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Branching of Banks and Union Decline

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Branching of Banks and Union Decline*

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This paper proposes a novel explanation for the decline in unions in the United States since the late 1970s: state-by-state removal of geographical restrictions on branching of banks. Bank branch deregulation reduces union membership *in the non-banking sectors* by intensifying entry of new firms, especially in sectors with high dependence on external finance. New firm entry, in turn, is associated with a reduction in union wage premium, and subsequently leads to adverse union voting. I provide empirical evidence for these channels using repeated cross-sectional and panel data of U.S. workers and union representation election outcomes.

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I. Introduction

The institutional structure of American labor markets changed dramatically in the last several decades. In the 1950s unions seemed permanently established in the U.S. economy, with union workers representing over a quarter of the civilian workforce and hundreds of thousands of workers voted annually to join unions. Changes in the organization of labor started to happen in the mid-1960s, when a steady decline in union membership began. By 2009, the proportion of union members stood at only 12.3 per cent, with even lower unionization rate among private sector workers. The "vanishing" of U.S. unions is depicted in Figure 1.

There are many potential reasons for union decline. These reasons include structural changes in the composition of workers (Dickens and Leonard [1985]), the political attitude towards unions during the Reagan administration (Farber and Western [2002]), the openness of the U.S. economy to international trade (Slaughter [2007]), high union wage premium and the resulting low firm profitability (Blanchflower and Freeman [1990]), increasing international competitive pressures (Freeman and Katz [1991], Abowd and Lemieux [1993]), intensification of management opposition to unions (Freeman and Medoff [1984], Freeman [1986]), and government provision of better working conditions and laws against discrimination, which lowered workers' desire to unionize (Farber [1987], Farber and Krueger [1993]).

This paper provides a novel explanation for the reduction in union membership. I argue that relaxation of geographical restrictions on bank branching within state borders has played an important role in the decline of unions in the non-banking sectors of the economy. Bank deregulation contributed to the decline of unions in the overall economy by spurring entry of new non-bank firms. Deregulation has also led to a reduction in union wage premium, suggesting that firm entry triggered cross-firm competition in product and labor markets, potentially increasing the demand for labor and its elasticity. Subsequently, the diminished attractiveness of unions resulted in adverse voting for union representation in elections held by the National Labor Relations Board. I provide evidence for these arguments exploring the cross-state and cross-time variation in the timing of bank branch deregulation.

I report several findings from analyzing data that span the 1977-2006 period, and combine information on union membership and coverage of prime-age men from May and Outgoing Rotation Groups Current Population Surveys (CPS) as well as Panel Study of Income Dynamics (PSID), timing of bank branch deregulation by state from Kroszner and Strahan [1999], external financial dependence for manufacturing sectors from Cetorelli and Strahan [2006], number of new incorporations per capita from Black and Strahan [2002], union representation election results from the National Labor Relations Board, wrongful-discharge protections from Autor, Donohue and Schwab [2006], and right-to-work laws from the National Right to Work Legal Defense Foundation.

The key for identifying the impact of bank deregulation on union membership is the cross-state variation in the timing of removal of geographical restrictions on bank branching. The gradual, state-by-state nature of bank deregulation allows to separate the impact of deregulation from the overall trend in union membership. The validity of the identification strategy rests on the assumptions that (a) the timing of bank deregulation was not affected by pre-existing political power of unions and (b) there are no unobserved factors that affect union membership and at the same time correlate with the timing of bank deregulation.

To validate the identifying assumptions, 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 have occurred later in states where labor unions had greater influence. Using a hazard model and incorporating the political-economy factors from Kroszner and Strahan [1999], I show that pre-existing union membership does not explain the timing of bank deregulation. This result is reinforced by a Granger [1969] causality test which indicates 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 examine the relationship between bank deregulation and union membership exploiting cross industry variation in their external financial dependence (Rajan and Zingales [1998]). I build on the work of Cetorelli and Strahan [2006] who show that bank deregulation had a significant impact on real economic activity of manufacturing firms with above-median dependence on external finance. If bank deregulation is causally associated with a reduction in union membership, one would expect a reduction in union membership only in sectors with above-median dependence on external finance. At the same time, there should be no impact on union membership in the other sectors. These sectors were unaffected by bank deregulation. The division of manufacturing sectors by external financial dependence tests the causal interpretation of the deregulation-unionism relationship. If there are unobserved factors that drive union membership and correlate with bank deregulation, then they should probably affect union membership in industries with both high and low dependence on external finance.

The results indicate that bank branch deregulation has a substantial, first-order impact on union membership. According to the most conservative estimate, bank deregulation reduces union membership by 1.1 percentage points, after accounting for state and time fixed effects. This is about 10% of the overall reduction in union membership between 1953 and 2009. The results are robust to inclusion of a variety of state characteristics, exclusion of the airline, trucking, and railroad industries that had their own deregulation episodes during the sample period, and across data. The main finding of a negative relationship between bank deregulation and union membership is confirmed using both the CPS and the PSID samples of workers. Finally, the results hold in worker-level regression as well as in aggregate state-level data.

The paper provides evidence for the mechanisms that drive the relationship between bank deregulation and union membership. First, bank deregulation affects union membership primarily in sectors with relatively high dependence on external finance. In these sectors bank deregulation had a first-order impact on firm entry (Cetorelli and Strahan [2006]). This suggests that bank deregulation reduces union membership through entry of new firms. Next, consistent with the hypothesis that firm entry may increase cross-firm competition for products and/or labor, I show that deregulation reduces union wage premium, thus making unions less attractive. I then use data from the National Labor Relations Board to show that following bank deregulation, workers are indeed more likely to vote against unions. Finally, I consider a possibility that deregulation affect union membership through firm entry by changing the composition of workers. Using a sample of workers from the PSID, I find little evidence that unions decline because of compositional changes of workers. Rather, the results suggest that deregulation, through its impact on firm entry, leads to decertification of unions.

This paper relates to a large literature that shows a first-order relationship between financial development and economic activity (see, e.g. King and Levine [1993], Jayaratne and Strahan [1996], Rajan and Zingales [1998]). If unions impose inefficiency in production, then this paper's results show a specific channel through which financial innovations increase productivity and growth in the economy. The "textbook" inefficiency of unions arises because unionized firms produce less than the equilibrium amount of goods due to higher cost of labor. There are other sources of inefficiency. Matsa [2010], for example, shows that collective bargaining leads firms to distort their capital structure. 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. Next section describes the process of bank branch deregulation, while Section III goes through the theoretical links between bank deregulation and the industrial structure of non-bank sectors of the economy. Section IV describes the empirical strategy and present the main results of this paper. Section V concludes.

II. Background

Geographic restrictions on banks have their origins in the United States 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 20th 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 and their clientele. The creation of checkable money market mutual funds made banking by mail and telephone easier, thus further weakening the power of local bank monopolies. Furthermore, 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. Finally, the failures of banks and thrifts in the 1980s increased public awareness of the advantage of large, well-diversified banks.

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. Table 1 documents the year of removal of geographical restrictions on bank branching.

III. Theoretical Considerations

Removal of geographical restrictions on bank branching dramatically increased creation of new branches (Amel and Liang [1992]), intensified competition between banks (Stiroh and Strahan [2003]), altered their risk-taking behavior (Goetz [2010]), and improved bank efficiency and performance, lowering both the costs and prices of banking services (Flannery [1984], Kroszner and Strahan [1998]). Rice and Strahan [2010], for example, show that in states more open to branching, small firms borrowed at interest rates 80 to 100 basis points lower than firms in other states. Lower interest rates resulted in accelerated flow of credit to businesses (Kroszner and Strahan [2006]), boosting entry of new non-bank firms (Black and Strahan [2002], Cetorelli and Strahan [2006], and Kerr and Nanda [2009]). The intensification of entrepreneurial activity, in turn, had a significant impact on the real economy by increasing the growth rate of states' real per capita income (Jayaratne and Strahan [1996]), raising entrepreneurial income (Demyanyk [2008]), and reducing both the volatility of income (Morgan, Rime and Strahan [2004], Demyanyk, Ostergaard and Sørensen [2007]) and income inequality (Beck, Levine and Levkov [2010]).

I argue that the main mechanism through which bank deregulation affected union membership is new firm entry. Black and Strahan [2002], Cetorelli and Strahan [2006], and Kerr and Nanda [2009] use different sources of data to document a strong, first-order relationship between bank deregulation and entry of new non-bank firms. Black and Strahan [2002], for example, estimate that the impact of bank deregulation on log new incorporations per capita is "quite large relative to the effect of the state business cycle on new business incorporations." (p. 2823). Cetorelli and Strahan [2006] find that deregulation increases the number of establishments primarily in sectors with needs for external finance. Finally, Kerr and Nanda [2009] show that bank deregulation increased the number of new start-ups and expanded the number of facilities of existing firms.

Entry of new firms may affect union membership through several mechanisms. First, entry of new firms may change the pool of workers by hiring previously unemployed workers. If new workers do not immediately unionize, firm entry may alter the proportion of union members by simply changing the composition of workers. Firm entry may also change the demand for workers. The resulting shift in the demand curve for labor will raise wages and reduce union wage premium, making unions less attractive.

Second, firm entry may increase cross-firm competition in the product markets. This may affect union membership through several mechanisms. First, according to Hicks-Marshallian laws of factor demand, an intensification of product market competition increases the elasticity of the demand for labor. Higher elasticity increases the wage-employment trade-off and makes it harder for unions to negotiate wage gains without compromising employment. If unions are concerned with maintaining employment of their members, they will be more reluctant to negotiate wage gains when operating in more competitive product markets. Product market competition, therefore, makes unions less attractive relative to non-union workplaces.

Furthermore, product market competition may spur innovation (Aghion, Bloom, Blundell, Griffith and Howitt [2005]) and the return to skill (Guadalupe [2007]). If the skill-biased innovation is primarily in the non-union sectors of the economy, it may attract skilled union workers to the innovating non-union sectors. This will reduce the proportion of union members (Acemoglu, Aghion and Violante [2001]).

Finally, recent evidence suggests that bank deregulation, through its impact on cross-firm competition, had a significant impact on the wages of black workers relative to white workers (Levine, Levkov and Rubinstein [2008]). To the extent that unions provide a shield against discriminatory practices, a reduction in wage disparities in the economy reduces workers' demand for unions.[1]

IV. Method and Results

These ideas suggest that deregulation of banks potentially plays an important role in explaining changes in union membership. The identification of a reduced form

1. Earlier studies examine the impact of competition between banks following deregulation on the gender composition of bank employees (Ashenfelter and Hannan [1986]) and their relative wages (Black and Strahan [2001]).

impact of bank deregulation on union membership is based on panel data regressions of the form,

$$y_{st} = \alpha_s + \beta_t + \rho d_{st} + \mathbf{x}'_{st}\boldsymbol{\theta} + \varepsilon_{st}, \tag{1}$$

where y_{st} is proportion of union members in state s and time t, d_{st} is a bank deregulation measure taking the value of unity in the post-deregulation period, $\mathbf{x}_{st}$ is a vector of state characteristics that vary over time, α_s is a state fixed effect, and β_t is a time fixed effect. I cluster the standard errors by state to deal with concerns with serial correlation (Bertrand, Duflo and Mullainathan [2004]).

The state fixed effect captures state level observable and unobservable characteristics that do not change over time. The year fixed effect captures common economy-wide shocks that affect union membership. The coefficient of interest, ρ, captures the reduced-form effect of bank deregulation on union membership. In the above specification, ρ is a generalization of the difference-in-differences approach where the effect of deregulation is estimated as the difference between the change in union membership before and after bank deregulation with the difference in union membership for a control group. In this specification the control group is constructed from the average of all states in the sample, rather than from a different set of states not experiencing any change in their branching laws. A positive and statistically significant estimate of ρ indicates gains in union membership after deregulation, whereas a negative estimate indicates a reduction in union membership following bank deregulation.[2]

Union membership for each state and year is calculated from Current Population Surveys for the years $1977-2006$, excluding the year 1982 when union status information is not available. The sample includes prime-age $(25-54)$ white men who work for wage and salary in the private sector, excluding workers in agriculture.

2. Let τ be the year of deregulation such that $d = 1$ for $t > \tau$ and $d = 0$ otherwise. The difference-in-differences estimate of the impact of bank deregulation, d, on union membership, y, is,

$$E\left(y_{s,\tau+k} - y_{s,\tau-j} | d_{s,\tau+k} = 1\right) - E\left(y_{s,\tau+k} - y_{s,\tau-j}\right) = \rho\left(1-m\right),$$

for some $k, j > 0$, where m is the fraction of the total sample that deregulated in a year $\tau + k$ and is assumed to be small.

Timing of bank branch deregulation by state is from Kroszner and Strahan [1999]. I drop Delaware and South Dakota because of large concentration of credit card banks in these states. The vector of state characteristics, $\mathbf{x}_{st}$, includes unemployment rate, real gross domestic product per capita, presence of right-to-work laws, and employment protection laws (Autor et al. [2006]). The individual-level CPS data are aggregated to state-year level for each of the 48 states plus the District of Colombia and 29 years, leaving a total of $1,421$ state-year observations. More details about the data and sample construction are provided in the Data Appendix.

IV.A. Main Results

The cross-state variation in the timing of removal of geographical restrictions on bank branching is key for identifying the impact of bank deregulation on union membership. The timing of deregulation is not perfectly correlated with the calendar time, which allows to control for the trend in union membership over the years. Moreover, having time series observations for each state allows to control for state observable and unobservable characteristics that do not change over time and potentially both affect union membership and correlate with the timing of bank deregulation.

Table 2 reports Ordinary Least Squares estimates of ρ from equation (1). The dependent variable in Table 2 is the proportion of union members in state s and year t, weighted by the sampling weights provided by the Current Population Survey. Bank deregulation is a dummy variable which takes the value of unity in the post bank branch deregulation period. The estimate of $-.014$ in column (1) suggest that after controlling for state and year fixed effects, bank deregulation reduces union membership by 1.4 percentage points. This result is statistically significant at 5% after adjusting the standard error of ρ for clustering at the state level.

The impact of bank deregulation on union membership is economically meaningful. The estimate of 1.4 percentage points corresponds to 15% of the standard deviation of union membership in the sample. Alternatively, 1.4 percentage points is about 10% of the overall reduction in union membership between the years 1953 and 2009.

Column (2) of Table 2 introduces time-varying characteristics of states. These characteristics capture the fact that workers' demand for unionism increases when economic condition worsen. Specifically, column (2) adds controls for states' unemployment rate and real per capita GDP. In addition, column (2) controls for an indicator which equals one if a state has right-to-work laws which captures workers' desire to unionize. The right-to-work law indicator is identified because some states introduced right-to-work laws during the sample period. The addition of time-varying state characteristics in column (2) reduces the negative impact of deregulation on union membership from $-.014$ to $-.012$, but still remains statistically significant at 10%.

The reduction in the estimate of ρ from $-.014$ to $-.012$ due to incorporation of economic conditions of states makes sense. In the terminology of equation (1), the difference between the estimates of ρ in columns (1) and (2) equals to the correlation between $\mathbf{x}$ and y (which is $\boldsymbol{\theta}$) times the correlation between d and $\mathbf{x}$. Since worse economic conditions usually increase union membership and bank deregulation has a positive impact on economic conditions (Jayaratne and Strahan [1996]), the estimate of ρ in column (1) overestimates the negative impact of deregulation on union membership.

Finally, column (3) adds a series of indicators for existence of employment protection laws in a state. These laws capture the ability of employers to fire workers and thus represent the environment of labor relations in a state. Specifically, these laws provide employees with protection against discharges that would, for example, prevent an important public policy such as performing jury duty, or discharges just before a substantial commission is due. The addition of labor protection laws further reduces the estimated coefficient of ρ to $-.010$, with statistical significance of 10%.[3]

3. The unemployment rate by state is from the Bureau of Labor Statistics, Gross Domestic Product is from the Bureau of Economic Analysis. The GDP series is adjusted to $2000 using the Consumer Price Index. Right-to-work laws are from Farber [1984], Table 1 and from the National Right to Work Legal Defense Foundation. Information on labor protection laws by state is from Autor et al. [2006]. These data exclude the District of Colombia and are available for the years 1978-1998. I assume that existence of laws has not changed between 1999 and 2006 and impose the 1998 information for the post 1999 period. There are 77 fewer observations in column (3) relative to columns (1)-(2) because of lack of data for 1977 (49 observations) and for D.C. in the years

The dependent variable in columns (1)-(3) is a simple proportion of union members in a given state and year, based on union status information of workers sampled by the Current Population Survey. This simple averaging does not account for the fact that individuals with different characteristics have different likelihood to join a union. Moreover, if bank deregulation is associated with changes in the composition of workers (Beck et al. [2010]), then it is important to account for these characteristics when analyzing the impact of deregulation on union membership.

In columns (4)-(6) of Table 2, the dependent variable is the average state-year value of residuals from a worker-level regression of union membership status on a series of dummy variables that indicate years of completed education (0-8, 9-11, 12, 13-15, and 16+), potential experience in the labor marker (age - years of completed education - 6), potential experience squared, and industry fixed effects, pooling all years and states. The resulting residuals represent unionization rates that are "adjusted" for personal characteristics of the workers. Years of education and potential experience in the labor market, for example, account for the fact that workers with different educational background and experience have different likelihood to join a union, while industry fixed effects allow for the average union membership to differ by industry.[4]

The results indicate that bank deregulation reduces the "covariate-adjusted" union membership by $1.3 - 1.6$ percentage points, depending on the set of state-level control variables. In the specification in column (6), for example, deregulation reduces union membership by 1.3 percentage points after controlling for state and year fixed effects, the unemployment rate, real GDP per capita, existence of right-to-work laws, and employment protection laws.

Table 3 repeats the analysis in Table 2 but excludes workers in the airlines, truck-

1978-2006 (28 observations).

4. After the 1992 Current Population Survey, years of completed education are available only in categories (see Polivka [1996] for a review of changes in the Current Population Survey). 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 years coded with the categorical education question, I use figures from Park [1994] to assign years of completed education to each worker based upon highest degree held.

ing, and railroad industries. Each of these industries experienced its own deregulation episode in the late 1970s and early 1980s that had affected their labor relations and union membership.[5] The estimates of the impact of bank deregulation on union membership in Table 3 are weaker than the estimates in Table 2 by 0.2 percentage points, with the specification in column (3) loosing its statistical significance. The estimates in columns (4)-(6), however, remain statistically significant. The most conservative estimate in column (6) suggest that after excluding workers in airlines, trucking, and railroads, bank deregulation reduces union membership by 1.1 percentage points.

The validity of the identification strategy rests on the assumption 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 have occurred later in states where labor unions had greater influence.

Using a hazard model, I show that preexisting union membership does not explain the timing of deregulation. The results are presented in Table 4. The estimates represent percentage change in the hazard of deregulation as a result of a marginal change in union membership. The hazard of deregulation is a likelihood that a state deregulates at time t, given that it has not yet deregulated. In column (1) union membership is the only variable that explains the timing of bank deregulation. Column (2) adds political-economy factors that had a substantial impact on the timing of bank deregulation (Kroszner and Strahan [1999]). Column (3) adds regional fixed effects that account for the possibility that unobserved regional factors affected the timing of deregulation.[6] All the estimates of the impact of union membership are statistically insignificant, suggesting that there is little evidence that preexisting unions membership shaped the timing of bank branch deregulation.

Figure 2 clarifies the dynamics of the relationship between bank deregulation and

5. See Card [1986], Card [1996], and Cappelli [1985] for the case of airlines, Rose [1987], Belzer [1995], Peoples [1996], and Belman and Monaco [2001] for trucking. The case of railroads is analyzed in MacDonald and Cavalluzzo [1996] and Talley and Schwarz-Miller [1997]. Peoples [1998] reviews the literature.

6. The regions are South, Northeast, Midwest, and West. The definition of the regions is as follows: South contains AL, AR, DC, FL, GA, KY, LA, MS, NC, OK, SC, TN, TX, and VA; Northeast contains CT, MA, MD, ME, NH, NJ, NY,PA, RI, VT, and WV; Midwest contains IA, IL, IN, KS, MI, MN, MO, NE, ND, OH, SD, and WI; West contains the remaining states.

union membership. Specifically, the figure traces changes in union membership for 10 years before deregulation and 10 years after deregulation *relative to the year of deregulation*. The figure plots estimates of $\mu_1 - \mu_{20}$ from,

$$y_{st} = \pi_s + \lambda_t + \mu_1 d_{st}^{-10} + \mu_2 d_{st}^{-9} + ... + \mu_{20} d_{st}^{+10} + u_{st}, \tag{2}$$

where y_{st} is proportion of union members in state s and time t, d_{st}^{-j} equals one for states in the j^{th} year *before* branch deregulation and equals zero otherwise, and d_{st}^{+k} equals one for states in the k^{th} year *after* branch deregulation and equals zero otherwise. The indicator for the year of deregulation is omitted from (2). π_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 (2) 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 2 provides evidence that bank branch deregulation led to a significant union decline rather than vice versa. Moreover, the figure reveals that bank deregulation has a "level" effect on union membership. Immediately after the deregulation, union membership is consistently 1 percentage points lower relative to the year of deregulation.

I argue that the impact of bank deregulation on union membership operates primarily through its impact on firm entry. This impact is illustrated in Figure 3. Specifically, the figure shows estimates of $\mu_1 - \mu_{20}$ from equation (2), where the dependent variable, y_{st}, is the natural logarithm of new incorporations per capita. The results are a graphical replication of the findings in Black and Strahan [2002] and use their data. As shown, bank deregulation has a significant and permanent impact on the number of new incorporations per capita. The next section makes a tighter link between bank deregulation, firm entry, and union membership.

IV.B. Estimates by External Financial Dependence

To provide more evidence on the potential link between bank deregulation and union membership, I use data from Cetorelli and Strahan [2006] on external financial dependence for manufacturing sectors. External financial dependence is the proportion of capital expenditures financed with external funds (Rajan and Zingales [1998]). This measure is constructed for manufacturing Compustat firms between the years 1980 and 1996 that have been in Compustat for at least 10 years. The reason for the 10-year restriction is to capture firms' demand for credit and not the amount of credit supplied to them. It has been widely documented that young firms are financially constrained and their debt is likely to be determined by the amount of credit offered to them (Fazzari, Hubbard and Petersen [1988]).

Estimates of external financial dependence for each manufacturing sector are provided in Table 5. A negative value indicates that the median firm in the indicated sector has free cash flow, whereas a positive value indicates that the median firm must issue debt or equity to finance its investment. Thus, leather and leather products is the least finance-dependent sector, whereas chemicals and allied product is the most finance-dependent. The reason for cross-sector variation in external financial dependence seems to be rooted in the underlying technological processes of the different sectors, as noted in Rajan and Zingales [1998], p. 563: "To the extent that the initial project scale, the gestation period, the cash harvest period, and the requirement for continuing investment differ substantially between industries, [the fact that there is a technological reason why some industries depend more on external finance than others] is indeed plausible."

Cetorelli and Strahan [2006] show that bank deregulation had a significant impact on firm entry in manufacturing sectors with above-median dependence on external finance. If bank deregulation is causally associated with a reduction in union membership, one would expect a reduction in union membership in sectors with above-median dependence on external finance. At the same time, there should be no impact on union membership in sectors with below-median dependence on external finance. Firm entry in these sectors was unaffected by bank deregulation. In fact, the division of manufacturing sectors by external financial dependence tests the causal interpretation of the deregulation-unionism relationship. If there is an unobserved factor that affects union membership and is correlated with the timing of bank deregulation, then it should affect union membership in industries with both high and low dependence on external finance.

Figure 4 traces the dynamics of union membership in a twenty-year window around bank deregulation using the specification in equation (2). The estimates of $\mu_1 - \mu_{20}$ are estimated separately by industries' median dependence on external finance. That is, I estimate equation (2) twice. The hollow circles represent changes in union membership in industries with below-median external financial dependence, whereas dark circles represent industries with external financial dependence above the median. As shown, there are no trends in union membership before deregulation for sectors with above- and below-median external financial dependence. Following deregulation, hollow circles remain flat, indicating little changes in union membership in sectors with below-median financial dependence. Dark circles, on the other hand, trend down after deregulation, indicating a decline in union membership in sectors with external financial dependence above the median.

The dynamics in Figure 4 reveal a potential mechanism through which bank deregulation impacts union membership. Deregulation increased firm entry and reduced firm size in manufacturing sectors with above-median dependence on external finance. Only in these sectors unions declined following bank deregulation. This suggest that unions declined, at least partly, due to entry of new firms and a reduction in the average firm size. Firm entry may impact unions by increasing product market competition, competition for labor, or both. Changes in product market competition will change the elasticity of the demand curve for labor, increasing the wage-employment trade-off. Changes in competition for labor will shift the entire demand curve, raising wages. Reduction in average firm size, in turn, may independently affect union membership if unions primarily target large firms.

IV.C. The Impact of Deregulation on Union Wage Premium

These mechanisms suggest that bank deregulation – through its impact on firm entry and the demand curve for labor – should lead to a reduction in union wage premium. I test this hypothesis in Figure 5. The figure illustrates 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. Specifically, the figure plots the location of union members in the non-union log real conditional wage distribution, before and after bank branch deregulation.[7]

The figure is 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 regressions separately for every year using the following specification,

$$\ln\left(wage_{ist}^{non-union}\right) = \mathbf{x}'_{ist}\boldsymbol{\beta}_t + u_{ist} \tag{3}$$

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 (3),

$$r_{ist} = wage_{ist}^{union} - \mathbf{x}'_{ist}\widehat{\boldsymbol{\beta}}_t \tag{4}$$

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 to the

7. 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.

two-step procedure described in equations (3) and (4). 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 of workers 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 5 demonstrate a significant reduction in union wage premium after bank deregulation across the entire distribution of wages. The median union worker, for example, corresponds to roughly 70th percentile in the non-union workers' wage distribution before deregulation. After deregulation, however, the median union worker falls to approximately 60th percentile.

The reduction in union wage premium following bank deregulation sheds additional light on the mechanisms that drive the relationship between bank deregulation and union membership. The results are also consistent with the notion that unions target less competitive industries (Ashenfelter and Johnson [1972], Lee [1978], Hirsch and Berger [1984]), negotiate wage premium for their members (Segal [1964], Christofides and Oswald [1992]), and that competitive forces erode their bargaining power (Freeman and Katz [1991], Abowd and Lemieux [1993]).

IV.D. Evidence from Panel Study of Income Dynamics

New firm entry may affect union membership by hiring previously unemployed workers and thus alter the composition of workers. To provide more evidence about potential changes in the composition of workers following deregulation, I use the Panel Study of Income Dynamics (PSID).

The advantage of the PSID, among other things, is information about each worker's tenure with the current employer. Examining workers with at least several years of tenure with the current employer allows to exclude new workers that may have joined the workforce following bank deregulation. The PSID thus helps to evaluate the hypothesis that deregulation affects union membership by increasing the pool of new, initially non-unionized workers. Such analysis is not possible with the Current Population Survey, which does not include information about tenure with the current employer.

Table 6 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 6 reports the estimate of θ from the following specification:

$$y_{ist} = \alpha_s + \lambda_t + \theta d_{st} + \mathbf{x}'_{ist}\boldsymbol{\beta} + \varepsilon_{ist} \tag{5}$$

where y_{ist} is union coverage indicator $(0-1)$ of person i who resides in state s in year 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, and $\mathbf{x}_{ist}$ is a vector of personal characteristics that includes years of completed education, tenure with the current employer, and tenure squared. Equation (5) is estimated at the individual level and not at the state–year level due to the relatively small sample.[8]

The specification in (5) 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 experiencing any change in the bank branching laws. The estimation of θ is subject to possibly severe serial correlation problem, which results in inconsistent

8. See Appendix Table 1 for more details about the construction of the sample. Appendix Table 2 lists the variables used in this analysis. I use union coverage and not union membership because it is available more frequently in the PSID.

standard errors (Moulton [1990]). Several factors make serial correlation an especially important issue in the context of DID estimation. First, equation (5) relies on a relatively long time period from 1977 to 1993. Second, union coverage is serially correlated. Lastly, bank branch deregulation indicator changes little within state over time.

When estimating equation (5) 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 et al. [2004]. 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 6 show a significant reduction in union coverage following bank branch deregulation. According to the most conservative estimate in column (3), deregulation reduces union coverage by 2.5 percentage points. The results in columns (1)-(4) hold with or without controlling for workers' personal characteristics and using alternative approaches to calculate the standard errors.

First, it is reassuring that the negative impact of bank deregulation on unionization found in the CPS also holds in the PSID, which is a completely different set of data. Second, the reduction in unionization is evident among workers with at least several years of tenure with the current employer. By focusing on these workers, I am able to evaluate the hypothesis that changes in unionization are driven by addition of new workers to the workforce. The results, however, provide little evidence for this hypothesis. On the contrary. The reduction in union coverage is evident among workers who have been working for the same employer for at least several years. This result sheds further light on the mechanisms underlying bank deregulation-unionization relationship.

IV.E. *The Impact of Deregulation on Union Representation Elections*

Preceding analyses provide evidence that the reduction in union membership following bank deregulation is driven by entry of new, non-financial firms and a reduction in union wage premium. Moreover, evidence from the PSID indicates that "mature" workers exit unions, suggesting that changes in union membership are driven by decertification of unions.

This section tests this hypothesis directly, by analyzing the voting patterns in union representation elections following bank deregulation. The data come from the National Labor Relations Board (NLRB), which includes the universe of establishment-level union elections from 1977 to 1999 and were used in Holmes [2006]. For each election, the data include information about the outcome of the election (union "won" or "lost"), the type of workers that seek union representation, the geographical location of the establishment, the three-digit Standard Industrial Classification code of its industry, and its size.

I construct an indicator which equals to one if a union lost representation election and equals zero otherwise. I regress this indicator on a series of establishment level characteristics that include its industry, type of workers that the union wants to represent (e.g., truck drivers, or guards), establishment size, and a type of election.[9] I then collect the residuals from this regression and average them for every state and year. Then, I estimate the dynamic impact of bank deregulation on state-year proportion of representation elections lost by unions using the specification in equation (2).

Figure 6 illustrates the dynamics of elections lost by unions in a twenty-year window around bank deregulation. During the ten years prior to deregulation, there are no apparent changes in voting patterns relative to the year of deregulation. After

9. Establishment size is a series of indicators for the following brackets of the number of employees: 1-9, 10-19, 20-39, 40-99, 100-199, 200-499, 500+. Type of election is a series of dummy variables for the following types of elections: election ordered by NLRB, election held pursuant to agreement for consent election, expedited election, election ordered by the regional director, or election held pursuant to stipulation for certification upon consent election.

deregulation, however, the proportion of lost elections is consistently going up starting from the third year after bank deregulation. On average, deregulation increases the probability of losing a representation election by 3 percentage points.

The results depicted in Figure 6 illustrate the adverse impact of bank deregulation on union representation elections as workers increasingly vote against the unions. This is consistent with the previous analyses which suggest that deregulation diminished the attractiveness of unions through changes in the union wage premium.

V. Conclusions

This paper proposes a novel explanation for the decline of unions in the U.S. in the last several decades. I argue that relaxation of geographical restrictions on bank branching within state borders has played an important role in the decline of unions in the non-banking sectors of the economy. The key for identifying the impact of bank deregulation on union membership is the cross-state variation in the timing of removal of geographical restrictions on bank branching.

The results indicate that bank branch deregulation has a substantial, first-order impact on union membership, corresponding to about 10% of the overall reduction in union membership between 1953 and 2009. Bank deregulation seems to affect union membership by spurring entry of new firms, reducing union wage premium, and eventually leading to adverse union voting.

The hypothesis that bank deregulation reduces union membership in the overall economy is not fully distinct from the existing explanations of union decline. The channels emphasized in this paper are consistent with previous studies that highlight the importance of competitive forces in explaining changes in union membership (Freeman and Katz [1991], Abowd and Lemieux [1993]). These channels are also consistent with Farber [1987] and Farber and Krueger [1993] who argue that changes in workers' demand for unions play an important role in union decline. The potential contribution of this paper is to highlight a new, previously unexplored trigger and the different channels through which it affected unionism in the United States.

Data Appendix

Union membership information is from May and Outgoing Rotation Groups Current Population Survey (CPS). These data are obtained from the NBER data collection section and Unicon Research, respectively. 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?".

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 $1,337,291$ observations. The first two columns in Appendix Table 1 provide more details on the sample restrictions imposed on the raw CPS files.

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.

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.

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 18,269 observations. The last column in Appendix Table 1 provides more details on the sample restrictions imposed on the original PSID files.

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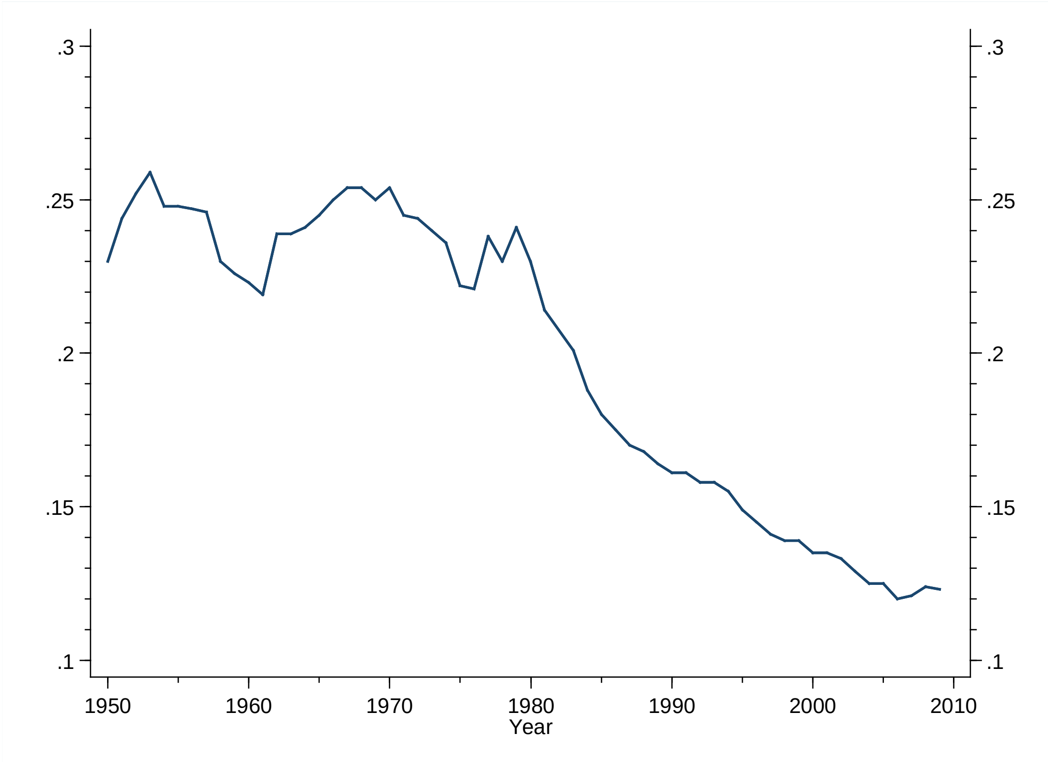

Figure 1 – Percent of Unionized Workers in the United States, 1950-2009

Source: 1950-1972 from Barry T. Hirsch and John T. Addison, *The Economic Analysis of Unions: New Approaches and Evidence*, 1986, p. 47; 1973-2009 from <www.unionstats.com>.

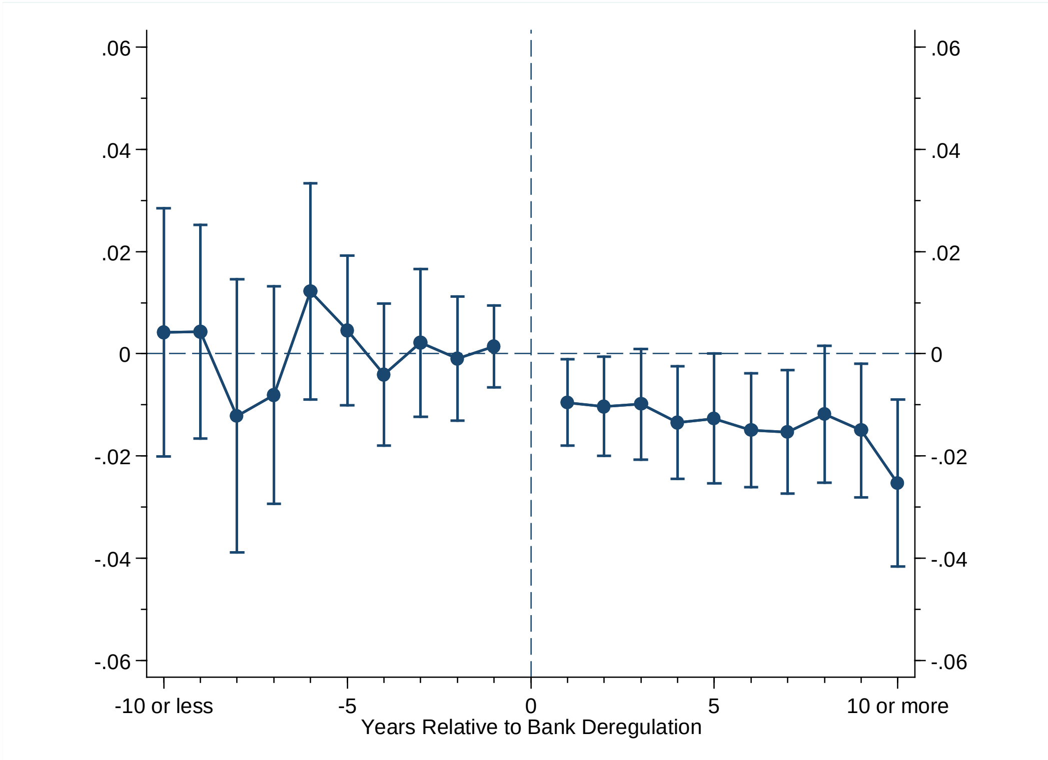

Figure 2 – The Dynamic Impact of Bank Deregulation on Union Membership

Note – The figure traces changes in union membership for 10 years before deregulation and 10 years after deregulation relative to the year of deregulation. Specifically, the figure plots estimates of μ_1- μ_{20} from equation (2). Each circle represents the impact of bank deregulation on union membership relative to the year of deregulation. Vertical lines mark 95% confidence intervals which are adjusted for clustering at the state level. Union membership is calculated from the May and ORG Current Population Surveys for the years 1977-2006, excluding the year 1982 when union status information is not available. The sample includes prime-age (25-54) white men who work for wage and salary in the private sector, excluding workers in agriculture. More details about sample construction are provided in Appendix Table 1. Delaware and South Dakota are dropped because of a large concentration of credit card banks in these states.

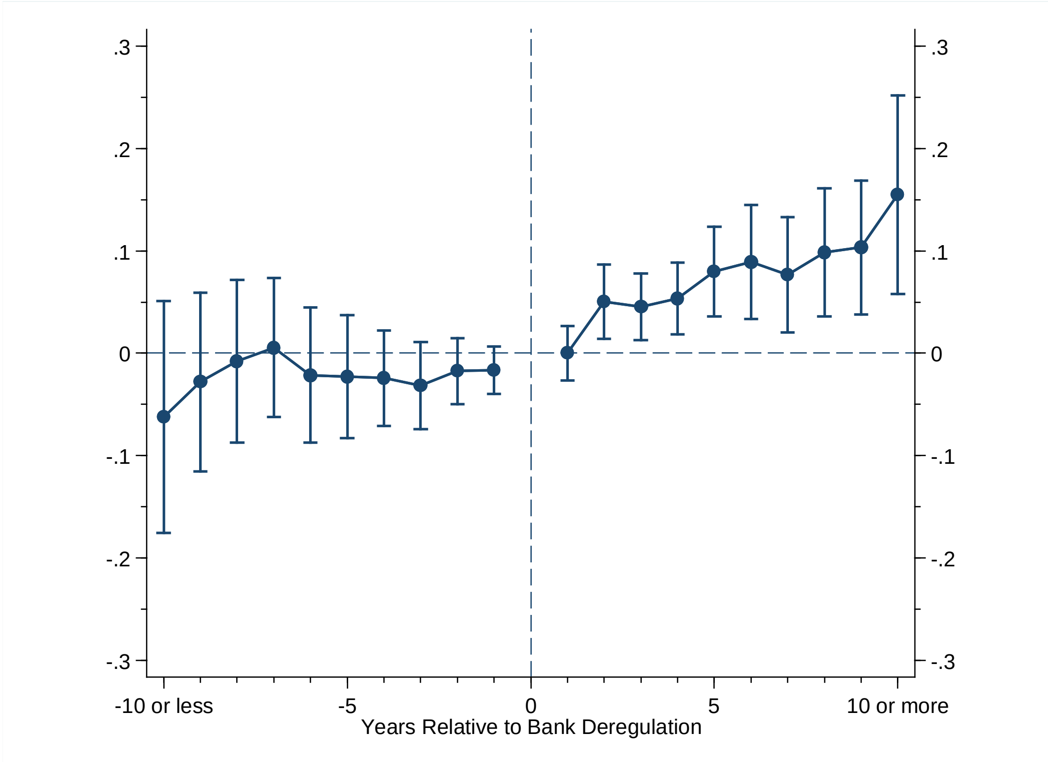

Figure 3 – The Dynamic Impact of Bank Deregulation on Log New Incorporations Per Capita

Note – The figure traces changes in the number of new incorporations per capita for 10 years before deregulation and 10 years after deregulation relative to the year of deregulation. Specifically, the figure plots estimates of μ_1- μ_{20} from equation (2), where the dependent variable, y_{st}, is the natural logarithm of new incorporations per capita. Each circle represents the impact of bank deregulation on the natural logarithm of new incorporations per capita relative to the year of deregulation. Vertical lines mark 95% confidence intervals which are adjusted for clustering at the state level. New incorporations per capita by state and year are from Black and Strahan [2002]. Delaware and South Dakota are dropped because of a large concentration of credit card banks in these states.

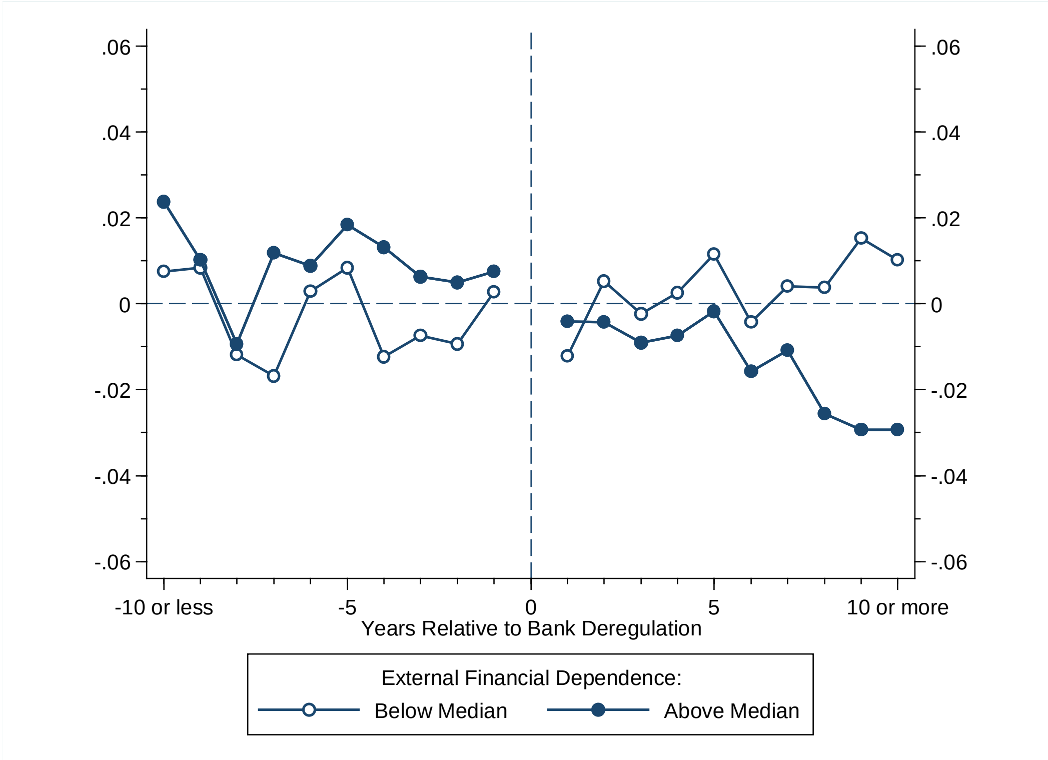

Figure 4 – The Dynamic Impact of Bank Deregulation on Union Membership for Manufacturing Sectors, by External Financial Dependence

Note – The figure traces changes in union membership for 10 years before deregulation and 10 years after deregulation relative to the year of deregulation, separately by external financial dependence. Specifically, the figure plots estimates of μ_1- μ_{20} from equation (2). Equation (2) is estimated twice: once for manufacturing sectors with external financial dependence below the median, and once for sectors with above-median dependence on external finance. External financial dependence for each manufacturing sector is from Cetorelli and Strahan [2006], and is provided in Table 5. Each hollow circle represents the impact of bank deregulation on union membership relative to the year of deregulation for manufacturing sectors with external financial dependence below the median. Full circles represent manufacturing sectors with above-median dependence on external finance. Union membership is calculated from the May and ORG Current Population Surveys for the years 1977-2006, excluding the year 1982 when union status information is not available. The sample includes prime-age (25-54) white men who work for wage and salary in the private sector, excluding workers in agriculture. The sample is further limited to workers in manufacturing. More details about sample construction are provided in Appendix Table 1. Delaware and South Dakota are dropped because of a large concentration of credit card banks in these states.

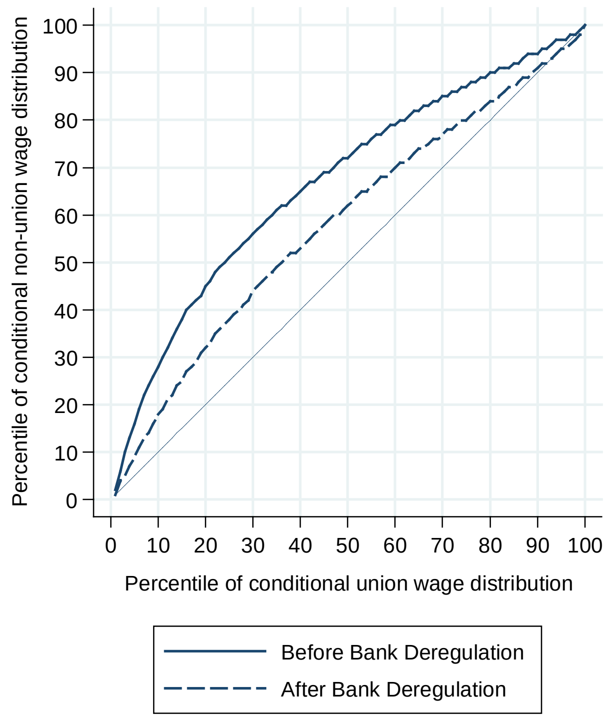

Figure 5 – The Impact of Bank Deregulation on Union Wage Premium

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 are obtained using the following procedure: First, I calculate residuals for unionized and non-unionized workers from equations (3) and (4). 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. Hourly wages are converted to constant 1982 dollars using the Consumer Price Index. In addition to the restrictions listed in Appendix Table 1, I limit the sample to 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.

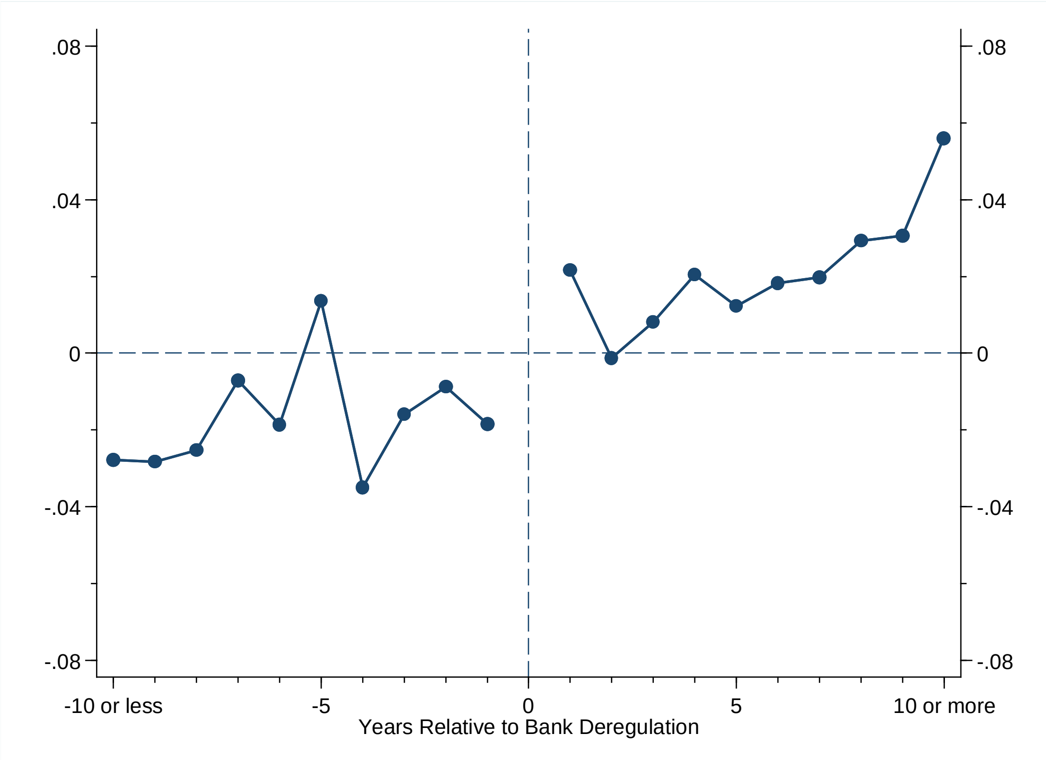

Figure 6 – The Impact of Bank Deregulation on Proportion of Union Representation Elections Lost by Unions

Note – The figure traces changes in voting patterns in union representation elections for 10 years before deregulation and 10 years after bank deregulation relative to the year of deregulation. Specifically, the figure plots estimates of μ_1- μ_{20} from equation (2), where the dependent variable, y_{st}, is a proportion of union representation elections lost by unions in state *s* and year *t*. Each circle represents the impact of bank deregulation on proportion of elections lost by unions relative to the year of deregulation. Voting patterns in union representation elections are from the National Labor Relations Board (NLRB) for the years 1977-1999. I construct an indicator which equals to one if a union lost representation election and equals zero otherwise. Then, I regress this indicator on a series of establishment level characteristics that include its industry, type of workers that the union wants to represent (e.g., truck drivers, or guards), establishment size, and a type of election. Establishment size is a series of indicators for the following brackets of the number of employees: 1-9, 10-19, 20-39, 40-99, 100-199, 200-499, 500+. Type of election is a series of dummy variables for the following types of elections: election ordered by NLRB, election held pursuant to agreement for consent election, expedited election, election ordered by the regional director, or election held pursuant to stipulation for certification upon consent election. I collect the resulting residuals and average them for every state and year.

Table 1 – Timing of Bank Deregulation

State	Year	State	Year
Alabama	1981	Montana	1990
Alaska	1960	Nebraska	1985
Arizona	1960	Nevada	1960
Arkansas	1994	N. Hampshire	1987
California	1960	New Jersey	1977
Colorado	1991	New Mexico	1991
Connecticut	1980	New York	1976
D.C.	1960	North Carolina	1960
Delaware	1960	North Dakota	1987
Florida	1988	Ohio	1979
Georgia	1983	Oklahoma	1988
Hawaii	1986	Oregon	1985
Idaho	1960	Pennsylvania	1982
Illinois	1988	Rhode Island	1960
Indiana	1989	South Carolina	1960
Iowa	1999	South Dakota	1960
Kansas	1987	Tennessee	1985
Kentucky	1990	Texas	1988
Louisiana	1988	Utah	1981
Maine	1975	Vermont	1970
Maryland	1960	Virginia	1978
Massachusetts	1984	Washington	1985
Michigan	1987	West Virginia	1987
Minnesota	1993	Wisconsin	1990
Mississippi	1986	Wyoming	1988
Missouri	1990		

Source: Kroszner and Strahan [1999].

Table 2 – The Impact of Bank Deregulation on Union Membership

	Raw Union Membership			Union Membership Adjusted for Workers' Characteristics		
	(1)	(2)	(3)	(4)	(5)	(6)
Bank deregulation	-.014**	-.012*	-.010*	-.016**	-.015**	-.013**
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
State and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Unemployment rate		Yes	Yes		Yes	Yes
GDP per capita ($2000)		Yes	Yes		Yes	Yes
Right-to-work laws		Yes	Yes		Yes	Yes
Employment protection laws			Yes			Yes
R^2	.93	.93	.93	.92	.92	.93
Number of observations	1,421	1,421	1,344	1,421	1,421	1,344

Note – The dependent variable is proportion of union members in state *s* and year *t*. Branch deregulation indicator equals one during all years in which a state permits in-state branching. Union membership is calculated from the May and ORG Current Population Surveys for the year 1977-2006, excluding the year 1982 when union status information is not available. The sample includes prime-age (25-54) white men who work for wage and salary in the private sector, excluding workers in agriculture. More details about sample construction are provided in Appendix Table 1. Delaware and South Dakota are dropped because of a large concentration of credit card banks in these states. The dependent variable in columns (1)-(3) is a simple proportion of union members in a given state and year, based on union status information of workers sampled by the Current Population Survey. In columns (4)-(6), the dependent variable is the average state-year value of residuals from a worker-level regression of union membership status (0-1) on a series of dummy variables that indicate years of completed education (0-8, 9-11, 12, 13-15, and 16+), potential experience in the labor marker (age - years of completed education - 6), potential experience squared, and industry fixed effects, pooling all years and states. The state-year proportion of union members in columns (1) through (6) are adjusted for sampling weights provided by the Census Bureau. Standard errors are adjusted for clustering at the state level and appear in parentheses. * and ** indicate statistical significance at the 10 and 5 percent, respectively.

Table 3 – The Impact of Bank Deregulation on Union Membership Excluding Airlines, Trucking, and Railroad Industries

	Raw Union Membership			Union Membership Adjusted for Workers' Characteristics		
	(1)	(2)	(3)	(4)	(5)	(6)
Bank deregulation	-.012**	-.010*	-.008	-.015**	-.013**	-.011**
	(.006)	(.006)	(.006)	(.006)	(.006)	(.005)
State and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Unemployment rate		Yes	Yes		Yes	Yes
GDP per capita ($2000)		Yes	Yes		Yes	Yes
Right-to-work laws		Yes	Yes		Yes	Yes
Employment protection laws			Yes			Yes
R^2	.92	.92	.92	.92	.92	.92
Number of observations	1,421	1,421	1,344	1,421	1,421	1,344

Note – The dependent variable is proportion of union members in state *s* and year *t*. This table differs from Table 2 in that it excludes workers in airlines, trucking, and railroad industries. Branch deregulation indicator equals one during all years in which a state permits in-state branching. Union membership is calculated from the May and ORG Current Population Surveys for the year 1977-2006, excluding the year 1982 when union status information is not available. The sample includes prime-age (25-54) white men who work for wage and salary in the private sector, excluding workers in agriculture. More details about sample construction are provided in Appendix Table 1. Delaware and South Dakota are dropped because of a large concentration of credit card banks in these states. The dependent variable in columns (1)-(3) is a simple proportion of union members in a given state and year, based on union status information of workers sampled by the Current Population Survey. In columns (4)-(6), the dependent variable is the average state-year value of residuals from a worker-level regression of union membership status (0-1) on a series of dummy variables that indicate years of completed education (0-8, 9-11, 12, 13-15, and 16+), potential experience in the labor marker (age - years of completed education - 6), potential experience squared, and industry fixed effects, pooling all years and states. The state-year proportion of union members in columns (1) through (6) are adjusted for sampling weights provided by the Census Bureau. Standard errors are adjusted for clustering at the state level and appear in parentheses. * and ** indicate statistical significance at the 10 and 5 percent, respectively. The *p*-value in column (2) is .10.

Table 4 – The of Bank Branch Deregulation and Pre-Existing Union Membership: The Duration Model

	(1)	(2)	(3)
Union membership	-.529	-.600	-.706
	(.635)	(.749)	(.701)
Small bank asset share		Yes	Yes
Small/large bank capital ratio		Yes	Yes
Relative size of insurance		Yes	Yes
Indicator if banks may sell insurance		Yes	Yes
Small firm share		Yes	Yes
Share of Democrats in state gov.		Yes	Yes
Indicator if state controlled by one party		Yes	Yes
Average yield on bank loans minus Fed funds		Yes	Yes
Unit banking law		Yes	Yes
Indicator if state changes bank insurance powers		Yes	Yes
Regional indicators			Yes
Number of observations	270	270	270

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 union membership. Standard errors are adjusted for state-level clustering and appear in parentheses. Union membership is calculated from the May and ORG Current Population Surveys for the year 1977-2006, excluding the year 1982 when union status information is not available. The sample includes prime-age (25-54) white men who work for wage and salary in the private sector, excluding workers in agriculture. More details about sample construction are provided in Appendix Table 1. Union membership is averaged to the state-year level using workers in all industries. Specifications in columns (2)-(4) control for political economy factors that affect the timing of bank branch deregulation (Kroszner and Strahan [1999]). These factors 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 5 – External Financial Dependence for Manufacturing Sectors

Sector	Two-Digit SIC Code	External Financial Dependence
Leather and leather products	31	-0.96
Tobacco manufactures	21	-0.92
Apparel and other textile	23	-0.61
Food and kindred products	20	-0.24
Fabricated metal products	34	-0.24
Furniture and fixtures	25	-0.23
Stone, clay, glass, and concrete products	32	-0.20
Miscellaneous manufacturing	39	-0.20
Printing and publishing	27	-0.07
Instruments and related products	38	-0.04
Industrial machinery and equipment	35	0.01
Transportation equipment	37	0.01
Primary metal industries	33	0.03
Rubber and plastic products	30	0.04
Lumber and wood products	24	0.04
Paper and allied products	26	0.06
Petroleum and coal products	29	0.09
Textile mill products	22	0.10
Electrical and electronic equipment	36	0.22
Chemicals and allied products	28	0.28

Note – The table show measures of external financial dependence for manufacturing sectors. 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 investments. Measures of external financial dependence represent the median value for “mature” Compustat firms over the period 1980-1997. Mature firms are those that have been on Compustat for at least 10 years.
Source – Cetorelli and Strahan [2006].

Table 6 – The Impact of Bank Deregulation on Union Coverage: Evidence from the Panel Study of Income Dynamics

	No Covariates		With Covariates	
	All Workers	3+ Years of Tenure	All Workers	3+ Years of Tenure
	(1)	(2)	(3)	(4)
Bank deregulation	-.028	-.039	-.025	-.035
(clustered s.e.s)	(.018)	(.019)**	(.018)	(.019)*
[block-bootstrapped s.e.s]	[.015]*	[.015]**	[.015]*	[.017]**
Number of observations	18,269	12,764	18,269	12,764

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 households from the "core" PSID sample who work for wage and salary. More details about sample construction are in Appendix Table 1. Appendix Table 2 lists the variables used in the analysis. 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 (3)-(4) also control for years of completed education (0-8, 9-11, 12, 13-15, and 16+), tenure squared, and the interaction terms between 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 3-4) 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.

Appendix Table 1 – Formation of Microdata Samples

	Current Population Survey		
	May Supplement (1977-1981)	Outgoing Rotation Groups (1983-2006)	Panel Study of Income Dynamics (1977-1993)
Total number of observations in the raw data	604,110	11,001,341	671,390
<u>Sample restrictions (observations deleted):</u>			
Head of household	…	…	(543,207)
Prime-age (25-54) in the year of the survey	(315,185)	(6,763,618)	(45,573)
Male	(150,459)	(2,195,285)	(20,866)
White	(14,851)	(280,646)	(20,764)
Works for wages and salary, not in agriculture*	(23,267)	(334,799)	(9,594)
Either working, or with job but not at work	(3,917)	(61,861)	(147)
Non-missing union membership or coverage**	(14,957)	(76,014)	(407)
Non-missing industry	(21)	(0)	(0)
Non-missing state of residence	(0)	(0)	(197)
Non-missing tenure	…	…	(149)
Non-missing years of completed education	…	…	(221)
Not residing in Delaware or South Dakota	(1,578)	(285,844)	(114)
Non-missing and positive sampling weight***	(0)	(3)	(11,882)
Total number of observations that satisfy	79,875	1,257,416	18,269
sample restrictions above	1,337,291		

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.

Appendix Table 2 – Variables Used in the Panel Study of Income Dynamics

Year	Head of Household	Age	Gender	Ethnicity	Tenure	Education
1977	ER30219	V5350	V5351	V5662	V5384	V5647
1978	ER30248	V5850	V5851	V6209	V5941	V6194
1979	ER30285	V6462	V6463	V6802	V6499	V6787
1980	ER30315	V7067	V7068	V7447	V7102	V7433
1981	ER30345	V7658	V7659	V8099	V7711	V8085
1982	ER30375	V8352	V8353	V8723	V8379	V8709
1983	ER30401	V8961	V8962	V9408	V9010	V9395
1984	ER30431	V10419	V10420	V11055	V10519	V11042
1985	ER30465	V11606	V11607	V11938	V11668	V12400
1986	ER30500	V13011	V13012	V13565	V13068	V13640
1987	ER30537	V14114	V14115	V14612	V14166	V14687
1988	ER30572	V15130	V15131	V16086	V15181	V16161
1989	ER30608	V16631	V16632	V17483	V16682	V17545
1990	ER30644	V18049	V18050	V18814	V18120	V18898
1991	ER30691	V19349	V19350	V20114	V19420	V20198
1992	ER30735	V20651	V20652	V21420	V20720	V21504
1993	ER30808	V22406	V22407	V23276	V22489	V23333

Year	Empl. Status	Self Employed	Union Coverage	State of Residence	Sampling Weight
1977	V5373	V5376	V5382	V5203	ER30245
1978	V5872	V5875	V5877	V5703	ER30282
1979	V6492	V6493	V6495	V6303	ER30312
1980	V7095	V7096	V7098	V6903	ER30342
1981	V7706	V7707	V7709	V7503	ER30372
1982	V8374	V8375	V8377	V8203	ER30398
1983	V9005	V9006	V9008	V8803	ER30428
1984	V10453	V10456	V10458	V10003	ER30462
1985	V11637	V11640	V11649	V11103	ER30497
1986	V13046	V13049	V13052	V12503	ER30534
1987	V14146	V14149	V14152	V13703	ER30569
1988	V15154	V15157	V15160	V14803	ER30605
1989	V16655	V16658	V16661	V16303	ER30641
1990	V18093	V18096	V18099	V17703	ER30686
1991	V19393	V19396	V19399	V19003	ER30730
1992	V20693	V20696	V20699	V20303	ER30803
1993	V22448	V22451	V22454	V21603	ER30864