

**Rent-Sharing and Discrimination:
The Effects of Deregulation on the Labor Market**

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7/28
10:00 AM

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Preliminary Draft
May 1999

Abstract: Until the middle of the 1970s, regulations constrained banks' ability to enter new markets. Over the subsequent 25 years, states gradually lifted these restrictions. This paper takes advantage of this deregulation to test whether rents fostered by regulation were shared with labor, and whether firms were discriminating by sharing these rents disproportionately with favored groups. We find that average compensation and average wages for banking employees fell after states deregulated. The decline in wages cannot be explained by changes in observable skills, and we find a larger decline in wages in states that had tighter *ex ante* restrictions on bank expansion. Male wages fell by seven to ten percent after deregulation, while women's wages fell by only two to three percent, suggesting that rents were shared mainly with men. In addition, women's share of employment in managerial positions increased following deregulation, which again suggests that employers discriminated against women prior to deregulation.

We thank Rebecca Demsetz, Fredrick Flyer, Beverly Hirtle, Kevin Stiroh and Joe Tracy for comments and Jennifer Poole for research assistance. The opinions expressed here are those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of New York or the Federal Reserve System.

I. Introduction

Do firms share rents with workers, even in the absence of unions? If they do, are these rents distributed equally across workers or do some groups benefit more than others? The answers to these questions have eluded us, primarily due to the difficulty of isolating rents to workers from the effects of compensating wage differentials, payments to unobserved human capital, and efficiency wages. This paper takes advantage of a unique series of events in recent history: the deregulation of state-level restrictions on bank expansion. We test the effects of this deregulation on the labor market and find evidence that rents were shared with labor when banking competition was limited by regulation and that, prior to deregulation, firms were able to discriminate against women by sharing these rents disproportionately with male workers.

Until the middle of the 1970s, regulations constrained banks' ability to enter new markets, either by opening branches or by owning banks in multiple states. Over the subsequent 25 years, states gradually lifted these restrictions. This particular deregulatory experience is quite different from prominent cases of *national* deregulation such as telecommunications and transportation, where change occurred all at once.¹ From a research standpoint, banking deregulation provides a valuable laboratory to explore the effects of regulations restricting entry. Since states deregulated at different times, we can control for both fixed differences across states as well as trends in wages in our statistical model. Our empirical strategy is based on the idea that state-level restrictions on banks' ability to expand across local markets inhibited competition

¹ For a review of the effects of deregulation in these and other industries, see Winston (1993), Winston (1998), and Peoples (1998).

and allowed the industry to enjoy rents.² Deregulation is tantamount to a shock that reduced these rents and allows us to observe how the labor market was affected.

There has been a trend in banking over the past two decades toward better educated workers. Real average annual compensation in banking rose from about \$30,000 (1997 dollars) to about \$40,000 per year between 1976 and 1996 (Chart 1), in part because a decreasing share of banking employees have a high school education or less (Chart 2). Chart 1 also shows that even after controlling for worker age and education, wages in banking rose during this period. During the 1970s, for example, wages in banking were about average after controlling for observable skills. During the 1980s and 1990s, however, wages of banking workers were four to six percent higher than in other industries, again controlling for observable skills. These movements in earnings and skills underscore the importance of controlling for aggregate trends; since most of the deregulation occurred during the 1980s, one might conclude by looking at Chart 1 that banking deregulation *raised* wages.³

To test whether rents were shared with labor in the regulated environment, we estimate whether compensation and wages fell after deregulation controlling for trends. We find that average compensation and average wages for banking employees fell (or rose less than the trend) after states deregulated restrictions on bank branching. The decline is statistically significant, and our estimate is robust across different datasets and model specifications. The decline in wages can not be explained by changes in observable skills -- we find declines in wages even

² For evidence that restrictions on bank expansion fostered rents in the industry, see Keeley (1990), Hubbard and Palia (1995) and Jayaratne and Strahan (1998).

³ For a detailed discussion of changes in the demand for skills in banking, see Demsetz (1997).

after controlling for age and education. Moreover, observable skills do not change after banking deregulation. We also find a larger decline in wages in states that had tighter *ex ante* restrictions on bank expansion (the unit banking states), providing support for our interpretation of deregulation as a shock to market competitiveness. Since there is no reason to believe that compensating wage differentials, unobserved skills, or efficiency wage premia changed following deregulation, we conclude that the higher wages prior to deregulation reflected rent sharing.

As competition increases, not only do overall rents in the industry decline, but so does the ability of individual employers to distribute these rents according to their own preferences; that is to say, the ability of firms to discriminate against particular groups of workers also declines. According to Becker (1957), increased product market competition should reduce the ability of firms to discriminate. As a result, deregulation should improve the relative position of disfavored groups.

To test this idea, we estimate how rents were allocated between male and female workers in the regulated environment, again by observing how each group of workers' wages changed after deregulation. We focus on the gender-wage gap because there are large numbers of both male and female workers in our dataset. In principle, one could test for discrimination against minorities; we do not do so due to data limitations. We find that male wages fell by seven to ten percent after deregulation, while women's wages fell by only two to three percent. The difference between the change in wages for males and females is statistically significant at the one percent level. Thus, more rents were shared with male workers than with female workers, consistent with Becker's idea that competition reduces firm's ability to "spend" their rents on favored groups. We also find that women's share of employment in managerial positions

increased following deregulation, which again suggests that employers discriminated against women prior to deregulation.

In the next section, we describe the deregulatory experience in banking that we will use to test for rent sharing. We then review theories of rent sharing in labor markets. The statistical methods and results are reported in Section IV, looking first at overall compensation and then more specifically at individual-level data and discrimination. Section V then discusses the implications of our results for wages in non-competitive markets, and Section VI concludes.

II. A Brief History of Bank Regulation

Restrictions on banks' ability to expand within a state through branching were initially imposed by the states in the nineteenth century.⁴ Small and inefficient banks supported these restrictions because they prevented competition from other banks.⁵ Although there was some deregulation of these branching restrictions in the 1930s, most states either prohibited branching altogether (the "unit banking" states) or limited branching until the 1970s, when only twelve states allowed unrestricted statewide branching. Between 1970 and 1994, however, 38 states deregulated their restrictions on branching. While branching was generally restricted, banking companies could expand in some states by forming multi-bank holding companies (BHCs).

In addition to facing restrictions on within-state branching, the Douglas Amendment to the 1956 Bank Holding Company Act prohibited a BHC from acquiring banks outside the state

⁴ Banks also face regulations of pricing and safety and soundness. For an overview, see Jayaratne and Strahan (1999).

⁵ Economides, Hubbard, and Palia (1995) show, for instance, that states with many weakly capitalized small banks supported the 1927 McFadden Act, which gave states the authority to regulate national banks' branching powers.

where it was headquartered unless the target bank's state permitted such acquisitions. Since no state allowed such transactions in 1956, the amendment effectively barred interstate banking organizations. Although states had the option to allow out-of-state BHCs to enter, none exercised that right until 1978, when Maine permitted such transactions. As part of the Garn-St Germain Act, federal legislators in 1982 amended the Bank Holding Company Act to allow failed banks and thrifts to be acquired by any bank holding company, regardless of state laws (see, e.g., Kroszner and Strahan 1996). Many states then entered regional or national reciprocal arrangements whereby their banks could be bought by any other state in the arrangement.

Table 1 chronicles the steps taken by individual states to eliminate geographic restrictions. The first three columns pertain to within-state expansion. The first column presents the year in which each state permitted branching by means of merger and acquisition (M&A) only. The second column reports the year in which each state first permitted unrestricted branching, thereby allowing banks to enter new markets by opening new branches.⁶ In most cases, branching by M&A occurred first, then unrestricted branching deregulation occurred soon thereafter; this time clustering will make it hard for us to isolate the impact of permitting new

⁶ In most cases, the date selected reflects the time at which the state finished the branching deregulation process. In four cases, however, we chose dates earlier than the literal end of the process of deregulation since the remaining restrictions did not appear to impose a meaningful constraint on branching. For instance, in 1982 Pennsylvania began permitting banks to branch in the home office county, in a contiguous county, in a bicontiguous county, or in the counties of Allegheny, Delaware, Montgomery, and Philadelphia. In 1990, Pennsylvania permitted unrestricted branching statewide. In the results presented below, we assume that by 1982 Pennsylvania permitted intrastate branching (despite the fact that the process was not finished until eight years later) since the effect of the 1982 law brought Pennsylvania so close to complete intrastate branch freedom. We follow a similar practice for the states of Ohio, Virginia, and Washington. Our results are not sensitive to the alternative dating of deregulation in these four states. Dates for deregulation are from Amel (1993).

branches.⁷ Column three reports the year in which the state permitted multi-bank holding companies, and column four reports the year in which states entered into an interstate banking agreement with other states.

Several developments contributed to the removal of geographic barriers limiting bank expansion. In the mid-1980s, the Office of the Comptroller of the Currency took advantage of a clause in the 1864 National Bank Act to allow nationally chartered banks to branch freely in those states where savings institutions (S&Ls and savings banks) did not face branching restrictions. The Comptroller's action was instrumental in introducing statewide branching in several southern states. Another impetus behind deregulation may have been the rash of bank and thrift failures in the 1980s, which increased public awareness of the advantages of large, well-diversified banks (Kane 1996).

Kroszner and Strahan (1999) suggest that the emergence of new technologies in both deposit taking and lending may have encouraged the elimination of geographic barriers by changing the nature of banking markets. For instance, the introduction of the automated teller machine in the late 1970s and the development of money market mutual funds increased competitiveness in deposit markets; at the same time, new information technologies diminished the value of the specialized knowledge that long-established local bankers might have had about the risks of borrowers in the community. These changes enhanced the ability of banks to compete in more distant markets. As a result, protected banks' incentive to defend restrictions

⁷ This pattern is consistent with a political-economy interpretation of the deregulatory process. By forcing potential entrants to buy their way into new markets, branching by M&A allows incumbent firms to collect some of the rents generated by the restrictions on entry. For further evidence, see Kroszner and Strahan (1999).

on branching and interstate banking has diminished over time, while expansion-minded banks' desire to see the restrictions fall has increased.

III. Rent Sharing and Discrimination

When government regulations restrict entry and expansion of firms, as in banking prior to the 1970s, firms are able to earn rents, and they can “spend” these rents in a variety of ways. High-rent firms can keep the profits themselves, they can use the rents to pay high salaries to their managers and workers, build marble bathrooms, lead a quiet life, and discriminate in both input and output markets against disfavored groups. Previous research suggests that restrictions on bank expansion through branching fostered rents and that these rents were consumed in part by higher cash profits and in part through inefficiency. Keeley (1990) shows that the capitalized value of cash profits, as measured by bank market-to-book asset ratios, fell following liberalization of branching restrictions. In addition, non-interest costs and loan losses declined following branching deregulation, suggesting that industry efficiency improved (Jayaratne and Strahan 1998).⁸ We are testing here whether this improved efficiency was due, in part, to reduced rent-sharing with workers.

Why would firms choose to share their rents with workers? One answer offered in the literature is that unions force firms to pay higher wages, but unions are all but absent in banking. Other theories emphasize fairness: workers in industries with high profits can “afford” to pay

⁸ Following deregulation, banks entered new markets by buying branches (Amel and Liang 1992), BHCs consolidated their subsidiary banks into branches (McLaughlin 1995), and small and high-cost institutions exited the industry (Calem 1994). Jayaratne and Strahan (1998) find that this restructuring reduced the market share of relatively inefficient banks and improved overall efficiency. As a result of the more competitive environment and greater industry efficiency, they find that bank interest rates on loans. Earlier studies by Berger and Hannan (1987) and Rhoades and Savage (1979) suggest that deposit rates were lower in states with restrictions on branching.

workers higher wages, and workers may be able to extract rents even without unions. Akerlof (1982) and Kahneman, Knetsch and Thaler (1986) both discuss the concept of fairness. Akerlof describes a labor contract as a “gift exchange” where workers give the “gift” of work in excess of the minimum standard and firms give the “gift” of wages in excess of the market wage.⁹ If workers think they are being treated fairly, they will work harder. If profits and rents are high, then it is “fair” for workers to have higher wages. As rents and profits fall due to increased competition, workers may be willing to incur lower wages because that is “fair”. These models are related to efficiency wage models because it may be profit maximizing to offer wages above the market clearing level, although efficiency wage models need not make predictions about the correlation between wages and profits.

Another reason why firms may share rents with workers is a failure of corporate governance. Managers may prefer to pay high wages even if owners do not. Recent evidence by Bertrand and Mullainathan (1998) suggests that managers pay higher wages when corporate control markets function poorly. They show that passage of anti-takeover laws which weakened corporate governance led to increases in wages. For a number of reasons, banking has poor corporate control. First, the takeover market may function relatively poorly due to asymmetric information about the value of the loan portfolio. Second, potential acquirers are limited to other banks or bank holding companies, and interstate banking restrictions further limited the number of potential acquirers to other banking organization operating in the same state as a potential

⁹The behavior is based on social norms and not individual behavior, so all individuals are given the same treatment. Consistent with the gift-exchange hypothesis, Fehr, Gächter and Kirchsteiger (1996) run a series of competitive market experiments and find a stable pattern of noncompensating wage differentials. They conclude that higher industry wages could be based on the idea of reciprocal fairness, where workers who feel they are being treated fairly will increase effort.

target bank. Third, the regulatory approval/disapproval process may also deter some acquirers. Perhaps as a result of these conditions, hostile takeovers that replace management are rare in U.S. banking (Prowse 1997).

As noted in the introduction, our empirical test is motivated by the idea that deregulation constitutes a shock to market competitiveness and hence the availability of rents. This allows us to test whether rents were shared with bank workers during the regulated period by determining what happened to their wages after rents declined following deregulation. Deregulation of branching restrictions provides a clean test because rents declined but the corporate control market was not affected. The effects of interstate banking deregulation on wages may reflect more than changes in rents, however, because corporate control markets became more active after this form of deregulation.¹⁰ As we show below, however, wages do not change after interstate banking. This may be because rents change little after interstate banking or because the effects of interstate banking can not be separated statistically from trends due to the time clustering (see Table 1).

Rents may also be shared disproportionately with favored groups. Becker's (1957) analysis of the economics of discrimination suggests that discrimination raises costs and is therefore difficult to sustain in a competitive market. According to Becker, the wage gap between men and women (assumed to be the disfavored group) of equal skill will decline

¹⁰ Because most states did allow bank holding companies to expand across the state by owning multiple banks, even when branching was restricted, branching deregulation had little effect on corporate control markets. However, evidence suggests that interstate banking deregulation improved corporate control. For example, Shranz (1993) found more takeovers after interstate banking and, as a result, reduced concentration of bank ownership and higher profits. Along similar lines, Hubbard and Palia (1995) found that compensation of senior management in banking became more sensitive to performance after interstate banking deregulation.

following deregulation because increased product market competition reduces rents and thus reduces a firm's ability to consume those rents by discriminating.

Employment practices in the banking industry have been the subject of earlier work on the relationship between labor market discrimination and market power. Ashenfelter and Hannan (1986) looked at a cross-section of markets in the Pennsylvania and New Jersey and found a negative and statistically significant relationship between market concentration and the share of female employment in each bank.¹¹ More recently, Hellerstein, Neumark and Troske (1997) focus on the relationship between profits and female employment across firms with market power, and find that firms that employ relatively more women have higher profits, as the theory predicts.

While both of these studies focus on the static implications of the Becker model (a negative relationship between product market competition and discriminatory practices), a recent study by Black and Brainerd (1999) focuses on the theory's dynamic implications -- that changes in the competitive environment will lead to changes in discriminatory practices. They look at the relationship between market power and discrimination by examining the impact of increased product market competition from international trade on the residual gender wage gap and find that increased competition from trade does increase the relative wages of women in manufacturing industries. Here, we also focus on the dynamic implications of Becker by observing how the gender wage gap changes following a shock to industry competitiveness.

¹¹This study also summarizes the early evidence from other studies on the relationship between employment discrimination and product market power.

IV. Empirical methods and results

Our empirical strategy is to use the deregulation of branching and interstate banking restrictions to identify shocks to the competitiveness of banking markets (that are exogenous to conditions in the labor market) and see how the labor market responded.¹² In our first set of tests, we use data from banks' financial statements to estimate how average compensation changed following deregulation for all workers in banking. Since the skill level of the average employee in banking has been trending upward over the past two decades, we then estimate the impact of deregulation on wages after controlling for worker age and education. This analysis uses a sample of banking employees from the Current Population Survey (CPS). The individual data also allow us to explore whether certain kinds of workers experienced greater changes in wages than others. In particular, we focus on changes in the relative compensation of women following deregulation.

A. Overall compensation in banking

We first consider how deregulation affected the average annual compensation for banking employees. The dependent variable equals the sum of all salaries and benefits paid to banking employees in a given state and year, divided by the number of full-time equivalent workers. These data are reported in the annual *Reports of Income and Condition* (the "Call Reports"). The advantage of this data is that it includes all banking employees; the disadvantage is that we can not control for worker characteristics.

¹² One might object that deregulation is not exogenous to labor market conditions. For example, perhaps labor unions supported regulations restricting competition in banking because rents were shared with workers. If this is the case, we should see deregulation occurring later in states where labor unions have greater influence. However, there is no correlation between unionization in banking (which is very low) or overall unionization rates and the timing of state-level deregulation.

We construct a panel data set containing the average compensation for each state over the 1969 to 1997 period. We use the dates reported in Table 1 to construct two indicator variables: one equal to 1 for states permitting branching by M&A and the other equal to one for states permitting interstate banking. In our basic specification, we use these indicator variables to estimate the effects of the policy changes in the following fixed effects model:

$$w_{st} = \alpha_t + \beta_s + \gamma_1 Branch_{st} + \gamma_2 Bank_{st} + \epsilon_{st}$$

where w_{st} equals the log of average annual compensation in state s at time t ; $Branch_{st}$ is an indicator equal to 1 for states without restrictions on branching by M&A; $Bank_{st}$ is an indicator equal to 1 for states that have entered into an interstate banking agreement. In this specification, β_s measures the state-specific component of compensation in banking; α_t controls for both business cycle effects and long-term trends. We drop Delaware and South Dakota from the analysis because these two states experienced a dramatic expansion in their banking sectors during the 1980s as credit card operations were moved there to take advantage of liberal usury laws. We also drop the year of deregulation. The resulting regression includes 1,336 observations out of a possible 1,421 state-year observations (49 states times 29 years).¹³

Because earlier research suggests that the most significant changes in industry rents occurred following deregulation of branching by M&A, we also estimate two slightly more complicated models to exploit our data more fully. The first accounts for the fact that states relaxed restrictions on within-state expansion in three steps: by permitting the formation of multi-bank holding companies, by permitting branching by M&A, and by permitting unrestricted

¹³ Washington, D.C. is the 51st “state.”

(*de novo*) branching.¹⁴ To account for this heterogeneity, we use a deregulation index equal to 0 if a state permits neither branching by M&A, *de novo* branching nor the formation of MBHCs; otherwise, the index equals the sum of the number of ways banks may expand within state. For example, if a state permits multi-bank holding companies and branching by M&A, the index equals 2.¹⁵

The second adjustment to the simple model accounts for the fact that some states began the period with no branching at all (the unit banking states), while other states merely limited branching to the city of the head office. In this model we interact the branching deregulation variables with an indicator equal to 1 if the state began the period as a unit banking state, as follows:¹⁶

$$w_{st} = \alpha_t + \beta_s + \gamma_1 Branch_{st} + \gamma_2 Bank_{st} + \delta_1 Unit_s Branch_{st} + \epsilon_{st}$$

In the simple model, we estimate that compensation falls by four percent following branching deregulation, but we do not find a statistically significant change following interstate banking deregulation (Table 2, column 1).¹⁷ This basic finding -- that changes following interstate banking are similar in sign but less statistically robust -- is consistent with earlier findings on the effects of banking deregulation on the structure and efficiency of the industry. It

¹⁴ Note that most states permitted MBHCs to operate across the state during our sample period.

¹⁵ This model contains only 1,311 observations since we drop more years associated with deregulation than in the simpler case where we consider only branching by M&A.

¹⁶ We classify states that prohibited branching but permitted banks to establish facilities as unit banking states. The unit banking indicator is one for the following states: CO, AR, FL, IL, IA, KS, MN, MO, MT, NE, ND, OK, TX, WI, WV, and WY.

¹⁷ We have also estimated these regressions using weighted least squares with weights proportional to the number of employees in banking in the state. In this specification, the decline in compensation following branching deregulation is larger and remains statistically significant at the 1 percent level.

may reflect the fact that actual changes to competition and market structure are less important following this type of deregulation, although, as noted above, improvements in corporate control may affect bank managers' ability to share rents with workers. Perhaps more important, most states deregulated their restrictions on interstate banking during a relatively narrow window of time, so it is difficult statistically to separate the effects of time trends from the effects of the deregulation.¹⁸

The model with the deregulation index is quite similar. According to these results, a state permitting unrestricted branching (i.e. one that permits all three means of within-state expansion: multi-bank holding companies, M&A branching, and *de novo* branching) would have compensation 5.4 percent lower (three times the coefficient on the branching index variable) than a state permitting neither branching nor MBHCs.

Consistent with our interpretation of deregulation as a shock to market competitiveness, compensation falls by 9.6 percent when formerly unit banking states deregulated (Table 2, column 3).¹⁹ Unit banking states began the period with much tighter restrictions on competition; the change in wages appears to be much larger when the deregulatory shock is larger. In fact, if we look at each unit banking state separately and compare compensation before and after branching deregulation, we find that 12 of the 15 unit banking states experienced a decline in compensation relative to non-deregulating states; the median decline in relative compensation for

¹⁸ Jayaratne and Strahan (1996) report that state-level economic growth accelerates following branching deregulation. We have estimated the models reported in this paper controlling for state-level economic growth. The coefficient on this variable is not statistically significant in any of the models, and none of our other results change when we include this variable.

¹⁹ Note that since the unit banking indicator does not vary over time, we can not estimate its linear (i.e. non-interactive) effect on compensation in the fixed effects model.

these 15 states was 9.7 percent, very close to the estimated regression coefficient.²⁰ These results make sense if rents are being shared with labor -- wages ought to decline most where deregulation has the biggest effect on competition, and they do.

As a further test for rent sharing, we explore the cross sectional relationship between wages and profits in the regulated and unregulated periods. According to our interpretation, there should be a stronger relationship between measured bank-level profits and compensation in the regulated period because measured profits reflected economic rents then. After deregulation, however, available rents declined, so measured profits largely reflected other factors like good economic conditions, risk, or luck.²¹ Thus, the relationship between compensation and measured profits should flatten after deregulation because less of the variance across banks reflected rents.²² That is, the signal-to-noise ratio in the explanatory variable (economic rents) should be greater during the regulated environment than in the deregulated environment.

We test this hypothesis using the following regression:

$$w_{tj} = \alpha_{st} + \beta_1 \pi_{tj} + \beta_2 \text{Branch}_{st} \pi_{tj} + \epsilon_{tj}$$

where t indexes time; s indexes states; and j indexes banks. This equation can include a set of year effects that vary by state (α_{st}) because we are using bank-level data. Thus, the regression coefficients are driven by differences across banks in the same state and year. The direct effect of deregulation is absorbed by the fixed effects.

²⁰ Among the limited branching states, 14 out of 23 states experienced compensation declines relative to non-deregulators.

²¹ Keeley (1990) showed that risk increased following deregulation because of declines in rents.

²² In fact, Jayaratne and Strahan (1999) show that cross-bank variance in measured profits declines by a little more than 10 percent after branching deregulation.

We use return on equity (ROE), equal to net income divided by book value of equity, to measure profits (π).²³ However, we want to compare the economic magnitude of changes in profits on compensation in different regulatory environments, and we know that the cross-bank variability in ROE declined after deregulation. To account for this problem, we scale each bank's ROE by the cross-bank standard deviation in a given state and year. In an alternative specification, we use an indicator equal to one for banks with above average (median) ROE relative to other banks operating in the same state and year.

As shown in Table 3, the relationship between measured profits and compensation flattened substantially after deregulation. The first specification suggests that a one standard deviation increase in ROE during the regulated period led to a three percent increase in compensation to bank employees. In the deregulated period, however, a standard deviation increase in ROE led to an increase in wages of less than two percent. The indicator variable specification suggests that high-profit banks paid their workers about three percent more than low-profit banks during the regulated years, but during the deregulated years they paid their workers less than one percent more than low-profit banks.

B. Discrimination and Wage Changes at the Employee Level

Until now, we have been considering the average annual compensation per employee in the banking sector. In this section, we use worker-level data to test whether changes in observable skills can explain declines in overall compensation. We then test whether rents were shared disproportionately with male workers.

²³ Since ROE has large positive and negative outliers, the values are trimmed at -10% and +20%. Note that similar results are obtained using return on assets as an alternative profit measure.

Wage declines and skills

If skills changed after deregulation, our measure of the effects of deregulation will combine the effects of skill changes with changes in rent sharing. To try to isolate the effects of rent sharing, we estimate the effect of banking deregulation on *residual* wages of banking employees using the March Demographic Supplement to the CPS data between 1977 and 1996. The CPS provides individual-level data with information on income, education, age, race, and gender.²⁴ Our sample includes individuals aged 18 to 64 who worked full-time in the civilian sector in the year prior to the survey; a “full-time” worker is defined as one who worked at least 30 hours in their usual work week and worked more than 48 weeks in the previous year. Self-employed individuals and individuals working without pay are excluded from the analysis. The wage data refer to real weekly earnings in the previous year in 1982 dollars.²⁵

We modify the basic specifications presented earlier, as follows:

$$wresid_{ist} = \alpha_t + \beta_s + \gamma_1 Branch_{st} + \gamma_2 Bank_{st} + \epsilon_{ist}$$

and

$$wresid_{ist} = \alpha_t + \beta_s + \gamma_1 Branch_{st} + \gamma_2 Bank_{st} + \delta_1 Unit_s Branch_{st} + \epsilon_{ist}$$

where $wresid_{ist}$ is the residual from a wage equation estimated *each year* on *all* individuals in the CPS and which includes education (classified as less than high school, high school, some

²⁴Unfortunately, the CPS does not collect information on actual labor market experience, so we use age as a proxy.

²⁵Consistent with work by Borjas and Ramey (1995) and Katz and Murphy (1992), workers earning less than \$67 in weekly wages in 1982 dollars are dropped from the sample. The wages of workers whose earnings are topcoded are multiplied by 1.45.

college, and college plus), age, age squared, sex, and race; by estimating separately for each year, we allow the returns to age and education to vary over time.²⁶ The equation above is then estimated on the subset of individuals who are employed in the banking sector. *Branch* and *Bank* are defined as above.²⁷

By using individual data, we can remove state-specific trends that are common to all workers in a state and national trends that are specific to banking workers. Our regression results are equivalent to a triple difference (difference-in-difference-in-differences). The difference we calculate is as follows:

$$\left(\begin{array}{c} \text{change in wages} \\ \text{of banking} \\ \text{employees} \end{array} - \begin{array}{c} \text{change in wages} \\ \text{of non-banking} \\ \text{employees} \end{array} \right)_{\substack{\text{state that} \\ \text{deregulated} \\ \text{in time } t}} - \left(\begin{array}{c} \text{change in wages} \\ \text{of banking} \\ \text{employees} \end{array} - \begin{array}{c} \text{change in wages} \\ \text{of non-banking} \\ \text{employees} \end{array} \right)_{\substack{\text{state that} \\ \text{did not deregulate} \\ \text{in time } t}}$$

In this case, we are comparing the change in wages of banking employees after deregulation to the change in wages of all other employees in that particular state, and comparing that to the same change in a state that is not being deregulated at that time. We therefore eliminate any state-specific trends (by comparing the change in wages of banking employees to the change in wages of other workers in the state) and any banking specific trends (by comparing the change in wages in deregulated states to the change in wages in states that are not deregulating at that time).

²⁶Any change in the residual we observe tells us when is happening to wages *net of* changes in observable skills or worker characteristics.

²⁷This two-step procedure is equivalent to a one-step procedure where we include the *branch* and *bank* variables and interaction terms between the state fixed effects and a banking indicator, and between the time fixed effects and a banking indicator.

Another advantage of using individual level data is that we can examine if the increased competition differentially affects different groups of workers. We can thus compare the effects by sex and occupation to see if some workers were more affected by deregulation than others.

As before, our estimates suggest that residual wages in banking fell by about 4.9 percent after branching deregulation (Table 4, column 1). This estimate appears consistent with the estimate of the change in overall earnings from Call Report data. Also consistent with the earlier results, we find that interstate banking had no significant effect on wages. When we use an index of intrastate branching deregulation as above, states that moved from no branching to unrestricted branching experienced a 6.9 percent decline in wages (Table 4, column 2).

When we allow for differential effects of deregulation for states that started the period with unit banking, we again find a larger effect on wages -- wages fell by about 3.5 percent for states that had limited branching but by about 6.4 percent for states that initially permitted only unit banks (Table 4, column 3). Again, this larger decline for unit banking states suggests that they faced even less competitive pressure prior to deregulation. The results using the index of intra-state deregulation (column 4) show a similar pattern; again, interstate banking has no significant effect on wages.

The exact timing of the effects of deregulation is unclear; most states passed legislation before the new laws went into effect. For instance, Maine passed legislation in 1975 permitting interstate banking in 1978. Our indicator variables are based on the law's effective date, not its passage date, so some of the effects of deregulation may be observed before the effective dates if banks anticipate the coming competitive environment. In addition to looking at when the changes in wages occurred, another important issue is whether the changes in wages following

deregulation are persistent.

To explore timing issues, we introduce two additional indicator variables to our model, one equal to one during the five years leading up to branching deregulation, and the second equal to one during the five year window just after branching deregulation goes into effect.²⁸ As Table 5 shows, we find some evidence that anticipated increases in competition affected wages -- the coefficient on the pre-deregulation indicator suggests a modest decline in wages during the five years leading up to branching deregulation that is statistically significant in the specification without the unit banking interaction term. Since the coefficient on the indicator for the five-year window following deregulation is not statistically significant, we conclude that the decline in wages is permanent.

Although we control for observable dimensions of skill to generate residual wages, there also may be concern that unobserved skills are changing. Perhaps banks hire more low-wage, low-skill workers after deregulation and there is really no change in rents to workers. Note that this possibility runs counter to the overall trend towards better-educated workers in banking (see Chart 2). As a further check for the effects of possible changes in skills, we test directly whether observable skills changed after deregulation. If observed skills did not change, it seems implausible that unobserved skills would change after deregulation. Specifically, we look at the average age and the proportion of workers in various skill groups in each state and year. We relate these proportions to our two banking deregulation indicator variables, along with year and state fixed effects. The skill groups are: the share of workers in managerial and professional

²⁸ We focus here on branching deregulation, since we found no changes in wages after interstate banking.

positions as defined in the CPS; the share of workers with a high school education or less; the share of workers with some college; and the share of workers with a college degree.

With the exception of the age variable, the dependent variables represent the proportion of banking workers in each state and year in a particular skill group. Greene (1997) suggests modeling estimates of a population proportion using grouped data with a logit model, where the following expression is assumed to hold:

$$\ln(P_{st}/(1-P_{st})) = \beta'x_{st} + e_{st}$$

P_{st} is an estimate of the population proportion in a skill group in a state-year, and β are the coefficients to be estimated (including both year and state effects).²⁹

While the share of workers with some college falls after branching deregulation, neither the share of high school nor the share of college-educated employees changes significantly after deregulation. Moreover, the average age of banking employees actually *increases* slightly after deregulation (Table 6). Since there is no systematic shift towards younger workers or workers with less education, changing skill composition does not seem to be able to account for the decline in wages.³⁰

Discrimination

²⁹In this model, the error variance is higher when the true population probability is near zero or one, and when there are fewer observed workers in each state and year. To account for this heteroskedasticity, we estimate the coefficients using a two-step, weighted least squares approach. In the first step, a consistent estimate of the coefficients is estimated based on OLS. The second step then re-estimates this model using the square root of the cell size times the product of the predicted population proportion times one minus the predicted population proportion to construct weights. For age, we use the average age for all banking employees in a state-year and weight by the square root of the cell size.

³⁰We also allowed for wages to decline differentially by skill group but found no significant differences across education groups or by age.

Having established that wages fell after deregulation, we next explore who faced the largest wage cuts. In particular, we focus on how deregulation affected male and female workers differentially. The Becker model predicts that product market competition will drive out discrimination, so an exogenous shock to competition through deregulation should lead to an improvement in women's relative labor market position if there were discrimination in the industry.

We first estimate the residual wage equation for female and male banking employees separately. Women's wages fell by 2.5 percent after branching deregulation.³¹ The model with the index of branching deregulation suggests that moving from a completely regulated to a completely unregulated environment reduces female wages by 3.9 percent (Table 7, columns 1 and 2). For male workers, the wage decline is much larger; male wages fell by 9.7 percent after branching deregulation in the simple specification and by 12.6 percent when moving from completely restricted to completely unrestricted branching (Table 7, columns 3 and 4). When we pool the sample and allow the coefficients on deregulation to vary for male and female banking employees, we can see that, prior to deregulation, males were earning somewhere between 3.8 percent and 6.1 percent more than females, and that after deregulation, male wages fell until they were approximately equal to those of females (Table 7, columns 5 and 6).³² This is consistent with Becker's theory.

³¹Note that the first stage of our estimation, when we generate the residual for each person in each year, is the same for all specifications. In Table 6, we first estimate stage two for all women in banking, then all men in banking, and then we pool them.

³²The coefficient of the difference in the pooled regression is slightly different from the actual difference calculated by differencing the coefficients in columns 3 and 1. This is due to the fact that the pooled regression imposes the constraint that the time and state effects be the same for men and women.

A second prediction of Becker's theory is that discriminating employers will hire fewer women. Discriminating employers could also prefer to keep women in lower positions than their skills would warrant. As a second test of the discrimination hypothesis, we therefore look at how the percentage of managerial positions held by women changes after deregulation. If firms were discriminating against women by keeping them in lower paying occupations, then we should see an increase in this percentage as product markets become more competitive after deregulation. As in Table 6, we use the logistic transformation of the share of managerial workers in a state-year that are women as the dependent variable. In fact, the share of managerial positions held by women does increase after deregulation (Table 8, column 1). The increase estimated from the logit model represents an increase in women's share of managerial positions of about four percentage points, or about 10 percent of the unconditional mean. This suggests that discriminating against a class of workers by placing them in low-wage occupations is costly; when competition increased after deregulation, firms were forced to improve the occupational status of women in order to cut costs.

Can the decline in the gender wage gap be fully explained by the shift in occupational status of women? To answer this question, we examine the wage effects of deregulation within occupational categories. If there were only composition effects but no wage effects, then male and female wages within occupational categories ought to change by the same amount following deregulation. Table 8 reports that wages of both men and women in managerial positions fell. Male wages fell by about seven percent and female wages fell by about five percent, but the difference is not statistically significant. Among low-skilled workers, however, wages for

women did not fall, while wages for men fell by about 14 percent.³³

These results suggest that women's improved relative wages are due to both improvements in occupational status and improvements in relative wages. Among lower skilled employees, men's wages fell sharply after deregulation while women's wages were essentially unchanged; employers seemed to share rents with low-skilled men but not with low-skilled women. In contrast, managers and professionals appeared to earn rents in the regulated environment regardless of sex, but women were being kept out of those occupations. After deregulation, some women moved up to managerial positions; those who stayed in lower-skilled occupations saw an improvement in their relative wages.

V. Implications for Wages in Non-Competitive Markets

One longstanding puzzle in studies of labor markets is the persistence of industry wage differentials. Explanations include compensating wage differentials, unobserved skill differentials, efficiency wages, and rent sharing.

Industry wage differentials can be driven by compensating wage differentials and unobserved skill differentials even when both product and labor markets are competitive. In the case of compensating wage differentials, some industries may have particularly distasteful jobs (garbage haulers) or very risky jobs (bridge builders) that pay more to compensate workers for the disutility of the job; the marginal worker is indifferent between the distasteful job at a high

³³One concern might be that we are picking up changes in the occupational structure at a finer level than the broad classification we used above (high skill versus low skill). To test this, we limited our sample of banking employees to bank tellers, a very narrowly defined occupation. Consistent with the results presented above, we find that among tellers, men's wages fell substantially after deregulation (almost 19%) while women's wages do not change. This is consistent with the explanation that within occupation categories of low-skilled workers, women's relative wages are improving.

wage and a more pleasant job at a lower wage. Some industries may also attract “better” workers; if some dimensions of skills are unobservable (to the economist), we may estimate persistent industry wage effects.

Efficiency wage models can also generate inter-industry wage effects in competitive product markets, although the labor market may not clear. Firms may pay higher wages than workers’ outside offer to increase worker effort. In these models, profit-maximizing firms pay high wages to raise the cost of losing the job and thus motivate hard work. The efficiency wage must therefore exceed the market clearing wage (see Akerlof and Yellen, 1986).

What do our results suggest about theories of wage setting? We observe that increased competition following banking deregulation is associated with lower wages. Compensating differentials could only explain this wage decline if jobs in banking became more distasteful after deregulation, which seems unlikely. For similar reasons, it seems unlikely that efficiency wage effects could be an important explanation, again because the nature of the kinds of jobs and tasks performed by banking employees was not affected by branching deregulation.³⁴ Moreover, as noted above, since observable skills did not change, we doubt that changes in unobserved skills can explain the wage cuts.

This leaves us with a rent sharing interpretation for our results: wages fell because rents in the industry fell. One interesting aspect of our results is that rent sharing seems to have occurred even without strong labor unions. Unions are all but absent in banking, in contrast to

³⁴One might argue that deregulation was associated with an increase in the monitoring of employees, which would lead to lower wages (in an efficiency wage argument). However, the data suggest that this was not the case; as seen in Table 5, the share of workers in managerial positions did not increase after deregulation, suggesting no apparent increase in the supervision of the workers.

other deregulated industries such as trucking, airlines, telecommunications and railroads.

Previous studies of the effects of deregulation emphasized the importance of unions in forcing the protected industry to pay rents to workers.³⁵ Our results suggest that rent sharing can occur even in their absence. This result is consistent with research by Katz and Summers (1989), who argue that the presence of unions post-dates industry rent sharing. Finally, the decline in discrimination also supports the rent-sharing hypothesis; as industry rents go down, firms are unable to favor particular groups of workers by paying them higher wages.

VI. Conclusions

It is difficult to isolate the effects of product market competition on the labor market. Banking deregulation provides a unique opportunity to explore these effects both because deregulation had important effects on market competitiveness and because deregulation occurred on a state-by-state basis at different times over the last two decades. Using the both Call Report data and CPS data, we find that increased competition from deregulation reduced wages in the industry by approximately five percent. Since neither skills nor the nature of jobs in banking changed after deregulation, we argue that a decline in rent sharing with the workers explains the result. As further support for this interpretation, we find that when rents declined more following deregulation in states that permitted no branching, wages declined more as well.

Increased product market competition also reduces firms' ability to allocate rents to different groups; that is, the ability to discriminate declines as rents decline. We find that the gap between men's and women's wages declines after deregulation. The decline occurred because of women's improved occupational status after deregulation, as well as because low-skilled male

³⁵ See, for example Rose (1987) and Peoples (1998).

workers' wages fell much more than low-skilled female workers' wages.

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Chart 1
Average Annual Earnings and Residual Log Wages in the Banking Industry, 1976-1996

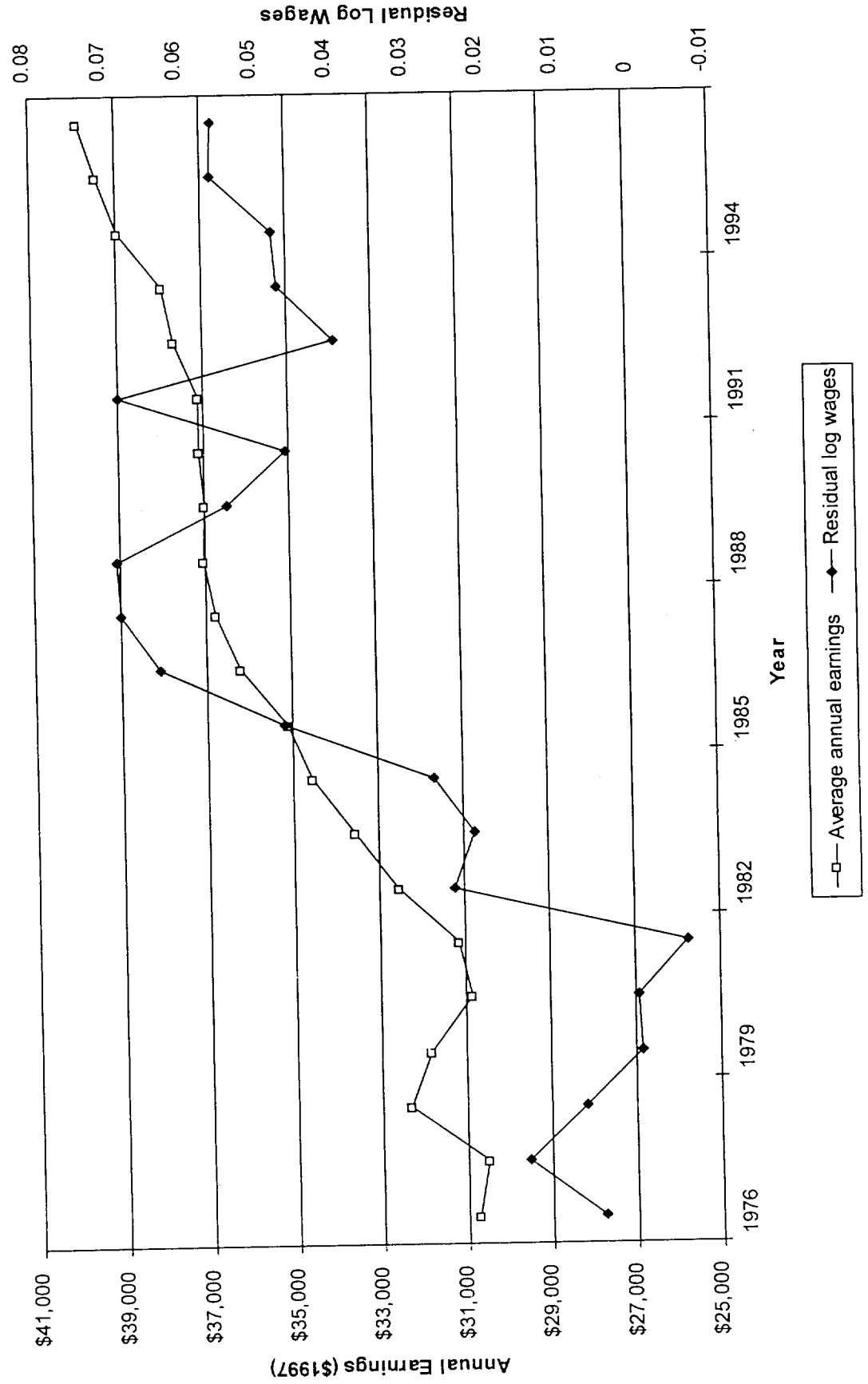


Chart 2
Share of Banking Industry Employees with a High School Education or Less,
1976-1993

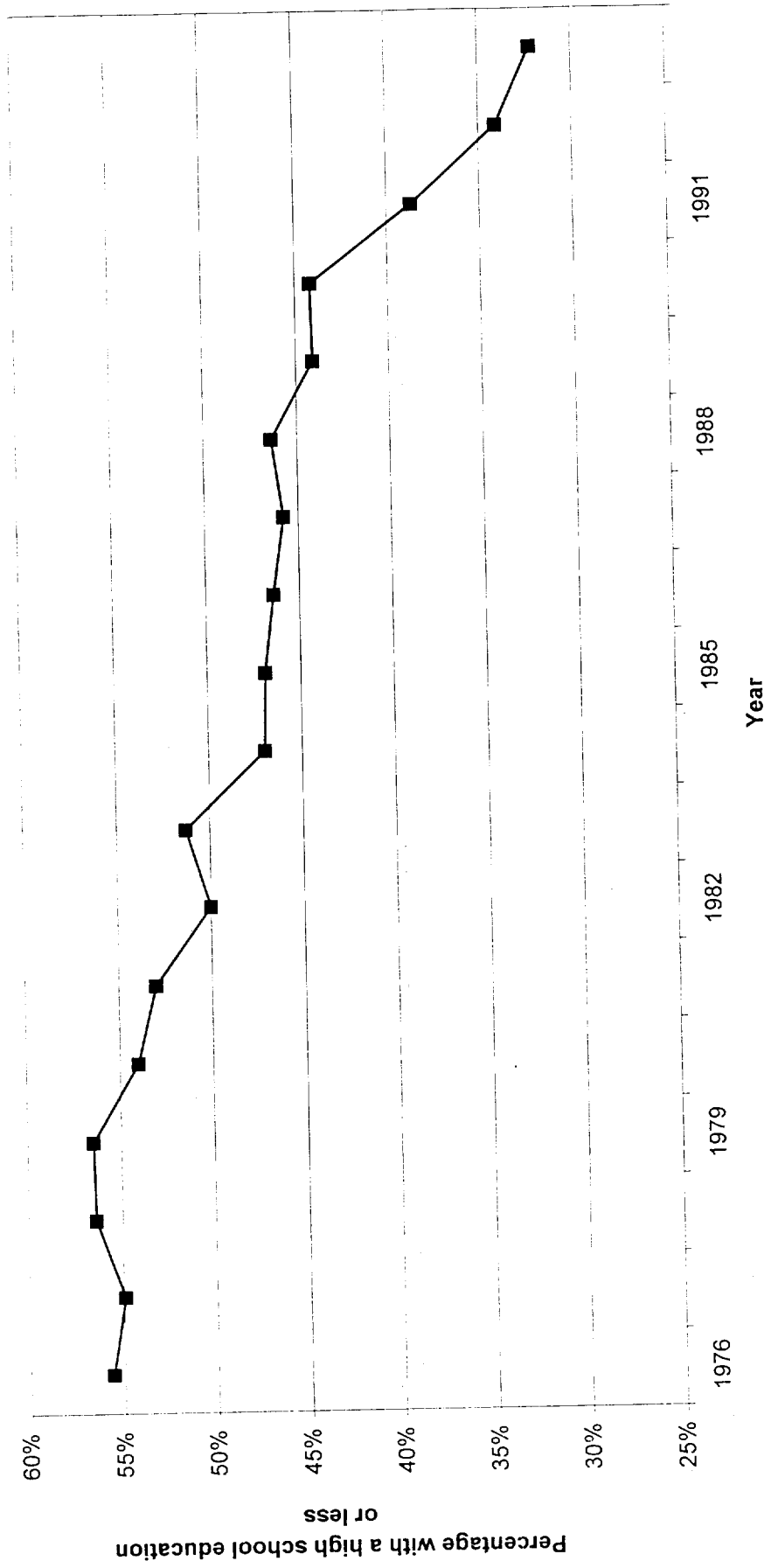


Table 1
Year of Deregulation of Restrictions on Geographical Expansion, by State

State	Intrastate branching via M&A	Unrestricted intrastate branching permitted	Multi-bank holding companies permitted	Interstate banking permitted
Alabama	1981	1990	<1970	1987
Alaska	<1970	<1970	<1970	1982
Arizona	<1970	<1970	<1970	1986
Arkansas	1994	**	1985	1989
California	<1970	<1970	<1970	1987
Colorado	1991	**	<1970	1988
Connecticut	1980	1988	<1970	1983
Delaware	<1970	<1970	<1970	1988
DC	<1970	<1970	<1970	1985
Florida	1988	1988	<1970	1985
Georgia	1983	**	1976	1985
Hawaii	1986	1986	<1970	**
Idaho	<1970	<1970	<1970	1985
Illinois	1988	1993	1982	1986
Indiana	1989	1991	1985	1986
Iowa	**	**	1984	1991
Kansas	1987	1990	1985	1992
Kentucky	1990	**	1984	1984
Louisiana	1988	1988	1985	1987
Maine	1975	1975	<1970	1978
Maryland	<1970	<1970	<1970	1985
Massachusetts	1984	1984	<1970	1983
Michigan	1987	1988	1971	1986
Minnesota	1993	**	<1970	1986
Mississippi	1986	1989	1990	1988
Missouri	1990	1990	<1970	1986
Montana	1990	**	<1970	1993
Nebraska	1985	**	1983	1990
Nevada	<1970	<1970	<1970	1985
New Hampshire	1987	1987	<1970	1987
New Jersey	1977	**	<1970	1986
New Mexico	1991	1991	<1970	1989
New York	1976	1976	1976	1982
North Carolina	<1970	<1970	<1970	1985
North Dakota	1987	**	<1970	1991
Ohio	1979	1989	<1970	1985
Oklahoma	1988	**	1983	1987
Oregon	1985	1985	<1970	1986
Pennsylvania	1982	1990	1982	1986
Rhode Island	<1970	<1970	<1970	1984
South Carolina	<1970	<1970	<1970	1986
South Dakota	<1970	<1970	<1970	1988
Tennessee	1985	1990	<1970	1985
Texas	1988	1988	1970	1987
Utah	1981	1981	<1970	1984
Vermont	1970	1970	<1970	1988
Virginia	1978	1987	<1970	1985
Washington	1985	1985	1981	1987
West Virginia	1987	1987	1982	1988
Wisconsin	1990	1990	<1970	1987
Wyoming	1988	**	<1970	1987

Table 2
Panel Regression Relating the Log of Total Earnings for Banking Employees
to Deregulation Indicators, Time Effects and State Effects
(Standard errors in Parenthesis)

	Simple specifications		Specifications with Unit Banking Interactions
Post-M&A Branching Deregulation	-0.040* (0.007)	-	-0.010 (0.007)
Post-Interstate Banking	-0.003 (0.010)	-0.005 (0.010)	-0.012 (0.010)
Branching Deregulation Index	-	-0.018* (0.003)	-0.006* (0.004)
Unit Banking * Post-M&A Branching Deregulation	-	-	-0.086* (0.009)
Unit Branching Deregulation Index	-	-	-0.043* (0.005)
N	1,336	1,311	1,336
R ² (Within)	0.67	0.67	0.69

The dependent variable equals the log of average annual earnings for full time equivalents at all banks operating in a given state in a given year, from year-end *Reports of Income and Condition*, 1969 to 1997. The model is estimated using a fixed effects model with both year and state effects. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped. The deregulation index equals 0 if a state permits neither branching via M&A, de novo branching nor the formation of multi-bank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multi-bank holding companies and branching via M&A, the index equals 2. The unit banking interaction equals 1 if the state began the sample with a complete prohibition on branching.

*Statistically significant at the 10 percent level.

Table 3
Panel Regression Relating the Log of Total Earnings for Banking Employees
to Profits in Regulated and Unregulated Environments
(Standard errors in Parenthesis)

	Continuous Profit Measure	Discrete Profit Measure
ROE scaled by cross-bank standard deviation	0.031* (0.001)	-
Post-M&A Branching Deregulation * ROE	-0.013* (0.001)	-
Above-median ROE indicator	-	0.030* (0.001)
Post-M&A Branching Deregulation * Above-median ROE indicator	-	-0.025* (0.002)
N	251,729	251,729
R ² (Within)	0.010	0.003

The unit of observation in these regression is the bank. The dependent variable equals the log of average annual earnings for full time equivalents at each bank, from year-end *Reports of Income and Condition*, 1975 to 1997. The model is estimated using a fixed effects model with year effects that vary by state. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped.

*Statistically significant at the 10 percent level.

Table 4
Panel Regression Relating the Log of Residual Earnings for Banking Employees
to Deregulation Indicators, Time Effects and State Effects
(Standard errors in Parenthesis)

	Simple Specifications		Specifications with Unit Banking Interactions	
Post-M&A Branching Deregulation	-0.049*	-	-0.035*	-
	(0.010)		(0.013)	
Post-Interstate Banking	-0.003	-0.005	-0.004	-0.005
	(0.014)	(0.014)	(0.014)	(0.014)
Branching Deregulation Index	-	-0.023*	-	-0.015*
		(0.005)		(0.006)
Unit Banking * Post-M&A Branching Deregulation	-	-	-0.029*	-
			(0.015)	
Unit Branching Deregulation Index	-	-	-	-0.019*
				(0.007)
N	23,809	23,260	23,809	23,260
R ² (Within)	0.005	0.005	0.005	0.005

The dependent variable equals the residual log wage for all banking employees in the March Current Population Survey (CPS). The log wage equation is estimated separately each year, and includes state effects, age, age squared, education and a race indicator. The sample includes data from 1977 to 1997. All models have year and state fixed effects. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped. The deregulation index equals 0 if a state permits neither branching via M&A, de novo branching nor the formation of multi-bank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multi-bank holding companies and branching via M&A, the index equals 2. The unit banking interaction equals 1 if the state began the sample with a complete prohibition on branching.

*Statistically significant at the 10 percent level.

Table 5
Panel Regression Relating the Log of Residual Earnings for Banking Employees
to Deregulation Indicators, Time Effects and State Effects
With Five-Year Pre- and Post-Deregulation Indicators
(Standard errors in Parenthesis)

	Simple Specifications	Specifications with Unit Banking Interactions
Post-M&A Branching Deregulation	-0.056* (0.017)	-0.044* (0.019)
Post-Interstate Banking	-0.007 (0.014)	-0.007 (0.014)
Five-Years before M&A Branching Deregulation	-0.020* (0.012)	-0.016 (0.012)
Five-Years after M&A Branching Deregulation	-0.007 (0.010)	-0.0006 (0.010)
Unit Banking * Post-M&A Branching Deregulation	-	-0.022 (0.016)
N	23,809	23,809
R ² (Within)	0.005	0.005

The dependent variable equals the residual log wage for all banking employees in the March Current Population Survey (CPS). The log wage equation is estimated separately each year, and includes state effects, age, age squared, education and a race indicator. The sample includes data from 1977 to 1997. All models have year and state fixed effects. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped. The deregulation index equals 0 if a state permits neither branching via M&A, de novo branching nor the formation of multi-bank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multi-bank holding companies and branching via M&A, the index equals 2. The unit banking interaction equals 1 if the state began the sample with a complete prohibition on branching.

*Statistically significant at the 10 percent level.

Table 6
Panel Regression Relating Measures of Average Skills of Banking Employees
to Deregulation Indicators, Time Effects and State Effects
(Standard errors in Parenthesis)

	Logit of Share of Workers in Managerial Positions	Average Employee Age (in Years)	Logit of Share of Workers with High School or Less	Logit of Share of Workers with Some College	Logit of Share of Workers with College Degree
Post-M&A Branching Deregulation	0.029 (0.028)	0.572* (0.318)	0.028 (0.031)	-0.108* (0.042)	0.047 (0.051)
Post-Interstate Banking	-0.035 (0.037)	-0.195 (0.422)	-0.030 (0.041)	0.053 (0.055)	0.035 (0.067)
N	947	947	947	947	947
Adjusted R ²	0.14	0.17	0.32	0.18	0.23

Each dependent variable is based upon the average value observed for all banking employees in the March CPS during a given state and year. For each variable except age, the dependent variable is the logistic transformation of the observed share of workers of each type; the regressions are estimated by WLS, where the weights are equal to the square root of the number of employees (i.e. the cell size) times the product of the predicted share times one minus the predicted share from a first-stage OLS model (see Greene 1993 for details). The regression of average employee age is simply weighted by the square root of the cell size. The sample includes data from 1977 to 1997. All models have year and state fixed effects. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped.

*Statistically significant at the 10 percent level.

Table 7
Panel Regression Relating the Log of Residual Earnings for Male and Female Banking Employees to Deregulation Indicators, Time Effects and State Effects
(Standard errors in Parenthesis)

	Females Only	Males Only	All Banking Employees
Post-M&A Branching Deregulation	-0.025* (0.011)	-0.097* (0.021)	-0.039* (0.011)
Post-Interstate Banking	0.001 (0.015)	-0.018 (0.028)	-0.003 (0.014)
Branching Deregulation Index	-0.013* (0.006)	-0.042* (0.011)	-0.017* (0.006)
Male Indicator	-	-	0.038* (0.010)
Male Indicator*Post-M&A Branching Deregulation	-	-	-0.032* (0.012)
Male Indicator*Branching Deregulation Index	-	-	-0.021* (0.006)
N	16,316	15,920	7,493
R ² (Within)	0.003	0.003	0.005
			0.01
			23,809
			23,260
			0.006

The dependent variable equals the residual log wage for all banking employees in the March Current Population Survey (CPS). The log wage equation is estimated separately each year, and includes state effects, age, age squared, education and a race indicator. The sample includes data from 1977 to 1997. All models have year and state fixed effects. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped. The deregulation index equals 0 if a state permits neither branching via M&A, de novo branching nor the formation of multi-bank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multi-bank holding companies and branching via M&A, the index equals 2. The unit banking interaction equals 1 if the state began the sample with a complete prohibition on branching.

*Statistically significant at the 10 percent level.

Table 8
Panel Regression Relating the Share of Women and Wages of Managerial and non-Managerial Workers to Deregulation Indicators, Time Effects and State Effects
(Standard errors in Parenthesis)

	<i>Female Residual Wages:</i>			<i>Male Residual Wages:</i>	
	Logit of Share of Managerial Positions held by Women (State-level Aggregate)	Managerial Workers	Non-Managerial Workers	Managerial Workers	Non-Managerial Workers
Post-M&A Branching Deregulation	0.169* (0.081)	-0.050* (0.022)	-0.015 (0.012)	-0.072* (0.024)	-0.140* (0.039)
Post-Interstate Banking	-0.061 (0.109)	0.020 (0.030)	0.003 (0.017)	-0.010 (0.031)	-0.021 (0.048)
N	944	4,611	11,705	5,312	2,181
R ²	0.11	0.01	0.003	0.02	0.03

Dependent variable for the share of managerial positions held by women is based upon the average value observed for all banking employees in the March CPS during a given state and year. We use the logistic transformation for this variable; the regressions are weighted by the square root of number of employees (i.e. the cell size) times the product of the predicted share times one minus the predicted share from a first-stage OLS model (see Greene 1993 for details). The results based on residual wages are based on individual banking employee data, as in Tables 4 and 6. The sample includes data from 1977 to 1997. All models have year and state fixed effects. The year of deregulation is dropped. Also, South Dakota and Delaware are dropped. For the regression in column 1, we report the adjusted R² from the full regression; for the other models, we report the R² from the “within” model.

*Statistically significant at the 10 percent level.