally, we show that within the nonagricultural sector, rights led to a reallocation of labor toward capital-intensive industries, as in Prediction 4.

#### *A. Rights Leads to Portfolio Changes*

In our first exercise, we test Prediction 1. The question here is, how did protecting women's property rights affect household portfolios? We use a difference-in-differences approach to estimate the impact of women's rights on portfolios between 1860 and 1870, as these are the only two years in which the census asked about both household wealth in general and the breakdown between real and moveable assets in particular. We study the impact of rights on the fraction of a household's wealth that was moveable. We find that the granting of rights did indeed increase the fraction of household wealth in moveable property. We further show that this result holds on the extensive margin: more households chose to hold nonzero values of moveable wealth after rights were given, and more households chose to hold no real wealth. We repeat these two analyses using data from state border pairs, in the spirit of Holmes (1998), which alleviates concerns that our results are driven by regional economic conditions.

Between 1860 and 1870 six states gave rights to married women: Colorado (1868), Illinois (1869), Minnesota (1869), New Hampshire (1867), Ohio (1861), and Wyoming (1869). Wyoming is not used in this exercise, as its population was only 9,000 in 1870. Indeed, our sample has only 15 observations for Wyoming in 1870, and none in 1860. These states comprised 24% of our sample in 1860 and 25% in 1870. We do not include the South to abstract from the effects of the Civil War and the emancipation of slaves (considered moveable property, except in Louisiana) on portfolios.

Table IA.II in the Internet Appendix repeats summary statistics on the fraction of people married, their age at marriage, and the age gap between newly married husbands and wives. Table I reports summary statistics on moveable assets, real assets, the fraction of a household's wealth that comprises moveable assets, and the fraction of households with at least some moveable property. Note that over 90% of adults were married at this time. Accordingly, we restrict our attention to married households, which form the vast majority of the sample to avoid getting mired in the differences between single and married households.

Because we only examine married households, we begin by documenting that the adoption of women's rights did not affect selection into marriage by using white male heads of households (men at least 15 years old). Specifically, we apply a difference-in-difference estimator to examine the effects of rights on several marriage market outcomes, such as the probability of being married, as captured by the dummy variable *Married*, *Newlywed* indicator, the age of newly married people, denoted *Age*, and the age gap between newly married

spouses, denoted Age Gap. Accordingly, we run regressions of the following form:

$$Y_{ist} = \alpha \cdot Switch_s + \beta \cdot Post + \gamma \cdot (Switch_s \times Post) + T_{st} + C_{hs} + X_{ist} \delta + \epsilon_{ist},$$

where  $Y_{ist}$  is Married, being Newlywed, Age, or Age Gap for individual  $i$  in state  $s$  and year  $t$ ,  $Switch_s$  is a dummy variable equal to 1 if state  $s$  gave women rights between 1860 and 1870,  $Post$  is a dummy variable equal to 1 in 1870,  $T_{st}$  is a dummy variable indicating whether the state the individual lived in was a territory in year  $t$ ,  $C_{hs}$  are fixed effects for the county in state  $s$  in which household  $h$  resides, which allows us to control for local economic conditions, and  $X_{ist}$  is a vector of age-year fixed effects.<sup>28</sup> We use census person weights and cluster standard errors by state-year. As our exercise only uses data from 1860 and 1870, we use the state borders in 1860 to cluster our standard errors.<sup>29</sup> Our parameter of interest is  $\gamma$ , which captures the effect of granting rights, after netting out general changes between 1860 and 1870.

Table II shows the results. All specifications include county fixed effects and the dummy variable indicating whether a state was a territory. Column (1) shows the effect of rights on the likelihood of an individual being married, and includes age-year fixed effects. Column (2) repeats column (1) on a sample of household heads under age 30. These two columns show that rights had no impact on the likelihood of an individual being married, even when we limit attention to the age group relevant for the marriage market. Column (3) shows the effect of rights on the propensity to be newly married (married within the 12 months preceding June 1 of the census year), and includes age-year fixed effects. Column (4) repeats column (3) for the sample of men under 30. These columns show that rights did not influence the flow into marriage. Column (5) shows the effect of rights on the age of newlywed men. Column (6) repeats column (5) for the sample of newlywed men under 30. These two columns suggest that rights did not affect the age at which people got married. Column (7) shows the effects of rights on the age gap between a newly married husband and wife, whereas column (8) repeats column (7) for the sample of men under 30. Column (7) finds no significant impact of rights on the age gap, while column (8) finds a statistically significant impact of about one year, that is, the age of women married by men under 30 tends to be one year higher when

<sup>28</sup> The coefficient on  $Switch_s$  is not separately identified from county fixed effects, as these in turn define state fixed effects. We leave it anyway for exposition. The same comment can be made regarding equations (1) and (2) below.

<sup>29</sup> Areas that were not yet states but were divided into more than one territory were assigned to a common territory based on where the greater part of the landmass was. For instance, Wyoming is included in our fixed effect for the Nebraska Territory, even though parts of modern-day Wyoming are located in what was Washington Territory or Utah Territory, as the Nebraska Territory was where most of its landmass was located. Colorado was reasonably equally divided between the Kansas, Nebraska, Utah, and New Mexico Territories, so we included it with Kansas Territory as that is where Denver is located. We also give separate state fixed effects to Virginia and West Virginia, which were soon to separate due to the Civil War, although our results are not sensitive to this choice.

**Table II**  
**Balancing—Rights Do Not Affect Marriage**

Standard errors are clustered at the state-year level in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ . All specifications include county fixed effects and a dummy for territory. Columns (1) to (4) and (7) to (8) include age fixed effects interacted with the 1870 fixed effect. The sample “All” uses all white male heads of household at least 15 years of age. The sample “ $\leq 30$ ” restricts the sample to ages 15 to 30. Columns (5) to (8) restrict samples to households whose members married in the 12 months prior to the census. *Switch* is a dummy variable equal to 1 if the state granted rights for the first time between 1860 and 1870, namely, Colorado, Illinois, Minnesota, New Hampshire, Ohio, and Wyoming.

Dependent Variable:	Married		Newlywed		Age of Newlyweds		Age Gap of Newlyweds	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Switch × Post	-0.006 (0.004)	-0.004 (0.007)	-0.001 (0.002)	0.003 (0.007)	0.415 (0.892)	-0.241 (0.403)	-0.835 (0.617)	-1.028* (0.517)
Sample	All	$\leq 30$	All	$\leq 30$	All	$\leq 30$	All	$\leq 30$
Obs.	79,531	18,621	79,531	18,621	1,270	990	1,240	979
$R^2$	0.152	0.291	0.077	0.101	0.532	0.489	0.747	0.655

compared to the women they married before rights were granted. This result becomes insignificant, however, when we drop two couples with extreme values for the age gap.<sup>30</sup> We conclude that women’s rights did not have a meaningful impact on selection into marriage, which validates our restriction to married households only.

Turning to the impact of rights on portfolios, we apply a difference-in-difference estimator to compare the portfolios of households in states that granted rights between 1860 and 1870 to the portfolios of households in states that did not switch legal regimes during this decade. To estimate the impact of rights, we run regressions of the following form:

$$Y_{hst} = \alpha \cdot Switch_s + \beta \cdot Post + \gamma \cdot (Switch_s \times Post) + C_{hs} + Z'_{st}\omega + X'_{hst}\delta + \epsilon_{hst}, \tag{1}$$

where  $Y_{hst}$  is the fraction of a household’s portfolio in moveable assets, a dummy indicating whether the household had any moveable assets, or a dummy indicating whether the household had any real assets for household  $h$  in state  $s$  and year  $t$ ,  $Switch_s$  is a dummy variable equal to 1 if state  $s$  gave women rights between 1860 and 1870,  $Post$  is a dummy variable equal to 1 in 1870,  $C_{hs}$  are fixed effects for the county in state  $s$  in which household  $h$  resides, which allows us to control for local economic conditions,  $Z_{st}$  is a vector of controls for state  $s$  in year  $t$  that includes TFP in the nonagricultural sector relative to

<sup>30</sup> In these two cases, the wife was nine years older than the husband, the largest value in this sample. These couples lived in Massachusetts and Pennsylvania, neither of which granted rights between 1860 and 1870.

the agricultural sector, the state's urbanization rate, the fraction of votes for the Democratic candidate in the most recent gubernatorial election, and the fraction of the population that is female, and  $X_{hst}$  is a vector of individual controls that includes age fixed effects and whether the household is on a farm.<sup>31</sup> All of the state and individual controls are also interacted with *Post*. In some specifications, we also include total household wealth in case rights affect portfolio allocations differentially at various parts of the wealth distribution. We use census person weights and cluster the standard errors by state-year, again using the 1860 state borders for clustering.<sup>32</sup> Our parameter of interest is  $\gamma$ , which captures the effect of granting rights, after netting out general changes between 1860 and 1870.

Columns (1) to (5) of Panel A of Table III reports our main results, using the fraction of a household's portfolio in moveable assets as the dependent variable. Column (1) includes the dummy variable for being in a state that granted women rights between 1860 and 1870 (*Switch*), a dummy variable for 1870 (*Post*), their interaction, and fixed effects for each county. Column (2) adds the state controls, interacted with *Post*. Ideally we would control for existing trends between these *Switch* states and others, but unfortunately we only have data for two years. However, these state-level variables allow us to control in part for political and economic factors that may be correlated either with the decision to grant married women rights or with portfolio choices. Interacting these controls with the 1870 dummy allows us to capture how these trends may have evolved after the Civil War. Relative TFP captures technological forces for industrialization, which may impact both the desire to grant women rights and, through firms' demand for loans, household portfolios. Urbanization rates capture the market for, and the level of, human capital (Geddes and Lueck (2002)). We note that urbanization rates are also a proxy for access to capital markets. The fraction of votes in the most recent gubernatorial election for the Democratic candidate captures the political environment and the level of special interest control (Geddes and Lueck (2002)). It has been argued in both the economic and historical literature that one reason states granted women rights was to attract women to regions with a large gender imbalance (Geddes and Lueck (2002)). We therefore include the fraction of the population that is female in our state controls. In the Internet Appendix, we further study the decision to grant women rights and motivate the use of the controls used here.<sup>33</sup>

<sup>31</sup> We do not include the territory dummy in our state-level controls for this exercise as we have no observations from states changing their territory status with which to identify this control. See Section VII of the Internet Appendix for a robustness analysis on our state control choices.

<sup>32</sup> Because we do not include the South, there are only 22 states, which is not enough for effective clustering. See Angrist and Pischke (2009, p. 319), who argue that the rule of thumb is at least 42 clusters. Statistical significance is quite similar when clustering only by state.

<sup>33</sup> Ideally, we would use more state-level controls, interacted with the 1870 dummy. However, considering that women's rights varies at the state-year level, we are limited in our degrees of freedom. This is especially true in the border analysis below, which has just 15 states in the sample. Including county fixed effects, which imply state fixed effects, and state-level controls, interacted with year removes degrees of freedom from the analysis, eventually undoing the identification of

**Table III**  
**Portfolio: Main Exercise**

Standard errors are clustered at the state-year level in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ . "Fraction Moveable" is the fraction of the household portfolio in moveable assets. "Extensive" is an indicator variable for the possession of nonzero amounts of moveable or real assets. *Switch* is a dummy variable equal to 1 if the state granted rights for the first time between 1860 and 1870, namely, Colorado, Illinois, Minnesota, New Hampshire, and Ohio (Wyoming is dropped due to small sample size). All specifications include the 1870 fixed effect (*Post*) and county fixed effects. State Controls include TFP in the nonagricultural sector relative to TFP in the agricultural sector, urbanization rates, the fraction of votes for the Democratic candidate in most recent gubernatorial election, and the fraction of the population that is female. Individual Controls include age fixed effects and a fixed effect for living on a farm. All State and Individual Controls are interacted with *Post*. The sample "All" includes all states not in the South region. The sample "Non CP" excludes community property states.

	(1)	(2)	(3)	(4)	(5)
Panel A: Fraction Moveable					
Switch×Post	0.010* (0.006)	0.023*** (0.005)	0.020*** (0.005)	0.019*** (0.005)	0.023*** (0.006)
$R^2$	0.102	0.102	0.191	0.203	0.190
Panel B: Extensive Margin, Moveable					
Switch × Post	0.013*** (0.004)	0.022*** (0.004)	0.023*** (0.003)	0.023*** (0.003)	0.027*** (0.004)
$R^2$	0.053	0.054	0.073	0.075	0.072
Panel C: Extensive Margin, Real					
Switch × Post	-0.017** (0.008)	-0.032*** (0.009)	-0.028*** (0.009)	-0.026*** (0.008)	-0.029*** (0.009)
$R^2$	0.119	0.120	0.217	0.241	0.217
Panel D: Common to All Panels					
State Control	No	Yes	Yes	Yes	Yes
Individual Control	No	No	Yes	Yes	Yes
Total Assets	No	No	No	Yes	No
Sample	All	All	All	All	Non CP
Obs.	57,785	57,785	57,785	57,785	56,998

Column (3) adds the individual controls. Although living on a farm is presumably endogenous, we include these controls to capture the differential impact of rights on people who live in different areas. The results are similar when not controlling for farm status. These regressions find that the effect of rights on household portfolios ranges from 1 to 2.3 percentage points, with all specifications statistically significant. As column (3) is the most demanding specification, it is our benchmark result for further analysis. Columns (4) repeats

women's rights on portfolios. See Section VII of the Internet Appendix for extensive robustness tests on the choice of these controls.

column (3), adding total household wealth to the regressors, as households with different amounts of wealth may respond differently to women's rights. Column (5) repeats column (3), dropping from the sample all community property states (sample "Non-CP"), as explained in Section II.A. Estimates range from 1 to 2.3 percentage point increase in the fraction of household portfolios in moveable assets, depending on specification, and are statistically significant in all columns.

To understand the driving force behind this portfolio change, Panels B and C of Table III repeat the pattern of Panel A looking at moveable and real assets separately. Specifically, in Panel B the dependent variable is a dummy variable indicating whether the household had any moveable assets, whereas in Panel C the dependent variable is a dummy variable indicating whether the household had any real assets. This analysis checks the extensive margin of household holdings of each type of asset. We find that property rights increased the extensive margin of household moveable asset possession by 1.3 to 2.7 percentage points, depending on specification. In contrast, property rights decreased the extensive margin of household real asset possession by 1.7 to 3.2 percentage points, depending on specification. Taken together, Panels B and C indicate that a large number of households may well have been constrained in their portfolio choices by coverture. All of the results in Panels B and C are statistically significant at the 5% level or better.

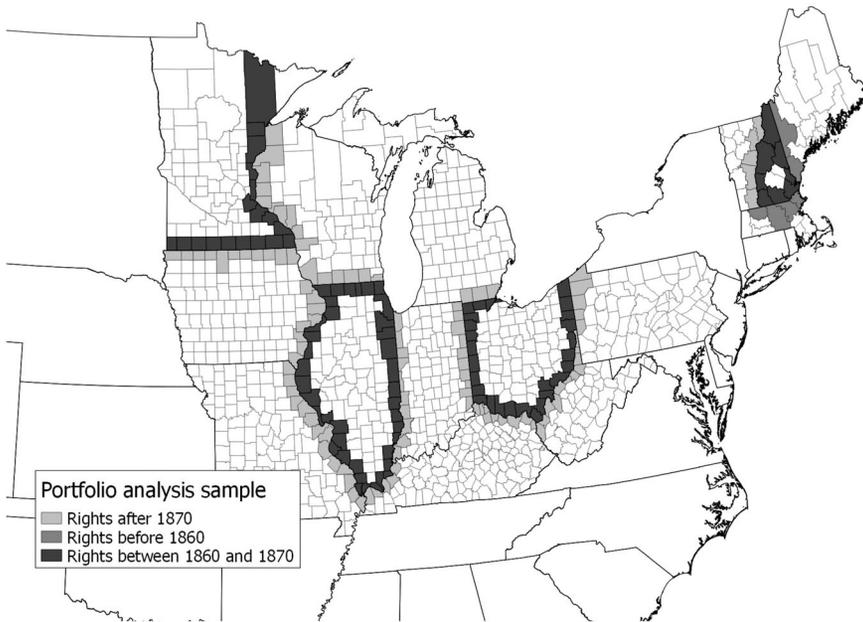
We next repeat the above exercises using state border pairs to control for local economic conditions, in the spirit of Holmes (1998). We take a difference-in-discontinuities approach, as in Grembi, Nannicini, and Troiano (2016). For this exercise, we only use states that gave rights between 1860 and 1870 along with their neighbors, and denote this sample "portfolio borders." As before, we drop Wyoming from our list of states that gave rights in this period due to its small sample size. Given that we are limiting attention to borders, we now drop Colorado as well due to its small sample size. Thus, our sample in this analysis includes Illinois, Minnesota, New Hampshire, and Ohio for states that gave rights between 1860 and 1870. Their neighbors include Iowa, Indiana, Kentucky, Massachusetts, Maine, Michigan, Missouri, Pennsylvania, Vermont, Wisconsin, and West Virginia.<sup>34</sup> Figure 2 shows a map of the border segments in question. See Section VI of the Internet Appendix for a full description of how this data set is constructed.

To replicate Table III with state border pairs, we adjust equation (1) as follows:

$$Y_{hsb(s)t} = \alpha \cdot Switch_s + \beta \cdot Post + \gamma \cdot (Switch_s \times Post) + C_{hs} + Z'_{st} \omega + \eta_0 D_{hcsb(s)} + \eta_1 D_{hcsb(s)} \times Post + X'_{hsb(s)t} \delta + \epsilon_{hsb(s)t}, \quad (2)$$

where  $b(s)$  denotes the section of a state adjacent to state  $s$ , in which household  $h$  resides, in year  $t$ . Relative to equation (1), there is only one new variable:

<sup>34</sup> West Virginia split from Virginia in 1863. However, the area of West Virginia that borders Ohio is identifiable in both 1860 and 1870.



**Figure 2. State borders, 1860 to 1870 portfolio exercise.** County borders are outlined for states included in the sample. Counties on the border between a state granting rights between 1860 and 1870 and control states are highlighted for emphasis.

$D_{hcsb(s)}$ , which is the distance from the centroid of the county  $c$  in state  $s$  in which household  $h$  lives from the border with state  $b(s)$ . Interacting this variable with  $Post$  gives us geographic trends from a state border. These trends are allowed to differ within a state when comparing distances from other state borders, between states compared to the same border, and over time.<sup>35</sup> These controls thus allow us to measure discontinuities at state borders taking into account geographic trends. This exercise looks at how this discontinuity changes over time, hence the term “difference in discontinuities.” All other variables are as described above. We use census person weights and cluster the standard errors at the state-year level.

Table IV mostly repeats Table III using the portfolio borders sample, as well as the distance controls  $D_{hcsb(s)}$  and  $D_{hcsb(s)} \times Post$ . Note that this exercise repeats columns (1) to (3) of Table III, as there are no community property states to drop in this sample. Column (4) repeats column (3) but drops Kentucky and West Virginia, as those states are Southern states. Column (5) repeats column (3) using total assets as a control. For the purpose of comparison, we refer to the results reported in Table III as the “main exercise.” Although column (1) does not find a meaningful effect of rights on the fraction of household portfolios in moveable assets, it finds a significant increase in the extensive margin of

<sup>35</sup> We use households only to measure geographic trends from the closest border. Thus, a household in Illinois on the border of Kentucky will not bias the linear distance measure for households in Illinois closer to the border with Wisconsin.

**Table IV**  
**Portfolio: Border Exercise**

Standard errors are clustered at the state-year level in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ . “Fraction Moveable” is the fraction of the household portfolio in moveable assets. “Extensive” is an indicator variable for the possession of nonzero amounts of either moveable or real assets. *Switch* is a dummy variable equal to 1 if the state granted rights for the first time between 1860 and 1870, namely, Illinois, Minnesota, New Hampshire, and Ohio (Colorado and Wyoming are dropped due to small sample size). All specifications include the 1870 fixed effect (*Post*) and county fixed effects. State Controls include TFP in the nonagricultural sector relative to TFP in the agricultural sector, urbanization rates, the fraction of votes for the Democratic candidate in most recent gubernatorial election, and the fraction of the population that is female. Individual Controls include age fixed effects and a fixed effect for living on a farm. All State and Individual Controls are interacted with *Post*. The sample “All” includes all *Switch* states and their bordering states. The sample “No South” excludes West Virginia and Kentucky.

	(1)	(2)	(3)	(4)	(5)
Panel A: Fraction Moveable					
Switch × Post	0.007 (0.011)	0.063*** (0.009)	0.055*** (0.011)	0.043*** (0.012)	0.056*** (0.010)
$R^2$	0.086	0.086	0.177	0.172	0.191
Panel B: Extensive Margin, Moveable					
Switch × Post	0.026*** (0.008)	0.068*** (0.009)	0.074*** (0.009)	0.067*** (0.011)	0.074*** (0.009)
$R^2$	0.057	0.058	0.078	0.076	0.081
Panel C: Extensive Margin, Real					
Switch × Post	-0.012 (0.014)	-0.069*** (0.011)	-0.060*** (0.013)	-0.054*** (0.016)	-0.060*** (0.011)
$R^2$	0.092	0.092	0.191	0.190	0.218
Panel D: Common to All Panels					
State Control	No	Yes	Yes	Yes	Yes
Individual Control	No	No	Yes	Yes	Yes
Total Assets	No	No	No	No	Yes
Sample	All	All	All	No South	All
Obs.	46,238	46,238	46,238	43,243	46,238

moveable asset holdings, and a negative (though insignificant) effect on real asset holdings. Columns (2) and (3) find somewhat larger estimates than their equivalents in the main exercise, and are statistically significant at the 1% level. Columns (4) and (5) show that these results are robust. Our estimates in Tables III and IV imply an increase of 1.0 to 6.3 percentage points in the fraction of a household’s wealth allocated to moveable assets. Given that the switching states originally had about 42% of household portfolios in moveables, this represents a 2.4% to 15% increase.

In sum, we find that granting women rights significantly increased the fraction of household wealth allocated to moveable assets, consistent with Prediction 1.

### B. Rights Leads to Financial Market Deepening

We now provide evidence on the relationship between granting economic rights to married women and financial markets. We show that granting women rights results in lower interest rates. We next show that rights lead to greater financial intermediation, as measured by bank deposits and loans. These results are consistent with the idea that granting women rights leads to a positive supply shock in financial markets.

Table V reports summary statistics for the real interest rate, deposits, and loans. Figure 3 plots state-year interest rate observations by the number of years before or after a state granted rights, net of year fixed effects. The figure shows nonparametric fitted lines for the periods before and after granting rights. As can clearly be seen, in the years leading up to rights being granted, interest rates hovered around a constant level with no clear trend. In contrast, after rights are granted, the interest rate falls immediately and continues to fall further over time.

To test the robustness of the relationship shown in Figure 3, we conduct a more formal analysis of the relationship between rights and financial variables. Our regression specification is of the form:

$$Y_{st} = \alpha \cdot rights_{st} + \lambda_s + d_{it} + T_{st} + X'_{st}\gamma + \epsilon_{st}, \quad (3)$$

where  $Y_{st}$  is the interest rate, deposits, or loans in state  $s$  and year  $t$ ,  $rights_{st}$  is a dummy variable denoting whether state  $s$  had given rights by year  $t$ ,  $\lambda_s$  is a set of state fixed effects using the political boundaries from 1860, as before,  $d_{it}$  are either year fixed effects or region-year fixed effects for each region  $i$ , where the four census regions are Northeast, Midwest, South, and West,  $T_{st}$  is a dummy variable indicating whether state  $s$  was a territory in year  $t$ , and  $X_{st}$  is a vector of state controls which include the fraction of neighboring states that had given rights, as well as financial controls, which include the maximum legal interest rate and dummy variables for whether a state had a bank reserve requirement, double liability for bank shareholders, and a state banking authority. We use population weights in these regressions and cluster standard errors at the state level. The population for each state is linearly interpolated between census years.

Table VI reports the regressions results. We begin by presenting results when the dependent variable is the real interest rate. Column (1) shows the baseline specification in which we regress interest rates on rights, and state, year, and territory fixed effects, and the fraction of neighboring states that had granted rights. The fraction of neighboring states that had granted rights controls for the potential impact of credit market openness. For instance, if New York grants women's rights, and thus deepens its financial markets, the

**Table V**  
**Summary Statistics: 1850 to 1920 United States**

Panel A of this table includes data on state-level financial markets and nonagricultural employment. Panel B of this table includes data on state-level characteristics used as controls in our analysis.

Variable	Mean	Median	<i>SD</i>	10th Percentile	90th Percentile
Panel A: Economic Outcomes					
<i>Real Interest Rate</i> (pp)	7.995	7.364	2.900	5.476	10.989
$\Delta$ <i>Real Deposits per Capita</i> (1920 Dollars)	3.766	2.301	12.099	-4.739	14.663
$\Delta$ <i>Real Loans per Capita</i> (1920 Dollars)	3.705	2.372	13.699	-4.562	13.399
<i>Fraction Nonagri. Employment</i>	0.541	0.526	0.207	0.284	0.819
<i>Ratio of High to Low KL Employment</i>	1.850	1.010	2.608	0.461	3.958
<i>Fraction Top KL Employment</i>	0.037	0.018	0.044	0.007	0.094
<i>Bottom KL Employment</i>	0.029	0.020	0.030	0.004	0.066
Panel B: Explanatory Variables					
<i>Territory</i>	0.096	0.000	0.294	0.000	0.000
<i>Nonagricultural TFP Relative to Agricultural TFP</i>	5.837	5.025	3.526	2.544	10.163
<i>Fraction Female</i>	0.470	0.488	0.058	0.399	0.507
<i>Urbanization Rate</i>	0.263	0.219	0.195	0.041	0.551
<i>Fraction Democrat</i>	0.437	0.446	0.164	0.232	0.615
<i>Fraction Female in School</i>	0.187	0.193	0.060	0.112	0.255
<i>Fraction Male in School</i>	0.176	0.184	0.059	0.089	0.240
<i>Adult Under 35</i>	0.502	0.497	0.072	0.424	0.586
<i>Fraction Neighboring States with Rights</i>	0.588	0.667	0.385	0.000	1.000

financial markets of Connecticut may be affected if its financial markets are not completely closed. Column (2) adds the various financial controls: the maximum legal interest rate, a bank reserve requirement, double liability for bank shareholders, and a state banking authority. The laws maximum legal interest rate, otherwise known as usury laws, determined the highest interest rate that could be legally charged in a state-year. The data up to 1891 come from Holmes

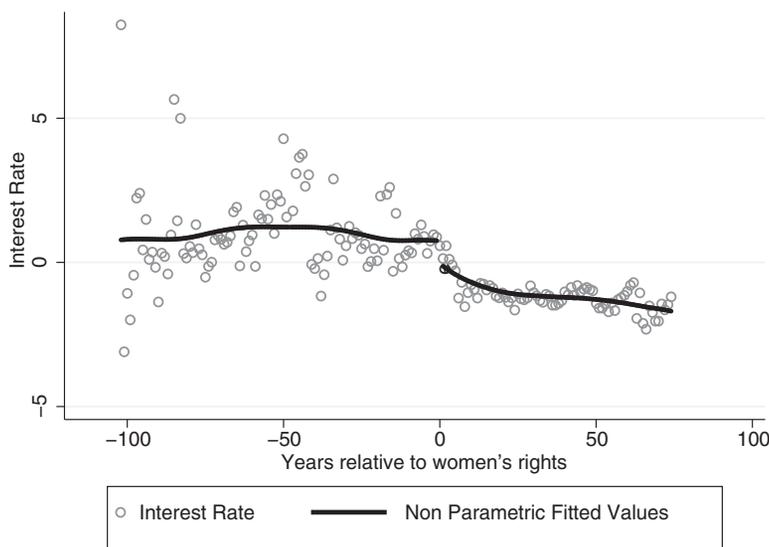


Figure 3. Interest rates before and after women's rights, net of year effects.

Table VI  
Rights and Interest Rates, Deposits, and Loans

Standard errors are clustered at the state level in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ . All regressions include state fixed effects, a dummy for territory, and the fraction of neighboring states with rights. Financial Controls include the maximum legal interest rate as well as dummies for a state having a reserve requirement, double liability for bank shareholders, and a bank authority. Regressions are weighted by state population.

Dependent Variable	Interest Rate			Deposits			Loans		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rights	-0.788** (0.328)	-0.666** (0.312)	-0.492* (0.289)	2.177** (0.937)	2.051** (0.898)	1.188* (0.655)	2.647** (1.058)	2.239** (0.964)	1.367* (0.743)
Year FE	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
Region × Year FE	No	No	Yes	No	No	Yes	No	No	Yes
Financial Control	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Obs.	1,971	1,971	1,971	2,506	2,506	2,506	2,508	2,508	2,508
R <sup>2</sup>	0.735	0.742	0.800	0.349	0.350	0.617	0.224	0.224	0.398

(1892). We extend these data through 1920.<sup>36</sup> Benmelech and Moskowitz (2010) argue that usury laws had meaningful effects on discounting activity and thus need to be taken into account when analyzing 19<sup>th</sup> century financial markets. A minimum bank reserve requirement directly affects bank lending and interest rates. Through added faith in bank stability, these reserve requirements

<sup>36</sup> See Internet Appendix Section V for a description of these laws, how well they were enforced, and how we construct this time series.

presumably affect deposits as well. Double liability laws for bank shareholders allow banks' creditors, such as depositors, to sue bank shareholders for losses in the event of a bank failure. As a result, these shareholders may pressure bank managers to be more cautious with lending and increased depositor faith, especially given that many managers were themselves shareholders of the banks they managed.<sup>37</sup> State banking authorities could deny charters to banks without sufficient capital, monitor banks for compliance with regulations, and help unwind failed institutions. These institutions discouraged excessive risk-taking on the part of banks, which in turn increased faith in financial markets. The data on these three financial controls come from Mitchener and Jaremski (2015).<sup>38</sup> Column (3) replaces year fixed effects with region-year fixed effects. In all specifications, the impact of women's property rights on interest rates is negative, statistically significant, and quantitatively meaningful. The coefficients suggest that granting rights to women lowered interest rates by about 50 to 80 basis points, or roughly 6% to 10% of the mean interest rate.

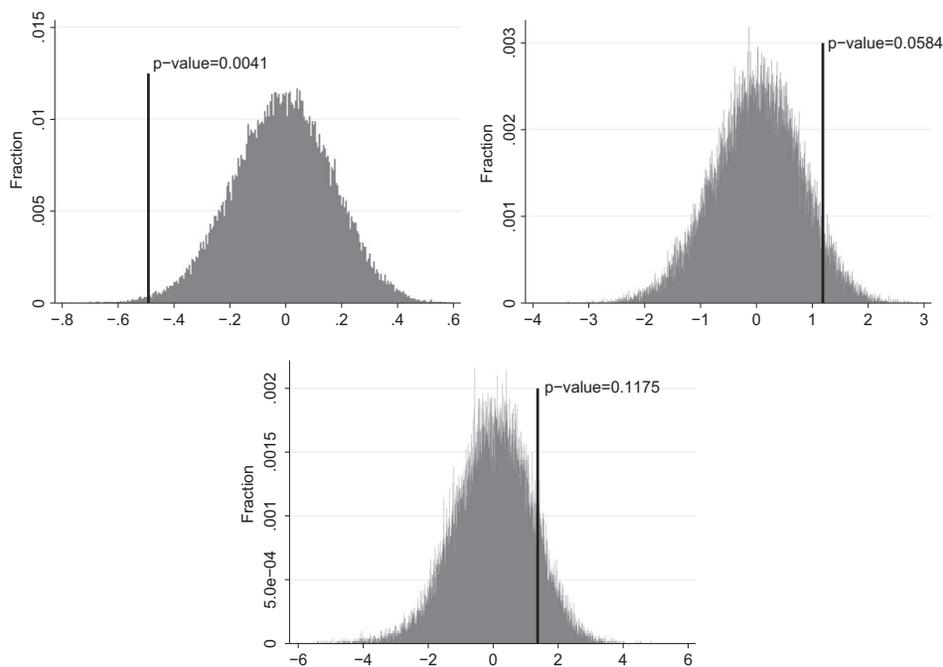
Table VI continues by showing the results of these specifications when the dependent variable is deposits or loans. Columns (4) to (6) follow the pattern above, with the dependent variable being deposits, whereas columns (7) to (9) follow this pattern with the dependent variable being loans. When rights are granted, there is an increase in money deposited in banks and consequently in loans from banks, reflecting an increase in financial intermediation. As before, the estimates are somewhat smaller and less precise when including region-year fixed effects rather than year fixed effects. Quantitatively, the point estimates suggest magnitudes equivalent to about 10% of a standard deviation, or about 30% to 40% of the mean of the dependent variables. Table IA.IX in the Internet Appendix performs robustness checks of these results, dropping community property states and states that gave rights after 1920. The results are very similar to those described here.

We next turn to a randomization test where we reestimate the model in equation (3) after randomly assigning a date for women's rights to each state. We then repeat the regression specifications from columns (3), (6), and (9) 50,000 times. During each iteration, we randomly assign a date that each state gave women economic rights, drawn uniformly between 1850 and 1920.

Figure 4 provides histograms for the estimates of  $\alpha$  along with our estimate (reported above) for the regression using the actual dates that states gave rights. The vertical line labeled "*p*-value" shows the fraction of cases in which the regressions with random dates yielded larger (in absolute terms) coefficients on  $\alpha$  for our exercises with interest rates, deposits, and loans than the regression with the actual dates yielded. Running our regressions on random dates yields estimates centered at zero, indicating that the model in equation (3) is unlikely to produce biased results. The results thus show that it is unlikely that our point estimates are a result of a random occurrence.

<sup>37</sup> See Macey and Miller (1992) for a thorough legal history of double liability laws.

<sup>38</sup> We thank the authors for sharing their data with us.



**Figure 4. Randomization test.** Results of 50,000 simulations of randomly assigned women's liberation dates. The top left panel shows the coefficient  $\alpha$  in the regression for the interest rate, the top right panel shows the coefficient for deposits, and the bottom panel shows the coefficient for loans.

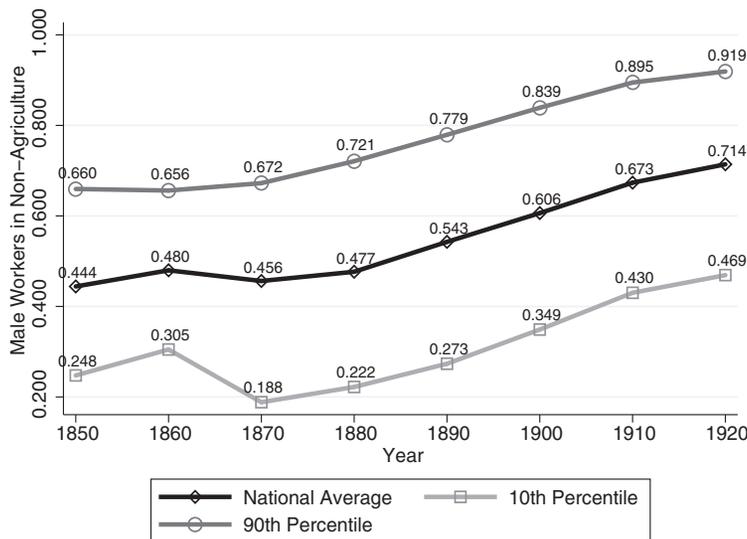
To summarize, we show that granting economic rights to women is associated with both lower interest rates and an increase in financial intermediation, which is consistent with an increase in the supply of loanable funds, as in Prediction 2, and with the portfolio reallocations discussed above.

### C. Rights Leads to Sectoral Labor Reallocations

We next turn to the dynamic effect of women's rights on labor allocations between sectors of the economy. We begin with a state-level analysis. We run an analysis exploiting state border pairs.

Table V reports summary statistics for the fraction of a state's male employment in the nonagricultural sector as well as our control variables, described below. Figure 5 shows the time-series fraction of male employment in the nonagricultural sector.<sup>39</sup> The line denoted "National Average" is the average for the entire country. The line denoted "90<sup>th</sup> Percentile" is the 90<sup>th</sup> percentile of states, as ranked separately each year, where the ranking is ordered by the fraction of workers in the nonagricultural sector in each state. The line denoted

<sup>39</sup> Note that we are looking at male labor allocations only, as married women were seldom in the work force in this period.



**Figure 5. Cross-state comparison of nonagricultural employment.** “National Average” is the fraction of male workers in nonagricultural employment in a given year for the entire United States. When ranking states in each year by this measure, “90<sup>th</sup> Percentile” is the value for the state with the 90<sup>th</sup> percentile of male nonagricultural employment, whereas “10<sup>th</sup> Percentile” is the value for the state with the 10<sup>th</sup> percentile of male nonagricultural employment.

“10<sup>th</sup> Percentile” accordingly represents the 10<sup>th</sup> percentile of states. This figure shows an overall trend toward greater nonagricultural labor as the country developed, as well as a fair amount of cross-state variation. In each year, the 90<sup>th</sup> percentile was roughly 20 percentage points above the mean, whereas the 10<sup>th</sup> percentile was 20 percentage points below the mean. Note that nonagricultural employment decreased dramatically after the Civil War, in the bottom 10 percentile of states, recovering to its antebellum level only between 1890 and 1900.

Except for the dates of rights being granted and TFP levels, all of our data for this set of exercises come 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) and Fernández (2014). 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.

Our empirical approach follows Wolfers (2006) in estimating the dynamic relationship between granting women’s rights and development. Accordingly, we estimate a regression that takes into account the temporal distance between a state-year observation and the date of women’s economic rights in that state.

Our specification is of the form:

$$L_{st}^{NA} = \sum_k \alpha_k \cdot rights_{st}^k + \lambda_s + d_{it} + T_{st} + X_{st}'\gamma + \epsilon_{st}, \quad (4)$$

where  $L_{st}^{NA}$  is the fraction of male workers in the nonagricultural sector in state  $s$  and year  $t$ ,  $t \in \{1850, 1860, \dots, 1920\}$ ,  $rights_{st}^k$  is a series of dummy variables set equal to 1 if a state had granted rights  $k$  years ago, where  $k \in \{\leq -30, -20, -10, 0, 10, 20, \geq 30\}$ , and  $\lambda_s$  are state fixed effects.<sup>40</sup> We again use the state political borders from the beginning of our sample, specifically 1850. As defined above,  $d_{it}$  are either year fixed effects or region-year fixed effects for each region  $i$ ,  $T_{st}$  is a dummy variable indicating whether state  $s$  was a territory in year  $t$ ,  $X_{st}$  is a vector of controls that includes the ratio of TFP in the nonagricultural sector to TFP in the agricultural sector, a dummy variable indicating whether the state permitted limited liability corporations, the fraction of the population that is female, the fraction of women in school, the fraction of men in school, the fraction of the adult population under age 35, and the fraction of neighboring states that had granted rights by year  $t$ . We use census population weights and cluster standard errors at the state level.

Table VII reports the regression results. All estimates are relative to a decade before rights are granted. Column (1) includes year and state fixed effects, as well as the territory dummy variable. Column (2) adds relative TFP as well as the dummy variable whether the state permitted limited liability corporations. Relative TFP captures the technological causes of industrialization, and thus labor allocations between sectors. The date a state started allowing LLCs comes from Hamill (1999). Prior to these laws, LLCs could be established only by special charter through the legislature. Easy establishment of LLCs allows for further business development, and thus industrialization and labor reallocation. Column (3) adds the fraction of the population that is female, the fraction of women in school, and the fraction of men in school. These variables allow us to capture human capital levels and the supply of substitutes for male labor (that is, female labor). Column (4) adds the fraction of the population under age 35. This allows us to control for a degree of labor market flexibility that might aid in the reallocation of workers between sectors. Column (5) adds the fraction of a state's bordering states that had given rights. This helps control for regional spillover effects. Column (6) repeats column (5) but replaces year fixed effects with region-year fixed effects.

Before rights were granted, there is no trend in industrialization. That is, given state and year (or region-year) fixed effects, as well as other controls, industrialization did not deviate from what would have been expected. Once rights were given, there is a statistically significant increase in the fraction of

<sup>40</sup> We use increments of 10 as our data are dependent on the decennial census. Recall that for states that granted rights in a noncensus year, we round up. Returning to our previous example, New Jersey granted rights in 1874, which for our purposes we round to 1880. Thus, the dummy variable  $rights_{st}^0$  takes the value of 1 for New Jersey in 1880, whereas the dummy variable  $rights_{st}^{10}$  takes the value of one for New Jersey in 1890.

**Table VII**  
**Rights and Reallocation of Labor**

Standard errors are clustered at the state level in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ . All specifications include a dummy for territory. Regressions are weighted by state population.

	Dependent Variable: % Male Workers in Nonagriculture					
	(1)	(2)	(3)	(4)	(5)	(6)
≥ 3 Decades Before	−0.008 (0.028)	−0.018 (0.026)	−0.030 (0.026)	−0.017 (0.023)	−0.016 (0.022)	−0.023 (0.020)
2 Decades Before	0.009 (0.019)	0.006 (0.018)	0.004 (0.021)	0.002 (0.017)	0.004 (0.016)	0.011 (0.018)
1 Decade Before	0	0	0	0	0	0
Rights Given	0.032*** (0.008)	0.034*** (0.009)	0.031*** (0.008)	0.032*** (0.009)	0.039*** (0.010)	0.026*** (0.008)
1 Decade After	0.046*** (0.015)	0.050*** (0.016)	0.048*** (0.016)	0.042*** (0.015)	0.049*** (0.015)	0.038*** (0.013)
2 Decades After	0.068*** (0.022)	0.073*** (0.023)	0.070*** (0.022)	0.056*** (0.020)	0.063*** (0.020)	0.050** (0.020)
≥3 Decades After	0.075** (0.028)	0.081*** (0.028)	0.076** (0.028)	0.060** (0.025)	0.064** (0.024)	0.052** (0.025)
Relative TFP		0.003 (0.004)	−0.000 (0.003)	0.002 (0.003)	0.002 (0.003)	0.006 (0.003)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No
(Year × Region) FE	No	No	No	No	No	Yes
Incorporation	No	Yes	Yes	Yes	Yes	Yes
Fraction Female	No	No	Yes	Yes	Yes	Yes
Frac. Female in School & Frac. Male in School	No	No	Yes	Yes	Yes	Yes
Fraction Under Age 35	No	No	No	Yes	Yes	Yes
Fraction Neighboring States with Rights	No	No	No	No	Yes	Yes
Obs.	356	356	356	356	356	356
$R^2$	0.937	0.939	0.952	0.957	0.958	0.970

the labor force working in the nonagricultural sector. The relationship is dynamic, increasing with respect to the amount of time since rights were granted, with an estimated total increase of 3.8 to 5.0 percentage points a decade after rights were given. The effects remain significant for at least 30 years. This finding shows clearly that granting rights is associated with an increase in nonagricultural employment. The results for column (6) are plotted in Figure IA.13 of the Internet Appendix. In the Internet Appendix, we show that these results are robust to using an alternative definition of nonagricultural labor employment, dropping all observations from 1890 (as the data for that year are imputed due to records being destroyed in a fire), dropping states that gave rights between 1870 and 1880, dropping community property states, and dropping states that gave rights after 1920.

A question that may arise is whether our results are simply a reflection of the fact that labor was moving relatively continuously from the agricultural to the nonagricultural sector, as shown in Figure 5. That is, if there was a trend toward development, then we might see that the fraction of employment in nonagriculture is increasing dynamically relative to any given date. Although our regressions show no trend before rights were granted, we double-check this hypothesis by using a randomization test as before. We take the regression specification from column (6) in Table VII and repeat it 50,000 times. During each iteration, we randomly assign a date for each state, drawn uniformly between 1850 and 1920, and proceed as if that were the date when women were granted rights in that state. Running our regressions on random dates yields estimates centered at zero, which indicates that the model in equation (4) is unlikely to produce biased results. The  $p$ -value on the figure suggests that our results are extremely unlikely to be a random occurrence. Full results are reported in the Internet Appendix.

Next, we repeat the exercises above using state border pairs to control for local economic conditions. See the Internet Appendix for maps showing the state border segments used between 1850 and 1920, as well as a detailed description of the construction of this data set.

To replicate Table VII with state border pairs, we adjust equation (4) as follows:

$$L_{sb(s)t}^{NA} = \sum_k \alpha_k \cdot rights_{st}^k + \lambda_s + d_{it} + T_{st} + P_{sb(s)} + X'_{sb(s)t} \gamma + Y'_{sb(s)t} \delta + \epsilon_{st},$$

where  $b(s)$  denotes the section of a state adjacent to state  $s$ , and  $P_{sb(s)}$  are fixed effects for the area of state  $s$  and  $b(s)$  that share a border. For example, the portion of Ohio that borders Pennsylvania is an observation, and shares a fixed effect ( $P_{sb(s)}$ ) with the portion of Pennsylvania that borders Ohio. The vector  $X_{sb(s)t}$  contains state-level controls that include the ratio of TFP in the nonagricultural sector to TFP in the agricultural sector, a dummy variable indicating whether the state permitted LLCs, and the fraction of neighboring states that had granted rights by year  $t$ , and the vector  $Y_{sb(s)t}$  contains controls at the state border level that include the fraction of the population that is female, the fraction of women in school, the fraction of men in school, and the fraction of the adult population under age 35. Note that, with the exception of  $P_{sb(s)}$ , all of the controls are exactly the same as in equation (4). However, we calculate the variables in  $Y_{sb(s)t}$  for each state border region separately. That is, the area of Ohio bordering Pennsylvania has a potentially different fraction of women in school than does the area of Ohio bordering Michigan. We use census population weights and cluster standard errors at the level of the state border pair.

Table VIII repeats Table VII after including state border pair fixed effects. In all specifications, there is no clear pre-rights trend in labor allocations. Upon granting rights, and 10 to 20 years later (depending on the specification), there is a quantitatively large and statistically significant reallocation of labor

**Table VIII**  
**Rights and Reallocation of Labor—Border Analysis**

Standard errors are clustered at the level of state border pair in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ . All specifications include a dummy for territory. Regressions are weighted by state border population.

	Dependent Variable: % Male Workers in Nonagriculture					
	(1)	(2)	(3)	(4)	(5)	(6)
≥ 3 Decades Before	0.034 (0.028)	0.043 (0.026)	0.035 (0.026)	0.029 (0.028)	0.029 (0.028)	0.008 (0.027)
2 Decades Before	−0.009 (0.015)	−0.004 (0.013)	−0.005 (0.012)	−0.009 (0.013)	−0.009 (0.013)	0.013 (0.014)
1 Decade Before	0	0	0	0	0	0
Rights Given	0.034*** (0.011)	0.025** (0.012)	0.032** (0.012)	0.031*** (0.012)	0.043*** (0.014)	0.066*** (0.014)
1 Decade After	0.051*** (0.017)	0.040** (0.019)	0.046** (0.020)	0.040** (0.019)	0.052** (0.021)	0.089*** (0.025)
2 Decades After	0.013 (0.021)	0.000 (0.022)	0.006 (0.024)	−0.000 (0.024)	0.013 (0.025)	0.046* (0.024)
≥3 Decades After	0.015 (0.027)	0.000 (0.030)	0.003 (0.031)	−0.007 (0.031)	0.005 (0.032)	0.038 (0.031)
Relative TFP		−0.007 (0.004)	−0.007 (0.004)	−0.006 (0.005)	−0.006 (0.004)	−0.007 (0.004)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No
(Year × Region) FE	No	No	No	No	No	Yes
Incorporation	No	Yes	Yes	Yes	Yes	Yes
Fraction Female	No	No	Yes	Yes	Yes	Yes
Frac. Female in School & Frac. Male in School	No	No	Yes	Yes	Yes	Yes
Fraction Under Age 35	No	No	No	Yes	Yes	Yes
Fraction Neighboring States with Rights	No	No	No	No	Yes	Yes
Obs.	1,338	1,338	1,338	1,338	1,338	1,338
$R^2$	0.855	0.857	0.859	0.864	0.864	0.910

from agricultural to nonagricultural employment. Our results suggest that, 10 years after rights are granted, 4.0 to 8.9 percentage points of male workers in a given area are reallocated away from agricultural employment. Although column (6) is the only specification to find statistically significant results more than two decades after rights, it is also the most demanding specification. In the Internet Appendix, we show that these results are robust to the same checks performed above. We also repeat the randomization test described above on column (6) of Table VIII, which confirms that our results are extremely unlikely to be a random occurrence. In particular, the “ $p$ ” values when rights are given and one decade later are less than 0.01, whereas the “ $p$ ” value for the estimate two decades after rights are given is less than 0.07. We conclude

that our main results from Table VII are not driven by geographical variation in economic conditions.

To summarize, granting women rights is associated with an immediate and dynamic increase in the fraction of the labor force that works in the nonagricultural sector, which is consistent with Prediction 3 and the credit market deepening shown above.

#### D. Rights Leads to Labor Reallocations Toward Capital Intensive Industries

We end the analysis by studying the dynamic effect of women's rights on labor allocations within the nonagricultural sector of the economy, by capital intensity. We show that the ratio of employment in industries with high capital intensity to employment in industries with low capital intensity has no trend prior to rights, and dynamically increases after rights were granted.<sup>41</sup> We break this result down to show that it is driven by increases in employment in capital-intensive industries, rather than a decrease in employment in industries with low capital intensity.

Table V reports summary statistics for the fraction of a state's male employment in industries with high capital intensity, industries with low capital intensity, and the ratio of the two.<sup>42</sup> Figure 6 shows the time-series ratio of male employment in industries with high capital intensity to industries with low capital intensity. The line denoted "National Average" is the average for the entire country, the line denoted "90<sup>th</sup> Percentile" shows the 90<sup>th</sup> percentile of states, ranked separately each year, whereas the line denoted "10<sup>th</sup> Percentile" similarly represents the 10<sup>th</sup> percentile of states. This figure shows an overall trend toward industries with higher capital intensity as the country developed, as well as a fair amount of cross-state variation.

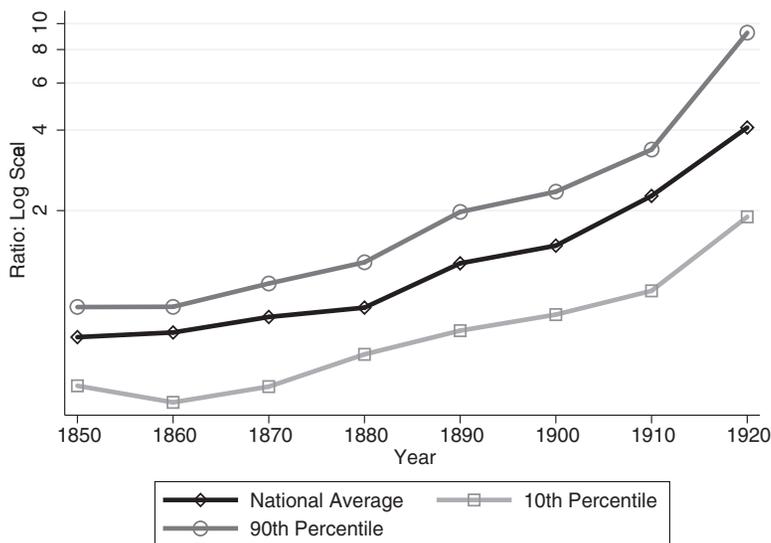
Our empirical approach is as before in estimating the dynamic relationship between granting women's rights and development. Our specification is of the form

$$Y_{st} = \sum_k \alpha_k \cdot rights_{st}^k + \lambda_s + d_{it} + T_{st} + X_{st}\gamma + \epsilon_{st}, \quad (5)$$

where  $Y_{st}$  is the ratio of male workers in industries with high capital intensity to low capital intensity, the log of the fraction of workers in high capital intensity industries, or the log of the fraction of workers in low capital intensity industries in state  $s$  and year  $t$ ,  $t \in \{1850, 1860, \dots, 1920\}$ ,  $rights_{st}^k$  is a series of dummy variables set equal to 1 if a state had granted rights  $k$  years ago, where  $k \in \{\leq -30, -20, -10, 0, 10, 20, \geq 30\}$ ,  $\lambda_s$  are state fixed effects, using the 1850

<sup>41</sup> This exercise is in the spirit of Rajan and Zingales (1998), who show that greater access to finance aids development of industries that rely on external finance.

<sup>42</sup> Note that the average employment in industries with high capital intensity is 3.66% of workers, whereas employment in industries with low capital intensity is 2.89% of workers. These numbers are as opposed to total nonagricultural employment, described above, which averaged 54% of employment. As such, employment in industries with either high or low capital intensity is approximately 14% of nonagricultural employment.



**Figure 6. Cross-state comparison of the high to low capital intensity employment.** “National Average” is the log of the ratio of male employment in capital-intensive relative to noncapital-intensive industries in a given year for the entire United States. When ranking states in each year by this measure, “90<sup>th</sup> Percentile” is the value for the state with the 90<sup>th</sup> percentile of this log-ratio, whereas “10<sup>th</sup> Percentile” is the value for the state with the 10<sup>th</sup> percentile of this log-ratio.

political borders for the fixed effect as before,  $d_{it}$  are year fixed effects or region-year fixed effects for each region  $i$ ,  $T_{st}$  is a dummy variable indicating if state  $s$  was a territory in year  $t$ , and  $X_{st}$  is a vector of the same controls as above—the ratio of TFP in the nonagricultural sector to TFP in the agricultural sector, a dummy variable indicating whether the state permitted LLCs, the fraction of the population that is female, the fraction of women in school, the fraction of men in school, the fraction of the adult population under age 35, and the fraction of neighboring states that had granted rights by year  $t$ . We use census population weights for these regressions as before.

Table IX reports the regressions results. All estimates are relative to a decade before rights were granted. Column (1) includes year and state fixed effects, as well as the territory dummy variable. Column (2) adds all of our remaining controls. Column (3) switches to region-year fixed effects and is thus the benchmark specification. Columns (4) and (5) break down the result from the benchmark column (3). Specifically, column (4) employs the specification from column (3), but uses the dependent variable log of the fraction of workers in industries with high capital intensity, that is, the log of the numerator of the dependent variable in column (3). Using the log allows us to measure the percentage change in the numerator of the main dependent variable. Finally, column (5) employs the specification from column (3), but uses as the dependent variable the log of the fraction of workers in industries with low capital intensity. All estimates include standard errors clustered at the state level.

**Table IX**  
**Rights and Employment by Capital Intensity**

Standard errors are clustered at the state level in parentheses. \* $p < 0.10$ , \*\* $p < 0.05$ , and \*\*\* $p < 0.01$ . All specifications include 1850 state fixed effects and a dummy for territory. Controls include the fraction of the population that is female, the fraction of the adult population that is under age 35, the fraction of females in school, the fraction of males in school, and an incorporation dummy. Regressions are weighted by state population.

Dependent Variable:	Ratio of High to Low KL			Log High KL	Log Low KL
	(1)	(2)	(3)	(4)	(5)
≥ 3 Decades Before	-1.679 (1.072)	-1.751 (1.303)	-1.728 (1.184)	-0.208 (0.232)	-0.036 (0.124)
2 Decades Before	-0.305 (0.392)	-0.211 (0.493)	-0.150 (0.437)	0.121 (0.160)	-0.019 (0.081)
1 Decade Before	0	0	0	0	0
Rights Given	1.518 (0.992)	1.979* (1.118)	1.913** (0.889)	0.291*** (0.068)	0.048 (0.061)
1 Decade After	1.502* (0.777)	2.103** (1.018)	2.036** (0.904)	0.343*** (0.113)	0.154* (0.090)
2 Decades After	1.958* (1.047)	2.672** (1.276)	2.551** (1.157)	0.407** (0.154)	0.237* (0.136)
≥3 Decades After	1.573** (0.766)	2.415** (0.990)	2.443** (0.929)	0.472** (0.199)	0.328* (0.177)
Relative TFP		0.197 (0.193)	0.327 (0.234)	0.024 (0.032)	0.019 (0.021)
Controls	No	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
(Year × Region) FE	No	No	Yes	Yes	Yes
Obs.	345	345	345	347	345
R <sup>2</sup>	0.627	0.659	0.689	0.899	0.931

Columns (1) to (3) show that before rights were granted there was no trend in the ratio of employment in industries with high capital intensity to employment in industries with low capital intensity. That is, given state and year (or region-year) fixed effects, as well as other controls, this ratio did not deviate from what would have been expected. Once rights were given, there is a statistically significant increase in the ratio of the labor force working in industries with high capital intensity. The relationship is dynamic, increasing with respect to the amount of time since rights were granted. Columns (4) and (5) show that increases in employment in industries with high capital intensity, rather than a decline in employment in industries with low capital intensity, drives the result. Thus, the result comes from a roughly 40% increase in employment in high capital intensity industries a decade after rights were granted. There is relatively slower growth in employment in industries with low capital intensity. In the Internet Appendix, we test the robustness of the results in Column (3) using the same robustness exercises as described above in Section IV.C, with the exception of the alternative definition for  $L^{NA}$ , which does not apply in this setting. Our results are robust to these tests.

Finally, we perform a randomization exercise, as before. We take the regression specifications from columns (4) and (5) in Table IX and repeat them 50,000 times. During each iteration, we randomly assign a date for each state, drawn uniformly between 1850 and 1920, and proceed as if that were the date when women were granted rights in that state. Running our regressions on random dates yields estimates centered at zero, indicating that the model in equation (5) is unlikely to produce biased results. The results show that it is extremely unlikely that our estimates on the growth of the most capital-intensive industries were random. Although it is likely that the least capital-intensive industries also grew as a result of rights, this is less likely than for the high capital-intensive industries, confirming our results described above. Full results are available in the Internet Appendix.

We do not perform a border pair analysis here, as the division of labor by capital intensity on state borders is much noisier. For instance, in the main sample, the 10<sup>th</sup> percentile of states had 0.66% (0.42%) of their labor force in high (low) capital intensity industries. In the border sample, almost 20% of observations find no workers in either of these industries. If certain areas of a state industrialized more than others, this should not be a surprising result. However, it adds too much noise to our analysis for reliable results.

To summarize, granting women rights is associated with an immediate and dynamic increase in the ratio of the labor force that works in industries with high capital intensity to low capital intensity, consistent with Prediction 4.

## V. Concluding Remarks

In this paper, we explore one of the greatest extensions of property rights in human history, namely, the demise of coverture with the granting of property rights to married women in the United States. We explore the economic ramifications of coverture's demise, and the resulting expansion of investor protection to women. We exploit the staggered nature of the timing of women's property rights in the United States to study the financial and economic impacts of these rights. We find that these rights led to dramatic portfolio reallocations, financial market deepening, reallocation toward the nonagriculture sector, and reallocation toward more capital-intensive industries.

Although the particular example studied in this paper exploits details of the laws of coverture, the general notion examined here could be applied to any area of the world where women or minorities are still denied economic rights.

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## Appendix A: Variable Definitions

We now describe in detail the variables we construct for our empirical analysis not otherwise described in the text.

TFP by sector uses data from Turner et al. (2007, 2011), and Turner, Tamura, and Mulholland (2013). We follow these authors in using a Cobb-Douglas production function, with the same elasticities, when calculating a combined TFP for the manufacturing and nonmanufacturing nonagricultural sector, as well as the TFP for the agricultural sector. Seven state-year observations are missing, which we impute based on a regression controlling for state and year fixed effects. Data on when each state became a state, rather than a territory, are from Geddes and Lueck (2002). The fraction of neighboring states with women's economic rights, by year, comes from the authors' calculation using modern state borders.

We now turn to the other controls for these regressions. All of these variables are calculated by state for each year using data from Ruggles et al. (2010). The *Fraction of females in school* is the fraction of females currently in school, and the same for *Fraction of males in school*. *Fraction female* is the fraction of the population that is female. *Fraction of adults under 35* is the number of people who are in the age interval [20,34) years old divided by the number of people in the interval [20,  $\infty$ ).

### A.1 Crosswalk from SIC to IND1950

We now describe the crosswalk between the two-digit SIC code, used in the Census of Manufactures, and IND1950, used in the population census, for our exercises in Section IV.D.

For industries with high capital intensity:

- (1) Textile Mill Products, SIC code: 22. IND1950: 436-449
- (2) Transportation Equipment, SIC code: 37. IND1950: 376-379
- (3) Paper and Allied Products, SIC code: 26. IND1950: 456-459
- (4) Chemical and Allied Products, SIC code: 28. IND1950: 466-469

For industries with low capital intensity:

- (1) Tobacco Products, SIC code: 21. IND1950: 429
- (2) Apparel and Other Textile Products, SIC code: 23. IND1950: 448, 449
- (3) Leather and Leather Products, SIC code: 31. IND1950: 487-489
- (4) Stone, Clay, and Glass Products, SIC code: 32. IND1950: 316-326

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### **Supporting Information**

Additional Supporting Information may be found in the online version of this article at the publisher's website:

**Replication code.**

**Appendix S1:** Internet Appendix.