

# Credit market competition and the gender gap: Evidence from local labor markets

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## Abstract

We exploit the exogenous variation in regional credit market competition brought on by banking deregulation to study the evolution of the gender gap in local US labor markets. We find that intrastate deregulation increased substantially female labor force participation rates, owing to two separate mechanisms: an increase in the rates of net job creation by private firms, and an expansion of services-producing sectors. In contrast, interstate deregulation was associated with a modest reduction in relative female labor force participation rates. The combined effect of intrastate and interstate deregulation is a 3-percent decline in the gender gap in local labor markets.

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# 1 Introduction

Between 1970 and 2000, in what was arguably the most significant change in labor markets at the time, the labor force participation rate for working-age women in the United States increased by 25 percentage points, from 46 percent to 71 percent. Figure 1 summarizes this development which represent a substantial closing of the gender gap in labor force participation. Prominent conventional explanations for this unprecedented convergence in the supply of market labor between men and women during this period focus on the waning social stigma surrounding married women's work outside the home (Fernandez, Fogli, and Olivetti, 2004; Goldin, 2006), on the the adoption of time-saving technologies due to technological progress in the home-durable-goods sector (Greenwood, Seshadri, and Yorukoglu, 2005), and on the introduction of oral contraception which facilitated women's investment in their careers (Goldin and Katz, 2002).

In this paper, we demonstrate the powerful contribution of a neglected factor in explaining the narrowing of the gender gap in local U.S. labor markets: the deregulation of the U.S. banking industry during the 1970s, 1980s, and early 1990s. Over this period, U.S. states gradually lifted restrictions on bank branching within the state and eliminated entry barriers to out-of-state banks. The positive impact of deregulation on the efficiency of local banking markets is well-documented: banks that were best able to route savings to the most productive uses expanded at the expense of poorer performers, loan losses and operating costs fell sharply, and the reduction in banks' costs was largely passed along to bank borrowers in the form of lower loan rates (Jayaratne and Strahan, 1997). We use the state-level variation in deregulation dates as a way to measure the impact of changing credit conditions on the gender gap in labor markets. Furthermore, to alleviate concerns about omitted variable bias at the state level, we compare the effect of deregulation on male and female labor market outcomes between narrowly defined adjacent geographic regions across state borders.

Using data on 1.9 million individuals between 1970 and 2000 from the Current Population Survey (CPS), we find that intrastate banking deregulation is associated with a lower gender gap in labor force participation rates. Our difference-in-differences estimates suggest that the removal of restrictions on intrastate bank branching reduced the probability that a woman in the age group 25 to 65 is not in the labor force by at least 1.4 percentage points, *ceteris paribus* and relative to an observationally similar man. At the same time, interstate deregulation does not appear to have the same effect: if anything, the gender gap in labor force participation widens somewhat in the years following deregulation. The net effect of the two types of

deregulation is a 3-percent decline in the gender gap in local labor markets.

The main result in the paper is robust to a number of confounding factors. First, we show that the trend we detect in the data does not predate deregulation. Second, the main result obtains after controlling for a wide host of individual background characteristics that can determine labor market outcomes, such as age, race, education, and marital status. It also survives when we allow for the impact of those individual characteristics to fluctuate with deregulation. Third, our main result still obtains when we control for the potentially differential impact across genders of concurrent socio-economic developments in local markets, such as unemployment, changes in home ownership rates, and the evolution of marriage markets. Crucially, we hold a host of unobservable background forces constant by including in our regressions interactions of geography, time, and gender dummies. We include  $State \times Year$  fixed effects which control for any unobservable state-specific time-varying factors that affect all economic agents in a state equally, such as changes in taxes or regulation. We also include  $State \times Female$  fixed effects which control for state-specific time-invariant factors that affect men and women differently, such as cultural norms prior to our sample period. Finally, we include  $Female \times Year$  fixed effects which control for gender-specific US-wide trends, such as the propensity of women with higher unobserved skills to enter the US labor market during the 1970s and 1980s (Mulligan and Rubinstein, 2008). We are thus fairly confident that our results are driven neither by unobservable differences between men and women in local markets, nor by unobservable geography-specific or gender-specific trends.

We base our main analysis on an arguably exogenous shock to the cost of credit, in combination with an empirical specification that accounts for an exhaustive set of potential confounding factors at the individual, gender, and state level. Even so, comparing individuals across states—with respect to bank deregulation events—can be open to a number of econometric problems. Pro-competitive banking reform can be induced by an expectation of future growth opportunities (unobservable to the econometrician) that can benefit women disproportionately, e.g., the expansion of sectors rich in jobs requiring female-specific skills. This could create a spurious correlation between bank branching deregulation and future changes in the gender gap. To address this issue, we modify the empirical strategy used by Card and Krueger (1994), Holmes (1998), Black (1999), and Huang (2008) and compare individual labor market outcomes for men and women across 28 contiguous Metropolitan State Areas (MSAs) separated by state borders, in cases in which one state deregulated its banking sector earlier than the other. Because these MSAs are immediately adjacent neighbors, or neighbors separated by only one other MSA, we expect them to be similar in both observable,

and more importantly, unobservable conditions, and to follow similar economic paths in the absence of changes in bank entry barriers. Our main result still obtains even in this considerably more restrictive specification, suggesting that we capture a genuine bank deregulation effect uncontaminated by concurrent unobservable adjustments—at the level of the state—in economic or labor market conditions that affect men and women differently.

We next turn our attention to the mechanisms that could explain the patterns which we uncover in the data. We propose, and then empirically test, three separate interpretations of our results. The first one is that banking deregulation precipitated a change in culture whereby as the local economy became more dynamic, women became more likely to join the labor market. Changes in social norms and cultural identities benefit women disproportionately and can have a significant impact on the gender gap over time (Akerlof and Kranton, 2000; Fernandez, Fogli, and Olivetti, 2004; Bertrand, Kamenica, and Pan, 2015). The gradual decline in gender bias leads, for example, to the democratization of hiring procedures (Goldin and Rouse, 2000). We test for the impact of deregulation on culture by constructing an empirical proxy for social norms derived from responses to a survey question in the General Social Survey about the "proper" place of women in society. The second interpretation is that by reducing the rates on corporate loans, deregulation increased the rates of new business creation (Black and Strahan, 2002; Cetorelli and Strahan, 2006), and ultimately the rate of job creation by the local private sector. The resulting increase in the demand for labor plausibly made it easier for non-working women to enter the labor force. The third interpretation is that post-deregulation, the industrial composition of the local economy shifted towards services-producing sectors. The production of services is relatively less intensive in the use of "brawn" skills, and relatively more intensive in the use of "brain" skills. Because men are better endowed of brawn skills than women, the growth in the service sector at the expense of sectors such as mining, manufacturing, and construction, has created jobs for which women have a natural comparative advantage (Goldin, 2006; Ngai and Petrongolo, 2017). It is possible that banking deregulation precipitated the secular shift towards a services-oriented economy and thus increased the demand for female labor.

We document a significant post-intrastate-deregulation increase in the rates of net job creation by business firms. We also document an increase in females' relative propensity to be employed in private sector jobs, at the expense of public sector employment. The post-deregulation increase in private sector activity thus helps explain the overall increase in female labor force participation following intrastate deregulation. The data also suggest that the share of service-sector employment increased in the wake of intrastate deregulation,

and declined following interstate deregulation, in both cases relative to the national trend. The differential effect of the two types of deregulation on the shift towards a more service-oriented local economy thus helps explain the different impact of intrastate versus interstate deregulation on the gender gap in labor markets. Finally, we also find a short-lived post-intrastate-deregulation decline in gender bias which can partially explain the immediate increase in female labor force participation. However, the decline in gender bias becomes insignificant five years after deregulation, therefore it cannot be credited with the long-term labor market impact of deregulation that we uncover.

Finally, we perform our tests on various sample splits in order to determine who benefited the most from deregulation. We find that married women, white women, and women younger than 45 years of age benefited disproportionately from deregulation on the extensive margin of the labor supply. In addition to pointing to important distribution effects of deregulation within the group of females, this qualification of our main finding has implications for models linking the gender gap in labor markets to family structures and fertility choices (e.g., Galor and Weil, 1996).

By now there exists a vast body of evidence on the impact of banking deregulation on various economic outcomes, such as state business cycles (Morgan, Rime, and Strahan, 2004; Demyanyk, Ostergaard, and Sorensen, 2007), personal bankruptcy (Dick and Lehnert, 2010), trade (Michalski and Ors, 2012), education choices (Levine and Rubinstein, 2013), income distribution (Beck, Levine, and Levkov, 2010), and racial inequality (Levine, Levkov, and Rubinstein, 2014). In particular, the latter two papers also use detailed individual level-data from the CPS, and they also consider labor market effects, showing that banking deregulation resulted in more inclusive labor markets and increased labor income relatively more for unskilled and for black workers. The principal contribution of our paper is to demonstrate the significant impact of bank branching deregulation on female labor market outcomes during the 1970s, 1980s, and 1990s. To our knowledge, ours is the first paper to establish a causal link between credit market competition and the narrowing of the gender gap in U.S. labor markets.

We also contribute to the literature which has sought to identify and quantify the gender gap in labor market outcomes, such as the gap in labor force participation (Greenwood, Seshadri, and Yorukoglu, 2005), the gap in earnings (Blau and Kahn, 2000; Bayard, Hellerstein, Neumark, and Troske, 2003; Guvenen, Kaplan, and Song, 2014), and the gap in hiring (Neumark, Bank, and Van Nort, 1996). While some analysts have argued that this gap is primarily driven by male-female differences in productivity and in work ex-

perience (O'Neill and Polachek, 1993; Mulligan and Rubinstein, 2008), a more wide-ranging view is that equally productive men and women face different job prospects and strike different wage bargains with their employees (Card, Cardoso, and Klein, 2016). Altonji and Blank (1999) find that after controlling for education, experience, personal characteristics, city of residence, occupation, industry, government employment, and part-time status, only about 27 percent of the gender wage gap is explained by differences in observable characteristics. Our paper is even more closely related to studies that map changes in credit market competition into market outcomes, by gender. For example, Cavalluzzo, Cavalluzzo, and Wolken (2002) and Hertz (2011) find that female-owned firms are less likely to apply for and/or to obtain bank credit than male-owned firms, both in the US and in an international context. Bellucci, Borisov, and Zazzaro (2010) find that female firm owners in Italy face tighter credit constraints when dealing with one individual bank. Alesina, Lotti, and Mistrulli (2013) find that after controlling for entrepreneurial risk, female borrowers pay higher rates, especially when their guarantor is a female, too. Fortin (2005) finds that the gender gap in employment and pay is higher in countries with anti-egalitarian views. Ongena and Popov (2016) find that in countries with higher gender bias, and after controlling for credit risk differences between female- and male-owned firms, female entrepreneurs are more frequently discouraged from applying for bank credit and more likely to rely on informal finance, even though banks do not appear to discriminate against female loan applicants. We contribute to this literature by demonstrating a causal link from banking competition to the gender gap in labor markets, through an increase for the demand for labor in the private sector and through a shift in the industrial composition of the local economy towards the services-producing sector.

Our paper is also related to the literature on the effect of financial market conditions on employment. One strand of research has studied the effect of firm debt on firm-level employment and wages (Lichtenberg and Siegel, 1990; Hanka, 1998; Falato and Liang, 2017). Another, more recent line of research has attempted to gauge the effect of access to external finance on employment, broadly demonstrating that positive (negative) shocks to the cost of external finance increase (decrease) labor demand, with material effect on equilibrium employment and wages (Campello, Graham, and Harvey, 2010; Chodorow-Reich, 2014; Duygan-Bump, Levkov, and Montoriol-Garriga, 2015; Bentolila, Jansen, Jimenez, and Ruano, 2017; Popov and Rocholl, 2017). Our paper contributes to this literature by demonstrating the heterogeneous impact that pro-competitive banking reform has across genders in labor markets.

The paper is organized as follows. Section 2 provides some background on banking deregulation and discusses the validity of using this policy innovation to study labor market outcomes. Section 3 describes

our data set. Section 4 explains our research design and discusses identification. Section 5 presents and discusses the main results of the paper. Section 6 discusses the implications of our findings and concludes.

## **2 Bank deregulation in the United States: Overview**

### **2.1 Bank deregulation: Institutional background**

Historically, states restricted banking within their borders. The McFadden Act of 1927 forced national banks to obey state-level restrictions on branching, and essentially prohibited cross-state banking. In addition, many states, in particular in the wake of the Great Depression, developed strict rules regulating the branching of banks within their geographic borders. In particular, restrictions on intrastate branching focused on the market share of holding companies, formed in turn as a response to many states limiting each bank to a single branch. Furthermore, the Douglas Amendment of 1956 prohibited banks from owning banks across state borders. These restrictions prevailed until the 1970s, when only 12 states plus the District of Columbia allowed unrestricted state-wide branching, and no state allowed interstate branch banking.

Starting in the second half of the 1970s, states gradually lifted two classes of restrictions. First, intrastate banking deregulation allowed banks to expand within the state in which they were licensed to operate, either via mergers and acquisitions, or via the opening of de novo branches. By 1994, 35 states lifted restrictions on the geographical expansion of banks within the state. This process increased competition in local markets, often breaking up effective monopolies. Second, in the early 1980s many states began to enter regional or national reciprocal arrangements whereby their banks could be bought by any state in the arrangement. Subsequently, in 1993 Montana became the last state to deregulate interstate banking. This process further improved competition among creditors in local markets, and increased the efficiency of bank services by eventually pushing less efficient creditors out of the market. Finally, in 1994, the Riegle-Neal Interstate Banking and Branching Efficiency Act removed all branching restrictions in states that had not deregulated their banking markets up to that point.

Typically (in 34 of 51 cases), states removed intrastate restrictions before they lifted interstate barriers to entry. Similar to Cetorelli and Strahan (2006), Kerr and Nanda (2009), and Dick and Lehnert (2010), we study the impact of the removal of both types of restrictions. Moreover, consistent with Kerr and Nanda (2009), we assign a deregulation year of 1970 to states whose banking sector was already deregulated in the

early 1970s. Table I reports intrastate and interstate deregulation events, by state and year.

The extensive literature on the effects of banking deregulation finds that deregulation was generally a positive development, leading to greater bank efficiency and competition, lower prices and higher quality, higher rates of new business formation, and higher economic growth (Jayaratne and Strahan, 1996; Black and Strahan, 2002). The fact that most states chose to remove these barriers at different times provides us with variation in the competitive environment facing lenders that is potentially exogenous with respect to individual labor market outcomes. We capture the effect of intrastate (interstate) branching deregulation by constructing an indicator variable equal to 1 in the years after a state permits branching by means of merger and acquisition within its borders (by out-of-state banks).

A binary measures by definition is unable to capture the speed with which the lifting of branching restrictions affected the size of the banking sector. Moreover, because all states gradually deregulate their banking markets, the statistical association between the deregulation dummy and labor market outcomes in later years can be driven by early deregulators. For these reasons, in addition to comparing mean labor market outcomes before and after deregulation, we also compare labor market outcomes across individual post-deregulation years. We thus compare to the pre-deregulation period states at different points in time which are equally distant from their deregulation event, controlling for state-specific and US-wide trends. This approach also allow for the impact of deregulation to be non-linear.

## **2.2 Bank deregulation: Endogeneity**

One question that has often been raised is whether banking deregulation was really an exogenous event. Jayaratne and Strahan (1996) argue that states did not deregulate their local banking markets in anticipation of future growth prospects. Kroszner and Strahan (1999) show that it is mostly economic and political factors (such as the relative strength of large banks and of small bank-dependent firms) that explain the timing of branching deregulation across states. Still, the exogeneity of banking deregulation to how friendly local labor market conditions are to females can be put into question. For example, states with a high share of educated females or with a high share of females in the labor force, as well as states with a narrower cultural gender gap, might relax bank branching restrictions sooner in anticipation of gains to both banks and females of improved credit conditions.

To address this potential issue, we construct three state-level measures of the relative economic and



cultural position of women in a state prior to deregulation. The first is the share of working-age females who have some college education (i.e., college drop-outs, college graduates, or individuals with a graduate or a professional degree). The second is the share of females out of all those in the labor force (i.e., self-employed, privately employed, or employed by the government). The third is the regional share of respondents in the General Social Survey (GSS) who answer "Strongly disagree" or "Disagree" when confronted with the following statement: "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family." We define the measures as early as the data allow. Figures 2, 3, and 4 show the relationship between these variables and the number of years it took the state to deregulate its local banking market after 1977. As can be seen, states with a higher proportion of educated women, with a higher share of working women, and with social norms that are more accepting of working women do not appear to be deregulating earlier.<sup>1</sup>

### 3 Data

Our empirical analysis uses individual-level information on demographic characteristics, employment, education, labor market participation, and income from the Integrated Public Use Microdata Series (IPUMS) (Flood, King, Ruggles, and Warren, 2015). We are interested in yearly observations, which are available in the March Annual Demographic Supplements from the U.S. Current Population Survey (IPUMS-CPS). Our sample consists of the survey years 1970–2000 in order to capture, for as many states as possible, the period before and after intrastate and interstate banking deregulations. We exclude the population that is retired or is of schooling age by dropping individuals younger than 25 and older than 65 years. To make sure that we are not capturing changes in employment within the banking sector itself, we drop all individuals working in banking, insurance, and real estate. For the 30-year period considered in our analysis, the final sample contains a total of 1,965,168 individuals across the 50 states and the District of Columbia.

We measure labor force participation on the extensive margin by distinguishing between individuals 'In the labor force' and those 'Not in the labor force.' This variable is available in IPUMS-CPS as of the beginning of the sample period (1970). In terms of types of employment, we construct three occupational categories for those who are in the labor force and are not unemployed: 'Self-employed', 'Employed in the private sector', and 'Employed in the public sector'; the latter category includes those serving in the Armed

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<sup>1</sup>The respective correlations are 0.11, 0.11, and -0.22.

forces. Furthermore, the respondents are classified into three educational categories: 'High-school or less' (includes all persons with 0 to 12 years of schooling), 'College drop-outs' (includes all respondents who have less than four years of college education), and 'College or more' (includes all respondents with at least a college degree). In terms of demographics, we use data on gender, age, race, and marital status.

Table II reports summary statistics for all individual characteristics of interest. The table shows that the average individual from our sample is female, white, married, and around 42.5 years old. 48.5 percent of the respondents are employed in the private sector, 13.6 percent are employed in the public sector, 8.2 percent are self-employed, and 25.7 percent are not in the labor force. The remaining 4 percent are unemployed. In terms of educational attainment, the average respondent has a high-school degree or less.

The table also reports summary statistics on various state-specific socio-economic factors which we control for in robustness tests. For example, on average 71 percent of all working-age women are married; two thirds of the individuals in the sample own their residence; the difference between gross job creation and gross job destruction is on average 2.74 percentage points, while the sum of the two, excluding net job creation, is on average 29.83 percentage points; and 46.47 percent of the working population is employed in services-producing sectors.

## 4 Empirical model

Our goal is to study how the deregulation of local banking markets affects the gender gap in labor force participation in local labor markets. We are interested in the probability of joining the labor force (the extensive margin of labor supply). To analyze this effect, we specify the following non-linear probability model:

$$\begin{aligned}
 Prob(NotInLaborForce_{i,s,t} = 1) &= \beta_1 Female_{i,s,t} \\
 &+ \beta_2 Female_{i,s,t} \times IntrastateDeregulation_{s,t-1} \\
 &+ \beta_3 Female_{i,s,t} \times InterstateDeregulation_{s,t-1} \\
 &+ \beta_4 X_{i,s,t} + \beta_5 \Psi_{s,t} + \beta_6 \Phi_{s,f} + \beta_7 \Theta_{f,t} + \varepsilon_{i,s,t}
 \end{aligned} \tag{1}$$

The dependent variable,  $NotInLaborForce_{i,s,t}$ , is an indicator variable which is equal to one if individual  $i$  in state  $s$  during year  $t$  reports not being in the labor force, and to zero otherwise.  $Female_{i,s,t}$  is an indicator

variable equal to one if individual  $i$  in state  $s$  during year  $t$  is a woman.  $IntrastateDeregulation_{s,t}$  is an indicator variable equal to one for states that have removed barriers to intrastate branching as of year  $t$ , and to zero otherwise. It equals one throughout the sample period for the 12 states plus the District of Columbia, which removed bank branching restrictions earlier.  $InterstateDeregulation_{s,t}$  is an indicator variable equal to one for states that have removed barriers to interstate branching as of year  $t$ , and to zero otherwise. These indicators capture changes in credit supply induced by the intensification of competition in the banking sector. In both cases, we drop all observations in a state during the year of deregulation. The coefficients of interest are  $\beta_2$  and  $\beta_3$  and they measure the impact of intrastate deregulation and of interstate deregulation, respectively, on the probability that a working-age woman is not in the labor force, compared with a similar man. For example, a negative coefficient  $\beta_2$  will imply that ceteris paribus, and compared to an observationally similar male, a female in a state that has lifted restrictions on intrastate bank branching is less likely to not be in the labor force than a female in a state that has not done so yet.

In addition, we include a vector of individual-level covariates,  $X_{i,s,t}$ , which includes age, race, education, and marital status. These control for demographic factors that can affect the individual propensity to join or not the labor force. We also saturate the model with interactions of state, year, and gender fixed effects.  $\Psi_{s,t}$  is a matrix of  $State \times Year$  fixed effects which control for unobservable state-wide temporal shocks that are common to all agents in a state-year. This is important as any variation at the state level in labor market conditions, labor demand, or growth opportunities can affect the estimates.  $\Phi_{s,f}$  is a matrix of  $State \times Female$  fixed effects which control for state-wide differences that are persistent over time and that affect women and men differently, such as cultural attitudes in place already before deregulation. Finally,  $\Theta_{f,t}$  is a matrix of  $Female \times Year$  fixed effects which control for any US-wide female trends. This is enormously important as differences over time in labor market outcomes between men and women at the state level can be driven by US-wide shocks to female labor supply which are unrelated to local deregulation, such as the propensity of women with higher unobserved skills to enter the US labor market during the 1970s and 1980s (Mulligan and Rubinstein, 2008). We do not include the variables  $IntrastateDeregulation_{s,t}$  and  $InterstateDeregulation_{s,t}$  on their own because their direct effect on labor force participation is subsumed in  $\Psi_{s,t}$ . The sample period is 1970–2000.<sup>2</sup> We employ models with robust standard errors clustered at the

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<sup>2</sup>This sample period allows us to capture a sufficient period before and after the banking deregulations. 1977 marks the beginning of the period of dramatic state-level deregulation. The wave of deregulations ends in 1994, when deregulation of restrictions on the ability of banks to expand across local markets was completed with the passage of the Riegle–Neal Interstate Banking and Branching Efficiency Act.

state-year level. We exclude individuals in schooling age or in retirement age by focusing on those aged 25–65. Finally, all estimates are weighted using sampling weights provided by the CPS.

The first concern with this empirical strategy is that the sample of "treated" (i.e., deregulated) states grows over time at the expense of "control" (regulated) states. For example, while in 1980 there are 35 states where intrastate branching is still regulated, in 1993 there are only three states which have not lifted branching restrictions yet (Arkansas, Iowa, and Minnesota; see Table II). Because all states gradually deregulate their banking markets over the sample period, a documented cross-sectional statistical association between the deregulation dummy and labor market outcomes in later years can be driven by early deregulators. To address this issue, we test the following alternative regression model:

$$\begin{aligned}
\text{Prob}(\text{NotInLaborForce}_{i,s,t} = 1) = & \beta_1 \text{Female}_{i,s,t} \\
& + \sum_{j=1}^5 \beta_2^j \text{Female}_{i,s,t} \times \text{YearAfterIntrastateDeregulation}_{s,t-1}^j \\
& + \sum_{j=1}^5 \beta_3^j \text{Female}_{i,s,t} \times \text{YearAfterInterstateDeregulation}_{s,t-1}^j \\
& + \beta_4 X_{i,s,t} + \beta_5 \Psi_{s,t} + \beta_6 \Phi_{s,f} + \beta_7 \Theta_{f,t} + \varepsilon_{i,s,t}
\end{aligned} \tag{2}$$

Here  $\text{YearAfterIntrastateDeregulation}_{s,t-1}^j$  is a dummy variable equal to 1 if an individual is observed  $j$  years after intrastate deregulation, and to 0 otherwise. Analogously,  $\text{YearAfterInterstateDeregulation}_{s,t-1}^j$  is a dummy variable equal to 1 if an individual is observed  $j$  years after interstate deregulation, and to 0 otherwise. Similar to Kerr and Nanda (2009), we cap  $j$  at 5, with  $j = 5$  capturing the full period of 5+ years after deregulation. This specification allows us to postulate that the deregulation effect is not linear over time, as well as to compare individuals at different points in time but at the same stage of deregulation in their state. In other words, we are treating equally individuals in Maine in 1978 and in Colorado in 1994 (in both cases, 3 years after intrastate deregulation) while controlling—with the help of fixed effects—for a US-wide female trends and for the fact that such individuals are subject to different state-specific trends. Here the coefficients of interest are  $\beta_2^j$  and  $\beta_3^j$  and they measure the impact during year  $j$  of intrastate deregulation and of interstate deregulation, respectively, on the probability that a woman is not in the labor force, compared with a similar man.

The second concern is that the employed empirical strategy can result in biased estimates in the presence of unobservable trends that differ across states at each point in time. For example, if banking deregulation takes place in Virginia and subsequently the narrowing of the gender gap in labor markets is faster than

the national average, the econometrician will conclude that the policy reform has produced a positive effect. However, this may not be a good comparison: when comparing Virginia (an early deregulator) to Arkansas (a late deregulator), it may be easy to find difference in labor market outcomes for males and for females simply because the labor markets of the two states do not necessarily move in the same direction or at the same speed. While in Models (1) and (2) we control for observable individual factors that determine labor outcomes, there may still be unobservable ones at work that differ across the two states, such as innate preference for consumption over leisure time. In the absence of good controls for those, it can be fallacious to interpret the narrowing of the gender gap in Virginia as the effect of a policy adopted in Virginia but not in Arkansas.

To address this issue, we follow closely the approach in Huang (2008) and we compare individuals across border areas of two neighboring U.S. states where banking deregulation is different across state borders. In such areas, factors that are difficult for the econometrician to control for, such as individual preferences, social norms, climate conditions, or the technological demands of the local economy, are very similar. Therefore, the effect of a certain policy change—bank branching deregulation, in this case—should be more precisely identified by comparing differential labor market outcomes across state borders.

As the individuals in the annual CPS samples do not report their county of residence, we perform this exercise at the level of the MSA. We determine MSAs that lie across state borders, that are of approximately equal size, and that experienced deregulation events separated by at least 3 years, and we re-estimate Models (1) and (2) in these samples. Using state borders, 14 events of state-level branching deregulation throughout the United States spanning our sample period can be evaluated. Table III lists all such "treatment" and "control" MSAs.

One final question about our identification strategy is whether the assumption that the control group is not affected by deregulation holds in the data. If this assumption is violated, then a positive coefficient  $\beta_2$  ( $\beta_3$ ) will not be measured because intrastate (interstate) deregulation increases labor force participation rates for women, but because it reduces it for men. Because our empirical model includes *State*  $\times$  *Year* fixed effects, we cannot estimate the effect of deregulation on each group separately. Nevertheless, we can test for the impact of deregulation—year by year—on men and on women separately in a model with *State* and *Year* fixed effects. We present the outcome of these tests graphically in Figures 5–8. In these, we plot the point estimates for pre- and post-deregulation years (thick line) and a 95 percent confidence interval

(dotted lines). It is clear from the figures that there is no statistical difference in the supply of market labor by males between the pre- and the post-deregulation period, both in the case of intrastate and in the case of interstate deregulation. At the same time, a significant impact of intrastate deregulation on the supply of market labor by females is readily apparent.

## 5 Empirical evidence

In this Section, we present the results of our empirical tests. In Section 5.1, we present the main results from tests where we study the effect of intrastate and interstate bank deregulation on the gender gap across local U.S. labor markets. In Section 5.2, we provide additional evidence from tests where we evaluate the parallel trend assumption, we control for potential confounding factors, and we estimate the effect of deregulation on narrowly defined economic zones across state borders. In Section 5.3 we test for the economic mechanisms which banking deregulation potentially activated to reduce the labor gender gap. Finally, in Section 5.4 we provide evidence on who benefited most from deregulation.

### 5.1 Banking deregulation and labor force participation: Main result

Table IV shows result from our Models (1) and (2) where we estimate the differential effect on labor force participation of banking deregulation across genders. We first evaluate Model (1), starting with a specification with individual controls and with  $State \times Year$  fixed effects (column (1)), and then we gradually add  $State \times Female$  fixed effects (column (2)), and  $Female \times Year$  fixed effects (column (3)). Columns (4)–(6) report the estimates of Model (2), using the same fixed-effects scheme. In all regressions, the standard errors are clustered at the state-year level, to allow for heteroskedasticity of the within-state shock.

The data suggest that controlling for demographics and for the full matrix of fixed effects, working-age women have a significantly higher probability of not being in the labor force than working-age men. Age has an inverse U-shaped effect on the probability of being in the labor force, peaking at around 41.2 years of age and then decreasing again. Married and single individuals are less likely to be in the labor force than those who are divorced, separated, or widowed. Black individuals are 2.6 percentage points less likely to be in the labor force than whites, Hispanics, and Asian Americans. Finally, those with some college education are 6.8 percentage points more likely, and those with at most a high-school degree 8.2 percentage points less

likely than college drop-outs to be in the labor force.

The coefficient on the interaction term between the *IntrastateDeregulation* dummy and the *Female* dummy is negative and significantly different from zero at the .01 statistical level across all specification, suggesting that relative to a similar man, a working-age female in a state which has lifted restrictions on intrastate bank branching is less likely to not be in the labor force, compared to a state that has not deregulated intrastate bank branching yet. The coefficient fluctuates across specifications, underscoring the importance of including an exhaustive matrix of fixed effects. For example, in the specification with *State × Year* fixed effects only (column (1)), the numerical magnitude is half as large as in the specification with *State × Year* and *State × Female* fixed effects (column (2)), suggesting that unobservable time-invariant state-specific factors that affect males and females differently introduce a downward bias in the estimation. At the same time, adding *Female × Year* fixed effects (column (3)) reduces the magnitude of the coefficient by a factor of four, lending support to the concern that the effect of deregulation is to some degree contaminated by gender-specific developments that are common to all states, therefore, it is crucial to control for US-wide gender-specific trends. Nevertheless, the effect of intrastate deregulation is still significant at the .01 statistical level even in this exhaustive specification. Furthermore, columns (4)–(6) demonstrate that the effect of intrastate deregulation on labor force participation on the extensive margin is non-linear, increasing gradually and significantly in the years after intrastate deregulation.

The economic effect we report in Table IV is of a sizable magnitude. The point estimate in our preferred specification that controls for individual characteristics, for state-specific trends that affect both genders equally, for state-specific time-invariant factors that can have different effects across genders, and for gender-specific US-wide trends (column (3)) implies that removing intrastate branching restrictions reduces the probability that a working-age female is not the labor force participation by 1.4 percentage points relative to a similar man. Alternatively put, in our sample, 88.2 percent of all working-age men and 61.5 percent of all working-age women are in the labor force. Therefore intrastate bank branching deregulation reduces the gender gap by about 5.2 percent of the unconditional difference ( $0.014 = (88.2 - 61.5) * 0.052$ ). Our results thus suggest that to some extent, the change in female identity over the past several decades from housewives to career-oriented professionals (Goldin, 2006; Juhn and Potter, 2006) has—in the United States at least— been aided by increased competition in credit markets in the wake of intrastate deregulation.

We also note that interstate deregulation has no consistent effect on female labor force participation across

specification. While in columns (1) and (2), it reduces the relative probability that a female is not in the labor force, this effect becomes positive—albeit borderline insignificant—once we add a gender-specific trend. However, its magnitude is not sufficient to cancel out the impact of intrastate deregulation. Numerically, the gender gap in labor force participation in a state that has undergone both an intrastate and an interstate deregulation is around 2.6 percent lower than in a state that is still regulated in both dimensions. We conclude that on the extensive margin, the process of banking deregulation decreased the gender gap in labor force participation, and it did so exclusively through the lifting of restrictions on intrastate banking.

## 5.2 Banking deregulation and labor force participation: Robustness

### 5.2.1 Parallel trend assumption

We now address a number of concerns with our methodology. The first such concern is related to the possibility that the convergence between female and male labor supply that we observe in the wake of deregulation may have predated the exogenous shock to local credit market contestability brought about by banking deregulation. While in our main empirical tests we have conditioned on observable characteristics, there could still be pre-existing trends related to unobservable factors. If this were to be the case, we might incorrectly interpret pre-determined trends as evidence of the positive impact of the policy of interest (see Roberts and Whited, 2011).

To test for different trends between the two types of agents, we perform a placebo test in which we compare labor force participation by men and women during the period before deregulation. In practice, we pretend that the deregulation episode (both in the case of intrastate deregulation and in the case of interstate deregulation) took place 5 years earlier than it actually did. Moreover, we set the end point of the new sample period at the year of actual deregulation. Without loss of generality, we replicate Model (2), whereby the effect of increased credit market contestability is allowed to be nonlinear post-deregulation.

The estimates from these tests are reported in Table V. If treatment and control observations were already facing different prospects before intrastate deregulation (the policy event contributing to the convergence in labor force participation rates, according to Table IV), one should observe a statistically significant convergence in the gender gap in local labor markets already during the pre-deregulation period. However, the data overwhelmingly rejects this notion. In particular, in the case of intrastate deregulation, the simulated impact of deregulation on labor force participation on the extensive margin is both statistically and economically



indistinguishable from zero (column (1)). The evidence thus strongly suggests that the effect of intrastate deregulation that we reported in Table IV is indeed driven by changes in the relative behavior of women specific to the period after banking deregulation. At the same time, the data reject the hypothesis of no pre-existing trends in the case of interstate deregulation, which is possibly because by shifting the date of interstate deregulation, we now capture the effect of intrastate deregulation that typically takes place several years before interstate deregulation.

### 5.2.2 Socio-economic factors

Because our regression models include  $State \times Female$  fixed effects, our estimates are not contaminated by pre-existing differences across states that can affect men and women differently in labor markets. However, it is possible that deregulation was accompanied by concurrent shocks to various socio-economic factors that may independently affect female labor force participation. For example, women may be less likely to enter the labor force in markets characterized by high unemployment (i.e., markets with a lot of unemployed men). Marriage markets could play a role, too: the provision of paid work by single women may be subject to a lower stigma, and so markets with fewer single women could benefit less from deregulation. Alternatively, labor supply by married women could be more elastic (Goldin, 2014), delivering the opposite effect. The extent of home ownership can play a role, too, if women need to move within the state for a job, or to contribute to the mortgage payment. If trends in these factors start picking up around the time of deregulation, and if they affect women more than men, then our estimates may be biased.

To address this issue, we include in our empirical specifications interactions of the the *Female* dummy with the following state-specific, time-varying variables: the share of married women, the unemployment rate, and the share of home owners. Table VI reports the estimates from these tests. The results make it clear that socio-economic conditions matter for the relative labor supply. In particular, labor force participation by women, relative to men, is lower in local markets characterized by a higher share of married women, plausibly because of lower societal acceptance of working women. In addition, in markets characterized by a higher share of home ownership, women are more likely to be in the labor force, consistent with models whereby women-home owners are more likely to join the labor market in order to relax mortgage borrowing constraints (Fortin, 1995).

Importantly, we continue recording a statistically significant impact of intrastate bank branching dereg-

ulation on the narrowing of the gender gap in labor markets. The point estimate of interest is little changed relative to the estimates reported in our preferred specifications in Tables IV. We conclude that our data continue to suggest a strong, independent impact of deregulation on the gender gap in local labor markets even after controlling for a number of concurrent socio-economic developments.

### 5.2.3 Heterogeneous effects

A somewhat related concern is that individuals with particular characteristics entered the labor force, or increased the number of weeks they worked, at different times in different localities for endogenous reasons. The inclusion of  $Female \times Year$  fixed effect accounts for the possibility that at the US-wide level, women of different ability entered the labor market at different points in time. However, it is possible that deregulation increased the return to experience or education, affecting men and women in a different way (Olivetti, 2006).

To test for this concern, we augment our empirical specifications with interactions of the variables capturing the timing of deregulation with empirical proxies for age and education. These estimates are reported in Table VII. Clearly, observable proxies for experience and ability have a larger impact on the gender gap in the post-deregulation period, relative to pre-deregulation. For example, older people become more likely to be in the labor force after intrastate deregulation. Moreover, the return to both unskilled and more skilled work increases post both types of deregulation, as demonstrated by a higher propensity of those with high school at most and those with college or more—relative to college drop-outs—to be in the labor force. Crucially, the main result of the paper still obtains: banking deregulation has a statistically significant effect on the gender gap in labor force participation after accounting for the possibility that deregulation raised the return to (observable) experience and ability. Once again, the point estimate of interest is practically unchanged relative to the estimates reported in our preferred specifications in Table IV, suggesting that the main result in the paper is remarkably stable.

## 5.3 Banking deregulation and labor force participation: Contiguous MSAs

In our tests so far, we have been comparing females' labor market outcomes—relative to males' labor market outcomes—in a deregulated state (the treatment group) relative to a regulated state (the control group). As we already argued, this empirical strategy can produce biased estimates in the presence of unobservable trends which differ across states and which affect males and females differently. Economic conditions can

be different in deregulated states at the time of deregulation, and labor markets in those can already be becoming friendlier to females for reasons unrelated to deregulation. Models (1) and (2) allow us to estimate the average effect of deregulation net of the impact of individual characteristics that can determine propensity to work and/or equilibrium wages. They also allow us to eliminate the impact of unobservable state-specific trends that affect all agents equally, and of US-wide trends that exhibit a differential impact on men and on women. However, our results can still be contaminated by a host of unobservable factors that vary over time and affect women and men in a state differently, making the population of a regulated state a poor control group.

To assuage such concerns, we proceed to adopt the approach in Huang (2008) and we compare individuals in adjacent MSAs across neighboring U.S. states, one of which is deregulated while the other is still regulated. These treatment and control pairs at the MSA-state level are created by matching the pairs at the county-state level in Huang (2008) with the corresponding MSAs from IPUMS. We take into account the fact that the counties assigned to an MSA are changing over time. The matching exercise results in 14 pairs of treatment and control MSAs which are listed in Table III.<sup>3</sup> The assumption is that two neighboring MSAs are really one economic area when it comes to unobservable factors such as growth opportunities or labor market conditions. Hence, any discernible differences in how fast the labor gender gap narrows can be attributed to changes in banking market conditions in one MSA but not in the other.

Arguably, this test relies on two important assumptions. The first one is that the cost of migration across geographic localities is non-negligible, preventing labor markets from clearing immediately. Empirical evidence suggests that cross-state labor migration in the United States is indeed limited, with around 1 percent of the population "moving for a job" from one state to another annually, even during an economic peak (Demyanyk, Hryshko, Luengo-Prado, and Sorensen, 2017), with the rates of migration considerably lower for married women. The second assumption is that firms and individuals located close to a state border in a regulated state cannot easily benefit from deregulation in the adjacent state, through better access to finance across the border. This assumption has substantial empirical support in the literature, too: for example, Petersen and Rajan (2002) show that during our sample period, the median distance between a firm and its creditor was only 4 miles, suggesting that indeed firms tend to borrow from banks in their immediate proximity.

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<sup>3</sup>The approach in Huang (2008) relies on comparing contiguous counties. However, the MSA represents the lowest level of geographical disaggregation in the IPUMS-CPS samples.

We report the estimates from these modified versions of Model (1) and (2) in Table VIII. As we are only using individuals from the 14 treatment MSAs and the 14 control MSAs listed in Table III, the number of observations is reduced to 29,774. The evidence is more mixed than in Table IV. In particular, we find that on average, intrastate deregulation did not significantly decrease the probability that, relative to a similar man, a woman is not in the labor force (column (1)); however, this is entirely due to an increase in this probability during the first post-deregulation year (column (2)). In the long run (5+ years), women are 6.3 percentage points less likely to not be in the labor force, which represents a 24 percent decline in the gender gap, given that women are on average 26 percent less likely than men to be in the labor force. At the same time, the data strongly suggest that interstate deregulation induced no discernible change in relative female labor supply.

#### **5.4 Banking deregulation and labor force participation: Type of labor market activity**

We now proceed to test for how deregulation affected the choice of labor market activity. This is important because a post-deregulation shift in the type of employment would give us a signal about the potential mechanisms involved. In Table IX, we test a version of Models (1) and (2) where the dependent variable is, in turn, a dummy equal to one if an individual is employed in the public sector (columns (1) and (2)), a dummy equal to one if the individual is employed in the private sector (columns (3) and (4)), and a dummy equal to one if the individual is self-employed (columns (5) and (6)), and to zero otherwise. These tests allows us to describe employment choice for those entering the labor force, compared to those who choose to stay out of the labor force.

Our results imply that controlling for age, married individuals are more likely to be self-employed and less likely to work in the private sector, a finding consistent with Hurst and Lusardi (2004). Black individuals are more likely to be employed in the public administration than to be either self-employed or employed in the private sector. Finally, higher educational attainment increases the probability of working in the public sector and of being self-employed.

Crucially, the data suggest that in the wake of intrastate deregulation, women become significantly more likely—relative to men—to join the private sector (columns (3) and (4)), and significantly less likely to be employed in the public sector (columns (1) and (2)). The effect is economically meaningful, too: column (4)

suggests that in the long-run, intrastate deregulation increased the probability that a working-age woman is employed in the private sector by 2.6 percentage points. This constitutes a 7.3 percent increase, given that the share of working women employed in the private sector is 35.4 percentage points higher than the share of working men in private employment. We also find that post-deregulation, women did not become more likely in the long run—relative to men—to be self-employed (columns (5) and (6)). At the same time, interstate deregulation has a negative effect on the probability that controlling for demographics, a woman is employed in the private sector (columns (3) and (4)). However, numerically the effect is not large enough to neutralize the increase in private sector employment brought about by intrastate deregulation.

Table IX thus documents a significantly higher post-intrastate deregulation propensity of women to be employed in the private sector, suggesting that private sector employment is to a large extent responsible for the post-deregulation increase in labor force participation rates.

## **5.5 Banking deregulation and labor force participation: Economic mechanisms**

We now turn to the economic mechanisms that possibly underpin the impact of bank branching deregulation on the gender gap in local labor markets, in combination with the increased propensity for private sector employment. We single out three such mechanisms that have been proposed in the extant literature: changes in social norms, an increased demand for labor, and a shift towards a more service-oriented local economy. We report the impact of intrabank and interbank deregulation on these mechanisms in Table X, Panel A, and we report their impact on the gender gap in local labor market in Table X, Panel B.

### **5.5.1 Social norms**

The first mechanism is related to changes in social norms in the wake of deregulation. Figures 2–4 show that states with a larger share of educated women, a larger share of working females, and a culture that is more accepting of working women did not remove bank branching restrictions earlier. This suggests that pre-deregulation state of social norms is not correlated with the timing of deregulation. However, it is entirely possible that deregulation itself affected social norms. For example, by improving local dynamism and by raising incomes (Jayaratne and Strahan, 1996), banking deregulation may have improved attitudes towards women, and in particular towards working women. Therefore, some of the impact of deregulation on the gender gap in labor markets may have come from an increasing acceptance of working women, in

turn increasing the elasticity of the female labor supply, or the willingness of employers to hire women.

To account for this possibility, we now proceed to construct a proxy capturing time-varying attitudes towards working women. We start by taking survey answers from the GSS over the period 1977–2000.<sup>4</sup> The data set contains a range of demographic and income characteristics. Importantly, it contains the variable "FEFAM" which is defined by the answer to following question: "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family." The answers are given on a scale from 1 to 4. From these answers, we construct a variable which we denote as "Gender bias". The variable is equal to 1 if the respondent answered "Strongly agree" or "Agree", and equal to 0 if the respondent answered "Disagree" or "Strongly disagree". We next estimate the following regression model:

$$GenderBias_{i,r,t} = \beta_1 X_{i,r,t} + \beta_2 \Psi_{r,t} + \varepsilon_{i,r,t} \quad (3)$$

where  $X_{i,r,t}$  is a vector of individual-specific characteristics for individual  $i$  living in region  $r$  at time  $t$ , and it includes age, education, gender, religion, employment status, and income.  $\Psi_{r,t}$  is a matrix of year dummies and of 8 region dummies equal to 1 if the individual lives in one of the following 8 regions: Mid-Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific.<sup>5</sup> Then we take the point estimates on each regional dummy from Model (3) to represent a region-specific time-varying estimate of local gender bias, controlling for income and demographics, and we call this variable  $SocialNorms_{r,t}$ . We then evaluate the following two models:

$$SocialNorms_{r,t} = \beta_1 IntrastateDeregulation_{s,t-1} + \beta_2 InterstateDeregulation_{s,t-1} + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t} \quad (4)$$

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<sup>4</sup>See Ongena and Popov (2016) for a similar approach; 1977 is the earliest year when comprehensive data are available.

<sup>5</sup>Because respondents in GSS do not report their state of residence, but only 1 of 9 regions of residence, there is arguably more limited geographic heterogeneity in this test.

and

$$\begin{aligned}
SocialNorms_{r,t} = & \sum_{j=1}^5 \beta_1^j YearAfterIntrastateDeregulation_{s,t-1}^j \\
& + \sum_{j=1}^5 \beta_2^j YearAfterInterstateDeregulation_{s,t-1}^j \\
& + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t}
\end{aligned} \tag{5}$$

where, as before,  $IntrastateDeregulation_{s,t}$  is an indicator variable equal to one for states that have removed barriers to intrastate branching, and to zero otherwise;  $InterstateDeregulation_{s,t}$  is an indicator variable equal to one for states that have removed barriers to interstate branching, and to zero otherwise;  $YearAfterIntrastateDeregulation_{s,t}^j$  is a dummy variable equal to 1 for states observed  $j$  years after intrastate deregulation, and to 0 otherwise; and  $YearAfterInterstateDeregulation_{s,t}^j$  is a dummy variable equal to 1 for states observed  $j$  years after interstate deregulation, and to 0 otherwise. The regressions also control for state and year fixed effects.

Columns (1) and (2) of Table X, Panel A present the point estimates of Models (4) and (5). We find that following intrastate banking deregulation, the regional share of those who "Strongly agree" or "Agree" with the statement "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family" declined significantly (column (1)). This decline is economically meaningful as it corresponds to a 13.5 percent of the interquartile range. At the same time, the evidence reported in column (2) suggests that the decline in gender bias was concentrated in the first four years after deregulation, and starting in the fifth post-deregulation year, cultural attitudes towards women are no longer statistically different from the pre-deregulation period. We conclude that while there is evidence for a positive impact of banking deregulation on cultural norms, this impact appears to be short lived.

### 5.5.2 Job creation and reallocation rates

Our next hypothesis is that the increase in relative female labor force participation is driven by a post-deregulation increase in labor demand driven by increasing activity in the private sector.<sup>6</sup> Jayaratne and Strahan (1996) argue that banking deregulation increased efficiency in the US banking sector, leading to reduced corporate lending rates and a general increase in bank lending. Black and Strahan (2002) and

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<sup>6</sup>This mechanism is akin to the one in Levine, Levkov, and Rubinstein (2014) who argue that the new firms that emerged post-deregulation were able to compete successfully by hiring minorities who were previously discriminated against.

Cetorelli and Strahan (2006) show that as a result, new business creation in the non-financial sector intensified after both intrastate and interstate deregulation, generating an increase in the share of small and medium-sized firms in the economy. Furthermore, Kerr and Nanda (2009) qualify this effect by demonstrating that the new firms that emerged after intrastate deregulation were mostly long-lived, while interstate deregulation mostly increased the churn of new entrants.

We would like to understand how these phenomena relate to the demand for labor in the private sector, and the resulting rates of job creation and destruction. We collect data, by state and year, on the rate of net job creation (defined as the difference between the gross job creation rate and the gross job destruction rate) and the rate of job reallocation (defined as the sum of the gross job creation and the gross job destruction rate minus the absolute value of net job creation) in the private sector, from the Business Dynamics Statistics of the US Census. Because we want to understand the evolution of employment in the whole economy, and unlike Cetorelli and Strahan (2006), we use data on all industries, not only on industries within the manufacturing sector. Table II demonstrates that the average net job creation rate is 2.74 percent, and the average job reallocation rate is 29.83 percent.

Armed with these data, we estimate the following models:

$$\begin{aligned} NetJobCreationRate_{s,t} = & \beta_1 IntraStateDeregulation_{s,t-1} + \beta_2 InterstateDeregulation_{s,t-1} \\ & + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t} \end{aligned} \quad (6)$$

and

$$\begin{aligned} NetJobCreationRate_{s,t} = & \sum_{j=1}^5 \beta_1^j YearAfterIntraStateDeregulation_{s,t-1}^j \\ & + \sum_{j=1}^5 \beta_2^j YearAfterInterstateDeregulation_{s,t-1}^j \\ & + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t} \end{aligned} \quad (7)$$

for net job creation, and

$$\begin{aligned} JobReallocationRate_{s,t} = & \beta_1 IntraStateDeregulation_{s,t-1} + \beta_2 InterstateDeregulation_{s,t-1} \\ & + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t} \end{aligned} \quad (8)$$



and

$$\begin{aligned}
JobReallocationRate_{s,t} = & \sum_{j=1}^5 \beta_1^j YearAfterIntrastateDeregulation_{s,t-1}^j \\
& + \sum_{j=1}^5 \beta_2^j YearAfterInterstateDeregulation_{s,t-1}^j \\
& + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t}
\end{aligned} \tag{9}$$

for job reallocation.

Columns (3)–(6) of Table X, Panel A report the point estimates of Models (6)–(9). We find a large, statistically significant increase in the rate of net job creation after intrastate deregulation.<sup>7</sup> The post-intrastate-deregulation increase in net job creation rates is fairly large, corresponding to 0.22 of a sample standard deviation. This evidence is consistent with an increase in overall entrepreneurship (Black and Strahan, 2002) driven primarily by long-term entrants (Kerr and Nanda, 2009). We record no change in net job creation rates following interstate deregulation. At the same time, column (6) demonstrates that in the long-run, job reallocation rates increased substantially—by about a quarter of a sample standard deviation—following interstate deregulation. This effect is consistent with the evidence of higher rates of post-interstate-deregulation entry by short-lived firms presented in Kerr and Nanda (2009).

### 5.5.3 Shift towards services-producing sectors

A third possible channel is related to changes in the sectoral composition of the local economy. Firms in the services industry tend to be smaller than manufacturing firms, including in the United States (Buera and Kabosky, 2012), typically because of differences in scale, technology, and contracting. Because small firms find it more difficult to access financial services due to greater information and transaction costs (Cestone and White 2003, Galor and Zeira 1993), an improvement in access to finance that ameliorates these frictions benefits disproportionately small firms. This argument has received empirical support in the literature, both in an international sample (Beck, Demirguc-Kunt, Laeven, and Levine, 2008) and in the context of US banking deregulation (Cetorelli and Strahan, 2006). Therefore, it is plausible that by reducing the cost and increasing the availability of credit, banking deregulation accelerated the local economy’s secular shift towards services.

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<sup>7</sup>This effect is entirely driven by an increase in gross job creation and not by a decrease in gross job destruction; results omitted for brevity, but available upon request.

The literature has offered a number of arguments for why women would be the natural beneficiaries of a shift towards a more services-oriented local economy. First, the production of services is relatively less intensive in the use of "brawn" skills than the production of goods, and relatively more intensive in the use of "brain" skills. As men are better endowed of brawn skills than women, the historical growth in the service sector has created jobs for which women have a natural comparative advantage because of their more intensive use of communication and interpersonal skills (Galor and Weil, 1996; Goldin, 2006; Ngai and Petrongolo, 2017). Second, the simultaneous presence of producers and consumers in the provision of services makes these skills relatively more valuable in services. The rise in the use of interpersonal tasks accelerated between the late 1970s and the early 1990s, and women tend to be over-represented in these tasks, suggesting that they are relatively more endowed in those increasingly valuable interpersonal skills (Borghans, ter Weel, and Weinberg, 2014)). Finally, a recent strand of the experimental literature highlights gender differences in other social attitudes such as altruism, fairness, and caring behavior which women tend to be better endowed with (Bertrand, 2011) and which may be more highly valued in services than in manufacturing jobs.

To test this hypothesis, we construct a state-specific measure of the importance of services in the local economy by calculating, for each state-year, the share of the working-age population that is in the labor force and that is employed in services-producing goods.<sup>8</sup> The resulting variable  $ShareServices_{s,t}$  measures the evolving share of service employment in a state. Then we estimate the following two equations:

$$ShareServices_{s,t} = \beta_1 IntraStateDeregulation_{s,t-1} + \beta_2 InterstateDeregulation_{s,t-1} + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t} \quad (10)$$

and

$$ShareServices_{s,t} = \sum_{j=1}^5 \beta_1^j YearAfterIntraStateDeregulation_{s,t-1}^j + \sum_{j=1}^5 \beta_2^j YearAfterInterstateDeregulation_{s,t-1}^j + \beta_3 \Psi_s + \beta_4 \Phi_t + \varepsilon_{s,t} \quad (11)$$

Columns (7) and (8) of Table X, Panel A demonstrate that the two types of banking deregulation have an opposite effect on the shift towards a more services-oriented local economy. In particular, intrastate

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<sup>8</sup>These include all sectors in the variable IND1950 coded as 500 and higher.

deregulation significantly increases the share of service employment, by approximately 1 percentage point in the long run, while interstate deregulation reduces it by approximately the same magnitude. Because the regressions include year fixed effects, these changes should be understood in relation to the US-wide secular trend whereby the share of employment in services-producing sectors increased from 35 percentage points in 1970 to 52 percentage points in 2000.

#### 5.5.4 Economic mechanisms and the gender gap

Having determined how the banking deregulations of the 1970s, 1980s, and 1990s affected social norms, job creation and destruction, and the share of service employment in the local economy, we now proceed to test for the impact of these variables on the gender gap in local labor markets. Table X, Panel B reports estimates from tests where we replicate Model (1), but we replace the two types of banking deregulation with the time-varying proxies for social norms (column (1)), net job creation rates and job reallocation rates (column (2)), and the share of employment in services-producing sectors (column (3)). In all cases, the variables in question are interacted with the *Female* dummy, which allows us to gauge their impact on labor force participation by women, relative to men.

The estimates reported in Table X, Panel B suggest that not all of the empirical mechanisms we identified matter for the gender gap. In column (1), we find that higher local gender bias is associated with a lower relative female labor force participation, confirming that cultural attitudes matter. However, this effect is only significant at the 15 percent statistical level. We conclude that the temporary decline in gender bias in the wake of intrastate deregulation does not help explain the narrowing of the gender gap in labor force participation in local markets. We also find that higher rates of net job creation in the private sector increase significantly relative female labor force participation (column (2)). In combination with the evidence in Table X, Panel A, this result helps explain the reduction in the gender gap following intrastate deregulation. Finally, a higher share of employment in services-producing sectors reduces the gender gap in labor force participation (column (3)). Given the opposite effect of intrastate and of interstate deregulation on the trend towards a more service-oriented economy documented in Table X, Panel A, this finding provides further evidence to the mechanisms underlying the main results in the paper.<sup>9</sup>

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<sup>9</sup>In unreported regressions, we also include interactions of the *Female* dummy with the two *Deregulation* dummies in these regressions. We find that once we control for the empirical channels, the impact of interstate deregulation becomes statistically insignificant, and the impact of intrastate deregulation remains significant at the 5 percent statistical level, but the magnitude

The positive (albeit arguably insignificant) impact of declining gender bias on female labor force participation can take place either because employers become more likely to hire women, or because wages paid for market work increased relatively more for women than for men, generating a shift in relative wages. The latter could happen, for example, because as a result of increased interaction with loan offices, women become better at bargaining (Beck, Behr, and Guettler, 2013), or because after joining the labor force, women’s marginal return to experience increased substantially more than men’s (Olivetti, 2006). Coupled with the fact that female labor supply was relatively more wage-elastic during this period (Goldin, 2006; Bredemeier and Juessen, 2013), this mechanism could explain the increase in the relative supply of female market labor. However, we find that the gap between average male and female wages appears to increase, not decrease, in the wake of intrastate deregulation. This is plausibly the equilibrium outcome of a post-deregulation influx of relatively cheaper female labor, rather than the driver of increased female labor force participation.<sup>10</sup>

## 5.6 Qui bono? Race, age, and marital status

The final extension of our analysis is one whereby we look at how a number of predetermined demographic characteristics interact with banking deregulation to determine shifts in labor force participation. This analysis is meant to further inform us about who benefited most from banking deregulation, as well as to help us think about the distributional consequences of banking reform, in terms of labor market activity. For example, it is reasonable to conjecture that shifts in labor market activity were accompanied by shifts in schooling or fertility choices, too. It is therefore important to understand which demographic groups experienced a larger impact of deregulation, in order to inform models which link the gender gap to family structures and labor supply (Galor and Weil, 1996; Kimura and Yasui, 2010).

In Table XI, we take these questions to the data by estimating Models (1) and (2) across three different sample splits. In columns (1) and (2), we split the model by race (white vs. non-white); in columns (3) and (4), we split the sample by age (age group 25–45 vs. age group 46–65); and in columns (5) and (6), we split the sample by marital status (married vs. single / divorced). We find that on the extensive margin, the positive impact of intrastate deregulation on the probability of entering the labor force obtains exclusively for white women (column (1)) and for married women (column (5))). This increase is also numerically larger of the effect declines by as much as half. This suggests that the empirical channels we identify explain all of the effect of interstate deregulation, and much of the effect of intrastate deregulation, on female labor force participation.

<sup>10</sup>Results omitted for brevity, but available upon request.

for women younger than 45 (column (3)) than it is for women older than 45 (column (4)). We conclude that white women, married women, and to some extent younger women were the main beneficiaries from deregulation, in terms of the extensive margin of the labor supply. Our finding is consistent with the common wisdom that these groups experienced the most dramatic changes in self-perception, and therefore the highest increase in the propensity to shift their identity away from household production and towards formal market labor, during this period (Goldin, 2006).

## 6 Conclusion

The relative equality of opportunities between men and women in labor markets across Europe and North America nowadays—compared with most of the rest of the world and with most of history—is one of the principal achievements of modern western civilization. While a gender gap still persists in labor force participation in the United States, this gap closed rapidly between 1970 and 2000. In this paper, we show that the banking deregulations of that period, which opened local credit markets to competition from other banks, have played a substantial and previously undocumented role in the evolution of the gender gap in labor markets. Our results suggest that intrastate banking deregulation reduced the gender gap in labor force participation rates by at least 5 percent. The main result obtains in specifications which control for a wide range of observable individual-specific factors that can affect labor market outcomes, as well as for unobservable state-specific trends, gender-specific trends, and state-specific gender differences that persist over time. Importantly, it also obtains when we compare individuals across contiguous MSAs sharing a state border where unobservable confounding factors tend to be similar. We also find that interstate deregulation reduces somewhat female labor force participation rates, but not enough to eliminate the gains from intrastate deregulation. We argue that taken together, our tests corroborate a genuine bank deregulation effect on the gender gap in labor markets.

We next turn to investigating the mechanisms at play. Banking deregulation increased the efficiency of credit markets and made credit more widely available and more affordable, thereby increasing the rates of new business creation and the overall number of private firms (Black and Strahan, 2002). We show that this process was associated with a short-lived decline in gender bias and a long-term increase in the rates of net job creations and in the share of overall employment in the services-producing sector. The latter phenomena further increased the demand for labor in the private sector, more so in jobs where women enjoy comparative

advantage. We demonstrate that the reduction of the cultural stigma on working women and the increase in the demand for service-sector employment helps explain the rise in female labor force participation in the wake of intrastate deregulation.<sup>11</sup> This evidence is akin to the results in Levine, Levkov, and Rubinstein (2014) who argue that banking deregulation boosted black workers' relative wages by facilitating the entry of new firms (labor demand effect) and by reducing the manifestation of racial prejudices (discrimination effect). However, in our case the labor demand effect, both in terms of level and composition, appears to dominate the effect related to shifting cultural attitudes.

Evidence abounds that microfinance has improved the position of women in developing countries, both in labor markets and in marriage markets (e.g., Ngo and Wahhaj, 2012). Our paper provides evidence of the impact of access to finance on the gender gap in labor markets in a developed industrialized economy. While microfinance tends to work by enabling low-income women to take loans to support tiny enterprises (Cull, Demirguc-Kunt, and Morduch, 2009), our evidence suggests that in the United States, banking deregulation benefited women mostly through a rise in the demand for labor in the private, services-producing sector. We leave for future research the question if, just like in the case of microfinance, US banking reforms have reduced female poverty and increased the woman's bargaining position inside the household.

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<sup>11</sup>An alternative mechanism which we do not explore, but which is plausibly at work, too, operates through an increase in the female labor supply as cheaper consumer credit facilitates the adoption of home appliances, such as washing machines, laundry machines, and freezers. For a conceptual argument, see Becker (1981).

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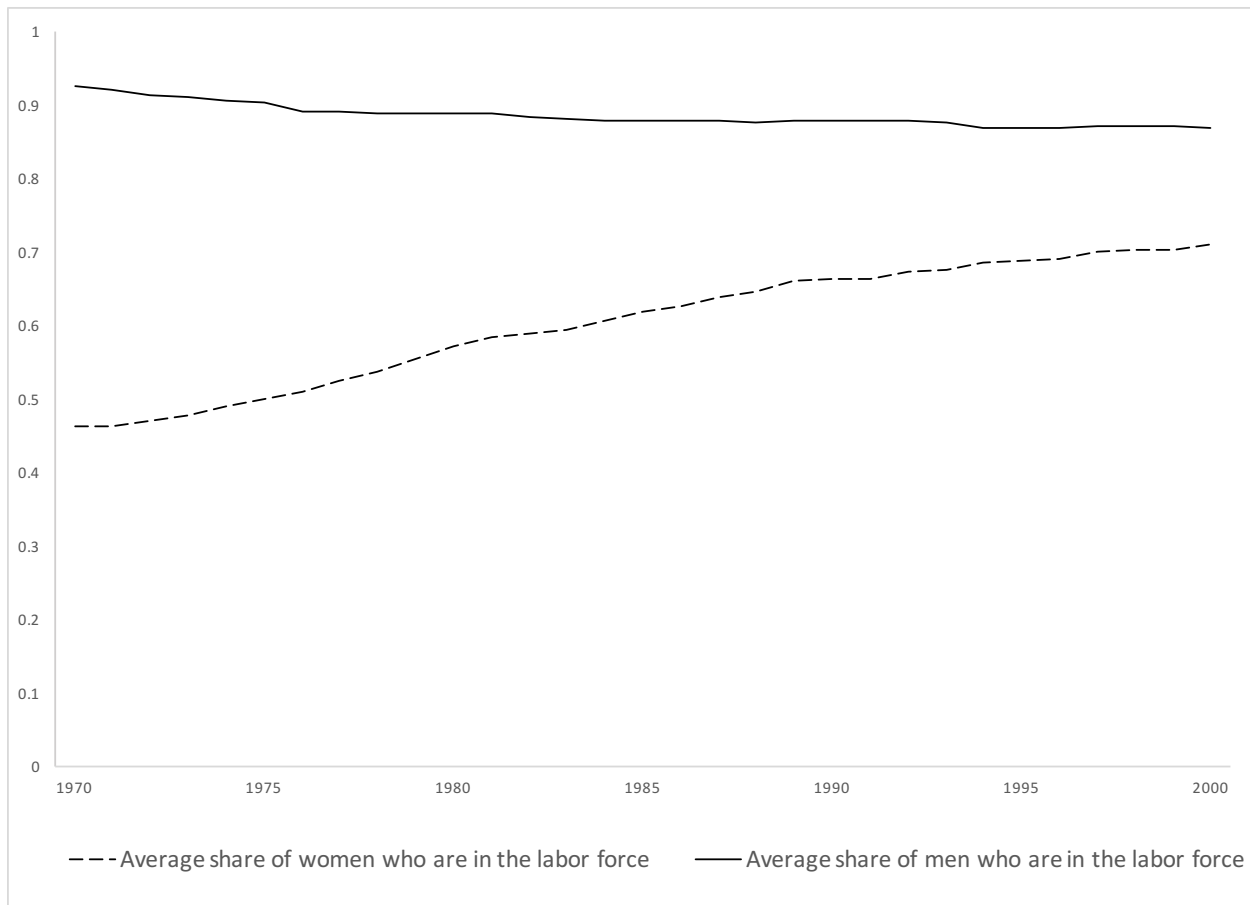
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**Figure 1:** Labor force participation of women and men

Note: The figure shows yearly averages of the share of women and men who were in the labor force in a given year. The shares are calculated as the proportion of women (men) who were in the labor force from the total population of working-age women (men). We use yearly individual-level data from IPUMS-CPS for the period 1970-2000. Respondents employed in the Finance, Insurance, and Real Estate sectors are excluded.

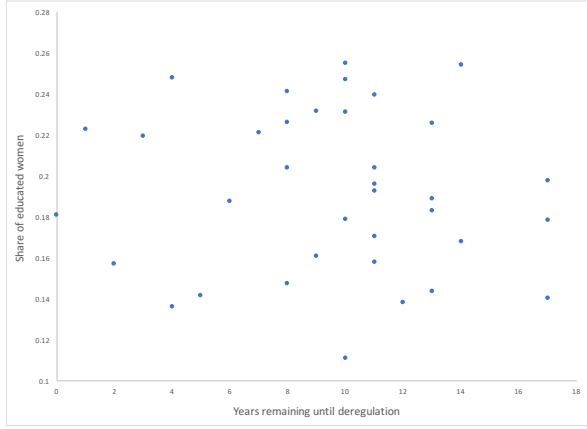


Figure 2: Share of educated women

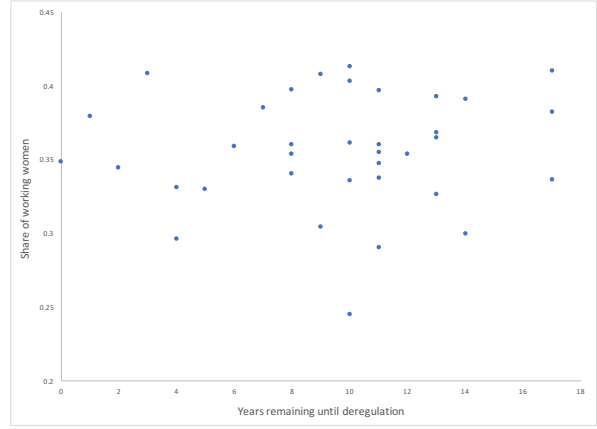


Figure 3: Share of working women

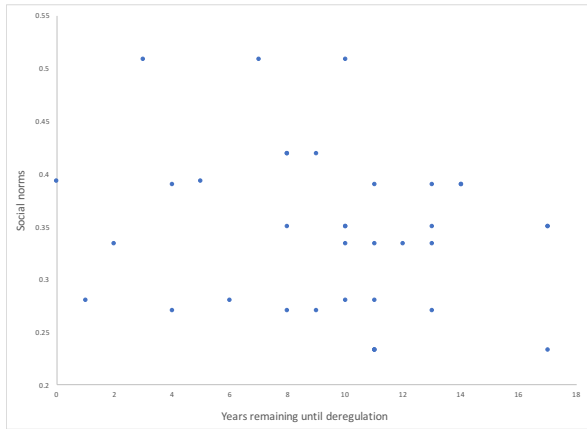


Figure 4: Social norms

Note: Figure 2 shows the years remaining until intra-state banking deregulation and the share of working-age women who have some college education (i.e. at least one year of college), calculated from averages at the state level in 1977. Figure 3 shows the years remaining until intra-state banking deregulation and the share of women who are in the labor force (i.e. self-employed, employed in the private sector, or employed by the government), calculated from averages at the state level in 1977. For both figures, we use yearly individual-level data from IPUMS-CPS. Figure 4 shows the years remaining until intra-state banking deregulation and the share of respondents in the General Social Survey who answer "Strongly disagree" or "Disagree" when confronted with the following statement: "It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family." The share is calculated in 1977 at the regional level.

Table I: Deregulation years by state

State	Intrastate deregulation year	Interstate deregulation year
Alabama	1981	1987
Alaska	1970	1982
Arizona	1970	1986
Arkansas	1994	1989
California	1970	1987
Colorado	1991	1988
Connecticut	1980	1983
Delaware	1970	1988
District of Columbia	1970	1985
Florida	1988	1985
Georgia	1983	1985
Hawaii	1986	1994
Idaho	1970	1985
Illinois	1988	1986
Indiana	1989	1986
Iowa	1994	1991
Kansas	1987	1992
Kentucky	1990	1984
Louisiana	1988	1987
Maine	1975	1978
Maryland	1970	1985
Massachusetts	1984	1983
Michigan	1987	1986
Minnesota	1994	1986
Mississippi	1986	1988
Missouri	1990	1986
Montana	1990	1993
Nebraska	1985	1990
Nevada	1970	1985
New Hampshire	1987	1987
New Jersey	1977	1986
New Mexico	1991	1989
New York	1976	1982
North Carolina	1970	1985
North Dakota	1987	1991
Ohio	1979	1985
Oklahoma	1988	1987
Oregon	1985	1986
Pennsylvania	1982	1986
Rhode Island	1970	1984
South Carolina	1970	1986
South Dakota	1970	1983
Tennessee	1985	1985
Texas	1988	1987
Utah	1981	1984
Vermont	1970	1988
Virginia	1978	1985
Washington	1985	1987
West Virginia	1987	1988
Wisconsin	1990	1987
Wyoming	1988	1987

Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970.

Table II: Summary statistics

	Observations	Mean	Median	St. dev.	Min	Max
Not in the labor force	1,965,168	0.26	0.00	0.44	0	1
Female	1,965,168	0.52	1.00	0.50	0	1
Age	1,965,168	42.45	41	11.57	25	65
White	1,965,168	0.87	1.00	0.33	0	1
Black	1,965,168	0.09	0.00	0.29	0	1
Other race	1,965,168	0.04	0.00	0.19	0	1
Single	1,965,168	0.12	0.00	0.33	0	1
Married	1,965,168	0.72	1.00	0.45	0	1
Divorced or widowed	1,965,168	0.15	0.00	0.36	0	1
High-school or less	1,965,168	0.60	1.00	0.49	0	1
College drop-out	1,965,168	0.26	0.00	0.44	0	1
College or more	1,965,168	0.13	0.00	0.34	0	1
Self employed	1,965,168	0.08	0.00	0.27	0	1
Employed (private sector)	1,965,168	0.49	0.00	0.50	0	1
Employed (public sector)	1,965,168	0.14	0.00	0.34	0	1
Unemployed	1,965,168	0.04	0.00	0.20	0	1
Share of married women	1,296	0.71	0.71	0.07	0.30	0.87
Unemployment rate	1,296	0.04	0.04	0.01	0.01	0.11
Share of homeownership	1,296	0.67	0.74	0.20	0.00	0.86
Social norms	882	0.09	0.07	0.07	-0.05	0.25
Net job creation rate	1,224	2.74	2.70	3.52	-16.70	27.10
Job reallocation rate	1,224	29.83	29.20	4.57	19	69.40
Services share	1,296	0.46	0.46	0.07	0.23	0.73

Note: This table presents summary statistics for the main variables used in the empirical tests. 'Not in the labor force' is a dummy equal to 1 if the respondent is not in the labor force (e.g. doing housework, or being unable to work, or going to school, etc.). 'Female' is a dummy equal to 1 if the respondent is a female. 'White', 'Black', and 'Other race' are dummies equal to 1 if the respondent is white, black, or other race, respectively. 'Single' is a dummy equal to 1 if the respondent is single. 'Married' is a dummy equal to 1 if the respondent is married. 'Divorced or widowed' is a dummy equal to 1 if the respondent is divorced or widowed. The education categories reveal the respondent's highest grade of school or year of college completed. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College drop-out' is a dummy equal to 1 if the respondent has between 1 and 4 years of college but no degree. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The employment variables 'Self employed', 'Employed (private sector)', 'Employed (public sector)', and 'Unemployed' classify the respondents according to their employment status and the occupation in which they worked the most hours. 'Self employed' is a dummy equal to 1 if the respondent is self-employed. 'Employed (private sector)' is a dummy equal to 1 if the respondent is an employee in private industry. 'Employed (public sector)' is a dummy equal to 1 if the respondent is an employee in public sector. 'Unemployed' is a dummy equal to 1 if the respondent is unemployed. 'Share of married women' is the proportion of women in a given state and year who are married. 'Unemployment rate' is the proportion of unemployed people in a given state and year who own their home. 'Share of homeownership' is the proportion of respondents in a given state and year who own their home. 'Social norms' is constructed as described in Model (3). 'Net job creation rate' denotes the difference between gross job creation rate and gross job destruction rate. 'Job reallocation rate' is calculated as 'gross job creation rate + gross job destruction rate - absolute value of net job creation rate'. 'Services share' is defined as the share of working-age population employed in services-producing industries. We use yearly individual-level data from IPUMS-CPS for the period 1970-2000. Respondents employed in the Finance, Insurance, and Real Estate sectors are excluded. The data on 'Net job creation rate' and 'Job reallocation rate' are from the Business Dynamics Statistics.



Table III: MSA pairs

<b>Treatment MSA</b>	<b>Treatment state</b>	<b>Control MSA</b>	<b>Control state</b>
Boston	Massachusetts	Manchester	New Hampshire
Boston	Massachusetts	Portsmouth-Dover-Rochester	New Hampshire
Cincinnati-Hamilton	Ohio	Cincinnati-Hamilton	Kentucky
Davenport-Rock Island-Moline	Illinois	Davenport-Rock Island-Moline	Iowa
Kansas City	Kansas	St. Louis	Missouri
Kansas City	Kansas	Kansas City	Missouri
Lima	Ohio	Fort Wayne	Indiana
Mobile	Alabama	Pensacola	Florida
Mobile	Alabama	Biloxi-Gulfport	Mississippi
Nashville	Tennessee	Cincinnati-Hamilton	Kentucky
Philadelphia	New Jersey	Philadelphia	Pennsylvania
Pittsburg	Pennsylvania	Wheeling	West Virginia
Portland	Maine	Portsmouth-Dover-Rochester	New Hampshire
Toledo	Ohio	Detroit	Michigan
Toledo	Ohio	Ann Arbor	Michigan

Note: The control MSAs St. Louis (Missouri), Biloxi-Gulfport (Mississippi), Cincinnati-Hamilton (Kentucky), Detroit (Michigan), and Ann Arbor (Michigan) are not direct neighbors of their treatment MSA, but directly adjacent to the MSA contiguous to the treated MSA (they correspond to the "hinterland counties" in Huang (2008)).

Table IV: The effect of banking deregulation on labor force participation

	Not in the labor force					
	1	2	3	4	5	6
Intrastate deregulation × Female	-0.0377*** (0.0068)	-0.0625*** (0.0059)	-0.0141*** (0.0031)			
Interstate deregulation × Female	-0.1016*** (0.0052)	-0.0879*** (0.0047)	0.0068* (0.0039)			
1 year after intrastate deregulation × Female				-0.0183 (0.0137)	-0.0342*** (0.0090)	-0.0060 (0.0053)
2 years after intrastate deregulation × Female				-0.0264** (0.0117)	-0.0437*** (0.0086)	-0.0132*** (0.0042)
3 years after intrastate deregulation × Female				-0.0270** (0.0117)	-0.0450*** (0.0082)	-0.0131*** (0.0048)
4 years after intrastate deregulation × Female				-0.0293*** (0.0099)	-0.0488*** (0.0080)	-0.0156*** (0.0047)
5+ years after intrastate deregulation × Female				-0.0423*** (0.0064)	-0.0822*** (0.0062)	-0.0186*** (0.0038)
1 year after interstate deregulation × Female				-0.0651*** (0.0083)	-0.0515*** (0.0068)	0.0043 (0.0049)
2 years after interstate deregulation × Female				-0.0669*** (0.0078)	-0.0520*** (0.0057)	0.0095* (0.0053)
3 years after interstate deregulation × Female				-0.0698*** (0.0061)	-0.0548*** (0.0047)	0.0085 (0.0054)
4 years after interstate deregulation × Female				-0.0700*** (0.0073)	-0.0525*** (0.0050)	0.0173*** (0.0064)
5+ years after interstate deregulation × Female				-0.1042*** (0.0044)	-0.0803*** (0.0045)	0.0152** (0.0061)
Female	0.3417*** (0.0042)	0.3309*** (0.0077)	0.4445*** (0.0095)	0.3408*** (0.0043)	0.3339*** (0.0067)	0.4426*** (0.0100)
Age	-0.0411*** (0.0003)	-0.0411*** (0.0003)	-0.0412*** (0.0003)	-0.0411*** (0.0003)	-0.0412*** (0.0003)	-0.0412*** (0.0003)
Age squared	0.0005*** (0.0000)	0.0005*** (0.0000)	0.0005*** (0.0000)	0.0005*** (0.0000)	0.0005*** (0.0000)	0.0005*** (0.0000)

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Table IV: Continued

	Not in the labor force					
	1	2	3	4	5	6
Married	0.0506*** (0.0017)	0.0509*** (0.0017)	0.0511*** (0.0017)	0.0507*** (0.0017)	0.0510*** (0.0017)	0.0511*** (0.0017)
Single	0.0583*** (0.0022)	0.0586*** (0.0021)	0.0582*** (0.0021)	0.0583*** (0.0021)	0.0586*** (0.0021)	0.0582*** (0.0021)
Black	0.0261*** (0.0022)	0.0262*** (0.0022)	0.0264*** (0.0022)	0.0261*** (0.0022)	0.0263*** (0.0022)	0.0264*** (0.0022)
High-school or less	0.0820*** (0.0012)	0.0817*** (0.0012)	0.0814*** (0.0012)	0.0819*** (0.0012)	0.0816*** (0.0012)	0.0814*** (0.0012)
College or more	-0.0684*** (0.0014)	-0.0685*** (0.0014)	-0.0681*** (0.0014)	-0.0682*** (0.0013)	-0.0683*** (0.0014)	-0.0681*** (0.0014)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Female FE	No	Yes	Yes	No	Yes	Yes
Year × Female FE	No	No	Yes	No	No	Yes
Observations	1,964,922	1,964,922	1,964,922	1,964,922	1,964,922	1,964,922
Pseudo R-squared	0.174	0.175	0.177	0.174	0.175	0.177

The table reports probit estimates of labor force participation. The dependent variable 'Not in the labor force' is a dummy equal to 1 if the respondent is not in the labor force (e.g doing housework, or being unable to work, or going to school, etc.). The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. '1 year after intrastate deregulation' is a dummy equal to 1 if intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 65 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970. The year in which each state deregulated is dropped. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table V: Parallel trend assumption

	Not in the labor force	
	1	2
1 year after shifted intrastate deregulation $\times$ Female	0.002 (0.005)	
2 years after shifted intrastate deregulation $\times$ Female	-0.000 (0.006)	
3 years after shifted intrastate deregulation $\times$ Female	-0.001 (0.006)	
4 years after shifted intrastate deregulation $\times$ Female	-0.006 (0.006)	
5+ years after shifted intrastate deregulation $\times$ Female	0.009 (0.015)	
1 year after shifted interstate deregulation $\times$ Female		-0.009* (0.005)
2 years after shifted interstate deregulation $\times$ Female		0.002 (0.007)
3 years after shifted interstate deregulation $\times$ Female		0.004 (0.007)
4 years after shifted interstate deregulation $\times$ Female		-0.004 (0.006)
5+ years after shifted interstate deregulation $\times$ Female		-0.139*** (0.008)
Female	0.458*** (0.012)	0.457*** (0.013)
Age	-0.042*** (0.001)	-0.042*** (0.001)
Age squared	0.001*** (0.000)	0.001*** (0.000)
Married	0.088*** (0.002)	0.088*** (0.002)
Single	0.067*** (0.005)	0.067*** (0.005)
Black	0.014*** (0.004)	0.014*** (0.004)
High-school or less	0.072*** (0.002)	0.072*** (0.002)
College or more	-0.092*** (0.003)	-0.092*** (0.003)
State $\times$ Year FE	Yes	Yes
State $\times$ Female FE	Yes	Yes
Year $\times$ Female FE	Yes	Yes
Observations	663,218	663,218
Pseudo R-squared	0.206	0.206

The table reports probit estimates of labor force participation. The dependent variable is 'Not in the labor force', a dummy equal to 1 if the respondent is not in the labor force. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. For the regressions reported in this table, both intrastate and interstate deregulation years are shifted five years earlier. For each state, the period considered ends at the deregulation year. '1 year after shifted intrastate deregulation' is a dummy equal to 1 if shifted intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 65 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the original deregulation year equal to 1970. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table VI: Controlling for socio-economic factors

	Not in the labor force	
	1	2
Intrastate deregulation × Female	-0.011*** (0.003)	
Interstate deregulation × Female	0.006* (0.004)	
1 year after intrastate deregulation × Female		-0.004 (0.005)
2 years after intrastate deregulation × Female		-0.011*** (0.004)
3 years after intrastate deregulation × Female		-0.010** (0.005)
4 years after intrastate deregulation × Female		-0.012*** (0.004)
5+ years after intrastate deregulation × Female		-0.015*** (0.004)
1 year after interstate deregulation × Female		0.003 (0.005)
2 years after interstate deregulation × Female		0.009* (0.005)
3 years after interstate deregulation × Female		0.008 (0.005)
4 years after interstate deregulation × Female		0.016** (0.006)
5+ years after interstate deregulation × Female		0.014** (0.006)
Share of married women × Female	0.250*** (0.043)	0.237*** (0.043)
Unemployment rate × Female	-0.066 (0.087)	-0.120 (0.088)
Share of homeownership × Female	-0.119*** (0.024)	-0.121*** (0.025)
Female	0.272*** (0.034)	0.281*** (0.033)
Age	-0.041*** (0.000)	-0.041*** (0.000)
Age squared	0.001*** (0.000)	0.001*** (0.000)

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Table VI: Continued

	Not in the labor force	
	1	2
Married	0.051*** (0.002)	0.051*** (0.002)
Single	0.058*** (0.002)	0.058*** (0.002)
Black	0.026*** (0.002)	0.026*** (0.002)
High-school or less	0.081*** (0.001)	0.081*** (0.001)
College or more	-0.068*** (0.001)	-0.068*** (0.001)
State $\times$ Year FE	Yes	Yes
State $\times$ Female FE	Yes	Yes
Year $\times$ Female FE	Yes	Yes
Observations	1,964,922	1,964,922
Pseudo R-squared	0.177	0.177

The table reports probit estimates of labor force participation. The dependent variable is 'Not in the labor force', a dummy equal to 1 if the respondent is not in the labor force. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. '1 year after intrastate deregulation' is a dummy equal to 1 if intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. 'Share of married women' is the proportion of women in a given state and year who are married. 'Unemployment rate' is the proportion of unemployed people in a given state and year. 'Share of homeownership' is the proportion of respondents in a given state and year who own their home. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 65 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970. The year in which each state deregulated is dropped. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table VII: Controlling for heterogeneous effects

	Not in the labor force	
	1	2
Intrastate deregulation × Female	-0.011*** (0.003)	
Interstate deregulation × Female	0.009** (0.004)	
1 year after intrastate deregulation × Female		-0.003 (0.005)
2 years after intrastate deregulation × Female		-0.010** (0.004)
3 years after intrastate deregulation × Female		-0.010** (0.005)
4 years after intrastate deregulation × Female		-0.013*** (0.005)
5+ years after intrastate deregulation × Female		-0.016*** (0.004)
1 year after interstate deregulation × Female		0.007 (0.005)
2 years after interstate deregulation × Female		0.012** (0.005)
3 years after interstate deregulation × Female		0.011** (0.005)
4 years after interstate deregulation × Female		0.020*** (0.006)
5+ years after interstate deregulation × Female		0.018*** (0.006)
Intrastate deregulation × Age	-0.004*** (0.001)	-0.004*** (0.001)
Intrastate deregulation × Age squared	0.000*** (0.000)	0.000*** (0.000)
Intrastate deregulation × High-school or less	0.012*** (0.003)	0.012*** (0.003)
Intrastate deregulation × College or more	0.011** (0.005)	0.011** (0.005)
Interstate deregulation × Age squared	0.000*** (0.000)	0.000*** (0.000)
Interstate deregulation × High-school or less	0.011*** (0.003)	0.011*** (0.003)
Interstate deregulation × College or more	0.029*** (0.005)	0.029*** (0.005)
Female	0.440*** (0.010)	0.439*** (0.010)
Age	-0.038*** (0.000)	-0.038*** (0.000)
Age squared	0.001*** (0.000)	0.001*** (0.000)

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Table VII: Continued

	Not in the labor force	
	1	2
Married	0.051*** (0.002)	0.051*** (0.002)
Single	0.060*** (0.002)	0.060*** (0.002)
Black	0.026*** (0.002)	0.026*** (0.002)
High-school or less	0.068*** (0.002)	0.068*** (0.002)
College or more	-0.092*** (0.002)	-0.092*** (0.002)
State $\times$ Year FE	Yes	Yes
State $\times$ Female FE	Yes	Yes
Year $\times$ Female FE	Yes	Yes
Observations	1,964,922	1,964,922
Pseudo R-squared	0.177	0.177

The table reports probit estimates of labor force participation. The dependent variable is 'Not in the labor force', a dummy equal to 1 if the respondent is not in the labor force. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. '1 year after intrastate deregulation' is a dummy equal to 1 if intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 65 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970. The year in which each state deregulated is dropped. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.



Table VIII: Contiguous MSAs

	Not in the labor force	
	1	2
Intrastate deregulation $\times$ Female	-0.010 (0.014)	
Interstate deregulation $\times$ Female	-0.025 (0.030)	
1 year after intrastate deregulation $\times$ Female		0.059*** (0.011)
2 years after intrastate deregulation $\times$ Female		0.017 (0.023)
3 years after intrastate deregulation $\times$ Female		-0.029*** (0.010)
4 years after intrastate deregulation $\times$ Female		-0.018 (0.015)
5+ years after intrastate deregulation $\times$ Female		-0.063*** (0.020)
1 year after interstate deregulation $\times$ Female		0.016 (0.030)
2 years after interstate deregulation $\times$ Female		-0.030 (0.025)
3 years after interstate deregulation $\times$ Female		-0.045** (0.018)
4 years after interstate deregulation $\times$ Female		0.001 (0.034)
5+ years after interstate deregulation $\times$ Female		-0.014 (0.037)
Female	0.263*** (0.036)	0.260*** (0.031)
Age	-0.039*** (0.002)	-0.039*** (0.002)
Age squared	0.001*** (0.000)	0.001*** (0.000)

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Table VIII: Continued

	Not in the labor force	
	1	2
Married	0.067*** (0.007)	0.068*** (0.007)
Single	0.050*** (0.014)	0.051*** (0.014)
Black	0.042*** (0.010)	0.043*** (0.010)
High-school or less	0.090*** (0.009)	0.090*** (0.009)
College or more	-0.069*** (0.015)	-0.069*** (0.015)
MSA $\times$ Year FE	Yes	Yes
MSA $\times$ Female FE	Yes	Yes
Year $\times$ Female FE	Yes	Yes
Observations	29,774	29,774
Pseudo R-squared	0.182	0.183

The table reports probit estimates of labor force participation using a selected sample of 14 pairs of contiguous MSAs derived from data provided by Huang (2008). The dependent variable is 'Not in the labor force', a dummy equal to 1 if the respondent is not in the labor force. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. '1 year after intrastate deregulation' is a dummy equal to 1 if intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 65 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970. The year in which each state deregulated is dropped. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by MSA-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table IX: Choice of employment

	Employed public sector		Employed private sector		Self employed	
	1	2	3	4	5	6
Intrastate deregulation × Female	-0.0112*** (0.0027)		0.0084** (0.0035)		0.0009 (0.0018)	
Interstate deregulation × Female	0.0025 (0.0031)		-0.0078* (0.0047)		0.0014 (0.0024)	
1 year after intrastate deregulation × Female		-0.0135*** (0.0044)		0.0120** (0.0055)		0.0041* (0.0024)
2 years after intrastate deregulation × Female		-0.0064 (0.0048)		0.0070 (0.0060)		0.0020 (0.0030)
3 years after intrastate deregulation × Female		-0.0068 (0.0046)		0.0032 (0.0057)		-0.0014 (0.0030)
4 years after intrastate deregulation × Female		-0.0113*** (0.0039)		0.0015 (0.0063)		0.0047 (0.0032)
5+ years after intrastate deregulation × Female		-0.0135*** (0.0032)		0.0117*** (0.0044)		-0.0015 (0.0022)
1 year after interstate deregulation × Female		0.0012 (0.0038)		-0.0058 (0.0059)		0.0008 (0.0032)
2 years after interstate deregulation × Female		0.0048 (0.0040)		-0.0129** (0.0058)		0.0019 (0.0033)
3 years after interstate deregulation × Female		0.0018 (0.0053)		-0.0047 (0.0067)		0.0032 (0.0036)
4 years after interstate deregulation × Female		0.0071 (0.0053)		-0.0131* (0.0070)		0.0036 (0.0034)
5+ years after interstate deregulation × Female		0.0053 (0.0051)		-0.0099 (0.0069)		0.0010 (0.0035)
Female	0.0564*** (0.0072)	0.0540*** (0.0078)	-0.0412*** (0.0090)	-0.0396*** (0.0093)	-0.0352*** (0.0063)	-0.0350*** (0.0063)
Age	0.0091*** (0.0003)	0.0091*** (0.0003)	-0.0102*** (0.0005)	-0.0102*** (0.0005)	0.0063*** (0.0003)	0.0063*** (0.0003)
Age squared	-0.0001*** (0.0000)	-0.0001*** (0.0000)	0.0001*** (0.0000)	0.0001*** (0.0000)	-0.0000*** (0.0000)	-0.0000*** (0.0000)

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Table IX: Continued

	Employed public sector		Employed private sector		Self employed	
	1	2	3	4	5	6
Married	0.0215*** (0.0011)	0.0215*** (0.0011)	-0.0260*** (0.0016)	-0.0260*** (0.0016)	0.0330*** (0.0009)	0.0330*** (0.0009)
Single	-0.0022 (0.0017)	-0.0022 (0.0017)	-0.0048** (0.0021)	-0.0048** (0.0021)	0.0027** (0.0013)	0.0027** (0.0013)
Black	0.0928*** (0.0021)	0.0928*** (0.0021)	-0.0716*** (0.0024)	-0.0716*** (0.0024)	-0.0615*** (0.0009)	-0.0615*** (0.0009)
High-school or less	-0.0923*** (0.0013)	-0.0923*** (0.0013)	0.0595*** (0.0016)	0.0595*** (0.0016)	-0.0036*** (0.0009)	-0.0036*** (0.0009)
College or more	0.1257*** (0.0026)	0.1257*** (0.0026)	-0.1356*** (0.0029)	-0.1356*** (0.0029)	0.0125*** (0.0012)	0.0125*** (0.0012)
State $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State $\times$ Female FE	Yes	Yes	Yes	Yes	Yes	Yes
Year $\times$ Female FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,460,710	1,460,710	1,460,710	1,460,710	1,460,710	1,460,710
Pseudo R-squared	0.065	0.065	0.030	0.030	0.048	0.048

The table reports probit estimates of employment choice. The dependent variable in columns 1 and 2 is 'Employed (public sector)', a dummy equal to 1 if the respondent is an employee in public sector. The dependent variable in columns 3 and 4 is 'Employed (private sector)', a dummy equal to 1 if the respondent is an employee in private industry. The dependent variable in columns 5 and 6 is 'Self employed', a dummy equal to 1 if the respondent is self-employed. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. '1 year after intrastate deregulation' is a dummy equal to 1 if intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 65 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970. The year in which each state deregulated is dropped. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table X: Empirical Mechanisms - Panel A

	Social norms		Net job creation rate		Job reallocation rate		Services share	
	1	2	3	4	5	6	7	8
Intrastate deregulation	-0.0131*** (0.0046)		0.7960** (0.3432)		-0.1786 (0.3433)		0.0036* (0.0022)	
Interstate deregulation	-0.0032 (0.0048)		0.3567 (0.3609)		0.4992 (0.3536)		-0.0056** (0.0026)	
1 year after intrastate deregulation		-0.0128* (0.0071)		0.5634 (0.3521)		-0.3382 (0.3834)		-0.0039 (0.0034)
2 years after intrastate deregulation		-0.0124* (0.0071)		0.9970*** (0.3829)		-0.9662** (0.4032)		-0.0021 (0.0036)
3 years after intrastate deregulation		-0.0131** (0.0067)		1.0448** (0.4596)		-0.4633 (0.4119)		0.0054 (0.0037)
4 years after intrastate deregulation		-0.0108* (0.0061)		0.5526 (0.4507)		0.4198 (0.5189)		0.0020 (0.0039)
5+ years after intrastate deregulation		-0.0076 (0.0064)		0.7348* (0.4318)		0.2796 (0.4362)		0.0098*** (0.0027)
1 year after interstate deregulation		-0.0043 (0.0059)		0.6881* (0.4076)		-0.0466 (0.4171)		-0.0067** (0.0031)
2 years after interstate deregulation		-0.0069 (0.0060)		0.4687 (0.4291)		0.3028 (0.4738)		-0.0052 (0.0033)
3 years after interstate deregulation		-0.0042 (0.0067)		0.1440 (0.4995)		0.6966 (0.4827)		-0.0052 (0.0041)
4 years after interstate deregulation		0.0049 (0.0061)		-0.0804 (0.5425)		0.7891 (0.4907)		-0.0145*** (0.0042)
5+ years after interstate deregulation		0.0081 (0.0065)		-0.2496 (0.5771)		1.1850** (0.5433)		-0.0101*** (0.0039)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	882	882	1,188	1,188	1,188	1,188	1,296	1,296
Adjusted R-squared	0.704	0.703	0.384	0.382	0.642	0.645	0.917	0.918

The table reports OLS estimates of various empirical mechanisms. The dependent variable in columns 1 and 2 is 'Social norms', constructed as described in Model (3). The dependent variable in columns 3 and 4 is 'Net job creation rate', defined as the difference between gross job creation rate and gross job destruction rate. The dependent variable in columns 5 and 6 is 'Job reallocation rate', calculated as 'gross job creation rate + gross job destruction rate - absolute value of net job creation rate'. The dependent variable in columns 7 and 8 is 'Services share', defined as the share of working-age population employed in services-producing industries. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. '1 year after intrastate deregulation' is a dummy equal to 1 if intrastate bank branching restrictions have been lifted one year before. Similar dummies are constructed for 2, 3, 4 and 5+ years, and for interstate deregulation. Banking deregulation dates follow Amel (1993). The year in which each state deregulated is dropped. The data on social norms come from the General Social Survey. The data on 'Net job creation rate' and 'Job reallocation rate' are from the Business Dynamics Statistics. The data on the employment in services-producing industries come from IPUMS-CPS. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table X: Empirical Mechanisms - Panel B

	Not in the labor force		
	1	2	3
Social norms $\times$ Female	0.042 (0.030)		
Net job creation rate $\times$ Female		-0.001* (0.001)	
Job reallocation rate $\times$ Female		-0.001 (0.000)	
Services share $\times$ Female			-0.527*** (0.043)
Female	0.316*** (0.010)		0.570*** (0.013)
Age	-0.042*** (0.000)	-0.040*** (0.000)	-0.041*** (0.000)
Age squared	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Married	0.054*** (0.002)	0.043*** (0.002)	0.051*** (0.002)
Single	0.061*** (0.003)	0.060*** (0.002)	0.058*** (0.002)
Black	0.028*** (0.003)	0.029*** (0.002)	0.026*** (0.002)
High-school or less	0.083*** (0.001)	0.082*** (0.001)	0.081*** (0.001)
College or more	-0.071*** (0.002)	-0.064*** (0.001)	-0.068*** (0.001)
State $\times$ Year FE	Yes	Yes	Yes
State $\times$ Female FE	Yes	Yes	Yes
Year $\times$ Female FE	Yes	Yes	Yes
Observations	1,311,217	1,711,221	1,964,922
Pseudo R-squared	0.181	0.168	0.177

The table reports probit estimates of labor force participation. The dependent variable is 'Not in the labor force', a dummy equal to 1 if the respondent is not in the labor force. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Social norms' is constructed as described in Model (3). 'Net job creation rate' is defined as the difference between gross job creation rate and gross job destruction rate. 'Job reallocation rate' is calculated as 'gross job creation rate + gross job destruction rate - absolute value of net job creation rate'. 'Services share' is defined as the share of working-age population employed in services-producing industries. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 64 years is included. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

Table XI: Sample splits

	Not in the labor force					
	By race		By age		By marital status	
	1	2	3	4	5	6
Intrastate deregulation × Female	-0.015*** (0.003)	-0.011 (0.008)	-0.014*** (0.004)	-0.010** (0.005)	-0.014*** (0.004)	-0.004 (0.006)
Interstate deregulation × Female	0.007 (0.004)	0.009 (0.010)	0.006 (0.005)	0.007 (0.007)	0.008* (0.004)	0.005 (0.007)
Female	0.466*** (0.009)	0.333*** (0.020)	0.454*** (0.011)	0.430*** (0.013)	0.516*** (0.010)	0.126*** (0.016)
Age	-0.042*** (0.000)	-0.037*** (0.001)	0.002 (0.001)	-0.131*** (0.002)	-0.049*** (0.000)	-0.030*** (0.001)
Age squared	0.001*** (0.000)	0.001*** (0.000)	-0.000** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.000*** (0.000)
Married	0.066*** (0.002)	-0.036*** (0.003)	0.043*** (0.002)	0.050*** (0.002)		
Single	0.054*** (0.002)	0.050*** (0.004)	0.054*** (0.002)	0.106*** (0.004)		
Black			0.016*** (0.002)	0.039*** (0.003)	-0.030*** (0.002)	0.090*** (0.003)
High-school or less	0.073*** (0.001)	0.128*** (0.003)	0.068*** (0.001)	0.097*** (0.002)	0.061*** (0.001)	0.120*** (0.002)
College or more	-0.069*** (0.001)	-0.053*** (0.004)	-0.048*** (0.001)	-0.103*** (0.002)	-0.062*** (0.001)	-0.064*** (0.003)
State × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Female FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Female FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,714,771	250,108	1,150,422	781,086	1,423,998	540,924
Pseudo R-squared	0.193	0.124	0.153	0.163	0.223	0.131

The table reports probit estimates of labor force participation. The sample is split by race into white respondents (column 1) and non-white respondents (column 2), into age groups between 25 and 44 (column 3) and between 45 and 65 (column 4), and by marital status into married respondents (column 5) and not married respondents (column 6). The dependent variable is 'Not in the labor force', a dummy equal to 1 if the respondent is not in the labor force. The regressions exclude respondents employed in the Finance, Insurance, and Real Estate sectors. 'Intrastate deregulation' is a dummy equal to 1 if bank branching restrictions within the state have been lifted. 'Interstate deregulation' is a dummy equal to 1 if the state has lifted interstate bank branching restrictions. 'Female' is a dummy equal to 1 if the respondent is a female. 'Married' is a dummy equal to 1 if the respondent is married. 'Single' is a dummy equal to 1 if the respondent is single. The omitted category in marital status is 'Divorced or widowed'. 'Black' is a dummy equal to 1 if the respondent is black. 'High-school or less' is a dummy equal to 1 if the respondent has between 0 and 12 years of school and obtained at most a high-school diploma. 'College or more' is a dummy equal to 1 if the respondent has at least a Bachelor's degree. The omitted category in education is 'College drop-out'. We use yearly household data from IPUMS-CPS for the period 1970-2000. Only the population aged between 25 and 64 years is included. Banking deregulation dates follow Amel (1993). For the states that deregulated before 1970 we set the deregulation year equal to 1970. The year in which each state deregulated is dropped. All estimates are weighted by sampling weights provided by the Current Population Survey. Standard errors clustered by state-year are reported in parentheses, where \*\*\* indicates significance at the 1% level, \*\* at the 5% level, and \* at the 10% level.

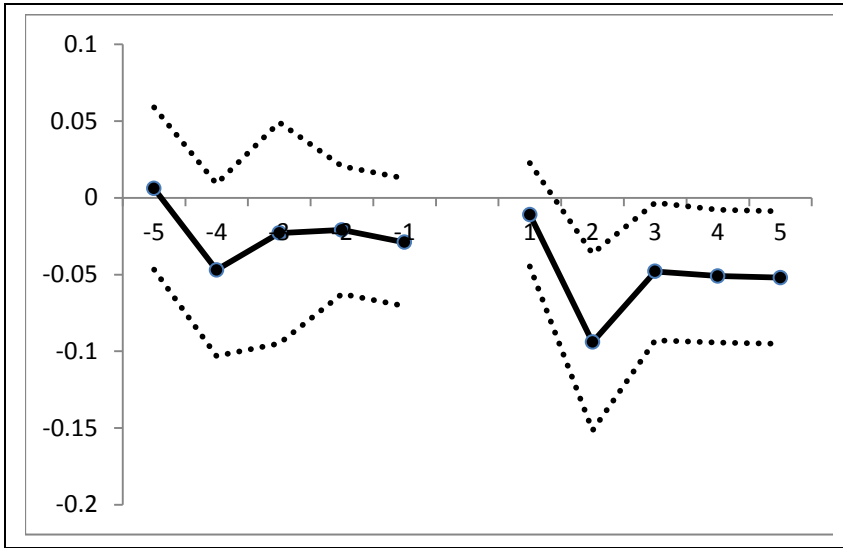


Figure 5. Years relative to intrastate deregulation and probability of not being in the labor force (females), OLS estimate and 95% CI.

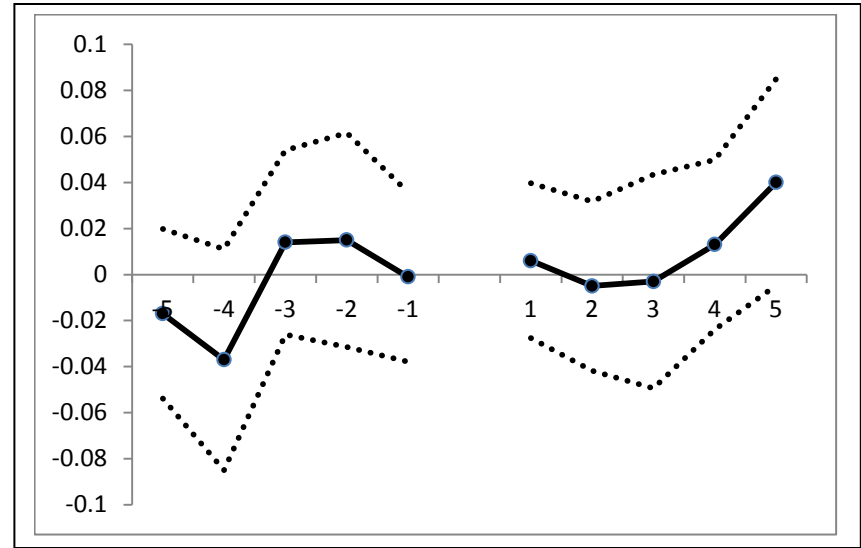


Figure 6. Years relative to intrastate deregulation and probability of not being in the labor force (males), OLS estimate and 95% CI.

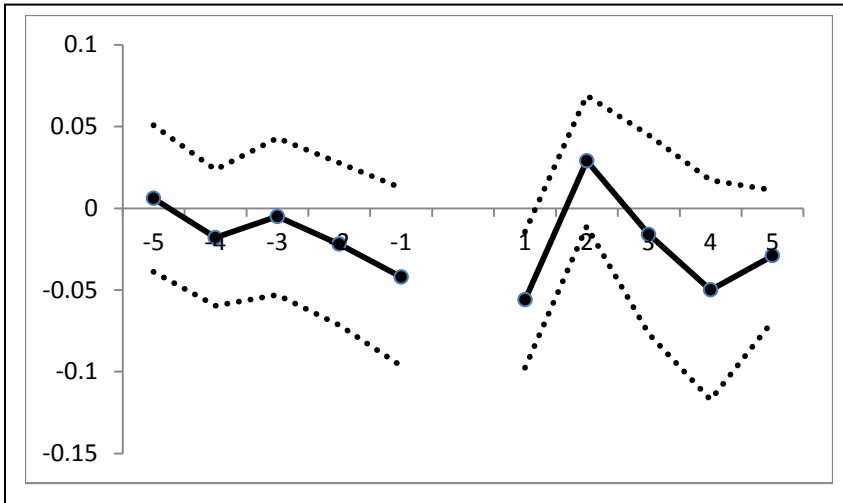


Figure 7. Years relative to interstate deregulation and probability of not being in the labor force (females), OLS estimate and 95% CI.

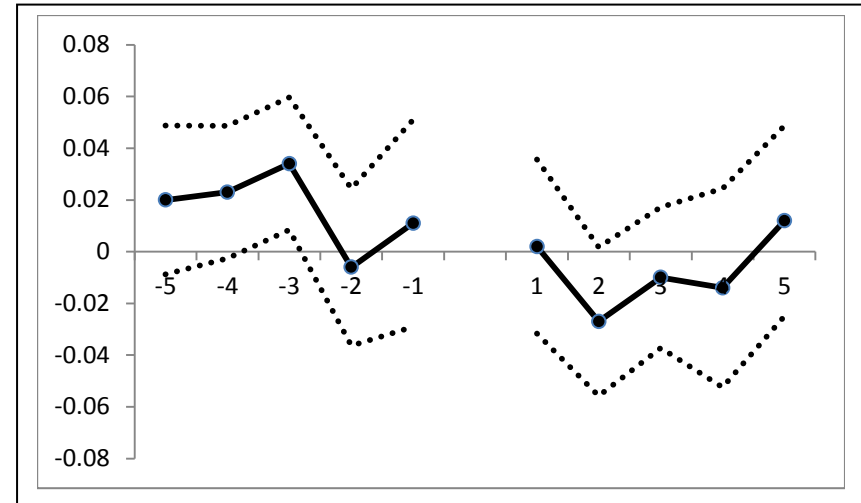


Figure 8. Years relative to interstate deregulation and probability of not being in the labor force (males), OLS estimate and 95% CI.