

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 gave married women rights starting in the 1850s. Before this “women’s liberation,” the doctrine of coverture strongly incentivized women to hold real estate, rather than financial assets such as money, stocks, and bonds. We exploit the staggered nature of coverture’s demise across US states to show that women’s rights led to shifts of household portfolios; a positive shock to the supply of credit; and a reallocation of labor towards non agriculture and capital intensive industries. Expansion of investor protection deepened financial markets and aided industrialization.

Keywords: Women’s liberation, financial innovation, investor protection, economic growth.

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“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 1870.¹

1 Introduction

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 married women rights in the second half of the 19th century. Before this “women’s liberation,” married women were subject to the laws of coverture.² 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, and bonds. This paper explores the economic ramifications of coverture’s demise, and the resultant expansion of investor protection to women. We exploit the staggered nature of coverture’s demise across US states to show that these rights had a large impact on household portfolios, credit markets, and labor allocations.

Under coverture, property was divided into multiple types. Moveable property, including money, stocks, bonds, furniture, and livestock, became the husband’s property entirely upon marriage.³ He could sell or give the property away, and even bequeath it to others.⁴ Real assets, such as land and structures, were placed under the husband’s partial control while remaining in the wife’s name. He could

¹ British House of Commons, April 14th, 1870 (Hansard 1870).

²Coverture 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.

³Moveable property is sometimes referred to as “personal property”. We use these terms interchangeably.

⁴There was a limitation on this freedom for “paraphernalia”, which was moveable property such as clothing and jewelry. Husbands could sell or give away paraphernalia, but not bequeath it.

manage the assets as he saw fit, including the income generated by the assets, but he could not sell or bequeath the property without his wife's consent.⁵

By differentially allocating property rights, coverture affected portfolio incentives not only of women, but of parents wishing to bequeath or gift assets to their daughters.⁶ Therefore, our first prediction is that undoing coverture should cause portfolios to shift towards moveable assets, such as financial assets, as removing legal constraints allowed households to purchase assets with higher returns or diversify their portfolios.⁷ This shift in portfolios towards moveable assets represents an increase in the supply of financial assets. Accordingly, our second prediction is that we expect bank deposits, and therefore loans, to increase, along with a reduction in interest rates, after rights are granted. An increase in the supply of loanable funds should aid industrialization, as entrepreneurs find capital to be cheaper and thus invest more. Greater industrialization yields a sectoral reallocation of workers. Accordingly, our third prediction is that coverture's demise leads to a shift in the labor force away from agriculture. Moreover, even within the non agricultural sector, cheaper capital causes greater investment in more capital intensive industries. Thus, our fourth and final prediction is that rights lead to a relative increase in employment in capital intensive industries.

Using the staggered nature of coverture's demise across US states we perform four sets of empirical exercises in order to check these four predictions. Massachusetts was the first state to give rights, in 1846, and by 1920 all but four states

⁵See Blackstone (1896) for the laws of coverture. For a summary of the general responsibilities husbands and wives had to one another under coverture, see Basch (1982) Tables 1 and 2.

⁶Consider a father who wants to bequeath his estate to his daughter upon death. He would face an incentive to hold his wealth in real assets. Uncertainty over the timing of death, along with portfolio adjustment costs, amplifies this concern. Indeed, parents did bequeath to daughters in the US. After the War of Independence, primogeniture, the norm of bequeathing all assets to the first born son, was abandoned. The default became to split inheritances of both types of assets equally among children, including girls. Indeed, Shamma, Salmon and Dahlin (1987) find that "By 1800 in most states, sons and daughters received equal shares in real and personal property; there was no longer any meaningful distinction for purposes of children's inheritances" (p.67).

⁷Interestingly, Baskerville (2008) studies the effects of women's property rights in Canada, and argues that there was a "silent revolution" of women becoming active in capital markets. In particular, he concludes that, after rights were granted, "If 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" (p. 237).

had granted rights. Accordingly, we use 1850–1920 as our sample time period whenever the data permit.

Endogeneity of women’s rights, namely omitted variable bias and reverse causality, is a natural concern. Accordingly, in Section 4, we discuss these issues, using the historical record of British parliamentary debate, the academic literature on these rights, and empirical evidence from the US. We argue that our exercises can be viewed as capturing the causal impact of granting women rights on economic outcomes.⁸

Our first exercise uses population census data from 1860 and 1870.⁹ We show that households in states that granted rights during this decade increased their holdings of moveable property as a fraction of their portfolio relative to households in states that did not change women’s economic status.¹⁰ Our estimates suggest that the fraction of portfolios dedicated to moveables increased by 5–7.5 percent. We show that our results are driven by an increase in holdings of moveable assets, rather than a decrease in real assets.¹¹

In our second set of exercises, we use state-year level data on interest rates (1878–1920) and National Bank loans and deposits (1865–1920), in order to show that the granting of women’s economic rights is associated with lower interest rates and an increase in financial intermediation. These empirical results are consistent

⁸States practically never retracted rights once they were given, presumably since they caused 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 in 1870. Dudley Field of New York, which granted rights 10 years prior, argued that “Scarcely 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).

⁹1860 and 1870 were the only years in which the census asked about both real and moveable asset holdings. The fact that the census asked about portfolio holdings of these specific types of assets, around the time coverture began to unravel, is suggestive of their importance.

¹⁰Combs (2005) uses a sample of British shopkeepers wives to show how property rights affected portfolio holdings in England. In contrast, we are able to exploit cross-state variation in the timing of these rights to explore changes in portfolio holdings among the general population.

¹¹The shift from real to moveable assets may have had significant economic consequences besides sectoral reallocation. Ferrie (2003) shows that higher holdings of moveable assets were associated with lower mortality rates, while there was no relationship between mortality and real asset holding. He suggests that the mechanism is through the greater ability of moveable assets to smooth consumption.

with a positive shock to the supply of loanable funds. Our results suggest that interest rates dropped by 50–80 basis points, in real terms, as a result of women’s rights, along with a significant increase in bank deposits and loans.¹²

In our third set of exercises, we use US population census data (1850–1920) to look at the fraction of male employment in the non-agricultural sector, a measure of development. Before rights are granted, there is no trend in development. Once rights are given, there is a statistically significant increase in the fraction of the labor force working in the non-agricultural sector. Our estimates suggest that granting women rights caused sectoral reallocation on the order of 5–7 percentage points of the male labor force by two decades after rights were given.

In our fourth and last set of exercises, we use the 1850 US 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 most to least capital intensive industries by state year (1850–1920). We find that this ratio exhibits no trend prior to rights, and dynamically increases after rights are granted. The result comes from a roughly 50% increase in employment in the most capital intensive industries by two decades after rights are granted.¹³

Implicitly, our theory is dependent on financial markets being important for industrialization during the 19th century, which was indeed the case. For instance, the literature has documented the importance of finance for New England textile firms (Davis 1960). Another example is the railroad. Investment in railroads between 1850 and 1880 in the US 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). Turning towards more standard macro measures of finance depth (King and Levine 1993), we find that by 1890 deposits in National banks alone were approximately 10% of GDP, and grew to 15% by 1910.¹⁴ This is comparable to the 17% ratio for all depository

¹²As a point of reference, the average real interest rate in our sample is 8 percent. Thus, a reduction of 50–80 basis points represents a 6–10 percent reduction in the real interest rate.

¹³There is little measurable change in employment in industries with low capital intensity.

¹⁴Including other financial institutions, such as state banks, would increase this number. We

institutions in 2000.¹⁵ Finally, Bodenhorn (2003) provides a summary of banking at the state level in the US during this time period, and the central role that banks played in the boom-bust cycle of the 19th century.

Our paper is related to the literature that documents how different legal systems offer different level of investors' protection, the effect these legal systems on the size of the financial sector and, in turn, the effect the size of the financial sector has on development. La Porta, Lopez-de Silanes, Shleifer and Vishny (1998) examine legal rules and their enforcement across countries. They found large variation across types of legal systems, such as between civil and common laws. La Porta, Lopez-de Silanes, Shleifer and Vishny (1997) use this variation in investors' protection to show that poorer investor protection is associated with smaller and narrower capital markets.^{16,17} We view coverture as a form of law that provided little protection to female investors. As such, we argue that coverture led to underinvestment in capital on part of women. King and Levine (1993), Levine (1997), Acemoglu and Zilibotti (1997) and Rajan and Zingales (1998), all of found that more financial intermediation positively affects development. Similarly, we find that the financial market deepening resulting from coverture's demise led to industrialization.

We proceed as follows. Section 2 describes our theoretical predictions for the economic effects of coverture's demise. Section 3 describes our data, both the timing of rights by states and our main outcomes of interest. Section 4 discusses the empirical strategy in this paper, including the implicit assumptions behind our exercises, as well as the econometric concerns of omitted variable bias and reverse causality in our cross-state regressions. Section 5 describes our regressions and results, including a variety of robustness exercises and randomization checks. We conclude in Section 6.

calculate this number using data from the Office of the Comptroller of the Currency, described in Section 3.

¹⁵ 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 (US).

¹⁶ Relatedly, Demirgüç-Kunt and Levine (2005) study how legal origin relates to the obstacles firms face in obtaining external finance.

¹⁷ More recently, Giannetti (2003), Brown, Martinsson and Petersen (2013), and Ponticelli and Alencar (2016) all show that investor protection leads to financial market deepening.

2 Theoretical Framework and Testable Hypotheses

In this section, we describe the hypotheses to be tested on women's liberation influencing financial markets and development. The basic premise is that coverture distorted portfolio allocations of single women, or parents of women, away from moveable assets. Accordingly, the demise of coverture should lead to a reallocation of portfolios towards moveable assets, as people pursue higher returns and greater diversification.

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

Moveable assets include stocks, bonds, and money in banks, rather than real estate. Accordingly, we expect this portfolio reallocation to increase deposits in banks, loans from banks, and reduce bank interest rates, as portfolios become reallocated towards moveable assets, representing 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.*

Greater financial intermediation and lower capital costs lead to growth in the capital intensive areas of the economy in a process of industrialization. Under the assumption that agriculture is less capital intensive than the rest of the economy, we expect to see labor shift away from agriculture.

PREDICTION 3 *Women's rights lead to an increase in the fraction of the labor force in non-agricultural employment.*

Finally, if capital becomes cheaper, we expect to see a reallocation of labor even within the non-agricultural sector. Specifically, we expect to see relatively greater employment in capital intensive industries as compared to industries with low capital intensity.¹⁸

¹⁸This is as opposed to growth in total factor productivity (TFP). If TFP is growing at the same rate for various industries, we would not expect to see a reallocation of labor.

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

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

3 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 US. In our empirical exercises, these rights will be our main explanatory variable of interest. We then discuss the data sources for the dependent variables in our empirical exercises.

3.1 Women's Property Rights in the US

Data on the timing of women's liberation by state comes from Geddes and Lueck (2002).¹⁹ They coded the year in which states first granted women rights over both their own property and their labor earnings.²⁰ We call this variable *rights*.²¹

The history of married women's economic rights began with "debt statutes", which shielded women's assets from their husbands' creditors, but did not truly expand women's economic rights. These acts encouraged fraud, as men would give their wives assets, which in turn were protected from creditors (VanBurkleo 2001, pp. 132).²² In general, laws were passed and updated over time.²³ We are interested in the date at which women no longer had a disincentive to hold moveable property rather than real property.

For instance, Alabama passed laws in 1846, 1848, and 1867. The first laws gave women separate estates that could be protected from their husbands' creditors.

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

²⁰For details, see Geddes and Lueck (2000).

²¹For more on the evolution of married women's property rights, see VanBurkleo (2001), pp. 125-138.

²²For an interesting economic analysis of the effects of these laws on household risk taking, see Koudijs and Salisbury (2016).

²³Interestingly, in the US, Michigan undid their 1844 property rights in 1846 (VanBurkleo 2001, p. 127), before regranting them in 1855.

The 1867 law gave women the right to own all types of assets, real and moveable, that she was gifted or inherited, but not necessarily dispose or manage them on her own. That is, her husband could no longer expropriate her assets, even if he had management rights over them.²⁴ Thus, there was no longer an incentive for women to hold real rather than moveable property. Accordingly, we use 1867 as the property rights date in Alabama, as in Geddes and Lueck (2002). However, true management rights only came later, as discussed in Geddes and Tennyson (2013).²⁵

Figure 1 shows the date that each state granted women rights. Massachusetts was the first state to grant rights, in 1846. Ideally, we would start our analysis in 1840. However, Ruggles, Alexander, Genadek, Goeken, Schroeder and Sobek (2010) has US census data beginning only in 1850 that is comparable over time. Accordingly, our analysis begins in 1850. By 1920 rights were granted in all states except Florida (1943), Arizona (1973), New Mexico (1973), and Louisiana (1980). However, we end in 1920 to preclude influence from the Great Depression and World War II.²⁶ Figure 1 shows spatial correlation of the timing of rights. We detail how we account for this below. Also interesting is that approximately a third of states gave rights between 1870 and 1880. We perform robustness exercises where we drop these states from our sample.

²⁴He was required to manage the property for her benefit. Kelly (1882) states “The separate statutory estate vests in the husband as trustee to manage and control it, together with the income, rents, and profits, but without any right or title thereto; whilst he is not required to account for the rents, income and profits of the separate statutory estate, yet they should be used for the wife’s benefit, and the support and maintenance of the family, and if he violates this trust he may be removed.” (pp. 295-296).

²⁵Similarly, rights were granted in waves in England. Married women received partial rights over property in 1870, specifically with regards certain types of savings/investment accounts, and inheritances up to 200 pounds, though the reform was not always upheld in court (Holcombe 1983, pp. 178-182). There was an update in 1874 to the 1870 law in order prevent fraud (Holcombe 1983, p. 191). A further and more significant update to property rights came in 1882 (Holcombe 1983, pp. 184-205), which more or less granted women equal rights as men. There were further minor updates through the 20th century (Holcombe 1983, pp. 178-182).

²⁶Our results are virtually identical when excluding these four states that did not give rights before 1920, but for brevity this robustness exercise is omitted below.

3.2 Data on Outcome Variables

We perform four sets of empirical exercises. The first shows the impact of women's rights on portfolio allocations at the household level. Accordingly, we begin by discussing the data related to constructing household portfolios. The second exercise shows the impact of women's rights on state level credit markets, consistent with changes in household portfolio reallocations. We therefore continue by describing the data on state-year interest rates and National bank deposits and loans. Our third exercise studies the dynamic impact of these financial market changes on labor allocations, by showing how women's rights affected the fraction of male employment in agriculture versus non-agriculture. We describe our measurement of state-year labor allocations with respect to the timing of rights. Our final exercise expands on labor allocations by looking within non-agricultural employment to show how rights led to a relative increase in employment in capital intensive industries versus low capital intensive industries. Thus, we describe how we measure which industries are and are not capital intensive. For details of the various other control variables used in our empirical analysis, see Appendix A.

3.2.1 Household Portfolios

Our analysis of how rights affected portfolio choices uses data from the US census.²⁷ In 1860 and 1870, and only these two years, the census asked households about holdings of real assets and moveable assets at the household level.²⁸ We deflate these numbers 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

²⁷The census data that we use refers to 'moveable' property as 'personal' property.

²⁸Technically, 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 this data look at household level data as it seems that most heads of households simply reported all assets as belonging to them. Indeed, Rosenbloom and Stutes (2008) argues that "Many 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 credits, also use this data at the household level rather than breaking down the assets between husbands and wives.

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 “contemporary dollar value of all stocks, bonds, mortgages, notes, livestock, plate, jewels, and furniture...” in 1870, and included the value of slaves in 1860.²⁹ We restrict our sample to married households that have non-zero total wealth (real property plus moveable property).³⁰

3.2.2 Credit Markets

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

Interest rate data is from Bodenhorn (1995), who has state-year level data from 1878–1920. 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 non-loan earnings from the numerator and various non-loan 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 1865–1920.³¹ We calculate the change in real bank deposits per capital and real bank loans per capital,

²⁹Moveable property of values less than \$100 were not recorded in 1870. Accordingly, for consistency, we recode any observations of less than \$100 in 1860, in real terms, to be \$0. Our results are unaffected by this censoring.

³⁰Including people without any wealth does not affect our main results. However, one of our dependent variables is the fraction of wealth that is moveable wealth, which is undefined when total wealth is zero.

³¹As discussed in Benmelech and Moskowitz (2010), ideally we would like to use data on both national and state banks, but it does not seem that data on state banks exist. The Comptroller of the Currency only supervised national banks, though it seems reasonable to assume that loan

by state-year, following Benmelech and Moskowitz (2010) in our choice of variables. We refer to these variables as “Deposits” and “Loans”, respectively. We calculate real interest rates, loans, and deposits by using a price deflator from Burgess (1920).

3.2.3 Labor Allocations

We use data from the decennial US population census (Ruggles et al. 2010) from 1850 to 1920 to calculate the labor force allocation between agriculture and non-agriculture. We calculate the fraction of men in the labor force, aged 20–60, who are in the non-agricultural sector by state-year, as defined by the IND1950 variable. For our baseline exercise, we consider agriculture to be IND1950 taking the value of 105, and count everyone else to be employed in the non-agricultural sector.

For our robustness exercise, we exclude from non-agricultural employment a broader set of people. The idea is that perhaps a broader definition of agriculture, such as forestry and fishing, could be argued to not be affected by distorted portfolio incentives, as women could own land. Additionally, since women could own structures, perhaps retail sectors, as in mom and pop shops, would not be affected. Accordingly, we excluded from non-agricultural employment those employed in Forestry (code 116), Fisheries (code 126), and a list of industries classified as “Retail Trade” such as food stores, shoe stores etc (codes 636–699). Finally we excluded personal services (e.g., dressmaking shops) and professional services (e.g., hospitals), codes 826 till the end of the index).

3.2.4 Capital Intensity

In order 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 time period, and thus is not influenced by women’s

and deposit data at national and state banks would be highly correlated. See footnote 11 in their paper for more.

rights.³² This census surveyed 5,550 firms, and included data such as labor and capital use, as well as three digit SIC code industries.

We collapse the data into 23 unique two digit SIC code industries.³³ For each industry, we calculate capital per worker as the the total amount of capital across the firms divided by the total labor input, and keep the industries in which there are at least 30 firms, leaving 16 industries. We take the top and bottom quarter of these industries to be the high and low capital intensive industries, respectively. See Appendix A for the list of most and least capital intensive industries, as well as our crosswalk between the 2 digit SIC code and IND1950 code.

4 Empirical Strategy

In this section, we explain the validity of using cross-state variation in the timing of women's rights in the US in order to provide empirical evidence to validate the model predictions studied in Section 2. We first discuss the implicit assumptions behind this exercise, specifically with respect to the openness of capital markets, and support this assumption with data and literature on US regional development and capital markets in our sample time period. Finally, we discuss the reasons why women were granted property rights along with the implications of various theories for potential omitted variable bias and reverse causality.

Our theory is dependent 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. This assumption is consistent with the realities of the time. It is well known that banking was highly regulated at the state level during the 19th century.³⁴ Empirically, there were large variations in interest rates. For instance, Breckenridge (1898) documents regional dispersion in interest rates of first class double-name commercial paper in the 1890s.³⁵ We

³²Massachusetts was the only state to give rights before this census, and only 4 years prior.

³³These are the 23 industries left after excluding firms in which industry is not known. Miscellaneous repair shops (SIC code 17), which we cannot map into population census categories, is dropped.

³⁴For an thorough history of US banking, see Calomiris (2000).

³⁵This is high quality corporate paper. Breckenridge argues that the comparison across cities of first class double-name commercial paper takes into account default risk, and thus compares discount rates across the US.

present a snapshot of his findings in Figure 2. 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 3.2.2, Figure 3 displays the evolution of interest rates over time by region. The figure shows 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. Secondly, there is a clear differentiation in the time trend across regions.³⁶

Accordingly, in all sets of exercises we include specifications with region-year fixed effects, which additionally helps control for differential regional effects of the US Civil War. While we do not take a particular stand, there is a large literature on the source of these regional variations in interest rates and why capital did not flow to correct imbalances.³⁷ Considering these realities, we continue with our analysis under the assumption of imperfect capital market integration.³⁸

We now turn to the question of why these rights were given. Our concerns are omitted variable bias and reverse causality. If rights were granted due to a different economic development, and that development in turn causes portfolio changes, financial market deepening, and sectoral reallocation of workers, then the empirical exercises we perform are not necessarily capturing the effects of rights, rather the other economic changes.

We begin with the existing literature on why men gave women economic rights. Doepke and Tertilt (2009) argue that men wanted to grant rights in order to give *other men's wives* power, which in turn would increase investment in the human capital of their children. Geddes and Lueck (2002) argue that coverture decreased women's incentive to work, as their earnings went to their husbands.³⁹ Notice

³⁶In addition to financial dispersion, it has been noted in the literature that there was regional price variation in the US during the 19th century (Coelho and Shepherd 1974, Haines 1989). The available regional price indices, however, do not cover the entire time period we use in our sample, so we cannot use them in our analysis.

³⁷For a summary and contribution to this literature, see Landon-Lane and Rockoff (2007).

³⁸Benmelech, Bergman and Seru (2011) show that regions in the US still depend on local finance. For instance, they show that a financial shock in Japan in the 1990s affected US labor market conditions in regions where Japanese banks operated.

³⁹This may have been a significant mechanism in England, where married women's labor force participation were high at this time, but they were under 5% in the US, making this mechanism less relevant. As we discuss below, marriage rates and age of marriage were not affected by these

that neither of these papers predict observed portfolio shifts, reductions in interest rates, increases in financial intermediation, or a sectoral reallocation of labor.⁴⁰ Therefore, if either of these were the reason to grant women rights, then the empirical exercises we explore can reasonably take the granting of women's rights as exogenous.

What if men granted women rights in order to undo the financial market distortions that we document? For instance, total factor productivity (TFP) growth in the non-agricultural sectors of the economy may cause a growth in the demand for capital that outstrips the supply of capital under coverture, when women (or their parents) have a disincentive to purchase anything but real assets. Eventually, this distortion would be so large that men might give women rights in order to cause portfolio reallocations, financial market deepening, and thus permit a sectoral reallocation of labor towards the more productive areas of the economy. At first glance, this is not an issue; under this story, we are simply estimating the effects of giving rights that men were hoping to induce. However, under this hypothesis, TFP is an omitted variable that is linked to portfolios, credit markets, and labor allocations, while also being correlated with women's rights. To address this issue, we control for TFP in the non-agricultural sector relative to the agricultural sector when studying portfolio allocations and labor allocations.⁴¹ We do not include this variable when looking at credit markets, as we only have TFP data on decennial census years, while our data on credit markets is annual.⁴² However, note that our results suggest that women's rights cause lower interest rates and more credit, consistent with an increase in the supply of capital. Growth

laws, so the margin between married and single women's labor force participation rates was not an issue.

⁴⁰Doepke and Tertilt (2009) predict rising human capital as a result of women's rights, which is presumably consistent with sectoral reallocation, simply a different mechanism than the one we discuss that is orthogonal to the credit market shifts observed. Geddes, Lueck and Tennyson (2012) find that the law changes had an effect on female schooling (relative to male). To control for this, we include the fraction of females in school and males in school, by state year, in our regressions.

⁴¹Turner, Tamura, Mulholland and Baier (2007), Turner, Tamura, Schoellman and Mulholland (2011), and Turner, Tamura and Mulholland (2013) develop state-level time series data that allows us to compute TFP for the agricultural and non-agricultural sectors. We thank the authors for making their data available to us.

⁴²The same issue arises with our other control variables, such as the fraction of men or women in school.

of TFP would presumably result in more credit and, if anything, *higher* interest rates consistent with a demand shock.⁴³ Furthermore, we include region-year fixed effects in all of our exercises, which helps address issues of differential development.

Finally, we turn to the issue of reverse causality. What if people suddenly started allocating more assets towards moveable property, which in turn caused the other economic changes we document, and then lobbied for women to be given property rights in order to keep the assets in their families? To see if this was a concern, we read the debate in the British Parliament on granting women property rights. The debate included great discussions about defending poor women against drunk husbands, and the potentially ill effects of women's rights on the harmony of previously male dominated households.⁴⁴ None of the discussion mentions this mechanism as a reason. Furthermore, we can compare the asset allocations of households in the US in states that granted rights to those that didn't. Table 1 compares the portfolios of households in states that granted rights between 1860 and 1870 to those that did not. The real asset holdings were very similar between these groups of states. However, the states that granted rights had significantly lower moveable assets, negating the reverse causality story.⁴⁵

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

5 Results

In this section, we empirically test the theoretical predictions described in Section 2, taking into account the issues raised in Section 4. First we show that women's rights had a large effect on household portfolios, as in Prediction 1. Then, we show that rights led to financial market deepening, as in Prediction 2. Third, we

⁴³Not including TFP in these regressions suggests that our results are biased towards zero, and thus we are underestimating the effects of rights on interest rates.

⁴⁴For instance, the House of Commons invited Mr. Washbourne, a Harvard law professor, to discuss the impact of women's rights on household harmony in Massachusetts, which had granted rights 25 years before England.

⁴⁵The fact that the households in the switching states saw a large relative rise in moveable property over time is consistent both with the hypothesis of this paper and the omitted variable bias story told above, reinforcing our need to control for TFP.

show that rights led to a dynamic reallocation of labor towards the non-agricultural sector, as in Prediction 3. Finally, we show that, even within the non-agricultural sector, rights led to a reallocation of labor towards capital intensive industries, as in Prediction 4.

5.1 Rights Leads to Portfolio Changes

The first exercise we perform is to 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. These are the only two years in which the census asked about household wealth in general, and the breakdown between real and moveable assets in particular. We study the impact of rights on moveable assets, real assets, and the fraction of a household's wealth that was moveable. See Table 1 for summary statistics on the fraction of people married, the age at marriage, the age gap between new husbands and wives, moveable assets, real assets, and the fraction of a households wealth that were moveable assets. Notice that approximately 95% of people were married at this time. Accordingly, we restrict attention to married households as to not get into the differences between single and married households.⁴⁶ We confirm that this restriction is innocuous below.

Between 1860 and 1870 six states gave married women rights: Colorado (1868), Illinois (1869), Minnesota (1869), New Hampshire (1867), Ohio (1861), and Wyoming (1869).⁴⁷ These states comprised 18% of our sample in 1860 and 19% in 1870.

Since we only use married households, we begin by documenting that rights did not affect the selection into marriage. Specifically we check the effects of rights on the probability of being married, as captured by a dummy variable denoted "Married", the age of newly married people, denoted "Age", and the age gap between newly married spouses, denoted "Age Gap". Accordingly, we

⁴⁶As noted in the introduction, the vast majority of unmarried women were widows or spinsters.

⁴⁷Wyoming is not used in this exercise, as its population was only 9,000 in 1870.

run regressions of the following structure:

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

where Y_{ist} is either Married, Age, or Age Gap, for individual i in state s in year t . $rights_{st}$ is a dummy variable denoting whether or not state s had given rights by year t . λ_s is a set of state fixed effects. Considering that our exercise is only using 1860 and 1870, we use the 1860 state borders for our fixed effects.⁴⁸ d_i are year fixed effects or region-year fixed effects for each region i , depending on the specification. T_{st} is a dummy variable that the state the individual was in was a territory at the time. X_{ist} is a vector of controls that includes age fixed effects, and race fixed effects. We use census person weights and cluster the standard errors by state.

Table 3 shows the results. Column 1 shows the effect of rights on the likelihood of an individual to be married. Column 2 repeats Column 1 with region-year fixed effects. Column 3 repeats Column 1, but restricts the sample to those aged 20-40. These three columns show that rights had no impact on the likelihood of an individual to be married. Column 4 shows the effects of rights on the age of those married within the 12 months preceding June 1 of the census year. Column 5 repeats Column 4 with region-year fixed effects. These two columns show that rights did not influence the age of newly married individuals. Column 6 shows the effects of rights on the age gap between newly married husband and wife. Finally, Column 7 repeats Column 6 with region-year fixed effects. These two columns show that rights did not influence the age gap between husband and wife. Thus, we conclude that women's rights did not have a meaningful impact on the selection into marriage. This validates our restriction of using only married households.

⁴⁸Areas that were not yet states but were divided into more than one territory were assigned to a common territory based on where most of the landmass was. For instance, Wyoming is included in our fixed effect for the Nebraska Territory, even though parts of modern day Wyoming were in Washington Territory or Utah Territory, as that was where most of its landmass was located. Colorado was reasonably equally divided between Kansas Territory, Nebraska Territory, Utah Territory, and New Mexico Territory, so we included it with Kansas Territory as that is where Denver was located. We also give separate state fixed effects to Virginia and West Virginia, which were soon to separate due to the Civil War, though our results are not sensitive to this choice.

Turning to the impact of rights on portfolios, we apply a difference in difference estimator to comparing the portfolios of households in states that granted rights between 1860 and 1870 and those that did not switch their legal regime during this decade. Ideally, we would control for existing trends between these two groups of states, but unfortunately we only have data for two years. To estimate the impact of rights, we run regressions of the following structure:

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

where Y_{hst} is either the fraction of a household's portfolio in moveable assets, the log of their moveable asset holdings, or the log of their real asset holdings, for household h in state s in year t .⁴⁹ $Switch_s$ is a dummy variable equal to one if state s gave women rights between 1860 and 1870. $Post$ is a dummy variable equal to one in 1870. Our parameter of interest is γ , which captures the effect of granting rights, after netting out general changes between 1860 and 1870. λ_s is a set of state fixed effects. Considering that our exercise is only using 1860 and 1870, we use the 1860 state borders for our fixed effects. Z_{st} is a vector of controls for state s in year t that includes a dummy variable that the state is in a territory, and TFP in the non-agricultural sector relative to the agricultural sector. X_{hst} is a vector of controls that includes age fixed effects interacted with year fixed effects, whether or not the household is on a farm, and whether or not the household is in a metropolitan area.⁵⁰ In some specifications we also include total household wealth in order to see how rights affect portfolio allocations at various parts of the wealth distribution. We use census person weights and cluster the standard errors by state.

Table 4 reports results where the dependent variable is the fraction of household wealth in moveable assets. Column 1 includes a fixed effect for being in a state that grants women rights between 1860 and 1870 ("*Switch*"), a dummy

⁴⁹We add 1 to moveable and real holdings before taking logs.

⁵⁰Living on a farm or in a metropolitan area is presumably endogenous. We include these controls to capture the differential impact of rights on people who live in different areas. The results are robust to not including these controls.

variable for 1870 (*“Post”*), and their interaction. Column 2 adds state fixed effects. Column 3 adds the rest of our controls except for total assets. Column 4 replaces the 1870 dummy variable with division and division-year fixed effects. Our estimates imply an increase of 1.8–3.2 percentage point in the fraction of a household’s wealth allocated to moveable assets. Given that the switching states originally had about 42 percent of household portfolios in moveables, this is approximately a 5–7.5 percent increase.

Turning to robustness, Column 5 of Table 4 repeats Column 4, using only states that gave rights after 1870 as the control group. Column 6 repeats Column 4 using total household wealth as an independent variable in order to show how portfolios change holding wealth constant. The results are qualitatively and quantitatively similar to the main results.

In order to understand whether the change in household portfolios allocated to moveable assets is driven by changes in household holdings of moveable or real assets, Table 5 repeats Columns 1–4 of Table 4 twice. Columns 1–4 use the log of household moveable asset holdings as the dependent variable, while Columns 5–8 use the log of household real asset holdings as the dependent variable. Granting rights increases household holdings of moveable assets and does little to real asset holdings. The result loses statistical significance when adding division and division-year fixed effects.⁵¹

Thus, we find that granting women rights significantly increased the fraction of household wealth allocated to moveable assets, consistent with Prediction 1, with the result driven by an increase in holdings of moveable assets, rather than a decrease in holdings of real assets.

5.2 Rights Leads to Financial Market Deepening

We now show the relationship between granting economic rights to married women and financial markets. We find that granting women rights yields lower interest rates. We then show that rights lead to greater financial intermediation, as measured by bank deposits and loans. These results are consistent with the

⁵¹On one hand, the increase in moveable assets seems to be less strong, on the other hand, it seems more likely that real estate holdings decrease, once division-year fixed effects are included.

idea that granting women rights leads to a positive supply shock in financial markets.

Table 2 reports summary statistics for the real interest rate, deposits, and loans. Figure 4 plots state-year interest rate observations by the number of years before or after a state gave rights, net of year fixed effects. The figure shows non-parametric 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 were around a constant level with no clear trend. In contrast, once rights are granted, the interest rate falls immediately and continues to fall further over time.⁵²

In order to confirm that the implications of Figure 4 is robust to controls, we continue with a more formal analysis showing the relationship between rights and financial variables. Our regression specification is of the form:

$$(1) \quad Y_{st} = \alpha \cdot rights_{st} + \lambda_s + d_{it} + X_{st} + \epsilon_{st},$$

where Y_{st} is either the interest rate, deposits, or loans, in state s and year t . $rights_{st}$ is a dummy variable denoting whether or not state s had given rights by year t . λ_s is a set of state fixed effects.⁵³ d_{it} are either year fixed effects or region-year fixed effects for each region i , depending on the specification. X_{st} is a vector of controls that includes the fraction of neighboring states that have given rights, and a dummy variable for whether or not the state was part of a territory. We control for the fraction of neighboring states that had granted rights as to control for the potential impact of credit market openness.⁵⁴ We use population weights in these regressions and cluster standard errors at the state level.⁵⁵

We begin by describing the results when the dependent variable is the real in-

⁵²Notice that this figure also shows a decrease in heteroskedasticity in interest rates after rights were granted. It is reasonable to assume that capital deepening leads to greater diversification, and thus less uncertainty (volatility) in growth and interest rates. Indeed, Acemoglu and Zilibotti (1997) develop a theory of development that links the degree of market incompleteness to capital accumulation and growth.

⁵³Since our exercises begin in 1865, we use the political boundaries from 1860 as before.

⁵⁴For instance, if New York grants women's rights, and thus improves their financial markets, this may impact the financial markets of Connecticut, if financial markets are not completely closed. This variable allows us to control for this potential effect.

⁵⁵Population for each state is linearly interpolated between census years.

terest rate. Table 6 shows the results of these regressions. Column 1 shows the baseline regression of interest rates on rights, with state and year fixed effects. Column 2 replaces year fixed effects with region-year fixed effects. In both 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 50–80 basis points, or roughly 6–10% of the mean interest rate.

Table 6 continues by showing the results of these specifications when the dependent variable is deposits or loans. Columns 4 and 5 follow the same pattern as above, with the dependent variable being the deposits; while Columns 5 and 6 follow this pattern with the dependent variable being the loans. When rights are granted, there is an increase in depositing money 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 using year fixed effects. Quantitatively, the point estimates suggest magnitudes equivalent to about 10% of a standard deviation, or about 30–40% of the mean of the dependent variables.

We next turn to a randomization test where we re-estimate the model in (1), where we randomly assign a date of women's rights to each state. Accordingly, we repeat 50,000 times the regression specifications from Columns 2, 4, and 6. During each iteration we randomly assign a date that a state gave women economic rights, drawn uniformly between 1850 and 1920.

Figure 5 shows the histograms for the estimates of α 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 α , 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 (1) is unlikely to produce biased results. The p-value on the figures suggest that our results were extremely unlikely to be a random occurrence, and, if anything, are even lower than the p-values found in the regressions above.

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

5.3 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.

Table 2 reports summary statistics for the percent of a state's male employment in the non-agricultural sector as well as our control variables described below. Figure 6 shows the time series fraction of male employment in the non-agricultural sector.⁵⁶ The line denoted "National Average" is the average for the entire country. The line denoted "90th Percentile" is the 90th percentile of states, as ranked separately each year, where the ranking is done by the fraction of workers in the non-agricultural sector in each state. The line denoted "10th Percentile" accordingly represents the 10th percentile of states. This figure shows the overall trend towards greater non-agricultural labor as the country developed, as well as a fair amount of cross-state variation. In every year, the 90th percentile was roughly 20 percentage points above the mean, while the 10th percentile was 20 percentage points below the mean. Note that the bottom 10 percentile of states decreased their non-agricultural employment dramatically after the Civil War, recovering to their 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 comes from the US census, conducted once per decade. Thus, we 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). Accordingly, New Jersey is coded as having granted rights in 1880. The advantage

⁵⁶Notice that we are only looking at male labor allocations, as married women barely worked in this time period.

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:

$$(2) \quad L_{st}^{NA} = \sum_k \alpha_k \cdot rights_{st}^k + \lambda_s + d_{it} + X'_{st} \gamma + \epsilon_{st},$$

where L_{st}^{NA} is the fraction of male workers in non-agricultural sectors in state s in year t , $t \in \{1850, 1860, \dots, 1920\}$, and $rights_{st}^k$ is a series of dummy variables set equal to one if a state had granted rights k years ago, where $k \in \{\leq -30, -20, -10, 0, 10, 20, \geq 30\}$.⁵⁷ λ_s are state fixed effects.⁵⁸ As defined above, d_{it} are either year fixed effects or region-year fixed effects for each region i , depending on the regression. X_{st} is a vector of controls that includes a dummy variable that the observation is a territory, the ratio of TFP in the non-agricultural sector to the TFP in the agricultural sector, 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 which have granted rights by year t . We use census population weights and cluster standard errors at the state level.

Table 7 shows the results for these regressions. All estimates are relative to a decade before rights are granted. Column 1 includes year and state fixed effects, as well as a dummy variable for being a territory. Column 2 adds TFP. Column 3 adds the fraction of the population that is female, the fraction of women in school, and the fraction of men in school. Column 4 adds the fraction of the population

⁵⁷ We use increments of 10 as our data is dependent on the decennial census. Recall that for states that granted rights in a non-census year, we round up. Returning to our previous example, New Jersey granted rights in 1874. For our purposes, we round to 1880. Thus, the dummy variable $rights_{st}^0$ takes the value of 1 for New Jersey in 1880, while the dummy variable $rights_{st}^{10}$ takes the value 1 for New Jersey in 1890.

⁵⁸We use the 1850 political borders for the fixed effect as that is the beginning of our sample time period.

under age 35. Column 5 adds the fraction of a state's bordering states that have given rights. Column 6 repeats Column 5 but replaces the year fixed effects with the region-year fixed effects. All estimates include standard errors clustered at the state level.

Before rights are granted, there is no trend in development. That is, given state and year fixed effects, as well as other controls, development did not deviate substantially from what would have been expected. Once rights are given, there is a statistically significant increase in the fraction of the labor force working in the non-agricultural sector. The relationship is dynamic, increasing with respect to the amount of time since rights were granted, with an estimated total increase of 5–7 percentage points by two decades after rights were given. This shows clearly that granting rights is associated with an increase in non-agricultural employment, a measure of development. Graphically, the results for Column 6 are shown in Figure 7.

Next, in Table 8, we perform three robustness exercises, using the specification from Column 6 in Table 7. Column 1 uses an alternative definition of the non-agricultural labor variable, as explained in Section 3.2.3. Column 2 drops all observations from 1890, as the data for that year was imputed.⁵⁹ Column 3 drops all states that gave rights between 1870 and 1880 from the analysis, as explained above.⁶⁰ As can be seen, these exercises show that the results are robust to these checks both qualitatively and quantitatively.

Finally, a question may arise as to whether our results are simply a reflection of the fact that labor was moving relatively continuously from the agricultural to non-agricultural sectors, as shown in Figure 6. That is, if there is a trend towards development, then we might see that the fraction of employment in non-agriculture is increasing dynamically relative to any given date. Although our regressions show no trend before rights were granted, we double check this hypothesis by the following randomization test. We take the regression specification from Column 6 in Table 7, and repeat it 50,000, as before. During each iter-

⁵⁹A fire destroyed census records from 1890.

⁶⁰This exercise eliminates approximately a third of our data. Accordingly, we do simply use year fixed effects, rather than region-year fixed effects, as to reduce the number of estimated parameters.

ation, 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.

Figure 8 shows the histograms for the estimates of α_k 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 higher coefficients on α_k for $k \in \{0, 10, 20, 30+\}$ 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 (2) is unlikely to produce biased results. The p-value on the figure suggests that our results were extremely unlikely to be a random occurrence.

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

5.4 Rights Leads to Labor Reallocations Towards Capital Intensive Industries

We next turn to the dynamic effect of women’s rights on labor allocations within the non-agricultural 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 are granted.⁶¹ We break this result down to show that it is driven by increases in employment in the capital intensive industries, rather than a decrease in employment in industries with low capital intensity.

Table 2 reports summary statistics for the percent of a state’s male employment in industries with high capital intensity, industries with low capital intensity, and their ratio.⁶² Figure 9 shows the time series ratio of male employment in in-

⁶¹This exercise is in the spirit of Rajan and Zingales (1998), who show that greater access to finance aids development of industries which rely on external finance.

⁶²Notice that the average employment in the industries with high capital intensity is 3.66 percent of workers, while employment in the industries with low capital intensity is 2.89 percent of workers. This is as opposed to the total non-agricultural employment, described above, which averaged 54 percent of employment. As such, employment in industries with either high or low capital intensity is approximately 14 percent of non-agricultural employment.

dustries 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 “90th Percentile” is the 90th percentile of states, as ranked separately each year. The line denoted “10th Percentile” accordingly represents the 10th percentile of states. This figure shows the overall trend towards 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:

$$(3) \quad Y_{st} = \sum_k \alpha_k \cdot rights_{st}^k + \lambda_s + d_{it} + X_{st}'\gamma + \epsilon_{st},$$

where Y_{st} is either ratio of male workers in industries with high capital intensity to low capital intensity, the log of the fraction of workers in high intensity industries, or the log of the fraction of workers in low intensity industries, in state s in year t , $t \in \{1850, 1860, \dots, 1920\}$, and $rights_{st}^k$ is a series of dummy variables set equal to one if a state had granted rights k years ago, where $k \in \{\leq -30, -20, -10, 0, 10, 20, \geq 30\}$, as before. λ_s are state fixed effects.⁶³ As defined above, d_{it} are either year fixed effects or region-year fixed effects for each region i , depending on the regression. X_{st} is a vector of the same controls as above; a dummy variable that the observation is a territory, the ratio of TFP in the non-agricultural sector to the TFP in the agricultural sector, 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 which have granted rights by year t . We use census population weights for these regressions as before.

Table 9 shows the results for these regressions. All estimates are relative to a decade before rights are granted. Column 1 includes year and state fixed effects, as well as a dummy variable for being a territory. Column 2 adds all of our remaining controls. Column 3 switches to region-year fixed effects. Column 4 uses

⁶³We use again the 1850 political borders for the fixed effect, as the beginning of our sample time period is 1850.

the specification from Column 3, but switches the dependent variable to be the log of the fraction of workers in industries with high capital intensity. That is, the log of the numerator of the dependent variable in Columns 1–3.⁶⁴ Finally, Column 5 uses the specification from Column 3, but switches the dependent variable to be the log of the fraction of workers in industries with low capital intensity. All estimates include standard errors clustered at the state level.

Columns 1–3 show that before rights are 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 fixed effects, as well as other controls, this ratio did not deviate substantially from what would have been expected. Once rights are given, there is a statistically significant increase in the ratio of the labor force working in the 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 shows that increases in employment in industries with high capital intensity, rather than a decline in employment in industries with low capital intensity, is driving this result. Thus, the result comes from a roughly 50% increase in employment in high capital intensive industries by two decades after rights are granted. There is little change in employment in industries with low capital intensity.

Finally, we perform a randomization exercise as before. We take the regression specification from Columns 4 and 5 in Table 9, and repeat them 50,000. 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.

Figures 10 and 11 shows the histograms for the estimates of α_k along with our estimate (reported above) for the regression using the actual dates that states gave rights, when using Columns 4 and 5, respectively, from Table 9. The vertical line labeled “p-value” shows the fraction of cases in which the regressions with random dates yielded larger coefficients on α_k for $k \in \{0, 10, 20, 30+\}$ than the regression with the actual dates yielded. Running our regressions on random

⁶⁴The use of log allows us to measure the percentage change in the numerator of the main dependent variable.

dates yields estimates centered at zero, indicating that the model in equation (3) is unlikely to produce biased results. Figure 10 shows it to be extremely unlikely that our estimates of the growth of the most capital intensive industries were random, while Figure 11 shows that it is only somewhat likely that the least capital intensive industries grew as a result of rights, confirming our results described above.

To sum up, 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.

6 Concluding Remarks

We have explored one of the greatest extensions of property rights in human history; the demise of coverture with the granting married women property rights in the US. This paper explores the economic ramifications of coverture's demise, and the resultant expansion of investor protection to women. We exploit cross-state variation in the timing of women's rights in the US to study the financial and economic impacts of women's rights. Rights led to dramatic portfolio reallocations, financial market deepening, sectoral reallocation towards non-agriculture, and towards more capital intensive industries.

While the particular example studied here exploits the details of the laws of coverture, the general notion could be applied to any area of the world where women and 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), Turner et al. (2011), and Turner et al. (2013). We follow them in using a Cobb-Douglas production function, with the same elasticities, when calculating a combined TFP for the manufacturing and non-manufacturing-non-agricultural sector as well as the TFP for the agricultural sector. There are 7 state-year observations 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, is from Geddes and Lueck (2002). The fraction of neighboring states with women's economic rights, by year, is by 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 number of people in the interval [20, ∞).

Crosswalk from SIC to IND1950

We now describe 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 5.4.

For the 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 the 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

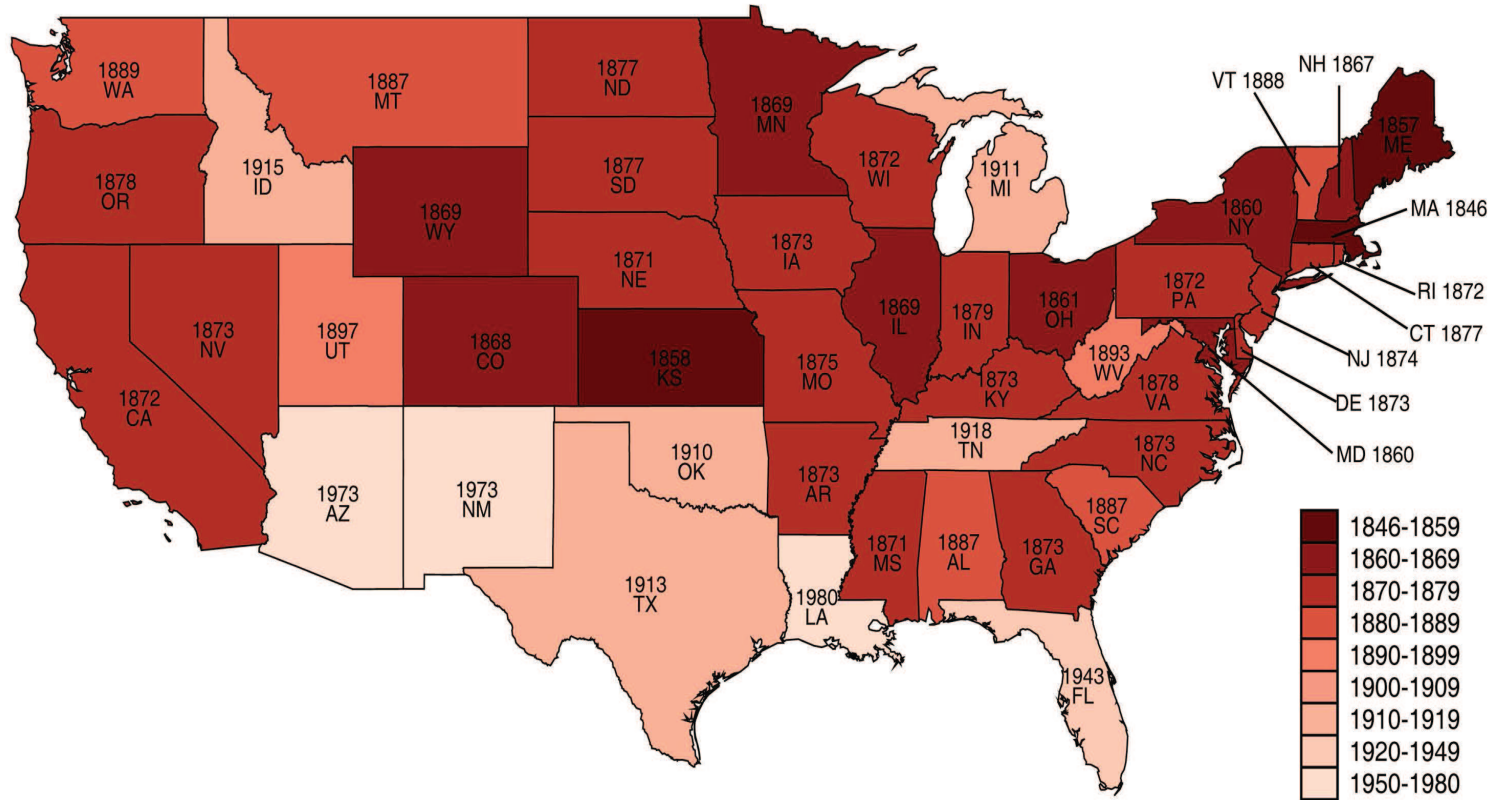


Figure 1: Timing of Women's Rights by State

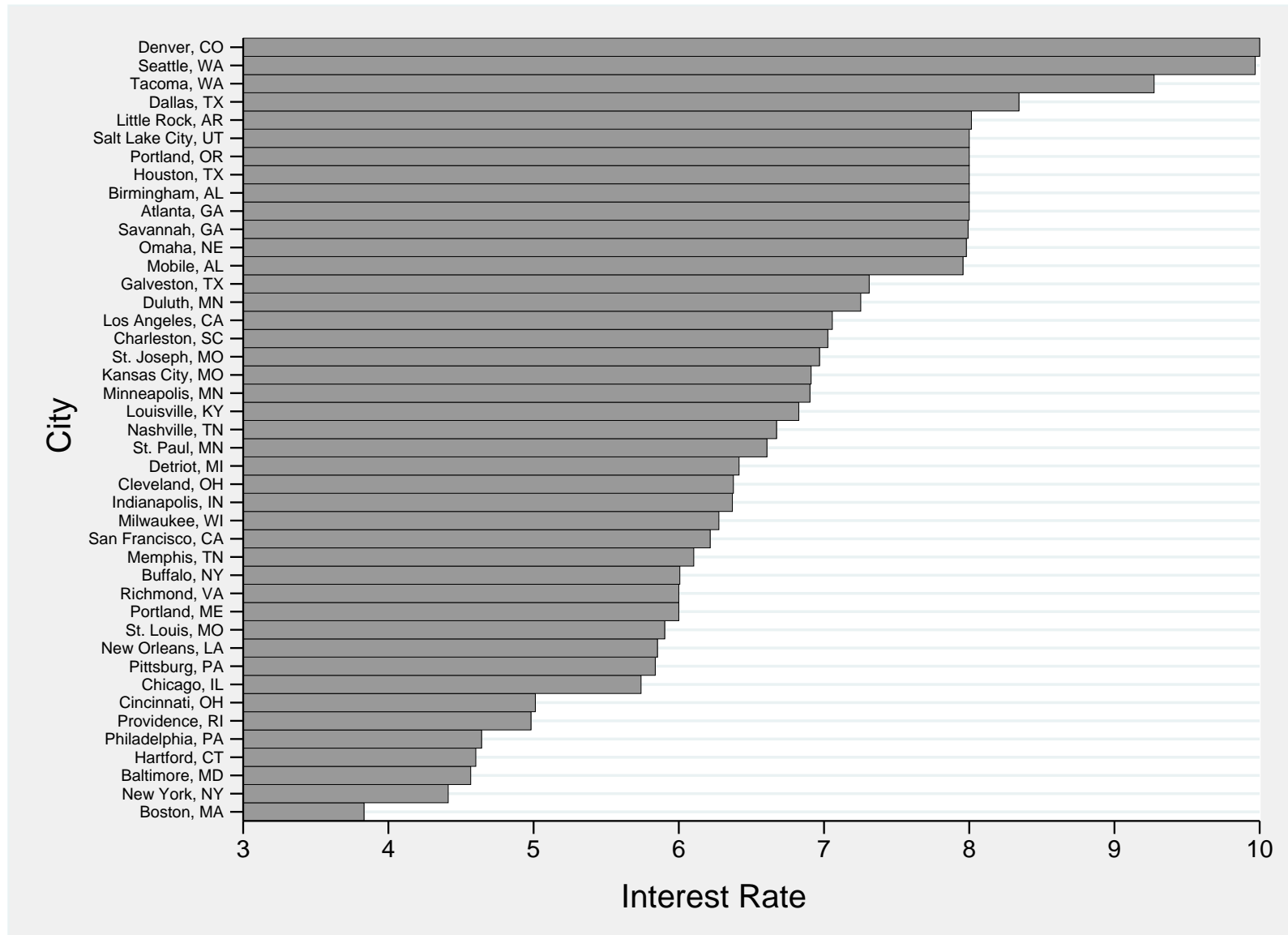


Figure 2: Dispersion of Interest Rates, 1893-1897. Source: Breckenridge (1898).

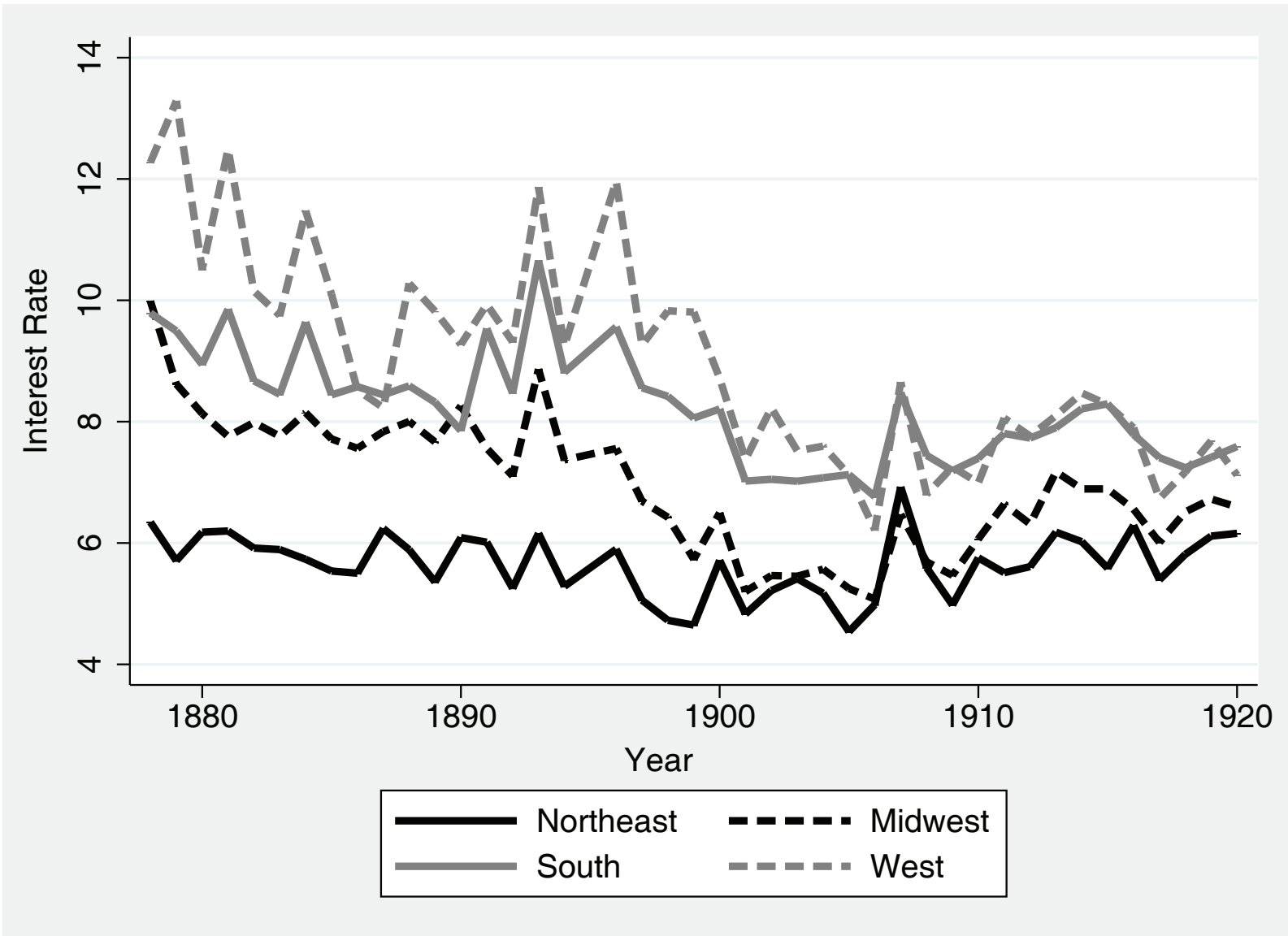


Figure 3: Cross Region Variation in Interest Rates



Figure 4: Interest Rates Before and After Women's Rights, Net of Year Effects

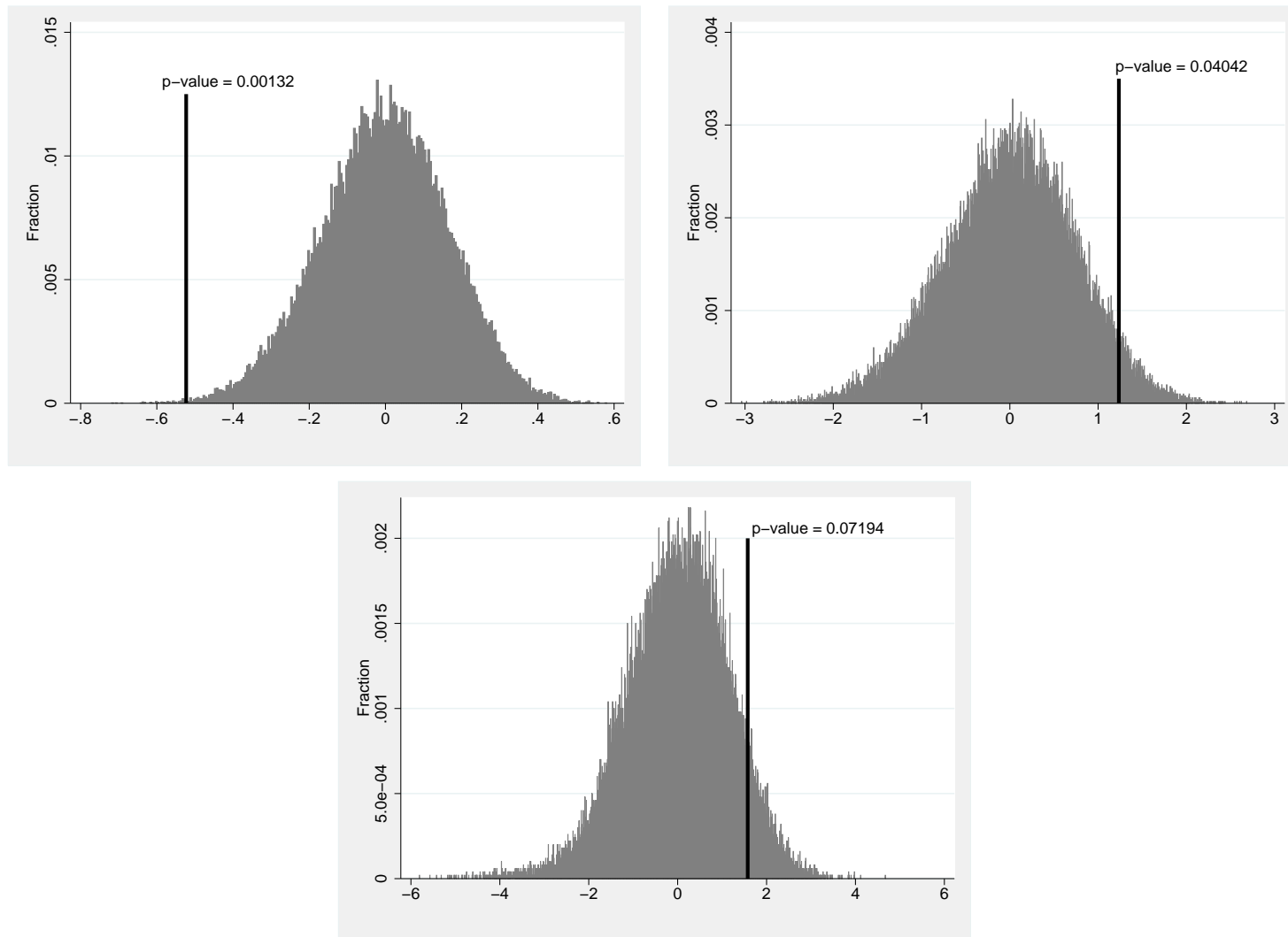


Figure 5: Results of 50,000 simulations of randomly assigned women's liberation dates. The top left shows the coefficient α on the regression for the interest rate, the top right shows the coefficient for deposits, and the bottom shows the coefficient for loans.

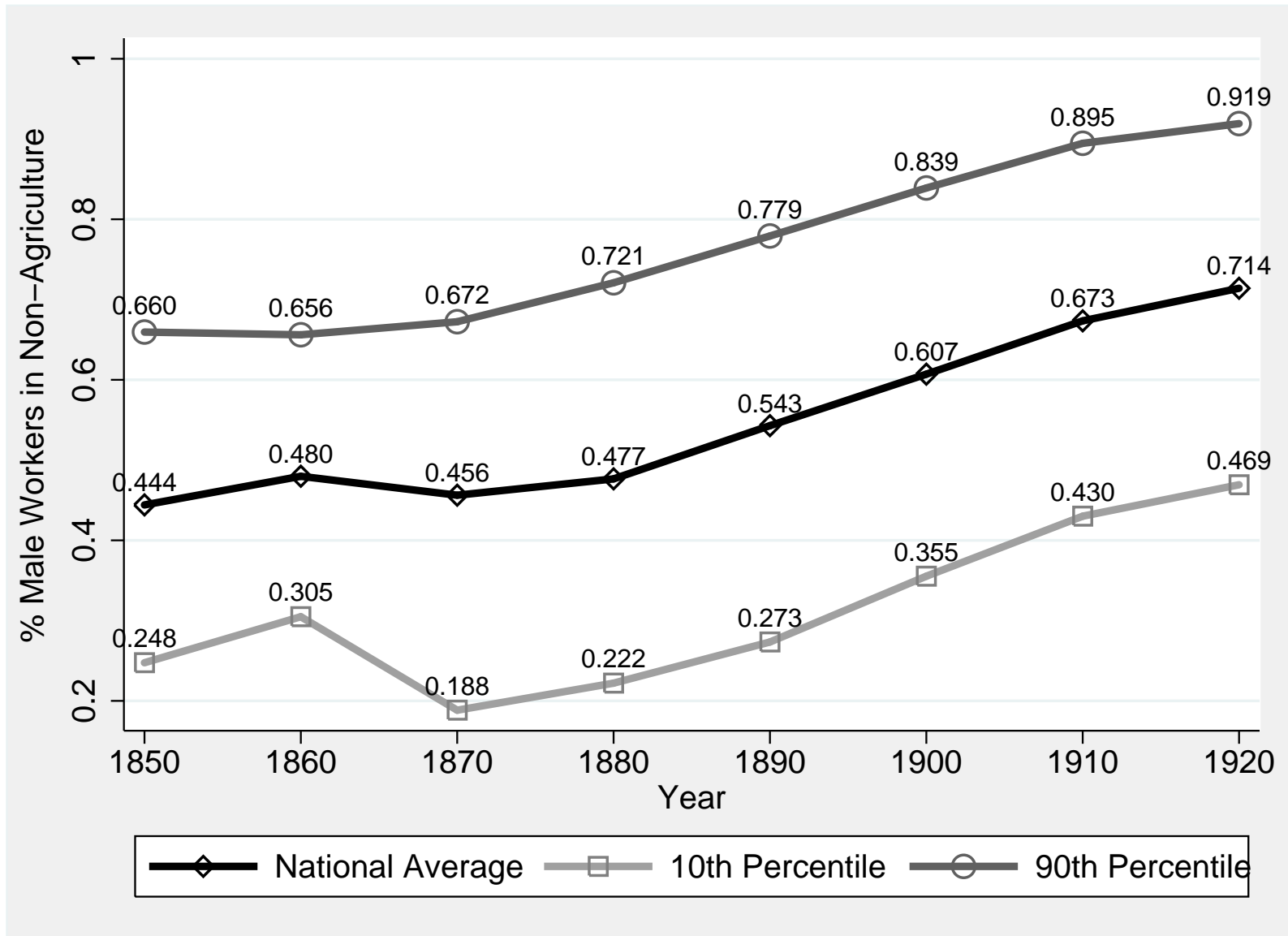


Figure 6: Cross State Comparison of Non-Agricultural Employment

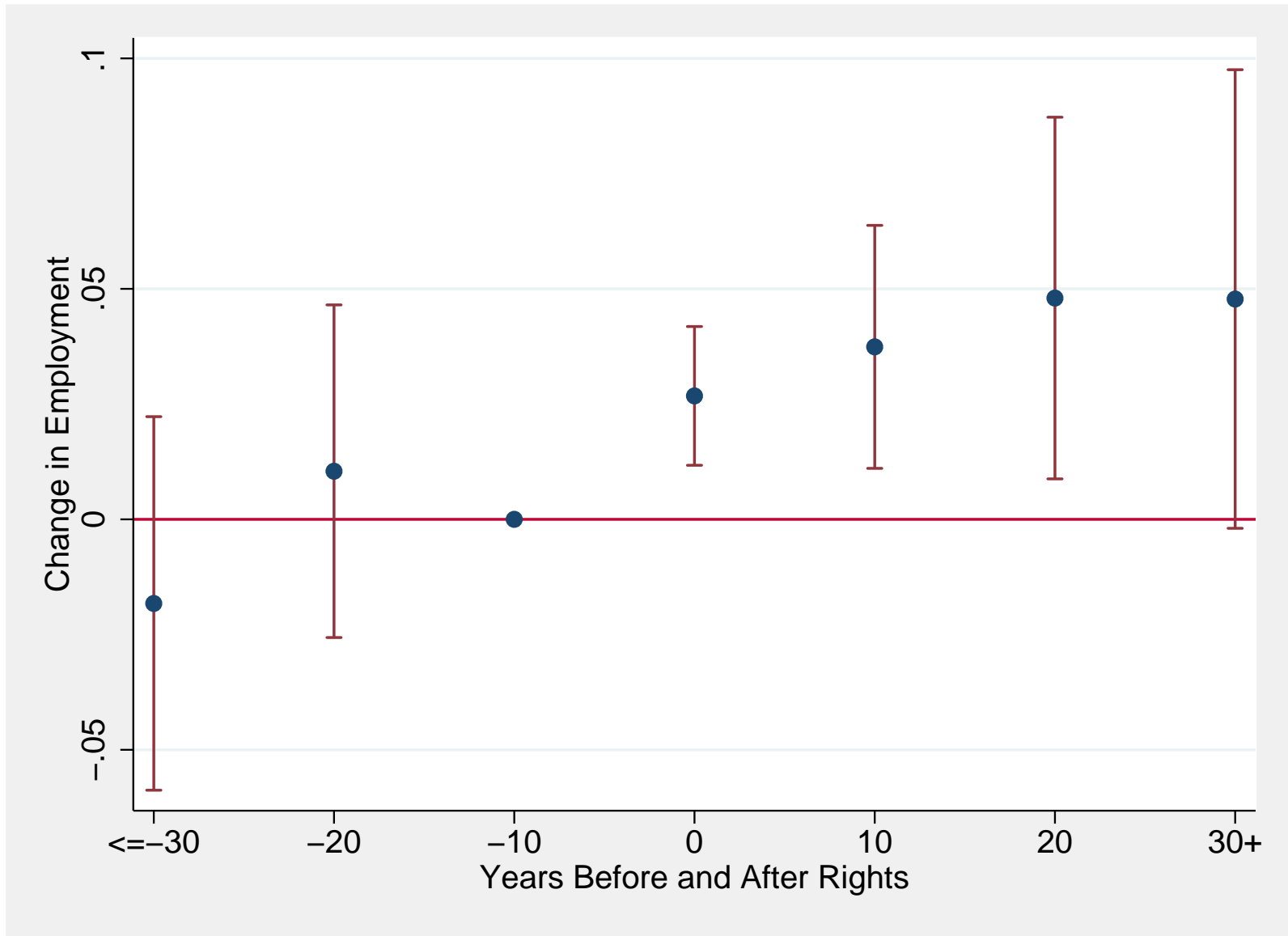


Figure 7: Dynamics of Non-Agricultural Employment, Before and After Rights.
Point estimates and 95% confidence intervals

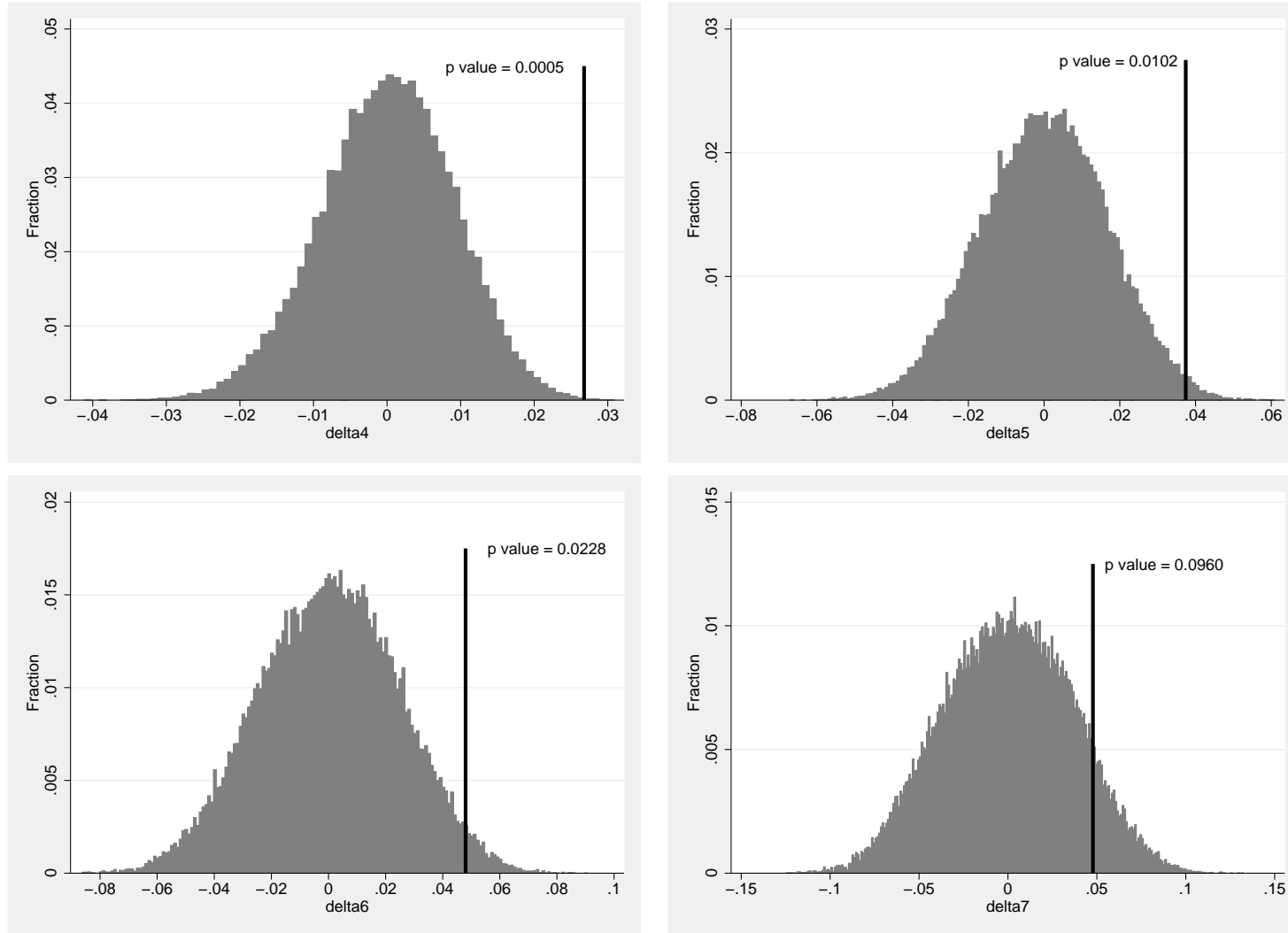


Figure 8: Results of 50,000 simulations of randomly assigned women's liberation dates. The top left corner shows results for α_0 ; the top right corner shows results for α_{10} ; the bottom left corner shows results for α_{20} ; and the bottom right corner shows results for α_{30+} .

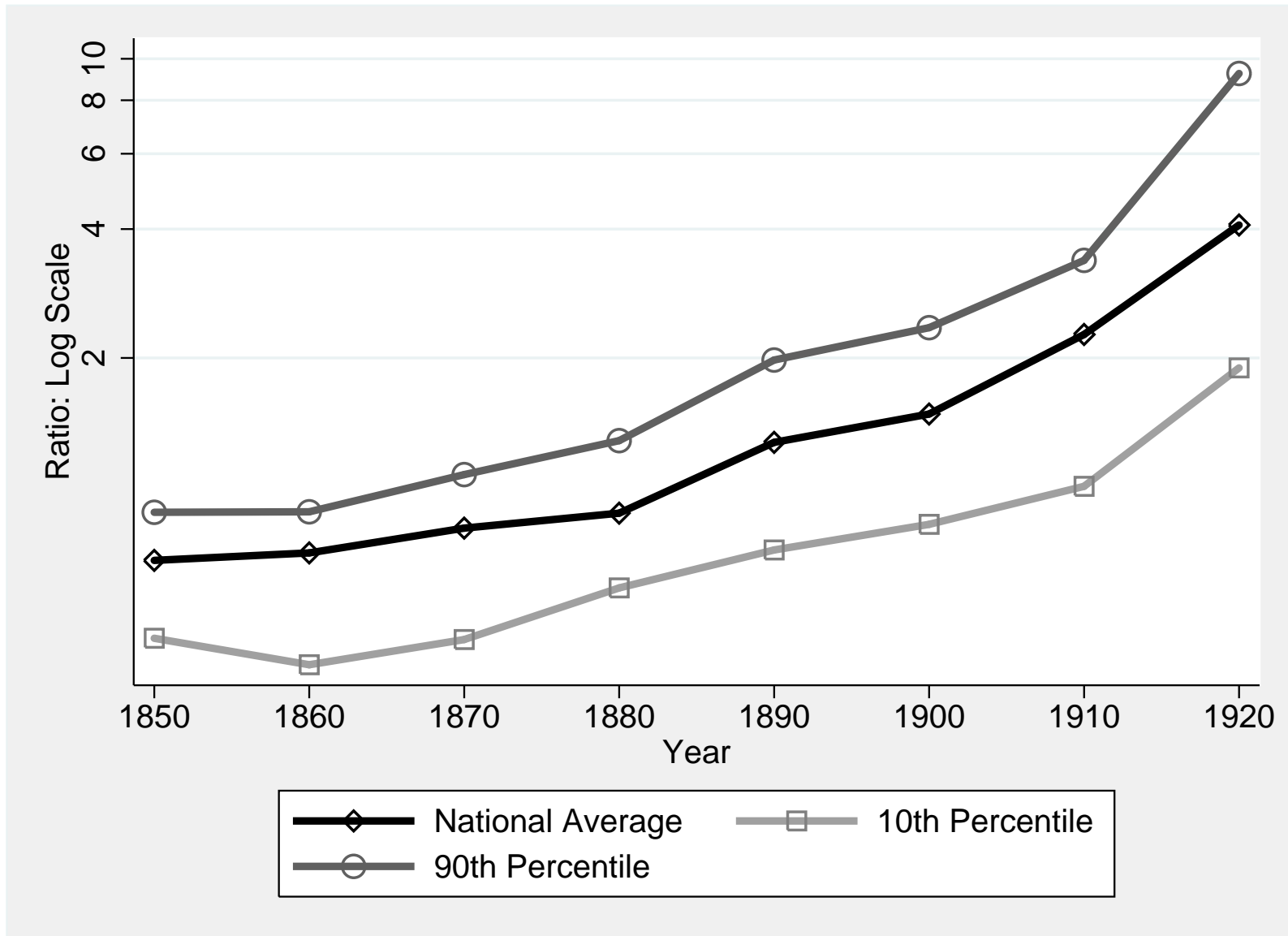


Figure 9: Cross State Comparison of the High to Low Capital Intensity Employment

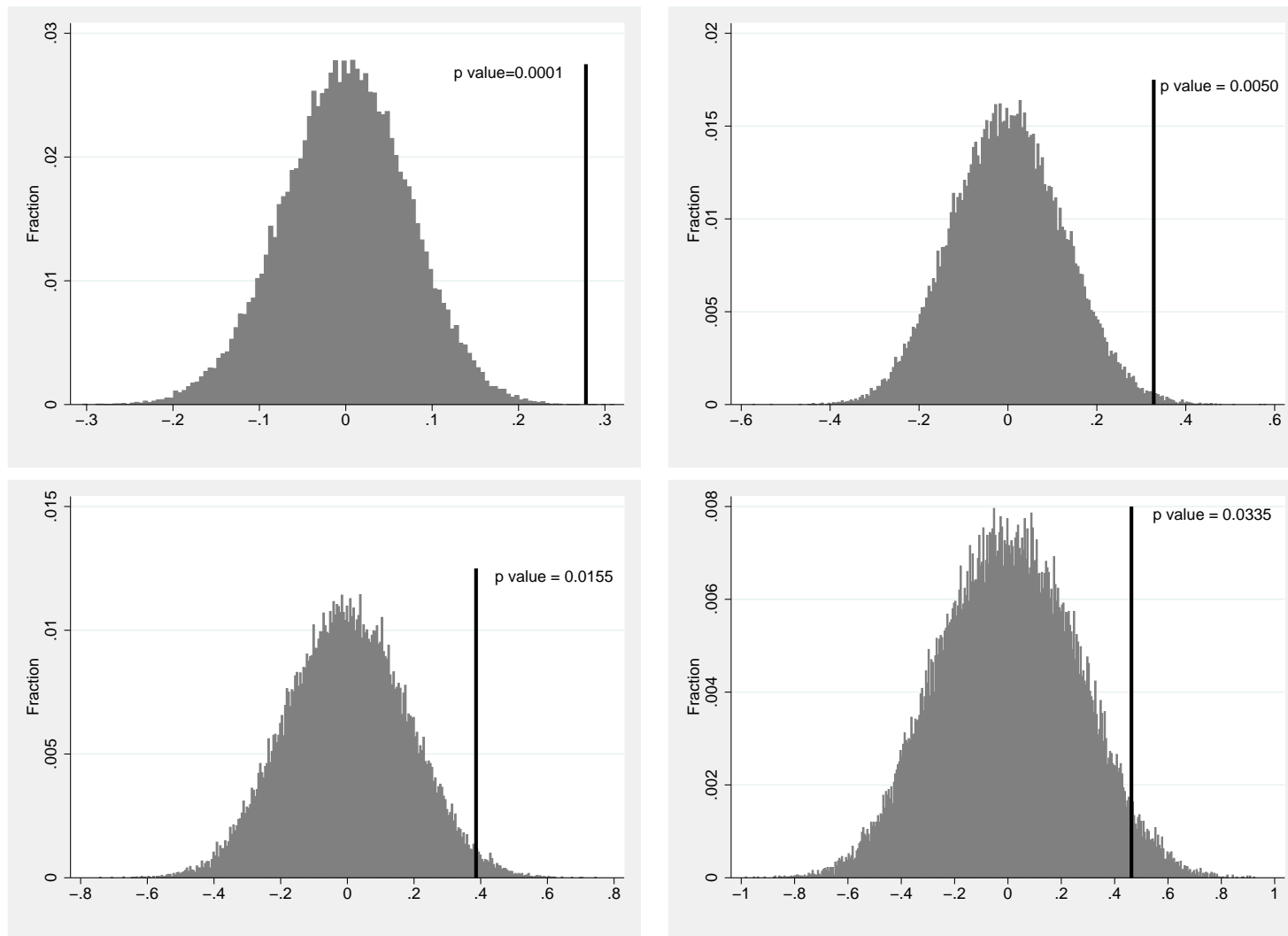


Figure 10: Results of 50,000 simulations of randomly assigned women's liberation dates on the log of the fraction of employment in the most capital intensive industries. The top left corner shows results for α_0 ; the top right corner shows results for α_{10} ; the bottom left corner shows results for α_{20} ; and the bottom right corner shows results for α_{30+} .

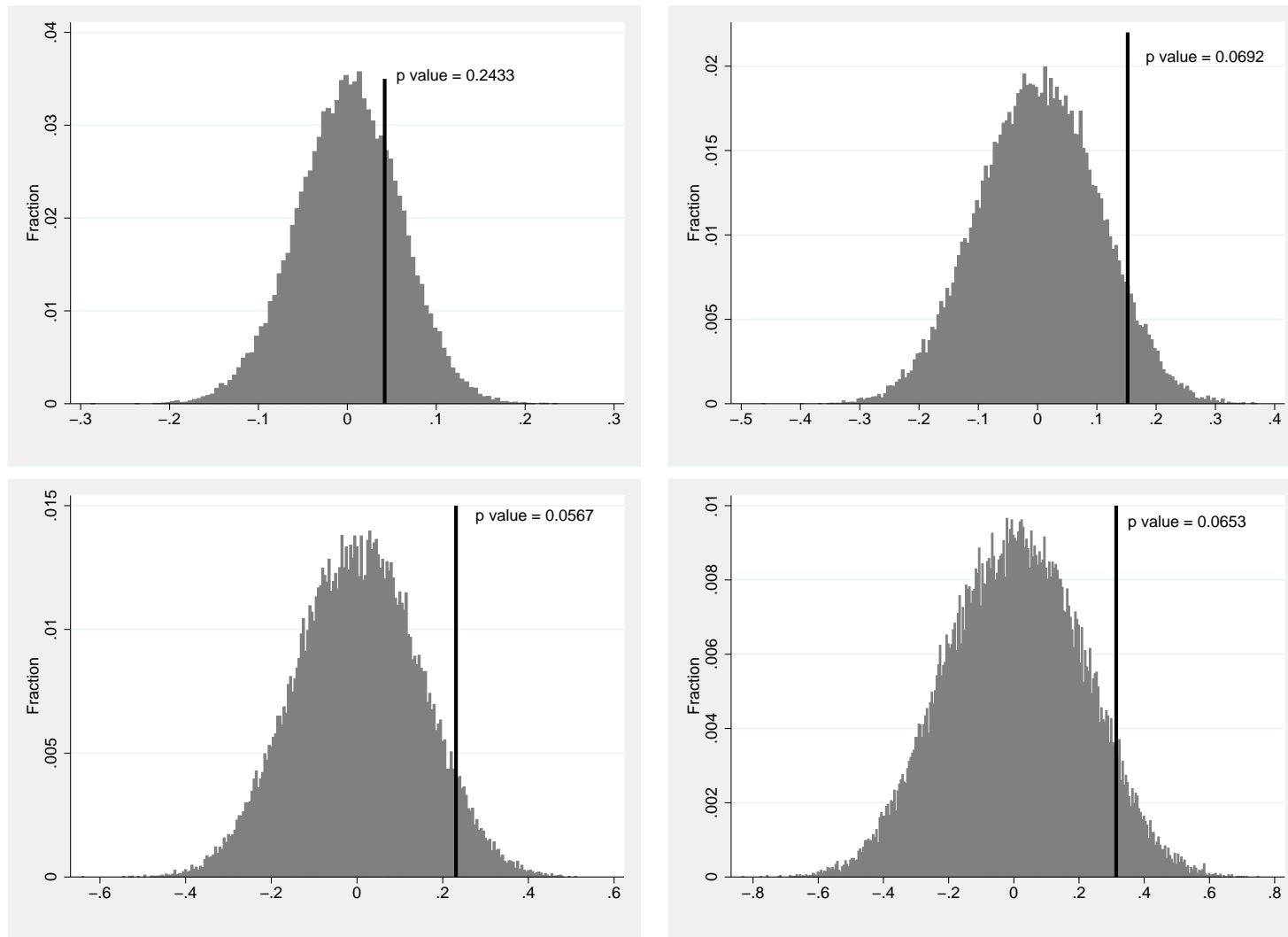


Figure 11: Results of 50,000 simulations of randomly assigned women's liberation dates on the log of the fraction of employment in the least capital intensive industries. The top left corner shows results for α_0 ; the top right corner shows results for α_{10} ; the bottom left corner shows results for α_{20} ; and the bottom right corner shows results for α_{30+} .

Table 1: Mean (# of Obs.) Portfolio Summary Statistics

Variable	Switching States		Other States	
	1860	1870	1860	1870
Fraction Moveable	0.43 (6,068)	0.42 (8,147)	0.51 (27,811)	0.47 (33,956)
Moveable Property (1870 Dollars)	1,090.76 (6,068)	1,264.20 (8,147)	2,677.87 (27,811)	1,407.70 (33,956)
Real Property (1870 Dollars)	3,463.55 (6,068)	3,567.31 (8,147)	3,559.73 (27,811)	3,031.90 (33,956)
Fraction Married	0.96 (15,207)	0.96 (20,733)	0.95 (74,108)	0.95 (105,859)
Age at Marriage (Newly-Wed)	24.99 (349)	25.02 (334)	26.03 (1,535)	25.21 (1,418)
Age Gap (Newly-Wed)	5.12 (346)	4.79 (332)	5.09 (1,500)	4.79 (1,394)

Table 2: Descriptive Statistics: 1850-1920 United States

Variable	Mean	Median	Standard Deviation	10th Percentile	90th Percentile
<i>Panel A: Economic Outcomes</i>					
Real Interest Rate (pp)	8.00	7.36	2.90	5.47	10.99
Δ Real Deposits per Capita (1920 Dollars)	3.79	2.31	12.18	-4.75	15.00
Δ Real Loans per Capita (1920 Dollars)	3.72	2.38	13.76	-4.57	13.28
% Non-Agri. Employment	54.10	52.65	20.75	28.42	81.86
Ratio of High to Low KL Employment	1.85	1.01	2.61	0.46	3.96
% Top KL Employment	3.66	1.83	4.38	0.66	9.42
% Bottom KL Employment	2.89	1.97	2.97	0.42	6.56
<i>Panel B: Explanatory Variables</i>					
Territory	0.090	0.00	0.286	0.00	0.00
Non-Agrucultural TFP relative to Agrucultural TFP	5.84	5.02	3.53	2.54	10.16
% Female	46.98	48.84	5.94	40.08	50.75
% Female in School	18.60	19.32	6.05	11.18	25.54
% Male in School	17.53	18.45	5.89	9.06	23.99
% Adult Under 35	50.27	49.78	7.19	42.36	58.65
% of Neighboring States with Rights	58.76	66.67	38.46	0.00	100.00

Table 3: Rights and Marriage

Dependent Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Married		Age of Newly-Weds		Age Gap of Newly-Weds	
Rights	0.004 (0.011)	0.000 (0.005)	0.006 (0.016)	0.873 (0.695)	0.577 (1.010)	0.096 (0.573)	0.497 (0.799)
Year FE	Yes	No	Yes	Yes	No	Yes	No
Division-Year FE	No	Yes	No	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	215,907	215,907	128,537	3,636	3,636	1,786	1,786
R^2	0.077	0.087	0.081	0.027	0.029	0.050	0.058

Notes. Standard errors are clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Other controls include age dummies, race dummies, and living in a territory.

Table 4: Rights and Fraction of Portfolios in Moveable Assets

	Dependent Variable: Fraction of Portfolios in Moveable Assets					
	(1)	(2)	(3)	(4)	(5)	(6)
Switch \times Post	0.032*** (0.009)	0.027*** (0.008)	0.027*** (0.001)	0.022** (0.010)	0.018* (0.010)	0.022** (0.010)
Post	-0.036*** (0.009)	-0.031*** (0.008)	-0.535*** (0.002)	-0.553*** (0.155)	-0.383*** (0.058)	-0.536*** (0.159)
Switch	-0.083*** (0.018)	0.161*** (0.006)	0.207*** (0.006)	-0.378*** (0.054)	0.077 (0.069)	-0.362*** (0.055)
Division \times Year FE	No	No	No	Yes	Yes	Yes
State FE	No	Yes	Yes	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	Yes	Yes
Total Assets	No	No	No	No	No	Yes
Alternate Control Group	No	No	No	No	Yes	No
Obs.	75,982	75,982	75,982	75,982	59,939	75,982
R^2	0.007	0.041	0.156	0.157	0.151	0.165

Notes. Standard errors are clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Other controls include age dummies, living in a territory, TFP in the non-Agricultural sector relative to TFP in the Agricultural sector, a dummy for living on a farm, and fixed effects for metropolitan areas. The “Alternate Control Group” uses as a sample only states that gave rights after 1860.

Table 5: Rights and Holdings of Moveable and Real Assets

Dependent Variable	Log Moveable Assets				Log Real Assets			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Switch× Post	0.291*** (0.101)	0.293*** (0.102)	0.243** (0.101)	0.079 (0.070)	0.018 (0.103)	0.056 (0.102)	-0.038 (0.099)	-0.205 (0.160)
Post	-0.382*** (0.091)	-0.377*** (0.089)	-0.060 (2.279)	-0.200 (2.213)	-0.145 (0.090)	-0.169* (0.088)	5.599*** (0.539)	5.698*** (0.670)
Switch	-0.293*** (0.089)	-0.032 (0.079)	2.510*** (0.761)	-0.349 (0.500)	0.350*** (0.102)	-1.511*** (0.079)	1.091 (0.655)	4.391*** (0.615)
Division×Year FE	No	No	No	Yes	No	No	No	Yes
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	No	No	Yes	Yes
Obs.	75,982	75,982	75,982	75,982	75,982	75,982	75,982	75,982
R ²	0.009	0.022	0.078	0.087	0.002	0.017	0.188	0.189

Notes. Standard errors are clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Other controls include age dummies, living in a territory, TFP in the non-Agricultural sector relative to TFP in the Agricultural sector, a dummy for living on a farm, and fixed effects for metropolitan areas.

Table 6: Rights and Interest Rates, Deposits, and Loans

Dependent Variable:	Interest Rate		Deposits		Loans	
	(1)	(2)	(3)	(4)	(5)	(6)
Rights	-0.801** (0.326)	-0.523* (0.289)	2.163** (0.946)	1.235* (0.665)	2.642** (1.069)	1.576* (0.857)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	Yes	No	Yes	No
Year×Region FE	No	Yes	No	Yes	No	Yes
Obs.	1,971	1,971	2,506	2,506	2,508	2,508
R ²	0.735	0.797	0.350	0.617	0.224	0.397

Notes. Standard errors are clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include a dummy for being a territory, and % of Neighboring States with Rights. Regressions are weighted by state population.

Table 7: Rights and the Dynamics of Non-Agricultural Employment

	Dependent Variable: % Male Workers in Non-Agriculture					
	(1)	(2)	(3)	(4)	(5)	(6)
≥ 3 Decades Before	-0.008 (0.028)	-0.011 (0.028)	-0.023 (0.027)	-0.010 (0.022)	-0.009 (0.022)	-0.018 (0.020)
2 Decades Before	0.008 (0.019)	0.007 (0.019)	0.006 (0.021)	0.002 (0.017)	0.004 (0.016)	0.010 (0.018)
1 Decade Before	0	0	0	0	0	0
Rights Given	0.032*** (0.008)	0.035*** (0.009)	0.032*** (0.008)	0.032*** (0.009)	0.040*** (0.010)	0.027*** (0.007)
1 Decade After	0.045*** (0.015)	0.050*** (0.016)	0.048*** (0.015)	0.041*** (0.014)	0.049*** (0.014)	0.037*** (0.013)
2 Decades After	0.068*** (0.022)	0.072*** (0.023)	0.069*** (0.022)	0.055*** (0.019)	0.063*** (0.019)	0.048** (0.019)
≥ 3 Decades After	0.074** (0.028)	0.080*** (0.028)	0.075** (0.028)	0.055** (0.024)	0.060** (0.024)	0.048* (0.025)
Relative TFP		0.004 (0.004)	0.000 (0.003)	0.003 (0.003)	0.003 (0.003)	0.006* (0.003)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No
Year \times Region FE	No	No	No	No	No	Yes
% Female	No	No	Yes	Yes	Yes	Yes
% Female in School & % Male in School	No	No	Yes	Yes	Yes	Yes
% Under Age 35	No	No	No	Yes	Yes	Yes
% Neighboring States with Rights	No	No	No	No	Yes	Yes
Obs.	356	356	356	356	356	356
R^2	0.937	0.938	0.951	0.958	0.959	0.970

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

Table 8: Rights and the Dynamics of Non-Agricultural Employment- Robustness

Dependent Variable:	% Male Workers in Non-Agriculture		
	(1)	(2)	(3)
	Alternative Definition: L^M	Without 1890	Without Rights btwn. 1870-1880
≥ 3 Decades Before	-0.019 (0.015)	-0.027 (0.023)	-0.016 (0.024)
2 Decades Before	0.006 (0.016)	-0.013 (0.009)	0.007 (0.026)
1 Decade Before	0	0	0
Rights Given	0.021*** (0.006)	0.025*** (0.008)	0.047*** (0.015)
1 Decade After	0.033*** (0.010)	0.037** (0.015)	0.084*** (0.021)
2 Decades After	0.040** (0.015)	0.044** (0.021)	0.107*** (0.025)
≥ 3 Decades After	0.035* (0.021)	0.049* (0.028)	0.122*** (0.030)
Relative TFP	0.009*** (0.003)	0.008** (0.004)	-0.001 (0.003)
Year FE	No	No	Yes
Year \times Region FE	Yes	Yes	No
Obs.	356	308	197
R^2	0.965	0.973	0.954

Notes. Standard errors are clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All specifications include a dummy for being a territory, as well as the fractions of the population that are female, under 35, the fraction of women and the fraction of men in school, and the fraction of neighboring states with rights. Regressions are weighted by state population.

Table 9: Rights and Employment by Capital Intensity

Dependent Variable	Ratio of High to Low			Log High KL	Log Low KL
	KL Employment			Employment	Employment
	(1)	(2)	(3)	(4)	(5)
≥ 3 Decades Before	-1.681 (1.072)	-1.794 (1.279)	-1.799 (1.188)	-0.210 (0.228)	-0.035 (0.124)
2 Decades Before	-0.304 (0.388)	-0.226 (0.481)	-0.201 (0.434)	0.114 (0.157)	-0.020 (0.079)
1 Decade Before	0	0	0	0	0
Rights Given	1.519 (0.992)	1.933* (1.127)	1.869** (0.910)	0.278*** (0.069)	0.042 (0.061)
1 Decade After	1.502* (0.777)	2.038* (1.007)	1.992** (0.908)	0.328*** (0.114)	0.152* (0.090)
2 Decades After	1.956* (1.046)	2.591** (1.250)	2.517** (1.167)	0.386** (0.153)	0.231* (0.134)
≥3 Decades After	1.576** (0.767)	2.409** (0.993)	2.515** (0.981)	0.463** (0.198)	0.314* (0.175)
Relative TFP		0.189 (0.193)	0.314 (0.234)	0.022 (0.033)	0.019 (0.020)
Other Controls	No	Yes	Yes	Yes	Yes
State FE	Yes	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 ²	0.628	0.656	0.685	0.899	0.931

Notes. Standard errors are clustered at the state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All specifications include a dummy variable for being a territory. Other Controls include the fractions of the population that are female, the fractions of the population that are under 35, the fraction of women and the fraction of men in school, and the fraction of neighboring states with rights. Regressions are weighted by state population.