The Division of Spoils: Rent-Sharing and Discrimination in a Regulated Industry

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Until the middle of the 1970's, regulations constrained banks' ability to enter new markets. Over the subsequent 25 years, states gradually lifted these restrictions. This paper tests whether rents fostered by regulation were shared with labor, and whether firms were discriminating by sharing these rents disproportionately with male workers. We find that average compensation and average wages for banking employees fell after states deregulated. Male wages fell by about 12 percent after deregulation, whereas women's wages fell by only 3 percent, suggesting that rents were shared mainly with men. Women's share of employment in managerial positions also increased following deregulation. (*JEL* G2, J3, J7, L5)

How are rents divided between owners and workers? Do firms share their rents with workers, even in the absence of unions? If so, are these rents distributed equally across workers, or do some groups benefit more than others? The answers to these questions have eluded us, primarily as a result of the difficulty of isolating rents paid to workers from 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 effect of this deregulation on the labor market and find 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.

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Until the middle of the 1970's, 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] In fact, national statistics can be quite deceptive; over the past two decades, real average annual compensation in banking rose from about $30,000 (1997 dollars) to about $40,000 per year between 1976 and 1996 (Figure 1), in part because a decreasing share of banking employees have a high-school education or less (Figure 2).[2] Figure 1 also shows that even after controlling for worker age and education, wages in banking rose during this period. These movements in earnings and skills underscore the importance of controlling for aggregate trends; because most of the deregulation occurred during the 1980's, one might conclude from Figure 1 that banking deregulation *raised* wages. From a research standpoint, banking deregulation provides a valuable laboratory to explore the effects of regulation on rent-sharing; because

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

[2] For a detailed discussion of changes in the demand for skills in banking, see Rebecca S. Demsetz (1997).

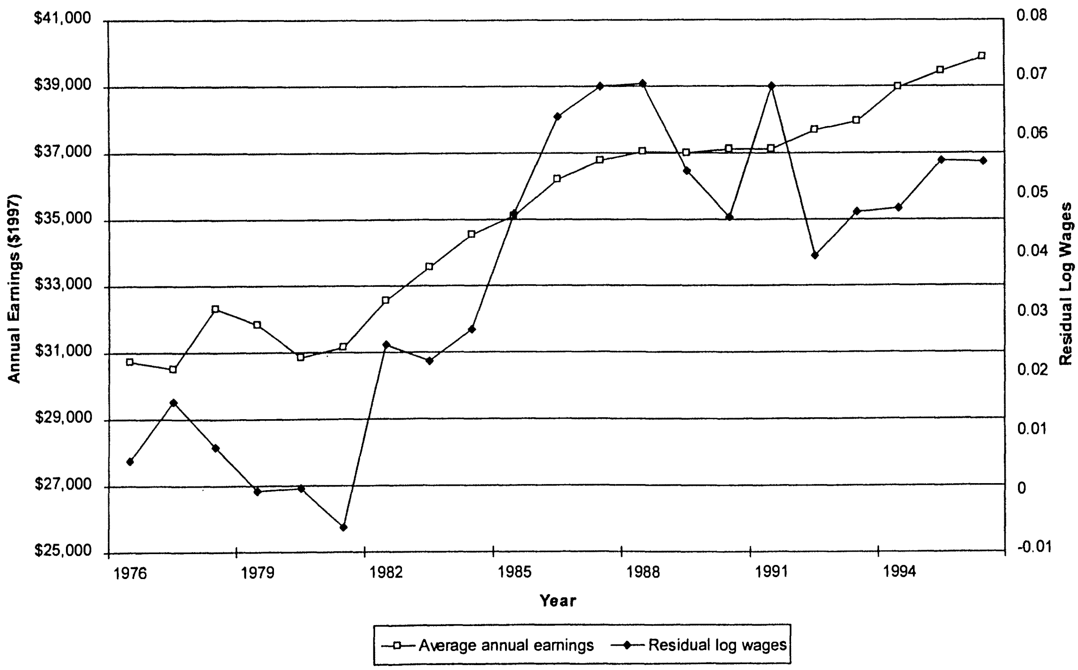

FIGURE 1. AVERAGE ANNUAL EARNINGS AND RESIDUAL LOG WAGES IN THE BANKING INDUSTRY, 1976–1996

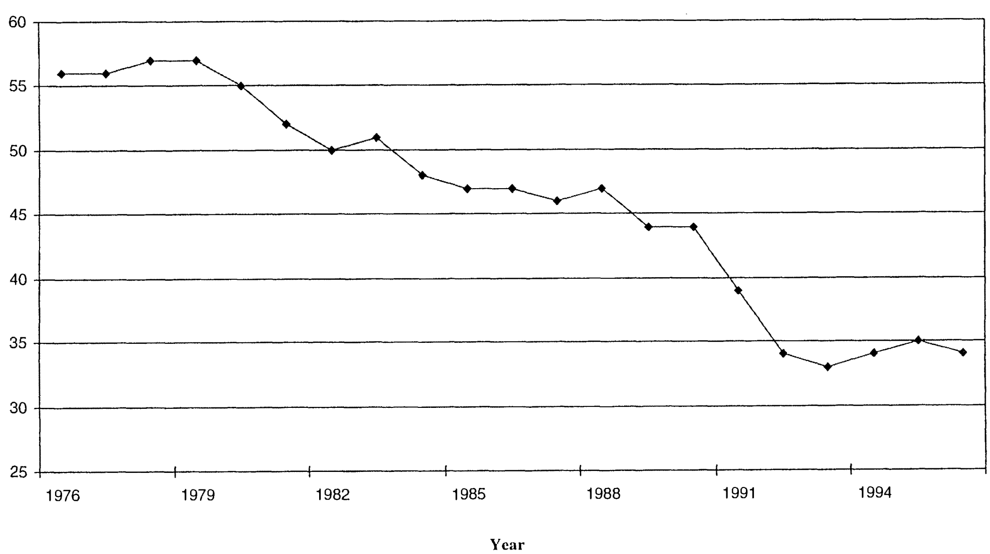

FIGURE 2. PERCENTAGE OF BANKING EMPLOYEES WITH A HIGH-SCHOOL EDUCATION OR LESS, 1976–1996

states deregulated at different times, we can control for both fixed differences across states as well as trends in wages in our statistical model.

Why would firms choose to share their rents with workers? Early significant work by Armen A. Alchian and Rubin A. Kessel (1962) suggests that profit constraints, either explicit or implicit in an industry, could lead to rent-sharing. Another answer offered in the literature is that unions force firms to pay higher wages, although unions are all but absent in banking. Other theories emphasize fairness: workers in industries with high profits can "afford" to pay workers higher wages, and workers may be able to extract rents even without unions. Firms may also share rents with workers because of a failure of corporate governance; managers may prefer to pay high wages even if owners do not.[3]

Our empirical strategy is based on the idea that state-level restrictions on banks' ability to expand across local markets inhibited competition and allowed the industry to enjoy rents.[4] Deregulation is tantamount to a shock to market competitiveness that reduced these rents and allows us to observe how the labor market was affected. To test for rent-sharing, we estimate whether compensation and wages in banking fell after deregulation (controlling for trends). We find that they did, and that the decline cannot be explained by demand shifts or changes in observable skills. 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.[5]

As competition increases, not only do overall rents in an industry decline, but so does the ability of individual employers to distribute those rents according to their own preferences; that is, the ability of firms to discriminate also declines. Gary S. Becker (1957) argued that discrimination raises costs and is therefore difficult to sustain in a competitive market. An earlier study of banking by Orley Ashenfelter and Timothy Hannan (1986) looked at a cross section of markets in Pennsylvania and New Jersey and found a negative and statistically significant relationship between market concentration and the share of female employment in each bank.[6] A more recent study by Black and Elizabeth Brainerd (1999) focused on the theory's dynamic implications—that changes in the competitive environment will lead to changes in discriminatory practices—and found that increased product market competition from international trade increased the relative wages of women in manufacturing industries.

Here, we also focus on the dynamic implications of Becker's model by observing how the gender wage gap in banking changed following deregulation. We find that male wages fell by about 12 percent after deregulation, whereas women's wages fell by only 3 percent. The difference between these changes is statistically significant at the 1-percent level. Thus, prior to deregulation, rents were shared disproportionately with male workers. We also find that women's share of employment in managerial positions increased following deregulation.

In the next section, we describe the deregulatory experience in banking. Our statistical methods and results are reported in Section II, looking first at overall compensation, then more specifically at individual-level data and discrimination, and finishing with robustness checks to test for demand shocks, changes in workers skills, and timing. Section III then discusses the implications of our results for wages in noncompetitive markets, and Section IV concludes.

I. 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. Small and inefficient banks supported these restrictions because they prevented competition

[3] Recent evidence by Marianne Bertrand and Sendhil Mullainathan (1999) suggests that managers do pay higher wages when corporate control markets function poorly. The takeover market in banking may function relatively poorly as a result of asymmetric information about the value of the loan portfolio. In addition, regulatory barriers may make hostile takeovers especially difficult in banking (Prowse, 1997).

[4] For evidence that restrictions on bank expansion fostered rents in the industry, see Michael C. Keeley (1990), R. Glenn Hubbard and Darius Palia (1995), and Jith Jayaratne and Strahan (1998).

[5] This evidence is consistent with earlier work on rent-sharing by David G. Blanchflower et al. (1996) and Andrew Oswald (1996).

[6] This study also summarizes the early evidence from other studies on the relationship between employment discrimination and product market power. More recently, Judith K. Hellerstein et al. (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.

from other banks.[7] Although there was some deregulation of these branching restrictions in the 1930's, most states either prohibited branching altogether (the "unit banking" states) or limited branching until the 1970's, when only 12 states allowed unrestricted statewide branching. Between 1970 and 1994, however, 38 states deregulated their restrictions on branching. Even though branching was generally restricted, banking companies could expand in some states by forming multibank holding companies (BHC's).

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 where it was headquartered unless the target bank's state permitted such acquisitions. Because no state allowed such transactions in 1956, the amendment effectively barred interstate banking organizations. 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 Randall S. Kroszner and Strahan (1996) for more details]. 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 multibank holding companies. The second column reports the year in which each state permitted branching by means of merger and acquisition (M&A) only, and the third column presents the year each state first permitted unrestricted branching, thereby allowing banks to enter new markets by opening new branches.[8] 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 branches. Column (4) 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 1980's, 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&L's 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 1980's, which increased public awareness of the advantages of large, well-diversified banks (Edward Kane, 1996). Finally, Kroszner and Strahan (1999) suggest that the emergence of new technologies in both deposit taking and lending tipped the balance in the political arena from the traditional beneficiaries of geographical restrictions (smaller banks) to more expansion-minded, larger banks.

II. 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.[9] In our first set of tests, we use data from banks' financial statements to estimate how average compensation for all workers in banking changed following deregulation. Because 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

[7] Nicholas Economides et al. (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.

[8] In most cases, the dates selected reflect the time at which the state finished the branching deregulation process. See Dean Amel (1993) and Jayaratne and Strahan (1998) for details.

[9] 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. There is also no correlation between the level of banking wages and the timing of deregulation.

TABLE 1—YEAR OF DEREGULATION OF RESTRICTIONS ON GEOGRAPHICAL EXPANSION, BY STATE

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

Source: Amel (1993) and Kroszner and Strahan (1999).
** States not yet deregulated.

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 of 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 these data is that it includes all banking employees; the disadvantage is that we can not control for worker characteristics.

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 1 for states permitting interstate banking. In our basic specification, we regress the log of average annual compensation in a state/year on the two deregulation indicator variables to estimate the effects of the policy changes. In this specification, we control for state-specific components of compensation in banking with a fixed effect, and we control for both business-cycle effects and long-term trends with year effects. The regression produces a generalized difference-in-differences estimator. By looking at the change in wages of banking employees after deregulation, we eliminate any state-specific effect on banking compensation that is constant over time. By comparing the wage change in states that deregulate to the wage change in those that do not, we eliminate any banking-specific trend that is common to all states.[10]

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 multibank holding companies, by permitting branching by M&A, and by permitting unrestricted (de novo) branching.[11] To account for this heterogeneity, we use a deregulation index equal to 0 if a state permits neither branching by M&A, nor de novo branching, nor the formation of MBHC's; otherwise, the index equals the sum of the number of ways banks may expand within state. For example, if a state permits multibank holding companies and branching by M&A, the index equals 2.[12]

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), whereas other states merely limited branching to the city of the head office. Because the unit banking states began the period with a tighter constraint on banks' ability to expand, we would expect a larger effect of deregulation on rents, and thus a larger effect of deregulation on wages. To test this idea, we interact the branching deregulation variables with an indicator equal to 1 if the state began the period as a unit banking state.[13]

In the simple model, we estimate that compensation falls by 4 percent following branching deregulation, but we do not find a statistically significant change following interstate banking deregulation [Table 2, column (1)].[14] This basic finding—that changes following

[10] We drop Delaware and South Dakota from the analysis because these two states experienced a dramatic expansion in their banking sectors during the 1980's 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). (Washington, DC is the 51st "state.")

[11] Note that most states permitted MBHC's to operate across the state during our sample period.

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

[13] We classify states that prohibited branching but permitted banks to establish facilities as unit banking states. The unit banking indicator is 1 for the following states: CO, AR, FL, IL, IA, KS, MN, MO, MT, NE, ND, OK, TX, WI, WV, and WY.

[14] 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

TABLE 2—PANEL REGRESSION RELATING THE LOG OF TOTAL EARNINGS FOR BANKING EMPLOYEES TO DEREGULATION INDICATORS, TIME EFFECTS, AND STATE EFFECTS

	Simple specifications		Specifications with average nonbank log earnings		Specifications with unit banking interaction terms	
Post-M&A branching deregulation	−0.040*	—	−0.037*	—	−0.010	—
	(0.007)		(0.016)		(0.007)	
Post-interstate banking	−0.003	−0.005	−0.015	−0.017	−0.009	−0.012
	(0.010)	(0.010)	(0.014)	(0.014)	(0.011)	(0.010)
Branching deregulation index	—	−0.018*	—	−0.020*	—	−0.006*
		(0.003)		(0.009)		(0.004)
Average log earnings for nonbank workers	—	—	0.708*	0.718*	—	—
			(0.099)	(0.102)		
Unit banking*post-M&A branching	—	—	—	—	−0.086*	—
					(0.009)	
Unit banking*deregulation index	—	—	—	—	—	−0.043*
						(0.005)
N	1,336	1,311	947	924	1,336	1,311
F (H_0: all regulatory variables = 0)	2.23	1.86	3.10*	3.14*	4.46*	4.33*
R^2 (within)	0.67	0.67	0.74	0.75	0.69	0.69

Notes: Standard errors are in parentheses. 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. Data for nonbank workers are from the March CPS. The sample period in the regressions using CPS data is limited to 1976–1996 because of limitations in the identification of state of residence prior to this time period. 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, nor de novo branching, nor the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multibank 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.

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 may reflect the fact that actual changes to competition and market structure are less important following this type of deregulation. 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.[15]

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: multibank 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 MBHC's.

One concern might be that there are state-specific trends in wages. Although we address this concern directly in the later regressions using individual data, we also try to control for it here by including the state average log earn-

deregulation is larger and remains statistically significant at the 1-percent level. Note that all tables report the F-statistics testing the null that all deregulation variables are jointly zero.

[15] 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 changes when we include this variable.

ings for nonbanking employees in a given year. Columns 3 and 4 of Table 2 show these results. We find that, whereas there do appear to be significant trends in wages, the inclusion of state average wages does not affect our results on deregulation.[16]

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 5).[17] 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 that of nonderegulating states; the median decline in relative compensation for these 15 states was 9.7 percent, very close to the estimated regression coefficient.[18] 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 factors other than rents such as differences in risk across banks or luck.[19] Thus, the relationship between compensation and measured profits should flatten after deregulation because less of the variance across banks reflects rents.[20] 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 by regressing bank-level log of annual compensation on profits interacted with our two deregulation indicators. Because we are using bank data, we can include a set of year effects that vary by state. 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.

We use return on equity (ROE), equal to net income divided by book value of equity, to measure profits.[21] We also estimate the model using a relative measure of profits equal to an indicator equal to 1 for banks with above-average (median) ROE, where the median ROE is based on 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. In the first specification, the coefficient on ROE falls significantly after deregulation. In the second, the coefficient on the indicator variable also falls, suggesting that high-profit banks paid their workers about 3 percent more than did low-profit banks during the regulated years, but during the deregulated years they paid their workers less than 1 percent more than did low-profit banks.

B. *Wage Changes and Discrimination 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 or time-varying state effects can explain away the declines in overall compensation we

[16] We calculate the state average nonbanking wages using the Current Population Survey from 1976–1996. Prior to this time period, there were limitations in the identification of state of residence. As a result, our sample size is reduced in these regressions.

[17] Note that, because the unit banking indicator does not vary over time, we cannot estimate its linear (i.e., noninteractive) effect on compensation in the fixed-effects model.

[18] Among the limited branching states, 14 out of 23 states experienced compensation declines relative to nonderegulators.

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

[20] 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.

[21] Because ROE has large positive and negative outliers, the values are trimmed at −10 percent and +20 percent. Note that similar results are obtained using return on assets as an alternative profit measure.

TABLE 3—PANEL REGRESSION RELATING THE LOG OF TOTAL EARNINGS FOR BANKING EMPLOYEES TO PROFITS IN REGULATED AND UNREGULATED ENVIRONMENTS

	Continuous profit measure	Discrete profit measure
ROE	0.622*	—
	(0.012)	
Post-M&A branching deregulation*ROE	−0.258*	—
	(0.020)	
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
F (H_0: all regulatory variables = 0)	629.6*	628.9*
R^2 (within)	0.010	0.003

Notes: Standard errors are in parentheses. 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.

observe. We then test whether rents were shared disproportionately with male workers.

Wage Effects Using Individual-Level Data.—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, controlling for worker characteristics, using the March Demographic Supplement to the CPS data between 1977 and 1997. The CPS provides individual-level data with information on income, education, age, race, and gender. Our sample includes individuals aged 18 to 64 who worked full time in the civilian sector in the year prior to the survey; "full-time" workers are defined as those 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.[22]

To modify our earlier results, we now regress log wages on a vector of individual level characteristics, including education indicator variables (classified as less than high school, high school, some college, and college plus), age, age squared, and sex and race indicators. Note that the returns to these characteristics are allowed to vary from year to year. We then introduce an indicator for whether an individual is working in the banking industry. This banking indicator is then interacted with our banking deregulation indicators to test how wages of banking employees change following deregulation. Because we are using individual level data, we are able to include state effects that vary by year; note that the level effects of deregulation are picked up by these time-varying state effects.

By removing time-varying state effects that are common to all workers in a state, the CPS data allow us to control for more than just national trends in banking and bank-specific but time-invariant state effects. These regression results are conceptually equivalent to a triple difference; 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 compar-

[22] Consistent with work by Lawrence F. Katz and Kevin M. Murphy (1992) and George J. Borjas and Valerie A. Ramey (1995), 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.

TABLE 4—THE MARGINAL IMPACT OF DEREGULATION ON THE WAGES OF BANKING EMPLOYEES (CPS DATA)

	Simple specifications		Specifications with unit banking interactions	
Post-M&A branching deregulation	−0.061*	—	−0.051*	—
	(0.011)		(0.014)	
Post-interstate banking	−0.0002	−0.002	−0.0001	−0.002
	(0.015)	(0.015)	(0.015)	(0.017)
Branching deregulation index	—	−0.030*	—	−0.022*
		(0.006)		(0.007)
Unit banking*post-M&A branching deregulation	—	—	−0.018	—
			(0.016)	
Unit banking*branching deregulation index	—	—	—	−0.018*
				(0.007)
N	809,367	790,565	809,367	790,565
F (H_0: all regulatory variables = 0)	14.1*	13.1*	9.6*	10.5*
R^2	0.38	0.38	0.38	0.38

Notes: Standard errors are in parentheses. The dependent variable equals the log weekly wage for all full-time employees in the March Current Population Survey (CPS). The log wage equation also allows for time-varying returns to worker characteristics (which include education broken down into less than high school, high school, some college, and college, age, age squared, race, and sex), time-varying state effects, banking-specific year effects, and banking-specific state effects. The coefficients presented above are the estimates on the interaction between a banking indicator and the deregulation variable. The sample includes data from 1977 to 1997. We drop all observations during the year of deregulation and for two states, South Dakota and Delaware. The deregulation index equals 0 if a state permits neither branching via M&A, nor de novo branching, nor the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multibank 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. Because we have multiple observations per state/year for both banking and nonbanking employees, we estimate robust standard errors where the observations are clustered on state, year, and industry (banking versus all other).

* Statistically significant at the 10-percent level.

ing that to the same change in a state that is not being deregulated at that time.

Our estimates suggest that wages in banking fell by about 6.1 percent after branching deregulation (Table 4, column 1).[23] 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 a negative, but not significant, effect on wages. When we use an index of intrastate branching deregulation as previously, states that moved from no branching to unrestricted branching experienced a 9.0 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 that states that began with tighter restrictions on branching had larger wage declines, although this larger decline is only statistically significant in one of the specifications—wages fell by about 5.1 percent for states that had limited branching but by about 6.9 percent for states that initially permitted only unit banks (Table 4, column 3). The results using the index of intrastate deregulation (column 4) show a similar pattern, but this time the unit banking results are statistically significant.

Discrimination.—Having established that wages fell after deregulation, we next explore who faced the largest wage cuts. In particular, we focus on whether 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 was discrimination during the regulated period.

To test this idea, we estimate the wage equation for female and male banking employees

[23] Note that the table presents the coefficients of the interaction terms between the bank employee indicator and the deregulation indicators. Banking employees constitute approximately 2.5 percent of the workers in our CPS sample.

TABLE 5—THE MARGINAL IMPACT OF DEREGULATION ON THE WAGES OF BANKING EMPLOYEES
DIFFERENTIAL EFFECTS FOR MEN AND WOMEN (CPS DATA)

	Females only		Males only	
Post-M&A branching deregulation	−0.029*	—	−0.125*	—
	(0.012)		(0.024)	
Post-interstate banking	0.012	0.009	−0.026	−0.027
	(0.017)	(0.017)	(0.027)	(0.029)
Branching deregulation index	—	−0.017*	—	−0.056*
		(0.006)		(0.013)
N	336,121	328,208	473,246	462,357
F (H_0: all regulatory variables = 0)	3.42*	4.23*	14.33*	10.14*
R^2	0.28	0.28	0.34	0.34

Notes: Standard errors are in parentheses. The dependent variable equals the log weekly wage for all male or female full-time employees in the March Current Population Survey (CPS). The log wage equation also allows for time-varying returns to worker characteristics (which include education broken down into less than high school, high school, some college, and college, age, age squared, race, and sex), time-varying state effects, banking-specific year effects, and banking-specific state effects. The coefficients presented above are the estimates on the interaction between a banking indicator and the deregulation variable. The sample includes data from 1977 to 1997. We drop all observations during the year of deregulation and for two states, South Dakota and Delaware. The deregulation index equals 0 if a state permits neither branching via M&A, nor de novo branching, nor the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multibank 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. Because we have multiple observations per state/year for both banking and nonbanking employees, we estimate robust standard errors where the observations are clustered on state, year, and industry (banking versus all other).

* Statistically significant at the 10-percent level.

separately. In the simple model, women's wages fell by 2.9 percent after branching deregulation. The model with the index of branching deregulation suggests that moving from a completely regulated to a completely unregulated environment reduces female wages by 5.1 percent (Table 5, columns 1 and 2). For male workers, however, the wage decline is much larger; male wages fell by 12.5 percent after branching deregulation in the simple specification and by 16.8 percent when moving from completely restricted to completely unrestricted branching (Table 5, columns 3 and 4). The decline in men's wages is statistically different from the decline in women's wages at the 1-percent level.

Discriminating employers could also prefer to keep women in lower positions than their skills would warrant. We therefore test whether the proportion of managerial positions held by women changes after deregulation. So that the dependent variable is unbounded, we relate the logit transformation of the measured proportion to our deregulation indicators along with both year and state effects.[24] As shown in Table 6, the share of managerial positions held by women does increase after deregulation (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 to cut costs.

[24] In this model, the error variance is higher when the true population probability is near 0 or 1, 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 reestimates 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. See William H. Greene (1993).

TABLE 6—THE MARGINAL IMPACT OF DEREGULATION ON OCCUPATIONAL DISTRIBUTION AND WAGES BY SEX

	Logit of share of managerial positions held by women (state-level aggregate)	Managerial workers		Nonmanagerial workers		Nonmanagerial workers (tellers only for banking)	
		Female	Male	Female	Male	Female	Male
Post-M&A branching	0.271*	−0.027	−0.089*	−0.035*	−0.194*	−0.005	−0.290*
	(0.161)	(0.024)	(0.025)	(0.012)	(0.042)	(0.023)	(0.091)
Post-interstate banking	0.131	0.033	−0.026	0.013	−0.015	0.008	0.030
	(0.174)	(0.041)	(0.028)	(0.018)	(0.050)	(0.031)	(0.117)
N	944	110,100	146,477	226,021	326,769	217,710	324,878
F (H_0: all regulatory variables = 0)	1.65	0.83	6.69*	4.47*	11.14*	0.05	5.21*
R^2	0.20	0.23	0.26	0.16	0.27	0.16	0.27

Notes: Standard errors are in parentheses. Dependent variable for the share of managerial positions held by women is based on 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 wage results use the log weekly wage for all male or female full-time employees in the March Current Population Survey (CPS) for each occupation group as the dependent variable. The log wage equation also allows for time-varying returns to worker characteristics (which include education broken down into less than high school, high school, some college, and college, age, age squared, race, and sex), time-varying state effects, banking-specific year effects, and banking-specific state effects. The coefficients presented above are the estimates on the interaction between a banking indicator and the deregulation variable. The sample includes data from 1977 to 1997. We drop all observations during the year of deregulation and for two states, South Dakota and Delaware. Because we have multiple observations per state/year for both banking and nonbanking employees, we estimate robust standard errors where the observations are clustered on state, year, and industry (banking versus all other).

* Statistically significant at the 10-percent level.

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 differential wage effects, then male and female wages within occupational categories ought to change by the same amount following deregulation. Table 6 reports that wages of both men and women in managerial positions fell (male wages by 8.9 percent and female wages by 2.7 percent), although the decline in women's wages is not statistically different from zero. The difference between the effects of deregulation on male and female managers' wages is statistically significant, although only at the 10-percent level. Among nonmanagerial workers, wages for women fell by about 3.5 percent, whereas wages for men fell by about 19.4 percent, and this difference is statistically significant (at the 1-percent level). Thus, women's relative wages improved after deregulation, in part because relative wages in low-skilled occupations improved, and in part because women were moved into higher-skilled occupations. The evidence that women's relative wages in managerial positions improved is less compelling, however.

A concern might be that within these two broad categories, there are changes in occupational structure. To rule this out, we also looked at the wage effects of deregulation within a very narrowly defined occupation—the bank teller (Table 6, columns 6 and 7). In particular, we separately estimated the effect of deregulation on male and female bank tellers' wages relative to those of nonbanking nonmanagerial workers. As with other low-skilled employees, male tellers experienced a large and statistically significant decline in wages, whereas female tellers experienced no significant decline in wages. This is consistent with the explanation that within occupation categories of low-skilled workers, women's relative wages improved.[25]

[25] In addition, when we estimate the equation for nonmanagerial workers and allow for differential effects of deregulation on tellers, we find that the decline in wages for bank tellers is *not* statistically significantly different from the decline in the wages of other low-skilled banking employees.

TABLE 7—THE MARGINAL IMPACT OF DEREGULATION ON THE WAGES OF BANKING EMPLOYEES WITH FIVE-YEAR PRE- AND POSTDEREGULATION INDICATORS (CPS DATA)

	Simple specifications	Specifications with unit banking interactions
Post-M&A branching deregulation	−0.073*	−0.066*
	(0.021)	(0.022)
Post-interstate banking	−0.003	−0.002
	(0.015)	(0.015)
Five years before M&A branching deregulation	−0.021	−0.019
	(0.012)	(0.012)
Five years after M&A branching deregulation	−0.003	−0.002
	(0.012)	(0.012)
Unit banking∗post-M&A branching deregulation	—	−0.011
		(0.016)
N	809,367	809,367
F (H_0: all regulatory variables = 0)	7.84*	6.25*
R^2	0.38	0.38

Notes: Standard errors are in parentheses. The dependent variable equals the log weekly wage for all full-time employees in the March Current Population Survey (CPS). The log wage equation also allows for time-varying returns to worker characteristics (which include education broken down into less than high school, high school, some college, and college, age, age squared, race, and sex), time-varying state effects, banking-specific year effects, and banking-specific state effects. The coefficients presented above are the estimates on the interaction between a banking indicator and the deregulation variable. The sample includes data from 1977 to 1997. We drop all observations during the year of deregulation and for two states, South Dakota and Delaware. The deregulation index equals 0 if a state permits neither branching via M&A, nor de novo branching, nor the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multibank 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. Because we have multiple observations per state/year for both banking and nonbanking employees, we estimate robust standard errors where the observations are clustered on state, year, and industry (banking versus all other).

* Statistically significant at the 10-percent level.

C. *Robustness Checks*

Timing.—Our basic specifications assume that the effects of deregulation on wages are immediate and permanent. In fact, the 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. Because the indicator variables used previously are based on the law's effective date, not its passage date, some of the effects of deregulation may occur before the effective dates if banks anticipate the coming competitive environment. Moreover, we want to test whether the decline in wages persists; the rent-sharing interpretation of the results suggests that the wage effects are permanent. To explore these issues, we introduce two additional indicator variables to our model: the first equals 1 during the five years leading up to branching deregulation, and the second equals 1 during the five-year window just after branching deregulation.[26] In Table 7, neither of these coefficients is statistically significant in either specification. Thus, declines in wages appear to occur only *after* deregulation, and those declines appear to be permanent.

Employment.—One potential interpretation of our results is that wages declined because demand declined after bank deregulation, possibly as a result of consolidation in the industry. We consider this explanation unlikely because the persistent wage declines found previously would require limited mobility across industries over a long period. Nevertheless, we test directly for evidence of demand effects by looking at changes in employment following deregulation.

[26] We focus here on branching deregulation, because we found no changes in wages after interstate banking.

TABLE 8—PANEL REGRESSION RELATING THE EMPLOYMENT IN THE BANKING INDUSTRY AS A FRACTION OF STATE EMPLOYMENT TO DEREGULATION INDICATORS, TIME EFFECTS, AND STATE EFFECTS

	Logit of share full-time workers, CPS	Logit of share managerial and professional workers, CPS	Logit of share nonmanagerial workers, CPS
Post-M&A branching deregulation	0.014	0.024	0.012
	(0.026)	(0.034)	(0.035)
Post-interstate banking	−0.017	−0.047	0.009
	(0.028)	(0.033)	(0.036)
Dependent variable mean	0.026	0.023	0.029
N	947	947	947
F (H_0: all regulatory variables = 0)	0.35	1.13	0.08
R^2	0.30	0.31	0.21

Notes: Standard errors are in parentheses. The dependent variable equals the total number of full-time banking workers in the CPS in a state and year, divided by the number of all full-time workers in the CPS in the same 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 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.

As shown in Table 8, employment in the banking industry changed little following deregulation. Column 1 reports the relationship between the share of total state employment in banking and bank deregulation. In particular, the dependent variable equals the logistic transformation of the percentage of total employment in a state and year in banking, divided by overall employment in that state and year. The shares are based on the March CPS data. Columns 2 and 3 then break the employment shares into managers and nonmanagerial workers. In all three cases there are no significant employment effects of deregulation. Thus, a decline in the demand for banking workers cannot explain the drop in wages.[27]

Unobserved Skills.—Although we control for observable skills, wages would decline if *unobserved* skills fell systematically after deregulation. 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 toward better-educated workers in banking (see Figure 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 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 logistic transformation of the proportion of banking workers in each state and year in a particular skill group.

Although 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 does not change (Table 9). Because there is no change in *observable* skills toward younger, less-educated workers after deregulation, it does not seem likely that

[27] We also tried using employment growth in banking in a state as the dependent variable and found similar results.

TABLE 9—PANEL REGRESSION RELATING MEASURES OF AVERAGE SKILLS OF BANKING EMPLOYEES TO DEREGULATION INDICATORS, TIME EFFECTS, AND STATE EFFECTS

	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	0.048	0.643	0.078	−0.177*	0.082
	(0.090)	(0.423)	(0.088)	(0.084)	(0.091)
Post-interstate banking	−0.102	−0.220	−0.059	0.103	−0.021
	(0.072)	(0.573)	(0.077)	(0.121)	(0.131)
Dependent variable mean	0.415	36.3	0.459	0.278	0.263
N	947	947	947	947	947
F (H_0: all regulatory variables = 0)	1.12	1.24	0.60	2.78*	0.55
Adjusted R^2	0.13	0.19	0.19	0.29	0.28

Notes: Standard errors are in parentheses. Each dependent variable is based on the average value observed for all full-time 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.

changing *unobservable* skills is the cause of the decline in wages.[28]

III. Rent-Sharing and Unions

Wages in banking declined after deregulation, we argue, as a consequence of the decline in rents stemming from enhanced product market competition. Rent-sharing alone seems capable of explaining our findings. Compensating differentials could explain the wage decline only if jobs in banking became less distasteful after deregulation, which seems unlikely. For similar reasons, it seems unlikely that efficiency wage effects could be an important explanation. If deregulation were associated with increased monitoring of employees, then the need to pay an efficiency wage could decline. The share of workers in managerial positions did not change after deregulation (Table 9), however, suggesting no change in the supervision of the workers and, hence, no change in the need to pay an efficiency wage.

This is the first study of deregulation that we are aware of to find declines in labor rent-sharing absent unions. Previous studies of the effects of deregulation emphasized the importance of unions in forcing the protected industry to pay rents to workers. Our results suggest that rent-sharing can occur even in their absence. This result is consistent with research by Katz and Lawrence H. Summers (1989), who argue that the presence of unions postdates industry rent-sharing.

Our study is close to that of Nancy L. Rose (1987), who estimated losses to workers following deregulation of the trucking industry. She finds that two-thirds of rents in the industry went to labor, largely due to the strength of unions. Presumably banking employees should not have been able to extract so much from owners without the help of unions. To test this idea, we construct a simple, back-of-the-envelope calculation for the share of rents that went to labor in the banking industry just prior to the time that states began deregulating (1975). We then compare our estimate to Rose's estimate for trucking.

To compute rents to owners, we rely on Keeley (1990), who found that the market-to-book asset ratio fell by 0.46 percent after branching deregulation and by 0.74 percent after states permitted the formation of multibank holding companies. In 1975, there were 37 states with

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

restricted branching (total book value of assets for all banks in those states equals $895 billion) and 15 that prohibited MBHC's (total book value of assets equal to $537 billion). So, branching deregulation cost owners of banks $4.12 billion (0.0046*$895) and MBHC deregulation cost them $3.97 billion (0.0074*$537). Following Rose, we assume a tax rate of 50 percent, which generates a loss of about $16.2 billion before taxes.

To calculate the decline in labor rents, we use the estimated effects of deregulation from the specification in Table 4, column 2, which uses an index of restrictions on within-state expansion to account for the consequences of both branching deregulation and MBHC deregulation. To estimate the decline in compensation, we multiply total compensation in 1975 (from the Call Reports) for each state by the value of the deregulation index after full deregulation (3) minus the actual value of the deregulation index for each state in 1975, times the coefficient on the deregulation index (0.03). Thus, we are estimating how much labor would lose if all states fully deregulated restrictions on expansion within a state in 1975. This generates an annual (pretax) loss to labor of $536 million. If we assume these rents would be earned in perpetuity with a discount rate of 10 percent, the present value of lost rents equals $5.4 billion. So, prior to deregulation, about 25 percent of rents went to labor, and the other 75 percent to owners. As noted previously, Rose found about two-thirds of rents went to labor. Thus, although rents may be shared even absent unions, labor appears to receive a higher fraction when unionized.

IV. 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 both Call Report data and CPS data, we find that increased competition from deregulation reduced wages in the industry by 4 to 6 percent. Percentage declines in wages for low-skill (and hence low-pay) jobs were larger than for those requiring higher skills. Because 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 wages declined more following deregulation of unit banking states, and that the cross-bank relationship between wages and (accounting) profits flattened significantly after deregulation.

Economic theory suggests that increased product market competition should reduce firms' ability to allocate rents to different groups; that is, the ability to discriminate should decline as rents decline. Consistent with the theory, we find that the gap between men's and women's wages declined after deregulation. The decline occurred both because women's occupational status improved after deregulation and because male workers' wages fell more than female workers' wages.

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