DEREGULATION, PROFIT AND MARKET STRUCTURE AN EMPIRICAL INVESTIGATION OF U.S. BANKS

EVAN A. SKORPEN

MAY 11, 2011

ABSTRACT. This paper builds upon the structure-conduct-performance literature on the banking industry. Using bank-level panel data from 1976-1994 we examine the effect and dynamics of branching deregulation on bank profitability. While the deregulation appears to pull median bank return on equity down slightly–variance is significant. We identify key bank characteristics that help explain the variation in banks' response to deregulation

1. Introduction

The shock of the recent economic crises has left us wondering about the future of banking in the United States. Given the wave of consolidation witnessed over the past five decades and the recent pressure to limit and penalize the size of banks tagged 'too big to fail', it has never been more important than now that we resurrect the age-old structure-conduct-performance Industrial Organization (IO) debate on the relationship between market structure and profitability in the banking industry.

These recent headlines critiquing banks which are too big to fail are, in many ways, new phenomena. As recent as the 1970s, banks were unilaterally restricted from branching outside their home state or purchasing out-of-state banks, and many banks had strict intrastate geographic restrictions as well[1]. These antique restrictions lingered for decades, and it wasn't until 1997 when the Reigle-Neal Interstate Banking and Branching Efficiency Act became fully phased in that banks were uniformly allowed to branch across states and state boundaries.

While many economists have found a positive statistical relationship between profitability and market structure[2], they have always chosen to circumvent the impact of deregulation on changes in market structure, and have chosen instead to study the more 'pure' market structure changes due to organic growth or mergers and acquisitions. These reports generally attempt to classify the profitability–market structure relationship into one of two distinct camps: the Market Power (MP) theory, or more recently, the Efficiency Structure (ES) theory.

[1] AN [2003]

[2] See Smirlock [1985], Berger [1995], Berger et al. [2004], Jeon and Miller [2002], Northcott [2004], and Tregenna [2009]

The intuition behind the MP theory contains two hypotheses: Under the structure-conduct-performance hypothesis (SCP), more concentrated markets lead to increased interest rate spreads as a result of market collusion and other imperfections. Under the relative-market-power hypothesis (RMP), on the other hand, banks with strong market shares may capture market power from product differentiation, which allow them to set advantageous deposit and loan rates. While subtle, these theories differ in the fact that RMP hypothesis suggests that only the largest banks will benefit from increased consolidation, while the SCP hypothesis suggests that all banks will benefit–regardless of market share or size.

The intuition behind the ES theory also contains two hypotheses: Under the X-efficiency hypothesis (ESX), banks with superior management more fully utilize their assets, and thus lower costs and increase profits. According to the hypothesis, these efficient banks simultaneously tend to grow due to their strong profits and management, which may results in increased market shares and bank concentration. Under the scale-efficiency hypothesis (ESS), on the other hand, profits are achieved through efficiencies of scale, and any observed relationship with market share is simply a proxy for bank size.

These contrasting theories have radically different policy implications for bank regulation and antitrust policy. If either of the MP hypotheses appears to motivate the profit-structure relationship, then bank consolidation would result in unfair pricing and a decrease in total surplus. If, on the other hand, either of the ES hypotheses appears to motivate the profit-structure relationship, then bank consolidation is simply a result of the competitive process and would result in increased efficiency and total surplus.

While many of these studies found statistically significant relationships, they have had difficulty uncovering any economically significant relationship–as their results would necessitate very large changes in size, efficiency, market share or concentration outside the range of their observed samples in order to see any substantive change in bank profits[3]. And thus, the debate remains unresolved.

Deregulation resulting in a relaxation of geographic restrictions offers a third type of change in market structure from which we can study the market structure–profitability relationship. Interestingly, these regulatory changes allow us to make progress in resolving the true impact of structure or profitability as they give us a window by which we can access and study the very large changes in bank characteristics necessitated by prior research. Therefore, testing the impact of deregulation in geographic restrictions will allow us to reach more robust conclusions on the true relationship between market structure and profitability.

This paper is divided into seven sections. In the following section we will introduce the specific geographic restriction we focus our analysis on and provide a brief background on other relevant developments in the US banking sector. In section three we present a review of relevant literature. Section 4 introduces our data and outlines the key assumptions of our analysis. Section 5 explains the econometric techniques used. Section 6 describes our results and Section 7 concludes.

[3]See Berger [1995]

2. Regulatory Changes and the Relevant Market

In 1995 Allen Berger, an economist for the Board of governors of the Federal Reserve, said, “Virtually all aspects of the U.S. banking industry have changed over the last fifteen years”[4]. The changes truly stretch to all aspects of the banking sector: The invention of online banking and ATMs revolutionized the ways clients interacted with their banks, an ever expanding money market forced banks to compete for funds in ways they never had to before, and a slew of regulatory changes to banks capital and reserve requirements reformed the way banks viewed their businesses. That said, perhaps the most staggering change came from the unprecedented wave of bank concentration. From 1976-1994, total bank assets increased by more than 400% despite more than a third of all banks closing their doors to business.

Underlying this consolidation not only growth in banking, but a trend of deregulation and relaxation of geographic restrictions on banks. These restrictions fall generally fall two categories: interstate restrictions on branching across state lines, and intrastate restrictions on branching within state boundaries. While the relaxation of both types of restrictions resulted in more expansive and competitive banking markets, the processes by which the restrictions were relaxed was fundamentally different.

2.1. **Interstate Branching.** In 1927 the federal government passed the McFadden Act in order to allow national banks to compete fairly with state-chartered banks. In the process, the McFadden Act effectively banned all banks from opening branches outside of their home state. That said, while this restriction banned all interstate branching, it did nothing to prohibit bank-holding companies from acquiring banks

[4]Berger et al. [1995]

across state lines–these multi-bank holding companies simply couldn't fold multiple bank assets and operations together.

After 19 such bank-holding companies exploited this loophole by aquiring multiple banks across state lines, the federal legislature passed the Douglas Amendment to the Bank Holding Company Act to prohibit future holding companies from acquiring banks outside of a holding companies home state without permission from the target banks' state. This amendment effectively banned all types of interstate branching until the 1982, as until that time no states permitted such out-of-state acquisitions.

In 1982, however, two things changed. First, federal legislators amended the Bank Holding Company Act to allow for failed banks to be acquired by out-of-state holding companies, and second, Maine, Alaska and New York allowed for out-of-state holding companies to acquire banks within their states as long as the acquirers' state permitted a reciprocal relationship. These changes proved to be the catalyst, as in the following ten years every state except Hawaii removed their restriction on out-of-state acquisition, and thus by 1992, bank-holding companies were permitted to acquire banks essentially country-wide.

It is important to note, however, that while this allowed for bank-holding companies to operate freely across the entire country, these bank-holding companies were still prohibited from merging individual banks, bank assets, or bank operations across state lines.

This last restriction on full interstate branching was finally removed with the passage of the Interstate Banking and Branching Efficiency Act of 1994, which resulted in all but two states permitting full and unhindered interstate branching by 1997 in all but two states.

2.2. **Intrastate Branching.** In comparison to the rather simple process of interstate deregulation, the relaxation of intrastate branching restrictions is complex and lengthy. In 1970, while many states permitted bank-holding companies from acquiring multiple banks within a state, only 12 states permitted unrestricted intrastate branching. The remaining states all imposed intrastate branch restrictions ranging from unit banking–where banks were only permitted to open one branch, to county branching–where banks were only permitted to branch in their home county, to extended branching–where banks were permitted to branch in their home county and, in some cases, neighboring counties.

As markets evolved and investors began to have alternative investment opportunities, banks struggled and applied pressure on state legislatures to relax these restrictions on intrastate branching. This was compounded in 1984 when the Office of Comptroller of the Currency found a loophole in the National Bank Act of 1864 which it used to allow nationally chartered banks to branch freely in states with intrastate thrift branchingJayaratne and Strahan [1996]. The success of these national banks strengthened the case for large diversified banks, and disrupting the profitability of geographic restrictions for states[5]. Lastly, technological innovations such as the ATM and money market mutual funds increased competition in deposit markets and removed some of the information asymmetries which had previously made distant loans so risky.

By 1994, the number of states permitting unrestricted intrastate branching rose to 50 states. The process by which state removed these restrictions generally was a two-step process. States generally permitted bank-holding companies to merge

[5]States changed fees for a bank charter, and they promised banks a monopoly on specific counties for large charter fees

subsidiary banks in the same state first, and soon after permitted banks to open branches freely within state borders.

3. Literature Review

Despite enormous amounts of literature on bank profitability and bank deregulation, little focus has been spent understanding the impact of deregulation of intrastate branching on bank profits. Consequently, for our purposes, relevant literature comes in two forms: First, I discuss previous literature which has studied the relaxation of intrastate branching restrictions, and second I address previous literature on the profitability—market structure debate in the banking sector.

3.1. **Market Deregulation Literature.** Jayaratne and Strahan [1996] first identified the power of studying the relaxation of intrastate banking restrictions in a panel study on the link between finance and growth. Prior to this study–while positive relationship between finance and economic growth had repeatedly been observed–little headway had been made in proving the direction of causality. By exploiting the the piecemeal nature of state branching deregulation, Jayaratne and Strahan could control for a vast assortment of omitted variables that had plagued prior research. Using the intrastate bank deregulation as a proxy for financial development, they conducted a panel difference-in-difference study to show that the deregulation of branch restrictions led to an accelerated real per capita growth rate and higher bank efficiency. Moreover, since they controlled for all cross-sectional and time-dependent omitted variables, they claimed that this was strong evidence supporting the theory that financial development causes economic growth.

These controversial findings instantly drew criticisms. Freeman [2002] challenged the findings of Jayaratne and Strahan by arguing that the timing of the deregulation in each state was endogenous, not exogenous. They showed that states deregulated when their growth rates were "on average four percentage points below trend." Consequently, Freeman argues that the timing of state deregulation was in part caused by these low-growth periods, and that the effect observed in Jayaratne and Strahan [1996] was in part a natural rebound from a period of abnormally slow growth

Garrett et al. [2005] build on Freeman's critiques of Jayaratne and Strahan when he suggests that the timing of the deregulation might not only be caused by the abnormally low periods of growth currently, but also by the expectation of future growth. He argues that were this the case, any correlation found by Jayaratne and Strahan would be spurious.

Taking into account these critiques, Huang [2008] takes an alternate approach to studying intrastate deregulation. Rather than comparing states which have experienced the deregulation to the average of those that haven't, Huang carefully picks 285 pairs of geographically neighboring counties separated by state borders in which the two counties witnessed the deregulation at very different times. By comparing the growth rates of these neighboring counties–using the former as the experiment and the latter as a control–Huang hopes to control for Freeman's criticisms. Similarly, since the timing of the regulation is decided at a state-level, he argues that "it is unlikely that economic conditions and the financial sector's structure in a county can influence regulatory decisions made by the state legislature," and such he claims to control for Garrett's criticisms as well. Conducting a similar difference-in-difference analysis, Huang now finds little support for the conclusions laid out in Jayaratne and Strahan [1996] and concludes that bank deregulation had no significant impact on growth rates.

While these studies ask very different questions of the market deregulation, the econometric techniques presented in Jayaratne and Strahan [1996] and Huang [2008] can help us better shape our analysis.

3.2. **Profitability–Market Structure Literature.** The relationship between bank profitability and market structure has been studied extensively in the past literature and the MP-ES debate which has followed has its roots firmly grounded in traditional Industrial Organization economics. Classic industrial IO economists tend to observe the industry as a whole, and thus try to explain profitability by observing trends in industry concentration. These ideas, presented in Bain [1951] as the Differential Collusion Hypothesis, argue that as concentration increases, collusion becomes more cost-effective, and thus firms collude more and become more profitable. Revisionist IO economists, on the other hand, tend to reject the idea that collusion is the primary motivator of increased profitability, [6] and thus they argue that the key issue is differences at the firm level.

As these ideas took hold in the literature on the banking sector, the first classical economists formulated the structure-conduct-performance hypotheses (SCP) and attempted to show a relationship between bank profits and the concentration ratio–the number of banks in a market.[7] These crude measures of concentration were exposed as inadequate in Rose and Fraser [1976], when they argued that the concentration ratio depends arbitrarily on the number of firms included and ignores the structure of the remaining firms in the market. Rose and Fraser proposed the use of the Herfindahl-Hirshmann index, or the Gini coefficient, which both take into account

[6]See Alchian and Demsetz [1972]
[7]See Benston [1976]

the size distribution of all the firms in the market. Using cross-sectional data on 704 banks across 90 MSA's in Texas, they conclude that "traditional measures of market structure–concentration ratios and number of banks–do not preform as well as some other static measures of market structure (especially the Herfindahl-Hirshmann index)."

This early SCP evidence was undermined by Rhoades [1977], which showed that only 30 of previous 39 studies were successful in finding any evidence to support the SCP hypothesis, and even when evidence was found, it was often muddled and weak—as was the case in Rose and Fraser [1976], where only 6 of 27 estimated equations produces any significant results at all. Given the number of equations estimated, this number is hardly over the 4 'significant' results one would expect were the variables completely unrelated.

Smirlock [1985] argued that "much of the frustration [in past studies] seems to be based on a priori acceptance of the fundamental axiom of the traditional SCP paradigm." Smirlock used data from 2,700 banks to run a cross-sectional test of the traditional SCP hypothesis, as well as the revisionist relevant market power (RMP) hypothesis[8]. He shows that once market share is included, concentration no longer contributes any explanatory power to banks profitability. He thus interprets the statistically significant relationship between market share and profit rates as evidence of the RMP hypothesis.

Berger [1995] introduced the two Efficiency Structure hypotheses to the field of study. To test all four hypotheses, Berger runs 30 cross-sectional regression including measures of concentration, market share, X-effeciency and scale-efficiencies. As

[8]As first seen in Ravenscraft [1983] and Mueller [1983]

Berger notes, previous studies could not distinguish between the RMP, ESX, and ESS hypotheses since each hypothesis predicts a strong profit–market share relationship. His findings support the RMP hypothesis and, to a lesser extent, the ESX hypothesis. Namely, Berger finds that both market share and X-efficiency are strongly correlated with profits when controlling for other factors, but, as required by the ESX hypothesis, X-efficiency does not appear to be correlated with higher concentrations. Lastly, Berger acknowledges that the economic significance of these market structure effects on profits appears to be rather small, suggesting that it would take very large changes in market share to have any noticeable impact on profitability.

Jeon and Miller [2002] advanced the literature when they conducted state-by-state tests of bank concentration on average bank profitability. Citing Radecki [1998], Jeon and Miller argue that since larger banks tend to set state-wide prices, the relevant market is not the MSA, but the state. To account for the efficient-structure critiques found in Berger [1995], Jeon and Miller implement temporal-causality regressions, suggesting that if the RMP holds, then increased market share will precede profits, whereas the efficiency-structure hypotheses would suggest that market power would trail increased profits. Their findings support a leading relationship from concentration to profits, supporting the RMP hypothesis.

Most recent studies seem to support the relative market power hypothesis which suggests that banks holding dominate market shares may capture market power, and thus increased profitability. That said, they make no claims as to the impact of regulatory shifts to bank market structure. Moreover, while many of the recent studies find statistically significant relationships between market share and profitability, these results are not economically significant–meaning that market shares would have to change by an unrealistically large margins in order to yield any sort of economically significant change in profitability[9]. Given that changes in market share of this degree are outside of range of their studies, these conclusions require more analysis. Thus, it is still an open question whether larger shocks to market structure–such as a relaxation of geographic restrictions–might have an economically significant impact on bank profitability, or is the profitability of the industry largely immune to any such changes in market structure.

[9]See Berger et al. [2004], Berger [1995] and Bikker and Haaf [2002]

4. Data

For this study, we will rely upon the Report on Condition and Income (Call Report) data provided by the Federal Reserve Bank of Chicago,[10] as well as the deregulation data provided in Jayaratne and Strahan [1996]. The call report data provides annual financial statements and location information for banks and other savings institutions from 1976-today, while the deregulation data helps us identify the timing of intrastate branching deregulation in each state.

4.1. **Defining Deregulation.**

The staggered nature of state deregulation provides an optimal opportunity to test the effect of geographic restrictions as we can exploit both cross-sectional and time series differences in order to control for omitted variables. Our analysis, however, depends upon our ability to pigeon hole the exact year in which intrastate deregulation occurred for each bank. Given that states typical removed geographic restrictions on intrastate branching in waves–first allowing holding companies to merge same-state banks and later allowing for banks to open new branches state wide–this is not an easy issue to resolve.

We choose to define intrastate branching deregulation on a state-by-state basis to have occurred once the state fully allows for holding companies to merge banks within a state. We utilize this definition for two reasons. First, applying this definition allows our report to be consistent with past studies on intrastate branch deregulation [11], and second, we argue that even though some independent banks may not have been able to open branch statewide after we deem them to be deregulated, they compete

[10] http://chicagofed.org/economicresearchanddata/data/bhcdatabase/bhcdatabase.cfm
[11] See Jayaratne and Strahan [1996]

with other banks that operate state-wide for the entire period after deregulation, and thus this definition marks the start of competition on the state level.

4.2. **Constructing the Panel.** Our analysis starts with a panel of annual return on equity measurements for every bank throughout the entire period from 1976 until 1994, which we construct from the annual financial statements listed in the Call Reports. We begin our times series in 1976 since this is the first year that Call Reports began recording profit figures, and we end our time series in 1994, as this marks the passing of the Interstate Banking and Branching Efficiency Act, which further relaxed geographic restrictions on bank branching.

Next, we construct a dummy variable for each bank i in each year t delimitating the status of deregulation, such that:

$$\text{Mkt Dummy}_{i,t} = \begin{cases} 1 & : \text{State permits intrastate bank branching} \\ 0 & : \text{State restricts intrastate bank branching} \end{cases}$$

With this panel of bank profitability and regulation times series, we can next limit ourselves to the an appropriate subset to study. Unfortunately, we cannot measure the impact of the deregulation in every bank since not every state observed the deregulation during our 19 year time horizon. If our study included banks in these states which didn't experience the event, we would have no data by which to estimate the deregulation effect. Consequently, we must restrict ourselves to banks which have market deregulation during the 19 year window, or, in other words, banks who had no had the deregulation in the first year, and banks who have had the deregulation by the final year. This restriction achieves another goal, however, as it balances our panel to those banks for which we have a complete 19 year time series.

This may not be enough, however, since banks that encounter deregulation during the first or last year of our study will also have insufficient degrees of freedom by which to derive a mean level of profitability before or after the event, respectively. This exposes an inherent tradeoff: by limit ourselves to banks which encounter the event during the middle of our time horizon, we will have the most degrees of freedom by which to get stable results; however, we will also be limiting the number of states we have to study, which could make our conclusions unrepresentative of the United States at large.

To mediate this variance–fit tradeoff, we define a buffer as the period at the beginning and end of the event in which we restrict the event from occurring during. As we increase our buffer size, the number of remaining states–and thus banks–decreases, but our degrees of freedom increases. To determine the appropriate buffer size, we can observe how restrictive a marginal increase in buffer size would be, and select the buffer size that minimizes the marginal cost of substitution between sample size and degrees of freedom.

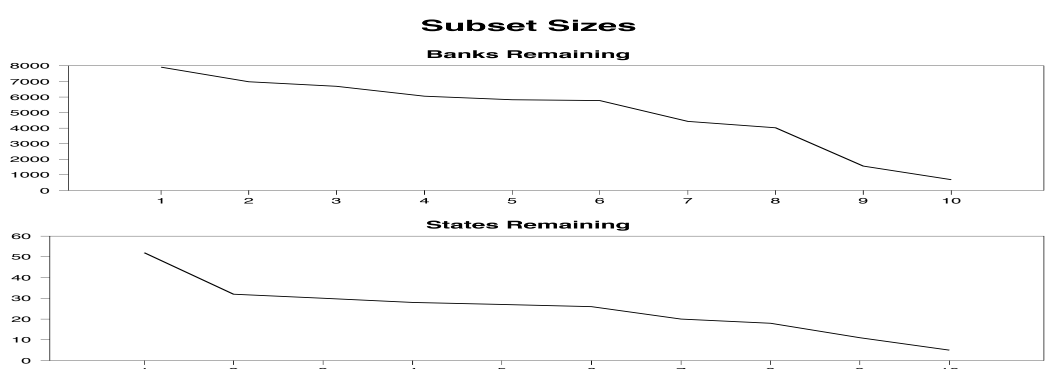

Figure 1. Examining the trade-offs of choosing buffers

Table 1. The trade-offs of choosing buffers

Buffers	Banks	Δ Banks	States	Δ States
0	7919	-	52	-
1	6974	945	32	20
2	6689	285	30	2
3	6048	641	28	2
4	5822	226	27	1
5	5770	52	26	1
6	4434	1336	20	6
7	4018	416	18	2
8	1557	2461	11	7
9	686	871	5	6

As you can see, simply restricting ourselves to states which exhibit the event in our observed time period cuts our sample down to 6974 banks in 32 states—an 11.9% decrease in the total number of banks and a 38.4% decrease in the number of observed states. While this large drop in states might be worrisome, the fact that greater than 88% of banks are still within our window of study suggests the states which experience deregulation at a period outside of our time horizon tended to be small. This buffer size is insufficient, as which a buffer of only one we could now have only a single degree of freedom before or after the change by which to calculate a mean level of profitability. According to the marginal rate of substitution, this tradeoff is optimized at a buffer size of four or five for the optimal number of states, and buffer size of five for the optimal number of banks. For this reason, we will utilize a buffer of five and restrict ourselves to the subset of 5770 banks across 26 states which experience deregulation between 1979 and 1989.

4.3. **Constructing the Market Structure Variables.** We are interested not only with each banks response to deregulation, but also in explaining variations in banks' responses. For this, we turn to the four leading hypotheses on bank profitability in order to understand bank characteristics which might contribute to any observed variation. Creating measurements of market share, concentration, size and efficiency of each bank prior to deregulation will help us understand how each of the four hypotheses can contribute to changes in bank profitability following deregulation.

First, we construct a measurement of market structure for each bank:

$$\text{Market Share} = \frac{\text{Bank Deposits}}{\text{Market Deposits}}$$

Where we define the market deposits as the sum of all bank deposits in each banks' relevant market. Further, we can understand a banks' relevant market prior to deregulation as its home county. Since some banks were permitted to branch in neighboring counties prior to deregulation, our definition of relevant market puts a lower bound on each banks' true relevant market. That said, these cases where rare, any evidence suggests that few banks took advantage of these opportunities even when provided[12].

With this simple definition, we can calculate:

- $\text{MS}_{\text{pre},i}$ = the market share of bank i one period before deregulation

[12]See , , and

Next, we use the Herfindahl-Hirschman Index to construct a measurement of bank concentration:

$$\text{Herfindahl-Hirschman} = \sum_{i=1}^{\text{\# of banks in market}} \left(\frac{\text{Bank i's Deposits}}{\text{Market Deposits}} \right)^2$$

Using the same definition of market deposits and relevant market as above, this allows us to construct:

- $\text{HHI}_{\text{pre},i}$ = the concentration of bank i's market one period before deregulation

To test the impact of efficient structure hypotheses, we need not only measurements of concentration and market share, but also bank efficiency and size. We construct a measurements of bank efficiency by first defining bank inefficiency as:

$$\text{Inefficiency} = \frac{\text{Non-interest expense}}{\text{Net income before extraordinary charges}}$$

Where non-interest expense includes anticipated bad debt provisions, salaries and benefits, equipment, property, taxes and loan loss provisions–where salaries and benefits comprise the largest portion of this expense for most financial institutions. Banks that can minimize their non-interest expense to revenue ratio are the banks that best utilize their employees and capital, and thus we define bank efficiency as:

$$\text{Efficiency} = 1 - \text{Inefficiency}$$

Given this metric, we construct:

- $\text{EFF}_{\text{pre},i}$ = bank i's efficiency one period before deregulation

Lastly, we define size as a simple measurement of bank deposits (in dollars) and we construct:

- $\text{Size}_{\text{pre},i}$ = The value of bank i's deposits one year before deregulation

This leaves us with for explanatory variables by which we can attempt to understand variations in banks' response to deregulation. Namely:

- $\text{MS}_{\text{pre},i}$ = the market share of bank i one period before deregulation
- $\text{HHI}_{\text{pre},i}$ = the concentration of bank i's market one period before
- $\text{EFF}_{\text{pre},i}$ = bank i's efficiency one period before deregulation
- $\text{Size}_{\text{pre},i}$ = The value of bank i's deposits one year before deregulation

5. Empirical Methodology

5.1. **Controlling for Correlations.** As we saw in Jayaratne and Strahan [1996], quantifying the change in bank profitability due to market deregulation is not as simple as comparing the mean profitability before and after deregulation. This follows since the timing of the market deregulation's may not be exogenous, as state legislatures decision to deregulate follows from years of lobbying by the banking sector, and thus it may be that some market factors are contributing to the timing of deregulation. If, for example, states tended to deregulate during periods when banks were performing unusually well, a simple comparison of profitability before and after the deregulation would accredit the deregulation with having a larger effect then it truly did, as some of the drop in profits would simply have been the bank profitability returning to more normal levels.

Underlying this is theory is the belief that bank return on equity in successive years could be serially correlated–meaning that a bank's return on equity in year t is dependent on its return in years $t-1$, $t-2$, etc. Only were this were the case could we expect to see trends in the pre-event regime that could "carry over" into the post-event years. To allow for this, we will allow for

$$y_{i,t} = f\left(y_{i,t-1}, y_{i,t-2}, \ldots, y_{i,t-p}\right)$$

where we will choose an appropriate p for each bank using the AIC information criterion to choose the best fit from a sample of $p \in \{0, 1, 2, 3\}$. We cap $p \leq 3$ since each additional lag reduces our usable degrees of freedom.

Correlation across time is not the only type of correlation we must account for, we must also allow for serial correlation between banks in a given year– as trends which effect large collections of banks could also bias our findings. For example, one would expect some national trends in bank profitability caused by recessions such as the Savings and Loan Crisis, or national regulation shifts. We can control for these industry-wide factors by removing the national annual mean return on equity from the return on equity of each bank in each year.

$$\tilde{y}_{i,t} = y_{i,t} - \frac{1}{5770}\sum_{i=0}^{5770} y_{i,t}$$

This new statistic, $\tilde{y}_{i,t}$ measures the residual profitability of bank i in year t, once accounting for the mean profitability of the banking industry in year t.

5.2. **Simplified Model.** Consequently, to address the possibility that there are carry-over effects and industry-wide effects, we can consider the model:

$$\tilde{y}_{i,t} = a_{i,0} + \sum_{j=1}^{p} a_{i,j}\tilde{y}_{i,t-j} + c_i z_{i,t} + \epsilon_{i,t}, \quad |a_{i,j}| < 1$$

where: $\tilde{y}_{i,t} =$ bank i's residual return on equity in time t

$z_{i,t} =$ a dummy variable for bank i that takes on the value of zero prior to market deregulation, and unity following it

$\epsilon_{i,t} =$ a white-noise disturbance

This model suggests that bank profitability in period t is a function of the past p years of profitability, a dummy for the market deregulation, and a white-noise disturbance encapsulating every other factor causing bank profits. Using this model, we can isolate the effect of the market deregulation given the possibility of carry-over effects in bank profitability.

5.3. **Full Model.** This model is not complete, however, as we are not only interested in how each bank responds to deregulation independently, but also how certain market structure characteristics impact a banks response to deregulation. Thus, a more complete model would take the form:

$$\tilde{y}_{i,t} = a_{i,0} + \sum_{j=1}^{p} a_{i,j}\tilde{y}_{i,t-j} + \left(\sum_{k=1}^{q} \phi_{i,k}\beta_k + c_i^*\right) z_{i,t} + \epsilon_{i,t}$$

where: $\phi_{i,k} =$ the k-th market structure characteristic for bank i

$\beta_k =$ a regression coefficient corresponding to the k-th market structure characteristic

$c_i^* =$ a bank-specific regression coefficient capturing the residual impact of deregulation for bank i, once we control for industry-wide factors

Notice that β_q depends only on the corresponding market structure characteristic, and not on the specific bank i. Given that $z_{i,t} \in \{0,1\}$, we only have one degree of freedom to determine a corresponding coefficient; and thus we can not determine the relative weight of market structure effects and bank specific effects on a banks response to deregulation. We can proceed with a two-stage regression by first noting that

$$\hat{c}_i = \sum_{k=1}^{q} \phi_{i,k}\beta_k + c_i^*$$

And thus, if we estimate the simplified model and compute $\hat{c}_i$ for each bank, we can back out β_k estimates for each of the observed market structure characteristics using a variation of generalized least squares on our cross-sectional data.

This is not straightforward, however, since each $\hat{c}_i$ is an estimated parameter, and variance in the sampling variance of each c_i can introduce heteroscedasticity into our model. For this reason, we can interpret regression residuals in our second-stage regression as having two components. A measurement error component, μ_{c_i} which arises from that fact that each $\hat{c}_i$ is the output of our first-stage regression and thus can be understood as an unbiased estimate[13] of some true parameter such that:

$$\hat{c}_i - c_i \sim N\left(0, \mu_{c_i}\right)$$

The second component is the 'random shock' component, c_i^*, which is the random error which arises from that even true realizations of c_i and $\phi_{i,k}$ are draws from a random distribution. In this case we interpret these random errors as the bank specific impact effect of deregulation. This allows us to understand our second stage regression as:

$$c_i = \sum_{k=1}^{q} \phi_{i,k}\beta_k + c_i^* + \mu_i$$

where σ is the standard error of our random shock components c_i^* and ω_i is the standard error of our measurement error μ_i. To simplify our analysis, we assume that our measurement errors are independent and uncorrelated. While this is a simplifying assumption, it is grounded in the fact that we control for both national and state factors that might cause such cross-sectional covariance.

Using a bootstrap, we shuffle the residuals in each time series to create 500 alternate time series that follow the same underlying distribution for each bank. From each of these new time series, we estimate a new AR(p) model and obtain an estimate of the deregulation coefficient $\hat{c}_i$. Observing the distribution of all 500 of these estimates for each bank, we can derive an asymptotically reliable estimate the

[13]We know our estimates of c_i are unbiased since the OLS first-stage regressions produce unbiased estimates of the true parameters

measurement error ω_i for each bank[14]. This is incomplete, however, as we do not have any knowledge of the variance of our random shocks, σ_i^2, and we have no way to estimate this variance directly.

Hanushek [1974] shows, however, that for large sample sizes, the expectation of the sum of squared residuals from an OLS second-stage regression is asymptotically equivalent to:

$$E\left[\sum_i \hat{\nu}_i{}^2\right] = E\left[\vec{\nu}^T\vec{\nu}\right] - \text{trace}\left[\left(X^TX\right)^{-1}X^T\Omega X\right]$$

$$\begin{aligned} \text{where: } \nu_i &= \sigma + \omega_i \\ \vec{\nu} &= (\nu_1, \nu_2, \ldots, \nu_i)^T \\ \vec{\omega} &= (\omega_1, \omega_2, \ldots, \omega_i)^T \\ \Omega &= \text{the variance-covariance matrix, which can be written as } \sigma^2 I + \vec{\omega^2} I \end{aligned}$$

From this, Lewis [2000] shows we can derive an unbiased estimator

$$\hat{\sigma}^2 = \frac{\sum_i c_i^{*2} - \sum_i \omega_i^2 + \text{trace}\left[\left(X^TX\right)^{-1}X^T\left(\vec{\omega^2}I\right)X\right]}{N-k}$$

Using this estimator, Lewis [2000] shows through monte carlo simulations that we can derive unbiased and asymptotically efficient estimates of the the true parameters and standard errors if we fit a weighted least squares regression using weights:

$$w_i = \frac{1}{\sqrt{\omega_i^2 + \hat{\sigma}^2}}$$

[14] See Pedroni and Park [2003]

where the weighted least squares regression can then be fit using OLS:

$$w_i c_i = \sum_{k=1}^{q} \phi_{i,k}\beta_k w_i + \left(c_i^* + \mu_i\right) w_i$$

5.4. **Computing the Model.** We can isolate the pre and post event return on equities to determine the total effect of the market deregulation for each bank. Notice that for t prior to the event, $c_i z_{i,t}$ is zero, and thus the model simplifies to

$$\tilde{y}_{i,t_{\text{pre}}} = a_{i,0} + \sum_{j=1}^{p} a_{i,j}\tilde{y}_{i,t_{\text{pre}}-j} + \epsilon_{i,t_{\text{pre}}}$$

Therefore we can determine the mean return on equity for a bank before the event by taking this expected value:

$$\begin{aligned} E\left[\tilde{y}_{i,t_{\text{pre}}}\right] &= E\left[a_{i,0}\right] + \sum_{j=1}^{p} a_{i,j} E\left[\tilde{y}_{i,t_{\text{pre}}-j}\right] + E\left[\epsilon_{i,t}\right] \\ &= a_{i,0} + \sum_{j=1}^{p} a_{i,j} E\left[\tilde{y}_{i,t_{\text{pre}}}\right] \\ \left(1 - \sum_{j=1}^{p} a_{i,j}\right) E\left[\tilde{y}_{i,t_{\text{pre}}}\right] &= a_{i,0} \\ E\left[\tilde{y}_{i,t_{\text{pre}}}\right] &= \frac{a_{i,0}}{\left(1 - \sum_{j=1}^{p} a_{i,j}\right)} \end{aligned}$$

Similarly:

$$E\left[\tilde{y}_{i,t_{\text{post}}}\right] = \frac{a_{i,0} + c_i}{\left(1 - \sum_{j=1}^{p} a_{i,j}.\right)}$$

Thus, the long term impact effect of the event can be thought of as the difference in means, or

$$\frac{c_i}{\left(1 - \sum_{j=1}^{p} a_{i,j}\right)}.$$

What is important to us, however, is not just the long term impact of the market deregulation, but also any short term reactionary dynamics. It is conceivable that over the long-term, banks might see little change in industry profitability, but that in the short term profitability figures would be in flux as banks became accustomed to the transition. Our autoregressive model allow us to measure these temporal effects. To understand and see these dynamics, we can rewrite our model using lag operators, L, which map $Ly_{i,t} \rightarrow y_{i,t-1}$ and likewise $L^2 y_{i,t} \rightarrow y_{i,t-2}$. Therefore:

$$\left(1 - \sum_{j=1}^{p} a_{i,j} L^j\right) \tilde{y}_{i,t} = a_{i,0} + c_i z_{i,t} + \epsilon_{i,t}$$

which allows us to interpret $\tilde{y}_{i,t}$ as a function of lags.

$$\tilde{y}_{i,t} = \frac{a_{i,0}}{1 - \sum_{j=1}^{p} a_{i,j} L^j} + c_i \frac{z_{i,t}}{1 - \sum_{j=1}^{p} a_{i,j} L^j} + \frac{\epsilon_{i,t}}{1 - \sum_{j=1}^{p} a_{i,j} L^j}$$

The second term captures the impact of the event on mean return to equity. Since by construction we set $|a_{i,j}| < 1$, this impact effect is the sum of the geometric series:

$$c_i \frac{z_{i,t}}{1 - \sum_{j=1}^{p} a_{i,j} L^j} = c_i \sum_{n=0}^{\infty} \left[\sum_{j=1}^{p} a_{i,j} L^j z_{i,t} \right]^n$$

To understand the transitional impact directly during the event, we observe:

$$\begin{aligned}
I_{i,0} &= c_i \sum_{n=0}^{\infty} \left[\sum_{j=1}^{p} a_{i,j} L^j z_{i,t} \right]^n \\
&= c_i \sum_{n=0}^{\infty} [0]^n \\
&= c_i
\end{aligned}$$

This follows since during the event, $L^j z_{i,t} = 0$ for all j. Similarly, to understand the transitional impact one year after the event, note that now $L^j z_{i,t} = 0$ for all $j > 1$, and so we observe:

$$\begin{aligned}
I_{i,1} &= c_i \sum_{n=0}^{\infty} \left[\sum_{j=1}^{p} a_{i,j} L^j z_{i,t} \right]^n \\
&= c_i \sum_{n=0}^{\infty} [a_{i,1} L z_{i,t}]^n \\
&= c_i + a_{i,1} c_i \\
&= c_i \left(1 + a_{i,1}\right)
\end{aligned}$$

More generally bank i's response x years after the event can be calculated as:

$$I_{i,x} = c_i \sum_{n=0}^{\infty} \left[\sum_{j=1}^{x} a_{i,j} L^j z_{i,t} \right]^n$$

Given that these measurements of these impulse response functions are non-linear, constructing confidence intervals in a typical fashion is problematic as we cannot easily measure standard error. That said, since our panel is representative of banks nationwide, we can construct confidence intervals simply by ordering $I_{i,q}$ for each q and looking at the quantiles corresponding to the confidence intervals we want to construct.

Moreover, we can now decompose c_i to obtain estimates of market structure coefficients β_q and a bank specific residual c_i^*. Now we see:

$$\tilde{y}_{i,t} = \frac{a_{i,0}}{1-\sum_{j=1}^{p} a_{i,j}L^j} + \left(\sum_{k=1}^{q} \phi_{i,k}\beta_k + c_i^*\right)\frac{z_{i,t}}{1-\sum_{j=1}^{p} a_{i,j}L^j} + \frac{\epsilon_{i,t}}{1-\sum_{j=1}^{p} a_{i,j}L^j}$$

and likewise

$$\left(\sum_{k=1}^{q} \phi_{i,k}\beta_k + c_i^*\right)\frac{z_{i,t}}{1-\sum_{j=1}^{p} a_{i,j}L^j} = \left(\sum_{k=1}^{q} \phi_{i,k}\beta_k + c_i^*\right)\sum_{n=0}^{\infty}\left[\sum_{j=1}^{p} a_{i,j}L^j z_{i,t}\right]^n$$

bank i's response to each market structure characteristic k, x years after the shock can be understood as:

$$I_{k,i,x} = \phi_{i,k}\beta_k \sum_{n=0}^{\infty}\left[\sum_{j=1}^{x} a_{i,j}L^j z_{i,t}\right]^n$$

and similarly, bank i's unexplained response to deregulation x years after the shock can be understood as:

$$I_{c_i^*,i,x} = c_i^* \sum_{n=0}^{\infty}\left[\sum_{j=1}^{x} a_{i,j}L^j z_{i,t}\right]^n$$

6. Results

6.1. **Long-run and Short-run Effects of Deregulation.** From Table 2, we see that after deregulation, long-term mean bank profitability fell by 0.631 percentage points, and long-term median bank return on equity fell 0.751 percentage points. Given a median bank return on equity of 11.89% prior to deregulation, this drop equates to a median decline in profitability of roughly 6.3%. Given that our model controls for all national factors as well as serial correlation between bank profitability in successive years, this presents strong evidence that not only was deregulation associated with a decline in average bank return on equity, but it was it was the cause of a decline in average bank profitability. Using the 10% and 90% quartiles, we see that 80% of banks in our sample faced between a -8.216% percentage point drop and a 8.013 percentage point increase in long run profits following deregulation. The huge degree of variability in banks' response to deregulation suggests that deregulation alone is not the cause of long term changes in profitability–perhaps other interactions terms cause this observed variability in banks' long-term response to deregulation.

Table 2. Long-run Impact of Deregulation on Bank Return on Equity

	Median	80% confidence interval
Pre-event ROE	11.89%	6.04% - 17.19%
Post-event ROE	11.26%	4.69% - 17.11%
Event Effect	-0.75	-0.82 - 0.80

Intuitively, the mean and median drop in long-term profitability make sense in a classical industrial economic sense: We would expect the relaxation of geographic regulations to decrease bank concentration and remove monopolies, oligopolies, and other market imperfections. Thus, we would expect the removal of these market imperfections to increase competition–which would reduce deadweight loss, drive down producer surplus, and decrease bank profitability. The high degree of observed variability doesn't negate this, it simply suggests that their must be other factors which cause bank's to respond differently to deregulation.

While it is reasonable that long-term change in profits would depend on more than just the deregulation itself, it is reasonable to expect that in the early years following deregulation, profitability in all banks might plunge to reflect transition costs banks must absorb in order to compete in the new deregulated market.

For this, we observe the median impulse response for the first ten years following deregulation. Rather than overshooting the long-run median decline in profits, the impulse responses suggest that median profitability fell monotonically for the first five years before settling around the long term median decline of 0.751 percentage points observed in Table 2.

Directly after deregulation, the median bank saw an impact effect of a 0.536 percentage point decline in profits. Over the next four years, median profits fell an additional 0.322 percentage points before reaching long-term levels.

When we observe the 80% confidence intervals associated with these impulse responses, however, we see no evidence of the overshooting that large transition costs would suggest. In fact, 80% of banks experienced between a 6.28 percentage points decline and a 6.08 percentage point increase in return on equity one year following deregulation–which can be thought of as the impact effect of deregulation. This contradicts the hypothesis of some universal short-term decline in profits–as it suggest to the contrary that nearly as many banks experienced an increase in profitability following deregulation as experienced a decrease.

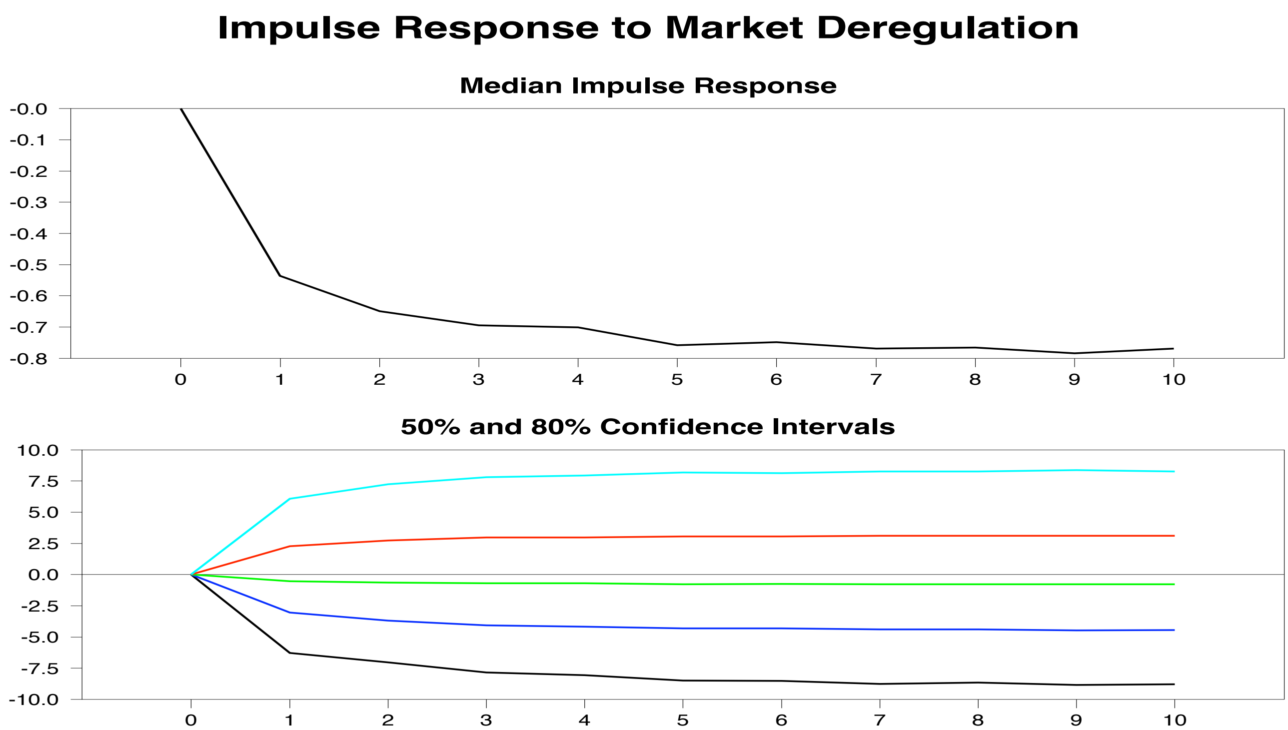

FIGURE 2. Median Response to Market Deregulation

6.2. **Interpreting the Variance.** The huge variability in banks' long run change in mean return on equity reflects the fact that other factors may play a large role in determining each banks' response to deregulation. Given that our model controls for national factors and trends in each banks' return on equity, the key factors unaccounted for in our are study are bank-specific factors which can differentiate one bank from another. Some of these factors are bank observable bank characteristics, and others are unobservable bank-specific factors.

In our second-stage regression we fit a weighted least squares regression to control for heteroscedasticity due to differences in the magnitude of the measurement error in our estimates of c_i. In order to reduce the impact out extreme outliers, we remove the 36 observations for which our estimates of $|I_{i,k}| > 1$ for $1 \leq k \leq 10$, since these 36 observations predict an unrealistic change in profitability greater than 100 percentage points. With the remaining 5534 observations, we determine the impact of four observable bank characteristics:

TABLE 3. Dep = weighted $\hat{c}_i$

Variable	**Coefficient** (Std. Err.)
Initial Efficiency	-0.025**
	0.012)
Initial ln (Size)	0.002***
	(0.002)
Initial Market Share	-0.013**
	(0.005)
Initial Concentration	0.020***
	(0.006)
Intercept	-0.027**
	(0.011)
N	5534
R^2	0.007
σ	0.035
F	9.94

From Table 3, we see that all four bank characteristics help to explain the variability in our dummy variable, c_i. Since the first impulse response $I_{i,0} = c_i$, we can understand the coefficients associated with the regression in Table 3 as helping to describe the variability in this impact effect. Moreover, since the impulse responses for each coefficient $I_{k,i,x}$ reflect the same autoregressive dynamics as the impulses responses from the simplified regression. We can derive the median long run effect of these relationships by comparing the ratios:

$$\frac{\text{median}\left(\text{Long Run Effect}_{\beta_k}\right)}{I_{k,i,1}} = \frac{\text{median}\left(\text{Long Run Effect}_{c_i}\right)}{\text{median}\left(I_{i,1}\right)} = \frac{-0.00751}{-0.00536} = 1.401$$

Thus, we can derive the long-run effect of each coefficient based on their impact effects by the relationship:

$$\text{median}\left(\text{Long Run Effect}_{\beta_k}\right) = 1.401\left(I_{k,i,1}\right).$$

From Table 3, we show that higher initial efficiency is associated with more negative impact effects from deregulation. This suggests that banks that minimized costs prior to deregulation tended to see a large decline in profits than their less efficient peers. Intuitively, this may seem problematic, but digging deeper, we see that on average, efficiency rose by 0.06 percentage points in the first year following deregulation, and by 2.82 percentage points over the five years following deregulation. Thus, it is reasonable to expect that banks that were more efficient prior to deregulation were less likely to see an increase in efficiency following deregulation. In Table 4, we model this relationship using OLS and see that a regression of initial efficiency on the five year change in efficiency, yields a strong negative relationship.

Table 4. Dep = Δ Efficiency

Variable	**Coefficient** (Std. Err.)
Initial Efficiency	-0.243***
	0.010)
Intercept	0.165***
	(0.008)
N	5534
R^2	0.089

These results support the claim that the negative relationship observed between initial efficiency and c_i is in fact spurious, and simply serves as a proxy for the impact that change in efficiency has on the change in profitability. It is telling, therefore, that while this relationship is significant to 3%, it is still the least significant coefficient in this second stage regression.

Next, we observe that larger banks are associated with more positive impact effects from deregulation. In fact, we can interpret these findings as suggesting that a bank with double the initial deposits of another bank is expected to have a 1.34 percentage point larger impact effect than the smaller bank, and a 1.88 percentage point larger long run effect. Two aspects of this result are noteworthy:

First, bank size was incredibly variable in our sample. The largest bank included in our analysis had $27,240,000 in initial deposits, while the smallest bank had only $482. The middle 50% of our banks had between $17,630 and $63,990 in initial deposits. This incredible degree of variance suggests that size differences of this scale were common. In fact, holding everything else constant, we would expect a from the 75th percentile in size to have an impact effect 2.49 percentage points higher than a back from the 25th percentile, and a long run change in profitability approximately 3.49 percentage points higher.

Second, it is important to realize that the above conclusions hold both concentration and market share constant. Thus, it is difficult to interpret the isolated impact of a larger bank size, as increases in bank size come hand in hand with increases to market share and concentration. For this reason a more appropriate intuition would be that given two counties with identical initial market shares and concentrations, the larger market (as defined by total market deposits) will out preform the smaller market following deregulation. In fact, we can now appropriately claim that given two markets with identical market structures, banks in a market with twice the total market deposits of its peer market will outperform banks in it's peer market by an average of 1.34 percentage points one year after deregulation, and by approximately 1.88 percentage points in the long run.

Next, we observe that larger initial market shares are associated with more negative impact effects from deregulation. Intuitively, this makes sense. Controlling for bank size, banks with larger initial market shares have more market power prior to deregulation, and thus they will be more likely to see a decline in profits when their market power is removed. Banks with smaller initial market shares are more accustomed to competing, and thus will be more prepared to deal with the more open deregulated markets.

That said, controlling for bank size and concentration, a bank with a commanding 75th percentile initial market share of 34% is only expected to have a -0.37 percentage point smaller impact effect than an equally sized bank with 25th percentile initial market share of 5.6%; and the bank with the smaller initial market share is still only expected to outperform by 0.51 percentage points in the long run.

Lastly, we observe that larger initial concentrations are associated with more positive impact effects from deregulation. This result, too, may seem puzzling at first, since higher concentration implies more market imperfections prior to deregulation, which we would associate with more negative declines in profitability when these market imperfections are removed. That said, holding some bank i's market share and size constant, an increase in market concentration implies tougher initial competition. Intuitively, we can interpret this as a decrease in relative market share, where

$$\text{Relative market share} = \frac{\text{Bank i's market share}}{\text{Largest competitor's market share}}$$

With this understanding, it becomes more intuitive that larger initial concentration suggest a higher degree of competition prior to deregulation, which would translate into more positive changes in profitability following deregulation.

Economically, this effect appears extremely significant. Controlling for bank size and market share, a bank in a competitive 75th percentile market prior to deregulation will face a concentration of 38% and is expected to have a 4.09 percentage point edge on a bank in a less competitive 25th percentile market with an initial concentration of 17.4%; in the long run, this effect is expected to lead the former bank to a return on equity 5.73 percentage points higher than the latter.

6.3. **Understanding the Effects of Measurement Error.** From Table 3, we identify four variables that help to explain variations in bank response to deregulation. While these coefficients were all statistically significant, a closer look reveals only an R^2 of 0.004, suggesting that our explanatory variables only explain 0.4% of the total variance in the data. Since there are two types of variance: measurement error which arises from the fact that we can only estimate the true dependent variable, c_i, and random error from the fact that our observations are random draws from some underlying distribution. The recorded R^2 captures the percentage of total variance explained by the model, and not the percentage of random error explained by the model. This is misleading, however, as we don't expect the model to account for any of the measurement error associated with our autoregressive estimates.

While we cannot measure or make claims as to the interaction terms, from the regression residuals and our bootstrap estimates, we can observe the variance due to measurement error and due to random error to be 0.0009034 and 0.001259, respectively. Ignoring issues arising from potential interactions, a simple ANOVA analysis suggests that measurement error could account for as much as

$$\frac{0.0009034}{(0.0009034 + 0.001259)} = 42\%$$

of the total variance. This suggests that the coefficients describes in Table 3 could explain a much larger degree of the variance in the parameter c_i then they do the estimated parameters $\hat{c}_i$, and then the R^2 portrays.

7. Conclusion

How do these findings line up in comparison to the vast literature on the effects of market structure on bank profitability? In so much as we can interpret the deregulation of intrastate branching as an exogenous shock to concentration and market shares, we present evidence that independent of other factors, a shock to market share and concentration has no statistically significant impact on average bank profitability. This presents evidence against both market power theories on bank profitability, which many past studies have supported.

Moreover, we show evidence to refute that claim that deregulation yields significant transition costs which would drive down short term profitability. In our study on the deregulation of geographic restrictions, we show evidence that banks respond quickly and efficiently to regulatory changes, and thus counter the fear of deadweight loss due to transition costs. Lastly, our analysis suggests that we can expect as much as

70% of the long term change in profitability following deregulation to be realized in the first year.

Next, we present evidence, that while deregulation does not change average bank profitability in the short or long run, it may be associated with a large degree of reshuffling of bank profitability. We determine three key initial characteristics which help explain which banks later emerge as 'winners' and which banks later emerge as 'losers' in the post-deregulation market. Given that these bank characteristics all interact and blend together, interpreting them individually is difficult. Instead, we offer a unified interpretation: following a shock to market share and concentration, big banks in highly competitive markets will fair far better than their smaller counterparts in less competitive markets. These findings are not only statistically significant, but they appear numerically significant as well: our results suggest that holding market share and initial profitability constant, a large bank in a market with 38% concentration is expected to see long run profits as much as 7.6 percentage points higher than a bank which is half its size in a market with only 17% concentration.

Moreover, if we assume that banks cannot significantly alter their market shares, market concentrations, and deposits in anticipation of a shock to market share and concentration, we can interpret these relationships not only as explaining the variation in banks profitability after deregulation; but, at least in part, as causing the variations in bank profitability after deregulation.

Care must be taken, however, before too strong conclusions are reached on the impact of deregulation on profitability and on the subsequent impact of banks initial market characteristics. Given the staggering variability present throughout our analysis, we must hesitate before clinging too tightly to the implications from this analysis–as the vast majority of variability in profits are still unexplained. As many economists have found before me, it seems that the true drivers of bank profitability are still unaccounted for.

What does this imply for the future of banking? While intrastate branch restrictions have long been removed, these deregulations are, in many ways, analogous to the recent trend of globalization in banking markets today. If the relationships identified in the era of intrastate branch restrictions still hold, then as banking become increasing global, we can expect industry profitability to remain relatively constant, and we can expect large competitive banks to fair better on average than smaller and more sheltered banks. These are promising claims for the future of U.S. banking, as the United States boosts some of the largest and most competitive banks in the world.

Appendix A. Econometric Procedures

Table 5. Variable List

Variable	Description	Specifics
Profit (a)	$\frac{\text{Net income before extraordinary charges}}{\text{Total assets}}$	$\frac{RIAD4300}{RCFD2170}$
Profit (b)	$\frac{\text{Net income before extraordinary charges}}{\text{Total equity}}$	$\frac{RIAD4300}{RCFD3210}$
Concentration	Herfindahl index on bank deposits	$\sum \left(\frac{RCFD2200}{\sum RCFD2200} \right)^2$
Market share	$\frac{\text{Total deposits}}{\text{Market deposits}}$	$\frac{RCFD2200}{\sum RCFD2200}$
Inefficiency	$\frac{\text{Noninterest expense}}{\text{Net income before extraordinary charges}}$	$\frac{RIAD4217 + RIAD4135}{RIAD4300}$
Size	Total deposits	RCFD2200

References

A.A. Alchian and H. Demsetz. Production, information costs, and economic organization. *The American Economic Review*, 62(5):777–795, 1972. ISSN 0002-8282.

B.D.A.S. AN. The real effects of US banking deregulation. *Review*, page 111, 2003.

J.S. Bain. Relation of profit rate to industry concentration: American manufacturing, 1936-1940. *The Quarterly Journal of Economics*, 65(3):293–324, 1951. ISSN 0033-5533.

G.J. Benston. The optimal banking structure: theory and evidence. *Compendium of issues relating to branching by financial institutions*, page 455, 1976.

A.N. Berger. The profit-structure relationship in banking–tests of market-power and efficient-structure hypotheses. *Journal of Money, Credit and Banking*, 27(2): 404–431, 1995. ISSN 0022-2879.

A.N. Berger, A.K. Kashyap, J.M. Scalise, M. Gertler, and B.M. Friedman. The transformation of the US banking industry: What a long, strange trip it's been. *Brookings papers on economic activity*, 1995(2):55–218, 1995. ISSN 0007-2303.

A.N. Berger, A. Demirguc-Kunt, R. Levine, and J.G. Haubrich. Bank Concentration and Competition: An Evolution in the Making. *Journal of Money, Credit & Banking*, 36(3), 2004.

J.A. Bikker and K. Haaf. Competition, concentration and their relationship: An empirical analysis of the banking industry. *Journal of Banking & Finance*, 26(11): 2191–2214, 2002. ISSN 0378-4266.

D.G. Freeman. Did state bank branching deregulation produce large growth effects? *Economics Letters*, 75(3):383–389, 2002. ISSN 0165-1765.

T.A. Garrett, G.A. Wagner, and D.C. Wheelock. A spatial analysis of state banking regulation*. *Papers in Regional Science*, 84(4):575–595, 2005. ISSN 1435-5957.

E.A. Hanushek. Efficient estimators for regressing regression coefficients. *American Statistician*, 28(2):66–67, 1974.

R.R. Huang. Evaluating the real effect of bank branching deregulation: Comparing contiguous counties across US state borders. *Journal of Financial Economics*, 87 (3):678–705, 2008. ISSN 0304-405X.

J. Jayaratne and P.E. Strahan. The finance-growth nexus: Evidence from bank branch deregulation. *The Quarterly Journal of Economics*, 111(3):639, 1996. ISSN 0033-5533.

Y. Jeon and S.M. Miller. Bank concentration and performance. *University of Connecticut Department of Economics Working Paper*, 2002.

J. Lewis. Two-stage approaches to regression models in which the dependent variable is based on estimates. *Typescript. UCLA*, 2000.

D.C. Mueller. The Determinants of Persistent Profits. *Washington, DC: Bureau of Economics, Federal Trade Commission*, 1983.

C.A. Northcott. *Competition in banking: A review of the literature*. Bank of Canada, 2004.

P. Pedroni and J. Park. Bootstrap unit root tests. Forthcoming in. *Econometrica*, 2003.

L.J. Radecki. The expanding geographic reach of retail banking markets. *Economic Policy Review*, 4(2), 1998.

D.J. Ravenscraft. Structure-profit relationship at the line of business and industry level. *The Review of Economics and Statistics*, 65(1):22–31, 1983. ISSN 0034-6535.

S.A. Rhoades. Structure-performance studies in banking: A summary and evaluation. *Staff Studies*, 1977.

P.S. Rose and D.R. Fraser. The Relationships Between Stability and Change in Market Structure: An Analysis of Bank Prices. *The Journal of industrial economics*, 24(4):251–266, 1976. ISSN 0022-1821.

M. Smirlock. Evidence on the (non) relationship between concentration and profitability in banking. *Journal of Money, Credit and Banking*, 17(1):69–83, 1985. ISSN 0022-2879.

F. Tregenna. An empirical investigation of the effects of concentration on profitability among US banks. *MPRA Paper*, 2009.