The Effect of Competition on Unions

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JOB MARKET PAPER

November, 2009

Removal of geographical restrictions on bank branching in the U.S. intensified competition in the non-financial sector by lowering entry barriers and by accelerating formation of new firms. I use bank deregulation as an instrumental variable to identify an exogenous increase in the competitiveness of the non-financial sector and evaluate its impact on union membership. Two-stage least squares estimates indicate that competition materially reduced union membership. The reduction in union membership is larger in states with greater changes in competition and in manufacturing sectors with relatively high dependence on external finance. Moreover, the reduction in union membership is associated with a reduction in the union wage premium and with an increase in working hours among workers who lost union representation. The results are consistent with the view that unions target less competitive industries and that shocks to competition increase the elasticity of labor-demand. Finally, this paper's results show that intensified competition explains about two-thirds of the reduction in union membership in the United States since the late 1970s.

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I. Introduction

The decline of American unions has been unrelenting since its peak in 1945 with an accelerated reduction in union membership since the late 1970s. At the same time, the U.S. economy has become increasingly competitive, with the number of new incorporations per capita increasing steadily over the last four decades. These trends are depicted in Figure 1.

What role did increasingly competitive markets play in the decline of union membership? One view holds that less competitive product markets allow unions to negotiate larger wage gains (Abowd and Lemieux (1993); Christofides and Oswald (1992); Hirsch (2008); Segal (1964); Stewart (1990)). According to Hicks-Marshallian laws of factor demand, the demand for labor is less elastic in less competitive product markets. This, in turn, enables unions to demand higher wages for their members in less competitive markets without risking large losses of employment. A countervailing view holds that unions could succeed in competitive industries by organizing workers across the entire industry. When an entire industry is unionized, firms may survive with higher union costs as long as their competitors face similar costs. Unions might also succeed comparatively well in competitive industries if monopolistic industries more effectively coordinate to fight unions and strikes (Levinson (1967)).

A central obstacle in estimating the causal effect of competition on unions is endogeneity: the presence of unions may affect the degree of competition in an industry. Such reverse relation may arise if firms are reluctant to enter markets with high union membership due to potentially lower profits (Abowd (1989); Clark (1984); Hirsch (1991); Lee and Mas (2009); Ruback and Zimmerman (1984)). This reverse causality may bias the causal effect of competition on unions and overestimate the potentially negative impact of competition on unions.

In this paper I present evidence on the causal effect of competition on union membership and coverage by using innovations in financial markets as instrumental variables for the entry of new non-financial firms. The financial innovations used in this paper are the state-by-state removal of restrictions on bank branching within state borders. These policy changes occurred in different states in different years. Furthermore, past research and evidence presented below shows that the timing of these liberalizations was exogenous to pre-existing conditions in the labor markets (Black and Strahan (2001); Kroszner and Strahan (1999); Levine, Levkov and Rubinstein (2008)), and that bank branch deregulation significantly increased the rate of new firm entry among non-financial firms (Black and Strahan (2002); Kerr and Nanda (2009)).

I report several findings from analyzing data that span the 1978–2006 period, and combine information on union membership and coverage of prime–age men from May and Outgoing Rotation Groups Current Population Surveys as well as Panel Study of Income Dynamics, on new incorporations per capita from Dun and Bradstreet, on timing of bank branch deregulation from Kroszner and Strahan (1999), on external financial dependence for manufacturing sectors from Cetorelli and Strahan (2006), on wrongful–discharge protections from Autor, Donohue and Schwab (2006), and on collective bargaining laws from Valletta and Freeman (1988).

The validity of the identification strategy rests on the assumption that bank deregulation is a legitimate instrument for entry of new firms. Consistent estimation of the causal impact of competition on union membership requires that (1) union membership does not predict the timing of bank deregulation; (2) that bank deregulation intensifies competition; and (3) that bank deregulation influences union membership *only* through its impact on competition.

I first examine whether pre-existing unionism affects the timing of bank deregulation. If labor unions supported bank regulation because rents were shared with workers, then deregulation should occur later in states where labor unions have greater influence. Using a hazard model and incorporating the political-economy factors from Kroszner and Strahan (1999), I show that neither the levels nor the rates of change in union membership or coverage explain the timing of bank deregulation. Moreover, results from Granger (1969) causality test indicate that changes in union membership did not precede bank deregulation. Both of these results help to rule out potential impact of unions on the timing of bank deregulation.

Next, I document that bank branch deregulation has a substantial, first-order effect on competition in the economy, as measured by new incorporations per capita. This effect is not only statistically significant, but also quite large relative to the effect of the state business cycle on new business incorporations.

Finally, I provide a few arguments that back up the exclusion restriction, which is the critical assumption that drives the bank deregulation IV story. First, some of the two-stage least squares (TSLS) estimates of the impact of competition on union membership are overidentified because several estimates could be constructed from subsets of the instruments. In all specifications the overidentifying restrictions are not rejected, thus providing some confidence in the validity of the instrument. More convincingly, I have estimated the impact of bank branch deregulation on union membership in a sample of manufacturing sectors with low dependence on external finance, a sample in which bank branch deregulation had no impact on competition (Cetorelli and Strahan (2006)). If bank deregulation affects union membership for reasons other than competition, I would expect deregulation to be related to union membership for this sample. On the other hand, if deregulation affects union membership only through its effect on competition, I would not expect any relationship in this sample. The estimates suggest that deregulation has no effect on union membership in manufacturing sectors with low dependence on external finance, a finding which supports the estimation framework employed throughout this paper.

The intensification of product market competition, as measured by the acceleration of new incorporations, reduced union membership. Two-stage least squares estimates of the impact of log new incorporations per capita on union membership indicate that a ten percent increase in new incorporations per capita results in a $1.2 - 1.9$ percentage point decrease in union membership. This result is robust to inclusion of personal characteristics of workers, industry fixed effects, wrongful-discharge protections and collective bargaining laws, and region-specific time fixed effects. I also find that the decline in union membership is associated with the decline in the union wage premium.

I supplement the TSLS analysis by exploring the "reduced-form" impact of bank deregulation on union membership among manufacturing sectors with different dependence on external finance. I build on the work of Cetorelli and Strahan (2006) who find that removal of restrictions on interstate banking, and not branching within states, increased the total number of establishments in manufacturing sectors with above median dependence on external finance. Consistently with the findings in Cetorelli and Strahan (2006), I find that only interstate bank deregulation reduced union membership and that the reduction in union membership occurs only in manufacturing sectors with external financial dependence above the median. These findings are consistent with the view that firm entry reduces unionism.

At a more descriptive level, I document a monotonic relationship between changes in competition and union membership by estimating these changes separately for each state. Specifically, I show that states with larger changes in entry of new incorporations per capita following bank deregulation experienced larger declines in union membership.

Still, there are several obstacles with inferring from the evidence above that firm entry reduces unionism by making states' economies more competitive. First, the average firm size fell following deregulation (Cetorelli and Strahan (2006)). If unions primarily target large firms, then a reduction in firm size may explain the fall in unionism independently from the competition channel. Second, the composition of firms and workers has changed after bank deregulation (Black and Strahan (2002); Kerr and Nanda (2009)). If workers do not organize immediately to bargain collectively with employers, then this might explain why unionism declines after bank deregulation.

To address these concerns, I use data from the Panel Study of Income Dynamics (PSID), so that I can trace individual workers throughout their employment histories. In particular, the PSID provides information of exact tenure with current employer, which enables to estimate the impact of firm entry on union coverage among "mature" employees that have been with their current employers for long periods of time. This excludes new workers in existing firms and workers in new firms that may not be union members for reasons that have nothing to do with the competitiveness of the economy.

The evidence from the PSID shows a reduction of $3-6$ percentage points in union coverage following deregulation. The reduction in union coverage is larger for workers with longer tenures with their current employer, which is inconsistent with the argument that changes in unionism after deregulation are driven by changes in composition of workers.

Based on the evidence, I conclude that intensified competitiveness of the U.S. economy has a first-order effect on declining unionism. Between 1978 and 2006 union membership among wage and salary male workers fell by about 20%. At the same time, entry of new incorporations per capita increased by about 100%. Using the most conservative estimates, a ten percent increase in new incorporations per capita reduces union membership by 1.24 percentage points. Thus a 100% increase in new incorporations per capita reduces union membership by 12.4 percentage points, or about sixty percent of the overall reduction in union membership. The economically large impact of competition on unionism is net of other potential explanations of union decline. By using industry fixed effect, I am estimating the change in union membership as a result of firm–entry within industries, thus accounting for the movement of workers from manufacturing towards the service sector. By having state fixed effects, I account for state–specific (and thus region–specific) observable and unobservable characteristics that do not vary over time. State fixed effects also account for the movement of workers to the South which has a long tradition of right–to–work laws. I also allow for the time trend in union membership to vary by regions by including region–specific time fixed effects. Inclusion of wrongful–discharge protections and collective bargaining laws accounts for differences in workers' demand for unionism. Finally, time fixed effects control for national changes in union membership and thus account for oil and energy crises that were especially harmful for the highly unionized automobile industry.

The large negative effect of competition on unionism that I estimate is consistent with the empirical findings in Ashenfelter and Johnson (1972), Hirsch and Berger (1984), and Lee (1978) who find a positive association between industry concentration and the likelihood of unionism and with the results in Slaughter (2007) who finds larger union representation in industries with lower global engagement. The results are also consistent the empirical literature that finds a larger union wage premium in less competitive industries (Abowd and Lemieux (1993); Christofides and Oswald (1992); Segal (1964); Stewart (1990)) and with the literature that finds a reduction in union bargaining power following deregulation in the airline (Cappelli (1985); Card (1986); Card (1996a); Cremieux (1996)) and the trucking (Rose (1987); Belzer (1995); Peoples (1996); Belman and Monaco (2001)) industries. This paper's results are also consistent with Farber (1987), Farber (1989), and Farber and Krueger (1993) who find that one of the main reasons for declining unionism in the United States is lower demand for union representation. Indeed, if unions can no longer provide higher wages for their members due to higher competitive pressures, then workers might not vote for further representation by the union. Finally, the results are consistent with Freeman (1988) who argues that most of the union decline can be explained by increasing management opposition to unions due to competitive pressures.

This paper's results, however, are at odds with the empirical evidence in Weiss (1966), who finds that for a given degree of union strength, a greater concentration in an industry yields a smaller rate of increase in wages. Weiss explains his findings by hypothesizing that in concentrated industries wages are already high, so that unions are not able to add much. Similarly, the results are inconsistent with (a) Nickell, Vainiomaki and Wadhwani (1994) who find very similar union wage premium in firms operating in markets with different degrees of competition as measured by market share and with (b) Kahn (1979) who doesn't find any significant relation between industry concentration and union membership. Finally, this paper's findings regarding union wage premium are consistent with the large literature that documents a substantial wage premium for union workers, but inconsistent with the careful study of DiNardo and Lee (2004), who find that workers in establishments that barely rejected union representation have very similar outcomes to workers in establishments that barely approved representation.[1]

This paper relates to a large literature that shows a first–order relationship between financial development and economic growth (Demirgüç-Kunt and Maksimovic (1998); Jayaratne and Strahan (1996); King and Levine (1993a); King and Levine (1993b); Levine and Renelt (1992); Levine and Zervos (1998); Rajan and Zingales (1998) are notable recent contributions to this literature). If unions impose inefficiency in production, then this paper's results show a specific channel through

[1]See Lewis (1986) and Jarrell and Stanley (1990) for a review of the union wage premium literature.

which financial innovations increase productivity and growth in the economy.[2] This paper thus relates to an emerging literature that examines the channels underlying the finance–growth nexus and advertises the role of labor markets in driving this relationship.

The rest of the paper is organized as follows. The next section discusses the history of bank deregulation in the United States. Section III presents the statistical model and identification strategy. Section IV describes the data. Section V presents the main findings and Section VI concludes.

II. History of Bank Deregulation in the United States

Geographic restrictions on banks have their origins in the U.S. Constitution, which limited states from taxing interstate commerce and issuing fiat money. In turn, states raised revenues by chartering banks and taxing their profits. Since states received no charter fees from banks incorporated in other states, state legislatures prohibited the entry of out-of-state banks through interstate bank regulations. To maximize revenues from selling charters, states also effectively granted local monopolies to banks by restricting banks from branching within state borders. These intrastate branching restrictions frequently limited banks to operating in one city (Flannery (1984)).

By protecting inefficient banks from competition, geographic restrictions created a powerful constituency for maintaining these regulations even after the original fiscal motivations receded. Indeed, banks protected by these regulations successfully lobbied both the federal government and state governments to prohibit interstate banking (White (1982); Economides, Hubbard and Palia (1996)).

In the last quarter of the 20*th* century, however, technological, legal, and financial innovations diminished the economic and political power of banks benefiting from geographic restrictions. In particular, a series of innovations lowered the costs of using distant banks. This reduced the monopoly power of local banks and weakened their ability and desire to lobby for geographic restrictions. For example, the invention of automatic teller machines (ATMs), in conjunction with court rulings that ATMs are not bank branches, weakened the geographical link between banks

[2] The impact of unions on productivity is not clear–cut. In a landmark study, Freeman and Medoff (1984) conclude that "... most studies ... find that unionized establishments are more productive than otherwise comparable nonunion establishments" (p. 169). Their conclusion, however, is challenged by Hirsch (2007) who concludes that "The most important point to bring away from the productivity evidence may be the *absence* of a large positive effect due to unions" (p. 211, italics in the origin). A case study by Krueger and Mas (2004) further shows that unions are associated with lower product quality.

and their clientele. Furthermore, the creation of checkable money market mutual funds made banking by mail and telephone easier, thus further weakening the power of local bank monopolies. Finally, the increasing sophistication of credit scoring techniques, improvements in information processing, and the revolution in telecommunications reduced the informational advantages of local bankers, especially with regards to small firms.

These national developments interacted with preexisting state characteristics to shape the timing of bank deregulation across the states. As shown by Kroszner and Strahan (1999), branch deregulation occurred later in states where potential losers from deregulation (small, monopolistic banks) were financially stronger and had a lot of political power. On the other hand, deregulation occurred earlier in states where potential winners of deregulation (small firms) were relatively numerous. Most states deregulated geographic restrictions on banking between the mid-1970s and 1994, when the Riegle-Neal Act effectively eliminated these restrictions.

Figures 2A and 2B illustrate the substantial changes in the geographical location of bank branches. The figures indicate the location of bank branches in Texas in the period before (figure 2A) and after bank branch deregulation (figure 2B). Although the figures depict changes in location of branches in a single state, similar results are found for virtually all states. The circles in the figures represent the total number of bank branches within a zip code. The size of each circle is proportional to the number of bank branches. As shown, bank branch deregulation increased the spread of branches within state borders and increased their concentration in already established locations, thus intensifying competition among bank branches.

The forces driving bank deregulation were exogenous to competition in the non-financial sector and pre-existing presence of unions. In particular, the timing of deregulation was neither shaped by new firm formation (Black and Strahan (2002); Kerr and Nanda (2009)), nor by the degree of income inequality (Beck, Levine and Levkov (2009)), or black-white wage differential (Levine et al. (2008)) in each state. Moreover, as shown in Table 1 neither the preexisting levels nor rates of change in union membership or coverage explain the timing of bank deregulation.

Table 1 shows estimates from a hazard model where the dependent variable is log expected time to bank branch deregulation. Each coefficient measures the percentage change in the hazard of bank branch deregulation as a result of a marginal change in: (a) proportion of wage and salary workers who are union members, (b) *changes* in proportion of wage and salary workers who are union members, (c) proportion of wage and salary workers who are covered by a collective bargaining agreement, but not necessarily union members, and (d) *changes* in proportion of wage and salary workers who are covered by a collective bargaining agreement, but not necessarily union members. Standard errors are adjusted for state level clustering and appear in parentheses. Columns (1)–(4) use wage and salary workers in all industries, while columns (5)–(8) use only workers in manufacturing. All specifications control for political economy factors that affect the timing of bank branch deregulation (Kroszner and Strahan (1999)). These factors are listed in the notes to Table 1. In all specifications the coefficients of interest are statistically insignificant, indicating that neither the preexisting levels nor rates of change in union membership or coverage explain the timing of bank branch deregulation.[3]

Interstate bank deregulation can not be examined in the context of a hazard model due to dependence of timing of deregulation between states. Nevertheless, Figure 3 provides some evidence that the timing of interstate deregulation was not affected by preexisting presence of unions.

The upper panel of Figure 3 plots the level of union membership and the rate of change in union membership before interstate deregulation and the year of deregulation for each state, where union membership is the proportion of wage and salary workers who are members of a union. The lower panel uses union coverage instead of membership, where union coverage is the proportion of wage and salary workers who are covered by a collective bargaining agreement, but not necessarily union members. The dashed lines are the regression lines that depict the degree of correlation between the timing of interstate deregulation and preexisting presence of unions. The absolute values of t-statistics for the correlations in the plots are: 0.53 in plot (a), 0.46 in plot (b), 0.35 in plot (c), and 0.47 in plot (d). The results in Figure 3 indicate that neither the preexisting levels nor rates of change in union membership or coverage explain the timing of bank deregulation.

An extensive literature examines the impact of both interstate and branch bank deregulation. Along many dimensions, branch deregulation exerted a dominant effect on banking system performance. For example, Jayaratne and Strahan (1998) find that removing branching restrictions improved banking efficiency by reducing interest rates on loans, raising them on deposits, lowering overhead costs, and shrinking loan losses. While interstate and branch deregulation both improved bank efficiency, branch deregulation exerted a more robust impact on efficiency when simultaneously controlling for interstate deregulation. Within the banking industry, Ashenfelter and Hannan (1986) find a positive impact of branch deregulation and the share of female employees across several banking markets in Pennsylvania and New Jersey. At a more aggregate level, branch deregulation also accelerated the growth rate of per capita GDP and personal income (Jayaratne and Strahan (1996),

[3]Estimation of the hazard model is explained in details in the Appendix.

Huang (2008)), lowered economic volatility (Morgan, Rime and Strahan (2004), Demyanyk, Ostergaard and Sorensen (2007)), reduced the gender gap in earnings and employment among bank employees (Black and Strahan (2001)), and reduced income inequality in the economy (Beck et al. (2009)).

More specifically for the purposes of this paper, interstate and branch bank deregulation intensified competition among firms in the *non-financial* sector by reducing barriers to entry. Black and Strahan (2002) find that deregulation helped entrepreneurs start new businesses, with the rate of new incorporations per capita in a state increasing by six percentage points following deregulation. Kerr and Nanda (2009) find that deregulation increased the number of new start-ups by six percentage points and expanded the number of facilities of existing firms by four percentage points across all sectors in the economy. Furthermore, they find a dramatic increase in both the entry and exit of firms, suggesting that deregulation increased contestability throughout the economy.

Figure 4 shows the dynamic impact of branch deregulation on log new incorporations per capita. Specifically, the figure plots estimates of $\theta_1 - \theta_{25}$ and the corresponding 95% confidence intervals from the following specification:

$$\ln{(entry)}_{st} = \alpha_s + \lambda_t + \theta_1 D_{st}^{-10} + \theta_2 D_{st}^{-9} + ... + \theta_{25} D_{st}^{+15} + \epsilon_{st}$$

where $entry_{st}$ is the number of new incorporations per capita in state s at time t. D_{st}^{-j} equals one for states in the j^{th} year *before* branch deregulation and equals zero otherwise. D_{st}^{+k} equals one for states in the k^{th} year *after* branch deregulation and equals zero otherwise. α_s and λ_t are state and year fixed effects, respectively. I exclude the year of deregulation, thus estimating the dynamic effect of branch deregulation on log new incorporations per capita relative to the year of deregulation. Vertical lines in the plot mark 95% confidence intervals, adjusted for state level clustering. The results in Figure 4 indicate that trends in log new incorporations per capita did not precede deregulation, which helps to rule out reverse causality. As shown, the impact of deregulation is insignificantly different from zero for all years before deregulation. After deregulation, on the other hand, new incorporations per capita increase significantly. The impact of deregulation becomes significantly different from zero in the second year after deregulation, grows for the next four years, then becomes steady for the next six years, and finally increases a little more to reach its highest level in the fourteenth year after deregulation.

III. Statistical Model and Identification Strategy

This section describes the procedure for estimating the impact of competition on union membership. It then outlines the main problems with such estimation when using non-experimental data and offers possible solutions to overcome these obstacles. The structural model of interest is:

$$union_{st} = \beta_0 + \beta_1 comp_{st} + \varepsilon_{st} \tag{1}$$

where $union_{st}$ is the proportion of union members in state s and time t, $comp_{st}$ is a measure of competition among firms in state s and time t, and ε_{st} is the error term. For simplicity of exposition I assume that all the relevant state characteristics as well as state and year fixed effects have been accounted for in previous steps. The coefficient of interest, β_1, measures the percentage change in union membership as a result of a marginal change in competition among firms. A negative and significant estimate of β_1 implies that firm competition reduces union membership.

There are several obstacles to estimating the structural coefficient of interest in (1). First, competition among firms is not directly observed. Instead, it is usually proxied by concentration ratios, barriers to entry, or various measures of market power. In this paper I will use entry of new incorporations per capita ($entry_{st}$) as a proxy for product market competition in each state and year. Thus, in practice equation (1) becomes:

$$\begin{aligned} union_{st} &= \beta_0 + \beta_1 entry_{st} + \left[\beta_1 \left(comp_{st} - entry_{st}\right) + \varepsilon_{st}\right] \qquad (2) \\ &= \beta_0 + \beta_1 entry_{st} + u_{st} \end{aligned}$$

where $u_{st} = \beta_1 \left(comp_{st} - entry_{st}\right) + \varepsilon_{st}$. The error term u_{st} contains the difference between $comp_{st}$ and $entry_{st}$. If this difference is correlated with $entry_{st}$ then the resulting Ordinary Least Squares (OLS) estimate of $\beta_1$will be biased towards zero due to attenuation bias.

Another obstacle to estimate the causal impact of competition on union is potential reverse causality. Firms may be reluctant to enter markets with high union membership due to potentially lower profits (Abowd (1989); Clark (1984); Hirsch (1991); Lee and Mas (2009); Ruback and Zimmerman (1984)). This relation can be summarized by the following equation:

$$entry_{st} = \gamma_0 + \gamma_1 union_{st} + v_{st} \tag{3}$$

The reverse relationship in (3) poses a problem for the OLS estimate of β_1 in (2) because it results in potentially non-zero covariance between *entry* and the error term u:

$$Cov\,(entry, u) = \frac{\gamma_1}{1-\gamma_1\beta_1} Var\,(u)$$

If $\gamma_1 < 0$ and $\beta_1 < 0$ such that $\gamma_1/\left(1-\gamma_1\beta_1\right) < 0$, the correlation between *entry* and u is negative, suggesting that in the presence of reverse causation the OLS estimate of β_1 in equation (2) overestimates the negative impact of competition on union membership.[4]

The biases associated with a "naive" OLS estimation of equation (1) may be solved by using an instrumental variable which is strongly correlated with entry of new incorporations and affects union membership only through its impact on new incorporations. Specifically, let D_{st} be an indicator which equals one in the years after bank branch deregulation and zero otherwise. Equations (6a) and (6b) provide the basis for an instrumental variable estimate of the effect of entry of firms on union membership:

$$union_{st} = \pi_{11} + \pi_{12} D_{st} + \varepsilon_{st} \tag{6a}$$
$$entry_{st} = \pi_{21} + \pi_{22} D_{st} + \mu_{st} \tag{6b}$$

The parameter π_{12} captures the "reduced-form" effect of bank deregulation on union membership. The parameter π_{22} captures the "first-stage" effect of bank deregulation on entry of new incorporations. The Instrumental Variable (IV) estimator of entry of firms on union membership is the ratio between the two parameters, namely π_{12}/π_{22}.

The validity of the identification strategy rests on the assumption that bank deregulation is a legitimate instrument for entry of new incorporations. Consistent estimation of the causal impact of competition on union membership requires,

[4] The covariance between $entry_{st}$ and u_{st} (for simplicity I will omit the indexing st):

$$\begin{aligned} Cov\,(entry, u) &= Cov\,(\gamma_0 + \gamma_1 union + v, u) \\ &= \gamma_1 Cov\,(union, u) + Cov\,(v, u) \end{aligned} \tag{4}$$

Assuming $Cov\,(v, u) = 0$, equation (4) becomes:

$$\begin{aligned} Cov\,(entry, u) &= \gamma_1 Cov\,(union, u) \\ &= \gamma_1 Cov\,(\beta_0 + \beta_1 entry + u, u) \\ &= \gamma_1\beta_1 Cov\,(entry, u) + \gamma_1 Var\,(u) \end{aligned} \tag{5}$$

Solving (5) for covariance between *entry* and u yields:

$$Cov\,(entry, u) = \frac{\gamma_1}{1-\gamma_1\beta_1} Var\,(u)$$

among other things, that union membership does not predict the timing of bank deregulation. If labor unions supported bank regulation because rents were shared with workers, then deregulation should occur later in states where labor unions have greater influence.[5] The results from the duration model in Table 1, however, indicate that neither the preexisting levels nor rates of change in union membership or coverage explain the timing of bank branch deregulation. Moreover, Figure 5 shows that changes in union membership did not precede deregulation. Union membership falls after bank deregulation. Both of these results help to rule out potential impact of union membership on the timing of bank deregulation.

For being a legitimate instrument, bank deregulation must be correlated with entry of new firms. Previous work has shown that by reducing barriers to entry, bank deregulation increased the rate of new incorporations per capita and start-up creation (Black and Strahan (2002), Kerr and Nanda (2009)). Figure 4 is a graphical depiction of this relation, which is the first stage of the IV estimation. The figure clearly shows a significant increase in the number of new incorporations per capita following bank branch deregulation. This effect is statistically significant at the 5 percent and is quite large relative to the effect of the state business cycle on new business incorporations. For example, an increase in personal income growth of one percent would generate an increase in incorporations of a little more than one percent initially, and this increase would eventually peter out after about six years.

The critical assumption that drives the bank deregulation IV story is that bank deregulation influences union membership *only* through its impact on competition. If bank deregulation influences union membership for other reasons, my approach is called into question. It is therefore useful to consider other potential impacts of bank deregulation. First, as documented by Cetorelli and Strahan (2006), the average firm size falls following deregulation. If unions primarily target large firms, then a reduction in firm size may explain the fall in union membership independently from the competition channel. Second, the number of new firms in the economy has risen after bank deregulation (Black and Strahan (2002); Kerr and Nanda (2009)) changing the composition of workers and firms. If new workers do not organize immediately to bargain collectively with employers, then this might explain why union membership falls after bank deregulation, again, independently from the competitive channel.

There are few arguments that back up the exclusion restriction assumption. First, Figure 5 indicates that union membership falls for many years following bank deregulation and that bank deregulation has a trend effect on union membership.

[5] The potential reverse causation in (6a) will create a correlation between D_{st} and ε_{st} and therefore bias π_{12}.

If changes in union membership are simply driven by addition of new workers who do not organize immediately to bargain collectively with employers, then we should see a level effect of deregulation on union membership, not a trend effect.

Second, some of the TSLS estimates in tables 3 and 4 are overidentified because several estimates of the impact of log new incorporations per capita could be constructed from subsets of the instruments. In all specifications the overidentifying restrictions are not rejected, thus further providing some confidence in the validity of the instruments.

Finally, and perhaps most convincingly, I have estimated the impact of bank branch deregulation on union membership in a sample of manufacturing sectors with low dependence on external finance, a sample in which bank branch deregulation had no impact on entry of new establishments (Cetorelli and Strahan (2006)). If bank deregulation affects union membership for reasons other than competition, I would expect deregulation to be related to union membership for this sample. On the other hand, if deregulation affects union membership only through its effect on competition, I would not expect any relationship in this sample. The estimates suggest that deregulation has no effect on union membership in manufacturing sectors with low dependence on external finance, a finding which supports the estimation framework employed throughout this paper.

The regression of interest throughout the IV analysis is:

$$union_{st} = \alpha_s + \lambda_t + \rho entry_{st} + \mathbf{X}'_{st}\boldsymbol{\beta} + \varepsilon_{st} \tag{7}$$

where $union_{st}$ is the proportion of union members in state s and time t, α_s and λ_t are state and year fixed effects, respectively, $entry_{st}$ denotes the predicted value of the natural logarithm of new incorporations per capita, $\mathbf{X}_{st}$ is a vector of time–varying observable characteristics of states, and ε_{st} is the error term. Union membership is the average state–year value of residuals from an OLS regression of union membership indicator on a series of dummy variables that indicate years of completed education (0–8, 9–11, 12, 13–15, and 16+), potential experience (age − years of completed education − 6) and its square, and industry fixed effect. The natural logarithm of new corporations per capita is predicted in the first stage by bank branch deregulation. I consider several ways to use bank deregulation as an instrument. First, I use a dummy variable which equals one in all year after deregulation and zero otherwise. I also use years since deregulation as well as a quadratic function of years since deregulation. Finally, I use a non–parametric function of years relative to deregulation to instrument log new incorporations per capita, by including a series of dummy variables for each year before and after bank branch deregulation. When estimating equation (7) I allow for possible serial correlation of errors over time by clustering the standard errors of the estimated coefficients at the state level.

IV. Data

To consistently estimate the effect of competition among firms on union membership I collect data on union membership, new incorporations per capita, timing of bank deregulation, as well as labor bargaining laws and wrongful-discharge protections for all states in the period 1978–2006. I supplement these data with estimates of external financial dependence for manufacturing sectors from Cetorelli and Strahan (2006). In accord with previous literature on bank deregulation, I drop Delaware and South Dakota because of large concentration of credit card banks in these states.

A. *Union Membership and Coverage*

Union membership and coverage are obtained from the May Supplement to the Current Population Survey (CPS) for the years 1978–1981 and from CPS Outgoing Rotation Groups (ORG) for the years 1983–2006. The 1982 Current Population Survey did not include any union status questions and thus is excluded from the analyses. The sample period is 1978–2006 because of limitation of some state characteristics discussed below.

I restrict the sample of CPS respondent to prime-age (25–54) white men who work for wage and salary, excluding those who work in agriculture and have non-missing union status. I further restrict the sample to those who work or with a job but currently not working. Respondents are counted as union members if they respond "yes" to the following question, asked to employed wage and salary workers: "On this job, is ___ a member of a labor union or of an employee association similar to a union?" Respondents who answer "no" to this question are then asked: "On this job, is ___ covered by a union or employee association contract?" Respondents are counted as covered if they are union members or if they are not members but say they are covered by a union contract.

I supplement the CPS data with union coverage information from the Panel Study of Income Dynamics (PSID) for the years 1977-1993. I use union coverage and not union membership because it is asked more frequently and available for all years. I do not use post 1993 data because information on the state of residence is not consistently available after 1993. Similarly to the CPS, I restrict the PSID sample to prime-age (25-54) white male heads of households who are not self-employed and belong to the "core" sample of 1968 families originally interviewed by the PSID and have non-missing union status. Respondents are counted as covered by a union contract if they respond "yes" to the following question: "Is your current job covered by a union contract?".

Data appendix provides more details on the sample restrictions imposed on the CPS and the PSID. Appendix Table 2 lists all the variables used for the construction of the PSID sample, while Appendix Table 3 provides further details about the construction of the CPS and the PSID microdata samples.

B. *Personal Characteristics of Workers*

The CPS files provide information on years of completed education as well as age at the time of the survey. Questions regarding years of completed education were changed starting from the 1992 CPS (see, for example, Polivka (1996)). After the redesign years of completed education are no longer available in a continuous form, but only in categories. I use a time consistent measure of years of completed education by constructing five categories for years of completed education: 0-8, 9-11, 12, 13-15, and 16+. To calculate potential experience in data years coded with the revised education question, I use figures from Park (1994) to assign years of completed education to each worker based upon highest degree held. Years of potential experience are then calculated as age minus assigned years of education minus 6, rounded down to the nearest integer value.

Years of completed education are also available in the PSID. Similarly to the analyses of CPS files, I construct five categories for years of completed education: 0-8, 9-11, 12, 13-15, and 16+. One of the advantages of PSID over CPS is availability of exact tenure with the current employer. This information will be crucial when examining whether or not changes in union coverage are driven by "mature" or "new" workers.

C. *New Incorporations Per Capita*

Information on new incorporation is obtained from Dun and Bradstreet. This series comes from the individual states and is available from 1964 until 2006. However, only post 1978 data are used due to limitations of wrongful discharge protections (discussed below) prior to 1978. New incorporations are adjusted to per capita terms by dividing new incorporations by population estimates, obtained from the U.S. Census Bureau. Following Black and Strahan (2002), I use the natural logarithm of new incorporations per capita as a proxy for competition among firms.

D. *Timing of Bank Deregulation*

Timing of bank deregulation is obtained from Kroszner and Strahan (1999). These data indicate the year in which different states permitted: (1) branching via mergers and acquisitions (M&As) through the holding company structure, which was the first step in the deregulation process, followed by de novo branching, and (2) out–of–state banking companies to buy banks headquartered in a state. Appendix Table 1 lists the years of bank deregulation for each state.

E. *External Financial Dependence*

Estimates of external financial dependence for manufacturing sectors are from Cetorelli and Strahan (2006), Table II. As in Cetorelli and Strahan (2006), I use two measures of financial dependence of manufacturing sectors. The first measure is the proportion of capital expenditures financed with external funds. A negative value indicates that firms in the indicated sector have free cash flow, whereas a positive value indicates that firms must issue debt or equity to finance their investment. The figures represent the median value for COMPUSTAT firms (that have been on COMPUSTAT for at least 10 years) in each sector over the 1980 to 1997 period. A second measure is the median ratio of loans to assets for small firms from the Federal Reserve 1998 Survey of Small Business Finance. Larger values of the loans/assets ratio indicate greater dependence on external finance.

F. *Wrongful-Discharge Protections and Collective Bargaining Laws*

Wrongful-discharge protections for each state are from Autor et al. (2006). These protections were created by U.S. state courts in the 1970s and the 1980s and limited the ability of employers to fire workers. Presence of these protections in a state might reduce workers' demand for unionism. In robustness checks, therefore, I include three dummy variables that indicate presence of "public policy", "good-faith", and "implied contract" protections. The "public policy" protection provides employees with protections against discharges that would prevent an important public policy, such as performing jury duty; "good-faith" prevents employers from firing workers for bad cause such as just before a substantial commission is due; and "implied contract" protection comes into force when an employer implicitly promises not to terminate a worker without good cause. Data on wrongful-discharge protections are available for the period 1978-1998 and exclude the District of Columbia. Between 1999 and 2006, I impute the presence of wrongful-discharge protections for each state by assigning the 1998 values.

The wrongful-discharge data are supplemented with data on labor bargaining laws from Valletta and Freeman (1988). These data describe the status of public sector collective bargaining policies for each state. Specifically, these data include indicators for whether or not states permit collective bargaining and striking. An updated version of these data is obtained from the National Bureau of Economic Analysis data collection section and includes public sector collective bargaining policies by state from 1955 until 1996. Between 1997 and 2006, I impute collective bargaining policies by assigning the 1996 values.

V. Results

A. *The Impact of Firm Entry on Union Membership*

In Table 2 I use bank branch deregulation to estimate the impact of log new incorporations per capita on union membership using Wald (1940) estimator. This estimator computes the impact of log new incorporations per capita on union membership as the ratio of the change in union membership to the change in log new incorporations as a result of bank deregulation. The change in union membership as a result of bank branch deregulation is $-.016$, while the corresponding change in log new incorporations per capita is .082. The ratio of these two changes, $-.194$, is a Wald estimate of the impact of log new incorporations per capita on union membership. The Wald estimator is likely to provide a consistent estimate in this case if bank deregulation is uncorrelated with determinants of union membership other than log new incorporations per capita.

The last row in Table 2 provides the OLS estimate of the impact of log new incorporations per capita on union membership. The OLS estimate is the coefficient on log new incorporations per capita from a regression of union membership on log new incorporations per capita and state and year fixed effects. The Wald estimate ($-.194$) is much more negative than the OLS estimate ($-.007$), suggesting that error in measuring competition in a state biases the OLS estimate towards zero much more than the potential reverse causation biases the OLS estimate away from zero. The Wald estimate suggests that a 10 percent increase in new incorporations per capita results in 1.94 percent reduction in union membership, which is about one–fifth of the standard deviation of union membership in the sample.

In Table 3 I use alternative specifications of bank deregulation to estimate the impact of log new incorporations per capita on union membership. Columns (1) and (2) replicate the OLS and Wald estimates from Table 2. In column (3) I use years since bank deregulation as an instrument for log new incorporations per capita, while in column (4) I use years since bank deregulation and its square. Finally, in column (5) I use a nonparametric function of years relative to bank deregulation by including a series of dummy variables for each year before and after deregulation. In all specifications I control for state and year fixed effects. The specifications in panel B additionally control for collective bargaining laws and labor protection laws.

In the linear specification of instruments, the instrumented impact of log new incorporations per capita in statistically insignificant from zero. The linear specification also has relatively low first stage F–statistic of 2.28 in panel A and 1.37 in panel B. Both the quadratic and the non-parametric specifications of instruments, on the other hand, yield a statistically significant impact of log new incorporations per capita on union membership. These estimates range from $-.124$ in column (5) of panel B to $-.164$ in (4) of panel A.

The TSLS estimates in columns (4) and (5) of Table 3 are overidentified because several estimates of the impact of log new incorporations per capita could be constructed from subsets of the instruments. The p-values of overidentifying restrictions tests presented at the bottom of each panel test the hypothesis that the different combinations of instruments yield the same estimated impact of log new incorporations per capita on union membership, i.e., that all instruments are exogenous. The p-values are calculated from a J statistic, which under the null hypothesis is distributed χ^2_{m-1}, where m is the number of instruments. In all specifications the overidentifying restrictions are not rejected, providing some confidence in the validity of the instruments.

Table 4 is similar to Table 3 except that all specifications in panels A and B also control for region–specific time fixed effects. This flexible specification allows for a different time trend in the different regions (North East, Mid West, South, and West). Most of the estimates lose their statistical significance due to larger standard errors. The estimates in the last column, however, remain statistically significant at the 5% (panel A) and at the 10% (panel B) levels.

To clarify the timing of the impact of deregulation on union membership, I examine the dynamics of the relationship between branch deregulation and union membership. In Figure 5 I trace the "reduced–form" impact of deregulation on union membership for every year before and after bank branch deregulation. I exclude the year of deregulation, thus estimating the dynamic impact of deregulation on union membership relative to the year of deregulation. Specifically, I plot estimates of $\beta_1 - \beta_{25}$ from the following regression:

$$union_{st} = \alpha_s + \lambda_t + \beta_1 D_{st}^{-10} + \beta_2 D_{st}^{-9} + ... + \beta_{25} D_{st}^{+15} + e_{st} \tag{8}$$

where $union_{st}$ is the proportion of union members in state s at time t. D_{st}^{-j} equals one for states in the j^{th} year *before* branch deregulation and equals zero otherwise. D_{st}^{+k} equals one for states in the k^{th} year *after* branch deregulation and equals zero otherwise. α_s and λ_t are state and year fixed effects, respectively. Vertical lines in the plot mark 95% confidence intervals, which are adjusted for state level clustering.

Equation (8) is a Granger (1969) causality test. The test is a check on whether, conditional on state and year fixed effects, past deregulation predicts union membership, while future deregulation does not. As shown, there are no upward or downward trends in union membership before deregulation which helps to rule out reverse causality. Rather, union membership falls significantly following bank deregulation. The pattern of coefficients depicted in Figure 5 provides evidence that bank branch deregulation led to significant union decline rather than vice versa.

To provide further evidence about the impact of firm entry on union membership following bank branch deregulation, I calculate percentage change in new incorporations per capita and union membership for each state as a result of bank deregulation. Specifically, for each state I contrast the average value of union membership in all years before bank deregulation from the average after deregulation and divide it by the average value of union membership in all years prior to deregulation. I adjust the changes for the fact that the post deregulation period is different for the different state by dividing percentage changes in union membership by years since deregulation. The interpretation of the resulting figures is the percentage change in union membership *per year* of deregulation. I follow a similar procedure for new incorporations per capita. I then sort states according to percentage change in new incorporations per capita. The results are shown in Figure 6.

The states of IA, AR, MS, KY, MN, WI, and MO have the largest percentage gain in new incorporations per capita following bank deregulation. These states also have the largest percentage decline in union membership. The states of NH, CT, OH, VA, OR, HI, and WA, on the other hand, have the smallest percentage gain in new incorporations per capita and the smallest decline in union membership. The results in Figure 6 depict the monotonicity of the reduction in union membership with respect to percentage changes in new incorporations per capita.

B. *The Impact of Deregulation on Union Wage Premium*

If unions successfully bargain a wage premium for their members and if bank deregulation reduces union membership by decertifying unions, then bank deregulation should lead to a reduction in union wage premium. Figure 7 provides a reduced form effect of bank deregulation on union wage premium by comparing the changes in union wage premium across the full distribution of wages of union and non-union workers. The figure plots the location of union members in the non–union log real conditional wage distribution, before and after bank branch deregulation.[6]

The plots was constructed using the following procedure: First, I regress log real hourly earnings of non–union members on five indicators of years of completed education (0–8, 9–11, 12, 13–15, and 16+), potential experience (age − years of completed education − 6) and its square, industry fixed effects, and state fixed effects. I run the regression separately for every year using the following specification:

$$\ln\left(wage_{ist}^{non-union}\right) = \mathbf{X}_{ist}'\boldsymbol{\beta}_t + u_{ist} \tag{9}$$

This forces the resulting residuals of non–union members to sum up to zero in every year. Next, I calculate residuals for union members, r_{ist}, based on their own personal characteristics ($\mathbf{X}$) and the estimated return to these characteristics from equation (9):

$$r_{ist} = wage_{ist}^{union} - \mathbf{X}_{ist}'\widehat{\boldsymbol{\beta}}_t \tag{10}$$

This procedure creates log real hourly earnings of union members relative to non–union members (they are the benchmark because their residuals are zero for every year by construction) who have the same observable characteristics and the same time–varying return to these characteristics. There are two main advantages of the two–step procedure described in equations (9) and (10). First, given the changes in the structure of wages since the mid 1970s (see Katz and Autor (1999) for a review), the procedure allows for the return to observable characteristics to vary over time. Second, the procedure not only allows to compare wages of union and non-union workers with the same observable characteristics but also forces the time–varying return to these characteristics to be the same for union and non–union workers.

Next, I keep 100 union workers, each corresponding to a different percentile ($1-100$) of union workers' log real conditional hourly earnings distribution and I calculate their position in non–union workers' relative log real hourly earnings distribution. I repeat this procedure before (solid line) and after (dashed line) bank branch deregulation. The results in Figure 7 demonstrate a significant reduction in union wage premium after branch deregulation across the entire distribution of wages. The median union worker, for example, corresponds to $78th$ percentile in the non-union workers' wage distribution before deregulation. After deregulation,

[6] Hourly wages are converted to constant 1982 dollars using the Consumer Price Index. I restrict the sample to prime age (25-54) white male wage and salary workers, whose real wages are above one–half of the minimum wage in 1982 dollars and who work at least 40 hours per week. I further drop workers with real wages above the 99th percentile of year–specific distribution of real wages.

however, the median union worker falls to $63rd$ percentile.

The reduction in union wage premium following bank deregulation, seems to be consistent with the large literature that documents a substantial $10\% - 15\%$ union wage premium but is inconsistent with the careful study of DiNardo and Lee (2004) who do not find any evidence for union wage premium in establishments that barely rejected union representation versus establishments that barely accepted it.[7]

C. *Estimates by External Financial Dependence*

To provide more evidence on the potential link between firm entry and union membership, I use data from Cetorelli and Strahan (2006) on external financial dependence for manufacturing sectors. Estimates of external financial dependence for each manufacturing sector are provided in Table 5. As in Cetorelli and Strahan (2006), I use two measures of financial dependence of manufacturing sectors. The first measure is the proportion of capital expenditures financed with external funds. A negative value indicates that firms in the indicated sector have free cash flow, whereas a positive value indicates that firms must issue debt or equity to finance their investment. A second measure is the median ratio of loans to assets for small firms from the Federal Reserve 1998 Survey of Small Business Finance. Larger values of the loans/assets ratio indicate greater dependence on external finance. As depicted in Figure 8, the two measures of dependence on external finance are positively correlated, with a correlation coefficient of .51.

Cetorelli and Strahan (2006) analyze the impact of interstate and branch bank deregulation on the number of establishments for sectors with different degrees of dependence on external finance. They find an increase in the total number of establishments in sectors with more financial dependence. Importantly, their results hold only for interstate bank deregulation and not bank branch deregulation.

Building on the work of Cetorelli and Strahan (2006), I estimate the impact of bank deregulation on union membership in manufacturing sectors, separately by the median external financial dependence.[8] If unions decline because of firm entry, then one would expect union membership to be negatively associated with interstate bank deregulation. Branch deregulation, on the other hand, should not have a significant impact on union membership within the manufacturing sector. In this respect branch deregulation serves as a "placebo treatment".

Before analyzing the potential impact of both types of bank deregulation on

[7]See Lewis (1986) and Jarrell and Stanley (1990) for a review of the union wage premium literature.

[8]The median proportion of capital expenditures financed with external funds is 0. The median loans/assets ratio is .3.

union decline in sectors with different degrees of dependence on finance, it is useful to check wether or not the workers in these sectors are similar in their characteristics. Specifically, my strategy would be called into question if, for example, sectors with relatively low dependence on external finance have no union members.

Table 6 reports mean values of workers' characteristics, separately by the median dependence on external finance. In columns (1) and (2) I divide workers by the median proportion of capital expenditures financed with external funds, while in columns (4) and (5) workers are divided by the median loans–to–assets ratio. Columns (3) and (6) report the difference between columns (2) and (1) and columns (5) and (4), respectively, and the accompanying standard errors. The results show that workers in sectors with above median proportion of capital expenditures financed by external funds are more educated than their counterparts below the median. There are no other significant differences with respect to union membership or coverage, potential experience in the workforce, weekly working hours, or full–time participation. Nor are there any differences between workers in sectors below or above the median loans–to–assets ratio (column 6).

The results in Table 7 show a significant decline in union membership following bank deregulation in sectors with above median values of loans/assets ratio (panel A, column 4). Consistent with the findings of Cetorelli and Strahan (2006), union membership declines only following interstate deregulation and not branch deregulation. A point estimate of $-.018$ means that union membership falls by 1.8 percentage points, which is about one–seventh of the standard deviation of union membership in manufacturing. The results also hold when controlling for collective bargaining and labor protection laws in panel B. In unreported regressions, I also confirm all the results in Table 7 when conditioning on region–specific time fixed effects. These results are available upon request. Somewhat surprisingly, there is no significant change in union membership in sectors above the median proportion of capital expenditures financed with external funds (column 2).

Overall, the results in Table 7 provide additional evidence for the negative relationship between firm entry and union membership.

D. *Evidence from the Panel Study of Income Dynamics*

The critical assumption that drives the bank deregulation IV story is that bank deregulation influences union membership *only* through its impact on competition. If bank deregulation influences union membership for other reasons, my approach is called into question. It is therefore useful to consider other potential impacts of bank deregulation. First, as documented by Cetorelli and Strahan (2006), the average firm size falls following deregulation. If unions primarily target large firms, then a reduction in firm size may explain the fall in union membership independently from the competition channel. Second, the number of new firms in the economy has risen after bank deregulation (Black and Strahan (2002); Kerr and Nanda (2009)) changing the composition of workers and firms. If new workers do not organize immediately to bargain collectively with employers, this might explain why union membership falls after bank deregulation, again, independently from the competitive channel.

To provide more evidence on whether or not firm entry reduces union membership by making the economy more competitive, I collect data from the Panel Study of Income Dynamics (PSID). The advantage of the PSID, among other things, is availability of exact tenure with current employer, which enables me to estimate the impact of firm entry on union membership among "mature" employees that have been with their current employer for long periods of time. This excludes new workers in existing firms and workers in new firms that may not be union members for reasons that have nothing to do with competitiveness of the economy.

Table 8 presents estimates of the impact of bank branch deregulation on union coverage using sample of prime age (25 – 54) white male heads of household from the "core" PSID sample who work for wage and salary. All estimates are Ordinary Least Squares and are weighted by sampling weights provided by the PSID. Specifically, Table 8 reports the estimate of θ from the following specification:

$$union_{ist} = \alpha_s + \lambda_t + \theta D_{st} + \mathbf{X}'_{ist}\boldsymbol{\beta} + \varepsilon_{ist} \tag{11}$$

where $union_{ist}$ is union coverage indicator $(0-1)$ of person i who resides in state s in time t, α_s and λ_t are state and year fixed effects, D_{st} is a dummy variable taking the value of unity in the post–branching period $(\tau > t)$, and $\mathbf{X}_{ist}$ is a vector of personal characteristics that includes years of completed education, tenure with the current employer, and tenure squared.[9] Equation (11) is estimated at the individual level and not at the state–year level due to the relatively small sample.

Equation (11) is a generalization of the difference–in–differences (DID) approach where the impact of deregulation is estimated as the difference between the change in union coverage before and after deregulation with the difference in union coverage for a control group. In this specification the control group is constructed from the average of all workers in the sample, rather than from a different set of workers not

[9] I use union coverage and not union membership because it is available more frequently in the PSID.

experiencing any change in the bank branching laws. To see this, note that

$$E\left(union_{is\tau+k} - union_{is\tau-j} | D_{s\tau+k} = 1\right) - E\left(union_{is\tau+k} - union_{is\tau-j}\right) = \theta\left(1-p\right)$$

where p is the fraction of the total sample that deregulated in a given year $\tau + k$ (which is small). The estimation of θ is subject to possibly severe serial correlation problem, which results in inconsistent standard errors (Moulton (1990)). Several factors make serial correlation an especially important issue in the context of DID estimation. First, equation (11) relies on a relatively long time period from 1977 to 1996. Second, union coverage is serially correlated and third, the bank branch deregulation indicator changes little within state over time.

When estimating equation (11) I therefore cluster the standard errors at the state level. I also follow the non–parametric procedure of block–bootstrapping the standard errors, as suggested in Bertrand, Duflo and Mullainathan (2004) and Angrist and Pischke (2009). I construct a bootstrap sample by drawing with replacement 49 matrices V_s, where V_s is the entire time series of observations for state s. I then run a regression of union coverage on bank deregulation dummy, state and year fixed effects and workers' personal characteristics and obtain the estimate of θ. I draw a large number (200) of bootstrap samples and calculate the standard deviation of the resulting 200 estimates of θ.

The results in Table 8 show a significant reduction in union coverage following bank branch deregulation. The reduction is only marginally significant when using workers with all levels of tenure. The reduction becomes larger, however, with workers' tenure. This result is inconsistent with the argument that changes in union coverage are driven by changes in the composition of workers. The largest reduction in union coverage occurs among workers with at least 6 years of tenure with their current employer. The results hold with or without controlling for workers' personal characteristics and using alternative approaches to calculate the standard errors. Overall, the results in Table 8 reduce concerns that deregulation affects union membership and coverage by changing the composition of the workforce.

Table 9 provides some economic insight about the economic behavior of unions. By using the panel nature of the PSID, I am able to track workers over time and analyze changes in their outcomes as a result of changes in union coverage following bank branch deregulation. Specifically, I am following the same individuals for five years before and five years after bank branch deregulation. In addition to sample restrictions described at the beginning of the section, I restrict the sample to workers who reside in the same state during the 10 year period before and after deregulation. I then divide workers into three groups: workers who lost union coverage after bank branch deregulation in a state (panel A), workers who gained union coverage (panel B), and workers whose union coverage status hasn't changed following bank deregulation (column C).

The different columns of Table 9 represent different outcomes. These outcomes are: log real hourly wages in column (1), log weekly working hours in column (2), log annual working weeks in column (3), weekly working hours in column (4), annual working weeks in column (5), and full-time full-year indicator which equals unity for those who report working at least 35 hours per week and at least 40 weeks per year.

Workers who lost union coverage following bank deregulation report working 4.2% more hours per week and 2.6% more weeks per year, but do not experience any significant changes in their wages or full-time, full-year participation. Workers who gained union coverage following deregulation, on the other hand, have an increase in wages of 18.9% without any changes in their employment patterns (working hours and weeks or full-time, full-year participation). As expected, there are no changes in any of the outcomes for workers who did not experience changes in union coverage.

The results in Table 9 are consistent with a simple model of union behavior presented in Farber (1986). Union members seem to enjoy higher wages and work less hours per week and less weeks per year. The results are inconsistent, however, with the findings in DiNardo and Lee (2004) who employ regression discontinuity design to analyze wages and employment outcomes in establishment that barely rejected union representation and establishments that barely approved it.

E. *Integrating the Competitive Explanation with Other Explanations of Union Decline*

Competition is not the only potential explanation for declining unions in the United States. Other, potentially more important explanations include the movement of workers from manufacturing to the service industry, large gains in employment in the South of the United States which has a long tradition of right–to–work laws, oil and energy crises that significantly hurt the automobile industry, and government provisions of better working conditions and laws against discrimination which lowered workers demand for unionism. A careful empirical analysis of the impact of competition on unions must take into the account the entire menu of potential causes of union decline.

Figure 9 evaluates the relative importance of more competitive economy in the menu of other potential explanations for union decline. The solid line represents the actual trend in union membership. The solid line with connected full circles is based on residuals calculated from the regression of a union membership indicator of each worker on industry fixed effect. This line, therefore, describes the hypothetical decline in union membership if there were no changes in industrial/occupational composition of workers over time. Similarly, the solid line with connected hollow circles is based on residuals calculated from the regression of union membership indicator of each worker on industry and state fixed effect. This line describes the hypothetical decline in union membership if there were no changes in industrial/occupational and regional composition of workers over time.

Finally, the dashed line is based on residuals calculated from the regression of union membership indicator of each worker on industry and state fixed effect as well as a series of dummy variables for each year before and after bank branch deregulation. This line represents the hypothetical decline in union membership if there was no bank branch deregulation in the United States (and no changes in industrial/occupational and regional composition of workers). If bank deregulation affects union membership only through its impact on competition, then the dashed line represents the hypothetical decline in union membership if the U.S. economy did not become more competitive over time. As can be seen, about two-thirds of the decline in union membership can be attributed to more competitive economic environment in the United States.

VI. Conclusion

Removal of geographical restrictions on bank branching in the United States provides a natural experiment for studying the effect of competition among firms in the non-financial sector on union membership. Bank branch deregulation helps entrepreneurship in the non-financial sector by fostering competition and consolidation in the banking sector.

Using bank branch deregulation as an instrument for firm entry, I find the resultant intensification of competition materially reduces union membership among non–agricultural wage and salary male workers. After accounting for other potential reasons for union decline, competition between firms seems to explain about three quarters of the overall decline in union membership in the U.S. since the late 1970s.

The results in this paper provide support for the view that unions primarily target less competitive industries. Moreover, the results indicate that the decline in unionism following bank branch deregulation is associated with a reduction in union wage premium and with an increase in working hours among workers who lost union representation. This is consistent with the view that shocks to competition in the product market increase the elasticity of demand for labor and therefore increase the trade-off between employment and wages.

There are several potential directions for future research. First, it would be interesting to study the impact of competition on actual voting for union representation. This will provide a deeper understanding of the mechanisms that underlie the competition–unionism relation. Second, this paper relates to a large literature that studies the causes of the recent rise in earnings inequality in the United States. One of the reasons emphasized in this literature is union decline (Card (1996b), Card (2001)). The results in my paper, however, indicate that competition plays a major role in explaining union decline. Future research should incorporate the rising competitiveness of the U.S. economy in the menu of potential reasons for changing earnings inequality.

Appendices

A Hazard Model

Let T be a continuous random variable denoting years until bank deregulation, and let

$$\theta(t) = \lim_{dt \to o} \frac{\Pr(t \leq T < t + dt | T > t)}{dt}$$

be the *hazard* function. Intuitively, $\theta(t)\, dt$ is the probability of deregulation in a short interval of length dt after t, conditional that the state is still regulated by time t. By the law of conditional probability,

$$\begin{aligned}
\lim_{dt \to o} \frac{\Pr(t \leq T < t + dt | T > t)}{dt} &= \lim_{dt \to o} \frac{\Pr(t \leq T < t + dt, T > t)}{dt \Pr(T > t)} = \\
&= \lim_{dt \to o} \frac{\Pr(t \leq T < t + dt)}{dt \Pr(T > t)} = \frac{f_T(t)}{1 - F_T(t)} = \\
&= \frac{f_T(t)}{\overline{F}_T(t)}
\end{aligned}$$

where $f_T(t)$ and $\overline{F}_T(t)$ are the probability density function and the *survivor* function of T, respectively. Thus, the hazard function can be written as

$$\theta(t) = \frac{f_T(t)}{\overline{F}_T(t)} = \frac{-d\overline{F}_T(t)/dt}{\overline{F}_T(t)}$$

This is a differential equation in t whose solution (subject to the initial condition $\overline{F}_T(0) = 1$) is

$$\overline{F}_T(t) = \exp\left\{-\int_0^t \theta(s)\, ds\right\} \tag{12}$$

Since $f_T(t) = -d\overline{F}_T(t)/dt$, from equation (12) we get

$$f_T(t) = \theta(t) \exp\left\{-\int_0^t \theta(s)\, ds\right\} \tag{13}$$

Consider state i which did not deregulate by 1980. The probability of still being regulated h years after 1980 is

$$\begin{aligned}
\Pr(T \geq 1980 + h | T > 1980) &= \frac{\Pr(T \geq 1980 + h, T > 1980)}{\Pr(T > 1980)} = \\
&= \frac{\Pr(T \geq 1980 + h)}{\Pr(T > 1980)} = \frac{\overline{F}_T(1980 + h)}{\overline{F}_T(1980)}
\end{aligned}$$

The probability that state i deregulated between 1980 and $1980 + s_i$ $(0 < s_i < h)$, is

$$\begin{aligned}\Pr\left(T = 1980 + s_i | T > 1980\right) &= \frac{\Pr\left(T = 1980 + s_i, T > 1980\right)}{\Pr\left(T > 1980\right)} = \\ &= \frac{\Pr\left(T = 1980 + s_i\right)}{\Pr\left(T > 1980\right)} = \frac{f_T\left(1980 + s_i\right)}{\overline{F}_T\left(1980\right)}\end{aligned}$$

Let N_R be the number of states still regulated in $1980+h$, and let N_D be the number of states that deregulated between 1980 and $1980 + s_i$. Assuming independence of timing of deregulation between states, the likelihood function is given by

$$L = \prod_{i=1}^{N_R} \left\{ \frac{\overline{F}_T\left(1980 + h\right)}{\overline{F}_T\left(1980\right)} \right\} \prod_{j=1}^{N_D} \left\{ \frac{f_T\left(1980 + s_j\right)}{\overline{F}_T\left(1980\right)} \right\} \tag{14}$$

Substituting equations (12) and (13) into equation (14) gives the following expression for the likelihood function

$$\begin{aligned} L &= \prod_{i=1}^{N_R} \left\{ \frac{\exp\left\{ -\int_0^{1980+h} \theta\left(s\right) ds \right\}}{\exp\left\{ -\int_0^{1980} \theta\left(s\right) ds \right\}} \right\} \times \\ & \prod_{j=1}^{N_D} \left\{ \frac{\theta\left(1980 + s_j\right) \exp\left\{ -\int_0^{1980+s_j} \theta\left(s\right) ds \right\}}{\exp\left\{ -\int_0^{1980} \theta\left(s\right) ds \right\}} \right\} \end{aligned} \tag{15}$$

Typically, the hazard function θ has two components. The first component describes the way in which θ shifts at any given point in time between states who have different characteristics and the second component is the time profile of deregulation which is common to all states. Following Lancaster (1979), I assume the following functional form for the hazard function:

$$\theta\left(t\right) = \psi_1\left(union\right) \psi_2\left(t\right)$$

where *union* is union membership in a given state and year. It is common to assume that:

$$\psi_1\left(union\right) = \exp\left\{\rho_0 + \rho_1 union\right\} \tag{16}$$

Assuming that the hazard function increases over time, the functional form for $\psi_2\left(t\right)$ can be expressed as:

$$\psi_2\left(t\right) = \alpha t^{\alpha - 1}, \alpha > 1 \tag{17}$$

Equations (16) and (17) completely specify the hazard function θ, and thus the likelihood function L in equation (15). Notice that from equation (16) it follows that:

$$\log \psi_1 = \rho_0 + \rho_1 union$$

The coefficient of interest is ρ_1 which stands for the percentage change in the hazard of bank deregulation as a result of a marginal change in union membership. Estimates of ρ_1 that maximize the likelihood function in equation (15) are reported in Table 1.

B Formation of Microdata Samples

May and Outgoing Rotation Groups Current Population Survey (CPS) samples are obtained from the NBER data collection section and Unicon Research, respectively. These data can be obtained from <http://www.nber.org/data/cps_may.html> and <http://www.unicon.com/>. I limit the CPS samples to prime age (25-54) white males, who work for wage and salary in the non-agricultural sector. I further limit the sample to individuals who either work, or have a job but currently not working. I exclude individuals with missing information on union membership, union coverage, and industry of employment. Finally, I drop individuals with missing or zero sampling weights. Following the literature on bank deregulation in the United States, I exclude Delaware and South Dakota due to large concentration of credit card banks in these states. The resulting CPS sample includes $1,317,387$ observations. The first two columns in Appendix Table 3 provide more details on the sample restrictions imposed on the original CPS samples.

When analyzing changes in the union wage premium following bank branch deregulation, I further limit the sample to individuals who report working at least forty hours per week (full-time workers), have hourly wages above \$1.675 (1/2 of the minimum wage in 1982) and below the $99th$ percentile of year-specific distribution of real hourly wages of full-time workers. Hourly wages are adjusted to constant 1982 dollars using the Consumer Price Index.

CPS measures of education are not standardized over time. Beginning with the 1992 CPS, education is no longer classified according to years of schooling, but according to the highest degree received classified into categories (see Polivka (1996) for a review). I therefore classify the workers in the sample into five educational categories, according to highest degree received: $0-8$, $9-11$, 12, $13-15$, and $16+$. I calculate years of potential experience as the maximum between zero and age (in the survey year) minus years of schooling minus 6, where years of schooling are imputed for the post 1992 period using figures in Park (1994).

The Panel Study of Income Dynamics (PSID) sample is collected by the Institute for Social Research at the University of Michigan and is available for free download at <http://psidonline.isr.umich.edu/>. The full set of variable used in the construction of the PSID sample is listed in Appendix Table 2. I restrict the PSID sample to prime age (25-54) white male heads of household from the "core" PSID sample of 1968 families, who are not self-employed and either work or have a job but currently not working. I further limit the sample to individuals with non-missing information on union coverage and non-missing state of residence. State of residence is not consistently reported in the PSID after 1993 and thus I do not use post 1993 data. Finally, I exclude individuals with missing tenure with the current employer and individuals who reside in Delaware and South Dakota. The resulting sample includes $18,329$ observations. The last column in Appendix Table 3 provides more details on the sample restrictions imposed on the original PSID sample.

In the PSID, hourly earnings are reported separately for hourly and for salaried workers. In 1993, wages are not reported for salaried workers. In this year only, I approximate wages for salaried workers by dividing their total annual earnings (in the survey year) by total annual working hours in the year prior to the survey. When analyzing wages, I limit the sample to individuals with hourly wages above \$1.675 (1/2 of the minimum wage in 1982) and below the $99th$ percentile of year-specific distribution of real hourly wages. Hourly wages are adjusted to constant 1982 dollars using the Consumer Price Index.

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TABLE 1
Timing of Bank Branch Deregulation and Pre-Existing Union Membership and Coverage: The Duration Model

	All Industries				Manufacturing Only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Union membership	-.710				-.165			
	(.747)				(.409)			
Change in union membership		-.227				.032		
		(.395)				(.174)		
Union coverage			-.606				-.220	
			(.716)				(.383)	
Change in union coverage				-.086				-.057
				(.339)				(.206)
Number of observations	270	270	270	270	270	269	270	269

Note - The model is a Weibul hazard model where the dependent variable is the log expected time to bank branch deregulation. The hazard of deregulation is a likelihood that a state deregulates at time *t*, given that the state has not yet deregulated. Each coefficient measures the percentage change in the hazard of deregulation as a result of a marginal change in either the level of union membership and coverage or changes in union membership and coverage. Standard errors are adjusted for state-level clustering and appear in parentheses. Union membership is the percentage of nonagricultural wage and salary employees who are union members. Union coverage is the percentage of nonagricultural wage and salary employees who are covered by a collective bargaining. In columns (1) – (4), union membership and coverage are averaged to the state-year level using workers in all industries. In columns (5) – (8), union membership and coverage are averaged to the state-year level using only workers in manufacturing. All specifications control for political economy variables that affect the timing of bank branch deregulation (Kroszner and Strahan, 1999). These variables are: (1) small bank share of all banking assets, (2) capital ratio of small banks relative to large, (3) relative size of insurance in states where banks may sell insurance, (4) an indicator which takes upon a value of one if banks may sell insurance, (5) relative size of insurance in states where banks may not sell insurance, (6) small firm share, (7) share of state government controlled by Democrats, (8) an indicator which takes upon a value of one if a state is controlled by one party, (9) average yield on bank loans minus Fed funds rate, (10) an indicator which takes upon a value of one if state has unit banking law, and (11) an indicator which takes upon a value of one if state changes bank insurance powers. Sample period is 1978 to 1994, excluding 1982, and the sample comprises 36 states that deregulated after 1978. States drop from the sample once they deregulate.

TABLE 2
WALD ESTIMATE OF THE IMPACT OF LOG NEW INCORPORATIONS PER CAPITA ON UNION MEMBERSHIP

	(1) Before Bank Deregulation	(2) After Bank Deregulation	(3) Difference (2) - (1)
Union membership	.195	.179	-.016**
			(.007)
Log new incorporations per capita	1.023	1.105	.082***
			(.030)
Wald estimate of the impact of log new incorporations per capita on union membership			-.194*
			(.113)
OLS estimate of the impact of log new incorporations per capita on union membership			-.007
			(.009)

NOTE – The sample size is 1,372 state – year observations and consists of 49 states between the years 1978 and 2006, excluding 1982. Delaware and South Dakota are excluded because of large concentration of credit card bank in these states. The year 1982 is excluded because union status questions were not asked in this year. Average union membership for each state and year is calculated using May Current Population Survey files for the years 1978-1981 and Outgoing Rotation Groups files for the years 1983-2006. The underlying sample includes prime age (25-54) white men who work for wage and salary, excluding those who work in agriculture. Specifically, union membership in each state and year is the average residual from an OLS regression of union membership indicator on five dummies of years of completed education (0-8, 9-11, 12, 13-15, and 16+), potential experience and its square, and industry fixed effects, pooling all years together. I use CPS sampling weights when calculating the residuals and averaging them to the state – year level. The number of new incorporations is from Dun and Bradstreet <http://www.dnb.com/us/>. New incorporations are divided by population estimates from the U.S. Census Bureau. Timing of bank branch deregulation for each state is from Kroszner and Strahan (1999). All specifications in column (3) control for state and year fixed effects. Wald estimate of the impact of log new incorporations per capita on union membership uses bank branch deregulation indicator as an instrument for log new incorporations. Branch deregulation indicator equals one during all years in which a state permits in – state branching. Standard errors are clustered at the state level and appear in parentheses. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent, respectively.

TABLE 3

THE IMPACT OF LOG NEW INCORPORATIONS PER CAPITA ON UNION MEMBERSHIP: OLS, IV, AND TSLS ESTIMATES

		Specification of Instrument(s)			
	OLS (1)	Dummy (2)	Linear (3)	Quadratic (4)	Non-param. (5)
		Panel A: Without Controlling for Labor Laws			
Log new incorporations per capita	-.007	-.194*	-.191	-.164*	-.150*
	(.009)	(.113)	(.207)	(.092)	(.084)
F-statistic of excluded instruments		7.61	2.28	7.67	9.66
[*p*-value]		[.008]	[.138]	[.008]	[.003]
<*p*-value of OIR test>				<.746>	<.957>
Number of observations	1,372	1,372	1,372	1,372	1,372
		Panel B: Controlling for Labor Laws			
Log new incorporations per capita	-.013	-.199**	-.259	-.141**	-.124**
	(.011)	(.098)	(.245)	(.065)	(.056)
F-statistic of excluded instruments		8.08	1.37	8.16	10.89
[*p*-value]		[.007]	[.248]	[.006]	[.002]
<*p*-value of OIR test>				<.271>	<.845>
Number of observations	1,284	1,284	1,284	1,284	1,284

NOTE – The sample size in panel A is 1,372 state–year observations and consists of 49 states between the years 1978 and 2006, excluding 1982. Delaware and South Dakota are excluded because of large concentration of credit card bank in these states. The year 1982 is excluded because union status questions were not asked in this year. Average union membership for each state and year is calculated using May Current Population Survey files for the years 1978-1981 and Outgoing Rotation Groups files for the years 1983-2006. The underlying sample includes prime age (25-54) white men who work for wage and salary, excluding those who work in agriculture. Specifically, union membership in each state and year is the average residual from an OLS regression of union membership indicator on five dummies of years of completed education (0-8, 9-11, 12, 13-15, and 16+), potential experience and its square, and industry fixed effects, pooling all years together. I use CPS sampling weights when calculating the residuals and averaging them to the state–year level. Column (1) reports OLS estimate of the impact of log new incorporations per capita on average union membership in a state. In column (2), I use bank branch deregulation indicator as an instrumental variable for log new incorporations per capita. Branch deregulation indicator equals one during all years in which a state permits in–state branching. In column (3), I use years since branch deregulation as an instrumental variable. In column (4), I use years since branch deregulation and its square as instruments. Finally, in column (5) I use a series of dummy variables for each year before and after deregulation. All specifications include state and year fixed effects. In panel B, I also control for collective bargaining laws obtained from Valetta and Freeman (1988) and later updated by NBER until 1996. Specifically, I include (a) an indicator which equals one if a state permits collective bargaining and equals zero otherwise, and (b) an indicator which equals one if a state permits striking and equals zero otherwise. All specifications in panel B also include three indicators for presence of laws that limit the ability of employers to fire workers. Presence of the laws by states was obtained from Autor, Donohue III, and Schwab (2006). These laws are: (a) "public policy" provides employees with protections against discharges that would prevent an important public policy, such as performing jury duty; (b) "good-faith" prevents employers from firing workers for bad cause such as just before a substantial commission is due; and (c) "implied contract" protection comes into force when an employer implicitly promises not to terminate a worker without good cause. Standard errors are clustered at the state level and appear in parentheses. * and ** indicate statistical significance at the 10 and 5 percent, respectively.

TABLE 4

THE IMPACT OF LOG NEW INCORPORATIONS PER CAPITA ON UNION MEMBERSHIP: OLS, IV, AND TSLS ESTIMATES WITH REGION – SPECIFIC TIME FIXED EFFECTS

		Specification of Instrument(s)			
	OLS (1)	Dummy (2)	Linear (3)	Quadratic (4)	Non-param. (5)
		Panel A: Without Controlling for Labor Laws			
Log new incorporations per capita	-.020**	-.276	-.133	-.214*	-.187**
	(.009)	(.180)	(.255)	(.117)	(.094)
F-statistic of excluded instruments		4.80	1.78	5.14	8.07
[*p*-value]		[.033]	[.188]	[.028]	[.007]
<*p*-value of OIR test>				<.513>	<.678>
Number of observations	1,372	1,372	1,372	1,372	1,372
		Panel B: Controlling for Labor Laws			
Log new incorporations per capita	-.017	-.229	-.049	-.194	-.138*
	(.011)	(.139)	(.204)	(.120)	(.072)
F-statistic of excluded instruments		4.68	1.54	3.49	8.27
[*p*-value]		[.036]	[.221]	[.068]	[.006]
<*p*-value of OIR test>				<.175>	<.469>
Number of observations	1,284	1,284	1,284	1,284	1,284

NOTE – The sample size in panel A is 1,372 state–year observations and consists of 49 states between the years 1978 and 2006, excluding 1982. Delaware and South Dakota are excluded because of large concentration of credit card bank in these states. The year 1982 is excluded because union status questions were not asked in this year. Average union membership for each state and year is calculated using May Current Population Survey files for the years 1978-1981 and Outgoing Rotation Groups files for the years 1983-2006. The underlying sample includes prime age (25-54) white men who work for wage and salary, excluding those who work in agriculture. Specifically, union membership in each state and year is the average residual from an OLS regression of union membership indicator on five dummies of years of completed education (0-8, 9-11, 12, 13-15, and 16+), potential experience and its square, and industry fixed effects, pooling all years together. I use CPS sampling weights when calculating the residuals and averaging them to the state–year level. Column (1) reports OLS estimate of the impact of log new incorporations per capita on average union membership in a state. In column (2), I use bank branch deregulation indicator as an instrumental variable for log new incorporations per capita. Branch deregulation indicator equals one during all years in which a state permits in–state branching. In column (3), I use years since branch deregulation as an instrumental variable. In column (4), I use years since branch deregulation and its square as instruments. Finally, in column (5) I use a series of dummy variables for each year before and after deregulation. All specifications include state and year fixed effects as well as region – specific year fixed effects. In panel B, I also control for collective bargaining laws obtained from Valetta and Freeman (1988) and later updated by NBER until 1996. Specifically, I include (a) an indicator which equals one if a state permits collective bargaining and equals zero otherwise, and (b) an indicator which equals one if a state permits striking and equals zero otherwise. All specifications in panel B also include three indicators for presence of laws that limit the ability of employers to fire workers. Presence of the laws by states was obtained from Autor, Donohue III, and Schwab (2006). These laws are: (a) “public policy” provides employees with protections against discharges that would prevent an important public policy, such as performing jury duty; (b) “good-faith” prevents employers from firing workers for bad cause such as just before a substantial commission is due; and (c) “implied contract” protection comes into force when an employer implicitly promises not to terminate a worker without good cause. Standard errors are clustered at the state level and appear in parentheses. * and ** indicate statistical significance at the 10 and 5 percent, respectively.

TABLE 5
External Financial Dependence for Manufacturing Sectors

Sector	Median Loans/Assets For 1998 Survey of Small Business Finance Firms	External Financial Dependence for Mature COMPUSTAT Firms
Leather and leather products	0.04	-0.96
Tobacco manufactures	N/A	-0.92
Apparel and other textile	0.13	-0.61
Fabricated metal products	0.12	-0.24
Food and kindred products	0.27	-0.24
Furniture and fixtures	0.36	-0.23
Miscellaneous manufacturing	0.31	-0.20
Stone, clay, glass, and concrete products	0.28	-0.20
Printing and publishing	0.33	-0.07
Instruments and related products	0.29	-0.04
Transportation equipment	0.06	0.01
Industrial machinery and equipment	0.21	0.01
Primary metal industries	0.31	0.03
Lumber and wood products	0.49	0.04
Rubber and plastic products	0.30	0.04
Paper and allied products	0.37	0.06
Petroleum and coal products	0.60	0.09
Textile mill products	0.47	0.10
Electrical and electronic equipment	0.14	0.22
Chemicals and allied products	0.33	0.28

Note – External financial dependence equals the proportion of capital expenditures financed with external funds. A negative value indicates that firms have free cash flow, whereas a positive value indicates that firms must issue debt or equity to finance their investment. The figures represent the median value for COMPUSTAT firms (that have been on COMPUSTAT for at least 10 years) in each sector over the 1980 to 1997 period. The loans/assets ratio is the median ratio of loans to assets for small firms from the Federal Reserve 1998 Survey of Small Business Finance.
Source – Cetorelli and Strahan (2006).

TABLE 6
Characteristics of Workers by External Financial Dependence

	Proportion of Capital Expenditures Financed With External Funds			Median Loans/Assets For 1998 Survey of Small Business Finance Firms		
	(1) Below Median	(2) Above Median	(3) Difference (2) - (1)	(4) Below Median	(5) Above Median	(6) Difference (5) - (4)
Union member	.333	.358	.025	.318	.346	.028
			(.032)			(.032)
Covered by a union	.348	.387	.039	.346	.362	.016
			(.033)			(.030)
Education < 12 years	.301	.221	-.080***	.224	.257	.033
			(.029)			(.026)
Education = 12 years	.383	.402	.019	.403	.400	-.003
			(.035)			(.029)
Education > 12 years	.316	.377	.061**	.373	.343	-.030
			(.028)			(.032)
Potential experience	19.9	19.7	-.186	19.6	19.7	.151
			(.622)			(.604)
Weekly working hours	38.7	40.1	1.48	40.3	39.5	-.788
			(1.40)			(1.389)
Working full-time	.897	.910	.014	.912	.902	-.010
			(.016)			(.014)

Note – The table reports mean characteristics of workers in manufacturing sectors with different degrees of external financial dependence. All data are for 1978, before bank deregulation in most states. Columns (1) and (2) divide workers by the median proportion of capital expenditures financed with external funds using data on mature COMPUSTAT firms. Column (3) reports the differences between columns (2) and (1) and the accompanying standard errors. Column (4) and (5) divide workers by the median loans-to-assets ratio for 1998 Small of Small Business Finance firms. Column (6) reports the differences between columns (5) and (4) and the accompanying standard errors. Standard errors are clustered at the state level and appear in parentheses. ** and *** indicate statistical significance at the 5 and 1 percent, respectively.

TABLE 7
The Impact of Bank Deregulation on Union Membership in Manufacturing Sectors By External Financial Dependence

	Proportion of Capital Expenditures Financed With External Funds		Median Loans/Assets for 1998 Survey of Small Business Finance Firms	
	Below Median	Above Median	Below Median	Above Median
	(1)	(2)	(3)	(4)
	Panel A: Without Controlling for Labor Laws			
Branch deregulation	-.002	-.005	-.011	.006
	(.016)	(.016)	(.015)	(.015)
Interstate deregulation	.002	-.014	-.004	-.018*
	(.014)	(.012)	(.015)	(.009)
R^2	.60	.70	.67	.68
Number of observations	1,371	1,365	1,364	1,366
	Panel B: Controlling for Labor Laws			
Branch deregulation	-.002	-.015	-.013	.000
	(.016)	(.015)	(.014)	(.014)
Interstate deregulation	.002	-.011	-.003	-.019**
	(.015)	(.011)	(.014)	(.009)
R^2	.59	.72	.69	.70
Number of observations	1,284	1,283	1,281	1,279

Note – The sample is at the state – year level and consists of 49 states between the years 1978 and 2006, excluding 1982. Delaware and South Dakota are excluded because of large concentration of credit card bank in these states. The year 1982 is excluded because union status questions were not asked in this year. Average union membership for each state and year is calculated using May Current Population Survey files for the years 1978-1981 and Outgoing Rotation Groups files for the years 1983-2006. The underlying sample includes prime age (25-54) white men who work for wage and salary in manufacturing. Union membership in each state and year is the average residual from an OLS regression of union membership indicator on five dummies of years of completed education (0-8, 9-11, 12, 13-15, and 16+) and potential experience and its square, pooling all years together. I use CPS sampling weights when calculating the residuals and averaging them to the state–year level. I calculate the residuals separately by the median external financial dependence. In columns (1) and (2), workers are divided by the median external financial dependence for mature COMPUSTAT firms. In columns (3) and (4), workers are divided by the median loans-to-assets ratio for 1998 SSBF firms. All specifications include state and year fixed effects. In panel B, I also control for collective bargaining laws obtained from Valetta and Freeman (1988) and later updated by NBER until 1996. Specifically, I include (a) an indicator which equals one if a state permits collective bargaining and equals zero otherwise, and (b) an indicator which equals one if a state permits striking and equals zero otherwise. All specifications in panel B also include three indicators for presence of laws that limit the ability of employers to fire workers. Presence of the laws by states was obtained from Autor, Donohue III, and Schwab (2006). These laws are: (a) “public policy” provides employees with protections against discharges that would prevent an important public policy, such as performing jury duty; (b) “good-faith” prevents employers from firing workers for bad cause such as just before a substantial commission is due; and (c) “implied contract” protection comes into force when an employer implicitly promises not to terminate a worker without good cause. Branch deregulation indicator equals one during all years in which a state permits in–state branching. Interstate banking deregulation indicator equals one during all years in which a state permits out-of-state banking companies to buy banks headquartered in the state. Standard errors are clustered at the state level and appear in parentheses. * and ** indicate statistical significance at the 10 and 5 percent, respectively.

TABLE 8
THE IMPACT OF BANK DEREGULATION ON UNION COVERAGE:
EVIDENCE FROM THE PANEL STUDY OF INCOME DYNAMICS

	No Covariates			With Covariates		
		Tenure			Tenure	
	All	3+	6+	All	3+	6+
	(1)	(2)	(3)	(4)	(5)	(6)
Branch deregulation	-.028	-.039	-.061	-.029	-.042	-.065
(clustered s.e.s)	(.018)	(.019)**	(.022)***	(.018)	(.019)**	(.021)***
[block – bootstrapped s.e.s]	[.015]*	[.015]**	[.020]***	[.016]*	[.016]**	[.020]***
R^2	.07	.08	.09	.12	.11	.12
Number of observations	18,329	12,803	9,297	18,329	12,803	9,297

NOTE – The dependent variable is union coverage indicator. The sample is at the worker level and consists of respondents to Panel Study of Income Dynamics (PSID) surveys in the years 1977 – 1993. The sample is restricted to prime age (25 – 54) white male heads of household from the "core" PSID sample who work for wage and salary. All estimates are Ordinary Least Squares and are weighted by sampling weights provided by the PSID. All specifications include state and year fixed effects. Specifications in columns (4) – (6) also control for years of completed education, tenure, and tenure squared. Branch deregulation indicator equals one during all years in which a state permits in–state branching. In parentheses I report standard errors which are clustered at the state level. In brackets, I report block – bootstrapped standard errors. I construct a bootstrap sample by drawing with replacement 49 matrices Vs, where Vs is the entire time series of observations for state *s*. I then run a regression of union coverage on bank deregulation dummy, state and year fixed effects and workers' personal characteristics (columns 4-6) and obtain the estimated impact of bank deregulation on union coverage indicator. I draw a large number (200) of bootstrap samples and calculate the standard deviation of the resulting 200 estimates of the impact of bank deregulation on union coverage indicator. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent, respectively.

TABLE 9
CHANGES IN WORKERS' OUTCOMES AS A RESULT OF CHANGES IN UNION COVERAGE: ESTIMATES FROM A PANEL OF WORKERS

	Log Wages (1)	Log Weekly Hours (2)	Log Annual Weeks (3)	Weekly Hours (4)	Annual Weeks (5)	Working Full-Time Full-Year (6)
	Panel A: Workers Who Lost Union Coverage After Bank Deregulation					
Branch deregulation	.017	.042	.026	1.642	1.468	.034
(clustered s.e.s)	(.036)	(.022)*	(.018)	(.914)*	(.695)**	(.042)
[block-bootstrapped s.e.s]	[.032]	[.017]**	[.015]*	[.706]**	[.569]**	[.037]
R^2	.48	.15	.18	.14	.19	.21
Number of observations	813	813	813	873	873	873
	Panel B: Workers Who Gained Union Coverage After Bank Deregulation					
Branch deregulation	.189	-.020	.031	-1.171	1.488	.053
(clustered s.e.s)	(.067)***	(.029)	(.047)	(1.078)	(1.254)	(.058)
[block-bootstrapped s.e.s]	[.059]***	[.027]	[.043]	[.897]	[1.222]	[.055]
R^2	.51	.26	.09	.26	.14	.18
Number of observations	562	562	562	624	624	624
	Panel C: Workers Whose Union Coverage Did Not Change					
Branch deregulation	.038	.006	.006	.116	.213	.013
(clustered s.e.s)	(.025)	(.008)	(.006)	(.358)	(.276)	(.010)
[block-bootstrapped s.e.s]	[.023]	[.007]	[.005]	[.302]	[.216]	[.009]
R^2	.26	.05	.05	.06	.08	.04
Number of observations	6,772	6,772	6,772	7,691	7,691	7,691

NOTE – The dependent variables are: log real hourly wages in column (1), log weekly working hours in column (2), log annual working weeks in column (3), weekly working hours in column (4), annual working weeks in column (5), and full-time full-year indicator in column (6). Full-time full-year workers are those who report working at least 35 hours per week and at least 40 weeks per year. The sample is at the worker level and consists of respondents Panel Study of Income Dynamics (PSID) respondents in years 1977 – 1993. The sample is restricted to prime age (25 – 54) white male heads of household from the "core" PSID sample who work for wage and salary. Additionally, I limit the sample to workers whom I can trace for five years before bank branch deregulation and five years after deregulation in their state. In panel A, the sample is restricted to workers who *lost* union coverage after bank deregulation. In panel B, the sample is restricted to workers who *gained* union coverage after bank deregulation. In panel C, the sample is restricted to workers whose union coverage status did not change after bank deregulation. All estimates are Ordinary Least Squares and are weighted by sampling weights provided by the PSID. All specifications include completed years of education, tenure, tenure squared, and state and year fixed effects. Branch deregulation indicator equals one during all years in which a state permits in–state branching. In parentheses I report standard errors which are clustered at the state level. In brackets, I report block – bootstrapped standard errors. I construct a bootstrap sample by drawing with replacement 49 matrices V_s, where V_s is the entire time series of observations for state *s*. I then run a regression of union membership on bank deregulation dummy, state and year fixed effects and workers' personal characteristics (columns 4-6) and obtain the estimated impact of bank deregulation on union membership indicator. I draw a large number (200) of bootstrap samples and calculate the standard deviation of the resulting 200 estimates of bank deregulation on union membership. *, **, and *** indicate statistical significance at the 10, 5, and 1 percent, respectively.

APPENDIX TABLE 1
Timing of Branch and Interstate Bank Deregulation

State	State code	Type of deregulation: Branch	Inter-state	State	State code	Type of deregulation: Branch	Inter-state
Alabama	AL	1981	1987	Montana	MT	1990	1993
Alaska	AK	1960	1982	Nebraska	NE	1985	1990
Arizona	AZ	1960	1986	Nevada	NV	1960	1985
Arkansas	AR	1994	1989	New Hampshire	NH	1987	1987
California	CA	1960	1987	New Jersey	NJ	1977	1986
Colorado	CO	1991	1988	New Mexico	NM	1991	1989
Connecticut	CT	1980	1983	New York	NY	1976	1982
District of Columbia	DC	1960	1985	North Carolina	NC	1960	1985
Florida	FL	1988	1985	North Dakota	ND	1987	1991
Georgia	GA	1983	1985	Ohio	OH	1979	1985
Hawaii	HI	1986	1997	Oklahoma	OK	1988	1987
Idaho	ID	1960	1985	Oregon	OR	1985	1986
Illinois	IL	1988	1986	Pennsylvania	PA	1982	1986
Indiana	IN	1989	1986	Rhode Island	RI	1960	1984
Iowa	IA	1999	1991	South Carolina	SC	1960	1986
Kansas	KS	1987	1992	Tennessee	TN	1985	1985
Kentucky	KY	1990	1984	Texas	TX	1988	1987
Louisiana	LA	1988	1987	Utah	UT	1981	1984
Maine	ME	1975	1978	Vermont	VT	1970	1988
Maryland	MD	1960	1985	Virginia	VA	1978	1985
Massachusetts	MA	1984	1983	Washington	WA	1985	1987
Michigan	MI	1987	1986	West Virginia	WV	1987	1988
Minnesota	MN	1993	1986	Wisconsin	WI	1990	1987
Mississippi	MS	1986	1988	Wyoming	WY	1988	1987
Missouri	MO	1990	1986				

Note - Dates of branch and interstate deregulation are taken from Kroszner and Strahan (1999).

APPENDIX TABLE 2

Variables Used for Analyses of Panel Study of Income Dynamics

Year	Head of Household	Age	Gender	Ethnicity	Tenure	Education	Empl. Status	Self Employed	Union Coverage
1977	ER30219	V5350	V5351	V5662	V5384	V5647	V5373	V5376	V5382
1978	ER30248	V5850	V5851	V6209	V5941	V6194	V5872	V5875	V5877
1979	ER30285	V6462	V6463	V6802	V6499	V6787	V6492	V6493	V6495
1980	ER30315	V7067	V7068	V7447	V7102	V7433	V7095	V7096	V7098
1981	ER30345	V7658	V7659	V8099	V7711	V8085	V7706	V7707	V7709
1982	ER30375	V8352	V8353	V8723	V8379	V8709	V8374	V8375	V8377
1983	ER30401	V8961	V8962	V9408	V9010	V9395	V9005	V9006	V9008
1984	ER30431	V10419	V10420	V11055	V10519	V11042	V10453	V10456	V10458
1985	ER30465	V11606	V11607	V11938	V11668	V12400	V11637	V11640	V11649
1986	ER30500	V13011	V13012	V13565	V13068	V13640	V13046	V13049	V13052
1987	ER30537	V14114	V14115	V14612	V14166	V14687	V14146	V14149	V14152
1988	ER30572	V15130	V15131	V16086	V15181	V16161	V15154	V15157	V15160
1989	ER30608	V16631	V16632	V17483	V16682	V17545	V16655	V16658	V16661
1990	ER30644	V18049	V18050	V18814	V18120	V18898	V18093	V18096	V18099
1991	ER30691	V19349	V19350	V20114	V19420	V20198	V19393	V19396	V19399
1992	ER30735	V20651	V20652	V21420	V20720	V21504	V20693	V20696	V20699
1993	ER30808	V22406	V22407	V23276	V22489	V23333	V22448	V22451	V22454

Year	State of Residence	Sampling Weight	Hours per Week (last year)	Weeks per Year (last year)	Annual Hours (last year)	Salaried or Paid by Hour	Hourly Earnings: If Paid Hourly	Hourly Earnings: If Salaried	Annual Earnings (current)
1977	V5203	ER30245	V5418	V5417	V5232	V5420	V5424	V5421	…
1978	V5703	ER30282	V5905	V5904	V5731	V5907	V5911	V5908	…
1979	V6303	ER30312	V6516	V6515	V6336	V6518	V6522	V6519	…
1980	V6903	ER30342	V7119	V7118	V6934	V7121	V7125	V7122	…
1981	V7503	ER30372	V7742	V7741	V7530	V7714	V7718	V7715	…
1982	V8203	ER30398	V8404	V8403	V8228	V8382	V8386	V8383	…
1983	V8803	ER30428	V9035	V9034	V8830	V9013	V9017	V9014	…
1984	V10003	ER30462	V10562	V10561	V10037	V10462	V10466	V10463	…
1985	V11103	ER30497	V11706	V11705	V11146	V11653	V11657	V11654	…
1986	V12503	ER30534	V13106	V13105	V12545	V13056	V13060	V13057	…
1987	V13703	ER30569	V14202	V14203	V13745	V14156	V14160	V14157	…
1988	V14803	ER30605	V15258	V15257	V14835	V15164	V15168	V15165	…
1989	V16303	ER30641	V16759	V16758	V16335	V16665	V16669	V16666	…
1990	V17703	ER30686	V18197	V18196	V17744	V18103	V18107	V18104	…
1991	V19003	ER30730	V19497	V19496	V19044	V19403	V19407	V19404	…
1992	V20303	ER30803	V20797	V20796	V20344	V20703	V20707	V20704	…
1993	V21603	ER30864	V22577	V22575	V21634	V22463	V22470	…	V22464

APPENDIX TABLE 3

Sample Restrictions Imposed on Microdata Samples

	Current Population Survey		
	May Supplement (1978-1981)	Outgoing Rotation Groups (1983-2006)	Panel Study of Income Dynamics (1977-1993)
Total number of observations in the raw data	486,668	11,001,341	671,390
Sample restrictions (observations deleted):			
Head of household	…	…	(543,207)
Prime-age (25-54) in the year of the survey	(253,284)	(6,763,618)	(45,573)
Male	(121,456)	(2,195,285)	(20,866)
White	(12,098)	(280,646)	(20,764)
Works for wages and salary, not in agriculture*	(18,518)	(332,373)	(9,594)
Either working, or with job but not at work	(3,170)	(61,861)	(147)
Non-missing union membership and coverage**	(19,326)	(76,041)	(417)
Non-missing industry	(10)	(0)	(0)
Non-missing state of residence	(0)	(0)	(197)
Non-missing tenure	…	…	(152)
Not residing in Delaware or South Dakota	(1,194)	(31,739)	(114)
Non-missing and positive sampling weight***	(0)	(3)	(12,030)
Total number of observations that satisfy sample restrictions above	57,612	1,259,775	18,329
	1,317,387		

Note – In the PSID the restrictions are different in that: * restricts to not self-employed; ** only restricts to non-missing union coverage; and *** restricts to the "core" sample of 1968 families.

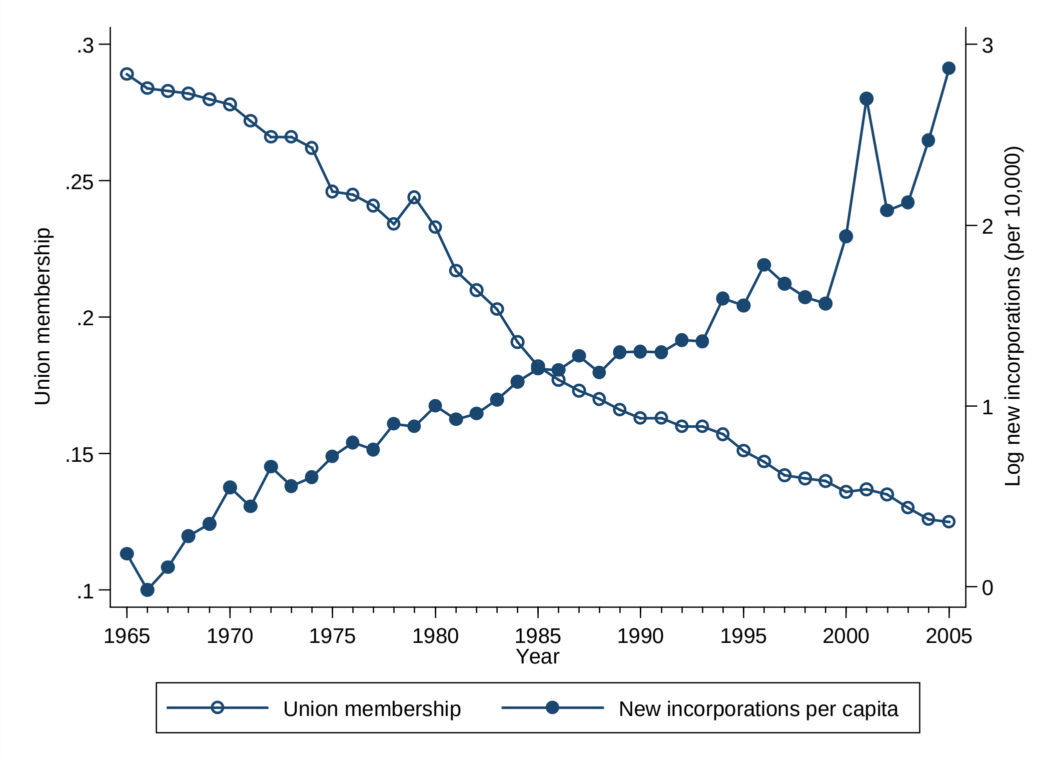

FIGURE 1

TRENDS IN UNION MEMBERSHIP AND NUMBERS OF NEW INCORPORATIONS PER CAPITA IN THE UNITED STATES

SOURCES – Union membership series are from <http://www.unionstats.com>. Number of new incorporations is from Dun and Bradstreet. New incorporations are divided by population estimates from the U.S. Census Bureau.

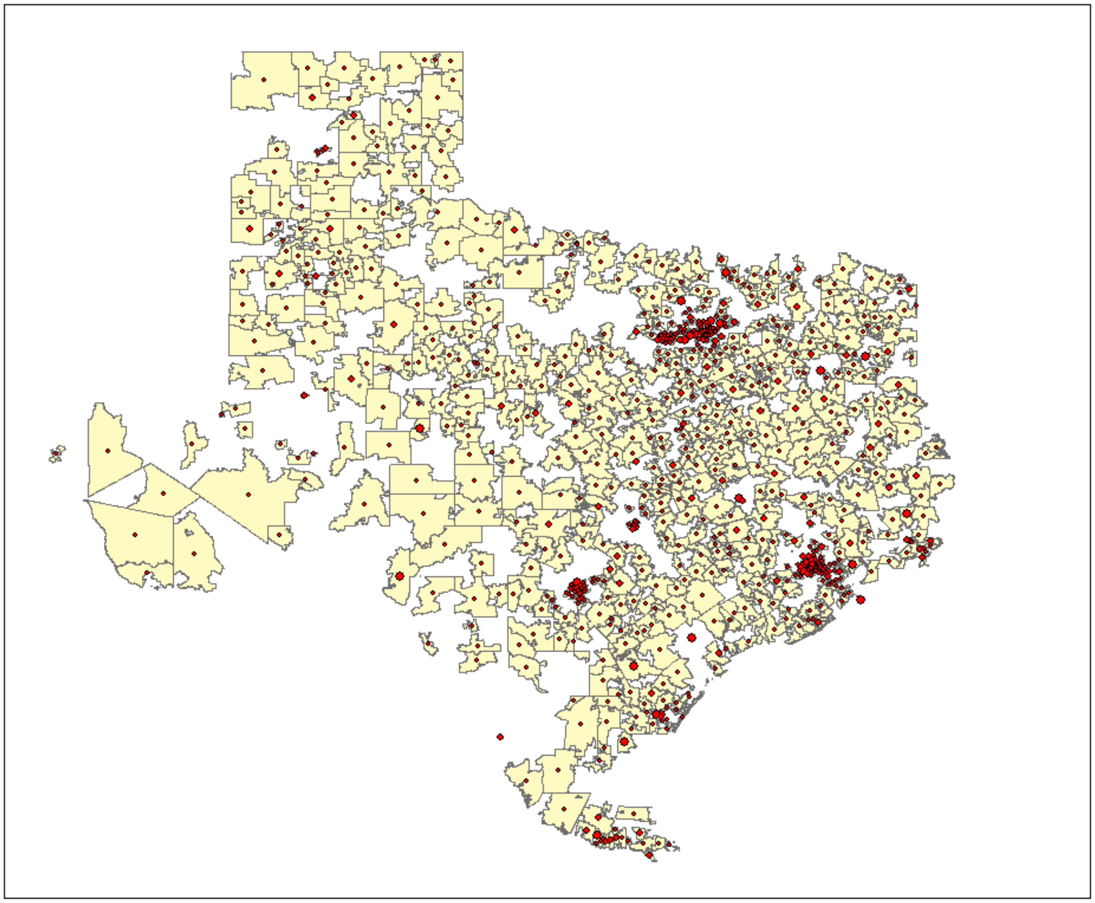

FIGURE 2A
Location of Bank Branches in Texas Before Bank Branch Deregulation

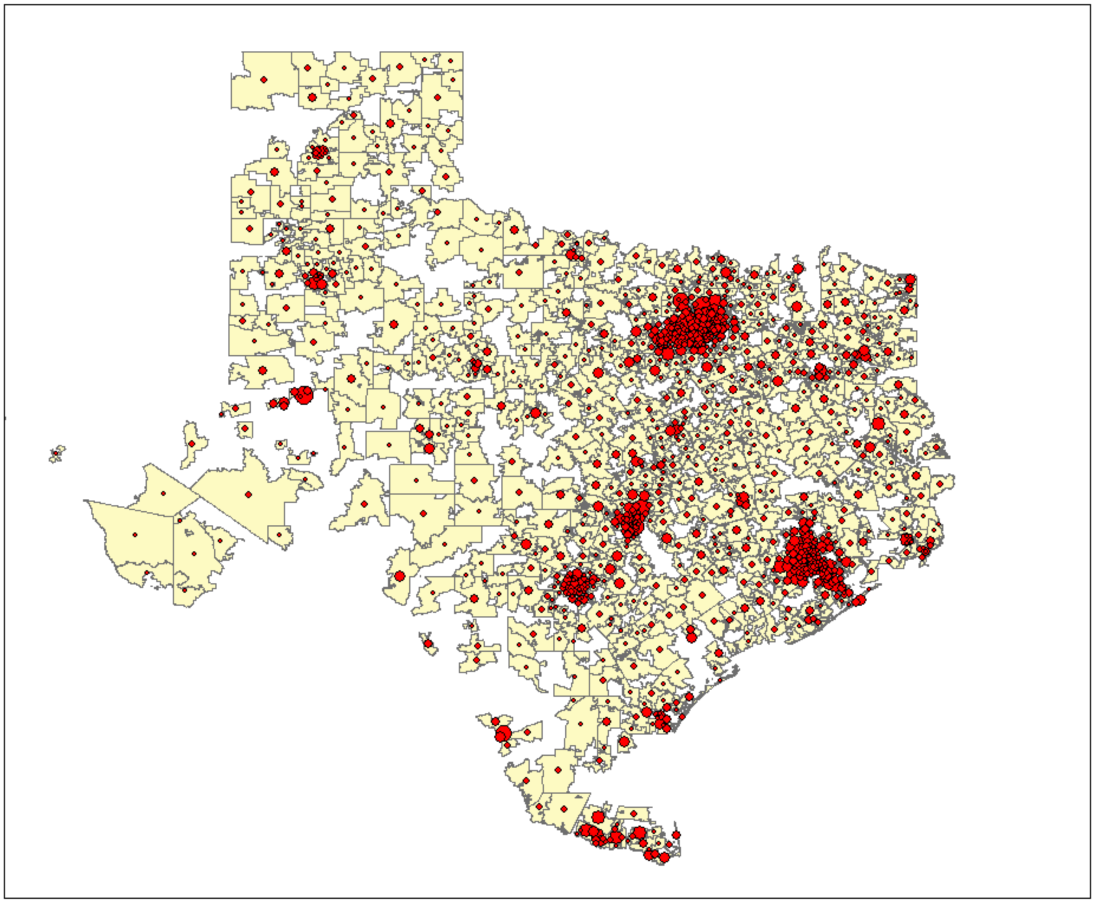

FIGURE 2B
Location of Bank Branches in Texas After Bank Branch Deregulation

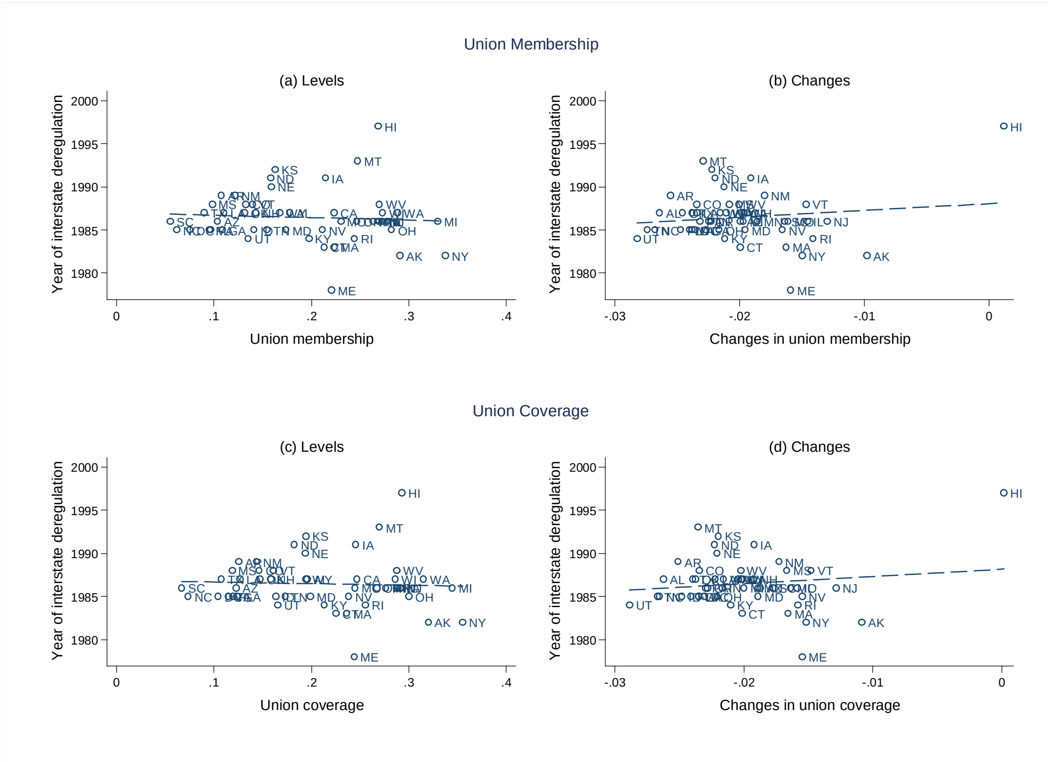

FIGURE 3
Pre-Existing Union Membership and Coverage and the Timing of Interstate Bank Deregulation

Note – Figure (a) plots the correlation between average union membership in all years prior to interstate deregulation and the subsequent year of deregulation for each state. Figure (b) plots the correlation between the *change* in union membership in all years prior to interstate deregulation and the subsequent year of deregulation for each state. Figures (c) and (d) are similar to plots (a) and (b), except that they use union coverage instead of union membership. The absolute values of the *t*-statistics in plots (a)-(d) are: 0.53, 0.46, 0.35, and 0.47, respectively.

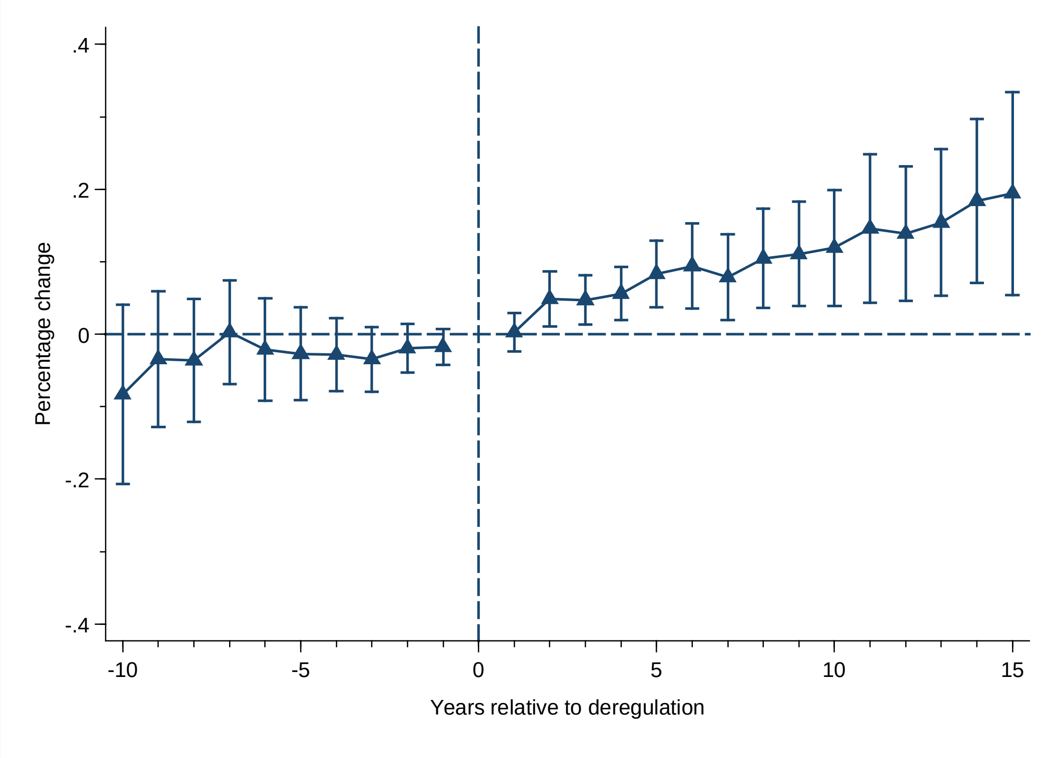

FIGURE 4
THE IMPACT OF BANK BRANCH DEREGULATION ON LOG NEW INCORPORATIONS PER CAPITA

NOTE – The figure shows the dynamic impact of branch deregulation on percentage of new incorporations per capita in a state. The impact of deregulation on new incorporations per capita is represented by connected triangles. Vertical lines mark 95% confidence intervals, adjusted for state level clustering. All estimates are relative to the year of deregulation. Specifically, I report estimates of $\beta_1 - \beta_{25}$ from the following regression:

$$\ln(\text{new incorporations per capita})_{st} = \alpha_s + \lambda_t + \beta_1 D^{-10}_{st} + \beta_2 D^{-9}_{st} + \ldots + \beta_{25} D^{+15}_{st} + \varepsilon_{st},$$

where *s* and *t* denote state and year, respectively. The D's equal zero, except as follows: D^{-j} equals one for states in the j^{th} year before deregulation, while D^{+j} equals one for states in the j^{th} year after deregulation. I exclude the year of deregulation, thus estimating the dynamic effect of deregulation on log new incorporations per capita relative to the year of deregulation. α_s is state fixed effect that accounts for observable and unobservable state characteristics that do not vary over time. λ_t is year fixed effect that accounts for national trends in entry of new incorporations per capita.

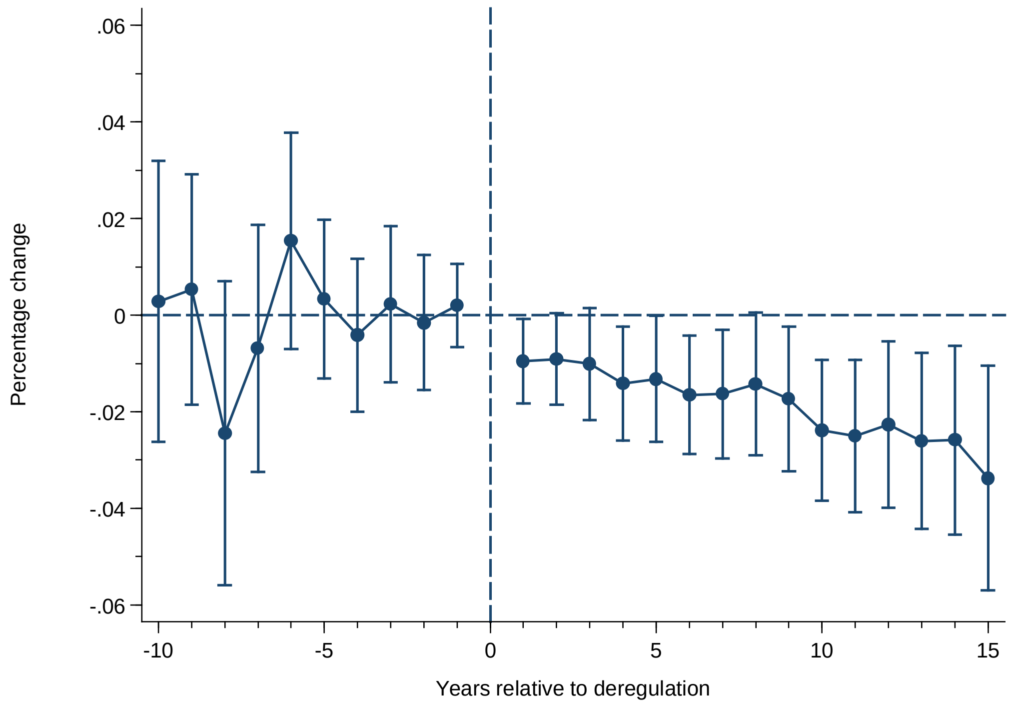

FIGURE 5
THE IMPACT OF BANK BRANCH DEREGULATION ON UNION MEMBERSHIP

NOTE – The figure shows the dynamic impact of branch deregulation on percentage of non-agricultural wage and salary union workers in a state. The impact of deregulation on union membership is represented by connected circles. Vertical lines mark 95% confidence intervals, adjusted for state level clustering. All estimates are relative to the year of deregulation. Specifically, I report estimates of $\beta_1 - \beta_{25}$ from the following regression:

$$(\text{percent unionized workers})_{st} = \alpha_s + \lambda_t + \beta_1 D^{-10}_{st} + \beta_2 D^{-9}_{st} + \ldots + \beta_{25} D^{+15}_{st} + \varepsilon_{st},$$

where *s* and *t* denote state and year, respectively. The D's equal zero, except as follows: D^{-j} equals one for states in the j^{th} year before deregulation, while D^{+j} equals one for states in the j^{th} year after deregulation. I exclude the year of deregulation, thus estimating the dynamic effect of deregulation on union membership relative to the year of deregulation. α_s is state fixed effect that accounts for observable and unobservable state characteristics that do not vary over time. λ_t is year fixed effect that accounts for national trends in union membership.

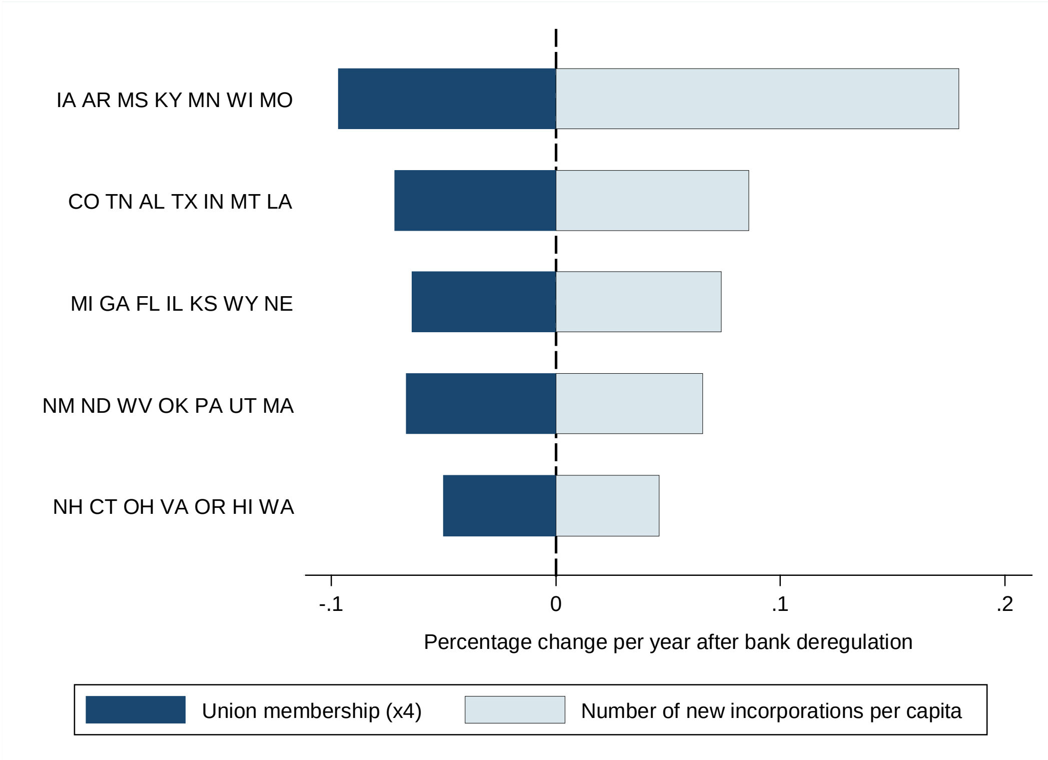

FIGURE 6
CHANGES IN NEW INCORPORATIONS PER CAPITA AND UNION MEMBERSHIP BY STATES

NOTE – The figure shows percentage change in the average level of new incorporations per capita and union membership as a result of bank branch deregulation. Percentage changes are divided by the number of years since bank branch deregulation. The figure plots the results separately for five groups of states, sorted by the impact of deregulation on log new incorporations per capita. The states of IA, AR, MS, KY, MN, WI, and MO, for example, had the largest increase in the percentage of new incorporations per capita after bank deregulation. The states of NH, CT, OH, VA, OR, HI, and WA, on the other hand, had the smallest increase in the percentage of new incorporations per capita after bank deregulation. Changes in union membership are multiplied by four for ease of illustration.

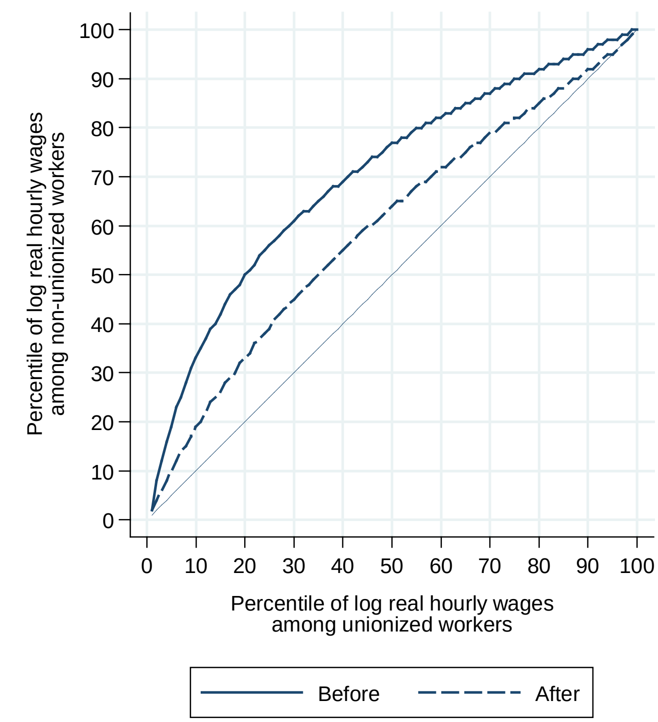

FIGURE 7

The Location of Unionized Workers in the Non-Unionized Wage Distribution Before and After Bank Branch Deregulation

Note – The figure shows the location of unionized workers in the distribution of log real hourly wages of non-unionized workers before and after bank branch deregulation. The results in the figure were obtained using the following procedure: First, I calculate residuals for unionized and non-unionized workers from equations (9) and (10). I keep 100 unionized workers, each corresponding to a different percentile of unionized workers' relative log real conditional wage distribution. Next, I calculate their position in the non-unionized workers' relative log real conditional wage distribution. I repeat this procedure before (solid line) and after (dashed line) bank branch deregulation.

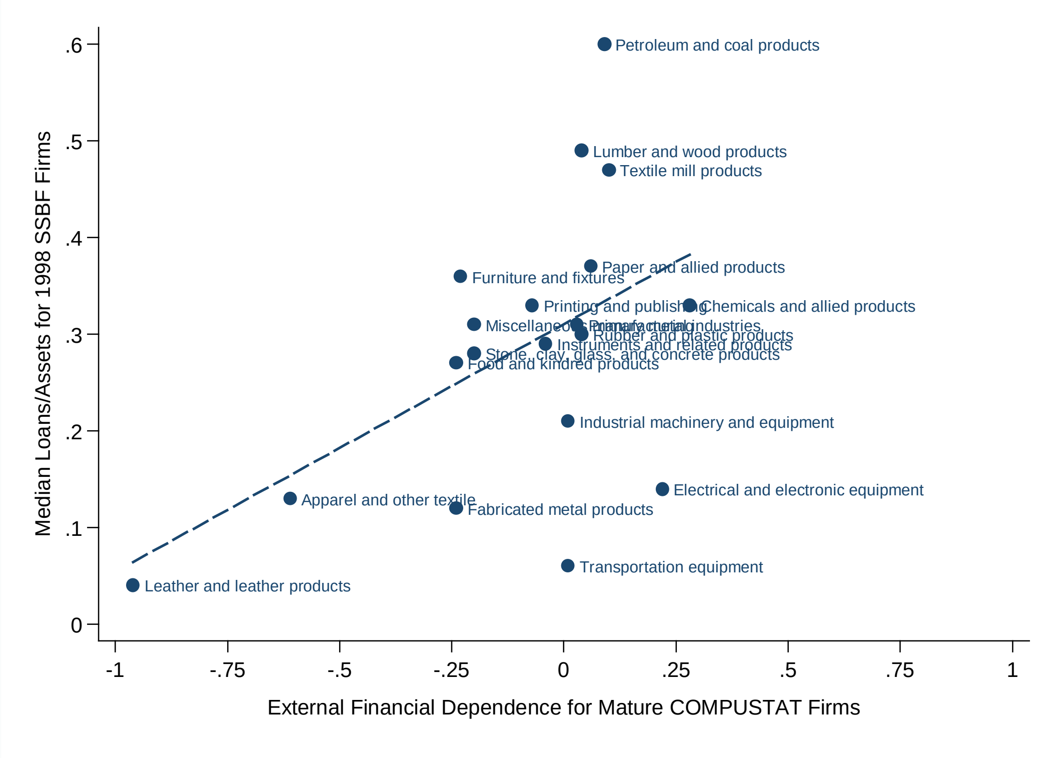

FIGURE 8

Correlation between Measures of External Financial Dependence among Manufacturing Sectors

Note – External financial dependence equals the proportion of capital expenditures financed with external funds. A negative value indicates that firms have free cash flow, whereas a positive value indicates that firms must issue debt or equity to finance their investment. The figures represent the median value for COMPUSTAT firms in each industry sector over the 1980 to 1997 period. Mature firms are those that have been on COMPUSTAT for 10 years or more. The loans/assets ratio is the median ratio of loans to assets for small firms from the Federal Reserve 1998 Survey of Small Business Finance.

Source – Cetorelli and Strahan (2006).

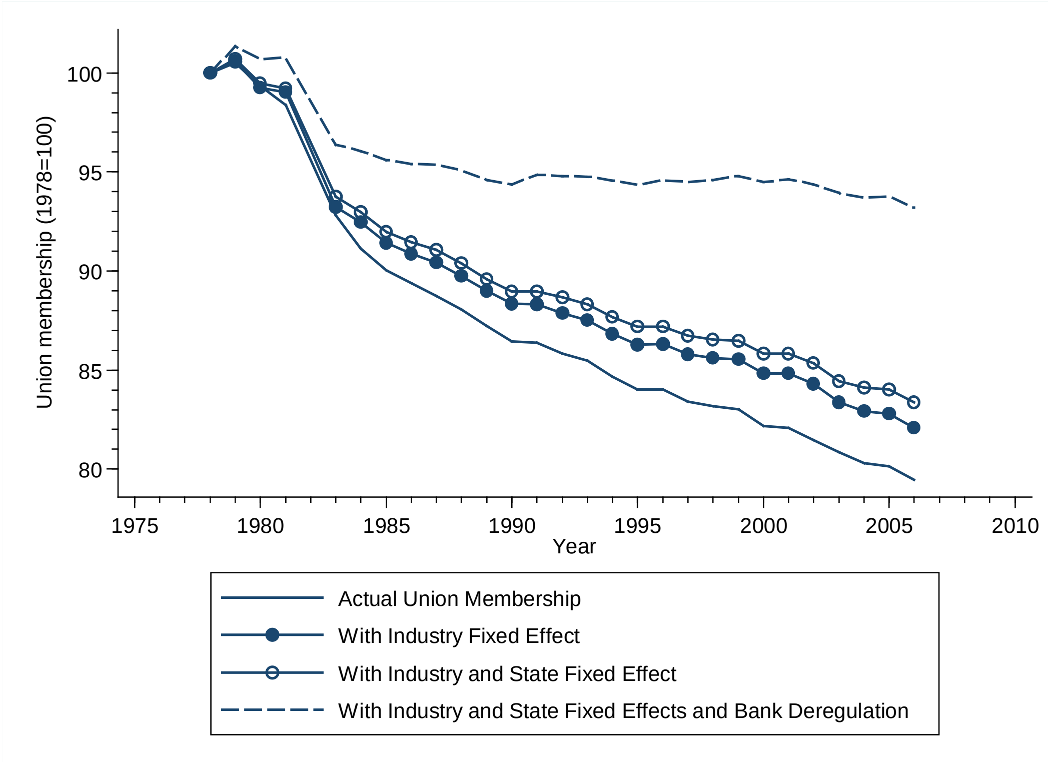

FIGURE 9
EXPLAINING THE DECLINE IN UNIONS

NOTE – The figure shows time series of union membership in the United States for the period 1978 – 2006. Average union membership for each year is calculated using May Current Population Survey files for the years 1978-1981 and Outgoing Rotation Groups files for the years 1983-2006. I exclude the year 1982 because union status questions were not asked in this year. The underlying sample includes prime age (25-54) white men who work for wage and salary, excluding those who work in agriculture. For ease of exposition, all figures are indexed to 100 in 1978. The solid line represents the actual trend in union membership. The solid line with connected full circles is based on residuals calculated from the regression of union membership indicator of each worker on industry fixed effect. Similarly, the solid line with connected hollow circles is based on residuals calculated from the regression of union membership indicator of each worker on industry and state fixed effect. Finally, the dashed line is based on residuals calculated from the regression of union membership indicator of each worker on industry and state fixed effect as well as a series of dummy variables for each year before and after bank branch deregulation. I use CPS sampling weights when calculating the residuals and averaging them for each year.