

State Commercial Bank Pay Structure and Industry Metrics in Response to Bank Branching Deregulation

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Andrew Granato

Department of Economics
Stanford University
Stanford, CA 94305
agranato@stanford.edu

Under the direction of
Professor Stephen Haber

Abstract

What was the state-level impact of bank branching deregulation on metrics such as profits, bank size, bank efficiency, and bank employee compensation? Did the timing of deregulation influence its effects?

In this essay, I will use the American state-by-state deregulation of bank branching restrictions that occurred between the 1970s and 1990s to analyze the effects of branching regulations on the banking industry within each state. I will use a difference-in-difference analysis of metrics including asset size, profits, income, and indicators of industry efficiency. I test my results with additional controls including state per capita GDP, if the state was a unit banking state, and others. I examine branching deregulation's effect on workers in the industry by race, gender, and state-level banking structure, as well as the effects of a state deregulating early vs. late. I confirm that branching deregulation lowered wages of workers in commercial banking and that wages of men fell further than those of women, but I counterintuitively find that the wages of Hispanics declined by more than those of whites. I also find that bank-level metrics like asset size rise overall, but that these changes are concentrated in non-unit banking states rather than unit banking states that experienced the greatest regulatory shifts over the time period analyzed. I also construct several measures of industry efficiency and find that, by those measures, deregulation did not make the banking industry more efficient.

Keywords: finance, bank branching deregulation, pay structure, bank size, bank efficiency, bank compensation.

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Contents

1. Introduction

2. Literature Review

3. Data

4. Methodology

5. Results and Discussion

6. Conclusion

7. Bibliography

8. Tables

Introduction

From the 1970s to the 1990s, the United States pursued an extensive policy of financial deregulation, repealing many of the rules set up during the New Deal period or even earlier. During this time, many of the local banks in the image of the famous bank in *It's a Wonderful Life* were replaced with the national megabanks that dominate headlines. One subset of regulations eliminated during this period was bank branching restrictions, which were state-level regulations that had previously blocked banks chartered in a certain state from accepting deposits or making loans at facilities elsewhere in the state as well as outside of the bank's home state. Calomiris and Haber (2014), as well as many other economists, have criticized these branching restrictions for purportedly setting up inefficient local and state banking monopolies, and have praised their removal for enabling competition and therefore greater consumer surplus.

Various studies have examined the impact of branching deregulation on aggregate bank performance and profitability as well as the wider impact on society, such as total credit availability and financial stability. Beck, Levine, and Levkov (2010), for example, examined the impact of such deregulation on state-by-state levels of general economic inequality, and various studies have used different datasets and methodologies to examine the systematic effects of such deregulation on the pay structure within the financial sector itself.

This paper will, using data available from the Federal Deposit Insurance Corporation (FDIC) Reports of Income and Condition and the Bureau of Labor Statistics (BLS) Current Population Survey (CPS) annual March Demographic Supplement, comprehensively examine the effect of bank branching deregulation on the commercial banking industry. I look at bank assets, income, profits, the number of employees, employee compensation, and proxies for efficiency. I examine both intrastate deregulation (allowing banks to have branches within the

same state) and interstate deregulation (allowing banks to have branches across state lines), as well as an index that takes into account both types of deregulation. I examine employee compensation changes by race and gender, as well as in states that began the period of interest with a total or near-total prohibition on branching (referred to for the rest of the paper as ‘unit banking states’) vs. non-unit banking states. Then, I examine if the timing of deregulation influenced the effect of deregulation, and perform robustness checks.

Literature Review

This section examines and synthesizes prior research on bank branching regulation, beginning with literature on the general changing scope of American financial regulation across the 20th century. I then explore the specific history of bank branching deregulation and its effects on the wider economy as well as the banking sector. I use these related studies done on bank branching and industry measures and wages to aid in constructing my own methodology.

Financial Regulation in the 20th Century

Over the course of the 20th century, the landscape of American financial regulation, of which bank branching regulations are a significant part, changed considerably. Phillipon (2006) generates a index of financial regulation taking into account four measures: bank branching restrictions, level of separation of commercial and investment banks, level of interest rate ceilings, and level of separation of banks and insurance companies. He indexes these variables from 0 to 1 and confirms that bank regulation massively increased in the 1930s during FDR's New Deal and then further in the 1950s, and then that virtually all of that increased regulation was repealed during the period of 1980-2000 under presidents Reagan, Bush, and Clinton. He finds that the relative wages of finance workers declined in the 1930s, but upon the deregulation beginning in the 1980s, the wages and education of finance workers began to climb steeply, and now the true finance wage premium over an equivalent job in a non-finance sector has increased to 1.3 (notably, Lindley and McIntosh find that the finance wage premium has increased greatly in the UK as well, citing an increase in rent sharing as the most likely explanation). Phillipon asserts that the composition of financial regulation is an important determinant of industry outcomes, including wages and relative wages; Phillipon and Reshef (2013) follows up and

expands on this claim in an international context. Phillipon's analysis underscores the significant role government regulation plays in the financial sector. However, we cannot use his results to examine the specific effects of branching deregulation because branching deregulation is only included as part of an index. Also, Phillipon is examining trends at the national level, and the other three variables of interest in his index occur at the federal level. Bank branching is regulated at the state level, and so requires a state-level analysis.

History of Bank Branching Regulation

Not all types of financial deregulation are synonymous, and while Phillipon's analysis is quite revealing for national trends, his methodology does not allow for examining the specific effects of different *kinds* of deregulation. I am specifically interested in the effects of bank branching deregulation, which in the United States were regulations that prevented banks from opening branches and issuing loans across state lines or within states.

Calomiris and Haber (2014) and Jayaratne and Strahan (1998) describe the history of the American financial sector with specific reference to bank branching. Calomiris and Haber characterize the existing of these regulations as a political bargain between small unit banks and populist politicians- small community banks wanted to wall off competition from heftier rivals and populists were suspicious of megabanks and wanted to prevent the concentration of economic power. Many politicians felt that big banks would only serve the rich (Hammond 1967). The federalist structure of American politics placed a fair amount of regulatory power at the state level, and so state governments were able to institute a fragmented regulatory system for branching. In the nineteenth century, state governments imposed branching restrictions, most of which lasted through the first seventy years of the twentieth century. In 1920, for example, the

United States had 27,000 banks, but less than 2,000 branches, meaning that the great majority of banks were unit banks that had no sub-branches. Beginning in the 1930s, during and following the economic trauma of the Great Depression, a few states deregulated intrastate branching, meaning that a bank in a state was relatively free to open other branches elsewhere in the state, and the national ratio of branches per bank climbed slowly. But it was not until the 1970s that deregulation began in earnest: between 1970 and 1994, the year that the Riegle-Neal Interstate Banking and Branching Efficiency Act guaranteed a full national intrastate deregulation, 38 states change their laws to permit extensive intrastate bank branching.

Additionally, because the Bank Holding Company Act of 1956 banned bank holding companies from acquiring banks outside of its home state unless the target bank's state permitted such an obtainment, interstate bank branching was de facto banned because no states allowed out-of-state acquisitions (Stiroh and Strahan 2002). The first state to change its laws to allow such activity was Maine, in 1978, and between 1978 and 1994 all other states followed suit. Thus, between 1970 and 1994, a system of branching regulation that was more than a century old largely disappeared. In the mid-1970s, the United States had about 14,000 banks and an average of two branches per bank; by 2000, the US had about 8,000 banks and an average of eight branches per bank (Calomiris and Haber 2014), a significant concentration in the banking sector that was exactly what the populist politicians of yore had warned against.

Why did the regime of branching regulations collapse? Kroszner and Strahan (1999) identify three key reasons. First, there was the invention of automatic teller machines (ATMs) that fulfilled some core bank functions but that courts had ruled did not constitute bank branches. ATMs allowed banks to project over significantly greater distances without running afoul of branching law. Second, checkable money market mutual funds greatly enabled banking through

the mail and phone. Third, communications technology advancements lowered the cost of using a faraway bank. Together, these changes undermined local banking monopolies, reducing their power and incentive to fight deregulation. As Calomiris and Haber argue, while much of financial regulation in the United States can be characterized as a political bargain between relevant constituencies, at no point since 1994 has there been a serious effort to reconstitute this particular political bargain.

Impact of Branch Deregulation on the Wider Economy

The empirical literature on American bank branching deregulation strongly suggests that branching deregulation had positive effects on aggregate economic indicators. Beck, Levine, and Levkov (2010) find that branching deregulation lead to lower state Gini coefficients due to a resulting increasing demand for low-skilled workers. Their methodology, a difference-in-differences analysis of the cross-state, cross-year change in variables of interest, is a standard example of the basic structure for an empirical paper in this subfield. They also identify structural factors that lead to branching regulations having a particularly large impact on a state's banking sector, including if the state was a unit banking state pre-1970, before the great wave of banking deregulation (the unit banking states are Arkansas, Colorado, Florida, Illinois, Iowa, Kansas, Minnesota, Missouri, Montana, Nebraska, North Dakota, Oklahoma, Texas, West Virginia, Wisconsin, and Wyoming). Levine, Levkov, and Rubenstein (2014) find that deregulation accelerated the entry of new firms and reduced taste-based racial discrimination against black people in the labor market in states with higher taste-based discrimination by about one-third. Strahan and Weston (1998) and Acharya, Imbs, and Sturgess (2008) also find, using the same basic cross-state, cross-year difference-in-difference analysis, that branching

deregulation increased loan diversification in banks and therefore resulted in an increase in lending to small businesses, contrary to the fears of 19th-century populists. Many other studies employ a similar basic methodology and find other effects: for example, Strahan (2002) finds that branching deregulation increased macroeconomic stability and overall economic growth. These analyses, while they do not directly deal with the effects of deregulation on the financial sector itself, help set a template for analysis of the effects of branching deregulation.

Bank Profits and Compensation

Some papers have directly examined the impact of branching deregulation on the financial sector. Stiroh and Strahan (2003) document a “transfer of assets to better banks,” by which they mean the most unprofitable banks, as measured by return on equity, were disproportionately likely to go out of business following deregulation while the most profitable banks were disproportionately likely to expand. Their results, based on data from the Federal Deposit Insurance Corporation (FDIC)’s Reports of Index and Condition (“Call Reports”), bolster Calomiris and Haber (2014)’s claim that branching laws were instituted under political pressure from rent-seeking, less profitable local banks.

Three studies have examined the impact of branching deregulation on financial sector employee wages and compensation. Hubbard and Palia (1995) find that interstate branching deregulation is associated with higher bank CEO pay and a higher correlation between bank CEO pay and bank performance. However, in contrast to CEO pay and Phillippon’s findings concerning skyrocketing finance wages in general, the literature has pointed to a quite different scenario for the average worker in commercial banking. Wozniak (2007) uses data from the Employment Cost Index Survey of the Bureau of Labor Statistics to show that “between-

establishment inequality increased dramatically” and that “manager wages fell while non-manager wages held steady, leading to a large decline in between-occupation compensation inequality.” After comparing the data to Current Population Survey (CPS) data, Wozniak finds that the between-establishment inequality result holds, but that the other results were confounded by the lack of gender and education variables in the CPI: mainly, deregulation led to declines in compensation for male employees but not for female employees, and male employees were over-represented in manager positions. She finds that the omission of gender from the ECI was quite important: that the declining gender differentials in banking following deregulation are the reason for the decline in the manager-to-non-manager wage ratio, with a more minor role for changing education attainment among non-managers. Observed wage declines among men in banking following deregulation were due to the decline of above-market wages (rents) under the stricter previous regulatory regime. However, because of “concerns about the ECI’s representativeness prior to 1987, the treatment group consists only of states that deregulated after 1987,” meaning that not all states were covered.

Black and Strahan (2001) use Call Reports and CPS data and also conclude that branching deregulation led to a decline in rent-sharing among employees of commercial banks, mostly among the male ones. They find that deregulation caused average wages in commercial banking to mildly fall; specifically, they find that deregulation caused male wages to fall by 12% and female wages to fall by 3%, a finding on which Wozniak elaborates. They also check and control for the previous existence of unit banking for their Call Reports and CPS datasets. In this paper, I will initially replicate and then expand on their approach and results.

Summary

The existing literature on the effects of bank branching deregulation finds that states acted to eliminate these laws due to a series of technological changes that made the previous political bargain over bank regulation untenable. Branching deregulation, which mostly occurred from the 1970s to the 1990s, is associated with state-level lower income inequality, decreased racial labor disparities, increased lending, and increased economic growth. Literature on the effects of branching deregulation on the financial industry itself indicates that branching is associated with low-profit banks going out of business to the benefit of high-profit banks, increased CEO pay, and lower average wages, particularly for men due to the erosion of rents.

I will examine the effect of branching deregulation on the industry more generally as well as wage inequality within the industry, by gender as well as race. I will also use newer data to examine the permanence of these shifts. I will start this process by verifying the results of Black and Strahan (2001).

Data

In this paper, I use two primary datasets. First, I use the Balance Sheet Data and Income Statement Data of the FDIC's Reports of Income and Condition (Call Reports) data, accessed from the Wharton Research Data Services (WRDS). These panel data include the annual income, profits, assets, equity, number of full-time equivalent employees, total compensation, and other bank metrics for each bank in the United States from 1976 through 2013. The state of each bank is included, enabling cross-state, cross-year regressions.

The second primary dataset I used was the March Demographic Supplement of the Current Population Survey (CPS) of the Bureau of Labor Statistics (BLS), which I accessed through IPUMS. These cross-sectional annual data is a representative sample of the U.S. population; the March Supplement contains detailed individual-level data of a person's state, age, sex, race and ethnicity, education, labor force status, occupation, hours worked, and income and income from wage and salary, among other variables. From this dataset, we can identify wages of employees who worked in the financial industry in specific states.

To adjust the wages in the CPS dataset for inflation, I used CPI data from the St. Louis Federal Reserve; I set all wages to the real wage for the 1982-1984 period.

For relevant regressions, I replaced topcoded CPS income values with swap values created by the Census Bureau.

I use dates on intrastate branching deregulation by mergers and acquisition (M&A) and interstate banking deregulation from Kroszner and Strahan (1999). Per the literature, the permitting of multibank holding companies by mergers and acquisition are the standard dates used for intrastate branching deregulation in academic studies and subsequent referrals to 'intrastate branching deregulation' will refer to these dates.

Methodology

For my first set of regressions using Call Reports Data, I regress the commercial banking average compensation on intrastate and interstate deregulation and year and state fixed effects data, as per Black and Strahan (2001). Such a regression format allows me to check to replicate their results on wages. This format is premised on the idea that deregulation functioned, as Black and Strahan put it, a “shock to market competitiveness” due to its elimination of rules that “allowed the industry to enjoy rents.” Black and Strahan use 1969-1996 Call Reports data, but the WRDS edition of the Call Reports dataset does not contain data prior to 1976, so I will use 1976-1996 data. The dataset is at the bank level and has the advantage of including bank-level data like profits as well as all workers in the commercial banking industry, but has the disadvantage of not identifying individual workers and their characteristics. I delete duplicate observations from the Balance Sheet Data and Income Statement Data Call Reports WRDS datasets and then merge them. I then clean the merged dataset by eliminating U.S. Territories, eliminating the slots for the 2014 and 2015 years due to missing data, and filling in several states and zip codes for 105 instances in which those variables were missing from the dataset. In accordance with the literature (Kroznor and Strahan 1999), I drop South Dakota and Delaware from the dataset as well due to unique banking laws that made them credit centers. To prep the dataset for regressions, I create a variable of average compensation per employee by dividing the total compensation paid out by a bank by the equivalent full-time number of employees at that bank and created variables to measure bank efficiency by dividing bank assets, income, and profits by the number of full-time equivalent employees. I add the dates of intrastate branching deregulation by mergers and acquisition and interstate deregulation as indicator variables that equal one a state has deregulated and zero otherwise. I also created an aggregate deregulation

index: a variable that can equal 0, 1, or 2 depending on whether a state has achieved intrastate deregulation, interstate deregulation, or both. Lastly, I add a unit banking indicator that identifies the 16 states that started the period as full unit banking states. I check for the effect on wages to replicate Black and Strahan, and then I go beyond Black and Strahan to examine a wide variety of industry metrics, including income, number of employees, profits, assets, and various efficiency metrics, also checking each one for unit banking vs non-unit banking state differentiation. For these dependent variables, I take the natural log of the state/year averages. I then extend the dataset to the year 2003 to check for that the effects have been fully realized for states that deregulated in the mid-to-late 1990s.

For my second set of regressions, I use CPS data to more finely examine the effect of branching deregulation on workers. The CPS dataset contains data from 1969 to 2016, but I delete years prior to 1977 due to insufficient geographic specificity in the state-identifying variable. Following convention, my sample is of individuals between the ages of 18 and 64 who worked full-time in the non-military sphere the year before the survey was taken. I exclude self-employed workers, people who worked for less than \$67 dollars per week (in 1982 dollars) in the previous year, people who worked less than 48 total weeks in the previous year, and people who worked less than 30 hours a week in a standard work week in the previous year. I take the natural log of the relevant dependent variable, the wage (adjusted for inflation), and add the dates of branching deregulation and the deregulation index that I had added to the Call Reports dataset. For each regression, I control for the state-year average overall wage and time-varying returns to age, age squared, gender, race, and education (broken up into less than high school, high school completion, some college, and college completion), and I include banking-specific year effects and banking-specific state effects, as in Black and Strahan (2001). These controls are designed to

control for changes in wider economy as well as the workforce of the banking sector; for example, Phillipon (2006) describes the steadily increasing level of education of workers in the industry. I generate an indicator for whether a worker is employed in commercial banking, and interact this indicator with the deregulation indicators. This estimate, as per Black and Strahan, compares “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 the time.” I also extend this dataset to 2003 to more fully capture the effects of deregulation. For the 1977-1997 dataset, I replace topcoded values using the methodology in Black and Strahan for the sake of replicating methodology, but for the 1977-2003 dataset, as per IPUMS instructions, I use a Census Bureau-constructed topcoding technique. Again, South Dakota and Delaware are dropped, as are the years of intrastate and interstate deregulation. I initially check to replicate Black and Strahan’s analysis of the effect on wages, including using a unit bank indicator and checking for differential impact by gender. I then check for differential impact by race to see if the reduction in taste-based racial discrimination found in the wider economy by Levine, Levkov, and Rubenstein (2014) can also be found within the financial industry.

Then, I use a similar regression structure and CPS data to check on whether the timing of deregulation influenced the results of deregulation. To do so, I test if the coefficients on the unit banking states and non non-unit banking states separately; I check if the deregulation magnitude differs between the first and second half of deregulators by chronology for both groups. The specific reasoning for the structure of these regressions is presented in the results and discussion section. Lastly, I perform robustness checks.

Results and Discussion

The results from my main regressions are presented in Tables 1-26. My regressions are in four main groups: regressions on Call Reports industry metrics, regressions on CPS data on individual workers, analysis of whether states that deregulation earlier or later were affected differently, and robustness checks.

Structural Industry Metrics

In these regressions (Tables 1-16), I use Call Reports data to identify the effect of branching deregulation on the state/year average of bank employee compensation, bank income, bank profits, bank assets, number of employees at a bank, and bank efficiency. Tables 1, 3, 5, and 7 contain, among other findings, regressions on employee compensation that replicate and expand on Black and Strahan (2001) by truncating the dataset at 1996, but unlike Black and Strahan my dataset does not contain the years 1969-1975, so my coefficients should not be expected to exactly match theirs. Tables 9, 11, 13, and 15 extend those regressions through to 2003 (the regressions will be extended even further as a robustness check).

As shown in Table 1, looking at results from the 1976-1996 dataset, intrastate and interstate deregulation contributed to a mild decrease in wages for bankers as well as general scaling up of the banking industry. These effects are broadly consistent with the existing literature on the subject. I find that the mean compensation of workers in banking decreases by 5.1 percent following intrastate branching deregulation, while there is no statistically significant change after interstate banking deregulation. This finding closely mimics Black and Strahan (2001), who find a decrease of 4 percent and no statistically significant change; again, this discrepancy is probably due to the inclusion of the years 1969-1975 in the Black and Strahan

dataset. Looking at the bank-level metrics, I find that intrastate branching deregulation is associated with an increase of mean bank income by 6.8 percent, an increase of mean bank profits by 18.3 percent, an increase of mean bank assets by 10.5 percent, and an increase of the mean number of bank employees at a bank of 19.6 percent; interstate banking deregulation is associated with an increase of mean bank income by 13.3 percent, an increase of mean bank profits by 28.6 percent, an increase of mean bank assets by 11 percent, and an increase of the mean number of bank employees at a bank of 11.2 percent (all results are significant at the 99 percent level). Clearly, deregulation contributed to an increase in bank size over this period. I also find that intrastate deregulation did not have a significant impact on both the industry average return on assets and equity at the 95% significance level, but that interstate deregulation raised mean return on assets by 32.6 percent and mean return on equity by 30.5 percent. The data thus suggests that it was interstate deregulation, not intrastate deregulation, that led to the increases in overall bank profitability relative to bank size. Overall, I do find strong support for the thesis that branching deregulation led to a significant increase in the size of American banks, while slightly decreasing wages at those banks.

I then examine several metrics to measure the efficiency of the banking sector. Much of the existing literature has focused on the pro-market nature of these reforms. Stiroh and Strahan (2003) find that “competitive reallocation effects transfer assets to better performers” and that “the net effect [of branching deregulation] is a substantial reallocation of market share toward better banks.” Given such an analysis, I hypothesized that branching deregulation would be associated with an increase in efficiency across various instruments for efficiency in the industry; however, as shown in Table 2, I found few results that corroborated this argument and more that contradicted it. I created three metrics to describe the efficiency of a bank: asset efficiency

(assets divided by the number of full-time equivalent employees), income efficiency (income divided by the number of full-time equivalent employees), and profit efficiency (profit divided by the number of full-time equivalent employees). These metrics capture the ability of a bank to command financial assets at a given level of employees; I regressed the state-level averages of these measures on the two deregulation indicator variables with state and year fixed effects. I employed three different sets of weights to the bank-level observations when creating the state/year average: one set was unweighted, one set was weighed by the percentage of assets/income/profits of a bank in a state/year unit, and one set was weighed by the percentage of employees in banking that worked at that bank in a state/year. With the set of unweighted observations, I find that in response to intrastate branching deregulation that asset efficiency declines by 8.2 percent, income efficiency declines by 9.6 percent, and no significant effect on profit efficiency; in response to interstate deregulation I find that profit efficiency increases by 28.2 percent and no other significant effects. With the set of assets/income/profit weighed observations, I find that in response to intrastate deregulation that asset efficiency declines by 10.5 percent, income efficiency declines by 13.1 percent, and profit efficiency declined by 40.6 percent, while interstate deregulation had no significant effects. With the set of employee weighed observations, I find that in response to intrastate branching deregulation that asset efficiency declines by 10.6 percent and income efficiency declines by 14.1 percent and that in response to interstate deregulation that profit efficiency increases by 16.5 percent, and no other significant effects. So, these coefficients fluctuate wildly when given different weights. But on the whole, I find that the evidence to support the hypothesis that branching deregulation made the banking sector more efficient in terms of assets, income, or profit per employee is mixed at best, and not strong enough to substantiate such a claim.

I check to see if my previous Call Reports results change substantially when, instead of using an indicator for intrastate deregulation and a separate indicator for interstate deregulation, I use an index that can equal 0, 1, or two depending on whether neither, one, or both types of deregulation have occurred. According to the results in Table 3, going from no branching deregulation to both intrastate and interstate deregulation is associated with a decrease in mean wages of 7.4 percent, an increase in mean bank income of 18.6 percent, an increase in profits of 44.2 percent, an increase in mean assets of 21.4 percent, an increase in the average number of employees of 33 percent, and an increase of mean return on assets of 34.8 percent and mean return on equity of 30.8 percent (coefficients on the variables in the table should be multiplied by two). Such results are quite similar to the results seen in Table 1 (when summing the effect of intrastate deregulation and interstate deregulation), indicating that there was not a strong synergistic effect from having both deregulations occur as opposed to just adding the change associated with each deregulation separately.

When I test the bank efficiency metrics to see what happens when a state goes from no branching deregulation to intrastate and interstate deregulation, the unweighed metrics show a decline in asset efficiency of 8.2 percent, a decline in income efficiency of 10.6 percent, and an increase in profit efficiency of 31.8 percent. The asset/income/profit weighed metrics show a decline in asset efficiency of 11.4 percent, no significant effect on income efficiency, and a massive decline in profit efficiency of 58.8 percent. The employee-weighted metric shows a decline in asset efficiency of 12.2 percent, a decline in income efficiency of 17.6 percent, and no significant effect on profit efficiency. These results are listed in Table 4. Again, these are substantial fluctuations in results depending on the weights, suggesting once more there is no

strong evidence to conclude that by these metrics the banking industry grew substantially more efficient following regulatory changes.

In Tables 5-8, following Black and Strahan (2001), I allow for differential effects of intrastate deregulation for the 16 states that began the period as unit banking states and the states that did not. Here, I find several surprising results that challenge the understanding of branching regulation's effects. The prevailing narrative in the literature is that there was a "competitive shakeout after deregulation" (Stiroh and Strahan 2003). Black and Strahan (2001) argue that there was a "decline in rent-sharing" with employees following deregulation, and cite their finding that wages declined more in unit-banking states than non-unit banking states as evidence. The link of causality is that unit banking states experienced a greater regulatory shift from 1976-1996 than non-unit banking states, and so the intensification of competition was greatest in the states in which the banks in states had previously received the greatest level of government protection from competition. Stiroh and Strahan (2003) write that "when markets are opened up by regulation, banks tend to expand into new markets by acquiring well-run banks and then competing against the remaining poor performers," increasing the market share of highly profitable (high ROE) banks. They argue their results suggest an explanation where "increased size is the result of successful performance as better firms grow at the expense of their weaker competitors." Following this explanation, unit banking states should have more strongly experienced a trend in which weak banks were culled off after the introduction of competition and their more profitable competitors gained, meaning that the average profitability of banks should have increased more in unit banking states than elsewhere, and that more generally unit banking states should have experienced a more intense effect of whatever the aggregate effect of deregulation was.

Instead, while I do corroborate the finding that wages declined more in unit banking states, I also find that essentially all of the increases in mean bank income, profits, and assets come from non-unit banking states (Table 5). I find that mean worker compensation decreases by 1.7 percent following intrastate branching deregulation in non-unit banking states, while the decline is 9.5 percent in the unit banking states¹. This finding closely mimics Black and Strahan (2001), who find a statistically insignificant drop of 1 percent and a (significant) decline of 9.6 percent; again, this mild discrepancy is probably due to the inclusion of the years 1969-1975 in the Black and Strahan dataset. But while I found earlier that deregulation was associated with substantial increases in bank metrics, I find that intrastate branching deregulation is associated with an increase of mean bank income by 14.7 percent in non-unit banking states but a decrease of 3.6 percent in unit banking states. Similarly, I find that intrastate branching deregulation is associated with an increase of mean bank profits by 27.5 percent in non-unit banking states but only a increase of 5.9 percent in unit banking states, and that mean bank assets increase by 17.1 percent in non-unit banking states but only an increase of 1.9 percent in unit banking states. The increase in average number of employees per bank (about 19 percent) does not change differentially between the two groups of states. Though I did not find significant changes in return on assets and equity following intrastate deregulation in the aggregate sample, when split up, the non-unit banking states experience a 13.8 percent increase in return on assets (significant at the 5.4 percent level, just missing the 5 percent standard) and a 12.9 increase in return on equity (significant at the 9.4 percent level) while the unit banking states experience only trivial, non-statistically significant increases in both categories. From these results, far from unit

¹ For this regulation and subsequent regressions in which the unit and non-unit banking states are split for intrastate deregulation, I follow Black and Strahan (2001) and still include the interstate banking deregulation indicator variable as a control. I find that the effect of interstate deregulation does not change significantly from the regressions in which all states are in the same group.

banking states experiencing the greatest impact from the deregulation waves, it seems as if the truth is precisely the opposite with only one exception: compensation of employees.

In Table 6, I re-examine my efficiency metrics, and find that while for non-unit banking states the intrastate deregulation impact was mostly insignificant, deregulation was associated with mostly greatly negative impacts on the efficiency metrics in the unit banking states. With the set of unweighted observations, for non-unit banking states I find no significant effects on asset efficiency and income efficiency and an increase in profit efficiency of 27 percent; for unit banking states I find a decline in asset efficiency of 18.8 percent, a decline in income efficiency of 24.1 percent, and a decline in profit efficiency of 15.4 percent. With the set of assets/income/profit weighed observations, for non-unit banking states I find no significant effects on asset efficiency and income efficiency and an decrease in profit efficiency of 18.4 percent; for unit banking states I find a decline in asset efficiency of 25.8 percent, a decline in income efficiency of 35.2 percent, and a gargantuan decline in profit efficiency of 70.2 percent. Lastly, with the set of employee weighed observations, for non-unit banking states I find an increase in asset efficiency of 3.4 percent, a decrease in income efficiency of 5.7 percent, and an decrease in profit efficiency of 9.1 percent that just misses significance at the 10 percent level; for unit banking states I find a decline in asset efficiency of 20.1 percent, a decline in income efficiency of 25.3 percent, and a decline in profit efficiency of 14.1 percent. Again, depending on the sets of weights, the coefficients fluctuate, but no matter what weights are applied, it is consistently the case that non-unit banking states are relatively unaffected compared to the statistically significant (and often quite large) declines in efficiency seen in the unit banking states. I still find no consistent evidence for the case that deregulation contributed to increases in banking sector efficiency; if anything, the evidence points the other way.

In Tables 7 and 8, I use the deregulation index instead of separating intrastate and interstate deregulation. Taking the effects of interstate deregulation into account (see Footnote 1), I again find that there was not a convincing synergistic effect from having both deregulations occur as opposed adding the intrastate and interstate deregulation effects while controlling for the other one together, for both the unit banking set and the non-unit banking set.

In Tables 9-16, I extend the dataset from 1976-1996 to 1976-2003 to be sure to capture the effects of deregulation in all states. For the sake of brevity, I will not describe the specific coefficients here in the main text. I find that the addition of these extra years does cause some shifting of results (coefficients shift by up to 8 percent, with an average shift of about 4 percent, as many coefficients do not shift), but never in a way so as to fundamentally change the results of the 1976-1996 dataset: essentially, all results and analysis are preserved, but with slightly different magnitudes. There are two exceptions: I find that when the states are split up into unit banking states and non-unit banking states, the coefficients on the change in return on assets and return on equity are no longer significant, except for the coefficient on unit banking states when examining return on equity, which is significant at the 8 percent level. These results indicate that adding the years 1997-2003 to the dataset does not systematically affect the nature of the effects of branching deregulation.

Given the surprising nature of the finding that the state average bank income, profits, and assets do not actually rise more in unit banking states than non-unit banking states, but do the opposite and barely seem to rise at all, I explore if the existing literature has explored how deregulation played out differently for the banking sector in unit banking states vs non-unit banking states. Specifically, I look for analysis beyond the observation that unit banking states experienced both greatest regulatory shift and the greatest wage declines. There is a wealth of

evidence that unit banking states' economies were affected more post-deregulation (Beck, Levine, and Levkov 2010) and that generally "inefficient banks that were protected by stringent entry barriers probably lost profits and market share while efficient institutions gained" (Jayarne and Strahan 1998).

I find one paper, Stiroh and Strahan (2003), that discusses "how deregulation enhanced competitive dynamics in the U.S. banking industry." Stiroh and Strahan discuss several findings that differentiate how deregulation affected unit banking states and non-unit banking states differently. They find that in "only the most restrictive unit banking states" "the simple [unweighted] [bank] exit rate increased after a state allowed intrastate branching, suggesting significant discipline of the small banks," while "large banks exited more frequently after interstate banking was allowed." They find that unit banking states display "a greater increase in the link between profits and market share": i.e. the more profitable a bank was before branching deregulation, the greater its market share increase after branching deregulation, with this effect being more pronounced in unit banking states. Overall, "the former unit banking states seemed to gain most from deregulation," as high- return on equity banks had the greatest expansion in market share there. My finding that the mean profits per bank of unit banking states does not meaningfully increase post-deregulation, especially when compared to the mean profits per bank of non-unit banking states post-deregulation, is in tension with this result, as is my finding that deregulation does not meaningfully increase mean bank return on equity, whether in the aggregate or in unit banking states.

Overall, I find that my regressions that directly test Black and Strahan (2001), I confirm their results. However, I find that when I test beyond wages and also test metrics such as bank income, profits, and assets, the banks in non-unit banking states account for the entire effect of

increasing bank size, while the banks in unit banking states seem to remain almost unchanged. I also find no compelling evidence that there is an interaction effect from a state completing both intrastate and interstate deregulation, and no compelling evidence that deregulation made the banking sector more efficient in terms of income, profits, and assets per employee. This is the case whether the dataset used mimics the years of Black and Strahan (2001), 1976-1996, or whether it is extended to 1976-2003. More research is needed to contextualize these results to explain how they fit in with the wider literature on branching deregulation.

Effect on Workers: Current Population Survey

In these regressions, I use Current Population Survey data to examine the effects of branching deregulation on workers in the banking sector. Because the CPS contains data on worker characteristics, including age, education, individual income from one's job, gender, race, and more, I am able to much more finely examine the influence of regulatory action on specific groups of workers rather than just an industry state-year mean income. As with the previous section, I initially check to replicate the results of Black and Strahan (2001) and then examine additional questions. I again use two sets of years of data: Tables 17-20 use 1977-1997 data to replicate Black and Strahan, and Tables 21-24 use 1977-2003 data to make sure that the full effect of deregulation in all states has been priced in. I test for the effects of deregulation on banking using both sets of data on all states as well as looking at unit banking vs. non-unit banking states, and look at intrastate and interstate deregulation separately as well as with a single deregulation index. Regressions using 1977-1997 data that test for intrastate and interstate deregulation separately and that look at either the impact on all workers or the workers by gender are modeled on Black and Strahan (2001) and generally but not exactly confirm their results;

coefficients do not ever differ from theirs by more than about 1.5 percent. This difference may be due to my control for the log of state-year average overall wage while they control for unspecified “time-varying state effects.”

In each set of regressions, I test for the impact on all workers in the banking sector as well as the impact differentiated by gender and race. Wozniak (2007) and Black and Strahan (2001) argue that the competitive shock of banking deregulation reduced worker rents that were disproportionately shared with men, resulting in a more significant decline for male workers than female workers. Beck, Levine, and Levkov (2010) also find that banking deregulation reduced racial wage discrimination in labor markets in states that deregulated their banking sectors, so I test to see if such an effect occurred in the banking sector as well. For race, I run regressions for four groups: non-Hispanic whites (referred to as ‘whites’), non-Hispanic blacks (referred to as ‘blacks’, Hispanics, and non-Hispanic Asians/others (grouped as such due to Census classification). I will discuss only my results for whites, blacks, and Hispanics as the category of ‘Asians/others’ is quite broad and drawing conclusions about such a group is quite difficult.

Table 17 suggests broadly that Black and Strahan’s results are accurate, but questions whether Becker’s taste-based discrimination hypothesis applies to the race of employees as well as gender. For example, Black and Strahan find that post-M&A branching deregulation is associated with a 6.1 percent decline in bankers’ wages while interstate deregulation had no significant effect; I find that intrastate deregulation is associated with a decline of 7.5 percent in wages (interstate deregulation has no significant effect, and had no significant effect on any other regressions in Table 17 except for column 6, which looks at Hispanics). Collaborating the collapse in rent-sharing hypothesis, I find that women’s wages fell by 5.3 percent after intrastate deregulation, while men’s wages fell by more than 11.2 percent after intrastate deregulation; the

difference is statistically different at the 3 percent level. Such a result supports the Becker model of competition reducing taste-based discrimination. However, when testing for differential effects by race, Becker's model does not explain the effect as cleanly. Such a model would predict that whites would see a greater decline in bank wages relative to blacks and Hispanics. However, while I find that whites experienced a wage decline of 6.6 percent after intrastate deregulation, blacks actually experienced a larger wage decline of 8 percent after intrastate deregulation (though they experienced a quite large 6 percent wage increase after interstate deregulation, albeit one that was not statistically significant). The difference in wage decline between whites and blacks is not statistically different, indicating that this result is not very robust. Hispanics actually experience by far the most significant wage reductions, of 14 percent after intrastate deregulation and 11.6 percent after interstate deregulation, both statistically significant. The white-Hispanic difference in wage decline is significant at the 7 percent level for intrastate deregulation and at the 1 percent level for interstate deregulation, confirming the result.

Such results cut against the taste-based discrimination prediction, so in Table 18 I run the same tests using the deregulation index. I find that the total effect of both intrastate and interstate deregulation does seem to have a slightly amplified effect relative to looking at both events separately as in Table 17: aggregate wages fall by 9 percent in total when both deregulations occur instead of the 6.2 one would expect from examining both individually. Men's wages fall by 15 percent instead of the 12.1 percent one would expect, and women's wages fell by 5.8 instead of the 3.3 one would expect. The decline between men's and women's wages is statistically different at a 4 percent level. This holds true for individual-race regressions as well: whites in fully deregulated states have wages 7.6 percent lower than they would have in a state in which neither deregulation occurred instead of 4.9 percent, blacks 5 percent lower instead of 2

percent, and Hispanics 26.2 percent lower instead of 25.6 percent. Again, the decline between the wages of whites and blacks is not statistically different, while that between the wages of whites and Hispanics is, this time at the 1 percent level. These results once more bolster the conclusion that men's wages did fall by a greater amount than women's wages, but also that whites did not lose wages significantly disproportionately relative to Hispanics, who actually lost the most by a fair margin.

I then test these results for differentiation between unit banking and non-unit banking states in Tables 19 and 20. Introducing the unit banking intrastate or deregulation index coefficient means that the base intrastate/deregulation index coefficient measures the effect on non-unit banking states, while the effect on unit banking states can be measured by adding the base coefficient to the coefficient on the unit banking-interacted variable. To be consistent with the 'competitive shakeout' hypothesis, states that have been identified as unit banking states should experience larger declines in wages than states that have not been identified as unit banking states. Looking at intrastate and interstate deregulation separately (Table 19), I find that there is only a slight difference (.7 percent) in the wage declines experienced by unit banking and non-unit banking states; this difference is statistically insignificant. I find that men in unit banking states experienced a statistically insignificant wage decline of 2.6 percent greater than the decline experienced in non-unit banking states, while women experienced a statistically insignificant .7 percent wage increase in unit banking states relative to non-unit banking states.² These results are not strong enough to be conclusive, but their contour is consistent with the taste-based discrimination hypothesis: because men were losing wages to depleting rents, a more

² When states are split into unit banking and non-unit banking states, the sample size becomes smaller such that the statistical difference between declines in wages between demographic groups usually fails to meet significance, though the difference in wage decline between white and Hispanic workers post-interstate deregulation was significant at the 1 percent level and the difference in wage decline between white and Hispanic workers in the deregulation index among non-unit banking states was significant at the 2 percent level.

comprehensive deregulation process should mean an increased hit to the rents and men should be affected more than women by the unit/non-unit bank differentiation. Neither whites, blacks, nor Hispanics see a statistically significant change from non-unit banking states to unit banking states; for blacks and Hispanics there may be too few observations per state-year to draw strong conclusions in any case. However, of the differences between the two groups of states, Hispanics did see the most negative wage declines due to deregulation in unit banking states relative to non-unit banking states, not whites. When I use a deregulation index (Table 20), I again find that the total effect of going from no deregulation to intrastate and interstate deregulation is generally of a slightly larger magnitude than the magnitude from examining each deregulation in isolation. For example, men in non-unit banking states experience a wage decline of 10.6 percent due to the combined effect of intrastate and interstate deregulation, while those in unit banking states experience an additional 6.6 percent decline instead of the 2.6 percent decline measured above. The deregulation index results also confirm that the reduction in taste-based discrimination is visible when dividing the employees by gender, but not by race. Consistently, Hispanics face a larger wage decline than whites.

In Tables 21-24, I extend my dataset from 1977-1997 to cover 1977-2003 to be sure to capture the effects of deregulation in all states. In Table 21, no results change by more than about 1.5 percent except for the Hispanics-only regression³. Using 1977-1997 data, I found that Hispanics' wages fell 14 percent after intrastate deregulation and 11.6 percent after interstate deregulation. Using 1977-2003 data, I find that the coefficients are declines of 7.4 percent and 12.8 percent, respectively (the white-Hispanic post-intrastate deregulation difference in decline is

³ With regard to the statistical differences in wage decline between demographic groups, I test the same demographics as in the 1977-1997 dataset and find that most differences become slightly more statistically different, reflecting the increased sample size of the dataset, except in the cases where relative wage declines of whites and Hispanics are being compared. I note all instances in which the statistical difference greatly diverges between the 1977-1997 dataset and the 1977-2003 dataset.

not statistically different and the post-interstate deregulation difference in decline is statistically different at the 1 percent level, and when using the deregulation index the difference is statistically different at the 4 percent level, a change from 7 percent, 1 percent, and 1 percent, respectively, in the 1977-1997 data). Such a result is a noteworthy change, but does not change the relative order of wage declines post-deregulation by race. In Table 22, again by far the most drastic change is of wages of Hispanics: instead of falling a total of 26.2 percent when going from 0 to 2 on the index, they fall by 18.4 percent- still more than the other groups.

When I test the results for unit banking/non-unit banking differentiation (Tables 23 and 24), I get some results that diverge by several percentage points from Tables 19 and 20, though they do not always meet statistical significance at the 5 percent level. That there would be substantially more change for the unit banking states than the states as a whole corroborates my claim that a dataset that extends to 2003 would more fully capture the effects of deregulation: out of the 9 states that deregulated intrastate branching after 1990, 7 are unit banking states (78 percent), while unit banking states made up only 9 out of the 41 states that deregulated before 1990 (22 percent). Unit banking states are thus disproportionately late deregulators and disproportionately likely to not have the entire effect of the deregulation on wages apparent in a dataset that extends only to 1997. For men, non-unit banking state workers' wage decline post-intrastate deregulation was 7.7 percent, but unit banking workers saw a much higher wage decline of 14 percent (an additional 6.3 percent that is significant at the 10 percent level, as compared to the 2.6 percent found in the 1977-1997 dataset). For women, the results remain essentially the same. These results further strengthen the argument that men lost disproportionately in the deregulation relative to women, and that the deregulation represented a blow to male rent-sharing in the industry. With regard to race, though these numbers do not meet

statistical significance at the 5 percent level, whites decline 3 percent more in unit banking states relative to non-unit banking states (1.8 was the percentage found in the 1977-1997 dataset), blacks gain 5.7 percent (11.7 was the percentage found in the 1977-1997 dataset), and Hispanics decline 6 percent (6.4 was the percentage found in the 1977-1997 dataset), which again suggests that Hispanics saw the largest wage declines post-deregulation in unit banking states relative to non-unit banking states⁴. Table 24, which shows the results for the deregulation index on the unit and non-unit banking states, follows similar patterns.

Overall, I find that in the regressions that mimic Black and Strahan (2001), I get similar results. However, I find that Becker's taste-based theory of discrimination, while predictive of the high wage decline of men relative to women, is not predictive of relative wage declines between races. Hispanics seem to have their wages decline the most post-deregulation, not whites, and while the magnitude of the difference is lessened if the dataset is extended, Hispanics still have a more substantial wage decline than whites in the 1997-2003 dataset. I find that, aside from the considerable change to Hispanic wages, the extension of the dataset to 2003 from 1997 also somewhat increases the differential between the effects of deregulation on unit banking states and non-unit banking states, which indicates that in 1997 the full effects of deregulation had not been completed, especially for the unit banking states that were disproportionately deregulated in the years immediately preceding 1997. I also find that a state deregulating both intrastate and interstate has a slightly greater combined effect than one would expect from looking at each deregulation's effect separately, indicating that there is an additional interaction effect, unlike in my Call Reports results.

⁴ See Footnote 1. In the 1977-2003 dataset, the difference in wage decline between white and Hispanic workers post-interstate deregulation remained significant at the 1 percent level and the difference in wage decline between white and Hispanic workers in the deregulation index among non-unit banking states just misses significance at the 10 percent level.

Effect of Timing of Deregulation

It has been thoroughly established that branching deregulation affected the structure and composition of the American commercial banking sector. The state-by-state nature of deregulation means that at the time when the last few states were deregulating, some states had permitted intrastate M&A branching for several decades. I now turn to the question of whether deregulating relatively early instead of waiting is associated with a differing impact on the banking sector. Because the great weight of evidence indicates that interstate deregulation had a quite minor effect on the industry relative to intrastate deregulation, I use intrastate branching deregulation dates.

With regard to intrastate M&A branching, the spread of deregulation dates is quite large. 12 states (including Delaware and South Dakota, which are excluded from the analysis) and the District of Columbia deregulated in this way before 1970. Another 15 states deregulated in the period 1975-1985, and the other 23 deregulated in the period 1986-1997. Intrastate deregulation dates are much more compact: 34 states (including the excluded Delaware and South Dakota) and D.C. deregulated in between 1984 and 1988, 8 states deregulated before that period, and 8 states deregulated after it. To split all 50 states into two brackets as evenly as possible, 1985 is the optimal tipping year. But because every one of the 16 unit banking states except for Nebraska deregulated intrastate after 1985, I need to avoid the bias caused by the disproportionate placement of unit banking states in the latter set. I deal with this issue by constructing two separate regressions. First, I look only at states that are non-unit banking states. For this subset of states, the dividing year to split the sample in two is 1980, and I compare the non-unit banking states that deregulated before 1980 to the non-unit banking states that deregulated after 1980.

Second, I look only at states that are unit banking states. For this subset of states, the dividing year is 1988, and I compare the unit banking states on either side.

An issue in reporting these results is that at this level of sample splitting, the sample size is too small to get any statistically significant results in the Call Reports dataset and in much of the subdivided CPS dataset. For example, looking at the sample of male commercial banking workers in states that are not unit banking states that deregulated up to 1980, I estimate that there are only around 22 people sampled per state-year observation on average, and for women and nonwhites that number would be lower. So, I report only the regressions for which at least in some cases a deregulation coefficient hit significance at the five or ten percent level: the cases for regressions on all workers and on only male workers. Even in this case, many results are not significant and coefficients can be swayed relatively easily, so they should not be treated as conclusive. I use the 1977-2003 dataset.

In Table 25, I look at non-unit banking states that deregulated up to 1980 and after 1980. I find that, overall, non-unit banking states that deregulated in the first period saw statistically insignificant declines in overall wages: 3.5 percent post intrastate deregulation and a .1 percent gain post interstate deregulation. For states that deregulated in the second period, workers saw a 7 percent decline in wages post intrastate deregulation and a statistically insignificant 1.4 percent increase in wages post interstate deregulation. For men, the coefficients seem to be somewhat erratic, reflecting the small sample size of banking workers: the first group saw an insignificant increase in wages post-intrastate deregulation of 6 percent and a insignificant decline post-interstate deregulation of 1.7 percent, while the second group saw a post-intrastate deregulation wage decline of 21.95 percent (and an insignificant .5 percent decline after interstate deregulation) While clearly the shift among men between groups seems exaggerated, on net, the

evidence points to states that deregulated in the second period as probably having seen a greater decline in wages after branching deregulation. Such a result is consistent with the explanation articulated by Calomiris and Haber (2014) and others: during the late 20th century, advances in technology and law made the old regulatory regime increasingly unsustainable. For the states in the latter period, these advances had been widespread for a longer period of time, and so deregulation was a larger shock to the system.

In Table 26, I look at the unit banking states that deregulated up to 1988 and after 1988, and find results in the same vein: workers saw a gain of 2.3 percent post-intrastate deregulation and a gain of 1.1 percent post-interstate deregulation, but saw a decline of 2.2 percent post-intrastate deregulation and a decline of 3.1 percent post-interstate deregulation in the latter group (all of those results were not statistically significant). For men, the earlier group saw insignificant declines of 7.1 percent post-intrastate deregulation and .5 percent post-interstate deregulation, while the later group saw an insignificant rise in wages of 8.3 percent post-intrastate deregulation and a decline in wages of 25.2 percent post-interstate deregulation that is significant at the 10 percent level. Again, these coefficients may be swayed by only a few data points due to the sample sizes, but once again the general result the results at least indicate that the second group of states is more likely to have seen a greater wage drop due to deregulation than the first group.

Robustness Checks

I implement three robustness checks in Tables 27-36.

First, I extend the Call Reports dataset from 2003 to much closer to the present day for the separate intrastate and interstate regressions (Tables 27-30). I go up to 2013 due to an absence of 2014-2016 data in WRDS. Adding years up to 2013 in the Call Reports Dataset has a

fairly similar effect to adding the years up to 2003, indicating that the effects of deregulation have been mostly settled. However, there are two exceptions. First, the decline in bank assets after interstate banking deregulation loses statistical significance. Second, the number of employees at a bank increases greatly (Table 27): by 30.2 percent after intrastate deregulation and by 14.6 percent after interstate deregulation (the 1976-1996 dataset found increases of 19.7 percent and 11.2 percent, respectively, and the 1976-2003 dataset found increases of 24.9 percent and 11.1 percent, respectively). Because the number of employees increases greatly without bank income, profits, and assets increasing, my metrics of bank efficiency also decline, regardless of weighting. When I split the sample into unit and non-unit banking states, the same phenomenon occurs, and asset efficiency metrics decline greatly after deregulation, particularly in unit banking states, where declines in efficiency often exceed 30 percent. The topic of increasing numbers of employees per bank is outside of the scope of this paper, and merits further research.

For the 1976-1996 Call Reports dataset, I also re-run the basic regressions with the log of state-year mean wage from the CPS dataset to allay a potential concern that there are state-specific trends in compensation in Tables 31-34 (because the 1976 data is not geographically well specified in the CPS dataset, I drop the year, so the regression is actually on 1977-1996 data). I find that adding this control does not affect the results, not moving coefficients by more than around 1 percent.

Moving to the CPS dataset, in Tables 35 and 36 I extend the CPS dataset to up to 2010 to check for if the full effects of deregulation have been accounted for (I go to 2010 due to the consistency of the topcoding formula through 2010). Extending the dataset does not affect the results beyond the results of the 1977-2003 dataset.

Conclusion

In this paper, I use a differences-in-differences approach to estimate the impact of bank branching deregulation on commercial bank wages and industry metrics like bank size and efficiency. I corroborate Black and Strahan (2001) and other authors by finding that, consistent with Becker's theory of taste-based discrimination, wages fell further for men than they did for women post-deregulation, indicating that rents that had been disproportionately shared with men had dissipated. However, taste-based discrimination is not predictive of the wage declines when broken down by race: wages of whites do tend to fall further than wages of blacks, but actually fall less than wages of Hispanics. I find, surprisingly, that though wages for workers in unit banking states fell further than those in non-unit banking states, the scaling up of the banking industry in terms of income, profits, and assets per bank occurred almost exclusively in non-unit banking states. I also find that, according to three simple metrics of efficiency, the banking industry did not become more efficient after deregulation, and in unit banking states actually became less efficient. Lastly, I find some evidence that points to a greater effect from deregulation when a state deregulates later, which is consistent with the literature.

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